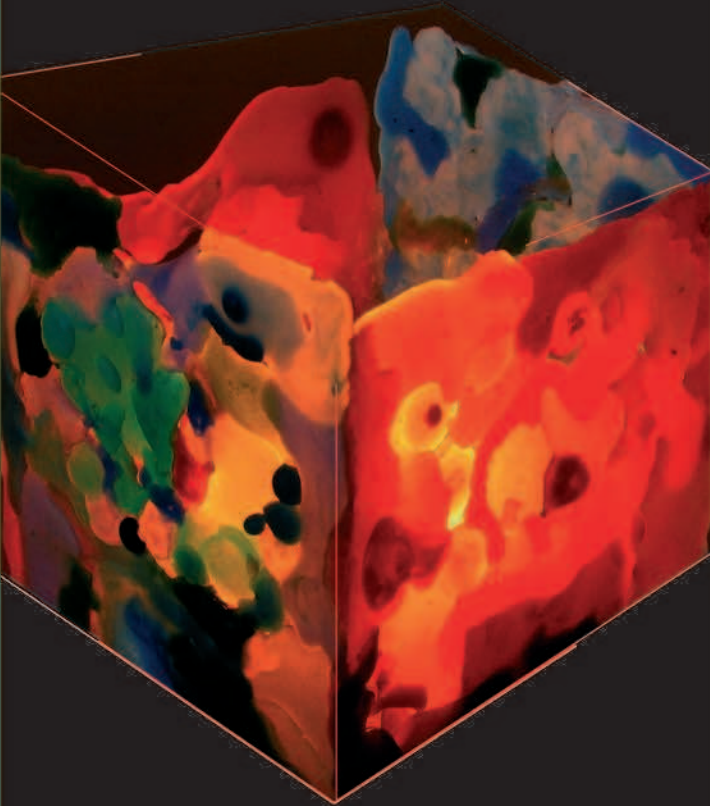


**Jurij FIKFAK
Frane ADAM
Detlef GARZ**
Eds.



QUALITATIVE RESEARCH

Q U A L I T A T I V E
R E S E A R C H



QUALITATIVE RESEARCH
Different Perspectives – Emerging Trends

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Q U A L I T A T I V E R E S E A R C H

D I F F E R E N T P E R S P E C T I V E S
E M E R G I N G T R E N D S

EDITED BY

JURIJ FIKFAK
FRANE ADAM
DETLEF GARZ

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VOICES OF QUALITATIVE RESEARCH: BETWEEN DIFFERENT PERSPECTIVES AND EMERGING TRENDS

JURIJ FIKFAK

The instruments of qualitative methodology are certainly one of the 20th century's greatest achievements among the different research methods in the humanities and social sciences, especially among those that deal with the discourse and world view of the other – the informant, his or her verbal, visual and other messages. In this period several important reflections on the forms and limits of research practice took place: reflections on the informant and the images and theories of the world with which he or she lives and co-creates his or her life; on the ways and means of recording and translating statements or field information into a language that is understandable to academic circles and the interested public; and last but not least, reflections on the researcher's world view with and through which he or she interprets and attempts to understand the culture of the other.

Concepts and authors – participant observation, diary notes and Bronislaw Malinowski [Stocking 1993]; phenomenology, the life world and Alfred Schütz [1970; 1972; Schutz and Luckmann 1973]; symbolic interactionism and Herbert Blumer [1986; Reynolds and Herman-Kinney 2003] and his followers, e.g., ethnomethodology and Harold Garfinkel [1967; Lynch and Sharrock 2003] or self-presentation and Erving Goffman [1959]; the Chicago school, the ethnography of the city, the outsider and Robert E. Park [Lindner 1990]; grounded theory and Barney Glaser with Anselm Strauss [Glaser and Strauss 1967]; thick description and Clifford Geertz [1977]; objective hermeneutics and Ulrich Oevermann [1979]; narrative interview and Fritz Schütze [1977] with Gerhard Riemann [1987]; sociohistoric hermeneutics and Hans G. Soeffner [1989; 1992]; “eclectic” approaches and e.g. Gabriele Rosenthal [1993; 1995] – to name just a few of them, have become an indispensable part of the self-reflection of every researcher.¹

There were two primary motives for organizing the conference on qualitative research at Bled in Slovenia in November 2000. First, to review several contemporary world trends in qualitative research, document them and explain them with concrete examples. And secondly, to ascertain the basic tendencies in this field in Slovenia. There was another important reason for organizing the conference: to allow Slovenian and foreign experts from different disciplines, especially those dealing with researching culture and society (ethnology and anthropology, sociology, social work), to present their knowledge and achievements and on the basis of these gain a starting point

¹ There are many overviews of qualitative research (history, methods, etc.). Among them are: Miles and Huberman [1994], Flick [1998; 2004], Denzin and Lincoln [1998a; 1998b], etc. One of the most important bibliography is about the field of ethnography and was prepared by Borut Telban: *Bibliography of fieldwork, research methods and ethnography in sociocultural anthropology* [2001].

for mutual collaboration and incentives for the further development of qualitative research.

The creative collaboration between the researchers from Institute for Ethnology ZRC SAZU (Jurij Fikfak) and the Institute for Social Sciences at the Faculty of Social Sciences (Frane Adam) allowed also the following connections: with the Faculty for Social Work (all in Ljubljana, Slovenia), the Pedagogic Institute of the Johann Gutenberg University in Mainz (Germany), the Institute for Sociology and Cultural Anthropology in Fredericton, NB, Canada, the Institute for Philosophy of the Innsbruck University, the Institute for Social History of the Vienna University, the Institute for Sociology in Łódź, the QUARC.DE company, the Institute for the Research of Socialization and Social Psychology at the Goethe University in Frankfurt am Main. The titles of the institutions speak eloquently of the basic fields we wished to cover with the organization of the conference and the preparation of the book.

The differences between the planned program of the conference, its realization and the final, published form are typical of research practice and were often also positively creative. The original desire to organize four thematic sections – an overview of qualitative research in Slovenia; the possibilities and potentials of computer assisted qualitative analysis; new conceptual approaches, e.g. biographic methods and narrative interviews; and the application of qualitative methods in encouraging democracy and civil society initiatives – was largely realized at the conference itself. However, the time between the meeting and publication affected a somewhat different form of the publication; new colleagues were invited to collaborate, certain new topics were covered, some participants didn't write a text or contributed a different one for the book.

The discussion by **Frane Adam** and **Darka Podmenik**, *The Reception of Qualitative Research in Slovenia* opens epistemological questions regarding the contemporary role and possibilities of qualitative research as well as defining the basic characteristics of its development in Slovenia, the most important works and authors of this methodology. In Slovenia is primarily quantitative oriented sociology, favored by the political establishment for several decades, implicitly still resistant to the qualitative research.

Will C. van den Hoonaard in *The Epistemological Framework of Qualitative Research: Is it still a Cinderella to quantitative research?* states that induction is the basic form of scientific experience and comprehension of the world, while the wheel of science and the spiral of research are the basic metaphors of research strategies. He has dealt with the development and digression of the concept in fourteen methodological books. One of the most important guidelines is a Sherlock Holmes-like orientation towards facts first, which must eventually lead to theory. It is a direction from the particular towards the general, but also a recognition of complex settings where the researcher does not invent but merely tries to understand the informant's point of view.

In his work *The Individual and the Societal* the **Reinhard Sieder** on the one hand introduces Ulrich Oevermann's objective hermeneutics and on the other hand the grounded theory of Barney Glaser and Anselm Strauss. Within the scope of this fusion of two methodological perspectives it is possible to compare his efforts with the research practice of Gabriele Rosenthal; however the qualitative research of Reinhard Sieder is throughout defined not only by the psychological but also by the cultural-historical understanding of the image of the world, which the informant presents through the example of Peter Treumann (the anonymous, altered surname is an allusion to a

man loyal to the state) who started his career in the Hitlerjugend (HJ) and finished it in the German Army. Through this example he attempts to define the relationship between individual decisions and the collective social *Zeitgeist*, also with reference to the forgotten Johann Gustav Droysen.

In the second part of the book, on the researcher and his or her “object,” **Tatiana Bajuk Senčar** in *The Role of Fieldwork in Contemporary Anthropology* “assesses the role and possibilities of fieldwork as an anthropological research method in light of anthropology’s coming to terms with the reality of globalization. Globalization in this context refers to a realization of the profound changes to the relationships between cultures which were previously assumed to exist separately from one another. How has this affected the practice of anthropology, a discipline defined by its focus on cultural difference? To answer this question the paper discusses critiques of fieldwork as a research method and the implications that this may have on developments in anthropological research and on the effectiveness of fieldwork as an analytical tool. Finally, the paper concludes with a brief discussion of the author’s own field research as an anthropological project within the interdisciplinary field of science studies in order to discuss present challenges to conducting fieldwork and the possible contributions fieldwork may have to offer anthropology as well as other disciplines.”

Jurij Fikfak in *From Ethnography to Autoethnography* presents several stories through which he problematizes first the viewpoint of the informant, which is fatally marked by his or her life story, and thus a tempting “object” for the researcher studying the everyday as an unending effort to overcome crisis and constitute the normalcy, the routine. The problematization of the viewpoint of the foreign researcher (the Westerner viewing East Europeans) requires a discussion on the influences of different systems on the life story of the scientist and the formation of an (auto)referential web of scientific discourse. The last story, of the meeting of researcher and informant, problematizes the colored, ideological world of informant and researcher and attempts to find the common ground on which they can come together. Ethnography and autoethnography are indispensable to the recording of practice.

The main question in the paper *The Pleasures and Dangers of Narrative, Introducing the problematics of the sociology of methodology* by **R. Danielle Egan** is: What happens when the researcher puts him or herself “on the stage?” Egan writes: “By exploring my position as an exotic dancer/researcher, I deconstruct the sticky wicket of honoring the voices of those with whom we work and the violence of objectivity. In so doing, I critique the construct of the truth telling both as a function of narrative and of methodological correctness. Through implementing the work of poststructural theory and Lacanian psychoanalysis, I pose the question of the methodological dangers in promoting ‘authentic’ voice within standpoint epistemology, while simultaneously seeking to avoid the objectification of narrative within positivism. In so doing, this text offers a conception of poststructural and psychoanalytic theory as a way in which we, as critical and politically committed methodologists, can both engage with narrative while recognizing its reliance on discourse and that which haunts language – the unconscious. By illuminating both the discursive and unconscious gaps which flow through language we can employ a form of methodology which is critically reflexive of its own practice and seeks to place practice within broader socio-historical contexts.” However, it is unclear to what extent the researcher’s own statements apply to herself as well. To what extent

her occupation with her own body, her own view of her body, reflects an interiorization of the male gaze – how the woman who displays herself, sees herself only after and through the man, who watches her [cf. Frank 2002a; 2002b].

Ulrich Oevermann in the *Manifest der objektiv hermeneutischen Sozialforschung*² states: “The main subject of Objective Hermeneutics (hereinafter OH) methodology are the latent structures of meaning and the objective semantic structures of expressive configurations...”; access to the social life world, to expressive configurations (*Ausdrucksgestalt*) is possible only indirectly, through the textualized document, the protocol. The researched reality is thus accessible and legible only through the analysis of this protocol.³ The starting point for or aim of the analysis of any protocol is not the subjective intent of the text; rather it is the reconstruction of meaning structures. While most qualitative research typically deals with coding and classification, OH is based on sequential analysis, which seeks to find *generative rules*. The basic premise of sequential analysis is that every life practice generates possibilities among which the subject then chooses. Among these choices Oevermann – basing on Charles S. Peirce and George H. Mead – posits crisis as the constitutive – normal – and creative part of any living practice and routine as the exception.

It is worth mentioning my own field experience researching the creativity of ritual practices, among human behaviors almost certainly the most “stable” or traditionally “fixed” of actions. Analysis shows that an integral part of ritual is uncertainty (a sign of crisis) whether it will fulfill all the criteria – the required model; to carry out a ritual as a desired picture of harmony – the fulfillment is thus actually the management of the crisis, of the conflict. Here remembering the old days of the ritual has the purpose of dimming negative experiences, mostly reawakening the world of childhood (primary socialization) and at the same time it entails an attempt to reshape and remodel the ritual.

Among the important concepts of OH are also the interpretation of the dynamics of the life world and the transformation of life practices. To this concept, however, the case description (*Fallbeschreibung*) is no longer sufficient, and the case reconstruction (*Fallrekonstruktion*), which allows us to discover the structural rules of concrete life practices, is required. This also allows us to interpret social change (*Sozialer Wandel*) and the emergence of the new as a basic, constitutive constant. Here the OH methodology has its own structural modus of generalizations, thus it is not subject to the subsumptional logic otherwise prevalent in qualitative research. The concept of case reconstruction (Oevermann assigns it the characteristics of abduction in the Peircean sense) allows us, after only a small number of examples chosen according to the criterion of maximum contrast (in the most complex examples 12) present all the possibilities and structural rules of expressive configurations.

In evaluating the data (*Datenauswertung*) Oevermann finds Clifford Geertz’s thick description questionable, as it is a description, at best of artistic or literary value, and not a methodic or controlled interpretation. To him it is critical to evaluate the data,

² Oevermann’s article is written only in German: my longer summary of his paper is translation as well as paraphrasing of some of key Oevermann’s sentences in statement from the *Manifest*.

³ Implicitly Oevermann emphasizes the text as the basic element – regarding the possibility of analysis – confirming Jurij M. Lotman’s theory of language as the primary communication system.

which is why he especially criticizes the subsumptional logic. Instead of a theory deriving from data, it often turns out that in interpretation all the distractive, “illogical” parts are dropped. Oevermann says, that “the data are not here to express or put forward a theory, but to show an authentic reality. ... theories are solidified reconstructions of the example.” Thus Oevermann finds important the authenticity of the reconstruction of the example and falsification through sequential analysis, rather than the utilization of theoretical concepts, and testing their validity and reliability. From all that has been said, it is possible to conclude that objective hermeneutics possesses a unity of basic and applied research, as it always deals with concrete examples and searches within them for latent structures of meaning.

Axel Fehlhauer and **Detlef Garz** in the paper “*And since it is the first time...*” *On the educational shaping of the body of the class* use the methods of objective hermeneutics to analyze a lesson, and find an initial uncertainty among pupils and teacher as well as the mechanisms that the teacher uses to (re)establish order and his authority. Thus the pedagogic hour is an interplay of eroticity and manipulation, which the teacher utilizes to set up a hierarchic relationship with himself in the role of master.

The second research basis of research is grounded theory, presented by Kaja Kazmierska, Blaž Mesec, Vanja Huzjan and Susanne Frieze. The narrative interview, one of the most important techniques of grasping reality in qualitative research, was developed by Fritz Schütze (later with Gerhard Riemann). It is thoroughly presented by **Kaja Kazmierska** in *Narrative Interview as a Method of Biographical Analysis*. She deals with the introductory question, which should trigger the narrative and allow the interviewee to relate and present his or her world view. Consequently the interviewee would to an extent reconstruct – animate the course of events, and at the same time indicate which layers of memory, which background constructs (*Hintergrundskonstruktionen*) have settled atop this memory. Here it is the temporal gaps, delays and topic changes, for example, which tell us that something important happened in the interviewee’s life at that point.

How qualitative research began to dominate the discourse in the field of social work is presented in the paper of **Blaž Mesec**. *A Case of Grounded Theory Construction in Social Work* presents a short overview of qualitative research in the field of social work in Slovenia to give an impression of the context in which the research, described as a case of grounded theory construction, was conducted. During the research a social services case involving child neglect and abuse was analyzed. The qualitative analysis comprised first the construction of a chronology of events, time patterns analysis, action-patterns analysis, and finally an analysis of implicit theories. The analysis is followed by the final theory which states that in this case a reactive, administrative, and moralistic strategy was used by the social services rather than a proactive, helping, empowering, pragmatic and systemic strategy.

In the research *The Qualitative Analysis of an Interview* **Vanja Huzjan** analyzes a conversation with the organizer of a village dance with the help of grounded theory. The selection of the main informant and the village holiday, an event critical to the self-understanding and self-image of the village, indicate the crisis in which the village found itself when an international research camp took place there. A very detailed analysis of the conversation showed the division of the youth into insiders and outsiders, their difficulties in accepting the village identity, possible changes in the relation

of the youth to village traditions; and last but not least it highlighted the difficulties of communication between researcher and informant and the influence of technique on the course of the conversation.

In the chapter on computer assisted qualitative analysis **Susanne Friese** – *Computer-aided Qualitative Data Analysis: An Overview* reviews and defines the characteristics of the most important current programs: ATLAS.ti 5, MAXqda & MAXDictio, The Ethnograph 5.08, and QSR Nud*ist 6, and in particular detail Atlas.ti. The detailed description is followed by an overview table including two additional packages (Hyper-Research and QSR NVivo 2). It is important to emphasize that the program is only a tool to allow the researcher easier and faster analysis of material.

Josef Zelger in *Qualitative Research by the "GABEK" Method* presents his program Gabek Winrelan, which allows both detailed description and interpretation of problems as well as practical development of goals and their correlation. It is a procedure, which includes the experiences of people and simplifies or allows the orientation of decision making in complex problem situations. It supports the development of strategic principles and the presentation of results in an interactive manner.

The work concludes with a paper on ethical questions, *Fostering Human Dignity: Some Inherent Ethical Dimensions of Qualitative Research*, by **Will C. van den Hoonaard**. The author tackles the questions that arise when a researcher using a qualitative methodology requests an ethics commission for approval of the research. Existing commissions (the author speaks of Canadian and American examples) are mostly composed of researchers dealing in quantitative, primarily biomedical studies, which have often also defined the standards of ethical research. Thus, the questionnaires themselves often contain elements which are either unacceptable or unmanageable for a qualitatively oriented researcher. Van den Hoonaard polemicizes with the in advance defined list of questions, the mandatory written consent of the informant; he asks himself about anonymity and points to various possibilities of resolving this issue.

Frane Adam helped the most to make the conference happen, collaborating in its organization, preparation and financial construction. The publication was also made possible with the co-financing organized by Detlef Garz and Blaž Mesec.

There are no scientific publications or conferences without sponsors, without institutions which kindly support the meeting and exchange of opinions and viewpoints, discussion and new ways of discovering the image of the other. The following institutions helped financially or organizationally in organizing the conference and publishing the book: the Ministry of Education, Science and Sport of the Republic of Slovenia, the Scientific Research Center – Slovenian Academy of Sciences and Arts and its Research Fund, the Institute for Social Sciences at the Faculty for Social Sciences, the Pedagogic Institute of the Johann Gutenberg University in Mainz, the Faculty for Social Work at the University of Ljubljana, and the Nova Gorica Polytechnic. For the organization of a seminar on computer assisted qualitative research with the program Atlas.ti we thank Matjaž Žnidaršič and the company Marand.

GLASOVI KVALITATIVNEGA RAZISKOVANJA: MED NASTAJAJOČIMI TOKOVI IN RAZLIČNIMI PERSPEKTIVAMI

JURIJ FIKFAK

Med različnimi raziskovalnimi metodami na področju raziskovanja družbenih in humanističnih ved, posebej tistih, ki imajo opraviti z govorico in s podobo sveta drugega – informatorja, njegovih verbalnih, vizualnih in drugih sporočil, je razpoložljivi instrumentarij kvalitativne metodologije gotovo eden najpomembnejših dosežkov 20. stoletja. V tem obdobju so se zgodili pomembni premisleki o oblikah terenskega dela in njegovih mejah; o informatorju in podobah, teorijah sveta, s katerimi tako ali drugače živi in so-ustvarja svoje življenje; o oblikah zapisovanja, prevajanja povedanega ali na terenu sporočenega v govorico razumljivo krogu raziskovalcev in zainteresirane javnosti; in končno o raziskovalčevih podobah sveta, s katerimi bere in skuša razumeti kulture drugega.

Pojmi in avtorji – opazovanje z udeležbo, dnevniški zapisi in Bronislaw Malinowski [Stocking 1993]; fenomenologija, življenjski svet in Alfred Schütz [1970; 1972; Schutz and Luckmann 1973]; simbolični interakcionizem, Herbert Blumer [1986; Reynolds and Herman-Kinney 2003] in miselni dediči, npr. etnometodologija in Harold Garfinkel [1967; Lynch and Sharrock 2003] ali prezentacija samega sebe in Erving Goffman [1959]; čikaška šola, etnografija mesta, outsider in Robert E. Park [Lindner 1990]; utemeljitvena teorija (grounded theory) in Barney Glaser z Anselmom Straussom [Glaser and Strauss 1967]; gost opis in Clifford Geertz [1977]; Ulrich Oevermann in objektivna hermenevtika [1979]; narativni intervju in Fritz Schütze [1977] z Gerhardom Riemannom [1987]; družbenozgodovinska hermenevtika in Hans Georg Soffner [1989; 1992]; "eklektični" prijemi in npr. Gabriele Rosenthal [1993; 1995] – naj naštejemo samo nekatere njih, so postali neogibni del samo-premisleka vsake/ga raziskovalke/ca.¹

Za organizacijo konference o kvalitativnem raziskovanju na Bledu v Sloveniji novembra 2000 sta bila odločilna dva motiva: pregledati nekatere sodobne svetovne tokove v kvalitativnem raziskovanju, jih dokumentirati in razložiti s konkretnimi primeri in ugotoviti oziroma razbrati osnovne težnje, ki se kažejo na tem področju na Slovenskem. Za organizacijo je bil pomemben še en motiv – da tako slovenski kot tuji strokovnjaki iz

¹ Med številnimi temeljnimi pregledi in priročniki kvalitativnega raziskovanja velja omeniti dela naslednjih avtorjev: Miles and Huberman [1994]; Flick [1998; Flick 2004]; Denzin and Lincoln [1998a; 1998b; 1998c; 1998d]. Med najbolj temeljitimi pregledi produkcije v angleškem jeziku na področju etnologije/kulturne antropologije (in s tem tudi kvalitativnega raziskovanja) je Boruta Telbana *Bibliography of fieldwork, research methods and ethnography in sociocultural anthropology* [2001], ki obsega naslednja poglavja: Uvod in terensko delo, Metode raziskovanja, Etika in terensko delo, Spol in terensko delo, Pisati etnografijo, Zgodovina in teorija antropološkega raziskovanja, Osebna poročila o antropološkem terenskem delu.

različnih disciplin, predvsem raziskovalci kulture in družbe (etnologija in kulturna/socialna antropologija, sociologija, socialno delo), predstavijo svoja znanja in dosežke, da bi na tej osnovi pridobili ustrezno izhodišče za medsebojno sodelovanje ter spodbude za nadaljnji razvoj kvalitativnih raziskav.²

Izhodišče konferenci je bilo tvorno sodelovanje med Inštitutom za slovensko narodopisje ZRC SAZU (Jurij Fikfak) in Inštitutom za družbene vede na Fakulteti za družbene vede (Frane Adam), ki je omogočilo nadaljnje povezave: s Fakulteto za socialno delo (vse v Ljubljani – Slovenija), s Pedagoškim inštitutom Gutenbergove univerze v Mainzu (Nemčija), Inštitutom za sociologijo in kulturno antropologijo v kanadskemu Frederictonu (New Brunswick), Inštitutom za filozofijo univerze v Innsbrucku, Inštitutom za socialno zgodovino univerze na Dunaju, Inštitutom za sociologijo v Łodzi, podjetjem QUARC.DE, Inštitutom za raziskavo socializacije in socialno psihologijo Goethejeve univerze v Frankfurtu na Maini. Naslovi ustanov in dosedanja raziskovalna praksa udeleženih raziskovalcev zgovorno kažejo na tista osnovna področja, ki smo jih z organizacijo konference in pripravo knjige zajeli.

Za raziskovalno prakso so razlike med načrtovanim programom konference, njegovo realizacijo in končno, objavljeno, podobo značilne in največkrat tudi tvorne. Prvotna želja, organizirati štiri tematske sklope – pregled kvalitativnega raziskovanja na Slovenskem; možnosti in potenciali računalniško podprte kvalitativne analize; novi konceptualni prijemi, npr. biografske metode in narativni intervju; aplikacije kvalitativnih metod za spodbujanje aktivnosti za demokracijo in civilnodružbene iniciative – je bila na sami konferenci v veliki meri uresničena. Čas med samim srečanjem in izidom dela pa je vplival na nekoliko drugačne podobe publikacije: k sodelovanju so bili pritegnjeni nekateri novi sodelavci, obravnavane so nekatere nove teme, nekateri udeleženci besedila niso napisali ali pa so za knjigo prispevali drugo besedilo.

Razprava **Franeta Adama** in **Darke Podmenik** *The Reception of Qualitative Research in Slovenia* odpira epistemološka vprašanja o vlogi in možnostih kvalitativnega raziskovanja v sodobnem času in definira osnovne značilnosti razvoja na Slovenskem, najpomembnejša dela in avtorje te metodologije. Pretežno kvantitativno usmerjena sociologija, ki je že več desetletij deležna tudi izrazite podpore političnega establišmenta, implicitno še vedno ne sprejema kvalitativnega raziskovanja.

Will C. van den Hoonaard v *The Epistemological Framework of Qualitative Research. Is it still a Cinderella to quantitative research?* obravnava indukcijo kot temeljno obliko znanstvenega zaznavanja in razbiranja sveta, kolo znanosti in spirala raziskovanja pa sta zanj temeljni metafori raziskovalnih strategij. Razvoj in digresije pojma razbira v štirinajstih metodoloških knjigah. Eno najpomembnejših vodil je Sherlocka Holmesa usmeritev k dejstvu, ki šele morajo ponuditi teorijo. Gre za smer od partikularnega k splošnemu, a tudi za prepoznavanje bolj kompleksnih prizorišč, pri čemer raziskovalec ne iznajdeva, pač pa poskuša razumeti stališča informatorja.

Reinhard Sieder v svojem raziskovalnem delu *The Individual and the Societal* na eni strani uvaja Ulrich Oevermannovo objektivno hermenevtiko, na drugi pa utemeljitveno teorijo (*grounded theory*) Barneya Glaserja and Anselma Straussa. V tem okviru, torej v združevanju dveh metodoloških perspektiv, je možno njegova prizadevanja in raziskovanje primerjati z raziskovalno prakso Gabriele Rosenthal; vendar je

² Program konference je dostopen na spletnem naslovu <http://www.zrc-sazu.si/isn/bled2000.htm>.

kvalitativno raziskovanje Reinharda Siederja vseskozi definirano ne le s psihološkim, marveč tudi s kulturno-zgodovinskim razumevanjem podobe sveta, ki jo informator predstavlja na primeru Petra Treumanna (anonimizirani priimek je aluzija na moža zvestega državi), ki je svojo kariero začel v Hitlerjugendu (HJ) in jo končal kot oficir nemške vojske. Na tem primeru poskuša definirati razmerje med individualnimi odločitvami in kolektivni družbeni *Zeitgeist*, pri čemer se sklicuje na pozabljenega Johanna Gustava Droysena.

V drugem poglavju RAZISKOVALEC/KA IN NJEGOV/NJEN "OBJEKT" **Tatiana Bajuk Senčar** v *The Role of Fieldwork in Contemporary Anthropology* razmišlja o vlogi in možnostih terenskega dela kot antropološke raziskovalne metode v razmerah globalizacije, ki je temeljito spremenila razmerja med kulturami, za katere smo domnevali, da živijo druga mimo druge. Kako so ta dejstva vplivala na raziskovalno prakso antropologije, katere fokus so kulturne razlike? Avtorica preišča kritike terenskega dela kot raziskovalne metode in implikacije, ki jih ima za razvoj antropološkega raziskovanja in učinkovitost terenskega dela. V sklepu avtorica predstavi svoje terensko raziskovanje v interdisciplinarnem antropološkem projektu in preišča o današnjih izzivih in prispevku terenskega dela za razvoj antropologije in drugih disciplin.

Jurij Fikfak v *From Ethnography to Autoethnography* na primeru nekaterih zgodb problematizira najprej pogled informatorja, ki je usodno zaznamovan s svojo življenjsko potjo – zgodbo, in je zato za tistega raziskovalca, ki raziskuje vsakdanjost kot poskus nenehne prevladovanja krize, konstituiranja normalnosti – rutine in s tem tradicije prav zato tudi mikaven "objekt." Problematizacija pogleda tujega raziskovalca (zahodnjaka na vzhodnjake) pomeni začetek diskusije o vplivih različnih sistemov na življenjsko pot znanstvenika in oblikovanje (avto)referencialne mreže znanstvenega diskurza. Zadnja zgodba o srečanju med raziskovalcem in informatorjem problematizira barvni – ideološki svet informatorja in raziskovalca in išče skupne točke, v katerih se oba lahko skupaj najmeta. Etnografija in avto-etnografija sta neogibni, sestavni del zapisovanja prakse.

Glavno vprašanje v razpravi *The Pleasures and Dangers of Narrative, Introducing the problematics of the sociology of methodology* **R. Danielle Egan** je: Kaj se zgodi, ko se raziskovalec/ka "na ogled postavi." Eganova (raziskovalka in eksotična plesalka) odpira problematiko reifikacije pripovedi v kvalitativnem raziskovanju, pri tem dekonstruira spoštovanje glasov tistih, s katerimi dela in nasilje objektivnosti, s tem pa tudi konstrukt govora resnice kot funkcije pripovedi in metodološke korektnosti. Z implementacijo del poststrukturalne teorije in Lacanove psihoanalize zastavlja vprašanje o metodoloških nevarnostih pri objavljanju "avtentičnega" glasu znotraj ustaljene epistemologije in želi obiti popredmetenje pripovednega znotraj pozitivizma. Besedilo tako ponuja koncept poststrukturalne in psihoanalitične teorije kot pot, na kateri se kritični in politično zavezani metodolog lahko angažira s pripovedjo in njeno veljavnostjo zanesljivostjo reliance diskurzivno in tistim kar lovi jezik – nezavednim. Z osvetlitvijo diskurzivnih in nezavednih lukenj v jeziku želi metodologijo, ki bo kritično reflektirala lastno prakso in jo umestila znotraj širšega družbenozgodovinskega konteksta. Vsekakor pa ni jasno, v kolikšni meri je tisto, kar pove raziskovalka, usmerjeno tudi na njo samo, koliko je v ukvarjanju z lastnim telesom, v lastnem pogledu nanj, prisotna interiorizacija moškega pogleda – kako se ženska, ki se izpostavi, vidi šele prek moškega, ki jo gleda [prim. Frank 2002a; 2002b].

V poglavju RAZLIČNE PERSPEKTIVE so zbrani prispevki na dva različna koncepta: *Objektivna hermenevtika in utemeljitvena teorija*.

Glavni cilji in metodološke inovacije objektivne hermenevtike so zapisani v manifestu *Manifest der objektiv hermeneutischen Sozialforschung*³ **Ulricha Oevermanna**. "Osrednji predmet metodologije OH so latentne strukture smisla in objektivne pomenske strukture izraznih konfiguracij..."; dostop do družbenega življenjskega sveta, do izraznih konfiguracij (*Ausdrucksgestalt*) je možen le posredno, le z ubesedenim dokumentom, tj. protokolom. Raziskovana resničnost je tako dostopna in berljiva le z analizo tega protokola. Izhodišče analize katerekoli protokola niso subjektivne intencije teksta, marveč rekonstrukcija objektivnih pomenskih struktur. Medtem ko je za večino kvalitativnega raziskovanja značilno kodiranje in klasifikacija, je za OH značilna sekvenčna analiza, ki ugotavlja generativna pravila in izčrpno analizira sekvenco za sekvenco. Izhodišče sekvenčne analize je, da vsaka življenjska praksa generira možnosti, med katerimi subjekt izbira. Ob teh izbirah je za Oevermanna – pri tem se naslanja tako na Charlesa S. Peircea kot na Georga H. Meada – kriza konstitutivni – normalni – pravzaprav ustvarjalni del katerekoli življenjske prakse, rutina pa izjema.

Ob tem velja mogoče opozoriti na lastne terenske izkušnje ob raziskovanju kreativnosti ritualnih praks, med človeškimi vedenji gotovo najbolj "ustaljenimi" ali s tradicijo "fiksiranimi" opravili. Analiza pokaže, da je sestavni del rituala negotovost (znamenje krize), ali bo zadostil vsem kriterijem – predpisanemu modelu [prim. članek Huzjanove]; izvesti ritual kot zaželeno sliko harmonije – izpolnitve je tako pravzaprav obvladovanje krize, konflikta. Pri tem ima spominjanje na stare čase rituala namen zbrisati negativne izkušnje, največkrat obuditi svet otroštva (primarne socializacije) in hkrati skuša sooblikovati – modelirati novo izvedbo.

Med pomembnimi koncepti OH je tudi razbiranje transformacije življenjskih praks in dinamike življenjskega sveta. Temu konceptu pa ne zadošča opis primera (*Fallbeschreibung*), marveč rekonstrukcija primera (*Fallrekonstruktion*), ki omogoča ugotoviti strukturne zakonitosti konkretnih življenjskih praks, ljudi; omogoča pa tudi razbiranje družbenih menjav (*Sozialer Wandel*) in nastajanja novega kot bistvene, konstitutivne stalnice. Pri tem so metodologiji OH lastne strukturne in ne konkretne, empirične posplošitve, zato ni podvržena subsumcijski logiki siceršnjega kvalitativnega raziskovanja. Sam koncept rekonstrukcije primera (Oevermann mu pripisuje značilnosti abdukcije v Peirceovem smislu) omogoča, da že malo primerov, izbranih po kriteriju maksimalnega kontrasta (v najbolj kompleksnih primerih 12) predstavi vse razpoložljive možnosti, strukturne zakonitosti izraznih konfiguracij.

Pri obravnavi podatkov (*Datenauswertung*) je za Oevermanna med najbolj spornimi Clifforda Geertza gost opis (*Thick description*), saj gre za opis, v najboljšem primeru umetniške – literarne vrednosti in ne za metodično kontrolirano interpretacijo. Pomembnejše je zanj ovrednotenje podatkov, pri čemer posebej kritizira subsumcijsko logiko, ki prevladuje v kvalitativnem raziskovanju. Namesto teorije, ki naj izhaja iz podatkov, pri čemer se pogosto izkaže, da pri intepretaciji odpadejo vsi moteči "nelogični" deli, Oevermann pravi: "podatki niso zato tu, da bi izpostavili teorijo, marveč da bi

³ Današnja oblika – redakcijo besedila je Ulrich Oevermann opravil septembra 2003 v Dubrovniku – je rahla, predvsem tehnična predelava verzije 2002, dostopne na spletnih straneh. Ker je besedilo objavljeno v nemščini, ga v povzetku deloma prevajam, deloma parafraziram.

pokazali avtentično resničnost” ... teorije so zgoščene rekonstrukcije primera. Zato sta namesto operacionalizacije teoretskih pojmov, preverjanja veljavnosti in zanesljivosti po Oevermannu pomembni avtentičnost rekonstrukcije primera in falsifikacija s sekvenčno analizo. Po vsem povedanem je mogoče sklepati, da je objektivni hermenevtiki lastna enotnost temeljnega in aplikativnega raziskovanja, saj se vedno ukvarja s konkretnimi primeri, v katerih išče latentne strukture smisla in objektivne pomenske strukture izraznih konfiguracij.

Axel Fehlhaver in **Detlef Garz** v prispevku “*And since it is the first time...*” *On the educational shaping of the body of the class* z metodo objektivne hermenevtike analizirata učno uro, pri čemer ugotavljata začetno negotovost učencev in učitelja in mehanizme, ki jih učitelj rabi, da bi vzpostavil red in svojo avtoriteto. Thus the pedagogic hour is an interplay of eroticity and manipulation, which the teacher utilizes to set up a hierarchic relationship with himself in the role of master.

Drugi raziskovalni nastavek je utemeljitvena teorija, ki jo predstavljajo Kaja Kazmierska, Blaž Mesec, Vanja Huzjan in Susanne Friese. Narativni intervju, eno najpomembnejših tehnik zajemanja resničnosti v okviru kvalitativnega raziskovanja je razvil Fritz Schütze (pozneje še z Gerhardom Riemannom). Za metodo, ki jo zelo podrobno predstavlja **Kaja Kazmierska** v *Narrative Interview as a Method of Biographical Analysis*, je značilno uvodno vprašanje, katerega namen je sprožiti tok govora, ki naj spraševanemu omogoči povedati, predstaviti svojo podobo sveta, posledično pa do neke mere rekonstruirati – oživiti najprej sam potek dogodkov, hkrati pa naj bi spreševani nakazal, katere spominske plasti, katere konstrukcije ozadja (*Hintergrundskonstruktionen*) so se usedle na ta spomin. Pri tem so npr. časovne praznine – zamiki ali miselni obrati največkrat tiste točke, ki povedo, da se je v informatorjevem življenju zgodilo nekaj bistvenega.

Blaž Mesec v razpravi *A Case of Grounded Theory Construction in Social Work* piše najprej o tem, kako je začelo kvalitativno raziskovanje prevladovati na področju socialnega dela. Kratki pregled poglobljanja posebej v utemeljitveno teorijo avtor temeljito ponazorja s primerom, v katerem so se službe za socialno delo ukvarjale z zanemarjenim in zlorabljenim otrokom in njegovo družino. Kvalitativna analiza obsega konstrukcijo kronologije dogodkov, analizo časovnih vzorcev, vzorcev aktivnosti in končno analizo implicitnih teorij. Analiza je sklenjena s sklepno teorijo, ki ugotavlja, da so socialne službe v tem primeru bolj uporabljale odzivne/reaktivne, administrativne in moralistične strategije kot proaktivne, pomagajoče, spodbujevalne, pragmatične in sistematične /sistemske.

V raziskavi *The Qualitative Analysis of an Interview* **Vanja Huzjan** s pomočjo utemeljitvene teorije analizira pogovor z organizatorjem vaškega plesa. Izbor glavnega informatorja in vaškega praznika, dogodka, kritičnega za samorazumevanje in samopodobo vasi pokaže na krizo, v kateri se je znašla vas, ko se je v njej dogajal tudi mednarodni raziskovalni tabor. Zelo podrobna analiza pogovora je pokazala razcepljenost mladih na in- in outsiderje, njihove težave pri sprejemanju vaše identitete, opozorila je na možne spremembe pri mladih v razmerju do vaških tradicij; navsezadnje je opozorila na težavnost komuniciranja med raziskovalcem in informatorjem in na vpliv tehnike na potek pogovora.

V poglavju o računalniško podprti kvalitativni analizi **Susanne Friese** - *Computer-aided qualitative data analysis: An overview* pregleduje in definira značilnosti da-

našnjih najpomembnejših programov: Hyperresearch, Ethnograph, WinMax, Nvivo, Nudist, posebej podrobno pa Atlas.ti. Pomemben je poudarek, da je program samo pripomoček, s katerim raziskovalec lažje in hitreje analizira gradivo. V obsežni in temeljiti preglednici nato primerja osnovne značilnosti obravnavanih programov, kodiranje, zajemanje podatkov.

Josef Zelger v *Qualitative Research by the "GABEK" Method* predstavlja svoj program Gabek Winrelan, ki omogoča tako podroben opis in razlago problema kot praktični razvoj ciljev in njihovo korelacijo. Gre za postopek, ki vključuje izkušnje ljudi in omogoča usmeritev ali olajša proces odločanja v kompleksnih problemskih situacijah; podpira razvoj strateških načel in predstavitev rezultatov na interaktiven način.

Delo sklene razprava o etičnih vprašanjih *Fostering Human Dignity: Some Inherent Ethical Dimensions of Qualitative Research* **Willa C. van den Hoonaarda**. Avtor načena vprašanja, ki se porajajo, ko raziskovalec, ki uporablja kvalitativno metodologijo, prosi etično komisijo za odobritev raziskave. Obstoječe komisije (avtor govori o kanadskih in ameriških primerih) so večinoma sestavljene iz raziskovalcev, ki se zavzemajo za kvantitativne, predvsem biomedicinske študije, in ki so praviloma tudi definirali merila etičnega raziskovanja; tako sami vprašalniki marsikdaj vsebujejo elemente, ki so za kvalitativno usmerjenega raziskovalca marsikdaj nesprejemljivi ali neobvladljivi. Van den Hoonaard polemizira z vnaprej definiranim seznamom vprašanj, obvezno pisno privolitvijo informatorja, vprašuje se o anonimnosti in odpira različne možnosti reševanja tega vprašanja.

Da smo organizirali prvo mednarodno konferenco, posvečeno kvalitativnemu raziskovanju, je zagotovo najbolj zaslužen Frane Adam, ki je sodeloval pri njeni organizaciji, pripravi in finančni konstrukciji. Natis sta z organizacijo sofinanciranja svojih ustanov omogočila tudi Detlef Garz in Blaž Mesec.

Ni znanstvene publikacije ali konference brez sponzorjev, brez ustanov, ki prijazno omogočijo srečanje za izmenjavo mnenj in stališč, za diskusijo in nova pota v odkrivanju podobe drugega. Pri organizaciji konference in natisu knjige so finančno ali organizacijsko sodelovale naslednje ustanove: Ministrstvo za šolstvo, znanost in šport RS, Znanstvenoraziskovalni center Slovenske akademije znanosti in umetnosti in njegov Raziskovalni sklad, Inštitut za družbene vede FDV, Pedagoški inštitut Gutenbergove univerze v Mainzu, Fakulteta za socialno delo univerze v Ljubljani in Politehnika Nova Gorica. Za organizacijo seminarja o računalniško podprtem kvalitativnem raziskovanju s programom Atlas.ti se zahvaljujemo Matjažu Žnidaršiču in podjetju Marand, d.o.o.

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THE RECEPTION OF QUALITATIVE RESEARCH IN SLOVENIA

FRANE ADAM AND DARKA PODMENIK

SOCIAL RESEARCH IN THE ERA OF POST-POSITIVIST (POST-NORMAL) SCIENCE

In the last decade it has become more and more apparent that a renaissance in qualitative research is on the horizon and that the increasing relevance and reputation of this approach is incontestable. The establishment of qualitative methods as an indispensable methodological tool should not only be understood as a shift in the epistemological approach of social sciences but also as a step toward interdisciplinary collaboration among different paradigms and fields. A quotation from the reader edited (recently) by Norman K. Denzin and Yvonna S. Lincoln illustrates this situation very well:

For more than two decades, a quiet methodological revolution has been taking place in the social sciences. The social sciences and humanities have drawn closer together in a mutual focus on an interpretative, qualitative approach to research and theory. Although these trends are not new, the extent to which the “qualitative revolution” has overtaken the social sciences and related professional fields has been nothing short of amazing. [Denzin and Lincoln 1998: VII]

In sociology and the social sciences these days, one can hardly find anybody who would openly oppose the scientific status of qualitative methods [see Corbetta 2003], but the whole picture is more multilayered and complicated than suggested by both authors.

How can we explain the increasing significance of qualitative methodology and the shift in professional habitus? In our opinion, these phenomena should be (re)considered in the context of broader epistemic and institutional changes in scientific activity in post-industrial, knowledge-based societies (which are at the same time globalized risk societies). It can refer here to the transition from a “positivist” paradigm based on methodological exclusivism and disciplinary monism to a more inclusive and eclectic post-positivist (or post-normal) state of scientific research and theorizing. This paradigmatic turn has refocused attention onto the “context of application,” on transdisciplinarity, on triangulation, and on soft variables such as cultural and social capital or civilizational competence [see Sztompka 1999].

There are different names for this paradigmatic turn besides the label post-positivism, which we prefer to use; some authors speak about post-normal [Funtovicz and Radetz 1993] or post-academic science, and about Mode 2 production of knowledge. The latter is referred to in a book written by Gibbons and others, who laid the groundwork for further discussions concerning new trends in scientific research and theory

building. The authors introduced the distinction between so-called Mode 1 and Mode 2 production of knowledge, which in some aspects overlaps with our understanding of the distinction between positivism and post-positivism.

Mode 1 is discipline based – the relation between basic and applied research is linear. By contrast, Mode 2 knowledge production is transdisciplinary and is characterized by circularity or “a constant flow back and forth between fundamental and applied, between the theoretical and practical” [Gibbons et al. 1994: 19]. In other words: the problem-solving context of research does not simply imply the “application” of an already codified corpus of data and knowledge but requires a new reconfiguration of knowledge; application is also a very important inspiration for theory-building. Here one has to take into account that “Working in an application context creates pressures to draw upon a diverse array of knowledge resources and to configure them according to the problem at hand” [Gibbons et al. 1994: 27]. The main conclusion we can make may be formulated as follows: What matters is the quality and relevance of data, regardless if they are gathered by quantitative or qualitative research instruments [see Flick 1998, on quality management]. Alternatively, expressed in an affirmative fashion: the best solution seems to be a strategic combination (triangulation) of various instruments, data sources and theoretical perspectives. This presupposes the increasing ability of self-reflexivity on the part of researchers, some kind of deliberative research design, where other interested actors (such as stakeholders, beneficiaries or clients) also co-operate in the process of “discursive reflexivity” [see also Alvesson and Skoeldberg 2000]. To put it differently: the conventional positivistic assumption that scientific method based on quantifiable data can alone guarantee access to objectivity and truth – at least in the context of application or in situations where the problem-solving capacity of science is required – is challenged by a much more complex research design which involves team work, the participation of “lay” actors (for instance, representatives of the local community) and practicing what Anthony Giddens calls double hermeneutics (the flow back and forth between theoretical and practical knowledge).

Now we have arrived at the heart of our discussion. Who is better equipped to meet the criteria of data quality and better experienced in reflexive methodology? Who has more experience in the participative and co-operative nature of the research process than qualitative (field) researchers? If this is true, they need not be worried about their future status, and the efforts in the past decades to develop a sophisticated repertoire of qualitative methods will be gratified. What is needed is an adaptation to new – roughly indicated – institutional and epistemic circumstances and the formation of a more appropriate professional habitus.

On the other hand, as Mode 2 knowledge production presupposes, qualitative research can take part in very important, large-scale, applied (problem-solving) research projects compared with those conducted by disciplines based on “hard data.” I would like to draw attention to the reader *Qualitative Sociology as Everyday Life*, wherein one of the authors reports that one of the biggest injections of private funding into British sociology at the present time is for ethnomethodological studies on the organizational use of new information technology [Dingwall 1999]. The ethnomethodologist’s eye for detail turned out to be very useful in identifying interactional and organizational blockades in the transfer of technology to high-tech industry, thus contributing at least indirectly to a reduction in production costs.

The social sciences are not able to generate solutions in the form of “pure” prescriptions and “categorical imperatives”; their role and expertise potential are more modest. They can produce hypothetical solutions in the form of scenarios, multiple options, and cost-benefit or SWOT analyzes. They can be very helpful by indicating and evaluating the side-effects and potential risks of certain decisions and policies (here one could refer to the growing use of qualitative methods in the framework of evaluation research). The so-called hard, (still) positivist oriented disciplines and schools are hardly better prepared for these tasks than soft, qualitative ones. Moreover, thanks to their capacity for self-reflection, the latter is in some regards more capable of dealing with societal problems and risks. On the other hand, post-positivism is not anti-positivism: it does not imply the abolition of the rules of scientific method. It does not mean the relativization of criteria in the sense of the post-modernist principle “anything goes.” On the contrary, it is a more demanding methodological approach since it requires a higher degree of reflexivity and a capacity for complex thinking. If we add to our debate about post-positivism some other dimensions characteristic of sociology and other related disciplines (to some extent also of economics) – such as “cultural turn,” the discovery of the significance that “soft” variables in the form of cultural and social capital, trust, value orientation, etc. can have for explaining the developmental or institutional performance of certain societies or regions [Putnam 1993; Inglehart 1997], the reconsideration of the micro-macro link, and the already mentioned emerging consensus about the primacy and advantages of triangulation – we obtain a more pronounced picture of this new trend.

In such a changing frame of reference, the qualitative research orientation is entering a new phase. The point is no longer a competition in terms of a zero-sum game between qualitative and quantitative, positivist and interpretative approaches or, on the other hand, “splendid isolation.” Instead the emphasis is on “reconciliation” and cooperation in the form of hybrid research teams and on establishing communication between different types of “epistemic cultures” [Knorr-Cetina 1999] within the social sciences.

There is no doubt that this renaissance in qualitative research has been put forward at first in some social science communities in Western Europe and North America and that the wave of revitalization of qualitative approaches is only just getting underway in Eastern (and Central) European research centers. But this doesn’t mean starting from scratch, in this region one can also find a rather interesting tradition of utilization and the recent development of these approaches and methods [see Kaase, Sparschuh, and Weiniger 2002].

In spite of the fact that in the last thirty years a number of successful research studies based on qualitative methods in Slovenian social sciences have been published, these methods still have marginal status. This marginality is above all caused by weak institutionalization and the inclusion of qualitative approaches in (post)graduate curricula. On the other hand, the interest and preference for this type of research is much higher than in past decades. This means that also in the Slovenian case we can speak of a renaissance in qualitative research and of the “post-positivist” turn. However, many researchers who are inclined to utilize this approach in their basic or applied projects are rather “self-taught” and it takes some time for them to reach a certain level of experience and competence.

THE HISTORY OF QUALITATIVE RESEARCH IN SLOVENIA

The very beginnings of qualitative approaches were located on the margins of the Slovenian sociological field, not only with regard to the research subject but also regarding the institutions – research institutes – where this kind of research was launched. In the 1970s and even earlier the social sciences in socialist Slovenia were under more or less direct political control, and in such an atmosphere researching “in vivo” with techniques such as field research, case studies and open interviews were a matter of personal preference and responsibility. In spite of many obstacles in the early 1970s, researchers at the Institute of Sociology and Philosophy at the University of Ljubljana¹ were the first to use qualitative methods in sociological research. Matija Golob was one of the first sociologists who included the method of participant observation in investigations of social stratification [Golob 1972], for which quantitative methods were predominantly employed at that time.

The first large, exclusively qualitative method research project *Preventive Volunteer Social Therapy Work with Children* was carried out by an interdisciplinary research group of sociologists, social psychologists, psychiatrists, and social workers in conjunction with a group of student volunteers [Stritih et al. 1977]. This was the first, relatively comprehensive, twice repeated (summers of 1975 and 1976) action research which dealt with the introduction and evaluation of alternative therapies in work with behaviorally disturbed children and adolescents. In addition to already recognized qualitative methods such as participant observation and personal diaries, the researchers also employed methods and techniques which were utilised for research purposes as well as for therapeutic ones: unstructured interviews, simulation games, psychodramas and role-playing. The sociological portion of the project also included institutional analyses, particularly in relation to the problem of resistance to institutional innovation.

In the 1980s the employment of qualitative methods had expanded to other Slovenian institutes. At the Institute of Criminology a number of projects with qualitative methodology were conducted, at the Slovenian Research Institute in Trieste/Trst in-depth interviews regarding the assimilation of the Slovenian minority in Italy had been employed in the fields of sociology and psychology [Susič and Sedmak 1983]. A number of projects using instruments of qualitative methodology were launched at the Higher School for Social Work (now the Faculty for Social Work).

At the end of the 1980s the use of qualitative methods was also extended to those areas which foreign sociologists had been studying by using qualitative methods for a good twenty years: marginal groups and youth subcultures. Researchers in interdisciplinary projects employed a combination of qualitative techniques in studies of adolescents and so-called new civil movements – feminist, peace, ecological – including case studies and semi-structured interviews [Ule 1989].

In the 1990s the use of unstructured and semi-structured interviews in differ-

¹ This institute was partly an asylum for academics and researchers who had lost their positions because of their politically or/and ideologically unsuitable convictions. Later the Institute for Sociology and Philosophy was transformed into the Institute for Sociology and in 1990 it was joined to the Faculty of Social Sciences under the name the Institute for Social Sciences.

ent disciplines and at different institutes expanded considerably. Frequently these techniques were combined with structured interviews or surveys. At that time other qualitative methods, known and studied from foreign research studies, were introduced. The subject matter and sociological sub-fields in which qualitative methods were implemented also remained varied throughout the 1990s.

Along with an ever more systematic inclusion of semi-structured interviews among the methodological research techniques which were generally accepted, researchers began to focus more on qualitative modes of text analysis, partly as a way to complement interviews and partly as an independent research method employed primarily in the context of an interpretative sociological approach.

The comprehensiveness of qualitative research techniques dictated the collaboration of different research institutions. One example of such was the research team composed of researchers and collaborators from the Center for Theoretical Sociology at the Faculty of Social Sciences and researchers from the Institute for Ethnic Studies, which carried out a project on Slovenian micro and macro social reality as well as on Slovenia's position in an international framework, with a particular focus on the formation of multiple identities [Adam et al. 1996].

Since the late 1990s the use of qualitative research techniques in combination with quantitative methods has been increasing not only in number and with regard to the different institutions, but also in that qualitative research has left the margins of the social science field. The results of the small investigation which we carried out among Slovenian researchers² show, besides the expansion of qualitative research, that mixed (qualitative and quantitative) research has been increasing, that research content is broadening and that the circle of researchers who regularly use these methods is becoming wider, especially regarding younger colleagues. The most frequently used qualitative technique remains the semi-structured interview. Regarding such, the life history or life course interview are gaining in importance. It is worth mentioning that the focus group technique is becoming more important as well.

A typical Mode 2 project was launched in 1998 by the Center for Theoretical Sociology at the Faculty of Social Sciences on behalf of the Municipality of Koper. It was a field research project with policy-relevant goals. Besides the basic task – to collect data and the opinions of different target groups, the research results should contribute to the revitalization of villages in that part of Slovenian Istria. The intentions of the project were to empower and encourage the participation of the local population in the context of an emerging natural (regional) park, the formation of a so-called study circle, and the expression and implementation of local actors' interests, ideas and options concerning new economic and agricultural activities (such as ecological farming or "soft" tourism) in the framework of sustainable development in this region [Adam et al. 2001].

In another comprehensive investigation relating to urban phenomena [Stanković, Tomc, and Velikonja 1999] researchers from the Department for Cultural Studies employed the methods of participant-observation, semi-structured interviews and the text

² In order to gain an overview of the recently conveyed qualitative researches, we sent a short questionnaire to the researchers who had been doing this kind of research for a longer period of time and through the "snow ball" sample gathered data among younger researchers. The questionnaire reached approx. 25 Slovenian researchers by e-mail.

analysis of primary and secondary sources to study youth subcultures (punks, bikers, gays and lesbians, metal-heads, rockabillys, ravers, skaters and skinheads).

The shift of subjects in Slovenian qualitative research to more complex and interdisciplinary ones has been pushed forward by the inclusion of Slovenian research teams in international, especially EU, projects. One such case is the project entitled *Education and Training for Governance and Active Citizenship* [ACS 2003] in the 5th EU Program. Six research teams from EU countries and Slovenia worked together for over three years. As stated in the final report of the project, one of the reasons for using qualitative methods was to capture the different socio-cultural contexts across a range of European countries. For that reason the project was designed with a participatory orientation and grounded in a participatory method. The links between research team(s), “policy makers,” professionals, active citizens from chosen samples and other participants were established by means of advisory panels, workshops, international team meetings and focus groups. Semi-structured life history interviews were also carried out [ETGACE 2003].

At the Faculty for Social Sciences the combination of qualitative and quantitative data analysis was utilized in comparative research on industrial restructuring in CEE countries [Rojec and Stanojević 2001].³ Nevenka Sadar coordinates the Slovenian research team in the international project initiated in the beginning of 2003 as part of the 5th EU program. The project, entitled *Gender, parenthood and the changing European workplace: Young adults negotiating the work-family boundary*, is based on a complex combination of qualitative and quantitative research techniques: documents and demographic data analysis, survey research, interviews with managers and focus groups with young parents. One of the project goals is to define national contexts for understanding young partnerships and parenthood in eight countries and the influence of national politics on family-friendly programs organized in eight EU countries.

The participation of Slovenian researchers in international projects is of the great professional value for them; not only do they gain insight into the most up-to-date methodological trends, but they learn communication, co-ordination and monitoring skills, as well. With regard to the traditional institutional framework which prevails in Slovenian academic and research organizations, the international projects are a great opportunity to gain more experience in team work and up to date organizational modes, such as “project” or “ad-hoc” ways of conducting and governing research.

A DISCUSSION ON EPISTEMIC AND PRACTICAL PERSPECTIVES OF THE QUALITATIVE APPROACH

There are very few theoretical or epistemological works published in Slovenia concerning qualitative methods or the qualitative paradigm. Frane Adam [1982] and Blaž Mesec [1998] have written the most on these topics. Adam's main ideas are gathered in his doctoral dissertation, which he defended in 1981 at Zagreb University and published in a revised version in 1982. In *A Critical Guide to Sociological Research* he dis-

³ In 1999 Stanojević [1999] edited a book based on research on five enterprises by means of semi-structured interviews.

cusses epistemological and general methodological issues of the qualitative approach as an alternative option to quantitative sociological research. In his opinion this relation is not exclusive and he is clearly in favor of triangulation. Special attention is paid to the concept of action research [Adam 1982: 195–239]. While he focuses on research method issues (for example, the role of the researcher, methods of collecting empirical data, etc.) and analyzes the organization of the research process in detail (especially from the point of view of the sequential approach), he dedicates less attention to issues concerning the processing and analysis of qualitative data [Mesec 1998: 6]. We should also note that he deals with these problems and provides useful solutions in a book written with Veljko Rus, an expert in industrial relations and organizational sociology [Rus and Adam 1986].

Blaž Mesec [1998] deals with the practical aspects of data collection and analysis in his textbook, which is the only work dedicated entirely to the issue of qualitative methodology. In this work he discusses all three aspects of social science research: epistemology, the organization of the research process and concrete models for gathering and processing data. Mesec is the Slovenian author who has written the most on qualitative methodology and action research in relation to social work [1988; 1993].

Tanja Rener [1993; 1995; 1996] has written a series of articles concerning the use of the biographical method in women's studies. The biographies of women as well as women's life histories represent a "subjective" reconstruction of a historical and social period as well as of the position of women in society. For this reason the use of biography as method has a completely different role in women's studies than in social science research [Rener 1996: 759–763].

One must also mention here the significance of Tina Kogovšek's master's thesis (conducted in the field of statistics and computer science) which also focuses exclusively on the issue of qualitative methodology. Her thesis deals with questions of measurement, validity and reliability while at the same time discussing the scientific status of the qualitative approach in the context of postmodernity and post-positivism [Kogovšek 1998].

QUALITATIVE METHODS IN THE CURRICULUM

The methodology courses for students in the social sciences at the three Slovenian universities – the ones in Ljubljana, Maribor and Koper – are still to a great extent "positivistically" focused: students primarily learn to use quantitative methods and statistical analyses in the research process.⁴ They are acquainted with qualitative methodology only in a superficial manner. The partial exception is a methodology class *Research methods in sociology and cultural studies* offered at the Faculty of Arts at the University in Ljubljana; in this class students are to some extent acquainted with both quantitative

⁴ The third university centre in Koper has been emerging together with the new Faculty of Arts where the students of cultural studies and social anthropology already have (or will have in the next few years) the opportunity to be educated in the use of qualitative methods. Also the Faculty of Management, whose publishing house decided to translate an English methodological textbook focused on qualitative approach, has shown interest in teaching their students the new methodological orientation.

and qualitative research methods. The same is true also for the methodology taught at the Faculty for Social Work. Yet the only subject that focuses exclusively on qualitative research methods is taught at the Faculty of Social Sciences at the University in Ljubljana, but only in the Cultural Studies Department. In the class *Methods of Qualitative Analysis*, third-year cultural studies students learn about the qualitative paradigm from the epistemological aspect and about concrete modes of implementing qualitative methods for the collection and processing of data. Great emphasis is placed upon including students in the research process, as well as in seminars and interactive-group work. Students are required to write a *seminar report* based on fieldwork. The literature used in teaching the above-mentioned course is mostly international (this is true also for the course taught at the Faculty of Arts); however, at the same time students should also become acquainted with qualitative research conducted by local researchers. The above-mentioned course also discusses action and evaluation research as well as quasi-experiments.

Slovenian textbooks and teaching material for methodology courses in the social sciences primarily discuss issues concerning quantitative empirical research. An example of this is Niko Toš's work *Methods of Social Science Research* [1975; 1988]. While the author also deals with themes which are relevant for qualitative research approaches (for example: participant observation, in-depth interviews, group discussions, etc.) it is clear that the quantitative paradigm predominates in the text as a whole. The fact that the author dedicated one chapter of the book to qualitative research [1975: 199–202] for the first time in 1988 – thirteen years after the book's first edition – is evidence of this (The above-cited chapter was also published in later editions of the book in 1997 and 1998). There he equates qualitative research with action research and in an elemental fashion presents it as an alternative to the quantitative approach.

Like the textbook mentioned above, the *Methodology of Sociology* [Flere 2000] is also predominantly focused on quantitative methods and on general, epistemological aspects of empirical research in sociology. One chapter is dedicated to the explanation of the difference between quantitative and qualitative methods. In the chapter that deals with concrete research methods the characteristics of the semi-structured interview, case study, qualitative content analysis and participative observation, are outlined.

There exists the only one work in book form, which may be used as a qualitative methodology textbook. This would be the previously mentioned work by Blaž Mesec *Introduction to Qualitative Research in Social Work* [1998]. In this work students may find strategies and phases of qualitative research as well as concrete guidelines on how to carry out a research project, and how to prepare and process data. Yet Mesec's book does not succeed in illuminating all issues pertaining to qualitative methodology (for example: epistemological issues, the history and typology of qualitative methods, etc.). The problem also lies in the sorts of research cases cited by the author, for they deal with subjects and issues particular to the field of social work. Cultural studies and sociology students would also need more examples from organizational and institutional life as well as a greater emphasis on linking micro and macro social realities.

Thus a new and up-to-date methodological text is needed which would provide students of the social sciences and the Slovenian public intellectual sphere a complete introduction to the issues of qualitative methodology which is – thanks to a recent – post-positivist turn, but also thanks to new computer-based programs for processing

data resulting from qualitative research – becoming more and more influential in the international social science context. Previously published texts, such as Mesec's work, will be a sound basis for advanced studies later on.

RECAPITULATION: THE “SLOW MOTION” MOVEMENT TOWARD POST-POSITIVISM

With regard to the first chapter, the question could be raised if the trends described above are indicative enough so that we could speak of the movement from Mode 1 (“positivist”) to Mode 2 (“post-positivist”) in Slovenian social science research.

In the 1990s, after the introduction of democracy, when it could be expected that the thematic range of research would expand and that researchers would focus on phenomena such as: (local) new elites, decision-making processes, political parties, non-governmental organizations, the research topics were enlarged to a very narrow extent.

As far as applied methods are concerned, we can observe a predominance of unstructured and semi-structured interviews, while the use of biographical methods has only just begun. The application of long-term fieldwork and participant observation is still a rarity. The use of computer-based programs to analyze qualitative data is also only in the early stages. Sociologists also make little use of more ambitious case studies, to say nothing of more in-depth phenomenological studies.

In the last three decades some of the qualitative methods recognized abroad have been used in research conducted in the field of sociology as well as in related fields; at the same time sociological qualitative research has remained unsystematic and with a lack of continuity. One can explain this in large part in light of the instability of financial resources for this line of research and the related problem of not being able to form stable research teams to implement qualitative research.

The Slovenian Universities' curriculum has become more susceptible with regard to qualitative research, however this approach is actually kept to its margins. In spite of this fact, the young researchers have shown much interest in the implementation of qualitative research methods.

One of the major steps towards the Mode 2 or post-positivist state has been taken by multidisciplinary research projects; some in the framework of Slovenian social sciences and especially those which have been conveyed in the context of European research program.

PERSPECTIVES AND TRENDS

Qualitative methods are gaining more and more recognition in social science research, and are being used also in the context of evaluative research and in combination with computer programs. They are important not only as research instruments in purely research frameworks but also for encouraging action and participatory research in fields such as: informal and social teaching (“a learning society”), civil initiatives, the development of a democratic political culture, the revitalization of local communities, solv-

ing unemployment, non-governmental organizations The qualitative researcher – who must go through training similar to that of a psychoanalyst – who has an understanding of group dynamics, negotiation skills, and a trained attention to detail while being capable of complex thought, is becoming an important actor in inter-cultural communication, solving conflicts, moderating workshops, and leading project groups and research teams. Even Slovenia, which has neither a great nor systematic tradition nor consistency in this regard, boasts some very successful individual models. We will have to act in a more co-operative and synergetic manner. Only in this fashion – as well as with international co-operation – will we be able to compensate for a small critical mass and limited resources.

On the other hand, the reform of study programs, especially the methodological curriculum, is urgently needed. The theory and practice of qualitative research (and triangulation with quantitative approach) should become an indispensable part of teaching on undergraduate as well as on post-graduate levels.

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THE EPISTEMOLOGICAL FRAMEWORK OF QUALITATIVE RESEARCH: IS IT STILL A CINDERELLA TO QUANTITATIVE RESEARCH?

WILL C. VAN DEN HOONAARD

In fairy tales, Cinderella and her ilk are a sorry lot. More importantly, though, the Cinderellas affirm the primacy of the natural family. This paper presents qualitative research as a Cinderella to quantitative approaches. Unlike the fairy-tale stepsister, the qualitative researcher feels less sorry for him- or herself, despite the primacy of quantitative research. This paper outlines the widely accepted attitudes towards the qualitative research of philosophers of science and of social scientists in particular. In doing so, it explores the epistemological framework of qualitative research. I hope to show that inductive research or exploratory research – the more general terms for qualitative research – was originally formulated from the perspective of confirmatory, deductive research and that there is no unanimity about the nature of inductive research in the social sciences. The first part of the paper invokes the approach taken by social-science textbooks, especially in their use of the “Wheel of Science.” The second portion highlights some of the distinguishing features of qualitative epistemology. Emerging from 2,000 years of somnolence, it was left to Francis Bacon, John Locke, and John Stuart Mill to reinvigorate the concepts of inductive reasoning originally outlined by Aristotle. As John Madge so eloquently puts it, with the Renaissance “there came a re-awakened delight in purposeful observation ...” [1965: 51]. Its resurrection in modern times has produced camps of antagonists and protagonists, whose struggles have not always clarified the epistemological framework of inductive, or qualitative research. Both the definition and purpose of inductive research are left wanting. Philosophers of science who have given serious thought to induction or inductive arguments, often define induction as the absence of deduction. For example, Anthony M. Coyne in *Introduction to Inductive Reasoning* defines induction as “arguments that are not deductive” [1984: 5]. Any attempt to define inductive arguments, others aver, would of necessity involve deductive reasoning [e.g. Barker 1974: 57] and that attempt, according to Wesley Salmon [1974: 52] would show “unseemly prejudice against one kind of inference.” Yet, as I intend to show, it is precisely the ever-present attempt to define inductive reasoning in light of deductive arguments that has “addled our brains” [Salmon 1974: 55]. Richard Swinburne in *The Justification of Induction* [1974: 2] offers the frank view that, in fact, “there is no precise agreed use for the expression ‘inductive argument.’” Pointing to the difficulty of making inductive arguments relevant to generalizations (i.e. can inductive knowledge predict the future?), philosophers have addressed the issue as “*doubts* about induction” [Edwards 1974], the “*tragic problem* of induction” by Donald Williams [cited by Edwards 1974: 29], and the “*problem* of induction” [Barker 1974]. Salmon declares induction as “*unsafe*” [1974]. And while many philosophers of science oppose induction as a scientific method (such as David Hume, Bertrand Russell, Karl Popper), the

relevance of induction remains as an “*interesting* doubt” [Russell 1974 [1912]], as a “*deep and important* confusion” [Barker 1974: 61], and as “the *celebrated* problem of induction” [Urmson 1974: 77]. They acknowledge the stepsister’s presence, but are unsure about her importance. As Coyne points out, the literature in philosophy on the justification of the stepsister, or induction, is “*extensive*” [1984: 263]. Yet proponents amongst philosophers of science have rooted inductive reasoning firmly in the deductive tradition. Causal analysis, probability theory, samples, sample distribution, probabilistic reasoning, hypothesis testing, and the hypothetico-deductive method are linked to inductive reasoning [e.g. Coyne 1984: 185]. Coyne points to valuable leads that one can find in mathematics books on probability and statistics [1984: 268] – a far cry from the nature of induction as currently practised by the qualitative researcher.

THE WHEEL OF RESEARCH

Given the troubled state of induction as articulated by philosophers of science (and by extension natural scientists), it does not come as a surprise that induction as the basis of qualitative research also carries the aura of a stepsister in the social sciences, reinforced by the idea that the dominant scientific enterprise is deductive. As I intend to show later, there have been some slight shifts in the valuation of induction, but the shifts have been slight and, on the whole, rather deficient. In earlier generations of research-methods textbooks (authored chiefly during the 1950s and 1960s), *qualitative* or *inductive research* is either absent or its appearance is minuscule as compared to such concepts as *deduction* and *hypothesis testing*. In eleven textbooks of this earlier generation, a mere 18.3% of 82 indexed items are devoted to qualitative research (as will be seen in a later section of this paper, some 35% of relevant indexed items in the current generation of textbooks make reference to induction and the like). One work stands out, however. John Madge provides remarkable foresight regarding the uses of inductive research, also offering a welcome historical overview of inductive thought [1965: 49-60] and a wholesome critique of statistical methods. At the height of this particular earlier generation, social scientists were supremely confident about the future of confirmatory research. Hans Zetterberg, for example, opines that “(s)ome fortunate members of the generation now being trained in sociology will be the first ever to orient themselves from the very start of their careers toward actual theory construction” [1965: 21], involving “lawlike propositions about society that can be supported by evidence” [1965: 22]. For him, “(t)heories summarize and inspire, not descriptive studies, but *verificational studies* – studies construed to test specific hypotheses” [1965: 28-29]. It was also more common at that time to refer to qualitative research as *idiographic* or *nomothetic* studies [Blalock and Blalock 1971; Naroll 1971; Madge 1965; Pyke and Agnew 1991] – terms that have entirely disappeared from methodology texts today. For our purposes, I explored the description and presentation of induction in fourteen of the most common contemporary methodology textbooks in North America. I shall focus on the *Wheel of Science* and use my critique of the Wheel as a stepping-stone to highlight how the inductive process differs from deductive work. An analysis of these texts displays a tendency, as a rule, also to see induction as a formal part of the deductive process of science. Social scientists of the dominant, deductive framework discount the value of induction, either by

exclaiming, “Where’s the theory?,” or by claiming that “all research is inductive” or that “the truth of a generalization ... ultimately rests upon induction” [Singleton and Straits 1999: 50-53], obscuring the unique features of induction as a scientific enterprise. The indices in eleven of these works reveal that, on average, induction receives 35% of all entries devoted to deduction and induction. Only two texts [Palys 1997; Neuman 1994] had a proportion of entries devoted to induction that were roughly similar to those devoted to deduction. On the surface, a student in methodology would be left with the impression that, for the most part, inductive research occupies a less significant, less relevant status than the elaborate entries on deduction. However, the distinctions run deeper than any index could indicate. Eight of the textbooks rely on *Wallace’s Wheel of Science*, a figure that shows the roundness of research, as it moves back and forth between theory and observation (see figure 1).

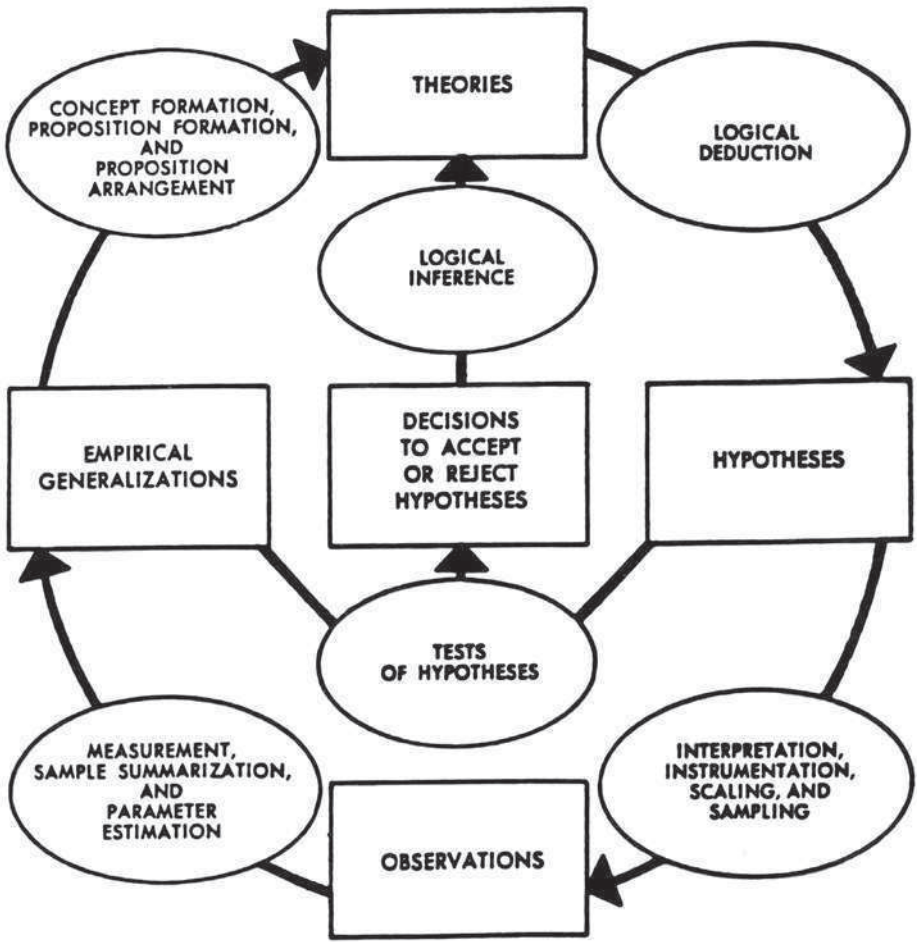


Fig. 1: Wallace’s Wheel of Science: the Logic of Science in Sociology. [Wallace 1971]

It is an appealing figure. It offers an even-handed treatment of both deductive and inductive approaches. It is a tolerant view: there is place for both approaches in social science as they lend support to one another. The idea of a “wheel,” moreover, is a “wholesome” depiction, in the sense that it attempts to unify and integrate diverse elements of the research process. It “makes sense.” Above all, it seems neutral and even handed in its presentation of inductive and deductive methods. The source of the “Wheel of Science” is Walter Wallace’s *The Logic of Science in Sociology* [1971], a 30-year-old work, published at the height of the use of the quantitative and deductive paradigm. The inclusion of such terms as *observation* (and by implication, induction) at that time must have appeared to be a significant step forward in sociology. The popularity of the “Wheel” continues. Coincidentally, Wallace himself never coined his figure as a “wheel;” that designation came later. Some of the later representations of the “Wheel” are quite elaborate (as shown in figure 2).

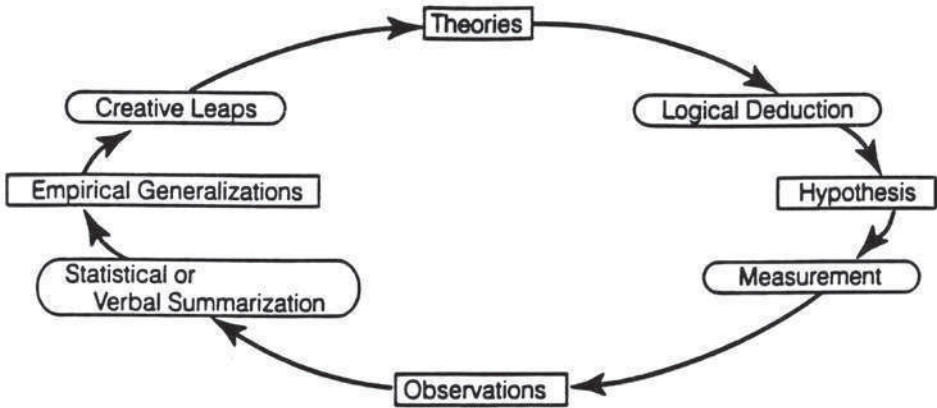


Fig. 2: Another representation of the “Wheel of Science.” [Stier and Clark 1999]

Others are quite simple, but no less effective (see figure 3).

There are, nevertheless, a few anomalies that stand out for those who practise qualitative research. First, the term *Theories* usually appears at the top of the “Wheel,” while *Observations* is more commonly found at the bottom. It is a performative figure, whereby the location of the terms on the Wheel is as important as the Wheel itself. I use the term “performative figure” in the sense that W. P. Robinson [1972] employs the term “performative language” to denote the fact that speaking or writing is sometimes as meaningful as the actions themselves. In our case, the location of research elements on the figure is as important a communicative strategy as the figure itself. Obviously, theories have a primary station in our research. The “Wheel of Science,” ironically, seems to preclude a rolling of the wheel, which would occasionally result in *Observations* at the top. Qualitative researchers share, I trust, a cynicism about theory, “at best a necessary evil,” that “is likely to get out of hand,” and that will “largely divorce data from day-to-

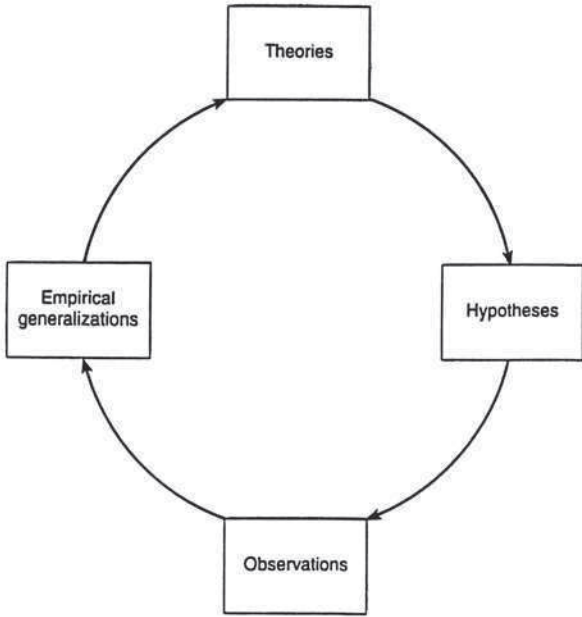
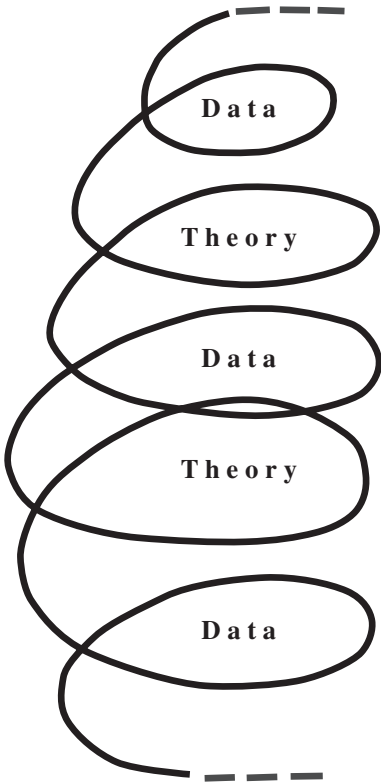


Fig. 3: A simpler representation of the “Wheel of Science.” [Baker 1999]

day digging into social life” [Becker 1998: 4]. Second, the later Wallace figure deviates from its original representation. In the original figure 1, the word *induction* is entirely absent, although *deduction* is indicated; in fact, most of the boxes in the figure are more closely aligned with terms that a deductive researcher is familiar with: *proposition formation, proposition arrangement, logical deduction, decisions to accept or reject hypotheses, hypotheses, test of hypothesis, measurement, sample summarization, parameter estimation, instrumentation, scaling, sampling*, etc. Only the following words, however, are identified with current qualitative research: *concept formation, empirical generalizations, observations, and interpretation*, although Wallace uses this last term in the sense of “specifying observable indicators of abstract concepts” [1971: 73]. Wallace distinguishes empirical research at the level of “exploratory” studies from hypothesis-testing studies. In his schema, exploratory studies rest on “still *unformalized and unintegrated* theoretical, hypothetical, and methodological arguments (emphasis mine)” [1971: 22] – a perspective that differs widely from the way a qualitative researcher sees his or her work. Third, the deductive character of Wallace’s scheme comes through very clearly by its use and place of *observation*. The referent *observation* in a deductive sense normally refers to the practice of observing experimental and natural data as in, for example, the interaction of molecules. In later textbooks, *observation* has become a shorthand for all manner of qualitative research. But there is a wide variety of qualitative research strategies which fall outside the *observation* label, such as interviewing, life-history analysis, or textual analysis. The term *observation*, therefore, dulls the full range of qualitative research tools that are ordinarily used. Even fieldnotes could be generated without observation, as was the case for one of my blind graduate students, who used the power of hearing to “observe” the goings-on in a local convenience store. Wallace’s schema



displays the full panoply of tools linked with deductive research, but barely does justice to inductive, qualitative approaches, even those not associated with *observation*, such as inductive typologies that organize empirical findings into broader descriptive concepts. Fourth, the schema rarely does justice to how research proceeds. As figure 4 shows, we might as well draw the research process as a spiral. When we have the interplay between data and theory, or between evidence and ideas which Charles Ragin [1994: 47] calls retroduction, the research process uncoils.

