

## **Epistemic Cultures in Sociology Between Individual Inspiration and Legitimization by Procedure: Developments of Qualitative and Interpretive Research in German and French Sociology Since the 1960s**

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**Abstract:** How do sociologists know what they know? Although sociology around the world is deeply shaped by processes of internationalization and shares many common points of reference, strong regional, linguistic and cultural differences persist. This concerns the general production of knowledge, applied theories, and methods as well as questions posed. In this contribution, we discuss the development of and differences inherent in epistemic cultures in qualitative and interpretive research in French and German sociology since the 1960s, drawing our observations from a document- and interview-based research project we directed from 2012-2014. Epistemic cultures are conceived as the forms, ways and practices of producing and legitimizing scientific (sociological) "output" involved in sociological knowledge-making. The production of knowledge is the core "action problem" that sociological inquiry confronts. This problem is addressed in rather different ways. Overall, French qualitative sociology tends to place trust in researchers' skills, competencies and inspirations in order to establish new knowledge. German qualitative and interpretive sociology prefers to establish legitimacy by focusing on well-defined procedures. We illustrate the unfolding of this difference between French- and German-based qualitative sociology in the early 1960s and discuss how it shapes their further development. In doing so, we intend to contribute to the current reflexive movement in sociology.

### **Table of Contents**

- [1. Introduction](#)
- [2. Methodological Implementation](#)
- [3. Epistemic Cultures in Sociology](#)
- [4. Epistemic Cultures Between National Embedding and Internationalization](#)
- [5. Sociological Epistemic Cultures of Qualitative Research in Germany and France](#)
  - [5.1 Introductory remarks on the concept of "qualitative" and on the French-German comparison](#)
  - [5.2 Legitimization by procedure: The development of qualitative and interpretive social research in the Federal Republic of Germany](#)
  - [5.3 "In shape for the field": Individualized inspiration and qualitative research in France](#)
- [6. Concluding Remarks](#)

[Acknowledgments](#)

[References](#)

[Authors](#)

[Citation](#)

## 1. Introduction

For some time now, sociology in German-speaking countries has been increasingly preoccupied with its post-war history, a development with parallels in other countries. This interest derives support not only from efforts to gain assurance concerning one's traditions, but also from the ubiquitous tendency to archive the history of the discipline and/or from the contingent need to gain a better understanding of one's own actions. We conjecture that it is also driven by the increasing international and transnational contacts between sociological fields scattered all over the world. The contacts in question are in part voluntary and in part enforced by funding institutions and lead to the clarification of profiles and lines of tradition. [1]

Contemporary sociology exhibits a "reflexive turn" on many different levels (including the turns in social theory since the late 1960s as well as, for example, the questioning and criticism of its ethnocentric and anthropocentric foundations). Work on the history of sociology also contributes to this reflexive turn, albeit in a completely different way: the motives explicitly or implicitly driving the self-enlightenment of the discipline concerning its history are largely genealogical rather than epistemological or political ones. The reflexive turn is prompted by the experience that very different things are discussed under the heading of "sociology" both at the local and global levels. [2]

This diversity certainly affects a wide variety of elements. The fact that research topics command attention in different countries according to different cycles can be explained relatively easily by the respective socio-structural as well as political contexts and the resulting problem situations within which funding programs are implemented and social expectations are formulated, and which provide the impulse for sociological thinking. A further factor is that theories do not gain a following or achieve dominance at the same time in different countries, something which is aggravated by delays in translation—it is not yet the case that everything appears in English nor does so speedily. However, there is a further form of diversity which originates in developments in sociological methods of knowledge production and shows that differences also exist between the validity claims raised by sociological research and between the associated notions of the "scientific character" of the production of sociological knowledge. [3]

This is not surprising at first sight when viewed against the background of sociological research on science (specifically in the sociology of knowledge), especially when we take into account that the role played by "thought styles" and "thought collectives" within scientific disciplines as first described by Ludwik FLECK (1979 [1935]) also applies to sociology itself. At the same time, however, little is known about the actual cultural forms and structures exhibited by the production of sociological knowledge. On the one hand, for years, contemporary developments have been exhibiting an internationalization of scientific discussion, which seems to be shaped by the dominance of Anglo-American inventories of knowledge, canonical works and debates, and leads to processes of convergence or even homogenization in the production of sociological

knowledge. On the other hand, it remains unclear what role institutionally, linguistically and culturally bound—or, to use an imprecise generalization, "national"—traditions of knowledge production and circulation play in this process. This prompts the question of what differences as well as interrelationships exist between *epistemic cultures* (given our concern here: *epistemic cultures of sociology*). [4]

In the present article, we take as starting point a comparative perspective on the field of qualitative and interpretive methods of social research in Germany and France, asking whether and to what extent differences between the respective epistemic cultures can be observed and, if so, how they are structured. Our arguments are based on results of an empirical study conducted between 2012 and 2014, funded by the Deutsche Forschungsgemeinschaft [German Research Foundation] (DFG).<sup>1</sup> In the following, we concentrate on differences between styles of knowledge generation founded in the respective epistemic cultures and must leave aside internal differentiations, cross-field affinities and the development of methodologies within individual sub-disciplines. These issues will be addressed in later publications. [5]

We employed the concept of epistemic cultures in sociology in the context of this project with reference to one specific sector, namely, qualitative and interpretive social research. Our main questions sought to understand the scope and delineation of this field of research as a scientific venture as well as the conceptual and methodological justifications for knowledge claims staked through research practices. In the course of our research, it quickly became apparent that this always includes the (generative) issue of how to attribute meaning to scientific *action* and how to solve associated *action problems*—or, to be more precise, the question of scientific knowledge *itself* as an action problem, i.e., indicating and concerned with challenges to be overcome in order to be considered meaningful under conditions not yet comprehensively defined. Such an approach seemed appropriate for purposes of uncovering the courses of development, conditions of emergence, frames of reference and self-understandings of our discipline beyond stereotypes and simplifying judgments. [6]

In the context of this article, "qualitative" or "interpretive" methods refers to commonly understood non-standardized procedures of empirical sociological research such as those employed both in the context of data production (e.g., semi-structured interviews, participant observation methods or audio-visual data recordings) and in the area of data analysis (interpretive procedures such as sequence analysis and coding). In German-speaking countries in particular, a mostly clear distinction is made between qualitative and interpretive approaches. Even though we mainly speak of "qualitative methods" for the sake of readability in the following, we are aware of the—certainly not uncontroversial—

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1 The present contribution was made possible by the project "Epistemic Cultures in Sociology,," funded by the DFG in 2012-2014 under the reference numbers KE 1608/2-1 and PO 1484/2-1. The project collaborators were Denisa BUTNARU, Maya HALATCHEVA-TRAPP and Oliver KIEFL, as well as Julia PETERS as student assistant. We would like to thank Jessica HUBATSCH for her reliable support in editing texts. Translations of German and French quotations by Ciaran CRONIN and us.

differentiation sometimes associated with this terminology. In German sociology, for example, some authors tend to use the label "qualitative" only for such non-standardized approaches that refer to criteria of scientific validity established by positivist (quantitative) methodologies; the label "interpretive" is reserved for "open" hermeneutic procedures which establish a different rationale for analysis (e.g., ROSENTHAL, 2005, pp.13-26). [7]

Regarding the epistemic cultures in sociology in the two countries, we assumed firstly that qualitative social research was established from very different starting positions in the respective sociological fields and that these starting positions played a major role in the subsequent, significantly different courses of development pursued in sociology in France and Germany. Our initial speculation was that, in the German-speaking world, qualitative social research developed against the background of well-established quantitative research, with a tradition of "interpretive sociology" extending back to classical theorists (in part following WEBER, in part drawing from hermeneutics) and a specific form of the critique of positivism in the context of critical theory. We assumed that this initial situation led to a heavy emphasis on the relation between theory, methodology and method in sociological practice, as well as on the "scientific soundness" of the methods of data analysis. Both these concerns would then shape the unfolding and the institutional stabilization of qualitative methods.<sup>2</sup> [8]

In France, according to our conjectural thesis, the demarcation between theory, methodology and methods, as well as the opposition between quantitative and qualitative approaches, played a less important role. Instead, we expected to find research-oriented theoretical paradigms with integrative methodological approaches advocated by prominent leading figures in the discipline. We suspected that, in France, the connection between research questions and the collection of data (especially including field research) would feature prominently against the background of structural anthropology and its methods as a well-established discipline and of the DURKHEIMian paradigm, while the question of securing the objectivity of qualitative analyses would play a subordinate role. [9]

Finally, we assumed that, against the backdrop of an unclear mixture of forms of internationalization of sociology and the continuation of "national traditions," developments in the respective fields of qualitative research would not exhibit a shared vanishing point on which the two epistemic cultures can be expected to converge in the short term. In summary, the principal initial questions we raised were the following (for a detailed account, see KELLER & POFERL, 2010):

- When and how did the topic of "qualitative/interpretive social research" appear on the agenda in the respective fields of sociology?
- What were the associated objects, epistemological assumptions, socio-theoretical and methodological foundations?

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2 On the thesis that a comparison between US sociology (sociopolitical engagement) and German-speaking sociology (epistemologically grounded distancing) reveals some differences as regards qualitative research, see, e.g., BETHMANN and NIERMANN (2015).

- Which methods of data collection and data analysis were developed and what changes did they undergo during the period under review?
- What demarcations and positioning processes took place and which approaches developed within the discussion over qualitative methods (internal structuring)?
- What status were qualitative methods accorded in relation to theory construction and the use of quantitative methods in sociology?
- To what extent was reference made to theoretical and methodological approaches in other neighboring disciplines in the humanities?
- What role do generational effects, professionalization processes and cyclical process play in this context? [10]

We begin with a brief account of the methods used in the study (Section 2). We then discuss the concept of epistemic cultures (in sociology) (Section 3), address the question of the international homogenization or heterogeneity of sociological knowledge production (Section 4), and discuss differences between the fields of qualitative and interpretive methods in sociology in Germany and France (Section 5). We will conclude our contribution with a few brief summarizing remarks about "research as a problem for action," and about the two different modes of addressing such a problem via different regimes of justification in German and French sociology. [11]

## 2. Methodological Implementation

The study presented was conceived at the theoretical and methodological level as a sociology of knowledge approach to discourse (SKAD) (KELLER, 2010 [2005], 2011; KELLER, HORNIDGE & SCHÜNEMANN, 2018). SKAD is a research program for analyzing social relations and politics of knowledge. In the present case, focus was on relations and politics of knowledge in sociology. SKAD takes its orientation from the methodology of interpretive-hermeneutic social research. It does not follow a standard procedure. Rather, adaptations have to be made in the light of the questions being investigated and the data that are available or that can be produced. When collecting, producing and compiling data (mainly text documents and interviews), SKAD makes use of the helpful proposals formulated in the methodology of grounded theory for a sampling procedure based on theoretical criteria and the respective analytical steps (STRAUSS, 1987). Analysis of the individual documents deemed to be central was carried out with the aid of an analytical grid to ensure faster text processing and as a procedure of abductive category formation (REICHERTZ, 2013a [2003]). Especially during the first months of the project, joint working sessions and discussions of the project group were held to examine complete texts. The later analyses were then conducted by the French and German research assistants, respectively, and focused on selected text segments.<sup>3</sup> [12]

3 For an explanation of SKAD, see KELLER (2011, 2013 [2003]), and KELLER et al. (2018). Since our subject matter was discourses in sociology, we adopted a corresponding discourse-analytical design. Apart from approaches that focus on the analysis of content, there is hardly any literature on proven methods for exploring extensive documents in interpretive social research. An important exception is PRIOR (2003).

