

What is Concept work? An Exchange

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

- a. How is it collaborative?
- b. What does concept-work make possible that is not possible with the use of branded concepts according to the individual project model?
- c. How does critical rectification happen? What role might informants play in such critical rectification, if any?

- GM: WHAT DOES CRITICAL RECTIFICATION MEAN? MY SENSE IS THAT THERE IS STILL AN INHOUSE SET OF USAGES/LINGO AMONG ARC RESEARCHERS—SO THERE IS STILL AN OPAQUENESS BY THE OUTSIDER LOOKING IN. A DESCRIPTION OF THE INFORMAL CULTURE OF WORK OR PRACTICES WITHIN ARC--SOMETHING VERY SIMPLE--MIGHT BE HELPFUL AND MAKE THINGS MORE EXPLICIT. I KNOW YOU GUYS ARE DOING A LOT OF WORK. I AM JUST NOT SURE THE FORM IT TAKES.
 - ARC: By “critical rectification” we mean: we can discuss our concepts and findings, and we can have an argument in which – through the application of shared standards and understandings – we can figure out that some formulation/idea/distinction was wrong, in need of greater precision, or in need of reframing in relationship to other contexts and distinctions.
 - Here is an extended example that might provide a sense of how this happens in terms of the “internal workings” of the collaboratory, per your question above. There are also some citations to give you a sense of how this process has related to the production of more traditional products of research (published articles and working papers):
 - When we began our current project, we defined security, following Niklas Luhmann, as the transformation of uncertain dangers into calculable risks (Collier, Lakoff, Rabinow, 2004). Thus, in our initial formulations (in our NSF grant proposal, for example), we proposed to study security “initiatives” that sought to transform emerging biothreats into calculable risks.

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

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

- *Here is where an empirical “sounding” (discussed below) played an unexpected role. We have a research assistant, Lyle Fearnley, who has been doing terrific work – through interviews and documentary research – on “syndromic” surveillance systems for detecting outbreaks of infectious disease. Government agencies charged with “security” questions have become very interested in the ability of such systems to detect a health*

“event” in a population in real time, particularly given the increased concern with the threat of a bioterror attack since 9/11 and the anthrax letters.

- *As we were engaged in this discussion of preparedness, Lyle was pursuing a historical chunk of this research (Fearnley 2005), which focused on the moment of “epidemiological transition” in the United States. One feature of this transition was a shift in the focus from diseases regularly occurring in a population to disease “events,” that is, diseases whose dynamics were not known. Experts thus identified a need for real-time identification of health events in a population. Their efforts are one important genealogical precursor to contemporary syndromic surveillance (and, in a certain way, to contemporary ‘preparedness’).*
- *Fearnley reported that first order actors, in this historical context (immediately after World War II), identified a distinction between the “archival” knowledge required for the management of epidemic disease and the real-time knowledge of populations required to deal with disease events. This distinction helped to clarify the dispute in the collaboratory, and pushed the process of conceptualization forward. We agreed that the initial distinction – between preparedness and “risk” – was not exactly the right distinction, and that the distinction between calculability and non-calculability advanced in the “risk” literature was also not quite the right distinction. Rather, the salient contrast was between a certain risk technology – insurance – and preparedness. Insurance is based on actuarial analysis that draws on archival knowledge of populations. Preparedness, by contrast, draws on techniques of what we later called “imaginative enactment” (Collier and Lakoff, 2006) to deal with low-probability, high-consequence events about which no archival knowledge exists.*
- *The payoff of this clarification can be identified on a couple different levels. On the one hand, it led to a critical intervention into discussions of “risk society” by people like Beck and Giddens (and responses to this work by scholars of “governmentality”). Given our research, we see two very fundamental problems in this literature. First, they have identified as important the emergence of events to which insurantal mechanisms don’t apply. But they have not yet found a way to investigate what comes next, or what techniques are used to manage such events. Second, we think that they have the basic distinction wrong. It is not a question of “calculability” per se but of the kind of calculability, the techniques of quantification, and the purposes for which they are used. It is not a question of “risk” but the techniques of risk assessment, how they change in relation to different sorts of objects, and how they are articulated in certain apparatuses. We now have a sophisticated vocabulary for thinking about these things. The theoretical literature does not.*

