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Scratch a Theory, You Find a Biography

A Conversation with Troy Duster

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ALONDRA NELSON (AN): Let's begin at the beginning. Can you speak about your formative influences, particularly your family and your grandmother, the well-known journalist and antilynching activist, Ida B. Wells?

TROY DUSTER (TD): My grandmother only comes to me through family oral tradition. I was born after she died. I know my grandmother through my mother, Alfreda Duster, who, of course, spent part of her time in my grandmother's professional world. She was not just the daughter; she was also the person who, as a young child and later as a teenager, was with my grandmother at meetings. And she watched, over the years, as my grandmother became more and more of a public figure.

So I knew my grandmother through my mother's eyes. And they are, you can imagine, rather clear-eyed, but also fogged by family dynamics. The part that may be most relevant to our discussion is that my mother became quite skeptical of being a public figure. My mother saw up close what she regarded as the unfair and improper treatment of her mother by the media and by, as she called them, "envious males," who were in the orbit of this very articulate and powerful woman and began to find ways to undercut what Wells was doing by saying she was in it for her ego.

My mother used to say all the time: "If you plan to have a life in the public sphere, you should do the right thing because it's the right thing. Because if you do the right thing because you think you're going to get rewarded for it—don't even head down that road." Hovering over me for a good part of my life was this notion that you're not going to get rewarded

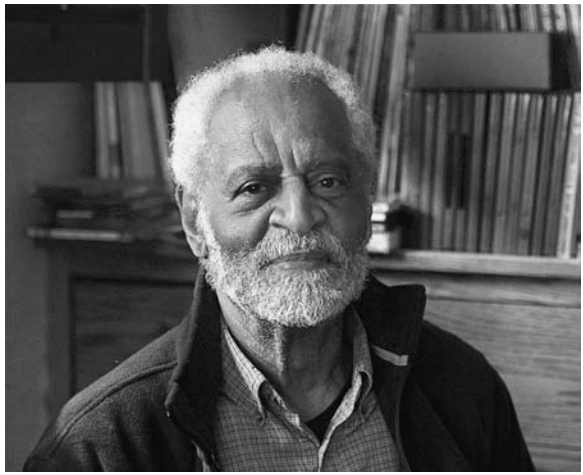

FIGURE 13.1.
Troy Duster. Photograph courtesy of Nancy Rubin.

for being a nice guy or for doing politically engaging social justice activities. Indeed, the more effective you are, the more enemies you will make. The applause may come after you're dead. My grandmother is a figure in my life in the sense that she becomes—what shall I call it—not so much a measuring stick but a kind of symbolic representation of what it means to do good works and, in some sense, to be rewarded for it posthumously.

AN: When you were growing up, were others aware that you were Wells's grandson?

TD: One of my mother's brothers was an attorney who would go around town saying, "I'm the son of Ida B. Wells." My mother really disliked that. So we grew up with the following echo always in our ears: "If I ever catch you taking credit for your grandmother . . . that's your butt. She can be blamed for you, in some ways. But you take no credit for her." So I grew up, all of my youth, not denying that I was the grandson of Wells, but never saying anything. Never. Years later people ask me, "Well, why didn't you tell me?" And I would say, "Mother." [Laughter.] So no, it wasn't at the tip of my consciousness.

AN: Would you say that you had a privileged childhood?

TD: No, I grew up poor in Chicago. My mother, however, grew up in privilege, as the daughter of Wells, this rather well-known public figure. Also, her father and Wells's husband, Ferdinand Barnett, was a figure in his own right. He was an editor and publisher of a newspaper—the first black-owned newspaper, the *Chicago Conservator*.

My mother, Alfreda, went to the University of Chicago. She studied philosophy—she had a bachelor's degree in philosophy. She studied sociology with Robert Park and Ernest Burgess. (Here's where people get that retrospective reading. People like to reread the biography. They go back through your life history and say, "Of course, that was inevitable," when, in fact, it was happenstance. Given this, people think I was destined to become a sociologist. No, not at all!)

My father, Benjamin Duster, had a college degree. He went to Indiana State Normal. State Normal meant you could teach in a high school. My father had that degree, but he really didn't want to pursue a career as a teacher. He preferred to be a journeyman carpenter, a journeyman plumber—he did all kinds of odd jobs. He wasn't of the kind of background that my grandparents thought was appropriate for their darling daughter.

I grow up in poverty because the Great Depression hits; I'm born into it. By this time my father is unemployed and my mother has five children. My father had odd jobs, but not much in the way of income. My mother did inherit a little cottage on the South Side of Chicago—right in the middle of what was to become one of the city's more dramatically impoverished ghettos—Thirty-Second and Prairie. But here's the contradiction—there is economic poverty and there is "cultural capital."

AN: I wanted to ask you about this. We know from the work that Park and his students were doing and from Horace Cayton and St. Clair Drake's important book *Black Metropolis* that there was a significant black elite community in Chicago. You grew up in a family of limited means, but did you have cultural capital by virtue of being Wells's grandson?

TD: My mother was not a part of the black elite. She had been booted out of the black elite community because of her marriage to my father, this journeyman carpenter and plumber.

AN: But is one ever fully "booted out" of elite status?

