

## **Athena on the Charles**

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In March 1999, MIT announced that for some time it had been discriminating against its women faculty in the school of science. After a five year investigation, the institute determined that -- what at first appeared to be personal disputes -- turned out to be *officially sanctioned and institutionalized* discrimination against women scientists.

Immediately upon the report in the New York Times of this historic confession by the President and Dean of Science of one of the nation's leading research centers, some of MIT's peer institutions publicized vehement denials that such reprehensible practices could be found at their universities. According to their spokespersons, neither Harvard, Stanford, Berkeley, nor Yale practiced such gender discrimination.<sup>1</sup>

This public confession of institutional discrimination against women scientists follows upon several decades of research on what is sometimes called the "woman" question in science. This research has tried to understand why there are so few women scientists, especially when compared to the growing number of women in other professions. Certainly, women were historically excluded from law, medicine, and the ministry (the other traditional professions). Certainly, women were historicaloloy excluded from law, medicine, and the ministry (the other traditional professions). And yet, the number and percentage of women studying for the professions is growing to nearly 50%. In science, the "pipeline" in Biology and Chemistry is no longer a problem, as the number of women graduate students is now also about 50%; but the number of women at the top of science, in all fields, just as the number of women at the top of the legal profession, as partners in large firms, or in medicine as heads of departments and hospitals, on boards does not seem to have changed.

If there are relatively few women scientists however, there is a surfeit of theories explaining why this scarcity of women scientists, especially elite scientists. Some argue that women actually think differently and the female "voice" and "ways of knowing" are not comfortable with abstract, perhaps reductionist, perhaps linear, scientific reasoning. Others suggest that women make choices, admittedly hard choices, that value family and emotional relations over the single-minded pursuit of career which often characterizes the very best scientists. Some explanations for the dearth of women scientists focus on patterns of schooling and socialization that encourages males to pursue math and science from an early age. This socialization channels young boys in ways that allow them to choose science or engineering as a career so that, (given the fall off and funneling over the years of education), as one observer has noted, "it takes 400 ninth-grade boys to get one Ph.D. scientist but 2,000 ninth-grade girls."

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<sup>1</sup> However, some other peer institutions pledged to look into the matter.

Considering the range of explanations, some centered on personality, others on cognition, yet others on socialization of the person, the gender issue in science is in a sense over determined: there are too many reasons for the persistence of too few women. With the exception of the educational funnel, which is more descriptive than explanatory, these theories do not persuade. Moreover, I have never liked explanations that blame oppressed groups for their subordination - not because the oppressed may not collaborate, they often do. But, such explanations - women's choices or ways of knowing - misdirect attention from those with greater power who benefit from systems of inequality and who have greater resources and capacity to do something about subordination. Moreover, explanations that focus on the action of individuals end up ignoring the most powerful forms of subordination, those that are buried within less visible structures - social organization and cultural habits - that create, reproduce and sustain inequality.

The MIT report on the status of women in its school of science created a unique opportunity to take a fresh look at what was becoming a tired and seemingly intractable question. But, it seemed to me that instead of continuing to focus on the "failure" of women to become scientists, we might want to explore the status of women in science by looking at these women as a series of success stories. By looking at how these women succeeded, we might, I thought, be better able to identify the organizational and cultural resources that support success and infer their differential gendered effects.

At MIT we have what has been repeatedly shown to be unusual: women who have overcome the educational, emotional, and cultural obstacles to become respected scientists in the highest tier of their profession. In fact, they are enormously successful women, not only tenured faculty at one of the very best places in the world to do science, but 40% are also members of the National Academy of Science or the American Academy of Arts and Sciences. They are recipients of numerous national awards, including awards by the President as America's most accomplished women. How did these women become major scientists? What followed from their professional achievements? What does scientific success mean for women? Is it the same as for male scientists?

