What is Education?

It seems to be common practice to present education and schooling as coterminous, even to use these words interchangeably: this is an error. Schooling is a field in which education is institutionalized, in which the habitus generated in teachers and in pupils establishes them as precisely these entities. The construction of habitus of course constitutes education, but this takes place in all fields, not just in schooling: education is a dynamic that occurs when a static state of being is activated as a state of becoming. Furthermore, both the static and dynamic states can be achieved at any level of analysis, so not only that of the individual, institutions, even nations, certainly the planet, can also undergo education (psychology or neuroscience might even take us to sub-individual levels, but I won't go into that here, fascinating though it is). The mechanisms of education are studied in various fields that each have a tendency to remain at their respective levels, though one suspects that they do well to communicate. In terms of schooling, liberal educators including Dewey, Piaget and Schön seem not to consider the teacher as such to be an essential element, though a teacher may encourage, facilitate or, of course, inhibit learning. Certainly, the claims frequently made regarding the school closures during the COVID pandemic that children were missing learning were absurd: schooling within itself kettles learning in subject disciplines, this does not exhaust learning (most people learn to speak their first language before ever going to school). The subject disciplines themselves are far more about discriminating between individuals, already stratified by social categories, than about any other form of use value (when was the last time you used a quadratic equation or even a formal arithmetic operation?); what one does learn at school, as I recall, is how to bully or be bullied (maybe both).

But we can leave school and focus instead on another institutionalized form of education: research, which is institutionalized in the university amongst other institutions, so:

What is Research?

In the first attempt at a foreword that I wrote recently I stated that research entails a transaction between *theory*, *methodology*, and *setting*. Almost as soon as I had completed the first version, however, I realised that I had committed a major omission: I had left out the individual characteristics of the researcher. This omission is familiar in the natural sciences, which are frequently written up in the passive mood, concealing the subject of the research activity. This also used to be common in social research, though is less so more recently, now that authors of research are encouraged to speak in the first person. So, research involves a transaction between researcher, theory, methodology and setting. It occurred to me that this set might be restructured as a two-dimensional scheme (see Figure 1 below).

This of necessity entails a degree of reconceptualising. For the first dimension I initially recruited Basil Bernstein's (1990) *classification/framing* binary, defining classification as distinctions between categories and framing as distinctions within categories. I pointed out in Dowling (2009), however, that classification (C) and framing (F) effectively perform the same function, but at different levels of analysis relative to each other. I used the organisation of a school to illustrate this: the room numbers and perhaps subject labels classify spaces, but the lesson that goes on in each space is framed in terms of the specific curriculum subject: the curriculum system of classification provides the addresses, each subject frames the business in each address. Dropping down a level, that which is framed as, for example,

mathematics, is itself classified according to topics (arithmetic, algebra, geometry, …) each of which is, in turn, framed in terms of their respective content and so forth. C/F is, strictly, a fractal dimension. This fractal nature rendered these concepts unsuitable for my purposes in this article. So, I'll recontextualise an alternative pair of categories from philosophy. Classification, then, I will replace with 'ontic discourse,'' referring to the collection of theories—systems of objects and their relations—that may be deployed or built in research. Framing I'll replace with 'epistemic discourse', which is to say the collection of methodological strategies or research processes. Ontic discourse (OD) might be glossed as the 'what' of research activity, epistemic discourse (ED) to the 'how'.

