
COLLABORATION, COORDINATION, AND COMPOSITION

Fieldwork after the Internet

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The essays in this volume are all in dialogue with the debates and controversies for which *Writing Culture* and *Anthropology as Cultural Critique* are emblematic. For a generation of anthropologists, and those with landed immigrant status in the discipline, like myself, these debates center around *writing* as a key component of the epistemological and practical challenges of anthropology. Doubly so do these issues present themselves for a generation dealing with the upheavals and transformative promises presented by the Internet, which crisscrosses the topological triangle of ontological, epistemological, and affective issues outlined by James Faubion in his essay. As someone trained to think anthropologically about these issues—but not exclusively anthropologically—I naturally tend in exploring them toward questions of **what difference the Internet makes for fieldwork (now)**. A 2002 article in *Current Anthropology* by Johannes Fabian serves as a starting point: it proposes that the Internet turns the old art of commentary on texts into a new genre of writing about ethnographic research. Fabian reported on his experience with the creation of a “virtual archive” of popular Swahili texts, which he translated and on which he wrote commentary.¹

All authors are members of the Rice University Department of Anthropology. This article was written by Kelty, but the material for the projects, including interviews, commentaries, and additional papers, was created by all participants. Additional commentary and material were provided by Geoffrey Bowker, Gabriella Coleman, and Michael M. J. Fischer. Research was funded by an Innovation Grant from the Computer and Information Technology Institute of Rice University (CITI) and the Center for Biological and Environmental Nanotechnology.

1. See Fabian 2002; the website he created with Vincent de Rooij is, quite despite the label “virtual,” still at <http://www.pscw.uva.nl/lpca/>.

Fabian's experiment poses several questions, which this chapter explores: Does it prove the need for a new kind of anthropology, or reveal new modes of fieldwork? Does it ramify existing problems long plaguing research and writing in the discipline? Is commentary really a newly transformative genre, or is it just glorified blogging? How can we tease apart millennial promises of Internet-inspired radical transformation from the clearly felt, but dimly perceived, material changes in how we research, write, disseminate, and receive our work, specifically in terms of the practice of fieldwork?

This chapter confronts these questions by reflecting on a series of experiments in fieldwork and writing conducted in 2003–2005 at Rice University. The project is similar to Fabian's in that the practical component is “merely” a compilation of interviews, primary sources, and commentary conducted around the theme “Ethics and Politics of Science and Technology.”² The project focuses on two areas: computer science and nanotechnology. Both are areas of vibrant research at Rice University (where our research was conducted) as well as “emergent” or “strategic” (van Lente and Rip 1998) sciences that are organized in response to broad ethical, political, and cultural demands. The material of the project is thus a substantial collection of interviews with scientists, commentary on these interviews by faculty and graduate students, and conceptual articulation of ongoing and new research problems related to existing and past research. The project is open-ended in two senses: first, it was not intended to reach its apotheosis in published articles or books, but to go on living in diverse print and electronic forms as long as there were (or are) graduate students or faculty interested in using or expanding the materials or addressing the problems; and second, it is open to participation by anyone wishing to make use of the existing materials, provided they contribute their work back to the project.

In reflecting on this experiment, this chapter touches three issues: 1) it responds to some of the issues raised by Fabian concerning the status of ethnographic materials and data, and of commentary as a “genre” of ethnographic writing after the Internet; 2) it distinguishes conceptually the practices of coordination and collaboration as they relate to ethnographic fieldwork after the Internet; and 3) it reports briefly on the outcome of the Ethics and Politics of Science and Technology projects (as they stood in 2005, when this article was written) and how they exemplify the first two issues.

The practice of “commentary” and the more general concept of “composition” are intended to augment or extend the familiar critiques around anthropological writing (such as those raised in *Writing Culture* [Clifford and Marcus 1986])

2. Project Website: <http://kelty.org/epit> and <http://kelty.org/epnano>.

by including the Internet and new media and the challenges and opportunities they pose. From the perspective of digital media, the concept and the practice of “writing” neither exhaust the challenges of conceptual innovation and anthropological analysis, nor are they sufficient to describe the range of practices in which anthropologists increasingly engage, from fieldnote writing to blogging, from book-lending to social book-marking, from letter-writing to listening and observing, from qualitative data analysis to collaborative interviewing, from draft articles circulated among informants and colleagues to public wikis. All of these activities involve writing of some kind, but the term hardly captures the complexity of the activities of organization and conceptual innovation that thereby result—much less the multiple ways the Internet and digital media impinge on them or pose new opportunities. At a pragmatic level, this profusion has affected every discipline, changing the micro-practices of everything from bibliographies to photography to the ease with which sophisticated software can transform “raw” material.

Despite all this, the only lasting marker of success in research in social and cultural anthropology remains the article or book—and in the last twenty years particularly, books that display the virtuosity and innovative genius of individual researchers, and not the expansion and consolidation of concepts and problems specific to communities of researchers, much less disciplines (Collier and Lakoff 2006).

“Composition” therefore is intended to be a somewhat broader concept that captures this diversity.³ We say “composition” here because it is more inclusive than “writing” (paintings, musical works, and software all need to be composed, as poetry and novels do). Writing implies the textual and narrative organization of language—still a difficult enough problem of composition, and still the gold standard; but it leaves out the composition of images and sounds, or especially how other kinds of objects are composed as part of an ethnographic project: documents, statistics, forms, legal documents, unpublished works, audio transcripts, blog-entries, and so on. The Internet neither solves this problem nor simplifies it—if anything it makes it more difficult at the same time that it provides the possibility of solving it. Fabian put it well: “no more than other ‘media’ does the Internet guarantee its own communicative or informative success. It does not preempt the labors of ethnographic representation; it only changes

3. Gregory Crane, creator of the Perseus Archive of Greek and Roman texts, articulated the concept of composition in an article in *Current Anthropology* from 1991 (Crane 1991). He distinguishes between “writing (a particular historically contingent task) and composing (what authors do, no matter what the medium or technology)” (293). Such a notion of composition as an activity distinct from writing is also explored in detail in the work of Mary Carruthers (2000, 2008).

some conditions, and it was to the latter that I wanted to direct our attention” (Fabian 2002, 785).

