

What is anthropological research -- II?ⁱ

An interview with Jean Lave by Steinar Kvale

Introduction

This interview follows up on an interview about anthropological research we conducted in 1991ⁱⁱ. The main topic then was specific practices for carrying out anthropological fieldwork. What emerged most strongly was the importance of pushing for conceptual organization and insight during the fieldwork phases of the research.

In the present interview we address other contextual and conceptual issues of anthropological fieldwork. The interview starts with questions about learning the craft of fieldwork through apprenticeship, and then addresses differences of perspective in the knowledge produced by a senior researcher and a Ph.D.-student in the field. Thereafter we discuss a researcher's changing understanding of the research topic throughout the fieldwork and the interdependence of projects throughout a lifetime of anthropological research. The last part of the interview turns to different practices for conducting qualitative research interviews, whether away from, or in the course of, everyday practices of the subjects. An interdependence of methodological approach and theoretical conceptualization --knowing as situated practice-- is emphasized and the interview concludes by raising problems about member-validation in anthropological research.

The interview was conducted in the context of a research course on producing knowledge, experience and everyday life. It took place in front of the class and was intended to serve two purposes – to clarify the nature of anthropological fieldwork and also, through an actual case, to illustrate the process of qualitative research interviewing. As it turned out the interview also brought forth tension between the two methodological approaches. This concerns in particular the issue of interviewing outside the context of the subjects' everyday life settings. It also concerns giving the researchers' interpretations back to the subjects for member validation.

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Research Apprenticeship

SK: I would like to follow up some of the issues from the interview we conducted back in 1991 on "What is anthropological research". Anthropological research is still not quite clear to me - neither practically nor conceptually. I will start with the difficult part – practice. Suppose a student approaches you and says, "I want to learn anthropological research. I would like to learn as a research apprentice with you on a project you are doing." Would you accept? If you said “yes”, what would you ask her or him to do?

JL: First of all, anthropologists complain to each other that nobody teaches their students how to do field research before they go to the field. We agree that that’s irresponsible, but even so good fieldwork training is the exception rather than the rule. My response to this dilemma has been to develop a research apprenticeship seminar. Occasionally I do take on a student one on one to work on a project with me -- another way to learn to do research. But it is difficult for everybody to work that way for a variety of reasons. And, of course, apprentices always learn best from each other.

The research apprenticeship seminar provides a way of teaching and learning field methods that I really enjoy, and I think works well. My impression of most research method courses is that you merely *read* about one method, then another method, then another. In the research apprenticeship seminar I begin the first day by announcing that each student is going to do an ethnographic research project. Before this class meeting is over I am likely to hear, "Well, what is the syllabus? What are we going to read during the course?" and I tell them that there is no syllabus. Instead, the work of learning to do ethnographic research is going to come from their research experience. Crucially, this will include comparing what’s going on at each step in their various projects. This happens fairly easily since all of the students are at roughly the same point in their projects at roughly the same time. It is through the similarities in their methodological difficulties – encountered in their very different projects – that their questions lead to a long and productive conversation about ethnographic research.

The research apprenticeship course takes a year. The first months involve learning to configure a research problem. We discuss what that means. How do you work out a research problem? What does it take to come to see that some facet, place, group(s), and/or activities in everyday life might be an interesting focus for exploring cultural, historical and theoretical issues? The next months are spent finding out about other people’s ongoing everyday practice, learning how to make field notes, learning how to follow unfolding events from multiple points of view and eventually intensive informal interviewing. Then the students spend the second half of the year working at analyzing the material they have amassed during their fieldwork and learning how to write an ethnographic paper. For me, all of that is what it takes to learn to do ethnographic research.

SK: That is what you do in the class on ethnographic research. What do you do when you have a

research apprentice on one of your own projects?

