

How qualitative (or interpretive or critical) is qualitative synthesis and what we can do about this?

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Introduction

This talk will not be a well-organized argument. Rather I intend it to provoke thought and debate. My goal is raise questions about how we can insure meta-ethnography is sufficiently qualitative in approach and outcome. Thus my title.

I am also speaking to scholars mostly in the health sciences—not my field. And I am an American (I attach myself to the whole continent not just the US). This means I have an academic style and set of discourses that may not track on yours as well as I would like. I apologize for when my discourse does not translate well into yours and hope you will interrogate me and my ideas to help with both clearer communication and sharper disagreement. Let me start with an origin story of sorts.

In the beginning-meta-ethnography

I fashion myself a critical, indeed, a post-critical (as one of my books is titled) ethnographer. And I must admit that critical ethnographers at work are a serious lot. In the bar, they, we, can be different but they, we, slip so quickly back into our serious personas. So when in getting serious about meta-ethnography, I am calling on the trope of mental anguish and dedication that accompanies efforts to change the world. And it is one of my conceits that I am about creating a more equitable world. My research is all about understanding racialization and class formation and working to undercut it in service of racial equity in the US. And as all of you know, we in the US have a long way to go in this. For tonight though, you should now be concerned. I do not believe in a value neutral research, or even objectivity in any straight forward sense (as a sociologist of knowledge, I do find it an intriguing claim to power and prescience).

Let me start by telling a brief story of how a working sociologist got tied up with meta-ethnography and subsequently the whole industry of qualitative research synthesis.

When Dwight Hare, who has now passed unfortunately, and I started to work on meta-ethnography (Noblit and Hare, 1988), we were also working on studies of schools and school system efforts at racial equity. I had started my ethnographic career studying school desegregation in the US. As we tell the story in our 1988 book (the little blue book, as one of colleagues refers to it) on meta-ethnography, my school desegregation ethnography was part of a set funded by the US federal government. When they were all done, we had found that desegregation had actually meant that African American culture, and African American student school performance, were being destroyed as white controlled school districts resisted, engaged in subterfuge and ultimately created a full backlash against efforts for racial equity. White supremacy is still at play in school politics in the US today in the name of school reform, choice, and so on. I am sorry to say that your school systems have taken on some of the reforms that were created as part of the backlash to school desegregation. They are effective at maintaining white control of schools in the US but there is no systematic evidence that they have done anything to improve schools. Again apologies.

But I digress—at the end of the desegregation ethnographies two efforts were commissioned to ‘make sense’ of what had been found. To cut to the chase—both failed. They added nothing to the dialogue. They were of no use in promoting racial equity or understanding how schools were caught up in producing racial difference when they were supposed to be integrating rather than differentiating Black and White students. The cross-site synthesis efforts failed because they did not actually think about what synthesis is. There were many shortcomings of these efforts, and Dwight Hare and I took a side step from our work on equity into the issue of what is synthesis and more importantly what is a qualitative approach to synthesis. So we began reading about research synthesis—a dry and uninteresting field—that had been linked at the time to efforts to disseminate knowledge. We also were intrigued with the then new quantitative meta-analysis. The desegregation ethnography ended up converting me into a full blown qualitative researcher but, in truth, I had been trained as a quantitative sociologist and had published quantitative studies. I still do some quantitative work, mostly as part of funded evaluation studies, but while my mind can do it, my heart is not in it. Meta-analysis was new back then; it was hot dogging research conventions; and it worked by adding data sets together—what Dwight and I ended up calling an aggregative logic.

‘Data’ of course was the result of an empirical indicator created by the researcher for specific purposes, In meta-analysis, the predicate of the prior sentence, ‘the result of an empirical indicator created by the researcher for specific purposes’, is in practice an elision—it disappears in the presumption of an objective, ‘real’ data point.

We knew we could not live with this for qualitative synthesis. Thus we began to read up on interpretation, finding that another elision. Interpretation was much akin to magic. If you said the right incantations (coded the data repeatedly, used the constant comparative method, and so on), then interpretations resulted. We began reading widely, and at the time reading all we could of the then (as now) ‘crisis of representation’ in Anthropology. In a sense, one could say the pursuit of meta-ethnography was actually us trying to figure out what interpretation was in the first place. It was clearly about meaning, signification and so on. As Geertz (1973) wrote, the ‘webs of signification’ was culture. The ethnographic account was not magic to Geertz but rather simply another ‘interpretation of interpretations’. The crisis of representation folks, George Marcus, Michael Fisher (Marcus and Fisher, 1986) and James Clifford (1988) amongst others, were merciless in adding that there is no ‘immaculate perception’ in interpretation by pointing out how ethnography was all about building Western superiority, exploiting resources for Western nations, and so on. The ethnographer was part of the colonizing process, and had served it well. Dwight Hare and I were fully engaged with this critique—even though I can now admit we did not really see its implications as fully as I do now.

Our limited understanding at the time, did lead us to reject ethnology as a clearly Western taxonomic construction—rendering the world into the Western gaze rather than achieving an understanding of others’ lives and cultures in ways close to the way they understood it. Our move was to first recognize that ethnographies are accounts—not ‘objective’ depictions—accounts created by people like us to represent a scene, a way of life, a set of experiences. (Since this time we would add more to the production of this depiction including positionality, partiality and multiplicity). The ‘data’ presented to our way of thinking were artifacts of the account. After all the authors selected what data to include. They may have built the account inductively from piles of coded fieldnotes (or now software generated coding retrieval sets) but it was their, not the participants, minds and hands that did it. Thus the account is properly the authors not the participants. If the authors used member checking this yields only a

confirmation of the authors' account-- not how life was lived and understood before the account was given to be 'checked'. This is not to decry tenets of qualitative methods—I make my living teaching qualitative methods and even co-direct a graduate certificate program in qualitative methods. But it is to say, that methods make the researchers' accounts more predictable, that they fit the standards of the current epoch, and so on. There is then a real tradeoff in us getting our methodological act together and our ability to render their account close to how they lived it. Surely some of the practices do move us closer but at the same time such practices are *our* lenses that refract *our* vision of the other—it is not their lens by any stretch of the imagination. Further, our interaction with them is always partial—time, space and context is always limited to the on-site investigation; access to their webs of signification is always relative and contingent; and so on.

