

Grounded Theory and Pragmatism: The Curious Case of Anselm Strauss

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Abstract: Sir Arthur CONAN DOYLE's stories featuring Sherlock Holmes are justly famous the world over. In *The Memoirs of Sherlock Holmes* (1993) one story entitled *Silver Blaze* contains an exchange between Holmes and a Scotland Yard detective as follows:

Gregory (Scotland Yard detective): "Is there any other point to which you would wish to draw my attention?"

Holmes: "To the curious incident of the dog in the night-time."

Gregory: "The dog did nothing in the night-time."

Holmes: "That was the curious incident."

In similar fashion I wish to draw attention to the curious case of Anselm STRAUSS: There is already a good deal of work pointing to the continuities between the Grounded Theory Method (GTM) and the Pragmatism of John DEWEY and Charles PEIRCE. This has usually focused on Anselm STRAUSS with his Chicago-influenced Pragmatist background, although STRAUSS himself never articulated the way in which Pragmatism informed or could be brought to bear on the method as it evolved from the 1960s onwards.

This paper argues that many of the contentious issues surrounding GTM can be resolved if they are understood against the context of some of the core tenets of Pragmatism, particularly the ways in which some of the more recent Pragmatists such as Richard RORTY have brought them back as a focus of attention. In so doing it raises the question of why, given his intellectual background and formation, Anselm STRAUSS did so little to bring Pragmatist ideas into GTM in its later embodiments and extended statements. That is the "curious incident" to which specific attention is drawn at several points in what follows; it remains a perplexing one, with perhaps no convincing solution, unlike the Sherlock Holmes mystery alluded to above.

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1. Issues and Aspects of Grounded Theory

1.1 The emergence and growth of GTM

It is widely acknowledged that one of the strengths of the early statements of the Grounded Theory Method (hereafter GTM—short for Grounded Theory Method) was the diverse backgrounds of the two originators—Anselm STRAUSS and Barney GLASER. This has usually been characterised in the following manner: STRAUSS hailed from the Chicago School of sociology, with its stress on qualitative, ethnographic research; GLASER from the more quantitative orientation associated with Columbia, New York. The outcome was a method, aimed initially at qualitative studies, which sought to offer researchers a rigorous basis for producing and justifying genuine conceptual insights. As such STRAUSS' progenitors are usually listed as MEAD (1934), PARK (1952) and BLUMER (1969); GLASER's as LAZARFELD (LAZARFELD & ROSENBERG, 1955) and MERTON (1949). [1]

Yet there are problems in simply stating these biographical facts as if they were self-evidently explanatory of some of the key features and developments of the method itself. It must be stressed that the early statements of the method were never presented as some amalgam of the intellectual and methodological antecedents of GLASER and STRAUSS. On the contrary, the entire project was one of innovation, and a break with existing traditions and practices. As Jörg STRÜBING pointed out in comments on an early draft of this paper, *The Discovery of Grounded Theory* (GLASER & STRAUSS, 1967) [hereafter *Discovery*] was first and foremost a manifesto, seeking to present a genuine alternative to the dominant quantitative agenda of the time. As such it did not provide much by way of an historical account of the two progenitors. STRAUSS makes this very clear in his 1994 interview in which he recalls that one of the key motivations behind *Discovery* was the authors' desire to encourage young

researchers to break away from the influence of Parsonian functionalism (LEGEWIE & SCHERVIER-LEGEWIE, 1994). [2]

This does not explain, however, why the subsequent development of GTM in its first three decades is notable for the almost complete absence of any extended acknowledgement to either of the two traditions from which GLASER and STRAUSS had emerged. As has been argued elsewhere (BRYANT & CHARMAZ, in press) this is one of the paradoxes in the development of GTM, and might partly be explained by the initial need of GLASER and STRAUSS to stress the novelty of the method; then latterly by the divergence between the two founders, with each keen to characterise their specific orientation. [3]

GLASER's intellectual formation prior to his collaboration with STRAUSS was speedy, focused and short; yet highly important in the light of the development of GTM. He had studied as an undergraduate at Stanford, and continued his studies in France while serving in the military. In the late 1950s he enrolled as a doctoral student at Columbia, where Paul LAZARFELD was the presiding influence. LAZARFELD's work was notable for many reasons and in several fields; not only sociology, but also public opinion research and mass communication studies. Although sometimes regarded as primarily a quantitative researcher it is probably far more accurate to regard LAZARFELD's work as an extended and concerted effort to take account of both quantitative and qualitative research approaches; using the strengths of each to complement the other, and also pointing out the weaknesses of each. [4]

Initially, but only briefly, GLASER's mentor was R.K. MERTON; as GLASER recalled at one of his seminars, "I was a doctoral student, doing research on sociology of science ... for about one afternoon" (London GTM Seminar, 2006). His specific doctoral research took off in an entirely different direction, almost as if impelled by a motivation to react against the sort of sociological theorising that MERTON represented. The influence of LAZARFELD, however, was more far reaching, and to some extent GLASER might be considered as one of the key adherents and developers of LAZARFELD's methodological ideas. GLASER himself makes this clear in his recent book on *Quantitative GT* (2008) where he clarifies the ways in which LAZARFELD's ideas influenced and presaged many key aspects of GTM itself. [5]

Having completed his PhD in 1961, GLASER teamed up with STRAUSS, and Jeanne QUINT, and from that time on has been indelibly bound up with GTM. Some have wondered why STRAUSS did not work with people from Chicago, or others associated with the Chicago school. Although he does not mention the circumstances in which they met, STRAUSS does note that he was enormously impressed with Barney GLASER and so took him on as an associate without even requesting letters of recommendation (LEGEWIE & SCHERVIER-LEGEWIE 1994). In particular he pointed out that what he had been doing intuitively, GLASER was also doing, but in a more systematic manner that drew on LAZARFELD's ideas. [6]

At the start of their collaboration STRAUSS had a far longer, deeper and more profound relationship with his intellectual background. He was already an established academic and researcher before teaming up with GLASER in the 1960s, and even after the appearance of the three founding texts of GTM (GLASER & STRAUSS, 1965, 1967, 1968), he continued to work in areas other than those related to GTM. Moreover he also undertook teaching engagements in Germany where his influence remains strong, and extends beyond his GTM writings; encompassing his wider methodological work as well as his specifically interactionist studies, and other research into medical practices and medical sociology (STRAUSS, 1959, 1982a, 1982b, 1984; STRAUSS, FAGERHAUGH, SUCZEK & WIENER, 1985), all informed by his interest in sociology of work and organisation. In the 1994 interview, STRAUSS mentions the influence of many specific figures in his intellectual formation, including PARK, HUGHES, and BLUMER; in the introduction to *Qualitative Analysis* (STRAUSS, 1987), he refers to the "general thrust of American Pragmatism" (p.5), particularly DEWEY, but also PEIRCE and MEAD. [7]

So whereas GLASER's overall theoretical context and range of publications are devoted almost entirely to GTM, STRAUSS was always far more diverse, and his range of publications encompasses a far wider remit than GTM. Many of these are highly regarded, particularly in the German-speaking world. Thus *Mirrors and Masks* (STRAUSS, 1959), which appeared in the US around the same time as GOFFMAN's *The Presentation of Self in Everyday Life* (1959), is far more widely known in Germany than is GOFFMAN's work; the opposite of what is found in the US and UK. Similarly *Continual Permutations of Action* (hereafter *Continual*), STRAUSS' final published book (STRAUSS, 1993), is highly regarded in Germany, but merits few references or citations elsewhere. [8]

1.2 Two views of "basic" GTM

STRAUSS and GLASER later fell out with regard to GTM, particularly in 1992 when STRAUSS, together with Julie CORBIN, published *Basics of Qualitative Research* (1990) (hereafter *BQR*)¹. GLASER saw *BQR*, with some justification, as undermining the method, although STRAUSS and CORBIN clearly felt that there was an unmet demand for something akin to a GT methods manual; which reinforces the point made earlier about the early GTM texts themselves. In the 1994 interview (LEGEWIE & SCHERVIER-LEGEWIE, 2004) STRAUSS recounts how both *BQR* and the earlier *Qualitative Analysis for Social Scientists* (STRAUSS, 1987) (hereafter *QA*) came about. *QA* was based on recorded transcripts made by students who attended STRAUSS' workshops. The book itself, therefore, derived from work with students already partially versed in some aspects of GTM. The later book, *BQR*, written jointly with Juliet CORBIN, was explicitly aimed at students starting out on their research, with perhaps little or no idea about qualitative research as such, and certainly no knowledge of GTM. [9]

1 A second edition appeared in 1998, and a third edition in 2008 (CORBIN & STRAUSS, 2008).

Some people have wondered why GLASER's wrath was targeted at *BQR* and not at *QA*, as there are clear continuities between the two books. *QA* makes specific reference to GLASER's work, and STRAUSS clearly saw no problem in linking *QA* to a generic approach—not a single method—that encompassed their early joint work and their later separate writings: Indeed in his introduction STRAUSS extols GLASER's book *Theoretical Sensitivity* (GLASER, 1978), and acknowledges GLASER's permission to quote extensively from it. (This had consequences for the reception and dissemination of GTM particularly in Germany as STRAUSS did not use direct quotes and so readers were unable to distinguish between his words and GLASER's.) STRAUSS stressed that the second half of Chapter 1 "is essentially his [GLASER's] except for some amplification" (1987, p.xiv). No doubt, whereas STRAUSS probably saw no great distinction between *QA* and *BQR*, GLASER saw a complete break with the approach that had been developed from *Discovery*. As a response, GLASER produced his own book on the basics of GTM; *Basics of Grounded Theory Analysis (BGTA)* (GLASER, 1992). This was specifically not a book to explain the basics of the method to a novice, but more to reclaim them from what GLASER perceived as their misappropriation. [10]

There is a double irony here. First, although *BQR* was indeed an imperfect and potentially misleading statement of GTM (see below), it was undoubtedly highly influential in widening the appeal and claimed use of the method itself. Second, given STRAUSS' background in the sociological orientation of the Chicago School (often thought of as solely symbolic interaction, but which also encompassed aspects of Pragmatism), it is surprising that he never drew on this in any concerted manner in his GTM writings; least of all in *BQR* which can be all too easily read as a somewhat mechanistic GTM cook-book with little or no reference to the role of the researcher as an active participant in the research context itself. [11]

GLASER's critique was targeted at the way in which STRAUSS and CORBIN removed or effaced what he saw as the key features of GTM, particularly those which GLASER himself had contributed to the method. (See the chapter in *BGTA* on "Intellectual Property", particularly the section headed "Glaser's Intellectual Input".) In *QA* STRAUSS makes specific reference to the twin traditions embodied by the two authors of *Discovery*, and this is also mentioned in the first two editions of *BQR*. So in a sense GLASER was justified in arguing that these two strands were being unravelled in what he saw as a potentially misleading fashion that threatened to undermine the innovative characteristics of GTM; hence GLASER's vociferous defence of what he calls *Classic Grounded Theory*. What is surprising, however, is the tone of *BQR* itself, given STRAUSS' background and his specific reference to the influence of American Pragmatism in *QA*. Although there are some aspects of the book which lend themselves to a more sophisticated reading, the general tone is at best philosophically naïve, with largely unexamined positivist or realist assumptions. Anyone disputing this needs to look carefully at the sections dealing with data, theorising and the like. [12]