This is the project I set out for myself when I began to think about this question. But, I must stop for a moment and offer a warning, -- as a form of caveat emptor - buyer beware.-- that most of what I will talk about will not answer all these questions. Neither is all of it new, and some will not be about women scientists. Moreover, it will not be thorough.

I have been waylaid in this project, and pulled along other paths. As I began to interview scientists about their lives, their careers, and their laboratories -- I had decided that doing life histories of women and men scientists would be the best way to answer these questions, I came across another story and I have been pursuing that. It was closer to the topics I had studied throughout my career, -- where I had more expertise, more hopeful of getting funding: that is, the way law works from the bottom up, primarily outside of

courtrooms, law offices, and police stations: how law appears in the daily and common places of everyday life.

Following this strategy of looking for law in the cracks and crevices rather than the public stages of social life, and being interested in the relationship of law and science -- what had pulled me to the story of MIT's confession of discrimination in the first place- I am currently tracing the forms of legal regulation of laboratory practices. Because this is a project that is tied to a time bound phenomenon - the invention of a new environmental, health and safety system for research laboratories, I have to keep on top of it. So, the specific work on women scientists goes on in between and alongside the laboratory research that is occupying most of my time.

With this caveat and framing, I would like to think about my remarks this evening as a set of provocations for discussion - an idiosyncratic collection of observations about the social organization of science that might prove useful in trying to understand the woman question in science. But I cannot in any way offer a definitive analysis. I would, moreover, like to focus on explanations that look elsewhere than on the personality, women's choices or ways of knowing. Instead I will focus on the social organization of science and scientific careers to try to begin at least to fashion an account of what happened to the women at MIT.

When sociologists talk about organization - they mean the arrangement of social roles (including authority), lines of communication, and not unimportantly, the arrangements of time and space. By calling this organization, we emphasize the distribution, dispersal, arrangement of these phenomenon as an independent aspect from the content. I will focus on three features of social organization - numbers, temporality, and social networks. I think it will become clear that they are interrelated. I will use them as heuristics to identify some of the gendered aspects of the organization of scientific work.

(1) Let me begin with **small numbers, perhaps the most important variable**. What do I mean? I do not want to explain why there are small numbers, but instead I would like to suggest some consequences small numbers have for the woman scientist.

Until recently, women have occupied the position of "token" - a term coined by Rosabeth Kantor - who used to teach in the Sociology Dept here at Yale the 1970s and 80s.

A token is a place holder, a stand-in for all other women. Sometimes tokens have advantages, being seen by being different in an organization where success is tied to visibility, being known. Sometimes, however, tokens experience the loneliness of the outsider, of the stranger who intrudes upon an alien culture and may become self-estranged in the process of assimilation. In both cases, the experiences and success of tokens is different from members of the dominant group. The turnover and failure rates of tokens, for example, is much higher than those of the dominant group.

I have prepared a picture of what tokens look like...

If we think about the MIT women, despite their ability to overcome very strong cultural and structural hurdles to becoming accomplished scientists, when they reached the pinnacle they sought, these women experienced impediments to scientific productivity, obstacles to accomplishment and recognition created by virtue of their small numbers. And, although these are unusual women by virtue of having become successful and elite scientists, within the positions they achieved, they behaved like many other people who find themselves in the same structural position marked as different and distinctive, even deviant.

Occupying the position of a "token," women scientists, like women lawyers, or black professionals, adopt recognizable coping strategies. Indeed, I think you are familiar with these strategies. But, let me emphasize the point. Although these activities are often attributed to women scientists, or to women, or to persons of color as somehow characteristic of that kind of person, it turns out that these are characteristics that attach to the position or role of token. They are not personality features. The position of isolate in the organization - in the drawing of being an O in the world of Xs - drives these strategies.