- *On the other hand, we have been able to confirm and build on this distinction in a number of other areas – for example, in the work that one of our research associates, Dale Rose, has been doing on the CDC and smallpox vaccination strategies. It is now also informing PR’s research into how life scientists and government regulators are proposing to regulate synthetic biology, as well as further historical work that SJC and LA are doing on preparedness and the political logics with which it has been associated.*
- **IS IT FIELDWORK OR JUST 'INVESTIGATION' ON THE MODEL OF THE JOURNALIST OR DETECTIVE? I ABJURE FIELDWORK STORIES, BUT A BIT OF THIS MIGHT BE NEEDED HERE.**
 - *ARC: This distinction between “fieldwork” and “investigation” is not clear to us, and we would be interested to know when something counts as “fieldwork” – whether, for example, the activity of a political scientist who spends two years in the field counts. Certainly, the term is used in that context. Or, to take a case from the collaboratory: Fearnley’s work has involved looking at documents, attending a couple conferences, and talking to a handful of experts. He has produced penetrating conceptual insight. And indeed, all of us are combining things like intensive discussions with experts, attending security-related events, and documentary research. Does this count as fieldwork?*
 - *A better approach to this question, however, might be to take a step back. An important impetus of our work is to think more about the status of “fieldwork” or “ethnography” in relation to the problem of methodology in anthropological discussions. We have been saying for a while (see Collier and Lakoff, 2000) that anthropologists tend – mistakenly – to limit discussion of “method” to discussions of fieldwork and writing. In our view, fieldwork is a technique – or, perhaps, a set of techniques – but not a methodology. There may have been previous configurations of the discipline in which it was the central technique of an anthropology that was committed to a rather holistic version of the culture concept, but it seems those days are gone.*
 - *We would prefer to deconstitute the idea of “fieldwork” and to ask what it is, more concretely, that is being talked about. It seems that in anthropology fieldwork can refer to interviews, observations of (and participation in) meetings, informal discussions; and also, close reading of documents produced by actors. Our main point is that it is good to reflect on these techniques, both individually and in their interaction, but that is not the same as reflecting on “methodology” – which concerns, among other things, how these techniques of data-gathering interact with concept formation and the establishment of collective standards, norms,*

and conventions to yield meaningful claims, and meaningful progress in thinking. So one way to put this point is that we are not focused on fieldwork per se, but on the process of interaction between concept-work and fieldwork. So the collaboratory has a number of people doing “fieldwork” of various kinds in various areas – syndromic surveillance, vaccination, synthetic biology, civil defense, strategic bombing, and so on. The question, then, is how to generate a process in which this collective work feeds into broader conceptual issues, and how, in turn, these conceptual issues generate specific questions to be approached through fieldwork.

ARC: Some background:

- a. Most discussions of method in anthropology have been mostly restricted to the practice of ethnography and writing.
 - b. These discussions left un-posed a series of questions: How does one decide where to do fieldwork? How are significant problems identified? What conceptual framework is used in the field?
- GM: WHAT ABOUT MARILYN STRATHERN, ON ONE HAND (THIS SEEMS TO BE WHAT HER DIFFUSE WRITING IS ALL ABOUT THESE DAYS; SHE CELEBRATES THE INDETERMINANCIES, THE SURPLUS OF FIELDWORK AND, ON THE OTHER, RHEINBERGER ON THE OTHER HAND, WHO HAS EVOKED A VERY PLEASING CONCEPTION OF PRACTICE THAT IS INCREASINGLY INVOKED BY ANTHROPOLOGISTS AS WHAT THEY DO. STRATHERN IS THE MESSY 'JUST DO IT' VERSION; RHEINBERGER OFFERS A NOTION OF DESIGN. IN EITHER OF THESE CASES, IS CONCEPT WORK CONSISTENT WITH WHAT THEY ENVISION. THINKING THROUGH YOUR PRACTICES IN THE CONTEXTS OF STRATHERN AND RHEINBERGER MIGHT BE INTERESTING.
 - ARC: *Both of these are helpful points of reference, and seem useful to us. We agree that developing techniques to generate unexpected findings is important (although we think that it is in need of further specification and can't be claimed uniquely for anthropology; after all, Rheinberger is talking about how natural scientists use research design as systems for generating surprise). What we think might be missing in both cases is what is often missing from discussions of method in anthropology: analysis of orientation, of significance, and of problem formation. These are the questions posed above: “How does one decide to do fieldwork?” “How are significant problems identified?”*

