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Naturalist Inquiry and Grounded Theory

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Abstract: The world of Qualitative Data Analysis (QDA) methodology became quite taken with LINCOLN and GUBA's book "Naturalist Inquiry" (1985). I have no issue with it with respect to its application to QDA; it helped clarify and advance so many QDA issues. However, its application to Grounded Theory (GT) has been a major block on GT, as originated, by its cooptation and corruption hence remodeling of GT by default. LINCOLN and GUBA have simply assumed GT is just another QDA method, which it is not. In "The Grounded Theory Perspective II" (GLASER 2003, Chapter 9 on credibility), I have discussed "Naturalist Inquiry" (NI) thought regarding how LINCOLN and GUBA's notion of "trustworthy" data (or worrisome data orientation) and how their view of constant comparison can and has remodeled and eroded GT. In this paper I will consider other aspects of NI that remodel GT.

1. What is Truth?

"Naturalist Inquiry" (NI) deals with a fundamental problem: "the concept of truth" (LINCOLN & GUBA 1985, Chapter 1). LINCOLN and GUBA formulate truth as "a systematic set of beliefs, together with their accompanying methods, a paradigm." They say, "a paradigm is a world view" which produces a methodology that arrives at a current set of beliefs. As such a paradigm arrives at a current truth! Then LINCOLN and GUBA take paradigm use very eruditely through three "paradigm eras, (pre-positivist, positivist, and post-positivist) periods in which certain sets of beliefs guided inquiry in quite different ways." They say "that if a new paradigm of thought and belief is emerging,

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it is necessary to construct a parallel new paradigm of inquiry.” Each paradigm as it emerges comes to “true understanding” and to “ultimate truth.” LINCOLN and GUBA assert that their book is in the third paradigm, post-positivism, in an “effort to mark out (their) place along the path to understanding.” And as “theories are remarkably immune to change,” thus “any conflicting fact can be accommodated by making adjustments. As paradigms are assaulted by facts that do not fit, the facts can be walled off.” This, of course, opposes Grounded Theory (GT) methodology, which compares all facts to conceptualize a place in the emerging theory. (The quotes in this paragraph come from Chapter 1, pp.15-31.)

Being drawn into this discussion of arriving at facts by changing methodology simply remodels GT, particularly from the naturalist post-positivist view which asserts the reverse of positivism, that is constructivism, regarding arriving at “relative” truths or facts. The reader can easily read their discussion. But this discussion, however it may be relevant to Qualitative Data Analysis (QDA) as it evolves into constructivism, is not applicable, even relevant to GT (see GLASER 2002).

First, LINCOLN and GUBA’s (1985) discussion’s underlying pattern simply focuses on changing views of worrisome accuracy, but always accuracy. It does not address the abstract nature of GT, which does not deal in facts or findings, but generates concepts that apply as explanations. The concepts are not facts, as I have reiterated over and over. They are variables that vary and are modifiable. They are integrated into a theory, which results in interrelated categories and their properties, highly applicable but *not* factual.

Second, GT does not generate an immune theory – immune to facts, which is, of course, a major problem of received theory. GT is inducted from systematically collected facts, which in the process for generating GT from data, constantly verifies its fit, relevance and workability, and adjusts (modifies the concepts and their relationships) the theory to the facts to achieve fit, relevance and workability. New facts are not “walled off.” They are compared into the GT to generate concepts. Thus a GT can be generated with whatever the paradigm and the methodology for achieving it, as “all is data” about whatever is going on (see GLASER 1998). GT is a flexible, conceptual, inductive methodology abstract of LINCOLN and GUBA’s (1985) discussion on finding the right truth, belief, to wit their focus on worrisome accuracy. GT generates concepts from systematically collected data, as opposed to LINCOLN and GUBA’s position that post-positivism generates descriptive facts that are effected by the way extant theory allows the researcher to see them.

“Meanings are determined by theory, they are understood by theoretical coherence rather than by facts,” LINCOLN and GUBA (1985) say quoting HABERMAS (LINCOLN & GUBA 1985, p.36). In contrast, GT generates theory, say processes, ranges or binaries, that may have very different meanings to varied people. For example the meaning of pseudo-friending as a

mechanism of client control, will have varied meanings to clients – such as toning, sounding phony, turning off, easing the tension, irresistible, sweet talking, making comfortable, etc, but whatever the meaning, pseudo-friending goes on (see GUTHRIE 2000).

GT abandons the falseness of theoretical coherence as establishing and blessing theory rather than grounding it in data. Ungrounded theoretical coherence is just a scholarly way of saying logical conjecture uncontrolled by facts is always neat and tidy given the good minds doing it. But it hardly means that the conjecture has any relationship to what is going on, and most often does not! This lack of relationship to the real world as is, this forcing the real world as one wants it to be, leads to making GT appear as a waste of time and a subterfuge, if one wants to use prior extant theory. LINCOLN and GUBA's (1985) erudite scholarship, quoting great thinkers, by which they put over their post-positivist naturalist paradigm will not get by the grounded theorist, however overwhelming the bow to idols. He/she has to discover what is going on: whatever it is but without preconception. At core in LINCOLN and GUBA's discussion is just another new set of ungrounded logical conjectures on "what truth is" which is moot and irrelevant to GT conceptions *and should not be allowed to remodel it* as another QDA. Let the QDA methodologist wrestle with the post-positivist paradigm.