The initial implementation of the project focused on the collection and analysis of documents. This was supplemented by a large number of interviews with protagonists of different approaches and discussions, which were used as sources of information and contextualization. We drew upon specialist publications in sociology from both countries as documentary evidence of the introduction and justification of methods of qualitative or interpretive social research. Since it was not feasible to provide a complete analysis of all relevant sociological publications that appeared between 1960 and 2000, several selection criteria were defined. The project focused on the academic sociological discussion about qualitative or interpretive methods as conducted in publications devoted to the foundation and dissemination of approaches and methods. These included in particular monographs, anthologies and journal articles from the years 1960-2000 with a clear reference to qualitative social research. No further restrictions were made to specific areas of qualitative social research—for example, biographical research, sociological ethnography, etc.—or to individual methods of data collection and analysis (for example, interviews, observation or document analysis). We did not examine the use of qualitative methods in applied contract research or other research outside academia. [13]

The texts analyzed dealt mainly with basic questions of qualitative and interpretive sociology and its methods. We excluded examples of applications and subject-related empirical studies in which the focus was on discussion of results. In compiling the corpus of materials to be studied, we conducted a general keyword-based catalog search (covering such terms as "qualitative," "interpretive," "method," "procedure," "interview," "ethnography," etc.) and supplemented this with an exhaustive review of tables and summaries of contents from specialist journals. This supplementary procedure proved to be particularly helpful for the 1960s and 1970s, since, on the one hand, corresponding guiding concepts of qualitative research became established only step by step, but on the other it was often impossible to tell from titles alone whether and how the corresponding contribution made reference to methodological strategies. For the French case, it proved necessary to also take journals with interdisciplinary and transdisciplinary orientations into account, because one of the characteristics of the discussion of methods in sociology in France was (and remains) that it does not have a clear place in publications devoted exclusively to sociology. We also consulted relevant publications on methods (introductions, lexicons and manuals) and sociological textbooks devoted to methodological questions. The excellent archives at the Bavarian State Library in Munich and at the library of the University of Strasbourg ensured us access to the relevant data. The significantly higher degree of digital accessibility and archiving of French publications from the 1960s and 1970s proved to be especially helpful in this regard. [14]

The publications from the years 1960 to 2000, selected in accordance with these guidelines, were analyzed successively with reference to the following questions: Who is writing? What conceptions and goals of qualitative-interpretive research are developed and presented? How is the role of the researcher or researchers conceived of? What methodological approach is introduced and how is it communicated and justified? The detailed analyses of a total of 576 text

documents (372 from Germany, 195 from France) were recorded in comprehensive work reports. In addition, contributions in which approaches were first introduced or presented as exemplary instructions for field research were selected for detailed analysis. In this process, analysis of individual text sequences were used to develop categories regarding figures of legitimation or justification for the necessity and quality of qualitative research presented in these passages. In addition we interviewed a total of 63 colleagues who were either already retired or on the point of retiring, as well as representatives of the middle and younger generations. The information thereby accumulated provided important clues especially concerning institutional backgrounds, competitive relations and relationships between the different approaches and their protagonists.<sup>4</sup> [15]

### 3. Epistemic Cultures in Sociology

The term "epistemic cultures," used by Karin KNORR CETINA (1999, see also LEPENIES, 1988 [1985]),<sup>5</sup> among others, has achieved widespread attention in a variety of scientific disciplines over the past decade (SANDKÜHLER, 2014). Its broad German reception is probably related to the interdisciplinary DFG Collaborative Research Program 435 "Wissenskulturen und gesellschaftlicher Wandel" [Epistemic/Knowledge Cultures and Social Change] and to the relevant study by Karin KNORR CETINA (1999) on high-energy physics entitled "Epistemic Cultures." However, the term can be traced back to Friedrich NIETZSCHE, used a short time later by Max SCHELER and also (if not verbatim, with similar conceptual nuances) by Ludwik FLECK (see FRIED & KAILER, 2003; ZITTEL, 2014). [16]

The concept of "epistemic cultures" emphasizes the importance of specific relationships between social actors, practices, institutional settings and material factors in the process of generating knowledge. The social structuring of epistemic cultures has been an enduring preoccupation of science studies (WEINGART, 2003). Initially, the "hard" (natural) sciences and the "soft" (human) sciences and their respective research communities were contrasted with each other, following Charles P. SNOW's thesis of "The Two Cultures" (1998 [1959]). Wilhelm DILTHEY (1977 [1894]) had previously distinguished the two epistemic cultures and correlated them with "explanation" and "understanding" respectively. More recent science studies has employed a conception of science tailored to scientific disciplines—so-called "disciplinary cultures"—and the specific ways in which they generate knowledge. In this approach it is a question of the extent to which a definable circle of scientific actors in a specific field of research develops

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4 We will discuss individual developments and the interviews in greater detail in a future book.

5 The word *Wissenskulturen* used here in the original German text could also be translated as "knowledge cultures." KNORR CETINA's book has the more specific English title "Epistemic Cultures"; the German title of that book is "Wissenskulturen," a term also used by other German authors and texts referred to (e.g., LEPENIES, 1989a) without particular reference to (and indeed in some cases long before the appearance of) KNORR CETINA's study. In the following, *Wissenskulturen* has been translated as "epistemic cultures," since we are dealing here only with scientific knowledge (unless otherwise indicated).

a specific way of producing, evaluating and circulating knowledge and in doing so differentiates itself from other (likewise scientific) actors. [17]

In the literature, the term *Wissenskulturen* [epistemic/knowledge cultures] is used, on the one hand, in a broad sense, as, for example, when Wolf LEPENIES (1988 [1985], 1989a) made a comparative study of the scientific cultures of entire countries (and their sociologies). In doing so, he painted a complex picture in each case, one which cannot be assimilated to overall national cultures, but shows the extent to which different positions confront each other, albeit with distinct "national characteristics" (1988 [1985], p.3). It would be suggestive, but nevertheless mistaken, to interpret the German title of this study—"Die drei Kulturen" ["The Three Cultures"]—as referring to the three countries studied and the history of sociology in each of them. However, what is meant is something different, namely that the social sciences constitute a "third culture" alongside the natural sciences and the humanities. The particular distinguishing feature of such a "third culture" is precisely that it includes the opposing epistemologies of the two other "cultures," And only then does the diversity of the socio-structural and institutional contexts which accounts for the fact that this "third culture" finds very different expressions in each of the countries mentioned come into play. [18]

On the other hand, a narrower conceptualization of "epistemic culture," which refers to how research processes are fashioned in concrete, material terms, can be found in Karin KNORR CETINA's (1999) pioneering work and in science studies. As regards the production of scientific knowledge, here the concept of epistemic culture aims at

"those amalgams of arrangements and mechanisms—bonded through affinity, necessity, and historical coincidence—which, in a given field, make up *how we know what we know*. ... I explore epistemic features such as the meaning of the empirical, the enactment of object relations, the construction and fashioning of social arrangements within science. The term 'epistemic' is intended to refer to cognition; thus it is a question of those strategies and principles that are directed towards the generation of 'truth' or equivalent cognitive goals" (p.1).<sup>6</sup> [19]

Against this background, *epistemic cultures in sociology* can be understood in our conception as more or less clearly distinguishable discursive and practical ways of producing, evaluating and communicating specific (sociological) knowledge. Components of such epistemic cultures are ways of doing things, including conducting research, developing arguments, publishing, using techniques and objects, distinguishing legitimate from illegitimate references, forming traditions, declaring certain works and figures to be canonical, developing forms of

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6 Note that the final sentence of this quotation is from the German edition—KNORR CETINA (2002, p.11)—and does not appear in the original English edition. The original English title of KNORR CETINA's book, "Epistemic Cultures," focuses more on the processes and practices of scientific knowledge production than the title of the German edition, "Wissenskulturen" (KNORR CETINA, 2002). KNORR CETINA (2005) uses both terms in English. "Knowledge culture" then refers to general modalities of knowledge production in societies (like in "knowledge society"); "epistemic culture" refers to scientific knowledge making in modern societies. Unless an English translation is cited, all translations of quotations from German or French sources are our own.

mediation, having specific forms of access to resources and being integrated into "Order[s] of Discourse" (FOUCAULT, 1972 [1971]).<sup>7</sup> [20]

Conceiving of epistemic cultures in this way guards against a narrow concept based solely on a theory or practice, on the one hand, and against and over-stylized, homogenizing concept, on the other. An example of the former would be a perspective directed exclusively to the observation of the concrete activity of research in real time. Such an approach fails to recognize that this constitutes a field of events which both emerged from and is integrated into a broad network of established modes of doing, accepted forms of publication, research questions deemed to be relevant, and so forth.<sup>8</sup> In contrast, we can speak of a homogenizing over-stylization when the heterogeneity, non-simultaneity, dynamics or even contradictoriness and fragmentation of the production of sociological knowledge is more obscured than it is grasped through a simplified attribution of a "typical national character."<sup>9</sup> The forms, scope and results of the production of sociological knowledge depend, therefore, not only on concrete research infrastructures and actual research procedures, but also on the perspectives and the horizons of experience, expectation and possibility opened up in a flexible structure of relations by the available epistemological positions, theoretical paradigms, citation traditions, existing controversies and coalitions, established methodological standards and developed methodological options. Such a constellation can form a thoroughly heterogeneous or even conflictual topography within a particular academic tradition while nevertheless sharing common features that distinguish them when viewed from an external perspective. [21]

At the same time, we did not rule out the possibility that the elaboration of common features on a more or less abstract level implies a process of typification or stylization that allows for alternative possibilities. Cultures are not homogeneous formations; on the contrary, they are characterized by niches, conflicts, sub-cultures and counter-cultures. The concept of epistemic culture, understood (not as a name for entirely different totalities but) as involving the dynamic interweaving and concentration of specific forms of production and justification of knowledge, promotes a sensibility for such forms of plurality and heterogeneity, but at the same time enables us to reconstruct typical elements and patterns.<sup>10</sup> Moreover, epistemic cultures are not closed "containers"; rather,

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7 "Order[s] of Discourse" is the correct translation of the French original title of FOUCAULT's quoted book, instead of "The Discourse on Language."

8 This is not to imply that such a mode of access necessarily misses its object. On the contrary, we expressly want to emphasize the importance of this perspective for corresponding research questions. KELLER and POFERL (2018) present a variety of empirical-analytical approaches to the sociological analysis of epistemic cultures.

9 The danger of such simplifications is illustrated by an example from the French context. While Edgar MORIN's study from the early 1960s on the star system in the mass media (1962) was hailed as an informative, innovative and valuable contribution to sociological research on the mass media, it was at the same time excoriated immediately upon its appearance by an "opposing coalition" (namely BOURDIEU & PASSERON, 1963) as proof of an abdication of sociological analysis.

10 On this see also the helpful distinction in PETER (2001) between cognitive, social and discourse-historical dimensions of the history of sociology. By the cognitive dimension is meant

they are (co-)constituted in empirically determinable ways by references to the outside and by processes of exchange and the transcending of boundaries—in other words, they are situated in a relational "field" (BOURDIEU, 1988 [1984]; 1990 [1987]) or "arena" (STRAUSS, 1978). [22]

The concept of epistemic cultures echoes Ludwik FLECK's (1979 [1935]) idea of "thought collectives" with specific "thought styles." These notions accentuate processes of social structuring and the individuation of forms of knowledge production. In his pioneering work, FLECK had identified the decisive factors underlying such an influence. Differences exist, as is well known, for example, in economic and technical practices—different cultural styles of management, corporate culture, engineering activity and construction. But, as FLECK showed, they can even be observed for the only apparently objective process of knowledge production in the natural sciences. The factors FLECK emphasized actually exert stronger effects insofar as the disciplines under consideration here—social sciences and humanities—are more closely integrated into "society" and concrete social contexts, which, at the very same time, represent their primary subject matter. Think, for example, of well-established lineages of research paradigms, differences in language and culture, political funding of particular research areas ("in the name of national wealth"), the (for reasons of historical path-dependency) always specific institutional and organizational structure of a concrete scientific field, and historical events and experiences (as, e.g. fascism, wars, slavery), among other things. This mixture of factors gives rise to the "existential determination of knowledge" (MANNHEIM, 1960 [1929], p.239). [23]

#### **4. Epistemic Cultures Between National Embedding and Internationalization**

Against the background of her studies on molecular biology and high-energy physics, specifically on what she calls their "epistemic machinery" (KNORR CETINA, 1999, p.3), KNORR CETINA emphasizes the fragmentation of the processes of knowledge production in contemporary societies and argues that this also holds for all scientific disciplines. Therefore, science has "a geography of its own. In fact, it is not one enterprise, but many, a whole landscape—or market—of independent epistemic monopolies producing vastly different products" (p.4). It remains unclear (and it may be a function of the specific fields of knowledge in the natural sciences) to what extent the above-mentioned geography is also shaped by linguistic or more strongly "nationally" bound traditions. The term

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the disciplinary context within which certain positions in sociology are presented, including the theoretical traditions and paradigms by which they are influenced. By the social dimension is meant the institutionalization processes and their main actors in their domain of scientific activity. The discourse-historical dimension aims at the formation processes that the scientific field undergoes, which are the result of the developments of the conceptual apparatuses, categorical distinctions, the dominant or marginal perspectives, novel syntheses, and so forth. The controversies within the qualitative paradigm, and between it and its disciplinary environment, represent in all three dimensions effective positioning processes in the field of qualitative social research. Of relevance here are also reflections on the sociological history of sociology by Christian FLECK (1999; see also FLECK, 2011 [2007] and MOEBIUS, 2004). FLECK (1999) calls for "abandoning the focus on individual cases" and a turn to the investigation of the "authors regarded as "connectable" at certain stages" (p.61), of the groups, collectives and organizational units of sociology.

