The Grounded Theory Review: An international journal

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From the Editor

In this issue, we present one of Barney Glaser's classic papers, The Local-Cosmopolitan Scientist, originally published in 1963. In this paper, we see how he used secondary analysis of survey data to conceptualize and propose a theory of local and cosmopolitan as a dual orientation rather than the perspective of the time which presented them as dichotomous. In his concise explanation of his methodological approach, we see the early emergence of classic grounded theory methodology and its power to use any data. Here he has worked quantitative data to generate concepts, sorting and organizing his ideas into a concise theoretical explanation that proposed new insights into previously accepted ideas. Glaser's early papers should serve as food for thought for those who continue to advocate that grounded theory is a qualitative method rooted in symbolic interactionism. As the book reviews and commentaries in this issue suggest, however, the methodological muddle of approaches that now seek to occupy the grounded theory landscape suggest otherwise.

Ekins (this issue) offers us a theory under development using a "grounded theory approach" in a discipline that is not well known for grounded theories. He writes beautifully and honestly, offering some interesting emergent concepts but as both Thulesius and Martin suggest in their commentaries on his paper, falls short of a full grounded theory. Their suggestions as to how he might proceed and possibly rescue his theory are valuable advice for many who find themselves conflicted in how to reconcile the myriad 'versions' labelled grounded theory and the advice of experienced qualitative researchers who espouse grounded theory from outside the classic methodology.

Rescuing the novice from methodological confusion appears to be the goal of two recent books on grounded theory methodology. From the perspective of the experienced classic grounded theorists who have reviewed the books for this

issue, there is still some way to go in the rescue attempt. While Glaser has often commented that grounded theory is a simple methodology for developing conceptual theory, empirically grounded yet abstract of the descriptive detail of people, time and place, he has also been known to suggest that it is an elite methodology requiring maturity on the part of the researcher in the ability to stay open to emergence, to tolerate regression in the analytical phase and to resist the perhaps well-intended but fatal efforts of more experienced – but not classic grounded theory experienced – supervisors and collaborators. It takes confidence, creativity, tolerance and intention to stay the course when so many are bent on rescuing through "practical" and "accessible" guides that seek to simplify a cognitively elegant methodology.

Good advice from those who know and practice classic grounded theory can make all the difference in distinguishing classic grounded theory from the many remodelled versions and in clarifying novice confusion. Christensen's methodological note on the literature review in classic grounded theory studies is a welcome response to the many questions on how to deal with the literature.

This issue is also my last as Editor of the Review. Beginning with my first issue in November 2004, I have had the privilege of seeing 21 issues come to life. This, of course, is only possible through the efforts of many individuals – authors, reviewers, and associate editors. To the many individuals who have been part of my learning journey in this role, I wish to offer my sincere thanks. I wish to offer a very special thanks to Barney Glaser for having the confidence in me to take on the role and for his continued support of the Review through Sociology Press.

I am delighted to announce that Astrid Gynnild will be guest editing the next issues. Astrid has served on the Peer Review Editorial Board since 2004, is an experienced classic grounded theorist and a frequent reviewer. Most recently, she has co-edited with Vivian Martin a new anthology of works on Barney Glaser and his legacy.

~ Judith Holton

The Local-Cosmopolitan Scientist[1]
Barney G. Glaser, Ph.D., Hon. Ph.D.

In contrast to previous discussions in the literature treating cosmopolitan and local as two distinct groups of scientists, this paper[i] demonstrates the notion of cosmopolitan and local as a dual orientation of highly motivated scientists. This dual orientation is derived from institutional motivation, which is a determinant of both high quality basic research and accomplishment of non-research organizational activities. The dual orientation arises in a context of similarity of the institutional goal of science with the goal of the organization; the distinction between groups of locals and cosmopolitans derives from a conflict between two goals.

Several studies in the sociology of occupations and of organizations have concluded that some professionals in organizations tend to assume "cosmopolitan" orientation that manifests itself in their working professional goals and the approval of colleagues throughout their professional world, in focusing on a professional career, and in a concomitant lack of loyalty to and effort for the organization. Other professionals tend to assume a "local" orientation that manifests itself in their lesser commitment to the profession in more concern with the goals and approval of the organization and in focusing on an organizational career.[ii] With the growing movement of scientists into research organizations, there has been some interest by sociologists of science in studying the many problems and strains generated by the often conflicting professional and organizational demands and practices that, in turn, generate the adoptive cosmopolitan and local types of orientations.[iii] A partial list of

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[1] This paper was originally published in *The American Journal of Sociology*, Vol. LXIX, No. 3, November 1963

these problems might include varying incentive systems, differential emphasis on publication of research results, types of authority and supervision related to the professional need of autonomy, divergent and conflicting influences on work situations, assignments and research problem choices, budgets of time and money, kinds of compatible work groups, focus of performance, multiple career lines and commitments.

The major goals of many research organizations, particularly industrial research organizations,[iv] are, of course, not consistent with the major institutional goal of science: advancing knowledge by basic research. They often emphasize goals of application, product development, and expert service. The scientist seeking a professional career (one based on pursuing an institutional goal) in an organization of this type becomes a "cosmopolitan", by and large directing his efforts to professional goals, rewards and careers. Insofar as the cosmopolitan is always looking within the community of research organizations for better professional positions and conditions[v] and has little "local" loyalty to inhibit his mobility, the result is a high organizational turnover. A professional career may be impeded by a too-long stay in the industrial context. Indeed, insofar as the industrial organization needs basic research, it becomes detrimental for it to try and induce the cosmopolitan to focus his efforts on the major organizational goals – product development, application and service – since that refocusing may reduce the quality of his basic research contributions.[vi]

Whereas studies of industrial research organizations have usually found scientists who have either a primary local *or* cosmopolitan orientation, I shall try to demonstrate a local-cosmopolitan orientation among highly motivated scientists in an organization devoted to the institutional goal of science. The congruence of goals reduces in considerable measure, if not completely, the strains between organizational and professional requirements that tend to generate distinct local and cosmopolitan types. My principal criterion for ascertaining the general orientation of these investigators will be the direction of their work effort. First, I will investigate the general performance-reward process of science; then I investigate the efforts of those who do well in their scientific

performance to meet organizational demands. From these findings on their professional and organizational contributions, I infer that the orientation of these scientists is both local *and* cosmopolitan. I conclude with a discussion of the implications of this formulation for the developing theory about local and cosmopolitan orientations of professionals in organizations.

The data for the analysis consist of answers given to survey questionnaires in 1952 by the total resident research staff (332) of a large government medical research organization devoted to basic research.[vii] In addition, some letters and documents give further information on the organization. My demonstration will be an effort to *explore* for plausible relations between variables, *not* to develop a strong case built on hard fact. While secondary analysis is well suited for exploratory work, to achieve the latter with old data is probably impossible. Accordingly, I shall use somewhat crude indexes and consider small differences that are consistent, highly suggestive, and that lead to an integrated picture of the local-cosmopolitan process. Since I am only suggesting, not testing, my language will not be riddled by the qualification rhetoric required in more rigorous demonstrations; my inferences will be designed to guide future research on local-cosmopolitan theory along (I believe) useful lines; and my primary effort will be to generalize as opposed to describing a real situation in detail.

The Performance-Reward Process

Motivation. In the institution of science perhaps the most important goal for the *typical* scientist is to advance the knowledge of his field by some form of basic research. A scientist, especially in training but throughout his career, is consistently reminded by colleagues that it is his job to advance knowledge by some increment, large or small. He internalizes the goal, and becomes, using Parson's term, "institutionally motivated" to achieve it.[viii] Therefore, before we know anything about the distinctive personality of this or that scientist, we can hypothesize that to some degree he will be motivated to advance knowledge by virtue of his professional

training and that his research performance will tend to vary directly with the degree of his institutional motivation.

Insofar as the research scientist is motivated to advance knowledge, both his *research work* on problems, hypotheses, and methods as well as his *results* are centrally involved because he has the potential for advancing knowledge at either stage.[ix] Irrespective of failures in results, he may have been quite original in his research work, and vice versa, he may have run rather a routine project into a contributing result.

As a measure of motivation to advance knowledge, I have selected the following two items that tap the *(a)* work and *(b)* result stages of the advancing knowledge process.[x]

"How much do you want? How important is (it) to you?"

a.) Freedom to carry out my own ideas; chance for originality and initiative.

b.) Contributing to basic scientific knowledge.

Degree of importance: (1) utmost, (2) considerable, (3) some or little, (4) no opinion.

Over half the investigators felt both freedom and contributing were of the utmost importance. Each item was dichotomized between "utmost" and the remaining categories since this was where the direction of association consistently changed in cross-classification with criterion variables. The two items were fairly strongly related (coefficient of association = .70). Investigators were considered to have high motivation if they felt both freedom in work and contributing results were of the utmost importance. Fifty-sex percent (186) of them were in this category. Among those of lower motivation, 27 percent (89) were high on one item and 17 percent were low on both items.

For further analysis I dichotomized the index into high and low, distinguishing those who were high on both items from all others. Three justifications for this are: (1) In many cross-classification checks the middle group proved to be more like those low on both items than those high on both items. Therefore, the index is reducible on statistical evidence.[xi] (2) We only need a dichotomized variable to

establish general relations between variables. (3) The dichotomization is at the median, saving cases for necessary cross-tabulation.

Performance - The performance score (developed three months after the survey data were collected) consisted of the assessments by colleagues in the work situation of each investigator's current research.[xii] Each assessment was based on five criteria: (1) Originality and Creativeness, (2) Wisdom and Judgement, (3) Rigor of thought and Precision of methods, (4) Persistence, Industriousness, and Efficiency, and (5) Contribution to the work of others. Three criteria (2, 3 and 4) focus directly on the research work, and two (1 and 5) focus mainly on research results. Thus, this index is based on the same aspects of advancing knowledge as the motivation index. Bearing out my hypothesis on the positive relation between motivation and performance, 19 per cent more of the highly motivated scientists (compared to those with less motivation) have been judged by their colleagues to have high quality performance.

Recognition – Concomitant with the development of institutional motivation is the expectation of reward for achievement of the institutional goal.[xiii] The strong institutional emphasis of science on this achievement-reward pattern is noted by Merton: "originality can be said to be a major institutional goal of modern science, at times the paramount one, and recognition for originality a derived, but often heavily emphasized, goal."[xiv]

The institutional emphasis on professional recognition holds for the research organization under study.[xv] A memo to all personnel described the promotion process as follows:[xvi] The immediate supervisor recommends the investigator to the institute director for promotion. If the latter agrees, he recommends the investigator's case to the promotion board. The board then thoroughly examines the investigator. A sample of his publications is read; prior and current supervisors are asked about him; and his qualifications are judged in terms of the following criteria: (1) Quality of work he has been engaged in, (2) Capacity to develop, (3) Capability in relation to other investigators, (4) Reputation in his field, (5)

Personal characteristics and ability to get along with others, and (6) Ability in the non-scientific work associated with his present and prospective position. If he passes this examination, he is recommended for promotion to the director of the organization, who follows the advice of the board in most cases.

The first four criteria clearly relate to the investigator's professional recognition by focusing on his past, present and potential ability to advance knowledge. I have shown elsewhere that professional recognition is also positively linked with getting along with others and with accomplishing non-scientific work.[xvii] Given the emphasis on professional recognition for advancement, it seems reasonable to assume this reward (recognition) for achievement will maintain motivation for further achievement.

The promotion process clearly indicated the importance of two types of professional recognition: (1) The immediate supervisor's evaluation and (2) Publications. Therefore, if each type of recognition is measured and combined in an index, we can approximate completeness in measuring both the fundamental range of professional recognition required by the organization, and an important patterned form of professional recognition for research work and results. Thus all three indexes are based on the two broad stages of advancing knowledge.

The questionnaire did not include information on actual supervisors evaluation nor did it include information on actual publications (extent or quality). It did not include two items that measure *felt* recognition from supervisors and in publications. They are:

a.) "How do you feel about the way your chief makes *evaluations* about *the quality of work you are doing?"* (1) Accurate, (2) Partly Accurate, (3) No Attempt, (4) No Answer.

b.) "In scientific or other professional papers about work to which you have made some contribution, *is proper credit given to your own contribution* by means of authorship or acknowledgement?" (1) Always, (2) Usually, (3) Seldom, (4) No Opinion.

Over half the investigators feel they received adequate recognition from the supervisor (53 per cent say "Accurate") and in publications, whether by authorship or acknowledgement (72 per cent say "Always"). To construct an index of felt professional recognition I have dichotomized each item between the highest category and all others. This dichotomization occurs as close to the median as possible, and at a statistical breaking point. In many cross-classifications of each item with other variables, the direction of association consistently changed between the highest category and the remaining categories. In combining these two variables into an index of felt recognition, 44 per cent of the investigators are high on both items; 37 per cent of the investigators are high on one item; and 19 per cent are low on both items. I have dichotomized the index between high and all others (low) for the identical statistical and substantive reasons earlier applied to the motivation index.

As suggested, professional recognition tends to maintain institutional motivation in this organizational context. Nineteen per cent more of those scientists who feel they have achieved high recognition (compared to those with low recognition) are highly motivated to advance knowledge.[xviii]

Process – The next step is to show in one table the following process recognition for advancing knowledge (which indicates past performance) tends to maintain motivation (a time sequence based on common observation), which in turn tends to result in high quality research performance (measured three months later). This will give us the basic links of the circular, general performance-reward process in science: research performance leads to professional recognition, which maintains motivation to advance knowledge, which in turn leads to more performance.

In Table 1 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 attaining recognition and accomplishing high quality research performance, this tentatively demonstrating the performance process.[xix] As a social pattern, this circular process will continue if the

performance measured here results anew in recognition.

TABLE 1: RECOGNITION, MOTIVATION, PERFORMANCE

	RECOGNITION (PER CENT)		
	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 I wish to suggest that, besides research performance, it is also possible to predict behaviour associated with research on the basis of intensity of institutional motivation. This is borne out of by one indicator of research behaviour: the amount of time in a typical work week the scientist puts into "performing my own professional work (or work under the guidance of my chief) such as research, professional practice, professional writing, etc." Fifteen percent more of the highly motivated investigators work 21 hours a week or longer on personal research. Furthermore, 11 percent more of those who work 21 or more hours a week on their own research have a high quality performance score.

In combining motivation, personal research time, and performance, Table 2 demonstrates 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.[xx] The magnitude of association between motivation and performance is *diminished* when the intervening effect of personal research time is removed.[xxi] This finding adds a subsidiary link to the performance-reward process as diagrammed in Figure 1.

TABLE 2: MOTIVATION, RESEARCH TIME, PERFORMANCE

	MOTIVATION (PER CENT)		
	Average	Less	Difference
High Performance	57	38	+19

	(186)	(146)	
Proportion with high performance who put: Twenty-One of more hours per week into own research	60 (142)	43 (89)	+17
Less than twenty-one hours per week into own research	48 (44)	35 (57)	+13

FIGURE 1: The performance-reward process in science
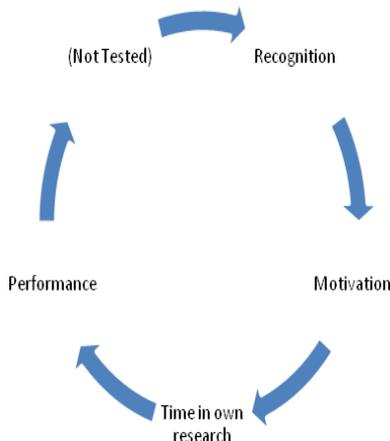
Scientists as Organizational Men

As a link in the performance process, time in own research has direct relevance to the research organization. Insofar as this process supposedly results in the continual fulfillment of the institutional goal of advancing knowledge one might be tempted to say that this is favourable for the organization since this is why the research organization has been created. *But is the process favourable?* Scientists in any organization have other activities and duties, besides their own personal research, that must be accomplished as part of

their organizational commitment. This typical investigator cannot be his own scientist all week long, as is indicated by the fact that the median number of hours put into "own professional work" in a typical week is 29.8.