For the second dimension of my scheme, I'll recruit a category distinction of my own, that of *discursive saturation (DS)*: this concept refers to the extent to which the principles of a practice are rendered linguistically explicit—high discursive saturation (DS^+)—or not, low discursive saturation (DS^-). The cartesian product of these two variables gives rise to the scheme in Figure 1. The scheme enables a theorised distinction and the relation between four sets of research strategies

	Discursive Saturation (DS)	
Level of analysis	High (DS^+)	Low (DS^-)
Ontic Discourse (OD)	*theory*	*setting*
Epistemic Discourse (ED)	*methodology*	*authorial charisma*

Figure 1: Research Transaction Space

The terms in the scheme are defined by their position in the table rather than by their more common meanings. So, 'theory' refers to the objects and relations that are defined in the research and are to be contrasted with the regions, objects, relations, and individuals that are presumed, but yet to be defined, in the 'setting'. Similarly, 'methodology' refers to that which will enable an argument to be made, whereas 'authorial charisma' is that which enables the particular form of the argument. This is a fluid space and, in particular, the setting is expected to become specified by OD/ED in the research process; initially, 'setting' simply points to a region of research interest. Similarly, 'authorial charisma' is expected to become increasingly specified by OD/ED as the style of the particular researcher or team becomes institutionalised. These developments progress with the transaction between the four modes; let's start with 'theory'.

Over the years, working with students embarking on their dissertations I have frequently encountered two misconceptions regarding theory: the first asserts that it is necessary to begin any research project with a theoretical framework. Now this position, of course, effectively writes off Grounded Theory (not a theory, but a method, or, strictly, a collection of methods), thematic analysis, and any other approach that seeks to build theory. The theory that is built may be a collection of defined concepts or themes that are relatively independent of each other or may be a joined-up theory as such. In either case the theory is clearly an output of the research activity, not an input to it. A theory might be described as a structured interpretation of the research setting. This being the case, it is clearly reasonable to put this

interpretation to the test in an exercise of data collection and analysis, but not all research is of this type. Commonly, we don't know much at all (or are suspicious about what we think we might know) about our research setting and some fields—educational media and technology, for example—are moving so fast that this is almost always the case. Under such circumstances, we should be exploring, not testing and starting with a theorising of the setting is likely to result in you finding out no more than you (think you) already know.

Sometimes a theory can be asked to do more than it is capable of. Some student dissertations that I've come into contact with have begun with the statement that the intention has been to use multimodality theory to analyse a media text or production—a film or a website or a vlog, maybe. One seminal work in the area of multimodality was the 1988 book by Robert Hodge and Gunther Kress, *Social Semiotics* and another, the 1996 book by Gunther Kress and Theo van Leeuwen, *Reading Images: the grammar of visual design*. Now essentially the grammar that they introduce in each of these books is descriptive. In terms of semantics, they reveal how meanings might be represented using semiotics or using visuals. All texts, however, are polysemic, which is to say, they are open to diverse interpretations. So as persuasive as these authors' interpretations are (and I'm not convinced), they are certainly not definitive. Indeed, in Dowling, 2009, c. 2, I have presented alternative and, I argue, a rather more persuasive interpretation of one of the images that Hodge & Kress analyse: a painting by the Byzantine artist Cimabue. Now my argument there is based on challenges to their interpretations of Durkheim's and also Bernstein's sociologies on which they base their *social* semiotics. Sociology is necessary to their multimodal/social semiotic analysis because they are claiming to have made sociological interpretations, so linguistics is operating in the wrong domain. Even without recourse to the correction of their sociology, their analysis of the Cimabue painting is suspect, including, for example, the claim that the haloes around the heads of the saints and angels etc fragment the collective rather than, in my analysis, constituting a 'style marker', thus uniting it. They also miss the bold and focal geometric organising structure of the image: the cross.

Now Hodge & Kress claim that the image constitutes a transparent signifier of a society in turmoil:

> The authors support their reading by offering a brief description of late thirteenth-century Florence as a 'city-state in turmoil'. They do not attempt an explanation as to why the dominant classes of a chaotic state would be expected to sponsor the production of chaotic cultural artefacts.
>
> Dowling, 2009. P. 34

Especially one to be located in the church of Santa Trinita in Florence, which was associated with the Medici family at the time of the production of Cimabue's painting.