For example, tokens are always in the limelight, -- look at the picture; you see the O much more than you notice the variation in the Xs. Publicity or attention is a double edged sword. So, tokens, feeling the public glare, often insist on higher standards for themselves than do members of the dominant group. Unwilling to provide what might be interpreted as evidence of lesser ability or skill, women scientists are overly cautious. They repeat experiments, check and recheck data, review manuscripts obsessively, lest an error or typo slip through. If they are to perform publicly, they often spend more time preparing, editing, practicing, being unwilling to take the chance that an off-hand account might be misinterpreted. They know they are being watched, seen more than others, and they are not wrong.

As a consequence of these coping strategies, professionals who occupy token positions, often appear guarded, unfriendly, less collegial, uncooperative. They seem to exist outside the circle of whatever the important, effective, in-group is. Or, if they manage to enter the in-group, they are accused of being a token, of not belonging.

Thus, women are often reluctant to collaborate because they know that women's contributions are often overlooked or undervalued -- because of that double edged sword, they are seen, they do not really belong, and are not given the same recognition for the same work.

For women scientists, being overly cautious in forming collaborations, in drawing conclusions from data, and in publishing results, has meant that they often seem less productive than their male colleagues and are rewarded less (with institutional space, honor and salary) because they seem to deserve less. Publishing productivity is an issue that came

up in more than half of the interviews I have conducted. Rather than publish ten papers describing variants of the same experiment, a women scientist is more likely to wait until the entire series of experiments is complete and publish one article covering the entire set so that all the loose ends and variations can be more fully explained.

To continue, women who work in a predominantly male environment are often expected to pass what some observes call loyalty tests: to behave like the men do, to fit in. They may be expected to act like the men do, laugh at sexist jokes, agree that women are too emotional and unstable, suppress female aspects of their body and person, so that they don't become an extraordinary focus of attention. And, "they make sure that although they are relatively unusual - being so few in numbers -, they are also not the one squeaky wheel in the organization." (Kantor)

For most of the women scientists at MIT, this was not hard for them. They believed that they had entered a world where their gender no longer mattered and that they could go along just like the men doing their science. As one of the women described the situation, "most young women entered MIT as junior faculty believing there was no problem. And people, even myself, thought civil rights and affirmative action had solved all that. So it was a very slow awakening of these 15 people as they progressed through their careers at MIT that something wasn't quite right. And what it was was a very subtle thing, you could hardly point to a single incident and say that's it."

(2) **Temporality** is a second aspect of social organization that distinguishes women and men. The most obvious example is, of course, the sequence of stages of the life course and the relatively fixed years of child bearing for women as against men. Women's biological clock has probably been the most often cited explanation for anything and everything having to do with gender stratification and discrimination. I would like to focus, however, on other aspects of temporality, that may intersect with but are not directly a consequence of the biological clock, in the sense that child bearing is biological, child rearing is a social and cultural phenomenon.

A major problem of parenthood for scientists, or any worker for that matter, is that children take time and energy away from scientific/ occupational pursuits. These effects appear to be gendered. In a survey of scientists by Sonnert and Holton, women are more likely to report experiencing tension between their personal and occupational roles (73% women, 66% men), and are more likely to say that children had a negative influence on their careers (32% women and 25% of men).

We know from much research that women manage two careers, the one for which they get paid and the one they do for family. Men sometimes put in time, doing chores, carpooling, babysitting. Although there are some magnificent exceptions, researchers have consistently shown that men are much less likely to manage and organize whether in the workplace - doing the informal work that keeps work groups productive - or doing the coordination that allows the various members of a family to do whatever it is they do. Thus, a good part of

women's lives is spent parsing time, treating it as a commodity to be negotiated and dispersed across their various roles and made available for others. I think this is what accounts for the greater tension women report in managing multiple roles.

