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Continual Permutations of Misunderstanding

The Curious Incidents of the Grounded Theory Method

ABSTRACT

This paper draws attention to the ways in which Grounded Theory [GTM] continues to be a target of criticism, misunderstanding, and ill-judgement more than 50 years after its first appearance. This disparagement originates in part from some key paradoxes in the method itself. Yet this is insignificant in contrast to the continuing antagonism emanating from outside the method, indicating critical limitations in the practices of the gatekeepers of the academic world. GTM seems to be the target of *continual permutations of misunderstanding*, and it is these that I wish to address, and I hope, dispel in what follows.

INTRODUCTION

Several years ago, I wrote a paper concerned with Anselm Strauss and Pragmatism; 'The Curious Case of Anselm Strauss' (Bryant, 2009). The title itself was derived from one of Sir Arthur Conan Doyle's Sherlock Holmes stories, 'The Adventure of Silver Blaze' (Conan Doyle, 1892/1993). It includes the following exchange between Sherlock Holmes and the Scotland Yard detective Inspector Gregory:

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

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

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

Sherlock Holmes: "That was the curious incident."

The importance of Holmes's observation is that often it is what does not occur that is important, rather than what actually happened. My article highlighted the curious case of Anselm Strauss, and the fact that he never drew explicit attention to the way in which Pragmatism informed Grounded Theory. Strauss's writings on Grounded Theory, including those with Barney Glaser (e.g. Glaser and Strauss, 1965b, 1967, 1968), and those with Julie Corbin (e.g. 1990, 1996), make little or no mention of Pragmatism. Yet Strauss was well versed in the work of Charles Sanders Peirce, William James, and John Dewey (Peirce, 1998, Dewey,

1929 & 1938, James, 2000), the founding figures of Pragmatism; initially through having been taught by Floyd House, then later as a key member of The Chicago School of Sociology, where the Pragmatist influence had come via G. H. Mead (1934).

Late in life, however, Strauss clearly felt the need to acknowledge this influence, pointing out in the opening chapter of his final book *Continual Permutations of Action* (1993) that Pragmatism was 'a red thread running through my work'. The influence of Pragmatism on the Grounded Theory Method [GTM] has been far more widely acknowledged since the 1990s, building on the work of those schooled in the German-speaking tradition of GTM initiated by Strauss in the 1970s, and continued by key grounded theorists such as Udo Kelle (2019), Jo Reichertz (2007, 2019), and Jörg Strübing (2007, 2019). Strauss's general silence on the issue, however, remains a mystery.

Yet there is a further type of 'curious incident' to which I wish to draw attention. One that refers to a persistent presence rather than an absence or non-appearance; namely the ways in which GTM continues to be a target of criticism, misunderstanding, and ill-judgement more than 50 years after its first appearance.

The continual disparagement of GTM originates in part, but only in part, from some key paradoxes in the method itself, many of which have only become fully apparent and widely understood since the late 1990s and beyond. Some of these criticisms are evidence of the significant innovations and challenges inherent and intrinsic to the method itself. After all challenges to the status quo usually arouse some degree of resistance and resentment. Consequently, there is some basis to regard the failure to appreciate the full value of GTM as arising in part from ambiguities and paradoxes in the method itself. Yet this is insignificant in contrast to the continuing antagonism emanating from outside the method, indicating some critical limitations in the practices of the gatekeepers of the academic world, including journal editors and reviewers, doctoral committees, research evaluators and funders. In general, GTM seems to be the target of *continual permutations of misunderstanding*, and it is these that I wish to address, and I hope, dispel in what follows.

The origins of GTM

Grounded Theory first appeared in 1965 and was initially called 'Substantive Theory' (Glaser and Strauss, 1965a)¹. It was then articulated and exemplified in the founding trilogy *Awareness of Dying*, *Discovery of Grounded Theory*, and *Time for Dying* (Glaser and Strauss, 1965b, 1967, 1968), and further developed in the late 1960s and 1970s with works such as *Status Passage*, *Anguish* (1970, 1971), and *Theoretical Sensitivity* (Glaser, 1978).

Yet widespread use of the method only developed sometime later, in the 1980s and early 1990s, initially with publication of Strauss's *Qualitative Analysis for Social Scientists* (Strauss, 1987), which incorporated a large verbatim section from Glaser's *Theoretical Sensitivity* (Glaser, 1978). Strauss and Corbin's *Basics of Qualitative Research* (1990) followed soon after.

So initially knowledge and use of GTM was restricted to a small and highly specialized group of people associated with the research projects and PhD program that Strauss had established at the University of California San Francisco [UCSF]. Strauss had taken up a professorial post there in the 1960s at the invitation of Helen Nahm, to whom *Time for Dying* is dedicated. Previously he was a key figure in Chicago, spanning both the early and the late Chicago Schools of Sociology. He was certainly a key figure, with an extensive reputation particularly in the USA and Germany. At UCSF his doctoral program was a key proving ground for GTM, but Kathy Charmaz – who was in the very first intake to the program – has told me that only one or two of the initial cohort used GTM for their theses. Hers was one of them; a particularly notable one, introducing the concept of *supernormalizing* to the sociological and health-care lexicon (Charmaz, 1990).

Strauss also taught and made contact with sociologists and social psychologists in Germany. He held several visiting posts around the world in the 1950s, but his first visit to Germany after the publication of *Discovery* was in 1975, when he was invited, by Thomas Luckmann, to spend time at the University of Konstanz. Luckmann was the co-author, together with Peter Berger, of one of the most highly regarded sociology texts of the late 20th century, *The Social Construction of Reality*, a key source of the constructivist view of knowledge (Berger & Luckmann, 1966). In Germany Strauss met, amongst others, Hans Georg

¹ The term 'Grounded Theory' does not appear in *Awareness*, in 1965. The theory of 'awareness of dying' is referred to as a 'substantive theory', and contrasted with a 'formal theory'. The first extensive use of the term 'Grounded Theory' is in *Discovery* in 1967.

Soeffner, who later spent time with Strauss at UCSF in the late 1980s and early 1990s; collaborating with Strauss and colleagues, including Susan Leigh Star and Adele Clarke, on a research project about 'illegals' in the Bay Area. In the 1980s Strauss visited Germany again, and produced a set of notes referred to in Germany as a *Study Letter*, that in effect was an outline of what later became *Qualitative Analysis for Social Scientists* (see Bryant, 2019).

Consequently, between the mid-1960s and the mid-1980s, knowledge and use of the grounded theory method was largely confined to these two highly specific and specialized groups. Yet by 1994 it was sufficiently well known and highly regarded for a chapter about the method to appear in the first edition of Denzin and Lincoln's *Handbook of Qualitative Research* (Denzin and Lincoln, 1994). The chapter was written by Strauss and Corbin who noted that 'it had taken more than 20 years for American sociologists to appreciate the *strong rationale* for qualitative research that lies at the heart of *Discovery*' (emphasis in original). On the other hand, GTM was now 'in vogue' and had 'diffused through the practices of academic research'. It had, in fact, become fashionable. This carried the risk that many researchers were claiming to use the method, but 'failing to accomplish more than a fairly mundane level of coding, certainly not moving on to theoretical coding, and subsequently to generating theoretical statements' (Strauss and Corbin, 1994).