2. Axioms

To firmly found their "naturalist paradigm," LINCOLN and GUBA state its axioms (LINCOLN & GUBA 1985, pp.36-38). They define axioms "as the set of undemonstrated (and undemonstratable) 'basic beliefs' accepted by convention or established by practice as the building blocks of some conceptual or theoretical structure or system" (p.33). In short these axioms are ungrounded by their words, so are of no use to GT. Yet LINCOLN and GUBA build a naturalist paradigm on these ungrounded conjectures. That is just plain forcing the research model and the data, which GT will have no part of.

Of course, the axioms, like all idealism, sound good and wise, but are (to repeat) ungrounded conjectures. They are LINCOLN and GUBA say:

realities which are multiple, constructed and holistic, knower and known are interactive, inseparable: only time and context bound working hypotheses are possible; all entities are in a state of mutual simultaneous shaping, so that it is impossible to distinguish causes from effects; inquiry is value-bound (p.37).

These axiomatic beliefs are just think-ups, ungrounded in research, but honoring idols (critical theorists) that LINCOLN and GUBA are enamored by. They are of no use to GT. GT is just focused on conceptualizing what emerges. If these axioms emerge, fine; if they do not, fine. They cannot be used to force the data as some kind of inalienable laws. GT has left this forcing behind.

Naturalist inquiry cannot be allowed to regress and default remodel GT back to what it was trying to correct.

Incidentally the fourth axiom on mutual simultaneous shaping may be getting at a theoretical code that may emerge: that is the random walk model. But LINCOLN and GUBA are very unformulated on this theoretical code that is well-known in hard science inquiries. LINCOLN and GUBA cite fourteen characteristics of operational naturalistic inquiry based on these axioms (pp.39-43, op.cit.). These characteristics compound the conjecturing as *oughts* or *forcings* to happen during research. Some are trite, some are just obvious or routine and some are supposedly borrowed from GT. I will go through them, but the reader should keep in mind their preconceived pacing and forcing nature and that a GT researcher just does GT according to whatever emerges as data, as pacing, as substantive codes, as theoretical codes in a substantive area. LINCOLN and GUBA's characteristics restrict the flexibility and autonomy of the researcher to discover GT and should not be allowed to remodel GT to another QDA.

1) *Natural setting*: "In naturalist settings, realities are wholes that cannot be understood in isolation from their contexts" (p.39). I have said in many of my writings that theoretical codes, like context, must emerge as relevant: earn their relevance (see e.g. GLASER 1998). They cannot be assumed or forced. I have seen many grounded theories that do not have context as a theoretical code and they are not lacking. "Wholes" also force the data. Whether or not the conceptions or categories depict "wholes" or dimensions of "wholes" is also emergent relative to the core variable and its resolving the main concern. "Wholes" is a pure QDA descriptive rule.

2) *Human instrument*: NI

elects to use him/herself as well as others as the primary data gathering instrument. Human instrument is capable of grasping and evaluating the meaning of differential interaction. All instruments are value based and interact with local values, but only the human being is in a position to identify and take into account these resulting biases (pp.39-40).

This characteristic is trite; we all know this. However it implies only qualitative analysis is done, which is ok for QDA, but not for GT. GT uses all as data, quantitative and qualitative, and often differential meanings and values biases are of no relevance, and if so they are just more data. It depends on what data is used in what combination and what emerges. Thus GT should not be remodeled into thinking that humans as instruments, differential meanings and value biases ARE ALWAYS an issue. Let us see first, and not force these issues.

3) *Utilization of tacit knowledge*: Here LINCOLN and GUBA legitimate what in their view is subjective: nuances, intuition and feelings, as opposed to language expressed data, because tacit knowledge mirrors the interaction, multiple realities and value patterns of the researcher. For GT when relevant

these are just more variables, but only when they earn their relevancy into a grounded theory. So ok, but emergent and not always. They are NOT to be forced by examinations when not relevant. And once made relevant they are as much a manifest data for category generation as any other. They are not tacit! We see here, in this characteristic, the beginnings of the constructivist approach of LINCOLN and GUBA.

- 4) *Qualitative methods*: “[NI] uses qualitative over quantitative methods because they are more adaptable to dealing with multiple realities” (p.40) and the effect of researcher posture and values on data collected. This is trite and redundant for QDA researchers who are committed to qualitative data. This is moot for GT because of its abstracting the data to conceptual categories on whatever is emerging.
- 5) *Purposive sampling*: “[NI] is likely to eschew random or representative sampling in favor of purposive or theoretical sampling because he or she thereby increases the scope or range of data exposed” (p.40). LINCOLN and GUBA say that purposive sample is an effort to uncover multiple realities, local conditions, local mutual shapings and local values in order to devise grounded theory. This is a classic example of borrowing GT jargon to put over a QDA method approach and one that forces the search for specific descriptions: multiple realities, and local conditions, shapings and values. Their search is for required, preconceived grounded descriptions, not emergent conceptual theory. Their impact is to remodel GT to a QDA method.
- 6) *Inductive data analysis*: “[NI] prefers inductive to deductive data analysis because that process is more likely to identify” (p.40) their preconceived, required descriptions cited above in 5. LINCOLN and GUBA continue that induction is “more likely to describe fully the setting and to make decisions about transferability to other settings easier” (p.40). Here they clearly are descriptive, not conceptual, oriented. And they conceive of generalizing as transferring descriptions from one unit to another unit. This does not apply to GT, which engages in conceptual generalizing (see GLASER 2001).
- 7) *Grounded theory*: “[NI] prefers to have the guiding substantive theory emerge from the data because no a priori theory could possible encompass” (p.41) the preconceived required description cited above in 5. Once again GT is jargonized to be applied to grounded descriptions of investigator values, contextual values, multiple realities, and their other descriptive concerns. This use of the word GT has nothing to do with conceptual GT as originated, BUT has had a large remodeling effect in QDA research. It tries to establish grounded description as GT, when it is rather the opposite. Also they do not refer to any procedures upon which GT as description is generated.
- 8) *Emergent design*: “[NI] elects to allow the research design to emerge rather than to construct it preordinately because it is inconceivable that enough could be known ahead of time” (p.41) about their preconceived, require de-

