

Caught in Collaboration

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The Field in Pieces

“Lecherous thoughts, marvelous sunset, walk around the island,” amazon.com tells us are among the “statistically improbable phrases”—not keywords but the “most distinctive phrases in the text of books” identified by its Search Inside!™ program—by which to tag Malinowski’s posthumously published *Diary in the Strict Sense of the Term*. Indeed, these seemingly unlikely tags are hardly off the mark: throughout the *Diary*, the lore and allure of in situ fieldwork dissipate variously into a sense of meandering boredom, agitation, irritation, preoccupation with other fascinations, work, the need for escape. The “statistically improbable,” it quickly becomes evident, is not even so much distinctive as quotidian. The following passage provides a taste:

Then lunch with a fellow and talk—about what?—In the afternoon: I lay down for a quarter of an hour, and started work—*bwaga’u* business. At about 5 stopped, was fed up. Excited, impossible to concentrate. Ate pineapple, drank tea, wrote E.R.M., took a walk; intensive gymnastics. Gymnastics should be a time of concentration and solitude; something that gives me an opportunity to escape from the [blacks] and my own agitation. Supper with a fellow who told me stupid anecdotes, not interesting at all. (Malinowski [1967] 1989, 188)

Such narratives about the field and fieldwork (and Malinowski’s is not by any means the only example), render even the most mythic conceptualizations of ethnographic fieldwork incoherent—not without meaning, that is, but disjointed, comprised of parts that must be painstakingly identified, sifted through, and organized to become ana-

lytically intelligible. With “villages” and “communities” now either dissipated by their own transformations within nation-states or rejected as questionable projections of wholeness by anthropologists, it seems less and less possible to speak of the field in a single breath, as a single place to which the lone anthropologist travels to undergo his or her rites of professional passage. So this essay is concerned with the “field” as an almost random assemblage of sites that come into coherence through the processes of fieldwork itself: the field as deterritorialized and reterritorialized, as it were, by the questions brought to bear on it in the course of research. This process necessarily entails much movement, as much between physical locations closer or farther apart as between ideological positionings or frames of reference (as I call them). Tracking this movement and understanding the relationships between sites, one’s own positioning within each, and the demands placed on the ethnographer coming-into-being—these, I believe are the means by which the field is made, quite alongside the objects of study that it yields then to ethnographic attention.

The narrative of Malinowski’s *Diary* suggests that this condition of the ethnographic field is nothing new—and yet, as I am about to suggest, the character of the deterritorialized field is in transition, and the demands it consequently makes of ethnography have fresh implications for how anthropologists can move about in it. To explain this further in the sections that follow, I use the example of my most recent ethnographic work on a National Institutes of Health/National Human Genome Research Institute (NIH/NHGRI)–sponsored “community consultation” project to research “Indian and Hindu perspectives on genetic variation research” in Houston (henceforth the ELSI-HapMap project, ELSI being the acronym for Ethical, Legal, and Social Issues in genetics). My role in ELSI-HapMap was largely prewritten—with some flexibility and room for innovation, but still largely given, and drawing my expertise as Indianist specifically into the frame of a bioethics initiative. Not only because this is urban anthropology par excellence and Houston is a vast, sprawling metropolis with a large and diverse Indian community, then, but even more as a result of the framing of this research as interdisciplinary and collaborative, the field seemed a bewildering assortment of pieces: a bric-a-brac assortment of people, institutions, interests, ideologies, skepticisms, locations, and expertise all forced into specific modes of contact with one another. And the

field demanded piecing together via various prescriptive collaborative means, all toward an end—“understanding Indian and Hindu perspectives on genetic variation research”—that was at once crystal clear and frustratingly unfathomable.

The narrative follows the evolution of this project with an eye to marking the disjointed character of the field and fieldwork that then calls for particular kinds of professional collaboration: indeed, gives older models of “community studies” a new lease on life and pressures particular kinds of “classic” ethnographic praxis into being. Collaboration in this context is both enabling and limiting, I suggest, but is nevertheless the overriding means by which a heavily deterritorialized and disjointed field is paradoxically given a (rhizomic) coherence of a kind, and new objects of ethnographic study acquire definition. Ethnographic “method” comes into being somewhere in the interstices of such a field in pieces, a product of interlocking expectations generated of and by anthropology. It is the emergence of such method alongside its ethnographic objects out of a bric-a-brac terrain jointed by the mechanisms of collaboration that this essay will explore.

The Inherited Field

The charge of the ELSI-HapMap study in Houston, as I have often described it to Indians in Houston, is fairly straightforward. The NIH would like to collect blood samples from 140 Indian Gujaratis to add data (and presumably depth) to an already existing haplotype map, which is a strategized cataloging of human genetic variation.¹ The original “HapMap” was compiled from four sets of samples collected internationally and named thus to indicate their sources: Yoruba in Ibadan, Nigeria; Japanese in Tokyo; Han Chinese in Beijing; and CEPH (Utah residents with ancestry in northern and western Europe). To these originary four were to be added a longer list that included samples from African Americans in Oklahoma; the Luhya in Webuye, Kenya; communities of Mexican origin in Los Angeles; metropolitan Chinese in Denver, Colorado; Tuscans in Sesto Fiorentino, Italy; and lastly, Gujarati Indians in Houston. The relationship between the different data-gathering phases of the HapMap project has never been fully worked out; in fact, the project was announced to be complete in October 2005, making the data that we were at the time yet to collect

from Indian Gujaratis a supplement, at best, to the completed HapMap. In any event, the Houston project has benefited from being last in line for having the flexibility to precede any request for blood samples with a full two years of ethnographic inquiry—much longer than the time allowed any of the other constituent projects, as the urgency to collect samples had almost entirely lifted.

But what was this period of ethnographic inquiry supposed to achieve? This being a “community consultation” initiative, the objectives were, all things considered, clear. We were to consult the Indian community in Houston over the question of their participation in HapMap, tracking everything from their understanding of genetics to their interests in genetic and biomedical research, from their decision to participate (or not) to the creation of “culturally appropriate” informed consent documents, and the nature of the returns they might expect for having contributed vials of blood. The reasons the NIH/NHGRI would support such extensive community consultation is, of course, a dramatic narrative in its own right that deserves more attention than I can afford here, though I will touch on the relevant themes a little later in this essay. Essentially, community review is a means to better anticipate and therefore circumvent the sorts of controversies that have dogged other blood sample-collection initiatives, the most notorious of which was the Human Genome Diversity Project (HDGP). In some ways a direct heir to this contentious past, the HapMap project frames itself as an ELSI initiative, meant to focus first on the Ethical, Legal and Social Issues raised by research in genomics, meant not to presume the unilateral support of communities, and meant to move forward with sample collection only with some clear sense of community consent—though the question of how “consent” is identified, negotiated, and documented remains still an open question (for example, see Foster, Eisenbraun, and Carter, 1997; Juengst, 2000; Sharp and Foster, 2000). As such, the project has a history that is quite independent of anything specifically Indian—but has always an air of possible controversy, the sense that things could go terribly wrong if procedures are not followed or the right questions not asked. The task at hand, under the watchful eye of institutional review boards (IRBs), colleagues, and collaborators alike, is nothing short of risk assessment and potentially also that of firefighting.

