

Biased Random Walk: A Brief Mathematical Biography

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Math genes run in my family: my mother and sister majored in math, and my father was an accountant. But I was the first of my family to go to graduate school, with much apprehension aimed my way by my parents, who had in mind for me a more traditional homemaker role.

Berkeley in the middle sixties was a revelation for someone who had just graduated from Cornell, a place that took seriously its duties *in loco parentis* for women. Among the things that were hard to miss, other than all the paraphernalia and activities of the start of the Sixties, was the lack of women on the math faculty, at least to my knowledge. There were also not many women graduate students, and so all of us got a fair amount of attention. This was not true of the men graduate students; at that time, it was widely said that the math department did not know, within 100, how many graduate students were officially registered.

This attention was not always flattering; some of those who were not doing well were gossiped about in terms that were more personal than mathematical. I wasn't aware of unpleasant gossip about me, but I was acutely and uncomfortably aware of the sexualized atmosphere around me. I wasn't subject to what has since been called sexual harassment; I was simply a young, not unattractive

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woman in a sphere that was almost entirely male. I was also doing well, which made me more of an enigma. It was hard for me to accept that I really was doing well; the unacknowledged sexual attention (and my own insecurities) made me question any praise or grade that I got. It was to be many years before I could accept that I was actually seen to have talent.

My graduate career did not go smoothly. After passing my qualifying exams with honors, I spent much of the next year engrossed in the Free Speech Movement and playing the guitar. I had an advisor, but I rarely saw him except at parties. I was ostensibly working on a problem in algebraic topology, but neither my advisor nor I was interested in that project, and he soon after changed his own field. I was rescued by Steve Smale, who would see me in the hall and suggest problems in dynamical systems. I rejected many of those out of hand (with some snottiness that I'm now embarrassed to remember) on the grounds that I knew nothing about the field; to my everlasting gratitude, he persevered anyway with more possible projects. Finally I agreed to work on one that seemed absolutely elementary, though hard.

I worked mainly by myself for a few months over a summer and got my first result. I then saw Smale (who had been out of town) and we had our first, and perhaps only, fight: he thought that it would be a thesis, and I insisted it was much too little for a thesis. He said other results would now come, and he was pretty sure I would be finished within the year. So, in September, at almost 24, I was on the job market, with a thesis I hadn't yet written up or even done. Smale was always pretty confident of his opinions, and in this case, as in many others, he was right. I had a year of very hard work to finish and write up my thesis, in addition to learning the field of dynamical systems in which my work was imbedded.

This year was one of the best and most memorable of my academic career. Smale was in the middle of re-energizing dynamical systems with many new ideas and a new program and had gathered around him a tight-knit family of graduate students (his first) and other faculty members, including Moe Hirsch and Charlie Pugh. This group adopted me, and I learned the field from the other students, Mike Shub and Jacob Palis. It was another, and wonderful, culture shock to go from

an atmosphere in which grad students were shadows in the halls to one in which I was questioned almost every day about what I had figured out since yesterday.

As Smale predicted, I did indeed come up with more results, but not without some pain and tears. At one point, I was sufficiently discouraged that I was ready to quit, and I went to the office of another faculty friend, Bernard Kripke, to cry on his shoulder. While I was there, he got a call from his then new wife Margaret, who was a grad student in biology. She was studying for her exams, also very discouraged and ready to quit. So, in an inspired moment, he took me home, mixed up a large jug of margaritas, and left us together to laugh, cry and get drunk. The next day she went back to her exams, and I to my thesis. She became a very well known biologist, and I finished my thesis, with some mathematical help as well from Ber.

My thesis was good enough (and my advisor had enough pull) that my first job was at MIT, as a C.L.E. Moore Instructor of Mathematics. I was the first woman in that fancy position, and so, once again, I didn't escape special attention. (This is an enormous paradox of my life; I was very shy, with massive anxiety attacks about being scrutinized and fantasies about being invisible, yet somehow steered my life so that what I did always placed me in a public setting within the lens of attention.) This time the attention was puzzlement: if I entered the common room, I was apt to be asked, "Why aren't there any good women in mathematics?" ("Present company excluded" was not said.) But, compared to Berkeley, I was mostly ignored, while given a lot of freedom and a light teaching load. I wanted that, because I was trying to figure out what to do next and wanted as much shadow as possible.

