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**Curiosity and Serendipity  
in Qualitative Research**  
by

Katarina Jacobsson, Kristina Göransson  
& David Wästerfors

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# Qualitative Sociology Review

## Volume IX Issue 2

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**Katarina Jacobsson, Kristina Göransson,  
David Wästerfors**  
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## Introduction to the Special Issue: "Curiosity and Serendipity in Qualitative Research"

**Katarina Jacobsson** is an Associate Professor in Sociology at the School of Social Work, Lund University, Sweden, and vice-chair of Research Network 20 Qualitative Methods. With a general interest in qualitative methodology and sociology of knowledge, her present research project focuses on documenting practises among human service workers. A recent text on methodology – "Interviewees with an agenda. Learning from a «failed» interview" (Jacobsson, Åkerström 2013) – is published in *Qualitative Research*.

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Qualitative research today is firmly established in most social science disciplines. The sheer quantity of published work and "method talk" about qualitative research is impressive, but however precise the articulations of methodological techniques or criteria, they do not seem to suffice. New ways of analyzing, theorizing, and understanding qualitative research also develop from unexpected findings, surprising experiences in the field, or even the subtle metamorphoses of a given research project during its methodological journey. Serendipity, or "happy accidents," is an inevitable aspect of qualitative research, yet seldom discussed. The role and meaning of curiosity and serendipity was highlighted at the ESA RN 20 Midterm Conference on Qualitative Methods at Lund University, Sweden, 20-21 September 2012.<sup>1</sup> Participants were invited to report on, exemplify, discuss, and expand their curiosity and serendipitous findings in relation to a series of well-known methodological and topical themes. Apart from more than twenty-five

<sup>1</sup> The conference was jointly hosted by the School of Social Work and the Department of Sociology. Generous funding was received from: The European Sociological Association; The Swedish Research Council; The City of Lund; The Department of Sociology and the School of Social Work, Lund University; The Faculty of Social Sciences, Lund University.

parallel sessions, the conference program included a number of keynote speeches by internationally renowned scholars in the field of qualitative research. Many conference participants have asked for the speeches in print. In this special issue we are happy to present the speeches by Paul Atkinson (Cardiff University), Margarethe Kusenbach and Donileen Loseke (University of South Florida), Thomas Luckmann (University of Constance), David Silverman (Goldsmiths' College, London University), and Malin Åkerström (Lund University).<sup>2</sup> The contributions will be introduced and published in the order they appeared at the conference.

Åkerström opened the conference arguing that serendipity is not only a phenomenon for the natural sciences but also occurs within the social sciences. However, she claims, we seldom speak about our work in terms of astonishing findings. Åkerström's point is that by sticking to a scientific ethos, the researcher raises the chances of serendipitous findings: for instance, by avoiding conventionality, adding a grain of disobedience, as well as retaining curiosity. Curiosity seems to be a prerequisite for serendipity, yet curiosity is not enough for granting serendipity; only the prepared mind will be able to recognize, and realize the meaning of a happy accident when it occurs.

In the joint keynote speech by Margarethe Kusenbach and Donileen Loseke, they regret

<sup>2</sup> The speech by Barbara Czarniawska will be published elsewhere (Dingwall, Robert and Mary Byrne McDonnell, [eds.] *Sage Handbook of Project Development and Research Management in the Social Sciences and Humanities*). The lunch session with Jaber F. Gubrium, who was interviewed by Anne Ryen, was primarily based on the chapter "Analytic inspiration," co-authored with James A. Holstein. The chapter will be published in Flick, Uwe, (ed.) *Sage Handbook of Qualitative Data Analysis*.

that much research on emotions have forced the social to the background in focusing on how individuals experience, manage, and display their emotions. They suggest a distinctly sociological view in which the questions concern "how people make meanings from cultural resources, and how these meanings make culture." Their contribution is a result of a fruitful (initially informal) collaboration in which they approach emotions from opposite directions: How can it be that events of international concern (such as the attacks of September 11, 2001, and the death of Princess Diana) tend to evoke the same feeling among vast heterogeneous groups of people? How can it be that people who seem to share the same cultural codes (such as, for instance, people living in mobile homes) tend to experience such diverse emotional patterns? Kusenbach and Loseke claim that distinguishing personal, subcultural, and cultural stories is essential in understanding emotions, as they harbor emotion codes and knowledge on *how* to feel. By "bringing the social back in" they see potentials for new connections between the micro and macro social worlds of meaning.

In reflecting on his long research career, Thomas Luckmann led us through a chain of events that changed the study of society and language into the emergence of the communicative paradigm in sociology. He reminds us that present-day researchers' assumptions of (re)constructions of reality might be taken for granted, despite the fact that it was unthinkable not too long ago. It took several theoretical battles and some "discoveries" of older traditions of the philosophy of language and social philosophy before language was viewed as communicative processes and

social reality as constructed through interaction. Yet, crucial for this change was not only theoretical advancements, Luckmann maintains, but technological innovation: the tape- and video-recorder made it possible to broaden the scope of the social sciences. Instead of studying merely the products of social interaction – for instance, food, factories, legal codes, or jails – the technology now admits the careful study of the production processes *per se*. For this purpose, Luckmann asserts sequential analysis, in which the researcher step-by-step traces “the processes by which social reality is constructed and reconstructed,” to offer the best empirical foundation for several social science fields, particularly the sociology of knowledge.

During a lunch session, David Silverman presented the fourth edition of his book *Interpreting Qualitative Data*, and took the opportunity to discuss the question “What counts as qualitative research?” Like Luckmann, he emphasizes the importance of the sequential organization of actions (including talk), assured that sequence is consequential for what we say and do. Silverman claims that the potential of qualitative research is underestimated: “Why can’t qualitative research study behavior?” He regrets the common practice of avoiding questions of social organization in favor of individuals’ perceptions, attitudes, and experiences (to and of

behavior). Silverman specifies his critique of the present state of qualitative research with the help of numerous examples from previous supervision of doctoral students and a published interview study. He concludes that the analytic endeavor of qualitative research is and should be different from journalism.

The last of this issue’s speeches were held by Paul Atkinson, who also gave a much appreciated taste of live opera singing. Creativity is essential to the ethnographer, Atkinson says and underlines that generating ideas involves playful imagination more than data coding techniques. If anything, the methodological textbook industry with its emphasis on mechanical data procedures threatens to restrain whatever curiosity was there initially. With examples from various fields – the glassblowing studio, an opera company, and printing works – he points out creative strategies for generating ideas in ethnographic research. Just like art, craft, and performance, the creative processes in research are indeed dependant on careful, methodical, and repetitive work, Atkinson argues, “but such work is never mechanical.”

With this combination of inspiring texts we hope to stimulate researchers in the common effort to enhance the momentum of qualitative research.

Jacobsson, Katarina, Kristina Göransson, and David Wästerfors. 2013. “Introduction to the Special Issue: «Curiosity and Serendipity in Qualitative Research».” *Qualitative Sociology Review* 9(2):6-8. Retrieved Month, Year ([http://www.qualitativesociologyreview.org/ENG/archive\\_eng.php](http://www.qualitativesociologyreview.org/ENG/archive_eng.php)).

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## Curiosity and Serendipity in Qualitative Research

**Abstract** This presentation argues that we seldom speak of our findings in qualitative research as serendipitous, although we have splendid possibilities to make surprising findings. In order to enhance the chances and sharpen our analyses we have to read broadly but also pay attention to details in our data. We should avoid societal or scholarly conventionality, even be disobedient to recommendations, if this blinds us to new meanings of our findings. The value of serendipitous findings lies in the fact that they diverge from conventionally held knowledge. Thus, we have to retain our curiosity, with the “strange intoxication” or passion that Max Weber wrote about in *Science as Vocation*.

**Keywords** Serendipity; Qualitative Methods; Curiosity

**Malin Åkerström** is a Professor of Sociology at Lund University in Sweden. Her research focuses on ethnographic studies of deviance and social control. She has published several books, including *Betrayal and Betrayers* and *Crooks and Squares*, and articles such as: “Slaps, Punches, Pinches – But not Violence: Boundary Work in Nursing Homes for Elderly” (in *Symbolic Interaction*), “Doing Ambivalence: Embracing Policy Innovation – At Arm’s Length” (*Social Problems*), and “Balancing Contradictory Identities – Performing Masculinity in Victim Narratives” (Åkerström, Burcar, and Wästerfors), *Sociological Perspective*.

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While preparing this talk on curiosity and serendipity, the theme of the qualitative network, European Sociological Association conference in Lund, I discovered that there were many books on serendipity using natural science, the Nobel prizes, et cetera, as illustrations (Meyers 2007; Norrby 2010). I came to wonder whether and how serendipity is relevant for social sciences and concluded that, yes, it is, but is not always presented as such, as I will elaborate later.

Strategies to enhance serendipity while acknowledging some of the hindrances in qualitative social science will be discussed. Some of these dimensions are similar to those in the natural sci-

ences, but some are unique to social sciences. Curiosity is regarded as a necessary but not sufficient ingredient for both discovering and researching one’s serendipitous findings; scientific curiosity is thus, something we must cherish and court. Furthermore, we must learn from natural science in not being too shy in describing our research in terms of “findings.” As mentioned above, in natural sciences, there is an abundance of examples of serendipitous findings.

Fleming’s discovery of penicillin is a case in point, perhaps the most well-known illustration of success in natural science coming to a researcher by chance, “happy accident,” or serendipity. His serendipity was sneezing into his Petri dish (a plastic bowl that chemists keep and grow their bacteria in), which led to his discovery of lysozymes.<sup>1</sup>

The life and work of another great scientist, Carl von Linnæus, provide another example. He was the 18<sup>th</sup> century botanist who developed the classification system of binomial nomenclature that we – or at least some of us – still use today. Linnæus is a good example because he began his university education at Lund University, in 1727.

Linnæus created his famous classification of plants according to their pistils and stamens. One day, he found a mutated butter-and-eggs (*Linaria vulgaris*) flower, which he could not place in his regular classification system.

He could have discarded his finding or even concealed it because it called his already-published system into question. He did not choose this way; instead, he was thrilled and curious because this

<sup>1</sup> In some descriptions it was a tear of his that fell into the Petri dish.

finding challenged his earlier opinion that species were constant and that all species had been created in the form that they currently existed. Thus, he published his findings.

This publication, in turn, led to an intense scientific debate in Europe.<sup>2</sup> In time, it also led to Darwin’s theory of evolution.

### What Can Be Learned?

Unexpected happenings are not, of course, all that is required. We need a benevolent context, space, and time for studies. It is important to give time to process and digest the unexpected. But, today, the politics of science carries elements of the opposite, with an emphasis on fast results and counting, and quantifying publications; indeed, paper content appears less interesting than their number. Furthermore, large, so-called “excellence grants” are given to established male researchers who publish more of the same, but fewer publications, according to a recent Swedish report entitled “His Excellency” (Sandström et al. 2010).

The unexpected, it is true, rests on a past: past knowledge, results, a trained eye for what is truly an unexpected finding. In the words of Pasteur, who is also known for his serendipitous results concerning bacteria: “Where observation is concerned, chance favors the prepared mind.”

Observations alone are not enough. To transform observations into “findings,” one needs curiosity and a will to take findings seriously, to keep on working with the meaning of the unexpected.

<sup>2</sup> See: <http://fof.se/tidning/2007/1/blomman-som-kunde-gjort-linne-till-darwin>, retrieved March 28, 2012.

At the time he found the butter-and-eggs plant, Linnaeus was an established researcher. Still, he was looking for new data, comparing them to his old findings, and revising his old schema of interpretation. This approach, then, resembles an interest in negative cases, as used in analytic induction (Katz 2001).

## Strategies to Enhance Serendipity in Qualitative Social Science Research

Naturally, several dimensions may support serendipity. Here, I discuss five: 1) the wide perspective, 2) the detailed study, 3) disobedience, 4) avoid being trapped by conventionality, and 5) remain loyal to the moral of *science* (and not to other agendas).

### 1) The wide perspective

In a recent book, *Happy Accidents*, on serendipity in modern medical breakthroughs, the author, Morton Meyers, notes the risks of being stuck in established modes of inquiry; the answer, he writes, may lie in a different direction that can be seen only when perception is altered. Meyers uses the example of the Russian painter Kandinsky, known as the “father of abstract art,” who late one night, on returning to his dark studio, found that he could not make out the subject on his easel, but was deeply moved by the shapes and colors. It was only later that he discovered that the painting was resting on its side. Nevertheless, this experience led him down the path of emphasizing the importance of forms and colors and deciding that “depicting objects was not necessary in my paintings and could indeed even harm them” (as cited in Andel 1994:637). Meyers then suggests

that too-close attention to detail may obscure the view of the whole. “Certainly, if one’s perspective is too tightly focused, gross distortion may result” (Meyers 2007:10).<sup>3</sup>

A way to enhance a wide perspective may be to read broadly, as the Swedish sociologists Christofer Edling and Jens Rydberg have illustrated in *Sociological Insights of Great Thinkers* by letting various sociologists write about how Shakespeare, Zola, Orwell, Strindberg, Kafka, and others can inspire us on themes such as stratification, consumption, and interaction. We can also read social scientists who are not necessarily in our own fields. A case in point is Harvey Sacks, who often referred to social anthropologists; a closer interpretation would perhaps be that he relied only on sociologists like Harold Garfinkel and Erving Goffman. The lesson to learn is that specializing in a narrow body of literature probably works against chances of serendipity. Rather, it is a broad and “lustful” reading list that helps, one that does not necessarily respect conventional boundaries.

### 2) The detailed study

A broader view or different perspective, however, is not the whole picture. A focus on *details* may also be quite fruitful for serendipity. Returning to Linnaeus, the focused study of the butter-and-eggs plant, homing in on pistils and stamens, was quite rewarding. For us, as social scientists, a case in point is, of course, the detailed study of conversation analysis in which something as ephemeral as a 5-second silence can be quite powerful. Read-

<sup>3</sup> Which can be compared to Richard Swedberg’s (2012) recent advice on generating theory, one of the many indications of theory and methodology not seldom being parallel enterprises.

ers of Harvey Sacks (2005) are probably struck by his repeated re-analyses. Examples such as “The baby cried. The mommy picked it up” are used so many times that one might get the impression of analytic mania, but the detail adds to successively more complex reasoning. Thus, the wide and broad view, as recommended by Meyers in his Kandinsky example and the minute observations of a Linnaeus, can both encourage serendipity.

### 3) Disobedience

Moreover, in reading books recently published on serendipity, it becomes clear that even if you need to know your field, there may also be benefits *in not being too obedient* to the recommendations taught by its authorities. Let me present what is, to me at least, an unexpected finding from my own research.<sup>4</sup> My disobedience came from not being very much in the field myself and running up against one of the basic assumptions of ethnographic work, that “you have to be there.” The research concerned an evaluation of a large, extremely expensive youth care project. This evaluation involved employed youth care coordinators (social workers by training). Due to a lack of time, I mainly relied on my co-workers’ field notes and interviews. The coordinators (or case managers) were presented as practical, person-oriented, “state-employed parents,” closely oriented to the youngsters and to their parents. From the field notes and interviews, however, it became clear that meetings, documents, rules, and regulations were central and inspired engaged talk among the professionals in the field, while the formal objects of their work, the youngsters, were obscured in a discursive shadow. Meetings for these coordinators were where “the Action is” (Goffman 1982), a context where they could

<sup>4</sup> The results are published in Basic, Thelander, and Åkerström (2009), and discussed in Åkerström (2011).

test their skills and competence in competition with other bureaucrats. That meetings were central for this category was indicated by the many meeting names and references that came up in an examination of the textualized data (Table 1). This cultural concern was similar to other naming practices noted in studies of varieties of rice (Brown 1965) or taxonomies among drug addicts (Agar 1994:73-88), for example.

Table 1. Varieties of Meetings.