I am not claiming at all that my analysis is definitive, just that it's better than that produced by Hodge and Kress (perhaps because I'm a sociologist and not a linguistician). The contrast, though, does illustrate the polysemic nature of the text; access to its meaning is not given by social semiotics or by multimodality (see Dowling, 2009. c. 5 in respect of the latter), these simply provide languages through which meaning can be relayed. I should stress that I am not challenging social semiotics or multimodality theory or, for example, film theory generally: these are all able to produce complex and useable languages, but they do not, in the absence of authorial interpretation, grant definitive access to meaning.

This is the case generally with the relation between theory and meaning, but it's not necessarily the relation between theory and explanation. Many academics, having built or otherwise acquired their theories, will tend to speak in the language of their

creation/acquisition. So, when asked to explain a phenomenon they will not present a research argument, but rather summarise an explanation in this language. So, I might say that the school mathematics curriculum distributes itself in such a way that some students are given access to a region—the esoteric domain—that grants them access to a career and others are denied this access and instead are restricted to an artificial discourse—the public domain—that offers no future either in mathematics or in its potential applications and that, furthermore, the candidates for each group are recognised, principally, in terms of socioeconomic class. The argument in support of this claim is to be found in Dowling (1998, 2009). This statement might be further simplified as: schooling contributes to the reproduction of social injustice. The evidence is obfuscated by academic arcana. In respect of the above summary, I could say that those given access to the esoteric may find that this too is a dead end and that, if they take their mathematical schooling further, they are likely to find that mathematics in the University bears little resemblance to the subject that fascinated them at the secondary level (Dowling, 2010; Dowling & Burke, 2012), though they may fare better in physics or engineering. This is my 'expert knowledge': when speaking with each other, academics speak as peers; when speaking to the public, we speak as experts; this is not really a healthily 'democratic' situation.

The second misconception about theory has been widespread amongst doctoral students panicking over what to do with the piles of qualitative data that they've accumulated and that seems overwhelming in terms of quantity and opaque in terms of meaning: "where can I find a theory that will help me make sense of my data?" I'll recontextualise another theory to illustrate the difficulty here, Jean Piaget's (1980) first stage of cognitive development: the assimilation/accommodation dialectic. Well, if we're looking for a readymade theory, then perhaps the data is to be assimilated to it. Naturally, the data is going to have to undergo some changes if it's to fit. This would be tantamount to what Barney Glaser (1992) refers to as forcing. This is not an acceptable solution if we are hoping to learn from the research setting. The alternative is that the theory must transform to accommodate the data. This is preferrable, but only if the theory, under transformation, really does fit the data. The approach was adopted by Kanako Kusanagi in her ethnographic-style study of the import of the Japanese teacher development programme, 'lesson study' (Kusanagi, 2022). This is the book for which I was writing the foreword to which I referred earlier.

Kusanagi's study involved interviews with teachers, a teacher survey and classroom observations. Here is an extract from my foreword:

> [Kusanagi] draws on theory from several sources, but not in such a way as to impose extant theory on her data. Rather, theoretical constructs introduced by Lev Vygotsky, Basil Bernstein and in my own work are themselves recontextualised in their use in her own analysis. The category 'recontextualisation' itself was initially my recontextualisation from Bernstein and has been subsequently deployed in [Kusanagi's] work. Vygotsky's 'zone of proximal development' has been productively combined with a recontextualisation of Bernstein's speech codes to construct two modes of pedagogic strategy, neither of which is deployed exclusively in the Javan school. Finally, my own reontextualisation of authority strategies from Max Weber has been again recontextualised by Kusanagi in her own analysis of the Javan school.
>
> Dowling in Kusanagi, 2022

The two pedagogic strategies that Kusanagi identifies are: i) 'elaborated pedagogic strategy' (EPS), which opens a zone of proximal development (zpd) in which support can be provided for the learner; and 'restricted pedagogic strategy' (RPS) that does not form a zpd and so no

support is provided for learning. Unlike the case with Hodge & Kress, Kusanagi recontextualises Bernstein, but does not misunderstand or misrepresent it: the theories enlarge their fields of application without undue distortion and without subjecting the data to undue violence. Nevertheless, the data has been theorised, the setting has become recontextualised as theoretical and methodological examples The untheorized setting and the raw data remain in their pristine state, ready for re-analysis by the same or by a different theory/methodology combination.