TD: My mother would have said yes. Alfreda did not have contact with black elites in Chicago until many years later, when she edited and published my grandmother's autobiography, *Crusade for Justice*. Then there was a reentry; there was a welcome. People would say, "Oh, there is our long-lost sister!" [Laughter.] But no, my mother would have said that she was not exactly welcome in black elite circles and that she did not actually aspire to that.

AN: When your mother, Alfreda, attended the University of Chicago, she was one of four blacks on the campus. Had Chicago's racial climate changed

significantly by the time you entered high school and college? For example, did you attend an integrated high school?

TD: My high school, Wendell Phillips High School, was integrated by Frank Wong. He was the only nonblack person in my high school. There was some trauma in my schooling when at age sixteen I went across town to begin studies at Northwestern.

AN: This journey was not merely across town. Evanston, Illinois—where Northwestern is located—is pretty far from the South Side of Chicago, where you were living.

TD: Yes. Metaphorically and literally. I got a real culture shock at Northwestern. I was one of only seven black students and one of the few brown students at the entire university. Moreover, I thought everybody else at the university was "white." I did not realize until my first few months there that there were high levels of stratification inside the world of white people, that there was interethnic hostility between the Jewish people and the Italians and between the Irish and the English WASPs. My first week or two on campus, I'm visited by some student whom I read as white. He says to me, "Welcome to Northwestern. You know, we've got a lot in common." I'm thinking, "Say what? Have you got a history I don't know about?" He tells me that he's Jewish and that he and I share this common bond. This is my first understanding of the appreciation people are going to have about the internal differentiation among whites. Some have asked me, "Is that why you became a sociologist?—because, all of a sudden, you had to navigate a world that was completely different from the previous sixteen years of your life?" There's something to it. But I think it takes a different form, too. In the home, I spoke English as I'm speaking it now. On the streets of Chicago, on the other hand, I had to talk a kind of "jive" talk—the equivalent of hip-hop today. I had to do what I later learned was called "code switching." I did it instinctively because I had to survive, and so did my brothers and sister. But I probably dealt with this more than my siblings because there was more of a gang life when I was growing up than there was in their youth. I had to be able to deal, to get from my house to the school and back. I found myself walking a tightrope between these two worlds. I became bilingual, bicultural, bimodal. At one point, I was also the only black male at Northwestern who wasn't an athlete. Do you believe that?

AN: I do. Yes, I do.

TD: I used to be asked sometimes, "What position do you play?" "You're on a sports scholarship, right?" The assumption was I was either a football

player or a basketball player. I also used to get stopped by the police in Evanston, especially in the evenings. One night one came up to me, and he said, "What are you doing here?" I said, "I'm a student." "What are you studying?" I had just taken this philosophy course, so I said, "Moral philosophy." He said, "Oh, who are you reading?" I said, "Nietzsche and Kierkegaard." He goes back to his police car and lets me alone. About a month later the same situation occurs: the same police officer and his buddy stop me. The buddy says, "What are you doing?" The officer I had encountered before says, "Oh, he's okay. He's into moral philosophy," and laughs. I became known to the Evanston police as this black who wasn't really an athlete.

AN: Were you a sociology major at Northwestern?

TD: No, I had majored in journalism. Journalism for me was what I imagined as a boondoggle: "Am I really going to get paid someday to go out and do what I really want to do, to write?" In my high school I had been editor of the school newspaper, and I did a lot of writing and I enjoyed it. So I get to college and I say, "Okay, what I'm going to do is end up being either a sportswriter and go to all of these games for free, get paid to go out and see these games, . . . or I'll be an international columnist, I'll fly around the world, I'll write about what I think is important." I had childlike aspirations about journalism. What I learned quickly is that editors put you on a beat that's really boring. For example, you'd go to city hall every day and check out the log to see who'd been arrested. So I became quickly disabused of newspaper journalism.

I moved on to radio and television journalism in my third year at Northwestern. I was in the backdrop, writing over and over again the material for the talent. "Talent" was used in those days, and still is in some corners now, as "the voice of . . ." on the radio, or "the face of . . ." on television. That's the talent. It was clear in 1956–57 that I was never going to be the talent. It was like in Mexico or Brazil today, where you just don't find black people as the public face of the media. So I say to myself, "What am I doing? Do I want to spend my life in the back of these newsrooms writing up this stuff? I don't think so." And then in my last year in college, there was a clear event that transformed me away from journalism, and, well, I backed into sociology.

AN: So you came to sociology through the "backdoor"—to borrow from the title of one of your books.

TD: I backed into sociology. I didn't come in saying, "Hey, I'm going to be a sociologist." I think this is true for a lot of people. Here's the event: In my

last spring of my senior year, there is a big accident on the elevated train. A motorman makes a mistake. There are about twenty serious injuries. The media arrive and they are able to talk to the motorman because the metal has cut into his thigh in such a way that he can't get out. He's just trapped in there. While the police and paramedics get devices to get the motorman out, the media sticks a mic in front of his face. They ask him, "How does it feel? What happened? . . ."

