Free from Numbers? The Politics of Qualitative Sociology in the U.S. Since 1945

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It has been well established now that quantification is not only a means to produce knowledge, but also a means of power. This insight has given rise to the famous and important body of works on *social studies of quantification* (Daston, 1988; Desrosières, 1998 [1993]; Espeland & Sauder, 2007; Gigerenzer et al., 1989; Krüger et al., 1987; Porter, 1995), which studied in many diverse fashions the historical conditions of the production of numbers and their social effects, denaturalizing quantities while at the same time re-specifying their authority. Most of these works suppose that, first, there was a state of affairs without numbers; second, that measures have been applied on it; and third, that the situation has finally become quantified. Desrosières (2008), who can rightfully be taken as the primary representative of this tradition, states this idea in a very clear equation: "quantification = convention + measurement" (Desrosières, 2008, p. 10). The crucial insight of this proposition is that this process is

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a social one–and not a natural or straightforward one–that deserves to be problematized and understood with the tools of the social sciences.

Yet, the contemporary excitement around "big data" makes one wonder if the problem should not be reversed. We hear today that the planet is increasingly populated by digital data (for example: "90% of the data harvested since the beginning of humanity have been generated in the last two years" [Dupont, 2015]). But we know that it is only an exaggeration in the long history of people being mesmerized with the mechanized production of the quantitative–to which the "big data" phenomenon belongs since it comprises many numbers, if not anything else. One must not forget that the decades 1820–1840 already witnessed an "avalanche of printed numbers" (Hacking, 1982). The invention of the Hollerith machine at the end of the nineteenth century and its adoption by bureaus of public statistics all over the world produced a "revolution in data processing" (Austrian, 1982). With the development of polls and sample surveys, The New Deal was a period during which the U.S. was entirely "statisticized" (Didier, 2009). Every period has had its own quantitative revolution related to technologies of data production and to creativity in the use of data. The fuss around big data proves only that our current era makes no exception: it is, as it was, filled with quantities.

Thus, since society is quantitative through and through, the real mystery might not be the amount of data that circulates and governs but on the contrary, the existence of social spheres pretending to remain free from numbers. If the world has already been quantified since at least the first half of the nineteenth century, are there some spheres that could remain exceptions, and how is this possible? What does the activity of purifying a social sphere from numbers consist of? What are the political endeavours associated with such a goal? Or, to put it differently, how can we account for the political production of the border of qualitative enclaves which exclude quantities?

To tackle these questions, I will go back to the history of what is now called *qualitative sociology*. Indeed, sociology is a discipline in which the great founders never chose between quantification and non-quantification. In France, Emile Durkheim and Gabriel Tarde, who were opposed in every respect, had two main points in common: first, each was the leader of a powerful current of sociology and strove to institutionalize it according to his own definition (against the other's), and second, both relied on quantitative reasoning among other arguments, as *Suicide* on the one hand and *The Laws of Imitation* on the other attest

(Durkheim, 1986 [1897]; Tarde & Parsons, 1903). In Germany, Max Weber, along with his definition of the longstanding "verstehen", also performed quantitative surveys (Brain, 2001; Pollak, 1986). Finally, in the U.S., the Chicago School of sociology never *chose* between the two (Abbott, 1999; Chapoulie, 2001). Sociology was founded as a science commonly using quantification as one of its diverse cognitive tools and methods. It entertained a "relaxed" relationship to quantities and qualities (to use Glaser and Strauss's (1967) expression). Thus, the branch of U.S. sociology that came to be labelled "qualitative sociology" during the 1970s made an astonishing move, apparently of the ascetic sort, in defining a discipline that would be freed from quantities.[1]

Why would one distinguish a sub-discipline by its absence of numbers? How did the conceptual pair "qualitative vs. quantitative" come to settle within sociology? What were the conditions in which sociology was produced and the publics it addressed that might explain this link? Finally, is it even possible to eradicate quantification and stay with conceptions encompassing qualities only?

Using the methods of the sociology of quantification, I will pay attention to both the epistemic and political forces that participated in the production of the border between qualitative and quantitative in sociology.[2] I will inquire into the political worth of the qualitative. It was within a very specific power field, ranging from the constitution of the Welfare State after WWII to the radicalism of the 1970s and finally ending in the liberal 1980s, that those who would ultimately defend a "qualitative" sociology forged and used their epistemic arguments separated from the quantitative. I will pay special attention to how these two aspects of the story were intermingled.

These questions can best be understood when it is clear from the onset that here "quantitative" has two different meanings. We will see that "quantitative" analysis had been defined by mainstream sociologists as one single method, that of *survey sampling or polling*. This is a first definition of quantitative, the one of our "actors" or "members". But we can see furthermore that there have long been many other methods of quantification, many uses of numbers, and, as has been proven by the late Alain Desrosières, that these different methods of quantification are consistent with different political endeavours (Desrosières, 2003).

Finally, it is worth mentioning that this paper is a sequel to the question of the appearance and legitimization of quantitative surveys in the American Government during the New Deal (Didier, 2009, 2020). Here, I follow the later fate of this method and trace how after WWII it came to be criticized. I aim to sketch the whole social life course of a statistical method, from its appearance to its decomposition. This paper is also an inquiry into the relationships between sociology and politics. The position born with surveys during the 1930s and the 1940s, of the sociologist as an expert advising political power, is here contrasted with that of the sociologist as a critic of any association with the power elites, the sociologist as a radical, a position that fully developed after WWII and came to be closely associated with "qualitative" methods. Finally, this paper is also a contribution to the "sociology of quantification". Rather than asking how qualitative things are quantified, I reverse this question and ask how it is possible, if ever, in a world already filled with quantities, to try and purify portions of it in the hope of establishing a "qualitative" enclave.

My first point will consist in emphasizing the seminal role played in the 1950s by Herbert Blumer and Aaron Cicourel in the fight against Lazarsfeld's definition of qualitative analysis. Both opposed a specific statistical method—surveys for the first and official statistics for the other, and they were not against quantification in general, which they in fact practiced. They opposed a specific political use to which the statistical method was associated. Then, we will see how their conclusions were refurbished by the young radicals in the 1960s and 1970s as a means to fight against the elite of the Welfare State. Finally, we will see that "qualitative sociology" as such appeared only during the 1970s as a weird association between the Lazarsdfeldian promoters of surveys and the neo-radicals opposed to it.

Excluding Quantities?

The two main sociologists embodying the tradition of "qualitative sociology", as far as they explicitly addressed their relationship to quantification, were Herbert Blumer and Aaron Cicourel. I will analyse their conception of the border between quantitative and qualitative research. I will thus clarify their critique of numbers and the social context in which they were expressed. Especially, I will clarify their relationship to the work of Lazarsfeld.

C. Wright Mills, in his *Sociological Imagination* (Mills, 1959), had a very influential critique of "abstracted empiricism" as a kind of sociology which, while transforming itself into a gigantic bureaucracy, turned the American public into a series of *masses*. Unfortunately, Mills died too young (1962) to take part personally in what later came to be called "Qualitative sociology" and actually, in his writings, never used the dichotomy qualitative/quantitative at all. So in our story, we shall treat his work as a resource for our actors, but not as an actor by himself.

Interpretation and Determinism

Herbert Blumer is credited with the invention of *Symbolic Interactionism*. This approach to human group life is deeply influenced by the philosophy of George Herbert Mead and the American pragmatist tradition. It locates the social primarily in situations of interaction between humans and between humans and objects. It focuses on the fact that members' action is guided and formed by a *process of interpretation* of the situation in which they are involved. This process of interpretation is an active one, and not a passive submission to outside forces. In Blumer's own words, members' "behavior with regard to what it notes is not a response called forth by the presentation of what it notes but instead is an action that arises through the interpretation made through the process of self-indication" (Blumer, 1969, p. 14). Placing the concept of interpretation at the heart of his concepts, Blumer has today among sociologists an "image as purely qualitative" (Abbott, 1999, p. 51). Indeed, *symbolic interactionism* became one of the core components of qualitative sociology.

The history of the growth of Blumer's opposition to quantification is quite complex. One has to keep in mind that until WWII, Blumer was in a very powerful situation in the American sociological field. He was a Professor of Sociology at the University of Chicago's Department of Sociology, one of the most distinguished and powerful departments in the country. From this position, he witnessed the fairly quick establishment of the partisans of statistical surveys, especially at Columbia.

The American Soldier

Blumer's powerful position was questioned in particular by the publication of *The American Soldier* edited by Samuel Stouffer and colleagues (Social Science Research Council (U.S.), 1949), a five-volume sociological study of the Army during the war. As Schweber (2002) shows, this book not only encountered huge public success, but was also heralded as the example to a *new* approach of social science, making important use of statistics. It bore on trends that began in the 1930s with the growing importance of polls on the one hand, and of the quantification of surveys on the other, associated with the growing power of welfare institutions, which were the primary users of this kind of knowledge, both at the local and the national level. *The American Soldier* was seen as the symbol of the will to promote statistics as *the* authoritative method in sociology. And, also problematic from the point of view of Blumer, it was associated with Harvard, since Stouffer, who earned his PhD from Chicago, had been hired by the University located in Cambridge, Mass., in 1946.

A panel was organized in 1949 by the American Sociological Association to discuss the book. Blumer was invited, and apparently criticized the book vehemently. The authors of the 5th and last volume of *The American Soldier* wrote that he adopted a "rivalrous posture" stated in a "vigorous negativism, which leads to the extreme attitude we have designated as *diabolic*" (Merton & Lazarsfeld, 1950, p. 227). His talk has apparently not been published, but Howard Becker (1988) states that the arguments were very close to his 1948 paper on polling, later re-published as the last chapter of *Symbolic Interactionism* (Blumer, 1969).

In this article about polls, Blumer does not attack quantification as such. He even states that he uses numbers himself, but in a very peculiar way: "I shall indicate by number the [six] features to be noted" (Blumer, 1969, p. 198). It is not that common to read a text composed in six parts!

He expresses two main criticisms of polls. First, polling does not *define* "*public opinion*", *its object*. It suffices itself by applying a technique, which indeed produces data, but it never takes time to define the concept on which data is produced. On the contrary, it relies on the "narrow operationalist position that public opinion consists of what public opinion polls poll" (Blumer, 1969, p. 197).