Suppose you look at it like this

Before turning to the category DS^{+}/ED—methodology—let's engage in a little 'armchair theorising'. It occurred to me to conceive of problem situations in terms of two variables. The first concerns whether the situation is to be played out according to explicit rules or according to a pre-determined structure that is known or discoverable in play. Sports and games would generally meet this criterion as would formal debates, *democratic* elections and other competitions. In some cases, the full structure may not be explicitly available at the outset and participants may attempt to discover it experimentally. An example of this situation might be a video game in which the rules or structure are encoded in the environment but may in part or in whole be tacit to begin with. There are other situations that do not seem to be rule-based and this may be deemed to be the case in social and humanities research, but not mathematics, which is concerned to explore formal systems having explicit premises. In either case, rule/structure-based contexts and those not having explicit rules or structure may or may not involve opponents. These two variables—rules/structure and opponent/no opponent—generate a 2 X 2 space as below.

	Rules/Predetermined structure	
Opposition	Yes	No
Yes	*compete*	*negotiate*
No	*puzzle*	*design*

Figure 2: Problem Situations

This scheme presents four strategies that may be deployed in problem situations. These categories are not mutually exclusive, so an individual or group may deploy more than one strategy at different points in their engagement. I should emphasise, the unit of analysis is the strategy, not the individual agency or agent that deploys the strategy: the scheme does not put individuals/groups into boxes! The determination of whether or not the problem is rule/structure-based or whether there is or is not an opposition is made by the subject of the action, eg the game player, and is not objectively given. We can play around with this scheme to refer to cases beyond those most obviously represented by the category labels.

For example, in research, the distinction between, on the one hand, a realist strategy (that presumes and seeks to discover an underlying structure to the setting) and, on the other, a constructivist strategy involving the claim that the structure of the setting is an artefact of the data collection and analysis itself, is that between *puzzle* and *design*, though the language

in which findings are presented may be deceptive. I may claim that 'my analysis *has revealed* that …', which suggests that I have solved a *puzzle*, when in my methodological discussion I have presented a constructivist methodology and so my solution should be presented as a *design*! The motive for such misrepresentation may have to do with marketing in the sense that *design* may be thought to be associated with aesthetic interests, while the solution to a *puzzle* may be interpreted as more functional. In the resolution of disputes in a social context, the strategies of one side may be presented in the context of a *negotiation*, whereas the agency in this case may be recruiting resources relating to a perceived underlying structure to the situation, so a move in *competition* is being presented as a *negotiating* strategy. The motive for deception here may be interpreted as relating to a perceived need to show 'fair play'.

The analysis here may be recognised as *analytic*, because I have included no *empirical* data. A reaction against this kind of sociology was what motivated Barney Glaser and Anselm Strauss to present and develop their work in 'Grounded Theory' (1965, 1967), not itself a theory as such, but a method for the generation of theory that is grounded in empirical data: note that there is a clue in the title: 'The *discovery* of grounded theory'; the theory is discovered by the method.

The terms that are used for my four strategies are *etic* or theoretical categories, which entails that their meanings are restricted to the way in which they have been defined in the text. Clearly, all four are also deployed in various contexts as *emic* or empirical categories. In these cases, the meanings of the terms are given in the ways in which they are used in language games (Wittgenstein, 1958). It is important to maintain the distinction between *etic* and *emic* categories and, in particular, to avoid challenging an *etic* term on the basis of an *emic* interpretation (or, indeed, vice versa).

