**ANTHROPOLOGY
of the CONTEMPORARY
RESEARCH
COLLABORATORY**

ARC

INSTIGATED BY
TOBIAS REES

CONCEPT WORK AND COLLABORATION IN THE ANTHROPOLOGY OF THE CONTEMPORARY

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Anthropology of the Contemporary Research Collaboratory (ARC) aims to develop new techniques of collaboration, modes of communication and tools of inquiry for the human sciences. At ARC's core are collaborations on shared problems and concepts, initially focusing on security, biopolitics, and the life sciences, and the new forms of inquiry.

**Concept Work and Collaboration in the Anthropology
of the Contemporary**

Contents

Introduction

Tobias Rees and Stephen J. Collier

In 2005, Stephen J. Collier, Andrew Lakoff, and Paul Rabinow formed the Anthropology of the Contemporary Research Collaboratory (ARC). Shortly thereafter, George Marcus, Director of UC Irvine's Center of Contemporary Ethnography, organized a colloquium on the ARC's conception of anthropological inquiry. The text presented by Collier, Lakoff, and Rabinow at Irvine (here, chapter I) stimulated response by Marcus and led to an email exchange between Marcus and Collier/Lakoff (here, chapter II). This exchange, provoked a discussion (whose full contents can be found at http://anthropos-lab.net/collaborations/concept-work) among a broader range of participants, including Rebecca Lemov, Chris Kelty and James Faubion. This discussion, in turn, inspired a conference panel that took place at the AAA meetings in November 2006 in San Jose, California (the papers are presented in Chapter III). This Exchange documents, then, a process that takes place all the time in academic life, but is rarely captured. Namely, the process through which scholars become familiar with each others' current work, begin a discussion, find fruitful points of intersection or disagreement, and organize a conference panel to explore these in greater detail.

The questions raised in this Exchange may be situated in a longstanding crisis of method in American anthropology. Beginning in the 1970s, anthropologists have engaged in sustained critical reflection on core concepts related to the study of "traditional," "pre-modern," or "primitive" societies. To some extent, this critique was linked to a reconsideration of the value these concepts had in understanding the classic objects of anthropological analysis. But it was also linked to the exploration of new research venues: stock markets, laboratories, pharmaceutical companies, development programs, and so on. It has been repeatedly noted that past concepts and past techniques do not self-evidently speak to these new terrains and objects.[1] But one core element of classic anthropological method has hardly ever been substantially problematized and made the object of discussion: the centrality of ethnography as the defining element of anthropological method. Even in those cases where ethnography has been an object of explicit critical reflection, anthropologists have tended to

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[1] Cf. for example Akhil Gupta and James Ferguson, *Anthropological Locations: Boundaries and Grounds of a Field Science*, Berkeley, the University of California Press, 1997; George Marcus, *Ethnography Through Thick and Thin*, Princeton: Princeton University Press, 1998; Michael J. Fischer, *Emergent Forms of Life and the Anthropology of Voice*, Durham: Duke University Press, 2003; Matti Bunzl, "Boas, Foucault, and the 'Native Anthropologist:' Notes toward a Neo-Boasian Anthropology," in: American Anthropologist, Vol. 106, No. 3, September 2004; James Faubion, George Marcus, Paul Rabinow, and Tobias Rees, *Designs for an Anthropology of the Contemporary*, Durham: Duke University Press, forthcoming 2008.

affirm that long-term fieldwork, and the unique perspective it supposedly brings, ought to remain the defining feature of anthropological method.[2]

The relationship between ethnography and anthropological method is at the center of these questions raised in the Exchange presented here. On the one hand, the participants consider various questions concerning the status of ethnographic authority, and its relationship to the broader problem of method. On the other hand, they explore other models of inquiry – particularly collaboration – and consider its relationship to the norms of ethnographic work. Key questions raised are: Should fieldwork still be regarded as an essential technique of knowledge production? Is it adequate to the changing empirical focus of anthropological production? What are the consequences of the privilege that continues to be given to ethnography? What are the legitimation functions of ethnography in contemporary anthropology? Should ethnography be regarded as a method or merely as one technique among several – some perhaps not yet invented – techniques? What practices and norms of inquiry might orient an alternative discussion of method in anthropology? And what role might collaboration and concept work play in such a discussion?

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[2] Cf. the above, particularly Gupta/Ferguson, Marcus, and Fischer. For an exception cf. Paul Rabinow, *Anthropos Today: Reflections on Modern Equipment*, Princeton: Princeton University Press, 2003.

I. What is a laboratory in the human sciences?

Stephen J. Collier, Andrew Lakoff and Paul Rabinow

"A new 'science' emerges where new problems are pursued by new methods and truths are thereby discovered which open up significant new points of view."[3]

Over the past year we have been developing a long-term collaborative program for work in the anthropology of the contemporary. Broadly speaking, our motivation for doing so arose out of dissatisfaction with what is at least one dominant model of knowledge production in the interpretive human sciences. This model – that of the "individual project" – rests on a myth of *sui generis* intellectual production. The individual project model assumes that interpretive and authorial virtuosity is the mainspring of good work. At its best, it produces genuinely innovative and original scholarship. At its worst, it results in workshops, conference papers, collected volumes and monographs in which the emphasis is placed on individual performance, and in which there is not much discussion or debate about what the key problems for the field are, and how to best approach them – nor is there evidence of shared norms that lead to better understanding of significant phenomena.

In contrast, we wanted to explore a model of academic production that would include individual work but that would also recognize the centrality of – and create organizational space for – serious collaborative work. By collaboration we have in mind two different kinds of work: first, the joint production of papers and research; and second, concept development, collective reflection, and shared standards of evaluation.

We decided to call this collective endeavor a "laboratory." On many important points this endeavor diverges from a laboratory in the natural sciences – as we will describe below. And yet, the rubric of a laboratory has provided a context in which to make explicit, and to critically examine, various aspects of how our collaboration is organized.

At this point, the laboratory remains very much in a process of formation. But over the course of the past year it has begun to function in a practical sense in a number of ways. The laboratory is centered around three principal investigators – Collier, Lakoff, and Rabinow – who have met regularly over this period. It has an institutional home at the Molecular Sciences Institute in Berkeley, California, but much activity has taken place in New York and San

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[3] Max Weber, Werner Sombart and Edgar Jaffé, "Objectivity in Social Science and Social Policy," in Weber, *Methodology*, p. 68, cit. in Rabinow, *Anthropos Today*, p. 36.

Diego. Close collaborative relationships with a broader range of students and colleagues have developed in Berkeley (between Rabinow and a number of graduate students working on security) and in New York (where Collier and Lakoff have collaborated with each other and with Lyle Fearnley). Finally, a number of preliminary projects – empirical "soundings" – have begun, on topics including syndromic surveillance, vaccination, synthetic biology, and risk management techniques.

It is also important to mention that this project is going on in close conversation with several other important attempts to explore new inter-connections among researchers in the human sciences, among them the UC Irvine Center for Ethnography Initiative, the Rice project on the anthropology of expertise, and the BIOS Center at the London School of Economics.

This discussion paper, then, is a kind of stock-taking of a project that is beginning to take shape, but is still in its early stages of development. First, it outlines our motivation for working on new forms of collective and collaborative work in the interpretive human sciences by describing our respective pathways to this project. Second, it describes how we arrived at the laboratory concept and some of the reflections it has provoked relative to dominant models of knowledge production in our part of the academy.

Background

Our motivation for forming a laboratory arose from both long-term interests in problems of knowledge-production in the interpretive human sciences and from short-term challenges to which we felt that a laboratory-type organization would be most able to respond.

For Rabinow, questions around how knowledge is produced in the human sciences have been long-term interests.[4] For Collier and Lakoff, reflection on knowledge production in anthropology began after returning from fieldwork. As is perhaps typical at this stage, questions arose for them such as: how to integrate detailed [their] research material with broader questions in the discipline? What broader claims could be made based on their particular research? These questions led to a series of conversations with Rabinow in Berkeley concerning problems of "method" in anthropology. Whereas most discussion of "method" in the discipline revolved around a specific technique of

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[4] See, for example, Paul Rabinow, *Reflections on Fieldwork in Morocco*. Berkeley: University of California Press, 1977; Paul Rabinow and William M. Sullivan, eds., *Interpretive Social Science: A Reader*. Berkeley: University of California Press, 1979; Hubert L. Dreyfus and Paul Rabinow, *Michel Foucault: Beyond Structuralism and Hermeneutics*. Chicago: University of Chicago Press, 1982. Paul Rabinow, *Anthropos Today: Reflections on Modern Equipment*. Princeton: Princeton University Press, 2003.

data-gathering – namely, ethnography – it seemed important to begin a discussion about the norms of knowledge production in the field, and about shared problems and concepts that might be collectively worked on and developed. These conversations took the form of attempts to specify the meanings and uses of certain conceptual tools for describing research objects – for example, terms like "apparatus," "assemblage," and "normativity." In other words, our effort was to move "methodological" conversation in anthropology beyond the discussion of ethnography.

Over the following years, we undertook, both among ourselves and with others, a number of efforts to initiate discussions about concepts that might link apparently diverse anthropological projects through common problems. Collier and Lakoff organized two AAA sessions related to problems of method and concept-formation.[5] Collier, with Aihwa Ong, put together an SSRC-funded workshop and co-edited a volume, *Global Assemblages*, to which both Lakoff and Rabinow contributed. The volume brought together scholars in anthropology, geography and sociology who shared an interest in concrete practices at the intersection of technology, politics and ethics. The hope was to generate a more sustained conversation about comparable findings and shared concepts, and to create a context in which a more substantive conversation might develop among scholars with knowledge about related issues.

Based on some of the contributions to this volume, Collier and Lakoff wrote an article, "Ethics and the Anthropology of Modern Reason," whose goal was to develop a concept that could both link together diverse individual research projects and generate novel insights through the comparison of cases.[6]

All of these prior efforts were rewarding at a number of levels. But from the perspective of developing new modes of collaborative and collective work, they were frustrating. Rabinow, for his part, found that the response to his books on method was limited, and that the institutional conditions for collective work in anthropology were disappointing. Meanwhile, *Global Assemblages* stemmed from a rewarding and productive event – a conference in Prague in 2002. But ultimately the project served the function that most collective publications in anthropology served – to offer a vehicle for roughly likeminded scholars to publish an article on whatever it was they were already doing. In this sense, as an effort at tightening a community around a clearer sense of common problems or debates, its success seems to have been limited. This was perhaps due to the pressures of individual production, and the difficulty of getting a sustained conversation going among far-flung people.

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[5] These included a panel on "Object and Method in Contemporary Anthropology" in 2000 and on "Technologies of the Human" in 2001.
[6] Andrew Lakoff and Stephen J. Collier, "Ethics and the Anthropology of Modern Reason." *Anthropological Theory*; Aihwa Ong and Stephen J. Collier, eds. *Global Assemblages: Technology, Politics, and Ethics as Anthropological Problems*.

These long-standing interests in collaborative work and inquiry were renewed by a series of challenges. A German graduate student at Berkeley, Tobias Rees, who had worked with Rabinow on *Anthropos Today*, proposed a doubtless naïve but nonetheless inspiring vision of a community along the lines of a group Hans Blumenberg was involved with in Germany. This group would meet periodically to pursue a kind of "philosophical symposium" where thinkers engaged in open and convivial exchange. It was unclear what exactly such a community would look like for anthropologists given the structure of the U.S. academy in the early 21ˢᵗ century, but it would clearly involve reflection on the generation of shared topics of inquiry and on the conditions under which collaboration could take place.

Meanwhile, Roger Brent, a molecular biologist and head of the Molecular Sciences Institute, approached Rabinow with a series of challenges: what did the human sciences have to say about biosecurity and biodefense? And what contributions had anthropology made to the broader, non-academic world since the days of Ruth Benedict? Rabinow took this challenge as an opportunity to invite Collier and Lakoff – located, respectively, in New York and San Diego – to reflect on what kind of collaboration might be possible.

This topic – biosecurity, and, more generally, new problematizations of security – was complex and heterogeneous. We all had areas of expertise that were orthogonal to but not directly about the topic. What is more, there did not seem to be compelling work either in anthropology or, more broadly, the areas of critical social theory upon which anthropologists customarily draw, that could orient us conceptually to contemporary security questions. Finally, this was a complex field that was developing simultaneously in many places. Leading labs in the molecular sciences were clearly one place to look. But biosecurity clearly would have to be traced through a number of other domains and sites in which simultaneous developments were taking place: public health organizations, security think tanks, the U.S. military, international organizations, and so on. Consequently, the issue was not only that the topic of security provided an excuse for doing something that we already wanted to do – i.e. work together. Moreover, this was a topic that seemed to demand collaboration, active work on concept formation, multiple soundings in diverse sites, and a research infrastructure that would allow an approach that was quite different from the individual project model.

Why Laboratory?

Initially, calling this kind of collaboration a "laboratory" may seem surprising, since on many important points *any* endeavor in the interpretive human

sciences has norms, practices, and goals that are very different from those of a laboratory in the natural sciences (**see table 1)**. Thus, the term "laboratory" does not reflect any aspiration to move anthropology to the stage of a mature discipline that would finally achieve a positivistic scientific rigor (presumably like economics). We are not suggesting that anthropology can or ought to be a natural science. Nor do we propose a return to the days of the Human Relations Area Files and similar efforts, which sought to generate universal claims about the human condition by sending individual field workers off to multiple sites and then gathering together the resulting data under the rubric of a general theory of social development.

