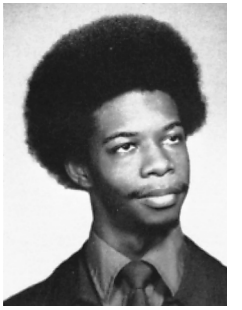
SYLVESTER JAMES GATES, JR.



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Both unusual and normal, is the way I described my childhood to someone recently. There are a couple of overriding relevant facts. One of them is that my father, a World War II veteran of the "Red Ball Express," was in the U.S. Army when I was a child, and so the first ten or so years of my life were spent on army bases with lots of moving. For example, my youngest brother was actually born in St. John's, Newfoundland, in Canada, and we lived there for about two years, from the time I was about two to four years old.

I started my education on an army base, Ft. Bliss in El Paso, Texas. I was very fortunate to have been an "army brat." The U.S. military was—and is still to this day—one of the few segments of this society where issues of merit, superior performance, and diversity have, in my opinion, been successfully managed. This also had the consequence that I began my education in a racially integrated environment. Thus, the assumptions of inferiority-superiority that underlie much of the dynamics of race in this nation are not burdens organic to my psyche, as I have seen in both African-Americans and European-Americans much of my life. From my experience in first through fourth grade, there was no observational basis to support such beliefs. I was thus a beneficiary of policies of integration long before these existed in most of the country.

The other overriding and important fact about my childhood is that my mother died when I was about eleven years old. That had some profound consequences in terms of my personality and implications, in some sense, for how I got to be a theoretical scientist. It was a normal childhood except for these rather unusual circumstances. It was a relatively happy one otherwise, and a very long one in many ways. I can recall being in high school and certainly junior high school and doing things that my stepmother thought were ones that younger kids should have been doing. These were things—reading comic books, drawing characters, et cetera—that she felt someone of my age shouldn't have been doing.

This is around what age, now?

Oh, fourteen, fifteen, maybe sixteen, something like that. So it was unusual in that sense. It was also unusual in the sense that people often ask me, or I have been asked, "How did you get to be a black scientist?" My answer is simple—I picked the right father. His career choice meant that I was shielded from many of the noxious influences and depriva-

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tions that might have occurred to a young black child growing up in my time and place. For example, a European-American teacher—Ms. Canteau—in fourth grade stands out in my memory.

My dad played a critical role in getting me to ask some questions about one's role in exploring the universe. He didn't do it in any fancy way, but he did do it in a concrete way. When I was between the ages of about four to eight, I had tremendous numbers of questions about lots and lots of things and I would go to my father, principally, to ask questions. My father never told me that the questions were stupid and he never refused to answer the questions. As a consequence, I got the idea that questions always had answers and that's certainly one of the things that propels one as a scientist—that questions have answers.

A few years ago in fact, right after a taping—I relayed the story on a PBS program, "Breakthrough"—I asked my father one day how it was that he was able to answer all my questions, because these questions ranged across the board. How was it that this middle-aged African-American soldier, who had not been able to afford to go to college, had access to all of this information? What he told me really astounded me. He said he remembered me having lots and lots of questions as a young child and yes, he remembered answering them all. The trick to answering them, however, which I had not remembered, was that when he didn't know the answer to a question, he would say, "Let's hold off on that one, I'll get back to you." In the intervening time between our discussions he would either go talk to someone, go to the library and look up a book, or go to another information source he thought appropriate and where the answer might be found. He would go do this and then return to tell me the answer. I never knew about this until two years ago.

So, it was an interesting childhood and, as I said, strange in some ways but normal in lots of others.

Now, where did you go to high school?

From ages eleven to eighteen, all my time was spent in Orlando, Florida. I went to high school at L. C. Jones High School. It was one of two black high schools in the greater metropolitan Orlando area at that time. Segregation was still the law in much of the South. I graduated in 1969. It was an interesting experience because that, and the years prior while attending high school, was really the first time that I lived in a racially segregated environment. It was my introduction, which was tremendously stressful, to black culture. It also gave me insights that I've used over and over in life.

I would have been a much poorer person had it not been for the fact that I was in an environment where there were plenty of nurturing African-American adults around. My teachers made it clear to me that they expected the very best from me, certainly academically and in all other ways. It is sort of the opposite to what you often hear about the present-day breakdown within the black community. Somehow, I seemed to have been in one of those bubbles in time and space where there existed a wonderful community. It was segregated, yes, but that didn't matter. I had values instilled within me, such as perseverance and striving for excellence. These things came from the family, the church, and the schools.

I tell people that the first person who served as a drill instructor to me in the use of logic was my high school geometry teacher, Ms. Edna Williams. We had to write proofs in geometry and this was my first exposure to the rigorous use of logic. She never accepted anything less than perfection. A lot of what has been my success was anchored right there in the black community of Orlando, Florida.

Talking about Orlando and this kind of environment, what about role models and mentors? You mentioned Ms. Williams. When you look at your background and you look at that period, who were the people who really stand out as being very influential?

Well, that's sort of a funny question for me to answer. Up until a few years ago, whenever I heard arguments about role models or the lack thereof, I didn't buy it. I just wondered, what are these people talking about? And then after conversations with many, many young people, it became clear to me that I had had so many role models that I hadn't noticed it. For example, there was Ms. Williams, who certainly demanded the rigorous use of logic. I had another teacher, Mr. Sanders, who demanded as much in algebra.

The most influential teacher, as you might have guessed given my career, was my physics teacher, a gentleman by the name of Mr. Freeman Coney. He had gotten a degree at Rensselaer, but given the circumstances of time and place, he found himself teaching high school. He was just an excellent teacher. I was a junior, the only one in a

class of seniors, when I took my first—and only—high school physics course. It was pretty comprehensive, going up to and including special relativity. There is a process called "Compton scattering," which capped off our high school physics course and introduced me to the ideas of Albert Einstein. These facts alone are pretty remarkable, given that this was a segregated school in the South during the mid- to late '60s. I have often wondered how many students at what we then called "the white schools" were as lucky to have such a teacher. I suspected that the central Orange County School Board had no idea of the type of excellence that could be found in the resource-poor but people-rich environment of Jones High.

So like I said, I was the beneficiary of numbers of role models, but the most powerful in my life has been and continues to be my father. You've met my father, so you know what kind of person he is. I'm amazed at my father's life, much more than I am of my own, because everything that I have become started from that foundation.

How did you find out about MIT?

There are two parts to that, too. The first part occurred when I was either thirteen or fourteen years old. There was a television show on at that time staring Danny Thomas called "Make Room for Daddy," and one episode had a nephew of his coming to visit their family. The nephew was supposed to be a genius. And what school did this nephew attend? Well, obviously MIT. At the conclusion of that program, I walked up to my father and said that I wanted to go to college at MIT.

In our family, from the time we had entered elementary school, it had been expected that we—the children—would all go to college. It was a question not of whether you went to college, but instead of where you would go to college. So I finally had an answer to this question and it was MIT. That was when I was thirteen or fourteen years old. My father's response was, "We'll see what we can do about that." And actually, I became the first member of our family to go to college.

By the time I was a senior in high school, I understood a lot more about this country and the role that race plays in molding the lives of African-Americans. I understood that given the history of the recent times—it was the late '60s as I was finishing high school—it was very unlikely that a young African-American man who had gone to a segregated Southern high school would ever get the opportunity to go to a place like MIT. So I had essentially given up the idea, even though I had always been a good student—I was co-valedictorian of my class.

This resignation was not based on some abstract sense of oppression. During my years in junior high at Jones, I met someone who was to be my closest friend throughout the time I was there. His name was Philip Dunn. He became my chess teacher shortly after our initial meeting. By the time we were in eleventh grade, after many, many matches, we had gathered around us a collection of like-minded friends. We formed a chess club, with Philip playing first table, me second, and my youngest brother, William, third. After getting one of our teachers to act as our sponsor, we were able to arrange matches with some of the other high schools. Since ours was the only "black" high school in Orlando, then all of our opponents were "white"—perhaps strikingly appropriate for chess. On the occasion of our first match, I was amazed at the facilities, materials, and infrastructure that were available in these schools open to European-Americans but not to us. I instantly understood the hand that I had been dealt, the long odds against me that were there on account of my race and no other reason.

My response to the profound difference between the American proclamation of liberty and justice for all and the reality of the color bar was a deep and all-encompassing cynicism. I had given up on my dream, even though I dutifully took my SAT's, doing pretty well—in fact, well enough so that I was selected as a semi-finalist in the National Achievement/Merit Scholar program. As a consequence, some very strange and interesting things began to happen which ultimately ended up with my going to MIT.

It started one morning in homeroom period. The school had a public announcement system and every morning the principal would come on with a list of things to say. So one morning, my name was called and all of my friends looked at me and said, "Gee, what have you done?" Typically, if the principal called you to the office, that did not foreshadow the imminent arrival of a pleasant experience. I got up, slowly walked down to the principal's office, and when I got there I was given a pack of envelopes. These envelopes contained invitations from colleges inviting me to apply for admission.

This went on, I believe, for some weeks and it was really amazing. It was almost like an athlete being recruited to go to college to play ball. Among the number of places that sent letters was MIT. But as I said above, by this time I was quite cynical. I had no intention of playing my role in a charade. I thought that they would go through the motions but in the end tell me no, because black people don't go to places like MIT.

As I would get these letters, I would take them home and my father would also read them. So he was well aware that I had gotten one letter inviting me to apply to MIT. A few weeks later I got a second letter from MIT and my father asked, "Have you done anything about applying?" I said, "No, I'm not going to apply to MIT." I had gotten some good offers in these letters in the sense of people encouraging me to apply. The place that I had decided to apply that was farthest from home was Michigan State University. They had put together a really nice package and seemed very serious about inviting black students to join their student body that year. In fact, this was the year that most of the major universities first actively began seeking to recruit capable African-American students.

