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Pushing Boundaries: My Personal and Scientific Journey

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Abstract

This autobiographical narrative offers a brief account of my journey and adventures in condensed matter physics (a.k.a. solid state physics) and some of the personal events that shaped my life and my career: my early years in Europe, my family's escape from the Nazis, growing up in Cuba, the difficult road into a field that was essentially closed to women, a personal disaster that knocked the wind out of my sails for more than a decade, and my return to a successful career in physics. In closing, I argue that, although we have made remarkable progress, we know but a thimble-full in our inexhaustible search for an understanding of the laws of nature.

To begin at the beginning, I was born in Antwerp in the Flemish-speaking part of Belgium in 1933, the year of Hitler's rise to power, an event that was destined to shape the trajectory of my journey through life. I was in first grade, three months short of my seventh birthday, when the Nazis invaded Belgium on May 10, 1940. My very large family—parents, brothers, numerous uncles, aunts, cousins—all fled toward Calais by any means available, mostly on foot. Calais was under siege by the Germans when we arrived (in disconnected family groupings that had lost touch with each other). The chaos and events that ensued in the following days and weeks are described by my younger brother Henry Morgenstein (1). During the battle for Calais, a number of the women and children in the family (my older brother Paul included) crossed the English Channel in a British boat and were taken to Surbiton, a suburb southwest of London, where the blitzkrieg was in full swing. After Calais fell to the Germans, the rest of us found each other. We spent several weeks living on a farm in Andres near Calais that belonged to a Monsieur Quehan. The Nazis eventually issued a command that everyone was to return home.

We then lived in Antwerp under German occupation for nearly a year, as the situation steadily worsened—yellow stars, curfews, searches by the Gestapo. Antwerp was under relentless British bombardment every night. We (my parents, younger brother Henry, and I—Paul was now in England) moved to the middle of the city away from our home very near the airport in Borgerhout, which the British targeted heavily. In the spring of 1941, the family once again attempted to flee (my Uncle Chiel: "עס ברענט מיר אונטער די פֿיס—the earth burns under my feet"). A sizable contingent succeeded in traveling through German-occupied France, entered northern Spain at Hendaye using false exit permits, and sailed to Havana, Cuba, several weeks later on a Spanish boat (the *Marques de Comillas*). Leaving Antwerp only one day later (the laundry needed more time to dry), my immediate family (Pa, Ma, Henry, and I) was not so lucky. By the time we arrived at the border between France and Spain, the border guards had been alerted to the fact that our exit permits were false. My father engaged the services of a smuggler to get us across the border to Spain; unfortunately, we were apprehended by a gendarme on a motorcycle, interned in Merignac, a concentration camp surrounded by barbed wire near Bordeaux, then transferred to Camp de la Lande, a *résidence forcée* near Tours. We escaped from the camp on a Sunday (my father's gift to me for my eighth birthday, he said) and smuggled across the *Ligne de Démarcation* between German-occupied France and Vichy France in the dead of night. I vividly recall arriving at a farm near the border around midnight. After no more than two hours' sleep (I was *so* tired), we[1] were woken up and succeeded in smuggling across the border—walking, and walking, and then running as fast as we could across an open field ("Ma, I need to go—*fait dans tes culottes*"). After a six-week sojourn in Nice (our cousins living in nearby Grasse avoided us—they were pretending not to be Jewish), we spent some weeks in Bilbao (where we had very little to eat), and ultimately sailed on the Spanish boat *Magallanes* (I remember the delicious little white rolls) to join the family in Havana toward the end of 1941, shortly before the Japanese attack on Pearl Harbor.

My recollection of these events is surprisingly sharp (but in disconnected segments). In particular, my memories of the last, successful attempt to escape were shared only by Henry, who was too young to know or understand what was happening, my mother, who could somehow not remember much of this at all, and my father, who died prematurely in a pedestrian accident in 1968 in New York; I had been too busy living life to ask him about the details and missing stretches.

Searching for the camps many years later on the fiftieth anniversary of the Normandy invasion, I learned that a barbed wire fence was erected shortly after our escape from La Lande. The

[1]The group included the four members of the Schwergold family, with whom we had escaped from camp, a British soldier for whom my parents were asked to pay, and a few others.

residents of the camp were transferred to Drancy (men first, then women and children, and lastly some remaining children whose parents had been sent earlier). All were transported east to the extermination camps in Poland by mid-1942. Most (but not all) of our extended family survived and were scattered all over the globe: Argentina, Belgium, Brazil, Britain, Canada, Cuba, France, Israel, and Switzerland.

Smuggling, carrying false papers, and bribing, all have negative connotations. We do what we must to survive. Three evil dictators played central roles in saving our lives: Rafael Trujillo issued visas to Santo Domingo (without which many Jews could not even *try* to escape), Francisco Franco allowed us to travel through Spain, and Fulgencio Batista allowed us to stay in Cuba. Alone and disconsolate, my very proper maternal grandmother, Bomama, was rescued at a tram stop by a hooker who (with the help of her colleagues of ill repute) hid her in the attic until the end of the war. Who is good? What is evil?

We spent the next five and a half years in Cuba. I was now a "displaced person," a refugee referred to by the local street population as "Polaca" (Pole, but they really meant Jew). I returned to school, learned Spanish, and then English when I switched to an American school because I was too young to be admitted to Bachillerato (high school). I learned to conjugate verbs in their myriad tenses, memorized all the Spanish adverbs in alphabetical order (a, ante, bajo, con . . . .), and memorized the squares of numbers up to 30 and the cubes of numbers up to 12; I read voraciously. I played the piano, gave a piano recital, loved the Orquesta Filarmónica de la Habana (led by the great conductor Erich Kleiber, who had fled Germany even though he was not Jewish), and trained with the Cuban National swim team, which was preparing to compete against Mexico when I left. I vividly remember the day my parents found out that those we had left behind were being gassed and burned, my beloved paternal grandmother (Bubeshi) included. I grew up on a beautiful island, in a sunny, idyllic environment—lush, beautiful, serene, a haven away from the chaos and the carnage.