For a meta-ethnography, this meant that we knew we were working with scholars' constructions—as the data turns out to be socially constructed. And in written accounts this construction is built into accounts of methods, in the theories used, in the researchers' worldviews—that is in the interpretations the authors developed. This is one reason why we recommended not limiting oneself to the results section. The interpretations are layered throughout the accounts. Focusing on the results only is a vestige of positivist objectivism—assuming the known is separate from the knower—which as above is a hard sell for people like me.

Thus we had to go with interpretations as that to be synthesized. There are plenty of critiques of this position including the critique that this means we are not actually getting to the research participants' experiences and perspectives. But that is where working with published accounts leaves us. In any case all knowing is mediated, there is no way around it. Meta-analysis may work on data but their data is also a scholarly construction based on the scholars' ideas about variables and measurements—a highly reductionist worldview. Anyway, the issue then becomes that all synthesis of existing studies is more akin to expert assessment than emic portrayal—whether quantitative or qualitative. All is not lost with this move—we are working with trained and experienced qualitative researchers for the most part and they were in the field directly—interacting with the research participants. The researchers do have a reasonable understanding of the field. In fact, the best understanding that is currently possible by our rules of research—remembering Geertz's assertion that qualitative research is always 'interpretations of interpretations' and so meta-ethnography is simply one more layer in that. Dwight Hare and I wrote the little blue book focused on ethnography—because ethnography involved extended engagement with the field. Qualitative research synthesis since the little blue book has demonstrated to me that meta-ethnography can be extended to include many forms of qualitative research. We are now finishing an article on meta-ethnography of autoethnographies as a case in point.

There was another equally vexing issue as we wrote about it then—the nature of language. Remember this was also the years marking the 'linguistic turn' that was part of what a friend of mine called the post-toasties: post modernism, post structuralism, etc. Here is where we parted with simple scientism that treated words, phrases and accounts as literal. Language is just too inexact; too loaded with context and history; too messy to be treated literally. We had to find a way to work with language that allowed sufficient space to allow some of the messiness in, in order that a meta-ethnography could produce something more than a simple retelling or aggregation of what was in the studies. Our move here was to embrace the metaphoric nature of language. Many people who have read the little blue book did not know what to do with this—and this is no doubt my failing. I should have been clearer about our intent.

Put simply, accounts-- the themes, phrases, and even the quoted data—are metaphors—‘as if’ characterizations. When I say ‘I’, for example, this ‘I’ is the individuated ‘I’ of the West. This ‘I’ was developed through the Reformation and the Enlightenment and extended through Capitalism and democracy. It is the ‘I’ behind rational choice models in economics. It is this ‘I’ that Foucault unpacked and that the post-structuralists and post-humanists are trying to see beyond. In any case, my ‘I’ is not the ‘I’ of rational choice models. My ‘I’ is filled with history and historicity, with myth and legend, with relationships, with power and denial. My ‘I’ is European and Native American. My ‘I’ emerged in the mountains of Appalachia. Who ‘I’ am was narrated as one of people George Washington, who, as a British officer before the American Revolution, came over the Appalachians to tell my French and Seneca ancestors they had to leave or face British troops. The spelling and pronunciation of my name was, as my great grandfather put it, are not important—because having lost the subsequent French and Indian War, it was a product of British tax rolls. It is ‘as if’ my ‘I’ is a collective representation not an individuated one, it is deeply historical, social and contingent. The question for synthesis is what the meaning of ‘I’ is and how much room it has for reinterpretation. Thinking of language as metaphoric allows us to recognize not all ‘I’s are the same and that metaphorically speaking they are not ‘as if’ in the same way. Can the rational choice ‘I’ of economists be seen as related to the social and historical ‘I’ that is more commonly acknowledged among subjugated peoples? Clearly here is where scholars enter the technical fray of qualitative research synthesis, and here is where discernment (rereading the texts to see unpack the nature of this ‘I’) and creativity (thinking beyond the literal to the imaginative possibilities) enter meta-ethnography. To my way of thinking, these are not equivalent ‘I’s. I could argue that the second may be able to encompass the former but not the other way around. But more importantly if one saw ‘I’ as a theme in a meta-ethnography this difference should push one to look more deeply at context of selfhood, to see the ‘I’ more fully, to see what range it has and what it can encompass, to wonder if they are refutations of each other than having a relative similarity, and so on.

This is just one example. Another might be when a patient says they ‘hurt’. This is a referent to a host of allusions—what they have experienced as non-hurting; what relation this hurt has with other hurts, what stance towards hurting their families and significant others take, and so on. It is ‘as if’ they ‘hurt’—not denying the claim but rather allowing it plenty of room—room for interpretation. ‘Hurt’ then might be more than an acute or chronic pain but rather an allusion to , a metaphor for, how they and their social networks locate themselves to wellness. As a former rugby player, I hurt all the damn time and for the most part I just assume that is the consequence of how I lived my younger life—and in general is not all that notable to me. But when I hurt in a new way, it is notable to me and I comment on it and sometimes even seek relief from it—that is organize at least part of my life in a new way related to this new hurt. Again we have to ask what is hurt a metaphor for? In a set of studies that all have hurt as a theme—is it the same hurt or can hurt be a metaphor for different things. I think the answer is always ‘maybe’ and maybe we should check this out in doing a meta-ethnography.