scriptions cited in 5. This is, of course, the spirit and approach of pure GT, BUT of course, a jargonized use for descriptions NOT GT. LINCOLN and GUBA again borrow GT jargon, not substance. GT lets whatever emerge as data and then is conceptualized into categories. GT does NOT seek a special emergent, as LINCOLN and GUBA wish, to force to get, say: a trustworthy multiple reality or local value.

9) *Negotiated outcomes:*

[NI] prefers to negotiate meaning and interpretation with human sources from which the data have chiefly been drawn because it is their constructions of reality that the inquirer seeks to reconstruct because inquiry outcomes depend upon the nature and quality of the interaction between knower and the known (p.41).

This, of course refers to LINCOLN and GUBA's (1985) constructivist orientation, which I analyzed in the above citation. Constructionism is NOT GT, but could emerge in a small amount of cases as just more categories. LINCOLN and GUBA's constructivist orientation applies in their case to accurate description, not the conceptualizations of GT.

10) *Case study reporting mode:* "[NI] is likely to prefer the case study reporting mode because it is more adapted to a description of" (pp.41-42) the preconceived, required descriptions cited in 5. It flies in the face of theoretical sampling as a pure GT procedure where many cases can be sampled. And also their affirmation seems to conflict with their notion of purposive sampling as stated in 5 above. This characteristic is trite for QDA researchers and GT researchers. It is what they do anyway.

How this case focus "provides the focus for both individual naturalist generalization and transferability to other sites" (p.41) is irrelevant for GT conceptual generalizations and a difficult task for descriptive generalization between units.

11) *Idiographic interpretation:* "[NI] is inclined to interpret data idiographically in terms of the particulars of case rather than nomothetically in terms of lawlike generalizations because different interpretations are likely to be meaningful for" (p.42) their preconceived, required descriptions cited in 5 above. This statement is irrelevant for GT conceptions, unless LINCOLN and GUBA consider "lawlike generalization" (p.42) as a synonym for conceptualization. Then it once again remodels GT to a QDA method for description. I will let the QDA methodologist worry about the obtuse meaning in 11, unless, again, it just resolves to constructivist thought. If so see my paper of constructivism cited above (GLASER 2002).

12) *Tentative application:* "[NI] is likely to be tentative (hesitant) about making broad application of the findings because realities are multiple and different, because findings are to some extent dependent upon the particular interaction between investigator and respondents ..." (p.42). Here we have the descriptive generalization problem (see GLASER 2001, Chapter 7), which

is a description about one unit be applied (same as generalization) to another unit. Transferability is a big QDA problem, oft debated. This has nothing to do with pure GT, which generalizes concepts, e.g. the study of client control in a veterinarian hospital applies to any area where client control exists – always with some modification by constant comparison if necessary. QDA methodologists seem to not understand this difference in generalization and hence their difficult non-concise *generalizing* arguments compared to the ease of GT generalizing.

- 13) *Focused determined boundaries*: “[NI] is likely to set boundaries to the inquiry on the basis of the emergent focus because that permits the multiple realities, etc., to define the focus because focus setting can be more closely mediated by the investigator-focus interaction etc.” (p.42). This sounds emergent, but is actually not. Boundaries are set by achieving the preconceived required descriptions cited in 5, which is a forced resolution to the research. This is diametrically opposed to the boundaries of pure GT, which are based on emergent theoretical saturation, constant delimiting, selective coding, theoretical sampling, core variable analysis, analytic rules and theoretical completeness. In short GT boundaries are based on emergence from the procedures of generating, in contrast to NI’s approach to gathering descriptions on the preconceived items cited in 5 as boundary making.
- 14) *Special criteria for trustworthiness*: “[NI] is likely to find the conventional trustworthiness criteria (internal and external validity, reliability and objectivity) inconsistent with the axioms and procedures of naturalistic inquiry” (pp.42-43). I have dealt with LINCOLN and GUBA’s view of credibility at length in “The Grounded Theory Perspective II” (GLASER 2003, Chapter 9). In short it does not apply to GT, and should not be allowed to remodel GT.

In sum, these characteristics of NI based on its axioms provide the foundation for their fuller elaboration in the remaining chapters of the NI-book. They are quite genuine for NI as a QDA method, HOWEVER when allowed, at points, to sweep GT into them as a QDA method, they unmercifully remodel GT. The result is that GT as an abstract conceptualizing method to generate theory is lost – totally lost. And further GT becomes subject to all the criteria for achieving worrisome accuracy of description, which do not apply, but have grave remodeling effects on GT.