Our "quota" number eventually rose to the top of the pile at the American Embassy, and we were granted a visa to enter the United States. We arrived in New York in March of 1947. We had finally been admitted into the country of our dreams, the land of promise, liberty, and unlimited possibilities! It was exhilarating, reminiscent of the day I suddenly realized that my line of sight had risen above the dining room table top—an incredible new world had opened to me. Few people value and treasure American openness and freedom more than do immigrants.

Following several months completing eighth grade (8B) in a junior high school in Bensonhurst, Brooklyn, I completed the ninth grade (9B) during the fall 1947 in a junior high school named Stitt in Washington Heights, a rough school where I was one of only two white students in my home class. I most decidedly did not fit in. I spoke with a strange accent and had a far stronger scholastic background than any of my classmates. More often than not, there was a knife fight between two students after school that invariably drew a crowd of eager spectators waiting for the day's entertainment. To this day, I am grateful to the classmates who took it upon themselves to walk me home, where my worried mother would be looking out the window to be sure I returned safely. I was absolutely appalled—in total disbelief—that students could be unsafe in school!

I attended the Bronx High School of Science for five semesters, entering at a time when girls were first being admitted to the school. Bronx Science was a great school; the students were very bright, the standards were high, and the curriculum and requirements were broad and demanding. My class of 1950 included Steve Weinberg and Shelly Glashow (of Standard Model fame), Danny Greenberger [of Greenberger–Horne–Zeilinger fame, now my colleague at the City College of New York (CCNY)], and many other luminaries in various fields.

Barnard College, which I attended next, had only one introductory course in physics, so I took my physics courses across the street at Columbia University with the guys. I was interested in many things: Spanish literature, French literature, math, chemistry, philosophy (until I took a course in it), maybe physics . . . . Music was my passion! I dreamed of being a pianist of the caliber of Vladimir Horowitz or Arthur Rubinstein—preferably the latter. I continued my musical studies seriously for many years—until it became impossible to do all the things I wanted to do because there were only 24 hours in a day.

So why did I choose physics? Physics was interesting, and it was the biggest challenge I had encountered. Unlike other things that I could do well with far less effort, physics was hard—it was a challenge. My father admired physics above all other disciplines, and I admired my father. He was an exceptionally intelligent, self-educated man who would have chosen to study physics if he had had the opportunity to acquire a formal education. Although I did quite badly during the first half of the first semester of introductory physics, I improved quickly.

The Columbia University Physics Department was a hotbed of activity in the 1950s. Many Nobel Prizes were won by the faculty while I was there, and a good few more were awarded after I left based on the work they did while I was there: the discovery of coherent radiation (the maser), parity violation, the discovery of the muon neutrino, the structure of atomic nuclei, and more. The Friday 5 PM colloquia were held in a packed lecture hall—it was the event of the week (in contrast with the typically low attendance that is common at present day talks).

During my time as an undergraduate, I encountered my first experience with the unequal treatment of women in the workplace. I took temporary summer positions after my junior and senior years at a Bell Telephone Laboratories site in Manhattan on Bethune and West Streets. While my male classmates, many of whom I knew (and who were not brighter than I was), were hired through the technical employment office, I was hired to do a similar job through the secretarial–janitorial office at two-thirds the salary. It was pretty annoying, but there was nothing to be done about it—that's how things were.

I owe an enormous debt to Professor Polykarp Kusch, who intervened on my behalf at various junctures in my career. Kusch held views about women in physics that were retrograde by current-day standards, but he was a fair and decent man. When I approached him in my senior undergraduate year to ask for a recommendation letter in connection with my search for a job or continuing my studies, he insisted on giving me a twenty-minute lecture: ". . .a physicist can marry a taxi driver's daughter, but a female physicist cannot marry a taxi driver." Despite this, Kusch arranged for me to work at the IBM Thomas J. Watson Laboratories. He later arranged for me to be admitted to graduate school at Columbia, and he was entirely responsible for a subsequent offer I received from Bell Labs (see below).

Near Columbia University on 115th Street between Riverside Drive and Broadway, Watson Labs was a truly unique institution. My boss Dick Garwin was there, as were Llewellyn Thomas (of Thomas precession), Irwin Hahn (of Hahn echoes), and other well-known luminaries. A handful of Columbia graduate students worked under their supervision. The proximity to Columbia made it possible for me to take a course or two each semester. I very much wanted to continue my education but hesitated, feeling it was not appropriate for me (a girl) to continue for an advanced degree. I was raised in an Orthodox Jewish family, part of a community where a woman was expected to marry, raise children, manage the household, and see to her husband's needs. A woman was not expected to work outside the home. In fact, it reflected badly on her husband's ability to provide for her if she "had to work." Although these expectations did not suit me in any way, such deeply embedded cultural expectations and biases are remarkably difficult to root out.

I am infinitely fortunate that my husband Phil, whom I married the summer following my graduation from Barnard, gave me the courage I needed to continue toward a PhD degree. I met Phil in my first-year Physics class. He was working toward double Bachelor degrees (a BA from Columbia College and a BS from Columbia Engineering). Phil is very bright and quick (he helped me through first-year physics). Although he had not planned to get an advanced degree, he was strongly encouraged by the engineering faculty to stay on for graduate work. At this point, I gave myself permission to proceed as well.