Varieties of Meetings
Workgroup meeting
Extra meeting
Morning meeting
Pre-meeting
Group meeting
Information meeting
Enrolment meeting
Local work group meeting
Mid-meeting
Network or family meeting
Staff meeting
Planning meeting
Reference group meeting
Recommendation meeting
School meeting
Soc-meeting [the social services]
Team meeting
Task meeting
Treatment meeting
Follow-up meeting
Week meeting
Video meeting
“Hand over” or referral meeting

Source: Åkerström (2011).

Apart from these, there were other references to meetings in the notes (Table 2).

Table 2. References to Meetings.

<b>Meetings referring to each other</b>
Last meeting
Next meeting
Meetings coming up
<b>"Meetings with" referring to various categories of people or institutions</b>
Meeting with social authorities
Meeting with parents
<b>Old and new forms of meetings</b>
Video meetings as opposed to "regular meetings"
<b>Time and place indicators</b>
Meeting room, meeting places
Frequency of meetings, meeting times

Source: Åkerström (2011).

An image of the case-managers as *Homo Administrativus* emerged, something that my collaborators had not noticed, possibly because they were too occupied by the daily tasks of this very intense evaluation, which involved lots of data collecting, as well as being emotionally intense because of the many sad stories concerning youngsters and parents. Indeed, one of my co-workers had more trouble sleeping at night than he had had after his war experiences in the Balkans. My collaborators

published excellent work, but did not note this particular meetings-focused trait. I am not arguing for the general benefits of being an "armchair ethnographer," but in some cases, it may be possible – and at times rewarding – if you are not researching in a very foreign context. In this case, as a university employee, I knew quite well the contexts and meanings of meetings.

The English philosopher, Francis Bacon, used the metaphor of the hunt when analyzing scientific investigation. To this metaphor, one may add that "if the game presents itself when we are looking for it, it may also present itself when we are not looking for it, or when we are looking for game of another kind" (Andel 1994:635). In my case, "meetings" was something I stumbled over when hunting for other phenomena in my material.

#### 4) Avoid being trapped by conventionality

One risk we face is that we stifle ourselves by being too conventional, in the form of trying to seek a safe haven in terms of contemporary debates on how to collect and analyze data. Such conventionality can arise from several sources. We might be caught intellectually by internal social science rhetoric of privileging qualitative studies over quantitative, policing ourselves in not using the latter, while quantitative data may be very useful for us. We may be persuaded by qualitative scholars privileging "naturally occurring data," while others defend the use of unstructured interviews, others prefer discourse studies of texts and documents. In grant proposals, you sometimes see an allusion to a certain software program for analyzing qualitative data, as rhetoric in itself, with no further arguments on what to feed the programs with.

There are also ways of analyzing that are in fashion. For a while, most dissertations and many articles assured us that they had used grounded theory. Now, with the popularity of the language turn, much is done on narratives and on discourse instead. But, even the new will eventually be in jeopardy, as evidenced in the title of an upcoming symposium: "Matter Matters: The Social Sciences Beyond the Linguistic Turn."<sup>5</sup> Quite often, the new is rhetorically contrasted with the old, without any further arguments or illustrations of what new discoveries have been made by the new perspective or may be made with the new. This is not to say that new perspectives are not necessary. They are needed, but to me, many fail to address the newness' potentials in discovering or in illuminating.

Furthermore, a lot of effort is made and rhetoric produced on an almost ideological level where social scientists get their identities; they hook up or marry one perspective or another. A social scientist can thus, come to be known as the "quantitative guy," an ethnographer, or a "CA woman." Instead of being known as someone who explains a social phenomenon, for example, gifts, divorces, having pets, et cetera. Catherine Kohler Riessman is more known for her narrative analyses than for her studies of childless

<sup>5</sup> The announcement "A symposium to be held at the Faculty of the Social Sciences Lund University, October 15-16, 2012" explains: "For all their differences, theoretical orientations, such as constructivism, deconstruction, discourse analysis, and conceptual history, share the underlying assumption that the study of linguistic entities holds the key to knowledge of the sociopolitical world. Yet, there is a growing concern that the linguistic turn has unduly limited the domain of inquiry, and now has exhausted most of its potential. In the ensuing efforts to escape the prison of language, many scholars have been tempted to speak of an ongoing material turn or new materialism within the social sciences" (see: [http://www.lu.se/o.o.i.s?id=29592&list\\_mode=id&calendar\\_id=10308](http://www.lu.se/o.o.i.s?id=29592&list_mode=id&calendar_id=10308), retrieved September 10, 2012).

women, masculinity and illness, and divorces. Kathy Charmaz is more known for her grounded theory than for her work on illness and identity.

Such identifying divisions are not common among historians, for instance, who talk about themselves, for example, as being "pre-medieval, medieval, or modern historians," or as being interested in women's history or in court history. Medical researchers may talk about themselves as scholars studying specific organs, such as the eyes or heart, or specific proteins.

Another observation: conventionality is integrated and propelled by modern research politics. As early as 1961, U.S. President, Dwight Eisenhower, who is known for coining the phrase "the military-industrial complex" in his farewell speech, spoke in the same speech about another important situation where academic research can be too dependent on – and thus, shaped by – government grants, "where a government grant becomes virtually a substitute for intellectual curiosity."<sup>6</sup> This tendency seems to have accelerated, now we are not only congratulated for bringing in grants; our bosses may report on how much money this or that person got, but not always on what the grant was intended to research.

#### 5) Remain loyal to the moral of science (and not to other agendas)

Retaining curiosity with the "strange intoxication" or passion that Max Weber wrote about in *Science as Vocation*, and keeping the passion for the unexpected may not always be easy. Fighting off conventionality is only one risk.

<sup>6</sup> See: <http://www.americanrhetoric.com/speeches/dwightdewarfarewell.html>, retrieved June 27, 2012.



The intrinsic value of serendipity findings lies in their going against commonly held knowledge. The history of natural sciences is full of examples of heroic people who were derided, but stayed on course.<sup>7</sup> One example is the treatment of the researchers who discovered that bacteria cause stomach ulcers in a time when the accepted dogma was that stress or other factors caused the problem.

We, social scientists, may not always be met by scorn from our colleagues when presenting our results; in fact, there may be too few scientific debates on social science results. But, in our case, as social scientists, we may have to wrestle with our own and others' beliefs in current sociopolitical or other types of societal trends. Many social scientists tend to be married to political beliefs, some are even activists, or at least, they have difficulties in separating science from reform agendas. If we are stuck in such lines of agendas, our research might be guided by these aims rather than driven by curiosity; we might even censure our curiosity if findings do not fit the current wisdom in a political field.

Publishing findings that run against such commonly held beliefs may, in some cases, be painful in ways that natural scientists do not experience; viruses cannot talk back, so to say, and they cannot blame a researcher for being heartless, racist, or conservative.

Many of the studies I have been involved in have evoked such responses. One illustration is a study of how staff at nursing homes talked about and

<sup>7</sup> For illustrations, see, for instance: <http://www.foresight.org/news/negativeComments.html>; <http://amasci.com/weird/vindac.html>, retrieved July 16, 2012.

dealt with elderly patients who were violent (Åkerström 2002), which invoked critique from colleagues, reviewers of articles, and from the audience when presenting talks; I was morally questioned on the subject: Why had I not written about *the elderly*? They were the ones who were abused, according to many media scandals. Another example concerns ethnicity. In a series of recent studies of policing ethnicity, we faced many instances where the researchers had difficulties not only in getting past gate-keepers in schools and institutions but also in writing up our findings, and presenting them at seminars. "Ethnicity," we were told, "is a very delicate subject."<sup>8</sup> A more well-known case in Sweden concerns the Swedish sociologist, Eva Kärffve, who was attacked by psychiatrists, patients, the Child Ombudsman, and many others for questioning the scientific bases of medical diagnoses of DAMP and ADHD (Kärffve 2000); and a well-known international example is the responses to Hanna Arendt's *Eichmann in Jerusalem: A Report on the Banality of Evil*.

### The Strength of Qualitative Studies in Generating Serendipity

This is a qualitative methods conference. As researchers using such methods, we are in a splendid position of making surprising findings. But, what is serendipity? It might be easier to define for natural science: their results can be clearer, unequivocal. Furthermore, these researchers pursue their work in a cumulative manner. For us, cumulative work might not always be possible,

<sup>8</sup> The studies are carried out within the program *Policing Ethnicity in a Diversified Sweden*, grant no. 2009-0011, supported by the Swedish Council for Working Life and Social Research (Peterson, Abby, and Åkerström).

or at least, not always desirable. Still, we have to be prepared because "chance favors the prepared mind," but not blinded by earlier results and common understandings.

Serendipity, I propose, for social scientists is the sum of those findings that are unexpected and contrast with earlier "social knowledge," whether this knowledge is derived from the social sciences or based on commonly held cultural assumptions. Many of the classic ethnographic works have become classics because they provide us with a new way of understanding a local culture, profession, or social phenomenon. Some were contrasts to established social science knowledge, as, to use a minor classic, Whyte did in *Street Corner Society*. He showed that the slum was socially organized, not disorganized, which ran against established truths among social scientists at the time. At other times, findings can contrast with more general societal assumptions, as did Humphreys' *Tea Room Trade*, which questioned current understandings held by policemen and the public about homosexuals.

The major strength of qualitative studies is the basic openness they provide. In general, we are not in the business of trying to test hypotheses that

already exist or have locked in our questions in the grid of a questionnaire, and we are not locked in by data provided by a database. So, we have to work to retain our curiosity and look out for interesting findings while we try to clear our mindsets of too many buzzwords or engage in applying for grants for their own sake. Whether we find our data in new material or through re-analyses of earlier collected material, we are – in our qualitative tradition – *apt* to make some surprising and lucky discovery, and the trick must be to make the research as open as possible to achieving this.

My point is that this ESA conference's theme, Curiosity and Serendipity, should be devoted to openness, in terms of being interested in various methods, techniques, and concepts that help us analyze our material and in being interested in – and enjoying – the new findings presented here. There are, as I mentioned initially, many books and articles on serendipity in natural science, describing the "happy accidents" of those who won Nobel Prizes, and so on. We seldom speak, however, of our findings in qualitative social sciences as serendipitous. I hope that this conference will be devoted to the awe and wonder of the magic of discovery.

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## Bringing the Social Back in: Some Suggestions for the Qualitative Study of Emotions

**Abstract** As a relatively new area of inquiry, it is not surprising that the research agendas and methodological tools of the sociology of emotions are still evolving. Our goal in this article is to offer new ideas toward emphasizing the social, as opposed to individual, dimensions of emotions in sociological research. What are the historical, cultural, and biographical structures and contexts of individual emotional experiences? What are the social and political antecedents of individual experience? What are the origins of social and cultural frameworks shaping individual experience? What are the social and political consequences of individual experiences? Broadly speaking, these questions are about how people make meanings from cultural resources, and about how these meanings make culture. And because these are questions about meaning, they necessarily require qualitative data and analytic techniques.

The second section of the article, written by Loseke, conceptualizes and explores emotions as systems of meanings. Rather than focusing on unique individual experiences, Loseke's starting point is the shared ideas and rules regarding emotions within a culture, and their manifestations in widely circulating narratives. The ensuing analysis focuses on the symbolic and emotion codes (e.g., victim) and structures of such stories, and on the work they do for individuals and for society as a whole.

The third part of the article, written by Kusenbach, begins with individual emotional experiences, yet seeks to account for the larger cultural patterns (life stories) that provide them with meaning. Kusenbach's research shows that residents of mobile homes, a stigmatized type of housing, employ a range of cultural narratives that furnish both negative and positive emotional experiences surrounding their place of living.

In sum, it is argued that both approaches generate new questions and insights, new kinds of data, and new methodological tools for a more sociological study of emotions.

**Keywords** Emotions; Sociology; Social Constructionism; Culture; Interaction; Qualitative Research Methods

Our interest in expanding the research agenda for sociological, qualitative studies of emotion results from our evaluation that current emotion research is underdeveloped and that this leads to rich possibilities for asking new kinds of questions and for developing new methodological techniques. The underdeveloped nature of sociological research on emotion stems, in part, from the relative recency of interest in emotion as a topic for empirical research. True, the *theoretical* importance of emotion has long been established – long ago, Aristotle argued that the most effective rhetoric involves appealing to both logic and emotion (Waddell 1990); classical-sociological theorists, including Marx, Comte, Durkheim, Weber, Simmel, and members of the Frankfurt School, have similarly argued that emotion is critical to social life (Shilling 2002). Yet, sociologists did not transform emotion into a topic for *empirical* study until the late 1970's, early 1980's, when books such as *A Social Interactional Theory of Emotion* (Kemper 1978), *The Managed Heart* (Hochschild 1983), and *On Understanding Emotion* (Denzin 1984) established emotion as a sub-field in sociology (Franks

and McCarthy 1989). In brief, sociological research on emotion has quite a short history, a characteristic leading to the underdeveloped nature of research agendas.

The sociological study of emotion has also been constricted by the relatively narrow set of questions forming the research agenda. While, by definition, sociologists recognize the *social* nature of emotion, it is nonetheless most common for studies to focus on topics about *individual* subjectivity. Common areas of interest include: how individuals are socialized to become emotionally competent (studies on primary emotional socialization are common in journals such as *Early Education and Development* and *Early Childhood Education*; Kunda and Van Maanen [1999] offer an example of emotional socialization in professional training), how individuals understand, experience, and manage their own emotions (for examples, see Gottschalk [2003] for emotion management in the Holocaust second generation, and DeVault [1999] for emotion work in family life), how individuals manage the emotions of others (see Thoits 1996), how emotional management of self

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and others can be a job requirement (Meanwell, Wolfe, and Hallett 2008 offer a review), and how interaction creates and maintains emotional experiences (see Boiger and Mesquita 2012 for a recent summary).

A recent review article describing the “sociological theories of human emotions” (Turner and Stets 2006) demonstrates how the study of emotion can be confined to questions about individual experience. The authors define “the five basic approaches” to the study of emotion as: *dramaturgy* (questions about emotion impression management), *symbolic interaction* (questions about relationships between positive emotions and perceived verifications of self-worth), *interaction ritual* (questions about how positive and negative emotions are aroused), *power and status* (the effects of power [authority] and status [prestige] on emotional arousal), and *exchange* (questions about the costs and benefits of particular emotions).

While continuing research on how individuals experience, manage, and display their emotions has been remarkably productive, it remains that such a focus relegates the social to the background. Indeed, the importance of the social in emotion is further diminished by complaints that sociologists have attended too much to emotions as cultural products and too little to the importance of evolution and biology (Turner and Stets 2006; Franks 2010).

Although much remains to be examined about the individual, lived experience of emotion, we believe it is time to add to the agenda, to expand beyond questions about individual subjectivity and its consequences. We argue that new oppor-

tunities for understanding the distinctly sociological nature of emotion would be created by *re-centering* attention from the individual to the social and cultural. When the social, rather than the individual, is the central focus, new questions emerge: What are the historical, cultural, and biographical *structures* and *contexts* of individual emotional experiences? What are the social and political *antecedents* of individual experience? What are the origins of social and cultural *frameworks* shaping individual experience? What are the social and political *consequences* of individual experiences? We suggest thinking of these questions as distinctly sociological, as about how people make meanings from cultural resources, and about how these meanings make culture. Because these are questions about meaning, they necessarily require qualitative data and analytic techniques.

Within our shared interest in changing the focus of the study of emotion from the individual to the social, our specific projects are quite different. Loseke’s project about conceptualizing emotion as systems of meaning is about developing *new types of data and new types of methods* for examining emotion. Kusenbach’s interest in how subjective experiences of emotion are shaped by folk understandings of these systems of meaning leads to *new types of questions*.

We will continue with each of us offering brief descriptions of our proposed lines of research. We offer these as works in progress, with the hope they might spark conversation about possibilities for new directions in sociological, qualitative explorations of emotion.

