

Tactical Biopolitics

Art, Activism, and Technoscience

**edited by Beatriz da Costa and Kavita Philip
with a foreword by Joseph Dumit**

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Interview with Richard Lewontin

Interview by Gwen D’Arcangelis, Beatriz da Costa, and Kavita Philip

Personal Background

Tactical Biopolitics (TB): How did you first get interested in population genetics, and how has this interest shifted over time?

Richard Lewontin (RL): Well, I got interested in population genetics by accident, the way one gets interested in anything. As an undergraduate I worked in the laboratory of a person who had been a student for his Ph.D. of the most eminent experimental population geneticist, Theodosius Dobzhansky, who was a professor at Columbia. So I met Dobzhansky. At one point I had a very bad undergraduate record, and I despaired of getting into graduate school. I thought a way to get in was go to Dobzhansky and he’d take me, which he did. So when I got my degree here [Harvard], I then went to Columbia to work with this famous population geneticist. I had second thoughts about it when I got there. Dobzhansky was not around—he’d gone off to do fieldwork—and I went up to another professor, who was a psychologist, with an idea for an experiment that would determine at what stage in the cell cycle DNA would be replicated. And he said, “Oh, that sounds like a very good experiment; I think it would be an excellent experiment, but if I take you as a student, Dobzhansky will never speak to me as long as I live. So go back where you came from.” So I went back downstairs and stayed as a student of Dobzhansky. And that’s how I came into it.

I also thought, foolishly, that population genetics, studying fruit flies and so on, had absolutely no consequences for human sociopolitical issues. And I was looking at that time to retreat somewhat from my previous political work. I thought, okay, I’ll get into this. And of course that was stupid, because it turns out that it’s very relevant.

TB: Would you describe yourself as passionate about population genetics? Do you feel the same way now as before?

RL: Well, I got into it . . . and it's my professional life.

TB: When did you first become involved in politics?

RL: When I was about thirteen. When I was in high school, the woman who is now my wife and I were founders of a left-wing political group in our high school, but I didn't think of it as having to do with science; it was just, politics came first. When I was in college, I hung out with people from the Communist Party, the John Reed Society—which was the Harvard undergraduate Communist Party—and I was always arguing with people about politics and stuff like that, but I didn't think about it in terms of science. And as I said, I decided I would sort of put that aside for the moment, and that's why I went into population genetics. But as human genetics developed . . . it became clear that population genetics was just as political as anything. I pretty well gave up any political activity when I was in graduate school; I became pretty careerist. And then I came back to it within a few years. I was involved with the [Black] Panthers.

TB: You've been talking about political groups, but were there some science and politics groups?

RL: There was a thing called Science for the People, which started out as Scientists and Engineers for Social and Political Action (SESPA). It was started by a couple of people who were academics. And I was in that from its early days. We would go to national meetings and challenge people like Teller and. . . Yeah, that was part of life.

Science for the People finally went under for financial reasons. Science for the People was a completely anarchist group; it had no membership, you were just part of it. We didn't have any offices. But we did publish a journal, *Science for the People*. What happened was that the people in *Science for the People* had particular interests and particular issues like racism, or like environment, or something like that. And what developed over the years, after the sixties—during the seventies, mostly—were single-issue groups. For example, local people interested in safety and health, or environment, like MassCOSH, the Committee on Safety and Health, and the local environmental, Greenpeace, and all that stuff. So people, instead of working within this general-purpose organization which dealt with all kinds of things—we dealt with workers' health and safety, sociobiology, racism . . . I mean, *Science for the People* had articles written on all kinds of stuff—instead of that, people went to their single-issue organizations, and that left *Science for the People* with nothing.

And so it finally folded. But there are offspring groups. For example, we now have in Cambridge the headquarters of a thing called Council on Responsible Genetics. [See chapter 23 in this volume.] Council on Responsible Genetics was originally the human genetics study group of Science for the People. Science for the People was divided into study groups; I was a member of a couple of them. And the people interested in racism and stuff like that all belonged to the human genetics or the sociobiology study group. Well, the human genetics study group evolved into Council on Responsible Genetics,

which is what we have now, an independent organization with a full-time director and a journal, and [it] gets funding from the Ford Foundation. So, what I'm trying to say is that Science for the People had the politics of the sixties—nonhierarchical, non-official membership, no officers, no central committee—it was a truly participatory, democratic sixties-ish organization. But as the sixties disappeared and people went back to more conventional political organizations, Science for the People disappeared, and in its place arose a number of special groups.

There are certain kinds of political consciousness, some of which last a long time. You have, for example, the Revolutionary Communist Party, which is still Maoist and has [as] its leader Bob Avakian, who is in exile and has been in exile for thirty years or so, and they're on the edge somewhere. We have a number of Trotskyists—for example, the Workers' World, a more conventional party. But then we have a lot of these organizations which are not along the lines of parties because people don't have that kind of political consciousness. They don't want a central committee; they don't want a line that you have to stick to. That, I think, is a major effect of the sixties on radical politics in America. The pushing to the background of organized, disciplined political parties on the Left, and their replacement with a more anarchistic, or more participatory democratic, point of view. For example, I was a member in Chicago, when I was a faculty member, of what you would call the faculty branch of Students for a Democratic Society [SDS]. Now I wasn't really a part of SDS, but it was the equivalent of SDS for faculty members, and we had that same politics—there were no officers; there was no discipline. I once said, "I don't know why you guys are all kidding yourselves; this is a Marxist group." And they really got very annoyed with me. They didn't want to be identified as a particular . . . although they were all Marxists. So that was the change in political attitude which the sixties brought in, and which is still with us.

Science and Politics

TB: Usually most scientists will call things universal in a certain way; and you've been heavy on talking about the historical specificity or the contingency of science. How do you respond to critics who claim that your politics taint your science?