I was one of a handful of women—five, six, ten at the very most—among the 200 graduate students who were studying physics at Columbia. Most often, I was the only woman in a class full of men. I was expected to meet the same standards, but the male faculty did not take me very seriously and did not expect me to contribute much (if anything) to the field—perhaps I would contribute as a teacher. This was spelled out for me during a meeting with my graduate advisor (and reinforced by subsequent events when I was looking for a position after completing my PhD). In truth, although I could ace the problems and do very well on the *written* exams, I really did not understand physics very well at the time—I bombed on oral exams. I actually learned to think like a physicist by teaching and by being an active researcher. It has been a continuing learning experience throughout my life.

My PhD dissertation, done jointly with a fellow graduate student, Erich Erlbach, under the supervision of Dick Garwin, concerned measurements of the attenuation of a magnetic field by thin (now referred to as Type I) superconducting films of lead (Pb) and tin (Sn) (2, 3). While we were doing the experiment, Bardeen, Cooper, and Schrieffer (BCS) published their famous theory of superconductivity (4). Central to their theory was the presence of an energy gap ($E = 3.5T_c$) in the density of electron states. Our measurements yielded one of the earliest experimental demonstrations of the existence of the BCS superconducting gap. We showed that the superconducting penetration depth λ could be deduced from the temperature dependence of the magnetic field attenuation; our data were consistent with an energy gap between $4.9T_c$ and $5.4T_c$ for Pb. At about the same time, a direct determination obtained from far-IR absorption measurements by Richards & Tinkham (5) yielded a value of $4.1T_c$.

After receiving my PhD, I remained at Watson Labs as Dick Garwin's postdoc for a year. I was pregnant with my first daughter, Karen. The powers-that-be insisted that I stay home starting at the beginning of the fourth month. This upset me greatly. I dug in my heels and managed to hang in for nearly the full nine months. (My obstetrician: "WHAT? You're still working!!?")

When Karen was born, I bowed once again to the expectations of my upbringing. I had not made any plans, thinking that motherhood might remind me of my proper role in life and that it might satisfy all my needs. I stayed home for many weeks to take care of Karen, becoming increasingly unhappy and very depressed—the world was rushing past me and I was stuck, stuck, stuck! The quandary: Without an income, how can I hire somebody to take care of Karen while I'm looking for a job? How can I look for a job without hiring somebody to take care of Karen? Unexpectedly (or perhaps not?), this was not much fun for Phil either. After a brief period, he insisted that I lift my self-imposed constraints: "Get out there and look for a job, we will manage it." And so began my first attempt to get a position in physics, a field quite completely dominated by men.

My experiences when I entered the job market were painful and demeaning. Physics was growing exponentially; physics departments were being established everywhere; industrial organizations were thriving, expanding, and hiring. The tech market was hot. At a job fair in January of 1962 that was held at an American Physical Society (APS) meeting in New York (yes, we could afford to meet in New York in those days), my fellow applicants (many of whom I knew) received a dozen or more requests for interviews. I did not receive a single inquiry—not one. Truly appalled, my good friend and colleague, Ed Stupp, succeeded in arranging two invitations: the interviewer for a small college in New Jersey adamantly insisted that I really wanted a part-time job teaching; the interviewer from American Standard (which makes toilets and bathtubs) suggested I establish an all-female laboratory modeled after the all-female orchestra that had been established to provide jobs for female musicians. To the best of my knowledge, all the graduate students who did their theses at IBM Watson Labs received offers to work at IBM in Yorktown Heights; I received an offer to work part time, period. There were numerous other indignities that could fill a book (which I will not write). The fact that I had a baby less than a year old did not help. I was asked multiple times by many, Why don't you take care of your baby, who is going to take care of your baby? A man would *never* be asked such a question. Indeed, we're no longer allowed to ask this during interviews—we have made some progress in the intervening years.

It was Kusch of Columbia who rescued me again. I asked to see him. After a lengthy discussion (which included "why don't you take a teaching job and take care of your baby?"), he agreed to help me ("we trained you so you deserve a chance to try"). I received a telephone call the very next day from Sidney Millman, the head of Physical Research at Bell Labs in Murray Hill, New Jersey, inviting me to come for an interview. I received an offer of a two-year position as a Member of the Technical Staff (MTS) working with Ted Geballe [whose prefatory article appeared in this journal several years ago (6)]. I jumped at the opportunity.

I believe that my time at Bell Labs was foundational for my subsequent career. Bell was a truly spectacular place. The science was aggressive, brilliant, and vibrant. To find out what was new, hot, and interesting, one needed only to join colleagues for lunch in the cafeteria, where all the latest and hottest physics was discussed. Typically, someone might suggest that you measure the resistance, specific heat, magnetic response—whatever you were equipped to do—to clarify some fascinating new issue or characterize some incredible new material.

When I arrived at Bell, it took me a while to figure out what I wanted to do with my time. For starters, I joined George Smith (of later Nobel Prize fame) in an experiment to measure the thermoelectric power of some V_3X superconducting compounds (7). Smith was a really impressive physicist—I learned a great deal from him. The experiment we did was simple, interesting, and fun.

