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From the Editor

This issue of the Review combines a strong focus on classic grounded theory methodology with our continued practice of featuring the work of novice grounded theorists. We begin the issue with a classic paper from Discovery, the seminal work of Glaser and Strauss (1967). **Theoretical Elaboration of Quantitative Data**, authored by Dr. Glaser, has been largely ignored by those who prefer to assign grounded theory to the qualitative paradigm. This paper, however, truly sets GT apart as a general methodology and not solely the purview of qualitative researchers. A careful reading may help to diffuse the persistent rhetorical wrestle that has developed through the fervent efforts of qualitative researchers to shape GT to the malleable boundaries and endlessly flexible designs that seem to define the qualitative research paradigm. As such, the QDA remodellers may have to rethink their conception of GT as simply a menu of method bits to be selected and combined as desired to produce, at best, conceptual description.

It is particularly interesting to note that Glaser speaks here of relaxing the rules of the quantitative paradigm to enable the generation of GT whereas the exact opposite has happened with the qualitative paradigm where the procedural package of classic GT has been so relaxed as to have effectively undermined the rigour of the methodology. This, of course, simply serves to emphasize the position of GT as occupying its own paradigm situated between the two traditional research paradigms.

Practically speaking, using quantitative data to do classic GT opens a vast realm of data to social scientists seeking to generate empirically grounded conceptual theory. It facilitates the process of analysis and conceptualization by offering a vast bank of conceptual indicators that have already been collected, organized and initially analyzed. As data collection is expensive

as well as highly time-consuming, learning how to work with extant caches of data can accelerate and densify conceptual elaboration in many studies. It may also encourage further scholarship among experienced GT researchers eager to further develop their analytic skill by learning to work with quantitative indices as their source of data.

Reading this leaves one wondering how anyone can continue to maintain that GT is a qualitative method - perhaps only out of ignorance of this important early work by Barney Glaser. It most certainly illustrates that GT cannot be confined to the qualitative paradigm but offers the same generative potential for conceptual theorization to quantitative research. Furthermore, it underscores the need for Glaser to write the book on doing quantitative GT!

The frequent misinterpretations and remodelled variations of grounded theory are the focus of Olavur Christiansen's paper (**A Simpler Understanding of Classic GT: How it is a fundamentally different methodology**). In seeking to offer a simpler explanation, Christiansen focuses attention on what he terms the "essential elements" of classic GT – the suspension of preconceived professional concerns in service to the emergence of what is really going on in the substantive field of study and the processing of this main concern through an emergent theoretical code as the core variable of the theory. He proceeds to detail how systematic adherence to these fundamental methodological principles through the application of the procedural package of classic GT produces a fundamentally different methodology than that commonly espoused as grounded theory within the qualitative research community where such requirements as extensive engagement with the literature, establishment of a preconceived theoretical framework from extant theory, interview protocols, etc. restrict and prevent the potential for theoretical emergence that is truly grounded in the data.

PhD candidate Naomi Elliott's paper (**Mutual Intacting: A grounded theory of clinical judgment in advanced practice in nursing**) offers a theory of the process by which clinical practitioners in community care settings work to assess

and adapt treatment so as not to jeopardize the patient-practitioner relationship, thereby keeping the relationship intact and in line with the patient's circumstances and needs. While Naomi's study is focused on clinical practice in community care, it holds general implications for the standard problem of goal oriented application. It proposes to explain how one determines the best application to goal when working within the constraints of what has to be applied and what the recipient of the application will allow, needs, and can tolerate.

To conclude this issue, we return to our focus on exploring the methodological development of classic GT. Two members of our Peer Review Editorial team – Alvita Nathaniel and Tom Andrews - offer their reviews of Dr. Glaser's latest book, **Doing Formal Grounded Theory** (2007). While many of us still struggle to master our skills in generating good substantive GT, Glaser challenges us to broaden our horizons by considering the many good substantive GTs "ready to be broadened and generalized into a FGT" (Glaser, 2007, p.1). After forty years of diligent scholarship on his part, his call begs our considered attention and scholarship!

- Judith A. Holton, Ph.D.

Submissions

All papers submitted are peer reviewed and comments provided back to the authors. Papers accepted for publication will be good examples or practical applications of grounded theory and classic grounded theory methodology.

Comments on papers published are also welcomed, will be shared with the authors and may be published in subsequent issues of the Review. See our website www.groundedtheoryreview.com for full submission guidelines.

Forward submissions as Word documents to Judith Holton at judith@groundedtheoryreview.com

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Theoretical Elaboration of Quantitative Data[1]

Barney G.Glaser, Ph.D., Hon.Ph.D.

Quantitative data is so closely associated with the current emphasis on verification that its possibilities for generating theory have been left vastly underdeveloped. However, some of our best monographs based on quantitative data indicate that they can be a very rich medium for discovering theory. In these monographs, discovery cannot be stopped, but breaks through both verifications and preconceived conceptual schemes to provide us with very interesting and important theory[i]. Yet, since the authors are still so focused on testing provisionally what they have discovered, their work is mostly written in the hedging rhetoric of verification. The result is that their statements present tests as merely "plausible suggestions." The plausibly suggested test should not be construed with our goal of the purposeful generating and suggesting of theory. The generating capacities of these sociologists and the richness of their research are, therefore, not given the fullest impetus.

Typically, discovery made through quantitative data is treated only as a byproduct of the "main work"- making accurate descriptions and verifications. When discovery forces itself on an analyst, he then writes his INDUCED hypotheses as if they had been thought up before the data were collected, so that they will seem to satisfy the logical requirements of verification.[ii] Purposeful generation of grounded theory is found usually, if at all, in short papers where a single carefully worked-out explanation of a hypothesis is offered, after an analytic wrestle between the rhetoric of tentative qualification and alternative explanations and the carefully researched, accurate data - a slight beginning for an adequate theory.

When the sociologist consciously starts out to suggest a

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[1] Originally published as Chapter 8 in Glaser and Strauss (1967). The Discovery of Grounded Theory: Strategies for qualitative research, pp.185-220.

theory plausibly, rather than test it provisionally, then he can relax many rules for obtaining evidence and verification that would otherwise limit, stultify or squelch the generation of theory. He must give himself this freedom in the flexible use of quantitative data or he will not be able to generate theory that is adequate (as we have discussed it) in terms of sampling, saturation, integration, density of property development, and so forth. In taking this freedom he must BE clear about the rules he is relaxing (which could not be relaxed for purposes of accuracy and verifications) and he should explain his position to readers. *The freedom and flexibility that we claim for generating theory from quantitative data will lead to new strategies and styles of quantitative analysis, with their own rules yet to be discovered*. And these new styles of analyses will bring out the richness of quantitative data that is seen only implicitly while the focus remains on verification. For example, in verification studies cross-tabulations of quantitative variables continually and inadvertently lead to discoveries of new social patterns and new hypotheses but are often ignored as *not* being the purpose of the research.

In this chapter, we shall present one new strategy of quantitative analysis that facilitates the generation of theory from quantitative data. It is a variation of Lazarsfeld's elaboration analysis of survey data.[iii] In our presentation we shall indicate how, at strategic points, the rigorous rules for accuracy of evidence and verification can be relaxed in order to further the generation of theory. To be sure, there are many styles of quantitative analysis with their own rules. Our focus here is an illustration of how these numerous other styles can also be flexibly adapted to generating theory. However, we do touch on some existing general rules of quantitative analysis (*e.g.,* indexing and tests of significance); the way they are relaxed for purposes of generating theory could apply to many styles of analysis. And we shall also develop some general rules governing how to relax the usual rigor of quantitative analysis so as to facilitate the generation of theory.

The organization of this chapter is based on the successive stages of building up to theory from quantitative data. We discuss in turn the most frequent sources of data used for generating theory, how one indicates his categories and

properties with the data, how one discovers hypotheses with his conceptual indices, and how the hypotheses are then theoretically elaborated. In an appendix to this chapter, we provide examples of theoretical elaboration. For some longer examples of certain specific points, we have referred the reader to other literature.

Secondary Analysis of Quantitative Data

The sociologist whose purpose is to generate theory may, of course, collect his own survey data, but, for several reasons, he is more likely to analyze previously collected data - called secondary analysis. Surveys are usually financed for providing large-scale descriptions of current populations; and the sociologist whose interest is in theory may not wish to be involved in this part of a study, for it takes considerable time and concentration that might otherwise be used for theoretical analysis. It is easier to analyze previously collected data, for then his only responsibility is to generated theory. Sometimes, of course, after the large-scale descriptions have been accomplished, the director of the study returns to his data to engage in secondary analysis for generating a theory on an idea initially stimulated by the earlier descriptive phase.

Generating theory is a more limited, narrowly focused effort (even though the theoretical concept may be very general) than presenting the broad description of a population given by the total survey. The description may involve thousands of questionnaire items, while the theoretical analysis only requires consideration of a few hundred.[iv] Therefore, the tasks of description and analysis can conflict unless the sociologist has adequate money and time (a likelihood only for the study director and a few assistants). Theoretical analysis of quantitative data is, of course, an opportunity to be taken by many sociologists other than study directors or their assistants,[v] and so most generation of theory from quantitative data will be based on secondary analysis.

Comparative analysis requires secondary analysis when populations from several different studies are compared, such as different nations or factories. Comparative analysis of groups internal to one study does not require secondary analysis, but again it often is.

Trivial data, such as found in market surveys on consumption of products, can also have very important theoretical relevance. For example, from a study of meat consumption one can gain knowledge about the life style of social classes. Secondary analysis is a necessity in such cases because sociologists with a theoretical bent do not usually collect such data.

When using secondary analysis of quantitative data for generating theory, one point must be kept clear. Because of the heavy emphasis on accurate evidence and verification of hypotheses, the analyst usually wishes to start out with the facts as facts. One limitation of secondary analysis is the difficulty of pinning down the accuracy of findings in what is necessarily a second hand view - often without much knowledge of collection procedures and meanings of data. Also, since populations are in constant change, we have no way of knowing whether a survey accomplished some years ago for other purposes still applies meaningfully to the specific population. This problem of accuracy is not as important for generating theory about a type of social unit as it is for describing a particular social unit or verifying a hypothesis. What are relevant for theory are the general categories and properties and the general relations between them that emerge from the data. These can be applied to many current situations and locations as very relevant concepts and as hypotheses of interest to sociologists and laymen, regardless of whether the specific descriptions yielded by the data are currently accurate for the research population. Secondary analysis, then, is uniquely well suited for the generation of theory but is often severely limited for description and verification- for which it is still mostly used, with a typical preamble about "limitations."

Another limitation of secondary analysis that makes its use in description and verification questionable, but does not affect the generation of theory, is the representativeness of the population studied. Accuracy is, of course, crucial in description and verification, and the sample must therefore be carefully chosen by some form of random sampling. Secondary analysis of a random sample chosen for other reasons may introduce systematic and random biases into the secondary study, making claims to accuracy questionable. Indeed, it is often

difficult to ascertain from previously collected data what kind of sample was taken for what purpose, since records may have been destroyed, lost, misplaced or made unavailable. Many important questions concerning the sampling become unanswerable, such as how many people did not respond, how many cards were lost, and how many questionnaires were not usable. But when theory is the purpose, there are two reasons why the representativeness of the sample is not an issue. First, the direction of a relationship used to suggest a hypothesis is assumed to exist until disproved, in both biased and unbiased populations; and, second, theoretical (not statistical) sampling guides the choosing and handling of the data.

What is more important for generating theory is the *scope* of the population, which can be increased when the analyst is less concerned about representativeness. Representativeness usually requires some purification of the original sample to obtain a clear-cut population for a smaller study; the sociologist's take for his analysis carefully stratified samples from a larger survey sample. This tactic cuts down on scope by weeding out the possible (but never proven) "contaminating" influences of some respondents. For example, one may wish to take all scientists out a national survey for study, but then, if he purifies the group by weeding out all but the PhD's, he loses the population scope that could have been afforded by keeping the scientists with the MD's, MS's, and BS's.

Concepts and Indices

In the last decade, the flexible use of concepts and their empirical indices in quantitative analysis has been advanced greatly by Lazarsfeld. A number of publications[vi] have carried his work on the "process by which concepts are translated into empirical indices." We wish to mention here only a few general points and urge the reader to study the footnoted references for the general argument and the examples.

When the discovery and generation of theory is the goal of a survey analysis, "crude" or "general duty" indices (as described in detail by Lazarsfeld) sufficed to indicate the concepts of the theory and to establish general relationships between them, which in turn become the basis for suggesting hypotheses for the emerging theory. Similar crude indices,

usually a single questionnaire item or a simple summation index of two to six items, are interchangeable when based on similar, but different indicators. "Interchangeability of indices," as Lazarsfeld demonstrates, means that we obtain the same findings in cross-tabulations with other variables when two indices of the same category are based on reasonably similar but different sets of indicators. Therefore, the analyst does not have to be certain that he has the most accurate index, judged on the basis of either precision or the best set of indicators.

Crude indices, when correlated with other variables, also yield the same relationships in direction as the more precise indices yielded by factor analysis, latent structure analysis, a Guttman scale, or elaborate scales involving dozens of items. Since for generating theory we are only looking for general relationships of direction - a positive or negative relation between concepts, and not either precise measurement of each person in the study or exact magnitudes of relationship - it is easier, faster, and considerably more economical to use the crude index. Even when crude indices result in obvious misclassification of some cases, they still yield the information necessary for generating a grounded theory.[vii]

Crude indices of categories or properties can also be based on either a single questionnaire item or a series of items summed into an index. However, for indices of the core categories, it is perhaps preferable to use two to six item summation indices, since the category will usually be based on at least two dimensions and each should be indicated by at least one item. Further, crude indices need only be dichotomized to obtain comparative groups, not cut into several groups. Whether an index is cut in two, three, or four groups, the same general relation will appear when it is cross-tabulated with another variable, provided that the cutting point is statistically established with criterion variables as a meaningful break in the data.[viii] Dichotomizing an index is financially economical and saves cases for cross-tabulation when the number of cases is small and when the analyst engages in multivariate analysis of three or more variables. Indeed, even if a trichotomous index is used, the analyst, except in cases of exceptional patterns, still ends up talking about the general positive or negative relation between two variables.

When generating theory, validation of a core index - demonstrating that the index measures the concept to a sufficient probable degree - need not be a special operation in which a theoretically relevant relation between two variables is sacrificed from the substance of the analysis itself to prove the validity of the argument, as is typically necessary in verifications.[ix] If the index "works" - that is, if it is consistently related to a whole series of variables that, when put together, yield an integrated theory - this is validation enough of a core index. Integration of the theory is, in fact, a more trustworthy validation of an index than the standard method of merely showing that an obvious relationship exists between the index and another questionnaire item, and that therefore the index must measure what it is supposed to measure.[x]

For example, the core index of "professional recognition" in *Organizational Scientists* (Glaser, 1964) could easily have been validated by showing that professional recognition is positively related to receiving promotions; but instead the whole book shows the validity of the index by the way the substantive theory on scientists' organizational careers is integrated.[xi] In fact, the theory becomes integrated around the core index of recognition because of the multiple relationships with that index, indicating that the theory works - it provides relevant explanations and consequences of organizational careers. Lazarsfeld's methods for specifying concepts and for selecting sub-sets of items to construct indices of the concepts are excellent for ensuring that categories will fit the data and will work or be relevant. This fulfillment of the two major requirements of grounded theory explains why the index becomes validated by the whole theory.

We make these statements in the service of generating theory. If the analyst wishes to describe or verify, these issues must be argued on different grounds, because his problems of precision, dichotomization, and validation of indices are different. The analyst must therefore be clear about his purpose. However, most survey analysts are *not* clear, because Lazarsfeld never has made the distinction between the purpose of generation and those of verification and/or description with accurate findings. He writes not of theory but of "empirical propositions" and "statistical relations." We see clearly how his

work on concepts and indices is valuable for generating theory through conceptual indices and general relations between them. But others who wish to discover "facts" and verify hypotheses, especially by secondary analysis, must argue for Lazarsfeld's methods on their own. Indeed, there are many sociologists who use his methods and stay on the empirical level of description or harp on their findings in the verification rhetoric, even when attempting to suggest theoretical hypotheses.

The survey analyst chooses his categories in the same manner as the researcher doing qualitative analysis. An initial scheme of concepts and hypotheses, usually applied to quantitative data in attempting verifications, is not needed. Concepts whose fit will be emergent are found in previous descriptive analyses with quantitative data, or in other quantitative or qualitative data on the same subject. Also, categories and properties emerge during the collecting and analyzing of quantitative data as readily as they do with qualitative. It must be remembered that qualitative data suggesting a category may also be used as another slice of data for the quantitative analysis.

