
Paul Rabinow

Interviewed by Frédéric Keck

Frédéric Keck (FK): You say you are doing fieldwork in philosophy. You are perceived as a philosophical observer in the domain of biotechnologies. You studied philosophy and anthropology at Chicago and then in France, where anthropologists often have a philosophical background. What were your influences in these two disciplines, and how do you see their relations?

Paul Rabinow (PR): I was interested in French philosophy and philosophers from when I was quite young. For some reason, Jean-Paul Sartre and Paris had intrigued me. He was kind of an intellectual fantasy hero for me even before high school. Who knows why? But in any case, it was something about the life of an engaged intellectual that was seductive.

In New York, I attended a science high school, Stuyvesant. Even if I enjoyed that, the math in particular, it was also clear that there were real geniuses in math and I wasn't one of them, and, furthermore, there was a limit to how interesting it was after a while. So by the time I went to the University of Chicago, I took a little bit of math—some advanced algebra—but I knew it wasn't the direction that I should pursue.

At Chicago there was a very broad and structured curriculum, which didn't assume that there were very sharp boundaries between disciplines and in overall knowledge. For instance, there was an obligatory course on non-Western civilization, so I chose Indian civilization, and it was taught by historians and anthropologists and philosophers and poets and linguists and Sanskritists. That approach is very much against the grain of the current American view of things, which holds that you should specialize right away, and you better start competing in whatever you're doing, or you're never going to make it.

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Along the way, I liked anthropology, starting with this Indian civilization course, and professors like Barney Cohn and Milton Singer. But my philosophic interests continued, and in my second year I stumbled into a course with Richard McKeon. I was taking Introductory Tamil and there were two people in the class and two professors: one was a Brahmin and one was a lower-caste person. The other student was a missionary who had lived in Madras for ten years and already spoke Tamil. I dropped that course and was sitting on the grass in the campus wondering what else to take, and a friend suggested this guy McKeon, who was teaching a course on the history of ideas and methods of the natural sciences. And that was the opening to a process of being engaged in an idiosyncratic philosophical tradition, which was simultaneously the history of philosophy and pragmatism. McKeon was famous for being an Aristotle scholar, but also always asserted that he was a student of John Dewey and that philosophy was supposed to be pragmatic. Now, it was never very clear to me how what he was doing was pragmatic, but it opened the perspective that raising broad philosophic questions should be in the world and also that one could go back to the Greeks because they were still relevant today if you thought things through in a certain kind of way.

I majored in anthropology. I was, and still am in many ways, alienated from the United States, and anthropology had the double attraction of making sure you got out of the United States but also out of the libraries and the super-academic debates within the disciplines. Anthropology at Chicago was very dynamic, with very smart people, very good students. Barney Cohn and Nur Yalman were encouraging and, to a degree, became intellectual mentors. Clifford Geertz was the superstar, and I guess I made the mistake of wanting to work with the big star. In any case, he was a very smart, complicated human being, and I went in that direction. Nur Yalman convinced the faculty to let me take the doctoral exam as an undergraduate, which had never been done before. I got the highest grade on the doctoral exam, so they were in a quandary about what to do because I had already finished my graduate coursework, but I was a Chicago undergraduate, and they had never previously admitted one into the graduate program.

We worked out a compromise, that I would go to Paris in 1965–66. I attended the seminars of Claude Lévi-Strauss, Louis Dumont, Jean Hyppolite, and Jules Vuillemin. It was wonderful both to be in Paris as a young man and to hear these extraordinary people lecture, and to watch what that level of high intellectual life was like. At that point there was no sharp distinction between the kind of philosophy that was going on in Paris and anthropology. Dumont had one version and Lévi-Strauss another, but neither of them would say, “You shouldn’t read philoso-

phy.” The same broad approach applied to Hyppolite as well—his understanding of the dialectic and of the history of philosophy wasn’t Marxist in a political sense, but it had overtones concerning its place in the world and the high stakes of thinking. I came back with a sense that I had the freedom to do anything I wanted, and Geertz was open to that. I wasn’t deeply socialized in the discipline the way people generally become in graduate school. And also I had all my friends from college, and I knew Chicago very well and I was very comfortable there, and McKeon was still teaching. I was fortunate to take a seminar on the preface to Hegel’s phenomenology from Hannah Arendt; I attended courses and lectures from Raymond Aron, Leo Strauss, Hans Morgenthau, and others. So I had a formation in which philosophy, anthropology, and experience were not separate.