"national," which is used here as a (problematic) abbreviation for the institutional structuring of the scientific sector shaped by the state and science policy, stands for intellectual traditions and regional linguistic proximities, but not for a "national character." The historically established ideal of scientific "objectivity," as present in Robert MERTON's (1973 [1942]) formulation of its guiding principles, implied that scientific practice (also in the social sciences) is undergoing a simple process of internationalization and convergence. But in fact, quite the opposite can be observed. Scientific epistemic cultures are currently strongly influenced not only by their disciplinary research interests and unfolding, but also by their particular historical and social contexts and traditions. [24]

In the case of sociology and other social sciences, the importance of epistemic cultures certainly pertains vis-à-vis their classical past. There seems to be a widespread consensus on this in the literature. In histories of sociology, it has become customary to speak of different classical national traditions in the development of sociology. By this are meant guiding paradigms and, as a general rule, corresponding "founding fathers" whose work was pioneering and formative for the establishment of sociology and its advancement in the national scientific fields. Thus, WEBER stands for the German tradition of interpretive sociology, DURKHEIM for the French tradition of the sociology of "social facts" and the Chicago School and symbolic interactionism for American-style pragmatist sociology of action. [25]

The development of sociology might have been profoundly shaped by reformist social policies (as in the United States); it might represent, in the guise of a civil religion, the promise of rational government and a moral science of integration (as in France); or it might establish itself as an analytical academic discipline (as in Germany). In each case there emerged very different paths to what can and should be generated as worthwhile knowledge. Further, depending on how it was positioned in relation to given neighboring disciplines in the humanities in its diverse original manifestations different justifications emerged for sociology's internal logic, its specific approach to the world (WAGNER, 1990, 2004). At the historical level, therefore, one can certainly speak of the formation of sociological specialist cultures along country-specific lines, a development which can also be observed in the re-institutionalization of sociology in post-war Europe and its further development in the United States. Yet in addition to this, the history of sociology was characterized from an early stage by international reception movements, also often associated with the mobility of individual figures, for example, the prolonged research visits of Robert PARK, Talcott PARSONS and Everett HUGHES to Germany.<sup>11</sup> [26]

To speak of elements of national style, therefore, in no way implies a return to simple stereotypes. Even the obvious reference to the organization and resourcing of teaching and research—at universities, research institutions, etc.—by the state implies major differences in how sociology (and other disciplines) is

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11 On the complex and historically long-standing interconnections between national traditions in sociology and transnational contacts between sociologists, see, e.g., CABIN and DORTIER (2000), FLECK (2011 [2007]), KORTE (2011), LEVINE (1995), and WAGNER (2004).

put into operation. Additional differentiating factors are political-ideological constellations and public and intellectual cultures, not to mention socio-structural factors such as migrations, population concentrations, and the like. For example, discussion and research in sociology in the United States and France has devoted far more attention to questions of ghettoization and exclusion than their counterparts in German-speaking countries. And such strikingly different thematic orientations certainly remain important distinguishing features of the sociological epistemic cultures shaped by language borders. [27]

Around 1990, a more wide-ranging discussion developed about the extent and consequences of the internationalization of sociology. While there was unanimous agreement about the existence of national traditions, different processes of internationalization were identified.<sup>12</sup> In the context of this discussion, Martin ALBROW (1990, pp.6ff.) presented a comprehensive proposal for distinguishing between five stages in the development of sociology and how it is structured and restructured taking "universalization" as a yardstick. He speaks of the "universalism of the classical phase of sociology," which was followed by a phase of "national sociologies" (which probably found its clearest expression in German sociology under National Socialism). After the Second World War, a period of "internationalism" ensued, which in our view can be conceived in large part as a process of "transatlantification" in which UNESCO and American foundations played a significant role.<sup>13</sup> ALBROW identifies a fourth phase of so-called "indigenization," in which the successful institutional establishment of sociologies in the respective state-organized scientific landscapes initially led to an inward focus. MÜLLER and SIGMUND (1999) recount that sociological debates in Germany remained largely closed national affairs into the 1980s and became increasingly internationalized only in the 1990s. At the same time, they stress the guiding or orienting function of American sociology for the German-speaking context. Finally, according to ALBROW, the current phase of "globalization" established itself (see also GENOV, 1989a). [28]

The observations outlined seem plausible in many respects. Moreover, the increasing importance of world congresses of the International Sociological Association (ISA) and of other forms of worldwide scientific networking, as well as the global "career" of specific approaches and perspectives, provide evidence of the correctness of the diagnosis of the last phase. A major problem with this discussion, however, is the sweeping and vague character of its overviews, which are based on general descriptions of the development of sociological debates

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12 See, e.g., ALBROW and KING (1990), GENOV (1989a, 1989b, 1991a), LEVINE (1995), MÜLLER and SIGMUND (1999), PLATT (2008), the special issue of *Current Sociology*, edited by GENOV (1991b), and the debate between Richard MÜNCH (1995) and Jeffrey ALEXANDER (1995).

13 This is also indicated by the international conference "Voyages transatlantiques" [Transatlantic Journeys] held in June 2007 at the University of Nancy and hosted by the Goethe Institute in cooperation with the Universities of Strasbourg and Nancy and the sections on the history of sociology of the International Sociological Association (ISA), the Association Internationale des Sociologues de Langue Française (AISLF) and the Association Française de Sociologie (AFS), at which papers were presented on, among other things, early personal contacts between American, French and German sociology. FLECK (2011 [2007]) has presented a detailed study on the emergence of empirical social research and the early "transatlantic relations" between German and American sociology.

rather than on concrete research. By now, the international and transnational networking of sociology is certainly far advanced (CHARLE, SCHRIEWER & WAGNER, 2004). In the area of quantitative methods in particular, the logics of standardization allow these methods to be used in similar ways across national boundaries, with the construction of international and supranational statistical databases playing an important role in this respect. [29]

By contrast, discussions of the current situation in French sociology start from a complex situation that leaves a more ambivalent impression. According to several authors, aspects of the internationalization of theoretical paradigms, methods and problems, promoted by international associations and their politics of research funding, go hand in hand with the continuation of national research traditions, essentially distinguishable by language borders. They have thus coalesced into country-specific manifestations of the sociological field (BERTHELOT, 1998, 2003). At the same time, other authors emphasize the increase in American influences (HEILBRON, 2008; MARTIN, 2004). However, little is known about the precise forms that have resulted from these interconnected processes. In his book on the development of empirical social research in the Germany, WEISCHER sums up the situation as follows: the national character of the establishment phase of the sociological field "has not disappeared even in recent times, which is bound up in particular with the fact that sociological discourses are located within (nationally shaped) social-political discourses" (2004, pp.33f.).<sup>14</sup> [30]

## **5. Sociological Epistemic Cultures of Qualitative Research in Germany and France**

### **5.1 Introductory remarks on the concept of "qualitative" and on the French-German comparison**

There are numerous differences between French- and German-language sociology—just as there are between sociologies in other countries. Such an assertion is not based on essentialism of any kind, but on the simple observation of historical institutional developments. This already applies to the respective theoretical landscapes, notwithstanding frequent mediation processes, usually as a result of personal initiatives, which promote the selective mutual reception of specific paradigms. Another factor is translations of the work of leading authors, examples being the successful Pierre BOURDIEU reception in German-speaking countries and the—albeit very belated—reception of Ulrich BECK's work in France. The turn towards pragmatist sociology, which has played a role in Germany since the late 1960s, has been apparent in France only since the mid-1990s, where it assumes markedly different forms. Moreover, the respective professional societies are a very recent phenomenon in France, whereas they have a long tradition in Germany. In fact, sociology found a much more secure institutional foothold at universities and research institutions in post-war Germany than in post-war France. The resulting contrasting institutional structures of the university and research landscape in the two countries foster quite different

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<sup>14</sup> With reference to professional fields, see also the international study by LAMNEK (1993).

teaching and research contexts. While in France today one can hardly keep track of the handbooks on *enquête de terrain*, that is, on sociological-ethnographic field research, in German-speaking countries one can count them on a single hand. The situation seems to be the reverse regarding general introductions on qualitative methods. [31]

The specificity of developments in science in Germany and France has been described not only for sociology but also for other disciplines, such as history (PLAMPER, 2004; RAPHAEL, 2005) or *Volkskunde* [German ethnology] and French ethnology with their respective traditions of field research. The studies in question largely confine themselves to simply pointing out the differences (CHIVA & JEGGLE, 1987). For quite some time, the sociology of intellectuals has offered more specific descriptions of the different modes of organization and functioning of public spheres and intellectual fields (e.g., GRUTZPALK, 2003). This includes particular political interests and affinities, as between DURKHEIM's sociology and the French government's interests in consolidating the French Republic. Another example with unique forms in France and Germany was the struggle of sociology for recognition vis-à-vis philosophy and history in the relevant scientific field (featuring especially underhanded and personal attacks of French philosophers against DURKHEIM or, in German contexts, the need to distinguish sociology from the historically oriented *Geisteswissenschaften* [humanities] and *Kathedersozialismus* [academic socialism], which was a major concern in Max WEBER's methodological writings).<sup>15</sup> Corresponding developments have also been described for classical sociology in both countries in particular (e.g., BERTHELOT, 2003; KORTE, 2011), although they can in no way be reduced to this aspect alone. As mentioned above, WEISCHER (2004) had pointed out that national contexts remain an important feature of the specific character of sociological cultures and explained this primarily in terms of how they are embedded in social policy. [32]

However, what makes the comparison between French- and German-language (qualitative) sociological research in particular so interesting? More wide-ranging analyses in social and cultural history that refer to the modes of production of (not just sociological) knowledge have repeatedly distinguished between the characteristic "Cartesian" scientific rationality of French culture and the "romantic," "metaphysical" or "hermeneutic" depth of the German interpretation of the world. In Cartesianism, the cognitive subject stands over against the world and, from this juxtaposition, systematically builds knowledge of the world in a rational, reason-guided manner based on a secure foundation that grows step by step. In contrast, romanticism, metaphysics and hermeneutics are described as modes of access to the world that can only know and understand what is already presupposed by their interpretive activity, since they are based on theoretical-philosophical or metaphysical knowledge of what the world is, of what constitutes its essence, or of its interwovenness with the being of the object of knowledge.<sup>16</sup>

<sup>15</sup> See, in addition to LEPENIES (1981, 1988 [1985]), also WAGNER (1990), and WAGNER, WITTRICK and WHITLEY (1991).

<sup>16</sup> See, e.g., MÜNCH (1986); on the complexity of the development of science in Germany and France in the nineteenth century, see KOPPETSCH (2000); for a different epistemological

Therefore, we ask: shouldn't this diversity—assuming it can be empirically observed—be reflected in the forms of knowledge production?<sup>17</sup> [33]

In the 1930s, Raymond ARON (1979 [1934-1935]), for example, pointed out the difference between German humanistic sociology and French empirical-positivist sociology. More recently, Jean-Claude PASSERON and Jean-Louis FABIANI (2013) have once again addressed the differences between the two sociological fields against the background of ARON's work. In his groundbreaking studies of the "three cultures" of the establishment of the social sciences in England, France and Germany, Wolf LEPENIES (1988 [1985], 1989b, 1989c, 1989d) had analyzed the complex relationships and demarcations of the three different sociologies in the founding phase of the discipline and consistently highlighted the specific features of the respective scientific landscapes. However, it is not easy to determine whether these different foundations continued to provide effective orientations for the development of different sociologies after the Second World War. Thus, vis-à-vis the history of sociology in post-war France, BOURDIEU and PASSERON (1967) assert that it took up DURKHEIM's "heritage."<sup>18</sup> In contrast, others (e.g., PIOTET, 2004) stress the exact opposite, that researchers expressly sought to distance themselves from DURKHEIM and not use procedures from his late work as a model (meaning the DURKHEIM of "The Elementary Forms of Religious Life," 1995 [1912]). Underlying this is the charge that DURKHEIM produced a second-hand armchair sociology, to which the appropriate response was to adopt a strict empirical orientation. [34]

Before we turn to respective developments in the field of qualitative methods, we first clarify what we mean by such a label. Qualitative research in sociology mainly shows up as a "theory/method package" (a term coined in the 1980s by Susan Leigh STAR and Joan FUJIMURA, see CLARKE and STAR, 2008, p.117). Such a package integrates epistemological foundations with subject-related theoretical assumptions, methodological reflections on research processes and concrete methods of data collection and evaluation. The relative proportions between these elements vary up to the, sometimes exclusive, accentuation of "methods." How can the epistemic culture of this kind of "qualitative social research" be described in greater detail? [35]

In contrast to other sub-fields of sociology (such as sociological theory or diagnoses of contemporary society), qualitative and interpretive methods are situated at the level of empirical social science. "Qualitative methods"<sup>19</sup> are widely understood as non-standardized procedures of empirical research in sociology

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version of the contrast, see SOEFFNER (2004). Here reference should also be made to the distinction between the "Gallic" and "Teutonic" styles of science in GALTUNG (1983).

17 Based on an analysis of discourses in environmental policy, KELLER (2009 [1998]) argues that this is less a matter of profound differences in mentality than of institutionally processed and stabilized procedures.

