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## Grounded Theory Methodology (GTM) and CAQDAS: An Exercise of Autobiographical Research and Methodological Reflection

*Miguel S. Valles* \*

*To my uncle Aniceto Martínez Poza, who became a boy in white  
and did the best for my health from childhood to adulthood  
In Memoriam.*

**Abstract:** In the first part of this paper I examine how a need to study the intellectual *roots* of Grounded Theory Method (GTM) emerged out of reflecting on my personal *GTM reception*. In the process of this I became more and more *methodologically aware* of the nature, that is, the genesis and development, of GTM. Following this, I argue that a similar process of ‘becoming aware of GTM’ can be traced in the writings of GLASER and STRAUSS. At the same time, the whole paper is in itself an attempt at practicing GTM, conditioned by the published versions of GTM and the process of reception in my case. I try to gain awareness of both the intellectual roots of GTM and the process of my reception of this methodology. In this paper I trace for the reader about the processual and contextual character of my GTM reception. I show how I finally reach the conclusion that the methodological principles of GTM existed before this approach was developed: they were not invented by the coiners of GTM. Moreover, I emphasise the existence of both professional and more private personal experience roots in the explicitation and genesis of GTM. In

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This article is the result of an invitation from Günter MEY and Katja MRUCK to the author, to contribute to this Supplement on grounded theory methodology. I want to express my gratitude to the editors for having offered me this challenge.

the second part of the paper, I explore the conceptual trace and historical-biographical depth of the more comprehensive debate about the particular relation between GTM and CAQDAS<sup>1</sup>. My main attention is the case of GLASER's posture as the *epicenter* of the debate. I show the reader the way I proceeded in generating typologies of basic cases and processes to give context to GLASER's posture. This contribution is composed from an autobiographical point of view and style of research throughout.

## 1. Preface

I feel that nobody writes once and for all seasons: I have had other<sup>2</sup> opportunities (see, for example, VALLES 2001 and 2005b) to express my own research practice experience and ideas in relation to the connections between methodology and technology. And, although this topic is announced in the abstract as part 2 of this paper, I start by mentioning it because it is my main focus in this article. At the same time, I consider it necessary to make explicit right from the beginning the perspective with which I face the issue of these connections: that is, from a biographical approach. More specifically my focus here is on *grounded theory methodology* (GTM) and on *computer assisted qualitative data analysis software* (CAQDAS). This last acronym is just the ice-berg visible portion of a category that includes a range of technological parts (hardware for instance, not only software) and technological processes (e.g., digitalization<sup>3</sup>).

As a social researcher with a special interest in methodology, I find exciting the experience of re-reading other people's works, (and mine), after a period of time has passed. The main reason for this is that one always discovers something new to cite or to think about. We are not the same reader yesterday and today, in the past and in the present. In this article my intention is to blend methodological reflection and research practice, re-writing and re-reading a selection of materials that may help in a controversy with its own history. Following GLASER's dictum "all is data" (GLASER 2001) and my preference for a biographical methodology, I consider it important not to leave aside the autobiographical perspective of the authors of those materials. That is why I consider it necessary to start with the following part first in order to help to under-

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<sup>1</sup> CAQDAS stands for Computer Assisted Qualitative Data Software.

<sup>2</sup> Some previous attempts to focus on these topics were faced in VALLES (1997, 2001, 2005a).

<sup>3</sup> See, for example, the special volume of *FQS* – edited by Graham GIBBS, Susanne FRIESE and Wilma MANGABEIRA – on using technology in qualitative research: <<http://www.qualitative-research.net/fqs/fqs-e/inhalt2-02-e.htm>>.

stand my evolving points of view on the matters which are dealt with mainly in the second part.

## 2. GTM in (Auto-) Biographical Perspective

The selection of this heading at the start of this contribution gives the reader a first clue about the author's approach to the complex question of what GTM means: I find it honest and efficient to offer an answer in a narrative way. An historical and biographical approach may help us to understand the temporal nature of the various processes implied. Let us name them for short: *GTM roots*, *GTM versions*, and *GTM reception*. These three nodes or categories are useful for the sake of thinking in the self-conscious style learned mainly (but not only) from the writings of GLASER and STRAUSS. These three *GTM expressions* are related to each other. They show different facets of the same object of research. Moreover, the study of the process of *GTM reception* in my own case led me to the study of *GTM roots* (and the related processes of genesis and development of GTM compared to other methods). And, in the process of becoming more aware of the existence of various public (published) versions of GTM, I have discovered private versions some of which I had elaborated in part by myself out of my incomplete and personal *reception*.

But I believe any collective and individual experience of knowledge reception opens the way to its adaptation or transformation, even if this was not what was intended. This is something that is a challenge to the original version of an intellectual work. However, in a sense this is not only negative or risky. As I shall try to show in the following pages, my experience is one of re-reading the writings of the developers of GTM, and finding each time new insights for my own research and teaching activities. One of those activities has been around the relation between GTM and CAQDAS. This is part of a more comprehensive debate (the relation between qualitative methodology and technology), the understanding of which involves the need to study some important issues and implied topics such as reception, roots and versions of GTM.

### 2.1 From GTM reception to GTM roots (and versions)

I know from the start that an intellectual autobiography focused on GTM is an *illusion* (BOURDIEU<sup>4</sup>). But my aim is not at all an exhaustive description; not even a general one. Instead, the autobiographical perspective is taken as a valid method for generating (eliciting) insights on that matter; that is, more specifically: what GTM means. In my sociological career I worked with the literature on biographical method before I developed an interest in, and re-readings of,

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<sup>4</sup> I refer here to one of BOURDIEU's essays entitled *L'illusion biographique* (1994).

the GLASER and STRAUSS 1967 book<sup>5</sup>. I must admit, though, to the increasing influence of their writings, from 1992 till today; either directly (GLASER & STRAUSS 1964, 1967; GLASER 1965, 1978, 1992, 1998, 2001, 2002, 2003; STRAUSS 1987; STRAUSS & CORBIN 1990, 1994, 1997) or indirectly (TESCH 1990; DENZIN & LINCOLN 1994; HOLSTEIN & GUBRIUM 1995; CHARMAZ 2000, 2006; DEY 2004; FLICK 2004; BÖHM 2004).

I remember myself as a graduate student of Sociology, in the Spain of 1979-81, and even as a postgraduate writing my doctoral dissertation between 1985 and 1988, when my attitude towards sociological theory was mainly one of *verification of existing theories*, rather than *generating theory* from data. From then on, and thanks to the successive approaches to GTM, I felt more obliged, to include expectations of conceptualization in every research practice, and more confident about doing so. Along with this mental process I noticed that some of the basic recommendations found in GTM had a familiar flavour (*sabor* in Spanish) to my academic and professional training in the past. For a long time I lived with the implicit working hypothesis of a network of methodological principles, transmitted by way of mouth or writings, without naming the term GTM. So, I decided (recently) to study *GTM roots* in order to fill this gap

A general and well known summary: GTM is – following GLASER (1992, p.16) – rooted in two mainstream traditions of social sciences research. One formed by the intellectual family of STRAUSS, whose thought was influenced by Robert Ezra PARK, William THOMAS, John DEWEY, George Herbert MEAD, Everett HUGHES and Herbert BLUMER. The other composed by the upbringing of GLASER, related to the names of Paul LAZARFELD, Herbert HYMAN, Allan BARTON, B. MCPHEE, B. BERELSON, Robert K. MERTON, Hans ZETTERBERG, Seymour LIPSET and Alvin GOULDNER. In short, the social research tradition of Chicago and Columbia universities of the first half of XXth century. There are also deeper roots, and we should mention at least the influence of the sociological works of WEBER, DURKHEIM and SIMMEL either for their importance for the (constant) comparative method<sup>6</sup> or their sociological thinking and practice.