The theoretical relevance of the concept is soon demonstrated by whether or not its index actually works in a multitude of cross-tabulations. If the index does not work, then the analyst should question the theoretical relevance of his concept before he questions the method of index formation. In quantitative analyses, it is typical to observe a non-emergent category derived from a logico-deductive theory (say, on self-image, role conflict, or status congruency), forcibly indexed - and then found to be related to nothing of theoretical relevance. The analyst then finds fault with the precision of the method of index formation, rather than with the relevance of a category derived from an undergrounded theory, since he seldom questions his faith in the logico-deductive theorist when the latter is a charismatic figure in the profession. Much survey analysis fails for this reason, but we hear failures only through friends; tact prevents citing examples.

It is possible to index any category, but while, with emergent categories, the analyst is almost sure to discover many relations between indices, "ought" categories, from

undergrounded theories are a risk. To stay on the empirical level, using no theoretical categories, is one alternative to taking the chance of directing theoretical research through logically deducted categories such an "anomie" or "authority relations." Yet people who do not trust logico-deductive theory, but wish to do theoretical work, could very safely attempt discovery of grounded theory as another alternative.

Discovering Hypotheses

In generating theory, preconceived hypotheses are not necessary for correlating or cross-tabulating two variables (called runs) with indices of core categories and properties. Indeed, the rule for generation of theory is *not* to have any pre-set or valued hypotheses, but to maintain a sensitivity to all possible theoretical relevances among the hundreds of possible runs afforded by large surveys. In contrast, necessarily preconceived hypotheses direct exactly what two variable correlations to use as tests in verificational studies. Indeed, verificational rules state that data should be collected for tests *after* the hypothesis has been formulated- though they seldom are. For generating theory the data can be collected at *any time*. As we have said, it is usually collected beforehand because most discovery and generation is a secondary analysis of data collected for other purposes, and because the hypotheses come after the analysis - they are suggested from findings, not tested with them.

In order to saturate all possible findings for suggesting hypotheses, the analyst may take his core concepts and run them with literally every other questionnaire item in the survey that seems remotely relevant to his area of interest.[xii] At this point, the theory of the core indices starts to emerge. Clusters of items are discovered as associated with the index. Indeed, this strategy (an unbelievable "sin" in verification studies) virtually discovers theory for the analyst by providing associations to be conceptualized and analyzed.

Table I Cosmopolitan Orientation			
	Motivation to Advance Knowledge		
	High	*Low*	*Difference*
Personal contacts outside organization are very important as sources of scientific information	56%	35%	+21%
If had to, would prefer to move to a university	72%	43%	+21%
Belonging to an organization with prestige in the scientific world is of the utmost importance	40%	21%	+19%
Very strong involvement with close professional work associates	40%	26%	+14%
Very strong sense of belonging to section (principal research group)	44%	27%	+17%
Basic research, as a result of clinical program, is likely to			
-benefit	42%	56%	-14%
-suffer	40%	29%	+11%
Those who would worry about a substantial emphasis on applied as well as basic research	38%	19%	+19%
Base for each percent	(186)	(146)	
Local Orientation			
	Motivation to Advance Knowledge		
	High	*Low*	*Difference*
Having an important job in the organization is of the utmost importance	30%	12%	+18%

Association with high-level persons having important responsibilities is of the utmost or considerable importance	55%	42%	+13%
Having a very strong sense of belonging to the organization	31%	19%	+12%
Interested in a higher level job in the organization which entails stimulating or advising subordinating professionals about their work	77%	67%	+10%
Interested in a higher level job entailing administrative planning or coordination	68%	56%	+12%
Base for each	(186)	(146)	

He induces a theory simply from the general relationships he has found. He need not concern himself with theoretical explanations of what he has found in comparison with what he was supposed to find, as is done in verification studies.

One comparative strategy for generating theory from findings is to compare clusters of relationships within the context of the emerging theory. For example, in Table I we see that "motivation to advance knowledge" (a crude index) is consistently related to two clusters of items, those indicating a *cosmopolitan* orientation – toward the profession – and those indicating a *local* orientation - toward their research organization. Thus we discover and suggest theoretically that highly motivated scientists within research organizations devoted to basic research (a structural condition) possess the property of being local-cosmopolitans.xiii Table II bears out the suggested hypothesis, by showing that in their work activities, highly motivated scientists are both local and cosmopolitan oriented: as more working hours and activities are added to the work week, the highly motivated scientists spend more time on both professional and organizational activities.

Table II Motivation to Advance Knowledge			
Consecutive addition of hours per week spent on various work activities	*High*	*Low*	*Difference*
21 or more hours: own research	76%	61%	+15%
36 or more hours: plus other professional productive work	63%	48%	+14%
41 or more hours: plus non-productive professional work	69%	48%	+21%
51 or more hours: plus other organization activities for total work week	55%	48%	+17%
Base for each percent	(186)	(146)	

Consistency Indices

These two variable runs showing clusters of associations are analyzed comparatively in two ways: *within* and *between* consistency indices. A consistency index is a list of single questionnaire items which all indicate the same category, such as cosmopolitan, and all relate separately to the core index in the same constant direction. The indicators are not added together first and then related to the core index, as in summation indices. Summation indices are best for the core categories, but consistency indices are best for the categories to which a core index is to be related. This strategy allows the analyst to see how the core concept relates to each individual indicator of another category. If inconsistencies in associations between the consistency index and the core index occur for what appeared to be substantively consistent indicators, they are quickly caught and compared to the underlying meaning of the differences within the set of indicators and the emerging theory.

Table III Percentage of Researchers with High Motivation as Related to Their Previous Experience			
Background Experience	*Previous Experience %*	*No Previous Experience %*	*Difference %*
Emphasis on advance of knowledge:			
University Employment	65 (180) ♦	45 (152)	+20
Research and teaching	61 (247)	40 (58)	+19
PhD. Education	62 (164)	40 (58) † 55 (110)#	+22
Emphasis on application of knowledge:			
Medical or clinical practice	58 (244)	55 (88)	+3
Hospitals	57 (111)	55 (121)	+2
Industry	58 (78)	56 (254)	+2
Private practice or business	58 (36)	56 (296)	+2
Government agencies	48 (117)	61 (215)	-13
U.S. Public Health Service	47 (68)	58 (264)	-11
	♦ Figures in parenthesis indicate number of cases.	† Education less than doctorate. # M.D.	

For example, in Table III we see that within the consistency index of applied experience, high motivation to advance knowledge (not to apply it) is not related to previous experiences in private or group practice, hospitals or industry. [xiv] These particular applied experiences then, we theoretically suggest, neither engender nor inhibit motivation negatively related to applied experience in government agencies and the U.S. Public Health Service, or (theoretically) why do these experiences inhibit or reduce motivation to advance knowledge? We suggest that it is because these two experiences, in contrast

to the first four, imply routine service in the application of knowledge.

If all items on "experience in application of knowledge experiences" had been combined first in a summation index, and then related to motivation, these inconsistent comparisons of groups within the consistency index (from which we discovered strategic structural conditions varying the core category) would have been missed; hence, so would an important hypothesis of the theory: the effect of "routine" applications on the scientists' motivation to advance knowledge. The property of "routine application" would have been missed had the analyst simply constructed a summation index, since all the items on applied experience would have seemed internally consistent when tested - all items positively related to each other. Therefore there would have been no suspicion that correlating an applied experience index with another index was actually summing inconsistencies.

Comparisons *between* different consistency indices are also used as a strategy of comparative analysis. We saw in Table I that, since high motivation is positively associated with both a local and cosmopolitan orientation, the analyst can suggest, on the basis of this comparison between consistency indices that scientists highly motivated in research are local-cosmopolitans in a basic research organization.

These two comparative strategies - comparing within and between consistency indices associated with a summation index - occur in three or more variable associations also; but then the analyst is using traditional analytic strategies, which we discuss in the next section. Also, once a detailed analysis of the association with a consistency index is accomplished, then the consistency index can be summed and dichotomized for further analyses with three or more variables. These analyses are more complicated, requiring reduction of details and the saving of cases for cross-tabulation. For example, the first part of Table III shows motivation to advance knowledge related to a consistency index on one kind of previous experience in science - experience emphasizing advancement of knowledge. Table IV shows the summation index of previous experience in science related to two other summation indices - motivation to advance knowledge and professional recognition - for the theoretical

purpose of suggesting hypotheses bearing on the interaction between the three indices.

Table IV Relation of Recognition to Science Experience for Research Workers with High Motivation			
Science Experience	*High Recognition %*	*Low Recognition %*	*Difference %*
Full	76 (46) ♦	69 (52)	+7
Some	68 (75)	42 (99)	+26
None	44 (23)	35 (37)	+9
♦ Figures in parenthesis indicate number of cases.			

Test of Significance

Statistical tests of significance of an association between variables are not necessary when the discovered associations between indices are used for suggesting hypotheses. Selvin[xv] has argued that this rule should be relaxed for all survey analysis, but he can take this stand only because he has not made the distinction between the generating and the verifying or describing purposes of research. He questions whether these tests are appropriate with survey data, since the statistical assumptions necessary to use them cannot be met with such data and also are ineptly applied according to general sociological theory. His critics, however, seem to be more concerned with keeping the tests of significance to ascertain accuracy of evidence used for verification and description.[xvi] We wish to stay clear of this controversy because we are making an argument concerned only with these tests in relation to the generation of theory.

Testing the statistical significance of an association between indices presents a strong barrier to the generation of theory while doing nothing to help it, since the resulting accuracy (if one can actually trust the test) is not crucial. These tests direct attention away from theoretically interesting relationships that are not of sufficient magnitude to be

statistically significant. The analyst usually does not think of the associations as a grounded foundation for a hypothesis, although weak associations may be highly theoretically relevant. Also, the test, not the relationship, may be weak.

Believing that he has no findings relevant for generating theory, the analyst also usually neglects to ask what the partial relationships look like under several conditions. It is easy to forget that partials may be statistically significant even if the general relationship is not because the partials can cancel themselves out. "Cancelling out" means that the relationships may be positive under one condition and negative under another; so that when combined the partial relationships cancel themselves out to result in a weak general association. However, it is theoretically very relevant and interesting to be able to say how conditions minimize, maximize, or cancel out a relationship. Also, even if partials are weak, the theoretical relevance of a weak relationship between two indices may be the *weakness* itself.

Believing in tests of significance can also dissuade one from trusting consistent but weak relationships within and between consistency indices. Yet consistency validates the merit of relationships when it comes to the plausible reasoning required in a credible theoretical analysis.[xvii] And, as just noted, whether the level of the relationship is zero, weak, or strong; it may, if relevant, be grist for the theory.

A belief in tests of significance can also, in the process, direct one's attention away from theoretical relevance of content toward confusing statistical significance with theoretical significance, and a statistical method labelled "analysis" with theoretical analysis. Merely being statistically significant does not mean that a relationship is or should be of theoretical relevance. Such relevance depends on the meaning of the association as it relates to the theory. Also, the statistical analysis methods (for example, "factor analysis", or "analysis of variance") are not theoretical analyses. They are merely techniques for arriving at a type of fact. It is still up to the analyst to discover and analyze the theoretical relevances of these facts. In sum, the basic criterion for generating theory is theoretical relevance, and the analyst should sample his quantitative findings on this basis.

In place of making tests of significance, the sociologist can establish working rules to fit his particular situation. For example, two rules for establishing an acceptable percentage-difference level are not to consider any relationship of, say, less than 10 per cent difference; or any relationship in which three people's changing their minds or being misclassified would change the percentage to below an established level. These levels change with the number of cases used, smaller numbers of cases requiring a higher percentage-difference level. Selvin has also developed an internal replication procedure for establishing the possibility that a relationship exists.[xviii]

Standing by the rules that he may have initially established for his research is pertinent only to the beginning phases of generating theory. When the analyst has achieved theoretical relevance with his data, consistency arises in percentage-difference levels as well as in content, and he will readily learn to understand when and why a lower difference is relevant as well as a higher one. The absence of a relationship becomes just as important as an increase above the consistent percentage level, for any degree of association (or lack of it) may be part of the theory. For example, in Table III the relationship of motivation to previous experience varies at consistent percentage-difference levels - positive (20 per cent) to zero (2 per cent) to negative (-12 per cent) - thus theoretically indicating that these levels are engendered by experiences emphasizing basic research, unaffected by those experiences emphasizing applied research, and inhibited by experiences involving routine service in applied research. In Table V, a consistent percentage-difference level of 10 to 16 per cent shows in comparative relief the theoretical relevance of the stronger and weaker relationship as conditions varying the effect of recognition on satisfaction with organizational personnel.[xix]

Table V Effect of Recognition on Scientists' Satisfaction with Diverse Organizational Personnel			
	Organizational Position of Scientists		
	Junior	*Senior*	*Supervisor*
Assisting Personnel Very and fairly satisfied	+10%	+5%	+11%
Scientific personnel Very satisfied	+16%	+5%	+22%
Leadership Very satisfied	+28% -11	+26% +11	+12% +16
Institute director Very competent Fairly competent	+10% +7	+7% +2	+28% _____

Liberties in Presentation of Data

When quantitative data are reported in verificational and descriptive studies, typically each association is given in table form with a technically exact discussion of it, and then the finding is qualified by tentative statements and alternative explanations or interpretations. This style of presentation need not be used in generating theory, nor, in fact, could it be used. The multitude of relationships on which grounded theory is based is so large that this style applied to each relationship would make the report of the theory unreadable - too long, cumbersome, and slow-moving - to colleagues and quite inaccessible to laymen. It is particularly important that both colleagues and laymen readily understand the theory, [xx] since quantitative data are usually not as interesting to read as qualitative, and do not carry the reader along as easily. Therefore, the analyst must take some liberties both in presenting tables and in making statements about them. Needless to say, the liberties in presentation should not in any way change the data upon which the theory is based; it is just that for generating theory not all data must be presented and stated in exact detail. Since the possibilities are great, each analyst must decide on various liberties according to his particular directions of effort.

Let us consider here a few general liberties of presentation. Unlike Tables I through IV, Table V is a table of percentage-difference. The proportions that were compared to arrive at the differences are left out, since they were not necessary for the theoretical analysis. If it is necessary to know about a particular set of proportions, they should be mentioned in text. However, the focus of analysis in this table was on comparing percentage-differences for indicating direction and magnitude of many relationships: that is, differences in satisfaction with organizational personnel accounted for by the high and low recognition achieved by scientists at different stages of their organizational careers. Both the direction and magnitude of these relationships were important for the analysis; if only direction of relationship had been important, the table could have been further simplified by leaving out numbers and using only plus and minus signs. These flexible renditions of quantitative evidence are in the service of generating theory. No information is lost, distorted, or purposively concealed. It is just that only enough information is presented to show, in the simplest possible manner, the grounded basis of the emerging theory. Verification requires a more detailed rendition of the data - showing all N's, sub-N's and compared high and low percentages - so that the reader can verify the verification for himself.

Because of the overabundance of separate associations necessary in generating theory (literally hundreds, in contrast to the few necessary in verificational studies), another general liberty may be taken in presenting tables, particularly two variable tables. Unless a whole configuration of consistency indices are shown together in a table for visual comparisons, it is enough to state in the written text two variable associations in their direction and (if necessary) magnitude; presenting a table would be repetitious. When theoretically necessary, proportions and N's can be provided in a footnote.

While verificational studies require exactitude, statements about associations can be more flexibly written when theory is the goal. For example, "more successful investigators have satisfactory research facilities provided to them as a reward by the organization" is a statement that assumes the reader understands that three liberties have been taken with this

reporting of a two-variable table. First, the "successful" investigators have been *compared* with less successful investigators - the statement is comparative. Second, "more" means proportionately more - the comparison is relative, not absolute. And third, that the organization provides these research facilities as rewards to the successful investigators is a theoretical inference from the finding that they simply have more satisfactory research facilities than the less successful investigators. Such a hypothesis is more readable than the precise, literal statement: "A higher proportion of those scientists with high professional recognition than those scientists with low professional recognition have satisfactory research facilities. We tentatively suggest that these facilities are provided as rewards to the more successful scientists by the organization."

These three liberties in writing can also sometimes be taken when rendering three-variable tables, and the table need not be put in text. But more often, as noted in the next section, three-variable tables have complex purposes - for example, an interaction table showing the joint effects of two variables on a third (example 4 below). A table and some explicit reporting of it are required for the theoretical inference to be easily understood as being based on evidence.

Theoretical Elaboration

The previous section presented the first step in our style of theoretical analysis of quantitative data: saturating core indices with all possible two-variable runs; discovering relationships among the runs with theoretically relevant consistency indices, summation indices and single questionnaire items; then analyzing the findings with theoretical inferences. The next step, which cannot be neglected, is *elaboration analysis* - to make three or more variable analyses in order to saturate categories further by developing their properties and thereby achieving a denser theory. Thus, the discovery of relationships among indices provides the analyst with beginning suggestions for a theory, plus a theoretical direction and focus for its elaboration.