It was only much later that I got to Dewey and took Dewey’s philosophy seriously to find conceptual tools, what we now call equipment. Neither McKeon nor Dewey ever really did the kind of inquiry I think anthropologists do, or some anthropologists do. Yet there seemed to me to be a good fit in the imperative to do both philosophical inquiry and anthropological participant observation together. Participant observation as an experiential, existential dimension of thinking is still very valuable, but I think that a lot of the conceptual work that interests me is not very common in today’s discipline of anthropology. It’s tens of thousands of people doing specialized things and with a lot of emphasis on humanitarianism and the NGO [nongovernmental organization] world and international agencies. It’s not to say that it’s not valuable; but it seems, broadly speaking, not very conceptual.

FK: What attracted you to Richard McKeon’s teaching was the possibility of linking the history of concepts and anthropological fieldwork. Does that mean pragmatism is something you rediscovered later in his teaching?

PR: McKeon’s perspective was that there was never going to be a comprehensive philosophic system. His argument was that Aristotle, Plato, Democritus, and the Sophists had to be taken together as differing from each other in complementary ways, that there were common problems that could be taken up and enriched by this pluralism—this is the Deweyan part (I later realized it was close to Hans Blumenberg). The answer to understanding these problems in the world wasn’t going to lie in a single system. You could use the systems to help you develop concepts and, later, equipment to investigate problems. There was a pluralism of systems from the very beginning of Western philosophy, and there was never a

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definitive convergence around a single system: the only time that happened was under totalitarianism. This orientation strengthened my antitheory, proconcept side and opened a quest for the anthropological experience of difference. I want to be a rigorous pluralist, however, not some “everything is fine, whatever they do is fine, whatever you think is fine” person.

FK: If the history of concepts brought you to fieldwork as a mode of inquiry, would you say you were disappointed by this experience?

PR: When I came back from Morocco in 1969, it was the period when, everywhere, but certainly in the United States, there was a lot of political turmoil around the Vietnam War. Some of my friends from college were in SDS [Students for a Democratic Society] and had become quite radical. There was a lot of talk about colonialism, but no one was actually very interested in colonialism. So here I was, having just spent a year and a half in Morocco, and I stopped in Algiers looking for the Black Panthers on the way back. And then a bit later on, I learned some Vietnamese. The people on the left were anticolonial, and they read Fanon, but they weren't actually interested in the colonies and the colonial experience. That social fact was crystallized most dramatically for me when a group of us came back from an antiwar march in Washington, and we all went to see *The Battle of Algiers* for a midnight showing. I knew enough Arabic at that point to know that the subtitles were wrong: they were Sartrean subtitles, but the Arabic was talking about religion, and for my friends that was totally out of the picture. So I deeply embraced the anthropological practice and experience, and I found it something that not many other people were really very interested in. I didn't want to stay in the Middle East for a range of reasons: it seemed stagnant and conflictual, and there was a lot of anti-Semitism, and while I wasn't a Zionist, I also was not an anti-Semite.

I wrote this book of reflections on fieldwork. Everyone says you're not an anthropologist unless you've done fieldwork. Well, why not conceptualize that? That boundary was heavily policed, however, and the only person who was allowed to cross it in a serious way was Lévi-Strauss. In the United States, that was considered to be some kind of highfalutin French nonsense. But everyone held on to the centrality of the fieldwork experience in anthropology. So I wrote a book whose chapters follow Hegel's *Phenomenology of Spirit*, which everyone takes to be personal, but its real theme was a conceptual exercise about the unfolding of knowledge and experience. Geertz told me it would ruin my career if

I published it, six presses turned it down, and in a certain way it was like the colonialism experience—untimely. I was saved by Robert Bellah, who got it published at the University of California Press, and one thing led to the next, and here I am. But it also led me to understand that a certain kind of philosophical work was still not very welcome in the social sciences, and a certain kind of anthropology and interpretive social science was not very welcome in philosophy, in the sense that American philosophy was increasingly analytic philosophy. The story was somewhat different in France because people like Pierre Bourdieu and others were, in their own way, open to these combinations.

Reflections is the only book that I know of which has polemic prefaces and post-faces. Bellah originally wrote one, and Bourdieu hated it and attacked Bellah and Geertz in his response and then instructed me on what I really should have done, which I found very amusing but still serious. And I would say that the structure of the book is conceptual, and it's about objects in the sense that different forms of subjectivation in knowing become objectives that the knowing subject must deal with.

