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How I became a qualitative researcher?

Abstract: *The present article presents the personal experience of the author with research methodologies. Some limits of the social scientific research are being analyzed, regarding two of the stages of research: theoretical framework and operationalization; this is the way in which the validity of the criteria and the construct validity came into discussion. At the same time, the character of sociological theories and their utility in scientific research are under discussion. Reasons for which qualitative is chosen are listed despite the constant disapproval of this method in Romanian sociology (and its marginalization in Central – East Europe). The advantages of qualitative research in socio-human sciences are presented (what is being researched, through what methods, with what results). The special case of using the focus-group at a large scale is being analyzed (its use without following two of the major qualitative principals: theoretical sampling and theoretical saturation). The article advocates for the usage of qualitative and it is written in a personal and provocative style.*

Key words: *sociological research methodology, qualitative research, quantitative research, validity.*

My goal in this article is to show the impact that the contact and later on the experience with social research methodologies has on the person learning and using them. I will illustrate this by the example of my personal experience. I will show how I became a qualitative researcher and I will be pleading for getting qualitative out of the anonymity it finds itself in Central – East Europe.

I. Introduction

The research methodology is one of the most important disciplines in any college syllabus that trains future sociologists (together with subjects that are aimed at learning the sociological theories and statistical procedures). It is being

studied during a period of at least a year and it prepares the students for the best known (and the most recognized) competence of sociologists: the research competence.

To begin with, a small observation: the usage of the terms qualitative and quantitative has been established (in the international literature also) in naming research methodologies. There are some authors that have proved the improper usage of these terms, proposing (eventually) the use of the terms non-scientific and scientific (see Onuț, 2009 and Rotariu 2009). I tend to agree with them and in this article I will be using the expressions scientific and unscientific. However, in the first pages I have kept to the traditional terms in order to make clear what I am referring to.

In training the majority of sociology students in Romania the research methodology equals with learning quantitative research methods and techniques (with one noticeable exception – focus-group, as a qualitative interviewing technique; I will return to that later in this article). The qualitative method is not recognized (in Romania) as the provider of new knowledge; it is at best recognized for its artistic and literary valence; it is also excluded from the analytical programs because of a declared fear that students who find statistics too complicated might have a reason for not learning it (of course, the list of reasons is longer – the longest and most elaborate is that of professor Onuț).

I have reasons to believe that the situation is similar in the majority of the Central and East European countries (considering, for example, the number of articles coming from these countries that deal with subjects related to qualitative research). In these countries, as in Romania, qualitative is, in most cases, given the same status as the use of focus-group, especially in marketing research.

In this article, I will try to show that there are a lot of arguments undermining the dominant position which the quantitative research occupies in Romanian sociology (and also in that of Central and East Europe); that the qualitative research methodology produces knowledge which is worth taking into consideration. I will not mention as argument (except in this introduction) the fact that the qualitative methodology has been used worldwide for decades, that it is a legitimate approach, frequently used and with very little controversy. Also, I will not mention the extremely numerous books and articles

that deal with subjects of qualitative methodology in the United States, Canada, West Europe, the numerous qualitative researches that are being performed there at this moment, renowned journals that publish exclusively articles dealing with the qualitative methodology, specific professional associations, etc.

Of course, in Romania, for example, sociology had a troubled history. Between 1978 and 1990, universities were not allowed to produce sociologists; sociology resumed its legitimacy only in the 1990. We may suppose it normal that the interest for qualitative developed with a corresponding delay (the first book written by a Romanian sociologist that dealt with qualitative research appeared in 1997 (*see* Iluț), after the qualitative began to take shape internationally at the beginning of the 20th century, witnessing an explosion in the 1960s and 1970s). What is not normal is the fact that since then the qualitative research has been so hardly tolerated, so much discredited.

Not long ago, at a sociological conference, I was presented to some other professors and sociologists, and, after a series of praises, I was qualified like this: “she has only one flaw, she is a qualitative researcher”. You become open of reproach, at least in Romania, when you publicly declare that you are an advocate of the qualitative methodology. I will describe in detail my experience with social research methodologies and the way I became a qualitative researcher.

II. How I discovered the social research methodologies?

I was admitted to the University of Brasov, the Faculty of Sociology in 1996. Our profes-

sor of “Sociological research methods and techniques” was Mr. Gheorghe Onuț. It goes without saying, we were studying the quantitative research methodology, and our compulsory literature included, among others, the books of Romanian professors: Chelcea (1975), Mărginean (1982), Miftode (1995), Rotariu and Iluț (1997), Vlăsceanu (1986) and of professors: Ackroid and Hughes (1992), Festinger and Katz (1963), Javeau (1992). Anyone who knows professor Onuț can easily imagine that along with my colleagues, I could not get anything else but a follower of the quantitative rigor (which I was feeling, preaching, respecting, using, thoroughly studying, etc).