I cannot present here the micro connections amongst all these names. Suffice to focus our attention on a couple of cases, in order to make explicit and briefly show the intergenerational transmission of a few but core methodological principles. Again, this search for *GTM roots* is mediated by my preference

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<sup>5</sup> A contextual supplement may be seen in VALLES and BAER (2005).

<sup>6</sup> Basically the constant comparative method (CCM) is a third approach to the analysis of qualitative data, proposed by GLASER and STRAUSS in 1967, consisting of the combination of the coding procedure of content analysis (first approach) and the style of analysis by inspection of a second approach practiced by some of the authors cited as source of influence. CCM aims at generating theory (as the second approach), filling the gap of the first approach, and does not pretend to test theory in the same way as would classical content analysis.

for a narrative autobiographical style. In fact, one of the questions that drives the search is: what about my own reception of GTM? A first answer is that there are some aspects I already practiced without knowing anything about GTM, without *methodological awareness*. Let us take the present writing as an exercise of research which follows some of the GTM principles, and not merely as a programmatic methodological reflection<sup>7</sup>.

## 2.2 First examples of today's GTM roots research and yesterday's research practice without full GTM awareness

In this section I give some examples of my current research into the roots of GTM and of my former research practice without full awareness of GTM. Firstly, I underline the methodological essence of the proposal of GLASER and STRAUSS (that is, comparison) and show how this acts as a clue to the existential context of its reception in my case. To do this I insert, now and then, autobiographical paragraphs which constitute the perspective chosen to deal with the matter of this chapter. See for example the title. Among the pieces of research experience remembered here is a former use of CAQDAS. This will help the reader to understand my posture in the debate GTM and CAQDAS as it is set out in the second part of this chapter. All in all, my intention is to synthesize the process of reception of GTM and CAQDAS in my case as a way of illustrating the knowledge acquisition of GTM over time.

The core methodological principle of GTM is, without doubt, comparison<sup>8</sup>. The first and principal part of the 1967 book of GLASER and STRAUSS proclaims the generation of theory through "comparative analysis". Already the abstract offered by the authors underlines the importance of this omnipresent principle.

In the first section – Comparative Analysis, we shall present a strategy whereby sociologists can facilitate the discovery of grounded theory, both substantive and formal. This strategy involves the systematic choice and study of several comparison groups. [...]. In chapter III we discuss theoretical sampling – the process of collecting data for comparative analysis designed to generate substantive and formal theory [...] in chapter V we offer our method for the comparative analysis of qualitative data. In chapter VI we clarify and

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<sup>7</sup> *Ethnography: Principles in practice* is the title Martyn HAMMERSLEY and Paul ATKINSON gave to their handbook, first published in 1983 (and translated into Spanish in 1994). Here I would like to follow a similar path: GTM basic principles, in context (the *existential context* of a particular *reception*), while addressing its practice via some examples of application.

<sup>8</sup> GLASER and STRAUSS (1967, p.21) notice that the logic of comparison is present not only in their proposed strategy of "comparative analysis", but also in the experimental and statistical methods. As the expression "comparative analysis" is used by some social researchers with the purpose of verification or "to refer only to comparisons between large-scale social units", GLASER and STRAUSS remark that their use is directed to the generation of theory and for use on social units of any size.

assess a number of previous comparative studies in terms of several important questions (GLASER & STRAUSS 1967, p.9).

The systematic character of the proposed comparative analysis is a recurrent message which occurs throughout the pages of this foundational work, where the reader is given the original examples and definition of that along with other key terms such as *substantive* and *formal* theory. In relation of this I must direct attention towards the root sources of the comparative method selected and applied by GLASER and STRAUSS for the purpose of grounding theory.

Drawing on the autobiographical thread again, another series of photographic and film graphic snapshots emerge from my memory. My specialization in sociology of population (human ecology, population theory, demographic analysis, urban sociology and so on) explains a greater familiarity in my case with the work of academics such as PARK. Later on, my increasing sympathy towards, and use of, qualitative methodology, together with the influence of the mastership of the Spanish sociologist Amando de MIGUEL, would converge in the enhancement of SIMMEL. For this reason, and also following the references made by GLASER and STRAUSS (1967) to the work of these sociologists, I have decided to select and highlight here their contribution to my knowledge of GTM.

A first re-reading of *The Discovery of Grounded Theory*, searching verbatim for what could be codified as *GTM roots*, led me to discover a similarity<sup>9</sup>, confessed by GLASER and STRAUSS (1967), between their proposal of grounded formal theorizing and “the kind of essay writing established many years ago by Georg Simmel, and nurtured by such contemporaries as Erving Goffman and David Riesman” (p.97). This style of research is presented as opposed to the named “logical formal theory” (ungrounded) and exemplified with fragments of the work signed by disciples of MERTON or PARSONS.<sup>10</sup>

Following the recommendation of systematic comparison which was so much underlined by GLASER and STRAUSS (1967), and which was also universal and previous to their work, I have proceeded to practice this in my successive re-readings. This is something I have complemented with the advice of sociological theory reference books that pay attention to the network of influences among classical and contemporary theoreticians (RITZER 1993; OLTRA, GARRIGÓS, MANTECÓN & OLTRA ALGADO 2004). This auxiliary task was fruitful, resulting in for example, the process of linking the network of influences from SIMMEL to PARK, and from the latter to BLUMER, ending in STRAUSS and GLASER.

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<sup>9</sup> This analogy is not free from critical considerations. That is, there is a lack of integration of concepts and a lack of comparative research which is sufficiently credible.

<sup>10</sup> Years later, GLASER (1992, p.16) relates the intellectual influences on his and STRAUSS' proposal, but only mentions the name of PARK; references to SIMMEL, GOFFMAN or RIESMAN are missing.

Finally, my second reading of GLASER and STRAUSS (1967), this time focusing my attention on the roots and nature of the influence related to the work of PARK and SIMMEL, led to various results which were especially relevant and that have increased my *methodological awareness* of the nutritious humus of GTM and of my research experience. It is worth noting the joint references to PARK and SIMMEL made by GLASER and STRAUSS in a chapter (sixth) about the assessment of comparative studies. They note that it is “a style of theorizing and of comparative analysis that is still used today (by David RIESMAN<sup>11</sup>, for instance)” (pp.153-54). And they add something I judge useful to quote here about the role of personal experience<sup>12</sup> and the readings far out from the field of sociology:

The generation of theory by Park and Simmel was based largely on data yielded by personal experience, on casual but not undirected observation, as well as on reading of wide scope. In consequence, when we read these men, we experience a kind of simultaneous double exposure to ideas of high abstraction closely linked with an immediately recognizable world. At the same time, we recognize also that their theory lacks integration – a matter to which they paid little attention and of which they were probably quite unaware (GLASER & STRAUSS 1967, p.154).