## Loseke: Conceptualizing and Exploring Emotion as Systems of Meaning

I will begin with a puzzle: From time to time enormous numbers of people sharing little in the way of practical experience, resources, or world views seem to unite in emotional evaluations of events that lie outside their personal lives and experiences. The death of Princess Diana in Great Britain in 1997, for example, led to an outpouring of grief and sadness throughout the Western world. Likewise, after the events of September 11, 2001 in the United States a variety of observers argued that Americans converged in feeling sympathy for the people who had perished that day and for the loved ones they left behind, in feeling anger and hatred toward the people responsible, in feeling pride in how America was responding, and in feeling a patriotic duty to “save civilization” from the terrorist threat (Loseke 2009).

Such apparent mass convergence in emotional experiences is a puzzle because *it should not be possible*. Modern industrial and post-industrial social orders are characterized by vast social and economic heterogeneity, moral fragmentation, and a loss of religious or tribal meanings. Each of these characteristics works against developing similar appraisals of the meanings of events and therefore, against developing more-or-less shared cognitive, emotional, and moral meanings.

My theoretical project is to account for the socially shared nature of emotion; my empirical project is to develop qualitative methods to examine the production, circulation, consumption, and

consequences of this process. Rather than beginning with individual, micro-level experiences and working “up” to social/cultural macro-level characteristics, I want to begin with culture.

In outline form, my argument involves relationships among cultural meaning systems and socially circulating narratives. I will define *cultural meaning systems* as more-or-less widely shared systems of ideas composed of symbolic and emotion codes. Within large, heterogeneous, mass-mediated social orders these systems of ideas are often embedded in and spread through *socially circulating narratives*. I want to explore how publicly circulating stories can contain, and hence, can relay to large audiences, sets of expectations, proscriptions, prescriptions, and moral judgments about the world, and how these can become resources that practical actors can use to make sense of self and others.

Because sociologists interested in emotion have tended to ignore culture, or to simply assume its presence and not examine its workings, I begin with a brief description of a social constructionist view of emotion, the theoretical contexts for my argument.

### The Primacy of the Social in Emotion

An extensive body of theory and research, often going by the name of social constructionism, envisions emotions as distinctly *social* in their origins, meanings, expressions, and consequences. This perspective begins with a simple observation: The primary determinants of emotion as experienced cannot be physiological or individual in origin because the subjective experience of emotion requires a cognitive appraisal of the

meaning of events, and such appraisals rely on language and socially determined meanings (Lerner and Keltner 2001). It follows that feelings are like other experiences in that they are social products based on beliefs, shaped by language, and therefore, culturally derived (Geertz 1973). Within this conceptualization, emotion becomes a “cultural phenomena, embedded in beliefs, symbols, and language, inextricably linked to social and cultural processes” (McCarthy 1989:51). When the foundation of emotion is theorized as social, it follows that more-or-less shared emotional experience requires more-or-less shared *cognitive meaning*.

### Cultural Meaning Systems

Shared meaning is possible because of cultural meaning systems, which are socially circulating ways to *think* and to *feel*. Symbolic codes (Alexander 1992), also called interpretive codes (Cerulo 2000), semiotic codes (Swidler 1995), and cultural coherence systems (Linde 1993) are ways to *think* about how the world works, how the world should work, of rights and responsibilities of people in the world. Examples of such codes include “The Standard North American Family” (Smith 1999), mothering (Gazso 2012), individualism (Bellah et al. 1985), violence (Cerulo 1998), and citizen and enemy (Alexander 1992).

Systems of meaning also surround ways to *feel*. Emotion codes, also called emotionologies (Stearns and Stearns 1985), emotional culture (Gordon 1990), and feeling rules, framing rules, and expression rules (Hochschild 1979) are *cognitive* models about which emotions are expected, when, where, and toward whom or what, as well as how emotions should be inwardly experienced,

outwardly expressed, and morally evaluated. These codes are resources that “allow members of a society to identify and discuss emotions, evaluate them as desirable or undesirable, and regulate them in line with values and norms” (Gordon 1990:29). Examples of emotion codes include sympathy (Clark 1997), fear (Altheide 2002), love (Swidler 2001), and closure to grief (Berns 2011).

The wider codes are shared, the more they are a part of what Durkheim called the “collective conscious” (Durkheim 1961). The more widely shared, the more codes are the “impersonal archipelagos of meaning...shared in common” (Zerubavel 1996:428). The more widely shared, the more codes can be a part of a “cultural toolkit” (Swidler 1986) that social actors can use as “schemes of interpretation” (Schütz 1970), “interpretive structures” (Miller and Holstein 1989) or “membership categorization devices” (Sacks 1972) to make sense of self and others.

Because symbolic codes and emotion codes are cultural level concepts, questions are raised: Where are these codes located? How do social actors know about them? Where do they come from? I turn now to *narrative* because a key feature of emotion discourse is its employment in narrative (Edwards 1999:279).

### Systems of Meaning as Narrative

In contrast to prior eras when academic observers deemed narratives – stories – as not worthy of attention because they are “unscientific,” modern day observers argue that understanding people and social life requires understanding how stories work and the work stories do at all levels of social life (Loseke 2007).

The sociological study of narratives tends to follow the same path as research on emotion in that the majority of interest has been on the characteristics and uses of stories told by and about individuals (Holstein and Gubrium 2012). In contrast, my interest in shared meaning leads me to stories that *circulate* in the social world. These are the stories of unique people or types of people that are contained in a variety of places, such as in the speeches of politicians and preachers, in the claims of activists and advertisers, in textbooks, in court and congressional hearings, in mass media of all sorts. Regardless of any “truth” (as that might be understood), these stories are told as true and they have many social uses: Socially circulating narratives describe types of people and proscribe relationships among people so they are an aspect of the symbolic universe (Alexander 1992), they are a foundational characteristic of movements for social change (Davis 2002), they pattern the work of courts of law (Amsterdam and Bruner 2000). Narratives about types of people become justifications for public policy (Schneider and Ingram 1993) and shape the organization of social services by offering workers ways to make sense of individual clients (Loseke 2007). Narratives form the background of thinking and filter perceptions in daily life (D’Andrade 1995). In brief, socially circulating stories do a great deal of work in social life.

My claim is that stories that are the most likely to be evaluated as believable and important by large, heterogeneous audiences are those containing the most widely and deeply held symbolic and emotion codes. These codes furnish the “skeletal structures on which social communities build their familiar stories” (Alexander 1992:294).

Story *authors* can use these codes to construct meaningful and emotionally compelling scenes, plot lines, characters, and morals; story *audiences* can use their understandings of these systems of meaning to evaluate the believability, importance, and emotional content of stories, as well as the extent to which socially circulating stories pertain to their own activities and agendas.

For example, consider the stock character of *victims* found in so many socially circulating narratives: The majority of social movement activities are about convincing disbelieving publics that one or another condition is producing victims and, therefore, must be changed; the work of criminal courts, and sometimes civil courts, is that of determining victim status; social policy often is about assisting people defined as victims. *Why* is the victim character so common in socially circulating stories? *What* are the characteristics of a character that will lead to the victim evaluation? I begin with the code of victim.

In daily life the status of “victim” is not given to all people experiencing harm. Victim is rather a symbolic code, a system of ideas, a term for a person (or type of person) evaluated as moral *and* as greatly harmed *and* as harmed for no good reason *and* as harmed through no fault (Holstein and Miller 1990; Best 1997; Lamb 1999). It is no mere coincidence that elements in this symbolic code of victim are simultaneously those in the emotion code of *sympathy*. According to Candace Clark (1997), on a case-by-case basis, individuals are evaluated for their “sympathy worthiness” and the common conventions for doing this evaluation are the same as those surrounding the evaluation of victims. That is, people – unique, known people in daily life or

unknown characters in socially circulating stories – who are determined to be sympathy-worthy are those evaluated as moral people who are greatly harmed for no good reason and through no fault. In turn, the emotion code of sympathy contains a behavioral expectation: A sympathy-worthy person deserves “help.” This is the very practical reason why there are “sympathy contests” in courts, public policy testimony, and social movement advocacy: Evaluations of stories are about determining practical responses to story characters.

Symbolic and emotion codes surrounding victims and sympathy are, therefore, resources to construct victim characters in narratives. Because there are enormous variations in how individual people will evaluate the precise requirements of being designated a “moral person,” of what, precisely, constitutes “great harm,” “good reason,” or “fault;” it follows that the narratives that will be the most effective in encouraging widespread sympathy will be those featuring characters whose morality and harm is *beyond doubt*, where there will be no doubt about the “reason” for harm nor about the innocence in creating that harm. Stated otherwise, agreement that a character is a “victim” and should be responded to as such is encouraged by dramatizing innocence, harm, and lack of intent.

Symbolic and emotion codes likewise create story *plots* and *morals* that have potentials to be evaluated by large audiences as believable and important. Consider the process of constructing public policy. In a not-so-distant past, observers assumed that the policy process could be understood by examining the self-interests of elites (Rocheffort and Cobb 1994). Now, there is increas-

ing attention to how the policymaking process most typically involves “causal stories” defining the problem, the cause of the problem, and the need for particular kinds of policy (Stone 1997). These causal stories have characters, called the policy’s “target population” (Schneider and Ingram 1993). Policy targets, such as the “welfare queen” (Asen 2002) and “poor women” (Mazzeo, Rab, and Eachus 2003), are constructed within moral universes (Mohr 1994), with expectable emotional responses such as sadness and desires to help “victims,” anger and desires to punish “villains.” Hence, justifications for the “Violence Against Women’s Civil Rights Clause” in the United States were accomplished through constructing the story of the “monolithic woman as a pure victim” deserving of sympathy and help (Picart 2003:97).

In summary, narratives do considerable work at all levels of social life, and systems of meaning – symbolic codes and emotion codes – are the building blocks to construct stories that have potentials to be evaluated as believable and important by more than a few people. Shared cognitive and emotional meaning is, therefore, the consequence of a *social* process and this process is reflexive: Shared meanings are most possible when the characters, plots, and morals of socially circulating narratives reflect the most widely and deeply held symbolic and emotion codes, the circulation of stories can be conceptualized as a form of “shared experience.”

#### **Narratives, Structures of Meaning, Emotion, and Qualitative Research**

Arlie Hochschild’s (1979; 1983) conceptualization of the importance of emotional framing rules,

feeling rules, and expression rules has led to a rich empirical literature about how individuals understand and use those systems of meaning in daily life. My suggestion is to continue such research but also to stand it on its head in order to investigate questions about the rules – symbolic and emotion codes – themselves. Meaning systems can be found in talk and in documents (see D’Andrade 2005; Quinn 2005; Loseke 2012).

Clearly and most certainly, I am *not* suggesting that symbolic codes and emotion codes have a life independent from the occasions of their use. On the contrary, codes are merely a resource that practical actors can choose to use – or to not use or to modify – on a case-by-case basis. Yet, focusing on the rules rather than on their uses raises questions that are increasingly important in our globalized, heterogeneous, morally fragmented, mass mediated world.

Some questions are about the *stories* themselves and will require document analysis methods (Loseke 2012): What are the contents of systems of ideas constructing the characters, plots, and morals of socially circulating stories that are leading political and social debates? How do stories, symbolic codes, and emotion codes vary according to time and place? What are the characteristics of competing stories? Which social actors are authoring the stories that are the most effective in the public sphere? What is the *political* work done by stories across historical and cultural stages?

Because meaning always is contingent and contextualized there are other questions in the form of “audience reception” that require interview or focus group data: What types of stories do spe-

cific groups of people find cognitively and emotionally appealing? Because stories authored for one audience increasingly become available to other audiences we should ask: How are stories intended for particular audiences understood by others?

My suggestion is simply that we could learn much about the social characteristics of emotion by de-centering the qualitative study of emotion from questions about individuals to questions about the social and cultural characteristics of social life.

#### **Kusenbach: Life Stories and Emotional Experience**

Episodes of widely shared emotions among citizens who tend to have little in common are not well understood. A related type of incident is also not well understood: Why are there *diverse* patterns of emotional experiences among members of social groups who, at least from the outside, appear to occupy virtually identical structural and cultural locations in society? These episodes call on scholars of emotion to break new ground where existing theories, concepts, and methods fall short. Despite their different starting points, both these puzzles lead toward exploring understudied *social* aspects and contexts of emotion, and lead away from emphasizing the uniquely situated characteristics of individual experiences. Comparing our inquiries produced the insight that the sharing of emotion codes can vary widely across scales, ranging from (within a culture) universally shared codes to smaller societal pockets in which a (limited) number of culturally available, and possibly even competing,

codes shape what and how people feel. The following pages offer some details on my ongoing analysis of recent qualitative research conducted in Florida mobile home communities.

In 2005, following a very active hurricane season the previous year, I became interested in and started researching disaster and community issues in Florida mobile home communities. Building on previous work on interaction and meaning in neighborhoods (Kusenbach 2006; 2008), I was interested in how study participants made sense of, and felt about, their homes and larger surroundings. Feelings of home, and more broadly feelings of belonging in, and attachment to, places have been virtually overlooked topics by sociologists studying emotions (Duyvendak 2011), and my current work aims at closing this gap.

Mobile homes, also called “manufactured homes” or “trailers,” are factory-built rectangular boxes on wheels that can be set up quickly almost anywhere. However, in spite of their name, mobile homes are rarely moved from their first location, due to their ever increasing size and the rising cost of relocation. In the United States, mobile homes first became popular as temporary housing during and after the World War II housing shortage, and they continue to offer inexpensive alternatives to site-built homes. Fueled by the dual mortgage and economic crises in recent years in the United States, consumer demand for mobile homes has been growing fast. In 2010, around 18 million people in the United States lived in about nine million mobile homes in virtually every state and region of the country. About ten percent of all mobile homes are located in Florida where one in twelve residents lives in such places. West Central Florida is the

birthplace of mobile home communities (Wallis 1991) and they are more numerous here than in any other region.

My current data include about fifty in-depth interviews with mobile home residents in a diversity of settings and social locations within the larger region, and about a hundred interviews conducted with households in four family (all ages) communities in the city of Tampa and vicinity. The interview data is complemented by many ethnographic observations, detailed community portraits, a small survey, and a photographic archive.

Living in a mobile home and mobile home community carries a negative stigma in mainstream American culture. A plethora of jokes, cartoons, TV shows, magazine articles, advertisements, and so on refer to mobile home dwellers as “trailer trash” (Kusenbach 2009). The label implies that these people are *deficient* on many levels: they are assumed to be poor, dirty, ugly, stupid, immoral, and even criminal. For instance, a 2006 full-page advertisement by the “National Center for Family Literacy” in the *New York Times* shows a picture of an extremely crowded and dirty looking mobile home park, with the accompanying text asserting that this is the type of environment one will escape by learning to read better and getting a GED (alternative high school degree). While depictions of mobile home residents and communities can be less obvious and extreme, the message they deliver remains virtually unchanged: In the United States, where displays of material wealth indicate superiority and success, living in a mobile home is a sign of inferiority and failure.

Virtually all research participants were familiar with the stigmatizing cultural stereotypes. They knew from popular media and sometimes from personal experience that their dwellings and neighborhoods are the targets of insults and jokes. Indeed, many mobile home residents actually *shared* the common belief that mobile home communities contain “trailer trash” people – they simply believed this not to be the case in their part of town, community, or street. The stock character of the typical mobile home resident as morally, economically, and culturally *deficient*, and moreover as *responsible* for those deficiencies, is immensely pervasive. Therefore, in an earlier paper (Kusenbach 2009), I investigated mobile home residents’ strategies of emotional and practical distancing from the pervasive view.

However, as data collection and analysis continued, I came to realize that *resistance* to the “trailer stigma” was not the only frame of reference that could be used to make sense of residents’ feelings regarding their homes and neighborhoods. I here propose that this is only one among several culturally circulating stories mobile home residents tell themselves and others. The discovered stories create cognitive meaning and order for the current, past, and future lives of research participants, and they reflect culturally accepted moral and emotional models of how one could be good, and of how one should feel in social situations and spatial contexts that deviate from mainstream scenarios. I now briefly characterize five such stories.