Similarly, the challenge of dealing with huge volumes of information—not only as a consumer of information, but as a producer as well—means experimenting with new modes of composition that can give specialist and generalist colleagues alike quick synoptic overviews of research materials and problems and trajectories without sacrificing the scholarly detail and individual virtuosity that has come to be valued in the discipline. It is in some ways an attempt to have our cake and eat it too: composition is a practice that crosses between writing understood as an artful craft and research understood as conceptual innovation (or more specifically, fieldwork understood as an epistemological encounter and a platform for shared conceptual work). The article and the book are, and will continue to be, the primary locus and container of concepts and ideas—but they will have been transformed by the array of new media and new technologies, each of which will have effects on the kinds of research and writing that are now possible. That is to say, somewhat awkwardly, that the book of the era of the Internet will look and act differently from the book of the era of the book.

Alongside the problem of composition, our research project also demonstrates the need to distinguish carefully and clearly between collaboration and coordination in anthropological work. Traditionally, collaboration has had two meanings in anthropology: collaboration among researchers, and collaboration between researchers and research subjects (on the latter see Lassiter 2005a). Both forms of collaboration are active here, but we distinguish between coordination and collaboration in the following way: coordination is the media-specific material and technical choices that allow a group of people to work together on similar topics, in the same places, in structured time frames, and with the same group of subjects. Collaboration is the conceptual and theoretical work that results, if it results. Coordination does not imply collaboration, but collaboration entails coordination.

By this definition it is possible to imagine both axes of collaboration—among researchers and between researchers and subjects—starting from different forms of coordination. Coordination between anthropologists takes different forms and happens at different time-scales than does coordination between researchers and their sites and subjects. While we would never suggest that anthropologists have not coordinated research among themselves, or with informants, or achieved collaboration in both cases, we would suggest that there has been little reflection on the media-specific differences of coordination that make a difference in attempting to foment collaboration of either form. Or to put it in terms of “connectivity,” there are scales and metrics of connection between researchers and between informants that can help elaborate which kinds of coordination

might yield conceptual innovation (which I associate with collaboration) and which do not.

For instance, the management of power relations between anthropologist and informant (both “up” and “down”) demands specific, material strategies and tactics of coordination. A researcher must design ways of interacting, observing, or participating, must lure informants into conversation somehow, must find ways to register (record, remember) a conversation, devise means of (co-)interpreting a conversation, transform it into an accessible written form, and disseminate it (these last two forms of coordination have been traditionally structured almost exclusively by the academic publishing industry), as well as managing the affective or emotional tensions of interaction, friendship, suspicion, and the like. In each given case these activities will be organized differently depending on the vagaries of the situation. Only in some cases will such coordination come to be, in hindsight, seen as collaboration; as for example when researcher and informant end up working together in the field, or writing together (e.g., Fischer and Abedi 1990); or when researchers literally work for an informant (in a corporation, for instance) or when researchers, in extreme cases of “going native,” cease to make such distinctions.

By the same token, the most elaborate and hierarchical forms of coordination in science do not necessarily imply collaboration—they often serve only to break up a problem into identifiable, exclusive chunks that will then be addressed by individuals (perhaps with some recursion through which collaboration again becomes possible within the chunks) who are in turn not expected to exercise virtuosity or genius except at the level of such general systematic planning and coordination. Classic forms of anthropological collaborations such as the New Nations project might be seen as a kind of middle ground—and, in some narratives of the project, produced irresolvable tensions as a result.

Given this distinction between coordination and collaboration, it is also clear that there is far less coordination between anthropologists (who are trained and evaluated in highly individualized frameworks) than there is coordination between researcher and research subjects (which is after all the one form of coordination that requires utmost attention in all cases of ethnographic research). Graduate students and faculty may keep in touch with colleagues who are in the field, but are very rarely invited to participate in the research in structured ways (though compare the account of “improvising theory” in Cerwonka and Malkki 2007). Traditionally, it is the disciplines and subdisciplines themselves that have served this coordinating function between academics’ projects (and hence also as the platform for collaboration) by keeping track of current problems and research directions, by disciplining scholars into forms of research that respond to these problems and by adapting problems to new and emergent phenomena.

In the age of inter-, trans-, multi-, and anti-disciplinary critique and innovation, however, the question is raised anew: If not by discipline, then how does one identify a significant problem, how does one become satisfied with the appropriate methods of research to pursue such problems, indeed, how does one determine to whom one is speaking about these problems and for what purpose, in the absence of strong disciplinary signals?

Befitting the role of ethnography as epistemological encounter, my understanding of these issues emerges from my own fieldwork experience among Free and Open Source software developers and advocates—an example and a template that I will return to throughout this article for what it can tell us about coordination and collaboration in anthropology. From this perspective, Free and Open Source software projects provide not just field sites, but exemplary cases of a response to a reorientation of knowledge and power that is also facing anthropology and other disciplines (Kelty 2008b). The response of Free and Open Source software is a template from which to imagine the more precise kinds of changes necessary in anthropology and a foil against which to judge proposals for new forms of writing or research.

The distinction between coordination and collaboration also has a strategic purpose in the “Ethics and Politics of Science and Technology” projects which are reported on herein: the intentional and sustained attempt to innovate forms of collaboration (based on practices of coordination) that do not sacrifice the eminently practical need for individual projects to develop, progress, and be evaluated as such. We seek to create coordinated projects that fertilize multiple individual, idiosyncratic interests, allow them to develop in parallel, and reap the benefit of continuous, partial collaborations. The “collaboration” emerges not from a top-down concern with answering a set of discipline-defined research questions, but through the problem-seeking activities of the individual researchers, conducted alongside one another, coordinated both through technical means and through structured engagement.

The Ethics and Politics of Science and Technology

Starting in 2003, we began two related experiments in ethnographic research on the general theme of ethics and politics of science and technology—one on computer science called **EPIT (Ethics and Politics of Information Technology)** and one on nanotechnology called **EPNANO (Ethics and Politics of Nanotechnology)**. Both experiments were conducted primarily at Rice University, the substantive topic areas reflecting two of the more vibrant research areas at this small

university, and each involved one or two faculty and around four graduate students. The methodology of the two projects, along with some of the preliminary results and conceptual work, is described herein.