My examples have been focusing on individual terms here but in the little blue book we were less interested in individual themes and more in the fuller interpretation offered—what I now term the *storyline* of a study. We argued that studies had to be translated into each other as wholes—that is create analogies between and amongst a set storylines that encompass the whole studies. Analogies, by the way, are extended metaphors—‘as if’ elaborated. I also should admit that this was not developed well-enough in the little blue book. I think the term *storyline* may better serve keeping the individual themes in their contexts.

Let me read a section from an article which is forthcoming in a special issue of *Ethnography and Education*, Sherick Hughes and I conducted a meta-ethnography of autoethnographies of teaching difference in higher education settings. We searched broadly for autoethnographies of teaching difference and found some 30 studies, we then began a process of winnowing (are these all autoethnographies, all of teaching difference, taking place in higher education, etc). This specification yielded 4 autoethnographies in higher education. Now the quoted section:

The careful reading and identification of interpretations in the texts highlighted the differences between the studies. The authors are different differently, teach different courses, use autoethnography differently, etc. Yet, there was an analogy that could pull these studies together. It is “as if” teaching difference is ‘working’ three identifiable sources of resistance.

(1) First, in the larger society, difference is stigmatized and Whiteness, White supremacy, and colorblindness are characteristic.

(2) Second, in the classroom itself, it is (remember --‘as if’ again) many white students are bodies of resistance and, while different in views and backgrounds, act out the Whiteness that permeates the wider society. The students are more actors of resistance and the society more the context in these accounts, but these intersect.

(3) Third, the academy itself acts in tandem ‘as if’ it is a Body of Resistance. The academy is less analyzed here but represented as both Whiteness and organizational oppressor, enforcing White superiority through its rules and denying the special case of teaching difference as well as teaching difference as a marked professor.

The teachers are ‘othered.’ Being “marked” as different (in race, gender, age, (dis)ability) stigmatizes the professor and thus students challenge and insult both the competence and authority of the teacher. There is also presumed pedagogy for this kind of teaching that involves the engagement of the personal, the embodied and the emotional. This pedagogy requires professors to use their bodies and personal experiences as ways for students to engage their experiences and emotions. Yet, for the scholars of color, this engagement becomes a contradiction. Prejudged by Whiteness as incompetent and of dubious authority, professors of color are read as weak –more evidence of lack of authority and competence. Professors of color use theory and content knowledge to combat their denigration, but this usage is partial. It also violates the presumed pedagogy as well as their own beliefs about how deep prejudice and racism is. The sole White, able bodied professor in the accounts above was able to get personal and her students made significant inroads into their whiteness and their role in a racialized society. She had to work against herself and norm of politeness in white women do to accomplish this shift. She has doubts about how much was accomplished which is largely consistent with the accounts of the marked professors. The accomplishment in all these cases was partial and troubling -- hope for a successful pedagogy seems muted.

Yet, in the absence of comparison to other accounts of what and how students learn in higher education courses the above conclusion may be too harsh. Each autoethnography provided strategies that seemed to have potential and, while not fail safe, may be the best for now. It may be that teaching difference in a society that enforces Whiteness must live with partial success and much pain and concern on the part of faculty. Notably, the accounts held out the

possibility that non-marked faculty could be effective at some level and under some conditions. Embracing teaching difference as a struggle bound up in hope and resistance seems to be the testimony of these accounts. The complexity of this interpretation demonstrates how the meta-ethnography of autoethnographies engages synthesis as translation through reciprocal analogy and how it can be applied to produce deeper insights than primary studies may have produced when interpreted in isolation.

So what was I illustrating here? First, when we created a chart of the themes for each interpretation and compared them, the terms used by the authors were different. They came from different disciplinary and positional (race, age, ableism, gender, etc.) backgrounds, taught different courses, and had different rhetorical styles. The differences pushed us beyond the terms to what was being said about the experience. This is where we needed what Ecker & Hulley (1996; 2000) refer to as ‘third order’ concepts that reframe the concepts used in the studies and gives a new perspective. Yet we went for a third order **analogy** not a list of third order concepts. Clearly one could use possibly build a third order analogy by focusing on the concepts but this effort seemed to take us down a ‘rabbit hole’ as the metaphor goes. Anyway, my point here is that the goal is an analogy amongst studies not a set of similar and/or dissimilar themes. Themes may be used to explicate the analogy but the analogy is the goal. The analogy as we wrote in the little blue book can have different forms, reciprocal (as in this case), reputational or line of argument (and I welcome more work on various forms) but in the end it is simply an interpretation of interpretations.

Up to this point you may think I am reinforcing how to do meta-ethnography correctly. Actually I am not. I have reviewed many, many qualitative synthesis projects for journals and have learned that a methodology has a life of its own. Louise Jenson (Thorne et al, 2004) once called the result of people doing qualitative synthesis projects a ‘meta-soup’. Everyone has their own approach. I recall one review process early in my career where I commented on how well the authors stuck with the book specified approach—which they had diligently—only to have another reviewer say that the authors had botched the procedure. Even on the second rewrite where I moved to the offense revealing that I was one of the authors (I know-- poor showing on my part) and did a chapter and verse account of the fit with the book—only to be told that I was obviously the second author and missed the whole point—that meta-ethnography was to be used to inform policy not comment on the field of inquiry. Now it is true that we set up the need for meta-ethnography in part because policy could benefit from syntheses but we never limited it to policy. The point—y’all (I live in the American South!) do whatever you wish with the methodology. I am happy if it sparks your interest and if you find out something interesting. But I do have the right to say what I think as well—as I am doing here. My concern is not how to do textbook meta-ethnography but how can we understand this as a form of qualitative research.