A second criticism is exposed in six points. The argument is that polling does not respect the actual "realistic" structure of public opinion formation. In particular, there are "key people" who play an important part in the production of public opinion. Yet, these processes through which public opinion is expressed are not consistent with the *sampling techniques* used by polls:

> In my judgment the inherent deficiency of public opinion polling certainly as currently done, is contained in its sampling procedure. Its current sampling procedure forces the treatment of society as if society where only an aggregation of disparate individuals. Public opinion, in turn, is regarded as being a quantitative distribution of individual opinions. This

way of treating society and this way of viewing public opinion must be regarded as markedly unrealistic. (Blumer, 1969, p. 202)

Blumer admits later in his text that polls did succeed in predicting the elections (of Roosevelt in 1936). But, "a ballot cast by one individual has exactly the same weight as a ballot cast by another individual. In this proper sense, and in the sense of real action, voters constitute a population of disparate individuals" (Blumer, 1969, p. 205). In the case of elections proper, the structure of the electorate is realistically comparable to that of a sample. But this is not the case outside of this very rare case.

Thus, Blumer argues first that opinion polling is "logically unpardonable", because it does not define its object of inquiry, and second that it does not respect the body of knowledge derived from empirical observation and from reasonable inference that one already has about the nature of public opinion. There is a third scandal in the eyes of Blumer, which is kept implicit in his text. It is that, given the success that these techniques encounter, the very key players in the formation of public opinion, to whom he gives such an important role, seem nonetheless to adopt and use polls in their endeavour.

He himself sees the social role of sociologists very differently. He served as an arbitrator for the steel industry during WWII. Arbitrators, in his view, are not "experts" advising the Government, but act as facilitators helping both parties finding a settlement in their dispute. As Cantril (1939) interestingly writes (since he was one of the founders of opinion polls), this role presupposes "objectivity" in a very different manner than that of the expert adviser.

These criticisms from Blumer can indeed be transposed to the surveys used in *The American Soldier*. An army, being strictly hierarchical, is anything but a population of disparate individuals. The "opinion" of an army is not defined in the book. Finally, for these very obvious reasons, it must have appeared very strange to Blumer that the commanders of the Army might appreciate the book. The opposition between the two kinds of sociology became even more violent when Stoufffer's book was used as a weapon for a direct and nominal attack against Blumer.

The Qualitative as Propaedeutic

Quantifiers replied to Blumer. In 1951, Henry Zentner, a young assistant professor at Stanford, published a paper (Zentner, 1951) in which he unearthed a contribution of Blumer about "Morale" published during the war (Blumer, 1943). He presented it as "the most careful and systematic conception" of morale at the time when it had been written, and proposed "to test, against the data reported in *The American Soldier*, the validity of Blumer's conception of the generic nature of group morale" (Zentner, 1951, p. 298). Zentner extracted information from the charts of the book and compared them to Blumer's analysis. He pinpointed what he saw as many weaknesses and went on to argue that Blumer's conception of morale was "grossly inadequate" (Zentner, 1951, p. 306). He concluded that morale was better defined by opinion surveys than by Blumer's methods.

Blumer felt compelled to comment. He wrote "why Mr. Zentner believes that he refutes my analysis is mystifying" (Blumer, 1951, p. 308). His own contribution was about the morale of the civilian population when Stouffer's book was about the army. Hence, Zentner's paper "does not even test my analysis much less refutes it", since "a theory or proposition is tested empirically by applying it to an instance of what the theory or proposition logically covers, not by applying it to something that falls outside of such a logical class" (Blumer, 1951, p. 308). There was clearly an attack but, argues Blumer, it did not hit. As he had stated earlier about polls, the object of inquiry is ill-defined and in this case it creates catastrophic confusion.

It is important for our purpose to note that the question of quantification as such is entirely absent from the debate.

The attack was bold coming from a young man such as Zentner, and maybe too bold since he seems to have completely disappeared from the field after the bout. But he expressed an idea that would have very important consequences: that Blumer's analysis was "essentially speculative and propaedeutic" and still needed to be empirically tested to gain actual authority (Zentner, 1951, p. 297).

This furrow is precisely the one that, since the 1940s, Paul Lazarsfeld was digging. Lazarsfeld repeated essentially the same message: "There is a direct line of logical continuity from qualitative classification to the most rigorous forms of measurement" (Lerner & Lasswell, 1951, p. 155). Or, stated slightly differently a few years later: "Not only is qualitative analysis large in volume, but it plays important roles in the research process, by itself and in connection with quantitative research" (Lazarsfeld & Kendall, 1982, pp. 239–240). His argument was first and foremost that qualitative and quantitative social science existed as two extremities on a continuum of methods. Lazarsfeld uses the pair of concepts with a frequency not encountered anywhere else—in particular, it must be insisted upon that Blumer never used it. Lazarsfeld is the one who decisively introduced the conceptual pair in sociology, and thus insisted also on the importance of the qualitative. It is most probable that his own sources, even though they are not explicit in the literature as far as I can tell, are in the Vienna Circle from where he came. He brought the dichotomy with him while emigrating to the U.S.

But it was only to subordinate qualitative research to quantitative research. He gives a biographical explanation to this hierarchy: as an assistant to Bühler in Vienna before immigrating to the U.S., he worked on the "qualitative attributes" of categories. And after arriving in the U.S. he discovered it would have helped him to use the "statistical methods" found in America (Zeisel, 1950, p. xvi). But he also gave many scientific justifications to the hierarchizing of the two kinds of research.

First of all, what he calls qualitative research is a necessary propaedeutic. One cannot directly begin any sociological work with statistics. Qualitative research is a first obligatory passage point (to use an awfully anachronistic concept):

> The operations of qualitative analysis which are raised essentially prior to quantitative research [are]: observations which raise problems, the formulations of descriptive categories, the uncovering of possible causal factors or chains of causation for a particular piece of behavior. (Lazarsfeld & Rosenberg, 1955, p. 267)

Thus, the qualitative steps in research are necessary for two reasons: they help establish the categories of further quantitative analysis—and categories must logically precede quantification. And they indicate or suggest possibilities of further relations between factors. The uses of "these operations [are to] stimulate and focus later quantitative research, and they set up the dimensions and categories along the stub of the tables, into which quantitative research may fill the actual frequencies and measurements" (Lazarsfeld & Rosenberg, 1955, p. 267).

But at the same time, the qualitative is essentially defined by the fact that it is "unsystematic", "impressionistic", not "objective" enough (Lazarsfeld & Rosenberg, 1955, p. 166) (1951, 166), it "remains an art" (Lazarsfeld & Rosenberg, 1955, p. 250):

> Research which has neither statistical weight nor experimental design, research based only on qualitative descriptions of a small number of cases, can nonetheless play the important role of suggesting possible relationships, causes, effects, and even dynamic processes. (Lazarsfeld & Rosenberg, 1955, p. 261)

The qualitative is defined by its essential incompleteness as regards the scientific endeavour, which only the quantitative can fulfil. The qualitative is systemically associated with the subjective, the personal, so that to become fully scientific it has to be made quantitative, that is, independent of any personal perspective, fully *objective*. As Daston (1992) put it, numbers help to produce "aperspectival objectivity"—a "view from nowhere"—where the places and persons are extracted from their use. Numbers also permit "mechanical objectivity" (Porter, 1995), a set of rules about how to make and deploy numbers that contain the discretion and biases of those using them.

Then, in the process of quantifying the qualitative, some variables remain what was called "qualitative" because they did not refer directly to a quantity. For example, the sex variables (male, female), race (Caucasian, Blacks, etc.), even modalities built from a quantitative variable (income brackets, etc.) are said to be qualitative. These types of variables were called qualitative but still, they allowed a statistical treatment.

Lazarsfeld became undoubtedly the star of sociology in the 1950s. He was a professor at Columbia, and earned very important research contracts thanks to the Bureau of Applied Social Research. He was also advising political figures. To give an example, it could not have escaped Blumer that Lazarsfeld had been invited to the Stanford symposium on "policy science" financed by the Carnegie Foundation on which the 1951 book is based, and Blumer was not. The new quantitative sociology, to use Lazarsfeld's vocabulary, was eclipsing the old Chicagoan. The pollster was stepping on the ground of the arbitrator. Blumer could not let it happen, especially since he was weakened even in his own university (Abbott, 1999).

Interpretation Cannot Be Overlooked

Blumer could not accept that his sociology was to be turned into a servant of an allegedly more objective one. He replied, and chose to name his enemy "variable analysis". This expression indicates the aim "to reduce human life to variables and their relations" (Blumer, 1969,

p. 127). An independent variable is identified and the analyst aims at measuring its effect on a dependent variable. Concretely, it implies the use of a questionnaire and of survey methods to gather field data that is to be transformed into variables. The variable is not *necessarily* quantitative though, even if it is indeed most of the time. This definition of the variable is a clear attack against the propositions of Paul F. Lazarsfeld.

Against the "application of the variable analysis to human group life", Blumer saw three "shortcomings" and one "crucial limit" (1969, p. 132). The first shortcoming is that there is apparently no "limit to what may be chosen or designated as a variable" (1969, p. 128). The sociologist can choose anything to be a variable that acts upon another variable, to the effect that often they do not address the real problem that is at hand in the situation studied. The second shortcoming is that often the variables are not generic and thus lack any abstract character. Most of the time, variables are in fact "bound temporally, spatially, and culturally" (1969, p. 130) and thus cannot provide any theoretical grasp of the situation. Finally, the variables rarely give the "fuller picture", the "context" in which members interact, even though for Blumer the latter is crucial to understand their action. These are shortcomings, because they are not necessary consequences of the variable analysis, they are simply often observed in practice.

Much worse, there is a limit within variable analysis that was not overcome until the publication of his paper. It does not account for the actual process that takes place in between the action of the independent variable at the beginning of any social process, and the dependent variable as the terminal part. "The intervening process is ignored or, what amounts to the same thing, taken for granted as something that need not be considered" (1969, p. 133). "One is content with the conclusion that the observed change in the dependent variable is the necessary result of the independent variable" (1969, p. 134).