The exam task for this class was to choose a book on methodology (from a rather long list) to discuss (we had to choose an idea, a term, a chapter, etc. which we had to present, criticize, analyze, etc.). Professor Iluț’s book – “The qualitative approach of the socio-human” – had recently appeared on the market and it was on the list. As I was reading it I knew that that was going to be the book I will present at the exam. I read passionately and every new idea discovered about qualitative methodology revolted me.

It was the first time I got to know something about the qualitative research and it seemed awful that such a thing existed at all and that it could be appreciated as science. I still have the notes with the ideas I presented at the exam, and here are some extracts.

So, I want to comment on the term “qualitative”. Involvement in a qualitative approach of the socio-human would imply, as I discovered in professor’s Iluț book:

– that sometimes you might not have an effective problem to study: “Sometimes not even the problems are preliminary presented, they are to be discovered and defined through such discussions [discussions which are absolutely free with members of the aimed community]”

(Iluț 1997; 90) and “*In the qualitative vision, the most helpful indication that can be given to someone taking up research work is ‘go in the field, start working and you will see’*” (p. 111)

– that the researcher will not undertake any preconceived documentation: “qualitative researchers do not speak about a preliminary phase of documentation regarding the place and the population selected” (p. 82), will not develop hypotheses: “the theory (concepts, descriptive-explanatory schemes, the assumptions) is born in the living process of the research and from the direct contact with the field – the thesis so dear to the qualitative researchers” (p. 32).

And I was commenting on these amazing discoveries: *putting myself in the shoes of the researcher that engages in this kind of approach I cannot help wondering what I am researching, what I am to have in sight, what I have to ignore, which is the purpose of the research???*

The findings continued: *sampling should be theoretical, and the selection of the study cases will be a problem of flair and intuition.* The questions also continued: *how much confidence can you have in flair and intuition? What confidence can you have in the results of a research performed by a person’s flair and intuition? How representative are the results of a research based on theoretical sampling?*

The results of such a research (I made those propositions in my second year in the college) can only be more or less subjective descriptions of some particular cases, phenomena or processes.

But “the ambition of some qualitative researchers is to do more than just simple descriptions and classifications, aiming towards theorizing and articulated theories. The analytical induction process is brought into discussion” (p. 55). Allow me (I asked, almost like a Caragiale’s character, in the exam room) to doubt the fact that articulated and valid theories can be achieved through analytical induction!

I then discovered *that selectivity plays a crucial role in the qualitative research* (professor Iluț remarks in his field notes: “*to note carefully should not be confused with recording everything or anything*” (p. 82)). And I commented: *without a previous plan, some hypothesis and a problem to solve, the selectivity required to the researcher can only be subjective. It is impossible to demand from a person, may he or she be a researcher, objectivity, especially in the case of participatory observation. His or her work cannot even be classified as descriptions but as impressions. What kind of scientific value can these impressions have (even when coming from scientists)?*

I then discovered that *the elaboration of a research report should be artistic and not scientific (any kind of expression is allowed, the use of stylistic means is encouraged)*. From this moment on, my contempt for qualitative approach was expressed rather eloquently:

The great concern of qualitative researchers is the form of the presentation and not the content. Except for the (maybe) indisputable literary value that this type of presentation of the research report may have, I have doubts about the scientific value, the new and valid knowledge that the research may bring. The purpose of the qualitative approach of the socio-human study is to enrich literature, isn’t it?

The only conclusion of this analysis of in the qualitative approach can only be that this approach is not only useless but it is also dangerous (the presentation of the results of this kind of research as scientific data).

I try to impose myself that naming this approach “qualitative” does not come as a consequence of considering the current meaning of the term. On the other hand, I cannot stop thinking of something of value when I hear the term “qualitative approach”. But what can be of value in this type of research?

I concluded with a citation from professor Iluț’s book. He proposed that in *socio-human studies an overlapping of “the wise quantitative with the rigorous qualitative should exist”* (p. 171). *In my opinion, I stated then, qualitative has no rigorism in it. The vagueness, the lack of clarity, the confusion and subjectivism has nothing to do with rigorism. Therefore, the phrase rigorous qualitative is, for me, something of grammatical lapse.*

This was my first encounter with the qualitative research (I passed the exam, of course, with a 10). This attitude towards it firmly established for many years after that. After graduating the college I remained in the faculty as a junior assistant. After a few years I became professor Onuț’s assistant and during the methodology seminars I explained the students how well the quantitative method works.

Certainly, the majority of sociologists from Romania (and probably a big part of those in the Central and Eastern Europe) would consider my observations and assessments regarding the qualitative research substantiated. I wanted to show that many of its shortcomings were per-

ceived even by myself at a certain point in time. I am aware of this negative attitude towards the qualitative approach because I had it once myself; and I am familiar with the criticism of this type of research, no matter how cutting it is.