To this point, then, I have so far located meta-ethnography as a product of the crisis of representation and thus we are working with researcher interpretations—not data. It is based in the linguistic turn where language is metaphoric thus providing space to work with concepts when doing a translation of accounts. It is also about analogies amongst the whole interpretations -- not concept to concept which has the net effect of stripping context away. All these steps were Dwight Hare’s and my way of keeping meta-ethnography from being simply aggregative—but in the ‘meta-soup’ of qualitative research synthesis that Jenson identifies—aggregation keeps creeping in.

What is qualitative about qualitative research synthesis?

Protecting against aggregative logic though is different than saying what should be. How are we to think about a **qualitative** research synthesis? Obviously that depends on what we think qualitative is. As I am Luddite when it comes to technology, I first went to my library and then broke down and went to the internet. As we know from research synthesis, a search procedure teaches lessons as well. One lesson is that the paradigm wars have had an effect on how we understand our craft. Time and again, the characterizations I found we bound to how qualitative research is different from quantitative research. That is true but 'duh'—we know that. Knowing by comparing to an opposite is poor substitute for knowing the essence of something in its own terms. I no longer have any interest in drawing such distinctions, but I know others are still fighting the paradigm wars. Further, there is a range of qualitative methods from post positivist to posthumanist—and this also affects how we speak about what qualitative research is. But let's muddle through what are the essential elements of qualitative research?

Some might be: Inductive, grounded (as in close to experience), multiperspectival, value explicit, co-constructed, contextual, and participatory. How do we embody these in synthesis, I ask? Because I want to expand rather than shut down our conversation, let me take on sets of characteristics one at a time both to elaborate the issues and to reveal some of the different ways we could be qualitative.

Descriptors of qualitative research and queries about meta-ethnographic practices

Let me use some sets of terms people have used and then create a question about what we should do in meta-ethnography or other qualitative research synthesis.

[No category is mutually exclusive; and the lists have decidedly different characters]

Spicer (1976): Applied anthropology

Emic— 'in the terms of the participants'.

For meta-ethnography then the focus could be on researcher perspectives—how far do we pursue this? Do we seek studies of professional and disciplinary cultures? Characterizations of our societal culture?

Historical—'situate studies in time and context'.

How can we capture change in a field of studies?

Holistic—'a tension between specification of phenomenon of interest and its environs' such that being too specific limits holism.

Can we resist reduction and preserve complexity and multiple contexts

Comparative—'compare multiple interpretations within studies and interpretations across studies'.

The effort to reduce studies may compete with comparisons within and across studies which are necessary to create an analogy. The chart of themes across studies is not the product but an interim step. It sets a puzzle to be explained. Do we compare with studies that are contextual around the phenomenon of interest. Do we seek out cross-cultural studies?

Patton (2002) Qualitative Research and Evaluation Methods

Michael Quinn Patton emphasizes the goals of verstehen, empathy and insight. The below draws only some of his points into this conversation.

Focus on meanings--how do we account the researcher perspectives and what we make of others meanings?

Context of social interaction--what is the social scene of qualitative research? Of meta-ethnography?

Understanding—how do we appreciate the practices and perspectives work in qualitative research and how they interact with understanding the phenomenon of interest?

Connections--Does help to think that our analogic product is an account of the mechanism –how things work together?

Context sensitivity—do we trace the multiple contexts out from the scene? Is the context of health care written into synthesis accounts?

Reflexivity--How does the researcher comes to understand her account? Do we reveal our reflexivity in meta-ethnography adequately?

Wendy Luttrell (2010): The Promise of qualitative research...

Wendy Luttrell has a rather different characterization of qualitative research. For her, it is best understood as “a social art form” () which is a portrayal many would find too unscientific. However, she led me to think about these:

Makes explicit the implicit--do we pursue these taken for granted understandings adequately in meta-ethnography?

Iterative--do we keep fieldnotes on our developing understandings are we read and synthesize studies?

Imaginative-- (Ah--the magic of interpretation) What can be made of the studies beyond what they say?

So far I have used texts that are seemingly more interpretive than critical in their approach. However, I do want to push meta-ethnography into the critical perspective as well. Since I know not everyone has a background critical ethnography, let me use Carspecken (1996) characterization of critical ethnography as an entry point. He argues that critical ethnography assumes a particular value orientation which involves (and I add questions about meta-ethnography):

1. Research is used in cultural and social criticism. To what extent do we do this with synthesis? Can we take non-critical accounts and make them thus?

2. Researchers are opposed to inequality in all its forms. Will meta-ethnography take on such a value orientation? As a first step, will synthesizers simply be value explicit?
3. Research should be used to reveal oppression and to challenge and change it. What is meta-ethnography's relation to the patterns of power and privilege?
4. "All forms of oppression should be studied." To what extent do we see meta-ethnography as focused on revealing dynamics of oppression and supremacy?
5. Mainstream research contributes to oppression and thus critical epistemology should presuppose equal power relations. How is meta-ethnography implicated in unequal power relations? Could it become more of a community or participatory practice?

(pp. 6-7)

Carspecken argues for a set of central points that also have implications for the practice of meta-ethnography:

1. Be extremely precise about the relationship of power to research claims, validity claims, culture, and thought. Should this be part of our deliberations and reporting?
2. "...make the fact/value distinction very clear and must have a precise understanding of how the two interact." This is especially important in the realm of evidence based practice today and leads me to ask can meta-ethnography question the facts and reveal the values behind them?
3. Be explicit about a theory of how symbols are used to represent reality, how this changes and how power is implicated in symbolic representation and changes in symbolic representations. This requires meta-ethnographers to have a rather sophisticated understanding of the symbolic world, making me ask: what "symbolic capital" (Bourdieu, 1986: 255) are the studies trading in and what symbolic capital is the meta-ethnography trading in?