In extracts like this it is easy to appreciate the genesis and development of the GLASER and STRAUSS proposal, which tried to systematize and improve the inherited research practices oriented towards the generation of theory. The influence of PARK<sup>13</sup> and SIMMEL include the key element of comparison in GTM. Of PARK<sup>14</sup> it is said that he “was always striking off ideas, generalizations, about social life”. And such generalizations (“rooted in personal observations and wide reading”) “also rested upon a method of implicit comparisons”. For instance, PARK’s concept of the marginal man or his hypothesis about a race relations cycle (GLASER & STRAUSS 1967, p.154).

To both classic authors, GLASER and STRAUSS dedicate epithets such as: “generators of theory”, “inventive discoverers of categories and properties”,

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<sup>11</sup> In a book published in Spain by a research group working on sociological theory from a narrative perspective, OLTRA and collaborators (2004) affirm that RIESMAN followed WEBER’s methodology of the *ideal types*; and also that he was inspired by the reading of *La rebelión de las masas (The revolt of the masses)* in the forties, the celebrated work of the Spanish philosopher ORTEGA Y GASSET originally published in 1930.

<sup>12</sup> On the role of “insights” (and, in general, on the role of reflexive experience of one’s own or other people’s) in the generation of theory, see the last chapter of GLASER and STRAUSS (1967).

<sup>13</sup> PARK went to Germany in 1899 to study Sociology in Berlin, where he attended SIMMEL’s classes; SIMMEL’s intellectual legacy is present in the studies of PARK on urban life (OLTRA and collaborators 2004, p.277).

<sup>14</sup> “Simmel was more systematic. But many of the same things could be said about him. Perhaps he drew more upon scholarly studies for his materials, but he used them and his personal observations in much the same implicitly comparative fashion” (GLASER & STRAUSS 1967, p.155).



“prolific generators of hypotheses”<sup>15</sup>. And to this profile they attribute the research inspiration that so many sociologists of the next generations have found in PARK’s and SIMMEL’s writings.

Back to my recent past research experience, I remember a study on ethnic discrimination (CACHÓN & VALLES 2003) where the concept of implicit comparison emerged not from data, but in the process of publishing the results. The Review editors insisted on the necessary comparative perspective that our article should have. That meant a European comparative perspective at the cross-national level. We argued that our study had an explicit and fieldwork comparison at the intra-national scale and an implicit (omnipresent during design and interpreting moments) comparison at the cross-national scale. We then cited the 1987 Presidential Address of the American Sociological Association (KOHN 1987). Certainly, this author prefers “to restrict the term, cross-national, to studies that are *explicitly* comparative” (implying a systematic use of comparable data collected from two or more countries). Although he previously admits that “many studies of single societies are implicitly cross-national, in that the investigators interpret their findings by contrasting what they learn about the country they actually study with what is known [...] about some other country or countries” (KOHN 1987, p.714). Obviously, when one reads this and the surrounding paragraphs of KOHN’s article, it is easy to notice that his methodological position is far away from the proposal put forward by GLASER and STRAUSS in 1967 and after.

Retrospectively, writing now in 2006, the research project on ethnic discrimination in the work place represents a milestone in my process of GTM reception. Previously, in 2000, I had published a methodological contribution in the Spanish context, titled *La grounded theory y el análisis cualitativo asistido por ordenador* (*Grounded theory and computer assisted qualitative analysis*). That is, the process of methodological reception co-occurred with a process of technological transition. In the research team, I was the only one with this double orientation: GTM and CAQDAS. This was something that was at the base of the decision to present a paper on my own at the Spanish National Congress of Sociology (September 2001). I tried to use the methodological theory learned from GLASER and STRAUSS (1967), STRAUSS (1987) and STRAUSS and CORBIN (1990), trying out meanwhile the programs NVivo and Atlas.ti. The pretension of joining both worlds (the old methodological tradition born and developed in a pre-computer science epoch and the new electronic tools lacking academic history), was stimulating but also upsetting. It was a moment of more enthusiasm coupled with innocence about the expectations of the partnership of GTM and CAQDAS. The former (GTM) provided a new jargon but similar content to the lessons learned bibliographi-

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<sup>15</sup> Compare these attributes to the less positive comments dedicated to PARSONS and MERTON of whom it is written that they: “played ‘theoretical capitalist’ to ‘the mass of ‘proletariat testers’” (GLASER & STRAUSS 1967, p.10).

cally and professionally. The notions of *core category*, *memoing* and *in vivo codes* were the more practiced, within a new mood favorable to the generation of concepts from data. The latter (CAQDAS) provided a sensation of more systematization, and visibility of an artisan-like process always wrapped up in a magic halo which had been less tangible and showable. Anyway, the real process of analysis, interpretation and formal presentation of materials was a mix of hand-made and machine-made (computer assisted) activities.

My enthusiasm for the better hardware and software tools available cyclically (every two or four years) reached a maximum near the end of 2001. From then on, I have started to feel more sceptical about the promises of the software, and more aware of the methodological nature of GTM. Certainly, the writings of GLASER (1998, 2001, 2002, 2003) have been decisive for my understanding of my present position in a debate referred to in more detail in the following pages. In my case, the debate about the effects of technology on a class of research methodology such as GTM has driven my rediscovery of the work of GLASER and STRAUSS.

### 2.3 On becoming aware and practitioner of GTM

While writing this article I feel more certain that *GTM roots* is the *core category* of my contribution to this HSR Supplement. Moreover, the whole paper is in itself an attempt at practicing GTM. At the same time, I try to gain awareness of GTM intellectual roots together with the process of reception in my case.

Here is an example of how, in the process of “developing the category” *GTM roots*, I find another related category named *methodological awareness*. In the preceding pages my argument was that, following reflections on my personal GTM reception, the need to study GTM roots has emerged as key. Through this I became more and more methodologically aware of the nature<sup>16</sup> of GTM. Now, my complementary argument is that a similar process of becoming aware of GTM can be traced in the writings of GLASER and STRAUSS. I shall give a piece of documentary data to ground my argumentation.

This and the previous paragraphs are part of a *memo* I struggle to classify. More important here, in order to show the backstage process of my writing, is the fact that I was rereading the conversational interview of Anselm Strauss with Heiner LEGEWIE and Barbara SCHERVIER-LEGEWIE published in

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<sup>16</sup> Basically (and from a historical social research point of view) the nature of GTM is related to at least three lines of research: its roots or genesis (intellectual or academic influences of GLASER and STRAUSS, but also their more personal and professional experience), its versions and later development (GLASER’s version, STRAUSS’s version, mainly) and its reception or use by other researchers. See also footnotes 27 and 31 for a definition of the nature of GTM from a more methodological and technical point of view.