JL: There are special problems with this second kind of research apprenticeship, given the historical constitution of academic life. For one, there is a hierarchical distribution of credit for the contributions made by different participants in joint projects, with students at the bottom. Another problem is that you can only defend the unique character of your own contribution if you do academic work entirely by yourself, at least in the U.S. I dislike both of these features of academic life and have experimented at various times with collective research groups and collaborative teaching. I did once take a "research apprentice" to the field with me. He found a field site in the Douro river valley, living in a Portuguese grape growing village for a year while I was in Porto working on the British Port wine merchant families. We agreed that he could not do a project that was intimately tied to mine. But we also thought that it would be interesting to see how the British in Porto and the Portuguese in the Douro river valley addressed their struggles with each other. We hoped to map these struggles, week by week, from our very differently located perspectives. Occasionally he would spend a few days in Porto and we talked in turn about our projects, and gave each other advice. This surely improved our fieldwork. At the end of a year of fieldwork and another year of separate analysis of our field materials, we concluded, though this may have been partly an artifact of our relations with each other, that the two communities were not engaged in the *same* sustained or intense identity-forming struggles with each other. They confronted each other in narrowly utilitarian negotiations over, e.g., the price of grapes and the timing of the harvest. But they did not seem to agree about the nature of their conflicts or the terms by which they engaged in conflictual identity-making relations.

Just a note on yet a third way in which I have tried to impart field methods to students. Once in awhile I have tried to cram preparation for fieldwork into a short period just as a student was about to leave for the field. I might spend a day or two walking around Berkeley with the student, giving advice about "how to" do fieldwork. However, when I have gone to visit such students in their field settings a few months later, they inevitably expressed chagrin that I did not explain ahead of time what they needed to know, while I was sure that these were the very things I had told them about in Berkeley. This experience has contributed to my belief that students need to be doing ethnographic research in order to be able to learn how to do ethnographic research -- thus the research apprenticeship seminar.

SK: What I hear you say, is that you promote independent research by letting the students be independent.

JL: I feel that I have no choice. I would prefer to work more collaboratively with students, but there is serious potential in this commodified world for exploitation of collaborators by senior anthropologists in the production of research, again with the qualification that I am speaking from my experience in the U.S. I owe my students scrupulous care to avoid putting them in a position where their work somehow gets identified with mine. The research apprenticeship seminar is an attempt to insure that "student independence" does not become an excuse for student ignorance about how to do ethnographic research.

Perspectival knowledge

SK: I would like to go back to your observations in Porto and your student's observations in the Douro. They led to – I think you said – entirely different knowledge. Could you specify how it happened that two anthropologists –an expert and a qualified novice researcher – should see such different things in similar social settings?

JL: One serious problem was that the connections I assumed existed between the British merchants and the Portuguese vineyard-keepers were too abstract to start with. I knew that over centuries vineyard keepers and the British wine merchants had been engaged in struggles, negotiating year by year over how the land was surveyed and given quality ratings, how many grapes would be produced, how they would be selected for making into wine, who would do that, what prices were set. But in fact in recent times the viticulturalists in the Douro and the British port wine families rarely encountered each other in person. The way the Portuguese farmers construed their relations with the British were very different from the way that the British construed their relations with the Portuguese farmers. They are not irrelevant to one another in a historical sense, but they were differently relevant in terms of their day-to-day practices.

SK: I just started wondering - now you are describing the Portuguese and the British as giving entirely different reports of their relations with each other. But we also have two American anthropologists giving two different accounts of these relations.

JL: Let me try to clarify a little. The political economic relations that these British port merchants and Portuguese viticulturalists were caught up in grew historically from a long association between Portugal and the UK. But on a day-to-day basis those relations are now predominantly mediated by the Portuguese state. The farmers rarely engage with representatives of the British port firms or families. Their notions of "the British" and their relations certainly involved old stereotypes and sentiments of resentment, but as part of structural relations of exploitation by the multinational corporations that now own the port houses. The British folks engaged in living their lives and doing business in Porto so as to keep from being mistaken for Portuguese, while denying their deep relations of dependence on Portuguese people, including Portuguese viticulturalists in the Douro. The material relations of the production of their lives lead them to engage with the presence - past, present, and/or future - of Portuguese "others" in ways that did not have as compelling immediate identity-producing connections as their relations with "other" Brits.