By "elaboration" we mean that the two-variable associations, which are the basis of theoretical hypotheses,

must have their structural conditions specified; their causes and consequences sought, with possible spurious factors checked for; and their intervening variables (delineating processes between the variables) discovered. Although this, of course, is Lazarsfeld's elaboration analysis,[xxi] we shall contribute something new to his method for our own purpose of generating theory. The next several paragraphs assume an understanding of elaboration analysis (which can easily be gained by a study of Hyman's rendition of it[xxii]). The notions on consistency analysis discussed in the previous section are subsumed in elaboration analysis.

Lazarsfeld has provided three ways of ordering the variables in an elaboration analysis: (1) temporal, (2) structural level of complexity, and (3) conceptual generality. Temporal ordering is simply the time sequence of the variables involved. Structural level of complexity is an ordering in terms of the encompassing structural levels that characterize the unit of analysis under study. For example, a nurse can be characterized by the ward she works on, the hospital she works in, the city in which the hospital is located, and the nation where the city is. Conceptual generality is an ordering by degree of abstractness of the variables. For example, a nurse says all patients should be bathed every day, which is specific opinion derived from a broader attitude of obeying all hospital rules, which attitude in turn derives from a basic value in medicine that nurses should obey hospital rules.

Lazarsfeld's elaboration analysis is seldom used in research except for the prime task of specifying the conditions of a finding; for this task, one need not understand or expressly use his formula. The reason for this lack of use is simple: the only type of ordering of variables that Lazarsfeld has actually worked out is temporal ordering - the other two types have only been suggested.[xxiii] Since survey data is typically cross-sectional in time, analysts are hard put to establish clear-cut, factual time orders in which colleagues will have confidence, because of the emphasis on accurate facts in verification and description.[xxiv] Usually there is too much temporal interrelation among cross-sectional survey variables - over time, either one could, and probably does, result in the other. Thus, elaboration analysis is often stopped in its tracks before it has a chance to

prove its usefulness. And the analyst who does not give it a chance stifles, rather than stimulates, his theoretical imagination. He has been taught not to let his imagination range on data that he cannot himself believe completely accurate, much less argue for their credibility with his colleagues. He has been taught to be sceptical of such strategies for survey data to the point of keeping an empty head about data felt unreliable.

Elaboration analysis is stimulating because the findings it produces fit the thought patterns of sociological theory. With it, the analyst can show interpretations, processes, conditions, causes, spurious factors, and consequences with actual data - not an interpretation of the data. The analyst can literally speak through elaboration tables. He need only infer from his indices the conceptual level of his talk since the tables provide the theoretical arrangement of the variables. But if temporal ordering is believed impossible in most cases, how can we allow theory to emerge from elaboration tables?

Theoretical Ordering

The theory can emerge from these tables if, at first, the analyst decides that his purpose is to generate theory, for then the accuracy of temporal ordering that would be required for verification and description is no longer crucial. He must then proceed to order his variables *theoretically:* a new principle of ordering. Lazarsfeld comes close to suggesting this principle with his "substantive" orderings by structural complexity and conceptual generality, for these are two specific examples of the general principle of theoretical ordering. But Lazarsfeld misses developing a general theoretical ordering principle because he does not consider their underlying similarity, nor how and why they can be used for the generation of theory. He missed this consideration because he is involved exclusively in establishing facts for description and verification. He never comes close to understanding that temporal sequence can be handled theoretically as well as factually.

Theoretical ordering of variables occurs by two strategies: (1) running all possible three-variable associations with each theoretically relevant two-variable association; and (2) running particular tables to fill in gaps or to answer questions, which

emerge as the theory develops, by arranging elaboration tables according to the dictates of the theory. From the findings in both strategies there emerge theoretical orderings of variables already integrated with core categories and hypotheses. The analyst then infers or suggests them as his theory.

Theoretical ordering of variables by all possible three-variable associations on core two-variable relationships is done by comparing the partial association percentage differences to the percentage difference of the original relationship. When the partials vary above and below the original relationship, then the analyst discovers conditions that minimize and maximize his core relationship. From these findings he generates theory stating "under what conditions" a phenomenon exists. Some of these conditions are antecedent to the original association and may be suggested as partial causes; others, which occur at the same time, may be called contingencies. When the partials are equal to the original relationship, then a particular condition does not vary the relationship. The analyst either regards it as theoretically relevant or ignores the finding.

When both partials are less than the original relationship (they never completely disappear), then the analyst must theoretically suggest whether the third variable is (1) an intervening variable, or (2) an antecedent variable. An antecedent variable that reduces partials may have several theoretical meanings. The original relationship may be spurious; that is, both original variables are the consequences of the third variable. This finding may be theoretically very relevant. For instance, "the more fire engines that come to a fire, the greater the damage" is a spurious relationship, with both factors accounted for by size of the fire. The antecedent variable may also suggest a process in which the third variable leads to one of the original variables, which in turn leads to the other. This inference can be tested with the second strategy of theoretical ordering, which is to answer to the question "Is this a process?" by rearranging the table to fit, thus testing for the theoretically assumed ordering of an intervening variable. If the inference proves correct, the analyst has found a value-added process - without the first variable the other two variables do not occur in process.[xxv] Thus the analyst can actively check on his theory as it emerges by testing assumed

theoretical orders. (This will be illustrated shortly.) Third, the antecedent variables always occur together and therefore are truly, not spuriously, associated, but they do not occur without discovered cause, which the analyst might wish to call a necessary condition. Thus fire engines and fires are truly associated, but are not found together unless someone has put in the alarm.

The first strategy of theoretical ordering is based on emergence: the data provides possible orders for the analyst. He need only induce theory about what he has found. This can be difficult when he has to overcome current training in quantitative analysis. He must remember that he is only looking for plausible orders among variables to suggest a theory. He is not looking for the "facts" of a description or verification. *He must think developmentally by remembering that only the data is static or cross-sectional - not his mind!* Although the data may admit of no temporal sequence, his creative imagination can consider any ordering principle for the related variables, and this principle becomes his ingenious suggestion. With imagination and ingenuity he can theoretically order his variables by time, structural complexity, conceptual generality, or in any other theoretical manner. His job is to suggest a theory based both on the *theoretically relevant order* or elaboration relationships and on the *content* of the variables he employs. He cannot think methodologically or statistically with symbols such as t factors or x leads to y; he must think theoretically about the content of his indicated categories and infer why the order of their possible relationships may be as he found them. In short, he must free himself from the exact rules of elaboration ordering as applied to descriptive and verificational studies, so he can be flexible in an imaginative, post hoc theoretical analysis of what he has discovered from the four elaboration possibilities: antecedent or current conditions (PA and PI), antecedent or intervening variables (MA and MI).

In generating theory as it emerges, the analyst first discovers two-variable relationships; second, he discovers their elaboration. Then he moves into a third stage, in which he starts generating possible further elaboration of two-variable relationships within the previous elaboration, using the second

strategy of arranging variables to test theoretical orderings. He looks through his data to find indicators for the concepts he thinks are related in theoretical ways to his emerging theory. Then he arranges his elaboration tables to test if they bear out his hypotheses (for suggestion, not verification), or to discover what actually happens. At this stage of the analysis, he is theoretically sampling his data as directed by his emerging theory and he is actively directing his further runs accordingly; much as the field researcher directs his final work towards theoretically sampling data on hypotheses for filling gaps and answering the remaining questions in order to saturate categories. And much as the field worker at this stage moves quickly between situations, achieving greater relevance with smaller amounts of data, the quantitative analyst may literally camp in the IMB machine room, having successive tables run to continually check his hypotheses as he thinks them through and theoretically samples his data for them[xxvi]. He knows what his data should look like in various runs, and the runs set him straight. By this time the analyst has looked at hundreds of directions provided by the first two stages of his research. Consistency and elaboration analyses join together to provide him a grounded basis for his theory. (The appendix to this chapter gives examples.)

Conclusions

The point of this chapter has been to illustrate the careful relaxation of rules surrounding quantitative analysis, a relaxation for generating theory. The styles of quantitative analysis are multitudinous, so our discussions here include but few illustrations pertaining to the rich veins in quantitative data that can be mined when analysts relax their rigor.

One topic that we have not yet dealt with in this chapter bears mention: comparative analysis within and between surveys. To be sure, the discovery of relationships and their elaboration are all based on comparative analysis of subgroups that are readily found in the same body of data. However, sociologists have yet to explore the many possibilities for generating theory by the active creation of diverse comparison subgroups within a survey (besides core index and typologies), and by the active search for comparison subgroups on other surveys. The various survey-data libraries scattered around the

nation now facilitate comparisons between surveys.

We can suggest a few general rules for beginning this kind of exploration. The analyst can use *similar* groups for comparison between surveys; they do not have to be identical. For example, "working class: may be indicated by residential area in one study, income in another (remember that crude indications are sufficient and interchangeable).[xxvii] Further, the analyst should search for ways of comparing quickly and easily the multiple comparison groups within many different, particularly large, surveys, since one or two surveys can easily run thin on data, and what is needed for a dense, adequate theory is a great amount of data. Also, multiple comparisons should be sought and flexibly done with qualitative data on other relevant groups.

In making these multiple comparisons, the analyst should constantly focus on generating and generalizing a theory not on the comparison of differences to verify or account for a fact. Generating from differences is not easy to manage with quantitative data, since sociologists are trained to verify, and verification from differences comes very easily with quantitative data. Verifying and accounting for facts by differences are subsumed in the process of generating theory; they are not the product of quantitative research for this purpose.

Appendix to Chapter VIII: Examples of Theoretical Elaboration

Following are several examples of theoretical ordering of elaboration tables, which tell the analyst if it is possible to suggest a theoretical statement. We focus primarily on the second strategy of theoretically arranging tables to discover possible orderings for hypotheses.

1. *The discovery and generation of a performance-reward process*. In a study of organizational scientists, the analyst discovered that scientists' motivation to advance knowledge was positively associated with professional recognition for doing so. This finding suggested the theoretical inference that recognition from others maintains motivation.[xxviii] The analyst

then elaborated this relationship by suggesting the following theoretical ordering: if recognition (which indicates previous performance) maintains motivation, then motivation should result in high quality performance in research and this, in turn, should result in more professional recognition. This ordering could then be suggested as a circular, snowballing, reward process for performance within science. The problem then became to order the elaboration tables to test if theoretically (not factually) this process was grounded.

In Table VI, the magnitude of association between recognition and performance is diminished when the intervening effect of motivation is removed. Therefore, high motivation tends to be a link between receiving recognition and accomplishing further high quality research performance, tentatively demonstrating the performance-reward process as a grounded basis for a theory of this process. As a social pattern, this circular process will continue if the performance measured here results in new recognition. [xxix]

TABLE VI

	Recognition		
	Average	*Less*	*Difference*
High performance	56% (144)	44% (188)	+12%
Proportion with high performance and:			
High Motivation	60% (96)	53% (90)	+7%
Low Motivation	46% (48)	37% (98)	+9%

At this point the analyst suggested that, besides research performance, it was also possible to predict behaviour associated with research on the basis of intensity of motivation. This assertion was borne out by one indicator of research behaviour: the amount of time in a typical work week that the scientist puts into his own research activities. Fifteen per cent more of the highly motivated investigators worked 21 hours a week or longer on personal research. Furthermore, 11 per cent more of those who worked 21 or more hours a week on their own research had a high quality performance score. (Note the

discovery of two additional associations.)

TABLE VII			
	Motivation		
	High	*Low*	*Difference*
High Performance	57% (186)	38% (146)	+19%
Proportion with high performance who put:			
21 or more hours per week into their own research	60% (142)	43% (89)	+17%
Less than 21 hours per week into own research	48% (44)	35% (57)	+13%

Next, in theoretically ordering motivation, personal research time, and performance (Table VII), it can be suggested that the highly motivated investigators will tend to put more time into their own research work, and that this time in turn will tend to result in higher quality performance. The magnitude of association between motivation and performance is diminished when the intervening effect of personal research time is removed. This finding then adds a subsidiary link to the circular performance-reward process (diagrammed below).

Performance-Reward-Process in Science

→Recognition→Motivation→Time in Own Research→ ↓

↑Performance←←←←←←←←←←←←←←←←←←←←←←←←

This theory is based on one possible one-time sequence. The reverse time is also possible: some investigators may have developed a high degree of motivation because they put in more than 21 hours per week. Hard work could generate interest. Therefore, we may have another time sequence in the performance process - longer hours in research leading to a high motivation, resulting in high performance. However, this cannot be suggested because the data leave it ungrounded. In comparing proportions downward in Table VII, among those with high motivation 12 per cent more of those who worked 21 or more hours a week on their own research had a high performance score. Among those with low motivation, 8 per cent more who worked 21 hours or more a week on personal research had a high performance score. The original relation

between time and performance, is a condition that creates a slightly stronger relation between the two. This is, of course, the time sequence originally assumed, which shows it to be the only theoretically grounded sequence.

This example indicates the discovery of two-variable relationships and their theoretical elaboration in order to generate a processual theory. The theory is suggested, not tested because obviously the temporal ordering is theoretical, not factual; the data were collected on one day, except for the performance index, for which data were collected three months after the survey. However, *even theoretical ordering provides checks on itself*; even when the two elaboration tables were rearranged, the order of the process did not change.

2. *Structural complexity process.* In the same study of organizational scientists, the following consequences of two different promotion systems in the organization were discovered. [xxx] The "recommended" system (in which initial consideration for a scientist's promotion was based on a supervisor's recommendation) resulted in more discrepancies between rank and actual responsibilities and in more unsatisfactory evaluations of the system than did the "routine" system (in which initial consideration for promotion was based on periodic reviews). Theoretically, it seemed that a process was involved, whereby the relative frequency of perceived discrepancies resulting from each promotion system was a reason for the relative number of unsatisfactory evaluations of each system. The analyst then arranged an elaboration table to test for this theoretical order (Table VIII), and the findings supported it - the partial associations (22 and 25 per cent) were less than the original associations (29 per cent), showing that discrepancies were an intervening variable between systems and evaluations. This theoretical process was supported by another consistency finding that among scientists in the "recommended" system there was considerably less satisfaction (29 per cent) with chances for a promotion.

TABLE VIII			
	Promotion System		
	Recommend	*Routine*	*Difference*
Evaluate promotion process as unsatisfactory	58% (184)	29% (145)	+29%
Proportion who evaluate promotion process as unsatisfactory and who observed discrepancies:			
Frequently	83% (59)	61% (28)	+22%
Occasionally	45% (125)	21% (117)	+24%

Here the theoretical ordering of variables is based on structural contexts at different levels, and assumes that the more encompassing level has a greater effect on the lesser level rather than vice versa. Thus "promotion systems" is a contextual unit that causes discrepancies in rank and responsibilities among personnel; while "discrepancies" is a property of the system that provides a structural condition affecting the way scientists evaluate their systems' promotion procedures. Thus, mixed into this structural level process are contextual properties of individuals or structural conditions under which they have a career (promotion procedures and characteristic discrepancies in rank and responsibilities) and for a system (evaluations); properties of a system (procedures, discrepancies, and dissatisfied individuals); properties of individuals (evaluations), and so forth - depending on how the analyst wishes to render and focus his theory. In short, even within this simple structural process, as found in one elaboration table, the analyst can find much grist for sociological theory.

3. *Theoretically rearranging on a table to test for alternative career processes.* The question arose about how those scientists who planned to move to relieve the pressure of a currently unsuccessful career have made this decision.[xxxi] They may (1) decide to leave the organization, and then choose the goal they plan to work for - perhaps still basic research (by going to a university) or perhaps a change of practice or applied research (by going to either a private, industrial or governmental research organization); or (2) decide to change

the goal of their work from basic research to another goal, such as applied research or "practice," and this change would necessitate leaving their organization as soon as possible.

Table IX is arranged to test for the sequence of factors in the first process: "plans to move" is tested as an intervening variable, coming between degree of recognition and preference for a preferred affiliation in a university, if the move is made. Since the original relation is nil, we discover that this theoretical elaboration test for an intervening variable is a test if the non-existent original relationship was actually a cancelling-out of a strong positive relationship (between recognition and preference under the condition of planning to move soon) and a strong negative relationship (between recognition and preference when planning to stay on in the organization). Thus this table corrects our theoretical ordering by yielding a finding that suggests that unsuccessful scientists who plan to move (11 per cent in Table X) have not yet planned to go on with either basic research or applied research or practice. They are still just planning to move because of a poor career, and they have not decided where or for what purpose.