3. More Details

These 14 characteristics are actually for experienced QDA methodologists rather simple, redundant and trite. They have been faced constantly in QDA research and methodology writings and in worrisome accuracy problems long before LINCOLN and GUBA wrote them up (see for example the extensive

reference list, pp.318 to 330, in MILES & HUBERMAN 1994). LINCOLN and GUBA elaborate on them at great length in a scholarly way in the remaining chapters of their book. I intend to skip and dip in these chapters to show yet again where GT is remodeled unmercifully by their QDA orientation.

Of course, I cannot analyze the remodeling of GT in each page of each chapter or this paper would itself become a book. I intend to give the idea of the style of remodeling conducted by GUBA and LINCOLN: a fractured style of multiple requirements at each moment in the research that is diametrically opposed to the autonomous flexibility of GT procedures to allow maximum emergence. LINCOLN and GUBA's apparent openness to the methods of emergence and to grounding is shut down constantly with the overwhelming multiple requirements of control.

The blocking of details will not be new to the reader. They will just be more grounded so the latent remodeling of GT pattern will leave few doubts. This will help GT researchers, who wish not to get swept up by naturalist inquiry and wish to handle the discussion with those researchers for whom NI is the path. Remember, as worthy a QDA method as NI is, it should not be allowed to co-opt and corrupt GT as originated. It should not borrow GT jargon and rhetoric to legitimate its very different procedures.

3.1 Paradigms

Harkening back to the beginning of this paper, the way people think is the way they want to know – a paradigm. It is the way they in which wish to make sense, to analyze. It is their worldview and normative “taken for granted” control of action. It is their unquestioned assumptions about life. A paradigm needs a methodology to arrive at “scientific” data. NI methodology, which provides LINCOLN and GUBA their science, contrasted with GT methodology is just quite different. It should not remodel GT as better or best.

NI wishes accurate description of the action regarding knowing and GT wants conceptualization of fundamental latent patterns occurring in the action. Their respective methodologies are different and result in different levels of abstraction. NI's methodology has a built-in, taken for granted, unquestioned assumptions many of which are directly linked to positivism, whatever their claims to a post-positive methodology. They are descriptive properties, the prime one being worrisome accuracy of descriptive findings. Another is the difficulty of unit generalization, because finding enough similarity between studied unit and another unit to generalize to is a troublesome task. Multiple realities, interaction-interpretation data and context feed into specifying accuracy and so do member checking and auditing. Value free in description is a problem no matter what the descriptive paradigm.

NI's approach to these descriptive issues are all inimical to the GT paradigm, it deals in concepts abstract of description's of time, place and people

and therefore it produces conceptual hypotheses. GT methodology stands on its own as a way to generate conceptual theory, or as a way of thinking conceptually. Thus applying NI to GT is remodeling and a takeover coming from assuming GT is a QDA method. Arguments over which paradigm to use and its methodology is useless. Neither fails, they are just different in their pursuit of different products. A merger between them will inevitably remodel GT as NI is a very popular ascendant QDA methodology.

Even as it tries to correct, the positivist grab of NI is shackling even though it claims post-positivism. It is deeply involved in credibility or trustworthiness – worrisome accuracy – objectivity and value free data problem and generalizability. It complies with positivism just to get to the “facts.” The writing of NI is tight details, bogged down in endless scholarship with no conceptual mastery. NI methodology is descriptive capture to the maximum as it quests its own legitimization.

GT legitimization stands on its own as grounded categories generated from data that it explains, not describes. It is an abstract of the rhetorical wrestles of NI in pursuit of establishing the credibility of its descriptive accuracy. GT is detachable from the data that it was generated from; it endures as conceptually general long after the collected data is stale from change. NI, of course, in its quest for accuracy is not detachable from the data it is describing and soon the description becomes stale.

Conceptual meaning of GT comes from the discovered latent patterns and pattern maintenance as the theory gets built up to a multivariate theory. The GT conceptual meanings persist and then perhaps are modified when the data changes or gets stale. NI meanings come from description of joint researcher-participants interactions and interpretations and change if they can keep up with descriptive changes. But usually they become stale with the data. This puts pressure on finishing a NI descriptive dissertation before the growing stale problem occurs. GT, of course, endures virtually forever and does not force premature finishing of a dissertation or research manuscript. Furthermore the generality of NI units is potentially a highly stale situation, whereas GT generality of concepts endures forever.

The preempting progression of NI description from pre-positivism through positivism to post-positivism seems controversial for GT when it is kept conceptual and not remodeled to a QDA method. Paradigm changes in researchers are slow and almost imperceptible. Therefore the novice is more open to the learning the GT paradigm for quickly taking it on and rigorously applying it in research. It is hard for those previously trained in QDA to change their paradigm from a QDA orientation to a GT orientation, because the skill levels and the latent learning curves are so different: descriptive skill procedures contrasted with ability to use conceptualizing procedures. NI remodeling GT, again, is a great loss to the latter.

3.2 Research design

LINCOLN and GUBA assert from the start of their book “what it means to design an NI study in view of our insistence that the design cannot be given a priori, but must emerge as the study proceeds” (p.225). This certainly sounds like the GT approach, but sounding is as far as its goes before the clamp down (forcing) for trustworthy description becomes their concern. They suggest ten design elements that would clearly derail a pure GT. They are as follows:

- 1) “Determining a focus for the inquiry”: This appears at first glance that they suggest having a substantive area in focus as in pure GT. But no they intend to not let a problem emerge. They assert that no inquiry can be conducted in the absence of a focus. For LINCOLN and GUBA the focus is on a professional problem, or an evaluation or policy. This is an a priori focus. Whereas in GT the problem emerges (see GLASER 1998, Chapter 8).