SK: The Portuguese and the British – and then you and your Ph.D. student.

JL: My former student wrote his dissertation about the formation of regional identity in the Douro, moving from very local contrasts between villages nearer to and further from the river, outwards to the region as a whole, contrasts instantiated by people who lived there. This was a fascinating project in its own right; especially since most cultural/geographical accounts of "regional identity" assumed that this is imposed from the outside. He focused on

relations that mattered in the lives of those viticulturalists as the formative puzzle that he wanted to address. Meanwhile, I began to understand British preoccupations with "being British in Portugal" increasingly as a matter of relations between different sorts of British people in Portugal, in the former British empire, and in the U.K. So I wouldn't say that our accounts were contradictory. Rather, we fashioned two different accounts about British and Portuguese identity-making relations.

SK: Okay, so your research did not contradict each other, but you were getting at different aspects from different perspectives.

JL: Yes. And these perspectives developed out of our different initial engagements in the fieldwork in the first place. They shaped partially, but importantly, the ways we understood what the interesting problems were to pursue.

SK: I understand that in anthropological research it is important to be aware of your own cultural presuppositions when you analyze and present your research findings.

JL: Well, yes. These are issues that anthropologists worry about. How do you take your own points of view, your own deep cultural formation, with you into your fieldwork? And how, during the course of the fieldwork, do you try progressively to become critically aware of your preconceptions? Would it be okay if I talked a little about the practical means you use to do this?

SK: Yes.

Changing Understanding

JL: When you do ethnographic research you keep fieldnotes chronologically. You write a lot, indeed, you spend much of your time in the field writing notes. For every hour you spend observing and participating in everyday events, you spend two hours writing about what happened. You describe as closely as you can what people have done and said, the contexts and events in which they were taking part, and the terms and ways in which they participated with each other. At the same time you keep an account of your own changing understanding of what is going on.

SK: Your own changed understanding?

JL: That is right. As you write, over time, you begin to understand things differently than you did at the beginning. Being able to reflect on the chronological accounts you have made, both about what you are seeing now and how your understanding is changing, are ways you gain robustness in arriving at conclusions. You obtain some confidence in the conclusions you are drawing on the basis of careful accounts of how you arrived at them.

SK: I would like to address these issues from a slightly different angle. In our earlier interview you said that good research is about sustaining a lifetime of interconnected research projects

that are closely related. That was in 1991. You had then done the Brazilian Indian study, the Liberian tailor study, and the California supermarket study. Since then you have carried out the Porto study. I would like to get into the intergenerational relations of the projects you have been doing, in particular the Liberian tailor study and the study of how to grow up British in Portugal. A hypothetical question: If you now were to go back to Liberia to redo the tailor study, if that were possible, would you do it exactly the same way as you did, or would you go about it differently?

JL: I would go about it very differently. I have lots of regrets about how I did that research (1973-1978). I would start with the notion that the question of how to become a tailor through apprenticeship is very much the same question as how to grow up British in Portugal. For instance, the tailor shops in the Tailors' Alley were actually interspersed with women's businesses in the Alley. This was the red light district of Monrovia as well as the primo place for men visiting Monrovia from the countryside to buy new clothes. A broad perspective to start with would have explored the meaning of what each and all of them were engaged in doing. How different this might have been from the approach I employed then! I was hanging around asking, "How do you sew the waistband on a pair of trousers?" It was a community, a complex community with a whole lot going on. I think that if I had focused carefully on relations across the contexts of the tailors' lives, I would have a much different understanding of the nature of apprenticeship. Furthermore, I did not spend enough time with the apprentices who did much of their work at night, practicing, and working with each other after the master tailors left at the end of the day. I didn't see these after-hours relations among apprentices as the important part of apprenticeship I now believe them to be.

Those are some of the simple ways in which I would do the apprenticeship research differently today. As I look back at the first account I made of it in 1981, I was still immersed, and still stuck, in the prevailing structural functional assumptions of anthropological theory of which I was gradually becoming critical. First I was just a little critical and then later very much more critical. But being critical isn't sufficient to lead you to a perspective based on different theoretical assumptions from the ones you are criticizing. The work that I have done since then was necessary in order to take on the theoretical perspective I now wish I could have brought to the tailors' apprenticeship project.

