

## **The Collaboratory Form in Contemporary Anthropology** **Stephen J. Collier**

### *An experiment in collaboration*

This paper relates to a collaborative enterprise I have undertaken with Andrew Lakoff and Paul Rabinow that we have decided to call the Anthropology of the Contemporary Research Collaboratory (ARC). In what follows I would like to say something about the collaboratory form as it relates to problems of method and collaborative work in contemporary anthropology.

In some sense our collaborative endeavor has been developing for a long time, and relates to longstanding concerns about concept work, method, and the form given to anthropological inquiry.<sup>1</sup> But it got going in earnest in spring 2005, when we began a new project on the contemporary biopolitics of security. Our sense was that entering into a broad and rapidly changing field like security posed challenges to the existing modes of inquiry in anthropology. We therefore determined that this new substantive project would have to be coupled with renewed reflection and organizational energy around collaboration and concept work.

One term we have used for thinking about this collective effort is “collaboratory.” “Collaboratory” gained currency, it seems, in the early 1990s, particularly in areas such

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<sup>1</sup> Our prior collective and individual work on these questions includes Stephen J. Collier and Andrew Lakoff (2000). Object and Method in Contemporary Anthropology. Annual Meetings of the American Anthropological Association. San Francisco. Paul Rabinow (2003). Anthropos Today: Reflections on Modern Equipment. Princeton, N.J., Princeton University Press. Stephen Collier, Andrew Lakoff, et al. (2004). "Biosecurity: Towards an Anthropology of the Contemporary." Anthropology Today 20(5): 3-7. Stephen J. Collier and Andrew Lakoff (2006). What is a Laboratory in the Human Sciences?, Anthropology of the Contemporary Research Collaboratory. Documents related to ARC's security collaborations can be found at [www.anthropos-lab.net](http://www.anthropos-lab.net).

as the natural sciences and computing. There is a narrow meaning of “collaboratory” – namely a distributed research network articulated by means of information technology. We prefer to think of our collaboratory in broader terms. Cogburn (2003) provides such a definition. He writes that “a collaboratory is more than an elaborate collection of information and communications technologies.” Beyond that, it is “a new networked organizational form that also includes social processes; collaboration techniques; formal and informal communication; and agreement on norms, principles, values, and rules” (Cogburn 2003, 86). In other words, a collaboratory is shaped by – and seeks, in its own way, to shape – many of the things that we normally think of as defined by a discipline: the norms, standards, and mechanisms of critical rectification that make it possible to conduct inquiry and contribute, in whatever way, to the production of knowledge and of tools for thought.

The core of ARC is ongoing reflection and communication in a now broadening circle of scholars about method and inquiry in the critical human sciences. Beyond that, it has involved a variety of specific collaborations. Some of these are well developed, with tangible products. Others are still in a process of formation.

Here, my purpose is not to reflect on these experiments themselves, although there is much of interest to say about them. Rather, I would like to take a step back, to say a bit more about our reasons for organizing our collective undertaking from a disciplinary perspective. These reasons have everything to do with the existing approach to method in contemporary American anthropology – at least the forms it takes in certain parts of the elite discipline in cultural anthropology.

### *The individual project model*

Broadly speaking, our impulse for taking more seriously the problems of collaboration arose out of dissatisfaction with what is at least one dominant model of knowledge production in anthropology specifically, and, more generally, in the interpretive human sciences. This model – what we have proposed to call the “individual project model”<sup>2</sup> – has, in our observation, a few salient characteristics. First, it views the authority of academic production as connected to individualistic elements of the fieldwork process and of writing; thick description, virtuosic interpretation and elegant writing are considered the mainsprings of good work. Second, the individual project model privileges experimentation with form in writing and in styles of fieldwork. It valorizes efforts to challenge or break away from existing norms. The mode of experiment, thus, is avant-gardist rather than scientific. Its aim is not the production of knowledge but calling into question existing norms.

A third characteristic of the individual project model is that “legitimate” contributions often take the form of “branded” concepts that are associated preeminently with specific authors. They do not necessarily lead to programs of research, but may be theoretical markers or points of orientation for a certain positioning within a field, or for a certain kind of politics.

At its best, this model produces genuinely innovative and original scholarship. There are, after all, virtuosi out there. But we also feel that it has some serious problems. The individual project model often results in workshops, conference papers, collected volumes and monographs in which the emphasis is placed on individual performance,

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<sup>2</sup> This analysis of the individual project model arose from conversations with Rabinow and Lakoff. Lakoff and I have discussed this individual project model elsewhere Collier and Lakoff What is a Laboratory in the Human Sciences?.

and in which there is not much discussion or debate about what the key problems for the field are, or, for that matter, serious debate and discussion about empirical material in a given area. Thus, although it may result in *collected* work, it rarely produces *collective* work, either on specific projects or on the formation of concepts and problems.