RL: Well, my response is very specific. We can take an example: my struggle with sociobiology. Sociobiologists, Ed Wilson in particular, say, "Well, you just have your attitude about the lack of rigid human nature because that's against your politics, and you're politically committed to a kind of open, changeable world, a revolutionary attitude, and you just don't like any science that is the opposite of that." And my response to that is that that's got the situation upside down. I've spent a lot—now I speak personally—I've spent a lot of my life, a lot of energy . . . I've put myself in difficult positions, some of them dangerous, some of them illegal . . . that's part of my political work. Why? Because

I would like to change the world in certain ways. Now, wouldn't I be a goddamned fool to do that, to go to all that trouble, to put myself in difficult positions, and so on, unless I really thought the world could be changed? It'd just . . . it'd be stupid.

You have to distinguish: well, there's a world I would like, but I'm not a utopian. There are lots of worlds I would like, but I can't have them because I don't think they're possible; I don't think we can get there from here. So whatever political things I've engaged in have been in pursuit of something that I thought was possible. Which means I think that ideas of rigid human nature and so on are baloney. So I have a very, very deep stake in the scientific correctness of that view. If that view is scientifically incorrect, I've been spending my life doing something stupid. So I want to know what's true about nature. It's not that I do the politics and therefore I lay it on Nature. I have to be sure that Nature really allows that.

The opposite is true for people like Ed Wilson, who want to say . . . who are happy with the way the world is. They don't have to know the truth about it, because suppose it's true that people are changeable. Even if they're changeable, they don't *have* to change. So you can make a perfectly consistent argument that the world we have now is the best we can do: "I wanna keep it that way . . . the fact that we could make a different world doesn't interest me . . . I wanna keep it *this* way." So . . . politics—the politics of "keep things the way they are"—in this respect, with respect to human society, does not depend on what is true about human nature; it doesn't have to be true . . . humans could be changeable or not changeable. But the politics which says "I want to change and I insist on change, and I work for change" has got to be based on the assumption that change is possible. So I think they have the argument upside down.

TB: What you were saying about taking a world as fixed versus changeable makes a lot of sense scientifically as well.

RL: Let me give you an example. I wrote a paper, which has been very widely referred to, on the amount of genetic variation within and between human races. Now, I wrote that paper on a bus going from Chicago to Urbana, Illinois. I had a table of logarithms and some big books of data, and I just sat there and did the calculations. I had no idea how that was going to come out. I did not write the paper with the intention of demonstrating that most human genetic variation was within races. In fact, I had the same prejudices that everybody had. Namely, that probably most human genetic variation is between geographical races; after all, skin color, and hair form, and that stuff is pretty obvious, so . . . it's probably gonna turn out that way . . . I was just curious. So I wrote the paper and it came out a particular way. That's an example of what I'm talking about.

TB: Right, you were open.

RL: I even had the initial prejudice that it would come out the other way. I was surprised.

Race

TB: Did it have a lot of implications for race?

RL: It had a lot of effects on people's thinking about race. But you have to decide on what you mean by implications for race. Suppose it turned out that most genetic variation was between races. Not all genetic variation is between races. Indeed, most of the human genome doesn't vary from individual to individual. The question is what you want to use race for . . . is race for you a social construct or a biological one? What this showed is that race is not a very useful biological construct, but I can still make a social construct out of it. I can still say God meant people like that to be slaves, even if they were genetically almost identical. I can say that racism in a social sense or the construction of social racial categories is independent of the truth of that question about genetic variation within and between races. And that applies in the other direction; even though eighty-five percent of human variation is within any local population, that doesn't tell me anything about some particular gene. Even though eighty-five percent is within a population, there must be genes in which most of the variation is between races; and there are a few, like the Duffy blood group, in which nearly all of the variation is between whether you're an Asian or an African or a European. Those are in the minority, but I can't prove any generality. I can't prove by the generality what's true in the specific case until I look at the specific case. So you have to be careful how you use that kind of information.

TB: Did you have a definition of race that you came up with when you did that experiment?

RL: You've asked exactly the right question. To ask how much genetic variation exists between races, you have to decide what a race is. . . . And that was not clear. That calculation has been done by other people based on DNA data and other data. They came to the same value of eighty-five percent within individuals within a local population, but the fifteen percent left over is between populations within a race and between races. And how do you decide which populations belong to which race? And different people got different answers depending on how they defined a race. A local population, you have no problem there. So, for example, when I wrote my paper, I had to decide are the Turks Asians or Europeans? How about the Finns? Everybody says Finns are Europeans, but they talk an Asian language—Finno-Ugric. And Hungarians, for that matter. Finno-Ugric and Turkish belong to the same Ural-Altaic group of languages, radically different from the Indo-European languages that Europeans speak. So where do you put people who come from India? What race are they? Are they Asians? How about people of the South Pacific?

So what I had to do is make up my mind about what I was going to do. So . . . it's just arbitrary . . . I made more races than usual, that's what I did. I took people of South-east Asia, of the Indian subcontinent—Urdu and Hindi speakers—and then put them in a separate group. I made a group of Oceanians, all those people in the Pacific. Then I

made mainland Asians—Chinese, Japanese, Koreans. Then Africa, I pulled all the people in sub-Saharan Africa into one race called African black. All Native Americans into one group, and so on. I mean, you have to do something. And it came out that if you do it that way, about six percent of all the variation among humans is between those big groups. But when other people pooled them in different ways, they got nine percent, because race doesn't have a clear definition. The fact is that, in the United States at least, the social definition of race goes very close to the "one drop of blood" rule. Are you white or black? Well, I'm black. Well, how do you mean you're black? You look white to me. Well, I had a great-grandfather . . . Why isn't a person with one European ancestor and one African ancestor white instead of black? They're just as white as they are black. But under social definitions they're black. There is no definition of race.

Tracing Ancestors

TB: With the Human Genome Project, race is being defined in particular ways. People are trying to trace ancestry back to particular parts of the world . . .

RL: If you wanna do that, you could try to do that. We have a group here. Skip Gates, from the African-American Studies program, has a program to trace ancestry back to particular tribes.