To a large extent this important statement about GTM has been eclipsed by Kathy Charmaz's chapter in the 2nd edition (2000), where she distinguished between *constructivist* and *objectivist* forms of GTM (Charmaz, 2000), outlining the basis for what is now regarded as the *constructivist* variant of the method. The chapter in 1994 is notable, however, because it acknowledged the way in which the method had spread and influenced qualitative research practice in general. Strauss and Corbin argued that *Discovery* had 'redefined the usual scientific canons for the purposes of studying human behavior' (p. 274). The method offered a new rationale for research, drawing attention to differing 'criteria of judgment ... based ... on the detailed elements of the actual strategies used for collecting, coding, analyzing, and presenting data when generating theory' (p. 274, quoting from *Discovery*, p. 224).

On the other hand, they stressed that *Discovery* 'over-played' the role of induction in the method, which had led to significant misunderstandings both by users and critics. Certainly, induction was a key aspect, but it had to be supplemented with other factors, including recognition of the 'potential role of extant (grounded) theories [i.e. existing theories

– whether grounded nor not] and the unquestionable fact (and advantage) that trained researchers are theoretically sensitized’ (1994, p. 277).²

Their chapter refers to Pragmatism, and the influence of Dewey and Mead on the method, but does not develop this line of thought. There is also a hint at the issue of what they term ‘the interplay between the researcher and the actors studied ... [which] is likely to result in *some degree of reciprocal shaping*’ (emphasis added). This seems to acknowledge the interpretative or constructivist turn; something that Strauss would have known about both from his grounding in Chicago and from his contact with Luckmann.

Strauss and Corbin conclude their discussion, pointing out that ‘no inventor has permanent possession of the invention— certainly not even its name— and furthermore we would not wish to do so. No doubt we will always prefer the later versions of grounded theory that are closest to or elaborate our own, but a child once launched is very much subject to a combination of its origins and the evolving contingencies of life. Can it be otherwise with a methodology?’ (p.283)³

While Strauss and Corbin were adamant that GTM was highly innovative and had an important part to play in highlighting the rigour and relevance of qualitative research, they were also well aware of, and made reference to, the troublesome aspects that beset the method;

- It is extremely fashionable – which is at least as much a disadvantage as an advantage;
- It is open to misuse, particularly in the form of a failure to develop codes into useful conceptualizations;
- The issue of induction continues to be over-played and is far too readily invoked by researchers in their publications and other outputs;
- The method is far too easily misunderstood, misjudged, and criticized by gatekeepers;
- There are issues of ‘ownership’ and ‘proprietorship’ and intellectual property with regard to the method.⁴

² Theoretical sensitivity is discussed briefly in a later section.

³ Strauss unfortunately died in 1996, so there is no way of knowing how he would have reacted to Kathy Charmaz’s chapter in 2000, nor to the ways in which the method has developed in the 30+ years since his death. We do know how Glaser responded, amongst other things from the review he wrote for *FQS* (Glaser, 2004).

⁴ See below for an explanation of this last point.

Current status of GTM

GTM continues to be extremely popular and even *fashionable*. By most counts it is by far the most widely used qualitative method, and in some fields of research it dwarfs all other methods. But as Strauss and Corbin suspected, being popular has a severe downside. The GTM label is often adopted and displayed when it is unwarranted. I know of several researchers who when publishing their work were advised to add a section on their 'method'. When the researchers then said they were not sure what that involved, it was suggested that they should claim to have used GTM since that would allow them to describe more-or-less anything they wanted!

In contrast to this popularity, GTM also has a high degree of notoriety. Perhaps one arises from the other. Journal editors tell me that when they see submissions reporting on studies using GTM, their first and often final response is to reject the paper out of hand. They justify this by explaining that GTM papers usually fail to state any clear research question, offer little or no review of the literature, and simply present 'results' in the form of numerous, incoherent 'codes' linked to long verbatim extracts from a small sample of interviews.

They are surprised when I agree that any such work should indeed be rejected – politely rejected – but that similar or equivalent strictures also apply to a significant proportion of other submissions, using more *traditional* methods; including quantitative ones. These often open with one or more hypotheses – often stated in highly ambiguous or over-simplified language – followed by data and analyses that seem to have little or no connection to the hypotheses, and an even less convincing relationship to the supposed 'findings' or outcomes. With regard to quantitative research findings we all need to be aware of the phenomenon of *p-hacking* (see Head et al 2015). In general we constantly need to be on our guard when evaluating research findings whatever methods the researchers claim to have used.

The GTM mantra

Unfortunately, there is a large amount of poor and misleading research, often finding its way into the mass of peer-reviewed, published papers and articles with which we are continually confronted. In part this is a result of the enormous pressure on academics to pursue research projects and publish at all costs. Yet people's critical threshold for 'acceptability' seems to be far higher when it comes to GTM. GTM papers are often singled

out for disparagement and rejection, while others of similar status and quality are deemed adequate and satisfactory.

Such 'curious incidents' are all too common, with GTM attracting far more criticism and disapproval than any other method. In part this may be caused by what I have called the *GTM Mantra* (Bryant 2017), a collection of phrases and forms of words used, or perhaps we can say intoned, by many GTM researchers, as if doing so will in itself help produce insightful and useful results. The mantra consists of a series of potentially misleading and contentious statements that offer easy targets for criticism.

- All is data
- GTM is an inductive method
- The theory will emerge from the data
- Start research with no preconceptions
- Don't read the literature
- Use GTM where there is no existing research

I regard many of these statements as 'accidents' rather than 'essences' of GTM, a distinction that goes back to Aristotle (see Bryant, 2017, Chapter 4). For GTM the accidental aspects arose largely from the historical context from which the method arose, and the fact that *Discovery* was fundamentally a manifesto for a new and highly innovative method. Manifestos require easily memorable slogans – or mantras. These must encapsulate the distinctions between the old and the new, often in an exaggerated manner that can all too easily lead to caricature and misrepresentation. Over time, however, many such declarations lose their initial impact, and instead become encumbrances, distractions, and the basis for misunderstanding. This has proved to be the case for GTM as can be seen by considering each of the above statements in turn.