Fig. 4: *The Research Spiral*. Source: Author's own design.

To demonstrate that the research process between evidence and ideas can be illustrated in a variety of ways, I offer an illustration of a snake ingesting data. As the snake takes in data, (fig. 5) its body grows and strengthens and will, in due course, need to shed its skin, making way for new growth. It is too tempting to leave out this metaphor: following the ingestion, a snake takes time to digest the data, offering a unique time for reflection. Theories, or ideas, that may have initially served their purpose well, will have to be shed, allowing for the interpretation of new evidence. Professors of methodology annually present students with a highly idealized version of the research process, much like Wallace's Wheel. What we need to do, according to Howard S. Becker [1996] is to give up "preaching about how things should be done and settl[e]ing for seeing how they are in fact done." This point drives hard at the epistemological stance of a qualitative researcher: he or she wants to see how social actors construct their social worlds and although ideology is an intrinsic part of that process, they do not mistake ideology as being solely responsible for the way the world is constructed socially. Fifth, deductive and "inductive" approaches are portrayed as a mirror of each other. Some are inclined to see induction as the "reverse" process of deduction [e.g. Smith 1991: 8]. What flows "down" are hypotheses and what flows "up" are empirical generalizations. An initial look at the scheme seems to allot the bottom part to inductive approaches and the top

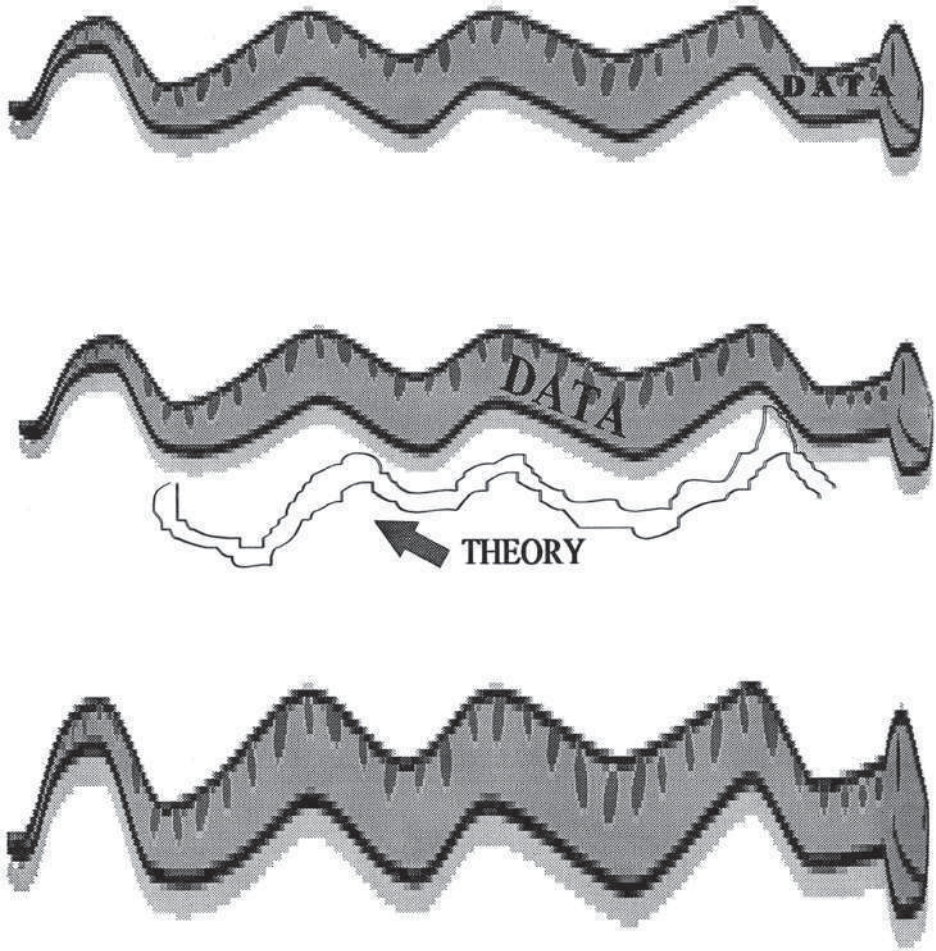


Fig. 5: *The Research Process as Snake*. Source: Author's own design.

part to deductive ones. Anyone familiar with the skyscape of a smooth river is well acquainted with this symmetry: the river reflects as a near-perfect mirror the reality of the skyscape. Yet, the processes by which a qualitative researcher arrives at theories are quite different from those inherent in the quantitative model. Moving “upward,” the qualitative researcher is careful to fit theory around data, mindful that his or her goal is to have data shape the theory, rather than have theory shape the data. We are indebted to Earl Babbie [1998: 60] who offers us this insightful exchange between Dr. Watson and Sherlock Holmes in “A Scandal in Bohemia” in order to illustrate the role of evidence (data) and ideas (theory):

Dr. Watson: What do you imagine that it means? Sherlock Holmes: I have no data yet. It is a capital mistake to theorise before one has data. Insensibly one begins to twist facts to suit theories, instead of theories to suit facts. [Doyle 1994 (1892): 7]

Such an approach gives primacy to the empirical world, avoids the use of variables, is guided by “how” questions (rather than “why”) [cf. Becker 1998: 58], and is devoted to building concepts that arise out of people’s experiences that might be applicable to other social settings. All the hard work is concentrated in the analysis of data, not in the preformulation of theory. This approach is hardly a mirror image of the deductive technique. In short, these convenient schemata of research are not only grounded in deductive, quantitative approaches, inadvertently downplaying or misrepresenting even the essentials of qualitative research, but are also idealized versions of how research should proceed.

DEFINING INDUCTIVE RESEARCH

As many textbooks continue to rely on the “Wheel of Science,” it stands to reason that defining inductive, qualitative research can be problematic. To underscore the problem, one notes that very often the examples of induction are derived from observations of a natural event, rather than social occurrences. For example, Emanuel J. Mason and William J. Bramble [1997: 6] connect cloudy weather and the decision to wear a raincoat as an example of induction. Nevertheless, we can broadly classify three definitions of induction from these methodology textbooks, although the definitions do spill into each other. First, for some, induction implies moving “from the specific to the general” [Mason and Bramble 1997: 6]. For Russell K. Schutt [1999: 42; cf. Adler and Clark 1999: 26], induction “begins with specific data, which are then used to develop (induce) a general explanation (a theory) to account for the data.” Babbie [1998: 4] offers a succinct, but helpful definition: induction “begins with concrete, specific observations and aims at identifying general principles governing what is observed,” or moving “from the particular to the general, from a set of specific observations to the discovery of a pattern that represents some degree of order among all the given events” [cf. Baker 1999: 41; Smith 1991: 8]. The best example of a second order of definitions comes from Charles Ragin [1994: 15] who avers that induction implies “the use of evidence to formulate or reformulate general ideas,” or a “process whereby the implications of evidence, especially new evidence, combined with existing evidence, for general ideas are assessed.” His definition makes a more explicit connection between evidence and ideas, as does W. Lawrence Neuman [1994: 41]. A third definition comes from Ted Palys whose work, *Research Decisions: Quantitative and Qualitative Perspectives* [1997: 46] offers a definition, preferred initially by Becker and more congenial to qualitative research, namely that induction hopes to “engage a phenomenon of interest on *its own terms* and to let theory emerge from the data [emphasis mine].” This is the crucial definition of qualitative research that sets it apart from deductive research. It has deeper ramifications for our craft than the first two rather lifeless definitions.

INDUCTION AND QUALITATIVE RESEARCH

The problems that a qualitative researcher faces implicate the first two inert definitions of induction. Qualitative research is merely seen as moving from the particular to the

general, or using evidence to formulate ideas. In that context, the dominant paradigm of normal social science sees qualitative research as hovering around the bottom of the “Wheel of Science,” a bottom feeder, if you will, that can only rise to the top by the aid of the deductive paradigm which will render hypotheses ultimately to test theory. Quantitative researchers by virtue of their dominance have colonized qualitative research. Still, qualitative researchers must explain their epistemology in the language of quantitative researchers [Becker 1996]. The language of inclusion and exclusion exemplifies this process. Quantitative researchers speak of the importance of including qualitative researchers in order to give us a whole picture of a social phenomenon; one seldom hears a qualitative researcher speaking of the need to include quantitative research in their domain to round off their understanding [D. van den Hoonaard, p.c.]. This language echoes the position of other minority groups, whether Natives in North America, or cartographers vis-à-vis people trained in Geographic Information Systems (GIS). It is a language that appears to make quantitative researchers more generous in their appraisal of qualitative researchers, and qualitative researchers less inclusive, less generous. Colonization extends into such areas as the ethical review of research and grant applications where quantitative research is seen as the norm. No doubt, institutional shifts are underway in granting institutions, allowing more room for qualitative research, but in many other areas, including student supervision and general regard of qualitative work, much remains to be done. But there is more to the difference between inductive and deductive research; it centres around the nature of the data we collect and analyze. We find the key to this difference in Palys’ definition, namely that the nature of induction is to “engage a phenomenon of interest on its own terms and to let theory emerge from the data” [1997: 46]. Engagement with the data makes a radical difference in the manner by which our research comes to fruition. The following sections highlight several significant epistemological features of qualitative research, namely recognizing the complexity of social settings, not knowing the extent of data to explore, avoiding as much as possible advance knowledge in the literature, emphasizing accuracy, and seeking out deviant, or negative cases [Becker 1998: 208]. In the first instance, a qualitative researcher, by moving away from variable research, *recognizes the complexity of social settings*. Here is an excerpt from Becker [1996] that refers to the sort of questions that fieldworkers ask about delinquent *activity*:

Fieldworkers usually want something quite different: a description of the organization of delinquent activity, a description which makes sense of as much as possible of what they have seen as they observed delinquent youth. Who are the people involved in the act in question? What were their relations before, during, and after the event? What are their relations to the people they victimize? To the police? To the juvenile court? Fieldworkers are likewise interested in the histories of events: how did this start? Then what happened? And then? And how did all that eventually end up in a delinquent act or a delinquent career? And how did this sequence of events depend on the organization of all this other activity? [Becker 1996: 56]

In the second instance, a qualitative researcher *does not know in advance* the full range of data they will collect [Becker 1996]. A qualitative research project is never finished, especially where data gathering is expertly done. Take interview data. Some researchers opt for transcribing and analyzing each interview before moving on to

the next interview. If the interviews are done well, i.e. yield rich data, there is a real possibility of having to conduct more interviews than if the interviews were shallow, in which case each interview will not generate new insights or ideas to further the interview process. To underscore the analytical weight of transcribing an interview after each occurrence, one can simply imagine the difference between analyzing ten interviews only after all ten have been transcribed *after* they have been conducted, as opposed to a research effort in which each interview builds upon the previous one. Such an approach flies in the face of standard question-and-response questionnaires used by quantitative researchers who find it important to maintain the format from beginning to end, enhancing the reliability of the research. Moreover, a qualitative researcher, by habit, tends to conduct a review of the literature towards the end of the data-gathering stage; this treatment of the literature ensures a broader scope of discovery. This highly distinctive element of qualitative research poses the greatest challenge to those who use quantitative techniques. The practice of abandoning formulaic reviews of the literature before the research starts seems to fly in the face of common sense in research. Yet, we must acknowledge that all enter research settings with assumptions – and some of these are greatly strengthened by a too-early review of the literature. Such a review may close our eyes to what should be investigated. Even the practice of reviewing the literature to ascertain what specific substantive areas have not yet been explored carries the assumption that social institutions do not alter, they remain unchanged over the years. As a fourth point of epistemological difference, a qualitative researcher *strives for accuracy*. The genesis of this difference resides in the fact that a good many qualitative researchers endeavour to find out “from people what meanings they are actually giving to things,” [Becker 1996] either as things they say or do. The goal goes beyond giving merely an accurate rendering of a social setting. It is one of the means at the disposal of a qualitative researcher to take the time to understand a phenomenon that is beyond the researcher’s own realm of experience and circumstances. In Becker’s words, “we should not invent the viewpoint of the actor” [1996]. The goal is also a major epistemological counterpoint to much of current research funded by governmental, social, and corporate agencies that favour ready-made questions and answers, rather than accuracy, in their attempts to find known answers (a form of Wal-mart Sociology) to mounting social problems. With an urgency to solve these problems, “errors of attribution” [Becker 1996] increase significantly. A qualitative researcher deviates from a deductive researcher in another way: *he or she seeks deviations* from the social or group norm. A quantitative researcher is in the business of confirming (or rejecting) hypotheses; a qualitative researcher is engaged in the process of discovery and exploration [Stebbins 2000] and is keen to find deviations from the norm. Such deviations tell us as much, or maybe even more, about a given social setting. In many respects, the search for deviation also acknowledges that every social setting contains a hierarchy of credibility that a qualitative researcher is forced to take into account. Such a hierarchy suggests

that the superiors of any hierarchical setup will be conceived of by everybody who accepts that legitimacy of that setup as knowing more about it than anyone else. Their questions, their proposed answers, their notion of what constitutes the area being looked at, all of that will be taken as obviously correct and sensible. [Becker 1986: 39]

This particular hierarchy can dictate the kinds of questions we ask, circumscribe the social setting we study, and make our research political: we question institutional information (such as official statistics) and by taking interest in groups that are not likely represented in formal social accounting, we inadvertently poke some people in the eye. We are now coming full circle, back to Wallace's "Wheel of Science." Where would one locate the above elements in the wheel? Where do we fit recognition of the complexity of social settings, our inability to configure in advance the extent of the data to be gathered, the lack of review of the literature in advance of the research, and our desire to seek out deviant cases? These, and other, techniques simply illustrate some of the ways that a qualitative researcher finds him- or herself at odds with the dominant epistemology of the social sciences. Hopefully, the time will come for this stepsister. One of the finest descriptions of the qualitative method that might secure a place of honour for this stepsister comes from noted American author, William Least Heat-Moon, who offers these insights into the obstacles he faced even when describing the topography of a county in the Prairies:

I am standing on Roniger Hill to test the shape of what I'm going to write about this prairie place. For thirty months, maybe more, I've come and gone here and have found stories to tell, but, until last week, I had not discovered the way to tell them. My searches and researches, like my days, grew more randomly than otherwise, and every form I tried contorted them, and each time I began to press things into cohesion, I edged not so much toward fiction as toward distortion, when what I wanted was accuracy; even when I got a detail down accurately, I couldn't hook it to the next without concocting theories. It was connections that devilled me. I was hunting a fact or image and not a thesis to hold my details together, and so I arrived at this question: should I just gather up items like creek pebbles into a bag and then let them tumble into their own pattern? Did I really want the reality of randomness? Answer: only if it would yield a landscape with figures, one that would unroll like a Chinese scroll painting or a bison-skin drawing where both beginnings and ends of an event are at once present in the conflated time of the American Indian. The least I hoped for was a topographic map of words that would open inch by inch to show its long miles. [1991: 14-15]

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THE INDIVIDUAL AND THE SOCIETAL

REINHARD SIEDER

Since the mid-1980s new key-words such as everyday life, gender, culture, experience and others have acquired the status of conceptual terms within the humanities. Moreover, many of these terms have come to provide labels for new disciplines or “joint ventures” such as the history and sociology of everyday life, gender studies, or cultural sciences (*Kulturwissenschaften*). These labels have predominantly been employed by scholars who began their careers in the 1970s and 1980s. Previous generations, and particularly the exponents of the so-called *Historische Sozialwissenschaft* (historical social science) in Germany and Austria, by contrast, had still concentrated on categories such as class, society and economy, and above all, on structures and processes, categories which dominated and innovated research during the 1960s and 1970s [Sieder 1990; 1994; 1997]. The fundamental change between these generations of scholars has been in the perception of what is actually of concern to society. This is a point I would like to elaborate in some more detail by looking at the concept of “structure.”

It was the French historian Fernand Braudel who most profoundly influenced the concept of an “*histoire des structures*” from the end of the 1940s onwards [1949]. Braudel’s ideas became popular in the German-speaking academia in the 1960s. Otto Brunner, for example, opportunely seized on the concept of an *histoire des structures* as a replacement for the ideologically tainted ideas of an “internal population hierarchy” (*innere Volksordnung*) or “the inner construction of human bonds” (*innerer Bau menschlicher Verbände*), which had been the features of national-socialist *Volksgeschichte* [1942: 185]. The Heidelberg historian Werner Conze, likewise welcomed the opportunity with open arms. He too picked up the concept of *structure* as a replacement for the antiquated notions associated with the concept of a social, legal and economic “constitution” (*Verfassung*) [1957]. Conze even went so far as to think that the label “structural history” (*Strukturgeschichte*) should replace that of social history altogether. He received practically no support for that, but together with Brunner and others he was nevertheless able to pave the way for the triumphal progress of the concept of structure in the historical social sciences, at least within the German speaking world. From Conze’s and Brunner’s time right through to the publication of Hans-Ulrich Wehler’s *Deutsche Gesellschaftsgeschichte* [1989] a society’s structures were always described foremost as *economic and social structures*, including politics and demography and – reduced to systems and institutions of wisdom and faith – culture. The first and second generation of the historical social science (*Historische Sozialwissenschaft*) always viewed these sorts of *structures* as the basic stage upon which all kinds of events appeared [see Kocka 1986]. Within this structural (not structuralistic!) paradigm, “structures” were viewed as being completely *external* to individuals, who are therefore structurally (over)determined.

And from this point of view, the study of personal accounts or documents only made sense in so far as they were able to help reconstruct the objective structural reality of the society in question. Individuals only possessed a fairly sketchy view of the world, they thought in ideological terms and behaved in a structurally determined manner, all the time never realizing that history was being made behind their backs.

From this perspective, those younger historians, ethnologists, sociologists and literature specialists who are concerned with the study of everyday life, gender studies, microstoria, historical anthropology, new cultural history and cultural studies, who all try to decipher the meanings that individuals and groups associate with their actions, are seen as *revisionists*. They are accused of being non-theoretical, of lacking conceptual rigor and losing themselves in thick descriptions of colorful historical episodes, without adding anything significantly new to social scientific knowledge. In the final analysis, therefore, this debate is aimed at *theoretical* questions, which of course have methodological consequences: How should we think about societal structures, about societal processes, and what is the place of acting of individuals within those structures and processes? And if – as younger scholars argue, “experience,” “meaning” as well as the “imaginary” and the “symbolic” should receive far more attention, will this open the way towards an interdisciplinary project of “sciences of culture” (*Kulturwissenschaften*)?

THE NEW PARADIGM OF PRAXEOLOGY AND WHAT IT CHANGED

From the mid-1980s onwards, the view became increasingly popular that the idea of structure – that is to say in terms of economic, social or political structures – could be a mode of *describing* the distribution of phenomena, but in itself *explains* nothing. This was because (as the young Karl Marx had already noticed) social, economic or political conditions emerge from the activities of lively social subjects; social reality is produced through the active appropriation (*Aneignung*) of those conditions by individuals and groups. In the 1970s and 1980s this was pointed out in sociology and ethnology, where Pierre Bourdieu [1976], Anthony Giddens [1988], and others developed new key concepts such as *habitus* or *field* or *practice*, in order to comprehend the activities of individuals and groups within a structured space of relationships. Giddens provided the missing link between individuals and structures, which the German historical scientists had failed to find: *structuring* or *structurizing*, which means the ongoing (re)production of structures by the individual's acting.

The “revisionist” generation, to which I belong, took up these theoretical tools and started, somewhat belatedly, to read some of the major works of interpretative sociology (*verstehende Soziologie*) in the succession of Max Weber which had never been given much attention by the German founders of historical social science. Studies on the social world (*soziale Welt*) and the life-world (*Lebenswelt*) by Alfred Schütz [Schütz and Luckmann 1974; Schütz 1974], or on the *Social Construction of Reality* by Peter L. Berger and Thomas Luckmann [1966; 1969], had an especially large impact on our thinking, because they argued that social realities were constructed purely in terms of the perceptions of the actors themselves. By this, the basic idea of the first generation of historical social science as well as of the “quantifying” paradigm, dominating academic

sociology in the 1960s, 1970s and 1980s, were very much brought into question, because in fact they had argued that society could and should be analyzed and measured in terms of (measurable) “structures” and “processes,” without empirically investigating the actual practices and meanings of individuals and groups.

The “revisionists” now understood the dialectical relationship between structures and processes on the one hand, and the individual’s actions, meanings and experiences on the other, in approximately the following way: From their very first moments of life, human beings are constantly confronted with already structured and symbolized outcomes of social acting. They grow up with and become socialized within a certain language, in patterns and styles, to behave as a male or a female, as an employee or a freelancer, and so on. Yet at the same time, it is individuals who appropriate these structured relationship-models, absorb and internalize them, and perpetuate and modify them through their actions. In short: Individuals *structurize* all kinds of their social relations and conditions by *doing* gender, by *doing* parenthood, by *doing* paid work, and so on. Obviously, some actors are more powerful than others, and some acting is “blind” or has unforeseen and unintended consequences. Although individuals act according to their very own interpretations, common patterns of acting appear. Larger groups of individuals can be orientated by the same ideas or ideology and sometimes may even act to reach a common goal; only this should be called a “collective subject” (*Kollektivsubjekt*), which social historians and sociologists took for granted, focusing on what they grasped as “social structures.”

Within this post-structural (not post-structuralistic!) paradigm, structures are no longer seen as existing mainly *outside* of individuals. The different power relationships which relate individuals to one other also are structured. What is more, structures get *inside* individuals, in the form of rules and principles which guide their decisions, differentiations, communications and actions, summarized as *praxis*, and in the form of their mental and body structures and dispositions, summarized as *habitus*. In short: individuals are *structured* as far as their *praxis* and their *habitus* are concerned, and they in turn *structurize* their relationships, social systems and institutions through their *praxis* and their *habitus*. Individuals are neither entirely free to implement their will, nor completely restricted by the already structured circumstances. Apart from a few exceptional cases, they generally find a certain space for acting and interpreting within which they must come to decisions and perform actions. The size of this relative autonomy changes and has grown during recent centuries, as Emile Durkheim [1961], Max Weber [1972], Georg Simmel [1984], Talcott Parsons [1975], Jürgen Habermas [1981], Ulrich Beck [1986], and other scholars tried to explain by means of diverse theories of “modernization” and “individualization.”¹

This reformulation of what the individual and what society is, and what cultural, social, economic or political structures should mean in respect to both of them, was far from being an empty rhetorical exercise. It actually changed the perceptions of the historical world. Hitherto unrecognized places and people with their attitudes and forms of acting took on meaning and importance in order to describe and explain social reality and its historical change. Mentality, physicality, ideology, even fantasy, magic and

¹ Similarities and differences in the modernization and individualization theories of these authors are discussed by Markus Schroer [2000].

myth now seem to be just as relevant as what had been grasped before as the “external” structures of states, feudal domains, municipal administrations or party constitutions. To take one example: Even such a strong and disciplining external structure as a military unit could only be fully described and explained by analyzing their internalization through officers and soldiers, whose actions externally reveal what has been absorbed, thus structurizing communication within the military as a social system. – If the internal is equally important as the external, because the one could not exist without the other, then it follows that personal interpretations, desires, feelings, myths and imaginations are not merely some kind of incidental mist, obscuring the scholar’s view, but rather, they must be interpreted as constitutive parts of socio-cultural reality.

This theoretical view produced the impulse to draw up corresponding research projects, together with appropriate methodical tools and techniques. Foremost, the theoretical reformulation of the individual’s status in society necessitated the search for *texts* and *narratives* of all kinds that give us indications about social acting and the experiences of people. This is but one way of describing the so-called linguistic turn that has taken place in the humanities in recent decades. In no sense has this been a return to the writing of history, sociology or ethnology as a purely poetical act (poetic story-telling), as some critics from the first generation of social science history have assumed. [Kocka 1984; Wehler 1998] Just the contrary happened. The changes represent an improvement in, and the refinement of investigative and analytical methods, which are thus able to grasp the meanings inherent in what we call structurized practices and practiced structures.

HOW TO NARRATIVES WHERE NO TEXTS YET EXIST

Anybody who, like myself, is primarily interested in 20th century history is often confronted with the lack of adequate written material or records dealing with relevant aspects of the lives of “ordinary people.” But unlike historians of other centuries, we have the opportunity to ask them about their past actions, experiences and feelings. I now want to go into more detail about one of the main research techniques used for this purpose, namely the *narrative interview*.

Unlike, say historical demography, which only takes very few pre-selected characteristics of people into account, we look for complex figures (“actors”) whose activities and experiences should become accessible to empirical research. But it is precisely here that we have to be most careful, because to re-construct activities and experiences is not as simple as it might sound. It is not possible to go into detail here, but obviously “experience” and “activities” or “practices” are connected in various subtle ways. “Experiences” always contain reflexive aspects of the actor/narrator and are clearly the outcome of his interpretations. So if we listen or read narratives concerning individual experience, we get the actor’s/narrator’s own interpretations and those views and interpretations he wants to communicate. Of course the interpretations of the actor/narrator do not arise purely by chance. They have a definite relationship to his social practices, and they are interconnected with his social and cultural environment (*Umwelt*). As one of the theories we adopted after a delay of some decades, namely the theory of symbolic

interactionism [Blumer 1969; 1973] teaches us, every individual in each situation is confronted with a more or less wide range of possible meanings. The individual looks for his own interpretation in order to choose between different possible actions. The different meanings are mostly reached through communicating with significant others involved in one's own social system (family, youth group, enterprise, party, church, etc.). Moreover, these possible meanings and interpretations are offered to the individual by various *media* (e.g. coffee-break-gossip, newspapers, movies or TV talk-shows). As I will argue later, we have to look for the traces of all such media and communications within the individual's narrative, or to put it in more linguistic terms: for various texts inside the text.

The first difficulty with doing narrative interviews lies in finding the appropriate initial question, or better phrased: the proper invitation to start narrating. Needless to say, this invitation must be comprehensible in linguistic and cultural terms, but it also must *define the space* in which the narratives should take place. This is a crucial point and not one so easy to always get right; if it is too large, the narrator is potentially intimidated and may react by asking counter-questions, which then compel the researcher to restrict the narrative scope in a second try. If the narrator has already started with his highly complex work of recalling, explaining and rephrasing former accounts, keeping our attention alive by postponing the climax and so on, as researchers our first and main duty is to be curious, patient and tenacious *listeners*. This is in order to make the narrator be as free as possible to bring forward his narrations by associating one memory-puzzle with the other, guided only by his feelings, affections, sentiments, ideas and values, last but not least, by the "subconscious" in the sociological sense and by the "unconscious" in the particular sense of psychoanalytical theory.

From a social science perspective, narration is a particular type of social action. It involves making a series of choices between narrative options, and these choices correlate in a certain way to a series of decisions the narrator has made as an actor in the past. Usually he will not be able to review all of his options arising from being an actor, and he will not even as a narrator looking back and being more "experienced." Nevertheless, he will use the rare occasion which the narrative interview (similar as some forms of psychotherapy) offers him to summon arguments and rejected possibilities back into his mind. Therefore, again, as researchers we are obliged to behave as curious, patient and tenacious listeners. "Storying" or narrating implies composition, which includes different types of constructing acts such as storying, reporting, the description of relevant situations or persons, re-constructing arguments which were used in a former decision-making, frequently involving significant others as well as dominant discourses, global and special evaluations, etc. But all this does not mean that the narrative simply *reflects* a past event or one's taking part in it, no more than is the case with any other kind of text used in the humanities. Like any text, the narrative is a construction and a series of interpretations. But we can assume that it is possible to ascertain the constructive principles, which refer to those principles or patterns which guided the decisions made by the actor in the past. In other words, we can postulate a certain (but not full) *congruence* between the practices and experiences of the actor on one hand, and his narration on the other. Nevertheless, there is a *hiatus* between the actor's experience in the past and his narration in the present. But this is true for all historical and sociological research and every type of "source" and "data." As Johann Gustav Droysen put it in

his famous *Historik* back in the mid-19th century, the past is completely, irrevocably gone, and what we produce in the present by whatever means or techniques, is always just an *imagination* of it [1943: 7].

Given that we want the narrator to recount experiences in a detailed and freely associative manner; we must therefore limit our own active part in this special conversation and try to maintain an *open* methodological stance. In the classic narrative interview, originally developed and described by Fritz Schütze [1978], we do not pose any further question during what I might call the “grand narrative survey” of one’s life story or of a certain period of life, or of a special field of activities, put on the agenda by our initial invitation. In this way, the narrator remains free to *compose, condense, select* and bring “his” stories and descriptions to an *end*. That is to say, the narrator is – just as he was an actor during his life-time – a definite and relatively autonomous actor and not at all some kind of researcher’s puppet.

When the opening narrative is finished, which the narrator usually communicates verbally or non-verbally, we begin to ask immanent follow-up questions. Selecting from a list of keywords or phrases jotted down while listening to the grand life survey, we invite the narrator to relate further aspects of topics which he has already brought up. Nine times out of ten, these immanent questions will be something along the lines of, “You mentioned XYZ. Could you say a little bit more about it?” Less often we miss the evaluation of an event which was told in a story or in a brief report. Then we ask: “You told me the story (you reported ...) of XYZ. But I have not yet understood what this event *meant* to you (or: what it means to you today, looking back). Could you tell me about it?”

Our experiences in several research projects have shown that there are everyday routines which do not lend themselves to narration, because they are not seen to be unusual at all or to have changed significantly. For some research topics, however, these are the areas of greatest interest: e.g. the rhythms and routines of housework, daily work in a factory, living together in a long lasting relationship, etc. If this is the case, I recommend adding a distinctive third phase to the interview which I call *reconstructing highly routinized practices*. Here, we ask the interviewee to reconstruct as precisely as possible a particular weekday, the most recent day of work at the factory, or whatever routines seem to be most relevant with respect to the general topic of our research. In a sense, therefore, we try and go hand-in-hand with the interviewee through such highly routinized fields of behavior, in the hope that it will bring up details and aspects which were not mentioned in the initial narrative survey, eventually contradict a story narrated before, or offer a still missing link.

In certain interviewing projects we arrange special meetings with the interviewees, in order to look at the historical material they might possess, such as private photo albums, video tapes or written life documents. We discuss and examine them and we might again ask for stories about a certain person seen in a picture or mentioned in the life document. Looking at pictures, videos and written documents often stimulates or revives narration, but it makes the most sense to undertake this kind of exercise on a separate occasion from the main narrative interview. When we accept the introduction of pictures or documents during the initial biographical survey, because the interviewee definitely wishes to do so, this might disrupt the compositional coherence of the master narrative. In general, I close a narrative interview with an invitation for the narrators to

present their own summary of the conversations in terms of what feelings came up or what ideas or stances may have been changed during the process.

As soon as possible after the interview I listen to the tape or watch the video record, which enables me (or the research group) to get an impression of the specificity of the case; after that I decide (or the group of researchers decides) what the next case should be. Normally we try to find another case which differs significantly from the former, because this helps us to become aware of the special aspects each single case allows us to discuss. If the interview is part of a larger research project, with an organized budget and fellow researchers, the tape-record will immediately be transcribed and the group of researchers will start analyzing the text (see below) in order to formulate first hypotheses which might orientate the search for the next case. This proceeding we call a *theoretical sampling* [Strauss 1994]. It means, roughly speaking, that the gradual refinement of our explanatory ideas – our provisional hypotheses – determines the on-going search for further cases (sampling). Ideally, each successive case will confirm more precisely and legitimate earlier preconceptions and hypotheses. The theoretical outcome can then be described as *grounded theory* [Glaser and Strauss 1967; 1979]. The procedure thus dissolves the conventional sequence of social scientific research, namely that of hypothesis formulation, followed by data collection and finished by data analysis and the formulation of a synthesis. The process is instead *recursive* in nature. The single steps are interchangeable and continually re-orientate themselves around one another.

This recursive process of qualitative research comes to an end when we feel that our hypotheses will probably not be substantially altered by the findings from additional case-studies. In other words, we should achieve a kind of saturation of empirical knowledge connected with a particular theoretical output. In practice, of course, other considerations may intervene, such as a limited budget or an urgent deadline. The main criterion in this kind of research is not statistical representativeness, because that would require a different sampling method, a different style of interviewing as well as a different type of text analysis. It is the discovery of theoretically relevant links that is – in my view – far more important than the frequencies or distribution of relations and phenomena which themselves are believed to be already clear. Theoretical returns may occur in the course of a handful of case-studies, or there may be no interesting results whatsoever from 50 or more cases. Either way, the precise total is itself virtually irrelevant. As I pointed out at the beginning, the kind of theoretical results that we are interested in is defined by social theory: We like to know how individuals are structured by the circumstances, communications and relationships in which they lived, and how they themselves have structured those conditions by means of their actions and interpretations.

TEXT ANALYSIS AND REPRESENTATION

To start with the basics: transcribing narrative interviews from tapes and videos follows a very simple rule: write down exactly what you hear! Of course there is no doubt that even the most precise transcription still loses vital information, such as the speaker's intonation, their tone and style of gestures, imitations and so on. Yet the written version of the oral narrative is indispensable, because it enables us to examine the text *in*

extenso, free from the flowing speed of the spoken word. However, it is often wise to compromise and go back to the original tapes or videos if we wish to analyze those parts of the text where the nuances of speech or physical gestures seem to be particularly meaningful.

There are currently a number of different methods of text analysis in use. In simplistic terms, these can be divided into two main groups: Firstly, various variants of *content analysis*, methods, which in one way or the other selectively examine texts according to research categories and, therefore, reduce the text. Secondly, forms of *sequential text analysis* which look at the text as a whole and in terms of how it came to be produced.² The first type (text reductionist methods) cuts up – either literally or metaphorically – a text and picks out certain parts of it; other parts of the text are simply discarded, either because they are considered to be irrelevant to the research in question, or because they do not fit into any of the pre-arranged categories. This procedure of content analysis is well known in sociology and can be either qualitative or quantitative in form, or even a combination of both [Mayring 1995]. The main disadvantage of this kind of analysis is fairly obvious: It is unable to discover anything other than what its theoretical categories have already defined in advance. The procedure is thus far more likely to *confirm* parts of a specific theory than come up with any new hypotheses, even if content analysis has become more sophisticated since the 1990s, thanks to the work of Philipp Mayring and others.

I will present in more detail an example of sequential text analysis which I put together in an eclectic way using procedures developed by Fritz Schütze [1978] and Gabriele Rosenthal [1995], objective hermeneutics (*objektive Hermeneutik*) by Ulrich Oevermann and others [Oevermann et al. 1979], the codification techniques of Anselm Strauss [1994] and, last but not least, Johann Gustav Droysen's writings on historical source criticism and interpretation [1943: 149–187].

The theoretical starting-point for my method of text-analysis is that which I set out in the first part of the paper. The individual is unavoidably compelled to perform as a socio-historical actor, and thereby to choose between different options and alternatives. Narration can only occur *ex post*, after the decisions between various options and the decisive action have taken place and became part of a private or non-private event. Hence, every act of narration (and every storying within the narrative interview) deals with the looking back at actions, arguments, interpretations, discourses and experiences in the narrator's past. For this it does not matter whether this past was yesterday or fifty years ago. Through this act of reflection and the recollection of previous actions, individuals feel obliged to justify, legitimize – even to distance themselves from their former decisions and actions. On the other hand, the special position of the individual as a narrator allows him to add aspects: In the meantime he or she has gained further experience in communications, heard of new interpretations, theories or discourses, and so on. Two fundamental requirements are to be followed throughout our text analysis: Firstly, we must try to reconstruct the social practices intrinsic to the individual's own life course, including the accumulation of different levels and dimensions of experience. Secondly, the analysis has to convert back into an open and hypothetically still undecided life path what in retrospect necessarily appears as a teleological outcome. These

² See the methodological survey by Siegfried Lamnek [1988].

two basic requirements are to be operationalized step-by-step, in a series of up to six analytical stages:

1) I ask the first sequence³ of the text: what events are being reported here? Which places, which people and what time period is being spoken about? These initial questions are answered according to the (well known to historians) technique of *pragmatic interpretation*, a phrase taken from Droysen's teachings on interpretation, which corresponds to "level 1" of Ulrich Oevermann's objective hermeneutics. In other words, we ask: Was does the narrator want to tell us? We find this out by paraphrasing the text sequence. At this point, we are interested in the so-called *manifest meaning* supplied by the narrator.

2) I examine the "historical" circumstances: the time, place, purpose, etc. which the narrator is speaking about, a process that Droysen termed the *interpretation of circumstances*. The most important and difficult questions at this stage of text analysis are: From which peculiar standpoint does the narrator speak in this sequence? This, for instance, requires some knowledge or at least some hypotheses on how many and what different standpoints of actors involved were given at this particular time and at this particular place. What did the actor, to which the narrator refers in a kind of *Ego-duplication* [Lejeune 1994: 13–51], know, and what did he eventually come to know later on? Which discourse impressed and orientated the actor at the time? Can we detect some textual traces of such a discourse within the narration? (In some cases this question will be asked more extensively in a special stage of text analysis, see stage five below.)

3) Next step is to ask what *circumstantial knowledge* (i.e. *latent meaning* in Oevermann's objective hermeneutics) can be added to the text sequence's manifest meaning from the standpoint of a well-informed (!) researcher. In some contrast to what Oevermann argues for his procedure, I deliberately include contextual knowledge in a very extensive way. So I might, for example, ask: What do we know about the school-system in say, Vienna, in the 1920s, if the narrator's story about his experiences at a Viennese school in the 1920s has to become analyzed, and is there special writing on it or do special film documents exist? Frequently, this special knowledge can only be acquired by additional research, which might interrupt the text analysis for some weeks or so, but which definitely enriches our circumstantial knowledge and will make our text-analysis far more reliable. Again, this additional special knowledge is employed in so far as it helps to evaluate the social, economic, political, cultural, etc. conditions in which the actor was performing, and how the field of interaction in which he participated, was *structured* by him and his co-actors. For these special research acts I write down *memos*. These memos make analytical sketches of how a certain information or theory could affect the interpretation of a single sequence, make connections with different or contradicting theoretical standpoints or point to possible linkages with other parts of

³ The problem as to how large the single text sequence should be, can normally be resolved in a fairly practical way: either there is a clear thematic break which defines where the sequences start and finish, or there is a difference in the type of text being constructed; this might be a story at one stage in the text, followed by a report or descriptions later. Or the sequence may be determined by a change in tone or style of speech, or by a change in speaker (turn-taking). In my experience, the average length of a particular sequence tends to be between 5 and 10 lines long. But this depends very much on the whole economy of the research process. The richer this economy is (in terms of money, time and personnel), the smaller the average sequence will be.

the narrative, and so on, in a way that will later make it easier to keep theories and text sequences in a system of references.

4) The fourth step is to investigate the *internal experience* in so far as it seems profitable to do so, and as long as it accords with the interpretation put forward up until that point. This is what Droysen called the *psychological interpretation*, which implies asking what internal processes are being spoken about that relate either to the actor or to the narrator. As in the previous stages, it is again necessary to confront the Ego-duplication, and to distinguish between exactly who is speaking about their personal experience: the narrator, or the historical actor (which the narrator was in his past)? In answering this question, we should be able to find a point of connection – a semantic trace – of this supposed mental process in the text itself, as a means of regulating our psychological fantasies. I fully agree with Oevermanns rule of being sparing with psychological explanations (*Sparsamkeitsregel*). But here again, I will employ extensive contextual knowledge, because it should be clear, that even psychological categories do not exist outside history. For example, in one interview the narrator told of his experience as a working-class child in Vienna and spoke about witnessing his parents having sexual intercourse. To help interpret this episode, I used the work of Siegfried Bernfeld, a Viennese psychoanalyst and former pupil of Sigmund Freud, as it dealt specifically with working-class families during that period in time. In this way I got information about a psychoanalytical discourse which was around at the time when the young man (the later narrator) tried to cope with his “prime-act”- experience [Sieder 1986a; 1986b; Bernfeld 1970; 1991]. Nevertheless, there are still certain precautions that need to be taken. No matter where it comes from or how relevant or prestigious it may appear, the use of a particular scientific theory does not allow me to draw definite conclusions about my case-study. Theories adopted from the wide and heterogeneous theoretical context of text-analysis offer just one way of reading and understanding the text sequence, which still has to compete with others.

5) I try and ask whether it makes sense to identify any form of ideology, morality or philosophy within the text sequence – what Droysen called “the interpretation of moral forces and ideas.” This kind of question is not always appropriate for every text sequence, but rather frequently, because there is nearly no public discourse without a moral, ideological or philosophical impact. And as I have mentioned earlier, the narrator feels himself very much urged to clarify his own position vis-à-vis morals, ideologies or philosophies (*Weltanschauung*) and their implications for his own decisions. To tell his (imaginary and preliminary) life-story is the most important way to keep this clearing going. I would then inquire: Where do these ideas, these morals, this philosophy come from? Do they correspond to major ideological currents in the actors or the narrators contemporary society? Do they belong to a particular political camp? Is it possible to make explanatory connections between ideology, morality or philosophy and the manner of actions or psychological patterns that I am going to analyze?

6) The last step is to imagine what the subsequent room for action is, and this again in a double sense: on the one hand, what was the actor’s potential room to maneuver, as regards the further course of his *history*? And on the other, what is the scope of the narrative space available to the narrator? Which is the likeliest direction that the narrator will take in telling his *story*? In taking this sixth step, I open up a range of possibilities and options, both historical and narrative, a variety which sometimes seems to

overwhelm us. But this corresponds to the second basic requirement that I talked about earlier, namely the need to convert the finished story back into a series of options. As I do throughout all steps of the text analysis, I enter these further possibilities as hypotheses onto a separate hypotheses-sheet. Hypotheses about specific sequences of text are simply marked according to their place in the transcript, which is a scrupulous protocol of the conversation, for instance: page 1, line 10–14; page 5, line 4–8, and so on. In this sixth step I refer to hypotheses on the possible subsequent course of action in the past as “historical sequence hypotheses” (HSH), and those concerning the possible subsequent evolution of the narrative as “narrative sequence hypotheses” (NSH). Narrative sequence hypotheses can usually be linked to ideas about the probable kinds of texts that will recur next in the narrative (i.e. whether the following text sequence will take the form of a story, a report, a description, or an argument).

At the end of this stage, if not earlier, I am in a position to decide if the text sequence has been sufficiently analyzed. I can then turn to the next sequence, open it (because I covered it whereas I analyzed the previous sequence) and begin the procedure once more in the way I have described. During the whole process, it will become apparent whether former hypotheses have been falsified, or which hypotheses have been further confirmed, or why a hypothesis turned out not to be fully accurate and in need of some alterations.

HOW TO PRESENT CASE STUDIES

In the meantime, a long lasting debate has been decided: There can be no story-telling without theory! Moreover, there are no theoretical discussions in the humanities which are not of service to the analysis of narratives. The point is rather that the story or history must be told in such a way that the narratives (or at least elements of the narratives) are made accessible to theory. Here, I would agree with Jörn Rüsen, who argues that the specific rationale of the historical sciences is their ability to provide *explanatory narratives*, or alternatively, *narrational explanations* [Rüsen 1990]. And I believe, this might also be true for most of the humanities, including the more recent types of movie- and other media analysis. Therefore, it is a question of presenting the one or more case-studies in a way that puts the theoretical elements in a visible context, thus enabling the reader to follow the development of a theory.

In doing so, we are able to combine *hermeneutic*, *analytical*, and *dialectic* operations. The hermeneutic part involves deciding about the meaning and sense of the text. As I have already demonstrated, this is not hermeneutics in the classic and historicistic style, which only asked for the intrinsic truth (or: the manifest meaning) of the author, but also the juxtaposing of *manifest* and *latent* meanings. The analytical operations concern everything that extends the knowledge of the actor/author/narrator: They imply the application and construction of different kinds of theory, which are not theories of the actor/author, the examination of comparative cases, the setting-up of a typology, and so on. In so far as these operations distrust the manifest meaning of the actor/narrator and try to de-construct how actors and narrators construct their meanings within an universe of discourses, there is at least some similarity to the procedure of *deconstruction* [Derrida 1992], which is usually thought of as being the opposite of any kind

of hermeneutics [see Bertram 2002]. It turns out, however, that hermeneutic procedures in the particular recent mode of the cultural sciences [Soeffner 1989; Soeffner and Hitzler 1994] and deconstructing procedures do not entirely exclude each other, but can be “combined” in an open process of text analysis. Lastly, the *dialectic* aspect requires looking at the changing relationships between the changing circumstances and the actor’s experiences and actions during his life time. This includes theories and discourses which may intervene in everyday life by the media and by those experts and professionals which argue in the name of these theories and discourses; frequently they have the power to introduce discourses and their crucial notions and values into the individual’s horizon of meaning (*Lebenswelt*).

How can we present all this to the audience? Qualitative, case-based research has the possibility of presenting either individual case-studies, or of making a comparison between a number of different case-studies. In the latter instance, it is also possible to set up a case-typology. The presentation of single-case-studies is normally done in article form, although occasionally these appear as monographs, often under the heading of micro-history or micro-sociology. To conclude, I would like to look briefly at one of my own case-studies [Sieder 1993; 1995], in which I dealt with some issues concerning the Hitler Youth (*Hitlerjugend – herinafter HJ*). I will focus here on the aspect of how this case-study is constructed in order to place the theoretical outcome in the foreground. Moreover, it should become clear that even the most theoretical case-study is nothing other than a special type of narration: an *explanatory narrative*.

As with every narrative, the presentation of this case-study begins with numerous items of information that implicitly enable readers to place themselves within the period and location of the Hitler Youth member’s childhood. The historical circumstances that surrounded the child in the 1920s are presented via a mixture of paraphrases and direct quotations from the narratives. The scene is set by describing a district of the city of Vienna, where he grew up, his family home, his father, mother, brothers and sisters, the essentially patriarchal family relationships, and the long-standing family traditions and ideology connected to their history as a bourgeois, military (officer-class) and acting family, together with their socially downwardly mobile status in the 1920s. I deliberately paint the circumstances and family relationships in a way which already contains a theoretical perspective: The social-cultural milieu is presented in such a way as to make the explanatory power of its socialization effects evident, and to make clear its links to other social-cultural milieus in that district of the city. The psychological (psychoanalytical) interpretation of the internal experience of these relationships is also presented, as in the following description of the father’s historically determined situation:

To him the family heritage was both an obligation and a burden. He suffered from being forever cast into the shade by the fame of his celebrated great-aunt. Yet he prided himself on belonging to one of the ‘oldest dynasties of actors in the German-speaking countries.’ (...) When he was forced to discard his uniform in October 1918, his world collapsed. He missed the stage and the corset previously afforded him by the military attire. Moreover he was unable to develop a bourgeois discipline of any sort. (...) He was one of those stranded people who had been robbed of the Emperor’s uniform. [Sieder 1993: 100]

Then I consistently try to apply *contextual knowledge* (see above) in order to evaluate the actor’s/narrator’s situational circumstances, and this eventually enables me to

formulate a first hypothesis, relating the history of the father to the socialization process as it has affected the life course of his little son up until this point, when the actor/narrator was nearly ten years of age:

My first hypothesis is that the structural opposition of a bourgeois family marked by social deprivation, on the one hand, and the solidarity in the street and the neighbourhood, on the other, caused Peter to develop a proto-political stance which was determined by the following ideology: Human beings are not equal since they possess different hereditary faculties. This is why human beings also bear different responsibilities. This Darwinistic understanding of societal structures was transmitted to Peter Treumann first and foremost by his father and the narrative tradition of the family history. After the disintegration of the monarchy in 1918 and the material decline of the family in the 1920s, the above concept was grossly at variance with the experience that only the solidarity of the street-gang or of the grown-ups in the neighbourhood, respectively, could ease the material plight. [Sieder 1993: 103–104]

This leads back to what I mentioned earlier about circumstantial interpretation and about psychological and ideological interpretation: Here, the presentation explains in a narrative way how elitist and social Darwinist attitudes and ideas began to root themselves in the individual, and how – over a period of time – these practices became “characteristic” of this individual. In other words, the interpretation presents the ways in which his main dispositions or his *habitus* developed and took effect. Proceeding from this, I recount Peter Treumann’s route into the Hitler Youth organization, together with the accompanying politicization of the father-son-conflict, which then forms a second explicit hypothesis: “The personal became increasingly political: the conflict between father and son intensified and was carried out more and more with explicitly ideological arguments” [Sieder 1993: 104]. Not by chance, a number of new figures enter the story at this point and are ascribed important functions in the conflict, such as an Austro-fascist Minister and former officer in the war (Emil Fey), who bolsters the father’s political opinions; or the socialist poverty counselor (*Armenrat*) and the national-socialist leaning school-teacher, who oppose the Austro-fascist system supported by Peter’s father. The narrator also introduces events such as the civil war of February 1934 or the failed *Putsch* by the illegal National Socialists in July of the same year, because they contribute substantially towards the son’s politicization – and promote the ideological split between father and son [Sieder 1993: 105–108].

The story continues with such determining experiences as contacts with girls from the League of German Girls (BDM, i.e. the National Socialist organization for girls and young women), actual attacks by Peter Treumann and his Hitler Youth colleagues on Jewish youths, and their social and physical battles with other youth groups, above all the so-called “Schlurfs” [Gerbel, Mejstrik and Sieder 2002]⁴ There is a further theory that the “Schlurfs” represented the very opponents of the Hitler-Youth because they

⁴ The term “Schlurfs” had been widely used as a derogatory name for adolescent “loafers” and “tramps” in Vienna since the early 1930s. Subsequently, the term was applied more specifically to youths who dressed and behaved in a certain way. After the NS takeover of Austria in March 1938, the term was adopted by the state authorities (the school administration, police, public youth welfare and juridical authorities).

too had grown up on the streets and were not afraid of picking fights with other youth groups, such as the Hitler Youth. The “Schlurfs” imitated US- fashion and demonstrated their anti-militarism and aversion to achievement. This group constituted the absolute negation of Hitler Youth ideals and symbolized it aesthetically, which again suggests a psychoanalytically informed hypothesis addressing what was going on in the actor’s psychic system:

In his imagination Peter Treumann projected what he had warded off himself on to the Schlurfs; apart from their consumerism and their refusal to perform in school or at work, it was above all their relations to girls and their liberal approach to sexuality which indicated their social inferiority. They embodied, so to speak, the negation of the HJ ideal and therefore appeared to be the natural enemies. [Sieder 1993: 112]

Treumann’s narratives on his encounters with Jewish youths in the streets of the 2nd district of Vienna and with girls from the German Girls’ League confirm and differentiate this hypothesis so far. The narrative-theoretical presentation ends with explanations as to why, after the *Anschluss* with Germany in 1938, Treumann becomes disillusioned with the increasing bureaucratization of the Hitler Youth organization and the loss of its elite status, to the extent that he resigns from the organization and volunteers for the German army, where he then begins an officer’s career (as his father did in the First World War).

The last part of this theoretical narrative discusses some of the highly theoretical aspects of the case in more abstract terms. This means explaining the narrative principles, which guided both the actor in living his life and which guided the narrator in the composition and performance of his life-story. It is here where the theoretical assumption, that there might be a partial congruence between the life *process* and the life *story* given by the narrator, has to be examined again. We have to distinguish between the practices of life and the performance of one’s life story, even if the principles and patterns of life practices can only be ascertained empirically from analyzing the narrative. Therefore it is necessary to go back to the narrative and explain the actor’s active involvement in the decision to embark on a career in the Hitler Youth (and later, the *Wehrmacht*), rather than any other. If we were to naively take at face value the narrative sketch provided by the narrator, that would imply that his becoming a member of the Hitler Youth was inevitable, a matter of compulsion. Yet this would mean the construction of a total *sub-jectum* which is deprived of every possibility of acting and interpreting its own accord, a puppet of history, so to speak. My main argument is that Peter Treumann’s specific and “individual” modes of interpretation, his sense of orientation and particular strategies of action, identification and power accumulation, influence his confrontation with the historical circumstances and choices in front of him. The narrative principles he used *indicate* the processes of adaptation involved, along with the formation of a particular view of the world and the people in it:

(...) three processes attract our attention: allegorization, typification, and homologization. Allegorization signifies the comprehension and interpretation of an abstract context by means of personification. The persons used for this purpose usually belong to the world of primary experiences (father, mother, godfather, Nazi teacher, the socialist Armenrat). Typification denotes the application of auto- and hetero-stereotypes to social groups: the

solidarity of the street gang, the antisocial attitude of the Schlurfs, the passivity of the Jews, the cowardice of the socialist fighters of February 1934, and so forth. (...) Homologization means that within the family, the street-gang, the HJ group and so forth, the historical subject discovers equivalents of the general social conditions. The patterns of interpretation, the ideological elements and phantasmagorias used in this process, are taken from the discourses the subject partakes in through hearsay, written texts and images. The social roles, positions, and characteristics of both family members, neighbours and friends on the one hand and of the respective adversaries on the other hand are projected on to society or parts of society by the adolescent (and still by the adult). One's own family does not function any better than the system of the Ständestaat; the Volksgemeinschaft operates in the same way as the HJ, only on a larger scale; in the competition in the street the young Jews show the same kind of behaviour as the Jews on the ramp at Auschwitz. [Sieder 1993: 114–115]

Other arguments in this explicitly theoretical part of the case-study relate to the social labeling of persons and groups, which can also be observed throughout the stages of personal development, and to the body sensations which help determine the development of a sense of masculine identity and distance this member of the HJ from girls and women, and from the feminine side of his own personality.

The willingness to disregard the physical and psychological costs of standing up for one's values, the latent tendency to self-destruction, and the identification with dead (murdered) heroes seem to have become habitual from childhood. These traits are constantly promoted by the various agencies of socialisation (street-gang, Pimpfengruppe, illegal Hitler Youth, Staatsjugend, Führerschule, Wehrmacht) and are lauded as "duty," "loyalty," "courage," and so forth. The subject took in the rewards for auto-aggression in order to pass them on again – for instance as leader of the HJ. [Sieder 1993: 116]

The analysis concludes with a final argument on the relation between the individual and the societal:

The HJ became the site of Peter Treumann's social life in adolescence. This social space organized and structured the dispositions of the adolescent. It continued to mould what had had its beginning in other social spaces (in the family, in the street-gang). On the other hand, Peter Treumann also took part in the forming of this social space through his actions and interpretations. [Sieder 1993: 116]

This sketch illustrating the constructive principles of a qualitative case-study and its theoretical outcome should make it easier for the reader to understand how the procedures of conducting narrative interviews and of doing sequential text analysis can operationalize the most prominent process in question: the interaction between the individual's social acting on one hand, and political, economic, social and cultural circumstances on the other. Or to put it more simply, to describe and explain how people make history, without being free to do so.

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THE ROLE OF FIELDWORK IN CONTEMPORARY ANTHROPOLOGY

TATIANA BAJUK SENČAR

Anthropology as a discipline has been historically defined by its focus on cultural diversity. Although this focus may have been influenced by different driving questions during the course of anthropology's history, anthropologists have always been dedicated to the study of cultures across the globe. How has this fundamental precept been understood within the discipline? How has this understanding affected the methods employed to answer the discipline's questions and what sort of presumptions have these methods embodied?

These questions have become more relevant in recent years as anthropology has been coming to terms with many issues, one of them being that of globalization. By globalization I am referring to two different processes. The first is linked to technological advances in communication and transportation which are understood to have brought the world closer in what David Harvey has termed a "time-space compression" [Harvey 1990]. The second is the realization that the relationship between distinct nations and cultures – presumed to exist separately from one another – is continually changing and does not necessarily coincide with our theories and models. While these two processes are interconnected, each has its own dynamic; one is not directly determined by the other.

In this paper I focus on the second process mentioned above – changing understandings of the relationship between cultures – and the way that concepts of culture are linked to the practice of ethnographic fieldwork. How do these changing perceptions inform the practice of fieldwork? What is the future of fieldwork in the discipline of anthropology?

As is well known, anthropology has been undergoing a great deal of change in recent decades. Critiques of the discipline, particularly of anthropology's links to colonialism and imperialism, have forced anthropologists to re-evaluate what they had considered to be a neutral, objective, intellectual project. Through the years these critiques have addressed virtually every aspect of the discipline, and have played a key role in expanding our knowledge of the multitude of factors that come into play in the production of knowledge concerning cultural difference: from the privileged, position/gaze of the researcher, the continual construction of the Other, to recording and writing processes. At the same time, these critiques seem to have generally overlooked anthropology's distinctive methodology: ethnographic fieldwork. Many have pointed out that the present understanding of fieldwork as one of anthropology's identifying features has precluded questioning its effectiveness. Instead, fieldwork understood as "intense, long-term research among a community of people" [Barfield 1997] has attained the status of a rite of passage that identifies "real anthropologists" and has become the criterion

by which to evaluate “real anthropology.” Anthropology, according to internal critics such as James Ferguson and Akhil Gupta, has thus become identified more by its methods than by its central questions. In their view, the very process of internal evaluation has allowed for the emergence of a paradoxical situation as anthropology struggles to maintain its identity vis-à-vis other disciplines:

On the one hand, anthropology appears determined to give up its old ideas of territorially fixed communities and stable, localized cultures, and to apprehend an interconnected world in which people, objects and ideas are rapidly shifting and refuse to stay in place. At the same time, though, in a defensive response to challenges to its “turf” from other disciplines, anthropology has come to lean more heavily than ever on a methodological commitment to spend long periods in one localized setting. What are we to do with a discipline that loudly rejects received ideas of “the local,” even while ever more firmly insisting on a method that takes it for granted? [Gupta and Ferguson 1997: 4]

While I would question according fieldwork an overly deterministic role in anthropology’s reassessing its present position, I would agree with Gupta’s and Ferguson’s argument cited above insofar as the unquestioned role of fieldwork within anthropology has allowed for a situation in which reassessments unfolding at the level of theory are not addressed in the realm of practice. This gap represents a lack of dialogue between fieldwork and changing priorities, questions, and concepts in anthropology; without a continual dialogue, the process of reassessment cannot come full circle, and anthropology as a discipline will not be able to deal with the theoretical and practical challenges that the present and future will bring.

Before going any further, I wish to emphasize that anthropology’s interest in the cultures of the world inherently implies that anthropology has always been global; in this sense anthropology as a discipline has played a positive role in portraying the richness of cultural diversity. Instead, the key question becomes how this cultural diversity has been imagined, portrayed and analyzed. Critics of the discipline have highlighted the relationship between the anthropologist/researcher and the Other, contesting the naturalness and self-evidence of this relationship as it has been traditionally construed, and pointing out that this relationship is informed by socially constructed notions of space, distance and culture.

James Clifford, for example, has incorporated anthropological fieldwork into a larger history of travel, distinguishing anthropologists from other world travelers (here including persons such as missionaries, traders, colonial officers, settlers) by their particularly disciplinary practice of travel and dwelling: the mobile anthropologist travels to the field, dwells among the culture under scrutiny, and departs from the field to write [Clifford 1997]. According to Arjun Appadurai, the notion of the anthropologist mapping out world cultures in this fashion requires the production of what he has termed the “incarcerated native” bounded in time, space and identity. The mobile anthropologist requires a localized and static field – which presumes a certain kind of Other and, of course, a certain construction of the culture to be observed. According to Appadurai, the challenge for the anthropologist lies in recognizing the fact of the mobility of those identified as Others and in assimilating this into his or her theories and methods:

As groups migrate, regroup in new locations, reconstruct their histories, and reconfigure their ethnic "projects," he ethno in ethnography takes on a slippery, nonlocalized quality, to which the descriptive practices of anthropology will have to respond. The landscapes of group identity – ethnoscapes – around the world are no longer familiar anthropological objects, insofar as groups are no longer tightly territorialized, spatially bounded, historically self-conscious, or culturally homogenous. [Appadurai 1991: 191]

The mobility to which Appadurai alludes is not simply physical but also intellectual, in that the peoples who are often subjects of anthropological research are not *a priori* determined by anthropological constructions of location and history but transcend this categorization and have a certain amount of control in how they articulate their identity. The non-localized quality of the ethnoscapes mentioned by Appadurai signals a subversion of this relationship as well as a de-centering of the anthropologist as the privileged observer. In fact, the challenge now lies in recognizing the constructedness of this global geography and addressing the implications that this has for anthropologists' position as fieldworkers and for the production of anthropological knowledge.

To continue with the theme of the non-localizable quality of the field, I wish to shift my focus to a particular phenomenon considered to be a product of globalization: the emergence of transnational cultural formations, that is, formations that transcend the boundaries of an individual culture. What sort of implications does the existence of transnational cultural formations have on our understanding of culture, cultural difference and the effectiveness of fieldwork? In light of the critiques of fieldwork sketched out above, is it possible to adapt fieldwork to this (non-localizable) set of circumstances?

In the second half of this paper I will address these questions through a discussion of the role of fieldwork in the interdisciplinary field of science studies and of my own research among Slovene economists. At first it might seem strange that I will be citing discussions outside anthropology to discuss the possibilities of anthropological fieldwork – however, this will operate as a means of counterbalancing the naturalized role of fieldwork within the discipline. Furthermore, I will be mentioning these discussions because they set the groundwork for anthropologists who then took on the transnational phenomenon of science as an object of study.

Science studies refer to a large body of research conducted by historians, philosophers, sociologists, scientists, feminist theorists, anthropologists, and other scientists. Here I will generally limit myself to recent trends in the Anglo-American schools. While science studies have deep intellectual roots, they were recently revitalized by researchers such as Thomas Kuhn, whose aim was to analyze science independently of the image that science has of itself [Kuhn 1970]. This task is extremely difficult, as science itself defines the prevailing standards for the production of knowledge, standards which also pervade the social sciences and (in part) the humanities.

The key contribution of Thomas Kuhn lies in his pointing out that the belief generally held among scientists (and others) of science as an objective progression of knowledge towards truth, and of scientific method as the only valid means of attaining truth, does not accurately reflect the sequence of research activities involved in the production of knowledge. Instead, through an investigation of the workings of the scientific community itself and the way it had incorporated scientific discoveries, Kuhn

developed his now famous model of paradigm shifts to account for the production of scientific knowledge.

Thomas Kuhn and others such as Paul Feyerabend [1978], whose work focused on scientific method, began analyzing science not according to the standards science itself produced, but instead as a social phenomenon. By social I mean that science is not defined solely by universal practices that transcend or exist independently of culture, but that science as a transnational phenomenon is also in part defined by cultural contexts. The focus of the history and philosophy of science shifted onto the producers of scientific knowledge and onto scientists as socially and historically contextualized actors; this inspired the first ethnographic laboratory studies aimed at exploring the role of social conditions in the process of scientific production, such as those of Bruno Latour, Steve Woolgar [Latour and Woolgar 1979] and Karin Knorr-Cetina [1981]. Turning the tables on their colleagues, these researchers observed the interactions of scientists in a controlled setting in which science was literally produced – among scientists in a laboratory – in an effort to understand the role of scientists in the way science develops.

The works of anthropologists rendered the use of the ethnographic method in science studies more precise. While the scholars cited above employed such methods to gauge the effect of so-called social factors on the production of science – and going so far as to affirm that scientific facts are socially constructed – anthropologists focused on the so-called social factors themselves. For example, anthropologist Sharon Traweek engaged in a study of physicists which presumed that they form a community of knowledge, a culture like any other that has come under the scrutiny of anthropologists. Traweek carried out her research in a high-energy physics laboratory in accordance with the traditional norms of anthropological fieldwork and produced a fine-grained ethnography of the culture of US physicists within the confines of their community of knowledge. Work in this vein not only expanded the horizons of anthropology and its concept of culture, but also brought anthropology's understanding of culture to bear on scholarly discussions whose main dilemma was how to conduct research on groups who by definition were considered to have no culture [see Traweek 1988]. For these researchers the notion of a culture of scientific expertise provided a means of thinking about scientists that was not informed by the ways that scientists viewed themselves: as pursuing a form of knowledge that operated according to its own laws, independently of any social or cultural factors. Instead, the aim of anthropologists was to transcend the distinction made by scientists between science and culture, which in effect privileges scientific method and practice as a way of knowing about the world.

Furthermore, the work done by anthropologists (work which in turn was based on the achievements of science studies researchers) effected a shift in what George Marcus termed the research imaginary in anthropology: “a level of consideration, crystallized as new strategies brought to the early conception of research” [Marcus 1998: 10]. The notion of science being a cultural formation and scientists being cultural actors, members of a community that may be studied as a culture, has reconfigured anthropologists' understanding of the relationship between science and culture. This has opened a new arena of research for anthropologists that enables them to pose new questions and design new projects pertinent to the contemporary world. The application of field research practices to studies of the realm of science has contributed to the development

of a new direction in science studies as well as the establishment of the anthropology of science.

The question now becomes whether, in light of the critiques of fieldwork that I have related earlier, fieldwork can be considered an effective methodological tool for the investigation of transnational cultural formations. I will discuss this in light of my own research experience with Slovene economists. The defining question of my research was the investigation of the cultural authority of economics during Slovenia's transition process: to try to understand how economic discourse as a form of knowledge and explanation operated socially, without accepting the response accorded by economics discourse itself, which is based on economics' image as a universally applicable form of knowledge which operates according to objective laws. The transitions from communism in Central and Eastern Europe and the emphasis placed on economic reforms as a solution seemed only to confirm the self-evident universality of economics as a system and as a form of knowledge of the world [see Bajuk Senčar 1998].

In an effort to ground this abstract image, and influenced by trends in science studies, I planned to shift my focus onto economists themselves, and conducted field research in Slovenia for a period of two years among Slovene economists. However, I soon realized that while a study of the internal workings of the community of knowledge composed of economists confirmed the existence of the community, it did not help me understand how economics as a form of knowledge operated socially. An ethnography of the culture of economists was not going to explain the cultural authority which they exercised outside the confines of their scientific community.

I also soon realized that I had assumed that knowledge about economics, and thus the roots of economists' cultural authority, flowed from inside the community of experts to the world beyond. Anthropologist Emily Martin, whose latest work is an ethnography mapping out the shift in American ways of thinking about health and the body, writes about the fact that this assumption, made by anthropologists as well as by other science studies researchers, is itself an image that scientists have of the relationship between the scientific community and the outside world [see Martin 1994]. Anthropologists' field research on the scientific community, though in itself an important step in understanding science, runs the danger of reifying the boundary between science and culture maintained by scientists themselves. The critique of fieldwork mentioned earlier concerning the dangers of over-localization are even more relevant in the case of transnational cultural formations.

In my case, the issue lay in a potentially reductive approach to the cultural identity of economists if I looked solely at economists' activities within their community of knowledge. In observing their practices I soon realized that most economists played very active roles outside the boundaries of their specialized community, either as consultants, media figures, businessmen, policy makers, politicians, or writers. In trying to track the role of economic knowledge and the authority exercised by economists, I could not limit myself solely to what occurred within the boundaries of, for example, the economics academy. Adding another layer to Appadurai's concept of ethnoscaples, the aim of my fieldwork became instead to follow the cultural geography mapped out by economists themselves as they traversed different cultural spheres and appeared in different media. Attempting to ascertain the different aspects of economists' cultural identity became the new framework for my field research, and allowed me to address

the cultural authority of economists from a different perspective. Furthermore, it enabled me to link the cultural authority of economics to certain values, formations, and actors within broader Slovenian society.

If one takes into account the efforts of science studies to analyze the transnational formation of science, one can conclude that despite the dangers of over-localizing one's field site, fieldwork can still be an effective methodological tool. Ethnographic fieldwork has enabled researchers to identify the extent of science's social power, which until now has precluded researchers from understanding how science operates as a social, cultural formation. The use of fieldwork has contributed greatly to reconfiguring perceptions of the relationship between science and culture in the contemporary world and has also effected a crucial shift in anthropology's research imaginary, particularly regarding how to conduct research on science. It should be emphasized that field research on science and scientists is not intended to reduce science to culture because it does not take as given the image of science propagated by science itself (i.e., that science exists as an autonomous sphere). Instead the intention of such field research is to identify some of the links between science and culture in order to imagine science and its relation to culture or cultures in more effective terms. In an analogous vein, my advocating the use of fieldwork in the field of science studies does not imply that fieldwork does not remain a potentially problematic tool. However, new trends in anthropology, including the anthropology of science, seem to show that once these problems are identified, there exist certain possible adaptations to the implementation of fieldwork. It is in these trends that I see a productive future for fieldwork, so long as its use is continually questioned and reassessed.

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FROM ETHNOGRAPHY TO AUTOETHNOGRAPHY

JURIJ FIKFAK

It is necessary to avoid the illusion that this paper is already finished and that the reader is confronted with a final reading. In *Hermes' dilemma and Hamlet's desire* Vincent Crapanzano writes that the ethnographer is only a messenger who produces texts about the foreignness of cultures and societies; and the nature of his or her presentation can never be anything but provisional [1992: 43]. However, this statement does not mean that anything goes; rather it implies a drawing nearer the "object" as the basic premise whereby the researcher tries to understand the foreignness of the cultures, societies and people that he/she encounters and studies.

When a researcher has been to the same place many times, when he or she represents him/herself and is also accepted by the village people as a professional stranger who is more or less allowed to look into their lives, to be a part of their events, to take pictures of them and their festivities, etc., it is often possible to hear the following statement from some village representatives:

You know more about us than we do.

This admiring as well as doubting and hesitant comment shows the practical, commonsense understanding that people usually express when they are confronted with non-commonsense knowledge, if we use Alfred Schütz's distinction of commonsense and sociological knowledge. However their statement also implicitly reveals the dilemma exposed by Michel Foucault, that the creation of knowledge or "truth" in an academic, dominating discourse is inseparable from the production of power [cf. Dreyfus and Rabinow 1983: 202–204].

Let us take these comments, attitudes, and different opinions as a starting point to define some elements of the relationship between researcher and informant. To do this I will try to invert my own position (with a bit of irony), using Jerry Seinfeld's sitcom as an example. All the men and women in the show are looking for the ideal partner, and the catch is that they are not allowed to find and get the ideal one. In one episode Jerry discussed with his friend George Constanza what the ideal partner should be like. One of the first assumptions was that she should be like him. This attitude is one of the most commonly accepted, as confirmed by Ayala M. Pines in her book *Falling in Love*: "Hundreds of studies from all over the world suggest that we love our reflection in the other" [Pines 2003: xvii]. Thus Jerry found somebody just like him. She spoke, acted, chatted, reasoned, and behaved the same way he did. She was neurotically obsessed with hygiene just like him. Even more: before he could say anything, she would tell a joke he wanted to say; it was almost unnecessary to speak, just a look at one another was enough. She

was like his clone; she mirrored or even anticipated him. As soon as he envisioned something, she had already done or realized it. And this sameness became wearisome and frustrating. He could not satisfy his need for recognition, i.e. for power. There were no surprising jokes, which would satisfy his need for fun. It was not possible for him to exceed or surpass her; at the same time the necessary self-establishing boundary became invisible and Jerry became lost in the world of her immanence to him.

Certainly there is a need for reciprocity and boundary: "The thing that I found most attractive about her was the fact that she was attracted to me ... She needed me, she needed someone who will respect her and I needed her" [Pines 2003: xvii]. The implicit conclusion is that what we like best in our relationship with another is the boundary between the other and ourselves, which can be momentarily transgressed. This boundary is implicitly suggested also in the dedication "For Art, my other I" [Ellis 2004: V], Carolyn Ellis wrote in her thorough introduction to the autoethnography. Instead of a game of crossing the boundaries, what happened in Seinfeld's case was immanence, a binding of the self. There was nothing left that could not be anticipated and/or agreed with. Faced with an inability to be unique and with an awareness that there was somebody constantly accompanying him like a shadow or a reflection in the mirror, his first solution was depression and his second was escape – flight. So he eventually broke up with her, as he did in every show. This excursus tells that a difference and boundary is implicit in every relationship [cf. Javornik, forthcoming]; admiration and fascination about the other as well as conflict potential. However, the researcher is a professional stranger [Agar 1996], "objective" and at the same time "doubtfully loyal" [Schutz 1970: 93–94].

Let's take another look at the statement, or sentence: "You know more about us than we do." We can assume that the main object of ethnology or cultural anthropology is the foreignness or otherness, which is established through its difference from the known. As researchers we borrow the space- and time-scapes of the other. Temporarily we live in the foreignness of the other's semiosphere [cf. Lotman 1990] and then leave it behind us to make conclusions and to present the reader with a final reading about them (him or her) in it. This statement shows the implicit power the researcher has in the field, and therefore it is not unusual that many times researchers (including myself) were and are seen as representatives of the state.

Nevertheless, this power is often questionable. The ambivalent nature of the researcher's power is clearly visible in the problematic relationship between the ethnographer's work and the institutions of the repressive apparatus, such as the police [cf. Fikfak 1999a: 33] and the department of welfare. A possible explanation of these problems is that the ethnographer (ethnologist, cultural anthropologist, or sociologist) who deals in qualitative research with minorities and marginalized members of society is often the only one who truly listens to them [e.g. see the question of ethics in the last chapter in this book by van den Hoonard]. Other institutions, including the department of welfare [e.g. see Mesec's text in this book], often merely control excesses and are interested primarily in control mechanisms, i.e. giving or taking legal custody. They do not attempt to proactively help the individual or the family and do not search for or encourage resources that the individual or the family may have at its disposal. The ethnographer is thus one of the rare outsiders willing to understand the informant in the light of the informant's world theory, without attempting to impose his own solutions to the problem.

Even this image is not quite accurate. In the informants' eyes, the researcher's questions and descriptions should establish a semblance of "village" common sense, and possibly as well present and support the idea of the harmonious past life (through the ethnographic present). Otherwise, his actions are often interpreted as interfering in somebody's life. The ethnographer becomes a "strange intruder" [cf. Portis-Winner 1994]. The power of the researcher is the invisible collected information, over which the informant has no control and which can be used or even misused against the informant. The researcher's power is also the ever denser and stronger hermeticism of the scientific language and discourse, a whole system of different explanations, wherein the informant easily gets lost [cf. Jackson 1989: 122]. On the other hand, in this uncertain world (and even more so in the past than today) this same researcher is often the only person interested in the informants' lives, especially if these are people on the margins of society, e.g. different ethnic, social, linguistic or other minorities. This intruder is observed and accepted with wonderment, admiration, and often with expectations of what he or she must do, and approval if he or she meets these expectations. Here we may recognize another example of the search for recognition and power at work; on one hand we find the marginalized group aspiring for public recognition through the qualified researcher and on the other a knowledge/research-based power that understands itself as having a license or permit to pursue broader social and political engagement.

Many social scientist (also in Slovenia) in performing this social and political battle or struggle for different minorities (sexual minorities, national minorities, the 'erased') become representatives of the oppressed other. Once a stranger, an outsider, they want to be insiders. Precisely at this moment when they become the agents of someone's interests they more or less (unconsciously) cross the border between scientific perspective and the common sense knowledge of politics, more precisely they transgress, melting the boundary between the self and the other [cf. Schutz 1972: 97ff.]; they no longer represent "the crisis of the 'natural attitude'". If "... the stranger represents a potential challenge to the ontological security of the self" [Gandesa 2003: 10], then in this case s/he shows a simple affirmation of political correctness or even engagement. The discourse is at once almost exclusive and no longer self-reflexive. A very interesting and common phenomenon in this perspective is the ascription of agency politics to the other party. The phrase "*You know us better than we do*" becomes synonymous with their representation of the other.

The researcher – agent of the other thinks that the minority, the research-object, can not satisfactorily represent itself, therefore s/he is the speaker who will do the job right. S/he ascribes, forces upon the research "object," the minority, their own understanding, "you know better what we want to do than we do ourselves." The "object" is often willing to accept this understanding, this underprivileged position, since a presence in the media is thus secured, important issues will be published and become popular, etc. However often the underprivileged are only an instrument, sometimes sacrificed or even abused for the researcher's ambitions and self-promotion. But even these initiatives to speak for the other, which try to have democratic implications and consequences, implicitly lead to the conclusion that such relationship is always unequal, asymmetric; and also the final concluding question: Who remains in the field once the researcher (in this case agent provocateur) has left?

The researcher seeks precisely this asymmetric point and the thrill it gives her or

him. The fascination for the strange, the other (the Other) who is different and thus immediately unavailable to us. At the same time, it is easier to research and see the other in action, in process, than it is to see oneself [cf. Schutz 1972: 102].

Now we can turn to another point of the statement. Tzvetan Todorov in *Morals of History* [1995] speaks of the change that occurred at the end of 18th century, when the terms human and social sciences were replaced by moral and political sciences. How profound is our common sense understanding of the human and social sciences as moral and political can be demonstrated with one example from the study of how the border between Austria and Yugoslavia in the reality and minds of the people living there was created and accepted [Fikfak 1992; Moser and Katschnig-Fasch 1992]. I was interested in the personal stories of men and women who had participated in one way or another in the drawing of the border between Austria and Yugoslavia after WWI, which split the village in half. One of the elder informants, who had been a teenager at the time, looked at my questions and understood my interests as political. After the interview I called him and requested another session, I wanted him to tell me more about past events and to explain some facts to me. I was astonished when he said, "I do not want to talk about politics," and hung up. At that time I could only assume that the personal engagement of his father and other relatives in the village in the drawing of the border in 1919, which was for me only a story about the search for an identity, was to him a political question. Looking back, from a different point of view, he was right. This informant preserved some essential information for himself. The real problem is that, while trying to obtain knowledge about so crucial a moment in the history of the demarcation, I did not realize it when I crossed the boundary between him and me in such a way that he felt threatened and was no longer prepared to respond. When and where did I cross line of privacy [cf. Hall 1990a; 1990b; Pellow 1996] that allows everyone to establish him- or herself as a self-reliant and confident person and decent heir of their family history? Where did I cross the very real line, where the history of him, his family and its relatives, who were most probably split by the decision for or against Austria or Yugoslavia, would show their political leanings, several of which were certainly wrong from the informant's contemporary point of view. On the other hand, in this same study we could see that another informant, the son of a peasant who sold his property in the newly established state of Austria and bought property in Yugoslavia on a hill in plain view of his former property, did not state that my questions were political. To him (and implicitly also to me, as for him I was almost certainly a representative of a state financed research agency) this decision was politically correct.

