

## **Toward a History of Collaboration in Anthropology**

**Rebecca Lemov**

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

For this panel my assignment was to ask: what can the history of collective work in anthropology tell us about twenty-first century research undertakings? On one level, the question is, Was this work ever anything more than “collected”—that is, simply massed together, the encyclopedia-slash-database approach: did it arrive at true collectivity of research? And then a pragmatic question: Is there a history of collective work in anthropology that is not all unusable, or usable only in the form of a desperately cautionary tale. Almost immediately on posing these questions I fell into a bind: the tendency is either to castigate or to lionize. Either to say, “We should do it like they did” – find and hold up appropriate exemplar – or “they were naive and worse than naive....” Aside from this type of Manicheanism I also want to avoid etiolation in this approach to the past in relation to present and future anthropology. For the strange thing is that these projects have for the most part simply disappeared.

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

### **ARCHIVE 1. CCS, IHR & OCM**

The Yale compendium has been called (and may indeed have been) the biggest filing cabinet in the world. Their aim was unblushing. The sum of the world’s contents

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

I don't want to spend too much time on the Files because in many ways they're the most "epistemologically naive" of the projects I want to consider, and they also bear a 19th-century taint (reminding one of earlier efforts by Spencer, Tylor, and the ill-fated Dutch scholar Steinmetz to build a 'roomful of drawers' containing the totality of information on a given subject). I will focus instead on an archival moment that has to do with an early shift. Originally the project belonged to one man, who envisioned it as a sort of lifelong prospecting venture in the comparative forms that culture takes. From 1929 to 1934, George Peter Murdock worked alone in libraries culling, distilling, condensing the facts of various cultures. At some point in the mid-30s, though, Murdock admitted defeat and joined with others. "It is a task beyond the powers of any single individual; I have tried my hand at it, and have done a fair amount of spade-work, but I realize that I cannot possibly accomplish it by myself..."<sup>1</sup> The result was that only did the Files become an intensively collective (not just collecting) enterprise, but it joined with the efforts at the Yale Institute to craft a unified theory of social life. These two developments were in tandem—and they suggest to me a transformation in the subjects and objects of knowledge.

Six anthropologists and sociologists convened under Murdock to discuss how to carry out the task of carving up the totality of cultural information into categories. After experimenting with different divisions, each scholar reported back with his conclusions on the most seemly break-down as it appeared to him. Two-digit numbers from 10 to 88 marked each major heading and a third or fourth digit marked each subdivision thereof, thus communicating with that numerical coding a confidence in the impartiality and neutrality of the divisions provided. After several months, a mimeograph of the scheme went out to "all kinds of people—economists, sociologists, geographers, anthropologists,

engineers, lawyers, housewives, industrialists” for criticisms; 100 useful comments came back.<sup>2</sup> The whole was adjusted and eventually published as the *Outline of Cultural Materials*. The OCM, along with a classifying scheme for the totality of societies, the *Outline of World Cultures*, would serve as indexes to the Files.

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

It's important to stress here that the files were not mere auxiliaries, a database-style handmaiden waiting to assist the grand theoretical labors of experimental psychologists and sociologists; rather, the files were themselves conceived as each embodying an experimental logic. Robert K. Merton agreed, and cited Yale's cross-cultural surveying efforts approvingly in his “Manifest and Latent Function.” In Merton's view comparison was anthropology's and sociology's close approximation of experimental logic—“quasi-experimental” is his phrase, and the cross-cultural survey “held large promise” in that regard.<sup>3</sup>

## **ARCHIVE 2. SILA 1942-3**

My second archival moment comes from the minutes of a meeting of a new project—the Strategic Index of Latin America—that had been recently contracted, in 1942, by the government (not the State Department, but its equivalent entity in regard to Latin America, the Office of the Coordinator of Inter-American Affairs). The meeting included representatives from different arms of government, the military, and various academic anthropologists. The particular document that interests me, titled just “Morning Session,” shows that things are not always exactly what one assumes, and that the surface of epistemological naivete is sometimes broken by ripples. Murdock began the meeting by giving a capsule history of how the by-now-nearly-epochal Yale filing project came to be and how this new project emerged out of it. As it happens, it was through Alfred

Metraux, who was at Yale in 1939-41, that the group originally made contact with Argentine anthropologists and a Spanish translation of the outline was published. Because Metraux and John P. Gillin were working on their *Handbook of South American Indians*, the Files organizers decided to compile a Latin American sample and use it as “a sort of laboratory on the basis of which social scientific research could be done.” Murdock also mentions that Bronislaw Malinowski, also at Yale, was involved in promoting the files at the outbreak of war.