The question, therefore, arises as to whether investigators with high motivation sacrifice their other organizational commitments for their personal research because of strong desires to advance knowledge.[xxii] If they do, and since this factor is a link in the performance process, then perhaps the above findings have unfavourable consequences for the organization. This process may require too much time for personal research, which may be disruptive for the organization as regards the scientists' fulfilling their organizational commitments.

Table 3 provides one answer to this question. The extra time that the highly motivated scientists put into their research is carried forward, as their weekly time schedule accumulates, with no sacrifice to other professional and organizational activities or commitments. The longer hours put into their own research (15 percent difference) as maintained by highly motivated investigators as time is consecutively added on (1) for other professional productive work (14 percent difference), such as performing services for others and working with close colleagues; (2) for non-productive professional work (21 percent difference), such as attendance at meetings and seminars, reading and dealing with people other than close research associates; and (3) for a total work week (17 percent difference), which includes all other organizational activities beyond their professional ones.

TABLE 3: MOTIVATION AND WORK ACTIVITIES

	MOTIVATION (PER CENT)		
	High*	Low**	Difference
Own research: Twenty-one or more hours	76	61	+15
Plus other professional productive work: thirty-six or more house	63	49	+14

Plus non-productive professional work: forty-one or more hours	69	48	+21
Plus other organizational activities for total work week: fifty-one or more hours	65	48	+17

* N = 186 ** N = 146

In fact, in response to the question, "How much time per week are you now spending on activities which could be shifted to other people or eliminated without impairing your present scientific or other professional work?" more highly motivated investigators suggested that less time be shifted to other people. Thus, in line with not sacrificing organizational work for their own research, the highly motivated investigators are less ready than those with low motivation to shift any additional work load of organizational life upon other men. Indeed, it would have been understandable if they had been more ready to shift activities not directly pertinent to their professional pursuits to other personnel, since they are motivated to advance knowledge, and any activity intruded into this effort might appear burdensome. *It would seem, then, that high institutional motivation tends to make these scientists both hard-working investigators and hard-working organizational men.*

The Distinction between Cosmopolitan and Local

This finding suggests that those scientists who are highly motivated to advance knowledge will be assets to the organization in two ways: (1) achieving the organizational goal, which is the same as the institutional goal of science and (2) meeting non-scientific organizational requirements that take time from research. Thus, the organizational will tend both to persist and to maintain its prestige (through accumulated individual successes) within the community of scientific organizations. The latter aim is very important for attracting and recruiting more capable, highly motivated scientists. Persistence and maintenance of prestige through

achievements of the institutionally designated goal need not always be related. There are numerous examples in the literature that show that attempts to meet requirements for persistence can subvert organizational goals.[xxiii]

This finding – that both research and non-research activities seem important and compatible to highly motivated scientists – *indicates, by the criterion of direction of work efforts, that these scientists are both cosmopolitan and local oriented.* They are oriented to achievement of the institutional goal and honorary rewards, and hence toward professional colleagues everywhere and toward success as members of their profession. They are also oriented to their responsibilities within the organization that provides them with the facilities for advancing scientific knowledge and thus gaining recognition, and with a prestigeful base for that cluster of organizational rewards called a promising career.

Further data support the presence of this dual orientation among highly motivated scientists. As hard-working cosmopolitans oriented to all professional colleagues they are more interested in contacts outside the organization as sources of information, in a move (if necessary) to a university environment (however, motivation does not account for more plans to move), and in belonging to an organization with prestige in the scientific world. Also, they feel a greater sense of belonging to and involvement with professionals within the organization. With respect to the professional or institutional goal, any suggestion of a change from basic research as the only organizational goal to its co-existence with applied research will be cause for concern.

As hard-working locals, the more highly motivated investigators desire an important job in the organization and association with persons who have high status and important responsibilities. In addition, more of them have a strong sense of belonging to the organization and are interested in higher level jobs that are more compatible with the institutional goal. That is, they tend to be interested in the supervision of subordinate scientists rather than in supervision of the organization.

In sum, this congruence of organizational and

institutional goals generates a local-cosmopolitan scientist when the scientist is highly motivated to advance basic knowledge. Devotion to both professional and organization is, in this case, not incompatible, as it tends to be for scientists in industry.

Local-Cosmopolitan Theory

This dual orientation of highly motivated scientists is especially important since, with few exceptions, the research literature characterizes *scientists* as either cosmopolitan or local. They are presented as two distinct types of scientists whose orientations and activities are, if not directly opposed to each other, not related. Shepard, in discussion dilemmas in industrial research, has said, "The research staff itself is likely to be divided into what Robert Merton calls the 'cosmopolitans' and 'locals.'"[xxiv] In his book on industrial scientists, Marcson reports that "it is possible to distinguish between two types of laboratory staff – professionally oriented and organizationally oriented."[xxv] Peter reports of a seminar on problems of administering research organizations, "In the first two of the seminars, some time was spent discussing another bimodal distribution of scientists, those described as 'cosmopolitan' and those called 'locals'."[xxvi]

I suggest that cosmopolitan and local can also be seen as two dimensions of orientation of the same scientist, each activated at the appropriate time and place as determined by the organizational structure within which he works. The question now arises as to whether or not there is a conflict between my findings of cosmopolitan-local orientation and the body of literature that treats the two orientations as distinct. Is one view more correct than the other? If we ask the question, "Under what conditions has each distinction emerged?" then we find that each of the views is accurate and applicable to the particular organizational situation under analysis.

The distinction between cosmopolitan and local scientists emerged during the study of research organizations in which the institutional goal of advancing knowledge is more or less in conflict with a major organizational goal of applying

knowledge. For example, in reviewing industrial research organization studies, Shepard states that the scientist's "motto" is "How much do we know about this?" whereas the businessman's motto is "What is the value of this to the company?"[xxvii] This conflict results in a "problem person: in the cosmopolitan and in a "good employee" in the local.

Scientists take sides in the conflict according to their goal priority; hence the social scientist studying the organization uses this criterion to divide scientists into two groups. The cosmopolitan group makes trouble for management in primarily pursuing the institutional goal and career, and the local group creates little problem in primarily pursuing the company goal and career. In sum, this distinction is a device for understanding organizational problems such as communication of results, turnover, multiple career lines, differential incentive systems, needs for loyalty versus expertise, and so forth.[xxviii]

Cosmopolitan and local as dual orientations of the same scientist emerged in our analysis of a research organization that emphasized the institutional goal. As there was little or no conflict between goals, there was no necessity to take a priority stand, or of being split into groups. Because of this congruence of goals, a local orientation helps to maintain the opportunity to pursue research and to have a career at a highly prestiged locale, both thoroughly consistent with the cosmopolitan orientation.[xxix] In using the notion of dual orientation, we end up talking of organizational benefits, not problems.

Further, I have found this dual orientation among highly motivated scientists, whereas Shepard, as well as the other authors cited, talks of all scientists. Thus, the two conditions that generate the emergence of either groups of cosmopolitan and local scientists, or scientists with a cosmopolitan-local orientation, are (1) compatibility of the organizational with the institutional goal, and (2) highly motivated scientists versus all scientists.

TABLE 4: SCIENTISTS` ORIENTATION

Institutional and Organizational Goals	Professional Motivation		
	High	Medium	Low
Same	Basic research Local-Cosmopolitan	-	Local
Different	Cosmopolitan	Applied research Local-Cosmopolitan	Local

One of the exceptions to viewing local and cosmopolitan scientists as different groups in the literature on scientists is the "mixed type" offered by Kornhauser.[xxx] The "mixed type" is oriented to both company and profession and is interested in "facilitating the utilization of technical results." This applied orientation existed under the conditions of a conflict between the institutional goal and the company goal and is an accommodation seemingly in favour of the company. Thus to date we have two general types of local-cosmopolitan scientists arising under different sets of specific conditions: (1) the basic research local-cosmopolitan and (2) the applied research local-cosmopolitan.

Table 4 locates the various general orientations of scientists to organization and/or profession likely to be generated by the two cited conditions: (1) congruence of institutional and organizational goals and (2) degree of institutional (or professional) motivation.

Last, the concern among the scientists in this study over the potential organizational emphasis upon the applied research goal suggests a few hypotheses about possible changes. If the organization starts to emphasize applied research, those highly motivated to do basic research may give up the basic research cosmopolitan-local orientation and become a definite group of cosmopolitans. The professional motivation of some may drop a little and then they are likely to become applied research local-cosmopolitans. The potential

conflict between institutional and organizational goals may generate these changes, which then could result not only in the loss of benefits to the organization cited in this paper but also in the accumulation of problems cited by those writers who have developed the distinction between cosmopolitan and local as two types of scientists.[xxxi]

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[i] Revised version of a paper delivered at the 1962 meetings of the American Sociological Association. I am indebted to the encouragement of Alvin W, Gouldner and the editorial help of Anselm L. Strauss in preparation of this paper.

[ii] The Terms "cosmopolitan" and "local" were first used by Merton to describe different types of community leaders (Robert K. Merton, *Social Theory and Social Structure* [Glenco, Ill.: Free Press, 1957], pp. 387-420). For a formulation of cosmopolitan and local as organizational types see Alvin W. Gouldner, "Cosmopolitans and Locals: Toward an Analysis of Latent Social Roles," *Administrative Science Quarterly,* II (1957-58), 281-306, 444-80; see also Alvin W Gouldner, "Organizational Analysis" in Robert Merton, Leonard Broom and Leonard Cottrel (eds.), *Sociology Today* (New York: Basic Books, 1959), pp.410-19. For particular studies see Peter M. Blau and W. Richard Scott, *Formal Organizations* (San Francisco: Chandler Publishing Co., 1962), pp. 64-74; Leonard Reissman, "A Study of Role Conceptions in Bureaucracy," *Social Forces,* XXVII (1949), p. 308; Theodore Caplow and Reece J. McGee, *The Academic Marketplace* (New York: Basic Books, 1958)m p.85 and *passim*; Harold Wilkensky, *Intellectuals in Labour Unions* (Glenco, Ill.: Free Press, 1956), pp. 129-53; Warren G. Bennis *et al.,* "Reference Groups and Loyalties in the Out-Patient Department," *Administrative Science Quarterly,* II (1958), pp. 481-500.

[iii] William Kornhauser, *Scientists in Industry* (Berkeley: University of California Press, 1962), esp. chap. v; Simon Marcson, *The Scientist in American Industry* (New YorkL Harper & Bros., 1960); Donald C. Pelz, "Some Social Factors Related to Performance in a Research Organization," in Bernard Barber and Walter Hirsch (eds.), *The Sociology of Science* (New York: Free Press of Gencoe, 1962), p. 357; Herbert A. Shepard, "Nine Dilemmas in Industrial Research," *Administrative Science Quarterly,* I (1956), 346; Hollis W. Peter, "Human Factors in Research Administration," in Rensis Likert and Samuel P. Hayes, Jr. (eds.), *Some Applications of Behavioural Research* (Paris: UNESCO, 1957), p.142; Clovis Shepard, "Orientations of Scientists and Engineers," *Pacific Sociological Review,* Fall, 1961, p. 82. Robert Avery, "Enculturation in Industrial Research," *IRE Transactions in Engineering Management,* March, 1960, pp. 20-41; Fred Reif, "The Competitive World of the Pure Scientist," *Science,* CXXXIV (1961), 1959.

[iv] Kornhauser, *op. Cit.,* p.133; Leo Meltzer, "Scientific Productivity in Organizational Settings," *Journal of Social Issues,* No. 2 (1956), p. 38; Marcson, *op. Cit.,* pp.81-82, 104; Shepard, *op.cit.,* p.347.

[v] Kornhauser, *op. cit.,* p.130.

[vi] *Ibid.;* see also Shephard, *op. cit.,* and Pelz, *op. cit.,* p. 358.

[vii] I am indebted to Donald C. Pelz of the Survey Research Center, University of Michigan, for providing me with these data.

viii Institutional motivation has been dealt with extensively in: Talcott Parsons, Essays in Sociological Theory (Glencoe, Ill.: Free Press, 1954), chaps. ii, iii, Merton, op. cit., pp. 214, 531, 555, 558-59; Robert K. Merton, "Priorities in Scientific Discovery," American Sociological Review, December, 1957, pp. 640-41. It should be noted that advancing knowledge as I deal with it here is institutional, a part of a normative pattern, not a mode of orientation that is simply natural to man. Thus, I make the distinction between institutional motivation (motivation based on internalized norms and goals) and typical human motives (assertive, friendly, ambitious, egotistic, etc.) as elements of concrete motivation.

ix Advancing knowledge is a process that, for any one scientist, is composed of many events. This process has at least two broad stages: research work and research results. Bernard Barber, in talking of "inventions and discoveries," says "they have two aspects, that of process and that of products, and these aspects must be distinguished" (Science and the Social Order [Gencoe, Ill.: Free Press, 1952], p. 193).

x I follow the procedure for index construction outlined and discussed by Paul F. Lazarsfeld, in Merton, Broom, and Cottrell (eds.), *op. cit.,* chap. ii, pp. 47-67; in "Evidence and Inference in Social Research," *Daedalus*, LXXXXVII, No. 4 (1958), 100-09; and with Wagner Thielens, *The Academic Mind* (Glencoe, Ill.: Free Press, 1958), pp. 402-7.

xi On reduction of property space see Alan Barton, "The Concept of Property Space in Social Research," in Paul F. Lazarafeld and Morris Rosenberg (eds.), *The Language of Social Research* (Glencoe, Ill.: Free Press, 1955).

xii This performance score cannot be construed as a measure of recognition, since, to be sure, the scientists were not made aware by the research team of colleagues' evaluations. The essence of recognition is that it is a *known* reward for one's work. For a complete discussion of the construction of this index of research performance see Donald C. Pelz *et al., Human Relations in a Research Organization* (Vol. II, Ann Arbor: Institute for Social Research, University of Michigan, 1953), Appendix C; and *Interpersonal Factors in Research* (Ann Arbor: Institute for Social Research, University of Michigan, 1954), Part I, chap. i, Appendix A.

xiii See Parsons, *op. cit.,* pp. 53-54, 143-44, 230-31, 239, for the formulation that the institutional norms reciprocally define relations between two classes of people or positions.

xiv Merton, "Priorities ...," *op. cit.,* p.645

xv This is not the only government medical research organization that bases promotions on professional recognition. There would seem to be many others. Meltzer reports for his national sample of 3000 physiologists that publication productivity for those in government was the same as those in the university, and that publication was as strong a factor in promotions in both contexts (Meltzer, *op. cit.*).

xvi Charles V. Kidd, "Resolving Promotion Problems in a Federal Research Institution," *Personnel Administration,* XV, No. 1 (1952), 16.

xvii See my *Organizational Scientists: Their Professional Careers* (Indianapolis: Bobbs-Merrill, forthcoming), chaps vi and vii, and see below for the relation of performance process to accomplishing non-scientific work.