At the time, I was taking a class with a guy named Whitman, the head of NBC news in the area. The next day, he tells our class, of about twenty-five people, that the NBC reporter refused to interview the motorman. CBS went in and got the story. So did ABC. Whitman said that was a dark day for NBC journalism because these two guys from the other networks scooped them. He wanted this event to serve as a lesson to all of us students to just put ideology aside, put everything aside except journalism. Get in and get the story.

There were three of us in the class who disagreed strenuously—my two roommates and me. Three of us—out of twenty-five students—were weighing in that there are other values to consider. Do news events happen in order to be reported, or is there something else going on? Whitman began to get angry with us. We were brash and young, and we were, in our own ways, arrogant in our moral superiority. But we were making a strong argument. And whatever argument he put up, we would counter it. Whitman got red in the face, and he said, "If that's your view, get out of journalism and get out soon." All of three of us left journalism within the next two months.

I told this story to Ray Mack, a professor of sociology at Northwestern. He was very young, only in his late twenties. He and I were friends. I came to know Ray because I had done well in his Race Relations class. Ray says, "Come to sociology. Go down the hall and see Wendell Bell." I had taken a class from Bell three years earlier and had gotten one of the better grades, and so he knew who I was. I went to see Bell. He was leaving Northwestern. He said to me, "I'm going to UCLA [the University of California, Los Angeles] next semester. Just apply for a teaching assistantship and you can come too. It's a done deal."

AN: When you were planning to speak with Ray about what had transpired in the journalism course, did you have the sense that he was going to appreciate your position because he was a sociologist?

TD: No doubt about it. He was also a kind of senior mentor, someone whom I could talk to about all of these issues. In my last year at Northwestern, he

and I were on a university committee together around race issues on campus and a controversy surrounding a Chinese American student named Sherman Wu. Wu pledged a university fraternity, and they accepted him. However, the national body said that he could not join the fraternity and threatened to kick the Northwestern chapter out of the organization if Wu remained in the chapter. It became such a public issue that Pete Seeger, the songwriter, wrote "The Ballad of Sherman Wu." So Ray and I had been colleagues, friends, coworkers, coconspirators, co-political, quite independent of academic stuff. I [would] go and talk to him about any topic. He would quickly say to me, "You know, you're actually a sociologist. You don't know it, but you are."

AN: So you are at UCLA in the late 1950s. Was sociology a new department?

TD: In the fall of 1957, I find myself in Los Angeles, you know, in the never-never land, after living twenty years in Chicago. All of a sudden I'm a teaching assistant, although I don't even have a degree yet. I'm teaching in a department of sociology, about which I know not a damn thing. But I'm at UCLA because of, you know, the old boys' network.

The department was founded in the late 1940s. Leonard Broom was the chair in its early phases. He had been the senior mentor to Wendell Bell, John Kitsuse, Sheldon Messinger, and a few other figures in the field. Phil Selznick was also there, before he moved to Berkeley. Now the real intellectual journey begins, because at UCLA, for the first time, I think of myself, and my work in a way that constitutes a trajectory. Up until UCLA, I'm floundering around.

Everything changes after I get to UCLA. I think I know about discrimination. I think I know about race relations. I don't know a damn thing. You know why?

AN: Because you had lived in a homogeneous black world for most of your life?

TD: Yes, I lived with black people all my early life. And then, only white people. And all the white people were on a college campus. And they thought of me as a charming mascot. "Here's Lucky. Speak, Lucky!" "Look at this charming boy. Nothing to be afraid of here. He talks like us." The highest compliment to be given to me by the white students was, "Actually, we didn't know you were black." All-black scene, first sixteen years; I go to college for four years, where it's an all-white scene. But I don't experience real discrimination in that period. Sure, I can't go to the barbershop; I can't go to the bowling alley. But that's life as usual. There's no trauma for me in that because I can leave Northwestern, take the subway and be back

on the South Side of Chicago in twenty-five minutes. Can't get a haircut in Evanston; can't get one anywhere? I can go back to Chicago's South Side. Want to listen to music? I go to Chicago's South Side. In other words, for me, life is still back in the black community. The four years at college were like an anthropological or cultural excursion.

When I get to UCLA, my eyes are opened. First of all, there wasn't much of a black community near UCLA. That's the first surprise. Then I go out and I try to get an apartment—a reality check. So for the first four months in Los Angeles, I'm living with my uncle, Herman. I've been looking and looking for an apartment. My uncle asks, "How's it going?" I say, "I can't get an apartment." He says, "Wake up and smell the coffee, young man. Where did you think you were?" [Laughter.]

So I get a wake-up in the public sphere in Los Angeles. Then there's school. I'm in class with really giant intellectual figures, who have an understanding of the world that so meshes in an easy and fluid way with my view of the world that I figure, now I understand. The two major figures are Harold Garfinkel [the renowned ethnomethodologist] and William S. Robinson [the influential statistician]. Why would these characters, who are so completely different, have such a resonance with me in my first year of graduate school? Well, Robinson was a methodologist. But he was the most unbelieving methodologist that you could ever imagine. He taught advanced statistics; he knew this stuff backwards and forwards. He would always say to us, you know there are far too many variables to control for what might be really happening in social life. . . . It wasn't as though we were talking to someone who was a humanist, who was putting down methods. We were talking to someone who knew methods. He would tell us stories about how Paul Lazarsfeld would run his datasets. Lazarsfeld might be told, "You've run the data in the wrong direction. It goes this way." And he'd say, "Okay, okay." Then he'd turn it around and get the opposite interpretation. So I grew up with Robinson's quantitative skills being used to debunk the notion that methods were the salvation and the answer—

AN: Or at least a very healthy irreverence for the statistical reduction of social life.