All is data – a form of words that is still widely used by many of those writing from various GTM standpoints. It seems to have originated with Glaser, and as it stands it can be a useful reminder to researchers to look beyond 'obvious' sources of data – e.g. interviews – and consider alternative or additional ones – documents, observations, published accounts, and digital resources. The problem, however, is that the recommendation is often taken to mean that 'data is all'; nothing else is needed. It also implies that what is meant by 'data' is simple and straightforward. The move away from what Charmaz termed 'objectivist' GTM

towards 'constructivist' GTM is founded on a more profound understanding of 'data', based on the work of constructivists or interpretivists dating from the 1960s.⁵

An inductive method – The claim that *GTM is an inductive method* was questionable already in 1994, but this is still quoted by students and other researchers. Charmaz has shared the anecdote about Strauss proclaiming to her in the late 1960s that GTM is an *abductive* method, something that has been articulated and widely appreciated by the German-speaking GTM community for far longer than it has been amongst the Anglophone community. Both *The Handbook of Grounded Theory* (Bryant and Charmaz, 2007a) and *The Handbook of Current Developments in Grounded Theory* (Bryant and Charmaz, 2019) include several chapters by key figures from the German-speaking GTM community, exemplifying this very clearly (see for instance Reichertz, 2007 & 2019; Strübing, 2007 & 2019; Flick, 2019; Kelle, 2019). Glaser and Strauss only make brief mention of induction in their early writing, but their claim that GTM 'is an inductive method of theory development' (1967, p. 114) has been taken up and propagated by large numbers of GTM researchers ever since. Given the way in which GTM was heralded as distinct from the orthodoxy of research centred on deriving hypotheses in a deductive manner from existing theories with a view to verification, it was not surprising that the inductive features of GTM were accentuated and over-emphasized. There is certainly an inductive strand to GTM, and Charmaz has always been keen to stress that the method features 'inductive data' with the aim of creating 'inductive categories'. The term 'inductive' in this context is intended to evoke the strategy of open-ended and exploratory investigation at the start of a research project, rather than using the term to characterize the logical process of developing concepts and theoretical insights, usually referred to as *inductive inference*. Glaser and Strauss's initial statement that GTM is 'an inductive method of *theory development*' is ambiguous at best and erroneous at worst. The development of theoretical insights and concepts using GTM is *abductive*, as Strauss recognized and proclaimed in the late 1960s. The erroneous claim regarding induction, however, leads to a further aspect of the mantra that continues to be widely proclaimed; the 'emergence' of theories from the data.⁶

⁵ Charmaz's chapter (2000) is a good starting point on this issue, also our chapter in the *Handbook*, (Bryant & Charmaz, 2007b). Her recent article on Social Justice Research and Critical Inquiry is an important updated and extended account (Charmaz, 2020).

⁶ The issue of abduction is not addressed here, but readers should refer to the work of Reichertz, 2007 & 2019; Strübing, 2007 & 2019; Flick, 2019; Kelle, 2019, as well as to Bryant, 2017, Chapter 13.

Theory 'emerges' from the data – One of the key problems with inductive inference as a logical process is referred to as 'the problem of induction'; i.e. the move from gathering data about specific instances which are then used as the basis for more general statements. The issue is one that has perplexed philosophers since at least the eighteenth century when David Hume raised it as a concern, and it continues to do so (Henderson, 2018). Consequently, it is unrealistic to expect non-philosophers to grapple with and clarify the issue; on the other hand the GTM claim that 'theory emerges from the data' is far too naïve and sets up an easy target for critics. For instance, Lööq Wacquant (2002) refers to the claim in arguing that GTM is founded upon 'an epistemological fairytale'.⁷ The claim derives from what Strauss and Corbin recognized as the over-playing of inductivism, since it implies that collecting data is more-or-less a necessary and sufficient condition for the development of grounded theories or concepts. Hence Wacquant refers to research strategies that operate on the inductivist idea, which he characterizes as the 'I-began-to-get-ideas-from-the-things-I-was-seeing-and-hearing-on-the-street approach to field-based inquiry.'

Taken at face value it is as if the researcher is merely an assistant or factotum; mindlessly collecting data. I am sure that, if pushed, people who refer to 'theories emerging from the data' would be all too keen to acknowledge the active role taken by GTM researchers in the development of their concepts, theories, models. But many of those who do stick to this formulation seem unable or unwilling to understand that the metaphor of theories emerging from the data is a dangerous and misleading one; that theories of all kind need to be understood to be 'constructed' rather than 'discovered'. Hence the title of Charmaz's book, *Constructing Grounded Theory* (2006, 2014), as opposed to Glaser and Strauss's, *The Discovery of Grounded Theory* (1967).

Since the 1990s, Kathy Charmaz and I, separately and in concert, have continually stressed that GTM needs to be de-coupled and disentangled from the positivist and naïve inductivism that runs through *Discovery* and other GTM writings, particularly those by Glaser. Overcoming positivism is not a once-and-for-all time activity; it rears its head in new guises such as neo-positivism, post-positivism, and various forms of realism. Critics need to

⁷ Wacquant clearly intends this as a slur on GTM, ignoring the many fairly-tales that are highly evocative and effective forms of insight. Wacquant's grasp of GTM is, however, flawed and inadequate; his criticisms are misdirected – see below.

understand and accept this, continually stressing what is, and what is not, encompassed by constructivism or interpretivism.

I find Richard Rorty's writings on this immensely persuasive and useful; particularly his very straightforward and direct distinction between the view that knowledge is 'discovered' on the one hand, and that it is 'created' on the other. This avoids the necessity to distinguish between the ever-increasing number of terms and labels used in much of the methods literature. Rorty's position can be summed up as follows: 'Truth is a property of sentences, since sentences are dependent for their existence upon vocabularies, and since vocabularies are made by human beings, so are truths.' In some of his later work he rephrased this in more provocative terms: 'Truth is what your contemporaries let you get away with.' (The quote is widely attributed to Rorty, and he certainly used it in lectures and seminars. The published version is as follows: 'I can sum up what I have been saying about appeals to experience as follows: experience gives us no way to drive a wedge between the cultural-political question of what we should talk about and the question of what really exists. For what counts as an accurate report of experience is a matter of what a community will let you get away with.' (2007, p. 11))

It is, however, important to understand what Rorty is *not* saying in this regard, since he, and by implication others of similar ilk, is often accused by critics of arguing that 'reality is the *product of consciousness*'. This is a common criticism of constructivism or interpretivism. For instance, Crispin Sartwell, who claims to have been a student of Rorty's, makes precisely this criticism in some recent writing.

Sartwell argues, contra Rorty, that:

...recent work in philosophy includes various forms of realism about the world: *the idea that reality is not the product of consciousness*, or of human perceptual structures or languages or interpretive communities, but exists independently. We don't make the world, as one might put it; the world makes us. (Sartwell, 2015, emphasis added)

By clear implication Sartwell accuses Rorty of arguing that 'reality is the *product of consciousness*'. This is absurd. In fact, it is such poor thinking that 'it is not even wrong' – a phrase coined by the physicist Wolfgang Pauli when commenting on work that was especially

ill-conceived. In *Contingency, Irony, and Solidarity* (1989), perhaps his most important book, Rorty explicitly argues that:

Truth cannot be out there—cannot exist independently of the human mind—because sentences cannot so exist, or be out there. *The world is out there, but descriptions of the world are not.* Only descriptions of the world can be true or false. The world on its own unaided by the describing activities of humans cannot. (Rorty, 1989, p. 4, emphasis added)

Rorty never denied the existence of reality, but he did repudiate the possibility of humans accessing or discussing reality in any non-linguistic fashion. One can only wonder how little Sartwell seems to have learned from what he claims to have been his close contact with Rorty, who many regard as the most important and most readable of 20th century philosophers (see Bryant, 2017, Chapter 17).