That was when?

That was 1969. My father's response was, "How are you going to pay for your education?" I said, "Well, I was going to ask you and Mom for help." I called my stepmother Mom. He said, "Really?" "Yes." "Michigan State?" "Well, you know that's probably where I'm going to end up going." "What happened to MIT?" "They don't let people like us go to places like that. I don't feel like wasting my time and being told no." "Well, you're going to apply to MIT."

The relationships in our family were such that what dad said was what happened. So I dutifully applied to MIT. Of course, the truth was that I desperately wanted to get into my dream school. But with the foolish but common bravado of a black teenage young man, I was going to be too cool to fall into the trap of a perceived put-down.

A few weeks later, one day I came home from school and dad was sitting in a rocking couch that sat out on our porch. This was highly unusual because very rarely did he arrive home before I got home from school. He was sitting there with a big smile on his face and I knew, the instant I saw his face, that I'd been admitted. I ran to him and got a big hug.

That's really a great story.

Maybe, but we have lots of weird stories like that about our lives.

So you got accepted. What happened after that?

I got accepted and I got an invitation to come to Project Interphase. That was the most important and intense academic experience I ever had up to that point in my life, and it remains so. It was the first time I had to be disciplined about homework and learning. Before, I had always been on "automatic pilot" with regard to school, at least since second grade. In high school, I had done things like read comic books in class.

In fact, my physics teacher has this amusing story that he has told a couple of times about how he came to realize that I might actually become a physicist. The story goes that one day in class he noticed that one of his students had a comic book hidden inside his textbook and was reading it. So he decided to embarrass the student into paying more attention. He asked the student a question and the kid answered without looking up. He asked the student a second question and once again the student answered. He finally asked the student a third question with the same response. At this point Mr. Coney decided that the kid might actually become a physicist. Of course, the kid was me. I had forgotten this until he reminded me of it a few years ago, in 1993.

So planning to go to MIT was a really serious transition to working hard in school, leaving behind the South with its segregation—or so I thought—and getting the opportunity to deal with other environments in this nation. I had grown up hearing about this "Athens of America" called Boston. It was a shock coming to Boston, because there I experienced more explicit racial hostility than anywhere I have ever lived. I thought then that it must be the most racist city in the country. These impressions were molded beginning with my experiences in 1969, which I might add were well prior to the period of busing and its attendant civil unrest which later attracted so much attention to the city.

How could you tell that you were experiencing this?

That was the simple part. European-Americans, natives of Boston, were very explicit about their feelings. At some point during the first couple of weeks of Project Interphase, one of my classmates—I think it was John Mack—offered to take

some of us in his car. A number of us black students wanted to ride around the city to just get a better feel for it. We piled into his car and went riding around. By accident, we ended up in South Boston and literally—I had never had an experience like this before—as we would ride down the street during that evening or late afternoon, people who had previously been sitting on their stoops talking to one another got up and starting chasing the car and/or yelling. I will never forget the impression that occurred to me, it was like being chased by a pack of wild animals! We were shocked and, fortunately, able to make a hasty retreat. This was my introduction to Boston, and I have other such stories.

That had never happened in Florida.

Nothing like that had ever happened any place previously in my life. It was quite common for groups of black students to be walking along Memorial Drive next to the river by MIT. European-Americans would be driving by and they'd just started yelling "nigger" right out of their windows, for no apparent reason. This was a commonplace event and another part of my introduction to Boston.

The summer program was very intensive.

It was very intensive—in fact, so much so that I was physically ill by the end of the summer. When I went back home at the end of Project Interphase, I slept for two straight days. During the second day, my father came into my room found me asleep and began to wonder what they had done to his son. Later, he said he was about to call MIT and inquire. I told him that I was just tired. Next something remarkable happened. He said, "Tired from doing the work?" I said, "Yes." "Well, do you think it is going to be too much for you to go back? Do you want to stay home?" "No." "Well, wait a minute. You're my son, and if it's going to be physically damaging to you, I don't want you to go back." It was really funny because I thought I had permission to fail. I told him, "No, I'll manage."

I went back because Interphase had geared me up for MIT psychologically and mentally, so much so that I had a strange freshman year. I had understood the magnitude of what would be expected of me from my time in Project Interphase. So the first semester I actually took a "light load," four courses. I found that Project Interphase had been more difficult than this. So during my second term, I took six courses including 18.03, the course in differential equations. Ordinarily, this is taken during the sophomore year at MIT. But since four courses had felt like less work than Interphase, I had thought that six courses might be just about right. Six courses turned out to be a little bit harder than Interphase, but I stuck out the six courses and finished the 18.03 course during my freshman year while I was still on the pass/fail grading system.

That's unusual.

Yes, it is a little bit unusual. As a kid, you do things because you don't know what is the expected. You just do them. By the time my freshman year was completed, I had completed 18.03 and was thus able to take lots more and more diverse courses for which it was a prerequisite during the first term of my sophomore year.

Did you at that point know that you were going to major in physics?

Well, those were my hopes. As I said, I took a physics course in the eleventh grade in high school with Mr. Coney. Within two weeks of beginning that course, I knew I wanted to be a physicist. But my interest in becoming a scientist went back to the fourth grade. An event occurred in the exchange between my father, me, and my myriad questions that pointed me in this direction. The event surrounded books that let me know that the stars in the night sky were places to which one might travel.

So nothing had changed your mind yet at that point?

Well yes, something did change my mind. Although I wanted to be a physicist, I didn't know if I was going to be able to become one. In fact, as you know, I have two bachelor's degrees from MIT. That's the result of an accident. It was not purposeful. What happened was that as an undergraduate I wanted to major in physics, but I had better grades in my mathematics courses than in my physics courses. So I declared myself a mathematics major during my sophomore year, but continued to take physics courses on the side because that's what I really wanted to do.

Then at some point during my senior year one of my fellow black students said, "Well, Jim, you've been taking courses in both departments, haven't you?" I said, "Sure." She said, "So why don't you just take both bachelor's degrees?" I had never known that you could get two bachelor's

degrees. I went to the physics undergraduate office, and they said that all you had to do was to take some lab requirements and that was it—and do a thesis. I had enough credits to separately take the bachelor's degree in physics along with the bachelor's degree in mathematics. I began work on an undergraduate thesis under the supervision of Professor Ingard in the physics department. I didn't quite get it finished in time in June to get the physics bachelor's degree, but I finished writing up the thesis—on a problem in acoustics—that summer, so that by fall I had also gotten my bachelor's degree in physics just prior to going to graduate school.

When you look at that undergraduate period, what are the highlights of your experience?

First of all, this was in the period of 1969–1973. This was the height of the "black revolution" and anti-war movements on campuses across the nation. In the vanguard of the former, there were the Black Panthers, the Republic of New Africa, the Nation of Islam, et cetera. The killings at Jackson State and then Kent State occurred during my undergraduate years. Woodstock was something people were talking about attending, and the first manned landing on the moon occurred during my first summer at MIT. It was a time that, upon looking back, I wonder how we all survived.

This all had an impact at MIT. From my dormitory room, I watched the "tactical police" march onto our campus in full battle regalia, also known as riot gear. This gave me a small sense of what it must have felt like to watch brown shirts and goose-stepping soldiers march in other places and times. The expression "power of the state" was rendered starkly real. I can recall members of my fellow African-American students at MIT deciding to quit school to "work for the revolution and Nation-time" by joining the Black Panthers or RNA. Others proselytized for these organizations attempting to enlarge the membership.

I don't recall the anti-war movement having such a large impact on most black students. Mostly I think this was because many African-Americans, even though we very well understood that disproportionate numbers of African-Americans were fighting in Vietnam, had the attitude that for a lot of us it was only by the sheerest chance that we had the opportunity to attend these universities. Had I been born a year earlier or started first-grade one year earlier, I know that there was no way that I would have had the chance to go to MIT. I recall that after the Cambodian incursion, a boycott had been called for among MIT students. I and a number of other African-American students ignored it. One day as I was going to class, a southern European-American fellow MIT student stopped me and tried to convince me that I should show solidarity and not attend classes. I told him that I could not afford to skip class and that too many people had fought for the right for me to be there talking to him.

On the other hand, you confront world-class scientists and engineers for the first time in your life and the standards that they uphold in their professional operations. I think for everyone who comes to MIT, that is a huge adjustment. As I had said earlier, I started to make that change during Project Interphase. The bulk of the transition occurred during my undergraduate career when I had Nobel laureate-caliber or near-so individuals as teachers and actually asking me questions! I usually managed to do okay.

Looking back, I'm not sure how I did it. I remember lots of all-nighters, "pulling all-nighters" as we called it, when I would stay up all night to do my homework. I also recall a lot of good comradeship with my fellow African-American students there. We were really a community. I don't know about anyone else, but I could feel the support of that community when things seemed about to fall apart personally. To have friends with whom you could go and shoot pool or bowl, sit down and discuss philosophy and the revolution, talk about what you were going to do after you graduate and why that was relevant to the larger black community, was a privilege and an invaluable resource.

A number of us black physics students had a mythical place that we called "the Colored Center for Theoretical and Experimental Physics—CCTEP." The name was both a joke and a serious challenge to the circumstances in which we found ourselves. I was also a founding member of the Black Student Union Tutorial Program, a self-help effort to assist some of our younger colleagues academically at MIT. As a member of the BSU bowling team, I found a form of physical competition that was my style. In my final year on that team, we won the intramural championship. Of course, the biggest highlight was graduation, which I did not

attend since neither of my parents came up. Nonetheless, this was a very, very happy occasion.