18 See also the detailed study by POLLACK (1978), who came to similar conclusions.

19 Here we take the liberty of falling back on the self-labeling commonly employed in the discipline, though we are aware that the term "qualitative methods" is problematic. For example, it is repeatedly pointed out in the literature that standardized or quantitative methods also rely on (steps or procedures of) interpretation.

which are used both in data collection (for example, guideline-based interviews, observation procedures, recordings of audio-visual data) and in data analysis (interpretation procedures such as sequence analysis, coding, and so forth). Speaking of *qualitative* and *interpretive social research*, at least in the German context, does *not* imply—according to the ideal conception—viewing individual techniques of data collection or data analysis in isolation. Rather, it implies certain relations between theory and methods, namely, an integrative contextual perspective on how the object is understood and its theoretical foundations, on research interests, on epistemological and methodological reflections and on the level of methodological-practical implementation. [36]

However, some indicators suggest that this already represents a very specific, far from generally shared understanding of qualitative research—one that is a particular, distinguishing feature of the German epistemic culture of such approaches. Therefore, only empirical research can show the extent to which, in contrast, a completely different understanding can also be found, for example, the isolated and technical use of methods (EBERLE, 2007, p.220; KNOBLAUCH, FLICK & MAEDER 2005, §4f.). As we use it, "qualitative social research" refers to what REICHERTZ (2009, §14) called "elaborated methods" (integrated sets of particular epistemic assumptions and self-reflection, concrete research interests and theory, methodology and methods) as well as to perspectives he identified as "ad hoc procedures," and to other approaches (MRUCK, 2007). However, we do not go as far as to adopt REICHERTZ's classification of "good" and "bad" procedures. [37]

One can speak in terms of the *fields* of qualitative methods in a heuristic sense to refer to the concrete shape assumed by the acceptable forms of structuring and dynamics of qualitative social research within sociology in the two countries. This is not a matter of a comprehensive field analysis in BOURDIEU's original sense, that is, one which throws light on the influences of social origins or the habitus, of power, status and struggles over resources in the academic field of sociology. In other words, it is not a matter of sociology as a power game in which different forms of capital can be accumulated or lost, and hence in which there are always winners and losers. Nevertheless, the positionings and approaches of qualitative research cannot be understood in isolation. They are part of a more or less dynamic network of reciprocal references and demarcations. In this sense, specific historical field constellations play an important role. In German-speaking sociology, for example, the *Positivismusstreit* [positivist dispute], or the distance between pure theory construction and empirical research are important.<sup>20</sup> [38]

At the same time, such constellations are always also transformed by the respective developmental dynamics, including "ground-breaking impulses" provided by a particular approach. The development and establishment of perspectives and methods, their presence in theoretical foundations,

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<sup>20</sup> On the positivist dispute, see ADORNO et al. (1976 [1969]); on the relationship between theory and research, see KALTHOFF (2008), and WEISCHER (2004). The distance between theory construction and empirical research becomes especially apparent in the development of the influential "grand theories" of Jürgen HABERMAS and Niklas LUHMANN. In contrast, BOURDIEU's theory of practice, for example, was developed in direct connection with empirical research.

methodological reflections and manuals on methods, are anchored in the respective national reference spaces of scientific discussions which take their orientation from language borders, history and conventions. Introductions, developments and justifications of qualitative research strategies situate themselves within the respective existing landscapes of sociological reflection and empirical research, which always involve a blend of consistency and change. In other words, they are embedded and institutionalized in academic and research-oriented scientific fields. In addition to the organizational and practical arrangements, these also include backgrounds and presuppositions grounded in the particular "local" history of sociology. This history provides with possibilities of connection to and dissociation from established, canonized or suppressed lines of argumentation, which can if necessary be traced back to the generations of the classics and illustrate the situatedness of approaches, procedures and research perspectives. In this regard, REICHERTZ (2009) recently provided some pointers concerning possible specific influencing factors and trends towards change in German qualitative research, its subject matter, developments and legitimations. Specifically he notes the charisma of "founder figures," developments away from commonality to competition, from theory and methodology to practice, and from the researching subject to the research process, among other factors. [39]

Based on a brief comparison of German and American trends since the 1970s, FLICK (2005) observes that the above-mentioned differentiating features of sociology in general also apply to qualitative social research. KNOBLAUCH et al. (2005, §2) also underscore the pronounced heterogeneity of qualitative social research in the various European countries which has not received much attention within the discipline:

"The French might investigate things in a quite different manner than the Poles would, the Germans again differ from the Spanish, even if they relate to the same method, etc. Moreover, at the conferences at which we participated, we realized how little we know about issues which are of great importance to colleagues from other countries—even if they work in the very same disciplinary field." [40]

KNOBLAUCH et al. attribute this diversity, among other things, to the different ways in which the respective academic fields are structured or to the role of national avant-gardes in qualitative research. The situation of qualitative social research in France—according to their assessment based on personal encounters—constitutes a clear site of exceptionalism. Whereas in most European countries, the interpretive paradigm, and hence a focus on meaning (sense-making), contexts, interpretation, understanding and reflexivity of the research procedure, are accorded central importance, this is only weakly represented in France. Quite in line with this, French sociology shows a significantly smaller disparity between quantitative and qualitative approaches (KNOBLAUCH et al., 2005, §5; see also ANGERMÜLLER, 2005). MRUCK (2007) describes in more general terms the major differences between the modes of qualitative research in various countries. EBERLE (2007) briefly notes that social science in France tends to place greater emphasis on achieving results and less

on the question of what constitutes a legitimate use of methods, whereas the discussion in German-speaking countries focuses on the latter question. [41]

In the following sections, we highlight a comparable striking difference in the respective epistemic cultures of qualitative research in sociology, but without implying that it represents the exclusive style of epistemic-cultural practice in each case. Some further caveats are also necessary. Our statements refer to a very limited area of this research, namely, the negotiations conducted in the form of texts of the most diverse kinds which are primarily related to research questions. The negotiations in question therefore concern the arena of public debate within the discipline in which legitimate ways of generating knowledge are developed, justified, discussed, challenged, rejected and affirmed.<sup>21</sup> [42]

## 5.2 Legitimization by procedure: The development of qualitative and interpretive social research in the Federal Republic of Germany

The disciplinary self-understanding of sociology in German-speaking countries is based on the assumption that during its consolidation phase, long before the period of interest here, classical authors (Max WEBER, Alfred SCHÜTZ, Georg SIMMEL, Karl MANNHEIM) offered a wide range of reflections on the foundations and procedures of qualitative social research. Until recently, however, the history of qualitative social research in the German-speaking world, like the more recent history of sociology as a whole, has been the subject of scarcely any reflection in social science, at least not in any sociological history of sociology (FLECK, 1999, p.59), even though the situation is currently changing noticeably. At most it received attention in introductory chapters of methodology textbooks, brief surveys or marginal remarks.<sup>22</sup> One partial exception is the study by WEISCHER (2004) entitled "The Enterprise of Empirical Social Research: Structures, Practices and Models of Social Research in Germany," which is comprehensive in orientation and scope. The author here focuses on the entire field of empirical social research and provides instructive pointers concerning its highly diverse post-war history.<sup>23</sup> [43]

21 One cannot dispute, of course, that there are many other areas of qualitative or interpretive social research to which what we say does not apply. These include the pragmatics and adventure of research practice, or the actual use of methods in subject-related research. Our approach is not one of observing social researchers as they conduct research in real time, but instead we analyze the textual traces left behind by their activity in publications. Following sociologists as they do their work is no easy task. After all, they have (almost) no laboratories, and some of what they do takes place on trains or in the cinema, even while jogging. Of course, the latter also applies to natural science.

22 See, e.g., FLECK (1992), FLICK (2005), GARZ (1995), HITZLER (2002, 2005, 2007), HOPF and MÜLLER (1995), KNOBLAUCH (2007), KÜCHLER (1980), LÜDERS and REICHERTZ (1986), MRUCK and MEY (2000), PRZYBORSKI and WOHLRAB-SAHR (2008), as well as the opening article by REICHERTZ (2007) on qualitative social research in the special issue on "Qualitative Sozialforschung—Ansprüche, Prämissen, Probleme" [Qualitative Social Research—Claims, Premises, Problems] of the journal *Erwägen Wissen Ethik* [Contemplation Knowledge Ethics] 18(2) by which the author triggered an extensive debate within the field about the "quality" of qualitative research.

23 The German title is "Das Unternehmen 'Empirische Sozialforschung': Strukturen, Praktiken und Leitbilder der Sozialforschung in der Bundesrepublik Deutschland," see also GERHARDT (2006). FLECK (2011 [2007]), on the other hand, concentrates on the period up to the mid-1950s; looking back even further, BONSS (1982) analyzes "Die Einübung des Tatsachenblicks"

WEISCHER distinguishes three phases of empirical social research in Germany: the founding phase (1949-1965), the "great period" of empirical social research (1965-1980) and the "normal activity" of empirical social research (since 1980). According to this classification, qualitative approaches already attracted interest during the founding phase, as reflected—for example, at the Sozialforschungsstelle Dortmund [Dortmund Center of Social Research]—in the shape of typologically oriented empirical studies in industrial and occupational sociology. Nevertheless, qualitative methods were still viewed with a "certain suspicion" (p.71). As a result, quantitative approaches initially achieved dominance, and early stages of development towards the "progressive marginalization of qualitative social research" (p.195) could be observed. This situation changed in the course of the positivism dispute, he argues, and with the reception of the "interpretive paradigm" from American sociology by German scholars around 1970 in which HABERMAS (1988 [1967]) played a decisive role (WEISCHER, 2004, pp.252ff.). Increasingly, according to WEISCHER, independent discourses on methods and methodologies became established which demanded that the use of methods be justified with a view to research topics and questions. Ultimately, qualitative methods also underwent a renaissance and became distinctly professionalized. Nevertheless, the separation between the two types of research remained significant (pp.425ff.). [44]

Accepting the accuracy of WEISCHER's analysis, our intent is to continue further to more closely examine a specific aspect in the establishment of qualitative research in Germany. This aspect also illustrates the considerable influence of critical theory and its early efforts to develop an empirically oriented form of social analysis. In West German sociology during the 1950s, numerous studies were carried out—especially in the fields of industrial sociology, occupational sociology and the sociology of work and youth sociology (ADORNO, 1979a [1952], 1979b [1959]). But concrete research, from the early 1950's on, came along with discussions about research goals, questions, conceptions and procedures. [45]

Several members of the Frankfurter Institut für Sozialforschung [Frankfurt Institute of Social Research] were involved here, not least of all Theodor W. ADORNO himself, who frequently addressed the relationship between sociology, theory and empirical social research (ADORNO, 1979a [1952], 1979b [1959], 2000 [1957]).<sup>24</sup> In doing so, he advocated a theoretically integrated and reflected form of empirical research which, on the one hand, repeatedly distanced itself from simple public opinion research, but on the other hand, did not adhere to all too simple general reservations concerning empirical approaches based on mass data. According to ADORNO (1979a [1952], pp.485ff.), the aims and tasks of a comprehensive social analysis, were to exploit the potential of empirical research for refuting prior theoretical assumptions, and could include both qualitative and quantitative approaches. However, ADORNO argued that care had to be taken to ensure that research itself is not deformed by its own categorical guidelines into a practice of confirmation:

[The Training of the Factual View] that occurred before sociology became institutionalized as such.

24 See Note 71 in MANGOLD (1960). On ADORNO and his "self-image as a social researcher," see also JUNG (2013).

"When I said that a theory of society was required to ensure even the empirical reliability of findings, this was exactly the kind of problem I had in mind. What constitutes a key group, for example, is not something that can be decided by statistics as such, but only by reflection on the actual distribution of power within society. From this you can see the current relevance of the relationship between quantitative and qualitative analysis for our science, because the insights that mediate between the statistical method and how it can be applied adequately to certain contents are largely qualitative in nature. Particularly in America, where quantitative methods have been pushed to their current level, the necessity of qualitative work is recognized not only as a supplement, but as a constitutive element of empirical social research" (pp.490-491). [46]

By the 1950s then, the concept of "qualitative analysis" was used within the Frankfurt Institute with reference to Allen BARTON and Paul LAZARFELD (1955) in particular, but also with a view to Bernard BERELSON's (1952) work on the qualitative analysis of media content. Above all, the text by BARTON and LAZARFELD (1955) was re-published several times by the Institute. The authors here underscore the meanings associated with this concept.<sup>25</sup> Although BARTON and LAZARFELD mainly advocate the use of qualitative methods in exploratory phases of research, the Frankfurt contributions to "group discussion as research method" represent a strikingly self-confident, independent site of qualitative research that clearly goes beyond the idea of a "mere preliminary stage of research" and emphasizes the need for qualitatively oriented studies:

"It is not uncommon for empirical social research to encounter materials that in terms of subject matter and content refer to key societal issues, but which are not amenable to processing and evaluation in accordance with the established methods. ... The real reason for the discomfort probably resides in the specific nature of the objects studied by sociology, chiefly in the irrational aspects of society that do not fit as seamlessly with the mathematical and natural scientific methods as is postulated where the rigorous development of sociological methodology is insisted upon. However, this does not obviate the need, whenever promising but recalcitrant materials are available, to try to master them at the methodological level instead of writing off their potential for research. This is the situation with the results of the group experiment reported in the second volume of the *Frankfurter Beiträge zur Soziologie*" (HORKHEIMER & ADORNO, 1960, p.5). [47]

"Gruppenexperiment" [group experiment] was the title of a report delivered by Friedrich POLLOCK (1955; cf. POLLOCK & ADORNO, 2011) about the institute's research activities.<sup>26</sup> 121 group discussions were conducted at the Institute of

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25 BARTON and LAZARFELD (1955) was reprinted in German in 1979, in a collection edited by Christel HOPF and Elmar WEINGARTEN entitled "Qualitative Sozialforschung" [Qualitative Social Research].

26 The subject of the group discussions, an idea imported from the United States, were the mentalities and political attitudes of people from different social milieus in post-war Germany. The research was funded by the US High Commissioner for Germany. POLLOCK (1955) contains contributions by the various researchers involved. POLLOCK and ADORNO (2011) present a collection of translated methodological texts on the group discussion experiment from the original 1955 book.

