



**ANTHROPOLOGY  
of the CONTEMPORARY  
RESEARCH  
COLLABORATORY**

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**WHAT IS A LABORATORY  
IN THE HUMAN  
SCIENCES?**

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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.

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## 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.”<sup>1</sup>

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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<sup>1</sup> 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.<sup>2</sup> 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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<sup>2</sup> 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.<sup>3</sup> 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.<sup>4</sup>

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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<sup>3</sup> These included a panel on “Object and Method in Contemporary Anthropology” in 2000 and on “Technologies of the Human” in 2001.

<sup>4</sup> 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 21<sup>st</sup> 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 of shared 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.<sup>5</sup>

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<sup>5</sup> 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**

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

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**

	<b>Individual Project Model</b>	<b>Laboratory for the Anthropology of the Contemporary (LAC)</b>
<b>Infrastructure and Institutional Organization</b>	<ul style="list-style-type: none"> <li>• Academic department in university.</li> <li>• Conferences, journals, academic presses.</li> <li>• Networks, loose affiliations, based on mentor relations, shared topic areas.</li> <li>• Concern about the legitimacy of hierarchies; role of hierarchy is hard to understand.</li> </ul>	<ul style="list-style-type: none"> <li>• Dependent on university but organizationally adjacent.</li> <li>• Virtual infrastructure linking a finite number of sites; meetings of principles; intensive work on discussion papers.</li> <li>• Ongoing relationships: role of intellectual trust (based on sense of shared concepts, problems); but also changing nexus of informal contact and collaborative work.</li> <li>• Explicit and openly discussed lines of authority for organizational decision-making clearly separated from authority in making knowledge claims.</li> </ul>
<b>Authorship and Originality</b>	<ul style="list-style-type: none"> <li>• <i>Sui generis</i> intellectual production; connections among authors mostly through shared invocation of “theory.”</li> <li>• “Branding” of original concepts by individual authors.</li> <li>• <i>Collected</i> work (in volumes, based on conferences, workshops).</li> </ul>	<ul style="list-style-type: none"> <li>• Recognition of diffuse character of authorship; individual authorship as a “problem” requiring negotiation, deliberation.</li> <li>• Emphasis on the development of shared concepts through a collective process.</li> <li>• <i>Collective</i> work – intense discussion, argument in production of texts.</li> </ul>
<b>Experimentation and Validity</b>	<ul style="list-style-type: none"> <li>• Experimentation with form in writing, styles of fieldwork.</li> <li>• Avant-garde effort to challenge/break away from existing norms.</li> <li>• Crisis in thinking about what constitutes a valid claim.</li> <li>• Authority connected to individualistic elements of fieldwork process and writing: “thick” description; virtuosic interpretation and writing.</li> </ul>	<ul style="list-style-type: none"> <li>• Experimentation as a way to put concepts to the test, established agreed upon demonstrations of adequacy.</li> <li>• “Secessionist” effort to conserve what remains contemporary in existing norms and to adapt them or innovate in new contexts in relation to new problems.</li> <li>• Search for impersonal methodological norms: Are concepts adequate for clarifying significant problems? Are concepts diacritical, i.e., do they make the distinctions that matter?</li> <li>• Recognition of legitimate authority based on knowledge rather than status.</li> </ul>

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.<sup>6</sup> 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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<sup>6</sup> Hans-Jorg Rheinberger, *Toward a History of Epistemic Things. Synthesizing Proteins in 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.