1.3 "An epistemological fairytale"

One of the responses to the ways in which GTM developed post-*BQR* was the articulation of a constructivist form of the method. This sought to resolve the "epistemological fairy tale" that lay, often unacknowledged, at the heart of GTM whether of the GLASER or STRAUSS/CORBIN variety. STRÜBING (2007) and others well versed in the GTM literature, particularly the writings of STRAUSS, have bridled at the ways in which the distinct strands of the two progenitors of the method have often been lumped together by those writing from a constructivist position. STRÜBING in particular, with his extensive knowledge of the Pragmatist literature, as well as his command of STRAUSS' work, criticises those such as Kathy CHARMAZ and me (BRYANT, 2002; CHARMAZ, 2000a; BRYANT & CHARMAZ, 2007a, b) for conflating STRAUSS and CORBIN's approach with GLASER's. To an extent STRÜBING is justified, since he can point to the specific sections in *QA* and *BQR*, and other writings, where STRAUSS alludes to Pragmatist ideas—particularly DEWEY; also to STRAUSS' attempts to engage with some of the philosophical ideas that concern issues such as the nature of *data*, the interaction between the researcher and the research context, and so on. Unfortunately, these issues were not really developed in a concerted manner by STRAUSS in his key GTM writings; an observation that STRÜBING himself makes (2007). The result was that an increasingly wide range of researchers claimed use of GTM—invoking the GT mantra, "all is data", often without any further elaboration, referring to *Discovery*—but usually with no further depth or explication. [13]

One of the key texts that influenced the Chicago School was W.I. THOMAS' paper on *The Definition of the Situation* (1923); particularly its oft-quoted apothegm "if men define things as real, then they are real in their consequences". And this can be applied to the *situation* around GTM in the 1980s and early 1990s; the reality being very much a case of a method that had got left behind in terms of its engagement with contemporary methodological debates—an unfortunate outcome given its innovative and radical potential. So whatever the reasons and justifications for this state of affairs, by the early 1990s GTM was indeed defined by many researchers as a naively inductivist method—whether of the STRAUSS-CORBIN or GLASER variety—and so this was a *real* consequence. This gave ample grounds for criticisms and negative evaluations of GTM, and it was CHARMAZ' work in particular that initiated the development of a more profoundly justified base for the method, particularly in the English-speaking world. [14]

The constructivist form engaged with the problematic issue of data and the active role of the researcher in the process of conceptual development. Ideas such as *emergence* and *discovery* were eschewed or reinterpreted accordingly. A longer account together with several later chapters exemplifying these developments can be found in the *Handbook* (BRYANT & CHARMAZ, 2007b). [15]

Whatever its strengths, weaknesses and ambiguities, the popularity of GTM grew and the method has become truly international, including not just the English-

speaking world, but also in the German speaking world where STRAUSS is widely regarded as a key figure in social theory as well as in research methods. There are also significant bodies of work using GTM amongst French and Hispanic researchers, and the Scandinavian and Asian GTM communities are also vibrant. [16]

The present context is that it is now possible to identify several different forms or schools of GTM. There are three broad approaches to GTM: the GLASER variety, as exemplified particularly in *Theoretical Sensitivity* and his many publications since that time, as well as those listed on the Grounded Theory Institute website²; the CORBIN and STRAUSS variety, specifically as exemplified in *QA* and *BQR* editions one and two; the BRYANT and CHARMAZ variety, particularly associated with CHARMAZ' extensive body of work (CHARMAZ, 1983, 2000a, 2000b, 2005, 2006, 2007, 2008a, 2008b, 2008c), and our recent collaborations such as the introductory chapters in *The Handbook of Grounded Theory* (BRYANT & CHARMAZ, 2007a, 2007b). [17]

Some have listed as many as seven different forms (DENZIN, 2007); our preferred term is "a family" as we (BRYANT & CHARMAZ, 2007a) explain in the introduction to the *Handbook*, although we recognise that the issue remains of how far one can go with altering or revising GTM basic tenets before one ceases to be doing GTM. STRAUSS also recognised this, hence his claim in *QA* that GTM is a "style of doing qualitative research ... with distinctive features" (1987, p.5). GLASER is distinctly unhappy with our position, as he makes clear in his book *Jargonizing: The Use of the GT Vocabulary* (2009) which is an extended analysis of all the chapters in the *Handbook*. [18]

1.4 GTM—Basis for qualitative research

The initial motivation of GLASER and STRAUSS was to demonstrate how some forms of qualitative research could claim a robustness and authority equal to quantitative research. This was an important project since qualitative approaches were seen as at best second class methods compared to quantitative ones. Rather than seeking entirely novel claims to respectability, GLASER and STRAUSS undertook the project of grounding GTM using many of the terms already established by the entrenched, quantitatively-oriented orthodoxy. Thus the early GTM texts provided justificatory procedures aimed at many of the targets already established for quantitative research; including use of research data, induction, open-mindedness, and prediction and control. In so doing, however, GLASER and STRAUSS often re-interpreted the terms, or raised new issues around them. [19]

For instance the idea of open-mindedness was always implicit or even explicit for all forms of research. The English phrase "begging the question", increasingly used in the sense of "*raising* the question", more accurately refers to the way in which some research hypotheses actually contain the conclusion in the premises

2 <http://www.groundedtheory.com/>

of the research itself. Quantitative research is usually seen as incorporating safeguards against this, including such ideas as the null hypothesis, double-blind testing and so on. But in the GTM context the concept of open-mindedness came to be understood to imply that researchers should not investigate the existing work and research literature most closely related to their topics. This led to many problems, particularly where research proposals were expected to make the case for funding or support in terms of the novelty of the work, considered against the context of what had already been achieved by others in the same field. [20]

On the other hand, there was also a fiercely critical element in GTM that directly eschewed some of the standard concepts of *proper* research—e.g. hypothesis testing, pre-prepared coding formats, verification, and extensive engagement with the literature. In some respects, however, GLASER and STRAUSS' forms of endorsement were even more stringent than those used by the quantitative research establishment itself: For instance their continued and unequivocal stress on the necessity for open-mindedness, the passivity of the researcher, the positivist or realist concept of data, and the reliance on induction. It is certainly possible to point to examples and extracts in the early joint statements of GTM, as well as the later distinct ones of STRAUSS and GLASER, that appear to offer a more active view of the researcher, a more sophisticated concept of data, and less strict adherence to induction; but again, the overall impact of these writings, in terms of the ways in which researchers have taken them on board, indicates otherwise. As will be seen later, this is what is so perplexing about GTM and in particular STRAUSS' role in its development. [21]

The three founding texts of GTM (GLASER & STRAUSS, 1965, 1967, 1968) are full of these ideas, although they are also replete with other statements that lead in very different directions; but it is the former ones that are usually taken up most avidly by those seeking to use the method. The result is that although the three texts have been described as "the best tutorial on GTM", this is not actually the case. *Discovery* (1967), *Awareness* (1965), and *Time* (1968) taken together offer extended examples and justification for the sort of research endeavours that GLASER and STRAUSS were so keen to encourage and promote; but their intended audience was at least as much their academic peers as the early career researchers they hoped to encourage. Thus the three books taken as a whole were simultaneously offering a critique of existing research practices and exemplifying innovative ones. *Awareness* contained an Appendix dealing with some aspects of the method; and *Discovery* was offered as an extended overview. The authors of *Discovery* were seemingly intent on at least three objectives; convincing practising sociologists and social analysts of the strengths and potentialities of GTM, illustrating their work with examples that would assist those already researching in these areas, and outlining the key elements of the method for teachers and novice researchers: the gatekeepers; the practitioners; the apprentices. Not surprisingly the reception by each group was somewhat mixed, and the efforts to achieve these disparate goals was at times counterproductive: Hence the continued clamour for a form of GTM manual. [22]

What was perhaps surprising, and certainly encouraging, was the way in which GTM became so widely recognised in the 10-15 years after publication of *Discovery*. During this period STRAUSS had begun to develop his work and research at University of California San Francisco (UCSF), and for a few years GLASER had worked as his assistant. The UCSF doctoral programme was small, and UCSF was hardly one of the key centres for sociological research; although it was certainly one of the key centres for research into medical sociology and health care in the widest senses. GLASER only held his post at UCSF for a few years, from which time on he has not held any academic teaching post, although he has been enormously active in propagating GTM, particularly in the form of his numerous publications and seminars. STRAUSS on the other hand continued to work and publish, and many of his doctoral students worked with him on several large research projects. Moreover many of his former students took up academic appointments and so increased researchers' awareness of GTM. [23]

By the early 1980s knowledge of GTM was beginning to spread, largely through the efforts of the former doctoral students from the UCSF programme, and also through STRAUSS lecturing in the US and Germany; although GTM was hardly a major item on many research methods programmes in the more traditional social research institutions. But there was a growing demand amongst PhD students eager to learn more about the method. According to KEARNEY (2007), STRAUSS lectured extensively across the US and in Germany, until he was forced to curtail such activities due to his ill-health. He was constantly asked for a set of guidelines on how to use GTM in research projects. STRAUSS sought to evade these requests for something akin to a GTM manual, but eventually, in collaboration with Julie CORBIN, he published *BQR*. This had at least two far-reaching results. First it led to an increased recognition of GTM, such that by the late 1990s, GTM was far and away the most widely claimed method for social researchers; according to some measures eclipsing all other methods put together. Second, it led to GLASER's very public critique of *BQR*, particularly his own rejoinder, *Basics of GT Analysis*, where he broke with STRAUSS in a very indignant manner—referring to STRAUSS and CORBIN's book as "without conscience bordering on immorality" (1992, p.5). [24]

Given STRAUSS' intellectual background, it might be thought that the split between the two founders was centred on issues that could be traced back to their respective and differing backgrounds and intellectual influences. But if anything, GLASER and STRAUSS were on the opposite sides to what might have been expected. STRAUSS, in the form of *BQR* was offering a somewhat mechanistic approach to qualitative research; with GLASER staunchly defending the open-ended, data-driven approach first put forward in *Discovery*. To an extent this was an ironic reversal of the bases of the initial collaboration, where STRAUSS had contrasted his own intuitive orientation with GLASER's more systematic one (see above). As far as GLASER was concerned, *BQR* presented a form of qualitative research that was shorn of all the richness and promise of GTM. STRAUSS on the other hand appeared to see *QA* and *BQR* fundamentally as more expansive statements of the method, more readily accessible to early career researchers. [25]