I spent the remainder of my time at Bell doing an experiment that was instrumental in solving a thirty-year-old puzzle. A resistance minimum had been discovered by de Haar, de Boer, and van den Berg (8). It occurred in some materials but not others for unknown reasons. In 1953, A.H. Wilson wrote:

> . . .[T]he resistance of some very pure specimens of gold passed through a minimum as the temperature decreased. The effect, though small, is unmistakable . . . . The cause of the minimum is so far entirely obscure. . . so that some new physical principle seems to be involved. (9, pg. 1)

There was much activity and ferment regarding the physics of localized magnetic moments when I arrived at Bell in 1962. Measurements had just been completed and published on alloys of the 4d transition series niobium-molybdenum-rhenium containing 1% iron (Fe) in which they traced the appearance and disappearance of a localized moment associated with the Fe (10). In an experiment that was technically quite straightforward, I measured the resistance of this same alloy series as a function of temperature and established the fact that there was a minimum in the resistance if, and only if, the sample exhibited a local moment (11) and its size was directly proportional to the percentage of Fe in the alloy (see **Figure 1**) (12, 13).

There existed two theoretical models (14–16) that attempted to explain the effect, but my measurements were inconsistent with both. Contemporaneously with my experiment, Jun Kondo was using third-order perturbation theory to show that the exchange coupling between the local moments and the electrons' spins leads to singular scattering of the electrons near the Fermi level via a $\ln T$ contribution, thereby providing an explanation for the resistance minimum (17). In a brief history of this achievement, Kondo wrote in 2005:

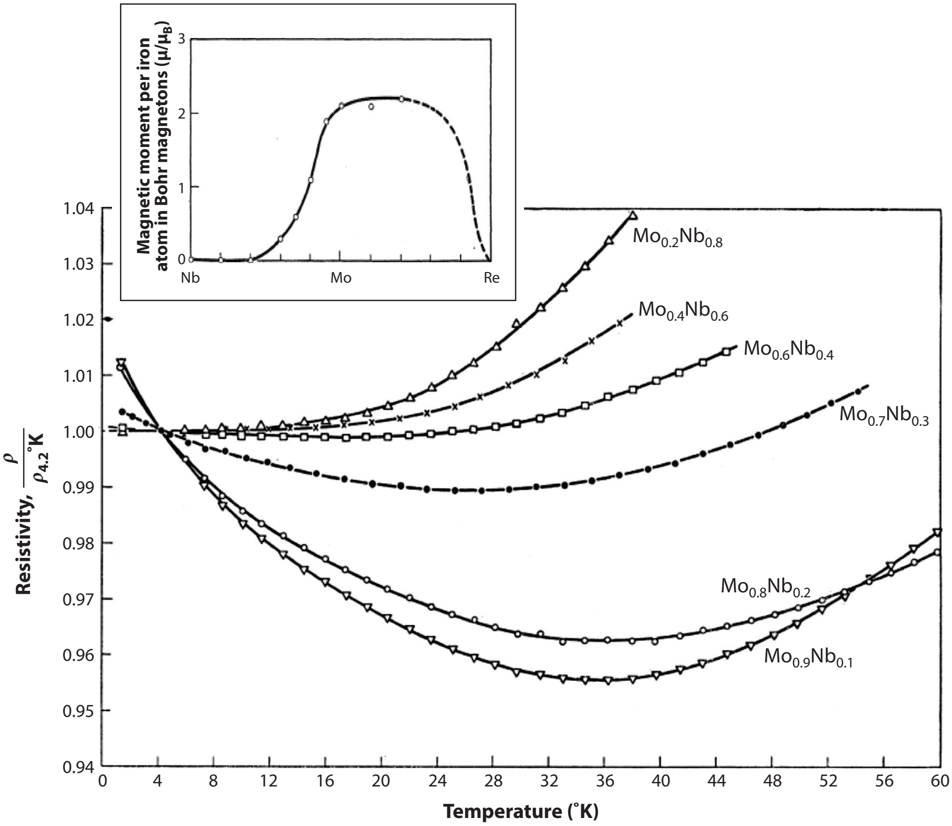

Figure 1

The resistivity as a function of temperature for $Mo_xNb_{(1-x)}$ alloys containing 1% Fe for different x, as labeled; the inset shows local moment formation in Mo-Nb-Re alloys containing 1% Fe. Note that there is a minimum in the resistance if, and only if, the sample exhibits a local moment. Adapted from Reference 11.

> The most convincing evidence that the resistance minimum takes place only when the impurity atom is magnetic was due to Sarachik. Alloys between 4d transition metals involving one atomic percent of iron were found to be magnetic with iron atoms having moment of about one Bohr magneton or nonmagnetic depending on the number of valence electrons of the alloy. Sarachik measured the resistivity of these alloys and found that the resistance minimum was observed when and only when the alloy is magnetic. (18, p.1)

Kondo and I discovered each other during this work. He noticed an abstract (19) I presented at the 1963 Conference on Magnetism and Magnetic Materials and sent me a preprint of his now-famous paper (17). Bull's-eye! I recognized instantly that he had the correct answer. In a nutshell, Kondo found that the conduction electrons act collectively to screen a localized magnetic moment by forming a many-body electron cloud of opposite spin in its vicinity. The Kondo effect was an early manifestation of the collective behavior of electrons—I believe this work is the most significant finding of my long career.

There was a problem with Kondo's perturbation theory solution: The ln$T$ terms diverge in the limit of zero temperature. This divergence became the focus of intense activity during the subsequent decade. The challenge was to find nonperturbational techniques to calculate the behavior for temperatures below T_K, the Kondo temperature. The theoretical efforts to resolve the "Kondo Problem" ultimately led to the renormalization group for which Ken Wilson was awarded the Nobel Prize in 1982 (20). The Kondo effect remains a central feature of our understanding of the behavior of electrons in solids. Moreover, it has now appeared on a lattice (in heavy fermion systems), in connection with quantum dots, and its applicability has been extended beyond the issue of magnetic moments.