III. Reasons for blaming sociological scientific research methodology

Professor Onuþ was my teacher not only during my college years, but also during the period I was his assistant. I learned a lot of things from him; I adopted a great deal of his ideas and dilemmas (without realizing that perhaps I am treating some of them as my own). I have to say that the good ideas present in this article might be his and the inaccuracies are exclusively mine.

During the research methodology seminars, trying to show the students how a scientific research is done I started to discover more and more complicated problems, aggravating the implementation of methodological requirements.

I will mention just a couple of major difficulties that signalled me the sociological scientific research is not what it seems to be. These difficulties gradually made me distance myself from the quantitative research. I got to the point I could not teach the students this type of research because I did not believe in it anymore (in the objectivity it demands, in its strict, controlled procedures, in its assumed validity). I was ashamed to present as scientific something that was not, to push implausible claims, to ask students to do what could not be done.

I do not deny that sociological scientific research has made sensational progress. It resulted in numerous objective gains for science (for example, some measurement scales, prob-

ability sampling, statistic representativeness, sets of statistical tests of significance, sets of rules for avoiding general and specific measurement errors, etc.). But there are many other problems that have remained unsolved and hidden under the rug while trying to make a social science something which it will never be – a natural science. I will come back to this idea later in the article.

Here are some questions regarding the quantitative research methodology to which I could not find satisfactory answers and which have seriously shaken my methodological conviction.

The theoretical framework is of principle importance in most social science research methodology textbooks. But there is one thing I cannot understand from any of them, namely, the way this framing is brought about. How can I teach others if I cannot understand it myself? For a hypothesis to be tested it must have operationally defined terms in the theories. For example, I cannot show that beautiful women are also intelligent until I have an operational definition for beauty and one for intelligence. In a scientific research we are to choose a theory in which we have to define the term *beauty* operationally. We should do the same with the term *intelligence*. There are theories stating that a beautiful woman is the one who is closest to the 90–60–90 figure standard, or other theories saying that beauty lays in the symmetry of the features. In the first case we have to measure the bust, waist and the hips of the women in the sample and then compare the results with the standards. This is the way in which we decide if they are beautiful. In the second case, we measure the length of their legs and

then compare results, we measure the length of both arms and compare the results, and we measure the distance between the left eye and the nose, then the distance between the right eye and the nose, and so on. The women with the closest results to each of the measurements will be declared beautiful. Without a theory we cannot decide objectively and legitimately if a woman is beautiful or not.

The theoretical framework problem lies in choosing the reference theory for a specific research. In sociology there are several theories that describe or explain the same phenomena. There are many theories of the concept of power, theories of elites, theories of deviance, of confidence, of conflict, of revolution, of suicide, of identity, etc. I think it's rather odd we do not have more theories on one and the same phenomena.

In exact sciences it is otherwise. Can we imagine a type of mathematics where $2+2$ does not always add up to 4 but sometimes 1 or 5? Can we imagine a world in which the theory that the Earth is flat coexists with the theory that the Earth is round? The problem with social science is not the existence of more theories of the same phenomenon, it goes to the fact that those theories coexist on an equal footing. Suicide is a phenomenon linked to mental disorders, but suicide is also a phenomenon linked to the level of social cohesion of a specific society. Deviance is the result of society failure trying to master and control human passions (the theory of social control), it is the result of the pressure of legitimate wishes that are encouraged, even prescribed by society, but remain unsatisfied because of the lack of means (the theory of social

contradiction), it is the result of interiorizing the group norms (the cultural theory), and it is also the consequence of the reactions of people to somebody's action (you are deviant if others recognize you as one), not a property of that action (interactionist theory).

More significant than the fact that there are more theories on the same phenomenon, more significant than the fact that these theories are equally valid and legitimate is the fact that there exist contradicting theories about one and the same phenomenon. How do we choose the reference theory from all of these?

Before discussing the problem of choice, other practical problems intervene: How do we know if we have taken into consideration all the existing theories or which are worth to be taken into consideration for our study? How do we know where to stop the search for new interpretations of the phenomenon under study? Any attempt to reunite all the socio-human theories in an encyclopaedia seems too far-fetched. And even if we managed to put together an inventory of those existent by the present moment, it would be outdated tomorrow. We pick the theory from the multitude of theories in front of us, according to the size of the library which we have access to and to our diligence and to the time allotted for reading. Aren't there too many intervening subjective factors that have nothing to do with science?

But what does theoretical framework actually mean? Professor Vlăsceanu speaks about "referential theory". Given the research problem, we rummage among the existing theories for the theory that will become referential for our scientific work. As referential theories may

serve those theories through which we can “read” the problem to be researched (those that refer to the identified problem or to elements connected to it, which propose an approach, which reflect conceptually the investigated reality). The object of knowledge can this way be seen through the “eyes” of the theory: “the researcher relates to the real object from the perspective of his theory of reference, considering it as the object of knowledge” (1982; 230).

The sociology textbooks suggest (at the theoretical framework stage) choosing the most “appropriate” theory as a reference for our study; choosing the best theory that will supply the most “adequate” operational definitions for measuring.

