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Creative Thinking in Qualitative Research and Analysis

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Abstract I would like to present the possibility of broadening the traditional methodological and technical skills of researcher and analyst, but also the intellectual capacity of the researcher associated with combining data, categorizing, linking categories, as well as the interpretation of the causes and consequences of the emergence of certain social phenomena. Some methodologies, methods, and research techniques are more conducive to creative conceptual and interpretive solutions. Therefore, I describe the serendipity phenomenon in such methodologies as grounded theory, ethnography, phenomenological research, and contemplative inquiry. The problem of intuition in qualitative research will be also described in the paper.

There will be presented also some suggestions how to be creative in qualitative research.

From the review of issues of creativity in qualitative research we can derive the following conclusions:

1. Creativity in qualitative research depends on the strength of *a priori* conceptualization and stiffness of the adapted methods of research and analysis.
2. If the methodology is more flexible (as the methodology of grounded theory), the researcher can get to phenomena that he/she has not realized and which are still scantily explored in his/her field of expertise.
3. The phenomenological and contemplative approaches allow the use of the investigator's feelings and experience as they appear in the studied phenomena, which usually does not take place in objectifying and positivistic research.
4. The investigator may therefore consciously use these methodologies and approaches that foster creativity.
5. The researchers can improve their skills in thinking and creative action by doing some methodical exercises (journal writing, writing poetry as a summary of the collected data, the use of art as representation of the phenomenon, the use of meditation, observation of the body feelings, humor, etc.).

Keywords Contemplation; Meditation; Creativity; Grounded Theory; Phenomenology; Serendipity; Qualitative Research; Ethnography

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Creativity is intelligence having fun.

Albert Einstein

A mind is like a parachute.

It doesn't work if it is not open.

Frank Zappa

Qualitative research is sometimes considered to be creative by definition, quality is not quantity, so strict rules do not apply. However, the development of procedures, techniques, research methods, and procedures for the analysis of qualitative data makes the study more precise. This development with some standardization may be a restriction to the creative approach to research and analysis of qualitative data. By analogy to quantitative research, we can say that it is difficult to be creative when everything is, from the beginning to the end, established and planned in the research process, including hypotheses.

Those who believe that qualitative research is by definition creative, justify that research methods are not as standardized as they are in quantitative research. There is a creative approach to the use of specific research and analytical techniques and methods. This dose of creativity, in the sense of arbitrariness, is intended to provide an unconventional approach to constructing research problems and solving them. Creativity would be here defined by the degree of openness of the researcher to new methodological solutions in research and technical data analysis. Openness would also apply to the use of multiple interpretation schemes. So, it is not just about extending the possibilities of methodological and technical research and analysis, but also about

the intellectual capabilities of researchers involved in data combining, categorization, categories linking, and the interpretation of the causes and effects of social phenomena.

One should therefore consider what does lie at the base of creative thinking in qualitative research. If we are talking about creative thinking, it is evident that we are talking about the researcher who thinks. We clearly associate the formulation “creative thinking” with the subject who thinks. It is rare for us to think of the idea that creative thinking can appear automatically in the research process under certain conditions which are to be conducive to the creative research process. However, if we think so, we can imagine the idea that some methodologies, research methods, and techniques are more conducive to creative conceptual and interpretive solutions. Such research methods include, in my opinion, the methodology of grounded theory, sociological ethnography, transformational phenomenology, or the use of non-standardized interviews in field research, or in contemplative research. Limiting pre-conceptualization allows us to enter into a context of discovery (*serendipity*). Methodology of grounded theory, sociological ethnography, phenomenological studies limit the number of assumptions taken before the study of explored phenomena. Phenomenological *epoché* is a clear example of this. Taking in the brackets our assumptions on the basis of which we perceive a phenomenon in everyday life opens us up to seeing phenomena as they are. We can also see more clearly what lies at the base of our lifeworld. We can realize what limits us in the research (*a priori* setting research areas and topics, categorizing and interpreting data and categories on the basis of pre-assumptions),

which makes us most likely to see the world in some dimensions, while other dimensions are inaccessible to us. Why, for example, do we neglect the spiritual dimension of a study that may affect the researcher?

By using the above methodology, we can easier enter the context of discovery, that is, to observe phenomena that were not in our field of interest prior to the start of the study (*serendipity*). By limiting the pre-conceptualization of research we are more open to phenomena that may unexpectedly appear in the field of research. The perception of these phenomena is possible by categories of our language. This language (especially colloquial language) is, however, metaphorical and does not restrict us in conceptualizing phenomena *in situ* when we have direct contact with the phenomenon under investigation. However, inspiration for the labeling of phenomena and research objects comes from the research area itself. Pre-conceptualization certainly limits us and does not give us opportunities for these inspirations. The use of everyday language is one of the most important skills of a qualitative researcher. Creativity is an immanent feature of colloquial language. This language is not so restricted in meanings as the scientific language. A creative researcher uses this commonsense vocabulary at the beginning of the study to freely label the phenomena he/she observes. Then, in a more rigorous way, he/she tries to clarify their meaning, and eventually build up definable categories of labels.

Thinking and Creative Work

If we recall Herbert Blumer's (1954) idea of sensitizing concepts, then we find there the potentiality

of sociological creativity. The sociological imagination is built in close relation to the observation of phenomena as they occur in a particular place and time. Blumer's approach to symbolic interactionism is an empirical approach that encourages the exploration of not investigated phenomena. Symbolic interaction, therefore, based on defining the situation by participants, imagining the role of the other, and negotiating identity by language, gives us the opportunity to use imagination, especially in new interactions or those that need to maintain a definition of situation and identity in the new context. Language operations in defining yourself and others, defining situations and phenomena, are related to its creative use.

Interactive phenomena "named" by sensitizing concepts allow us to see these phenomena in their full coherence and spatial-temporal localization (Charmaz 2006). The researcher, in a specific place, uses the native language and his/her own scientific language, and **combines two modalities** that prompt him/her to open up new dimensions of the phenomena and to make them more precise. **Creativity is, among other things, a derivative of the collision of these two contradictory tendencies.** The term "homelessness" means not having a home, however, we know based on stereotypes that a homeless person does not have his/her own home. Nevertheless, the person has some space to live and sleep and eat, the home is moving together with the body and the equipment of the person. The society defines the homelessness also by legislation; and yet—who is the homeless person? Is he/she a vagabond or a bum? A criminal or a victim of the socio-economic system? The commonsense meanings of the term

indicate the direction of looking for the data and later the analysis of the data. Even the persons being researched use these stereotypes.

Herbert Blumer (1954:7) distinguishes definitive concepts and sensitizing concepts. These first relate to what is common to a certain class of objects, and these are the definition of features of a certain category of objects, while the sensitizing concepts point out to us only the direction in which we can observe the phenomenon. They make us sensitive to certain features without specifying the final version and quantity of the objects tested. They allow you to remain open and look for further characteristics and contexts of their occurrence. It is important to be sensitive to specific questions related to a given phenomenon (Charmaz 2009: 27). The questions come from the language that we use.

Sensitive concepts show us the directions of our observations and do not accurately point out the characteristics of a given phenomenon or object. The phenomena of everyday life have their specificity associated with their location in a particular context. By studying what is common to a given phenomenon in different contexts, we also study what is unique. These specific qualities can be our discoveries if we are open enough and careful to seek what are the unique characteristics of experiencing the phenomena in their often-unique context. A creative approach to research would be a skillful attempt to focusing on:

1. a detailed description of the researched phenomenon (what is its content and how the phenomenon proceeds),
2. then its interpretation in relation to its context,
3. and individual experiences of phenomenon by individuals and social defining it at the group level.

Thus, adapting the idea of sensitizing concepts allows us to develop a creative approach to research and analysis of phenomena. The concept thus contains the immanent features of creative thinking and new actions (openness, continuous questioning, searching for specificities). The guiding principle here is to sensitize the researcher to something that is worth looking for and exploring rather than pointing out the exact location and set of features of a given phenomenon. Blumer referred his concept primarily to the “naturalistic” research, in this case, studies of the observation of everyday life phenomena as they occur in their natural environment. However, the ending point is to create the generic concept and they lose the uniqueness of experiencing the phenomenon. And here the symbolic interactionism needs some inspirations from other perspectives, as, for example, phenomenological style of research. What about the “homeless” person that does not feel homeless? He/she could think that the city or the whole world is his/her home. The suffering of the homeless people is assumed and probably it can be proved in the research, however, if the suffering is not experienced, is the homeless person less homeless? What about the meaning of homelessness connected with freedom? Here is a quotation from the grounded theory research report on embodiment in homelessness:

Yet, other youth were capable of understanding and incorporating a more abstract sense of home based

on a clear sense of self in relationship to their bodies. Consider this youth's statement, "I don't have a problem with being homeless. I am happy to be homeless because I can start a new life where nobody has control over my body." This youth was less concerned with the physical space that she was living in and more concerned with the freedom being homeless gave to control her body. [Sy 2010:99]

If the homeless person is mindful of his/her existential situation, he/she could feel home differently: "you can feel home if you are at home in your body" (Sy 2010:100).

The body can also not accept taking more drugs and the person wants to get out of the homeless situation and seeks help from others (Hall 2010).

Definitions of the concepts come from outside, the living experiences from the inside.

If we use the sensitizing concepts in our research plans and research itself, as it often happens in qualitative research (ethnographic studies, grounded theory, phenomenological research), then we have to think about what is creative thinking. Without precisely defined concepts, we must put up new research questions and seek answers to them in the most versatile, often unconventional way. So, we need the sensitizing concepts. Creative thinking is related to finding new solutions to problems we know or to connecting new ideas with existing and known ones. Creativity is therefore going beyond the known means of problem solving, beyond socially acceptable patterns of perception and action. Generally, we want to prove some thesis or hypoth-

eses also in qualitative research. While proving, we look for similar instances of the phenomena and we want to find the generic features of them. But, there are side effects or other crisscrossing phenomena that are not exactly proving our theses. Following the scientific procedures, we drop them to find proofs. They are our goal.

Therefore, creativity requires originality and courage. It could be connected with refusing the sensitizing concepts. What is conventional is not creative. Originality is, however, insufficient in creativity; we should add here the usefulness or appropriateness (Runco and Jaeger 2012:92; see also Runco 1988).¹ Not all original concepts, hypotheses are used in explaining or interpreting the studied phenomena, so they do not necessarily belong to creative solutions. Creativity is often combined with the notion of genius (Scheff 1990) and/or imagination creating something new that deviates from what has existed so far. It is also important to see that society evaluates the creative work at each historical time differently. Creativity is variously understood in different historical times. Creative work must be given some value by a specific audience. You can be "individually creative" (original), but not "original historically," that is, the work is not accepted by the audience at a certain time. Of course, it may happen that the

¹ Research on creativity is a very well-developed field of investigation within psychology. It is enough to see *Creativity Research Journal* to realize how much has been done in this regard. In this article, however, I will focus on the conditions of existing theoretical and methodological approaches that allow for creative attitude to solve some research and methodological problems in qualitative approaches in the social sciences and the humanities. These problems are connected with the repeatability of solutions to the theoretical questions or the stuck in the research or analysis when we do not know how to answer some questions that emerge during the investigation or analysis.

originality will be accepted by the public after the death of the creator, as was the case with the work of Vincent van Gogh (Boon 2014).

Here is a standard definition of creativity, which also refers to the need of acceptance of creative work by others: “The creative work is a novel work that is accepted as a tenable or useful or satisfying by a group in some point of time” (Stein 1953:311).

However, creativity must in some way be adapted to reality at a specific historical moment, to be settled down there. The originality alone is not enough to be a creative person. Inventing new words, concepts, hypotheses, interpretations does not come from the brain, it comes from deep reflection or insight.

Considering creativity from the individual’s point of view, we may say that sometimes the epoch is not adjusted to new ideas created by the individual. However, in general, our creativity is blocked by the psychological and psycho-social barriers associated with the situation here and now, where we operate and create. If we have not created the need for critical thinking, then we cannot question the existing patterns of thought and patterns of action. Criticism is the basis of creative thinking. The concepts conceived are usually supported by group thinking. Being opposed to group thinking requires courage and confidence in yourself and your thoughts. You should look more deeply into the concepts, also sensitizing concepts. This courage, in turn, is associated with having a high self-esteem, which allows one to oppose the opinion of the group and the appearance of emotion of shame or embarrassment (see: Scheff’s concept of genius [1990]; Konecki 2008; 2014:31-32;

Pawłowska 2013).² In addition, if we are in bad working conditions, with unfair criticism, excessive burdens, we have an increased number of barriers for creative thinking, as it often happens in academy. Thus, mental barriers can meet and strengthen social constraints and eventually limit our creative capacity.

Creativity in Contemplative Studies. Contemplation as a Tool of Creativity

The use of sensitizing concepts and vernacular terms is part of creativity, from another side we have the techniques of research and analysis of data. Although we should remember that the techniques are used by the researcher that thinks and that mind’s work is included in the investigation. Seeing the techniques as an integral part of the research is very important in understanding the lack of creativity or its abundance.

² A very important factor of creativity is laughter. When we experience shame, we should start laughing to dissolve it to save our self-esteem. High self-esteem is an important condition of creativity (Scheff 1990:173). Shame blocks it. The good-humored laughter also dispels chronic shame (Scheff 1990:175). Then, the mind is less blocked and can find new connections and solutions.

Samuel Johnson, George Gordon Byron, Richard Wagner, Friedrich Nietzsche were inveterate laughers, “the great geniuses should have been laughers” (Scheff 1990:175). Nietzsche connected even his creativity with laughter when he was writing *Zarathustra* in 30 days. Concerning Richard Wagner, there is evidence that he experienced many times prodigious laughter. His wife, Cosima, mentioned in her diary 300 hundred times about the attacks of laughter. The entries in diary are from the periods when Wagner was the most creative in writing his music (Scheff 1990:173-174). Laughter is supporting creativity of so-called “easy creators” that generate their work very fast, without revisions, as mentioned Nietzsche, or Goethe who wrote *Werther* in 24 days. They were free from the self-censure (Scheff 1990). Then, humor rather supports creativity than suffering and sadness as David Lynch, a movie director, said: “A lot of artists think that suffering is necessary, but in reality, any kind of suffering cramps the flow of creativity” (see: <https://vimeo.com/182093266> [retrieved July 30, 2017]). To see it, we need some deep insight into the work of our mind.

Qualitative research can be treated as creative by definition. For example, a non-standardized interview can be an extremely useful tool in reaching new phenomena, but it should be connected with insight (Janesick 2015:54). By analyzing and interpreting the meanings from the interview, we get a new view of the phenomenon based on the interview, which could be really deep insight into the situation. But, how to achieve the insight?

Qualitative research can be contemplative, if we care about continuous and full communication with the respondents and we are able to obtain periods of silence (pauses in our thinking to gain even deeper insight). What decides on qualitative research as contemplative inquiry is, according to Valerie Janesick, the consciousness of thought/thinking in each component of qualitative research.

The definition of contemplative qualitative research according to Valery Janesick (2015:22) is as follows:

This practical concept is useful for those of us using qualitative methods to make sense of people's lives: we are connected to our participant/s whether or not we wish to be. I call this approach contemplative qualitative inquiry. The contemplative component has to do with the stillness and silence of thinking with a meditative orientation.

"I use the term contemplative inquiry to refer to qualitative techniques that place a deep and serious emphasis on thought in every component of a study of the social world" (Janesick 2015:34). Thus, contemplative research serves to understand the Other and the social worlds, not to prove or

verify hypotheses. It is necessary here to keep full openness and realize our own assumptions. Then we can be as close as possible to social reality, to touch it in a direct way, because we also have insight into our own cognitive apparatus and identity of the researcher. We concentrate how the mind works.

Valerie Janesick uses the metaphor of Zen Buddhism to extract contemplative elements from qualitative research, but also tries to apply certain rules of Zen Buddhism to research practice. Qualitative and Zen research are:

- a. holistic,
- b. context is important,
- c. body and mind are instruments of cognition,
- d. both approaches emphasize ethics and the principle of "do not harm,"
- e. both traditions use "storytelling" as a research and reporting tool that is used in teaching as well,
- f. both cognitive traditions emphasize discipline in thinking and writing, perseverance, urgency, intuition, and creativity (Janesick 2015:34).

So, since Zen is creative, the inspirations from it can only enrich the creative qualitative research. Zen plays with language, with words, it is seen especially in Zenic poetry, haiku. The approach to analyze documents, photographs, and artifacts involves the consciousness of compassion and creativity, because we deal with the world described by the language

(see: Janesick 2015:81). The use of poetry, photography, drawings, art, or even jokes by a researcher can be a significant addition to the written narrative. Experimenting with photographs during qualitative research can help us to interpret phenomena, to reach their meanings. Putting the photos in some sequences could help to see the phenomenon in a different way and inspire to new interpretations of the researched phenomenon (Konecki 2009).

Qualitative research can be helped with mindful activities. For example, to write in the evening what we experienced during a given day, or free and fast writing within the time limit, for example, 15 minutes could be useful. It may be important to keep a journal of meditation. This can help you to clear your mind of your thoughts and understand your assumptions (Konecki 2016). Meditation also helps to maintain mindfulness while interviewing and focusing on the here and now while talking to a participant (Janesick 2015:32).

Writing poetry can be helpful for interpreting different texts or documents, for example, what is included in a given document can be interpreted by a poem, what Janesick advises her students. Poetry is another instrument to capture meaning (Janesick 2015:86-88). Poetry captures deep meaning. Poetry is open, creative, showing new perspectives of interpretation. Writing poetry after reading documents or transcribing can be an impulse for new and creative interpretations of texts.³ Poetry can

³ "According to Husserl, poetry and fiction are primary aspects of phenomenological thinking. Free variations are imaginings designed to clarify essential elements of phenomena. Phantasies are informed by eidetic intuition. Phantasies also clarify such intuitions: one can say in strict truth that

help us to reach the meanings of a given phenomenon. It allows us to notice things imperceptible (Heidegger 2005) and to find also the eidetic features of the phenomenon.

Very important in developing creative research skills (e.g., limiting the pre-conceptualization and influencing one's own assumptions on perceived phenomena, interpretative abilities, etc.) is to write a reflective journal (Janesick 2015:132). It is writing for joy, for better understanding, and playing with creativity. Writing a reflective journal is also a contemplative activity, allowing the researcher to reflect on his/her role and identity in the study, and to be free from the limitations of his or her identity, or to observe these limitations. In the reflective journal, we also record our feelings and impressions from interviews and the use of other research techniques.

Intuition

Referring to the above considerations of contemplative research and their connection to creativity, meditation can be considered as a technique to support the emergence of intuition. Meditation,

'feigning' ['fiction'] makes up the vital element of phenomenology as of every other eidetic science, that feigning is the source from which the cognition of 'eternal truth' is fed" (Husserl 1982:133 as cited in Bentz 1965:49).

Heidegger also attaches great importance to poetry in discovering the secret aspects of being that may emerge in the style of writing. What is important is the understatement that is between transitivity and what is constant and unchangeable; important is what is coming and the poet has intuition to predict it (Heidegger 2005). This poetic style of writing can also contribute to the discovery of hardly recognizable meanings and configurations of meanings, premonitions about future events in ethnographic research. Poetry can certainly be used in the journal of reflection to derive those undiscovered meanings and feelings for the future.

and therefore the practice of concentration and/or mindfulness over a particular object, raises many questions in the individual about his/her existential condition of being in the world. One such question is: "Who am I?" (Konecki 2018). An interesting phenomenon of consciousness may be that the answer to this question, however often it will appear, is never the ultimate answer. The researcher asking this question during the meditation practice can reach the limit of cognitive ability. Answers may be many, but they do not reflect the psycho-physical state in which the researcher is at the moment. If the researcher cannot answer the question of who he/she is, how is it possible to seek answers to this question about the respondents? Who are they? What do THEY mean? What are their identities? And what is my identity? Is it sensible to ask for permanent features of the identity of OTHER, since I-researcher cannot identify them in myself?

When I ask myself in meditation, "Who am I?," and I find no answer, this kind of self-experience makes the boundaries of my *ego* porous and builds doubts about the existence of the researcher as a separate being. I start seeing connections with the "outer" world, which turns out to be my world, because I am close to it, I question the properties of it, so I touch it directly. I am not outside; my space is in this space *here and now* and it determines my perception of the world (Merleau-Ponty 1962). Researching the homelessness, I, as a researcher, can ask, "Who am I?" in the moment of the research. I have a home, family, income, I am not addicted. So, how am I going to understand the homeless person who is in a completely different situation? If I am looking for the structural conditions of his/her fate, where do

I come from with these assumptions? I come from a home-world, so my worldview is a home-world view, and completely external to the lifeworld of the homeless person. How would I behave in his/her position? These issues could become the subject of my contemplative practices, or in the poetry, or in reflective journals, or in deconstructing some concepts related to homelessness phenomenon (for example, the concept of deviation).

By practicing meditation and contemplation of phenomena considered as obvious, I get rid of prejudices, assumptions that usually duplicate and restore existing knowledge. If I create a space where nothing exists for a moment, I can observe images, words that I notice now, which I did not notice before. Intuition is the observation of things that are habitually imperceptible.

Intuition appears as a vague image, a feeling, or some inner vague power: "The more a person practices bringing on the calm inside and listening, with the special mode of attention which characterizes intuitive listening, the more precious will be his awareness, the more subtle will be the sensations perceived" (Petitmengin-Peugeot 2002:71-72). Essential here may be the observation of the feelings of the body, which often informs us earlier and better about what is happening in ourselves and outside us. The body and its position often determine the meanings we give to the world and how we understand it and how we perceive it (Byczkowska-Owczarek and Jakubowska 2018).

Asking questions in silence when the researcher is alone with himself/herself, he/she may exceed cer-

tain limits of consciousness. He/she may ask questions that would not necessarily appear in formal scientific discourse (during conferences, debates, etc.). By asking whether the world is limited and unrestricted at the same time, he/she reaches the boundary of cognition, gets rid of the assumptions and the whole corpus of empirically and logically confirmed knowledge. We remain open at this point of time and space for an intuitive experience. How to express this experience? In what language? At what moment and in front of what kind of audience? Is it the subject matter for separate considerations? Although, we should remember that the image of the audience is often important in restricting our imagination and suppression of the ideas that come from the intuitive thoughts.

Intuition can be understood as “first thought,” as in the case of Einstein who used intuitive understanding in solving scientific problems (Scheff 1990:145).⁴ Moreover, it is associated with spontaneity (Scheff 1990:163). Intuition is “virtually instantaneous and an unlabored solution to problems insoluble in an analytic mode” (Scheff 1990:58). It can be expressed with words, but also with images, although it is often necessary to wait for it to be expressed in any language that is understood by others. Visualization of the phenomenon is a very important part of getting to the intuition (Isaacson 2007, chapter 2). Putting the phenomenon in images shows us the features that are not yet dressed in words.

⁴ “A new idea comes suddenly and in a rather intuitive way,” Einstein once said. “But,” he hastened to add, “intuition is nothing but the outcome of earlier intellectual experience” (Isaacson 2007, chapter six).

Meditation can help to nurture intuition.⁵ If, of course, we regularly practice. The core of intuitive word comes from the Latin word *intueri*—“to look inside” without invoking rational thinking. Through meditation we can see the coexistence of the multiplicity of things in the world. Observing or feeling these relationships, bodily feelings about interdependence are extremely important in creative work when we try to associate certain phenomena or objects to create new relationships and perhaps even objects. Recognizing the interdependence of things enables us to find new relationships between them and makes us more creative (Konecki 2016; 2018). Creativity is contained in the body (first feelings indicate that something new appeared), in the first associations, in the first thoughts that arise while solving a problem. However, these feelings do not always appear when we are facing this problem. The problem is in us, it is being developed subconsciously and the solution often appears in unforeseen circumstances.

Phenomenological Epoché and the Limiting of Conceptualization

Creativity and freshness of the view on the reality that surrounds us (and is in us, at the same time) can be obtained by using the phenomenological inspiration. In the phenomenological study, very im-

⁵ Meditation is used by artists to have the possibility to see the whole situation and to have intuition when “to catch the ideas.” Here is an opinion of a movie director, David Lynch: “Ideas are like fish,” he says, “and you don’t make a fish, you catch the fish...In Transcendental Meditation, the twisted stress becomes like water in the sun on a hot pavement. It just evaporates. You don’t relieve the stress. So, you get a stress-free nervous system filled with the being and there are no dark corners. There’s just light, and you see the great big beautiful picture” (see: <https://vimeo.com/182093266> [retrieved June 30, 2018]).

portant is the so-called phenomenological reduction (*epoché*), that is, the bracketing of our assumptions of the conceptual framework, and structures of perception and experience (Rehorick and Bentz 2008:11-12). *Epoché* allows us to study intentionality instead of causality, allowing us to avoid reification of concepts and phenomena (Englander 2016). *Epoché* also allows us to get access to the intuition. We can become free from the obstacles of assumed knowledge. We have two aspects to take in the bracket of our knowledge. The first is about the suspension of scientific knowledge about the phenomenon, the theory about it, so what we have learned. The second aspect concerns the acquisition of knowledge of a given phenomenon from our common knowledge, culture (Rehorick and Bentz 2008:12).

It is important to extract important features of the phenomenon without using our cognitive, socialized filters. "For Husserl, as for Kant and Descartes, knowledge based on intuition and essence precedes empirical knowledge" (Moustakas 1994:26, see also 58).

If we suspend our knowledge and do not refer to existing theories, we can more and more discover what really appears in our lives. Intuitions get started. Often these seem to be obvious structures of experience, but they are not fully aware by the subject. Studying them can be revealing. The substantive dimension of the context of discovery may also emerge here, by suspending our knowledge we can perceive phenomena and related experiences directly, without cultural and scientific filters, related to the current knowledge of the phenomenon. The freshness of vision can ultimately have a scientific significance, and in many cases, it could be a scien-

tific discovery. The phenomenon of "wonderment" here plays a significant role: "Wonderment challenges us to see, to notice, and to take stock of the features of our habituated everyday lifeworlds" (Rehorick and Bentz 2008:5). Wonderment, it is the first step in the creative exploration of the phenomenon that we perceive, often in unusual circumstances, because in everyday life it is often customary and difficult to observe.

Very important in phenomenological research is also the "lonely" self-reflection of the researcher, basing on his/her own experience of the world. While being alone we can discover many features of phenomena without being influenced by others. Phenomenological researcher, Clark Moustakas, writes that his natural inclination was to avoid people and their views. While being alone he could experience the phenomenon directly (Moustakas 1994). Solitude helps in this.

We may observe some similarities between the concept of *epoché* and the Buddhist meditation on emptiness (Bentz and Shapiro 1998:52; Depraz, Varela, and Vermersch 1999; Simpson 2009:61-62; Janesick 2015; Konecki 2016). Methodical removal of knowledge from the mind for some time, beyond the cleansing and hygienic application of meditation, can help to find phenomena and their structures as they emerge here and now without *a priori* concepts that structure our perception of reality.

Considering the possible combinations of *epoché* with Buddhist meditation we can formulate a pragmatic approach to *epoché* that could be characterized by three successive phases:

- a. A phase of *suspension* of the habitual thought and judgment, the basic possibility of a change in the attention which the subject gives to his own experience, which represents a break with a “natural” or unexamined attitude;
- b. A phase of reflective *conversion* of attention from “the exterior” to “the interior”;
- c. A phase of *letting-go* of a reception of the experience (Depraz 2002:122).

Artists as painters, in order to be creative, must also suspend their knowledge, which turns out to be “artificial,” obtained otherwise in the perception of already seen paintings. Even if his/her perspective is rooted in some social context, the freshness of his/her gaze is based on a deep understanding of this context and, ultimately, his/her detachment from the canons. The worldview is burdened with our ideas, especially connected with historical periods (Strzemiński 1958). Reaching the very nature of phenomena, light and observation of the phenomenon here and now let the artist discover new insights into nature. Then, it is only possible real creativity and discovery of new painting principles.

To be creative he/she should see things that are in his/her perspective on a “pre-reflexive” level. This is the openness to Other or object of observation (which is important in sociological and generally humanistic studies) in order not to see things through social and cultural filters as it happens in sociological research based on the full conceptualization of research, either quantitative or qualitative. The full conceptualization is based on the

scientific filters or, as it happens in qualitative research, on *in vivo* codes coming from the vernacular language.

The study of phenomena is possible through self-observation and autobiographical reflection using the phenomenological techniques: horizontalization (Rehorick and Bentz 2008; Simpson 2009), methods of “bracketing” of existing knowledge and the adoption of “imaginative variations,” that is, taking into account many formal different points of view concerning the existence of the phenomenon. Moreover, there should be concerned the horizons of the phenomena, that is, to look at a given phenomenon here and now from different points of view, to have the possibility to reconstruct and understand a given phenomenon by experiencing it (Rehorick and Bentz 2008; Simpson 2009; Kacperczyk 2014; 2017). All of this can have a transformative effect on the perception of oneself and of one’s life. This could be a researcher that explores his/her experiences (Rehorick and Bentz 2008).⁶

Methodology of Grounded Theory— Limiting of Conceptualization

The methodology of grounded theory has the potential of creativity in limitation of pre-conceptualization. It suggests the limitation of pre-conceptualization (Glaser and Strauss 1967), what can be extremely important in the exploratory research of human actions and interactions. It puts a premium on emergent conceptualization (Glaser and Strauss

⁶ In addition, journaling and the analysis of journals can be important here while using also mindfulness techniques (Giorgino 2014a:10; see also Giorgino 2014b).

1967:37). Emergency in which the researcher is engaged is the creative process. So, before leaving to the field, it is suggested to not take too many assumptions about the course of phenomena that we want to investigate. If we want thoroughly to investigate the phenomenon and/or its experience, we try to be as close as possible to the phenomena or participants who are involved in it. When we investigate a phenomenon, we often discover other phenomena associated with it, and this first phenomenon and its relationships we did not anticipate at the time of preparing for the study. Sometimes we can direct our research attention to other phenomena we did not expect to find in the area under investigation. If we have such situations, then we are experiencing a “context of discovery” (*serendipity*).

We can specify two situations related to the context of the discovery. One is the substantive contexts of discovery, when we find empirical cases in a given place and time (institution, social group, community, country, or state) of a particular phenomenon that is not described so far in scientific texts. So, we have a chance in a new and fresh way to describe a specific phenomenon in a given area (if we do not have too many presumptions and ready concepts).

On the other hand, the theoretical context of discovery in the methodology of grounded theory would refer to new connections between the categories that emerged by encountering perceived relationships (more or less accidental) between empirically observed phenomena (see: Konecki 2008:183). With this approach, the substantive context of discovery precedes the theoretical context of discovery. Although it is conceivable that the construction of

grounded theory follows the collection of various extant case studies and the empirical studies that have been carried out, when we do comparative analysis, without the personal involvement of the researcher in field research. At that time, it is the situation in which the analyst can experience the effect of “aha” (Topolinski and Reber 2010) and he/she also sees new relationships between categories or configurations of meanings. The theoretical context of discovery may in various ways relate to a researcher’s personal involvement in fieldwork, but does not need to be linked to it, however, it is usually linked to the use of a constant comparative method.

In general, by limiting pre-conceptualization in qualitative research, we increase the chance to “discover” new relationships between the phenomena and the categories that describe them. Such innovations come from the creative potential of the limited pre-conceptualization. We do not impose categories and hypotheses derived from existing theories or empirical studies. Therefore, the context of discovery is an immanent feature of grounded theory methodology that builds creative context for research.

Generally, grounded theorists look for the patterns and the causal conditions of the phenomena (Sy 2010:104). However, it would be interesting to use the *epoché* in a minimalistic way and clean the mind and see what is left if we abandon our researcher identity that is directing our search for reasons of phenomena. What happens if we avoid the causal thinking and concentrate only on the conscious perception of the reality by the participants? What

happens if we would like to put on the proverbial shoes of the homeless person? The use of the contemplative techniques could be helpful in the empathetic understanding of the other. The compassion is not a by-product of the research in such a situation, it becomes an integral part of it. So, preparing for the appearance of such feelings is important also for grounded theorists that look for the theory. His/her feelings should be also part of the analyzed research situation because he/she is a part of the situation whether he/she wants it or not.

Creating the theory in a creative way means combining the deduction with induction, but in a very open manner. The deduction could be based on thought experiments. The thoughts experiments could be very fruitful for creating the theory, even without the proofs of empirical data or experiments, as we know from the creative biography of Albert Einstein (Isaacson 2007, see chapter six). Einstein imagined, for example, that he travels on a beam of light and at that moment he asked many important questions about the perception of time and space. The induction could be inspiring, but the deductive, concise, and rigorous way of elaborating the concepts is the final act in constructing the theory (Einstein 1919). However, the grounded theory starts from the induction and replete with the deductive way of formulating the hypotheses that are still being grounded by further sampling and analyzing of the data. It is an iterative process. We can call it an abductive way of reasoning (Strauss 1987:12; Charmaz 2006:186). Therefore, to have more deductive and consequently abductive skills the limiting of pre-conceptualization seems to be indispensable. The meditative and contem-

plative practices could be very helpful here. The thought experiment of being the homeless person during the harsh winter and warm summer could bring us many interesting comparative questions that could give inspiration to the theoretical sampling. When and where should we make the next field explorations? We can also formulate hypotheses about the embodiment of homelessness in referring to the weather conditions or climate conditions.

Ethnography and the Context of Discovery

Fine and Deegan (1996) distinguish three types of contexts of discovery (*serendipity*) relevant to ethnographic research:

1. Temporal context of discovery. It is the ability to find new sources of data, to be in specific places at a certain time, which can be decisive for further observation and analysis.
2. Relational context of discovery. Finding the right informants, often by chance, is extremely important for discoveries. The description of the relationship itself can also be interesting for further analysis as an empirical material.
3. Analytical context of discovery. There is a process of linking data with existing theories or formulating proposals to modify them that allows us to find a basic category or metaphor for integrating empirical data. This results in an emergent conceptualization of the problem (problems) based on collected empirical data.

Contexts of discovery appear parallel with planned actions, though they are unplanned and difficult to predict. Although the micro-ethnographical research is embedded also in the macrostructures (Fine 1991; Wojciechowska 2015; Müller 2019), on the micro level there are not only processes and psycho-social phenomena, but also the researcher himself/herself (Chomczyński 2017; 2018). However, as can be seen from the above catalog of contexts of discovery in ethnography, much depends on the researcher's ability and skills to be mindful of time, space, and knowledge that emerge (Konecki 2018). Discovering is hard work related to performing specific activities, both in the field and in conceptual work related to the study of theoretical concepts. The ethnographic discovery does not appear automatically, the researcher's active participation in it is of great importance. So, I think that the contemplative practices like meditation, yoga, or writing the reflective journals could help the ethnographers to be mindful during the field research to make the serendipity happen, it means to notice these situations. The world could be serendipitous if we have the observational skills to see the world around, but also to see what is happening in us. The signals of strangeness or wonderment or surprise could appear in our bodies or in some short moments in our mind. Being mindful and experienced in noticing the moments is indispensable to include serendipity dimension in our research. Here, the creativeness of the researchers means to have a deep insight to see more outside.

Robert Merton has a different view on serendipity. He argues that "under certain conditions, a research finding gives rise to social theory" (Merton

1968a:157). He believes that by analyzing empirical data and by accident, new hypotheses can be discovered, even those not studied by the researcher. This situation is about research experience, in which we observe some fact that is:

- unexpected—empirical research to verify the hypothesis creates a by-product incidentally—unexpected observation associated with the theories that were not taken into account at the beginning of the study (Merton 1968a:171);
- anomalous, it does not match existing theories and/or established facts;
- strategic for research—it must be very important, in some way, for the existing theory (Merton 1968a:158–162; see also Merton and Barber 2004).

It is evident in Merton's conception that the context of discovery is merely a by-product of the main purpose of research. In empirical research, we verify the hypotheses already set forth. In addition, we have an unexpected observation of certain facts to existing theories. The idea is to use already existing theories that can explain the surprising data or widen existing theories instead of discovering new theories. In the theoretical sense, nothing new was discovered here. Certain unexpected facts were theoretically explained. The environment in which surprising discoveries are made, according to Merton, is *institutionalized serendipity*, that is, scientists in certain environments and institutions exchange ideas and information with each other and inspire each other. Thus, one can say that there is a "pattern of the context of discovery." According to Merton,

the “accident” and the ability to see it is inherited in the social structure, and not in what is commonly considered a coincidence or a stroke of luck (Merton and Barber 2004; see also Merton 1968b:4, 7).

The environment in which the context of discovery takes place does not necessarily have to be institutional, as Robert Merton argues. It could be a psychological atmosphere in which the researcher lives. His/her propensity to laugh and feel relaxed and distanced from himself/herself could be more important than institutional conditions (Scheff 1990). The lessened concentration on ego by meditative and contemplative practices could open the mind and heart of the researcher and increase the empathetic skills that allow him/her to feel more and discover more from the described lived experiences of the participants of the research.