What is more, there are many ways in which the practical organization of our collaboration differs from a laboratory in the natural sciences. It is not confined to a single site but is, rather, multi-sited. Initially, as noted above, Berkeley and New York are the major centers of activities in our lab, although it may grow to incorporate other sites. Our project does not involve the kind of division of labor or hierarchy found in a scientific lab. We do have an established hierarchy when it comes to dealing with administrative questions. But in matters of substance, we have none of the scientific lab's sense that the intellectual direction is set by a "head" of the lab. Rather, research is tied together through a looser structure ofshared interests that are mutually inflected through discussion and concept development.

That said, we have found the model of a laboratory helpful in thinking about our goals for this project, and for the kinds of questions we want to raise. We are very much intrigued by the idea of greater rigor and seriousness in subjecting our claims to tests of adequacy through experiment. But it is intriguing and challenging to ponder whether they could rest, as in a lab, on collective agreement and impersonal norms. At the same time, thinking about our collective endeavor as a laboratory has provoked reflection on the forms of interpersonal interaction and the infrastructures appropriate to – and necessary for – such an endeavor. Here work from the social studies of science has provided some useful insights. This work has shifted understandings of how scientific knowledge is generated from concerns with theories of scientific method to an emphasis on concept development, material practices of experiment, and informal norms that make possible trust and credibility. Both in the natural sciences and in our vision of a laboratory in the human sciences the *context* of a laboratory is critical to successful experimentation: informal norms, interpersonal relationships, material infrastructures, etc., are all crucial to how concepts, experimental objects can be stabilized, criticized, and worked on in the process of scientific inquiry.[7]

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[7] See Karin Knorr-Cetina, 1992 "The Couch, the Cathedral and the Lab: On the Relationship between Experiment and Laboratory Science", in A. Pickering (Ed.), *Science as Practice and Culture*, Chicago, University of Chicago Press.

Table 1
A Natural Science Laboratory versus a Laboratory in the Human Sciences

	Natural Science Laboratory	Laboratory in the Interpretive Human Sciences
Goals	Generate and stabilize novel objects of knowledge and intervention Develop knowledge or technical capacities that can be reproduced beyond the space of the laboratory	Develop concepts that make it possible to identify significant phenomena Reframe problems; diagnose stakes in problematic situations Focus on specificity, making contingency of things visible
Material-institutional form	Physically bounded; dependent on experimental devices; funding is critical Authoritative role of Lab Director in determining research priorities	Physically dispersed; virtual infrastructure; loose and flexible interrelations between projects Seniority guides key organizational decisions but not directions of research or validity of claims
Everyday practices	Many people working in different roles on given experiment Lab meetings to coordinate activities, develop focused lines of investigation	Development and refinement of concepts; proliferation of sites Independent research, comparison of findings
Authorship and originality	Contribution to "discoveries" credited through journal authorship Erasure of personality of individual researcher in collective practices of normal science	Creation of knowledge remains author-centered Explicit reflection, Negotiation around various forms of authorship
Relationship to broader field	Competition/ collaboration with other laboratories pursuing similar lines of investigation	Loose ties to other human science investigators
Relationship of investigator to objects of investigation	Transformation, objectification	Adjacency, which *may* include transformation, objectification

The rubric of the laboratory has also forced us to think actively about the nature of collaborative work, originality, and authorship, and about the relationship of collective tasks such as concept building to what seem to be individual tasks, such as ethnographic fieldwork or focused historical research. Our object of inquiry is too extensive and heterogeneous to be successfully approached according to the traditional model of the single ethnographer in a field. Thus there are things we can achieve in a joint project that could not be done individually. In turn, our sense is that the collaboration and argument enriches and improves the individual work we are doing. Moreover, the collaboration has provided an opportunity to try out new ways of generating knowledge in the human sciences.

At the same time, the collective project demanded reflection – on authorship for example: we needed new ways of thinking about how knowledge is generated and how credit is given. Here it is useful to contrast the laboratory model with the individual project model.

The Individual Project Model versus the LAC

In developing our thinking about the laboratory model, it has been useful to distinguish it from the individual project model, mentioned above (**see table 2**). Obviously such a distinction always has the risk of caricature. In developing it, we do not mean to attribute any particular position to specific authors or groups of authors, but rather to propose some generative contrasts that, we hope, can serve to promote more explicit reflection on matters of collaboration and the norms of knowledge production in our field.

(1) Infrastructure and Institutional Organization

Work according to the individual project model is done, for the most part, by scholars who hold professorships in universities, and they derive financial and institutional support from universities. The major infrastructures for communicating work among scholars are conferences, journals, and academic presses, along with personal communications among loose networks of like-minded thinkers. On the one hand, the individual project model is not interested in explicit reflection on collective norms, since the focus is on individual production. On the other hand, collective decisions at the level of the institution (eg. hiring or tenure) must be made. This means that tacit norms guide institutional decision. The laboratory also depends on the university, at least in the sense that most participants (whether graduate students or faculty) are dependent on financial support from the university. But its structure is adjacent to a university. It is also adjacent to the institutions of professional association conferences, journals, and academic presses. Members of the laboratory – either individually or collectively – engage in these institutions. But the

Table 2: The Individual Project Model versus the LAC

	Individual Project Model	Laboratory for the Anthropology of the Contemporary (LAC)
Infrastructure and Institutional Organization	• Academic department in university. • Conferences, journals, academic presses. • Networks, loose affiliations, based on mentor relations, shared topic areas. • Concern about the legitimacy of hierarchies; role of hierarchy is hard to understand.	• Dependent on university but organizationally adjacent. • Virtual infrastructure linking a finite number of sites; meetings of principles; intensive work on discussion papers. • Ongoing relationships: role of intellectual trust (based on sense of shared concepts, problems); but also changing nexus of informal contact and collaborative work. • Explicit and openly discussed lines of authority for organizational decision-making clearly separated from authority in making knowledge claims.
Authorship and Originality	• *Sui generis* intellectual production; connections among authors mostly through shared invocation of "theory." • "Branding" of original concepts by individual authors. • *Collected* work (in volumes, based on conferences, workshops).	• Recognition of diffuse character of authorship; individual authorship as a "problem" requiring negotiation, deliberation. • Emphasis on the development of shared concepts through a collective process. • *Collective* work – intense discussion, argument in production of texts.
Experimentation and Validity	• Experimentation with form in writing, styles of fieldwork. • Avant-garde effort to challenge/break away from existing norms. • Crisis in thinking about what constitutes a valid claim. • Authority connected to individualistic elements of fieldwork process and writing: "thick" description; virtuosic interpretation and writing.	• Experimentation as a way to put concepts to the test, established agreed upon demonstrations of adequacy. • "Secessionist" effort to conserve what remains contemporary in existing norms and to adapt them or innovate in new contexts in relation to new problems. • Search for impersonal methodological norms: Are concepts adequate for clarifying significant problems? Are concepts diacritical, i.e., do they make the distinctions that matter? • Recognition of legitimate authority based on knowledge rather than status.

laboratory is based on other infrastructures – virtual infrastructures are particularly important – and other kinds of interpersonal relationships, which have to be explicitly worked on and cultivated. Finally, the laboratory has explicit lines of authority, particularly in matters that are purely administrative. But it is the aspiration of the laboratory to separate these formal hierarchies from authority in making knowledge claims.

(2) Authorship and Originality

The individual project model is based on what we think is a myth of *sui generis* intellectual production. In anthropology, this tends to mean that the force of creative energy   is assumed to arise from a unique encounter with the field, and from the interpretive and authorial virtuosity of an individual. "Thick description" and "brilliance" are the marks of good work. Prominence is gained through "branding," by which individual scholars are associated with specific concepts that they have invented. The product of such work may be *collected* in volumes that serve the purpose, largely, of assembling what authors are already doing under a single cover. But collected volumes are rarely more than the sum of their parts, and they rarely reflect a collective process of conceptualization and thought.

The aspiration of the laboratory, by contrast, is to more fully recognize the diffuse character of authorship, as it is formed through conversations, borrowed concepts, and exposure to the work of scholars working on related topics. In this sense, in the laboratory setting authorship is a "problem" to the extent that assigning individual authorship is always problematic. As a consequence, the norms of credit and of authorial claims are made an explicit object of reflection and discussion. Finally, a laboratory creates *collective* rather than *collected* work. That is, it seeks to create work that is truly shaped by the collective context in which it is generated.

(3) Experimentation and Validity

One important norm of work in the individual project model is "innovation," not only in the adequate description of phenomena but in the form of writing and in theory. In this sense, it seems to follow many aspects of the model of the artistic avant-garde. It seeks to challenge or break away from existing norms. And the act of innovation, as in the artistic avant-garde, is very much focused on the individual creative experience. The validity of such innovation, therefore, is profoundly personal. It seems, however, that this avant-gardist model has not, in the interpretive human sciences, led to a satisfactory model for thinking about what counts as good work, or about what counts as an authoritative claim.

In a laboratory, by contrast, "experiment" does not refer to textual experiment. Rather, it refers to "controlled experimentation" that might lead to critical rectification of concepts and claims. In the course of experimentation concepts are put at risk through their use and interaction with cases – either they work or not. Here some insights about how experimental systems work in the natural sciences may prove fruitful. These systems are material and discursive arrangements for generating new things; they involve developing and sustaining a set of shared objects.[8] This vision of experimentation and validity the validation of knowledge-claims seeks to be *depersonalizing* rather than emphasizing the virtues and talents of an individual author.

An Experimental System

How, then, does the laboratory function in practice? We are engaged in several different kinds of work, including: regular meetings among the principals to hash out ideas, which have led to several jointly authored papers; targeted collaborations on specific projects with other members of the laboratory – for example, Collier's work with Lyle Fearnley on syndromic surveillance; field experiments, in which two or three members of the lab interview a security expert together; and an experiment in teaching a graduate seminar with a laboratory approach, now being undertaken by Rabinow.

A critical part of the laboratory's projects is to develop or hone conceptual tools and put them in motion – in writings, presentations, and conversations. We have been working on several different types of such tools. Some concern our relation to our field of inquiry – examples are "second-order observation," "adjacency" and "technical criticism." Other concepts seek to describe the types of objects we are interested in, such as "apparatus," or "normative rationality." Finally, there are conceptual tools for analyzing the problematization of security. Here we have been developing the concepts of "preparedness" and "vital systems security."

Collier and Lakoff constructed these latter concepts in relation to their own empirical soundings, such as historical research on civil defense and emergency management, as well as close work with colleagues in the laboratory. For example, Lyle Fearnley's research into syndromic surveillance helped them to elaborate a key distinction between insurance and preparedness as forms of rationality. Similarly, Dale Rose's work on the smallpox vaccination program helped them to see how elements of public health apparatuses may be retooled, through a rationale of preparedness, into aspects of vital systems security.

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[8] Hans-Jorg Rheinberger, *Toward a History of Epistemic Things. Synthesizing Proteinsin the Test Tube*. Stanford: Stanford University Press 1997.

Thus we are honing concepts as tools that can function in an experimental system; and trying to establish standards amongst ourselves. What seems unclear at the moment, and what we are exploring, is how far these experimental systems can be extended, and what kinds of collectivities they might include.

II. What is Concept work? An Exchange

Stephen Collier, Andrew Lakoff, George Marcus

ARC (SJC and AL): George posed a number of questions about what "concept-work" is and how it differs on the one hand from branded terms such as "friction", and on the other hand from "field-work" as method:

a. How is it collaborative?

b. What does concept-work make possible that is not possible with the use of branded concepts according to the individual project model?

c. How does critical rectification happen? What role might informants play in such critical rectification, if any?