All of these things, and many more, went on within the community of black students at MIT during my time there. That sense of community was a great source of support when things looked bad and you needed to have friends surround you. It was like being in a war and having these people in a foxhole with you.

Talk a little bit more about that. When you look at the community at that time, was it more embedded in the organizations or was it more embedded in residence halls? Where was all this community?

That's a complicated question to answer. Partially, it was certainly embedded in living groups. ("Chocolate City," a predominantly African-American living group in one of MIT's residence halls, had not yet come into existence.) It sort of evolved out of one of the living groups, one in Burton-Conner, or New House, or whatever. The point is that it is an output of a process, not an input. There were similar groups in other halls—for example in East Campus, where numbers of my friends such as Dave Lee lived. A smaller such group, of which I was part, lived in Baker House, my dormitory. Ron Blount, my roommate during my freshman year, and Elliott Borden were members of this group. So in a sense, you had your living groups where the smaller black communities existed. These were also components of the BSU, since we all participated in its activities.

What about influences in terms of faculty during that period, particularly on the academic side?

First of all, there are a few of my instructors who played a very big role when I was an undergraduate. The first was probably Vera Kistiakowsky, who was my instructor in the physics course 8.01. Throughout most of that course during the first semester of my freshman term, I had struggled. At the mid-term there was scheduled an advisory meeting. I recall her sitting down and asking me, "Well, Jim, what's going on? You seem to be having trouble." Yet by the end of the course I had done very well, especially on the final. At my final meeting with her I remember her being very happy—a smile on her face—with the mastery I had shown—"Look at what you have done on the final. You really learned the material, period." To get that kind of encouragement from a professor, what student would not want to hear something like that? She was certainly a standout in my mind.

This was during your freshman year, right?

Yes. Some other people played a very important role. There was Brian Schwartz, no longer with MIT, but then affiliated with Concourse, an independent study program, and with the Bitter National Magnetic Lab at MIT. Brian more than anyone else really started me seriously thinking about how to become a good teacher. He did that while he was an instructor in Project Interphase. At that time, summer 1971, I was a calculus tutor and in later years became an instructor also. But Brian was the first person who made me think about what it meant to be a good teacher and what one has to do to become one. He was certainly an outstanding example, and I have the imprint of his presence stamped on my entire career as a teacher. It was because of my joy at being a teacher that I began to think about a career in academia, in addition to wanting to become a physicist.

Other people include Professor Margaret MacVicar, with whom I did not interact so much as an undergraduate, but certainly more so later. There were a couple of people who were crucial in my going to graduate school in physics. One was Professor David Frisch and the other was a retired vice president whose name escapes me.

Al Hill?

That's right, Dr. Hill. They, more than anyone else at MIT, are responsible for my continuing on to graduate studies there in physics. So I bear these imprints as I have gone through my life. Then finally, of course, there was Jim Young, whom I met on one occasion as an undergraduate. I think I was a junior, and the regular lecturer for one of our courses in electrodynamics was away. Jim was a guest lecturer. He came in, gave a lecture none of us understood, and I said to myself, "I never want to work with that guy!"

You mentioned shifting from being an undergraduate, but is there any other highlight that you want to mention before we move on to talk about how you made your graduate decision?

Highlights of my undergraduate experience are far too numerous for me to describe. It would take hours and hours just to relate lots and lots of things. Most important for me were my friends, without a doubt. Sure, there were requirements of

the course work and what have you, but it was the people who made MIT a livable place, a place where I could survive and eventually thrive. People like Elliott Borden and Ron Blount were great friends.

Let me tell you just one story about something I like to call "the Great Race." Elliott ran for the MIT track team. I did not do much of anything competitively, except to bowl. One day Elliott looked at me and said, "Gates, look at you. I bet you're so out of shape because you don't get enough exercise." I said, "Well, I'm sure that I could beat you at whatever it is that you think you are good at in track." I was probably a little crazy in those days, like most young men.

So we decided to have a race. The race was to be a bridge circuit—a middle-distance race. We started from Baker House, ran down the river until we got to the Boston University bridge, crossed over the river, ran up in front of BU back to the Harvard bridge and returned to MIT. Elliott figured that he would just burn me and a couple of the other fellows who also decided to join us. So anyway, we had the race and to make a long story short, as I recall it, I won! He was amazed because I had never done any athletic training for this challenge.

And he was on the track team?

Yes, he was on the MIT track team. So that sort of thing happened among friends. I began bowling due to my friend, Ron Blount. I took it up during my freshman year while we shared a room in the Baker House dormitory. Ron was a fantastic bowler when I met him. Bowling became very important to me while I was a student.

I was never a very good test taker the entire time I was in school. I seemed always to have the experience of sitting down at an exam, spending at least twenty minutes to half an hour with absolutely nothing written on my paper except my name, because I was simply astounded by what questions were being asked. I was in shock, basically. I would sit there and then with around half of the test period remaining, I would slowly begin to bring some kind of order to my thinking and salvage something. I'd jot down a little and then a little more as understanding would dawn on me. I'd jot that down. At the end of the exam, I'd be going as fast as I could trying to get the thoughts out of my head and onto the paper. Then there would always be a couple of problems which I now knew how to solve, but it was time to quit. Then about five minutes after the exam, more answers would show up in my head. You cannot tell the instructors then!

Tremendous amounts of frustration would build up from this pattern, and the way that I kept myself sane was to go bowling. I'd bowl four, five, or six games in a row and physically work out the tension. In those days there was a bowling alley at the student center at MIT. It was for me a life saver.

In the basement.

That's right, in the basement. So bowling was very important. It was the comradeship of friends that got me through as an undergraduate. My professors were great in terms of the technical competence, but that's different.

How did you decide on your graduate program?

By the time I was a senior and about to get two bachelor's degrees—one in mathematics and one in physics—and still with an idea that I loved physics, I had a very clear understanding that grades are important markers for admission to graduate school. So I applied to a number of places across a range of disciplines. I applied to Stanford, to their plasma physics program, I believe, and to their applied mathematics program. I applied to Michigan State again, I think. At MIT, I applied initially only to the mathematics department for admissions to their applied program.

This is where Inez Hope, a fellow student and friend, Al Hill, and Dave Frisch came into play. I had by the time of my graduation been convinced that I would not have the opportunity to get into graduate school in physics. I simply did not think my grades were good enough. The inability to perform well on tests showed, and I was not a straight "A" student in physics. Inez, during a conversation about graduate schools, suggested that I could talk to Al Hill about graduate school because she thought he was someone who could give me good advice.

So I went to see him, he sat me down, and we talked. I guess he got the idea that I was a pretty serious guy about studying physics. He said, "Well look, I don't know how good a physicist you are. How did you do on the graduate exams?" I gave him the numbers. He said, "Well, that's not so hot." I said, "Yeah, I know. That's why I'm not thinking about trying to get into graduate school in physics." "What test did you take?" "It was in mathematics." "What does that have to do with physics?" "Well, I

didn't take the physics exam." "Look, I'll send you around to one of my colleagues, Professor Frisch, and we'll see what we can do about your getting to take a physics exam."

The way the system worked in the department those days was that first-year students had to take something called a qualifying physics examination. They used this to assess what was the nature of the undergraduate background that incoming students brought with them. So they essentially gave me one of the recent past versions of this exam and I did pretty well on it. Why my usual exam panic was not there, I have no idea. So even though I hadn't taken the usual achievement test in physics as a graduate student on the Graduate Record Examination, I had this other examination which sort of indicated that, gee, this guy's gone through four years and actually learned some physics. So this result was used as part of the decision as to whether I should be admitted to the graduate physics program at MIT.

To my very great surprise, I got admitted to a complete physics program offering a wide range of options for an area of specialization. I was then confronted with the question of whether I should study mathematics or physics as a graduate student. Also, the question of whether I would do so by leaving the metropolitan Boston area arose. I had to face the question of packing up all my stuff—in those days, it seemed like a lot—and moving it across country to as far as possibly Stanford in California. In the end, I decided that it was too much of a bother and decided to stay at MIT.

This is a perfect example of just how silly my decision-making process was sometimes when I was a young person. This was one of the most important decisions of my life and, to some large degree, the decision was made out of laziness!

Now, that particular phase certainly suggests that one has to have some real strong advisors in terms of your field of interest. How did that emerge in terms of the people who became your advisors at that time?

Do you mean as an undergraduate?

As a graduate student. I'm moving to your graduate level.

Well, this is a difficult question for me to answer. None of the individuals whom I named in the story above were formally assigned to me as advisors. In fact, to me the most remarkable thing about the story was my luck in having friends who knew with whom I ought to be discussing such matters and the fact that I had just enough sense to follow the advice of the people whom I encountered. You will recall my earlier telling you that by the time I graduated from high school, I was quite cynical about the way that things really work in our world. I still think it was dumb luck that things worked out pretty much as I had dreamed while a senior in high school.

During my time as an undergraduate, there were certainly people formally assigned to me as advisors. Although I would normally see these people once or at most twice a semester, I never actually sought them out for advice. I sort of always knew what I wanted to do, so I did not need advice on direction. At least, I thought it was more clever or more efficient to watch what other graduate students with aspirations similar to my own were doing. For example, in my second year of graduate school, I noticed that a lot of the graduate students with whom I had been taking courses were then starting to work on research problems with faculty members. That prompted me that it was time to go and talk to someone about doing research.

I never really relied on advisors the entire time that I was a student. A lot of this stemmed from my natural distrust of "the system."

If I remember correctly, your official advisor was Jim Young.

For my Ph.D., yes. He was indeed my thesis advisor.

How did that happen?