[xviii] For other evidence that recognition supports motivation see Donald C. Pelz, "Motivation of the Engineering and Research Specialists" (*General Management Series,* No. 186 [New York: American Management Association], p. 30). He reports that for a national sample of 3000 physiologists, the number of publications and acknowledgements is positively related to intensity of motivation.

[xix] Various sources exist for a full discussion of Lazarsfeld's elaboration analysis of which this is MI type. For the primary source see Paul F. Lazarsfeld, "interpretation of Statistical Relations as a Research Operation," in Lazarsfeld and Rosenberg (eds.), *op. cit.*: see also Lazarsfeld and Patricia L. Kendall, "Problems of Survey and Analysis," *Continuities in Social Research*, eds. Lazarsfeld and R. K. Merton (Glencoe, Ill.: Free Press, 1950), and Herbert Hyman, *Survey Design and Analysis* (Glencoe, Ill.: Free Press, 1955), chap. vii. One could say that the table also shows that motivation leaves to recognition, which in turn leads to performance (14 percent and 16 percent are less than 19 percent). But this is the same process I have described in the text. For motivation to result in recognition implies that there was some performance intervening; for recognition to lead to performance implies that there was some motivation intervening.

[xx] I have based this finding on the one-time sequence. It is also possible that some investigators may have developed a high degree of motivation because of putting in more than 21 hours per week. Hard work could generate interest. Therefore, we may have another time sequence in the performance process of longer hours in research leading to high motivation which results in high performance. However, this is not so. In comparing proportions downward in Table 2 among those with high motivation, 12 percent more who work 21 or more hours a week on their own research have a high motivation score. The original relation between time in own research and performance is 11 percent. Therefore, high motivation, instead of being and intervening variable between time and performance, is a condition that creates a slightly stronger relation between the two. This is, of course, the time sequence I have originally assumed, which shows it is the sequence that prevails in the population under study.

[xxi] I used the "21 or more hours per week" break in the distribution, since it is at this point that the consistent direction of association between time and motivation changes. This distribution ranges from 7 percent who work less than 15 hours per week on their own research and 7 percent who work over 46 hours a week.

[xxii] That this is an important consideration for the organization is indicated by one of the six criteria used in evaluating the scientists for potential promotions: "writing or editorial ability, effectiveness on boards and committees, ability to organize his and others' work, administrative judgement and other traits relevant to his performance on his current job and the job for which he is being considered" (Kidd, *op. cit.*). This criterion indicates that the scientist's worth to the organization is based also on the non-scientific work he has been asked to do.

[xxiii] The foremost example is Philip Selznick's *TVA and the Grass Roots* (Berkeley: University of California Press, 1953).

[xxiv] "Nine Dilemmas ...," *op. cit.*

[xxv] *Ibid.,* p. 18

[xxvi] Ibid

[xxvii] Ibid. A conflict in goals is also the criterion for separating local and cosmopolitan scientists used by Marcson, op. cit., Peters, op. cit.

[xxviii] That the distinction between types of scientists has much potential use in the analysis of problems surrounding the research organization's need for both loyalty and expertise is forcefully brought out in Gouldner, "Cosmopolitans and Locals," op. cit., pp. 465-67.

[xxix] Blau and Scott, *op. cit.,* pp. 70-71, in comparing county agency caseworkers and Bennis' data in professional nurses, note that opportunities for a professional career in an organization coupled with restricted opportunities in competing organizational generate local orientations among professionals. Whether they still remain cosmopolitan or not was not discussed. Their analysis is, therefore, consistent with mine on the local dimension.

[xxx] Kornhauser, *op. cit.*, p. 122. Another exception is Avery's *(op. cit.)*, "The career question confronting the technical man is not, typically, whether to commit himself wholly to localism or cosmopolitanism. Rather he is likely to be constrained to try to extract advantaged from both sources." Gouldner *(op. cit.)*, and Blau and Scott *(op. cit.)*, also have mixed types in their tables but do not discuss them in text. They focus on the distinct groups. Caplow and McGee also note a mixed orientation among professors in high-prestige university departments *(op. cit.)*, p. 85 (see also Warren G. Bennis, *op. cit.,* pp. 481-500).

[xxxi] For an analysis of the generation of cosmopolitan and local factions because of a change in goals see Paula Brown and Clovis Shepard, "Factionalism and Organizational Change in a Research Laboratory," *Social Problems,* April, 1956, pp. 235-43.

The Literature Review in Classic Grounded Theory Studies: A methodological note

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The place and purpose of the literature review in a Classic (Glaserian) Grounded Theory (CGT) study is to situate the research outcome within the body of previous knowledge, and thus to assess its position and place within the main body of relevant literature. The literature comparison is conceptual, i.e. the focus is on the comparison of concepts. The literature comparison is not contextual, i.e., it is not based on the origin of the data. This, of course, means that the literature comparison has to be made in a selective manner.

It is obvious that relevant literature for conceptual comparison cannot be identified before stable behavioral patterns have emerged. Therefore, it is obvious that these literature comparisons have to be carried out at later stages of the research process, and especially towards the end. This restriction with regard to preliminary literature studies does not prevent the researcher from carrying out literature studies in order to find a loosely defined research topic that fits to his/her interests. However, if the researcher believes either that he/she can derive the participant's "main concern and its recurrent solution" from this literature, or that he/she can ignore the empirical discovery of this "main concern" as the first stage of research, the choice of CGT would be meaningless.

To study the literature as the first stage of the research with the deliberate purpose to define the research problem is a common pre-framing solution. If this were the case, the choice of CGT would be a meaningless choice. If the researcher wants to preconceive the research problem, he/she should choose another research method. The researcher may preconceive the research problem by defining it in accordance with what he/she thinks is most relevant, or what the literature claims to be most relevant, or by spotting gaps in the literature in order to identify untested hypotheses. If a

researcher has decided to use Glaser's GT, a preliminary study of the literature in order to derive the research problem would be waste of time. The research problem, when empirically discovered from behavioral data, may be very different from what the extant or originally identified literature assumes it to be.

To avoid the preconceiving and tainting influences from pre-existing literature and pre-existing concepts during treatment of the data, it is recommended that no literature studies in related fields are carried out before the empirical data work is finished and the theory has been generated from the data. However, studies that have applied CGT in closely related fields of enquiry could give some clues. Reading of them is recommended but only after the core category of a study has emerged when coding of data for "emergent fit" could be an option.

Reading methodological literature does not need to be avoided. To read literature on CGT methodology may be necessary during the entire research process. It is even recommended to read totally unrelated literature or fiction, poetry or drama for analysing and recognizing behaviour patterns and their relationships. Systematic reading or "explication-de-text-reading" of unrelated literature in order to obtain general training in the discovery of behavior patterns and of relationships between these patterns is also recommended.

To facilitate an appreciation of the delimiting of the literature review, it may be helpful to review the reasons behind the delimiting of the study itself. Due to the choice of research methodology, the research has been delimited to the main concern and its recurrent processing or solving for the people being studied. Essentially, what this means is that in generating the theory, the researcher has taken the approach of delimiting the study to what is highly important and/or problematic for those being studied. The agenda of those being studied - their substantive interests - sets the agenda for the research. The research outcomes are grounded in this agenda. The use of this particular research methodology is rare, and it is in a sense "contrary" to the accepted view in its avoidance of a pre-framed "professional interest" perspective. It avoids "a priori" and favours the "a posteriori", especially regarding concept fit and the avoidance of delivering research

that is grounded in the agendas of the established professional research communities rather than in the agenda of those being studied. The standard "professional interest" approach for delimiting a research work is different. The standard approach attempts to delimit research to what is seen as professionally important and hence suitably-professionally problematic for the researcher and the research community involved. This may be due to attachment to a particular research methodology, or due to adherence to a particular research program and its particular heuristic and "hard core". Thus, the agenda of the researcher or his/her research community sets the agenda for the research by pre-framing it from the perspective of their own research community. These researchers deliver research outcomes that are grounded in this research agenda. All research is grounded, but this is a different concept of grounding that has nothing to do with the meaning of the concept of grounding, as this term is used in a CGT study. The consequence of the standard approach means a pre-framed grounding in pre-existent literature, in a pre-determined theoretical perspective and pre-determined conceptual usage. Thus, there is much "a priori" in place before the start of research, and the "a posteriori" requirements are fulfilled by statistical testing or data description. Thus, the criteria for literature review easily become standardised. These particular standards for a literature review cannot apply to a CGT study. This is not because a CGT study is considered better – it is not considered better, it is just different.

The different research approach of CGT methodology also means that the outcomes of it conceptually may be very different from what is almost all-pervading in the literature. This also means that the potentials for discursive and meaningful comparison to other literature may be restricted due to some degree of incommensurability. This also means that saturation in the literature comparison will be more easily achieved. Saturation means that the addition of new literature to the literature review does not provide new or noteworthy conceptual properties, or new or noteworthy insights or perspectives. Usually, literature reviews of CGT studies are much shorter than literature reviews of more traditional studies. Firstly, it is delimited to the emergent concepts. Secondly, by saturation, the comparison delimits itself.

Besides being conceptual, a CGT literature review should be discursive in its comparing - it should not be merely passive-describing or listing. A discursive comparison is marked by analysis and analytic reasoning. It may correct the pre-existent literature according to grounded indications, and it may give directions for new research, also evidentiary research. A discursive comparison of the literature also entails finding indications of fit to concepts in the pre-existent literature that may indicate usability. As mentioned before, "fit" is another term for validity, but it means fit in action and usage, not via testing. In a discursive comparison it may become necessary whenever possible to synthesize much of the literature, and thus in a sense to transcend it. This synthesizing may be carried out in different manners. It may, for example, be carried out by delimiting a comparison to a group of paradigms or research programs. It may, for example, occasionally also be carried out by comparing just one particular piece of representative literature (an article or book) that is fairly representative of vast amount of literature. A discursive and conceptually delimited comparison to the literature also means a process that is somewhat coherent from topic to topic. Unavoidably, some issues that some readers might find relevant will be excluded, and some issues others might find less relevant will be included – given their theoretical or methodological perspectives. Thus, much literature will be reviewed without being included or referred to in the treatise. That literature is bypassed in this manner does not mean that it is considered less meritorious or less relevant in general. The opposite may actually be the case. It only means that it is just not considered important in the given context of conceptual comparison – a comparison that follows the chosen methodology.

Thus, the literature review and comparison will be conceptually and not contextually delimited. Conceptual delimiting means comparing emergent concepts - substantive codes, theoretical codes, conceptual hypotheses - to pre-existent concepts and hypotheses in existing literature. No comparison will be made to literature where conceptual relatedness cannot be found. In a sense, the compared literature is seen as new "data" to constantly compare to the emergent theory. It is seen as new "data" that may modify or refine the theory, as new "data" that may give new perspectives on the emergent theory and its prospective role

in the literature, as well as "data" that might benefit from a different perspective.

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Theories in Progress Series

The Rediscovery and Resurrection of Bunk Johnson – a Grounded Theory Approach: A case study in jazz historiography

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Abstract

This paper was written in the beginning phase of my transitioning from grounded theory sociologist (Ekins, 1997)[1] to grounded theory musicologist (Ekins, 2010)[2]. In particular, it provides preliminary data for a grounded theory of 'managing authenticity', the core category/basic social process (Glaser, 1978) that has emerged from my ongoing grounded theory work in jazz historiography. It was written whilst I was 'credentialising' (Glaser, 2010) my transition to popular music studies and popular musicology. In consequence, it incorporates many aspects that are inimical to classic grounded theory. As with so much of Straussian and so-called constructivist grounded theory (Bryant and Charmaz, 2007), it roots itself in G.H. Mead and a social constructivist symbolic interactionism – *inter alia,* a legitimising (authenticating) strategy. Moreover, as is typical of this mode of conceptualising, the paper fills the void of inadequate classic grounded theorising with less conceptual theorising and more conceptual description. Nevertheless, the article does introduce a number of categories that 'fit and work', and have 'conceptual grab' (Glaser, 1978; Glaser, 1992). In particular, in terms of my own continuing credentialising as a classic grounded theorist, it sets forth important categories to be integrated into my ongoing work on

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[1] Ekins (1997) consolidates the grounded theory analysis of 'male femaling' set forth in Ekins (1993).

[2] Ekins (2010) sets forth a grounded theory of 'mainstreaming' with reference to traditional jazz, the mainstreaming of authenticity, and the relevant popular music studies literature.

managing authenticity in New Orleans revivalist jazz, namely, 'trailblazing', 'mythologizing', 'debunking', and 'marginalising', in the context of the 'rediscovering' and 'resurrecting' of a jazz pioneer. More specifically, the paper is offered to classic grounded theorists as a contribution to preliminary generic social process analysis in the substantive area of jazz historiography.

Introduction

This article focuses on a highly mediated event in the history of jazz. I conceptualise the 'event' as the 'rediscovery and resurrection' of William Geary 'Bunk' Johnson (c. 1879 [?1889] – 1949), a jazz pioneer more commonly known as Bunk Johnson.

Bunk Johnson was regarded as one of the top New Orleans jazz trumpet players in the period 1905-1915, before the recording of jazz. Between 1915 and the early 1930s, he toured the Southern States in minstrel shows and circuses before retiring from music in the Great Depression years. He settled in New Iberia, Louisiana, where he worked in the rice and sugar fields. He had lost his teeth by 1934 and was unable to play trumpet (Hazeldine and Martyn, 2000). He was 'rediscovered' in 1938 by a group of jazz enthusiasts researching early jazz. Fitted out with new teeth and a new trumpet, he first recorded in 1942. He recorded extensively in New Orleans, San Francisco and New York, between 1942 and 1947, before his death in New Iberia, in 1949. To many writers and enthusiasts, within that tradition of so-called 'authentic' New Orleans jazz which privileged New Orleans as the birthplace of jazz and which privileged those black 'stay at home' (Godbolt, 1989: 13) musicians who did not migrate to Chicago (or New York, or San Francisco) in the early 1920s (Charters, 1963; Stagg and Crump, 1972), the rediscovery and resurrection of Bunk Johnson is, arguably, the most important single event in the history of New Orleans jazz revivalism (Stagg and Crump, 1972). I am particularly concerned with issues relating to how this event has been placed within a particular type of narrative; the issues around unpacking and problematising the event; and alternative modes of historical writing through which histories are constructed.

Theoretically, my standpoint is rooted in a social

constructionist 'sociology of knowledge' approach to jazz historiography. Pivotal to my approach is George Herbert Mead's theory of time and the past (Mead, 1929b; 1932; Maines, Sugue, and Katovich, 1983), namely Mead's view that 'reality is always that of a present' (Mead, 1929b: 235); that all histories are social constructions from the standpoint of the present; and that 'no matter how far we build out from the present, the events that constitute the referents of the past and future always belong to the present' (Maines, Sugue, and Katovich, 1983: 161). As Mead stated 'We speak of the past as final and irrevocable. There is nothing that is less so' (Mead, 1932: 95). Rather, 'the long and short of it is that the past (or some meaningful structure of the past) is as hypothetical as the future' (Mead, 1932: 12). As Mead (1929b: 235) puts it:

> The past which we construct from the standpoint of the new problem of today is based upon continuities which we discover in that which has arisen, and it serves us until the rising novelty of tomorrow necessitates a new history which interprets the new future.

