

UKLEF e-seminar 1: Historical bodies and historical space

Presenters: Jan Blommaert (Tilburg University/University of Jyväskylä) and April Huang (University of Jyväskylä)

Discussants: Mark Allen Peterson (Miami University) and Adrian Blackledge (University of Birmingham).

24 February – 5 March 2010

On Wed, Feb 24, 2010 at 10:23 AM, Tom Van Hout <tom.vanhout@gmail.com> wrote:

Dear all,

Welcome to the first ever UKLEF e-seminar. During the next 10 days (24th February - 5th March, 2010), we'll be discussing a paper by Jan Blommaert and April Huang entitled: "Historical bodies and historical space".

Jan is Professor of Linguistic Anthropology at Tilburg University and Director of the Babylon Research Center. The full paper is [available here](#).

The discussants are Mark Allen Peterson and Adrian Blackledge. Mark Peterson is associate professor and acting chair of the Anthropology Department at Miami University. Adrian Blackledge is Professor of Bilingualism at the University of Birmingham.

The seminar will start with our discussants' comments posted to the list. The presenters will then respond to those comments, after which the discussion will be open to the floor for further questions and comments. To post your thoughts, simply write directly to the list (LING-ETHNOG@JISCMail.AC.UK).

Best wishes,

Tom

On Wed, Feb 24, 2010 at 10:32 AM, Tom Van Hout <tom.vanhout@gmail.com> wrote:

Here are Mark Peterson's comments on Blommaert & Huang. Adrian Blackledge will post his comments later today.

Tom

Theoretically Sophisticated Ethnographies: A Commentary on Blommaert and Huang

Mark Allen Peterson
Miami University

I want to thank Tom Van Hout and the other organizers of this e-seminar for inviting me to take part, and to thank Jan Blommaert and April Huang for offering such a thought-provoking paper for me to comment on.

In what follows, I want to first consider the meaning of Blommaert and Huang's term "theoretically sophisticated ethnography". I then want to pursue their exploration of the Scollons' theorizations of ethnography beyond its virtues in countering the sin of synchrony to discuss how it might be used to also undermine another linguistic sin, that of assuming (and asserting) the coherence of semiotic systems.

Good ethnography involves thick description—detailed accounts of the movements of bodies and objects and voices in and through time and space—combined with thick contextualization—layers of wider histories, spatial classifications, categories of persons, ideologies and cosmologies that the elements in the description point to and which give them relevance, meaning and agency for the participants. These descriptions, however naturalistic, and however much the events described may be public, are always a result of ethnographers in interaction with interlocutors, interactions themselves mediated by objects and produced in the same time-space as the events recounted.

Ethnography has become popular, as Blommaert and Huang suggest, in part because it is a method of research into human action that promises to deliver new information and insights. This promise stems from two elements. First, ethnography assumes that we can learn *from*, and not only *about*, the people we study. Second, ethnography is an open-ended method in which theory is generated in part as the ethnographic process itself proceeds. It presumes less than many other methods, so it can learn more. On the other hand, it is for these same reasons time-consuming and extremely inefficient (in the sense that the ethnographer inevitably collects far more information than might seem needed for the questions being asked).

There are roughly four things you can do with ethnographic description as you turn it into analysis. First, you can mine it for discrete examples of some larger social process you wish to assess; that is, convert ethnographic material into illustrations of the steps in an

argument. You can, in other words, use it anecdotally. Second, you can draw on your ethnographies to make broad generalizations expressed as laws or regularities. This is the classic movement of positivist science applied to social action, to extrapolate from multiple examples situated in time and space to generate synchronic models. Third, one can deny the possibility of structural and systemic regularity, and insist on the utterly local character of all knowledge (“local” in each case being itself a construction of the ethnographic imagination). A fourth is to generate theory from the uniquely situated ethnography to structural and systemic regularities of social communication. It is this latter Blommaert calls “theoretically sophisticated ethnography.”

I am not quick to reject any of these approaches. Whatever critiques may come from others about ethnographic methodologies—and I’ve heard many of them as I sit on interdisciplinary dissertation committees—all of these uses of ethnography have their place. For example, even ethnographic anecdotes—brief thick descriptions of events or situations—are useful as counteranecdotes to the more superficial journalistic anecdotes mined by public intellectuals the likes of Thomas Friedman.

That said, I generally agree with Blommaert’s characterization. A shift toward creating more complex theoretical toolkits rooted in the practice of ethnography itself (or is it the practices of ethnography themselves?) can only be a good thing. I think this primarily involves a shift toward a more sophisticated theorization of what we have traditionally called “contextualization”. Instead of arbitrarily layering political, economic, social or historical contexts onto ethnographic data, a theoretically sophisticated ethnography would seek to explore the ways the ethnographic situation—with its nexus of bodies and objects interacting in a space--indexes larger contexts.

The focus here, and in the work by Ron and Judith Scollon on which Blommaert and Huang focus their commentary, is on ethnography of communication, whose greatest strength within the larger world of ethnographic research has been the capacity to relate micro-level description to macrosocial forces. In addition to nexus analysis, the rise of interest in linguistic performance, indexicality, and fractal recursivity all represent efforts to create broaden and deepen the theoretical repertoire in ethnography.

In a way, Blommaert and Huang’s reading of the Scollons’ work offers us a very Malinowskian approach—a more deeply theorized version of the model Malinowski put forth in the classic *Coral Gardens and their Magic* in which the “context of culture”, the cultural knowledge an individual possesses as a result of their membership in an intercommunicating society is used to define and interpret and organize action in a concrete situation—a place with bodies and objects and actions unfolding in time.