1.5 A critical non-engagement

Whatever the rights and wrongs of the disagreement, what was noteworthy was that both forms of GTM continued to shy away from engaging with issues about data, induction, and the role of the researcher. All of which were key topics for debate amongst social scientists in the 1970s and 1980s. This evasion or prevarication may well have contributed to the rising popularity of GTM, since it appeared to avoid all the epistemological pitfalls that seemed to befall other methods—particularly qualitative ones. The GTM-oriented researcher could claim that whatever the findings might be, they were grounded in the data; and this could be demonstrated by the strong conceptual links between the data, codes, categories and resulting substantive grounded theory itself: and with regard to many fluent and persuasive GTM accounts, whatever their provenance or foundation, this was indeed the case. Those well versed in the relevant history and literature can point to STRAUSS' role as a key figure linking Pragmatism and Symbolic Interactionism, acting as a conduit for the ideas of DEWEY and MEAD. But this simply increases the perplexity regarding his lack of attention to these sorts of issue in his GTM writing. [26]

The result was that by the late 1990s the research literature was full of claims to be using GTM; some clearly focused on STRAUSS and CORBIN's *BQR*, some drawing on GLASER's *Theoretical Sensitivity*, and some using both plus *Discovery*. *BQR* was particularly important in this regard since students could point to something akin to a methods manual, with clearly outlined steps and stages presented in a systematic manner. Thus research proposals based around this could be understood by those unfamiliar with qualitative research in general, and GTM in particular. But if *BQR* lent some weight to the argument for using GTM, two other aspects worked against this. First there was GLASER's condemnation of *BQR*, encompassing his critique of STRAUSS' trajectory not only after but even during their initial collaboration. Second there was the increasingly common way in which some researchers were claiming to use GTM while clearly not doing so in any manner whatever. GTM came to be seen as a way in which researchers could evade issues such as familiarity with the literature, clear framing of research questions, and forms of data analysis. [27]

So through the late 1980s and early 1990s anyone attempting to understand what GTM was from this plethora of ideas, concepts and criticisms was likely to end up in a state of utter confusion and exasperation. What rescued GTM from simply slipping into disrepute and misuse was the increasing number of researchers who persevered with the method and managed to produce well-respected and widely referenced research reports and publications often regardless of the particular GTM line to which they claimed adherence. Examples include Adele CLARKE, Joan FUJIMURA, Susan Leigh STAR, Fritz SCHUETZE, Isabelle BASZANGER, Wanda ORLIKOWSKI; in some cases the authors of these studies claimed adherence to GLASER's GTM, others claimed adherence to STRAUSS and CORBIN, and others either seemed to be unaware of the split between the founders or simply ignored it. [28]

1.6 Growth and renewal

By the latter half of the 1990s there was both a body of GTM-based research, and amongst those using and teaching the method, a realisation that statements about GTM had to contend with issues about data, induction, the role of the researcher, prior knowledge, team working, plus other specific institutional issues such as approval by an Institutional Review Board [IRB], ethics committee or equivalent, hypothesis testing, issues of gender, race, ethnicity and so on. In many regards the critical role was played by those who advanced a view of GTM based unequivocally and centrally around interpretivism or constructivism, some of them building explicitly on the Symbolic Interactionism with which STRAUSS was also associated, given his background with the Chicago school. From these bases it was possible to take into account the active role of the researcher, deal with issues around data and knowledge, and engage with the general changes in and challenges to concepts of science, knowledge, and general theoretical development. Moreover this allowed an aligning of GTM with a different set of justificatory procedures for research outcomes, based on the precept that knowledge was provisional, consensual, and dependent on the researcher's (or researchers') perspectives. It might be contended that these sorts of concern were always present in GTM, but it is only in the work of GTM researchers subsequent to GLASER and STRAUSS that these issues are made more explicit, and put to greater effect, particularly in the light of the development of an avowedly constructivist form of GTM. [29]

This is not to say that researchers following GLASER's or STRAUSS and CORBIN's writings were not producing valuable and insightful results, some undoubtedly were, and continue to do so; but all too often the methodological justification and claimed research basis were woefully inadequate or inappropriate. The constructivist version was not without its detractors, but at least this was an argument between explicitly stated and contending views of epistemology and ontology, rather than what appeared to be simply an evasion of these issues altogether. [30]

Yet at the heart of the constructivist turn lies a paradox. Once one moves away from a position whereby knowledge claims are seen to be founded on some form of true and accurate representations of reality—terms such as positivism, post-positivism, realism, representationalism, and foundationalism all apply here—then there is a tendency to move towards an uneasy relativism and a form of epistemological special pleading. This can easily lead to the ultimate caricature of post-modernism, whereby any and every claim to knowledge is upheld as equally valid; based on the tenet that all forms of knowledge claim are relevant or contextually appropriate or legitimate. This is itself a knowledge claim with pretensions to universal validity. So out and out constructivism is a contradiction in terms, as many positivists and others of their ilk gleefully and correctly point out. [31]

With this in mind, GLASER's un-reconstituted, *classic* position, which evades and avoids all engagement with issues of epistemology and the like, has at least as

much merit as the constructivist one. (The same can be said of STRAUSS and CORBIN's.) If GLASER's GTM rests on a fairytale in which the handsome grounded theorist always lives happily ever after with the data from which the relevant concepts emerge; the post-modern constructivist's GTM is just as incredible, with a whole host of handsome grounded theorists living happily ever after, each with their own, incommensurate data from which they idiosyncratically develop their constructed concepts. [32]

The net result is that the GTM landscape appears somewhat flawed. The failure of both GLASER's and STRAUSS' variants to engage with concerns regarding the nature and status of data, induction, and key issues surrounding knowledge claims offers too many obvious targets to those who wish to condemn GTM outright. Furthermore the dispute itself allows critics to dismiss GTM as self-contradictory and ill-founded. On the other hand GTM is dismissed by those advocates of traditional, quantitatively-oriented approaches because of the GTM position with regard to initial hypotheses, testing and validation, and engagement with the literature and other requirements of the disciplinary authorities. Even though advocates of GTM can point to all manner of statements and arguments that justify the method, all too often this is to no avail. [33]

GLASER and STRAUSS' concern, in *Discovery* in particular, that GTM should in some ways match up to the same or equivalent standards of robustness and rigour that characterised the traditional methods has proved to be something of a poisoned chalice. The gate-keepers of the traditional research communities have largely rejected the claims of GTM as a method for achieving rigorous research results. This is somewhat ironic given that there is in many cases broad if superficial agreement on many epistemological points between GLASER and the positivist advocates of the primacy of quantitative methods—e.g. their views of data, the role of induction, the passive role of the researcher (although in many other key regards their views are diametrically opposed). [34]

Moreover anyone looking to STRAUSS' restatement, or rather STRAUSS and CORBIN's, as a form of clarification will be disappointed; *BQR* may well have been the text of preference for researchers using the method from the late 1980s onwards; but GLASER's criticisms do indeed hit home: Anyone seeking to use GTM simply from a reading of *BQR* would have departed significantly from the ideas put forward, albeit in a highly nuanced manner, in *Discovery*, *Awareness* and *Time*; and in so doing would have been in danger of losing much of the richness and promise of the method. [35]

The constructivist version, particularly as it has developed in the recent work of CHARMAZ and myself—both separately and in concert (CHARMAZ, 2000a; BRYANT, 2002; BRYANT & CHARMAZ, 2007a, b)—does at least take up the methodological challenges of the last 30 years, and begins to provide a clear and more robust basis for researchers and those involved in examining and accrediting research plans and outputs. But there is still the issue of the extent to which this form of GTM can provide and sustain knowledge claims against the criticisms of the positivists, realists, or whatever. Added to which, the GTM

community as a whole has yet to clarify the ways in which concepts, substantive theories and formal theories afford a basis for knowledge, practice and further research. [36]

All of this is not to detract from the clear popularity of the method in all of its forms; nor should it be thought for a moment that these issues are unique to GTM. Concerns about data, induction, knowledge claims, robustness, verifiability, replication and so on beset all methods and forms of enquiry and research; GTM just happens to exemplify and illustrate many of them in particularly clear and graphic fashion, and their potential resolution in this context offers insights above and beyond GTM itself. So in dealing with some of the issues around different parts of the family of GTM, a more generic form of clarification is on offer: one with more *grab*. [37]

2. GTM and Pragmatism

2.1 STRAUSS and Pragmatism

A starting point for the resolution or remedy lies in a key feature of STRAUSS' own background; indeed in an area with which he continued to engage up to and including his very last writings: Pragmatism. What will be argued in the remainder of this paper is that STRAUSS' Pragmatism was almost certainly a key influence in the early development of GTM, but was largely dormant or neglected in the ensuing developments and splits of the 1980s and 1990s. This disregard can now be seen as a critical weakness in the general development of GTM, and overcoming it offers a profound and promising path for the development of GTM specifically and the agenda for social research in general. [38]

The importance of Pragmatism for GTM was noticed by some of its adherents, particularly those in the German-speaking world such as Hans JOAS (1987), Jo REICHERTZ (2007), Udo KELLE (2007) and Jörg STRÜBING (2007). In some instances their interest was itself ignited by STRAUSS himself or by those who had themselves been influenced by and collaborated with STRAUSS. But even they have had to admit that STRAUSS himself failed to draw upon Pragmatism in any sustained manner in his published GTM work. [39]

STRAUSS was always keen to stress that American Pragmatism was a central component in his intellectual formation. According to STRÜBING (personal communication), STRAUSS was introduced to the work of DEWEY and JAMES by Floyd HOUSE, his teacher while he was an undergraduate at Virginia University in the 1930s. Later as he developed his interests in both psychology and sociology, STRAUSS used ideas from DEWEY and MEAD as his own work on action, structure and process developed. DEWEY and PEIRCE are mentioned in *QA*, and the first two editions of *BQR* open with a quote from DEWEY's *Art as Experience* (1934). In his final book, *Continual*, STRAUSS makes extensive reference to Pragmatism in the introduction where he charts his own intellectual development, but this is specifically oriented around the work of DEWEY

understood through the teaching of G.H. MEAD and later developments of the Chicago school. [40]

2.2 Varieties of Pragmatic experience

The term *Pragmatism* itself was popularised by William JAMES³, although he always credited Charles PEIRCE with its origination; albeit giving an incorrect source⁴. PEIRCE himself later tried to distance himself from the positions advocated by JAMES and DEWEY, offering the term "Pragmaticism" for his own position. (PEIRCE himself noted that it was a term so ugly that no-one else would use it—see PEIRCE, 1998.) Pragmatism was seen as a specifically American, and somewhat peripheral contribution to philosophical thought, and after DEWEY (who left no obvious successors) it fell into relative neglect from late 1930s until revived and revised from the 1980s onwards by the neo-Pragmatists, particularly and most significantly Richard RORTY, but also Hilary PUTNAM (1995) and Donald DAVIDSON (1985)⁵. [41]