Methodologically, the article is rooted in a sociological grounded theory approach (Glaser and Strauss, 1967; Glaser, 1978) to historiography which seeks to thematize alternative and competing histories, in terms of generic social processes (Glaser, 1978; Prus, 1987). I situate my use of grounded theory methodology within the 'narrative turn' in contemporary social science and cultural studies (Maines, 1993; Maines and Ulmer, 1993; Plummer, 1995; Ekins and King, 2006), which views alternative and/or competing histories in terms of 'stories' with a view to unpacking the stories or narratives researched, with reference *inter alia*, to their origins, developments, and consequences.

Specifically, I argue that that plurality of histories of the rediscovery and resurrection of Bunk Johnson have emerged within one or more of four modes of 'story telling' which I term 'trailblazing', 'mythologising', 'debunking' and 'marginalising'. Trailblazing presupposes a positivist theory and methodology of social science and sees its history as a progressive discovery of 'truth' (Giddens, 1974). Mythologising may usefully be linked to W.I. Thomas's (1928: 572) 'theorem', that 'if men (sic) define situations as real. They are real in their consequences.' Within this narrative mode, what 'counts as

truth' is paramount, without prejudice to its 'actual truth', or, indeed, without prejudice to any view taken on the nature of truth, whether a correspondence theory, coherence theory, 'objective relativist' theory, or whatever (Mead, 1929a; Robischon, 1958). 'Debunking' refers to that mode of jazz history within which Johnson's discovery and resurrection is 'debunked' in regards to Bunk's own story of his role in the history of jazz; in regards to historical accounts which have placed him as central to the early history of the genre; and in regards to Bunk's role as a leading figure in New Orleans jazz revivalism (Feather, 1946; 1959; 1987). Finally, 'marginalising' refers to that mode within which particular histories are ignored or sidelined within dominant historical narratives. For example, Bunk Johnson's discovery and resurrection is largely ignored within the processes of mainstream jazz canonisation consolidated within academic jazz studies as it has emerged since the 1970s (DeVeaux, 1991; Meeder, 2008: 86).

My central argument in terms of popular music studies historiography is that the degree to which this reliability, veracity or partiality of a given source, is problematised varies according to the mode of historical writing. More fundamentally, the meanings of reliability, veracity and partiality in regards to the 'story' of the rediscovery and resurrection of Bunk Johnson are, in large measure, contextual to the mode within which they are considered.

Trailblazing

'Trailblazing' in regards to the rediscovery and resurrection of Bunk Johnson emerged within the turn to earlier jazz traditions that followed various disillusionments with so-called 'swing era' jazz (Schuller, 1992) at the end of the 1930s. One of these emergents – was the so-called New Orleans jazz 'revival'.

Carr, Fairweather and Priestley (1987: 416) define 'revivalism' as 'The conscious return, by a new generation of jazz musicians, to an earlier style or form of jazz'. They continue, most importantly, for our purposes: 'The term is most generally applied to the re-adoption of New Orleans jazz (either in its sophisticated Oliver/Armstrong incarnation or in the more basic styles of George Lewis and Bunk Johnson) by young musicians in the late 1930s in the USA and the early

1940s in Great Britain and Europe.' This definition highlights the dichotomising that took place, within New Orleans jazz revivalism, between enthusiasts of the 'sophisticated' New Orleans migrants to Chicago/New York (such as Oliver, Armstrong and Jelly Roll Morton) and enthusiasts of the 'more basic' New Orleans-based 'stay at home' (Godbolt, 1989: 13) musicians (such as Lewis, Johnson and Jim Robinson).

Revivalism undoubtedly had its nostalgic components. The early advocates of this revival (e.g., Blesh, 1943; 1949) were happy to be referred to as 'purists', for instance. They sought a past 'purity' in jazz which they considered to have been 'contaminated' by swing era jazz. However, as Gendron (1995: 32) puts it:

> These purists were driven not only by nostalgia but by a revulsion toward the swing music industry, which by shamelessly pandering to the mass markets had in their eyes forsaken the principles of "true" jazz. A small spate of sectarian journals appeared on the scene to give vent to these revivalist views and concerns. They set themselves off as the only alternative to the two dominant mainstream jazz journals Downbeat and Metronome, which were altogether beholden to the swing phenomenon'.

Arguably, this formulation supports Pickering and Keightley's (2009: 936) central argument 'that nostalgia can only be properly conceptualized as a contradictory phenomenon, being driven by utopian impulses – the desire for re-enchantment – as well as melancholic responses to disenchantment.'

Put another way, these 'purist' revivalists sought a jazz authenticity that the alleged 'progress' of the swing era had destroyed for them. Elsewhere, I have distinguished the 'authentic' as the 'original', as the 'real thing', the 'non-commercial', the 'sincere', the 'emotionally direct', and the 'pure' (Ekins, 2009). All these components were variously hungered for by the 'purists' and they set the background for the rediscovery and resurrection of Bunk Johnson, and 'explain' the appeal of Bunk, once re-discovered, who for his devotees both embodied and epitomised all these facets of authenticity – more so, than the 'sophisticated' Oliver/Armstrong/Morton incarnation of revivalism.

As Suhor (2010) points out in his review of Bruce Boyd Raeburn's (2009) book *New Orleans Style and the Writing of American Jazz History*, 'What we today call "jazz journalism", "jazz criticism" and "jazz history" began, like jazz itself, as the work of passionate amateurs.' The rediscovery of Bunk occurred during the preliminary research work by 'passionate amateurs' for the seminal pioneering book of jazz scholarship, *Jazzmen* (1939). In particular, the editors of *Jazzmen*, Ramsey and Smith (1939), had engaged fellow jazz enthusiast and record collector William (Bill) Russell to write what became the two chapters on 'New Orleans Music' and 'Louis Armstrong'. Louis Armstrong had told Russell that the really important trumpet player to track down was the man who later turned out to be Bunk Johnson. Bunk, according to Armstrong, 'would know all about the early days of jazz in New Orleans' (Berger, 1994: 88, drawing on information supplied by Eugene Williams). Bunk, it transpired, had given up playing after the loss of both his instrument and his teeth following a bandstand murder fracas in 1931 (Raeburn, 2009: 113). Not to be put off by such difficulties, a group of enthusiasts, of which Russell was to play by far the most important long-term role, first tracked him down (to his home in New Iberia) and then bought him a new horn and new teeth. In due time, these enthusiasts then arranged recording sessions in New Orleans with a band comprised of New Orleans 'stay at homes', several of whom who had played with Bunk in the 1920s and earlier.

However, Bunk's recordings were not the first by these 'stay at home', previously unrecorded elderly jazzmen. Two years before Bunk's first recording in 1942, Hale Broun had come to New Orleans with the intention of recording Bunk, but found that Bunk's teaching commitments in New Iberia – several hours out of town – made it impossible. Instead, Broun recorded the legendary (previously unrecorded) trumpet player Kid Rena with an assembled group of New Orleans-based musicians[3]. It was this recording, in particular, that fired Bill Russell's enthusiasm to record Bunk with a similar band.

Russell's comments on this session provide an excellent

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[3] Now available as 'Prelude to the Revival, Vol. II – Kid Rena, 1940', *American Music*, AMCD-41.

introduction to his mindset at the time. Russell (1942: 28-30; cited in Raeburn, 2009: 12) writes of this Broun session:

> considerable confusion still exists in regard to the question of "authentic," "classic" and even "recreated" New Orleans jazz . . . hot fans who have wondered just how a full New Orleans jazz band, would sound, at last have that opportunity.' (Kid Rena) 'who like Armstrong and Oliver learned the blues from Bunk (led the band) with a lack of precision in ensemble and section playing . . . (as the very essence of) the rough and ready, knock em' down and drag out style of music which we call New Orleans hot jazz . . . Many of us will probably never know what the great King Bolden's band was like, but this album gives us the first chance we've had to hear the nearest thing to it.

When Russell finally got his chance to record Bunk, he used the same trombonist – Jim Robinson – with George Lewis on clarinet. While Rena was well past his prime, prematurely aged through alcoholism and ill health, Bunk – according to Russell – was sublime, particularly in the way his choppy lead trumpet reduced the melody 'to magnify the intensity of the ensemble' (Raeburn, 2009: 126). 'He (Bunk) really simplifies the tune. This is what my teacher Arnold Schoenberg used to call "reducing" a tune. Reduction rather than elaboration of the melody.' Moreover, Bunk's "variations", often subtle, are ingeniously constructed (Russell, 1942; reprinted in Hazeldine and Martyn, 2000: 265).

For Russell, then, not only is Bunk the missing link between Buddy Bolden, who according to the jazz foundation myth led the first jazz band (Marquis, 2005), and Louis Armstrong – the first sophisticated soloist in jazz, but both his trumpet style and his band style are indicative of the earliest forms of 'authentic' early ensemble jazz. Moreover, Bunk is a brilliant proponent of the style – unlike the 'past his prime' Kid Rena. Furthermore, not only was Bunk a very important musician for Russell, he was also an important source of early jazz history. Bunk had a 'phenomenal memory' (Wagner, 1993: 270); he was a prolific letter writer; and he was only too keen to set Russell right on early jazz history and his own role in that history. Indeed, as is evident, from a close reading of *Jazzmen* (1939), the editors Ramsay and Smith were

enormously influenced by Bunk Johnson in their reading of early jazz, just as Bill Russell was. Indeed, the first preliminary page, after the title page, features part of a letter from Bunk to the editors:

> Now here is the list about Jazz Playing. King Bolden and myself were the first men that began playing Jazz in the city of dear old New Orleans . . . and I was with him (Buddy Bolden) and that was between 1895 and 1896 . . . And here is the thing that made King Bolden Band be the First Band that played Jazz . . . so you tell them that Bunk and King Bolden's Band was the first ones that started Jazz in the city or any place else. And now you are able to go now ahead with your Book. (Ramsey and Smith, 1939)

But what of the reliability, validity and partiality of these 'passionate amateur' jazz history writers and their work *Jazzmen*? Fitzgerald (2008: 4-8) traces the evolution of the study of jazz from the 'recreational' to the 'scholarly'. He touches on a central and recurring problem in any study in jazz historiography that arises from the vexed interrelations between the writings of jazz enthusiasts and jazz journalists, on the one hand, and the writings of academically trained cultural studies/popular music studies/jazz studies/American Studies/Black Studies/Afro-American studies writers, on the other. On a stereotypical view, the enthusiast and journalist is not overly preoccupied with many of the virtues held dear by academics – rigour, discipline, peer review, and so on. In particular, the enthusiast and/or journalist rarely problematises the nature of his writings; whereas the academic is trained to do precisely that. We might say that the enthusiast and journalist just 'does' history, whereas the academic is continually reflecting on the nature, reliability, and validity of the 'knowledge' s/he produces, which is the constant companion of his 'doing' history. Or as Thornton (1990: 954) puts it: 'Histories necessarily exclude, but academic histories are in a position to problematise what has been left out.'

In the context of the discovery and resurrection of Bunk Johnson, however, we should be wary of dichotomising the jazz enthusiast and jazz journalist, and the academically trained commentator, too sharply. When he died in 1992, the *Times* of London summarised Bill Russell's contribution to

New Orleans jazz revivalism thus: 'Bill Russell was the single most influential figure in the revival of New Orleans jazz in the 1940s.' As Kukla (1998: 3-4) puts it, drawing on the *Times* obituary: 'Russell . . . had kindled interest in the subject in his thought-provoking contributions to the 1939 book *Jazzmen*. He furthered it by helping to rediscover and later record the pioneer trumpeter Bunk Johnson, and he consolidated it through the series of recordings of other pioneers he made for his American Music record label from 1944-1955'.

Specifically, it was, principally, within the interrelations between Bunk Johnson and Bill Russell that one of the two major strands of New Orleans jazz revivalism emerged – that strand with Bunk (with George Lewis) as figurehead. In particular, Bill Russell's *American Music* label established a collection of recordings which came to define 'authentic' 'old-style' New Orleans jazz. These recordings included the first recordings made by a number of other New Orleans trumpet players, including DeDe Pierce and Kid Thomas Valentine, who were to play central roles in the subsequent history of New Orleans revivalism right up to their deaths, respectively, in 1973 and 1987, and beyond (Ekins, 2006; 2008).

Moreover, Russell, through his various writings and his collection of materials on Bunk established by far the largest collection of Archival material available on Bunk. In the 1950s, he played a major role in the establishment in the Archive of New Orleans Jazz at Tulane University, New Orleans, now the William Ransom Hogan Jazz Archive (Fitzgerald, 2008: 77-91), and later received an honorary doctorate from Tulane University, *inter alia*, in acknowledgement of this. On his death in 1992, the bulk of his vast jazz collection, including, his material on Bunk Johnson, passed to The Historic New Orleans Collection, where it is housed in Williams Research Center and provides the finest single collection of Bunk Johnson materials available to the present day researcher.

Thus, although in one sense Russell and his colleagues were 'recreational', they were also 'semi-academic', 'amateur scholars' (Fitzgerald, 2008: 4-8). Most of them, including Russell, had academic training in other disciplines. Perhaps, the best way of conceptualising the early enthusiasts of Bunk is to say they followed the approach Stebbins (1982; 2007)

refers to as 'serious leisure'. In particular, 'They transferred the rigorous methodology of their formal training to their non-professional "hobby"' (Fitzgerald, 2008: 7). Moreover, as a recognised profession of 'jazz studies' with its own university degrees evolved, these pioneering 'amateurs' frequently took up important roles within the profession.

Bill Russell (1905-1992) was a particularly good example of this. He was, initially, trained as a violinist and composer, gaining a diploma in violin, music history, and music theory from the Quincy Conservatory of Music, in addition to his Music and Education degree from the University of California. He studied with Arnold Schoenberg in California; and with Ludwig Becker and Max Pilzer, respectively concert-masters of the Chicago Symphony and New York Philharmonic (Kukla, 1998: 4). By the early 1930s he had emerged as a composer of avant-garde music for percussion instruments. Soon afterwards, however, his fascination for jazz – particularly, of its important for American musical and cultural history – began to override all his other interests. When he first heard Bunk's playing, the die was cast. From thenceforth, he devoted his life to the study of New Orleans jazz, and to the collection of oral histories and other materials. New Orleans-style was quite simply 'the best music (he) had ever heard'. His highly regarded book on Jelly Roll Morton was published soon after Russell died (Russell, 1999). But his book on 'New Orleans Style' was never finished, and after decades of research on Bunk, his 'Bunk Book' was barely started. Rather it was left to a British jazz enthusiast and expert (Mike Hazeldine), in collaboration with an unscholarly musician and expert (Barry Martyn) to write Bill Russell's books for him (Hazeldine, 1993; Hazeldine and Martyn, 2000; Russell, 1994).

Once qualitative research methods became an established part of academic social science – whether in social anthropology or sociology – every elementary research methods text book warned against the dangers of being too heavily reliant on one 'key informant'. Moreover, with the advantage of hindsight, it is easy to note the tendency of jazz musicians (amongst others) to construct histories around themselves. Arguably, too, Russell became somewhat infatuated with Bunk. However, Russell was an extraordinarily meticulous researcher; he had a 'phenomenal memory' (Wagner, 1993: 270); his oral history methods were

in advance of his time; and he was constantly cross-checking 'facts' with his other sources. Like so many research pioneers, he cannot be criticised for not following standard methods procedures because there were none (cf. Welburn, 1986; Porter, 1988).