EPIT, the initial project, was funded by the Computer and Information Technology Institute (CITI) at Rice University—an institute devoted to brokering relationships across the schools and disciplines and, through funding, lectures, lunches, and other activities, to encouraging individuals to seek out and form collaborations with people in faraway disciplines. CITI is one of four such institutes at Rice (the others are in Biology, Environmental Science, and Nanotechnology) whose aims are to foster interdisciplinarity and enhance communication of research results across departmental and disciplinary borders. CITI's institutional mandate is to see research that would not otherwise be funded, or necessarily even performed, seeded on their dime and then, if possible, leveraged into larger funding sources. They were therefore successful in convincing members of the anthropology and philosophy departments (albeit scholars trained in science studies and philosophy of science) to develop a project that loosely asked the question: **What kinds of ethical or political issues currently face working engineers and computer scientists and how can we collaborate to address them?**

From the perspective of the anthropologists and the philosopher involved, the project was **not conceived of as beginning with philosophical definitions of ethics; instead, we sought only to investigate, ethnographically, the projects and practices of particular computer sciences, in the hope that our dialogue with and observations of particular computer scientists would bring out several “ethical” issues to be refined over the course of the project.** On the one hand, such a goal is trivial: **nearly any practice worthy of the name can be understood in terms of its ethical dimensions.** On the other hand, we sought to discover **not only what the scientists themselves considered ethical problems, or what “normal” practices they engaged in, but especially the problems that confronted them for which they had no name—emergent, unusual, or contingent issues for which their training did not prepare them.**⁴ More often than not, they referred to these issues (issues that were not part of everyday practice) as ethical. The choice of scientists and research areas was therefore strategic.⁵

4. On this subject, we are guided by a number of recent attempts to theorize the modern transformation of the ethical: Fischer 2003; Rabinow 2003; Faubion 2001b; Lakoff and Collier 2004.

5. Indeed, the ability to make such a strategic choice depended largely on the extensive topical knowledge of one of the research group members (Kelty), who had already conducted significant research in this area and was therefore attuned to which research areas might be most salient for our questions; a second project on nanotechnology has therefore proven somewhat more challenging in that there are no experts among us, making the choice of subjects a slightly less principled one.

The second project, EPNANO, was funded by the Center for Biological and Environmental Nanotechnology (CBEN), an NSF-funded center devoted to exploring the “wet-dry” interface between natural and human systems, and emerging nanomaterials and particles. All of the NSF centers funded through the National Nanotechnology Initiative (NNI) also have educational/outreach programs and social and ethical impact programs as part of their purview. In the case of CBEN, the researchers are funded through the latter program and are expected to contribute research to the center.

The nanotechnology project was also conceived as a way to discover, through ethnographic research, the emerging issues being labeled ethical or political; the demand for such research has been extremely high in the last four years, and the availability of funding for social science research on nanotechnology stands to transform the kinds of topics and issues that science studies, policy studies, technology assessment, science ethics, and other associated fields might pay attention to. In 2005, three NSF Centers for Nanotechnology and Society were funded to pursue this research (see, e.g., Guston and Sarewitz 2002). Whereas computer science has a sense of its history, the tools it has developed, and its concepts and problems, nanotechnology represents a much earlier stage of the constitution of research fields and projects, and a large part of our research has been the investigation of who participates in it and how they conceive of its novelty and historical emergence.

Method

In both experiments, the research protocols have been similar. The research has been primarily participant observation and interview-based, with graduate students and faculty pursuing leads to hang out in labs, attend meetings, or conduct associated research and reading. A significant component of the “coordination” is the coordination around the various formal interviews; it usually follows the same protocol. The research group meets to read and discuss materials that have been provided by the interviewee—usually impenetrable and obscure research publications, of which we can normally parse only the first sentence. Of the abstract.⁶ The research group delves into these areas and develops a loose set of

6. One of the small satisfying successes of this project was the fact that anthropology graduate students—most of whom were avowedly uninterested in computer science—went from being intimidated and bewildered by the research articles to being comfortable with and unthreatened by the language in the space of six months. They only really came to believe the abstract assertion that there is a difference between knowing a lot of science and knowing the meaning of science via the practical experience of exploring and discussing the works with practitioners. This, if anything, is confirmation

questions together based on discussions and reading. A subset of the group conducts an interview and transcribes it. The transcription is sent to the interviewee for comment, and the research group meets again to determine which, if any, of our questions were answered, which need further clarification, and what direction to head in for a second set of questions and interviews. The development of question areas and the response to the interview are guided not by a predefined set of research questions, but primarily by the research interests of the participants, many of whom are graduate students whose dissertation projects have nothing to do with computers, nanotechnology, or ethics. As a result the range of questions can be quite surprising and the kinds of connections startling to the interviewees.

Over the course of the two experiments, the process has been given a more formal or abstract description consisting of more or less explicit stages: problem, inquiry, and analysis.⁷ The process is intended to be circular: problems are identified in such a way that inquiries about those problems make sense (ranging from questions to ask in interview, to archival questions to follow up on, to observational questions to pursue in a lab); inquiries are meant to yield some kind of raw data: interview transcripts, descriptions, papers or documents, and the like; inquiries serve as the basis for analysis (writings and discussion of various sorts); and analyses are intended to refine problems for the next round. The circular process itself is the basis for experiments in “composition”—experiments that aim to fix this process in multiple media: as an accessible and navigable website (navigable to both researchers and potential readers); as a glossary of concepts that emerge from the problem-inquiry-analysis circle; and as papers, articles, and other standard scholarly forms. Coordination between researchers takes place in all three stages. Coordination between researchers and subjects takes place primarily in the inquiry stage, but overflows into the others to the extent that interviewees are interested in reading or contributing to the material.