A crucial difference—an important advance—is that Blommaert and Huang, and the Scollons, like Bourdieu, want to locate the context of culture not in the mind—that is, as systematic knowledge that precedes action—but in the body, one of the agents that acts. In doing so, these authors not only emphasize materiality, as Blommaert and Huang point out, but they bridge the heuristic gap between the context of culture and the context of situation by relocating the context of culture *in* the context of situation, as part of the

material mix of bodies, places and objects, all of which have agency in any given meaningful situation. This attention to embodied knowledge requires us to pay close attention to the many physical details through which cultural knowledge is simultaneously made and expressed.

It strikes me that one need not altogether reject mind in this formulation. While rejecting, or at least significantly reducing the role of cognition—which is to say the whole notion that cultural knowledge in the form of codes or laws or scripts carried in the brain *precede* action, so that action is a carrying out of pre-existing cultural ideas—one might fruitfully consider the role of Mind in a Batesonian sense, an approach that locates Mind *in* the interaction of bodies and objects in space. For it is a crucial aspect of the semiotic processes described by the Scollons and by Blommaert that they are also cybernetic, which is to say *learning* processes. There is a significant feedback loop in the creation of habitus, embodied knowledge, or enskillment. When one enters a nexus and events go well—for example, all the little microprocesses involved in the student handing in his paper to the teacher operate correctly—there is a reinforcement of habit, a deeper sedimentation of embodied knowledge. When events go awry, much more is signaled that embarrassment. A semiotic *breach* occurs that must be repaired.

The paired concepts of breach and repair could, like interaction order, be usefully borrowed and amended from Goffman (Frame Analysis). The notion of repair takes us beyond habit into a potentially active process involving assessment, interpretation imagination, and creativity.

Consider again the example of the student turning in his paper to the professor. Eyes meet. The student begins pushing the paper forward. The professor's hand comes up to meet it. Each actor is reading the microsignals correctly. But something goes wrong. The paper falls to the floor. The other students, recognizing that a breach in the interaction order has occurred, begin to giggle. Their giggling causes embarrassment. The faces of the two men turn red, perhaps causing the giggling to increase. Now the two bodies begin to bend or crouch to recover the paper. Their movements are awkward compared to the smoothness of the paper-handing act that went awry. Each is trying not to bump or jostle the other, trying awkwardly to read ambiguous signals. No longer are they acting out of habitus; now they are trying to interpret what went wrong, to assess the awkward situation, to work out the best way to repair it. We are still within an interaction order, for such orders include rules for repairing a breach. But the universe of possible responses is suddenly much greater. Should the professor apologize? The student? Should the professor accuse the student of being clumsy? There is a myriad of possibilities, each pregnant with semiotic possibilities and, once enacted, consequences for future interactions between these agents.

What happens, though, when some aspect of the interaction order itself is breached? Let me offer an example also drawn from a U.S. college campus. A student is walking across the campus. She sees a friend a dozen yards off, and waves in a gesture of acknowledgment. It is a habitual gesture that ought to draw a habitual return wave because both students understand the codes imposed by this social space. But it does not.

The other student walks on without acknowledging the gesture. A breach has occurred with enormous potential consequences. On the one hand, the second student may not have seen the first. But on the other, she may have deliberately chosen not to respond, which has important semiotic consequences, implying a disruption of a social relationship.

The interaction order contains within it culturally appropriate strategies for repairing the breach. Among young female high school and college students in the U.S., the student who waved cannot usually directly ask her friend why she did not wave; she must rally a network of mutual acquaintances who will approach the friend and ask her why she failed to wave. They will report back: if the friend merely didn't see her, all is restored to equilibrium. But if the act is deliberate ("because I saw her with my boyfriend last night"), a great deal of social energy is expended as members of the mutual social networks interpret the situation, take sides, suggest strategies and so forth. Habitus plays a role, but there is also a great deal of overt strategizing, interpretation, and creative exploitation of ambiguity that occurs when there are breaches in expected outcomes of habitual semiotic actions.

This brings us to the main point I wish to make here. Synchrony is not the only sin of classical linguistics. Another is emphasizing the coherence and boundedness of sign systems, or rather the semiotic capacities of all actors entering into communicative spaces. Blommaert and Huang's exploration of the Scollons' work, including the examples offered, seem to assume homogenous intersemiotic communities of persons with similar life experiences and hence similar habitus such that the activities in a nexus occur almost automatically. It is put most succinctly when Blommaert and Huang write:

All of us have acquired the codes valid in such micro-spaces, and all of us are capable to shift in and out of such codes when we enter and leave such spaces. The next space will impose different codes, and again we will be familiar with them.

This assumption of the familiarity of all actors with the codes imposed by the spaces into which they enter is present throughout this essay, and I think it underestimates the capacity of theoretically sophisticated ethnographies to contribute to understanding communication and cultural learning in the increasing intercultural relations spawned by contemporary global flows.

I am particularly interested in the potentialities of nexus analysis for analyzing situations in which similar prior experiences do *not* shape the embodied knowledge of interactions. What happens when historical bodies enter into agentive spaces where the body histories and spatial agencies are not congruent?

Let me illustrate what I mean by offering a counterexample to the illustration offered by Blommaert and Huang at the end of the essay. Dr. Blommaert enters into the crosswalk, a space with a particular history which serves as a key context for understanding the open palm as a gesture of thanks to the automobile that stops at the crosswalk. This interests me because the gesture takes on very different, yet related meanings in the United States,

where pedestrianism is rarer and the meanings of crosswalks are usually secondary to the streetlights or stop signs that regulate traffic within and around them. Here, the open palm gesture is more often used by automobile drivers. It may be used as a gesture of thanks to the driver who lets another merge into traffic in front of him, or sheepishly, if a car accidentally cuts off another driver, who has to slam on her brakes to avoid a collision.