2004<sup>17</sup> (although the original date of the interview is 1994). I stopped the reading because I felt the need to write a memo entitled *GTM roots as core category*. The rest of the manuscript was:

The finding of a key relation between GT Roots and methodological awareness, both in my case and in the case of Strauss, is rewarding. I try to develop the two categories, so as to reveal more explicitly the processes implied, as well as the nature (genesis and development) of GT, etcetera. And, in sum, we can expect the achievement of more theoretical integration if this substantive theory is related to and compared with other theoretical efforts focused on the process of socialization (e.g., becoming experts) and of assuming identities. Also with the theory on awareness contexts; that is, a theory on awareness of identity and social interaction. Do not forget the triangulation micro-macro referred to in Glaser & Strauss article of 1964 [Memo, 5/4/06].

These notes had come up while reading the life story of STRAUSS (written in 1994), about his abandonment of University life in 1958 and his dedication to field work in the psychiatric wards of a big hospital (Michael Reese Hospital). He refers first to the collaboration with Howard BECKER and Blanche GEER in the *Boys in White* study. Then he says that he started extensive research on psychiatric institutions, with some of his students (BUCHER, SCHATZMAN and others) published in 1961 with the title *Psychiatric Ideologies and Institutions*<sup>18</sup>. Recalling this research experience in 1994, STRAUSS refers to it as an incipient practice of *theoretical sampling* and *grounded theory* in fact, although without the *methodological awareness* that will be finally explicit and formalized with Barney GLASER a few years later. Here is a piece of the documentary material<sup>19</sup>:

[...] I was starting this study [On psychiatric wards]. With two of my students: Rue Bucher and Leonard Schatzman [...] And I was looking to a grounded theory without knowing it. What we were doing was studying two hospitals. Each hospital had different departments. And we were comparing all of them. And we also compared psychiatrists with three different points of view, and nurses [...] lay people [...]. So I was beginning to do grounded theory without knowing it.

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<sup>17</sup> The full text of the interview is available both in Spanish and in German. See: Volume 5, No. 3, Art. 22 – September 2004 of *FQS*. The audio files of the original interview in English are also available at: <http://www.qualitative-research.net/fqs-texte/3-04/04-3-22-e.htm>.

<sup>18</sup> This work is the one assessed following the criteria presented in GLASER and STRAUSS (1967), chapter VI. The following evaluation comment is eloquent: “The monograph seems deficient principally in its integration of theory. Although in its concluding chapter, a number of related propositions are developed and discussed – which probably add to the reader’s sense of integration as well as contributing to the actual integration – an examination of the volume shows that much more integration could have been achieved had the investigators been more aware of the need for it” (p.158).

<sup>19</sup> The verbatim is taken from the audio file number 5 available at: <http://www.qualitative-research.net/fqs-texte/3-04/04-3-22-e.htm#g5>. The words of the interviewer, Heiner LEGEWIE, are within the special brackets [].

In the same interview, STRAUSS refers also to *Awareness of Dying*, published together with GLASER in 1965, and considered to be the immediate research antecedent of their 1967 book: *The Discovery of Grounded Theory*. He adds: “So we did the study together and in the course of doing the study we developed the methodology of grounded theory”<sup>20</sup>. But, notice the next amendment: “I was already doing it, but had no names for it”. STRAUSS points out that GLASER’s training with LAZARUSFELD was decisive when they faced the task of naming and conceptualizing their procedure. So it seems reasonable to affirm, from the interview of LEGEWIE and SCHERVIER-LEGEWIE, that the STRAUSSian GTM has strong roots in field research<sup>21</sup>. STRAUSS says that in this type of research “one must focus on the processes developing along time and one should also show the diverse perspectives and actions in which the actors are involved” (LEGEWIE & SCHERVIER-LEGEWIE 2004, para. 39). Something which corresponds strongly with GLASER’s emphasis on processes, especially in his sole-authored methodological works.

Summing up, the autobiographical narrative registered by LEGEWIE and SCHERVIER-LEGEWIE in 1994 offers a first answer to the initial question posed here: what does GTM mean?. That is, GTM can be understood as something intuitively practiced before GTM is coined by GLASER. I decided to go on analyzing (line by line) the interview material referred to, in which I understand one can find summarized the GTM version of STRAUSS. The category *GTM roots* is still the main one, because beyond the versions of the first coiners and experts or the versions of next practitioners (either followers or critics), my approach is that GTM is something that has a history, that has evolved.

I realize that my inquiry around the so called category *GTM roots* has been biased towards one of its forms: *intellectual*<sup>22</sup> *GTM roots*. Now, thanks to the documentary interview material provided by *FQS*, I can see the existence of *personal and professional experience GTM roots*. Another form of *GTM roots*: STRAUSS narrates how in 1960 he settled down in San Francisco, beginning to work at the Medical School, and discovered via observation “that people had great problems facing the dying in hospitals”. So he decides to start doing a study on dying in hospital, something he judges to be of interest both to doctors and other health personnel. Moreover, and after commenting that “six months

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<sup>20</sup> Quotation from the interview record available at: <<http://www.qualitative-research.net/fqs-texte/3-04/04-3-22-e.htm>>. Open audio file 6. Start: 1:49:900.

<sup>21</sup> It is worth remembering, at this point, that STRAUSS’s main training is in sociology and anthropology. We should also not forget the methodological work published in 1973 by SCHATZMAN and STRAUSS, *Field Research*, where they illustrate with numerous references to their research in hospitals their style of inquiry, both sociological and anthropological. Their system of field notes is reminiscent of the *memoing* in GTM.

<sup>22</sup> By *intellectual GTM roots* I mean the academic influences that help GLASER and STRAUSS give birth to their grounded theory proposal. That is, the influence of SIMMEL, PARK and other scholars.

later” he could count on the assistance of Barney GLASER, he confesses: “the two of us had had biographical experiences with death”. He refers to the death of his mother, and of GLASER’s father; focusing especially on the pretense contexts and reactions of their relatives, and even of their own, when confronting something inevitable (LEGEWIE & SCHERVIER-LEGEWIE 2004, para. 43)<sup>23</sup>.

The 1994 interview with STRAUSS, by LEGEWIE and SCHERVIER-LEGEWIE (2004, para. 59, 61-62), provides two more relevant elements for defining GTM:

- something that may be synthesized in three main methodological imperatives<sup>24</sup>;
- something that may be practiced following either fully extended or abridged forms or procedures.

It is important to note the emphasis of STRAUSS, throughout the interview, on the nature of GTM. He remarks that we are faced with “a methodology and a style of researching and analyzing social phenomena” rather than just a mere method. He ends up revealing the partial visibility of that research style in his case, together with the theoretical and methodological roots. LEGEWIE and SCHERVIER-LEGEWIE (2004, para. 58) registered this extract in their conversation with Strauss. “It could be said that my work has only made explicit some fragments of that style and that I did its development starting from my needs as interactionist and field researcher”.