• GM: WHAT DOES CRITICAL RECTIFICATION MEAN? MY SENSE IS THAT THERE IS STILL AN INHOUSE SET OF USAGES/LINGO AMONG ARC RESEARCHERS — SO THERE IS STILL AN OPAQUENESS BY THE OUTSIDER LOOKING IN. A DESCRIPTION OF THE INFORMAL CULTURE OF WORK OR PRACTICES WITHIN ARC — SOMETHING VERY SIMPLE — MIGHT BE HELPFUL AND MAKE THINGS MORE EXPLICIT. I KNOW YOU GUYS ARE DOING A LOT OF WORK. I AM JUST NOT SURE THE FORM IT TAKES.

o ARC: By "critical rectification" we mean: we can discuss our concepts and findings, and we can have an argument in which – through the application of shared standards and understandings – we can figure out that some formulation/idea/distinction was wrong, in need of greater precision, or in need of reframing in relationship to other contexts and distinctions.

o Here is an extended example that might provide a sense of how this happens in terms of the "internal workings" of the collaboratory, per your question above. There are also some citations to give you a sense of how this process has related to the production of more traditional products of research (published articles and working papers):

o When we began our current project, we defined security, following Niklas Luhmann, as the transformation of uncertain dangers into calculable risks (Collier, Lakoff, Rabinow, 2004). Thus, in our initial formulations (in our NSF grant proposal, for example), we proposed to study security "initiatives" that sought to transform emerging biothreats into calculable risks.

o In his initial fieldwork, Lakoff began to examine one such initiative: a collaboration between the Cold-War era think task RAND and a firm that

specializes in modeling catastrophic events, Risk Management Solutions. The goal of the collaboration was to integrate terrorism expertise with probabilistic models of catastrophic events in order to make possible a market in terrorism insurance. The case presented an interesting twist on our initial formulations. Lakoff was struck by how this collaboration employed scenarios as non-quantitative techniques for approaching security threats whose likelihood and possible impact could not be calculated. He observed that these security techniques of "preparedness" could be distinguished from Luhmann's understanding of security as risk- management, in that they did not necessarily involve quantification (Lakoff 2005). This distinction – of risk versus preparedness – seemed consistent with a large theoretical literature on risk (that includes central contributions by Beck and Ewald), which claims that contemporary society must deal with threats that are incalculable. But Lakoff's initial work on scenarios suggested a direction of inquiry one might pursue to fill in the lacunae in this theoretical literature, identifying the kinds of techniques used to manage incalculable threats.

o This initial distinction seemed worth trying out more broadly as an element in the toolkit of our collaboration around security. Thus, we began to look collectively at some documents in which "preparedness" was articulated as a normative rationality for dealing with security problems – including a DHS national preparedness plan that had just been released (these conversations were ongoing in spring and early summer 2005). An argument arose in the collaboratory. On the one hand, the DHS document relied heavily on a set of scenarios of catastrophic events, and thus did not seem to be engaged in "risk-based" calculation. On the other hand, it drew on techniques of quantification and calculation, for example, in risk-based budgetary distribution formulae that closely resembled similar formulae that Collier studied in his work on social welfare in Russia (Collier 2004). In other words, "calculation" and techniques of quantification seemed to be an important part of the DHS approach. And this framework for "preparedness" was quite explicitly engaged in risk management, apparently confusing our initial distinction between risk and preparedness. A further specification and conceptual refinement, we ultimately agreed, was required in order to adequately characterize how this form of security functioned.

o Here is where an empirical "sounding" (discussed below) played an unexpected role. We have a research assistant, Lyle Fearnley, who has been doing terrific work – through interviews and documentary research – on "syndromic" surveillance systems for detecting outbreaks of infectious disease. Government agencies charged with "security" questions have become very interested in the ability of such systems to detect a health "event" in a population in real time, particularly given the increased concern with the threat of a bioterror attack since 9/11 and the anthrax letters.

o As we were engaged in this discussion of preparedness, Lyle was pursuing a historical chunk of this research (Fearnley 2005), which focused on the moment of "epidemiological transition" in the United States. One feature of this transition was a shift in the focus from diseases regularly occurring in a population to disease "events," that is, diseases whose dynamics were not known. Experts thus identified a need for real-time identification of health events in a population. Their efforts are one important genealogical precursor to contemporary syndromic surveillance (and, in a certain way, to contemporary 'preparedness').

o Fearnley reported that first order actors, in this historical context (immediately after World War II), identified a distinction between the "archival" knowledge required for the management of epidemic disease and the real-time knowledge of populations required to deal with disease events. This distinction helped to clarify the dispute in the collaboratory, and pushed the process of conceptualization forward. We agreed that the initial distinction – between preparedness and "risk" – was not exactly the right distinction, and that the distinction between calculability and non-calculability advanced in the "risk" literature was also not quite the right distinction. Rather, the salient contrast was between a certain risk technology – insurance – and preparedness. Insurance is based on actuarial analysis that draws on archival knowledge of populations. Preparedness, by contrast, draws on techniques of what we later called "imaginative enactment" (Collier and Lakoff, 2006) to deal with low-probability, high-consequence events about which no archival knowledge exists.

o The payoff of this clarification can be identified on a couple different levels. On the one hand, it led to a critical intervention into discussions of "risk society" by people like Beck and Giddens (and responses to this work by scholars of "governmentality"). Given our research, we see two very fundamental problems n this literature. First, they have identified as important the emergence of events to which insurantial mechanisms don't apply. But they have not yet found a way to investigate what comes next, or what techniques are used to manage such events. Second, we think that they have the basic distinction wrong. It is not a question of "calculability" per se but of the kind of calculability, the techniques of quantification, and the purposes for which they are used. It is not a question of "risk" but the techniques of risk assessment, how they change in relation to different sorts of objects, and how they are articulated in certain apparatuses. We now have a sophisticated vocabulary for thinking about these things. The theoretical literature does not.

o On the other hand, we have been able to confirm and build on this distinction in a number of other areas – for example, in the work that one of our research associates, Dale Rose, has been doing on the CDC and smallpox vaccination strategies. It is now also informing PR's research into how life scientists and

government regulators are proposing to regulate synthetic biology, as well as further historical work that SJC and LA are doing on preparedness and the political logics with which it has been associated.

• IS IT FIELDWORK OR JUST 'INVESTIGATION' ON THE MODEL OF THE JOURNALIST OR DETECTIVE? I ABJURE FIELDWORK STORIES, BUT A BIT OF THIS MIGHT BE NEEDED HERE.

o ARC: This distinction between "fieldwork" and "investigation" is not clear to us, and we would be interested to know when something counts as "fieldwork" – whether, for example, the activity of a political scientist who spends two years in the field counts. Certainly, the term is used in that context. Or, to take a case from the collaboratory: Fearnley's work has involved looking at documents, attending a couple conferences, and talking to a handful of experts. He has produced penetrating conceptual insight. And indeed, all of us are combining things like intensive discussions with experts, attending security-related events, and documentary research. Does this count as fieldwork?

o A better approach to this question, however, might be to take a step back. An important impetus of our work is to think more about the status of "fieldwork" or "ethnography" in relation to the problem of methodology in anthropological discussions. We have been saying for a while (see Collier and Lakoff, 2000) that anthropologists tend – mistakenly – to limit discussion of "method" to discussions of fieldwork and writing. In our view, fieldwork is a technique – or, perhaps, a set of techniques – but not a methodology. There may have been previous configurations of the discipline in which it is was the central technique of an anthropology that was committed to a rather holistic version of the culture concept, but it seems those days are gone.

o We would prefer to deconstitute the idea of "fieldwork" and to ask what it is, more concretely, that is being talked about. It seems that in anthropology fieldwork can refer to interviews, observations of (and participation in) meetings, informal discussions; and also, close reading of documents produced by actors. Our main point is that it is good to reflect on these techniques, both individually and in their interaction, but that is not the same as reflecting on "methodology" – which concerns, among other things, how these techniques of data-gathering interact with concept formation and the establishment of collective standards, norms, and conventions to yield meaningful claims, and meaningful progress in thinking. So one way to put this point is that we are not focused on fieldwork per se, but on the process of interaction between concept-work and fieldwork. So the collaboratory has a number of people doing "fieldwork" of various kinds in various areas – syndromic surveillance, vaccination, synthetic biology, civil defense, strategic bombing, and so on. The question, then, is how to generate a process in which this collective work feeds into broader conceptual issues, and how, in turn, these conceptual issues

generate specific questions to be approached through fieldwork.

ARC: Some background:

a. Most discussions of method in anthropology have been mostly restricted to the practice of ethnography and writing.

b. These discussions left un-posed a series of questions: How does one decide where to do fieldwork? How are significant problems identified? What conceptual framework is used in the field?

• GM: WHAT ABOUT MARILYN STRATHERN, ON ONE HAND (THIS SEEMS TO BE WHAT HER DIFFUSE WRITING IS ALL ABOUT THESE DAYS; SHE CELEBRATES THE INDETERMINANCIES, THE SURPLUS OF FIELDWORK AND, ON THE OTHER, RHEINBERGER ON THE OTHER HAND, WHO HAS EVOKED A VERY PLEASING CONCEPTION OF PRACTICE THAT IS INCREASINGLY INVOKED BY ANTHROPOLOGISTS AS WHAT THEY DO. STRATHERN IS THE MESSY 'JUST DO IT' VERSION; RHEINBERGER OFFERS A NOTION OF DESIGN. IN EITHER OF THESE CASES, IS CONCEPT WORK CONSISTENT WITH WHAT THEY ENVISION. THINKING THROUGH YOUR PRACTICES IN THE CONTEXTS OF STRATHERN AND RHEINBERGER MIGHT BE INTERESTING.

o ARC: Both of these are helpful points of reference, and seem useful to us. We agree that developing techniques to generate unexpected findings is important (although we think that it is in need of further specification and can't be claimed uniquely for anthropology; after all, Rheinberger is talking about how natural scientists use research design as systems for generating surprise). What we think might be missing in both cases is what is often missing from discussions of method in anthropology: analysis of orientation, of significance, and of problem formation. These are the questions posed above: "How does one decide to do fieldwork?" "How are significant problems identified?"

o So Strathern argues that an important aspect of fieldwork is its indeterminacy – that one goes into the field and one collects more than one "needs," because, presumably, one does not yet know what is significant. This seems right, and resonates with our process in the current project. For example: a year ago, we had no way of knowing that we would be working on theories of strategic bombing or the history of exercises in the military! And the only way to "discover" that these things were relevant to our project was to go into the field with at least some indeterminacy, and a sense that there was a period of thrashing about without knowing what is going on that had to be part of the process. But we would want to add that one does not do this in an unstructured way, or in a way that is not guided by an understanding of what significance is. Where does one go? Whom does one talk to and what about?

What books does one read? To describe this simply in terms of a method that collects more than it needs, that emphasizes indeterminacy is just mystification. There are reasons that we go one place rather than another: we are interested in bio-power, we are interested in expertise, we are interested in the interaction between security and social welfare, etc. So we think that the naïve "entry into the field" stories are just bad accounts of what anthropologists actually do. So we want to describe how a process of relatively open-ended searching might be linked to a rigorous process of concept-formation.

o We would say the same thing about Rheinberger's understanding of experimental systems in the sciences as techniques for generating surprise. Again: on one level this seems right. As PR has often argued, the point of studying emergence is that one does not know what one is going to find. But this way of being "experimental" is not avant-gardist. It does not simply try to undermine existing norms or to create "surprise" for its own sake. A scientist needs norms, conventions, and shared understandings about interesting or significant problems to make an experimental system meaningful. These norms, conventions and understandings will be different for a natural scientist and for an anthropologist, and we are suggesting that more reflection on what they are in anthropology is needed.

ARC: These [questions] lead to further methodological issues:

How are knowledge claims generated, and defended? How might such claims contribute to broader discussions – and to a project that advances thought? In response, we began to develop a way of collectively developing and refining concepts in relation to findings in "the field".

• GM: THE LANGUAGE HERE REMINDS ME OF THAT OF AN EARLIER FORMAL TENDING PHILOSOPHY OF SOCIAL SCIENCES. WORTHY QUESTIONS TO ASK BUT THE LANGUAGE SEEMS A THROWBACK — IS THIS INTENTIONAL? COULD CONFUSE SOME OF YOUR AUDIENCE.

o ARC: We want to be provocative, at least in the sense that we want anthropologists to be less complacent, and more critical and reflexive, about a range of taken-for-granted assumptions about method that have taken shape over the past couple decades. But we don't think that this is a throwback. It seems to us that these are quite contemporary debates that are being thrashed out in anthropology and elsewhere.

o In asking questions like "how might move thought forward" we are not talking about a naïve objectivism. Rather, it is a pragmatist epistemology that is consistent with Dewey, Rorty, etc. It acknowledges, as Paul has written (2004) that any reasonably coherent theory of scientific knowledge acknowledges that it is based on concepts that are constructed. But this does not mean that one

does not seek to advance understanding, successfully and productively reframe problems, make defensible claims about the world, or clarify concepts in a fashion that can be subjected to critical rectification through impersonal norms.

• GM: ARE REFINED CONCEPTS THE ACTUAL FINDINGS IN/FROM 'THE FIELD' OR ARE THESE FINDINGS SOMETHING ELSE? AGAIN, FIELDWORK SEEMS TO BE DISTINCT (AND INDIVIDUAL) BUT I AM NOT SURE. ALSO I AM NOT SURE WHAT THE FIELDWORK IS? INTERVIEWS ONLY? BONDING WITH PARTICULAR INFORMANTS/ENTERING THEIR WORLDS IN A SUSTAINED WAY?

o ARC: We do think that refined concepts are one of the important things you get from our kind of inquiry. But they are important because they are a dynamic part of inquiry: they are helpful for identifying significant problems, identifying sites that are worthy of investigation, and of developing new kinds of critical and reflective understanding of significant issues. The discussion above about "risk" and preparedness is an example.

o On "fieldwork" – it would, again, be helpful for you to say more about what that term specifies for you. As noted above, we tend to think in terms of specific techniques, such as expert interviews, close reading of documents, etc. Bonding with informants per se has not been the emphasis of the project so far. But there is another element of what we are up to that is, in some sense, characteristically anthropological, which is the attempt to see how different security rationalities work by "entering the world" of experts who are working them out.

ARC: How do we find/ agree on a shared problem? How do we think we have made progress on a solution – both individually and collectively? Here it may be useful to describe how our work on contemporary security has unfolded.

The collaborative process required a shared sense of what constitutes a significant "finding." Here there were a few important common points of reference:

The emergence of a new problematization as an event; an interest in looking at recombinations of existing elements into new forms; the study of rationalities, and their concrete instantiation in dispositifs; an interest in how human life is taken up as a political problem and is subject to technical intervention; the assumption that one studies this by looking at the practices of experts; the aim not of making a broad generalization or theorizing, but of specific diagnosis.