The scheme itself preconceptualises the settings in which problems are engaged. In this case the scheme has been developed in terms of *etic* categories, but whether their origin is analytic (as in this case) or empirical, my interpretation is not to claim that this is the way things are, but rather to offer the invitation: suppose you look at it like this!

The variables in this scheme might be described as ontological, metatheoretical or about the objects of our theory. Here is Martin Hammersley on another metatheoretical issue:

> [This] point of view is that the choice of context by ethnographers is necessarily arbitrary, in the sense that a host of different stories could be told about any situation, each one placing it in a different temporal and spatial context. From this perspective, ethnography is simply one means among others for telling stories about the social world, stories that need not be seen as competitive in epistemic terms. Of course, given this orientation, there would be a puzzle as to why anyone would go to the trouble of engaging in ethnographic fieldwork. Why not just write fiction in the manner of novelists and short story writers?
>
> Hammersley, 2006; pp. 7-8

No, No, No! That which constitutes anthropology and, indeed, the reporting of research in any other 'academic' discipline as distinct from the writing of novels is, first and foremost, that it is expected to foreground its methodology. This is an explicit requirement in respect of any thesis submitted for the award of a doctorate at my institution (and I would imagine most others in the UK at least) and is an empirical feature of writing generally in social research and the humanities, as well as the natural sciences. Novelists may explain their methods, but this is rarely incorporated into their novels. Furthermore, novels are read differently from anthropology: they may be regarded as artistic expression or entertainment (maybe both),

however the author intends them. Of course, anthropology may incorporate these characters as well, but these will generally be understood as secondary functions, whereas one or the other is expected (by readers, I'm guessing) to be a primary function of most novels. Another key function of academic writing is education, and this may or may not be in the mind of the novelist[1]. None of this is to diminish the value of novels or of fiction in general. I have, I hope, learned an enormous amount from novels and, indeed, from fiction in other media: it provides me with explorations of the ways in which people (and other entities) might be imagined, of the creative ways in which language might be deployed and developed, and with metaphorical structures that excite my own imagination. For the most part I will have no truck with the question of absolute 'truth'. There are, naturally, certain activities for which the question of truth becomes paramount, though perhaps these are not as widespread as is commonly thought. I would like to repeat one observation before moving on from metatheory:

> … only theoretical objects may be discovered; an empirical object is merely encountered.
>
> Dowling 2007; p. 191fn.

Which is another way of putting the distinction between DS^{+}/OD and DS^{-}/OD in Figure 1 above.

Some Methodological Misconceptions

I've introduced methodology—DS^{+}/ED—as the discourse that enables an argument to be made. I want to consider a few misconceptions that I have encountered routinely in postgraduate student work and elsewhere. I should start at the beginning; the assumption that it is essential to begin with a well-formed research question. Now my point of difference here is with the word 'essential'. Of course, there are situations in which one or more research questions are entirely appropriate, where, for example, one is following up on issues raised in previous research by oneself or others, so that the current research aims to develop existing discourse rather than start from scratch. The latter, however, is often the situation that I find with students. In many research situations, especially working with Masters and Doctoral students, I find it unhelpful to formulate precise research questions at the outset. After all, a well-defined research question has to be composed in terms of categories that, of necessity, to a greater or lesser extent preconceptualise, or theorise the setting. If the setting is one that is new to the researcher, or if they are engaging in the practice of making a familiar setting strange so as to see though the familiarity, then preconceptualising is counter-productive. Indeed, I've just reviewed a research article[2] that had been submitted to a mathematics education journal. The researchers set themselves a research question that they stuck to rigidly in their quantitative study. Although the analysis was quantitative, the article included some reported discussion between very young (First Grade) students to illustrate the authors' coding principles. Some of these discussions struck me as really interesting and ripe for qualitative analysis. Unhappily that was not to be allowed, so everything that was interesting (as far as I was concerned) in the research was deliberately ignored by the researchers.

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[1] I recall (I hope correctly) a TV interview with Martin Amis in which he said that his principal aim in writing was to educate.