John DEWEY in particular has been re-discovered and justifiably elevated into a philosophical grandee in recent years; to a large extent a result of RORTY's efforts. His preferred term for what we now understand as Pragmatism was *Instrumentalism*. In the context of GTM, it is interesting to note that DEWEY was a colleague of G.H. MEAD in Chicago, but later moved to Columbia. This was no more than coincidence, and in the case of Columbia, DEWEY left no lasting influence in the form of adherent or research students. (DEWEY did leave an influence at Teachers College—now part of Columbia, but a separate establishment in DEWEY's time.) On the other hand, DEWEY's influence on STRAUSS is readily apparent from the latter's own writings as will be shown below. [42]

Although there is no single school of Pragmatism, there are a number of key themes shared by most if not all those who adopt that label or have had it thrust upon them. One of the primary tenets, particularly stressed by DEWEY (1917), was the disavowal of what he termed "the spectator theory of knowledge", with its connotations of passive observation of an accessible and unproblematic reality; a world-in-itself awaiting discovery. In its place DEWEY proposed "the experimental theory of knowledge", where all knowledge is seen as provisional, and is judged in terms of how useful it is for the knowing subjects. RORTY took up both of these specific aspects in his book *Philosophy and the Mirror of Nature* (1980) that attacked the concept of a correspondence theory of truth—i.e. the idea that truth claims could be judged in terms of how close they were to reality itself. [43]

For Pragmatists knowledge exists in the form of statements or theories which are best seen as instruments or tools; coping mechanisms, not once-and-for-all-time

3 The title of this subsection echoes JAMES' work *The Varieties of Religious Experience* (1902).

4 William JAMES 1898 lecture at UC Berkeley, but earlier works had laid the foundation, see <http://www.iep.utm.edu/p/pragmati.htm> [accessed: June 9, 2009].

5 MURPHY's discussion of Pragmatism is a useful introduction and guide to further reading of both primary and secondary sources (MURPHY, 1990).

truths. Consequently we must always allow that all and any of our current tools may be surpassed in the future—this is what is meant by the term *fallibilism*. RORTY sums this up with the argument that any of our ideas must be open to doubt (including this one); but on the other hand not all at once. He evokes NEURATH's image of our conceptual toolset as a raft on which we are floating; where in the long term we will have to replace all the planks, but only one at a time! (STRÜBING offered the important clarification of this precept in his comments on an early draft of this paper, pointing out that PEIRCE himself stressed that by "doubt" he meant "practical doubt", not a generic and all-encompassing form of Cartesian scepticism.) [44]

What DEWEY averred was that there can be no Archimedean fixed points from which to observe reality, and no appeals to "raw experience"; hence no universal and context-free claims to truth. All knowledge is provisional, and has to be judged in terms of its usefulness within some set of confines—hence DEWEY's term *Instrumentalism*.⁶ [45]

RORTY takes this still further in stating that there is no extra-linguistic reality for us to represent, and no extra-linguistic way of representation. DEWEY himself was perhaps more nuanced, but few writers are less nuanced than RORTY! The result is that, for Pragmatists such as RORTY, knowledge is not a hierarchical structure, with science or philosophical insight at the top and other lesser forms, such as *common-sense*, or practical wisdom in inferior positions. Rather knowledge is a web or a network of statements rather than an edifice, and the value of any form of knowledge is its usefulness and applicability which may be constrained in terms of time and place and user. RORTY approvingly quotes NIETZSCHE's dictum that what passes for "truth"—or more poignantly "The Truth"—is in fact "a mobile army of metaphors, metonyms, and anthropomorphisms" (quoted by RORTY, 1991, p.3). [46]

Another key aspect of Pragmatism, and an exceedingly useful one, is what might be termed the "so what?" principle, or the difference principle—i.e. for any argument, particularly one about metaphysics or foundations or similar, one must ask "what practical difference would it make if either I or my opponent was correct/incorrect?" If the answer is "none", then forget it!⁷ [47]

2.3 Fallibilism

There are two key corollaries of the Pragmatist position, shared by many Pragmatists of different hues, not simply DEWEY and RORTY. The first is the rejection of any quest for certainty, a position with political and ethical

6 Much of what follows concerning Pragmatism is derived from RORTY—he makes extensive use of the work of other neo-Pragmatists such as DAVIDSON and PUTNAM, but they are not in complete agreement on all issues. The collection on *Rorty and his Critics* (BRANDOM, 2000) illustrates the issues on which there is both substantive agreement and disagreement.

7 In some cases these differences of opinion may be well worth pursuing. DEWEY's ideas about *difference* are echoed in Gregory BATESON's oft quoted definition of information—"the difference that makes a difference" (1979)—and takes on a deeper meaning when associated with DEWEY. BATESON also took up PEIRCE's idea about abduction.

ramifications. In far too many cases certainty leads to despotism, fanaticism or intolerance; it also blocks progress. VOLTAIRE famously observed that while doubt is uncomfortable, certainty is ridiculous⁸. The Pragmatists argue that certainty is more than ridiculous, it can lead to "cocksure dogmatism" and worse; the only alternative is fallibilism. This should not, however, be confused with scepticism, particularly of the Cartesian variety which proceeds by disbelieving anything that appears doubtful with the eventual aim of arriving at a basis of certainty. [48]

The first corollary then springs from the Pragmatist rejection of Cartesian Rationalism; the second corollary relates to a rejection of Lockean Empiricism which is premised on a model of the mind as an empty vessel, which is then filled by the senses that a person experiences. LOCKE used the term *tabula rasa*, a blank slate, coupled with the dictum that "*Nihil est in intellectu quod non prius fuerit in sensu*" [Nothing is in the understanding that was not earlier in the senses]⁹. For Pragmatists the mind is neither self-contained, which is one of the key precepts of Cartesianism, nor is it an empty vessel embodied in a passive observer¹⁰. On the contrary, DEWEY's specific objective was to counter what he termed "the spectator view of knowledge", instead placing action and emancipation at the centre. The following quote is worth producing in full as it neatly encapsulates some of the key features of Pragmatism.

"According to such Cartesianism, the mind is a self-contained sphere whose contents —'ideas' or 'impressions'—are irredeemably subjective and private, and utterly sundered from the public and objective world they purport to represent. Once we accept this picture of the mind as a world unto itself, we must confront a host of knotty problems—about solipsism, skepticism, realism, and idealism—with which empiricists have long struggled. Pragmatists have expressed their opposition to this Cartesian picture in many ways: Peirce's view that beliefs are rules for action; James's teleological understanding of the mind; Dewey's Darwinian-inflected ruminations on experience; Popper's mockery of the 'bucket theory of the mind'; Wittgenstein's private language argument; Rorty's refusal to view the mind as Nature's mirror; and Davidson's critique of 'the myth of the subjective'. In these and other cases, the intention is emancipatory: Pragmatists see themselves as freeing philosophy from optional assumptions which have generated insoluble and unreal problems." (McDERMID, 2006, n.p.) [49]

So although there is indeed a wide "variety of the pragmatic experience", many of the issues discussed so far are common to most of them. Moreover the trend that starts from DEWEY and extends to RORTY is the key one with regard to GTM, particularly since STRAUSS himself makes continued reference to DEWEY in his

8 <http://www.people.ubr.com/authors/by-first-name/v/voltaire/voltaire-quotes.aspx>

9 This is termed "The Peripatetic Axiom", and this version is attributed to Thomas Aquinas http://en.wikipedia.org/wiki/Peripatetic_axiom.

10 Hence the aphorism from PLUTARCH which we chose for the Handbook—"The mind is not a vessel to be filled, but a fire to be ignited" (BRYANT & CHARMAZ, 2007c).

own work, and traces his own intellectual development through DEWEY, MEAD and what he terms "Chicago Pragmatism".¹¹ [50]

2.4 Pragmatism—A red thread and a lifeline

STRAUSS himself in charting his own intellectual development (*Continual*) stressed the importance of Pragmatism (specifically Chicago Pragmatism which he contrasted with Chicago Sociology) in his own intellectual formation, and the ways in which it remained a constant influence on his work—"a red thread running through my work" (1993, p.22). Yet he did this largely against the background of his work on developing a theory of action. He mentions GTM but only in the sense that the

"methodology as developed in close and equal collaboration by Barney Glaser and me, evolved out of this sense of complexity that we shared; in this instance, the complexity of interaction and interactional forms that we were studying as played out in the care of dying patients" (1993, p.12). [51]

As was mentioned earlier (Section 2.1), there is already a substantial body of work on the links between Pragmatism and Straussian GTM, but the more general and mutually supportive relationship between GTM as a whole and Pragmatism needs to be brought to the forefront of debate. What I wish to offer in sections 3 and 4 of this paper is a foundation upon which the "red thread" can be broadened and strengthened in such a way that many of the disputes and ambiguities that have beset GTM can be resolved or dispelled. [52]

GTM developed in the late 1980s and early 1990s against the background of the distinct standpoints offered by GLASER and STRAUSS/CORBIN respectively. But this divergence itself developed against, but remote from, a background of philosophical and cognitive upheavals whose roots can be traced back to the work of Thomas KUHN, Harold GARFINKEL and others that date from the 1960s (KUHN, 1962; GARFINKEL, 1967). As has been argued extensively elsewhere (BRYANT, 2002; BRYANT & CHARMAZ, 2007b) neither GLASER nor STRAUSS or CORBIN ever responded directly to what has been termed the *post-modern turn*. The result was that for many GTM was simply beyond the pale in methodological terms, completely isolated from the various epistemological and ontological debates that had been unleashed with the post-modern disenchantment with traditional Enlightenment values and concepts such as science, knowledge, and understanding. Yet there was dissatisfaction with this deafening silence from within GTM, and it led to a series of efforts to articulate a more robust and resilient basis for the method, engaging with the debates of the time. CHARMAZ in her work since the early 1990s spearheaded what has become termed "Constructivist GTM", later reinforced by my own work (initially developed independently), our joint efforts, and also those of others such as CLARKE (2005). [53]

¹¹ For some reason STRAUSS never refers to William JAMES in any of his references to Pragmatism.

The net result of this has been that GTM now has a firmer conceptual basis, since those using the method can now point to various ways in which the method's key concepts, claims and orientations can be located against existing research and methodological issues and concerns. This is particularly important for PhD students and researchers seeking publication outlets and career opportunities—something that lay at the heart of GLASER and STRAUSS' initial endeavours. But it has also led to some confusion both with regard to the method itself and to the more general issue of the knowledge claims that researchers can make. [54]

With regard to the more general point, one of the impacts of post-modern thinking has been a tendency to a position of relativism which in its more extreme forms implies that all knowledge claims are merely matters of opinion or orientation to be accepted, tolerated or rejected as questions of personal taste: *De scientia non est disputandum!*¹² In other words claims to knowledge, like matters of taste, are subjective and so not open to argument. This can be seen as a strength on the part of the proponents of post-modernism, and as a fatal weakness by its critics. Moreover even in its weaker forms, the post-modern turn has severe implications for concepts such as evidence, data, and explanation. The attraction of GLASER's classic GTM is that it offers clear and deceptively simple statements such as the following:

1) "The analyst can start anywhere in the data and trust to the emergence in open coding" (1992, p.48).