Here I would like to draw on the many studies of lawyers to illustrate how time management and commodification creates gendered advantage and disadvantage. In her study of small firm lawyers, Carroll Seron asked each person she interviewed, men and women, to describe a typical day. Most of the men, married or single, with and without children, worked more than a full time week, by working early in the morning, late at night and on the weekends, or all three. Barring extraordinary circumstances, men who worked these expanded hours tended to be relieved of time-consuming private obligations; they were able to expand hours at work flexibly and independently by having family support at home. Women, on the other hand, who worked expanded professional hours tended to hire help, could not changle hours without planning, and did not usually work expanded hours in all three domains, morning, evenings and weekends.

Most of the women Seron interviewed answered her question about the organization of time by describing the work they did before they left for the office, doing housework, getting the children ready for school, preparing the housekeeper for the day's events. Most of the men began their description of their work day with the time they arrived at the office and then had to be asked to back up and describe what they did before leaving home. The men did not incorporate the time at home in their description of their obligations.

All of the women attorneys interviewed, married or not, who had children, were the partner most responsible for child care. This means finding and supervising household workers, locating appropriate services, and substituting when other arrangements do not work out. The men who worked a normal day, remained the partner least responsible for home and child care. This has been confirmed by dozens of studies.

What Seron underscores, however, is the importance of flexibility as a benefit men enjoy. One way of the other, the work of others - wives, housekeepers, babysitters, or a mother in law - relieved these attorneys from private obligations. Although they do not always use the time to expand work hours, they do have access to that time, should they decide to put in more, or to use it for leisure, something the women reported having little or none of.

In this and several other studies, all but a negligible few of the attorneys who worked less than full time, were female. The politics of part-time work or flexible hours is well documented in the studies of lawyers, as well as other occupations. The question remains open whether it is replicated among scientists. Technological changes have made it possible for more people to work from home but doing so is nonetheless stigmatized. I will conclude this topic with two examples.

A few weeks ago, I was told that one of the largest law firms in Boston had denied one of its most energetic and successful young women associates the option of working 2 days a

week from home. She had already gone to 80% instead of 100%. Several attorneys were already working from home; others were working 80%. The combination was apparently a problem. The firm's managing partner explained the decision to the younger attorneys as a consequence of the inconsistency in performance among those who were working part-time. Some were working much more than 80% but getting only 80% pay, others billing fewer than 80% of the firm's standard expectations, yet earning 80% salary. None of this had to do with working from home, but that just added skepticism to the entire practice. When working from home was about community, more acceptable than when about family or childcare reliability. The firm was looking to work out something more equitable, they said, but still flexible.

In another study of lawyers in Denver, one attorney reported that she never told anyone when she was going out of the office to do something for or with her children. She realized, she said, that lots of the other attorneys were regularly going out to play squash or golf, or long lunches, and that this was considered legitimate as business generating activity. And well it might be. But she was generating human capital, she said. So, she just worked with her door closed and most of the time, they did not know where she was when she was not there. She did, however, feel isolated and uncollegial.

There is no question that part time professionals are stigmatized - directly when they are taken off the tenure track or indirectly when they become the butt of others' jokes about their light schedules. Coworkers express disapproval through snide comments, sarcasm, slights and non-verbal behavior. In law firms, clients sometimes complain about what they interpret as being inadequately served. A few part time attorneys have reported dismissive treatment, which included not being given business cards.

Labeling plays an important role in attaching stigma to part-time status. Although pay is proportionately reduced, your commitment and professionalism is questioned by assuming the "deviant" status of part-time.

Let me conclude this discussion of temporal dimensions by emphasizing that a lot of science cannot be done at a distance, or electronically, or part-time. There are temporal sequences and rhythms to research, as the shift in my own research agenda suggests. It is certainly possible to read journals, write papers, review proposals at home. But it is not possible to supervise students, meet with your group, chat with colleagues, participate in department meetings and gossip, or conduct the experiments from home. It is also not possible to stop experiments in process because the babysitter is sick. Being in the lab is the mark of being an experimental scientist -- a mark of pride. One scientist told me when I asked what had begun this career, "I was a lab rat; it was live, drink and make love in the lab. And I loved it." Laboratory life is not a part-time occupation, at least according to every scientist I have interviewed thus far.