FK: *Reflections on Fieldwork in Morocco* was considered part of the reflexive turn in anthropology. Your next book, *French Modern*, was more concerned with objects, which was also a way to write about your fieldwork from the perspective of the archives.

PR: *French Modern*—I wanted to go to Vietnam, but it turned out an American couldn't go to Vietnam after the peace treaty, even if French scholars could. So I was in a quandary as to what to do at that point, and then I decided to think about archival work. In between, Michel Foucault came to California in 1979, and that is a story I have told elsewhere. I was in the early stages of this project, and so it was really in many ways the encounter with Foucault that led to the idea that one could do a genealogy of the social. As it turned out, the group of architects who had worked in Morocco and done a great experiment in urban planning had also worked in Vietnam and Madagascar, and a little bit elsewhere; there was a kind of thematic actor connection among these things. But the book was really about the genealogy of the social.

FK: Looking at urban structures was a countermovement toward the *Writing Culture* movement, which was more introspective and literary?

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PR: In the group of *Writing Culture* in 1984–86, the two outsiders were Talal Asad and I. And it's not that we were totally opposed to what was going on. I think what Jim Clifford and Jean Jamin were doing was very interesting and important, although it never really developed in a full way of art and writing. I had taught for a few years with Clifford; we had a seminar where the Berkeley group would go down to Santa Cruz one month, and he and his students would come up here one month. We had Foucault and Edward Said, Hayden White, and a number of very interesting people attend. I was interested in that discussion about form and writing and subjectivity but always had a somewhat different angle from Clifford, and then from Marcus and from some of the others. So whereas I was sometimes identified with *Writing Culture*, there was some creative tension there.

FK: In *French Modern*, you stress the role of the specific intellectual, a term you borrowed from Foucault, in the production of norms. Does it announce the work you did later on the Human Genome Project?

PR: *French Modern* was a genealogy of the social. I was convinced that anthropology and philosophy in a broad sense needed to be nominalist, that is, to take supposedly universal categories and historicize them. After I had done that with the social, François Delaporte and Georges Canguilhem suggested doing the same thing for “life.” And, hence, the projects are not quite as distinct as they seem, although there was a shift certainly in the object and in the way one went about exploring it. This was the time, around 1990, in which the genome-sequencing project was being organized. The Bay Area was a center of that activity, as well as of the new biotechnology industry. . . . The idea that life was not a universal category, that it could be nominalized and historicized, watching it take form *here*, seemed like a promising zone of inquiry.

I was very interested in spaces other than the university in which truths about life were being invented, experimented on, and put into practice. I've always had a somewhat less than enthusiastic relation to the university—and the biotech industry was definitely an interesting place in those years, the way in its early days Google was and other places were later. There was collaborative work going on, and discoveries were being made, and they welcomed me. There was a kind of participant observation, or the possibility for a philosophical observer.

FK: How did you enter the labs in California? How was it different from going into villages in Morocco? What is the role of a scientist like Tom White in your discovery of the world of biotechnologies?

PR: In addition to the general context of the genome-sequencing projects and the rise of the biotechnology industry, there was a big political fight in this department. Vince Sarich, a professor of physical anthropology, was teaching an introductory course with eight hundred students. He had made some important genetic discoveries that clarified the understanding of the biological clock of mutations, so he had scientific legitimacy, but he was teaching these undergraduates that homosexuality was genetic, that blacks were less intelligent, et cetera. Some students walked out and protested, and there was a panel in which he and Nancy Scheper-Hughes were screaming at each other. Sarich could care less about what she said; broadly speaking, Nancy was saying the right things, but she was saying them in such a way that it was hard to see what you could do with them. I was sitting in the audience with Tom White, whom I had met through some friends, and we both agreed that another position had to be invented. The question I wanted to ask Sarich and Nancy was, what locus on which chromosome are you talking about? And since I knew they couldn't answer that question, the idea was to move into the truth domain, "dans le vrai," in a way that was serious. It seemed possible at that point, less possible now, that there were important genetic determinations going on in various instances of schizophrenia or whatever. And therefore, as critical intellectuals, we should know enough molecular biology, just as we needed to know enough Arabic to be able to be seriously critical about what was being talked about on colonialism. So that led Tom White to invite me to work with them at Cetus Corporation. He said: "Well, my wife, Leslie Scalapino, is a poet, and they're reading *Writing Culture* in their reading group, and you're the least chauvinist of the group among the men, so she said it would be okay if I worked with you. You don't have to explain what you're doing; I know what you're doing. Why don't you come observe us?" So that's how that happened.