But Blumer insists that any modification of the dependent variable has necessarily occurred through a process of interpretation. "The interpretation is not determined by the variable as if the variable emanated its own meaning. If there is anything we do know, it is that an object, event or situation in human experience does not carry its own meaning; the meaning is conferred on it" (1969, p. 134). The variable analysis simply discards the very core of any social action. Blumer concedes that, sometimes, it happens that interpretations are stabilized, it "occurs and recurs". But this must be verified each and every time since "anything that is defined may be redefined" (1969, p. 135). Finally, Blumer states that "the question of how the act of interpretation can be given the qualitative constancy that is logically required in a variable has so far not been answered" (1969, p. 136). More generally:

> In the area of interpretative life, variable analysis can be an effective means of unearthing stabilized patterns of interpretation, which are not likely to be detected through the direct study of the experience of people. Knowledge of such patterns, or rather of the relations between variables which reflects such patterns, is of great value for understanding group life in its "here and now" character and indeed may have significant practical value. All of these appropriate uses give variable analysis a worthy status in our field. In view, however, of the current tendency of variable analysis to become the norm and model for sociological analysis, I believe it is important to recognize its shortcomings and limitations. (Blumer, 1969, p. 137)

Thus, the variable analysis is content in studying the part of social life in which the interpretative process is either absent or stabilized. But for Blumer, this seems to be obviously a very small part of life, and the less interesting one, the part of life that is completely *deterministic*. He criticizes variable analysis for its incapacity to account for interpretative operations performed by humans, part of what has been called much later "the creativity of action" (Joas, 1996). Now, does this criticism of variable analysis mean that Blumer rejected any quantification and was purely "qualitative"? I would like to prove the contrary.

The Quantifier Blumer

Blumer was against surveys, but he was for quantification, conceived very differently. First, it is important to keep in mind that Blumer did not accept the dichotomy qualitative vs quantitative that appeared in the writings of Lazarsfeld. He rarely used the word "qualitative", and avoided elaborating on the dichotomy itself. He chose to criticize "the variable analysis" and not the quantitative techniques. Variables were not doomed to be limited in scope and impetus; they were so only in the hands of limited sociologists whose investigations are limited in scope and impetus.

These precautions were not only rhetorical. Some of Blumer's first publications were two books that both came out in 1933. One was *Movies and Conduct* (Blumer, 1933), and the other *Movies, Delinquency and*

Crime (Blumer & Hauser, 1970 [1933]). The second book was co-authored with Philipp M. Hauser, a master in quantitative techniques who would eventually become the Director of the Bureau of the Census (1949), and this book was full of figures and tables! Both books asked the question whether the movies, which in the 1930s had become one of the most popular entertainment industries, lead youth to crime because crime is depicted in motion-pictures, or on the contrary, whether motion-pictures protect them from becoming criminals, because they show its condemnation? Both books utilize data which was gathered from nearly two thousand students through interviews, observations and students' "motion-picture autobiographies" in which informants were asked to write in narrative form their motion-picture experiences. In addition, a survey questionnaire was distributed to two populations: a sample drawn from high-school children and a sample drawn from young inmates (male and female). The surveys are analysed only in the book with Hauser. Thus, even though it is clearly Hauser who performed the quantitative analysis, Blumer did publish some quantitative analysis under his name.

Apparently, Blumer did not remember it as an error of his youth, but quite on the contrary, as twenty years later, when in 1952 he would leave Chicago, where he had lost much of his personal influence, to join Berkeley, he tried for several years to recruit Hauser with a "formidable salary". Upon Hauser's refusal, he made comparable offers to quantitativists Leo Goodman and Otis D. Duncan, who also ended up refusing (Abbott, 1999, p. 51).

These events prove that Blumer really thought that it was possible to produce interesting quantitative analyses, even in the case of methods involving "variable analysis". He fought hard to colour quantitatively the team that he had been dreaming to build up in California. Abbott (1999) argues rightly that this team was the result of a community of a Midwestern habitus. Sure, but this community would have been discarded if their sociology had been incompatible.

Even after his teamwork with Hauser was over, Blumer continued to produce research using numbers, but of a completely different nature than the one he addressed in his criticism of "variable analysis". Apparently, Blumer did not participate in the conduct and analysis of surveys anymore, but in his empirical work he always listed the things that were indicated and interpreted by the members of the interaction he observed. And Blumer counted the elements of these lists.

An impressive example can be taken from a posthumous book on industrialization (Blumer, 1990). In the introduction to the book, the editors Maines and Morrione insist on the fact that it is not possible to determine the exact date when its content was written, because the book comprises a collection of essays which were published at different times. But Blumer first wrote on industrialization when he was in Brazil in 1958 and chapter six of the book was published as an article in 1971. Thus most of it has probably been written after *Symbolic Interactionism* (Blumer, 1969) was published.

In this book, Blumer asks how to conceive "industrialization in terms of how it operates on group life" (1990, p. 42). Strikingly, Blumer goes on listing nine lines, or dimensions—nothing more and nothing less—through which industrialization entered group life:

> In its gross aspect, industrialization is the introduction or expansion of a manufacturing system of production. As an agent of social life, it has to enter into group life. This sets the very important tasks of identifying the lines of entry, instead of merely juxtaposing the manufacturing system to group life. [...] My analysis leads me to identify nine lines of entry that are important, common to industrialization and, I believe, reasonably comprehensible of what occurs in industrialization. [...] The scheme brings us out of the vagaries and confusion that encumbers scholarly conceptions of industrialization. The scheme is definitive, it is tied to the manufacturing scheme of production, and it allows an empirical tracing out of what happens socially in industrialization. (Blumer, 1990, p. 49)

Later in the book, he questions whether there could be one more dimension, only to reject it. Thus Blumer holds on firmly to the number 9. This example is striking. Not only is it rare to insist on the number 9, but it is not an isolated case in his writings. Very often Blumer looks for the entities that are "taken into account" in an interaction, and actually counts them for the sake of clear and distinct conception of the process. It is impressive to note how often (should I count?) he uses the rhetorical figure of numbering the elements contained in lists. See, for example, the chapter on polls that we analysed earlier where he mentioned six critical features, or the chapter on Mead where he counts to five the consequences of his conception of objects (1969, p. 68), and to six those of his theory of joint action (1969, p. 71). Blumer appears as a *canvasser* of elements, all of which more or less abstract, must be "noted", "taken into account", "indicated" in an interaction for it to be interpreted by the members, and only secondarily by the sociologist. Blumer had *bricolaged* his own specific quantitative method that was compatible with interactionism: the canvass of concepts and their quantitative identification.

Finally, we find ourselves with two opposite views. On the one hand, there are the Lazarsfeldians coming from a positivistic model of action and science. They conceive social actors as affected by causes, of which they are not necessarily aware, determining their behaviour. These behaviours once aggregated, might create social problems, as proven by the Great Depression. The government, being on a higher level of action than the actors, can act on these causes, using work and social projects, as during the New Deal. The sociologist produces objective information about the causal mechanisms at hand in using statistical survey techniques, and advises the government thanks to this specific knowledge (Didier, 2009).

On the other hand, there are Blumer and the Symbolic Interactionists, influenced by the American Pragmatists. Here, the actors' main characteristic is their ability to confer meaning upon their environment. Certain entities to be found in the environment of the actors find "lines of entry" into these actors' lives, and the latter react to them according to how they interpret them. Sometimes, several actors are led into conflicts of interpretation, which might become actual social conflicts. In this case, an arbitrator helps finding a settlement—which is a mode of action opposite to that of the government in the preceding model, because the actors are the agency, not the passive objects, of causal forces. The sociologist might himself be an arbitrator, or might take part in the arbitration, because he knows how to identify the pertinent entities in the context. To this aim, he indeed might use numbers, but of a specific kind. Numbers count pertinent social entities or lines of entry, but not humans, and they are used as their identifiers. These two models of society both have a conception of actors, of the government, of the social role of sociology, and of quantification, but they organize these specific "actants" in an opposite manner (to use an expression from semiotics).[3]

Questioning the notion of the "variable" and "variable analysis", Blumer did not refer to the dichotomy between the quantitative and qualitative. He refrained from using the very vocabularies of enemies that he saw becoming powerful enough to weaken his own position, epistemologically as well as socially. He saw important shortcomings in the actual practice of survey analysis and experimental design, and argued that these methods were limited to the restricted part of group life where interpretation is stabilized so that interaction *looked like* a determination—an argument which was actually on par with C. Wright Mill's "massification" and which remains very powerful today. Manifesting a "besieged mentality" (Katz, in Emerson 2015), he fought against the pretention of the pollsters to speak objectively about the world, and was scandalized by the fact that so many opinion leaders would listen to pollsters, arguing that they were in fact reducing everything to a false determinism. But his enemy was not quantification in general, only its use by the Lazarsfeldians.

Ethnomethodology Between Accounts and Official Power

The fight against the Lazarsfeldians was not only in the hands of the symbolic interactionists. Ethnomethodology, originally developed by Harold Garfinkel in the mid-1950s, elaborated another criticism of statistics (and also of symbolic interaction), which bears on their political consequences. Aaron Cicourel, a pillar of this strand of sociology, is responsible for this.

The situation of the ethnomethodologists in the 1960s was completely different, nearly contrary, to that of the symbolic interactionists. The ethnomethodologists had no strong institutional base; they were only a small group of young scholars not fully united, working mainly in California, and thus in universities much less powerful than Chicago or those of the East Coast, and these scholars were striving to be recognized. They had few allies, since symbolic interactionists varied in their opinion towards ethnomethodology, from indifference for a strand of research that they saw redundant to a respectful but fairly distant interest. Still, their criticism of quantification had wide consequences and was very often used by those identifying themselves as "qualitative sociologists" afterwards. As we will see, first through the study of the work of Garfinkel and then that of Cicourel, their criticism did not oppose all and every quantification.

As demonstrated by Heritage (1984), ethnomethodology was a reaction against Parsons' model of scientific action, and bore on Alfred Schütz's phenomenological sociology. It is a comprehensive sociology and, like symbolic interactionism, it converged strongly with the American pragmatists. Ethnomethodology was interested in how actors theorize *by themselves* their *Lebenswelt*, and in understanding how action is based on mundane cognition. As Heritage (1984, p. 36) put it, ethnomethodology's "proposal to develop a 'generalized social system built solely from the analysis of experience structures' thus presented a direct attack on the very domain which Parsons had omitted from consideration: the realm of approximate judgments and reasonable grounds which constitutes the common sense world". One of the ways to know about society is obviously statistics, and thus ethnomethodologists did not take long to launch studies of this kind of object.