TABLE IX

	Recognition		
	High	*Low*	*Difference*
Prefer to move to university	62% (144)	63% (188)	-1%
Proportion who prefer and who plan to:			
Move soon	66% (12)	69% (36)	-3%
Stay for time being or permanently	58% (130)	57% (152)	+1%

Table X is arranged to test the second-mentioned process in making plans to move. Preference for the university or for other organizations is tested as intervening in the decision to move as soon as possible made by those who lack recognition. Again, planning to move because of low recognition is *not* a result of planning to change work goals- both partials are not less than the original relationship of 11 per cent. What this table tells us is that the scientists' plans to move as soon as possible materialize (15 per cent) under this condition of a

certain preference for moving to a university where their research goals would be the same. On the other hand, plans to move soon hardly materialize (7 per cent), if at all when the scientists prefer an organization offering them another work goal.

TABLE X			
	Recognition		
	High	*Low*	*Difference*
Plan to move as soon as possible	8% (144)	19% (188)	-11%
Proportion who plan to move as soon as possible and who prefer to move to:			
University	10% (84)	25% (111)	-15%
Other organization	7% (60)	14% (77)	-7%

Thus, theoretical arrangements of elaboration tables, while not necessarily bearing out our theoretical guesses, discover for us *what is going on* (in, say, the decision to leave an organization because of a failing career). They fill gaps in the total theory of organizational careers and answer our specific questions.

4. *Specifying joint effects of conditions.* Seldom are both partial associations less than the original association; the most frequent finding specifies antecedent or contingent conditions that minimize and maximize relationships. These findings yield perhaps the most frequent of theoretical statements: the varying conditions under which a phenomenon exists. As we have said, the specification of conditions may apply to a single index, but as an elaboration procedure it applies to two or more variable relationships. Antecedent conditions (such as previous research experience, Table III) may, if the theory warrants, be suggested as partial causes. Conditions occurring at roughly the same time are called contingencies, denoting whether a relationship is contingent on a condition that makes it more or less pronounced. Further, for his theory, that sociologist may choose to reverse the temporal order of his specifications of conditions to obtain statements on the varying consequences of diverse aspects of a condition (types, dimensions, or degrees of

the condition). Thus, this type of elaboration table yields findings that suggest several ways to generate a theory.

TABLE XI Percentage Who Are Very Satisfied With Job Security

	Felt Recognition		
	High	*Low*	*Difference*
Organizational Position:			
Junior Investigator	67% (57)	43% (84)	+24%
Senior Investigator	70% (40)	58% (60)	+12%
Supervisor	73% (47)	73% (44)	___

Joint effects is another theoretically interesting way of talking about the specification on conditions. In Table XI we see the joint effects of scientists' organizational position and degree of professional recognition on their satisfaction with the security of their job in the organization. A standard means for rendering this table is to say that when we hold organizational position constant, professional recognition only makes for job security in the investigator position. But "holding constant" is a notion used in verification of theory, when the analyst is trying to reduce the contaminating effects of any strategic variable not in focus with his variable of interest.

To view the table in terms of joint effects of two conditions on a third lends itself better to generating theory, since no variable is assumed a constant; all are actively analyzed as part of what is going on. For example, in Table XI we see that as a scientist's organizational position advances (or for the theory, as his career advances), professional recognition becomes less important for job security (the percentage differences decrease). Another joint effect for theoretical inference is that, as the scientist's career advances, he becomes more secure in the organization through seniority, and less dependant upon his degree of professional recognition for this security (under "low recognition" security percentages increase with position). Or the analyst might say that a scientist with professional recognition to his credit tends to have a secure job no matter what his organizational position. (See percentages under "high recognition.") Thus, statements of joint effects tell

us how conditions interact together to affect a third variable - and this is theoretically rich and relevant information.

TABLE XII Proportion of Junior Investigators Who Are Very Satisfied With Security Of Job

	Recognition		
	High	*Low*	*Difference*
Promotion System			
Recommend	63% (30)	37% (51)	+26%
Routine	69% (26)	50% (32)	+19%

Two other ways of making inferences about this table are in terms of "differential impact" and "differential sensitivity."[xxxii] For Table XI the analyst can say that position has a *differential impact* on the relationship between recognition and security. In Table XII, we see the differential impact of promotion systems on junior scientists' satisfaction with job security under different conditions of professional recognition. These, again, are forms of contextual and conditional comparative analyses. Referring again to Table XI, the analyst can say that the security of the scientists with low recognition is very sensitive to organizational position, while the security of scientists with high recognition is insensitive to organizational position - thus indicating the *differential sensitivity* of the successful and unsuccessful in their job security.

Finally, the analyst can generate minimal and maximal *configurating* conditions (a useful theoretical model) for his theory from a joint-effects table like Table XI. To be at the beginning stages of a career without recognition is to feel comparatively little satisfaction with job security. Maximum security comes at the peak of one's career in the organization, because of tenure. Though it took professional recognition to achieve this position, recognition is no longer a condition for job security.

We could suggest more ways to generate theoretical statements from joint-effects tables, as well as from the first three illustrations of elaboration tables. However, we wish only to conclude from these brief illustrations that *if quantitative*

data is handled systematically by theoretical ordering of variables in elaboration tables, the analyst will indeed find rich terrain for discovering and generating theory. We hope by our slight but purposeful loosening of the rules, via our principle of theoretical ordering, that elaboration analysis will be used more than heretofore. Its richness for research has not yet been tapped because of difficulties in using it on cross-sectional survey data to produce accurate facts for description and verification.

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[i] For examples see James Coleman, "Research Chronicle: The Adolescent Society," and Seymour Martin Lipset, "The Biography of a Research Project: Union Democracy," in Philip Hammond (Ed.), *Sociologists at Work* (New York Basic Books, 1964).

[ii] This way of presenting one's work in a publication on research is not chicanery, but an established form in many circles of science. See Bernard Barber and Renee C. Fox, "The Case of the Floppy-eared Rabbits: An Instance of Serendipity Gained and Serendipity Lost," *American Journal of Sociology*, 64 (1958), pp. 128-29.

[iii] Paul F. Lazarsfeld, "Interpretation of Statistical Relations as a Research Operation," in Lazarsfeld and Rosenberg (Eds.), *The Language of Social Research* (Glencoe, Ill.: Free Press, 1955).

[iv] For example compare the theoretical analysis in Barney G. Glaser, *Organizational Scientists: Their Professional Careers* (Indianapolis: Bobbs-Merrill, 1964) to the description from the same study using over 100 different IBM card decks and comprising four volumes. *Human Relations in a Research Organization*, Volumes I and II (1953) and *Interpersonal Factors in Research*, Parts I and II (1957) (Ann Arbor, Mich.: Institute for Social Research). For another example see Hanan C. Selvin, *The Effects of Leadership* (New York: Free Press Glencoe, 1960).

[v] See Barney G. Glaser, "The Use of Secondary Analysis by the Independent Researcher," *The American Behavioral Scientist* (1963), pp. 11-14.

[vi] Paul F. Lazarsfeld, "Problems in Methodology," in R. Merton, L. Broom and L. Cottrell (Eds.), Sociology Today (New York: Basic Books, 1959), pp.47-67; "Evidence and Inference in Social Research," Daedalus, LXXXXVII (1958), pp.100-109; and with Wagner Thielens, The Academic Mind (Glencoe, Ill.: Free Press, 1958), pp. 402-407.

[vii] It is at this point, Lazarsfeld suggests, that technicians, who perhaps have no generative powers, take flight into precision by blaming their crude methods and trying to refine their indices instead of thinking about what they have found.

[viii] In constructing a summation index, the analyst first obtains one more group than the number of indicators he is using: four indicators lead to five groups. Before combining these groups he should cross-tabulate the five groups

with a criterion variable- he knows the relationship exists- to find out between which groups the direction of the relationship changes. He then combines all those groups positively related to the criterion variable and all those negatively. He cannot just dichotomize the index where he pleases, because he may reduce its discriminating power by combining positive and negative degrees.

[ix] For an example see Lazarsfeld and Theilens, *op. cit.,* pp. 89-90.

[x] This is a specific case of Zetterberg's rule that the total integration of a theory tends to make any one of its parts "highly plausible." See Hans L. Zetterberg, On Theory and Verification in Sociology (Totowa, N.J.: Bedminster Press, 1963), Chapter 6.

[xi] Glaser, *op. cit.*

[xii] If the analyst has enough time and money, he can run the index open (use all groups) and then dichotomize them at the breaking point for each item. This will yield more diverse information on each relationship and make the index more sensitive. This strategy is an alternative to dichotomizing on a criterion variable, but is cumbersome; and once the analyst is sure his break in the index is the most sensitive one, it may seem a waste of time for the yield of information.

[xiii] For the theoretical discussion of Tables I and II, see Glaser, *Organizational Scientists: Their Professional Careers, op. cit.,* Chapter 2.

[xiv] For the theoretical discussion of Tables III and IV, see Barney G. Glaser, "Differential Association and the Institutional Motivation of Scientists," *Administrative Science Quarterly*, 10 (1965), pp. 81-97.

[xv] Hanan Selvin, "A Critique of Tests of Significance in Survey Research," *American Sociological Review*, 22 (1957), pp. 519-27; "Statistical Significance and Sociological Theory" (July, 1960) (mimeographed, University of California, Berkley)

[xvi] Robert McGinnis, "Randomization and Inference in Sociological Research," *American Sociological Review*, 23 (1958), pp. 408-14; Leslie Kish, "Some Statistical Problems in Research Design," *American Sociological Review*, 24 (1959), pp. 328-38; and critical comments by David Gold and James Beshers in *American Sociological Review*, 23 (1958), pp. 85 and 199

[xvii] That consistency validates is a basic pattern of plausible inference. See G, Polya, *Patterns of Plausible Inference* (Princeton, N.J.: Princeton University Press, 1954), Vol. II, Chapter XII

[xviii] Selvin, *op. cit.*

[xix] For theoretical discussion of Table V see Glaser, *Organizational Scientists: Their Professional Careers, op. cit.,* Chapter 6

[xx] In contrast, verifications usually only require the understanding of a small group of colleagues working in the area.

[xxi] See references in footnotes 3 and 6

[xxii] Herbert H. Hyman, *Survey Designs and Analysis* (Glencoe, Ill.: Free Press, 1955), Chapter VII

[xxiii] Herbert H. Hyman, Survey Design and Analysis (Glencoe, Ill.: Free Press, 1955), Chapter VII

[xxiv] The evaluator of an article for one journal remarked on an elaboration tale, "More generally the whole argument about establishment vs. persistence (or stability) of the relationships suffers because the author really has no other time trend data- that is necessarily implied in statements about persistence or stability." The paper was rejected because temporal order was not an incontrovertible fact.

[xxv] See for a discussion of this type of process Neil Smelser, *Theory of Collective Behavior* (New York: Free Press of Glencoe, 1963), Chapter I.

[xxvi] This is a frequent activity among some survey analysts; see Coleman's discussion of continually having tables run as he thinks them through, *op. cit.,* pp. 203-204

[xxvii] See Herbert H. Hyman, *Political Socialization* (Glencoe, Ill.: Free Press, 1959), for examples of combining similar categories for comparative analysis

[xxviii] Glaser, *Organizational Scientists: Their Professional Careers, op. cit.,* Chapter III

[xxix] See *ibid*, p. 32

[xxx] *Ibid.,* Chapter III

[xxxi] *Ibid.,* Chapter VIII

[xxxii] *Ibid.,* Chapter IV

A Simpler Understanding of Classic GT: How it is a fundamentally different methodology

Ólavur Christiansen

Abstract

The author reduces the research rationale of classic grounded theory (GT) methodology and the consequential classic GT research procedures and stages down to their essential elements. This reduction makes it possible to compare classic GT to other research methodologies in a manner that is simpler and yet concise. This methodological analysis and synthesis has been conducted while applying and after having applied the classic GT methodology in practice in a major project. The fundamental differences between classic GT versus other adaptations of GT, as well as other qualitative-inductive research approaches, are mainly explained by the very different approaches in solving the problem of many equally justifiable interpretations of the same data, and by the consequential differences in research procedures, and how they are applied.

Comprehension of methodological differences in details will always be relevant. However, an uncomplicated and still concise explanation of the differences between these methodologies is necessary. "Grounded theory" (GT) is used as a common label in the literature for very different research approaches. This simpler approach of comparing the methodologies will be helpful for researchers, who might want to consider several options when deciding which research methodology to use, and who need quickly to understand some of the most essential methodological elements.

Introduction

For prospective researchers, who wish to consider several options when deciding which research methodology to use, it can be bewildering when "grounded theory" is used as a common label in the literature for very different research methodologies. During the research process that led to the theory of "opportunizing" in business (Christiansen, 2005; 2006) the author made some observations and lived through

some experiences that could be helpful to others who might want to utilize Glaser's prescribed set of classic grounded theory (GT) research procedures, or other adapted GT procedures, or other mainly inductive-qualitative research procedures in e.g. economics, business and management research. This article is based on a systematic treatment of these observations and experiences.

Glaser's prescribed set of GT research procedures are definite with regard to their usage and research rationale (Glaser and Strauss, 1967; Glaser, 1978; 1992; 1998; 2001; 2003; 2005). In this article, these procedures will be referred to as classic grounded theory methodology or classic GT. Strauss and Corbin (1990; 1998) have prescribed a set of research procedures that also are specific, and this set of procedures is also called "grounded theory". However, the research rationales that are attached to these two different sets of "grounded theory" procedures are clearly different, and consequently, and despite some apparent similarity, these two sets of research procedures are also very different. It is also obvious that there is a much wider diversity regarding applied research procedures in studies labelled as "grounded theory" studies in the literature. It has even been claimed that almost any qualitative research can be labelled as a "grounded theory" (Simmons, 1995).

Research methodologies almost by definition are different. They each have a different raison d'être, set of procedures and standards. Methodological diversity has its raison d'être and there is nothing wrong in it. To make judgments regarding general superiority or inferiority of methodologies may be pointless. However, to mix procedures of different researcher methodologies, which have different research rationales, may give a set of research procedures that do not represent a consistent method. A best choice of methodology depends on fit to the individual researcher's purpose or skills, or the contextual purpose, and any research outcome has to be judged according to the raison d'être, procedures and standards of the methodology applied.

The purpose of the article is to suggest a simplified and yet concise approach by which to compare research procedures that are labelled as GT, as well as other mainly inductive-

qualitative research methodologies. The basis for this comparing will be a reduction of the classic GT research rationale and the consequential classic GT research procedures and stages down to their essential elements. Thus, instead of only focusing on the many differences within the many details, focus can be delimited to the differences in the fundamental research rationales of the methodologies, and the consequential fundamental differences in the research procedures and stages of research.

This simplified basis for comparing will, of course, sum up and highlight the fundamentals of classic GT. It will not necessarily sum up and highlight all the essential features of the other methodologies. However, it will be enough to give an explanation for the methodological differences that are most fundamental, and which may be most the problematic for prospective researchers to understand.

The Classic GT Research Rationale

The rationale for using classic GT methodology, or its raison d'être, can be summed up and explained in different ways. One example is the following: "A methodology was needed that could get through and beyond conjecture and preconception to exactly the underlying processes of what is going on so that professionals and laymen alike could intervene with confidence to help resolve the participants' main concern surrounding learning, pain and profit." (Glaser, 1998, p. 5).

To "get through and beyond conjecture and preconception to exactly the underlying processes of what is going on in the resolving of the participant's main concern", the research area or the general research topic must, of course, be known. However, the researcher has to minimize his/her preconceptions and this requires that not even the research problem should be preconceived. It has to be allowed to emerge from the systematic collection and treatment of data during the research process. Due to its rationale, classic GT methodology is predominantly empirical and inductive – what counts is only what the data relate. The methodology is for the generation of a theory directly from data that explains as much as possible with as few concepts as possible, and what are explained are the behaviour patterns of those being studied. The research

outcome is a conceptual theory. Substantive concepts are stable latent patterns that image the area being researched. These concepts are generated from the systematic treatment of the data and should not be preconceived. These concepts should represent a considerable abstraction of time, place and people, and should have name labels that fit vis-à-vis what actually goes on in the resolving of the main concern, and be firmly grounded in the data by interchangeable data indicators. The purpose is certainly not conceptual descriptions with many concepts; such conceptual descriptions just convey stories that are bound to the specificity of time, place and people. The methodology can be used not only on qualitative data, but also quantitative data, but in practice it is mostly used on qualitative data.

Another way of expressing this rationale could, for example, be as follows: (1) to delimit the study to the main concern and its recurrent solution of those being studied (their substantive interests), and (2) to prevent preconceived professional concerns to mask what actually goes on in the field of study, and instead to stay open and let patterns emerge from the data. I will refer to these two points as the two hallmarks of the methodology. The following text will further explain the meaning of these two hallmarks and their significance in classic GT.