FK: After doing a genealogy of the social in *French Modern*, you forged the notion of biosociality, which became quite successful. You proposed to describe forms of the social in relation to specific knowledge about genetic locations.

PR: I wrote that at the beginning of the research, very quickly, which sometimes happens. It was written against sociobiology, and Sarich and those people: instead of fantasy genes, a set of genes determining evolution and who we were, there was new knowledge about the correlation between diseases and genes, which was going to be taken up and used by people who were supposedly affected. With the AIDS epidemic, it seemed to me that the disease groups would be influential in shaping the way genetic information would be used from the sequencing project.

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I never actually pursued this concept very much, but a lot of other people have. When I came to France to work at the CEPH (Centre d'Etude du Polymorphisme Humain), I realized that the genome project was organized as a biosocial project, which was not true here and not true in Iceland or in England.

FK: What you observed in Iceland was not properly biosociality; you call it “moral landscapes.” Is it a different idea?

PR: DeCode’s project in Iceland seemed like a total social fact because of the unique situation of Iceland, with a small population, an even smaller population in the past, and very good health records kept by the Danish health administration when Denmark was the colonial power. The argument of DeCode’s director, Kari Stefansson, was that this conjunction of population genetics, medical records, and sequencing was going to reveal massive numbers of deep truths about all the genetic bases of health and pathology. At the time, it seemed like a plausible claim.

It was attacked by bioethicists both in Europe and in America. For example, when I asked Henry Greely at Stanford, head of the bioethics department, “When are you going to Iceland?” he said, “Why would I go to Iceland? I’m busy.” He believed firmly that he already knew what the moral issues were. One of the main issues, which the European bioethicists emphasized, was whether you were automatically part of the population study and had to opt out or whether you had to opt in first. The ethicists felt this was scandalous. I was in discussion with some friends, Allan Pred in particular, who pointed out that in Scandinavia, you’re born into a church, but you can opt out; there was apparently nothing unethical about the idea that you were born into a large institution, and then as you come of age and thought about it, if you want to opt out, you can opt out. So this condemnation seemed like cultural imperialism of the supposedly universal European and American perspective, whereas if one thought about what was going on in Iceland, when people want to opt out, they had the right to opt out. There was nothing unethical about it, any more than demanding if you were born into a church, which they always neglected to say because that would be politically incorrect. But if you were against that, you should have been against the church, too; that would have been a radical position, but the ethicists are not radicals; they’re moralists, and they are busy; they know the problems and most of the answers already.

They had no interest in anthropology whatsoever; they already knew because of their moral theory that the project was going to lead to all kinds of horrible things. The fact that ultimately Kari Stefansson as well as the EU experts and

Stanford professors turned out to be wrong, again, doesn't seem to interest anybody. It got so polemical that I decided that the consequences for the Icelandic people were too high and that as an outsider I had no real stakes in the matter, and therefore Gisli Palsson [professor of anthropology at the University of Iceland] should be the one to take up the main writing, and he has. Thus the project neither achieved its ambitious original goals nor produced any Nazi-like ethical consequences.

FK: You also found this tension between ethics and life sciences in France, with the Comité Consultatif National d'Éthique. How did your reflection on pragmatism inform your participation in the debate about bioethics?

PR: I considered ethics, in part from pragmatism, but in part from Foucault, as a practice and not a theory. And this was also a period during which Foucault was frequently in California, the period of his lectures in the last three years of his life, which we are still working on. And it seemed to me that, as a philosophically oriented anthropologist, one needed to know how things were being problematized in Iceland or in France. And they were very different in those places. The idea that because the genome sequence could be found on your computer it was being problematized ethically and politically and scientifically and commercially in the same way was empirically not true.

FK: When you entered the field in California, Iceland, or France, you answered a demand for ethics. How did you reframe this ethical demand?

PR: Well, that's a tough question. In the United States, the ethical demand seemed to me in part a commitment to something like the truth, "rester dans le vrai," as it seemed that a lot of the critics of the genome project, as well as a lot of its supporters, were not *dans le vrai*. Some were saying, "This is the secret of life"; others like Troy Duster would say we're about to become Nazis through the back door; Jürgen Habermas was also afraid of the Nazis. And it seemed to me that that had very little to do with what was going on, because what was going on was being reproblemated, and knowing beforehand what the problems were and what the ethical stakes were did not lead to good scientific work. I think that anthropology is a distinctive science. It is based on inquiry that yields a form of knowledge that leads in a haphazard way to a kind of testing and further experimentation.