Social Research, beginning in winter 1950, on which Diedrich OMER (1953) and Volker von HAGEN (1954) wrote their dissertations (see MANGOLD, 1960, pp.9f.). Such group discussions had predecessors in American models developed in the late 1940s in the context of motivation and market research, among other things, and are still used today in essentially unaltered form as focus groups. Moreover, MANGOLD (1960) discusses a number of studies from Chicago School and close scholars that dealt with groups (such as Frederic THRASHER's "The Gang" 2000 [1927], and William F. WHYTE's "Street Corner Society" 1993 [1943]). And he refers positively to critiques (e.g., BLUMER, 1976 [1933]) of established (big data) mass communication and opinion research. Against this background, MANGOLD (1960) sought to provide a systematic justification of the "method of group discussion," Even though the method

"is gradually developing into a standard method of market and opinion research ... a comprehensive and systematic investigation of the methodological and theoretical implications of the different approaches and of the compatibility of the particular conditions of the group situation with the envisaged goals of the investigation has not yet been conducted. ... The present work tries to provide such an analysis" (p.14). [48]

According to MANGOLD, this is what necessitated the development of special

"criteria for the validity and generalization of discussion material ... The difficulties consist in providing a more precise description of the social structure of so-called informal communication situations, the relationships between the individual interlocutors and the internal structure and meaning of situation-specific group norms" (ibid.). [49]

In his study, MANGOLD first discusses problems of "failure rates," "non-responders," the "comparability of individual reactions" and the influence of "group control," among other things. His explanations focus largely on the analysis of several discussion protocols, best described as a mixture of the reproduction of excerpts from the protocols and the researcher's comments on passages which are interrupted by longer interpolations of an analytical or descriptive nature. Such predominantly content-oriented evaluations are followed by comparisons in accordance with socio-structural criteria. In this respect, essential elements of what much later Ralf BOHNSACK [1991] called "dokumentarische Methode" [documentary method] are already in place here, including its application to group discussions. BOHNSACK elaborated this approach, supplemented other theoretical references and extended it by systematizations of the analytical steps. [50]

A methodological systematization seemed necessary at this juncture because "qualitative analyses" (HORKHEIMER & ADORNO, 1960, p.5) of the discussions with "foremen," "groups of warehouse workers," "groups of farmers," "groups of miners" and others were called into question "according to the well-established rules of the scientific community." Supposedly, there were "methodological deficiencies"—on the grounds that they lacked "objective validity." Therefore, "Mangold's book is intended as a guide to further research" (pp.5f.):

"[Its] aim is to define the possibilities and limits of the method for the systematic and controlled survey of opinions, attitudes and behaviors and to derive proposals not only for the evaluation, but also for the concrete design of the procedure itself" (p.6). [51]

HORKHEIMER and ADORNO nevertheless note that the procedure of group discussions alone is not sufficient. The complexity of the objects, the methods of data collection and their results can scarcely be comprehended in "purely objectifying" procedures. Rather, the "comprehension of the matter itself" requires "more subjectivity: more experience and interpretive power on the part of individual researchers" (p.7). At this precise point, a striking proximity to phenomenology (Edmund HUSSERL) becomes apparent.<sup>27</sup> "Comprehending the thing itself" implies both *analytical competence and the appropriate application and handling of procedures*; the one cannot be had without the other. [52]

Together with the preface authored by HORKHEIMER and ADORNO, MANGOLD's text is one of the earliest examples of the development of a systematic account of analytical procedures of qualitative social research. In his book, he made it clear that, despite the Frankfurt School's far-reaching critique of positivism, the alternative was by no means sought in a philosophical-sociological stance of deep intuition of essences or the like. Rather, based on a specific conception of the object of investigation, efforts were made to address the related research questions with a maximum of scientific rationality and reproducibility—and to thereby secure recognition by leading empirical researchers. [53]

Despite the general references to studies of Chicago School sociology mentioned above, this did not involve referring to more specific and, indeed, much more comprehensive discussions from the American literature, which were present. For example, two such collections of articles on methods were edited by René KÖNIG (1972a [1952], 1972b [1952]). Their titles and abstracts seemed to provide access to current methodological developments in the US, at least for those who spoke English.<sup>28</sup> Instead, the representatives of the Frankfurt School began with a systematic, text-based justification of the analytic process and its validity. The path of systematization adopted was in line with the continuity of subsequent methodological developments, even though only a few researchers picked up on it. For example, the efforts made by POPITZ, BAHRDT, JÜRES and KESTING (1957) to provide a precise record of the working environments and world views of workers seem to us to exhibit an "elective affinity" with the Frankfurt School approach. With a strong background in philosophical anthropology (PLESSNER, 1928) they referred to a phenomenological approach which was originally conceived by Edmund HUSSERL as an exact rational procedure for analyzing the "things themselves" (2001 [1900/1901], p.168).<sup>29</sup> It aimed at excluding

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27 Theodor W. ADORNO had submitted his doctoral dissertation *on "Die Transzendenz des Dinglichen und Noematischen in Husserls Phänomenologie"* [The Transcendence of the Material and Noematic in Husserl's Phenomenology] at Frankfurt University in 1924 (see MÜLLER-DOOHM, 1996, 2009).

28 Since the early 1950s, René KÖNIG had been trying to promote very diverse methodological approaches from American sociology in Germany; see MOEBIUS (2015).

29 See DREHER (2018, pp.XI-XV) for a discussion of the philosophical backgrounds of POPITZ et al.

philosophical or empirical speculation of any kind. In a most similar way, the first anthologies of "genuinely" German-language contributions to the development of methods at the end of the 1970s also revolve around the question of *methodological control of the interpretation process from different perspectives, with different procedural justifications, methods and objectives* (e.g., HEINZE, KLUSEMANN & SOEFFNER, 1980; SOEFFNER, 1979). [54]

This brings us back to the more general discussion of the development of qualitative research. One can speak of its professionalization insofar as individual positions and procedures, indeed the entire spectrum of qualitative methods were consolidated. They assumed the form of textbooks and were established in university classes and training courses, periodicals and specialist publications, positions within the university, workshops on methods, discussions of standards, and so forth.<sup>30</sup> Furthermore, professionalization of qualitative research manifested itself in the recommendations of the Deutsche Gesellschaft für Soziologie (DGS) [German Sociological Association] on the structure of sociology study programs (REHBERG, 2003a, 2003b). The founding of an independent research committee of the DGS on "Qualitative Methoden der Sozialforschung" [Methods of Qualitative Social Research] in 2003, and in the establishment of professorships for qualitative methods, are additional markers of such processes (see KNOBLAUCH, 2008, pp.214f.).<sup>31</sup> [55]

At the same time, the dispute initiated by the existing (quantitative) methods section of the DGS and its then-spokesman over the newly founded committee shows that, until very recently, the lines of conflict could by no means be regarded as having been smoothed over.<sup>32</sup> The more recent professionalization of the field of qualitative social research is reflected in new journals such as *FQS*, *Sozialer Sinn* [Social Meaning] and the *Zeitschrift für qualitative Bildungs-, Beratungs- und Sozialforschung* [Journal for Qualitative Research in Education, Counseling and Social Issues]. According to HITZLER's (2002) assessment, it is

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30 Here professionalization refers to the level of academic sociology itself, not to the professional practice of sociologists outside the academic domain. On the professionalization of recent German-language sociology, see the overview of the discussion in BAND (2004); on professionalization processes in francophone sociology, see PIRIOU (2008); and more generally on the professionalization of sociology, see, for example, LAMNEK (1993).

31 Further information is provided by the [Memorandum für eine fundierte Methodenausbildung in den Human- und Sozialwissenschaften](#) [Memorandum for Sound Training in Methodology in the Humanities and Social Sciences], initiated mainly by Günter MEY in 2007 and signed by numerous social scientists. Then there is the much frequented [Berliner Methodentreffen Qualitative Forschung](#) [Berlin Meeting on Qualitative Research Methods] that has been held since 2005. In addition one might point to the offers of [qualitative methodological workshops](#) at the Gesellschaft Sozialwissenschaftlicher Infrastruktureinrichtungen GESIS [Society For Infrastructures in the Social Sciences] and the long-standing [Magdeburg Bundesweiter Methodenworkshop zur qualitativen Bildungs- und Sozialforschung](#) [National Workshop on Methods in Qualitative Educational and Social Research].

32 Presumably it is no coincidence that the special issue of the (for some) most important sociological journal in Germany, the *Kölner Zeitschrift für Soziologie und Sozialpsychologie* [Cologne Journal for Sociology and Social Psychology] on "Methoden der Sozialforschung" [Methods of Social Research], edited by Andreas DIEKMANN (2004), does not contain any contribution on qualitative methods. The relationship between qualitative and quantitative research remains tense, even though the label "mixed methods" suggests a willingness to communicate, at least for some colleagues (JUNGBAUER-GANS, 2008; KELLE, 2007; REICHERTZ, 2008; SCHULZ & RUDDAT, 2008).

characterized by efforts to establish connections and engage with theoretical approaches far removed from empirical research, while references to sociological diagnoses of society or of the present are largely absent (see also the discussion in KALTHOFF, HIRSCHAUER & LINDEMANN, 2008; KELLER, 2014).<sup>33</sup> [56]

Even prior to the publication of WEISCHER's (2004) study, the widespread myth that qualitative social research only began in Germany in the 1970s had been corrected (see, e.g., FLICK 2005, §12). Such assessments overlooked the empirical studies on workers mentioned as well as the fact that, as argued above, post-war representatives of critical theory, in particular Theodor W. ADORNO, had already made commitments to qualitative inquiry, and not only at the general level of epistemological debates, but very concretely as regards the acceptance conditions of scientific knowledge production.<sup>34</sup> Furthermore, the above-mentioned qualitative methods of data collection used since the 1950s, like group discussions and subsequent analytic reflections in Werner MANGOLD (1960) exemplify such an early presence. "Open" interviews and accompanying observations, as applied, for example, in phenomenologically-based studies of POPITZ et al. (1957) on the "Gesellschaftsbild des Arbeiters" [Worker's Image of Society], which took as their point of departure the lived experiences of workers, were connected with qualitative evaluation strategies. HOPF and MÜLLER therefore speak of the "considerable role" played in the 1960s by qualitative "methods in the collection and analysis of social scientific data" (1995, p.54). [57]

However, these varied and sporadic approaches were largely eclipsed by the dominance of quantitative social research and did not lead to the emergence of a broadly accepted qualitative sociology with a distinct profile. In fact, such developments did not materialize until the 1970s, except for the mid-1960s first larger imports of US pragmatist philosophy and sociology presented by Jürgen HABERMAS (1988 [1967]). The real "coming out of qualitative approaches" is connected in the introductory literature<sup>35</sup> with the adoption of symbolic interactionism, ethnomethodology and conversation analysis or of the "interpretive paradigm." In addition they are related to Aaron CICOUREL's (1964) critique of quantitative methods in sociology, published in German in 1974, or are attributed to the return of emigrant sociologists such as Thomas LUCKMANN.<sup>36</sup>

33 The contributions in MEY and MRUCK (2014), based on plenary addresses at the "Berliner Methodentreffen" [Berlin Meeting on Qualitative Research Methods] present different assessments and categorizations of the field of qualitative and interpretive social research in Germany.

34 See, among others, ADORNO (1979c, 1979d, 2000 [1957]), KRACAUER (1952), and POLLOCK (1955). Qualitative social research can be traced back (also) in the German-speaking world to the nineteenth century (MAUS, 1973; SCHNELL, HILL & ESSER, 2011). Above all, the Marienthal study by JAHODA, LAZARFELD and ZEISEL (1971 [1933]) played a pioneering role in twentieth-century German-language based sociology.

35 See, e.g., FLICK (2005), GARZ (1995), HOPF and MÜLLER (1995), KLEINING (1995), and KNOBLAUCH (2007, 2008).

36 In 1965, LUCKMANN returned from the United States to take up a professorship at Frankfurt University. In 1966 the book he co-authored with Peter L. BERGER, "The Social Construction of Reality" (BERGER & LUCKMANN, 1966) appeared, in which the authors combine numerous influences of the symbolic-interactionist tradition with traditions in European philosophy and sociology. It was translated into German in 1969. First German translations of American qualitative research and its theoretical grounds were published by, e.g., ARBEITSGRUPPE

This reception was promoted by many guest visits of German-speaking researchers in California beginning in the 1970s (among others, Jörg BERGMANN, Ralf BOHNSACK, Uta GERHARDT, Gerhard RIEMANN, Fritz SACK, Fritz SCHÜTZE, Hans-Georg SOEFFNER, and later, for example, Thomas EBERLE and Hubert KNOBLAUCH). They met with local representatives of symbolic interactionism (such as Herbert BLUMER), of the closely related grounded theory methodology (such as Anselm STRAUSS) and of the intellectually neighboring ethnomethodology (Aaron CICOUREL, Harold GARFINKEL). In addition, there were visits in the opposite direction, such as Anselm STRAUSS' residency in Constance in 1975, organized by Richard GRATHOFF. The German import of a version of "research-based learning" (REICHERTZ, 2013a [2003], p.33) which relies primarily on interpretation groups (RIEMANN, 2005) was a result of the contacts with STRAUSS and his working methods. Predominant orientations in the then important context of Bielefeld's department of sociology were towards GLASER and STRAUSS, while references to conversation analysis featured prominently in Constance (REICHERTZ, 2013b; STRAUSS, 1987). It is difficult to overestimate the influences exerted in this regard especially during the 1970s by the heterogeneous American tradition of pragmatic-interpretative sociology on qualitative and interpretative research in the German language. Indeed, these influences were in a certain sense even constitutive of the latter. [58]

The gradual consolidation of qualitatively oriented research paradigms and their methods in the German-speaking world can be characterized primarily by their strong theoretical, methodological and methodical orientation to the controlled recording of interpretive processes and interpretations of social actors and by reflection on scientific interpretation processes—specifically, by what we call *legitimation through procedures*.<sup>37</sup> HOPF and MÜLLER (1995) here identify noticeable "improvements in level in both methods and content" (p.65) since the early 1970s, among other things as regards the "verifiability of interpretations of qualitative data" (p.66). They observe for that period, in addition to the increasing use of computers for analytic purposes, a contrast between methods geared more towards the analysis of content and those oriented more to the interpretation of individual interaction sequences (that is, sequential analysis and hermeneutic methods). With his distinction between "methods as art" and "methods as technique," Hubert KNOBLAUCH diagnoses a generally conflictual form of restructuring of the current field (2007, §12). [59]

Recent developments appear to some observers to involve a progressive specialization, which has the drawback of increasing confusion and competition between schools that now relate to each other in purely negative terms.<sup>38</sup> According to REICHERTZ (2009), no specific trend can be discerned, but instead

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BIELEFELDER SOZIOLOGEN (1973, 1976), and HOPF and WEINGARTEN (1979). WITZEL (1982) presented a detailed first overview of such work. KELLER (2012) provides a comprehensive introduction.