Statistical Accounts

In 1954, shortly after having completed his doctoral dissertation at Harvard, Harold Garfinkel had been hired by the sociology department at UCLA, and he began field work in UCLA's hospitals. Aiming to create a sociology of the way group members produce day-to-day knowledge and *account* for it, very early on he had the idea to study the production of hospital statistics *as a sociological object*. He coined the expression "rate producing process" as early as 1956, meaning the study of the process through which quantitative rates are produced. Cicourel acknowledged that "the conception of the 'rate-producing' processes as socially organized activities is taken from the work of Harold Garfinkel, and is primarily an application of what he terms the 'praxeological rule'" (Kitsuse & Cicourel, 1963, p. 132).

Expanding his questioning on the production of rates, Garfinkel focused mainly on three aspects of quantification (Heritage, 1984). First, following the work of cognitive psychologist Eleanor Rosch, he questioned the categorization performed by statistical coders (in the context of a psychiatric institution). He observed that coders, even when a set of rules is provided to them, tend to proceed independently of that rule, through "ad hoc" practices so that the code chosen fits best their understanding of the whole situation of the case at hand. Garfinkel coins this as "interpretative realism", by which he means that the coders treat the data as signifying the whole social order. This is a capital point for his demonstration that members do indeed have a theory of the macro level of society: they, too, are able to generalize. Second, Garfinkel became interested in the ways in which "aggregate responses to questionnaire items" were used, especially when they seemed contradictory. Once again, Garfinkel highlights the fact that "the questionnaire user has to bootstrap a way beyond the literal 'face value' of the response in order to see them as evidences of a whole social arrangement" (Heritage, 1984, p. 166).

Finally, Garfinkel addresses the problem of “official statistics”. He points here to three levels of “anxiety” about their use. First, their insufficiency (the fact that they might lack enough information on the cases), second, the extent of the error, especially in sampling, that they may contain, and third, the limited adequacy of the definitions and procedures to the topic at hand (Garfinkel, 1967).

In these studies, Garfinkel is not “nihilistic”, to use Heritage’s (1984) phrasing. Garfinkel does not oppose quantification nor does he advocate “the abandonment of coding” but, on the contrary, he recognizes that “the unavoidable gap between data and its sense is unavoidably and irreversibly bridged, at least in part, by a coding process having unknown characteristics”, which deserve to be inquired into by the sociologist (Heritage, 1984, p. 162). When aggregated responses are contradictory, Garfinkel is “insistent that he is not criticizing, ironizing, correcting” the data (Heritage, 1984, p. 167). Rather, he is looking for a way to understand what their properties and deeper meanings are. And the observation that official rates are “made out socially” leads him to think that “an immense array of accounting practices and their organizational exigencies, previously occluded from the view by the preoccupation with accuracy, are laid open as possible avenues of investigation” (Heritage, 1984, p. 175).

Garfinkel gave several examples in two chapters of his *Studies in Ethnomethodology* (Garfinkel, 1999 [1967]) of how he thought his analysis of statistics as a social object could be productive for the use of statistics as a cognitive tool; how his analysis could help in using quantitative tables. He also showed that studies would allow us to deepen our understanding of the social processes through which members produce knowledge about the society they live in:

> The actors’ account – whether they take the form of questionnaire responses or of the statistical rates produced by bureaucratic agencies – cannot be unproblematically treated either as disembodied descriptions or as the ‘relaxed’ or ‘loose’ versions of objective states of affairs which can subsequently be tightened up by the judicious application of social scientific methodology. On the contrary, no matter how firmly such accounts are proposed [they] still await an analysis which situates them, with all their exigencies and considerations, within the socially organized worlds in which they participate as constituting and constituted elements. (Heritage, 1984, p. 178)

Thus, Garfinkel was interested in the *epistemic* consequences of his findings, but always remained suspicious about their political consequences. He argued for "ethnomethodological indifference", which meant for him that he did not want to make any judgement on whether "members" did say the truth or not. Yet, later, especially in the 1960s, this was interpreted by many readers as *political* indifference.

Ethnomethodology has had important consequences in American sociology, especially within conversation analysis. Douglas Maynard, in particular, at the University of Wisconsin, built on this research approach in analysing the conversations between interviewers and interviewees in surveys and polls in a very inspiring and consequential manner (Maynard et al., 2002). More generally, every scholar working in the field of sociology or history of quantification listed earlier in my introduction to this paper owes something to the seminal work of Garfinkel. And one unexpected consequence (to Garfinkel himself) of his work has been that it helped shape a very strong criticism against quantification itself.

Measurement by Fiat

In the beginning of the 1960s, Aaron Cicourel prolonged Garfinkel's argument about statistics into an actual criticism epitomized by the expression "measurement by fiat" (Cicourel, 1964, p. 12), even though, interestingly enough, he took this expression from a statistics handbook. The author of the latter explained that sometimes there was no scientific knowledge on a fact or characteristic to be measured. It was thus necessary to use an "arbitrary definition" of the fact, which led to a "measurement by fiat" (the name of a legally binding command or decision entered on the court record by the judge) (Torgerson, 1958). Cicourel turned this practical argument into a criticism. The quantities he had in mind were not survey data produced through questionnaires, but official statistics produced in the course of the bureaucratic treatment of public problems.

Among other things, he and his co-author John Kitsuse analysed official statistics on criminality and deviance. Together, they stressed that a difficulty arises "as a consequence of the failure to distinguish between the social conduct which produces a unit of behavior (the behavior-producing processes) and the organizational activity which produces a unit in the rate of deviant behavior (the rate producing process)" (Kitsuse & Cicourel, 1963, p. 132). Kitsuse and Cicourel highlight that actors, in daily life, account for some behaviours as being identical and others as being different. But there is no reason to believe that the categories used by the official administration engaged in the "rate producing process" respect necessarily those of the actors. On the contrary, "what such [official] statistics do reflect, however, are the specifically organizational contingencies which condition the application of specific statutes to actual conduct through the interpretations, decisions, and actions of law enforcement personnel" (Kitsuse & Cicourel, 1963, p. 137). Criminal categories are imposed, as if it were by fiat, by official institutions upon social life:

> In modern societies where bureaucratically organized agencies are increasingly invested with social control functions, the activities of such agencies are centrally important 'sources and contexts' which generate as well as maintain definitions of deviance and produce populations of deviants. Thus rates of deviance constructed by the use of statistics routinely issued by these agencies are social facts par excellence. (Kitsuse & Cicourel, 1963, p. 139)

The official rates are not a valid indication of everyday practice and the beliefs of members, but they are facts that have been isolated from the social setting they pretend to represent. Official statistics belong to the arsenal of control of bureaucracies. According to Kitsuse and Cicourel (1963), these pretend to aim for the *welfare* of the weakest elements of the population, but in fact they produce by fiat the population of deviant people, of the unemployed, of the poor, etc. Yet, despite this very powerful, critical conclusion, Cicourel still remained interested in the use of statistics.

The Quantitativist Cicourel

Cicourel did not reject quantification, but proposed a better use of statistics. He became interested in fertility in Argentina and, being well-trained in mathematics, launched research on the topic using a survey method—that is, an ad hoc questionnaire that he had written himself on the topic (Cicourel, 1974). The objective of this survey was to capture "the actor's theory and method of accounting for and producing his everyday social organization" related to fertility. Cicourel established a very cautious methodological procedure, in which respondents were interviewed several times successively, so that the interviewer could be either changed, if he or she did not fit to this precise family, or get acquainted with them, and fixed-choice questions were avoided as much as possible. "The type of interviewing conducted was intended as an alternative strategy to the conventional survey" (Cicourel, 1974, p. 87). The aim was to take into account the interviewer–interviewee interaction and to capture the accounts of day-to-day action scenes as articulated by the interviewees. The survey would not impose its own categories onto the respondent, but adapt to the ones of the interviewee. The result is a book with lots of methodological statements, important analyses of direct observations and field notes taken during the interview, and a whole load of tables and charts analysed at length. Much later, in an interview, Cicourel made plain that he does not oppose quantification in general, but only certain methods of quantification: "I am not opposed to quantification or formalization or modeling, but I do not want to pursue quantitative methods that are not commensurate with the research phenomena addressed" (Witzel & Mey, 2004). Those who, like Cicourel, really grapple with quantification, do not reject it as a whole; they sort methods out.

Much later, Kitsuse wrote a presidential address to the Society for the Study of Social Problems (Kitsuse, 1980) that helps qualify the political consequences of Cicourel's epistemological position. Kitsuse had a very personal experience with the authoritarian tendencies that inhabit any state, and the American one in particular, since as a second-generation Japanese American, he was imprisoned in an American internment camp in 1942–1943. He shows that Cicourel remains in an epistemological scheme, first identified by Gouldner (1968), coherent to the Welfare State, in which sociologists attribute to deviants "a vulnerability and subordination to the moral authority of what is commonly characterized as white, middle-class, protestant culture and society" (Kitsuse, 1980, p. 6). This conception implies that the sociologist, like the state, sees the deviant as "the passive 'man-on-his-back' seemingly incapable of resisting or opposing the inexorable process of attribution of abnormality and inadequacy, stigmatized as morally defective, progressively excluded and subordinated as deviant" (Kitsuse, 1980, p. 7). The deviant remains essentially politically passive in his treatment by both the state and the sociologist. And I would add that this remains true even in the work of adapting categories proposed by Cicourel when he was working in Argentina.

The scandal inherent to the theory of "measurement by fiat" comes from the implicit presupposition that statistical categories do in fact succeed in formatting the deviant. The latter is supposed to have no effective means to fight back, bend the categories or destroy them. Due to the "official" nature of these statistical categories, they are supposed to have enough inherent power to indeed impose themselves. Opposing such a view, Kitsuse proposed that sociologists should notice that in the 1980s, it became clear that deviants were "coming out all over" to "publicly demand their rights to equal access to institutional resources" (Kitsuse, 1980, p. 3).