Well, that's also a story. Of course, you can get tons of these stories from me if you keep asking. Remember I told you that I had met Jim Young two years earlier as an undergraduate. So in my second year of graduate school, I began going around talking to various professors about perhaps doing a Ph.D. under their supervision. I knew that I was interested in theoretical or mathematical physics research. I also knew that I wanted to do something on gravity, so it was a big issue in my mind whether I should think about general relativity or about particle physics. In fact, I also knew that it was actually quantum gravity—something that really did not exist—on which I wanted to work.

In the end, I decided that general relativity and working with people trained in general relativity was not the way for me to get to my goal and that particle physics was much more "physi-

cal"—grounded in experimental observation. So I went to a couple of faculty members in the Center for Theoretical Physics—Professors Ken Johnson, Roman Jackiw, and Jim Young. I first went to Ken, walked into his office, introduced myself and said a few words about what I'd done as an undergraduate and graduate student, and sort of asked him if he was taking on any graduate students. The response was, well, that he really would not advise me to think about working in this field. I was highly insulted! This person essentially knew nothing about me and yet he was there making a snap judgment that if I heeded would not have let me pursue the career I really wanted. He explained that the basis of his advice was the tremendous nature of the competition, how fads—most often of little lasting value—regularly shake up the field; it's a highly non-linear field—the politics are something awful and just plain difficult to work in. I was still insulted to the hilt and walked out angry.

Many years later, I was discussing our field with an older colleague and he revealed that the statistics are that about one in forty new Ph.D. holders in our field go on to become tenured professors in the field. Of course, now when students first come to me to ask if I will advise them, my initial response is almost always that they should consider another area of physics. Ken did something very valuable and valid for me, although at the time I did not recognize it.

What do you suggest such students do?

Initially, I tell them go do something else. I tell them essentially what Ken told me—go do something else, make your life simpler. If a student will be turned off by my telling them that they cannot do this field, then they don't need to become my student. It takes a determined personality—a thick skin—to attempt to work in this field. If a student will come back a second time, even if it is not to discuss it with me, then it shows that internally they have a sense of self-confidence that will direct their efforts toward weathering their future travails. I very much believe in this. There's a second part to the Ken Johnson influence in my career, but I will come to that shortly.

Next I went to Professor Jackiw. He was the person that all of us theory students wanted to work with, at least figuratively speaking. He was the guy with the best reputation among us graduate students. There is a mathematical result of deep physical significance which bears his name, the "Adler-Bardeen-Jackiw anomaly." We graduate students were like, "Yeah, who wouldn't want to work with him?" But he had lots of students already, as a consequence. I sort of delayed my approach to him and by the time I did speak with him, he said, "I really have too many students as it is. Why don't you talk with some other people in the CTP?"

So at that point, another friend—a female senior undergraduate in another department—suggested that I talk with Professor James Young in order to get some advice about approaching faculty members, and also that I should seek his advice on solving the problem about getting started at research, but not necessarily to become his student. His reputation for being incomprehensible was still very much in effect. However, he had also been the thesis advisor to Dr. Shirley Jackson, which showed that a student could reach the Ph.D. under his supervision.

So I went to talk with him and he asked what I had done. We had a very pleasant conversation. I told him what was my background and what were my areas of interests. He said something like, "Well, let me think about it for a few days and I might come up with some useful advice to give you. I don't want to take you on as my own student because I don't have any appropriate problems on which you might work. I'm sure with some thought, I can recommend what you should do next." A few days later, he actually called back and said, "I have a problem that might be appropriate for a beginning student after all. So do you want to discuss it?" We did and, in the end, the problem he described seemed very interesting and something that with some effort I could complete. It was a question he and some collaborators on a research paper had not answered.

So I said, "Sure, I'll work on this stuff." Very quickly I found that I was learning new material. I had reached a certain level of mastery to do the particular problem he had asked me to research and, I think to his surprise, it was done in fairly short order. I reported my results to him, but we did not publish it. My result supported an assumption that had appeared in their paper without rigorous proof. Interestingly enough, however, the method that I used to solve this problem—technically, "finding the most general but irreducible

form of a Higgs potential given an arbitrary group and field representations"—appeared later as a published paper by a faculty member at another university. The fact that I had independently thought of this method prior to an actual professor, gave me a tremendous boost.

So at this point, I basically became Jim Young's student. I got sucked into it by the material. For me, it was just an excellent student-advisor relationship. In the end, what Jim provided for me was complete freedom to do exactly what I wanted to do. Although he started me out doing research in a particular area, it transpired that by the time I had completed his assigned problem, I started generating my own questions in the area and that led to another few research papers. By that time or shortly thereafter, I realized that if I kept my focus on this topic it would be very difficult to distinguish myself from many other young people, because there were essentially "cookbook recipes" that one could use to carry out this type of research. So I began to look around for something that I thought would enable me to make my own mark that could clearly be discernible.

I made a survey of the research literature—this was 1976—and I found this one topic called "supersymmetry" which had only been proposed beginning in 1974. It was really new and therefore had to have lots of room in which to do fundamental research. I also had the intuition that this subject had the potential to become something extremely important, because it was based on an idea that had never before been proposed in theoretical physics. So I picked this as my next area in which to work and told Jim that's what I wanted to do next. He asked if there was anyone in the department with whom I could discuss this. It turned out that no one in the MIT physics department knew anything about this stuff. So I basically ended up teaching it myself—to me—by reading the research literature. Jim was always there to discuss things when I got stuck, but otherwise he let me have my head in attacking this. I assume that he, by then, had enough confidence in me to think that I would not get myself into trouble.

Very quickly I was able to get to the forefront and then I was able to contribute my own new ideas that had never previously appeared in the physics literature and which contributed to the progress of this new topic in physics. It was pretty exciting stuff for a pre-doctoral graduate student. Jim was perfect because a lot of advisors would not have permitted a student to strike out so totally in an untested direction. They would have advised the student to take a safer direction. When I began working on supersymmetry, there seem to be a general attitude among theoretical particle physicists of, "Gee, that stuff you guys do isn't really the 'best' theoretical work under way." Now more than twenty years later, the topic of supersymmetry is one of the dominant themes in the entire field—supersymmetry, superstrings. All of this follows from a sort of natural evolution in the development of the topic.

So I found myself doing research that is at the foundation of much of today's activity in the field. In fact, just about three or four days ago, I noticed a citation by some researchers in Japan of my very first paper written on the topic in 1977. In that paper, I gave the first suggestion of something called a "supersymmetric gauge phase factor or SUSY Wilson loop factor." I have been very fortunate in my career in one way. I have been the first person to describe a number of results which later have shown themselves to be of huge interest in my field. Another such example is something called "the N = 1 formulation of N = 2 SUSY YM." I first wrote about this in 1984, and now this is a part of an exciting development called "Seiberg-Witten theory." In fact, my thesis in 1977 became the first one at MIT that included research on problems in supersymmetry. So I was extremely lucky in my choice and the accident of the sense of my aesthetic or intuition that I bring to the business.

You are at the cutting edge of it. If I hear you correctly, you chose a field that had just emerged two years before you actually got into it.

That's right.

You're an undergraduate and graduate product of MIT. What kind of advice would you give a young Jim Gates coming in the door on the undergraduate level as well as the graduate level?

I often tell people that I don't give advice, but I will tell people what I found to be useful as I was going through the various early stages of my career. The most important matter, both at undergraduate level as well as the graduate level, was my mental health. A lot of it had nothing to do with intellect, but a whole lot to do with how I felt about me. These are not necessarily things I would

want a student to emulate. For example, when I was an undergraduate, almost every semester for about two weeks I would not go to class. The reason was because I would instead be sort of using that period to mentally clean up whatever was going on inside of my head. Now that meant that, of course, I did no homework. Obviously, I would not schedule my "vacation" so that I missed any tests.

So what took place on the vacation? Well, I sort of just sat around in a sense and took stock of the mental furniture and rearranged it as necessary. And this process worked for me as an undergraduate, even though when I "returned" I was behind in some sense in my classes. In graduate school, I don't recall having to do this. I think it is paramount that one look to one's own happiness and find ways, even if they are only small ones, to make oneself feel a sense of accomplishment and with options to exercise, no matter how bleak things appear.

The bowling was also a large part of that for me. As I said before, to get over the frustration I felt with the way exams would go for me, I'd go practice bowling, and think often about the ball's angular momentum vector while doing so. I think I mentioned that in my final year at MIT, my team won the bowling championship. It was a very sweet victory, as we defeated a team to which we had lost the championship two years earlier by just two pins! My contribution to the championship says something about how much practice I had gotten in.

There is an epilogue to this. When I was at Caltech as a postdoc, 1980–1982, my teams won the championship of their bowling league, too, for both years. So clearly I had to have had lots and lots of frustration with exams, which meant lots and lots of practice. I tried to keep my personal life in some orderly manner, with varying degrees of success and failure. That was extremely difficult, since as a young adult I was grappling with issues of choice, behavior, self-esteem, and the respect of those who mattered around me. I found I had to have places to put those things and the time to sort through them. Friends mattered tremendously and assisted me in keeping my perspective.

My choice of friends was critical to me. For example, there were three of us buddies during my freshman year. We called ourselves "SyRonSyl"—two guys and a lady—and we discussed philosophy and all sorts of things. We composed aphorisms—for example, "That which pleases little changes, that which changes little pleases." We went to the movies together, and so on. While these final stages of reaching maturity were occurring, assuming I actually did reach it, there was the challenge of an intellectual awakening under intense academic pressure. This lent itself to some really weird effects in my life. The way I learned to integrate in calculus owed itself to a dream; I had one friend who, upon being awakened, shouted out "pi" to six decimal places, or so I was told.

So all sorts of areas were important and had to simultaneously be managed. I had to find a sense of balance and maintain that internally. A sense of self-worth was the key. I did not ignore its maintenance. Those are the things I consider important to say on these issues. Those things that are deeply personal cannot and must not be ignored.