However, two 'facts', particularly, have come to haunt Russell, as we shall see. In the first place, subsequent scholarship was to argue that Bunk lied about his age (see 'Debunking' below). This 'story' says Bunk was ten years younger than he claimed. Thus to argue as Russell, and his coterie did, that Bunk was the missing link between Buddy Bolden and Louis Armstrong was, so many critics said 'bad history'. It was 'bunk'. Bunk would have been too young to play with Bolden. In the second place, as jazz scholarship became more sophisticated, increasingly more sophisticated theories of the origins and development of jazz were constructed. For instance, *Jazzmen* had rooted the origins of jazz in the 'story' of the importance of the New Orleans 'Congo Square' slave meetings as being vital in providing the rhythmic foundations of jazz. Again, it rooted its 'story' of the development of jazz, in the migration to Chicago, seen as following the closure of Storyville – the Red Light district of New Orleans – in 1917. Both of these 'theories' have subsequently come under heavy attack (see, especially, Collins, 1996). Again, much of the structure of *Jazzmen* lays down the beginnings of a jazz canonisation, which contemporary theorists might wish to render problematic.

However, on balance and in its own terms, a convincing case can be made that 'Even without an "institutionalized means of validating adequacy of training and competence of trained individuals," (Russell's) work met all the accepted standards of the definition of the professional (Stebbins, 1977)' (Fitzgerald, 2008: 7-8). Even today, many of the leading writers on jazz have no directly relevant formal training. Moreover, as McDonald (2006) point out, jazz is still under-represented within popular music studies and canons of popular music. Certainly, there is minimal work on New Orleans jazz revivalism in cultural studies and/or popular music studies, and what there is tends to focus on the British experience (Goodey, 1968; McKay, 2003; 2004; 2005; Moore, 2007).

Significantly, in a series of interviews two years before he

died, Russell convincingly defended his earlier views on Bunk, notwithstanding the subsequent critiques both of Russell's jazz scholarship and Russell's allegedly inflated view of Bunk's musical abilities and Bunk's importance in the history of jazz[4] (Russell and Pointon, 1992).

Mythologising

'Trailblazing', as a mode of historical writing, purports to discover 'truths'. Thus Russell thought he had made a major rediscovery with his written and recorded work with Bunk. Many writers still maintain that his contributions to *Jazzmen*, although in need of refining, still set many of the parameters of jazz history today (Hazeldine, 1993: xi). These early jazz 'historian' pioneers may not have problematised the reliability, veracity, or partiality of a given source, in the fundamental way, presupposed by critiques of positivism in the social sciences, but within their broadly 'positivist' view of the world they did in regards to the 'facts' they unearthed. In regards to their aesthetic judgements as to the value of the music they commented upon, they would – had they been reflective – presumably have followed the positivist distinction between statements of fact and statements of value, and considered they were making value judgements, subject to opinion and debate.

When we move to the historical mode of writing I term 'Mythologising', however, we enter a different terrain in regards to reliability, veracity and partiality of sources. Here, we might say, the 'value' (the 'meaning') accorded the

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[4] This BBC Radio 3 series 'Bunk and Bill' had its origins in a long interview Mike Pointon and Ray Smith did with Bill Russell in 1990, some of which was used for the radio program. According to Pointon (personal communication) Russell 'reluctantly agreed that it might lead to a book if transcribed'. Subsequently, Pointon and Smith have added material on Russell and a book is forthcoming, probably, this year (2010). Pointon is a jazz musician and prolific jazz writer. He is self-taught as an oral historian. In my email interview work with Pointon, supplementing my face-to-face interviews, I had written: 'Is it possible to have copies of your Bunk programs? My assignment is in "historiography" – "The Discovery and Resurrection of Bunk Johnson"'! As, is the way with these academic courses, it will be a question of setting out the various "narratives" on this - NOT coming out with some "truth"!' Significantly, Pointon replies: 'I warn you, though - it IS the truth, at least from Russell's perspective!' Original letters of the correspondence from Pointon to Russell on this program are included in the Bill Russell Collection, Williams Research Center, New Orleans.

rediscovery and resurrection of Bunk Johnson, subsumes the 'truth' 'value' of that rediscovery's component 'facts'. The emphasis now becomes the W.I. Thomas 'theorem': 'If men (sic) define situations as real, they are real in their consequences'. On the pragmatic theory of truth (neither a correspondence theory nor a coherence theory, but an 'objective relativist' theory), 'fact' and 'value' are interrelated in complex empirical ways, and – except analytically – are inseparable (Mead, 1929a; Joas, 1993). In the present context, however, it is sufficient to argue that it was the very 'mythologizing' of Bunk's rediscovery and resurrection that enabled its impact to be so great within what became a world-wide New Orleans jazz revivalism movement, that continues to this day.

Mythologising takes two major modes: those that I consider in terms of 'romanticising' and 'splitting', respectively. The former is a not uncommon mode of historical writing. For the latter, I draw upon psychoanalytic formulations of splitting (e.g., Freud, 1940).

The romanticising 'mythologising' story is built upon a mythologized Bunk. It also built upon a mythologised English trumpet player Ken Colyer, who became the undisputed leader of a 'purist' Bunk orientated sect within British New Orleans jazz revivalism (Melly, 1984). After first hearing the Bunk Johnson records that filtered through to England in the mid-late 1940s, Colyer devoted his life to a career which sought to popularise 'authentic New Orleans jazz', by which he meant, primarily, the music of Bunk Johnson and the other 'stay at homes' who he heard and, in some cases, played with and recorded during his trip to New Orleans in 1952/1953 (Colyer, 1952/1953; 1970; 2009).

Bunk was mythologised by his worldwide devotees for the more hagiographic writings about him, which stressed the circumstances of his rediscovery, his new set of teeth, his alleged 'petulance', his heavy drinking (Sonnier, 1977: 14) and so on. In the same vein, Ken Colyer was mythologised (romanticised) for his dramatic pioneering pilgrimage to New Orleans, his surly character and his heavy drinking. Colyer joined the merchant navy and waited until he got to a port near New Orleans, at Mobile, Alabama, whereupon he jumped ship and headed for New Orleans on a Greyhound bus. After several months in New Orleans, he was incarcerated in the

New Orleans parish prison for overstaying his visa and/or, according some accounts, playing with black musicians in a segregated New Orleans (Colyer, 1952/1953; 1970; 2009; Pointon and Smith, forthcoming).

Both Bunk and Ken Colyer were lionised by their devotees. In Britain, the formation of a Bunk Johnson Appreciation Society, in the late 1950s, subsequently re-named the New Orleans Jazz Appreciation Society, in the early 1960s, was the stomping ground of many of those New Orleans-style English musicians, discographers, record producers and writers who came to prominence from the 1960s onwards (Pointon and Martyn, 2010: 3; Burns, 2007). This pioneering work was subsequently developed in Sweden by the Bunk Johnson Society. These developments, in collaboration with American enthusiasts, have led to the institution of a Bunk Johnson Festival in Bunk's home town of New Iberia. Moreover, there is now a Bunk Johnson Collection housed in a special room in the New Iberia Parish library; a named 'Bunk Johnson Park' in New Iberia; and the beginnings of a Bunk Johnson Collection in the Bayou Teche Museum, New Iberia. Many members of this jazz sub-world attempt serious scholarship themselves about Bunk and jazz (more Trailblazing). However, even in these cases, it was often the mythologizing that remained the anchor and inspiration of their endeavours. More often, most enthusiasts rest content with the mythologizing.

The proponents of early revivalism split themselves off sharply from the 'modern' bebop movement that was the other major response to the disillusionment with the 'swing era' in the late 1930s and early 1940s. This gave rise to the so-called 'jazz wars' of 1942-1946 – first, between adherents of jazz and swing and later between devotees of earlier jazz and bebop (Gendron, 1995). Soon afterwards, as we have seen, the revivalist movement split into two, those following Oliver/Armstrong/Morton and those following, principally, Lewis and Johnson.

A second 'jazz wars' then developed, particularly in England, between what for a while were called the 'traditionalists' following Ken Colyer vs. the 'revivalists' following the revivalist tradition begun in San Francisco by Lu Watters, in Australia by Graeme Bell, and in England by George Webb, in the 1940s and 1950s. In the 1960s, the

terminology changed. The dispute was now between those who favoured 'classic jazz' (Oliver/Armstrong/Morton) and what became known as 'Contemporary New Orleans Jazz', following the 'second wave revivalism' that developed from the early 1960s (Bissonette, 1992) onwards, in New Orleans, following, *inter alia*, the opening of Preservation Hall, where musicians in the Bunk/Lewis tradition played, including Lewis, himself, until his death in 1968 (Carter, 1991).

In point of fact, none of the earlier trailblazers who had championed Bunk Johnson had made the split in revivalism as sharply as many of the world-wide jazz traditionalists/revivalists did and continue to do[5]. Here, we might say, there was yet more mythologizing. Moreover, the trailblazers, themselves – particularly Bill Russell – became increasingly mythologised. Note, for instance (Hazeldine, 1993: ix):

> It is impossible to over-estimate Bill Russell's importance to New Orleans music. As a research and historian he was without question *the* authority. He not only started out searching out pioneer jazz musicians before anyone else, but knew them all better. Over the years he interviewed and collected material on more New Orleans musicians than all the other researcher and writers put together.

> Without his insight and single-minded dedication, Bunk would have remained and been forgotten in New Iberia and the recordings that are the very foundation of our understanding of this music would not have been made. Without his leadership and inspiration, two generations of jazz research and the rediscovery of hundreds of New Orleans musicians would never have happened. Tulane Jazz Archives would not exist, nor would Preservation Hall and scores of jazz record labels that have continued his pioneering work.

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[5] For example, the drummer who most interested Bill Russell was Warren 'Baby' Dodds. Baby Dodds played on both the 'classic' Louis Armstrong Hot 5's and Hot 7's of the 1920s and on almost all of Bill Russell's Bunk Johnson recordings. Indeed, Russell issued three volumes of his *American Music* LPs featuring Baby Dodds soloing and talking about his drumming. In the same vein, Russell spent a large part of his professional life writing his book on Jelly Roll Morton.

Debunking

All the full length studies of Bunk variously either refine ('refining') the trailblazing or the mythologizing (Hillman 1988; Sonnier, 1977; Hazeldine and Martyn, 2000). An early paper by Berger (1947), written at the height of the 'purism' and 'cultism' surrounding the initial rediscovery and resurrection of Bunk, set the standards for the story which critiques Russell's research methodologies, in the service of debunking Bunk. However, most 'debunking' is ritualistic, either as part of a critique or 'rubbishing' of revivalism, generally – as in the 'jazz wars' of 1942-1946 (Gendron, 1995); or of the Johnson/Lewis variant for being especially weak musically – as in many of the record reviews, before an alternative revivalist jazz canon was constructed (see below – 'Marginalising').

The butt of the 'rubbishing' is variously Bunk's lying about jazz history and his place in it; and the weakness of the music he produced during his resurrection. On one view, he was an old, worn out man, when he was recorded, however good he may have been in his day. On another view, there is no particular evidence to suggest either he was ever amongst the top ranks of jazz trumpet players, or that his place in early jazz history should be accorded any particular importance.

Mostly, these views are asserted, rather than argued for. His worth is simply dismissed ('dismissing'). I will leave for another time detailed consideration of this 'debunking', and dwell rather on the more important points made by Berger (1947) which set the standards for subsequent debunking.

Berger finds the 'cultish activity' surrounding the rediscovery of Bunk to be 'offensive'. He argues that the authors of *Jazzmen* were led astray by Bunk's self-aggrandisement – 'the chroniclers of jazz must exercise more selectivity if there is to be any trustworthy historical literature on the subject' (Berger, 1947: 96). In particular, he asserts that it was 'in accepting Bunk's word so uncritically that some of the writers of *Jazzmen* made their greatest error' (p. 96), before going on to argue the errors in Bunk's account according to his own interview material with other musicians. Relatedly, Berger argues that other musicians have legitimate claims to importance in jazz history, which the emphasis on Bunk tends to conceal, before concluding: 'The superlative-

minded jazz connoisseurs, always pointing out the "best" this or the "first" that, inevitably tend to diminish the stature of other great players, both Negro and white. The jazz community can do with fewer ready-made judgements from oracles about who is the *best* cornetist or the *best* trombonist, or the first collector to hold Bunk's mouthpiece or see his teeth in the glass on the window sill' (p. 99).

Other challenges to Bunk's veracity appeared later in Gushee (1987), for example. But it was the scholarly Marquis's 'trailblazing' research on Buddy Bolden (Marquis, 2005) whose detailed consideration of the evidence on Bunk's birth date , placed Bunk's birth date ten years earlier than he claimed, thus making it impossible for him to have played with Bolden. This may well become the accepted view in early jazz scholarship. Interestingly, some Bunk devotees are now beginning to reconstruct their own histories of Bunk arguing 'Does it really matter (his birth date)? It takes nothing away from a remarkable musician and a charismatic personality' Hazeldine and Martyn, 2000: 19).

Marginalising

In his 1942 *Jazz Quarterly* article extolling the genius of Bunk, as an 'old-style' New Orleans lead trumpet player, Russell had written: 'naturally almost every sin known to European musical culture is committed – lack of precision, out of tunefulness, smears, muffs – in other words we have with us once again the well known "sloppy New Orleans ensemble" – but an ensemble whose unpredictable rhythms, vitalizing accents, and independence of parts (even when playing isometrically) are more thrilling than any symphonic group' (Russell, 1942, reprinted in Hazeldine and Martyn, 2000: 265).

These 'sins' detailed by Russell played a large part in Bunk's marginalisation in the history of jazz canonisation. No amount of emphasis upon 'expressive', 'from the heart' playing taking priority over sophisticated or even adequate technique affected this marginalisation. Moreover, jazz canonisation in academic jazz studies favours an evolutionary story of 'progress'. Bunk could be rejected as a candidate for such canonisation. In consequence, Bunk is either ignored entirely, or his role is marginalised as outside the mainstream of jazz history. Often, this marginalising is of the entire

revivalist movement. In Townsend (2000), for instance, a 194 page book with an embracive intent – *Jazz in American Culture* – the 'revivalist' movement is afforded a mere seventeen line paragraph with no mention of any actual revivalist musician. Meeder's (2008: 86) treatment is illustrative of exemplary marginalising of Bunk, when Bunk is mentioned in such 'jazz studies' texts. Significantly, Meeder is a graduate student of the prestigious Masters' programme in Jazz History and Research at Rutgers University. In his chapter on 'Bebop and Moldy Figs', he writes:

> While critics squabbled, older musicians enjoyed increased attention and reinvigorated careers. Louis Armstrong returned to small New Orleans style combos in 1947, and made some of his best recordings in fifteen years . . . Sidney Bechet also made a comeback . . . In 1945, Bechet formed a working group with trumpeter Bunk Johnson which did not last long but began a resurgence of interest in both players. Johnson, who was born in New Orleans in 1889, became a sort of fetish for jazz purists. Because of his age (he had claimed to be ten years older than he was), and the false claim that he had taught Louis Armstrong, Johnson was lauded by many as the authentic voice of jazz. No mind was paid to the fact that his playing did not really resemble recordings of the New Orleans musicians from the 1920s. On the contrary, his quiet, simple approach to playing (and his choice of repertoire, since he did not shy away from popular tunes of the 1940s) indicates that Johnson was part of a scene of New Orleans musicians who had developed their own tradition of music apart from the mainstream of jazz.