Concerning the concept of Merton, it is very difficult to find a unified pattern of discovery context that could be structuralized. If we find a pattern, it will be rather a context of justification rather than a context of discovery (Reichenbach 1938:5-7). The context of justification refers to showing that theoretical claims have been formulated in accordance with scientific rules, but this does not show actual action/interaction in the real and complex situation of discovery. This is often done in a social world in which the researcher participates (e.g., the social world of photography, art, sociology, ethnography, or on the border of these worlds, etc.) or which he/she is currently analyzing (Becker 1982). The balancing on the border of many social worlds is very creative for the researcher, he/she has to be more concentrated to see and connect the knowledge from many fields and build his/her own and new corpus

of knowledge. The researcher’s activities, which take place in some social worlds, can become an inspiration for his/her discoveries of the social world processes, as social world of climbing (Kacperczyk 2016) or social world of companion animals’ lovers (Konecki 2005) or hatha-yoga practitioners (Konecki 2015). Contacts and interactions with other participants in this world (worlds) influence upon mutual inspirations, adding knowledge, perceiving someone’s knowledge from different perspectives, changing horizons, refusing/bracketing own assumptions, and eventually leading to scientific discovery.

Conclusions

From the above review of the issues of creativity in qualitative research we can derive the following conclusions:

1. Creativity in qualitative research is dependent on the strength of *a priori* conceptualization and stiffness of the adapted research methods. The more conceptualization before the research, the less innovative findings. Sensitizing concepts could be the choice because they are not limiting us to check and verify their validity, but they open to us some possibilities of seeing the phenomenon, although we are still going in some direction, even the sensitizing concepts have the temporary descriptive definitions. Who is the homeless person? We have many official definitions that direct us in our research. However, we can see that the definitions refer mainly to material conditions of life, not having a home. Legal designations seem here decisive. However, homelessness has also other dimensions that could be investigated if we

are not so much addicted to the ready concepts. The phenomenon also has a psychological, medical, esthetical, and ethical dimension.

2. If the methodology is more flexible and pragmatic (such as the theory of grounded theory or phenomenology), the researcher can more easily and creatively come to the phenomena that he/she had not realized before and which are still under investigation in his/her field of knowledge. The researchers coming from different methodologies could inspire themselves by referring to procedures from other methodologies that could be inspiring or useful in the field work. *Epoché* could be combined with the limitation of pre-conceptualization in grounded theory style of research. Grounded theory researchers could not only limit his/her plans and concepts before going into the field, but also bracket his/her assumptions and knowledge using the procedures of phenomenological *epoché*.
3. Combining phenomenological and contemplative methods makes it possible to use in the inquiry the researcher's experiences related to feeling and experiencing of the studied phenomena. Living experiences of the researcher are not used as a method of getting the empirical materials in objectivist and positivist studies. We can here use the protocols from the researcher's natural history of experiencing the research not for checking the validity of the research, but for inspiration to analyze the empirical findings. The body of the researcher, if mindfully observed, could tell us how he/she felt and what could be felt in the situation, for example, being homeless.
4. The researcher may therefore consciously use certain methods that promote creativity in phenomenological and contemplative approaches. Concerning the phenomenon of homelessness, the phenomenological research shows that not only the material conditions of life (as is indicated in the grounded theory research of Konecki 2009) are important for homeless people, but also the strategies to keep the hope for change. Imagined future home, connections with friends and family are important for surviving on the street that is often the source of hopelessness and despair (Partis 2003). The spiritual dimension could also be important here as we can see in the phenomenological research of Hall (2010). Therefore, the techniques that are more oriented on exploring the consciousness of human beings are very valuable in investigating the phenomena.
5. An investigator can improve his/her thinking and creative thinking skills by performing certain exercises (bracketing his/her assumptions, writing a journal, poetic summaries of collected data, applying meditation, doing constant comparisons, humor, etc.). The ability to suspend knowledge, contemplation (diversion of attention to the inside), "letting go" activity can therefore be practiced. So, all the techniques and methods from different methodologies that I mentioned in the paper could be helpful in building creativity in the qualitative research.
6. Participation of the researcher in the social world/s sensitizes him/her to certain phenomena, which he/she may combine with existing concepts or theories and create innovative theoretical connections. The living on the borders of the so-

cial worlds is very inspiring and creating the context for serendipity because we could see more while basing on different corpuses of knowledge, as being on the border of the photography world, art world, and sociology world (Becker 1982) or on the border of painting world and sociology/scientific world (Fine 2004). However, to combine these pieces of knowledge, we should be mindful and able to see more than from only scientific perspective and already well-known concepts.

For creativity in qualitative research, it is important to be open to direct experience and to see the phenomenon as it is (from the point of view of contem-

plative research and phenomenology and symbolic interactionism and methodology of grounded theory and the context of discovery). Very helpful to the researcher's openness may be the use of contemplative and mindfulness techniques (Konecki 2016), as well as yoga practice (Konecki 2015) and the limitation of the conceptualization of research, as it is suggested in grounded theory. Openness also involves paying attention to the side effects of the original research goal (serendipity effect), and it is also important to accept chanciness during the project, even if the observed cases seemed not to be related to the research project themselves. However, it may be important for the project itself, also in the substantive sense.

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Social Problems and the Contextual Compromise: Subjectivity, Objectivity, and Knowledge in Everyday Life

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Abstract This article builds on recent sociological debates about the explanatory importance of claims-making contexts and the continuing challenges associated with subjectivism and objectivism in social problems research. The sociology of knowledge is used to illustrate how the contextual compromise that has sustained social problems theory and method for at least two decades is based on a number of erroneous assumptions about subjectivity and objectivity in the tradition of phenomenological analysis. To strengthen recent discussions about the contextual dimensions of claims-making activities and framing techniques, the article critically assesses the curious neglect and continuing misrepresentation of the sociology of knowledge in constructionist analyses of social problems.

Keywords Claims-Making; Social Problems; Contextual Constructionism; Phenomenology; Sociology of Knowledge

In the early 1920s, sociologists began arguing that what makes social problems socially problematic is not only, or even mainly, the objective harms that social-structural conditions pose to human well-being. Rather, sociologists such as Case (1924) and Frank (1925) pointed out that the problematic aspects of social problems more commonly stem from people's value judgments about

harmful structural conditions, whether they are partial or complete, real or perceived. In the absence of value judgments assigning moral worth to social-structural phenomena that are perceived unfavorably at certain moments in time, these early 20th-century sociologists recognized that no social-structural condition in and of itself constitutes a social problem.

Throughout the 1930s and 40s, sociologists built on the value-conflict perspective by highlighting the limitations associated with explaining social problems on the basis of the ostensible harms they pose (e.g., Waller 1936; Fuller 1938; and see Fuller and Myers 1941). While more familiar analogous theoretical developments took place in ethnomethodol-

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ogy, phenomenology, and labeling/deviance theory leading into the 1960s (e.g., Becker 1963; Schur 1965; Berger and Luckmann 1966; Garfinkel 1967), it was not until the 1970s that sociologists started to systematically think beyond objective structural harms by placing claims-making activities and social problems framing at the center of analysis (Kitsuse and Spector 1973; Conrad 1975; Pholf 1977; Spector and Kitsuse 1977). A lively debate about social problems theory and method played out in American sociology over the following two decades (e.g., Schneider 1985; Woolgar and Pawluch 1985). By the early 1990s, the social constructionist or definitional perspective had revolutionized how social problems research was done (Holstein and Miller 2003).

Since the mid-1990s, the influence of social constructionism on social problems research in disciplines spanning sociology and anthropology to management studies and nursing has been considerable (Loseke 2015). Innovative studies on the social construction of social problems have explored issues ranging from the impact of new digital technologies on social problems framing (Maratea 2008; Kampf 2014; Walsh 2016) to the ways in which government claims-making activities can actively deny allegations of government torture (Del Rosso 2011; cf. Cohen 2001). While there is little doubt that social constructionism has made a decisive impact on scholarly debates about the nature of social problems for at least a half century, sociologists have nevertheless recently returned to debates about the explanatory importance of claims-making contexts and the continuing challenges associated with subjectivism and objectivism in social problems research.

In his attempt to bypass the familiar standoff between objectivism and subjectivism in the sociology of social problems, for example, Thibodeaux (2014) argues that social problems theory suffers from a lack of integrity. Because social problems researchers have continued to blur claims and conditions (understood as subjective interpretations and objective realities, respectively), he says, “social conditions must be brought back in” (Thibodeaux 2014:830). For Thibodeaux, returning to social conditions does not signal a regression to objectively harmful conditions but rather the need to divert the empirical focus of analyses away from naïve forms of objectivism qua social facts and towards the social, political, and economic opportunity structures in which claims are made and acted upon. In Thibodeaux’s assessment, investigating social problems as the study of claims-making contexts eliminates the need to separate objective conditions from subjective interpretations of social problems by reimagining constructionism as the comparative study of the timing and prominence of how objective condition-contexts influence the production and reception of social problems claims and frames.

Just as Thibodeaux emphasizes the importance of investigating claims-making contexts, Weinberg (2009; 2014) similarly encourages social problems researchers to better account for the social-structural contexts within which claims-making activities take place. Aiming to preserve the legacy established by John Kitsuse, he calls on sociologists to account for the normative warrant that research subjects attribute to their own claims and to the claims of others. For Weinberg, however, the Kitsusian emphasis on actors’ perspectives need not come at the expense

of sociologists' interpretations of what actors count as such warrants. To move beyond the antinomy of subjectivism and objectivism, Weinberg (2009:62) therefore urges sociologists to pay greater attention to how "putatively problematic conditions, once assembled as meaningful objects of discourse and practice, might become dialectically related to the discursive claims made about them." In other words, claims-making activities are vitally important to the sociological study of social problems, but empirical analyses of social problems rhetoric in practice cannot be adequately explained without reference to the normative structural contexts from which claims-making activities emerge.

Finally, in one of the most recent arguments about the importance of attending to claims-making contexts in social problems research, Nichols (2015) defines contexts in terms of the pre-existing social fabric that affects who can make claims and what claims can be made, including when, where, and why certain claims are levied and the responses they elicit. In this way, Nichols points to the analytical importance of attending to context work: the implicit assumptions that claims-makers and sociological analysts rely on to make assertions about types of people, social settings, and social scale, as well as the ways in which meanings about certain sets of phenomena are indexed to meanings about others. For Nichols (2015:78), paying greater attention to the social-structural dimensions of context work not only avoids the counterproductive debate between strict and contextual constructionists that has defined social constructionism since the early 1990s. It also reaffirms the methodological importance of the interpretive tradition by understand-

ing how human beings create meanings and then apply them in social interaction (for similar arguments see: Ayukawa 2015; Furedi 2015; Xu 2015).

The resurgence of debate about conditioning contexts is an exciting development in the sociology of social problems. This article builds on the recent debates by revisiting the basic premises of the sociology of knowledge (more commonly known as the social construction of reality). The analysis illustrates how the contextual compromise that has sustained social constructionism for at least two decades—that claims-making activities are contextually situated in relation to objective social and material conditions—is based on a number of erroneous assumptions about the meanings of subjectivity and objectivity in the tradition of phenomenological analysis. To strengthen recent discussions about the contextual dimensions of claims-making activities and framing techniques, the article therefore critically assesses the curious neglect and continuing misrepresentation of the sociology of knowledge in social problems theory and method.

Claims-Making and the Activities-Based Approach

Social problems researchers are well aware of the fact that the claims-making or activities-based approach to studying social problems was formally inaugurated with the publication of Malcolm Spector and John Kitsuse's *Constructing Social Problems* (1977). Prior to the publication of this groundbreaking book, Kitsuse and Spector (1973) reworked earlier contributions to the value-conflict perspective

(Waller 1936; Fuller and Myers 1941) by explicitly conceptualizing social problems as generic processes of making claims about “putative” conditions. They were aided, if only indirectly, by Peter Berger and Thomas Luckmann’s (1966) celebrated treatise, *The Social Construction of Reality: A Treatise in the Sociology of Knowledge*. Berger and Luckmann’s text not only helped to popularize the term social construction but also to shape sociological theorizing on the importance of institutionalized stocks of knowledge, practical activities, and routine behaviors in everyday life. Howard Becker’s (1963) contributions to the sociology of deviance and labeling theory provided additional background support to facilitate the favorable reception of *Constructing Social Problems*, supplemented by Herbert Blumer’s (1971) famous definition of social problems as a form of collective behavior and Armand Mauss’s (1975) influential argument that social problems represent a special kind of social movement activity.

Spector and Kitsuse (1977:1) introduced *Constructing Social Problems* with the provocative statement that, “There is no adequate definition of social problems within sociology, and there is not and never has been a sociology of social problems.” What they meant was that social problems research lacked intellectual coherence and a common sense of purpose and direction. To be sure, by the late 1970s there was no shortage of sociological research on a variety of issues being identified as social problems. What was missing from the research literature, Spector and Kitsuse argued, was a clear, shared, and reliable definition—a conceptual framework—to explain what actually constitutes a social problem in the first place.

In the absence of a coherent theory or method capable of linking social problems research across the otherwise disparate topics that sociologists were investigating under the guise of the sociology of social problems, Spector and Kitsuse observed that sociologists overwhelmingly relied on their own tacit assumptions about harmful conditions to select and investigate topics that they then represented as objective social-scientific research findings. Owing to the poorly developed state of social problems theory and method, contributions to the field were often characterized by an assortment of descriptive characteristics about topics that sociologists arbitrarily selected on the basis of their own normative value judgments.

As sociologists whose primary interests were focused on understanding the ways that people enact meanings about social problems in everyday life, Spector and Kitsuse were keenly interested in how social problems are constructed through assumptions about and changing perceptions of problematic phenomena. The latter included the tacit assumptions of sociologists and other kinds of specialists. Where they parted company with the majority of mainstream sociologist, however, was in their steadfast methodological assertion that the appropriate subject matter for social problems research is not objectively problematic social and material conditions but rather the forms of generic social interactions involved in making assertions of grievances or claims about putatively problematic social conditions and arrangements.

To develop social problems as an independent or stand-alone area of empirical sociological research,

anchored by a central subject matter and a common set of theories and methods, Spector and Kitsuse therefore began by conceptualizing social problems in terms of claims-making activities. They argued that the phenomena we commonly think about as, and characterize in terms of, social problems do not represent objective worldly conditions whose inherently problematic features inexorably activate corrective measures to reduce harm. By contrast, Spector and Kitsuse maintained that social problems are properly defined as assemblages of discourses and claims brought into existence through a sequence of capricious human interactions. The latter is composed of many different kinds of claims-makers who engage in many different kinds of practical activities to define putative conditions as social problems and to generate viable strategies for their remediation.

Spector and Kitsuse's argument that the phenomena we commonly identify as social problems cannot be separated from the subjective judgments and definitional activities of claims-makers was, as it remains, controversial. In their attempt to develop the activities-based approach to social problems theory and method, they essentially advised sociologists to avoid making any reference to the influences that social problem conditions (i.e., the actual things that social problems claims are ostensibly about) have on how people subjectively interpret harm. For Spector and Kitsuse, every time sociologists lay claims to the influences that objectively meaningful social problem conditions have on problematizing activities—for example, the influences that deviant behaviors, social pathologies, or structural inequalities have on framing processes—they not only fall into the trap

of conceptualizing social problems as an effect of some more fundamental condition (essentially making the study of social problems about something other than claims-making activities). They also fail to appreciate how the definitional perspective is oriented towards understanding the ways in which social problems are constituted through, rather than external to, different configurations of interacting people. In this way, Spector and Kitsuse did not simply develop a new strategy for investigating the issues that sociologists had for generations identified as otherwise inherently problematic social problem conditions. What they provided was a distinctively (albeit underdeveloped) phenomenological approach (see below) to conceptualizing social problems exclusively in terms of social interactions called claims-making activities.

Ontological Gerrymandering

Spector and Kitsuse's activities-based perspective provided sociologists with a provocative, if contentious set of theoretical and methodological arguments to address the shortcomings associated with condition- or harm-based approaches to studying social problems. In several important ways, the limitations that Spector and Kitsuse (1977) identified with condition-based approaches pushed sociologists to think harder about what actually makes a social problem socially problematic. In doing so, however, their arguments also posed an unanticipated set of theoretical and methodological challenges that contemporary sociologists continue to struggle with.

The challenges stem from Spector and Kitsuse's original proviso that sociologists should investigate

all of the “subjective” claims-making activities that groups of people participate in to create and sustain meanings about “putative” conditions that they define as social problems. By using the term subjective to describe claims-making activities, Spector and Kitsuse were not arguing that claims should be analyzed in terms of personal opinions or biased perceptions. Nor were they suggesting that the truth about social problems is radically relative or entirely arbitrary. Rather, their intention was, in the first instance, to avoid reifying social problems as unchanging and inherently harmful phenomena by focusing on the forms of social interaction that give rise to changing definitions of putatively problematic conditions, be they entirely compatible with or in complete contradiction to various kinds of evidence we tend to call objective.

Throughout the 1970s, influential studies on the social construction of social problems investigated the social construction of everything from alcohol abuse and automobile accidents (Gusfield 1975) to hyperactive children (Conrad 1975). Many of these studies were published by the Society for the Study of Social Problems (SSSP)—an organization originally founded in 1951 to provide an alternative perspective on what was perceived as mainstream elitist, scientific sociology (Abbott 2001). It is, therefore, no surprise that many of the studies on claims-making and problem framing, with their connections to labeling theory, were presented in the SSSP’s journal, *Social Problems*. But, as Steve Woolgar and Dorothy Pawluch (1985) explained, by the early 1980s sociological studies on the social construction of social problems had come to rely on a combination of three specific rhetorical techniques that, when applied in

empirical research studies, actually contradicted the main premises of the definitional perspective.

According to Woolgar and Pawluch, studies on the social construction of social problems typically begin by identifying certain behaviors or conditions to investigate in a matter-of-fact way (e.g., abortion). Following the identification of the condition under investigation, sociologists then proceed to explore changing claims about the same, ostensibly unchanging condition (e.g., pro-life, pro-choice). Finally, after showing how “subjective” definitions about seemingly “objective” conditions fluctuate over time, social problems researchers emphasize the variability of “subjective” social definitions in relation to unchanging “objective” conditions. The purpose of emphasizing the historical variability of social definitions, Woolgar and Pawluch explained, is to confirm the theoretical argument that social problems frames do not derive from the inherent features of objectively problematic conditions but rather subjective or situation-specific claims-making activities.

To illustrate how social problems researchers rely on a rhetorical explanatory framework to demonstrate the importance of the definitional perspective, Woolgar and Pawluch singled out Pfohl’s (1977) well-known study of the social construction of child abuse. Pfohl (1977) began his study by introducing what he identified as the age-old practice of child beating as an important topic for sociological investigation. He argued that although the ancient practice of physically punishing children had been “subjectively” defined as a necessary disciplinary practice prior to the nineteenth

century, nineteenth- and early twentieth-century social reformers (such as activists associated with Society of the Prevention of Cruelty to Children) increasingly replaced the disciplinary frame with one centering on “child abuse” as a form of social deviance and, later, a crime. For Pfohl, subjective changes in the ways in which the objective condition of child beating has been defined over time demonstrate that the contemporary social problem frame of child abuse derives from the socio-historical circumstances that claims-makers find themselves in rather than the actual condition of child beating itself.

In Woolgar and Pawluch view, the emphasis that social constructionists like Pfohl place on claims-making activities (such as the routine behaviors that were involved in early twentieth century child welfare advocacy, e.g., petitioning lawmakers, holding rallies, producing printed materials) is entirely compatible with the definitional perspective. But, they queried how sociologists like Pfohl could logically conceptualize changing social problem frames (e.g., discipline, child abuse) in terms of subjective claims-making activities that construct or enact definitions of problems while at the same time making their own tacit and unexamined claims about the unchanging status of the condition (i.e., child beating). In other words, Woolgar and Pawluch pointed out that Pfohl and sociologists like him rhetorically deploy terms like “child beating” as though they are impartial descriptions of an otherwise objective and unchanging social reality, yet selectively use other terms like “discipline” and “child abuse” to signify historically subjective moral evaluations that are deserving of sociological scrutiny.

Woolgar and Pawluch called this common rhetorical approach to studying the construction of social problems “ontological gerrymandering.” Ontological gerrymandering, they explained, is a form of analytical boundary work that rhetorically portrays some phenomena (e.g., child beating) as historically fixed and unchanging (i.e., objective) while depicting other phenomena (e.g., definitions of discipline and abuse) as historically variable and susceptible to change (i.e., subjective). When sociologists engage in the boundary work of ontological gerrymandering, they make certain implicit assertions about how social conditions “really are” (even when they merely identify some topics for investigation and ignore others) to, paradoxically, demonstrate how problematizing definitions or claims cannot be explained on the basis of assumptions about how social conditions “really are.” By relying on their own unexamined assumptions about the consistency of conditions (i.e., ontological assumptions about “what is”) to demonstrate the irrelevance of conditions for explaining problematizing activities, Woolgar and Pawluch argued that sociologists were essentially conceptualizing the definitional activities of claims-makers as malleable social constructions while at the same time assuming that their own claims about conditions represent objective and unchanging truths (i.e., a form of gerrymandering or selectively manipulating the analytical boundaries of social reality).

The Bifurcation of the Social Constructionist Perspective

Woolgar and Pawluch’s critique of the rhetorical strategies that social constructionists implicitly

rely on to investigate how claims-making activities constitute social problems qua human social interactions was meant to remind sociologists about the original methodological arguments that Spector and Kitsuse made (and why they made them). Woolgar and Pawluch specifically emphasized the explanatory difficulties that sociologists encounter when they fall back on their own value judgments about problematic conditions (like child beating) to develop analytical insights into social problem claims-making activities. In line with Spector and Kitsuse, Woolgar and Pawluch reasoned that if social problem framing cannot be explained on the basis of actual or perceived harmful conditions qua material facts, then the notion that harmful material conditions can productively contribute to a theory of claims-making activities about social problems definitions should be abandoned. The latter includes sociologists' putative assumptions about the invariance of objective conditions in relation to changing subjective definitions.

When Woolgar and Pawluch pointed to the common practice of ontological gerrymandering, their aim was to strengthen research on the social construction of social problems. Their critique had the opposite effect, however, by inadvertently contributing to the bifurcation of the social constructionist perspective. On the one hand, a group of constructionists sympathetic to Spector and Kitsuse's version of the activities-based approach doubled down by insisting that the phenomena we commonly identify as social problems are inseparable from subjective or situation-specific claims-making activities. The so-called strict social constructionists, as they came to be known (e.g., Ibarra and Kitsuse

1993), heeded the charge of ontological gerrymandering by affirming the explanatory irrelevance of material conditions in sociological explanations of social problems claims-making activities. Their argument was not that the reality of harmful material conditions (e.g., poverty, war, violence) equates to nothing more than a set of capricious human interpretations and perceptions. Their point, rather, was that constructionists need to resist the seduction of reifying social problems by remaining focused on the claims-making activities and forms of human social interaction through which interpretations of social problems are enacted and maintained (Holstein and Miller 2003).

On the other hand, responding to the perceived deficiencies of the strict version of constructionism, a competing group of constructionists (who were, incidentally, mostly responsible for recasting the strict constructionists' methodological arguments in terms of a theory of the reality of material harms themselves) rejected the view that social problems researchers should ignore objectively verifiable social and material conditions. The latter included the objectively verifiable socio-historical contexts in which certain kinds of claims are made. Similar to the strict constructionists, the "contextual" constructionists argued that claims-making activities are, in the main, crucially important for understanding how social problems are framed and acted upon. Like the strict constructionists, the contextual constructionists remained committed to investigating the "subjective" or definitional aspects of social problem construction. But, the contextual constructionists also insisted that just because social problem definitions are constituted through

claims-making activities does not necessarily mean that sociologists are unable to discriminate among and assess the validity of various claims in relation to objective evidence about the actual conditions that are being investigated. As Best (1989:247) put it, “calling a statement a claim does not discredit it.”

To demonstrate the importance of the contextual version of social constructionism, Best (1993) offered the example of claims-making activities about Satanism in the 1980s. By the late 1980s, he explained, US television talk shows and popular media outlets were routinely laying claim to the menace of blood cults involved in sexual abuse and human sacrifice. Set in the broader socio-historical context of social concerns about serial murder, missing children, heavy-metal music, and child sexual abuse, he explained, Satanism was only one component in a “great web of evil” (Best 1993: 110) occupying the popular imagination at the time.

For Best, the limitations of strict constructionism are exemplified by the example of Satanism. Because strict constructionists are mandated to inquire into the claims-making activities or actual behaviors that have brought the social problem frame of Satanism into being, they examine claims made by police officers, social workers, journalist, and ritual crime specialists. What they refrain from doing is examining, first, if there are in fact any actual people who engage in Satanic worship (i.e., assessing the validity of the actual condition) and, second, explaining why claims about Satanism appeared specifically in the socio-historical context of the 1980s (rather than the 60s or 70s). According to Best, by trying to avoid the contamination of naïve forms of objectivism that

characterize both condition-based approaches and constructionist ones that are guilty of ontological gerrymandering, strict constructionists essentially threw the baby out with the bathwater. They did so, he insisted, by shunning all assumptions about empirically verifiable social conditions *and* the social settings and historical contexts that condition social problem frames at certain times, in particular places, and across social spaces.

The contextual version of social constructionism has been appealing to social problems researchers over the past quarter century. Perceived as a sort of compromise position within social constructionism, the contextual approach has been used to examine the “objective” socio-historical circumstances in which claims appear (e.g., the post-9/11 era) while at the same time striving to make a practical contribution to public discourse by continually assessing the validity of “subjective” claims-making activities in relation to objectively verifiable conditions (such as US governmental claims-making about weapons of mass destruction after the 9/11 attacks on Washington and New York).

The challenge that contextual constructionists continually encounter, of course, is that they run the risk of reproducing the same fundamental problems that were initially identified in condition-based perspectives. Not only do they demonstrate the kind of selective relativism that Woolgar and Pawluch warned against in research on the constitutive features of claims and frames. Possibly more damaging, by explaining the subjective dimensions of social problem framing as internalized features of “objective” socio-historical circumstances (at least

in the absence of the more detailed or nuanced framework elaborated on below), they inadvertently foster the impression that claims-making activities are merely a priori effects of social-historical conditions (cf. Thibodeaux 2014; Weinberg 2014; Nichols 2015). As critics have been quick to point out, it is not therefore clear what role claims-making activities actually play as a constitutive (or world-making) component of social problem frames if they are explained as the inevitable expression of objective socio-historical conditions.

Social Problems and the Sociology of Knowledge

One of the regrettable features of the cleavages that developed within social constructionism is that the debate was (as it remains) staged in either/or terms. Partially owing to the conceptually imprecise language used in Spector and Kitsuse's (1977) original writings, social problems have been repeatedly conceptualized as *either* subjective definitions *or* objective conditions. To reiterate, Spector and Kitsuse did not use the term subjective to imply that claims-making activities are akin to personal opinion. Rather, they deployed the term subjective to investigate the claims-making processes through which meanings about social problems are created, maintained, disseminated, reproduced, and/or transformed.

Despite Spector and Kitsuse's clear intention to formulate a theory of claims-making qua routine practical human activities, however, their framework was, as it remains, tacitly construed by constructionists and condition-based researchers alike as a choice that social problems researchers face: to

either investigate subjective-idiosyncratic definitions or objective-material and/or socio-historical conditions (or, in the contextual version of social constructionism, to blend them). It is worth reiterating that Spector and Kitsuse never declared that objectively harmful material conditions do not exist independently of human consciousness and definitional activities. In fact, in one of their seminal statements on the definitional approach they readily conceded that, "It is an empirical question whether certain types of conditions are correlated with or associated with certain types of claims" (Kitsuse and Spector 1973:148). Their point was simply that there are no guarantees that harmful material conditions will be problematized (or that harmless ones will not) for the simple reason that social problems *definitions*—and hence "social problems" as such—are always constituted through claims-making activities of some kind. Spector and Kitsuse therefore stressed the importance of developing a theory of social problem claims-making activities that does not pivot on (or gerrymander) the reality of material harms.

Setting aside the circumscribed way in which Spector and Kitsuse used the term subjective to characterize social problems as a sequence of human interactional accomplishments, most of their attention in the 1960s and 70s was admittedly focused on demonstrating why objective material conditions are not reliable indicators of social problem framing. Because Spector and Kitsuse concentrated on the explanatory limitations associated with harm-based perspectives, they did not sufficiently explore the theoretical underpinnings of their own approach (Loseke 2003). Had they taken time to better clarify

the ways in which they were working with a phenomenological framework linked to the sociology of knowledge—one that places an explanatory premium on routine activities, processes of human meaning-creation, and especially the interpretive socio-historical resources that people draw on to navigate everyday social life—much of the confusion about the status of objective conditions in social problems research on claims-making activities might have been avoided.

It is, in this regard, more than a passing irony that the bifurcation of social constructionism took the form of a subjective/objective standoff when we consider that Spector and Kitsuse loosely, but unmistakably structured their understanding of social problems on Peter Berger and Thomas Luckmann's (1966) *The Social Construction of Reality: A Treatise in the Sociology of Knowledge*. Nowhere is this influence clearer than in Ibarra and Kitsuse's (1993) refined explanation of the definitional perspective, where they explicitly talk about the importance of institutionalization, typification, vernacular resources, moral universes, and the language-bound character of social reality.

In *The Social Construction of Reality*, Berger and Luckmann go to great lengths to dissolve the distinction between subjective and objective social reality by highlighting a fundamental paradox characterizing human social existence: namely, that human beings create (rather than discover) social realities (or ways of being human) that they subsequently experience as something other than ongoing human accomplishments. Examples are all around us: religious systems, family arrangements, racial/gen-

der identities, sexual mores, and moral codes. Not only are such realities (be they norms, values, expectations, roles, rituals, customs, or conventions) subjectively constituted through reciprocal typifications—that is, shared ways of apprehending oneself, others, and the social world—that develop into easily recognizable collective habits and routines (i.e., institutions). They are also maintained through everyday practical activities that have become objectively embedded in reified stocks of knowledge that are available to each member of a social group through the processes of primary and secondary socialization (see: Berger and Luckmann 1966:128-189). In other words, despite the fact that all social realities are enacted rather than discovered on the basis of “subjective” human interpretations that impose a predictability on subsequent social interaction (a kind of certainty about the everyday social world that enables continuing interpretation, innovation, and reconstruction), institutionalized common stocks of knowledge are nevertheless objective in the sense that they are available to all members of a group as historical resources that shape and direct social interaction. As Berger and Luckmann (1966:61, original emphasis) famously put it, “Society is a human product. Society is an objective reality. Man is a social product.”

It follows that the truth-value of social reality as an objective human accomplishment cannot be verified as correct or incorrect by applying certain social-scientific techniques (or, worse, subjecting different definitions of reality to normative evaluations). This is why Berger and Luckmann (1966:1) took care to define social reality narrowly as, “a quality appertaining to phenomena that

we recognize as having a being independent of our own volition ('we cannot wish away')." What this definition of social reality implies is that the relationship between human beings-as-continual producers of social realities and social realities-as-institutionalized constraints on enduring human interactions is properly conceptualized as dialectical rather than dichotomous: human beings produce and maintain meanings about the social world through routine practical activities that, once habituated, institutionalized, and reified act back on and shape human meanings about and routine activities in the social world (even as they are maintained by them).

And yet, despite the fact that social realities do not exist independently of ongoing human interactions, the relationship between human beings-as-producers of social realities and social realities-as-constraints on human (inter)actions is not equivocal. While it is true that social realities are both foundationally constructed and continually maintained on the basis of institutionalized human beliefs and dispositions (revealing the character of society as dialectical), there are nevertheless obvious differences in the spatial and temporal dimensions of constructing and maintaining/reproducing social realities. In plain terms, we are all born into existing social realities that are highly institutionalized, to the extent that they appear to us as both natural and inevitable (e.g., language, cultures, customs, rituals, routines). Those reified realities (social-structural patterns that we cannot wish away) profoundly influence our "subjective" thoughts and actions in everyday life, even as they depend on them for their continuing existence.

The assertion that social reality is simultaneously subjective and objective is therefore more than an oxymoron: it speaks to the ways in which human meanings about the social world are created by human beings, and how historically constituted social stocks of institutionalized knowledge simultaneously constrain the possibilities of human action, especially as they are transmitted to future generations.

Berger and Luckmann's dialectical conceptualization of social reality as simultaneously subjective and objective is essentially what Spector and Kitsuse were driving at when they formulated the definitional perspective on social problems. The definitional perspective is oriented towards investigating the methods or practical activities that people engage in as they create, institutionalize, and reproduce social problems as "moral objects." The latter term, introduced by Ibarra and Kitsuse (1993) to clarify the original focus of the definitional approach, refers to the ways in which claims-makers selectively, but discernibly draw on vernacular resources such as rhetorical idioms (i.e., conventional modes of expression), cultural motifs (i.e., figures of speech), and even socially sanctioned claims-making styles (e.g., protests, news stories, memes) to portray some people, conditions, and/or experiences as problematic, troubling, and in need of remediation (Ibarra and Kitsuse 1993).

The interplay between claim-making activities that take place in particular social settings (i.e., the subjective experiences of social reality) and the vernacular resources that are drawn upon to diagnose problems and proffer viable solutions (i.e., the

objective dimensions of social reality) is crucial to this formulation. By emphasizing the conditioning role that vernacular resources as a general class of *moral* objects play in instances of social problem claims-making activities, Ibarra and Kitsuse were essentially trading on the kind of conceptual distinction that Saussure (1924) famously made concerning the arbitrary constitution of speech and language. Rather than opting for the contextual compromise of blending the activities- and condition-based approaches, Ibarra and Kitsuse, like Spector and Kitsuse before them, for all intents and purposes understood objective and subjective reality as different temporal moments in a unified social reality (the essence of Berger and Luckmann's sociology of knowledge). In this way, they not only avoided the seduction of gerrymandering putative conditions (Spector and Kitsuse 1977) or condition-categories (Ibarra and Kitsuse 1993) by unambiguously conceptualizing social problem conditions as a human interactional accomplishment. They also avoided the lure of explaining claims-making activities as an effect of more fundamental underlying socio-historical conditions by conceptualizing claims as simultaneously conventional, as well as emergent (Ibarra and Kitsuse 1993).

Conclusion

The notion that social problems are socially constructed has enjoyed considerable support among sociologists since the 1970s. Following a lively debate about the scope of social problems theory and method in the 1980s, constructionists settled on a contextual compromise that granted explanatory

importance to both objective material conditions and subjective social definitions. The contextual compromise represented a seemingly practical solution to perceived divisions among constructionists, providing sociologists with a theoretical justification for distinguishing between legitimate and exaggerated social problems claims. Notwithstanding the appeal of contextual constructionism as a pragmatic framework for debunking fanciful social problems frames (inadvertently blurring the distinction between the sociology of social problems and conventional moral panic studies), with few exceptions adherent to both the "strict" and "contextual" version of constructionism have all but ignored how the constructionist perspective qua institutionalized patterns of making claims and asserting grievances originally contributed to the sociology of knowledge.

In this regard, the resurgence of debates about the future of social constructionism in social problems research presents an important opportunity to revisit the explanatory significance of the sociology of knowledge in the constructionist tradition. Of all the recent contributors who have drawn attention to the importance of claims-making contexts, Weinberg (2009; 2014) arguably comes closest to this understanding when he identifies a dialectical relationship between claims-making activities and the vernacular resources that condition and are conditioned by social problems claims and frames. Yet like so many other contributors to contextual constructionism, there is a conceptual slippage in his otherwise exemplary theoretical critique, whereby objective conditions are sometimes conceptualized as institutionalized stocks of knowl-

edge (e.g., norms, routines, patterns of interaction, styles of rhetoric) and other times as real world material facts that are unaffected by claims-making activities.

In its most benign form, the continuing neglect of the sociology of knowledge has contributed towards the ongoing tendency among contextual constructionists to either reify or not fully explain what it means to assign explanatory power to opportunity structures and claims-making contexts. In its more malignant form, it contributes towards the kind of critique offered by DelloBuono (2015), where he argues that the unsustainable orthodoxy that characterized social problems theory into the early 1990s has given way to a conception of the ahistorical claims-maker devoid of real structural context.

To be sure, the perennial neglect of Berger and Luckmann's dialectical theory of society reflects a wider disciplinary confusion about the processes involved in constructing social reality (Vera 2016). What is being suggested in this article is not that Berger and Luckmann's sociology of knowledge offer a ready-made methodological strategy for investigating social problems claims-making activi-

ties. Rather, the argument is that by paying greater attention to the phenomenological orientation that gave rise to the constructionist approach to social problems in the 1970s, at least some of the ambiguity about the meaning of social-structural context that continues to haunt contextual constructionists could be avoided.