Here enters something called “Culture” with a capital “C”: Chinese

culture, Mexican culture, Masaai culture, Indian culture, the Culture of a definite People. I should note that not all the HapMap community review projects included anthropologists, and not all placed particular emphasis on the collection of ethnographic data, but there was a definite “Culture” with which to contend, nonetheless. This concept was both intimately familiar and strikingly alien to me. I knew and recognized it, of course, as the presumed object of ethnographic research: variously a source of fascination, a demarcation of difference, a cordoned-off territory, an object to revere, contemplate, catalog, or champion. But here also was Culture operationalized by ELSI, rendered into a useable something that is then amenable to deployment as a tool of procedural ethics in genetic research (Kelty 2004). And I, it would appear, had been recruited not as an independent observer but a facilitator of the process that was to investigate culture and render it a workhorse for bioethics, with the beneficiaries being (my) community on the one hand and researchers on the other. I will have more to say about my positioning between groups and their respective interests later on. For now I want only to note that the ELSI-HapMap project gave me a role in the operationalization of culture, as an interpreter-translator of Indian ideas into useable material for ethics that could, say, filter into informed consent documents so as to “appropriately” inform, or, hurt no cultural sentiments in the research process. My assigned role was that of cultural broker in a transactional chain that led from something called “community” to something called “genetics.”² Certainly, this was unlike any role I had ever been handed before as I was being called upon to put culture (and my cultural expertise) to “good” use and, as I would quickly discover, also harness my position as an area academic for the betterment of my community. The fact that I am Indian was not incidental to either role. Quite the contrary, it had a highly specific use-value, a (presumed) closeness to both “culture” and “community,” with the very position of the native anthropologist taken to set the objects of classic ethnography into motion—this time not merely for the sake of abstract ethnographic understanding but rather in the specific interests of ELSI.

In short, the inherited field turned me, and everyone else in our research group, into cultural producers of the very kind whose work I might otherwise have been interested in scrutinizing.³ For how else might we have completed the task of generating interest in genetic re-

search (and our study in particular), perhaps producing educational materials or just generally keeping the community informed about the project? How would we, in all good conscience, staff booths at community events and recruit volunteers for interviews and focus groups without communicating some sense of the worthiness of our efforts? The choice was between being invested and being disingenuous, and neither a comfortable position, for if one allied me wholly with ELSI research or pitted me straightforwardly against it, the other entirely masked my professional affiliation and commitments. Perhaps being overly sensitive to the comments of colleagues who disparagingly named HapMap the unfortunate successor of the HGDP, I was also acutely affected by the atmosphere of deep skepticism that prevailed among many (although not all) scholars engaged in the social/ethnographic study of science—for whom any truly ethical position vis-à-vis genomics would have to be a skeptical, critical, and distanced one. Kelty (forthcoming) notes a certain anthropological predilection for exclusivity, the avowed professional interest “in remaining anthropologists rather than joining in and becoming part of their field”—whatever that wider field may be—for the sake of maintaining “critical distance.” Indeed, I have occasionally been struck at the relief with which anthropologists sometimes approach one another at consortium gatherings or other interdisciplinary meetings as though discovering long-lost comrades in the embattled fields of expertise, the given designation “anthropologist” apparently obviating all intradisciplinary differences in approach and training. Such resistances have considerable impact on molding the possibilities and limitations of collaboration, by always already preframing any hopes of “friendship with the sciences” with skepticism about science’s lack of care, or only incidental care, for the objects of ethnographic concern (Fortun and Fortun 2005, 50–51).

Indeed, our own wariness as researchers involved (and maybe therefore implicated) in the ELSI-HapMap study cannot be underestimated. Ours was a project steeped in what Mike Fortun has called an “ethics of suspicion,” an anticipation of everything going awry especially when genetics are involved (Fortun 2005). This was a formulation we in the research group quite embraced. The sampling phase of the research would not proceed without active community involvement and support, we insisted, to ourselves as much as anyone else, from the very inception of our study. Our annual NIH/NHGRI workshop presentations on

the issues inherent to community consultation research were geared toward documenting what concerns and risks we *didn't* find among Indians in Houston. We even dubbed what we *did* find—which was a broadly speaking very healthy regard for knowledge, the very validation of “pure,” unoperationalized knowledge about culture that we may otherwise have celebrated—“anticlimactic,” as my research assistant Corrie Manigold once noted. From where we stood, there were significant risks involved in taking on more-or-less close, rather than obviously distanced, roles within the ELSI-HapMap project. The field we found and ourselves unfolded was thus heavily polarized, overdetermined by critique in which ELSI research represented just what James Watson cynically imagined: a move “to preempt the critics” (cited in Weiner 2000, 169). Our collaborative positionings were laden with ironies.

So there are two meanings of the word “caught” from the title of this essay that are both relevant to understanding the field and its formation: the sense of being embedded in a nexus of relationships that each make their own demands, but also *found out*, caught red-handed with hands in cookie jars as it were, taking the multiple demands seriously, allowing ethnography to become a tool in the hands of other interests that are properly the objects of critique, and then needing to explain the undoing and redoing of ethnography and one's complicities within these schemes. The point is not so much that ethnography can be professionally risky business, but that the terrains in which it is produced and in which it acquires meaning are many and can hardly be avoided, and that an accounting of the demands of each of these is what pressures research into existence. The inherited field positioned me, with not much apparent wiggle room, on the one hand, in a research community comprising bioethicists, physical anthropologists, population geneticists, legal historians, and sociologists, and, on the other, amidst a “community” comprising various and sundry religio-cultural organizations, and, of course, the people running these organizations from diverse personal and professional backgrounds. Finding myself the point of articulation between these two loosely defined “groups,” much more so than my research colleagues for being both Indian and Indianist, the sense of divergent commitments to divergent sets of interests (on both sides) made the work consuming, both personally and professionally. I longed frequently for Malinowski's pineapple, tea, gymnastics, and other such modes of escape.

