The Renaissance of Qualitative Methods

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Abstract: Even though qualitative research was first done more than a century ago, the first texts that tried to define its methodology appeared only eighty years later, in the late sixties. This article explores the reasons why there was such a long delay and why many sociologists who worked using qualitative methods and techniques for a long time did not care about the need for methodological training.

After having seen how qualitative methods and their use in contemporary social science developed, the author outlines the future prospects for qualitative research. With the freedom usually given to those that construct scenarios, we can identify at least five directions: (a) the major formalization of the methods; (b) the development of data analysis; (c) the marriage between computers and qualitative research; (d) the necessity of qualitative methods in a multicultural society; and (e) the implications for applied research.

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1. Introduction

Even though qualitative *research* has been around for more than a century, the first text that tried to define its *methodology* did not appear until the late sixties. *The Discovery of Grounded Theory: Strategies for Qualitative Research* by GLASER and STRAUSS (1967) is in fact commonly known as the first articulated contribution to qualitative methodology.[1] [1]

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1   The book by GLASER and STRAUSS is not an actual *handbook* but only a rationalization of *their* way of doing research (Jaber GUBRIUM, personal communication). Before the appearance of this text there were a few systematic, sporadic and tentative attempts at the participant-obser-vation approach such as the "diary" by MALINOWKI posthumously published in 1967, or the book by JUNKER (1960), a member of the Chicago School, but these works were not widely known.

Why was there such a long delay? Was it, for example, because in the so-called *Chicago School,* where many sociologists worked using qualitative methods and techniques it seemed that the questions of method were for the most part left to the initiative of the individual researcher (MADGE 1962)? Why was it only in the late 1950s—thanks to the works of scholars such as Howard S. BECKER, Aaron V. CICOUREL, Blanche GEER, Barney G. GLASER, John KITSUSE, and Anselm STRAUSS—that an awareness of the necessity of a qualitative method and systematic research was born? Lastly, the most perplexing question: how did qualitative researchers train themselves methodologically in the decades before GLASER and STRAUSS' work appeared? [2]

2. The Past

The reasons for this delay are multiple, complex and contradictory. They are theoretical, technical, and political. In the first place it is probable that the term "methodology" was for a long time considered, at least from the beginning of the 1950s, an exclusive prerogative of the experimental method, followed by the *survey,* almost one of its synonyms. In other words it was rather common to identify the particular and specific procedures used in laboratories and in public opinion poll institutes as the only scientific methods available. This conceptual juncture, on the one hand between methodology and experiment and on the other between methodology and survey, was favored amongst the positivist intellectuals (and then neo-positivists) during the first decades of the nineteenth century when the existence (in addition to the necessity) was supported for one method only, formulated and universal for all sciences. Until the end of the 1950s, in social sciences, the methodologist was a particular type of scholar, devoted to improving the method of survey and of inquiry. Maybe s/he was the only one to believe in the necessity of dedicating himself/herself to the *form* of the knowledge and not only to its substance (theory), as the most authoritative figures in the field did, for the first half of the 20th century. In fact, until the end of the 1950s, methodologists were the first expert scholars in polls and surveys. It was not that they failed to use participant observation, document analysis, or in-depth interviews: Paul F. LAZARSFELD applied these methods several times. But methodological reflection upon them was generally extemporaneous, marginal, and non-systematic.[2] Perhaps they believed (wrongly) that these methods were hardly systematic in their nature or not appropriately connectable. Or perhaps (even worse) they conceived them exclusively as useful means to improve the questionnaire, that is, as instruments to be used *before* or *after* a survey, as support to the one, which remained by far the best form of collecting information (for an example of this position see BARTON & LAZARSFELD 1955). Whatever the answer, qualitative methods continued to possess a subordinate role. Given these premises, qualitative researchers' mistrust regarding methodology can be understood (although not justified): the term was a synonym for quantitative method. [3]

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2 An exception to this tendency is represented by Rensis LIKERT, who dedicated particular care and attention to methodological aspects of in-depth interviewing (see CONVERSE 1987).