### Victim Story

Individual versions of this cultural story most strongly reflect the pervasiveness of the “trailer

trash” stigma, yet they offer an interesting twist. Those participants who most readily resorted to stigmatizing stereotypes while talking about *other* mobile home residents vehemently rejected these views in reference to *themselves*. Tellers of victim stories fully blamed others for their “trashy” lifestyle and character, yet they forcefully claimed innocence for their own current circumstances and, in complete agreement with the emotion code of “victim” as described above, demanded sympathy (Clark 1997) for themselves. These research participants offered accounts of neglect and abuse by parents, former partners, employers or strangers, and they told stories of job loss, injury, physical and mental illness, or simply bad luck. Interestingly, participants who thought of themselves as victims frequently reported feeling “trapped” in their homes and communities, they tended to strongly dislike most or all of their neighbors, and appeared bitter, angry, and depressed.

For instance, Myrtle, a White resident around forty who lived with her husband and two children in a park we happened to call “Happy Place,” in her case an ill-fitting pseudonym. The following excerpt shows a segment of her conversation with Marc, one of the graduate student researchers:

Marc: Are you planning to move?

Myrtle: One day. This is not where I want to grow old. No! [laughs]

Marc: Why is that?

Myrtle: It’s in a trailer! It’s in a park! I don’t like living in mobile home parks.

Marc: Why is that?

Myrtle: Well, because you live too close, trailer park drama. (...)

Marc: How would you describe this park?

Myrtle: You work for it, or are you affiliated with [Happy Place] mobile home park in any way? [laughs]

Marc: No, no, this is anonymous research.

Myrtle: I hate this park! I hate, hate, hate it! (...)

Marc: What do you like best about living here?

Myrtle: Nothing! There's nothing positive about this place.

Myrtle here expresses dislike for both her home and her neighborhood. After making sure that she does not have to fear repercussions for speaking honestly, she even admits to "hating" the park. In the beginning, Myrtle suggests that living in a "trailer" and "park" is reason enough to be dissatisfied. Her answer indicates that she suffers from the cultural "trailer stigma" regarding mobile home parks. However, as the interview goes on, she actively depicts her neighbors and park managers as lazy, unintelligent, unclean, and malicious. Both her own negative perceptions of others and, ostensibly, public negative perceptions of her, come together in Myrtle's visceral discomfort with her current situation which she cannot change at the moment.

In contrast to Myrtle and others like her who depicted themselves as "victims," many study participants talked about how comfortable they felt in their current homes and neighborhoods, and how much they liked their neighbors. Many displayed a sense of pride in their current accomplishments and no desire to leave. Generally, most study participants seemed quite happy with their lives.

Yet, how is this possible? How can emotional experiences regarding home and community be so completely different, sometimes within the

same community or on the same street, among people who overlapped greatly in their social and cultural locations? Again, my point is that the experience of, and resistance to, the "trailer trash" stigma is not the only narrative lens that mobile home residents have adopted in making sense of their lives. They embraced other, equally powerful and "American" stories which provided legitimation and framing for more positive feelings.

### Homeownership Story

For instance, some participants told the powerful story of achieving the "American Dream" through homeownership. The key idea in this narrative is that owning property is a sure indicator of being middle class (economically successful), and that being middle class is a sign of moral decency and good character. Immigrants and Americans of color especially seemed to enjoy the success of having obtained formal ownership of a home, even though it may not be the perfect kind. A variation of the homeownership story was also told by some White participants who eagerly claimed that owning a mobile home is actually "better" than owning a site-built home because its greater affordability frees up more money for other forms of conspicuous consumption which further cements evidence of obtaining the "American Dream."

### Meritocracy Story

A third cultural story is distinctly American as well. It is the story of meritocracy which includes the expectation of, or at least hope for, upward social mobility. It says that you may have to start at

the bottom, but with hard work and strong moral principles you have a very good chance of moving up the social ladder. Here is an example.

Arnold is a White man in his early twenties who shared a home with his girlfriend and their baby, also in "Happy Place." Arnold was unemployed at the time and planned to join the United States military as soon as he recovered from a work-related injury.

Marc: What does your family...what do your friends think about you living in a mobile home park?

Arnold: It is what it is! People got to start off doing what they got to do, you know, you got to crawl before you walk, man! You don't start off being a millionaire.

Marc: What do you believe other people think about those who live in a mobile home park?

Arnold: I don't care. It's not a problem with me. My whole family lived in them. We've been grown up, born and raised in them, you know, more than half my family lives in them till this day...I'm happy, I'm happy with what I got, you know what I mean?

Arnold here describes his life as following a trajectory of upward mobility. Considering his family history, Arnold considers it acceptable ("it is what it is") and "not a problem" that he currently lives in a mobile home. It makes good sense because "you don't start off being a millionaire" in this cultural story. Arnold expresses much hope for himself to be able to "walk" at some point, a step up from his current "crawling," which seems the appropriate stage for a man of his age and family heritage. It is notable that Arnold does not engage Marc's question on the "trailer stigma." It is, presumably, irrelevant to Arnold because it does not define what his own life is about.

The meritocracy story of hard work and future upward mobility was commonly told by younger adults of all races and ethnicities who had grown up poor or working class, and who had not yet experienced persistent problems, only modest beginnings.

### Identity Story

This powerful narrative lens was commonly invoked by participants who had made a conscious choice to move into a mobile home because it was in agreement with their current lifestyle or life stage, or generally their character. A good example is given by Fred and Pamela, a White married couple in their fifties, also living in "Happy Place."

Marc: Have you ever lived in a mobile home park before?

Fred: Oh yes. Lived in one in Colorado, lived in one in New York...I like mobile home parks because you're right in between a house and an apartment. You don't have your neighbors right up your nose all day long. And yet, you don't have all the major maintenance of a house. It cuts down on maintenance, plus it gives you a little bit of space. Ah, most people don't like mobile homes, but I really do. They fit my lifestyle really good.

Pamela: Yeah. It's kind of like you got that nature right, right there. So you don't feel like this [is the] urban jungle.

In this interview excerpt, Fred and Pamela describe how mobile homes are a good fit with their lifestyle and personal preferences. Like Arnold above, Fred downplays the bad reputation of mobile homes. It does not seem to matter to him, one way or the other, because he has already defined mobile homes as ideal places for him and his wife.



Fred's version of the identity story resembles those of other middle aged people, mostly Whites, who describe themselves as "country people," independent-minded folk who enjoy living in slower-paced settings, surrounded by nature and people like themselves. For instance, when asked about the people in her community, Donna, a divorced White woman in her fifties living in a rural park we called "Countryside Village," told us that they are "country people, good people, regular old-fashion type people" like herself, and that she loves living in her community.

Finally, variations of the identity story are commonly told by senior mobile home residents who have moved to Florida from colder climates for retirement and who have typically completed successful careers and downsized from larger, site-built homes.

### Truth Story

The fifth and, for now, last cultural story is occasionally told by adults who have experienced challenges in life, including downward social mobility. These people believe that living in a mobile home community, despite not being a choice, is leading them to a new kind of "truth." The truth story says that all things happen for a reason: a higher being or just "life itself" puts people in places where they need to learn, or remember, what is essential in life: family, community, or other virtues and values, as opposed to material possessions and social status.

An example is provided by Javier, a Latino immigrant in his thirties, married with three chil-

dren and currently living in a predominantly Latino community we call "Siesta Club." Javier grew up solidly middle class and enjoyed success as a business owner in the United States before his family experienced health and economic problems.

I have friends that are telling me...why didn't I just move to an apartment house or purchase another home? And what I tell them is the following: I've had everything I've ever wanted in life and I've lost that. And to have almost lost my wife has definitely taught me something different, as far as what's valuable in life. And to be honest with you, I don't value materialistic things any longer, or I don't see materialistic things the way I used to. So, living in a community like this, I don't see it as a step down, I don't see it as an issue of improving: I see it as an opportunity to be closer to my family. I feel that my relationship with my neighbors is far...more of a warmth, a warmer feeling than living in your private home, where your next door neighbor is 40 to 50 feet away from you. I don't mind it at all.

In the excerpt, Javier emphasizes that he no longer values possessions, at least not as much as he used to, and he no longer cares about social prestige, he is indifferent to it (not a "step down," not "improving"). Having gone through serious crises has taught Javier to find contentment in living in a community where he can enjoy quality time with his family and find meaningful, even "warm," relationships with neighbors, meaning things that are "valuable in life."

There are several versions of the truth story some of which are supported by explicit religious or spiritual beliefs. What these variations have in common is that they provide a different, maybe even alternative, system of values and rewards for life experiences and social positions. This

particular narrative lens may not be as common as others in contemporary American culture, yet it has a firm place in today's deeply religious United States and provides a strong interpretive frame for feelings of happiness and connection (Wilkins 2008).

### Discussion

On the whole, the (thus far) five cultural stories I distilled from the larger data sets contain symbolic and emotion codes that are part of contemporary American culture. These narrative molds are blueprints for cognitive and emotional evaluations of self and others. They provide resources for people to use while making sense of their own and others' identities, experiences and life trajectories, and generally the surrounding social world.

Interestingly, a range of such stories was adopted and told by people who are often defined by outsiders as one "kind of person" – those who live in mobile homes, an explicitly negative stereotype which is even wielded by some mobile home residents themselves. In other words, I meant to show that there are considerable disjunctures in how demographically similar people experience, cognitively and emotionally, similar social and physical environments.

My above descriptions might suggest that cultural stories can be loosely associated with more specific social characteristics: locations of age and/or life stage; of race, ethnicity or immigration; of *relative* social status in comparison with significant others or previous personal experiences. More analytic comparison within and across data sets remains to be done in order

to establish firmer links between the adoption of specific cultural stories and specific social characteristics. Yet, I do not believe that one of these variables can, or should, be reduced to the other. Social locations and characteristics, even specific ones, are important background factors, yet they do not determine or predict how and what people borrow from the cultural toolkit to make sense of the world and their place within. As Brown-Saracino succinctly states in her analysis of the "wildly divergent and even conflicting cultural orientations" among demographically homogeneous gentrifiers: "we must resist the temptation to turn to demography, rather than to ideology and cultural practices, as a primary marker of ideological alignment" (2009:212).

The same argument is expressed in a recent article by Salcedo and Rasse (2012:104f.), who found an "enormous diversity in experiences, values, expectations and lifestyles" among the urban poor in Santiago de Chile, which could not be further reduced to social structures and locations. Their analysis of the "narratives of upward social mobility" and "narratives of expectations for the future," which oscillate between "optimistic" and "pessimistic," provide cognitive and emotional frames for situated experiences and partially resemble the stories I described here.

If one finding stands out, it is the high degree of agency and creativity, as opposed to victimhood and external definition, which most study participants displayed in finding, embracing, personalizing, and embellishing available life stories. However, and this is the key point of this section, we should not mistake the nuanced

variations of *cultural* patterns with a need to resort to individual-level, psychologizing conceptions and explanations of emotion.

### Some Analytic Comments

As seen and described, my main data sets are rather traditional within qualitative research. They consist of ethnographic observations, in-depth interviews, and a hybrid of the two which I have previously called “go along” (Kusenbach 2003; 2012). I analyze my data in traditional ways, first by immersing myself in the rich details and then abstracting increasingly general patterns, some of which firm up and hold steady when confronted with more data – a process that is often called “grounded theory” or “analytic induction.” Both traditional datasets and analytic methods are commonly used in contemporary studies of emotions (for examples, see Meanwell et al. 2008). However, what I suggest to be uncommon are the kinds of questions asked during analysis, and the places in the data where one looks for relevant information.

We need to take seriously the idea that some very important aspects of emotion are not readily observable in situated contexts. One reason is that certain emotions, such as feelings of home or belonging, are complex, multi-layered, and most often simply too frequently taken-for-granted to be noticed or explicitly “managed” in daily life. And second, even though all emotions are felt and expressed by specific individuals in specific places and times, often in interaction with specific others, their origins and larger meanings transcend situations.

In the mobile home research, we were rarely able to *see* how participants felt about their homes and neighborhoods because these feelings were not explicitly or unambiguously expressed in their actions and other personal manifestations, at least not routinely so while my research assistants or I were around. Further, presumably because of their comparatively muted occurrence in the drama of daily life, study participants did not have very much to *say* about these kinds of feelings when asked directly. Observations and accounts of situated emotional experiences, therefore, did not yield thick data or allow for deep analytic insights, even though there were many clues in the fieldwork suggesting that interesting and meaningful emotional processes were going on.

During the fieldwork, we noticed, however, that study participants liked to show and tell us about how they have improved and personalized their homes since moving in. They freely spoke about how they grew up, what they did for a living, how they came to be where they were, what they were hoping to do, and where they were hoping to be, in the future. Many enjoyed talking about being “normal” people, “average” Americans, and having a special knack for tackling problems and accomplishing goals on their own. It was in these rather peripheral parts of the interviews, in the informal conversations that often took place before and after interviews, and during home tours or accompanied walks through the neighborhoods, in which participants conveyed their emotional orientations regarding home and community most clearly and nuanced, often embedded within larger, situation transcendent stories.

I suggest that hearing these stories as an important component of emotions depends on a *widening* of the analytic lens that is most commonly applied in contemporary studies of emotion. It requires asking new kinds of questions that go beyond the minutiae of how emotions are experienced and managed, and it may at times require innovative understandings of what kinds and sources of data are most relevant.

### Conclusion

Throughout his analysis of “How Emotions Work,” Katz emphasizes the larger, situation transcendent contexts that give shape and meaning to emotional experience:

[t]he narrative dimensions of people’s lives are easily neglected when socially situated interaction is analyzed. In the study of emotions, sociological research that neglects people’s trans-situational concerns often becomes a sterile examination of how people represent their emotions, express their dispositions and indicate what they are feeling. Such studies fail to address the origins of what is distinctive in emotional experience. (1999:324)

Here and elsewhere, Katz highlights the importance of narratives and life stories for making sociological sense of how individuals experience emotions. We agree that narratives, and especially the intertwining of personal, subcul-

tural, and cultural stories, are essential in understanding emotion.

In addition to asking *what* it is that individuals actually experience when they feel, and instead of looking for the origins of feelings deeply inside their psyche, we need to more seriously consider questions about the *social* nature, origins, and consequences of emotion. The social is the primary construct employed by our colleagues researching topics related to emotion, such as identities, knowledge, or institutions, and it should centrally guide sociological inquiries of emotion. In many ways, studying emotion is like studying the grammar and words of a language: people would not be able to speak, and (as many sociologists believe) even think, without a language. Likewise, we argue that people would not know *how* to feel without emotion codes which are embedded in larger cultural systems of meaning – some variations of which were called “life stories” above.

Our call for more attention to the *social* aspects of emotion resonates with other calls for more comparative, historically informed, and generally situation-transcendent ways of studying social life in other, long-standing domains of qualitative research. Ultimately, returning to, or elevating the social in these and other domains of study promises new links between the micro and macro social worlds of meaning.

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## The Communicative Construction of Reality and Sequential Analysis. A personal reminiscence<sup>1</sup>

**Abstract** This paper presents a historical view of the emergence of what is known as the communicative paradigm. Through a personal reminiscence of his long career, Thomas Luckmann entangles the main sources of what was a radical shift of the role of language and communication in the humanities and social sciences. In doing so, Luckmann shows that the epistemological and ontological assumptions on which the contemporary study of social interaction and communicative processes rely were practically non-existent half a century ago. While sociology and linguistics seemed to exist in separate universes during Luckmann's student days, a dialogical approach to language and social life eventually appeared – for example, in ethnomethodology, conversational analysis and French structuralism – and laid the foundation to the (today taken for granted) idea that social realities are the result of human activities. Human social reality and the worldview that motivates and guides interaction are mainly constructed in communicative processes. If social reality is constructed in communicative interaction our most reliable knowledge of that reality comes from reconstructions of these processes. Such reconstructions have been greatly facilitated by technological innovation, such as tape- and video-recorder, which, alongside theoretical advancements, may explain the timing of the communicative turn. Finally, this paper marks the benefits of sequential analysis in enabling us to trace step-by-step the processes by which social reality is constructed and reconstructed.