Collaboration as Method

This distinct sense of discomfort generated by a particular kind of positioning within the field alerted me to what I have come to see as a characteristic of the field itself: its disjointed nature, its sometimes overwhelming sense of being a collection of segments—places and positions and commitments—disconnected and therefore in need of ethnographic grounding. I had encountered such a field before in a prior project that traced expressions of Hindu political identity from middle-class Hindu homes to political party offices and media debates, ethnicist rhetoric becoming a resource for new critique and refashioned feminist identity in the spaces of women's groups and broadly Left politics in India (cf. Reddy 2006). No position, in no place, was entirely comfortable for me as (Indian, Hindu, middle-class, upper-caste, even NRI, or nonresident Indian) anthropologist; being in any one involved a level of complicity, with all the ethical heaviness that complicity inherently implies.⁴

In the ELSI-HapMap work, too, there are a series of very distinct sites that come into dialogue with one another. By this time in the history of genetic research, however, heated criticisms of sample collection had given way to a set of procedures that replace the charges and dilemmas of complicity with a methodology called “collaboration” that shapes fieldwork. Let me clarify. Collaboration here is not so much the possibility of working jointly with informants and interlocutors (cf. Lassiter 2005a, 2005b), although that is never precluded, but is at once a recognition of disciplinary distinctiveness, specializations, and expert cultures, a form of professionalism, and a method by which to tackle matters that straddle the boundaries of science, ethics, culture, legality, religion, and more.⁵ Collaboration did not appear in our grant application as an explicitly rationalized research strategy, nor was it a recognized “method” of ethnographic research. By the time of our research, however, it was a presumed strategy of turning disciplinary “pluralism into a strategic resource” (Fortun and Cherkasky 1998, 146): of hitching different forms of expertise to a single carriage and therefore training each, in more or less significant ways, to make sure the carriage does ultimately get pulled. Differences matter here, far more than commonalities, although they must somehow be aligned to become meaningful means to larger, predetermined ends. The identity of fieldworker as contrarian, which had been so useful in situating myself in the fraught

fields of Indian feminist activism and religious politics, was unnecessary here, even counterproductive. What mattered far more now was disciplinary particularity, the valuation and, indeed, valorization of disciplinary difference.

The unease of recognizing, valuing, and committing to labor across disciplinary difference (all of which Fortun and Cherkasky see as inherent to collaboration) has at least one important methodological outcome that I want to highlight here. By identifying anthropology as the key to “Culture,” and thus as the discipline to bridge the social and the biological sciences via the conduits of “ethics” (cf. Marcus 2002a), collaborations between the biological and “human” sciences also generate “anthropology” to the extent that they generate certain specific expectations of what anthropologists exclusively do. For one, as I have said above, anthropology is charged with operationalizing knowledge about culture. But how is it that anthropologists are to gather their data so as to preserve the uniqueness of their contributions? Not so much through interviews, which are common to other disciplines, not so much through other forms of face-to-face contact with people, but primarily through something called “participant observation.” The method seems to require both much justification and no explanation at all: how quantifiable data will be extracted from “hanging out” naturalistic observation, how bullet-point ends will be met by such meandering means, how ethics will be handled in the absence of formal consent documentation certainly bears spelling out. The method itself, however, as somehow self-evidently a form of “cultural immersion” seems to require little further rationalization. In this, “participant observation” becomes virtually synonymous with “fieldwork” in anthropology, a pithy, catchall phrase to capture the uniqueness of ethnographic method and, indeed, of anthropology itself. It also becomes (oddly) a sort of disciplinary defense, a sign of the value and the esoteric impermeability of the discipline that protects the anthropologist’s place at the collaborative, not to mention financially lucrative, tables of ELSI research. It becomes the reason for the collaboration and simultaneously the means by which ethnography produces value for ethics in science. If collaboration as methodology generates some of the connective tissue between the diverse disciplines brought into dialogue through ELSI research, then it does so also by generating select tropes that come to stand in for what ethnography concretely is.

Here I should add that few other community consultation projects made such room for “participant observation” in their research protocols. Indeed, other anthropologists I met at NIH/NHGRI-sponsored workshops on community consultation remarked that we were lucky to have had even such support for ethnographic method in our collaborative exercise. In most other projects, focus groups and interviews were the primary modes of data collection and were more-or-less strictly cordoned off from distinctively ethnographic modes of investigation. Qualitative research, broadly conceived, stood in for ethnographic fieldwork, involving ethnographers simply as skilled practitioners of qualitative research techniques. Even focus groups, which represent a methodology employed in market research far more than in classic ethnography, become ethnographic by association with the qualitative. In the end, however, the result was the same: anthropology and its practitioners were transformed in accordance with the needs of ELSI research and its constituent collaborations.

The object of research, it bears restating, is given, and clearly so: understand community perspectives on genetics (for the sake of capturing risks and needs), develop “culturally appropriate” recruitment strategies, develop “culturally appropriate” consent documents, and—almost as an afterthought although a hugely crucial one—collect 140 blood samples from Indian Gujaratis. With an apparently definite “Indian and Hindu” Culture in play, the outcomes of this research for the NIH/NHGRI are anything but elusive. Anthropology in this context is an instrument, a stepping stone, the means by which to mobilize Culture for the sake of (bio)ethics and then in the interests of Science. And, precisely as a means to some other nonanthropological end, the means of ethnography, which are its in situ methods with their qualitative emphasis, are far more important than its modes of, say, analysis, which also could be said to give the discipline its unique stamp. The collective laboring of collaboration at this stage of research is not primarily to establish the NIH/NHGRI’s goals, which other collaborations (such as that of the HapMap Consortium) have already established. The collective labor of collaboration at this stage is primarily to establish the means by which these goals are best achieved, and to implement these, with sometimes less, sometimes more emphasis on ethnography itself. Anthropology is reduced, as a result, to its classic fascination with “culture” as object, and to its classic method in the form of “par-

ticipant observation,” its qualitative emphasis. It is not just that anthropology is transformed in accordance with ELSI research, then, but that it is (largely) expected to perform either a prior or distilled version of itself.

It is not a coincidence that such oddly classic formations as “culture” and “participant observation” become the emblematic of a fetishized ethnography precisely as “culture” (as object of study) and field methods have been filtering through successive decades of disciplinary transformations. Nor is it a coincidence, I think, that “culture,” as a definite association with a people, comes to be methodologically mobilized within the HapMap project alongside the category “populations,” which the International HapMap Consortium recognizes as scientifically valuable but culturally quite imprecise (2004, 469, box 2). This is not to critique HapMap as much as it is to recognize that outmoded ideas of ethnography, “the way we *don’t* do things any more,” exist as a normative, even prescriptive, means to rein in other objects (like genetics) that are obviously and simultaneously in motion. “Collaboration” as the *modus operandi* of research done in such a framework necessarily generates stultified expectations of ethnographic praxis and makes anthropology instrumental to the large promises of “world health” while simultaneously yielding itself to new ethnographic inquiry (International HapMap Consortium 2004, 474). “Collaboration,” too, like culture, is itself everywhere these days, itself a bit “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

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. (Kelty, forthcoming)

And yet it is precisely the friendly feel-good quality of the term that renders collaboration a stable methodological tool to “break up a problem into identifiable, exclusive chunks” that could, but don’t necessarily, pave the way for “conceptual and theoretical work” (Kelty, forthcoming)

ing). Whether any conceptual and theoretical work in fact ensues, or whether collaboration is limited to what Keltly identifies as simply “coordination” is of course a separate question. In thicker or thinner form, collaboration becomes the tidily synchronized way in which we do now do things, at least some of the time, its backside of messy and multiple entanglements becoming fresh material for independent ethnographic reflection (an example of which is of course the present essay). The conceptual and methodological tools used in such collaborations may be worn, outmoded, or weak for strictly ethnographic purposes, in other words, but their utility is not lost in cross-disciplinary exercises like ELSI-HapMap. Rather, projects like ELSI-HapMap themselves serve then to bring new ethnographic objects into view: both I and my colleague Jennifer Hamilton (who was the project’s study manager for all but its final year) were independently advised at the outset of the study simply to “wait and watch the project unfold.” In other words, our involvement in the study was valuable just for the ways in which it positioned us as witnesses to science, bioethics, and indeed even culture at work. The operationalization of a fairly outmoded ethnography pointed, quite ironically, into fresh ethnographic terrain.