TD: A healthy irreverence, yes! At the same time, I was taking classes with Garfinkel. What Garfinkel was saying that deeply resonated with me was, in effect, "It ain't necessarily so. . . . What you see on the surface of social life is fragile, it's fluid; it can change like that." This insight was useful for thinking about the race riots of the 1960s. Or, later on, Sarajevo: people had lived next to each other for twenty, thirty, forty years, and then, boom,

something could happen. People who had been friends, or actually in the same family together, could turn on each other on a dime. It resonated with me that social life is fragile and much more intricate than we can perceive. Garfinkel talks about this in his earlier works. He was talking about how the assumptions that are deeply embedded in human experience are not available, except they are also on the surface. There is this contradiction.

One of Garfinkel's buddies was Erving Goffman. Goffman used to come to UCLA in those years—the late fifties. Erving took a tiny bit of liking to me—but [was] always jocular; [we] never [had] a serious conversation. It was always, "Well, young man, I see that you've come this far, and let's see where you're going. . . ." The difference between Goffman and Garfinkel was astonishing to me. Garfinkel had students like groupies. His charisma was extraordinary. They often didn't last because, like groupies, there's a love relationship. And it gets too close; it gets sullied, and then, boom, in the turn of a pin, the turn of a dime, you're gone. That was Garfinkel, but he had all kinds of students who loved him and hated him. Erving never had a student. He was the Lone Ranger of sociology.

AN: At UCLA things are starting to fall into place for you intellectually. It is the heyday of the race relations approach and ethnomethodology is ascendant. What other questions is the field of sociology grappling with at this time? What questions are you grappling with?

TD: Well, you're asking about the zeitgeist, and the zeitgeist is hard to capture. But I think you're onto something. It was a period in which sociology looked as if it were on the rise. [Robert K.] Merton was in his heyday. Parsons was in his heyday. And the field had a sense that making a difference in the world was almost inevitable. You got into a sociology department and you knew that whatever your work was on, whether it was prisons, education, medicine, or civil rights, it didn't matter what it was—sociology had a perspective, a voice that was being listened to around the world. Certainly, by the time Kennedy is elected in 1960, there is this notion that sociologists are going to be—like economists—

AN: Like economists today—

TD: That's right. Even then, economists had a collective voice, more than sociologists did. In some sense, political science did not have the coherence of sociology at this time, but it had its own high-profile practitioners. When I was in graduate school, sociology was seen as an intellectually viable, almost vanguard position on social justice issues. We were where the action was. It wasn't in political science. It wasn't in economics. We used to laugh at the idea that Chicago would really celebrate this young whippersnapper

coming up, Milton Friedman: "What? It's all about free markets? Are they crazy in Chicago?" That was a joke. Well, the joke was on us.

AN: Do you remember a particularly formative or influential course at UCLA?

TD: I took two or three courses from Garfinkel. One was a graduate course in ethnomethodology, in which I felt that I was way out of my league. I thought, "I don't know what these people are talking about." I'm in this seminar with Garfinkel. It's like I'm at a Ping-Pong match. But I'm not playing. I'm watching these players. And they are smart as hell. I've always thought so. I'm not in their league. I'm just trying to figure out what I can do in this league.

Some of the other people in Garfinkel's seminar were Egon Bittner and Aaron Cicourel, who had been at Cornell and came to UCLA just to work with Harold. In this seminar, there were all these figures who were senior to me. I was twenty-one. Bittner was, I think, thirty-seven. Bittner came back to sociology from philosophy. He had been in Buchenwald. I could sit at his feet and listen to the stories from Buchenwald from Egon. I was young; I didn't know anything about the world. Peter McHugh, who was at the time, I think, twenty-nine or thirty, had been working in Hollywood as a writer, left and came back to sociology. Cicourel came back to sociology after studying experimental psychology. In other words, not everybody around me in those years was a sociologist, but they were people who came to sociology because they had the sense that something was happening—that there was intellectually fermenting soil, where you could actually do a lot. That was the environment.

AN: And you moved from UCLA to Northwestern for your doctoral studies?

TD: Yes. Ray Mack sends me a letter. He says, "I've become the chair of the department. And I have a fellowship for you from the Ford Foundation." So I go back to Northwestern.

Scott Greer, a Northwestern sociology professor, once pulled me aside and told me something that he was right about, and I never forgot. In his thick southern drawl, he said, "Now let me tell you one thing, Troy. Be very careful what you choose to write your doctoral thesis on. It's a character-defining act. Whatever topic you choose, it's going to shape who you are for the next ten years and maybe twenty. It's going to take you at least twenty years to get out from under whatever it is you choose. So you better choose something you want to be." It was dramatic. But it sunk in.