Note that I did this work alone—my coauthors (Ernie Corenzwit and Lou Longinotti) made the samples. I understood instinctively that a male collaborator would surely have reaped any credit rather than I, a phenomenon that is still a problem documented by a number of recent studies. I received some limited recognition—for a while I was known as the "resistivity girl." My 1964 paper garnered relatively few citations until the mid-1970s and then essentially disappeared from view. Numerous extensive reviews on the resistivity minimum and the Kondo effect were written that did not mention the experiment that was central to the issue until A.C. Hewson (21) and Kondo (18) himself more recently pointed to it as pivotal for the development of the field. Electrons behave collectively to screen the inconvenient presence of local magnetic moments; electrons must follow the laws of nature. People (women as well as men) engage in a collective dance to screen younger physicists' contributions, particularly the contributions of young women; but people have freedom of choice, and they have now begun to exercise it by giving credit to women's work when credit is due. I was annoyed, but I was too busy to worry about it—I just moved forward doing new things.

Although my position at Bell was that of an MTS, I effectively had a two-year appointment as a postdoc. There were quite a number of such "postdocs" at Bell, and only a counted few were offered a permanent position after the two years. I would have loved to stay. I subsequently learned (indirectly) that I had earned a pretty low rating (the bottom third) during my stay at Bell.

So, I was once more looking for a position. Thus began another challenging period in my life. I felt I was ready to take a faculty position, but I was considered only for a (second) postdoc position wherever I applied; that was most unusual in those heady times, when science (particularly physics) was threatening to engulf the entire federal budget. Arranged again by Ed Stupp, I received an offer from Philips Laboratory, just north of New York City, at a salary several thousand dollars below the going rate. I objected, placed an inquiry, and was told that the offer was in line with industry-wide practice regarding women; I had no choice in the matter. But I *did* have a choice: I turned it down even though I had no other options at that point.

Having earned his PhD earlier than I did, Phil had worked a couple of years at IBM, and then as a faculty member in the Columbia University Engineering School; we were both looking for a position now. Well ahead of his time, Phil understood and made room for what's now referred to as the "two-body" problem: "Your husband has offers to join the faculty of several top-ranked universities; you have a beautiful baby girl—what's the problem?" Phil did not accept any offer (Michigan, Maryland, others), waiting until we both found positions in the same geographical area—he took a faculty position at New York University as an Associate Professor, and I received an offer from CCNY as an Assistant Professor. CCNY was the *only* university that offered me a faculty position.

I joined the Physics Department in the fall of 1964, just as Harry Lustig was embarking on a highly successful plan to upgrade the department with funding from a National Science Foundation "Center of Excellence" grant. This was a truly exciting time. In the next few years, the department grew in size and recruited three special overscale Distinguished Professors: Bunji Sakita, Herman Cummins, and Mel Lax; others included Henry Semat, Mark Zemansky, Danny Greenberger, Joe Birman, Bob Alfano, and many more. It became a truly outstanding department, but it did not achieve the high visibility it would have garnered if it were part of an Ivy League school rather than a public institution known for training recent immigrants and students of limited financial means.

As a woman, I was pegged from the start as a teacher of first year physics lectures. I've taught other courses as well, mostly at the undergraduate level. Everyone assumes that women are particularly good teachers—this is a generalization that is often incorrect. I was just OK. One of my constant pursuits through the years has been to improve my teaching; I've tried, and continue to try, different approaches and different methods to engage students more actively in the learning process, with mixed success.

I submitted a research proposal to a couple of federal agencies as soon as I arrived at CCNY. To my surprise and utter joy, my proposal was funded by the Air Force office of Scientific Research (AFOSR)! A representative from AFOSR, a CCNY Dean, and I met to determine the overhead rate; we agreed on 20%. The relatively modest amount of funding provided by the grant was sufficient to cover several graduate students, liquid helium and supplies, travel, some equipment, repairs, and on and on. Single-investigator grants now charge far higher overhead and provide barely enough money for one student and the bare-bones costs of all other necessary items. Running a viable research program now requires grants from several funding agencies and/or a combination of several multi-investigator grants. The consequences are clear: more proposals, more reports, and reduced productivity. Also, more hassle and less fun.

I was pregnant with my second daughter, Leah, during my first year at CCNY. Problems again: I was due at the beginning of August, and the chair insisted I take a leave during the spring 1965 semester. Once again, I absolutely refused. I went to war and, with the help of some of my (male) colleagues, I succeeded in "muting" the chair's decision. I was allowed to do my teaching and research, but I was not to show my face (or any other part of me) in the Faculty Dining Room for the remainder of my pregnancy. How things have changed!

To help me set up my lab, Bell Labs generously donated the equipment I had used there. With drawings and advice provided by Ernie Corenzwit, I built an arc-melter to make my own samples, assembled a group, and embarked on a research program to measure the transport and magnetic properties of various materials. We continued investigations of local moment systems, studied the formation of giant magnetic moments (22), measured thermopower, and so on. I was promoted with tenure three years after my arrival and to full professor three years later. The research was productive, the teaching was satisfying, the family was doing great; life was good.

This now brings me to the most painful period of my life. We had engaged the services of a housekeeper, Annie Meier Froelich, whose main responsibility was to take care of Karen and Leah. At the very beginning of the fall 1970 semester, I came home at the end of a day (September 10) to find that Annie, Leah, and our car were gone. Annie's body was found in the car twelve days later in Monkton, Vermont—no sign of Leah. With the help of the FBI, bloodhounds, the state of Vermont, helicopters, radio, television, and newspaper articles (and many self-proclaimed seers and prognosticators who contacted us), we searched and searched for Leah. A group of my colleagues from the CCNY Physics Department traveled to Vermont to join the search. Leah's body was found many weeks later toward the end of October in a garbage can in Dorset, Vermont. Annie had killed her within the first few days after their disappearance.