In France, Daniel Cohen invited me to come in as a philosophical observer and gave me a free hand to do anything I wanted there; the ethical committees really

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had nothing much to do with them. The really interesting work was with the AFM (Association Française contre les Myopathies), which made a very heavy bet that the dystrophies had a genetic cause, which they seemed to (and eventually did), and that that would be identified before you could arrive at a cure. That seemed to me an ethical use of science, but the truth claims remained to be established. They all *hoped* that finding some of the genes for some of the dystrophies would lead to finding a cure, but it hasn't, even though a lot was learned about the genetic mechanisms involved. And so the AFM shifted to gene therapy, and so far it hasn't been successful. That seems to me honest and ethical but not moralistic and also opposed to actor-network theory; the mere mobilization of large networks of actors and things doesn't necessarily work. And hence there is a truth component and an experimental component, which I'm very committed to, which I find absent in some other cases.

FK: This ethical demand is framed differently when there are concerns over biosecurity. Anthropologists are asked to talk about the social consequences of scientific discoveries when they can be used for terrorist purposes. When you entered the field of biosecurity, after 2001, particularly as it organized research on synthetic biology, did that involve a different type of participant observation?

PR: The work that I have done with Gaymon Bennett and Anthony Stavrianakis was centered on that issue. It was equally untimely, equally frustrating, but, nonetheless, we've done the inquiry that no one else has done. The security issues had two important dimensions. First, is there a security risk? The answer is yes, even though a lot of people don't want to hear that, or they want to hear it in Washington, but they also want to know it's under control, hence "dual use." We have a whole chapter on why "dual use" is a figure that's profoundly misleading for a number of reasons. "Dual use," among other things, enables the bioscientists to think that they have no responsibility except to do what they're doing and that somebody else is supposed to take care of security and ethics, and they just go about their business. That business in America, but also increasingly elsewhere, is now about entrepreneurial activity, about patenting, about raising money. If there are evil Muslims out there, that's somebody else's job, but the very claims of synthetic biology being able to engineer life, which are dubious claims, are not examined. We talk about ramifications because scientific research and social consequences are not separate, not predictable, not known, and there is uncertainty involved.

Second, the line we were then advocating was to think about preparedness. A capacity for preparedness should be developed in the scientific community, not least because the freedom of research and thought in the academy is under threat. It certainly was under the previous administration, and it probably is in this administration, such that if there were to be a biosecurity event, whether accidental or not, there would be a real reduction of the freedom of research, which is already being highly restricted by commercial interests and the amount of money required to carry out some of this work.

We were excited and in favor of seeing what synthetic biology was going to do, and we were the only human scientists who were close to it for the first five years. We were in continuous, close contact with what was going on in the labs. We made many attempts to engage in discussions, with only minimal success. This was a period of intense consolidation of the brand of synthetic biology; money was flowing from Washington, and while the molecular biologists were almost always cordial, they had neither much interest nor time available for in-depth exploration. For example, we tried for several years to introduce the topic of preparedness, but it was resisted.

At one level, the mode of subjectivation of the bioscientists was not unexpected. What we were more frustrated and angered by, however, were the responses of the human and social scientists. For example, we were explicitly excluded from the Presidential Commission for the Study of Bioethical Issues, strange as it may seem, because we knew what we were talking about. Rather, they wanted general ideas with which one could continue to produce clouds of discourse. Our colleagues in the ethics and human sciences connected to synthetic biology projects in England and France as well as in the United States have told us off the record that they found our work to be good and true. They would not say this in public because they were afraid their funding would be cut off. While the US biologists were basically indifferent, the audit society in England was not, and people felt they couldn't speak openly. Once we got over our irritation at the repeated appearance of this trope, we decided it formed part of our anthropological inquiry. Here is how truth and ethics work in the twenty-first century.

FK: When you worked on synthetic biology, your reflection on conceptual equipment took a new turn: it involved the design of a website, as a way to get closer to life scientists' mode of publication. At the same time, you don't write that many articles. Is it because you don't want to be involved in the economy of journal publication?