So I said, "Okay, I'm going to do something I really want to do." I decided I was going to work on what I called "the social response to abnormality." It was about how members of families or people in the circle of the mentally ill deal with the illness. A very Garfinkelian idea, right? The

mentally ill express certain kinds of behavior: the community normalizes it or shuns it, says, "I didn't see it," or comes forward and says, "This is a problem; you better take care of it. Get this person into some kind of a therapy." I decided that I was going to write about not mental illness per se but the social response to mental illness.

AN: What happened after you finished your dissertation?

TD: I took a post at UC Riverside. After I had been there about six months, I get a visit from a guy named Gaulden, a medical doctor and the head of Norco Corona—the California Rehabilitation Center for Narcotics. He comes into my office and says, "They tell me that you're the person who knows about control and deviance. Well, [California] Governor Pat Brown has set aside money for rehabilitation centers, and the mandate is that they must have a component of social research. Would you be willing to do this?" Initially, I say, "Well, it's not my area. I don't know much about drugs." But I twisted it around for a while in my mind and decided, "What's to lose? I'll go and spend two months. If it doesn't work out, I'll turn around and go the other way."

I am a young assistant professor at Riverside. I'm being offered this money to go and just hang out and learn something new. That's when I learned that you can become an expert on a topic pretty fast. I did about six months of reading. I read the whole history of nineteenth-century thought on drugs. Then [I] began interviewing inmates at Norco Corona. There were 350 people, many felons, who because they were heroin addicts got their sentences commuted to go to Norco Corona for a one- to ten-year commitment—a fluid incarceration.

AN: And Norco Corona was imagined as a research institute?

TD: Yes, more specifically, research about drugs. What's going on with drug addicts and how can you cure them. The whole institution was designed for therapeutic purpose.

It's sociology in the early sixties. It's the therapeutic community—so we're not criminalizing, penalizing; we're trying to find a way to help these people. This is before the so-called war on drugs. This is 1963. Nixon is not in power. Kennedy is. As you were asking at the very beginning of this exchange, what was the zeitgeist? The zeitgeist was, we're going to reframe the debate. We're going to change this whole discourse from criminalization to rehabilitation.

AN: On the face of it, this project was volitional. But, in fact, the prisoners were being offered, say, incarceration at Alcatraz for twenty years versus commitment to the Norco Corona facility for one to ten years?

TD: The problem was that many of them were there on misdemeanors; they could have been out in a year for a misdemeanor. But they chose Norco, because they thought it was a therapeutic community. Many of them had been there for two and three and four years and beyond, because the staff didn't think that they were appropriately rehabilitated.

AN: How long were you at UC Riverside?

TD: I wasn't there long. Riverside was a racist community. In this setting, anything I said was front-page news. No matter what I said, they would say, "Black Radical Professor Attacks America!" I wasn't anything resembling Bobby Seale, but I was portrayed as the Eldridge Cleaver of the region. I used to get hate mail and lots of death-threat phone calls.

In 1965 there is a meeting of the school board on race relations. I'm called upon by the black community and as a member of the local CORE [Congress of Racial Equality] chapter to give this talk about why Riverside can deal with racial issues and not have a Watts on our hands. The event was in a small auditorium; it was standing room only. It's a hot Faulkner night in August; the air is thick. About a third of the way through my talk, someone interrupts and says, "Who'd you come here with tonight?" I say, "I came here with my wife, who is from Sweden. Why don't you welcome her to the free world?" And I went on for maybe about a minute with this sardonic notion that, in America, I'm sure that she will receive a gracious reception. There was a loud groan in the audience. I knew I had gone too far.

Afterward, the death threats continued. It reached a point in the late fall of 1965 that things got really rough. My wife was a very engaged gardener; she had this great garden on the side of a hill. They began to dump garbage onto the garden. My wife was a trouper; she understood. But it escalated; the phone calls escalated. And then she began to be affected by it. It became taxing on my wife.

The university chancellor had gotten wind of this, and he called me into his office and said, "We'll give you a police escort, and we're willing to move you onto campus." I didn't want to live like that. Cicourel at Berkeley had also heard about this. He talked to Goffman; Burton Clark, who was at the Center for Studies in Higher Education; and a few others. They thought this was intolerable and invited me to move to Berkeley. In January 1966, I came to Berkeley to work at Clark's higher education center, and I also taught half-time in the Department of Sociology.

AN: Were the racial politics at UC Berkeley more bearable?

TD: Soon after I arrive at Berkeley, all hell breaks loose—it's called the sixties, the late sixties. Stuff is coming down. I mean, it's really coming down.

On the one hand, I'm heavily recruited by Huey [Newton] and the Black Panthers. Huey and I have mutual friends, such as Fay Stender, who was the attorney for George Jackson and very much involved with the Panthers. She and I were friends. And Fay and Huey were close, let's say. She was later the target of an assassination attempt, and after being shot by Edward Glenn Brooks and left partially paralyzed, she committed suicide. [David] Horowitz is the key. He sets up my meeting with Huey. In those years he's a leftist, an unreconstructed leftist. So he raised the idea of the meeting, with Fay's agreement.