Determining a problem on an a priori focus provides for a NI inquiry (1) the boundaries of the study or the proper terrain of the inquiry and (2) determines the inclusion-exclusion criteria for new data. Of course, GT boundary and inclusions are emergent solely on theoretical saturation of categories and their properties, and delimiting tactics for data collection – theoretical sampling and data analysis, theoretical completeness, memo bank saturations, open to selective coding, etc. These GT procedures tap emergence. The LINCOLN and GUBA NI focus forces a priori boundaries. Remember that GT is emergently bounded and the data is bounded by the generated theory. A QDA description, like that from NI, can be endless without forcing bounding criteria which are required to judge an end to the research which is paradoxically called full description. GT emergent boundaries are built on relevance and fit, while NI boundaries, being arbitrary, easily give descriptive non-valence data, however much the human or professional interest. For GT, data inclusion is always emergent as theoretical sampling continues for the emergent categories. To buy into the a priori NI focus would severely block pure GT.

- 2) “Determining fit of paradigm to focus”: According to LINCOLN and GUBA the paradigm has axioms, or basic beliefs, and they must be adhered to in the focus of a research. If NI is the paradigm of choice, they say, the initial design should reflect consideration of the axioms. This is quite an order for a researcher to keep in mind. It imposes a complex ideology to implement at the same time as the inquiry proceeds. Compared to GT, this is an inordinate non-grounded forcing of the research, if the reader can remember the fourteen axioms cited above. GT’s axiom is simple: let’s see what is going on and it’s, “whatever emerges.”

For example, LINCOLN and GUBA say: “First, is the phenomenon something about which respondents are not likely to be forthcoming” (p.229). Or can we trust the respondents to tell it like it is, so we get NI. This is irrelevant for GT, it is what is emerging – *properline* data – and it resolves

the respondents' "conflict in interests" if there is even a conflict. "Half-truths or falsehoods that respondents supply," "suspect of deception, lies or fronts may characterize an inquiry scene" (p.231) and these bother LINCOLN and GUBA. They compromise their worrisome accuracy or in their terms "trustworthiness." For GT this is excellent data on what is going on e.g. organizational cover-ups or automobile sales.

The occurrence of conditional constraints that block NI, like untoward standards of an audience or committee, are just more data for GT on what is going on. The latent control becomes a category in the more comprehensive generated theory.

- 3) "Determining the fit of inquiry paradigm to the substantive theory selected to guide the inquiry": This is only okay for GT when using a substantive GT with emergent fit to the new data, *not* to a paradigm. NI can go either way: using an extant theory or letting one emerge. The extant theory can be a GT or just a forced one. LINCOLN and GUBA say "it is important to assess the degree of fit between paradigm and substantive theory" (p.232). Fit to a paradigm, not to the data, legitimates received theory application a priori. This is clearly inimical to GT. Even a GT that does not have emergent fit to the new data can be characterized as a forcing theory even though it fits the GT paradigm.
- 4) "Determining where and from whom data will be collected": NI identifies the phenomenal group wished to be studied and then it goes for descriptive redundancy – informational isomorph – or getting new information reaches diminishing returns. Again GT is not bounded by such criteria. GT goes for theoretical conceptual saturation of categories not redundancy and theoretical sampling goes for site spreading (see GLASER 1998, Chapter 10, op cit) once the initial site for research is saturated.
- 5) "Determining successive phases of the inquiry": NI has three phases: "orientation and overview," "focused exploration" and the "member check." There are significant overlaps between these phases. One can be doing one at the same time as the other, while providing a timeline for each. GT procedures are not phased as such. GT procedures are cycled and go on simultaneously, sequentially, subsequently, serendipitously and scheduled when possible. This phasing emerges with the theory as it drives the research. For example, some researchers discover the core category at the beginning and go directly to selective coding and some do not discover it until much later in the research after much theoretical sampling, saturation and memoing.
- 6) "Determining instrumentation": LINCOLN and GUBA say "the instrument of choice is the human. The human is the initial and continuing mainstay" (p.236). The human may be one or a team of persons. Composing teams and then their continual training for improvement is an issue for LINCOLN and GUBA. For GT, whatever instruments that bring results are used; and they are always used by the researcher, who is human. So what LINCOLN

and GUBA are saying goes without saying, but with flexibility for GT. What is clear also is that the constant perfecting of human instruments by supervision and mutual scrutiny by equals for LINCOLN and GUBA is a constraining, stifling condition for GT flexibility.

Perfecting human instruments also is part of the LINCOLN and GUBA's driving quest for trustworthiness demanded by them, which is not relevant for GT conceptualizations. Tight control over their human instruments sounds too bureaucratic and stultifying for GT discovery. It is in stark contrast to the autonomy given to the GT researcher, who is not characterized as an instrument, but as just a researcher interested in generating a substantive theory. This talk about "instrument" sounds very positivistic to be a view of credibility. LINCOLN and GUBA say "the human instrument provides an easy way to obtain member checks to make apparently non-credible data credible" (pp.239-240). These problems of worrisome accuracy do not apply to GT conceptualization.