(p. 9)

In my own career, I found critical ethnography to not be sufficiently aware of its own positioning as a form of knowledge. This led me and a group of scholars to consider a "post-critical ethnography" (Noblit, Flores and Murrillo, 2004). There is much more to this story but let me use one chapter from that book to illuminate a little of what that asks of critical ethnography. Mike Gunzenhauser (2004) explored what he saw as the "four promises of critical ethnography" which were: 1. Giving voice (to the less powerful); 2. Uncovering power; 3. Identifying agency; 4. Connecting analysis to cultural critique. These are a little different than Carspecken's points. For meta-ethnography, they lead to obvious questions about: How do we give voice to the less powerful? How do we uncover power? How do we connect analysis to cultural critique? (And because early critical ethnographies overemphasized the totality of oppression) How do we use meta-ethnography to reveal human agency and suggest ways for the less powerful to face and challenge their oppression?

Post-critical ethnography, Gunzenhauser argues, adds two more promises. These promises emerge from the history of critical ethnography which emerged from the concerns of women and people of color. Ellsworth (1988), for example, found that critical ethnography itself could be oppressive in its totalizing view and in its white patriarchy. Parker and Lynn (2002), and many others, noted the tendency to

emphasize class over race in critical ethnographies. There were many more concerns than these, but the point is that critical ethnography itself could be oppressive. Gunzenhauser argued that this could be remedied by at least in part creating a post-critical ethnography that added promises of self-reflexivity on the part of the critic and a commitment that critical ethnography would be non-exploitative and not use the study participants as fodder for the critic's preferred perspective. Adapting these for meta-ethnography then: How can we consider our own critical views and critique them? How can we use meta-ethnography to be reverent of people who have been studied and upon whom the results of a synthesis may be inflicted?

Post critical ethnography is less a method than a perspective. Hytten (2004) proposed that post-critical ethnography was to be (and again some questions it suggests for meta-ethnography):

Educative--who benefits from our synthesis most? Trickle down effects?

Emancipatory-- who is freed? Who becomes knowledgeable?

Empowering-- For whom? In what senses?

In addition, we (Noblit, Flores and Murillo, 2004) saw post-critical ethnography as:

Invoking Positionality—what is the positionality of the synthesizers and how does relate to that of the original authors of the studies??

Worrying objectivity – What is the synthesizers understanding of the nature of qualitative knowledge and interpretation?

Worrying representation—Since texts flatten life, how synthesizers represent that being synthesized and the synthesis itself?

Clearly, considering how qualitative, interpretative and/or critical meta-ethnography raises a host of questions that need addressed. Again, I am not defending the original idea of meta-ethnography and, while I have a perspective on how I wish meta-ethnography would be used in the world, I am not promoting any one best approach. There are many ways to answer the above questions, and my hope is that this paper is provocative enough that people will find reason to argue, disagree and ultimately to create multiple approaches. But before I move on, let me offer up one more cut that emerges from the crisis of representation in Anthropology. James Clifford (1988) in *The Predicament of Culture* sets out a rather different set of tropes for a post-crisis ethnography. He offers (and I add meta-ethnographic queries):

Polyphonic-how are the multiple perspectives in the studies and in the synthesis attempts preserved?

Interrogative-what are we questioning with a synthesis?

Inventive-What new do we construct with a meta-ethnography?

Subjective-How is a meta-ethnography a personal and/or collective quest and of what kind?

Exegesis-What is meta-ethnography an expression of?

Partial-How does synthesis live with its inability to know all?

Cultural critique-What we questioning in the wider world and what are contributing to that world?

Interpretation and critique is about making sense of things and interrogating interpretations. People do it. Ethnographers do it. Meta-ethnographers do it and should do more of it. A qualitative research synthesis is more qualitative when it results in making sense of something than describing a scene or experience and its component parts. The latter is important to understand but its role is to explicate the interpretation –not supplant it.

The developments in qualitative research challenge how Dwight and I thought about meta-ethnography back in the late 1980s and the way meta-ethnography has been done since then. Among other things, this tells us that qualitative synthesis lags behind qualitative methods. Meta-ethnography has a lot of catching up to do and I look forward to how this proceeds. But with this invitation I also want to identify a particular problematic. This problematic suggests that we cannot count on the ‘corrective’ of fieldwork to keep meta-ethnography qualitative.

A particular problematic of meta-ethnography as a form of qualitative research

I have taught fieldwork and advanced data analysis and interpretation for some 33 years. I must admit then to being a methodologist—one who teaches students ‘how’ qualitative research ought to be done. There is a contradiction in this for me. While I do think it is important to have the ball of tricks that methods courses offer—doing what I teach will not yield a qualitative study. The study is an encounter with others or other scenes, real or perceived, and oneself. I tell my students the course I am teaching them will get them started and then I will continue to teach irrelevant things until the students begin to have ‘breakdowns’. I normally tell them that this is not a psychic breakdown, although this does happen at times. There are many types of breakdowns that I will not elaborate here but a usual one is when the study intentions do not capture what the encounter teaches. I’ll come back to this later but for now we should acknowledge that the breakdown that happens in the face of people—in scenes in which the qualitative researcher must interact—is especially poignant.