The Panthers are in turmoil. They want respectability. I go to Huey's apartment, and after an hour's conversation he asks if I'll become chief of staff. He says to me, "You're my man." I say to myself, "What? Did I just hear him right?" Newton sees that I'm not really impressed, and he runs and gets manuscripts that he's been working on with J. Herman Blake to show me he's a real intellectual. Then he shows me his poetry. He starts over again, offering me use of the top floor of his penthouse for parties. The more he talks, the more he thinks I'm going to sign on. No way!

On campus, on the other hand, the more radical and nationalistic of the black students had a strong ambivalence toward me. I'm the only black faculty member working as their ally on all of these university committees. But I have not committed myself to the revolution. So they're thinking, "Who is he?" Black studies, African American studies needed intellectual leadership; I publish a piece in the *Daily Californian* voicing my support of these developments, saying that I'm in favor of the fact that black people are now mobilizing. I make an argument that I can play a role, but not the leadership role. If you want someone who's black by your own accounts, it couldn't be me.

AN: After several years at Berkeley you were invited to join President Jimmy Carter's Commission on Mental Health. How did this come to pass?

TD: I had written a book called *The Legislation of Morality*.

AN: This was published in 1970.

TD: Yes. In this book I wrote about the history of opiates—particularly morphine and heroin—and also other drugs. I argued that [what had changed was] not the drugs . . . but their user populations. In the past century and a half, the public presentation of who's using had been completely inverted. In the late nineteenth century, white upper-class, middle-aged females were the primary users of morphine. By 1925, just forty years later, the users were primarily young, working-class or unemployed, increasingly of color, and male. The drugs didn't change; the people who

seemed to be using them changed. There was a complicated set of reasons for this shift; this is what I was interested in.

After this book was published, I got to be known—and this is back to Greer—as "the drug person." Howie Becker said to me at the time, "Welcome to the drug circuit." And I said, "Howie, you don't understand. Listen, this isn't a book about drugs; this is about the shifting transformation in use patterns. . . ." He said again, "Welcome to the drug circuit." And he was right.

I was invited to join the National Academy of Sciences panel on substance abuse. Our job was to address the funding of social science research on this topic. I felt as if I had been let into the back room of the golf club. The big boys were allocating the resources. I thought that since this is the Carter administration I will have a voice among people who have views that resonate with mine. Boy, was I wrong. I heard colleagues from the natural and biological sciences engage in vicious, mean-spirited attacks on research programs focused on poverty and its relationship to mental stress and mental illness. And mental illness was being described as being all about neurotransmission patterns and genes. I heard all of this vituperative language about how social scientists had perverted the mission of the National Institute of Mental Health (NIMH) and thereby diverted attention from the "real problem." The real problem, they said, is the genetics of schizophrenia; the real problem is the genetics and biochemical aspects of heroin addiction and of alcoholism. The real problem of violence is that the causes of these maladies lie inside of the body. The attitude was, the social scientists have taken this issue of violence down the path of poverty. It's the wrong path. Get a hold of those neurotransmissions and we will have it figured out. I found myself on a presidential research advisory committee to the National Institutes of Health that was overwhelmingly, relentlessly, and determinedly hostile to social science. That is the beginning of the story of how I became involved in science studies of genetic and biological explanation of complex behaviors.

Around the same time, I became director of the Institute for the Study of Social Change, an NIMH training program on the Berkeley campus. It was quite ecumenical in its orientation, including public health, economics, anthropology, and medical sociology. Reagan comes to power in 1980. Soon after, I get a visit from a project director at the NIMH who tells me that I need to put a physician on the staff of my program. He tells me that I need to reorient the program. He didn't explicitly say make it biological, but that's what he meant. He wanted to "medicalize" the program.

The Carter commission task force and this encounter with the NIMH director sent me back to Robinson's insights. I'm thinking, "Are they kidding?" To focus on mental illness solely as a medical matter would be to control for only a few biochemical variables, when in fact there have to be many environmental variables that are involved. My training back at UCLA starts to click in, and I begin to discover just how much the architecture and scaffolding of their methodology for the genetic explanation of mental illness is deeply flawed. For example, there isn't even an agreement among psychiatrists about what schizophrenia is phenotypically. Since they haven't got an agreement on the phenotype, they broaden the category to "schizophrenia spectrum disorder." A similar thing will happen years later with autism. So now we're going to find the genetics of a spectrum?

AN: You would later serve on the committee considering the ethical, legal, and social implications (ELSI) of the Human Genome Project. How did you go from the Carter commission to this appointment in the early 1990s?

TD: During the eighties, I'm really on a mission to reveal the social forces at play in the history of genetics and, therefore, its contemporary manifestations. That's when I begin to write *Backdoor to Eugenics*. In earlier work, I was making claims about cultural perspectives on biological knowledge. This work had put me in a zone where people working on genetics knew about my research. I was asked to join the ELSI committee but also the National Advisory Council for Human Genome Research—of which ELSI is in some sense a subsidiary. After [I spent] about a year on these committees, a member of the commission said to me, "You're not nearly as anti-science as your reputation." It seems that one gets framed as either pro- or anti-science.