SK: Your report from the Port study supports what you are saying –in that study you are much more attentive to the historical, social and economic conditions of the social practices you are studying. At the same time, at least in the article that I read, you are less specific. In the Liberian tailor study you are describing how master and apprentice are sitting together on a two-person bench and sewing while in the Portuguese study you are more general, interpreting the British port merchant families' lives with a few examples. Or is that not correct?

JL: Yes and no. Given my theoretical orientation at the time, I reasoned out my task something like this: I asked myself, "What is apprenticeship? Apprenticeship is learning to make clothes. I need to find out how apprentices learn to make clothes. They learn to sew, then they learn to cut out each garment. So I should narrow down the scope of my inquiry to the

technical accumulation of a certain kind of knowledge and skill.” It led to the sort of detail you have noted. I do like those concrete details. Indeed, I hadn't thought of that, until you mentioned it, as one of the nice things about the tailors' study.

By the time I began the Port project (1992-1996) I had spent some years reconsidering questions of what constitutes learning. Perhaps this made me think, well, what are the kinds of changing practices in which British port family members are engaged and how are they participants in them? That certainly did shape the grain size at which I addressed their lives. And the two research studies aren't the same, you are right. You have made me think that perhaps there are advantages to each of them – and of course disadvantages as well.

Each research project I have done has been intended as an introduction to the previous one. So the relations between my research projects have been complex in both directions. You come to see what you did before differently when you do a new project.

Interconnectedness of Lifetime Projects

SK: Are there more things about the relation of learning from the Liberian and the Port studies you would like to bring up?

JL: The book *Cognition in Practice* from 1988, based on the collaborative Adult Math Project, was an attempt to learn how I might transform my theoretical understanding in much more thorough-going terms than I was able to do in the tailors' apprenticeship project. I wouldn't have understood the importance of the broader historical contextualization that is central to the Port project before I had done that work. The connections between the Liberian project and the Port project are mediated through the work discussed in *Cognition in Practice*. So part of the difference between the tailors project and the port project is theoretical change. It is also the case that I have tried, over time, to move away from the idea that to learn is to learn in narrow ways, to accumulate information, knowledge, or skill. Part of the theoretical change since *Cognition in Practice* has been to say: “Look -- maybe knowledge, especially knowledge excised from learners' lives and identities, doesn't matter all that much.” I once experimented with refusing to use the term “knowledge” for six weeks. Any time it came up I tried to figure out more precisely what I meant to say instead. The point was to stop and ask: “Does ‘gathering information’ or ‘acquiring knowledge’ have any meaning independent of people engaged in changing knowledgeability as part of changing their identities, changing the character of themselves as persons?” I think that I have better ways of talking about these issues now. I would say that changing who you are subsumes “knowledges” and changes their meaning, though never entirely as you choose. So I am not going to ask knowledge questions first, when addressing issues about learning, I am going to ask about what are the changing identities and trajectories that people are producing - thus, “How do persons become British in Porto?”.

SK: You talked the other day at this course about learning through *doing* anthropological research – you learn about the field and at the same time you learn about doing anthropological

research and maybe also about yourself. Would you like to expand on that?

JL: One of the things that anthropologists don't talk about very often is that they do more than one research project in their lives, though few anthropologists carry out more than three and often not more than two. The connections between the projects are unavoidable, I would argue, although sometimes one doesn't start out believing in them. Nonetheless they are really there, and often they are crucially important. I believe we should give up this notion that the proper conclusion to a research project is a finished generalization, or a declaration about what is universal and important in the world. Instead we might consider research as something that changes our understanding of social practice in a changing world, including your own. When you engage in ethnographic fieldwork you cannot but approach it with a theoretical perspective. It shapes how you encounter that research. But if the research goes well you change your understanding of the empirical material and at the same time this changing understanding has a significant impact on your theoretical perspective. If neither of these things changes, why bother doing ethnographic research? You could either stay home and speculate about theory, or you could do empirical work in the field and forget to write about it except in the most specific terms.