[2] The conditions of the peer reviewing process do not allow me to give further information as the review process is still under way.

Barney Glaser (1992) suggests two strategies to avoid imposing preconceptualisations or extant theory on the empirical. Firstly, use of a 'grand tour' question to begin an interview. Glaser suggests 'How's it going?' I'm not sure that I would find this particularly helpful; my response to such a question might be, 'Fine. How's it going with you?' which wouldn't progress the interview very far.

If the researcher is interested in an activity or practice—say mathematics teaching—that they know the interviewee engages in, then: 'Would you tell me something about a recent mathematics class' and follow up their response with probes. This leaves the researcher out of control regarding how the participant prioritises their experiences. They might, for example, tell you that their classroom is positioned alongside a quadrangle in which the head of the science department maintains a number of beehives and, since it was April 1st, they had sent one of the students to the head of science with a bottle of talcum powder and a note asking the head of science to send the bottle to the teacher in the classroom on the opposite side of the quadrangle and to include a note saying that the day being rather sunny, perhaps that teacher would spray some of the powder on the outside windowsill as this would deter the angry activity of the bees. The participant might report that their students had lined the classroom windows to watch the performance. The teacher in the opposite classroom suddenly became aware of the audience and realised that they had been had! This response would not tell the researcher much about mathematics teaching, but it would say something about the practice of the participant more generally. Glaser's research practice was always to prioritise the interests of the participants rather than impose their own. There is no doubt that this approach generated some fascinating and unexpected results. Not for everyone, perhaps, but ensuring that the interviewee has scope to tell *their* story and not yours is a point worth attending to. The second of Glaser's strategies designed to avoid preconceptualisation is the use of 'open coding' of the data without attempting to fit it into pre-existing categories, but I'll come to data analysis later.

Quantitative research generally aims to generalise from a sample to the population from which the sample was drawn. This being the case, it is important that the sample is representative of the population. This can be attempted in a number of ways. Random sampling is often used in this respect. This strategy entails that each member of the population (more correctly, the sampling frame that is constructed from the population) has an equal probability of being included in the sample. This criterion may be modified in various ways for practical reasons, but any modification weakens the claim to representativeness.

Qualitative research does not aim to generalise from a sample to a population, not least because the sample is likely to be too small.[3] Also, qualitative interview questions generally aim to respond to answers to previous questions, so each interview will be a different experience, rendering aggregation problematic. The extension of qualitative findings is limited to the sample itself. This means that it is not necessary or even appropriate to argue representativeness. Qualitative research generalises, firstly by the accumulation of cases and secondly by the generation of theory, which may be challenged or added to or tested by subsequent qualitative or quantitative research or which may add to the theoretical sensitivity of the researcher in a subsequent study. Of course, one looks for similarities and differences across cases and this strategy is deployed in the move from substantive to formal grounded theory. This, though, is not the same as setting out to construct a representative sample for

[3] The standard error for a sample is inversely proportional to the square root of the sample size, so the larger the sample, the smaller the standard error. A large standard error (small sample size) indicates that the sample mean is likely to be unrepresentative of the population mean.

qualitative analysis: looking for similarities and differences between cases comes after the initial research has been completed, not before it starts.

Having performed preliminary analysis of your first interview(s) or section(s) of data, ask yourself, 'where should I go/what should I observe/whom should I interview next? This strategy from Grounded Theory is *theoretical sampling*. It is generally a productive way to build a sample in response to your developing analysis that does not restrict you to one form of data. It is, however, not always going to be possible. If you are conducting research in a school, for example, you may find that the principal will want to know details of your proposed sample at the outset. This is their call, not yours, but you might try persuading them of the value of theoretical sampling and promise to keep them apprised of your activities.

