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## Scientific Research Activity and its Self-Reflexive Consideration

*Franz Breuer\**

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**Abstract:** »Wissenschaftliche Tätigkeit und ihre selbst-reflexive Betrachtung«. How did my social-scientific worldview and my preferred methodological approach on selected research topics get to the point where they are “in the end”? I am talking about the development of my positioning as a researcher in the field of tension between personal-familial, professional-disciplinary, university-institutional, political, and societal relationships. Here, the concept of scientific work/activity as performed by researchers in context plays a central role. I connect this to the postulate and the practice of an accompanying self-reflexive consideration as a component of my personal research style. In my eyes, the norms of scientific knowledge production do not result from an abstract “logic of justification” (Popper 1992) but are rather guided by the idea of an integrative inclusion of the social-scientific object of knowledge, the role of the personalized research subject, and the procedures of theory generation based on data, interactively produced within the framework of grounded theory (Glaser and Strauss 1967).

**Keywords:** Epistemology, grounded theory, personal biography, research style, data concept, self-positioning, psychology, social science.

*But epistemology is always and inevitably personal.  
The point of the probe is always in the heart of the explorer:  
What is my answer to the question of the nature of knowing? (Bateson 1979, 87-8)*

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## 1. A Personal Story

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I am writing here about the development of my positioning as a social scientist in the field of psychology, resulting in a “qualitative” methodology and method, which I call *reflexive grounded theory methodology* (RGTM). Factors that have influenced the course and the outcome of this approach can be related to familial conditions, the circumstances in my academic field of choice, the historical circumstances of society and politics (including the student movement of the 1960s and subsequent years), and the changing nature of the higher education system from then to the present – in addition to my subjective processing of these circumstances. The story is set in a (West) German context, and some socio-historical, political, and university-institutional circumstances are specific to this local setting. The fact that I grew up in Germany during the time of the “cold” confrontations between two opposing political systems, with their contrasting world views and with missiles aimed at each other, is not insignificant. The capitalist Federal Republic of Germany (FRG) and the socialist German Democratic Republic (GDR), with their big brothers USA and USSR in the background, faced each other as antagonists until the end of the 1980s.

RGTM is a qualitative social-scientific research style that represents a certain (re-)accentuation of grounded theory in the sense of its founding fathers Anselm Strauss and Barney Glaser (Glaser and Strauss 1967). Particular importance is given to knowledge prerequisites and to the procedures followed by the researcher, constituting a combination of attitude, theory, and methodology. Focus is placed on generating new theoretical concepts based on an in-depth examination of a research concern and the data obtained in the respective research field (mostly drawn from conversations/interviews or participant observation). The “spirit” of this approach is based on its creative-abductive attitude toward theory generation in a bottom-up manner, along with its self-reflexive thematization of the individual characteristics, action, and experience of the researcher as a means of producing knowledge.

The RGTM research style is therefore based, among other things, on the assumption that the individual character of the researcher is relevant for what he/she does in this specific role. In that sense, I am writing here about my personal life and professional history in conjunction with the formation of a specific research methodology and method (for a similarly intended retrospective, 20 years earlier, see Breuer 2000a).

According to a pearl of wisdom offered by Søren Kierkegaard, life can only be lived forwards and understood backwards. The description of the course of progression here might lead to the impression of a purposeful movement. However, I constructed the presented coherence. Along the way, I did not perceive it to be like this for a long time – the (fully) mature impression first

arose in (auto-)biographical retrospect. But every biography remains open to interpretation: its different readings are countless. Other interpretations are always possible.

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## 2. Childhood and Adolescence: The Beginnings

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I grew up in a lower-middle-class catholic family in the region of Sauerland (a rather remote area in the Central German Uplands in the state of North Rhine-Westphalia in West Germany). My parents had two sons. My brother was ten years older than me. I witnessed his pubertal fights with our parents as a little boy but did not understand much about them. I remained a mostly silent and sympathetic observer in the periphery, helpless and empathically torn between those involved. Even as a child, I tried to unravel the negotiations within my family, but it was difficult for me to maintain an inner distance.

At the graduation ceremony after my *Abitur* (the final year of secondary school), I gave a speech as a representative of my class in front of my parents, teachers, and fellow classmates in which I described in quite a perky manner the social structure of my class, characterizing the different groups and cliques. My classmates liked it: they applauded. The school principal did not like it. He jumped up at the end of my short speech and added the things that, from his point of view, would have been appropriate for a pupil to say on this occasion. In his view, in particular the acknowledgements had been missing. From today's perspective, I would say: that was my first grounded theory study. The reactions to it were divided, an experience with potential for continuation.

When I was young, my brother was my biggest idol. He had become a writer and had already enjoyed some initial success (As a late document, for example: Breuer 1980). I loved to go and visit him. He lived in a bohemian milieu in a large city to the south of Germany. I was fascinated by his friends, their performance, and everything around them. There, various kinds of pills (both stimulating and sedating), hashish, and, above all, alcohol played a major role. It was fun and exciting at first, but as time went on, there were increasing problems and crashes. I witnessed my brother lose control of his drinking. My attempts to help and to intervene failed. I was studying psychology at that time and therefore felt this was my "calling." Words of encouragement no longer had any effect.

In the subsequent period, I tried to maintain my own psychological balance by setting boundaries. I reduced contact over several years. Instead, I dealt in a professional-psychological way – among other things in my seminars at the university – with alcoholism and the subsequent problems in the family system. On the one hand, I distanced myself from the family hotspot; on the

other hand, with regard to my research style, I developed a preference for field research and field involvement at that time. I made, for example, contact with local Alcoholics Anonymous<sup>1</sup> and Al-Anon<sup>2</sup> groups and integrated the experience into my teaching and research.

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### 3. What and Where to Study?