Supposing we are diligent, and we have a large library and a lot of time at our disposal, and we have made a huge inventory of theories for us – how do we actually make our choice in the end? Do we choose the simplest, the prettiest, the best known, the most used theory? The one which supplies the “most operational” definition of terms? Can a choice made like this be objective?

Of course, not. The choice is arbitrary or value-laden. Kuhn shows that “what highlights a careful study of scientific life reality, is that the views of which are compared competing theories do not work as methodological rules that could determine unequivocally a certain choice, but as values that influence it” (1999; 44).

The validity with which a scientific research praises is the criterion validity. Of course, the social scientific researchers do not make a lot of fuss about it (especially because it might put them in difficulty). It shows if the research

is well grounded in theory, if we have chosen the operational definitions that best reflect the studied phenomena. How can the researcher account for the validity of the criterion measure of the chosen theory? He cannot, and he does not!

The researcher has chosen the theory he likes best, he knows best, the one he can find easily and the theory that is easier to use, etc. If we do not choose objectively, following precise methodological criteria, can what we produce be qualified as scientific?

Another important stage in a sociological scientific research is sociological operationalization. Variables to be measured from the hypothesis must be operationalized (those that are abstract and cannot be measured directly). The operationalizations must be deductive (extracted from theories) and not inductive (extracted from the researchers experience or from information collected by others). Professors Rotariu and Iluț (1997; 180) argue, rightly, that “in the absence of a clear theory, an analysis, no matter how thorough it might be, cannot guarantee the identification of all the dimensions of a concept”. As long as you do not operationalize the measuring concepts in theories, you cannot justify objectively why you chose to measure this indicator and not another, why one dimension is missing in the operationalization and why another one appears, etc.

But are the sociological theories a source of operational definitions? How many theories can give us dimensions and especially indicators (meaning directly measurable entities) for the concepts they convey? Let’s suppose that these are enough, that we have enough choice and that we found in them not only dimensions

and indicators, but also information about the weight of any of them in the economy of the variable to be measured.

In sociology we work pretty often with complex variables, which cannot be measured directly, for example, religiosity, anomie, autonomy, domination or even “just” consumer attitude or behavior. What does the operationalization of these concepts contain? How many dimensions, how many indicators? Would it be a surprise to find thousands of them? Can we measure everything in a research? Of course, we cannot! It is here where we make a selection of variables and indicators. Sometimes I wonder when someone comes up with an exhaustive operational definition, but I especially wonder how researchers reach the effective operational definitions (on which they make the measurement instruments). How do they make the selection of dimensions and indicators? They choose the most “representative” for the measurement! But on what criteria do they choose some of them as being more representative, on what criteria they eliminate the others as “non-representative”?

I believe that sociologists – scientific researchers do not even get to the point of having problems regarding this choice because their measurements are not based on exhaustive operational definition. Actually, I believe that the majority of them do not even work with deductive operationalizations.

In these conditions, how many of the measurements are needed to prove the construct validity (I chose the most representative indicators to measure what I have to measure)? It cannot be proven and researchers do not even try because the choice of indicators to be measured is arbitrary or value-laden!

The researcher probably chose the dimensions that came with the indicators or the dimensions for which the indicators were easier to “guess” or the most common dimensions. But is the result scientific if it is obtained from the subjective choice of what is to be measured?

Content validity is most often invoked by social scientific researchers. This may be because it is the most “visible” and easy to impute. Many efforts have been concentrated on it and a lot of studies take pride in really measuring what they have to measure and not something else. Still, the question remains whether the fact that you are measuring what you want to measure has any value as long as you cannot justify objectively the choice of what you want to measure (i.e. the terms of the theory – chosen how?, the dimensions and indicators of the concepts – chosen how?).

I became more and more unsatisfied because of so many questions without a reasonable answer and they continued to bother me regarding the quantitative research. This is the context in which I came across some dusty books in the library: Denzin (1998a, b), Flick (1998), Strauss and Corbin (1990). And during a summer month I devoured every single word from those books. I felt like I was reading really good novels. It seemed natural to look at people, at their actions, at their words as the authors suggested.

IV. Why do I believe more in the non-scientific sociological research?

In 2006 I wrote a “Handbook of qualitative research in socio-human sciences”. I was eager to share with the students what I had discovered and what I had understood from the books I mentioned earlier.

In this book I wrote that “being newer, the socio-human “sciences” were obliged, in order to be recognized, to adopt the scientificity criteria that were used till then in the evaluation of natural sciences. Therefore, they were looking for the “positive” knowledge based on systematic observation and experiment. They had to describe and explain objectively, through measurable results, the phenomena related to individuals and society. These claims have been assiduously pursued for many years and by many researchers” (p. 10).

I sincerely believed (and I still do) that being stubborn about reaching the positivist standards and not taking into consideration the specific and distinctive character of the socio-human sciences can only bring damage to them.