My GTM research practice on GTM itself does not end here. The reader has been told of the processual and partial character of our reception of GTM. I have affirmed that the methodological principles of GTM are prior to this approach; and I have also pointed out the existence of both academic and personal experience roots in its explicitation and genesis. The generation of theoretical concepts is the final aim of the coding tasks and sampling decisions, precisely because the researcher attempts to go from mere description to conceptualization. Here we also try to reach the conceptual level, practicing a sort

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<sup>23</sup> “It may be said that that experience of ‘waiting expectant to death’ served as ‘sensitizing concept’ when entering this field” (STRAUSS interviewed by LEGEWIE & SCHERVIER-LEGEWIE 2004, para. 43). STRAUSS admits also that he included his own experience (as patient) in two studies (STRAUSS & GLASER 1975; STRAUSS & CORBIN 1988). And confesses: “I always took advantage of my own experience to check the cases of chronic illnesses we were analyzing” (STRAUSS interviewed by LEGEWIE & SCHERVIER-LEGEWIE, 2004, para. 85).

<sup>24</sup> STRAUSS underlines three basic aspects of GTM: 1) theoretical coding; 2) theoretical sampling; 3) comparisons (“done in relation to phenomena and contexts, and from which theoretical concepts result”). In my opinion, comparison is the medullar aspect element of GTM, as has been expressed in section 2.2. Elements 1 and 2 may be nested in 3, as has been indicated above. Notice that the referred comparison is oriented towards the generation of theory, and so are the sampling and coding. This is something that has been emphasized by GLASER in his works of 1998 and 2001, subtitled: *From Description to Conceptualization*.

of sociology of methodology. That is why in the course of the elaboration of this paper, a concept (*awareness existential contexts*) has emerged while re-reading the 1964 article by GLASER and STRAUSS, *Awareness Contexts and Social Interaction*.

I have added “existential” to the original expression (“awareness context”<sup>25</sup>) in order to try and link with my own thread of inquiry; that is, my recent methodological research (VALLES 2005a, 2005b). In both writings, I start including brushstrokes of existential context (macro and micro). This is not only the result of GTM influence, but also of my preference for the biographical method and the influence of other methodologists (MARSAL, De MIGUEL, IBÁÑEZ, BERTAUX). In the present article, I try to provide some existential contextual elements that help understand the co-occurring processes of: *GTM reception*; *technological transition* (going digital) in the practice of qualitative analysis; and becoming a qualitative researcher. This involves not only the co-occurring processes of *GTM reception*, but also the *GTM roots* processes. The latter may be understood as implied in the concept *existential context*, both at the macro and micro level. I mean, for instance in the case of GLASER and STRAUSS, the roaring or swinging sixties in USA and Europe universities, as a socio-historical moment together with the professional and personal biography from which the macro-situation is watched.

Here is a piece of data that reflects the *existential context* in which GTM was born. STRAUSS remembers the student movement of 1968 as a protest against capitalism and the upsurge of neoMarxism in sociology, the rediscovery of interactionism, and the blossoming of ethnomethodology and phenomenology (LEGEWIE & SCHERVIER-LEGEWIE 2004, para. 49). STRAUSS adds that, before publishing the 1967 book, they already had “the intuition that people longed for renewal”; and decided to write “for the young people”. He reveals that the title itself, *The Discovery of Ground Theory*, indicated what they aimed at: “not, as was usual in other methods books, the verification of theory, but the discovery of emergent theories from data”. So he finally concludes: “Grounded theory is not a theory, but a methodology to discover theories snoozing in the data”.

In some way, we could talk of another type of *GTM roots*; that is: *GTM contextual roots*. STRAUSS gives three reasons for the birth of GTM: 1) legitimize qualitative research; 2) “attack functionalists like Parsons or Merton”<sup>26</sup>; 3) “clarify the possibility of developing a theory taking the data as the point of departure”<sup>27</sup>.

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<sup>25</sup> See GLASER and STRAUSS (1964, pp.670-671) for a specific and general definition of *awareness context*. In this reference one can also find the relation between types of contexts of awareness of identity and interaction, and the theoretical work of George H. MEAD on the one hand and GOFFMAN's on the other.

<sup>26</sup> In the conversational interview with LEGEWIE and SCHERVIER-LEGEWIE (2004, para. 52), STRAUSS affirms that GLASER and himself wanted to revolt against the “transmitted

Apart from these declared reasons, we want to leave open here the door to future research efforts on other reasons<sup>28</sup>. The re-reading of GLASER and STRAUSS's 1964 article has provided some material in which to ground our working hypothesis and interpretation. One piece of documentary base is the relation among contexts of "awareness of identity and interaction" and the work of relevant scholars. George H. MEAD and "open awareness context"; Erving GOFFMAN and "closed and pretence awareness contexts". The authors of *The Discovery of Grounded Theory*<sup>29</sup> had already criticized GOFFMAN's *Presentation of Self* for its "restricted range of contexts" and the lack of interest towards the transformation processes from one type of context to the others. They had, instead, called attention to MEAD's emphasis on "the developmental properties of interaction" and "his processual, rather than substantial treatment of the self" (GLASER & STRAUSS 1964, p.674). These authors also warn us about the tendency of GOFFMAN "to leave implicit the structural conditions imposed by the larger social units".

It is pertinent here to leave room for these elaborated criticisms because it may help to make explicit the fairly alternative or complementary analytic approach of GLASER and STRAUSS. They claim their methodological proposal to be more sociological, and a way of connecting the micro and macro levels that a social researcher needs to balance.

The focus on structural conditions increases the likelihood that the microscopic analysis of interaction will take into account the nature of the larger social structure within which it occurs. The usual structural approach in sociology tends to neglect microscopic analysis of interaction and also inhibits attention to its developmental character. Our paradigm encompasses in one developmental scheme the twin, but often divorced, sociological concerns with social structure and social interaction. Neither<sup>30</sup> need be slighted, or forgotten, for a focus on the other (GLASER & STRAUSS 1964, p.678).

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theories". And that was the reason for a book which was so "aggressive, activist" in its orientation

<sup>27</sup> In the interview referred to, STRAUSS remarks: "Nowadays we can still find qualitative researchers opposing to it. Many of them are pleased with ethnographic descriptions of the old Chicago School. And the postmodern think that it does not even make sense to develop systematic theories". Notice the allusion to the two Chicago schools: the old and more anthropological; the second, more sociological, theoretical or conceptual.

<sup>28</sup> Let me remind the reader that one of the quality criteria considered specific to the nature of GT is *modifiability* (GLASER, 1978, p.142). *Fit*, *workability* and *relevance* are the other three, according to the reference cited and the more recent work of GLASER (2001), although this has been a matter of dispute which we have dealt with in another place (VALLES 2005b).

<sup>29</sup> Anselm STRAUSS shares with GOFFMAN the same mentor, Everett HUGHES, and the same field of research: psychiatric wards.

<sup>30</sup> Thirty years later, in the 1994 interview with LEGEWIE and SCHERVIER-LEGEWIE (2004, para. 36), STRAUSS recalls that the idea of a connection between the micro and macro levels of analysis had been the steering thread of his 1959 book *Mirrors and Masks:*

The interested reader may find many other related references to GOFFMAN in the 1967 book by GLASER and STRAUSS. See, for example, chapters III (Theoretical Sampling) and VI (Clarifying and Assessing Comparative Studies). The mentioned “restricted range of contexts” becomes now “circumstantial sampling”, and the new proposal to overcome its deficiencies is named *theoretical sampling*.