The next ten years or so were very difficult. I recall the day, (a year or two after Leah died) when I suddenly sat up straight on my psychiatrist's couch holding my head: "Everything I say is acrid and corrosive, it burns my mouth as I say it; I absolutely *MUST* stop." In retrospect, I believe that was a defining moment; it was the beginning of my long road back. I stayed busy—idleness is impossible for me. The walls of my large apartment are covered with the needlework I did during those years. I guided my students toward finishing their PhD degrees, returned to teaching, served for a three-year period as Executive Officer of the City University of New York (CUNY) PhD Program in Physics (hated it!), and did some (very little) physics in a collaborative mode. But the curiosity, energy, drive, and excitement that had driven my earlier research were missing.

I gradually inched back starting with a sabbatical year (1979–1980) at the University of Tel Aviv, ostensibly working with Guy Deutscher, a plan that did not work out as well as I had hoped. Without teaching, committees, research, and all the other responsibilities I normally had as a faculty member, I had to ask myself every morning, "What do I do with my time today?"

I had to develop a different rhythm. I had time to read and ponder, to go to seminars, and to travel a bit. I came back to New York at the end of that year with renewed energy and the will to get back to my lab. I started by writing small proposals to apply for internal (CUNY) research support, with mixed success. I was no longer at, or even near, the forefront where the interesting stuff was happening. With the small amount of money I did manage to obtain and with the occasional help of an undergraduate student, I started to run the low-temperature Faraday balance magnetometer that a former graduate student (Jim Haddad) had built. In the mid-1980s, I succeeded in getting funds from the Department of Energy. Thus began the most productive (25-year) period of my life, starting when I was in my early 50s.

During the 14 years since Leah's death, the larger of my two labs had become a departmental storage room filled with shelves and cabinets full of stuff (old rugs, glassware, you name it). I circulated a memo asking everyone to remove their belongings. No response. So I distributed another memo informing everyone that if I had not heard by the end of the week, everything was going out with the garbage. And so it did. I succeeded (on the second try) in obtaining funds from the National Science Foundation to purchase and install a dilution refrigerator that would enable reaching the temperatures needed to obtain data relevant to the physics I was planning to do.

My inspiration originated again from ongoing work at Bell Labs. There was a great deal of beautiful, ground-breaking work emanating from Bell about the metal-insulator transition that occurs as a function of dopant density in phosphorus-doped silicon (Si:P), an archetypical, strongly correlated disordered system (23). One of the challenges was to disentangle the roles of disorder and interactions, a theme that still occupies center stage in condensed matter physics (and elsewhere) to this day.

This was the problem I wanted to explore. With my students and postdocs, I proceeded to measure the resistivity, magnetoresistance, Hall effect, and magnetization of p-type Si:B as a function of dopant concentration, magnetic field, and uniaxial stress. Si:B was an interesting choice because, unlike in Si:P, spin-orbit effects are strong. Furthermore, little was known about the transport in a magnetic field. We measured the critical exponents approaching the metal-insulator transition for various universality classes (strong spin-orbit, symmetry breaking by a magnetic field, etc.) (24); we obtained data that showed the change in the hopping conductivity of insulating Si:B from Mott variable-range hopping to Efros–Sklovskii variable-range hopping as the temperature is reduced (so that the energy available for hopping became comparable or smaller than the Coulomb energy) (25); we explored the presence or absence of quantum interference in various circumstances and in different materials; and lots more. It is on the basis of this body of work that I was elected to the National Academy of Sciences (NAS) in 1994 and chosen as the L'Oréal-UNESCO *For Women in Science* laureate for North America in 2005. Curiously, election to the NAS provided the spark that led to my most-highly cited articles.

At the March 1995 meeting of the APS, Sergey V. Kravchenko reported data that showed an apparent metal-insulator transition in a high-mobility two-dimensional layer of electrons formed in a silicon MOSFET (metal-oxide-semiconductor field-effect transistor) (26). Consistent with the localization theory of the "gang of four" (27) and supported by several beautiful experiments, everyone "knew" that a metallic phase was not possible in two dimensions. No one believed

Kravchenko's claim, but I was entranced. Sergey joined my group shortly after that, despite my misgivings that a postdoc position was inappropriate for someone so senior. It turned out well for both of us.

These were exhilarating times. During the next few years, we published a series of highly cited papers that reported evidence for a metal-insulator transition in 2D (28, 29). We showed in a 1996 paper that, in addition to scaling with temperature, the resistivity scales with applied electric field (30). This caught people's attention. Shortly thereafter, we showed that the unexpected metallic behavior is suppressed by an in-plane magnetic field (wow!) (31). These papers, and many others that followed, drew a great deal of interest, attention, debate, and about a dozen theoretical models. Much of the controversy focused on whether we were seeing a true metal-insulator transition in 2D where everyone expected that metallic behavior is forbidden. We had entered a new regime in which the electrons' interaction energy is comparable with or greater than their kinetic energy—strongly correlated electron systems. The behavior of these systems is still not well understood.

During this period (in the mid-1990s), I received a call from a program monitor at AFOSR who suggested I submit a proposal within the next five days on a topic in magnetism for which he might be able to provide funding. Never had such a thing happened to me, and never has it happened since.

When Eugene Chudnovsky joined the faculty of Lehman College (a sister CUNY institution) in the 1980s, he tried to interest me in joining him in a search for macroscopic quantum tunneling of magnetization (MQTM); I was too busy at the time. I now contacted Eugene to discuss the phone call from AFOSR—we decided to join forces, and we obtained the funding!