Thus do 'marginalising' stories deal with all the trailblazing and mythologizing work on Bunk that I have detailed! Not surprisingly, perhaps, the trailblazers and mythologisers, themselves, largely ignore such work and continue with the development of their own alternative canon, set forth in the core texts of Ramsey and Smith (1939); Charters (1962); and Stagg and Crump (1972), for instance. Developments include such work as Bethell (1971); Carter (1991); Turner (1994), to say nothing of the myriad of articles in such core alternative canon enthusiast's magazines as *New Orleans Music*, and similarly inspired publications and internet sources,

comprising an 'alternative canonic hub' (Karja, 2006) of Bunk inspired revivalism.

Towards a Conclusion

I have been concerned to situate the rediscovery and resurrection of Bunk Johnson within a popular music history studies and cultural studies framework. It must be said that no detailed study of Bunk exists within these frameworks. Indeed, as McDonald (2006: 125) makes explicit there is a need to combat 'the under-representation of jazz within popular music studies and canons of popular music', more generally. In these regards, this essay is a pioneering and innovative work. In particular, I have situated my treatment of Bunk within a theoretical framework drawing on G.H. Mead's theory of the time, the past and history, and a methodology rooted in grounded theory. G.H. Mead's work is pivotal to much sociological (symbolic interactionist) social constructionism, but the approach is under-utilized and under-explored in popular music studies and cultural studies, and, indeed, in the related fields of Jazz Studies, Black Studies, African-American Studies, and so on. In particular, Mead's theory of the past is especially under-utilised and under-explored in popular music history, cultural theory and in the social sciences, generally. For the purposes of this article I have simply presupposed the approach. Further work would need to develop the approach's implications and its similarities and dissimilarities with other approaches that are or might be utilised in popular music history studies.

Because of the unfamiliarity of my chosen substantive area in popular music studies history, I chose to elaborate the historical modes of 'trailblazing' and 'mythologizing' at the expense of a more detailed treatment of 'debunking' and 'marginalising'. In any event, however, detailed questions of reliability, veracity or partiality of a given source do not really arise in the debunking and marginalising modes, because neither mode takes either the trailblazing or mythologizing stories seriously. Indeed, they do not take the rediscovery and resurrection of Bunk Johnson seriously! Nevertheless, further work on the marginalising mode would profit from the sort of sophisticated conceptual and analytical distinctions made on canonic discourse by Everist (2001) and Weber (2001).

Finally, to conclude on the question: What has been missed out of the various accounts we have considered? From the grounded theory approach of this article, story telling within each of the four narrative modes considered tends to ignore (miss out) aspects that the other modes highlight. To go further than this would be to argue for an essentialism or 'real' past historical 'truth' that Mead's theory of the past belies. For Mead, and, in consequence for this essay, reality is always rooted in the present and with each new present the possibilities of new pasts will arise.

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A Commentary on Ekins (2011)

Hans Thulesius, MD, Ph.D.

According to the author, Richard Ekins' case study on jazz history "provides preliminary data for a grounded theory of *managing authenticity*". The paper is well written, entertaining to read and I can recognise the style from Ekins' paper male femaling[1], one of my favourite GTs. The four concepts of trailblazing, mythologising, debunking and marginalising are catchy, interesting and make sense. Ekins says he has not done classic grounded theory but a conceptual description influenced by constructivist approaches. This is probably one explanation to why the main concepts in the paper are not tied together in a recognizable theoretical coding pattern. At least not to me. And the reason may be that the author has some more memo-sorting to do. This is an important part of classic GT, but often left out. By hand sorting memos, the integration of concepts to each other and to the core variable is improved since it stimulates writing of memos on memos. As such, the theoretical coding of the theory is stimulated - how concepts relate to each other as a typology, process, or any other possible conceptual constellation that explains what is going on in the substantive area.

Another way of expanding the theory could be through a literature search for the core variable and the four concepts of trailblazing, mythologising, debunking and marginalising. To compare literature data in the diverse fields where these concepts appear would provide more data to be compared and eventually make the theory more mature.

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[1] Ekins, R. (1993). On Male Femaling: A grounded theory approach to coross-dressing and sex changing. *Sociological Review*, 1(1), pp.1-29.

A Commentary on Ekins (2011)

Vivian B. Martin, Ph.D.

Early in his article Richard Ekins concedes that the work before us is "inimical" to classic grounded theory. Unlike many who fly the flag of grounded theory, Ekins, the author of a well-regarded study of what he calls male femaling, or male cross dressers, is a student of grounded theory, but it appears that in his latest phase of research, jazz historiography, he is grappling with combining grounded theory with some of the strictures of his new substantive area. He offers what is in effect a case study of the rediscovery of New Orleans jazz pioneer William Geary "Bunk"Johnson, as an exploration in "managing authenticity," his core category. I will discuss some basic classic grounded theory breaches he might reconsider, forgoing constant comparison and adopting existing theories among them, as well as the challenge of navigating a grounded theory study that is faithful to classic strictures but also aligned with a discipline's norms. I briefly address the challenge of conceptualizing and transcending data in a field, jazz historiography, where excessive description is one of the ways in which authenticity is achieved.

The subtitle of Ekins' paper, which invokes the "case study," alerts us to the fact that there is some method-mixing under way, and that usually does not augur well for classic grounded theory. Case studies, like many other methods, are useful for certain types of research. But one has to decide which method works best for the subject at hand. Ekins has essentially produced a case study with a few grabby concepts; there is nothing close to a theory with integrated concepts here, despite the claim of a core category. Managing authenticity, which is what Ekins has identified as his core category, mainly works as a conceptual label here rather than a core of a theory. This outcome is the result of how Ekins proceeded with his work. Case studies, at least the single case study as Ekins has executed it, come loaded with assumptions and preconceived concepts largely because they have been chosen specifically because of what they are perceived to contain. Ekins sought to study the resurgence of

a near-forgotten early jazz trumpeter and has produced an article about the four different ways in which Bunk Johnson's place in jazz history is narrativized over time. The four modes are: trailblazing, mythologizing, debunking and marginalizing. These concepts are Ekins' contribution to the discourse around Bunk Johnson and his place in jazz history. Ekins' work starts with this resurgence as a foregone conclusion and then proceeds to give conceptual labels to the highly descriptive story that unfolds.

Ekins has not produced a classic grounded theory—nor does he claim to have done so. He aligns himself with symbolic interactionist and constructivist perspectives and has situated his work in these perspectives and influences such as George H. Mead's theory of the past, a priori commitments very much counter to classic grounded theory. In starting with extant literature, any theory-building he would have hoped for is held hostage to the built in assumptions he has imported.

Working a pre-formed agenda is just the first falling domino here. Case studies, as the well-regarded collection edited by Charles Ragin and Howard Becker (*What is a Case, 1992*) discusses, can take many forms. A researcher can have hundreds of cases or just one. But that's not how ground theory works. At the heart of grounded theory is constant comparison, something not so easily accomplished with one case, excruciating details aside. To take his work further, Ekins needs to delve in and examine other cases of rediscovery and related discourses. This would give many more concepts, including some that might allow him to move beyond what Mead's theorizing accomplished. Of course, this can only be tackled through fidelity to the process of open coding, memoing, theoretical sampling, selective coding, theoretical coding, and sorting. His current, albeit tentative core concept, of managing authenticity had not veered far from the "Managing, "constructing," or "negotiating" core concepts so common in grounded theories (authenticating might work a little better here).

An important thing to say here is that I really enjoyed reading Ekins' article. It is good scholarship, and within the fields he invokes, popular music studies, cultural studies, jazz historiography, much of what he has done here would probably be admired. For a classic grounded theorist, though,

the work raises another important issue: the challenge of extending classic grounded theory, with its insistence on high conceptualization over description, to fields where description of evidence is highly valued. We speak of altering "illustration dosage" as a way to please reviewers, and many of us have thrown in the extra quotes or examples to win the publication nod. But historic works contain much elaboration of detail in order to convince readers the writer has the goods. Footnotes can handle some of the data, but the argument is built through description and detail, much like the explication in Ekins piece. The details make it difficult to see beyond the specific situation.

The solution would seem to be to commit to classic grounded theory fully (as paradoxical as that may sound). Bunk Johnson's case is really just the open-coding phase with ideas to test. A skillful execution of constant comparison across a number of cases as a form of theoretical sampling would ultimately build a theory dense and impressive enough to transcend disciplinary worries because the developed theory would truly be unique and add a contribution in areas where Ekins has identified a void. The concepts and the supporting indicators can easily be illustrated through the kind of matrices and tables that are convincing, or at least provide a credible argument, even for historians. Constant comparison would be the not-so-secret weapon.

I hope the author forgives any presumptions on my part. It may well be that by invoking more constructivist approaches he has written off classic grounded theory. That would be too bad. A little bit of grounded theory goes a long way, but only classic grounded theory, faithfully applied, can identify the intertwined patterns waiting to be discovered and turn them into a fully integrated theory.

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Response to Hans Thulesius and Vivian B. Martin on Ekins (2011)

Richard Ekins, Ph.D., FRSA

I appreciate the valuable comments from Hans Thulesius and Vivian Martin. Both reviewers usefully and most helpfully pinpoint salient issues to be considered in forging future routes for my ongoing research in jazz historiography. I agree with Vivian Martin that 'authenticating' works better for early jazz and first wave USA revivalism (including Bunk Johnson) and would like to say that at the time I wrote the abstract, I was favouring 'managing authenticity' over 'authenticating' as the core category in order to provide a more embracive category within which to consider early jazz, the 1940s New Orleans jazz revival (including Bunk Johnson), and the world-wide New Orleans jazz revivalist movement which continues to this day. I have, indeed, written elsewhere of all the various phases of New Orleans style jazz considered in terms of the core category/basic social process of 'authenticating', e.g., 'Authenticity as Authenticating - The Case of New Orleans Jazz Revivalism: An Approach from Grounded Theory and Social World Analysis' (Ekins, forthcoming).

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Book Review: *Grounded Theory: A practical guide* (Birks & Mills, 2011)[1]

Astrid Gynnild, Ph.D.

What is grounded theory and how is a grounded theory study carried out? Several recent publications have been trying to answer these questions, building on a literature list on the topic that covers more than 40 years of research and scholarship. According to the authors, this book is aimed at beginner researchers, research students and experienced researchers from a variety of disciplines who are unfamiliar with grounded theory.

The first three chapters of the book deal with essentials, planning and quality processes of grounded theory research. Three later chapters deal with data collection, analysis and "theoretical integration", and in the last three chapters the authors provide tips about presenting a grounded theory, evaluation and application of it and how it might be further contextualized.

The book aims to be an easy-to-read volume with learning objectives on top of each chapter, suggested reader activities, examples of memoing, and many figures and tables. So far, so good. The authors have obviously put much work into the overall design of the book to make it as reader-friendly and easily accessible as possible. As experienced teachers in nursing they seem to be aware that providing an introduction to a new field is a challenging task, no matter the topic.

However, as a "Practical Guide" in grounded theory, this book does not resolve any of the hitherto unresolved issues in the split grounded theory domain; rather, the book contributes to even more confusion. In their foreword the authors state that they "hope to demystify some of the complexities associated with grounded theory". Their intent is to provide a balance between engaging in what they call the

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[1] Birks, M. & Mills, J. (2011). *Grounded Theory: A practical guide*. London: Sage Publications.

internal intellectual dialogue that is required and "meeting the practical requirements of undertaking research at a graduate level". Their ultimate goal appears to be of a bridge building character; they want "to provide a balanced view of grounded theory methods, without adopting a dichotomous position", and they add that there is much to be learned "from all antecedent grounded theorists" (p. 3).

Signalling a stance as pragmatic bridge-builders of a scholarly method that has taken off in different directions might initially sound like a good idea, and it might work well with other methods. But it is inherently problematic in a book meant as an introduction to a methodological approach that is based on a thorough grounding of data. To the extent that there might be any propositions presented in this book, they are evidently not the result of a systematic analysis of data. Rather, the stated philosophy of apparent fairness, in this case, seems to be resolved by non-systematic switching between references to Strauss/Corbin, Glaser and Charmaz respectively, and next to a dozen other writers mostly from the Strauss/Corbin and Charmaz section. It appears that the authors have relied heavily on a rather diffuse method of skip-and-dip when collecting data for this book; they have picked a little bit here and a little bit there and created yet another mix which they call essentials of grounded theory methods. At the same time as they recognize that they are talking about different methods, the differences in methodological approaches between these methods are not explicated.

As a consequence, concepts like abstraction and conceptualization, which at least in classic grounded theory are fundamental for the generation of new theory, are hardly mentioned. Theoretical sensitivity is devoted one and a half pages, but without referring to the seminal work, "Theoretical Sensitivity", by Barney Glaser from 1978. The use of gerunds in grounded theory, which was an important issue for grounded theory co-founder Barney Glaser in many of his books, are erroneously traced back to Cathy Charmaz. This just to mention a few examples of lacking respect for, or knowledge about, the use of primary and secondary sources.

To use a term from classic grounded theory terminology: this book is not well grounded in baseline data. On the one hand, the necessity of methodological congruence and

procedural precision is emphasized throughout the book. On the other hand, as exemplified above, problems arise when authors of an introductory methods book themselves are not equally as systematic in their analysis thus, it appears paradoxical when the authors repeatedly state that the quality of a grounded theory study is demonstrated by the rigor in the conduct of one's research.

To clarify my point of departure with this book I would like to explain that my insights in grounded theory do not stem from the Strauss/Corbin or Charmaz branch. I have learned grounded theory from 'ground up' by reading the classical works of Glaser/Strauss and later by being taught grounded theory by Barney Glaser at his seminars and by studying his more than a dozen publications about the methodology. I have generated several grounded theories and know that the classical way of doing grounded theory works, and produces a theory that works, fits and is relevant. Since most of what is in this book does not stem from Glaser's grounded theorizing, I assume that it comes from other branches of the same root which have also been challenged for the lack of scholarly endeavour.

An important lesson taught in classic grounded theory is that of growing open. Thus, I started reading this book with an open mind, truly curious and excited of what new knowledge it would bring about grounded theory. The cover looked nice and the contents list sparked my curiosity even more. Since I have not conducted any study following the Strauss/Corbin, Charmaz or any other derived grounded theory procedures myself, I was really curious and interested in how that would be carried out. If I were a novice grounded theorist, I would probably have been even more curious to get a good overview of the field - in order to be able select a path that might best fit with my interests and personality.

The dilemma, then, is that this book does not present any new, well integrated approach based on a constant comparative analysis of grounded theory methods, nor does it clarify well what the procedures for existing directions are. A well integrated theorizing of "grounded theory methods" would have filled a void in the literature about grounded theory, and I would have been very happy to read it. But we will probably still have to wait for a while to get that.

What I did find interesting in this book in the end,

though, was the last chapter, which deals with situating grounded theory in the context of current debate. Several issues raised here awaits further analysis and debate.

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Book Review: *Essentials of Accessible Grounded Theory* (Stern & Porr, 2011)[1]

Reviewed by Odis E. Simmons, Ph.D.