- *So Strathern argues that an important aspect of fieldwork is its indeterminacy – that one goes into the field and one collects more than one “needs,” because, presumably, one does not yet know what is significant. This seems right, and resonates with our process in the current project. For example: a year ago, we had no way of knowing that we would be working on theories of strategic bombing or the history of exercises in the military! And the only way to “discover” that these things were relevant to our project was to go into the field with at least some indeterminacy, and a sense that there was a period of thrashing about without knowing what is going on that had to be part of the process. But we would want to add that one does not do this in an unstructured way, or in a way that is not guided by an understanding of what significance is. Where does one go? Whom does one talk to and what about? What books does one read? To describe this simply in terms of a method that collects more than it needs, that emphasizes indeterminacy is just mystification. There **are** reasons that we go one place rather than another: we are interested in bio-power, we are interested in expertise, we are interested in the interaction between security and social welfare, etc. So we think that the naïve “entry into the field” stories are just bad accounts of what anthropologists actually do. So we want to describe how a process of relatively open-ended searching might be linked to a rigorous process of concept-formation.*

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

ARC: These [questions] lead to further methodological issues: How are knowledge-claims generated, and defended? How might such claims contribute to broader discussions – and to a project that advances thought? In response, we began to develop a way of collectively developing and refining concepts in relation to findings in “the field”.

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

- ARC: *We want to be provocative, at least in the sense that we want anthropologists to be less complacent, and more critical and reflexive, about a range of taken-for-granted assumptions about method that have taken shape over the past couple decades. But we don't think that this is a throw-back. It seems to us that these are quite contemporary debates that are being thrashed out in anthropology and elsewhere.*
- *In asking questions like “how might move thought forward” we are not talking about a naïve objectivism. Rather, it is a pragmatist epistemology that is consistent with Dewey, Rorty, etc. It acknowledges, as Paul has written (2004) that any reasonably coherent theory of scientific knowledge acknowledges that it is based on concepts that are constructed. But this does not mean that one does not seek to advance understanding, successfully and productively reframe problems, make defensible claims about the world, or clarify concepts in a fashion that can be subjected to critical rectification through impersonal norms.*
- GM: ARE REFINED CONCEPTS THE ACTUAL FINDINGS IN/FROM 'THE FIELD' OR ARE THESE FINDINGS SOMETHING ELSE? AGAIN, FIELDWORK SEEMS TO BE DISTINCT (AND INDIVIDUAL) BUT I AM NOT SURE. ALSO I AM NOT SURE WHAT THE FIELDWORK IS? INTERVIEWS ONLY? BONDING WITH PARTICULAR INFORMANTS/ENTERING THEIR WORLDS IN A SUSTAINED WAY?
 - ARC: *We do think that refined concepts are one of the important things you get from our kind of inquiry. But they are important because they are a dynamic part of inquiry: they are helpful for identifying significant problems, identifying sites that are worthy of investigation, and of developing new kinds of critical and reflective understanding of significant issues. The discussion above about “risk” and preparedness is an example.*
 - *On “fieldwork” – it would, again, be helpful for you to say more about what that term specifies for you. As noted above, we tend to think in terms of specific techniques, such as expert interviews, close reading of documents, etc. Bonding with informants per se has not been the emphasis of the project so far. But there is another element of what we are up to that is, in some sense, characteristically anthropological, which is the attempt to see how different security rationalities work by “entering the world” of experts who are working them out.*

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

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

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

- GM: YES, BUT HOW DOES THIS TRANSLATE INTO FIELDWORK EFFORT IN A MORE LITERAL WAY? IS ALL OF THIS HAPPENING IN THE FIELD? OR IN THE LAB? MY OLD QUESTION -- IS FIELDWORK WITH EXPERTS CONCEPTUAL WORK TOO -- IF SO, HOW IS THIS CONCEPTUAL WORK DIFFERENT FROM THE CONCEPTUAL WORK OF THE RESEARCHERS?
 - ARC: *This is a helpful question. As you know, a major part of Paul’s approach (at least since French Modern, but really it is very central to Reflections on Fieldwork) has been the study of observers “in the field” who are also engaged in concept work. And this is definitely part of our past and current projects. We are interested in studying experts, or, better, what Hacking calls “styles of reasoning” that experts employ, and in understanding how these styles of reasoning give shape to institutional responses that are part of new problematizations.*
 - *But we should emphasize that – unlike those who have been trying to draw an analogy between anthropological knowledge and the knowledge of the experts we study – we don’t think that all forms of knowledge are structured in the same way. It seems to us that there is a clear distinction between the kind of concept work that we do and the kind of concept work done by those in the field – the “first-order” observers.*
 - *Let us give an example. We have been following (in the sense of reading the writing of, and listening to the talks of) a guy named Stephen Flynn. Flynn is one of our exemplars of a “vital systems security” expert. He is defining a very distinctive conceptual and practical position in contemporary debates around how to deal with security problems. So, for example, he has been engaged in a debate with another expert – one James Carafano, who is, in our terms, a “sovereign state security” guy – over the problem of port security. Flynn wants to see it as a vital systems security issue (we need to have systems assurance technologies that allow us to identify rapidly the source of a security breach in the international shipping system so that “auto-immune response” does not shut down the system). Carafano thinks that shipping is a highly unlikely channel for*

terrorist attack, and that it would be a waste of billions of dollars (and an unacceptable burden on international commerce) to install such a system. Much better, he argues, to find the bad guys and kill them – a classic case of what we call a logic of “interdiction.” So these “first order” observers argue, try to convince politicians, influence spending, marshal evidence, etc. Those are the stakes of their game.