Hence, as social problems researchers contemplate the future of constructionism (Loseke 2015), their discussions and debates should not hinge on the antiquated dichotomy between subjective definitions and objective material conditions. Nor should contextual constructionists uncritically rely on an undifferentiated notion of objective socio-historical conditions to prop up their accounts of social problems claims and frames. Recent contributions to rethinking constructionism have acknowledged the importance of empirically investigating contexts of meaning. What they have yet to do is provide a clear theoretical and methodological way to move beyond the explanatory problems that continue to characterize contextual constructionism. Berger and Luckmann's widely neglected sociology of knowledge is a useful way to begin thinking about the future of contextual constructionism.

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Getting Prepared to Be Prepared: How Interpersonal Skills Aid Fieldwork in Challenging Contexts

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Abstract This article deals with fieldwork in challenging research contexts that make preparation for field research particularly difficult. Challenging contexts include generally insecure places, politicized contexts, and unknown settings. Drawing on our experience in the field, we discuss four challenges that are common across these contexts: access, positionality, researcher well-being, and research design and data collection. Bringing together insights from fieldwork with urban elites and in the countryside, this paper describes problems that occurred in both settings and identifies a set of interpersonal skills that helped the authors to tackle the challenges of the field and seize the opportunities it offered. This article posits that recognizing the importance of certain interpersonal skills, namely: openness, empathy, humility, and flexibility, precedes the identification of practical tools. Interpersonal skills, instead, focus on a general attitude that underlies researchers' capacity to make informed choices about specific courses of actions, preparing fieldworkers to be prepared to confront problems once they arise.

Keywords Fieldwork; Research Strategy; Challenging Contexts; Qualitative Research

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Trapped in a town center as an angry crowd cuts power and telephone lines. Being sexualized and harassed by key informants. Being denied access by high-ranked technocrats. These are only a few of the most difficult challenges in doing fieldwork that the recent literature on qualitative methods addresses (see, in this order: Nilan 2002; Mügge 2013). Recent years have seen a growing openness by academics to discuss the seemingly “unscientific” aspects of social enquiry. These elements of the investigation process are rarely discussed in publications and final stages of research trajectories. However, there is now a considerable number of articles, including previous issues of this journal, that address the specific challenges of conducting research under dangerous circumstances (Wong 2015), in politically unstable rural environments (Mukeredzi 2012) and on sensitive issues (Roman 2016). Various journals have added to the literature with round table discussions (Ortbals and Rincker 2009) and special issues (Goode and Ahram 2016). In addition, several book-length discussions are available to help the social researcher “survive field research” (Sriram et al. 2009; see also Thomson, Ansoms, and Murison 2012; Lunn 2014; Kapiszewski, MacLean, and Read 2015). Do we need yet another report on the challenges and lessons learned from fieldwork?

We believe the answer is yes. Graduate students who set off to the field for the first time are still often ill-equipped to confront the realities of fieldwork. As a political scientist who found herself forced to bribe the Russian police wrote, the best recommendations she counted on at the time were only some “helpful hints regarding the problems one might find in the field and some preliminary thoughts on how to

handle certain situations” (Johnson 2009:321). Since then, the field has made some progress, but nevertheless, we still lack some of the cross-cutting insights that can prepare us to face the difficult tasks of doing research in challenging contexts.

Specifically, this article hopes to make three contributions. First, we conceptualize challenging research contexts more broadly than prior contributions that address fieldwork in areas of ongoing or recent armed conflict (Goodhand 2000; Barakat et al. 2002; Wood 2006). Challenging contexts include not only dangerous places where political violence is acute, but also localities where researcher mobility is restricted by other variables, such as high crime rates, for instance. Challenging contexts are also present where access is difficult in logistical terms, and they include situations of extreme political polarization. Lastly, fieldwork in any unknown or unfamiliar place also creates a particularly challenging context. Although, in our case, we encountered the “unknown” doing research in the global south, this contribution is equally relevant for those conducting fieldwork in developed countries, provided that the particular context is unfamiliar to the researcher. The bigger the cultural, linguistic, and life-world gap between the researcher and the research environment, the more of the precious resource time is needed until the researcher is able to get the most out of fieldwork. After all, our objective in the field is not merely to survive (Sriram et al. 2009), but to gain the original insights one can only obtain in the field.

The second contribution lies with the combined experiences of the two co-authors on the challenges

we discuss. Specifically, we draw on experiences where the field consisted in major cities with the targeted experts being found in offices guarded by security personnel in ties and shirt cuffs, and others where transportation infrastructure was lacking and the relevant participants, indigenous people living in rural communities, were therefore hard to access. Common to these challenging contexts, we identify four overarching challenges that arise in the course of the research process: gaining access, positionality, researcher well-being, and data availability. We illustrate each of them with concrete examples and propose strategies to respond or mitigate them.

Lastly, the various solutions we propose identify specific strategies to deal with particular challenges and offer practical advice, with an emphasis on four overarching interpersonal skills that are crucial to success in the field: openness, empathy, humility, and flexibility. Although these interpersonal skills are found in some of the existing literature, their treatment tends to be rather implicit in political science, and they are rarely discussed as a diverse set of skills. This most likely occurs because there is an open debate over the extent to which such traits can be learned. In this debate, we take the position that good fieldworkers are made and not born, and that we can learn from others' experiences.

Generally, skills are defined as a "goal-directed behavior that is acquired by practice" (Proctor and Dutta 1995 as cited in McEnery and Blanchard 1999:156). In particular, interpersonal skills refer to "goal-oriented behaviors, including communication and relationship-building competencies, employed

in interpersonal interaction episodes characterized by complex perceptual and cognitive processes, dynamic verbal and nonverbal interaction exchanges, diverse roles, motivations, and expectancies" (Klein, DeRouin, and Salas 2006:81). Simply put, it refers to "the skills employed when persons interact with one another" (Klein et al. 2006:81). While scientists generally concur that interpersonal skills are to some extent innate to humans, research in the fields of business, economics, human geography, and psychology shows ample scope for individuals to develop and improve them. Focusing on these four skills, we believe, will prepare researchers to face the particular problems that are inherent to challenging contexts.

Building on the combined research experience of both co-authors, in research contexts that are broadly defined as challenging, the next section describes the four interpersonal skills mentioned above and how they help getting the most out of fieldwork in challenging places. The third section explains the fieldwork settings and the methodology used for the purpose of this contribution. The remainder of the article is organized along four challenges, in the order the field researcher is most likely to confront them: access, positionality, researcher well-being, and research design. Although a neat separation between them does not exist, we believe that the sequence is representative of many fieldwork experiences. Accordingly, it is only after having gained access that the researcher is directly confronted with issues of positionality. Researcher well-being, clearly the most cross-cutting issue in the fieldwork process, was in our experience most pronounced after we had already spent some time in the field.

Lastly, an informed revision of the research design takes place only once the data collection process in the field has already made progress. We illustrate how interpersonal skills help overcome the respective problems associated with each of the four challenges and how they may even turn challenges into chances.

Challenges Meet Interpersonal Skills: How Personal Attributes Help Navigating the Field

The challenges we deal with are not unique to challenging contexts. Gaining access, positionality, researcher well-being, and research design are considerations relevant to all research sites. Nevertheless, they tend to be more salient in challenging contexts, either with respect to the nature of the problem or the strategies they require in response. Take snowballing techniques, for instance, which are generally advocated to overcome access problems, but appear hardly suitable in politicized contexts where referral chains are interrupted by disconnected social networks (Cammett 2006). Yet, as we shall explain below, the researcher who is open and flexible can use existing cleavages to his or her advantage in order to bridge them.

To decide on the most beneficial strategy to deal with a given problem and seize the opportunities of the field, we identified a particular set of interpersonal skills as particularly relevant. Table 1 summarizes the challenges and the correspondent interpersonal skills to address or mitigate them. The skills highlighted in bold letters are those we found most relevant for each challenge.

Table 1. Relevant interpersonal skills to handle challenges and seize opportunities in the field.

Challenge	Interpersonal skill
Gaining access	openness, flexibility, humility
Positionality	flexibility, empathy, humility
Researcher well-being	humility, flexibility, openness
Research design and data collection	openness, flexibility

Source: Self-elaboration.

Since the terms are borrowed from the field of business and management studies and the skills needed in that discipline are not necessarily the same social scientists require, it is necessary to explain what each of them means.

By openness, we refer to a general disposition to experience situations that are unfamiliar or unusual, from the researcher's perspective. Openness is especially relevant in challenging fieldwork contexts where little is likely to turn out as planned and where the ability to respond quickly and smoothly to unforeseen situations can be crucial for the success of the research endeavor. A lack of openness is likely to result in frustration and, consequently, lesser efficiency, which leads to the loss of precious time in the field. As Table 1 shows, we found openness particularly relevant to gain access, in the dialectical process of data analysis and refining the research design.

A certain degree of openness is a precondition for flexibility, which represents the second interpersonal skill we identified as crucial in our research given

that successful fieldwork requires the disposition to change. In addition, flexibility requires a proactive attitude to adapt and reorganize one's tangible and intangible resources. We find that flexibility is key when assessing the relationship we establish with research participants and while evaluating how the fieldwork experience affects us and the quality of our research. Besides, a flexible attitude comes in handy when networks of informants are closed and fieldworkers need to open new channels of communication, as well as in the process of data analysis and research design when evidence has to be used in new and creative ways.

Thirdly, empathy is the ability to identify and understand other peoples' emotions. The need for scholars to be empathic towards research subjects has been amply discussed by fieldworkers, especially in the field of anthropology (McLean and Leibing 2007). Challenging contexts, however, make it potentially more difficult to be empathic: stressful circumstances and cultural differences are merely two of the reasons why. As Table 1 shows, we found empathy to be especially important in order to deal with some of the dilemmas of positionality we faced when reflecting on how our presence as researchers affected other participants.

Finally, humility refers to the recognition of the researcher's self in relation to the research objective and those who take part in it. Humility does not imply any form of understatement or modesty in one's aspirations. Quite to the contrary, it implies that researchers must express their expectations and needs explicitly, for instance, in their hope to interview a person of higher rank on a subject

matter they do not usually grant appointments for. Another example of humility is when a researcher confesses to be ignorant of a local insight everybody in the field seems to know of. Humility may even entail expressing physical needs such as thirst during an interview on a long day of fieldwork packed with meetings, something that tends to be seen as counterproductive to the image of the professional researcher. Put differently, then, humility means acknowledging our dependence on certain insights and individuals, as well as the possible limits to what is achievable in the field. Understood this way, we believe that humility helps researchers gain access and deal with challenges of positionality, that is, the dynamics of the researcher-researched relationship, as well as to ponder our own well-being in the context of field research (see Table 1).

The Research Experience: What We Did and How We Made Sense of It

To assess how the four interpersonal skills defined above help address the problems that may emerge while conducting field research in challenging contexts, we draw on the combined dissertation experience of both authors, which together comprised a total of five extended periods of fieldwork between 2011 and 2014. One project explored the management of territorial conflicts between pairs of states in two different regions. The comparative study involved fieldwork in four South American and six Southeast Asian countries. The second project sought to understand how different types of indigenous organizations affect ethnic party performance and the nature of political participation in six Bolivian rural towns. Despite the vastly different research objec-

tives, the two projects used similar methodological strategies for data collection. These were almost exclusively employed in the field.

Both projects relied heavily on interviews, which were complemented by data from official documents and media analyses, as well as participant observation in public and closed events. The two projects also involved a limited amount of archival work. Neither of us had prior experience in any of our fieldwork sites, and the first contact with the areas took place only when the research projects began.

The process of developing the conclusions presented in this paper began several months after having concluded the fieldwork. Back at our respective home institutions, friends and colleagues occasionally enquired about our fieldwork experience. Sharing insights in these informal settings set off a process of reflection, that later developed into a systematic effort after we were both asked to give a seminar on fieldwork for a methods summer school in political science. Ironically, it was during the presentation that we realized striking similarities both in the problems we had encountered during fieldwork and in the solutions we had found—often going through the same processes of trial and error. At first, given the rather dissimilar contexts of our field research, we thought it was not possible to come up with a common set of conclusions. Enquiring about the management of territorial disputes required locating informants mainly in the institutions placed in the administrative centers of the countries studied. Navigating bureaucratic hurdles and traffic in South America and Southeast Asia's large capital cities had little to do with the lack of road infrastructure

and the difficulty to access closed rural communities that characterized fieldwork for the project on political participation in rural Bolivia. In the latter context, a great deal of the field research entailed gaining access to indigenous leaders in remote communities and to community assemblies that were not open to outsiders. We then realized that despite superficial differences, both settings presented similar challenges and common strategies to navigate the difficulties, convincing us of the possibility to draw conclusions with a broader frame of reference, which we chose to define as challenging contexts.

The interpersonal skills discussed in this article were not consciously employed during our research. Instead, they are the result of a process of deep reflection on why we made particular practical choices after unsatisfactory and at times unpleasant experiences. In consequence, the following sections leave out many of the practical fieldwork advice found in the existing literature. Instead, we focus on what we see as a general attitude underlying the capacity to make informed choices about exactly these practical tools, namely: the interpersonal skills that influence how the field responds to our academic necessities. To illustrate how skills lead to concrete solutions, the following sections use selective examples, some that were common and others that were rare, but altogether critical experiences that eventually turned us into better researchers.

Gaining Access: Making and Keeping Contacts

Gaining access to key informants in challenging contexts is the first task researchers are confronted

with when initiating a field-based project. Three interpersonal skills proved especially relevant to successfully identify and secure access, as well as to earn the trust of our informants. These were: openness, flexibility, and humility. Openness was crucial to help us identify networks of informants and establish first contacts in an unfamiliar setting, while flexibility allowed us to devise a wide range of strategies to contact our interviewees. Lastly, by placing our informants at the center of our field research, humility helped us gain their trust.

Openness

Challenging contexts are, by definition, uncertain, which makes identifying contacts, gaining access to them, and earning their trust particularly difficult. Thus, when we first arrived to the field, we approached the most familiar group of people: academics. Local scholars provided us with an initial sense of what relevant actors we should reach out to, but as one of us quickly noticed in Bolivia, their suggestions sometimes excluded informants that later proved key for the research project due to their ideological biases.

As an illustration, one of the authors' first interviewees asked, "What have you read on Bolivian politics so far and who do you plan to talk to?" Somewhat disappointed on her response, he suggested, "*Compañera*, I will tell you who to contact and what to read" (Author interview, La Paz, July 14, 2011). The interviewee's use of the word *compañera* to refer to her alerted the author of his potential political biases, as in left-wing political circles, *compañera* means comrade or co-partisan. The researcher found out

that in Bolivia this term is used among members of a more leftist and class-based faction within the ruling party and by peasant union leaders. The interviewee's recommendations on what to read and who to contact therefore reflected this particular point of view on Bolivian politics.

Similarly, during another interview, she noticed that the informant greeted her using the word *hermana*, that is, sister. The term *hermana* is used by groups who assert that indigenous principles should guide the government, thus signaling a substantially different political viewpoint. While *compañera* and *hermana* were words familiar to the researcher, she was unfamiliar with their particular meaning in Bolivia. Correspondingly, it took her a while to understand that those two words not only indicated two different political perspectives, but also—and more important for her fieldwork—two different groups of interviewees that did not necessarily converse with one another. Consequently, although the researcher relied heavily on her academic contacts, the openness to understand the unfamiliar use of these two terms led her to better identify relevant networks of informants, their political biases, and how these might have affected her identification of other key informants.

Contacting and identifying relevant informants poses a significant challenge, especially in unfamiliar settings, where researchers are not always aware of the implicit protocols and behavioral codes to approach people. Once informants in Bolivia had been identified and contacted, for example, the researcher experienced unanticipated waiting times and even the cancellation of meetings before one of her

informants mentioned that there was an informal rule to call fifteen minutes before each meeting “to reconfirm.” It takes a fast and open learner to understand that behind such complications there can be different procedures rather than mere unreliability. Openness also helped the researcher navigate different organizational norms to request interviews. When seeking to interview the leaders of one of the main indigenous organizations in Bolivia, the researcher was invited to participate in an assembly, which began with a ritual that required all attendants to participate. The ritual consisted of standing up before an altar to pay respects to the Pachamama (mother earth), and to collect coca leaves to chew during the meeting. Her openness to experience this unfamiliar situation allowed her to gain access to the main leaders, as well as other members of the audience that perceived her as someone interested in their organization and culture.

Being open to different courses of action to gain access to informants is even more relevant when studying rural communities, which tend to be rather closed and distrustful of outsiders. One of us learned the hard way that travelling to rural communities without following the proper procedures for contacting people is likely to translate into utter failure, as individuals would refuse to talk to her. Comments such as “show me that you have authorization to be here” illustrate the lack of trust by community members. The researcher, therefore, had to be open to the different approaches that were necessary to secure access and trust in rural and urban settings, even though this required more time and effort on her part. Unlike urban areas where a phone call or e-mail would do, rural, indigenous com-

munities tend to be more distrustful of foreigners, which makes it essential to have a “bridge-builder” from the community who invites the outsider and introduces her to the key actors. When invited by a community member, especially an indigenous authority, the author found that most people were willing to talk to her, as she was perceived as an official guest or the friend of someone important. On one occasion, being the guest of the local indigenous authority granted the researcher an invitation to an event reserved solely for authorities. In that occasion, she had the unique opportunity to observe how politics were discussed among the leaders and how they related to one another, something that outsiders rarely witness. Without that critical link, which transformed an unauthorized intruder into a privileged participant in authority circles, it would have been impossible to conduct research in the rural areas. Naturally, the same applies to other closed communities, such as elite groups or religious sects.

Flexibility

Interviewing elites proved challenging for other reasons, as administrative and security procedures often block direct contact. However, being flexible helped devise two effective strategies to access elite informants. First, contrary to common textbook advice that important informants should be left for later in the process once researchers are already well-prepared and furnished with information by others (Hertel, Singer, and Van Cott 2009), we found that key elite informants were worth contacting early on. Not only did this strategy help to signal our respect for the elite’s hierarchy, but also served to entice collaboration from their subalterns. In effect,

one of us set up an appointment with the director of a division of the Indonesian Foreign Affairs Ministry, but frequented the same office several times before the meeting took place to interview assistants and consult brochures. When she eventually met the director, she already knew the office and could start the interview on familiarized terms.

Second, when contacting elite informants directly did not work, we found that switching to their immediate surrounding for information or as an access point was the most effective strategy. Where it first looked impossible to contact political, military, or indigenous leaders, we reached out to their advisors, long-term assistants, social activists and academics they worked with (also known as “soft contacts,” see: Kapiszewski et. al. 2015). The latter group proved especially helpful to gain access to higher-level informants. In her research in Southeast Asia, one of us made ample use of the fact that academic institutions are often closely connected to the government or traditional political parties. Not all too differently, the phenomenon of the revolving-door between academia and politics is found in many Latin American states, especially in the foreign policy and security sectors, as the relevant academic circles are generally small. It was also proven important to research potential political connections in advance. However, it was fundamental to make sure that academics with privileged access would not become reluctant gatekeepers. After one encounter in which the researcher challenged an academic on his—in her view—ideologically tainted publications, he withdrew a previous offer to arrange an interview with a former minister on the grounds that “he would not like your point of

view.” The lesson learned from this circumstance appeared on the same transcript of the interview: “Always formulate scrutinizing questions strictly in terms of academic debates when talking to people with an academic vocation, even if you think they are wrongly placed in academia.” Despite this particular experience, politicians, social movement leaders, and military personnel with an academic past or vocation were generally sympathetic to meet with us and happy to serve as a link with their political peers and seniors.

Flexibility was also crucial when we were faced with research settings marked by high polarization, as we initially believed that strong cleavages, regardless of their kind, would impair our ability to conduct research. However, circumventing sensitive issues proved awkward, as one of the researcher’s attempt to de-emphasize the obvious political polarization in Venezuela, when asking about political decision-making processes, led her interviewees to look at her in disbelief before speaking on the matter. Instead, through a flexible attitude that allowed us to quickly change strategies, it was possible not only to navigate particular divides, but also to use them in our favor. If Chávez’s supporters in Venezuela think one thing about a particular issue, what do you, as the opposition, think? Another example was when one of us had talked to a high-level politician in Singapore about an international crisis, she disclosed a particular piece of information to his counterpart in Malaysia, asking the latter to take position on the information given by his Singaporean counterpart. The interview was immediately granted as it gave the official an opportunity to defend his position.

This situation proved contrary to the general advice to avoid contentious issues when researching in polarized settings (Cammett 2006; Johnson 2009), since exposing certain sensitive information sparked the interest of many to tell “their side of the story.” In fact, when one of us mentioned that part of her research was precisely the issue of political polarization, people were even more enthusiastic in participating.

Humility

In addition to openness and flexibility, there is a third skill that helped us overcome many of the challenges detailed above: humility. This skill, nevertheless, operated differently depending on whether we were dealing with high-ranked informants, such as politicians, military officers, and indigenous leaders, or grassroots interviewees at the community level. Humility proved essential to signal that we really cared about our interviewees’ opinions. In dialogues with elite informants, a humble attitude reinforced power positions and thus, their authority and expertise on certain subjects. A simple comment such as, “I would like to talk to you, since you are the expert on the subject,” triggered immediate positive reactions from many interviewees and signaled that we were there to learn. This was especially relevant when interviewing military personnel with high ranks who are used to interact within hierarchical structures when talking about issues related to their work. Drawing on this skill, however, also proved useful when one of us interviewed an important politician, who, honored by the recognition of his academic trajectory, gave the researcher copies of most of the books he had published.

When speaking with grassroots participants, humility was generally welcomed with gratitude, as it meant giving voice to often-marginalized people who do not normally have the chance to tell their life stories. For one of us, humility was expressed by informing the interviewees about the importance of their contribution to the research project given the general (and her own) ignorance about how life and politics are practiced at the local level in rural areas and across indigenous communities. Showing a humble attitude also contributed to change initial positions of distrust towards the researcher among the same, rural community members in Bolivia. Being perceived as respectful or as recognizing certain authority helped us establish rapport with our informants and opened the way for second interviews and additional networks.

Positionality: Who Am I and Who Are They?

Like most post-graduate students today, before going to the field we had been exposed to literature on researcher identity—gender, age, race, class, religion, and nationality—and how it shapes the researcher-researched relationship (Kapoor 2004; Orbals and Rincker 2009). We left for the field highly self-conscious about the contrast (or so we thought) between our social position and that of our local target groups. Eventually, however, we both learned that we need a greater degree of flexibility in self-identification than we had initially imagined. This section highlights the benefits of being flexible in performing different identity roles in our role as researchers. In addition to flexibility, empathy and humility also helped avoid compromising the research and the well-being of those involved due to issues related to positionality.

Flexibility

Both of us felt we departed for the field from a comfortable position. We carried the name of a reputable home university and believed to be well-prepared academically to embark upon field research. White and from a middle-class background, we had grown up with the possibility to get to know environments of diverse origins and where the boundaries of class and religion were relatively more fluid, which had allowed us to develop an attitude towards openness and empathy regardless of these categories. We were prepared to be declined access for our young(ish) age, which we thought might be seen as a sign of lacking seriousness. Interestingly, however, this turned out to be an advantage at least to get our foot in the door. Both urban elites and people in the countryside were surprisingly receptive to our requests and generous with their time and information. Deliberately and explicitly put at the center of our interest, they were often enthusiastic to share insights they most likely discussed only within closed networks. As other female researchers noted, looking “unthreatening” is amongst the most efficient door-openers in the field (Chiswell and Wheeler 2016:232). If our researcher identity proved beneficial for *getting* an interview, however, we felt that it raised the hurdle for getting a *good* interview. Not being taken seriously meant the information we were given in many of our first interviews was irrelevant, imprecise, or greatly exaggerated.

Identity dynamics and the need for flexibility do not end at the door (see: Krasznai Kovács and Bose 2014). The most common setting in our fieldwork experience was one in which we dealt with older males,

some of whom quite apparently thought we could be sent off without a serious conversation. Following the often-repeated textbook advice, we sought to overcome the obstacle by demonstrating that we were well-versed in our subject matters. However, this only helped to a certain extent. Frequently confronted with questions such as, “So, have you already visited all the famous tourist sites?” or “Which of our beautiful beaches do you like most?,” we realized that people seemed to assume that entertaining oneself in places far away from home was, if not the primary, then at least a secondary goal of the research visit. To dismiss such assumptions, we began to share insights into what fieldwork actually implied. This meant giving up on our initial idea that as professional researchers we needed to adhere to a strict separation between our research activities and private life during fieldwork. Instead, to make clear that one’s life in the field was a working life, we shared details about our daily schedule and research progress with interlocutors and interview partners. As this improved the quality of our interviews, we began casually dropping comments that put in evidence the time dedicated to work activities during the weekends, for example, to prepare upcoming lectures at local universities or to complete reports. Flexibility in what our role required allowed our counterparts to understand the genuine intellectual effort behind the undertaking and helped them see their own potential role in it.

The most salient feature of identity we perceived to be problematic in our relations in the field was gender. Unexpectedly, some of the most unsettling situations in this regard occurred in relation to other women when these perceived us as a threat and

protested our professional relationship with their male colleagues or partners. One of us was challenged by the partner of an indigenous leader who began shouting, “I am his wife,” as she threw her chair away in anger, during the celebration that followed a local assembly. This woman protested what she considered excessive attention on the part of her partner, who sat next to the researcher and only talked to her during the gathering. These types of situations risk blocking access, especially to other women. After this damaging experience, the researcher made sure that, when possible, she established contact (with the anyway few) women directly to avoid being associated only to a male contact.

In relation to men, some interviewees voiced their opinion that, “Young ladies don’t need to bother about this.” Following the well-established advice to avoid strong reactions and relax (Mügge 2013), playing the unthreatening researcher, we ignored this type of comment in order to not jeopardize our research. Not all of the remarks were as easily ignored, however, and over time we both felt increasingly unsatisfied with the ostrich strategy of simply burying our heads in the sand. To compensate for what we felt was a compromise between our research objectives and our position as self-confident women, we adopted two strategies.

First, we became more flexible at performing gender, accepting that in fieldwork, “making compromises and being compromised [sic]...is to be expected since all research studies are a complex mix of opportunism, compromise [sic], serendipity and skill” (Ross 2001:163). The benefits of performing gender stereotypes were clear when it gave the

“young lady” the power to put her interview partner into the flattering position of being the only resourceful person to help her out on a topic she *did* want to bother about. Specifically, it helped keep the interviewee talking, getting the researcher another appointment or an event invitation. Conscious that performing gender stereotypes surely does little to enhance the position of females generally, we obviously did not feel proud of using this strategy. In addition, therefore, we relied on a second approach that required not only flexibility, but also empathy and humility.

Empathy, Humility

The second strategy to counter disadvantageous gender stereotypes consisted in paying greater attention to identity roles other than gender. Apart from being female, we had a role as academics, as representatives of an internationalized education system, of a certain religious group, nationality, age group, et cetera. To identify which of these characteristics could establish a more equally leveled playing field that would also put the research participant at ease, empathy was needed. At the same time, establishing a positionality in which both the researcher and the participant felt comfortable required humility in order to recognize and deal with context-specific hierarchies. Education, for example, is a characteristic that is dominant in the context of political science research, which put our position as aspiring PhD researchers above most of the academic experience of our interviewees. Humility proved relevant to acknowledge this situation and the fact that there are other characteristics that establish roles with a reverse power relationship. Thus, education is easily re-defined from a university

degree (what the researcher has) to local or practical knowledge about the subject matter (what the participant has). Emphasizing characteristics other than the dominant ones, such as gender, proved beneficial to put ourselves and the research participant at greater ease.

By combining gender and other identities, we sought to establish an equilibrium where we could be seen as insiders while remaining at a sufficient distance from the research field to observe and study the phenomenon of interest appropriately. Insidership can be created—even in vastly different cultural contexts—through the use of common points of identification. Given the long history of disagreements over borders, being a Chilean researcher in neighboring Bolivia is not necessarily an advantage, but being from a rural province in Chile established a form of “us” that created trust. While empathy helped us point out these areas of identification, our interviewees often elucidated them themselves. Elites who had travelled in Europe remembered their holidays in the Italian city of Florence, where the home institution of one of us was located. Alternatively, Italy’s soccer team Juventus Turin did the trick to get a conversation started.

On the other hand, accepting and performing the outsider position provided opportunities such as joining a group of Malaysian Defense Ministry officials for a conversation over tea after office hours, something that female Malaysian researchers and possibly other female Muslims could not simply do on their own. Differences also allowed us to ask blunt questions without being rebuffed. One of us learned that Thai soldiers brought amulets with them when

being deployed to a disputed border. When asked for the reason why, the obvious ignorance of this tradition allowed the soldiers to freely deliberate on how much they entrusted their military performing to faith, and how much to their own fighting capacity. In this case, naïveté had clearly turned into an advantage (see also Townsend-Bell 2009:312).

Researcher Well-Being as an Ethical Responsibility

Fieldwork may take a high physical and psychological toll on researchers (Moscuza and Lunn 2014). Most guidelines on qualitative research focus first and foremost on the impact fieldwork has on participants and the ethical responsibilities this involves for the researcher. Very little has been said about how field research affects the researchers themselves (exceptions are Belousov et al. 2007; Mitchell and Irvine 2008; Smith 2014). Researcher well-being is important for more than the investigators’ personal sake, but also because it affects the quality of our work and is a requirement for a number of academic standards such as the unbiased reporting of facts and the separation of personal feelings from the professional task. Challenging contexts raise, in particular, a series of issues that affect researchers’ well-being, both physically and mentally. Openness to a different rhythm of daily life, unknown traditions, and potentially distrustful people, to name a few, is the first essential attitude to prevent negative emotional and physical reactions during field research. However, once the circumstances have begun to affect the researcher in a negative manner, a strategy to cope with these situations becomes necessary. In this section, we

report how humility and flexibility proved key to maintain our well-being in the course of our fieldwork experience.

Humility

Humility is the first skill we drew on to make sense of unsettling feelings such as discomfort and insecurity, triggered by field research in challenging contexts. By allowing us to recognize how we felt, a humble attitude helped us deal with different situations such as negative emotional responses, uneasiness about sensitive issues or distrust to certain informants. It also led us to seek help in order to prevent these feelings from affecting our research.

As it was previously discussed, both of us experienced situations with male interviewees that affected our emotional well-being (see also Sharp and Kremer 2006; Mügge 2013). Being asked out for dates by male interviewees or dismissed for being young women led to feelings of frustration that took an emotional toll that was sometimes not noticeable until we returned to our home institutions and began organizing our fieldwork material. In the course of fieldwork, however, peer counseling was crucial. Seeking our peers' help began by humbly acknowledging that such experiences had negatively affected us personally and possibly professionally. In order to prevent distressing interview situations from affecting the quality of our interpretations, discussing these issues, especially with female colleagues either in the field or who shared our recent fieldwork experience, was helpful to untangle the potential effects they could have on our writing process.

Sharing them helped us process our experiences and feel less isolated, as most of our female colleagues had experienced similar situations during their fieldwork, many times without even pondering their effects on their research. Furthermore, through these discussions, we also gathered practical advice that allowed us to prevent these situations by, for instance, meeting with interviewees during office hours when other people were around; respectfully but clearly conveying our discomfort when someone held our hand during interviews; rejecting invitations that were clearly inappropriate; and clarifying the terms of our professional relationship with male informants from the outset to prevent misunderstandings. Humility thus turned into an empowering tool that made our fieldwork experience safer and prevented potential biases in our writing process.

Similarly, while none of us investigated particularly sensitive issues, unsettling topics became part of our projects without our anticipation, such as human rights abuses and political repression. Initially, we pressured ourselves to deal quickly with our emotional reactions and to not let them interfere with the successful completion of the field research. However, humility to recognize that time was sometimes necessary to adapt or to find the right strategy to cope with our rejection to certain interviewees or particular experiences was crucial. Pondering the feelings these interviewees triggered, we noticed that discomfort and distrust were the most common reactions. Acknowledging that such feelings were not a sign of weakness, but a rather healthy reaction to emotionally challenging situations allowed us to assess whether these

were making way into our interpretations of facts. Making the emotional reactions to sensitive issues part of the research process was important, since it led to the adoption of strategies that looked to corroborate the information collected and cancel out potential biases. For example, one of us decided to “pair” her interview materials by matching informants that triggered negative emotions with others with similar characteristics, but who did not elicit such feelings. With this she was able to identify the influence of distressful situations.

In another experience related to foreign policy-making during a specific period of Latin America’s military dictatorships, one of us kept being told about repressive measures against domestic political opponents, including mass violations of human rights. Although the researcher insisted that her investigation concerned foreign policy-making only, some individuals kept referring to human rights violations, either justifying or downplaying them. Feeling instrumentalized for the personal catharsis of the former perpetrators, she found herself tempted to stop interviewing that particular relevant group of persons. Eventually, however, rather than shying away from the negative feelings the interviews caused, she came to embrace them as a valid reaction. Ultimately, the human rights violations that had initially been a tangential issue to her research became a major finding as a pattern emerged when the topic came up in the interviews.

As it is detailed in the following section, flexibility was also necessary to address our emotional response to sensitive issues and allowed us to change our strategies to be able to use the material in the most objective way possible.

Flexibility

The cultural gap and the lack of knowledge about the proper practices in foreign countries triggered feelings of insecurity and sometimes anxiety. A flexible attitude allowed us to rely on new strategies that helped us feel safer and more comfortable.

Conducting research in unfamiliar settings, away from our usual support networks and where we were constantly reminded of our foreign status at times triggered feelings of isolation (see also Palriwala 2005). This loneliness was heightened by the few opportunities there were to discuss some of the challenges we faced on a daily basis with our local informants and scholars, either because there was little time, or because some of our emotional reactions were simply not shared by insiders. We found that sharing these emotions with other researchers was useful to prevent these feelings from affecting the quality of our fieldwork, even if these did not know the precise context of our field. The local institutions we were affiliated with provided emotional support during the few, but impactful moments where we felt disconnected from our environment and overwhelmed by a particular situation (see also Wong 2015). Acknowledging that we were struggling with certain situations required humility, as well as the flexibility to temporarily put aside our roles as professional researchers during the moments we felt emotionally strained.

Likewise, one of us found that establishing contacts with local graduate students at a similar phase in the research process was helpful, as it made the foreign fieldworker feel less isolated and provided

a support group both academically and emotionally. When possible, we also decided to partner with other researchers, especially those with better knowledge of local culture and conditions. One of us, for instance, began taking field trips with a local PhD student who was also conducting his dissertation research, and who helped her navigate cultural differences by explaining what certain activities entailed and the appropriate procedures to gain access to the community. Generally, taking breaks from fieldwork helped overcome the instances when we felt overwhelmed and allowed us to return to the field with renewed energies.

It is important to emphasize that the numerous strategies to cope with the mental and physical consequences of doing field research continued to be relevant upon our return from the field. When revising some of the early drafts of the thesis, written immediately after fieldwork, we realized that emotions had found their way into our writing despite our intentions to be “objective.” Therefore, it is important to be aware of these issues whenever one uses material collected during field research, even if significant time has transpired. Indeed, we found that some distance helped better assess how emotions were affecting the research processes. Since personal well-being eventually influences the quality of academic work, the researcher’s well-being needs to be considered among other ethical responsibilities of fieldwork.

Research Design and Data Analysis

The fourth major challenge examined is the difficulty to achieve accuracy and external validity

when conducting research in challenging contexts. Data that is hard to access and data that is simply non-existent renders the best of research designs futile and fly in the face of the expectation that serious social science ought to look “scientific.” The preference for formal research methods surely varies across disciplines, but it is no exception to read that “many political scientists...dismiss research in foreign countries as a waste of time and money” (Hertel et. al. 2009:305). With a commitment to produce original research, however, openness, together with a degree of flexibility, provides the researcher with several strategies that can help overcome particular difficulties in gathering the “right” data. This section shows that missing information does not necessarily require radical changes to the original research design. Openness and flexibility, in contrast, can help to change focus and identify “alternative” data that fill in the void of missing data.

Openness

When the data collection method risks failing, we often turn too quickly to our research design to make what we believe to be necessary changes. However, substantial changes are not always necessary nor are they always advisable (Höglund 2011:118; see also Zulauf 1999). As a matter of fact, in social research, the method should be “the servant, not the master” (see chapter seven, Firebaugh 2008). In our experience, we learned about the lack of specific data once we were in the field, with few possibilities to reallocate resources. Given that major changes to our projects were beyond what was possible, we needed a different solution. With time,

we realized that maintaining an open attitude towards unknown contexts led to a learning process in which it was possible to identify alternative data by interpreting information previously deemed irrelevant for our research design through a new lens.