When I lived in Cairo, I engaged in far more pedestrian activities than in the U.S. In most of Cairo, pedestrian crosswalks are not respected, as Blommaert's example indicates they tend to be in Antwerp; nor are stop lights and stop signs, as they would usually be in the U.S. As I would pick my way through slow moving traffic to cross streets at crowded Cairo intersections, I would frequently flash this "thank you" gesture, physically identical to Blommaert's but produced by a different habitus, at cars who were forced to slow their forward movement or even stop to avoid hitting me. Although I had only used this gesture as an automobile driver, never as a pedestrian in the U.S., my embodied knowledge interpreted the situation as one that required the gesture.

All around me other Cairenes, some in galabiyyas, some in business suits, were engaged in the same process of crossing the street but without any such gesture. Most watched the automobiles progress with a calculating eye toward the intersection of movement, time and space, but there was no effort to interact with the drivers of the cars. Indeed, one day a handful of students crossed the street with me and afterwards mocked my gesture, asking if I was going to hold the cars back with my bare hands. Rather than a gesture of thanks, or even an imperative ("Stop!"), they read it as an act of foolishness, an indication of my fear expressed though an instinctive but impossible effort to hold the cars back, perhaps analogous to the (more effective) use of the open palm to ward off the evil eye. My students' metacommentary led me to greater alertness to the local interaction order, and my use of this gesture ceased.

These examples are intended to draw our attention to the way that embodied knowledge *gets* embodied, as a continually ongoing process of reinforcement, or of breach → repair → learning. Theoretically sophisticated ethnographies of the sort Blommaert and Huang are celebrating are sensitive not only to the fact that "each act of communication is at once exceptional and typical, that it always consists of completely new forms of patterning and organization, while it derives its communicability from sharedness and recognizability of patterns" but also that this very sharedness and recognizability derives not from a prior historical process of learning but from an ongoing but historically shaped process of learning.

In sum, a theoretically sophisticated ethnography of the sort advocated by Blommaert and Huang, that is at once sensitive to the historical process of the embodiment of knowledge *and* the agentive, non-neutral nature of space offers us not only a way out of the synchrony implicated in "The Bamileke are matrilinear" but also the implication of intersemiotic coherence. Rather, it offers a theory of learning, of ongoing processes of interpretation, embodiment and habituation, but also breach, reinterpretation and knowledge production.

On Wed, Feb 24, 2010 at 1:49 PM, Adrian Blackledge <a.j.blackledge@bham.ac.uk> wrote:

UKLEF e-seminar

A response to Jan Blommaert and April Huang

Adrian Blackledge, University of Birmingham, UK

I welcome this invitation to respond briefly to this stimulating article, which takes as its point of departure the sophisticated ethnographic work of Ron Scollon and Suzie Wong Scollon, and in particular their books, *Nexus Analysis* (2004) and *Discourses in Place* (2003). Blommaert & Huang pick up on the Scollons' notions of the 'historical body' and 'historical space', which propose that the historical dimension is of major importance in ethnographic work. In the few words available here I aim to raise questions to provoke further discussion about how we are able to identify and explain empirically the 'historical body' and 'historical space'.

First, how do we identify and explain the historical in the body? There can be little doubt that different people have different "unconscious ways of behaving and thinking" (Scollon & Scollon 2004:46). Blommaert and Huang summarise this concisely in referring to "bodies as repositories of histories and experience" (p. 14). Another sophisticated ethnographer, Pierre Bourdieu, argued that history is incarnated in the body. For Bourdieu "habitus, the product of a historical acquisition, is what enables the legacy of history to be appropriated" (2000:151). More precisely, it is the "deep-rooted dispositions of the bodily hexis" (1991:88), rooted in posture, "a way of bearing the body" (2000:144), that articulates a more-or-less unconscious sense of 'how to be' and 'how to behave' in a social arena. Scollon & Scollon similarly propose that "a lifetime of personal habits come to feel so natural that one's body carries out actions seemingly without being told" (2004:13). They acknowledge a debt to Bourdieu's notion of habitus, but prefer 'historical body' "because it situates bodily memories more precisely in the individual body".

Second, how do we identify and explain the historical space? Scollon & Scollon took 'space' to be historically patterned, and an actor in semiotic processes. Blommaert & Huang point out that it is the connection between space and normative expectations that makes space historical, as these expectations "have their feet in the history of social and spatial arrangements in any society" (p. 10). Scollon & Scollon argue that visual semiotics and the semiotics of the discourses in place have "a materiality and a history" (2003:206). Bourdieu found metaphors from the worlds of sport ('field') and economics ('markets') to be helpful in explaining the interaction of the historical body with the historical space. For Bourdieu, in fact, it is not possible to speak of the body without at the same time speaking of the world with which it interacts. It is this ethnographically observable "encounter between the habitus and a field, between incorporated history and an objectified history" that is the object of analysis for both Bourdieu, and for Scollon & Scollon. In *Discourses in Place* Scollon & Scollon point to the convergence of the

histories of four meaning systems: (i) the habitus of the social actor (ii) the interaction order (iii) visual semiotics, and (iv) the place semiotics of the discourses in place. Analysis of this convergence of histories in interaction offers a focus of analysis. For Bourdieu, the demands of the 'field' are easily met by those who are born into the game, and therefore have a "feel for the game" (1990:66). When habitus is produced by experience of the game, and the game is 'natural', or 'sensible', those who have a feel for the game are more likely to succeed. When habitus is not born into the game, and is uncertain of the rules of the game, success is not guaranteed.