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

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

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

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

- GM: HOW IS #3 DIFFERENT FROM #1?
 - ARC: *This is discussed a bit above: It seems to us that #1 subscribes to a kind of naïve empiricism that suggests everything one needs for understanding, or diacritical analysis, or inquiry, or whatever, is waiting for you in the field. All you have to do is “be there” with the proper “anthropological” ethos of indeterminacy, interest in finding surprise etc.*

As noted above, this seems to us both methodologically unsupportable and a bad account, in any case, of what actually happens in anthropological inquiry.

- *We gave one example – above – of how we think that a dynamic process of conceptualization might relate to “fieldwork.” Perhaps it would be helpful to give another that indicates the costs of the first approach noted above:*
- *There has, as you know, been a huge amount of writing in the “critical social sciences” – including in parts of anthropology – about security in recent years, especially after 9/11. In reading this literature, it’s astonishing to what extent it frames the basic problem of security today as one of “militarization” of the civilian sphere. If you want an example, check out something written recently by Ann Stoler (an article in the Radical History Review, for instance), who is working on some of the same documents produced by DHS that we are working on (in other words, the “field” is the same). For her, the story of DHS is a story about militarization. What is striking from our point of view is that she has not even posed the question: “what is security?” One of the advantages of making this an object for critical reflection before diving into a research site is that we now understand that security does not always mean guys with pressed uniforms and shiny boots. What is more, the history of preparedness in the United States specifically includes a very deep concern with the relationship between “military” and “civilian” affairs, and the concerns with militarization have most powerfully come from conservatives who are against the federal government, and for the system of free enterprise. So at least one thing that #1 can get you is confused, both empirically and politically.*

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

- GM: WHAT ARE EMPIRICAL SOUNDINGS? SORRY FOR THE PICKINESS.
 - ARC: *A sounding means dipping a fathom into the water to test the depth. – so, metaphorically, this means dipping into the water to get some quick empirical orientations that can feed into problem-formation and the refinement of concepts. In the example we gave above, the work that Lakoff and Fearnley were doing early on gave us some empirical soundings to check the depths: are our concepts right? Have we framed the scope of our domain of interest properly? An advantage of using such soundings is that they allow us to assess a field without actually*

committing ourselves to long-term fieldwork in the old sense, in part because we don't know what the problem is.

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

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

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

- GM: WHY IS THIS NOT MOST COGENTLY THE WORK OF THE HISTORIAN RATHER THAN THE ANTHROPOLOGIST? TELL ME AGAIN WHY SORTING OUT THE HISTORY OF CERTAIN ESTABLISHMENTS OF EXPERTS DOES NOT DO MOST OF THE WORK OF MAPPING THE CURRENT CATEGORIES IN USE. THE BURDEN OF ARC (OR LAC) IS TO DELIVER A FINDING THAT IS DISTINCT FROM WHAT THE HISTORIAN DELIVERS OR A SHREWD POLITICAL SCIENTIST DELIVERS WHO HAS BEEN AROUND FOR AWHILE -- SAM POPKIN COMES TO MIND WITH WHOM I SPENT A YEAR AT CASBS.
 - *ARC: The distinction you are drawing here between anthropology and history is not clear to us. In the sense noted above, we would think of historical research as a technique of inquiry, rather than a methodology. (The fact that history is a “discipline” is another problem that probably deserves some reflection.) We feel – and the practice of anthropologists would seem to justify this feeling – that this technique is appropriately employed by anthropologists in various contexts. What we are trying to talk about is not on the level of technique but on the level of methodology or mode of inquiry. So, from this perspective, history versus the anthropology of the contemporary seems like the wrong distinction.*

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

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

- GM: YEAH, IT IS THE NATURE OF THIS EMPIRICAL MATERIAL I WANT TO UNDERSTAND -- WHAT IS ITS RAW FORM? IS IT ALREADY 'PROTO' CONCEPT WORK? HOW COULD IT BE OTHERWISE WHEN YOU WORK WITH EXPERTS -- SAM POPKINS -- IN HARNESS?
 - ARC: *Per the above, we are observing their practices of conceptualization – how they develop and operationalize concepts. But as noted above, our aims are different.*