In meta-ethnography, we have breakdowns as well. For example, intention must yield to the studies we come across in our search. However, it is not a breakdown in an encounter with others. Typically, the breakdown happens in the context of one’s peers, the research team. It is heady and leads to all kinds of panic, thought, insight, and usually a lot of planful behavior (new search procedures, rereading the texts with a different lens, developing matrices of possibilities, reconsidering intentions and audiences, etc.). Nevertheless, it is not a breakdown experienced in the context of the live Others. One might say it is more a metal breakdown among colleagues, less an outgroup and more an ingroup. When doing fieldwork, the breakdown is a lot like full frontal nudity on a public beach. It exposes you not to like minded people but to people who are not like you. You find yourself unexplainable in the context of their becoming understandable. You are exposed to them and yourself—and both are hard to face. However, since my students are in a social encounter of difference, it usually works out—with a lot of soul searching, member checking, informant interrogation, and fieldwork role realignment. Of course, some students bull their way through, refusing to acknowledge that their original perspective was out of

whack but thankfully this turns out to be an object lesson for the other students, who witness how this reveals the researcher not the researched. The disorientation experienced in fieldwork, for most, leads to all kinds of new perspectives but for those who simply bull through, the accounts devolve into blaming the others, into hyper assertions of knowing (omniscience), and coding and themes substituting for interpretation and/or critique. Ironically, these 'bull through' students often are the most critical of the Others but in large part because what the Others taught was too dangerous to the students' worldviews to be allowed recognition. For the other students, all my teaching of methods thankfully gets lost as they try to work through and against the encounter that challenges their worldviews and assumptions. It is the fieldwork encounter that produces the account—less so the methods and certainly not my methods courses. Fieldwork often then acts as a corrective to the 'will' of the researcher.

Meta-ethnography is more a library or even laboratory scene—scholars working with texts (but not usually ancient texts around which mystery abounds). Our texts are produced by people like us. They get published when they fit some set of standards and tenets of language, practice, and performance that the research team has some experience with, and to some extent accepts. Meta-ethnography is an encounter with ourselves as ourselves—and we treat ourselves very seriously in all this.

Simply put, meta-ethnography is not an encounter with the Other in the same way as primary fieldwork. It is an encounter with professionally produced accounts by professionals with similar worldviews and experiences. Meta-ethnography may cause breakdowns but they are not the same ones as in primary fieldwork, and moreover the field cannot be counted on to provide a corrective to our worldviews, commitments, careers, and wills. We must do this for ourselves. We are sole the guardians of gate. It is worth considering then what keeps meta-ethnography or other form of qualitative research synthesis from being simply group think, from being an exercise in profession hubris, from slipping the moorings of qualitative research partially or entirely. I would argue that a post-critical, meta-ethnographic turn would be of great service in this encounter with ourselves. As above, a post-critical approach turns critique back on the researcher. In the post-critical approach, positionality and reflexivity is about discerning the interpretive lenses we use and identifying the assumptions in the same and then critiquing those assumptions to see who benefits from them, how power is being wielded, how ideology is being promulgated; and what options for agency and empowerment are possible.

This suggest to me we need to experiment with processes for meta-ethnography that articulate the type of synthesis (interpretive, critical, post-critical, feminist, critical race, etc.). From this, we should develop a set of 'commitments' or 'expectations' for the synthesis that is appropriate for the 'type'. Moreover, the process may also include types of research team actions that were designed to guard against group think and/or slipping away from qualitative processes and reasoning. I would personally argue against a formalization of a set of 'correct' reasonings or practices and more a process of dialogue about how we as synthesizers are learning to be as qualitative as possible while doing meta-ethnography.

A comment on knowledge resulting from a meta-ethnography

There are those who wish for a rather 'conclusive' result from a synthesis product. Interpretations are constructions, they are situated knowledges. It is likely that 2 different synthesis teams will have different syntheses. Positionality counts. That said, I suspect there are times when syntheses do lead to some definitive understanding—but my cynical self believes this means either the studies are decisively focused from the beginning (that is, the type of desired knowledge was predetermined by the research team) and/or process of getting to the final set of studies eliminated too much variety. My colleague at

UNC, Margarete Sandelowski (See Thorne et al, 2004) though has a rather different positionality from mine, honed in her initial synthesis of HIV positive women—who were dying. For her, something needed to be done and synthesis was one way to get to that. I fully respect this even if it is not my perspective. The general point then is that positionality affects the kinds of interpretations people make even in meta-ethnographies. For me, positionality is less about what Goodall calls: ‘fixed positions’—race, class, gender, sexual orientation—and more about the perspectives being brought to the work. In health studies, what is your orientation? Is disease to be eradicated, or is that a chimera? Are we privileging the health care professional in the synthesis or the patient? And so on.

Who do you bring to meta-ethnography and how will the reader know the salience of this? Goodall also argues that positionality enables voice. Barbara Paterson and her colleagues (2001) and Margaret Sandelowski have different positionalities in their work and different voices as well. Like Patricia Hill Collins (1990) argues in regards to standpoint epistemology. Everyone speaks from somewhere and the reader deserves to know enough about the author to be able to take their position into account. Hiding behind method or objectivity disables readers.

I want to be clear that I fully understand disciplinary and national differences in what is respected speech. I, an American, use ‘I’ in my studies or ‘we’ when the authorship is really a ‘we’. That is respected in qualitative work in the US (mostly). That may not work for all disciplines and nations, but the emphasis is not on a sense of American familiarity but rather a sense of what important beliefs one brings to meta-ethnography. Readers deserve to know the color of your lenses, to use a dated cliché.

Interpretation and critique have some relation to methodology but it is not as close as one would think. Harry Wolcott (1980) used to argue that good methods will not necessarily yield good interpretations. Doing a good interview for example is a joy to behold but the interpretation of the interview is what makes it meaningful—literally. Wendy Luttrell (2010) has argued for ‘good enough’ methods as the mark. Impeccable methods may signal a displacement of the interpretive goal. If your goal is to understand how people make sense of their world then your methodology should be subservient to that goal. My many years as a journal editor leads me to believe that methodology sections in most articles are best understood as a ‘performance’ of methodology rather than a realistic account of what was actually done. We can assess the performance of course but that is rather different than understanding what happened in the field.