37 See HITZLER (2002, 2005, 2007), HOPF and MÜLLER (1995); KNOBLAUCH (2008), MRUCK and MEY (2000), and the exemplary early article by HOFFMANN-RIEM (1980), but also the numerous proposed classifications and labels of paradigms, trends and basic principles (e.g., in FLICK, 2005; MRUCK & MEY, 2000, 2005; STEINKE, 1999).

a cyclical "up and down" of themes and preferences (§§1-8), in which "elaborated methods" (which are integrated into theoretical paradigms) can be distinguished from "ad hoc methods" (§§14-16). Nevertheless, this distinction is controversial and commented by MRUCK (2007), for example, as problematic, if not inadmissible.<sup>39</sup> REICHERTZ (2009) argues along similar lines as in his above mentioned controversial contribution (REICHERTZ, 2007) and plaid for higher standards of verifiability to confront problems of qualitative research. He calls, among other things, for further reflection on theoretical and methodological foundations. According to EBERLE (2007), such a controversy seems to be a very specific problem of the "German scene" (p.219). [60]

### **5.3 "In shape for the field": Individualized inspiration and qualitative research in France**

Whereas in Germany a division of labor can be observed between "pure" sociological theory construction, empirical research and social diagnosis, the situation in France seems to have been marked from the outset by a stronger integration of theory and empirical research. This was manifest in different forms in the four dominant schools of sociology of the 1970s and 1980s—namely, Raymond BOUDON's (1981 [1979]) theory of the "rationally choosing actor," Pierre BOURDIEU's (1990 [1980]) theory of practice, Michel CROZIER's (CROZIER & FRIEDBERG 1980 [1977]) organizational analysis and Alain TOURAINE's (1977 [1973], 1988 [1983]) actor-oriented theory of action (see BERTHELOT, 2003). In France, the discussion about the demarcation between quantitative and qualitative methods has also played a far less prominent role since the 1960s than in German-language sociology. In contrast, methodologically integrative approaches under the umbrella of theoretical paradigms were more widespread at the level of publications especially in the 1970s and 1980s. Moreover, BOURDIEU, CHAMBORDON and PASSERON (1991 [1968]) in their book on "Le métier de sociologue" [The Craft of Sociology] represented, vicariously as it were, the influential position of epistemological monism which rejects the separation between quantitative and qualitative approaches. Elsewhere, BOURDIEU stated in an interview, that "[a] number of false controversies, long dead and buried (for example, internal versus external analysis in literary studies, quantitative versus qualitative techniques in 'methodology') exist only because professors need them to organize their course syllabi and exam questions" (BOURDIEU & WACQUANT, 1992, p.181).<sup>40</sup> [61]

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38 For examples of relevant contributions, see BOHNSACK, MAROTZKI and MEUSER (2006), FLICK, KARDORFF and STEINKE (2000), HITZLER and HONER (1997), and KÖNIG and ZEDLER (1995).

39 MRUCK argues against REICHERTZ's call for extensively consolidated "elaborated procedures" on the grounds that this would represent an inadmissible restriction of the perspectives, interests and cognitive potentials of qualitative research, which seems unjustifiable specifically in the international domain.

40 For similar positions, see PASSERON (1995, 2013 [1991]), and the overview in GROULX (1997). See also SOEFFNER (2004, p.67), who rejects the global opposition between quantitative and qualitative analytical methods in favor of the distinction between "Cartesian" and "hermeneutic" science.

Authors of introductory accounts of the recent history of French sociology concentrate primarily on presenting competing overall approaches to the discipline (BERTHELOT, 2003; LAHIRE, 2005). The development and situation of French sociology until the mid-1970s are generally described in terms of the competition between the "big four," noted above: the BOURDIEU school (theory of practice, genetic structuralism), the school around Raymond BOUDON (rational choice theory, methodological individualism), the school of Alain TOURAINE (actor-centered sociology) and Michel CROZIER's institutionalist research on organizations. [62]

Discussions of methods focus mainly on methods of data collection, with methods of analysis receiving much less attention. Data collection methods are dominated by field research. There has been a marked increase in interest in this area since the mid-1980s. In recent times is justified primarily by appeal to the Chicago tradition and symbolic interactionism, but also with reference to ethnomethodology and pragmatism. There has been a comparable increase in the literature on interviews, where the main focus is also on the intended use of such interviews for data collection, with questions of analysis again receiving only brief mention. Data processing privileges forms of content analysis that often rely on the tradition of structural linguistics. In a brief impressionistic report on qualitative methods in France, ANGERMÜLLER (2005) points to the fact that the label "qualitative" plays a much less important role, even though non-standardized methods can be assumed to be widespread. However, reflection on these methods is more prevalent in French-Canadian social research (PAILLÉ, 2006). [63]

Aside from the above-mentioned surveys of theoretical schools, French sociology has begun to address the history of empirical social research in recent times.<sup>41</sup> MASSON (2008), for example, presents nine exemplary and groundbreaking empirical quantitative and qualitative studies from the period 1951 to 1999. His main concern is to trace the contents, theoretical and methodological scope, and innovative power of the respective studies. He also addresses how they were financed and how they connected with the surrounding field of French sociology. MARTIN and VANNIER (2002) examine the influence of psychological methods (for example, interview and conversation techniques) on the development of post-war French sociology. In recent years, various conferences have also dealt to some extent, and usually with a very specific thematic focus, with the developments in empirical sociology and how it is embedded in national and international sociological traditions.<sup>42</sup> [64]

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41 HEILBRON (2015) provides a comprehensive overview of developments in French sociology with a focus on institutional structures and paradigms.

42 See, e.g., MARCEL (2005) and VANNIER (2000) on the history of research at the Centre d'Études Sociologiques (attached to the Centre National de la Recherche Scientifique) between 1945 and 1968 and the conference of the Centre Edgar Morin/Institut Interdisciplinaire d'Anthropologie du Contemporain on "Les grandes enquêtes pluridisciplinaires des années 60-70 en France: Bilan et perspectives" [The Major Multidisciplinary Research Projects of the 1960s and 1970s in France: Assessment and Prospects], which was held in Brest in May 2008 and focused primarily on the early work of Edgar MORIN.

BOURDIEU and PASSERON (1967) describe how, from the 1960s onwards, sociology prevailed against the strong dominance of existentialist philosophy, on the one hand, and of structuralist anthropology with an emphatic claim to scientificity, on the other. According to them, the connection to DURKHEIM played a formative role in this process. Significantly, this glosses over the fact that, in the 1950s, French sociological researchers initially made explicit efforts to dissociate themselves from the "armchair sociology" of DURKHEIM's late phase. Instead, they opted for field research in factories and communities, or studies on mass media. These, all were heavily influenced by American models—directions of research from which BOURDIEU also clearly distanced himself in the formation of his school. The conflicts from the 1960s noted above continued to resonate in BOURDIEU's repeated polemical attacks on "microsociology" or "spontaneous sociology" for failing to make the "epistemological break" with everyday reasoning.<sup>43</sup> However, these disputes are *not* directed against qualitative social research per se. The latter, as is shown by BOURDIEU's work in particular, is employed almost as a matter of course as a very special set of theoretical and methodological instruments. At the same time, the strong, albeit competing, positions of ethnology and anthropology promoted ethnographic research as a "natural" option in sociology as well. [65]

Let us now take a closer look at the situation in France in the 1950s. The post-war years were marked at the political level by the establishment of far reaching programs of state planning for social development, which increasingly influenced the reconstitution of sociology. Among the important sociologists were Raymond ARON, Georges FRIEDMANN, Georges GURVITCH and Jean STOETZEL. While ARON and GURVITCH seemed more theoretically oriented, FRIEDMANN and STOETZEL represented highly contrasting empirical orientations towards American models. Thus, they were very influential, because the re-establishment of sociology in post-war France was essentially shaped by the turn to empirical research. This constellation involved two major different directions. On the one hand, there was survey-oriented quantitative research, on the other hand, and more important for our concern here, there was case-oriented field research geared toward producing monographs on working conditions, but also on urban and rural development and the like. The leading location for this research was the Centre National de la Recherche [CNRS; National Research Center]-funded Centre d'Études Sociologiques, which was founded by Georges GURVITCH (professor of sociology at the Sorbonne) in 1945 and headed by him until 1949, when Georges FRIEDMANN (professor at the Conservatoire National des Arts et Métiers) took over the reins until 1951. He was succeeded by Maximilian SORRE (honorary professor at the Sorbonne) until 1956 and then by Jean STOETZEL (professor at the Sorbonne), a promoter of quantified approaches. Therefore, the founding and establishment phase of the Centre d'Études Sociologiques is significant in marking the beginning of the orientation towards "indispensable fieldwork" (POLLAK, 1978, p.41). [66]

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<sup>43</sup> See, for example, the conversation between BOURDIEU and PASSERON (1966), and also BOURDIEU et al. (1991 [1968], pp.17ff. and pp.24ff.).

Sociology in post-war France was in a desolate condition, compared to which the situation in Germany can only be described as well-established. With just four professorships, it had almost no foothold in French universities and institutions of higher education, and its foothold in research institutes was equally insecure (MASSON, 2008):

"In spite of its apparent autonomy, the first post-war generation of French sociologists worked under a twofold exclusion. Excluded from both the intellectual scene and the universities, they were not involved in professional public contract research either (represented by the INSEE and the INED). The sociological studies were not the products of a specific project or of a definable vocation, but represented a hesitant and ambivalent response to the almost insurmountable tensions between two worlds, namely, between Sartre and statistics" (HEILBRON, 1991, p.365).<sup>44</sup> [67]

It was not until the early 1960s that the institutional anchoring of sociology underwent rapid expansion. Between 1960 and 1964, for example, the number of sociologists at CNRS rose from 56 to 90, and there was a steady increase in the demand for sociological research from the state (DROUARD, 1982, pp.69ff.). Among the early and most important impulses that proved to be decisive for revitalizing the discipline—the literature makes frequent references to its "second birth" (see, e.g., CHAPOULIE, 1991)—were the aforementioned Centre d'Études Sociologiques, a so-called "laboratory," directed by the philosopher Georges FRIEDMANN from 1949 to 1951. [68]

FRIEDMANN's work focused mainly on transformations in the world of work.<sup>45</sup> Like many of his colleagues, he had previously been in the United States where he had contact with, among others, the research in the sociology of work and industry at Harvard, including the Hawthorne studies (ROETHLISBERGER, DICKSON & WRIGHT, 1966 [1939]), and with studies by Everett C. HUGHES (e.g., 1958). FRIEDMANN propagated corresponding approaches in France. In the early 1950s, a research stay in the United States was a "must" for young sociologists, since the United States was regarded the "new homeland" of the social sciences (DROUARD, 1982; VANNIER, 2010). Later, such a reference to US sociology included the "opposite" perspectives of quantified research too. Paul LAZARFELD, a major player in US quantitative social sciences research, then played a crucial role, especially during the 1960s, through the mediation of Jean STOETZEL and Raymond BOUDON. STOETZEL, who was more oriented to social psychology and opinion polls, was a friend of LAZARFELD and developed the quantitative pole of French sociology. He soon gained support from Raymond BOUDON, who co-authored works with LAZARFELD and edited the French translation of his writings (see, e.g., BOUDON & LAZARFELD, 1965, 1966). BOUDON also became a member of LAZARFELD's "Bureau of Applied

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44 INSEE = Institut National de la Statistique et des Études Économiques, the French national institute for statistics and economic research founded in 1946; INED = Institut National d'Études Démographiques, the French national institute for population research founded in 1945.

45 German translations of his studies appeared, among others, in the series of the Frankfurt Institute of Social Research. He had already become acquainted with HORKHEIMER, ADORNO and POLLOCK in 1934 (FRIEDMANN, 1959).

Social Research" in 1960, and LAZARSELD taught frequently in France in 1962-1963.<sup>46</sup> [69]

FRIEDMANN's maxim for sociology at the Centre d'Études Sociologiques was above all "not to do it like DURKHEIM."<sup>47</sup> This may come as a surprise in view of DURKHEIM's outstanding role in the foundation of sociology in France. However, it was mainly a derogatory reference to DURKHEIM's late work, the aforementioned study on "The Elementary Forms of Religious Life" (1995 [1912]). DURKHEIM was accused of engaging, in this book, in speculative armchair sociology without field contact, relying on second- and third-hand data. Thus, doing things differently from DURKHEIM primarily implied conducting empirical research of one's own. Given the orientation to the American model, this meant:

"The definition of empirical research at this stage was very vague. Working empirically meant collecting data by all available means: opinion polls, large-scale questionnaire surveys, interviews, observation, and so forth. As Georges Friedmann often put it, it was mainly a matter of 'going and having a look'" (MASSON, 2008, p.12). [70]

Such a maxim immediately calls to mind the attitude to research of the Chicago tradition. At the same time, talking about a "research institute" should not disguise the fact that the available resources were very limited. In the mid-1950s, working conditions were miserable and there was a lack of contacts with universities (MARCEL, 2005). Nevertheless, the list of members of the Centre reads like a who's who of French social science in the late 1960s (HEILBRON, 1991, pp.372ff.). Numerous case studies on individual sectors of industry and work were produced in the context of the Centre, such as TOURAINE's study on "L'évolution du travail ouvrier aux usines Renault" [The Development of Shop-Floor Work at Renault Factories] (1955). These studies often took the form of monographs based on unspecified field research or on several on-site visits (in TOURAINE's case, in the factories). [71]

In the early 1950s, another study carried out at the Centre also played a major role in French sociology. In 1952, the philosopher and ethnologist Paul-Henri CHOMBART DE LAUWE who, among other things, also conducted field research in Cameroon and later understood himself as both an anthropologist and sociologist, published "Paris et l'agglomération parisienne" [Paris and the Parisian Conurbation]. In this impressive study he presented the findings of an extensive research project on the Paris conurbation involving 21 collaborators, which reflects the influence above all of Ernest BURGESS and the Chicago tradition of urban research. Jean-Daniel REYNAUD, Henri MENDRAS, Edgar MORIN and many others also worked at the Centre d'Études Sociologiques. According to accounts in the literature, FRIEDMANN assigned all of them areas of work. It should be noted, however, that although they all had a wealth of experience and

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46 On the relationship between BOUDON and LAZARSELD see also MESURE (2013).