If the research process really works you come out with some kind of middle level analytic findings and tools that are useful to other people as they go about their research, but which also open new questions. So what do you do next? There you are with these new questions. You go on, and hopefully as you go on each project changes in response to the questions that your previous project raised. That is what I call interesting research.

SK: I have two responses to what you are saying. I believe that changing as you go along contributes to developing new knowledge. But if you learn too much and if you get too wise, when you are doing a specific project, will it ever be finished? The Ph.D. students sitting in this room are doing projects that have to be completed within three years. What happens if they start learning all the time and get much wiser – will they have so many new insights that they don't manage to hand in their Ph.D. dissertation after three years.

JL: We talked yesterday about this notion of research as a craft and a notion that no specific project can ever be complete. You can always see things that you should know more about. You recognize new contexts that are important to things that you are already working on, and you know that if you don't pursue them there are going to be things you misunderstand or don't understand as well as you would like. Coming to terms with this practical reality and figuring out where to draw limits and boundaries is part of what is involved in learning how to do ethnographic research. That is one of the things that happen in the research apprenticeship seminar. Students talk a whole lot about these issues in concrete terms with respect to their projects. They learn how important it is to figure out where and why to cut and shape the scope of their projects in particular ways.

The other thing is that if you are convinced by the argument I have been making about the open-ended nature of research findings and conclusions, you give up grand closure as both

impossible and undesirable. At the end of each of my projects I would say that I quit-at-a-certain-point, rather than that I finished them. Then you try to make of “quitting” something useful and interesting to yourself and others.

Interviewing in Context

SK: I want to hang on to this issue, but address it from a different angle. What about control of your research?

JL: What do you mean – “control”?

SK: Well, let us take our interview here. Control is often treated in relation to reliability, or replication of findings. But in the course of this interview, at least, I am learning about anthropological research, and maybe you are also learning something by formulating your experiences. We both may have changed during the interview and in principle it is impossible to repeat this interview or verify the knowledge we are producing -- if you think of traditional reliability as intra- and inter-subjective reproducibility.

JL: I actually think it is more difficult *not* to repeat ourselves, but I know that’s not what you are getting at in your question. Fifty years ago anthropologists usually chose a cultural site in which to do their fieldwork, wanting to make sure that no other anthropologist had been there before. That placed a heavy responsibility on anthropologists to provide detailed evidence; to consider and explain how they came to know what they knew, and to assess the strength of their own materials as they wrote about them. I take seriously, as an issue of ethnographic method, questions about how anthropologists come to have confidence in their own materials and analyses. There are a whole variety of techniques that anthropologists employ in the field to address these questions.

SK: Such as?

JL: Okay, how about an example or two. For one thing I almost never conduct a formal interview in the field, or even anything approaching a formal interview, until a couple of conditions are met. One is that I must have shared a wide variety of participation in ongoing daily events with the people involved, to the point that whatever the issues are I might like to talk with one person about at length, we are able to talk about specific occasions, events and conversations we have participated in together. That usually takes a long time in the field.

Another thing is that there is a funny, delicate character of really powerful ethnographic interviewing. I guess I would describe it as having a twenty-four hour conversation with someone in whose life I have become a peripheral co-participant for a period of time. This depends on my growing understanding of other people's lives. But it also and equally depends on the growing understanding of other participants about what it is that I am trying to understand.

What you look for across a series of interviews, especially if they are focused on a common set of issues that affect a variety of participants differently, is for *differences* between what people tell you. You try to explore how they understand events and their meanings. Perhaps you wonder what is going on when three or four people, making jokes together, begin to argue instead. Or, one morning you go to a meeting and something happens in the meeting - quickly - and you figure that there is more to it than you are able to comprehend. These are moments that invite the impulse to "interview." Go and talk to each participant - separately - and ask them about how they understand what was going on. Try to see at the time how they are different in their ways of participating, and in their accounts.