- 7) "Planning data collection and recording": LINCOLN and GUBA agree to all forms of data collection and focus on fidelity and structure. While they like the fidelity of taping and video, they see the benefit, as in GT, of the non-threatening and selective nature of field notes. But they have a purpose for description, not conceptualization, so their view of field notes tends to the accuracy idea, not latent pattern discovery.
Regarding the structure, though LINCOLN and GUBA start with open interviews they wish to get to constructing "detailed and specific interview and observational protocols, so they can check off pre-structured responses" (p.240). In contrast, GT theoretical sampling varies constantly in openness and some light structure is there to help theoretically conceptual saturation, only to open up again as other categories are pursued. This is part of the constant cycling of the research as memos are built up and matured.
- 8) "Planning data analysis procedures": LINCOLN and GUBA say that "data analysis will be carried out in an open-ended way following the steps called in the 'constant comparative method' " (p.241). I reviewed their remodel of the constant comparative method in a paper (see GLASER 2003, Chapter 10, op cit.). They compare for description purposes using negatives; they do not compare to conceptualize whatever emerges as in GT. They are consistent with GT in their suggestion that NI begins with the very first data collection.
- 9) "Planning for logistics – the project as a whole, field excursions both prior to entering and while in the field, following activities, and closure and termination" (p.242). This section deals in heavy detail with schedules, budget, policy boards, peer debriefing, external audit. The reader can see that LINCOLN and GUBA require that oversight again is tight control and stifling in the name of trustworthiness. This may be contrasted to GT research in which casual development and progress based on emergence is

designed into the flexibility, on sight judgment and autonomy of a GT researcher or research team.

- 10) “Planning for trustworthiness”: The major issue for LINCOLN and GUBA is trustworthiness. In the preface they state: “Chapter 11 raises the thorny issue of trustworthiness. Why should the reader of an inquiry report believe what is said there?” On page 287, LINCOLN and GUBA introduce Chapter 11 (after much beating up the issue at every turn before this page) with the following paragraph:

All the while the naturalist must be concern with trustworthiness. In the final analysis, the study is for naught if its trustworthiness is questionable. Activities such as maintaining field journals, mounting safeguards against common distortions, arranging for on-site team interactions, triangulating data gather referential adequacy materials, doing debriefings, and developing and maintaining an audit trail are all directed either to increasing the probability that trustworthiness will result or to making it possible to assess the degree of trustworthiness after the fact.

At the beginning of Chapter 11 LINCOLN and GUBA described their dreaded fear – accusations of untrustworthiness: “the naturalist inquiry soon becomes accustomed to hearing charges that naturalist studies are undisciplined: that he or she is guilty of ‘sloppy’ research, engaging in ‘merely subjective’ observations, responding indiscriminately to the ‘loudest bangs or brightest lights’ “ (p.289). The reader can read this extensive detailed chapter on how to establish 1. truth value, 2. applicability, 3. consistency and 4. neutrality. The synonyms for trustworthiness abound unmercifully and incessantly on how to achieve this desired “objective” trustworthiness.

LINCOLN and GUBA say at the end of Chapter 11: “The techniques discussed in the preceding pages apply specifically to the establishment of credibility, transferability, dependability and confirmability” (p.327). As an afterthought they add in even three more trustworthy making techniques. The techniques vary on the odious harassment of routine QDA researchers, if they care to follow them.

I will let the QDA methodologists take on LINCOLN and GUBA, as well they might to save the credibility of qualitative description from this worrisome accuracy onslaught by these doubt sewers of honest researchers and their best efforts. Here we return to the purpose of this article, which is to show the remodeling of GT by QDA approaches, so as to extricate it from the ascendant QDA methodology. LINCOLN and GUBA have no sense of what data really is; they still buy positivist objectivity and therefore are so worried about accuracy. Their preface statement and subsequent assertions on trustworthiness are sociologically and simply naïve! First socially structured vested fictions run the world. If the reader doubts this, he/she should take on his/her local structures fictions with truth and see how far they get socially and personally. The power of these fictions are a functional requirement of social organization. Due process changes are infrequent.

Second, in view of these social fictions, predominant data received in field work, documents, videos etc. is properline data and vague data and not far behind is professionally interpreted data (see GLASER 1998, op cit) The researcher can trust to not get baseline or true data. The researcher will not get what is actually going on usually, but will get the properline data on how to see it, how to interpret it and how to blur it with vagaries. For the GT researcher this is what is going on to maintain current social organization. From this data he/she generates an abstract theory to explain action in the substantive area, because this kind of data is system maintenance data. I have read many GT papers that in arriving at conceptualization of the latent patterns – categories – they can show the actuality of goings on by its properlining.

I will leave to the QDA researchers to decide what kind of accuracy descriptions they will arrive at from these inaccurate forms of data. And I leave it up to them whether or not they refer to the incessant, extensive discussion of LINCOLN and GUBA to achieve their goal, to wit they say:

The case study mode lends itself well to the full description that will be required to encompass all of the facets and make possible understanding on the part of a reader judgments about the trustworthiness of such a process, which cannot be made with conventional criteria: criteria devised especially for and demonstrably appropriate to NI are required (pp.359-360).

But to toss out these forms of data inaccuracy is a great loss to GT, if it is remodeled by NI. Since “all is data” for GT, these data must be used.