As ethnographers we do "know more" about informants than they do, since we have the power to tell the "truth" about them, to speak instead of them. But this image is often very deceptive: Which images of the other are available to us? Which images of their lives do they wish to convey to us?

Just some decades ago the field of ethnology treated this other as the researcher's object; he was a "given" or objectified through cultural objectifications. This fascination and aspiration for a full description, this Pygmalion's ambition, was realized and fulfilled with mimetic descriptions. We can state with Juri M. Lotman, that the "evolution of similarity between art and life is subjected to reductive strategy" [Grigorjeva 2003: 218].

However, in the world of indeterminacy this object/subject is no longer available

in the same way as s/he was. We can ask ourselves, can we understand the other or can we conclude that it is in the limited nature of our knowledge and comprehension that this other always and unavoidably escapes us and refuses to be the steady object of our description, observation, or analysis. The other tests our sensitivity in the field; this other is available to us only through the use of the chief mechanism of cultural evolution and cross-cultural exchange as Lotman defined it: dialogue.

STORY ABOUT THE GIRL¹

Our implicitly un-obstructive, inter-subjective or even neutral attitude – which should shape our quest(ioning) for (so called objective) facts and allow us to describe somebody's cultural fact, e.g. a festivity or even his/her culture as foreign – are often unsuccessful. Our informant, the other, sees differently. He or she divides time and defines the space of the community in a different way: through the model of folk culture, the model of the normal course of a life/life trajectory, and eventually or ideally, through the narrative interview, also the story of the private time and space of the self.

The other's time, his past and present, as well as the time of the Self, are not directly available. They are not accessible without assistance, without mediation. One of the instruments used for solving this issue were and are different narration techniques. The choice of narration techniques largely depends on scholarly habitus: which procedures either from part of the "unquestioned routine," or within the traditional repertoire of the ways questions should be answered. As a whole the choice of writing procedures demonstrates the author's basic affiliation to some stream or direction (of thought?); a text also shows the fine movements of the paintbrush [see Ginzburg 1988] which betray the author, his/her understanding and composition of the image of the world. In every case his/her narrative is connected with a problem of double or triple structuring.

Several years ago at a research camp in an Istrian village we interviewed several people, especially older men and women. Our aim was to gain some knowledge regarding the identity of the village, and their life histories, with the village festival as the focus of our observations.

From an interview with an older woman I will now introduce some translated quotes. One of the main events during the village festival is a dance. On the question of dancing the woman (I) answered as follows:

I: ... *it just happened that one good-looker, who knew how to dance, she danced on the table, she got herself on it, ya, ya, ya, ya, she is still alive, she went and they danced on the table, oh what a dance, which is, whatever was, noo (welll) ...*

R: *And the others, did they applaud, or what?*

I: *And they laughed, all kinds it was, it was more comic, more, someone says. To some of them, to them ... it is natural to say something, you know, some of them could not, haa, haa, some of them could not, not to ... ya, well, I, this would be about this, now, I don't know, what this might mean (SIGH) when w(e) ee for example I don't know, affected ee,*

¹ Stories presented here, were published in Slovenian language [Fikfak 2000; 2001]. However, these stories are here extended.

we were terrible ee when we this ee gave b(irth) (SHE DOESN'T FINISH) w(hen) we had this little, this now (SHE MUMBLED SOMETHING SILENTLY) it was terrible shock, hear me mme, it is not needed, necessary perhaps, because it doesn't have, is not ... Such a shock, that, ee ee we didn't really get over through and all, it was terribly hard, there wasn't this soon ... when afterwards we had the boys, it was when she was two years old (SHE SPEAKS VERY SILENTLY, THEN SHE RAISES THE VOICE) Terrible shock, such a shock, I am telling you, that ten years I didn't speak in the factory about her, I couldn't (NOISE FROM THE STREET, NOT UNDERSTANDABLE). After that they said to me, such, now ... we were really sorry for this feast, it was over, we were really sorry ... I think, I am returning back ... we, were really sorry ... t(hat) t(hat) now one year we are free, it was very hard for me, ya and such young girls danced at the edge of the dancing place, you know. On Monday, on Monday they left everything, left everything.

The quote raises many questions. One of them is regarding the informant's perception of time and space. Not only are the time and space this informant has lived in different from ours; what the meanings and statements regarding the village festival, are perceived through her experiences and events in her life. At the beginning of the interview (before the passage quoted here) the interviewee makes a sharp distinction between the festival in the village that we were researching and the village where she was born. She answered, "I will speak about the festival in the village I was born in." She defines herself as competent only for her youthful experiences. In this quoted passage she still speaks about her youth in the other village, etc. She speaks of her handling of events, of her involvement in them. Her narrative at the beginning (including passages I did not quote) could be described with one of Gabrielle Rosenthal's three definitions of a storyteller [1987] – "As it was" – the position or attitude that researchers saw as the ideal of ethnographic description for a long time. Her narrative is constructed mostly in terms such as "they or we were doing, dancing, etc." on the level of the village's common sense knowledge of the model of the ritual: i.e. on the level, where the village (actually their representatives – opinion makers as well as bearers of tradition) itself defines what is and what is not appropriate to a feast and feasting, which rules were or are important, etc. At this level we do not know about the distance and ambivalences they have to the practice of tradition. We can describe her narrative as an "ethnographic past," i.e., the narrative is within the rules that were usually translated by ethnographers into the dramatic – ethnographic present. This generalizing presentation that accommodates and at the same time shapes and reproduces the common sense understanding of ritual practice was unexpectedly rich for the usual "ethnographic presentation" in our case, as it was widened or extended through concrete examples of dancing, specifically mentioned persons, etc.

After initially answering the question of what the watchers did, she suddenly marks her switch into something else, into a personal perspective of events. The marking words *they say*, are the starting point for the explanation of ability, of who can or is able to say something and who is not. Then the turnabout comes: "*to some of them it is natural to say something ... some could not ... well, this would be about this, now, I don't know what this might mean*", and she switches her narration to storytelling about her child. She makes a lot of switches in the sentences when she uses either *I* or *we* as a person/s who experienced this drama; in these cases her speech seems very fragmented.

Behind the image of such logo-ruined storytelling we can assume that she is telling this story for the first time in this context. She marks the end of a very personal narrative with the sentences: “*I think, I am returning back ... we, were really sorry ... t(hat) t(hat) now one year we are free, it was very hard for me, ya.*” And then she switches back to the question she was asked at the beginning.

Her position is understandable from her behavior when her child is recognized over and over as unable to take care of herself or fully participate in village life at any festivity in the village she married into and where she lives. Her life in the village is simply defined through her dramatic experience of giving birth to a mentally retarded baby, for her there are at least two times and spaces, her past time and space is her happy youth long ago; while taking care of her adult daughter defines and limits the joy of her present space and time.

It was this personal perspective of the experience of time and space and the cutoff points in one’s life which produce divisions in time that was probably the most important issue that was almost unwittingly collected from the life histories of the people of that village.

This is the stage where the researcher has to confront and elaborate the various times. The difficulties that arise with a mentally retarded child, or in another example a snakebite that results in the victim’s loss of a leg, mean the difference between an ordinary life course and a particular event which marks and completely changes the expected course of events and the whole life that follows. The model of the ritual the researcher usually deals with becomes the framework within which different actions take place, with different people taking on different roles [cf. Gerndt 1979], as well as the site of the differentiation of the expected and unexpected perception of the event. The perspective from which the informant describes the ritual in her youth is expected and described in its normalcy in relative detail, regardless of events, illnesses and deaths in her family. However, the perspective which the researcher expects and asks for regarding the ritual in her new village is labeled by her child, whose life could not take its normal course as the other children’s did. Furthermore, we are dealing with a child who, according to the mother’s own understanding, could not fully take part in all the joys and rituals that she experienced in her own youth. For this reason she first declined to speak about this second period and at the same time implicitly described it from a labeled perspective.

From this apparently fragmentary text it is obvious that the narrator, within the scope of her understanding, anticipates what the researchers are interested in, very likely the model of the ritual. At the same time she very precisely distinguishes what she has a right to speak about – the time of her unmarked youth, which is characterized by a normal socialization (we can describe it with the concept of normalcy) – and where her right is questionable (*what this might mean*) or she cannot speak of it. At the same time the form of the dialogue – defined by the interviewers introductory question regarding her experience and understanding of the world and a neutral question about what the others were doing – allowed her to talk about the reactions of the others, thus opening that level of discourse where the disparity that formed between her normal experience, the normal course of ritual events, and her labeled experiences was all the greater. She relates the level of her experience that she would most likely never have wanted to communicate to an ordinary person asking a question.

Our informant, the other, saw differently, she divided time and defined the space of the community in a different way: her youth with the model of folk culture that was presupposed with the model of a normal, ordinary life trajectory; the idea of the narrative interview that we tried to introduce at the summer camp also presented her with the possibility to speak about her unspoken traumatic experience, her traumatic chronotope of herself.

AN AMERICAN IN THE EAST OR THE SELF AND THE OTHER

Not so long ago I heard a presentation from an American ethnologist; twenty years ago he had participated in field research done by researchers of folk culture in an Eastern European country. The main topic of his presentation was the relationship between him and his colleagues, regarding which he spoke about their theoretical and social background, and how he was perceived and accepted. I was the only representative of all the countries who had experienced a communist regime. However, I objected to some of his implications and misunderstandings of life in the former socialist countries. The first concept he introduced was eastern hospitality, which he saw as servility. My first impression was that we can describe hospitality as an important part of etiquette in these countries; secondly, we can speak about an eastern cultural concept of approaching a foreigner that is different from the attitude shown in western countries. In most eastern countries it is still a habit to welcome a foreigner, to fill his work- and free time, i.e. to be “responsible” for him. And most western researchers are willing to temporarily accept their “regressive, inferior” position.

The answer unveils different models of socialization and the internalization of behavior patterns [see Berger and Luckmann 1988]. The behavior of a “typical” western [cf. Hall and Hall 1989] scholar shows individuality and independence. The behavior is at the same time impregnated with competitiveness and professional uncertainty. This assumption does not entail a disqualification of the “western” type of communication, which has certain other advantages compared to the “eastern,” e.g. it is easier to keep track of giving and taking offers or invitations; it could happen that to buy someone a drink would be seen as interfering with his/her privacy. A stranger should be very careful when s/he tries to interpret a foreign culture. Michael Herzfeld pointed out that it is hard for the anthropologists, “to see the prejudices that their own work inadvertently conveys – prejudices, moreover, that it often shares with nationalistic ideologies” [1997: 156]. The American colleague sometimes showed a typical colonial and oversimplified understanding of foreign culture. What he understood as servility was actually a very complex cultural concept of hospitality, deeply rooted in the culture of “primitives.”

Another moment was also very important. I was accepted in almost the same way as the American during my visit to the GDR: to the East Germans we were part of the socialist, communist system, but on the other hand we had passports and were able to move as freely as the American. I remember an academic exchange one year before East Germany collapsed. We were standing with an East German historian – a volunteer in front of the Brandenburg gate. This gate was to her a sign of another system, and unlike her I had a key that could open and let me through this door into what was to her a world of unlimited possibilities. With a new car, with a passport, which allowed me

to go through this gate, I was for her a signifier of this other world and as such I was welcomed in the trivial, oppressively ordinary, everyday life of the GDR. We could say that for the eastern scholar a foreigner was a window to another world, sometimes a visit from a foreigner was even a prestigious event.

In the former system, the farmers and peasants – the main “producers” of folk culture – were, besides dissident intellectuals, the most maginalized social stratum. They were especially hospitable to the researchers who were interested in their life, who saw them as experts in folk culture and tradition. There was no need to pay informants [ex. Caplan 1995].

The American in the East expressed his opinion about “eastern” cultural concepts and research practices, a so-called romanticism. We can say that this “romanticism” was based on folklore oriented research [Fikfak 1999b], it was mainly rooted in the historical geographic method. The polemic point I want to introduce is that the postwar system had an ambivalent relationship with folk culture that was in any case negative –and/or intolerant of expressions connected with confessional or religious practice that were not in line with the mainstream of state ideology. This research of folk culture could show [cf. Fikfak 1999b], that it was not possible to present the country as atheistic. The invisible and unclear power of the folk-culture phenomena was always an obstacle to the mainstream ideology and governing structures. At that time there was a difference between the mainstream of sociological and folklore studies. Quantitatively oriented sociological research was very popular and generously funded: among other things, anonymous individuals were asked about their religious practices and beliefs, sexual habits, etc., and the researcher needed little effort to prove that the world of beliefs and practices was syncretic and inconsistent. He was satisfied that the results could not fit the expectations of church and clergy, who tried to shape the people’s behavior in line with a coherent system of church and religious dogma. On the other hand, the researcher of folk culture proved that religious practice still exists despite all the school and media education based on so-called “scientific socialism” and atheism as a norm. And it is very interesting and characteristic that in this postwar “enlightenment-al” reorganization of the world two traditions became subversive: the carnival – Mardi Gras – and religious practice. Folk culture is, as Bahtin found in his research of Rabelais’ creativity [Bahtin 1978], a subversive, and uncontrollable point, which every government, both state and church, wants to control, and which in every case escapes these control mechanisms.

The last point of my polemic with my American colleague was the question of auto-ethnography [cf. Okely 1995; van Maanen 1995; Bochner and Ellis 2002]. The American colleague investigated and evaluated other researchers from the field twenty years ago, and he did not let us know what material (memory or documentation) his thoughts were based on, what his or their theoretical assumptions were at that time and what they are now. His auto-ethnography would allow us to see his prejudices, what feelings he had, how he changed his attitudes, how deeply he was involved in the eastern culture he researched, etc.

When we conduct research in and on a certain community, we always find it difficult to consider our intentions, our values, and ourselves. Gerhard von Kutzschenbach in *Feldforschung als subjektiver prozess*, found that only the data and theories of a certain society’s functions which the researchers were looking for were presented. Some of them

found harmonic, organic societies of illiterate people as opposed to the heterogeneity and conflictual nature of modern industrial societies; other authors who anticipated the free development of personality, emphasized the possibilities offered by modern society [Kutzschenbach 1982: 48–49]. But this is not the only consequence: researchers' values establish not only their perspective within the described culture, they also decide on the steps and instruments used for analysis. Therefore it is an important issue who the addressee is, who the publisher, who the sponsor of the research. [1982: 51–52].

One very important issue is also the so-called culture shock. We should take and reflect upon it as a relevant and unavoidable experience on the way to accessing and acquiring knowledge about a foreign culture. We can define this shock as the collision of two different cultures, two different times. How these different times defined my understanding of the other, I can tell with a story about listening to the radio.

FLAGS, FLAGS

May 1st (Labor Day), about 15 years ago, was the first time I visited a village in Italy where the Slovenian minority near Trst/Trieste lives. I saw a red flag on top of the May-tree, the red flag together with the Italian and Slovenian (with a red star) flags in front of a monument to dead partisans and victims of fascism in WWII. The same red flags and red stars can be seen today, 15 years later.

One of the questions that arose was: "What does this red color mean to the people living there?" I can offer some provisional directions with two answers/comments I have heard during my fieldwork.

The first answer was: "We fought under this Slovenian flag (with the red star) in front of this monument and for this occasion there will be no other Slovenian flag or we would not be here." This red flag, this red star was for me something not understandable; therefore I have to present my experiences, to move from ethno- to auto-ethnography, to "the ethnographic I" [cf. Ellis 2004]. I remembered a scene with the flag where the red star was torn out (in former communist countries). The red star in this case represented the entire communist system, all of its repression, the control wielded by the party secretaries who wrote reports about our social-political correctness. The almost unanimous vote for an independent state of Slovenia was part of this process, and throwing down the red flag or the red star was an important element in this process.

In front of me was a man – a Slovenian, a former partisan and also an Italian citizen – who did not undergo this kind of internalization of the socialist and communist movement and education. For him, living in a country with a ruling Christian democratic party, mostly anti-Slovenian in orientation, this flag was a sign of battle, of rebellion, of resistance against Italian oppression. The red (workers') flag on top of the may tree was also a victory of Slovenian cleverness over Italian bureaucracy. This flag and red star were signs of the Soviet and later the Yugoslav model. At first he had problems with Tito's opposition to Stalin at the time of the Informbiro (in 1948 and for several years afterwards) when a majority of Slovenians were anti-Titoist. The second shock for him was the collapse of Yugoslavia, which also represented the collapse of many communist as well as Panslavic ideas. If the declaration of independence meant deciding

in favor of freedom for the majority of Slovenians, who lived in the former Yugoslavia, it was most likely viewed as a capital mistake by the majority of Slovenians in this and neighboring villages in Italy.

When we now try to define a very sketchy semiotic scheme of binary oppositions, we could say that the same symbols have different meanings.

For some Slovenian researcher, or for a Slovenian living in former Yugoslavia these symbols, e.g. the red flag, red star, old Slovenian flag, could be equated with the communist system, the domination of one party, the “scientific explanation” of the world, the repression of different opinions, etc. However, these discussions about the past are part of the transition nowadays, e.g. the question of the sign TITO or NAŠ TITO (our Tito) near the Slovenian-Italian border, which presupposes different explanations. Symbols are used or many times revitalized to (re)define not only the past but also to (re)assure the legitimacy of ideological offspring/descendants.

For the majority of the people in this village, these symbols represent their fight against fascism, their participation in WWII on the side of the partisans. Not only the old Slovenian flag with the red star together with the red workers’ flag and the Italian flag, but also the monument to the partisans and victims of WWII has a special meaning and position in the life of the village people. Its meaning and role is ritually defined and celebrated two times in year, on Nov 1st and on May 1st. Since the year 1975 this monument has played a very important role in the initiation rite – the young men’s rite of passage that happens on the night of April 30th to May 1st.

I want to point out another statement. A few years ago I was in the same village, it was after midnight on the night of this rite of passage. I was talking with an informant and he told me about the radio, “You know, we didn’t listen to Radio Trst A (even long before independence), we only listened to Radio Koper and Radio Ljubljana.” To him and to them Radio Trst A was Anglo-American, run by individuals who had emigrated after WWII from Slovenia. I felt that for him, his family, and a large part of this population, the world of the Slovenian postwar emigration was excluded from public and private life. Their image of the world is also impregnated with their connection to the Soviet Union or later Yugoslavia. In first months of Slovenian independence they had difficulties understanding Slovenia’s separation from Yugoslavia, it seemed contrary to the Panslavic concept, which is still a part of the self-understanding of a large part of the minority. Even today you can hear the Serbo-Croatian song about Yugoslavia *Od Triglava do Vardara* at almost every party. A joke published in the regional journal *Primorski dnevnik* (Trieste/Trst) speaks of the nostalgic relationship to Yugoslavia, as father and son discuss whom they should support in a football match, Yugoslavia or Slovenia.

For the researcher who speaks the “same language” and who lives the “same culture,” it is difficult to understand that a representative of the minority is and was living in a different imprint of time, in a differently defined space from “us here,” on this side of the border [Merku 1999].

It is incorrect to produce research without reflecting on our own biases regarding basic “leftist” or “rightist” assumptions. Discourse about national identity, about minorities, about political options and opinions, is always implicated or influenced by selection and information biases; thus I have to recount some of my experiences. In the sixties, and to a lesser degree in the seventies, *Radio Trst A* was for me a window into another world, a world that kept a distance from the events in Yugoslavia. It was interesting be-

cause of its melodic, sometimes exalted speech, which I never heard on Radio Ljubljana. It offered me a different point of view as well as authors excluded by the socialist media. But even more important was probably another point. I was convinced that this radio represented a place where the endangered Slovenian minority in Italy found for itself a common language and interests as was the case before WWII [cf. Tuta 1999].

The statement I heard from my informant, namely that they did not listen to Radio Trst A, shows that the primary issue after WWII was not being Slovenian. This central space was and still is simply politically divided into left and right, into “red” and “white.” With these comments my informants gave to me a manual on how to grip the handle of the door of otherness. This story about the radio or about the red star is a story about different experiences of time. What would have been at an earlier time merely a route to the ethnography of an internally divided minority thus becomes also a route to auto-ethnography. The story about the other is not only a story about the informant, it is at the same time also a story about myself and my perception, it is a bottom-up view. Through ethnography about the other I came to the ethnography of myself, my perspective, my ideological and other interests, images, worldviews, etc. What I do is akin to what Katherine Frank has implicitly pointed out, the image she has of herself is unavoidably connected with the images and desires men have about her [Frank 2002a]. In this perspective it would be possible to extend the definition of auto-ethnography used by Deborah E. Reed-Danahay, who points out that no matter how we define *auto-ethnography*, one “of the main characteristics of an auto-ethnographic perspective is that the auto-ethnographer is a boundary-crosser, and the role can be characterized as that of a dual [or multiple] identity” [Reed-Danahay 1997].

What is the relation between me and this informant? First of all, we can see in it a structurally conditioned conflict. The informant’s point of view and timing were very different from mine, with other values, other perspectives. There is anxiety as well as freedom in the meeting with the stranger; they

need to be viewed as two sides of the same coin: anxiety arises because modern practices are radically contingent and open-ended. Otherness is the condition for the possibility of the very dynamism and freedom of the modern self (Kymlicka 1996; Parekh 2000). The image of the stranger is thus necessarily doubled: he is simultaneously an opportunity and a threat. While post-modern feminism and post-colonial theory and multi-cultural conceptions of citizenship view the relation to the other as an opportunity for the creation of new emancipatory, hybrid forms of subjectivity, right-populism views the stranger as a threat. [Gandeshha 2003: 10]

Certainly there is no objective, unaffected or neutral point of view that I use in research, as Kutzschenbach and many others have pointed out. In any event, this view is defined by certain assumptions, in this case with the attitude that any ethnic, linguistic, racial, social or other minority, if I follow the statement of Nancy Fraser [2000], has a right to live, express itself and be recognized. But how could any specific, unique culture in this world take its own place, how could it survive? We are confronted with the fact that the no longer unified, auctorial voice is split into the voices of the researcher and the voices of the other – the informant. The chronotope of narrative splits into many sounds, into many voices, into heteroglossy or polyphony (after Mihail Bahtin’s terms). The task of transcribing and analyzing all these voices, all these melodies and rhythms,

all these disappearing and reappearing, deeply rooted and sometimes distorted memories [Schachter 1997] remains unsolved. The uniqueness of any culture that we meet or have met through the research process brings us to the main source of ethnological or anthropological research. Johannes Fabian states:

Before ethnography can be a problem of meaning (hermeneutics) or signification (semiotics), let alone of explanation (positivist science), it is constituted as a praxis. When we study other cultures, our theory of their praxis is our praxis, I once said, and the foremost problem of ethnographic writing is not the extraction of meaning or the matching of signifier and signified but the meeting – I prefer confrontation – of kinds of praxis, ours and theirs. [Fabian 2001: 4]

In this field between their and our praxis, between ethnography and auto-ethnography, between provisional and final reading we can comfort ourselves with Hoelderlin, who wrote:

Das Unmittelbare, streng genommen, ist fuer die Sterblichen unmoeglich, wie fuer die Unsterblichen ... (The immediate, strictly taken, is for mortals impossible, as well as for immortals ...) [Allemann 2000: 180]

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THE PLEASURES AND DANGERS OF NARRATIVE: INTRODUCING THE PROBLEMATICS OF THE SOCIOLOGY OF METHODOLOGY

R. DANIELLE EGAN

I sat down to write this chapter on feminism, methodology and the politics of doing participant observation. I was interested in the politics of negotiating speech and vision, about the methodological quagmire of hearing something that contradicts observation and I hit a snag. A snag that brought me to the Marquis de Sade and to the unconscious, a snag that compelled me to think about new immersions, new re-visions of narrative, and sociological method. This rupture moved me in both visceral and intellectual ways to think about contradictions, limitations, and the problematics of “honoring voice” as that which is beyond sociological critique [see Behar 1993] – and the compounded limitations of expertise found in realist ethnographic texts. That is to say, I began to ruminate on how to problematize narrative without falling into the sticky wicket of positivist expertise – which, at base, shares both the pleasures and dangers of Sadism. In my attempt to problematize the sadistic aspects of positivism, I questioned how to avoid reifying narrative as the whole truth and nothing but the truth. Given these concerns, how do those engaged in overtly political sociological methods such as feminist, participatory or collaborative and critical methods (all of which I am committed to) engage with the voices of others without granting them immunity from contradiction, foreclosures and complexity? Lastly, how can we become comfortable with what is not visible and the traces and specters that inform ethnography as a modality of inquiry? This chapter questions the underlying assumptions of qualitative methods. What follows is my foray into this gap and the resultant questions that plague me here, and from which I have yet to emerge. In this sense my immersion is anything but complete. Given the newness of this query, this chapter should be read as an initial engagement with the epistemological assumptions of narrative and qualitative methodology.

My questions began with my ethnography of the sex industry and it is this experience that serves as the grounding for this story. However, to spend too much time on this story, the story of a real live nude researcher would lead me from the gap and it is precisely in this space that I wish to remain [for more on my project see Egan 2003; 2004a; 2004b]. Therefore, this chapter is a sort of footnote. Footnotes function as the diversion an author resorts to during the writing of a text. The place which might be too distracting for a reader of the (w)hole work, but where many creative inquiries can take place. Footnotes are often the site of the articulation of snags and the place where the messy and not fully comprehended are placed. Since one of the central components of our work as methodologists is precisely the site of messy and complex fields of the social, we might be more attuned to diversion and, therefore, footnotes might be precisely the place to start this discussion. However, in order to have a more informed

understanding of how this chapter emerged, it is necessary to reconstruct the context to more fully understand the diversions that will follow. Let me start with the “main body” (that is, the original text or at least a section of it) and move onto the diversions that emerged in the writing of this chapter.

SEX WORK AND THE STRANGE CARNALITIES OF A REAL LIVE NUDE RESEARCHER

My research explores the complex and contradictory relations between exotic dancers and their regular customers. In order to understand the often unsaid aspects that formed the relations between dancers and the men who “fell in love” with them. To understand these complex relations I conducted ethnographic research. This methodological tool made sense. It allowed me to view these interactions in their “natural” setting, to view the contradictions and map out a schema of the seemingly taken for granted aspects marking the relations between dancers and customers. Moreover, it provided the methodological justifications for participation in the scene as an exotic dancer. Participant observation, with its use of informal interviewing and observational techniques (which allowed me to view the mundane and, in my case, not so mundane practices of the club), gave me a more complex vision of club and the interactions of dancers and their regular customers.

Ethnographic tools provided an opening into a world of contradiction where words and actions do not always match up. Moreover, it created intellectual and visceral illuminations with regard to interactions and ways in which power circulated within the club. However, it was precisely this aspect of ethnography that led to the gap in which I am presently situated.

As a feminist, I initially viewed my work as “honoring the voices of others,” particularly the voices of marginalized women working within the sex industry. I also wanted to take seriously the voices of their male regulars and the ways in which they “made sense” of their experiences within this particular context. This call to honoring, inspired by feminist theorists and methodologists such as Dorothy Smith, Shulamit Reinharz, and other standpoint epistemologists, asks us to give voice to those who have otherwise been silent or silenced in the academy [Smith 1990; Reinharz 1992]. This epistemological framework informed my endeavor. However, upon “entering the field” and viewing the contradictions between words and actions, I found that an unabashed honoring would, in effect, deny the complexities of the interactions between dancers and their regulars as well as the ways in which people made sense of themselves in their narrative constructions of themselves.

Another layer of complexity was found in the auto-ethnographic focus of my research. How was it possible both to represent my experience while recognizing the partiality of my own experience in the club? Remaining cognizant of the seeming “authenticity” of personal experience, I sought to point towards the shadows that haunted my own accounts. In my re-memories of my experience there were inevitable occlusions and gaps. In effect, I began to question how one could write auto-ethnography without reifying the narratives of the experience of the author? How one could recognize the importance of experience while not collapsing it to represent the Truth of the situa-

tion? How can one de-center personal experience while illuminating its power and foregrounding its partiality? To, as Derrida theorizes, actively resist the romance with metaphysics of presence [Derrida 1978].

My concerns, at first, did not seem so out of the ordinary ... after all it is precisely this contradiction that makes ethnography in general and participant observation in particular, powerful methodological tools. But as I began to write about this I was distracted and hit the snag, the snag that formulates the query of this piece. How do we, as methodologists, also grapple with that which is beyond simply viewing and hearing and hovers throughout all that we do? How do we take seriously the unconscious – that thing or process that falls between words and clouds vision? That thing which, at best, slips in at the most inopportune and unexpected places and which often escapes transcribed words and copious field notes. That thing which may help us avoid the pitfalls of reifying narrative and the sadism of expertise.

These contradictions and complications led me to rethink the epistemological underpinnings of the qualitative method and the necessity of problematizing standpoint, interpretative, and postpositivist epistemes, as well as the importance of problematizing the vanguard positionality of sociological supremacy. Finally, these concerns lead me to move into places which displace truth and our attempts at capturing it and move towards representations that foreground polyvocality and the traces that often escape articulation [Derrida 1978].

**FOOTNOTE 1:
I AM I SAY: STANDPOINT EPISTEMOLOGY AND THE POLITICS
OF DISLOCATION**

Standpoint epistemology asks us to theorize from where we are situated in the world [Smith 1990]. Taking seriously how our socio-cultural positions inform not only how we come to know the world, but how the position of an author informs her or his own writing of the world, standpoint renders visible the man behind the curtain, who under the guise of objectivity claims the voice of the all-knowing wizard who only speaks the truth. This form of epistemology creates a much-needed transparency, highlighting the positionality and value-laden aspects of all truth claims. Standpoint shows how most canonical texts are anything but objective and most often represent a white masculine position that has not only occupied a hegemonic position within knowledge construction, but has been naturalized as objective. In doing so, standpoint epistemology foregrounded both the importance of local knowledge and the necessity of reflexivity in writing.

This form of epistemology emerged from critical social movements such as feminism, civil rights, and queer rights, which called for socio-political rights for people who have faced both institutional and interpersonal forms of discrimination and violence [Denzin 1997]. As these movements made their way into the academy, they problematized androcentric, racist and homophobic discourses and called into question both the canons of various disciplines and the ways in which knowledge was validated and produced. As a result, the importance of local knowledges and marginalized voices became a central import for feminist, queer and critical race scholars. Narratives of and

by populations that had been constructed as Other became important political and academic endeavors. This foregrounding of the local and marginal sent fissures through the academy producing new critical forms of methodological and textual practices [e.g. Reinharz 1992; Etter-Lewis and Foster 1996; Behar 1993].

As someone educated in both women's studies and sociology, emphasizing the importance of the voices of the oppressed was an endeavor I sought in my own work. Moreover, it provided a place in the academy where I could see my experiences as a white working-class woman represented and viewed as intellectually and politically important. It was for this reason that, when I proposed my research, "honoring voice" was not only methodologically central to my work, but also a furthering of my feminist politics. However, upon entering the field and working as an exotic dancer/researcher, contradictions between formal interviews and informal talk, between notions of self and practice, between categorical separations in speech and slippages in observations, emerged. I began to question the centrality of narrative as truth – as a static mode, or as something that held everything for us to see; I began wondering about the specters of speech – those things that are beyond articulation and linguistic representation [Derrida 1978]; I began to see how both speech and the written word fell short – both limited by the deferral – both marked by *differance* [Derrida 1978].

The problematic component of standpoint is not its political and/or academic agenda, but rather its belief in the authenticity of narrative. Such authenticity engenders a reification of speech as a form of truth that moves narrative toward a form of stasis and rigidity that falls outside of sociological critique and becomes, in effect, the whole truth of the person who is speaking. It is for this reason that I am calling for an analysis beyond honoring, that places narrative beyond the individual, which acknowledges speech as a discursive function. A move towards the recognition of discourse does not deny positionality; it explores the historical weight of our knowledges, their connection to power, as well as the residuals that always fall beyond articulation. Creating a form of knowledge that decenters the view from nowhere, but does not reify the view from somewhere. A view that emphasizes the free-play of language and does not produce static or rigid meanings. I am seeking a poststructural method and analysis that renders visible how our sense of self and the various truths we hold reside within both dominant and alternative discursive regimes, which carry the weight of history. To this end, I am searching for a form of methodology that recognizes the limitations of language in and of itself.

I would like to say that I feel standpoint is an important political project and that the emphasis on local knowledge is something that we, as people who do work on the social, must still pursue. We must continue to present the voices of those who occupy marginal positions which are often occluded as a result of the symbolic and material vectors of power that circulate within our current cultural and disciplinary regimes. But to stop at that and to view those voices as if separate from the various discourses, which form the subjectivities and narratives of our contemporary regime, is problematic. To reify narrative and language as that which is without gaps, holes, and traces of the unconscious is equally troubling. The need to promote the recognition that all analysis is only a partial analysis, due to language and its holes and because we too are moved by the structures of the unconscious which promote blinders and occlusions, projections and transferences, is of equal import. It is this triple recognition of the par-

tiality and gaps of narrative, language, and interaction that will aid us in more complex ethnographic practice and keep us from the dangers of sociological supremacy and its sadistic underpinnings.

**FOOTNOTE 2:
BEARING ALL: THE SADEAN UNDERPINNINGS
OF METHODOLOGY**

Man undertakes nothing that is not subject to the scrutiny of his reason.
[de Sade 1990 (1790)]

[The] Sadean man denies man, and this negation is achieved through the intermediary notion of God. He temporarily makes himself God, so that there before him men [participants] are reduced to nothing and discover the nothingness of being before God. [Blanchot 1963: 70]

The sociological supremacist – who claims the expertise of an all-knowing perspective – is in fact a Sadean figure in that he makes himself a God who knows his objects better than they know themselves. From this position of sociological supremacy nothing escapes the scrutiny of his reason. He freezes the ambiguity and free play of social phenomena, flattening complexity in an attempt to secure the meaning of his own findings [Derrida 1978]. Plumbing the depth of his subject in order to find the truth, it is his vision that becomes the infallible doctrine of sociological knowledge claims [Clifford and Marcus 1986; Minh-Ha 1991]. His vision dissects his objects, rendering them fully knowable. Like de Sade, whose rational calculus is able to discern the nature of his victims vis-à-vis his perfected methods, the all-knowing objective sociologist is able to lay bare his phenomenon through the validity claims of his measures. His job is to further perfect his methods in order to make them more precise, more reliable, in doing so erasing contingency so that all things may transparent [Minh-Ha 1991].

This is both a pleasurable and dangerous venture. It is pleasurable because there is an aspect of pleasure in coming to know the other, coming to consume it and making clear those things that provoke the anxiety and paranoia of unknowability [hooks 1992]. It is dangerous because it denies complexity and thus experiential integrity to those we come to study. This is not to say that sadism is always a conscious and/or rational pull, it is closer to the Hegelian dialectic wherein the master is never fully aware of how much he seeks the recognition of the slave in order to perpetuate his own position [Hegel 1977]. Moreover, as Hegel posits, violence is never rational – quite the opposite – it is the irrationality of violence at work in the constitution of consciousness [Hegel 1977]. The question then becomes, how does this modality of violence become transformed in the epistemology of positivism that I would argue is in fact a sadistic epistemology? It is embodied in both the disembodied position of the researcher as well as in its textual practices – in the practices of writing which violently foreclose the gaps of language and freeze the space of the researcher as all knower and in a God-like position. It is also embodied in the denial of the ways in which both sociologists and the people they work with are written on by culture as well as write it. More insidiously, the sadistic under-

pinning of positivism manifests itself in the false ideology of the autonomous rational researcher who is immune to unconscious drives and has the ability to see and rationally decipher all. Is there not something more compassionate about the sociologist? Of course, much of sociology was formed under the commitment of knowledge for more than just knowledge's sake. However, the position of the realist expert who stands apart from her or his subjects is grounded in a Sadean epistemology.

Originally, my argument focused on the classical positivist quantitative researcher, who flattens the experiences of his or her subjects into quantifiable code, rendering their experiences valid or invalid according to the confirmations or disconfirmations of null hypotheses. Then I remembered the various communities of color, reservations and poor communities that have stopped granting access to ethnographic researchers due to the mis-interpretations and occlusions represented in realist texts [for a broader discussion of this, please see Clifford and Marcus 1986; Rosaldo 1989; Minh-Ha 1991]. These expert texts take on the quality of a biological or corporate patent whose copy-righted quality deters others from studying the same experiences under the guise of well-established literature [Clifford 1986]. However, feminist ethnographers, critical poststructural anthropologists and sociologists have begun to grapple with the problematics of realist writing and to experiment with texts that are more self-referential and strategically play with the fictive elements of the narratives of participants and researchers. This is embodied in Stephen Pfohl's allegorical ethnography *Death at the Parasite Café* [1992] and Kathleen Stewart's deconstructive ethnography *A Space on the Other Side of the Road* [1996], to name but a few. I too work in this genre in my ethnography on dancers and their regular customers.

I want to further this exploration by addressing the more insidious aspect of a Sadean epistemology, namely, the problematics of autonomy and rationality – the lack of recognition of the unconscious and the unconscious projections that take place in the research project. It is this concern which dovetails with my previous footnote addressing the problems of reifying or deeming narratives as something authentic. In trying to grapple with these, I am moving into the deepest part of the gap – how do we, as people who are researching the social, and the various personal embodiments found in the social, represent experience in such a manner that it does not reify personal accounts or fall into the sadism of expertise?

FOOTNOTE 3: REPRESENTATION, PARTIALITY AND READING OR MISREADING BETWEEN THE LINES

The unconscious is structured like a language. [Lacan 1964]

In Lacanian psychoanalysis the unconscious is viewed in linguistic terms because we can only grasp its presence when it makes itself known in language [Lacan 1964]. “The unconscious is the discourse of the Other” ... meaning the unconscious is the effects of the signifier on the subject and thus it is the signifier that is repressed and reemerges in the formation of the unconscious vis-à-vis jokes, symptoms, parapraxis, and dreams [Evans 1996: 176]. Due to its linguistic connection, the unconscious in La-

canian psychoanalysis is not an interior formation; rather, it is trans-individual. Therefore, if we begin to take seriously a Lacanian vision of the unconscious as that which emerges through traces in language and as slips, we can then produce methodological practices that recognize the gaps, which emerge vis-à-vis the structures of language in and of itself.

Language for Lacan is always already plagued by absence as it is severed from the real and locked within the symbolic order (which he defines as our dominant system of language) marked by fantasy and interrupted by the unconscious. The unconscious leaks in when you least expect it and makes trouble for the seemingly given aspects of language. It illuminates the fragility of the language as that which guarantees authentic experience. By doing so, the subject who is supposed to know is always to some extent left occupying a tenuous space. How then can we challenge authenticity by acknowledging a subject who cannot possibly know everything? Authenticity or truth claims become suspicious in this category because the borders of narratives and language itself can never be secured. As such, even the most copious note taker and even the most precise transcription is always found lacking.

The unconscious operates as an unknown knowledge and moves within and between the narratives of the people with whom we work as well as within our own observational and textual practices [Lacan 1977]. I contend that we need to take the trans-individual notion of the unconscious seriously. In so doing, we can acknowledge the slips that fall within and between the lines displacing both the authority of the sociological author as well as the authenticity of narrative. All sociological knowledge and the qualitative methods we employ in that search for knowledge are in effect marked by gaps, slips and the unconscious. Knowledge production should be seen as, occupying a site of in-betweenness, haunted by that which we cannot know about ourselves; what evades us in the narratives of the people with whom we work and the ambiguity that marks the space of intersubjectivity. In other words, we are locked within the prison house of language.

What does this mean for our representations? Where do we go from here? How do we begin to explore for traces of the unconscious? Again I suppose we are returning to the gap. It is this that I am interested in exploring in the future. How can we be comfortable in our partiality and take seriously both the slips and gaps of narrative, as well as the partiality of our own visions? It is by taking psychoanalysis and the unconscious seriously that I believe we can come to new representations of ethnographic texts as well as new ethnographic practices. In so doing, we can seriously and critically engage and deconstruct the narratives of the women and men with whom we work and reflexively acknowledge our own visions as filtered and enhanced by our own psychoanalytic drives. Instead of honoring our expertise, we can move toward embodied engagement and follow what is said and what is not, what we say and what we do not and begin touching the in-between. Moreover, we can acknowledge the limits of narrative as that which is comprised of riddles and can never be tied down or made static. In doing so, we can move towards a qualitative methodology that acknowledges the limits of representation as that which can never promise or guarantee its own meaning and which resides in an ambiguity marked by the traces of the unconscious.

Does this mean that we do away with our project of “giving voice” or throw away the project of qualitative research because we cannot secure the authenticity of our

claims? I argue that we need to acknowledge the gap and illuminate its presence without doing away with systematicity – while simultaneously acknowledging the imperfections and partiality of knowledge.

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MANIFEST DER OBJEKTIV HERMENEUTISCHEN SOZIALFORSCHUNG

ULRICH OEVERMANN

DIE METHODOLOGIE DER OBJEKTIVEN HERMENEUTIK ALS PARADIGMA¹

Die Methodologie der objektiven Hermeneutik stellt in meinem Forschungsschwerpunkt seit langem erprobte Methoden und Techniken der Sozial- und Kulturforschung bereit, die sich vor allem dazu eignen, auf wenig erforschten Gebieten und bei neuen, noch wenig bekannten Entwicklungen und Phänomenen, die typischen, charakteristischen Strukturen dieser Erscheinungen zu entschlüsseln und die hinter den Erscheinungen operierenden Gesetzmäßigkeiten ans Licht zu bringen. Es handelt sich um eine Methodologie, die bewußt und strategisch darauf aus ist, die Ebene der bloßen Deskriptivität, die im 19. Jahrhundert methodisch die Erfahrungswissenschaften von der sinnstrukturierten Welt bestimmt hat, zu verlassen und zu überwinden zugunsten einer erschließenden und aufschließenden Gegenstandsanalyse.

SCHLÜSSELBEGRIFFE UND -THESEN DER OBJEKTIVEN HERMENEUTIK

1. Latente Sinnstrukturen und objektive Bedeutungsstrukturen statt subjektiver Dispositionen

Zentraler Gegenstand der Methodologie der objektiven Hermeneutik sind die latenten Sinnstrukturen und objektiven Bedeutungsstrukturen von *Ausdrucksgestalten*, in denen sich uns als Erfahrungswissenschaftlern von der sinnstrukturierten Welt die psychischen, sozialen und kulturellen Erscheinungen einzig präsentieren, und in denen wir als Lebenspraxis uns selbst verkörpern sowie die uns gegenüberliegende Erfahrungswelt repräsentieren. Konstitutionstheoretisch steht hinter dieser Bestimmung die Prämisse, daß die kategorial von den stochastischen Welten verschiedenen Bedeutungswelten uns als verstehbare dadurch gegeben sind, daß die Bedeutung von Ausdrücken grundsätzlich sprachlich durch generative Algorithmen erzeugt werden. Damit soll ge-

¹ Es handelt sich um die gründliche Überarbeitung und Ergänzung eines Textes, der ursprünglich im März 1996 unter dem Titel „Konzeptualisierung von Anwendungsmöglichkeiten und praktischen Arbeitsfeldern der objektiven Hermeneutik (Manifest der objektiv-hermeneutischen Sozialforschung)“ verfaßt wurde und als download im Internet verfügbar war und noch ist unter der Adresse <http://www.objektivehermeneutik.de>. (März 2002)

sagt sein, daß die sprachlich erzeugten objektiven Bedeutungen den subjektiven Intentionen konstitutionslogisch vorausliegen und nicht umgekehrt der je subjektiv gemeinte bzw. intendierte Sinn die objektive Bedeutung von Ausdrücken erzeugt. Allerdings ist damit keineswegs behauptet, daß nicht die je subjektiven Intentionen als Äußerungen des Lebens ihrerseits als auf anderes nicht reduzierbare eigenlogische Wirklichkeiten dessen zu gelten haben, was man konstitutionstheoretisch in unseren Wissenschaften am präzisesten mit dem Grundbegriff von Lebenspraxis umschreibt, und insofern der Generierung von Ausdrücken und Ausdrucksgestalten ihrerseits dynamisch vorausgehen. Es soll nur in Rechnung gestellt sein, daß methodologisch ein überprüfbarer und erschließbarer Zugriff auf diese eigenlogische Wirklichkeit von Intentionen bzw. intentionalen Gehalten, generell: der innerpsychischen Wirklichkeit von Empfindungen, Affekten, Vorstellungen, Kognitionen und Volitionen, nur vermittelt über Ausdrucksgestalten möglich ist, in denen sie sich verkörpern, ebenso wie in der Unmittelbarkeit der Vollzüge der Praxis selbst sowohl die dialogische Vermittlung wie die selbstreflexive Vergegenwärtigung solcher innerpsychischer Wirklichkeiten auf jene Verkörperung in Ausdrucksgestalten notwendig angewiesen ist. Intentionale Gehalte, generell: innerpsychische Wirklichkeit zum Gegenstand wissenschaftlich-methodisierter Erkenntnis zu machen, setzt deren methodisch greifbare Verkörperung in Ausdrucksgestalten voraus. Einen unmittelbaren Zugriff auf sie haben wir nicht einmal in der Introspektion, ganz abgesehen davon, daß die Introspektion methodisch gesehen eine höchst problematische Quelle von Protokollen innerpsychischer Wirklichkeit ist. – Latente Sinnstrukturen und objektive Bedeutungsstrukturen sind also jene abstrakten, d.h. selbst sinnlich nicht wahrnehmbaren Konfigurationen und Zusammenhänge, die wir alle mehr oder weniger gut und genau „verstehen“ und „lesen“, wenn wir uns verständigen, Texte lesen, Bilder und Handlungsabläufe sehen, Ton- und Klangsequenzen hören und alle denkbaren Begleitumstände menschlicher Praxis wahrnehmen, die in ihrem objektiven Sinn durch bedeutungsgenerierende Regeln erzeugt werden und unabhängig von unserer je subjektiven Interpretation objektiv gelten. Die objektive Hermeneutik ist ein Verfahren, diese objektiv geltenden Sinnstrukturen intersubjektiv überprüfbar je konkret an der lesbaren Ausdrucksgestalt zu entziffern, die ausdrucksmaterial als Protokoll ihrerseits hör-, fühl-, riech-, schmeck- oder sichtbar ist.

Es ist also ein Verfahren, das sich auf die „verstehbaren“ Gegenstandsbereiche der Sozial-, Geistes- und Kulturwissenschaften nicht dadurch richtet, daß es, wie alle sonstigen Methoden dieser Wissenschaften, primär deren subjektiven Niederschlag oder subjektive Repräsentanz im Bewußtsein der Handelnden nachvollzieht oder zu erschließen versucht. Das wäre grundsätzlich mit Unsicherheiten behaftet und ein Verfahren, das selbst noch der zu untersuchenden Praxis des Verstehens angehört. Viel mehr macht die objektive Hermeneutik ernst mit den Konsequenzen der grundlegenden Erkenntnis, daß jede subjektive Disposition, d.h. jedes psychische Motiv, jede Erwartung, jede Meinung, Haltung, Wertorientierung, jede Vorstellung, Hoffnung, Fantasie und jeder Wunsch methodisch überprüfbar nie direkt greifbar sind, sondern immer nur vermittelt einer Ausdrucksgestalt oder einer Spur, in der sie sich verkörpern oder die sie hinterlassen haben. Zutreffend entschlüsseln läßt sich daher eine solche Disposition erst, wenn man zuvor die objektive Bedeutung jener Ausdrucksgestalt entziffert hat. Erst dann kann man zur begründeten Erschließung der Struktur der subjektiven Disposition selbst übergehen. Die übrigen Methoden der Forschung leiden darunter, daß sie

entweder diesen Schritt der Vermittlung über die objektiven Bedeutungs- und Sinnstrukturen einer Ausdrucksgestalt überschlagen und auslassen oder von vornherein die Ebenen von Sinn- und Bedeutungshaftigkeit menschlichen Handelns ganz ausblenden und sich reduktionistisch auf die Beobachtbarkeit äußeren Verhaltens beschränken.

2. Zwei verschiedene Empirie-Begriffe: Die Lesbarkeit von Sinnstrukturen und die Wahrnehmbarkeit von stochastischen Welten

Eine angemessene Methodologie der Sozial-, Kultur- und Geisteswissenschaften muß mit der alten Anschauung brechen, derzufolge die Gegenstände der Erfahrungswissenschaften an die sinnliche Wahrnehmbarkeit gebunden und insofern konkret seien. Sinn- und Bedeutungsstrukturen sind grundsätzlich abstrakt. Sie lassen sich als solche sinnlich nicht wahrnehmen, aber sie sind dennoch empirisch und als empirische erfahrungswissenschaftlich analysierbar. Sinnlich wahrnehmen läßt sich an den Sinngebilden bzw. den Ausdrucksgestalten immer nur der ausdrucksmateriale Träger, in dem sie faktisch protokolliert sind – also die Weiße des Papiers eines bedruckten Textes und die Farbe und Form der typographischen Zeichen; der auf einem Oszillographen abbildbare Klang der mündlichen Rede, die plastische Textur eines gestalteten Gegenstandes, usf., aber was da wahrgenommen wird, ist nicht selbst die Bedeutung oder der Sinn der Ausdrucksgestalt, sondern nur deren materiales Substrat. Weil Bedeutung und Sinn selbst nicht wahrnehmbar sind, sie aber gleichzeitig genau das konstituieren, was die Lebenspraxis des Menschen, sein Handeln und dessen Objektivationen als Erfahrungsgegenstand kategorial ausmacht und von der Naturdinglichkeit menschlicher Erscheinungen systematisch unterscheidet, müssen wir mit dem auf David Hume zurückgehenden Begriff von Empirie brechen, für den empirisch nur das ist, was durch die Wahrnehmungsinne in den erkennenden Geist gelangt („Nihil est in intellectu, quod non fuerit in sensu“), und alles, was dieses Kriterium nicht erfüllt, metaphysisch, und damit außerhalb der Reichweite der Erfahrungswissenschaften liegt. Deshalb überschreitet die objektive Hermeneutik die mit dem Hume'schen Empiriebegriff gekoppelte implizite dogmatische Ontologisierung von Realität und erfahrbarer Welt und folgt einem methodologischen Realismus, indem sie als empirisch alles das ansieht, was sich durch Methoden der Geltungsüberprüfung in der Gegenständlichkeit erfahrbarer Welt nachweisen läßt. Das trifft auf die objektiven Sinn- und Bedeutungsstrukturen von Ausdrucksgestalten fraglos zu.

3. Ausdrucksgestalt, Text und Protokoll statt Meßdaten

Das Gesamt an Daten, in denen sich die erfahrbare Welt der Sozial-, Geistes- und Kulturwissenschaften präsentiert und streng methodisch – im Unterschied zu: praktisch – zugänglich wird, in denen also die sinnstrukturierte menschliche Praxis in allen ihren Ausprägungen erforschbar wird, fällt in die *Kategorie der Ausdrucksgestalt*.

Unter dem Gesichtspunkt der Strukturierung von Sinn und Bedeutung, also dessen, was sie symbolisieren, werden Ausdrucksgestalten als *Texte* behandelt. Für Texte

gilt entsprechend, daß sie – wie die Bedeutungs- und Sinnstrukturen, deren Zusammenhang sie herstellen – als solche der sinnlichen Wahrnehmung verschlossen sind und nur „gelesen“ werden können. Unter diesen methodologisch erweiterten Textbegriff fallen selbstverständlich nicht nur die schriftsprachlichen Texte der Literaturwissenschaften, sondern alle Ausdrucksgestalten menschlicher Praxis bis hin zu Landschaften, Erinnerungen und Dingen der materialen Alltagskultur.

Unter dem Gesichtspunkt ihrer ausdrucksmaterialien, überdauernden Objektivierung werden diese Texte als *Protokolle* behandelt. Dabei kann es sich um gegenständliche Objektivierungen in Produkten, um hinterlassene Spuren, um Aufzeichnungen vermittels technischer Vorrichtungen, um intendierte Beschreibungen, um institutionelle Protokolle oder um künstlerische oder sonstige bewußte Gestaltungen handeln, und die Ausdrucksmaterialität kann sprachlich oder in irgendeinem anderen Medium der Spurenfixierung oder der Gestaltung vorliegen. Protokolle, als die ausdrucksmateriale Seite von Ausdrucksgestalten, lassen sich selbstverständlich sinnlich wahrnehmen.

Die Grundbegriffe von Text und Protokoll beziehen sich also unter je verschiedenen Gesichtspunkten auf den je identischen Gegenstand von Ausdrucksgestalt: Text meint deren symbolischen Charakter und Protokoll deren ausdrucksmateriale Erscheinung. Damit hängt methodisch ein wichtiger Umstand zusammen: Unter dem Gesichtspunkt des Textes sind Ausdrucksgestalten ebenso wie die objektiven Sinnstrukturen grundsätzlich der Zeitlichkeit und Räumlichkeit von Praxis enthoben, als Abstrakta sind sie gewissermaßen zeit- und raumlos in dem Sinne, daß ihre Realität auch dann nicht verschwunden oder gelöscht ist, wenn sie nicht von einem produzierenden oder rezipierenden Bewußtsein aktual, d.h. an einer bestimmten Raum-Zeit-Stelle, gelesen und subjektiv intentional realisiert werden. Unter dem Gesichtspunkt der Ausdrucksmaterialität des Protokolls allerdings sind sie raum-zeitlich gebunden wie jede sinnlich wahrnehmbare Wirklichkeit. Wenn an keiner Raum-Zeit-Stelle von einer Ausdrucksgestalt mehr ein einsehbares Protokoll als deren ausdrucksmateriale Seite vorliegt, dann sind auch alle ihr entsprechenden latenten bzw. objektiven Sinnstrukturen des Textes trotz ihrer grundsätzlichen Zeit- und Raumlosigkeit gelöscht. Das unterscheidet sie entschieden von bloßen Ideen im Sinne der platonischen Philosophie. Das gilt auch noch und gerade für den Grenzfall eines Protokolls, wie es in der Erinnerung vorliegt. Unter dem Gesichtspunkt der Textförmigkeit ist die Erinnerung ja gerade ein Vorgang, durch den die Flüchtigkeit der in der Unmittelbarkeit eines Hier und Jetzt vollzogenen Handlung oder Wahrnehmung, z.B. einer Beobachtung, prinzipiell unbegrenzt häufig vergegenwärtigt und damit auf Dauer gestellt werden kann. Darin realisiert sich die Zeitlosigkeit der objektiven Sinnstrukturen von Ausdrucksgestalten. Unter dem Gesichtspunkt des Protokolls allerdings, unter dem die Erinnerung ja grenzfällig die isolierte Beobachtung des einsamen Subjekts in den Status der intersubjektiven Überprüfbarkeit hebt, d.h. wiederholbar macht, ist sie, die Erinnerung, entweder, sofern sie aufgezeichnet worden ist, eine objektivierbare konkrete Ausdrucksgestalt, die als Protokoll an einer bestimmten Raum-Zeitstelle identifizierbar ist, oder ein raum-zeitlich indiziertes konkretes Ereignis der Wiederholung der Vergegenwärtigung eines Gedächtnisinhaltes, die als solche grundsätzlich protokollierbar wäre.

Daß letztlich Protokolle die einzige methodisch zureichende Grundlage für zwingende Schlußfolgerungen in der erfahrungswissenschaftlichen Erforschung der sinnstrukturierten Welt abgeben, läßt sich allein schon an folgendem bemessen: Wollten wir

ein vorliegendes Protokoll eines wirklichen Vorgangs als unvollständig oder verzerrt kritisieren, so müßten wir dazu wiederum ein anderes, plausiblerweise günstigeres oder zumindest in seiner Selektivität andersartiges Protokoll zu Rate ziehen. Niemals stünde uns der protokollierte Vorgang selbst zum kritischen Abgleich zur Verfügung. Denn er ist einmalig an eine unwiederbringliche Raum-Zeit-Stelle des Sich-Ereignens gebunden und außerhalb des flüchtigen Hier und Jetzt des unmittelbaren Erlebens und der unmittelbaren Erfahrung als solcher endgültig vergangen und nicht mehr greifbar, es sei denn in hinterlassenen Protokollen. Auch unsere *Erinnerung* an diesen Vorgang ist ein solches Protokoll und unser *Gedächtnis* taugt als Abgleichfolie erst dann, wenn wir es in eine prinzipiell erzählbare Erinnerung umgewandelt haben.

Das führt im übrigen zu dem nicht nur für die Erfahrungswissenschaften von der sinnstrukturierten Welt, sondern für alle Erfahrungswissenschaften gleichermaßen zentralen methodologischen Argument, demzufolge die Beobachtungen, ganz im Gegensatz zu den landläufigen Auffassungen, wie sie vor allem neuerdings in der Systemtheorie kultiviert werden, als solche methodologisch vollkommen unerheblich sind. Methodologisch ist einzig und allein von Belang, welche Protokolle die Beobachtungen, die als solche kognitive Operationen in der unmittelbaren Lebenspraxis selbst sind und deshalb als flüchtige Vollzüge an das „Hier und Jetzt“ der Handlungsgegenwärtigkeit gebunden bleiben, hinterlassen haben. Deshalb ist auch die Charakterisierung des Unterschiedes zwischen den Naturwissenschaften und den Geisteswissenschaften nach dem Kriterium, die ersteren hätten es mit Beobachtungstatsachen zu tun und die letzteren mit interpretativen oder normativen Tatbeständen, schon immer irreführend gewesen. Für beide Wissenschaftsabteilungen gilt gleichermaßen, daß sie ihre theoretischen Erklärungen und Deutungen mit den Erfahrungstatsachen zu konfrontieren haben, die ihnen einzig und allein in Form von Protokollen von Ereignissen und Prozessen gegeben sind. Der Hinweis auf die Dignität einer Beobachtung durch ein isoliertes Erkenntnissubjekt, die nicht zugleich protokolliert worden ist oder durch Zeugen nicht bestätigt werden kann, ändert daran nichts. Denn eine solche Beobachtung kann methodisch relevant auch nur ins Spiel gebracht werden durch die Erzählung einer Erinnerung an sie und das ist dann ein Protokoll, wenn auch häufig ein wenig verlässliches.

Protokolle nun haben die grundlegende, methodisch immer in Rechnung zu stellende Eigenschaft, daß sie nicht nur die methodisch-wissenschaftlich unübersteigbare Grenze zur protokollierten Wirklichkeit selbst markieren und damit die Notwendigkeit, sich methodologisch auf die Analyse von Protokollen bzw. Texten oder Ausdrucksgestalten explizit zu konzentrieren, was in den übrigen Methoden nicht zureichend reflektiert wird, sondern darüber hinaus sowohl die protokollierte Wirklichkeit als auch die protokollierende Handlung oder den protokollierenden Vorgang selbstreferentiell zu protokollieren, so daß beide Schichten in der Analyse sich voneinander lösen lassen, um die protokollierte Wirklichkeit unvermischt herauszupräparieren.

Mit Hilfe dieser Kategorien von Ausdrucksgestalt, Text und Protokoll läßt sich eine vollständig veränderte, die gegenstandsabhängigen Unterschiede zwischen den Einzeldisziplinen in den Psycho-, Sozial-, Geistes- und Kulturwissenschaften auf ganz neue Weise vereinheitlichende Methodologie der Datenerhebung und Datenauswertung einrichten, im Vergleich zu der die bisher bekannten und gebräuchlichen Methoden und Techniken als vorläufig und zudem dem Gegenstand, den sie analysieren sollen, selbst paraphrastisch noch viel zu sehr verhaftet sich erweisen.

4. Objektivität statt Subjektivität

Indem die objektive Hermeneutik sich, unabhängig davon, welchen konkreten Gegenstand sie zu analysieren hat, immer primär auf die Rekonstruktion der latenten Sinnstrukturen bzw. objektiven Bedeutungsstrukturen derjenigen Ausdrucksgestalten richtet, in denen sich der zu untersuchende Gegenstand oder die zu untersuchende Fraglichkeit authentisch verkörpert, kann sie in demselben Maße Objektivität ihrer Erkenntnis bzw. ihrer Geltungsüberprüfung beanspruchen wie wir das selbstverständlich von den Naturwissenschaften gewöhnt sind. Dies einfach deshalb, weil jene zu rekonstruierenden Sinnstrukturen durch prinzipiell angebbare Regeln und Prozeduren algorithmischer Natur präzise überprüfbar und lückenlos am jederzeit wieder einsehbaren Protokoll erschlossen werden können. Diese Objektivität wird erst dann gelockert, wenn es in weiteren Schritten darum geht, von den objektiven Sinn- und Bedeutungsstrukturen auf die Bewußtseinsrealität oder innere psychische Realität der an der protokollierten Wirklichkeit beteiligten Subjekte zurückzuschließen. Aber diese Rückschlüsse sind auf der Grundlage der vorausgehenden Erschließung der objektiven Sinnstrukturen sehr viel zuverlässiger und sicherer durchzuführen als ohne deren Einschaltung. Anders ausgedrückt: Es ist kein sichereres Verfahren der Erschließung subjektiver Dispositionen vorstellbar, wenn man nicht von vornherein Magie und intuitive Wahrnehmungsleistungen der Praxis selbst sowie unaufschlußreiche Inhaltsangabe zum Bestandteil einer wissenschaftlichen Methode machen und damit die Wissenschaft ihrer methodenkritischen Funktion berauben will.

Der Unterschied zu den Naturwissenschaften besteht einzig darin, daß nicht prinzipiell durch die Sinneskanäle wahrnehmbare, der stochastisch verfaßten Welt zugehörige Ereignisse, sondern sinnstrukturierte, prinzipiell sinnlich nicht wahrnehmbare, also abstrakte Gegenstände, nämlich Bedeutungs- oder Sinnwelten, untersucht werden. Im Unterschied aber zu den übrigen Methoden im humanwissenschaftlichen Bereich wird dieser Zugang zu den abstrakten Bedeutungswelten nicht um den Preis der Subjektivierung bzw. um den Preis der Zurechnung der Erfahrungssphäre, die sich nicht mit der Kategorie von Materie, Körper oder Leib deckt, zur Kategorie von Subjektivität erkaufte wird, sondern der objektiven Erkenntnis erhalten bleibt. Mit anderen Worten: Die objektive Hermeneutik ist nicht eine Methode des Verstehens im Sinne eines Nachvollzugs subjektiver Dispositionen oder der Übernahme von subjektiven Perspektiven des Untersuchungsgegenstandes, erst recht nicht eine Methode des Sich-Einfühlens, sondern eine strikt analytische, in sich objektive Methode der lückenlosen Erschließung und Rekonstruktion von objektiven Sinn- und Bedeutungsstrukturen.

Wenn nun auf diese methodisch gesicherte Objektivität sowohl des Gegenstandes als auch der auf ihn bezogenen Erkenntnis so großen Wert gelegt wird, so hat das ausschließlich eine methodologische Bedeutung. Keinesfalls ist darunter zu verstehen, daß Phänomene der Subjektivität und subjektive Befindlichkeiten nicht untersucht werden könnten oder sollten. Im Gegenteil: Eine wissenschaftlich präzise Erforschung von Phänomenen der Subjektivität, die mehr sein soll als bloße unaufschlußreiche Wiederholung von Alltagsüberzeugungen und -eindrücken nur in anderen Worten, wird erst durch diese methodologischen Klärungen ermöglicht. Wenn nämlich die für das praktische Handeln notwendige Verstehensform der Rollen- und Perspektivenübernahme und der Einfühlung und Empathie zum wissenschaftlich-methodischen *Verfahren* er-

hoben wird statt zum *Gegenstand* der Humanwissenschaft, dann gelangt die wissenschaftliche Analyse über eine bloße verbale Neueinkleidung alltagspraktischen Wissens nicht hinaus, bleibt also Pseudo-Wissenschaft.

5. Sequenzanalyse statt Klassifikation

Das Analyseverfahren, durch das sich die objektive Hermeneutik von allen anderen Methodenansätzen radikal unterscheidet, ist die Sequenzanalyse. Sie lehnt sich an die Sequentialität an, die für humanes Handeln konstitutiv ist. Dabei wird unter Sequentialität nicht ein triviales zeitliches oder räumliches Nacheinander bzw. Hintereinander verstanden, sondern die mit jeder Einzelhandlung als Sequenzstelle sich von neuem vollziehende, durch Erzeugungsregeln generierte *Schließung vorausgehend eröffneter Möglichkeiten* und *Öffnung neuer Optionen in eine offene Zukunft*. Insofern jedes Protokoll ausschnittshaft je konkrete Lebenspraxen authentisch ausdrückt, die sich darin verkörpern, bildet es auch den realen sequentiellen Prozeß ab, in dem sich diese konkreten Lebenspraxen, die an einer protokollierten Wirklichkeit handelnd beteiligt sind, ganz konkret in eine offene Zukunft entfalten.

Die Sequenzanalyse macht darauf aufmerksam, daß jede konkrete Praxis im menschlichen Leben eröffnet und beschlossen werden muß, damit verbindlich und strukturiert gehandelt werden kann. Deshalb ist es immer aufschlußreich, die *Eröffnungs- und Beschließungsprozeduren* eigens zu untersuchen. Ein typischer, elementarer Fall von Eröffnung und Beschließung ist die Begrüßung. Sie zeigt uns exemplarisch die Einrichtung einer reziproken Verbindlichkeit in der Kooperation, ohne die Praxis sich nicht vollziehen kann. Zugleich gibt sie zu erkennen, daß erst durch Eröffnungen eine sequentiell mit Problemlösungen, Geschäften und Geselligkeiten füllbare *Praxis – Raum-Zeitlichkeit*² hergestellt wird. Aber diese Praxis – Raum-Zeitlichkeit *muß* nicht gefüllt werden. Wir können einander begrüßen, ohne weiter miteinander zu kooperieren. Dann brauchen wir auch die eröffnete Praxis nicht eigens durch Verabschiedung zu beschließen. In einer solchen Begrüßung fallen Eröffnung und Beschließung ineinander. Die Praxis – Raum-Zeitlichkeit bleibt gewissermaßen leer. Daraus ist ersichtlich, daß Eröffnung und Beschließung, zwei herausgehobene Stellen der spezifisch sozialen Sequentialität, konstitutionslogisch der sachhaltigen Praxis vorausgehen haben.

Diesen herausgehobenen Sequenzstellen ist eigentümlich, daß sie immer beides bedeuten: Eröffnung und Beschließung zugleich, wenn auch jeweils eine dieser beiden Markierungsfunktionen dominant ist. Denn die z.B. durch eine Begrüßung vollzogene Eröffnung ist immer zugleich auch die Beschließung einer vorausgehenden Praxis – Raum-Zeitlichkeit und die Beschließung einer Praxis markiert zugleich auch die Eröffnung einer nachfolgenden Praxis – Raum-Zeitlichkeit.

² Hier ergibt sich eine kleine terminologische Schwierigkeit. Gemeint ist wörtlich eine Praxis-Räumlichkeit und Praxis-Zeitlichkeit als einheitliches Gebilde, weil Praxis-Raum und Praxis-Zeit über den allgemeineren Begriff der Sequentialität ineinander übersetzbar sind. Aber der Terminus Praxis-Räumlich- und Zeitlichkeit wäre eine Ungetüm und der Terminus Praxis-Raum-Zeit wäre mißverständlich. Ich ziehe es vor, die Raum-Zeit-Verbindung zu erhalten. Man könnte an die beiden Schreibweisen Praxis-Raum-Zeitlichkeit oder Praxis-Raum-Zeitlichkeit denken. Ich ziehe letztere vor: Die Bindestriche sollen unterschiedliche Ebenen der Verbindung markieren.

Diese für die soziale Sequentialitätsstruktur elementare und kennzeichnende Gleichzeitigkeit von Eröffnung und Beschließung kommt nun aber nicht nur den herausgehobenen, in ihrer Primärbedeutung explizit eröffnenden oder beschließenden Sequenzstellen zu, sondern auch jeder einzelnen Sequenzstelle in einem je schon eröffneten Praxis-Raum. An jeder Sequenzstelle werden bis dahin noch offene Möglichkeiten geschlossen und neue Möglichkeiten eröffnet.

Am einfachen Beispiel der Begrüßung kann nun ein wesentlicher Grundzug der Sequenzanalyse auf einfache Weise veranschaulicht werden: Es wird darin grundlegend zwischen *zwei ganz verschiedenen Parametern in der Determination von Sequenzen unterschieden*. Ein *erster Parameter von Erzeugungsregeln* besteht aus dem Gesamt an Sequenzierungsregeln, durch die an einer je gegebenen Sequenzstelle die sinnlogisch möglichen Anschlüsse erzeugt werden und auch die je möglichen sinnlogisch kompatiblen vorausgehenden Handlungen festgelegt sind und entsprechend erschlossen werden können. Diesen Parameter muß man sich vorstellen als eine *Menge von algorithmischen Erzeugungsregeln sehr unterschiedlichen Typs*. Dazu gehören z.B. ganz elementar die Regeln der sprachlichen Syntax, aber auch die pragmatischen Regeln des Sprechhandelns und die logischen Regeln für formale und für material-sachhaltige Schlüssigkeit. Dieses Gesamt an Sequenzierungsregeln erzeugt an jeder Sequenzstelle je von Neuem einen Spielraum von Optionen und Möglichkeiten, aus denen dann die in diesem Praxis-Raum anwesenden Handlungsinstanzen per Entscheidung eine Möglichkeit auswählen müssen. Welche Auswahl konkret getroffen wird, darüber entscheidet ein *zweiter Parameter von Auswahlprinzipien und -faktoren*, der alle Komponenten und Elemente der Disponiertheit der verschiedenen beteiligten Lebenspraxen oder Handlungsinstanzen umfaßt. Das *Gesamt der Dispositionen einer je konkreten Lebenspraxis* macht deren Eigenart oder deren Charakter, sequenzanalytisch ausgedrückt: deren Fallstruktur, aus.

Beide Parameter müssen analytisch klar unterschieden, aber gleichermaßen berücksichtigt werden. Mit dieser Unterscheidung hebt sich die Sequenzanalyse der objektiven Hermeneutik deutlich von anderen Analyseverfahren der Sozialwissenschaften ab, in denen, wenn die Sequentialität überhaupt berücksichtigt wird, die beiden Parameter im Begriff der Erwartung und der Erwartungs-Erwartung, also zu unserem zweiten Parameter, zusammenfließen, so daß eine von der subjektiven Perspektive der beteiligten Handelnden abstrahierende, objektivierende und erschließende Analyse der Sequentialität nicht mehr möglich ist.

Die analytische Differenzierung der beiden Parameter der Sequenzanalyse enthält auch eine Klärung der *Probleme der Interdisziplinarität* im Verhältnis zwischen der fachspezifischen Forschung im Ensemble der Sozial-, Kultur- und Geisteswissenschaften. Der Parameter I der Erzeugungsregeln läßt sich ohnehin nicht einer bestimmten Disziplin zurechnen. Er gehört zum gemeinsamen konstitutionstheoretischen und methodologischen Dach aller Erfahrungswissenschaften von der sinnstrukturierten Welt gleichermaßen, wengleich die theoretische Erhellung dieser Regeln und ihres Status in der jüngsten Vergangenheit vor allem in der Linguistik und der Kognitionswissenschaft erfolgte. Aber es wäre von vornherein unsinnig, sie gegenständlich mit Präferenz einer bestimmten Disziplin zuzurechnen. Disziplinspezifische Differenzen in der Thematisierung von Gegenständen und Gegenstandsaspekten sowie in der konkreten Methodik und Forschungspraxis treffen nur für den Parameter II der Auswahlprinzipien

und -faktoren zu. Aber gerade hier führt der sequenzanalytische Ansatz der objektiven Hermeneutik zu einer erheblichen Vereinfachung der Problemlage. Denn es würde in ihr wenig Sinn mehr machen, die unterschiedliche Zugehörigkeit von Variablen, Merkmalen, Eigenschaften und Gegenstandsaspekten zu den verschiedenen Disziplinen aufrecht zu erhalten und die damit verbundenen unterschiedlichen Betrachtungsweisen weiter zu trennen. Entscheidend ist nämlich für das sequenzanalytische Vorgehen, diese verschiedenen Aspekte, Merkmale und Eigenschaften unter dem Gesichtspunkt ihres je fallspezifischen Zusammenwirkens in der Totalität einer Fallstrukturgesetzlichkeit als eine Einheit zu sehen. Entsprechend machte es wenig Sinn, die je disziplinspezifischen Hypothesen und Verallgemeinerungen subsumtionslogisch jeweils zur Anwendung zu bringen und den konkreten Fall darin jeweils abzubilden. Vielmehr besteht eine Fallstruktur und die sie erklärende Gesetzlichkeit ja gerade darin, daß die von den verschiedenen Disziplinen je unterschiedlich in den Blick genommenen und thematisierten Gegenstandsaspekte in ihr zu einer realen Einheit von Strukturproduktion und -transformation dynamisch verschmelzen und sich amalgamieren. Es kommt deshalb alles darauf an, diese Einheit ins Zentrum zu rücken und in ihrer Gesetzlichkeit zu bestimmen, die ohnehin nicht einer je spezifischen Disziplin zugerechnet werden könnte, auch wenn es zunächst den Anschein haben könnte, daß den unterschiedlichen Aggregierungsebenen von Lebenspraxis – von der einzelnen Person bis zu einer komplexen, z.B. nationastaatlichen Vergemeinschaftung – auch unterschiedliche Zuständigkeiten von Disziplinen korrespondieren. Aber auch das erweist sich dann als eine systematisch vordergründige und nicht haltbare Einteilung, einfach deshalb, weil diesen Aggregierungsstufenunterschiede nicht eine Systematik der Unterschiede von Fallstrukturgesetzlichkeiten entspricht. Diese stellen ja jeweils gerade das reale Ergebnis eines Zusammenspiels sehr verschiedener Faktoren auf sehr unterschiedlichen strukturellen Aggregierungsniveaus dar. Ebenso wenig kann man fallübergreifend von einer durchgehenden Systematik in der relativen Gewichtung von einzelnen Faktoren ausgehen. Ihr Gewicht fällt viel mehr je fallspezifisch ganz unterschiedlich aus in Abhängigkeit davon, welchen Gebrauch die je konkrete Bildungsgeschichte einer Lebenspraxis von ihrer je konkreten Ausprägung gemacht hat. Fallstrukturen sind eben nicht als bloß passive, extern determinierte „Schauplätze“ der Kombination von in sich eigenlogischen Gesetzmäßigkeiten zwischen disziplinspezifischen Variablen anzusehen, sondern die jeweilige Resultante eines offenen Prozesses der Strukturtransformation, die auf das prinzipiell zur Individuierung und Autonomie auffordernde Strukturproblem antwortet, einer Strukturtransformation, die wir deshalb Individuierung oder Bildung nennen, und entsprechend ist auch das Zusammenspiel der verschiedenen Faktoren je fallspezifisch. Dem korrespondiert wie selbstverständlich die per se nur interdisziplinär durchführbare sequenzanalytische Fallrekonstruktion.

Die Leistungen der Sequenzanalyse sind vielfältig und für die objektive Hermeneutik insgesamt zentral:

1. Indem an jeder Sequenzstelle auf der Ebene des Parameters I die je eröffneten Möglichkeiten gedankenexperimentell expliziert werden müssen, bevor man sich anschaut, welche dieser Möglichkeiten faktisch eingetreten ist, gewinnt man die Folie, auf der der tatsächliche Sequenzablauf, der sich ja immer in Abhängigkeit vom Parameter II als vollzogene Auswahl bzw. Entscheidung unter den möglichen Alternanten ergibt, seine fallspezifische, präzise Kontur und Bedeutung. Dadurch gelangt die Sequenzanalyse

zu einer genauen Bestimmung des protokollierten Ablaufs, eine Bestimmung, die bei allen anderen Verfahren, die, statt sequenzanalytisch vorzugehen, die erhobenen Daten immer nur statisch-klassifikatorisch einordnen können, in dieser Präzision nicht möglich ist. Schärfer ausgedrückt: Die realitätsaufschließende Kraft klassifikatorischer, die Sequentialität von Sozialität ausblendender Verfahren ist in den Humanwissenschaften von vornherein sehr begrenzt. Diese verbleiben in der Deskriptivität.

2. Die Sequenzanalyse schmiegt sich dem realen humansozialen Geschehen in seiner Grundstruktur an und ist deshalb nicht, wie die sonst üblichen Meß- und Klassifikationsverfahren, eine dem Gegenstand äußerliche Methode, sondern eine der Sache selbst korrespondierende und ihr gemäße. Tatsächlich muß im praktischen Leben auch im Prinzip an jeder Sequenzstelle unter den noch offenen Optionen in eine offene Zukunft entschieden werden. In der bei weitem überwiegenden Zahl der Fälle geschieht das jedoch subjektiv gewissermaßen unbemerkt aufgrund von Routinen, die ursprünglich Krisenlösungen waren.

3. In die Sequenzanalyse ist gewissermaßen eine permanente Falsifikation eingebaut. Denn an jeder nächsten Sequenzstelle kann grundsätzlich der Möglichkeit nach die bis dahin kumulativ aufgebaute Fallrekonstruktion sofort scheitern. Ein strengeres Falsifikationsverfahren ist in der Methodologie der Humanwissenschaften schlechterdings nicht denkbar. Man sieht daran auch, daß die Sequenzanalyse sichtbar macht, daß das wissenschaftlich-methodische Falsifikationsverfahren dem Zusammenspiel von Krise und Routine in der prinzipiell in eine offene Zukunft hinein handelnden Praxisstruktur homolog nachgebaut ist. Ebenso wie neue Hypothesen aus einer Erkenntnis-krise hinausführen können und sich bewähren müssen, muß in der Praxis selbst um den Preis des Überlebens das Scheitern einer Überzeugung als Krise und als Bewährungsprobe akzeptiert werden. Der Unterschied ist nur, daß in der Praxis solche Krisen als negative Ausnahmen gelten, während sie in der fallibilistischen Erfahrungswissenschaft bewußt, das Scheitern der Praxis simulierend, herbeigeführt werden.

6. Krise statt Routine

In der Praxis bemerken wir diese krisenhafte Entscheidungsstruktur nur in seltenen Fällen, weil wir in der Regel die Entscheidung schon immer durch eingespielte Routinen vorweg getroffen haben. Aber diese Routinen sind ursprünglich einmal entwickelt worden als Lösungen einer Krise, die sich bewährt haben und im Bewährungsprozeß sich zu Routinen veralltäglichten. Nur im praktischen Grenzfall, wenn Überzeugungen und Routinen überraschend scheitern, oder wenn von vornherein etwas Neues gesucht werden muß, wenn also eine Krise manifest vorliegt, wird uns die Entscheidungssituation und -ungewißheit als solche bewußt. Für die Sequenzanalyse aber ist in scharfer Differenz zur Perspektive der Alltagspraxis nicht die Routine, sondern die Krise der Normalfall und nicht die Krise, sondern die Routine der Grenzfall. Das kann man sich daran klar machen, daß jeweils die Routine die Schließung einer offenen Krisensituation bedeutet und umgekehrt die Krise die Öffnung einer geschlossenen Routinisierung. Deshalb ist nicht die Krise aus der Routine material abgeleitet, sondern die Routine aus der Krise. Krise und Routine gehören zu den Grundbegriffen der objektiven Hermeneutik wie Sequentialität, latente Sinnstruktur, Authentizität, etc. Soziale Phänomene

lassen sich grundsätzlich in einem ersten Ansatz aufschlußreich danach differenzieren, ob sie primär der Sphäre der Krise oder der Routine angehören.