It seems like I remember something about you going to Harvard.

Yes, there was something about me going to Harvard. In fact, Ken Johnson, the same professor who judged me to my discouragement, played a critical role in this. When I was finishing my Ph.D., Professor Young had the idea that he wanted to have me nominated to become a candidate as a junior fellow of the Harvard Society of Fellows. Nomination for this could only be done by members of the organization and former junior fellows. In our department, Professor Johnson was one such person. So in fact, Professors Johnson and Jackiw along with Professor Young were responsible for opening that opportunity for me to go to Harvard.

These are Nobel laureates?

No, however, they are both at a high level of accomplishment and vastly well recognized for their contribution to the field of physics. Unfortunately, neither has been recognized with that prize.

They're on that level, though.

Yes, they're certainly top-ranked theorists by anyone's standard. So they nominated me to become a junior fellow of the Harvard Society of Fellows at Harvard University, the significance of which was not clear to me at the time.

What did it mean?

What did it mean? Well, it matured me professionally in a way that I appreciate now much more than at the time. It sort of got me used to a type of life in academia and academicians that I found extremely useful to have as knowledge and experience. It showed me that even those who are considered the best across a broad array of disciplines were not that different from ordinary people, i.e., me. Along these lines, I particularly recall being appalled at some of the views that I found. A young junior fellow in the social sciences once commented that he thought that the fate of Native Americans was perfectly moral because the European settlers who forced them from their traditional homes were able to use the land more efficiently to support human life. I thought that argument would be welcomed among common thieves. I could see one making the argument that he was justified to take my possessions because he could use them "more efficiently."

Let me talk a little bit about the process and then come back to your question. To become a junior fellow is considered a very prestigious accomplishment for one's career. The process includes the Society looking at documentation on your potential for future contributions to your discipline. But there is another component in being selected. They try to look at you in a broader context. They were not just asking, "What would Jim Gates become as a physicist?" The question was, "What would Jim Gates become as a scholar and citizen?" Part of this assessment process included an interview in what I then called a "star chamber proceeding." Of course, it was not really so, but to me it seemed in advance that it might as well be.

I was called to appear before some of the senior fellows of the Society in a room where they were seated around several tables positioned into a U-shaped arrangement. I sat in a chair at the open end of the "U." Distinguished senior fellows were seated all around the outer edge of the "U" and asked a series of questions. I believe I did pretty well in this. Foremost, there began questions from people in my discipline that established that I was not a complete dunce therein. After that, the questioning opened up to a more general phase. I recall one questioner asked me of my interests outside of physics. What did I do for hobbies? One of the things that I did then, and still now, was to read and study history. For example, during my freshman year at MIT, I had written a paper entitled "Bethe, Oppenheimer and Teller," about a well-known incident after the end of the Second World War. My revelation had an amazing effect on the room. They recognized that they had before them a physicist with a serious interest in an academic but non-scientific area. We started talking a little bit about what periods of history I found most interesting and why I thought that history was an interesting topic with which everyone should be familiar. A few weeks later, I learned from Professor Young that I had been selected.

Now back to the question, what did it mean? Well, junior fellowships are intended to allow young people maximum flexibility to intensively research their chosen field prior to the time they become apprentice faculty members, assistant professors. So I would do my postdoctoral research at Harvard in their physics department. I had already begun a trajectory studying the topic of supersymmetry. Like at MIT before, there appeared to be no one at Harvard who knew anything about it. This was certainly true of the senior faculty and postdocs already there. I thought that I'd continue teaching myself in isolation except via links to a larger community through the research literature.

However, it was not completely true that no one there had an interest in the topic. First of all, I met Warren Siegel, who was to become my best friend and collaborator. Warren was also a new postdoc who had been an undergraduate at Berkeley and who, like me, began to teach himself in isolation about supersymmetry. We were introduced by one of the secretaries, Blanche Maabe, at the beginning of the academic year. Within a few minutes of meeting, we were deep into an argument about the nature of geometry for a theory of "supergravity." Initially, I concluded that he was crazy and he likely concluded that I was a dunce. But later that year, we began to work together to develop the first mathematically rigorous and geometrical theory of supergravity in superspace. This is sort of a successor to Einstein's theory of general relativity but with an added level of complication, called "pre-potentials," with which Einstein never had to contend.

I also met Ed Witten for the first time. I had seen but not spoken to him a year earlier, during the visit of a distinguished Dutch physicist to Harvard. Ed is presently considered the most influential theoretical physicist of my generation. He was a postdoc and also a junior fellow. By the way,

both Ed and Warren are geniuses in my field, although Ed is more widely recognized as such. Both of them are geniuses. I know this with complete certainty. They are the only two people I have met in my career about whom I can make this statement. The majority of the researchers in my field have logical thought processes which for me are completely understandable, even if sometimes with some difficulty. We sort of think in what I call a "linear" manner, reasoning from point A to point B, et cetera. But true genius does not work like that. Instead, it is "nonlinear" and capable of going from point A to Z with no apparent stops in the middle.

This had good and bad implications for me when I had this realization. First of all, working directly with Warren was a shock. He is so smart that my first reaction was, "My God, if this is what all my competition is in this field, I have made the wrong choice." Well, fortunately for me, by working with Warren I came to learn that people like him and Ed where extremely rare. Most of the competition was just like me, so I thought that was okay. I could deal with competing with the vast majority of other theoretical physicists.

You could deal with third place, right?

I could deal with competing with people just like me because I figured that I work hard, they work hard. That was okay with me. I was sure that I would win some and I'd lose some, but I'd be okay in the end. But if it had been the case that Ed and Warren were the standard, default theoretical physicist, it would not be a fair competition. I would need to do something else. Well, they are not the standard.

The thing that I really appreciated about working with Warren was that he caused me to grow as a physicist also. He was just an intellectual powerhouse, and one had to react in one of two ways—either you run and cower somewhere or you throw caution to the wind and say, "I don't care how stupid I appear, these are my ideas and I will let you confront me on them just as I will confront you on yours." Out of this process, there emerged a rigorous and geometrical theory of supergravity.

As a physicist, I came to understand that I had to trust my intuition in a way that I had never understood as a student. Warren had a saying that you should never begin a calculation before you know the answer. That is in some sense correct. It is a little like composing music. One has to have the idea for what will become a tune and this precedes placing the notes on bars in a score. There's a conceptual framework that one should have that is outside of the mathematics we use to do physics. The measure of the talent of a theoretical physicist lies in how close his mathematical "tunes" are in describing nature.

I came to such realizations during this period while working with Warren. I've never been afraid to tell people exactly what I think is right, even if it appears 180 degrees out of sync with what most people think is correct. This has allowed me to repeatedly derive interesting new results before numbers of others. I also learned that even genius has "logical blind spots." There are peculiar ways in which any one person thinks that potentially allow that individual to conceptualize something in a given problem even prior to a genius.

You do not have any hesitation about indicating what you think, even if it's totally different from anyone else's comments.

I have never had such a fear since working with Warren. That's a consequence of my interaction with him. Being independent in one's direction of thought is very important, especially in my field. As Professor Johnson had warned me many years ago, there are regular "fads" that roll through the field and one's sense of direction is critical in attempting to assess whether any one may prove of lasting value.

Many years ago, a young fellow-physicist asked me if I had noted how our field was like a strange football game. First, a person kicks the ball as hard as he can and then a crowd of people run off after it. The first person in the crowd who gets to the ball then repeats this, but does so in a totally random direction and the crowd takes off after it again!

During my postdoc at Harvard, the independence of thought proved to work well. However, it did so because there were a couple of fine people with whom I could talk about supersymmetry, including Warren. Another such person was a graduate student, Martin Rocek, who was about to finish his doctoral program. Through Martin, I met Marc Grisaru, a professor at Brandeis University. We sort of hung out at Harvard and wrestled out ideas about supersymmetry that later

led to our book on the topic that was written at Caltech. As we were all leaving Harvard, another physics professor I know commented to me that Harvard was "losing all of its superheroes." By the time of our departure, the topic still had not gained respectability generally.

Now, are these the people with whom you worked later at Caltech?

Yes. What happened was that the four of us sort of became a team or, in the language of us physicists, became "a quasi-bound state." We created a book called *Superspace* in 1984, which was the first comprehensive and advanced treatment of the topic of supersymmetry. So yes, these are the same people I first met at Harvard. It has only been in the last two years that a work as comprehensive in scope has appeared.

I remember the conversation at the time in which you were so excited about these guys.

Oh, yes. It was a very, very great group of guys with whom to work.

Where are they now?

From 1986 to 1988, Warren was a colleague of mine here at the University of Maryland at College Park. He's presently at the Institute for Theoretical Physics at the State University of New York at Stony Brook. Marc Grisaru is still a professor at Brandeis University. Marc and I actually wrote a research paper last year, together with one of his students, for the first time in a number of years. He and I, along with others, will likely continue to work on another paper. Warren and I remain the best of friends after these numbers of years. Certainly in the last ten years, I've probably talked to him—that's not quite true, but was up until quite recently—more than I have to my own wife. Warren and I still communicate quite often via internet. He is absolutely my dearest and closest friend.

That's fabulous to have a friend like that.

Hey, having a genius for a friend ain't bad. We don't generally work together now. It's been years and years since we've actually done research together, but just to have someone of that caliber with whom I can discuss physics is really, really nice.

It's a tremendous advantage. When you finished Harvard, you began to do several postdocs, if I remember correctly, and traveled all over. Talk a little bit about that period.

After Harvard, there was a postdoc at the California Institute of Technology (Caltech). The way that it happened was that in 1980 John Schwarz, who a few years later would become widely recognized for the proposal of "superstrings," had met me while I was a postdoc at Harvard and had become interested in the work that Warren and I carried out investigating the foundations of supergravity. So I joined Warren at Caltech as a postdoc for two years, 1980–1982.