2) "GT comes from data, but does not describe the data from which it emerges" (2001, p.4).

3) "Constructivist Grounded Theory (GT) is a misnomer. GT can use any data; it remains to be figured out what it is. In my book 'The Grounded Theory Perspective' (GLASER, 2001) I wrote a chapter (11) that dealt with 'all is data.' I said:

' "All is data" is a well known Glaser dictum. What does it mean? It means exactly what is going on in the research scene is the data, whatever the source, whether interview, observations, documents, in whatever combination. It is not only what is being told, how it is being told and the conditions of its being told, but also all the data surrounding what is being told. It means what is going on must be figured out exactly what it is (sic) used for, that is conceptualization, not for accurate description. Data is always as good as far as it goes, and there is always more data to keep correcting the categories with more relevant properties.' (p.145)" (2002, [1]) [55]

This evades and avoids the epistemological morass opened up with the advent of post-modernism; although the issues themselves have a far longer history, dating back to the earliest ideas that philosophers and others offered with regard to the nature of knowledge and the activities involved in knowing. STRAUSS and CORBIN are slightly more ambiguous in their writings, but generally tend towards

12 The original version is *De gustibus non est disputandum*—"there is no arguing about tastes".
http://en.wikipedia.org/wiki/De_gustibus_non_est_disputandum.

GLASER's position on these aspects, particularly given STRAUSS' use of GLASER's work in his writings—see above¹³. [56]

Unfortunately this evasion cannot be sustained, particularly for PhD students and early career researchers who are expected to be able to position themselves against the current context of debates on methods and philosophical positions such as positivism, constructivism, and the like. This is particularly important for all researchers using and advocating use of qualitative methods, since there is usually a greater onus on such researchers to justify and ground their approaches—those using quantitative methods tend to have an easier time in this regard, although they may have to endure severe questioning about the details of their use of a particular method or model. [57]

The constructivist turn in GTM may then be seen by some as complicating the method, but this was unavoidable if GTM was to continue to be regarded as a valid research approach in many disciplines. Moreover, as will be demonstrated in the next section, working through the constructivist critique to a GTM that is more explicitly Pragmatist results in a far more coherent GTM which can be justified and defended expansively and profoundly: in effect turning the red thread into a lifeline. [58]

3. Key Features of GTM Reconsidered in the Light of Pragmatism

3.1 The issues for GTM

The ramifications of the constructivist critique and restatement of GTM were important in drawing attention to a number of core aspects of the method; in particular concerns about research questions and hypotheses, iteration of data gathering and analysis, the role of the researcher in aspects such as derivation of codes and concepts, the status of *theory*, the role of practice, and the use of *data*. Each of these will be considered in turn in the sections that follow. At this stage a more general point needs to be made concerning the way in which Pragmatism in its recent articulations, particularly RORTY's, offers a way of moving beyond the inevitable but unavoidable upheaval caused by the constructivist turn to a more robust GTM that sheds new light on the initial GTM statements of GLASER and STRAUSS, as well as their later distinctive writings. The work of RORTY and others provides a basis for a re-interpretation and re-statement of some of the key features of GTM which draws the seemingly disparate ideas of GLASER, STRAUSS, and the constructivists closer together. [59]

This re-interpretation of the roots of GTM, linking it far more specifically to Pragmatism, can help to counter the most extreme and uncomfortable aspects of the upheavals alluded to earlier. For example some of the most awkward aspects of GTM, particularly in the early writings of GLASER and STRAUSS, emanate from the use of terms such as *fit*, *grab*, *saturation*, *work*, and *theoretical sensitivity*. Anyone expecting traditional criteria for precision and clarity is likely to

¹³ GLASER's position remains consistent in this regard as is evidenced in his most recent book (2009)—see Chapter 2 *Data Worries*.

be severely disappointed and perplexed by such statements; and they are not simply present in the early texts. GLASER continues to use them in his writings, and STRAUSS in *Continual* talks of a concept "earning its way" (1993, p.53)—a phrase he attributes to "my colleague Barney Glaser". In many regards statements such as these are often seen as troublesome features of the method. Students ask repeatedly for some clear and concise criteria for developing and evaluating their concepts and codes, and the use of such inexact terms and folksy idioms can result in them feeling that GTM cannot really offer a viable route to research insights. Those assessing and evaluating such research proposals find themselves similarly discomforted. Moreover those surveying the methodological scene will treat such critical terminological ambiguities as being indicative of severe conceptual weakness in the method as a whole. How does one explain to a sceptical examiner or assessor what is meant by grab or fit, or a concept earning its way? [60]

Those teaching GTM often find it hard to articulate anything more precise than some fairly impressionistic guidelines when confronted by students requesting elucidation of these aspects of the method. Yet if we move away from what the Pragmatists term "the metaphysics of the real", and instead adopt the Pragmatist view that theories and concepts are best considered in terms of their usefulness rather than their truthfulness, then a whole host of features of GTM appear in a far clearer light. Those who recognise and have written about the Pragmatist leanings in STRAUSS' work might contend that this is hardly surprising; but again it must be stressed that the actual articulation of this orientation is precisely what has been lacking in GTM to date. [61]

3.2 Engaging with the literature

GTM developed in direct response to the context of US academic social science research in the 1960s. In their early collaborations GLASER and STRAUSS correctly identified some of the key inherent weaknesses in a system largely oriented to tinkering with the grand theories of the grand theorists. GTM was offered as a radical and distinct alternative, hence the stress on GTM explicitly disavowing many of the standard research practices prevalent at the time—i.e. GTM researchers should not immerse themselves in the authorised literature, should not prepare hypotheses for testing and validation, should aim primarily at developing their own concepts and categories as a result of some personal and direct engagement with a specific research domain, rather than from secondary or tertiary sources. [62]

One of the persistent issues that troubles researchers thinking about using GTM is engagement with the literature. GLASER has always maintained the view that researchers should shy away from the literature most relevant to their substantive research context—although they should immerse themselves in as much literature as possible on other, potentially related issues and topics. This is problematic in many regards since researchers are often asked to present a research proposal that situates the planned research against existing work and publications. Moreover, by definition, if they are planning to use GTM, then they

may not have any detailed awareness of the research issues and hence of the associated literature; they may be researching into areas with which they already have extensive familiarity. Although GLASER has some reasonable grounds for his admonition, it is more misleading than helpful in the current context of research practice. If grounding in the data is crucial, then there is no reason why the extant literature cannot be part of that data¹⁴. Moreover if we move away from the idea of GTM as an inductive method, and instead put stress on the ways in which theoretical sensitivity can be encouraged and advanced as a form of abduction, then this issue simply disappears. One can never enter a research area with an empty head; one can try to do so with an open mind, but sometimes it is precisely one's prejudices—in the sense of prior judgements—that provide a basis upon which innovative insights can be developed. The Pragmatist concept of abduction takes account of this; insights can come from anywhere. Indeed for PEIRCE abduction was the only source of innovative insights, while induction and deduction were methods for justifying or developing already existing postulates (see below Section 3.8). [63]

To a large extent GLASER and STRAUSS' call for researchers to be more ambitious, aiming to develop their own concepts rather than testing existing grand and not-so-grand theories, is still relevant. So too is their stance against research orthodoxies, and this applies even to the "uneasy orthodoxy" that GTM has become (see BRYANT & CHARMAZ, in press). Hence researchers need to develop their own views about GTM, and present them for consideration. [64]

The key issue, however, arises from the debates about knowledge claims that have developed in the period since *Discovery* first appeared, which now challenge some of the assumptions that GLASER and STRAUSS made about research preparedness—and in some regards shared with those who were the targets of their critique. For example the idea that researchers can approach a topic without any preconceptions, or having rid themselves of all prejudices and bias, is still perhaps taken seriously by some, but largely derided by most. On the contrary, it is now widely understood that it is often precisely people's prejudices that enable them to produce innovative insights and alternative models and accounts. In challenging the hypothetico-deductive model of the times, GLASER and STRAUSS specifically contrasted it with an open-minded, inductive model; one that avoided engagement with the authoritative canon. In the ensuing years these recommendations have lost their shock value, and are too often seen as obstacles to use of GTM itself. [65]

The Pragmatist position¹⁵ referred to above is that there are no fixed points from which reality can be observed, and hence no appeals to *raw experience*. This undermines the GTM precept of not engaging with the literature if the main justification for such an admonition is that by avoiding this engagement, the reader-researcher will be in a more neutral or unbiased position: there are no

14 GLASER clearly agrees with this since his recent book is a grounded theory based entirely on one book—*The Handbook* (BRYANT & CHARMAZ, 2007c).

15 A reminder that this is my interpretation of "the Pragmatist position", derived almost entirely from my reading of DEWEY and RORTY.

such positions. This is not to say that valuable insights and innovative conceptualisations can only come about following an engagement with the literature, particularly the grand theories and grand theorists; on the contrary there is still value in the GTM precept of initiating one's research with flexibility and openness that may preclude a formally stated hypothesis or even a more casual research question.¹⁶ [66]

3.3 Iteration between data gathering and analysis

GTM was initially labelled as the method of constant comparison, and this remains a feature of all varieties of GTM, although its role is far less central in STRAUSS and CORBIN's approach than it is in GLASER's. But what is common to all variants is the iteration of data gathering and analysis, including continual comparison of the findings of one stage with that of the next. Here we have a classic instance of the way in which Pragmatism helps dispel some of the most problematic dichotomies and dualities that otherwise seem to present unavoidable obstacles and distractions to researchers. Essentially the task of data gathering can never be completed, and in any case is hardly the mindless exercise that is sometimes presented or envisaged. There is always some form of analysis going on even as the data is being gathered, but in GTM there is an explicit recognition of the iterative relationship between the two, and the ways in which they inter-relate and guide each other. As will be seen below, in discussing the issue of data as such, the imagery evoked by data *gathering* is itself highly misleading. By extension this more mindful and insightful view of the iterative process of data-gathering-cum-analysis explains the idea of *theoretical sampling* that is key to GTM. Theoretical sampling is a means whereby researchers can develop their initial theoretical analysis, fleshing out and enhancing the concepts identified in the earlier stages of their work. It stands in contrast to representational or initial sampling, also to random sampling. For some critics of GTM, it might seem to be a case of researchers looking for confirmation of their initial ideas, as opposed to trying to falsify or disprove them; but from the Pragmatist point of view, it is far more a case of researchers seeking out the ways in which their concepts actually *work* in elucidating the specific research context. A good example of this can be found in the founding texts of GTM where two key concepts were identified—*awareness* and *trajectory*—each one then being further developed in specific and distinct ways: hence the two studies *Awareness* and *Time*. [67]