The actor's first feature, for Cicourel, was his ability to produce his own account of social reality, even of its macro-structure. Not only the sociologists have a conception of the whole social order, but anybody within society. The government, when it pretends to help or re-educate those that it calls "deviants", in fact produces the category, and subordinates those that are categorized, especially through epistemic tools such as official statistics. The role of the sociologist is to unearth the accounts of the deviants, to help make their worldview visible and respectable. Thus, on the one hand, he criticizes official statistics imposed on the existence of "deviants", and on the other, he produces, among many methods, his own quantitative methodology provided that it remains commensurate to the research phenomena.

Blumer's and the ethnomethodologists' criticisms represent the two main strands of critique addressed to quantification by those who would later on be associated with "qualitative research".[4] As we have seen, these critiques take place in the wider context of developing theories of society, accounting for the characteristics and agency of social actors, of the government, and of the sociologist. They also comprise a definition of the good and bad uses of quantification. Thus the criticisms are addressed in fact to specific methods of quantification and are complemented by alternative quantitative practices. Within sociology, these two sets of criticisms were emitted from two completely opposite positions in terms of audience and power. Symbolic interactionists were initially dominant, and tried to prevent being overwhelmed by the new quantitativist contender; ethnomethodologists were on the contrary minuscule and fought a battle as bravely as they could, surfing on the recognition they were enjoying.

The sociologists in question were aware that they were not entirely condemning quantification, but only certain methods, as can be inferred from the fact that, in the 1950s and well into the 1960s, they did not use the dichotomy "quantitative vs. qualitative" sociology. It belonged to the very heralds of surveys, led by Lazarsfeld, who crafted the label "qualitative research" as a propaedeutic to quantitative analysis. Therefore, the next question that we have to answer is why and how symbolic interactionism and ethnomethodology finally ended up being considered "qualitative". Why is it that this label took consistency, when it initially belonged to the enemy? The first step to answer this question is to take into account the appearance of a new actor on the sociological scene, the coming-of-age "young radical", during the 1960s.

Radical Sociology, Quantification and the Welfare State

The beginning of the emergence of the 1970's spirit of radicalism within American sociology can be backdated quite precisely, to the 1968 annual meeting of the ASA, the cornerstone of sociological orthodoxy. President Philipp Hauser, who had co-written with Blumer *Movies, Delinquency, and Crime* (Blumer & Hauser, 1970 [1933]), which involved quantitative materials, and who was later appointed Director of the Bureau of the Census—and thus one of the, if not *the*, most prominent figure in the use of statistics in sociology—had invited Wilbur J. Cohen, Secretary of Health, Education, and Welfare, to give the keynote presentation at this conference. This invitation demonstrates the strong association that existed at the time between the quantitativists, who held top positions within the sociological academic world, and the political elite of the American Welfare State. The invitation provoked a fierce opposition from young sociology students who called themselves "radicals". They were:

> as rejecting of those who purvey sociological research on underdogs to the overseers of the welfare state as they are of caterers to the warfare state. To the Sociology Liberation Front, Cohen's 'guest of honor' status was an unacceptable example of what Gouldner (1968) has called the 'blind or unexamined alliance between sociologists and the upper bureaucracy of the welfare state'. (Roach, 1970, p. 228)

The meeting ended up in a mess and gave rise to a schism within the professional organization of the sociological field (Roach, 1970). This emerging radicalism in sociology was under a paradoxical influence. On the one hand, the new "radicals" were deeply influenced by the simple desire to reject the templates of the past: family, state interventionism, sobriety, war. This rejection is well embodied by Abbie Hoffman's book *Revolution for the Hell of It* (Hoffman, 1968), which does not propose much, except the joy and amusement of destroying everything from previous generations, including, as far as this paper is concerned, the University system. It was also associated with a fierce opposition to any alliance with the institutions of the Welfare State. This is also exemplified by the fact that Howard Becker was wearing a T-shirt at the ASA annual meeting depicting an unkempt, hairy, cartoon hippy saying "Hey Kids, Let's Fuck the State"; an ironic proposition mixing destruction and fun (discussion with Jack Katz).

On the other hand, sociology was a discipline that could provide intellectual tools to understand the system, its injustice and boredom, and thus help either fix it or destroy it. Since institutions and "the system" were identified as the problem, sociology seemed to be a straightforward answer to it. Thus, sociology was at the time attracting a large number of new students eager to change society *through sociology* (Turner & Turner, 1990).

So, in this conflict, how was quantification seen? How was "qualitative sociology" transformed in this turmoil? Behind the widespread non-articulated contempt and suspicion towards quantification (called "oversimplification" by the heralds of quantification Reitman, 1978), there were in fact two quite different strands of argument. The first one built on the post-Marxist tradition of the Frankfurt School, and here sociologists tended to be influenced more by Cicourel's arguments. The other strand was more "Blumerian", and stood thus more in the tradition of the American pragmatists.

Are Quantities Fascist?

One immediate consequence—next to the creation of the highly influential journal *Social Problems*—of the radical sociologists' actions was the foundation of *The Insurgent Sociologist* in 1969 (which later would become the journal *Critical Sociology* from 1988 onwards). Influenced by C. Wright Mills, neo-Marxism, and radical feminism, the initial goals of the journal were to organize the actions of the different activist groups, among other things, to ease communication between the Western Union and the Eastern Union of Radical Sociologists, and the Sociology Liberation Front, and to help define what radical sociology should look like. The first issues of the journal looked like street pamphlets with very short, explosive papers, unsigned, and full of images and caricatures. In contrast to previous generations, what was exhibited here was a completely different style of sociology. One cartoon ironizing the use of figures has been reproduced below (see Fig. 13.1). This caricature was published in the second issue of the journal.

The "Mo-Jan" system depicted in Fig. 13.1 stands for *Mo*rris *Jan*owitz, one of the founders of military sociology. One can see that in the text published next to the image of the rocket a certain positivist and quantified tone in sociology ("82.5%") is mocked. The reference to the rocket and "Camelot special" ironizes the role of the army in financing research (as we will see below), and finally Janowitz's book proposing an "urban control of racialized riots" with the help of the disciplinary tools of sociology are at the heart of the students' exasperation.

Soon thereafter, the *Insurgent Sociologist* published a paper entitled "Accidents, Scandals and Routines: Resources for Insurgent Methodology" (Molotch & Lester, 1973) addressing the role of quantification. Examining the news from a Garfinkelian perspective, Harvey Molotch and Marilyn Lester argued that ethnomethodology provided methods to suspend the belief that an objective world exists. They showed that the news content of the mass media is the "result of practical, purposive, and creative activities on the part of news promoters, news assemblers and news consumers" (Molotch & Lester, 1974, p. 101). Noticeably, the proposition that statistics measured reality "by fiat" played a key role in their argumentation. As they wrote: "Cicourel (1964) makes an analogous argument with respect to the creation of a juvenile delinquent" (Molotch & Lester, 1974, p. 103). Ethnomethodology was used by the authors as a tool to criticize not simply a fabricated reality, but a *politically biased* fabricated reality. According to Molotch and Lester, ethnomethodology helped to avoid "be[ing] duped into accepting as reality the political work by which events are constituted. Only by accident and scandal is that political work transcended, allowing access to 'other' information" (Molotch & Lester, 1973, p. 10). The politicization of quantities highlighted by Cicourel was thus ushered into a general criticism of a reality fabricated by the ruling elite.

Interestingly enough, soon afterwards, the same journal published a paper entitled "The New Conservatives: Ethnomethodologists, Phenomenologists and Symbolic Interactionists" which was influenced by neo-Marxism. Here, among other things, it was argued that the approaches at stake—especially ethnomethodology—are inherently conservative, and therefore not radical, for two reasons. First, they "implicitly deny the generalizability of any theory of social change", thus are opposed to the notion of revolution. Second, they "picture men

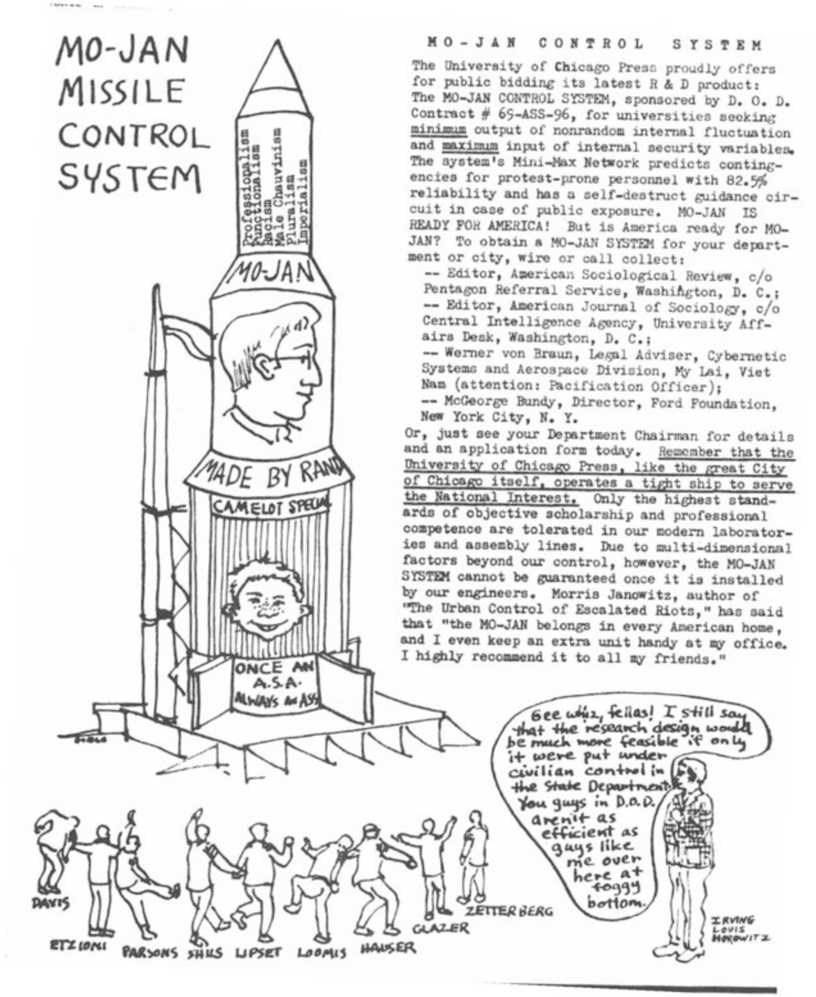

Fig. 13.1 "Breaking out of the Hothouse" (*Source Insurgent Sociologist*, Vol. 1, No. 2, p. 8. Reprinted with permission from *Critical Sociology*. Scan gratefully provided by the University of Michigan Library [Special Collections Research Center])

as individual entrepreneurs, and use the language of the market-place in extending laissez-faire individualism to contemporary social theory" (McNall & Johnson, 1975, p. 49). Both these features are associated with the tendency to mainly use data about *individual cases* and, the authors regret, very rarely "samples and replicable measurement techniques" (McNall & Johnson, 1975, p. 62). Radicalism was definitely still the object of a conflict of definition from within, as much as the roles of statistics in it.