Immediately after Caltech, I was shocked to find myself an assistant professor of applied mathematics back at MIT. To some degree, it was a case of "you can't go home again." When I was a student, the entire time I was an undergraduate and graduate, I absolutely loved the place in the sense that it was my intellectual home. It was the place that, intellectually, I had blossomed. It was a place that I had been convinced was different from most other "Ivy League" universities in this country, because I thought that at MIT it was much more likely that as an African-American I would get a fair hearing on my research accomplishments. I had felt something akin to this as a student. Coming back as a faculty member and looking at some of the things that I had not been able to see as a student, I concluded that it was, in fact, subject to the same failings as others in its cohort of universities. After spending time at Harvard, I essentially developed a very negative view about the ability of most of the nation's "best" universities to accurately and impartially judge the accomplishment of any African-American scholar in a technical discipline.

Say a little more about that.

That's my personal opinion. I don't know what else to say.

Give some details about why you think that is the case.

I believe that I had access to case studies, in the sense that there have been individuals—a few individuals like me—who had gone before into these systems and for whom, in my opinion, tragically unfair consequences followed. In other words, there have been African-American individuals before me who were just as capable, just as accomplished as large numbers of their colleagues of other ethnic origins, and yet that was not sufficient for them to become tenured faculty members in these universities. An African-American chemist I know has said it best—"I am tired of seeing that

African-American academicians in technical disciplines must always be much better than the average university professor at any given institution to even be considered for such a tenured appointment. Why can't we just be as good as their average faculty member? Why can't some of us be good-average-bad like everyone else?"

For me the treatment of these individuals acted as a way to probe these universities, much the same way as any scientist or engineer probes a "black box." It is called linear response theory. One puts in some input then awaits the system's response. For me, the inputs were these people's professional lives and the outputs were the consequences for their careers. On that basis, I made a judgment. For MIT, due to my long affiliation with the institution, I had access to information on numbers of African-American scholars. I could call some names, but that might not be fair to them. This is my opinion, and I don't see why I should not tell anyone who asks.

What's your viewpoint regarding why these institutions operate this way? They have to see what we see.

I'm not sure what they see. At a very early stage in my professional life, I concluded that academia is not really very different from a country club. How do you get to become a member of a country club? Well, you get in if enough of the members want you in. Many will object that there is the important issue of merit. I agree that there is a component of merit. However, in my opinion the fact of the matter is the following: When one looks at the early portion of the careers of the cohort of individuals who go on to ultimately become the faculty at the nation's most prestigious universities, there are very, very few individuals who are identifiably heads and shoulders above the rest. The rest of the cohort looks pretty much indistinguishable, as far as I can tell. Therefore, the sorting of this vast remainder must occur based on something other than purely merit.

It is here that the human interaction comes into play. This becomes part of the evaluation process. The recognition for work that any individual does depends on three factors: a) the quality of the work, b) the number of people who study and use it as a basis to contribute to the progress of the field, and c) the community's accepted mythology of the origination of new ideas. The latter two depend critically on interpersonal interactions, how effectively you interact with your colleagues, how open they are to listen to your ideas without pre-judgment. In a curious way this depends on their "extending the benefit of the doubt," something I will try to explain. These kinds of things are in operation in the process it takes to become a tenured faculty member at these institutions. These are things where, as an African-American, I cannot change the equation. I can only control the first factor.

Let me provide you a story in extreme detail illustrating these points. This story comes from much later in my career, but it illustrates most graphically some of these points. Lots of laypersons think that scientists are somehow essentially different from the rest of our species. The popular myth is that we are calculating, totally logical beings without true emotions and certainly divorced from the superstitions and prejudices that plague humanity. For theoretical physicists, this view is doubly believed. I have seen enough scientists to know that nothing could be further from the truth. My perspective is accurately described by something Warren said upon hearing this story, "Perhaps you have only recently become aware of something that I discovered shortly after entering physics—there's nothing in particular special about physicists."

As you may know, anonymous peer review is one of the "sacred cows" of scientific evaluation. Also, not all scientific journals are held in equal esteem. The peer review process consists of one's work going to another physicist who remains anonymous, should be an expert, and renders a recommendation as to whether the work is correct, new, and interesting enough to warrant publication in a journal. Clearly, such a system must be supervised to guard against abuse. It is the role of an editor to see to this.

In my part of the field of physics, many consider the journal *Physics Letters B* to be among the best. One of the editors there is an English theoretical physicist, Dr. Peter Landshoff. Another physicist who plays a key role in my story is Edward Witten of Princeton, who was named by *Time* magazine as one of the world's 100 most influential people.

With this background, let me begin my narrative. Some years ago, I appeared on a PBS program called "Breakthrough: The Changing Face of Science in America." For one of the promotional

shots for the program, I am standing at the window of a train traveling across Siberia to a place called Tomsk and saying, "Laa . . . aast night I had an idea." In fact, while traveling and being filmed, I had a new insight into the class of mathematical physics models called "N = 4 superstring theories." My insight suggested that there were more of these things than had previously been suggested, and why that was the case. But when one of those flashes occurs, the next step is usually—for me—a very long set of calculations to prove that the insight is correct. As I said before, it is sort of like composing music. A "flash" of a tune occurred. This event worked out great for the film crew, because they had really caught me "in the act of being creative," as one of them said.

Well, these calculations and some others were finally completed a year and a half later, in the spring of 1995. By that time, I was working with Sergei Ketov—a Russian physicist, collaborator, and friend—and we wrote a paper in which the mathematical proofs that supported my idea were presented. He wanted to submit it for publication and, since he did so from Germany, it went to a senior editor at *Physics Letters*, Dr. Landshoff.

After a while, Sergei received a response which indicated that our paper had been rejected because, according to the referee, it must be seriously in error. We resubmitted it explaining that our results were a new example of something called a "variant superfield representation," along with a request to the editor for the opinion of a second referee. I first noted that these "variant superfield representation" mathematical objects existed in 1981 in a paper I wrote with Warren Siegel.

Subsequently, another letter of rejection arrived with no evidence that a second referee had been consulted. Additionally, the same first referee stated that we had "shot ourselves in the foot" by bringing up the issue of variant representations. He claimed this had no application to our work. I was extremely insulted, since I was the one who had even invented the phraseology. It would certainly stand to reason that I would know what it meant, unless I was extremely incompetent or mentally incapacitated.

We then resubmitted asking yet again for a second referee and, in addition, told the first referee where to look in our paper to be able to construct a mathematical proof that we were correct. A third round of communications began with another rejection from the first referee, who refused to construct the proof, claiming that there existed prior a mathematical theorem that forbade what we proposed. On account of this argument, the first referee simply asserted there was no need for him to follow our suggestion, which would have proven the fallacy of his argument. Apparently, still no second referee's opinion had been sought and thus the editor once again rejected the paper. Of course, the ludicrous feature of this exchange was that the theorem quoted by the referee was totally irrelevant to the point we were making.

At this point, I lost my temper and wrote a rude letter in July of 1995 in which I pointedly asked why there had been no second referee sought for this matter. I also undertook a recitation of the history of my role in the development of this topic, as well as an explanation of the whole concept of "twisted and variant representations." I pointed out how our N = 4 results were a direct generalization of my earlier N = 2 results, and explained in great detail how these results were derived. Most importantly, I gave in the letter a version of the proof that the referee refused to complete. Finally, I offered the referee some advice as to where he might look to gain a more complete understanding of supersymmetry, starting with our book *Superspace*, of which Warren Siegel and I had been among the co-authors in 1984.

For a fairly long time, there was no response. Eventually, after much prompting by us to the editor, he was able to get a second referee to respond. The second referee agreed with the proof, recommended that the paper be accepted, and suggested a few other items. Accompanying this report was a message from the editor in which it was stated that the paper was now accepted after we addressed the points raised by the second referee. Although I disagreed with some points, I undertook a revision of the paper anyway. When I had received the message accepting the paper, I threw away all of my documentation on this matter, something I would rue later.

After a considerable period, I later received an additional message from the editor that consisted of an incorrect calculation by the first referee, a concurrence by the second referee, and a handwritten sentence—apparently by the editor—on the typed written report that indicated that the

paper was suddenly "un-accepted." I told him I had never heard of a paper being "un-accepted." I asked him to reconsider his decision. In our field, it is usually the tradition that when two experts disagree, the matter is resolved in open debate in the community. I thought that the proper course of action was to accept our paper and invite the first referee to respond in the open literature. The editor refused. The way the referee's report was written is the most dishonest piece of scientific writing I have seen in my entire career. It reached an incorrect conclusion by misusing something that I pointed out in my July letter.

At this point, I decided to petition a number of the members of the board of the journal in order to have a reversal of the editor's decision. I sent information packets which contained a complete recitation of the facts, an orientation on the topic and related work, and most importantly, the proof. I also informed them of what I thought to have been the editor's unethical behavior in "un-accepting" the paper.

After this effort, I was told that they would get back to me. When they did, it was truly amazing for me. I was informed that the entire board had had discussions about this case and that the editor, Landshoff, assured them that he was handling it. After a short while, he informed me that his referees were still certain of their positions and that the paper could not be accepted on this account. More startling to me was that I was given to understand that he maintained that there had never occurred a communication to me in which it was indicated that the paper had been accepted! In simple words, I was being called a liar in front of colleagues, some of whom I had known for over a decade.

It was the most painful experience I have had in my professional life. Still seeking some moral and rational response, I next took this matter up with a senior publishing editor of the company that publishes the journal. Again, I was told that the editor claimed the paper had never been accepted and additionally that expert referees had supported him in this decision, so nothing could be done.