As researchers read and reread transcripts, make annotations, and attempt to think through their own research questions and interests, a heterogeneous body of objects begins to develop—results of inquiry and analysis. Often the objects take very conventional forms: audio and video files of the interviews, transcripts, reading notes, face-to-face research meetings, shared ideas and annotations on

of what one participant asserted concerning the project: “interviewing is not ethnography.” The relatively long-term immersion in the material and the interaction with both subjects and colleagues concerning it were essential, if ephemeral, aspects of the project, and confirmed our assertions (when questioned by computer scientists) that there was no “algorithm” for ethnographic research.

7. These three terms first emerged from discussion with Andrew Lakoff and Steve Collier in comparing research processes with that evolved in the Anthropology of the Contemporary Research Collaboratory (ARC): <http://anthropos-lab.net/>.

the transcripts, related news stories, and events and analytical writings in various stages. These objects can take form through classic ethnographic techniques, such as literally cutting up transcripts and putting them in coded piles, as well as with digital tools, such as wikis, content management systems, and online editors of various kinds for collaborative mark-up of transcripts. No single tool is intended to become the de facto tool of inquiry or analysis, though we do have a commitment to tools that are freely licensed (Free Software/Open Source) in order to ensure legal and technical archivability. Tools that allow multiple users to edit the same texts at the same time and record both the commentary and who made the commentary are particularly sought after.⁸ The interviews themselves tend to be open-ended, though we are careful to give the interviewees a chance to read and discuss the transcripts prior to the second interview.

Once a suitable body of transcripts, articles, notes, and observations have been shared and read by everyone, the next step is defining constraints and goals for short commentaries or analyses. It is here that the problem of composition is explicitly broached: the problem of making commentary fit into a synoptic genre; that is, to make it more than a proliferation of details and notes. At the same time that participants are asked to think about their own detailed interests, they are asked to think about making those interests respond to a core set of concepts that would allow a new reader to understand and navigate the material we produce. Practically speaking, commentaries have taken the form of assignments (we call them “missions” which participants can experience either in deference to anthropology’s past or with a James Bond–style enthusiasm), usually to produce less than 2,000 words focused on responding to a particular concept or problem. These assignments might ask all participants to write something about a particularly rich passage or two in a transcript; or to undertake etymological, rhetorical, or linguistic analyses; or to analyze transcripts with respect to a particular concept (“trust,” “scale,” “security,” “membrane,” etc.); or to pose or answer a question in relation to a given article or book. The goal of such assignments is not artificially to restrict the work of participants, but instead to promote a kind of contrastive approach. The value in such assignments lies not in their ability to encourage virtuosity, but instead in their ability to instantly provide a kind of

8. The array of tools for this kind of collaborative writing have mushroomed in the period 2005–07, in part because of innovation around wikis, but wikis tend not to record the identity of the contributor in an obvious way; blogs and content management systems are almost always structured around a text followed by a string of threaded comments, making it hard to insert comments as annotations to a text. In part online collaborative word processing has been driven by Google’s challenges to Microsoft. Writely.com (now “Google Docs”) was the first successful example, though not quite easy to use and not Free or Open Source. Other such tools are likely to proliferate. Tools for annotating and sharing video and audio are even rarer, as are Free/Open Source qualitative data analysis tools; the TAMS analyzer (Weinstein 2006) is one important step in this direction.

Rashomon-like perspective on a given phenomenon. Often the assignments are developed through discussion and argument—but the writing of them is generally conducted in private. The key to reading such documents is to know how they all respond to a central mission. It has always been a goal to make these texts link to one another, to the mission, and to the details in the transcripts that they are referencing, although it hasn't necessarily worked in practice (for both technical and methodological reasons).

The objects produced by the research process proliferate quickly: notes, questions, transcripts, highlighted transcripts, annotated and condensed transcripts, lists of keywords, sets of annotations, commentaries on the transcripts and annotations, structured assignments, commentaries that responded to a structured set of questions, commentaries that emerged according to the interests of individual researchers, definitions and conceptual glossaries, conference papers and articles, reviews and blogs, and so on. Such a heterogeneous set of documents is a familiar body of work for most social scientists, and often the work of synthesizing this into a single coherent article or book is what is most highly valued. While this project does not shy away from that challenge, it nonetheless highlights two different challenges: 1) making visible the process and materials that constitute that work, documenting in some form the work of conceptualization that goes into the valued articles, books, and concepts developed by anthropologists; and 2) maintaining an openness to new directions that would otherwise be closed off by the demands of individualized article and book production. Indeed, the very challenge of imagining what that kind of openness looks like has not been an explicit concern of social scientists: What technical, social, and practical constraints are necessary to keep such a project “alive”? And, if a project is not to reach its apotheosis in a book or article, what might be the signal that such a project is in fact complete? In the case of the first project (EPIT), while the interviews and annotations done by the initial group have come to a standstill after a successful workshop that included three outside researchers, work on the development of ideas and articles related to this initial project continues for one faculty member (Kelty) and at least one graduate student (Kayaalp).

In many ways this research process was absolutely conventional: it involved only interview, observation, transcription, and analysis followed by a workshop and discussion. It was therefore indistinguishable from most work in cultural anthropology—which was good news for the graduate students, whose self-definition as professional researchers is at stake. However, there are two aspects of this process that we consider to be novel and that were raised as questions throughout the project. The first was the question of commentary: What form could it take that would make it stand out as more than a footnote or a response? Should we aim at producing some kind of “case study”? Should we turn to the

figure of the Talmud (this seemed to give too much credit to both the interviews and the commentaries)? How should we organize (hierarchize?) the commentaries? Do we do so at the outset, or wait until they are written? How do we give them structure, but keep them open-ended? Do the available software tools liberate us to think about composition, or straitjacket us into particular modes of presentation?

The second was the need to clarify the meaning of collaboration. Many students who participated in these experiments were fundamentally frustrated by my perceived failure to say why we were doing this research: What were or are the goals, what did I (Kelty) want to find out? From one perspective collaborative research is appealing (especially paid collaborative research) precisely because it gives students defined research tasks, for which they are not responsible in any profound intellectual sense, but through which they might learn “how research is done.” By contrast, my injunction that they generate their own questions and follow their own interests and leads was always met with approval, but confusion: How then is it “collaborative”? What makes it different from the individual research projects they are asked to pursue as graduate students? It is through this tension that the distinction between coordination and collaboration was developed.