Withstanding the LINCOLN and GUBA scholarly flower talk is not easy for GT. NI strictures will crush, bash, coopt and corrupt the innocent GT researcher, who does not yet understand the process of constant modification by proliferation of properties of categories using the constant comparative method of conceptualization and all the procedures by which it is supported, especially conceptual saturation, delimiting and theoretical sampling. NI will ruin a good GT by default remodeling. LINCOLN and GUBA foster this outcome by constantly seeding their book with GT jargon and remodeling some GT procedures. NI derails conceptual purpose, with the time wasting of excessive data collection, descriptive analysis and the constant myriad of “checks” on accuracy, such as audit trails, member checks, logs etc., etc.

Let me give a brief example of how GT is modified to include more data, which apparently makes it look inaccurate, but increases explanatory power. In a general theory of cautionary control Barry GIBSON (1996) has generated a hypothesis from his dentaling data – the more intensively formulated the rules of cautionary control, the greater of the growing deviance from the rules that varies by a typology of implementors. If one looks to the operating room one can say that the greater the rules of cautionary control the greater the compliance with them, by adding visibility and the strictness of enforcement because of dire consequences.

So we have the variable of adherence to cautionary rules varying from growing deviance to growing compliance. Airport security is somewhere in the middle but we need data. We have here exemplified the power of modification increasing the explanatory power of a GT on cautionary control. Indeed, accuracy is moot here: a non-issue and to have applied it would have mused the GT to untrustworthy in contrast to modifications increasing its explanatory power.

NI has put the sociology of worrisome accuracy on the map. Then LINCOLN and GUBA took on the self-appointed task to course the route to it: they call it trustworthiness. Then they implied by default and by express clarity that the research was not worthy of belief without it, *so* best follow their course! Let, I say again, the QDA methodologists stave off this of their routine research. All I can say here is that GT is not immune to this doubt sewing, until seen in its pure conceptual light – conceptual theory – and *not* as another QDA method. I am saying to the reader: Do not let NI remodel GT and block pure GT research.

LINCOLN and GUBA in their zeal for accuracy do not realize what data truly is, its variable true kind of accuracy for social organization and their distortion of absolute accuracy. LINCOLN and GUBA do not understand the abstract freedom of conceptualization from time, place and people. In advocating their throttling credibility framework they do not understand the humble nature of researchers in the field just trying their best with limited resources. *Nor* do they understand their abuse of consumer's intelligence and ability to judge their complex trustworthiness, to screen descriptions through their culture and position perspectives and to take things tentatively or under advisement until more data occur. Consumer care is not mentioned once in their book and what is the research for, if not for them! GT considers at length all these problems.

Much of this may not be understood by the reader without a thoughtful reading of and a returning referencing to my books: "Theoretical Sensitivity" (GLASER 1978), "Doing Grounded Theory" (GLASER 1998) and "The Grounded Theory Perspective" (GLASER 2001). I keep trying to pierce, to get to essentials and to summarize LINCOLN and GUBA's massive, detailed, non-conceptual onslaught. I am sure the reader has my incessant point in mind. To continue writing up in this paper the detailed analysis in my memo bank on trustworthiness techniques would be too extensive. However in interest of the reader, I would return to the same point: leave GT out of NI. GT is not for excessive techniques for establishing accuracy of findings, it is not for replicable description. It is simply for conceptual theory induction and constant modification. It does not require the prolonged fieldwork and culture soaking of QDA methods for description and especially the lengthy rigorous techniques of assuring credibility offered in NI.

QDA researchers and GT researchers do the best they can within their skill level. To worry about dishonesty in routine research is an insult to the responsible, honest level of researchers. They say that member checks, logs and auditing prevent researchers from altering data to suit their theory. If altering does occur in an isolated case, there is little to protect us against it anyway, until corrected by future research, if then. Trust in the researcher is a research value that applies absolutely. Of course in GT, unlike NI, there is no temptation to alter data to suit theory, since the theory is generated inductively from the data. Data is not forced to fit a theory. In GT, theory is not generated based on preconceived professional wish and career opportunism.

GT is always as good as far as it goes and is then modified by constant comparison with more data. It produces stable enduring concepts with immense grab. For example, the category of pseudo-friending as a form of client control is spawning theory extensions in many areas. It is easily generalizable to many situations of people control. Categories are reifications with good fit, but still can be changed to rename the same latent pattern. Modification not accuracy is the issue. I prefer the category of credentializing, others like licensing, degreeing, permitting or qualifying, but the pattern is the same.

4. Mutual Shaping

LINCOLN and GUBA (1985) write a very erudite chapter on a critique of causality. They state: “why scientists have been enamored of the causality concept ... if causes are the key to prediction and control, knowledge of causes is tantamount to power” (p.129). Their critique of many definitions of causality: deterministic, linear, necessary, sufficient, multiple, timing is fine. They come up with the concept of mutual simultaneous shaping as a replacement. It refers to

everything influences everything else in the here and now. Many elements are implicated in any given action, and each element interacts with all of the others that change them all, while simultaneously resulting in something that we label as outcomes or effects. But the interaction has no directionality, no need to produce that particular outcome, it simply happened as a product of the mutual shaping (p.151).