By now it was May of 1996. In December of the prior year, I began work on another closely related paper in which I set out to prove that the argument of the referee was nonsense. However, I chose an indirect route which by implication demonstrated the erroneous nature of the first referee's assertion. Since the crux of the referee's argument was some special features of our work, if I showed the referee was wrong here, it should have allowed him or her to deduce the error of their argument. The later paper was written with two of my students, submitted to another editor at *Physics Letters*, accepted, and published with no difficulty.

I then resubmitted the original paper, pointing out that the subsequent paper should lay to rest the fallacious argument of the referee. I resubmitted to a second editor, but was informed that since the original submission had gone to the editor, so must the resubmission. Needless to say, in due time, once again I received another rejection. The rationale was that this work did not address the referee's objection that there was a prior mathematical proof of the first referee. This "proof" was, of course, the same incorrect claim that this referee had made to get our paper "un-accepted."

At this point, I almost despaired and surrendered to this obfuscation and perfidy. This was just before I went to Russia in the summer of 1997. On the way there, I decided to take a book on the life of General George C. Patton. I was engaged in a serious historical study of World War II, and this was one of my readings. For reasons I cannot explain, this was the perfect reading material for me and acted as a palliative. I recalled that Edward Witten had in the spring of 1997 written a paper on something related to our work. I thus resolved to contact him after my return to solicit his input on the proof that I had constructed two years prior. At that point, I purposely provided him with only the starting point of the proof and asked his opinion about the implications of the proof. I supplied him with no other information of the surrounding circumstances.

Very shortly, Witten completed the proof and arrived at precisely the same conclusions as written in the Gates-Ketov paper. His proof was almost line-for-line the same as mine from two years earlier. He also pointed out that the implications of this proof were completely clear. We were right. He sent me a letter containing his completion and comments on the implications. I resubmitted the paper once again and included a copy of Witten's communications.

Instead of accepting this, the editor directly contacted Professor Witten. There were apparently some faxes sent back and forth between the two, although I was not privy to them. Needless to say,

Witten reassured him of the authenticity of the documents and also reiterated the physics implications of our work. But after the editor had contacted Witten, he still refused to accept the paper. Instead, he wrote that there must have been something intrinsic to the way the paper was written that had misled the, by then, four referees. This was a totally nonsensical argument, because none of the referees had ever complained that they were "confused" by the paper. They just claimed, under influence of the first referee, that it was impossible for it to have been correct.

At this point, I informed him that my patience had just ended and if the responsibility for additional consideration of my paper was not passed on to another, less emotionally invested, editor, then I was prepared to initiate a legal process against everyone involved that would, after much expense in both time and money, reach an inevitable result. During all the latter phases of this process, I kept the appropriate *Physics Letters* board members, as well as the senior publisher, completely informed. I wanted there to be no chance of my future intentions being misunderstood.

On November 5, 1997, I received a fax from the editor in which he finally relented, though I had suggested this on numerous previous occasions. The responsibility for the paper was passed on to another editor, Luiz Alvarez-Gaume, who promptly accepted the paper and this long, sorry episode finally began to come to an end. The paper was split into two and appeared in the April 1998 edition of *Physics Letters B*. Toward the end, I believe the board realized what was occurring and likely asserted itself to see that this issue was truly resolved on the basis of the science, not emotions.

This whole episode was completely mystifying to me. As far as I know, I have never met the editor. Thus, I have absolutely no basis for understanding his actions. As for the first referee, I have a suspicion as to his identity, but due to our anonymous system of review, they can only remain in this realm. However, one thing that this illustrates is how the progress of science itself is such a miracle, given that those of our species who pursue this endeavor are fallibly human in our behavior. Progress in science begins with the admission of our own fallibility. The body of scientific knowledge in all places and all times may be presumed to be fallible. That is the strength of the discipline.

Given this long recitation, I cannot with complete certainty say that I know that my race was an underlying and unstated issue. I also cannot say that it was not. It has been my experience, and one perceived by other African-American scientists, mathematicians, and engineers with whom I have spoken, that numbers of questions regarding our competency arise at all stages of our careers. This is not the norm in our fields. As another friend of mine has said, "We cannot expect the benefit of the doubt." When new ideas are first presented, the listener must first extend this. This concept is very critical in the working of science. Otherwise, it is impossible to have new ideas assessed seriously. If this is withheld, the advance of science itself is impeded.

However, I take this skepticism as a challenge. One of the nice things about the sciences, as opposed to many other human endeavors, is that there is one right answer, and this does not depend on the subjective observation of someone else. It is also not subject to the whims of democracy—one can stand alone with a correct answer against a host of misguided colleagues. On the other hand, we exist in a system where anonymous peer review—not solely of research papers, but of the totality of our accomplishment itself—is subject to all the collective views of our peers. As my story illustrates, to get this all to come out right is not always so easy.

Although I know you don't give advice, how do you give clues to young African-American scholars whom you see coming through the field and not understanding some of these things?

Well, I tell them a couple of things. The first of which they should be aware is that the IQ of the African-American community did not exponentially increase with that individual's birth. I have time and time again met young African-Americans who somehow think that they are the first "really smart" African-American who has ever existed. Don't be led into the trap of somehow thinking that you are so special.

Next, possessing an accurate awareness of the surrounding human environment is of critical importance. The "system" is unfair and you need to understand that from the beginning. If that is your mindset, you are much more likely to figure out a strategy for your individual success. Following the "rules" as applied to everyone else will likely not work for you. If you are not ready

to deal with the reality of your situation, whining does not help. Be prepared to do something else.

Those are very important methods.
It's how I've lived my professional life. I had these in a sense when I began in college. To me, these things were obvious.

I remember very clearly at MIT when Wes Harris, John Turner, and I went to speak with the chairman of the mathematics department, about the time you were considering the offer from Maryland. We wanted to know if there was a successful retention strategy for keeping you at MIT. As I remember, you did not ask for a completely matching counter-offer.
As I recall, this was after or around the time that the University of Maryland was recruiting me. The debate around MIT at that point was what counter-offer could they extend that would be sufficiently attractive to keep me there. I essentially said I'd like three things. First of all, the offer from College Park was a tenured associate professorship in the physics department. At MIT, I would only have liked to have been considered for promotion to associate professor without tenure. This was, and I believe is, quite common there. For item two, I really wanted to have the opportunity to lead the MIT Office of Minority Education on a more permanent basis. The position was open and I did serve in an interim capacity. As you know, one of the main activities of that office was running Project Interphase. Having been a tutor—the calculus and physics instructor during fourteen consecutive years—I believe my history working with that program is unmatched.

There is no question about that.
By the end of that time, I think I had a lot of real insights into what would have been efficacious to increase the success of minority students at MIT. I've actually forgotten what was the third component to what I would have regarded as a serious effort at retention. The response, as you know, I considered highly inadequate. I believe that individuals must hold institutions to certain standards, just as we are held to standards by them. An institution will make investments in the individuals that it judges to be in its best interest. I only know how to interpret a failure to do so in one way, and the rules of the marketplace clearly indicate a direction an individual should take.

I also did not think that the institution was able to make a fair judgment of my sustained commitment to goals which it proclaimed, and I felt that there was likely not to be a fair hearing of my science. That was likely the third issue. I had requested an independent exterior reading of the quality of the science I had done, because I felt there was very likely to be a prejudiced view, especially from one of my colleagues, Professor Daniel Freedman.

In the math department?
Yes. Dan was one of the people who began the earliest but conceptually incomplete formulation of the theory of supergravity, using a technique called "the component method." He was also known to be vehemently opposed to the type of approach that Warren and I had developed, the so-called "superfield pre-potential method," to its highest level in supergravity theory. There were stories of how he had forbidden graduate students at Stony Brook, where he was prior to coming to MIT, from even studying the superfield method—precisely the type of thing I was an expert in developing by that time. In fact, while we were colleagues, he had found some results that he initially claimed were impossible to find using our more comprehensive superfield approach. Needless to say, I accepted the challenge and proved that these results were perfectly easily understood within the context of our approach. In the process, I had to introduce a new generalization—"active central charges"—to the mathematics, but that was simple from my perspective.

This was some of the final work I did at MIT, and it led to one of my most widely recognized contributions—"twisted representations"—to theories of supersymmetrical physics. I thus felt that he had a definitive disposition. The process by which tenure is granted is one of strict confidentiality and, in such an atmosphere, a negative comment can easily poison the well. I therefore believe that I had a justified reason for concern about my long-term prospects. I'm not saying this was an example of racial discrimination, it was sort of an intellectual disagreement about what I do. However, given that there was in my mind no reason to think my approach would be given any benefit of the doubt, I was not interested in staying. There simply seemed to be no mechanism by which a truly fair hearing was going to occur, given the personnel I expected to be involved.

As I said, I think that I laid out a case of what I had done and what kind of science I was doing.

I was completely prepared to leave, which I did. From the present-day perspective, when the type of approach that we took in those early days is now the rule in our field, it is completely clear that the routes that we were pioneering are exactly those that permit the widest and most fundamental advances in this type of theoretical physics. I must say that my decision to leave was such a right one for my career that I can now look back and laugh at how I could have wondered whether I should leave.

That decision, I should say, had a curious input from a Nobel laureate, Abdus Salam. The summer prior to my departure from MIT, I had been a visitor at the International Centre of Theoretical Physics in Trieste, Italy. The founder and director of ICTP was Dr. Salam. He had had to leave his homeland, Pakistan, many years earlier in order to pursue his dream of doing state-of-the art research in theoretical physics. This had been my third or fourth visit to ICTP. He had always been very supportive of me from the time of our first meeting. I guess he recognized that I was somewhat of an oddity, to say the least, and that in a small way I too was on a journey into the unfamiliar.