Recognising who is in charge is an ethical issue. Research ethics has become increasingly significant in funded research, personal research and student research since about 2003 in the UK. I was a member of a small panel tasked with creating a research ethics review form, initially for use by academics preliminary to their own research, and later to student research. The initial form was, to my recollection, a page-and-a-half. My institution subsequently established a bureaucratic structure, including a research ethics administrative officer and a chair of the research ethics board at professorial level. The form for student research is now 16 pages. I've found it to be a general rule that whenever a bureaucratic structure is established it has to find work for itself and the number and length of forms and meetings increases rapidly and substantially. Now, I'm not at all critical of the University's current procedures: research ethics is an important matter that was very much undervalued in social research at the end of the twentieth century. A negative outcome of the current protocol, however, is a knee-jerk reaction to the ethics review form, which mentions an information sheet and consent form to be, respectively, given to and signed by research participants. Now, sometimes these are going to be necessary, but many of the cases that I come into contact with are not dealing with sensitive or complex issues. I advise students to describe their study and the implications for respondents orally and audio record this description and the participants' consent to be interviewed/observed. It gets a little more complicated when the consent of parents/guardians is needed, but in most cases, this can also be handled without getting them to sign a form, which even I can find a little threatening, especially if the content is unfamiliar. So, we must treat research ethics seriously, but not go overboard on bureaucracy.

The next issue that I have a problem with is the question of whether or not to prepare an interview schedule. My response to this is similar and related to my response to the issue relating to research questions. Some studies, and in particular those for which a clearly worded research question is deemed necessary, will proceed more productively if a schedule of items is framed in advance. The structure of the schedule may vary from a questionnaire, which would be used in a survey, to a short, more loosely structured list of issues to be covered in the interview or, indeed, in an observation. My view is that a qualitative interview works best if it feels, from the interviewee's perspective, like a conversation in which the participant does most of the talking. A student of mine recently handed me an interview transcript that did indeed resemble a conversation in which the student was an equal participant with the supposed interviewee. I suggested that the student re-describe this event in their dissertation as indeed a conversation that had been planned as such. So, the student joined the list of participants in this study. This seemed to have worked out quite well. This raises another issue, that is, that published methodological guidelines should be regarded as exactly that and not as rules to be followed blindly. In the end, the researcher has to make their argument and, in doing so, defend their decisions: this is not to be done simply by inserting the name and date

of publication of a methodology textbook in much the same way as Mark Twain accused 'most people' of using statistics: 'the way a drunkard uses a lamp post, more for support than illumination'.[4]

The last two misconceptions relate to post-interview/observation work. The first concerns translation. Many of my students over the years, being far better linguists than me, are able to conduct interviews in the first language of their interviewees, which is often not English. Generally, their inclination is to transcribe their recordings and then translate into English prior to analysis. Now I speak some Japanese, some French, and I used to speak some Portuguese, but none of these well enough to follow an interview transcript in one of these languages. So, if a student would like some advice on analysis, they are going to have to translate. However, to translate is to do violence to the text, so analysis is far better carried out in the language in which the interview/observation was conducted. Only after this will it be necessary to discuss the translation of anything that is going to be included in a dissertation, including, of course, analytic concepts that will have been generated, and these may be best left in their original form, though terms in Japanese or Chinese or other languages that are not normally written in the roman alphabet may benefit from transliteration.

Finally, I come to the crux of the matter: the analysis of qualitative data. I'll begin with Grounded Theory, firstly, because this is the most widely used approach in qualitative social research and, secondly, because I have generally approached the teaching of qualitative analysis with GT. Grounded Theory was originally introduced by Barney Glaser and Anselm Strauss in the 1960s (1965, 1967) and this was followed by multiple books by Barney Glaser, fewer by Anselm Strauss, often writing with Juliet Corbin. Strauss and Corbin (eg 1998) took a somewhat different line from that of Glaser; Kathy Charmaz, a former student of Glaser's introduced what she referred to a constructivist Grounded Theory (eg 2014), believing, it seems, that a philosophical underpinning was needed. I have yet to be given what I regard as an adequate explanation for this necessity (I'm generally suspicious of 'necessities' and 'essentials'). The diagram below charts phases in classic (ie Glaserian) GT. Virginia Braun and Victoria Clarke (2006) present what they refer to as 'thematic analysis', as an approach used in qualitative psychological research. They claim that this is a distinct method, but, as far as I can see, it follows classic GT with the exception of the phase of theoretical coding: thematic analysis terminates with a collection of themes without necessarily tying them together to produce a joined-up theory, though their themes are clearly a theorising of their settings.