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But let us go back once again. I graduated from high school in 1967. With regard to deciding “What now?”, I was at a loss for ideas. My brother, my role model with literary ambitions, had inspired me to think about doing something in the artistic field. I wanted to study theater science, with little experience in this field and very vague ideas about what exactly was involved in pertinent studies. My father, who was supposed to finance my studies, did not support this choice. He would have liked me to become a teacher, like him. For me, in contrast, it was clear: certainly not a teacher, not a chance! We struck a compromise: psychology as a major. For my father, this seemed close to pedagogy, but at that time the job descriptions for psychologists were still quite nebulous. For me, the field was “exotic enough.” Theater studies as a minor. About the place of study: I moved from the Sauerland county town to the European metropolis of Vienna, one of the few locations with a German-speaking university where it was possible to study theater science, a rare discipline at the time. And Austria – being abroad – allowed me to escape military service in the Bundeswehr<sup>3</sup> for some time. My father still called the military “Wehrmacht,”<sup>4</sup> and he himself had had negative experiences with it, from which he wanted to protect me. Therefore, he approved of my Vienna idea, even against the wishes of my mother, who did not want to let me go so far away from home at the age of 17. At that time, I experienced Vienna as a gray city that did not prove kind to me: almost like in the movie *The Third Man*.<sup>5</sup> I had a hard time with social interaction. After two semesters, I was disappointed with theater studies and decided to drop it. I enjoyed psychology. However, I have always maintained a “literary ambition” with a humanities/cultural-scientific and hermeneutic orientation, as I would put it from today’s perspective. But I was not able to discover and cultivate this approach in university psychology at that time.

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<sup>1</sup> <https://www.anonyme-alkoholiker.de/> (Accessed November 20, 2020).

<sup>2</sup> <https://al-anon.de/> (Accessed November 20, 2020).

<sup>3</sup> Federal Armed Forces.

<sup>4</sup> Name of the German armed forces during the National Socialist era and the Second World War.

<sup>5</sup> See: <https://www.youtube.com/watch?v=U1LTnOvPiZQ> (Accessed November 21, 2020).

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## 4. From Vienna to Münster

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At the end of the 1960s, Vienna was an ordinary university in the traditional style, still somewhat affected by the (German) student movement, which everybody was talking about. There were attempts to set up an Austrian radical left student organization similar to the *Sozialistischer Deutscher Studentenbund* (SDS; *Socialist German Student Union*): *Sozialistischer Österreichischer Studentenbund* (SÖS; *Socialist Austrian Student Union*). Their first public appearance was a “teach-in” on the topic *Art and Revolution* in June 1968. It took place in the university’s second largest auditorium. The event turned into a happening with Austrian performance artists who later became well known (Günter Brus, Otto Mühl, Peter Weibel, Oswald Wiener, Valie Export, and others). It became a scandal with nationwide resonance (“uni-orgy,” “uni-piglets”), arrests, psychiatric evaluations, trials, and detentions (among other things, for “degradation of Austrian symbols” and “violation of morality and modesty”): a full-blown Austrian affair.<sup>6</sup>

I lived at that time as a subtenant on the “other” side of the Danube with a nice conservative family who invited me at times to eat Schnitzel with them on Sunday’s dinner. They asked me about this scandal at the university that they had read about in the newspaper. When I told them I had been there as a bystander, they did not speak to me for several weeks.

In 1969, I transferred to the University of Münster<sup>7</sup> to continue my studies in psychology and my colorful palette of changing minors. There, a different image of university became apparent, at least among “my” psychologists. I was accepted into a sub-institute with a newly appointed chair who had a certain openness and tolerance for the ideas and concerns of the developing student movement, a man from the *Social Democratic Party of Germany* (SPD). He had brought along a team of unorthodox employees who were ready to shake up traditional structures. It felt like many doors were open, intellectually and scientifically. The experts on the discipline often admitted: We (also) do not know it (any better), but we can research and develop it! The feeling was “We can reinvent the world (of psychology) over again!” “Progressive” ideas had a place, and it was the time of the anti-authoritarian. Smoking was a matter of course everywhere, even in seminars. The commitment of many students was immense. Even at night, the crank handle of stencil printers could be heard turning in institute rooms. Blue staining stencil matrices, which had been previously produced on the typewriter, were attached to printer rolls to make handouts, questionnaires, or leaflets for the next day. Sometimes, in my eagerness, I spent the night in the institute rooms.

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<sup>6</sup> See: <https://geschichte.univie.ac.at/de/artikel/das-jahr-1968> (Accessed June 7, 2020).

<sup>7</sup> Münster: a medium-sized city in North Rhine-Westphalia with about 200,000 inhabitants at the time and about 30,000 students at the university and other colleges.

At that time, “left” theory played a major role, and a variety of new topics and approaches were developed and tried out – in student “grassroots groups,” for example. My first grassroots group (on “sexual economy”) dealt with Wilhelm Reich’s book *Genitality in the Theory and Therapy of Neurosis* (Reich 1980), which brought me into contact with psychoanalytical ideas that were otherwise largely ignored and devalued in academic psychology. We naturally studied the Marxist “classics,” and I worked in a group dealing with youth education in trade unions alongside psychologists, educationists, and sociologists.

I got a job as a student assistant. The pay was sufficient, which meant that I was no longer financially dependent on my father’s monthly checks and, on top of all that, I had an income during the semester breaks. I worked on various research projects, in some cases assuming responsibility for the content. The academic milieu fascinated me, and I received friendly support there. After my psychology diploma in 1972, I got a job in a third-party funded project, and after a while I was able to switch to a permanent position as research assistant. It was a time of institutional expansion in university psychology, and many new positions were created, both mid-level academic positions and professorships. In this respect, things were going well for me professionally – all except for the threat of military service that I still had to complete darkening my mood, but that is another story.

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## 5. Materialistic Science and Psychology: Between 1970 and 1980

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In 1975, the book *Materialistische Wissenschaft und Psychologie* (Materialistic Science and Psychology), by a “collective of authors at the Psychological Institute of the University of Münster,” was published by the publishing house of Pahl-Rugenstein (Autorenkollektiv Wissenschaftspsychologie 1975). The book can still be ordered online today (November 18, 2020), second-hand starting from €1.50. The collective consisted of about 15 to 20 authors, psychologists of different academic positions, ranging from recent graduates to professors. The anonymity was not due to the modesty of the authors but rather had to do with the, at that time, widespread West German practice of so-called *Berufsverbote* (the state spying on “enemies of the constitution” and keeping them out of public service) and the related preliminary measures (Rigoll 2013). All feared for their academic careers, if they were subject to such pursuits. None of the authors listed the book on his/her publication list when applying for a job, myself included.