**Keywords** Communicative Paradigm; Sociology; Linguistics; Social Realities; Sequential Analysis

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It stands to reason that the humanities and social sciences are more profoundly embedded in the society and culture of their time than the physical sciences. To be sure, all scientific activity is situated historically and culturally, but the humanities and social sciences are so situated in an additional sense. Their medium of communication is a particular language rather than

<sup>1</sup> My presentation is partly based on a lecture given at the symposium in honor of Per Linell in 2004 at the University of Linköping, and a later version presented at the University of Prague in 2009.

a universal algebra, and a particular language also constitutes the human reality investigated by them. They are, therefore, reflexive disciplines in a sense the physical sciences are not, and they are more directly influenced by the worldview of the society in which they are located. In their striving for objectivity and systematic accumulation of knowledge the humanities and social sciences must reckon with this inevitable circumstance.

When investigating and interpreting the history of national literatures, the organization of their local societies, laws, and economy, the humanities and social sciences tend to distinctly exhibit – in addition to paradigmatic traditionalism – particularistic traits. Even the disciplines that try to penetrate language and social life as universal aspects of the human condition, such as anthropology and sociology, tend to suffer from the same weakness. Modern social theory and the modern theory of language provide good examples for this observation. During the early stages of their formation, the major scholarly traditions of these theories, French, British, German, American, and Russian, followed somewhat different paths. Nonetheless, they did have two things in common beyond their subject matter. Contrary to what one would expect, and with few exceptions, they shared a lack of interest in the older traditions of the philosophy of language and social philosophy. Less surprisingly, they also ignored one another. One notable exception at the beginning of the twentieth century was the Durkheim-Meillet collaboration at the *Année Sociologique*, another, more general one, could be found in German and American ethnology. However, in these two countries ethnology was less closely connected to sociology than in France.

The mutual avoidance of sociology and linguistics is rather difficult to explain. After all, a systematic connection between the theory of language and the theory of society had been proposed by Wilhelm von Humboldt in the early decades of the nineteenth century.<sup>2</sup> For various reasons, Humboldt's thought exerted only a limited influence during the nineteenth and the first half of the twentieth century. Traces of his thinking can be found in the mainly German investigations of semantic fields, and in the American Sapir-Whorf simplification, even distortion of Humboldt in the so-called linguistic relativity hypothesis. Both had a static, correlational outlook, quite the contrary of Humboldt's emphasis on language as communicative process.

The situation changed strikingly in the relatively short time of my academic career, from when I was a student to this day. As a living witness to this change, I may be allowed to reflect on these changes in a personal perspective. Looking back, I feel that I am justified in saying that the change was profound; with some slight exaggeration one might call it a paradigm shift. I can testify to the fact that here is a world of difference between what was taken for granted in my student days in linguistics and in sociology, as well as social psychology, and the assumptions on which we rely today in the study of social interaction and communicative processes.

In the late forties, when I began studying comparative linguistics in Europe, the dominant ap-

<sup>2</sup> The introduction to his study of the Kawi language, *On the Diversity of Human Language Construction and its Influence on the Mental Development of the Human Species*, was published posthumously in 1836. Humboldt, although no dwarf, was standing on the shoulders of giants, Vico, the Earl of Shaftesbury, Hamann, Herder.

proach was either philological in the old sense or what appeared as abstract structuralism to an impatient student who was looking in vain for *la parole* in the study of *la langue*. Arriving in the United States of America at the beginning of the fifties, I still took a Master's degree in philosophy, but then switched to sociology. As a student of Alfred Schütz, I was spared indoctrination in structural functionalism, which appeared to me to be just as far removed from social life as the dominant trend in linguistics seemed removed from the uses of language. Structural functionalism, as the widely accepted theory of society, and structuralism, as well as, somewhat later, generative grammar as the reigning approaches to language, seemed both static and abstract, remote from social reality and human communication. To use Humboldt's own terminology, they were concerned with the *εργον* [ergon = work] rather than the *ενεργεια* [energeia = energy] of language and social life. Given the nature of the reality they studied, I was also disappointed to see that sociology and linguistics were not closely connected, in fact, it seemed that they existed in separate universes. Although I retained a strong interest in the uses and functions of language in human social life even after becoming a sociologist, I was struck by the fact that sociology in general, and even what then went by the label of a sociology of language, was linguistically naïve to the point of ignorance. At the same time, the notions of social interaction and social structure in linguistics, even in the budding sub-discipline of pragmatics, were of a very modest home-grown variety. Half a century ago, this state of affairs was taken for granted by most practitioners in the two disciplines.

It is not my purpose to detail the changes in the two fields and the concurrent rapprochement between certain, by no means negligible, parts of the disciplines involved. I should like to point to the main sources of the change, however, the shift to what has been variously called the communicative paradigm.<sup>3</sup> I am not quite sure how developments in linguistic pragmatics contributed to the change in the relationship of language theory and social theory. The verdict on a direct influence on empirical studies of Wittgenstein, who was much quoted, especially in some conversational analytic quarters, must remain open. So-called speech-act theory, also much quoted, was far removed from the realities of communication.

Another, somewhat older, source of this change, had a direct connection to Humboldt. Curiously enough, Humboldt's thought had not been neglected in Russia as much as elsewhere. The main proponent of Humboldt's thought was Aleksandr Potebnja.<sup>4</sup> Through him Humboldt's influence reached Bakhtin, the formalists, and Roman Jakobson. The Western "discovery" of Bakhtin-Vološinov's emphasis on dialogue and genre in their philosophy of language and culture decidedly contributed to a change in the prevailing orthodoxies.

The proponents of a dialogical approach to language and social life were no longer ignored. In

<sup>3</sup> My review of the sociology of language for the *Handbuch der empirischen Sozialforschung*, edited by René König, contains a relatively detailed account. The revised version of 1979 took note of many more changes in theory and research than my contribution to the first edition in 1969. Yet, while I reviewed the work of Vygotsky, Goffman, Gumperz and Hymes, Garfinkel, Sacks, Schegloff, and others, Bakhtin and Vološinov were still missed by me even then.

<sup>4</sup> Aleksandr Potebnja, *Mysl' i jazyk*, Moscow 1862 (see Lachmann 1997).

France, first in the work of Lévi-Strauss and then in that of Pierre Bourdieu, the seeds sown in the Durkheim-Meillet connection bore belated fruit. Another early source of the change, about forty years ago, was the program of an ethnography of communication proposed by John Gumperz and Dell Hymes. About the same time, the work of Alfred Schütz fed two other sources that directly and indirectly helped to establish the communicative paradigm in social theory. One was ethnomethodology and its offspring, conversational analysis, and the other was the "new" sociology of knowledge, with one of its offsprings, communicative genre theory. Suffice it to say that what you are thinking and doing today would have been unthinkable when Garfinkel was a student of Parsons, corresponding with Schütz, about the time when I was a student of Schütz.

What, then, are the assumptions which we take for granted in our investigations to such an extent that they appear trivial now, and which would have met with puzzlement or rejection then?

Social reality is not *simply* presented to observation, if observation is defined naturalistically. "Objectivity" and "measurement" in the social sciences do not mean precisely the same thing as in the physical sciences. The physical sciences seek to explain a cosmos which has nothing to say – except in a purely metaphorical sense. It is a world to be looked at, described, and explained "objectively." The social sciences, on the other hand, investigate a world which *has* something to say, which, in fact, was saying something long before there were any scientists listening. The social world is naturally artificial, to use a term introduced by Helmuth Plessner.<sup>5</sup> The

<sup>5</sup> See: Plessner (1964; 1975[1928]).

traditions of life by which human societies are organized are an inter-subjective accomplishment. They are the "naturally artificial" result of long chains of interaction by "naturally artificial" human beings.

Although the human species did evolve naturally, of course, the human social worlds are not a direct evolutionary product; they are the products of something that *emerged* from evolution and is subject to its own level of causation. Human *history* is self-made. Traditions and institutions are not genetic programs. They are constructed in social interaction, and once they become established in the collective memory of a society, they are again transmitted in interaction. Traditions and institutions may appear less tangible than buildings and artifacts, but they are equally real.

In sum, historical stocks of knowledge and historical institutions are constructed, maintained, transmitted, transformed, and occasionally destroyed in social interaction. Obviously, social interaction is more than individual action, but it presupposes individual action, action that is meaningful to those who engage in them, whether it leads to results that were intended, or, painfully, when the consequences of interaction differ from those that were originally anticipated.

Evidently, the meaning of individual action is essentially subjective – yet, most of it is derived from social stocks of knowledge, which are built up in communicative social interaction. Social interaction consists of coordinated, interlocking individual actions. Individual action, in its turn, presupposes intentional activities whose meaning is mainly derived from social stocks of knowledge.

The idea that social reality is a human historical “accomplishment” is not new. It was anticipated by a long line of philosophers and historians, from Aristotle and Thucydides, to Vico, Montaigne and Montesquieu, and further to Adam Smith and Marx, to mention the most important ones. Nonetheless, a comprehensive formulation of this idea had to wait until the twentieth century. Max Weber, and after him Alfred Schütz, and after them others, among them Peter Berger and I, took up the main epistemological and methodological issues connected with it. Building upon their views, I shall look at the relationship between individual and collective levels of reality, at the links between action, knowledge, and the communicative construction of social worlds.

Before coming to that, I may briefly anticipate the basic methodological conclusion from these observations. Apart from the details of the epistemological question *how* “data” are to be constructed, and the ontological question from *what* they are constructed, there should be a basic agreement on the basic principle of ontological realism: that social realities are the result of human activities. The data of the social sciences are derived from these realities. Because they were constructed in meaningful social actions in a historical social world, they are to be *reconstructed* as data for the social sciences in a way that preserves rather than destroys their essential meaningfulness and historicity.

Nowadays, after the long dominant “positivist” era has come to an end, it seems to be widely accepted that “data” are “facta.” This means that “data” – whatever reality they may represent – are acknowledged to be communicative constructs. Given the peculiar nature of social reality

it is hardly surprising that there is no generally agreed answer either to the epistemological question precisely *how* or to the ontological question from *what* the data are constructed. Controversies about the way these questions should be answered were acute in the social sciences. However, the realistic position is that “data” are communicative constructs of the investigators based upon direct or indirect, for example, instrumentally mediated, observation. What is observed, however, are not simple, purely behavioral facts but social interaction, both direct and indirect, and its historical results.

Not all human activity is communicative in the usual sense of the word. Animals are hunted, fields are tilled, shelters are built, children are nurtured, enemies are fought. Yet, as these simple examples show, even what is not primarily communicative interaction, is usually facilitated and accompanied by it. Human social reality and the worldview that motivates and guides interaction *is* mainly constructed in communicative processes.

Reconstructions of social reality, a particular kind of data-producing activity in social science, are communicative acts by definition. Reconstructions are, of course, not restricted to the social sciences. They are a highly important communicative activity on the primary level of social discourse.<sup>6</sup> Reconstructions of past events feed the collective memory of families, social groups and classes, institutions and entire societies.

If all that seems obvious, even trivial today, I should like to emphasize that it was anything

<sup>6</sup> See: Bergmann and Luckmann (1995).

but that a little more than a generation ago. I have already tried to indicate the foremost theoretical reasons for the changes that led to the emergence of what has been called the communicative paradigm in sociology.

If social reality is constructed in communicative interaction, and if it is pervasive in social life, our most reliable knowledge of that reality comes from reconstructions of these processes. However, an elementary difficulty with the analysis of communicative interaction, as of all social interaction, is the transformation of communicative processes into data susceptible to analysis.<sup>7</sup> This difficulty may explain why in the social sciences data of a different kind were preferred. As against the fleeting processes of interaction and communication their quasi-objective products appeared stable, thus, permitting both unhurried and verifiable analysis. The methodological preference in social science for art and artifacts, actuarial statistics and registers, documents, and other “material” objects, and for codable answers to interview questions was based on the assumption that action processes were beyond exact description and that the subjective components of ephemeral events were not objectifiable. The methodological bias which arose from the technical difficulty in pinning down the processes of social interaction came to distort the theoretical view of human reality.

Interestingly, the last link in the chain of events that changed markedly the assumptions and practices in the study of society and language during my own lifetime is not represented by a theoretical advance but by a technological innovation.

<sup>7</sup> In the following I use some passages from my paper on the interpretation of dialogue (Luckmann 1999).

A precise analysis of the processes of social interaction, in which all the various material and immaterial components of social reality are constructed, depends on the possibility of “freezing” these processes for later, repeated inspection.<sup>8</sup>

Possibility became fact less than a hundred years ago.<sup>9</sup> However, systematic social science use of the developments, which permitted auditory and then also visual recording of such processes, began much later. The analysis of the products of social interaction, from food, clothing and tools, factories, churches, jails, and cemeteries to legal codes, birth registries, music scores, and literature, certainly continues to be essential for an understanding of social reality. After all, they are what human communication and interaction is *intended* to produce. However, in the past decades, taking the new technologies for granted, we have been in an increasingly better position to direct our efforts to an analysis of the “production process” in relation to the “product” and in relation to the “consumption” of the “product,” that is, to an analysis of interaction and dialogue both as a part of social reality and as a source of much of social reality. And, technological innovation continued to add to the arsenal of instruments by which the widest imaginable variety of social interactions could be recorded, providing the data for sequential analysis.

This is the enterprise in which many of us were and are involved. The pioneer, Harvey Sacks, inspired a notable group of followers and successors.

<sup>8</sup> Cf. Bergmann (1985).

<sup>9</sup> Just about a century ago, one of the first uses, if not the first, of a phonograph was made for recordings of Montenegrin heroic epics, as a source of comparison for Homer’s *Iliad* and *Odyssey* (cf. Parry 1930; Lord 1960).

Later, communicative genre theory used sequential analysis. As the data and most of the publications of the latter enterprise, in which I was active for many years, are in German, and translation of primary data of this nature is almost impossible, the results of our investigations did not reach monolingual investigators elsewhere. The international dialogue study group at the Reimers Foundation in Bad Homburg and those involved in the now regrettably defunct center of communication studies at the University of Linköping fared somewhat better. However, the study of the widest range of social phenomena using sequential analysis continues to be undertaken, as I noted, in the homeland of that method, the United States of America, and also in

its second home, Great Britain, for example, in pioneering work of political rhetoric and the equally well-known studies of work.

Let me conclude: Sequential analysis is not the only so-called qualitative method – how ill-conceived that term is! – and, qualitative methods are not the sole salvation of sociology. Yet, I am convinced that sequential analysis provides the empirical foundation for an essential component of contemporary social theory, in particular for one of its branches, the sociology of knowledge. It enables us to trace step-by-step the processes by which social reality is constructed and reconstructed. And that is not a minor matter.

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## What Counts as Qualitative Research? Some Cautionary Comments

**Abstract** Many PhD students begin as unconscious Naturalists or Emotionalists using interview studies to report people's "experience" of an unquestioned social "problem." An analysis of articles in one journal shows that this naïve use of interview data has become the common currency of qualitative research. In a critique of one such article, I show how interview studies may simply reproduce interviewees' own accounts, glossed over by a few social science categories. By "mining" interviews for apposite extracts, such researchers lose sight of how *sequence* is consequential for what we say and do. Much more needs to be done if qualitative research is not to be just a set of techniques but an analytic project, different from journalism.

**Keywords** Qualitative Research; Interviews; Data Analysis; Social Theory

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This paper has an unusual genesis for a journal article. It began life as a presentation at a Meet the Author session at a conference. In this paper, I link an account of what I say in a new edition of one of my books (Silverman 2011) to a more general discussion of the present state of qualitative research. The comments made by colleagues in other plenaries (published in this volume) make me optimistic about our field. However, as I show in a critique of a recent journal article, all may not be so well.