At this point, the meeting takes a turn: the anthropologist Julian Steward interjects, and brings an almost Aristotelian note to the discussion: “I do not wish to bring up a philosophical discussion at this point as to when a fact is a fact, but it is my firm belief that a fact is not a fact until it is related to a problem. We can’t deal with isolated facts, but rather we must deal with problems and select the literature from which the facts are obtained on the basis of the problems...just getting facts won’t be useful...[unless] related to specific problems.”<sup>4</sup> It’s an interesting moment—not that the suggestion was picked up explicitly, although a few participants bandied about the idea for a while, but that it suggests an attention to problematization: Had Michel Foucault entered the conversation at this point, he might have observed that problematization entails “how a situation appears as a possible question,” and perhaps continued, “This elaboration of a given situation into a question, this transformation of a set of difficulties and troubles into problems to which diverse solutions are proposed as responses is the epont of problematisation, the specific work of thought.”<sup>5</sup> The files were becoming more than vast repositories or compendious encyclopedias.

### **INTERLUDE c. 1942: Margaret Mead Breaks Her Tooth**

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

People disagree about when exactly cybernetics as a research movement and an activity was born (it was not named, officially, until 1947, by Norbert Weiner; and it never, through a series of perhaps fortuitous accidents, had its own centralized research

laboratories). Cybernetics has been defined recently, in fact, by the French scholar Jean-Pierre Dupuy, as a sort of long and multi-sited conversation that took place over many years and involved people talking to each other who almost never talked to each other. The legendary Macy Conferences from 1946-1953 were key arenas, everyone agrees. The start to that conversation, however, was a preliminary meeting, a kind of pre-Macy Macy meeting, in 1942, in New York. The topic was one perhaps unlikely to lead to a grand synthesis of human social and biological systems: cerebral inhibition and the workings of hypnosis. An interdisciplinary group gathered, including physiologist Arturo Rosenblueth, neurologist and poet Warren McCulloch, foundation officer Frank Fremont-Smith, and intellectual entrepreneur Lawrence K. Frank, as well as a psychoanalyst and the husband-wife team of anthropologists Margaret Mead and Gregory Bateson.

Amidst talk of many things, Rosenblueth came forward and presented a sneak preview of the basic ideas for a paper he was to publish the next year, 1943, along with Norbert Wiener and Julian Bigelow.<sup>6</sup> Rosenblueth's presentation was really a manifesto for something new, and the main point was to argue that one could now talk about teleological behavior (that is, purpose) in any system while also remaining true to the demands of adequate explanation. The ghost in the machine didn't have to be blind. Due to negative feedback, also known as teleological mechanisms or servomechanisms, you didn't have to throw out the baby (of adequate scientific models) or the bathwater (of the formerly metaphysical realm of teleology and purpose). Machines and mice were no longer *models* for human function; they were made of the same stuff. As Rosenblueth said somewhat cryptically, "The ultimate model of a cat is of course another cat, whether it be born of still another cat or synthesized in a laboratory." It was, these proto-cyberneticists thought, a revolution against simple-minded linear reasoning, reductionism in science, and the blinders of disciplinary thought. On exposure to this epitaph-for-behaviorism and prolegomenon-for-cybernetics, Warren McCulloch was excited. Gregory Bateson was excited. Margaret Mead broke her tooth and didn't notice until after the meeting was over, so excited was she.

This presentation in particular inspired the creation of a series of later meetings under the rubric and title of "Circular and Causal Feedback Mechanisms in Biological and Social Systems"—later changed, but not until 1947, to the more felicitous

cybernetics. It was because of Bateson, in particular, that the conferences planners added “And Social.” Soon there were massively collaborative meetings, including Mead and Bateson, Clyde Kluckhohn and Talcott Parsons, and from Columbia Paul Lazarsfeld and Robert K. Merton, as well as Wiener, McCulloch, Claude Shannon, and John von Neuman.

### **ARCHIVE 3. CIMA 1947-51**

The third archival incident takes place during the further expansion of the filing domain and its accompanying experimental impulse. The local occasion was the establishment of the U.S. Navy’s Occupied Area in the former Japanese Mandated Islands in the territory of Micronesia spread out over a million square miles of ocean—is a historical event that explains the appearance, in 1947, of 41 physical anthropologists, linguists, ethnographers, sociologists, and “human and economic geographers” on these same islands. Social scientists from more than 20 institutions fanned out over 12 island clusters in what its organizers called “the largest cooperative research enterprise in the history of anthropology.”<sup>7</sup> All researchers carried instructions to take notes in duplicate and send their carbon-copy reports back regularly to both the Navy and the NRC. Under the aegis of a principled “Early Availability of Information,” regular Interim Reports, Final Reports and even “undigested field-notes” made their way into the project’s files. Total war was to be followed by total anthropology.

The moment I want to point to concerns the question, Why this inter- and cross-disciplinary project was labeled “anthropology” in its title and most of its descriptive documents? In the archival record, the project first appears as the “Coordinated Investigation of Micronesian Peoples,” and bears the perhaps inauspicious acronym “CIMP.” In subsequent funding documents, its authors abandoned this title in favor of the “Coordinated Investigation of Micronesian Anthropology,” or CIMA, presumably with the aim of giving stress to the scientific and methodical *study* of the varieties of Micronesian culture and society—via anthropology--rather than a simple presentation of the array of “Peoples” to be found there. In other words, the authors were emphasizing the scientific and analytical nature of their investigations. In all likelihood, the classification of CIMA as anthropology was due to factors both practical and theoretical. To be sure, anthropologists organized, fronted, and promoted the project initially. But in

a broader sense, it was an attempt to be “total” in pursuit of knowledge, typical of a movement in American social science to build an anthropology consisting, by definition, of knowledge extending beyond the modern discipline of anthropology into all fields bearing on the human.