In the second place, handbooks or textbooks were never written simply because the need was not felt to codify research practices (CLIVE SEALE, personal communication). While statistics appeared as a doctrinal argument and therefore seemed to require special training, fieldwork seemed to be something that could be learned just by doing it (HOWARD BECKER, personal communication). Everett HUGHES and Herbert BLUMER, both at the University of Chicago, only wrote a few notes (which were rather abstract and related more to the logic of the research than to the techniques of study) because they didn't believe in the necessity of fixed fieldwork techniques (PLATT 1995, p.92). They believed that such methods couldn't be taught but could only be apprehended in the field: "This approach was epitomized by Hughes' injunction, sternly repeated to generations of students, that the only way to learn field methods was to 'get the seat of your pants dirty' in real research" (FIELDING 2004, p.29). This belief meant that the dominant mode of methodological training was not formal instruction but apprenticeship (PLATT 1995; FIELDING 2004, p.29). This learning-by-doing bias resisted for many decades: in most anthropology departments, field research methods were not taught until 15-20 years ago. It was believed that it was enough to send people to the field, where they would have to learn by trial and error (Howard BECKER, personal communications). [4]

In the third place, qualitative researchers seemed to conceive of their research simply as a means to reach a goal (the knowledge) and not as an aim in itself or as an independent research area upon which to build an autonomous field of study—something that happened with the progressive emancipation of methodology, sociology, and psychology. In other words, while the tendency for exclusively methodological research was making its way, qualitative researchers continued to conduct only substantive research with the primary aim of creating sociological theory. If their principal aim was to reach substantive knowledge and/or build theory, rules (especially those as typically punctilious and fussy as methodological rules) might well become an obstacle and distract from the principal aim. [5]

That is probably how qualitative researchers thought at the time, and not without reason: shortly thereafter, MADGE (1962, p.147), an authority on methodology, wrote: "it must be admitted that there can be something rather stultifying in concentrating on methods of investigation, so that there is almost a negative correlation between the rigor of methods and the importance of results." [6]

It is exactly this type of end from which qualitative researchers have always tried to escape. From this perspective, then, this anti-methodological, rule-recalcitrant and romantic attitude, which characterized a historical phase of qualitative re-search, appears comprehensible (again, even if not justified). Furthermore, during this period the intellectual atmosphere changed; scientific philosophers such as FEYERABEND (1975) and "repentant" methodologists such as PHILLIPS (1973), directly influenced by the former, openly led, with well-founded reasoning and arguments, and without reserve, the anti-methodological insurrection. [7]

Unfortunately all this accomplished for sociologists who continued to use survey methods was a strengthening of a prejudice that in time consolidated itself widely: qualitative research produced results of uncertain validity because they were obtained through methods and techniques the reliability of which was unproven. Perhaps the principal reason for the lack of qualitative textbooks lies here: for more than eighty years textbooks were not written because qualitative researchers didn't feel seriously threatened or questioned by their quantitative colleagues (Jaber GUBRIUM, personal communication).[3] Even though HUGHES had developed "participant observation as a distinct methodology because he and his students had to justify their procedures against constant criticism from statisticians" (FIELDING 2004, p.28), only in the 1960s was the necessity felt to contrast the quantitative positions systematically. In fact, according to Barry A. TURNER (1988, p.112), the main reason that impelled GLASER and STRAUSS to write their book was above all political: supplying a methodological text for students and researchers to quote every once in a while when they presented a research project to institutional organizations which were usually opposed to qualitative research. Furthermore, "they set out to codify qualitative methods fearing that otherwise it would disappear from the curriculum under positivistic orthodoxy" (FIELDING 2004, p.30). TURNER (1981) and later STRAUSS and CORBIN (1990) "allowed grounded theory to be taught as systematic procedure that was as rigorous as quantitative methods", even if "Glaser's approach better describes what many qualitative researchers actually do, carrying forward the Chicagoans' dilemma over whether qualitative method can be taught or only learned" (FIELDING 2004, p.31). [8]

GLASER and STRAUSS' book came out in a particularly contentious atmosphere (now absent because qualitative methods are considered legitimate), due to the necessity of defending themselves from quantitative sociologists and supplying students with adequate and reasonable answers (Howard BECKER and Jaber GUBRIUM, personal communication). If we forget this encirclement syndrome, we will not understand why, in the current multicultural context of methodology, that book today appears unusual and parochialist (Jaber GUBRIUM, personal communication). [9]