• GM: YES, BUT HOW DOES THIS TRANSLATE INTO FIELDWORK EFFORT IN A MORE LITERAL WAY? IS ALL OF THIS HAPPENING IN THE FIELD? OR IN

THE LAB? MY OLD QUESTION — IS FIELDWORK WITH EXPERTS CONCEPTUAL WORK TOO — IF SO, HOW IS THIS CONCEPTUAL WORK DIFFERENT FROM THE CONCEPTUAL WORK OF THE RESEARCHERS?

o ARC: This is a helpful question. As you know, a major part of Paul's approach (at least since French Modern, but really it is very central to Reflections on Fieldwork) has been the study of observers "in the field" who are also engaged in concept work. And this is definitely part of our past and current projects. We are interested in studying experts, or, better, what Hacking calls "styles of reasoning" that experts employ, and in understanding how these styles of reasoning give shape to institutional responses that are part of new problematizations.

o But we should emphasize that – unlike those who have been trying to draw an analogy between anthropological knowledge and the knowledge of the experts we study – we don't think that all forms of knowledge are structured in the same way. It seems to us that there is a clear distinction between the kind of concept work that we do and the kind of concept work done by those in the field – the "first-order" observers.

o Let us give an example. We have been following (in the sense of reading the writing of, and listening to the talks of) a guy named Stephen Flynn. Flynn is one of our exemplars of a "vital systems security" expert. He is defining a very distinctive conceptual and practical position in contemporary debates around how to deal with security problems. So, for example, he has been engaged in a debate with another expert – one James Carafano, who is, in our terms, a "sovereign state security" guy – over the problem of port security. Flynn wants to see it as a vital systems security issue (we need to have systems assurance technologies that allow us to identify rapidly the source of a security breach in the international shipping system so that "auto-immune response" does not shut down the system). Carafano thinks that shipping is a highly unlikely channel for terrorist attack, and that it would be a waste of billions of dollars (and an unacceptable burden on international commerce) to install such a system. Much better, he argues, to find the bad guys and kill them – a classic case of what we call a logic of "interdiction." So these "first order" observers argue, try to convince politicians, influence spending, marshal evidence, etc. Those are the stakes of their game.

o For us, something very different is at stake. We are interested in the broader rationality that they are working with, and in arriving at a conceptualization of it that points toward critical diagnosis: i.e. that suggests where it comes from, and what is at stake in its current formations. We are open to the possibility that a productive exchange could develop with this kind of conceptually-oriented first order observers, but we are quite clear that the aims and norms of their knowledge practices are distinct from ours.

ARC: In the security project, an initial question was: "what is a bio-security threat and what apparatuses are emerging to manage it?"

• GM: IS THIS QUESTION OF THE PROJECT AS WELL AS A QUESTION THAT THE EXPERT SUBJECTS ASK?

o ARC: Per the above, experts in the field are asking themselves what the threat is, and how to best approach it; we are analyzing how they do this – how they pose problems of security, how security becomes a certain kind of problem. Of course, this distinction is not ours. This is what Foucault said he was doing in all his methodological writing (how did madness become a certain kind of problem? How did criminality become a problem? How did sexuality become a problem?), although his description of the object of analysis shifted over time (episteme, discourse, apparatus).

ARC: At this stage, one could: (1) move directly into fieldwork in a site of "biosecurity expertise," and describe what actors are doing; (2) develop and seek to brand a concept that functions by itself and seems to offer a position of critique; or (3) pause and try to figure out what is meant by "security" - not in an abstract way, but in the way that it is being used by experts in domains associated with security today.

• GM: HOW IS #3 DIFFERENT FROM #1?

o ARC: This is discussed a bit above: It seems to us that #1 subscribes to a kind of naïve empiricism that suggests everything one needs for understanding, or diacritical analysis, or inquiry, or whatever, is waiting for you in the field. All you have to do is "be there" with the proper "anthropological" ethos of indeterminacy, interest in finding surprise etc. As noted above, this seems to us both methodologically unsupportable and a bad account, in any case, of what actually happens in anthropological inquiry.

o We gave one example – above – of how we think that a dynamic process of conceptualization might relate to "fieldwork." Perhaps it would be helpful to give another that indicates the costs of the first approach noted above:

o There has, as you know, been a huge amount of writing in the "critical social sciences" – including in parts of anthropology – about security in recent years, especially after 9/11. In reading this literature, it's astonishing to what extent it frames the basic problem of security today as one of "militarization" of the civilian sphere. If you want an example, check out something written recently by Ann Stoler (an article in the Radical History Review, for instance), who is working on some of the same documents produced by DHS that we are working on (in other words, the "field" is the same). For her, the story of DHS is

a story about militarization. What is striking from our point of view is that she has not even posed the question: "what is security?" One of the advantages of making this an object for critical reflection before diving into a research site is that we now understand that security does not always mean guys with pressed uniforms and shiny boots. What is more, the history of preparedness in the United States specifically includes a very deep concern with the relationship between "military" and "civilian" affairs, and the concerns with militarization have most powerfully come from conservatives who are against the federal government, and for the system of free enterprise. So at least one thing that #1 can get you is confused, both empirically and politically.

ARC: Our early empirical soundings made it clear that "biosecurity" and "security" were terms that were in flux, with multiple possible referents, not necessarily shared among the various actors we were looking at. Thus, they were not "analytic." We needed to develop concepts that would enable us to define productive sites of inquiry and move toward diagnosis.

• GM: WHAT ARE EMPIRICAL SOUNDINGS? SORRY FOR THE PICKINESS.

o ARC: A sounding means dipping a fathom into the water to test the depth. – so, metaphorically, this means dipping into the water to get some quick empirical orientations that can feed into problem-formation and the refinement of concepts. In the example we gave above, the work that Lakoff and Fearnley were doing early on gave us some empirical soundings to check the depths: are our concepts right? Have we framed the scope of our domain of interest properly? An advantage of using such soundings is that they allow us to assess a field without actually committing ourselves to long-term fieldwork in the old sense, in part because we don't know what the problem is.

o It is also worth noting that this is an example of an area in which collaboration, along with some hierarchy and some division of labor, offers something essential that an anthropologist working individually might not be able to do. What we call a "sounding" in Fearnley's case was actually six months of intensive work on a project that also was his senior honors thesis at Columbia. So it was a big project. But for us it came at a step in our broader project that we could use it in the way we described. And, given the authority relationships, this did not pose any problems (although it did take due care with respect to credit, etc.).

ARC: We shared the background assumption that something about the relation of "security" to "biopolitics" was important to figure out. This assumption was somewhat contingent: it had to do both with our backgrounds and with the fact that we had gone in to the project concerned with something we were calling "biosecurity," but whose contours were unclear.

The shared question gradually became: "how has collective security been re-problematized in the U.S., in the wake of the Cold War and 9/11?"

• GM: WHY IS THIS NOT MOST COGENTLY THE WORK OF THE HISTORIAN RATHER THAN THE ANTHROPOLOGIST? TELL ME AGAIN WHY SORTING OUT THE HISTORY OF CERTAIN ESTABLISHMENTS OF EXPERTS DOES NOT DO MOST OF THE WORK OF MAPPING THE CURRENT CATEGORIES IN USE. THE BURDEN OF ARC (OR LAC) IS TO DELIVER A FINDING THAT IS DISTINCT FROM WHAT THE HISTORIAN DELIVERS OR A SHREWD POLITICAL SCIENTIST DELIVERS WHO HAS BEEN AROUND FOR AWHILE — SAM POPKIN COMES TO MIND WITH WHOM I SPENT A YEAR AT CASBS.

o ARC: The distinction you are drawing here between anthropology and history is not clear to us. In the sense noted above, we would think of historical research as a technique of inquiry, rather than a methodology. (The fact that history is a "discipline" is another problem that probably deserves some reflection.) We feel – and the practice of anthropologists would seem to justify this feeling – that this technique is appropriately employed by anthropologists in various contexts. What we are trying to talk about is not on the level of technique but on the level of methodology or mode of inquiry. So, from this perspective, history versus the anthropology of the contemporary seems like the wrong distinction.

o As to Popkin: we could read him and try to give a better answer. It is entirely imaginable that a sensitive political scientist could come up with the distinctions we have come up with. There are certainly (Weberian) traditions in political science that do terrific work around concept formation (the literature on democracy and democratization, for example). That said, the literature in political science doesn't convince us that the discipline has produced the kind of insight that interests us. But this is not because we are "anthropologists", and, to the extent that we have found scholars working on related problems, they often aren't anthropologists. We've had productive engagements with critical geographers and cultural sociologists, for example.

ARC: There was then an iterative process in which we proposed analytic distinctions, which were related to historical events/ processes (WWII, Cold War, welfare state, neoliberalism) - and tried out those distinctions against empirical material we were generating – through discussions with experts, through analysis of documents, through conversations with our student researchers who were doing focused investigation.

• GM: YEAH, IT IS THE NATURE OF THIS EMPIRICAL MATERIAL I WANT TO UNDERSTAND — WHAT IS ITS RAW FORM? IS IT ALREADY 'PROTO' CONCEPT WORK? HOW COULD IT BE OTHERWISE WHEN YOU WORK WITH EXPERTS — SAM POPKINS — IN HARNESS?

o ARC: Per the above, we are observing their practices ofconceptualization – how they develop and operationalize concepts. But as noted above, our aims are different.

ARC: We were looking for moments of mutation, of recombination, that could help clarify the characteristics of the "objects" (eg. the UPMC Biosecurity center, the National Preparedness Guidance, etc) we were dealing with.

• GM: I WOULD LIKE TO KNOW MORE ABOUT THIS — SEEMS LIKE THIS IS DONE IN THE SOLITUDE OF THE STUDY OR SEMINAR.

o ARC: By "this" you mean the moments of recombination? They are, of course, "actual" in the sense that they happen as events in the organization of things, institutions, etc. What we can do in the context of the lab is to come up with concepts that allow us to identify moments of significant recombination. So, for example, SJC and AL are now tracing the various political configurations of techniques associated with vital systems security. The raw material is constituted by things like: Experts identify a new problem in a given analytic and conceptual frame, and endeavor to formulate a response.

ARC: Gradually, a number of distinctions that we felt comfortable with emerged. For example, preparedness vs. risk as forms of rationality; or the three forms of security: sovereign state security, population security, and vital systems security.

• GM: ARE THESE DISTINCTIONS 'RESULTS' THEN? HOW DO THEY WORK AS RESULTS? THIS SEEMS VERY SMART AND CONVINCING, BUT HOW IS IT DIFFERENT FROM THE TYPICAL CONCEPTUAL WORK OF THE SOCIOLOGIST WHO IS IN THE BUSINESS (GIDDENS) LIKE OF MAKING DISTINCTIONS LIKE THESE AS ANALYSIS. WHAT IS THE STABILITY OF SUCH CONCEPTS — DO THEY STABILIZE THE PROJECT? BECOME ITS VOCABULARY TO SLOW DOWN CHANGE OR THE CHIMERICAL PLAY OFCONCEPTS AMONG ACTORS (THE CRITICAL TEMPORALITY QUESTION)?

o ARC: A quick answer with respect to Giddens is that he is producing "theory" about modernity. So, modern subjectivity involves a technologized, calculative, relation to the self. You can go study that, but you don't really learn anything new – you just confirm over and over again what you already theorized was the case (we moderns have calculative subjectivities, etc.). ARC is producing concepts rather than theory. Their value is to help one see problems, tensions, motion in a given situation and to try to understand it. To identify and find significance in singularities. So our concepts, per Weber, help us identify the significant features of individual situations. So, for example, in the security project, we are not diagnosing a general state of security today. Rather, we

have an analytics that lets you see how particular elements are in motion in particular sites.

• GM: ALSO THE EXPRESSION HERE IS QUITE CONVENTIONAL — ANALYTIC DISTINCTIONS, BINARIES — LOOKS A LOT LIKE WHAT SOCIAL SCIENTISTS DO — SO AGAIN, MIGHT BE MISUNDERSTOOD AS A REINSTANTIATION OF THE CONVENTIONAL IDEA OF SOCIAL SCIENCE INTO INTELLECTUAL ENVIRONMENT WHICH HAS BEEN QUITE A BIT MORE TURBULENT IN RECENT YEARS.

o ARC: Can you say what you mean by "the conventional idea of social science"? We do think that there is much to learn from the other social sciences, even if we don't ultimately share their aims and techniques (this point is expanded upon in our "What is a Laboratory" paper (Collier, Lakoff, Rabinow, 2006).

o We have certainly learned some of the lessons of the reflexive turn, but feel that it needs to be taken in a different direction. Obviously we are not interested in doing naïve, objectivist social science. One difference from at least a clichéd understanding of conventional social science is that we don't think we are producing objective truths about the world but rather analytics that identify significance. But here is where we think that more reflection on the kinds of claims anthropologists make would be helpful. After the "reflexive" turn, anthropologists have not stopped making claims about the world. Our question would be: To what extent are these claims accompanied by a structure of accountability or responsibility: that is, what are the norms of adequacy, conceptual coherence, or adherence to something like "the empirical" that make these claims valuable contributions to thinking about the present?

ARC: A next step, it seems, would be to explain what it means to say that these concepts "work".