How do you know when you are done doing interviews? Of course you are never done, but when you find that after a number of searching conversations you are rarely surprised to hear this person over here talking differently from that person over there; when you can anticipate what the differences will turn out to be, then perhaps you are ready to consider quitting - but not finishing. Note, however, that if you do not go after differences between interviewees, and unanticipated complexities that emerge differently from one interview to another, you are fated to merely confirm what you already know.

If you believe that social practice is complex and contradictory, asking people what they do similarly in different situations is not sufficient. So another reason for doing a lot of participant observation is to discover how in some situations, people do one kind of thing and then they do the opposite, often at the same time. This should not lead to claims that they are irrational, but should provoke speculation about the organization of social life in practice.

Then the kind of interviewing situation you want to create is not one where you try to control questions - and potentially answers - by demanding that "the natives" just answer your prepared questions. An ethnographic interview asks, in effect, "Please help me understand what else I need to know about in order to understand better what has happened, what is going on, and why things are the way they are." "Interviewing" in ethnographic contexts, becomes a kind of collaboration. It is not a matter of saying: "Please be the anthropologist and analyze the situation in my terms for me." Rather, it involves several parties exploring in their different ways the question, "Can we collaborate from our different stances and locations as participants, and if so, how?"

SK: Well, that pulls the carpet out from under what many of us interview researchers are doing. We may just walk into a new setting and believe that after talking together with an interview subject for an hour we can get to know what we want to know.

JL: Yes, I think this proposition is worth critical consideration. But one useful way to approximate an anthropological approach while still talking fairly briefly with relative strangers to good effect is that described by Klaus Nielsen yesterday. He told us about working in several different bakeries himself before he began to do some interviewing in those and other bakeriesⁱⁱⁱ. That is certainly a helpful step in carrying out productive interviewing.

I carry around an image of ethnographic research as ripping a small piece of social life out of the endless social fabric, with ragged edges and unfinished threads going in all directions. It is inevitable that my knowledge, my familiarity with complex community life, and my personal participation get thin at various points. I then try to keep the main things that I write about well within the bounds of the most intense encounters at the heart of my fieldwork.

SK: You don't want to tear what happens out of its social context.

JL: That's right, though ultimately this is impossible to escape. I try to be a bit conservative about it, that is I try to stay not at the edges of my knowledge, but somewhere towards its centre whatever that might be.

SK: Not to publish ideas, which are at the edge, but wait until you have them corroborated from many different instances and observations.

JL: That seems right. Now that I am an old anthropologist, I have numerous boxes of field data sitting in my office gathering dust. There are piles of field notes I have never attended to, never got around to writing about, never made anything out of. It is astonishing how little of the field materials, artifacts, memories and experiences, how little of the everyday life of those you have sought out in ethnographic projects, how little of your field work, ever sees the light of day. This is no doubt a good thing.

Relation of Method and Theory

SK: I would like to get back to the issue of desituating your data from the social practices they are part of. Have you any thoughts on the relation between your methodological approach and your theorizing about cognition and learning as situated social practices?

JL: First I would like to make one general comment about situated learning. I think the concept of "situated learning" has been persistently misused, both in workplace and education sciences. Etienne Wenger and I proposed what we called an analytic framework for inquiring into practices of learning in the widest possible variety of social settings. These might include conventional educational settings, such as apprenticeship situations or perhaps schools. But we emphasized that we did not intend this framework as a prescriptive guideline for "how to" improve anything. The analytic apparatus we proposed for inquiring into situated learning is *not* a bible for fixing schools; "communities of practice" is *not* a vision for improving workplace organization. Reproachful responses were common: "You don't care about fixing schools or improving workplaces!" Not true. In *Situated Learning* we argued that educators and management researchers take schools and work organizations as all too natural, and in doing so forget that before they jump to try to fix them, they need to know what "schools" or "workplaces" are. We argued that there is no point in trying to fix something before you know what it is you are trying to fix.