The same is true with meta-ethnography. The accounts of how the search went, the claims to being systematic or exhaustive, read much too smoothly to be actually what happened. A truer account probably would have false starts, missteps, misgivings and an acknowledgement of simply running out of time, money, patience and/or ideas. The details of how the interpretation was approached is often more sketchy still, and often slides to citing texts and listing steps.

Emma France and her colleague’s account (France et al, 2009) of what’s wrong with meta-ethnographic reporting have done an admirable job detailing what the reporting is and isn’t. To overly summarize their review, authors of meta-ethnographies are often only loosely doing meta-ethnography per say (Jenson’s meta-soup image is called up again!). Authors assert what they have done, rather than show how they did it. As the paper moves from the search to the synthesis-- things become more opaque. This all is lead up to the devastating finding that it was not clear in some 38% of the papers if anything new was offered at all. I know the distinction between normal science with its slow plod of accumulating findings, and paradigm shift (when what was known before is accounted for in a new way that also takes

in what was not able to be explained under the normal science regime (Thomas Kuhn, 1970)), but this makes me wonder why bother with all the sheer hard work of research synthesis if so little is gained in practice?

We need more studies of studies such as these. We need a conscientious effort to study the methodologies of synthesis, of meta-ethnography.

Importantly, we do even know if different methods yield different results. I do know from experience that different search parameters and keywords do yield somewhat different results. That is good to know. But I less sure we know that it makes an interpretive difference. I edit *The Urban Review*, a scholarly journal about urban education and equity and recently we were trying to decide whether to proceed with a rejoinder to a systematic review of the literature. The complainant had a point—the review did not catch some studies—and the complainant’s rejoinder recommended a procedure that would have found the missed studies. All to the good. But my Editorial Board asked a simple question that proved vexing to the complainant. How would what the original articles explained and concluded be changed by the more inclusive procedure? The complainant was not been able to convince the Editorial Board that there was a substantive difference. This the Editorial Board was reluctant to expend reviewer time, then engage the original author in a response to the rejoinder, and so on if there is no difference in what is said to be the current understanding of the topic. Let me be clear—I am in favor (sometimes –I will come back to this below) of energetic, systematic, exhaustive searches for studies. But do we know that it makes a difference? In fields which the literature builds on itself would it be likely to make much of a difference? In fields marked by controversy, I suspect it would. But the truth is we simply do not know. The same is true of interpretive goals and strategies. Would Sandelowski and I come up with different results based on differences in procedures? Maybe. But I more convinced that we would come up with different results based on what we think is the ultimate goal—critically interpreting a field of study versus drawing conclusions about what should be done. Anyway, the point is we need methodological studies done to know what differences make a difference in the interpretations that result.

I am promoting such methodological studies not just to know more about what works to what end. Such studies can also be used to prompt our continued consideration of how to make meta-ethnography more qualitative, interpretive and critical. Methodological studies can force us deeply into our practices and the reasoning behind our practices. They can reveal our assumptions and check if these assumptions actually derive from our intent to be interpretive or critical or from some other source. For example, I above noted I am ‘sometimes’ in favor of systematic exhaustive searches. The reason I say sometimes is that I am concerned that such searches may turn out to a methodological slipup—which helps set up the last section to this paper.

Rethinking systematic search in meta-ethnography

Clearly, systematic, exhaustive search is only one of the ways people find the studies they wish to synthesize. But it is an example of the types of processes that we should carefully reconsider—and ask how is this a qualitative process? One could argue that a systematic, exhaustive search process is akin to a survey research logic—in that being systematic and exhaustive leads to some belief that we have the population of studies and then any findings are (statistically?) significant and somehow generalizable (note definition of this term is problematic). But in qualitative research the goal of having all possible scenes available for study has never been seen as a driving logic. Rather the goal is to find out what is

interesting in the scenes we have, and what they offer. We push our investigation into the scenes we have as the first move and then compare across scenes as a second move (if at all).

Let me suggest that maybe we should consider other logics than the survey –getting all the cases—and do something along the way to getting the cases that allow us to say something. In a future work I hope to expand on this and be more creative about it but for today let me use an article by Mario Small to suggest some alternatives. I am using his work here as an analogy. He was working on a critique of how we think about case study selection arguing that all too often qualitative researchers face a dilemma: “whether to emulate the basic principles in quantitative *social* sciences in establishing evidence for qualitative work” or not (Small, 2009: 6). To my way of thinking, Mario Luis Small leads us to the idea that we should be less controlled by systematicity in the search process and more invested in doing with what qualitative research does well.

He notes that reviewers for qualitative studies are often subject matter experts and not on qualitative methods per say. Thus the reviews end up forcing qualitative studies into the logics of quantitative research with an explicit concern that they should address quantitative research concerns of representativeness and generalizability as well as serve to illuminate and/or support the generalizations of quantitative research. This of course is the status of second class citizenship in science—and of course denies the possibility that qualitative research does something of its own that cannot be done in quantitative studies—for example, develop insights, identify processes, illuminate cultural beliefs and practices, and develop theoretical models, He argues that qualitative studies will never be able to effectively respond to issues of representativeness and generalizability in large part because they are built into quantitative logics and thus we have to accept those logics to deal with them. He terms the attempts to deal with these issues as a “form of imitation grounded in language, or, more precisely, the adoption of words with only a superficial (and at times incorrect) application of their meaning.” (P. 10). He sees the solution as “involving developing alternative languages and clarifying their separate objectives, rather than imitating the languages of classical statistics for problems to which it is not suited.” (p. 10). He notes that having a set of cases that cannot be seen as representative “is not a ‘bias’ problem but a set of cases with particular characteristics that, rather than be ‘controlled away’, should be understood, developed, and incorporated into (her) understanding of the cases at hand” (p. 14). I refer to this with my doctoral students as understanding what you have a study *of*, rather than what you wanted the study to be about. I call this problem the ‘overzealous affinity for initial research problem’ but he is speaking more about avoiding a quantitative logic when doing a qualitative study.