Although Porr is a relative newcomer to grounded theory, Stern has been at it for many years (she received her PhD under Glaser and Strauss in 1977). She has been instrumental in introducing many students to grounded theory, particularly in the nursing field, as well as making notable contributions to grounded theory literature. As Stern's (1994) observations and insights suggested, constructivist versions of grounded theory emerged and spread in part because grounded theory was often being taught by teachers who themselves had a superficial, distorted understanding of the methodology, because they had learned it "minus mentor." Given her observations, insights, and writings, when I began reading *Essentials,* my expectations were high. But, after reading it, I concluded that, in some important ways, it falls short. Given Stern's considerable experience and previous contributions to grounded theory, it is ironic that *Essentials* contains more confusing and subtly inaccurate content than a book written for neophyte grounded theorists should. Although I think it is a noble effort with useful information, it contains material that is at variance with classic grounded theory, yet this isn't made clear to the reader. Because Stern and Porr failed to make a clear distinction between classic and other forms of grounded theory, many readers, particularly neophytes, will of course expect that what they present in this book accurately represents essential canons of all types of grounded theory, including classic. Readers will carry the understandings and misunderstandings gained from the book into their research and discussions with other neophytes and individuals who express interest in grounded theory.

As Stern (1994) herself pointed out, grounded theory has been "eroded" over the years. This erosion has led to the distinction pointed out by Charmaz (2000, 2006) between

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[1] Stern, P.N. & Porr, C.J. (2011). *Essentials of Accessible Grounded Theory.* Walnut Creek, CA: Left Coast Press.

"classic" or "Glaserian" grounded theory versus "constructivist" grounded theory. Any book about grounded theory, particularly an introductory book that purports to be about the essentials of grounded theory, should begin by clarifying this important distinction, lest it not contribute to more erosion of the methodology. Stern and Porr neglected to make this distinction clear, which begets potential misconceptions throughout the book.

In Chapter 1, they use the general term "grounded theory" without clarifying whether they intended for the book to be about classic or other versions of grounded theory. The following quote suggests that they maybe meant for the book to be an introduction to classic grounded theory, because it is in these two books that the fundamentals of what eventually came to be termed "classic" or "Glaserian" grounded theory, are laid out.

> In this book we drawn primarily from *Discovery of Grounded Theory* (Glaser and Strauss, 1967) and *Theoretical Sensitivity* (Glaser, 1978) to lay out, as accurately as possible, essential groundwork and procedures for formulating explanatory theory (p.37)

However, in other locations in the book they legitimize and even encourage actions that are clearly outside the boundaries of classic grounded theory. For example, in Chapter Two, "Brief History of the World (of Science)," in their section titled, "Your Theoretical Lens" (pp. 30-33), they discuss, legitimize and encourage importing "theoretical lenses" and "explicit interpretive frameworks." They provide examples, such as Wuest's (1995) proposal that grounded theorists "can attach a feminist epistemological framework to grounded theory in an effort to privilege the voices of women," Kushner and Morrow's (2003) recommendation for constructing a framework consisting of feminist teachings combined with critical theory, "in order to adequately sensitize grounded theorists to issues related to alienation, power and domination," as well as their own research in which they say they used symbolic interactionism as their theoretical foundation and interpretive framework. Stern stated that she used an "eclectic" theoretical lens, combining symbolic interactionism, family dynamics and therapy, and dramaturgy. Using imported, preconceived theoretical lenses is proscribed in classic grounded theory, which suggests that

they may have intended for the book to be about constructivist grounded theory.

In my view, the main problem with the book is the authors' failure to inform readers of the critical distinctions between classic and constructivist versions of grounded theory. This theme appeared throughout my review because most of the other problems I see in the book could have been avoided, or at least mitigated, if they had made this distinction clear and informed the reader about the critical differences so that they could make an informed choice as to which version of grounded theory they wanted to pursue. This slurring[i2] of classic and constructivist grounded theories will be misleading to all but the most informed readers, particularly neophyte readers. Readers who are uniformed of the differences between classic and constructivist versions of grounded theory will carry these misconceptions through the book and beyond, contributing even more to the erosion of grounded theory. It does a disservice to classic grounded theory to not clarify this critical distinction, at the outset of the book. Given that this was the original grounded theory, this is a major oversight.

It also made it difficult for me to know what methodological principals to use in judging the veracity and accuracy of the book. Although there is overlap, methodological principles are not uniform across the various forms of constructivist grounded theory and certainly not between classic and constructivist grounded theory. However, the principals of classic grounded theory have been clearly established and articulated, initially by Glaser and Strauss in *Discovery*, and many times since in Glaser's myriad grounded theory related books. So as not to contribute more to the erosion and slurring of classic grounded theory and because the principals of classic grounded theory are well laid out, I decided to judge the book from the perspective of classic grounded theory.

Thus, the primary aim of my review was to assess the extent to which what the authors present is consistent with and clear in its portrayal of classic grounded theory, rather

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[2] It is a bit ironic that Stern was co-author of an article discussing "methodological slurring" between grounded theory and phenomenology (Baker, Wuest, & Stern, 1992), yet *Essentials* commits the same transgression in relation to classic and constructivist grounded theories.

than the variations on grounded theory that evolved after *Discovery* and *Theoretical Sensitivity*. Had they made and adequately discussed the clear distinctions between classic and other versions of grounded theory I could have proceeded differently.

In particular, I looked at whether or not I think that the book will be useful to neophytes, because they would be the ones to whom it would be most essential. If neophytes find it useful others should be able to find it useful as well. I received my Ph.D. under Glaser and Strauss in 1974. Beginning in my grad school years, I have about forty years of experience teaching classic grounded theory (hereafter referred to simply as grounded theory, unless otherwise specified) to neophytes from academic as well as professional fields as well as supervising numerous grounded theory theses and dissertations. My substantial experience at helping neophytes understand and conduct grounded theory has given me a good sense of the difficulties and struggles that they experience in understanding and learning to conduct grounded theory and what it takes to overcome them. This made it easy for me to assess the extent to which I think the book will serve as a useful introduction to grounded theory.

In Chapter 1, " Why This Book?" Stern and Porr address their purposes in writing *Essentials of Accessible Grounded Theory* (hereafter referred to as *Essentials*).

> As we see it, the published literature is written for the informed rather than the uninformed; translation: the language has tended to be what Phyllis calls sociologese rather than Standard English. The esoteric terminology has caused 2 problems: a) non-sociologists failing to grasp the jargon of the original text make up their own version of grounded theory or b) professionals, novice researchers and students alike attempting to understand this social science research approach throw up their hands in frustration...We have written this monograph in what we hope is a lucid, concise and accessible format in an effort to clear up some of the mystery and confusion surrounding grounded theory. Essentials of Accessible Grounded Theory will serve as a compass for trans-disciplinary undergraduate and graduate

students, neophyte researchers or institutional and
community based experienced researchers wanting to
conduct inductive qualitative research to generate
theoretical explanation about a concern, issue or
situation involving human phenomena. (pp. 13-14)

Unfortunately, beginning with the above introductory
selection and continuing throughout the book, their
discussion frequently lacks the clarity that would have been
afforded by making a clear distinctions between classic and
constructivist grounded theories.

Furthermore, in my view, ironically, their disregard for
grounded theory jargon adds to the confusion that they
purport to be clearing up. In my forty years of teaching
grounded theory I have found it to be very important for
students to become familiar with the jargon early in their
learning process. Once the jargon is understood, it provides a
language and means of cognitively imaging the components of
the methodology and how they work together to generate a
grounded theory, engendering sustained and enhanced
understanding as they conduct their actual research. It helps
neophytes understand what they are doing, why they are
doing it, what to do next and how to do it. It also provides a
common language for students to share their understandings
with each other, which serves an important learning function.

Yes, it can be initially difficult to grasp for some
learners, but I have found over and over that it pays off
because it engenders a deeper, lasting understanding of the
methodology that enables learners to become independent
grounded theorists and carry their skills forward into their
careers, including teaching others grounded theory. Without
the advantages of grounded theory jargon, neophytes
understanding of grounded theory is superficial and limited.
One of my students recently telephoned me and said, "I'm
worried about myself." I replied with some concern, "Oh, why
is that?" She answered with a smile in her voice, "Because
now when I read Glaser I *understand* him!" Our ensuing
conversation made it clear that struggling with the jargon
until she "got it" was very valuable, frustrating as it was at
times. I can't imagine trying to teach or learn grounded
theory without the jargon. The thought strikes me as being
similar to trying to do a grounded theory without concepts.

Another confusion introduced in the above opening

material comes from the phrase, "researchers wanting to conduct inductive qualitative research." Glaser has made it clear that grounded theory is a general method, not a qualitative method. As Glaser wrote:

> Thus please remember that although grounded theory has captured the imagination and zest of qualitative researchers, that there are many monographs of inductive theory generation--usually published by the Free Press--done with quantitative data and that quantitative methods of data collection and analysis provide most of the underlying methodology models of analysis in grounded theory. (1992, p. 17)

> Grounded theory does well with qualitative data, but it has rightfully no part in the wrestle between quantitative and qualitative…. Grounded theory was not discovered to foster a qualitative ideology. (1998, p. 43)

The authors could have clarified this issue had they included Glaser's distinction between qualitative analysis and qualitative research. As Glaser (1992) wrote,

> It is important to keep the distinction clear between qualitative analysis and qualitative research to forestall confusion…Qualitative analysis means any kind of analysis that produces findings or concepts and hypotheses, as in grounded theory, that are not arrived at by statistical methods. To repeat, qualitative analysis may be done with data arrived at quantitatively or qualitatively or in some combination. (Glaser, 1992, pp. 11-12)

Without this clarification, readers may be left thinking that quantitative data are not appropriate and useful in a grounded theory study. Although, on page 50 they do introduce the Glaser dictum that in grounded theory "all is data" (see e.g. Chapter 11 in Glaser, 2001), and they do include "surveys," they don't mention the word "quantitative" or make it clear that "all is data" includes all forms of quantitative data, despite the fact that Glaser (2008) published an entire book on quantitative grounded theory. If neophytes don't understand this they may place unnecessary limitations on their study.

One of my biggest concerns about *Essentials* is that it

subtly encourages what might be termed *"constructivism light."* An important distinction to make here is that between *fundamental* (unavoidable) versus *intentional* constructivism. Because we necessarily use language, through which meaning is formed and conveyed, fundamental constructivism is unavoidable in both classic and constructivist grounded theories. However, intentional constructivism was designed out of classic grounded theory. To the contrary, it is designed *into* constructivist grounded theories. However, although *Essentials* doesn't overtly encourage extreme forms of intentional constructivism, it unwittingly encourages constructivism light, which can nonetheless derail the full grounding of a theory. The constructivism light I see in *Essentials* occurs because of lack of clarity and seemingly minor departures from tenets and procedures of classic grounded theory, innocent as they may appear. Here again, I see this as problematic primarily because of the authors' failure to clearly distinguish between classic and constructivist grounded theories. Had they made this distinction, at least readers would know that they were not being encouraged to neglect one of the most important canons of classic grounded theory--to be as non-constructivist as possible.

The Role of Constructivism in Grounded Theory

To serve as proper context, it is important to clarify the relationship of grounded theory to constructivism. At its base, the constructivist position is that all meaning is constructed by humans--what Glaser and Strauss referred to as "meaning making." This is in contrast to "objectivism." In the social/behavioral sciences, the objectivist position holds that social reality exists independent of the human mind, or as Durkheim (1938) put it, "society is prior." Although I suppose one could argue otherwise, these two positions are often assumed to be contradictory to one another. There are two categories of constructivism that are relevant to all versions of grounded theory, including classic grounded theory--constructivism related to the people being studied and constructivism related to the researcher.

Participant constructivism

Some authors claim that Glaser is an objectivist, assuming an underlying objective reality, despite much

evidence to the contrary and a paucity of evidence that supports that claim. They appear to infer this from his graduate school studies in sociology at Columbia University, which had a positivist, quantitative methodological bent,[3] as well as his use of the theoretical code "basic social process." In relation to participant constructivism, they ignore such clear statements as:

> GT is a perspective based methodology and people's perspectives vary. And as we showed in "Awareness of Dying" (Glaser & Strauss, 1965) participants have multiple perspectives that are varyingly fateful to their action. Multiple perspectives among participants is often the case and then the GT researcher comes along and raises these perspectives to the abstract level of conceptualization hoping to see the underlying or latent pattern, another perspective. (Glaser, 2002, p. 2).

This and other statements made by Glaser make it clear that he sees grounded theory as being about ongoing behavioral patterns of research participants, including latent patterns, with full recognition that meanings are emergent social constructions. As he states (Glaser, 2002, p. 3), "The constant comparative method discovers the latent pattern in the multiple participant's words..." In other words, the patterns are an outcome of meaning making.

Researcher constructivism

Staunch constructivists maintain that all meaning is a human construction, without exception, a sentiment with which I agree, at the fundamental level. However, it is important to distinguish between *fundamental* constructivism which is universal (and therefore unavoidable) and *intentional* constructivism on the part of the researcher. As I mentioned above, it is important to note and for readers of *Essentials* to understand that intentional constructivism was designed out

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3. Although he was influenced by this, particularly its rigorousness and the ideas of the social statistician, Paul Lazarsfeld, Glaser didn't adopt it; he used it to inform grounded theory. By the same token, the Sociology Department at Columbia was also heavy in speculative theory. He didn't adopt this approach either. His ideas that led to grounded theory were to a great extent a reaction to what he termed the "theoretical capitalism" of that approach to theory. With what he learned and observed at Columbia, he designed a rigorous methodology for generating theory systematically grounded in data that was open to anyone.

of classic grounded theory. To the contrary, it is designed *into* constructivist grounded theories. Of course, *fundamental constructivism* is endemic to both constructivist and classic grounded theories. However, researcher formulated *intentional constructivism* is not an all or nothing category. It comes in gradations.

Just because pure objectivity is unattainable does not mean that as grounded theory researchers we should throw out the baby with the bath water and embrace researcher constructivism. Intentional constructivism may be appropriate for other methodological approaches, but embracing it violates what may be the most central canon of grounded theory, which is to be as non-constructivist as possible and let the theory emerge from the data. For this reason, although I think the term is apropos, I regard constructivist grounded theory as an oxymoron or at best quasi-grounded theory. Glaser (2002) referred to it as a "misnomer."

Another important distinction to make here is the difference between the underlying objectivism of the objectivist position and merely being *conceptually* objective. Glaser's position on conceptual objectivity:

> Let us be clear, researchers are human beings and therefore must to some degree reify data in trying to symbolize it in collecting, reporting and coding the data. In doing so they may impart their personal bias and/or interpretations—ergo this is called constructivist data. But this data is rendered objective to a high degree by most research methods and GT in particular by looking at many cases of the same phenomenon, when jointly collecting and coding data, to correct for bias and to make the data objective. (Glaser, 2002, p. 6)

This is not a claim of pure objectivity; it is merely a statement regarding maximizing objectivity to the extent possible. This is what classic grounded theory was designed to accomplish. Neither Glaser or Strauss ever claimed pure objectivity.

A few examples of discussions in *Essentials* that encourage constructivism light follow. There are many more scattered throughout the text but not enough space here to cover them all.