Whatever one's position might be regarding the sort of argument propounded by Rorty, a GTM researcher who simply asserts that 'their theory has emerged from the data' will surely increase the scepticism and disbelief already prevalent amongst editors, reviewers, and other gate-keepers.

Avoid all preconceptions – The issues of *inductivism* and *emergence* as expressed in many GTM texts provide tempting and easy targets for critics: so too does the instruction to *avoid all preconceptions*. In *Discovery* the position was unclear since in the text itself Glaser and Strauss stressed that GTM research should be undertaken without preconceptions, but then immediately included a footnote explaining that researchers never commence their work with a *tabula rasa*. In 1994 Strauss and Corbin quoted Ian Dey's witticism that 'an open mind is not the same as an empty head'.⁸ Yet innumerable GTM PhDs and research papers still include statements along the lines of 'I avoided all preconceptions'. Glaser has tempered this in some of his recent writing, now arguing that researchers' preconceptions should 'be suspended for the GT research'.

Keep in mind that preconceived concepts do not have to be forgotten. They are just to be *suspended* for the GT research so the researcher is open to the emergent. *Why let them get in the way?* (Glaser, 2012, emphasis added)

⁸ (1999, p.251) The phrase is also attributed to Edward Tufte

This does little to resolve the matter, and simply adds to the easy target for anyone looking for weaknesses in the method. Elsewhere (Bryant, 2019) I have discussed the largely inadvertent, but persuasive and decisive consequences of the metaphors we use when we talk about cognition; developing ideas expressed by Michael Reddy (1979). Given the prevalence of these issues, I am not singling out Glaser's position for specific criticism, other than to indicate that it exemplifies the ways in which cognitive metaphors can operate and mislead. Yet once the issue has been drawn to people's attention, it cannot be ignored.

The metaphors we use influence and direct the ways in which we think about things, with both positive and negative ramifications. Ridding ourselves of using metaphors is not possible; neither is it advisable, since the power of metaphors – by definition, opening up new ways of thinking and imagining – is important, particularly in research approaches aimed at articulating innovative and challenging conceptualizations. *Grounding theory in the data* is a very powerful, illuminating, and innovative metaphor!

Don't read the literature – When I sent some draft chapters of my book (Bryant, 2017) to people for comment, one response was that the issue of 'not reading the literature' was no longer relevant; people understood that this was neither possible nor workable. Yet researchers continue to be misled by this to the extent that people have contacted me to say that they had planned to use GTM but cannot as 'I already have familiarity with the literature'. I hope that eventually this 'accidental' advice from Glaser and Strauss will be forgotten – despite Glaser continuing advocacy. In summary, researchers cannot and should not ignore the relevant literature. All too often they are already immersed in it; that is why they are interested in the topic as a research project. Moreover, if they have not engaged with the literature, how can they claim that their research will make a contribution; they may simply be repeating work already done or failing to contend with the most recent developments in the field.

No existing research – Similar considerations also apply to the issue of using GTM where there has been no previous research. Claims along these lines might once have been credible, although of course proving a negative is highly complex; failure to find something is far more often a result of not looking hard enough or looking in the wrong places. In the age of Google this is almost impossible to demonstrate. Any online search will undermine any simple claim that there has been no sort of research on the planned topic or issue. It is unwise to set this as a criterion for using GTM. Again, the rationale for this in Glaser and Strauss's

early work is understandable; they were keen to persuade novice researchers to steer away from the well-ploughed furrows of existing theories and well-researched topics. This claim can no longer be justified, researchers need to explain the reasons for their choice of research topic without resorting to claims that are difficult or impossible to substantiate. Indeed, the issue of what motivates researchers is an important if largely overlooked issue in the research literature. Glaser and Strauss each had a clear and personal motivation for their initial research into death and dying: a recent parental bereavement – Glaser’s father and Strauss’s mother. Personal motivations do not in any way invalidate the research, on the contrary they may help to enhance the insight and overall findings. Researchers should not shy away from clearly stating their starting points for their research, including any personal motivations; leaving it to reviewers and readers to decide the extent, if any, to which these may have unduly influenced the outcomes.⁹

The GTM Mantra can be regarded as a significant and initially self-imposed target for criticism and misunderstanding. We can understand how it came about if we look at the context from which GTM ‘emerged’, a context in the USA where qualitative research was very much the poor relation in contrast with quantitative research. In the 1960s the dominant model in US social science was centred around derivation and verification of hypotheses from the theories of the ‘great men’ – and at that time they were all men. Glaser and Strauss described doctoral research students as the proletariat, tinkering with the theoretical capital handed down to them in the great academies. Collecting one’s own data, developing one’s own concepts, using innovative approaches to qualitative analysis, and making constrained theoretical claims were significant and radical challenges to the status quo. This required some level of exaggeration or over-playing of the characteristics of GTM, and it is unfortunate that although Strauss was aware of this to some extent in the 1990s, more nuanced and insightful understandings of GTM only began to develop some years later, dating from the 1990s; Charmaz’ work being the leading example. Moreover, such developments have themselves been continually criticized by other grounded theorists – particularly by Glaser himself. For instance, although he contributed a chapter to the Handbook in 2007, he later published an extended criticism of all but two of the chapters in the Handbook – his own and

⁹ Thornberg’s paper on ‘Informed Grounded Theory’ (2012) offers a clear and convincing argument correcting the last two points regarding the literature review and lack of any existing research. The term itself – *informed* GTM – should not, however be taken to imply that other variants are *uninformed*.

that of his close colleague Judith Holton – characterizing all the rest as ‘jargonizing’ GTM; i.e. using the vocabulary of the method but failing to understand or implement the method correctly (Glaser, 2009).

There is, therefore, some basis for people to misunderstand and distrust research claiming use of GTM, with some responsibility lying with GTM writers themselves. Yet for more than twenty years those intimately concerned with developing and teaching the method have offered persuasive clarifications and alternatives to the mantra, focusing on the ‘essences’ in contrast to the ‘accidents’ of GTM.