. . . Why do they want to do that? I don't need to be a scientist to know that Oprah has African ancestry, or that Skip Gates has African ancestry. I didn't have to test your blood to know that you have Asian ancestry. So why do I wanna trace it back to a particular place, which is what genomics studies are doing? It's the same nuttiness, if I might say, that pushes people to want to know all about their family trees. Somehow your identity for people depends . . . for those people, not for me! I have my identity . . . One of my favorite stories is about one of Napoleon's marshals, who was asked by a nobleman who his ancestors were. And he said, "I am my own ancestor." "I am the ancestor." You know, you make your life, whatever it is. But I confess that the world is full of people who try to get credit or something or identity according to their ancestry. I think it's crazy. I think it's crazy, for example, when people who are adopted children wanna know who their real parents are. What do you mean, who your real parents are? Your real parents are the ones who brought you up. What do you gain from that knowledge? Except this very funny sense that you don't know how to express your own identity, and that it helps you. But it's irrational, from a scientific point of view.

TB: It could give one a sense of solidarity with a group . . .

RL: But what kind of a crazy solidarity do you get from that? Look, we all came out of Africa; it's just a question of more or less recently. My ultimate ancestors were African, just like yours. People are always doing that, but it doesn't mean that it has some independent, scientific importance or validity. The studies of the genetics of the caste system

in India go all the way back to the 1950s, before anybody heard of DNA. I had a fellow graduate student in Columbia, from India, who . . . found evidence that the different castes were genetically different . . . but of course, they're different . . . because they're isolated genetically from each other because they're not allowed to marry across caste lines. Of course . . . but [so] what?!

TB: But you don't see political utility, though?

RL: No, I think political disutility. I think it substitutes . . . it reinforces an arbitrary division of people along lines which don't correspond to most genetic variation and which have almost a . . . in the end a bad effect because people who are in power, whoever they are . . . I mean, look at the situation in Africa today, where tribalism is producing murdering people everywhere. My tribe is . . . I'm in power, and you're different . . . you're an out-group. It's the biologicization of historical variation that gives people an excuse. Because, look, let me try it from a completely different standpoint so you see where I'm going. There are people in the gay community who want very much to prove that being gay is biological. When I talk to them about it, they say, "Well, we don't want people to say that you're gay because you chose to be gay, because then they can say that you can un-choose." We wanna say to people, "We don't have a choice in the matter; that's what we are. We're biologically gay and there's nothing that can be done about it, so knock it off! It's like having wavy hair."

What they don't understand is, for the political and social forces who want to expunge homosexuality, can't stand it, for whom it's horrible, if they become convinced that it's biological . . . how do you get rid of something that's biological? You kill people. We have the Nazis as the classic example. They said, Gypsies, Jews, it's in their blood; they don't belong to the pure race. How can we purify the world? We have to kill all the bad ones. We can't convert them. So I think that the people in the gay rights movement who are pushing the biological unchangeability and necessity of sexual identification, gender identity, are doing a very bad thing for themselves.

Biological Determinism

TB: So let's talk a little more about genetic determinism.

RL: Look, we need a little history here. Geneticists since the beginning of genetics, in the twentieth century, have been biological determinists. It goes with the job. Geneticists are the ones who keep talking about "genes determine this" and "genes determine that," and "genes make this" and "genes make that" . . . all kinds of biologically wrong things. But they say it all the time. And geneticists are in the everyday business of looking at DNA or doing crosses between organisms and seeing which kinds come out. And they can't . . . they don't want to fool around with issues of physical and social environment; I mean, that just makes life complicated. There are some books over there on the top shelf

which—they're now online, but they used to issue them. There's a big red book there which is all the mutations of *Drosophila* [fruit fly], and there's thousands of those, and descriptions of them. And if you look in there, you will see that every mutation has got the notation RK1 or RK2 or RK3. Those are the rank mutations; every mutation is ranked. A rank 1 mutation is a mutation which, if you've got it, you are absolutely distinguishable from individuals who don't have it . . . the white eye mutation in *Drosophila*. If you're homozygous for the white eye mutation, you have a colorless eye and it doesn't matter what the temperature is, it doesn't matter how old you are, it doesn't matter anything.

A rank 5 mutation is a mutation which, if you've got it . . . under exactly the right environmental circumstances, if you look at the right age, maybe twenty-five or thirty percent of you show the trait but the rest don't. *Drosophila* geneticists don't like rank 5 mutants, because *Drosophila* geneticists want to make a cross of this individual with that individual and see the result and know and be able to identify by the look of the organism what its genes are. If you have a rank 5 mutation, just because you look normal doesn't mean you don't have the mutant gene. So they avoid those. They're listed in the book, but no sensible *Drosophila* geneticist will work with rank 5 mutants, despite the fact that most mutants are not rank 1. Rank 1 mutants are special mutants. And what they never tell you is that before a *Drosophila*-ist would start to do crosses with a particular mutant, they would go to a lot of trouble to make sure that any other genes that might interfere with the expression of that mutant are gotten rid of.

So what I'm trying to say is that if you're a geneticist, you're in the business of studying genes, not phenotype, and the trouble is, until there was DNA sequencing, the only way to study genes was to look at the organism, or maybe its proteins. Then along came proteins, and I spent a certain part of my life looking at proteins. But for most of genetics, between the beginning of modern genetics early in the twentieth century—1910 or so—until 1970, geneticists studied genes by studying organisms. So they have a strong commitment to the view that the organism is made by its genes. Because if you don't believe that, then how can you study the genes by looking at the organism? And they narrowed their investigations down to those cases where there was no ambiguity.