In this respect, my own work on Free Software provided a useful foil and template for understanding our goals (Kelty 2008b). One of the most well known claims about Free Software is that it involves the collaboration of hundreds if not thousands of volunteers who work together on the creation of highly complex software projects, like the Linux operating system kernel. Common wisdom suggests that this mode of collaboration (or “peer production,” to use Yochai Benkler’s term) is somewhat anarchic and free-wheeling—a bazaar, not a cathedral (Benkler 2006; Raymond 2001). Further research has revealed that it is not anarchic at all, but that it does proceed in ways strikingly different from those in which a conventional software company might proceed. Although it is driven by volunteerism and individuals are free to work or not work on whatever they choose, it is nonetheless extremely highly coordinated.

The Linux project, for instance, consists of a leader (Linus Torvalds) with a hierarchy of lieutenants, each responsible for different parts of the kernel; a mailing list on which all participants are free to communicate (the Linux Kernel Mailing List, or LKML); and a source code management system that keeps track of who writes what and when and allows for a certain degree of automatic management of asynchronous, distributed contributions from participants around the world. There are, however, no goals and no planning. The project privileges a particular form of adaptability at all cost—whatever someone creates, it can be incorporated so long as it passes a series of tests having to do with a largely unarticulated, but learned, intuition about technical elegance, functionality, and

the structure of the kernel itself. Torvalds and lieutenants facilitate this kind of contribution, but do not direct it. As a result, the Linux kernel does a great many things, some of them relevant only to very obscure architectures or uses, some of them useful to every user—but it was never designed to do any of them.

Of course one should ask: How do people know what to do? In some ways, this is the role of pedagogy: the construction of a disciplinary structure within which it makes sense to pursue one kind of problem rather than another. Linux makes sense because generations of students have been taught what an operating system is and should look like by studying UNIX (in the 1980s and 1990s) and Linux (today). The coordination of contributions to Linux is largely routine and invisible. People learn what to do and how to do it, and they simply do so. What emerges, sometimes, but not always, are forms of collaboration: co-work, co-labor, co-thinking about how to identify problems and functions, and how to solve them. Much of this work takes place on the LKML, simply as a kind of question-and-answer discussion, often with flame-wars around controversial topics. As people settle into these collaborations, coordination sets the stage: the structure of lieutenants, the mailing list, and the Source Code Management (SCM) tool sets the constraints around how that collaboration will unfold, and more important, keeps track of it and manages it as an experiment. The success of a collaboration is in the outcome, not in the justification or planning—higher risk, higher reward, less bureaucracy and planning mentality.

As a result of this particular kind of coordination, the Linux kernel emerges as a collaborative project only in a weak sense: it is an architecture that allows multiple kinds of individual contributions to be included, and privileges this “adaptability” above any other design criteria.

The EPIT and EPNANO projects share some of this commitment to adaptability—even though there are significant differences (far fewer participants, a completely different domain of knowledge). The reason the commitment to adaptability is privileged is because one of the core conceptual claims of ethnographic fieldwork, especially in the tradition referenced here, is that ethnography is an epistemological encounter, one that might require, as George Marcus put it somewhere: “a theorem [!] of planned incompleteness, but not sloppiness or indeterminacy.” Adaptability in the Linux kernel is a precise form of planned incompleteness—a way of insisting on openness to new questions and directions (and this takes legal, technical, and social forms). Ethnographic fieldwork shares this commitment over against a commitment to research design that sets questions in advance and for which fieldwork is mere data-gathering. The EPIT and EPNANO projects are ways of making visible that activity through coordination.

In the EPIT/EPNANO case, the initial gambit is that “sites” or the areas of study (computer science and engineering or nanotechnology) are capacious enough to allow for a wide array of directions to pursue, each in his/her own way, upon a shared platform that is coordinated fieldwork. Coordination is achieved through the technologies (shared content management systems, e-mail, collaborative tools for editing), but more importantly through setting up “missions” with defined time-frames and limits, and identifying projects (like clarifying concepts or collecting data) that people can pursue on their own.

If, in the case of Linux, contributors know what an operating system looks like, then the relevant comparison in anthropology is that researchers know what an anthropological research question is and how to research it. But fieldwork is precisely the tool by which such research questions become fully formed, or, at least, by which they transform from an initial speculative proposal into a practice of forming concepts in specifiable contexts. If Linux were a conventional engineering project that proceeded through strong design management, it would seek merely to adapt the idea of “operating system” to changing conditions. Similarly, a strong disciplinary focus merely adapts traditional anthropological questions (anthropology of exchange, African political institutions, kinship studies, linguistics) to changing conditions (kinship studies confronts reproductive technologies; anthropology of exchange confronts the new economy, etc.).

But Linux does not proceed by strong design criteria; it proceeds only with respect to what contributors produce and what the leaders accept as “working”—a nontrivial problem that entails a fantastically refined engineering knowledge and aesthetic sensibility and is analogous to the problem of “composition” raised here. Contributors to Linux “compose” new ideas and forms out of the existing platform without regard for, or against, the historical plans, meanings, and functions of “operating system.”

In our case, fieldwork as a coordination platform is intended to open up the same possibility: that fieldwork can pursue multiple different significant problems based on the intuitions of the fieldworkers and the coordination constraints of the project. Composition thus is analogous to coming up with a protocol for making all of these partially restricted contributions of multiple participants “work” together—a nontrivial problem that entails a somewhat less fantastically refined, but nonetheless deep sense of scholarly pursuits generally, anthropological problems in particular, and an aesthetic sense of form in new media.

Such an experiment, if successful, amounts to a strongly, perhaps radically “conventionalist” approach: research problems and approaches do not refer back to disciplinary questions or to “theory” as a guiding hand but directly to the practice and outcome of fieldwork itself—an immanent theory and critique.

And such a practice opens one up to going beyond going native, and into the domain of finding friends and mentors in the field, not only in the discipline.