But it is not just the social status and emotional engagement. There are real organizational needs attached to the lab time. As one Nobel prize winner explained to me when I asked him how much time he spends in the office, "Don't you see how I keep my door open, and how my office is next to the lab. I have to be here. If I am not here working all the time, then they are not. They (he was referring to the students and post-docs), take their cues from me. I need to be here all the time so the work gets done."

(3) A third feature of women scientists careers that I want to mention is the shape and size of **social networks**. Because women scientists are a small proportion of the scientific community, because they are dispersed across the community in different institutions, organizations, and laboratories, they end up having more restricted social networks in extent, size and similarity of members. A social network is a space of interaction and connections. They are formed by exchanging information and elaborate to form distinct varieties of relationships and hierarchies.

Women scientists form fewer social relationships at their place of work than do men; their social and professional networks have less overlap, and many of their networks are less dense than are those of male scientists. In other words, there are fewer channels through which women scientists receive and exchange professional and institutional information.

This has significant consequences for women's careers, most notably that women receive less informal, unofficial relevant information; they are simply outside the loop. They learn about the organizations in which they work more from their own experience and from official documents than from stories about others' experiences and perceptions. Lacking the networks within which to tell stories - Yale Women's Faculty Forum probably has become just such a network - without these networks, women individualize, personalize, and internalize characteristics and experiences that are not features of individuals are aspects and consequence of a social organization or structure of inequality.

One of my respondents who was working at a medical school talked about this in her interview. This biochemist talked about how at each stage of her career -- until the last decade, she was the only woman or one of two in her cohort . She felt isolated, without friends to talk to about the problems she experienced in the laboratory, no one with whom to check whether what she was experiencing, others experienced too.

Looking back on her career, she is in her mid 50s, she observed changes in the opportunities for women scientists, but she was not entirely optimistic. In telling me of her education and decision to become a scientist, she said that she now realized that she had had peculiar mentors; she didn't realize it at the time. The professor for whom she worked in her first years in graduate school never spoke to her. He set her to work on a problem and then never gave her any further instruction or supervision. She struggled, without experience, without knowing what the routine practices were, or usual obstacles she would face. She did not realize that everyone in the lab had difficulties with their experiments and that things did

not move smoothly. She also did not realize that this was a very peculiar professor, who rarely supervised anyone. It wasn't because he was disappointed in her work. She was a novice, not knowing what to expect of her supervisor who was very different from the college professor who had encouraged her to go to graduate school. She loved lab work and had gone into science because of it. But she was failing, she thought because the experiment was not working out. She interpreted this as her fault and decided to leave graduate school.

It turned out that she married one of the undergraduates in a lab for which she was the TA, and when he decided to go to medical school, she re-enrolled in a Ph.D. program at the same institution. She says that without his support, collaboration over the years - they are a team - she is not sure she would have continued. But it came at a price, she said. Her career always followed his, and the work they have done together for most of their careers was her project originally. She says that she is a very well recognized scientist. I have seen her cv and can confirm this by her work on prestigious boards, committees, funding agencies, blue ribbon panels etc. But, he had been the professor of medicine these several decades, she a senior scientist who supervised the research and the lab; the work is internationally recognized, but she was given an academic appointment only recently when they switched institutional homes.

I asked how many friends and colleagues she had with whom she regularly lunched, went to exercise class, with whom she talked informally about science, the lab, things like that. She said a few, mostly the people who worked in her lab and maybe a few with whom they collaborated. She didn't get into the politics of the university, or that kind of thing; never participated in the organization all that much. She had, in my conceptual language, very restricted network, limited to international community working on the same problem but not in her organization or institution.