Now, they study DNA, but they've inherited that. So geneticists say that genes are self-replicating. Genes are not self-replicating. DNA can't do anything. The cell makes new DNA by copying old ones. They say genes make proteins. No, genes don't make proteins. The cell makes protein out of amino acid, using information in the genes. But the genes don't make anything. But that constant reiteration of genes are self-reproducing, they make the organisms—that's what geneticists have always talked about. The social consequence of that has been from the early days that almost all geneticists were strong racists and believed that every aspect of human behavior was caused by genetic difference. Almost all famous geneticists were one kind of racist or another, even those who were antiracist, so to speak. Look, a famous geneticist like Fisher, who founded population

genetics, he was a real racist. Most British geneticists were racists. Even people like H. J. Muller, who politically started out as a Marxist, and who would not be called politically conservative, nevertheless believed that genes determined just about everything, and he was a very strong eugenicist.

So, eugenics was a very important part of genetics for a long, long time. There were some anti-eugenics movements around the time of the Second World War when it became known what the Nazis were doing by their racial theories. But that lasted about ten years after the war, and along came Jensen's famous article "IQ and Race," and the thing started up again. So what I'm trying to say is that the people who study DNA, most of them, believe that the genes determine the organism. And you have to struggle against that concept.

TB: So not much has changed, is what you're saying.

RL: That's right. What the Germans did was to make it politically unpopular to be a eugenicist and a racist, but then people have short memories.

TB: In your book *It Ain't Necessarily So*, I saw the same; you were going back and forth with someone that had this revamped version of the brain size argument, except some variables were taken out, put in. It was the same thing, and I was thinking, "Isn't this from a hundred years ago?"

RL: Yeah, so those people still exist. But eugenics is not big stuff now.

. . . Well there's not a big movement to prevent people from marrying or having children based on their genome. It's been replaced by a medical predictive form of genetics, which is where they look at your genes and say, "Well, look, if you have a kid, it's likely that your kid will blah-blah . . . so be careful." But there they're sticking pretty closely to diseases rather than anything else. Nevertheless, you know Mr. Shockley, the famous physicist, supported for years a sperm bank in which people with high IQs would donate the sperm—men with high IQs—and women would say, "Oh I want a smart kid" But I would say—although I don't know that they admit it, so it's a guess on my part—but my guess is the majority of geneticists, of working geneticists, believe that genetic differences are pretty important in determining whether you have high IQ. I bet they wouldn't come out and say it, but I still have a feeling that they do.

Using Science

TB: How do you feel about the prevalent use of animal models?

RL: An awful lot of human behavior is analogized through animal behavior. So you talk about rape in animals, all that kind of stuff that was in sociobiology; you take human behavior and you lay it on animals. An interesting case is—I'm not gonna say this is generally true, because we don't have enough knowledge—the maze-bright and maze-dull rats. It is the case that you can select by selectively breeding a strain of rats that will learn

much more rapidly to go through a maze and another strain that will learn rather poorly. When those rats were looked at, however, a funny thing was discovered; . . . the maze-bright rats, the ones who learned quickly, were partly deaf and partly blind. So it turns out that the reason they are maze-bright is not because they are any smarter, but because, being partly deaf and partly blind, they're not distracted by all kinds of irrelevant cues from outside, so they can pay attention to what they're doing. So you haven't selected for intelligence; you've selected for not being aware of the world around you.

You can select for animals that would be better at doing some job . . . mice, you can select mice to run mazes . . . maze-bright and maze-dull rats . . . that's been used for a very long time to imply that differences in human behavior are consequences of differences in genes. So we don't have any new evidence, better evidence; it's always the same thing. You take some rats; you get them to perform some task; you breed from those who successfully perform the task and from those who don't successfully perform the task. And you keep doing that, and pretty soon you get a strain of rats that are successes and a strain of rats who are not. The question is what have you selected for. And what that has to do with what you and I are doing now. I mean, we're going to make it up. I mean, you say there's smart rats and dumb rats. I don't know what it means to be a smart rat or a dumb rat.

. . . Did you ever take an IQ test?

TB: Yes.

RL: How old were you?

TB: In third grade, really young.

RL: Okay, there you are. I don't know whether it was third, but I took it in elementary school. Anyway, I was sitting in a classroom, sun's shining in the window, the kid are fidgeting, the kid next to you hasn't had a bath, there are noises, little noises, and you are supposed to concentrate on meaningless, contentless questions. Now I had a thought that if most of your senses were dull—you didn't hear too well and you didn't smell very well—you'd do a much better job at it because you wouldn't be distracted by all those senses, things that are coming in. So I think kids who did well, had a high IQ test, were kids who didn't hear too well, and didn't have good olfactory sense, and stuff like that. Well, I just made that up, but that's all I'm trying to say: that the senses are competing with each other for information.

. . . Look, the one thing you have to understand about scientists is that they do what they know how to do. They can't do what they don't know how to do. So they do what they know how to do, and they try to pretend that what they've done is an answer to the question they had.

. . . What else are they gonna do? Say "I don't know"? Scientists hate saying "I don't know."

. . . And what they hate even more than saying "I don't know" is . . . "I don't know how to find out" or . . . "Not only do I not know how to find out, but no one will ever

find out.” Scientists by their training are brainwashed to believe that if you work at it enough, you can know everything.

. . . Scientists are not allowed to say, “You know, there’s a lot about the world no one will ever know.” Not because it’s mystical or spiritual, but because we have not enough of time in the world. Look, our species has been on Earth for . . . I don’t know, a million years. So we have a few million years left to go, and we have only a certain amount of time and energy and money to do scientific work, and I can well believe that we will never understand the human nervous system, central nervous system. Not because it’s intrinsically impossible to understand. We don’t have the time; we don’t have the money; we don’t have the energy; we’re not smart enough. I don’t know how to put it. But the belief that everything will be found out about the world is just stupid.

TB: So the last thing I want to ask you, do you see any positive role for the Human Genome Project?

RL: I can’t actually think of many, except the possibility of finding markers that will be useful for diagnosis.

TB: For disease?