Thus LINCOLN and GUBA (1985) still keep the preoccupation with causality, but in their way. In GT we call mutual shaping the interaction of effects; it was, originated by LAZARFELD and used at Columbia University 20 or more years before LINCOLN and GUBA’s description. If they had read “Theoretical Sensitivity” (GLASER 1978), they would see it as just another theoretical code. So much for their eruditeness – lacking and selective.

From the point of view of GT their preoccupation with causality is a pet theoretical code for themselves and their referred others. It is forcing the theory

that may be integrated by possibly many other theoretical codes. In “Theoretical Sensitivity” I listed 18 theoretical coding families. In “Doing Grounded Theory” (GLASER 1998) I listed some more. There are still more. Which one to use in GT is a question of emergence during sorting of memos. Theoretical codes must earn their relevance as all variables in GT. Causality, however defined, is just one. The most popular one. A frequent pet to some researchers is basic social process. Our book on “Awareness of Dying” (GLASER & STRAUSS 1967) was built on a typology code that emerged. A recent dissertation I read (BROWN 1996) was based on the binary code. Some coding is just a range, a degree or dimensional. It depends. Again I can only warn that NI can remodel GT into a search for causality and severely restrict its generation of theory.

Lastly, LINCOLN and GUBA (1985) state that “phenomenon of study, ... *take their meaning as much from contexts as they do from themselves*” (p.189, italics in original). This, again, is a forcing of a theoretical code for GT, however well it may work for NI. How a context influences a latent pattern – a category – regarding meaning is emergent, not forced. Meaning for GT may come from wherever it may be discovered. Contextualizing meaning may or may not be relevant for a theory’s explanation of how a main concern is continually resolved.

5. Grounded Theory

LINCOLN and GUBA demoted GT to just a consequence of the more general NI paradigm, ostensibly to put it into perspective, but latently to bury as just a QDA method that should follow NI. Although they refer to it virtually everywhere in their book, they give it merely four direct pages of discussion. Their discussion remodels GT automatically to a descriptive QDA method. They say “Grounded Theory, that is theory that follows from data rather than preceding them, is a necessary consequence of the naturalistic paradigm that posits multiple realities and makes transferability dependent on local contextual factors” (pp.204-205). We obviously had something else, very different in mind when codifying GT methodology in “The Discovery of Grounded Theory” (GLASER & STRAUSS 1967). The “something else,” which was our purpose and paradigm, is so far from NI paradigm that it is not worth spending time on. The differences are founded in the contrast between description and conceptualization and run wide and deep.

LINCOLN and GUBA consider GT as “local” theory, which has a descriptive implication and that is all. All substantive GTs have general implication far beyond the more local population used in the research. They are a foundation for developing formal theory to follow on the general implications as more data is compared into the theory. Also if site spreading theoretical sampling (see

GLASER 1998) is used the data goes far beyond local. Actually it is hard to restrain generalizing of the concepts of a substantive theory before an emergent fit is achieved.

LINCOLN and GUBA have no skill in handling their cited two attacks on GT: 1. “GT is inadequate because it is underdetermined because given a set of facts it is always open to multiple interpretation and can be extended indefinitely” (p.207). These are descriptive properties not relevant to conceptualization. GT discovers and conceptualizes the latent patterns of what is going on. It is always relevant. If a GT is accused as being interpretive, which is probably meaningless, it is a very relevant interpretation. I have not seen more that one theory emerge at any time in a research. Also as discussed above indefinite extension is wrong. GT is bounded by its delimiting properties. But like all theory, whether grounded or not, new research can extend it by modification. 2. “Second, it is argued that Grounded Theory is impossible to devise, because the raw data are themselves facts only within the framework of some other (perhaps implicit) theory. Thus a theory can only discover itself. That facts are “theory-laden seems to be well accepted among epistemologists” (p.207). This attack by LINCOLN and GUBA, a tautology, is meaningless and purely destructive. It goes nowhere. It makes GT meaningless, since it misses the point that GT conceptualizes data, but does not describe it.

Clearly this short, direct, rather empty treatment of GT combined with the prolific use of its jargon throughout their book and remodeling of its procedures in the rest of the book indicates the lack of LINCOLN and GUBA’s study of the GT books and their lack of experience in doing GT.

6. Final Comment

By now the reader has the idea, so I will stop this analysis of differences that could go on for many, many more pages, which could result in a book of contention like my “Basics of Grounded Theory Analysis” (GLASER 1992). At every turn LINCOLN and GUBA’s conception of NI co-opts and corrupts NI’s use of GT terminology and procedures. It remodels by default GT to just another descriptive QDA method. For GT, constructionism and value-free are just more variables in the data, causality which they call mutual shaping is just another theoretical code which may or may not emerge to integrate the description or conceptualization. “The only generalization is that there is no generalization,” LINCOLN and GUBA say in their Chapter 5 (p.110), which is both trite and an admission that generalization is very difficult to establish in QDA. For GT conceptual generalization is easy and frequent. “Establishing trustworthiness” (Chapter 11) takes credibility to an extreme degree – auditing and member checking – and none of their positions apply to GT (see above on credibility).

While a worthy QDA method, NI cannot be allowed to remodel GT at every turn. Under the guise of detailed, incessant scholar affirmations, LINCOLN and GUBA have co-opted, corrupted, mauled and mugged GT for their own purposes without any experience in actually doing a GT as originated. The genuine “grab” of GT – the idea and the terminology – has made its use quite productive to some and highly exploitable by others.

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