The essences of GTM

In earlier accounts I have described the variants of GTM as ‘a family of methods’, building on Wittgenstein’s idea of ‘family resemblances’ – i.e. the members are unique but can be grouped together because they share common features (Bryant, 2019). My summary of these common features is as follows:

1. *Coding-cum-analysis-cum-memoing* – The form and strategy for coding in GTM was innovative in many ways, including its starting point, and its iterative aspect. GTM coding eschewed prepared coding grids or guidelines, advocating a far more open-ended starting point.
2. *Memoing* - which was absent from the earliest writings but is now a key aspect of GTM.
3. *Substantive and formal theory generation* – this developed in part from Merton’s idea that research should aim at theories of the middle ground – both Substantive Grounded Theories and Formal Grounded Theories are examples of this.
4. *Purposive/convenience sampling followed by theoretical sampling* – Qualitative sampling was and still is misunderstood, particularly by those whose research experience and expertise is largely or wholly from a quantitative background. GTM offers a basis for clarifying sampling issues for qualitative research in general (see Morse and Clark, 2019).
5. *Theoretical saturation* – This is often seen as a weakness of GTM, but, used correctly, it is in fact a strength of the method, since unlike many methods, it offers the rationale for claiming to have reached an interim end-point for the research.
6. *Use of the relevant and appropriate literature* – Literature is used initially to establish the basis and justify the rationale for the research. Crucially, at later stages, researchers should have recourse to relevant and appropriate literature as an additional and critical form of data

against which interim and later analyses can be positioned. In GTM terms this is a feature of theoretical coding. The relevant and appropriate literature in later stages will often be substantially different from that used in the initial phase, as the research develops in unanticipated ways. (See Martin, 2019, Thornberg and Dunne, 2019, Charmaz, 2014, Chapter 11, and Bryant, 2017, Chapter 12, for discussion of the ways in which the literature can and should be incorporated with GTM; also Bryant, 2019 which includes a section headed *Don't call it 'a literature review'!*)

7. *Openness to serendipity* – Research can and should lead to unforeseen insights. GTM specifically stresses the need to be open to surprises and serendipity. As Einstein observed: 'If we knew what it was we were doing, it would not be called research, would it?'

8. *Quality Criteria* – Glaser and Strauss refer to fit, grab, work, modifiability. Charmaz has developed and reframed these as credibility, originality, resonance, usefulness (Charmaz, 2006, 2014).

9. *Pragmatism* – This is critical, particularly the ways in which GTM now has to be understood as a method for what I have termed 'enacting abstraction and abduction' (Bryant, 2017, Chapter 17).

Some of these are important for many other, if not all other, methods; but several are unique to GTM, and their combination is certainly specific to the method. More critically, taken together they embody a far more persuasive, coherent, rigorous, and effective method and rationale for undertaking qualitative research.

Charmaz has recently (2017) offered her account of the key *strategies* that 'form the core of grounded theory'. Given her status as the doyen of Constructivist GTM, I would urge readers to refer to her paper, noting that her discussion of what she term 'Continuities in Grounded Theory' emanates from a different orientation to my list of 'core features'. The two accounts are complementary. Charmaz's offers a more detailed explanation of the core *processes and procedures* of GTM, as well as some of the *products and presentation* aspects of the method.¹⁰ She presents this in order to provide guidance and clarification of GTM given that it is 'a contested method from within and without'. A brief and highly accessible overview is provided in Flick's recent *Doing Grounded Theory* (2018).

¹⁰ In other writings I have distinguished between ten aspects of methods, grouped in five group of two, all beginning with the letter 'P' – process and procedure is one such group, products and presentation is another – see Bryant, 2017, Chapter 2, including Table 2.3 & 2.4 pp.31-33.

The under-selling of GTM

If *Discovery* and the GTM Mantra can be seen as contributing to GTM being overplayed or over-sold, the method has also been 'under-sold', with many writers on GTM arguing that the method is 'simple and straightforward'. This partly explains the popularity of the method, as well as the extent to which it is so easily misapplied and misused.

Glaser and Strauss were clear from the outset that the method afforded novice and early-career researchers a basis for developing confidence in their own abilities to conceptualize; aiming to provide new conceptual insights, rather than toiling as underworkers in the well-trodden paths laid out by those more entrenched in the academy. Glaser has sustained this form of encouragement, arguing that GTM is 'just straightforward conceptualization integrated into theory' (Glaser, 2004, paragraph 41, emphasis added). He immediately follows this assertion, however, with a detailed characterization of theoretical sensitivity, defined as consisting of 'two essential characteristics [of the researcher] ... the personal and temperamental bent to maintain analytic distance, tolerate confusion and regression while remaining open, trusting to preconscious processing and to conceptual emergence'. (Glaser, 2004, paragraph 43).

There is an irony here, since what I would regard as Glaser's most important book on GTM – *Theoretical Sensitivity* (1978) – is centred on precisely the aspect of the method which much of his other writing seems to under-play or avoid; namely the critical role of the researcher in the progress from grounded data and codes, to forms of abstraction and conceptualization. In fact, Glaser himself recognizes and understands this since he refers to theoretical sensitivity as encompassing a range of skills or abilities – for instance, '... the ability to develop theoretical insight into the area of research combined with the ability to make something of these insights. ... the ability to conceptualize and organize, make abstract connections, visualize' (Glaser, 2004, paragraph 43).

Glaser and Strauss coined the term 'theoretical sensitivity' in their earliest writings on GTM. They defined it as the ability of a theorist to 'conceptualize and formulate a theory as it emerges from the data'. They argued that this skill has to be acquired and developed through practice and application, and is 'forever in continual development' (Glaser and Strauss, 1967, p. 46). The theoretically sensitive researcher 'thinks in theoretical terms about what he knows'; and also needs to exhibit 'two other characteristics. First, it involves his personal and

temperamental bent. Second, it involves the sociologist's ability to have theoretical insight into his area of research, combined with an ability to make something of his insights.¹¹

It may be that Glaser and Strauss, in an effort to encourage early-career researchers and others to develop their own insights, wanted theoretical sensitivity to be understood as something that does not require decades of experience to acquire. Yet this should not be seen as undermining the view that it is also a set of complex skills that require constant revision and enhancement, and which should never be taken for granted.

It is noteworthy, however, that in the paragraph immediately following this characterization of theoretical sensitivity, Glaser and Strauss stress that '[P]otential theoretical sensitivity is lost when the sociologist commits himself exclusively to one specific preconceived theory (e.g., formal organization) for then he becomes doctrinaire and can no longer "see around" either his pet theory or any other. He becomes insensitive, or even defensive'. So the skill is not only one of building up and mustering what they refer to as 'an armamentarium of categories and hypotheses on substantive and formal levels', but also encompasses being able to avoid the trap of seeing things in preconceived terms and categories.

Glaser has argued that not everyone is able to conceptualize in the manner required by GTM. Strauss and Corbin seem to have had similar thoughts, hence the point they make that many research findings, claiming to be the outcome of GTM, often fail to 'accomplish more than a fairly mundane level of coding', never moving on to theoretical coding and conceptualization. Again, the criticisms of the gatekeepers relating to GTM build upon paradoxes and ambiguities in the writings on GTM itself. The method is aimed at novice researchers, who can and should be encouraged to undertake their investigations without having to go through long and arduous processes of accreditation. At the same time, there is an all too common danger, not restricted to early career researchers by any means, of failing to develop the level of analysis beyond the mundane and merely descriptive.

From my own extensive experience with PhD students and other users of GTM, researchers understand this issue all too well, and can and do develop their theoretical sensitivity as they progress. The rich and profound accounts by four of my recent PhD

¹¹ Dating from the 1960s, the use of 'he', and 'his' would have gone largely unremarked, although it would clearly have jarred for many. I have not amended the extracts, but all pronouns should be understood in gender-neutral terms.

students (Bryant, 2017, Chapter 19) are eloquent examples of this. Yet I would also argue that theoretical sensitivity needs to be accompanied and reinforced with what I term *methodological sensitivity*, defined as the skill or aptitude required by researchers in selecting, combining, and employing methods, techniques, and tools in actual research situations (Bryant, 2017, p. 36).