But what exactly is my work beyond the operationalization of ethnographic knowledge? What does it mean to make HapMap an object of study, encrusted and contained as it is by worn-out notions of ethnography? The grants I wrote as a doctoral student seeking funding for dissertation research compelled me to articulate responses to such questions at the outset. All the pages submitted for NIH review, by contrast, defined NIH goals and made those mine for four years, but configuring any ethnographic goals beyond was a separate, independent task. As clear as my objectives were, as clearly defined as my methods, my own tasks were hazy and ill-defined. Recognizing this fact in advance, the associate dean at my university asked me once if the HapMap research was really to be considered “research” for my own purposes: would it yield publications? I rationalized, as advised, that it was all about positioning, buying myself access to a field that would invariably yield at least some research products. But, again, what was this field? And what did I plan to study in it?

Before I address these questions, however, I need first to tackle my given object of study, the “community,” and a second mode of collaboration that also brings the disjointed field and its objects into better view.

“Community Consultation” as Collaboration

As with “culture,” here again in “community” was a concept both intimately familiar and overwhelmingly alien: on the one hand, an object with presumed coherence, a focus of ethnographic expertise, and on the other, a group as diffuse and disparate as the city of Houston itself, a “beguiling linguistic fiction” with vague and elusive referents (Comaroff 2005, 127). Our relationship as a group of researchers to this “community” was given in the form of a methodology known as “community consultation.” The community was to be engaged or consulted over issues related to genetics in order to discern risks, possible harms, and expectations of returns in advance of sample collection. We were to investigate the following: (1) so as to establish a “community advisory group” or some equivalent body to serve as liaison between the community and scientific bodies: Who speaks for the Indian community? Where are the sites of authority? How are authority and voice ordered? And (2) so as to assess interest and risk, and manage both: What were the expectations of researchers and the entire process of sample collection? “Community,” presumably already intelligible in all its depth to anthropologists, especially native ones, was now to be made navigable for the sake of ethics of ELSI.

“Community consultation,” to offer only a cursory summary here, gains force and spurs further debate as a consequence of pressing demands for group or collective recognition, sovereignty and identity, which “transform the context and substance of population genetics research” and in this “help define what the ‘principled conduct of research’ might mean in practice” (Brodwin 2005, 148). Community review has really only one foundational premise: that communities have a crucial stake in their futures and in their representations and therefore need to be at least aware of and ideally involved in research that involves them. This definitive premise emerges largely as a result of prior encounters between researchers and community but is consolidated in the furor over the Human Genome Diversity Project, an international initiative to collect blood samples from select indigenous communities to anchor understandings of human evolution. Groups targeted for sampling sharply criticized the project for its biopiracy and biocolonialism, further demanding a role in defining research agendas, interpreting the facts, and acquiring the right to the (monetary) benefits of research itself. The overwhelming response to the HGDP as a global-

ized iteration of the politics of recognition rendered human population genetics forever “politically vulnerable,” as Paul Brodwin has remarked (2005, 148, 169), from that point on. The premise of community review, I wish to highlight here, is a particular response to this deepening sense of political vulnerability.

Community review—in other words, a prior notion of *community* and the premise for *review*—comes into our ELSI project in Houston, then, as a prepackaged, preemptive move in anticipation of political assault. In this, it defines the nature of my contact with Indians in Houston and guides the sorts of questions to be addressed with them. Its mode is wholly representational, which is to say that participants must on some level identify with the principle of representation: the fact that some groups or cultures and some (genetic) populations and some views are underrepresented, and that efforts such as ours are meant to address these historic imbalances. So also does community review very nearly expect the communities in question to see themselves as politically vulnerable.⁶ Ethics then can help negotiate the vulnerability of researchers, on the one hand, and the vulnerability of communities, on the other, by bringing these into dialog: you tell me where your rights begin, so that I can determine where my nose ends, to reverse the popular dictum. Not only is this model of ethics culturally and historically specific, it is also procedurally overdetermined. Virtually all our decisions as researchers were subject to IRB scrutiny. Confidentiality and consent needed to be explained over and over. Paperwork needed signing. Documentation of all sorts, from meeting minutes to mileage to assiduous quantification of “participant observation,” needed generating. Decisions about from whom to collect samples needed to be made—Indians resident in Houston, who had had opportunities to participate in the consultation, or Indians visiting Houston for regional Gujarati congresses with no prior knowledge of the community engagement phase of our work? Did the distinction matter? The infrastructures of ethics, precipitated in community review which was itself premised on a sense of political vulnerability, were the obvious and not-so-obvious guides to just about everything we could do.

The individuals with whom I met and interacted generally understood little of this background, or paid it only passing heed. Only those who had themselves been professionally involved in addressing health or other disparities among Houston’s diverse racial and ethnic groups

identified readily with the premises of our work. Most others variously ignored the consent documents, dismissed them as “legal mumbo-jumbo,” or simply saw them simply as the constraints under which we (researchers) had to operate. The questions we were asking seemed valuable but rather irrelevant to “the community”—a fact which was borne out by the reluctance of some institutions to lend our efforts time and support: “it’s not that genetics isn’t important,” I was sometimes told or shown in so many polite ways, “it’s just that we are not doctors. Our priorities are different” (cf. Reddy n.d.).⁷

On the other hand, there was also a model of “collaboration” actively advanced by many with whom I spoke. Less theorized than “community consultation,” to be sure, there was nonetheless a discrete set of expectations that derived from a recognition of my position within the ELSI-HapMap group, that were therefore brought to bear on me and my work: sing at the temple, help organize health fair, help organize community events, attend said events (held with relentless regularity in Houston), lend support to various and sundry cultural initiatives, become a “torchbearer” for the establishment of an India Studies program at the University of Houston (I was sent a poster mockup that named me as one such), and more along similar lines. This was not collaboration as professionalism or interdisciplinary harnessing of differences, but collaboration as volunteerism, personal favor, and personal commitment to something still abstractly assembled as “the community.” Here, however, the concept was less anthropologically inflected, a much more straightforward reference to “Indians” as a diasporic group within the United States. And the pressing need for this community in diaspora was not so much one for representation, especially at a moment when Indian institutions, organizations, and activities are all but commonplace, Indian Americans are increasingly visible in local, state, and even national politics, and most measures of advancement indicate superlative progress. Certainly, there was almost none of the political, or for that matter cultural, vulnerability on which “community review” so centrally depends for its rationale. Rather, the pressing need—the sense of vulnerability that impels action, as it were—was much more for links to be maintained with Indian cultural traditions, all the more as newer generations are born and grow up outside India, and for “Indian culture” (that construct again) to be somehow merged with mainstream American life.⁸ Indeed, my own participation in the

HapMap study gained meaning and value precisely because it fit this mold: something done for the betterment and development of “the community.” And already so positioned, it followed logically that I should then contribute ever more for the sake of that abstract goal.