The work on the book was preceded by a “Capital” reading group in the early 1970s. Over the course of – approximately – one year, we had weekly

evening meetings to read Karl Marx's very unwieldy book *Das Kapital* together, which we used as a basis for the fundamental breakdown and foundation of our understanding of the world and which we therefore wanted to study at great length. At the time, reading *Das Kapital* was a common "practice" in the academic milieu. After we had gone through Volume I, we started with Volume II – only to discontinue soon thereafter, tired and impatient. We finally wanted to try our hand at transferring what we had read and understood to the science of psychology and at designing a counter-draft to so-called "bourgeois psychology." And again there were – for an estimated two years – regular evening meetings of a group we called *WiPsy* (*Wissenschaftspsychologie*; psychology of science), in which we made our way through the philosophy and the political economy of Marx, Engels and Lenin, the Marxist epistemology from GDR texts, the theory of state monopolistic capitalism (Stamokap), Soviet and GDR psychology, and left psychology approaches from the West (very strong at that time: Klaus Holzkamp<sup>8</sup> and his sphere in West Berlin). We were practicing a serious yet joyful science. After the *WiPsy* meetings, we played table tennis in the attic of our institute, and late in the evening we went out together to a chicken grill in Münster. Not everyone involved was on the same page at the time; there was a broad undiscussed spectrum of leftist ("progressive") positions, whose basic orientation was "ideologically" closely related to the DKP<sup>9</sup>, partly due to the GDR origin of a lot of our discussed literature. In the end, there were problems with the editorial office at the Pahl-Rugenstein publishing house, which – though we did not realize this at the time – depended heavily on GDR support and guidance. Obviously, our positions were not entirely "straight" in their eyes.

When I read the book with today's eyes, it makes my hair stand on end for many pages: the adoption of foreign thoughts, dogmatic positioning, pseudo-legitimization through citations of authoritative texts, an unquestioned certainty of the world view, a strong and undoubted notion of truth and partisanship, a finalized attitude towards knowledge – all evidence of a clumsy appropriation process. The struggle for a progressive social attitude and positioning was – in my view – a unifying element for the authors: It was about making psychology useful in research and applying it in a socially "emancipatory" way to achieve partisan commitment in a voluntaristic sense. Psychological findings and technologies/practices, which were becoming increasingly important in social contexts, were to be withdrawn as far as possible from the grasp of capital interests. Our concern was to develop alternatives.

One of my matters of interest at that time was the epistemological and socio-theoretical purpose of science (psychology, social science) under historically changing socio-economic circumstances, or in Marxist terms: the

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<sup>8</sup> Still worth reading: Holzkamp 1972.

<sup>9</sup> Deutsche Kommunistische Partei, ([West-]German communist party), in FRG since 1968.



development of the science of psychology into an “immediate productive force.” In order to trace this characteristic, I – together with a befriended co-author – reflected on the notion of scientific activity. This expression had, in our eyes, the advantage of abolishing or integrating the usual scientific-theoretical triad of perspectives of “context of discovery/development,” “context of justification,” and “context of application/utilization.” And, moreover, the characteristics of the researcher/recognizer could be conceptualized as a subject. The perspective of “science as an activity” or “... as (social) work,” carried out by “physical subjects-in-context,” has since – with a different focus – solidified and substantiated in my work, even beyond Marxist convictions, and thus also confirmed the turn to the reflexive observation of my own role as a scientist.

Besides academic-professional activity, the “organizational question” was in vogue at that time: Marxism was all well and good on paper, but now in practice! And a central element of this practice was membership in a left or communist party. The question “How do you stand with the working class?” was posed to me seriously – and the answer should be the basis for a decision to join the party. Several good friends had joined the *DKP* and sometimes tried to tempt me to enter. But I could not decide. On the one hand, I had subscribed to the *uz*<sup>10</sup> alongside the *FAZ*<sup>11</sup>, while on the other hand, I read *The History of the Russian Revolution* (Trotsky 1980) and *My life*, by Leon Trotsky (1970), and *Anarchy's Brief Summer*,<sup>12</sup> by Hans Magnus Enzensberger – all with sympathy. The mainstream of “really existing socialism,”<sup>13</sup> as it was later called, did not come off well here without me seeing the disaster behind it. But the question of how to profess and position myself politically and the ambivalent spectrum of feelings persisted: lacking sense of belonging on the one hand, playful freedom and openness of thinking on the other. This was accompanied by the “guilt” of the respective indecision and inaction: a persistent feeling of social marginality, of being on the outside of relevant groupings. The developments of the members of the group of WiPsy writers soon diverged into different political and professional directions.

In 1977, I published a slim book on the *Introduction to the Philosophy of Science for Psychologists* (Breuer 1977) in which the categories of “scientific activity” and “scientific problem” were guiding concepts. In this book, the Marx-Engels foundation had receded into the background and had been made quite unremarkable. I did not want to lay it on thick anymore. The scientific-theoretical conceptualizations were still colored by this spirit but more easily digestible for different groups of readers. The book – published by a Catholic

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<sup>10</sup> *unsere zeit* (our times), the daily newspaper of the *DKP* at that time.

<sup>11</sup> *Frankfurter Allgemeine Zeitung*: a widespread sophisticated conservative newspaper.

<sup>12</sup> Enzensberger 2018.

<sup>13</sup> Self-characterization of the system of the GDR and other socialist states in Eastern Europe since the 1970s.

publishing house in Münster, which was rather marginal for psychology – was relatively successful (three editions), and I used it for years as the basis for my introductory lecture in the psychology curriculum. The response to this topic among psychology students was limited, however: the decline in participants over the course of the semester was always noticeable. At that time, though, philosophy of science was still considered an obligatory part of the method examination for an undergraduate degree in psychology. This changed later, in the 1990s, when a background in this field was considered more and more irrelevant in the mainstream of the discipline.

In 1989, a fourth edition of my book was released (Breuer 1989), about three times as extensive as the first text. Apart from the title, it had hardly anything to do with the previous version. The focus remained on the activity and occupational perspective: on what actual scientists do. With this new conception, I had to some extent freed myself, regarding my independent positioning and attitude. With today's eyes, I still enjoy reading this book.

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## 6. From “Behavior” to “Action”: Basic Paradigms of Psychology and the Path to an Independent Research Style

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Psychology in West Germany was strongly influenced by U.S. viewpoints after World War II. In the 1960s, experimental psychology and behaviorism (behavior and learning theory) in particular were on the rise. The naturalistic notion of science was taking over the terrain. Behaviorist theory had advantages under various scientific criteria from this point of view, such as intersubjective observability, operationalizability, experimental testability, simplicity, relative ease of implementation in practice. Classic and operant conditioning were gaining popularity as universal learning principles, and both U.S. and Soviet psychologists agreed here on the basic concepts (Skinner and Pavlov as prototypical representatives). A booming psychological praxeology during this period that translated such principles into intervention procedures was the clinical psychological treatment technique of behavior therapy. In the 1970s and 1980s, many types of fast-acting procedures were developed for the reconditioning of psychological disorders. On this basis, *programmed learning* also found supporters in educational contexts for a while.