I occasionally give talks about apprenticeship, social practice, social practice theory, and

everyday learning, with examples chosen *because* they involve non-schoolish learning contexts. But in discussions following such talks listeners revert within five minutes to talking about school. None of us finds it easy to conceive of learning outside schools. It is instructive, then, to focus on the multiple contexts of everyday life as they *are* the locus of learning. These should be of interest, not simply as they are touched by the shadows of schooling, but as contexts of learning in their own right. Of course, in the world in which we live a multitude of aspects of schooling figure in the organization and meaning of many aspects of our lives, probably more pervasively than we recognize. This suggests that interesting questions about learning in everyday life might address both its very different qualities as well as its schoolish reverberations -- and of course, their relations.

Now, returning to your question about method, I think it is axiomatic that whatever your theoretical perspective, it pervades every aspect of your research, including of course, one's "methods." It is not surprising, then, that my work is informed by social practice theory. Notions of situated learning and the relational character of practice assume the multi-contextual interconnected ways in which people live their lives, which in turn require ethnographic means to address. Research methods that would hack bits of social life out of the multiple contexts and relations in order to study them with rigor and reliability, just distort them out of all reason and interest.

SK: I hear you describing an inherent unity of your methodological and your theoretical approach.

JL: Yes, I think that is right. There is a word for this view of theory/practice relations. I would say I have a research "problematic". A problematic is theory in practice and practice in theory – inseparable relations, all of a piece. This shapes profoundly the kinds of research one does, what palette of research questions are salient and addressable, through what theoretical concepts and means of inquiry. All of these hang together as a single phenomenon and, of course, have characteristic limitations.

Member Validation and Anthropological Research

SK: I would like you to get back to something you mentioned a while ago – that you wanted to interview people only after you had spent intensive time with them and observed and participated in their practices. Within qualitative interview research today there is much talk about member validation.

JL: What is that?

SK: Well, it is the notion that you validate research findings by going back to the members you have researched to have them verify or disconfirm your analysis of what they are doing.

JL: Yes, well I have strong reactions to that. I think that this procedure involves a naïve understanding of the nature of the practice of research -- in the following way. There certainly are

anthropologists who argue that validation in the terms you describe is a serious responsibility. For some of these folks the purpose of anthropologists is to become a voice for the people they have been studying. A good anthropologist, by this account, strives through her or his work to become a better voice for native concerns, native interests, and native whatever.

There is another point of view – the opposite one – expressed in some books in the 1980s and 1990s. These are critical of the authorial privilege of anthropologists who they describe as principally sitting in their university offices writing "definitive" accounts of distant cultures for the edification of their colleagues. They shouldn't do that, goes the argument, but that is what anthropologists do because that is the currency through which reputation and prestige are acquired in anthropology.

But instead of either a field-centered or university-centered characterization of anthropological practice, we might better think of anthropology as an historical conjuncture in which academic researchers have fashioned a peculiar form of scholarly work. They begin with years studying in the university, their home ground. Their relations with their colleagues are formative in shaping the peculiar perspectives with which they eventually approach field research. Then they absent themselves from the university, cut off their ties with their home base as radically as they can, and submerge themselves in life-made-as-strange-as-possible, somewhere else. After a year or two they turn around and come back to the university. It is in the repeated restructuring of their lives, projects, relations and understanding – made in crossing the boundaries among their disparate contexts -- that the character of anthropology is given its characteristic substance.

By this account there is then no way in which an anthropologist, who lived for a year or two in a community far from the university, but then returned to the university to sort and analyze field materials, read, and engage in discussion with colleagues for another year or two, should use or could use a local/native standard for whether they have "done it right." Whether they write an account of their research in the context of the university and its compelling concerns, or as a complex matter of relations between the deep concerns of these radically disjunctive contexts, the purpose of anthropological analysis is not congruent with the lived concerns of the people under study. Why would you then send what you have written to folks to check whether you have articulated what they would have said in your place? Nonsense, that's ridiculous. Anthropologists and those they study have different perspectives and different lives, different personal trajectories, and different historical trajectories and relationships with the phenomenon of their lived lives and each other.