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PR: With bios-technika.net, we have a very large set of equipment, which again few seem interested in, but which I think is very powerful. We're rethinking fieldwork as participant observation, in which we understand observation much more in the sense of Niklas Luhmann's term *Betrachtung*, which is as both an intervention and an observation. The reason to do the synthetic biology project was that we were participants, and even if it was blocked, we were not passive. And hence that raises, I think, some of the questions about what comes after the initial stages of participant observation—the kind of writing, the kind of form one gives to one's scientific results and insights changes, because one is in a different mode of subjectivation in relation to what's going on.

Anthony and I have written a book, *Demands of the Day*, in which we're providing a kind of Deweyan conceptual and inquiry-oriented approach to what happens after fieldwork, now that most anthropology is not going mainly to exotic places.

As for the publication of articles in scientific journals, there is much tension there. Since more and more of bioscience is secret, a reassurance from these people is not very comforting. Phil Campbell has opened a debate about the rise of retractions in the major scientific journals. Studies have been commissioned about what's going on; originally, people thought it was mainly plagiarism, and that's apparently true in China. But real fraud exists as well, and some is found in the elite universities of the world.

This discovery does not mean the truth comes out. In England you can retract an article, but if you're asked to explain why you retracted it, you fall under the libel laws, so there's no explanation. In the United States if you retract an article and are asked to explain why you retracted it, the patent officers get upset. So we're in a situation in which scientists are increasingly not in a position to speak the truth about some of the broader instances of what they're doing, and in fact they're learning not to speak the truth, so it seems to me others of us ought to be asking some of these questions.

The first article I attempted to get published was as an undergraduate, when, with the encouragement of Nur Yalman, I wrote a review article on Lévi-Strauss's *Mythologiques*. I submitted it to the *American Anthropologist*; the editor wrote back saying it was well done, but they were not interested in the topic. I have tended to stay away from journals since. The political correctness, identity politics, and proper tone in American journals hold little interest for me, so I write books and give interviews!

FK: The ethical questions of life sciences are often framed in the language of risk. The concept of biosociality was about risk taking and pushing for science to know about risk, whereas in biosecurity risk appears as a generic threat that limits scientific research. Would you say that there was a shift in the language about risk from the 1990s to the year 2000 and that this shift could be captured in the turn from biosociality to biosecurity?

PR: Remember that risk is a concept that comes from insurance and is based on insurance technology with long series of statistical data. What we're facing now is that there are no such long series; hence risk is probably not the right concept. Uncertainty, and then the Black Swan idea of relatively unlikely events having massive ramifications, is more the arena we're all interested in thinking about. The security apparatus and the biosciences still think through this whole question of risk, but risk is a different kind of intelligibility. And hence they're not facing up to very much of anything, and so far, as far as we know, nothing's happened. But if the idea is that the Black Swan event happens, the ramifications will be major.

FK: How did the knowledge you had about genomics help you to understand what scientists were doing in synthetic biology? How does it enable you to say what could happen—if something happens?

PR: Their claim, which is a plausible one, was, what do you do once the genomes are sequenced, and, furthermore, once the genome sequencing in human beings but also in other organisms doesn't actually provide the Holy Grail, the key to life? The overwhelming majority of the leading bioscientists entered a pool before they announced how many genes there were, and everybody was wrong (except Jean Weissenbach in France), predicting between 80,000 and 120,000 genes. It turns out that there are some twenty thousand—odd genes, so the whole understanding of the gene was wrong. Then they discovered multiple species of RNA, interference RNA, all kinds of RNA, which is actually doing a lot of the work that people absolutely arrogantly said five years before was done by genes. So the scientific understanding of what an organism is and a genome's place in it has changed, and it's not just an information game of reading off DNA sequences. As Craig Venter argues: If nature itself is swapping genes and manipulating genomes and reworking itself, bricolage—like all the way through—why shouldn't the boys and girls of the biosciences do that as well? So the move from

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sequencing to synthesis can be seen as both the failure of sequencing to provide all the answers it had hoped for and a quest for new technological possibilities to do some of the things that the previous genome people claimed was about to become available.

FK: Would you say that synthetic biology is not only hype, because you could see what specific knowledge it relied upon?