### 3. Rediscovering GTM while Doing Research on the GTM and CAQDAS Debate

While experiencing a series of peaks of enthusiasm and valleys of apathy in relation to the use of computers in qualitative research, I decided to face the issue of pros and cons of using CAQDAS when totally or partially following GTM. At first, and from a position of being a technophile, my search was oriented towards the advantages and disadvantages of specific programs (NVivo, Atlas.ti, MaxQDA, ...).<sup>31</sup>

I must admit the fascination I felt when trying the first Windows version of Atlas.ti and NVivo, delivered on a compact disk (in 1997 and 1999 respectively). Both programs included audiovisual recordings about the way some routines worked. But NVivo also had a series of elaborated tutorials for self-learning. Perhaps the just mentioned fascination was due, in a greater part, to my previous experience with some NUDIST versions, when the hegemonic operating system was MS-DOS.<sup>32</sup>

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*The Search for Identity*. The same year of GOFFMAN's famous work, but much less known.

<sup>31</sup> In 1999 I had elaborated an invited contribution in a Spanish methods handbook that was published in 2000 with the title: *Grounded Theory and Computer Assisted Qualitative Analysis*. Previously, I had written a textbook on qualitative research (VALLES, 1997) in which I echoed the works of TESCH (1990) and WEITZMAN and MILES (1995).

<sup>32</sup> Maybe, my fascination was related also to previous knowledge of TEXTPACK (software designed to help in the classical content analysis approach to textual material). It had been used in a summer school course on *Textual Analysis*, at the University of Essex, by Ekkehard MOCHMANN. Or, perhaps the origin of my enthusiasm should also be traced in the lack of a PC during the typing (with a typewriter) of my dissertation in 1987-88.



### 3.1 In search of the conceptual trace of a more comprehensive debate

The influence of GTM in my research practice is summarized in this heading. I abandoned the pretension of a merely descriptive approach and decided to select a few but relevant conceptual contributions, trying to generate my own conceptual contribution (more detailed results are in VALLES 2005a, pp.148-151). It is sufficient to annotate here, as an example, the concept *dual impact* proposed by GIBBS, FRIESE and MANGABEIRA (2002). They deal with the impact of new technology on qualitative research, within a future panorama of *digital convergence*.<sup>33</sup> To be precise, their statement of the effects on the final analysis and also on the recording and collection of information coincided with my more ample phrasing: the relation of technology (not only software) and methodology (around the whole process of research) as the background issue.

My own conceptual contribution about qualitative research and the irruption of PC software in the case of Spain was summarized in two expressions: *methodological tradition* and *technological transition*. The former referred to the 1967 proposal of GLASER and STRAUSS, but it was the second concept that was the main focus of attention (the *core category*). I wanted to name the process of evolving from a situation of a hegemony of manual styles of research to another of a general use of technological resources (hardware and software).<sup>34</sup>

The concept *technological transition* (associated with *demographic transition* in my *curriculum vitae*) had various advantages. On the one hand, it underlined the processual nature of the phenomenon under study. On the other hand, and in a similar vein to the well-known population theory, it was expected that it worked as a *paradigm* (KUHN 1962) promoting empirical research. Applying this foresight to the case I was studying, I planned the following lines of research enquiry: in what phase of the technological transition is the Spanish case? What are the patterns of adoption and use of new technologies by qualitative social researchers in Spain? What processes of reception and adaptation were taking place?

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<sup>33</sup> I find the statement of these authors clearer than the writings of FIELDING and LEE (1998) or FIELDING (2000), although there are numerous conceptual contributions resulting from the CAQDAS Networking Project (<http://caqdas.soc.surrey.ac.uk/>). For example, the distinction of users and patterns of adoption in the use of qualitative software, the generational and school of thought issues as other causes, as well as the technological factor.

<sup>34</sup> A history of social research practice in relation to technological phases remains to be done. A recent contribution is the study "Recording Technologies and the Interview in Sociology, 1920-2000", by Raymond M. LEE, inaugural lecture for the course 2002-03 at Royal Holloway, University of London, Surrey.

### 3.2 Generating typologies of basic cases and processes to give context to GLASER's posture

When microcomputer tools arrived in our lives in the 1980s, one could observe a favourable feeling among social researchers with a busy past calculating all sorts of percentages. Even if the use of a PC (personal computer) was merely to type hand-written papers with a word processor, there was a receptive atmosphere. However, at a later date, such as 1998, one could find just published the question: "Why use computers in qualitative research?". This was the title given to chapter 3 by FIELDING and LEE (1998). They tackled "the assessment of CAQDAS advantages". Under this heading the most elaborated conceptual approach of these authors was presented: their processual perspective. There are neither advantages nor disadvantages in an abstract sense, removed from the historical and biographical circumstance of the researcher. Both the software and the user keep on changing. So the elaboration of a special typology of users and software needed to consider the *researcher generation* and the *software generation* (FIELDING & LEE 1998; FIELDING 2000; GIBBS, FRIESE & MANGABEIRA 2002).

I decided to elaborate further my category *technological transition* and use it in the generation of a typology focused on basic social processes and types of analysts.<sup>35</sup> Definitively, I oriented my inquiry not towards an upgrade of advantages and disadvantages but to the analysis of discursive postures: favorable, against, or doubting in the context of users with more or less experience, methodologists, technologists or mixed types. My intention with the latter was to contextualize GLASER's posture.

Table 1: Typology I: Basic *Types* of Analysts and Technological Transition Processes According To Their Past And Present Experiences Of Qualitative Research

Main Research Practice		At Present	
		<i>Manual Modes</i>	<i>Techno assisted Modes</i>
In The Past	<i>Techno assisted Modes</i>	Techno-repentant <b>Inverse Transition</b>	Technoavant-garde <b>Posttransition</b>
	<i>Manual Modes</i>	Technophobes <b>Pretransition</b>	Technoenthusiasts <b>Technotransition</b>

<sup>35</sup> I believe either the writings of LEE and FIELDING (1991, 1995) or FIELDING and LEE (2002) lack a more elaborated definition of a typology of qualitative software users. Nevertheless, in the paper presented by Ray LEE in the First Seminar on Computer Assisted Advance Qualitative Research (at Granada, November 2001, organized by Centra), he suggested the distinction of three types: *naïve adopters*, *old hands* and *craft users*.

In a first typology (see table 1) I tried to generate a basic theoretical sample and at the same time a formal theory, grounded both in personal and professional experience and other cases reported in the literature.<sup>36</sup>

My observations of different generations of Spanish social researchers, together with the knowledge of other analysts from abroad, contributed to a dynamic view of this typology. That's why I combined the double biographical research experience: past and present. Moreover, I understood that the future dimension was implicit too, as well as the more detailed situations of cases in between the cells.

Although in the elaboration of this typology I must acknowledge the influence of GTM (especially the recent writings of GLASER), there are also other roots. On the one hand, I have been influenced (from 1985 till today) by a Spanish sociologist, Amando de MIGUEL, who studied with LAZARSELD in the late 1950s and early 1960s. On the other hand, there is the lesson learned from TESCH (1990): not to neglect the intense analysis centered on the case and the process, in favour of an excessive attention to the analysis centered on the variables or the relationships among conceptual categories. This is an issue noted and published in VALLES (1997, pp.393-400).