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

- GM: I WOULD LIKE TO KNOW MORE ABOUT THIS -- SEEMS LIKE THIS IS DONE IN THE SOLITUDE OF THE STUDY OR SEMINAR.
 - ARC: *By “this” you mean the moments of recombination? They are, of course, “actual” in the sense that they happen as events in the organization of things, institutions, etc. What we can do in the context of the lab is to come up with concepts that allow us to identify moments of significant recombination. So, for example, SJC and AL are now tracing the various political configurations of techniques associated with vital systems security. The raw material is constituted by things like: Experts identify a new problem in a given analytic and conceptual frame, and endeavor to formulate a response.*

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

- GM: ARE THESE DISTINCTIONS 'RESULTS' THEN? HOW DO THEY WORK AS RESULTS? THIS SEEMS VERY SMART AND CONVINCING, BUT HOW IS IT DIFFERENT FROM THE TYPICAL CONCEPTUAL WORK OF THE SOCIOLOGIST WHO IS IN THE BUSINESS (GIDDENS) LIKE OF MAKING DISTINCTIONS LIKE THESE AS ANALYSIS. WHAT IS THE STABILITY OF SUCH CONCEPTS -- DO THEY STABILIZE THE PROJECT? BECOME ITS VOCABULARY TO SLOW DOWN CHANGE OR THE CHIMERICAL PLAY OF CONCEPTS AMONG ACTORS (THE CRITICAL TEMPORALITY QUESTION)?
 - ARC: *A quick answer with respect to Giddens is that he is producing “theory” about modernity. So, modern subjectivity involves a technologized, calculative, relation to the self. You can go study that, but you don’t really learn anything new – you just confirm over and over again what you already theorized was the case (we moderns have calculative subjectivities, etc.). ARC is producing concepts rather than theory. Their value is to help one see problems, tensions, motion in a given situation and to try to understand it. To identify and find significance in singularities. So our concepts, per Weber, help us identify the significant features of individual situations. So, for example, in the security project, we are not diagnosing a general state of security today. Rather, we have an analytics that lets you see how particular elements are in motion in particular sites.*
- GM: ALSO THE EXPRESSION HERE IS QUITE CONVENTIONAL -- ANALYTIC DISTINCTIONS, BINARIES -- LOOKS A LOT LIKE WHAT SOCIAL SCIENTISTS DO -- SO AGAIN, MIGHT BE MISUNDERSTOOD AS A REINSTITUTION OF THE CONVENTIONAL IDEA OF SOCIAL SCIENCE INTO INTELLECTUAL ENVIRONMENT WHICH HAS BEEN QUITE A BIT MORE TURBULENT IN RECENT YEARS.
 - ARC: *Can you say what you mean by “the conventional idea of social science”?* We do think that there is much to learn from the other social sciences, even if we don’t ultimately share their aims and techniques (this point is expanded upon in our “What is a Laboratory” paper (Collier, Lakoff, Rabinow, 2006).
 - *We have certainly learned some of the lessons of the reflexive turn, but feel that it needs to be taken in a different direction. Obviously we are not interested in doing naïve, objectivist social science. One difference from*

at least a clichéd understanding of conventional social science is that we don't think we are producing objective truths about the world but rather analytics that identify significance. But here is where we think that more reflection on the kinds of claims anthropologists make would be helpful. After the "reflexive" turn, anthropologists have not stopped making claims about the world. Our question would be: To what extent are these claims accompanied by a structure of accountability or responsibility: that is, what are the norms of adequacy, conceptual coherence, or adherence to something like "the empirical" that make these claims valuable contributions to thinking about the present?

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

- GM: OK, THEN, THIS IS THE CRUX-- 'WORK' INDEED -- HERE IS THE ETHNOGRAPHY AND THE DISTINCTION OF THE ANTHROPOLOGICAL FORM OF KNOWLEDGE... SO, IN PAUL'S TERM -- ONWARD, LET'S HAVE MORE JUST AT THIS CRUCIAL POINT.
 - *We have tried to outline above how the concept work and empirical soundings lead to progressive conceptual clarification and reframing of significant problems. That feels to us like a lot – indeed, since we don't believe in "theory" it seems like the most one can ask for. Another example would be the importance for us of the three-fold analytic that we developed between vital systems security, sovereign state security, and population security. For us, this opened up and organized a vast field of empirical problems whose significant interconnections were not clear to us before. Although we are still working on how to explain this distinction to various audiences, we think it is extremely significant and diagnostic. We have outlined some of the ways above.*