First, openness let us see more than a single way of reading data. One of us learned this insight after a frustrating experience at the Cambodian President's Office. After several meetings in a unit that had temporarily been established there, she obtained digital copies that were promised to be "highly relevant." The USB stick was safely brought home, but when it was inserted in a computer, a virus alert popped up and the antivirus software left the infested files inaccessible. Several attempts to recover the files failed, and the person who had made them available reacted angrily at the request to facilitate the data once more: "You asked for the information, you got the information. There is no more for me to do." Only after the initial frustration ebbed away, the researcher realized that the fact the unit's computers carried viruses did in fact support her previous impression that it was of rather limited importance in the decision-making process. The anecdote did not make it into her PhD thesis—the "chaotic circumstances" of data collection seldom do (Nilan 2002)—but the conclusions drawn from an alternative reading of the (missing) data did. Similarly, after initial interviews with the leaders and rank-and-file of one of the main indigenous organizations in Bolivia, the researcher was rather puzzled to notice that the responses to her questions were almost exactly the same. They replicated what seemed to be an "official" discourse. While she was first disappointed, she later understood

that together with a heightened sense of loyalty, this unified discourse suggested that members of the organization were constrained by a strong internal discipline. This particular aspect of the organizational structure turned into the central argument of her thesis.

Secondly, openness also helped us recognize what actually constitutes data. In one of our research experiences, a negotiator in an international conflict claimed he had established a close relationship with his counterpart on the opposing side. Such a reading was in line with the official position of his government, but contradicted what other negotiators had previously told the researcher. To evidence the allegedly close ties he had created, the interviewee said he could contact his counterpart any time on a messenger service where they were friends. Positively surprised, the researcher replied that she would soon travel to the place his counterpart was located at and asked for an introduction. Despite repeated requests, the contact was never made. At first, the researcher regarded the experience as no more than a failed attempt to expand her network of informants. However, her perception changed months after when she found herself in a similar situation, but this time the negotiator effectively got her an interview with his counterpart. Surely experiences like this one do not represent pieces of data or evidence on their own; however, cumulatively they are essential to make sense of the field in a way that other methods most likely cannot.

To make the most of such seemingly random experiences, we consider it relevant to repeat once

more some well-established advice: note-taking is crucial. Even for an open researcher, the process of recognizing and re-interpreting data requires time and single experiences often get forgotten during intensive periods of fieldwork. Together, we possess 21 notebooks full of interviews, reports, and observations from the fieldwork experiences described here. Looking in retrospect, nevertheless, we wish we had documented in an even better way our own impressions and the casual discussions and jokes from the field. Given that this type of regret occurs frequently, researchers are well-advised to always write down too much rather than too little.

Flexibility

While openness introduces a way to deal with missing data by re-interpreting information, flexibility helps find proxies in a creative way and adapt our strategies to collect data. This necessity arose strongly in the case of Malaysia, where an often arbitrarily applied internal security act complicated access to even the most trivial information. As it turned out to be impossible to know what aspiring navy officers were taught about specific security problems, the researcher was tempted to dismiss the case study. However, when a local academic came up with an alternative solution where the freely accessible essay topics for the final examination of the military academy could be used as a proxy for what they had studied in class, the country could be incorporated into the investigation. Thus, identifying the proxy provided a cost-free and relatively easy way out of what had initially appeared to be a problem with no solution.

Conclusions

A war-torn country, a community of difficult access, and research on polarized political settings all represent what we described as challenging contexts. Challenging contexts, regardless of their type, are similar in that the researcher is faced with high levels of uncertainty. We have identified four particular challenges fieldworkers face when researching in these contexts: access, positionality, researcher well-being, and data collection. These may not be unique to challenging contexts, but so are their intensity, the responses they require, and the opportunities they provide. Advice on how to confront these challenges often comes in late, once the fieldworker has already had to deal with them and perhaps had done so in a suboptimal way.

Based on our experiences, we offered several suggestions for scholars on how to prepare themselves for field research in challenging contexts. These can be synthesized into four broad categories of interpersonal skills, which we believe allow the researcher to find practical solutions to potential problems as they arise. First, openness to new contexts, cultures, and experiences is key for researchers to fully embrace and enjoy the process of fieldwork. Second, empathy is crucial to understand the viewpoint of those who participate in our research projects, as well as our own role in the research process. Third, a healthy portion of humility helps recognize that many things are beyond our understanding, that often we need to ask for help, and that it is necessary to level the playing field of power asymmetries between ourselves and our research subjects. Finally, field researchers need flexibility to adapt to uncertain and changing

circumstances in order to alter the course of action when a network of informants is closed, when data is not available, and when faced with unsettling or potentially risky situations.

Good fieldworkers are not born; they are made. Our approach emphasizes not particular practices, but skills that will allow fieldworkers to better assess which specific strategies are adequate when facing a challenge. Precisely because of its breadth, a skill-based approach will prove more effective to deal with the uncertainty of challenging contexts across a variety of research settings, including rural and urban settings and elite and grassroots participants, as we have shown.

The suggestions put forward in this article, we hope, will help scholars improve the quality of their field research and consequently of their research projects. By concentrating on interpersonal skills, the task of “going to the field” can become a professionally and

personally beneficial experience. Specifically, having a broader toolkit to deal with the unexpected aspects of conducting field research will better prepare the fieldworker to gauge the costs and benefits involved in the decisions they make, to be rigorous while enjoying the research process, and to be prudent while seizing the opportunities challenging contexts will bring them.

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On Entering the Field: Notes from a Neophyte Researcher

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Abstract Qualitative field research can capture the life worlds and definitions of the situation of informants often not reported in quantitative studies. Post hoc reflections of how more seasoned researchers define, assess, and interpret the process of entering the field and the interview dynamic between the researcher's subjectivity and the subjectivity of informants are widespread in the qualitative research literature. However, seldom are the personal stories and reflections of neophyte researchers voiced in published accounts. This article accounts for my experiences in researching the "dirty work" of front-line caseworkers and the importance of practicing empathy while managing a boundary. I emphasize the practical sense-making challenges of managing a delicate balance between under and over rapport in researching homeless shelter caseworkers as an occupational group. My experiences underscore the challenging dynamics of maintaining a professionally oriented research-role, as well as the crucial importance of boundary work and distancing as practical strategies to qualitative interviewing.

Keywords Qualitative Research; Dirty Work; Constructivism; Boundary Work; Casework; Stigma

In the preface of *Experiencing Fieldwork: An Inside View of Qualitative Research*, Shaffir and Stebbins (1991:xi) make the claim that "reports about field research usually describe the methods and tech-

niques of the research. Less often do they tell the researchers social and emotional experiences: anxiety and frustration, as well as exhilaration and pride in achievement." Researchers doing fieldwork must be very sensitive to the impression that they exude on their informants and the connections that they make when entering the field. This critical phase of the research process establishes the groundwork for the prospective collection of data from people who have uniquely different perspectives and for maintaining relations that can help the researcher overcome problems that arise in the field, such as the anxiety of first encounters and a balance in rapport.

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This article discusses some of the practical, social, and emotional dilemmas I encountered as a novice researcher undertaking graduate work to explore the “dirty work” of frontline caseworkers in the “homelessness sector.” The fundamental aim is to heighten researcher’s awareness of their own presence in the field and the impact their presence may have on their informants. I underline the practical and emotional challenges of managing a delicate balance between under and over rapport. My experiences underscore the challenging dynamics of maintaining a professional research role, as well as the crucial importance of *boundary work* and *distancing* as strategies to avoid role confusions and the profoundly complex and emotionally-laden nature of the interview. Although recent literature on qualitative interviewing and fieldwork has overemphasized the role of emotions and intimacy, I conclude that there is a key balance to be struck between professional detachment and human emotion. Researchers have long highlighted the importance of professional distance (Lupton 1994), balance in rapport (Miller 1952; Gans 2003), and role distancing (Goffman 1981) to avoid the complex problems of both role confusion and the potentially conflict-ridden territory of researcher as “friend.” Through my field experiences, I discover that both researcher and informant negotiate boundaries through the interactive dynamic of the interview and that becoming “friends” may not be in the interest of both parties. Research informants also, interactionally, set “demarcation lines” through their own perceptions, expectations, and bodily expressions of the research encounter, conferring certain role expectations onto the researcher. There is no way to predetermine what to expect in fieldwork. The uncertainty of the field itself un-

dercuts any clear-cut how-to- guides. Because each interview encounter is contextually unique, both researcher and research negotiate a situational sense of the interview. Fieldworkers, especially neophytes new to the doings of qualitative research, must learn to think on their feet.

One underlying concern in qualitative research is the relationship between the researcher and the informant. What kind of relationship is it? Reflexivity is one way in which the quality of the data may be ensured. Moreover, I wished to avoid what Stanley and Wise (1983) term the hygienic representation of research, namely, where issues and emotional dilemmas are sanitized from accounts of the fieldwork process. I kept a journal to reflect on each interview encounter, as well as the general research process.

A focal point of the research was to examine the “dirty work” (Hughes 1962; Emerson and Pollner 1976; Sanders 2010a; Phillips, Hallgrimsdottir, and Vallance 2012) of frontline caseworkers and the ways that they sought to engage in esteem enhancing strategies to construct positive self-definitions in what they do. Frontline caseworkers frequently engage in work that involves duties against a strenuous backdrop, including a complex client base that is socially stigmatized, long hours, lack of resources and continuing themes of trauma, death, and crisis. These “dirty contexts” set the tone for frontline work. These contexts also render frontline caseworkers more susceptible to increased workplace stress and burnout.

The focus, however, was not about having my perspective frame the discussion, but about understanding

their definitions of these “dirty work” contexts. Frontline caseworkers qualify as “dirty workers” by their proximity to physical, social, and emotional dirt. These workers are exposed to infectious disease, violence, danger, and hazardous substances. Their work is also socially tainted and emotionally taxing: they must form and sustain relationships with stigmatized publics, at times having to provide care and services to ex-criminals and sex offenders. These interactions evoke a courtesy stigma (Goffman 2009). What was their “definition of the situation” of these “dirty work contexts?” How did they construct and reconstruct their understandings of the work they do? What is “dirty” is a social construction. One’s dream job can be another’s sought-for prerogative. Therefore, understanding the perspective of actors became a crucial part of this research.

Researchers within the qualitative research tradition in the social sciences are not disembodied and dispassionate observers but are actors in their own right. The researcher, as a human being (Gans 2003), attempts to make sense of the research experience. It is not possible to bracket or completely reduce, to some zero level, the researchers’ own reflections and emotions. They must be accounted for. Perhaps it is not desirable either (Tillmann-Healy and Kiesinger 2001; Perry, Thurston, and Green 2004). Even when some of the research questions cause distress for informants, researchers must necessarily respond affectively in some way. Unless we are machines. Which we are not. Thus, it is essential for researchers to manage their affective stances and their own ideas about the research experience. This involves acknowledging and integrating them into the research process itself (Mitchell and Irvine 2008). But,

it also points to an inherent dynamic between researcher and informant. By accounting for the messier parts of fieldwork, the researcher can provide the reader with a greater and more unique comprehension of the research topic and process. This reflexive process helps to underscore and illuminate the interactive elements, namely, what Wojciechowska (2018:122) calls the “interactional and interpretational contexts” involved in the research undertaking that enhances interpretation and understanding (Clingerman 2006; Watts 2008).

As Shaffir (1999:681) writes, in almost a *Garfinkel-esque* fashion, “self-reflexivity underlying the why’s and how’s of the research would yield a more honest accounting of how ethnography was actually accomplished.” The research must be accounted. Researchers are thoroughly involved in social action and as such, they must come to define the interactive reality that is presented to them. Neophyte researchers must learn the ethnographic work by doing the ethnographic work. My early fieldnotes emphasize the importance of learning on “the fly”:

I have strong convictions concerning the research process. One is to ensure that I provide an accurate representation of my informants’ work experiences. At the end of the day, the researchers must refer to analytic frameworks as theoretical instruments to help make sense of a messy social reality. I am becoming increasingly concerned about the data collection process and how the interview context is shaping this process. Verily, I wish to report comprehensively on my reflections of becoming a researcher. As all becoming is, this will be difficult. Like the frontline caseworkers I have interviewed so far, who tell me

that they learn as they go, I know that I too will have to learn on the fly.

Establishing a Presence

I began the process of each interview by making appointments vis-à-vis email with the permission of either the director or the manager of the shelter. Upon arrival, shuffling my pocket anxiously for a cigarette, I pace the sidewalk nearby. I always made sure to be five or ten minutes early to ascertain my earnest self-presentation as a researcher. Every shelter requires the use of an intercom at the main entrance. Identifying myself and explaining that I had an appointment with a caseworker, I was permitted to enter the building. At each site, I was greeted by a worker at the front desk who was balancing various tasks at once: welcoming me as I sat on a nearby chair waiting for my informant to meet me, taking phone calls, addressing the needs of the residents, filling out papers, and answering the intercom. Quite frankly, I felt like I was intruding, considering the heavy workload caseworkers manage on a routine, daily basis.

I was a stranger to the hustle and bustle of this type of work. Guilty for taking the time out of their busy schedules who were not themselves involved in the research. Smiling awkwardly at the workers passing by, I waited patiently, composed, for my informant to arrive. I would soon be greeted by the informant and taken to either their own private office, or some other private office in the building.

“Are you Julian?” I steer my head towards the incoming voice, my informant, with soft, dainty fea-

tures, smiles. “Give me two minutes please, I just have a few things left to do.” “Is that okay?” “Absolutely! I completely understand.” My response is firm and kind. I waited for her to return, as was the case for most of my informants—they were always busy doing something. The workplace is compact, frontline caseworkers rushing past each other in a hurry. There were times when my informants were interrupted by their coworkers during the interview—work related business. I observe an area where family residents relax, their kids playing, and caseworkers making sure to entertain them, to act as company. Upon her return, we fled to a room to conduct the interview. I always made sure to introduce myself, explaining that I was interested in learning more about their work. “Act as if I know nothing!” I would say.

I took the role of the “naïve learner” to effectively grapple with a proper balance between under and over rapport. Douglas (1976) suggested distancing oneself from research participants by playing the novice role, or “play the boob.” It was difficult to be totally upfront about researching “dirty work.” The term “dirty work” may arouse a flurry of emotions. So, I decided to show “saintly submissiveness,” using various “ploys of indirection” to divert people away from the real purpose of the study. I merely convinced them that I was interested in something else, namely, in their “work experiences.” I found this to be helpful because it was general enough and workers can more freely discuss their definitions of the situation. Thus, I remained faithful to my purposes. I would say, for instance, “I am interested in doing a phenomenological study of caseworkers lived work experiences.” This remained seemingly

abstract and unthreatening. By “playing the boob,” as a tactical self-presentation, the appearance of naïveté allowed me to legitimately ask questions about taken-for-granted features of caseworker’s lives. The image of the naïve learner allowed me to ask questions that, under normal circumstances, would produce discomfort. As Adler and Adler (1987:17-18) have pointed out, “researchers may differentiate themselves demographically from their respondents, feign a novice or ignorant status over extended periods of time (‘playing the boob’; Douglas, 1976), use physical positioning to situate themselves on the periphery of the action, and communicate distance and detachment through their body language.”

These strategies allowed me to generally avoid framing the information I was receiving from informants (thus, from the field) as ethical dilemmas. I placed myself as a learner. I also realized that, being myself a novice researcher, it was easier to take on a naïve role, which helped to balance rapport. If I spoke too much to an issue, would I risk silencing them? The self-projected image of the naïve learner, therefore, prevented any kind of over-identification and closeness while simultaneously allowed informants to open themselves gladly to someone who expressed genuine interest, ignorance, yet curiosity into their work lives. Once the anxiety released me, I fell safely into my own skin. The recruitment process also became relatively more stable. My *gift from heaven* was in having readily secured access to the field. However, despite my “gift from heaven,” that is to say, my great fortune in having acquired access into the field to conduct interviews with emergency frontline caseworkers in a major Canadian city, the

process was, in fact, for me, one of great anxiety. Immediate access, I found, does not necessarily equate to acceptance, especially from the perspective of the caseworkers themselves. The bureaucratic procedures of field access—the tedium of emailing back and forth with managers, scheduling, discussing, and convincing different people from the administrative body the worthiness of this research was simply a matter of crossing my fingers. They were more straightforward. Either they liked the project, or they did not.

The research process, on entering the field and beginning my early interviews, which I considered exploratory, was doubtlessly nerve-wracking. Crazy ideas, in the beginning phases of the research, circulated madly through my head: “Why would they want to study us?” “Argh, another researcher. I am busy, stop taking up my time!” I was fraught with anxiety, envisioning embarrassments, disasters, and knowing my own shyness in interaction, the possibility of provoking awkward silence. The craft is not something passed down theoretically. The craft of field research and qualitative interviewing is a pragmatic reality. I adopted a professionally defined research role, which meant a process by which informants come to regard the researcher as earnest, relatively competent, and most importantly, committed. Active listening, body language, and projecting a sense of being personable were essential.

I knew from the get-go that these tense experiences had to be managed if I was to conduct my interviews effectively. William Shaffir (1999:680) notes that qualitative researchers should disclose “their

paranoid fantasies, embarrassments, and the like.” In other words, researchers should remain open about their personal difficulties, quandaries, and emotional experiences encountered during the research process. Introspection is neither good or bad. It has its merits. Its flaws. However, I have found that it did help me to a limited degree. It made me more conscious of foreseeing contingent field situations and at the very least, provided me with the opportunity to think-through some of the potential problems that could arise. In other words, self-reflectivity allowed me to mentally prepare for prospective burdens, tensions, disappointments, failures, and so doing, conjure up possible resolutions. Apart from keeping this tightly abreast my mind, all the way through the research process, I knew that it was crucial to engage informants, from the very beginning, amicably and develop rapport (Shaffir 1991).

My immediate priority, upon entering the field and establishing a presence, was in constituting a bounded social interaction, namely, a connection that was simultaneously distanced yet amicable, kind, and comforting. Managing one’s emotions as a researcher involves a process of acknowledging and even integrating them into the research (Hubbard, Backett-Milburn, and Kemmer 2001; Holland 2007). According to Sanders (2010b:112), “emotional experience is central to doing ethnography. The fieldworker is routinely confronted with the uncertainties of being a stranger in other people’s home territories and having to navigate through the field without the comforting compass of a testable hypothesis.” The reflexive practices and emotional effects on researchers, however, have been less reported (Sword 1999; Dickson-Swift et al. 2007).

I used a few tactics to promote informants’ acceptance of my situated presence. This was necessary to make informants’ more comfortable in revealing their experiences. I initiated casual conversation upon meeting the caseworkers. I wanted to project a sense of demureness. I did this by not dressing or talking too pompously. Goffman (1959) used the term impression management to describe how people consciously attempt to persuade how others think about them through their appearances and demeanor. This lends insight into how people view themselves, but also how they want to be viewed by others. When I met an informant for coffee to discuss recruitment, she had mentioned how North Street is becoming more of an artistic hub. I immediately took this as an opportunity to establish a mutual conversational milieu. We then spoke about art more generally. “Have you heard of Robert Mapplethorpe?” I asked. “There is a current exhibit at Montreal’s Musée des Beaux-Arts on Sherbrooke.” I showed her images of his photography. She then told me about some of her favorite artists, some of whom I knew, some of whom I did not. We sat for coffee and continued the conversation. This conversation began as small-talk and eventually characterized itself as a dialogue. We became more incisive. By allowing each other to exchange diverse points of view on art, into matters of cohesive understanding that illuminated new insights about each other, we enabled a space, together, to talk about experiences. Art itself is a matter of meaning-making and sense-making, in that we seek to make sense of our experiences of looking, creating deeper levels of conversation *about what matters*. I perceived this conversational interchange as a *moyen* for talking about experiences more generally, hopefully instilling

a sense of comfort in the informant to eventually talk about her work experiences. It was, in a word, a conversational means to enter the occupational social worlds of emergency frontline caseworkers, in a perhaps indirect way.

This somewhat opportunistic move also helped to ease the initial tension of first encounters. Whether we like it or not, most of us seek to distill the anxiety of first encounters by adopting taken-for-granted (and opportunistic) strategies to bring people closer together. Beginning with small-talk, finding common ground, discussing our shared affinities to make way into the bulk of the matter. It allowed me and informants to forge some mutual connection. Other times, the simple talk of dogs was useful, and so on. These conversational “opening wedges” to my mind, served as potent catalysts for a deeper inquiry into informants’ lived social reality. The self is not static, granting that doing qualitative interviewing is something that really does change the self. Like anybody you want to further get to know, you must find some common ground or shared affinity. That is, some mutual ground to make shared experience intelligible and therefore, at least, initial connection possible. People are not expected to robotically provide you with their life stories and experiences, nor should they be. They are not mechanical dispensers. In a way, you must show them why they should let you into their social worlds, one being that the project is perceived by them as worthwhile, and that you, as researcher, provide them with a sense of comfort, respect, and trust. Like any other interaction with another human being. Research is no exception. In the interviewing situation this meant finding common ground that

we could situate ourselves in, whether it was art, dogs, or something else.

Therefore, rather than crafting a thoroughly distanced approach, I was inviting informants into getting to know who I was as well, therefore not remaining too aloof, withdrawn, or cold. I was in the world with others. Yet, my role was not that of a frontline caseworker. Goode and Maskovsky (2002) have argued that there is no such thing as too close in qualitative research. Others have sought to establish friendships (Murphy and Dingwall 2001). These friendships may even last after exiting the field (Rock 2001). Being amicable with one’s research informants does not intrinsically constitute a problem. Thompson (2002), for instance, found that informants may expect researchers to take on an explicit friendship role. Bourgois (1995) attended Christmas office parties while conducting field research on crack dealers. Adler and Adler (1991:174), for instance, befriended their neighbor who happened to be a drug smuggler, they write, “over the years we became close friends with both him, his (ultimately divorced) wife, and his whole network of associates, spending frequent time together, testifying at his various trials, and taking him into our home to live for seven months after he was released from jail.” Over-rapport, however, between researcher and researched could introduce complications into the field research process and depends principally on the field context and the situational sense of the interview. I have discovered in my own research that taking on the role of friend may not be intrinsically beneficial or rewarding. I had come to reckon that the taxing and busy work worlds of caseworkers, for one thing, undercut any avenues to friendships.

They had given their time. But, they had also established firm boundaries. They allowed me to talk with them at their workplaces, but outside of that, a strong *work-life boundary* was maintained. Work remained at work and any research about their work remained at work. Caseworkers learned to compartmentalize. I accepted this and came to the realization that when researchers and their informants become exceedingly close to one another, it can compromise the researchers standing in the field. Data may be spoiled, research focuses rendered unfocused, findings may be altered (Cassell and Wax 1980; Adler and Adler 1991; Taylor 1991; Wolf 1991; Fleisher 1998; Brinkmann and Kvale 2005). I accepted this. What is taken for granted may no longer be critically evaluated, namely, overripe relationships, I believe, can prevent the researcher from being able to critically observe and assess what is being taken for granted. Thus, when relationships in fieldwork are overripe, it can obscure the kind of relationship that is being maintained. I have found that managing a delicate balance between over-rapport and under-rapport served best in my field research and interviewing.

However, this did not mean that I was not empathetic. I practiced empathy while managing a boundary: the professional and working research role. In a sense, informants increased my awareness of the responsibility I had in practicing those boundaries (Dickson-Swift et al. 2006) that Gilbert (2001:12) argues involves “maintaining a clear internal sense of difference from the other.” I could never shake off the sense of being a researcher and so my experience was in cultivating a social and emotional balance, *an attempt to be close but not too close*. This was vital in ascertaining that I did not “lose myself”

in the reality of frontline caseworkers by either becoming overly friendly or morally judging them. The likeliness for friendship arising in the field is predominantly a boundary issue (Duncombe and Jessop 2002). The purpose was to neither sympathize nor befriend, but to empathize, which is fundamental to understanding. Fieldworkers may lose sight of this fundamental difference. Ethnographic fieldwork and interviewing certainly requires empathy, but it does not necessarily impose any moral obligations for sympathy.

These experiences confirm pre-existing fieldwork literature (Kavanaugh and Ayres 1998; Shaffir 1999; Duncombe and Jessop 2002; Gans 2003; Dickson-Swift et al. 2006) that maintaining a professional research role and distance is profoundly complex, controversial, and emotionally laden. I found that maintaining this boundary in practice was not easy, especially when themes were emotive and required a reciprocal, empathic move on the part of the researcher. I realized that there is a balance to be struck between professional distance (i.e., objectivity) and human emotion, without losing sight of our compassion as fellow human beings (Goodrum and Keys 2007).

The researcher’s relationship to the researched “requires rapport combined with objectivity” (Miller 1952:98). Herbert Gans (2003:91), for instance, writes that “if one becomes too identified with the people being studied, one is likely to ignore behavior they consider undesirable or unethical, and this can lead to partial or distorted findings.” Overly identifying with the professional, working role—remaining exceedingly dispassionate, cold, and

machine-like—as well as overdeveloping closeness are both blind spots in fieldwork. Some connection must be made. Otherwise, the research process becomes too administrative, sapping-out the humanistic qualities, and leaving unacknowledged the social reality of the research itself. In the words of Miller (1952:98), “the researcher should not become a mere machine.” The blind spot—becoming a mere machine—of under-rapport can prevent researchers from recognizing that there is not intrinsically any difference between the social interactions in the field and other social interactions apart from our aims as researchers.

By asking the researched about their work experiences, I thought this framing would increase informants’ comfort and contribute to my credibility because it is a topic meaningful to our lives: work is an “important source of self-identification” (Shaffir and Pawluch 2003:893). From that point, I informally directed the talking of these experiences towards my research aims. These experiences would provide insight into explanations of the experience of doing “dirty work,” work meaning, competing demands, and the construction of self-definition in and through work. For the most part, when my role as researcher was established, informants readily entered dialogue with me. One even said to me, “I know how difficult it must be to get good quality data, but you were open with me and I am willing to talk freely.” They demonstrated relaxed postures and divulged quite personal information, of course, to their own recollection, suggesting that they did feel at ease with me. These experiential accounts, I believe, enhanced the quality of the data collected (Shaffir 1991).

I emphasized the importance of understanding the informants’ concerns and perspectives about issues affecting them at work and the meanings they attach to their work lives. That is, whether, and how, their work was a source of self-identification. If I used words or terms that frontline service providers found inappropriate in explaining their experiences, I used their terminologies and definitions: “You are caring for clients?” I asked. “Not just caring, validating! There is an important difference!” For frontline service providers, working in non-profit organizations, servicing disenfranchised and stigmatized publics meant understanding the past and current events of clients. By respecting and honoring their individuality, frontline workers created meaning in their interactions and discovered their true “helping” potential. The frontline case managers role is to provide a non-judgmental, empathetic environment by understanding the contextual nature of the client’s situations and to aid in the discovery of the client’s own recovery potential, ranging from addiction to independent-living, by targeting client-centered goals and supporting them to repurpose their life-situation. This was, according to frontline caseworkers, distinct from caring in that its central obligations were to facilitate relief and validate suffering to promote understanding. But, most importantly, it was about maintaining humanizing relations. Validation was not just caring. It was an attempt made on the part of the frontline caseworker to situate themselves with their clients and to honor their stories and their suffering. Namely, to respect and recognize stories that have been, in the context of the public, made invisible. In other words, validating meant making visible what was invisible. By paying close attention to the meanings

they attached to their actions, experiences, forms, and practices of care, I was able to prevent any distancing on the part of informants, towards me, by respecting their definitions of what they were doing and avoiding language that alienated them or did not align with their experiences. Otherwise, I would have appeared as insensitive, careless, and indifferent to their definitions of the situation.

Caseworkers understood that working with homeless sex offenders were stigmatizing interactions. They also understood the social costs at stake. According to Sanders (2010b:105-106), “in addition to the ‘dirt’ that may rub off on ethnographers because of their ongoing contact with unsavory social worlds, fieldworkers are involved in employing a method that is typically regarded as inferior, commonsensical, or ‘unscientific.’” Caseworkers, as well as researchers, therefore, who concentrate their efforts on deviant populations who are in jeopardy of moral castigations are themselves in jeopardy of moral judgments. Jankowski (1991:16) observed that, “before going into the field, I decided that to do this research, I would have to remain neutral to behavior that society considered criminal.” What about the behavior or work that society generally considered deviant? When caseworkers confessed to remaining non-judgmental in their services to homeless sex offenders (or present or past criminals), at first, my spontaneous reaction was one of both incredulity and repugnance, “How could you stand there and listen to them?” Like Jankowski (1991), I was aware that caseworkers dealt with a complex client base and decided that it would be best to remain neutral to the way they defined their work activities and the ideological rationalizations for why they did what

they did, despite the moral judgments that society may cast on such service. If I remained thoroughly neutral and detached, caseworkers would think me rude—like the “rest of them”—judgmental and dispassionate. But, I was not disinterested. Remaining neutral was merely a matter of expressing a degree of respect and constituting a *frame of analysis* that helped to avoid defining what was being said and done as a moral or ethical dilemma.

Goffman (2009) described how stigma spread out in waves of diminishing intensity. Those who work directly or act in association with people who are socially stigmatized may spread from the stigmatized person to those close to him or her. Caseworkers thus suffer a “courtesy stigma.” The stigma is transferable twice-removed to people who associate with the socially stigmatized (i.e., people or places). Although caseworkers bear more social costs of working with socially stigmatized clients, I discovered that researchers themselves can also bear social costs, in a diminishing intensity. Friends and family, especially, warned of the risks and dangers of doing research in a homeless shelter, “With the fentanyl crisis and drug users, why do you want to hang around them or near them? Why the hell do you want to spend your time studying those places!” “They are all criminals and drug addicts!”

My early fieldnotes reflect my edginess with hygienic issues and my general discomfort of being in a shelter:

One thing that caseworkers emphasized as a physical danger was fentanyl. Fentanyl was a major concern. One caseworker told me that, “I’ve known so

many of my clients that have died that I've started to forget their names." Most deaths resulting from fentanyl overdose. They tell me that just a bit of it can be a great danger. They tell me to be mindful. If you want to stay in the shelter, be mindful. Well, what if I come into contact with it in the shelter? It can be anywhere and it's invisible to the eye! I am actually very uncomfortable and uneasy with being here. Not only that, yesterday, while observing an intake interview, the caseworker left momentarily to print some papers and the intake client stared straight at me. He wouldn't budge. He then blurted, "What the fuck are you looking at?"

Many people outside the academy, whom I knew and frequented, could not understand why I would do this sort of research and did not see its importance. In fact, when I spoke candidly about my research during a party, my cousin said, "Okay, Julian," and walked away. Further, many ridiculed my research on "dirty work" as something not to be taken seriously and treated it as unworthy of study. It was the subject of jokes and laughter. Some were shocked and did not know how to respond. A friend of mine said after telling of my research that, "I always knew you were a little weird."

I therefore began to manage the information I would communicate about my research. This was done either through withdrawal or concealing the true nature of the study. I began to become extremely edgy and uneasy when people asked about my research, "A thesis? A thesis about what?" "Dirty work? Sociology? What about sexology? That's dirty! [laughs]." It was never taken seriously. I was constantly hassled about it. So, I began to redefine my

research more abstractly, presenting it as a "study in work and occupations," "service work," "non-governmental organizations," "the sociological nature of work," or "a phenomenological study of work" remaining both vague and general as to manage and negotiate a certain presentation of self. I was mounting a performance to display myself in a particular manner, but also to conceal the feelings of unworthiness, inadequacy, and ridicule that I felt previously through other people's reactions. I thus avoided "stigma symbols" that would spoil my self-image as a worthy academic. I then would attempt to change the topic instantly, so to avoid further questioning, "So! How are you?" Therefore, at times, I felt "silly" and "degraded" when people laughed or treated my research topic with sarcastic humor and contempt. For instance, some friends used to joke, quite aggressively, "They gave you money to write about that crap." In all, these experiences allowed me to acquire a shared understanding with caseworkers about the outside worlds relative hostility to their work. For instance, when I told a relative that caseworkers sometimes work with "sex offenders," he said, "I couldn't do that! They must be a little wack." So, this confirmed a lot of the experiences that caseworkers were describing, as I had heard them from occupational outsiders myself. It was not thought of as "legitimate research."

"We Don't Take Welfare Scum"

I felt anger at many of the stories that caseworkers told me about the hardships that their clients faced: from being called "scum" by landlords, ridiculed by physicians, scornfully looked down upon by nurses, their heartbreaking stories of addiction and family

violence. Landlord stigmatization of persons experiencing homelessness and addiction affected me the most. It was also a major problem for caseworkers themselves, as they were the people responsible for mediating relations with landlords under the Canadian Housing First (HF) policy and programmatic approach to homelessness. Caseworkers directly witnessed incidences of abuse and discrimination. Landlords labeled their homeless clients as “dirty,” “irresponsible,” “all junkies,” “hopeless,” “manipulative,” and “stupid”. The following interview excerpt underscores the caseworkers’ frustrations in dealing with landlords:

We spend so much time and energy negotiating with landlords and finding housing. Trying to convince landlords why they should rent to this person...it’s really frustrating because they don’t want them really. They think they are all a bunch of dirty junkies. Untrustworthy and blameworthy. They are vilified. But, in order to meet our monthly quotas to the city, sometimes you just have to take the easiest client, which is... sort of contrary to the whole Housing First policy because priorities should be for the chronically homeless, those with the highest acuity and the highest needs. But, most of these clients have mental health problems, addictions, and an ingrained lifestyle which makes it more difficult. We end up neglecting the needs of the chronically homeless because they are too difficult to manage with the time constraints the city offers. We are burdened by the pressure of time.

Some landlords will straight up say, when you call them, what do you do for a living? “Well, I am not calling for myself, I am calling on behalf of a client”—and they are like, “Oh, well, what does your client do

for a living?” and I say, “They are on Ontario Works (OW) or ODSF.” Right away, they snap saying, “We don’t take welfare scum,” many have made these sorts of remarks to me on the phone and hung up immediately. I hate it, don’t they understand? I have also had somebody say, “They are all drug addicts!” So, that stigmatization makes our work very difficult. It’s illegal too. It takes a lot of time and energy to deal with the bureaucracy of the human rights commission or to file a claim. Most clients don’t even want to, they are scared or don’t have the means to do so. This problem is with small private landlords. The bigger ones rarely reject. Nonetheless, housing is shrinking. How can you have a Housing First policy without homes? Some have disabilities too, which prevents them from working. The government has granted them with this because they recognized their disability. How can you hold that against them?

Emergency caseworkers are constantly framing client advocacy and therefore the way in which the HF policy is implemented (Lipsky 2010). The stigmatizing discourses of the “homeless drug addict” and the “blameworthy homeless client” made caseworkers’ work much more difficult. These elements were designated by workers as some of the “dirtier” parts of their job, undermining their service ideals and helping potential. Although I experienced a variety of emotions throughout the interview interactions, from anger, sadness, to indignation, I was careful to maintain a neutral attitude about what the caseworkers told me. By managing a stance of affective neutrality, I was negotiating my role as researcher in our interaction, namely, positioning myself as a researcher. I had to deal with my embodied experiences. I had to suppress the feelings

of indignation to manage a performance that was compatible with the professional, working research role I was presenting. However, I always nodded to show understanding, but never did I express full-fledged endorsement of their frustrations. I listened and paid attention. According to Dickson-Swift and colleagues (2007:68), “active management of feelings is central to research on sensitive topics as researchers often change the way they would normally act while engaged in research.” This neutrality, nonetheless, adhered to moral principles of reciprocity, respect, and trust: treating the researched as equals in our conversation. I listened to caseworkers’ recount repeated accounts of misunderstandings their clients faced, the lack of sensitivity to their personal needs by government agencies, medical professionals, and the likes, and the minimization of their concerns. Institutions, it appeared—through the storytelling of caseworkers—truly made the homeless feel invisible. Yet, although these accounts are themselves important, I was interested in understanding the experienced subjectivity of what it meant to be on the frontline. When caseworkers spoke about their clients’ experiences—as they often did—I reframed the discussion in such a way as to have them account for their work experiences, to speak about their subjective experiences: “So, tell me, how does the stigmatization of your clients influence your work?” She responded, “Well, people begin to think that you have the same mentality as them.”

Capturing a Moment in Life: Interviews as Social Occasions

My interviews were conducted in an anti-positivist way. From a positivistic interview standpoint, the

informants’ knowledge is perceived as a thing to be extracted; as simply a conduit for information. However, in my best efforts, I attempted to treat the interview as a social occasion, namely, as an event taking place in social reality. In this sense, I had to situate myself as a social actor—as an interviewer. I perceived the interview as a social event, a process whereby, through our interactions, me as a researcher and the researched were collaborating jointly in knowledge production. Certain questions had my informants take time to reflect on their own understandings of work and their experiences. They were themselves, it is true, attempting to discern and order their experiences, coherently, which is, for the most part, taken-for-granted—a natural attitude in the Husserlian (Husserl 1970; 2013; Heap and Roth 1973) and Schützian sense (Schütz 1967; 1970). But, this was not achieved on their own. Like Kubátová (2018), I saw the interview as an encounter. The conversational interchange between me and my informants allowed for certain kinds of understandings and meanings to be constructed: the way they understood my question, as well as the way I responded to their answers. This interchange produced unique conversational avenues which could only be achieved naturalistically rather than with strict, albeit aloof, adherence to a structured interview guide. I therefore saw the interview process contingently: unpredictable, spontaneous conversational instances emerging out of our interaction that would have not occurred had I stuck necessarily to my interview guide and structured the interview in a determinative way. A determinative approach would be treating knowledge as a thing; informants as mere conduits for information. But, the interview is a social dynamic. Interview informants are prac-

tioners of everyday life, which means that they themselves are constantly trying to make sense of their lived social realities.