7. Dynamik statt Statik; Veränderung statt Konstanz

Indem nun die Sequenzanalyse in dieser Betrachtungsweise jede Sequenzstelle als Stelle einer potentiellen Krise behandelt, zeichnet sie den realen Ablauf des praktischen Lebens als je fallspezifische Gesetzmäßigkeit nach. Solange eine konkrete Lebenspraxis sich routinisiert verhält, *reproduziert* sie ihre Fallstruktur, d.h. ihre eingespielte Lebensgesetzlichkeit und ihre Identität und Charakteristik. Erst wenn sie sich in einer manifesten Krisensituation befindet, *transformiert* sie ihre Ablaufgesetzlichkeit und verändert ihre Fallstruktur. Aber in *beiden* Fällen handelt es sich um Prozesse, also um dynamische und nicht um statische Erscheinungen.

Daraus folgt ein für die Humanwissenschaften *vollständig veränderter Struktur-begriff*. Strukturen sind jetzt nicht mehr in einer sonst üblichen leeren formalen Bestimmung eine Menge von Elementen, die in einer zu spezifizierenden Relation zueinander stehen. Sie sind vielmehr für je konkrete Gebilde, die eine Lebenspraxis darstellen, genau jene Gesetzmäßigkeiten, die sich überhaupt erst in der Rekonstruktion jener wiedererkennbaren typischen Auswahlen von Möglichkeiten abbilden lassen, die durch einen konkreten Fall aufgrund seiner Fallstruktur bzw. seiner Fallstrukturgesetzlichkeit getroffen werden. Eine Fallstrukturgesetzlichkeit bzw. eine Fallstruktur kennt man erst dann, wenn man sequenzanalytisch eine vollständige Phase in deren Reproduktion oder Transformation rekonstruiert hat. In dieser Betrachtung fallen nun Struktur und Prozeß zusammen. Struktur ist nicht statisch vom dynamischen Aspekt des Prozesses unterschieden, wie das etwa im statisch-komparativen Analyseansatz immer der Fall ist, sondern ergibt sich überhaupt erst als aus dem Prozeß erschließbar.

8. Fallrekonstruktion statt Fallbeschreibung; Individuierung versus Individualisierung; Autonomie versus Heteronomie

Auf diese Weise lassen sich Fallstrukturgesetzlichkeiten schlüssig rekonstruieren, die je konkrete Lebenspraxen, seien es einzelne Personen, Gruppen, Vergemeinschaftungen wie Familien oder Gemeinden, Organisationen unterschiedlicher Größen: kurz jegliche Aggregation von Handlungsinstanzen, die eine eigene, historisch gebildete Identität haben, kennzeichnen. In klassifikatorischen Ansätzen ist das grundsätzlich nicht möglich. Und die „case studies“, die heute häufig als Illustration in Studien verwendet werden, sind nichts anderes als *Fallbeschreibungen*, die die Individualität eines konkreten Falles nur dadurch erfassen, daß sie sie in einer größeren Anzahl von klassifikatorischen Merkmalen abbilden, so daß mit deren erhöhter Anzahl die Wahrscheinlichkeit der Wiederholung einer identischen klassifikatorischen Merkmalskonfiguration stark abnimmt. Aber diese Individualitätsidentifikation bleibt bei genauer Betrachtung rein negativ, residual. Sie schließt das Innere einer Fallstruktur nicht auf und sie leistet

in ihrer Trivialität deshalb nicht mehr als die Bezeichnung einer ganz einfachen Einzigartigkeits- oder Singularitätsfeststellung, die im Prinzip für jedes empirische Ereignis gilt, weil jedes Ereignis konkret an einer einzigartigen bzw. singulären Raum-Zeit-Stelle stattfindet, an der ein anderes Ereignis nicht auch noch stattfinden kann.

Humanwissenschaftliche Strukturanalysen müssen aber in der Lage sein, über diese *triviale Individualität oder Individualisiertheit* hinaus das zu erfassen, was als das Ergebnis eines *individuierenden Bildungsprozesses* gelten muß. Denn Lebenspraxen, konkrete Handlungsinstanzen mit einer Subjektivität sind jeweils historisch-konkrete Gebilde, die sich – wie dieser Begriff schon bezeichnet – in einem je individuierenden Bildungsprozeß entwickelt sowie eine Identität ausgebildet haben und sich durch Strukturtransformation tendenziell immer noch in eine offene Zukunft weiterbilden und entfalten können. In diesem Bildungsprozeß formen sich Fallstrukturen mit einer je eigenen Fallstrukturgesetzlichkeit, in der zugleich die grundsätzliche Potentialität einer mehr oder weniger stark entwickelten *Autonomie jeder Lebenspraxis* als Freisetzung von äußerer Determiniertheit oder Fremdbestimmtheit konkret zur Bestimmung kommt. Eine Humanwissenschaft, die diese Autonomie nicht in Begriffen des Allgemeinen wissenschaftlich artikulieren und fassen kann, wird ihrem Gegenstand von vornherein nicht gerecht, denn für das humane Leben ist dessen Autonomie prägend und bestimmend. Es macht aber einen entscheidenden Unterschied, ob dieser Begriff von der Autonomie wie hier aus der Sache selbst sequenzanalytisch entwickelt ist oder bloß als ethisches Postulat von außen an die Phänomene unserer Wissenschaften emphatisch herangetragen wird.

Autonomie der Lebenspraxis wird in der objektiven Hermeneutik als *widersprüchliche Einheit von Entscheidungszwang und Begründungsverpflichtung* gefaßt. Gemeint ist damit, daß jede mit Subjektivität ausgestattete Handlungsinstanz sich in manifesten Krisensituationen, in denen die alten Routinen gescheitert sind, zu einer Krisenlösung entscheiden muß, obwohl geprüfte Begründungen und Argumente noch nicht zur Verfügung stehen. Dennoch aber muß diese Entscheidung mit dem Anspruch auf grundsätzliche Begründbarkeit getroffen werden. *Im Vollzug solcher krisenhaften Entscheidungen in eine offene Zukunft konstituiert sich die Autonomie der Lebenspraxis.* Die innere Füllung und Bestimmtheit nimmt diese je besondere Autonomie in der *Fallstrukturgesetzlichkeit* an, die eine objektive Struktur ist und scharf von dem bewußtseinsfähigen Selbstbild einer Handlungsinstanz unterschieden werden muß, mit dem sie faktisch niemals, auch nicht im Idealfalle, zur Deckung gelangt. Die Fallstrukturgesetzlichkeit operiert jenseits des bewußtseinsfähigen Selbstbildes und ist umfassender als dieses.

Die Bestimmung von Fallstrukturgesetzlichkeiten oder: die Fallrekonstruktion, ist nun weit mehr als eine bloße Fallbeschreibung im Sinne einer klassifikatorisch gewonnenen Merkmalskonfiguration. Sie bestimmt einen konkreten Fall gewissermaßen von innen her positiv und sie erklärt mit der Explikation das Zustandekommen und die Motiviertheit einer Merkmalskonfiguration, bei der eine Fallbeschreibung immer stehenbleiben muß, dadurch, daß per Sequenzanalyse die sich reproduzierende oder transformierende Fallstruktur direkt falsifizierbar erschlossen und expliziert worden ist.

Die Bestimmung von Fallstrukturgesetzlichkeiten und die Rekonstruktion von Fallstrukturen sieht nun die objektive Hermeneutik als das eigentliche, zentrale Er-

kenntnisgeschäft der Humanwissenschaften an. Auch wenn es nur um die Analyse von Dokumenten, von einzelnen Werken oder von Situationen oder auch von hoch standardisierten und routinisierten Phänomenen jeweils gehen sollte, so ist doch immer deren Einbettung und Ableitung aus konkreten, je historischen Fallstrukturen letztlich unverzichtbar, wenn man zu einer wirklichen ergebnisträchtigen Bestimmung gelangen und die Überschreitung der bloßen Deskriptivität zur Explanatation hin erreichen will.

9. Sozialer Wandel statt Statik; Neues statt Bekanntes

Indem jeweils die Sequenzanalysen zu Fallrekonstruktionen führen, wird als Normalfall der objektiv hermeneutischen Strukturanalyse der soziale Wandel, die Entstehung des Neuen angesetzt und vom Grenzfall der Statik und Nicht-Veränderung abgehoben. In den sonstigen Methodenansätzen der Sozialforschung erscheint dagegen umgekehrt der soziale Wandel als „Extrafall“, was man daran sehen kann, daß eigens eine Theorie des sozialen Wandels vor die Klammer der einzelnen gegenstandsspezifischen Forschungsgebiete gestellt wird. Damit wird kategorienfehlerhaft suggeriert, man könne bei den normalen Gegenstandsanalysen davon absehen, daß sich die soziale, kulturelle und psychische Welt permanent im historischen Wandel und in der Entwicklung in eine offene Zukunft hinein befindet. Daß für die objektive Hermeneutik dagegen die konkrete Identifikation dieser Historizität der psychischen und sozialen Phänomene keine zusätzlichen Probleme aufwirft, wie für die anderen Ansätze, mag man auf einfache Weise am folgenden ablesen: Wenn man sequenzanalytisch ein erstes Protokoll einer konkreten Lebenspraxis auf eine Fallstrukturgesetzmäßigkeit hin rekonstruiert, dann kann man bezüglich des Ergebnisses nicht entscheiden, ob sich der untersuchte Fall gerade in einer Phase der bloßen *Reproduktion oder schon der Transformation seiner Fallstruktur* befunden hat. Denn die Sequenzanalyse rekonstruiert immer einen prozessualen Ablauf. Erst wenn man dieses erste Ergebnis mit der Analyse eines zeitlich vorausgehenden Protokollsegments vergleicht, läßt sich diese Unterscheidung sichern. Für die objektive Hermeneutik ist also die Transformation der Normalfall und die Reproduktion der Grenzfall, zu dessen Nachweis es einer zusätzlichen Rekonstruktion der Fallstruktur bei einem weiteren Protokoll-Segment aus einer anderen Zeit bedarf.

In dieser Feststellung spiegelt sich, inwiefern im sequenzanalytischen im Unterschied zu einem klassifikatorischen Verfahren die Historizität und die Zukunftsoffenheit menschlicher Praxis direkt erfaßt wird und deshalb diese Methode zu Recht als eine dialektische gelten darf, weil sie den fließenden Übergang von A nach Nicht-A lückenlos zu rekonstruieren vermag, statt ihn durch die Maschen einer noch so dichten klassifikatorischen Erfassung fallen zu lassen, in der immer nur statische Momentaufnahmen von Zuständen möglich sind. Das gilt für alle Varianten eines statisch-komparativen Ansatzes, in dem bekanntlich der identische Gegenstand als Merkmalsträger zu verschiedenen Zeitpunkten im identischen Variablensystem vermessen wird, so daß eine Zeitreihe von Meßwertekonfigurationen entsteht. Die Veränderungen über die Zeit lassen sich dann zwar als Profile von Verbindungslinien zwischen den – vorgeblich – bedeutungsgleichen Meßwerten abbilden, aber die Dynamik des Veränderungsprozesses selbst ist dadurch nur indirekt angezeigt, aber nicht prozessual rekonstruiert. Wie

diese Profile zu interpretieren sind, ist eine Funktion einer von außen an sie herangetragenen Plausibilität, nicht das zwingende Ergebnis einer immanenten Erschließung.

10. Strukturgeneralisierung statt empirischer Generalisierung

Damit ist auch schon angedeutet, daß die Generalisierung der Forschungsergebnisse solcher Sequenzanalysen mit dem Ziel einer Fallrekonstruktion kein wirkliches Problem ist und daß die üblichen Einwände, man könne bei geringen Fallzahlen keine Verallgemeinerungen vornehmen, gegenstandslos sind. Sie gelten nur für den sehr eingeschränkten Fall, daß statistisch gesicherte Schätzungen von auf Frequenzen beruhenden Meßwerten oder Zusammenhangsmaßen vorgenommen werden sollen. Solche Generalisierungen beziehen sich aber nur auf *empirische Generalisierungen* nach dem Muster „je mehr von x, desto weniger (oder mehr) von y“ und diese können ihrerseits nach dem Induktionsschluß von n auf n+1 gesichert werden. Aber solche Generalisierungen haben nur einen sehr begrenzten Erkenntniswert und sind auf keinen Fall zur Strukturkenntnis geeignet. Zwar wird in der forschungslogischen Literatur immer so getan, als ob durch solche empirischen Generalisierungen Strukturen bestimmt würden, aber in Wirklichkeit handelt es sich nur um statische Meßwerte- und Zusammenhankskonfigurationen, die allenfalls als Momentaufnahmen eine dahinterliegende Struktur an der Oberfläche verkörpern und indizieren, aber nicht wirklich aufschließen. Die Bestimmung erfolgt im Grunde durch von außen an diese Konfiguration herangetragene, ungeprüft aus irgendwelchen Primärerfahrungsquellen des Forschers beigezogene Interpretationen, die allenfalls Plausibilität, aber keine methodische Schlüssigkeit für sich in Anspruch nehmen können.

Dagegen stellt eine Fallrekonstruktion eine *Strukturgeneralisierung* dar. Während die empirische Generalisierung zum Argumentebereich *durch standardisierte Messung oder Klassifikation vergleichbare Beobachtungswerte* an einer bestimmten empirischen Menge von Merkmalsträgern (z.B. Stichprobe) hat, ihre Beobachtungsreihe also aus Merkmalsträgern besteht, die unter dem vorgewählten Gesichtspunkt von allgemein meß- oder klassifizierbaren Merkmalen konstituiert wurde, verhält es sich bei der Strukturgeneralisierung, die man auch mit einem Ausdruck von Charles Sanders Peirce im Unterschied zu einer quantitativen Induktion als qualitative Induktion bezeichnen könnte, ganz anders: Ihr Argumentebereich besteht nicht aus einzelnen beobachteten Meßwerten, sondern aus *rekonstruierten Sequenzen* (formal durchaus vergleichbar den Sequenzen der Molekularbiologie). Da nun, wie vorher gezeigt, diese Sequenzen sich aus dem Zusammenspiel von sinnlogischen Erzeugungsregeln (*Parameter I*), die die Möglichkeiten schaffen und die Zukunft jeweils eröffnen, und von Auswahlmaximen (*Parameter II*), die die Fallstrukturgesetzlichkeit ausmachen, ergeben, liefert die Strukturgeneralisierung sowohl Aussagen bzw. Darstellungen von Regeln auf der Ebene des Parameters I als auch Fallstrukturgesetzlichkeiten bzw. Gesetzmäßigkeiten, die den Fall als solchen in seiner Besonderheit charakterisieren, auf der Ebene des Parameters II.

Insgesamt lassen sich *sieben Hinsichten* unterscheiden, in denen die Strukturgeneralisierung operiert. Davon betreffen nur zwei den Parameter I, die übrigen den Parameter II:

1. Jede einzelne Fallrekonstruktion ist schon als solche eine Strukturgeneralisierung. Denn ihr je konkretes Ergebnis, das man früher als Darstellung eines Typus bezeichnet hätte im Unterschied zu einer Kategorisierung oder Klassifikation eines Einzelfalles in einer vorgefaßten Kategorie oder Klasse, bildet einen konkreten Fall in seiner inneren Gesetzmäßigkeit ab, die seine Autonomie bzw. den Grad seiner Autonomie als das Ergebnis seiner Individuierungsgeschichte ausmacht. Wie häufig dieser Fall sonst noch vergleichbar oder ähnlich auftaucht, wieviele weitere „token“ dieses „type“ es also empirisch gibt, ist für diese Hinsicht der Strukturgeneralisierung vollständig unerheblich. Denn es wäre absurd, wollte man willkürlich ein Kriterium einführen, wonach erst ab einer bestimmten absoluten oder relativen Frequenz dieser je konkret rekonstruierte „Typus“ als solcher eine Realität hätte. Im Unterschied dazu lassen sich natürlich empirische Generalisierungen von Anfang an und wesensgemäß nur auf der Basis einer Mehrzahl von Beobachtungen treffen.

Aber es wäre nun platter Dogmatismus, der sich allerdings in die Methodenliteratur der empirischen Sozialforschung faktisch häufig einschleicht, wollte man die „empirische Generalisierung“, die mit dieser Beschränkung behaftet ist, als einziges Modell für die Generalisierbarkeit wissenschaftlicher Analyse-Ergebnisse gelten lassen. An der Unterscheidung von „type“ und „token“ läßt sich nämlich beispielhaft erläutern, warum nicht die empirische Generalisierung sondern die Strukturgeneralisierung der wissenschaftslogisch allgemeinere Fall ist und warum deshalb die Strukturgeneralisierung forschungslogisch der empirischen Generalisierung vorgeordnet ist und nicht umgekehrt; mithin die empirische Generalisierung nicht ohne die Strukturgeneralisierung angemessen bestimmt werden kann, aber die Strukturgeneralisierung durchaus ohne den Bezug auf die empirische Generalisierung.

Das Verhältnis von „token“ zu ihrem „type“ überschreitet die Relation zwischen einem Element (Ereignis oder Merkmal) und seiner Kategorie oder Klasse. Ich muß nämlich die Kategorie oder Klasse vorweg bestimmt haben und kennen, bevor ich ein Element in einer Messung ihr zuordnen kann. Diese Zuordnung ist also immer das Ergebnis einer Subsumtion. Dagegen kann ich jederzeit an einem „token“ eine Fallrekonstruktion als Typusbestimmung durch Sequenzanalyse vornehmen, ohne diesen Typus vorher zu kennen oder bestimmt zu haben. Eine Fallrekonstruktion ist als Strukturgeneralisierung immer eine genuine, ursprüngliche Typusbestimmung. Vom Ergebnis her kann nachträglich eine Vergleichbarkeit mit einer oder mehreren anderen Fallrekonstruktionen so festgestellt werden, daß sich diese mehreren Fälle als „token“ desselben „type“ erweisen. Aber es macht für die konkrete strukturgeneralisierende Fallrekonstruktion keinen Unterschied, ob ihr Ergebnis: die Bestimmung eines allgemeinen Typs, anläßlich eines konkreten Falles zum ersten oder zum wiederholten Male vorliegt. Auch beim ersten Mal wird ein Fall zugleich als „type“ und als dessen „token“ bestimmt.

Daraus geht zwingend hervor, daß faktisch und logisch die Fallrekonstruktion als Strukturgeneralisierung der Konstitution einer Fall- bzw. Beobachtungsreihe vorausgehen muß, die einer empirischen Generalisierung bzw. einer quantitativen Induktion zugrundeliegt. Um überhaupt eine solche Beobachtungsreihe, z.B. eine Stichprobe herstellen zu können, muß ich schon über die Bestimmung eines allgemeinen Typs bzw. über die allgemeinbegriffliche Bestimmung eines Zusammenhangs zwischen mindestens zwei Merkmalen verfügen. Diese vorausgehende Bestimmung ist aber nichts anderes als eine Strukturkenntnis, die als das Ergebnis einer strukturgeneralisierenden Fallre-

konstruktion anzusehen ist. Mit dem späteren Peirce können wir sie auch als abduktiven Schluß bezeichnen, mit dem eine wissenschaftliche Erkenntnisoperation überhaupt erst beginnen muß, bevor die in der üblichen positivistischen Wissenschaftstheorie einzig thematischen anderen Schlußweisen, der induktive und der deduktive Schluß, zu operieren beginnen können.

Nur wird gewöhnlich in der „quantifizierenden“ empirischen Sozialforschung diese Vorausbedingung der Strukturgeneralisierung für die Konstitution oder Herstellung einer Beobachtungsreihe als Argumentebereich einer empirischen Generalisierung nicht mehr reflektiert, sondern stillschweigend übergangen. Darin liegt die Quelle eines verkürzenden Dogmatismus.

Jede konkrete Fallrekonstruktion führt also zu einer Strukturgeneralisierung bzw. der Bestimmung eines allgemeinen Typus. Dessen Allgemeinheit ist in gar keiner Weise von der Häufigkeit seines Vorkommens bzw. von der Frequenz seiner „token“ abhängig. Selbst wenn er in einer „Grundgesamtheit“ tatsächlich nur einmal vorkäme, hätte er diese Allgemeinheit eines Typus, die die Fallrekonstruktion in der logischen Form einer Strukturgeneralisierung bestimmt.

2. Bei jeder konkreten Fallrekonstruktion wird nicht nur der im sequenzanalysierten Protokoll verkörperte manifeste Fall zur Explikation gebracht, sondern es werden darüber hinaus andere, weitere Fälle bestimmt, die dieser Fall seinen objektiven Möglichkeiten nach in seiner weiteren historischen, kulturellen und sozialen Umgebung, seinem Milieu, prinzipiell hätte werden können, aber nicht geworden ist. Denn die Sequenzanalyse erfordert es, an jeder Sequenzstelle im Sinne geltender Erzeugungsregeln den Spielraum von Möglichkeiten konkret gedankenexperimentell zu explizieren, mit Bezug auf den der konkrete Fall seine Strukturgesetzlichkeit als immer wiederkehrende Systematik seiner Entscheidungen und Auswahlen abbildet. Diese gedankenexperimentelle Konstruktion von je konkreten Möglichkeiten kommt aber einer Explikation von weiteren Fallstrukturen gleich, so daß man mit *einer* Fallrekonstruktion immer schon mehrere Fälle kennt und nach kurzer Zeit in der fallrekonstruierenden Untersuchung einer Fallreihe zu einem bestimmten Untersuchungsthema sich alle weiteren Fälle als „déjà vues“ erweisen. Darin liegt im Hinblick auf die Generalisierbarkeit der Ergebnisse eine enorme Ersparnis.

3. Jeder untersuchte konkrete Fall ist in höher aggregierte Fallstrukturen eingebettet: eine Person in eine Familie oder Primärgruppe, diese in ein konkretes Milieu oder per Beruf in einen bestimmten gesellschaftlichen Sektor oder ein soziales Subsystem, diese wiederum in eine Region oder einen gesellschaftlichen, historischen Typus und diese(r) wiederum in eine umfassende Gesellschaft als Totalität. Insofern stellt jede rekonstruierte Fallstruktur eine je konkrete Variante einer einbettenden, übergeordneten Fallstrukturgesetzlichkeit dar und liefert über sie eine allgemeine Erkenntnis. Sobald mehrere solcher, auf gleicher Aggregierungsebene liegender Fallrekonstruktionen in einer Fallreihe bekannt sind, lassen sich die Generalisierungen dieser Hinsicht erhärten.

4. Bei jeder fallspezifischen Sequenzanalyse werden Erfahrungen über die Geltung bzw. Nicht-Geltung von Regeln der Erzeugung von Sinnstrukturen gemacht, die als Basis für eine Falsifikation vorausgehender Regelrekonstruktionen oder für eine Modifikation bzw. Innovation von Regeln explizierender Theorien prinzipiell dienen können. Zwar widmet sich die objektive Hermeneutik nicht eigens solchen

Regelrekonstruktionen, sondern beläßt es bei der intuitiven Inanspruchnahme des Regelbewußtseins des „native speaker“, aber es können immer Strittigkeiten in der Ausformung dieser Intuition in der Konkretion der Sequenzanalyse vorkommen, die erst durch eine schrittweise Explikation der intuitiv in Anspruch genommenen Regeln zur Entscheidung gebracht werden können. In der Regel werden daraus kaum erkenntnisträchtige Generalisierungen für die Teilbereiche der Phonologie, Syntax oder der einschlägigen Logik resultieren, weil diese Teilbereiche inzwischen sehr gut erforscht sind, aber für die Teilbereiche der Sprechakttheorie, der Präsuppositionen, der konversationellen Implikaturen, der textlinguistischen oder diskursregulierenden Schematisierungen, aber auch der praktischen Schlüsse und vor allem der Prinzipien der sozialen Kooperation und der kollektiven Normierungen der je historischen Praxis sind durchaus von Fall zu Fall jenseits der Fallrekonstruktion selbst relevante Generalisierungen zu erwarten, die die den Parameter I betreffenden theoretischen Kenntnisse erweitern und bereichern können.

5. Jede Fallrekonstruktion kann potentiell Fallstrukturgesetzmäßigkeiten sichtbar machen, die bisher nicht bekannt waren und auch real bisher in der Praxis nicht vorkamen, so daß sie als eine Erneuerung bzw. das Ergebnis einer sozialen Veränderung in der Praxis selbst gelten müssen. Solche Erneuerungen bzw. Veränderungen sind potentiell folgenreiche Änderungen in der Menschheitsgeschichte und werden am Anfang nicht massenhaft, sondern vereinzelt auftauchen und sich dann im Falle ihrer Bewährung erst ausbreiten. Wenn die Fallrekonstruktion – gezielt, vermutend oder unerwartet – auf solche Neuerungen trifft, die potentiell Modelle der Zukunft darstellen, nimmt sie mit der fallspezifischen Strukturgeneralisierung zugleich eine Generalisierung bezüglich der Zukunftsentwicklung vor, die etwas anderes darstellt als eine bedingte Prognose auf der Basis einer statistischen Hypothesenüberprüfung bzw. einer empirischen Generalisierung. Sie expliziert dann nämlich mit dem Anspruch auf Vernünftigkeit auftretende Krisenlösungen, die in sich als Modelle von Praxis die bisherigen Rationalitätsmaßstäbe überschreiten. Es liegt auf der Hand, daß sich solche Zukunftsentwicklungen viel eher in detaillierten Fallrekonstruktionen zum Vorschein bringen lassen als in statistischen Frequenzanalysen, in denen sie als „Ausreißer“ oder außerhalb einer formalen, statistischen „Normalität“ liegend „untergehen“.

6. An solche Generalisierungen schließen sich zugleich der Möglichkeit nach Generalisierungen mit praktischer Wirkung an. Es kann nämlich sein, daß die unter (5) angesprochenen Neuerungen nicht nur rein empirisch als Typen – unabhängig von ihrem tatsächlichen Problemlösungspotential – die Zukunft bestimmen werden, sondern daß sie darüber hinaus auch tatsächlich im Sinne der praktischen Vernunft zwingende Lösungen bieten, deren Rationalität sich eine mit Anspruch auf Vernünftigkeit handelnde Praxis in Zukunft nicht entziehen kann. Dieser rationale Zwang aufgrund des besseren Argumentes muß sich natürlich empirisch faktisch nicht durchsetzen oder kann in seiner an sich gebotenen Ausbreitung erhebliche Widerstände und Verzögerungen erfahren. Die Fallrekonstruktion, die solche Neuerungen erfaßte, wäre für sich im Sinne einer sokratischen Mäeutik ein wichtiger Beitrag bei der Durchsetzung solcher Praxisveränderungen. Ich spreche hier nicht von bewußt geplanten Neuerungen, die gewöhnlich unter dem Titel „Innovation“, „Entwicklung“ oder „Erfindung“ thematisch sind und deren Ausbreitung strategischer Bestandteil ihrer selbst sind, sondern von solchen Neuerungen, die sich „naturwüchsig“ in der geschichtlichen und individuellen

renden Praxisbewährung als Modelle der kollektiven und individuellen Lebensbewältigung ergeben.

7. Analog zu solchen aus fallstrukturgesetzlichen Transformationen sich ergebenden Neuerungen können natürlich auch im Bereich der Sinnstrukturen erzeugenden Regeln – vor allem im Unterbereich sozialer Normierungen, aber auch im Bereich einzelsprachlicher Regeln – Neuerungen bzw. Transformationen auftreten, deren frühzeitige Registrierung wissenschaftlich von großer Bedeutung sein kann.

Alle diese unter den sieben Gesichtspunkten zusammengestellten Dimensionen der auf der Basis von Fallrekonstruktionen möglichen Strukturgeneralisierungen beziehen sich auf Strukturkenntnisse, die in den Humanwissenschaften durch Operationen der empirischen Generalisierung gar nicht oder nur sehr beschränkt generiert und artikuliert werden können. Auch darin zeigt sich, daß die Strukturgeneralisierung eine genuin dem Strukturalismus zugehörige Erkenntnisoperation darstellt, die den handlungstheoretisch affinen methodischen Vorgehensweisen fremd ist.

11. Sequenzen von maximal kontrastierenden Fällen statt Stichproben

Natürlich wird man auch in der objektiven Hermeneutik eine Untersuchungsfrage nicht durch eine einzige Fallrekonstruktion, sondern eine *Reihe von Fallrekonstruktionen* empirisch beantworten. Aber in scharfer Differenz zur Logik der Ziehung einer *Zufallsstichprobe* für die statistische Hypothesenüberprüfung bzw. die Schätzung von relativen Frequenzen auf der Basis empirischer Generalisierungen, bei der die einzelnen Elemente voneinander unabhängig sein müssen, werden hier die Fälle einer Untersuchungsreihe sequentiell in Abhängigkeit voneinander nach dem *Kriterium des maximalen Kontrastes* ausgewählt. Die Logik der Sequenzanalyse wird also sinngemäß auch auf die Anordnung der Fallhebungen und Fallauswertungen einer zusammenhängenden Untersuchung angewendet.

Man geht dabei in der folgenden Weise vor: Es werden zunächst Protokolle nur eines Falles ausgewählt oder erhoben, der für die jeweilige Untersuchungsfrage zentral ist. Natürlich müssen auch die Protokolle selbst aus der Lebenspraxis des Falles Ausschnitte abbilden, die für die Untersuchungsfrage von Bedeutung sind. Dann analysiert man das Material dieses ersten Falles einer Reihe detailliert und ausführlich, so daß an seinem Beispiel möglichst viele Antworten zur Untersuchungsfrage entwickelt werden und möglichst präzise erste Strukturgesetzlichkeiten des die Untersuchungsfrage betreffenden Gegenstandsbereiches herauspräpariert werden können. Erst wenn das maximal geleistet ist, erhebt man das Material des nächsten Falles. Dieser wird nun so ausgewählt, daß er nach Maßgabe der Erkenntnis zum ersten Fall maximal mit diesem kontrastiert. Wiederum wertet man dieses Material aus, bevor man den nächsten Fall so erhebt, daß er seinerseits maximal mit den vorausgehenden Fällen kontrastiert. Man schreitet so immer weiter voran. Die Auswertungen werden beim jeweils nächsten Fall exponentiell abnehmend kürzer, weil immer weniger an Erkenntniszuwachs über die den Gegenstand kennzeichnenden Strukturgesetzlichkeiten hinzukommt. Man bricht die Fallreihe ab, wenn evident geworden ist, daß der Erscheinungsspielraum innerhalb des Gegenstandsbereichs für die Zwecke der Modellrekonstruktion im wesentlichen ausgeschöpft ist. Dafür kann ein prinzipielles Kriterium nicht angegeben werden, aber

die Erfahrung zeigt, daß angesichts des Umstandes, daß mit jeder Fallrekonstruktion immer mehr Fälle als der im Fallmaterial tatsächlich verkörperte zur Geltung kommen, in der Regel zehn bis zwölf Fallrekonstruktionen auch für komplexere Untersuchungsfragen ausreichen, um hinreichend gesicherte Antworten zu erhalten. Man ist dann in der Strukturkenntnis und in den Strukturgeneralisierungen so weit vorangekommen, daß die Erforschung von strukturtheoretischen Folgefragen ökonomischer ist, als die weitere Detaillierung der gesuchten Antworten. Diese Detaillierung zu vervollständigen, lohnt sich dann letztlich nur noch im Hinblick darauf, relative Häufigkeiten über die Verteilung von verschiedenen Typen und – darauf beruhend – Schätzungen über die relative Stärke des Zusammenhangs zwischen einzelnen Strukturelementen zu erhalten. Dazu sind dann die üblichen spezifischen, auf standardisierten Messungen beruhenden Erhebungen in größeren Stichproben als Ergänzung zum Zwecke der verwaltungsrationalen Entscheidung über den Einsatz standardisierter praktischer Problemlösungen notwendig.

12. Klinische, nicht-standardisierte statt standardisierte Datenerhebung und rekonstruktionslogische statt subsumtionslogische Auswertung

Seit einiger Zeit hat sich die sogenannte „qualitative Sozialforschung“ ein gewisses Terrain und eine gewisse Anerkennung gegenüber der sogenannten „quantitativen Sozialforschung“ erobert. Aber immer noch zeigen allein die Vorlesungsvorzeichnisse in den Sozialwissenschaften noch, daß – mehr oder weniger dogmatisch – unter den Methoden der empirischen Sozialforschung wie selbstverständlich die der „quantitativen“ verstanden werden und die der „qualitativen“ als bei Bedarf diesen „Normalfall“ ergänzende Zusätze. In den unter dem irreführenden Begriff der „qualitativen“ oder „interpretativen Sozialforschung“ zusammengefaßten, untereinander recht heterogenen und keinesfalls miteinander kompatiblen Methoden findet sich in den mit diesem Titel mittlerweile im Umlauf befindlichen Lehrbüchern auch die objektive Hermeneutik.

Aber aus deren Sicht ist die Unterscheidung von quantitativen Methoden einerseits und qualitativen oder interpretativen andererseits nicht haltbar. Denn zum einen müssen an jeder quantifizierenden Forschungsmethode „qualitative“ oder „interpretative“ Momente notgedrungen beteiligt sein. Wie anders wollte man die Merkmals- oder Eigenschaftsdimensionen der quantifizierender Operationalisierungen von Begriffen denn bestimmen? Rein quantifizierend ist das gar nicht möglich. Zum anderen aber – und das ist von größerer Bedeutung – ist diese Unterscheidung viel zu vage und mehrdeutig, weil sie die wesentlichen Unterschiede zwischen einer traditionellen, auf quantifizierendes Messen abstellenden Methodologie der Sozialwissenschaften und einer auf Fallrekonstruktion und konkrete Strukturanalyse ausgerichteten Methodologie falsch benennt.

Die objektive Hermeneutik trennt in der Betrachtung der mit der mißverständlichen Gegenüberstellung von quantitativen und qualitativen Methoden gemeinten Unterschiede zunächst einmal zwischen den *Methoden der Datenerhebung und der Datenauswertung*.

13. Zur Ebene der Datenerhebung

Auf dieser Ebene müssen grundsätzlich zwei Dimensionen unterschieden werden: (1) Die Methoden und Praktiken des sozialen Arrangements bei der Datenerhebung und (2) die Techniken der Protokollierung bei der Datenerhebung.

Da jedes Datum für uns als Ausdrucksgestalt einer Lebenspraxis und damit als Text und als Protokoll zu gelten hat, ist von vornherein der Weg dazu geöffnet, nicht nur mit eigens zum Untersuchungszweck erhobenen Daten operieren zu müssen, sondern auch von der Untersuchungswirklichkeit selbst schon hergestellte Ausdrucksgestalten (Dokumente, Objektivierungen, Akten, Briefe, Publikationen, etc.) als gleichwertig heranzuziehen. Dadurch werden die empirischen Überprüfungsmöglichkeiten der Forschung wesentlich erweitert und bereichert und es wird nebenbei der Graben zur Geschichtswissenschaft zumindest methodisch eingeebnet. Denn diese hat es naturgemäß viel mehr mit recherchierbaren Daten zu tun, weil sie an ihrem der Vergangenheit zugehörigen Gegenstand in der Regel keine eigenen Erhebungen mehr vornehmen kann.

1. *Die Methoden und Praktiken des sozialen Arrangements bei der Datenerhebung*: Der Generierung eines jeden Datums liegt ein spezifisches soziales Arrangement zugrunde. Auch bei den standardisierten Messungen der sogenannten quantifizierenden Sozialforschung ist, wie der Begriff der Operationalisierung schon in sich birgt, der Erhebungsvorgang vor allem in ein bestimmtes soziales Arrangement der Operation des Messens eingebettet. Dieses soziale Arrangement muß bei einer vom Forscher selbst vorgenommenen Datenerhebung bzw. – generierung genau vorbedacht sein. Bei der Sammlung von Daten, die die untersuchte Wirklichkeit selbst produziert, muß der dabei mitbeteiligte pragmatische Rahmen der Produktion genau rekonstruiert werden. Die entscheidende Dimension für die Variation zwischen verschiedenen sozialen Arrangements läßt sich mit den gegenüberliegenden Polen von „standardisiert“ und „klinisch, bzw. nicht-standardisiert“ bezeichnen.

Es liegt auf der Hand, daß der Vorteil standardisierter Verfahren vor allem in der Ökonomie der Erhebung und der späteren Auswertung³ liegt, aber nicht, wie in der Regel unterstellt, in der besseren Vergleichbarkeit über verschiedene Merkmalsträger hinweg, und schon gleich gar nicht in der angeblich größeren Präzision. Viel mehr erkaufte man gerade mit der Standardisierung des sozialen Arrangements bei der Datenerhebung erhebliche Nachteile bezüglich der Prägnanz der Daten. Das soziale Arrangement bei der Erhebung muß zum einen der spezifischen Charakteristik des Gegenstandes und der Fragestellung und zum anderen der Lebenslage und Situation des je konkreten Falles bzw. Merkmalsträgers angemessen sein und sich mimetisch anschmiegen. Das ist nur in Analogie zum klinischen Vorgehen möglich.

³ Im subsumtionslogischen Ansatz läßt sich im Grunde genommen zwischen der Datenerhebung und der Datenauswertung nicht mehr sinnvoll forschungslogisch unterscheiden, weil die Datenauswertung mit der Operationalisierung der Begriffe und mit der Art der Datenerhebung gewissermaßen vorprogrammiert ist und nur noch mechanisch erfolgt. Ein eigenständiges Interpretationsproblem beginnt erst wieder nach den statistischen Hypothesenüberprüfungen bzw. nach den induktiven Herausrechnungen von Konfigurationen in Faktoren- und „cluster“-Analysen. Diese Deutungen sind dann aber der statistischen Präparation von Daten äußerlich, diese verhalten sich grundsätzlich jenen gegenüber wie Artefakte.

2. *Die Techniken der Protokollierung bei der Datenerhebung:* Auch für die Variation der Techniken der Protokollierung bei der Datenerhebung wird die zentrale Dimension durch die Pole „standardisiert“ und „nicht-standardisiert“ gebildet. Unter standardisierten Techniken der Protokollierung werden hier z.B. das Ankreuzen von standardisierten Vorgaben bei Befragungen oder das Anfertigen von Strichlisten bei Beobachtungen, aber auch das Anfertigen von ethnographischen Beschreibungen nach einem vorgegebenen Gliederungsschema verstanden. Nicht-standardisierte Techniken sind vor allem alle direkten, „natürlichen“ Aufzeichnungen von Vorgängen, Handlungen und Äußerungen durch Aufzeichnungsgeräte wie Film- oder Videokameras, Fotoapparate und Tonbandgeräte. Zwar sind hier die Geräte hoch-standardisiert, aber nicht die Aufzeichnung, denn sie protokolliert „unintelligent“ und „semantisch unselektiv“ alles, was an physikalischen Impulsen der protokollierten Wirklichkeit die Aufzeichnungsgeräte erreicht.

Aufzeichnungen sind gewissermaßen „natürliche“ Protokolle und den von intelligenten Subjekten angefertigten Beschreibungen in jedem Falle vorzuziehen, weil diese immer schon von einer – in der Regel vereinseitigenden – Interpretation geprägt sind. Die „Verdichtungen“ solcher Beschreibungen behindern die spätere Datenauswertung eher als daß sie sie befördern. Der Begriff der „*dichten Beschreibung*“ (Clifford Geertz) hat hier in neuerer Zeit viel Verwirrung gestiftet. Darunter können nämlich drei ganz verschiedene Dinge verstanden werden. *Zum einen* eine tatsächlich literarisch gelungene Beschreibung einer zu protokollierenden Wirklichkeit, die den Namen „Verdichtung“ auch verdient. In ihr wird durch künstlerische Gestaltung eine Wirklichkeit nicht abgebildet, sondern zu einer eigenlogischen Wirklichkeit auf höchst subjektive Weise transformiert, aber gerade durch diese Subjektivität in eine erkenntnisträchtige, in sich stimmige Kohärenz gebracht. Es handelt sich um eine im Modus der künstlerischen Prägnanzbildung verfahrenende Erkenntnisleistung per Darstellung, die den Namen der „Verdichtung“ im Sinne einer ästhetisch suggestiven Fallrekonstruktion auch verdient. Nur ist festzuhalten, daß es sich hier um Kunst und nicht um Wissenschaft handelt, um eine Erkenntnisform also, deren Wahrheitskriterium in der zwingenden Suggestivität der sinnlichen Präsenz ihrer Darstellung besteht und nicht in der Methode der Logik des besseren Argumentes. *Zum anderen* wird darunter das Resultat einer vom Forscher vorgenommenen Wirklichkeitsinterpretation verstanden, die ihrerseits schon auf irgendwelchen Daten beruht. Dann handelt es sich eigentlich nicht um eine Beschreibung, schon gar nicht um eine Technik der Protokollierung, sondern um eine Analyse. Schließlich wird *drittens* darunter eine ethnographische Beschreibung verstanden, die in besonderer Weise auf Prägnanz und Wesentlichkeitserfassung aus sei. Sofern es sich hier wirklich um eine Beschreibung handeln sollte, so ist ihr immer die Aufzeichnung vorzuziehen. Denn wenn sie wirklich „dicht“, also verdichtend sein sollte, und dennoch nicht literarisch oder schon eine theoriegeleitete Interpretation, so enthält sie immer schon Abkürzungen und selektive Zusammenfassungen, die später nicht mehr mit einem detaillierten „natürlichen“ Protokoll abgeglichen werden können und deren Subjektivität man deshalb unkritisch und unüberprüfbar ausgeliefert ist. „Dichte Beschreibungen“ sind also, wenn sie wirklich Beschreibungen sind und nicht schon methodisch kontrollierte Interpretationen oder Analysen, allenfalls als künstlerisch-literarisch gelungene von eigenem Wert.

Innerhalb der nicht-standardisierten Techniken der Protokollierung ist also als

weitere, eingebettete Unterscheidungsdimension die zwischen technischer Aufzeichnung an einem Ende und verdichteter, künstlerischer Gestaltung am anderen Ende anzusetzen. Dazwischen liegen die Varianten der Beschreibung. Diese liefern die schlechtesten Daten und die nachteiligsten Techniken der Protokollierung, weil bei ihnen die je subjektive Interpretation bzw. „Konstruktion“ schon immer in die Erhebung der Daten eingeht, so daß diese Kontamination nachträglich nicht mehr geschieden werden kann und so die Überprüfungsrelevanz der erhobenen Daten entscheidend eingeschränkt wird bzw. eine den Fallibilismus entscheidend beeinträchtigende schlechte Zirkularität mit sich bringt. Vorzuziehen sind für die Forschung immer die bezüglich der involvierten Sinnstrukturen vollkommen unintelligenten und deshalb unselektiven technischen Aufzeichnungen. Sie können als „natürliche“ Protokolle gelten.

Bezüglich der Datenerhebung ist also an die Stelle der Unterscheidung von „quantitativer“ und „qualitativer“ Sozialforschung jene von „standardisierten“ und „nicht-standardisierten, klinischen“ Methoden zu setzen.

14. Zur Ebene der Datenauswertung

Wesentlich wichtiger als die Methoden der Datenerhebung sind für die Qualität der Forschung die Methoden der Datenauswertung. Eine noch so gute Datenerhebung nützt nichts, wenn die Auswertungsmethoden nicht gut sind. Und noch so schlechte Daten können bei guten Auswertungsmethoden immer noch sehr aussagekräftig sein. Deshalb ist es für die objektive Hermeneutik besonders wichtig, die Probleme der Datenerhebung von den Problemen der Datenauswertung klar zu trennen, – eine Trennung, die in der üblichen Unterscheidung von quantitativer und qualitativer Sozialforschung untergeht. Dadurch kommt z.B. auch nicht in den Blick, daß sich „qualitativ“ erhobene Daten, „quantitativ“ auswerten lassen und umgekehrt.

Die entscheidende Dimension der Unterscheidung zwischen der traditionellen „quantitativen“ Sozialforschung und der „qualitativen“ bzw. „interpretativen“ wird in der Regel bei den üblichen Betrachtungen gar nicht deutlich gesehen. Ich bezeichne sie als die Differenz zwischen *subsumtionslogischen* und *rekonstruktionslogischen Verfahren*. In der Bezeichnung „qualitative Inhaltsanalyse“ z.B. geht sie unter, denn dies ist ein Verfahren, in dem Texte eindeutig subsumtionslogisch ausgewertet werden und nicht rekonstruktionslogisch. Dennoch wird es als „qualitativ“ bezeichnet.

Subsumtionslogische Verfahren an dem einen Ende entsprechen der üblichen wissenschaftslogischen Auffassung, wonach – in dieser Schrittfolge – Hypothesen aufgestellt, aus bewährten Theorien abgeleitet werden, deren Begriffe als Variablen zu Meßinstrumenten operationalisiert werden, deren Reliabilität und Validität in Stichproben geprüft werden muß, die zur Vermeidung von Zirkularität unabhängig von den Stichproben für die Hypothesenüberprüfung sein müssen, die dann entweder bestätigende oder widerlegende Ergebnisse zeitigt, so daß im letzteren Falöl eine Deutung entwickelt werden muß, die zu einer neuen Hypothese führt. In dieser Auffassung, deren Ideal im übrigen selten wirklich eingehalten wird, liegt einerseits eine starre Trennung zwischen Theorie und Daten vor, insofern die Äußerungen der zu messenden Wirklichkeit unter vorgefaßte operationale Indikatoren, Kategorien oder klassifikatorische Begriffe eingeordnet, d.h. subsumiert werden. Sobald an diesen „Äußerungen“ etwas sichtbar wird,

was unter diese vorgefaßten Kategorien klassifikatorisch nicht paßt, wird es „abgeschnitten“, denn berücksichtigt werden könnte es erst, wenn die Operationalisierungen selbst geändert würde, wozu dann eine neue Stichprobe gezogen werden müßte. Andererseits ist das Verhältnis zwischen Theorie und Daten im subsumtionslogischen Ansatz zu eng, ja zu zirkulär, denn von der Wirklichkeit, an der ja die Theorien maximal scheitern können sollen, wird nur das hindurchgelassen, was unter die Operationalisierungen der Theorie paßt, um deren Prüfung es ja letztlich geht. Diese theoriesprachliche Hermetik kommt schon einer Immunisierung der Theorie gegen die Wirklichkeit gleich, so daß der berühmte Vorwurf, es werde in dieser Art der empirischen Sozialforschung hinten herausgeholt, was vorne hineingesteckt worden sei, nicht unberechtigt ist, wenn er auch nicht alles trifft, sondern nur einen vermeidbaren *Grad* der Immunisierung.

Der rekonstruktionslogische Ansatz dagegen, in dem im Unterschied zum subsumtionslogischen eine klare Trennung zwischen Datenerhebung und Datenauswertung überhaupt erst möglich wird, wendet zur Realitätsaufschließung und zur Datenerzeugung nicht operationale Indikatoren und klassifikatorische Begriffe an, sondern verfährt in der „Sprache der Wirklichkeit“ selbst, d.h. er rekonstruiert unter Bezug auf die in der Wirklichkeit selbst operierenden Sequenzierungsregeln die je konkreten objektiven Bedeutungen bzw. latenten Sinnstrukturen von Textsegmenten. Dabei wird das Protokoll der Wirklichkeit nicht von vornherein auf die zu prüfende Theorie oder Hypothese hin durch vorausgehende „Operationalisierung“ zugeschnitten, sondern semantisch unselektiv einer vollständigen Sequenzanalyse unterzogen. Die Wirklichkeit hat also tatsächlich eine maximale Chance, die Theorie zu Fall zu bringen. Die Daten sind nicht dazu da, die Theorie, sondern die Wirklichkeit selbst *authentisch* zum Ausdruck zu bringen. Erst wenn diese unverstellte Wirklichkeit, wie sie uns in authentischen, „natürlichen“, aufgezeichneten Protokollen begegnet, durch je lückenlose Sequenzanalyse als Totalität⁴ rekonstruiert worden ist, wobei nicht ein einziger objekttheoretischer Begriff explikativ verwendet wird, sondern ausschließlich in der „Sprache des Falles“, d.h. ausschließlich mit Bezug auf die lesbaren, hörbaren und sichtbaren Zeichen und Markierungen des je zu rekonstruierenden Protokolles operiert wird, bringen wir die innere Struktur der Wirklichkeit selbst als abstrakte Sinnkonfigurationen vor uns, die dann schon als reale Strukturen den Gehalt von theoretischen Begriffen und Modell ausmachen. Theorien sind in dieser rekonstruktionslogischen Auffassung nichts anderes als die Konvergenz von Fallrekonstruktionen, als geronnene Fallrekonstruktionen.

⁴ Das übliche, leider auch von Popper in der berühmten Schrift „The Poverty of Historicism“ verwendete Argument gegen die wichtige Kategorie der Totalität, man könne sie nie erreichen, weil ein konkreter Gegenstand sich nie erschöpfend beschreiben ließe, und sie sei deshalb eine sinnlose Kategorie, ist eben von vornherein nur gültig und auch nur gemeint in einem subsumtionslogischen, klassifikatorischen Ansatz, in dem die konkreten Gegenstände unter eine beliebige Zahl von Kategorien oder Merkmalsdimensionen rubriziert werden. Sobald man aber im Modell der Sequenzanalyse rekonstruktionslogisch denkt, erfaßt man genau jene Totalität eines Falles, die in seiner Fallstrukturgesetzlichkeit verkörpert ist. Die Phänomene im Gegenstandsbereich der Naturwissenschaften sind tatsächlich als Gesetzmäßigkeiten, als Regularitäten, extern durch Gesetzmäßigkeiten determiniert, die wir in Gesetzhypothesen artikulieren. Aber die Fallstrukturgesetzlichkeiten einer Lebenspraxis bestimmen diese intern, machen als deren Lebensgesetzlichkeit den Grad von deren Autonomie aus. Diese würde in einem subsumtionslogischen Vorgehen dogmatisierend unterschlagen werden, insofern es immer schon die reelle Subsumtion unter eine externe Determination präsupponiert.

Im rekonstruktionslogischen Vorgehen ist also das Modell nicht das Ergebnis einer nach Zweckmäßigkeitsgesichtspunkten gerechtfertigten Konstruktion des Wissenschaftlers, das von außen an die Wirklichkeit herangetragen wird und sie mehr oder weniger zweckmäßig abbildet, sondern immer das Ergebnis einer Rekonstruktion von Sequenzen. Insofern verkörpert es Strukturgesetzmäßigkeiten der Wirklichkeit selbst, sie operieren in der Wirklichkeit.

Der subsumtionslogische Ansatz in der traditionellen Sozialforschung lehnt sich an ein – so gar nicht zutreffendes – Verständnis der naturwissenschaftlichen Forschung als „normal science“ an. Und in der Tat bildet er eine wesentliche Komponente des Vorgehens in den Naturwissenschaften, der Natur der Sache entsprechend. Das liegt aber daran, daß im naturwissenschaftlichen Gegenstandsbereich tatsächlich die erscheinende Wirklichkeit in der Regel nicht rekonstruiert werden kann, weil sie in sich ja nicht sinnstrukturiert ist, also nicht nach bedeutungserzeugenden Regeln selbst sich protokolliert, wie die sinnstrukturierte Wirklichkeit der Humanwissenschaften das tut. Der Naturwissenschaftler muß zwar auch Protokolle seiner Beobachtungen und Messungen herstellen und diese sind natürlich auch sinnstrukturiert, aber er protokolliert dabei nicht eine in sich schon ein Protokoll darstellende, sinnstrukturierte Wirklichkeit, sondern er subsumiert die für ihn relevante Wirklichkeit unter operationale Indikatoren, unter durch Theoriebildung erst ermöglichte Meßinstrumente bzw. er gewinnt seinen Gegenstand erst durch vorausgehende relevante Beschreibung und Präzisierung. Allerdings scheinen sich die Verfahren der zeitgenössischen Molekularbiologie bei der Dechiffrierung von DNA-Strängen und RNA-Matrizen als sequentiellen Strukturen zumindest vom Vorgehen her sehr stark einem Modell der rekonstruierenden Sequenzanalyse zu nähern, das auch den Kern der objektiven Hermeneutik ausmacht.

Sequenzanalyse und Rekonstruktionslogik sind miteinander verschweißt, das eine ist ohne das andere nicht möglich. Deshalb sind auch alle gegenwärtig unter „qualitativer Sozialforschung“ rubrizierten Verfahren, die nicht dem Modell der Sequenzanalyse entsprechen, letztlich keine rekonstruktiven, sondern subsumtive Auswertungsverfahren. Erst eine Dimensionierung der Methoden der Datenauswertung nach der zentralen Unterscheidung von Rekonstruktionslogik und Subsumtionslogik vermag hier Klarheit zu schaffen. In der Rekonstruktionslogik wertet man sequenzanalytisch in der Sprache des Falles aus, in der Subsumtionslogik klassifiziert man unter theoretische Begriffe. In der Rekonstruktionslogik ist das Verhältnis von Theorie und Daten dort scharf getrennt, wo es in der Subsumtionslogik tendenziell zirkulär ineinanderläuft: Die Daten haben als semantisch unselektive Protokolle der Wirklichkeit in der rekonstruierenden Sequenzanalyse eine maximale Chance gegen die zu prüfende Theorie, die Konfrontation der Theorie mit den Daten ist nicht durch die Vorabfilter der Operationalisierungen gemildert. Dagegen besteht eine vollkommene Ungetrenntheit zwischen Theorie und Daten im rekonstruktionslogischen Ansatz dort, wo im subsumtionslogischen die Getrenntheit sehr groß ist: Die theoretischen Begriffe und Modelle lassen sich vollständig auf sequenzanalytisch rekonstruierte Strukturen zurückführen, die als Totalitäten gelten können. Theorien sind geronnene Fallrekonstruktionen. Der Übergang von Theorien zu Daten und zurück bildet in der Auswertung in sich eine Totalität, beide Bereiche lassen sich unter diesem Gesichtspunkt gar nicht mehr voneinander unterscheiden. Dagegen ist die Auswertung im subsumtionslogischen Ansatz durch die Operationalisierung schon „vorprogrammiert“. Sie vollzieht sich nach der Datenerhebung letztlich

nur noch mechanisch. Den Daten stehen die Eigenlogik und Überschüssigkeit der theoretischen Begriffe gegenüber. Sie gehen nicht einmal in den Operationalisierungen des fundamentalen und derivativen Messens auf, und in den Verfahren des konventionellen Messens oder des „measurement by fiat“, über das die Humanwissenschaften nicht hinauskommen, wird in den subsumtionslogischen Ansatz noch einmal zwischen Theorie und Daten eine – willkürliche – Trenngrenze der auf bloßer Vereinbarung beruhenden Konventionalisierung eingeführt.

15. Authentizität der Fallrekonstruktion und Falsifikation durch Sequenzanalyse statt Operationalisierung theoretischer Begriffe, Prüfung der Validität und Reliabilität von Operationalisierungen und statistische Hypothesenüberprüfung

Was über die Differenz zwischen einem subsumtionlogischen und einem rekonstruktionslogischen Ansatz gesagt wurde, gilt auch für diesen Gesichtspunkt, unter dem die forschungslogische Differenz der objektiven Hermeneutik zu anderen Methodologien der Humanwissenschaften allgemeiner gefaßt wird. *Authentizität bzw. Gültigkeit der Ausdrucksgestalt* ist ein wichtiger Grundbegriff der objektiven Hermeneutik. Er hat für sie ungefähr den Stellenwert, den der *Rationalitätsbegriff* für die Handlungstheorien hat. Gewöhnlich wird der Begriff der Authentizität für die Relation zwischen Selbstbildern eines Subjekts und dessen wirklichen Lebens, für die Gültigkeit eines Dokuments oder für die Relation der Gültigkeit zwischen einem Kunstwerk und der ihm entworfenen fiktionalen Realität reserviert. So z.B. in den Ästhetik-Wissenschaften und in den historischen Wissenschaften. Der Authentizitätsbegriff der objektiven Hermeneutik ist umfassender und allgemeiner. Er bezieht sich ebenfalls auf die Relation der Gültigkeit, deren Gegenbegriff der der Fälschung oder Ungültigkeit wäre. Aber als Grundbegriff der objektiven Hermeneutik ist ganz allgemein die elementare Relation der Gültigkeit zwischen jeglicher Ausdrucksgestalt und der in ihr objektiv verkörperten Lebenspraxis gemeint. Jede Ausdrucksgestalt – und verkörperte sie ein noch so beschädigtes, noch so pathologisches Leben – besitzt in mindestens einer Hinsicht Gültigkeit. Sie verkörpert immer noch authentisch das Verfälschte, das Mißlingen, das Beschädigte, das Verrückte, sonst würden wir es als solches gar nicht erkennen können. Selbst ein vollkommen verderbter, bewußt gefälschter, nicht einmal mehr eine tatsächliche Pathologie gültig verkörpernder Text würde immer noch eine Relation der Gültigkeit zu dieser Fälschungspraxis aufweisen.

Diese grundlegende Authentizität von Ausdrucksgestalten, Texten und Protokollen hat demnach eine dialektische innere Struktur. Ihre Gültigkeitsrelation verweist selbst bei jenen äußersten Beschädigungen einer Praxis, die in ihr sich gültig verkörpern, noch auf die Möglichkeit des Anders-Seins dieser beschädigten Praxis. Denn indem dieser Rest an objektiver Authentizität qua Ausdrucksgestalt nicht getilgt werden kann, bildet er gewissermaßen jenseits der Verdüsterung des Subjekts noch die Nabelschnur zur objektiven Vernunft. Er enthält damit ein Selbstheilungspotential der Praxis, das sie sich in dem Maße eröffnen kann, in dem sie sich der objektiven latenten Sinnstruktur der Ausdrucksgestalten ihres Beschädigt-Seins zu stellen vermag.

Die zuvor referierten geläufigen Verwendungen des Begriffs von Authentizität in

den Geisteswissenschaften sind diesem Grundbegriff nachgeordnet, sie setzen seine Gültigkeit voraus. Denn erst, wenn eine objektive Authentizität von Ausdrucksgestalten nachgewiesen ist, macht es Sinn und wird es methodisch überprüfbar zu fragen, wie authentisch ein bewußt hergestelltes Werk oder Dokument, ein Bild einer Praxis von sich selbst ist. Auf dieser nachgeordneten Ebene der Verwendung des Begriffs geht es darum, wie gut eine in einem „gemachten“, edierten Text je angestrebte Verkörperung praktisch gelungen ist.

Fallrekonstruktionen beziehen ihre Geltung aus der methodisch expliziten und in ihrer Geltung überprüfbaren, sequenzanalytisch verbürgten Rekonstruktion jener Gültigkeit oder Authentizität, die jeglicher Ausdrucksgestalt durch Regelerzeugung der Sinn- und Bedeutungsstrukturen basal zukommt. Sie stellen in sich wiederum Gültigkeit beanspruchende Ausdrucksgestalten der Explikation des objektiven Sinns jener primären Ausdrucksgestalten dar, die der Gegenstand der methodisch kontrollierten Rekonstruktion waren. Die empirische Wahrheit der Fallrekonstruktion der objektiven Hermeneutik ist also nicht eine Funktion isomorpher Messens im Sinne der Zuordnung numerischer Relationssysteme zu empirischen, durch operative Manipulationen erzeugten Relationssystemen, also nicht eine Relation der Abbildung oder der Korrespondenz zwischen Begriff und sinnlichem Eindruck bzw. Erfahrungsdatum, sondern eine Funktion der Authentizität einer objektiv gegebene Authentizität von latenten Sinnstrukturen rekonstruierenden und explizierenden Sequenzanalyse. Diese wird gesichert, indem, wie gezeigt, an gegebenen Textsegmenten ein lückenloser sequenzanalytischer Nachweis geführt wird, und nicht, indem willkürlich einzelne Sequenzstellen als zur Klassifikation geeignete Belegstellen herausgegriffen werden.

Genau aus dieser Konstellation folgt, daß es für die rekonstruierende Sequenzanalyse ein Problem der Validität und der Reliabilität von Meßinstrumenten oder von Operationalisierungen nicht geben kann. Eine solche Frage an sie heranzutragen, wäre in sich schon ein Kategorienfehler vergleichbar dem, nach der Musikalität von Steinen zu fragen. Die Gültigkeit von Sequenzanalysen läßt sich unmittelbar am Text bzw. Protokoll selbst direkt und lückenlos überprüfen. Eine weniger vermittelte, direktere und zwingendere Gültigkeitsüberprüfung läßt sich in den Humanwissenschaften gar nicht denken. So dreht sich die üblicherweise hermeneutischen oder interpretativen Verfahren gegenüber ausgesprochene oder stillschweigend unterstellte Verdächtigung zumindest eines Restes von subjektiver Beliebigkeit in der Auslegung im Vergleich zur angeblichen Ein-Eindeutigkeit standardisierter Meßverfahren im Falle der objektiven Hermeneutik um: Im Lichte ihrer Sequenzanalyse erscheinen nämlich die standardisierten Messungen als Abstraktionen von jener direkt an der „wörtlichen“, authentischen Ausdrucksgestalt vorgenommenen Sinnrekonstruktion, der sie letztlich, wollen sie überhaupt noch den Gegenstandsbereich der sozialen Welt analysieren, auch verhaftet sind, ohne es allerdings zu explizieren. Nur *weil* sie davon abstrahieren müssen, um die für die Bewältigung großer Fallzahlen und großer Datenmengen und die für die vermeintliche Objektivität von diagnostischen Individualkennungen bezüglich eines „geeichten Maßstabs“ unumgängliche Standardisierung in der Datenerhebung und – auswertung zu erreichen, müssen sie, gewissermaßen artefiziell und sekundär, ein erst jetzt entstehendes Folgeproblem der Reliabilität und Validität standardisierter Instrumente lösen. Das ist als Lösung eines der Logik des subsumtionslogischen Ansatzes

immanenten Problems auch ganz folgerichtig und in Ordnung. Es wird diese Lösungsstrategie nur falsch, ja geradezu ein Kategorienfehler, wenn sie sich aus dieser Immanenz unkenntlich herauslöst, verselbständigt und so zum methodologischen Normalfall deklariert wird, an dem jegliche Methodologie und methodische Vorgehensweise zu messen sei. Deshalb kann man an allen Methoden der „qualitativen“ Sozialforschung, die das Problem der Reliabilitäts- und Validitätsprüfung für sich übernehmen, dieses Merkmal als zuverlässigen Indikator dafür ablesen, daß sie dem Modell der rekonstruktionslogisch verfahrenen Humanwissenschaft nicht zugehören und hinter dem Terminus „qualitativ“ sich nur irgendeine tatsächlich „weichere“ Methode der Datenerhebung oder -auswertung verbirgt.

Vergleichbares gilt für das Problem der Operationalisierung. Es wäre unsinnig, an die objektive Hermeneutik den Maßstab der Operationalisierung theoretischer Begriffe anzulegen. Wenn Begriffe und Modelle einer Theorie die Realität treffen, dann muß ihr Gehalt zwingend aus den Sequenzanalysen der entsprechenden Gegenstände herauspringen. Wofern das nicht der Fall ist, sind auch diese Begriffe nicht gültig. Es ist auch nicht so, das – wie üblicherweise angenommen – erst die Vergegenwärtigung des Gehalts oder der Bedeutung der theoretischen Begriffe im Horizont des theoretischen Argumentationszusammenhangs, dem sie angehören, den Blick des Sequenzanalytikers auf „Lesarten“ oder Zusammenhänge aufschließend lenken würde oder könnte, die ihm sonst verborgen blieben. Eine detaillierte, unvoreingenommene, das Prinzip, daß jede les-, hör- und sichtbare Partikel eines Protokolls explizit einzubeziehen ist, befolgende Sequenzanalyse, ist der sicherste und zuverlässigste Weg dazu, die Aufmerksamkeit auf tatsächlich vorliegende Zusammenhänge zu lenken. Würde man nämlich sich über das im Protokoll Markierte hinaus durch theoretische Begriffe dazu verführen verlassen, in die Protokolle etwas hineinzulegen, was zwar mit ihnen kompatibel ist, was aber durch ihre konkrete Gestaltung nicht erzwungen ist, d.h. in ihnen nicht markiert ist, dann würde die Sequenzanalyse unter der Hand zur unaufschlußreichen, in sich zirkulären Subsumtionslogik verkommen und ihre aufschließende Kraft einbüßen.

Etwas anderes ist es mit der Forschungspsychologie der Produktion von Einfällen von Lesarten, die zu einem Protokollausschnitt passen. Zu ihrer Produktion ist alles erlaubt, was nützlich ist. Und es wäre geradezu aberwitzig, wenn der objektive Hermeneut dabei sein ebenfalls zu seinem Leben gehörendes Wissen über Theorien als eine Erfahrungsquelle unter vielen anderen künstlich nicht anzapfen würde. Aber das hat nichts mit der forschungslogisch einzig relevanten explikativen Verwendung theoretischer Begriffe in ihrer logischen Funktion als theoretische Begriffe zu tun.

Es liegt auch etwas anderes vor, wenn es nicht um die objekttheoretischen Begriffe geht, in denen die in den Fallrekonstruktionen entzifferten Strukturgesetzmäßigkeiten sich formulieren lassen, sondern um jene konstitutionstheoretischen Begriffe, in denen sich die objektive Hermeneutik als Methodologie und nicht als forschungspraktische Kunstlehre der Bedingung der Möglichkeit nach begründen läßt. Diese Begriffe und die Theorien, die mit ihnen formuliert werden, wie z.B. die modernen Theorien der Syntax, der Sprechakttheorie, etc., müssen in ihrer Geltung schon immer in Anspruch genommen werden, ohne daß sie konkret in der Sprache des Falles sich ohne Rest nachweisen lassen. Aber mit dieser Einschränkung, die inhaltlich für die subsumtionslogischen Ansätze ebenso gilt, kehrt der rekonstruktionslogische Ansatz nicht zu einem subsumtionslogischen zurück. Viel mehr bezieht er aus diesen Theorien die Begründung für die

Bedingung der Möglichkeit, rekonstruktionslogisch umfassend vorgehen zu können und nicht bei den Verkürzungen der Subsumtionslogik stehen bleiben zu müssen.

16. Einheit der Normativität und Deskriptivität sowie Analytizität von konstitutionstheoretischen Grundbegriffen

Das wirft die Frage des konstitutionslogischen Verhältnisses der konstitutionstheoretischen und methodologischen Grundbegriffe zur prinzipiell offenen Erfahrung auf: Wie erfahrungsgesättigt sind und können diese Grundbegriffe sein, wenn doch anscheinend ohne sie die erfahrbare Welt nicht erschlossen werden kann. Diese ist die Frage, die z.B. Karl Marx in seiner Gesellschaftstheorie im Bilde des doppelten Ganges vom Konkreten zum Abstrakten und wieder zurück vom Abstrakten zum Konkreten aufgeworfen, aber keinesfalls zufriedenstellend beantwortet hat, weil ihm die theoretische Bestimmung eines rekonstruktionslogischen Verfahren dazu fehlte. Mit den sprachtheoretischen und kognitionstheoretischen Errungenschaften der neueren Zeit, die sich die objektive Hermeneutik in ihrer Selbstbegründung zunutze macht, teilt sie hier die Prämisse einer erkenntniskonstitutiven Zirkularität, in der naturwüchsige Kompetenzen des Urteilens, die wie ein „tacit knowledge“ operieren, immer schon die Praxis anleiten, der sie andererseits erfahrungsgesättigt durch rekonstruierendes Konstruieren, durch das, was bei Piaget „reflektierende Abstraktion“ heißt, entnommen worden sind.

Für einige dieser Grundbegriffe, die sich auf die Konstitution der Lebenspraxis als Totalität beziehen, und nicht nur auf die Rekonstruktion von deren epistemischen Strukturen der Gattung, also vor allem für die Begriffe von Autonomie, Individuierung, Bewährung und Authentizität gilt in diesem Zusammenhang darüber hinaus, daß sie notwendig zugleich normativen und deskriptiv-analytischen Charakter haben und darin keineswegs ein zu beseitigender Mangel zu sehen ist, sondern ein konstitutionstheoretisch zwingender Zusammenhang. Deskriptiv-analytisch bezeichnen solche Begriffe je ein universales Strukturproblem, vor das jede Lebenspraxis in ihrem Bildungsprozeß gestellt ist und das sie, ob sie will oder nicht, zu bewältigen hat. Normativ hingegen bezeichnen diese Begriffe zugleich den Grad des Gelingens in dieser Problembewältigung, deren Dimensionierung eben immer nur vom Ende des kontrafaktisch geltenden, idealisierten Gelingens her darstellbar ist.

17. Einheit von Grundlagenforschung und angewandter Forschung statt Trennung zwischen beiden Forschungsarten

Die objektive Hermeneutik kann gar nicht anders als je fallspezifisch vorzugehen, d.h. die Sequenzanalyse direkt auf Protokolle anzuwenden, die immer je konkrete Einzelfälle verkörpern. Weil das so ist, unterscheidet sich in ihr das Vorgehen in der Grundlagenforschung grundsätzlich nicht von dem der angewandten oder praxis-immanenten bzw. praxisbezogenen oder -begleitenden Forschung. Denn in beiden Funktionen muß im Sinne der objektiven Hermeneutik auf der Basis von Einzelfallrekonstruktionen prozediert werden. Man kann das auch so ausdrücken: Im Hinblick auf die

jeweils notwendige Einzelfallbasis erscheint die Grundlagenforschung der objektiven Hermeneutik wie eine auf unmittelbar praktische Problemstellungen bezogene angewandte Forschung. Im Hinblick auf die Vorgehensweise in Form einer detaillierten Sequenzanalyse an wenigen Protokollausschnitten erscheint dagegen die praxisbezogene Forschung aus der Perspektive der objektiven Hermeneutik wie eine handlungsentlastete Grundlagenforschung. Der einzige, vernachlässigenswerte Unterschied zwischen einer Grundlagenforschung und einer praxisbezogenen Forschung im Rahmen der objektiven Hermeneutik könnte darin gesehen werden, daß in der Regel in der praxisbezogenen, „angewandten“ Forschung unter den Bedingungen der größeren Nähe zum Praxisdruck die Ausführlichkeit und Detailliertheit der Sequenzanalyse, eher noch: der Umfang der einer Sequenzanalyse unterzogenen fallspezifischen Materialien geringer ausfallen kann und im Hinblick auf diese Parameter größere Toleranzen hingenommen werden können.

18. Das Verhältnis der objektiv hermeneutischen Fallrekonstruktion zur klinischen Praxis und zur testpsychologischen Diagnostik bzw. zur szientistischen Messung⁵

Ebenso wie das abduktive Schließen auf den Ebenen der Erkenntnistheorie und der Methodologie der Erfahrungswissenschaften seine strukturhomologe Entsprechung in der alltagspraktischen Erkenntnisleistung einer Krisenbewältigung, in der neue Erfahrungen gemacht werden müssen, und in der müßigen ästhetischen Erfahrung und Gestalterschließung hat, beides Erkenntnisweisen, die wir auch unter den Titel der Intuition rücken können, bildet die objektiv hermeneutische Operation der Strukturgeneralisierung explizit ab, was implizit in diesen intuitiven Modi der Erschließung von primären Erfahrungsdaten sich vollzieht. Damit ist zugleich auch eine Affinität der rekonstruktiven Forschungslogik zur naturwüchsigen Konstitution von Erfahrung in der Alltagspraxis unter Krisenbedingungen konstatiert und komplementär dazu, daß es sich beim subsumtionslogischen Vorgehen stattdessen um eine szientistische, einer formellen Reduktion korrespondierende Verkürzung handelt, die der Umwandlung von materialer in formale Rationalität im Sinne von Max Weber gleichkommt.

Einer analogen Differenz begegnen wir, wenn wir die Grundtypen der praktischen Verwendung wissenschaftlich-methodischer Problemlösungen ins Auge fassen. Sie können *zum einen* entsprechend einer Deduktion von Prognosen aus bewährten Hypothesen abgeleitet oder entsprechend einer Erklärung durch den Nachweis einer entsprechenden Randbedingung erfüllt werden. Das setzt eine Subsumtion des Falles, im deduktiv-nomologischen Schema logisch äquivalent mit den Randbedingungen oder dem Antezedens⁶, bzw. eine Subsumtion der Konklusion unter die Kategorien bzw. Begriffe für die zweite Prämisse und die Konklusion voraus. Diese Anwendung

⁵ Der Text zu diesem Spiegelstrich ist in Teilen meinem Aufsatz „Die Methode der Fallrekonstruktion in der Grundlagenforschung sowie der klinischen und pädagogischen Praxis“, in: K. Kraimer (Hrsg.), Die Fallrekonstruktion. Frankfurt a.M.: Suhrkamp, 2000, S. S. 58–156, entnommen.

⁶ Tatsächlich werden in der Wissenschaftstheorie und auch bei Peirce im deduktiven Schlußschema die Randbedingungen oder die zweite Prämisse als Fall und die Konklusion als Resultat bezeichnet.

können wir dem üblichen Sprachgebrauch folgend eine *technologische oder manipulative* nennen. Sie erfordert nichts anderes als klare Regeln der Operationalisierung von theoretischen Begriffen, so daß eine eindeutige Zuordnung von Erfahrungsdaten zu diesen Begriffen gewährleistet ist. Ich habe nachzuweisen versucht, inwiefern das für die empirische Wirklichkeit der Lebenspraxis nur für den eingeschränkten Fall der Routinisierung möglich ist und inwiefern die Lebendigkeit und Autonomie der Lebenspraxis darin von vornherein unbegreifbar bleiben muß.

Zum anderen kann die praktische Verwendung in einer Bereitstellung von Problemlösungen bestehen, die einer konkreten Lebenspraxis für die Lösung ihrer aktuellen Krisen nützlich sind unter der Voraussetzung, daß zu ihrer Umsetzung ein wissenschaftlich kompetenter Experte gebraucht wird. Da eine Subsumierbarkeit der je fallspezifischen Krisenkonstellation unter die wissenschaftlich vorgehaltene Problemlösung von vornherein ausscheidet bzw. die je fallspezifische, in die Individuiertheit und Autonomie der in der Krise befindlichen Lebenspraxis eingewobene Motiviertheit der Krise nicht prägnant treffen kann, erfordert hier die sachgemäße Anwendung der verallgemeinerten Problemlösung auf den Fall deren Übersetzung – nicht: mechanistisch-standardisiert, sondern „dem Geiste nach“ in die Sprache des Falles. Anders ausgedrückt: Damit der Experte die wissenschaftlich begründete Krisenlösung zur Anwendung bringen kann, muß er zuvor den konkreten Fall in seiner Dialektik von Allgemeinheit und Besonderheit, in seiner Fallstrukturgesetzmäßigkeit verstanden haben. Dieses Fallverstehen läßt sich aber nicht durch Deduktion aus der anzuwendenden Problemlösung gewinnen, auch nicht – wie gezeigt – durch induktive empirische Generalisierung über fallbeschreibende Meßdaten, sondern nur durch rekonstruktive, dem abduktiven Schließen homologe Strukturgeneralisierung auf der Basis einer Operation der Fallrekonstruktion. Ein solches Fallverstehen homolog zum Prozedieren der objektiven Hermeneutik steht aber der vor die Klammer der konkreten Praxis gezogenen Standardisierung einer wissenschaftlich begründeten Problemlösung ebenso entgegen wie die Abduktion den Schlußmodi von Deduktion und Induktion. Damit also eine wissenschaftliche Problemlösung in der professionalisierten Praxis der stellvertretenden Krisenbewältigung für eine beschädigte konkrete Lebenspraxis krisenlösend produktiv werden kann, muß sie um eine erfolgreiche Operation des rekonstruktiven Fallverstehens ergänzt werden. Der professionalisierte Experte muß diese beiden wissenschaftslogisch nicht zur Deckung zu bringenden Komponenten: die prinzipiell formalisierbare verallgemeinerte Problemlösung und die fallverstehende Rekonstruktion der Krisenkonstellation, in sich vereinigen und beiden gleichermaßen gerecht werden.

Das gilt nun aber keineswegs nur für die diagnostische Einsicht in die fallspezifische Krisenkonstellation, sondern erst recht für die daraus folgende Interventionspraxis in einem Arbeitsbündnis mit dem Klienten. Es kommt dabei über die Operation des Fallverstehens hinausgehend hinzu, daß in der prinzipiell den Klienten in Abhängigkeit bringenden Asymmetrie der Intervention dessen Autonomie – bestehend in seinen „gesunden“ bzw. autonomen Anteilen – respektiert und in bindender Selbstverantwortlichkeit beteiligt werden muß, da eine Intervention nur erfolgreich sein kann, wenn es ihr gelingt, die Selbstheilungskräfte der zu behandelnden Praxis maximal zu wecken. Diese Rückbindung der Interventionspraxis an die Selbstheilungskräfte des Klienten erfordert eine ständige, dosierte krisenhafte Auflösung von – erstarrten

– Routinen und eine ständige begleitende rekonstruktive Einsicht in die je aktuelle Krisenkonstellation.

Den ersten Modus der technologisch-manipulativen Anwendung bewährten Wissens können wir als den *ingenieurialen Modus* bezeichnen. Er erschöpft sich in der subsumtionslogischen Methodik und in den Schlußweisen der Deduktion und Induktion.⁷ Der zweite, ein Arbeitsbündnis mit dem Klienten einschließende Modus der Anwendung wissenschaftlich begründeter Problemlösungen auf Krisenlösungen einer je konkreten Lebenspraxis nimmt selbst die Form einer autonomiefähigen Praxis in der Intervention im Arbeitsbündnis an. Diese *interventionistische Praxis* wird am ehesten anschaulich in der ärztlich-therapeutischen Praxis, sie umfaßt aber auch die Wissensanwendung in der pädagogischen, konsultatorischen und rechtspflegerischen Praxis. Ich nenne diese in Form einer Interventionspraxis durchgeführte Wissensanwendung eine professionalisierte Praxis.

Es sollte deutlich geworden sein, daß nicht diese letztere Wissensanwendung ein Spezialfall der ersteren oder eine spezifische Ausweitung dieser ist, sondern umgekehrt den Normalfall des Verhältnisses von Wissenschaft und Praxis repräsentiert, in Beziehung zu dem die ingenieuriale Anwendung einen Spezialfall darstellt, ähnlich dem asymmetrischen konstitutionslogischen Verhältnis von Rekonstruktion und Subsumtion. Die Einheit von Theorie und Praxis läßt sich nicht philosophisch oder erkenntnislogisch herstellen, sondern vollzieht sich – selbst praktisch – ausschließlich in der Praxis des professionalisierten, klientenbezogenen Handelns.