3. The Second Return of Qualitative Methods

As is already well known, qualitative methods were introduced into sociology at the end of the first decade of the twentieth century through the work of the Chicago School (Robert E. PARK, William I. THOMAS, Florian ZNANIECKI and many others). Its dominion continued for about twenty years, until the survey took on a dominant role. The 1940s and 1950s witnessed years of supremacy for Parsonian theory, denoting the prevalence of quantitative methods. At the beginning of the 1960s, we had the first resurgence of qualitative methods due to the work of anti-Parsonian theoretical approaches: symbolic interaction and

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3 Strong friction between the two components had occurred a long time before. For example, until 1935 qualitative researchers were often publishing their work in the *American Journal of Sociology,* the best-known sociology journal. Later, it became difficult to publish without "a sea of equations decorating one's argument" (FIELDING 2004, p.30) so qualitative researchers created their own journal called the *American Sociological Review.*

ethnomethodology gave a fundamental impulse to the return of qualitative methods. Nonetheless, in the course of the 1960s they were again obscured by the return of quantitative methods, favored by the diffusion of software that permitted analysis of enormous quantities of data in a relatively brief period of time. In fact, quantitative research, which until the end of the 1950s belonged to the pre-computer era and was conducted by hand with pen and pencil and the help of simple table calculators, punch cards, etc., became an advanced technical enterprise. As such, it loaded itself with big promises and expectations that it could not fulfill. Towards the end of the 1960s we saw a second return of qualitative methods with a definite legitimization and institutionalization (FIELDING 2004, p.32). Different from the two previous phases (the 1920s and 1930s of the Chicago School, and the symbolic interaction and ethnomethodology of the 1960s), this new resurgence produced a methodological literature, including the first textbooks on participant observation, in-depth interviews, discourse and conversation analysis, textual analysis of documents and, more recently, on focus groups. [10]

This tendency was amplified during the 1990s, which saw a sudden, authentic increase of qualitative methodological texts. For example, if we look at the catalog of Sage, one of the best known and internationally widespread publishers, we notice the following distribution: between 1980 and 1987 10 textbooks were published; between 1988 and 1994, 33 textbooks were published; between 1995 and 2002 well over 127 were published. [11]

The reason why qualitative methods became fashionable again in the 1980s and 1990s is not clear and not easy to explain. It appears to have little to do with an intrinsic element of the methods themselves, that is, the ostensible capacity of qualitative methods to describe, understand, and explain social phenomena. Instead, the reasons are to be found elsewhere, for example, following the sociology of scientific knowledge in relation to science and society. As MELUCCI (1998, p.21) observes, you cannot help but notice that during the 1980s, in business and management techniques, the concept of "quality" with all its derivatives (total quality, total quality management, quality control, the quality of services, etc.), has become fundamental to the point of radically modifying the traditional productive processes, commercial and managerial. Probably the success of quality over quantity, and of flexibility over standardization (two fundamental hinges both of the survey and of Fordism) had repercussions in sociology, an area of study that, more than any other, is sensitive to social change. Despite this, while there was a decline in Fordism, there was no corresponding decline in the survey, even though its supremacy was already weakened. [12]

4. The Future

After having seen how qualitative methods and their use in contemporary social science developed, we can ask ourselves: what are the future prospects for qualitative research? With the freedom that is usually given to those that construct scenarios, we can identify at least five directions: (a) the major formalization of the methods; (b) the development of data analysis; (c) the marriage between computers and qualitative research; (d) the necessity of qualitative methods in a multicultural society; and (e) the implications for applied research. [13]

4.1 A major formalization of the methods

We noted earlier that the main elements of evolution and diffusion of qualitative research were its institutionalization (through the introduction of qualitative methods courses in universities) and the birth of a copious production of textbooks. This trend is in continuous growth and does not seem to have encountered any setbacks. It is accompanied by the monumental (and costly) works published recently: DENZIN and LINCOLN (1994; 1999), BRYMAN and BURGESS (1999), TITSCHER, MEYER, WODAK and VETTER (2000), ATKINSON, COFFEY, DELAMONT, LOFLAND and LOFLAND (2001), BRYMAN (2001), REASON and BRADBURY (2001), GUBRIUM and HOLSTEIN (2002), FIELDING (2003), SEALE, GOBO, GUBRIUM and SILVERMAN (2004), MILLER (2005). [14]