One of the facts that had been discovered in the MIT study of the women in the school of science was that their research had been funded differently than their male colleagues. The women were paying a higher percentage of their own salaries out of their grants than were the men. This meant that that the women had to raise more money to support the same size research group than did their male colleagues. And, we know that women tend to receive, on the average, smaller grants than the men. And further, as much research has shown, productivity in science is intimately connected with funding. Yet for years, these women did not know how the department funds were distributed or how their colleagues were using their grants. They were never in the leadership roles, and they were outside the gossip network. As the interviewee I just quoted said, they didn't "pay all that much attention to the organizational stuff." They had been excluded and hadn't recognized the scientific consequences, or if they did, they assumed that they deserved what they got.

The MIT report emphasized this unequal distribution of resources between male and female faculty. What the committee and the institute leadership found absolutely persuasive, however, was that the inequality was found not in some of the categories or even half, but in

*every variable* they measured: lab space, salaries, proportion of funding from the institute, nominations for prizes, there was a difference by gender. One or another variation by gender could be explained by noise, or by reasonable situational determinants. But not every variable; statistics do not work that way and as scientists they knew that.

Moreover, in all the interviews, the women faculty talked about marginalization, what I am calling restricted social networks. If they recognized that they were left out of committees, and from particular opportunities, they had explained it to themselves as an individual problem. "I am difficult;" "I need to learn to get along better." "My standards are perhaps too high;" "I am not working hard enough;" "I can't put in the time."

One of the consequences of being so few in number, having less time because you have more and diverse responsibilities, and having restricted networks, women scientists tended to sustain a more formal conception of their role as scientists, to value as we have seen the lab work rather than the committees, the organizational stuff for which they do not have time. They separate the official from the unofficial, discount and discredit the informal, insist on higher standards within the formal criteria, and sometimes strive for an unrealistic perfectionism within their science. In effect, being marginalized, the women scientists often saw their institution and profession from a distance and in its relatively abstracted, idealized and reified picture of itself, where hierarchy is a product of merit and reward. Thus, what they could see best, which was themselves, must be the explanation for whatever was happening or not happening.

With restricted social networks, they did not learn as much (or as easily) about the back stage activities of informal information and exchange that supported the front stage performances of hierarchy based on merit and desert. Cronyism was publicly frowned upon and not part of the public face, but being outside of the central networks, the women were never quite aware of how much informal networking and patronage went on. Of course, they knew of some, but what gets discussed over a beer or in the locker room they never learned on a reliable or routine basis: about this or that project being generated in the defense department, a new collaboration in the department, or about how Joe Green's graduate student has done some marvelous work and we ought to have him out. This is not to say they never saw it, but their picture was circumscribed by the narrowed more formal official lens with which they experienced the institution.

So, if going along was rewarded, and being a woman was irrelevant, and if they could not reconcile their idealized conception of meritocracy with their own experiences, how did these women scientists come to see that being women and scientists put them in a very different position than their male colleagues? How did they recognize these subtle forms of discrimination since they were apparently invisible to themselves as well as to the leadership of the institute? How did these elite women scientists negotiate this final set of hurdles? How did they manage success twice over? I think the answer is quite simple. **By talking to each other and telling stories.**

When they began seeking each other out, and then explained why they were calling or wanted to have lunch, the women began telling stories to each other. They began to see that what each had experienced as personal marginalization was in fact general and gendered; it was experienced by all the women with whom they spoke. In a sense, you could say, they overcame small numbers by holding up a mirror for each other to see the multiple refractions develop.

Were these women feminists? Were they conscious of their subordination and discrimination before they mobilized? I think not. This event grew from a dispute, perhaps even a series of disputes involving one woman who finally had had enough and had the courage to let what would normally be considered a private, departmental matter go beyond the local turf. The norms of privacy and department autonomy - the official and unofficial structure of the institution that both keeps people in their places and at the same time supports its creativity - had to be breached. That was the moment of change. This is key in my mind, by violating the taken-for-granted norms, the local culture and structural supports for how things are routinely done was exposed, revealed, and made available to their appropriation.