Substance

Given the methodological orientation, the broad theme of our experiments is focused on forms of immanent critique, especially novel questions and answers about ethics and politics of science and technology. The EPIT project revealed three areas of such interest: norms of practice in computer security, the “immanent critique” of electronic voting machines, and the status of “trust” and “risk” among computer scientists. The EPNANO project by contrast has uncovered a number of different issues which, at the time of this writing, were still in the process of formulation (and later developed into a concern with nanotechnology and responsibility [Kelty and McCarthy n.d.]).

EPIT

While numerous anthropological projects have been completed or are under way that study hackers, geeks, and Internet-based social movements, there is relatively little recent work on academic computer scientists (notable and formidable exceptions are Star 1995; Bowker 1997; Bowker 2001; Forsythe 2001). Academic computer scientists represent a crucial point of comparison, especially for recent work on Free Software/Open Source and hacker ethics and politics (Coleman 2004). Several EPIT project participants were fascinated with the discourse around bugs, security, and the norms concerning “good and bad bug hunting.” Security researcher Dan Wallach explained in detail the mechanisms whereby one hunts for and finds bugs in software—much the way a hacker (or cracker) would search for vulnerabilities that might be exploited for good (or evil) purposes. Wallach (like other academic security researchers) is concerned with building a career out of this activity, and two salient points emerged from the discussion.

The first was that Wallach talked of his “research pipeline”: “Find a bug, write a paper about why the bug was there, fix the bug, write a paper about how you fixed the bug, repeat.”⁹ His list of publications bears this out, numbering more than forty by his early thirties. Wallach’s narration of the norms of bug hunting was the explicit focus of one researcher (Potoczniak) as well as a topic of frequent discussion. On the one hand, they revealed much about the very fragile

9. Dan Wallach/transcript #1 available through the EPIT website.

line between legitimate and illegitimate types of research in the computer security research community and even to some extent how cultural background can play into the construction of these lines.

The lines between legitimate and illegitimate research were also central in interviews with Peter Druschel, whose work on peer-to-peer systems raised difficult questions about how to legally pursue research that can result directly in more efficient and robust file-sharing programs (Kelty 2008b). Druschel was also a point of contact for comparison with studies of hackers and Open Source programmers, and our work suggests that there is still a significant difference between the vocation of the scientist and that of the tinkerer or bricoleur—one that has less to do with savage or civilized minds than with the bureaucratic, deliberate, and methodical practice of contributing to science in a structured and cumulative form. By contrast, hackers and tinkerers engage in fast and furious creation of often mind-bogglingly clever or elegant code, which is nonetheless inscrutable or difficult to use on account of its idiosyncratic creation by hackers or Free and Open Source projects with ad hoc management and little concern for replicability or archivability of results. They have “users” who need to be satisfied—computer scientists usually do not.

A key point of entry for researchers into the arcane interests of computer security researchers was the surprising use by Dan Wallach of the concept of a “gift economy” to explain the practice of finding and revealing security flaws in corporate software. It was surprising simply because it’s always surprising to see concepts from anthropology circulating in a foreign place, but, more important for anyone who has had the pleasure of associating with geeks, hackers, or Free/Open Source software aficionados, they know that the concept of “gift economy” circulates quite widely (first used by Rheingold 1993, then in Ghosh 1998 and Raymond 2001) as a folk explanation of reputation and value creation. So it was doubly surprising to see it used by Wallach (who admitted that he probably knew about it only because of Raymond) to refer in this case to the specific activity of “giving” a security flaw to a corporation (rather than “giving” it to the world—i.e., making it public), in return for which he was “given” a \$17,000 computer.¹⁰

This exchange was deeply formative for Wallach, and he often uses it to explain to his own students the proper “ethical” practice of finding, reporting, revealing, and expecting payment (or not) for security flaws. At the time of this event, Wallach was a graduate student and the Internet was just emerging as an everyday object; he was thus one of the first people in the history of the discipline to perform this kind of action and one of the first to call it research. Prior

10. This story is in Dan Wallach/transcript #1.

to this point, security flaws were simply problems a company might or might not choose to deal with—Wallach and a handful of others introduced a way for them to become an issue of public safety or commercial accountability. Wallach further illustrated the security flaw gift economy through his explanation of Netscape’s early “bugs bounty” program—in which Netscape paid \$1,000 for security flaws (beginning in roughly 1996). According to Wallach, this system was very soon abused, providing as it did an incentive for people to find as many bugs as possible and then demand payment for them. Wallach refers to this explicitly as “blackmail” and differentiates it from the ethical incentives that he had (very rapidly) evolved to make security research into more of a cat-and-mouse game between researchers and corporations, as opposed to what he saw as a kind of protection racket.

A second area of intense interest for the EPIT project—primarily because of the high-profile aspect of the research—was Dan Wallach’s involvement in the nationwide (and eventually global) controversy over touch-screen electronic voting machines (EVMs). Wallach was a co-author on the report that analyzed the Diebold electronic voting machines and found them vulnerable to various kinds of attacks (Kohno et al. 2004). As was clear from discussions with both Wallach and Moshe Vardi, CS researchers are reluctant to grant absolute security status to any software system—it is therefore not at all surprising that the EVMs contained security flaws that could be exploited to rig an election or change the results. This was not, however, the core of the controversy.

The EVMs made by all of the major manufacturers by 2003 (helped along by the Help America Vote Act, which disbursed large amounts of funding to local election officials around the country, who in turn signed contracts with EVM companies) were touch-screen-only machines. None of them possessed any mechanism that would allow 1) a voter to verify in some tangible way (i.e., other than on-screen) that the machine was recording a vote correctly or 2) an independent recount of the votes based on something other than the potentially compromised software itself (i.e., a paper ballot). The research that Wallach and friends conducted, therefore, was not necessarily directly connected to the main problem—that is, though they proved the existence of flaws in the machine they were not asking Diebold to make the machine one hundred percent secure. They were instead asking them to provide an independent verification of the vote, when the machine is broken or compromised. In some ways, it could be simply stated as a core engineering principle: make the function of the machine robust. The call went out, both from Avi Rubin of Johns Hopkins and from a well-known CS researcher from Stanford, David Dill, for a “voter-verifiable paper trail.”¹¹

11. Two sites with more information on the controversy are <http://www.verifiedvoting.org> and <http://www.voterverifiable.com>.