RL: For disease. Other than that . . . As a geneticist, if I were just interested in studying the evolution of the human genome or something like that, then the Human Genome Project is very useful for me. But if you mean useful as humanly useful, I don’t see it. Look, I thought the HGP was a general waste of time. But if suddenly we got it for free, I wouldn’t be against it. The *Drosophila* . . . I’m perfectly happy to know the comparative genomics of different species of *Drosophila*, because I can make use of that in a certain number of experiments. They provide me with experimental tools. But I don’t have to know the whole genome. Now we’re getting to a deep political issue about science, which is that an awful lot of what scientists do is of no use to anybody, and never will be, and is positively bad for people.

What about anthropology? What has anthropology ever done for the people that it’s studied? If I were a Brazilian Indian, why in hell would I want to tell anthropologists . . . and the anthropologists say, “Ah, well, we can tell you your origins,” . . . and I said, “What do you mean you can tell me my origins? I know my origins; I got a story; I’m perfectly happy with my story. Why do I want your story?” This belief . . . that to know everything about the material world is necessarily—except for pure intellectual interest and joy of doing it—useful in some other sense is nonsense. Most of what scientists do will never be of use to anybody.

TB: Rarely said.

RL: You know, we’ve got a museum here where people are doing taxonomy and trying to get the correct relationships between different species . . . Who cares? I mean, I care about the relationships among *Drosophila*, because then, if I know the relationships, I can

use the differences between the genes to make inferences about certain evolutionary processes. But that's not to say that it's of any benefit to people.

TB: Right, personal interest.

RL: Purely intellectual.

TB: So what about gene therapy? Do you think that it's too sketchy?

RL: Two things to say about gene therapy. You know that we do not yet have a single case of success with gene therapy?

TB: I didn't think so.

RL: No, we don't. One of the reasons is an everyday reason, and that is that if you change the genes in a certain number of cells . . . you haven't done it in all of the cells . . . cells are turning over in the body all the time. They're turning over and dying and being replaced by other cells. Now what's happening with these people who have to get re-treated every six months or every year is that the small group of cells that did get transformed don't have any progeny cells anymore. They died, and the other cells took over, and now they're back where they started from.

TB: Oh, I didn't know that.

RL: One thing we know is that cells are dying and being replaced constantly. And if the successfully changed cells die and don't replace . . . for a while they do, but the random chance is that they'll disappear, that cell line will disappear. . . . The other problem for human gene therapy is that we do not have in humans the technology to insert genetic material into a place in the genome that I decide in advance that it is going to go. It is a very important point that has to do not just with human gene therapy, but has to do with so-called genetic engineering, with plants and so on. There are few organisms in which I can put the gene, the introduced DNA, exactly where I want it, using viruslike particles and so on. In that case, I can stick the good gene exactly in the right place, so that the controlling elements are controlling, but if I throw genes at random into the gene, they'll pop in anywhere, they'll pop in the middle of some other gene and destroy that gene's activity. That's the chief danger of genetic engineering. . . .

How do I know when I put a gene in you to solve some problem, it doesn't wind up in the middle of your hemoglobin gene?

TB: Yeah. So how do you specifically place? You can't?

RL: Not in humans, you can't. You've gotta have just the right kind of viral setup and so on. We couldn't do it in *Drosophila* until a few years ago, when a special method was invented. So that now you can in fact. . . . no, I'm sorry, you still cannot put a gene in *Drosophila* anywhere you want. What you can do, is you can arrange to put a piece of DNA in and it'll go someplace, and then you can take out part of it, and you can take out different parts of it so you can see what the effective . . . all in the same place . . . but

in *Drosophila* I cannot have site-specific insertion. Very, very few organisms—in bacteria you can do site-specific insertion. You cannot do site-specific insertion on any higher organism that I know of . . . not in people, not in mice, not in *Drosophila*. So there's a big chance you'll screw up the organism. That's the second reason why gene therapy is bad.

But the main political reason why gene therapy is bad is that only very rich people can afford it.

TB: As usual . . .

RL: But it's worse than most cases. It's the kind of therapy that is extremely expensive, so it diverts possible resources from the real things we should be spending resources on, the things that are killing—well, making most people sick—just for the benefit of one person. And secondly, a lot of it is not gene therapy; a lot of it is trying to make your kid prettier or smarter or something like that. . . . tailor-made babies, right? That's the propaganda! Now, it's not actually being done. But a lot of the propaganda is, make babies to order. Wanna blue-eyed baby? We can arrange that. So, for all those political reasons, the diversion of resources, and for scientific reasons it won't work anyway. And finally there's an ideological problem, which is that it reinforces the notion that we can cure everything, you can live forever . . . It just gives a false notion of a kind of physiological utopia which is not possible and again diverts attention from what we should be doing.

TB: Regular disease, chronic diseases . . .

RL: AIDS . . .

TB: Well, the whole Human Genome Project seems that way . . .

RL: But that's just a way to get money. You have to have money.

Biosecurity

TB: Let's talk about biosecurity.

RL: What is biosecurity?

TB: How do you think the national security climate post 9-11 is affecting how biology is practiced?

RL: Well, I don't think they're having much effect on most of biology. Now, of course, I'm not privy to those particular branches of biology, but generally speaking . . . Look, I go back to think about the way in which a whole variety of security issues and fear of the Communists and so on . . . what effect they had on science in the sixties and fifties, and few people were severely hurt by that. It had almost no effect on scientists in the lab. It really didn't. I know that it's not a fashionable thing to say, but the fact of the matter is that the House Un-American Activities Committee and McCarthyism and all that did ruin some people's lives, but they had no effect on science in general. I mean, I sent for my Freedom of Information Act file, most of which is completely blacked out so I can't

read it. But all the time when the FBI was watching me, I was getting money from the Atomic Energy Commission to do my scientific research. One of my Professors, L. C. Dunn, was a member of almost every so-called fellow traveler group that existed in America, and he was completely supported by the Atomic Energy Commission.