Insofern nun die Operationen der Fallrekonstruktion und Strukturgeneralisierung auf der Basis der Sequenzanalyse in der objektiven Hermeneutik nur die explizite, methodisch gesteigerte Version einer naturwüchsigen alltagspraktischen Erkenntnisoperation im Zusammenhang von Krise und Muße sowie einer als Kunstlehre durchgeführten Operation des Fallverstehens in der professionalisierten Praxis darstellen, gilt, daß diese Methode der Fallrekonstruktion *zum einen* diese mehr oder weniger naturwüchsigen Erkenntnisoperationen erklärt und expliziert. – Das ist im übrigen nicht identisch mit Begründen im Sinne von Einrichten dieser Praxis. Denn es ist ganz wichtig festzuhalten, daß die naturwüchsige, intuitive Operation der Fallrekonstruktion im Fallverstehen sowohl der krisenhaften Alltagspraxis als auch der Klinik sich ohne irgendeine Angewiesenheit auf eine vorausgehende theoretische Begründung im sachhaltigen praktischen Vollzug konstituiert – so wie man professionalisiert sein kann, ohne je etwas von der die professionalisierte Praxis explizierenden und theoretisch erklärenden Professionalisierungstheorie gehört zu haben.

Zum anderen eignet sich die explizite Methode der Fallrekonstruktion gerade deshalb dann, wenn die naturwüchsigen, zum Pol der Intuition hinneigenden Operationen des Fallverstehens, die ja im Vergleich zur expliziten Begründung in der objektiven Hermeneutik immer eine Abkürzung der Praxis darstellen, selbst – aufgrund dieser Abkürzung – in eine Krise geraten sind, zur praktisch folgenreichen Klärung. Man nimmt dann sozusagen eine praktische Auszeit, um handlungsentlastet in detaillierter Sequenzanalyse das intuitive Fallverstehen aus seiner Krise herauszuführen. Insofern eignet

⁷ Dabei ist allerdings immer zu berücksichtigen, daß die ursprüngliche Produktion des ingenieurial verwendeten Wissens ebenfalls die abduktiv aufschließende Strukturkenntnis von primären Erfahrungsdaten voraussetzt.

sich die Fallrekonstruktion zu einer Art methodischer Beratung in schwierigen Fällen der Praxis und zu einer Art methodischer Supervision zur Prophylaxe gegen einen verzerrenden Einschiff der Routinisierung im naturwüchsigen Fallverstehen.

Das führt zu einer eigentümlichen und in sich offensichtlich sehr mißverständnisträchtigen Konstellation im Verhältnis von (1) subsumtionslogischer Forschung und Wissensanwendung, (2) klinischer, professionalisierter Interventionspraxis und Forschung und (3) objektiv hermeneutischer, expliziter Fallrekonstruktion auf der Basis detaillierter Sequenzanalysen. Wir haben bisher immer nur den Gegensatz von (1) und (3) behandelt. Jetzt sind wir auf das Kontinuitätsverhältnis von (2) und (3) in der Dimension von Implizitheit bis Explizitheit gestoßen. Faktisch haben aber die Vertreter der klinischen Praxis aufgrund dessen, daß sie gezwungen sind, unter großem Zeitdruck wirksam abgekürzt zu handeln und mit Hilfe ihrer wirksam abkürzenden Berufserfahrung sich mehr auf die entscheidungsorientierte Risikoabwägung als auf die explizite, lückenlose Begründung zu konzentrieren, nur geringes Verständnis für den Zeitaufwand und die Elaboriertheit der detaillierten Sequenzanalyse. Sie sehen dann nicht genügend die fruchtbaren Ergänzungen, die zwischen (2) und (3) möglich sind.

Zum einen ist die explizite Fallrekonstruktion der objektiven Hermeneutik gar nicht für den Normalfall einer quasi routinisierten Intervention gedacht, sondern nur für den schwierigen Fall, in dem die abkürzende Routinisierung versagt bzw. für den Supervisionsfall, in dem die immer möglichen aberrativen Einschiffe dieser Abkürzungen überwacht und korrigiert werden sollen. In diesen Fällen, in denen die routinisierte professionalisierte stellvertretende Krisenbewältigung selbst in eine potentiell immer drohende Krise geraten ist, kann paradoxerweise das scheinbar viel zu zeitaufwendige Verfahren der expliziten Fallrekonstruktion eine nicht nur präzisierende, sondern auch zeitökonomische Klärung herbeiführen.

Zum anderen kann die Aneignung des expliziten Verfahrens der Fallrekonstruktion auch dem Praktiker dabei helfen, sich über sein naturwüchsiges Fallverstehen Rechenschaft abzulegen und daraus eine Rechtfertigung für ein scheinbar unwissenschaftliches, weil mit hohen intuitiven Anteilen bzw. Anteilen einer Kunstlehre behaftetes Vorgehen zu beziehen.

Das ist schließlich zum dritten eine wichtige Grundlage, die Gegensätze zwischen (1) und (2) so herauszuarbeiten, daß nicht automatisch dabei die szientistischen, subsumtionslogischen Verfahren als den „informelleren“ klinisch-fallbezogenen überlegen oder als deren Modell erscheinen. Anders ausgedrückt: Die Fallrekonstruktion der objektiven Hermeneutik rechtfertigt den fallbezogen und tendenziell intuitiv-berufserfahren vorgehenden Kliniker gegen den nur vermeintlich präziseren, subsumtionslogisch testenden Wissenschaftler und eröffnet ihm ein konkurrierendes Modell der Begründung, Rechtfertigung und Präzisierung seiner klinischen Verfahren, nicht nur generell auf der Ebene der allgemeinen theoretischen Begründung, sondern auch speziell und konkret im strittigen, schwierigen Einzelfalle.

Man kann dieses Verhältnis von (1) subsumtionslogischer Szientifik in standardisierter Forschung, (2) klinischer, fallverstehender Interventionspraxis und (3) objektiv hermeneutischer, expliziter Fallrekonstruktion auch als eine Dreiecksbeziehung sehen, in der drei typischen „Beziehungskonflikte“ auftauchen: (a) zwischen (1) und (2) der Konflikt zwischen einem angeblich operational präzisen, aber faktisch fallunspezifischen Vorgehen auf der einen Seite und einem intuitiven, auf Berufungserfahrung sich

verlassenden, in Gestaltwahrnehmung prozedierenden Vorgehen auf der anderen Seite; (b) zwischen (2) und (3) der Konflikt zwischen einer entscheidungsorientierten, der Realität des Zeitdrucks gerecht werden Abkürzungsroutine einerseits und einem detaillierenden, explizit handlungsentlasteten Verfahren in „Langschrift“ andererseits; und schließlich (c) zwischen (1) und (3) der Konflikt zwischen dem subsumtionslogisch-klassifikatorischen Paradigma auf der einen und dem rekonstruktionslogischen, strukturanalytischen Paradigma auf der anderen Seite.

Die Verfahren der sequenzanalytischen Fallrekonstruktion und der Strukturgeneralisierung können generell als Explikation der Operationsweise der naturwüchsigen Konstitution von Erfahrung gelten. Um so erstaunlicher ist es, wie lange es dauert und wie schwer sich die Erfahrungswissenschaften tun, bis sie die Beschränkung auf Deduktion und Induktion aufgeben und abduktive Verfahren ausbilden. Es ist, als ob die Aufrechterhaltung von Wissenschaftlichkeit überfordert wäre, wenn sie zu sehr in eine Analogiebeziehung zu den naturwüchsigen Erkenntnismodi gerät. Diese Motivierung der Reserve ist durchaus ernst zu nehmen, so lange jedenfalls, solange die Zulassung dieser Nähe nicht zugleich einhergeht mit einer durch hinreichende Explizitheit des methodischen Vorgehens und seiner theoretisch-methodologischen Begründung gesicherten Distanz.

Hat man sich diese Analogiebeziehung und Affinität der expliziten Fallstrukturrekonstruktion zu der Konstitution von Erfahrung außerhalb der Wissenschaften sowohl in Alltagspraxis als in ästhetischer Muße einmal vor Augen geführt, dann ergeben sich überraschende Entsprechungsverhältnisse zu vielen Erkenntnisleistungen, die den subsumtionslogischen Rahmen sprengen müssen: Die ästhetische Erfahrung ist schon mehrfach genannt worden. Ihr liegt analog die basale Naturerfahrung zugrunde. Der diagnostische, physiognomische Blick auf Natur und Mitmenschen gehört dazu, die intuitiven, spontanen ersten Reaktionen auf eine Krisenkonstellation, die Art und Weise, wie man auf Neues sich einstellt und bereit ist, Routinen zu überschreiten, sei es im Spiel oder gar im Ernst.

“AND SINCE IT IS THE FIRST TIME ...” ON THE EDUCATIONAL SHAPING OF THE BODY OF THE CLASS

AXEL FEHLHABER AND DETLEF GARZ

The central idea leading our following reflections is certainly not new: It is that teaching means being involved in an important and ambitious task as a person, not only to have a job or “occupation / carrying out a profession.” The famous German philosopher and educator Otto Friedrich Bollnow already knew and was able to demonstrate that teachers have to present their innermost existence, their “Stärken und Schwächen” (strength and weakness), to their students, and cannot hide in their field of teaching competence alone. Therefore they become susceptible and vulnerable¹ [Bollnow 1959]. A person who has to face the hazard of exposing themselves could be hurt and discriminated against.

From a sociologist’s point of view, Axel Honneth sees in constellations like these a “disrespect” in the sense of a misrecognition of the person, that is, a “discrimination of individual patterns of life.” If “the significant others” (in our context the members of a school class) contribute to the “evaluative degrading of certain patterns of self realization,” then the individual experiencing such a social devaluation also suffers a loss of personal self-respect (*Selbstachtung*) [Honneth 1990: 1047].

In principal one could respond to this fact with an adequate professionalization of teachers in building up a “habitus” in the sense of Bourdieu – like other professions (e.g. pastors, therapists and medical doctors) are already striving for – this could help them handle their practice.

The numerous difficulties coming from these attempts were recently documented in detail [cf. Combe and Helsper 1996]. They seem to be so extensive that lasting results seem unattainable in the short term. It needs to be remembered that the moral infrastructure of society does not maintain a protecting mechanism for teachers. Even though this group was assigned the mandate of mediating the central educational value of formation (*Bildung*), it is inadequately prepared.

Mindful of these widespread difficulties, our article will focus beneath this level, so to speak, focusing our thoughts on the analysis and subsequent better design of the instructions. We have shown elsewhere [cf. Fehlh Haber and Garz 1998; Aufenanger and Garz 1996; Garz 1996; 2000] how such a research strategy is led by certain intuitions.² To sum up, we argue at this point that empirical qualitative research methods can be used in these contexts to connect academic (university based) education with ped-

¹ Here we want only to point out the connection of hurt and illness of teachers.

² Regarding related reflections in social work, see Hauptert and Kraimer [1991]; regarding adult education, see Kade [1990].

agogical practice. This involves teaching the establishment and implementation of a “hermeneutical propaedeuticum” at universities, namely:

- to pass on qualitative research methods,
- with practice cases relating to the practice in question,
- leading to the building of a habit,³ which will in turn
- improve the competence of acting in accordance with practice.

The following considerations are supposed to show how an interpretation of a lesson focusing on a relatively short, but – for a school lesson – central interaction, will lead to a better understanding of the events. The method used is the objective or structural hermeneutics developed by the sociologist Ulrich Oevermann at Frankfurt University [cf. Garz and Kraimer 1994, Garz 1997; Oevermann 2001; Wagner 2001; Wernet 2000].

CASE STUDY

The following interpretation is based only on the objective action and interaction problems (the deep structure) that emerge in the protocol at hand. The interpretation sets aside the subjective intentions (the surface structure) of the protagonists. Hence the interpretation becomes independent of the involved people and can stress the mechanism of action and interpretation appearing between teachers and students. In doing such we are not interested in assigning guilt to the participants.

But only the abstraction draws attention to actual problems and difficulties in terms of actions and interactions. The interpretation refers to the complex interactions in school on a theoretical level and clarifies the danger of ignoring the mixture of the interactive relationships clearly. To understand what goes on in schools requires an understanding of the “constitutive brokenness” of the teacher in being both a person and having a role. That is, we are not talking about conflicts in terms of different roles [cf. Dahrendorf]; rather we situate the conflict between the teacher in her or his wholeness and her or his acting according to different roles.

There is another reason to speak about the brokenness of the teacher’s personalities. The teacher has to, on the one hand, handle the demands of being rational without “forgetting” that this demand rests on a bodily and emotional basis. This basis can only be eliminated at the price of a rationalistic lesson, a lesson such as the concept of programmed instruction, which gained fame in the 70s and perished for good reasons shortly after. The opposite concept, on the other hand, also proves deceptive. This is the idea that a lesson is supposed to be structured only on a bodily-emotional orientation. In this context the body and its emotions are going to be elevated leading to the fatal concept of an “educational Eros” as the center of instruction in classes⁴ [cf. Maasen 1995].

³ We define habit not technically, but as a “quality of the soul,” which is achieved through drill.

⁴ Parsons fixed the change from the affective thought in the role oriented behavior at the moment of the child’s transition from family to school, in the scene of “Where it was, I shall become.”

The aim has to rather be a combination keeping both elements together and controlling them in the sense of a contradicting unit (*widersprüchliche Einheit*). Then the permanent creation of the new can be understood indeed as an expression of an “irrational,” charismatic and emotional act. But this process is inseparably interconnected with reason, knowing that charisma has to focus on the subject of education and not on the teacher.

THE CASE

The case interpreted here presents a lesson in religious education, given by a (male) teacher in the seventh grade. Our interpreted sequence starts immediately at the beginning of the lesson, but is subsequent to a non-documented scene; it ends in singing a canon two times.

Every part of this earth / is holy to my people

Several ways of reading (i.e. interpreting) this line are possible:

R1⁵ This way of beginning a lesson is unusual both for teacher and students. It might be the first time that the teacher wants to start a lesson with a song. If so, there should have been a prior, more or less long lasting, process of finding mutual consent regarding the common task. At the very same time the teacher should have studied both the text and melody of the canon at least once with the class.

That would actually mean that we are not confronted just with an opening sequence, but that the total content of this lesson would be filled with singing in the context of religious education, with exercising and singing this song. It is to be expected that the students will express themselves positively or negatively towards the whole scenario.

R2 This way of starting the lesson is not a unique happening, but it is the usual beginning for this teacher with the religious lesson, at least in this class. It is usual in this class to start together with a song. This song (the canon) is possibly new for the students. It is to be expected, if this reading is true, that the teacher will get straight to the subject of the lesson.

One question remains: What does it mean to start a lesson with a song or a canon in religious education? It is not far fetched to think that the teacher utilizes the church liturgy so that the idea of religious education differs from the other subjects. On the one hand, the emotions of the students will be incorporated through singing. On the other hand, there might be an integration of the students into the group, calling them out of their singleness into the community or even into a congregation. A third disciplinary

⁵ With R1, R2 ... Rn we characterize different ways of reading (*Lesarten*) a document that are created as thought experiments with the implication that they must be compatible with the text.

effect can be a byproduct of this way of opening a lesson, insofar as the students follow the teacher and act together to build a group, a body of sound, to “Praise the Lord,” (we assume that songs in religious education are thematically related to God) they follow the will of the teacher.

Hence singing a song would be part of an opening ritual, in which the teacher executes the act of discipline in the beginning of each religious education class. The meaning of the word and the musical form of the “canon” suits the aspect of disciplining. In Greek, the word “canon” means “guide line.” A canon is the “sternest form of music, of musical imitation; it is faithful to the pitches” [DUDEN]. The canon has the following important elements:

The repetition that allows the climax of the canon and its musical elements to appear at the end. At the end of a canon there is the harmonious melting together of the individual elements. Clinging to the guideline of the theme needs to be endured by the group itself – in replying to the singing or by newly started groups. For that purpose it is necessary to be more self-disciplined in comparison to singing a simple song together. Last but not least, it is necessary for a conductor to give cues to the groups, so that the singing, the cooperation and the opposition of the diverse groups will not lead to chaos but will develop into a compact body.

A first conclusion: The form of the canon with its requirements disciplines the students in order to accomplish the task of social cooperation. It demands at the same time listening to others and ignoring them. This is made easy on the one hand by the task of repetition and on the other hand by following the instructions of the conductor. In this sense it is a pseudo-interaction, since each group is submitting to a “behavioral-goal.” Actually they are not allowed to listen to each another with regard to accomplishing their goal successfully together and in unity. This way of opening the lesson can be classified as close to a rite: Imitating, repeating and “pseudo-interaction” seem to be elements of this case structure.

Imitation and repetition as elements of the case structure include the following:

The canon as a musical form requires interaction between teacher and students with mainly imitative and repetitive elements. The teacher conducts to a certain degree a repetitive pseudo-interaction. A subject with musical and religious content should be repeatedly and exactly copied by the students. In the center of this sequence there is not acquisition as an act of a discursive debate, but acquisition as an act of emotionally burdened imitation and a repeated copying of a musical guide line.

An important question is whether these elements will be repeated throughout the whole lesson. Will imitation and repetition be reproduced on other levels of the lesson as well?

The text of the canon has its origin in a Native American tribe and a nature religion. At first sight it is surprising that it is used as canon to sing in a Christian (most likely evangelic) religious education class. It becomes less remarkable once we learn that this canon – connected with meditative dancing – is well received at evangelic church holy days.⁶

The fact that this text was sung during a religious lesson, refers to the fact that the

⁶ We have gotten this hint from a participant in our research colloquium who had participated in such a church holy day event.

teacher understands his religious lesson in its tendency to be “multi religious.” The slogan “Preserve the Creation” seems to be an important leading principle in the lesson.

The previously worked out element of imitation also seems to be present at the content level of the effective canon, so that we can already speak of the reproduction of the elements of the case structure. Both teacher and students imitate a nature religion’s understanding of reality, which, by the way, is not compatible with the point of view of Christianity.

From of a Christian theological point of view, this text is not quite unproblematic because the act of creation and not the creator Himself is declared to be holy. In particular: The “I” that speaks here is itself a part of the people. It is a part of the tribe, which speaks of honoring “this” – his – earth. It is not about the globe, but the environment, the soil of the particular population, to whom the “I” belongs.

Problems for a lesson in religious education would arise if the “I” of the text were equalized – in a simple and uncritical analogy – with the Judeo-Christian God. He would ask His people, the Christians, not to honor him, but His creation. Out of this it follows that the creation would be an idol and therefore it would oppose the Christian teaching of the holiness of God. It is also problematic as we do not know whether the author is speaking about children or youths, before or after the turning point of religious confirmation. Do some children belong to other religions and participate in the theology lesson because of a lack of alternatives and are now counted amongst the people of the Christian God? If so, an absurd expectation is asked from them (“Every part of this earth be holy unto thee”), because they, on the one hand, do not (yet) belong to the addressees (“my people”). On the other hand, the failing of this expectation will have to be noticed in reality. The exploitation of the earth was ironically enforced by the Christian European culture and its economic misbehavior. They legitimated themselves by the divine call to make the earth submit [Gen.1.28]. The students should first understand that the text does not include a Christian-universal expectation, but that it is about the living space of a tribe and a people they do not belong to.

The only expectation in singing this text in a Christian theology lesson could be a “world” ecumenical one, namely to “honor” another religion for being aware of its own position. In this case it would have to be expected that the teacher discussed the text of the song properly with the students and showed them the differences between the Christian understanding of reality and God and that of the Native American text. This, in turn, would have been asking a lot from seventh graders.

If this is not the case, if the canon is sung just for the sake of imitating the Native American reality, then we can speak about an opening ritual – even if with “good intentions.” But the effect of disciplining will after all be domineering, stressed through the character of the commentary, which is mediated by the text. Its origin and interpretation cannot be understood by the students.

What is the outcome of this kind of disciplining? The students will submit to the will of the teacher. He conducts them. There is a paradox: the students are split into groups to act against each other in order to build a harmonious group. They rebuild, so to speak, “the people” through their polyphonic voices on the content level of the text. The Christian religious education class, conducted by a “prophet” of the God of Creation or the “Creator” himself melts together in the end into a harmonious union, a “peoples’ body.” On the level of instruction the teacher creates in an analogical man-

ner an “educational people” and an “educational body” – the class. On this level we can speak of the teacher copying the “I” of the text into a Christian context: The students will be instructed by the teacher to sing a song with a nature religion content. They are encouraged in a certain way toward a disciplined copying (concerning an understanding of reality not understood by them) as a form of getting closer to the content.

Teacher: *Great, wonderful. So, now sit (excuse me?) down.*

(15 seconds of slight restlessness. 2:41)

The teacher speaks after ending the canon. He asks the students to be seated again and gives a two word feedback about the good performance of the canon by the students.

“Great” seems to relate to the well performed cooperation of the different groups, while with “wonderful” he seems to express an aesthetic judgment with regard to singing in harmony. The shortness of the feedback stresses the impression that the singing of the canon is indeed an opening ritual.

The teacher ends the common opening ritual with “so” and leads to the actual lesson by asking the students to sit down. It becomes obvious that the students were seated at first, then were asked to stand up and sing standing. Now they take a seat after being asked again. The opening ritual relates to the body of the student in more than one way. The teacher, due to his authority, enforces a change regarding the students’ bodies in the classroom. Because this modification happens collectively the teacher with this ritual precisely forms a “class body.” It seems as if later on the teacher wants to give a friendly touch to the order he gave to the students, adding the word “please” to his words quickly and at a quite inappropriate moment. But: the transcript signalizes at this point that the utterance of the teacher was hard to understand, therefore this interpretation is speculative.

Overall it seems that the teacher wants to use the discipline of the performance by the students in singing the canon to segue to the start of the lesson. In doing this he seems to be successful. There is only a short period of restlessness as the students return to their seats.

Teacher: *Somebody has asked when we are going to sing this song “Swing low, sweet chariot.”*

All students in confusion: *No, no, no.*

Oh yes, oh yes. Yes. (Hitting on the desks)

During this time of transition one student obviously asked the teacher a question that he passes on to the whole class. Immediately the class splits into two parts, one opposing and one agreeing group. It is remarkable that the teacher phrases his question anonymously. It is not a question asked by a concrete student, but “somebody” has asked, more precisely: somebody is actually asking while the teacher is asking the class. To whom does this somebody refer?

- R1 The teacher can not possibly define who asked the question. In the interest of making a collective democratic decision possible it would have been appropriate for the teacher to demand who asked the question first and then pass it on to the students.
- R2 There is the possibility that the teacher for some reason did not want to mention the student who had asked the question in the first place. S/he is not liked by the classmates (s/he is very eager or others ignore him/her). The teacher wants a straight answer, not influenced by the popularity of the one who asked and not influenced by antipathy. Another reason could be that the teacher himself has an antipathy against the student; he degrades her/him by making the question anonymous with the word *somebody*.
- R3 The question was asked by the teacher. He would like to sing the song with the students, but is afraid of his suggestion being rejected, for example because he has a bad relationship with the group. For that reason he presents himself anonymously.

All three readings have in common that the teacher keeps the origin of the question in the dark, in keeping the asker anonymous. The person who asked the question is released of the responsibility of standing up for her or his suggestion and demonstrating it to others. Thus for a democratic decision the opposing person is missing, and the class is not able to relate to somebody either in agreeing or rejecting. In this case the students have the right to reject a serious answer or not to give their personal opinion toward a question “without an origin.”

- R4 Another rather isolated way of reading concerning the utterance of the teacher shows that the students, if they reject giving a serious answer to the question, can say that they have not been asked as individuals: the addressor himself could be meant by “*somebody*.” The teacher addresses the students in a neuter manner and from a great distance. Instead of asking ^{*7} “When would you like to sing the song...?” or * “You have been asked when we...” or * “We have been asked when we...” a diffuse neuter, a *Somebody*, is asked. This *Somebody* defines a collective, a class or – in the context of our interpretation – the “educational body of the class.”

It is clear that the elements of “repetition” and “imitation” have recurred. The teacher passes on the question asked by the student in an unfiltered manner, as a speaking-tube. It comes to a pseudo-interaction between teacher and student, caused by the diffusion between the addressee and addressor. The class is addressed as a union, starting in the second half of the sentence, more exactly: as a “we,” since the teacher feels himself in some way a part of it. This is astonishing because the teacher just repeats or copies the question of the student. In this question the “we” has a particular speaker, the class

⁷ With * we describe the different thought experimental action options which are possible in this context but not established in this existing text.

as a whole of the different students. The teacher becomes a “part” of the *we* through repetition and imitation. His distance to the class melts together to a unit with the class. The whole sequence draws a picture of a transition from the diffuse distance of the teacher to the class toward a diffuse unit of the teacher with the class.⁸

The diffusion caused by the anonymity of the questioner and the addressee opposes the democratic openness and transparency that was purposely initiated by the teacher. This is reflected in the immediate chaos of agreeing and disagreeing. The teacher confuses the class with his question. The class is disciplined by the canon and it is divided into groups. Now the order he had established in the beginning is falling apart. It becomes obvious that his handling of the situation in the continuation of the lesson is counterproductive.

What is agreed on, what disagreed on? In any case this is not the question the teacher had asked. Since it is directed toward a time (when?) and not if the questioned song should be sung at all. If the students had answered the question of the teacher, they would have given a suggestion for a date. Since they did not do that, but proposed a simple yes-no scheme, they disqualified themselves as competent interaction partners for the teacher.

Another striking thing about the teacher’s question is the phrasing further on: “this Swing Low, Sweet Chariot.” It signals a degrading quotation, at least a touch of creating distance. The wording is placed between two more neutral alternatives:

- * *this* song “Swing Low, Sweet Chariot”
- * “Swing Low, Sweet Chariot.”

The teacher seems to handle the song “with his fingertips” in presenting it before the class. He possibly does not like this song very much himself. Indeed, the class must have sung it before, since it is in its repertoire. If the teacher is just acting as a speaking-tube, could the ignored student have expressed his dislike toward this song by saying: “this ‘Swing Low, Sweet Chariot,’” but why then would the teacher ask when this song should be sung again?

A date was asked for when the class should do something which has already been generally degraded. Therefore, if it is set for a specific time, when the certain thing will be a threat, a “something” that is seen by some as boring, uncomfortable and too hard or otherwise “negative,” then the question of setting a date will be followed by a denial by those who dislike the whole idea, even more, the answer would be: “Never!” Then the question about the necessity of when to sing the song would become a question of singing it at all.

The reaction of the students shows that they seem to understand the question precisely in this way and if the teacher passed it along as the “speaking-tube” of a student he misjudged its direction. He is now confronted with the necessity of making a decision. If he was serious about passing the question on to the students, then he should now

⁸ If we follow up on this idea of the diffuseness of the relationship between teacher and students, we could answer the question by looking at the structure as if it were not about a diffuse social relationship, according to Oevermann, but a relationship imitating a parent-child-relationship or a marriage relationship. Hereby the element of imitation could be on the level of the interaction between teacher and class, reproduced in an unexpected form.

realize that the question of setting a date was interpreted by his students as whether the song should be sung again at all. He could have then informed the students that they had given an answer to a question which had actually never been asked. And an answer of “never” would not be accepted by him or it would require a new vote. Then it would be necessary to decide if the song should still be a part of the class repertoire.

Teacher: *Shush* (10 sec. silence)

We can gladly do that at the beginning of the next lesson.

The teacher establishes order with a para-lingual signal, with a long lasting hissing, followed by silence. The class reacts to this signal seemingly as the teacher expected, acting very disciplined again after a short outburst. The teacher then sets the date for the group singing the song “Swing Low, Sweet Chariot” for the following religious lesson.

It becomes obvious that the teacher had posed the question to the students only pro forma. Looking back, it seems that the students understood this as well, since they did not agree to the heart of the question: the date. It is clear that the teacher simply ignores the “No-Sayers.” He does not recognize that the group opposes singing the questionable song at all.

The behavior of the students seems to be based on former experiences with this teacher’s planning habits, which is revealed again by his decision. His ability to prepare for singing a song seems to reach only to the next lesson. Making a date is therefore unnecessary. It is for that reason that the students answer with “yes” and “no.”

In the beginning the teacher seemingly had the idea of putting the question to the class for a democratic vote, now we see that this was a pseudo-democratic ritual. It shows that the teacher acts according to a scheme which displays him as a member of the body of teachers. The relationship between the teacher and students is clearly a hierarchy, even if he adds: “gladly,” giving the students more generosity.

Hypothesis: Until now the beginning of the lesson has run in the form of a ritual. At its end the class as a community is constituted by qualifying and disqualifying individuals. At first the canon stresses this since it can only be mastered when the teacher performs as a conductor. He poses the question about the song in order to deliver the answer as a ring master who orders and settles what is to be sung during the next lesson. The teacher is not serious about the question. It is only part of a pseudo-democratic ritual, a pseudo-decision-making process, after which the teacher decides the result himself because the class has proven “unable” to solve the problem. It seems that the ritual serves not only to constitute a relationship of hierarchy between teacher and class, but it also serves in terms of forming an educational body, the corpus of the class (a kind of “class-It”) that reacts with emotions and close to the body. That the class, not the individual student, is the counterpart of the teacher became clear when he posed the question anonymously and did not let either the “No-Sayers” nor the “Yes-Sayers” speak up.

The – falsely understood – text of the canon seems to affect the relationship between the teacher and students: the teacher more or less creates “his people” – a class obeying his commands. From this point of view the opening ritual comes close to an act

of Creation, whereby the question, what it is that the teacher, the Creator, will present to his Creation – the class – an analog to the text “*this earth*,” still is not answered. Staying within this picture, it can only be about the “Creation” of the teacher, which is supposed to be sanctified by the students.

Teacher: I would like to remind that today we will be revising for our upcoming written exercise, as we did last time.

The teacher starts by reminding the class of something that is going to happen soon. He explains what the subject matter of the upcoming written exercise will be. Obviously the teacher speaks about only a written exercise, avoiding the word “examination” or “test.” Why?

Connected with an examination or test is a final end point and a “grading.” By contrast the word exercise seems less dramatic, because it is repeatable and therefore implies a corrigible task. The teacher could use the word “exercise” in analogy to a sport lesson. Here the training is an exercise; a given movement can be practiced until a best result according to the standards of the grading instance is achieved. An exercise in this sense is at the same time the means and the goal. The example of the sport lesson also shows that the grading of a sport exercise is very subjective. We know this from the repeated disputes concerning the marking during the Olympic Games or world championships; it is subjective because it is dependent on the preferences, ideas and interests of the judges.

Thus a written exercise could evoke even more insecurity in the students than a test or an examination. The seemingly comfort arousing word “exercise” might change into discomfort, because the teacher can express the subjectivity of his preferences and moods.

The provisional status of the exercise, as a product of the teacher, throws a light on him: he is still practicing himself. Seemingly not having worked out a finished class examination or fixed a test, he is still looking for the best run through the right questions and the appropriate grading measurement. To repeat the revision with the students seems to serve his own “practicing the practice” and shows that he is not sure about his own business. Only one thing is sure: That the date for the exercise will unavoidably come about– for him as well.

Going back to the canon (to its form and text) and to its context (the Christian religious lesson) and the “religious” interpretation we have proposed, it becomes clear by now what is meant by the “Creation” of the “Creator” (i.e. the teacher). It is the inevitably approaching exercise, which needs to be prepared by the teacher for the students.

Each part of this exercise is supposed to be holy to “his people”– the class. But he becomes a part of the “people,” because of the tremendous and fascinating “Holiness” [Otto 1917] which is coming toward all of them. This seems to be the reason for the rather strange introduction of the lesson subject. This is the basis for the repetition of the summary which is supposed to proceed exactly as in the prior lesson.

Whom does the teacher remind ? The object (“I would like to remind you/me”) is missing in this sentence. It seems that he speaks into the “open” and above the students’ heads. Why does he need to expressly remind the students that he wants to repeat an exercise in which the most important content will be summarized? It seems that he wants

to remind the students allusively about the upcoming exercise. The utterance does not have a particular subject but only a goal: the written exercise. The teacher does not say what he wants to summarize, only for what he wants to summarize.

The exam weighs heavily and threatens the students like the sword of Damocles. The teacher seems to nearly threaten with his examination. It is presented to the students as a “grievous task,” rather than a test of the knowledge and skill residing within the student.

What was before in the text a diffused *Somebody* who asked a question, has now become a personalized and anthropomorphized exercise which is unstoppable, directed towards the student. It is as diffuse as the *Somebody*, but it seems to threaten the students. The exercise seems to be unattainable to the students. Even more: all of the teaching seems to be directed towards a climax that is going to be reached at the beginning of the exercise.

The potential power of the speech act of the teacher works as a threat through disorientation and by giving the feeling of an unstoppable direction, a direction which leads towards a dangerous climax which is in turn pushed and pulled by the teacher. Therefore, the teacher is not only the “Creator” of the class and the exercise, he also becomes the prophet of a coming crisis, in which his creation has to prove itself. And again, the structural element of repetition comes to the fore. It seems that it is the teacher’s intention to increase the imitation of the exercise that he creates. In contrast to the repetition, something new arises on the horizon: the written exercise.

Teacher: And since it is the first time that you are writing a written exercise with me, it will be a little difficult, since we have to get used to one another, because every teacher has his own ideas on it.

R The teacher is possibly new to this job; he is a novice. He cannot judge the class’s academic achievements yet, therefore he wants to repeat the contents thoroughly. The teacher announces possible problems to the students which could arise because of the fact that the students and the teacher are not a good team yet. The students do not know the teacher’s ideas about written exercises.

This long sentence is more complex and intricately structured than the one before. The teacher seems to put forth great effort into speaking proper German. He informs his students matter-of-factly about possible difficulties of the exercise to come. However, it is questionable if his students can follow his way of talking, the more so as he switches to a completely different level.

In the prior speech act there was at least a touch of content. Now priority in the speech is given to the relational aspect. It is no longer about the content to be studied by the class but about the one-sided process of making an effort to get used to the specificities of the teacher. This is quite difficult for the students. Given the fact that the teacher focuses now on the relationship aspect, “What kind of relationship is the teacher looking for when constructing his relationship to the class?”

This question is not answered by pointing to the fact that the teacher attempts to establish an educational relationship with a class he only recently started teaching. Of interest from a research perspective is the pattern of connection according to which the

teacher creates his relationship with the class. We have already found a starting point in this interpretation. The relationship is designed analogously to the canon and it entails the very same confusion of categories in terms of the Creator, the people, and the creation. This confusion results from the fact that a Native American text with a nature religion content will be sung in the occidental musical form of a canon and during Christian religious lessons. Christian religious education is formed according to an explicitly monotheistic religion of Creation and Salvation; and this content determines the mindset of the teacher and the students. The text is given in a manner such that those who are not members of the tribe cannot understand its content. Out of the religious and cultural context emerge latent structures of meaning which are the results of a mixture of Christian occidental imaginations with native structures.

On the assumption that this latent structure of meaning influences all participants in the lesson, the question is who occupies which position. Thus far we have found these positions:

- The Creator: Teacher
- The Prophet: Teacher
- The people: The class
- This earth (the people preserving creation) equals the (written) exercise.
- The sanctification equals love and obedience toward the teacher through “recognition” of his “Creation.”

That is to say that the teacher, as a prophet of himself, arranges at the very same time both the ritual of internalization with regard to the opening of the lesson and the performance of the history of creation (genesis). The hour of probation draws unavoidably closer for his people. The people, i.e. the class, will be reformed through this ritual by means of discipline and charisma with the goal of unifying “the people” with the prophet.

In now choosing our educational focus – a focus preoccupied by the “educational Eros” derived from Greek thought and the Christian tradition – we have another possible interpretation. The teacher is at the center of attention as the “leader” and “builder” of the child. He is looking forward to fulfilling this task through “gymnastic exercises” of the body and “educational Eros” among others. His creation is the “whole” child that he is going to form through “exercise” and “Eros” in order to make the students suitable for ethical life and society.

In our case “education” is the formation of a class. The addressee is not a single child or student, it is the entire class as an “educational” body (as the carrier of the diffuse *Somebody*). This body is in need of exercise, of a process of repeating exactly trained elements, only then will it be able to perform this process without failure.

Let’s have a closer look at the buildup of the different sequences:

Teacher: *And since it is the first time*

The teacher makes it clear that a first time is connected with the exercise. Therefore the exercise is a turning point. Of what does the first time consist in this context?

- R1 It is the very first exercise written in the class. In that case we would be talking about first graders. This seems to be out of the question, since the class has knowledge of English, demonstrated by the fact that they know the song “Swing Low, Sweet Chariot.” Besides that, it would be very unusual to write the very first school exam in the discipline of religion and not e.g. in German. In addition, a written exam would require that the students could write and this is normally not the case with first graders.
- R2 In fact it is the first exercise in the discipline of religion. Because we know that the study deals with seventh graders, we can assume that the students have had religious lessons before and that they have had written exercises in their class before.
- R3 The class will be writing an exercise with this teacher for the very first time, because the teacher just started. He is either a beginner in his profession or took over the class only recently.

The last way of interpretation seems to be the most reasonable one. The problem is that it now becomes “serious” for both sides. The students have to produce an independent performance for a certificate. The teacher has to confront the students with a situation wherein they depend on themselves to find answers to the demands posed. He can help them only if he defines his demands and expectations clearly from the very beginning. Irrespective of this, they will get “hurt” by him through the grades they receive. This is a test of endurance of the relationship between teacher and students. It will change. If we can assume that this interpretation is correct, students and teacher are in a “liminal situation” [Turner 1967; 1969] (a border situation), i.e. they are in a transition to a new quality regarding their relationship. The question is how the teacher is going to design this transition.

The teacher could be pointing with his comment towards changes which will inevitably occur in the relationship between teacher and class when worst comes to worst. In this case the relationship would be arranged for the future; for the time after the crisis. Later on it can be pointed out that there was a hint given before the beginning of the crisis that a possible crisis related to a change in the relationship might come about. When the teacher announces the crisis and the change to come with a hint of the first time, the remark can have a preventive character with regard to a possible failure. The teacher, as initiator, expresses that he was reflecting about the first time in terms of its significance. He makes the topic a subject for all, but claims for himself an edge concerning the reflection made, which is in turn the fundament of his leadership pretensions.

He announces and defines a crisis situation with his explicit remark regarding a first time in the future. In doing so he presents himself as a “Charismatic” (in the definition of Oevermann) who announces his competence (ability) to resolve a crisis already by warning of a possible crisis (being ahead in thought regarding the subject) and he therefore requests that others follow him. The teacher presents himself as his own prophet. Seen in this way, the promise to overcome the first time and the newness relating to the crisis situation is introduced by the teacher for all those who are ready to follow him.

Teacher: that you are writing a written exercise with me, it will be a little difficult

The third reading is correct. The speaker presents himself as the very crisis memento; he is the risk of the situation to be encountered. The newness for the addressees is basically that they will write an exam with this special teacher. They will have to deal with a crisis, a crisis that is (in) the person itself, who presents himself as a “personal union,” a crisis solving charismatic. The speaker reinforces this pressure enhancing remark, explicitly pointing out the consequence of the coming situation, namely that “it will be a little difficult.”

“It will be difficult” seems to say that there can be no doubt of encountering a more or less dramatic crisis. The speaker uses the indicated future and so closes, in a way, the future. He does not use the conjunctive (it could ... be difficult), wherewith he could have touched upon the possibility and probability of a crisis, without excluding that it might not become dramatic at all. In this manner the speaker forecloses the principally open future. He knows beforehand what is going to come. He knows that the expected situation will be dramatic, because he is the dramatizing element of the crisis. For the addressee the only gleam of hope lies in the remark of the speaker “that it will be a little difficult.” But this short remark is not a good choice for the purposes of comforting; it is too vague in its wording. What kind of little is he speaking about?

Result: the relationship will be practically tested. That it will come more or less dramatically to a head seems to be unavoidable. And: it seems only (or mainly) to touch the opponent of the speaker, who seems to be in a suffering position.

Hypothesis: the speaker presents himself as the charismatic prophet of a closed future, a closed future that will happen through him and that can only be overcome by him. Considering the aspect of an initiation ritual, we see the addressee introduced into a relationship of dependency which entails, because the teacher is both crisis initiator and solver, a totalitarian-sadistic undercurrent.

A new dimension to the teacher’s statements appears if viewed from yet another perspective, a perspective that – sure enough – imposed itself during the interpretation. His relationship to his class stands in the center of the teacher’s statements; a relationship that is going to change due to the exercise and will thus gain a new quality. Therefore we have to speak about the meaning of the “first time” in a relationship. A “first time” that also implies that it is getting serious. Our question, which results in an abductive manner from the process of interpretation, is: in what kind of a relationship is “the first time” of significance? In combining the two elements: 1. Stressing the aspect of the relationship and 2. Stressing “The first time” in a relationship, we come to a way of reading that is utterly unexpected. A reading that says that the teacher in this context frames the relationship to his class, in which he is teaching as a novice, according to the pattern of a sexual relationship – a relationship in which an experienced man is going to seduce a (young) woman.

Following Overmann, we could say that a charismatic person suggests to his followers a crisis. How can the addressees react towards this suggestion?

First, they can – for whatever reason – accept it. Secondly, they can withdraw from the situation, if there is a relationship based on mutual consent that can be terminated at all.

But in the case of a coercive relationship or an institutionalized relationship that cannot be ended without the danger of penalties, the situation holds a high risk. Then the speaker is in a position of strength and the addressee is in a state of dependency, in a way she or he has to follow.

Options: we are going back to the interpretation. It can be expected that the speaker now addresses the difficulties that follow from the fact that the exercise is being done by him for the first time; i.e. he should come forward with a reason concerning the difficulties that are going to burden the shared relationship. The questions run as follows: Why is it a problem that the addressees will do the exercise with him? Is there something regarding the speaker that difficulties are to be expected?

The speaker can – in his capacity as a charismatic and prophet who solves the crisis – give instructions concerning the behavior of the addressee. She or he will have a guarantee of overcoming the crisis when they are obedient to instructions. The difference from option one would be that the speaker indicates the reasons for the difficulty in the form of an indirect reaction. The addressee in turn can draw a conclusion by thinking about the reasons for the crisis that seem to lie in the speaker himself.

since we have to get used to one another

The reason for the expected “difficulty” does not depend on the teacher himself, all persons involved need to get into the process of getting used to one another. The focus here is on adaptation. And adaptation does not ask for a dispute but for repetition or a better routine. Only then can something unfamiliar, even if unaccepted, at least endure (“one gets used to anything”). What one will have to get used to is mostly negatively connoted. Getting used to something can happen in an active or a passive mood, in every case it stands against the usual habits. What do we have to get used to with another person? Obviously to their idiosyncrasies, moods, ticks and so on, or simply put: their habits.

Since time X is coming very soon, “getting used to it” should happen very quickly. But getting used to one another is a longer process and can no longer be performed before the beginning of X, since there is not enough time. What is left if a process of getting used to one another is impossible? The only thing remaining is a one-sided adjustment “for the time being.” The teacher should have known that a getting-used-to process is a process of mutual adjustment and that it takes some time.

And since it is the first time

That the addressees perform X with the speaker for the first time seems to be itself a part of the process of getting used to one another, as well as the problem that derives from it for the addressees. The process of getting used to one another has a goal in the distant future. Thus the hope of a mutual adjustment seems to be rescued, since the speaker did not exclude his own efforts for the time after X. But for the time being this has to be the rule: the crisis solution seems to be a one-sided task performed by the addressee toward the speaker, who is at the same time the hot spot and the crisis solving charismatic (his own prophet): the smooth routine of submission is presented as an “acid test” to the addressees. To master it will guarantee the successful graduation of X by the addressee.

The speaker could now present what he is ready to do in the future in order to enhance to the process of getting used to one another. For this he would need to put some effort into adjusting himself and into supporting his promise. He could point out his own qualities and those of the addressees, which they both need to get accustomed to. To do so he would need to open up more. He would need to give up a part of his usual self-presentation. The pressure for the addressees will increase if he does not do so. They see themselves surrendered to the teacher, who leads them into a contingent future, which is at the same time closed since only one thing is known for certain: they can prove themselves only by means of adjustment and submission to the speaker.

This contradictive unit of an absolute contingent and a total “closedness” is a sign of a totalitarian regime. It will even use “terrorism” to reach its goals.

because every teacher has his own ideas on it.

The speaker belongs to the body of teachers. Its members are characterized by having their *own ideas*. These ideas are not defined very clearly, but the addressees nevertheless have to get used to it. Now it becomes definitively clear, that a one-sided adjustment is expected. Anyway, the teacher does not give any hope that he is willing to change his *ideas*.

The addressees do not get any further information from his statement which might indicate the speaker is a calculable opponent. They know only that he sees himself explicitly as a member of the teaching staff. He insists, as do other members of this group, that his ideas are followed. He informs them of his ideas only when they are trespassed and he enforces them subsequently. At least this seems to be one of *his own ideas*.

The teacher does not refer here to his position as a teacher, but he refers to diffuse imaginations, to something not to be foreseen, because it shows itself only in the actual situation. As such, these “imagination” are more likely irrational, affective and “close to the body.” If we are to believe the speaker: they are shared by all teachers, being the common knowledge of teachers in general. This “body of teachers” is something other than “the role or the person of the teacher.” The “body of teachers” is something irrational.⁹ It is a body where one learns, where the “class body” learns. The “teachers’ body” needs the formation of an opposite, the formation of the “class body” to become active.

The reference to the “teachers’ body” is also a reference to a kind of collective educational Eros; a reminder of the physicality of teachers and students in the ancient world. Today this can rightly exist only symbolically or “virtually.”

The relationship between teacher and student as a “physical relationship” has to be symbolic. The teacher in our case constructs the exercise after the pattern of the “first time” in a sexually accentuated love affair: the teacher imitates with his speech a heterosexual relationship, in which an older “experienced” man speaks soothingly to a girl or a young woman before their first sexual intercourse. Both are in a situation in which to lose their innocence to one another.

⁹ At this point the idea of using the word “teachers’ body” or “body” should be given more attention than we can give here. The procedure of the scientific taking possession of the body through adjustment and submission should not be forgotten. According to Hobbes, the object of philosophy is a body of any kind, that making we can conceptualize, therefore rebuilt rationally [Breuer 1995: 191].

RESULT

Our hypothesis is that the teacher presents himself as a “staff union” (in a personal union) of creator and prophet toward the class. He creates the relationship toward the class, towards his people, as an initiation rite, which is supposed to prepare for a crisis in the mutual relationship. On a different level the teacher constructs the change of quality in the relationship, because the written exercise is introduced by him analogously to the sexual relationship of an elderly, sexually experienced man and a girl or young woman.

The relationship of the teacher and the class seems to be a “physical relationship” between two “educational bodies” – the “teachers’ body” and the “class body.” The structure of a diffuse social relationship is based on this “physical relationship.” For the teacher as well as for the class the written exam is “the first time,” an “exposing” and “unifying” thing; and he, the teacher, shows the class what counts to him.

The teacher wants to “seduce” the class to study for an exam, which must be inevitably expected, the same way that a young woman has to inevitably expect the moment of her defloration (the first time, at least if she is heterosexually oriented). The teacher as an experienced lover announces to the class, as a virgin, that the relationship will have to change drastically. The platonic level has to be changed, and penetration is now inevitable (through the exercise that must come): the experience of pain, related to this act, will delete the non-binding nature of the relationship.

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NARRATIVE INTERVIEW AS A METHOD OF BIOGRAPHICAL ANALYSIS

KAJA KAŻMIERSKA

INTRODUCTION

Biographical method is a way of conducting social research based exclusively on first-person accounts of events and processes that constitute the research subject, and on the basis of these accounts the processes are described and explanatory hypotheses formulated. [Szczepański 1973: 619–620]

Such a definition of biographical method from today's perspective is but a general indication of the research option. It says nothing about the analytical procedure or methodological and theoretical perspective. Thus, it does not necessarily describe the same procedure as that adopted by William Thomas and Florian Znaniecki, the founders of the biographical method. They studied biographical materials assuming that in social life subjective factors play a decisive role while their representations are an invaluable source of knowledge about social processes [1918]. The biographical method, worked out by Thomas and Znaniecki, was one of the most widely used tools in the Chicago tradition of the 30s and 40s. As structural functionalism was gaining ground, biographical method was overshadowed by the supposedly more efficient and "scientific" survey methods. Polish sociologists, though, continued to cherish Znaniecki's tradition after the war.

And it is this phenomenon (together with a rather faint response from other countries) that is responsible for the fact that in non-Polish writings on Polish sociology this method was sometimes called the Polish method. [Dulczewski 1975: 33]

Currently, interest in biographical analysis has revived. This has been accompanied by the growing popularity of new theories in sociology (ethnomethodology, phenomenology, and symbolic interactionism) that view the individual as an active agent and social reality as constructed by an individual. Such orientations created the need for other than statistical methods [Helling 1990]. Moreover, biographical analysis greatly profited from the technique of tape-recording as it became possible to register face-to-face interaction and spontaneous story-telling. New theoretical and methodological thought flourished, and biographical materials could be used in different research options.

The biographical approach is now adopted by different schools of thought (symbolic interactionism, phenomenological sociology and ethnomethodology, Marxism and structuralism) and with regard to a much greater number of sociological issues and research fields (social mobility, social awareness, socialisation, sociology of profession, medicine, crimi-

nology). Nowadays there is a growing interest in the way biography is constructed and reconstructed by institutions and individuals, that is, findings are not taken at their face value, as representation of “subjective reality” but as context-bound and sensitive to the context of time, interests, interaction. [Helling 1990: 15]

Thus, various approaches to biographical method differ along the lines of data collection, method of analysis or theoretical perspective behind the research acts. These different approaches to biography have been discussed by many authors [cf. among others Denzin 1970; 1989; Bertaux 1981; Hałas 1990; Rokuszewska-Pawełek 1996; 2002].

As Ingeborg Helling underlines, one of the decisive factors in differentiating between research perspectives is the concept of the subject. The definition of the research subject determines the approach to biographical material, that is, biography can be studied as a topic or as a means by the researcher working on biographical data in order to answer sociological questions. When the researcher focuses on the subject – the aim of the analysis is to answer the question “what?” and when the structure is the centre of his/her attention – the question “why?” is addressed. In the latter case, the researcher is interested in “*interactional conditions for generating data and the structure of individual meaning in constructing courses of events*” [Helling 1990].

Different options of biographical research have been combined in the method of narrative interview [Rokuszewska-Pawełek 1996]. The term “method” is not accidental, as its founder – Fritz Schütze – introduced a convention of research into life stories that includes a specific technique of data collection whose purpose is to obtain a so-called (auto)biographical narrative and the method of analysis built on a set of congruous theoretical assumptions.

Narrative method and *objective hermeneutics*¹ are the two most widespread biographical orientations in contemporary German sociology. Norman Denzin classifies them as objective approaches that are a continuation of the “natural history” approach adopted by the Chicago school [1989]. Defining the field of research as the analysis of phenomena extended over a period of time, which is processual in nature, Schütze alludes to the concept of “natural history” by Robert Park [Schütze n.d. b]. The originator of the narrative interview also refers to Thomas and Znaniecki’s formula of the in-depth analysis of individual cases found in the materials reconstructing the past. However, the new quality of recorded biographical narrative and specific analytical procedure allowed Schütze to introduce the methodological innovation of “dynamic retrospection.” And this methodological novelty is largely responsible for the unique-

¹ The term *objective hermeneutics* was coined by Ulrich Oevermann. The analytical procedure carried out under this orientation presupposes the difference between “meaning structures” independent of the interviewee’s consciousness and “presentations intentionally given” by the interviewee. The distinction between the two layers is drawn on the basis of a careful analysis of a biographical interview and consideration of all the available, yet not undertaken by an individual, options of action, whose number gradually diminishes in the course of the analysis. The subsequently excluded options enable a precise description of the given case, the assumption being that its very essence can be reached by finding out what it is not or could not be. The analysis of a case is carried out by a group. During in-group discussions all the phases of biography and potential actions are delineated and addressed individually [Denzin 1989; Prawda 1989].

ness of the research process which differs from that applied by symbolic interactionists aiming at “grasping a social phenomenon *in actu*” [Prawda 1989].² Thus, Schütze merged different narrative traditions: interest in formal substantive matters – in **what** people have to say – and interest in formal features of narratives – in **how** they tell their stories. This merger of perspectives is related to the idea that by systematically focusing on the how, a researcher can reach a deeper understanding of narrators’ experiences and make his/her own forms of understanding explicit and intersubjectively controllable [Riemann 1999].

My description of narrative method will include the characteristics of the instrument itself, basic theoretical assumptions and the step-by step process of the analysis in question.

NARRATIVE INTERVIEW AS A METHOD

Despite the name “interview,” collecting materials such as personal biography differs from the traditional methodology in social sciences. The purpose of the narrative interview is to elicit a story of self-lived experiences that is not a sum of responses to questions but extempore narrative, not interrupted by the researcher’s interjections. The narrative interview aims at obtaining from an individual – called here the narrator and not the respondent – a first-person account of his/her life or its aspects.³ As a result, the researcher can work on a comprehensive, tape-recorded, sometimes a few hours-long story of the narrator’s life.

It might seem that giving a narrator a “freehand” makes the narrative interview an easy technique and takes away some of the interviewer’s functions that s/he has when conducting a traditional interview (e.g. constructing a battery of properly formulated questions). In reality, though, to obtain a high-quality narrative interview requires specific skills. These will be discussed together with interview phases⁴ below:

- 1) **Initial phase.** Although this stage of the interview is not recorded, it is very important for creating an atmosphere of encouragement. Thus, the researcher tries to win the narrator’s sympathy and confidence by means of so-called small talk. It should be remembered that telling an autobiographical story requires considerable intellectual, emotional and sheer physical effort on the part of the interviewer and interviewee. A tape-recorder plays a significant role in the setting and the moment it appears or is about to be switched on, the narrator often feel uneasy. Thus, one of the interviewer’s tasks is to get the interviewee accustomed to the device.

² It should be stressed here that Schütze’s inspiration for processual aspects of reality derives from symbolic interactionism. He alludes to Erving Goffman’s notion of a “moral career” and, most importantly, Strauss’s concept of trajectory. Another important methodological source of inspiration for Schütze, supportive of the analytical procedure, have been the principles of grounded theory of Barney Glaser and Anselm Strauss [Glaser and Strauss 1967; Strauss 1987].

³ Needless to say, such a procedure of data collection is based on specific theoretical grounds, which will be discussed below.

⁴ When describing an interviewing procedure, I use Hermanns’ article [1987].

- 2) **The phase of stimulating a narration.** At this stage the researcher explains to the narrator what kind of response s/he would like to elicit. It should be stressed that the researcher is interested in the *personal story of the narrator's life* and his/her *personal experiences*. It should be clarified and stressed that the interviewer is interested in the life story of this particular narrator. Otherwise the narrator might think that, similarly to other interview situations, s/he is to represent a given group and express its beliefs, etc. If this happened, a story told during the subsequent phase of the interview would not meet the requirements of biographical story-telling.

- 3) **The main-story phase** constitutes the key phase of the interview and is a spontaneous (i.e. not prepared in advance) and uninterrupted life story. In other words, the interviewer's task is to listen carefully to (sometimes several hours long) narration without redundant queries or commentaries, i.e. an interruption which may distort the story. As writings on narrative interviews show, and congruous with my own experience, this is not at all an easy task. The habit of asking questions imposed by "classical" research methods as well as the scheme of everyday communication that "excludes" the silence of an interaction partner, are difficult to get rid of. The difficulty is magnified in interactionally problematic situations such as moments of extended silence when the interviewee is thinking about what to say next or gets emotional. The latter situation, often accompanied by tears, is extremely hard for both the narrator and researcher. Neither is it easy to suppress the "natural" impulse to immediately ask the narrator for more information or clarification. The interviewer should remember that his/her task during this phase of the interview is to keep the story-telling going by means of signs of non-verbal communication that show interest in the narrator's story. As I have mentioned, these are skills acquired in practice. As it turns out, the researcher's participation in this phase of the interview does not boil down to just refraining from asking questions. The researcher has to listen attentively and constantly analyse the story, which is vital for the next stage of the interview.

- 4) **The phase of additional inquiry** starts when the interviewee has definitely ended his/her narrative. At this stage the researcher may **ask for clarification**, that is, ask questions related to the main narrative and pose **theoretical questions**. The former requires the mentioned ability of careful and even analytical listening (during the narration the researcher can take notes). These questions are asked to elicit more details on the matters where inconsistencies or implausibility occurred, with regard to incomplete textual patterns or other points not fully understood by the interviewer. This is a very significant and difficult phase of an interview, and sometimes inexperienced interviewers may be unable to spot all the issues that should be subject to inquiry. Information gained during this phase is used at a later stage when reasons for leaving out events or inconsistency in story-telling are looked at.

In the second part of this phase theoretical questions are posed in order to elicit the narrator's opinions and additional commentaries as to important (for the topic) themes.

- 5) **The closing phase** is the phase of “getting back to normal.” This involves switching off the tape-recorder and embarking on an ordinary conversation. This phase is not that vital for data collection (although it might happen that the researcher receives a handful of additional information related to narration). However, from the ethical point of view, which is closely observed by the narrative method, this is an extremely significant stage. When a meeting comes to an end, the researcher ought to devote some of his/her time and attention to the narrator treating him/her as an interaction partner and not only a “story-telling machine,” as the narrator has devoted his/her time, good will and vested trust in the interviewer. This is also the right moment for the narrator and researcher to express their doubts and dispute diverse views.

The material obtained in such a way is transcribed according to a certain conventions and analysed along the lines worked out by Fritz Schütze. Before I discuss the method of analysis in detail, let me emphasise that the interaction between the researcher and narrator does not presuppose symmetrical conversational relationships.

And so Schütze sets up a clearly artificial situation to elicit the primary and natural structure, that is, off-the-cuff story-telling. [Prawda 1989: 95]

Thus, the narrator tells a story with its own dynamics, goes back in time, and the situational context of interactional conditions for generating it is suspended. This can be observed in cases when the narrator initially does not know how to embark on narration, feels uneasy, does not know what to begin with, and finally “gets carried away” by the story-telling and surrenders to its rules. When narration is well underway, the presence of a listener has little effect on a life story. Undoubtedly it affects the secondary order of presentation, that is, formulating generalisations, synthesis, assessments of the narrator’s own life, and some forms of self-presentation. From this point of view a narrative is the result of interaction and a product of both the participants [Prawda 1989].

THEORETICAL FRAMEWORK

The above strategy of data collection is governed by certain theoretical assumptions. One of the assumptions is that there is

a homology between the structure of the organisation of the experience of the life events and the structure of the autobiographical narration. [Hermanns 1987: 49]

This means that the phases of biography experienced by an individual in real life are reconstructed in the same order in the structure of the narrative. Due to this, the narrative of biographical experiences reveals what has happened in a social dimension from the point of view of individuals participating and acting in it [Schütze n.d. a]. Such a reconstruction of life course events in a narrative (which is a comprehensive story that puts together the described events) aims at recapturing sequences of events that display the continuity of the biographical experience typical of a human being. Thus, the task of narration is to render the processual nature of life course events, and the researcher can analyse the issues of interest to him/her not as static phenomena but

describe the way a given phenomenon emerged, developed and possibly disappeared. In other words, the advantage of the narrative interview is not the fact that it helps to collect information about the events which cannot be gathered by any other research methods but the fact that only in a narrative does an individual give a *processual* account of life course events [Hermanns 1987].

At the level of an individual biography, the process reveals itself in a sequence of action schemes defined by Schütze (see below). On the one hand, this makes a comparison of individual biographies and the study of particular forms of a life course possible, and, on the other, their entanglement in social processes can be described. The former is an analysis of the phenomenon of a human life course, the latter – of social phenomena from a biographical perspective [Prawda 1989]. Thus, narrative reveals social worlds and processes connected with life course events.

For the basic theoretical assumptions of the narrative interview to be fulfilled, an obtained narrative should meet certain criteria. One of them is the rule described by William Labov and Joshua Waletzky according to which the homology between experiential structures and narrative order exists in extempore (i.e. not prepared in advance or routinised) narration told to an interaction partner [Schütze n.d. a]. This is why it is so important to write down an accurate transcription of a tape-recorded interview using the symbols that correspond to the numerous, including non-verbal, features of interaction and construction of a narrative.⁵

The analysis begins with the identification of the overlaying scheme of communication. This includes the verification of the spontaneity of story-telling and the identification of narrative, descriptive and argumentative components, which makes it possible to assess the prevalence of one textual form over the other, with narration usually being predominant [Schütze n.d. a]. In other words, each of the listed linguistic schemes of communication plays a particular role in capturing reality. According to Schütze, the real course of human actions is most accurately depicted by the narrative scheme of communication. Thus, narration reconstructs the processual course of events while argumentation contains the narrator's tentative explanations and motives for actions undertaken, theories of self-identity and life assessment from the present perspective. Differentiation between the schemes of communication is one of the main advantages of the narrative interview. It equips the researcher with the tool to identify (and at the same time compare) narrative fragments which carry information about the course of events and check them against "ideological" statements which essentially show what an individual really thinks irrespective of what s/he does [Hermanns 1987].

A story arranged around sequentially ordered events acquires its own dynamics – retro-spection is accompanied by currently generated meanings and existential reflection. The narrator is working on his/her life history, is involved in internal dialogue – static experiences and "sequence points" acquire the processual logic of events, the effort is made (for

⁵ Here are some of the symbols most commonly used in my transcripts: very – the underlined word or phrase means it was stressed by the narrator; / – a slash means that the word or phrase was abruptly interrupted; ... – silence; [– square brackets indicate that both the narrator and listener were speaking simultaneously. In addition, metalinguistic information can be given in brackets, such as comments on the speed of speech, way of speaking, emotions displayed by the narrator as well as pauses or disruptions, etc.

the sake of the listener and oneself) to synthesise the reason why certain situations and states happened. [Prawda 1989: 91]

ANALYTICAL PROCEDURE

1. Schemes of communication

A description of the mentioned textual forms of narrative interview constitutes the first step in the analysis of the scheme of communication. At this point we also assess the quality of the material by stating the degree of indexicality, that is, checking whether it contains detailed descriptions, staged interactional situations, quotations of the narrator's and other people's speech, etc. [Hermanns 1987]. Indexicality defines the narrativity of the text and shows whether the narrator, when telling his/her story, gave in to the rules of extempore story-telling. An autobiographical narrative should contain the subject of a life story, a sequence of events and framing of social processes, namely social worlds and milieus [Schütze n.d. a].

Narrative is governed by three main constraints. These are:

- 1) the narrative constraint to condense – the narrator is unable to tell everything and therefore has to make choices and stick to the main story line;
- 2) the narrative constraint to go into detail – if the narrator has mentioned something, s/he has to, by implication, tell other related events for the story-telling to be credible and plausible;
- 3) the narrative constraint to close the form – the narrator has to tell an episode, event, etc, to the end [Hermanns 1987].

The analysis of the scheme of communication is a precondition for further work with the material since, as shown here, the researcher checks whether the narrative interview meets the basic requirements which make further analysis possible. Some of the subsequent steps include: a formal sequential analysis of the text, an analytical abstraction of the generalisations that appear in the text, a juxtaposition of the generalisations found in other narratives similar in topic and form, an elaboration of the theoretical model, and verification of the theoretical model by testing it against other empirical materials.

Below there follows a step-by-step description of the analytical procedure, with an emphasis on the formal sequential analysis.

2. Formal sequential analysis

The task of the formal sequential analysis is to identify the so-called units of narration, taking into account incorporated background constructions (see below) and suprasegmental parts composed of units. After the text is divided into units, segments

and suprasegmental parts, and these are defined in terms of a thematic concatenation of events (i.e. the order of content-structures is established) according to presentation style, change of perspective, etc., their connectedness in a sequential order and types of interdependence (e.g. the hierarchical order of textual segments) are described. The analysis provides the researcher with an insight into specific and general social features characteristic of socially significant, biographical and socio-historical processes [Schütze n.d. a]. This is the key phase of text analysis, the findings of which are presented by the researcher in a written study.

2.1 Process structures

The formal sequential analysis helps to identify specific biographical processes in a given narrative, that is, the four process structures introduced by Schütze. Process structures underline the sequential organisation of biographical experiencing. They make it possible to describe individual phases of a narrator's biography as a sequence of experiences unfolding over time. A given process structure is characterised by a specific type of action undertaken within its framework – either intentional or outwardly conditioned. Apart from intentional acts typical of social action, there are also phenomena that take away an individual's control of his/her life course. Therefore, from the individual's standpoint, biographical phases can be described in terms of either “acting,” “experiencing” or “enduring” one's life. The stances of “acting” and “experiencing” mark out a framework for the most vital biographical choices and interrelations between external and internal conditioning for biographical processes, that is, objective and subjective dimensions.

The following four process structures described by Schütze were generated through theoretical saturation [Glaser and Strauss 1967], as a result of the analysis of over one hundred narrative interviews:

- 1) **Biographical action schemes** (*biographische Handlungsschemata*) – correspond to an **intentional** biographical principle. An individual has a biographical plan and planned identity work and tries to implement the project. Such a plan may include general biographical projects concerning e.g. a given phase of a life cycle (such as to start a family, have children, etc.) or one of its dimensions (e.g. a formulated and carefully carried out plan of a professional career). Another example of this mode of process-structure is the desire to experience something new. This can be governed by an unexplained impulse that, in time, will become biographically momentous. Still another version of such an action scheme is an irresistible desire for change. An individual feels unfulfilled under the existing life circumstances and wants to change them by searching for situations that would bring about options for new biographical chapters (e.g. travels, a change of profession, social worlds, etc.). Thus, this type of process structure is typical of intentional and planned actions undertaken in order to implement a given biographical project.
- 2) **Biographical trajectory** (*Verlaufskurven*) refers to a situation of being suppressed by circumstances that take control of an individual's life course (e.g. sudden ill-

ness, death of someone close, war). The trajectory corresponds to the biographical process of **experiencing** accompanied by the feeling of suffering. A biographical trajectory is related to the new situation when an individual loses control of his/her life as all the customary orientation patterns fail. This is a long-term process, going through various stages described in detail by Schütze. In the course of this multi-phase process an individual is initially driven, overwhelmed by outer forces, experiences a growing disorganisation of life and uncertainty, and cannot take intentional action. The process is aggravated by an identity crisis, inability to establish social contacts and at the same time dependence on others. The individual's existential world and scope of activities are narrowed down. Gradually, step by step, an individual works out new interpretations of the situation so as to finally tame the circumstances forced upon his/her life – and manages to “escape from” the trajectory. To take control of this process means to replace the biographical process structure of experiencing by intentional action and this often means taking up the biographical action scheme [Riemann and Schütze 1992; Schütze 1997].

The trajectory seems to be the process structure most fully described by Schütze. The term was coined by Anselm Strauss who referred to a type of biographical process exhibited by ill persons kept in health-care institutions. Schütze broadened the notion of trajectory to mean any biographical process bearing the characteristics mentioned above.

Process structures are related to both individual biographies and groups, especially in the case of experiencing totalitarian regimes, war, migration, and “man-made” or natural catastrophes [Apitzsch and Inowlocki 2000]. Thus, Schütze [1992] identifies individual and collective trajectories, which are related to collective experiences.

- 3) **Institutional patterns of the life course** (*institutionelle Ablaufsmuster*) are forms of representing biographical structures when an individual's actions are governed by institutional patterns, an individual accepts expectations with regard to him/her, tries to live up to these expectations and faithfully carry them out in the course of his/her life. Needless to say, institutional conditions continuously shape the biographies of members of society. Think of e.g. the norms governing the individual phases of a life cycle. Institutional patterns are the main biographical structure when they constitute the predominant mode of biographical experiencing, and constantly shape the individual's life course (e.g. the form of an institutional schedule for organising biographies can be a career pattern – becoming a student, soldier, etc.)
- 4) **Biographical metamorphoses** – an unexpected, positive change in one's life which makes it possible to discover new possibilities and/or talents and which leads to a radical change in one's life. A characteristic feature of unexpected prospects in life is that they cannot be introduced by the biographical action scheme (e.g. discovery of earlier unknown talents, a lot of money won in a lottery). Biographical metamorphosis is usually accompanied by an identity change or transformation [Schütze 1981].

The identification of process structures, being both the forms of presentation and biographical experiences, is the final stage of formal sequential analysis and helps to outline social and biographical processes present in the narrative. It should be stressed here that process structures are sought not only for the sake of their identification but also for the discovery of their internal dynamics, e.g. by showing how one structure changes into another in an individual's biography.

2.2 Some of the terminology used in formal sequential analysis.

Before I go on to describe the subsequent steps of the analytical procedure, I would like to introduce some of the notions used in formal text analysis. These include such terms as "theoretical statements," "background construction," "fading out of awareness," "argumentative statements" and "coda."

Theoretical statements show an individual's attitude to the presented life course events. They usually appear at the end of a narrative segment and provide a summary, retrospective evaluation or comment on the reconstructed biographical experiences. When theoretical statements occur in other places of the narration, they are the signs of "narrative disorder." Verbalised "disorder" indicates that an individual has a problematic attitude to the reviewed phase of life or suffers identity problems with which s/he is trying to deal (e.g. by fading – see below – certain experiences out of awareness or, on the contrary, by uncovering and explaining them to oneself and the listener) [Schütze 1984; n.d. a].

A background construction (*Hintergrundkonstruktion*) is a story incorporated in the main story line whose function is to specify, explain and sometimes embellish the master narrative. The first two reasons for introducing a background construction have to do with Schütze's constraints of extempore story-telling that are responsible for the credibility, well-formedness and plausibility of a narrative (see three narrative constraints). Background constructions are a part of narrative segments and cannot be produced outside them. Whenever a disorder in the sequence of narrated events occurs, a background construction is introduced into the main story line in order to sustain credibility both in terms of the orderly presentation of life course events and the viability of the experiences accompanying them [Schütze 1992].

An example of the latter would be the case of the former German soldier analysed by Schütze, whose narrative contains a background construction providing explanations of the source, kind and intensity of the narrator's feelings of guilt that stayed with him for the rest of his life and resulted from the narrator's feeling of helplessness. The background construction not only legitimizes the narrative but also points out significant instances of fading difficult experiences out of the narrator's awareness. It carries information about the traumatic experiences which did not occur at the right moment in the sequence of events. The background construction contained a story whose content fits into the war experiences which should have appeared earlier in the narrative, whereas the narrator introduced it when telling about his family problems encountered long after the war [Schütze 1992].

Background constructions which occur in such a context are linked with another theoretical notion used in the formal sequential analysis – the **fading out of awareness**

(*Ausblendung*) [Schütze 1989]. This term refers to partial or total omission of an important phase from biographical story-telling. Schütze identifies the following two types of fading out of awareness:

- 1) **Current fading out** of awareness i.e. from the present perspective of reconstructed biographical experiences. Such aims at obscuring past biographical experiences in the presence of the interaction partner and imagined audience. Present instances of fading out can easily be detected by way of formal sequential analysis as they distort the line of story-telling by e.g. violating its basic rules. Thus, such instances of fading out create discrepancies and tensions in the presentation of biographical experiences, result in cutting off the narration and generalisations incongruous with earlier presented life course events;
- 2) **Past instances of fading out** are unintended and supposedly influence subsequent biographical experiences in the narrated phase of the life history. Past instances of fading out are more difficult to detect and their identification can be facilitated by the fourth phase of the interview described above (asking questions for elaboration and clarification). As a result, the here-and-now presentation of a biography can be subsumed in various ways. To begin with, the past instances of fading out can govern biographical story-telling, which would create gaps in the presentation of life circumstances or the discrepancy between biographical events and the general pattern of interpretation. Secondly, they may be subject to the narrator's reflection and "biographical work" conducted in between the narrated experiences and the interview situation. An example of this would be the occurrence of background construction as well as retrospective evaluations of past events and reflections on other viewpoints, or else pointing out the difference between the past and present perspectives [Schütze 1989].

The narrator makes **argumentative statements** to explain to himself/herself and the listener the problems that remained unsolved in previous phases of his/her biography. This commentary occurs by the end of the narrative as the narrator feels the need to comment on such experiences and relate them to his own self-concept [Schütze 1984; n.d. a].

The **Coda** or the end of the narrative is one of the rules governing extempore narrative. The function of this scheme of communication is to relate experiences of the past to the present perspective. In the coda the narrator assesses the impact of the past events on his/her present situation, especially in terms of identity transformation. The coda consists of two parts. In the first part the narrator sums up the life story and measures the effects of the depicted biographical experiences. In the second part the narrator "has finished" with the past and turns to statements formulated from the present perspective [Schütze 1984; n.d. a].

The listed notions do not exhaust the list of terms used in the formal text analysis. They are, though, the ones most frequently used in the structural analysis of a narrative, describe the course of experiences and point out the peculiar characteristics of a given life history. This can again be exemplified by the analysis of the background construction in the biographical interview with the former German soldier.

3. Analytical abstraction

The formal text analysis is complemented by another phase, that of analytical abstraction, during which the researcher extracts characteristic and unique categories of the narrative under study. The most important task is to describe interdependencies, passages and relationships between biographical process structures which altogether constitute a biographical whole (*biographische Gesamtform*) [Schütze n.d. b]. When carrying out the phase of analytical abstraction, the researcher should also decide on the categories the analysed text shares with other narratives. In addition, the researcher tries to uncover the narrator's self-theoretical work. The study of theoretical comments is conducted in the earlier phase of textual analysis, and at this point the researcher focuses on a reconstruction of self-concepts extracted from the biographical comments and relates them to the actual life course events as presented in the narrative. Discrepancies point to problems with the narrator's self-identity construction (e.g. as a result of traumatic experiences or experiences s/he is ashamed of, which distort his/her self-concept). Moreover, the analysis of this inconsistency is facilitated by certain features of the text such as a narration disrupted by structures of argumentation or a fading out of awareness. Another aim of the above findings is to outline socio-biographical functions of self-theories (e.g. the legitimizing of difficult phases of life, dubious actions, etc.).

4. Comparative analysis

The next step is comparative analysis. During this phase categories generated from the first studied transcript and proposals formulated on the basis of analytical abstraction are compared with other texts along the lines of *their* categories and *their* proposals generated by way of analytical abstraction. This procedure should result in marking out the theoretical complexity of the research field. Comparative analysis provides grounds for the preliminary overview of empirically grounded and tested dimensions, groups of characteristics, sets of possibilities in order to define the phenomenon in question. Every subsequent case brings in new categories until theoretical saturation is achieved [Schütze n.d. b].

In comparative analysis two mutually non-exclusive strategies can be employed. One of them is to select entirely different cases. This helps to measure out a theoretical framework for the phenomenon in question. The other strategy involves selecting a case that, despite similarities, exhibits certain distinctions. Such a strategy helps to increase the "density" of dimensions and categories related to the process under study.

The final stage of this phase includes the selection of a few cases which would constitute so-called "portrait chapters." These are used for presentation and discussion of the master cases which are the milestone of an exhaustive analysis. Portrait chapters exemplify the key biographical and social processes included in the theoretical model formulated on the basis of the comparative analysis [Schütze n.d. b].

5. Generating a theoretical model

A theoretical model is generated to describe certain types of socio-biographical processes. Most importantly, it should point out the relationship between biographical and social processes. This relationship can be described in two ways:

- 1) One way is to formulate a theoretical model by studying the entanglement of the individual's biography in a given social process. The description of this social process will constitute a "natural history" (in Park's terms), the concatenation of typical stages and turning points that are the subject of analysis.⁶ When a theoretical model is constructed along these lines, the following questions are asked: In what way does the social process develop under different biographical conditions? How do biographical responses differ with regard to the same social process? Can one define typical forms of the realisation of this process in biography, and what are they? What critical situations or turning points can distort the realisation of a given biographical process in the biography and how do individuals with different biographical roots manage them? [Schütze n.d. *b*];
- 2) Another way of formulating a theoretical model is to define typical relationships that exist between biographical processes and the studied social phenomenon. Schütze gives the example of the course of a migration process which goes through several phases in an individual's biography: a biographical action scheme (emigration plans), trajectory (facing a new reality), a new biographical action scheme (gaining control of the trajectory, becoming accustomed to new conditions). Thus, the researcher acquires a general knowledge of the phases of a given process. This knowledge is used to describe the way the process unfolds in the life history of a given individual e.g. the process of becoming an emigrant. The question the researcher asks himself/herself is: How do individual process structures evolve under different social conditions (e.g. for migration)? [Schütze n.d. *b*].

Schütze stresses that the two above strategies of constructing a theoretical model are complementary. They both take into account the process-like nature of biographical reality and formulate explanations that take into account sequences of biographical experiences. In practice a researcher takes up one of the strategies, thus making a choice between a sociological and socio-psychological approach. When a researcher decides upon the second strategy, s/he takes up a sociological approach and his/her research subject are social, organisational and institutional characteristics of processes. And if a researcher chooses the first strategy, the analysis is socio-psychological in nature and focuses on individual and/or collective preconditions for social processes that are at the same time the stimulus for biographical processes and the subject of biographical work [Schütze n.d. *b*].

Theoretical models are built around the key theoretical frameworks. These are in line with the heritage of the Chicago school of sociology and the symbolic interaction-

⁶ Schütze illustrates this using the example of a student's educational career being composed of entrance exams, class attendance, tests, exams, defending an MA thesis, a job-search, etc.

ism that traditionally analysed biographical and social processes. Schütze claims that theoretical frameworks are formed around the descriptions of the key process structures of social reality and that is why they can be found in every dimension under study. The main theoretical frameworks are: career patterns, patterns of structural descriptions of a life cycle, patterns of the narrator's attitude to his/her life course, patterns of development and the transformation of social worlds, social milieus and social movements [Riemann and Schütze 1987].⁷

6. Verification of a theoretical model

A theoretical model can be verified by way of confronting it with the empirical material not used for generating the model. This is especially useful in the case of interviews used as portrait chapters. Other materials collected under the same project but not analysed in detail can be used for verification. Here verification means checking all the categories generated for the entire stock of collected materials. The verification process is accomplished when the researcher is unable to find any more negative cases (i.e. cases that do not fit into the model), theoretical gaps, and when subsequent categories generated on the basis of theoretical abstraction and comparative analysis speak in favour of the categories of the theoretical model.

CRITICISM OF THE NARRATIVE INTERVIEW

The stages of the analytical procedure presented here in a nutshell constitute the set of methodological postulates worked out by Fritz Schütze. It should be mentioned here that a researcher does not always take all the mentioned steps. It is quite a common practice for a researcher to go as far as comparative analysis and portrait chapters, that is, master cases of a certain type. In any case a researcher faces the question of whether the findings of the biographical analysis are representative, plausible and reliable. And this is what criticism of the biographical method and symbolic interactionism is targeted at:

As for "theoretical limitations" of both orientations (focus on the subjective and choice of micro-perspective), it will suffice to say that all theories in the social sciences imply a preference of scale and type of studied phenomena and favour certain cognitive styles (...) This formal limitation does not diminish the cognitive value of the approach as regards socially significant issues. [Prawda 1989: 95]

Criteria for quality assessment should take into account a given theoretical-and-methodological option. Thus, the verification procedure used to assess the representativeness of biographical findings differs from the "traditional" one applied in the social sciences. In this case we speak of "theoretical representativeness" – Schütze believes that certain relationships, courses of social processes appear whenever conditions for

⁷ A detailed description of the issues tackled within a given thematic block and examples of respective studies are given in the article by Riemann and Schütze [1987].

their occurrence are fulfilled. These conditions, in turn, change under the influence of the socio-biographical, situational, and socio-historic ramification of the phenomenon under study. Such changes do not invalidate the model but “positively” transform or enrich it [Schütze n.d. b].