And I made use of a figure of speech. “Lying in a bush and prowling for a deer, the man puts some words together; he forces them to work together and sound pleasingly. He composes something that will be named as *poem*. Other men, in moments of boredom or inspiration, do the same. They become great poets. They are *poets*. They invented and realized it in accordance with their mind, structure and possibilities. Women, rather than waiting calmly for the inspiration that would have led them to compose a new “wonder”, made to fit their mind, structure and possibilities, they struggled restlessly to make “poetry”. No matter how hardworking or inspired, they will never be men and they will never compose “poems”. This not only qualifies them as helpless, because they cannot create something similar, but they are also “guilty” because they robbed the human race of the pleasure of enjoying non-invented

“wonders”. The natural sciences are men, the socio-human sciences are women. The poem is the positivism, and its wonder waits to be discovered” (p. 11).

The qualitative research seems to keep in mind the fact that the purpose of the socio-human science research is different than the purpose of the natural science research. So, the discussion is about knowing something else and through other means. In conclusion, the evaluation of this knowledge can only be made by other, than the positivist, criteria.

“If the qualitative research is evaluated using the positivist criteria, than it and its results cannot be “scientific”. But an evaluation using these criteria cannot be made; it is not appropriate. The qualitative research suggests a different type of knowledge (of a different nature). One thing is to aspire to discover some laws, to generalize some data (knowledge evaluated by “conventional” criteria) and it is something different to strive to discover profound information, to get to understand (knowledge evaluated by “naturalistic” criteria)” (p. 12).

Even though the evaluation criteria for the qualitative research are weak and even if it does look nothing like a scientific research, it is fair to recognize your limits (to show how much you can obtain), rather than trying to hide your weaknesses (claiming you have obtained what cannot be obtained).

Therefore, I consider that the scientific research, as it is applied in natural sciences, is not appropriate for the study of people. The subjects of the research here are not substances, plants, numbers or electric circuits; they are people and they talk, they can lie, they can dissimulate,

they can change their attitudes, behavior or statements according to social desirability, they defend their reputation, they take care of their image, etc. The research situations in socio-human sciences do not excel when it comes to control; the studied phenomena are rarely (so I do not say never) exactly repeatable, etc.

Proposing a new research methodology in the socio-human sciences brings to discussion a new principle – that of the plurality of the scientific methodology and it presupposes the independence of socio-human sciences from natural sciences.

And even if we do not call it science, and even if we do not call it research, and even if we do not call it qualitative, we cannot deny the fact that this something exists and that this something, or maybe a better something, is needed in social sciences.

Qualitative research is being carried out in the natural environment of the studied phenomenon or in the natural environment of the subjects, it does not create artificial situations, it does not provoke events, it does not bring its subjects in labs. The researcher, more than the operators, finds himself in the field observing, interviewing, etc., he treats his subjects as equals, he understands them, he suffers and he is happy with them if needed, he is not a stranger for them and he does not treat them as such, he lets them talk, he respects their points of view, he does not any second pretend he knows better what is going on with them, what they think or feel, how and why they do what they do. He pays attention especially to the complex and deep ideas and feelings; he looks for information about the way they think, what they appreciate,

how people interpret different things, not only what they say they vote, consume, buy, etc.

Qualitative research involves a great opening towards the field, it favours the point of view of the subjects, trying to describe and explain the studied phenomena without preconceptions, unlike the scientific research which elaborates a priori hypothetical propositions (putting in foreground the researcher's point of view), which later build up the situation and verification tools to conclude, in most cases, that the hypothesis is confirmed (it always seems amazing to me how rarely the hypothesis is refuted, and it does not come as a surprise that many beginning researchers think that if the hypothesis is refuted the research has failed).

The image obtained as a result of qualitative studies is a process, capturing details (like a movie which lets you get inside the subjects mind, in their homes, etc.) unlike the one obtained from the scientific studies which is static, dealing with the exterior of the subjects (like pictures that capture only surface information). The qualitative researcher, unlike the scientific one, is aware and recognizes the fact that the social research implies the subjectivity of the researcher and that it cannot be free of values, that it can be fundamentally affected by errors generated also by these evidences.

A renowned Romanian historian wrote about historians something which I think is true also for sociologists: "if you look up in the dictionary, you will find that "objective" is "something that exists outside the conscience", so, logically, can only be applied to the study of objects, things, inert matter. The historian deals first of all with humans – individuals or com-

munities, therefore subjects are not objects, and in order to understand these subjects he must be subjective” (Djuvara 2010; 6).

In my opinion, the sophisticated statistical machine (worthy of all respect) has pushed us further away from the social knowledge (as it is supposed to be). What percentage of the variables we measure are numerical or continue? Why do we need accurate statistical procedures if we can use them only in exceptional cases? Doesn't the description in percent, on graphics of human realities simplify a little bit too much their complexity? Are we not trying to mould statistically and mathematically interpretations, feelings, representations?