I am not anti-science; I am interested in the preframe—the deep domain assumptions that are a part of the fabric of thinking about the genetics of crime, schizophrenia, alcoholism, violence, drug use and abuse, et cetera. Let me be rhetorical for a moment. If you were to deal with a population of people who were already categorized as violent criminals, the assumption would be that you have that population at hand and that everybody else in the other population is normal. Yet a lot of people who are violent never get caught by the criminal justice system. Any methodology that begins with those who are already in the category misses the social processes that got them there. The behavior of the police and the prosecuting attorney's office is more predictive of who gets into the criminal justice system. People want me to talk about the social "implications" of

this genetics research as opposed to how the methodology is flawed, how the science is problematic. Focusing on the preframe disrupts this narrative.

AN: You've been speaking about how your research has been counterposed to that of scientists, such as when your colleague at the National Academy of Sciences was surprised that you're not anti-science. In recent years, some social scientists have made what we might call the genetics turn. There was an issue of the *American Journal of Sociology* a few years ago that explored "genetics and social structure."[1] And, recently, Harvard sociologist Robert Sampson has said that "sociology has nothing to fear from genetics."[2] Do you now find yourself being counterposed to both scientists and social scientists?

TD: Great question, and the answer is hopefully as good as the question. What's happened in the past several years is that there's a wave, and the wave gets bigger and bigger. Sociology has always had a wing of itself that has science envy, that wants to outscience the scientists, that wants to give more affirmation to those who do a certain kind of research—quantification becoming the poster child for this. Physics was the first envy, but in the past twenty or thirty years, biology has become the second version of this. The biologist is more likely to be seen as the neutral, white-coated scientist who can get at "the reality" of things. One version of this is that some think that biologists can get at the reality of race. So even though people call themselves A, B, or C, [biologists say] let's go into the genetic structure and look at the markers and see what they really are.

There's this idea that reality lies in the biological version of human categories. This is very attractive to researchers. The big science money is going to those who are looking at the genetics of such matters as diabetes, obesity, and asthma. You have the researchers interested in the social and environmental forces that help explain the skyrocketing trajectory of these problems, but, as I've said, for the past forty years or so, that's not really been considered "real," "serious," or "hard" science. Young people getting their PhDs in sociology today can tell which way the wind is blowing. The wind is blowing toward neurotransmission patterns for mental illness, not toward the social forces that might help explain who becomes mentally ill or who becomes an alcoholic. It is all about genetic structure.

Also, now here's Sampson and Dalton Conley and others, who are saying let's have joint ventures with the geneticists and the biologists. Let's all get under the same tent; let's do research together. The conventional wisdom of my young colleagues is that it is the right way to go and that

people like me insist on being mired in a time when sociologists didn't touch genetics or biology.

The assumption of these colleagues is that the research table is level—or that we are all on a level playing field. We're all going to get a seat at the table, and that table is not thought to be tilted. You're the anthropologist, you're the economist, you're the sociologist, molecular geneticist, neuroscientist, cognitive scientist. We're all going to design this huge study of a thirty-year cohort and assess the relationship and relative contributions of the environment and genes. In a genuine collaboration across disciplines, let's find out what's really going on. How could you not agree with that, that getting together with your colleagues, in a collective enterprise, that this wouldn't produce more and better knowledge? Well, the playing field is not level. Science is stratified. The economists will look at the economics of it, sociologists at the social implications of it, and so on. And now comes time to put together the report. It is not an equal interpretation.

When it comes time to report the results of this interdisciplinary study, we are going to find that the genetic or biological component will dominate the explanation of what "really happened." For example, any long-term collaborative study of high rates of alcoholism among Native Americans will find that displacement, disenfranchisement, high unemployment, and easy access will explain upwards of 70 percent of the variance. But if the tribe has some marker more highly correlated to the alcohol dehydrogenase gene, that will become the "real" finding. Audiences tend to respond to findings about poverty among such groups with "We already knew that!" And so the new finding about a genetic marker dominates the explanation. The people who think in terms of sitting around the table as "equal research partners" miss the overwhelming truth that there is a hierarchy of the sciences and a hierarchy of interpretation, regardless of the greater analytic power of the social sciences in explaining rates of alcoholism between groups.

AN: Let's talk about your book *Backdoor to Eugenics*, which has become foundational to the sociology of genetics. This was a prescient work when it was published in 1990, well before the decoding of the human genome. What kind of reception did it receive?

TD: The historian Dan Kevles said to me—I'll try and remember the quote the best I can—"Back in the period when eugenics was being practiced, the fact that there were so few protections in place [for ethnic and racial minorities] made it possible for state eugenics programs to target them." He said, "Now your concerns about eugenics are pretty much displaced because we

have so many political barriers in place that will keep that from ever happening again." I think he missed the point. The whole point of *Backdoor to Eugenics* was to say, yes, we're not going to find a version of this today that's going to come out and say, "We don't want those people to produce." But we are going to find much more subtle ways of talking about good babies and bad babies. There is a resuscitation of this old idea, but in new clothing.

The earliest backdoor—and I think I was right about it—is going to take the form of people saying we don't want to make babies who have particular genetic conditions. Who could be against that? Of course, there are implications down the road that concern me. But my concern in that book was not only saying, uh-oh, we've got to stop this train because the implications down the road are bad for black people. My point was that this train itself is on the wrong track. It is making the assumption that the genes are destiny. That all we have to do is get a hold of this genetic structure, go in there and zap it—in the 1990s that was the language—the promise of gene therapy. We are going to solve cystic fibrosis, sickle-cell anemia; we're going to solve it all with gene therapy. But wait a minute. On the scientists' own terms, that didn't make any sense, because of the high variable expression of these disorders. How are they going to get to the gene? I was not just criticizing the implications or the troubles just around the corner. I was saying that what's important is what happens before you even get to the corner.