One example of constructivism light can be found in Stern and Porr's discussion in their "Literature Review" section (pp. 49-50), where in reference to a preliminary, or what they call a "primary" review, they write, "whether grounded theory or one of the other qualitative research methodologies, a search of the relevant literature is not only needed, it's required." Although uninformed dissertation and IRB committees may require it and you respond by gaming the system (see my below discussion about this), doing a preliminary literature review of the relevant literature most certainly is not part of classic grounded theory because you don't yet know what literature is relevant. If committee pressures make it unavoidable, classic grounded theory mentors should at least help students develop the skill of being able to suspend what was derived from a preliminary literature review, serve as honesty brokers when they see preconceptions creeping in, and watch to make sure the student is remaining honest to the data. In most, if not all, of the many classic grounded theory dissertations I have supervised, the data took the research and eventual theory, and thus what literature became relevant, to a place that could not have been imagined at the outset. Preliminary literature reviews could have derailed the natural emergence of the theory, or at least been a waste of time.

Another example of constructivism light can be found in their "Variation" section (p 31-32) in which they stated,

> Grounded theory methodology should encompass data from multiple sources as a way of clarifying and validating the meaning of behaviors. Different slices of data will ensure a proportioned view of participant perspectives as to why people are behaving as they do. The back-and-forth checking rechecking of various viewpoints correct for partiality to any one point of view you will want to grow concepts on several slices of data of all shapes, sizes and colors. For example, including participants representing more than one demographic characteristic or multivariate ethnicity constitutes data diversity.

There appears to be an assumption inherent in this selection that there will be a single "correct" meaning of the behaviors of different participants, rather than multiple meanings and behaviors and that different data sources are required to

clarify and validate it. However, the different meanings and behaviors of participants is *variation*, which is important in generating a grounded theory. One does theoretical sampling to discover variation, but although Stern and Porr included a Chapter (13) on theoretical sampling much later in the book, they don't mention it here which suggests that the above selection is not about theoretical sampling. Who's meaning shall prevail; the researchers? If so, that is researcher constructivism.

The statement is also an example of what Glaser (2001) terms "worrisome accuracy." Rather than concerning oneself with which meaning is worthy of "validation" (*all* of the meanings are valid!), which implies that accuracy trumps conceptualization, a grounded theorist should view the data as a source of indicators to be coded and conceptualized. Although what I have said here may not reflect what the authors intended to say, what they said is unclear enough that even I, far from a grounded theory neophyte, couldn't discern their intended meaning. I can't imagine that a neophyte could discern it more accurately.

Yet another example of constructivism light can be found in Stern and Porr's discussion of interviewing, in which they encourage the use of interview guides for the initial interviews (they provide an example on pages 54-55). Their justification is that "Both the researcher and the participant are nervous during the first interview" (p. 52). They do say that "Once the first few interview transcripts are coded, however the interview guide can be discarded" (p. 53). Interview guides require the researcher to surmise what is relevant or at least potentially relevant. But, grounded theory is about *what is relevant to participants*, not the researcher. And, that is to be discovered, not presumed. It is usually the first few interviews (assuming the initial data source is interviews) in which what is relevant to participants begins to emerge, which sets up the direction of the research. The use of researcher formulated, preconceived questions at the outset could easily lead the research away from what is most relevant to participants and towards the researcher's preconceived relevancy. A suitable way to avoid this is to begin interviews with a "grand tour" inquiry related to a

general topic area.[4] This allows the interviewee to talk about what they want, on their terms, within the topic area. The process should not be derailed through the use of interview guides, particularly at the outset. As Glaser (2002) states,

> If the data is garnered through an interview guide that forces and feeds interviewee responses then it is constructed to a degree by interviewer imposed interactive bias. But, as I said above, with the passive, non structured interviewing or listening of the GT interview-observation method, constructivism is held to a minimum. (p. 3)

Another example from *Essentials* that encourages constructivism light and even constructivism not-so-light, and may also contribute to the defensive status that grounded theory seems to be stuck in, is evident in Chapter 4, "The Launch." In this chapter, Stern and Porr encourage acquiescence to dissertation and IRB committees when it comes to meeting expectations and requirements that may be appropriate for deductive forms of research, but are inappropriate for grounded theory and most certainly for classic grounded theory. With this advice they encourage students to game the system rather than stand up for the integrity of grounded theory. Grounded theory was first introduced in 1967 and because of the dominance of deductive research and speculative theory has been on the defensive ever since. Rather than continue to give in to this, I think it is time that grounded theorists at least try to push back.

Isn't it about time as classic grounded theorists that we at least attempt to educate non-grounded theorist dissertation and IRB committee members? If the efforts don't succeed in individual situations, the student can always rewrite and re-submit, which eventually may at least open some eyes and

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[4] A grand tour inquiry is a broad inquiry that elicits a response from the interviewee, but does not lead them towards a specific focus, response, or set of responses. I prefer "inquiry" over "question" because phrasing it as a question can introduce subtle preconceptions that can be inherent in words that are typically used to introduce questions, such as why, how, what, who, when, and such. For example, rather than phrasing it as, "What's it like to work here?" which would steer the interviewee towards an evaluative response, a more open inquiry might be phrased, "Please tell me about working here" Phrased in that manner, the responded has the opportunity to begin with what is truly relevant to them.

make a difference over time.[5] Unless we begin to encourage and do this, grounded theory will be perpetually relegated to second class status, despite 45 years of ever expanding world-wide use in theses and dissertations. Waiting for this to be resolved on its own and gaming the system hasn't worked yet, and it likely won't unless we encourage and train our students to be better prepared and more confident in their defense of the methodology. E*ssentials* would have been a good opportunity to at least introduce students to this possibility. I have urged and aided my students in doing this, and over time it has worked because before they submit their proposals they understand the method well, in large part because they understand the jargon and how to explain it to others. My students can now write, and defend if need be, honest dissertation and IRB proposals, without gaming the system by creating preconceived research questions, doing premature literature reviews, formulating interview guides, and such, all of which for neophytes may lead to constructivism light, particularly if they are not closely supervised by an experienced classic grounded theorist who can serve as an honesty broker.

When reading through *Essentials* I encountered many other locations that were at variance with classic grounded theory, as well as some that were unclear and apt to cause confusion, particularly for neophytes. They were too numerous to make note of. I decided to focus primarily on what I see as the main problem with the book, which is the absence of a clear distinction and explanation of the differences between classic and constructivist grounded theories and the constructivism light that results. If the book were about constructivist grounded theory I would have fewer issues with it because once you allow intentional constructivism boundaries are fuzzy, making it more difficult to find fault. The boundaries of classic grounded theory have been well established and articulated, first by Glaser and Strauss in *Discovery* and then by Glaser in his many subsequent books and papers. *Essentials* does not do justice to classic grounded theory. Instead, unfortunately, I think it may only contribute to the further erosion of the original methodology.

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[5] It may be less advisable to risk this on grant applications unless maybe it is part of a mixed method design.

It is personally difficult for me to be as critical as I have been in this review of the work of valued colleagues, particularly one of such long standing as Phyllis Stern. But, as a reviewer, I had to call it as I saw it. In short, although I appreciate the effort, I will not be recommending this book to my students for fear that it would promote more confusion than elucidation.

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Book Review: *Essentials of Accessible Grounded Theory* (Stern & Porr, 2011)[1]

Reviewed by Tom Andrews, RN, B.Sc.(Hons), M.Sc., Ph.D.

Grounded Theory (GT) has been subjected to continual modification to fit various ontological and epistemological positions. Although not an explicitly stated purpose, it is nonetheless encouraging that a book has been published to counter its continual misinterpretation and adaptation. This book by Stern and Porr (2011) is a very welcome addition to the GT literature. It is aimed clearly at under-graduate and post-graduate students as well as novice researchers. It is explicitly aimed at explaining classical GT rather than "other versions" and so draws on the numerous writings of Dr. Glaser for its source material.

GT can be difficult to understand because it is an advanced methodology but made more complex by the fact that its study requires students to read several different textbooks. Also, GT is written about in ways that make it hard to understand. While there will always be a need to read original sources, at last here is a book outlining the principals and practices in just one volume. The book is very well written in a style that is easily understood and comprehended by its target audience. The concepts and procedures are clearly discussed while being faithful in the main to GT as originated by Glaser and Strauss. The premise of the book is that GT is a way of thinking and not just a way of doing (p.27). This emphasises from the start that GT is not simply a series of procedures to be applied. Post-graduate students, particularly those doing advanced degrees through research are expected to engage in philosophical issues surrounding research. The section on philosophical and theoretical underpinnings is therefore to be welcomed and is discussed in a way that is easily understandable. This will form a good basis for further reading on ontological and epistemological issues. However the conclusion that GT is based on symbolic interactionism (SI) is not supported in recent assertions by

[1] Stern, P.N. & Porr, C.J. (2011). *Essentials of Accessible Grounded Theory.* Walnut Creek, CA: Left Coast Press.

Dr. Glaser that this is simply another theoretical code that is used if relevant.

The tables provide a very useful summary of terms, which can be accessed quickly. There are numerous examples provided of coding and theoretical coding. These work well and provide effective illustrations of how these are operationalized. Initially I wondered if Appendix A was necessary, but it is effective at providing examples of theoretical coding. In books discussing GT the constant comparison method is either not discussed or only very briefly, but not so here. It is emphasised and explained well.

Following discussion of interpretative frameworks, students may well wonder if they should be using one in order to "direct the researcher's gaze to where to look and think about data". Although to be used as a sensitizing tool rather than an interpretative one, nonetheless it has the potential to lead to preconception, something that Dr. Glaser cautions against, particularly since it is prefaced by the word "interpretative". Students may well form the impression that an interpretative framework is needed prior to data collection based on the discussion on pp.31-32. Also in this section the suggestion is that SI may be used as a framework together with "… any number of interpretative frameworks". Readers may be left wondering when SI should be used as a framework and when as a theoretical code. Dr. Glaser insists that this should emerge from the data just as the theory does. Emphasising the integrative functions of theoretical coding and that they provide a theoretical framework would have reduced any potential confusion. Also it would have been worth mentioning that not all GT studies are basic social processes.

While the examples used in the section on selective coding are useful, it would benefit from a more comprehensive discussion of the issues around this step in the coding process such as when to change from open to selective coding. Students new to GT get confused between the different terms and explaining the differences for example between properties, codes and concepts would have been very useful.

This book will provide an invaluable first reader in classical GT, a book that researchers can return to act as a quick reference and for clarification purposes. It will not, nor

does it claim to, replace reading the original writings of Glaser and Strauss and latterly Glaser. It does what it says on the cover: it is accessible and covers the essentials of GT.

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Stern & Porr (2011) Response to Reviewers

Phyllis Noranger Stern,DNS, LLD (hon.), FAAN

To Dr. Simmons

At the outset we want to thank Dr. Simmons for his review of *Essentials of Accessible Grounded Theory*. Our goal with *Essentials* was to demystify grounded theory to afford the reader a solid grasp of traditional grounded theory. Dr. Simmons is notably a grounded theory expert and scholar, and we are pleased that he took the time to review our monograph. While there are supportive insights shared in Dr. Simmons' review, we should address those claims that do not resonate with our intentions.

Response to Claim #1
Dr. Simmons remarked:

> As Stern's (1994) observations and insights suggested, constructivist versions of grounded theory emerged and spread in part because grounded theory was often being taught by teachers who themselves had a superficial, distorted understanding of the methodology . . . .

We do not use the term "constructivist versions of grounded theory" within our monograph. We believe constructivist epistemology bears little application and would only serve as a source of confusion to someone brand new to grounded theory methodology. Grounded theory emerged and spread not "because of distortion by teachers" as Dr. Simmons claims, but because methodology evolves, and as co-developer, Glaser, often stated, grounded theory is meant to be modified, adopted and adapted by researchers representing diverse disciplinary traditions.

Response to Claim #2
Dr. Simmons remarked:

> Essentials contains more confusing and subtly inaccurate content than a book written for neophyte grounded theorists should. Although I think it is a noble effort with useful information, it contains material that is at variance with classic grounded theory . . . .

Our work is substantiated by Glaser's writings, the work of Strauss and their mentees/protégés. We endeavored to ensure that the monograph's content would not in any way contradict the seminal works. The canons of Glaserian grounded theory were introduced and explicated with due diligence. We presented, for example, four fundamental principles (discovery never verification, explanation never description, emergence never forcing and the matrix operation) that Glaser (1994) asserts are key to every successful grounded theory project.

Response to Claim #3
Dr. Simmons remarked:

> In Chapter 1, they use the general term "grounded theory" without clarifying whether they intended for the book to be about classic or other versions of grounded theory.

We chose to incrementally introduce esoteric terms as needed in keeping with a simple and accessible format. Early on, though, we mention "traditional" grounded theory. And as Dr. Simmons had stated, we made it clear, when it was appropriate (on page 37) that we had drawn "primarily from *Discovery of Grounded Theory* (Glaser & Strauss, 1967) and *Theoretical Sensitivity* (Glaser, 1978) to lay out, as accurately as possible, essential groundwork and procedures for formulating explanatory theory."

We also use the label "Glaserian" in Footnote 2 wherein we state, "In this book we have chosen to stay close to the classic work (Glaser & Strauss, 1967; Glaser, 1978) as much as possible, or what Stern has coined *Glaserian* grounded theory, the Julliard of solid qualitative research." Admittedly, we wondered how useful this would be to the neophyte. Would one expect that the neophyte is familiar with the terms classic, classical, Glasserian, or Strausserian?

Response to Claim #4

Dr. Simmons remarked:

> However, in other locations in the book they legitimize
> and even encourage actions that are clearly outside
> the boundaries of classic grounded theory. For
> example, in Chapter Two, "Brief History of the World
> (of Science)," in their section titled, "Your Theoretical
> Lens" (pp. 30-33), they discuss, legitimize and
> encourage importing "theoretical lenses" and "explicit
> interpretive frameworks."

Clinicians and academics bring to qualitative inquiry a
disciplinary theoretical lens in terms of why people act the
way they do. Today's grounded theorists, especially with our
advanced understanding of human behavior, are often
examining not new topics, but new aspects of topics or
human phenomena, and what emerges are nuanced
explanations of human adaptability in select, and often,
complex situations. "For example, a psychologist may
conceive of transition from employment to retirement as a
developmental task; a social worker may consider it a
stressor; and, a physical therapist may see it affording time to
establish an exercise routine. If each professional explores the
topic as researchers their theoretical contributions will be
equally beneficial but dissimilar because they will have
approached the entire research enterprise according to their
unique tradition, interests and context." A grounded theorist
will make it explicit what she is looking *at* in terms of
observed behaviors, but does not, and this is the difference,
does not bring preconceived suppositions as to what she is
looking *for* in the data. The grounded theorist allows "the data
to speak" and in so doing the process of adaptation emerges.

Concluding Comments

Dr. Simmons has provided us opportunity to pause and
reflect, and we maintain that our monograph-sized book
informs the neophyte of the essentials necessary to conduct a
grounded theory study.

To Dr. Andrews

Thank you for your thoughtful review of *Essentials.*

You've given us advice and direction as we move forward with our project of explaining Glaserian grounded theory—no easy task.

It's reassuring that you *got it*. I'm especially glad that you think Appendix A was worth including: I thought it would either be helpful, or it would bomb.

I've agreed to write a chapter on Glaserian grounded theory for Cheryl Beck's forthcoming book, *Routledge International Handbook of Qualitative Research.* Cheryl's idea was that each chapter might start with a literature review. Funnily enough, such a lit search is impossible, because authors fail to distinguish between classical GT, and variations on the theme—it's only purists like you and me who give a rap.

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