SK: Are you warning against what is called "going native" - against reducing anthropological reporting to what the real natives are "really saying"? I think you are suggesting that ethnographic research is built on relations between the anthropologist's world and its defining concerns on the one hand, and the "native" world and its defining concerns on the other hand.

JL: Yes. There wouldn't be ethnographic research otherwise. It exists in this complex to-ing and fro-ing between multiple contexts chosen because they are radically separated from each other; an interesting idea -- a little strange as a recipe for a scholarly discipline, but in practice very interesting.

SK: So you do not use the members' acceptance or validation of what you have done?

JL: Well perhaps, under carefully considered, limited circumstances. I do care about getting as close as I can to ethnographic validity as one aspect of my projects. To engage in ethnographic research you start out with some theoretical conception of a problem that interests you -- some relations in the world that seem important to you as a scholar, and that you think you could illuminate by investigating in great detail in some other cultural/historical context. My experience has always been that once I get to my chosen field site I cannot see *anything* going on that looks in any way related to the interests with which I arrived. It has also been the case that when I have come home eventually I have always come to the opposite conclusion: yes, the things that interested me and that I was trying to pursue were absolutely going on, but in ways I could not have conceived of before I went there - and came back. This movement through radically disjunctive contexts *transforms* my understanding of what it might be to talk about the issue I began with. That is what I hope for when I embark on an ethnographic research project.

SK: If your Porto study subjects were to read the articles you have written about them, how do you think they would react? Would they find some situations as you describe distorted and offensive?

JL: I asked sympathetic peripheral participants in Porto to read my papers on growing up British, actually, and took their comments seriously in revising them. But the central figures in these papers would disagree strenuously with my analysis, I think. This raises yet another set of issues that have to do with, among other things "studying up." The anthropologist Laura Nader is well known for her observation, probably twenty years ago, that there is a bias in our field: a strong tendency to study people with less power and fewer resources and possibilities than ourselves. She argued that we also needed to study the rich, the powerful, the corrupt, and the privileged. Paul Duguid, one of the founders of the Porto project, has carried out extensive research on the history of the port trade. Historical materials on elite participants in the trade were relatively easy to find. It was much more difficult to find out the circumstances, relations and voices of the poor and powerless. We gradually came to agree that anthropologists cannot "study up" any better than historians can "study down."

Why can't anthropologists study up? It is not impossible, but ethnographic fieldwork requires a respectful, concerned and empathetic relation with the people whose lives you are trying to come to understand. Friendship is important in your relations with research participants. It requires mutual trust. Relationships in the field are often complex, extending across a year or two or forever. I quit the Porto project before I might have, because I found that I was so politically out of sympathy with the people whose privileged lives I was trying to study that I

could not go on with research that depended on their trust.

SK: This leads to the ethico-political Issues of qualitative research, which I would like to pursue more extensively at a future occasion. Rather than opening this important topic now, this may perhaps be a good place to bring this interview to a close.

JL: Yes. The tough questions you have raised will be on my mind for a long time to come -- they deserve continuing thought, and of course, deeper and better answers.

SK: Thank your for this conversation.

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- ⁱ This interview was conducted at the research course “Producing knowledge, expertise and everyday life”, chaired by Ole Dreier, at the Danish Psychological Research School. It took place October 21st, 2004 at the University of Copenhagen, and has since been edited. We wish to thank Lone Hansen and Mikala Hansbøl for transcribing of the interview.
- ⁱⁱ Published in “News Letter from Centre of Qualitative Research”, # 10, 1992 ([Hwww.psy.au.dk/ckmH](http://www.psy.au.dk/ckmH)) and *Qualitative Studies in Education*, 1995,8(3), 219-228: Lave, J. & Kvale, S. ”What is anthropological research? An interview with Jean Lave by Steinar Kvale”
- ⁱⁱⁱ Nielsen, K. (2005, in press). Learning to do things with things – apprenticeship in bakery as economy and social practice. In: A. Costall & O. Dreier (eds.). *Doing Things with Things*. London: Ashgate.