If one shares the key theoretical assumption of the narrative interview that forms of linguistic representation correspond to real life experiences, then the soundness of the material lies in its narrative structure. Thus, the quality of a given interview is assessed during the first stage of analysis when the overlaying scheme of communication is identified. The reliability of the findings can be checked by repeating the interview with the same narrator. It is not a common practice, though, as the principle of homology implies the same narrative representation of self-lived experiences every time.⁸

Sceptical remarks against narrative analysis attack the interactional conditions for generating data and the discrepancy between the past and present perspectives revealed by narration. As for the former, I addressed the issue when discussing different phases of the narrative interview. The latter issue subject to criticism can be tackled by addressing the principles of presenting biographical experiences in a narrative. As mentioned above, the researcher can tell the present from the past perspective by identifying schemes of communication (argumentation and narration). This enables analytical identification of the two perspectives, which does not mean that they are not affected by each other. On the contrary, the researcher has a chance to observe the narrator’s biographical work. Biographical work includes working on past experiences (sometimes on the spot), comparing the past and present self-concepts, checking the past perspective against the present one, etc.

Another line of criticism has to do with the question of whether “historical truth can derive from biographical accounts” [Apitzsch and Inowlocki 2000: 56]. Some of the researchers applying the biographical method take such a stance:

The sociologist as the reader or listener of life histories is basically unable to determine whether a description of a past life is what really happened, or what the subject only imagines his or her past to be. [Osterland 1983: quoted by Apitzsch and Inowlocki 2000: 56]

Undoubtedly, the narrative interview cannot be treated as a source of historical information. Biography can be treated as a means of gaining information about social reality. As mentioned above, the researcher is more interested in the way the narrator constructs his/her life story and the choice of events, etc, and not so much in the real sequence of events. On the other hand, placing the narrative in a given context of time

⁸ Such an example of conducting another interview with the same interviewee in order to assess the reliability of material is the case of Ms Stegmann described by Rosenthal and Bar-On. The narrator took part in Dan Bar-On’s study of the children of people who actively participated in carrying out the holocaust. Two narrative interviews were conducted with Ms Stegmann. The first one was conducted by the author of the project, who, being a Jew, represented the victims of the narrator’s father. The second interview was conducted in the presence of Rosenthal, who introduced herself as a descendant of active supporters of Nazism (the granddaughter of a high-ranking member of the Nazi Party and the daughter of the leader of the girls’ organisation *Jungmädel-Bund*). In both interviews, conducted in the presence of listeners with extremely different identities, the narrator presented her biographical experiences in *the same way* [Rosenthal and Bar-On 1992].

and space does affect the interpretation of the text. And this is why narrative analysis should be accompanied by an in-depth study of the relationship between a life story (narrated biographical experiences and events) and a life history (their real course) [Rosenthal 1993].

There is yet another source of controversy. Some researchers of biography believe that a life story is first and foremost a testimony of social reality. Such a belief is shared by e.g. Daniel Bertaux, who writes: "The assertion that life stories do not faithfully describe lived experiences is speculative and lacks empirical justification" [1996: 4]. This stance, called by Bertaux a "realistic approach," is the opposite of the "constructive approach" adopted by researchers who focus on a life story as a text and treat it as a narrative in itself.⁹

The researcher carries out the analysis of material following a detailed procedure. This does not eliminate instances when the researcher, adopting a certain perspective, forces his/her own frame of interpretation upon the material and "makes" it fit into the earlier generated categories or "vision" of interpreting a given phenomenon. Formal rules of interpreting the text and checking one's own perspective against other viewpoints is a safeguard against such situations. The researcher applying the method of narrative interview should discuss his/her views with others, from which both the researcher and the analysis will profit.¹⁰

It should be emphasised that one of the main characteristics of the method is its "openness," that is, the fact that it can be continuously improved, enriched, used partly and modified.¹¹

Last but not least, one objection has to do with the ethical aspects of narrative interviewing. Some researchers argue that it "is a kind of data collection which practically comes closer to spying and investigation." Certainly a researcher does not intend to investigate a narrator. Nevertheless, some objections can be raised. Usually a narrator imagines that the researcher will refer to his/her life history, to the events which s/he is talking about. S/he has no idea about the structural text analysis. A narrator does not realize that every word is carefully analysed, including slips of the tongue. S/he does not know that a researcher is not only interested in what a narrator says (the life history) but also how it is told (the life story) and what the reasons are for a certain construction of

⁹ The discussion initiated by Bertaux in *Biography and Society Newsletter* was continued in later issues, too [e.g. Davies 1997].

¹⁰ For example, workshops during which materials are discussed start with comments from all the participants on the analysed interview. Every participant points out the aspects which are of special interest to him/her. The project coordinator listens to suggestions and takes the floor as the last speaker. This preliminary discussion gives the researcher a broader view of the analysed material and draws his/her attention to the issues s/he was unaware of.

¹¹ Here mention should be made of "The analysis of Rudolf Höss's autobiography" (*Analiza autobiografii Rudolfa Hössa*) conducted by M. Czyżewski and A. Rokuszewska-Pawelek [1989–1990]. The authors modified the analytical categories introduced by Schütze and took some of the steps of biographical analysis. The researchers focused not on the real life course but the way experiences were presented in narrator's autobiography. Another example would be the KBN (Scientific Research Committee) project "Biography and national identity" carried out in 1992–1994 by the research team of the Department of the Sociology of Culture, University of Łódź. A collection of narrative interviews was analyzed by the individual authors of the final report in theoretically and methodologically diverse ways.

one's biography. In other words, the narrator usually refers to the "traditional" image of data analysis and believes that the researcher will choose the best of what he/she says, the most interesting events and omit those which are not elaborated on. However, the formal sequential analysis equips the researcher with the tools to discover the experiences that have faded out of awareness, classify them and interpret the reasons why the narrator projects his/her story in a given way. We must question how far the researcher can go with the interpretation. As long as the text of the interview is treated only as a text, the situation is only potentially ethically dubious. But if a given individual – the author of the story – is confronted with the results of the analysis, the situation becomes much more complicated. The narrator agrees to tell his/her life story (perhaps with quite different expectations as to the research analysis in mind), whereas the researcher analyses it in depth, in detail and comes up with something the narrator does not want to show.

Thus, we must question to what extent the researcher can or should investigate such personal material as a life story. As a rule the narrator's name, details connected with places, and all other indicators that could help identify the person are changed. Nevertheless, there is always a chance that the narrator will be identified, or will identify himself/herself. Therefore, the researcher should always bear the following question in mind: to what extent can one's narrative be treated just as research material.

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A CASE OF GROUNDED THEORY CONSTRUCTION IN SOCIAL WORK

BLAŽ MESEC

QUALITATIVE RESEARCH IN SOCIAL WORK IN SLOVENIA

The first serious attempt at qualitative research in the field of social work in Slovenia was the project on *socio-therapeutic summer camps for children* – Rakitna [Stritih 1975; 1977; 1979]. As path-breaking as it was, practically and methodologically, the report on the research was to my mind a collection of (sometimes brilliant) interpretations grounded not in systematically collected and analyzed empirical evidence but rather in anecdotal evidence, personal recollection, and reminiscence. This is not to say that they were not empirically based, rather that their empirical groundedness was not systematically made explicit. Nevertheless the interpretations validated one another and were more or less accepted by the community of researchers acquainted with the topic.

In my first independent qualitative research on a *summer camp of a boy- and girl-scout unit* [Mesec 1980; 1981] a group of professionals and student-volunteers joined the unit, with the consent of its leadership, to enable the carrying out of preventive tasks with socially endangered children and youth in a local community. As an active participant in the process, I described the conflict which developed between the old leadership and the newcomers and theorized about the strategy of community social work which had been used. Using my scarce field notes, diary and rich recollection I described the phenomenology of the conflict and diagnosed the strategy as a combination of Rothman's [1968] consensual and conflict strategies. Later I reflected on the procedure as a "jump to theory" without proper evident groundedness in empirical data.

For my second research project [Mesec 1986] I joined the *local Red Cross chapter* and participated in the meetings of its board and other activities for several months in order to study the style of the volunteer organization in providing help to the elderly in the community. This time I secured proper empirical evidence: a research journal, including interview notes, minutes of the meetings and organizational documents. In the analysis I used prepared check-lists of functions and a coding procedure. The coding system was based on the dimensions of organizational structure and functions. The result of the analysis was a description of the chapter's functioning in terms of community organization theory, the style of volunteer action and a diagnosis of the organization in terms of fulfilling social functions according to Pincus and Minnahana [1973].

In *Scripts of Survival* [Mesec 1989], a research project which coincided with the societal crisis in former Yugoslavia, my interest was in techniques used by people in securing their social and economic well-being, *status quo* or, in more extreme cases,

their survival. Some 30 interviews with randomly sampled citizens of Ljubljana were analyzed, using free reductive (abstracting) coding procedures with only the broadest categories predetermined. The results were: a description of variables and their modalities; a typology of “scripts” or responses to crisis; a hypothetical model that tried to explain the antecedent factors and the conditions of the different responses.

The forms and content of informal help-transactions were the theme of *The forms of informal help* [Mesec and Dragoš 1993]. Semi-structured interviews conducted by students on a random sample of families in Slovenia were coded to give a taxonomy of forms of help with the following dimensions: steadiness of help, broadness of the network of a habit, nature of the relationship between actors, type of help and content of help.

In *A day in a psychiatric ward* the diary of a volunteer visitor to the women’s department of the hospital was used [Mesec 1994]. A single short paragraph, actually a sentence concerning the temporary leave, was analyzed using the method of the *explication of meaning*: seeking associations to given words, describing the possible characteristics of the context. The result of this process, of which every step was precisely noted, was a theory about possible implicit theories of actors – patients on one hand and doctors or personnel on the other. The conclusion was that the implicit theories of the actors were contradictory and taken together cannot lead to satisfactory therapeutic results.

After a process of reductively coding the paraphrased interview notes, the research project on a *Self-help group of women* [Mesec 1998], victims of marital abuse, resulted in a model that described the experiential state of the women at the moment of their entering the group, the supporting processes within the group and the women’s experiential state after a year with the group. In the analysis the experience or the mental image of the future was stressed.

A CASE OF GROUNDED THEORY CONSTRUCTION

Probably the most important research of this type, as far as consequences for Slovenian social work are concerned, was the study *Social work with family*. A family with a long history of parental alcoholism and abuse and child neglect was analyzed. The research was conducted by Gabi Čačinovič Vogrinčič (1990; 1992) and Mesec [Mesec and Čačinovič-Vogrinčič 1996]. The case file covering some ten years of interventions by the social services was analyzed. The analysis led to a grounded theory which consists of a description of the model of intervention of the social service (the “administrative model” – Model A) and a description of an alternative model, developed *per negationem* (the “counseling model” – Model B).

CASE DESCRIPTION

In 1979 the future mother of the children moved into the house of the future father’s parents to live with him. She was thirty at the time, with a primary school education and not previously convicted. Her extramarital partner was in his late thirties at the time, with an unfinished primary school education, previous convictions and a drink-

ing problem. He was employed at a nearby plant as a driver and she soon got a job at the same plant as a cleaning woman. They built their own home near that of his parents and moved in before the birth of their first child. The mother worked the afternoon shift. In 1982 their first son was born, his development was normal and he was not neglected. In 1984 their son died of carbon monoxide poisoning in his sleep while his parents were working in the stables. The mother was shortly expecting a second child and they were both very traumatized. A few days later their second son was born. He was healthy, loved and well cared for during his mother's maternity leave (one month before birth and eleven months after). However, even during this period the police reported that the parents often left their child alone. The family had moved back to the old house of the father's mother, who was an alcoholic. The child's father and grandmother often got drunk together, quarreling and fighting in the child's presence. The mother fled with her child to her parents, but her partner forced her to return. The quarrels continued and the child's father was often violent towards both his mother and wife.

One day the child's mother arrived uninvited at the center for social services to seek help in this situation which was unbearable for her. She asked for help finding a morning shift job so that she could care for her child in the afternoons. The social worker proposed that she place her son in a government institution or with a foster family temporarily. She did not agree, saying she would rather live alone with her son. Despite her statement, the social worker asked her to bring her child to the social service center, so they could arrange a temporary placement in a children's home. Then they would try to help her and intervene with the company to change her shift and to find an apartment for her. They would also help her partner get into therapy for his alcoholism. The case file states that "Despite agreeing," she did not bring her son to the social service center. Afterwards the social service "examined the option" of institutionalizing her son without the consent of the parents. During a home visit by the social worker and community nurse they concluded that the child was well cared for and showed no signs of possible neglect. The father was also adamantly against the child being taken from the family. The social workers decided that "For the time being it is not necessary to intervene, though the situation should be checked frequently." The community nurse suggested to the child's mother that she should ask the child's maternal grandmother to baby-sit, but the mother did not act on the suggestion. The nurse also suggested to the social workers that they request that her employer change the mother's work shift and that they help the father seek employment and get therapy for his alcoholism. There is no evidence to conclude that any of these proposals were realized.

Following these events there is no data regarding the case for more than two years. In the spring of 1987 a daughter was born to the parents. The mother brought her to the community clinic for regular checkups three times, and after that several more times, though at irregular intervals. The daughter was brought regularly for her vaccinations. The girl appeared to have trouble walking and her development seemed retarded. Shortly thereafter the police informed the center for social services that the parents were neglecting both children. The parents did not respond to an invitation to visit the social service. By letter the social service informed the mother's employer of the situation in the family and requested that they change the mother's shift from the afternoon to the morning.

In 1989 the social worker visited the family and found (“to her astonishment”) that the mother was still working the afternoon shift, “because the plant could not change her shift and/or because the morning shift was not as well paid.” In the afternoons the father took care of the children and the mother did not complain about it. Both children were normally developed for their age. The worker further states that “Despite the fact that there have been no reports of the parents neglecting their children recently, it is still necessary to monitor the family and to help the father cure his alcoholism.” Shortly thereafter the police had to intervene with the family. The father and grandmother had been arguing over the care of the children. During the argument the father broke a window in his mother’s house and the son ran away to a neighbor. The son ran away again the next day. He waited at the neighbor’s house until his mother returned from work that night. A home visit followed and social service professionals ascertained that conditions in the family were improper and that the daughter was in fact neglected. The mother responded to the social services summons and promised to cooperate in trying to persuade the father to go into therapy. She proposed that her children should be given to her mother for care as she was aware that the threat of her children being taken away by the social services was very real. At the home visit the father “agreed to go into therapy. We agreed that I [the social worker] would prepare everything necessary and inform them of the hospital admission date.” The father behaved repentantly, asserting that he would never drink again. He was very persuasive and the social worker decided not to have the children taken away.

One day the son took refuge at the neighbor’s house again, after wandering all over the village, crying and saying that he did not dare go home because he was afraid of his father. His father came after him and began to strangle him in front of the neighbors. The son turned blue and lost consciousness. The social service decided to immediately take both children away because of the direct threat to their lives. They were both placed in a children’s home. The oral order was pronounced in the home of the family in the presence of the police. The parents visited their children in the institution, the mother more regularly than the father. The local court initiated an investigation into the parents on suspicion of assault and neglect of juveniles. The parents both received suspended prison sentences and the father additionally received a sentence of obligatory alcoholism therapy.

THE ANALYSIS

The qualitative case analysis comprised four successive stages: (1) constructing a chronology of events, (2) analyzing the time patterns, (3) conducting an analysis of “action-patterns” and (4) an analysis of implicit theories. The analysis was followed by the final conceptualization (formulating a grounded theory).

1. Constructing a Chronology of Events

On the basis of the chronologically arranged file we reconstructed (1) the chronology of events, followed by an overview in the form of (2) an abridged chronology which was later presented in (3) a chronological diagram and followed by (4) a synchronized

(combined) chronology which plots the actions (events) taken by the various actors in the case, and (5) a corresponding synchronized chronological diagram representing the events regarding the different actors. The final step in the construction of the chronology of events is (6) a chronological summary of the key periods in the development of the case. We will omit the details of the construction and presentation of the chronology and go right to the analysis. The analysis of action-patterns comprised: (1) an analysis of the general time-patterns of the events (actions), (2) an analysis of event sequences, and (3) an analysis of the logic of intervention (implicit theories of the social service).

2. Analysis of Time Patterns

1st Pattern

Pattern. In the simple diagram we can observe a *growth in the number of events* over time from the first contact with the family to the final pronouncement. There is an increase in negative events in the family and in the measures of the various services involved. This general increase can be seen on both sides, in the family and in the services.

Interpretation. In the course of the decade during which the family was monitored, a step-by-step increase in the number of events can be observed. This is the case with the number of events within the family (the activities of the family members), as well as with the number of interventions and measures by different services, especially those of the Center for Social Work. An increase in the intensity of events can also be observed: in the family – alcoholic deterioration, increasing violence and threats; in the services – increasing pressure, control, threats and sanctions. A gradual deterioration in the family's condition and the growing pressure and seriousness of the measures taken can be observed over the ten years of monitoring the family.

Concepts. The words which come to mind when thinking about what happened in the family include: increasing tension, conflict, disintegration, violence and "threat to life." The words which come to mind when thinking about the services include: increasing control, pressure, threats and sanctions. The concepts are GRADUAL INCREASE, ESCALATION: There is a rise in the number of events over time and a worsening of the problems.

2nd Pattern

Pattern. The second characteristic revealed by the diagram is *the repeated interruption of this rising line*. The events-in-time-curve has three peaks. The peaks are separated by distinctive incisions; *there are discontinuities in the course of events*. Throughout the period of monitoring the case, large gaps can be found in the documentation. There are long periods of no events, of latency, sometimes of several years. This holds true for the events within the family as well as for the measures taken by the services, and leads to the conclusion that there were long breaks in the treatment of the case.

Interpretation. The action is not continuous, it is sporadic, interrupted. If the content of the events is taken into consideration, it can be said that the periods with events have the character of "shocks", outbursts of violence, followed by the intervention of the services. These are followed by quiet periods, periods of "latency," when the services did

not work with the family. We can assume that these breaks were due to a calming-down of the family situation and to keeping incidents in the family within bounds that did not draw the attention of the environment.

Concepts. DISCONTINUITY: There is a discontinuity in both the reporting of incidents in the family and in the reporting of these incidents by the services. INTERCHANGE: There was an interchange of periods when violence was clearly manifested with periods of latent violence or even no violence, and at the same time an interchange of active and passive periods of the social services.

3rd Pattern

Pattern. The synchronic diagram shows the time distributions of events on the side of the family and on the side of the services separately. *The curves have similar runs:* each has three peaks. This corresponds to the image of the “three peaks,” seen on the combined diagram. Both curves are almost completely alike. Events in the family-curve are slightly more dispersed; events in the services-curve are somewhat more concentrated towards the end. There is a symmetry of events: the events in the services follow the events in the family. As long as there are no events in the family, there are no actions by the services.

Interpretation. The complete course of events shows that the events on the part of the services follow events in the family. This points to a mutual dependence of events in both spheres and leads to the assumption of reactivity on the part of the services. Actions on the part of the family have the character of outbursts of problems (conflicts, violence). Similarly, actions on the part of the services have the character of outbursts of interventions (agreements, threats, sanctions), all meant to settle the outbursts in the family. One can compare this situation to an electric or light pulse in contrast to continuous light. We can imagine the family living in a dark cave. What a difference it would have made if the family had been provided with the constant, although weak, light of, for example, a small fire, instead of experiencing the strong light of a lightning-bolt accompanying a thunderstorm every few years! These metaphors, although they might seem remote and exaggerated, help us to comprehend the basic characteristics of the course of events. In the case at hand the occurrences are *impulsive* and this is their basic characteristic. When there is an outburst of violence different measures are taken, as if a fire needed to be put out.

Concepts. There is a CORRELATION in the time sequence of the events in the family and in the services. All three patterns together suggest the concept of OUTBURSTS and EXTINGUISHING, outbursts of problems and of measures to extinguish them, and, finally the concept of the services' REACTIVITY: The services did not work with the family in the periods between the outbursts; they waited until something happened in the family, then they tried to settle things down.

3. Analysis of Sequences of Events (Inter-Actions)

Thus far we have studied general patterns in the distribution of events over time. Now we are going to concentrate on the sequences of individual events. The connections between the events, as regards their content, will be of interest.

1st Pattern

Pattern. If there is bad news from the family, it is followed by an intervention; if there is good news or no bad news, there is no intervention, no working with the family.

Interpretation. Negative behavior is punished while positive behavior is neither rewarded nor supported effectively or sufficiently. The services concentrate on negative occurrences, “excesses” or “defects.” The services do not ask about the positive aspects of life in the family, about its own resources and potential. Such potential doubtless exists within the family, otherwise it could not have undergone long periods without any disturbing events. In this sense, long periods in the life of the family are lost as regards appropriate treatment, for the empowerment of the family.

Concept. Since we mentioned the concept of reactivity above, we can now propose another concept: AN ORIENTATION TOWARDS CONTROL AND NOT TOWARDS EMPOWERMENT.

2nd Pattern

Pattern. When the first child died in unclear circumstances the family needed help in order to master the burden. However, the *family did not receive such support*, because the event was characterized as an accident; therefore no violation of social norms on the part of the parents was involved. They did not ask for help and the social services did not intervene.

Interpretation. Such an accident would be a serious burden for any family, even if no other burden, such as criminal prosecution, is added. Even if the parents are not formally accused, they feel (or should have felt) guilty about their child’s death. The police knew that, and therefore kept the parents under surveillance. In any case, it should have been necessary to work through this experience with them and assist them in carrying out their role of parents in the future.

Concept: ORIENTATION TOWARDS DISTURBING BEHAVIOUR AND NOT TOWARDS THE FAMILY’S DISTRESS.

3rd Pattern

Pattern. When there is an incident in the family, the social services come and take a look to see if the situation is bad enough (a mild intervention) *to justify an administrative measure* (a serious intervention). The situation in the family calms down, and this is viewed as reason enough to postpone a serious intervention.

Interpretation. The services wait for an excessive incident which would justify the application of formal *administrative measures*, as if no social work *counseling measures* were available. This orientation towards administrative measures keeps the attention of the services on an estimation of the seriousness of the family situation and does not allow their attention to focus on other aspects which could support the family. It looks as if the services did not believe in the possibility of achieving anything without applying formal administrative sanctions (the removal of the child from the family, a prison sentence); or as if they believed that social work consists solely of applying those sanctions and of the tasks which accompany them.

Concept: ORIENTATION TOWARDS ADMINISTRATIVE MEASURES AND NOT TOWARDS COUNSELING WORK.

4th Pattern

Pattern. There is an act of violence in the family, the neighbors call the police, the police intervene, the police inform the Center for Social Work, the community nurse suggests that some measure be taken, the center takes action (for example: a visit to the home).

Interpretation. The Social Work Center is the final stop, the last in line. It is the last to learn what is going on and is given instructions on what to do by everybody else. It may be inevitable that it is the last to learn about an incident in the family, but carrying out the instructions of others certainly is not inevitable. It appears that the community nurse is the one who is more engaged and pro-active (when she suggests that a morning shift position be arranged for the mother) rather than the social workers who should be managing the case.

Concept: REACTIVE, NON-PROACTIVE HANDLING OF THE SITUATION: THE SERVICES AS THE EXECUTOR OF ORDERS, NOT AS INITIATOR AND ORGANIZER OF ASSISTANCE.

5th Pattern

Pattern. The mother comes for help and suggests a solution. The social worker does not accept it, and is also not ready to negotiate to find a compromise. She apparently persuades the mother of the suitability of her own solution. The mother seems to agree, and signs the protocol, but then simply does not keep her promise.

Interpretation. The fact that the mother sought help by herself is, in terms of social work theory, extremely valuable. It should have been used to involve her in the problem-solving process. The social worker tried to do that but in an ineffective way which repelled the mother. Even if the mother's suggestions may not have been fully acceptable, the social worker should have paid attention to their content. Instead of negotiating a compromise solution with her, she tried to "persuade" her of the correctness of her own, pre-made decisions. The mother obviously agreed to them on "foreign territory," but when she was confronted with the difficulties of the existing situation at home, she gave in and did not keep the promise made to the social worker. The social worker did not see the unrealistic nature of the mother's compliance, and therefore continued with this action-pattern.

Concept: THE IMPOSITION OF A *PRIORI* SOLUTIONS, NO JOINT SEARCH FOR MUTUALLY ACCEPTABLE SOLUTIONS.

6th Pattern

Pattern. The social worker devotes her time to visiting the family and always secures the assistance of other services, usually a community nurse, and sometimes the police. No attempt is made to exert pressure on the employer to secure a more convenient work shift for the mother. A letter from a social worker is regarded as sufficient. There is no personal visit to the company and no assistance from other services.

Interpretation. Arranging appropriate working hours for the mother seems to be a very important measure since it would secure childcare during a critical part of the day. In spite of that, only a minimum of available measures was taken (the sending of a letter with a request); but on the other hand all available control "machinery" was used to exert pressure upon the family. A single letter to the company proved to be ineffective, with no consequences as regards the considerations of the social service.

Concept: EXERTING PRESSURE UPON THE FAMILY, NOT UPON THE SOCIAL ENVIRONMENT.

4. Analysis of Implicit Theories

Implicit theories (“theories in use” according to Schön [1983]) are the concealed views of actors. Such views are not articulated and made explicit by the actors, but are revealed through their actions. Revealing such theories is an uncertain and vague task. Therefore they are only cited here as hypotheses, as possible formulations of the views concealed behind the actions of the services and individuals in the aforementioned case to show what is meant by this procedure. Let me name only two such theories which are possibly concealed behind the actions of the social services in this case:

a) “*The main reasons for the problems should be removed, then it will be easier to deal with everything else.*” According to this theory, the main reason for the problems in this case is the parents’ alcoholism, above all the father’s. The efforts of the social worker are primarily oriented towards persuading the father to start treatment for alcoholism. The social worker is probably thinking along the following lines: “If the principal reason is not removed, all further efforts are in vain.” The social worker does not try hard enough to help settle other matters which are not directly connected to the treatment of alcoholism. By settling those affairs (the working hours of the mother, the mother’s employment in general, other possibilities of child-care, other assistance for the family provided by relatives and neighbors) the father’s alcoholism might not be as disturbing and fatal. Other possibilities for saving the mother and children may exist. Finally there might also be other possibilities for stabilization or possibly even treatment of the father’s alcoholism. Efforts should not be directed toward the “main reason” for the problems, but rather, we should strive to bring about any possible change. The situation as a whole is the problem. We treat and change single parts, thus affecting the situation as a whole.

b) “*There is the threat of child-neglect involved in this case.*” The social worker diagnoses the case as one with a “threat of child neglect” and not as one of possible family distress with manifold origins and circumstantial conditions (cohabitation, conflicts with the grandmother, alcoholism, the mother’s working hours, the death of the first child). The situation, however, also has manifold possibilities for normalization and for allowing a positive development to prevail. The actions of the social worker are directed towards preventing child-neglect, and not towards relieving distress and helping with the self-organization of the family. She has two dimensions in her mind: alcoholism and child neglect. The ideal in her opinion would be to undermine the fundamental evil, alcoholism, and get treatment for the father. As she is not successful in that, she concentrates on the problem of child neglect. The central question becomes *whether the children are being neglected enough*, or whether it is clear enough that they are being neglected, *to be able to impose an administrative measure* in order to protect the children. When estimating the degree of neglect (upon visiting the home) and the necessity of an intervention in order to protect the children, the time which could be used for different methodical approaches (counseling, negotiations, and representation) is wasted.

FINAL CONCEPTUALIZATION: THE FORMULATION OF A GROUNDED THEORY ON THE ACTIONS OF THE SOCIAL SERVICE

Finally, the results of our analysis are summarized in a theory. Maybe the term should be put in quotation marks to emphasize the following: (1) The result of the analysis is not necessarily a complex interwoven and hierarchical system of explanations; it can be limited to the observation of a rule, of a pattern, the construction of a term, typology, etc., and (2) it is a hypothesis which needs to be examined against the same or similar material and then supplemented accordingly.

When summarizing the partial findings in a theory, it is not improper to exceed the limitations of the initial material to a certain degree. In fact, it is not possible to form concepts without a leap away from or above the empirical material. Any concept is a unity of empirical referent and theoretical thought. It is possible that the concepts do not have immediate support in the material, but can be inferred from it. Our case is a good example of how the concept of one mode of social work strategy (that which appeared from the analysis of the material) *implies* the concept of another mode which is not directly contained within the material. Sometimes the inventing goes even further: for example, when we formulate a concept analogous to an empirically extracted one. In all these cases the “inventions” are connected to what is merely abstracted, not invented out of pure fantasy. Nevertheless we have to bear in mind that the concept gained by a gradual process of abstracting is – in the proper sense of the word – an invention too. It is a thought, a mental construction, not a thing or a fact.

Analysis of the empirical material brings us to a point which allows us to identify *two social work strategies* which were or could have been applied in our case. Provisionally these are called strategy A or the CONTROL (ADMINISTRATIVE) STRATEGY and strategy B or the STRATEGY OF HELP (COUNSELING STRATEGY). Their characteristics are:

A. Control (Administrative) Strategy

1. *A passive-reactive pattern of action.* The actor (the center for social work) waits until events which can be defined as deviant occur in the family and then it reacts by taking measures. This is true not only at the onset of the case. Such an orientation is particularly evident at the end of the latency periods, when only a new excess leads to an intervention.

2. *An orientation towards administrative measures.* Despite ten years of work on the case and numerous opportunities to assist by means of counseling, negotiation, or advocacy, social care ends in administrative intervention. The reason seems to be the initial definition of the situation as being a problem which would or will require an administrative intervention once a certain limit has been reached.

3. *An asymmetry of power between the social services and the client (the family).* This asymmetry is shown not by the fact that the services decided on an administrative intervention in the end, since it was justified, as the children were in mortal danger. It follows from the fact that the entire procedure was dominated by *the services' idea* of how to solve the problem (that the father had to start treatment and the children were to be

transferred to a children's home). In addition, the ideas and suggestions of the mother (and other family members and relatives, for example the father's sister) who wanted to preserve the family and keep the children by any means possible, were not taken seriously or regarded as a starting point for work on the problem. Setting too demanding and hardly realizable tasks for the client is not empowerment, it only seems to be. From a distance this might be seen as idleness on the part of the social worker, who perhaps thought about the mother that "*she* should do that, why should *I* make all the effort," or manipulation "if it does not work, then the children will have to be taken away."

4. *Moralistic-punitive orientation*. The taking away of the children and the sentence pronounced on the parents did not only protect the children, and express a justified condemnation of the parents' maltreatment of the children, but also served as an expression of the belief that the punishment has a preventative function and will bring the parents to their senses. It will teach them proper behavior. There is no questioning whether, or in what way, such measures work.

5. *Linear-causal thinking* is the social service's dominant mode of thinking in this case. Distress and problems were defined as a consequence of certain causes, primarily the father's alcoholism. All the attention, efforts and energy were spent on fruitless attempts to remove this cause. In this way a cycle of escalating pressure and deviation, a spiral of failure, began.

B. The Strategy of Help (Counseling Strategy)

1. *A proactive pattern of action*. The periods of latency during which the family calmed down and there were no excesses, should have been used to observe how it managed to maintain such (supposedly more favorable) conditions. The service should have supported those aspects which could help the family to preserve such "quiet periods" for a longer period of time. The family should have been observed for times when its efforts to maintain quiet periods became unsuccessful and the family needed support. The social worker should have led the case, she should not have – like the family – been a victim of the events.

2. *Orientation toward methods of social work*. Administrative intervention is one of several methods of social work, not the only one. It is used most frequently in cases of extreme necessity and has serious consequences. Therefore, the strategy of help concentrates on using other methods which can be effectively applied by social workers. The theory of social work describes an array of approaches and "skills" which can and should be used besides the administrative approach [Lüssi 1991].

3. *Empowerment of the client (family)*. The action of the social worker should not be oriented towards the implementation of a previously defined "solution to the problem" by applying the power/authority of the services, but rather towards the empowerment of the family. Efforts should be aimed at helping the family look for its own resources, power and possibilities of development; towards the search for the positive. The tasks assigned to the family or social services should be realizable, not too demanding, and their implementation accompanied by a feeling of being successful. The social worker should have considered whether there even existed a possibility that the wife could persuade the husband to start treatment; if this was not the case it would have been better

not ask the wife to do so. She should have considered whether there was any chance that her employer would change the mother's working hours solely on the grounds of a humble written request. If such a chance was slim she should have tried another approach. When working, our aim should be to achieve success and not to look for an excuse for a failure by being "active."

4. *A pragmatic-realistic orientation.* The social worker should be oriented towards what is realizable and possible to achieve, without having to make a negative intervention in the family. This is the way to avoid a moralistic-punitive stance and the eradication of "main causes" at all costs. It is better to orient ourselves towards "patching up minor holes" and making small pleasures and successes possible, towards reaching short-term rather than extensive agreements, establishing optional contacts, and building-up a "network of good relations." We are aware that minor changes can bring about a more extensive reorganization of life, which can make life endurable, despite an "unsolved" problem.

5. *Systemic epistemology.* The last – but in terms of its content, primary – component of this strategy is a different approach to knowing which acknowledges the universality of circular patterns of mutual causation, and the systemic nature of changes, meaning that small changes or mutations within a systemic whole can lead to large transformations [Stritih and Možina 1992].

CONCLUSION

We have shown how we constructed a grounded theory within the tradition of Glaser and Strauss out of an ordinary social work file. A specific – albeit not rare – characteristic of the file used was that it covered a time-span of some ten years. This circumstance dictated the use of chronological analysis, which in itself is a quantitative procedure, but which allowed for qualitative coding too. Is our theory valid? We have direct and indirect indicators of its validity. The direct indicators are a number of theses by our students on other cases of social work intervention which showed a similar pattern. The indirect indicators include the general agreement voiced by social workers in the audiences of our informative lectures on the research that the analysis corresponds to their experiences and that, in fact, this was what was happening in Slovenian social work.

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THE QUALITATIVE ANALYSIS OF AN INTERVIEW

VANJA HUZJAN

CASE: ORGANIZING A VILLAGE FESTIVAL

Qualitative analysis is aimed at the worldview and system of relevance of the informant. Unlike quantitative analysis, which examines hypotheses, qualitative analysis discovers new relations: the informant (the research object) with his/her perspective enters the researcher's consciousness and thus provokes the subject's, the researcher's, self-reflection. The research takes place in the interaction between subject and object/subject.

Alfred Schütz describes the real life-world, which the researcher thematizes, as the "original sphere of reality, in which we make sense of and interpret our experiences and actions before every science" [Adam 1987: 1590–1591]. This is the world of the individual's pre-reflection, *common sense* understanding of society, dependent on upbringing and the tradition of the socializing environment: it defines the horizons within which we understand our world and allows the members of a community to *theoretically* explain the functioning of a given phenomenon.

Barney Glaser and Anselm Strauss, founders of *grounded theory*, suggested to researchers that they "discontinue their practice of bringing theories to the field and gathering data with the goal of disconfirming or verifying those theories" [Tesch 1992: 23]. Instead, they have described a method whereby the researcher temporarily gives up his/her theoretical concepts, collects information and tries to derive a theory or hypothesis from them. In this way the researcher approaches everyday life and thematizes that which is self-evident to the actors and is the object of reflection only in crisis, when the existing body of knowledge cannot provide a routine solution to new problems. Because human thought and action can not be experienced directly, as they are known only to the actor, the researcher discovers how an individual explains his real-life world by asking, listening and later analyzing the recorded narration. Glaser-Strauss' grounded theory attempts to "reconstruct that theory, within which the individual perceives and sets concrete real-life questions and the answers to them" [Fikfak 1999: 15].

WE – THEM

While researching the importance of a village festival in a village in the Slovenian southwest in 1992 I spoke with the chief organizer of the event, Jan (19 years old), during the

preparations for the celebration. One of the questions of the research¹ was whether the boys would organize a dance at the most important village festival:²

V: *Well, come on an' tell me, what was up with this festival, I was talkin' with Blaž yesterday an' he told me there wouldn't be a dance, you know, an' today you were already preparin', it'd be interestin' ...*

Everybody answers at once: *He's trash, he's trash metal.*

M *Blaž isn't, we aren't, like, the same music.*

V: *What's that mean? What's this, trash?*

Everybody at once: *Almost, well, you know, like hard core. You know what's hard core? Sorta, like, just like heavy, well, that kind of music.*

J: *It's a kind, for example, (1s) like a car. (Laughter in the background.) A guy's got a Golf, another's got, I don't know, (I don't understand the word), Yeta. (1s) But this is different, there's the trash metal ... (5s, many voices, can't hear anything, 3s pause.)*

V: *Well, OK. (1s) Blaž is different. (My laughter.)*

N1: *Blaž is trash peas[ant]-o- metal.³*

M: (laughter for several seconds) ... *peas-o metal ... (more laughter)*

The young people did not answer my initial question about the preparation of the dance; instead they spoke about Blaž, who had implicitly denied their competence to organize the dance. In my question to Jan, I mentioned Blaž in order to tell them what I had heard and to learn Jan's thoughts about the relations between the present group of young people, to which he also belongs, and Blaž (V: "... *it'd be interestin' ...*").

The reaction of the group of young people was very energetic, as they speak all at once. They immediately define Blaž: "*He's trash, he's trash metal.*" I am interested in what kind of person is "trash metal." Jan tries to explain it to me and teaches me, that "trash metal" is a kind of music and chooses an analogy with cars. The situation seems very funny to those present. He then adds: "*But this is different, there's the trash metal ...*" The word "*there*" indicates another world, another reality ("*But this is different.*") and locates Blaž outside the community. Not here, but there. Marjeta's sentence: "*Blaž isn't, we aren't, like, the same music ...*" likewise disqualifies Blaž, who is an outsider to them. The exclusion is manifested in the opposition WE (are) – HE (isn't). In the world of the young he is not competent to explain the organization of the dance because his behavior and real-life world are different from the common, dominant cultural pattern of the village.

The citation shows an identification of music with thought: "*That's one kind of*

¹ I recorded the conversation with the chief organizer, Jan (19 years old), about organizing the dance in 1992 in a village in the Primorska region (the southwest of Slovenia) during the preparations for the celebration of a village festival (15th August – the Assumption of Mary).

² The selected parts of the conversation were not transcribed phonetically in Slovenian. In English an (imperfect) attempt has been made to reproduce the "feel" of the dialects spoken. Seconds indicate the pauses between passages of speech. Words written in capital letters are explanations regarding the narrator's speech and indicate how and in what circumstances something was said. Simultaneous speech is indicated with the "#" sign.

³ In Slovenian "treš kmeto metal"; "kmet" (peasant, farmer) and "met" sound very similar, giving a rhyme and making the joke more effective, the translation to "peas-o metal" is an attempt at capturing this effect.

music, one kind of thinking.” The music Blaž listens to is “trash,” which is like “hard core,” only “heavy.” Why does Blaž identify with this kind of music? And why do the others recognize him through it?

Peter Berger and Thomas Luckmann state that during the process of socialization “the individual does not adopt only the role and stance of others, but at the same time also adopts their world” [1988: 124] and internalizes their language. The identification therefore takes place within the horizon of a determined social world. They also reason that the phenomenon of the *outsider* is the result of unsuccessful socialization, which can be the result of “the heterogeneity of the socializing environment” or the “transmission of markedly incompatible worlds” [1988:153–156]. What are the consequences of unsuccessful socialization? People with a value system different from the majority system have to find their place outside the village community. This position “outside” marks and punishes them at the same time. Unsuccessful socialization thus opens the questions: How is Blaž’s person marked, what is his identity like?

Returning to the textual analysis, “heavy metal” can mean weight, solidity, immobility, figuratively it signifies a powerful personality, a forceful influence. “Hard” and “heavy” are in many ways synonyms, while “core” marks the centre, the fundament; figuratively “hard core” would mean a tough nut, hard to crack: a person that is difficult to understand, a riddle. Implicitly there is also an allusion to a genre of erotic production. “Trash” means a worthless commodity, refuse, garbage, figuratively it can mean trouble, difficulties in getting along. It seems that Blaž’s position in the community is easily identified by the music and way of thinking denoted by the translated words. Who is this Blaž, who is standing outside (a certain defined social horizon), not respecting the common cultural pattern of the village? Is his identity defined precisely by his position, his outsider status? Better yet, what would Blaž have to do to become an insider?

N1 (Unknown 1) and Marjeta say: “Blaž is ‘(trash) peas[ant]-o metal’.” What does “*peas-o*” mean: A psychological characteristic or behavior different than or opposite to the majority? In this case the adjective “*peas-o*” has a pejorative meaning. The bricolage of otherwise incompatible attributes shows a self-demarcation of the young from different music and which could be “peasant,” as well as an attempt to stigmatize Blaž as an outsider. Questioning the organization of the dance has unexpectedly defined Blaž’s position within the community and the basic values of the group of young people.

In light of the relation between insider and outsider, it is important to ask oneself what must a young boy do to become competent to prepare the dance? Erving Goffman [1987] emphasizes that each actor must perform his role in order to maintain a common definition of reality. One who, for whatever reason, does not help maintain the common construction of reality is an outsider. An institution endows an individual with the authority to speak and, according to John L. Austin [1990], to do. Bourdieu states:

Hence the efficacy of the performative utterance presupposes a set of social relations, an institution, by virtue of which a particular individual, who is authorized to speak and recognized as such by others, is able to speak in a way that others will regard as acceptable in the circumstances. [1994: 8–9]

Such an individual can be called an insider.

Before analyzing the relationship between insider and outsider we might give a thought to the following excerpt:

V: *I jus' wanted to ask you somethin', (2s) somethin' real important, yeah, why was it like so late (1s) this year? You simply decided, if you hadn't decided ...*

J: *No, you know, (2s) (PENSIVE) a long time, a long time nobody (unclear; 2s) ... the dance. (1s) Then, (1s) two weeks ago I told him, to 'im, (1s) that Darko, yeah, the one with the band, Mars.*

V: *That's the guy, the Hrastoučan?*

J: *The guy, who'll be 'ere now. (Somebody calls: "Marjeta!" and Jan stops talking for 1 second.) Now, I tell him, that, if he'd play 'ere.*

V: *Hm.*

J: *'N then (2s) it was all alright, he told me, we had a deal, there wasn't any better to get. You (1s) can't prep music like a week before. (1s) Like this: I was sick, still am, still am. (4s) Nobody prep'd anything, (1s) no. But without a dance in Hrastovo, that'd be somethin' weird. (1s) A guy promised he'd make us run to Split (Marjeta laughing.), if it wouldn't happen, (Marjeta laughing.) another, that he'd kill us, (1s) now it has to be.*

V: *Who?*

J: *The dance, the dance. #*

V: *I know. Who said that? #*

M: *It's tradition. # A guy. A guy 'ere.*

J: *Older.*

V: *Older, younger?*

J: *Older.*

V: *Aha, Older. (3s)*

J: *Tha's how we decided. (1s)*

V: *Aha.*

The question “*why was it like so late (1s) this year?*” is almost a request emphasizing the importance of this question. I was interested why they organized a dance at all that year and if it was even possible that there wouldn't be a dance for the Assumption of Mary: “*You simply decided, if you hadn't decided ...*” Precisely this addition to the question directed Jan's narrative and indicates breakthrough in our conversation: because I acknowledged Jan's competence he became more trusting, he reconstructed the (back) story that resulted in him becoming the chief organizer. After finding out, that for “*a long time, a long time nobody*” decided, he explains the circumstances in which he implicitly took on the role of organizer: “*Then, (1s) two weeks ago I told him, to 'im, (1s) that Darko, yeah, the one with the band, Mars ... Now, I tell him, that, if he'd play 'ere.*” For a long time (the repetition “*a long time, a long time*” indicates the duration) nobody in the village took the role of chief organizer, then Jan agreed with Darko, that he (Darko) and his band would play. How could the agreement with the musician be the key event that put Jan into the role of chief organizer? Jan offers a possible answer himself: “*But without a dance in Hrastovo, that'd be somethin' weird.*” He was aware that there had to be a dance (also because of our summer research camp) and that Hrastovci give him the right to organize the dance; because of this attributed competence he felt a moral obligation, which was supported by threats: “*A guy promised he'd make us run to Split (Marjeta laughing.), if it wouldn't happen, (Marjeta laughing.) another, that he'd kill us, (1s)*

now it has to be.” To my question, who said this, Marjeta replies: “A guy. A guy ‘ere.” Jan adds: “Older.” The authority of the older generation of villagers demands the renewal of traditions: the tradition is articulated by the authority it personifies. However, despite the authority’s demands being clearly articulated, the tradition has the appearance of something fixed and unshakeable, something above the individual: “It’s tradition.”

The village still identifies strongly with tradition. The traditional (routine) patterns of behavior assure the security of everyday and holiday life. For this reason my question: “Who said that?” was completely naïve from the point of view of the village youths. That it’s a question of tradition, the authority of the elders, is completely obvious in their world-view as it is characteristic of silent (unsaid) knowledge that everyone is presupposed to possess it.

The selected passage reveals both Jan’s position in the village and the influence of village authority on events in the village, on what happens. Who, then, is the insider? The elder insider is the one who possesses the power of authority and demands, that the tradition and the values stemming from that tradition be preserved. The youth who decides to be chief organizer, is in fact a “candidate” for entry into the community of elders. With the initiation, that is, by preparing the dance, the organizer proves that he is entitled to certain competences within the communities and is thus entitled to be an insider.

Therefore the complete opposite of the outsider Blaž are the insider youths who participated in the conversation: Jan and his assistants. As organizers of the dance they are competent and thus legitimate representatives for the transmission of tradition. An insider heeds the rules set by tradition, which is personified by the older members of the community, who by their behavior and language manifest their authority. When an insider respects the rules and behaves according to them he indirectly nurtures them and thus preserves the general cultural pattern of the village. With such an attitude the community accepts him: the authority of the village rewards his attitude by granting him certain competences.

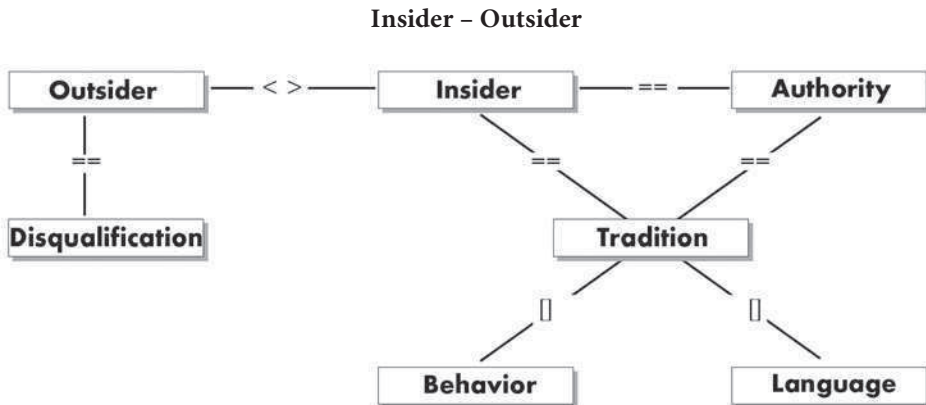


Figure 1: The categories of insider and outsider are connected as opposites. The insider is the authority, for he personifies tradition by maintaining the traditional world-view with his behavior and language. The outsider departs from the traditional norm with his behavior, thus he is outside his community, disqualified and incompetent. The outsider holds up a mirror to the insider by delimiting the generally acceptable world-view with his behavior.

RELATION TO TRADITION

We have found out that the older villagers still identify strongly with tradition, but what about the youths?

J: *Yeah, once there wasn't a dance in the evenin', no. (1s)*

V: *How then?*

J: *After mass (1s) the dance started (1s) and till three, four, well the four -thirty (1s) in the sun, right, (word is indistinct) they bored you, till three.*

V: *Aha, this dance doesn't take place anymore?*

J: *It does. Will. This year it will.*

V: *Where, here?*

J: *It will. Marko knows, he's been filming, other years. When mass ends, (1s) there's like a (1s) peasant (EMBARASSMENT) show. (Laughter)*

N7: *What show?*

J: *First there's the show in front of the (1s) church. (7s) An' then there's a procession up, with an accordion in the morning and then it's on the dance ground and they dance. It's called: first dance.*

V: *First dance. (2s) But the party ... (2s)*

N7: *In the evening.*

J: *In the evening. (1s) Y'know what, now I don't know ...*

M: *Y'know how he kills you ...*

J: *I dunno, I bet, you can't drink during the day, right. (Laughter)*

M: *That's for the older ones, like, who're at mass, for everyone at mass, then they go all together ...*

N7: *'Bout three beers and they're good for.*

M: *An' then ... he drinks a bit, an' it shows, what he drinks. A little like this, an' they're (laughter, indistinct). (2s)*

Jan defines the first dance: “*When mass ends, (1s) there's like a (1s) peasant show.*” It is interesting that N7 (Unknown 7) sounds surprised: “*What show?*” which demonstrates that N7 doesn't think of the dance as a show and that in his world the tradition is not questioned (yet). How can we interpret Jan's statement?

What does the first part of the (syntagma) description “peasant” mean? Do the youth still identify with the adjective “peasant,” which describes life in the village? Is the “*peasant show*” the one in which the older people perform, those who still identify with a peasant lifestyle? The momentary reflection (1s) points to several possible interpretations: It is possible that Jan's use of the attribute “peasant” distinguishes two generations; or does Jan feel the need, because of the foreign questioner, to set up a distinction from the village; however, the “*peas[ant]-o*” in Blaž's case definitely has a negative connotation, as it defines him as an outsider.

What about the “show”? When Erving Goffman [1987] analyzes the structure of social interactions (from the perspective of the stage), he is interested in the way in which an individual presents himself and his activity to others, how he directs and controls the impressions others have created of him. The show has the nature of a routine and the actor is usually unaware of how cliché his play is. With the term “show”

Goffman characterizes all the activities of a certain participant in a certain situation, which serves to influence any of the other participants. The audiences, watchers and other participants also contribute to the show. The show is therefore determined by roles (lead and supporting actors), scene, props, audience, etc. The basic work of stage manager and director is carried out by the tradition in our case. The scope is already given, only the individuals assuming the various roles change over time. They do not create the roles themselves, rather they choose from among already institutionalized possibilities.

Is Jan’s position already clear? In his unease with the “peasant show” and with Blaž we can see the opposition of the modes of understanding the world: the worldview of the youth and the worldview of the older people. Jan, as a 19 year-old belongs to the world of the young; the relation of the young to tradition also helps to define his relation. What is Jan’s relation to the tradition? On one hand it is defined by Jan’s social role, on the other by the ambivalent relation of the youths to tradition, a group to which he belongs. Jan, as chief organizer of the dance, makes a sacrifice in order to restore the tradition (he is an insider), but as a youth he has (on a certain level) a cynical relation to the tradition (he is an outsider).

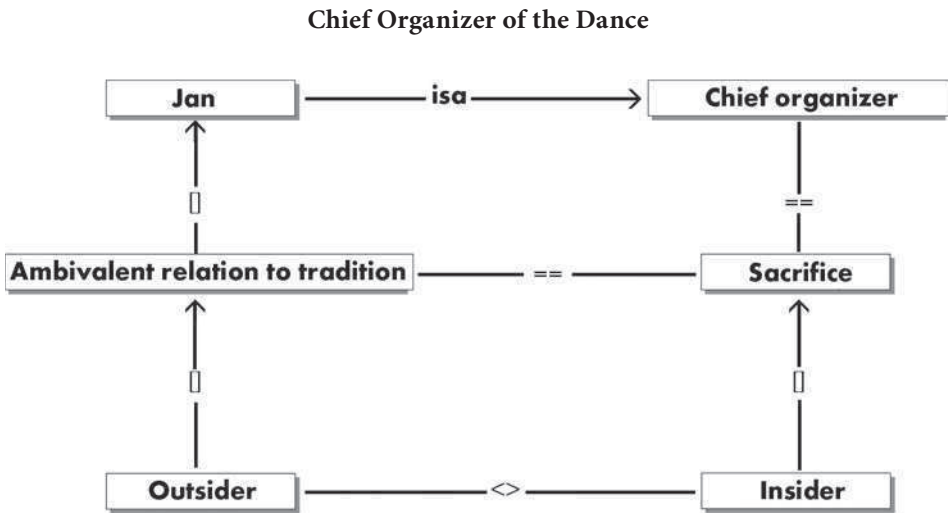


Figure 2: Jan is faithful to tradition in the role of chief organizer, his sacrifice is the decision to renew the tradition, his membership in the group of youths in the village links him to an ambivalent relation to tradition, which puts him into the position of an outsider.

In the cited passage the young question the behavior of the older people at the first dance. The elders, who should be an authority to the youths, are already drunk in the afternoon according to Jan. Marjeta is of the same opinion: “An’ then ... he drinks a bit, an’ it shows, what he drinks.” N7 comments that the older people drink “Bout three beers and they’re good for.” The youths critically evaluate the behavior of their

elders. Besides the already analyzed passage, which mentions the threat of the older man that the youths would have to run all the way to Split, we should also mention the following one:

V: *What about the older people? (3s) They aren't satisfied? (2s)*

J: *See, I'll tell you like this (CONFIDING): one year we had here, (1s) it was Baltazar. You heard of Baltazar?*

V: *Yeah.*

J: *Aannnd they didn't play the accordion. And one of them said right away, that they would the next day, that pots and pans would fly ...*

V: *Yeah, is that the same guy? #*

J: (ANGRILY) *And you see right away, that something isn't, no.# (1s)*

V: *That's the same guy who sa.. (said, VH).*

J: *No, that's another. Lenart. (2s) You don't know 'im.*

V: *How?*

J: *Lenart. (3s)*

M: *Hey, what's this the old guy said?*

J: *No, the one who's in Germany.*

M: *The older folks traveled here (unclear 2s) from Germany an' everywhere.*

J: *Migrant workers.*

M: *The workers come for opasilo, right. And then, if they don't get their old music, beef⁴ ... (laughter)*

J: *The Slovenian tradition. Like Bosnians with their kolo, like that we've got the accordion. (3s)*

V: *All over Slovenia. (1s)*

The first passage tells us that the dance had to be organized because that is what tradition (authority) demands, even though the youths may not have been interested. In this way the youths submitted to the wishes of the older authority. From the second passage we can see that it is the same with the preparations for the dance itself: the organizers respect the musical preferences of the migrant workers, who want accordions when they come home for the holidays. But it is precisely the “*accordion*” that creates problems with the selection of the band: the youths would prefer a band that plays popular music, but nevertheless consider the wishes of their elders (authority), which are expressed in the form of threats: “*Annd they didn't play the accordion. And one of them said right away, that they would the next day, that pots and pans would fly ...*” The position of the migrant workers is unusual: despite not living in their home village, despite living outside, they are inside. With their musical preferences they maintain tradition and in relation to the youths signify authority. They are insiders who actively influence the choice of the band and thus interfere with the preparations for the dance.

The youths thus have an ambivalent relation to the authority of their elders: on the one hand they submit to their demands, while on the other they critically assess their behavior. What can be the consequences of such an ambivalent relation of the youths

⁴ *Beef* – in the original: *hoveje* or rather *goveje* literally means beef or cattle music and expresses a negative attitude to Slovenian country folk music.

to authority and tradition? What happens if the idolization of authority disappears? In this case we see a crisis of existing routines and not least a break with a certain form or mode of functioning. This crisis manifested itself that year in Hrastovo with the problematization of the organization of the dance. The elders, who possess the competence to convey tradition to the new generation, implicitly put their position of authority into question with their behavior at the dance and threats before the dance.

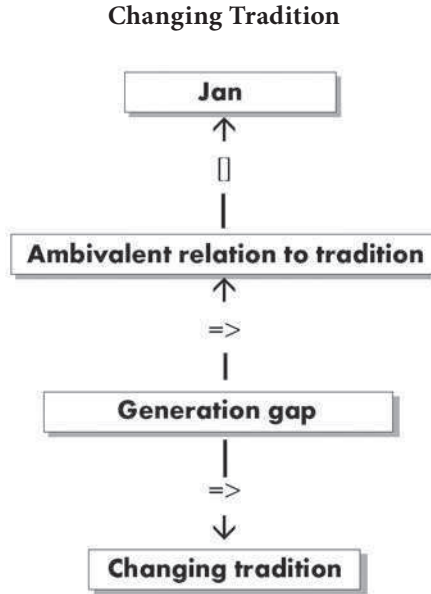


Figure 3: The community of village youths to which Jan belongs has an ambivalent relation to tradition and does not accept it in its entirety. Doubts appear when the new generation can no longer adapt to the contents defined by tradition, when the tradition is no longer useful as a framework and tool for functioning in the world. The ambivalence of the youths is therefore a result of the generation gap. In the future the tradition will change because of “today’s” ambivalence towards it among the youths. The changing tradition demonstrates its re-productive character: tradition is a process of constant searching for and determining of meanings.

INFORMANT – RESEARCHER

A rereading of the citation shows what my commentary to Blaž’s disqualification was: “Well, OK. Blaž is different.” Even though the researcher should stay value neutral, this seems impossible, as in that case she or he would not be a participant. Although a non-member of the group and unacquainted with the subject, the researcher is present during the conversation (research) and thus a part of the community on a certain level. The negative relation of the group to Blaž is unpleasant to me, but I cannot say this because I need Jan’s favorable disposition as he is my informant. Pierre Bourdieu finds:

Tact is nothing other than the capacity of a speaker to assess market conditions accurately and to produce linguistic expressions which are appropriate to them, that is, expressions which are suitably euphemized. [1994: 20]

The censorship of language thus derives from the circumstances of the social field.

The passage which deals with the relation between insider and outsider also problematizes the relation between informant and researcher. Although Robert S. Weiss stresses that informant and researcher are partners in “developing research information,” and that their relation is “determined as one of equals, even though they have different responsibilities” [Weiss 1995: 134–135], the interaction situation suggests a different picture: it is determined by the asymmetric relation between the subordinate and the authority, at the same time it is determined by the meeting of two life worlds that do not postulate any value by themselves.

The researcher enters the interaction first as a student. This form of entry is sufficiently democratic and relaxes the informant’s perception of the researcher as an authority; at the same time this is the position that allows a researcher to come to know and understand the reality of the other and his life world. If we allow the informant to teach us we will discover not only what we imagined, but also that which we did not notice. The strategy of uttering functions as an additional message, an additional text.

Jan is aware that I come from a different life world and explicitly accepts the role of teacher that I ascribed to him and he teaches me: “*It’s called: first dance.*” And “*We call this: korta.*” Jan takes the same position at the beginning of the conversation, when he explains to me that in the village they do not call the festival *opasilo* but *sveta Marvelka* and adds that “*the opasilo version came from Možnica.*”

What does the recording of a conversation, the use of a tape recorder, mean in this relation? Even though I was interested in the organization of the dance in my research, I was not interested in analyzing the event itself, rather I analyzed primarily how the informant perceives and reacts to it, which made recording the conversation inevitable. However, a running tape recorder reminds people that their words are being recorded. “Even when people have ceased to pay attention to it, its presence restrains them” [Weiss 1995: 53]. Jan’s position on the recording was clear, if never explicitly stated: “*Good thing they’re flat* (the batteries – VH),” and “*Hey, stop, she’s recording!*” or “*Say, this isn’t recording?*” The use of a tape recorder can deter people from being honest, as the recorded material could discredit them. We must ask ourselves: Can a researcher use the position of authority she or he has in the informant’s eyes and “in the name of science” record the conversation even though s/he is aware that the narrator is silently opposed?

The relation between informant and researcher is at the same time the interaction of life worlds. “*It’s strange. Strange that we’re talkin’, as we don’t know each other,*” ponders Jan. The sentence reveals the unease that comes from the interaction between strangers.

Steven Webster [1982] explains that there are always reciprocal pretenses between anthropologist and host which reflect their situational agreement that they are welcome in one another’s culture. However, if we suppose that the anthropologist’s research stems from a desire to get to know the different, then she or he feels strange and foreign in the field, which is not home. The informant probably feels equally foreign or strange when she or he enters into interaction with the researcher. Models of the

domestic world, ordinary typification schemes, are therefore useless when entering into an interaction and new ones have to develop [Berger and Luckmann 1988]. The language itself is thus modified by censorship, which stems from the structure of the social field [Bourdieu 1994]. When Jan tried to speak without his dialect and translated his dialect into colloquial Slovenian, and when I tried to follow his dialect, we can see the common desire to bring our realities closer through our language. In this sense the use or word play on the words “Hrastovc” and “Hrastovčan” (someone from the village of Hrastovo) is telling. Jan corrects himself at one point at the beginning of the interview: “*This ‘ere is (1s) also a (1s) Hrastouc, Hrastoučan ...*” The correction indicates self-censorship. It seems that Jan is aware of the tape recorder and that he is talking with a person from a different speaking environment, who might not understand him if he does not speak proper Slovenian. Perhaps he feels that “Hrastovčan” is the proper Slovenian word. Later I remind him of what he was talking about with his own words: “*A Hrastoučan and four companions,*” certain that I am using his dialect. I do the same at another point: “*That’s the guy, the Hrastoučan?*” Both Jan and I wish to get nearer one another: Jan is certain that he is speaking proper (literary) Slovenian, while I am certain, that I am using his dialect. Then Jan says “*... Hrastouceu an’ the...*” This finally leads me to consider whether I am using the word incorrectly and I ask him: “*It’s Hrastouc, not Hrastoučan?*” and Jan replies: “*Hrastouc.*” The situation is funny, but clearly illustrates the common desire to bring our realities closer together through language.

Relation Between Informant and Researcher

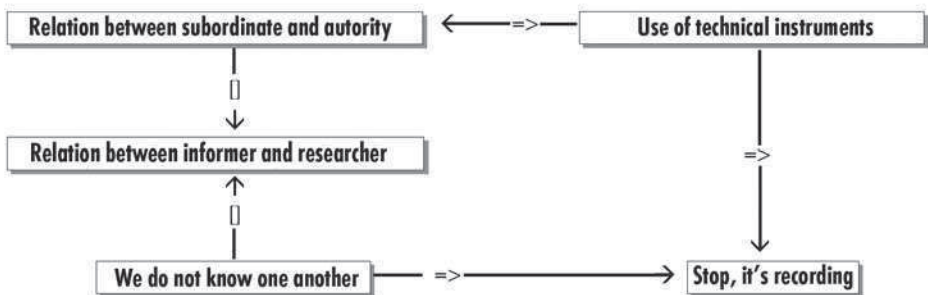


Figure 4: The meeting of two (strange) worlds (we do not know one another) is part of the dialectical relation between informant and researcher, between subordinate and authority; it is manifested by the use of technical instruments. Because the informant knows that the record will be permanent, he controls his speech: stop, it's recording.

CONCLUSION

Qualitative analysis includes the phenomenological and hermeneutic traditions. The phenomenologist discovers how an individual explains his or her empirical world from the way he or she speaks about it. The researcher’s relation to the informant is respect-

ful, non-aggressive, and allows the informant to (re)present him or herself. Research in the field, which is the area of interaction, takes place through a dialogue between otherwise different cultures. The involvement of the researcher is something that not only cannot be avoided but which it also makes no sense to avoid. Research in an environment different from the everyday world of the researcher sets up a boundary, constitutive of hermeneutic interpretation: thus the temporal distance (the time a text is created and the time it is interpreted) and cultural difference weld together the continuity of habit and tradition. On the one hand, I can understand the other at least because we share the common experience of humanity, on the other hand the perspectives of the other and myself (object and subject) interweave and join in the dialogue, creating new perspectives. The ethnological narrative is therefore an interpretation that connects the world of the informant and that of the researcher for the reader.

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COMPUTER-AIDED QUALITATIVE DATA ANALYSIS: AN OVERVIEW

SUSANNE FRIESE

INTRODUCTION

The aim of this chapter is to provide an introduction to computer-aided qualitative data analysis (CAQDA) software. Unlike statistical analysis packages, CAQDA software does not analyze data. Moreover, CAQDA software is a tool that supports the process of qualitative data analysis. The advantage over manual techniques is that the use of software frees the user from those tasks that a machine can do much more effectively, such as searching for words, or retrieving coded data segments. When using software, it becomes much easier to analyze data systematically and to ask questions that otherwise would not be asked because the manual tasks involved would be too time consuming. Even large volumes of data can be structured very quickly and clearly presented with the aid of software.

To set the stage, first of all a general model of the computer-aided qualitative data analysis process is presented. According to John Seidel [1998], independent of any particular methodology, the three basic steps of the qualitative data analysis process are: noticing things, collecting things and thinking about things. In explaining these three steps, special emphasis is placed on how they translate into computer functions. With this as background information, we look at criteria that can be used when selecting a particular software package. These criteria include software functions such as data import, export and output, ways of coding and presenting coded data, search and retrieval functions, means of visualizing the analysis process, and team support functions on the one hand and personal computer knowledge, available hardware, and project requirements on the other hand.

The second half of the chapter is devoted to providing an overview and evaluation of four popular programs belonging to the CAQDA software genre (in alphabetical order): ATLAS.ti 5, The Ethnograph v 5.07, MAXqda, QSR Nud*ist 6.

The computer-aided qualitative data analysis process

When one wishes to use a software package to analyze qualitative data, it is essential to work with a method that is based on a code-and-retrieve approach. The computer is very good at finding things like a string of characters or coded data segments in a large variety of combinations. However, it is still the researcher who needs to tell the computer what each data segment means by means of coding. A clear distinction needs to be made between quantitative content analysis programs such as Sonar Pro-

fessional, FolioVIEWS, etc. and the CAQDA software packages here described. The focus of CAQDA software is on thick descriptions, *verstehen* and context rich analysis, and not on the quantification of qualitative data. Therefore, the first questions that need to be answered before deciding on a software package is what kind of method should be applied to *collect and analyze* data. A well-known method is the grounded theory approach originally developed by Glaser und Strauss [1967]. Other approaches include biographical life history research, case studies, phenomenology, and ethnography. Within each of these methods, various methods of data collection and data analysis are described [for an overview see Creswell 1998]. Independent of any particular method, Seidel [1998] suggests that qualitative data analysis is a breaking up, separating, or disassembling of research materials into pieces, parts, elements, or units. He calls this the process of *noticing and collecting*. With facts broken down into manageable pieces, thinking is facilitated to a great extent. The researcher can now sort and sift the pieces, searching for types, classes, sequences, processes, patterns, or wholes. The aim of this process is to assemble or reconstruct the data in a meaningful or comprehensible fashion. Thus, qualitative data analysis consists of essentially three parts: noticing things, collecting instances of these things, and thinking about these things.

When reading your data, you start noticing things. You are likely to mark those things in the margins and add a few notes. Over time, you find other instances of the same noteworthy things in your data and you start collecting those things under the same name. You begin to create code words. Over time, you build up a coding system. The next logical step is to think about those things you have noticed and collected. For this to be possible you need to find and retrieve these instances in your data. In this process the underlying structure of your data will become more and more obvious. You will be able to see sequences, patterns, hierarchies, and wholes that had been hidden before in the mass of your data. While you are thinking about your data, you might want to go back to the original un-fragmented text, you might want to re-code some passages, add new code words or discard some old ones.

TRANSLATING THIS PROCESS TO A COMPUTER-AIDED ANALYSIS

Noticing things = Creating a project and reading your data files

If you use a software package for analyzing your qualitative data, then the preparation of one or more data files will be your first task after having collected the data. Text data can be prepared within any of the commonly used word processors such as MS Word, WordPerfect, AmiPro, etc. Depending on the software package, you need to observe certain formatting rules. Some packages still only allow you to work with text-only data, other packages also accept Word (.doc) or rich text files (.rtf).

After you have prepared your data files, the next step is to create a project and to import your data files into the program. Now you can embark on the task of reading your data files.

Collecting things = Coding data files

When reading through your data, you will soon start noticing multiple instances of certain occurrences and you will want to mark them with a code word. In order to do so, you highlight the data segment in question with the mouse and assign one or

more code words to it. Some software packages restrict the number of codes you can assign to one data segment, others have no limitation. But even if a restriction applies, the restrictions are generous enough so that you probably will not notice that there is one. All packages allow overlapping or nested segments, as it is common when working with qualitative data that their meaning is ambiguous and not always confined to whole sentences or paragraphs. Therefore being able to apply overlapping codes is essential and one of the many advantages of using a software package. Nonetheless, coding the data material and developing a suitable coding system is a laborious task also when analyzing the data with the aid of software. As mentioned above, computers cannot think and therefore this task has to be undertaken with much care. This having been said, software packages are also superior to manual techniques during this part of the analysis. With the aid of software, it is much easier to modify coded data segments, to rename codes globally or locally, or to reverse coding. Notes, including first interpretations or clarifications, can be attached to data segments so that one does not end up with post-it notes or scribbled handwriting all over the place. With the help of the retrieval functions the software packages provide, it is also very easy to find these notes again.

Thinking about things – Searching data files

Once a coding system has been developed and the data have been coded, data segments can be searched, retrieved and displayed in a number of ways. A search operation can be very simple, i.e. based on just a single code word. However, it is also possible to build more complicated search requests that consist of multiple code words linked by Boolean, proximity, or contextual operators. Such requests can also be combined with variables such as gender, age, education or other data attributes.

The retrieval of data segments makes it easier to see things in your data and to think about them. It helps the researcher to recognize relationships that previously went unnoticed because they were disguised by too much noise in the data. Some packages allow the visualization of the established relationships in the form of networks or graphic models.

What to look for when deciding on a package

In the following section a number of software features are listed that will be used in the description of the four software packages in the second half of this chapter. If you are not yet familiar with any CAQDA software, the questions that are posed may not make much sense to you when first reading them. Therefore, it might be necessary to consult this section again after having read the software descriptions. Note down your requirements and wishes and work your way forward again.

DATA ENTRY

With regard to data entry, the following questions are worth investigating: What kind of data do you want to analyze and is this a format supported by the software? Some programs can only handle text documents; others also support the analysis of graphic images/photographs, audio and video data. If you only want to analyze text

documents, it might be worthwhile to investigate the suitability of programs that require documents in text-only format versus those that allow the importation of rich text or Word documents. The learning curve for the latter is often steeper as these programs generally offer more features and are more complex. For some purposes, a simple program might also be sufficient. Another issue to look at is the ability to edit the data material once it is imported into the package and coded.

CODING

What is the smallest chunk of text that can be coded? How are the already coded text segments and code words displayed? Can I use free codes (i.e. codes that are not connected to any text passages)? Is automatic coding possible?

SUPPORTED ANALYTIC APPROACH AND METHOD

As mentioned above, it is very important that you first decide on a method before you decide on a software package. Knowing what you potentially want to do with your data, the following questions may help you to decide which software package best suits your needs: Is the program variable or case oriented, does it lend itself to an inductive or a deductive type of analysis. Does it include content analytic approaches?

MEMOS AND COMMENTS

Analyzing qualitative data entails a great deal of writing. A significant amount of analysis happens while writing notes and memos. Therefore, it is vital to look at the features that the various software packages provide in order to support this process. What kinds of objects, memos, and comments can be written and where? How can you track that you have attached or inserted a memo or comment somewhere? Is it made visible and how? Further analytic support is provided when you can link memos and comments to other objects. Is this possible?

DATA RETRIEVAL

The search functions and features are essential as it is here where the power of the computer can be fully utilized. Investigate what can be searched and how search results are displayed. Can you search for words, strings of text or text patterns? Can the finds be coded automatically? The purpose of the text search and auto-coding is to allow you to “dive” into your data and to give you a quick feel for what is there and what to look for. Another application is to code structured information such as the responses to entire questions, or responses in focus group transcripts.

Searching for coded text segments is a significant part of the analysis after having coded the data. Therefore, you need to check the ways this is facilitated by the software package. How conveniently can coded data passages be accessed? Can they be displayed in context? What kinds of search operators are available? The search operators allow you to ask questions like: Show me all the data passages where Code_A and Code_B overlap / where B follows A / all passages coded with child codes (lower order) of A,

etc. Another important search function is the ability to restrict searches to particular documents or codes, or according to other criteria. This allows you to ask questions like: Show me all the data passages where Code_A and Code_B overlap, but only in interviews with female respondents, age 21–30.

OUTPUT AND DISPLAY OPTIONS

You may not necessarily always want to work in front of the computer. You may miss going through stacks of real paper, and at some point in time, you may want to be able to transfer your analytic work to a word processing program to write the final report. For all of this, you need output functions that facilitate the transfer from the CAQDA software to a printer or another software package. Therefore, you should pay attention to the output destinations offered by the software. Can output be displayed in an editor, saved to a disk, and printed on paper? Can you select only parts of a result for reviewing, editing or saving? Is there an easy way to access the context of search results? Is it easy to identify where retrieved data passages come from, i.e. how specific is the source tag?

In addition, a number of packages provide export functions to interface with other programs such as SPSS or mapping software. Depending on your chosen method and analysis aims, these might also be features to look out for.

OTHER FEATURES

Some users prefer to organize their coding system in a hierarchical manner. Most programs allow for this, but all in different ways. Thus, if you are interested in a hierarchical representation, pay attention to how this is realized and what best suits your needs and working style.

Teamwork is also supported in different ways by different packages. When working in teams, it is important that a program offers a merge function to combine work done by various team members. Another issue to pay attention to is whether it is possible to distinguish work done by different team members.

FURTHER DECISION CRITERIA

There are simpler, and thus easier to learn packages, and more complex programs with a steeper learning curve that might best be learned by attending a professional training session. Therefore, you should ask yourself what kind of computer user you are. If you think of yourself as a novice or not very experienced user, and insecure regarding issues related to data management, a simpler program might be a better choice for you. This might also be the case if you only need some basic functions and do not need the advanced options provided by the more sophisticated packages.

A further decision criterion should be whether the software is only to be used for a single project, or also in the future for other projects and possibly by other users. If only chosen for a specific project, then a program can be chosen that fits the specific requirements of that project. If one is purchasing for the long term, it might be better to choose a package that offers additional features even if they may not be required at present.

LAST BUT NOT LEAST ...

Rely on your intuition when deciding on a program. All companies offer a demo version and tutorial that can be downloaded from their respective websites [see “Software Overview” on pages 220–229]. Look at the demo versions and go step-by-step through the provided tutorials and sample project(s). The time invested will be well worth it. A program may provide all the features that you need, but you may feel uncomfortable with the user interface and the ways the data is handled. This is something you can only determine by gaining some hands-on experience yourself. Reading this chapter or the program descriptions on the company websites can never substitute for personal experience.

One last remark before I move on to describe four of the packages – do not expect to learn qualitative methodology just because you are using CAQDA software. You should have at least some basic knowledge of qualitative data analysis techniques before you embark on a computer-aided analysis. The purpose of CAQDAS is not to manipulate or direct your analysis; its purpose simply is to support it. The user should be the one manipulating the software package to make it perform those tasks that best suit your method of analysis.

SOFTWARE DESCRIPTION

The second part of this chapter is devoted to describing four software packages in detail (in alphabetical order: ATLAS.ti 5, MAXqda & MAXDictio, The Ethnograph 5.08, and QSR Nud*ist 6). The detailed description is followed by an overview table including two additional packages (HyperResearch and QSR NVivo 2) to complete and round off the review. HyperResearch like ATLAS.ti is a program that offers functions to analyze pictures, sound and video data, in addition to text documents. NVivo is the younger sister of N6, and offers more advanced features such as the import of rich rather than just plain text.

The following functions are considered in both the detailed description and the overview: Data Entry, Coding, Text Search and Auto Coding, Memos, Data Retrieval, Mapping and Team Support. The detailed descriptions also include some supplementary features that only apply to specific programs. The overview table, in addition to the main headings provides information on system requirements, pricing, user support, and support of quantitative analysis approaches.

ATLAS.ti 5

DATA ENTRY

Four document media types are supported: text, graphic, audio and video data. Text documents can be imported as text-only documents, as rich text files, or as Word documents. If documents are not yet converted to rich text format, ATLAS.ti 5 provides automatic conversion of different file formats to RTF. As documents are assigned to a project and not imported, they do not become part of the project file and thus file size does not play a role. ATLAS.ti can handle large data sets and large video files also do

not create a problem. The overview table at the end of this chapter lists the multimedia file formats that are accepted by ATLAS.ti.

Documents can be grouped into so-called families. This is a way to assign variables by creating for example a female family, a male family, families containing interviews from respondents between the ages of 21 to 30, between the ages 31 to 40, and so on. These can later be used to restrict or filter search results.

Documents that have been assigned to ATLAS.ti can be edited at all stages of the project.

CODING

Coding is an interactive process and the results are immediately visible on the screen. Code words are displayed in the margins and the length of the coded data passage (=quote) is marked by a bracket. The smallest text unit that can be coded is one character; the smallest unit in audio files is a millisecond, in video files a frame. Graphical quotes are rectangular sections of the original image.

There are no restrictions with regard to the number of levels that can be coded nor with regard to the length of a code word.. All code words in ATLAS.ti are interactive. If you click on a code word once, the text passage coded with this code word is highlighted. If you double-click on a code word, the code word's comment is displayed. The modification of already coded data segments is quick and easy.

Many drag and drop operations are available to facilitate coding, the modification of coded data passages, and the unlinking and exchanging of code words. For novices, an option is provided to deactivate the many drag and drop operations to make it easier to first concentrate on the basics.

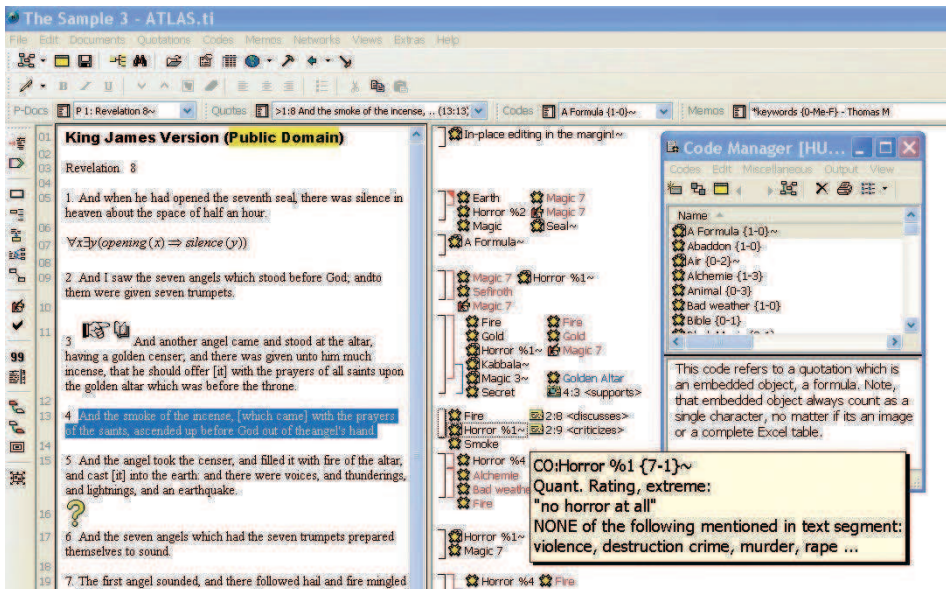


Fig. 1: The ATLAS.ti 5 user interface with open Code Manager and activated code and code definition.