• GM: OK, THEN, THIS IS THE CRUX — 'WORK' INDEED — HERE IS THE ETHNOGRAPHY AND THE DISTINCTION OF THE ANTHROPOLOGICAL FORM OF KNOWLEDGE... SO, IN PAUL'S TERM — ONWARD, LET'S HAVE MORE JUST AT THIS CRUCIAL POINT.

o We have tried to outline above how the concept work and empirical soundings lead to progressive conceptual clarification and reframing of significant problems. That feels to us like a lot – indeed, since we don't believe in "theory" it seems like the most one can ask for. Another example would be the importance for us of the three-fold analytic that we developed between vital systems security, sovereign state security, and population security. For us, this opened up and organized a vast field of empirical problems whose significant interconnections were not clear to us before. Although we are still working on

how to explain this distinction to various audiences, we think it is extremely significant and diagnostic. We have outlined some of the ways above.

III. American Anthropological Association Panel, San Jose, California, November 2006.

1. Introduction: On emergence

James Faubion, Rice University

The conceptual terrain of emergence as it concerns us here today begins to take on teical contours in the British philosophical tradition of the mid-nineteenth century. It is very much a middle ground. It stands between a mechanism that presumed the ultimate resolution of all causal processes into the single plain of the physical and a vitalism that rejected the physical determination of life processes, of consciousness, of will and intention. Occupying the terrain of emergence in the work of such logicians as John Stuart Mill are properties, entities and systems that (in one or another sense) depend upon the phenomena and the processes of which they are constituted but are logically and so ontologically irreducible to them or explicable in their terms alone. There is thus something a little mysterious about things emergent and their mystery has left them the focus of philosophical controversy to this day. The concept of emergence has come to admit of "strong" and "weak" varieties as well as varieties of merely epistemic purchase without any ontological import at all. The latter-day positivists who are known as "causal fundamentalists" generally understand emergence accordingly to be an aspect only of our ignorance of how things really work and not an aspect of how they work in fact. I don't think that our panel includes even a single causal fundamentalist, however, so I feel I have license to proceed unapologetically.

Emile Durkheim's "society" is an emergent entity, a thing constituted of the human beings and institutional orders that compose it but, as Durkheim famously put it, a "reality *sui generis*." Yet, Durkheim's sociology does not focus on emergence as a problematic, as a systematic source of methodological and substantive questions. We might want to argue about whether Weber's *Protestant Ethic and the Spirit of Capitalism* is not in fact an emergentist treatise, or whether Marx shouldn't really be read as an emergentist, as Louis Althusser effectively read him, rather than the quasi-vitalist teleologist that he (like Hegel before him) so often appears to be when read without Althusser's guidance. Whatever we might conclude, I think it fair to say that emergence per se has been the systematic focus of neither methodological nor substantive investigation until recently and still not at all widely.

Its neglect seems to have little to do with ontologically uncertain status, about which only analytical philosophers are inclined excessively to worry. Perhaps it

has something to do with its simply being taken for granted. Yet, serious attention to emergent phenomena has also been occluded and displaced due to countervailing habits of attention of a much more dominant and enduring sort. I have three such habits in mind and will address each of them briefly in turn.

The first is a preoccupation with the codification and formalization of the laws or general law-like principles of social and civilizational development. If Darwin's adaptationism had not so rapidly suffered transformation into social Darwinism, had "the survival of the fittest" not so immediately overshadowed mutation as the prevailing slogan of the biological-sociological analogy, emergentism might have had more time to yield a proper program of social and cultural research. The intellectual and social ecology that persists with industrial capitalism and colonial imperialism from the middle 1800s to the 1960s, however, had little in it to facilitate the ascendance of any program centrally engaged with questions of the conditions of the production and effects of the unexpected, the contingent, the para-physical, the hybrid, the cyborgic, the rhizomatic. This was the stuff of strange laboratories in Moscow and even stranger enclaves outside of Carmel, California, but it had and could have no place in the Program for Social Relations at Harvard, much less the Laboratoire d'ethnologie in Paris. Under the force of that ecology, even Weber could not sustain the emergentist diagnostics of the "specific irrationality" of the growth of capitalism upon Calvinist subsoil. In the twentieth century, even his fascination with the *hiatus irrationalis* that seeming marks so much of the historical process gives way to a properly developmentalistic sociology of rationalism and the rational differentiation of "spheres of value."

The second habit of attention or inattention I have in mind is that of a preoccupation with the now very well developed problematic of sociocultural reproduction. In Durkheim as in the British tradition of structural-functional anthropology, that preoccupation unfolds as the investigation of the conditions that do and do not sustain solidarity or the maintenance of society as an integrated whole through time. Focusing on the production and repetition of the same, the problematic of reproduction endures in social theory from Durkheim to Pierre Bourdieu, whose theory of practice may prove to be its consummation. It has considerable reinforcement, however, not merely in the British but also in the French and U.S. anthropological traditions, so long as and for as long as they sustain a categorical portrait of the primitive and the traditional as ahistorical and anti-historical and a method of inquiry directed toward the revelation of recursive patterns of thought and feeling and conduct and yielding monographs inscribed in the infinitival conditionlessness of the ethnographic present.

The third habit is already immanent in the second and the first as an aversion to or at least a setting aside of the historical process in its particularity. The

Germans exhibit considerably less of such an inclination than the French or the Americans, for many of whom even today German "historicism" is precisely what is wrong with German social thought, Marxist or Weberian. Durkheim's variant is standard nineteenth-century fare and its precedent is specifically biological; for Durkheim as for other organicists, Malinowski included, "genetic" processes are one matter and structural-functional interdependency quite another. With Lévi-Strauss and if with considerably different implications still with Bourdieu, "history" is by definition the opposite and the antithesis of "structure." It is difficult even to begin to develop an analytical apparatus suitable to inquiry into the emergent without granting to the historical process a structurally generative and not merely structurally destructive role. It is further difficult to develop such an apparatus without casting historically events in their radical particularity as logically (and so ontologically) necessary conditions of emergent phenomena, since the unfolding of standard--lawful or law-like--causal processes produces not emergent but instead merely resultant (and so logically and ontologically reducible) outcomes.

If one of the properties of the contemporary is that it is emergent, then the contemporary itself had at best a marginal place in our social and intellectual ecology and so in anthropology between the middle 1800s and the 1960s. Most of the instruments we have agree that we no longer reside within that ecology, at least in some respects. Our present is that of computer capitalism, digital democracy, and virtual personhood. Information theory is our Holy Grail and information theory happens to be far more serviceable an apparatus for the conceptualization and characterization of emergence than most of the theories that preceded it. It permits us to think of (strongly) emergent phenomena as "uncomputable" relative to a given computational or algorithmic system. Should we seek to appease the causal fudamentalists, it permits us to think of (weakly) emergent phenomena as those that, depending for their existence on the iteration and aggregation of its constituent causal interactions (M. Bedau, "Downward Causation and the Autonomy of Weak Emergence"). It effortlessly affords a definition of novelty or the unexpected. Whether we are conscious advocates of or subliminal devotées of either the weak or the strong versions of emergence, it carries us to the threshold of the relation between any ontology of emergence and the relative weight that might have to be given in engaging with emergent phenomena and conceptual innovation and so to the collaborative work that conceptual innovation must involve.

So should we then think of this panel as the manifestation of a certain "informational modernity" whose great, seamy mesh of systems open and closed and the uncomputable hiatus and hybridizations between them press emergent contemporaneity to the forefront of any adequate attention to the everyday? Perhaps. Yet, if one is to believe the newspapers, and especially the Science pages of the New York Times, it would seem that quite another regulative idea governs the present horizons of social and cultural analysis,

namely that of the evolutionary-psychologistic explanation of absolutely everything. Perhaps the newspapers are wrong. In any case, I'm sure our panelists will weigh in on this and many other subjects as they see fit.

2. Notes on the Contemporary Imperative to Collaborate, the Traditional Aesthetics of Fieldwork That Will Not Be Denied, and the Need for Pedagogical Experiment in the Transformation of Anthropology's Signature Method

George E. Marcus

This collection of notes, propositional in form, and all, of course, debate-able, is a preparatory step to a more formal essay that attempts an account of a major transition occurring in anthropology's signature fieldwork-ethnography tradition embedded in the habits and learned aesthetics of its professional culture. Leaving aside here the question of how this transition came to be in anthropology's recent history, I am more interested in the current challenges that the ecology of designing and implementing ethnographic research today presents to the still powerful culture of method in anthropology, especially as it is manifested in the production of apprentice research by anthropologists in the making. These notes will help me to see, I hope, how distinct pieces of the story need to be put in narrative relation to one another.

In terms of previous writing, these notes represent a further meditation upon the emergence of multi-sited ethnography,beyond the understanding of it through the 'following' metaphor that I introduced in the 1990s. Now more than then, I perceive powerful pressures that challenge the viability and ambitions of ethnographic research in its mythic scenes of Malinowskian or Boasian encounter, however revised by 1980s critiques, and beyond certain limiting scripts for it through which it still thrives. It is on its frontiers or edges of contemporary application (for which research in realms of technoscience and society, among other kinds of expert knowledge forms, has been a crucible of applied experiment), in which ethnographers redefine the time-space and practical boundaries of their projects in multiple theaters of reception, that basic questions of scale, function, purpose, and ethics are being asked anew. Ethnographic writing and the reading of ethnographic texts, as in the 1980s, remain important perspectives here, but the production of research itself within its professional culture, behind these still traditional forms, and not limited by conventional thinking about method within tales and procedures of fieldwork is where the theoretical action is now, so to speak.

The present challenge to the pursuit of the low tech phenomenology of ethnography – face-to-face – to which anthropologists remain committed, within the ecology of changing scales and forms of inquiry, driven by technology, and the idiomatic response it seems to be eliciting in the name of collaboration is perhaps where to begin.

I. Collaboration

The spectral figure of fieldwork as collaboration has long haunted the overwhelmingly individualist conventions of producing ethnography. From time to time, the exposure of the repressed or suppressed collaborative relations of fieldwork have served the purposes of critique (as in the 1980s) or the effort to make fieldwork normatively collaborative in the highly politicized terrain of social movements among the peoples who have been anthropology's traditional subjects.

And there has been a long, but intermittent history of collaborative research in anthropology in its own self-organization and in its joining interdisciplinary projects, corresponding to periods of expansion, optimism, and the availability of resources in the development of university disciplines (famously, for example, the Torres Strait,and the Chiapas project; infamously, the Neel/Michigan studies of the Yanomami).

In the context of the history of fieldwork, it has been primarily ethical concern that has driven the motivation to encourage an explicit , normative modality of fieldwork as collaboration. In the context of the history of anthropology as an institution, it has been primarily disciplinary ambition and sometimes intellectual excitement in the making and breaking of reigning paradigms that has driven collaboration in the past.

But, today, I believe that the clear salience of a norm encouraging collaboration in anthropology has a different generic source and a different expression than in the past.

The dominant form of collaboration of the present era is the technology driven collaboratory (wikapedia: "an environment where participants make use of computing and communication technologies to access shared instruments and data, as well as to communicate with others"). Collaboratories have dramatically encouraged the adoption and experiment with forms of collaborations within the traditions and cultures of inquiry across many disciplines and in the way that universities are restructuring themselves, and in some,like anthropology, however positively collaboration was valorized in the past, the current tendency, originating in efforts to organize knowledge making within the oceanic realm of connectivity, is experienced as pressure, as imperative to which the reaction, while it might be creative, is also anxious, sometimes defensive.

The aesthetics of research practice are deep within and constitutive of the professional culture of anthropology, which as I will address is strongest in apprentice pedagogy and in the norms of receiving results of research, together still holding the powerful professional culture of ethnography in place within its

traditions, and they will not be denied, under current pressures and imperatives. While deserving a complex treatment, these aesthetics are individualist, face-to-face in nature, as in the mythic scene of Malinowskian, and more lately Geerztian encounter. The creative, experimental question at the moment is not (or not yet) how are these aesthetics are to be overcome, but how are they to be adapted to equally powerful pressures to produce ethnographic knowledge within the terrain and ecology of collaboratories. (*)

The problem for ethnography in assimilating collaborative strategies and norms of research practice, finally, is not so much to preserve doctrinally the individualism it entails (that is the preservation of individual performance, expressions, and rewards of inquiry), by providing a cocoon or a protective mimicry for it in the current environment, to make it pass like a form of the 'native' emergent collaboratories today, but to preserve what is very valuable and precious of an older, simpler technology of knowing that the individualist aesthetic of ethnography entails even in its new environments of collaborative and distributed knowledge forms, organized in oceanic cyber-space, which it engages in closely observed conventional sites, in laboratories, in board rooms, in villages, and other existential locations. So experimental collaborative strategies of ethnography now in anthropology arise not so much from its history of ethical concern for the other, so to speak, but from new ecologies and scales of research which challenge anthropologists to produce the scene of fieldwork and its aesthetics within and across scales that are now hyperorganizing as collaboratories, that are imbued with 'the vision thing', imaginaries of practice that are conceived in emergence. And it seems to be the job of a wide swath of social/cultural anthropological research today to work through these 'native points of view'—to evoke the old interpretative object of ethnography— as imaginaries of anticipation and possibility found within the collaboratories, or assemblages, of institutional and other sorts of actors in the contemporary.