Jonathan Friedman (now at Amherst College) joined my group shortly thereafter as a graduate student; he chose the search for MQTM as his thesis project. In collaboration with Javier Tejada of the University of Barcelona and Ron Ziolo of Xerox, we discovered MQTM in Mn_{12}-acetate, a prototypical, highly symmetric, molecular magnet. This material is composed of magnetic clusters of sizable (by atomic standards) spin magnetic moments $S = 10$ regularly arrayed in a crystal with uniaxial anisotropy that traps the spins in the up or down direction, giving rise to hysteretic behavior at sufficiently low temperatures (see the data and the double well potential shown in **Figure 2**). Steps in the hysteresis loops due to resonant quantum tunneling occur when the applied magnetic field causes the energy of levels corresponding to different quantized spin projections on opposite sides of the anisotropy barrier to coincide. **Figure 2** shows results obtained for powder samples and single crystals.

We reported these findings at the 1995 Conference on Magnetism and Magnetic Materials (32, 33). Our May 1996 *Physical Review Letters* (33) paper reporting MQTM has garnered nearly 1,900 citations to date. A sizable European community of chemists and physicists had been looking for evidence of MQTM in this material—the Europeans published cleaner results on single crystals in *Nature* in the fall of 1996 (34). The discovery of MQTM stimulated enormous new activity and expanded the field of molecular magnetism tremendously. For our work on molecular magnets (35), Jonathan Friedman was awarded a share of the Agilent Technologies Europhysics Prize in 2002, and I shared the Buckley Prize with David Awschalom and Gabe Aeppli in 2005 "for fundamental contributions to experimental studies of quantum spin dynamics and spin coherence in condensed matter systems."

Another finding that was great fun was our discovery of magnetic deflagration, spear-headed by then-graduate student Yoko Suzuki (36). It had been known for some time that, rather than reversing by way of a controlled sequence of steps, the magnetic moment sometimes exhibits an abrupt, complete reversal in a single step (see the near-vertical lines in **Figure 2**); these events were referred to as magnetic avalanches. We discovered that the "sudden" flip proceeds through the crystal as a spin-reversal front traveling at speeds that are two orders of magnitude below the speed

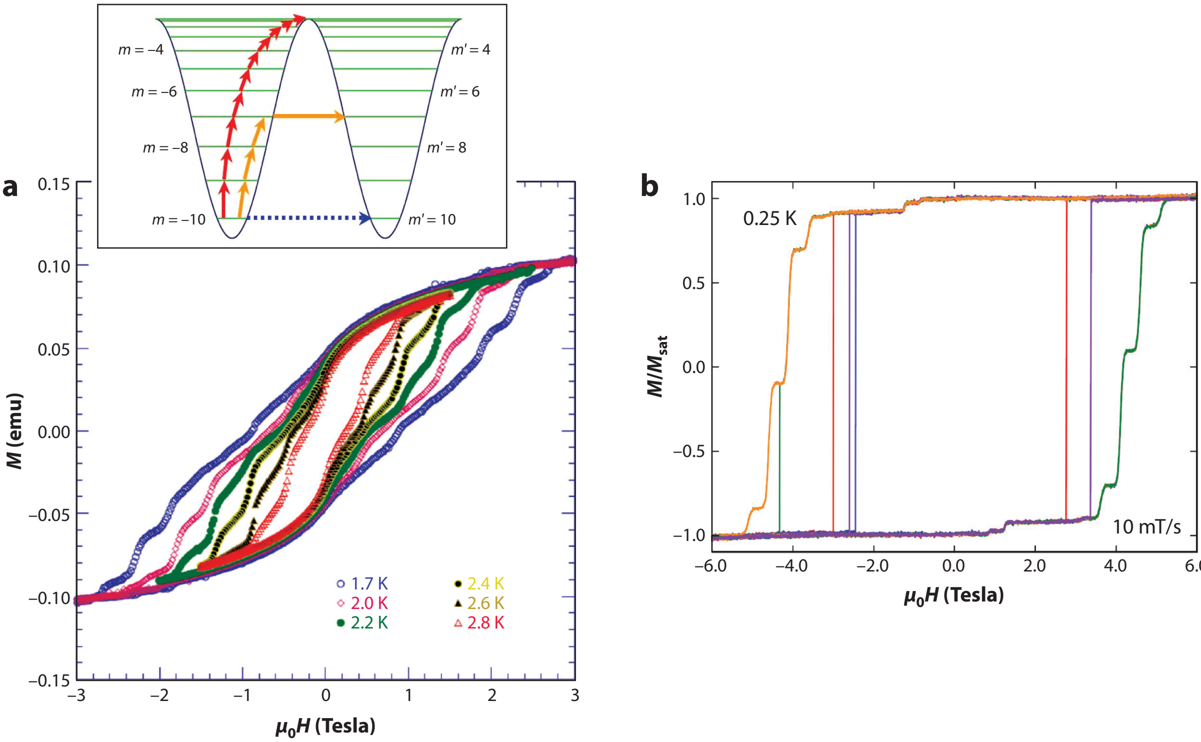

Figure 2

Magnetization as a function of magnetic field applied parallel to the easy axis of the molecular magnet Mn_{12}-acetate. (*a*) Data for an oriented powder at different temperatures, as labeled; the inset shows the double-well potential in zero field. Steps in the hysteresis loops occur whenever the applied magnetic field causes the energy of levels corresponding to different quantized spin projections on opposite sides of the anisotropy barrier to coincide. Adapted from Reference 33. (*b*) Data obtained for a single crystal at 0.25 K; the (nearly) vertical lines denote abrupt reversals of the magnetization or magnetic deflagrations.

of sound in a process akin to chemical deflagration. Magnetic relaxation is a reaction–diffusion process in which the reaction is the reversal of spins that release Zeeman energy locally and the diffusion refers to the transmission of the energy to adjacent material. When the locally released Zeeman energy cannot be removed by thermal diffusion, an instability occurs that gives rise to a front of rapidly reversing spins traveling through the sample at constant speed. **Figure 3** shows a simulation of the process of magnetic deflagration executed by Yoko's fellow graduate student (and now husband), Kevin Mertes.