All the same, qualitative research is taking new paths and is building a "new qualitative language approach" (GUBRIUM & HOLSTEIN 1997), designed to carry on a dialog with qualitative methods facing criticisms and problems at this level. Within this development, practical proposals are situated to improve the *reliability* of the instruments and the *validity* of the results of qualitative research (HAMMERSLEY 1990, 1992; SILVERMAN 1993; MILES & HUBERMAN 1994; PERAKYLA 1997; SEALE 1999; GOBO 2001). These include suggestions to sample appropriately (CORSARO 1985, STRAUSS & CORBIN 1990, BECKER 1998), instructions on how to collect field notes systematically (SCHATZMAN & STRAUSS 1973; SORADKEY 1980; EMERSON, FRETZ & SHAW 1995) and how to construct models with ethnographic data (CORSARO HEISE 1990), suggestions for a more rigorous research design (MARSHALL & ROSSMAN 1989; LECOMPTE & PREISSLE 1993; MASON 1996; MAXWELL 1996), and suggestions for the communication of results (WOLCOTT 1990; MARX 1997). These are the realms in which major formalization is being introduced, formalization which, if protected from the excesses of the mathematicalization of social sciences, can be conjugated with a "reflexive method." In fact, this formalization encourages the researcher to explain his/her reasoning, intuitions, and tacit knowledge, for the purpose of offering to the readers sufficient information for criticism and dialog within the scientific community. Further formalization would introduce a further intersubjectivity and qualitative research would benefit from the increased precision and conceptual clarity profile. Indirectly this tendency presents itself as the last *chance* for the more reactionary and conservative sectors of methodology to acknowledge the science of qualitative

research. Obviously, we cannot forget that, as Howard BECKER puts it, even if there are general types of problems that are the same for all field studies (for example, how to access the field), the solutions are always specific and relative to the time, space, and subjects of that particular study and thus not amenable to standardization (personal communication). [15]

4.2 The development of data analysis

Until a few years ago, one could see a curious change of roles: quantitative methods were known for being strongly developed on the "data analysis" side (so much so that this expression immediately brought to mind statistical techniques), while qualitative methods showed a lot of experience on the "data collection" side. The strength of the one was the weakness of the other and vice versa. Recently however, things are changing with respect to data collection for quantitative methods and data analysis for qualitative methods, the true Achilles' heels for these approaches. [16]

Following the initial attempts of the 1980s (on the part of the conversation analysts and discourse analysts), in the 1990s data analysis was considered the central problem and no longer elusive (STRAUSS & CORBIN 1990; SILVERMAN 1993; MILES & HUBERMAN 1994). SILVERMAN (2000) underlines several times how data analysis is decisively more important than the collection of data itself, so much so that, to abbreviate or facilitate this phase, he encourages working on data gathered by other researchers (secondary analysis) or otherwise found in the public sphere (documents). A remarkable inversion occurred with respect to the traditional qualitative methods that privileged other phases of investigation with a pathetic adoration for research conducted in exotic or esoteric *settings.* The attention given to data analysis has already produced interesting procedures for the testing of hypotheses of deviant cases (BECKER & GEER 1960; MEHAN 1979; FIELDING & FIELDING 1986), and for member validation (GOULD, WALKER, LANSING & LIDZ 1974; DOUGLAS 1976; SCHATZMAN & STRAUSS 1973; BLOOR 1978; EMERSON & POLLNER 1988, 1991), together with soph-isticated procedures for the analysis of talk (conversation analysis and discourse analysis), in-depth interviews (RIESSMAN 1992) and videotaped materials (GOTTDINIER 1980; CORSARE 1982; BAUER & GASKELL 2000). [17]

4.3 The marriage between computer and qualitative research

"The blind love between computer and content analysis" that ROSITI (1980) ironically stigmatized has recently bloomed between computer and qualitative research. There are many software packages (CAQDAS), born initially to analyze both documents and verbal texts, which were later developed to analyze musical texts and audiovisuals. If *content analysis*, after an initial love, never played then the protagonist role within quantitative methods producing many delusions among first-time *fans,* as *textual analysis* seemed to be headed toward a better destiny: courses dedicated to CAQDAS, new versions of software, new publishers born purposely for the *business,* and increasing research articles based on CAQDAS. As far as methodology goes, with the use of software, as precision and reliability

of classification increases, it improves its rigorous reasoning and it guarantees data inspection, which was considered one of the historic pitfalls of qualitative research. [18]