But neo-Marxism was not entirely opposed to ethnomethodology. David J. Sternberg (1977) proposed a radical rereading—as the title of his book attests—of the concept of measurement by fiat. He deals with the famous F-scale invented by Theodor Adorno and others (Adorno et al., 1950). Given the huge impact of this work, it is important to explain its role within the question of quantification. Adorno discovered the practice of statistics when he first reached the USA in 1938 and—through Horkheimer—worked under Lazarsfeld at Columbia. He hated the experience. As he wrote, "I collided with the positivistic habits of thought" (Adorno, 1998, p. 220). But later, in the 1940s, after having settled in California, he began to work on a project that would eventually lead to the book about *The Authoritarian Personality* (Adorno et al., 1950). The book achieved a successful conjoining of Marxism and Freudism in trying to identify the psychological roots of Nazism. Besides, it was a methodological rarity, since it made large use of statistical surveys, and of the conceptual pair qualitative vs. quantitative (the expression "qualitative analysis" is in the title of the 4th part of the book). This time, Adorno deeply enjoyed the experience. He loved the atmosphere in which he worked: "the kind of cooperation in a democratic spirit that does not get mired in formalities […] was for me probably the most fruitful thing I encountered in America" (Adorno, 1998, p. 232).

Likewise, he praised the scientific achievement of the research, particularly because the "teamwork spirit" made possible an intelligent use of statistics: "The aporia – that what was discovered purely by quantitative means seldom reaches the genetic deep mechanisms, while qualitative discoveries can just as easily lose their generalizability and therefore also their objective sociological validity – we tried to overcome" (Adorno, 1998, pp. 232–233). In particular, the F-scale, a tool measuring the individual propensity to authoritarianism, was invented in Berkeley in a "free and relaxed environment […] in a manner that by no means coincided with the usual image of the positivism of the social sciences" (Adorno, 1998, p. 233). Afterwards, Adorno became generally suspicious towards empiricism and never used statistics again. But he nonetheless did not publish any general argument against quantification, most probably to stay true to this happy experiment (Genel, 2013, p. 91). And, in the 1970s, he left the U.S. in an *ambiguous* overall stance towards statistics.

Sternberg, in his book (Sternberg, 1977), gave an example of how the positions of the Frankfurt School could be radicalized. Discussing Adorno's F-scale and Cicourel's measurement by fiat, he argued that the F-scale was used widely by many American official bodies of administration, and he concluded: "the F scale has to do with fascism all right, but not in the sense its designers intended it. *Its* findings, *not* the people that it finds, are Fascist" (Sternberg, 1977, p. 43). Sternberg pushes Cicourel's argument to the point of arguing that statistics as a whole, even Adorno's F-scale, are fascist, insofar as they impose categories of social control upon society.

Sternberg is a good example of how the criticism of quantification made by ethnomethodology was radicalized by many scholars of the New Left, associating surveys with state authority and concluding that they are therefore fascist—even when discussing Adorno's work, to whom such a qualifier must have seemed quite strange! But the reception of Steinberg's book was far from laudatory. Reviewers qualified Sternberg's book as involving a "simplistic approach" (for example Reitman, 1978), and the overall judgement of this book and those alike was that such an inference could not be taken seriously. It was stepping outside the range of the sociologically admissible. It was definitely hard to call Adorno a fascist! And, indeed, it must be said that Sternberg did not make a career in the discipline of sociology. Rejecting all quantities as fascist did not hold. Symbolic interactionists, for their part, constructed another argument about figures, to which we will now turn.

Light Travelling: Numbers as Gleanings

Howard Becker and Louis Horowitz can be taken as representatives of the interactionist trend in radical sociology. Becker, directly influenced by both Blumer and ethnomethodology, had crafted "labelling theory" which shifted the focus from the causes of peoples' deviant behaviour to the definition of people and behaviour as deviant. In 1972, the *American Journal of Sociology* organized a remarkable symposium entitled "Varieties of Political Expression in Sociology", which was published as a special issue in June that year. The collection does not comprise any explicitly critical or Marxist sociologists, but papers, such as the article by Merton (1972), or the paper by Lipset and Ladd (1972) which presents an analysis of data from a comprehensive survey of 60,000 academics to explore "the actual political views of sociologists" (Lipset & Ladd, 1972, p. 68), and many other fascinating contributions. Also, Becker and Horowitz were invited to this symposium and took side with radicalism: "Both because of our own political position and for the sake of congruence with current discussion, we will take the tack of sociologists who conceive themselves, or like to conceive themselves, as radical sociologists" (Becker & Horowitz, 1972, p. 59). Their argument makes perfectly clear how they see the link between statistical methods and politics.

In their contribution to the symposium (Becker & Horowitz, 1972), they began by claiming that radical sociology can be good sociology. They define the latter as being "true to the world", especially when it analyses the causes of events, even in the most limited sense of the term "cause". Especially, and most important for our purpose, they insisted that, in principle, all the known methods of the discipline can be useful: "With all their faults, interviews, participant observation, questionnaires, surveys, censuses, statistical analysis, and controlled experiments can be used to arrive at approximate truth" (Becker & Horowitz, 1972, p. 50). It has to be said that in his whole career, Becker never expressed rejection of quantification. In a collection of methodological papers of his, he noticed that during fieldwork observation, "the observer will also find it useful to collect documents and statistics (minutes of meetings, annual reports, budgets, newspaper clipping) generated by the community or organization" (Becker, 1970, p. 79). Thus, like the ethnomethodologists, he insists that the quantities found in the field are interesting objects of study. And later, he highlighted that between "qualitative and quantitative" methods "the similarities are at least as, and probably more, important and relevant than the differences. [...] The same epistemological arguments underlie and provide a warrant for both" (Becker, 1996, p. 53; but see also Becker, 1958).

Thus, the specificity of radical sociology does *not* lie in its methods. It lies in its "distinctive contribution to the struggle for change" (Becker, 1996, p. 53), as on the one hand it provides the knowledge to critique inequality and lack of freedom, and on the other hand it provides the basis for implementing radical utopias. As Becker (1996, p. 53) put it, "the constructive aspects are rooted in the positivist tradition, and the critical aspects in the Marxist tradition".

One of the core concerns in the struggle for change is the attribution of causes to the events. All events have an infinity of causes, beginning with the presence of air that allows the humans to breathe. Thus "the assignment of causes to events has a political aspect", because "when sociologists link a cause to an event or a state of affairs, they at the same time assign blame for it" (Becker, 1996, p. 58). It is the specific causes chosen by the sociologist that make him radical. As Becker writes:

> In general, radicals will judge a sociological analysis as radical when its assignment of causes, and thus of blame, coincides with the preferred demonology of the political group making the judgment. (Becker, 1996, p. 59)

For the radicals, a shocking example of conservative attribution of causes was what came to be known as the "Moynihan Report". In 1965, Daniel Moynihan, then Assistant Secretary of Labour in the U.S., issued a report entitled *The Negro Family: The Case for National Action*. It was an entirely statistical report dedicated to understanding the causes of poverty in black families. In fact, the report attributed poverty to the disorganization of the black families themselves (Rainwater & Yancey, 1967). This argument provoked a huge intellectual controversy, because implicitly it was *Blaming the Victim* (Ryan, 1971). Radicals (and others) were shocked that such an important representative of the Welfare State could produce arguments that neglected so obviously the oppression exerted by white people on black people, and that a self-described "liberal" could engage in such a conservative political assault.

Having such a counter example did not help the radical in identifying the pertinent causes of any social process, those causes that are at the same time true to the world and belong to radical demonology. Becker & Horowitz (1972) argue that there are three "obstacles" to a radical sociology, three specific elements that oppose the pursuit of its objectives. These are:

> (1) The conservative influence of conventional technical procedure, (2) Commonsense standards of credibility of explanations, and (3) The influence of agency sponsorship. (Becker & Horowitz, 1972, p. 62)

Let us review each of these shortcomings in turn. (1) Research means testing the deductions made from existing theories on data suitable for making such a test (cf. the controversy between Blumer and Zentner mentioned above). This is done through a method, statistical or not, that restricts the range of causes to be tested to what the researcher had in mind when he conceived his research. As Becker and Horowitz write:

> But some techniques, indeed, require sociologists to leave out things they *know* might be important. Thus, it is difficult, though not altogether impossible, to study certain kinds of power relationships and many kinds of historical changes by the use of survey research techniques. (Becker & Horowitz, 1972, p. 62)

Becker and Horowitz do not get more explicit. But knowing their proximity to Blumer, it seems clear that the elements that sociologists *know* that might be important for the attribution of causality are linked to the interpretation process that Blumer highlighted. Here, Becker and Horowitz reuse Blumer's argument about surveys—including Blumer's precautions and lack of radical condemnation.

(2) Sociologists, similar to other members of society, tend to believe more in the versions of the elite than those of other people, because the elite runs the organizations. That is, they tend to believe "official versions and analyses of most social problems", and thus they "find it hard to free [themselves] from official analysis, sufficiently to consider causes not credited in those versions" (Becker & Horowitz, 1972, p. 63). Becker and Horowitz here refer not only to Blumer but also to Cicourel's argument about the performing effect of official statistics, producing the causes of social problems. We believe official statistics, because their "version" is that of the elite.