However, none of this big theory takes us closer to a practical method to interpret the historical body in historical space. I will very briefly refer to two examples. Scollon & Scollon, and Blommaert & Huang, refer to the 'interaction order' of the university lecture hall. Scollon & Scollon found in their research on educational equity in universities in Alaska in the 1980s, that some students in the classes they observed "have little or no knowledge or experience of the discourse practices of the university argumentative or essayist type" (2003:48). There were normative expectations in the university: an interaction order which falls into place because that is how it is historically constituted. For some of the students the experience of being at university may be akin to being born into the game. When habitus encounters a social world of which it is the product, it is "like a 'fish in water': it does not feel the weight of the water, and it takes the world about itself for granted" (Bourdieu and Wacquant 1992:127). This *practical knowledge* may be described as having a 'feel for the game' without really knowing how or why one has a feel for the game. The rules of the game were learned as part of the dispositions of the habitus. All is well in the relation between habitus and the social world, as long as social actors have a 'feel for the game', or are as 'fish in water'. Conversely, then, for other students the academic experience is akin to being a 'fish out of water', as the interaction between the historical body and the historical space is anything other than an "almost miraculous encounter between the habitus and a field" (Bourdieu 1990:66). In their ethnographic study, Scollon & Scollon found that some Alaska Native students did not meet the normative expectations of the university, and were more likely to drop out. Their point here is that the nexus of the students' historical body and the historical space of the university did not constitute a miraculous encounter; the students did not feel as fish in water. And this not-so-miraculous encounter was visible "in the moment-by-moment discourse of university classes" (Scollon & Scollon 2004:117).

In a second example, Bourdieu's ethnographic study of enforced bachelorhood in traditional French rural society (reproduced as *The Bachelors' Ball*, 2008) was one to which he frequently returned. In Bourdieu's detailed observations of the Christmas Ball in the back room of a café, we encounter in close-up the historical space of the Bearn, and the historical bodies of the participants. For the young women, fashionably dressed and coiffured, all is well in their interaction with the social space. Among a group of young men, army conscripts, townsmen, and clerks, there is a self-assurance and bodily confidence. Standing in the shadows at the edge of the dancing area is a third group, older men, not dancing, yet edging forward towards the dance floor. They will not dance. When one is invited to do so he exaggerates a clumsiness and heavy-footedness with which this group of older bachelors is typically associated. Bourdieu's point here is that

“bodily hexis is a social signum” (2008:85), as one’s ‘demeanour’ is immediately perceived by others as a symbol of economic and social standing. The bachelor peasant is “embarrassed by his body, and in his body” (86). These are men forced into bachelorhood by the *historical space*, constituted as a combination (a nexus) of traditional marriage rules, economics, and family tradition, and the *historical body*, which is clumsy, ill-attired, and *n’ey pas de here*. In Bourdieu’s ethnography, for sure, we see clearly the encounter between the historical body and the historical space.

Finally, I concur with Blommaert and Huang’s summary that every aspect of observable practice is historically loaded, and drags with it those histories. We need further studies to exemplify just how we go about observing the historical in the interaction of body and space.

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On Thu, Feb 25, 2010 at 12:45 PM, Michael L. Bakalinsky <m.bakalinsky@mail.ru> wrote:

Dear all

My response to Jan and April's paper attached, and pasted below.
Michael L. Bakalinsky

UKLEF e-seminar

A response to Jan Blommaert and April Huang
Michael L. Bakalinsky, Zaporizhya National University, Ukraine.

I am very privileged to send my comment to the article under discussion.

I find the work of Blommaert and Huang indeed interesting and thought-provoking as well. The authors are focusing on the issue that is on the agenda in the word of arts. As the criminal anthropology expert, I find the statement that "one of us was recently invited to write a piece in which ethnography figured prominently for a methods handbook in criminology" is quite to the point. One more passage that is worth citing is as follows: "ethnography to move from the uniquely situated events it describes to structural and systemic regularities in interpretation. This has implications for ethnography, to be sure, but also for a broader field of studies of human conduct, including linguistics and sociolinguistics". It might finally take ethnography to the place it belongs to, i.e. into the cohort of equally-valued social studies, and might as well give keys to scholars doing research on linguistic anthropology to solve the dilemma of the XX-century anthropology, i.e., to make a clear-cut formulation of language study role in culture research.

However, on starting analyzing the paper mentioned, the lines as follows arrested my attention: "+ ethnography has always had a doubtful reputation as well. It [ethnography] was under-theorized+". This is where I am fully disagree with the honorable authors of the survey. I think that it is at least impolite to say that ethnography is under-theorized, for in this case the ethnography as a science does not exist (a universal scientific axiom states that to be called "SCIENCE", a schooling requires methods and theory!!!). A question may rise: how do Blommaert and Huang understand the very concept of ethnography? According to a generally accepted standpoint, ethnography is the study at first hand of living cultures, and together with ethnology it comprises anthropology, either cultural or social. If they follow the definition mentioned, than how come do the respected authors did not take into account the works of the Fathers of the British anthropology, both the XIX-century (Tylor, Lang, Frases), and XX-century ones (Radcliffe-Brown, Malinowsky)?