Doctoral students, and others, when advised that GTM might be a suitable method worth some consideration for their research, are often sceptical since they find themselves confronted with the possibility of undertaking research that

- does not start from existing theories or widely regarded models and frameworks,
- does not involve collecting quantitative data,
- has a novel approach to coding and data gathering,
- incorporates complex but challenging and potentially fruitful ideas about engaging with the research context in different ways at different stages,
- involves confronting the literature in a fashion distinct from the usual 'literature review' at the start of the research.

Nevertheless they usually take up the challenge and in most cases are more than capable of understanding and responding to the demands that all of this places on them; in the process developing both their theoretical and methodological sensitivities.

Unfortunately others seem to have had different and discouraging experiences. Wu and Beaunae for instance refer to 'navigating the path of grounded theory' as 'a long walk through a dark forest' (2014). To a large extent their distress emanates from issues completely unconnected to GTM, but which are common to all researchers, particularly those early in their career and without employment security. They refer to 'coding, theory development and time constraints' (252) as the three main issues faced by GTM researchers. In coding, 'issues invariably arise for budding researchers'; 'theory development presents its own set of challenges', and 'the amount of time it takes to effectively complete a GT research study may be unreasonable for doctoral candidates' (252). Although one might sympathize with the authors to a limited extent, one might wonder why they ever thought a PhD would be devoid of 'issues' or challenges, straightforward, and quickly completed!

Wu and Beaunae's paper exemplifies a number of problems, none of which arises from GTM. These include failures on the part of their academic advisors, who seem to lack any understanding of GTM, which did not prevent them from advising and directing their

students. Also institutional issues emanating from the process of institutional review. All of this exacerbated by several crucial misunderstandings of GTM demonstrated by the authors themselves.

Accounts by doctoral researchers are an important and useful resource that needs to be enhanced and developed; not restricted to GTM, but encompassing all research strategies and methods. But it is unlikely that a similar submission would have been published if it had concerned any other method, particularly a quantitative one.

Unfortunately many academic gatekeepers do not seem as insightful as many early-career researchers. Hence the continuing and widespread failure by many to engage with and understand GTM research. Similar issues may well apply to other research methods, but in a far less widespread and pervasive manner. This is yet another aspect of the very curious ways in which GTM continues to confuse and confound so many authoritative people in the methods world, across a wide range of disciplines.

GTM and Social Science

A further curiosity of GTM is exemplified by two noted theorists, L iq Wacquant and Manuel Castells.

As I have already mentioned, in his extended review of three books on the urban environment, Wacquant (2002) described GTM as ‘an epistemological fairy-tale’, a description he applied to GTM and ‘diagnostic ethnography’. Interestingly he offers no references to GTM itself in the review, so it appears that he assumes his sociological readership will understand his target. Indeed, it seems that his criticism of the inductivism of GTM was clearly sufficiently well understood amongst sociologists in 2002 not to require further explanation. Perhaps the constructivist view of GTM had by then not filtered through to Wacquant, now a Professor at University of California, Berkeley, but who also worked at the University of Chicago earlier in his career. The full context of his comments, however, reveals that he is actually praising Mitchell Duneier, the author of one of the books under review, for what we would now understand as a form of abduction that is increasingly understood as a key aspect of GTM.

So he went about “fishing” for questions to which these informants might have answers. But his *problematic did not emerge inductively*, as in the epistemological fairy tale of “grounded theory” or “diagnostic ethnography”: it resulted from the

projection, onto the sidewalk, of Duneier's personal interest in morality and "respectability". Duneier must be given credit for the candor with which he acknowledges it: 'I hadn't formulated a precise research question. I had no theories that I wanted to test or reconstruct, and I didn't have a particular scholarly literature to which I knew I wanted to contribute. ... I sought mainly to diagnose the processes at work in this setting and *to explain the observed patterns of interactions of people*. I also have a general theme that guides me in collecting data *in all of my work: whether and how the people I am with are or are not struggling to live in accordance with standards of "moral" worth.*' (Wacquant, 2002, pp. 340–41; emphasis added)

Wacquant's short-hand dismissal of GTM is, therefore, actually made within the context of describing an approach that looks remarkably similar to a credible and well-crafted grounded theory study

- no precise research question,
- specifically avoiding verification of existing theories,
- deliberately not scouring the literature,
- and seeking to explain general patterns based on a general view of what was going on in specific urban settings.

Note also that the final part of Wacquant's commendation of Duneier's work clearly resonates with the ways in which a great deal of recent GMT research is intertwined with issues of social justice; an aspect that Charmaz has pioneered in recent years (Charmaz, 2011).

While praising Duneier's work, Wacquant criticizes one of the other books under review for artificially overlaying the 'narrative of deindustrialization and racial exclusion ... onto field descriptions', and with a low 'ratio of analysis to narrative and interview transcripts' – i.e. retrofitting the data to existing theories, with too much description and too little analysis; precisely what both Strauss and Corbin, and Glaser were pointing out in their writing in the 1990s.¹²

A further and related curious example is provided by Manuel Castells in his 2009 book *Communication Power*, where he refers to his work as a 'grounded theory of power', and

¹² NB The criticism that qualitative research often lacks analysis and fails to move beyond 'mere description' is paralleled in quantitative research by criticism of poorly defined hypotheses and failures to offer firm grounds for claims of statistical significance.

again in 2016 in his autobiographical article 'A Sociology of Power: My Intellectual Journey', which covers 'five decades of my academic life, from 1965 to 2015'. A journey which Castells claims 'highlights the common thread that brings together my intellectual project through a great diversity of topics: the *quest for a grounded theory of power.*' (Castells, 2016, p. 5, emphasis added)

In these writings, Castells brings to mind the dog that did not bark in the night-time. Despite using the specific phrase, a grounded theory of power, Castells fails to refer to Glaser and Strauss, Glaser, Strauss and Corbin, Charmaz, or any other GTM resources anywhere in either the book in 2009 or the article in 2016. The extensive bibliography (Castells, 2009) is devoid of references to any GTM writings. And in case this appears to be simply a one-off coincidence, in his 2016 autobiographical essay he refers to 'the embrace of grounded theory as a strategy of theory building', and 'my deliberate option to engage in grounded theory rather than in grand theory'.

Castells describes his approach as follows: 'I would start with theoretical constructs but always use them as research tools to be modified and systematized only in terms of their usefulness in the process of discovery' (2016, p. 4). This is not an indication of a clear or acceptable understanding of GTM in any form.

Castells' failure to include any references to accounts of GTM appears to be not simply an oversight, but rather the result of his having little or no understanding of the method itself. This is really bizarre and curious. If I wrote about the development of the digital age, including use of the phrase 'The Network Society', and did not refer to Castells' work,¹³ with at least a reasonable account of its key points, it would justifiably evoke severe criticism, accompanied by well-founded doubts about my academic credentials.