Constructivism in qualitative research emphasizes a collaborative approach to the interview (Denzin 2001; Charmaz 2003; 2006). Constructivism correctly understands the interview context as a predominantly sense-making activity; an experience and a negotiated effort between both researcher and researched, namely, a co-construction of knowledge (Lincoln and Guba 1985; Adler and Adler 2012; Wojciechowska 2018).

In many instances, when I asked research subjects to discuss their interactions with clients, or when I asked them which clients they identified more with, my informants referred to various social roles. For example, one of my informants could identify more intensely with mothers and their stories. She would claim, “Speaking as a mother, I could identify with her more closely.” Others had identified with the situations of their clients as well. One informant recounted a story where she was easily able to identify with a client whose mother was suffering from cancer. The informant was in a similar predicament, which facilitated a process of constructing a more trustworthy relationship. My informants thus actively took on many roles. Therefore, I could not simply focus on him or her as an occupational member, although this was my purpose. I had to recognize how a variety of roles enter and blur the boundaries within and between occupational identities. In other words, the expression of a *stable occupational identity* became more difficult to discern, on my part, considering the changing relations be-

tween worker and client. Workers used various social roles (mother, daughter, etc.) in life when their relational relevancies became appropriate, in order to identify and empathize with clients as much as possible.

The occupational identities of caseworkers, therefore, were negotiated contextually, to help embolden relations and reinforce the interaction between worker and client. Their pasts, personalities, and personal experiences (i.e., as previous social service users) came to the fore when they felt that they could effectively relate to clients to better enhance the development of trust. Therefore, I thought, that to treat informants as having a stable occupational identity would be to reify the concept of occupational identity and to ignore the role-complexities involved within occupational identities. It would be misguided, considering that, truly, we act out multiple roles, contingent on and modulated by social interaction. In other words, by paying specifically close attention to informants’ narratives of how they build relations with clients at work, I sought to discern and understand the multiple roles informants enacted. Caseworkers learned to present many faces. When identification with clients became more difficult and awkward—sometimes leading to complete interactional failures—workers adapted a more professional role, where they sought to achieve a delicate balance between expressing concern for the client, while simultaneously maintaining a professional distance—a benign detachment—all in good faith. This contradictory social position also allowed caseworkers to remain emotionally sound. Role-playing, therefore, to an extent, were also strategies for emotional self-management. Therefore, the “individual

worker,” that is, the emergency caseworker, does not always act out a single role, but rather acts out roles strategically, to acquire compliance, trust, and respect from clients. His or her identity is more situational, being both shaped by the interactive processes of client-centered practice and the needs of their clientele. Interactive adaptation, namely, a keen awareness of the changing nature of social interactions, is perceptively felt among caseworkers, who must necessarily engage in various role-maneuvers to better build and sustain relationships with their clientele.

If we take strict, procedural, and structured guidelines of survey interviews, for instance, we can see that instead of producing more objective data, the structured interview and its confining, rigid rules can be understood as simply producing another version of truth; one that reflects primarily the assumptions of the researcher, as much as it tells us about real, lived experiences and attitudes. Take rigidly structured surveys that adhere to responses measured (operationalized) in accordance to categorical formats such as “Highly Agree,” “Agree,” “Neutral,” “Disagree,” and “Highly Disagree.” What does this tell us? It may tell us the likeliness or the unlikeliness of something, say our attitudes towards abortion. However, it leaves many questions unanswered. One must therefore be attentive to the ways in which interviewing, as a social occasion, takes part in *the active creation of meaning and reality*. Interviews are a part of daily life. Where my informants involved themselves in empathetic, routine understanding of their clients, I too engaged in empathetic understanding—I had to understand how caseworkers understood and per-

ceived their own interactions in the social world: Why did they do the things they did? Why that way and not another? And why was that meaningful to them?

Reflecting on the Interview(s) and Their Dynamic

What is meant by the interview? Or more precisely, what is an interview? Doing interviews undoubtedly led me to seek an answer to this question—I was making it a part of my everyday reality, as my predominant research tool. Apart from reflecting on my interview experiences, I began to consider the rationale of interviewing, as a research method. My immediate impression was that it was effective in getting at an in-depth, profound understanding of lived experience. It was effective in capturing a moment of real social life—in its immediate situation. We read interviews in the newspaper, in celebrity magazines, in journals. A consequence of this is a tendency to simplify. Our familiarity with the interview is that there is an interviewee who answers questions posted by an interviewer. Despite this popular, commonplace understanding, the interview, from my experiences, is a much more complex and critical instrument. Interviewing goes beyond simple fact gathering—what are facts if facts are to be interpreted?

I am reminded of Max Weber’s *verstehen*: reality must be understood, and this means that the researcher attempts to construct meaning and interpretation from the empirical facts, in the context of conversation. Sure, the interviewer, the researcher, is doubtless involved in some fact-gathering pro-

cess. But, these facts do not speak for themselves. Even if the researcher were to completely ignore his informants' sense-making of these facts, he or she would also have to involve themselves in a process of sense-making. The interview thus makes sense-making interactional. In other words, the interview is an interpersonal process of sense-making. Lest not forget that Weber himself adhered to a Neo-Kantian methodology (Eliaeson 1990; Ciaffa 1998): the researcher is, despite his or her diligent and scrupulous duty to maintain objectivity, bringing something to the facts. They must be faithful to interpreting these facts objectively. The researcher goes into the field, collects data, and then interprets this data. The process of interpretation takes this data and organizes it into concepts or categories to make sense of what is going on. The human mind does this. And the human mind seeks sense. That is, the process of interpreting makes the social world *intelligible*. This process is constructive. Weber (2017) called these ideal-typical constructions, Schütz (1967; 1970), phenomenologically speaking, in a similar tone, understood these as intersubjectively constructed typifications or "everyday ideal types." But, I found that this process of *interpretive construction* does not always occur *ex post facto*, it occurs in the interview. During the interview, I had already found myself interpreting and attempting to make sense of what was going on, and this determined the way I was to pose the next question. The interaction, in other words, shaped the interview context. So, in a sense, I myself was embedded within a chain of interactions, faced with the immediacy of a social situation, attempting to grasp, interpret, and understand. Immersing oneself in these chains of interactions entails a willingness to genuinely

want to hear, to understand an individual and their lived social world.

A concern that kept on propping up in my interview experiences, as I reflected on them, was how to provide a way for my interviewee, my informant, to speak in his or her genuine voice. What kind of space, or interview milieu was I to provide to reinforce and support this? It also meant reducing the power relation between interview and interviewee. Power relation? Despite rigid, structured interviews, a power relation does occur. We are getting what we want from them, not in a mischievous exploitative sense, but rather we are trying to elicit certain kinds of responses, determining some frame whereby certain responses emerge, almost presupposed. This is not their genuine voice, or, perhaps, is it? The purpose of qualitative research, to my understanding, is to reveal the interviewees' stories, experiences, and meaning-constructions of their lived social reality, in their authentic voice. A power relation can emerge, I think, whenever we prevent this from happening. Preventing this from happening is not only an exercise of power, but it risks the fundamental premise upon which qualitative research rests: relativism and therefore objectivity. To be objective is to permit genuine participant responses. This is what it means to heed to perspective, namely, to methodological relativism and therefore to sincere social science. Charmaz's (2003; 2006) constructivist approach to grounded theory, for instance, assumes the relativism of multiple social realities. Certainly, this does not go without any mistakes: we are trying to understand another's experiences. No easy task. Despite my constant self-doubt, it has only helped to reflect on methodological issues and the very deep

complexity and labor-intensive difficulties of conducting interviews.

It is difficult to know exactly where the unstructured interview is going (Corbin and Morse 2003). Therefore, I needed to decide whether to encourage informants to talk about a particular topic, remain silent, or whether to stop the interview if I was upsetting them—to “switch gears.” According to my field notes:

There was one informant that appeared to express ambiguity about the interview experience. She also downed herself about giving the right answer. I assured her that I was interested in her experiences, not in absolute truth and that she need not worry. I also established disclosure limits. I knew that her reality was intriguing, complex, and that she had an important story to tell about her work experiences, yet, when she seemed reluctant to delve further, I did not insist on a probe, but left it as is. In a way, I had to read her non-verbal cues, to ensure participant comfort. When we discussed parts of the job that the informant did not like, she replied, “Cleaning the toilets, it’s embarrassing. I don’t get why we have to do that.” I then pressed her to explain why she thought it was embarrassing. While answering, she would tap her foot repeatedly. I read this as a message of discomfort, tension, or a sense of feeling annoyed. I would move quickly to the next question or skip it entirely. As I did, she began to appear more at ease. The foot stopped thumping and her bodily movements moved more freely. Perhaps the question was an unwanted one. At the outset of the interview I informed her that this interview is voluntary and that she had every right to refuse to

answer questions. But, sometimes we are shy or unwilling to object. So, we express ourselves indirectly. By respecting her personal boundaries, I was able to acquire a level of trust, which proved essential to gathering valid data. As the interview proceeded, by implying a level of boundary-maintenance, she was more at ease and the conversation began to flow better, more casually. The interactive dynamic of the interview, I found, is therefore fundamental to the sense-making process: who they were, how they appeared, interacted, and how they expressed themselves non-verbally.

The interview should not be used as interrogation or confession, or as a counseling session (Corbin and Morse 2003). Informants may not be interested in discussing things that the researcher may want to know. As the above example demonstrates, it was inappropriate to probe informant’s issues that appeared to upset or potentially annoy them. Researchers should listen attentively to what is being spoken. What is being spoken may not always be verbalized in words. These non-verbal cues are open to interpretation and make the interview interaction—as well as the cultivation of boundaries—complex. A level of boundary work in maintaining our respective roles works at the level of sense-making in social interaction.

Other informants were more at ease to describe some of their distressing tasks. Others also revealed their distressing pasts. According to Corbin and Morse (2003), being overly concerned about the potential risks implies that distress aroused by talking to a researcher is greater to friend or family. Contrarily, they argue that researchers may be

more willing to show interest and to empathize. Many of my informants expressed the need to have somebody listen to them. After the interview, I always asked how they thought the interview went. Gladly, most said it went well, noting that I was “attentive, calm, and soft-spoken.” Many thanked me for taking the time to speak with them. One informant claimed that, “You made me feel very comfortable, at ease.” Interviewing itself can be regarded as a kind of emotion work (Hochschild 1979; 1990). I felt myself sharing an experience with my informant. In moments, I found myself regulating and managing my own emotional reactions. I had to engage with a series of emotionally challenging situations. Caseworkers were routinely involved in markedly dirty (and distressing) activities. While caseworkers have become increasingly untroubled by feces, urine, and vomit—as something they “got used to,” to “just wear the gear”—their “death work” always remained increasingly troubling. It was something they could never get used to. Death “happens in hospitals.” According to one caseworker:

One guy was puking blood...he was shaking and saying gibberish. I was shaking also; my nerves were shot. No matter how much training, you are just so scared. I've also had a guy who had a seizure because he used. I gave him CPR...you gotta do it...you are praying and praying for the paramedics to arrive. It is not an easy thing to cope with. It was hard to see...I've worked closely with a bunch of clients who passed away. It really bothered me because they just finished their recovery program and relapsed. You can never get used to that...it breaks your heart.

The following excerpt is from my field notes:

When caseworkers discussed the deaths of some of their clients, they always lowered their heads and become both visibly sad and tearful. I would feel myself shaking up inside. My eyes softened and began to well. I realized, in that moment, that it was vital for me to maintain some critical distance and to empathize. I had to pull myself together and mount a performance I did not feel. I wasn't a stone. I knew Mark [informant] trusted me. To disclose his personal feelings the way he did...it takes a level of trust. I had demonstrated active listening and gave him the space he required—always. I regarded this as a form of empathic understanding...while remaining centered...and respecting the emotional situation and by that, I mean giving him *his space*. It was Marks moment, not mine. Even though I wanted to cry—I wished to share in his distress—it was his turn, not mine. I couldn't reverse our roles. I had to remain detached...while showing concern and understanding. In a way, that is what I imagined what was expected of me. Imagine if I broke down? Is that what he needs? It was not my story that mattered. I rationalized that caseworkers must keep it in all the time...their distress. Perhaps it was his turn to have someone listen to him. He then validated my assumptions when he told me, “Thanks for remaining solid with me. I just needed someone to listen...not to speak or say anything, just to listen.”

Harley, a research informant, also divulged her own experiences as a previous service user. She told me about her experiences with domestic violence. In those moments, I remained sensitive and empathetic. According to Shaffir, Stebbins, and Turowetz (1980:iv), “the intensity of the fieldwork process is

typically accompanied by a psychological anxiety resulting in a continuous presentation and management of self when in the presence of those studied.” Managing self and emotions became crucial in situations where informants became tearful or expressed heavy feelings of anger. Researchers may feel uncertain about whether self-disclosure about their own experiences is helpful or even appropriate in the interview context. When informants became tearful or expressed anger during interviews, I asked them if they wished to take a break. I always made sure to acknowledge their emotions. I felt myself moved to respond to informants simply as a fellow human being (Dickson-Swift et al. 2007), demonstrating concern and empathy about the pain and difficult experiences they were describing. However, I had clearly established a research role and my job was not to be a friend. If I had shared too close of an experience, I would have caused confusion about the purpose of our meeting. To a degree, I had to manage my emotions during the interview interaction according to the feeling-rules (Hochschild 1979) that regulate appropriate research encounters, while simultaneously continuing to acknowledge their stories. In those moments, I wanted to cry. Seeing their pain was overwhelming. I had not expected it. But, as a professional researcher, I had to make sure that the interview remained an interview. My own emotional self-disclosure had to be curbed. I had to maintain the boundary between our roles, which did involve continuous emotion work to sustain an emotional equilibrium: negotiating a centered, demure, and empathic distance, while recognizing that the boundaries I carved are the contingent outcomes of our situational interactions. Verily, I felt dissonance between the performance I mounted

and the emotions I was feeling. Listening in a sensitive and empathetic manner can be emotionally draining. Often, I found myself sharing with participants’ feelings of loss, sadness, and anger (Corbin and Morse 2003). I frequently felt exhausted following the interview.

Dickson-Swift and colleagues (2007) interviewed qualitative researchers with a focus on emotion work and discovered a series of techniques that researchers used to deal with strong emotions. A common strategy was to abide to the normative feeling-rules of professionalism. As Atkinson and Hammersley (1994) argue, striking a balance between over-rapport and under-rapport in the field and within interview context (and encounter) can result from the lingering emotional challenges intrinsic to fieldwork. For me, this was an invaluable strategy, especially as a novice researcher. Further, much emotion and *boundary work* are involved in balancing over and under rapport (Miller 1952). In those instances, a neutral emotional display was required (Kleinman and Copp 1993). I had to ensure that when our conversations came to sensitive topics that I demonstrated understanding, care, and attentiveness, regardless of whether or not I actually felt these. I had to display myself in a way that showed me to be engaged when, in a deeper sense, I was not. I thought that if I shared too much of my views or emotions on a topic, I would risk silencing them. So, I was reluctant to do that. Yet if I remained too aloof, might I appear indifferent, unresponsive, or even disagreeable to their values or plights? I had to cultivate a balance. I had to at times act when I did not get it. I had to take on the role of the *keen listener*. Or, resist the temptation of overly identifying with

some of their responses. It was difficult, an instance of dread I would say, I felt, because it kept settling me in a position of ambivalence: compelled to think quickly about what to do in each specific situation, in response to each response. Field interactions and interviews, I realized, are deeply uncertain. Building rapport was one way to do this. But, this always runs the risk of being or appearing as inauthentic—and they may know it! Observing this phoniness may also create disengagement. I had to remain professionally interested, unassuming, and reticent. Remaining cool, collected, and presenting myself as professionally interested and as a keen listener was one way to overcome perceived phoniness. They knew it. I knew it. I came here to do what I had to do. They knew I was a researcher, not a caseworker. I did not share their occupational social worlds. We respected our differences and learned to respect each other's boundaries.

Subjectivity or Machines?

The process of self-reflection, knowledge, and experience are integral to the research process and should be conceived of as fundamental to the interpretive process. Imagine a researcher without any *subjective relation* to any of the content and experience of the realities of interviewing. Could a machine do this in that case? Process all the information without being affected by it? Perhaps a machine. But, as human beings, we are not machines. There is something that it is like for me to experience these interviews. As such, I feel myself to be called to give considerable thought to my experiences, emotions, and to explicitly reflect upon the way it acts as a frame of analysis. Quite frankly, researchers who do not draw

from, or discuss, their own personal experiences and emotions during the research process, at least to some degree, in the explication of the research process, are in some ways being dishonest. They are not machines. I certainly hope not. I have felt elation and happiness, a sense of purpose and doing something worthwhile. Other times, in full disclosure, I have felt anxiety and frustration in response to informants who at times gave one- or two-word answers to some of my questions. These feelings had to be managed. I could not disclose to them my intimate feelings under certain circumstances of increased frustration. I had to remain composed and perseverant. I too engaged in some shape or form, what Hochschild (1979) calls surface-acting. However, when the interview, as an extended conversation, developed, I engaged in deep-acting to try and discern what my informants meant by some of their experiences.

When Sylvia, a research informant, spoke about her recourse to acting as an empowering agent for her clients, she said to me, "I just can't help them if they are not helping themselves. My job is to support, not to do things for them." She appeared dismal. She had pulled her head down in sadness. I could tell that it bothered her. She wanted to help clients when clients were not helping themselves, but she was also aware of the detriments of emotional overinvestment. We sat there in a moment of silence. I tried to empathize with Sylvia by attempting to situate myself in her world, not in an objective world of structure and relations. But, in her world. "I understand," I told her, "It is a strain I see, and it can be hard when caring for others and yourself seems to clash in some ways. But, remember, you are doing

good work.” In that specific moment, I sought to reaffirm the positive self-definitions Sylvia had previously ascribed to herself and her work earlier in the interview, to ensure that matters of comfort and our interview experience remained one of sincere understanding. Charmaz (1991:275) reported that, “as a researcher, I sought to have people tell me about their lives from their perspectives, rather than to force my preconceived interests and categories upon them. So, I listened.” The emotional context, therefore, establishes, to an extent, the interaction coherency of the interview relationship. Besides making sense-making and data collection an interactional process, the role of performed emotions is pivotal to the contextual framing of data collection. Like Ezzy (2010), I tried to imagine what kind of person I had to feel myself as to make the informant comfortable and willing to reveal her story. It was about locating the reality of her experiences [in the case of Sylvia], not mine. She responded with a smile, “Thank you. It is tough sometimes. But, I know I am here for a reason. That’s what it is...you don’t want to shove them away because in the end it’s all worthwhile, yet...at the same time it’s a stressor.” The interview, to my mind, had to be one of committed interaction and sincerity while simultaneously adhering to the moral principles of respect, kindness, and reciprocity. It was one of “communion,” not conquest (Ezzy 2010).

Reflecting on how Sylvia and other informants spoke about their work experiences, as well as their sense of occupational belonging, I realized that in our interactions, a major thematic and analytical focus arose: although frontline staff wanted to reduce the pressure of their work, whether that pressure

be due to their clientele or organizationally rooted, they were also reluctant to reduce their caseloads by turning clients away. I reiterated to Sylvia if, “interacting with and helping clients is what made your work both difficult and important?” By reiterating this question back to Sylvia, I hoped to cue her into confirming the meaning such a statement had to her, thereby making sense-making interactional. In a way, I was struck by the nuanced complexity of the craft of interviewing. What reality were we referring to? It can go many ways. Frontline staff were overwhelmed, but wanted to genuinely help. They managed this contradictory position by redirecting to occupational experiences of satisfaction, experiences that kept them believing that they were helping people in need and repurposed their work as necessary and vital. Certainly, their sentiments and beliefs did not reduce the reality of workloads and client-related tensions, but it did help frontline staff to redefine their dilemmas as worthwhile and moral, hence allowing them to negotiate the meaning and importance of their work. Amy, another informant, spoke quite candidly about her experience with managing such a contradictory caring role, “the interaction with the clients is what I love about this work. Yet, it is also what is the hardest to do because sometimes it really drains you...You realize eventually that it is a highly stressed environment.” In all, what I did come to recognize, thematically, was that the consequences of emotion work and its benefits, for frontline staff, were closely linked. What made their work hard also made their work meaningful.

Surface-acting became a necessary buffer when understanding had to be left to the device of data

analysis, because perhaps in that specific interview encounter, I did not quite understand what was being said. Understanding was not always immediately felt and experienced on my part. Yet, to keep the conversation relational and human, I have to give a sense that informants were being understood, so they would remain open with me, in their own voice, about their experiences. This performance is not sheer deceptive trickery. It may appear that way. But, I regarded it as a momentary façade indispensable to acquiring a richer understanding of their situations. It was a way of framing the interactions in a specific way and at least, keeping them going. But why? Because accounts are constituted interactively. Not just by the researchers thought-process, but by the very engaged interaction, which informs and modulates the development of certain kinds of talk between researcher and informant. The interactive process of the interview, I mean to say, has profound implications for both the epistemology of sociological research and the quality of the data outcomes. One must be transparent about the interview process by reconsidering their own interview experiences and interactions with informants to demonstrate precisely how knowledge-claims, or thematic focuses emerged. Even my own anxiety, to a certain extent, framed the interactive dynamic of the interviews in their early phases. I became more withdrawn, discouraging probing strategies. My anxious feelings, so to speak, limited certain kinds of interactions, henceforth setting limits on what kind of data was and could be collected and therefore influencing the construction of accounts. However, after completing two to three interviews, I became more comfortable. After each interview, I would reflect to see how my framing, through my own in-

dividual proclivities, could be altered to yield more fruitful data. Therefore, researchers should conduct themselves reflexively at all times. They should re-analyze their experiences of the context of the interview situation and reconsider their accounts of data. This self-referential frame, which I did adopt, helped to provide an increased depth on the topic studied. By approaching the interview situation through a frame of contingency—although we may become terrifyingly paranoid of the always-possible alternatives in constructing accounts—however, we may also come to the realization that contingent possibilities in the interactionism of the interview situation do exist. This self-awareness merely prompted me into recognizing that researchers and informants interactively accomplish the constructing of accounts.

Nonetheless, to have a right and privilege to construct (interpret) informants' representations of their lived realities, one must necessarily, first and foremost, make their informants feel relaxed, be natural, demonstrate empathy, understanding, and serious interest in what informants are saying or had to say. Being simply detached or objective would not get me the data I needed. I needed personal data, real-stories, and experiences. Maintaining the former only sustains one's outsider status. One must be-with rather than remain outside. Although the status, in my experience, of the *outsider looking in* can never be fully removed, it can be modified to some degree. In such cases, simple modifications can be the best thing one can do in the present moment.

Before and after the interview, I thought that engaging in an informal chat could help to raise comfort

levels. I found this very helpful. Simple changes in body language could be witnessed, as informants appeared to relax and feel more comfortable with me. I would ask them, "How their day was going?" for instance. In another instance, we laughed together (over something funny that happened) which eased tension. After the interview, some informants had described the interview as "cathartic," as a weight lifted. This helped them to infuse the interview with purpose. Therefore, informants noted that it was cathartic and beneficial to them to discuss openly about their experiences. When conducting research on death and other sensitive or distressing topics, Ansell and Van Blerk (2005:72) observe that researchers may not be causing the distress, but "merely provoking it into the open." While this can be uncomfortable for the researcher, the interviewee is not necessarily "harmed" by the experience (Ansell and Van Blerk 2005) and it may, in fact, be cathartic. After the interviews, many of my informants expressed appreciation and were thankful for being given the opportunity to share their work worlds. They saw the interview as a moment to "let everything out." Some workers also saw the interview as an opportunity to vent, especially about the "dirtier" parts of their work:

Me: Is there anything else you would like to add about your experiences? Anything else that I missed that you would like to talk about?

Informant: I really enjoyed it! You got me all...it was nice to vent!

I like being given the opportunity to discuss this stuff. Not many hear our voices...so it's an opportunity to vent and reveal the real work we do here.

I always found, for the most part, that informants expressed a willingness to help me in my research understanding. Corbin and Morse (2003) point out that informants usually express willingness to participate in the study because they want something in return. People want to tell their stories. The opportunity to be interviewed, if the informant sees a potential return for him or her can make qualitative research intrinsically reciprocal. Some may want to voice concern about something, express their experiences, vent, to speak to an empathetic listener, or express an eagerness to help the researcher. In other words, researchers, informants know, will make the attempt to understand them, especially their own definitions of the less appealing parts of their work, where family and friends may not. They therefore saw the interview as a suitable time and place to "finally talk about our work without fearing the response." Many of my informants also simply wanted to help and saw the project as worthwhile. Another typical comment was, "I am just so happy I could help! I am helping someone get their degree!" They expressed gratitude and believed themselves to be caring and kind people willing to help others. Many expressed appreciations at the point of contact. Despite having at times noting the emotionally draining dimensions of their work, they expressed overall a positive experience and they said that they received many gains from involvement. For instance, one informant bluntly told me, "Thank you so much for caring!" Many expressed that they had taken-for-granted many of their experiences and that the interview helped to understand themselves better. Furthermore, many said that they never had the opportunity to be and ex-

press who they were, totally, to reveal their authentic work experiences. They also expressed appreciation that someone was giving recognition to their work and lives. That they were attempting to see how important it was for them.

What Is an In-Depth Interview?

What is the in-depth interview? I followed an (semi)unstructured interview style. Why? Because one can never know for certain what form an interview is going to take nor what kind of form the interaction will generate. The unstructured interview style recognizes, in my view, the uniqueness of each interview. This, of course, did not mean that I did not prepare questions. It is just that I allowed the interaction to take precedence. The interaction allowed for major thematic focuses to arise. I then redirected the interaction to these focuses. This, I believe, adheres to the basic premises of grounded theory. But, I decided to integrate a phenomenological perspective by focusing emphatically on work practice and experiences. There is a world out there. But, my focus, thus far in this research, has not been on the world as such. Rather, it has been on the subjects-in-the-world, namely, on my informants' experiences of the world and in this case study, their experiences on the frontline, the meanings they derive from their work, their occupational culture, and the interactions with clients. How do my informants experience the suffering of others? Their clients? The less appealing parts of their work? How do they experience the process of work? Is it mundane, taken-for-granted? Straightforwardly unrewarding? Or positively rewarding? Why? How? This is the *social phenomenological* ten-

dency in my research. We must begin with subjective experience, with experiencing the informant as an individual, like any of us, trying to make sense, subjectively, about our own position within a kind of social reality. Take the experience of art, for instance. If I were to study this phenomenologically, I would not, however, it would not be an entirely bad idea, examine the texture or reduce a painting to its fine parts, perhaps learn about its technical construction. Rather I would try to examine how the subject has experienced the painting, or the visit to a museum. I would not focus on the museum as such. I would not be particularly interested in how many entrances the museum has, or water fountains. Or toilets. I would be more interested in knowing the way my respondent experienced these objects. Not the objects in themselves.

Most importantly, in all, such experiences express the meaningfulness of relationships. That is, they express the meaning relationships have for us. I went into the field with no preconceptions, knowing nothing at all. I was, in some ways, the "village fool." This is what piqued my interest, learning about their experiences. Early scientific and quantitative research approaches were underpinned by the positivist ontological and epistemological perspective (Becker 1996; Cheek, Onslow, and Cream 2004). This perspective viewed reality as something concrete and tangible, something that can be measured, observed, and understood as an independent reality. As objectively understood. Further, the lived intricacies of individuals and groups, in quantitative approaches, are not being captured by variables. Grills and

Prus (2008) argue this point when they claim that independent variables are fundamentally social categories that we use to define situations. They are social categories, rather than causal determinants. Independent variables thus represent social categories that leave the complex realities of “humanly engaged matters” (Grills and Prus 2008) unexamined.

I remain unconvinced by this ontological approach when confronting reality as experienced. I do not see much utility with an ontology of objective independence when the complexities of religious identification, occupational belonging, collective identity, wishes, and meanings of informants are sutured. That is not to say that there is no such thing as reality. That would be silly. It is just to say that reality is constructed rather than given, experienced as real rather than reified. Qualitative research approaches, I have come to reckon, see the nature of social reality as less tangible and more (inter)subjective. I am a qualitative researcher insofar as I was trying to discover concrete instances of the subjective. In the path of Schütz (1967; 1970), in which reality is conceptualized as socially and mentally constructed and thus fundamentally (inter)subjective (Prus 1996; Laverly 2003), my qualitative investigation began with experience of the life-world of my informants. Within this perspective, there are multiple realities that are specific to the individual or groups that create them. As Prus (1996:22) has written, “People are seen to develop (multiple) worldviews or definitions of reality as they interact with one another and attempt to incorporate particular objects of their awareness into their

activities.” This justifies a focus on the lived experience of research informants. With qualitative research especially, I do not see any other place to begin.

It is important to note that what I have demonstrated in these field notes is a largely reflective, albeit introspective exercise. I was positioned as a learner and I am still learning the “tricks of the trade.” That is what is so curious about qualitative research. Nonetheless, one should avoid being “narcissistic, overly reflexive, and not scientific” (Denzin 1997:xv). The personal experience of qualitative research should help to clarify the research process, however, it should not “result in an obsessive preoccupation” (Shaffir 1999:677). Our personal experiences must not override the process of acquainting ourselves with the perspectives of others. Field notes and field reflections provide an important bridge, I think, between field and analytical themes. In this instance, one could rightly say that I am situated in Dante’s limbo. But, a special kind of limbo; a peculiar limbo that every researcher gets him or herself caught up in. One could say that at this specific moment, I am somewhere between science and life.

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An Analysis of Biographies in Collective Memory Research: The Method of Socio-Historical Analysis

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Abstract This article explores the use of biographies in qualitative research about collective memory. It is argued that commemorative ceremonies, as well as changes appearing in macro-level structures within the time-span of individuals' life histories need to be included when analyzing biographies in collective memory studies. The article suggests enhancement of the biographical case reconstruction method (Rosenthal 1993; 2004) with two additional stages: analysis of the experienced past with more emphasis on socio-historical transformations; and inclusion and analysis of the ethnographical data collected from collective mnemonic practices. By providing empirical data from the research conducted with political exiles in Germany, these analytical steps of the method of socio-historical analysis are demonstrated in detail.

Keywords Biographical Research; Collective Memory; Mnemonic-Practices; Narration, Socio-Historical Analysis

Methods of interpreting biographies are a vital issue in memory studies, since the interaction between historical events and their representations in present societies are reflected in individuals' reconstructions of the past, hence construct-

ing we-groupings and collective consciousness. C. Wright Mills (1959) was one of the first sociologists who emphasized the importance of the interaction between biography and history in his classic work *The Sociological Imagination*. He argued that even troubles that seem to be personal and individual have their roots in structural transformations, and he claimed that studying a case without considering its structural transformations is like studying "the process of birth, but ignoring motherhood" (Mills 1959:147). Therefore, he asserted that "no social study that does not come back to the problems of biography, of history, and of their intersections within a society has completed its intellectual journey" (Mills 1959:7).

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In this article, I try to demonstrate how collective memory scholars could deal with the analysis of individual accounts (biographies) and biographies' intersections with structural transformations. I argue that the method of biographical case reconstruction is appropriate to the study of collective memory, since in analytical steps, the researcher has to connect life histories with historical upheavals and compare them with present reconstructions (life stories). Nevertheless, for a deepened understanding of collective memory, more attention should be paid to the analyses of the past political, economic, historical events (structural transformations) and their relation to life histories; secondly, life histories and life stories need to be analyzed in an ongoing dialogue with the data collected at commemorative ceremonies; and finally, hypotheses should be generated at a socio-historical level, rather than as psychological interpretations of individuals' actions, decisions, and expressions.

In what follows, I firstly outline the methodology of biographical case reconstruction with its advantages and limits for memory scholars. I then briefly explain Halbwachs' theory of collective memory. After demonstrating the dynamic social structure of collective memory, I will introduce socio-historical analysis of the biographies based on the empirical data from my research with political exiles living in Germany. I will elaborate on the inclusion of collective mnemonic practices (rituals such as commemorations) and public representations of the past (museums, monuments, memorials, etc.) to the biographical narratives. Finally, I will discuss how hypotheses need to be generated in the socio-historical interpretation of the biographies in memory studies at the macro level.

Analyzing Experienced Lives and Narrated Memories

To analyze and compare life histories and life stories as they are constructed as parts of a whole, the researcher needs to conduct biographical narrative interviews through which s/he will be able to have the whole life story of the narrator. The method of biographical narrative interviewing, as developed by Fritz Schütze (1976; 1983), allows us to grasp experienced events in a flow of narration. The biographical case reconstruction method's main characteristic is its distinction between the experienced life (life history) and the told life (life story). The approach of analyzing life history and the told life story separately is common to some other qualitative methods that deal with biographical narratives, such as: case history, longitudinal methodology, and narrative psychosocial analysis. Analyzing experienced life and told life stories separately is generated by researchers' interest in the interaction between the individual stories and social and cultural milieus.

The comparison between life histories and life stories is a useful approach to understand how individuals construct their experienced past lives—their life histories—within the present context. In other words, this analytical step helps us to understand the dynamic structure of the past and how it is being reconstructed within the present social frameworks. Making a chronological order of the objective data (date of birth, education, employment, information regarding family history, political and historical events that could be documented) from the narrations helps to compare what has been experienced

and how past experiences are being narrated. However, the researcher has to do an additional search of archives, newspapers, and any other records to find other historical events, which might have affected the biographers' lives in the past. This is also essential for the comparison of life stories (narrated past) and life histories. In the biographical case reconstruction method, researchers aim to understand what individuals experienced, what meaning they gave to their past experiences, and how they interpret that meaning in the present (Rosenthal 2004:49). Memory scholars, however, who intend to understand the effects of a past event or era on today's (and possibly future) societies, need to investigate how biographies are embedded in macro-level social transformations and vice-versa, instead of focusing on a "biographically constituted context." Unlike traditional phenomenological life-world analysis (Schütz 1962; Luckmann 1983), which begins with experiences of the individuals, for memory studies, it is vital to understand in which conditions and under which circumstances facts are experienced.

Socially Constructed Memory

With historical and social context of experienced past and past being re-made in the present, I refer to two main features of Halbwachs' collective memory theory, namely, memory as a social construction and as a dynamic process rather than a thing. A collectively shared perspective of the past is necessary not only for group solidarity, but also a group's "identity" depends on a common understanding of past things. Individuals perceive things together with their values and ideas,

which are open to change as the individual's position in society changes (Mannheim 1952). The construction of the past and recall are based on collective consciousness, and "it is in society that people normally acquire their memories. It is also in society that they recall, recognize, and localize their memories" (Halbwachs 1992:38). For a better understanding of the social frameworks of memory, Halbwachs (1992:76) analyzes various milieus "in which all men—or most of them—spend their lives," for example, family, religious groups, and social classes.

To provide solidarity and continuity, we-groups modify the memory of the past according to the necessities of the present. This is a dynamic process of constructing the past, and of generating a collectively shared present which is based on an "agreed" common past (Hobsbawm 1983). This agreed common past is transferred among generations to strengthen members' feeling of social belonging (Rosenthal 2016). Past and present are always in interaction in the collective memory of groups, since the continuity and solidarity of groups are essential for their survival.

Mills (1959:149) finds this effort of understanding single facts with the use of historical materials similar to the Marxist principle of historical specificity which refers to understanding any specific society according to the conditions, the institutions, the ideologies, and power relations existent in a specific given period. In the following, I try to demonstrate, through empirical cases, how we can understand biographical narrations of the past by keeping the principle of historical specificity in mind.

Socio-Historical Analysis of the Biographies

The characteristics of collective memory argued above, namely, that memory is socially constructed and has a dynamic structure, necessitate revision in the method of biographical case reconstruction for the scholars who want to understand how a particular event or period is being remembered in the present and what the effects of past events are on the present social context. The distinction between the experienced past and narrated past in the analysis of biographies is quite helpful in understanding the link between narration, experience, and memory (Rosenthal 2006; 2016). However, based on the hermeneutic tradition (Oevermann 1979), the method of biographical case reconstruction puts more emphasis on the structure of the interview text and individual interpretations of single accounts than social transformations from which the narrated past is generated and re-generated.

Although in her article about the development of the qualitative longitudinal case history method, Thomson (2009) thinks that the *individual provides a key to unlock the social*, she asserts that “interview-based research” is inefficient to understand the social context. She argues that even an analysis of two accounts, told life and experienced life, does not help to solve the problem of understanding the social and historical context. To overcome this gap, the method of socio-historical analysis suggests changes from the early stages of data collection to the analysis of collected data. Accordingly, in addition to the biographical narratives, detailed data from the field research is required. Researchers

should collect data from ceremonies, commemorations, meetings that are relevant to the topic of the research, and the memoirs and audio-visual data that are available.