America was lucky, and we're still lucky, I think, that the people that are doing this are not very . . . that we don't have a uniformly integrated State apparatus of the fascist kind. We have individuals who are making political hay by doing . . . but the state is not organized in such a way that there's much constraint on people's freedom to do whatever the hell they want. And that's a fact. I'm not saying it couldn't be . . . but we just don't have that . . . we didn't have it in the heyday of the anticommunist movement, and we don't have it now. So that's one thing.

Now, much of this simple security stuff is a product of the military itself. And so why do we have smallpox in laboratories? If we don't have any out there, then . . . We have it because we're afraid that other people will attack us with smallpox, so we need to develop defenses, and also because we would like to be able to threaten them. So it's part of the counterweaponry that the problem arises in the first place. Anthrax, I mean, why does anybody have anthrax in a laboratory? Anthrax is not a public health problem. Again it's because, on the one hand you want to protect yourself in case somebody else has it; on the other hand you want to be able to threaten them. So most of the simple biosecurity business is a product of the military itself. I mean, I don't want to say we shouldn't have people working at the CDC or even Fort Dietrich on how to protect me against smallpox, because there might be some nitwit out there who wants to use it against me. So I'm in that funny position. If I could get rid of it on a world scale, I would. But if I can't get rid of it on a world scale, why would I not want to develop vaccines and so on to prevent it? I think that's one of those contradictions that have no solutions. You cannot unilaterally disarm from biological weapons unless you are willing to become a tiny corner of the world, which the United States is not willing to become.

TB: Do we need biodefense?

RL: I don't know. This is all secret work. We know nothing about it. If I could rule the world, I'd get rid of it all. But I can't rule the world, and I don't know how to make judgments about it. I don't know how much laboratory space, and money, and time are required to keep up on smallpox . . . or anthrax protection . . . I just don't know. And nobody outside of that system knows, either. We were going to have a very high-level containment facility here many years ago. A bunch of us went and testified. I testified against it; they, in favor of it. They pulled the usual . . . they arrived in their white coats, with test tubes, and said, "We're scientists; you can trust us." And my main claim to the City Council was that you can't trust scientists because they only do what's interesting to them and pay no attention to anybody; so if you really want to be careful, don't let them do it.

Look, this raises a very interesting issue. Suppose Cambridge is going to have regulations about what science of DNA-level technology can be done. Who's going to make the decisions? You're not going to let the scientists make the decisions, even though they said, "You can trust us, after all we're . . ." So you say okay, well, we have to let the public make the decision. So we have to form an outside group. Who are you going to put on the committee? Are you going to walk down to the central square and point at people at random and say, "You're on the committee"? You can't do that because people have to be highly educated in this material before they can make decisions. So therefore you take academics or biologists, but they already have a vested interest. And this is a long-standing legal problem in the United States or anywhere. When you want to have a regulation of something, who do you make the regulators? You have to make the regulators people who understand the technology. Who are the people who understand the technology? People who already have a vested interest in it.

TB: The government.

RL: Or industry. We don't know . . . look, let me just diverge a little bit and tell you a story. Fifteen years ago or so, I can't remember anymore, a group was formed in California using a public interest law firm to sue the University of California because of all the money they put into agricultural research that was a benefit only to very rich farmers, to corporations who were involved in processing food, and stuff like that. And the claim of this group, which I was a participant in, was that the legislation which set up the agricultural experiment station system in California, the State University agriculture school at Davis, was, according to the law, to benefit farmers, farmworkers, consumers. Our claim was that all the research that was being done, did not benefit farmworkers—on the contrary they exploited farm workers, did not benefit consumers, was only a benefit to farmers, and to the richer farmers. And we wanted that to change. That was a wonderful trial. We could so easily demonstrate that the agricultural experiment station, the whole agricultural experiment station system in California, was rigged against labor, and against a huge constituency. And we won the case. And you might be interested to know that the judge in the case was the father of Bob Avakian.

TB: Who's that?

RL: Bob Avakian? He's the head of the Revolutionary Communist Party. But his father, Sparky Spurgeon Avakian, was a judge in California. At any rate, the University of California lost the case. Then came the problem. Okay, the court judges in favor of the plaintiffs: the University of California must be required to do research that benefits consumers, farmworkers, and small farmers. Since we cannot look at every research proposal which would interfere with academic freedom, among other things, we have to have some group that will generally oversee the direction of work. It's up to you, the plaintiff . . . to tell us how we should form this group. We couldn't do it. How did we make the remedies? We couldn't make a remedy which said that we, in particular, will oversee. First of all,

every state in the Union had got some interest in it . . . they filed amicus briefs because they didn't want anybody interfering with the agricultural research they did in North Dakota, or Kansas, or Iowa. And that's where we failed. We won the case, but we failed to suggest sensible remedies because we could not invent a way of forming a judgment . . . a town . . . that would not contain people who were not already deeply interested in the issue. The result was that the judge ruled that it would have to stay in his hands, and he would himself make the judgment. Well, it was simple, . . . and so the whole movement failed in that sense, and nothing changed.

TB: So what do we do?

RL: Well, I mean, now I just have to go back to old politics. When you live in a hierarchical and class society, you're stuck with some aspects of that hierarchical society. We're going back to that whole political issue which I told you about, which we had in Chicago . . . if you had a participatory democracy, that would be one thing; but we don't. There are models. I'll tell you an example of a model at work. The chicken slaughterhouse workers in Canada were getting all kinds of warts and other kinds of bad things from handling chickens. They were getting viruses. They were getting other viral diseases. They went to public health school at the University of Quebec in Montreal, and got a group of the public health people to start giving evening classes to which the workers went to learn all the science necessary for this particular question. They weren't trying to give them Ph.D.s. They were teaching them the science they would need. And they succeeded in doing that, and the result was that the slaughterhouse workers' union was then able to negotiate with the owners of the slaughterhouses along lines that would protect their health.