The emergence of forms and norms of collaboration in ethnographic method today, alongside and operating within its complex objects of study –themselves collaboratories—would function as cocoons or incubators of concepts, ideas, shared with subjects, which serve to rescale and slow them down, and modulate them to the tempo at which anthropologists have traditionally done their work. Anthropological collaboration of this sort would create a belated, but relevant form of ethnographic knowledge in relation to the scale and pace of its contemporary objects and contexts of study. (**)

So there are two functions of collaboration now in the reinvention of anthropological ethnography—one is to create the conditions within the bounds of research projects to generate the kind of results that ethnography has traditionally contributed and valued – perhaps conceived as concept work that requires a space and tempo that slow things down. Collaboration thus creates

the opportunity for the process that is distinctive of ethnography. (A treatise would now be required to describe systematically what would actually happen to the tropes, habits, and aesthetics of the anthropological tradition of research thus preserved. This treatise should be pedagogical in nature as I will argue, since this is where method is most at stake in anthropology today).

The other function is to create an adapted identity and space for ethnographic projects to operate in the collaboratory arrangements of others as subjects. The individual fieldworker in these complex spaces is increasingly an alien, uneasy presence for which mere affiliation with a disciplinary or professional community/collective is not a sufficient surrogate for belonging to a collaborative research effort of varying scale. Collaborations built into ethnographic research provide identity and space in topological terms to relate the human-scale of ethnography, to which its aesthetics of method remains committed, to the complex scales of collaboration in which it must define its own objects and boundaries.

So collaboration can be in any ethnographic project an ambiguous process. On the one hand, it is a proffer to subjects to create the classic conditions of fieldwork; on the other hand, it is a proffer to colleagues to produce collective work. I want to pursue this ambiguity by briefly referring to my understanding thus far of a notable current effort to innovate an anthropological scale research collaboratory: the ARC (The Anthropology of the Contemporary Research Collaboratory), based at UC Berkeley.

I have been privileged to follow the evolution of this project and to have conversations with its principals. Its development thus far is worth a full account as a case study in the reinvention of anthropological research aesthetics, but here I want to contemplate it in relation to the differing approaches to collaboration that it more generally illustrates.

ARC has two primary identities interestingly integrated and managed. Initiated by Paul Rabinow and his former students, it is both a project that does research on biosecurity—its contemporary modalities, paradigms, and institutions—and seeks to experiment and design new forms and norms of inquiry with roots in anthropology, but as informed by the broad transformation in theory and practice during the 1980s and 1990s that characterized academic disciplines concerned with the study of culture. On the one hand, it has produced a collaborative form that seems of the conventional social scientific sort generated by an ecology of expectations, determined by sources of funding, and the institutional cultures of expertise and science with which ARC interacts –in this sense, and in a formal way, it has 'gone native'. On the other hand, in its collegial intimacies, through the research that it has proposed to do, and understanding the moving ground of older methods, it has the ambition of innovating practices of once ethnographic inquiry by viewing its research tasks

as experiments in this regard. The ARCists are second order observers of their own research functions.

This can result in a dizzying complexity if it were not for the natural logic of group process—decisions to go one way rather than another, to determine emphases. In my outsider's observation and interest in this process thus far, I am fascinated by two models of collaboration which ARC suggests, that I can only sketch here, each of which poses a way that the development of collaboration within the current professional culture of anthropological ethnography might go.

A key question here is the conceivably variable role of the individualist project of ethnographic research, as the component or modality of the ARC that evolves collaboratively. By one model, let's say, the science version of ARC, (and the one that I think it actually favors), the principals develop an increasingly coherent perspective on particular topics; they process fieldwork as data for their own concept work—collaborative artifice and innovation is concentrated in the work of the principals. The collaboration within the scene of fieldwork—traditionally repressed and underdeveloped—while recognized is clearly subordinated, as an object of experiment, to the collaboration of the principals in their concept work. The creation of knowledge in the scene of fieldwork itself—partial to the traditional mythos of fieldwork—is displaced for innovations in collegial collaboration. This is a true diminution of the individualist project and its ideologies.

By the other model, let's say the avant-gardist one of experimentation, the accent is on the found collaborations in fieldwork investigation, making something of the long repressed collaborative basis for developing ethnography in the field. It is closer to the longstanding ideologies of fieldwork with the individualism diminished in favor of developing the collaborative impulse always there, but now not out of ethical concern, but from the conditions that constitute the subjects and objects of ethnography today.

In this version, the ARC finds and assimilates diverse projects of ethnography, already going on out there, with speculation and surprise; it is porous to the collaborative forms and norms being innovated in fieldwork and its own collegial collaborations are driven and stimulated by this. It sacrifices precision and analytic power in results for constantly pushing the categorical boundaries of biosecurity paradigms. It remains a bit of the outlaw in these paradigms, as anthropology has traditionally preferred to be as part of its aesthetic. In the science model, the work becomes more refined as it expands—there is increasingly better control of the conceptual apparatus rather than openness to inclusion of diverse topics, and research on its peripheries. Participation in working on, changing the major paradigms of biosecurity matters more than critique from the margins.

Now ARC has both of these collaborative styles within it, and as such it is one prototype for how the reform of method out of anthropology might be grown. But ARC in its experimental ethos is more than a research project that inventively engages the imperative of collaboration within the sensibilities of the anthropological tradition of ethnography. It also has pedagogical intent, moving toward becoming a design studio of sorts for rethinking and altering the norms and forms of dissertation training and production in anthropology. I actually believe these changes are occurring on a widespread basis, but more circumstantially and by negotiating older models and the conditions of fieldwork than by articulation, design, and rethinking what fieldwork becomes in a broader sense of the research terrain. This is precisely what discussions of collaboration—its meanings, ideologies, present forms –precipitate. So now I want to consider the question of apprentice pedagogy in becoming an anthropologist and its strategic importance as a site for considering the rearticulation of the norms and forms of ethnographic research.

II. Pedagogy

Classic anthropological ethnography, especially in its development in the apprentice project/dissertation form, was designed to provide answers, or at least data, to questions that anthropology had for it. Nowadays, anthropology itself does not pose these questions. Other domains of discussion and analysis do—some academic or interdisciplinary in the conventional sense; others not— and thus it is a contemporary burden of projects of anthropological research— and especially apprentice ones—to identify these question asking domains— domains of reception for particular projects of research -- as part of learning the techniques of research itself. In this development, the function of the research project is not simply descriptive-analytic, to provide a contribution to an archive or debate that has been constructed by the discipline—it hasn't. At best contemporary anthropology provides a license and an authority to engage, not a reception itself. No wonder then the current dominant impulse and fashion at the core of the discipline to call for a public anthropology—it remains to think through what this means beyond doing good. In this license, the function of ethnographic research out of anthropology becomes a mediation in some sense; it sutures communities and contexts together in addressing those communities, in presenting its results in constructed contexts of collaboration as a key issue in the increasingly broader design of research beyond mere fieldwork.

Indeed students are pursuing questions that fieldwork itself in its conventional aesthetics can't answer. And it is in the process of apprentice research –in dissertation making—that an anthropologist is most subject to these aesthetics and regulative ideals of research practice as they are imposed, not by rules of

method, but by the profound and redundantly instilled psychodynamics of professional culture. Here the process on its own is not at all stuck, but in transition. What is missing is an articulation of these changes—and talking of the observable vulnerabilities of the old practices as a way to systematically formulate alternatives and modifications (e.g., the reading of ethnographies does not so much serve in any straightforward way, as it once did, of teaching method—exemplars to follow or moves to try out—as collections of 'symptoms' that provide clues to alternative pedagogical strategies. So ethnographies no longer reflect the classic fieldwork situation, but rather the broader topology of research, encompassing classic fieldwork, that requires a more complex notion like design).

This is where anthropological models of collaboration, discussed earlier as a contemporary imperative and condition of inquiry across disciplines, could make a considerable difference. They immediately suggest a broader frame for constructing research than that which is focused on the norms for preparing for and conducting conventional fieldwork and then reporting on it in a dissertation. At present, as a halfway measure, what prevails is a renewed experimental ethos for the conduct of ethnographic research which makes a virtue of the contingencies deep within its traditional aesthetics, and which works very well for the exceptional talents who enter anthropological careers by embracing this experimental ethos.(***) In producing standard work, however, the experimental ethos serves far less well—it produces more often rhetorically driven repetitive versions of singular arguments and insights. A fuller account is badly needed of what kinds of questions contemporary ethnography answers, with and in relation to whom, what results it might be expected to produce on the basis of what data. All of these very elementary questions are in urgent need of being addressed again with ingenuity and theoretical insight. There are a number of ways to produce such a reconsideration by looking ethnographically at current negotiations and compromises with the aesthetics of method in the course of dissertation projects as they unfold. At present, if one listens to student tales of fieldwork today, what transpires is far more complicated and interesting than expectations of fieldwork reporting allows for. To probe the collaborative dimensions of contemporary research, which the present ideological tendencies surrounding collaboration encourage anyhow, would generate informally and formally different accounts of fieldwork, leading to a much needed broadening of the pedagogical expectations of dissertation research.
I also want to conclude this section, as I did the first one, with the discussion of a particular example, this time referring to my own effort to implement a so-called para-site experiment in the pedagogy of graduate research, through the recently established Center for Ethnography at UCI.

I reproduce the Center's explanation of this experiment:

We invite graduate students engaged with ethnography at UCI and elsewhere

to propose projects where the Center event can serve as a para-site within the design of specific research endeavors. This theme signals an experiment with method that is directed to the situation of apprentice ethnographers, and in turn stands for the Center's interest in graduate training and pedagogy as a strategic locus in which the entire research paradigm of ethnography is being reformed:

The Center As Para-site in Ethnographic Research Projects:

While the design and conduct of ethnographic research in anthropology is still largely individualistic, especially in the way that research is presented in the academy, many projects depend on complex relationships of partnership and collaboration, at several sites, and not just those narrowly conceived of as fieldwork. The binary here and there-ness of fieldwork is preserved in anthropology departments, despite the reality of fieldwork as movement in complex, unpredictable spatial and temporal frames. This is especially the case where ethnographers work at sites of knowledge production with others, who are patrons, partners, and subjects of research at the same time.

In the absence of formal norms of method covering these de facto and intellectually substantive relations of partnership and collaboration in many contemporary projects of fieldwork, we would like to encourage, where feasible, events in the Center that would blur the boundaries between the field site and the academic conference or seminar room. Might the seminar, conference, or workshop under the auspices of a Center event or program also be an integral, designed part of the fieldwork? – a hybrid between a research report, or reflection on research, and ethnographic research itself, in which events would be attended by a mix of participants from the academic community and from the community or network defined by fieldwork projects. We are terming this overlapping academic/fieldwork space in contemporary ethnographic projects a para-site. (1) It creates the space outside conventional notions of the field in fieldwork to enact and further certain relations of research essential to the intellectual or conceptual work that goes on inside such projects. It might focus on developing those relationships, which in our experience have always informally existed in many fieldwork projects, whereby the ethnographers finds subjects with whom he or she can test and develop ideas (these subjects have not been the classic key informants as such, but the found and often uncredited mentors or muses who correct mistakes, give advice, and pass on interpretations as they emerge).

We would like to sponsor and design Center events, workshops, mini-conferences, seminars, meetings simply – that would further this dimension of fieldwork.

1.The usage is inspired by the concept for the 8th volume of Late Editions, the *fin de siècle* series of annuals, edited by George E. Marcus through the 1990s: Para-Sites: A Casebook Against Cynical Reason, Late Editions 8, Cultural Studies for the End of the Century. University of Chicago Press, 2000.

The first event that represents such an experiment occurred on November 5. Jesse Cheng, an advanced graduate student, is studying a movement among activist lawyers to mitigate the death penalty in capital cases. A former practicing lawyer, Cheng is working with them and in other directions that their activities suggest to study the operations of the death penalty through the para-ethnographic, descriptive-analytic work that the mitigation lawyers produce in their advocacy. He conducts his own investigation through the forms of their investigation. This is the analogous space of the classic 'native point of view' but without a compass in traditional practices to do this kind of research that requires collaborative conceptual work to enable a project of anthropological ethnography.

(****) This work needs a context, a space, a set of expectations and norms, better than the opportunistic conversations that occur in just 'hanging out'. The para-site experiment is intended to be a surrogate for these needs of contemporary research that are certainly anticipated in practice but still without norms and forms of method. It encourages addressing issues of design before a concept of design has reinvented the expectations of pedagogy in anthropological training. Undoubtedly, the para-site will take different shapes and participations between the field and the conference room in other dissertation projects. But in all cases, it is a response to the imperative to materialize collaborative forms in contemporary ethnographic research.

Notes

(*) alternative formulation from rough notes: So one could say that collaboration talk is about institutions' reactions/anxieties to being immersed or cast into cyber space, and responses depend very much on what a discipline's forms of inquiry have been.

In this regard, anthropology has been an extreme case. It has a face-to-face immediate experience technology of giving form to knowledge. In this traditional relam, it has had critiques and recognitions of collaboration but this is tied to the scene of fieldwork with which it is comfortable.

More recent talk and pressures toward collaboration are something different—it represents the need to do fieldwork in different terrains, scales, and also to participate in intellectual projects, blurred with fieldwork, in these terrains and scales.