(3) Finally, agency sponsorship might put conservative limits on a radical search of causes. It is not necessarily the case that they are politically biased, but when they fund research it is to solve an operational difficulty, so that they, too, limit the range of the answers that are worth giving. In particular, they tend not to see their own operations as being the cause of the problem. Although Becker and Horowitz do not discuss this directly, but one of the main "organizations" at stake here was the military itself. Even though the 1960s were the decade when the Army began to lose its near exclusivity in financing public research, it remained the main finance provider (Moore, 2008, p. 34). Again, the authors refer

to a contribution of Blumer published in a book edited by Horowitz entitled *The Rise and Fall of Project Camelot* (Horowitz, 1974) about the amazing story of a project financed by the army to use social sciences in the goal of predicting (and thus controlling) revolutionary upsurge in South America (Camelot is mentioned on the caricature Mo-Jan system, right above the *Mad* face on the rocket).

Thus, "the remedy for that is to travel light, to avoid acquiring the obligations and inclinations that make large scale funds necessary" (Becker & Horowitz, 1972, p. 64). It is obvious that, here, to "travel light", that is without the money of the Army, is also to renounce the surveys that were among the most expensive research techniques of the times. But it is not against *any* quantities, on the contrary. As stated above, collecting figures on the field or using any available figures is not shocking to them at all. In this, the 1970s radicals act towards numbers as gleaners towards ears of corns abandoned in the field. They are not cultivated; they are simply used when found here and there. Radical figures are gleanings.

This argument made a much bigger splash than the other one about the fascist character of quantification. For example, Alvin Gouldner, who was himself a core figure of radical sociology, especially since the publication of his *The Coming Crisis of Western Sociology* (Gouldner, 1970) acclaimed both authors in a later comment of the special issue, writing that "their effort to characterize radical sociology is one of the more probing I have seen" (Gouldner, 1973, p. 1079). Articulating general arguments against quantification did lead to contradiction or unrealism. So, the best was simply to ignore them, or maybe be ironic or sarcastic about them.

In conclusion, radicalism changed the relationship of sociology towards quantification. On the one hand, there was indeed a definitive condemnation of any use of quantities, bearing on Cicourel's "measurement by fiat" argument and expanding it to the point of calling "fascist" any process of quantification. This was a radical rejection of quantification, but it was paid for by an expulsion from the sociological academic field. On the other hand, Blumer's heirs, represented here by Becker and Horowitz, built the "travelling light" argument. For them statistics and quantification can be useful, and often are, both when produced by the researcher and when collected in the field. But most of the time statistics and quantification force the researcher to cope with the "demons" of power (the Welfare State, the Army, large companies), because they require a large infrastructure and funds. Thus, the safest, for radicals who did not want to compromise with these demons, was simply not to use such methodological tools. The argument wound up not exactly *against* quantification but only *without* surveys. Even though not really to the taste of the most powerful sociologists, this one could still be swallowed by the academic field.

One question that remains is that if the radical sociologists wanted to stay away from the liberals in charge of most of the power institutions, for whom was their knowledge produced? As Jack Katz has argued (Katz, 2015), the public of the radical sociologists was the youth that at this time that was flowing in the universities, and especially in the sociology departments (Turner & Turner, 1990). Radical sociology was oriented towards the students—and professors who saw themselves primarily as teachers. The actual institutionalization of a "qualitative sociology", that as we have seen was seldom mentioned before the 1970s, came out of this movement.

Institutionalization of a "Qualitative Sociology"

We have described the criticisms which had been expressed towards quantification by sociologists. Their arguments were defensive, against the wave of quantities that washed over their discipline. But, from the 1970s onwards, the strategy of those opposed to surveys changed: they began to make the category fit to their own work. We will see that they would address themselves to the large number of students that were flocking to sociology departments by publishing textbooks and the creation of a new journal. Finally, we will use the Jstor database to measure the success of the enterprise.

Common Ground

One of the very first books to use the word "qualitative" on the cover, actually in the subtitle, was Glaser & Strauss's, 1967 *The Discovery of Grounded Theory, Strategies for Qualitative Research* (Glaser & Strauss, 1967) which immediately received a lot of attention and success worldwide. The subtitle would have the public think that it would be a fierce engagement against quantitative analysis. But actually, those who read it discovered that this was not the case. The book performed splendidly as a classic, albeit difficult, rhetorical tour de force: it consolidated the divide between the categories of "qualitative" and "quantitative", but only to show simultaneously the authors' exceptional ability to overcome it. The authors dug a ditch, so that everyone could see how well they were able to jump over it.

Indeed, contrary to Blumer and the ethnomethodologists, Glaser and Strauss *accepted* Stouffer and Lazarsfeld's reading of the development of sociology. They accepted the dichotomy between qualitative and quantitative and observed that, since the 1930s, quantitative research "swept over American sociology" because quantitative methods had developed "systematic canons and rules of evidence on such issues as sampling, coding, reliability, validity" etc. which were much more "rigorous" than the equivalent canons used by empirical qualitativists remaining "too impressionistic". And thus, "qualitative research was to provide quantitative research with a few substantive categories and hypotheses" (Glaser & Strauss, 1967, pp. 15–16). Qualitative sociology had come to be dominated.

But they also argued that the fundamental function of sociology was the discovery of theories based on data. Their book was supposed to be a handbook for *abstraction*, and thus an attack against those logico-deductive theorists who promoted the *verification* of theories through quantitative data. The authors called this opposition "*generation vs. verification*" of theory (Glaser & Strauss, 1967, p. 12) and proposed "strategies" to perform the former. In this, they were once again very close to the American pragmatist tradition and indeed referred often to C. Wright Mills and Blumer. In particular, they worked on the categories established by the former and they opposed the kind of sociology that C. Wright Mills had baptized "Grand Theory" (Glaser & Strauss, 1967, p. 10), meaning a theory severed from any empirical ground. There is obviously a pun between "ground" and "grand" theory.

But they also argued that, although there had been a historical connection between the quantitative and verification theories, this connection was only contingent. There was no epistemological necessity to it. On the contrary, abstraction could be performed on both kinds of data, qualitative or quantitative:

> Our position in this book is as follows: there is no fundamental clash between the purposes and capacities of qualitative and quantitative methods or data. What clash there is concerns the primacy of emphasis on verification of generation of theory – to which heated discussions on qualitative versus quantitative data have been linked historically. We believe that

> each form of data is useful for both verification and generation of theory, whatever the primacy of emphasis. […] In many instances, both forms of data are necessary. (Glaser & Strauss, 1967, p. 17)

According to Glaser and Strauss, both qualitative and quantitative data constituted a common ground on the basis of which theories could be built. With both, the researcher had to use or establish sampling methods, move from substantive to formal theory, and proceed to comparisons among sets of data. The main difference was simply that when using quantitative data for the development of theory, the researcher had to "relax the usual rigor of quantitative analysis so as to facilitate the generation of theory" (Glaser & Strauss, 1967, p. 187). She had to simply use "freedom and flexibility" with her data (Glaser & Strauss, 1967, p. 186). Any kind of data could thus support the generation of theories, if wisely utilized. But, if theory building could be achieved with both, why did they then nevertheless insist on qualitative research? They argued the following:

> We focus on qualitative data for a number of other reasons: because the crucial elements of sociological theory are often found best with a qualitative method, that is from data on structural conditions, consequences, deviances, norms, processes, patterns, and systems; because qualitative research is more often than not, the end product of research within a substantive area beyond which few research sociologists are motivated to move; and because qualitative research is often the most "adequate" and "efficient" way to obtain the type of information required and to contend with the difficulties of an empirical situation. (Glaser & Strauss, 1967, p. 18)

The argument amounts finally to a question of different emphasis, not of opposition between the two. Glaser and Strauss accepted a dichotomy between qualitative and quantitative research where the latter was constructed to be dominating the former. Quantitative sociology was supposed to be more "scientific", more "rigorous", more "accomplished" than qualitative sociology. Yet, Glaser and Strauss reversed the stigma (to use the title of one of Goffman's books) highlighting qualitative research's particular suitability for the generation of theory from data. Through their work, qualitative became "better", even though "relaxed quantities" could do a comparable job.

Glaser and Strauss's book enjoyed an impressive success and participated importantly in establishing methodological guidelines that would

Table 13.1 Reproduced from Schwartz and Jacobs (1979, p. 5)

Data	Goals of sociology	
	Positive science	*Actor's point of view*
Use of numbers		
Use of natural language		

"travel light", that is guidelines that would not involve quantification and yet at the same time be considered scientific. The book was followed by a series of other methodological books on qualitative methods that would give the same argument. The very first book published under the title *Qualitative Sociology* (Schwartz & Jacobs, 1979) is particularly striking. It presents symbolic interactionism and ethnomethodology, next to quantitative research, in a fourfold empty table (Schwartz & Jacobs, 1979, p. 5) (see also Table 13.1):

But the authors don't explore the table at all. They are interested only in data based on natural language and the actors' point of view, i.e. the right hand lower cell. They don't discuss any of the other categories, or try to fill the cells out. This strategy is the one that would generally be adopted by the many textbooks on qualitative sociology that would be published in the 1970s, such as Filstead (1970), Lofland (1971), Bogdan and Taylor (1975) or, later, Taylor and Bogdan (1984).

Another academic innovation important for the institutionalization of "qualitative sociology" was the creation of the eponymous journal *Qualitative Sociology*. The first issue came out in May 1978. In this issue, the journal's title is neither explained nor justified. It is only stated on page 2, along with the list of editorial board members, that the journal is dedicated to "qualitative interpretation of social life" and that "manuscripts dealing with the qualitative analysis of social life" are welcomed. Noticeable is a letter to the editor where the author expresses his happiness to witness the birth of the journal, because he feels "disenchanted with indiscriminate number-crunching and the attending tendency for the process to become an end in itself". Nonetheless, "the editors discussed their own reaction to this letter and concluded that in fact do not see [their] project as an attack on quantitative sociology" (*Qualitative Sociology*, 1978, Vol. 1, No. 1, p. 163).

References to the label "qualitative sociology" by those who ignored, or sought to oppose, quantification became important first and foremost in sociological *textbooks*. The label was addressed primarily to the young students flocking the university. It was intended to hawk the good word to students and help newly hired undergraduates. It also created a legitimate spot in a department curriculum and provided positions for professors entering the job market.