When researchers are confronted by an overwhelming set of collected data, some of them may find relief by concluding that the cultural, social or economic organization of life is complex enough to allow a number of equally justifiable interpretations. The rationale of classic GT is to meet this unique challenge by a unique solution. This is to find the core variable as the first stage of the research. This is the first hallmark of the methodology. As a concept, the main concern and its recurrent resolution of those being studied is summed up by the core variable of the emergent theory. After finding the core variable, the subsequent research and the generated theory is delimited to the core variable and to what is related to the core variable – the theory thus becomes a theory around the core variable. In other words, as the first stage of research, the main concern and its recurrent resolution of those being studied has to be conceptualized or summed up and explained

by one concept, which becomes the core variable. The core variable has to be allowed to emerge from the systematic treatment of the data during the research process, and should in no way be preconceived, and this is accomplished by adhering to the second hallmark. A fitting name has to be given to the concept that emerges as the core variable. By its naming, the core variable represents that particular behaviour pattern that is highly important for the participants, but also problematic. It is what drives and directs these people's behaviour. The core variable is that particular concept that is most related to the other concepts of the emerging theory. The core variable is also that concept of the theory that explains most of the variation in the data or in the studied behaviour. The problem of "numerous equally justifiable interpretations of the data" is minimized by finding the core variable. Consequently, the research is deliberately set out to follow the agenda of those being studied, the substantive interest relevancy of those being studied, and not any preconceived agenda of some professional research community or individual researchers, or their deemed professional interest relevancy. This is also the second hallmark of the methodology.

The second hallmark of classic GT has been referred to as "staying open and letting patterns emerge from data" and its opposite is "logically deducing, logically conjecturing, preconceiving (and possibly testing or quantitatively verifying some auxiliary hypotheses)". The orthodox GT analyst does not know a priori what he/she is looking for. Thus, much of the induction in orthodox GT is not tantamount to the ordinary induction, or the inductive principles used by different hermeneutic research procedures. Instead, classic GT induction is "assumption free" as well as "assumption based", but this latter only applies when these assumptions correspond to what already has emerged as more or less stable patterns in the data (Hartman, 2001, p. 37). This means that there is a "classic GT form of induction", which is different. Coupled with the first hallmark of classic GT, this helps keep the substantive interests of the participants in the field of study in focus, to avoid the compulsory, preconceived interests of the established research community, and to focus on what actually goes on in the field of study. In other words, the research is delimited to what is empirically discovered to be the most relevant and

problematic for the people being studied, not what a priori is deemed most relevant by the researcher (or by those being studied). For the researcher this means a minimizing of preconceptions and a suspension of prior knowledge and understanding regarding the area of research. Sometimes it may even be an advantage to be completely without any prior knowledge about the area of research prior to conducting the research. Such a statement, of course, flies in the face of positivist, rationalist and many other research positions. Yet, in the fairy tale, "The Emperor's New Clothes", it was only an innocent and ignorant little child that could do justice to reality by shouting out: "He is naked!".

Due to its rationale, the classic GT methodology has no attachment to any particular theoretical-disciplinal paradigm (Kuhn, 1996), theoretical perspective or theoretical-disciplinal research program (Lakatos, 1970). Ontological and epistemological positions may also contain pre-framings or preconceptions. Due to its rationale, the classic GT methodology is almost free of logically derived assumptions regarding ontology and epistemology. Its basic assumptions are limited to this: "Because man is a meaning-making creature, social life is patterned and empirically integrated. It is only a question of applying a rigorous and systematic method for discovering and explaining these patterns. Thus, just do it." (Glaser, 2004). The classic GT methodology is for the study of behaviour or behaviour patterns, not for the study of people or units as such. To generalize on units or people is difficult by any means. To generalize on behaviour is easier. Behaviour patterns transcend the borders of units.

Classic GT methodology can be conceived as a methodological paradigm or methodological research program, but it is not a usual one. The methodological procedures are the outcome of doing classic GT research on classic GT research since the early 1960s, i.e. the methodology is itself a classic grounded theory and thus essentially empirically generated. That the methodology is very different does not mean that it is better. For certain research tasks, and for certain very relevant and necessary research tasks, it would be a very wrong choice. For other research tasks, it could very well be an option. This is true especially when new perspectives may emerge regarding

what actually goes on in a field of study. However, even though new concepts and models may emerge, it does not necessarily mean that other concepts and models are wrong. The classic GT rationale is to increase and not to decrease methodological diversity and options, including ontological and epistemological options. When the classic GT rationales are stated as (1) "to keep the main concern and its recurrent solution of those being studied in focus" and (2) "to prevent preconceived professional concerns to mask what actually goes on in the field of study", this does not mean that use of other methodologies by default will lead to the opposite result. It may even be a strength if many different methodologies can be applied within a given research task. Methodological choice is not a question of enabling a researcher to reach the "absolute truth line", but to come closer to it. Social life has many facets, many realities may emerge in approaching "the truth line", and there cannot be any ultimate finality in any classic GT theory generation.

Of course, those being studied in a classic GT research know much more about what they do than any researcher. No classic GT researcher can or should compete with these people in their contextual knowing and describing. However, these people have not conceptualized nor conceptually explained what they do and how they accomplish it. The researcher, on the other hand, uses his/her license to conceptualize. Thus, the researcher can empower these people by providing them with an empirically grounded theory that conceptually explains what actually goes on and how they recurrently resolve their main concern. If some changes are needed, then these people would be empowered to accomplish them.

The Consequential Classic GT: The research procedures and distinct terminology

The research rationale of classic GT is made operative by the classic GT research procedures and by a distinct classic GT terminology. With reference to the research rationale, many of the procedures explain themselves. Firstly, it is difficult to "get through and beyond conjecture and preconception to exactly the underlying processes of what is going on in the resolving of the participant's main concern" without taking a predominantly empirical and inductive approach in the systematic collection and treatment of data. However, this inductive approach is not

the same as the "ordinary" inductive approach. This inductive approach is basically "assumption-free" and only "assumption-based" when these assumptions represent emerging stable patterns in the data. Anything else may be preconceptions, and preconceptions have to be minimized. Thus, a distinction may be made between (1) deductive logic based on a priori knowledge (which is minimized), (2) inductive logic where non-grounded assumptions also may direct the research process, (and which also is minimized), and (3) "the classic GT form of induction", where data takes the lead of the research process and where only grounded assumptions count. Suspension of prior knowledge and minimization of logical-deductive elements does not mean the elimination of them; neither does it give "objectivity". However, it makes a big difference. The data have also to be collected without any tainting of the researcher's possible preconceived notions, and this means that the researcher starts without any predetermined or preconceived research problem. Actually, one cannot know what one is studying before one has had a chance to look at the data - it has to "emerge" first. Literature reading has to wait to the end of the research. Only the data provides the control, and the task of the researcher is to be able to follow where the data lead him/her (Lowe, 2005).

Secondly, it is difficult to "get through and beyond conjecture and preconception to exactly the underlying processes of what is going on in the resolving of the participant's main concern" without the specific procedure of conceptualization by the method of constantly comparing. This procedure of conceptualizing thus becomes the main inductive procedure for the systematic treatment of data. The research rationale also requires delimiting, and the procedure of conceptualization is inherently delimiting, and the summit of this delimiting is achieved by finding the authentic core variable.

Possibly the most important and the most problematic issue for any researcher who uses the methodology is conceptualization or concept generation. To conceptualize means to discover and to name latent patterns and relationships between latent patterns as they emerge in the data and are verified by interchangeable data indicators.

Further, to conceptualize means: "to discover and generate new categories and their properties, instead of being forced to use received concepts." (Glaser, 1998, p. 133)

By coding (or conceptualizing or categorizing), the data are analyzed by being cut into slices that are constantly compared, and subsequently they may become synthesized and put together again differently according to the "pattern fit" and the various relationships. By coding, fitting names are given to each stable pattern, which convey explanations regarding the main concern and its recurrent resolution of those being studied. This takes place in a process of data collection and data coding that usually becomes iterative and involves much reworking.

There are two main types of building blocks of theory. These are substantive concepts or codes and theoretical concepts or codes. Substantive concepts are stable latent patterns that summarize the empirical substance of the data and signify the underlying meaning, uniformity and/or pattern. Theoretical codes, on the other hand, signify the relationships between substantive codes. For substantive concepts there is a hierarchy of levels. Any substantive concept has a level of abstractness vis-à-vis time, place and people. The more a particular underlying meaning, uniformity and/or pattern represents an abstract of time, place and people, the higher is the concept's conceptual level. The core variable is the substantive concept of the theory that has the highest conceptual level, and it is most closely related to all the other lesser-level concepts. Sub-core variables are below the core variable in conceptual level and very closely related to the core variable. Categories are below sub-core variables in conceptual level, but are related to some sub-core variables. A property is another type of concept; it is a conceptual characteristic of a category, sub-core variable or core variable, or a concept of a concept. Consequently, a property has a lesser conceptual level than the concept to which it refers. Data (qualitative or quantitative) are contextual descriptions that are bounded to the specificity of time, place and people and are at the lowest conceptual level. Theoretical codes are usually on a higher conceptual level than substantive concepts, as they signify more general phenomena (different kinds of causes, correlation,

processes with at least two stages that account for variation over time, loops, inseparable part-wholeness structures, etc.). (Glaser, 1978, pp. 93-115; 1992, pp. 38-39, 75-76).

A distinction is made between substantive coding and theoretical coding. There are two types of substantive coding. They are open coding and selective coding. Open coding is for finding the core variable. Selective coding is applied when the core variable has emerged and selective coding is delimited to concepts or data fragments that are related to the core variable. Theoretical coding is for recognizing or discovering the type of relationships between substantive concepts.

Classic GT is a form of latent pattern analysis of qualitative or quantitative data, but in other respects it is quite unlike, e.g. factor analysis. It originates from multivariate quantitative methodology (Glaser, 1998, p. 27). Yet, the methodology does not rely on any form of measuring or any counting. It does not rely on index construction of any kind, but on interchangeable indicators found in the data (Glaser, 1978, pp. 55-65). Glaser recommends that emergent categories (different latent patterns) should not be listed during the data work, and that data indicators should not be counted (Glaser, 1998, p.137).

The methodology is rarely used on only quantitative data, despite the fact that it is far easier. It has to be high calibre quantitative data, and such data on behaviour may be costly to obtain. When the methodology is used on qualitative data, the use of it has to be entirely technology-free (Glaser, 2003:17-44). Apart from mere writing purposes, the use of special computer software for coding or for sorting of categories or coded data is not recommended. Use of computer software may lead to a built-in pre-framing, incompatibility regarding forced choices, as well as incompatibility regarding flexibility, pacing and attention to what goes on in the data.

In the next section, more will be explained about classic GT procedures and terminology.

The Consequential Classic GT Stages of Research

Because focus is on behaviour patterns that transcend the limits of individual units, the data are collected by theoretical

sampling and not by statistical or representative sampling. In the beginning phase of theoretical sampling, the differences among the sampled units are maximized. Analysis and synthesis of the data then determines what unit to sample next. The data should be collected without any tainting of the researcher's possible preconceived notions from pre-existing theory, and the significance of the data should never be pre-judged, for example, by assuming that variables such as age, sex, income, size, type of business, etc. are important. When the interview is used in data collection, ungrounded or predetermined questions should be avoided. Instead, the interviewee should just be encouraged to talk freely about his/her main concern and its recurrent solving. This may be done in different ways, depending on what the interviewer finds appropriate in the given context. When the core variable has been revealed, more grounded questions may be asked. Audio or video is not recommended during interviews, and it may not be a good idea to take notes as well during interviews. This may inhibit the interviewee in giving genuine and original data. Instead, the data may be recorded afterwards, and the coding of it should begin immediately. (Glaser, 2001, pp. 165-184).

The procedural stages of the research are generally sequential, but once the research process begins, they are often conducted simultaneously or serendipitously according to the requirements of the particular research. Following the preparatory stage of not preconceiving the problem, and the data collection stage, an overview of the subsequent stages is as follows (Simmons, 2002):

As mentioned, there are two procedural stages of substantive coding, open coding and selective coding. Common to them is the procedure of constant comparative analysis. This means constantly comparing or relating data or data incidences (line by line) to emerging concepts (ideas), then relating concept (ideas) to other concepts (ideas) or their properties.

Open coding, which has the purpose of finding the core variable, allows coding of anything and everything in the data. The analyst asks three general questions of the data: "What is this data a study of". This ultimately leads to the discovery of the core variable that subsequently becomes the focus of the

research. The next question is: "What category or property of a category does this incident indicate?" (This encourages thinking conceptually and to avoiding contextualizing or "story-telling"). The third question is: "What is actually happening in the data?" (This alerts to possible theoretical codes).

The next procedure of selective coding is carried out when the core variable and its major dimensions and properties have been discovered. Selective coding means delimiting the coding to concepts or data fragments related to the core variable, but in other respects the procedures are the same while in the process of constantly comparing. Theoretical coding is to recognize or discover how the substantive concepts may relate to each other as hypotheses to be integrated into a theory. Theoretical coding is facilitated by the procedure of sorting (see below).

The procedure of memo-writing is a must in a classic grounded theory study. Memos are the "theorizing write-up" of ideas about substantive codes and their relationships. The writing of memos triggers insight and new ideas, and provides a record of grounding. While coding gives conceptual familiarity with the data, emergence happens while memo-writing. Data are always available, and can be analyzed at any time, while ideas are fragile. They should be written down at the earliest possible moment. Memos are always modifiable as more is discovered about the topic. Data collection, analysis (coding), sorting, and memo-writing are ongoing and overlap. (Glaser, 1978, pp. 82-92; Glaser, 1998, pp. 177-186). Conceptual familiarity with what conceptually occurs in the data has to reach a certain threshold before insight can strike gradually or suddenly or in abundance - or in other words: before emergence of concepts can occur. It requires theoretical sensitivity and creativity, but hardly more logic than what can be summoned by a small child in solving a jigsaw. Activation of more complex logic than that can easily trigger logical elaboration, and when an analyst relies on logical elaborations and deductions instead of what the data conceptually tell, he/she has actually abandoned the methodology. However, in theoretical sampling, a bit of *logic* is used in deciding where take the next sample. In theoretical coding, *prior knowledge* and *logical understanding* of as many theoretical codes as possible will be helpful. This

means that while classic GT is predominately inductive regarding the research area and the research problem, it is also a specified inductive-deductive mix.

The procedure of sorting refers not to data sorting, but to conceptual sorting of memos and accompanying data. By default, it also involves constant comparing. As explained, this has to be done manually, and a pair of scissors and a number of paper boxes may be useful. Sorting may become appropriate at any time during the course of the research. The final sort frames or constitutes the first draft of the write-up.

Once the researcher feels confident in his/her theory, he/she can begin to analyze and integrate relevant existing literature into it. A classic GT comparative literature review examines and compares the concepts rather than the contexts from whence the data came. Contextual literature without conceptual relatedness is not integrated, but non-contextual literature (i.e. from other disciplines) should be integrated if relatedness is found. Such a comparison may modify the theory, and it may of course also add to or correct the pre-existing literature. Usually, it is difficult to find relatedness in contextual literature. Consequently, the literature review is usually short.

The key issue comes down to the methodology's as well as the researcher's capability to reveal a credible theory from the data that explains with parsimony and scope. This means the capability to make allowance for and to trigger the emergence of concepts that (1) fit to the data, (2) work to explain, and are (3) relevant for those being studied. Yet, there is also a 4th criterion for assessment. This criterion probably applies to all research, which literally means "search again". A generated orthodox GT is "asymptotic" in the meaning that it approaches what goes on, but most likely, it will never reach any ultimate or final "truth line". Further research, involving new data, may bring it closer to the ultimate "truth line" or the asymptote. Therefore, a generated classic GT is modifiable. It should be open to modification, and consequently fit as a tool for learning. (Glaser, 1992, p. 116).

The Challenges for a Novice Classic GT Researcher

There is no reason to expect that it is easier for a beginner

to use these procedures than it is for a beginner to use advanced quantitative-statistical procedures in research. Yet, the innate and required abilities to learn these different sets of procedures may be very different. While attempting to achieve autonomy in the use of the classic GT methodology, the novice classic GT researcher has to relinquish all theoretical-disciplinal autonomy over the research process, and to surrendered this autonomy and control to the data. This cannot be done without humility and without extended tolerance for extended periods of confusion, while not controlling "as usual". The task of the researcher is to follow where the data might lead him/her while conceptualizing by constantly comparing, memo-writing, sorting, etc. From this relinquishing of autonomy, another kind of autonomy has to emerge. This is researcher autonomy as the researcher gradually learns to use the research procedures as prescribed. Such autonomy is not obtained without accomplishing a major research project. However, this is a description of a good outcome. A different outcome is quite possible if no qualified methodological coaching is available, and the need for such coaching may be underestimated. The need to emphasize the classic GT research procedures and stages of research as necessary requirements for fulfilling the classic GT research rationale may also have been underestimated. These relationships are fundamental for fully understanding whether or not classic GT is the right methodology to choose for a given research task and research purpose, and also for understanding the methodology.