Professor Rotariu somehow confessed at a conference (I hope I am not distorting the meaning of his words): I never succeeded in trying to confirm explicative models in actual life, this is just not possible; human phenomena do not follow explicative models which you can confirm statistically.

And if it cannot be done, why do we still bother? Would it not be more productive to focus our efforts on promises rather than on impossibilities?

I am sure that 1000 quantitative studies, 1000 statistics or cold and dry graphics say less than one qualitative study. The results of qualitative research are extremely suggestive and this increases greatly their applicability.

Here are some examples of the projects of qualitative research done, under my coordination, by the students from Social Work Department each year (they help make the voice of some forgotten, marginalized and even discriminated subjects heard). Cancer patients

who talk about the way they handle the disease, about what keeps them alive, what bothers them at the people they interact with and about the shock they have when they look in the mirror and discover they do not have eyelashes or eyebrows anymore; women molested by their husbands, who talk about shame, about their mothers who taught them they were to endure; gipsy children who relate about their first days of school, about the fact that, at the age of 7, they discover that “water can come out of the wall”. The help given to these people can be, as a consequence of this kind of study, 1000 times more efficient.

Post-modern sociology set itself apart from the model of the science that produces laws, absolute truths; it became contextual; the great theories faded in front of the local ones. This situation stimulates the application of the qualitative research methodologies. We can call it non-scientific, and it seems likewise, but it is also more adequate to the human realities and to the contemporary characteristics of their studies.

Even if we stick to the big theories, how were they developed? How “scientific” are Marx's or Webber's theories? Babbie (2010; 461) lists them within the comparative-historical research, which he calls “typical qualitative”.

I do not believe that there is a sociologist who disregards the works of Goffman, Bourdieu, Baudrillard, Bauman, etc. We consider them great theoreticians of the field; we interpret different life or professional contexts using their theories. Does anybody laugh at their theories? Does anybody consider them literature or any other form of art? Is it not

knowledge what they made (if they have not applied experiments, surveys on representative populations, etc.)?

In 2007, Goffman was the sixth most quoted author in social and humanistic sciences. What about his method? Was it not qualitative research? Among other things, he did an extensive observation that lasted a year, in a mental hospital (posing as an employee) describing the world of the patients; he interpreted the life of the people as a drama representation (describing the actors, the stage, the audience, the costumes, etc., as they appear in day to day life).

Is there a more metaphoric and at the same time meaningful discourse (which hits you like a lightning once you discover it) than that of Baudrillard? And is his writing fiction? Does the metaphor of the obese or that of the obscene have another value besides the stylistic one? Do not concepts like habitus (Bourdieu) or practical consciousness (Giddens) have an extraordinary explicatory power for human behavior, attitude, etc., but are completely un-operational at the same time?

I stated in an earlier paragraph that the sociological theories are very far from the situation in which they could give operational terms in order to facilitate and legitimate the measuring. Among the shortcomings of social theory, Blumer (1964) mentions the fact that: „social theory is conspicuously defective in its guidance of research inquiry. It is rarely couched in such form as to facilitate or allow directed investigation to see whether it or its implications are true”.

The concepts, the author goes on to say, have a vague meaning and they lack a precise

specification of attributes: „it should be evident that concepts in social theory are distressingly vague. Representative terms like mores, social institutions, attitudes, social class, value, cultural norm, personality, reference group, social structure, primary group, social process, social system, urbanization, accommodation, differential discrimination and social control do not discriminate cleanly their empirical instances”.

I think we can agree with Blumer that the majority of the concepts in social theories are “sensitizing concepts”: „A sensitizing concept lacks such specification of attributes or benchmarks and consequently it does not enable the user to move directly to the instance and its relevant content. Instead, it gives the user a general sense of reference and guidance in approaching empirical instances. Whereas definitive concepts provide prescriptions of what to see, sensitizing concepts merely suggest directions along which to look. The hundreds of our concepts—like culture, institutions, social structure, mores, and personality—are not definitive concepts but are sensitizing in nature. They lack precise reference and have no benchmarks which allow a clean-cut identification of a specific instance and of its content. Instead, they rest on a general sense of what is relevant”.

The author thinks that these characteristics of the sociological concepts are not caused by the immaturity of social science but by the nature of the empirical world that we study. The need for a qualitative approach of the social (I add) is given by this nature and I also sustain what Blumer says: “what we are referring to by any given concept shapes up in a different way in each empirical instance”. Qualitative research

uses these concepts (where their imprecision and their lack of clarity brings full benefits) as follows: „Socio-psychological concepts such as family, victim, stress, stigma, and so on are defined in a loosened manner at the beginning of the study; they are not given operational definitions so that the researcher could explore the way in which the concept manifests and how it is given a particular meaning in the set of circumstances which are being investigated” (Schwandt 2007; 274).