I strongly recommend the distinguished authors Blommaert and Huang to re-formulate the statement about the so-called "under-theorized" nature of ethnography, for it not only underestimates the works of the founding fathers of this science, and deprives all its advocates from the right to be called scientists (not just story writers).

On Thu, Feb 25, 2010 at 3:21 PM, Mark Allen Peterson <petersm2@MUOHIO.EDU> wrote:

The question of ethnography's theoretical sophistication is complicated in that ethnography has become something of a portmanteau word that means many different things to many different people. For some early cultural studies types, ethnography meant virtually any effort to move outside a text to data suggesting what readers might think about the text. Those of us who work frequently with interdisciplinary committees often find either very squishy notions of ethnography when we work with humanities scholars, and disdain for ethnography by hard science types.

Then there is a distinction between people who regard ethnography as a method of data gathering and writing quite apart from theory and analysis, on the one hand, and people who look at theory as something that arises from the ethnography.

In my comments I was careful to explore some of these points. I took Jan and April's point to be that ethnography exists in a continuum between more and less sophisticated forms, and that for them, theoretically sophisticated ethnography is that which both a) treats theory as arising from the ethnographic process and b) makes some of the processes by which this occurs explicit. In their paper, they hold up the Scollons as exemplars of this.

It will be interesting to hear what Jan and April have to say on this point.

Mark

Thank you Mark and Michael. I'd now like to invite Jan and April to post a brief response, after which the floor will be open to all.

Best wishes,

*Tom
e-seminar moderator*

On Fri, Feb 26, 2010 at 9:01 PM, Tom Van Hout <tom.vanhout@gmail.com> wrote:

Here is Jan's response to the ongoing discussion about theoretical sophistication in ethnography. The floor is now open to everyone on this list.

*Best,
Tom*

Dear all,

I wish to thank Tom for setting up this e-seminar, and I am very happy with the comments given to the paper I co-authored with April Huang. April herself missed the e-seminar: she is in Taiwan now preparing for her marriage. So her voice will remain distant in what follows.

Let me comment on some of the points that emerged from the discussion here, and provide some clarification for some of the points April and I made in our paper.

1. The paper was driven by the assumption that ethnography is a theory, to be more precise a theory of situated (contextualized/reflexive) knowledge production on situated (contextualized) social objects. This, I hope, should have been more than clear from the paper itself (as well as from other writings by myself). When in our paper we said that “ethnography was under-theorized”, we were referring to the views of others, of the many people who find that ethnography has a doubtful reputation (the kinds of people Mark encounters on interdisciplinary research meetings). I hope thus to have answered Michael’s objection: ethnography does have a doubtful reputation in the eyes of many, including several prominent ethnographers, because of its perceived lack of theoretical depth. Our attempt, however, was to argue that such theoretical depth could indeed be found, provided one engages with the right arguments and tactics of ethnography and takes them into the practice of theorizing. It always strikes me that few people actually do that, and that so many people deeply involved in ethnography possess only clumsy arguments in their own defense. There is very little epistemological and methodological debate going on, e.g. about the status of ‘facts’ and ‘knowledge’ in ethnography – very little profound reflection has taken place since George Marcus’ powerful interventions in this field.

2. Someone who definitely theorized about ethnography (as part of a larger social-scientific edifice and program) was Bourdieu, and Adrian rightly points out the many points of similarity, expansion and commentary resulting from a confrontation of Bourdieu’s work and that of the Scollon’s. In 2004, I put together a panel called ‘Bourdieu the ethnographer’. Papers in that panel engaged with Bourdieu’s explicitly ethnographic theoretical statements. In spite of what I found to be a quite compelling case in our favor, several very distinguished US anthropologists afterwards commented that they were still convinced that ‘it was a stretch’ to call Bourdieu an ethnographer, because he was ‘a structuralist’. Implicit here is a denial that ethnography could be structuralist, or

could (in a more general sense) speak to social structure and tell us something, for instance, about the actual nexuses of structure and agency. Both the body and space, to April and myself, can be seen as clear and obvious sites where structure and agency intersect. The body by means of its historical ‘load’ as well as because of the synchronic (but structural, i.e. having a historicity of their own) learning processes described by Mark; space because of its non-unified, heavily segmented and demarcated nature. In both areas – the body and space – we emphasized the crucially normative dimensions that characterize these objects, and we believe that normativity is precisely the site where intersections of structure and agency can be investigated. Mark’s points about this issue are well taken.

3. With respect to that, the following must be said. Mark takes issue with the statement “All of us have acquired the codes valid in such micro-spaces, and all of us are capable to shift in and out of such codes when we enter and leave such spaces. The next space will impose different codes, and again we will be familiar with them.” Mark takes this to be a suggestion of absolute sharedness of norms and the general coherence of social systems, while, in actual fact, it was part of a localized, situated description of the event documented in the paper – the crossing of the street. It is evident that norms (as orders of indexicality, i.e. implicit complexes of connections between signs and social frames) are among the most variable features of the social and cultural world. To the extent that our statements suggested a denial of this point, we hope to redress that here. In fact, much of what Hymes called ‘narrative inequality’ revolves around the lack of fit between expected behavior and produced behavior in specific social and cultural arenas. That is: it revolves around differential access to particular orders of indexicality, and this is an effect not just of difference, but most often of inequality.