Through protest, public speaking, activism, and organizing, these CS researchers, along with other activists and organizations (such as the Electronic Frontier Foundation), were successful in convincing several states retroactively to require EVM makers to include a vote-verifiable paper trail on the machines they sell.

The problem of verification in this case includes a palimpsest of transparency issues—adding up, as one might suspect, to a pretty opaque problem. In the first place, Wallach and friends never actually had access to a real Diebold voting machine for their study. They had only a collection of source code that had somehow or other been “liberated” from Diebold’s webservers (according to Wallach, activist Beverly Kaufmann claims to have “found” the code lying open for anyone to see on a webserver based perhaps in New Zealand).¹² Wallach and friends’ article (Kohno et al. 2004) was therefore based not on the “official” Diebold machine, but on the only code they could obtain. Of course, from Wallach’s perspective, there was no other choice: in order to inspect any of the corporate machines, Wallach would have had to sign a nondisclosure agreement that would effectively stifle his ability to publish the results of any research. So they decided to base their research on the liberated code, risking violation of trade-secret law, which, as Wallach likes to point out, not without some glee, is a felony offense in Texas.¹³

A second layer of opacity comes at the level of certification. Because of the trade-secret status of the software code owned by the corporations, certification by the U.S. government happens in secret and, according to Wallach, both the certification process and the certification itself are classified, unavailable for review by anyone. Wallach is justifiably appalled by this state of affairs—but ever the scientist, he offers an explanation in the form of “regulatory capture”: that government bodies set up to regulate the process and mechanics of national elections in the United States are effectively controlled by the corporate interests creating the machines. While this response may sound cynical, it is actually a kind of analytic device that allows Wallach to pinpoint where the weaknesses are in the system—that is, they are not only in the code, but in the democracy itself.

A third layer of opacity concerns what happens in the voting booth. The controversy concerns the fact that many voters and many voter organizations are appalled at the activism of CS researchers, because they see it as a big step backwards. For these organizations, EVMs represent huge strides in achieving equality and access at the voting station: blind voters can vote alone with an audible ballot, elderly voters can increase the font size or fix a miscast vote, and voters with motor disabilities are better accommodated by the machines. In addition, election officials see great benefits in eliminating paper: they see the machines,

12. This story is recounted in Dan Wallach/transcript #2.

13. Ibid.

and rightly so, as potentially more accurate, and certainly much faster at counting votes, than any other system.

CS professors, however sympathetic (or not) they may be toward such concerns, are pointing to something else: the superimposition of particular technical structures onto long-standing democratic structures. They decry the potential for the effective re-legislation of democratic election policy. CS researchers, possibly for the first time in history, find themselves arguing that we should trust technology less and use more paper. They can see clearly that it is not the technology that is at issue, but the structure of democracy and the protocol by which elections are carried out, recorded, verified, and ultimately legitimated.

The implication of this experience, for our research group, is that deep within computer science, there is an immanent critique of government and one which CS researchers find themselves reluctantly and somewhat awkwardly trying to articulate in the only language they know—that of science. Part of the goal of this project, then, has been the attempt to articulate, through dialogue and commentary, the nature and meaning of this immanent critique. Of course, given the very high-profile nature of this issue, we will not have been the only people attempting to do so, but the conclusion for science studies and anthropology remains the same (and one demonstrated beautifully twenty years ago in *Leviathan and the Air-pump* [Shapin 1985]): that at its heart, scientific practice is also about social stability and legitimacy and that where such arguments erupt, one can clearly see both the state of the art and the art of the state.

EPNANO

The EPNANO project is still under way, and so the substantive issues remain open. There are however, some clear lines of interest. The most significant of these is the role of research into the potential health and environmental impacts of nanotechnology, an area that Rice, through CBEN, has seized on and turned into a robust field of inquiry. Mark Wiesner, one of the original Principal Investigators at CBEN, has been a core interlocutor in describing the process by which a certain kind of bargain was struck with respect to research on environmental and biological effects. Rather than simply funding research into toxicology or hazards, CBEN has promoted instead research that focuses on health and environmental uses of nanoparticles and on the challenges of engineering safe nanoparticles from the get-go (Kelty 2008a).

Several “problems” are being refined by this aspect of the research: the question of reflexivity in science and the creation of structures of “anticipatory governance” (Guston and Sarewitz 2002); the tension between risk calculation and other forms of foresight, prediction, and preparedness; the increasing separation

of Environmental and Health Safety (EHS) issues from other areas of “social” implications (including legal, ethical, and cultural issues); the question of nanotechnology as a “weakly contextualized science” (Nowotny, Scott, and Gibbons 2001) or as a “strategic” science (van Lente and Rip 1998) that defines a worldview and a general strategic focus, rather than being focused on solving specific problems of interest to the state.

Several researchers in the project have also found an interest in looking at “new objects” such as nanoshells, nanotubes, nanorods, nanocars, membranes, or nanotube fibers. When such objects can only be imaged through complex technologies and devices, how does one orient one’s epistemological questions? Two research areas in particular have drawn our attention: membrane engineering in Wiesner’s case and vacuum technology in the case of Kevin Kelly. Building on work by Cyrus Mody (Mody 2001) on purity in the lab, this research direction has clear aesthetic appeal for EPNANO researchers—ranging from the material culture of science to the more metaphorical and allegorical uses of filters, membranes, and vacuums in understanding researchers’ self-understandings as part of an emerging science. Naficy, for instance, posed the question of the “social-as-membrane” apropos of Wiesner’s description of the interplay of science, economics, and environment, and Dib has explored the idea of nanoparticles as liminal objects that reflect prudence and ambivalence among researchers. Powell pursued a project more historical in nature, focused on the institutional creation of the National Nanotechnology Initiative and other agencies and institutes that promote nanoscience and nanotechnology.