Wacquant and Castells, two leading and authoritative figures – candidates for potential inclusion in the ranks of what Glaser and Strauss referred to as the 'theoretical capitalists' – invoke the term 'grounded theory', but do not seem to understand what it involves. Wacquant, having criticised GTM, then goes on to praise the work of one writer, whose approach can be readily understood as GTM, even if the author himself, Duneier, was and probably continues to be unaware of this. Castells, on the other hand, introduces the

¹³ Notably Castells' trilogy *The Information Age: Economy, Society and Culture*. This comprises *The Rise of the Network Society* (1996, 2009), *The Power of Identity* (1997, 2009), and *End of Millennium* (1998, 2010).

phrase ‘a grounded theory of power’, not as a critical description, but as a term of pride and affirmation. He wants to be seen and understood as someone who has developed and enhanced our understanding of a key concept in political and social science. Yet he sees no need to demonstrate an understanding of the term GTM itself.

To echo Alice in Wonderland; ‘curiouser and curiouser’.

The situation is even more paradoxical and complex since some writers have criticised GTM, arguing that ‘grounded theory presumes that empirical observations are inherently sociologically relevant’, and that ‘[T]he emergence of theoretical codes or categories from the data is dependent on the ability to grasp empirical phenomena in theoretical terms, or, in other words, on “theoretical sensitivity”, a competence that demands training and background in sociological theory’ (Tavory and Timmermans, 2019). While others have offered the opposite argument, agreeing with ‘Gibson and Hartman’s (2014) observation that *the lack of engagement with sociology hampers grounded theories*’ (Reichertz, 2019, emphasis added)

A far more important and profound link between GTM and social science can be found in the work of Adele Clarke, a student of Strauss who took up not only GTM but also Strauss’s work centred on his social worlds/arenas perspective.¹⁴ The outcome from this she terms *Situational Analysis*; which ‘both extends and goes beyond’ GTM (Clarke, 2009, p.197). Clarke adopts Strauss’s ideas on GTM, including his joint work with Corbin, but stresses that the aim of identifying *basic social processes* anchors GTM in a modernist world-view that is no longer adequate either in theoretical terms or methodological ones. There is a necessity for GTM to be taken ‘around the post-modern turn’ in the form of situational analysis, in which ‘the root metaphor for grounded theory shifts from process/action to social ecology/situation’ (p.199). Clarke argues that this move builds on ‘four facets’ that add to and develop GTM in significantly new ways: ‘Chicago School social ecologies ...the deep tap roots for the social worlds/arenas/discourses theory’; ‘Foucault, discourse studies, and moving beyond the knowing subject’; explicitly taking ‘nonhuman elements in a situation’ into account; ‘the concepts of implicated actors and actants in situations’ (p.198).

¹⁴ This brief overview of Clarke’s work can only hint at its promise and power. A good starting point for further study is Clarke, 2009, as well as her core text (Clarke, 2005; revised version Clarke et al, 2018). See also Clarke and Friese, 2007 and Clarke, 2019.

In some senses this parallels constructivist GTM, particularly as found in Charmaz's work. Both criticize the way in which Glaser and Strauss, both together and separately, wrote about their data as 'distanced experts' (Charmaz, 2000, p.513), resulting in deceptively rational and objective analysis. Each offers a more open-ended and participative approach that is still grounded in the data. In addition, however, Clarke explicitly wants to offer an approach to the social sciences relevant for an age characterized by 'partialities, positionalities, complications, tenuousness, instabilities, irregularities, contradictions, heterogeneities, situatedness, and fragmentation—complexities' (Clarke, 2005, p.xxiv). In so doing she offers a bridge for qualitative researchers to access the work of Foucault (1972), Latour (2005), and Haraway (1991), amongst others; simultaneously, if unwittingly, echoing concerns that resonate with the writings of Zygmunt Bauman, particularly those on *Liquid Modernity and Ambivalence* (2000, 1991).¹⁵

Perpetuating Misunderstanding from Within

Strauss and Corbin argued in 1994 that it had taken more than 20 years for people to begin to understand the full impact and importance of GTM. Now, more than 50 years on from *Awareness and Discovery*, this is still the case, and the situation is perhaps somewhat worse, since the volume of misunderstanding of the method seems to have grown. Old criticisms, to a large extent aimed at GTM slogans or 'accidental' facets of the method, continue to be made, and to these have now been added further ones, often from authoritative figures whose knowledge of GTM seems outdated or severely mistaken. What we have is a situation of *Continual Permutations of Misunderstanding*.

One of the issues I mentioned earlier was that of intellectual property, specifically issues of 'ownership' and 'proprietaryship' with regard to the method. Readers may well be familiar with, or at least have heard of, Glaser's *Basics of Grounded Theory* (1992). Bearing the subtitle *Emergence versus Forcing*, it is the very short but very angry book that Glaser wrote following publication of Strauss and Corbin's 1st edition of *Basics of Qualitative Research: Grounded Theory Procedures and Techniques* 1990. Sometimes people refer to the book as marking the start of the dispute between Glaser and Strauss; but Strauss never responded directly to Glaser's attack.

¹⁵ Bauman's extensive writings encompass a resource of enormous significance across the social sciences and related disciplines.

The only place I have found where Strauss might be seen to refer to Glaser's position, albeit obliquely, is the AUTHORS' NOTE included in the Strauss and Corbin chapter from 1994: 'This summary statement represents the authors' views as participants in, contributors to, and observers of grounded theory's evolution. *Others who have been part of this intellectual movement will differ in their views of some points made here and the relative importance we give them.*' (Strauss and Corbin, 1994, p. 273, emphasis added)

Glaser opens his 1992 book describing the efforts he took to persuade Strauss to withdraw *Basics of Qualitative Research*, or at least to correct and revise it fundamentally. Chapter 17 of Glaser's book is given the heading 'Intellectual Property', and Glaser argues, with some justification, that his contribution to and ownership of the method has been neglected or effaced by Strauss and Corbin. Yet Glaser has also argued that the method is itself a grounded theory. For instance, in 2004 he wrote that 'GT methodology is itself a GT that emerged from doing research on dying patients in 1967, It was discovered, not invented' (Glaser, 2004, paragraph 75, emphasis added). The title of their 1967 book was *Discovery of Grounded Theory*, yet if the method really was discovered then it is hard to make a case for 'ownership'. In the UK children used to be taught that Captain Cook 'discovered' Australia and that Christopher Columbus 'discovered' America. Neither, of course, makes sense since Australia and America were there all the time, as were the people living there. If Glaser had argued that the method was 'invented' he would have a far stronger case for claiming intellectual ownership, but he specifically rejects this.