In the past when anthropology has tried to formulate collaborative research for itself there was something of this pressure to participate in larger scale, conventional social science (so the Harvard style of collaborative project coming out of social relations; other such projects reflecting development paradigms). The impulse to collaborate now has something of this but it is more pressured, more imperative. Partly because collaboration has become a general ideology of research in institutions that incorporate technical change in information and its own styles. Partly because the identity of ethnography in terms of the lone fieldworker is no longer sufficient. Partly because the surrogate ideological collaborative community of the the discipline itself, the professional community is found wanting especially in anthropology (anthropology now develops along its interdisciplinary peripheries while having an empty center).

So in anthropology now, it is not just collaborative ethnography in the old mise en scene-- an unacknowledged function of old relations and implicit working relations which are no longer supportable—this is the ethical concern/critique for ethnography, once again, but that is not where the call for collaboration comes from now

(**) Rough notes: collaboration is a kind of solidarity around human-scale low tech knowledge production which ethnography tries to preserve—outside of being commodified by being made useful to subjects as clients, patrons, and more powerful facilitating interlocutors

(***) The influence of 'experiment' as a satisfying characterization of the present state of fieldwork challenged by its complex terrains is, for example, the resonance for some anthropologists in new terrains of Hans Jorg Rheinberger's formulation of 'the experimental system' with biochemical research in mind for their own research practices, on the hand, and on the other, some recent *bon mots* of Marilyn Strathern which are consistent with the spirit of Rheinberger and celebrate the resiliency of "plain old" ethnographic inquiry in the midst of very elaborate collaboratories.

Compare:

Rheinberger: Experimental systems are to be seen as the smallest integral working units of research. As such, they are systems of manipulation designed to give unknown answers to questions that the experimenters themselves are not yet clearly able to ask.

Strathern: Social anthropology has one trick up its sleeve: the deliberate attempt to generate more data than the investigator is aware of at the time of collection, a participatory exercise which yields material for which analytical protocols are often devised after the fact…ethnography allows one to recover

the antecedents of future crises from material not collected for the purpose, to anticipate a future need to know something that cannot be defined in the present...

Personally, I find this a very appealing mystique for ethnography, and it COULD perhaps actually function in this way, but I think that the Strathern rendition of a Rheinberger like experimental virtue of ethnographic research is deeply flawed because the equivalent virtues of Rheinberger in ethnography seem to arise from the application of its time-tested aesthetics. From my perspective, they can only arise from the latter's revision. In this sense, collaboration is anthropology's experimental system and could be thought through as such.

(****) The following is the reaction I sent to the student who orchestrated the first para-site experiment, at UCI, Nov. 4, within his research. It deals with how (1) the form of paraethnographic engagement, that defines the basis of epistemic collaboration in contemporary fieldwork, might be located and clarified through the para-site surrogate for collaboration in the absence of explicit norms for it in the present state of training ethnography; and (2) how such para-site needs a 'third' – a common object or a specific community of reception to address—like high minded debates about the death penalty—as the basis for the complicit solidarity on which collaboration might be created in contemporary contexts of research, full of causes and activist motivations:
Jesse,

That was a great event and sets a very high bar, appropriately, for the development of para-site experiments as a feature of the Center. Thanks so much for your skill, intelligence, and energy in making it happen. Also, what a group of fascinating people to make fieldwork out of.

Just a couple of personal notes:

For me, the key to exploring 'reflexive knowledge' ethnographically among expertises and 'projects' of various sorts in the world is to locate/discover where and how it is constituted paraethnographically, so to speak – to find a 'form' amidst practices. In our session, this moment materialized after lunch, when Russ revealed in response to my question that all of this elaborate research is built into the advocacy process as a front-loaded phenomenon in a situation of anticipation. And then at the end, Bill crucially associated this 'space' with the formulation of the nature of contemporary ethnography itself as anticipatory, in the bon mots of Strathern that he (and I) likes. So this is a space of both 'fact-finding" and the imaginary, depending upon the development of reflexive knowledge. The question remains of what the role of the ethnographer/ fieldworker is in this 'found' space of para-ethnography. To describe it?, to analyze it ?, to partner with it? to encourage the development of it? to pass it on, represent it elsewhere by some sort of mediation...?

and this gets to some of the remarks of the final discussion about what the stakes for anthropology are in research like this – for its own project – and not part of helping to strategize (epitomised by Roxanne's fortuitous performance) – and when anthropologists in collective work among themselves have no adequate reception for this research. Well, my current solution to this is that work in anthropology like yours has to be designed with a 'third' primary area of reception for ethnography in mind – that is neither, the community of anthropologists who are not prepared to discuss such work deeply, nor the subjects themselves who have their own purposes and interests in developing your work with you. So what is 'third' – well, I evoked high-minded, often high literati discourse on capital punishment that usually has no subtle knowledge of ethnographic objects/subjects (with the reflexive knowledge work that goes on inside them), but cumulatively is really important in effecting change. So ethnography in its production is inherently dialogic where the key partners to dialogue are often not just the 'natives'. This means the very conception and design of projects of ethnographic critique should incorporate a deeply understood (itself ethnographic in nature?) dimension of intended reception outside the scene and interests of fieldwork itself--another way to, or sense of, multi-sidedness? In this mode, the ethnographer sees the function of his work as mediation in a very specific politics or topology of knowledge that incorporates anticipated reception.

This second point is more for me than for you in my interest in remaking the norms and forms of pedagogy.

So thanks for everything,

G.

3. Toward a History of Collaboration in Anthropology

Rebecca Lemov

For over a decade I've been following the trail of a history of collaborative research in the human sciences. Originally I got interested in these kinds of projects by means of a single anthropological device, the Yale files--originally named the Cross-Cultural Survey, later renamed Human Relations Area Files, sometimes known in the press as "Yale's Bank of Knowledge" and in their heyday in the corridors of the Yale Institute simply called "The Files." This knowledge-processing apparatus appealed to me as one of those forgotten objects—once fussed over, now neglected, but that continues to ask its unanswered questions among the ruins of its own self-generating conditions, Can you put the world in a box if you turn it into information first?, and Can you make of anthropology a systematic enterprise in knowledge production? It seemed to me in the mid-1990s when I was in graduate school that nothing could be more out of fashion, epistemologically speaking. To be out of epistemological fashion meant that all the going assumptions (for example, about the "made" quality of facts, the entwinements of knowledge and power, and most specifically that anthropology was about questioning and upending given categories, not creating them) mitigated against the seeming naivete and even affront constituted by these approaches. Yet as I followed this trail of research and apparatuses, files and coding devices, looking in archives and into rather dusty and rarely cracked experimental science journals, I got more interested. This was more than a tale of social scientific folly on a mass scale, of Organization Man come to tame the quirkiest of sciences (anthropology) and normalize it once and for all. At issue it seemed to me was experimentalism itself, and the space and function of laboratory inquiry. The impulse I was observing more generally I labeled "the laboratory imagination," anthropology in communion with what are often called "the related social sciences" (sociology, psychoanalysis, experimental psychology), as well as biology, ethology, and physics. This grand and sometimes grandiose movement of cross-disciplinary cross-research and cross-talk, of collective knowledge-making and collective research arrangements, when you spent some time looking at it closely, was actually quite exorbitant in its workings.

For this panel my assignment was to ask: what can the history of collective work in anthropology tell us about twenty-first century research undertakings? On one level, the question is, Was this work ever anything more than "collected"—that is, simply massed together, the encyclopedia-slash-database approach: did it arrive at true collectivity of research? And then a pragmatic question: Is there a history of collective work in anthropology that is not all unusable, or usable only in the form of a desperately cautionary tale. Almost immediately on posing these questions I fell into a bind: the tendency is either

to castigate or to lionize. Either to say, "We should do it like they did" – find and hold up appropriate exemplar – or "they were naive and worse than naive...." Aside from this type of Manicheanism I also want to avoid etiolation in this approach to the past in relation to present and future anthropology. For the strange thing is that these projects have for the most part simply disappeared.

On the one hand, they can look at first glance like just a big bunch of acronyms– and who's interested in acronyms? These projects-indexes-matrixes-plans-dreams-utopias are effectively invisible not because they are hidden; rather, they are hidden in plain sight. Aside from adopting them as one's new "privileged ancestor," what can be made of this all-too-mixed yet extremely provocative history? The substance of my report will be to fasten onto four archival "moments" and discuss the strange and strategic implications of each for team-work and concept-work in anthropology and the human sciences. As it happens, these archival fragments I've chosen do not for the most part comprise triumphal moments when everything came together and a grand new synthesis emerged, although one does find such moments; rather, for my purposes here, I will discuss several somewhat ordinary moments (for example, a long bureaucratic meeting in Washington, DC, an altered grant application, a change in acronym), in order to draw out some threads, minute and perhaps not exemplary but bearing on the object at hand: they are small pivots, moments at which something shifted, and by bringing them together I hope to suggest a few ideas about how to weave what might be called, borrowing from Danny Hillis, 'history with a future.'

ARCHIVE 1. CCS, IHR & OCM

The Yale compendium has been called (and may indeed have been) the biggest filing cabinet in the world. Their aim was unblushing. The sum of the world's contents was to be converted to text-based code, stored on file cards, and maintained in a systematic fashion so that the resultant data could be recombined or extracted or processed at will—and all this without a computer, relying on human processors and long-suffering typists. By the mid-1960s the files held 65.8 million index cards. And why should anyone blush? After all, this form has only proliferated—think, for example, of two not insignificant successors, the collaborative knowledge project we call the World Wide Web, and the Total Information Awareness database movement in government.

I don't want to spend too much time on the Files because in many ways they're the most "epistemologically naive" of the projects I want to consider, and they also bear a 19th-century taint (reminding one of earlier efforts by Spencer, Tylor, and the ill-fated Dutch scholar Steinmetz to build a 'roomful of drawers' containing the totality of information on a given subject). I will focus instead on an archival moment that has to do with an early shift. Originally the project

belonged to one man, who envisioned it as a sort of lifelong prospecting venture in the comparative forms that culture takes. From 1929 to 1934, George Peter Murdock worked alone in libraries culling, distilling, condensing the facts of various cultures. At some point in the mid-30s, though, Murdock admitted defeat and joined with others. "It is a task beyond the powers of any single individual; I have tried my hand at it, and have done a fair amount of spade-work, but I realize that I cannot possibly accomplish it by myself...."9 The result was that only did the Files become an intensively collective (not just collecting) enterprise, but it joined with the efforts at the Yale Institute to craft a unified theory of social life. These two developments were in tandem—and they suggest to me a transformation in the subjects and objects of knowledge.

Six anthropologists and sociologists convened under Murdock to discuss how to carry out the task of carving up the totality of cultural information into categories. After experimenting with different divisions, each scholar reported back with his conclusions on the most seemly break-down as it appeared to him. Two-digit numbers from 10 to 88 marked each major heading and a third or fourth digit marked each subdivision thereof, thus communicating with that numerical coding a confidence in the impartiality and neutrality of the divisions provided. After several months, a mimeograph of the scheme went out to "all kinds of people—economists, sociologists, geographers, anthropologists, engineers, lawyers, housewives, industrialists" for criticisms; 100 useful comments came back.10 The whole was adjusted and eventually published as the *Outline of Cultural Materials*. The OCM, along with a classifying scheme for the totality of societies, the Outline of World Cultures, would serve as indexes to the Files.

How the files integrated into the Institute of Human Relations is another story that I won't dwell on, nor the question of how exactly texts were "processed" into text-based units of information. Suffice to say that the files' integration into the Institute's work was rapid. Murdock began working with John Dollard (Chicago-trained sociologist and jack of all trades, former protege of Sapir) and within a few years he had written a memorandum titled, "Proposed Program for Anthropological Research under the Direction of the IHR as part of a Coordinated Program of Research Aimed at the Achievement of an Integrated Social Science."

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9 George P. Murdock memorandum, "Proposed Program for Anthropological Research under the Direction of the IHR as part of a Coordinated Program of Research Aimed at the Achievement of an Integrated Social Science," c. August 1939, Yale Archives, YRG 37-V, IHR, Series II, Box 11, Folder 11-95.
10 Creation of taxonomic system as recalled by Murdock in minutes of "Meeting of Advisory Board of Strategic Index of Latin America," August 29, 1942 National Archives, State Dept., R.G. 229, Entry 1, Stack Area 350, Row 76, Comp. 2, Shelf 4, Box 134.

It's important to stress here that the files were not mere auxiliaries, a database-style handmaiden waiting to assist the grand theoretical labors of experimental psychologists and sociologists; rather, the files were themselves conceived as each embodying an experimental logic. Robert K. Merton agreed, and cited Yale's cross-cultural surveying efforts approvingly in his "Manifest and Latent Function." In Merton's view comparison was anthropology's and sociology's close approximation of experimental logic—"quasi-experimental" is his phrase, and the cross-cultural survey "held large promise" in that regard.11

ARCHIVE 2. SILA 1942-3

My second archival moment comes from the minutes of a meeting of a new project—the Strategic Index of Latin America—that had been recently contracted, in 1942, by the government (not the State Department, but its equivalent entity in regard to Latin America, the Office of the Coordinator of Inter-American Affairs). The meeting included representatives from different arms of government, the military, and various academic anthropologists. The particular document that interests me, titled just "<u>Morning Session</u>," shows that things are not always exactly what one assumes, and that the surface of epistemological naiveté is sometimes broken by ripples. Murdock began the meeting by giving a capsule history of how the by-now-nearly-epochal Yale filing project came to be and how this new project emerged out of it. As it happens, it was through Alfred Metraux, who was at Yale in 1939-41, that the group originally made contact with Argentine anthropologists and a Spanish translation of the outline was published. Because Metraux and John P. Gillin were working on their *Handbook of South American Indians*, the Files organizers decided to compile a Latin American sample and use it as "a sort of laboratory on the basis of which social scientific research could be done." Murdock also mentions that Bronislaw Malinowski, also at Yale, was involved in promoting the files at the outbreak of war.