The suspension of prior knowledge and the keeping of preconceptions in check will usually lead to long periods of seeming deadlock, confusion, even depression, while no stable patterns are seen in the data. In such a situation it becomes tempting to find another solution than "to discover the core variable first" for solving the problem of "many equally justifiable interpretations of the data". A pre-framed professional concern or preconceived theoretical perspective may replace the role of the core variable.

In such a situation it may also become an option to apply the different GT procedures that are prescribed by, e.g., Strauss and Corbin (1990; 1998) as an alternative. The Strauss-Corbin version of GT also applies a core variable, but

this core variable is found at a later stage of research to sum up or integrate the findings. (Strauss and Corbin, 1998, pp. 143-161). This core variable has not the role to delimit the study from its start in order to solve the problem of "many equally justifiable interpretations of the data". Furthermore, the Strauss-Corbin version of GT applies the procedures of "axial coding" and the "consequential/conditional matrix" (ibid., pp. 123-142, 181-199). These represent a different coding paradigm that replaces the role of theoretical coding, sorting and partly substantive coding in classic GT, and the role of the "classic GT form of induction". This coding paradigm more restricted. It favours the generation of concepts that fit within a narrow range of theoretical codes. These are mostly the theoretical codes of symbolic interactionism or the stimulus-organism-response model (ibid., p. 128). As opposed to this, the classic GT researcher has to be open for the emergence of any type of theoretical code, and their number may range between 40 and several hundred (Glaser, 2005, pp. 17-30).

If the researcher needs to pre-frame his/her study, to predefine the core variable, or to define the core variable at the end of the study, or to use a given theoretical perspective as a substitute for finding the core variable as the first stage of research, or does not want to use "the classic GT form of induction", then classic GT definitely will be a wrong choice of methodology.

An Approach to Compare Methodologies that is Simpler

Detailed explanations of the many methodological differences are of course necessary, and are especially valuable when provided by the methodological pioneers. Barney Glaser (1992) has given his own detailed account of the differences between classic GT and the version of GT that has been prescribed by Strauss and Corbin (1990). Glaser's critique can easily be misunderstood. Glaser does not claim that classic GT is a better methodology. Glaser just concludes that the Strauss-Corbin version of GT is fundamentally different from classic GT methodology, and that this very different methodology should be referred to a different name:

> It is a "new" conceptual method, uniquely suited to qualitative research, that simply uses the grounded theory name, with the author having no realization of what grounded theory was in the first place – what it was in goals, methodology, freedom, level of abstraction, constant comparison, naturalism, emergence, trust and care about what the participants perceive and what their problems are. (Glaser, 1992, pp. 123-124).

Jan Hartman (2001) has also provided a detailed account for the differences between these two different "grounded theory" approaches. In Hartman's view, the most important idea perhaps behind grounded theory, as it was conceived by Glaser and Strauss (1967), is that the theory that is generated has to emerge without being influenced by a priori theoretical assumptions, and that all elements in the theory have to be grounded in data. Hartman concludes that the Corbin-Strauss GT procedures will not always be able to fulfil this original intention behind grounded theory. (Hartman, 2001, pp. 41-42). This also means that the de facto rationale of the Corbin-Strauss GT methodology is different from classic GT rationale.

In this article, the two "*hallmarks*" of classic GT have been used to explain the classic GT research rationale. Jointly these two "hallmarks" justify the pivotal role of the *core variable* in solving the problem of "multiple equally justified interpretations", the role of the very different "*classic GT form of induction*" to prevent preconceptions and to facilitate grounding, and the role of the procedure of "*conceptualizing while constantly comparing*" while applying the "*classic GT form of induction*" for the detection of stable latent patterns in the data. When this frame is used for comparing methodologies, the fundamental difference between classic GT and logical deductive or hypothetical-deductive approaches is obvious. The fundamental differences between classic GT and other mainly inductive-qualitative or hermeneutic research approaches, as well, do not need further elaboration.

The *first and second hallmark* of classic GT, i.e., the role of the *core variable*, and the very different "*classic GT form of induction*", are enough to highlight a fundamental difference. That many of these other methodologies also use procedures for

coding and *comparing* of qualitative data, as well as *memo-writing* does not eradicate this difference. Because the "*classic GT form of induction*" and the role of the *core variable* differ from other inductive-qualitative approaches, the classic GT procedures for *coding*, *constantly comparing* and *memo-writing* and *sorting* are applied very differently. To assume that procedures with the same name mean equivalent procedures only leads to confusion. Because of the differences between the classic GT and the Strauss-Corbin set of research procedures, these two sets research procedures could lead to the emergence of dissimilar *core variables* and dissimilar sets of *substantive concepts* within the same area of research.

The Role of Symbolic Interactionism

Many authors have linked *symbolic interactionism* with Glaser's classic GT. There are many examples, and it is beyond the scope of this article to comment on them (Alvesson & Skoldberg, 2000; Denzin & Lincoln, 2000; Creswell, 1998; Morse, 1994). It has even been stated that *symbolic interactionism* is the foundational philosophy of the original or classic GT. If this were true, this would mean that any prospective classic GT research had to start with a preconceived or predefined theoretical perspective, namely the perspective of *symbolic interactionism*. If this were true, classic GT would be inconsistent and hence meaningless. Dr. Glaser has carefully explained that *symbolic interactionism* is *not* the foundational theoretical perspective of classic GT. Classic GT is a general inductive methodology that presumes no discipline or theoretical perspective or data type (Glaser, 2005, pp. 141-160). In his book from 1998, Dr. Glaser gives an account of how his acquaintance with the Chicago school of *symbolic interaction* through Anselm Strauss gave him "*a chance to analyze qualitative data by applying my quantitative ideas to qualitative data*". It also gave him a chance of fully absorbing the notion that man is a meaning-making animal (Glaser, 1998:32). This may have been an important step for a researcher, who previously had been accustomed to quantitative research procedures, but this does not mean adherence to the methodological and theoretical perspective of *symbolic interactionism*. However, the *axial coding paradigm* of the Strauss-Corbin version of GT is directed towards some pre-

selected theoretical codes (Strauss and Corbin, 1998, p. 128), and these are quite compatible with symbolic interactionism.

Some Examples that Highlight the Difference

The difference between classic GT and other versions of GT can be illustrated by some examples. Frederic Lee has made some attempts to apply GT methodology within the context of macroeconomics (Lee, 2002a:4; Lee, 2002b; Lee, 2005). However, Lee's research problem is entirely set within the paradigm of post-Keynesian economics and heterodox economics without any focus on what is the most important and problematic for those being studied. This means that classic GT will be unsuited for Lee's research task and research purpose, and consequently, Lee applies another version of GT.

One example of a GT study in business that deliberately avoids classic GT is Tomas Brytting's study of "Organizing in the small growing firm" (Brytting, 1991). About the core variable Brytting writes: *"The study's "aspect" or "core variable" was set at the outset: "organizing processes in small firms". An analysis <u>à la Glaser</u> would not have defined that core variable until later on in the research process. With this study's data, Glaser might have ended up with a theory about sensemaking in the small firm…/…My view in this study has been that generation of theory might benefit from the same systematic and cumulative ambition that guides the testing of theory."* (Ibid., pp. 209-210). Due to Brytting's research purposes, another version of GT was a more fit choice for him. However, Brytting's understanding of a *core variable* has nothing to do with the *core variable* in classic GT, and it does not correspond entirely to the meaning of the core variable in the Strauss-Corbin version of GT. Brytting preconceives the notion of "sensemaking", and "organizing processes in small firms" is just his general research topic.

In her book, "*Grounded Theory in Management Research*", Karen Locke (2001) explains the use of the Corbin-Strauss version of GT. However, it is remarkable that she does not take Dr. Glaser's clear position seriously. Dr. Glaser states that the Corbin & Strauss version of GT is an entirely different methodology. (Ibid., p. 71). Locke labels both as grounded theory. Consequently, her readers do not obtain any clarity

regarding the difference between these two research methodologies. Neither do her readers obtain any clarity regarding the classic GT research rationale and the consequential classic GT research procedures and stages of research. For example, Locke misses the pivotal role of the *core variable* in classic GT and she does not mention the procedure of *sorting*. She also states: "*Certainly, the school of thought, namely symbolic interactionism, that informed the understanding of social reality expressed in grounded theory's research practices, appears to have been left behind.*" (Ibid., p. viii). Thus, for Locke, correct use of GT means to view and treat the data through the "glasses" of one particular theoretical perspective, namely the perspective of *symbolic interactionism*. Avoidance of any such pre-framing is part of the classic GT research rationale. This may be the clearest difference between classic GT and other versions of GT.

Conclusion

When the essential elements of classic GT are used as a frame of reference, a simpler and yet concise comparison of classic GT and seemingly similar methodologies can be achieved. The essential elements are: The first hallmark of classic GT, ["to keep the main concern and its recurrent solution of those being studied in focus"], the finding of the consequential *core variable* as the first stage of research, and the subsequent and consequential delimiting of the research to the *core variable*. These elements minimize the problem of "many equally justifiable interpretations of the data".

The Corbin-Strauss version of GT finds a substitute solution to this problem. This solution is not necessarily an inferior one. It solves the problem of "many equally justifiable interpretations of the data" by viewing and treating the data through the "lens" of a restricted range of possible theoretical codes and hence pre-selected theoretical perspectives and possibly also predetermined professional concern. Consequently, there is no need to find the *core variable* as the first stage of research, or any need or urgency to find it at all.

The second hallmark of classic GT ["*to prevent any preconceived professional concerns to mask what actually goes on in the field of study*"] cannot apply in the same way, or apply

at all, in the Corbin-Strauss version of GT. This second hallmark is tantamount to the "*classic GT form of induction*", and it is inconsistent with the axial coding paradigm of the Strauss-Corbin version of GT. As a consequence, the procedures of conceptualizing (coding) have to be applied differently in the Corbin-Strauss version of GT.

Because the Corbin-Strauss version of GT finds a substitute solution to the problem of "many equally justifiable interpretations of the data", a user of this methodology needs not to endure long periods of seeming deadlock, confusion, even depression, while no stable patterns are seen in the data. It will always be easier to interpret the data through the "glasses" of a pre-determined theoretical perspective, and this will ultimately yield the findings of a standard solution. To deem this solution inferior however, is pointless.

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Mutual Intacting: Keeping the patient-practitioner relationship and patient treatment intact

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Abstract

The aim of this grounded theory study was to discover the main concerns of clinical practitioners when making clinical judgments in the community care context and to explain the processes they used to resolve practice problems. Interview data from twenty-one advanced practitioners working in various mental healthcare and accident and emergency settings in Ireland was collected. In this paper, the process of clinical judgment is conceptualised as 'Mutual Intacting'. It proposes that clinical judgment comprises three stages: *situated patterning*, *intacting therapeutic relationship*, and *intacting therapy*. 'Mutual Intacting' explains how clinical practitioners make clinical judgments through a process of adapting treatment so that the patient-practitioner relationship is maintained and treatment is delivered in a way that takes account of the patient's circumstances.

Background

The importance of understanding how clinical judgments are made is highlighted by the professional and policy literature about advanced practice in nursing (National Council, 2004; Royal College of Nursing, undated). The ability to make clinical judgments is an essential skill required for all areas of professional practice; however, it is the level of clinical judgment which involves initiating and delivering therapeutic interventions that differentiates advanced practitioners from other grades in nursing. From an international perspective, developments in nurse prescribing have resulted in a growing number of nurses who are responsible for prescribing medication and for making clinical judgments affecting direct patient care (International Council of Nurses, 2001). These developments place clinical judgment firmly on the research agenda with questions concerning the relevance of the knowledge base that currently informs clinical practice.

Current explanations of clinical judgment in nursing tend to be extrapolated from the knowledge gained from the hypothetico-deductive approach (Elstein, 1978) and the related information processing theory (Simon, 1978; Newell & Simon, 1972), and Benner's (1984) work on intuition. According to the hypothetico-deductive approach, practitioners work through a process of cue acquisition in order to generate potential hypotheses then further cue and data collection to confirm or negate each hypothesis so that eventually a single outcome or diagnosis is reached. The main contribution of this approach is that it provides a systematic analytical process for clinical practitioners when making a diagnosis. Assumptions within the hypothetico-deductive approach are based on normative cues; that is, the association of clusters of cues with a particular diagnosis is based on knowledge derived from generalisations. This excludes a small, but nevertheless, important part of the patient population. Patients who present with atypical symptoms when compared to the general population or patients who present with an individual set of symptoms unique to them are effectively outside of the 'norms' and this limits the usefulness of the hypothetico-deductive approach in clinical practice. Another limitation, noted by Buckingham and Adams (2000a), is that the majority of research studies focus on biomedical signs and symptoms and on how clinical practitioners process these cues. In contrast, there is a paucity of research considering the role of psychosocial factors as cues in clinical judgment. This is an important gap, particularly in view of the evidence on patient behaviour in chronic illness which demonstrates that significant cues may be unrelated to the illness or, alternatively, patients may have learnt to minimise or view persistent symptoms as being 'normal' (Paterson *et al.*, 2001).

An alternative explanation of clinical judgment, intuition, is said to involve the rapid and unconscious processing of data (Cader *et al.,* 2005; Buckingham & Adams, 2000b, Hammond, 2000). Contrary to the view that intuition does not involve analysis, intuition entails the use of heuristics or 'mental rules of thumb', which are short cuts to making clinical judgments (Hallett *et al.,* 2000; Cioffi, 1997). Whilst Tversky and Kahneman (1982) describe three different types of heuristics; namely, representativeness, availability and anchoring and

adjustment, a common cognitive activity throughout all heuristics involves pattern recognition. Pattern recognition is essential to intuition and clinical practitioners, through experience, learn how to recognise and act on appropriate patterns (Easen & Wilcockson, 1996). Opinions regarding the contribution of intuition to clinical judgment in nursing are divided. Some consider intuition important to nursing practice (McCutcheon & Pincombe, 2001; Cioffi, 1997); others point to a commonly cited criticism of intuition that links errors in human judgment with heuristics and bias (Thompson, 2002). This criticism, however, is now being challenged as further research in cognitive psychology regarding the use of heuristics demonstrates that simple rules, which yield quick decisions, can be highly accurate (Ayton, 2005).

Both approaches, hypothetico-deduction and intuition, provide some insight into the cognitive aspects of clinical judgment; however, they fail to consider other aspects including what is the clinical practitioners' main concern and what strategies are used to resolve practice problems. Given that advanced practitioners are often making clinical judgments in situations where patients are actively involved in their own care, this is an important gap in understanding the process of clinical judgment. Having broadly identified the research area from the literature, the problem, however, did not emerge until the researcher entered the clinical practice area and began the inductive process of grounded theory inquiry.

The Research Method

The aim of this study was to generate a substantive theory that explains how advanced practitioners make clinical judgments effecting direct patient care in community care settings. Grounded theory (Glaser, 1998; Glaser & Strauss, 1967) was selected as it provides for the systematic and inductive generation of theory from data and consequently, offers a viable means of developing theory that is relevant to everyday clinical practice. From an advanced practice perspective, the development of practice-based theory is important, so that practitioners can have access to useful and dependable knowledge. This has led practitioners and researchers to develop numerous middle-range theories that are considered highly relevant for specific aspects of clinical

practice (Brown, 2005).

Advanced practitioners working in community care settings in Ireland were invited to take part in the study. For the purpose of this study, an advanced practitioner was defined as an autonomous practitioner with nursing qualifications who was responsible for initiating and providing therapeutic interventions and for managing a patient caseload. Each participant was provided with information about the research and gave written consent prior to the interview. Theoretical sampling was used later in the research process to develop the key categories that were emerging from the initial data analysis. Theoretical sampling led to data collection in contrasting clinical judgment contexts: new and established patients; long-term mental health and acute accident and emergency (A&E). Comparative data were used throughout the process of data analysis. Importantly, it provided a means of exploring how clinical practitioners adapted their decision-making in these different clinical situations. At a point when theoretical saturation had been reached, a total of twenty-one clinical practitioners had been interviewed. The sample consisted of fifteen practitioners from mental healthcare and six from A&E. Fifteen were female and four were male. As part of negotiating access to clinical practitioners working in six healthcare organisations, institutional consent was obtained and, where required, from the appropriate research ethics committee.

Data collection took place directly in the clinical practitioner's clinical area immediately following patient treatment. Interviews were based on the clinical judgments made for actual patient care. An important grounded theory maxim is that researchers enter the research field with open questions to allow the participant's own story to unfold without the direction of pre-conceived questions. Therefore, the guiding questions used throughout the interviews focused on eliciting what were the clinical practitioners' main concerns and how they addressed or resolved such concerns when making clinical decisions. These open questions proved useful in facilitating clinical practitioners to tell their story. The use of such open interview questions also enabled multi-layered storytelling whereby during the same interview clinical practitioners could

revisit key issues/concerns and tell another 'mini-story' which provided yet further detailed information about their main concerns.