4. What has been said in point (1) above can be extended in more general terms as follows. The theoretical richness of ethnography depends on the ways in which an ethnographic epistemology can be blended with methodological insights from elsewhere. Bourdieu’s work is exemplary in this sense: starting from a profoundly ethnographic epistemological point of departure, he brings questions into methodological fields of a wide variety of sorts (statistical surveys being among the most visible, but one should not overlook the neo-Marxist dimensions of his work either). It is, however, not the case that anything goes. Ethnography has a materialist and realist foundation, and those deep-theoretical options push us towards some frameworks and not towards others. Mary Scott recently asked me about the difference between ethnography and Gunther Kress’ social semiotics. I would say that here, too, ethnography has everything to win by keeping its horizon open. Kress’s social semiotics starts from fundamentally commensurable questions, and takes language (or rather: semiotic) data in a theoretical field of force that combines excellently with that of ethnography. In the other direction, I am very hesitant about cognitivist or mentalist models of human action. As Mark rightly points out, if ‘mind’ has to be made useful to ethnographic research, then it has to be in its Batesonian (or Vygotskian) interpretation. Note that this is not a rejection of ‘mind’; it is a rejection of cognitivist and mentalist concepts of mind. I am also doubtful whether the anthropology of people such as Tylor and Frazer has much to offer, methodologically, to its ethnographic successors. I believe Johannes Fabian has written the conclusive funeral

march for that kind of anthropology, and Geertz, Sahlins, Clifford & Marcus and many others have added variations to it.

In conclusion: I hope that these reflections on the theoretical status of ethnography can be continued. As mentioned above, there is not much of it around. The effect of that is, unfortunately, that we keep getting flak from others because we look under-theorized, anecdotal and idiosyncratic. We need to work on the arguments we have in our own favor. Thanks to all the commentators for their generous and insightful comments. And let's have more of this.

Yours, Jan

On SAT, Feb 27, 2010 at 7:52 PM, Stephanie Kent <stephanie.kent@FULBRIGHTMAIL.ORG> wrote:

Hello everyone,

I'm very excited by the paper and discussion so far. I'm also (somewhat) surprised that the focus has landed upon the theoretical sophistication of ethnography instead of on what struck me as the most significant point presented by Blommaert and Huang, which involves the ramifications of confronting "the elision of real time and real space from the purview of analysis."

Mark Peterson's assertion that theoretically sophisticated ethnographies have tremendous "capacity...to contribute to understanding communication and cultural learning" is (to my Batesonian mind!) the whole point. Everything else, while interesting, is a distraction from the power of ethnography to act as an intervention in the steady-state perpetuation of inequalities. I realize this is a strong statement, and I do not mean to disregard the range of conversations seeded and thus possible from everyone's contributions so far. However, I suggest that one of the reasons ethnography is regarded ambiguously is because we have not rigorously demonstrated its time-based power.

The reason that I qualified my surprise (above) is because the responses are indicative of the norms expected among academics engaging a theoretical topic. If we look at ourselves as 'in' a nexus, then we can witness ourselves reading and engaging ideas in this "space" at this "time" according to a familiar mode of discourse. (Even, if I can test my comprehension, a particular interaction order.)

"Space" and "time" are themes that Blommaert and Huang explicitly name as crucial, component parts of theoretically sophisticated ethnography. I'm fascinated by the way in which "time" then disappears from the example, remaining obscurely only through references to "history." I am probably confused on the meanings of "synchrony" and "diachrony" - but as far as I can tell, the example of the zebra crossing is as delimited in time (to an eternal present) as it is bounded in space. The indexical qualities of semiotic behavior as acting upon the future are present mainly at the level of inference - and (if I dare say so), only if one is really seeking.

Keeping the movement of time open and available to perception and interpretation is a real challenge. It seems that any effort to describe or explain or illustrate any kind of semiotic behavior, such as a nexus, necessarily clips away time. This may be the most serious problem of representation: its apparent inability to retain dynamism. I wonder if we might - together - attend to the indexical qualities of this particular e-seminar interaction and use it (somehow, i.e., ethnographically) to reframe or pose a new object of study that would (as an effect) constitute theoretical sophistication for ethnography in terms that non-ethnographers can grasp.

best regards,
steph

On MON, Mar 1, 2010 at 12:47 PM, Ellen Van Praet <ellen.vanpraet@ugent.be> wrote:

Txs to Jan, April, Mark, Adrian, Michael and Stephanie for inspiring comments and contributions, and to Tom for moderating and setting up this e-zebra crossing, this virtual *micro-space with a particular interaction order*.

Because the time and space we share is limited, I will try to be concise. As an aside, these are (some of) the contextual conditions under which my comment is constructed: I have a fully intact set of teeth, and I am a post-doctoral assistant at Ghent University (Belgium) where I study (verbal and non-verbal) interaction at meetings from a linguistic ethnographic perspective, struggling to connect micro-level description to macro-social forces.

My question/comment is the following:

Is the danger of theorizing ethnography not that it will “freeze” an approach/method/paradigm at the time of observation? In their Manifesto for Ethnography (2000:5) Paul Willis and Mats Trondman set the tone for much of contemporary ethnographic writing and thinking. The opening paragraph of the article explicitly urges for “an open manifesto” and expresses the hope for it to encourage the production of a wide range of ethnographies, “thereby being developed, refined and criticized **without ever being locked up as a given system of thought**”.

In other words: is an attempt to lock ethnography up in ‘theoretical sophistication’ not incommensurable with its very ‘dialectic of surprise’ (Murphy, 1972:121)? In short: too close for comfort?

Admittedly, any paradigm strives for self-justification, for affirmation of its values, in other words for validity: “What does the group collectively see as its goals; what deviations, individual or collective, will it tolerate; and how does it control the impermissible aberration?” (Kuhn, 1962:209). We, ethnographers, are no different. There is no such thing as value-free science.