Theoretical sampling (Glaser and Strauss 1967) is the main principle of sampling in the biographical narrative interview method. Selection of historical facts seems to be a more complicated issue. While dealing with excessive amount of historical facts, researchers are expected to select some of those facts while excluding others without developing a biased perspective towards the research subject. To overcome the problem of limiting facts, I suggest collective memory researchers adhere to the framework/design of the research, as well as to their main research question (points of interest) by asking continuously, “which of these facts/events/transformations help me to understand that specific event and past experiences of the biographers?” Second, data about historical events and social changes needs to be tested analytically with the interview materials to understand if there is an interaction, or it does not have a crucial effect on the researched subject. Finally, it is important to discuss historical data with other researchers to include other perspectives and to avoid developing a biased perspective towards the researched event, subject, or period.

The first step in the socio-historical analysis of biographies, similar to the biographical case reconstruction method, is distinguishing lived life and narrated life; in other words, the past as experienced and the past as narrated. In both analytical steps, socio-historical conditions and the effects of

those conditions on the narrations of individuals and narrations in social contexts are included in the analysis. The steps in the socio-historical analysis of biographies are as follows:

- a1. Analysis of the biographical data (chronology of the experienced life)
- a2. Analysis of the past social, economic, and cultural changes (chronology of the historical events)
- b1. Life-Story of the Individual
- b2. Story of the past as re-constructed in collective representations
- c. Comparison and categorization

The analysis of the experienced past is similar to the biographical case reconstruction method. However, in the socio-historical analysis of the biographies, we need to distinguish between a chronological ordering of events in the life-course of the biographer and historical events in general in order to see the interplay between the biographies of individuals and the social structures. The hypothesis should also be based on the interaction between events in general and biographical changes. The first step of the analysis, thus far called the experienced past, helps memory researchers to understand the social frameworks of memory in the past, whereas the second step of the analysis, namely, the narrated past, facilitates understanding of the present reconstruction of the past and how that specific past is being reconstructed in group activities, which in turn affects individual remember-

ing. In the following sections, I will elaborate on the use of the method through empirical data from research about remembrance of the 1980 Turkish military coup d'état.

The Research and the Case of Merve

The research about the remembrance of the 1980 military coup d'état lasted almost four years between 2009 and 2013. It was conducted in both Germany and Turkey for a comparison between the reconstruction of the past by exiles who live in Germany and people who stayed in Turkey.¹ This comparative structure aimed at understanding how the remembrance of the same past event is reconstructed in the present by members of the political left. In addition to the biographical narrative interviews and expert interviews, field notes were gathered on commemorative ceremonies and other mnemonic practices, places, and tools (such as museums, monuments, memoirs) through the participant observation method.

I am going to use the case of Merve² to elaborate the method of socio-historical analysis of biographies in

¹ For the study I conducted 19 interviews, nine with women and ten with men. Nine of these interviews were conducted in Turkey and the other ten were done in Germany. The oldest interview partner was 61 and the youngest was 43 years old at the time of the interview. Excluding these two, the average age of the interviewees was 54. Most of the interview partners, apart from the aforementioned oldest and youngest, explained that they began to have sympathy for the left movement when they were in high school and were engaged in the left movement towards the end of the 70s. Another interesting characteristic of the biographies common to many is the similarity of their migration background. In seventeen interviews, interviewees talked about moving from rural Anatolian towns to big cities.

² All personal names, as well as the names of the locations, institutions, and organizations are pseudonyms.

detail.³ The interview was conducted in December 2010 in Germany. Merve was born in 1958, in a small city on the Black Sea coast of Turkey. She was the youngest child of five (one brother and three older sisters). Her father owned a restaurant in the city, and her mother was a housewife. The family moved to S. (a small city in the south east) when Merve was 6 years old, because her brother got a job there as a judge. In 1968, the family had to move again to city A., which is a relatively big city in the south, and there Merve finished primary school and started middle school. In 1973, after an exam to get into the Faculty of Education, she registered in a regular high school in her hometown. In 1975, while studying at high school, she met her future husband, Mehmet.

Mehmet was born in 1948 and was teaching literature at Merve's high school. Merve started working as a clerk in a state institute after her second unsuccessful attempt to study at the Faculty of Education. During her relationship with Mehmet, she began sympathizing with the leftist movement and started taking part in local activities. In 1977, Merve married Mehmet, who at that time was one of the leading revolutionaries in the city, and active in the teachers' union. After their marriage, they had problems with the state authorities because of their

³ In order to explain the interaction between biographical narrations and collective narrations, I refer to the main characteristics of mnemonic practices in the case of the 1980 military coup d'état, based on the findings of my research that lasted about four years. Because of space limitations, I am not able to discuss more cases (similar and counter), or provide a detailed analysis of commemorative events. Moreover, the aim of the paper is not to analyze a social phenomenon as a whole, rather to explain the methodological approach of socio-historical analysis of the biographies by focusing on one case as a sample to demonstrate the use of the method.

involvement with leftist organizations, and therefore were expelled to other cities several times.

On the morning of 12 September 1980, Merve, her sister, and Mehmet were arrested at her parents' house and taken to the Faculty of Education in the city, where they were all tortured. Merve was released at the beginning of November and Mehmet was transported to a Martial Law Inquiry Center in a neighboring city for questioning, which took 75 days. After the questioning period he was imprisoned in the military prison. Due to the brutal torture, Mehmet suffered from serious health problems between 1981 and 1984, while he was still in prison. After a medical report stating, that "Mehmet has throat cancer at the level of risk of death," he was released from prison. The family fled to Germany for treatment in March 1984, where they requested asylum.

In 1986, Merve gave birth to their daughter, their first child, and three years later, in 1989, their son was born. Merve and Mehmet were both active in the establishment and activities of a human rights organization in Germany. In 2006, Mehmet's health problems got worse and he suffered from an embolism, and he died in March 2007.

At the time of the interview, Merve was 52 years old and working part time in the social services sector. Based on her biography and the data from the book she has written in memory of her husband, the chronological ordering of her lived life could be made from her date of birth until 2010, the date of the interview. I demonstrate here only the events in Merve's life history until her release from the prison in 1980.

Table 1. Chronology of Merve's life history.

Date	Events
1958	Turkey, small town (Black Sea Coast) Gender: Female Father has a restaurant 4 older siblings (one brother and 3 sisters) Migration of family from I. (city) to A. (town) 16th November: Merve was born in (town)
1963 (5 years old)	Merve starts primary school Father closes the restaurant
1964-1965	Brother graduates from the Faculty of Law in A. (western city) and starts working as a judge in (eastern city) One of Merve's sisters gets married and gives birth to Merve's first nephew/niece Family moves from A. (town) to S. (an eastern city) (except one of her sisters) Merve continues primary school in S.
1968-1973	Family moves to (big city in the south) Merve finishes primary school and starts middle school
1973	Merve finishes middle school Merve moves to (town where she was born) and there she starts high school
1975	February: Mehmet (born in 1948) starts teaching literature at high school (the same high school where Merve is a student) Merve meets Mehmet, who is her future husband Involvement in X (leftist organization) Graduates from high school Registered at the Faculty of Education in (big eastern city)
1976	Merve starts working at the Institution of Forestry She is fired from this first job after 3 months She finds another job at a credit institution
1977 (19 years old)	18th June: Engaged to Mehmet (28 years old) 16th November: Married to Mehmet
1978	Merve's involvement in Civil Servants' Organization Mehmet is active at (Teachers' organization / Left)
1979	Merve is expelled to G. (city at the Black Sea Coast) Mehmet is expelled to B. (city in the east) On Mehmet's first day in B. (city), he is attacked at the hotel where he is staying and goes back to A.
1980 (22 years old)	Mehmet is expelled to K. (small conservative town) Merve and Mehmet go to K. together for 2-3 days and return to A. 12 September: Merve, her sister, and Mehmet arrested early in the morning They were brought to School of Education in A. for questioning Both Merve and Mehmet are tortured The first meeting of Merve and Mehmet since the coup (after 30 days) at the prison Mehmet is sent to Martial Rule Inquiry Centre in another city E. End of October: Mehmet is brought back to A. prison Mehmet is sent back to Inquiry Centre in E. Merve is released (end of October, beginning of November) Starts running a newspaper kiosk with another friend

Merve's life history, although chronologically ordered with every possible experienced event by using additional sources, is on its own not effective in understanding in what circumstances it was "made." Therefore, before starting a sequential abductive procedure, we need a detailed chronology of the economic, historical, and social upheavals and transformations that appeared during this period of Merve's life. This second step in the chronology of past events in general is fairly common to all interviews for a particular subject of research, in this case the 1980 military coup d'état in Turkey. Nevertheless, in this step, the memory researcher should work like a historian who tries to connect the micro effects of macro-level changes and vice-versa. This means for each biographer, we need to do additional research

to find out the socio-historical conditions they experienced in that specific region and time: what other events happened in the place where the biographers lived, the organizational and institutional changes in which they were involved, et cetera.

The experience of rapid changes mostly does not come up in the biographical narratives, since they become part of ordinary life. The biographers do not go into the details of these ordinary events; they may assume that these are already known by the researcher. Therefore, before going into a detailed analysis of Merve's narration, it is helpful to look at the table of events which is used in the analysis of each interview for the chronology of the past socio-historical events of the 1980 military coup d'état.

Table 2. Chronology of the past events concerning the 1980 military coup d'état in Turkey.

Date	Events
1960	27.05.1960 military coup d'état against the Democrat Party (DP) government (Execution of the Prime Minister Adnan Menderes and other members of the DP)
1971	12.03.1971 2nd military coup d'état
1972	30.03.1972 Kizildere Massacre 6.05.1972 execution of 3 THKO members: Deniz Gezmiş, Yusuf Aslan, Hüseyin İnan
1974	Cyprus Conflict
1977	1 May: Bloody May 05.06.1977 (national elections, CHP obtained the largest share of the vote—41%)
1978	05.01.1978 new government is established by Ecevit 16.03.1978 attack against university students in Istanbul. 7 killed, 41 wounded 04.09.1978 massacre in Sivas 09.10.1978 Bahçelievler massacre (murder of 7 TİP militants in Ankara) 26.12.1978 massacre in Maras (150 Alevis killed by extremists in Maras)
1979	11.07.1979 military operation in Fatsa Ecevit government withdrawn New right-wing coalition government established on 25.11.1979 27.12.1979 a warning letter from generals to the president
1980	24 January: IMF's enforcement of economic regulations and approval of neo-liberal economic plan May-July: massacre in Corum (57 Alevis killed, according to official records) 12 September: coup d'état

Table 2 consists of the political, economic, and social upheavals experienced between 1960 and 1980, during the time-span that is the subject of the research. Many of my interview partners were born between the years 1955-1960, and involved in leftist politics towards the end of the 70s, thus they have gone through the events listed above.

In the case of Merve, after the sequential analysis of her biography, we can see the frequency of radical shifts in her life. The first period of her life could be summarized under the concept of mobility: since she was born, she had to move from one town to another, and from one city to another city with family members. The second period of her life consists of her marriage, education, and employment, as well as when she was politically active. Between 1978 and 1980, Merve and her husband were expelled many times. Looking at this data, the hypotheses regarding the first period is generated based on the family's decisions, and for the second period her and her husband's political engagement and activities.

If the rising social movements of the '70s and its effects on youth are excluded when looking at Merve's biography, one could falsely interpret that she and her husband had extraordinary life experiences as leftist militants. One could even think that Merve and her husband Mehmet were "marginal," "illegal," and "radical" militants. However, knowing the political ambivalence and violence in the country, the frequency of murders even in the rural sphere, massacres of civilians, strikes, students' and youth movements' activities, the hypothesis should refer to the commonness of political engagement in society, as well as state institutions' oppressive actions and

how individuals' lives were affected by those conditions. A similar example is Merve's narration about her and her family's mobility between the years 1958-1975. However, the hypothesis should be generated through comparing two tables: the chronology of the biographer's life history and the chronology of the socio-historical events. While generating hypotheses about Merve's family's frequent mobility, one needs to ask in what socio-political conditions the fact of mobility took place, in addition to generating hypotheses on personal choices, individual motivations, and other micro-level interests.

The first date in the table of socio-historical events is the military coup d'état against the Democrat Party (DP) government in 1960, followed by the second military coup d'état in 1971. From the beginning of the 1950s, the DP government supported mechanization in agriculture as arranged by the Marshall Plan agreed between the US and Turkish governments in 1948. The second key project of the DP government, following the Marshall Plan, was to build road networks in the country instead of investing in railways (the negative effects of not investing in railways were experienced especially during the oil crisis of 1973). The short-term effect of agricultural mechanization was unemployment among the rural population that did not own any means of production other than their labor. This labor was no longer needed in the mechanized rural areas and hence the 1950s also witnessed a dramatic acceleration of rural-to-urban migration in Turkey. Both push and pull factors were behind this movement, as conditions in rural areas differed widely across the country. The development of the road network also contributed to the new mobility (Pamuk 2008:282).

The effects of these political upheavals could be observed in Merve's life history. Mobility, which is common to other interviewees who come from rural families, could be understood in relation to macro-level structural changes summarized here. The most visible results of this internal migration from rural areas to cities were housing problems, and, in the long term, social, cultural, economic, and political conflicts between "newcomers" and the settled urban classes.

Having done this research on the political and economic structures, my hypothesis about Merve's family's mobility focuses more on the possible conflicts family members might have experienced with the urban classes and their attachment and interaction with other "newcomers." Following the analysis of the life history and the socio-historical data, the narration(s) of the past need to be analyzed by doing thematic field analysis as suggested in the biographical case reconstruction method. The next section will demonstrate how collective memory scholars can analyze life narrations and narrations of the past in mnemonic activities.

Analysis of Narrated (Reconstructed) Past

The analysis of the narrated past aims at understanding how the past is being remembered in the present and how collective memory is being reconstructed within the present social frameworks. Therefore, as in the case of the analysis of the experienced past, the analysis of the narrated past also has two main steps:

b1. Life story of the individual

b2. The story of the past as re-constructed in collective practices/representations/presentations

The second step of the analysis of the collective commemorative practices, which is missing in the biographical case reconstruction method, is based on data collected during ethnographical fieldwork. Each piece of data collected (memos, visual materials, meeting notes, letters) in the field should be classified according to the types of events, and then sequentialized depending on the text structure. The aim of this stage is to understand which of the past events are frequently "re-visited" in collective ceremonies, in what context and in which structure those events are being narrated, and which of the events are left out of the collective narrations in these present-day constructions of the past. Through this analysis of the collective narrations we try to comprehend the interaction between the memory of the past as re-constructed by groups and individuals' reconstructions. To put it another way, the objective in this analytical step is to understand the presentist characteristics of collective memory, as Halbwachs suggests.

Similar to the biographical case reconstruction method (Rosenthal 1993; 2003), we sequentialize the biographer's narration depending on the turn-takings, textual types, and thematic shifts. For thematic field analysis, collective memory researchers should put more emphasis on how biographers make sense of their biographies in relation to the experienced socio-political events for a better understanding of the "embeddedness of the biographical account in social macro structures" (Apitzsch and Siouti 2007:7). I shall demonstrate this step of analysis by

using Merve's narration. What follows is the part of Merve's narrative about her high school years, her meeting with Mehmet, and their marriage, and involvement in the left-wing movement, until the morning of the military coup d'état:

and we were married. Mehmet is at that time an important person for (Location-City) (Revolutionary Organization). Our marriage has been so good. We cooked together, set the dinner table together. I mean, it was a kind of marriage, you know, it is always said theoretically, but many revolutionaries do the other way in practice. It is again the male hegemony at home. Our house was so different. Everybody used to visit us, the young. They stayed in our house, we eat and drink together. One takes a shower, the other takes one of my pullovers from my wardrobe; one wears my pullover, the other Mehmet's. Actually, it was such a life like a commune. It was a house everyone wished to come and stay. We had books, and lent them to friends, discussions, talks. I was in the organization of civil servants. We established it and I was working there. Mehmet was in (Organization-teachers' union). Our organizations were in the same neighborhood. We go to organizations after work, and from there to home. I come home late at nights and wash the clothes in the machine, whirr... No time. At the weekends seminars, we went to well...awareness raising, local activities, et cetera. It was a very busy three years. There were not too many fascists in (Location-City) anyway. Usually people are social democrats. But, the fascists could //...// come from other cities and attack. I mean the things like murders were done by unknown perpetrators, could happen in the darkness. I mean our house was, we were going through pathways in

a forest. Every day on the way home (we were worried) if an attack would happen. As you know, those were the days, on the news we heard about one or two missing teachers every day. (Location-city) had many funerals from other cities. Many of them were teachers. //...// We lived in such a good period. A life where we got to know friendship; a life full of love and where we could die for each other. I was a civil servant and Mehmet was a teacher, ahh //...//. I was exiled twice. How many times was Mehmet exiled? //...//. Three or so, he was also exiled. The change of decision takes such a long time of course, the change done by the high court, Supreme Court. When he started working, they exiled him again to somewhere else. He was exiled to (Location-Town). He went there, and the night he went they put a bomb in front of the hotel. Well, they threatened him and so on. It was a very fascist town. He came back and later //...// ahh... One day the movements, everywhere in Turkey, the workers were demonstrating for their rights, students and workers became more aware. Women started to become more aware, and wanted their free rights. We were in my mother's [house], one night //...// in the morning. But, the day before we painted our apartment, a friend came from (Location-city). He said, "You painted the house, but the future, the near future does not seem likely." He meant that something like a coup d'état could happen. I mean, we knew it and they knew it. Friends who were in charge did not do enough (preventive steps). Okay, they began in the villages. I mean, they began to dig the things, shelters and so on, but when I think now, we weren't cautious enough. That night we stayed at my mother's. Early in the morning came the police cars, announcements, the military, and a curfew.

Sequentialization of this part of the interview according to text types is as follows.

Table 3. Sequentialization of the interview with Merve.

Text type	
Report	Marriage. "Mehmet was at that time an important person for (Organization-Revolutionary)."
Narration	The early part of the marriage, like communal life with friends.
Report	Involvement in organizations
Narration	Attacks of "fascists" and fear because of daily kidnappings.
Description	She works as a civil servant and Mehmet as a teacher.
Report	Exiled to other cities and threat against Mehmet in a town.
Narration	Rise in the revolutionary movement in Turkey.
Narration	The morning of the military coup.
Argumentation	Friends knowing before, but were not prepared against military.
Narration	"Early in the morning came the police cars, announcements, the military, and a curfew."

In combination with this sequentialized data, we can now focus on the thematic field analysis by generating hypotheses according to thematic shifts and turn-takings. In this account, there is no turn-taking between interviewer and biographer; rather, it is

a flow of narration with frequent changes in structure between narration, report, description, and argumentation. Merve's and her husband's involvement in the left organizations and being exiled to other cities and towns come up as reports, whereas attacks, kidnappings, growth of the revolutionary movement, and the morning of the military coup d'état come up as narration.

The narrated life story should be analyzed together with the collectively narrated past: namely, the narrations of the past as re-constructed in collective practices, representations, or presentations. Mnemonic practices such as ceremonies and commemorations are particularly important for collective memory studies. This set of events and ceremonies is the phenomenon of ritual action, which is not only formal action that has expressive purposes, but also the practice of meaning-making and internalizing it. "Rites have the capacity to give value and meaning to the life of those who perform them" (Connerton 1989:45). By practicing ritual behavior, members of a group show their loyalty to that group and strengthen the idea of "we." The meanings of the ritual act which are embodied and transferred in and by symbolic texts, images, and figures are usually perceived and practiced without questioning. This does not mean that their existence is generated from some kind of abstract spiritual phenomenon, although they often appear to be, but from the material conditions of the present and the concrete needs of groups. We need to focus on how myths and rituals are formalized and performed, and on the ways they are effective in transmitting values, notions, ideas, and, in general, social memory, instead of seeking "to understand the 'hidden' point

that lies ‘behind’ ritual symbolism” (Connerton 1989:53).

Similarly, commemorative ceremonies are practices of constructing the past—who/what to remember, and how to remember. In other words, “the commemoration does the memory work for us” (Gook 2011:17). They are acts of collective remembering which in turn fill the gaps in individual recollections, emphasize some of the names, places, and events (i.e., ideas, notions, and values)—positively or negatively—and underestimate or exclude “others.” Commemorations are tools for making sense of history and constructing an agreed past. It is memory in practice, remembering with other members of the “we” group; in short, generating, strengthening, and “polishing” the collective identity. “The remnants of experience still live in the warmth of custom, in the repetition of the ancestral” (Nora 1989:7). Commemoration and rituals are the mediums that carry history/tradition into the present; they are the “place” where individuals can participate.

In my fieldwork, I participated in commemorations, conferences, and other ceremonies in both Turkey and Germany regarding the 1980 military coup d’état, mostly as a part of the audience. In some cases, I was invited as a guest by the organizers who knew about my research. Since my research question was about the remembrance of members of the political left in Turkey, I followed those mnemonic activities of the organizations that have connections with the left-wing movement in 1970s’ Turkey. I particularly followed the events in which my interview partners were involved either as organizers or as guests. Additionally, I have visited memory sites

(museums, monuments, prisons, exhibitions) regarding the past of the military coup.

The data from the collective activities needs to be categorized based on the thematized events and the structures of the narrations. While generating the hypothesis, collective memory scholars should ask what comes into commemorations, which events are narrated and how, what is left out, and how they are being told or presented. For memory sites and tools we also need to ask which images are dominating, what the order of the visual data is, and how it is presented. For instance, in my research of both memory sites and mnemonic practices, the narrations of the executions of members of the left movement are based on heroization and victimization. The past before the military coup d’état and the characteristics of those executed are glorified with the concepts of “the good old days,” “collectivism,” and “good friendships,” whereas the times after the military coup d’état are labeled with the terms “individualism,” “selfishness,” and “insecurity.”

In the comparison of the analysis of the commemorative events with biographical narrations, one can see the commonalities or differences between the individual construction of the past and social constructions of the past. It is worth analyzing which of the remembered events is similarly narrated, and which differ in commemorative ceremonies and memory sites. This helps us to discover the dynamics of reconstructing the past of a particular period or particular event(s) for a specific group of people (victims, survivors, perpetrators, or post-generations). In the case of Merve, she also refers to the “collectivism” between friends that is glorified, and

it is not difficult to assume that post-coup *d'état*, Merve's narration will continue with the destroyed relationships among friends and lack of solidarity, and hence the individualistic society in the present. For a better understanding of her narration of the "friendship," we need to generate our hypotheses considering the construction of this concept in the mnemonic practices. Similarly, while generating hypotheses on her narration of her husband, which is marked with concepts of "proud" and "pain," we need to consider the hegemonic discourse of victimization and heroization in the commemorative activities of the revolutionaries in general.

In my research on the commemorative events, I have observed the male dominance in the construction of the past of the revolutionary movement. Literally and visually, the gender of an "ordinary natural" revolutionary is pictured as male. Only in special circumstances, for instance, when talking about the resistance at the Mamak Military Prison, are the revolutionaries defined as "our female comrades," so "we" naturally refers to men. In the conferences and commemorative events, women are invited as witnesses to talk about their lost loved ones—husbands, brothers, or sons—but not to speak about their own experiences as revolutionaries. At conferences about the coup, there are usually separate sections named "Women and the '78s," "Women and 12th September," or "Women and the Revolution." This attitude is also observed at the Shame Museum, where the exhibited objects are selected by the organization of Revolutionary 78s. One small section on the top floor is given to female revolutionaries to exhibit their memories of 12th September. Even there, women are related with motherhood as baby clothes are exhib-

ited. Similarly, at Ulucanlar Prison Museum, there is almost nothing exhibited about the women revolutionaries who were imprisoned there and moreover the then-women's wing now serves as the gift shop of the prison museum. While generating a hypothesis on Merve's "overemphasis" on her husband's political activities, in addition to the hypotheses we generate on her personal explanations and structure of her narration, we need to consider the interaction between her narration and main characteristics of the narrations at the commemorative practices. In this case, we may, for instance, ask how the gendered image of "revolutionary" in collective representations and narrations affect Merve's narration of her husband's and her own political activities. This hypothesis needs to be tested by comparing similar and counter biographies.

For the step of final categorizations, both life story and life history analysis need to be compared and types and categories need to be constructed at an abstract level of the theory of collective memory. Some of the questions that need to be asked are: What are the main characteristics of remembering the past? Which actors play important roles and how? How do the group members remember the past in the present? Which events have left marks on the memory of this particular era and why? Similarly, which events are forgotten and why? Which of the political and social conditions are effective in remembrance of this particular event/era today? Asking these questions allows us to discuss remembrance of the researched event or era at the abstract level of understanding and contribute or challenge the theories concerning collective memory and biographical research.

Conclusion

In this article, I have discussed the method of socio-historical analysis and its use in memory studies. Similar to other qualitative research methods dealing with life stories, such as oral history, the life-history approach, the qualitative longitudinal and case-history methods, and biographical case reconstruction method, the method of socio-historical analysis aims at understanding the life stories within their social and historical contexts and tries to provide a “thick description” of a researched social phenomenon. To understand the effects of structural transformations on actors’ decisions and actions, longitudinal studies require “continuous research in the same small society over a number of years” (Holland, Thomson, and Henderson 2006:5). Based on the same need, the method of socio-historical analysis suggests analyzing social and political upheavals of the past that might have affected the narratives of the biographers.

In addition, the method of socio-historical analysis suggests a dual analysis of biographical narratives and narratives of mnemonic events in their socio-historical past and present contexts, which makes the

method particularly important for memory studies. Using the data from field research about the remembrance of the 1980 military coup d’état in Turkey, I have demonstrated two additional steps used in the method of socio-historical analysis: (1) chronology and analysis of the past social, economic, and cultural changes; and (2) analysis of the narration of the past as re-constructed in collective representations and practices. Through the case of Merve, I have demonstrated how biographies could be analyzed in relation to past socio-historical changes and present mnemonic practices. Such an analytical attempt to understand the interaction between biographies and socio-historical transformations could be perceived as an attempt to understand the use of the past by opposing groups (victims/perpetrators, authorities/general public, minorities/majorities, oppressed groups/dominants), power relations, and the dynamics of social belonging. Here, the method is discussed in relation to the theory of collective memory. However, I do believe that scholars from other disciplines could also benefit from this method, since the socio-historical analysis of narrations entails a discussion of memory in relation to construction of group solidarity, identity, and ideology.

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Yes! Kissing Too... The Child Would Not Be Hurt in Any Way: Social Constructions of Child Sexual Abuse in the Ga Community in Ghana

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Abstract The United Nations Convention on the Rights of the Child obligates nations to protect children from all forms of abuse including sexual. However, child sexual abuse is a complex phenomenon which is difficult to understand as a result of cultural reasons. In view of this, the programs, policies, and interventions put in place to curb the phenomenon of child sexual abuse globally can only be effective if they are relevant to the social and cultural setting within which it happens. This is an exploratory study that sought to investigate social constructions of child sexual abuse in the Ga community. The study uses qualitative research methods to collect data from 42 respondents via six focus group discussions across three selected towns in the Greater Accra Region of Ghana. The findings showed that sexual activities between adults and children that were regarded as acceptable did not constitute child sexual abuse. However, sexual behaviors between adults and children that were regarded as unacceptable were viewed as serious, hence, considered as sexual abuse. The findings of this study illustrate the need for appropriate cultural interventions in curbing the phenomenon of child sexual abuse in the Ga Community in Ghana.

Keywords Child Sexual Abuse; Acceptable Sexual Behaviors; Non-Acceptable Sexual Behaviors; Transactional Sex

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Introduction/Research Problem

Child sexual abuse (CSA) is a human rights issue which affects millions of children globally. In view of this, the United Nations Convention on the Rights of the Child, UNCRC,¹ puts an obligation on almost all states to protect children from all forms of maltreat-

¹ See: <https://www.unicef.org.uk/what-we-do/un-convention-child-rights/>. Retrieved July 03, 2019.

ment perpetrated by adults, including sexual abuse. Although there are laws in most states to protect children from sexual abuse, the reality on the ground is different. Today, significant figures of CSA cases continue to be reported by various professionals worldwide, including the police. The World Health Organization, for instance, reports that about 223 million children have experienced a form of sexual violence in their lives (United Nations Report 2006). Similarly, in Ghana, the Domestic Violence and Victim's Support Unit (DOVVSU) in their annual report estimated that about 1,304 children were defiled in 2014. Despite the alarming rates of CSA in many parts of the world, the official reports represent just a small percentage of the reality on the ground (Finkelhor 1994). This is mainly as a result of cultural reasons which bring variation in its definition (Saewyc, Pettingell, and Magee 2003).

The social constructionist argues against the idea that only one reality of CSA exists in the world and supports the view that alternative realities exist (Yerby 1995). As a result, there have been disagreements among researchers and practitioners regarding behaviors that constitute CSA (Russel 1983). This is because societal norms make it difficult for behaviors that constitute CSA to be clearly defined (Back and Lips 1998; Jones and Jemmott 2010). This view substantiates the thought that the reality of CSA as defined by the UNCRC, which is the basis for most laws protecting children from sexual abuse in modern times, may not reflect the reality of CSA in most societies. This is because people's experiences, realities, and perspectives of CSA are based on their socio-cultural circumstances, and these become their reality. In some cultures, there are behaviors that are

considered as sexual abuse, but are approved norms in other social settings. Thus, variation in people's attitudes and reactions regarding CSA is dependent on cultural guidelines which distinguish between suitable and unsuitable behaviors (Guma and Henda 2004). Irrespective of the variations in the attitudes and reactions of people regarding CSA in different cultures, sexual abuse affects the development of children in every society.

Due to the serious effects of CSA on its victims, it has been classified as a public health issue (Madu, Ndom, and Ramashia 2010). Research shows that children who are sexually abused go through serious mental, physical, psychological, and social effects (Roberts et al. 2009; Green et al. 2010). These effects can be reduced based on the social support children receive from their communities. However, cultural beliefs and practices can make children feel shameful and guilty about their experiences, which make disclosure difficult (Azad and Leander 2015). The removal of cultural barriers on CSA disclosure will make it possible for victims to openly talk about their experiences (Ronai 1995). According to Ronai (1995), the lack of openness about the subject of CSA amount to a denial of its existence and taking appropriate measures to address the situation. This empowers offenders to continue their cycle of abuse, since they will not be punished for their actions. In societies such as the Ga community in Ghana, cultural beliefs and practices regarding CSA victimize victims of CSA instead of providing them with the necessary support.

Ghana prides itself on being the first nation to ratify the UNCRC in 1990. It has also endorsed other international conventions like the African Charter on

Human and People's Rights and the Charter on the Rights and Welfare of the African Child and enacted several laws and policies that are to protect children from abuse. These include the amendment of the Criminal Code Act 1998 (Act 554), the Children's Act 1998 (Act 560), and the Domestic Violence Act 2007 (Act 732). Irrespective of these laws and policies to protect children from abuse, emerging evidence shows that the magnitude and frequency of CSA have not changed much in Ghana (Boakye 2009; DOVVSU 2014). This is as a result of cultural reasons which present the phenomenon of CSA from different lenses. The social constructionist theory posits that norms that construct CSA vary across cultures. In societies where violence against children is socially accepted and not perceived as abusive, children are made to suffer in silence instead of receiving support for their traumatic experience (Tetteh and Markwei 2018). Instead, they are regarded as deviants and blamed for their sexual abuse. Thus, making them vulnerable and feeling helpless about their situation (Tetteh and Markwei 2018). Hence, this study sought to identify socially acceptable behaviors that normalize the phenomenon of CSA in the Ga community. The intent of this study is to interrogate the extent to which the social construction of sexual abuse endangers children to CSA and propose measures in addressing the situation that defines CSA in the Ga community in Ghana.

Methodology

Research Design and Study Area

The study employed a qualitative research design. This approach was necessary because it allowed the

researcher to present a complex and holistic view of the world of the participants studied (Creswell 1998). The design also provided an opportunity that captured the subjective views of the participants' perceptions and unique experiences of sexual abuse (Neuman 2011). The data for this study is part of a broader study which looks at the socio-cultural perspective of CSA in the Ga community in Ghana. Focus group discussions, in-depth interviews, and case studies were used to solicit information for the socio-cultural context of CSA in the Ga community. However, for the purpose of this article, qualitative data from the focus group discussions which describe the social constructs of CSA in the Ga community are presented.

The study was conducted in the Ga community which lies in the southern part of Ghana, not more than 10 kilometers from the coast of the Akwapim hills. The Ga community is situated within the Greater Accra Region of Ghana and is also its capital. The population of the Ga as of 2001 was about 587,412 persons, with the greater number of them living in the Greater Accra Region of Ghana (GSS 2000). The Ga community has a decentralized traditional political administration centered on the six major settlements of Ga Mashi, La, Osu, Teshie, Nungua, and Tema (Brukum, Pwamang, and Tonah 2009). Due to globalization and migration, many parts of the Ga traditional area have become ethnically heterogeneous (Wellington 2002). These changes have, however, brought few improvements in the lives of many ordinary Ga residents, "with many Ga settlements suffering from increased poverty, overcrowding, and a generally poor infrastructure" (Brukum, Pwamang, and Tonah 2009:165).

Statistics from the DOVVSU (2014) show that the Greater Accra Region alone recorded 1,466 cases of CSA in the first nine months of 2014. This figure was described by many child rights advocates and the media as alarming. Again, the region recorded 553 reported cases of CSA, the highest among all the ten regions of Ghana in 2005 (DOVVSU 2006). Although these figures are relevant in the selection of the region for the study, it may not represent the exact situation on the ground, since most cases of CSA are usually not reported.

Socio-Demographic Characteristics of the Study Participants

The ages of the participants in the focus group discussions ranged from 20 to 70 years. Out of the 42 participants who took part in the study, 12 of them were between the ages of 20-40, 15 were between 41-51, 10 between 52-62, while five were between the ages of 63-73. The age distribution of the participants is a reflection of persons in their early adulthood, mid adulthood, and late adulthood and this represents different generations. In terms of education, 15 had primary, five secondary, and six tertiary educations, while 16 had not attended school. With regards to marital status, 15 of the respondents were married, five widowed, 12 single, and 10 divorced or separated. Also, the data showed that nearly all the participants had children. Further, all the participants stated that they were Christians, with a few stating that they did not belong to any religion and others said they were traditionalist, thus practicing African Traditional Religion. It was also realized that the majority of the participants engaged in low earning

occupations such as petty trading, fishing, fish mongering, and petty artistic jobs. However, some of the participants were unemployed.

Participants Selection and Sampling Technique

Three Ga towns (James Town, La, and Teshie Maami) were purposively selected for the study because these towns are occupied by large numbers of Ga indigenes. Individual research participants were also selected through the convenience sampling technique. This allowed participants to be selected based on their availability and willingness to participate in the study. The inclusion criteria included the participants being an indigene of the selected community and should come from similar socio-economic backgrounds. There was no gender discrimination for inclusion, both male and female community members qualified to participate in the study.

Data Collection Procedure

Focus group discussions (FGDs) were used to collect data for the study. This method enabled the author to capture broader views about the subject being studied in an open and non-controlling manner (Green 2007). Semi-structured interview guide was used to interview the participants. The author consulted the local community members to assist in contacting the research participants (Boira, Tomas-Aragones, and Rivera 2017). The composition of the group was segmented by the respondents' sex; hence, two separate group discussions were held for males and females. At La, each of the groups had the same number of participants,

eight, while at James Town and Teshie more females participated as compared to males. At James Town, seven females participated in their group, while the males' group was made up of six participants. At Teshie, the females' group comprised of seven people, while the males' group was made up of six participants. Therefore, a total of six focus group discussions were held in the three selected communities with a total of 42 participants. Participants were also selected based on their socio-economic backgrounds to enable easy communication and interaction in the groups. Irrespective of the participants' similarities in terms of their educational background, occupation, marital status, among others, some were opinion leaders in their communities through their positions as leaders of women, men, and youth groups. The respondents sampled enabled the researcher to have a deeper understanding of the social constructs of CSA in the Ga community.

The researcher facilitated all the discussions with the help of a Research Assistant who recorded and took notes while the discussions were going on. Before the discussions, the facilitator and Research Assistant introduced themselves to the groups and spelt out rules for the discussions. Participants were humbly asked not to interrupt when someone was making a contribution or responding to a question, and were also to show respect for each other. They were assured that there was no right or wrong answer to any of the issues discussed and persons could feel free to express their thoughts. These rules made all the discussions very interactive and insightful. Each focus group discussion lasted between 45 minutes and an hour.

Ethics

The researcher carefully considered all the ethical issues involved in dealing with a sensitive topic like CSA. Ethical approval was sought from the Noguchi Ethical Clearance Board of the University of Ghana. Verbal and written consent was sought before proceeding with the study. Anonymity of participants was also ensured by disguising their names through the use of pseudonyms.