Classic GT seeks to discover a single 'core category' following which 'selective coding' concentrates on those codes that relate to this. I tend to want to generate more than one core category, which I then organise as the cross products of two to generate 2-dimensional relational spaces (sometimes extended to 3-dimensions) such as the ones in Figures 1 and 2 above.

[4] http://www.quotss.com/quote/Most-people-use-statistics-the-way-a-drunkard-uses-a-lamp-post-more-for-su

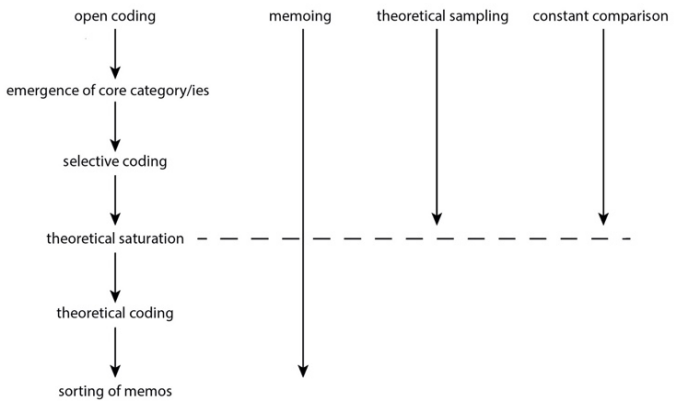

Figure 3: Stages in Classic GT

The way in which I have described the initial process of analysis in GT is as follows. It begins by selecting a unit of text, which may be as small as a single word or a few words, and 'coding' it according to what you think it says is going on and writing a 'memo' describing the meaning of the code. This process is repeated as you move through the text. After a while, when you will have generated a number (not specified) of codes, these codes are then compared with each other and with the text to which they refer; this is referred to as 'constant comparison'. By re-defining and combining some of the codes, the list of codes will be reduced and the process is repeated until the whole text has been coded and memoed. I won't describe the process further here other than to say that the stage of 'theoretical saturation' has always seemed to me to be a worthy, but unattainable ambition: it's the point at which no further codes are emerging.

I've recently realised, however, that although this is the way that I've taught analysis, it's not the way I've conducted it. I have always taken what might be described as a more hermeneutic strategy. This entails moving between considering the whole text or, better still, two texts that are different in some, as yet indefinable way, and looking at smaller units. What I'm trying to do is get a sense of what the text is doing, in whole and in parts, which need not necessarily coincide: there is no necessity for the text to be consistent in its strategies. If I'm starting with two texts, I am aiming to be able to say in a theoretically precise, DS^+ way, just how they are similar or how they are they different: I want them theorised.

Towards (or away from) the ineffable

In this article. I have talked only about the DS^+ categories of OD and ED, that is, theory and methodology. That is, of course, because the DS^- categories, setting and authorial charisma are ineffable by definition. Once we start to speak about our setting we have begun to theorise it, similarly with authorial charisma. As I have repeated above, only theoretical (ie DS^+) objects can be discovered, empirical (ie DS^-) objects can only be encountered. Yet it's in the encounters between setting/authorial charisma and theory/methodology that the research process must proceed. Some research may place more emphasis on the setting, perhaps this article has placed more emphasis on the author. This author has spent years working under the naïve assumption that all of my peers are interested only in the quality of the work they produce and that they encounter and that they teach: where has my sociology been when it has been most urgently needed?

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