4.4 The necessity of qualitative methods in the multicultural society

Contemporary societies are characterized by highly diverse ethnic and linguistic concentrations: while at one time the presence of "foreigners" was a rare phenomenon, today even in small Italian towns we find people from Asia, South America, Africa, Eastern Europe, and the Balkans. Many of them do not know the Italian language well, are poorly educated, suspicious of native researchers, and competent in different interactional models and rituals from the Western world. These are all characteristics that make the use of a questionnaire particularly difficult, and the necessity arises to find methods and techniques that are less standardized, more flexible, and more respondent-centered, that are capable of adapting to the social actor who is under study and to his/her linguistic, cultural, and social difficulties. Tools or instruments must be found that, instead of imposing a particular interactional and response model, should be at least partially adaptable to the subjects. Maybe this is one of the reasons for the increasing diffusion of qualitative methods: their capacity and flexibility to adapt to different situations. [19]

4.5 Applied research

In sociology, qualitative methods had their apprenticeship in the study of professions, social deviance, and urban sociology. Those that gave them a strong boost were Robert E. PARK and William I. THOMAS (the founders of the Chicago School), researchers who were zealous in a grand practical sense. In a way, we can say that qualitative methods are born with a strong practical vocation, turned towards finding answers to problems of immigration, integration, and social deviance. In the past century, this vocation has taken different paths, among them that of applied research or the "clinic." This term, rather sadly, paints that vast sector that gives counseling to an enormous number of clients: firms, institutions, services, corporations, and communities. Even if the demand is often born from the necessity of discovering *tacit knowledge* and describing *processes* —that is, practices that are more or less organized in local and situated cultures —applied research also puts upon itself the aim to induce change in the actors, adding to the knowledge purpose an operative intention. Applied research has seen, then, the birth and the development of different approaches and particular techniques, which have been applied in many sectors:

- health and deviance (social services, AIDS, drug addiction, hospitals and clinics, doctor-patient relationships)
- companies (consulting)
- the so-called Non-Places (supermarkets, stations, airports)
- institutions (schools, jails, court houses, police districts)

- market research (even if limited to depth interview, focus groups and mystery shoppers)
- evaluation research [20]

In applied research, next to the traditional qualitative methods, other "active" methods have been added with the intention of letting the actors of society realize the situation in order to modify it. Examples of this type are the experimental and social-psychological approaches of *action research* by Kurt LEWIN and his collaborators. Leon FESTINGER and Harold H. KELLEY, had a big influence on the birth of other approaches, such as: a) the managerial *cooperative research* of Ronald LIPPITT and Gordon L. LIPPITT; b) the *participatory research* of William Foote WHYTE that follows the social-technical approach of humanization of the organizations; c) the *action science* of Chris ARGYRIS and colleagues that is situated in the process of consulting; d) the *empowerment* of Julian RAPPAPORT that deals with the improvement of the individual capacities of the actors; e) the *intervention sociologique* of Alain TOURAINE; f) the *socio-drama*; etc. [21]

Today's tendency, in harmony with these approaches, sees the continuous invention of techniques and *ad hoc* procedures that are born according to the needs of those who ask for consultation, or to the subjects' new problems, or according to the arguments studied. Recently to this end the following have been used:

- focused ethnography (KNOBLAUCH 2001), a technique that contrary to traditional ethnography conceives only a brief stay in the field because the objective is to study specific, circumscribed and aimed problems;
- shadowing, an ethnographic technique that consists in following, "as a shadow," a subject in his or her daily activities (WOLCOTT 1973; BRUNI, GHERARDI & POGGIO 2004);
- on line research (MANN & STEWART 2000);
- the double's interview (ODDONE, RE & BRIANTE 1977, GHERARDI 1990), in which the interviewee is asked to imagine the interviewer as his double and to give him all the useful suggestions to be substituted on his job by the double-interviewer without being discovered by his colleagues;
- secondary analysis of archival data (CORTI & THOMPSON 2004) or of previous research (AKERSTROM, JACOBSSON & WASTERFORS 2004). [22]

Acknowledgments

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