Following the indications of the editors of this special volume, I obviate a detailed exposition of results (VALLES 2005a). However, let me remark that the concept *technological transition* was again the *core category* (in the sense given to this expression in GTM) in another typological proposal included in that article. In any case, I warned of the existence of generational variants in all the cases considered. This brought to fruition my preference for biographical methodology, combined with the received version of GTM in my research practice.<sup>37</sup>

The predominance of the biographical approach in my academic trajectory (reinforced at present) has made me think about the convenience of studying the processes of reception (of methodologies and technologies) in a longitudinal, historical-biographical and generational way. At the same time, the influence of GTM (especially its insistence in *theoretical sampling* and the generation of *substantive* and *formal theory*) has come to reinforce the elaboration of types (cases, processes) and their corresponding comparison. But one has to

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<sup>36</sup> There is an ample bibliography on the debate around the risks or advantages of using technological assistance in qualitative research (in general, not only GTM). See a sample of references in VALLES (2001). I want to make special mention of the studies by LEE and FIELDING (1995) and FIELDING and LEE (1998, 2000) on the experiences of software users in UK.

<sup>37</sup> For example, considering my case, I have experienced moments of enthusiasm and technorepentance. The former corresponds to the years 1997-2001, when I acquired familiarity with the software versions available. I collaborated also in the reception of that software in my university, both in the area of research and teaching. From 2001 till today I have felt more technosceptical, and at the same time I made efforts to understand the posture represented by GLASER.

add the sociological training at the base, as illustrated in the following quotation taken from research on GTM and CAQDAS.

Notwithstanding the limitations<sup>38</sup>, the works by Fielding and Lee are extraordinarily useful. Their preferential attention for the experiences of *novice* users means these methodologists adopt a posture of relative equidistance in relation to the more interested technoavant-garde or technoenthusiasts (software developers, software sellers and trainers) as well as in relation to technophobes or technosceptical that see their craftsmanship or their position in the knowledge and acknowledgement market at risk (VALLES 2005a, p.156).

In the research presented here to illustrate how GTM worked when studying the relation between GTM and CAQDAS, I opted (as the next step) to focus on the polar positions. Once again, I must say that in making this sampling decision I was influenced by my reception of GTM. Conceptual progress was taken as a priority over descriptive coverage, although from my approach to semiotics and discourse analysis I had also learned that it is sufficient to feel the pulse of the discourse of an extreme position in order to acquire qualitative material of the confronted positions. I decided to gather material for the case of people who were more favorable to the fusion of technology with methodology. So I completed the prolegomena of the presentation of GLASER's case, as representative of the more reticent to the reception of new technologies by qualitative researchers, particularly by GTM practitioners.

I found very interesting material in a special issue of the recently published *Qualitative Research Journal*, in 2003.<sup>39</sup> They had reunited the papers of a conference centered only on the software developed by the firm QSR (N6 and NVivo), to avoid – they said – “focusing on software characteristics”, instead of “questions of research and teaching”.<sup>40</sup> One of the speakers, Chris THORN, Director of Technical Services at the Wisconsin Centre for Education Research, gave a grounded critique of the traditional positions and research practices observed.

It has been my experience (and I have discussed this with others who use and develop analytical software) that university-level qualitative methods instructors are often indifferent to (or even hostile towards) qualitative analysis software. It is seen as too complicated and ‘getting in the way of the analysis’. I have heard concerns that software packages ‘lead to premature closure’ or ‘distance one too much from one’s data’ – despite the fact that the speaker is

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<sup>38</sup> The weakest side of the sample, admitted by the authors (FIELDING & LEE 1998, p.3) is that the users were in their greater part “early adopters of software” and the programs had become easier to use meanwhile. That is, there is lack of sufficient information to compare the processes and cases of *technological transition* and the range of postures represented by us here in table 1.

<sup>39</sup> The electronic document was consulted in 2004, at: <<http://www.latrobe.edu.au/aqr/>> .

<sup>40</sup> My participation, in 2001, in a Seminar focused on various CAQDAS and GTM, in Granada (Spain), gave me a chance to observe interesting critiques of software developers (specifically the lack of transferability among the programs).

likely to have had little or no experience with the software in question (THORN 2003, p.7).

The contrast of the two extreme, “conflicting and unarticulated”, approaches to the computer as epitome of technology appears also in the editorial of the mentioned Journal. This opposition expresses itself in terms of fear and mysticism. Fear that the machine makes the researcher look silly; or that the computer mechanizes the process of analysis, leaving aside the reflection and interpretation of qualitative material. In contrast, it identifies “a mystical belief that the computer will churn out the analysis”. In order to overcome this bifurcation, similar to the technophobia and technophile postures drawn here in table 1, they propose a third via or intermediate position. That is, neither technological terror nor mystical reverence. They admit the possibility of an inadequate use of present-day technological resources, but are also certain of its utility to think and execute tasks which were less possible in the past.

### 3.3 GLASER’s position as epicenter of the debate on methodology (GTM) and technology (CAQDAS, taping, typing, ...)

In this sampling decision, conceptually conducted, I have been influenced both by GTM principles and by the more universal and previous *case study* method or *field* method. STRAUSS and CORBIN (1994) made clear they preferred *theoretical density* instead of GEERTZ’s *thick description*. And GLASER, in his recent writings, insists once again on the need to generate concepts instead of just searching for a descriptive coverage. In either case, the point of convergence in this apparent disparity seems to be in the more intensive study of a few cases, strategically chosen (for their relevance or pertinence in relation to our research purposes).

Our reasons for an approach centered on GLASER may be summarized as follows: GLASER’s methodological writings (1978, 1992, 1998, 2001, 2002) constitute a countercurrent to the attraction towards new technologies experienced by some social researchers in the past twenty years. Moreover, his attitude may be seen as paradoxical because the methodological tradition of GT (GLASER & STRAUSS 1967) has been invoked as a methodological foundation by the developers of software designed to assist qualitative analysis (it is sufficient to mention the programs Atlas.ti, NVivo or MaxQDA here in this respect).

It is, in sum, the result of a sampling operation based on documentary material and even supported by the lived experience. I’ll provide some autobiographical data in support of this latter aspect. In the Seminar on Computer Assisted Qualitative Research to which I referred earlier, held in Granada in November 2001, the organizers invited GLASER. But the international terrorism of September 11<sup>th</sup> 2001 modified the planned schedule and changed GLASER’s participation to a telephone conference. The meeting had reunited

CAQDAS users, developers and methodologists with an interest in GTM. GLASER was expected to deal with the relation of GTM-CAQDAS. His intervention provoked a vivid debate and replies from the software developers that were present. Although I had no program to defend I felt GLASER's words cooled the warm and technophile atmosphere of the previous sessions. I put the next comment and question to GLASER to summarize his position: "I have only noted negative aspects about the use of computers if the researcher wants to generate theory. Is there any positive aspect, any advantage?". GLASER's answer was concise: "As word processors only, because they inhibit grounded theory. They are not necessary".