A code word list is automatically created and updated while you code your data. Behind each code word, two numbers are displayed – the first number tells you how often a code word is used, indicating the frequency or groundedness of a code word; the second number informs one of the links to the code word that exist; thus indicating the density of a code word. A code manager offers additional functions to manage your code word list conveniently. Codes can be grouped in so-called code families and data segments can be retrieved based on their family membership. Furthermore, codes can be ordered hierarchically with the aid of the network tools (see “Mapping” below).

Figure 1 shows the ATLAS.ti 5 user interface. A coded document is loaded. In addition to codes, hyperlinks and memos are also shown in the margin. This can be recognized by the different symbols used. The code ‘Horror %1’ is highlighted in context and the code definition has been activated via a double click. The Code Manager is open as is commonly the case when coding a document.

TEXT SEARCH AND AUTO CODING

All primary documents can be searched using a procedure that is similar to what might be known from word processing programs. To find instances that are anchored at the beginning/end of a line or sentence, certain number patterns, etc., GREP expressions can be used. Search patterns that are potentially used more than once can be saved.

The text search function only searches through primary data, but not through other texts such as comments or memos. In order to search through all texts, the so-called Object Crawler can be used. It finds all first instances of an entered search term in all specified object types.

Text searches can be combined with auto coding. This means that you can search for a string of characters in the text and code the hits, the surrounding word, sentence or paragraph automatically with a selected code word. This process can run fully automatically, or you can control it by deciding each time a match is found whether or not you want to code the instance.

MEMOS AND COMMENTS

For virtually every object in ATLAS.ti you can write a memo or a comment. Comments are directly attached to the object they relate to and cannot be handled independently. A comment can be written for the entire project, each primary document, quote, and code word. Commented objects are indicated by a tilde sign (~) in the respective object lists.

Memos are independent objects and can be attached to quotes, codes and other memos. When attached to quotes, this is indicated in the margin next to the coded segment (see Figure 1). When linked to codes or other memos, this can be made visible in the network editor (see below). As a way to facilitate memo management, memo types can be defined and used for sorting and filtering purposes.

Both memos and comments can be formatted as rich text just like primary documents, including embedded objects.

DATA RETRIEVAL

ATLAS.ti offers a wide variety of options to search for coded data segments, a total of 14 operators including Boolean, proximity, and semantic operators. Hits lists can be cleaned up or filtered according to specified criteria such as only those quotes from interviews with female respondents. All finds can be displayed in context or reviewed in an editor, saved to disk or printed. The output contains clear source tags, but unfortunately these cannot easily be removed to display a generic view if desired.

Each query can be saved in the form of a super code. A super code contains the query and each time you click on a super code, the query is executed again. This is a useful feature when using more complex queries that you want to re-run or re-use at a later time in your analysis. Super codes can also be turned into regular codes, presenting a snapshot of the current stage of the analysis.

MAPPING

A distinct feature of ATLAS.ti is the way the various objects of an ATLAS.ti project can be visualized and linked to each other in so called network views. Unlike in other programs that offer a mapping or modeling feature, connections between codes and connections between quotes can be named. The program offers seven relation types, but these can be edited or new ones can be defined. Objects can be linked visually in a network editor, and also via menu functions.

The place of objects in a network editor is not static, in other words, all objects can be moved around freely without having to obey a default structure. From a network view you can access your primary data directly and display them in context. Figure 2 shows an ATLAS.ti network view in auto-color mode. Auto-coloring indicates the groundedness and density of a code. The more frequently a code has been applied, the redder it is colored; the higher the number of connections to other codes, the bluer it is colored. Frequency and density are also displayed as numbers behind the code words (the display of nodes and links, and the auto-color mode are optional features). Also shown in figure 2 are a linked memo, the context menu for a code node, and the type of relation between two connected codes (i.e., *is part of*, *contradicts*). A comment has been written for the *contradicts* relation as indicated by the tilde sign (~).

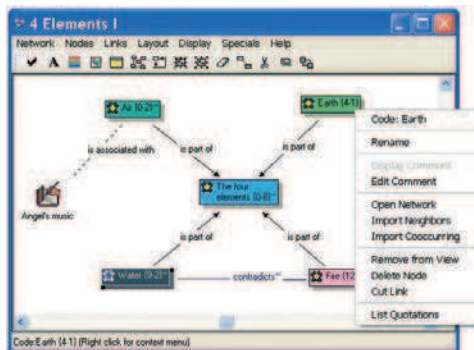


Fig. 2: An ATLAS.ti network view.

Network views can be saved as graphic files and edited in a graphic program. They can also be copied to the clipboard and directly inserted into Word files or PowerPoint presentations.

You can create a hierarchical view of your coding system by linking codes either in network views, by using the link menu option or by dragging and dropping in the code manager. The hierarchy of codes can then be displayed in the form of a sideways tree similar to the display of folders and files in Windows Explorer. Codes can however not be manipulated in the tree view, only via the above mentioned functions.

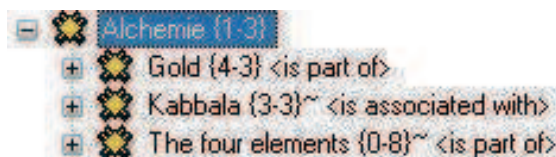


Fig. 3: Hierarchical display of codes in ATLAS.ti.

TEAM SUPPORT

ATLAS.ti supports team work. All users can be given a user name and if logged in, all new entries are 'stamped' with the name of the logged in user. Thus, all work done on a project such as creating code words, memos, etc., carries the name of the current user (=author). This way it is possible to trace the analytic steps of the various team members. Documents can be analyzed simultaneously when working on a shared network drive, given that each user works on his/her own project file. When documents are edited, the modifications are transmitted to all users of the same document and the document in question is synchronized. All affected coded data segments will be adjusted. When splitting a project into sub-parts, these can be put together using the ATLAS.ti merge function. The ATLAS.ti merge function offers a variety of strategies to either add or merge the various object types such as documents, quotes, codes, memos, networks or families.

OTHER FEATURES

Hypertext

ATLAS.ti features the ability to link quotes within and across documents, in such a manner creating a hypertext structure. The original sequential document is de-linearised, broken down into pieces, which are then reconnected, making it possible to traverse from one piece of data to another piece of data regardless of their original positions. This is a unique feature of ATLAS.ti.

Additional Output and Export Options

Projects can be saved as HTML documents and published on a website. In addition, an XML export option is available. This offers the possibility of representing data in a form that does not restrict their usability to the context and output options pro-

vided by ATLAS.ti. With the aid of style sheets, users can then create their own reports, beyond the options ATLAS.ti offers.

A further option is the export of the entire coding system form of an SPSS syntax file. When run by SPSS, a data matrix is created containing all codes as variables and all coded segments (quotes) as cases.

Word Frequencies

The ATLAS.ti Word Cruncher creates a list of words for the selected or all primary documents. This feature is useful for a simple quantitative content analysis. A stop list can be used to exclude certain words, characters or patterns from the frequency count. Results can directly be opened in Excel.

SUMMARY

ATLAS.ti 5 is one of the more complex CAQDA programs offering a large number of tools and options for analyzing qualitative data. In order to learn the software, you need to set aside some time, or if possible, attend a training course. If you do, however, invest the time and effort, you will be able to use a product that incorporates up-to date Windows technology and offers powerful data analysis tools. The highlights include the fact that you can assign Word or rich text documents, image, sound and video files to an ATLAS.ti project. The interactive display and handling of codes and coded text passages, as well as the hypertext and network functions are unique in ATLAS.ti. You may not utilize all functions the first project you use it on when still learning the program, and not all tools can always be exploited in all projects, but with this package you certainly gain the advantages of software that is extensible for future use.

MAXqda

DATA ENTRY

With MAXqda you can only analyze text documents in rich text format. Documents can be assigned to certain text groups such as articles, documents, interviews, etc., and organized into sets such as interviews with female or males, documents from 1950 to 1960, etc. Text groups and sets serve in later stages of the analysis as filter devices to retrieve coded segments that match certain specified criteria such as only responses from female interviewees working as teachers in primary schools.

Documents that have been imported into MAXqda can be edited at all stages of a project. Certain precautions have to be taken into account when working in teams.

CODING

The smallest unit of text that can be coded is one character. Text is selected by highlighting the appropriate passage with the mouse. All code words are represented in the form of a sideways tree hierarchy similar to the type of tree structure in Windows Explorer.

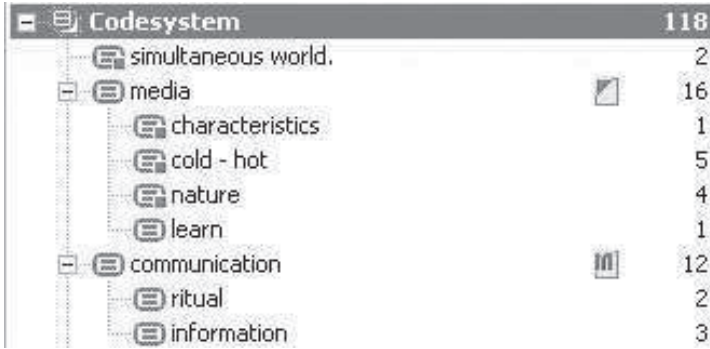


Fig. 4: Code system in MAXqda.

The structure of the tree can be manipulated by dragging and dropping codes to other places in the tree. Symbols next to the code words indicate whether a memo has been written for a code. Code frequencies are listed at the right of the code system window.

There is no limit with regard to the number of code words you can use or with regard to the number of nests and overlaps that result from the coding process. The code word tree can be up to ten levels deep. Modifications of the coding system is possible by copying or moving all coded text at a code word to another code word. The code words of individual coded text passages can not be modified. If this should become necessary, you first need to delete them and assign them again.

A coded text looks as follows in MAXqda:

Fig. 5: Coded text and activated memo in MAXqda.

In the left margin of the displayed text, MAXqda offers three types of information: In the first column of the left margin, you see the codes displayed against the text. The coded segments are referenced by a straight line with a little square box in the middle. When moving the cursor to this box, the code word for this segment is displayed. After attaching a memo to a text passage, a yellow post-it symbol is shown in the second column. When moving with the cursor over this symbol, the memo text is displayed. The third column lists paragraph numbers.

MEMOS

Memos can be attached to documents and codes. Each memo is date and time stamped. Optionally, an author and code words can be associated. This information can be used to filter, search and sort the memos. A further option is to define memo types by selecting different memo icons. Memos are in rich text format.

TEXT SEARCH AND AUTO CODING

MAXqda offers you a number of options for searching the text. You can search in all or only selected documents or document passages, in retrieved text passages or in memos. Search strings entered can be combined using the logical operators AND and OR. The results are displayed in a table, which can be sorted by headers. When clicking on a result in the table, the match is highlighted in context. Search results can also be saved and exported as RTF files. Rather than just exporting the search string found, additional context can be exported as well.

The results table also offers a function to code the search results with a selected code word. One can either code just the hits or select any number of paragraphs surrounding the search string found.

DATA RETRIEVAL

The retrieval of coded text segments follows a simple logic which is particular to MAXqda. The retrieval logic is based on activating codes and documents. If one wants to retrieve all coded segments of Code_A from interviews 1 and 5, one needs to activate Code_A and interviews 1 and 5. The coded segments are immediately displayed in a separate window. A retrieved segment is clearly indicated by a source tag and when selected is displayed in context. The results of a search can be saved under a new code word.

It is also possible to conduct some more complex retrievals using Boolean and proximity operators. A total of ten logic operators such as OR, intersection, if inside, if outside, followed by, etc., are available. In addition, you can use variables as filters in searches. For instance, you could ask for all occurrences of Code_A where it overlaps with Code_B in all the responses of men, age category 5, living in Munich. Variables can be managed in tables and exported or imported as text (tab-delimited) files.

TEAM SUPPORT

MAXqda offers team support in two ways. Team members can either work with the same master version of a project (option A), or the different members of the team can divide their work (option B). When choosing option A, team members can never work simultaneously on a project; only one person at a time can code a text and then the entire project has to be given to the other team members. When choosing option B, team members can work simultaneously on a project, but only on different parts. When a team member has coded a particular text or part of a text, this text can be transmitted to other team members. The same applies to memos.

Another option is to merge two projects. New texts and all memos will be inserted, codes and variables will be merged.

OTHER FEATURES

Scoring Relevance

Each coded segment can be given a weight between 0 and 100. This can be used as an indicator of how relevant or how typical a code is for a certain category. Depending on the kind of analysis, the weights can be treated as fuzzy variables. When creating a table for all codings or for the codings at a particular code, the relevance scores (weights) are included.

Quantitative Content Analysis Functions

If you are interested in quantitative content analysis, MAXqda offers the add-on module MAXdictio. This module allows you to analyze vocabulary and to create dictionaries. You can examine the vocabulary used in the text to find out which words can be found in which text passages or the full text. Among the functions offered are, for example, the creation of word frequency lists for the whole text, marked passages, in text groups or in text sets, the creation of an index of selected words from one or more texts, the building-up of word-based dictionaries, the export and import of dictionaries from MS-Office programs such as Excel, or the further processing of the results in SPSS and Excel. All content analytic functions can be integrated into the functionality of MAXqda. Thus, if you are interested in a combination of quantitative and qualitative content analysis, the combination of MAXqda with the add-on module might be the right choice for you.

SUMMARY

MAXqda is a convincing program due to its simple logic and structure. It is likely to appeal to users who prefer to think of their coding system as a hierarchical construct consisting of higher and lower order codes. Conceptual level relationships cannot be mapped within the software. Nonetheless, the software provides all features necessary to perform a qualitative data analysis and by keeping it simple, the learning curve is not as steep as for other software packages.

The add-on module MAXdictio gives the software a unique profile allowing for the combination of qualitative and quantitative content analysis methods.

The Ethnograph v5.08

DATA ENTRY

THE ETHNOGRAPH supports the analysis of text data. Before importing the data, it is necessary to format them in a particular way. An Ethnograph data file has a 40 character line and hanging paragraph indents. All of your data must be saved as ASCII text. The program provides an editor that facilitates the creation and re-formatting of data files into the format required by The Ethnograph. The special formatting rules allow the

program to identify speaker or section turns. Thus, there is no need to code speaker or other sections and variables can be directly attached to them.

CODING

It is possible to code your data interactively. This means that you can view and read the text of your data files on screen while you are coding. For each coded segment the boundaries are marked in the margin and the code word appears one line above the coded text segment. Code words can be conveniently selected from either the Code Book or the Tree View. Both are automatically created and updated when coding.

The smallest size of a text segment you can code is one line; the maximum length of code words is 10 characters. This is a necessary restriction, otherwise it would be impossible to display all of your code words within the text (see fFigure 6 below). Longer definitions of up to 500 characters (roughly half a page) can be written into the code book. Each coded segment can be defined by up to 12 code words and these code words can be nested or overlapped up to 7 levels deep.

Within the Coding Window simple text search and code search functions are available.

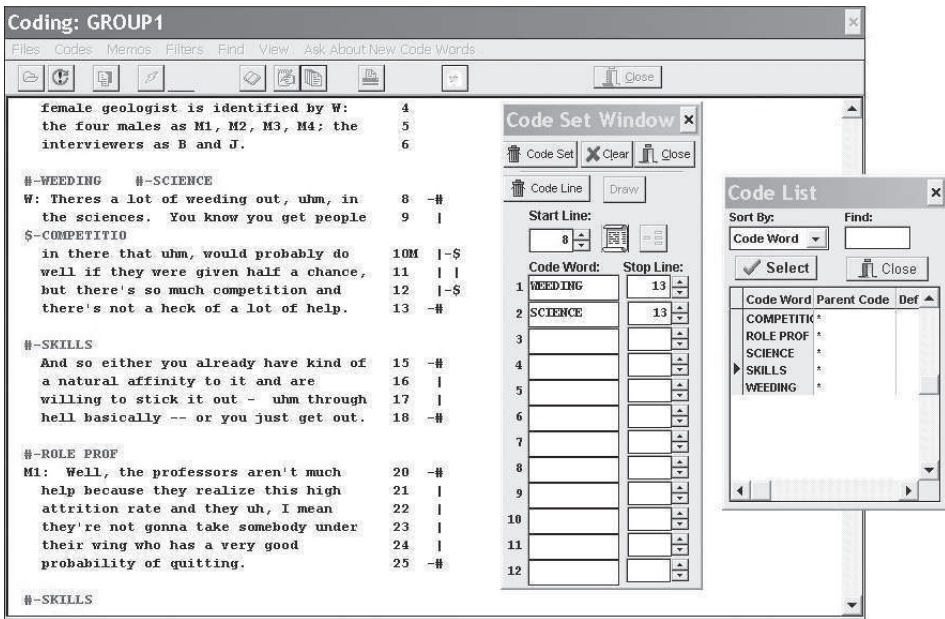


Fig. 6: Coding window in The Ethnograph.

The coding system can be structured hierarchically by assigning parent and child codes in the code book, or by dragging and dropping codes in the Tree View. Parent codes are higher level and child codes lower level codes. A code can be both a parent code of a subgroup of codes and a child code of a super-ordinate code.

A hierarchical presentation of the coding system is possible in the tree view, which displays the codes in the form of a side-ways tree. You can either view just the tree or

both the code book and the tree side by side (see Figure 7). By collapsing or opening the various branches of the Tree View, it is possible to focus one's view on just a particular area of the coding system.

MEMOS

Three types of memos are available: Text Memos, Project Memos and File Memos. You can attach up to 26 memos to each line in a data file, up to 1000 memos to a project and up to 1000 memos to each data file. Each memo can be up to 32 pages in length and is date and time stamped. In addition, the initials of the author can be entered, memo categories can be defined and up to three code words can be associated with a code word. The memo categories, code words, line numbers, date and time of creation/modification, the author's name and the memo title can all be used as sorting devices.

All memos can be accessed from a variety of places in the project. Text memos for example are identified by the letter M, which is displayed next to the first line number of the segment the memo is attached to (see Figure 6). A double click on the M opens the memo window.

DATA RETRIEVAL

THE ETHNOGRAPH search tool nicely integrates the various search options and filters that can be used in a search. You can search for segments coded by a single code word, or for segments coded by multiple code word. In the latter case, Boolean and proximity operators can be used to combine a string of up to five code words. Multiple code word searches can be saved and called up again, reused or edited for later searches.

Figure 7 shows the selection window for codes in a single code search. Codes can either be selected from the code book or the tree view. The parent/child structure of the coding system allows you to use the equivalent of a semantic DOWN operator (i.e., SCIENCE with kids).

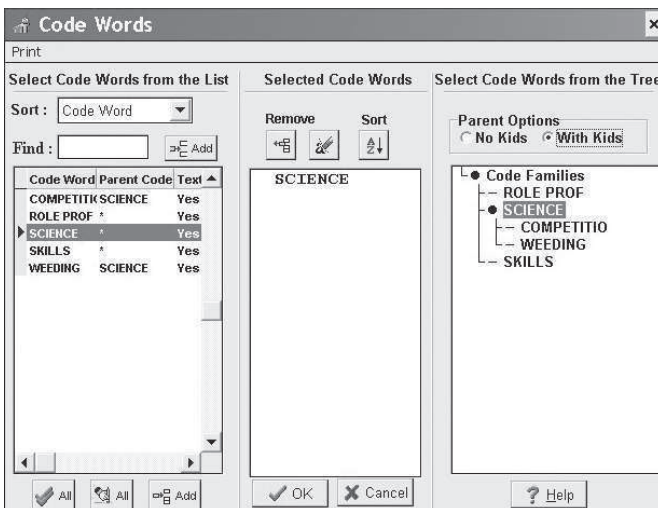


Fig. 7: Single code search showing the code book and the tree view.

In addition to codes, speaker or section identifiers can be used in a search; variables and file codes can be used to restrict searches to certain documents or attributes.

An identifier search retrieves all instances of a certain speaker or of text passages marked with a specific tag. Identifiers are defined in the process of transcribing the data (see data entry). A common use of identifiers is to mark speaker turns, e.g. the name of the interviewee or the names of participants in a focus group. Those identifiers are referred to as Speaker Identifiers. If your data consists of documents such as letters or newspaper articles, you can identify them by date and source. These types of identifiers are called Section Identifiers. When employed as part of a search, both Speaker and Section Identifiers can be used as if they were code words.

For each identifier, variables such as gender, age, occupation, and so on can be entered. In a search, this information can be utilized by retrieving text passages coded by single or multiple code words in combination with selected variables. Thus, you can for instance ask for all comments about a particular topic that can be attributed to only male interviewees between the ages of 25 and 30 with an income of at least \$50,000 per year. Variables in *THE ETHNOGRAPH* allow fine grained searches in that you can choose not only categories but also enter continuous numeric values.

File Codes also serve as variables in that they allow the attachment of specific attributes to an entire document. File codes like variables can be combined by AND, OR and NOT operators. The difference between file codes and variables attached to identifiers is that you can assign up to 16,000 file codes to a data file but only 40 variables to an identifier. Thus, the program offers more than enough options to restrict and filter searches.

Search results can be reviewed on screen, printed or saved to disk, and displayed in three different ways:

- In the form of coded text segments (with or without the surrounding context, line numbers, boundaries, code words or memos).
- In the form of summary reports. Summary reports give you a count of all hits. An additional option is to display all of the start and stop lines of the coded segments that have been found.
- As frequency counts. Frequency outputs list the percentages of all hits within and across data files.

Before saving or printing search results, segments of interest can be marked and the output can be restricted to only the marked segments. Text output is saved in ASCII text format, all quantitative output is saved in the form of tables and can be imported to a statistical package for further analysis.

The entire search results or only selected segments can be saved as a new data file, which is automatically imported into the project and available for further analysis and coding.

OTHER FEATURES

Splitting and Combining Files

It is possible to split data files and to combine data files. When you combine data files, you have the option of combining only selected parts of the data files, e.g. lines

1 to 500 from data file X and lines 350 to 1300 from data file Y. Inserted sub-headers inform you of the original source. Splitting and combining data files can be done on the basis of numbered or coded versions of your data files. This process does not affect the coding. You can save and print numbered, coded or generic versions of your data files.

SUMMARY

THE ETHNOGRAPH is one of the simpler programs for analyzing qualitative data and it is probably the easiest of all programs to learn. The procedures are straightforward and by design, they are strictly separated, so that one does not get lost or confused by multiple overlapping windows. However, experienced Windows users might expect more in terms of user friendliness. The DOS roots of the program are still noticeable. File and folder names that are longer than 8 characters are not accepted or are abbreviated. Data, code, search and memo procedures cannot be run side by side. For instance, one cannot add additional files to a project while the coding or search window is open. One cannot search while the coding or memo window is still open, etc. This may not be experienced as a problem and some may even appreciate the clear separation of functions. THE ETHNOGRAPH certainly offers very good search functions in combination with easy to handle variables. The option to work with section and speaker identifiers is a unique feature of THE ETHNOGRAPH. Other programs offer similar but less elegant solutions. Overall, THE ETHNOGRAPH is a program that offers good value for money; it is the least expensive of all programs discussed here.

QSR Nud*ist 6 (QSR N6)

As many people who first see or hear the name of this product wonder how this name might relate to qualitative data analysis, here is the explanation:

QSR stands for Qualitative Solutions and Research (the name of the company that has developed this piece of software)

NUD*IST stands for Non-numerical, Unstructured Data * Indexing, Structuring, Theorizing. Today the software is mostly referred to as QSR N6; the number six indicates the version number.

DATA ENTRY

QSR N6 can be used to analyze text data. The required format for importing data into N6 is plain text. The smallest segment that can be coded is a text unit, which can be set to lines, sentences or paragraphs. A paragraph is defined as two successive hard returns. This needs to be taken into account when transcribing or formatting the data.

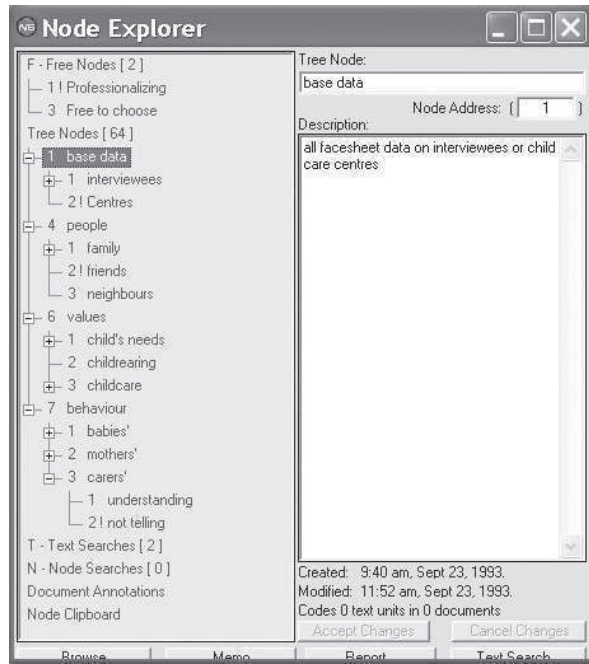
New data can be appended to existing documents and documents already imported can be edited. This includes inserting or removing text units, sub-headers, and annotations.

CODING

Coding can be done interactively while you read the text on screen or, if you prefer, on a hard copy of your data. Coding done on paper can be transferred to the computer by using text unit numbers as reference points. All coded segments are stored in a so-called node. Coded segments are not indicated inside the text or in the margin area. They can only be made visible by specifically requesting a report that includes coding stripes for selected code words (= nodes).

Nodes can be understood as containers that have a node address and node title. There are different types of nodes, those holding coded data segments, results of text or node searchers, document annotations or the content of the clipboard when cutting or copying node contents. Nodes containing coded text passages can either be handled as 'free' nodes, or can be integrated into the index tree. The index tree offers the ability to structure and organise your codes hierarchically. All nodes are displayed in the Node Explorer (see Figure 8 below). A description for each node can be entered into the right window-pane of the node explorer.

Fig. 8: QSR N6 node explorer displaying free nodes and index tree.



The Node Explorer offers a sideways representation of the coding system. Within the Node Explorer you can cut, delete and merge nodes, you can browse the text that is coded at a node, you can change node addresses, write and review memos or modify the coding at a node. In addition, the node explorer is a very handy device when it comes to searching. From there you have direct access to the results of the search operations (see Search Functions). To efficiently build up a hierarchical index system, certain rules should be followed. For an introduction to creating a hierarchical category scheme, see Richards and Richards (1995).

The index tree can also be displayed graphically in the form of an upside down tree. The top of the index tree is the root and from there, all the branches ramify. An overview of the entire tree is provided, however the user can only access two levels at a time (see figure 9). Nodes can be manipulated in either the side-ways or the graphical display.

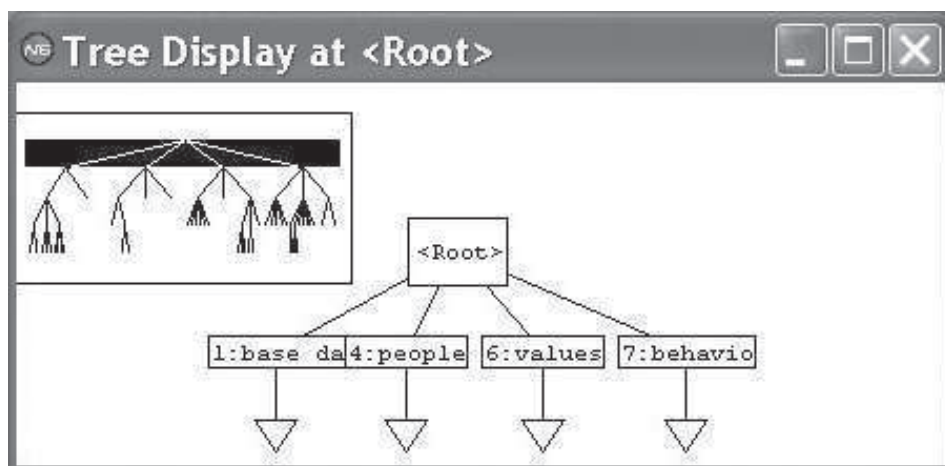


Fig. 9: Tree Display in N6.

It is possible to create the tree before you start coding your data. In fact, it is very common to set up certain parts of the index tree at the start of a project. This part of the tree is generally referred to as the base data tree. It can be used for coding factual information such as gender, age, race, occupation, or time series. Since this is a routine task, this process can be automated by using command files. If the factual information is available in the form of a spread sheet, you can use the Import Table function for generating the base data tree and to code your data. This feature of the program allows you to link quantitative information that you have about your data with the qualitatively rich content. The document base or case data and coding at a particular node can be exported as plain text or tab-separated tables.

MEMOS AND ANNOTATIONS

In addition to writing a description for each document and note, you can attach one memo to each document and node. Memos are indicated with an exclamation mark (!) before the object they belong to and their content in plain text format can be cut and pasted somewhere else. Memos can be retrieved via the context menu or when creating a report.

Annotations are short notes that can be attached as a text-unit to any text-unit in a document. All annotations are coded automatically at the Document Annotation node in the node system. In order to distinguish annotations from the rest of the text in your documents, they are put into <<brackets>>.

TEXT SEARCH AND AUTO CODING

In N6 you can search for words or phrases in either the documents or the coded text segments. The search can be restricted by a number of criteria. In addition, GREP expressions can be used for pattern searches. You can keep all finds, or review the finds

first and decide which ones you want to keep. The resulting hits are automatically coded at a new node in the Text Search area of the node system.

DATA RETRIEVAL

The search engine in N6 is a very powerful tool. It offers 17 different ways of asking questions about your data including Boolean, context, sequence, proximity, matrix, and vector searches. The results of a search are saved to the node clipboard. In addition, a node containing the search results is automatically created and saved in the Node Search area of the node systems. Results of matrix or vector searches can be exported as plain text or tab-separated tables.

Node searches can be restricted to selected documents and nodes. As in N6, the text coded at the base data nodes is a form of defining data attributes or variables, this is a way to combine codes with variables. Thus, being able to ask questions like: Give me all text related to attitude x, but only from female respondents of age group 1.

Having the results of a search saved at a node, you can ask further questions about specific parts of your data. This way it becomes possible to build up more and more layers of analysis and to go deeper and deeper into your data.

For utilizing the search functions in N6 in the most efficient manner, you are advised to build up the index system following the rules of hierarchical category schemes.

TEAM SUPPORT

QSR offers as an add-on to N6 a free companion tool to merge projects. This tool allows you to compare and contrast two different projects or to build a central project from multiple sites. It is also possible to use the merge tool across different platforms (i.e., Macintosh and PC platforms).

OTHER FEATURES

Command Files

Many processes in N6 can be automated by using command files. Command files contain written instructions that can be executed by N6 to carry out different processes, such as importing documents, setting up the base data tree, add and delete coding, copying and pasting parts of the tree, deleting nodes, displaying the tree, searching text and nodes, setting restrictions, or making and printing reports. N6 makes the use of command files fairly simple by providing a number of templates. Thus, command files can be written without knowledge of the underlying syntax.

Model Building – Exporting the Index Tree

N6 does not offer a model building function itself, but the index tree can be exported to either Decision Explorer or Inspiration. This allows you to break away from the hierarchical structure enforced by N6 and to freely play around with the nodes, thus to enter the phase of model building. While you lose direct access to your raw data, you can still view and edit memos.

SUMMARY

QSR N6 is a good choice for those that prefer to organize their coding system in a hierarchically manner, or that have data better suited to such a structure. Even though N6 (as compared to previous versions) features the inclusion of free nodes into the index system, the program would lose much of its power if you choose not to utilize the tree. Comparable to the THE ETHNOGRAPH, N6 has recognizable DOS roots. File and folder names can be no longer than 8 characters, otherwise they are abbreviated. All imported documents need to be saved as plain text, output formats are also plain or tab-separated texts. Unique to the QSR products (i.e., N6 and NVivo) are the matrix and vector searches and the possibility to display and export the search results as tables. Another distinctive feature is that all text and node searches are automatically saved at a node and can easily be subjected to further analysis.

You can learn to use the program by yourself with the help of the provided tutorial, but a professional training session is especially useful to start on the right foot in terms of setting up the index system.

SOFTWARE OVERVIEW

	ATLAS.ti 5	HyperResearch 2.6	MAXqda
Data Entry	Media types: Text (txt, rtf, doc), graphic (jpeg, bmp, tiff and others), audio (wav, au, snd, mp3), video (avi, mpeg, mov, qt) Support of DBCS languages (i.e., Japanese, Chinese, Korean) both on DBCS and non-DBCS systems Right to left setting possible. Editing of coded documents supported	Media types: Text (txt), graphic (jpeg, bmp, gif, png, pict), audio (wav, aif, mov, mp3), video (avi, mpeg, mov, swf, gif)	Media types: rich text Editing of coded documents supported
Coding			
Smallest unit that can be coded	1 character	1 character	1 character
On-screen coding	By marking the text with the mouse	By marking the text with the mouse	By marking the text with the mouse
Display of code words and boundaries	Code words and boundaries are displayed in the right margin (text and graphic).	Code words are displayed in the left margin.	Code words and boundaries are displayed in the left margin. Each code or sub-code can be given a colour attribute.
Length of code words/node titles	Not limited	64 characters	64 characters
Layers of coding	Not limited	Overlapping codes are difficult to see as no code boundaries are displayed in the margin.	Code tree can be 10 layers deep.

REFERENCES

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The Ethnograph 5.08	QSR N 6	QSR NVivo
Media types: Plain text + special formatting rules, can be done semi-automatically in the built-in editor.	Media types: Plain text + up-front decision about length of text units Editing of coded documents supported	Media types: Rich text not including embedded objects like pictures or tables Editing of coded documents supported
1 line (= 42 characters).	1 text unit	1 character
By marking the text with the mouse or by entering start and stop lines	By marking the text with the mouse or by entering start and stop text units	By marking the text with the mouse
Code words are displayed within the text, the boundaries are displayed in the margin.	Up to 26 boundaries of coded segments can be displayed in the margin if requested.	Code words and coding stripes can be displayed in the margin.
10 characters, longer definitions can be written into the code book.	Not limited	Not limited
12 code words per segment, 7 layers deep.	Not limited	Not limited

	ATLAS.ti 5	HyperResearch 2.6	MAXqda
TEXT SEARCH AND AUTO CODING	String, category and GREP searches. Results of text searches can automatically be coded, the process can be controlled.	Supports text searches for phrases or words. All search results can be autocoded.	Supports text searches for words or phrases in documents, coded text passages and memos. Searches can be restricted to selected texts. Search strings can be combined using two logic operators. All search results can be autocoded.
MEMOS	One or more memos can be attached to documents, quotes, codes and other memos. A comment can be written for all object types.	You can write one memo (annotation) per coded segment. A descriptive comment can be written for codes.	One or more memos can be attached to text passages, one memo can be attached to each code.
DATA RETRIEVAL			
Search operators	Tree sets of operators (Boolean, semantic and proximity operators), in total fourteen operators are available.	Three Boolean and four proximity operators are available.	A total of ten Boolean and proximity operators are available.
Search filters / Can searches be restricted?	Yes. Documents, a group/combination of documents, or a group /combination of codes can be used to filter/restrict searches.	Yes. Searches can be restricted to selected cases (= documents).	Yes. Searches can be restricted to selected documents, sets of documents, and specific variable values.
Display of search results	ATLAS.ti generates a hit list displaying a source tag and the first 30+ characters of the coded segment (quotation). When clicking on a find, it is highlighted in the original context. Before creating a full report, you can first go through the result list and edit it. The report contains complete information about the source of the hit and the search function used.	Reports can be created for the currently selected codes or cases. The following options are available: code frequency, source material, annotations, master case and code lists, list of selected cases, case and code selection criteria, list of selected cases and codes, and master source file list. Thus, it is up to you to decide what kind of information and source tags are provided. Other programmes include this information by default.	In MAXqda the results are immediately displayed in a separate window. Source tags clearly identify the find. When selecting a find, it is displayed in its original context. When running a search on a selected code, search results can also be displayed in table format. The original data segment can be accessed from within the table.

The Ethnograph 5.08	QSR N 6	QSR NVivo
<p>Simple string searches Auto coding not supported</p>	<p>String, category and GREP searches in all or selected documents and nodes. Results of text searches can automatically be coded, the process can be controlled.</p>	<p>String, category and GREP searches in all or selected documents and nodes. Results of text searches can automatically be coded, the process can be controlled.</p>
<p>Memos can be attached to a project, a data file or to lines of text.</p>	<p>One memo can be attached to each document and node. In addition, a description can be written for each document and for each node.</p>	<p>Memos can be linked to documents, selected passages or nodes. Source documents can be classified as memos.</p>
<p>Boolean and proximity operators are available.</p>	<p>N6 offers a total of seventeen Boolean and proximity operators. In addition, matrix and vector searches can be performed.</p>	<p>Six Boolean and six proximity operators are available.</p>
<p>Yes. Variables including specific variable values, identifiers and file codes can be used to restrict/filter searches.</p>	<p>Yes. Searches can be restricted to certain documents or nodes.</p>	<p>Yes. Searches can be restricted to certain documents, nodes, node and document attributes. It is possible to specify attribute values.</p>
<p>The Ethnograph immediately displays the full results, offering you very detailed information about the source of every hit, including the relevant speaker or identifier of the hit, and the search function used. Search results can be displayed as text (i.e., text of the coded segments), a count of all hits, or the percentages of all hits within and across data files.</p>	<p>N6 saves the results of a node search to a node in the Node Explorer. Results can be reviewed by requesting the node browse option. Source tags clearly identify the find.</p>	<p>Like in N6, results are saved to a node in the Node Explorer. In addition, results can be displayed in table format. The cells can optionally display the co-ordinates (node address), number of characters coded, number of documents coded, number of coding references, and number of sets it is a part of. With a double click on a cell, the coded segments can be inspected. Source tags clearly identify the find.</p>

	ATLAS.ti 5	HyperResearch 2.6	MAXqda
Search output	Search results can be displayed on screen, in a text editor (mostly as rich text), saved to disk or printed.	Reports can be displayed on screen or exported as text files.	Search results can be displayed on screen (copied to the clipboard if desired) or saved as a rich text file. When running a search on a selected code, the table can be exported as text only or an html file. The exported html table includes the full text of the coded segments.
Further analysis of search results	Search queries can be saved to super codes and used in further searches. Super codes can be turned into regular codes by creating snapshot codes and subjected to further analysis. Reports of search results can be copied into a memo and assigned as primary documents. This way, they can be coded and analysed further.	HyperResearch offers the unique feature of testing hypothesis. This means that you can define rules. If a rule applies, action and consequences can be specified, like adding or removing codes to/from a case.	As the window with the search results and the original data can be displayed side by side, further coding or modifications to the coded data segments are possible. The entire search result can also be coded to a new or existing code.
WORKING WITH VARIABLES OR ATTRIBUTES	Possible by working with document families or by coding sections. Tables including dichotomous and non-dichotomous document variables can be created in Excel and imported into ATLAS.ti. The variables are then assigned automatically to the specified documents. When coding is required, attribute codes can be assigned automatically, if unique identifiers have been inserted into the text.	The underlying idea of HyperResearch is to work with cases. Depending on how you set up your project, it is possible to analyse data based on certain case attributes.	Variables can be assigned to documents. They can either be defined and entered into MAXqda or imported as tab-delimited tables.
MAPPING	All ATLAS.ti objects (documents, codes, quotes, memos, family groups, super codes, and other networks) can be mapped in the network editor. When connecting codes with other codes, or quotes with other quotes, named relations can be used using simple or arrowed lines. Access to the underlying source material is possible at all times. A hierarchical display in the form of a sideways tree of all objects or of codes only is available.	The code mapping tool allows you to graphically portray master codes and possible relations between them. The code map can also be used as a filter device.	The coding system is displayed in the form of a sideways tree.

The Ethnograph 5.08	QSR N 6	QSR NVivo
<p>Search results can be displayed on screen, printed or saved to disk. The output can be restricted to selected criteria or to marked segments.</p>	<p>A node report can be requested and its content can be copied and pasted into Word or a text editor for further processing.</p>	<p>Node reports are displayed in a text editor. From there, they can be saved or printed. Matrix tables can be exported as text or SPSS file.</p>
<p>Only possible when introducing saved search output as a document to a project.</p>	<p>As search results are saved to a node, they can be handled like any other coded text, i.e. browsed and coded further.</p>	<p>As search results are saved to a node, they can be handled like any other coded text, i.e. browsed and coded further.</p>
<p>Variables can be assigned to documents, speaker and section identifiers. Excel or ASCII text tables can be imported and exported. The Ethnograph offers the easiest solution when it comes to working with variables.</p>	<p>Variables are managed by coding the data material accordingly, either by coding entire documents or sections. For the latter to be possible, sections need to be marked by a unique identifier. The process can be handled automatically by running a command file to create the so-called base data tree.</p>	<p>Attributes like age, gender, profession, etc. can be assigned to documents and nodes. When working with focus group data, the text of a focus group participant first has to be coded to a node. Like in N6, this process can be handled automatically.</p>
<p>The coding system can be displayed in the form of a sideways tree.</p>	<p>The coding system can be displayed either in the form of a hierarchical tree structure, or in the node explorer as a sideways tree.</p>	<p>You can draw models in which items like documents, nodes, attributes or other models can be joined by simple or arrowed lines. The items are linked to the objects they represent. Models can be tied to certain subsets of documents, nodes or attributes. This allows for developing model series. The appearance of each item can be set individually. The coding system can be displayed in the node explorer as a sideways tree.</p>

	ATLAS.ti 5	HyperResearch 2.6	MAXqda
COMBINATION WITH QUANTIAITVE CONTENT ANALYSIS APPROACHES	A list of words including frequencies can be created. A stop list can be used to exclude certain words, characters or patterns. Results can be opened in Excel.	Only simple text searches are possible.	Simple text searches and pattern searches. The add-on module MAXdictio is available, which can be used to perform quantitative content analysis. Add-on tool for online survey research (Form2Data).
QUANTITATIVE OUTPUT	Lists of code words including frequencies (plain text) / cross-tabulation of code words and primary documents (plain text) / lists of word frequencies (tab-delimited or comma separated files) / document variables as tab-delimited or comma separated files / entire coding system as SPSS syntax files.	Code frequencies can be exported (plain text).	List of codes including frequencies as tab delimited files / lists of coded segments (all or only selected code) as tab delimited or html files / variable matrix as tab-delimited files.
TEAM SUPPORT	User accounts can be defined and users can log in. All new entries are stamped with the name of the currently logged in user (= author). The names of authors can be changed if necessary. A set of co-authors can be defined for a project. Projects can be merged employing a number of different strategies. A team may work simultaneously on the same documents on a shared network (albeit only in different project files). If documents are edited by one user, this is transmitted to all users of this document.	Not supported	Memos include an author field. Team members can code a particular text or part of a text. This text can be transmitted to other team members. Simultaneous work however is not possible. A project merge option is provided.
SYSTEM REQUIREMENTS			
Platform	PC / Apple Macintosh with Virtual PC installed / PCI based Sun Ultra Workstations with SunPCI Coprocessor	PC and Apple Macintosh	PC
Processor	Minimum: Pentium/AMD 133 MHz (Recommended: Pentium/AMD 900 MHz or higher)		Pentium 2 or better
RAM	Minimum: 64MB (Recommended: 256MB or higher)	Windows: 32 MB Mac: 64 MB	64MB (minimum)
Monitor	Minimum: 800 x 600, True Colour (Recommended: 1024x768, True Colour)	640 x 480 display resolution, 8, 16, or 24/32-bit colour	Recommended 1024 x 768 resolution

The Ethnograph 5.08	QSR N 6	QSR NVivo
Only simple text searches are possible.	Simple text searches and pattern searches are possible.	Simple text searches and pattern searches are possible.
Code word index (plain text), summary output of search results as text files, frequency output of search results as tab-delimited text files or v4 Excel files. Export of variables as ASCII text or Excel files.	Tables as tab-separated text files for documents or case base data, coding at a node or matrix/vector searches / When creating a report for a node, percentages of text units across and within data files are provided (plain text).	Nodes in all or a set of documents including character counts, numbers of paragraphs, documents and passages coded / character counts or number of passages coded at each node / results of matrix searches are displayed in table format. All tables can be exported as txt or dat (SPSS) files.
Memos include an author field. Coded data files can be combined.	A free project merge tool as an add-on to N6 is available.	Users with administrative functions, team members and guests can be defined. Administrators can determine the user privileges of team members and guests. Merge software can be purchased separately. A new feature offered by the NVivo merge software (as compared to the N6 merge tool) is that projects can be compared on the basis of similarities and differences.

PC	PC	PC
486 or better 33 MHz	100 MHz or better	400 MHz or better
8 MB	Microsoft Windows Me 64MB minimum Windows 2000, XP 128MB minimum 16MB virtual memory	Microsoft Windows Me 64MB minimum Windows 2000, XP 128MB minimum (256MB recommended for XP) Virtual memory: 100MB
Colour VGA Monitor	800 x 600 resolution is acceptable (1024 x 768 recommended)	800 x 600 resolution is acceptable (1024 x 768 recommended)

	ATLAS.ti 5	HyperResearch 2.6	MAXqda
Available disk space	Minimum: 25 MB free (Recommended: 45 MB)		
Soundcard	For processing audio and video data	For processing audio and video data	–
Operating System	Minimum: Windows 98SE W ME, NT 4.0 SP6, W2000 SP3, W XP, W2003 (Recommended: W2000 and XP)	Windows: 95/98/NT, or Windows 3.1 with Win32s Mac: 68040 or better with System 7.5 or higher (Power PC preferred)	Microsoft Windows 98, Windows ME, Windows NT 4.0 or Windows 2000
PRICE	Educational: US\$ 470 Standard: US\$ 950 Student: US\$ 180 Merge tool included	Educational: US\$ 370 Standard: US\$ 644 Student: n/a	Educational: US\$ 445 Standard: US\$ 745 Student: US\$ 250 Merge tool included
USER SUPPORT	Manual, tutorial, FAQ, email support, mailing list, workshops	Manual, tutorial, FAQ, email support	Manual, tutorial, FAQ, Internet user forum, user meetings, workshops.
LANGUAGE	Software and manual: English	Software and manual: English Customer Service Information: English, German, Spanish, Portuguese, Italian, French	Software and manual: English, German, Spanish
HOME PAGE	www.atlasti.com	www.researchware.com	www.maxqda.com

The Ethnograph 5.08	QSR N 6	QSR NVivo
18 MB hard drive space required	15 MB (plus space for project files)	40–125 MB (plus space for project files)
–	–	When linking multimedia date bites
Windows 3.1, 95, 98, some precautions when installing under Windows XP, NT or 2000	Microsoft Windows 95, Windows 98, Windows Me, Windows NT 4.0, Windows 2000 or Windows XP	Microsoft Windows 95, Windows 98, Windows Me, Windows NT 4.0, Windows 2000 or Windows XP
Educational: US\$ 295 Standard: US\$ 515 Student: US\$ 200	Educational: US\$ 340 Standard: US\$ 560 Student: US\$ 150 (12 months limited licence) (product maintenance: add US\$ 65/110) free merge tool	Educational: US\$ 445 Standard: US\$ 735 Student: n/a (product maintenance: add US\$ 85/140) NVivo and Merge for NVivo: US\$ 590/935 (product maintenance: add US\$ 200/120)
Manual, tutorial, FAQ, email support, mailing list, workshops	Manual, tutorial, FAQ, email support (only available for users with maintenance agreement), email user discussion list, user conferences, workshops.	Manual, tutorial, FAQ, email support (only available for users with maintenance agreement), mailing list, user conferences, workshops.
Software and manual: English	Software and manual: English	Software and manual: English
www.qualisresearch.com	www.qsr.com.au	www.qsr.com.au

QUALITATIVE RESEARCH BY THE “GABEK®” METHOD

JOSEF ZELGER

1 INTRODUCTION

It is one of the central tasks of the social sciences and humanities to show ways and means of creatively processing complex problems in our society. Such problems are generally unstructured and difficult to define. Frequently they are tied to dynamic situations affecting a large number of individuals or groups of individuals. On the other hand, the experiences gained by those affected, e.g. the employees of an institution, contain creative problem solving potential. The GABEK® method (GANzheitlich BEwältigung von Komplexität – Holistic Processing of Complexity) suggests itself when trying to make systematic use of this potential. GABEK provides a new method, which can be classified as a qualitative research method. GABEK can be used for both a detailed description and explanation of a problem situation as well as for the practical development of aims and measures and their correlation. It is a problem solving procedure that includes the experiences and aims of all participants and individuals affected. GABEK develops its potential as an instrument of orientation and decision making, particularly in complex problem situations. It supports the development of strategic principles and operational measures relating to these. The PC-program WINRELAN® (Windows Relation Analysis)¹ developed specifically for GABEK applications, also allows the presentation of GABEK results in an interactive way and in accordance with the interests of the audience.

The example used in the presentation

As an example of the results obtained by GABEK [Zelger 1991; 1994; 1999a; 2000b] I have used the INTAS-project 97–30221 *Ethical Conflicts in Medicine: a Cross-Cultural Comparison of Norms, Attitudes, and Subjective Evaluations in Austria, The Netherlands, Georgia and Ukraine.*²

¹ WINRELAN 5 (© 1992–2004 J. Zelger, Innsbruck) was developed for GABEK-applications by Josef Schönegger & Josef Zelger, Department of Philosophy, Section Knowledge Organization, University of Innsbruck, Austria.

² The project – paid by INTAS Brussels and the Austrian Ministry of Science encloses a qualitative study by GABEK *WinRelan* and a quantitative one. On the basis of the qualitative Research a quantitative questionnaire was developed. The cooperating team is: Zelger, J., K. Mühlbacher, M. Gustin (A), U. Hentschel (NL), L.F. Burlatchuk, St. Smovzh, E. Krainikow (UA), Kvatskava, N. Sumbadze, K. Dolidze (GE).

In order to reduce the size of the database, I will present here only the results of open interviews in *The Netherlands*.³ In extended open interviews, 20 laymen or patients and 20 experts in medicine were asked about their opinions on medical ethics. The answers of laymen and of experts were evaluated by GABEK-*WinRelan* separately. The texts of our open interviews were first imported from Word into a new *WinRelan*-file. I shall use the *WinRelan*-file with the results obtained from patients as an example.

Questions and results of a standard GABEK project

In order to provide an overview we have summarized the chief questions and results of a standard GABEK project. The numbers next to the results refer to the chapters of the article. Here I will only describe typical results and some theoretical preconditions. Learning to use the GABEK method with PC-support through *WinRelan* ordinarily takes a training period of from 3 to 5 days.

Questions	Results are given in the form of	Chapter
How can we reconstruct common concepts used in social groups?	Association graphs	2-3
How can we construct a holistic theory about the contents of the verbal database on various levels of complexity?	Gestalten-tree	4-6
How can we represent cognitive assessments expressed in the verbal data?	Evaluation lists	7
How can we identify causal assumptions of the respondents?	Causal opinion networks	8
Which topics are relevant from the respondents' point of view as values, possible aims or measures?	The order of cognitive relevance	9
How can we represent the emotional load of the verbal data?	Emotional graphs	10
Which groups of verbal data should be analyzed separately?	Statistical frequentness	11
How can different social groups learn from each other?	The simulation of dialogues	12

2 THE REPRESENTATION OF VERBAL DATA BY MEANS OF AN INDEXING SYSTEM

In order to construct a meaningful order for the unsorted verbal data we need an indexing system. The development of an efficient indexing system requires two substantial operations: first we must determine what a "text" should be, i.e. the text units must be established. Such units should be meaningful short sections that form a mental unit, like

³ I thank Prof. Dr. Uwe Hentschel (Department of Psychology of the Leiden University in The Netherlands) for allowing me to use the dutch data in the presentation of GABEK.

a spoken record or a short spoken sense unit. Secondly, the content-relevant lexical concepts must be identified in each sense unit.⁴

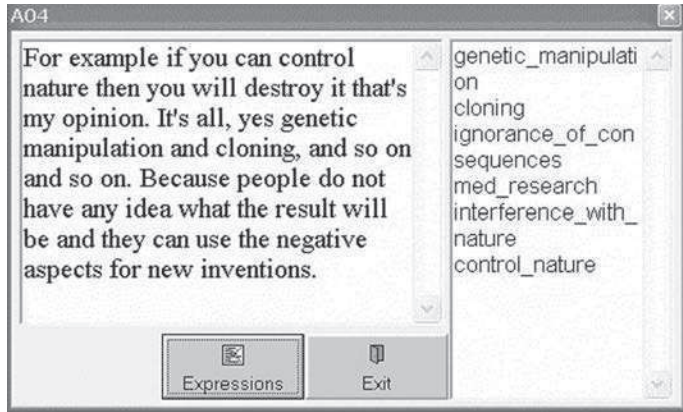


Fig. 1: The text unit "A04" with key terms. The text is taken from one of the patient's interviews.

"Text units" or "Sense units" are not to be understood as mere formal units, but as meaningful, coherent thoughts, which represent a comprehensible text containing at least three and at the most nine relevant lexical concepts. [As to the theoretical basis, cf. Zelger 1991; 1994; 1995; 1999a; 1999b; 1999c; 2000b.]

The definition of *key concepts* occurring in each text unit is referred to as *object-linguistic coding*. In this way, an indexing system is developed which can be represented as a formal network of expressions. However, on the PC the *indexing system* appears in the form of a list, which can be arranged alphabetically or according to frequencies. In alphabetical order, the list also serves to eliminate *synonyms and homonyms*.

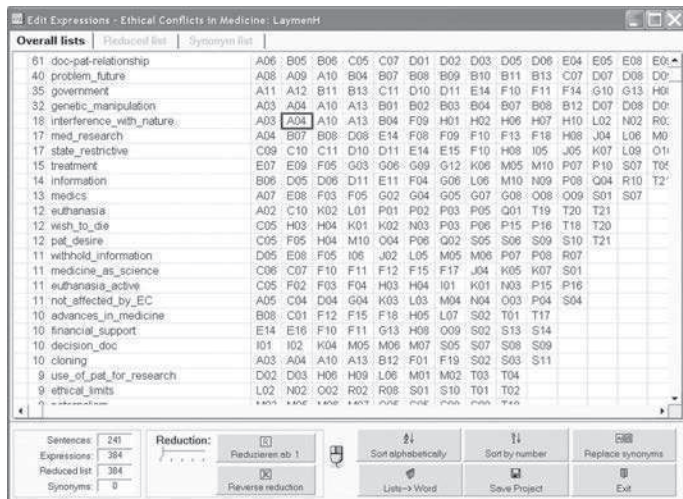


Fig. 2: The indexing system as a list of key expressions represented in order of frequencies. The alphanumeric expressions on the right side are the names of text units, which can be read by double clicking on them (see figure 1).

⁴ The relevance of concepts is based on the consideration that a concept is content-relevant if one cannot eliminate it without changing the central meaning of the statement.


The indexing system enables the user to find all the text units within the verbal database in which the term occurs for any given concept. It permits the user to identify for any selected text unit all further texts that are in any way connected to the given text, so that they can be understood as content supplementations to the selected text.

Such an indexing system will allow different forms of (graphical) representation of linguistic knowledge, all of which can be used for interactive inquiries on the PC and can be exported into Word for Windows.

3 THE REPRESENTATION OF ASSOCIATION GRAPHS, MINDMAPS AND CONCEPTUAL STRUCTURES

According to Ludwig Wittgenstein, the meaning of a term is given by its use in a language game in a concrete life world. Therefore, the meaning of important terms that emerge in the course of discussions can be reconstructed on the basis of all interview texts in which the term was used. They are exactly the ones which are always invariably or logically associated with the definiendum. This means that everyone who understands the language and uses the concept correctly also links the appropriate terms of the definiens with the definiendum – even if this does not always happen consciously or explicitly.

The drawing of association graphs

To reconstruct the meaning of a term in the context of our verbal data base we open the linguistic network .

The expression list appears. In this list we mark an expression which is used frequently in the verbal data. Let us select *problem_future*. For concepts we use definiens which are very often connected with the definiendum in our verbal data base, since the concepts almost always connected with the selected term in many different texts are relevant for the meaning of the latter. So we receive the following graph:

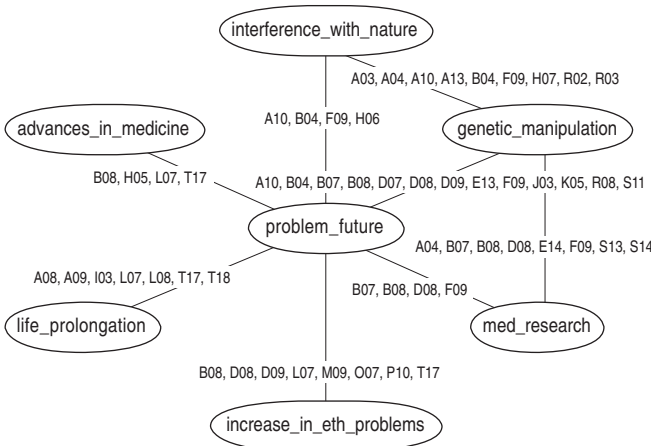


Fig. 3: The association graph for *problem_future* in the context of the patients' interviews. Only those concepts are shown which are related to *problem_future* in at least 4 text units. The text units are indicated in the middle of the connecting lines. The texts can be read on the PC by clicking on any line connecting the terms in the graph.

In the graph of figure 3 which was produced automatically from all texts of Dutch patients with the explicit content of “problems future” we can see that the most usual associations (at least 4 denominations) claim that (ethical) problems in the future will be connected with *genetic manipulation*, *medical research* and *advances in medicine*, with *life prolongation* and with the conviction that these will *interfere with nature*.

Extending the association graph to a mindmap

The association graph represents conceptual associations of a frequently occurring term selected by the analyst. It can be extended by taking specific terms as the center of a new network of associations (e.g. *interference_with_nature*, *genetic_manipulation*, *medical_research*) in figure 4) In this way “mindmaps” of social organizations can be constructed on the basis of the verbal data.

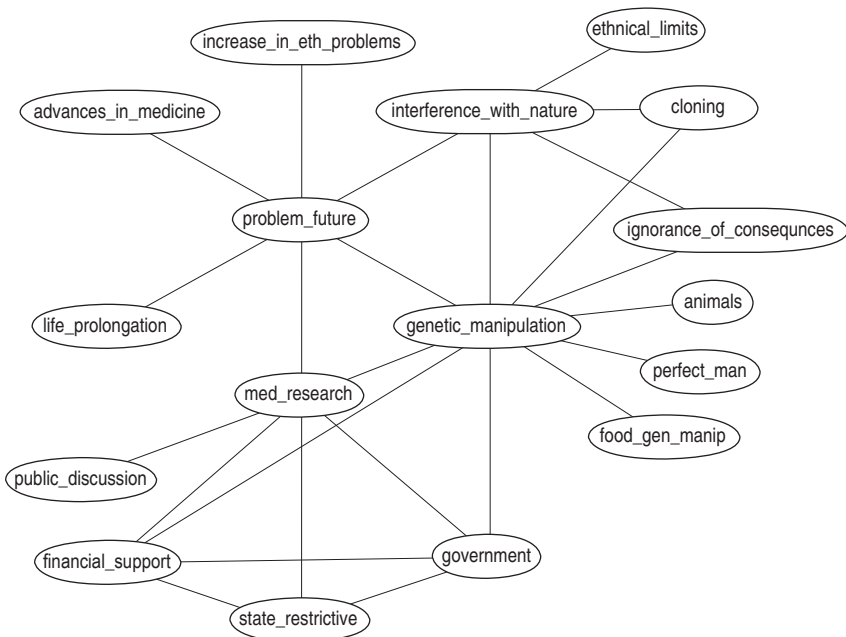


Fig. 4: Mindmap of the concept *problem_future*. The relations are confirmed by at least 4 text units. It shows the most usual associations with it.

Comparing association graphs of different groups

Using different verbal data it is possible to compare the linguistic use of terms by different groups in order to determine possible common features and divergences. So one can even learn about the linguistic use of terms in everyday language discussions, i.e. the use of central terms in relation to the situation and the social organization.

	H05	P10	H06
3	problem_future	problem_future	problem_future
2	restore_to_health		restore_to_health
2	disease_genetic	disease_genetic	
1	prenatal_diagnosis	secondary_prevention	use_of_pat_for_research
1	embryonic_research	treatment_early	drugs
1	advances_in_medicine	treatment	interference_with_nature
1	embryo	increase_in_eth_problems	
1		predict	
1		cancer_pat	
1		genetic_engineering	

In the cluster G006 the statements H05, P10, H06 are connected to a sentence group of three sentences by the three nodal terms

problem_future restore_to_health disease_gnetic

H06 *[Ethical problem in the future] It comes back really to a point mentioned earlier. Umm, I think that experimenting with people that could in fact lead to medicines or the cure for an embryo or something like that, that should not be permitted because I am simply of the opinion that that may not be interfered with.*

H05 *Now, what I really already mentioned at the beginning, medical science will progress [in the future] so far that we eventually will be able to see in the embryo what sort of medical defects there are indeed just by looking at the DNA structure of the unborn baby and that you eventually, maybe through simple medical procedures, illnesses can be diagnosed and cured.*

P10 *[I foresee the ethical problems in the future will increase.] Well, I think increase because before, if someone was sick well, uh, you knew after they became sick. Now with the use of genetic and DNA research, or however you call it, illnesses can be predicted earlier and that has maybe good consequences if early treatment can prevent the disease or slow it, but on the other hand, what does it mean for people who learn that they will die of cancer in 20 years or whatever*

Using all the answers of the verbal database we obtain a very complex linguistic net of sentences connected by nodal concepts. In our case the verbal database is provided by 241 answers of 20 Dutch patients. These were coded by 384 key notions. 232 concepts resulted as nodal notions which connect sentences with each other in the data base. By a cluster analysis, 37 clusters of sentences were produced. One of these clusters was represented in the above matrix of expressions. But can we assume such clusters are relevant? Of course this does not have to be the case. The statements within a cluster can contradict each other. Normally they are redundant and very often they do not contain relevant knowledge applicable to a new problem situation.

To continue we have to answer the question of how we can transform clusters of sentences into meaningful groups of statements. Thus we introduce the concept of a *linguistic gestalt*. Each text group should have the form of a linguistic gestalt: we eliminate the redundant statements in the cluster and search in the database for further

sentences which are meaningful complements. Of course a large number of conditions must be fulfilled:

5 THE RULES OF LINGUISTIC GESTALT-BUILDING

In order to explain the conditions of linguistic understanding [cf. Zelger 1999a; 2000b] the process of perception is investigated. Our world is perceived not by isolated sense data but through perceptive gestalten [Stumpf 1939; Smith 1988]. Stumpf identifies gestalten as complex relations between sense impressions. We select the relevant sensory information in terms of more or less regular distances between sense data.

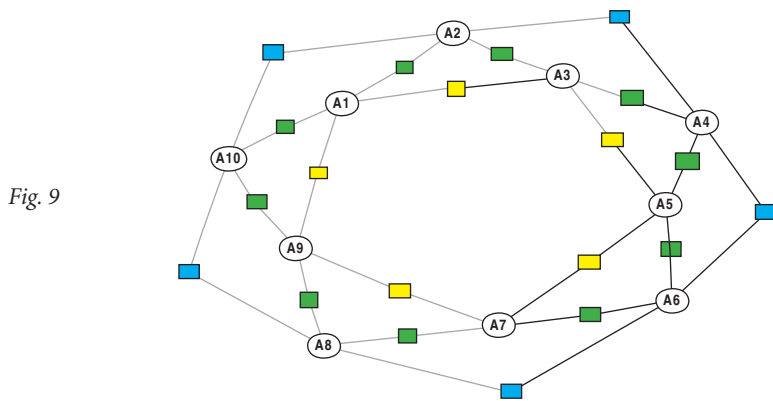
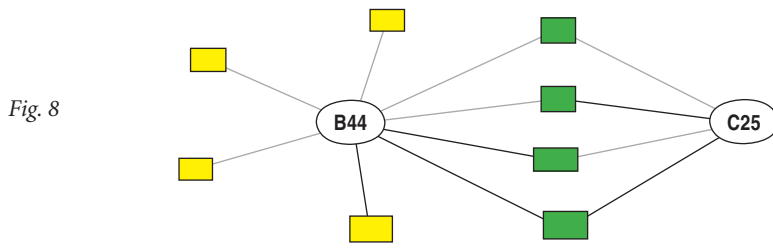
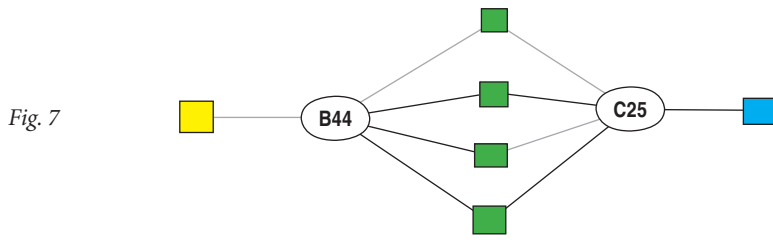
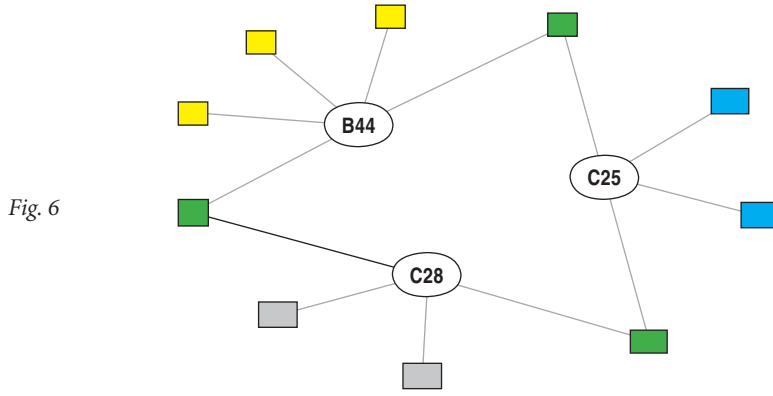
Accordingly, verbal data are understood not as isolated concepts but as coherent linguistic complexes, that is, groups of statements forming meaningful wholes. Thus, we can introduce the concept of a *linguistic gestalt* in analogy to the concept of *perceptive gestalten*.

A *linguistic gestalt* is an abstract entity. It presupposes grouping in parts. These parts are statements (i.e. relations between concepts, in our terminology, text units or file cards). The linguistic gestalt can be distinguished from the larger linguistic context through the interrelation of the text units with each other. The linguistic gestalt is seen as a specific meaningful group of text units which fulfils the following criteria:

- a) *Formal connectivity*: All text units (sentences) within the group are closely connected to each other, i.e. each text unit in the group must contain at least three key concepts which also occur in other text units of the same group.
- b) *Formal variety*: The text units (sentences) within the group must be sufficiently distinguishable from each other. Each text unit must contain something new so that it appears as a kind of complement to all other text units in the group. This entails that,
 - ba) Each text unit in the group must be distinguished from all other text units in the same group. Any pair of text units S_1 and S_2 must contain at least three concepts which are not nodal concepts connecting S_1 and S_2 . As a consequence, any pair of text units containing the same key concepts is not admitted.
 - bb) The key concepts of one text unit in the group may not be included in the set of the key concepts of another text unit in the group.
- c) *Formal distance*: The group of text units (sentences) should not contain too many text units so that all relations between the text units can be reconstructed as a unit of meaning. The necessary steps to move from each text unit to any other text unit in the group must not exceed a maximum of two steps.

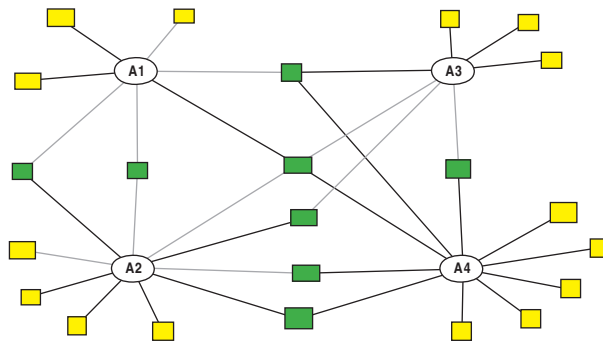
For illustrative purposes, we will show four formal structures which infringe upon the formal rules: In figures 6 to 10 a circle indicates a sentence (text unit) and a rectangle a key concept. Nodal concepts show the connections between text units.

According to the rule of *formal connectivity* (a) structure (6) is forbidden. (7) and (8) are not allowed by the rule of *formal variety* (ba) and (bb). (9) is excluded by the rule of *formal distance* (c).



The structure in figure 10 fulfils all the formal conditions of a linguistic gestalt.

Fig. 10



A linguistic gestalt must furthermore meet semantic and pragmatic conditions:

- d) *Semantic demonstrability*: A group of text units (sentences) is rendered meaningful only if it is possible to demonstrate all relations between the text units intersubjectively. Therefore, an ideal paradigmatic example must exist fulfilling all the conditions assumed. In particular, in communicative learning situations it is necessary to refer to models, examples, and applications in a given modeling facility. As suggested by Pask [1976; 1992], “modeling facility” refers to the material immediately available to the individual with which he can produce observable units, e.g. models which represent all the relations assumed by the text units in the group.

When analyzing texts it is not easy to comply with the above conditions. In particular, the rules of *formal variety* and *formal connectivity* oppose each other. They lead to a kind of equilibrium. The more the sentences are internally differentiated, the more likely that they are not sufficiently internally networked. Furthermore, the closer the network, the more likely it is that the internal differentiation does not suffice. The PC-program *WinRelan* provides interactive methods to select groups of text units from the unsorted verbal database which fulfill the rules for linguistic gestalten.

Below we give an example of a linguistic gestalt.

The linguistic gestalt interference_with_nature

The text group is built by the text units B04, H07, N02, L02, A10 und T01:

B04

[I see genetic manipulation as a problem:]Because in this way we are interfering in nature. On one hand, something is developed which nature absolutely never intended, for example the mouse with an ear grown from cells. And on the other hand, you don't know what the long term consequences are and then of course say in the plant industry or plant improvement it's said that yes, we can make the plants resistant to specific weeds, insecticides, certain insects, but what are the consequences of genetic manipulation on the plants in the long term. Certain consequences that we don't recognize now will surely become evident in

the future. So once again, on the one hand, the aspect that we are doing things that really nature, as we understand it normally, never intended and on the other hand, the uncertainty about our actions now on the affected organism in the future.

H07

In relation to human beings no [no circumstances under which new discoveries are acceptable], but if it was as we now have indeed with the manipulation of food, look I don't know exactly what the effects are from genetic manipulation of foodstuffs, but I can imagine that you finally develop a certain plant or vegetable that in a Third World country under very dry conditions would grow well and thereby fight the war on hunger. Now in that case I would encourage it, but as soon as it interferes in a fundamental way with a human being, then I say no, not that.

N02

[An example of ethical problems in medicine about the beginning of life:] At the beginning of life, how far should we go with technical means to help people fulfill their wish for children. And to what degree do we allow technology to take over for nature.

L02

[For example about creating life], there are recently more possibilities for deciding on having children, not only about their sex but also maybe intelligence. I think that if I were a medical worker that would be very problematic for me. Where does it stop you know, where do all these possibilities end, the interference in human life.

A10

[example of the problems in the future] and the other thing is what I already said about cloning and genetics. Because it means that you control nature too much, you interfere too much, and in my opinion there is a balance in nature. And if man interferes too much the balance will be destroyed and we don't know what will happen then. The outcome is not clear, so we will destroy ourselves, it does not matter, but ...

T01

[In my opinion the most important ethical problems in medical science are:] How far can you go, with continuous weighing of how far man can intervene in life. I have the idea that they want to go still further in medicine and that they want to keep it in their hands and I don't really agree with that.

The text units quoted above are interrelated by the following nodal concepts which determine the chief content of the text group:

	B04	H07	N02	L02	A10	T01
6	interference_ with_nature	interference_ with_nature	interference_ with_nature	interference_ with_nature	interference_ with_nature	interference_ with_nature
3	ignorance_of_ consequences	ignorance_of_ consequences			ignorance_of_ consequences	
3	genetic_manipulation	genetic_manipulation			genetic_manipulation	
3			ethical_limits	ethical_limits		ethical_limits
2			medicine_reproductive	medicine_reproductive		

	B04	H07	N02	L02	A10	T01
2	problem_future				problem_future	
2	food_gen_manip	food_gen_manip				
2				human_life		human_life
2			children	children		

A group of sentences like the above example is already too complex to be conceived as a whole. We must focus on one sentence after another in order to apprehend their connections. Later we will use *linguistic gestalten* as parts again to build still hierarchically higher wholes of meaning. We want to combine linguistic gestalten to form more complex *linguistic hypergestalten*. Therefore we must now reduce the complexity of the above text group. We represent the linguistic gestalt by a summary, one short statement or a few very short ones. It is a kind of condensation which allows further processing of the central ideas of the linguistic gestalt. The summary also is called “selective representation.” The formation of the summary again is governed by rules:

- e) *The formal rule for selective representations:* For the condensation of contents, as a rule we use the key expressions occurring more than once in the text group, i.e.: *The nodal expressions of the text group are the key expressions of the selective representation.*

With rule (e) we presuppose that the contents of linguistic gestalten are determined primarily by concepts occurring as nodal concepts in the text-group. In our case, the nodal concepts occurring more than once in the text-group and used as key-concepts to form the summary are: *interference_with_nature, ignorance_of_consequences, genetic_manipulation, ethical_limits, medicine_reproductive, problem_future, food_gen_manip, human_life, children.*

As a semantic rule the following must be fulfilled by the summary:

- f) *The semantic implication-rule:* In every situation (example, model) in which all the sentences of the text group are true, also the summary must be true, i.e. *the selective representation must follow as a semantic implication from the text group.*

Finally a pragmatic rule must be fulfilled by the selective representation, i.e. the summary:

- g) *Pragmatic applicability:* *The selective representation should be relevant for a person x in the situation S and the point in time t in the sense that x believes in S at t that the summary is applicable as perception-, orientation- or action patterns in a possible new situation.*

Using the above sentences about the topic *interference_with_nature*, we create a summary and call this the *selective representation* of the corresponding linguistic gestalt. It runs as follows:

An important ethical problem in medical science are the ethical limits of interfering in natural development and reproduction, which will become a bigger problem in the future. Modern technology is replacing nature, for example the genetic manipulation of embryos (deciding the characteristics of children), human beings or food. The question is how far man can intervene in life and nature without destroying the balance of nature.

If all the above rules (a) – (g) are fulfilled, then the text group together with the summary form the linguistic gestalt.

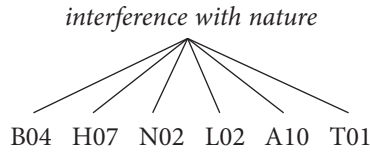



Figure 11: The linguistic gestalt *interference_with_nature* formed by 6 text units and their selective representation.

In order to obtain a coherent and holistic representation of the verbal database, all responses must be compressed into hierarchically structured text groups and represented in the form of a *gestalten tree*. This is a hierarchical order of texts generated step by step from bottom to top by the GABEK method in a way such that each text on a higher level follows logically from a text group on a lower level. This leads to the result that linguistic gestalten always have a deep structure (the substantiating text group) and a surface structure (the summarizing text).

6 THE HOLISTIC REPRESENTATION OF VERBAL DATA IN THE GESTALTEN-TREE

According to the above procedure, the 241 answers of different patients in the Netherlands were condensed in the subprogram *editing Gestalten*  to 23 linguistic gestalten and to summaries of them. These again are used to form *hypergestalten* – according to the same rules of gestalt-building: when all text groups have been processed in this way, a set of summaries (selective representations) of text groups will be available to which the cluster analysis is again applied. The summaries of gestalten form text groups on the next higher level. For instance our selective representation of *interference_with_nature* was combined with six other summaries of linguistic gestalten to the hypergestalt *problem_future* (see figure 12).

When all the text groups on this level are transformed into *hypergestalten* then we do the same recursively on the next higher level to form *hyperhypergestalten*.

The whole structure is called *gestalten-tree*. It can be seen as an ordered overview of all the verbal data.

As in a puzzle, sections (in *WinRelan* statements, i.e. responses) are arranged into a pattern. These patterns – (in *WinRelan* thematically interconnected text groups, which due to the fulfillment of the semantic, syntactic and pragmatic rules, are called linguistic gestalten – see chapter 5) – again serve as puzzle pieces for the construction of even larger patterns. The large patterns (linguistic hypergestalten), which result from this information-compressing process, are finally joined to form an overall view (gestalten tree).

On the highest level of the gestalten tree for our project, the most important results of the investigation are summarized in two short texts. These are general trends similarly expressed in many interviews.




If we look at the opinions of the patients, we will find the following two texts, which can be regarded as total summaries, on the 4th level of the hyperhypergestalten. If someone who has only a minute to spare asks for the result of the project, he can be given an answer with the aid of the following two texts, representing the results of the GABEK-analysis still in object-language as stated by respondents:

Hyperhypergestalt *future_problem*

An important ethical problem for the future is genetic manipulation. The technological development (advances) in medicine and medical science (medical research) could create new ethical problems in the future.

Hyperhypergestalt *treatment_decision*

Morally incorrect behavior in the doc-pat relationship occurs if the doctor withholds information about the patient's treatment. It should be within the autonomy of the patient to decide about his/her own treatment (pat_desire).

We open the subprogram  Gestalten tree). There the resulting hierarchical structure of our gestalt building operations appear. We click on the icon  and select the term problem_future. Then all the texts are marked in dark green color  where the concept *problem_future* occurs.

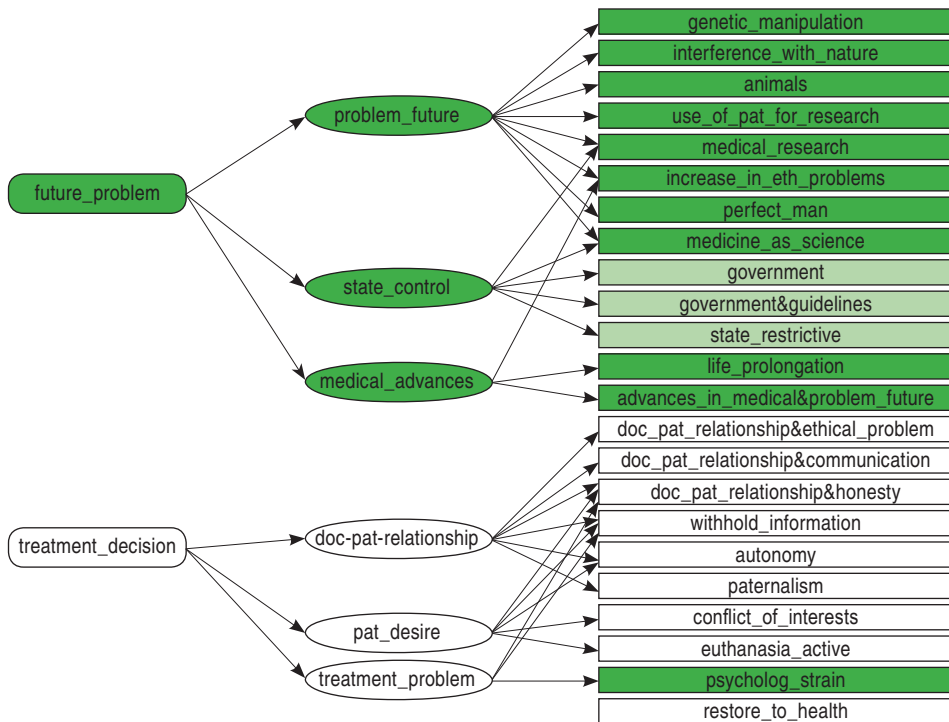


Fig. 12: Gestalten tree with marked texts about the topic *problems_future*. One is able to cut out parts of the gestalten tree according to any key concept (as for instance the green part of our gestalten tree).