Over time, the behaviorist theory cluster was enriched by cognitive components and by cybernetic thoughts. People were often dissatisfied with the fact that the “organism” in behavioral theories was modeled in a stimulus-dependent, reactive way, as a black-box conception without inner-psycho processes and without self-regulation. These voids were filled with system-

theoretical ideas and the inclusion of so-called cognitions (goals, motives, plans). The computer metaphor came into play as a basic model concept for the psychology of behavioral regulation. In this context, the concept of behavior, which was coupled with the idea of externally controlled conditioning processes, was sometimes replaced by the concept of action in which goal-directedness and self-control were more central. From the mid-1970s to the mid-1980s, there was a rise in action theories from a variety of disciplines beyond psychology. Interdisciplinary impulses also came into play. Hans Lenk edited a multi-volume anthology *Handlungstheorien interdisziplinär* (Lenk 1978ff.; Interdisciplinary Action Theories). In addition, in “left” psychology, approaches developed that established connections to activity theory or the so-called cultural-historical school of Soviet psychology (e.g., Galperin, Vygotsky, and Leontyev; Kölbl 2006; Slunecko and Wieser 2014, 347-52).

These action or activity theoretical approaches were attractive to me for a number of reasons. Besides the possibility of linking them to psychological theories with a Marxist foundation, the connectivity to other disciplines and the theory-integrative potential seemed promising to me. In my dissertation ([...] *Action regulation in academic studies*, 1975 [Breuer 1975, 11-78]), I tried to operationalize action-theoretical approaches using questionnaires about personal orientation concepts for conducting academic studies.

In this context, models of different levels of control and regulation evolved via the concepts of “activity, action, operation.” A frequent topic was hierarchical-sequential action control (e.g., in the field of occupational psychology; Hacker 1973; Volpert 1974). Such conceptions of hierarchy, however, echo the organizational principles from the factory, the church, or the military more than they can grasp the interpretative horizons, ambivalences, and fragilities of the everyday experience and action of individuals. Moreover, the ideas of purposefulness and rationality of action were overestimated. At times, remedy was sought in the concept of heterarchical organization, which is characterized by changeable and flexible control functions (Raeithel 1983). Also, different types of action were distinguished: besides purposive action, for instance, there were other-directed forms such as rule-governed action or the narrative model of action (Straub 2002, 351-379).

A series of psychological awakening conferences took place under the umbrella topic of action theory (sponsored by the *Swiss National Science Foundation* and the *Volkswagenwerk Foundation*) in Langnau/Emmental (Switzerland) and in Reisingburg Castle (Bavaria) in 1982 and 1983. Efforts to bring together the diverse approaches and threads of action-theory provenance for clinical psychological contexts were made at a series of meetings at the University of Landau (Pfalz; Renaud van Quekelberghe [Breuer and van Quekelberghe 1984] between 1982 and 1984.) These were the meetings in which I myself was involved – certainly there were more.

How mental control, regulation, functions, and operators are modeled changes constantly in the context of theory construction in psychology, varying by domain, by basic scientific perspective, and by the research apparatus available. Modeling metaphors change over time. For a while, I believed in the future prospects of the concepts of activity and action as a discipline-integrating guide. Specifically, at that time I attempted to analyze professional action in the field of psychological counseling and therapy using action-theoretical ideas and a range of methods that were developed especially for this purpose (Breuer 1991).

The action paradigm, however, lost its appeal in psychology in the years that followed. The theory-integrative promise was not kept, and the “movement” dispersed. Approaches diversified: Learning theories underpinned by cognitive psychology moved on their path of success toward recognition in the West German health insurance system as a psychological healing procedure in behavior therapy; adaptations from Soviet and GDR psychology lost their appeal with the downfall of “really existing socialism”; the orientation of psychology increasingly shifted to neuroscientific models. Methodologically, the approach proved unwieldy, as human action can only be captured in a very reduced way with the experimental-quantifying inventory of methods that dominates the mainstream psychological canon. Although there is still talk of action-theoretical ideas here and there, they remain separated and specialized within their limited perspectives.

My “academic circumstances” had consolidated to some extent by the early 1980s. I had completed my psychology diploma in 1972, my doctorate in 1976, and my habilitation in 1980. In this respect, everything was going smoothly. I subsequently tried to get a professorship outside my “home university” of Münster – the next step along the customary career path. I did not succeed, however. There are various explanations for this. On the one hand, the job situation at German universities was not developing well: Many recently established professorial posts had been filled right under my nose with relatively young colleagues; the university expansion phase had come to an end, and the number of positions available for someone freshly qualified as a professor had declined considerably. I had an unclear portfolio in terms of (sub-)disciplinary assignment (was I a general, developmental, clinical, professional, or educational psychologist?). Furthermore, I did not present myself well: I did not perform well on job applications. Over several years and a series of applications, I followed the usual academic career goal – always on the verge of leaving Münster. This kept me in an intermediate state, a provisional arrangement, for quite a while, which, under the surface, was frustrating for me. However, I was not really “existentially” threatened by this situation insofar as I had the privilege of holding a permanent position at the mid-

academic level. As an assistant professor (*Privatdozent*<sup>14</sup>) and later as professor by special appointment (*außerplanmäßiger Professor*<sup>15</sup>), I was then able to acquire the status of professor – an odd, somewhat complicated dual existence in the West German university system that no longer exists in this form today. My precarious situation also brought privileges, not least because I was not under the obligations and constraints of a “subject representative of XY,” and instead I could pursue my thematic interests, courses, and research topics quite independently beyond canonical boundaries. This also included the beneficial circumstance of being supported for a long time by a collegial and friendly environment: Although I was considered to be “not all there” – behind my back some doubts were expressed about the scientific status of what I was doing – I was left to do “my thing” for the most part, albeit (in terms of the material and human resources made available to me) on a small scale. I had a relatively large degree of freedom “in the system,” one could almost say a certain fool’s freedom. In return, I exhibited “good behavior,” for example, by taking on administrative tasks. These circumstances made possible, determined, and accompanied the developments of my views and positioning that I describe for this stage.