A Bipolar Category

To measure the students and professors' role in institutionalizing "qualitative sociology", we will now use easy quantitative methods ourselves, since, as Gabriel Tarde has argued, they can help us follow the "imitation trends" of an innovation (Didier, 2010). Once the "tribe" of "qualitative sociology" was knotted together, we might ask who got interested in it and reused the label. Here, statistics are not used, as they often are, to set up the "context" of a social event, but on the contrary to follow the social effects of this event. To this end, JStor helps us conveniently. The interface "Data for Research" makes it fairly easy to track quantitatively the use of any expression (association of words) in JStor's entire database.[5]

My searches resulted in the following. The word "qualitative", as far as it is related to the words "research", "method" or "sociology", takes off right after the war. "Qualitative research" and "qualitative method" raised much faster and higher than "qualitative sociology", which actually began to rise later, in the 1950s (see Fig. 13.2).

But sociology was not the only discipline experiencing a consolidation of the dichotomy between quantitative and qualitative. A search by disciplines shows that social work, on the one hand, and several biology specialties (such as developmental and ecology), on the other, are among the most important ones driving the results for "qualitative research" and "qualitative method" presented in Fig. 13.2.

Now, let's zoom in to study "qualitative sociology" itself. I excluded publications before WWII, when they were mainly noise, and I cut off my search after 1985, when many of the actors had changed, and the publication rate had generally grown and results were hence no longer as informative. This being done, it appears that the 20 authors that used most often the expression "qualitative sociology" (names are followed by the number of articles using these words) were not only those who belonged to the "tribe" as defined by *Qualitative Sociology* (see Table

13.1). On the contrary, many among them (Alexander, Duncan, Blau, Goodman) (see Table 13.2) were scholars who were famous for their *use* of statistics. In fact, it appears that Lazarsfeld's definition of qualitative sociology had been as powerful as his advances in qualitative research, so that the quantitativists participated themselves in establishing a second school of qualitative research, as defined initially by their famous predecessor—and thus obviously making also massive use of quantities.

What were the topics addressed by these qualitativists? The distribution of the keywords of the papers allows the hypothesis that those who used the expression "qualitative sociology" did so in two different contexts. Table 13.3 below shows the first cluster of keywords, used in 800 to 1500 papers, which are words associated with the "abstracted empiricism" kind of sociology: variable, model, population, per cent, table, class.

Used in only 380 to 400 papers, we find a different semantic group comprising: member, field, pattern, and person (see Table 13.4). The fact that the amount of papers using this second set of words is so different from the amount using the first set of words leads us to think that they

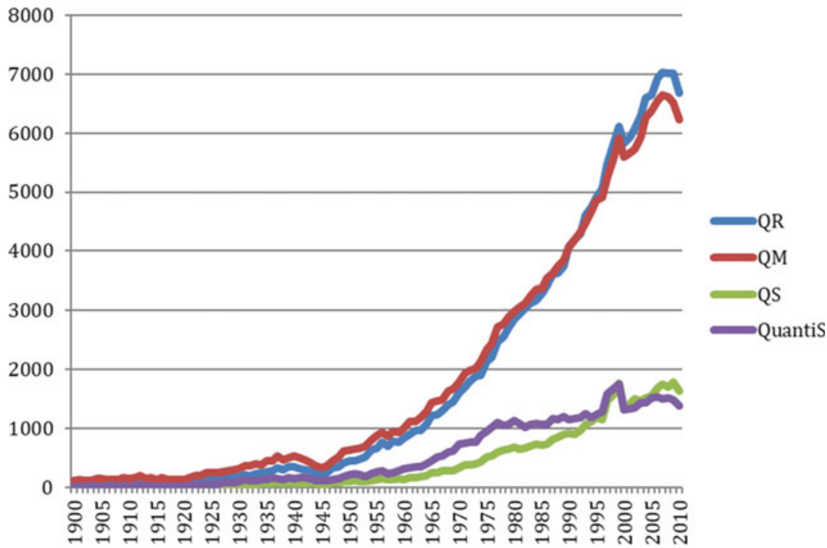

Fig. 13.2 Yearly distribution of the expressions "qualitative research", "qualitative method", "qualitative sociology" and "quantitative sociology" in the Jstor full database

Table 13.2 Name of author and number of their articles containing the expression "qualitative sociology", 1945–1985

Karl L. Alexander	16	John Hagan	12
Kenneth C. Land	16	Aaron M. Pallas	11
Otis Dudley Duncan	16	Glendon Schubert	11
Peter M. Blau	16	J. David Singer	11
Helen M. Robinson	15	James S. Coleman	11
Leo A. Goodman	15	Michel Vale	11
Helen K. Smith	13	Peter H. Rossi	11
Seymour Martin Lipset	13	Samuel Weintraub	11
Charles Tilly	12	David Knoke	10
David Riesman	12	David Snyder	10

Source JStor database, author's own compilation

Table 13.3 Twenty most used keywords of papers using the expression "qualitative sociology" and their number of appearance, 1945–1985

Variable	1559	Population	971
Theory	1353	Percent	967
Study	1305	Table	958
System	1207	School	951
Student	1154	Problem	937
Model	1136	Class	918
Behaviour	1105	Education	907
Political	1105	Change	892
Science	1078	Child	837
Analysis	1010	Family	788

Source JStor database, author's own compilation

Table 13.4 Twenty successive keywords beginning at the row 75 associated with the expression "qualitative sociology", 1945–1985

Member	446	Pattern	404
Unite	439	Empirical	401
Organizational	429	Activity	399
Field	427	Approach	398
Specie	426	Person	396
Rural	419	Power	394
Hypothesis	407	Interaction	392
Politics	407	Teaching	390
Historian	405	College	389
Number	405	Occupational	389

Source JStor database, author's own compilation might also be related to two different sets of papers. If this hypothesis were true, then the data would prove also that the "interpretative" tribe remained less productive—probably because they were much smaller in number.

These data about the papers listed in the JStor database let us lead to think that the label "qualitative sociology" did indeed take shape consequent to the conceptual innovations that we have described. But "qualitative sociology" is a category where two sets of "good examples" of papers are in opposition. On the one hand, there are those which belong to the "Lazarsfeldian" cluster where qualitative and quantitative are in a hierarchy. On the other hand, there is a set of papers pertaining to an "interpretative" definition of the qualitative influenced by Blumer, Cicourel and their intellectual descendants which seeks to set itself apart from such "quantitativist" uses of qualitative information.

Conclusion

The story of "qualitative sociology" begins right after WWII in a paradox. It was imported from German-speaking Europe, defined and used, first and foremost, not by opponents to quantitative methods, but, on the contrary, by Lazarsfeld in an inherent—but dominated—relation to quantitative analysis. Qualitative analysis was for him a propaedeutic to quantitative sociology or the use of "qualitative" statistical variables. At the same time, many sociologists started to oppose the apparently unstoppable rise of polls and survey analysis and expressed strong arguments against these methods. Blumer raised the problem of the neglect of members' ability to interpret situations, and Cicourel furbished the measurement by fiat argument. These arguments were reused and pushed to their limit a decade later by the "radical sociologists" in their denunciation of the drawbacks of the Welfare State, seen as closely associated to quantitative surveys. But these sociologists did not explicitly ban quantification, they simply did without surveys, they "travelled light". And it was only at the very end of the 1960s that the category "qualitative sociology" became institutionalized, especially through textbooks and curricula.

What's more, it should be remembered that the sociologists studied here criticized *a method* of quantification, not the general use of quantities. The social spheres that pretended to be free from numbers had in fact been *purified only* from *a certain method of* quantification. Dequantification is the result of an activity aiming to suppress certain kinds of quantities. Society is everywhere quantitative, only some spheres have banished certain methods (of quantification). All sociologists referred to here, still used quantities in one way or another. It is thus apparently not possible (nor desirable) to completely wipe out quantities from an epistemic system. All that actors have been doing is rearranging quantities, reorganizing them with or without one another, reshaping their relations in new and innovative fashions. But they never completely quantified nor qualified society; rather, they rearranged the quantities that they found already within.

In this respect, in a 1984 special issue of *Qualitative Sociology* entitled "Computer and qualitative data", the editors insisted that "large mainframe computers" had changed sociology since 1946, but that they were expensive and owned by third parties who could control and influence the research (Conrad & Reinharz, 1984). According to the editors, since the war, computers had been in the hands of either the (Welfare) State or big (capitalist) companies. But they also remarked that very recently, microcomputers had appeared and had become so cheap that every single researcher could now have his or her own. Thus they raised a new question: "How can the personal computer aid that group of sociologists who do not rely on mathematical analysis of data but who search their data for patterns and meanings"? (Conrad & Reinharz, 1984, p. 4). Stated differently, microcomputers are the material tool of knowledge making compatible to "travelling light", and at the same time they are dealing with something close to mathematical analysis.

Contemporary radical sociologists might notice that the conditions that justified the rejection of quantitative reasoning in the 1970s have nowadays lost their relevance. Today, the baby boomers are old, the Welfare State is weak, and everybody has a personal computer and an internet provider through which one can access a number of fascinating databases. A wealth of new methods independent from those "demonized" by the radicals in the 1970s is available. At this point, it seems to me that the dichotomy qualitative/quantitative barely has teeth anymore and could diligently be forgotten. As we have argued elsewhere, today, radical sociologists should all be also "statactivists" (Bruno et al., 2014).

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Notes

1. The opposition to quantification was in the 1930s the feature of a conservative ethos, criticizing standardization, state centralization and progressivism, associated to numbers (Boltanski, 2014). It became clearly progressive after WWII.
2. There were debates about quantification in other disciplines, especially in anthropology, but here we will concentrate only on sociology.
3. It is important to keep in mind that at the time Blumer was losing ground in sociology on two sides. On the one hand, he was much less empiricist than the Lazarsfeldians. He was proposing philosophical-like arguments against the data used by the quantitative researchers. Nonetheless, empiricism was then, indeed, exciting. And, on the other hand, he was also missing important innovations in philosophy itself—especially the developments of phenomenology showing that individuals are always embedded in relations to others. So, even on the "qualitative" side, he was seen as being slightly outdated.
4. Other arguments have been advanced concerning numbers, but they are ecumenists in that they seek a wise articulation of the relationship between the quantitative and qualitative, not an opposition. For instance, Erving Goffman never published about the relationship between the quantitative and qualitative. He was apparently simply not interested in the question.
5. The web address is http://dfr.jstor.org/. I want to thank warmly Erik Gjesfjeld for introducing me to this very useful resource.

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