Here, the norm of departmental autonomy was violated, and this minor act of deviance, initiated the rest. Importantly, this norm is violated routinely by those who would invoke informal networks to garner resources for whatever project they are interested in - to get more space, additional funds, another position. But, if you see the organization through its official lens, you don't do that. You follow the rules, you go along, you apply competitively through channels, you don't rock the boat, you don't question departmental autonomy.

Once the story of a dispute about who was teaching what course got outside the individual department, however, the women in several departments and then in all the departments in the school of science began talking to each other. They invented the network which had not existed before. It was not a revolution, although it may, we can only hope, have a revolutionary future. It is still a work in progress.

But let me emphasize the point, they moved, and yes moved slowly, from the level of individual dispute, to organizational claims, to institutional blame and collective action by (1) violating a local norm, (2) then telling stories to each other, (3) finding in those stories familiar experiences and reflections, and (4) finally identifying the patterns (structures) of inequality in the stories.

So, did feminism play a role in the careers and organizing efforts of the MIT women? Were these women scientists feminists before they brought their concerns to the administration of MIT?

I think feminism played little or no role in the careers of the MIT women or in their committee's efforts. Once their network energized, however, and they began to talk to each

other regularly, and then to talk to the administration collectively, things began to change. Through this talking and storytelling process, they began, and note I say began because this is as I said a work in progress, they began to develop a feminist consciousness. No longer content to seek specific, sometimes individual, remedies for particular grievances, they are now organized in each of the five schools of MIT, researching the gendered conditions of work, reformulating policies for recruitment and hiring, as well as pushing extended family leave policies.

Finally, what role did science as science play in this story of successful mobilization and organizational change? Was there something special about MIT that is not characteristic of other institutions? Was this a process that was uniquely characteristic of scientists, or are there lessons that can be learned and applied to settings and groups who do not rely as heavily on empirical data and analysis for making claims?

Some of the MIT women faculty attribute the institution's unusual action in confessing discrimination and responding positively to the women's petitions to shared modes of thinking and analysis. Rather than speaking in a different voice, or exhibiting specifically women's ways of knowing, the household explanation at MIT is that as scientists, we could not ignore the data. "So, I think it was a group of scientists as administrators and a group of women approaching a problem in a similar way" that made this happen. "We went out, we got all the data. We measured everything; we got the lab space, the resources. We added data tables, full of tables, lots of data...The President [of MIT] said to me the other day, 'we're scientists, we looked at the data. What could we say but go with the data?' So it is a very remarkable story. As soon as they had the data down in writing ... [the Dean] immediately began to fix things and very quickly changed people's lives."

Much contemporary litigation, whether claims for civil rights or tort actions for damages, are about data: Which side of a dispute has the right data? Whose interpretation of the data is more persuasive? The data are rarely clear, in science or in law. Yet for these scientists, the data was transparent. This is a more complicated part of the puzzle. This deserves more attention and is a very interesting subject for analysis and another paper.

Let me conclude. Often, efforts to understand and remedy discrimination must contend with arguments that there is no real discrimination, that the differences that do appear does not matter, that the difference is desired, or that even if there is difference, it is not intended. This is especially so when the relatively subordinate group is distributed widely across a social structure so that, for example, not all women scientists are subordinate to all male scientists. Clearly, the women at MIT were doing much better than male scientists at the University of Connecticut, no less Connecticut College. Under these conditions, it is often a formidable struggle to convince those who benefit from the institutional discrimination, especially where there is good will and no formal or legal bar, that discrimination exists. Moreover, it is particularly difficult to convince the well intended that organized subordination can be a product of inadvertent and unintended consequences of what otherwise seem like reasonable,

productive, ethical, and legal practices. Thus, a confession of institutional discrimination, as MIT made, is a particularly unusual event and one worth pondering further. I am working on it and will stay on the case a while more.