I will begin by briefly explaining just what is different about the fourth edition of *Interpreting Qualitative Data* (IQD). It is intended as an undergraduate introductory qualitative methods text which complements the postgraduate focus of *Doing Qualitative Research* (Silverman 2013). IQD is not simply an undergraduate research project book but an introduction to the theory, methods, and practice of qualitative research. This is reflected in three chapters new to this edition.

A chapter on research design seeks to demonstrate the challenges faced by the students in carrying out a small research project and to offer some simple solutions. This is complemented by a chapter on data analysis which deals with the nitty-gritty issues of confronting data for the first time and contains sections on contemporary approaches to data analysis, including grounded theory and narrative analysis. Finally, a new chapter on focus groups offers detailed discussion about how to analyze focus group data.

The underlying philosophy of the book is, however, unchanged. It can be summed up in the following way:

- Qualitative research is not simply a set of techniques to be slotted in to any given research problem;
- This means that it is important to concentrate on data analysis rather than simply data gathering;
- In particular, at the very start of qualitative research, analytic issues should be to the fore. Contrary to the common tendency simply to select any given social problem as one's focus,

I try to demonstrate that research problems, at any level, need to be analytically defined;

- However, this does not mean that we should unthinkingly follow the quantitative model of prior hypotheses, based on pre-defined variables. In qualitative research, it is often best to gradually work towards a topic by confronting data with questions about the "whats" and "hows" of interaction;
- My position throughout derives from a *constructionist* stance informed by a refusal to accept taken-for-granted versions of how the world is put together and an attempt to reveal what is extraordinary about the ordinary features of everyday life.

Other plenary talks (published in this volume) show that I am not alone in making these claims. In their discussion of research on "emotions," Margarethe Kusenbach and Donileen Loseke document a movement away from figuring out the states of individuals' psyches towards a concern with how "emotions" are constructed in naturalistic environments. This is complemented by Holstein and Gubrium's (2011) refusal to treat interview data as simple reflections of states of mind and their insistence on the study of the social organization of interview talk in the context of its "scenic" resources. As Thomas Luckmann demonstrates, such an anti-psychologistic perspective derives from the turn towards mundane language originating in the work of Alfred Schütz. This turn is evidenced in Paul Atkinson's suggestion that we reconceive apparently "small" happenings as extraordinary events with complex choreographies.

It will at once be apparent that these positions are in some respects antithetical to many traditional conceptions of good research practice. Sticking to any given research design, while standard good practice for our quantitative colleagues, usually is insufficient if we are seeking to pursue answers to the question “What is going on here?” To document properly the choreography and scenic resources of any milieu, it is usually necessary to look out for new cases and new sources of data while we are in the field. This is why Malin Åkerström emphasizes how good qualitative research projects regularly twist and turn, reconceptualizing their research question and appropriate data.

Despite the consensus I have described, it would be wrong to suggest that qualitative researchers agree about their craft. Our field is undoubtedly “pre-paradigmatic” in Thomas Kuhn’s sense. The Constructionist position recommended here is routinely contested by Naturalists and psychologically-oriented Emotionalists who appear uninfluenced by the linguistic turn or even unaware of it (see Gubrium and Holstein 1997).

And, if we focus on students writing ostensibly “qualitative” dissertations, the picture is often very disconcerting. For the past twenty years, I have run workshops for such students on four continents and in many social science disciplines. The overwhelming number of my students seems unaware of the theoretical basis of their approach. Most are unconscious Naturalists or Emotionalists who usually use interview studies to report back on people’s “experience” of an unquestioned social “problem.”

My evidence is, of course, anecdotal. But, as Barbara Czarniawska points out (2012), it fits with

the gap between what established researchers can do and the limited resources of research students, and the considerable constraints upon their work. Many of the latter are registered within Departments with few staff with qualitative backgrounds and/or a model of scientific research, which demands pre-designed research questions, measures, and hypotheses.

Of course, the institutional context of student research extends beyond Departmental cultures. What do students see when they turn the pages of social science journals that specialize in qualitative research?

A few years ago, I did a quick survey of one such journal. I looked at the nine issues of *Qualitative Research in Organizations and Management* appearing in 2008-2009. Of the 18 research articles published there during that period, 16 used interviews, one was based upon focus group data, and one analyzed document. Despite the relevance of naturalistic data for qualitative research (e.g., Potter and Hepburn 2007), this supports the contention that open-ended interviews are the default data of choice for most qualitative researchers.

Of course, as we all know, there are no such things as “good” data. In principle, there is no reason to reject interview data since everything depends upon your research question. However, ultimately, one looks for intelligence and critical reflection in how any data are *analyzed*.

In this respect, these sixteen interview studies were very disappointing. Fully, fifteen of this sample treated their data as a simple window on experiences. For these researchers, apparently, the linguistic turn never happened. The exception

was a paper which programmatically suggested interviews might be treated as situated accounts and hence, adopted a Constructionist stance. Unfortunately, this only made me wonder why editors of an academic journal published near the end of the first decade of the twenty-first century would find such a suggestion newsworthy.

This meager evidence goes a little way to sketching some of the institutional context in which apprentice qualitative researchers are reared. However, there is a broader *cultural* context, which may shape how they view “good” research. Some years ago, Paul Atkinson and I argued that we live in an “Interview Society” (Atkinson and Silverman 1997). In contemporary societies, the interview is seen as the default mechanism through which we can understand another’s thoughts and emotions. Think of how reports of crimes seem incomplete without an interview with victims or bereaved families. Think back also to the recent London Olympics. Television coverage extended far beyond the athletic performances. Indeed, many networks devoted much more time to biographic conversations with athletes and their families and pre- and post-event interviews dominated by such questions: “What were your emotions?” Even before the athlete spoke, breathless TV commentators would speculate: “What must (s)he be feeling?”

What is going on here? First, for interviews to work like this, we must think of ourselves as discrete individuals with personal experiences and goals. This emergence of the self as a proper object of narration may be a relatively modern phenomenon. For instance, in feudal or aristocratic societies, one was primarily identified through membership of a collectivity (e.g., peasant, aristocrat, etc.).

Second, the interview also demands subjects who are happy to confess their innermost thoughts and emotions to the appropriate professional. Today the professional who receives their confession is no longer usually a priest but a therapist or media interviewer.

Third, the Interview Society requires mass media technologies and myths, which give a new twist to the, no doubt, perennial polarities of the private and the public; the routine and the sensational. Judging by the bereaved family members who regularly appear on our TV screens, such technologies and myths generate subjects who are not only happy to confess but seem to feel that their once-private emotions are somehow validated when revealed to a media interviewer.

I suggest that this Interview Society may be the hidden backdrop to what generally counts as qualitative research. A few years ago, I came across an advert asking for applications for a research post on a study of “how psycho-social adversity is related to asthma morbidity and care.” The text of the advert explained that this problem would be studied by means of qualitative interviews. My immediate question was: How can qualitative interviews help to address the topic at hand? The problem is not that people with asthma will be unable to answer questions about their past nor, of course, that they are likely to lie or mislead the interviewer. Rather, like all of us, when faced with an outcome (in this case, a chronic illness), they will document their past in a way which fits it, highlighting certain features and downplaying others. In other words, the interviewer will be inviting a retrospective “rewriting of history” (Garfinkel 1967) with an unknown bearing on the causal problem with which this research is concerned.

This is not to deny that valuable material may be gathered from such a qualitative study. But, rather it suggests that data analysis should address an altogether different issue – narratives of illness in which “causes” and “associations” work as rhetorical moves.

By contrast, a quantitative study would seem to be much more appropriate to the research question proposed. Quantitative surveys can be used on much larger samples than qualitative interviews, allowing inferences to be made to wider populations. Moreover, such surveys have standardized, reliable measures to ascertain the “facts” with which this study is concerned. Indeed, why should a large-scale quantitative study be restricted to surveys or interviews? If I wanted reliable, generalizable knowledge about the relation between these two variables (psycho-social adversity and asthma morbidity), I would start by looking at hospital records.

This asthma study seems to have been designed in terms of a very limited, if common, conception of the division of labor between qualitative and quantitative research. While the latter concentrates on data which shows people’s behavior, qualitative research is seen as the realm where we study in-depth people’s experiences through a small number of relatively unstructured interviews. This had led to what I perceive to be two blunders in the design of qualitative research. First, a failure to recognize that some research questions might be better studied using largely quantitative data. Surely, the causal question posed here can be better addressed via a questionnaire administered to a large sample of asthma patients or by a survey of hospital records to see if there is any correlation between an asthma

diagnosis and referrals to social workers and/or mental health professionals.

The second blunder is that the research design as stated appears to misunderstand the wide potential of qualitative research to study such things as the careers of asthma patients. Why can’t qualitative research study behavior? For instance, why not conduct an ethnographic study which observes whether (and, if so, how) doctors in hospitals and primary care facilities elicit histories from their patients relating to psycho-social problems? Why not study social work and hospital case conferences to see if such problems are recognized and, if so, what action is demanded? In short, why assume that qualitative research involves only researchers asking questions of respondents?

Moreover, the research design elects to present the main research question to respondents themselves. This causes two problems. First, as is well known in quantitative surveys, if respondents are made aware of your interests, this can affect their responses. Second, it can lead to lazy research in which careful data analysis is simply replaced by reporting back what people have told you.

As Clive Seale has pointed out:

[t]his is a very common problem in all kinds of studies, but particularly ones where people mistakenly use a qualitative design to answer a question better suited to an experiment or quasi-experimental design. People decide, say, that they are going to see if TV violence encourages violent behavior. Instead of doing a survey of what people watch on TV and a parallel survey of their tendency to violence, and then seeing whether there is a correlation (hoping that there are no spurious reasons for such a correlation of course), they just select a group of people and ask them (more or less) “do you think TV watching causes violence?” (personal correspondence)

One further example of an interview study will make my point. Laura Sheard (2011) was interested in the much discussed topic of female drinking and the dangers to which women were exposed when they went out to drink at night. She interviewed 40 women in the north of England about how they used spaces in the night-time economy and consumed alcohol.

This raises the issue of why one should prefer interview data. Sheard responds in this way:

[q]ualitative research places importance on understanding the social world through the perceptions, attitudes, and experiences of individuals. In-depth interviews represent one of the best possible ways in which to access the experiences, thoughts, and opinions of women on the sensitive topic of personal safety through the medium of a “conversation with a purpose” ... This method was chosen instead of other qualitative methods, such as focus groups or participant observation, as it was felt to be the greatest way of “mining” the richness and depth needed for a topic of this contextual, sensitive, and individualistic nature. (2011:623)

We might note how Sheard identifies qualitative research with individuals’ “perceptions, attitudes, and experiences” and consider how this common view neglects social organization. More relevant right now is Sheard’s version of interviewing as “mining.” What does mining look like in practice?

We can answer this question by looking at Sheard’s report. Here is an extract:

[b]eing alone and in alcohol-centered spaces was discussed by many women. Some would never go into a pub by themselves, even if they were meeting others. One woman would intentionally arrive 15 minutes late when meeting friends to avoid having to be in a pub or bar by herself. (2011:624)

Now consider the similarities between what Sheard says here and what a journalist might write about such interviews. In both cases, I suggest, you simply describe what people tell you that bears on the topic in which you are interested. For both journalists and many qualitative interviewers, what people tell you is treated as a (more or less accurate) report on people’s perceptions of your topic. And instances of what they say can be offered in support of your interpretation.

Here is one example. Sheard observes that: “[a] few of the older women interviewed believed their dislike or avoidance of being alone in a pub was related to age and generational differences” (2011:624). She cites the following interview extract in support of her observation:

#### Extract 1.

Interviewer: Would you ever tend to use spaces like pubs or bars or alcohol-centered spaces?

Participant: I do go out to the pub, but only with my husband. I’ve never been in a pub without somebody with us. I’ve never walked in on my own. I’ve never had a reason to. If I was meeting somebody it was always outside and then we would all go in.

Interviewer: Why is that?

Participant: I don’t know. Maybe it’s my age and thinking that women shouldn’t go in the pub by themselves ... Like I said, I’ve been in with my husband and my daughter, but not on my own. A lot of lasses do now though, don’t they? [Marie, 47 years, cleaner]

Source: Sheard (2011:624).

There are two points of note about Extract 1. First, this transcript lacks indicators of the pauses, overlaps, and stressed sounds that are part of everyday speech. So, we lose some degree of contact with how the participants made sense of each other’s talk.

Second, the information that Sheard provides in parentheses is deeply problematic. People can identify themselves by many more characteristics than name, age, and occupation, for example, marital status, sexual preference, leisure tastes, et cetera. So, in choosing the set of identifiers used here, Sheard is guiding her readers to a particular set of interpretations. This deflects attention from the actual categories that speakers themselves use.

Moreover, like so many qualitative interviewers, Sheard simply restates part of what her interviewee says using the participant's own terms (e.g., "age") mixed with social science categories (e.g., "generational differences"). She simply does not attend to the way in which we shape our answers in terms of the question asked and in relation to how the questioner has been identified (in this case, as a researcher).

Indeed, there may be something even more subtle going on in this extract. Notice how the Interviewer's first question can be heard as asking for a "description." When this answer is finished, she might have asked for another description. But instead she asks "Why is that?"

In everyday conversation, unlike courts of law, assessments of insurance claims or classrooms, descriptions often routinely suffice and are not challenged. To ask, as here, "why is that?" can, thus, be heard as a *challenge* to account for your behavior. And, interestingly enough, her interviewee responds *defensively* in the following ways:

- beginning with "I don't know" and then "maybe,"
- appealing to her age as a warrant for her account,
- implying that her behavior may be old-fashioned ("A lot of lasses do now though, don't they?"),
- inviting agreement to this assertion ("don't they?").

So, by "mining" her interviews for apposite extracts, Sheard, like so many interviewers, loses sight of how *sequence* is consequential for what we say and do. But, to her credit, in Extract 1, she has at least provided her readers with a relatively long extract which includes the interviewer's questions.

Elsewhere, unfortunately, Sheard reverts to simply providing answers without questions and using these answers in a purely illustrative way to support her claims about the data. This is shown in Extract 2.

#### Extract 2.

*Sheard's claim:* Press coverage and media reports of women being "drug-raped" were at the forefront of the minds of the women interviewed. Significant caution was practiced around consumption of alcoholic drinks in order to avoid becoming a victim of drink spiking.

*Her evidence,* as this woman explains:

I'm very cautious about my drink and where it is and not leaving it and it's the same thing if there are girls in the bar [when she is at work as a bartender] then I will say to them "don't leave your drinks on that pool table" 'cause it takes seconds, doesn't it? You can't one hundred percent protect yourself 'cause in the one second that you turn your back from the bar and turn back round then something could have gone into it. But, I think you just have to be very aware of who is around you and where your drink is. [Zoe, 22 years, bar worker]

*Source: Sheard (2011:627).*

Although we are given a fairly long extract of Zoe's talk in Extract 2, we simply do not know how Zoe's response is positioned in the flow of prior talk and, therefore, can only speculate about how she is shaping her answer accordingly. Moreover, as with Extract 1, I would argue that there is a problem in the information about interviewees provided after each extract. As already noted, there are endless ways in which we can describe our identity. When

researchers choose particular identity-characteristics (in these cases age and occupation), they neglect others (e.g., marital status, number of friends, siblings, etc). In doing so, they favor particular ways of interpreting what people are saying.

Ultimately, however, Sheard fails to answer the question about why, if she is interested in gender and the night-time economy, she did not use naturalistic data, for example, go out on the street and/or study what women write about their behavior on social media? The unanswered question she leaves me with is: What's so wrong with ethnography?

## Conclusions

Understandably, my arguments are sometimes interpreted as taking an anti-interview stance and recommending one narrow version of Construc-

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tionism implied by ethnomethodology and conversation analysis (CA). Let me respond in this way.

First, to recognize the importance of the sequential organization of actions (including conversation) does NOT mean that qualitative research can only properly follow CA.

Second, however, it does mean that, if you want to work with interviews or other kinds of manufactured data, you need to analyze sequences of talk and attend to narrative construction. Ultimately, qualitative research is not just a set of techniques but an analytic project different from journalism.