Once the first interview was completed the process of data analysis began. The systematic analysis started with open coding, whereby the interview transcript was analysed line-by-line. Glaser's (1998) key questions, namely, 'what category does this incident indicate?' and 'what property of what category does this incident indicate?' were asked constantly during the process of data analysis. The emergence of categories from the open coding and constant comparative analysis was the trigger for starting selective coding. Interview transcripts were analysed again; this time using the newly developed codes to test if they patterned out. The purpose of selective coding was to delimit coding to those categories relevant to the emerging conceptual framework (Glaser & Holton, 2004). Importantly, selective coding also provided verification that the emergent theory fitted the practice of clinical practitioners in the substantive area. Of the early substantive codes that emerged during the initial data analysis some (for example, 'Levelling') endured and became visible throughout subsequent data collection and analysis. Other early codes (for example, 'See-Saw Debating') were not substantiated during further data collection and analysis and were ultimately superseded by other codes. Memoing was used throughout data analysis to put down on paper any thoughts and ideas that came up. These memos became the powerhouse of the research process in the sense that they mapped out what was happening and provided the impetus and direction for subsequent data collection. Theoretical sampling was used to collect further data from specific areas; in this case, contrasting new with established patients and chronic with acute patient care situations.

In this study, the emergent categories were derived directly from the rich descriptions provided by the clinical practitioners and through the systematic analysis of data. One of the main categories, 'intacting therapeutic relationship', was developed as it became clear from the clinical practitioners' accounts that keeping the patient-practitioner relationship intact was an important part of the clinical judgment process. Comparative analysis of different incidents revealed that

avoiding break-upable moments was a strategy that clinical practitioner used to increase the likelihood of maintaining a therapeutic relationship. Comparative analysis of new versus established patients demonstrated that there was a relationship between avoiding break-upable moments and the stage of the patient-clinical practitioner relationship. With new patients, the clinical practitioner is more likely to avoid anything that jeopardises the relationship whereas she is more likely to take the risk of using interventions that challenge patients once she is sure that a clinical patient-practitioner relationship has been established.

The Emergent Theory

From data analysis of 33 in-depth interviews that explored practitioners' experiences and concerns in various clinical judgments, 'Mutual Intacting' emerged as a basic social process. It explains how clinical practitioners make clinical judgments through a process of adapting treatment so that the patient-practitioner relationship is maintained and treatment is delivered in a way that takes account of the patients' circumstances. The theory of 'Mutual Intacting' (see Figure 1) consists of three stages: 'situated patterning', 'intacting therapeutic relationship', and 'intacting therapy'. 'Situated patterning' describes how clinical practitioners use such strategies as selectively looking for evidence in order to identify patterns, gauging levels of priorities, situating clinical judgment in the context of the patient's circumstances and the clinical practitioner's professional and core value systems as part of patient assessment. 'Intacting therapeutic relationship' describes how clinical practitioners build up and then maintain their relationship with patients by getting alongside patients, building up the patient-practitioner relationship whilst maintaining professional boundaries, avoiding situations that interfere with the relationship and moderating patient treatment so that a therapeutic relationship is built-up and then maintained throughout the course of patient treatment. Finally, 'intacting therapy' describes how clinical practitioners use strategies such as providing information, guiding patients towards reaching therapeutic goals, working around problems that could interfere with treatment and avoiding situations that could block progress so that patient treatment is

maintained and ultimately completed.

Why 'Mutual Intacting'? How did this emerge as the core concept? As data collection and analysis progressed, it became clear that the clinical practitioners' main concerns were twofold: firstly, to maintain the patient treatment and, secondly, to maintain the therapeutic relationship. For example, the strategies that clinical practitioners used in order to get alongside patients to avoid break-upable moments during nurse-patient encounters and to de-limit boundaries indicated that they actively worked at developing and then maintaining the patient-practitioner relationship; that is, keeping it intact. 'Intacting' best summarised the complex strategies used in keeping the therapeutic relationship together, uninterrupted and undamaged. Itcaptured the essence of what had emerged from the data. Furthermore, the dynamic relationship between 'intacting therapeutic relationship' and 'intacting therapy' was evident from the ways in which clinical practitioners described moderating patient treatment in order to keep the therapeutic relationship intact and conversely, from the ways they described needing to establish the therapeutic relationship before starting patient treatment. The relationship between the two concepts, 'intacting therapeutic relationship' and 'intacting therapy', was based on their inter-dependence insofar as clinical practitioners actively and simultaneously worked at keeping both intact. 'Mutual Intacting' encapsulates this key process and conveys the sense of joint dependence, inter-connectedness, interaction and reciprocity which emerged from the data.

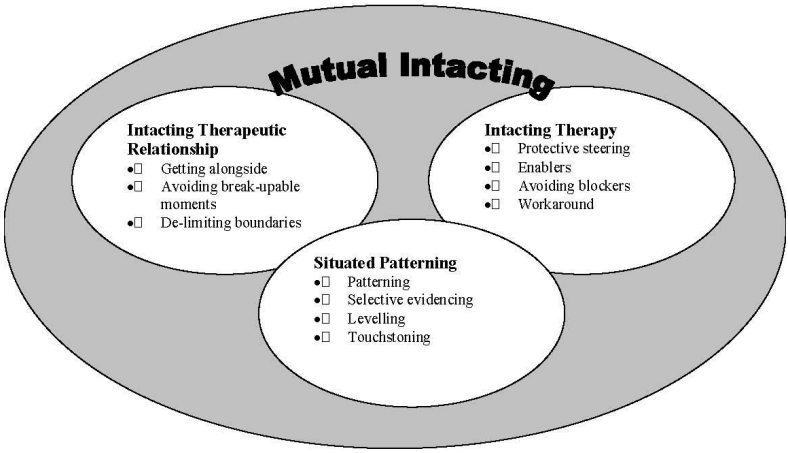

Figure 1: The Theory of Mutual Intacting

Situated Patterning

As part of the process of patient assessment, clinical practitioners are highly aware of the importance of reaching a diagnosis and of having a comprehensive understanding of the patient's problem. 'Situated patterning' is essential not only in deciding which clinical intervention to use at the start, but also in evaluating the effectiveness and on-going use of treatment. Practitioners are aware of the importance of making sense of the patients' problems before deciding which treatment is needed, and they work at achieving this by taking pieces of information during patient assessment and constructing them into patterns that they can recognise. Clinical experience is essential insofar as repeated exposure to similar types of problems or patient presentations enables practitioners to build up their own reference library of patterns; this forms the basis of their assessment of the patient's problem and diagnosis. Furthermore, practitioners also build up a reference library of treatments of which they have first-hand experience and have found to be effective in the past. Practitioners link the choice of treatment to ones that previously have worked and are considered to be 'tried and tested'. 'Situated patterning' not only involves matching patients with past experience of similar patients but also in putting the clinical judgment in context so that treatments can be moderated to suit the patient's specific needs. In order to complete 'situated patterning', practitioners

use a range of different strategies including 'selective evidencing', 'levelling' and 'touchstoning'.

Selective Evidencing

As part of the process of looking for patterns, practitioners search for evidence and use strategies such as looking for tell-tales, selective questioning and back-upping to collect the key information that is needed for making a clinical judgment. Selective evidencing is an important part of clinical judgment; however, practitioners use it differently depending on whether they are dealing with new or established patients. For new patients, practitioners use selective evidencing to develop recognisable patterns that support a diagnosis whereas for established patients, they focus more on establishing whether the treatment is effective.

During patient sessions, one of the key strategies is looking for tell-tales, whereby practitioners filter the dialogue and observe the patient's behaviour looking for indicators as to the nature of the problem or for positive/negative indicators as to how they are responding to treatment. In many situations, these tell-tales are subtle and practitioners are constantly alert to detecting indicators that are relevant to the patient's problem. As one practitioner explains:

> It may be the way they said it. It may be the amount of emphasis they put on. It may be the fact that he actually diverted from it in the first place. But there is usually something that alerts me… it's like you are able to separate the chaff from the wheat and you are able to go down the particular route that you are looking for.

Importantly, if there are any gaps in the information needed by the practitioners they use selective questioning to fill in these gaps. Selective questioning is used to: rule-out various factors; ascertain more fully the circumstances surrounding the problem; address specific concerns about the patient's situation, particularly about safety issues; and, assess how patients are responding to treatment. If sensitive issues such as domestic violence /abuse or sexual issues are involved or if the patient-practitioner relationship is not established, practitioners avoid direct questioning, which may have the effect of closing down the lines of communication and, consequently, be counter-

productive to obtaining necessary information. Instead, practitioners tend to wait until the patient-practitioner relationship is more established and then carefully collect the information needed to complete 'situated patterning'. Therefore, there is a link between selective evidencing, patterning, and maintaining the patient-practitioner relationship; namely, the practitioners work at completing the pattern whilst also keeping the patient relationship intact.

Practitioners also use back-upping particularly in situations where there is a high degree of uncertainty or risk associated with the clinical judgment. Common sources of back-upping include healthcare colleagues or tests such as X-rays, blood tests or psychological tests. In situations where the patient is not considered a reliable source of information, the patient's family or friends are a useful means of validating the patient's history. There are limitations to using family and friends as back-ups; namely, that the patient's confidentiality is maintained and some patients may not agree to having them present during history taking and assessment.

Levelling

'Situated patterning' also involves levelling whereby practitioners calculate what treatment priorities, risk of consequences and level of organisational support. Importantly, as part of the initial assessment, practitioners gauge the patient's comprehension level because it is perceived to be linked to the patient's ability to understand and follow healthcare advice. It is for this reason that practitioners note the patient's ability to understand language, the patient's age, gender, occupation and level of education so that they can moderate the vocabulary or indeed the information given to patients.

Touchstoning

Finally, 'situated patterning' involves touchstoning whereby practitioners refer to the guiding principles underpinning their actions. For example, practitioners sometimes refer to theoretical principles they have learned about in their professional training courses that are considered relevant to the particular patient problem. In situations where the healthcare institution has clinical guidelines developed

specifically for the practitioner's scope of practice, they ensure that clinical judgments meet these guidelines. As this practitioner explains:

> …our approach to patients would be quite similar you know, again I think it is because it is a safe and well outcome for the patient but, the guidelines have to be there for safe practice, but you can adapt them…not going outside of our scope of practice either.

Although clinical guidelines are used as part of everyday clinical judgment, they are not used as a rigid framework perceived as restricting clinical judgment but rather as a set of flexible guidelines that can be adapted to the specific situation. In touchstoning, practitioners also refer to ethical principles drawn from their professional code of practice especially in relation to protecting the patient, doing no harm and maintaining confidentiality. This includes recognising the importance of the patient's right to choice in deciding whether or not to accept treatment. The degree to which touchstoning occurs varies across the different clinical situations. In most situations, the level of touchstoning is low insofar as practitioners just briefly refer to the guiding principles and are aware that they set the parameters for their scope of clinical practice. In contrast, the level of touchstoning becomes high in situations where a difficult conflict needs to be resolved.

Intacting – Therapeutic Relationship

For clinical practitioners, 'intacting- therapeutic relationship' is considered critical to effective treatment insofar as the quality of clinical diagnosis or assessment is conditional on the patient's willingness to provide the necessary information for 'situated patterning'. The therapeutic relationship needs to be established before treatment can begin and furthermore, the patient's willingness to follow the practitioner's treatment advice or to continue with treatment is conditional on the therapeutic relationship. Clinical practitioners, therefore, are highly aware of the importance of firstly establishing and then maintaining therapeutic relationships with their patients.

Getting alongside

'Intacting-therapeutic relationship' involves getting alongside whereby practitioners work at engaging their patients. In situations where practitioners work in busy clinics with a lot of different people around, they actively work at creating an atmosphere where patients can feel that they are getting individual attention. In order to build up a one-to-one relationship, practitioners organise to see their patients in a quiet area or private room away from interruptions. Practitioners also use other strategies such as positioning the patient alongside them as opposed to sitting behind a table, avoiding using an interrogation or interview approach and mimicking a friendly, homely situation in which the patient can feel at ease.

Avoiding break-upable moments

Keeping the therapeutic relationship intact involves avoiding break-upable moments, which includes avoiding anything that can interfere with the patient's level of trust in the practitioner or which distresses/angers patients so that they want to end treatment before it is completed. Practitioners continuously monitor patients for indicators that the relationship is deteriorating.

De-limiting boundaries

Although the practitioners' main concern is to maintain a therapeutic relationship, it can also involve the use of de-limiting boundaries. Particularly with first appointments, practitioners avoid situations that cause embarrassment and instead work at helping patients feel at ease. Although strategies are used to get alongside and to befriend the patient, boundaries are also laid down to limit the level of friendship. Practitioners are aware of the importance of being friendly and supportive to patients; however, they are equally aware of the need to maintain a balance between being friendly and maintaining a professional boundary.

Intacting Therapy

An important feature of clinical judgment is the strategies that practitioners use to maintain treatment and keep it progressing towards the stated goal. 'Intacting therapy'

explains that practitioners are also concerned about maintaining treatment and achieving the treatment objectives. Whilst treatment is considered important, practitioners do not carry it out if it risks jeopardising the patient-practitioner relationship. It is for this reason that practitioners moderate treatment. Under conditions where treatments are carried out over a prolonged period of time, it is moderated for the purpose of bringing patients back for further treatment sessions. If patients do not attend their appointments, they cannot be treated and consequently this can delay recovery. Under conditions in which treatments are completed in once-off sessions, practitioners moderate treatment for the purpose of using treatments that are acceptable to patients. This increases the likelihood that patients will follow the healthcare advice and make the best possible recovery. The extent to which practitioners can moderate treatment, however, is limited and they work within the boundaries of accepted practice. 'Intacting therapy' involves a variety of different strategies; namely, protective steering, enablers, avoiding blockers and workarounds that help maintain treatment and keep it progressing towards a completion.

Protective steering

Protective steering is one strategy used by practitioners in guiding patients towards reaching their therapeutic goals and shielding them from setbacks during the process of making a recovery. It involves leading patients rather than telling them to do something. Practitioners provide information in the form of verbal advice or written leaflets that support patients in making the 'right' decisions. In some situations, specific advice sheets have been developed and are used in combination with verbal advice for the reason that it increases the likelihood that patients would understand and follow the advice.

In situations in which a patient's ability to understand information is considered limited, practitioners avoid giving complex information and use alternative strategies whereby patients can have their information needs met. Therefore, decisions regarding what information to give the patient are based on the practitioner's judgment of whether it is helpful to patients. For some practitioners, protective steering not only concerns *what* information is given but *how* it is communicated.

Different styles of communication are used for the purpose of emphasising important elements. Whilst practitioners actively guide patients in taking a particular line of action, on the other hand, they are aware of the importance of not forcing advice on patients. Protective steering also involves supporting patients as they navigate their way through a complex or unfamiliar healthcare system. The main reason for this aspect of protective steering is to facilitate the smooth movement of the patient through the healthcare system so that patients get what they need to facilitate recovery yet do not become overly dependent on the services.

Workarounds

'Intacting therapy' is also characterised by workarounds whereby practitioners work at resolving actual problems that interfere with treatment. Problems can be classified into three main types; namely, problems relating to patient characteristics, ineffective treatment, and organisational arrangements. For problems relating to patient characteristics such as the patient's intellectual level or ability to cope with illness/ treatment, practitioners moderate and adapt treatments to suit the patient's individual circumstances. For problems relating to a mismatch between the individual patient needs and the facilities available within the organisation, practitioners work within the system so that patients can continue with treatment. An important pre-requisite to workarounds, therefore, is the practitioners' knowledge of the healthcare system insofar as knowing what treatments are available and how to access them. As this practitioner explains:

> knowing what is available…you are in the system for a while, so you know. I have been there for about 12 years, so I know the wards well. I know all that, so to actually leave this job is kind of very difficult starting another job cause you are in the system…knowing the ground level running some of these programmes helps.

This kind of knowledge results from years of working in the service during which time practitioners have direct experience of what treatments are effective and for which type of patient problems. In contrast, practitioners are more cautious about

using services they have not used before. Consequently, the degree to which practitioners use workaround is dependent not only on their knowledge of what services are available but also on whether they have first-hand knowledge of their effectiveness.

Enablers and avoiding blockers

In addition to protective steering, practitioners use a combination of enablers and avoiding blockers to maintain patient treatment. As one practitioner explains:

> …you work on it in different ways, you find what we call an in-road, and it does feel sometimes that you are going up these roads and you are getting somewhere and it's a cul-de-sac. There is a block. You have to come back down and try again, some other route in and usually you find it.