He notes, and I want to make much of this, that qualitative research instead of being derailed into representativeness can uniquely contribute the ability to “uncover mechanisms and tracing processes” (p. 22) in a scene. He argues for example that one effective strategy is to avoid representative cases and go for unique cases—the rare reveal relations and connections in ways the average case may not. Hold ‘relations and connections’ in your head as we move on—it is a theme for me. But he says finally that there are really only three possible solutions to the dilemma above: 1. Ignore the problem—and I think there is reason to say this is the basic approach not just one option, one has to ignore the quantitative considerations as a place to start; 2. To qualify the work as provisional or ‘hypothesis generating’; and 3. conceive of it from a different perspective and language of inquiry. He argues the 2nd is simply a reactive position and not epistemologically grounded. But the third has possibilities.

He offers two alternatives: extending the extended case study logic and 'sequential interviewing' (in his work on case studies but remember the analogy is to how we do meta-ethnography).

Extending the extended case study

He builds on and critiques Burawoy's (1998) approach and that of the Manchester School of Gluckmann and others (cf. Mitchell, 1983). Put simply the extended case method does not work across studies but from a study to its contexts "which examines how the social situation is shaped by external forces" (Burawoy, 1998: 6). Instead of statistical significance it seeks societal significance. "...It tells us about society as a whole rather than about a populations of similar cases" (Small, p. 20). It refines or reconstructs a theory rather than identifying an empirical fact" (Small, p. 21). It produces and improves theories and suggests that unique and deviant cases can be beneficial in this in particular because they reveal the assumptions in the usual cases. The Manchester School also adds time into the equation of mechanism. "... to trace how events chain on to one another and how therefore events are necessarily linked to one another through time" (Mitchell, 1983: 194) --again uncovering 'mechanisms'. In this, "...extrapolation is based on the validity of the analysis rather than the representativeness of the events" (Mitchell, 1983: 190) [note using representativeness here as 'empirical fact'] where validity is about "the process by which the analyst draws conclusions about the essential linkage between two or more characteristics in some explanatory scheme" (Mitchell, pp. 199-200). (This has similarities to Geertz's (1973) claim that interpretation is akin to clinical inference.) Extrapolability is a logical inference not a statistical one. A single case study can "justifiably state that a particular process, phenomenon, mechanism, tendency, type, relationship, dynamic, or practice exists" (Small, p. 24).

For meta-ethnography we could adapt this logic and give up the search for 'like cases' in favor of ones that are nested others (a health practice, nested in professions, organizations, health policies, social understandings of health, economies, etc.). Such meta-ethnographies would provide unique theoretical and empirical contributions—and be quite different from that we offer now with exhaustive searches for 'like cases'.

Small's second language alternative is sequential interviewing. Those of you who do interview studies will recognize this as case study logic as opposed to sampling logic. "Case study logic proceeds sequentially, such that each case provides an increasingly accurate understanding of the question at hand" (Small, pp. 24-5). This works in such a way that the last case yields little new information—saturation has been achieved. Small argues that "...case study logic is probably more effective when asking how or why questions about processes unknown before the start of the study" (p. 250).

For meta-ethnography, this is often how we read the studies but a lot gets lost along the way. When tend to read for themes for example rather than tracking the emerging and developing understanding or interpretation that is the whole account offered in the study. For example, each study would lead to refinement of the mechanism that holds all the themes together rather than simply add new themes. This would help meta-ethnographies focus on the full storylines being synthesized rather than on a list of themes.

In any case, my point is not that this is the way we should go now. But rather that this is the kind of thinking that meta-ethnography would most benefit from us doing. We need methodologists thinking about how meta-ethnography is qualitative, interpretive, critical, post-critical, feminist, critical race and so on. We need to conduct methodological studies to see what difference different approaches make.

An ending

This talk has been about the history and future of meta-ethnography. It's coverage of these is at best episodic rather than thorough. It is meant to provoke thinking and even controversy. Writing a formal conclusion and tying things neatly together would vitiate those goals. So I shall end with some quotations that resonate with some of my remarks. These are from Peter van der veer (2014) and all are about the act of comparison:

the purpose is not to come to some general truth, but to highlight something that is not general, something specific without any pretense to general truth, but definitely of broader significance. (p.2)

What has to be curbed is the quite understandable desire to say something general about, say, religion as a universal entity (as a cultural system) or a particular society's religion in general (as in 'the religion of Java') or about the general and comparable features of a world religion's manifestations in different societies ('Islam observed'). The move from fragment to a larger insight is a conceptual and theoretical one and not a form of generalization. It does not come from mere observation, but is theory laden. Theory should here be taken in its original sense of observing and contemplating. (pp. 2-3)

... conceptual engagement in which translation plays a central role. (p.3)

Comparison...as a reflection on our conceptual framework as well as on the history of interactions that have constituted our object of study. (p. 4)

Comparison is thus not a relatively simple juxtaposition and comparison to two or more different societies, but a complex reflection on the network of concepts that underlie our study of society as well as the formation of those societies themselves. It is always a double act of reflection. (p. 4)

We turn a critical eye on universal pretensions of models that are solely based on a putatively isolated Western experience. (p. 3)

These quotes reverberate in my brain when I think about meta-ethnography. Meta-ethnography can ask much more of us as scholars than is the current practice. It can also remind us that meta-ethnography is implicated in wider social forces. Van der veer argues that what the conceptual models we create are not universally applicable (regardless of our methodology) but rather "are *universalized* and thus have universal impact to the extent they are backed by global power" (p. 4). What we does matter and in ways we may not like. I hope this paper keeps you thinking—I know it spurs my mind.

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