I do agree with him, however, that GTM is itself an example of a grounded theory. According to Glaser and Strauss a grounded theory must be modifiable; modifiability is one of the key criteria of a substantive GT according to the early GTM texts. Glaser himself makes modifiability a central issue in *Theoretical Sensitivity*, noting in the concluding chapter, 'In sum, new uses and directions of Grounded Theory are just beginning to be proliferated. *Grounded theory is a general methodology for generating theory.*' (Glaser, 1978, p. 164, emphasis in original). Chapter 10 of *Theoretical Sensitivity* then outlines several examples of researchers beginning to use GTM in new ways and across new fields, something that Glaser refers to in an enthusiastic manner.

Strauss and Corbin also welcomed such developments, viewing the method as a child developing in its own ways, albeit not always with the approval of its parents. Glaser, on the

other hand, now seems intent on keeping tight control of any offspring, labelling any wayward ones as not really GTM, or remodelled GTM, or jargonized GTM.

In this endeavour he is wittingly or unwittingly supported by writers who readily engage with efforts to discriminate between 'which kinds of research count as grounded theory and which do not' (Apramian et al, 2017). In my 'grounded theory of grounded theory journal articles' (see Bryant, 2017, Chapter 14), I derived the central concept of *Methodologizing*, and included a sub-concept *Positioning*, referring to ways in which authors locate their approach against the variants of GTM. Apramian et al exemplify this while seemingly offering an overview of 'four schools of thought on grounded theory' (360). The first indication comes in the introduction, where they refer to the founders – i.e. Glaser and Strauss – as 'launching a caustic debate, when, as was pointed out earlier, Strauss never responded to Glaser's diatribe. Their *position* is then unmistakably confirmed when they argue that '*The Discovery of Grounded Theory ... was Glaser's effort to write a manual for how to create theories that persist at the highest levels of academia and popular culture'* (363, emphasis added). This not only effaces Strauss, but indicates a poor understanding of *Discovery*, which in no way serves as a manual for GTM. Apramian et al compound their confusion, and that of their readers, with a bizarre series of contentions that should have been picked up in the review process.

For instance, they imply that there is general acceptance of Glaser's *coding families* across all four schools, albeit that 'Grounded theorists across the four schools favour some of these families over others' (367). This is simply not supported by the evidence from a wide range of GTM outputs – PhD dissertations and journal papers. Very little use is made of any of Glaser's 41 coding families, apart from occasional use of 'The 6 Cs' family – causes, contexts, contingencies, covariances, and conditions'; Glaser's recent writings make little reference to any of them.¹⁶

Apramian et al mischaracterize many aspects of the different schools, starting with the failure to understand that the earliest writings were a joint effort of Glaser and Strauss, and cannot be encompassed under the Glaserian school. Indeed there is considerable evidence for the argument that Strauss's earlier work in Chicago, prior to his collaboration

¹⁶ They refer to Glaser's view of GTM as 'parsimonious' despite this plethora of coding families which are often seen as bewildering and incoherent.

with Glaser, can be understood as embryonic GTM – see Bryant, 2019. They also misconstrue Charmaz’ work, stating that ‘the Charmazian grounded theorist is left with carefully *describing* the kinds of stories people tell us’ (371, emphasis added). They arrive at this mistaken conclusion based on their contention that Charmaz’ approach is reliant on ‘gerund-based coding’, and that ‘[G]erunds move theory towards description’ (371). Charmaz certainly advocates the use of gerunds, but so too does Glaser – for instance in his edited collection *Gerund Grounded Theory* (1996) – and this is no way precludes theorizing. In fact one wonders how closely Apramian et al read Charmaz’ work, since they state that a third of her book *Good Days, Bad Days* (1991) is devoted to one process – *living one day at a time* – when it only extends over 7-8 pages; also arguing that Charmaz makes it her core process and extends it to everyone: all completely erroneous.

Journal papers such as Wu and Beaunae, and Apramian et al exemplify a continued failing in evaluation of GTM work. Many authors have found that their GTM-based submissions have been reviewed by those with little or no understanding of the method, or with a prejudice against it. Similarly many published GTM-oriented papers exhibit errors or highly questionable contentions that should have been challenged in the review process, at least leading to some form of qualification or modification.¹⁷

To conclude – for now ...

GTM, 50 years on from its inception, is in a curious position. It is highly popular, widely criticised, often claimed without justification, and seems to arouse particularly high levels of prejudice and misunderstanding on the part of those who ought to know better. Some of the responsibility for this arises from the method itself, and from some of the publications of its initial ‘inventors’. GTM was and still is radically innovative, and clearly continues to present a challenge to academic practices; grounded theorists are still often viewed as rebels or outsiders. The method did and has ‘redefined the usual scientific canons for the purposes of studying human behavior’ (Strauss and Corbin, 1994, p. 274). It offers a new rationale for research, drawing attention to differing ‘criteria of judgment ... based ... on the detailed elements of the actual strategies used for collecting, coding, analyzing, and presenting data when generating theory’ (p. 274, quoting from *Discovery*, p. 224). Indeed it is so innovative

¹⁷ And I am fully aware that this submission will and should be subject to precisely this process!

that the full ramifications and impact of GTM continue to elude the grasp of many seasoned and experienced researchers.

In many fields the quantitative view still predominates, and many continue to uphold the saying that 'If you cannot measure it, you cannot (control) improve it.' The actual quote, from the physicist Lord Kelvin, is as follows:-

... when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely in your thoughts advanced to the state of Science, whatever the matter may be.

(https://en.wikiquote.org/wiki/William_Thomson)

Yet I think many would profoundly disagree with this, or at the very least question its full force. Qualitative research needs to be regarded as on a par with quantitative research, and the manifesto elements of *Discovery* are critical in making that argument. Margaret Kearney, one of the contributors to the 2007 Handbook and a grounded theorist of some renown and expertise, takes the matter even further. In an email to me she argued 'research is typically quantitative, but qualitative research can enable and direct quantitative research, as well as having large value on its own. Furthermore, qualitative research can do better than quantitative research to pave the way to important change by identifying what should be measured (such as awareness levels or contexts) that previously was not appreciated as such.' Indeed, I would argue that whereas quantitative research might offer some idea of the extent of a problem or issue, it is only qualitative research that will offer the kind of insights that might indicate what needs to be done.

I first met Barney Glaser when I responded to a call for papers for his sessions on GTM at a conference in 2005. He was clearly suspicious of me, as I had already published several papers on GTM critical of his position. When I gave my presentation my powerpoint slides consisted of 20 or so images of sharks; no text, just pictures of sharks. As I concluded my talk I asked if anyone was wondering about the sharks – of course they all were – so I explained that in Woody Allen's film *Annie Hall*, one of the characters says that 'A relationship, I think, is like a shark. You know? It has to constantly move forward or it dies.'

So too with any method. GTM needs to keep moving forward if it is to sustain its claim to have fit, grab, and utility, or in Charmaz's terms credibility, originality, resonance, and

usefulness. A method that can serve as a guide for researchers keen to generate innovative and insightful conceptualizations in the form of grounded theories. In this way we can perhaps begin to overcome and finally dismiss the continual permutations of misunderstanding that surround GTM, and move on from the very curious position it seems to hold in the mind of many who ought to know better.

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