The expectation of service (for the community), I have argued elsewhere, itself rationalized community participation in our study, whether in its ethnographic or sample-collection phases. Giving time or blood was tantamount to serving some greater community good, in other words (cf. Reddy 2007). The fact that this expectation extended to me was logical, of course, and its pressing nature made me acutely aware of the distinctions between the overlapping models of collaboration in play here: one a “paid mandate” (that would after four years be neither paid nor a mandate), the other wholly voluntary, based on ethical commitments to communal ideals; one rooted in a history of research violations and controversies, the other moved by the imperatives of diaspora; one invested in the reformulated and procedural “Ethics” of ELSI, the other invested in ethical commitments quite incognizant of ELSI. Both sets of collaborations were deeply invested in such concepts as “culture” and “community” but for purposes that were quite at odds with one another. Not only was I made aware of the two divergent models of collaboration, I was also pressed closely between them: asked to give of my time and my energies beyond the demands of professional and paid obligation, and asked for a commitment to things Indian in a way that the ELSI project was just not framed to incorporate. My community—mine because I did (and do still) identify with it and because it claimed me, in turn—disregarded entirely the “30 percent” quantification of my annual participation on our grant. Instead, I was reminded that most people who really contributed to community development did so after they were done working ten-hour days. And, having been hired on the ELSI-HapMap project as an Indianist, precisely for my close ties to the Indian community, and being the only Indian in our research group, suffice it to say that I felt obligated to build and maintain the very ties that I was presumed to have. I felt crushed between two sets of expectations, two registers that each claimed culture and community in divergent ways, and above all by the demand that anthropology as discipline would have the ready-made means to link these meaningfully.

What did it mean to return the benefits of genetic research to the Indian community in Houston? For us, as researchers, the moral im-

perative to “give back” flowed logically from our own responses to the fallout of the HGDP and from such wider movements as resulted in the Declaration of Belém, which effectively instituted the principle of redistributive justice as central to any kind of ethical prospecting research.⁹ So, it was obvious that something needed to be returned for the favors of time and blood, but what? We were not collecting samples for commercial use, so there was no question of royalties flowing back, even if geneticists down the line might have generated royalties. In any event, Indians in Houston are not in need of community development projects or communal toilets or schools, but instead frame their priorities in terms of needing to foster ties to “Indian culture” and to bring this into the American mainstream. What was needed was not the return of monetary benefits, but the return of work, effort, time, and above all identification with the community’s notions of culture: presentations on genetics at area cultural institutions, presentations articulating Gandhian principles with medical practice at universities or other sites of research. As an anthropologist and as an Indian, I saw it as necessary not merely to document but also to incorporate these priorities and the conceptions informing them into our research. As an Indianist with my own proclivities, I found it difficult to live up to such expectations, especially when they demanded an overwhelming focus on Gandhi (at a time when stringent critiques from some quarters have perhaps bolstered allegiance to Gandhian precepts in others). As a paid researcher with the ELSI-HapMap group, I recognized the impossibility of asking colleagues for participation beyond the time allotments given in our research protocol, or beyond the mandated three years of our grant’s term. The expectations of community far exceeded the capacities and parameters delimited by our study.

The Double Bind of Genres

The deterritorialized field unraveled, then, into a series of expectations manifested in distinct sites; multiple commitments to multiple publics, each quite important, each quite inescapable. The parameters generated by the ELSI-HapMap project were those of a classic double bind—in which my commitments as an ethnographer to the community I was studying were implicated as much as my commitments as an Indian to the community to which I belonged. Even so, the means by which to

navigate the double bind were not straightforwardly to be found in the company of anthropologists. Why? Here, again, were two additional sets of expectations to consider.

The notion that “science is political” has become virtually axiomatic in social science discourse, an all but predictable conclusion that seems nonetheless to foster the “ethics of oppositional critique” of which Mike Fortun has written (2005, 161). The prevailing ethos of critique within the discipline had a profound bearing on the directions of my work, as I was soon to discover. The orientation of HapMap being in some ways undeniably political, given its heritage in controversies like the HGDP, how could I ignore this or even set it aside? No matter how apolitical the Indian community’s own positions on the issues of ethics in genetics were? What was the nature of the choice I was making, and what was its rationale? Such were the questions posed (not unkindly) to me at a panel presentation made at the Society for Cultural Anthropology meetings, for my paper had been written in the voice of an Indianist. This was one additional set of expectations brought to bear on my work by none other than an audience of anthropologists. Here was, among other things, the atmosphere of deep skepticism about science and genomics that had, in ways, framed our project from the outset, and there was no circumventing it. By contrast, I wished for a useful means by which to take stock, say, of the “ressentiment” that not only marks much of science studies writing (Fortun 2005, 164), but also characterizes the responses of at least some geneticists to analyses generated via an increasingly privileged ELSI research. If critique was the overriding framework guiding analysis, then there seemed the need to admit its multiple manifestations, not to presume that critique is the exclusive purview of the social sciences. Was there a way to reasonably account for the frustrations of geneticists over, for instance, the centrality accorded “race” in science studies writing? How could one avoid presuming, implicitly or otherwise, that if the scientists designing genetics research were only “more humanistic, more ethical, more responsible, or had better values to begin with, we wouldn’t be faced with the ‘implications’ that justly preoccupy our attention” (Fortun 2005, 164)? What did it methodologically and conceptually mean to demand that geneticists be more humanistic, more ethical, and more responsible? Not unrelated, were the “well-meaning,” antiracist motivations of HapMap organizers and other scientists phenomena with which to reckon, as Jenny

Reardon has done in her writing on the HGDP (2004), but not merely as oddities or apparent contradictions. And, finally, I wished for a way to incorporate the so-called “emic” and the seemingly anticlimactic Indian apolitical affirmation of the inherent value of “science” into an analysis of ELSI-HapMap itself. Not taking such views into account would have been tantamount to suggesting, on the advice of fellow anthropologists, that the very ethnographic perspectives I had documented were naive and ill-informed for their apolitical orientation, at worst, or that they belonged in a separate “cultural” register, not amenable to integration with “mainstream” thought, at best. The choices delineated by the prevailing modes of critique within the discipline seemed untenable even as they defined the very framework of any possible research based on ELSI-HapMap.