3.4 Derivation of codes, categories, and concepts

As was noted earlier (Section 2.1) RORTY approvingly quotes NIETZSCHE's dictum that "The Truth" is "a mobile army of metaphors, metonyms, and anthropomorphisms". The quote continues to the effect that the metaphors become worn out, like well-used coins where the image has been effaced through constant exchange. This is readily apparent in GTM writings where the term

¹⁶ Apart from the Pragmatist argument against strict adherence to this GTM principle, there is also a *pragmatic* one, since in many contexts it is mandatory that there be some form of literature review in any research proposal, whether for a PhD or a specific research project.

emergence is used. *Discovery* is replete with use of the term; concepts "emerge" from the data, theories "emerge" from the concepts and so on. In some of his later writings, STRAUSS reduced his use of the term, although it still appears in both *QA* and *BQR*. The issue is not that the term is metaphorical itself, but that use of this metaphor can obscure the issues of where codes, categories, concepts, and theories come from, and the processes involved in their derivation and articulation. Indeed the metaphor implicitly lends weight to the spectator view of knowledge that DEWEY sought to undermine. [68]

BQR relies less on emergence than some of the earlier works, but STRAUSS and CORBIN still opt consistently for the passive form in describing the method—i.e. the data *is* collected, *is* used. The outcome is that little or no consideration is given to the active role of the researcher—and the spectator view of knowledge and the idea of emergence are thus reinforced.¹⁷ [69]

In probing the concept of emergence in this manner, one inevitably raises the question: "Where do ideas come from?" GLASER and STRAUSS specifically developed GTM as a method that would encourage and give confidence to researchers to develop new ideas; but in so doing they seemed to imply that simply accruing or gathering data would lead to new concepts and then to new theories: hence their claim that GTM was an inductive method. Continued use of the term emergence, and an under-developed understanding of induction reinforce this assumption (see below Section 3.7). [70]

One alternative to the idea of emergence is the distinction between the context of discovery, which can be seen as a psychological or personal process, and the context of verification or validation.¹⁸ But this goes directly against what GLASER, in his inimitable fashion, has termed "immaculate conceptualization" (1978, p.8). GLASER's concern is to stress the necessity for the grounding of concepts as opposed to encouraging free-floating conjectures. On the other hand there is an extended literature on classification, from HUME onwards, and the activities of discrimination and categorising have themselves been subject to extensive analysis; indeed one of STRAUSS' students—STAR—has co-authored a notable text which discusses the ways in which classification systems develop as social constructs (BOWKER & STAR, 1999). [71]

In a fashion similar to the discussion in Section 3.2 above, the initial GTM position requires some revision, albeit with continued stress on the original rationale. The metaphor of emergence needs to be jettisoned. It may well prove to be the case that if the same data is supplied to a number of researchers or other colleagues, the same or a similar concept will *emerge* for some or all of them. But it is equally

17 This is still the case in the 3rd edition—CORBIN and STRAUSS (2008).

18 In the English-speaking world this is widely associated with Karl POPPER, but as the *FQS* editors pointed out, it originated with the work of Hans REICHENBACH.

Note from the *FQS* editors: Hans REICHENBACH introduced the terms in Chapter I "Meaning" of *Experience and Prediction* (1938, Chicago: University of Chicago Press; see [http://www.comnet.ca/~pballan/Reich\(1938,chapter1\).htm](http://www.comnet.ca/~pballan/Reich(1938,chapter1).htm) and also <http://plato.stanford.edu/entries/reichenbach/> for a short introduction of REICHENBACH and POPPER).

likely that a whole range of differing concepts will be identified. GTM, like all research methods and philosophies of knowledge, has to take account of the role of the researcher or observer. This is nothing new, nor is it unique to qualitative social research. It cannot be avoided by using a passive voice—i.e. the concept *emerged*—there must be unequivocal recognition of the active role of the researcher. A Pragmatist development of this issue stresses the active, non-spectator view of the researcher, but also places equal emphasis on the grounded-ness of any knowledge or concepts that are developed. From the Pragmatist viewpoint, concepts are tools, and the value of a tool is not its universal validity, but its usefulness in a specific context. So while the metaphorical baggage associated with emergence must be discarded, the stress on the grounded-ness must be retained. GLASER's dismissal of immaculate conceptualisation retains its critical force since the value of any conceptual argument needs to be convincingly anchored in a substantive research context. [72]

3.5 Status of "theory"

The core concern of GTM is to develop new theories, but what exactly is "a theory"? Would we recognise one if we saw one? How could we distinguish between strong and weak claims for something being a theory? The term itself is ambiguous, and currently that ambiguity is proving particularly troublesome. In some cases the term theory denotes a firm basis for further knowledge development; for instance EINSTEIN's *theory* of relativity. But in other cases the word denotes something more like a conjecture, a speculative claim rather than a well-founded one; for instance someone's theory about why the English football team consistently fail to perform well at the World Cup. [73]

This is not only troublesome, but also has political and educational ramifications, for instance in the continuing debates concerning the theory of evolution; targeted by those keen to promote an equivalence between this theory and what used to be called "creationism", but is now termed the "theory of intelligent design" [ID]. Those advocating ID often point to what they term "Darwinism" as something that is "*only* a theory"—i.e. theory in the sense of a speculative claim—whereas those who see ID as merely one current form of Biblical fundamentalism see the *theory* of evolution as a theory in the sense of a well-attested basis for future research and insight. [74]

So in one sense "theory" implies something along the lines of "conjecture" or "supposition"; but in another sense it implies something with an enhanced status, attained only after exhaustive efforts to test and challenge it. This latter meaning is certainly what GLASER and STRAUSS had in mind in arguing for theories to be grounded; although when a grounded theory is first developed it can hardly claim this status, which can only come with time and the attention of others. GLASER and STRAUSS were clearly aware of this distinction between a grounded theory when first proposed, and its potential for further development. In their work (GLASER & STRAUSS, 1967, 1971; STRAUSS, 1987; GLASER, 2006, 2007) they continually stressed the distinction between what they termed "substantive" and "formal" theories, which amplifies this point. Substantive

theories are closely linked to the context in which the research is grounded; only later can these theories become formal, after they have been taken up and used against other contexts and possibly by other researchers. Trying to clarify the distinction between substantive and formal theories is difficult in the abstract, although the GTM literature offers many examples starting with STRAUSS' work on *Negotiations* (1978) and GLASER and STRAUSS' work on *Status Passage* (1971). [75]

The problems that people have with the term *theory* can be readily resolved through an understanding of how it has been used by GLASER, STRAUSS and others in the GTM context: Theories, like concepts, have to earn their way, otherwise they are speculative or simply idle banter. This resonates with DEWEY's view of knowledge as instrumental, so that theories are best seen as tools. Some tools are highly specific to a task, others are more generally adaptable. RORTY (1998, Chapter 1) extends and clarifies this by arguing that just as there is no universal tool, so too there are no universally applicable theories. (Not even the most expensive conceptual Swiss Army knife!) Thus the theory of evolution, just like the theory of relativity, works and has grab and is useful. Those arguing in favour of alternatives need to demonstrate that their options do likewise. It is pointless simply arguing that the theory of evolution is based on "facts" or "data", while ID is merely based on belief. It is far better to use the instrumental approach to theories, and so be able to point to the ways in which the theory of evolution has earned its keep. [76]

This can also be used as a criterion for theories which are now seen as incorrect or surpassed in some manner. For instance many of NEWTON's theories were displaced by EINSTEIN's, but they certainly proved their worth by providing a basis for many aspects of scientific work and engineering since their inception. By adopting this Pragmatist position one can appreciate why some theories do indeed earn their keep, without recourse to claims about universality, truth, science, and facts. The kernel of this is also at the heart of GTM, although not spelled out in this manner. [77]

Theory for GTM then holds a central role and is a primary objective. But it is important that researchers understand that grounded theories are not meant to be either "speculative" (recall GLASER's admonition against immaculate conceptualisation), nor are they meant to be seen as universal explanations. On the contrary, researchers should aim to develop their own theories, based around central concepts that can be justified by the ways in which they can be shown to have been derived from the iterations between engagement with the research context and conceptual analysis. Claims for extension or application to other contexts may then come at later stages, but even if these cannot be borne out, the initial theory should still retain its *grab* and *fit*.¹⁹ [78]

19 GTM does raise an interesting distinction between theories and concepts, but this also applies to other theories; the theory of evolution, the theory of relativity, each is focused on a central concept—plus other associated concepts—which taken together afford ways of understanding the world and acting with and within it. STRAUSS himself makes an important point in QA when he mentions the ways in which he and GLASER developed some of their early work on dying. They found that there were two pivotal concepts; awareness and trajectory, but rather than

3.6 Practice and practice-led disciplines

The importance of GTM for practice-led disciplines has always been seen as a key strength of the method. The early GTM research was carried out in medical institutions, partly because STRAUSS had by that time (early 1960s) moved to the School of Nursing at UCSF, where he founded the Department of Social and Behavioral Sciences, and also as a result of the personal experiences of bereavement in medically-controlled contexts that GLASER and STRAUSS had each, separately, encountered in the early 1960s. But these factors alone cannot account for the way in which the method was taken up so widely and enthusiastically by practitioners in a wide variety of medically-oriented practices. It is often forgotten that GTM had three progenitors, the forgotten one being Jeanne QUINT (later Jeanne QUINT BENOLIEL) a professional nurse who joined with GLASER and STRAUSS in their earliest GTM studies (see QUINT, 1967). [79]

QUINT later achieved renown in her chosen profession, and it seems reasonable to suggest that GTM was from the outset oriented towards practice as a result both of her contribution and the fact that GLASER and STRAUSS themselves were keen not only to outline an academically-oriented innovative research method, but also to ground their ideas in concrete practices. The initial studies were embedded in the context of caring for the terminally ill, and there is clear continuity in the later work at UCSF with its orientation towards performance-led research in the general area of medical and social care. [80]

From the perspective of Pragmatism, the issue of the relationship between theory and practice is one of how useful the former is with regard to the latter; theories are simply judged in terms of their utility. If a new theory has no impact on existing practices then we are in the realm of DEWEY's difference maxim—any dispute between proponents of the old theory and the new one is not of any practical concern; although they may prove to be important at a later stage. New theoretical insights, whether in the form of grand theories, conceptual models or some such, need to be judged in terms of the differences they make to people's practical understanding and actions—hence STRAUSS' continual interest in theories of action and interaction in social settings. [81]