What is more, the individual project model does not encourage work on shared norms that lead to better understanding of significant phenomena. Quite the contrary, it might be argued that this model has produced a crisis in thinking about what constitutes a valid or interesting claim in at least some parts of our discipline. There are, of course, tacit norms. But it is not clear that these norms relate first of all to the validity of knowledge claims.

### *Ethnography and Method*

The identification of the individual project model is meant as something of a provocation. Hopefully someone would want to have a fight about whether this is actually the dominant mode of knowledge production in certain parts of American anthropology, or to defend the value of this kind of work. But in making explicit the elements of the individual project model, we have another aim: namely, to try to think more clearly about the present predicament of method in anthropology.

Most contemporary discussion of method in cultural anthropology has focused on the question of ethnography. Ethnography, simply, is seen as anthropology's method, and, conversely, anthropology is sometimes defined by the fact that one has conducted ethnography. Thus, the oft-heard query "where's the ethnography" means: is this really anthropology? There is much to be said about this continued emphasis on ethnography in

anthropological discussions of method, not least the puzzling fact that it survived the supposedly devastating critique of ethnographic authority that took shape in the 1970s and the 1980s (Marcus, Rabinow et al. 2007). Equally surprising is that, although it explicitly reacts against many elements of “classic” ethnography, the individual project model has held on to these emphases of the ethnography-centric methodological discussion. Ethnography, in its various modalities of fieldwork and writing, is where, for the individual project model, the interest, and the fame and the glory, all lie.

One important result of this continued attachment to the classic staples of methodological discussion is that the individual project model fails to give a very good account of itself as a mode for conducting inquiry. The model rests on what we would argue is a myth of *sui generis* intellectual production. The emphasis on the individual’s contact with the field, or the process of writing does not offer a plausible account of how the generation of knowledge actually happens.

Let me try to indicate what I have in mind by thinking through a topic to which George Marcus has given a great deal of thought – namely, the question of pedagogy, training, and the process through which students are transformed into scholars (Marcus 2007). What happens when an aspirant anthropologist, with the individual project model in hand, goes to the field? Beyond the normal difficulties of going to an unfamiliar place, there is the additional trauma of having to make up what you are supposed to do there as you go along. One must go through a process of thrashing about, not only to figure out what exactly you are supposed to be studying, but what, actually, the central questions ought to be.

In one view of ethnographic fieldwork, this pain is, as it were, precisely the point. There is a kind of existential passage that is considered, still, to be very much a marker of successful fieldwork in anthropology: You go. You suffer. You figure out what you are doing. And you are the better off for it. There is certainly great value in a relatively open-ended process of searching for problems and objects that transforms an anthropologist's relationship to a field. And, no doubt, the discomfort of not knowing what you are looking for may have some salutary effects. What deserves more reflection is the question of how you get there in the first place.

As in any other discipline, in anthropology the process of choosing a field site or any other site for making observations involves a tremendous narrowing of vision. This narrowing is based on a prior choice about what might be important to know, about what, in other words, the problem is. One of the significant shortcomings of the individual project model – and of at least one important part of cultural anthropology in the U.S. today – is that it does not reflect upon, or offer a plausible account of, how it is that one knows that a problem is a problem, or that a particular site would be a good place to study it. This is not to say that individual anthropologists don't spend a lot of time thinking about these issues. In fact, they *do* spend a lot of time thinking about these issues. But the discussion of method in anthropology, at least in recent decades, has not expended a great deal of energy reflecting on them explicitly.

Our view – and a key premise in trying to establish a collaboratory for the anthropology of the contemporary – is that the identification of “method” with “ethnography” in anthropology is unfortunate and, moreover, debilitating. It leaves out of the discussion important elements of what inquiry is all about: the definition of

significant problems, the identification of sites in which these problems might be investigated, and the process through which some kind of data gathered from these sites is shaped into a claim about what is going on, and redirected toward the formation of new problems.

A starting point for a new kind of methodological discussion in anthropology in the United States would be to recognize that method refers to this entire range of activities. This might sound uncontroversial enough. But it forces us to acknowledge something that might sound somewhat less uncontroversial, namely, that *ethnography is not a method*. It is, rather, one possible technique in one segment of the broad problem of method. From this perspective, the insistent question “where’s the ethnography” – the methodological litmus test for those who police the discipline – sounds rather incoherent. It is the rough equivalent to formal modelers in political science who reduce the question of method to purely technical questions concerning the internal coherence of models – victims, as it were, of a category error.

The tables should be turned. It is not that those who do not practice ethnography have to justify what makes them anthropologists. Rather, proponents of ethnography have to justify why this funny technique, invented in other times and places for entirely different purposes and problems, should be appropriate to the kinds of things that anthropologists study today. There are legitimate answers to this challenge. But the important point is the form of the question itself: The challenge is not to justify a piece of work as ethnography, but to justify ethnography – or, for that matter, any other technique of fieldwork – in methodological terms. The important problem is one of method, not of technique.