As the lesser of two evils, I gravitated somewhat defensively toward retaining my commitments to “community,” India studies, and to my ascribed identity as Indianist. But the problem of segregation dogged me still. With the fields of science and technology studies and the social studies of science now well instituted as “areas” in their own rights, where did the older “area studies” models fit in such reformulated intellectual terrain? The “Indian and Hindu” perspectives that the ELSI-HapMap research sought would no doubt have made for an interesting addition to the annals of bioethics, but only as a segregated chapter with not much more value added than that. Retaining too closely the identity of an Indianist in the company of scholars of science seemed to run a similar risk: here, too, my work might be of interest to Indians and other Indianists, but beyond those audiences, it would be largely an interesting chapter on “culture” added to the annals of science studies, another model of giving to add to the existing mixes. So the challenge in this struggle over genre, it seemed to me, was not merely that of navigating the binds precipitated by ELSI-HapMap, but of learning to speak to the different audiences within the discipline of anthropology itself, as also to the distinct professional and intellectual compulsions these groups represented. Subject and object (or area) demanded integration, all the more since ELSI-HapMap tied both together within the framework of its expectations.

Arthur Kleinman has made a distinction between moral processes and ethical discourse, where the moral is a dimension of practical, localized engagements with specific social worlds and the ethical is

abstract, principle-based, a debate over codified values, the space of (bio)ethics itself (Kleinman 1998, 363–65). For the “moral” community represented by Indians in Houston, blood is largely and unproblematically a possession but a wholly alienable one, to be freely given for an easily identifiable “greater good” represented by genetic research. For a community of scientists, particularly bioethicists and those others allied with bioethics initiatives (including anthropologists), blood is an abstract but ethical problem, one which marks out a terrain fraught with anxiety, the perpetual threat of controversy, and all the attendant legal and institutional protections and precautions. Kleinman sets out then to “develop the case for *experience*,” arguing that “the concern with ethical discourse far predominates over an orientation to moral experience” and that his own professional positioning prepares him for such an approach (Kleinman 1998, 373). What I have tried to demonstrate in this narrative, however, is that it was harder by far in the context of the ELSI-HapMap research to make the same choice. It is a significant comment on the current predicament of anthropology as a discipline that we find ourselves allied with both sides on questions like that of blood sampling: as ethnographers who discern the contours of practical, localized engagements with specific social worlds, and as ethnographers working within the scaffoldings of established bioethics projects (such as the HapMap), who track abstract debates on codified values lifted from some local contexts and brought to bear on others. Even further, I mean to suggest that we find ourselves caught in between the “subject” and “area” pulls of the discipline particularly when called to be anthropologists in the prescriptive “ways we don’t do it any more.” Area specialty, of course, often remains crucial to ethnography for professional identification and on the job market besides. But for that it also runs the risk of becoming a niche identification in an age when any exclusively “area” approach to ethnography is not only dated but is also diffused by the predominance of the more topically driven, interdisciplinary approaches of cultural studies, science studies, and the like. The case for “moral experience” that Kleinman builds, which is centrally the case for immersed local, cultural engagements, is therefore both valued and marginalized within the discipline ironically in much the same way that “Indian and Hindu perspectives” are valued and marginalized in wider conversations about bioethics in genetic research. Shifting from one collaborative context to the next, ethnography has perforce to deal alternately with the shifting value of its objects of study.

The double bind of genres, the sense of being caught in between multiple and divergent sets of expectations precipitated both by specific projects and the social science study itself, each invaluable in its way, seems increasingly emblematic of the character of fieldwork, and seems increasingly to define the parameters for any analysis that can then logically follow. Something called “ethnography” is perpetually undone and redone, as a result, fetishized, protected, or freely reinvented. Ethnographic “value” is thus forever in the process of being translated, reconstituted, and recirculated; ethnography is made up as one moves through collaborations and the various sites that give it meaning. Not only does the ethnographer become a cultural producer in his or her own right, but ethnography, too, becomes both by-product and end-product of such endeavor.

Between Thick and Thin

The by-products of the ELSI-HapMap work (the press releases, the statistical data, the synopses of “community perspectives”) are of course end-products in their own right, and of a different order than the anthropological desired ends of such research in which I am—we all are—observers and the things being observed: participant observation with a vengeance, I dare say. The ELSI-HapMap project, it should be said, too, internally tolerates little methodological bricolage. This is an NIH-funded initiative that allows latitude but demands rigor of the kind that quite plainly produces data that is of PowerPoint clarity, intelligible to diverse audiences of medical practitioners and researchers. As rich as the ethnographic data generated, as useful as the analysis, it does not obviously speak to audiences other than social scientists. In that it works somewhat against the goals of the project and, I would argue in the spirit of collaboration, even the desired ethnographic outcomes. Snowballing subject recruitment techniques, or even the multi-sited method that follows a thing, a metaphor, a plot/story/allegory, a conflict (Marcus 1995) may be increasingly commonplace or quite unquestioned in the course of ethnographic field research. Such methodological strategies are, however, red flags for other audiences with more positivist leanings, to whom also anthropologists must speak with greater than anthropological authority. Failure to do so is not just the failure of research-mandated collaboration, but a foreclosure of any

hope of releasing “anthropology” from its given, prescriptive forms and of making it more broadly relatable to medical or basic science research. So, within the project, method is consistently vetted, means to particular ends specifically chosen and justified to IRBs, funders, physical anthropologists, medical anthropologists, and ourselves alike. Method simply cannot be made “out of a rhetoric of circumstance” (Marcus 2002b, 198)—not because ethnographers have such expanded freedoms to experiment but as a result of specifically devolved conditions of constraint.

Method is another story entirely, however, beyond the deadlines, mandates, and regulations of the ELSI-HapMap project. Here is ostensibly free terrain where predetermined methodologies are both scarce and sparse, not least because it is difficult to devise both questions and method to encase a project that already possesses both questions and method, and in whose execution one is primarily (and currently) involved. “In classic ethnography,” George Marcus has written:

thickness was a virtue, thinness was not; in multi-sited fieldwork, both thickness and thinness are variably expected, and accounting for the differences in quality and intensity of fieldwork material becomes one of the key and insight-producing functions of ethnographic analysis. This accounting for the variability of thickness and thinness of ethnography is the most substantive and important form of reflexivity in multi-sited projects. (2002b, 196)

Not just to account for variability, I venture to add, but (reflexive) mechanisms to track thick and thin, to track the transformations of thick into thin, are equally key to stitching together a field inherited in disciplinary pieces. Movement into, out of, and in between collaborations in multi-sited research is such that meaning is neither uniform nor stable. “Culture” described thickly as a “stratified hierarchy of meaningful structures in terms of which twitches, winks, fake winks, parodies, and rehearsals of parodies are produced, perceived, and interpreted” must be filtered, distilled, and reduced for HapMap into a thinner-by-far version of itself (Geertz 1973, 7). And yet, HapMap is thick with its own negotiation of “science” and “ethics” that begs documentation tailored to particular outcomes: defining new approaches to population genetics, or new ethnographic objects. Sites are not either thick or thin but produce meaning and demand coherence by configuring thick and thin to meet given ends.