Here is an example: one of the things I did for my PhD program was the conceptualization of the identity of managers in Romania. I started from the very loose definition of identity given by Gadrey (1998). He defines identity as a set of circumstances which make a person that specific person. And analysing the interviews I had with the managers I discovered the circumstances that made them managers (relational, material or professional circumstances, circumstances linked to childhood, teens or maturity etc.). Represented schematically (like an operationalization), the identity of the studied managers – as a set of circumstances – qualitatively discovered, covers 7 dimensions and 50 indicators.

There are very numerous social research situations in which the measuring of the variables using the scientific methodology is not enough. For example, we can measure the voting intention, but what if we need to understand how the voting options are formed? Most of these situations are solved by wide admittance and usage (even in Romania) of a qualitative group interview technique: focus-group.

Even in its structural forms, the focus-group is still a non-scientific or qualitative technique.

Professor Iluț (1997; 92) affirms that the group interview is especially qualitative “also because of the fact that here we are not interested primarily in the way people say a thing or another, but what people say, how they say it, and how what is said comes into being, etc.”.

We cannot say that what we are trying to obtain through a focus-group is not knowledge (as we cannot say that what we obtain through the implementation of other qualitative data collection techniques is not knowledge). Of course, it is a different kind of data: more complex, profound, non-numerical, very hard and useless to quantify. But this kind of data is not useless. It reveals, for example, the way in which political opinions are formed, the way in which voting options are aggregated, situations and ways in which these opinions change or are structured. This is the data on which campaign strategies are based and then elections are won. Not to mention the numerous successful marketing campaigns that were designed using information obtained through focus-groups. Why couldn't the individual qualitative interviews produce new knowledge? Why do we consider as good the results obtained in 2, 3 focus-groups but not the data obtained from 20, 30 individual interviews?

If we “give in” and consider “valid” the knowledge produced by focus-groups, why is it so hard to accept biographical interviews as a source of knowledge (or diaries, or pictures, etc.). If we accepted the existence of theoretical sampling (and we argue that we use it in achieving focus-groups) would it not be better to apply it “by the book” (by the “qualitative book”)? Theoretical sampling does not mean to sample a

priori. It does not mean to only suppose (according to the objective) it would be better to add to the focus-groups workers and intellectuals, young people and old people, men and women, etc. Theoretical sampling means to discover who else you have to interview during the collection and analysis of the data. It means to decide who enters the sample according to what you find out along the way. For example, you find out that young people vote as their friends do, but with the “less young” people things are more complicated. This means you will have to select additional “less young” samples (even divided on significant subsamples of adults and old – according to the analysis) in order to find out in detail the way in which they vote.

I heard many scientific researchers say that they used in their studies theoretical sampling. What actually hides under this name is, in most cases, the non-probabilistic sampling (often one of its type is accidental sampling). In order not to look bad (from a scientific point of view) researchers remark that they did a theoretical sampling even though what they applied does not meet its criteria.

What kind of people we bring in a focus-group still seems to be a problem which is being handled (wrong). But it is a bigger one for the scientific researchers who try to solve problems through non-scientific methods. How many focus-groups do we need to form? Two, three, answer the eager ones. Anyway, it should be more than one... Actually, the analysis of the data is what precisely indicates how many we need. It is a condition which was missing in the qualitative model: achieving theoretical saturation. If we form a focus-group with young

people, one with adults and another with old people it does not mean that we already have three group interviews and that it is enough. Normally, we should form another focus group with each of these categories. And then analyze the data. If the second focus-group with the young people brings us new information about the way in which they form their voting intention it means we did not reach theoretical saturation. And this also means we will need a third group interview with other young people. And if the third one brings us new information, we do another one and so on. We should proceed like this with the other categories too.

I consider the incorrect and incomplete assumption of the qualitative procedures to be dangerous. If we have to accept them as necessary, at least let's use them honestly; let's (re) know and respect their principles.

Therefore, in lack of something better, qualitative research at least takes into consideration the fact that we study people and human entities; it gives really good, complex, profound, meaningful results, with much more sense, with an amazing suggestive power, results which deal more with feelings, thoughts, attitude, with people in general; it works better with the post-modern sociological approaches, it uses more the sociological concepts and in a more productive way (considering their characteristics); it has proven to be very useful (see the use of focus-groups) and it is necessary to understand it in order to use it correctly.

V. “Once I was a positivist”

I never had the impression that my experience with research methodology was different

from that of the others; actually I felt the transition from positivist to being sensitized and impressed by the comprehensive was something normal, established a priori. Still I had a wonderful experience when I discovered Bertaux's (1981) study which started with the phrase: "Once I was a positivist". Of course it would be a bold claim on my behalf to say I identify myself with the French sociologist (under whose name was written: *Centre National de la Recherche Scientifique*, Paris) but his formation makes me think I am on the right track too.

Undoubtedly, Bertaux knows what science means. He took math, physics, electronics classes; his first degree was that of engineer; he worked in the field of artificial intelligence and military engineering, and as he says: "I thought sociology could become a true science, and I was eager to make it more scientific" (p. 29). He did mobility studies and he was recognized as a specialist in processing statistical data.