Another level, concerning my own person, comes into play here. My feeling of stagnation, general insecurity, and latent frustration in my “career-related waiting loop” led me to engage in a group therapy setting of depth psychology during this time. I was able to present my participation to the outside world as further training in psychological supervision in order to save face, and I was even able to claim it on my annual tax return. It was not easy for me to engage with this special culture of interaction and the self-reflection and self-opening practices associated with it. It took me several years until I was able to draw a recognizable benefit from it. On the one hand, I relate this experience to my decision to make personal peace with my professional situation, which did not meet my own expectations. It allowed me to accept my living situation in Münster with a somewhat definitive character and to start creating a life for myself in this place – here and now – in a new and conscious way. On the other hand, I was able to use the modes of reflection and communication I had learned in the group therapy for my own ideas of a self-reflexive attitude in scientific activity, for the practice of interactive exchange in group-based seminars in university, and finally as a tool for generating ideas in research groups (more on this below).

In the course of the 1980s, a change took place in my scientific-psychological imagination and work, which can first be characterized on the level of my methodological positioning. In my dissertation I had written about the socialization process of students in different subjects. In a large third-party funded

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<sup>14</sup> Academic title held after habilitation, which carries no relevance to a paid professional position.

<sup>15</sup> Professorship (at that time) with corporate privileges, but without a corresponding regular position.

project, we had sent out voluminous questionnaires with a large number of Likert-scale items and calculated the scale values in a variety of ways using the SPSS software for statistical analyses that had just emerged at the time. In the project, however, I never came into physical contact with any of the students being researched. Everything went back and forth (at that time still) via postal mailing. Afterward, as part of my new research domain on the professional socialization of psychological therapists through study and practice, I began to get in touch with my research partners not only by postal contact and questionnaires but also in the form of face-to-face interactions. During these interviews, I gained richer, more differentiated, and more profound insights into their working methods and development than I was able to do by means of the questionnaire method (Breuer 1979). However, I lacked the methodical tools to utilize this experience as data to social science theories in an intersubjectively acceptable way. On the one hand, there was a problem with regard to the sample size: There were always too few cases according to validity standards within the framework of the usual quantifying evaluation methods. On the other hand, I did not have access to the linguistic material for appropriate evaluation – that is, the extensive interview transcripts as data produced by this approach.

This sparked a long meandering search through different methodological and methodical conceptualization worlds: biographical analysis, content analysis, conversation analysis, ethnomethodology, ethnology, ethnography, objective hermeneutics, and finally grounded theory (GT). By the end of the 1980s, this had also gained significant popularity in the German-speaking social sciences. This was especially the case in sociology, whereas hardly anyone in the field of psychology had heard of it at that time.

My initial text for the appropriation of GT thinking was Anselm Strauss' book, *Qualitative Analysis for Social Scientists* (Strauss 1987), which was soon also available in German. I read the book first for myself, then together with psychology students in higher semesters who, like me, were looking for alternatives to the methodological mainstream approach in psychology. Often, with regard to the choice of a topic for their final research paper (thesis), they wanted to work on a personal "heart and soul" issue, and they were often motivated to face the personal side of their research process reflexively and to share this in our colloquium group.

The Strauss book and the GT approach were a direct hit for my further social science development. Different ideas and concerns to which I adhered were brought together here, for which I had not found an integrative framework until then. Now I had a theory-method package that was right for me: an action-theoretical orientation, a conception of social action as a basic characteristic of subject-related thinking; a methodological system beyond specific evaluation instruments, an entire research path from beginning to end, thus also connectable to epistemological questions and positions; learnable

procedures (of interpretation/encoding) for dealing with textual data in a rule-guided manner; a creative trait of opening spaces for independent theory invention and development; and finally, the possibility of a plausible conceptualization of one's own person, role, and activity as a personal researcher, an epistemological guiding principle with freedom for shaping the component of self-reflexivity (Breuer 1999a, 193-309).

It took some time until I was able to move more or less confidently in this methodological field and its recursive processes and to develop my own research style. In doing so, self-reflection has increasingly become an important concern.<sup>16</sup> This was not only in the scientific theoretical sense of positioning scientific activity in a specific (social, cultural, interactive, and so on) context; self-reflection also became significant in a concrete-instrumental way by using its potentials for personal topics, the steadfastness of one's own point of view, and/or perspectivity. I analyzed this within the framework of a constructivist conception of research as interaction and as the concentration of one's own mental, physical-bodily, and affective-emotional resonances in the research process (Breuer and Roth 2003). The resonances of the (own) researcher's body became, on the way of their reflexive focus, potential "windows of knowledge" for the purpose of self-realization and object elucidation.<sup>17</sup>

In our working group, mainly consisting of ambitious and advanced students, we adopted the GT approach by means of reading, discussing, and testing – at first without connecting with like-minded people elsewhere. Such people did not exist in psychology at our location anyway, and we rarely found them in psychology at the national level.<sup>18</sup> The group process was of great importance in the development of this research style inspired by grounded theory methodology and methods – a joint acquisition of the mindset and the style of interpreting data (mostly interview transcripts). The group became a support for developing social-scientific confidence in a skeptical disciplinary environment. Furthermore, it became an essential tool for the

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<sup>16</sup> In 2002 and 2003, together with Katja Mruck and Wolff-Michael Roth, I edited two issues of the new open-access online journal *Forum Qualitative Sozialforschung / Forum: Qualitative Social Research* (FQS) on the topic of Subjectivity and Reflexivity in Qualitative Research: <http://www.qualitative-research.net/index.php/fqs/issue/view/21> and <http://www.qualitative-research.net/index.php/fqs/issue/view/18> (Accessed November 21, 2020).

<sup>17</sup> Pierre Bourdieu's often cited criticism of a "narcissistic reflexivity" in contrast to the postulate of a scientific (sociological) reflexivity coincides with the idea outlined here only to a limited extent. The postulate of reflexivity goes beyond the scientific and the sociological into the realm of the psychological/personal. However, the objective does not amount to narcissism but rather to object elucidation in light of the personal preconceptions and resonances of the research subject. The fact that this usually results in some sort of idiosyncratic self-realization is a (more or less pleasant) side effect.

<sup>18</sup> An exception is the contact with Jarg Bergold, Heiner Legewie, Günter Mey, and Katja Mruck (all in West Berlin), which led to the foundation of the online journal *Forum Qualitative Sozialforschung / Forum: Qualitative Social Research*: <http://www.qualitative-research.net/index.php/fqs/index> (Accessed November 21, 2020).

analysis of data and for the intersubjective comparison of interpretations. We developed a group culture of communication, exchange, self-reflection, and interpretation of data as well as personal self-resonances from our research. The discussion of one's own references and difficulties was allowed, and there was space and enthusiasm for developing creative thought.