AN: Can you imagine that sociology could be engaged with genetics in a fruitful way?

TD: That depends entirely on the frame of the research project and the subject of the investigation. For example, the more determinative, the more definitive, the more, to use scientists' language, the penetrance of the gene, the less sociology has to say. So if it is the case ultimately that they do find out that a particular kind of autism has a 95 percent penetrance, now sociology has very little to say about the social causes but will be able to address the social implications. But if and when it is determined that autism has only about a 20 percent penetrance—now the question is, what are the triggers in the environment that might shape it, and then of course the social forces that are going to help you explain the trigger are going to be far more determinative than any quest for specific genes or markers. What's this got to do with ecumenical research? We want to know: Where is the trigger? What is the trigger? When does it happen? At this point, geneticists don't have much to say about these questions. Even somebody like [the acclaimed molecular biologist] Lee Hood would

say, "We have so many combinations of genes." Well, all right, I'm asking, what's the implication of that, in terms of the science, not the social implications, but in terms of the science? You're going to explain hypertension in terms of science? Well, you've got six hundred genes that all contribute to hypertension. What's going to manifest itself in the person's hypertension is going to have to deal with the social fact of who gets to walk around Saks Fifth Avenue without being followed. And many geneticists are going to say right away, "Well, that's not science. We can't go there. How are we going to monitor whether your stress levels go up when you walk into a department store and someone follows you? That's not science. Science is when we have now located the twenty-sixth gene that is related to hypertension." I rest my case.

AN: Today the public comes to know genetics via direct-to-consumer DNA testing. What do you think of this recent phenomenon?

TD: Direct-to-consumer is an issue where one could make a good critique even at the level of methodology. That is happening. You can see more and more people in the field of genetics saying, "Whoa, wait a minute here!" So that part doesn't concern me, because I do think there is an increasing critical consciousness now among people who are in that field.

AN: Do you think that you have had a role to play in the creation of that critical perspective?

TD: Yes. But there were several other people who played very important roles in that whole direct-to-consumer thing and who, I think, understood the methods a lot better than I did and made strong, good statements. I may have played the role of a node, putting people together who were better at the methodological critique than I am.

On the other matter of race and genomics: this is not going away. It's going to get bigger, and this is where I think all of us have a role to play. Geneticists are neither bad guys nor good guys, but there are markets, and this is a business. We're going to see the increasing concern with defining markets in terms of ethnic populations. Ten years ago, expressing concerns about the confusing and increasing intersection of race and genomics phenomenon was viewed by many as unnecessarily alarmist. At the time, the American Anthropological Association had just issued a statement that race was a flawed concept for looking at human differences. This opened the door for an effective political assault on the use of race to redress past and current grievances. Ward Connerly, for example, would put on the ballot in California an initiative to ban the collection of data by race. "We don't need no damn affirmative action. The anthropolo-

gists have now told us that race is a deeply flawed concept and that it's history's stain. Let's just get rid of race. Let's just not talk about it, folks; let's just go be postracial." So no, the realpolitik of postracialness and the increasing convergence of genomics of race are in play for an inexorable clash. On the one side, we've got the aspiration of those who want to not see race for public policy purposes, those who say that the geneticists and anthropologists have told us that there is no such thing as race. Meanwhile, big pharma [the pharmaceutical industry] is looking for markets; they're talking about personalized medicine. By personalized, what they mean, really, is a configuration of issues and categorization of where the individual fits best in those categories. But that configuration is most likely going to be a fit to existing taxonomies. Here's the other side of the coin: I refer to Paul Rabinow and his articulation of "biosociality."[3] There's a version of this school of thought that is postracial. Now we're going to newly categorize all the people with a specific genetic disease, and they're going to be a new category, a new social group that transcends race, class, and ethnicity. We shall see which wins out, but I am betting that a biological definition of the group is not going to transcend existing social categories. Because those touting personalized medicine still today use old racial categories, we now have the reintroduction of the old taxonomies through the twenty-first-century lens of genetic markers. This is what I have called "the molecular reinscription of race."[4]

Notes

1. Peter S. Bearman, Molly A. Martin, and Sara Shostak, "Exploring Genetics and Social Structure," *American Journal of Sociology* 144 (supplement).

2. Robert Sampson, quoted in Patricia Cohen, "Genetic Basis for Crime: A New Look," *New York Times*, June 19, 2011, C1.

3. Paul Rabinow, *Essays on the Anthropology of Reason* (Princeton, NJ: Princeton University Press, 1996).

4. Troy Duster, "The Molecular Reinscription of Race: Unanticipated Issues in Biotechnology and Forensic Science," *Patterns of Prejudice* 40 (2006): 427–41; Duana Fullwiley, "The Molecularization of Race: Institutionalizing Human Difference in Pharmacogenetics Practice," *Science as Culture* 16, no. 1 (2007): 1–30.