That's what you need. What you need are interested parties who will be educated on the specific issues, will spend enough time to learn what they have to learn for their own benefit, and then go there and demand . . . Labor unions, . . . when they were powerful, were a very important source of that kind of stuff for industrial health. Workers themselves would oversee their own health, provided they were educated. And so what you need to do is set up workers' schools. Now of course the unions are less and less powerful in America, and I don't know what to do about that. But you see it's part of the whole system.

Let's talk about these containment labs. The head of the lab doesn't do any work in the lab. The head of the lab sits in his office. The workplace remains the place to organize. There are scientific workers in every laboratory. There are people who just do everyday technical work. They are exposed to all of these germs. They should be organized. And they probably know a lot about it; they don't have to be educated. They should organize. It'd have to be organized from the inside. You have to have small participatory groups not from the outside, picked at random from the public. It's important from the standpoint of what you're doing to look at the makeup of government advisory committees on

scientific issues and see who they are. They're almost always presidents of universities, heads of technological companies. They don't go into the lab and ask some lab worker to be on the committee, do they? It's always the people who are running the world who are on those committees. And that's where the real politics is. The real politics is to get people from the bottom of the hierarchy into . . . power, to make those decisions. That's a very heavy political question.

There was a time when unions were strong. But even then . . . when I was in Chicago, I tried to get Walter Reuther, a name which you're probably not familiar with. He was perhaps the most famous and powerful union leader. He was head of the United Auto Workers when they were really big stuff. I tried to convince Reuther and his brother to deemphasize at the next go-round of negotiations an increase in hourly wage and instead make demands about pollution, because the workers were in fact living side by side with the factories in workers' housing around Chicago, over the Indiana line. There were big steel mills and auto plants.

Those workers were getting poisoned heavily by stuff coming out of the chimneys. I wasn't getting poisoned; I lived far away. But they were getting poisoned. And I said, "Look, what you got to do is get the organizing, the negotiating team, and demand investment in pollution control for the health of the workers themselves. Reuther wouldn't buy it, because he regarded wage demands as the easiest thing to do . . . it's not that he was against it in principle . . . he just thought it wouldn't go.

TB: Right, priority.

RL: So we need more; at that time we needed more consciousness-raising among the general public, among the workers and the people, about the dangers of pollution. Fortunately we're not in that position now, because that work's been done. The American public is conscious about pollution. Unfortunately, we no longer have a powerful labor movement.

TB: What about the people representing science to the public? For example, artists, journalists, and corporations?

RL: I wouldn't be too vulgar in my explanation. . . . but it's too easy to say that it's being pushed by the corporations and the scientists are not responsible. They *are* responsible. And the artists are responsible. The artists are participating in that same consciousness. After all, I'm not a scientist; I'm an artist, right? I have to believe what the scientist tells me. Who am I going to believe if I don't believe the scientist? Look, I think this has much broader implications than just the art world. It has to do with the feature articles, and the reporting, and the writing, and the press, and the TV, and so on about science in general. It has to do with science journalism. The New York *Times* has a lot of science journalism. They even have a weekly science section. And the stuff in there is terrible.

I mean, really terrible. Nicholas Wade and Gina Kolada . . . they're awful. They're really awful. They vulgarize everything. They love it when some scientist makes an

announcement, “Scientists today have announced the discovery of a gene which may one day lead to a possible cure for . . .” And they, they’re . . . so I think that brainwashing of the public goes on very, very successfully and constantly through . . . the public media. And, question, what is to be done about it? Now, having bad-mouthed Gina Kolada and Nicholas Wade and their friends, I have to take a step back. I have had for some years active participation, not in the last couple of years, but . . . with . . . the Knight Fellowships at MIT, which are fellowships for science writers. They come to MIT, they study science . . . and I used to go there and give them talks and discussions.

And what I found was that science writers are actually very sophisticated about science. The problem is not what’s in their heads. The problem is this: science articles, in newspapers, magazines, radio, and so on, are in competition for space with other kinds of news. If I’m a science writer and I write, “Well, a scientist today claims to have found the gene that . . . may one of these days lead to some blah blah blah, but you know, they really don’t know much about it, and it’s all very complicated because the environment is important and genes don’t determine anything,” I can’t get that article in the paper. If I want column inches, I have to have something dramatic. And so, by the very nature of print publications and radio and TV, where space and/or time slots are at a premium, if you don’t say something dramatic, you don’t get in.

So our problem is not with those stupid science writers; our problem is their profession is bound by a larger constraint. I don’t think they need to be any more educated than they are; I think they need to be freed from . . . Now we have models. They don’t happen to be American models, but we have models. For example, we have what’s called the *feuilleton* . . . French newspapers have sections called the *feuilleton* section—Italian newspapers have the same thing—in which serious articles of some length are written about intellectual and scientific issues. I write for *Corriere della Sera*, for *La Stampa*, for *Le Monde*. I don’t write often for them, but my friends ask me if I’ll—I don’t write them in Italian—I write them and then they translate them. You talk about the books I’ve written . . . some of that stuff appears in *Le Monde*, which is a daily newspaper in France, one of the big daily newspapers. I mean, these are not little, . . . *Le Monde* is a big paper. *Corriere della Sera* is the largest-selling newspaper in Italy . . . and so on. But they have a tradition of getting people to write seriously about serious scientific issues.

TB: I am interested in your article *Applied Biology in the Third World*. Could you talk a bit about biopiracy?

RL: Some kinds of biopiracy could benefit countries which have very little money to spend on science.

TB: What do you mean?

RL: Well . . . it depends what you mean by piracy. If you believe in the patent system, then if I use something that you have a patent on in my own country without paying for it, isn’t that a form of piracy? The issue is the role of property and private property and

patenting, and the problem again is a historical one. Patents were put into the Constitution of the United States. Why? Well, a very sensible argument. Namely, we want to have innovation, but nobody will innovate if they can't get something out of it. So we have to invent a system which, on the one hand, will allow them exclusive rights for some period of time but not forever, and they're certainly delicately balanced. And if you have a system of private property, . . . then you do have to guarantee to innovators the fruits of their innovation, or something, and I don't know any solution to that problem except . . . to get rid of private property.