47 See PIOTET (2004, pp.122f.); for a general account of FRIEDMANN's importance, see also GREMION and PIOTET (2004).

education—in the French Resistance, in journalism and in history or philosophy—none had studied sociology. [72]

From among the few explicit formulations of an independent methodology of qualitative research for the 1960s, we would like to select the contribution by Edgar MORIN, in relation to whom the *thesis of individualized inspiration* can be demonstrated particularly clearly. By this thesis we understand an (implicit) mode of justification for the production and validity of sociological knowledge which relies on the unquestionable expertise of the researcher.<sup>48</sup> [73]

In 1966, MORIN, whose work during the 1950s had focused mainly on mass media, published "La démarche multidimensionnelle en sociologie" [translated in abridged form as "The Multidimensional Method" (MORIN, 2002 [1966]) in the *Cahiers Internationaux de Sociologie [International Journal for Sociology]*. The journal had been founded by Georges GURVITCH in 1946 and was edited from the mid-1960s on by Georges BALANDIER. MORIN had been the journal's editorial assistant in the early 1960s. The empirical background of this article were interdisciplinary studies of the modernization of rural France in which sociology had in fact played a rather marginal role alongside agricultural sciences and other disciplines (MORIN, 1971 [1967]). Supported by a new type of public funding of research, MORIN had been sent by his mentor FRIEDMANN to a small coastal village in French *Bretagne* [Brittany], to investigate "cultural institutions."<sup>49</sup> There was no precise plan for how he should proceed—"The path is created when walking. The method arose on the move" (MORIN, 2013 [1967], p.9).<sup>50</sup>

"Refusal of a questionnaire-based investigation, privileging of conversations in chance or prepared encounters; ... extended conversations captured on tape which, after one or two hours, brought to light deep-seated obsessions. No *a priori* program, but instead a continuous strategy that changed its priorities and objectives in line with the findings gathered at each meeting of the research team" (ibid.).<sup>51</sup> [74]

Of interest in MORIN's article is the footnote placed right at the beginning of the article by the editors of *Cahiers*: "Here E. Morin presents reflections and suggestions that are less confusing for an ethnologist working in the 'field' than for a sociologist. For the latter, they have the character of a suggestion intended to provoke a methodological debate" (Note de la Direction, in MORIN, 1966, p.49). Following the high profile of field research within the FRIEDMANN circle in the 1950s, French sociology, as mentioned above, was increasingly dominated

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48 The echoes of the mode of justification described by Luc BOLTANSKI and Laurent THÉVENOT (2006 [1991]) as the "world of inspiration" are intentional.

49 The name of the village in question, Plozévet in Brittany, for reasons of "anonymization," was spelled "Plodémet" in the title of the original French edition of the study, and hence also in its 1971 English translation.

50 Quotes from the French re-edition, published in 2013 with a new preface by MORIN. In the following, additional page references for citations in square brackets refer to the (abridged) English translation of MORIN's original article (MORIN, 2002 [1966]).

51 While we conducted our research in 2011 and 2012, the magnetic tapes are stored in the town hall of Plozévet, in the office of Bernard PAILLARD, who accompanied the original project as a student assistant and who still spends several weeks there every year studying the community.

by his successor Jean STOETZEL, quantifying opinion and survey research drawing on LAZARFELD. Therefore, the readership of the *Cahiers* had to be prepared for what followed. [75]

MORIN first presents his "field research" in the Finistère region of Brittany—a complex study of the micro-society of a community in its relations to the macro-society. The article, he writes, will deal more specifically with the problem of what research in the "field" (*terrain*) actually involves:

"Therefore, we required a method that adapts to the multidimensionality of the phenomenon studied, that favors the emergence of the concrete data which constitute its individuality, that allows the development of a thinking which integrates this data into a more inclusive whole and determines the nature and characteristic features of this whole" (1966, p.50) [passage not included in the English translation]. [76]

The methods of standardized inquiry, MORIN continues, are not suitable for this purpose. The study could not include every household, but had to constitute its sample in the course of its implementation. The concrete data collected included "hard," reliable documentation (tape recordings, maximally open interviews, observations and notation of occurrences) which ensured that the various dimensions of the phenomenon studied were recorded using different approaches. The aim was to allow correction and verification as the process of interpretive thinking unfolded. Thus use was made of: 1. "phenomenographic observation" related to ethnographic observation, 2. the interview and 3. participation in group activities (social praxis) (1966, p.51) [MORIN, 2002 [1966], p.155]. [77]

MORIN next specifies the nature of these data collection tools as follows: *phenomenographic observation* includes both public and private places, ideally the totality of the phenomenon studied, including the observer in the act of observing. At the same time it should be "both panoramic ... and analytical," emulating both BALZAC and STENDHAL—the "encyclopaedic description" of the former and the eye for "significant detail" of the latter (*ibid.*).<sup>52</sup> The mass of accumulated documentation turns into a repository [*réserve*] in which the "data are transformed into signs" (*ibid.*). For the rest, the procedure demands the entire spectrum of attention. A research diary is helpful, according to MORIN, not least as a means of reflecting upon the role of the researcher. [78]

The *interviews* (see MORIN, 1966, pp.52f. [2002, pp.155-156]) were conducted according to random criteria and social structural characteristics, in accordance with the requirements of "maximum significance" and diversity. In addition, there

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52 Both nineteenth-century French realist authors combined the analytical precision of individual observation with an ability to comprehend entire social contexts. They are regarded as portraitists of their time with strong affinities with sociology. The fictional works of Honoré de BALZAC (2008 [ca. 1830-1850]) were devoted to the encyclopedic project entitled the "Comédie Humaine," a comprehensive portrait of then contemporary French society in all of its diversity through the experiences of fictional characters (see also LUKÁCS, 1952). STENDHAL gave his novel "The Red and the Black" the subtitle "A Chronicle of the Nineteenth Century" (2002 [1830]; see also DUBOIS, 2007).

were "pseudoconversations," "limited" and non-directed "in-depth interviews"—the whole art consisting in directing "the interview toward areas of non-directivity." Intuition and sympathy, according to MORIN, were more important in this regard than "preconceived rules" and techniques. An interview was successful if it developed into actual "communication," if it transported the researchers towards the "final unexplored continent of the modern world: the Other" (p.53). Finally, there was participation in *group activities and social praxis* (1966, pp.53f.; [2002, pp.156-157], which sometimes took place in experimental settings. The guiding principles were the "maieutic principle," the "non-directive principle," the "principle of primitive experimentation," the "principle of psycho-sociological 'Socratism'" (p.54 [2002, p.157]), and the shared interest in research. [79]

According to MORIN, all of these approaches now confront the "fundamental methodological problem: the relationship between the research worker and the field" (p.55) [p.157, translation amended]. This calls for detachment and objectivity as well as participation and sympathy. Researchers are at once "possessors of special knowledge" and "true friends," one among equals and yet those who possess the "mana" of knowledge. Although one's interlocutor is and remains irrevocably an object, a "counteroffer" is always required.

"Standard inquiries take elaborate precautions about obtaining their data, forgetting that this also depends on who is obtaining them. We paid more attention to the personal qualities of the workers we recruited than to their technical qualifications. The multidimensional method requires a curiosity open to all dimensions of the human phenomenon, and the full use of varied aptitudes. Each worker is versatile in that he must practice observation, interview, and group action" (p.56 [p.158]). [80]

Contrary to what is generally assumed, thinking counts in all of this more than calculating, questioning more than the questionnaire:

"It is worth noting that full engagement of one's personality requires optimal inner conditions, or what athletes refer to as 'form'. Any dip in form has an immediate effect on the quality of the work. It will be readily understood that a method in which art and personal dispositions play such a major role is radically opposed to standardized methods" (p.57). [81]

It goes without saying that the research project, which was implemented in several waves of data collection, was accompanied by the development of concepts based on the research itself, among other things in accordance with the idea of "significant oppositions, polarized antagonistic totalities" (p.59). In this to and fro between concept-formation and phenomenon, the relationship between "the microcosm and the macrocosm of society" unfolds:

"The field confronting the researcher is huge. ... The field must be integrated into a totality without disintegrating it. We tried to develop a methodology that requires full intellectual engagement. We tried to combine ethnographic and sociological approaches in our research. We tried to combine history and sociology in the formation of concepts ... We forced ourselves to establish a phenomenology, that is,

to understand the phenomenon investigated as a complex, nascent totality, and we took our orientation from a multidimensional anthropo-sociology. In this undertaking, in which gentleness and rigor are at risk of destroying each other ... we privilege raw materials and elaborated thinking" (p.61). [82]

In no way do we want to claim that Edgar MORIN's essay had a formative influence on the epistemic culture of qualitative research in France—the contrary would be closer to the truth. Nevertheless, he captures in an exemplary way how researchers—not just any researchers, but those who combine all of the requisite aptitudes—become the competent and singular solution to the requirements and problems of research. [83]

A comparable implicit attitude can be found in other approaches of qualitative research. We can only suggest this here: In addition to ethnography, non-standardized interviews with a background in psychology constituted a major part of the data collection methods in sociology in the 1960s. At the turn of the 1970s, initial attempts were made to systematize such interview's analysis by drawing upon structuralist approaches (MICHELAT, 1975; RAYMOND, 1968). Here, the competence of the researchers was also the dominant factor, albeit with a more precise methodological orientation. However, this generated hardly any response in the years that followed. In a similar way, biographical research influenced by Daniel BERTAUX (1976, 2010 [1997]) was very heavily reliant on the "epistemic" competence of the individual researcher and his' or her's capacity "to discover," and "to know." [84]

On the whole, it can be stated that there were considerably fewer publications on qualitative methods in the French context than in the German-speaking countries. Among this smaller number, comparatively greater explanatory attention was paid to methods of data collection than to methods of data analysis. All things considered, the category "qualitative" did not become the crystallization point of the identity of a specific group in terms of which independent epistemological and methodological considerations are worked out, at least not insofar as these considerations concern analytic procedures. Above, we quoted a formulation chosen by Pierre BOURDIEU in an interview (BOURDIEU & WACQUANT 1992, p.181) that the contrast between "quantitative" and "qualitative" is a thing of the past. This statement was already inherent to another, much earlier key text of French sociology, "The Craft of Sociology," by Pierre BOURDIEU, Jean-Claude CHAMBRORDON and Jean-Claude PASSERON (1991 [1968]). It can be seen in the light of their opposition to the sociologies of LAZARFELD, BOUDON and STOETZEL, on the one hand, and to the monographs from the circle around FRIEDMANN, on the other (as practiced, certainly in an idiosyncratic way, by MORIN). BOURDIEU and his colleagues lay claim to a "third way," which sought to surpass the (to them) "naive" scientificity proclaimed by the LAZARFELD tradition as well as the (again according to them) "naive" and "spontaneous" sociology of everyday life by the reflexive procedure of "objectifying objectification" (BOURDIEU, 1990 [1980], pp.30-41).<sup>53</sup> [85]

53 BOURDIEU argues that social science must not remain at a first stage of objectification which is a product of its research methods and makes possible a break with everyday perception; rather,

Since the 1990s, the clear fourfold division of francophone sociology alluded to earlier has been replaced by greater fragmentation (CABIN, 2000; LAHIRE, 2005; MOEBIUS & PETERS, 2004). For the period from the mid-1970s onwards, DOSSE (1998 [1995], pp.133-138) diagnoses a "paradigm shift" via a "return to action" and, as he indicates by the subtitle of his book, a general "humanization of the social sciences" in France. In this turn, the work of Paul RICOEUR, for example, but also the tradition of pragmatism, and in general qualitative approaches to social research, play an important role. The late 1980s, with a considerable delay in comparison with the German-speaking world, marked the beginning of a renewed reception of the American interpretive paradigm of the Chicago School and symbolic interactionism (e.g., GUTH, 2004; LE BRETON, 2004). Ethnomethodology (e.g., DE FORNEL, OGIEN & QUÉRÉ, 2001) also gained acceptance. Ethnographic approaches and the *enquête de terrain* [field research] became widespread (BEAUD & WEBER, 2003; CEFAÏ, 2003). Currently, such ethnographical studies and variations of field research seem to have become the dominant form of empirical sociology in France (in a problematic way for some of the colleagues we interviewed). All in all, the strong presence of pragmatist traditions is unmistakable (CEFAÏ et al., 2015). Finally, a controversial French variant of "interpretive sociology" has also been developed (BERTHELOT, 2003; WATIER, 2002), which cultivates a very specific conception of "understanding." These are primarily post-empirical, essayistic and impressionistic-intuitive works which are opposed to a "strict" understanding of methods (e.g., MAFFESOLI, 1985; SANSOT, 1971, 1991). [86]

Similar to German sociology, increasing trends towards professionalization can be observed (for instance, introductory books, the establishment of sections within the sociology associations,<sup>54</sup> and occasional conferences on questions relating to qualitative methods). Some authors now place greater emphasis than before on the difference between qualitative and quantitative research (e.g., MUCHIELLI, 1994; PAILLÉ & MUCHIELLI, 2003). This explicit profiling, however, is more a product of social scientists from francophone Canada and French researchers who are oriented to the debate there. To date, the Association pour la recherche qualitative and the Réseau international francophone de recherche qualitative (RIFReQ, which include more than sociology) have been primarily organized from Canadian sociologists. The response in France has come from education studies rather than from sociology, even though the core of the organization is shaped by sociology.<sup>55</sup> The discussions held at the conferences on the current state of qualitative social research revolve around the relationship between professionalization, standardization and the innovative power of

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the constructedness and positioning involved in this process of objectification must be subjected to reflection if it is to produce a suitably reflexive form of knowledge.