PR: Well, there is no doubt, from Max Weber forward and others since 1917, that modern science is technological mastery and manipulation of matter, which in Weber's view leaves questions of significance and meaning aside. This is the trajectory the biosciences have very much followed. Now, to take for granted that they can manipulate the whole genome seems to be totally wrong. Obviously, they can manipulate it, but in what way, and where it'll work and where it won't work seems to be something ethically, anthropologically, philosophically, and scientifically of interest. There have been two big claims so far, that they were going to cure malaria, which is long delayed, and produce biofuels, which has stumbled over questions of scale. On the other hand, many technologies of manipulating DNA are being experimented with, and definitely things will happen from that. *What* things we don't know. All the hype about "we're going to engineer life"—I think as anthropologists we might want to be more specific than that, and then we might know what the ethical issues are. So there's a broad ethical issue of the fact that people are not speaking the truth, but there are more specific ethical issues about what it is that we should be concerned with, and we don't really know what that is yet.

FK: In *Demands of the Day*, you follow the ontology of these new objects that are made by synthetic biologists. And this goes back to questions you had before, about emerging forms. How did this idea of emerging form change when you looked at the ontology of synthetic biology?

PR: Foucault makes a striking distinction between ideas, values, and forms; almost everyone in the social sciences wants to talk about either ideas or values. Most of the ethical debate is about values, which I think is linked to consumer capitalism and is not what ethics is about. Ideas are mostly what historians do: history of ideas. Understanding and creating forms is a different sort of challenge: What are the forms in which knowledge and ethics are practiced? It's a different kind of enterprise, and it seems to me that anthropology might well be suited to

exploring these projects, but also to questioning itself as to what form anthropology has given to its knowledge in the twenty-first century. The monograph is less appropriate for anthropology because culture and society are not the comprehensive terms anymore. And hence the question of form in anthropology seems very present.

This change is linked to the necessity of collaboration, which already exists in the biosciences. No one would think that individual researchers can work by themselves. We still have a nineteenth-century view of science: producing our little genius works in a monograph in the tenure system, and promotion is still based on it. But my view is that if that continues, then either most of the nineteenth-century social sciences will disappear or they'll become historical disciplines. But we're not trained to be historians, and, in any case, recent history is not what historians are trained to study. So there's a big void there. If you take ten years to do a piece of fieldwork, write a thesis, get a postdoc, get a job, get divorced, get married, et cetera, your original research data is out-of-date.

FK: Do you think that the forms of social sciences should be transformed in relation to the forms of life sciences?

PR: No, in relation to the world. If you go to Google, you'll see that they take that very seriously and have for a long time. Genentech did it first; Google's working on different objects. But the idea that everyone at Google is working in a cubbyhole by themselves is laughable. They work in teams; they work at an accelerated pace; they understand innovation as a central part of this. Now, that's not to say that we want to imitate Google, but that's partially the type of object that we're claiming to understand. So it's the life sciences, but it's everything: it's finance capitalism, it's new epidemics, it's a whole range of topics and objects that anthropology can either address in the twenty-first century or it will disappear or become antiquated. In order to confront our world, the temporality and form and practice need to change.

FK: I want to ask you about the role of art in your thinking about form. I was struck by the fact that when you talk about nature, you talk about art. Nature is not a notion that you like very much.

PR: Unless it is accompanied by artifice—then it interests me a great deal.

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FK: So you try to think about what it means to say that art imitates nature without a pre-given notion of nature. The two artists you write about are Paul Klee and Gerhard Richter. How did you choose these artists? How did you get involved in the literature around them?

PR: This is the next topic that I'm starting to write about, so come back and I'll give you a better answer in a little while. Paul Klee was someone I've always liked, from when I was actually fairly young, and I don't know exactly why. I never particularly liked Pablo Picasso. Later on, Klee's relation to the Bauhaus increasingly interested me because he was part of one of the collaborative experiments which sought to bring art and craft and industry and architecture into new relationships and theater and poetry and sexuality and dance into a different relationship with each other. The experiments at the Bauhaus were a starting point of modernist experimentation, and the fact that both the Right and the Left hated them made me feel like this was an intriguing experiment. The Bauhaus was the kind of a venue Foucault later called a *foyer d'expérience* [crucible of experience and experimentation]. It is also the case that Klee had a particular relation to form and nature that other artists didn't. If you read [Gilles] Deleuze and Foucault, among others, they point to Klee as a major artist of the twentieth century—not Paul Cézanne and certainly not Picasso. There is also a relation to music—Pierre Boulez wrote a whole book on Klee, which is quite interesting in terms of form and composition.