Analysis

All the interviews were translated and transcribed into English. Thematic analysis was used to organize and categorize the data according to patterns and structures that connect the themes (Polit and Hunger 1997). The purpose of this was to have a full and clearer picture of the subject. This enabled the researcher to search for patterns in data which included recurrent behaviors, beliefs, and practices (Neuman 2011). The six steps involved in thematic analysis by Braun and Clarke (2006) were followed. At the first phase, the researcher familiarized herself with the data. At the second phase, initial codes were generated. The third phase involved the collation of the codes into potential themes and the gathering of data relevant to each theme. At the fourth phase, the themes were reviewed. At the fifth stage, the themes were defined and named. The sixth phase involved the production of the research report. The researcher crosschecked the interviews with the participants at a later day to ensure consistency and truthfulness in data. Furthermore, the researcher constantly crosschecked the transcribed data with the audio recordings to ensure validity of data.

Results

Using thematic analysis, three main themes emerged in the focus group discussions. Acceptable sexual activities, non-acceptable sexual activities, and beliefs about victims of CSA contextualize participants' construction of CSA. This distinction demonstrates the forms of CSA that were treated seriously and those disregarded by community members.

Acceptable Sexual Activities

In the FGDs, participants were asked about their views on what constituted CSA in their community. A differentiation was made between acceptable and non-acceptable sexual activities between adults and children. The sub-themes that emerged under this theme are consensual sexual activities, non-penetrative sexual activities, and transactional sexual activities with children.

Consensual Sexual Activities

The majority of the participants stated that sexual activities that children consented to could not be regarded as abuse. Such activities according to some of the participants did not receive negative reactions from community members. Some of the research participants believed adults who engaged in such activities with children could not be faulted, since the children consented to the act. For example, it resonates in the following responses:

It is normal for girls to allow their lovers to have sex with them. This mostly happens when the child's family is poor. Such lovers provide for the child's fam-

ily and ensure that the child's needs are met. Have we all not had younger lovers before? Yes, such relationships are normal because the man does not force the child. They both agree to have sex and there is nothing wrong with that. [Male participant, FGD, Teshie]

When a grown man has a *Joley* [lover] who is a child and he decides to have sex with her and she does not prevent him from doing so, it is accepted in this community. Nobody will blame the man for forcing to have sex with a child when she permits. Even if the girl gets pregnant and the man decides to marry the child, there is nothing wrong with it. This is because the child will be protected from stigma. If a child gets pregnant and the man decides not to marry her, it is a disgrace to the child's family. [Male participant, FGD, La]

The truth is that having sex with our beautiful girls is very common in this community. There is no man in this town who is not guilty of this. Once in a while, you want the young ones. If the child also wants it, that is good because you will not be blamed for having sex with a child. Nobody will say it is abuse because you did not force the child to have sex with you. It is a normal practice for one to be able to have fun with a lover. There is nothing wrong with it at all. [Male participant, FGD, Teshie]

According to the participants, having a *Joley* (lover) was an acceptable norm in the Ga community and, as a result, adults who were in intimate sexual relationships with children were not faulted. *Joley* is a Ga term for an intimate partner; two people who engage in an intimate relationship refer to each other as the *Joley*. It was not out of place for children as

young as 11 years old to have a *Joley*. The main motive behind having a *Joley* is sexual pleasure, with financial gain as a secondary objective. Parents and other persons in the community who get to know about such relationships do not frown upon them, perhaps due to the benefits derived from such an association. This is because the respondents believed such relationships were consensual ones, which benefited both parties. In this form of relationship, just like any other intimate sexual engagement, children could give consent to their partners for any sexual activity they deemed necessary. Most respondents regarded this as a form of pleasure rather than as abuse. This means in situations where children are lured by adults to consent to sexual activities with them, the offenders are not found guilty of the offence. Children who fall prey to this form of sexual activity are likely to keep their experiences to themselves for fear of society's backlash.

Non-Penetrative Sexual Activities

Some of the research participants disclosed that when adults fondled children by touching or sucking their breasts, kissing, or touching them in other sexual manners that did not involve sexual intercourse were not regarded as serious sexual offences. Almost all the participants who held this perception agreed that although sexual activities are not proper, they are pardonable because they do not harm the child in any way. In the words of this participant:

It is normal for a man to touch a child's breasts or buttocks, this is not so good, but it is normal. Yes, kissing too. The child would not be hurt in any way so it is not so bad. It is part of the growing process of

a child. You can playfully touch the child's breasts and run away...it is normal. These are games we play in the neighborhood and there is nothing wrong with it. Ask every adult in this community and find out if they never played such games when they were growing up. It is to initiate a child into adulthood. [Female Participant, FGD, La]

Another participant stated that:

There is no man in this town that does not do some of these things [smiles]. Once in a while, you want the young ones to play with. Touching their breasts or buttocks is nothing serious. It is a good feeling to the adult and interesting for the child too, I suppose. This can never be abuse because the child's womb will not be destroyed, it is harmless. [Male participant, FGD, Teshie]

A number of participants believed non-penetrative sexual activities with children did not qualify as abuse, since they did not pose any threat or harm to the child. This was best described as "playing with the child" instead of abuse. For example, according to this participant:

Oh...we all play with the girls sometimes. We hold their breasts and sometimes hit their buttocks. When they are young, they look very fresh and you are forced to touch them to know how it feels. There is nothing wrong with that. We all play with them and nobody would say you have raped the child because you are just having fun. [Male participant, FGD, Teshie]

The unwillingness of community members to see these behaviors as CSA can make its disclosure

very difficult for children. This is because no action would be taken against the perpetrators. Perpetrators would therefore see it as normal to engage in non-penetrative sexual activities with children such as touching and sucking their breasts, touching their buttocks, among others. These sexual behaviors have serious implications on the development of the child, just as it is with penetrative sexual activities. However, a number of the participants in this study stated that most adults in the community engage in non-penetrative sexual activities as a form of fun and not to harm the child. This behavior makes it difficult for children who are sexually abused through this means to be able to report their abuse and seek assistance.

Transactional Sexual Activities

Transactional sex in most communities, including that of the Ga, usually takes the form of “sugar daddy” relationships where the men are older than the girls and economically stable enough to be able to provide for them (Luke and Kurz 2002). Although girls are usually the victims of such relationships, boys also get involved in them. The findings of the study showed that it was a common practice for children to engage in sexual relationships with adults in order for their needs to be met. This is seen as normal by some participants in this study who believe most families find it impossible to meet the needs of their children. The majority of respondents stated that both the child and the adult who got involved in such a relationship benefited from it. The child mostly derived monetary benefit from the relationship, while the man obtained sexual pleasure. Adults who engaged in transactional sex with chil-

dren were not seen as abusers. For instance, these participants stated that:

Most children in this community engage in sex with older men as a source of survival. Young girls sometimes seduce men to have sex with them in exchange for money. It is a life style in this community because most people are poor. Parents sometimes force their children to engage in such activities. How can you blame a man who provides money for a needy child in exchange for sex? It is a business and the children understand what they are doing. [Male Participant, FGD, James Town]

Children who got involved in this kind of activity were perceived as persons who understood the consequences of their actions. For instance, a participant stated that:

When I was 17 years old, I had a girlfriend who was 12 years; I was having sex with her. Her family members saw it as normal and did not have any problem with it because I was taking good care of her. I was working by then. I used to give her money and pay her school fees. Her parents were very poor so they could not take care of the girl. [Male participant, FGD, La]

Some of the participants stated that, irrespective of the age of a child, it was not frowned on for children to engage in transactional sex with adults in the Ga community. This is because these activities are said to benefit both the child and the adult. The child gets monetary benefits, while the adult’s sexual needs are met. These children are usually manipulated by adults, including their parents to exchange their bodies for survival needs, and can

be considered as victims instead of persons who understand the implications of their actions.

Non-Acceptable Sexual Activities

From the perspective of the participants, what they consider as CSA are non-acceptable sexual activities with children. This is described under three sub-themes: sexual activities between adults and children which resulted in physical injuries, sexual activities between persons of the same sex and children with a wide age gap.

Physical Injuries

Some of the research participants in this study noted that an important element that was considered in regarding a sexual activity with a child as abuse was an obvious physical injury inflicted on a child after abuse. One participant, for instance, explains that:

When you go to the hospital and you are told *aplagbeken* [the child is injured], you have no choice than to punish the perpetrator. When the child's health is affected, there is nothing you can do but to report to the police. It is about the child's life so the one who did that would have to be punished because it is abuse. The child's life is destroyed and the effect will be with her forever. This is abuse and the perpetrator will have to be punished. [Female participant, FGD, La]

Similarly, in the words of another participant,

When the child's womb is destroyed, you have to do something...you have to let the person know that he is wicked and a fool. The person would have to be

jailed for life! Sometimes they even infect the child with HIV; the shock alone would let you do something serious to the person who did that to the child because the child has been abused. [Male participant, FGD, La]

It was mentioned that any sexual abuse where the child's health was seriously affected attracted sanctions from the community or was reported to the police. For instance, a situation where medical reports indicated that a child's womb was destroyed or that such a victim sustained serious physical injuries or was infected with HIV caused an outcry among community members. In such situations, every effort was made for the perpetrator to be punished. It was also realized that such persons usually fled the community for fear of being beaten or hurt by an angry mob before being sanctioned.

Same Sex Sexual Relations

A sexual relationship between two females was referred to as *supi*, while that between males was known as *kojobesia* in the Ga setting. Adults who engaged in such relationships with children were harshly treated and punished severely in the community. Some participants in the focus group discussions assert that:

Oh, it is not good for a woman to have a girl child as her *supi* [lesbian]. Yes, *supi* and *kojobesia* are very bad and it is not normal in this community. *Supi* and *kojobesia* are a curse. We need to really pray for this community. It would destroy us if we do not stop it. Adults who engage in sexual activities with children of same sex can be arrested and punished because it

is seen as sexual abuse in this community. The child will grow up engaging in such evil acts. [Female participant, FGD, James Town]

When men engage in sexual activities with boys, it is a curse. People get surprised why older men do that to the little boys in this community. They are training them to become *kojobesia*. Men and boys having sex is abnormal, just like women and girls. In this town, when you are caught, the youth can beat you and even drive you out of this community because you are seen as evil. [Female participant, FGD, Teshie]

It is *supi* which is bad. The old women who have girls as *supis* should stop. This is not good at all. It is a spirit. Why? Something should be done about it. I just do not understand it. It is not normal...hmmm. [Male participant, FGD, La]

My daughter, it is the *supi* oh...the spirit of *supi* has come to live with us here in La. The elderly women are going in for small girls to do their evil things for them. *Supi* is the vilest activity in this community. The women who have money are destroying our children...this is evil. When this happens, everybody will say it is child abuse. [Female participant, FGD, La]

Some of the participants stated that adults who engaged in sexual relations with children of the same sex brought a curse on themselves, the child, and the community as a whole. They mentioned that adults who are found culpable of such offences receive instant justice through mob action as they are beaten and forced to leave the community. Others believed offenders sometimes receive natural justice through the ancestors as they are struck dead on

some occasions. Spiritual meanings were often read into such behavior as participants believed it was not normal. It was also noted that adult offenders, as well as children who were involved in such acts were made to go through purification rites in order to cleanse themselves of the curse.

Wide Age Gap

Sexual relations between adults and children where the age difference was relatively wide were also regarded as sexual abuse according to some participants in this study. The norm in most cultures in Ghana, where men are permitted to marry younger women, makes it acceptable for sexual relations to take place between older men and younger children. The Ga communities do not frown upon such sexual relations, especially with the consent of the child. Based on the findings of the study, it is regarded as abuse only when the age gap between the two is relatively wide. A number of research participants stated that they find it very difficult to come to terms with adults who had sexual intercourse with infants or children as young as five years old. According to these participants:

When the age gap is too wide it is crazy. The man can kill the child in the act. How can a 70-year-old man have sex with a 4-year-old girl? It is wickedness. You do not need anyone to inform you that this is abuse. Adults who do that have a big problem and they need to be cured. [Male participant, FGD, Teshie]

Ah...no adult in his right senses would have sex with a baby or a toddler. If somebody does that to my child, I would kill the person because he does

not deserve to live. Even if it is not my child and it happens to someone else, the whole community will discipline the person. We call this child abuse and it is a serious issue in this community. [Female Participant, FGD, La]

Some held the view that such adults were not fit to live in their communities as their behavior amounted to killing or destroying the lives of the children. This was regarded as a serious form of CSA which the Ga society frowns upon. Acceptable and non-acceptable sexual behaviors in the Ga community, according to the participants, form the basis of the social construction of CSA in the communities studied.

Discussion and Conclusion

The main objective of the study was to explore the social constructions of CSA in the Ga community in Ghana and to interrogate how this perpetuates the incidence of CSA. A number of themes emerged from the focus group discussions highlighting the constructions of CSA in the Ga community. The results have shown that the constructions of CSA in the communities studied are embedded in two main themes: acceptable and non-acceptable sexual behaviors. These themes demonstrate how sexually abused children are victimized in the community studied.

Acceptable Sexual Behaviors

Although some argue that all forms of CSA are for the sexual gratification of older persons and affect the development of children (Johnson 2004), this

view is not accepted in all situations in the Ga community according to the findings of this study. Acceptable sexual behaviors are sexual activities between adults and children that individuals in the Ga community did not regard as serious; hence, were viewed as normal and not considered as CSA. These behaviors include consensual sexual relations between adults and the children, non-penetrative sexual activities, and transactional sexual activities with children.

Consensual sexual activities between adults and children in the communities studied did not receive negative reactions from community members because they were regarded as normal. The World Health Organization regards CSA as any sexual activity between adults and children in which the child is not developed enough to give informed consent (WHO 1999; 2004). This is also captured in the laws of Ghana which state that a child below 16 years is not qualified to give consent for any sexual activity with an adult (The Criminal Code Amendment Act [554] 1998). This suggests that any consent given by children who are below 16 years of age for sexual activities is regarded as illegal in the laws of Ghana. Yet, in the Ga community, this is not the case for most people. Thus, an emerging issue in this regard is the clash between modern law and culture. The statutory laws which aim at protecting children from their sexual abuse are not put into practice by most people in the Ga community. Although the laws of Ghana criminalize children giving consent for sexual activities, this did not reflect in the behaviors of people in the communities studied. This is illustrated in their attitude towards the concept of *Joley*.

Respondents stated that the concept of *Joley* (having a lover) was an acceptable norm in the Ga community and, as a result, adults who were in intimate sexual relationships with children were not faulted. This is because the respondents believed such relationships were consensual ones which benefited both parties. In this form of relationship, just like any other intimate sexual engagement, children could give consent to their partners for any sexual activity they deemed necessary. Most respondents regarded this as a form of pleasure rather than as abuse.

Participants also disclosed that non-penetrative sexual activities such as fondling, touching, or sucking older children's breasts, kissing or touching them in other sexual manners that did not involve sexual intercourse were not considered serious offences in the communities studied. Most participants agreed that although these behaviors are not proper, they are regarded as normal because they do not harm the child in any way. These forms of contact are often regarded as an expression of power relations in which the body is used as a sexual object (Inter-Parliamentary Union 2009). Nevertheless, these non-penetrative sexual acts are viewed among the Ga as not serious enough to receive any negative reaction from community members.

Transactional sex was also regarded as an acceptable sexual activity in most of the communities studied. The Ga community as seen from the study is made up of persons of low socio-economic background. Parents and guardians mostly engage in low earning occupations, a situation which makes it difficult for them to be able to cater for the needs of their

children. It is therefore a common practice to see children from such backgrounds engaging in sex in exchange for money and other favors. Children who got involved in this kind of activity were perceived as persons who understood the consequences of their actions.

The acceptable sexual behaviors as defined by the Ga community illustrate "the incongruity between its [CSA] common incidence and the inability of people to talk about it amounts to a monstrous denial of child sex abuse, but such a denial is an entrenched norm" (Ronai 1995:418). This culture of silence according to Ronai (1995) isolates victims of CSA from opportunities that would make it possible for them to share their experiences with others. The only option available for these victims of CSA is for them to accept the meanings and labels placed on them by their abusers (Ronai 1995). Children in the Ga community who experience forms of sexual abuse described as acceptable are compelled to be silent about their situation. These children will believe they are deviants instead of being victims (Ronai 1995).

Non-Acceptable Sexual Behaviors

The findings of this research suggest three forms of sexual behaviors that were considered abnormal in the Ga community. Sexual activities with children that were regarded as abnormal received some attention from community members. These are sexual activities that resulted in physical injury of the child, sexual activities between persons of the same sex, and sexual relations between adults (usually males) and children with a wide age gap.

Physical Injury

It was mentioned that any sexual abuse where the child's health was seriously affected attracted sanctions from the community or was reported to the police. For instance, a situation where medical reports indicated that a child's womb was destroyed or that such a victim sustained serious physical injuries or was infected with HIV caused an outcry among community members. In such situations, every effort was made for the perpetrator to be punished. It was also realized that such persons usually fled the community for fear of being beaten or hurt by an angry mob before being sanctioned. CSA has been identified as having serious immediate and long-term physical and health effects on children. The short-term physical effects of CSA have been recognized as bruises, wounds, or fractures, and infections including HIV (Zambia Ministry of Education 2011). The long-term effects, on the other hand, include chronic pelvic pain, painful menstruation, pelvic inflammatory disease, irregular vaginal bleeding, and complications with child birth (Heise and Garcia-Moreno 2002). Physical injuries caused to a child through CSA were therefore regarded as a serious crime in the Ga community and offenders are punished.

Same Sex

Studies show that same sex relationships are not accepted in most African settings, but viewed as a taboo and un-African (Morgan and Wieringa 2005). According to Morgan and Wieringa (2005),

persons involved in same sex relationships often lived in secrecy due to the stigma associated with their practice. A recent report in the media states that suspected gays and lesbians at a birthday party celebration in James Town were attacked by a mob with bottles and cutlasses and their houses destroyed (see: myjoyonline.com). The implication of this on children who are sexually abused by adults of the same sex can have their experiences shrouded in silence and secrecy for fear that society will be prejudiced against them. It is, therefore, difficult for abused children in this category to receive help from their communities, if any exists.

Wide Age Gap

The norm in most cultures in Ghana, where men are permitted to marry younger women (Azu 1974), makes it acceptable for sexual relations to take place between older men and younger children. The Ga communities do not frown upon such sexual relations, especially with the consent of the child. Based on the findings of the study, it is regarded as abnormal only when the age gap between the two is relatively wide. The study showed that it was difficult to come to terms with adults who had sexual intercourse with infants or children as young as five years of age. Such acts were regarded as child rape which mostly involves injury to the child. This form of sexual act is said to traumatize a child who sometimes displays abnormal behavior. An adult who commits such an act is believed to deserve punishment in the Ga community.

Implications for Policy

Although this study has some limitations, the findings of this study have some implications to policy. Firstly, the study shows that among the Ga, socio-cultural factors, more than the legal definition, influence their constructions of CSA. As a result, adult members in the Ga community are likely to abuse children sexually when the laws are not enforced. Therefore, it is imperative on the part of the government to enforce and strengthen the laws that seek to protect children from sexual abuse in the Ga community. Additionally, community sensitization programs should be organized periodically by the Department of Social Welfare, the DOVVSU of the Ghana Police Service, and the Commission on Human Rights and Administrative Justice (CHRAJ) in collaboration with local authorities to educate community members on the legal stance of CSA. The media should also be given proper training on how to report cases of CSA, since this is a major avenue where perceptions about the phenomenon are formed.

Limitations and Directions for Future Research

The strength of this study is in its ability to allow the voices of the community members to share their realities of CSA in their communities. However, the study has some limitations. The small sample size, which is typical of qualitative studies, makes it difficult to generalize the findings of the study to the whole of the Ga commu-

nity. The study is also confined to one region of Ghana. It is recommended that comprehensive study similar to this one be carried out across different socio-cultural settings to present diverse views on the social constructions of CSA in Ghana.

Conclusion

This study has increased our understanding of the social constructions of CSA and described the extent to which these cultural beliefs perpetuate the cycle of CSA in the Ga community. Acceptable sexual activities and non-acceptable sexual activities between adults and children defined the social constructions of CSA in the communities studied. These distinctions provide an avenue for sexually abused children to internalize beliefs that blame them for their abuse. Victims of CSA are therefore perceived as not living up to the standards society has set for them instead of punishing the offenders (Tetteh and Markwei 2018). The implication of this is that these victims do not receive social support to deal with their traumatic experience. The media narratives and (mis)representations of CSA can influence public perceptions, attitudes, and actions that people have towards the problem. Hence, the constructions of CSA in the Ga community in modern times are largely influenced by its representation in the media. As a result, the media can play a key role in correcting the misconceptions about CSA through proper reporting. A clear understanding of CSA will minimize its occurrence and also provide effective support to its victims.

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Religion and Culture of Origin. Re-Shaping Identity in the Integration Process: A Case Study in Sicily

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Abstract What happens when people of different cultures, values, religion live together? Sociological studies on immigrative phenomenon often swing between immigration and integration policies. These policies actually reveal the difficulty of the host society to institutionalize new models of social differences accompanying multiculturalism. Immigrants who “arrive” continue their life in a place where they do not passively participate in the passing of time, but become actors. Pressed by the hegemonic culture of the host society to adapt, do not cease to practice their religious and origin cultural expressions, often in conditions of urban spatial and social marginalization, they resist assimilation with ethnic persistence strategies. Considering the impact of religion and origin cultural values on expression of differences, it is important to consider their role in the integration process. And, above all—facilitate or hinder integration? These dynamics have been analyzed in a research study on immigrants’ integration process in Palermo. The main results are presented in this paper. In this case study, the research’s data hypothesizes a theoretical model of integration in which immigrants, free to express their religious and cultural differences, tend to reduce their perception of minority.

Keywords Integration; Religion; Identity; Multiculturalism; Adaptation; Assimilation

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International migrations produce profound social changes in receiving societies. In varying contexts of reception, immigration is followed predictably by immigrants' adaptation efforts towards culture of receiving society. Their modes of integration may take a variety of forms within the society. The possible outcomes (welcoming, indifference, or hostility) are shaped by the complex interaction of the contexts of incorporation, into which the immigrants are received by society, with the resources and adaptations of immigrants (Portes and Rumbaut 2014). The increasing number of immigrants in Europe and the recent terrorist attacks gave rise to fear and insecurity. The public debate frequently focuses on the role of religion and cultural values of immigrants as main barriers to immigrant integration in Europe, impossible to reconcile with Western lifestyle. It shows that different modes in living religiosity can explain immigrants' integration process. The main European integration models are in decline or in crisis (Vertovec and Wessendorf 2010) right at a time when political rhetoric revolves around the contradictions of emergency actions. That is because the *integration policies* have pushed immigrants, uprooted by choice or necessity from their context of origin, to live on the cultural and physical margins of the host societies. These policies reveal the difficulty of our societies in institutionalizing new patterns of social differences associated with multiculturalism. On the one hand, they protected the cultural hegemony of the host society, on the other hand—they underestimated the importance of the expression of minority differences, pushing ethnic specificities to a convergence of immigrants' values and culture towards the local ones. The immi-

grant who "arrives" continues his/her life in a place where he/she does not passively participate in the passing of time, but he/she becomes an actor. Immigrant communities populate places where they work, consume, meet. They do not stop practicing their religious and cultural expressions of origin. The debate on the role of the religious dimension in the cultural identity processes of the societies is complicated by the immigration flows and the effects of assimilation policies. In some cases, religion is a factor of irreducible difference, in others, it is a factor of integration. As to the construction of identity processes (Seidler 1986; Cochran 1998), the central question concerns: How can religion contribute to the debate about identity? This implies a series of considerations on issues related to the collective identity (Leege and Kellstedt 1993). How to permit the respect of ethnic traditions that are intolerable in the West? How much tolerance are Western societies able to activate to facilitate integration processes? This question underlies another: How much freedom are Western societies willing to grant to the public practices of immigrants which are different from their own? Until now, their response has been contradictory: practices are tolerated as long as they do not violate our laws. This article presents results from a research carried out in Palermo, a city of South Italy where immigrants' integration process has effects on identities of ethnic communities who are self-perceived and are perceived by natives as "different" for language, religion, culture. The analysis of the research data is focused on a case study from which to propose a general theory about immigrants' integration in which immigrants tend to assimilate easier and tend to lean towards faster integration

when: 1) there are wide spaces in the expression of religious and cultural differences of origin, 2) integration policies facilitate citizenship rights, from work to the regularization of legal status. The case study presented shows this.

Research Methodology and Theoretical Context

The research aims at analyzing the role of religion and culture of origin in the process of integration of immigrants with the local community, in which the independent variable is the degree of freedom enjoyed by immigrants in the public expression of ethnic differences. This case study integrates qualitative data and ethnographic observation. The approach adapted to develop the research pattern is the grounded theory of Glaser and Strauss (1974). The non-participant observation and statistical data used complete the description of the investigated scenarios. The data collection was carried out in 2016, through 60 interviews to immigrants residing in Palermo, grouped by age, gender, and ethnicity. The ages of respondents ranges between 20 and 60 years. All Maghrebians interviewed are Muslims, as are the members of the Bangladeshi community. Those from Eastern Europe are almost all Orthodox Christian. The sub-Saharan ethnic groups mainly belong to Evangelist, Catholic, and Muslim cults. The Tamil interviewed are evenly distributed between the Hindu and Catholic faiths.

Using a semi-structured format, the interviews investigated: biographical paths; faith and religious values; public and private daily practices; cultural and religious practices. The analysis examined

moreover the reasons for the choice of country of emigration, and the experiences of subjective identity in the spheres of religion. For each of these areas, analyzed through content analysis. In the analysis of the data, a multi-theoretical framework was used to broaden the perspectives of understanding the phenomena. Among these the theory of the status passage (Glaser and Strauss 1971), in which society is considered as a network of dynamic and changing relationships, pushing to reflect on the new representations and configurations in which the social decomposes and recomposes. The original theoretical datum of this research, I believe, is to add the frame of the status passage of immigrants, which in the original analysis of emergent theory of status passage was undersized. The model of status passage resulted particularly useful in redefining identity in acculturation processes, but above all in those of integration where real status changes happen. In these cases, the social actors involved are: the immigrant as the passagee (the one who is involved in the passage); the community of the natives as agent (implicated in conferring the shape of the passage and its control). The relations between the two poles take place in the arena (or context) of the host society of Palermo. The effects of relationships determine reciprocally effects on new identity and structural configurations, both for the immigrants and the native community. In the case of immigrants, the transition from irregulars, aliens, guests, to integrated individuals occurs, but in a segmented way of the different areas of sociality, work, identity. In the case of the native community, for the new configurations of multiethnic society that must reorganize the integration policies in order to respond to unpublished rights and duties social issues.

In the following analysis, I report extracts of the interviews I consider most relevant to the description and understanding of the phenomena investigated. The theoretical perspective of the “differential adaptation” (De La Garza and Ono 2015) has qualitatively outlined the analysis of the innovation/ethnic-persistence dialectics. The decision to refer to ethnicity rather than nationality was due to the sociological relevance of the factors that constitute and distinguish ethnic groups: a name, myths of descendance, a shared history, a shared culture, a sense of solidarity among the members, and the reference to a territory, even if it is different from that in which you live (Smith 1998:52). These factors define the differences among individuals of the research’ target population. The research excluded native Palermians, choosing to focus on the perception of difference by immigrants. That is, on those who express their identity through distinctive factors in a foreign world.

Background research gave shape to the hypothesis that to greater freedom enjoyed by immigrants in the expression of public differences would correspond the reduction in their being perceived as a cultural minority in multicultural contexts where adaptation is declined through syncretism, cultural contagions, and assimilation. I have defined also the field of study using hypotheses and theories that in literature have provided significant answers on the issues in question, starting from the definition of integration. Defining integration is complicated. It does not mean to circumscribe the semantic field, but to give it a political intention and a social value. Integration is the daily experience of meeting between different entities. It is a process that

should ideally lead to a high level of social cohesion and reciprocity. About integration as path, scholars (Penninx, Spencer, and Van Hear 2008) seem to agree that in determining the degree of integration of immigrants, the focus should be placed on the importance of subjective and institutional variables (work, legal status, local immigration policies, etc.). Penninx and Martiniello (2007) define integration as *the process of becoming an accepted part of society*. In this definition, the final outcome is desired, but not certain. In immigration research, the use of the term is alternated with programmatic definitions for new analysis. For example, integration and assimilation are terms often used interchangeably. In Alba and Nee (1997), the concept of assimilation is used less and less because of the disappearance in immigration discourse of “ethnic” and “racial” to define identity characteristics. Today these distinctive elements tend to be conveyed in a more politically correct concept of “cultural differences.” Adaptation and assimilation are terms that seem to chase each, when trying to define an immigrant’s integration process when the end goal is the similarity with the local population.

Some ethnic groups (Amish, Jews, etc.) emphasize religion as the core determinant of identity (Gordis and Ben-Horin 1991). Others use religion as a means to preserve cultural traditions and ethnic boundaries (Yang and Ebaugh 2001). This is to say that religious fervor in immigrant communities serves two purposes: first, as an actual devotion to the religious symbols and rituals; secondly, to create a cultural support system for immigrants filtering external influences of mainstream local culture in order to preserve communal solidarity

in an ethnic immigrant community (Christ 2016). Many scholars seem to converge on the idea that religion is an important component in collective experiences of migrants (Hagan and Ebaugh 2003), from the decision to leave to the support in settlement process. According to Hirschman (2004), migrants' paths are accompanied by religion that offers functions of: refuge, respect, resources. In this model, the analyzed experiences hypothesize how religion, places of religious expression (churches, mosques, temples), and the relative institutions provide, in addition to spiritual support, various types of resources (information for housing or work, language courses) that allow immigrants to face many of the problems encountered in the process of settlement in an alien community. More complex is the model of Portes and DeWind (2004) which describes the different roles played by religions in the immigrants' integration processes. It is in this model that religion is a point of support for the processes of selective acculturation (Portes and Rumbaut 2001). Moreover, "religion plays a crucial role in the construction of identity, in the reproduction of meanings and in the formation of values" (Levitt 2003:251). This function is particularly important in the alienating experience of the migrant, that far from the original context in which everything was familiar, usual, ordered, predictable tackles pressing existential and identity questions. Separated from many aspects of previous life, immigrants often cling to religion as an element of continuity and connection to their own world.

In conclusion, at a time when the migration experience changes the connotations of collective iden-

tity, culture, and religious values are a set of traditions and granite rituals that individuals express in reality. Culture and values are not immutable. The dynamic nature of their interweaving reproduces and recreates in social interactions, points of contact, and balance with the social reality, faced for the first time. According to Bruce (1992), this interpretation underestimates the deepest convictions that guide individuals in shaping a sense of belonging to the one's own community, in relation to religion. Two examples of this hypothesis, which in the research were analyzed in a comparative key, are: the Islamic *ummah* and Hindu *dharma*. The comparative focus on these communities depends on the fact that in the first case, Islam is the second religion professed in Italy. In the second, the Hindus in their path of acculturation are the protagonists of a religious syncretism.

The General Scenario

Palermo is Sicily's administrative capital and one of the largest cities in South Italy. It is a city with a strong multi-ethnic historical, architectural, and religious identity. Today Palermo is one of the main landing and reception points of immigrants rescued through humanitarian missions in the Mediterranean Sea. The vestiges of ancient Arab, Norman, Byzantine, Angevin, and Spanish colonizations are present in its urban structure. Palermo hosts a Grand Mosque. Recently the Catholic Church has allowed the use of a church to the "Jewish brothers," for their religious activities. Palermo is a city in the midst of a great transformation, in which changes are driven by events both economic and cultural, but also by forms of mul-

tiethnic contagion influenced by a mix of global and local forces, that can be considered, on the one hand, a logical consequence of economic development (thanks, for example, to the entrepreneurial vitality of immigrants, which benefits the local wealth and employment rate); on the other hand, these forces also contribute to cultural and anthropological changes, because of all forms of *reverse multiculturalism* adapted by natives thanks to the presence of immigrants. In this form of multiculturalism, the native community integrates with the immigrant community. The latest statistics¹ (2016) reported a total foreign population of about 36,000 foreigners registered at the office of resident population, making up approximately 4% of the total resident population, with a growing annual trend of 2%. Palermo can be thought of as a multi-ethnic puzzle containing at least 128 different ethnic groups, if one considers, for example, that the Mauritian community in Palermo is comprised of 4 different ethnic groups (Hindu, Tamil, Telegu, and Marahati).

In general, the employment sectors in which immigrants from Southeast Asian countries and Eastern Europe find their place in Palermo are the private care (domestic workers and caregivers). Commerce for those coming from the countries of the Maghreb. The restaurant sector is “occupied” mainly by the Chinese. The residential distribution of immigrants in Palermo over the last decade is primarily a result of rental costs. For many years, the historical center of the city was the primary residential neighborhood of immigrants. Today, immigrants

are distributed throughout the city, except in the wealthiest neighborhoods. Research has highlighted an important reversal in the trend of the period of residence of immigrants in the Sicilian capital. In the studies conducted by Cole and Booth (2006:26), Palermo was known among migrants as a place one could live and work undisturbed even without a residence permit, before dispersing to other places as soon as the permit was issued. Today, there is an increase in the number of immigrants residing in Palermo who do not want to leave, because they have a job and are joined by their families. From the political administrative point of view, freedom enjoyed by immigrants is high, above all in relation to that of immigrants resident in other Italian cities, where various municipal ordinances intended to exclude immigrants from the fruition of local welfare benefits, limiting working practices (such as sales of ethnic foods or street trading in the city center), banning religious symbols and practices. These *local policies of exclusion* (Ambrosini 2014) in Palermo have been totally absent. Indeed, in 2015, in the midst of an unprecedented flow of migrants, Palermo’s administration has drafted and signed the “Carta di Palermo.” It is a statement of intent that conceives *the right to mobility as a right to the human person* and dreams of *a citizenship of residence*. The paper wants to give answers to: How do immigration and security work together? Among the proposals of the document, one is in sharp contrast to the current national and European immigration laws: the abolition of the residence permit. The Mayor of Palermo, L. Orlando, promoted and supported this initiative declaring that whoever arrives in Palermo is Palermitan.

¹ See: <http://osservatoriomigrazioni.org/>. Retrieved July 04, 2019.

The Biographical Path of Immigrants: The Reasons for the Choice of Country of Emigration

Often there are stories of those who flee from violence and poverty, caught among hope, illusion, and disillusion. The interviewees explain why they chose Palermo and Italy as their destination. The interviewees reveal that these places seem to have a strong appeal for some categories of immigrants. In many cases, Italy and Palermo are chosen because there is already a relative or an acquaintance who lives there, which can facilitate reception and integration. In this case, the bridging as social capital is a decisive factor in the choice of destination (Portes 1985) because through the social capital networks they enjoy information, imitative stimuli and logistic resources for the reception, accommodation, and help in finding work when they arrive. The religious factor of origin is another of the reasons for choosing Palermo and Italy, especially for Catholic immigrants, who do not seem to face the differences in religious values related to lifestyle.

Indian woman, 23: I came directly from India to Palermo. I decided to join my mother who had moved to Palermo for business reasons. Both for me and for my mother, the choice of Palermo was due to the presence of my aunt, and thanks to her we had the opportunity to be hosted immediately and quickly acclimatize.

Vietnamese woman, 37: I have been in Italy for over 15 years. I immediately came to live in Palermo. My sister was already living here, and so I decided to come to find a job in Italy. I chose Italy because Italy is the seat of the Vatican and of the Catholic Church.

And since I am Catholic, I wanted to feel at home. I have an Italian baptismal name given to me in Vietnam.

In other cases, experiences of emigration in other contexts in Northern Italy generate transfer decisions to the South where the difficulties of acculturation are fewer, the labor market is less regulated, and the climate of hostility is lower.

Bengali man, 30: Italy was for me a place where I could have a better future. After a year in Vicenza I decided to come to Palermo because the people of Northern Italy are colder, they are more racist, while in Sicily people are warmer and more willing to accept you.

As regards the decisions to migrate, the interviews not exclusively lead back to subjective initiatives, but family strategies for maximizing income and increase in welfare opportunities for the entire family unit. These strategies usually coincide with the paths of upward social mobility, according to the models of the places of destination. Sometimes these strategies are in open contradiction with the religious values and the family hierarchy of the origin country.

In these cases, it happens that the professional and family role of immigrant ethnic groups, traditionally linked to the restrictions of the roles of women, oscillates between persistence and innovation. As in the case of the extra-domestic work of Muslim and Hindu women. At the same time, the man reorganizes his social and family role also in the domestic context, both in work that he shares with his partner, either with its own limitations in terms of power

and authority, tending towards a greater symmetry with the partner. These role changes in the private sphere are identity bifurcations, turning point, in the biographical path of individuals that renegotiate with themselves and their own cultural world the fidelity values and the cost of identity treason.

Tunisian man, 44, Muslim: Since I have been in Palermo, the life I have with my wife has changed. She works outside the home to earn more. Maybe we were westernized. In fact, I do not feel the landlord. Now there are two.