Almost ironically, trying to validate our research we try to provide a description of the “practices characteristic of a particular group of people” (Duranti, 1997:85): we try to pin down a community construct of values. Any attempt at theorizing ethnography should beware of the inherent dialogical, never-ending process of this endeavour? Simply because, as with any community construct of values, ethnography is 'a site of encounter', always in flux and changing?

All the best, Ellen

On TUE, Mar 2, 2010 at 10:07 AM, Tom Van Hout <tom.vanhout@ugent.be> wrote:

Dear list,

We are discussing Jan Blommaert & April Huang's paper "Historical bodies and historical space" which is available at:

<http://www.ling-ethnog.org.uk/documents/papers/blommaert2010.pdf>

There is still plenty of time for comments and/or questions. Please post directly to the list (ling-ethnog@jiscmail.ac.uk) or contact me off list (tom.vanhout@ugent.be) in case your post does not go through.

The e-seminar ends Friday, March 5.

All the best,

Tom

On TUE, Mar 2, 2010 at 6:36 PM, Stephanie Kent <stephanie.kent@FULBRIGHTMAIL.ORG> wrote:

Hello all,

I want to thank Ellen for her comments; they are akin to mine albeit more clear.

The opportunity to read and interact with everyone's thoughts has coincided nicely with some thinking I've been doing in the days following a presentation I just gave on 'paradigm consciousness' to the Translation Center at UMass-Amherst. A few people who were not at that talk have commented to the weblog entry I posted.

My responses to them are obviously informed - and improved - by the content of this e-seminar. (If you're curious, the immediately relevant one to this e-seminar is linked [here](#).)

I hope there's more to come before the seminar closes!

steph

On WED, Mar 3, 2010 at 12:18 PM, Peter Brannick < pjbrannick@GMAIL.COM> wrote:

Thank you all for your comments, I'm finding all this immensely helpful!

A point I'd like to return to (and would love to hear what others think) relates the deeper philosophical issues raised by Jan about the nature of knowledge, points I find deeply (and positively) challenging. For me they have all sorts of implications for questions like 'what is data?' and what do we mean by empirical study. Now before reading further you should probably know I'm a part-time PhD student about halfway through, so if I say anything that's perhaps a little naïve, be good enough not to remind me of it in a couple of years!

I can't help but agree that the lack of confidence felt within LE when confronted by our sceptics is compounded by the fact that we try to justify what we do using 'their' measures, instruments or conceptualisations.

I was rereading the Scollons' contribution to the Ethnog issue of the Jnl Sociolinguistics (2007). They make a point about Boas' efforts to use his science to combat rascism. The fundamental problem with the approach taken, according to the Scollons, was that he accepted the prevailing (Herdian) paradigms on race etc, thereby boxing himself in from the start.

I think we face a similar situation from a theory of knowledge perspective in LE. By accepting the underlying universally-accepted notions of knowledge we're boxing ourselves in. Although it's true that if you had three ethnographers in a room you'd have four opinions on what ethnography is, I think the commonality we share as LEers is a dissatisfaction with not just ways of seeing, but what constitutes the 'stuff' see, or what can count as 'stuff' to include in our research (see Rampton's LEF Position paper 2004).

Perhaps as Jan suggests (if I'm reading what he says correctly) the best way to respond to the usual (and sometimes quite tediously regurgitated) arguments is to question the long-assumed validity of the concepts and methods that were largely borrowed from the physical sciences in the first place to give social scientists some credibility. Jan's point is, to my mind, far more strategic and has far more potential. It's not just that we're not able to engage in such arguments effectively, but rather we might well be advised to turn such arguments into a dialectic on epistemological issues, because at the end of the day, this is the underlying question. The other sheer delight I derive from my research (those glimpses of paradise I'm rewarded with as I toil!) is the absolute questioning nature of LE that applies to everything we do, that is utterly disposed to take stock, as it were, of the direction of language/communication research. This of course has implications for other social sciences - but we'll leave that for another day!

I think simply to create research paradigms that 'look' robust as a defence against our critics is not enough and will always leave us looking thin (though I'm not saying they should be anything other than robust). Perhaps by raising our issues with 'conventional' models, and thus moving the locus of the argument away from what could almost be

considered superficial (ie surface), we might help advance our science and understanding of what counts as knowledge.

Peter

BTW if anyone's having probs from others regarding the 'credentials' or 'pedigree' of ethnography in general as a science, have a glance through Vermuelen's final thesis chptr (2008 - from Leiden http://media.leidenuniv.nl/legacy/vermeulen_summary.pdf). It makes interesting reading as he traces ethnography back to the early 1700s and German scholars working at the Russina Academy of Sciences, Uni Gottingen (sorry can't find the umlaut) and the Imperial Libray at Vienna.

On THU, Mar 4, 2010 at 1:25 PM, David Barton <d.barton@LANCASTER.AC.UK> wrote:

As a discussion paper for Linguistic Ethnography (LE) I found Blommaert & Huang's paper invaluable for many reasons. Most importantly, it provides a clear and insightful reading of the Scollon's contribution to the study of language, clarifying and linking several of their key terms. In this way it helps develop a broader language of description for LE. The paper and the comments are bristling with ideas, and I would just like to briefly comment on one aspect, the role of theorising. This is important because everything needs a theory, and when talking about theorising we need to specify what we want a theory of. Specifically, LE needs a theory of language, of language in the world, and it is that which the Scollons offer. A discourse analysis type theory of language is not adequate on its own as it grinds to a halt when confronted with materiality. A theory based only on spoken language is inadequate when encountering the contemporary textually mediated world including online worlds. LE needs a theory of texts and of practices and the Scollon's work helps cement ethnographic approaches and discourse approaches solidly together. However, I found that when discussing theory, the paper jumps back and forth a bit between focussing, on the one hand, on ethnography as an integral part of general anthropology and, on the other hand, on ethnography as the defining approach of the developing field of Linguistic Ethnography. Sometimes, it shows the richness of theorizing going on in many places, at other times it laments the lack of theorizing. I think that theorization is going on in many more places and one of the roles of LE is to bring these strands together more.