I don't know quite how I stumbled across Richter. My wife and I saw some of his paintings here in San Francisco at the Museum of Modern Art, and I didn't like them, but there was something about them that was intriguing to me, so later I started to look at them a little more closely. Richter was doing an interesting high-wire balancing act. He had left East Germany, but he had learned skills there, including painting, and about antitotalitarianism. He's obviously deeply involved in the art market, but he can't be reduced to that. He's not a high modernist who's against all forms of representation, but he's not nostalgic. He seems to occupy a very untimely position. He is constantly in motion. And I think the assemblage of the work he does and what he does with his critics, and how he relates to exhibitions, is quite interesting and full of contradictions, which I think that he's aware of but that the art critics have trouble fitting him into their theories. He seems to enjoy and manage their discomfort.

I've grown to like some of his paintings. Richter says he hates nature and writes that nature is amoral, meaningless, and brutal. Yet he has produced a whole series of striking landscapes as well as large-scale paintings of silicon atoms. To

me, he is exploring these objects in a contemporary mode: neither modernist nor nostalgic.

FK: But it's striking that these two artists don't really work with science. On the other hand, they are engaged with the real, like science. Have you thought about working with artists on the kind of science you are interested in?

PR: Well, I've certainly paid some attention to that, but I think it is mediocre art, and it's largely representational or didactic. It's rather banal and meant to shock. I've paid attention to it partially because the synthetic biologists have sponsored some of it; it seems to me part of their apparatus of propaganda as much as anything else. Now that may change over time, and I can't rule that out and I don't know everything that's going on. I think you actually learn more about nature looking at Richter than at bio-art.

FK: If you write about relations between art and nature, does it mean you're moving away from life sciences? Why did life sciences occupy you as a relation between truth and power, and what about it is not satisfying anymore?

PR: Well, I think the original project, that what was at stake was life, seemed plausible in 1990 and in some ways has become routinized. The miracles haven't happened, and the diseases haven't been cured. The genome is *not* the answer to all of biology. In fact, no one really knows exactly what genes are. Yet the anti-technology, antiscience veins in Europe and America are so strong that it frightens me. So it seems to me an ethical obligation to actually know what's going on. I read *Genetic Engineering News* and *Nature Biotechnology*, and I find them interesting. It's part of what seems to me now a *citizen* obligation. There was just a ballot initiative in California to label GMOs [genetically modified organisms], to label food as genetically modified, which lost. All the Nobel Prize winners were opposed to it; I was opposed to it, because I think that all of our food is genetically modified. There is basically *nothing* natural, in the old sense of the word, in anything we eat. It meets health and safety standards, it involves new modes of breeding and selection, et cetera.

It's not impossible that some of these engineering claims that are largely based on engineering new pathways may work. And, in fact, I hope they work in some domains. It would be very nice if they could reengineer the pathways that produce cancer or diabetes or whatever. And maybe they will. I hope they do. But you also

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have an obligation to say, “Well, guys, you haven’t actually done it.” The same applies, unfortunately, with AIDS. A hundred billion dollars has been spent on a virus over the course of thirty years, and they haven’t cured anybody. They’ve improved life conditions, and that’s great. But they haven’t actually cured anybody. We need to think about that.

Our experience with synthetic biology has also convinced me that there’s very little dialogue and that the superspecialization of the sciences and of the humanities and of the social sciences, which Weber diagnosed early on, is at a point now that it’s very, very hard to actually engage across those boundaries in the United States and, to a lesser extent, in Europe. People begin to specialize and to prepare themselves to get into Harvard or Stanford in kindergarten. There is no overall curriculum at Berkeley. And in places like Stanford, there’s somewhat of a curriculum, but it’s mainly how to be an entrepreneurial capitalist in a global setting. So dealing with these people in terms of participant observation and attempted exchange is not very interesting when all is said and done. So I’m looking to do something else. I’ve paid my dues with the life sciences, and maybe I’ll do something else for a while.

FK: And have you thought of working with other scientists in physics or mathematics?

PR: No, because I think the work to be done now is on anthropology, and not on the discipline per se—George Marcus does that—but on the *idea* of anthropology. Not in a philosophical-anthropological sense of producing ideas, but of a *practice* of anthropology of the twenty-first century. And that’s what actually interests me. And what I’ve been doing now may be decoupled from the life sciences. In the United States, there are a lot of different things going on, but the political correctness, identity politics, and many tacit orthodoxies stand in the way of more incisive modes of thinking.

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