Once during my visit, we had lunch at his special table in the ICTP cafeteria. In a conversation which I will not forget, he said, "So you are now at MIT?" I said yes. He also expressed an interest in my considering an offer from ICTP. This caused me quite some effort to sort out. Like many, many African-Americans before me, I had found the consequences of being black in Europe so different, so liberating, that the idea of emigrating from the U.S. could not be casually dismissed.

Dr. Salam continued, "Are you going to stay there?" "I don't know." "Well, are you married to MIT?" That comment alone started me on an evaluation process as to exactly what it was that I thought was so valuable about my remaining at MIT. My final judgment was that I was going to be as good or as bad a scientist whether I stayed there or not. Fortunately for me, leaving was an excellent decision for my career. I went into an extremely productive phase where, for example, I was able to make distinctive contributions to the developments of superstring and heterotic string theory. Most of the things that I am truly proud of as a research scientist I've done right here at College Park. Furthermore, I cannot see how I would have been able to make such contributions had I stayed at MIT. In fact, I am convinced that my career as a research scientist would have ended had I remained.

Let me say a few more words about this matter. My career would likely have ended for reasons that are counter-intuitive, being that MIT is known as an institution dedicated to first-class research. As a young professor in the mathematics department, the teaching responsibilities I had were substantially greater than those of most young physics professors. If this had continued until the time I would have been considered for tenure, then clearly the persons against whom I would have been judged would most likely be theoretical physicists; unfortunately, from my view, these people would have had a much larger amount of time in which to carry out research. When I brought this concern up with administrative faculty in the department, it appeared to me that this observation was not taken seriously. It was as if no one previously had ever thought about this.

A story will perhaps illustrate this point more clearly. Shortly after I first received the appointment in the mathematics department at MIT, I was in Harvard Square at the Harvard Coop bookstore. I came across Luiz Alvarez-Gaume, who had that same semester received an appointment as a new assistant professor at Harvard in their physics department. When we noticed each other, congratulatory remarks were exchanged at first. Then we began to discuss what was going to happen next. Well, Luiz was taking a leave of absence to go to the Institute for Advanced Studies at Princeton to work with Ed Witten on some research problems. In fact, they derived some beautiful results that were to play an important role in our field. However, when Luiz told me of his impending departure, I thought, "How interesting. I have been assigned to teach three courses my first semester. In the future, when I am to be evaluated for tenure, he would be one of the persons against whom I would be compared. Now how does this equation work? Luiz is to go off to Princeton with nothing to do but research with Ed, and I have three courses to teach. Who is most likely to have gotten the best research done? At tenure time who is most likely to look like the better scientist?"

I concluded that this was crazy, because although MIT was the environment that had produced me as a scientist, it would now be the environment that would stifle my efforts to do science.

Since it was the doing of physics that I loved, it was clearly incumbent upon me to find a more hospitable environment. So a few weeks into my appointment as a new assistant professor, I began to think about how to change my environment. It was another eighteen months before I succeeded.

All I can tell you is that, after watching your case, I became for the first time definitely convinced—and still am—that MIT is not interested in having a decent number of African-American scholars on its faculty. That was clear to me.

You did not ask me about traveling. Should I say something about that?

Yes.

I have traveled a fair piece in the world, too much in a sense: I'm cutting back these days. With the birth of my twins, I've become much more a homebody, even though next month I'm going to be spending a week or so in Russia.

Yes, I enjoy traveling tremendously. That's something I think I inherited from my father, with his having been in the Army and us moving while I was a kid. I like to observe people in their native environments, I like to see different cultures. You can only truly gain a greater insight into this nation after you go outside of it and look back at it from another society. There are things that you will think about as an American that you would not have thought unless you go somewhere else.

You mean, you can appreciate America better?

I don't mean it necessarily that way, but there will be distinctions and differences that you will have definitely illustrated in other societies that you would never think about as an American. You will also learn that you are peculiarly an American. You come to understand that in much of the world you will be viewed as an American, no matter where you go. You come to understand that you are detectably an American to other people, no matter where you go. It gives you a different understanding of what it means to be an American.

Talk a little bit about your experience here at the University of Maryland, and at Howard University.

After I left MIT, some people from there called and asked, "How are you doing?" My statement then was that it was like dying and going to heaven. Nothing has happened in the intervening years to make me change that statement. I have an absolutely lovely time here. When I first arrived here, I was shocked because it was the first place in my professional career where resources were readily made available to me to carry out my research without a struggle. I didn't know such a place could exist, so I was absolutely amazed at that.

This department and university have always been very supportive of my efforts. Some years ago, I received an extremely generous offer from another university which, when I first saw it, seemed guaranteed to insure my departure from College Park. That did not happen, and it didn't because this institution matched the outside offer. This is a stark contrast to the experience from earlier in my career. The university here offered a retention effort that included support for my research program, as well as the opportunity to do something about which I had had dreams from my time in CCTEP.

During those days, one of the topics of continuing debate was how our physics degrees would be relevant to the larger African-American community. A similar such question was whether it was better to take an advanced degree from MIT and go to work in a minority-serving institution or in one that serves the nation more generally. I was able to take a leave of absence, finally, to work in the physics program at a historically black university—Howard University.

Howard, that's a whole other world. I think that succinctly I can say that. I'm extremely proud of the fact that I was the chairman of the Howard physics department. During my tenure of service and the following year, this service led to the initiation of new sponsored research programs in excess of fourteen million dollars. This was done in the form of two large new grants supported by the National Aeronautics and Space Administration and the Department of Energy. The former was to support the creation of an interdisciplinary and interdepartmental research center focused on atmospheric science. The latter grant supported the creation of a synchrotron radiation experimental program at Howard and affiliation with a Department of Energy laboratory, the Advanced Photon Source at the Argonne National Laboratory.

The critical feature that allowed the accomplishment of this second program was my successful effort to attract Dr. Walter Lowe from AT&T Bell Laboratories. This program was formally initiated after my return to College Park. In fact,

when I left Howard, it was not clear that this effort would be successful. I found out that all the effort had paid off when I was on an airplane on my way to Europe. I had picked up a copy of *Black Enterprise* magazine and found a brief announcement that the largest purely research grant ever given to a historically black college or university was being made to Howard University. Given the—shall we say?—unusual way of showing hospitality that we initially received from two Department of Energy representatives at Argonne, Dave Monkton and Ed Temple, the success of this proposal was deeply gratifying for me.

It was nationally publicized.
Yes, in a number of places. I must admit, I took very great pleasure in that so few people knew the complete story of my involvement in that.

I have noticed that you have a way of doing those things.
Yes, but I like to do things without leaving fingerprints, sort of like a cat-burglar. It has been a very great pleasure to be in the background of little history-making events.

It's amazing the sort of things that you have done and that the general public has no sense of because you have planned it that way.
That's right. Up until this interview, and a few other things, I have always striven to keep it that way. This, in fact, goes back to the time when I was an undergraduate at MIT. One of my fictions about being successful as an undergraduate was that I could hide in plain sight, which was an expression I had for what I sometimes claimed to be doing as a freshman. If you were in a big lecture class with a couple hundred other students, and if you never called attention to yourself—in test performance and homework grades—then how could the instructor discriminate against you? How could he even know you?

So it had always been my philosophy that if I could hide in plain sight, I'd do it. I had carried that pretty far in my life until the recent thing with the PBS series, "Breakthrough: The Changing Face of Science in America" and "A Science Odyssey: Mysteries of the Universe," promos, et cetera. It probably will be impossible to be so stealthy in the future.

Well, it's good that occasionally somebody takes the ball and runs with it to the extent that you controlled that particular piece, so that particularly the generations that come after you actually know that you were there.
I don't know about controlling anything. Essentially, it's like a pleasure that they don't know that you were there. The BSUTP, which we talked about earlier, is a great example. It is there still, I believe, functioning at MIT and serving minority students, and almost no one knows how it started. I really love that, the idea of doing something that has an impact on a later generation and they don't know how it got there. It's sort of a great joke for me.

I've reached sort of a mid-point in my career where I have achieved a certain level of recognition for the research that I've been able to carry out. My initial cynicism from my days as a senior in high school about the manifold unfairness of many things in this society has been largely vindicated. For example, I have had guns drawn on me twice in my life, both times by policemen with no good reason for doing so. Thus, as an African-American man, when I worry about crime, I have two sources of concern—those crimes committed by criminals and those committed by individuals under badges of official sanction.

The game of academia is often dishonest and unfair, not unlike lots of human endeavors. Things do not just occur according to rules of intelligence and rigorous mathematical logic. It is a fiction that we presently live and work in a color-blind meritocracy. Discrimination exists, but it is also not a perfect evil, and because of this fact, it has been possible for me to thrive as an African-American scholar in theoretical physics. Issues of race and discrimination can often be ambiguous. For example, once an expatriate European-American physicist commented to me after hearing my talk, "I have never heard a black man speak the white man's magic so well?" How am I to interpret such a remark?

As my life's story shows, there are people of all ethnic groups, without whom I could not have survived. The fact that those people who have not been supportive are exclusively European or European-American is a natural consequence of the demographics of my field and should not be given simplistic racial interpretations. I have tried diligently to develop and hold opinions that are informed by a factual basis of observation. For example, my writings about the need for affirmative action have been such an attempt. But I did

not start off liking physics because I thought I would become rich and famous. I started off liking physics because I like to do physics. I take satisfaction in the theoretical physics that I've been able to create and, if I'm very lucky, one day we will find that some of this is the physics of our universe. If so, I will be able to leave a legacy in physics that can become the basis for another generation's advances. To me, that is special.

Technology and the Dream

Reflections on the Black Experience at MIT, 1941–1999

By: Clarence G. Williams

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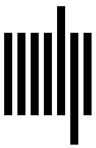

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