My role in such group contexts has naturally changed over several decades of following this research practice. My expertise and confidence have grown, and the age gap and social distance to the other group members have increased. I now act as a representative of a research tradition that is being dealt with in textbooks. Since leaving active university service, I no longer work as a supervisor and reviewer for graduate theses but rather as an advisor for social science projects (often dissertations) in a variety of subjects in many places. I rarely have to deal with projects from psychology anymore. The field is far removed from this kind of research methodology.

In the first book publication about our conception and in several example projects with data-based theory designs of this research style, we chose the headline *Qualitative Psychology* (Breuer 1996) in the silent hope of drawing attention from the psychology community. However, the relevant resonances remained very modest, even those from neighboring disciplines – after all, it was (only) about psychology. I did not escape from the marginal zone in this way. This prompted me to break away from the disciplinary focus on psychology in subsequent textbook publications, to give the GT enterprise the name “Reflexive Grounded Theory” (Breuer 2009), and to publish it in a broadly oriented social science publishing house.

For a long time, however, efforts to connect our research style across disciplines had limited success. Pointedly summarized, a typical reaction from sociology looked like this: The grounded theory approach belongs to us, ergo we do not have much to learn from psychologists! With regard to the idea of self-reflection in this context: We have always done it that way, it is nothing new for us! In fields with less sharpened methodological identity and expertise, RGTM was more readily accepted (among others in pedagogy, medicine, history, cultural studies, music, political science). In my experience, the much-flagged maxim of inter- or transdisciplinarity quickly reaches the limits of practical implementation for representatives of the discipline when it “gets to the crunch” of traditional boundaries and resources: for example, with regard to citation habits and cartels.

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## 7. Choice of Research Topics

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During my time in the institutional university setting, I had the privilege of choosing for the most part the topics I wanted to work on. In addition, the field of psychology – despite all the reservations I developed over time about



the mainstream there – offered the advantage of justifying many topics that occupied and interested me from a psychology standpoint. On the one hand, this may be an indication of the developmental status of the discipline: few lasting and long-term paradigms, instead fast-moving topics and theory conjunctures. On the other hand, this was advantageous for me in that I could follow my changing thematic interests and choose focus points that resulted from personal and biographical encounters and entanglements.

My early thematic research interests – socialization in university studies and socialization in the work of psychological counselors and therapists – came about, as I would characterize it in retrospect, from opportunistic considerations (for example, job offers in research projects). These interests were also fostered by the fact that I did not have to move too far out of the contexts I was familiar with (university milieu, psychological therapeutics). In the course of turning to the grounded theory style of research, the subject areas became increasingly tied to my own personal world. I registered this entanglement and made more and more efforts to reflexively work with this proximity. For example, in reference to my brother who was addicted to alcohol and witnessing the co-dependencies in his family, I turned for a while to the topic of alcoholism and its consequences for the family. My friendship with an ambitious director of a special education facility was the starting point of a field research project on school development for the hearing impaired. My self-perceived status of not belonging to a range of social contexts led me to search for social marginality in different areas of life (the book *Abseits* [Aside]; Breuer 1999b). When my widowed mother was no longer able to live independently and I accompanied her in her move to a retirement home, the topic of “aging and family” with its many facets, upheavals, and status passages became relevant to me. I have dealt with the topic of “succession and passing on” (Breuer 2009; between generations) in a multi-layered way over many years. The personal reference is not obvious here. Childless, I have no family-generational successor; in academia, where I had spent much of my professional years as professor by special appointment, my long-term university position was completely rededicated after my retirement, so that in this respect I have left no lasting traces in the form of a relevant institutional successor. The continuation of a “life’s work” in a sense is something I try to achieve by developing and passing on the reflexive grounded theory research style (for example, in workshops), although I seem to have somewhat succeeded only since I retired from active employment with the university. Moreover, a number of my students have not succeeded in the university context but made their way in very different areas of individually tailored professional practices (Breuer 2020, 205-20).

At a scientific theory level, the handling of the personal interests and involvements condensed in such a way that I tried to clarify more precisely the entanglement and affectivity of the individual in the context of scientific

activity: one's own attitude toward the problem in focus, the "pre-conceptions," the personal history with the object of investigation as an example case or as a prototype of xy. Their descriptions can sometimes be used as a first datum and/or as a heuristic to clarify the subject matter. In addition, a basis can be created to trace the subjective knowledge perspective with regard to the chosen topic (for example, regarding values, partisanship, expectations of normality, appetites, aversions, taboos, and so on). Through a de-centered view of the "tunneling" and the "blind spots" in one's own perspective, a freer handling of perspective assumptions and divergences can be achieved, and the possibilities and margins of one's own view can be expanded, diversified, made more flexible, and shaped in a moment of reflection.

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## 8. Subject of Knowledge – Methods – Object

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A long-standing issue in my preoccupation with scientific activity in the social sciences is the relationship of the epistemological subject (m/f/d) with the chosen object of knowledge. This relationship is constituted by certain methodologies, methods, and procedures of knowledge. Since the 1980s, my beliefs have changed quite a bit.

I approached the topic of subject-object mediation at the philosophical epistemology level. In my first reflexive positioning, I had made friends with the Marxist idea of reflection – albeit with certain reservations. I had registered very well that, even for Soviet advocates of this view, certain relativizations of the absoluteness and objectivity of products of cognition became apparent because of their inevitable production by human cognitive activity (for example, Rubinstein 1957).

Ideas that placed the side of the subject in the epistemological relation entirely in the center emerged. Multiple variants of constructivism focused on the generation of human cognitive content by the "conscious system": neurobiological, cognitive-theoretical, cybernetic, linguistic, social, and cultural-theoretical concepts of the production of world perceptions and interpretations gained considerable followers. Arguments for connecting object-related constructs to a reality beyond the subjective world of autofictions became defensive. Many researchers with epistemological interests turned into avowed constructivists. Whether this positioning also had consequences in the practice of social science research, however, often remained nebulous. At times, this seemed to have a similar significance as the affiliation of a research person to the Catholic or the Protestant confession. I found constructivist considerations convincing, but I did not want to give up the idea of the existence of an external-material world completely without being able to support the truth of this assumption with proof. Above all, I wanted to find a way

in which the constructivist world of thought had not only the status of a declarative foreword but also one in which it could be grounded in methodological research practice.