At this point, the meeting takes a turn: the anthropologist Julian Steward interjects, and brings an almost Aristotelian note to the discussion: "I do not wish to bring up a philosophical discussion at this point as to when a fact is a fact, but it is my firm belief that a fact is not <u>a fact until it is related to a problem</u>. We can't deal with isolated facts, but rather we must deal with problems and select the literature from which the facts are obtained on the basis of the problems…just getting facts won't be useful…[unless] related to specific problems."12 It's an interesting moment—not that the suggestion was picked

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11 Robert K. Merton, "Manifest and Latent Functions," *Social Theory and Social Structure* (Glencoe, IL: Free Press, 1957).
12 "Meeting of Advisory Board of Strategic Index of Latin America," Morning Session, August 29, 1942 National Archives, State Dept., R.G. 229, Entry 1, Stack Area 350, Row 76, Comp. 2, Shelf 4, Box 134.

up explicitly, although a few participants bandied about the idea for a while, but that it suggests an attention to problematization: Had Michel Foucault entered the conversation at this point, he might have observed that problematization entails "how a situation appears as a possible question," and perhaps continued, "This elaboration of a given situation into a question, this transformation of a set of difficulties and troubles into problems to which diverse solutions are proposed as repsonses is th epont of problematisation, the specific work of thought."13 The files were becoming more than vast repositories or compendious encyclopedias.

INTERLUDE c. 1942: Margaret Mead Breaks Her Tooth

At this point it seems felicitous to turn to a meeting that was taking place almost exactly at the same time as Steward was asking his question in Washington, When is a fact a fact? How do you problematize knowledge? This in fact was the project of cybernetics, and it was in and by means of the human sciences that cybernetics became truly collaborative.

People disagree about when exactly cybernetics as a research movement and an activity was born (it was not named, officially, until 1947, by Norbert Weiner; and it never, through a series of perhaps fortuitous accidents, had its own centralized research laboratories). Cybernetics has been defined recently, in fact, by the French scholar Jean-Pierre Dupuy, as a sort of long and multi-sited conversation that took place over many years and involved people talking to each other who almost never talked to each other. The legendary Macy Conferences from 1946-1953 were key arenas, everyone agrees. The start to that conversation, however, was a preliminary meeting, a kind of pre-Macy Macy meeting, in 1942, in New York. The topic was one perhaps unlikely to lead to a grand synthesis of human social and bioilogical systems: cerebral inhibition and the workings of hypnosis. An interdisciplinary group gathered, including physiologist Arturo Rosenblueth, neurologist and poet Warren McCulloch, foundation officer Frank Fremont-Smith, and intellectual entrepreneur Lawrence K. Frank, as well as a psychoanayst and the husband-wife team of anthropologists Margaret Mead and Gregory Bateson.

Amidst talk of many things, Rosenblueth came forward and presented a sneak preview of the basic ideas for a paper he was to publish the next year, 1943, along with Norbert Wiener and Julian Bigelow.14 Rosenblueth's presentation

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13 Foucault's view of problematization are quoted in Paul Rabinow, "Steps Toward an Anthropological Laboratory," Discussion Paper, February 2, 2006, Anthropology of the Contemporary Research Collaboratory.

14 Arturo Rosenblueth, Julian Bigelow and Norbert Wiener, "Behavior, Purpose and Teleology," *Philosophy of Science* 10 (1943).

was really a manifesto for something new, and the main point was to argue that one could now talk about teleological behavior (that is, purpose) in any system while also remaining true to the demands of adequate explanation. The ghost in the machine didn't have to be blind. Due to negative feedback, also known as teleological mechanisms or servomechanisms, you didn't have to throw out the baby (of adequate scientific models) or the bathwater (of the formerly metaphysical realm of teleology and purpose). Machines and mice were no longer models for human function; they were made of the same stuff. As Rosenblueth said somewhat cryptically, "The ultimate model of a cat is of course another cat, whether it be born of still another cat or synthesized in a laboratory." It was, these proto-cyberneticists thought, a revolution against simple-minded linear reasoning, reductionism in science, and the blinders of disciplinary thought. On exposure to this epitaph-for-behaviorism and prolegomenon-for-cybernetics, Warren McCulloch was excited. Gregory Bateson was excited. Margaret Mead broke her tooth and didn't notice until after the meeting was over, so excited was she.

This presentation in particular inspired the creation of a series of later meetings under the rubric and title of "Circular and Causal Feedback Mechanisms in Biological and Social Systems"—later changed, but not until 1947, to the more felicitous cybernetics. It was because of Bateson, in particular, that the conferences planners added "And Social." Soon there were massively collaborative meetings, including Mead and Bateson, Clyde Kluckhohn and Talcott Parsons, and from Columbia Paul Lazarsfeld and Robert K. Merton, as well as Wiener, McCulloch, Claude Shannon, and John von Neuman.

ARCHIVE 3. CIMA 1947-51

The third archival incident takes place during the further expansion of the filing domain and its accompanying experimental impulse. The local occasion was the establishment of the U.S. Navy's Occupied Area in the former Japanese Mandated Islands in the territory of Micronesia spread out over a million square miles of ocean—is a historical event that explains the appearance, in 1947, of 41 physical anthropologists, linguists, ethnographers, sociologists, and "human and economic geographers" on these same islands. Social scientists from more than 20 institutions fanned out over 12 island clusters in what its organizers called "the largest cooperative research enterprise in the history of anthropology."[15] All researchers carried instructions to take notes in duplicate and send their carbon-copy reports back regularly to both the Navy and the NRC. Under the aegis of a principled "Early Availability of Information," regular

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15 "Bulletin re CIMA Project," May 13, 1947, NAS-NRC Archives: ADM: EX Bd.: Pacific Science Board: CIMA.

Interim Reports, Final Reports and even "undigested field-notes" made their way into the project's files. Total war was to be followed by total anthropology.

The moment I want to point to concerns the question, Why this inter- and cross-disciplinary project was labeled "anthropology" in its title and most of its descriptive documents? In the archival record, the project first appears as the "Coordinated Investigation of Micronesian Peoples," and bears the perhaps inauspicious acronym "CIMP." In subsequent funding documents, its authors abandoned this title in favor of the "Coordinated Investigation of Micronesian Anthropology," or CIMA, presumably with the aim of giving stress to the scientific and methodical study of the varieties of Micronesian culture and society–via anthropology--rather than a simple presentation of the array of "Peoples" to be found there. In other words, the authors were emphasizing the scientific and analytical nature of their investigations. In all likelihood, the classification of CIMA as anthropology was due to factors both practical and theoretical. To be sure, anthropologists organized, fronted, and promoted the project initially. But in a broader sense, it was an attempt to be "total" in pursuit of knowledge, typical of a movement in American social science to build an anthropology consisting, by definition, of knowledge extending beyond the modern discipline of anthropology into all fields bearing on the human.

But what really interests me is neither the possible practical nor probably disciplinary angles, but a kind of shift one sees emerging in "anthropology" as a domain: in now saying, "Coordinated Index of Micronesian Anthropology," it is suddenly anthropology itself that is the object of concern, anthropos, rather than, quite literally, people. It's as if the object has shifted, but not yet in a way that can be defined.

ARCHIVE 4: 5 cultures 1949-55

Harvard's Five Cultures project ran at mid-century from 1949 to 1955, and was an attempt to be more systematic than ever before in capturing the essence and workings of culture in general. To do this its planners seized upon a quite specific 50-square-mile area of land just outside Gallup, New Mexico. Known to some of its more immodest residents as "The Pinto Bean Capital of the World," the area was also known to Harvard's team of social scientists as a place where five cultures – Zuni, Mormon, Spanish-American, Texan, and Navaho – had settled, each of its own accord, at least two generations before. As a result of this accidental convergence, the crossroads in the New Mexico desert was a sort of laboratory, "an excellent social science laboratory where five cultures present a variety of relationships in a complex, but manageable cross-cultural situation," wrote Clyde Kluckhohn.

To this small area the project dispatched over 65 field workers, including anthropologists, historians, political scientists, sociologists, and psychologists of both an experimental and Freudian stripe; along with 29 analysts, and 4 clerk-typists making it the biggest cooperative project of its kind ever undertaken (bigger, obviously, than Yale's Coordinated Index, just preceding it). Preexisting systematic work in Ramah since 1936 had involved 30 different anthropologists participating (for one to three years each); from their notes and records, over 150,000 items had been extracted, entered on slips, and classified in files at the Peabody Museum. In 1949, the project's overseers combined this earlier filing system with Yale's system under Murdock's *Outline of Cultural Materials*. They modified the system to incorporate the filing of values, beliefs, and subjective materials. Taking a veiled swipe at Murdock, project director Clyde Kluckhohn assured his funders, "[W]e are doing something beside the Sears Roebuck type of inventory." Researchers such as David Schneider, Clifford Geertz, and Robert Bellah, like everyone, sent their field notes back to be filed in the Peabody Museum.

What I want to focus on is not the debatable success or failure of 5 cultures. One can ask, What did all this effort actually end up with? A number of publications; many meetings and exchange of memoranda; a report of the exhaustion of the people of Ramah, one of whose residents was heard to say, "Where does Harvard get all the queer people it sends to Ramah?"16 What interests me is Five Cultures' relationship to the Laboratory of Social Relations, which was designed as an anti-bureaucratic zone of free experimentation: five cultures was a laboratory within a laboratory, in essence. (And then there is Alfred Horowitz's failed 1953 grant proposal, which argued, "the concept of values is the modern thinkers' answer to the problem of including a teleological mechanisms in any explanation of human behavior.") They were interested experimenting with teleological mechanisms, self-regulating systems, self-domestication, and autopoeises. As one social scientist at the Laboratory of Social Relations declared, "*Men are now re-combined...to provide the best collection of raw material from which...a system can be built.*17" What I have been asking throughout all these projects and archival fragments is: how did these new arrangements arise and what new objects of knowledge came into being as a result?

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16 Ramah resident quoted in memorandum to Five Cultures project, May 2, 1951, excerpting a recent letter from fieldworker Wilfrid C. Bailey, Harvard Archives, UAV 801.2010.

17 Robert Freed Bales, "Statement of Proposed Work," Memorandum to Laboratory of Social Relations, March 20, 1952, Harvard Archives, UAV 801.2010.

Commentary on the history of collaboratories

All this constitutes, in the words of Kingsley Amis's Lucky Jim a "strangely neglected topic" in anthropology. One particular aspect of these projects now largely neglected, and which I have wanted to extract by means of these assorted archival fragments, could be called the "laboratory" approach to ideas—something like C. S. Peirce's pragmatic conception of the scientific inquirer "carrying his mind into his laboratory." The value of these projects was their collaborative, collective nature. This was experimental—in two senses, one flawed, the other promising. The naive objectivism of modeling-a-social-science-of-the-future on physics was, well, naive. A more promising clue to what they offered is gained from looking at a recent statement of Lakoff and Collier in exhortation of a new approach to anthropological inquiry. They contrast two models for arriving at anthropological knowledge, the individual, avant-garde-ist and the collective, experimentalist (laboratory) approach, and argue that the individual-project model of experimentation has exhausted itself in pursuit of textual innovation. "In a laboratory, by contrast, 'experiment' does not refer to textual experiment. Rather, it refers to 'controlled experimentation' that might lead to critical rectification of concepts and claims. In the course of experimentation *concepts are put at risk* through their use and interaction with cases – either they work or not."18

The problem with these earlier collaborative projects was not their collective, sharing-of-data approach, nor yet the naivete of trying to experiment with "human data." The problem was in a misplaced epistemic faith: the faith that facts (bare facts) were the place at which the experiment took place. There was a certain commodification of facts occurring, at times. Here, I have tried to emphasize other moments, pivots, fleeting occasions when, I would argue, the experiment takes place at another level, that of the concept ("honing concepts as tools that can function in an experimental system"). The concept, in being put at risk, can be shared in a way that data could not be. I have tried to begin to extract an archeology of these epistemic things.

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18 Collier and Lakoff, 2006.

4. 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 concerning about concept work, method, and the form given to anthropological inquiry.[19] 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 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

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[19] Our prior collective and individual work on these questions includes Stephen J. Collier and Andrew Lakoff (2000). Object and Method in Contemporary Anthropology. <u>Annual Meetings of the American Anthropological Association</u>. San Francisco. Paul Rabinow (2003). <u>Anthropos Today: Reflections on Modern Equipment</u>. Princeton, N.J., Princeton University Press. Stephen Collier, Andrew Lakoff, et al. (2004). "Biosecurity: Towards an Anthropology of the Contemporary." <u>Anthropology Today</u> **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 <u>www.anthropos-lab.net</u>.

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"[20] – 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

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[20] 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?.

problems. The individual project model often results in workshops, conference papers, collected volumes and monographs in which the emphasis is placed on individual performance, 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."[21] 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

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[21] The most systematic work along these lines has been organized by Chris Kelty in his collaboration on nanotechnology.

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 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 20th 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 20th century; contemporary vaccination programs against the background of 20th 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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