Also, in reading the paper it is important to remember that it was written as a contribution to a journal special issue devoted to Ron Scollon who died just over a year ago - and please note that Suzie Scollon is still working and writing, and contributed to the Language in the new Media Conference in Seattle last September.

Thanks again for this thought provoking paper,

David Barton, Literacy Research Centre, Lancaster University

And see Barton, D. & U. Papen (eds.) (2010 in press). The anthropology of writing: understanding textually-mediated worlds. London: Continuum.

On THU, Mar 4, 2010 at 3:52 PM, K H Pahl <K.Pahl@SHEFFIELD.AC.UK > wrote:

Hi everyone

I have been reading this discussion on the margins and have been unable to delete it as it is so interesting.

i would say, from my perspective that the paper has helped me not only theoretically but methodologically.

I am currently conducting a study on the Uses of Literacy in Rotherham, using an ecological perspective, drawing on Neuman and Celano (2001) and Nichols et al (2009).

This article helped me understand how historical space shapes literacy and language in use, specifically in one example, the re-building of an old swimming pool as a community library and its re-use by young people as a youth club for roller skating, craft activities and drama rather than a library.

By paying attention to historically situated spatial and linguistic interaction I could understand what was happening.

Language plus the material world, literacy as a materially situated social practice needs this theory to also enable a methodology that links things with people (language in use plus materiality) with history to make sense of things.

Thanks again

All good wishes

Kate

PS for more on people and artifacts as a methodology see

Pahl K and Rowsell J(2010 in press) *Artifactual literacies: every object tells a story*. New York: Teachers College Press

Thank you David and Kate. If anyone would like to follow-up on some of these issues or post a brief comment, now is the time. The e-seminar ends tomorrow (Fri 5 March).

Tom

On THU, Mar 4, 2010 at 9:34 PM, David Zeitlyn <d.zeitlyn@kent.ac.uk > wrote:

2 different responses to B &W

I too enjoyed the paper and the discussion, so here are my small contributions

Fabian synchronicity and generalisation

I don't want to get too drawn into the problems with Fabian's argument, especially since he provides an important corrective to styles of description which deny history and change. The problem is that his attack on asynchronous writing are taken to encourage those who refuse to generalise (the so-called crisis of representation). In this paper we see it in the quoted phrase "The Bamileke are matrilineal". Let me try and give an account of the sorts of socio-linguistic evidence which has been available for a century or more (centuries if you consider the locally available evidence) on the basis of which I think it is reasonably safe to assert the proposition "The Bamileke are patrilineal" (sorry I should explain that my research is primarily in Cameroon). It comes down to accountability in the ethnomethno sense. There are events, for example after a death in which inheritance and succession to titles are sorted out. My understanding is that if a man inherits from his father this is not seen as problematic and no account is given. On the rare occasions that a man inherits from for example his mothers brother this is understood locally as odd, unusual and requiring accounts explaining why it is happening. So conversational features such as repairs provide locally accessible and shared (thereby amendable to study) means by which an analyst can assemble evidence which may be summarised in generalisations such as The Bami are patrilineal (ideally with time limitations so perhaps we want to say 'The Bami have been patrilineal for at least the last 100 years').

My thought is that if Jan B had tried to cross the road in Jakarta he'd not get far with his gesture. There are locally understood conventions of how to cross roads (which he recognises) but one of the ways in which Fabian is deployed by some is to deny that one can make generalisations about the difference between crossing roads in Belgium or Indonesia

My other thought is to put in a plug for the relevance and importance of the work of Jean-Pierre Warnier in this discussion. Starting as a old style anthropologist but then influenced by Bayart and returning to Mauss on techniques of the body he's ended up in a similar position to the Scollons if I follow B&W. I think those interested in ethnographic approaches to speech will find a lot in the following:

Jean-Pierre Warnier. 2007. *The Pot-King: The Body and Technologies of Power*. African Social Studies Series, 17. Leiden: Brill Academic Publishers.

On FRI, Mar 5, 2010 at 2:48 PM, Jan Blommaert <j.blommaert@uvt.nl > wrote:

Dear all,

many thanks for this wonderful exchange of ideas. I learned a great deal from it, and encourage others to submit papers for similar exercises. It's a privilege and a great way of circulating and weighing ideas, approaches and data.

Many thanks again,

Jan Blommaert (also on behalf of April Huang)

On FRI, Mar 5, 2010 at 2:48 PM, Tom Van Hout <tom.vanhout@gmail.com > wrote:

Dear all,

The e-seminar is now closed! I thank Jan Blommaert and April Huang for submitting their paper, our discussants Mark Peterson and Adrian Blackledge and the list members for generously sharing their thoughts and comments.

A transcript of the e-seminar is attached and will be made available on our website. We've also created a **very** short survey (5 questions!). Completing it should take no more than 2 or 3 minutes.

<http://surveys.poll daddy.com/s/50DD4B9104EC7CB6/>

We look forward to your comments and suggestions.

All the best,
Tom