The issue is what the right balance is between encouraging innovation and making sure that it doesn't prevent any . . . for example, how will Third World countries get it? . . . Well, the way they should get it is to steal it. I'm a great defender of that. . . . The real dangers to Third World countries are not by marching off with plants and things like that. The real danger is in biotechnology and with putting genes into plants that grow here to make it unnecessary to buy crops from foreign countries. For example, genes that allow soybeans to produce palmitic acid oils, so-called palm oils, which have been put into them. So now you can grow palm oil in the middle of Iowa, but that means that the Philippines, which depends tremendously—a huge fraction of Philippine oil workers depending on harvesting and processing palm oil—have gone out of business. I mean, Third World agricultural economies are being destroyed by . . . Look, much of Third World agricultural economies, except for the food they grow, is for the world market for specialty crops. If I grow a specialty crop in Iowa, I don't need them. So that's what the whole tropical oil thing is happening.

They put the genes for caffeine production in soybeans. Now, it doesn't taste like coffee, but there is caffeine in it. And caffeine itself is a very important industrial product in the United States. It goes into all kinds of soft drinks . . . all got caffeine in them. It used to be that you had to buy coffee from Central America and get the caffeine out of the coffee, but you don't have to do that anymore. So a very important pressure in biotechnology by the seed companies, and in universities, is to develop strains of commercial domestic varieties . . . soybeans . . . which will be able to produce all kinds of specialty stuff, which will destroy tropical infrastructure . . .

So I'm much more concerned with that kind of an issue, than of going to tropical countries and grabbing a plant. That's been going on for a long time. The advantage that these tropical countries had at one time was that those plants only grew well in the tropics, so nobody can . . . the rubber countries didn't care where their rubber trees grew . . . what do they care? And they tried, you know, substituting guayule for rubber during the war, but. . . it didn't work very well, and now anyway rubber is out of the picture. Nobody's into rubber; it's all synthetic. See, that's the other threat to Third World agricultural economies: the substitution of petroleum-based synthetics for natural products.

TB: Okay, so you view that as harmful because of the economic issue.

RL: Well, yeah. If you're not Venezuela selling oil to the United States, and you're Guatemala, or Brazil, which doesn't have any oil, and you used to grow a lot of rubber and you don't grow it anymore . . . it's an important source of income. It's not a source of income anymore. It's also true in Indonesia. Indonesia was a very important source of rubber. Dead. So Third World countries depended a lot on the export of agricultural commodities, which have slowly but surely been either replaced by synthetics or by inserting the genes for important types of production into domestic products.

TB: What kind of audience are you targeting, or are you trying to create, for your work? In other words, what is your ideal of a well-informed layperson engaging with science?

RL: Well, we're really talking about my limitations. My limitations in the ability to communicate are such that I can only communicate with people with a fair amount of education. Now we're talking about not what I would like to be able to do, but what I know how to do. Almost everything I write is only seen by people with a lot of education. All that stuff I write in the *New York Review*, all those books we talked about, *It Ain't Necessarily So*, or *Triple Helix* . . . who reads that stuff? I mean, only college people, college-educated people. Now what I try to do is to at least internationalize it, so it reaches out to other countries. A lot of those books have . . . been translated into a lot of languages. But I'm not kidding myself. I'm not J.B.S. Haldane writing a column on science that the daily worker can really read. It's not true. For one thing, I don't know how to do that, because I don't know where the outlets are. Who's gonna publish science for the citizen, so to speak? *New York Times*? It has its own science writers and its own thing they wanna do. Writing *feuilleton* columns for European papers? But look, *Le Monde*, who reads the *feuilleton* section of *Le Monde*? Not every . . . most people. No, I mean that's a contradiction that I have no way of . . . no solution for. I really don't. For one thing, you would have to crack TV. TV is everything. I don't think the newspapers matter that much. Well, what TV do you have to crack? Not public broadcast. But those other stations don't do that kind of stuff.

. . . So, I don't have an answer. Look, I mean there are some contradictions that exist for me because the society won't . . . they're not changeable . . . without a revolution, it's not a revolution. I mean, there could be a filtered-down effect, I suppose, if you could convince enough people with a certain level of education that what's been given to them is bullshit, that might themselves have some spreading effect . . . But look, I come back to what I said before: the greatest force possible for education of working people was the trade union movement. The destruction of the trade union movement in America is a very great catastrophe in many respects, including that one.

TB: What you're saying about the trickle-down does make sense because one of my friends has just now entered biology grad school and has read your works and was very excited that I was going to come interview you. And he is—in terms of practicing responsible science or trying to negotiate some of these contradictions—he's at least aware. He

doesn't have a solution. He does have that same contradictory impulse; he just likes doing science, the research. . . . He also wants to change things politically. But he has that pull; he knows that a lot of what he's doing is just his own thing. But I have this hope that he'll figure something out; something he'll do will somehow . . .

RL: There's another issue buried there which we didn't talk about. And that's the question of legitimacy. If you write and speak about things that are not part of your professional work, you have to have a certain legitimacy. Now people use that legitimacy to spread all kinds of ideas. Ed Wilson used his legitimacy as an ant professor to spread sociobiology. I use my legitimacy as a geneticist to spread other things. So legitimacy is very important. The only way to maintain that legitimacy is to keep producing science. My metaphor is the metaphor of the bank account. Every time you write something for the general public, you withdraw something from the bank account. And you gotta put something back. If you stop putting something in, pretty soon you're going to go bankrupt. And that's true even for people whose bank accounts are immense. Not me; I don't have such an immense account. But you take people, people who have Nobel prizes. If they don't keep doing science, their bank accounts become empty, because they decide, "Oh, I have a Nobel Prize, so I can talk about anything." And so they talk and they talk, and pretty soon people pay no attention. So that's a very important reason for the politically active person in science to continue to do the scientific work. I do. I'm retired, but I still do my science.