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## Ethnography and Craft Knowledge

**Abstract** The paper has twin themes: the creative work of ethnographic interpretation, and the ethnographic interpretation of creative work. Illustrated with reference to recent and current fieldwork on craft, dance, and opera, it suggests some ways in which the ethnographer might creatively engage with her or his chosen fields. It criticizes the current view of “grounded theory,” which is found to be far too procedurally driven, in favor of more creative explorations of data.

**Keywords** Ethnography; Art; Aesthetics; Creativity; Grounded Theory

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In keeping with the theme of the conference, I want to reflect on creativity in and out of the field in the conduct of ethnographic research. I will illustrate it briefly with reference to some of my recent and current fieldwork. In essence, my argument is this: Too much emphasis is currently placed on techniques and procedures of data collection and data management, which too often passes for “analysis.” The remarkable industry that has grown up around the ideas of grounded theory, or the almost equally large literature on the use of software for coding, are cases in point and their forms of textbook knowledge are in danger of swamping the global market in methodologi-

cal work, and – more dangerously – in helping to stifle the essential creativity in ethnographic work. Equally, I want to resist the notion that this creativity is equivalent to serendipity. The latter suggests something fortuitous, whereas I want to suggest that a certain kind of creativity is at the very heart of the ethnographic enterprise.

I am fascinated by a constellation of phenomena: skill, aesthetics, art, and craft. I am studying those things in a small series of studios: a glassblowing studio, a printer’s studio, and a potter’s workshop. I aim to add a goldsmith in the near future. I have also been writing about operatic master classes for young singers (Atkinson forthcoming [a]; Atkinson, Delamont, and Watermeyer forthcoming). I have also written about Argentine tango classes (Atkinson n.d.). I confess that this research is almost completely curiosity-driven, reflecting the privilege that comes with seniority. I shall return to this current work later. Although my own interest is entirely personal and curiosity-driven, it should also be acknowledged that there is a growing literature in this general area: see, by way of example, Calhoun and Sennett (2007), Grasseni (2007), Buszek (2011), Taylor and Littleton (2012). My small-scale, detailed, and intensive observations recall one or two classics in the genre (e.g., Harper 1987).

In contrast to the somewhat procedural and formulaic approaches I have just alluded to, not enough thought and attention are given to the nature and generation of ideas. Productive ideas are not born of inspiration, but they are not derived from procedures of data sorting and inspection either. Rather, they come from multiple interactions: with the field rather than with decontextualized data; with other social settings; with other

ideas; with other disciplines. In other words, *having* ideas and *using* ideas are themselves part of the craft of ethnography.

In my own case, ideas also come from desperation. Contrary to what one might glean from the methods literature, ethnographic data collection is rarely perfect, or even approximates to perfection. In discussing this with students I always like to use the analogy of Chicken Marengo. As you will recall, the dish subsequently known as Chicken Marengo was “invented” by Napoleon’s cook on campaign. He had to try to concoct something out of whatever he had available under campaign conditions. He found a chicken and a crayfish, and lo-and-behold – Chicken Marengo! So, rather than a smooth transition from research design, to analysis, to theory-building, I experience much more frequently the silent cry of “How on earth do I *make* something out of this?,” given that my data always seem incomplete, the analysis patchy, and the ideas sketchy. All being well, Chicken Marengo, or something like it, is the result. [Of course, complete failures are rarely visible.]

So, fear is one of the mothers of invention. And good ideas are not born simply from procedures of data manipulation. Unfortunately, a great deal of what is written and talked about research methods gives the wrong impression. In particular, that odd industry that flourishes in English-speaking literature – especially “grounded theory” – is potentially misleading. I suggest that not because there is anything very wrong with the basic idea of grounded theory. In essence, it conveys the cyclical, iterative character of sociological thought, the interactions between data and ideas, the emergent but purposeful nature of research design in fieldwork. But, the basic ideas, which are a very

good description of creativity in any kind of social research, have been turned into a series of formulae and procedures – which are more likely to be deadening rather than creative. [For examples of the now extensive secondary literature on grounded theory, see: Birks and Mills (2010); Bryant and Charmaz (2010); Charmaz (2012); Urquhart (2012).] Worst of all is the endless, repetitive emphasis on *coding* data. As if ideas were going to emerge from mechanistic trudging through one's data and repeatedly coding it. Now, do not get me wrong – coding can be a useful way of organizing one's thoughts. It is especially useful when sharing a data set among a research team – but it has very little to do with the real work of creative analysis. In previous publications, I have long linked this baleful tendency to the global influence of software for qualitative data analysis (see Atkinson, Coffey, and Holbrook 1996), and I remain convinced that the influence of such software has not been entirely benign. For recent examples, see Bazeley (2007); Friese (2011).

There is an abiding problem with a lot of work and pedagogy in the social sciences – textbook-knowledge. Now, I do not believe that fieldwork is dependent entirely on tacit skills. And, I do not believe that one cannot learn from all sorts of things and all sorts of people. But, there seems to be an over-reliance in contemporary methodological training and textbooks on dogmatic, simplified models. Too many writers, students, and researchers rely on crude versions of so-called “paradigms.” These are invoked as matters of faith and unreflective loyalty. All too often the adherents of so-called paradigms display little understanding of the actual research traditions they claim to represent. The results tend to be textbook

knowledge, consisting of lists, typologies, and definitions that have little or no relationship with the real inspirations of social research. [This is a recurrent problem in the social sciences: theory and method taught in isolation, with little reference to the practicalities and exigencies of real-world research.]

Let me give just one example of the sort of thing I dislike. Tavory and Timmermans (2009) published a paper in the journal *Ethnography* entitled “Two Cases of Ethnography.” They argue that, in essence, there are only two research strategies available to ethnographers – *grounded theory* and the *extended case method*. The proposition itself is clearly absurd, and so was the characterization of the two allegedly opposed paradigms. The extended case method was represented in terms of a theory-driven research strategy, while grounded theory was caricatured as entirely data-driven. The authors suggest that almost all sociological ethnography is informed by one or the other of these research strategies. This seems almost complete drivel to me.

In the world of real research, however, it is surely abundantly clear that it is by no means necessary to follow textbook knowledge of research methods. Did Clifford Geertz follow grounded theory or extended case method, to take just one example? Once posed, the question seems quite ridiculous. Indeed, have the most significant contributions to modern sociology or anthropology ever demonstrably been governed by such rule-governed research strategies? I think not. Think of the major studies you really admire and have been influenced by. Think of influential men and women whose ethnographies we repeatedly read. Were they constrained by grounded theory? Did

they celebrate the extended case study method? No. Moreover, although it is difficult to prove the negative, it is hard to think of many – if any – major studies that seem especially complex, subtle, or theoretically rich because the data were densely coded or sorted using qualitative data software.

What endures in sociological or anthropological literature is not governed by adherence to some set of procedures. The ethnographies that have had real significance over the years have, of course, been notable for the quality and density of their ideas, not because they have followed a particular set of procedures. So, are there strategies of generating ideas that are not based on obsessive coding of data? Yes. They are the sort of things that Howard Becker (1998) wrote about in his clever and witty book *Tricks of the Trade*. Let me illustrate a couple of them from my own work.

*Inversions.* Several years ago I conducted fieldwork in an opera company (Atkinson 2006). I spent months watching rehearsals and performances, hanging out in some of the opera company's departments (such as props and casting). My main preoccupation, however, was essentially dramaturgical: I focused on the day-to-day work of making an opera happen. One of my guiding principles was to take Erving Goffman and stand him on his head. Specifically, it was an attempt to take Goffman's dramaturgical metaphor seriously. Goffman's dramaturgical metaphor enjoins us to “study everyday life as if it were theatre,” but in general we know rather little about how the theatre, acting, and directing are actually accomplished. So, Goffman's formulation is a classic example of *ignotum per ignotius* – studying what we do not know through something we know even

less well. So, my mantra was “study the theatre as everyday life,” or “study the everyday life of the theatre.”

*Being literal.* Let me illustrate this from my current fieldwork on craft workers, and in particular my fieldwork on glassblowing. I have taken classes on glassblowing myself, and I have also spent time watching and photographing in a major glassblowing studio in London. The conventional literature on craft writes about embodied knowledge, often about the trained *hand*. Likewise, Sudnow's (1978) remarkable phenomenological account of piano-playing is called *Ways of the Hand*. So, let us be really literal about this: What about the feet? Obviously, you do not make pieces of blown glass with your feet. But, of course, the hand is useless on its own, it has to be part of a bodily gestalt. So, balance, posture, choreography also need to be thought about. So, I think about posture, the angles of the body, the rhythms of the body, the co-ordination of different workers' bodies in the confined space of the studio. So, we can think about the studio as a site of choreography – how glassblowers' work is co-ordinated, how the workers move, and how they develop physical rhythms of movement. In themselves these remarks are not very profound, perhaps, but I find them productive and suggestive – pointing towards what Herbert Blumer called “sensitizing concepts,” what he also called “directions along which to look” (see Atkinson forthcoming [b]).

Of course, we cannot have good ideas in a vacuum. So, there is always a need for awareness of literature, including as thorough a sense as possible of research traditions. Also, reading ethnographies is a discipline in its own right. I was brought up in the anthropological tradition as an

undergraduate student: it was assumed that one would always have a monograph as one's general reading – by the bed as it were. Without thorough reading one cannot hope to have the right repertoire of ideas for observations and insights to bounce off. Thinking creatively can also be nourished by reading creatively: especially reading widely, well beyond the confines of one's narrow specialism.

So, what is the difference? I think that in methodological literature, too much stress on procedures, and too much on principles of manipulating data, and not enough about *ideas*. I suppose another way of putting it might be that the original inspiration of grounded theory has got obscured. Indeed, a sensitive reader might well object that despite my earlier comments about the dead hand of grounded theory, the general thrust of my remarks – on creative thinking, and the dialogue between ideas and data – is precisely what Glaser and Strauss (1967) intended in their original formulation. And, that is quite right. What I am advocating here is a recognition that the true inspiration of Glaser and Strauss is not captured by methodological formulae, obsessive coding, and inductive reasoning.

So, let us see what I might mean. There is nothing original in these observations, incidentally – just some things that I think need to be reiterated, stated once again rather than discovered or stated for the first time.

In essence, I think we need to concentrate on turning small happenings into big ideas and big ideas into local phenomena. In other words: as ethnographers, we are always dealing with the local, the contingent, small-scale events. We need

always to be thinking how we can translate or transform those phenomena into bigger issues, wider theories, continuities with other strands of social-science, and so on. When I say “big” ideas, I do not mean that we should be constantly searching for the grand narratives of social theory. I certainly do not mean that we should only be thinking in terms of global social processes are sweeping cultural change. What I mean is *generic* ideas that transcend local, specific, contingent phenomena, and that generate analytic ideas, such as ideal-types.

As already outlined, I am studying various sites of performance and artistic production. I have written on opera rehearsals already, and have also written about master classes for young opera singers. I have been spending time in a glassblowing studio. I have also spent some time observing the work of a potter and a printer. I have learned to dance tango. I intend to do more work on tango, and to incorporate some fieldwork with goldsmith. Now, I don't need to wait on a grounded theory process of coding large volumes of data in order to know *why* I am working on these things. First, I am interested in aesthetics. I happen to believe that too few sociological or anthropological studies have taken serious account of the ethno-aesthetics art worlds and of artistic work. Like a lot of social science, sociological studies of art worlds tell you lots about the social world, but very little about the art. Equally, a great many sociologists today like to write about performance and performativity. Often what they have to say is rather vacuous. And it is not necessarily based on any detailed, concrete acquaintance with actual performers and performances.

So, my interest in these settings is not gratuitous. It is driven by a clear sense of sociological or anthropological ideas. Moreover, I do not need to pore over detailed data in a purely inductive way in order to find cross-fertilization. I have already talked about the choreography of the glass studio. And I suggested that that derives from taking the idea of the ways of the hand and transforming it with a sort of perverse logic. But, of course, it also comes from my thinking about tango. What is literally choreography in the dance studio becomes metaphorically choreography in the glass studio. In much the same way, the opera master class might make me think about the Italian concept of *sprezzatura* – the apparently effortless performance. And, in turn, that makes me think what *sprezzatura* might look like in the glassblowing studio (for example).

In tango, as in many such settings, there is a discourse of authenticity. This is not least focused on the authenticity of tango in and from Argentina, as opposed to its “ballroom” version. The former is an improvised dance, and a social one. It is grounded in the social obligations and etiquette of the *milonga* in Buenos Aires and beyond. [The *milonga* is the social event at which tango is danced, and it is a setting thoroughly governed by its own conventions.] Ballroom tango is highly contrived, a stereotyped exaggeration in competition style (see Savigliano [1995] on tango).

Likewise, the authenticity of “far away” can be invoked elsewhere. The printer I have observed works in the style of Japanese woodblock prints, using Japanese-made tools, and invokes the style of Japanese woodblock printing in her own

work. Potters frequently reference the Japanese tradition of pottery in their contemporary work, even when they are not consciously following Japanese models. So, the theme of authenticity, linked to practices and aesthetics of elsewhere, and of the past, link across my research sites. It furnishes a *topos* in the local discourse of aesthetics. As it does in the opera master classes I have observed – where there is a thread of reference to the tradition of *bel canto* singing, in which a tradition is preserved, as is the singing voice that produces a distinctively beautiful style. So, authenticity evokes the preservation of tradition – as do various other artist-makers. The glassblowing studio preserves and celebrates a tradition of glassblowing, and craft making that goes back centuries, uses the same equipment, and passes through the studios of the glass-makers of Murano.

Again, one does not necessarily make these linkages by inductive coding. You do not necessarily derive them by deductively working from grand narratives of sociological theory either. They do not emerge like hens hatching out from eggs. You cannot just sit on your data and hope that they will come out. They have to be worked at, reflected upon, played with, and modified.

I could go on. The figure of repetition is profoundly significant in all these settings. Craft making in glass, ceramics or in wood-block printing is thoroughly repetitious. It depends on the careful and controlled enactment of practices over and over again. In printing, the need is to ensure that the registration of each successive impression is accurate. That notion – of registration – serves as a metaphor for the repetitive need for accurate reproduction (but

not identical replication) across all my research sites. Rehearsals and master classes display the significance of repetition in the interests of artistic interpretation and performance, for instance.

Each studio or rehearsal space is a small place. Sometimes it holds just one maker with barely enough room for an assistant, another artist, or me. My task is to take those small spaces and make them bigger – conceptually bigger, as they can expand to create an ever wider and denser network of associations and linkages. The smallest, most local of phenomena can thus, be developed into a wide set of conceptual, formal ideas. I have used some ideas from my current work because they are so transparent that I do not need to lead you through densely detailed ethnographic materials. But, let me conclude by turning things back on the research process itself.

What I have hinted at for craft, art, and performance applies equally – and with considerable force – to the conduct of ethnographic research itself. It is creative work, dependent on improvisation that is in turn dependent on repetitive, disciplined work. The creative processes are dependent on that work: on careful, methodical, and repetitive activities. But, such work is never mechanical. It does not depend just on the precise replication of formulaic procedures. It depends on a creative, improvisatory engage-

ment with several things. Like the craft worker, the ethnographer engages directly with her or his materials, physically and imaginatively. Embodied skills and educated eyes – the gaze of the craft – interact. Aesthetic and intellectual imaginations, traditions, and innovations interact in the craft of ethnography, just as they do in the ethnography of craft.

Finally, the attentive reader may wish to raise the following objection. I began by criticizing unduly mechanistic and formulaic approaches to ethnographic analysis, and I linked that criticism in part to the influence of “grounded theory.” Yet, it may be argued that much of what I have just outlined is in fact a version of grounded theory itself: the repeated interactions between ideas and data, the use of comparisons, the search for generic concepts that link and transcend local circumstances. Surely, one might suppose, these are among the inspirations of grounded theory, as originally formulated by Glaser and Strauss (1967). Indeed they are. The original formulation of grounded theory was not a set of recipes and formulae. It was a general description of how any form of social inquiry can be conducted in the interests of generating new ideas, elaborating on existing ideas, and doing so through an attentive reading of data (of any sort). My overall intention, therefore, is to encourage a recovery of that initial inspiration.