While the projects described above are substantively specific (anthropology of science and technology) and focused on the use of new media and Internet-based tools, the conclusions I draw about composition, coordination, and collaboration are meant to apply across all domains of qualitative ethnographic work. Our informants-cum-collaborators are just as likely today (probably more so) to be producing and distributing their own media about the cultural and social issues that plague them—and they may do so either directly, with the increasing spread of new technologies, or indirectly, through the kinds of cultural changes that this increasing production and circulation of media can have on social groups of all kinds. Strategies of making and controlling media are as diverse and as culturally complex as the content of the media themselves.

But a more troubling issue for anthropology is how anthropologists and their informants might learn new strategies for making stable and robust conceptual connections across radically different projects (not just across area expertise or discipline, but outside of the academy and into unfamiliar forms of scholarly and critical practice). As I mentioned at the outset, the discipline has historically

served as the mainstay of coordination of the making of conceptual connections. Whether it is the endless debates about the culture concept, the rich literatures in kinship, exchange, or political institutions—it is through disciplinary channels that an anthropologist working on heritage claims in Mexico (Breglia, this volume) might connect with a scholar working on expatriate Iranians in Washington, D.C. (Naficy, this volume), or South Korean venture capitalists (Chung, this volume). However, in a world populated by new objects (especially technical objects), new events, and new forms of organization, anthropologists are far more likely to find the kind of conceptual *simpatico* in surprising places—especially with other disciplines, often with informants themselves. In addition, the demand for inter- and trans-disciplinary research is high enough (i.e., our funding is increasingly contingent on it) that a new body of scholarship is in fact emerging, for better or worse, that is not structured through disciplinary channels. Are the classic channels of anthropology sufficient in these cases? Is there something more?

My example of the Linux kernel and of the stratagem of “adaptability” over against structured design is one kind of answer to this question: there are possibilities that have emerged which cannot fit into the disciplinary framework, and we do not know how to take advantage of them. Certainly there is nothing radical about the two experiments I have conducted—they remain projects that respond, in part, to certain kinds of disciplinary constraints, both at a substantive level (i.e., the “culture” of scientists and engineers) and at the level of practice (i.e., interviews and participant observation). But they are propositions about the possibility of a new form of shared and adaptable research method. A shared conceptual vocabulary, distinct from that entrenched in the journals and texts of anthropology, is certainly easier than ever to work on in common with others, even in public; likewise, the sharing of “data” (the material gathered at the inquiry stage) no longer need remain obscure, but can also be made public (or semi-public among a group of scholars) and worked on in common. Such practices might in turn become the discipline of anthropology (again), but to date they remain experimental. We don’t yet know how these changes should be formulated and adopted, and thus modesty in proposing transformation is to be admired.

Perhaps the most important implication of these changes concerns the question of exclusivity. Anthropologists are by professional disposition interested in remaining anthropologists rather than joining in and becoming part of their field. Other social scientists show less compunction: political scientists work for campaigns and for foreign policy institutes; economists become civil servants and chairmen of the board. Anthropologists, especially those of the “critical” stripe, are far less comfortable joining in—or if they are it is, as Marcus points

out, because they increasingly come to graduate school from an experience with NGOs, activist organizations, or other groups. The perceived virtue of this resistance is “critical distance”—but such a claim all too easily papers over the realities of contemporary fieldwork. In fact, it is an important reason to critically distinguish coordination from collaboration.

Collaboration is too weak a word to describe the entanglements that are by now thoroughly commonplace in cultural anthropology: entanglements of complicity, responsibility, mutual orientation, suspicion and paranoia, commitment and intimate involvement, credit and authority, and the production of reliable knowledge for partially articulated goals set by organizations, institutions, universities, corporations, and governments. Collaboration is perhaps too feel-good, too friendly a notion for the commitments, fights, and compromises that anthropologists frequently make in order to pursue some kind of conceptual innovation. And this is to say nothing of the problematic insertion of “ethics” and “institutional review boards” into the game.

So, for instance, in the ethics and politics experiments described here, scientists and engineers occupy a historically more powerful position in institutions, public life, and policy than anthropologists do. A common reaction to this situation (especially pronounced in some strands of STS) is to adopt the position of the critical analyst who unmasks the hidden structure of belief or interest operative in the activities of science and scientists. But such an approach is also inherently adversarial and blocks access to scientific and technical practices that might already be critical vis-à-vis some other more or less powerful constellation of interests. In our projects, therefore, we have explicitly sought out a different form of engagement: a search for what we refer to as “immanent critique” in the language and practices of scientists. Practically this means developing a process of dialogue, reinterpretation, and commentary that would render explicit the critical practices of scientists and engineers, in areas that might also interest social scientists (the stealth transformation of democratic practice by electronic voting machines; the politics of research on quasi-legal peer-to-peer technologies; and the production of critical research on the environmental and health effects of nanotechnology). While it may well be possible to perform critical, deconstructive, or oppositional readings of the informants we deal with, our attention has instead focused on finding those voices where they appear in the field first and trying to amplify them, transcode them, or simply explain them in our own work.

Naturally, there exist divergent audiences for such an endeavor: at least one is that of anthropology and science studies, which would seek to learn how such critiques are relevant to conceptual and theoretical problems “at home”; but at least one other is the scientists and their constituencies themselves, for whom

our (“social science”) research is increasingly a highly sought-after commodity—filling a much lauded, but ill-understood need for “social, ethical, and cultural” aspects/impacts of science and technology (indeed—just to emphasize the buttered side of our bread—were it not for this need, none of the research presented here would ever have been funded). Composition, therefore, confronts new challenges that extend (if not alter) the familiar notions of style, audience, idiom, jargon, and readability. Our primary audience will not always be anthropologists or social theorists, but it is social theorists or anthropologists who will always determine the value of, need for, fundability of, and quality of our work. Experimenting with commentary and composition is therefore a way to rethink the kinds of texts and new objects we produce so that they might fill both needs and respond to the reality of trying to do so (e.g., articles and books, documents and files, databases, software, content and version management, vocabularies and taxonomies, archives and search engines, and so on). It is a challenge to create new kinds of objects (not digital vs. analog, but new compositions of both) that are intelligible and that assist in conceptual innovation within anthropology, science studies, and the interpretive social sciences generally, and may even respond to the multiple, partial, incompatible demands we find in the field.