### *Method and collaboration*

This distinction between ethnographic technique and the broader problem of method brings me back to the central theme I wanted to raise in this paper: the question of collaboration and anthropological inquiry.

One legitimate aspect of this question is, in fact, the relationship between collaboration and the *technique* of ethnography. There is nothing that says, per se, that ethnography must be an individual endeavor. Various collaborators in ARC have experimented with collective interviews and fieldwork “encounters.”<sup>3</sup> These and other experiments indicate that, although ethnography can be individual it need not be.

Less equivocation is appropriate if we ask about the broader relationship between *method* and collaboration. If ethnography, as a technique, can, in principle, be individual, method cannot be. It is necessarily collective. The reason, simply, is that the identification of significant problems and the definition of what counts as a contribution can only be defined in some kind of collective context, in which there are shared norms, shared standards, and shared means of critical rectification through which it is possible to agree on what counts as a significant problem and what counts as a contribution to thinking about it better.

It is no coincidence that this observation brings us back to Cogburn’s definition of the collaboratory: a “networked organizational form that also includes social processes; collaboration techniques; formal and informal communication; and agreement on norms, principles, values, and rules” (Cogburn 2003, 86). That, upon reflection, seems like a

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<sup>3</sup> The most systematic work along these lines has been organized by Chris Kelty in his collaboration on nanotechnology.

reasonable scope for the problem of method in anthropology today. In ARC we have been working on all these various things: norms, principles, techniques, processes and so on. Here I don't want to discuss any of them in particular, only to insist, again, on the general form of the problem: one of method, not technique; one of anthropological inquiry, not ethnography.

Let me conclude by trying to bring these reflections together with an example from our current project on the biopolitics of collective security. I want to offer just one illustration of how the collaboratory form provides the space for a different kind of methodological work. Early in the project, we were forced by the exigencies of grant writing for the National Science Foundation to define fieldsites as part of the "method" section of our proposal. As it turned out, we did not conduct ethnographic fieldwork in any of them – although thinking that we might proved helpful for other reasons.

Rabinow, whose initial proposal was to work on an organization called the Molecular Sciences Institute, shifted his attention to synthetic biology. He had worked on the biosciences for well over a decade. Thus, in our terms, he had a general orientation to the problems of this field. Consequently, it was possible for him to make a discerning judgment about where, from the perspective of our new project, the action was: namely, where one might find the most potent vectors of transformation in the relationships between the biosciences, ethics, ontology, and security. In this light, Rabinow's immediate tasks were defined as crafting new concepts and tools as new things happen in the domain of synthetic biology. Along with some collaborators at ARC, he has situated himself at sites of initiative in the field of synthetic biology, trying to craft concepts and terms appropriate to its study. This is, in some ways, a classic plunge into the

ethnographic field, though one whose modality is contemporary – oriented, in Faubion’s terms to a problematic of emergence rather than a problematic of reproduction.

Andrew Lakoff and I – who have undertaken a related but distinct program of research – were in a different situation. Although we tried looking in detail at a few specific sites, we constantly had the sense that we were *not* oriented, that we did not have the concepts required to point us to significant problems in the field. We were drawn to novel mutual inflections of apparatuses of social modernity and emerging security assemblages. So, for example, an institution like the Department of Homeland Security in the United States crosscut domains of security and biopower in what seemed like intriguing ways, a fact brought home in particular by the experience of Hurricane Katrina. But we didn’t really have a sense of what the problem was: where did new forms come from? What was significant about their transformations in the present? How did a concept or an organizational form like Homeland Security emerge?

Our response to this situation was not a step forward into ethnography but, in a sense, a step back; away from that productive narrowing of vision and the commitments it entails and into a broader set of genealogical and conceptual questions in which we are currently engaged. In doing so, we also tried to set up a new kind of collective work. At the same time we were conducting broad genealogical work on security in the 20<sup>th</sup> century, we supervised students working on specific projects that suggested more focused lines of genealogical and contemporary inquiry: contemporary syndromic surveillance against the background of health surveillance over the course of the 20<sup>th</sup> century; contemporary vaccination programs against the background of 20<sup>th</sup> century attempts to control disease outbreaks in a population.

This collaborative effort has yielded in abundance the usual products of anthropological work – journal articles, conference papers, commentaries on current problems, and, soon, books; and it has done so in a rather abbreviated time frame (Fearnley 2005; Fearnley 2005; Fearnley 2005; Rose 2005; Collier and Lakoff 2006; Collier and Lakoff 2007; Collier and Lakoff 2007; Lakoff 2007). It has also produced, we think, what are today somewhat less conventional products – a series of mid-level terms such as imaginative enactment, archival knowledge, distributed preparedness, and vital systems security. These are not branded concepts, and we hope they will not come to be branded. Rather, they are collectively produced terms that mark significant distinctions and significant problems in the field that we are studying. Our hope is that they will also be useful as tools for others. The test of our contribution will lie in how far they can be extended, and in what kinds of collectivities they are able to include. The same test holds for our collaboratory as a whole.

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