What I am searching out here is a praxis that takes stock of the fact that field sites (and the ethnographers who encounter them) exist simultaneously in multiple forms: even as “the field” unravels into a series of nodes and pathways and signposts pointing both ways at once, it retains the coherence of collapsed bits and pieces—that often seem anything but logically whole to the interlocutors within. Sarah Strauss, reflecting on her work on yoga as a transnational phenomenon, observes that she certainly could have written a “traditional” ethnography, but in so doing “would also have failed completely to represent the Rishikesh which I experienced, knowable only within the context of movement and change” (1999, 189). She’s right of course, but one wonders if her comment would apply just as well for those who are ashramites or *sadhaks*, come to Rishikesh for other kinds of study, in search of a place that coheres and endures and whose enduring coherence is precisely tutelary. In my own work on caste, too, I’ve had to grapple with the fact that while ethnographic theory now can slash essentialisms with ease, rendering them unforgivably “thin,” these very discounted formulations of identity stabilized continue to be powerfully central to new formulations of Dalit political identity (Reddy 2005). The variation of thick and thin seems roughly coeval with such variations in the demand for coherence and meaning that remains stable, at least until the next movement into new disciplinary or conceptual or political space.

“But isn’t this just the distinction between the “etic” and “emic” of classic anthropological theory?” someone asked at my presentation at the Society for Cultural Anthropology meetings in 2006 (which Jennifer Hamilton and I collaborated to co-organize) in which I followed some threads of data as Indianist but in that left out any mention of the outer encasings of HapMap. I take the question as a prompt not so much to choose between etic and emic, or between “traditional ethnography” and ethnography “knowable only within the context of movement and change,” but as a prompt to bring these differentiated frames into conversation (as I then attempted in an essay on blood based on our engagement with HapMap, a necessarily individual undertaking [see Reddy 2007]). It is an awkward task, at best: how does one neatly draw together an apparently straightforward commitment to science and the value of knowledge with, for instance, the (rather too damningly) critical suggestion that Indians were selected as a HapMap population precisely because they were not likely to oppose its ends—to produce what

Jennifer Hamilton once called a HapMap of the “ethically compliant”?¹⁰ How would I, as a link between a community of researchers and a community of Indians, convey this unsettling possibility to my interlocutors or otherwise take it into account? Such questions notwithstanding, what method emerges from the overlapping contexts of collaboration must necessarily undertake the awkward task of stitching together the differentiated and opposed terrains of collaboration—or the terrains opposed (paradoxically) by the contexts of collaboration. What are the sites that demand coherence? What sort of coherence is it, and how is it enacted, “produced, perceived, and interpreted” (Geertz 1973, 7)? How do subject and object, topic and area interact, and what does their interaction yield to analysis?

The point is not just that a new ethnographic object comes into view and that that object is HapMap itself. The point is also that the diverse array of collaborations on which HapMap (and a good number of science studies projects like it) is built yields an equally diverse array of expectations and commitments, professional, personal and variously political in nature. Each of these needs to be negotiated; each of these demarcates a set of parameters which constrain, but also crucially define, the possibilities for ethnographic method and analysis. And each of these needs to be dialogically linked to track an elusive “culture” as it sometimes stands still, and sometimes refuses stable definition.

Recoding the Field

The metaphor of the rhizome, thus far buried, bears some unearthing in conclusion. “Every rhizome,” write Deleuze and Guattari

contains lines of segmentarity according to which it is stratified, territorialized, organized, signified, attributed, etc., as well as lines of deterritorialization down which it constantly flees. There is a rupture in the rhizome whenever segmentary lines explode into a line of flight, but the line of flight is part of a rhizome. These lines always tie back to one another. (1987, 9)

Collaboration, to my mind, has a distinctly rhizomic flavor: it contains lines of segmentarity from which it derives form and by which it is driven; so also does it rely on and produce a field of multiplicities; it contains the mechanisms for deterritorialization. And yet, such lines of

flight as the rhizome sets in motion don't offer Malinowskian visions of escape. Far from it, they tie back to one another as they are inevitably still part of the rhizome, much as the collaborative exercise necessarily oversees the reconvergence of directional vectors. Dualisms are meaningless, but restratification is always imminent.

The value of the metaphor in closing lies particularly in the suggestion that the ethnographic field has not only an inherently disjointed quality but is cut into disciplinary pieces so as to be reassembled by the successive arrangements of collaboration itself. The inherited field is also made with the participation of the anthropologists who claim it, debate it, and compete over it. The labor of recoding is, then, guided in no small measure by the expectations, commitments, and parameters generated betwixt and beyond anthropology, by the collaborative exercise: in the case of the ELSI-HapMap, the history of ethical violation that generates (funding for) ELSI, the methodology and rationale for community review, the IRB oversight, the demands made of anthropology and of anthropologists, by peers within the discipline as much as by colleagues in other fields. What room there is for innovation—and there is this room, more so in our study for the presence of three anthropologists at the table—is nonetheless guided by the ways in which we are each called upon to be ethnographers within the frames of ELSI research and beyond it: a function of positioning, expertise, and other professional considerations besides. Method comes into being, incompletely and never entirely perfectly, in a field defined by such limits.

The rhizomatic character of collaboration is also evident in the fact that it has no real conclusion: such work as ours in the ELSI-HapMap project ends almost arbitrarily when funding runs out (although the Coriell Institute, where the samples we helped collect are housed, provides some funds for continued contact with the community advisory group), and quickly becomes “preliminary data” for new RoIs (perhaps the most sought-after category of NIH funding) and RFAs (requests for application) to the NIH. Quite likely, as advisors averred, it was all about positioning. The success of ELSI-HapMap itself makes a case for further research on a now “bioavailable” community, if I may adapt Cohen's (2005) term for this context: one whose members recognize only too clearly that their very willingness to take part in genomic research positions them strategically, too, to be able to watch science in motion, to be able to ask questions about the scientific logics of sample selection,

or about how further research using HapMap data is being prioritized. Ethical compliance, were we to term Indian willingness as such, does not preclude questioning and demanding more, as I quickly discovered at a community advisory group meeting organized in late April 2008, shortly after the formal conclusion of ELSI-HapMap. The operationalizing of ethnography in the end positioned not just us in the research group but equally those of our interlocutors who wished themselves to be strategically positioned (whether to ask for personal assistance in seeking jobs for nephews and sons, professional aid with writing grants and carrying out other research related to HapMap, assistance in building India studies, or anything else beyond). Collaboration revitalized obsolete tropes of anthropology and its practitioners paradoxically in service of the futuristic, yielding new bioavailable publics and new objects of ethnographic inquiry alike. It yielded a field of strategic positionings, the fresh allure of never-before-seen-views for all those involved. In this sense, the speed of science, especially of such technosciences as genomics, and its own collaborative needs, makes it always already a moving target for ethnography—whose own “unbearable slowness,” as Marcus (2003) puts it, is such that the catalytic powers of collaboration are quite necessary to retain science as a viable object of interest. The lines of flight are many and demanding of pursuit across a field in so many thick and thin pieces. Ethnographic praxis is both subject and object in this landscape, interminably caught in collaboration.