54 The methods section of the Association française de sociologie (AFS) comprises quantitative and qualitative approaches; the Association internationale des sociologues de langue française (AISLF) has had a working group on qualitative methods for several years.

55 Even if, for example, the "1er colloque international francophone sur les méthodes qualitatives: Bilan et perspectives de la recherche qualitative en sciences humaines et sociales" took place at the Université Paul-Valéry (Montpellier III) in 2006 (subsequent meetings in 2007 in Trois Rivières, Canada, and in 2009 in Lille, in 2011 once again in Montpellier, in 2013 in Fribourg, and in 2015 in Montpellier). The attendance varies between 80 and 150.

qualitative methods. Similar to the situation in German-speaking countries, France has also seen a flood of specialized publications on qualitative social research in recent years, although once again they tend to focus more on questions of data collection than on those of data analysis (see also BEHRMANN, 2008).<sup>56</sup> In addition, there has been an increase in the use of software-supported analytic procedures. [87]

## 6. Concluding Remarks

In this article, drawing on two specialist texts from the early 1960s, we have outlined the empirically-based observation that, while French methodology discussion in the area of qualitative research emphasized the inspiration of the researchers against the background of ethnography, the simultaneous German discussion focused mainly on the methodology and procedures of textual analysis of recorded data. This observation provides a surprisingly clear indication of the specific epistemic-cultural logic or rationality that would develop in the respective sociological fields of qualitative research beginning in the 1950s and 1960s. [88]

Considering our initial hypotheses in the context of the wider project, some of our initial assumptions were confirmed, whereas others had to be modified. One consistent finding was the difference of the respective epistemic cultures of qualitative research, which also influences their respective receptions of American sociology in particular. What calls for a more nuanced description, however, is how this diversity unfolds. In the German-speaking countries in the early 1960s, the approaches of critical theory and the Frankfurt Institute for Social Research, especially as regards their critique of positivism, strove for strong methodological control of the process of analysis while at the same time trying to develop corresponding strategies. In France, in contrast, the main period of strong theoretical integration extended from the 1970s until the end of the 1980s. In its early phase, the more pronounced individualistic development of sociological knowledge production was marked by an explicit repudiation of DURKHEIM's late sociology and by forms of *ad hoc* research which take their orientation from American field research. These continue to exist to the present day, albeit exposed to increasing methodological reflection from the 1990s onwards. [89]

Another important result of the project is the importance of US sociology, which came as a surprise to us. Beginning in the 1950s, American sociology had a profound influence on how sociological research was structured in the two countries. In France during the 1950s, research practice in sociology was initially constituted as an imitation and improvised application of American models, in

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<sup>56</sup> On qualitative methods in general, see, e.g., ALAMI, DESJEUX and GARUBAU-MOUSSAOUI (2009), MUCCHIELLI (1994, 2004), PAILLÉ and MUCCHIELLI (2003); on ethnographic fieldwork, ARBORIO and FOURNIER (2005), BEAUD and WEBER (2003), CEFALI (2003); on observation, PENEFF (2009), PEREZ (2004); on biographical methods, BERTAUX (2010 [1997]), PENEFF (1990); on the Chicago School, CHAPOULIE (2001), GUTH (2004); on interviews, KAUFMAN (1996). See also the comments on "understanding" and on the interview in the study on "The Weight of the World" (BOURDIEU et al., 1999 [1993]) and the series "L'enquête et ses méthodes" [Inquiry and its Methods], edited by François DE SINGLY.

particular those derived from Chicago sociology, within the framework of field research, for example, in the automobile industry. Here, with very few exceptions, the researchers were amateurs and autodidacts who had not enjoyed any formal training in sociology, but instead developed their epistemic references *ad hoc* or "in the course of research" and on the whole undertook few referencing activities. The 1960s were dominated by the academic establishment of sociology in which the work of Paul LAZARFELD and quantitative opinion research influenced by him provided the hegemonic guiding idea of sociological research. In addition, a few explicit but mutually conflicting positions on sociological method emerged at the interface between sociology, ethnology and anthropology, such as the "total field study" outlined by Edgar MORIN or the combination of ethnography, structural analysis and quantifying surveys pursued by Pierre BOURDIEU. The concept of "qualitative" gained a reception mainly in terms of qualitative content analysis of American media research following Bernard BERELSON and its adaptations by Paul LAZARFELD. At this time, however, it did not constitute a relevant identity category for these fields of sociological work. [90]

The following decades did not bring any significant changes. Sporadic attempts were made to systematize the use of interviews; repeated *ad hoc* pleas were made for ethnographic approaches. In addition, a form of biographical research was cultivated that regards biographical narratives as a mode of access not to individuals' personal experiences but to regularities of social practices. The 1980s and 1990s were marked by stronger reception of selected approaches from the United States and their idiosyncratic integration into the French sociological landscape. Currently one classic figure from the second period of the Chicago School, Howard S. BECKER, plays a leading role in sociology in France. The main protagonists of his reception are Jean-Michel CHAPOULIE (2001), Jean PENEFF (1990, 1992, 2009), and Henri PERETZ (2004). Systematic elaborations of methodological approaches appeared as isolated enterprises in a sociological field marked by the basic tension between individualized studies, on the one hand, and highly integrated research production within the framework of a few sociological paradigms, on the other. [91]

In Germany, qualitative and interpretive social research exhibited a markedly different development. Here too, it is difficult to overestimate the influence of American sociology, market research and opinion research during the 1950s and 1960s. However, these developments encountered a sociological field which had been characterized since the early 1950s by a distinctly higher degree of institutionalization than its French counterpart. Here, the "return" of critical theory played a prominent role from the latter part of the 1950s onwards, but also the "Cologne School" around René KÖNIG, which was deeply shaped by methodological developments in American sociology. Also worth mentioning is the research tradition around Hans Paul BAHRDT and Heinrich POPITZ, among others, which was influenced by Edmund HUSSERL and Helmut PLESSNER (who in turn temporarily headed the Frankfurt Institute). This tradition also used non-standardized approaches in the field of industrial and labor research. Against the background of philosophical anthropology and HUSSERLian phenomenology, it was interested in a precise, comprehensive recording of work processes. In this

respect it was both similar as well as—of course, given its particular theoretical background— quite different from its French counterpart. In addition, further research approaches in American sociology (or at least some of their foundations) became known mainly through René KÖNIG and his work.<sup>57</sup> [92]

By the early 1960s, efforts began in the Frankfurt context to transform not only the collection of data (since the 1950s, group discussions) but also the analysis of data into an epistemologically sound (or at least convincing) matter which should also withstand the judgment of "hard research" (see in particular MANGOLD, 1960). In the 1960s, this constellation coincided and interacted with a gradual reception of American pragmatism and associated approaches in sociology, among other things through the work of Jürgen HABERMAS, the return of Thomas LUCKMANN to Germany, and in particular the translations of the ARBEITSGRUPPE BIELEFELDER SOZIOLOGEN (1973, 1976). In the 1970s there followed not only the formation of a distinct identity building around the concept of "qualitative social research" (HOPF & WEINGARTEN, 1979), but also the development of sophisticated methodologies of qualitative and interpretive data analysis based predominantly on sequence analysis. These developments culminated successively in several independent "major paradigms" (narrative analysis, objective hermeneutics, reconstructive social research and hermeneutic sociology of knowledge). Each focuses centrally on the accuracy of analytic procedures and precise explications of the analytic process, applied to *collected data*. *Such data is fixed via recordings, can be reproduced, and thereby made accessible for readers invited to retrace every single step of analysis*. Furthermore, there were imports from the United States, insofar as they presented appropriate proposals for precise methodological approaches (such as ethnomethodology, conversation analysis, grounded theory methodology, and qualitative content analysis).<sup>58</sup> These approaches underwent further canonization in the 1980s and 1990s. [93]

What would a preliminary summary of the results of our project look like? If we compare the German and French situations, it is striking that the label "qualitative" or "interpretive" played a much more central role in the formation of sociologists' professional identities in the German context. They became the theme or core of heated internal discussions (and sometimes also confrontations) about the "right paths" to the formation of knowledge, a discussion that distanced itself clearly from both more theoretical approaches and from quantitative research. As is true of any scientific activity, the *question of knowledge* is the fundamental starting problem for qualitative social research. The responses to this problem vary. In the German-speaking context of qualitative and interpretive social research, the solution is sought primarily in *procedures that regulate the relationship of the researchers to the world they study and analyze*. This relationship seems to be in need of a high level of reflection and structuring. It is therefore not unquestionably given, but is essentially quite *problematic*. The relationships among the researchers as subjects, the objects of research and the

<sup>57</sup> On the importance of Edmund HUSSERL in the recent German-language methodological discussion, see PLODER (2014); on René KÖNIG, see the study of MOEBIUS (2015).

<sup>58</sup> E.g., MEY and MRUCK (2007) on grounded theory methodology.

ways of acquiring knowledge require strong instrumentation and methodically controlled confirmation and objectification. In the French context, by contrast, the possibility of scientific access *to the world* can be linked to a *competence of researchers (be it explicative or descriptive) for whom procedural issues are of secondary importance, where this competence is for the most part assumed to be self-evident.* [94]

The features described are not exclusive to the respective nations and certainly do not represent a complete description of the field of research. Moreover, there are numerous variations within and across the two countries. Nevertheless, our findings suggest that the differences outlined are striking, constitutive features of epistemic cultures which have structuring effects on both discourses and the practices of research. We have captured this in the concise contrastive formula: "the researcher as problem and solution" (KELLER & POFERL, 2015, p.191). Where the researcher appears as a problem, procedures seek to eliminate the individuality of the researcher as a source of distortion and to provide the methodological basis for an (at least socially) objectifiable cognitive achievement. Where the researcher (and not the procedure) is the guarantor of an unquestionable analytical competence, as in the French case, although support can be provided for the collection of data, the evaluation of the data does not require any further explanation—the authority and unquestionable competence of the experts serves as a guarantor of its quality. This essential difference runs through the most diverse methodological approaches. In it is manifested the more individualistic orientation of French research as opposed to the procedural legitimation of research in the German-speaking world (which is "ideally" the task of group work). It also promoted the widespread vogue for ethnographic work in France, with its implication of the exclusive field expertise of individuals, while inhibiting its use in German-speaking sociology. Ironically, such a distinction cuts across the key difference sometimes associated with German and French traditions of analysis (that is, hermeneutic versus Cartesian analytical models): the German counterpart of the often quoted French "epistemological break" (BACHELARD, 2002 [1938], p.235) with the reality to be analyzed is *refraction by procedure*, which underlies German-language qualitative and interpretive research. [95]

Our chosen concepts of "individualized inspiration" and "legitimation through procedure" are reminiscent of two of the justification orders described by Luc BOLTANSKI and Laurent THÉVENOT (2006 [1991]) for the competing moral justice orders of French society. According to our analysis, epistemic cultures also exhibit competing orders of justification that distinguish the important from the unimportant, the successful from the failed, and promote or prevent very different things depending on how they ascribe sociological value to an analysis, a research question and its implementation in research. Where preference is given to artistic inspiration, greater attention is paid to the individualized genius of knowledge of the world than where the procedure is regarded as the acceptable guarantor of the result. Can this be captured by the contrast "Cartesian versus hermeneutic"? In our view, the situation is more complicated: *both* modes of

epistemic-cultural justification can be used and find expression within both the Cartesian *and* the hermeneutic traditions. [96]

On the whole, then, the differences outlined here are not idiosyncratic features of individual texts that have been singled out in a selective way. Rather, these are distinctive "logics of research," of the generation of scientific knowledge through the practices of interpretation. To put the point differently in conclusion, what we find particularly interesting in MANGOLD's work (1960) is that the book contains the complete transcriptions of some discussions as an appendix and also repeatedly quotes and comments on transcription protocols in the main text. In the German case, the "recording" and "methodical fixation of fleeting reality" (BERGMANN, 1985), the transcription and the associated demonstration of the work of interpretation become the core and anchor point of a procedural mode of securing knowledge. On the other hand, a sociological epistemic culture that accords a central role to ethnography is forced to accept the researcher's ingenuity, competence and sincerity, including their writing ability, as a figure of justification. It cannot replace these elements with procedures. A form of sociology, which, as in the French case, relies on individualized inspiration, seems to provide greater scope for ethnographic inquiry. [97]

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59 CORDES = Comité d'organisation des recherches appliquées sur le développement économique et sociale [Organizing Committee for Applied Research on Economic and Social Development], at the time an organizational unit at the French ministry of planning.

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