3.7 Data, induction, and deduction

As has been stressed throughout the foregoing, it is crucial that researchers understand that they inevitably take an active role in the process of "data gathering"; this is not simply a matter of harvesting something that is naturally occurring. As STRÜBING (2007) argues, citing JAMES and MEAD, for Pragmatists reality is something that is always in the making, and data is something that social actors "carve out" from reality; an activity that is socially located, not simply an individual, isolated accomplishment. GLASER and STRAUSS' early work, and even their later, individual writings do not make this

seeking to tie them together, each one became a central focus for a separate study and grounded theory—*Awareness* and *Time*. So the theory around each was then pitched at a higher level of abstraction than the concept.

clear in any manner: Continued use of the metaphor of emergence, or the passive voice that effaces the active researcher, are misleading (see Section 3.4 above). This is not to imply that the Pragmatist argument can go by default, in a fashion similar to the more general claims about constructivism and interpretivism, these must be clearly stated and justified: but so too must any alternative or converse one of realism, positivism or post-positivism. This involves dealing with the issue of the active researcher, or perhaps to borrow GLASER's excellent point about gerunds, active *researching*. It also involves dealing with the issues of data, induction, deduction, and abduction. [82]

GTM has from the start been associated with induction; largely this was self-professed by GLASER and STRAUSS as part of their reaction against the deductive approach prevalent in the 1960s. The GTM concept of induction is firmly wedded to that of data, and the *grounding* of any resulting conceptual models in that data as a result of an inductive process. The GTM context of induction manages to evade the usual problem of what is termed "the riddle of induction", since the resulting theory or set of concepts are substantive—i.e. they resonate with the context from which they have been derived, but do not make any wider claims as such. In this sense they have earned their way, but only up to a point. [83]

The riddle of induction is often exemplified in terms of swans (POPPER, 1961). Anyone observing swans in the Northern hemisphere until the 17th century would have noticed that they were all white; so there seemed no reason to doubt the validity of the statement that "all swans are white". It was only when Australia was explored by Europeans that people in the Northern hemisphere realised that black swans existed; hence the title of TALEB's recent book *The Black Swan* (2008), which deals with the inherently unpredictable. While it is understandable that GLASER and STRAUSS sought to distance their method from the deductive orthodoxy of the times, continuing to refer to GTM as an inductive method has come to be misleading, particularly for early-career researchers with only a limited understanding of terms such as induction and deduction. [84]

GLASER and STRAUSS' use of the term induction is best seen as idiosyncratic, and GLASER's continued use as distinctive and different from the issue as outlined in standard philosophy texts. This is bound up with his assumptions about data, which is still a major issue for GTM, as it is for many other subject areas, disciplines and methods. For GTM too many people take GLASER's slogan "All is data" (see Section 2.4) to mean that "Data is all". GLASER is correct to stress that GTM is first-and-foremost a method that starts from in-depth engagement with the research context, and this involves developing data, evidence, or whatever term is preferred. But this raises a number of issues that then need to be clarified, even if they cannot be easily resolved. [85]

Once the problematic issue of data is raised it is not possible simply to retreat behind the claim that "it's all there in the data". To an extent GTM avoids this with the concept of substantive theory, which makes a far more constrained knowledge claim; substantive grounded theories are theories about the

substantive context from which they have been drawn. They are the classic exemplars of theories of the middle range. But the data upon which they have been grounded can themselves be brought to account by others looking at the same context from a different perspective. STRAUSS' use of the term "slices of data" (1987, p.27) can be turned back on itself by wondering how the data could have been sliced in a different way; but STRAUSS himself fails to pose this. Instead for STRAUSS and CORBIN, as for GLASER, data is an unexamined, even *immaculate* concept. [86]

This is particularly perplexing with respect to STRAUSS given his detailed knowledge of DEWEY and MEAD; but there is an even more critical silence in his work with regard to the issues of induction and deduction. In *QA* STRAUSS did distance himself from sole reliance on induction, particularly from the rather vague form mentioned in *Discovery* and the other early GTM texts. He refers to verification in his initial outline of GTM, explicitly countering the charge that GTM is an inductive method; then expands on this by arguing that GMT starts out as inductive, but should then lead to the production of hypotheses that can subsequently be verified based on deductive inferences. He adds a footnote (1987, p.12) to the effect that PEIRCE also discussed the concept of abduction, but STRAUSS takes this no further. It is unfortunate that he offers nothing more detailed, since PEIRCE's concept of abduction seems to fit so well with the agenda of GTM—centring around novel insights leading to conceptual and theoretical innovations. [87]

3.8 Abduction and theoretical sensitivity

It might be countered that PEIRCE's concept of abduction is merely another name for what GLASER terms "immaculate conceptualization" or POPPER's idea of "conjecture"; but it might just as easily be seen as related to the concept of theoretical sensitivity, which is so central to GTM that it is perhaps the pivotal point from which all forms of GTM develop. It is crucial to the early collaborative work of GLASER and STRAUSS, it is intimately associated with all GLASER's writing, and STRAUSS makes it the central point of *QA*—which is one of GLASER's key criticisms in his response to *BQR*. In the earlier sections I have argued that the repeated statements about GTM being an inductive method are highly misleading; not least because they rely on a largely unexamined, even ungrounded, characterisation of both induction and data. The corollary of this is that there is a far firmer basis for associating GTM with abduction, specifically through the concept of theoretical sensitivity. [88]

As we noted in the *Handbook* (BRYANT & CHARMAZ, 2007a) this latter term itself is elusive, but critical. Judging by their own writings and comments from their students and colleagues, both GLASER and STRAUSS possessed remarkable theoretical sensitivity—and GLASER continues to demonstrate this in his seminars. REICHERTZ (2007) argues that the term abduction combines the rational and the imaginative aspects of research, and this is precisely what theoretical sensitivity is meant to encompass. GLASER's characterisation of the term "theoretical sensitivity" (1978), as being "sensitive to theoretical issues while

scrutinizing the data", only goes so far; particularly since both the terms "theoretical issues" and "data" are ambiguous—and the former is itself the subject of the characterisation. GLASER could justifiably contend that the richness of the issue cannot be encapsulated in a single phrase, hence his entire book on the topic; one which should be compulsory reading for all researchers, not only those interested in using GTM. But this only reinforces the argument that the term is so important that it cannot be left floating free, linked only to an idiosyncratic and misleading concept of induction. [89]

REICHERTZ (2007) makes the point even more forcefully in discussing the necessity for understanding theoretical sensitivity as a form of abduction, since the result is to bring together the logic of discovery with the logic of justification within the context of methodological considerations. By so doing it alerts researchers to a key issue that should be central for all researchers, but perhaps applies most particularly to those undertaking qualitative research: Some aspects of research really do depend on the skills of the specific researcher; methods alone are necessary but not sufficient. Researching is not simply the case of collecting data or evidence, the researcher is a key factor in the research landscape, a link in the chain that reaches iteratively around data, codes, concepts and tentative theories. Again GLASER's gerund perspective (1996) needs stressing here; it is the activity of *theorising* that is crucial, and it is also crucial who is doing the theorising.²⁰ [90]

CHARMAZ offers a definition of abduction as follows—

"a type of reasoning that begins by examining data and after scrutiny of these data, entertains all possible explanations for the observed data, and then forms hypotheses to confirm or disconfirm until the researcher arrives at the most plausible interpretation of the observed data" (2006, p.188). [91]

REICHERTZ offers another—

"Something unintelligible is discovered in the data, and on the basis of the mental design of a *new* rule, the rule is discovered or invented and, at the same time, it also becomes clear what the case is. The logical form of this operation is that of abduction. Here one has decided (with whatever degree of awareness and for whatever reasons) no longer to adhere to the conventional view of things" (2007, p.219). [92]

REICHERTZ notes that the term itself was first used by Julius PACIUS in the late 16th century as a translation of ARISTOTLE's term *apagoge*, which referred to the third form of inference after *induction* and *deduction*. But it was PEIRCE who really brought it to people's attention in his work some 300 years later. PEIRCE was intent to argue that abduction was the only form of inference that actually extended knowledge, since both induction and deduction rely on developing existing knowledge. As REICHERTZ points out, PEIRCE changed his ideas on

²⁰ Interestingly *BQR* has an index entry for "theorizing"; *QA* does not; and most of GLASER's books have no index at all.

the relationship between the three forms of inference in his writings, but he always saw abduction as a form of reasoning that develops from surprise or sudden flashes of insight. [93]

There seem to be at least two ways in which abduction operates in generating new knowledge. In the first instance it is very much like POPPER's idea of a conjecture, and in a fashion similar to POPPER (1992), PEIRCE argued that after such a guess or conjecture had been made, it should then be followed by generating and testing hypotheses. The second way, however, is the one that CHARMAZ and REICHERTZ characterise; in this case it is far more grounded and linked to data, observations, or other forms of evidence. To this extent it resonates with induction. Induction on its own, however, does not lead to new knowledge, but rather a reformulation of what is already known. As REICHERTZ points out, GTM is not simply inductive as this would merely lead to a re-casting of the data "on a more abstract level" (2007, p.223). The added aspect, which takes GTM into the abductive realm is theoretical sensitivity. [94]

In QA theoretical sensitivity is defined as "sensitivity to thinking about data in theoretical terms" (STRAUSS, 1987, p.21). Later on STRAUSS made the telling point that in order to develop theoretical sensitivity "wide reading in the literature in one's field and related disciplines is very useful, and probably a prerequisite" (STRAUSS, 1987, p.300). The later BQR has no mention of the term, but only for "sensitivity". As was mentioned earlier large sections of QA were taken verbatim from GLASER's book *Theoretical Sensitivity (TS)*. GLASER himself in his critique of STRAUSS, particularly BQR, argues that STRAUSS ignores the essential meaning of both *Discovery* and *TS*. For GLASER theoretical sensitivity "refers to the researcher's knowledge, understanding, and skill, which foster his generation of categories and properties" (1992, p.27). [95]

As we pointed out in our introduction to the *Handbook* (BRYANT & CHARMAZ, 2007a) the concept of theoretical sensitivity is central but paradoxical to GTM—we discuss it as one of those aspects of the method that we describe as being seen as "simple yet skilful" (p.16). The paradox resides in the importance placed on such sensitivity, coupled with the difficulty in developing this skill. Theoretical sensitivity is critical to GTM, but describing theoretical sensitivity or explaining how one can develop this skill is not easy. Yet far too many GTM writers argue that the method is simple or straightforward, particularly when it comes to this aspect. Anyone who has taught and/or used GTM will know that this is not helpful; hence HESSE-BIBER notes that one of her students pointed out that one of the most difficult skills "is the ability to see what is in the data" (2007, p.330). [96]

Students and others who worked with STRAUSS remark on his powers of insight. Similarly STRAUSS himself was so impressed with GLASER's skills that he immediately engaged him. What may have come easily and naturally to this pair was what PEIRCE would have termed abductive reasoning; the ability to infer the best possible explanation from the data in whatever form it is presented. The contribution of GLASER and STRAUSS was not simply that they had these skills, but that they then managed to develop a detailed account and exemplars of what

was involved in this skill that would assist others to follow in their steps. But it is not something that is "simple and straightforward", although use of fundamental GTM aspects can greatly enhance a researcher's abilities in extending knowledge in PEIRCE's sense. [97]