One idea to get closer to this goal is to connect with the concept of perspectivity. Knowledge – to me that seemed and seems to be compulsory – cannot be separated from the standpoint of cognition. This has been made plausible in different disciplinary theoretical worlds (close to me: Goodman 1978; Fleck 1979; Kuhn 1962; Mannheim 1982). A standpoint of cognition is always shaped by a subjective component (place, time, preconceptions, thinking style, attitudes, group ties, and so on). Knowledge is tied to perspectives/perceptions. In connection with the idea of using differing or contrasting information, which I had found to be a central heuristic idea in Gregory Bateson (1979) and in the context of grounded theory methodology, the further thought was to use the comparison of (participant, observer, and so on) perspectives as a methodological tool of cognition and thus transcend the limitations of individual/specific perspectives. My contact with the social psychological action theory group around Urs Kalbermatten and others in Bern (Switzerland; see von Cranach et al. 1982) had also served to provide impetus for this. This was related to the idea of triangulation as a methodology of knowledge. However, this principle is not to be understood here in the sense of a validity test, since there is no binding standard beyond the cognitive or linguistic. The aim here is to diversify the ways of looking at an object or a field of objects and their respective connection to presuppositions of knowledge from the side of the subject. I expect both an epistemological and a socio-worldly practical gain from this.

From a theory of science perspective – as a result of my disciplinary socialization – I had to deal with normative methodologies as well, for example, with critical rationalism (Popper 1992) as a widespread creed in psychology. On the one hand, there are epistemological and methodological reservations (Klaus Holzkamp was a central thinker for “left” psychologists; Holzkamp 1972, 1983). On the other hand, I was impressed by the elaboration of linguistic and logical systematics in the traditions of logical empiricism and critical rationalism. Here, my sense for the immediacy of cognition and language, the importance of linguistic clarity and unambiguity in scientific conceptualizations, the distinction between theoretical and empirical or observational terms, and different levels of language and their problematic interrelation were sharpened – for instance, the search for a “basic language” or the mediation of theoretical and empirical terms and sentences via indicators or operationalizations.

I was impressed by Thomas Kuhn and Ludwik Fleck’s historicizing view of science, which took a completely different look at scientific knowledge: a descriptive/historicizing perspective, the reconstruction of how knowledge progresses in different disciplinary fields. In this context, the focus was mostly

on the natural sciences, or in Fleck's case on medicine. Their theses posed a challenge to the usual methodologies, which are essentially characterized by a system of rules based on formal logic. They claimed serious inconsistencies between research logic and scientific practice: Usually postulated norms of scientific logic had been systematically contradicted by recognized scientists. Scientific theorists tried to close this rationality gap with new conceptions of scientific theory systems, in which the traditional strict falsification principles of Popper's doctrine were no longer of central importance: the so-called structuralist conception of theory (Stegmüller 1979; and others). In this spirit, there were also interesting new designs in psychology and its disciplinary theory building – for example, describing psychology as structured in and by research programs. In doing so, analogical references are made to Kuhn's conception of paradigms (for example, Herrmann 1976).

A central question emerged in this context: Can science theory – in the usual sense of the term theory – be understood as a system of statements with an empirical field of application (in research processes, research action) that can also fail on the basis of experience and is, therefore, falsifiable? Or is science theory a persisting “science doctrine” that is elevated above the contradictory data of observed scientific processes and which has the character of an extremely stable catalog of imperatives?

The historicizing and structuralist approaches remained limited in their consequences; in psychology, they were mostly perceived as exotic irritations of normal operation. Their development and implementation soon fell into a deep sleep; by now, only historians of the discipline of psychology are interested in those approaches. The situation is different in the field of sociology and ethnography of science, where a flood of studies on the socially and institutionally integrated work of researchers has been unleashed (Knorr-Cetina 1981; Latour and Woolgar 1979; and so on). They are concerned, among other things, with the reconstruction of work practices and modes of communication in local research groups and laboratories. A lot of things there do not go by the textbook.

For me – in the context of the aforementioned scientific-theoretical redefinitions – there was now room to focus on the reconstruction of the research practice of real research people. I no longer tried to conceptualize the subject of knowledge merely abstractly as a “conscious mind” or as a brain swimming in a nutrient solution, but rather as a “person with body and soul,” with a cultural imprint, a history, socialization, a personal attitude, corporeality, emotionality, and an “appeal.” He or she encounters his or her research objects by interacting with anthropologically and structurally identical, but personally different, research partners in a context-dependent social exchange.

A basic idea behind these considerations was that the criteria of scientific logic should not have top priority for knowledge methodology and methods.

Rather, a broader and more complex system of appropriateness considerations for psychological research is at issue. In this context – as an integrative term – stands the notion of the appropriateness of the methods (Bergold and Breuer 1987, 20-52), which is differentiated into several aspects in my methodological positioning.

The “conception of man” underlying the research (which often remains implicit) is important here: the idea of the “Other” constructed through methodological access (See Breuer 2005a, 57-102). In ethnology, this is often referred to as *othering*. It concerns the decision as to what is important and what is unimportant about the object of study. Which characteristics are included in its modeling? Here, the idea of the “reflexive subject” as the center of the research (Groeben and Scheele 1977) serves as a guideline for me. At the anthropological level, I assume that my research partners have the same qualities and abilities that I claim for myself (as a researcher and everyday person). Therefore, I attribute to them the ability to think about themselves, their life situation, and life history and to provide information. Not all of this information has to be “correct,” but it is considered significant for the theory/construction – even if divergences occur (see below). For example, I pay attention to the ways of speaking, the terminology, and the vocabulary that are used by the research partners and take them into account for the formation of constructs in my theory.<sup>19</sup> Further aspects that come into play include the conditionality of agreement, the interchangeability of the roles of the research subject and the research object, and the self-application of the developed theory by the researchers themselves. The use of these criteria as a guideline should mean that this is how the ideal form of research situations is characterized and that shortfalls are possible in principle but should be justified.

Such characteristics of the image of man can be implemented successfully in research scenarios that show features of the everyday world: in those situations rather than in strongly condition-controlled artificial settings of experiments that are considered in psychology as the royal road to the justification of knowledge. Researchers enter the social world of their research partners and/or use research formats that connect to their everyday familiar practices. The methodological orientation veers towards field research, ethnography, participant observation, conversational communication with the research partners, storytelling, and the like.