Many of the techniques, for example keeping a diary, are specific to a particular type of treatment. Nevertheless, they serve the function of keeping patients actively involved with their treatment until the next appointment. Practitioners use avoiding blockers for the reason that it prevents disruptions to therapy. Avoiding blockers could be considered as a parallel to the practitioner's use of avoiding break-upable moments in maintaining the therapeutic relationship. Avoiding blockers involves avoiding any treatments that are considered counter-productive to patient progress.

Discussion

When compared to existing clinical judgment theory, there are several notable differences in how clinical judgment is explained in 'Mutual Intacting'. One difference is found between the approach used by clinical practitioners, conceptualised here as 'situated patterning', and that described by heuristics. In heuristics, the clinical practitioner's reasoning is said to involve a process of associating current patient presentation with prior experiences of similar situations (Cioffi, 2000; 1997). Likewise, clinical practitioners in this study actively looked for patterns which not only matched the patient with previous experience of similar patients but also identified a 'fit' within the patient's own usual behaviour or usual state of

health. An important difference in 'situated patterning', however, is that the process also includes the application of rule-based systems either in the form of verification by external sources, application of theoretical principles of treatment or ethical principles concerning patient rights to choice and self-determination. Previous clinical judgment research in nursing has tended to juxtapose the two forms of reasoning as either information processing (Corcoran-Perry *et al.*, 1999; Lajoie *et al.,* 1998; Fowler, 1997; Narayan & Corcoran-Perry, 1997; Greenwood & King, 1995; Grobe *et al.,* 1991; Corcoran, 1986) or intuition (Cioffi, 2000; 1997; Benner 1984,). By contrast, 'situated patterning' suggests that in everyday clinical practice, practitioners use a mixture of different forms of reasoning.

Another difference is found between 'situated patterning' and that described by the hypothetico-deductive approach. In the hypothetico-deductive approach, assessment is dominated by the identification of signs and symptoms as cues, and matching these against pre-set, normatively defined cues; together, these processes comprise a diagnosis. By contrast, in 'situated patterning' the emphasis is on assessing a wider range of cues that takes into account the patient's subjective experience of illness, including the ability to cope with illness or treatment. Thus, patient assessment and diagnosis involves understanding the patient's problem, is not limited to finding a diagnostic label but instead positions the problem within the patient context. Previous research supports the finding that nurses take into account the context in which a patient's problem exists (Rydon, 2005; Clark, 2004; Haworth & Dluhy, 2001; Offredy, 1998). Similarly, the way that clinical practitioners in this study 'knew' the patient in terms of family circumstances, social/ work life, previous experiences of treatment, issues causing concern/ anxiety, lifestyle habits and preferences, suggests that clinical practitioners are able to interpret the problem differently for each patient. Although the type of presenting problems differ across mental health and A&E areas, clinical practitioners take into account the particular set of conditions surrounding the problem, so that each problem is situated within the patient context.

A key feature of 'Mutual Intacting' is that it identifies the main concerns of clinical practitioners when making clinical

judgments and the strategies used to resolve these concerns. Data suggests that the clinical practitioners' concerns are twofold; firstly, in maintaining treatment by ensuring the delivery of care to the patient, thereby facilitating patient recovery and, secondly, in maintaining a therapeutic relationship with patients. The main strategy used by clinical practitioners to resolve these concerns involves moderating the treatment to take account of the patient's needs. Clinical practitioners continuously adjust treatment and make subtle adaptations so that treatment is presented in a form that is acceptable to patients. Importantly, 'Mutual Intacting' highlights the dynamic relationship between maintaining treatment and maintaining a patient-practitioner relationship.

Other research on clinical judgment reports that nurse practitioners often negotiate treatment plans with patients. This may be a compromise initially, leading to compliance at a later stage (Offredy, 1998). Similarly, the strategies of protective steering, workaround, enablers and avoiding blockers identified in this study suggest that clinical practitioners actively reason throughout assessment and treatment to work out the 'best' way forward for the individual patient. In the context of midwifery, Levy (1999) conceptualised the processes by which midwives facilitate informed choices for pregnant women as *Protective Steering*. Levy's study, which portrayed midwives as 'walking a tightrope' between meeting the wishes of pregnant women and acknowledging their own concerns about ensuring a safe delivery, is now considered dated and a product of a medically dominated maternity service (Maimbolwa, 2006; Mander, 2006). In this study, protective steering refers to a process of information giving in the context of navigating a way through the healthcare services and of enabling the patient to recover from chronic mental health problems or acute minor injuries. It is, however, one strategy that fits within a more complex explanation of clinical judgment.

Another key feature of Mutual Intacting is the way in which it conceptualises clinical judgment as a social encounter. In contrast to the traditional approaches of hypothetico-deduction or intuition that view clinical judgment as cognitive reasoning by the individual, Mutual Intacting highlights the

patient-practitioner interaction as an integral part of clinical judgment. In the mental health literature, it is well established that a therapeutic relationship between clinical practitioner and patient is of central importance to mental healthcare (Clark, 2004; McGuire *et al.,* 2001). In this study, clinical practitioners in mental health contexts used a number of strategies to develop and maintain a therapeutic relationship for the reason that bringing patients back was essential to continuing treatment. An unexpected finding, given that clinical practitioners in A&E see patients on a once-off basis, was the extent to which 'intacting-therapeutic relationship' was also used in the acute care setting. The reasons for developing a therapeutic relationship by A&E clinical practitioners differed from those in mental health contexts. In A&E, establishing a relationship with patients was fundamental to patient consent for procedures such as physical examination, suturing of wounds, immobilising fractures and application of Plaster of Paris. Furthermore, establishing a therapeutic relationship was important, firstly, for the purposes of diagnosis in that it influenced the patient's willingness to disclose relevant information and, secondly, in terms of compliance in that it influenced the patient's level of trust and willingness to accept the healthcare advice.

Empirical support from other studies identifies the links between the patient-practitioner relationship and treatment. For example, partnership and involvement in clinical judgment are identified as key determinants of patient satisfaction and acceptance of healthcare advice (Winefield *et al.,* 1995). Taylor (2006) also identifies knowing the patient and gaining their trust as key factors for the reason that it enables nurses to 'get' patients to work with them. For Morse (1991), it is critical that both the nurse and the patient are involved in negotiating healthcare. If either is unwilling to be committed to resolving the healthcare problem, then a unilateral relationship will develop where one side tries to manipulate the other, including patient withdrawal from the health service. The patient-practitioner relationship, therefore, is inextricably linked to the effectiveness of patient treatment.

Limitations

Although clinical practitioners from all areas of healthcare

practice who met the inclusion criteria could have participated, only those working in mental healthcare and A&E were involved in this study. It is recognised that other concepts may have emerged if the study had been extended to include other areas of clinical practice. Further research to determine if the emergent theory holds in other areas of clinical practice and in other areas of professional practice is necessary.

Conclusion

The theory of 'Mutual Intacting' provides an expanded understanding of clinical judgment that challenges traditional approaches of reasoning; namely, hypothetico-deduction and intuition, to consider issues relating to the patient context and the integration of association and rule-based forms of reasoning. Importantly, 'Mutual Intacting' conceptualises clinical judgment as a social encounter in which the establishment and maintenance of a patient-practitioner relationship is central. It sensitises advanced practitioners to consider clinical judgment as a social interaction and how these issues influence the process of clinical judgment in community care contexts. 'Mutual Intacting' is an emergent concept and is one perspective that is premised on the clinical practitioners' understanding of clinical judgment. As a conceptual explanation of clinical judgment, however, 'Mutual Intacting' is limited to the context from which it is derived. Further theoretical development is needed so that the concept is modified through a process of further theoretical sampling drawing from other areas of clinical practice within nursing and indeed, from other healthcare professions.

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Book Review: Glaser, B.G. (2007). Doing Formal Grounded Theory: A Proposal

Alvita K. Nathaniel, PhD, APRN, BC

Forty years after developing the classic grounded theory method with Anselm Strauss, Barney Glaser has published the long-anticipated follow-up monograph that details the method for generating formal grounded theory. Through the years, Glaser continued writing about substantive grounded theory, but formal grounded theory remained in the background, lacking a clear definition and distinctive method. Although his previous monographs offer hints about formal grounded theory, this is the first definitive guide for researcher-theorists. It is Glaser's aim that this monograph will provide the inspiration and direction needed by researcher-theorists who will then generate formal grounded theory. The intended audience for this book is grounded theorists who have previous experience developing substantive grounded theories.

In 1971, Glaser and Strauss wrote *Status Passage*. This was the first formal theory. Through the years, both Glaser and Strauss wrote tidbits about formal grounded theory, but they never clearly explicated the method. As a result, few formal grounded theories exist. Describing and delineating formal grounded theory in a variety of ways, scholars in many disciplines attempted to fill in the gaps left by Glaser and Strauss. In this book, Glaser systematically, thoroughly, and meticulously answers those scholars, refuting some and validating others. Yet, he recognizes that since there are few published formal grounded theories, the method cannot be totally explicated. Nevertheless, enough formal grounded theories do exist for this first attempt at method clarification and procedure formulation.

Glaser points to common impediments that derail many researcher-theorists. These impediments include lack of support from PhD committees, regression into conceptually barren qualitative research, logical-deductive speculation (rather than grounding), and "super think" divorced from reality. He clearly identifies these derailments as he lays out

procedures for generating formal theory. Glaser explains that the generation of formal grounded theory pursues the general implications of a core variable. Using constant comparison, the researcher expands the general implications by generating grounded conceptual categories about it from many different areas and by expanding abstract conceptual generalizations. The researcher uses constant comparison to generate further concepts related to the core category. Grounded formal theory is not an explication of descriptive differences and similarities in a substantive area. Rather it is conceptualizations about the core category, abstracted from the particulars of time, place, and persons. Because it is empirically rooted, conceptualized, generalized, and free of particulars, it potentially applies to many substantive areas.

Except for theoretical sampling, the procedures for formal grounded theory are the same as those for generating substantive grounded theory. Glaser suggests that the researcher samples widely in other substantive areas and populations. Data comes from "wherever" and may include newly generated empirical data from other substantive areas, extant literature focusing on the core category or its general implications, or data generated from previous qualitative descriptions. Glaser writes, "theoretical sampling swings wide." Much like with substantive grounded theory, the researcher-theorist constantly codes the data for categories and their properties, analyzes each day by constant conceptual comparisons and successive delimiting based on the general implications of the core category. Glaser is careful to point out that newly identified categories do not change meanings of the theory. They merely extend and modify the core category and give it broader generalization. The researcher writes conceptual memos and seeks saturation of new indicators that vary the original categories and their properties. Glaser suggests that as saturation occurs and contexts change, the researcher can more clearly see the abstract application to many new areas.

Glaser identifies many uses of formal grounded theory. Academic uses include lectures, readings, consultations, correcting extant theory by modification, giving deeper but transcending understandings, extending the general

implications of theory, and the cumulative building up of theory. Formal grounded theory may be used to guide other research since it gives clear theoretical direction to the research by its grounding. And because it is abstract of people, place and time, it is easy to apply to many substantive areas.

Along with many experienced grounded theorists around the globe, I have thought much about developing a formal grounded theory. But, I did not know exactly how to begin. Thus, I've eagerly awaited this monograph. It was not a disappointment. This book gives clear direction to experienced grounded theorists who wish to develop formal theory. However, like formal theory itself, the writing is very dense. It is not an easy-to-read book for beginners. In order to appreciate fully its nuances, the researcher-theorist must be experienced in classic grounded theory and must have previously read widely from Glaser's previous books and monographs. If a reader has read Glaser's previous work, though, he or she might be distracted by the many references to the classic grounded theory versus qualitative data analysis controversy. Although this controversy is an important one, worthy of discussion, it is almost immaterial to those of us who are dedicated to the classic method. Even so, this book is a treat because the substance is there—waiting to be discovered.

In conclusion, I highly recommend this book to experienced grounded theorists who are interested in developing a formal grounded theory. Since Glaser was co-originator of classic grounded theory, this is the definitive resource. The book answers critics, corrects misunderstandings, and clears up confusion. It clearly defines formal grounded theory and offers a step-by-step approach. Along with Glaser, I hope that that this monograph will provide the inspiration, direction, and method needed for future formal grounded theory research.

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Doing Formal Grounded Theory: A review

Tom Andrews PhD

This is the latest in a family of Grounded Theory books by Glaser that continue to build on previous work and make the methodology much more explicit. Its purpose is quite simply to provide Grounded Theory researchers with a set of procedures that can be followed to generate a Formal Grounded Theory (FGT). Despite several chapters in previous books that deal with generating formal grounded theory it has been given scant attention by researchers and this book aims to reverse this. It brings together and synthesises these previous writings in one book and seeks to specify much more clearly what is meant by a formal grounded theory. As with other more recent books by Glaser, this one is based on data in that the procedures outlined are come from previously generated formal grounded theories. However, Glaser cautions that this is based on limited data since not many FGTs exist yet and as more are generated, the method will become more explicit. The book has been eagerly anticipated by grounded theorists and it does not disappoint.

From the beginning, Glaser emphasises that such theory is not "grand theory" about a theoretical code but a conceptual extension of a substantive grounded theory core category using GT generating procedures. There is a natural tendency to see the applicability of core categories everywhere, beyond the data that generated them. There is a very useful and thought provoking differentiation between descriptive and conceptual generalisation that anyone interested in trying to understand the difference between qualitative methodology and GT would benefit from reading. The discussion of the struggle of qualitative research in dealing with issues of generalisability and transferability is based on extensive reading of the qualitative methodological literature. This struggle is essentially about the near impossibility of making generalisations based on descriptive, unit based findings. There is clear differentiation made between the conceptual nature of GT and routine qualitative data analysis (QDA).

However despite this when it comes to generalisation, there is a tendency in QDA writings to reduce GT to another descriptive methodology with near total miss of its conceptual nature. This is another example of default remodelling which serves to block GT at every turn and is dealt with extensively by Glaser in other writings (see Glaser, 2003). Ultimately the discussion furthers the argument that the end product of a GT study is very different compared to that of a qualitative study-conceptualisation as opposed to description.

In discussing the general implications of the core category, this book will be invaluable to PhD students since most theses are expected to discuss this issue. It will guide and encourage them to think and write about the conceptual generalisation of a core category and would have been invaluable to me when asked about the general implications of my core category at my PhD Viva Voce. It encourages researchers to think carefully about the issue but not to engage in speculation. Also, students are often expected to make an appeal to or suggest future research. This book will enable them to do this in ways that are consistent with GT. For example, they could suggest how their core category could be developed further from a SGT to a FGT.

Glaser emphasises that there are many substantive grounded theories just waiting to be extended to a FGT and the encouragement for experienced Grounded Theorists is that a little data goes a long way in generating one. The procedures used are the same as for generating a SGT but theoretical sampling is different since the core category has already been generated, but this is the only difference. The core category does not change. While this may be stating the obvious, nonetheless one of the pitfalls that Glaser warns about is loosing sight of the core category by dropping into description rather than conceptual comparisons and reverting to generating a more complete SGT. This is essentially about using the literature from any field as more data, comparing it conceptually to the core category. It is the key to generating a FGT and is quite simple yet challenging: loose description and conceptualise. Theoretical sampling according to the core category will guide the literature review. However data can be used from whatever source. However it is is seldom necessary

to go into the field to collect further data for comparison, since there are many data sources to turn to for this. This means that FGT research costs very little money to do, can be done as part of everyday academic reading and without the time constraints associated with generating a SGT for a thesis, very encouraging for and appealing to GT researchers.

Generating a FGT cannot be based on revisiting a SGT in order to make it more comprehensive. Nor can it be done by rewriting a SGT by leaving out substantive words. This is raising the conceptual level mechanically. A FGT must be based on data whatever their source. As in all GT, there are no shortcuts: the method must be followed and a FGT generated by using research to broaden the scope of the theory by conceptual comparative analysis of different substantive areas. The book ends with a very useful and convincing chapter on the uses of FGT which could act as convincing evidence when applying for research grants.

To summarise, this book is a very welcome addition to Grounded Theory methodology by clearly outlining how to generate a Formal Grounded Theory. It is both challenging and thought provoking. The challenge is to generate FGT using any data source by conceptual comparison. Again we are challenged to think conceptually in terms of the literature rather than simply doing a traditional literature comparison. Nonetheless it should encourage experienced GT researchers to now have the confidence to generate FGT. However while this is the explicit aim of the book, there is something here for everyone given that GT is an advanced methodology. For example, PhD students will find the chapters dealing with "Conceptual Generalisation" and "Generalizing: the Descriptive Struggle" particularly useful. We now have a beginning text on how to generate a FGT. The invitation to all of us who have developed a SGT now is to do so.

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