Interviews indicate that for immigrants from South-east Asia (especially for the Bengalis, the Mauritian, and the Sri Lankans Tamil and Sinhalese), their acculturation process is retained by differentiation of certain characteristics of the Western culture, such as: individualism, stress, or the lack of respect for the environment.

Among these Asian ethnic groups, Tamil and Sinhalese escaped from war and inter-ethnic conflicts. But, it is inevitable that the contact between the two hostile ethnic groups in the country of origin is different in the host city. They have no certainty of the final purpose of their journey, living with nostalgia and fear the return because it would mean going back to what they escaped from. This consciously transitory condition is only ideal because they know that they cannot return to their country of origin at least until a solution to the conflict is reached. This constraint, on the one hand, has accelerated the adaptation process; on the other hand, it has generated feelings of strong attachment to one's own land and to traditions even in a foreign land, in Palermo,

where is present the biggest Tamil community in Italy, active both on the political and religious level and for the maintenance of cultural traditions.

The immigration path is different for Mauritian, for whom the idea of the project is mainly economic. For them the permanence in Palermo is linked to the improvement of their economic condition. They are the most available to humble and tiring jobs, to cohabitation with compatriots to maximize savings. That is because their goal is the return and the pause is temporary. The habit of ethnic and religious multiculturalism in the country of origin is a cultural factor that facilitates socialization and the maintenance of one's own identity.

Mauritian man: I come from Mauritius, which have always been multicultural islands where all live in peace and we know how to live with different cultures. And the same I did here in Palermo.

Faith and Religious Values. Re-Shaping Identity

Religion and values of origin seem to constitute strong elements of guidance to action. Such behaviors (both the prohibited and permitted) do not appear to be factors impeding integration. In fact, they are actual spiritual and emotional supports that help immigrants counterbalance the cultural stress of being uprooted. The analyzed data lead to confirm the value of religion for immigrants: a) as a medium to transmit values and cultural elements; b) as a spiritual, material, and social resource, that intervenes at different stages of the migration process, as shown by Ambrosini (2007). The

analyzed data showed that for some categories of immigrants the universe of values coincides with that of the religious sphere. There is an overlap in these dimensions, especially in the older cohorts of men and women of North African and Southeast Asian Muslims. Moral values related to good and evil continue to run parallel to what is permitted or forbidden by their faith, especially in the private sphere. Religious prohibitions are expressions of ethnic values onto which difference is radicalized. The clothing of Western women is judged by such immigrants as proof of the sexual objectification of women and the devaluation of their moral integrity.

Moroccan Muslim man, 30: I do not like that here the woman walks naked, wears her skirts too short. It is a provocation to us, men. The women in my country cannot go out uncovered, and they can only go outside if they are accompanied.

Bangladeshi Muslim man, married, 47: In Palermo, women are too free, they dress uncovered and are more social with men: for example, they kiss to say goodbye. I do not accept that my wife goes to work alone, or that she behaves and dresses like the Italian women. My daughter will be free to choose whom to marry, but the important thing is that he is a man who works, is honest, and Muslim.

Data also show that younger cohorts, more educated, and more recently come to reside in Palermo, exhibit a critical attitude towards traditional values, in regards to behavior in both the public and private spheres. This includes the cases of premarital sex, abortion, or homosexuality.

Ghanaian man, Muslim, 25: Although my religion would lead me to express another kind of thought, I believe that we are all now modern. So I consider prohibition of sex before marriage to be an outdated concept. About homosexuality: I have nothing against homosexuals.

In these cases, a high level of education seems to activate in people a critical reflection of the complex relationship between daily reality and the secularization of values. In my opinion, cultural adaptation is the deciding factor in the change of heart among these individuals who have been exposed to confusion and disorder in the course of a life that has lost its linearity, from country of origin to country of arrival, and which required an adaptation process forged through the autonomy of choices. But, behind the critical opinions of traditional values and their role in individual actions, there are more complex reasons based on the context in which one lives. In the construction of one's own life path, choices are made according to the pressures of one's environment.

Moroccan man, muslim, 35: Marrying an Italian woman whose religion and culture are different from mine would not be a problem; in fact, I'd like it, precisely because they have a freer mindset and culture. But, I would marry her only if she was ready to become a Muslim. I cannot change my religion. Since I came to Palermo, I haven't practiced my religion much. However, in my country, I follow all the religious precepts, such as in the case of divorce. Divorce is always a bad thing, although in the case of a divorce, only the man can ask it. Women can't allow themselves to, as the Qur'an states, and I agree.

In some communities, the culture of origin prevents the activation of perspectives of re-socialization that would be able to generate new identities. In terms of identity, the immigrant that transitions between past and present, it is not yet, and it is not anymore, sliding among roles and moving among different statuses.

The Mauritian community (mainly Hindu) in Palermo is a perfect example: Seemingly homogeneous in terms of religion and culture, after a first period in which the Mauritian immigrants face the typical crisis of cultural adaptation and occupational integration. Over time the community has revealed its heterogeneity, especially in regards to particularistic cultural expressions. The professed faiths depend on their immigration. At the time of English colonization, workers were recruited from various parts of India and employed on the island, where there were already other immigrants that other European colonizers had moved from other countries. As a result, this policy inadvertently brought together people of different beliefs, languages, and castes. A social order was created by spreading the identities in which social fates suffered the repercussions of estrangement, exploitation, and forced metissage. Already at that time for the homogeneous communities in a foreign land, religion satisfied the need for a guide to action and belonging, for emotional shelter in one's own group, where one could find customs, rituals, or symbols. But, different groups were completely separated from each other, and each group's members closely observed practices of ethnic self-preservation, such as the prohibition of mixed marriages. The groups are differentiated by their provenance from the Indian subcontinent and from the power

relations that, at the time of independence from England, were concentrated in the Hindu group. Thus, the Hindu, Tamil, Telegu and Marahati, different in background and culture, became and continue to be minorities, even in Palermo, where the Mauritian community has reproduced through religious expressions the distinctions of Indo-Mauritian groups that colonialism contributed to forming.

Public and Private Daily Practices. Between Persistence and Innovation

The traditional theoretical model on the assimilation of immigrants, hypothesized in the Chicago School in the last century (Gordon 1964), seems to express a certain relevance in the present research in which assimilation oscillates between persistence and innovation of the origin's culture of immigrants. In Palermo, data show how assimilation oscillates between attitudes of rejection of the "new culture" and of defense of that of origin. This happens through practices whose expression changes in the public and private sphere. Although the interviews bring back differences of individual experiences, the analysis made it possible to grasp in the different ethnic groups trends of the analyzed practices.

About the adaptation processes, in the analysis, immigrants tend to homogenize on two conceptual poles: an innovative type in which the adaptation consists in the innovation of values, religious practices, clothing, food, et cetera, having incorporated in their cultural sphere new features, harmonizing them with the traditional, with obvious contradictions, but not rejecting them or abandoning them altogether. The second category identifies those who

continue through persistence practices to preserve on foreign soil the cultural origin elements.

Summarily, in the first category, are more present male immigrants of Islamic faith, and long residence Hindu. Among the innovative, the younger immigrants of Sub-Saharan African origin and the Tunisian ones are the most active. The level of education and their inclusion in educational and professional circuits seems to characterize the changes in the *innovatives*. In reality, the difference between the innovative and the retentionist is played on the dimensions of the public-private. In general, those who have an active social role in public tend to greater innovation. In contrast to individuals who instead concentrate the expression of identity in the private and tend to conservation. The hypothesis is that immigrants adapt identity conservation modes when “forced” by the public visibility to distinguish themselves, and be recognized, mainly by their own community.

Research data suggest a kind of multiculturalism in which the identity expressions of communities do not protect the interior space, the one in which a culture is perceived as a homogeneous and pure, but rather they intervene on the intermediate border areas, where, according to Appadurai (2001), the supplement experience is a surplus of meaning. The analyzed interviews explain the methods for adaptation of immigrants, through the adaptation of behavior patterns, roles, power, clothing, Italian culture, and in several cases, more properly those of Palermo.

Mauritian man, 50, Hindu: I called my son Giuseppe; I called him with an Italian name because if we had

given him a typical name of Maurizie, definitely he would not have felt a Palermitan and perhaps it would have also been discriminated because it's strange. Here I and my family eat the beef and then do not follow much the prohibitions that the Hindu religion preaches. Since I came here I learned and I realized that there must be equality between men and women.

The Muslim and Hindu immigrants in the private sphere shall implement conservation in practices of their cultural identity by food, clothing, religious practices. Among these communities, immigrants seem to be adapting conservative identity modalities when “forced” by the visibility in public to be distinguished and recognized, primarily by their own community. If the public dimension of religious differences did not emerge as an identity marker, it does not emerge nor even the difference *tout court* as an evocative political value of a comparison (or antagonism) to be composed on a cultural level. In Palermo, the conservation of foreign culture is exercised in cramped places, precarious, and remedied with difficulty, which become places of worship, schools, and culture of origin. As did the Tamil who teach the *Bharata Natyam* to girls, their typical dance. The Tamil community in Palermo has founded three of their dance schools in which lessons are held in the Tamil language, so that little girls learn the language that they do not know or are likely to lose because they were born elsewhere.

The teacher: In this dance and music, there is much of our culture and our history. So it is important that here, in Palermo, the Tamil girls approach this dis-

cipline. During the lessons I talk and explain everything in Tamil, so that children learn the language they do not know or they risk losing.

In the case of other ethnic groups of Southeast Asia, the maintenance of original language, and the habit to not speak Italian at home, should not be considered as an adaptation refusal, but as an essential requirement for maintaining family relationships and to conduct family practices “in the same language as always.” The language of the traditions undergoes, on the one hand, the strength of what the older generations know and intend to hand down, or at least maintain. On the other hand, the opposition of new generations more willing to reinterpret traditional cultural expressions through modalities closer to adaptation. As in the case of food with traditional dishes with the addition or substitution with local ones.

Senegalese woman, 36, chef: The main dish in my country is rice with meat or fish. My son, however, prefers pasta to rice. Eats it every day at school like all other children.

An identity marker of differentiation is the traditional clothing, used only for religious ceremonies or civil feasts of the country of origin. Considerable differences in behaviors were noted among men and women of the Muslim faith. The latter are more oriented to compliance with cultural traditions and especially of their social role in public.

Muslim Tunisian woman, 45: I do not wear the veil neither here nor in Tunisia. I do not wear it by choice. You feel the need to wear the veil perhaps when

getting on in years, because being a mother, grandmother, aunt, you never have to attract anyone, and the veil gives you composure, discretion. When I go to the mosque, I never put perfume because it attracts the attention of men, although men are put forward, the smell in the air expands. When I go there, I wear a suitable gown, non-adherent. The shapes of the body not to be seen. Prayer is a sacred moment and sin is always present. For this reason, women pray behind the men, because seeing a woman on her knees bent is not indicated at the time of prayer.

The resistance to assimilation, in the context of the research, has not produced in immigrant communities identity claims to protect at all costs their identity through ethnic pride events. A differentiation identity marker is the traditional clothing worn only during religious ceremonies or civil celebrations of the origin country. Considerable differences in behaviors were detected among men and women of the Muslim faith. The latter are more oriented to respect cultural traditions and their social role in public.

Interviews indicate that in Palermo many immigrants fully realize their projects, respectively, in a medium span of time: they return home with money accumulated; in a brief span of time: transferring money to those who remained at home. The possession of the title of citizenship is irrelevant for immigrants when a residence permit will allow them to satisfy the reasons why they emigrated. This explains their disengagement in terms of sharing of social cohesion, including the adaptation of utilitarian daily practices, in which cultural and religious traits that are immanent in their homeland

are secularized. Cultural integration projections are instrumental to their needs.

Muslim Tunisian woman: When I needed the doctor, I had no problems. Even in hospitals. I have also been to the gynecologist; he was a man, for me it was not a problem. God gave intelligence to man to make us heal.

They do not advance requests in respect of their cultural precepts also because they have activated ethnic persistence practices in an atmosphere of great freedom of expression of differences. Thus, immigrants in Palermo live in a state of cultural contradiction, and, more importantly, of inertia in regards to the protection of their values.

In the perspective of the status passages, the passagee (in this case, the immigrant) may want to avoid completing his/her passage (towards a full integration) because he/she feels good within its status or because it is safer than it would be outside of it (Glaser and Strauss 1971). Therefore, the immigrant tries to slow down or reverse this process. In these cases, the practices of persistence and conservation of ethnic differences are configured as degrees of balancing the power of control between immigrants (passagee) and natives (agent).

Living the Islamic *Ummah*. Being *Dharma*. Super-Conformism, Ethnic Persistence

The dynamic nature of culture and values, and their interweaving, reproduces and finds out points of contact with the social reality faced for the first time.

Culture and religious values guide individuals in shaping a sense of belonging to the community. The comparative focus on the Islamic *Umma* and Hindu *dharma* reveals how religion intervenes in assimilation processes weaving significant interactions with the host community, to the point that minorities reduce the self-perception of the minority.

Ummah is a common Arabic word meaning “people, group,” or “nation.” The concept of *Ummah* might seem to correspond to the western understanding of a nation, but there are important differences. In the Muslim way of thinking, the only *umamah* that counts is the Ummah Islamiyyah, the Islamic Community, an entity that theoretically comprises all Muslims throughout the world, whatever their national origin.

In Italy, Islam is the second religion professed. Now Islam is an “immigrant” religion. In perspective, it will be a “resident” religion. Over the years, Islam will become a “transplanted” religion, not only because immigrants will continue to arrive, as for the second and third generations that are living and growing in Italy. Following the terrorist attacks of September 11 2001 in New York, a Western hostility towards Islam grew up. This hostility generated a defensive reaction in the Muslim immigrants regardless of ethnicity or origin, and strengthened their religious identity. The common experience of Islamophobia creates a unique suffering community that brings together the most ethnically disparate Muslim communities, creating in the public sphere hegemony, an assertive political Muslim identity (Birt 2009). The traditions and values of origin for Muslim immigrants are not an obstacle to integra-

tion, but tensions in the effort to adaptation, including super-conformism and apparent contradictions, that evaporates when the family welfare becomes an element of self-realization and change of status. It is mostly Muslim residents for a long time in Palermo to be interested in these typically Western paths of upward social mobility.

Tunisian woman, Muslim, 42: In Italy, I am very well, no lack of work and one earns well compared to our country. We have managed to graduate our girls, to buy some apartment in Tunisia, that we rent as a bed & breakfast. My husband is a very traditional guy. He always decides what is to be done. In a few years we plan to go back to Tunisia, where we will be no more poor as in the past.

In studies of integration processes, the foundations of a transformation of European cities can be traced with characteristics of the medinas of the Arab world, more and more characterized by an Islam perceived by clothes, rituals, schools, foundations, places of worship. Europe would become, in this case, a transnational social space of a community that expatriates and recognizes oneself (Maréchal 2003).

Palermo is particularly welcoming for Muslims. In Palermo, there are five mosques, 4 of which are precarious spaces (basements, garages) independently managed and financed by the faithful. Muslim community is therefore very homogeneous from the ethnic and identity point of view. However, belonging to the same faith does not seem to be an integration factor among Muslim immigrants of different ethnicity. The attendance of the mosques of

Islamic faithful is divided by ethnicity. The “Grande Moschea” is attended almost exclusively by North Africans, while Muslim immigrants who come from sub-Saharan Africa and Southeast Asia prefer other mosques, all located within the Historical Town. The same division happens for other religions: Orthodox, Hindu, Pentecostal, et cetera. North African immigrants are Muslims, Africans, Asians, and among them, even of different ethnicity, those who speak different languages and dialects. About the ethnic fragmentation of worship places, the most plausible explanation is that being often cramped places, these are unable to accommodate all the faithful who in fact are divided according to the proximity factor of identity and housing. An alternative hypothesis to the previous one is that the prejudice and the strategies of ethnic discrimination where “ethnic” criterion is the social category “discriminative” on which to base the hierarchical order in the arrival communities. In this case, it would tend to detract from or defame a group reputedly averse, to be banished, because of the competition for material and symbolic benefits through the development of distance and differentiation attitudes (Bourdieu 1983). The difference and the social distance that immigrants articulated in reference to religious practices and frequency, generate a state of disintegration among immigrants, not strictly religious, that accentuates the “ethnic” criterion as a social category “discriminative” to establish new identities on which to base the social order.

Hinduism is not just a religion, understood in the Western sense, related to dogma, church, clergy, divine revelation, but it is a set of beliefs, practices, cults, and rites that articulate all aspects of a man's

life. For Hindus, *dharma* is a way of life in which the religious component is inseparable from the socio-cultural. It is a way of living and being in agreement with society and its culture. Into more orthodox Indian culture, the meaning of *dharma* is implicit in the word itself. *Dharma* (law, justice, duty) is the religion, the eternal immutable law, order, balance of creation, the moral law imprinted in the conscience of every individual to which the human being must conform. In the case of Hinduism, the identifying element is the *dharma*, which distinguishes Hindu from those who are not. *Dharma* is to the Hindu what makes a man a Hindu. Only those who were born on Indian soil are Hindu. Membership in the Hindu religion absorbs the class membership. From a sociological point of view, religious values are deeply nestled in social organization characterized by the respect of tradition that becomes a defensive instrument against the processes of transformation and gender inequality. In my analysis, for the individuals, Hindu of Tamil and Mauritian communities, the religious belonging and the collective identity reinforce each other. In these communities, the wish (or need) to develop and strengthen their own cultural identity emphasizes the religious dimension of membership.

The Sinhalese are mostly Buddhist, the rest Christians. Most of Sri Lankan Tamils are Hindu or Christians. In Sri Lanka, among Tamils and Sinhalese, there are many caste divisions as the effect of Hindu influence coming from the subcontinent. Castes are rigid and unchanging. The transition from one caste to another is virtually impossible. Emigration phenomena have mitigated the rigidity of the obligations connected to religion and the so-

cial order of caste. In the analysis of the interviews, it is evident how the concept of caste survives only in the memory of the adult generations. With emigration the need to adapt to any type of work has caused the break of the link between caste and job, flattening social distinctions. Only for the occasion of weddings a trace of social distinctions emerges, in those cases in which a young Hindu marries with a Hindu of different caste or a native Catholic. In these cases, marriage is not forbidden, but only hindered. This family organization is therefore guided by values which guide the actions of the subjects also in the wider society. Rather, they constitute an identity marker. This could hypothesize an essentialist vision of difference, in which individuals are unaware reproducers in traditions and rituals that postmodernity recedes to a frozen world, always equal to itself, while the rest of their world keeps running, but far.

Tamil woman, 22, Hindu: My family has been in Italy for over 20 years. In Palermo we feel perfectly integrated. I study at the university. I dress like Italian girls, but some traditions are unchanged. Last year my brother got married. My father addressed an intermediary who deals with combining marriages. He was looking for the best combination of people and families. We, Tamils, have a booklet of our profile that is done with the help of the stars. The intermediary with this booklet seeks the most suitable person, even with the economic level and the status of the families because we still have the castes that differentiate people.

When it comes to gender differences, in the context of origin, women tend to lean towards a natural "so-

briety" even in the private sphere. In public, they are careful with their behavior. They avoid showing off, attracting attention. Within the family, a woman is totally subordinated to her husband, her mother, and older sisters of her husband. Men are ascribed the tasks to provide for the family's livelihood, the household expenses, clothing for their wives and children. The freedom of the woman's spending is limited to food. Emigration has however diminished the rigor that characterized the status of women in the concept of *dharma*.

The hybridization and the religious syncretism phenomena represent the creative spirit that resides in every culture, activated by the need to redefine the inclusion or exclusion coordinates. As in the case of the Catholic Tamil community that in Palermo is protagonist of religious syncretism between the symbols of their culture and the local one. The syncretism process goes appropriating of practices and places dedicated to the cult of St. Rosalia patron saint of Palermo (such as the Feast, and the pilgrimage to the sanctuary). These dynamics have a strong impact in the identity—symbolic complex of natives who feel as stripped of a non-material identity resource that they must share in spite of themselves. The Hinduism of the communities from Sri Lanka and Mauritius found in Palermo a wide space for expression, in which it was created a syncretism of symbols and rites, aimed at maintaining the values and social organization of origin. A dedicated prayer space can be found in all of the homes, decorated with images of the gods of the Hindu pantheon, and with a place reserved for the image of St. Rosalia, venerated by Tamils for formal connections with an original cult

from the homeland. This led to the annexation of the saint into the Hindu pantheon. In short, the syncretism of the Hindu cult of St. Rosalia is a religious expression perpetrated into the logic of ethnic persistence. If the identity markers, such as religious ones, can be factors of social distance in host societies, a strong sense of religious belonging makes the elements of distinction more meaningful. Sometimes an immigrant community in the effort to accelerate the process of social integration in the new society goes to assume a more detached attitude towards cultural and religious traditions. This does not seem to occur in the Tamil community in Palermo. The saint is venerated devoutly according to the syncretism realized between the Christian religious cult and the Hindu one. The veneration of St. Rosalia tends to defeat the daily difficulties, and at the same time to re-establish the community's sense of identity. In conclusion, the syncretism tamil of St. Rosalia is a coherent expression of patterns of action differentiated from the faith, but also chosen according to the aims of the people and within the environmental situations in which they live.

In conclusion, the two communities by pervading religious values in daily practices, apparently follow paths of integration in different ways. The Islamic community seems to swing between super-conformism and apparent contradictions. The Hinduist seems more oriented to ethnic persistence and to tradition.

In reality, the two paths are expressions of a differential adaptation expressed in some areas of life and not in others. The degree of difference is expressed

by different groups of immigrants when they use different tactics and different paths towards the integration. Even when religion is an essentialist factor of intense cultural and value alterity, however, it does not prevent integration.

Cultural and Religious Practices: How Religion Changes the Face of Multiculturalism

Places of worship have a high symbolic identity value where membership is expressed and the community meets. It is the place where the biographical steps of the faithful are celebrated with rituals that combine the history, culture, and values of the community. The analysis of religious phenomenology in Palermo revealed that the difference and the social distance that immigrants articulate in religious practices generate a state of disintegration among immigrants. This difference, not strictly religious, accentuates the “ethnic” criterion as a “discriminating” social category to establish new identities on which to base the social order. Different ethnic groups with the same religion (e.g., Italian and Ghanaian evangelists) do not frequent the same places of worship because of the language of the rites. A casual observer will notice the alternation of families, groups, different crowds for clothing. These differences mainly concern sub-Saharan immigrants who wear clothes of their ethnic tradition at religious celebrations and when they meet on holidays with their countrymen.

Ghanaian man, 34, evangelist: I pray in an evangelist church that is frequented by people who come from different parts of the world and who speak different

languages. So we do the shifts. On the day of the ceremony, the morning is dedicated to the celebration in English. In the afternoon, instead, in French.

From a religious point of view, the most relevant change is the visibility of the religious expressions of the immigrant communities. These changes have a strong impact on the identity-symbolic complex of natives who must share them in spite of themselves. The problem will be increasingly important for the growth of the number of Catholics in the countries of Southeast Asia and Africa, that is, from the countries with the strongest emigration towards Italy and Palermo. In the future, we will have to imagine increasingly multi-ethnic, but also multilingual places of worship, attended by Ghanaian congregations at 8am, Tamil at 12, Rumanians at 5pm. The various dioceses will have to organize celebrations in different languages, with celebrants who know more than one language. In Palermo, younger immigrants tend to be more numerous than older residents. So, the newly established immigrant families will celebrate marriages and baptisms, but also confessions and funerals. In multiethnic Palermo, some parishes have already begun this path by granting spaces for the expression of cultural and religious cultural differences. These possibilities assume significance of ethnic persistence, but also of integration to the territory and to the host community. So it happens that in the historic center the outdoor space of the parish of St. Clare, which also operates with cultural mediators, is enjoyed every Sunday by immigrants who gather to celebrate their holidays. Here the Ghanaian community, just as they do in the small villages of the African country, have symbolically elected a king and a queen. The *Ohemaa*

(the queen) and the *Ohene* (the king), who are real points of reference for the community. Like many of their countrymen, they do humble and poorly paid jobs, but on important occasions the king and the queen show off a lion's skin on the naked back and a crown that make them protagonists.

Far from the Caribbean Notting Hill London, even in Palermo immigrants celebrate on the city streets. Already in September 2016, hundreds of Tamil immigrants celebrated the "Vinavakar Chathutdy," an important Hindu festival, celebrating Ghanesha, one of the most loved deities of the community. The celebration was held inside one of the Hindu city temples, in a popular district of the city. For the first time, the city institutions were present (the Mayor, a Councilor). The streets were then populated by the faithful who marched through a procession of the chariot of divinity, through the streets of the neighborhood intrigued, including songs and traditional dances of the Tamil community. There are two elements to be emphasized. First: the public celebration in the same territory that the natives have always reserved for the procession of the Saint of the district. Second: the presence of the city institutions that has formally legitimized the freedom to express religious and cultural differences publicly. In the context of informal policies on integration in Palermo, for a sort of *par condicio*, even for other Islamic and Jewish religious ceremonies, they were celebrated in the presence of political institutions.

The spaces required by the community for the celebrations (and granted by the public administration) are public. On the occasion of the end of

Ramadan, the meeting of the Islamic faithful in the wide lawn of the "Foro Italico" is now customary. On this occasion, in the same place faithful of the three monotheistic religions gather in prayer.

Conclusions

The research was focused on the relation between public expressions of cultural and religious differences of origin and local integration processes and the role of religion in the cultural identity processes of the societies; it had permitted to describe the mode of immigrant adaptation to the culture and everyday social practices with the local communities.

The analyzed data tend to qualify the religion for the immigrants: a) as a medium to transmit values and cultural elements that define the cultural heritage; b) as a spiritual, material, and social resource that intervenes at different stages of the migration process. In Palermo seems to emerge an integration framework in which, according to Gans (1992), immigrants tend to lean towards unprecedented forms of cultural adaptation, because they enjoy broad autonomy and freedom on the desire to access to the social practices and lifestyles of the Palermo community. In the case study presented, religion is not a factor of irreducible difference, but of spiritual refuge and identity protection, even if reshaped by assimilation. The analysis delineated the adaptation declinations through syncretism and cultural contagions. Immigrants, schematically divided between conservative and innovatives, participate in the construction of a local model of integration in which the typicality is characterized:

- a. by socio-political context, welcoming, which facilitates the integration process;
- b. by specific ways of adapting to different cultures. The product of the relationship between differentiation factors and local culture has highlighted specific characteristics on the social construction of everyday life and the links with the urban space, which seems partially free from the negative representations of the difference;
- c. by faster integration for freedom of expression of differences.

The identity expressions in the private tend instead to lean towards the conservation, even if between contradictions and ambiguities. The hypothesis is that immigrants adapt identity conservation modes when “forced” by the public visibility to distinguish themselves, and to be recognized, mainly by their own community. This condition, however, tends to compose oneself both through assimilation dynamics to the local culture, either through recovery strategies, preservation, and expression of differences of origin. In the framework of the status passages, the research seems to show that immigrants as passagee change their public role model when under pressure of adaptation to the local culture and lifestyle. The voluntariness (and therefore the desirability) of the status changes in immigrants is strongly dependent on the control actions of the agent. In this case, it depends on the degree of freedom that the native community grants for the expression of differences. The prevalent characteristic of the native community as “agent” is not of limitation or restriction, but tends to lean towards the indifference. Then, why do the

natives allow wide margins of freedom to the immigrants’ expressions of differences? My hypothesis is that in Palermo indigenous community does not have much to defend in economic terms. Indicators on quality of life are the lowest in Italy. The high percentage of overall unemployment and socio-economic hardship of large sections of local population have a low impact on discrimination practiced in the labor market. Discrepancies between immigrants and natives are minimal, and not likely to generate feelings of right to similarity nor the typical instances of relative deprivation in immigrants (Loch 2009). Moreover, the distance between citizens and institutions is unbridgeable, for the lack of trust that they inspire, especially at a local level. So on the social ladder many differences with immigrants are reduced. This nullifies the danger that the “immigrants” get access to resources (missing or absent). The “Us First,” trumpeted by the Northern Italian local subcultures, does not make sense in Palermo.

Finally, in the framework of the integration policies, the general theoretical elements that are deduced from the analysis reveal that the integration process comes close to segmented assimilation, in which immigrants are assimilated in some areas of life and not in others according to the model proposed by Portes and Zhou (1993) in which different groups of immigrants follow different paths towards non-assimilation through modes of preservation of their identity.

The tensions of adaptation are influenced by migration dynamics when in the same territory resident immigrants and newcomers coexist. In these cases, it determines a competition between cultural mi-

minorities in which religion is a determining variable. As pointed out in other studies in Sicily (Ferrante 2011), the result is a push of newcomers to the lower rungs of the social hierarchy. In scenarios such as those described, religious affiliation also gains a renewed strength and legitimacy of the role of individual and social behavior.

The public dimension of religious differences does not emerge as an identity factor. It does not emerge even when the difference is an evocative political value of a comparison (or antagonism) to be composed on a cultural level. Immigrants themselves do not even invoke the tolerance that in other European countries minorities claim for the defense of cultural identity that they perceive as threatened. My hypothesis is that in Palermo immigrants act in a public sphere where the assimilation process has reshaped the cultural and religious differences, no longer connoted by their divergence from Western tradition. This does not mean that Palermo has realized a perfect integration. To be more accurate,

it means rather that in a socio-political climate of low pressure to integration, and in reality, instead of a place of substantial freedom of expression of differences, immigrants in Palermo do not need to claim their identity spaces.

You cannot impose a common identity shared by law, and even more in a coercive sense. When multiculturalism has encouraged ethnic minority groups to maintain and preserve their distinctive features, these groups have gone to the creation of parallel communities, but isolated and disinterested in the integration. A forced integration was established according to cultural patterns that do not belong to individuals, especially of immigrants. If they should enjoy greater freedom in expressing their “cultural” differences and qualifying their daily interactions, social or economic, in urban spaces, as evidenced from the research data in Palermo, probably they would spontaneously start an integration process more fluidly, not tied to negative representations of the difference.

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Book Review

**Nowotny, Helga. 2016. *The Cunning of Uncertainty*.
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Helga Nowotny is a scholar of great repute who has contributed greatly to our knowledge of science and technology, societal development, and social time in late modernity. In *The Cunning of Uncertainty*, Nowotny reflects upon uncertainty as an intrinsic part of human existence. Throughout the book's five sections (including a preface and an epilogue), Nowotny invites readers to embark upon a journey during which we will consider, challenge, and even change our understanding of the various kinds of uncertainty that we encounter. Her main argument is that societies ought not try to annihilate uncertainty (as such efforts are inevitably bound to fail); rather, we need to embrace uncertainty—learn to cope with it and engage with it in productive and constructive ways.

Initially, Nowotny explains that the human “craving for certainty,” that is, the human eagerness to rule out doubts and diminish fears by anticipating that which lies ahead, is hardly new. In earlier times, when humanity placed its faith in oracles and divinatory practices, the future, like fate, was viewed as already set, although not yet revealed. Modernity saw belief in oracles and divinatory practices fade as belief in sci-

ence rose (underpinned by the Enlightenment). Perceptions of the future changed: in the mid-eighteenth century, the future appeared not as a predetermined entity, but as an open horizon. This new view of the future as horizon brought with it a sense of optimism and confidence that the future could be managed and controlled. In the latter part of the twentieth century, that optimism slowly transformed into mounting doubts. These doubts manifested themselves in a view of the future (or rather futures) as something that should be predicted in order to mitigate or avoid unwanted outcomes. Nowotny argues that this change in the meanings ascribed to the notion of the future has led to our present view of the future as fragmented, volatile, and fragile. Drawing on the historian Reinhart Koselleck, she states that the future moves closer to the present as a consequence of the wish to map out probable futures from a present vantage point; or more succinctly put: “The present is... extended into the future” (p. 33). Gradually, uncertainty has become associated with a sense of threat linked to the anticipation of future deterioration. Perceptions of the future are thus likely to be characterized by feelings of fear *vis-a-vis* threats—perceived, imagined, or both—that “will materialize unless one

reacts by flight or fight” (p. 18). This, argues Nowotny, has to do with an increased tendency these days to confuse and conflate risk, danger, and uncertainty. Originally, risk referred to the conscious act of daring to put something valuable at stake. Risk-taking could, accordingly, generate gains, as well as losses, whereas danger generally alludes to something harmful only. Recently, the concept of risk has been robbed of its original meaning: risk has been reduced to a calculable and manageable object that needs to be contained, prevented, and mitigated. For this reason, risk is increasingly confounded with the concepts of danger and uncertainty. This confusion may increase our feelings of fear as we attempt to tame the future. Nowotny calls for caution, however, warning that fear may lead us into decisions based upon a too-narrow view of the future. Additionally, fear and uncertainty make poor partners, since the former cripples our ability to cope with the latter.

We expect science to generate knowledge that may assist humanity in navigating the waters of uncertainty. Nowotny argues that, while science has surely enhanced humanity’s capability to deal with uncertainties, to make predictions, and to anticipate risks, even the most elaborate algorithm can never fully guard against surprises or the unexpected. The contemporary idea of “future making” reeks of overconfidence, as the twist and turns of the “non-linear dynamics of complex systems” (p. 32) can never be fully foreseeable; similarly, the unintended consequences of human purposeful action can never be fully captured and/or assessed. Yet, despite the fact that predictions are always provisional (and contextual), Nowotny notes that science can still extend the range of those predictions. For

instance, the wealth of data available today constitutes an important tool for making better and more far-reaching predictions. Nowotny also notes another merit of big data: namely, its potential to nudge science towards asking *what* questions, rather than *why* questions. What is more, she underlines the importance of keeping in mind that “[d]ata are not simply given” (p. 44) and ought always to be understood in the context in which they were produced. In science, uncertainty is a driving force, as it excites, challenges, and pushes scientists towards the realm of the (as yet) unknown. Hence, “[s]cience at its very best thrives on the cusp of uncertainty” (p. 116). Venturing into the forests of the unknown creates space for unanticipated discoveries that may produce important new knowledge: in other words, for the pleasant surprise of serendipity. Nowotny is critical of systems, funding agencies, for instance, that insist on *a priori* information: that is, on assurances of what they will receive. She is convinced that the attempt to transform scientific uncertainty into certainties only displaces the uncertainty; as such it may actually hamper the knowledge production and the genuine long-term vision of science. Nowotny, is, furthermore, with regard to funding systems, critical towards, for example, grant applications that, in several instances, are steeped in a rhetoric of “over-promising.” Nowotny highlights the perils of applicants adapting to and/or applying such a rhetoric. Promising “too much” (naturally a subjective judgment), if the other party ends up disappointed, may have undesirable consequences such as the erosion of relationships built on trust.

Nowotny explains well the duality of uncertainty. On the one hand, uncertainty may evoke feelings of fear

and/or insecurity; on the other hand, it may be a space where human creativity can flourish. Any knowledge that springs from our attempts to reduce uncertainty merely leads to new questions—making uncertainty cunning, indeed. Like the Lernaean Hydra, it grows two new heads for every one chopped off.

Altogether, I view *The Cunning of Uncertainty* as a brilliant and insightful reflection on the complex topic of uncertainty. For those operating in future-making fields (politicians, scientists, policy-makers), it may serve as both a reminder and an eye-opener in the way it reframes the importance of a critical mindset towards the hopes we pin on supposed certainties. The book is a nuanced piece of writing, and Nowotny lays out, challenges, and scrutinizes her own arguments from various angles throughout. I find the way in which she does this to be particularly appealing. The book addresses our current situation without resorting to black-and-white thinking. It is neither alarmist nor overly optimistic. It is not, by any means, a step-by-step manual for planning the future (nor indeed for ignoring it). Rather, Nowotny puts forth a host of original ideas and leaves it to readers to form their own opinions. She frequently draws on a historical perspective, and she also points out the importance of context. In my view, the most intriguing part of the book is certainly Chapter 1, “Craving

for Uncertainty,” which presents several of her main arguments. Her own ambivalence towards the developments (especially the technological ones) and the tendencies she describes is consistent with her overall perception of knowledge as provisional and contingent upon context. Nowotny advises that the scientific enterprise should be driven by posing the question of *what* (or *how*), rather than that of *why*. Here, I would offer a minor reflection: If we lose sight of the “why,” does not the “what” risk becoming ahistorical? That being said, I am convinced that Nowotny does not advocate an ahistorical science. Nonetheless, I believe we should think twice before we switch to the “what” at the expense of the “why” (co-existence is, in my view, preferable). I also think that Nowotny’s ideas may prove valuable to various research fields: criminology, for instance, where the vocabulary of risk, prediction, and prevention is prevalent, and potential pitfalls in the pursuit of certainty (usually articulated in terms of security) have made themselves known (c.f. Zedner 2009). *The Cunning of Uncertainty* is important for a wide audience—officials, scholars, and policy-makers, but also anyone interested in the complex situation in which we find ourselves today. I warmly recommend the book to those interested in the late modern condition, notions of the future, and the multifaceted relationship between science and technology.

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