Furthermore, for the construction of theory and the presentation of research results, the aspects of the image of man and the everyday world are important: The reconstruction of the worlds and perspectives of the research

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<sup>19</sup> Constructs of first and second order: following Edmund Husserl and Alfred Schütz in their *Phenomenological Sociology*. When coding conversation or interview texts in grounded theory, attention should be paid to what are called *in vivo codes*, also thought of as terms that can be used for theory building from the vocabulary of the research field.

objects is not only about their reproduction but also about their screening, conceptual systematization, and contrast (of perspectives). The design of research situations and the presentation of research results offer the potential for research partners or members of the research field to gain knowledge for themselves, thus enabling them to “decenter” their previous views, patterns of action, and so on (“I have never seen it like this before!” and so on) – self-enlightenment with regard to their life/situation and its contingencies, their realized and unrealized possibilities of action, and their “spaces of possibilities.”

Social science data are usually generated through interactions between researchers and their research partners. There are exceptions to this rule, which I will not discuss here. Thus, data not only represent characteristics of the object, but they are also products of a social situation and an interactive event, which is influenced and sustained by several (at least two directly involved) participants. In the social sciences, there is a widespread consensus to classify such interactive effects as cognitive interferences and to try to control or exclude them through technical means. The fact that it very much makes a difference which person, with which characteristics, conducts a conversation, enters the research field as an observer, and so on, seemed significant to me early on. I was clueless at first regarding the question of how to deal with this and what methodological consequences should be drawn from it. Most people suggest not getting too involved with the field and the subjects of observation there: to beware of what we call “going native.”

In the 1980s, I discovered the book *From Anxiety to Method in the Behavioral Sciences*, by Georges Devereux (1968). It is one of the books that made the most lasting impression on me in my life of scientific reading. Here, I found a groundbreaking conceptualization of the phenomena of the individual and interaction, which interested me from a methodological standpoint. The fact that this was a psychoanalytic world of thought did not bother me. The central concept of countertransference from psychoanalytic teaching seemed highly plausible to me, and comprehensible in its research-methodological turn when transferred to the (own) person of the researcher and the interactive research contact. Because of the widespread fear of contact with psychoanalysis, I have usually said in psychological discourse contexts that one need not become a psychoanalyst to work with this idea: There are also other vocabularies and worlds of thought in which the intended can be made plausible. For my presentation here, I will stick with the terms Devereux suggested in the aforementioned book.

Researchers react emotionally and affectively to what they face and encounter in the research field: problems and issues, research partners, field members, etc. Reactions are determined, among other things, by the researchers’ own characteristics as a person and their socialization history. Certain things that they encounter, or that they are confronted with, are pleasant

or unpleasant to them: they are attracted or frightened by those things, they are willing to watch or they prefer to look away. Sexuality, violence, and dying and death, for example, stand out as easily comprehensible subject areas in which such things come about. In principle, however, this applies to every thematic research problem in a more or less distinctive way. Fear of such things is – according to Devereux – a prototypical reaction of the researcher, on the basis of which the methodological consequences can be spelled out. Research methods serve, among other things, to bring the researcher's fear of the problems being worked on and the people being studied to a manageable, bearable level. Central to this are the questions: How close do I let the problems and research partners get to me, what is the nature and locus of the partition between the focused object and me as a researcher? Do I choose methods of distance or proximity (questionnaire, online contact, or participant observation in the field)? Do I let the object reach into me in a way that triggers thoughts and feelings? How much do I wish to be disturbed by it? Where do I draw my limits of what is tolerable? How attentive am I to resonances from the research contact that are triggered in/on my body? Where do I place my own affections, insecurities, attractions, associations, and bad dreams?

The reading of such bodily resonances is an approach that Devereux – in decided opposition to the usual concepts of empiricism or data – emphasizes as the central approach in his kind of research. In doing so, an empathic notion of experience becomes important, which opposes the usual effort to desensitize (prototypically through the use of technical apparatuses of measurement, and so on) and contrasts it with a concept that emphasizes the empirical nature of the experience and of the bodily engagement (Breuer 2000b, 33-50; 2005b, 99-118).

The phenomena of the research field result in specific triggers or irritations in/on the body of the researcher – and these have to be registered self-consciously and attentively. They can always be read in two directions: with regard to the characteristics of the researcher and with regard to the properties and characteristics of the focused object. A high degree of self-attention is required here, and ways must be found to cultivate sensitivity to such phenomena: To take note of them and to deal with them interpretively and progressively. For the research style of grounded theory, this opens a window of insight that can be heuristically enriching for the generation of theories.

What is true for the researchers is also true for the research partners and the research field. The researchers release something there: emotions, affects, thoughts, actions, and attitudes. This is related to their personal and structural characteristics: age, gender, ethnicity, roles, ascribed partiality, appearance, clothing, demeanor, smell (in sensual and metaphorical terms), and so on. The modes of action of field members are fundamentally different in the presence of researchers than in their absence. Information is

addressed to researchers with particular characteristics that make certain things easier or more difficult, possible or impossible (access and aversion, trust and distrust, openness and closedness, and so on). Devereux uses the term *stimulus value*: an appeal that researchers have for their field or the members in that field. This, too, can be seen as an opportunity for insight into the object instead of a hindrance and disturbance to knowledge – provided that attention, sensitivity, and productive interpretative work are employed regarding such phenomena.

In this context, methodological procedures are needed to guide, inspire, and help understand such kinds of empirical approaches. They require a proper sensorium, the cultivation of self-attention, careful documentation, and artful interpretation (Breuer, Mey, and Mruck 2011, 427-48). In RGTM, we work with self-reflexive standpoint clarifications, such as through detailed explanation of topic-related pre-conceptions at the beginning of the research and continuing throughout the process, using the personal research diary as a tool, with self-reflexive memos about field experiences (problematic or pleasant experiences, irritations, puzzling encounters, and so on), by means of sharing and joint reflection on experiences and data in a research or interpretation group, and possibly also with research supervision. However, these practices are not readily available from the beginning; they must be developed and shaped in a process of appropriation and organization.

What has emerged from the evolution presented here is a style of research that places the exploratory characteristics of grounded theory methodology in the foreground. The scientific work is complemented with an experiential approach that, in addition to the conventional “distal” data sources, also relies on bodily (“proximal”) moments of experience of the researchers and their self-reflection as a window of knowledge. This quintessence of the personal development path described here is presented in my “last book” (Breuer, Muckel, and Dieris 2019), a textbook on reflexive grounded theory, which I recently published with my two former collaborators and co-authors Petra Muckel and Barbara Dieris in 2019 (for a book review in English, see Offenberger 2019, 309-15).

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