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

#### **ARCHIVE 4: 5 cultures 1949-55**

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

To this small area the project dispatched over 65 field workers, including anthropologists, historians, political scientists, sociologists, and psychologists of both an experimental and Freudian stripe; along with 29 analysts, and 4 clerk-typists making it the biggest cooperative project of its kind ever undertaken (bigger, obviously, than Yale’s Coordinated Index, just preceding it). Preexisting systematic work in Ramah since 1936 had involved 30 different anthropologists participating (for one to three years each); from their notes and records, over 150,000 items had been extracted, entered on slips, and classified in files at the Peabody Museum. In 1949, the project's overseers combined this earlier filing system with Yale's system under Murdock's *Outline of Cultural Materials*.

They modified the system to incorporate the filing of values, beliefs, and subjective materials. Taking a veiled swipe at Murdock, project director Clyde Kluckhohn assured his funders, "[W]e are doing something beside the Sears Roebuck type of inventory." Researchers such as David Schneider, Clifford Geertz, and Robert Bellah, like everyone, sent their field notes back to be filed in the Peabody Museum.

What I want to focus on is not the debatable success or failure of 5 cultures. One can ask, What did all this effort actually end up with? A number of publications; many meetings and exchange of memoranda; a report of the exhaustion of the people of Ramah, one of whose residents was heard to say, "Where does Harvard get all the queer people it sends to Ramah?"<sup>8</sup> What interests me is Five Cultures' relationship to the Laboratory of Social Relations, which was designed as an anti-bureaucratic zone of free experimentation: five cultures was a laboratory within a laboratory, in essence. (And then there is Alfred Horowitz's failed 1953 grant proposal, which argued, "the concept of values is the modern thinkers' answer to the problem of including a teleological mechanisms in any explanation of human behavior.") They were interested experimenting with teleological mechanisms, self-regulating systems, self-domestication, and autopoiesis. As one social scientist at the Laboratory of Social Relations declared, "*Men are now re-combined ... to provide the best collection of raw material from which ... a system can be built.*"<sup>9</sup> What I have been asking throughout all these projects and archival fragments is: how did these new arrangements arise and what new objects of knowledge came into being as a result?

### **Commentary on the history of collaboratories**

All this constitutes, in the words of Kingsley Amis's Lucky Jim a "strangely neglected topic" in anthropology. One particular aspect of these projects now largely neglected, and which I have wanted to extract by means of these assorted archival fragments, could be called the "laboratory" approach to ideas—something like C. S. Peirce's pragmatic conception of the scientific inquirer "carrying his mind into his laboratory." The value of these projects was their collaborative, collective nature. This was experimental—in two senses, one flawed, the other promising. The naive objectivism of modeling-a-social-science-of-the-future on physics was, well, naive. A more promising clue to what they offered is gained from looking at a recent statement of

Lakoff and Collier in exhortation of a new approach to anthropological inquiry. They contrast two models for arriving at anthropological knowledge, the individual, avant-garde-ist and the collective, experimentalist (laboratory) approach, and argue that the individual-project model of experimentation has exhausted itself in pursuit of textual innovation. “In a laboratory, by contrast, ‘experiment’ does not refer to textual experiment. Rather, it refers to ‘controlled experimentation’ that might lead to critical rectification of concepts and claims. In the course of experimentation *concepts are put at risk* through their use and interaction with cases – either they work or not.”<sup>10</sup>

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

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1 George P. Murdock memorandum, "Proposed Program for Anthropological Research under the Direction of the IHR as part of a Coordinated Program of Research Aimed at the Achievement of an Integrated Social Science," c. August 1939, Yale Archives, YRG 37-V, IHR, Series II, Box 11, Folder 11-95.

2 Creation of taxonomic system as recalled by Murdock in minutes of “Meeting of Advisory Board of Strategic Index of Latin America,” August 29, 1942 National Archives, State Dept., R.G. 229, Entry 1, Stack Area 350, Row 76, Comp. 2, Shelf 4, Box 134.

3 Robert K. Merton, “Manifest and Latent Functions,” *Social Theory and Social Structure* (Glencoe, IL: Free Press, 1957).

4 “Meeting of Advisory Board of Strategic Index of Latin America,” Morning Session, August 29, 1942 National Archives, State Dept., R.G. 229, Entry 1, Stack Area 350, Row 76, Comp. 2, Shelf 4, Box 134.

5 Foucault’s view of problematization are quoted in Paul Rabinow, “Steps Toward an Anthropological Laboratory,” Discussion Paper, February 2, 2006, Anthropology of the Contemporary Research Collaboratory.

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

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

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

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

10 Collier and Lakoff, 2006.