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Notes

Research on which this paper is based was part of an NIH/NHGRI study titled “Indian and Hindu Perspectives on Genetic Variation Research,” conducted in Houston from 2004 onward. My thanks go to our research group in Houston—Rich Sharp, Janis Hutchison, and particularly Jennifer Hamilton—for all the explicit and implicit conversations about

our chance collaboration. This paper began as a Center for Ethnography workshop presentation at the University of California, Irvine in March–April 2006; I owe much gratitude to George Marcus and Jim Faubion (as ever) for providing opportunity, intellectual framework, and impetus to finesse my descriptions of the methodological issues that the ELSI-HapMap project in Houston has raised. A version of this essay is soon to be reprinted in *Fieldwork Is Not What It Used To Be: Anthropology's Culture of Method in Transition* (edited by James D. Faubion and George E. Marcus, forthcoming from Cornell University Press).

1. The International HapMap Project (www.hapmap.org) is a collaboration among scientists in Japan, China, Nigeria, the United Kingdom, and the United States, formally launched in 2002. Its goal is to create a haplotype map of the human genome, to describe common patterns of human DNA sequence variation. Differences in individual bases of the DNA sequence are called single nucleotide polymorphisms (SNPs, or “snips”). Sets of nearby SNPs on the same chromosome tend to be inherited in blocks, and their pattern on this inherited block is known as a haplotype. Blocks may contain large numbers of SNPs, but only a few “tag SNPs” are sufficient to uniquely identify haplotypes in a block. Such reasoning makes it possible to reduce the number of SNPs needed to examine the entire genome from 10 million common SNPs to 500,000 tag SNPs. Genome scans that seek to find genes that affect diseases will therefore become both more cost effective and efficient. The International HapMap Project itself does not attempt to correlate genetic variants with diseases but to make information about variation available to other researchers who may then carry out disease-specific research programs. All samples are stored at and distributed from the Coriell Institute in Camden, New Jersey.

2. And not just my role, to be sure: each of us involved with the ELSI-HapMap study in Houston played broker to greater or lesser degrees. This said, my “ethnic background” shaped my position in ways that set my discomforts and commitments somewhat apart from others in our group, as I am about to suggest.

3. Indeed, we were each interviewed by Jenny Reardon for her own NSF-sponsored research titled “Paradoxes of Participation: The Status of ‘Groups’ in Liberal Democracies in an Age of Genomics” in June 2006.

4. The ethnographic literature on such uncomfortable alliances suggests that such experiences are not exceptional. See, for instance, the essays in the 1998 issue of *Science as Culture*, guest-edited by Kim Fortun and Todd Cherkasky.

5. Our original research protocol made some provision for collaboration with interlocutors, via the creation of what we called “working groups,” small groups of interested individuals with whom we would work to hammer out the details of informed consent—primarily by creating an informed consent document that was culturally attentive and thus appropriately communicative. We quickly discovered, however, that our interlocutors were hardly concerned with such legal-ethical demonstrations of trust, more than willing to take us and our commitments on good faith (especially since we each were associated with reputed area institutions), and were far more concerned with the greater “good” served by their participation in studies like ELSI-HapMap. Our plans for the working group interactions were therefore melded together with a series of focus groups conducted at specific institutions, and we proceeded to assemble a community advisory group (CAG) slightly in advance of the original plan. The CAG model was less explicitly collaborative: members were to provide comments and feedback, or sanction for any critical decisions we made regarding sample collection, the naming of the samples, and so forth. They represented more of a periodic “community check,” if I

may, in the formally post–data gathering phases of our project. They also have served, far more than collaborators, as the entry points through which to reestablish contact with “the community” periodically, to keep them informed about and interested in the fates of the samples whose collection they helped facilitate. Our last meeting with the CAG was in April 2008, just after the project’s formal conclusion. Its success, as I discovered, was critical not just for its own sake but equally to establish the ethics of ELSI-HapMap: what we showed by staying in touch with our interlocutors was that this was *not* a model of “helicopter research,” as a colleague later remarked. The two forms of collaboration, one with the community and one with colleagues intersect here: continued community consultation, whatever form this now takes, is instrumental as a response to the critics of sample collection, whether informants or colleagues.

6. But not too vulnerable: Native Americans were deliberately not selected for sampling because of their overwhelmingly critical response to the HGDP (International HapMap Consortium 2004).

7. Of course the comment that “we are not doctors” assumes that only physicians would be interested in the outcomes of genetic research, an interesting perspective in its own right that is, oddly enough, well in line with the emphasis of the International HapMap Project on health-related outcomes. And the comment ignores the number of Indians who are themselves physicians in Houston alone, not to mention the clout of AAPI (American Association of Physicians of Indian Origin) nationally. The simplest reading is that the comment is intended to limit further conversation, but it also points to the disjuncture between “community” and “genetic research” as entities that come to be allied only within the context of community engagement studies.

8. So in the past years cultural performances of music and dance put on by students and local artists have become regular offerings at the Miller Outdoor Theater in Hermann Park, a statue of Gandhi now towers over an odd assortment of busts of Latin American figureheads at the Rose Garden, and the local Kannada organization organizes yearly seminars in collaboration with the Museum of Fine Arts Houston, inviting the participation of scholars from Europe, India, and all around the United States. The initiative to establish an India Studies program and/or chair at the University of Houston is also in the same vein. Finally, we received much praise from community representatives for organizing a Grand Rounds Lecture at Baylor College of Medicine and the Methodist Hospital on Gandhian ethics of nonviolence and medical practice. The wish clearly was that discussions bringing Indian ideas into other, non-Indian contexts, and encouraging the interaction of “community” with “academia” would continue.

9. And not just to prospecting-based research either: think of “corporate social responsibility” (CSR) as an increasingly central sign of business ethics, often demonstrated by the promise of “giving back” in the marketing, CSR rhetorics of companies as ubiquitous as Target and Starbucks.

10. Personal communication.

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