In fact the dispute between GLASER and STRAUSS can be seen to emanate in part from precisely this issue. The early GTM works demonstrated the power of the method, but something more was required in terms of explaining the method to the research community and to budding research students. *Discovery* itself was more manifesto than methods manual (see Section 1.1), and the succinct Appendix to *Time* was far more useful in the early stages as a *how-to* guide to the method. In the late 1960s and early 1970s use of GTM spread mostly through the face-to-face teaching that STRAUSS and GLASER offered to the students on the doctoral program at UCSF; something we termed "Masters and Apprentices" (BRYANT & CHARMAZ, 2007a, p.4). But as news and use of the method spread, there was a clamour for a more detailed guide to GTM. Strauss claimed that he wrote *QA* precisely in response to this continuous demand from students. The later work, *BQR*, came about when readers pointed out that *QA* itself assumed too much knowledge of the method from the start. GLASER's attack on both books centred on the contrast between what he termed "emergence" versus "forcing" (1992); and perhaps it was inevitable that any effort to offer a step-by-step guide to GTM would be open to this sort of criticism. But it can be argued that GLASER himself also offered similar guidelines to students of GTM in the form of his "Basic Social Processes" and "Theoretical Codes" (1978, chapters 4 & 6). He stressed that these were only to be used at later stages of analysis, but then so too did STRAUSS and CORBIN with their coding paradigm. In his writings since the late 1990s, GLASER has made little or no mention of these factors, which perhaps indicates that he has had second thoughts on the matter. It may be that STRAUSS would have similarly sought to move on from *BQR*, although the 2nd edition was well under way even before he died. There is clearly a constant and incessant demand for a guide to GTM, hence the appearance of a 3rd edition of *BQR*; in the library at my university there are 10+ copies of the book, and at least nine of these are on loan at any one time. [98]

Theoretical sensitivity is a core and critical aspect of GTM, and linking it to abduction helps to illuminate its characteristics and its role. It remains at the centre of attention for GLASER, although it appears to diminish in importance for STRAUSS and CORBIN. CHARMAZ provides an excellent discussion in her book (2006, pp.135-140), where she sees the terms as evoking something "playful" as opposed to a mechanical process of reasoning: again a clear link to abduction. [99]

Yet again it seems as if STRAUSS set his writings in GTM and his other works on parallel courses that never met. His stress on action and the thin red line of Pragmatism that runs through the non-GTM works are never brought to bear on his later GTM writings. He specifically, if briefly, addresses the issues of induction in *BQR*, but fails to develop the idea of abduction that seems so useful and applicable. [100]

4. A Re-Invigoration of GTM through Pragmatism

What I have sought to offer is an explanation of the ways in which Pragmatism can resolve and clarify many of the key issues that have arisen from various critiques and developments of GTM. This builds on work that others have already started linking GTM with Pragmatism, providing the basis to re-state and highlight ways in which the method can be further articulated and applied. [101]

By adopting a Pragmatist perspective on GTM a number of problematic aspects of the method can be resolved; moreover the core strengths of the method come into clearer focus. First and foremost many of the issues separating the different writers on GTM can be cast aside. Thus whether researchers see themselves following GLASER, or STRAUSS (and CORBIN), or BRYANT and CHARMAZ, or any other variants, the key issue becomes the extent to which their substantive research produces conceptual innovations and theoretical insights that prove useful: this is to reiterate DEWEY's point about Instrumentalism. The epistemological issues that separate different strands, or branches of the GTM family, can then be set to one side provided that people's research writings do not seek to make strong epistemological claims; the ultimate criterion of good research should be that it makes a difference. [102]

This then reinforces the Pragmatist position that theories and concepts are best seen as tools; tools are assessed in terms of usefulness for particular tasks and applications. This can similarly be applied to methods, and GTM can then be assessed in terms of the concepts and theories that have been developed through use of the method; whichever version that has been adopted. [103]

Furthermore the Pragmatist position on truth also highlights the stress put on the development of concepts and theories by GTM. These outcomes are to be judged against the context from which they were developed, and can then be taken as working hypotheses or theories for potential extensibility to other settings. They can also be taken back to the initial context and used to inform practices, procedures and policies: Hence the strong tradition of GTM amongst practice-led disciplines. [104]

All of this taken together helps substantiate the way in which terms such as *fit*, *grab*, *work*, and *modifiability*, should be understood as part of the method, and can be applied to the method itself. All claims to knowledge must be seen as instrumental and provisional. Researchers can make claims for their own findings, but further developments and wider claims rely on a range of possible sources including further work by the researcher, additional research by others, and responses from those who were active in the initial research context itself. This last point is not explicit in all forms of GTM, but the constructivist form emphasises this unequivocally. This is also supported by the Pragmatist assertion that any attempt to provide clear demarcations between lay insight and expert knowledge will ultimately prove unsustainable. [105]

One aspect in which the Pragmatist orientation might prove troublesome to some forms of GTM is the rejection of the spectator view of knowledge. For Pragmatists, developing ideas about the world is an activity which can never be completed, the focus is on *knowing* rather than knowledge. Thus researchers need to be aware of their own role and position in the activity of researching, and this brings in aspects such as positionality, orientation, diversity and reflexivity (see MRUCK & MEY, 2007 for an extended discussion). But those who refuse to entertain such issues in their work, or who claim that GTM can deal with all such perspectives without specifically addressing them as such, can also fall back upon the Pragmatist position that since all knowledge claims are provisional and open to doubt, so too are those which argue for specific inclusion of such issues. The only judgement to be made is with regard to the usefulness of the conceptual or theoretical outcome. [106]

In fact this positionality applies to GTM itself, as GLASER readily attests in *Theoretical Sensitivity* where he stresses that "Our perspective [i.e. GLASER and STRAUSS' GTM] is but a piece of a myriad of action in Sociology, not the only, right action" (1978, p.3). [107]

What is often missing, or implicit, in GTM writings is the context in which researchers operate. The early GTM work was carried out by GLASER, STRAUSS and QUINT, and later projects were carried out by STRAUSS and small teams of researchers (STRAUSS et al., 1985). Moreover research findings are reported in journals, conferences and so on. So there is always a community or audience for research at some stage. On the other hand researchers studying for a PhD are usually lone researchers, and so it is not surprising that much of the GTM literature appears aimed at this audience. But ultimately the reception and assessment of people's research goes on within a community, with an audience of peers as well as the ineluctable gatekeepers. It may also involve practitioners, research subjects and other participants. RORTY in typically provocative mood sums this up with the apothegm that "what counts as an accurate report of experience is a matter of what a community will let you get away with" (2007, p.11). These consensual and collegial aspects are all too often left implicit in GTM writings, but the Pragmatist stance helps correct this since it emanates precisely from a concern with knowledge as a continuous social activity. [108]

All of the above demonstrates that those who have already established the links between GTM and Pragmatism have opened up an important path for developing the method. If Pragmatism was a red thread running through STRAUSS' work, it became invisible in the context of his GTM writing. This was unfortunate since it meant that he was unable to articulate how the ideas of DEWEY, MEAD and others come to a new fruition in the method itself. The result was that STRAUSS' own version of GTM was left open to a wide range of criticisms, including GLASER's; many of them entirely justified. More critically it left the method itself open to a range of criticisms from which it has only recently begun to be extricated. A consideration of the strange case of ANSELM STRAUSS along the lines suggested should continue the work of those who have rediscovered the link

between the method and Pragmatism, re-evaluate the work of STRAUSS as a whole, and impel GTM forward as a rigorous, vigorous, and developing research tool. This paper is offered as a further step in that advance. [109]

As I explained in the opening section of this paper, I took the subtitle of this paper from a Sherlock Holmes story. In this concluding section I can again refer to Sherlock Holmes as a paradigm example of someone with supreme abductive skills and theoretical sensitivity—a grounded theorist before the discovery of grounded theory: a skill he shares with other real or fictional detectives, and also with people such as crash investigators, physicians, and others who have the ability to produce a diagnosis, often to the surprise of others, but which is based on a leap from the evidence or data to the inference of the best possible explanation. [110]

What GTM and Pragmatism have in common is a concern with people's engagement with the world, reliant on detailed observation and insight, followed by never-ending and iterative efforts to comprehend, persuade and enhance. To quote one of Oscar WILDE's aphorisms: "It is only shallow people who do not judge by appearances. The true mystery of the world is the visible, not the invisible".²¹ [111]

5. A Note on Pragmatism, Post-Modernism and Relativism

One of the key problems with constructivism is that it can easily end up as a full blown relativism or post-modernism. In such cases anyone proposing such a viewpoint is quickly seen to be in a paradoxical position. The claim that there is no objective ground for truth, and that all truth claims are contextually specific is itself a truth claim, and hence it is open to its own critique. RORTY offers a clear way out of this, specifically drawing on DEWEY and his contemporaries; but offering his own style of argument. RORTY's key concern is to counter all forms of foundationalism; i.e. all forms of argument that are premised on there being an ultimate foundation for knowledge claims—e.g. "the truth" or "objective reality". Those proposing post-modernism or relativism are correct to point to the weaknesses in the arguments of those who simply presume a foundation for knowledge claims, usually premised on the idea that truth is a reflection of reality or "a mirror of nature"; but they then fail to see their own foundations. Pragmatism avoids this, particularly in the work of RORTY, who credits DEWEY and his colleagues with a great deal, although it is RORTY himself who is responsible for articulating a coherent argument along these lines, steering away from relativism and the paradox of constructivism. In his critique of SEARLE, who desperately wishes to retain some form of "objective truth and validity" RORTY offers the following.

"What we say is that you gain nothing for the pursuit of such truth by talking about mind dependence or mind independence of reality. All there is to talk about are the procedures we use for bringing about agreement among inquirers" (1998, p.72). [112]

21 http://thinkexist.com/quotation/it_is_only_shallow_people_who_do_not_judge_by/262237.html

Furthermore RORTY is adamant that attention to such procedures only has one objective—and it is worth giving the full quote. "Sociologists and psychologists might stop asking themselves whether they are following rigorous scientific procedures and start asking themselves whether they have any suggestions to make to their fellow citizens about how our lives, or our institutions, should be changed" (1998, p.70). In other words how do their suggestions add up in terms of civic fit and grab and earning their way; GTM already has this idea at its core. Recent developments around GTM are rediscovering this, and highlighting that the purpose of research is to perpetuate DEWEY's concept of knowledge as a conversation. Conversations do not reach an end point, but continue as the context demands and as new contexts and participants appear. STRAUSS makes this point early on in QA, and in general talks of theory as an ongoing process; GLASER similarly sees theorising as a perpetual activity, with knowledge claims being at best provisional: So here there is a clear consensual basis shared by the founders of the method itself. [113]

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