In the gestalten tree we find the two texts on the left side. By clicking on the appropriate fields, the corresponding text is displayed.

If someone asks for a *justification* for these two texts, then one will go to the lower level of the gestalten tree. There the hypergestalten are situated, which describe the individual ranges of topics in greater detail. The justification of the first Hyperhypergestalt *future_problem* through three hypergestalten reads as follows:

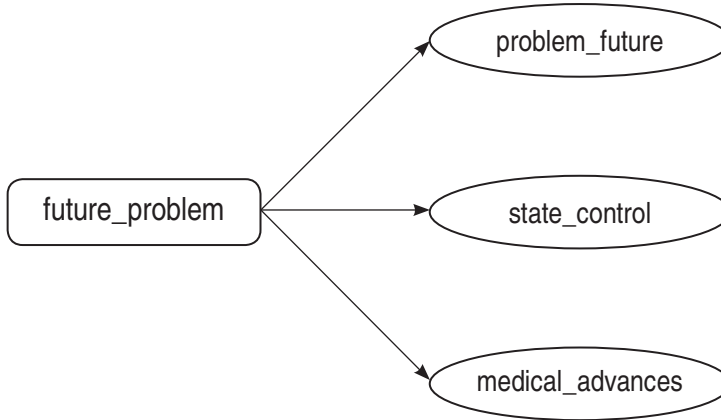


Fig. 13: Foundation of the Hyperhypergestalt *future_problem* through three summaries of hypergestalten.

Hypergestalt problem_future

An important ethical problem in the future is genetic manipulation. Also, because progressive medical development and medical research interfere with nature creating the “perfect man” more and more. This can increase ethical problems. The consequences of this kind of medical research should not be ignored.

Hypergestalt state_control

The government or the State should stipulate (restrictive) laws regarding the ethical problems in medicine. The ethical problems should be a part of public discussion too. The government has to control certain medical research like for example genetic manipulation, which could become a bigger ethical problem in the future but they should also support medical science or research financially.

Hypergestalt medical_advances

Because of the technological progress (advances) in medicine and medical science development, new ethical problems will increase in the future. There will be more possibilities to prolong life and the question is how far we should go with this kind of treatment.

Below these texts there are the gestalten which justify the hypergestalten. Here ranges of topics from the analyzed data supporting the hypergestalt *problem_future* are represented in detail.

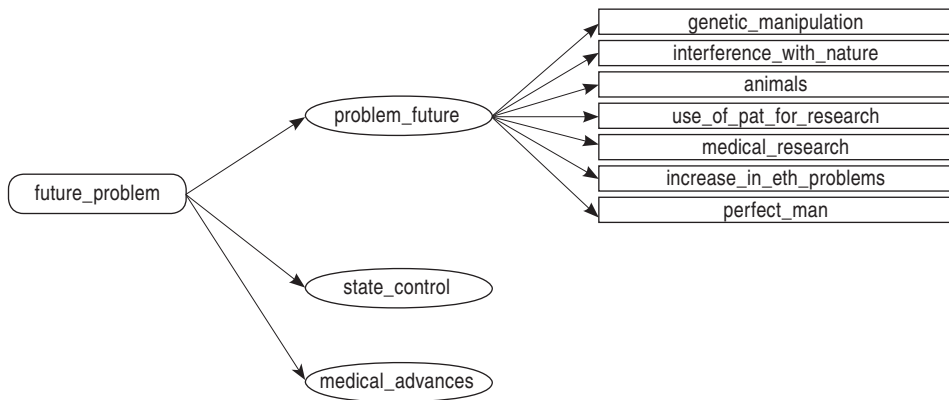


Fig. 14: Foundation of the text *problem_future* (*hypergestalt*) through three summaries of *gestalten*.

Gestalt genetic_manipulation

Genetic manipulation will be an important ethical problem in the future. In medical research experts are trying to map out the human gene picture. On the one hand, it'll help with incurable diseases but on the other hand it could be manipulated (for making "perfect" humans) and become commercial.

Gestalt interference_with_nature

The interference of medical science with the natural development and the reproduction of nature will become an important ethical problem in the future. Modern technology replaces the nature, for example genetic manipulation of embryos (deciding of their characteristics), of human beings or of food. The question is how far man can intervene in life and nature and where the ethical limits are (so as not to destroy the balance of nature).

Gestalt animals

Animal experiments and genetic manipulation with animals are ethical problems. The interference with nature or human beings is an ethical problem too and will increase in the future.

Gestalt use_of_pat_for_research

Sometimes medical doctors use information for research or experiments. In doctor-patient-relationships, for medical research, the doctor has to ask the patient for permission to use the patient for research or for drug tests. The problem which could increase in the future is that medical research must be controlled in order to reduce the interference with nature.

Gestalt medical_research

Medical research, as for example genetic manipulation, could produce ethical problems. In the future ethical problems will increase. They should be a part of public discussion. The government should make decisions about research too and support it financially. The consequences of this kind of medical research should not be ignored.

Gestalt increase_in_eth_problems

Medical development (advances in medicine) will increase (even more) the ethical problems in the future. Some of them will be: the possibility of genetic manipulation, "computer medicine" (people as numbers), xenotransplantation, the artificial lengthening of patients' lives, the artificial giving of life, the use (abuse) of secondary prevention.

Gestalt perfect_man

One of the ethical problems in the future is cloning by genetic manipulation. People are trying to be smarter than God and they'll try to make their image more beautiful than it already is – a perfect man.

On the level of gestalten again we can *ask for justification*. In such a case we click on the corresponding field of the gestalt (on the right hand of figure 12, for example on the gestalt "animals"). Then the original answers of the respondents appear, which are the premises from which the summary of the gestalt is deduced.

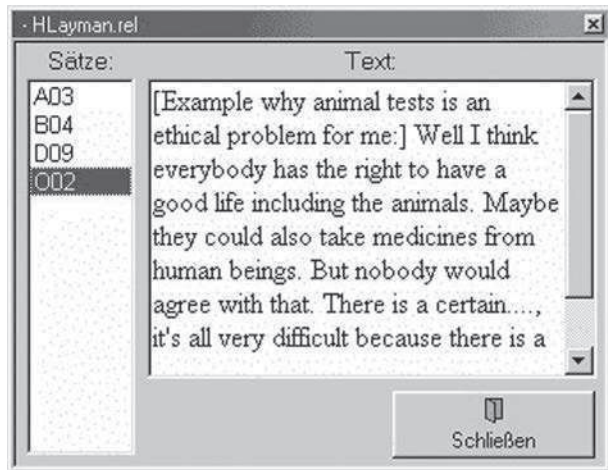


Fig. 15: Original answers which are summarized in the gestalt animals.

So the gestalten tree is to be understood as a deductive system of texts. Each level of texts supplies a more or less complex representation of the total situation. All summaries are formulated in the everyday-language of the respondents. This theoretically founded procedure makes it possible for the people concerned to find themselves within a complex opinion network and thus understand themselves and other people better. *Therefore GABEK is a learning procedure.*

In the GABEK procedure, a description of the phenomenon under investigation thus takes place on different levels of complexity. If the reader of a GABEK project report wants an overview of the results, he can look at the texts on the highest levels. If he however wants to have detailed answers to questions, he will go on to deeper levels. This is because each text of the different levels, except the level of the verbal data, is a summary which was created by the analyst in accordance with clearly defined rules and which can logically be derived from the texts on which they are based.

The order is a fractal one, as all textgroups on all levels fulfill the same formal rules and the higher levels of the gestalten-tree refer to the same topics in a holistic guise as the lower ones. We took *sentences* as relations between key expressions, *gestalten* as relations between sentences, *hypergestalten* as relations between gestalten, etc. Each hypergestalt is then a formal net of nets of nets of key-concepts. Its nodal concepts are also concepts on all the lower levels. Therefore we obtain a self-similar structure.

The gestalten-tree is now our linguistic map.⁵ Clicking on the names of gestalten, hypergestalten or hyperhypergestalten, we get the summarizing texts. If we want an explanation of any topic we read the corresponding texts on the lower level. Thus by clicking on any field each text of the gestalten-tree can be explained by texts containing more details.

The highest statements are more general, the lowest more specific and complex. The highest texts further our understanding in many situations; the lowest are more informative for concrete problem solving and decision making.

The highest provide an overview of the whole and are networked in the wide linguistic context. They are interrelated with the complete verbal data base. The lowest show strong emotional loads. Therefore they are more interesting for motivation. According to the specific goals of data presentation, we can switch from top down to bottom up.

7 THE REPRESENTATION OF ASSESSMENTS BY EVALUATION LISTS

Normal linguistic texts first of all contain descriptions. Furthermore, common language expressions contain prescriptive judgments, and thus value judgments and norms. In order to represent prescriptive judgments of a verbal database clearly, apart from the basic coding, evaluation coding is necessary. This requires that we arrange the features, states, situations, actions, processes, which were evaluated positively or negatively by the respondents, in the form of lists.

In our example we will answer only the question: which real phenomena, attributes, states, situations, actions are expressed and how were they evaluated by the respondents?

Here we show parts of lists of exclusively positive, predominantly positive, exclusively negative and predominantly negative evaluations contained in the verbal data of patients.

The sign “+” indicates positive evaluations of actual objects, attributes, states, situations; the sign “-”, negative ones. Neutral assessments are not displayed here. The indices on the right hand of the lists refer to the corresponding statements in the responses of patients. By clicking on any of them, the corresponding original text appears. The proportion of positive and negative evaluations shows that the topic “ethics in medicine” is judged as a serious problem.

⁵ Rudolf Wille in his lecture “Conceptual Landscapes of Knowledge: A Pragmatic Paradigm of Conceptual Knowledge Processing” at the International Conference *Conceptual Knowledge Processing* (Darmstadt, February 1996) explained the chief scientific activities using the metaphor of a landscape with our activities in it (exploring, searching, recognizing, identifying, analyzing, investigating, deciding, improving, restructuring, memorizing). As examples he pointed at various projects in the field of *Formal Concept Analysis*.

Actual situation

Positive

+	4	control_state	E15 H08 J04 J07
+	2	medicine_preventive	C01 F08
+	2	donation_of_organ	C06 C07
+	2	responsibility_state	S15 S16
+	2	Communication	E09 P09
+	2	ethics_committee	J05 J07
+	2	med_point_of_view	F04 I02
+	2	private_hospital	G02 G08
+	2	euthanasia_active	F04 H04

Predominantly positive

+	10	state_restrictive	C10 D11 E14 E15 H08 I05 J05 K07 L09 S13
-	1	"	Q07
+	4	Medics	F03 G02 G04 G08
-	3	"	E08 S01 S07
+	4	decision_doc	M06 S05 S08 S09
-	1	"	M05
+	3	restore_to_health	I04 J06 K06
-	2	"	H02 H10
+	2	contact_to_pat	E09 P09
-	1	"	E10
+	2	euthanasia_passive	T09 T11
-	1	"	Q02
+	2	Guidelines	C10 H08
-	1	"	Q07
+	2	public_discussion	D11 P12
-	1	"	E02
+	2	Treatment	E09 K06
-	1	"	E07

Negative

-	13	interference_with_nature	A03 A04 A13 B04 H01 H02 H06 H07 H10 L02 R02 R03 T02
-	8	withhold_information	D05 E08 F05 J02 L05 M05 P07 R07
-	7	cloning	A03 A04 A13 B12 F01 S02 S03
-	7	coma_pat	T06 T07 T08 T09 T10 T11 T18
-	7	use_of_pat_for_research	H06 H09 L06 M01 M02 T03 T04
-	6	conflict_of_interests	C05 H03 H04 P06 Q02 S05
-	5	politicians	A11 F14 F15 F18 F19
-	5	gods_in_white	D06 O05 P09 R05 T14
-	5	ignorance_of_consequences	A04 B04 E14 F13 H07
-	4	medicine_repairing	C01 C02 C03 C11

-	4	advances_in_medicine	B08 L07 T01 T17
-	4	treatment_error	E04 E07 E08 E10
-	4	economy	B03 B11 S15 S16
-	4	animal_experiment	B04 B12 O01 O02
-	4	life_prolongation	I03 L07 L08 T18
-	4	lack_of_drugs	G01 G03 G09 G12
-	4	over-treatment	T02 T04 T06 T23
-	3	pollution	B09 B10 B11
-	3	dementia	A09 M02 M03
-	3	neglect_pat	H04 O04 T04
-	3	complaint	G03 G07 L08
-	3	time_pat_has_left	I04 T03 T05
-	3	interest_research	M01 T03 T04
-	3	operation	C01 C03 F16
-	3	troublesome_pat	G03 G04 G05
-	3	prenatal_diagnosis	H01 H10 K05
-	3	concealment_of_research	S02 S03 S12
-	3	perfect_man	F01 J01 L02
-	3	abortion	H01 H02 K05
-	3	technical_jargon	D05 D06 R07
-	3	raise_pat_hope	S10 T04 T14
-	3	experts	F17 T15 T16
-	3	doc_working_as_commercial_company	C08 F16 S01

Predominantly negative

+	3	genetic_manipulation	B02 E16 K06
-	12	"	A03 A04 A13 B04 B08 B12 D08 J01 R01 R02 R03 S12
+	6	Government	D11 H08 I05 S13 S15 S16
-	9	"	A11 B11 F14 K07 O09 O10 Q07 T19 T21
+	1	Death	I03
-	2	"	E04 E06
+	1	Legislation	E15
-	2	"	T19 T21
+	1	pat_desire	H04
-	2	"	S09 S10
+	1	refusal_to_fulfill_desire_of_pat	S09
-	2	"	C05 H04

Statistics

Evaluations overall:	595	100.0 %
of which positive	105	17.6 %
of which negative	318	53.4 %
of which neutral	172	28.9 %

Finally, it should be added that in the interviews no particular evaluations were requested. Rather, all evaluations were expressed spontaneously by responses to open interview questions. The evaluation lists show the values of Dutch patients at the time of the questionnaire which are related to ethical conflicts in medicine. These values may refer indirectly to their weltanschauung.

The evaluation lists are an interesting means to compare the desires and concerns of different groups – possibly conflicting groups.

8 THE REPRESENTATION OF CAUSAL ASSUMPTIONS BY NETWORK GRAPHICS.

Apart from descriptions and evaluations in normal language texts, causal assumptions are also expressed. These are opinions on cause-effect connections, based on empirical experiences or discussions. We can also see the causal assumptions of the respondents as arguments for the reasonable control of our actions.

In order to isolate causal assumptions from the verbal data, the electronically stored texts of each record sheet must be read again and coded in a new way. Expressions which are connected by causal relations are called causal variables or simply variables, since they refer to situations that are experienced as changeable. Of course causal analysis also depends particularly on the question of analysis.

Ordinarily we will ask the question: does the author of the text believe that the growth of the variable A leads to an increase or decrease of the variable B? There are very different normal language statements applicable to such coding, e.g. quantitative relations (in the form of "the more A, the more B"), real relations between cause and effect ("A is a cause of B"), qualitative relations specification ("if A improved, then the quality of B would also increase"), statistical generalizations ("if A, then mostly also B" applies), and many others.

In addition we can distinguish between causal influences which have either a favorable or an unfavorable effect on another variable.

The analysis of causal assumptions leads to complex nets of opinions about influence relations, which can be used in new situations for orientation. A multiplicity of executed projects showed that causal networks which represent hypothetical relations contribute substantially to preparing decision planning in new situations⁶.

Additionally, a color can be assigned to each variable in the causal expression list. Different colors will be assigned to basic values, main goals, subordinate goals, meas-

⁶ In addition, this type of coding of relations, which we mostly use for the representation of causal opinions, can also be used for other purposes. Principally one can represent each type of two-place relations, e.g. communication relations between departments of an organization or between individuals. Instead of causal variables only names of persons or of departments would be used between which a communication relation of a certain type is assumed, for example when it is being said that there are business relations between two organizations or cooperation relations between individuals.

Furthermore, process plans, for which temporally arranged relations between operations, prerequisites of operations and results of operations are being coded, can be represented on the basis of experiences of employees.

ures, or invariable basic conditions. There are guidelines for this procedure. But the distinction between intrinsic basic values and main goals, lower goals or measures is made by the analyst.

In our project, ethical values were coded in blue, ethical conflicts or problems which may also express aims gray, goals in green, measures in yellow and conditions which cannot be changed in bright yellow.

Now causal assumptions can be plotted in the subprogram *Causal net graphs* automatically. E.g. one can select a variable, for instance *problem_future* and ask which other variables would influence (ethical) problems for the future.

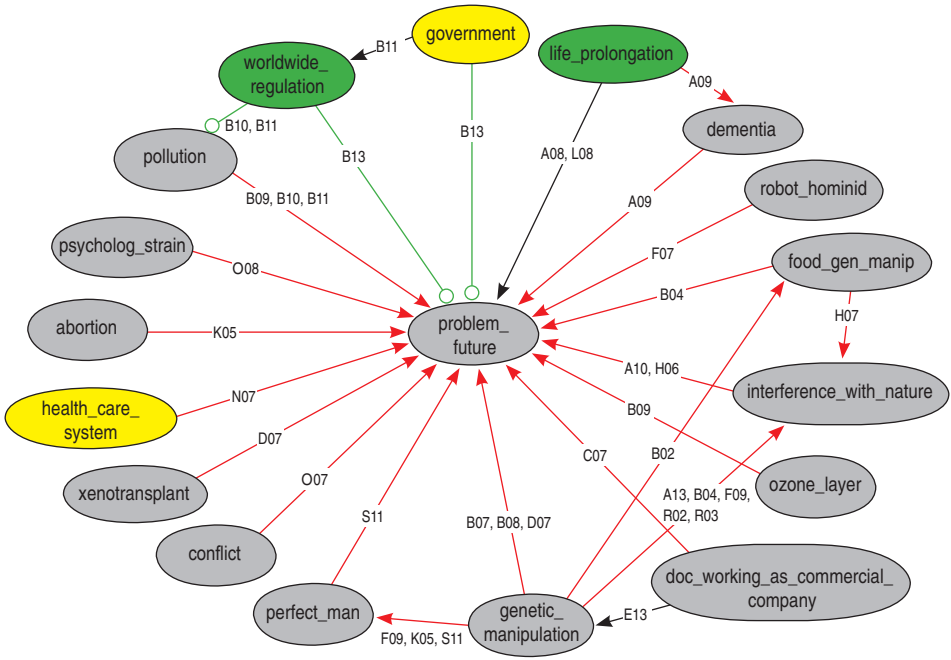


Fig. 16: Influence variables relating to problems_future according the patients.

We see that almost only unfavorable influences are presented (red arrows). Besides three black arrows (influences considered ambivalent), the patients express only worldwide regulations and the government which could reduce problems in the future (green lines with a small circle).

In order to reduce complexity in the causal network as well, we can draw only those arrows which are supported by at least two (or more) text units. If we define the minimum number of sentences as 2, then the following figure 17 results:

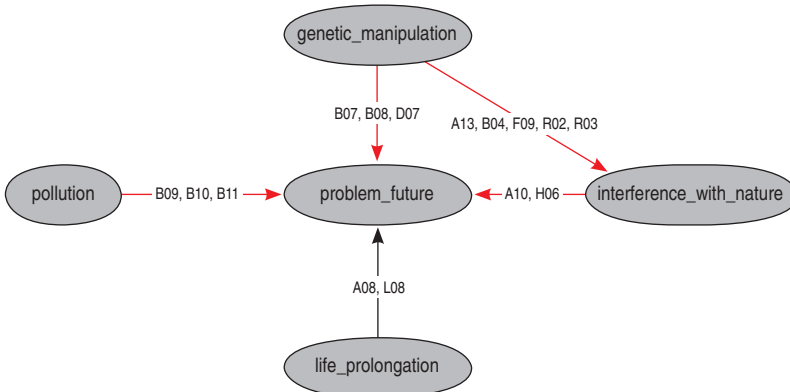


Fig. 17: Influence relations relating to problems_future according the patients, founded on at least two texts. Pollution, genetic manipulation and as an effect of this, interference with nature leads to more problems in the future, which is assessed as negative (red arrows). Beside this, life prolongation can also lead to problems in the future which is seen as an ambivalent influence (black arrow).

As in the linguistic network, we can also navigate through the whole net in order to enlarge the graph.

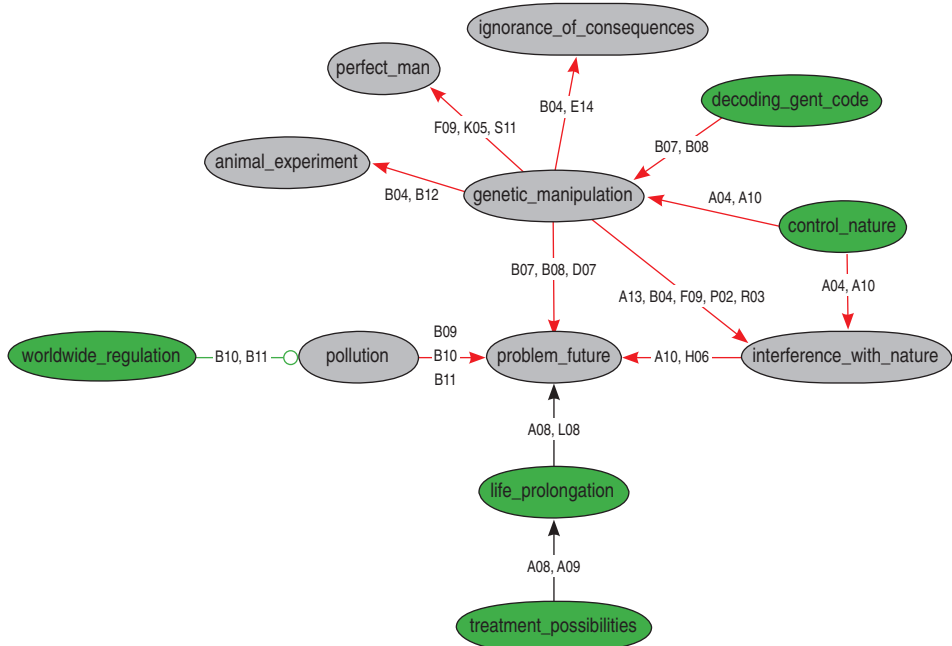


Fig. 18: A larger causal net based on the variable problems_future, where each causal assumption is supported by at least two text units. Here we also see a positive influence relation in green color. World wide regulations can reduce pollution (where the reduction of the variables is indicated by a small circle).

In addition to proceeding from problems (*problem future*) to their assumed causes (see above), we can also take the reverse path and proceed from measures to their assumed consequences. So we can ask how the government may influence the situation:

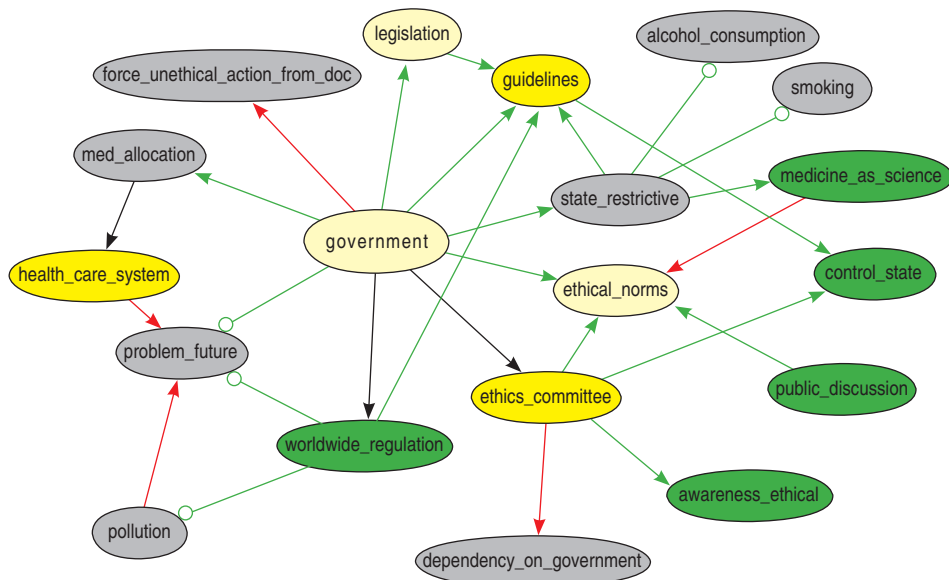


Fig. 19: Possible influences by the government are predominantly positive.

Each assumed causal relationship is supported by texts. By clicking on the connecting arrows with the right mouse key, one can immediately read the texts in which the causal assumption was expressed. Graphs as well as texts can easily be transferred and printed out in Word.


9 THE REPRESENTATION OF THE ORDER OF COGNITIVE RELEVANCE

We have already come across an abundance of different results. Now the question arises which topics are really important from the view of the respondents. Which results have to be considered under any circumstance? If we want to know what is particularly important and relevant to those concerned, then GABEK supplies unique results. We use three criteria for weighing the topics:

- i) A topic A is judged more relevant than a topic B if A reaches a higher level in the gestalten tree. The reason for this criterion is that results on higher levels (hyperhypergestalten or hypergestalten) are applicable in more cases than results on lower ones (gestalten or responses).
- ii) A topic A is judged more relevant than a topic B if A is evaluated more often by the respondents as positive or negative than B.
- iii) A topic A is judged more relevant than a topic B if A is embedded in more causal relations than B, i.e. if A has more effects or is influenced by more causes than B.

A topic which ranks high on all the three criteria is certainly very important in the minds of the respondents.

From this a relevance list is generated, which indicates at a glance which addressed ranges of topics are of special interest to the persons concerned (viz. Zelger 1999c, 2000a).

We open the subprogram *Relevance Analysis*  and obtain a list with 5 areas, whereby the highest level of the gestalten tree in which the key concept or topic still occurs is indicated in the 1st column (blue). In the 2nd Column (white) the appropriate term is displayed; in the 3rd to 6th column (green) the number of the positive and negative evaluations is mentioned; in columns 7 to 9 (red) the number of causal relations, and finally, in the last column the assigned colors (color coding) are pointed out.


Relevance list

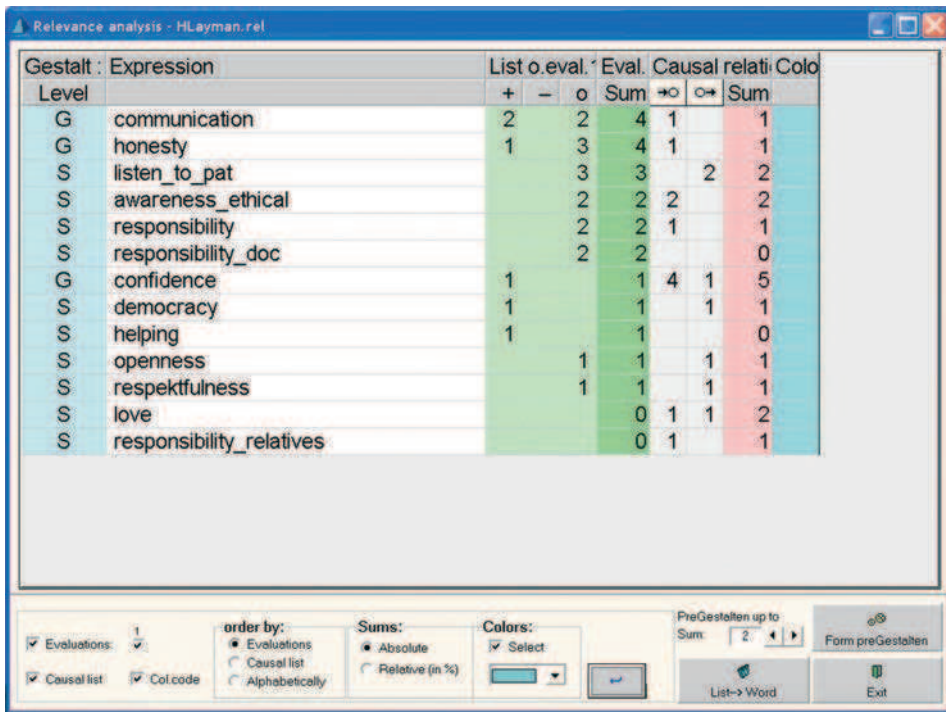
Gestalt	Expression	List of.eval.		Eval.	Causal relations			Color
		+	-		→o	o→	Sum	
Level				Sum				
HH	genetic_manipulation	3	12	19	7	11	18	
HH	Government	6	9	18	2	9	11	
HH	interference_with_nature		13	16	10	3	13	
HH	state_restrictive	10	1	15	1	4	5	
HH	Information	1		11		2	2	
HH	withhold_information		8	9	2	4	6	
G	medics	4	3	8			0	
HH	paternalism	4	4	8	1	2	3	
G	cloning		7	7	3	4	7	
S	coma_pat		7	7			0	
S	euthanasia		2	7	2	1	3	
HH	med_research	2	2	7	1	4	5	
G	use_of_pat_for_research		7	7	4	1	5	
HH	conflict_of_interests		6	6	5	1	6	
G	ethical_limits	1		6	1		1	
HH	euthanasia_active	2		6	1	2	3	
S	euthanasia_passive	2	1	6			0	
G	guidelines	2	1	6	4	1	5	
HH	life_prolongation		4	6	1	4	5	
S	life-death		2	6			0	
HH	public_discussion	2	1	6	2	1	3	
HH	autonomy	1		5	1		1	
HH	decision_doc	4	1	5	2		2	
HH	financial_support			5		2	2	
S	gods_in_white		5	5		1	1	
HH	ignorance_of_consequences		5	5	5		5	
S	medicine_preventive	2		5		7	7	

S	politicians		5	5		3	3	
G	restore_to_health	3	2	5	3		3	
HH	advances_in_medicine		4	4	2	4	6	
G	animal_experiment		4	4	1	4	5	
G	communication	2		4	1		1	
S	complaint		3	4			0	
HH	control_state	4		4	2	1	3	

Fig. 20: The list of relevance for patients.

Operations of selection

The list can be reduced according to different keys, as can be seen in figure 21. It can be ordered according to the frequencies of the evaluations listed (Bewertungen), the causal listing (Kausalliste) or alphabetically (alphabetisch). Each option must be confirmed by . In the case of more than one evaluation list, further selection options appear. Both the causal list as well as the evaluation list can be switched off. The values can also be displayed in percents in order to be compared with other project results.



The screenshot shows a software window titled "Relevance analysis - H.Layman.rel". The main area contains a table with the following columns: "Gestalt : Expression", "List o. eval.", "Eval.", "Causal relati", and "Color". The table lists 17 expressions, each with a "Level" (G, S, HH) and numerical values in the other columns. Below the table is a control panel with several sections: "Evaluations" (checked), "Causal list" (checked), "Col.code" (checked), "order by:" (radio buttons for Evaluations, Causal list, Alphabetically), "Sums:" (radio buttons for Absolute, Relative (in %)), "Colors:" (checked, Select), "PreGestalten up to Sum:" (set to 2), "Form preGestalten", "List-> Word", and "Exit".

Gestalt	Expression	Level	+	-	o	Sum	→○	○→	Sum	Color
G	communication		2	2	4	1			1	
G	honesty		1		3	4	1		1	
S	listen_to_pat				3	3		2	2	
S	awareness_ethical				2	2	2		2	
S	responsibility				2	2	1		1	
S	responsibility_doc				2	2			0	
G	confidence		1			1	4	1	5	
S	democracy		1			1		1	1	
S	helping		1			1			0	
S	openness				1	1		1	1	
S	respektfulness				1	1		1	1	
S	love				0	0	1	1	2	
S	responsibility_relatives				0	0	1		1	

Fig. 21: Fundamental values of patients ordered according the number of evaluations.

When we click on “*select color*” and we choose e.g. *blue* then according to our color coding we receive the values expressed by the respondents (as blue is reserved for fundamental values).

In the same way, we also can select higher aims, goals, measures or fundamental conditions which can be ordered according to our relevancy criteria.

So basic values or main goals, for example, can be selected and represented in *Win-Relan* separately (As it is shown in figure 21).


Basic values are important for the development of mission statements and for orientation purposes, main goals serve for the basic adjustment and justification of projects and intermediate objectives and measures are needed for their concrete implementation.

10 REPRESENTATION OF THE EMOTIONAL RELEVANCE

Of course the three criteria of relevance (chapter 9) do not include all aspects of relevance. Besides these cognitive perspectives, the emotional load of verbal expressions has to be taken into account. So we ask the question of how we can represent the mood in which the responses were given.

To analyze the emotional load of the statements [cf. de Mause 1979; Tafertshofer and Zelger 1982] a special kind of fundamental coding is required (cf. chapter 2). We must include as key terms both the emotionally loaded expressions, symbols, metaphors, body language expressions as well as unexpected special and unusual concepts. On the other hand, we do not pay any attention to the rational content which very often hides the emotional content of the statements. We especially omit all the logical operators. As emotional feelings do not have negations, we do not take the negations into account. (According to Freud, the unconsciousness also has no negation: thus if I say: “I do not want to talk about violence”, then on the emotional level I still enter the topic “violence.”) Furthermore, we do not pay any attention to abstract terms which designate ideals or theoretical constructs.

Drawing of emotional graphs

After such an emotion coding, we start the subprogram *linguistic network* . In the expression list we select a concept for which we want to display the emotional connotations. Let us choose *medics* (physicians). It is the perspective under which we look at emotions and feelings connected to it. Then we do not display the expressions very often connected to “medics” as we did in the association graph in order to determine contextual meaning of the term. Here we do something different. We select only the emotionally loaded terms connected to our key term “medics.” Most of them occur only once or a few times as emotional feelings often are hidden in ordinary language texts. Then for “medics” the following expressions appear in the proximity of the term.

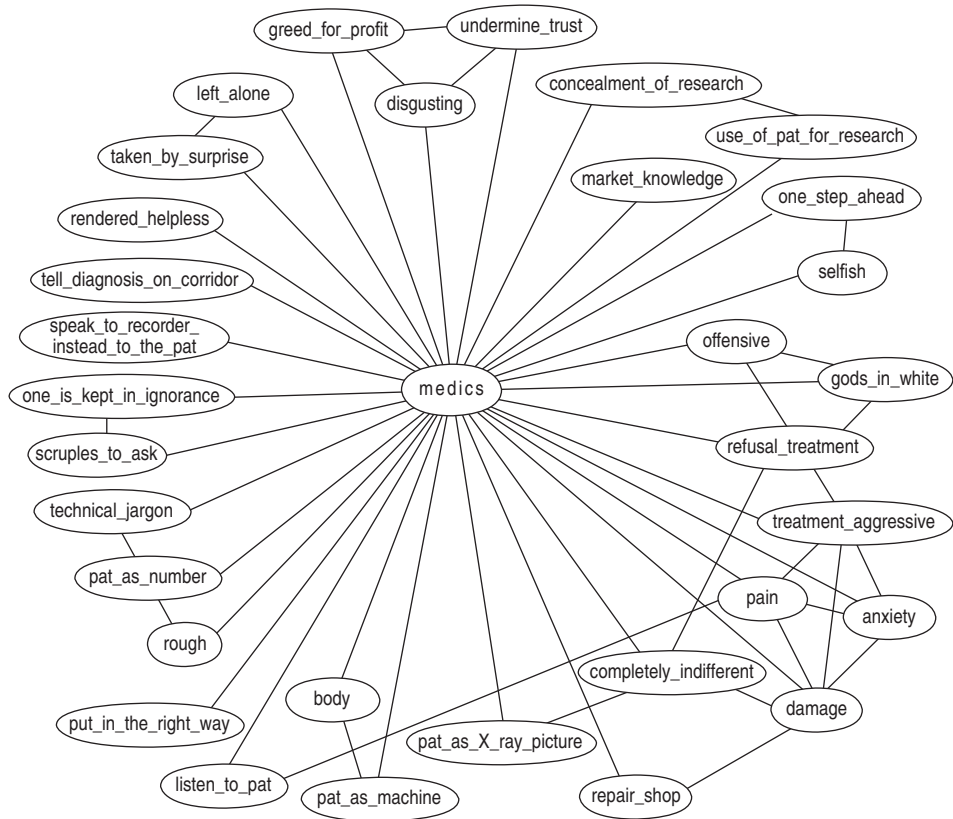


Fig. 22: The emotional graph of Austrian⁷ patients in the perspective of medics. The figure shows topics which ordinarily are not discussed in an explicit way but can still be detected by a deep analysis of the texts.

In order to compare the results of the emotional graph (figure 22) with the corresponding association graph, we display this in figure 23. Here only those terms are shown which are connected with “medics” at least 5 times.

We see that the graph of figure 22 has no one term in common with that of figure 23. In the emotional graph (figure 22) regarding “medics” we find groups of interconnected emotionally loaded terms which express much more negative than positive emotions.

We assume that the emotional feelings of the respondents are also perceived in ordinary communication although not fully consciously. For problem solving this is

⁷ The emotional analysis must be executed using the authentic original text. Now I don't understand Dutch and the English translation of the Dutch texts would change too much of the emotional content. Therefore the example of fig. 22 is taken from the Austrian part of the project where interviews were conducted in German. Only after the emotional analysis were the concepts translated into English.

relevant due to the motivating role of emotions. They move to reactions, to measures and action. Therefore, in judging the relevance of proposals, not only the cognitive perspective should be taken into account but also the emotional one.

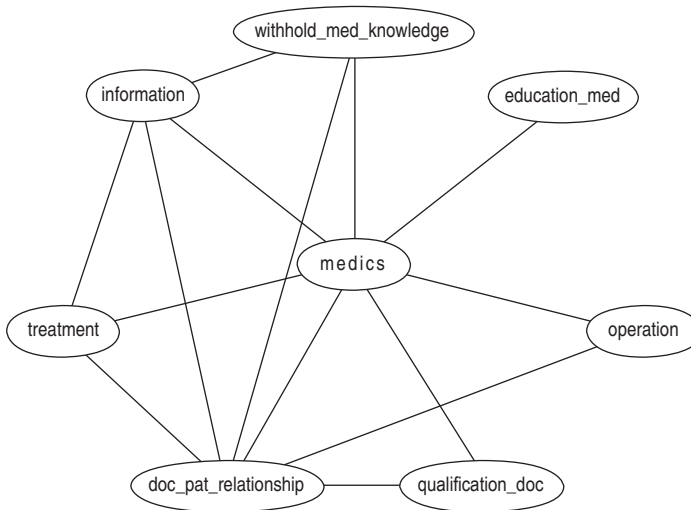


Fig. 23: Association graph of Austrian patients for medics. The graph shows the topics which are explicitly discussed by many respondents.

11 REPRESENTATION OF STATISTICAL FREQUENCIES

GABEK distinguishes two basic ways of coding: apart from the object-linguistic coding mentioned above, through which the lexically relevant terms are identified, there is the possibility of meta-linguistic coding (as used in other qualitative data analysis software systems). The latter clearly stands out against object-linguistic coding, since relevant criteria or categories which do not have to occur in the text are assigned to the text by the researcher. These can be:

1. Text-related criteria: These are categories to which the contents of a text unit are attributed. Examples of text-related criteria could be: arguments in favor of a measure, arguments against it, economic perspectives, technical features, definitions, theoretical concepts, examples, individual cases, etc.
2. Names of individuals: These refer to the individuals who formulated the text unit (generally anonymously) and were interviewed.
3. Criteria related to individuals: Finally there is the possibility of assigning features to the individuals, e.g. age, sex, education, vocational qualification, etc.

Thus sections of verbal data can be selected or combined arbitrarily according to the criteria – be it raw data or data already processed – and statistics can be produced.

The meta-linguistic coding of all text units permits in the subprogram *Statistics* Σ a direct analysis of the frequencies for all pairs of criteria or for any combinations of criteria, either in absolute values or in percents. The frequencies can refer to persons or

records. In the following illustration 24 we refer the specification to persons in absolute values. For this we load the merged document of patients and experts. In this case we cannot find interesting differences which would suggest analyzing parts of the data separately, as there are no interesting criteria to apply.

In other cases, by representing statistical frequencies WinRelan offers an instrument which supports the analysis of different verbal data separately or as a whole.

Criterion	Sum	expert	layman	male	female	20 - 30	31 - 40	41 - 50	51 - 60	61 -	Ne [^]
expert	20	20	0	9	11	0	0	0	0	0	
layman	20	0	20	10	10	4	2	8	6	0	
male	19	9	10	19	0	2	1	4	3	0	
female	21	11	10	0	21	2	1	4	3	0	
20 - 30	4	0	4	2	2	4	0	0	0	0	
31 - 40	2	0	2	1	1	0	2	0	0	0	
41 - 50	8	0	8	4	4	0	0	8	0	0	
51 - 60	6	0	6	3	3	0	0	0	6	0	
61 - and older	0										
Netherlands	40	20	20	19	21	4	2	8	6	0	
A	1	0	1	1	0	0	0	1	0	0	
B	1	0	1	1	0	0	0	1	0	0	
C	1	0	1	1	0	0	0	1	0	0	
D	1	0	1	1	0	0	0	1	0	0	
E	1	0	1	1	0	0	0	0	1	0	
F	1	0	1	1	0	0	0	0	1	0	
G	1	0	1	1	0	1	0	0	0	0	

Fig. 24: Statistical frequency specifications concerning persons in absolute values. This refers to the merged documents of patients and experts.

12 THE SIMULATION OF DIALOGUES

Of course different groups of individuals can hold very different opinions. We will now see how these opinions of opposite groups can be compared.

The sub-program *project comparison* presupposes that a database or several verbal data records have already been analyzed in the form of *gestalten trees*. If this has been done, the program can serve many different functions. It can be applied to look up texts that are *comparable, similar, opposite or contradictory to a determined text unit*. It can be used as a *learning tool, i.e. a tutorial*, for example, when a young employee is to be introduced quickly and effectively to habits, experiences, or the linguistic usage of an enterprise or of a branch far away from the headquarters. The program is also suitable for the *introduction to a complex theory* of the humanities or social studies, or it can be used as a program for the innovative and unusual *linkage of ideas*. However,

the *simulation of dialogues* and the associated management of *conflicts between groups* is intended as the core application of the program *project comparison*. (For the theory of the dialogue see Zelger 2000b).

For this we load the projects of the conflicting groups A and B next to each other into the sub-program “project comparison.” For each project three fields are available on the display. In the first field the gestalten tree emerges, which can be more or less unfolded. It covers the whole knowledge that is available about the group A, in the sense of “tacit knowledge.” If one opens the gestalten tree and selects a text – on any level of the gestalten-tree – the key terms of the selected text appear in a second field. They represent the “focus of our attention,” that is, our conscious contents at the given point in time. Simultaneously, the appropriate text, which contains the key terms, appears in the third field as a product of the basic knowledge (tacit knowledge) and of the (conscious) key terms.

If we want to react to a preceding text of group A with a text of group B, this cannot take place arbitrarily. Rather, a response should contain some key terms that were used in the text of A. Therefore, we define the “area of possible linguistic reactions in relation to a preceding text” as that part of a gestalten tree in which only texts occur that have at least one term in common with the preceding statement.

A small dialogue between patients (group A on the left side) and experts (group B on the right) runs as follows:

Group A: patients

interference_with_nature

An important ethical problem in medical science are the ethical limits to interfering in natural development and its reproduction, which will become a bigger problem in the future. Modern technology replaces nature, for example the genetic manipulation of embryos (deciding of their characteristics), of human beings or of food. The question is how far man can interfere with life and nature (to not destroy the balance in nature).

perfect_man

One of the ethical problems in the future regarding genetic manipulation is cloning. People are trying to be smarter than God and they'll try to make their image more beautiful than it already is – a perfect man.

Group B: experts

U85

I think there will be [ethical] problems [in the future] regarding reproduction; the selection of genetic material to bring people into the world with as few deficiencies as possible or reproduction totally without the need for a man or woman; turning genetic material into people. That would be, I would think, an unethical use of genetic material [because] I think that is too much an interference with nature.

Genetic_Engineering

An important ethical problem in the future in medical science, through new technologies and genetic engineering, will be cloning (for example the cloning and implanting of embryonic cells or the reproduction of your own cells) of human beings. The question is if this will be an unethical use of genetic material or not.

The dialogue – produced almost automatically – begins with the summary “interference with nature” of group A. As a first answer an original response U85 of an expert is found in the “area of possible reactions in relation to the preceding text” of group B. Then the dialogue continues by gestalten of the groups A and B. The connection of the four texts as in figure 25 shows that the connection of the four texts fulfils the rules of our chapter 5.

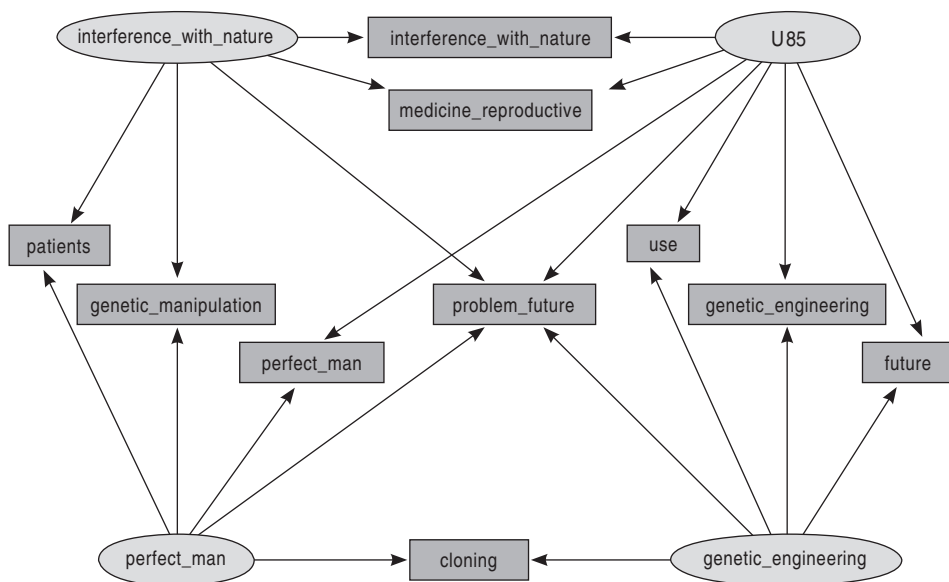


Fig. 25: The connection of the simulated dialogue with four statements builds a new linguistic gestalt.

According to our rules of chapter 5 we form the summary:

For the future ethical problems are expected as effects of genetic engineering and cloning, where reproductive techniques of medicine are used to create the perfect man. This will interfere with natural development and is assessed as an unethical use of the genetic material of patients.

The summary together with the four texts result in a new linguistic gestalt.

The summary of the text units connected to a gestalt as result of the dialogue is then the content which was learned by the two dialogue partners. I believe that dialogue is always the basic form of learning. The software WinRelan GABEK offers a procedure by which it is possible to simulate dialogues also for people who cannot meet each other or who are not prepared to come together. GABEK can be seen as an instrument to initiate real dialogues too.

As experience shows, even in opposing groups there are often contents on which they agree. This is emphasized when striving for conflict resolution. Such discussions can be summarized as a consensual result according to the rules of the linguistic gestalt formation [Zelger 1999a; 1999b].

Basically, when there are conflicts between two groups, each group will at first be presented with their own results. In a second step one will demonstrate the points of agreement of both parties. It is not until later that one would also try to show each party the opinions and arguments of the other party, so that they learn to understand each other mutually. Such procedures have already contributed to conflict resolutions in difficult situations [examples see in Pothas and De Wet 1999; Zelger 1999c].

13 CONCLUSION

In conclusion I would like to cite a few sample projects to indicate the variety of applications of GABEK.

GABEK has been used for evaluation purposes (of the Italian Ground School Reform in South Tyrol), for quality control (in a hospital), for product evaluation (of vehicles), conflict resolution (in South African Industries); furthermore there have been projects on goal development (of a university), minority research (in South Tyrol) and town research (Tepito in Mexico City). Acceptance research in Georgia, Holland and Germany, intercultural management in Thailand, China, Brazil, Europe and market research in South Africa and Austria have also been amongst the standard applications. Projects on organizational and staff development and innovation research are also in the core research area of GABEK. Finally, there were a number of projects on medical ethics, clinical psychology, sports medicine, school didactics for different fields, personality psychology and research on marginal groups.

In summary, we recommend that in complex problem situations one does not proceed by deciding on certain suggestions but rather uses the recent experiences of all the colleagues and of those affected. Only after this step should experts develop the problem. Planning thus becomes easier. Due to the simple method of obtaining information (some open-ended questions) and the relatively few hours of expert advice, the expenses incurred with GABEK are less than those of conventional methods. The great advantage lies not only in the holistic and more comprehensive view of the situation and the more realistic evaluation of the consequences, but also that those affected are included in the planning process, whether they are municipalities, offices or people in business. The possibility of introducing authentic opinions is taken seriously. A plan is justified by the authentic quotations of colleagues and others affected. Many individuals are able to identify with the plans and are therefore ready to support their realization.

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FOSTERING HUMAN DIGNITY: SOME INHERENT ETHICAL DIMENSIONS OF QUALITATIVE RESEARCH

WILL C. VAN DEN HOONAARD

There has been an explosion of interest in “ethics” worldwide. In Canada alone, for example, there were 5,000 ethics meetings and conferences in 1999, compared to 100 in 1994 [Curtis 1999: D1]. Similarly, there has been a staggering rise in the number of ethicists, ethics companies, ethics policies in business, administration, sports, and science. Publicly funded research has not escaped the fallout of interest in ethics codes. Ethical review of all research, including social-science research, has become the norm in North America and is making its way to Europe. The British Isles have already adopted stringent ethical review procedures for research. No fair-minded researcher would (or could) resist any ethical review of his or her proposal. As is the case with the implementation of any social policy, there are unintended consequences, which often affect the most vulnerable groups on hand. This paper suggests that qualitative researchers have had to bear the brunt of the unintended consequences of ethical review, although the social sciences, in general, also seem to have suffered when coming to terms with the new ethical codes.

Rather than directly addressing the general failure of the new ethical codes to consider proposals with a qualitative-research dimension, this paper hopes to explore some inherent ethical dimensions of qualitative research as a preserver of human dignity. This paper considers the historical source of ethical review, identifies the problematic nature of ethical review (as it currently stands), and demonstrates some of the unique ways that qualitative research, by its nature, promotes human honor and dignity.¹ In the process of explaining the ethical components of qualitative research, I hope to underscore some of the chief characteristics, and strengths, of qualitative research. Qualitative research adumbrates a large variety of methodologies. It is typically associated with participant observation and interviews, but also includes textual analysis, conversational analysis, visual sociology, and so on. It is also known by such labels as naturalistic research, inductive research, field research, or case-study research. As someone who has conducted extensive field research and a large number of interviews, I find Ted Palys’ summary definition of qualitative research [1997: 46] very cogent. He avers that qualitative research hopes to “engage a phenomenon of interest on *its own terms* and to let theory emerge from the data (*italics mine*).” This is the crucial definition of qualitative research that sets it apart from deductive or quantitative research. It is research that is centered on the meanings and social constructs of the research participants themselves. At the outset, then, qualitative research has

¹ More recently, Eliot Liebow’s work [1993] already a celebrated classic in field research, exemplifies the highest ethical standards of research.

the potential to preserve and foster human dignity as an essential aspect of ethical research.

The social-science community has long been interested in research ethics, especially on account of a number of high-profile cases of breaches in ethics, such as experiments on black men with syphilis in Tuskegee, Alabama [Barber 1976], on unsuspecting laboratory participants by Stanley Milgram [1963], and the study of a doomsday cult [Festinger et al. 1956]. As a consequence, there is a sizeable body of literature that either looks at the ethical dynamics of these specific cases or offers general introspection regarding research ethics in psychology [e.g. Stanley et al. 1996], in the social sciences [e.g. Homan 1991], in history [e.g. Levin 1995], in educational research [e.g. Burgess 1989], and in anthropology [e.g. Punch 1986; Bulmer 1982], to name but a few.

What is missing from the critical literature, are the situational experiences of members of research-ethics boards (hereinafter REB) as they evaluate and come to terms with qualitative research proposals and qualitative researchers. There is no major empirical research study published that details the dynamics between REBs and researchers. The few instances that speak of experiences with REBs are buried in ethnographies [e.g. Flowers 1998] and knowledge of this hidden literature is only spread by word-of-mouth. As developments in the field of research ethics are so recent, there is naturally very little in the literature, except for W.C. van den Hoonaard [2001; 2002], the Centre on Governance, University of Ottawa [2000], L'Institut National de Recherche Sociale [2000], Shea [2000], and the American Association of University Professors [2001].

The research shows that because the new formal ethical review is based on the principles and epistemology of deductive research, the review process erodes the thrust and purpose of qualitative research ...

THE HISTORICAL BACKGROUND OF FORMAL ETHICAL REVIEW OF RESEARCH

The ethical review of scientific research emerged from the aftermath of the horrors of World War II when medical experiments furthered lugubrious social aims. The Nuremberg Code tried to forestall the use of such experiments by codifying viable, ethical guidelines for medical and scientific research. While the Code remained an ethics watershed, scientific research would take at least another 30 years to catch up to the Nuremberg principles. As scientific experimentation with human subjects was thought to be directed towards more noble goals, such experiments ranged from exposing soldiers to atomic blasts [Welsome 1999], imposing medical procedures on prisoners or forensic in-patients [Regehr 2000] and experimentation with drugs, whether LSD, DepoProvera, or drugs near market entry.

When the latent effects of these experiments became manifest, with the rise in lawsuits, the public and government became acutely aware of the ethical implications of medical research and, indeed, of all research. Ethical guidelines became explicit, first through professional societies and then, more vigorously, through the State. The American Sociological Association, for example, introduced its ethics code in 1971 [Friedrichs

1983] followed by the establishment of Institutional Review Boards (IRBs) in 1981 with the so-called “45 CFR 46” rule [Shea 2000].

During the latter years of this evolution of ethical norming, i.e. in the early 1990s, the Medical Council of Canada, the Natural Science and Engineering Council of Canada, and the Social Sciences and Humanities Research Council of Canada initiated the process of developing the *Tri-Council Policy on Ethics Involving Human Subjects* (“the *Policy*”). The first draft of the *Policy* was issued in 1994. It was revised and reissued in 1998. The three Councils quickly adopted it later that year. Universities now also use the *Policy* to cover non-funded research, including by undergraduates in some cases. The *Policy* came into effect in 2000.

There are no simple explanations for the rise and seeming permanence of ethics codes for research as they become anchored in the life of research institutions. Aside from the role of the Nuremberg trials in planting the roots of ethical review in the social sciences, one must also consider other social forces that have sustained the contemporary movement of the ethical review of research, namely the rise of a litigious society, the prevalence of the “culture of fear,” the fetish of individualism, the loss of professional autonomy, or the use of social forces as an instrument of control.

First, according to Jenefer Curtis, it is the decline of religion and the rise of “intense individualism” that have created a moral vacuum which “ethics” is now trying to fill. “Ethics” has replaced “morality” [Curtis 1999]. One consequence of the rise of individualism and the decline of the sense of community is that a “culture of fear” now animates social relations [Furedi 1997]. This culture of wariness of strangers, notably of researchers, reinforced the development of research-ethics review. As a member of an ethics review board stated to me: “Today’s academic research environment, with its public (and non-public) scandals, provides a strong incentive toward regulation and monitoring ...” [Scott 2000].

Second, the centralization of functions by the State [Nelkin 1982], coupled with legal recognition and grants-in-aid for professional associations, is exchanged for the loss of professional autonomy. The loss of professional autonomy has abetted this process, replacing in-house professional codes of ethics with externally derived codes of a universal nature.

Finally, increasing interdisciplinary interaction has resulted in the dominance of one or more disciplines. Some scholars [e.g. Altheide 2001] aver that ethical review is a means of exercising control, while others [such as van den Hoonaard 2001] see ethical review as the unintended control of qualitative research by quantitative-research approaches. Drawing most heavily on evidence in Canada, the United States, and England, in the fields of anthropology, education, nursing, psychology, and sociology, I have found that moral panic occurs throughout the research-ethics review process, which situates the deductive model of research in the normative position, proclaiming the rest as non-normative.

Whatever the social force that sustains formal ethical reviews, it is clear that ethical review bears directly on the research lives of the many researchers who must submit stronger ethical testimonials of their plans.

THE “QUIET CRISIS” IN ETHICAL REVIEW

Despite the *Policy's* comprehensive guidelines, researchers have experienced serious problems in accommodating their research to the codes [e.g. Pool 1998]. For example, on 8 March 2001, the Canadian Sociology and Anthropology Association embarked on a *Survey on Ethics in Sociological and Anthropological Research* as a result of the quiet crisis that is affecting their scholars when attempting to follow the Tri-Council ethical policies. To be sure, many of these problems were nearly the same as under previous ethics regimes, and Herbert Kelman [1968] had already sparked a debate on such in the United States. But the *Policy* has a stronger, public mandate to oversee research than earlier policies. As such, the problems have become more critical for researchers before they start conducting their research. Canadian researchers are not alone; in the United States and in England social researchers are facing the same obstacles in their attempts to secure ethical approval [Friedrichs 1983; Vidich et al. 1981].

This quiet crisis has a direct bearing on the way social researchers conduct their research, as the following examples illustrate. There are practical limitations regarding ethical review. There is, first of all, the matter of having to promise confidentiality without legal recourse that would ensure maintenance of it [Comarow 1993]. Social researchers still cannot count on REBs for legal defense, as shown by the difficulties experienced by a Ph.D. student at Simon Fraser University (Vancouver, BC, Canada) when asked by police to surrender confidential data [Jones 1998].

Second, one may also wonder about the ethical problematic of *quantitative* researchers, i.e. regarding REBs vetting qualitative research applications using *quantitative* research as the norm. Murray L. Wax alludes to the “gravest ethical problem” which is “posed by unknowing and overzealous IRBs, and by governmental regulators attempting to force qualitative ethnographic studies into a biomedical world” [cited by Shea 2000]. Evaluations of the ethical components of research increasingly include consideration of the research methodology itself from the perspective of the dominant quantitative-research paradigm (such as when “validity” is invoked) [Janet Stoppard, p.c., 1 Oct. 2000]. This new step is akin to the change of name and function of the American Psychological Association Committee for the “*Protection of Human Participants in Research*” to the Committee on “*Standards in Research*” [Rosnow et al. 1993]. Ethical review is moving away from purely ethical consideration of research projects to assessing the suitability of the *method itself*.

Third, ethical infractions are on-going and are going underground [Homan 1991]. As rules proliferate, so do loopholes, resulting in a creeping cynicism about research-ethics review. One researcher has redefined himself as a “conceptual artist” in order to avoid going through Institutional Review Boards in the United States or Research Ethics Boards in Canada [Shea 2000].

Preserving Human Dignity

Human dignity in research-ethics codes is a “moral imperative” that should underlie all research (see the *Policy*, “Context of an Ethics Framework”). In considering the distinctive features of qualitative methods, what is quite obvious is the act of preserving the

human dignity of the research participants. The following discussion highlights some of the essential elements in qualitative research that contribute to the dignity of preserving human dignity, namely, the prevalence of formulating the research questions *after* the research has begun, the nature of qualitative research as a low-risk endeavor, keeping the research participants in touch with the research, the creative use of pseudonyms, the matter of consent forms, and the nature of “voice” in qualitative research.

Formulating the research questions *after* the research has begun

There is an *over*-emphasis on research dimensions that seem inappropriate to qualitative researchers, such as knowing well in advance of the research the questions that will be raised in the course of interviews or field research [Irvine 1998]. A qualitative researcher *does not know in advance* the full range of data he or she will collect [Becker 1996] and cannot presume in advance what areas are significant for the research. After all, the lives of many sociologists, if I take my own as an example, are fairly circumscribed. The range of our knowledge is limited by our experience in life, in addition to gender, class, or race. In an effort to forestall inventing the motives of our research participants (e.g. “why did he drop out of school?”) we need a methodology that leaves questions quite open because we just do not know the social world of the research participants we are stepping into. This epistemological stance is reflected in a number of techniques that escape the attention of research-ethics boards. First, the interview questions are left relatively open, to allow the research participant to dictate aspects that he or she finds important or worthwhile. Second, the interview guide can change with each subsequent interview as themes or topics emerge or prove themselves not conducive to the research. Such an approach differs from standard question-and-response questionnaires used by quantitative researchers who find it important to maintain the format from beginning to end, enhancing the reliability of the research.² Third, as much as possible, we avoid advance knowledge obtained in the “literature,” because we want to emphasize accuracy (again, not “invent” the motives of the social actors). As I communicated at a Slovene qualitative-research conference:

Moreover, a qualitative researcher, by habit, tends to conduct a review of the literature towards the end of the data-gathering stage; this treatment of the literature ensures a broader scope of discovery. This highly distinctive element of qualitative research poses the greatest challenge to those who use quantitative techniques. The practice of abandoning formulaic reviews of the literature before the research is begun seems to fly in the face of common sense in research. [van den Hoonaard 2000: 13]

If seeking out deviant, or negative cases [Becker 1998: 208] is part of the qualitative research process, we cannot know of these instances in advance either.

² In this vein, researchers often refer to the inclusion and analysis of answers to open-ended questions as “qualitative research.” A qualitative researcher would cringe at such a reference.

Maintaining a low level of risk

Qualitative research, unlike some medical research, does not usually involve risk to the research participants. Many researchers are painfully aware that there might be many unintended results of one's research, but they also know that one cannot always know those results. While there is no perfect foresight in that regard, risks might be exaggerated. Even interviewing people who have experienced personal traumas has a salubrious, rather than negative, effect. It is widely known that such interview participants have benefited greatly from in-depth interviews. In a study on teenage girls, for example, whereby the research topic invited disclosure of experiences "that require[d] professional follow-up or intervention," the researchers found that "the adolescent girls appreciated the opportunity to discuss their experiences and concerns with interested adults who inhabit a world outside their everyday realm of parents and school" [Anonymous C 2000: 6]. Social researchers like these, delving into emotive topics, have not found interviews cascading into the sort of troubled waters that require work by therapists. Still, it would be helpful to have phone numbers or persons available, but the premise should be that professional interviews are, for the most part, quite trouble-free.

The exaggeration by REBs of the risk of social research has led to unnecessary problems in social interactions with research participants [Kelman 1968]. Such highly-charged statements to the effect that "locked" cabinets will be used to store the gathered data, and that tapes will be "destroyed," for example, might create an unwarranted aura of secrecy and risk that heightens fear and the perception of risk and danger, despite the fact that many interviews employing a qualitative approach usually take the form of "conversations." Such terms are usually embedded in consent forms and make for uneasy conversations and interviews even in otherwise harmless research endeavors.

In any case, the risk of qualitative research is also greatly muted by the fact that researchers use no more than 10–20% of transcribed text, whether from fieldnotes or participant interviews, if that much. However, the researcher should pay particular attention to the inadvertent use of distinctive expressions of an interview participant that might identify him or her among members of the researched community.

Keeping the research participants in touch with the research

Born of the positivistic model in research-ethics review, review boards are inclined to suggest two approaches that keep the research "subject" in the loop of the research. First, it is sometimes suggested that the researcher return the transcribed interview text to the research participant. This approach, they argue, has the benefit of the research participant approving what has been written on their behalf. Conceptually, this approach has its seeming strength as it empowers the research participant with a say in their own knowledge production. In practical terms, it has grave limitations. Beyond the extraordinary budgetary considerations, there is always the challenge of interview participants coming face to face with their own spontaneous words but now transcribed and available in written form. Ungrammatical speech, locutions, and verbal penchants abound in our everyday language; when these conventions are available in script, we

become more painfully aware of them. If the transcription is returned to the research participant, the participant may, unnecessarily, feel inclined to deny the researcher the right to use the text. As any experienced researcher knows, the final text that appears in a publication is usually edited and although a researcher may want to retain, as much as possible, the original flavor of the quote, he or she is also committed to presenting the quotes in a “cleaned-up” version.

The other suggestion involves the researcher being asked to offer the research participant a summary of his or her findings at the end of the research process. If the research participant is not regularly brought into the research process, the receipt of a summary of the research findings may prove rather disembodied. Some research participants may then realize the larger context in which his or her words appear.

There is, however, a third approach which many qualitative researchers find very beneficial. He or she can augment the empirical and analytical rigor of the findings by sharing the relevant draft portion of any publishable text with the interview participant during the analytical stage of the research, both to forestall misquotes and the like, but, more importantly, to engage the interview participant in an on-going analysis of the researcher’s own findings. If the research participant chooses to offer more information or to correct a factual finding, the researcher can incorporate the “feedback” either into the main text or into the endnotes. The overall subtlety and comprehensiveness of the research will be heightened through this courteous, yet pragmatic approach to research.

The only drawback of this approach are the extra costs associated with the photocopying and mailing out of the relevant portions of the text — something that some qualitative researchers often do not consider when planning the research budget.

Creatively using pseudonyms

The rapid adoption of the use of pseudonyms suggests that there is wide agreement on this issue. For those who undertake qualitative research, the issue of pseudonyms is much more subtle. While currently working on a new book dealing with gender and cartography [van den Hoonaard, forthcoming], for example, I have found it enormously useful to retain the original interview participant’s name throughout the transcription and analysis of the interview tapes. The long-term retention of the original name allows me to behold the whole person’s identity in my mind, facilitating my research and offering a better representation of that person’s voice. As soon as I converted the name into a pseudonym, I lost the “sense of the person.” There are still other dimensions that weigh on the matter of pseudonyms.

Some researchers are of the opinion that in replacing bona fide names with pseudonyms (even though some interview participants have agreed that their real name be used), as mandated by ethical review, they are disempowering the interview participants, taking away their “voice.” Some interview participants are quite explicit about wanting the researcher to mention their names.

If the researcher does choose to use pseudonyms, he or she can either ask the interview participant to select a pseudonym as a sign of courtesy and respect, or the researcher can select one on his or her own. In the former case, the interview participant

often selects a name that he or she wished she had or a name that, for example, comes from a favorite village. In the latter case, the researcher must consider an appropriate or relevant name. For example, in one of my research projects I had to make sure that the pseudonym the interview participant allowed me to choose would be one that was prevalent when she was born 71 years ago. A researcher may also select a pseudonym as a mnemonic device, such as in the case of an interview participant from an area where President Roosevelt used to live and naming that interview participant “Eleanor,” after the President’s wife.

Figuring out the onus with consent forms

The use of signed consent forms is one of the most highly contested areas from the perspective of qualitative researchers. Signed consent forms, some researchers claim, have become a “fetish” [Bradburn, cited by Shea 2000]. It might be easy for Ethics Committees to stipulate the requirement of using the signed consent form in all research involving interview participants, but the situation is far more complex and requires a lengthy and detailed understanding of the ethnographic field research method.

The idea of informed consent of the individual, which R. Charbonneau [1984: 20] describes as the “cornerstone of all Western ethical codes,” is extremely difficult to implement when studying groups living on the margins of society. Aside from problems of illiteracy [Charbonneau 1984: 21], the difficulties multiply as one investigates not only those whose lives are touched by social problems, but also areas of secrecy, whether of marginal or even elite groups. It is ironic that just as society is devoting more resources to “applied” research, the legal emphasis on signed consent is undermining the study of social problems.

The difficulty is heightened when both the advantages and risks are hard to assess and where potential participants must “have complete knowledge (to give informed consent)” [O’Neill 2000]. An insistence on using such forms may, in some cases, destroy natural anonymity. Whereas before it would have been possible to retain the full anonymity of those being interviewed, it is now necessary to acquire a name and signature. One can imagine many instances where the insistence on a signed consent form may be unwise or tactless. Studies of street-corner men, poachers, hookers, fishermen, drug users, professional thieves, the homeless, and, in general, the socially defined problems of society, would simply elicit an angry response. The difficulty even persists in obtaining a signature from those who occupy positions of power and prestige.

Ethnographic research, in the light of its purpose, is conducted from the point of view of “participant observation.” A researcher of this methodological tradition endeavors to understand the whole sub-culture surrounding the so-called “subjects.” To achieve such an understanding, the researcher endeavors to leave the culture intact. This involves a process whereby the researcher respects that culture with the least possible intrusion. The researcher must be careful not to aggressively assert his or her rights or views. The introduction of a signed consent form violates the very essence of ethnographic research, because human “subjects” would regard the requirement of such a form as *coercive*. The inappropriateness of using a signed form of consent is further highlighted by the fact that ethnographic research does not entail experimentation.

Such experimentation could, for example, involve the placement of “surface electrodes,”³ or, not infrequently, hidden tapes, whereby subjects are told one thing, and the desired research plans are hidden from the persons under study. This approach to research is quite alien to ethnographic research. In the case of interviews, the datum rests within those interviews, consistent with the purpose of the research being made clear to interview participants.⁴

In the experience of qualitative researchers, the requirement of using signed consent forms may paradoxically take away the right of an individual to participate in research. A colleague/informant at one of Fredericton’s universities, although eager to participate in a research project, would have been effectively barred because she refused to sign such a form even though the researcher had closely followed the approved procedure mandated by her ethics committee [Anonymous D 2000]. Here is an account of a nurse/sociologist who has taken on the task of interviewing people in home-care situations:

I gave each of them [caregiver and the older person] a copy of the consent form and asked them to review it and then I would answer whatever questions they had. Both Mrs. McK [caregiver] and George [older person] read it carefully (took time).

George says that he has a question for me about the form ... He looks at me rather thoughtfully and asks, “What difference will it make whether I sign it or not?” Thinking he might be having second thoughts about my being there for the study, I reminded him that he could ask me to leave his home if he didn’t want to participate. “No, you don’t understand my question,” he replies. “Will you do anything differently here this morning if I sign the form than if I don’t sign it?” I can see that he is not concerned about my being there but seems to be questioning the need for his signature. I’ve just driven for 45–50 minutes to be at this home. If he’s not objecting to my being there and knows why I am there, would I actually have to leave if he doesn’t sign? These thoughts are racing through my head. While I’m thinking about this, George leans over and signs the consent. I still have another form for George to sign, the consent for the follow-up interview with the family caregiver. [Anonymous E 2000]

Some of the solutions proposed by ethics committees are just plain silly, but must be followed, nevertheless. For example, a member of a departmental ethics committee told a graduate student to turn her face the other way when she was doing participant observation in a group which had at least one human subject who had not explicitly given consent to the research. It is hard to know what to do under those circumstances. Such circumspect behavior creates more problems than warranted and casts doubt among the subjects about their own behaviors and beliefs.

The researcher develops a “natural bond with the interview participant: in all likelihood, the interview participant has been previously aware of the presence of the re-

³ “Consent Form – Digital Myoelectric Controls Study,” provided to the author by the Ethics Review Committee of UNB, 30 October 1989.

⁴ One researcher [Anonymous D 2000] who conducted research in a retirement community in Florida just could not visualize pulling out a consent form from her bathing suit as she was chatting with community residents while standing around in the pool.

searcher, and a fair amount of common stock of knowledge is available about each other — not unlike any real-life situation. Reflecting on my own 30-year research experience, it is indeed difficult to imagine my relationship to the 200 or so interview participants without such a bond. Invariably, such ties either evolved out of, or became, natural friendships or relationships involving trust. The use of a signed consent form could break such a bond. Other interview participants regard them as a disloyal act on the part of the researcher. This dilemma occurs even though one is known as a researcher, a fact that is stated in advance of any research project.

This vexing problem was brought to my attention recently by a graduate student doing fieldwork in Nunavut. She had obtained ethics approval from her department and her proposal was approved by the Nunavut Council on Research. Both groups insisted she use consent forms, which “seemed to ferment the stereotype of a researcher from outside as self-centered” [Anonymous A 1999]. In addition to having a signed consent form and a mission statement, the student had to repeat the process for the interpreter, who also had to be provided with an “Interpreter Confidentiality” form. Each sheet was not only drawn up in two languages, but copies were made for all parties involved, including research agencies — a total of 22 sheets of paper had to be available and signed before the interview could begin. In the words of the researcher:

The woman looked at all of the papers, looked at the consent form, and refused to sign it, stating that her daughter had told her not to sign anything. I had to respect that ... I wondered just how different the interview might have gone had I not felt the need to be so formal with her. When I left she refused to even keep the (unsigned) participant consent form.

Every other interview started with a confusing first five minutes and the interviewees would say, “whatever,” whenever she would explain why she had all of these forms. The researcher concluded that the “consent forms were obtrusive and established an atmosphere of formality and mistrust.” [Anonymous A 1999]

The use of consent forms signed by interview participants becomes particularly problematic when engaged in research on sensitive topics. Grace Getty who researches men who have sex with men reports that that she has devised a method of allowing interview participants to sign the form with a code that they had made up. She reports that “few of them would have been willing to have their name attached even to a consent form that was kept separate from the tapes, etc.” [Getty 1999]. Obtaining signed consent forms are also problematic in other ways. As Felice J. Levine points out in the case of research on children:

parents often fail to sign and return written consent forms, not because they object to their children’s participation in research, but simply because they do not have the time or take the time to read the forms and sign them. [Levine 1995: 81]⁵

⁵ The United States House of Representatives passed in April 1995 the Family Privacy Protection Act, which requires that in all cases parents must give written consent before their children can participate in research [Levine 1995: 81].

Each year, qualitative researchers bring similarly discouraging stories to the attention of their colleagues. In 1999, the issue became so compelling that qualitative researchers organized the impromptu session referred to above. One Master's Degree student, in correspondence, described the process of ethical review as "HORRIBLE, ridiculous (her emphasis)" [Anonymous B 2000]. She further explained that the person "in charge of approving or disapproving [research applications] knows nothing about social-science research."

If signed consent forms create confusion on the part of the ethnographer, what could one say about the interview participants? Would not such forms be interpreted as a form of waiving control? Might not the interview participants guess that such forms are, after all, designed to protect a research-grant awarding body or some other institution, rather than them? Would not such an interpretation promote cynicism? How could one, moreover, be apprised of all the risks involved in research? Remember that ethnographic research does not resort to experimental procedures where the effects may include physical or mental pain. It would be a gross overstatement of fact to claim to know, on the part of both the interview participant and the interviewer what those risks are.⁶ Both parties make an educated guess what those risks are — and both are free to enter into the interview situation; no more can be expected under such circumstances.

From the perspective of ethnographers or those who partially employ the ethnographic approach, mandated signed consent forms interfere with academic freedom. While research-ethics committees insist on the use of such forms, the ethnographer will be precluded from practicing his or her craft. Research funds will not be forthcoming and such research may have to move away from the protection of the university community. Ethnographers will have to practice their craft on a personal basis, leading to other sets of problems. When a research participant refuses to sign a consent form (but still wishes to be interviewed), do the ethical codes take away that participant's right to participate in research?

Struggling with the issue of "voice"

Increasingly, social researchers have come to understand the unmitigated nature of deductive approaches because in recording their findings, deductive researchers amplify their own voice, rather than that of the research participants. For qualitative researchers, there is a growing recognition of the struggle to find the right place between the autobiography of the researcher and the biography of the research participants [Mills 2000; Chahal 2000]. How do we represent the voices of research participants in our writing? As Hertz has noted, we can find this representation along a continuum: from a scholar's "code language to demonstrate the author's theoretical alliances," to having the research participants as viable co-authors [Hertz 1996: 7]. In bald terms, do we speak for ourselves (as researchers) or do we let our research participants speak? As Gretchen Rossman and Sharon Rallis [1998: 199] aver, the "author has to be vigilant that

⁶ This point, of course, does not apply to illegal activities which pose their own problems as regards having interview participants sign a written form of consent.

she is accurately and ethically representing the voice she has chosen.” In following this particular element of qualitative research, the researcher can attain the preservation of human dignity — the ethical basis of all research.

CONCLUSION

Although qualitative researchers need to be mindful of the “quiet crisis” that has seized the ethical review of research (in terms of applying the norms of quantitative research to qualitative research), they can take heart in the fact that the intrinsic aspects of qualitative research are about preserving human dignity. It is human dignity that lies at the core of all ethical considerations. This paper outlined a number of these intrinsic features.

First, qualitative researchers are likely to formulate the research question after they have entered the field. Such an approach precludes them from inventing the motives of their research participants and fosters, as well, an openness towards the definitions and meanings that the participants themselves assign to the things they experience, say, and do.

Second, qualitative research usually has a “low-risk” factor. For example, research participants who have undergone traumatic life-changing events have found that qualitative, in-depth interviews have had a therapeutic effect on them (although that was not the intention of the interviews). Qualitative researchers are eager to avoid the use of evocative words to describe their research process (e.g., the avoidance of such words as “destruction” to refer to the disposal of interview tapes).

Third, as qualitative researchers should ideally keep in touch with the research participants throughout the course of their research, they reinforce the element of human dignity in their work. This paper suggests some of the ways researchers can achieve constant interaction with research participants.

Fourth, a perceptive choice of pseudonyms offers qualitative researchers an interesting, and often very effective, means of preserving the human honor of their research participants.

Fifth, the intelligent rejection of the use of signed “consent forms” allows interviews and the like to proceed like any other human interaction in any other social setting. The rejection of the use of such forms allows for a more equitable relationship, de-emphasizing a hierarchical and destabilizing pattern of interaction.

Finally, when qualitative researchers become fluent in struggling with the issue of “voice” in representing their research findings, they will find yet another means of preserving human dignity.

There are, no doubt, other facets of qualitative research that promote human dignity. This paper has enumerated only those which I have experienced directly in my own work. Although ethical review of research should be maintained, it is clear that when research ethics boards become apprised of the nature of qualitative research that inherently fosters human dignity, they are more likely to see themselves as nurturers of research, rather than as controllers. For the qualitative researchers themselves, they will be able to see their way through this “quiet crisis,” knowing that their research has a remarkable potential to maintain the human dignity of their research participants and, ultimately, of themselves, too.

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The instruments of qualitative methodology are certainly one of the 20th century's greatest achievements among the different research methods in the humanities and social sciences, especially among those that deal with the life-world of the other. In this period several important reflections on the forms and limits of research practice took place: reflections on the informant and the images and theories of the world with which he or she lives and co-creates his or her life; reflections on the ways and means of recording and translating statements or field information into a language that is understandable to academic circles; and last but not least, reflections on the researcher's world view with and through which he or she interprets and attempts to understand the culture of the other.

Some of these issues were discussed at the conference on qualitative research (Bled, Slovenia 2000): epistemological questions by Frane Adam, Darka Podmenik, Will C. van den Hoonaard, and Reinhard Sieder; the role of the researcher and his or her "object" was problematized by Tatiana Bajuk Senčar, Jurij Fikfak, and Danielle Egan; Ulrich Oevermann, Axel Fehlhauer, and Detlef Garz discussed the latent structures of meaning and the objective semantic structures of expressive configurations; grounded theory approach was presented by Kaja Kazmierska, Blaž Mesec, and Vanja Huzjan; Susanne Friese reviews the characteristics of the current programs for computer-aided qualitative analysis; Josef Zelger presents his program Gabek Winrelan. The work concludes with a paper on ethical questions by Will C. van den Hoonaard.

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