And now for some closing remarks. Given my background, I quite naturally became active in human rights and women's issues. Human rights violations persist and will surely continue in various parts of the globe—it is crucial that we continue to do battle to try to help individuals who are trapped in these terrible situations. By contrast, opportunities have improved for women in the sciences, and their prospects of being accepted and succeeding have improved substantially. I do not mean to imply that we've solved this problem—far from it. But it is unquestionably true that we've made a great deal of progress.

As one of the very few women in physics during my early years, I was invited and agreed to serve on many advisory committees in the US and abroad. In addition, I was active as a member of numerous committees of the APS; these activities culminated in my election to the APS Presidential

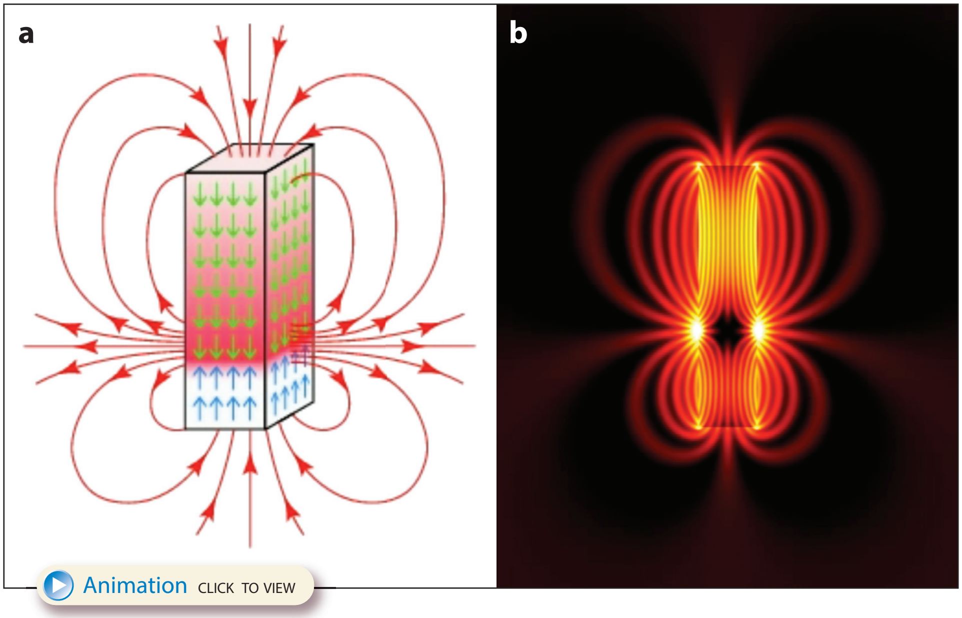

Figure 3

Animations of a schematic (*a*) and a computer simulation (*b*) of a magnetic deflagration initiated at the top end of a sample and propagating downward as a spin-reversal front at subsonic speed with a consequent release of Zeeman energy. Computer simulation courtesy of Kevin Mertes.

line—a year as vice president, a year as President-Elect, and then President in 2003 and Immediate Past President the year following. When I was young, I wanted to play the piano like Arthur Rubinstein—an unattainable dream. Little did I dream that I would become President of the APS, leading and shaping policy for the society and traveling throughout the world to represent American physics.

Remarkably, despite the difficulties that many of us are encountering as we try to obtain funding, our field is alive and well. We're making incredibly rapid progress in honing and increasing the exquisite sensitivity of our tools; measuring and controlling on unbelievably short length-scales and timescales; fabricating unimagined new materials; and discovering new phases, topological effects, time crystals, and much more. Many fascinating questions remain in condensed matter physics; the same is surely true in neighboring fields—plasmas, atomic and nuclear physics, sand piles, cosmology, and on and on.

And yet, we know but a thimble full. Two examples come to mind. We have struggled to understand quantum mechanics since it was born a century ago. It is a beautiful theory that "works" incredibly well. But what is this beast, and what is its meaning? We're learning to harness it, to control it, and to actually move macroscopic-sized objects. We have managed to manipulate the Heisenberg uncertainty principle into corners; we have pinched, probed, jostled, and poked wave functions ever so lightly to try to extract information without collapsing them. And the features and consequences of the entanglement of quantum mechanical states are truly mind-boggling. I find this area of research absolutely riveting. On another front, the Standard Model works remarkably well—it predicted the Higgs boson; we found the Higgs boson. However, although we have been able to describe the weak, strong, and electromagnetic forces within a common framework, we have been unable to incorporate gravity into a common framework despite decades of trying. Furthermore, more than 90% of the Universe has gone missing in the form of dark energy and dark matter. Have we framed the riddle this way in order to hold on to a theoretical framework we know and love? Is this simply masking an incomplete understanding of the fundamental laws of nature?

The most profound question is that of awareness. I have no problem with the fact that you are aware, or that the elephant in the zoo is aware. Science is just beginning to make some progress toward understanding "awareness." But the real mystery is *self*-awareness. Why me?

My self-awareness will soon be extinguished. For the moment, I've been having one hell of a ride!

DISCLOSURE STATEMENT

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