These and other fragments of GLASER's oral discourse were completed and contextualized afterwards through revising his written work (VALLES 2005a, 2005b). GLASER himself invited the audience to consult his just published book (GLASER 2001) and some previous ones (GLASER 1978, 1992, 1998).<sup>41</sup> The reading and re-reading of this methodological work helps us to understand better the reasons for the aspects confronted in this debate. As we have already noted in the preceding paragraphs the analysis of one side of the debate reveals the opposite side. The outbreak of CAQDAS has pushed GLASER to underline the limits of a "genuine orthodox GT" (GLASER 2001, p.3). In his attempt, the comparison of GT perspective with the more general one called QDA (qualitative data analysis) is key to understanding the reluctant position (methodologically grounded) of this author against technological assistance.

In his brown book (GLASER 2001) the author warns about the variety of qualitative perspectives and differentiates GT from other modes, opposition to generalization-description being the main difference. He states: "GT exists on a conceptual level and is composed of integrated hypotheses and QDA methods<sup>42</sup> produce description with or without conceptual description mixed in" (GLASER 2001, p.2). Moreover, he affirms that there is a process of "adoption, adaptation, cooptation and corruption" of GTM in such QDA forms. The singularity of his methodological proposal points, once and again, to the generation of concepts and their interrelation via hypotheses. Abstraction with empirical roots is confronted against a description which is too much concerned with the number of cases and the *verbatim* of participants or informants. GLASER's moderately alternative approach is grounded in its *transcendental* character: "GT transcends the time, place and people of any and all units sampled and conceptually generates the fundamental patterns yielding hypotheses

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<sup>41</sup> In my case, I felt motivated to read or re-read his work and adopt or reinforce a position of greater scepticism in relation to CAQDAS. My place in table 1 went from the enthusiastic cells to the more sceptical ones.

<sup>42</sup> The variety of QDA methods that GLASER considers as different to GTM are both "positivistic" and "social constructionist and interpretive QDA methods". He considers all of them a threat to GTM, because of the risk of "cooptation and corruption"; of "confusion and fusion".

which can explain the behavior of the participants as they go through the patterns” (GLASER 2001, p.5).

In order to make more explicit GLASER’s reasons for his reluctant position towards the use of CAQDAS, one should consult his writings on *theoretical memos* of 1998, which were an attempt to update his work of 1978. Here we can see that his posture against software is concretely against such programs as NUDIST.<sup>43</sup> In 1998 NVivo had not been launched yet, and when its developers launched it in March 1999, it was with a series of improvements in the *memoing* possibilities. We consider these details important because they help us to understand both GLASER’s position and the opposite one. As an analyst one must compare both parties in relation to the same software and in the same period of time. For instance, GLASER (1998, p.178) argues that there are restrictions imposed by the software when the researcher tries to annotate ideas in a “free, flexible and fluid” way (*memoing*).<sup>44</sup> He finally expresses the requirement that a specific software should be designed by someone with experience in GTM (GLASER 1998, pp.185-186).

The alternative approach is that software developers are entitled to ask for an assessment of their products from an expert user. So I made the sampling decision to look for an assessment produced by a ‘double’ expert – someone with expertise in both the technology and methodology being considered. We found Silvana di GREGORIO, with qualitative research experience in a pre-computer epoch and software expertise in the present. She has published her *testimonio* on some potential improvements of NVivo in relation to GTM, paying special attention to the memoing feature in NVivo (Di GREGORIO 2003). She affirms that “while the memo possibilities are rich in NVivo, the memoing feature is somehow ‘hidden’ compared to coding features”. We share this assessment and add that it is fairly perceptible if one compares the same features in Atlas.ti, for example.<sup>45</sup> Now it is easier to understand GLASER’s reluctance towards the versions of NUDIST available before 1998 with their more rudimentary *memoing* features.

Although one may think that there is plenty of room to improve the mentioned features in future updates, the main obstacle continues to be the researcher’s training in methods and techniques. The importance of *memoing* in

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<sup>43</sup> GLASER (1998, p.105) reveals that he had also tried, in the 60s, Mac Bee and indexing cards. His opinion about these and other mechanic resources of coding had been published. That is: they are adequate for the *arithmetic concept-indicator model*, but not for the *constant comparative concept-indicator model* (GLASER 1978, pp.71-72).

<sup>44</sup> The taking of notes and its coding is a key task in GTM. GLASER (1978; and together with STRAUSS 1967) had already revealed his pre-computer technology (paper cards, carbon paper and scissors...). He still prefers that to the computer options of the 60s and the present-day ones. It is clear that he knows NUDIST, but not the grade of his expertise.

<sup>45</sup> I did the test of writing a paper using NVivo. It is easy to get lost in the ever growing number of codes which are so easy to create, unless you work with a scheme as a complementary document.

GTM is something that some software users seem to forget or ignore. We particularly think of analysts who avoid reading the material under study and try to get, as soon as possible, a file readable using quantitative analysis programs.

Finally, while studying GLASER's case, I discovered that his reticent position towards technological aids included *taping* and *typing*. This extension of the category *technology* emerged in the course of our re-reading of GLASER's writings. Here is one of the fragments that triggered our attention. Notice the relation, established by the author, between "technological traps" and the activity of *memoing* as understood from a GTM perspective.

Technologies, especially computers, which produce inadvertently their own brand of ideational product by their constraints, are an easy cop out on the full power of memoing [...]

But I am open to computerization if I can be shown, how constant comparing, how delimiting from interchangeability of indices, how saturation, how other delimiting, how latent patterned relevance, how theoretical completeness, [...] sampling [...], how full descriptive coverage can be stopped, how conceptual level can be maintained, how memo jots [...] can be inputted and sorted flexibly with no waste of time [...]

The constant deburdening and delimiting of the grounded theory process on its route to a theory, is lost by the copious collections and process of computers and tape recorders. Thus taping and computerization offer the safety net of technology, yielding a formulated product over a freer grounded theory analysis (GLASER 1998, pp.185-186).

While reading this series of quotations I remembered the positive role played by recording technologies in relation to research techniques since Carl ROGERS decided to record his therapeutic sessions. From this stand point I considered GLASER's position rather unjust. But, on the other hand, I tried to understand GLASER's reasons. I found that this author was not pretending to talk only of qualitative analysis style. There was his expression "grounded theory package", qualifying a particular perspective that requires more "flexibility and freedom" than the common qualitative research. Once again we realized that the emphasis was on the purpose of increasing the *emergent* instead of the *preconceived*.

At this point of the research process concerning both a general debate (methodology-technology) and a particular focus (GTM-CAQDAS and GLASER's case), I had the feeling of having reached a degree of saturation sufficient to write a paper.<sup>46</sup> In other words, I had something to communicate to my colleagues interested in social sciences methodology. Precisely whilst writing the drafts of the paper I was clarifying and ordering the findings that emerged along the research road. My research practice was not pure GTM but it was influenced by it. This influence came from the writings of GLASER, but also from the published works of many other authors (STRAUSS, CORBIN,

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<sup>46</sup> Something that I really did in the VIII. Spanish Congress of Sociology, in 2004, at Alicante.



CHARMAZ, GIBBS, DEY, Di GREGORIO, FIELDING and LEE, LONKILA ...). The main contribution of the present paper is the effort to make explicit a process of becoming more aware of GTM roots and versions. In so doing a trace of a particular process of GTM and CAQDAS reception has emerged.

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