Bertaux confesses that he realized „that the scientificity of sociology is a myth. If there is such a thing as sociological *knowledge*, the way to reach it is not through quantitative methodology. And the main obstacle towards it is precisely the belief in sociology as a science" (p. 30).

I will try to sum up this article by presenting the French sociologist's ideas from his study.

Even if sociology cannot become a science (as physics, biology and others), it does not mean that social knowledge does not exist. „Neither sociology nor economics, history or anthropology will ever become sciences like the natural sciences. Social life is made out of struggles whose outcomes are unpredictable.

There are no 'social laws' like physical laws, that is: eternal, totally accurate, acting upon everything in the universe. [...] If social science is not possible, it does not mean that social knowledge is an illusion" (p. 41).

We can produce new and useful knowledge through interviews and qualitative observations or through qualitative analysis of social documents (exactly as the focus-group knowledge is produced). The way in which we produce scientific social knowledge now does not fit the characteristics of our subjects; knowledge obtained like this has major shortcomings.

Through questionnaires, the sociologists – the scientific researchers – manage only to stifle the voice of the subjects (they reduce the answer options; they simplify feelings, thoughts, complex behaviors, etc.). Even though the subjects talk and think, sociologists do not listen to them and they think for them. Talking about the difference between natural and social sciences, Bertaux observes: „the objects sociology examines do talk. They even think. And the sociologist is only one of them, one among many, a human being among human beings. So that, in order to talk not as a simple human being but as a 'scientist', he has no other way than first to reduce them to silence. If given a chance to talk freely, people appear to know a lot about what is going on; a lot more, sometimes, than sociologists" (p. 38).

This way of producing science, the way in which the results of knowledge are presented has distanced the beneficiary of knowledge from sociology: „sociology is not much read these days. And this is a paradox. If sociology

were a specialized science like, say, biochemistry or electronics, one could understand that none except specialists would read about it in specialized publications. But sociology is not that. Its contention is that it deals with institutions, cultures, forms of social life, social relations, in other words, with the very texture of social life as people live it. And yet it seems that these very people who should be primarily interested to hear what we have to say about their societies, turn instead towards reading history or anthropology, not only because it is 'dépaysant' (exotic), but also because the discourse of these disciplines makes quite often for highly enjoyable reading" (p. 32).

So, even if the scientificity of sociology is a myth, social knowledge is not an illusion; it is necessary to let our subjects talk (as they have this ability), it is only normal to do everything possible to make sociology be read. I plead to make sociology a pleasant discourse to be read in Scârneci (2011).

I was once a positivist, too, and I think that nothing can take me there again. Even if I am aware of the many shortcomings of the qualitative approach, I do not want to make myself guilty of ignoring the possibility to discover undistorted social world.

VI. Conclusions

I showed in this article what made me set apart from the positivistic approach of the

social, which I think are the major flaws of this approach and the dangers it generates; I showed what made me get closer to the qualitative approach and its merits (even if only as a promise).

Qualitative studies are indispensable for the discovery of profound social; they should be recognized at least because of the important value of the hypothesis they generate.

Not recognizing and undermining the qualitative approach in Romania (considering it as second hand knowledge in Central – East Europe) preserves the status of "left behind" for our sociology. In 2010 I sent an article to a great qualitative research journal in the United Kingdom. The reviewers expressed their amazement regarding my enthusiasm, long gone in Western Europe, regarding the discovery of qualitative; they were astonished by my pro qualitative arguments (there you do not have to justify qualitative, it is an accepted approach, on equal grounds with positivism). Since then I cannot to find my place (I am supposed to be old fashioned for Western Europe and in Romania I am pushed out of sight).

I wrote this article in a style which I think can be called qualitative (I am aware of the fact that it is not typical and hard to accept by many sociologists – scientists): I was emotionally involved in my writing, I was subjective at times, I used the first person, I narrated experiences, I tried to make the reader understand, I challenged him.

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SANTRAUKA

KODĖL AŠ PASIRINKAU KOKYBINIO TYRIMO BŪDĄ?

Straipsnis parengtas remiantis asmenišku autorės, dirbančios tyrimo metodologijų srityje, patyrimu. Analizuojami sociologinio mokslinio tyrimo trūkumai, susiję su dviem tyrimo pakopomis: teorine struktūra ir operacionalizacija. Viena vertus, svarbu kriterijų ir konstrukcijų pagrįstumas, kita vertus, sociologinių teorijų taikymo moksliniams tyrimams patikimumas. Aptariamos kokybinio metodo pasirinkimo priežastys ir aplinkybės, rodančios, kad šis metodas Rumunijoje ir Centrinėje Rytų Europoje yra marginalizuojamas. Svarstomi įvairūs kokybinio metodo privalumai, įskaitant plačios apimties *focus-grupių* pavyzdžius. Straipsnio autorė nevengia kokybinio tyrimo būdo apologijos provokacinio stiliaus.

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