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## Book Review

**Barone, Tom and Elliot W. Eisner. 2012.**  
*Arts Based Research*. Los Angeles, London,  
New Delhi, Singapore, Washington, DC:  
Sage Publications

An inspiration for writing a review of the book *Arts Based Research* was an incentive coming from its authors, which was addressed for all potential readers and encouraged them to search mutually for adequate criteria of evaluation of projects and texts that belong in this research tradition. Since the methodological thought of Elliot W. Eisner and his disciple Tom Barone has been close to me for many years, I decided to reply to this invitation in a constructive way, as an educational dance based researcher (Kubinowski 2003) and a methodologist of qualitative educational research in general (Kubinowski 2010). The authors of the reviewed book suggested and justified a list of a few basic criteria of evaluating the quality of arts based research, for example, incisiveness, concision, coherence, generativity, social significance, evocation, and illumination. In a summary of the appropriate fragment they wrote:

[s]o, finally, we invite you, the readers, to use your own judgment in applying these criteria to the ex-

amples of the works of arts based research included in this book and to those many that are not included. But, we also urge you to use your imagination in ascertaining other criteria that may emerge from your encounters with arts based work in the future. As an informed and imaginative reviewer and critic of examples of arts based research, you may serve to ensure that those works positioned to achieve the purpose of raising questions about important social issues in a powerful manner will more likely be made available to others. (p. 155)

Before I move on to extend the list of the important criteria with three new ones, for example, idiomaticity, synergeticity, and emergence, I would like to point out, in my opinion, the key virtues of the book from the perspective of contemporary qualitative research and make a basic critical remark. I would also like to highlight the fact that by a critical remark I mean the pedagogical gift, which, as I suppose, is concurrent with the intentions of already classic concepts of educational connoisseurship and criticism as proposed by E. W. Eisner (1976).

Book Review: Barone, Tom and Elliot W. Eisner. 2012. *Arts Based Research*. Los Angeles, London, New Delhi, Singapore, Washington, DC: Sage Publications

The main virtue of the book is its competent, synthetic, and exhaustive presentation of the essence of arts based research and of suggestive examples of its use in the deepened understanding of the selected educational issues. This lecture, which is unrivalled in the available literature as far as high methodological quality is concerned, was prepared not only by the experienced researchers but also the creators of this paradigm, and its leaders and propagators. In the following chapters they discuss the essence of arts based research, the reasons for its application beside some other social research, the question of its scientific nature, predisposition to its effective realization, its target group, the issue of fiction as a key epistemological category, its political and ethical context, a choice of adequate criteria of its evaluation, the role of theory in its designing and practicing, and fundamental ideas of humanistic epistemology resulting from the research. A discussion of any aspects of arts based research is preceded by an analysis of a given conceptual field with the highlighting of ambivalence in understanding the basic methodological categories and the profiling of their meaning adopted by the authors. For instance, concepts such as: research, fiction, criteria, theory, et cetera, are ambiguous. Also the specific features of arts based research are presented intentionally in an open, pluralistic form, not specified in content, which stems from the rooting of the authors' methodological thinking in pragmatism (J. Dewey) and neopragmatism (R. Rorty), cultural anthropology (G. Geertz), theory of literature (M. Bakhtin), aesthetics and theory of art (E. Gombrich, S. Langer), or even the feminist thought (J. Kristeva), and most of all, the philosophy of science (T. Kuhn, S. Toulmin), and contemporary interpretation of qualitative research (N. Denzin, Y. Lincoln).

What is more, the reviewed book is full of innovative, deeply humanistic epistemological, methodological, and pedagogical theses, which are still considered quite bold in the academic world. Out of these, attention should be paid particularly to clear criticism of the positivistic paradigm of social sciences, based on the statistical method, of simplistic standardization, behavioral approach, and a tendency to quantify all the qualitative data in a reductive manner and to measure statistically humanistic phenomena, yet in combination with resigning from a confrontational approach in the traditional discussion between the qualitative and quantitative researchers. It is even postulated that the two of them could be complementary in their view of reality. What is also interesting is the distinction between research and science, in which the scientific research is just one of many means of knowing and understanding the human being and his/her world. It is connected with the postulate of humanistic-like redefinition of science, particularly in relation to social sciences. The authors present a set of convincing arguments for bringing the specific features of art and artistry to the ground of scientific activity, or in a broader context, research activity, thus, promoting the concept of a research project and a scientific text as a work of art, and of a researcher as a special kind of artist. Inspired by the thoughts of R. Rorty, they discuss the idea of epistemological humility, pointing out that the aim of arts based research is not to discover and explain the universal absolute truth but rather to deepen the understanding of a unique phenomenon or a case in the contextual approach, and to interpret this phenomenon in a polyphonic and ambivalent space of various ethical, aesthetic, axiological perspectives. From the point of view of knowledge about the

human being, arts based research is an alternative and complement to the conventional scientific knowing, and is a special kind of social research which is placed within the new paradigm of integral humanities. The interpretation of arts based research by T. Barone and E. W. Eisner offers a clear, multi-dimensionally interpreted pedagogical aspect, which is expressed not only through educational examples but also in the main ideological message within their teleological scope. To be precise, the final aim of their usage in social research should be, according to the authors, the drive towards pro-humanistic and pro-democratic changes of the contemporary world and people, which relates this interpretation very clearly with the traditions of critical theory in social sciences, and the resulting critical pedagogy.

The publication has more to offer. This review may only cover some part of it. Let us come back to the evaluation criteria of arts based research. In my opinion, the three criteria, namely: idiomaticity, synergeticity, and emergence seem to be the most important ones in the quality evaluation of the contemporary qualitative research. They were not acknowledged in the criteria lists offered by T. Barone and E. W. Eisner. It is surprising as the book, while discussing the following aspects of arts based research, frequently refers to those criteria in various ways. Hence, concepts such as: idiom, synergy, and emergence are used constantly while describing the constitutive features of the research, then by analogy, idiomaticity, synergeticity, and emergence should be found within the basic evaluation criteria. The criterion of idiomaticity, as far as arts based research is concerned, refers mostly to the quality of understanding and using idioms typical for particular artistic sub-fields, whether in a research project or a scientific

text. It can also be used in the corresponding sensory data and forms of representation, and also in the difficult tasks of translating one artistic/media/cultural idiom into another. The criterion of synergeticity entails having a closer look at synergy quality between art and science, which is reflected in the hybrid arts based research, and which is aimed at accepting the used solutions both by the art critics interested in the knowledge-related values of art, and by the scientific reviewers who are sensitive to the artistic value and open to various ways of knowing and understanding. The criterion of emergence is used in this case to understand the appropriateness and adequacy of using artistic logic and media in order to deepen the understanding of social phenomena, which is often limited to the conventional scientific knowledge, and which try to avoid the label of art for art's sake. That is why the choice of an artistic discipline and a specific artistic medium has to be the result of a methodologically justified decision and not a matter of chance, fashion, or showing-off. It seems to me that the proposed three criteria of evaluation of arts based research are just a reinterpretation of the authors' intentions, and result from the philosophy of this paradigm.

As a dance based researcher, I will allow myself to make a critical comment. The book quotes in full the three original research studies as examples of texts which represent arts based research. All of them make use of verbal medium and literary art. The whole book does not contain any visual images, such as photography or drawing, nor any examples of arts based research concerning theater, music, and dance in a broader context. The only publications today concern qualitative research supplemented by DVD's, which present audio and audio-visual examples, however, these

may have not been the intention of the authors. It seems, though, that thinking and writing about arts based research by T. Barone and E. W. Eisner is too dominated by literature. Although they write about film, photography, dance, music, they do not try to break with the dominance of verbal representation, which is already outdated in qualitative research. The kinesthetic understanding of dance entails a much more deepened and adequate research than merely watching dance and then representing it by means of words. The same applies to music and theater. However, the authors understand that kind of idiomaticity perfectly, which is evidenced in their comments on non-literary arts. Undoubtedly, they know many social research projects which are based on the theater, dance, music, fine arts, performance, et cetera (Knowles, Cole 2008). Why did they resign

from presenting a more developed, multi-sensory, and multi-dimensional image of contemporary arts based research, which would represent distinctly different aesthetics?

To conclude, I would like to share the wish of E. W. Eisner and T. Barone, also present in the book, for community of arts based researchers to be made stronger by new, creative members all around the world. Let me formulate this wish in the 20<sup>th</sup> anniversary of organizing the inaugural institute in educational arts based research at Stanford University, and in the 80<sup>th</sup> anniversary of E. W. Eisner's birthday. Let the emergent "dance" of qualitative diversification last in the synergic "circle" of researchers/artists community with idiomatic "figures" of approaches, metaphors, "gaps," and representations.

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## Book Review

**Manderson, Lenore. 2011. *Surface Tensions. Surgery, Bodily Boundaries and the Social Self.* Walnut Creek, CA: Left Coast Press**

Lenore Manderson's *Surface Tensions* is a must for any social scientist interested in body, medicine, disability, gender, and processes of social construction of various embodied phenomena.

The book is like an Alfred Hitchcock movie: it starts with an earthquake and then the tension rises. The earthquake is a prologue: a shocking, yet positive *Perdita* story, which presents the experience of a woman who, due to cancer, has first received a stoma and some time later lost her breast due to having a mastectomy. This, and other similar stories, arrange the book and are great examples of how people manage their lives, social interactions, and rebuild their identities after serious bodily change. Lenore Manderson calls them, deservedly, catastrophes of the body and depicts life afterwards.

In chapter one, "The body as subject," the Author outlines the two-way influences of body and identity, poses important questions of how a life can be managed after a serious bodily change, when not only our physical part is ruined but the rest of our life as well. Manderson questions "Descartes fingerprints" in today's thinking about bodily issues. She proposes another way of looking at body – all human experience is incarnated; we interact with the surrounding world through our bodies. Therefore, a healthy body is invisible, as it sets almost no limits to its "owner." Bodies become visible when they are in pain, lose some function (for example, sight, ability to move some body part, et cetera), or start to look different. These changes require a great deal of identity work, as we are presented to ourselves and others through our bodies. The chapter also raises some questions concerning the

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technological development in helping people after serious bodily alterations and depicts ways in which body and science interact. Illness or disability are not only challenges for medicine but, as well, social phenomena dependant on culture.

In this introductory chapter, the Author also shows how body might become a source of social inequality, not only as a source of stigmatization (for example, urine incontinence) but also as an effect of global policies. The chapter also presents some of the methodological and theoretical procedures used in the research. As the Author claims, the book is centered around "people who have had little choice in controlling their bodies, surgeries, and bodily trajectories," which is visible in the rest of the publication as Manderson cites her informants very often, illustrating various phenomena present in the text.

In chapter 2, "Our cyborg selves, and other modern tales," Manderson examines the history of the disabled body. A reader might find descriptions of (r)evolutions in medicine and technology, medical and surgical practice, from early experimental work to current medical procedures causing serious ethical debate. For me, the discovery of a long history of (successful and unsuccessful) attempts at body transformations was a fascinating part of the book.

Chapters 3 to 6 are built around interviews with people after serious corporeal change. They are loaded with citations and stories of people who try to deal with the great alteration of their physical "surface" and everything that goes along with it. We may read about their everyday experiences, their sexual relations, their strategies and actions undertaken to have life as normal as possible.

Chapter 3, "Visible ruptures: The art of loving with lack," undertakes a detailed depiction of life after amputation of (a) limb(s) or loss of its function. With this part of her book, Manderson invites us to the world of people whose bodies cannot be controlled anymore, who need to negotiate the new ways of using the body, living their lives with prostheses, wheelchairs, or being dependant on others. And although Manderson's interviewees underline numerous times that "they are normal, they just don't have a leg/arm/hand," it seems to be only a wish. Due to a loss of limb, a physical part of the body, their identity in the eyes of interactional partners becomes "flat." The most important information is the information about disability, so identities from before the alteration (social roles, personal characteristics, et cetera) become less important, or even invisible.

In chapter 4, "Body basics: living with a stoma," Manderson raises several insightful questions about the everyday life of people who have lost continence. Their main interactional problems are caused by the fact that one of the first things that defines a child's development is the ability to control continence of bodily waste. Adults who have lost this ability are treated as not quite respectable. What is more, bodily waste, which is "normally" invisible for interactional partners and almost invisible for the person, becomes evident and needs to be managed. The ways of concealing the fact that one has a stoma become central for those people, as they want to be treated as normal in social relations.

The next chapter, "The feminine in question," is entirely dedicated to stories of women who have lost their breasts due to cancer. Manderson starts with a description of the role of breasts in women's

lives (symbolically and physically), explaining the great importance of this part of the body as the one which defines womanhood. Mastectomy deprives women of this, so some of them decide to have their breast(s) reconstructed. In this chapter Manderson shows that the human body is deeply gendered and that our identities do lie in the flesh, which becomes evident when some parts of it disappear. As in earlier chapters, what the Author shows is ways and strategies of concealing “the difference” by dressing up, hiding, and special behavior.

Chapter 6, “Replaceable parts: the end of natural life,” deals with themes related to transplantation, which is receiving a body part from a living or dead donor. This process poses many ethical questions right from the beginning: can the recipient ask for an organ, if this usually means someone’s death?; is it actually possible to thank the dead donor’s family enough?; can we thank our life donor enough?; do we carry some piece of the other person’s life in the transplanted body part?; what are the non-medical criteria of choosing a life donor among family members?; and many others. What was very interesting and non-stereotypical was the way that Manderson, as an anthropologist, explains why donations after death cannot be treated as a gift (which is usually presented this way by the health care system). She explains that the concept of gift is a way of exchanging and strengthening social bonds with relatives and close friends. This is impossible in the case of transplantation as the recipient and the dead donor’s family are not supposed to meet or even know each other’s identity.

In “Conclusion: necessity’s children,” Manderson returns to the general cultural themes concern-

ing medicine, health, and illness and changes of these phenomena. What used to be a normal sign of aging has now become a curable pathology, and is paid for by the patient or public health system. Demographic changes, economical factors influencing medicine, fast developing science shape today’s ways of perceiving disabled body. And what was quite astonishing for me: although we now have many options for disabled people, they do not seem to feel more normal. It’s even worse, as their bodies become less natural, are dependent on machines, which makes disability even more strange.

I must admit that the numerous advantages of the book has significantly influenced my way of thinking about disability and altered bodies. Primarily, because it is Manderson’s informants who became the most important in this book – their stories, their experiences, the way they describe their own bodies and everyday lives after the change do make an impression. The Author has shown great empathy and a deep insight, which allowed her to be sympathetic with her informants, but still find a proper analytic distance and analyze bodily alteration from various points of view.

The book shows how identity is socially related with the physical body, and how social selves are made “flat” through disabled bodies. They become “flat” as the lack (of body part or function) comes to the front of social interaction and determines course. As Goffman (1963) stated: the disabled person is stigmatized.

The book introduces a quite controversial notion of normality, being normal, and as a consequence: being abnormal. In sociology, this term is used

very rarely, but it seems to be the best one to describe the aims and identities of disabled people. Being normal is in the centre of attention and actions of disabled people and of those *normals* (Goffman 1963) who interact with them.

Despite being well grounded in data, the book also has some weaknesses. For me, as a sociologist, the book lacks enough theoretical contribution. I would expect more generalizations, while the Author chooses to concentrate more on presenting the stories than building a theory. The book would have profited from developing more

general conclusions as Manderson based her work on really unique data.

What is more important, I am not quite convinced about the purpose of including films or works of art presenting embodiment issues. I found these parts not as interesting as the rest of the book and I am not really sure of the Author’s intention to include them in the book.

Despite my doubts, I can recommend the book as a must for every researcher who studies disability, sociology of the body, gender, and many other embodied phenomena.

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