I had loved being in Berkeley and very much enjoyed the mixture of geometry and analysis I had used in my thesis, "On Commuting Diffeomorphisms." But I also knew I didn't want to continue the program Smale had set out to uncover the "generic" properties of diffeomorphisms. I wanted to work on something related to the so-called "real world." And I was not very qualified: my training was so abstract that, although I described properties of differential equations in my work, I never had to solve one!

The two years at MIT were hard times for me. I was working by myself to try to redefine what I might do with mathematics and was still unconvinced that I had any talent for it. I had dragged around my various “issues” through graduate school, and after my Ph.D. they insisted on coming front and center. So, in addition to being clueless about how I wanted to proceed professionally, I was dealing with an undiagnosed depression. I spent a lot of time doing nothing.

When I was not doing nothing, I was sometimes trying to read biology (I had taken no bio courses in undergrad or grad school) to look for interesting mathematical issues. I was especially taken with the idea of self-organization, including morphogenesis in biology. There was virtually no math biology theory at that time. I read what was available, which included Thom’s catastrophe theory and a body of work by Prigogine. I read stacks of papers and decided that, though I admired the deep mathematical work of Thom, he was no more an applied mathematician than I was at the time, and I needed a master more grounded in the nitty-gritty of physical phenomena. Prigogine’s work, in a different way, presented theory I couldn’t connect to things I wanted to understand better.

While back at square one, I was approached by a grad student in chemistry who was trying to understand the recently discovered phenomenon of self-organization in an oscillating chemical system, the Belousov-Zhabotinsky reaction. Since, by that time, I had acquired a (somewhat exaggerated) reputation for being a local expert in catastrophe theory, he approached me, believing that if I taught him this theory, he would be able to delve into the secrets of this new science. After many sessions talking with him, I concluded that catastrophe theory would not be of use, but the phenomenon was just what I had been looking for. The grad student was disappointed and dropped it (figuratively speaking—the reaction involves concentrated sulphuric acid!) in my lap and went off to work with Prigogine.

By this time, I was teaching at Northeastern University, relieved that I could find a job (and in Boston) after a highly unproductive post-grad pair of years. Northeastern was psychologically easier for me than MIT: it was not then a high-pressure research university, and opinions about what constitutes good math were less uniform (or uniformly enforced) than in the major research

departments. It was also filled with colorful faculty, allowing me to blend in with my own strangeness: being a woman mathematician who wanted to work in an area that didn't yet exist.

The Belousov-Zhabotinsky reaction was the catalyst for my return to mathematical life. But I knew I needed help. The only mathematical literature I then knew of that dealt with self-organization in a scientifically detailed way was fluid mechanics, using bifurcation theory. To get references for this, I decided to go back to MIT and see someone who, I was told, was a master of that field. As it turned out, he was away that day, so I knocked two doors down. Lou Howard, another expert in fluids, was in—and interested. After he found out why I wanted these references, we talked mainly about chemistry and self-organization. He then became my collaborator for the next seven years. I regard that time as my real post-doc, as he gently initiated me into new ways of thinking about science and math (as well as how to solve differential equations!). My own, different, mathematical personality, which I would never have been able to develop without his mentorship, was strong enough to show through in our joint papers. This period was also the only time I did experiments for publication, using an old darkroom (previously a men's room) that Lou had commandeered from the fluid dynamics laboratory.

I stayed at Northeastern for seventeen years. During that time, Lou moved, and our mathematical marriage ended (and, separately, my real one to Gabriel Stolzenberg began). I began working on projects by myself, mostly on issues involving geometric singular perturbation theory. Lou's crowd knew me mostly as Lou's junior colleague. A review of an early NSF proposal of mine said flatly, "She never did anything good before working with Howard, and will never do anything good after." NSF ignored that review and I got funded. The reviewer was in fact wrong on both counts: my thesis has taken on a life of its own in the topology community because of its unexpected implications for foliation theory, and I managed to muddle through some more good work after Lou, both by myself and with others.

My next chief collaborator was Bard Ermentrout. We met for the first time in Washington; I was all dressed up for a panel meeting, and Bard, who was then a postdoc in DC, sauntered in for our

appointment in his hole-filled t-shirt and cut-offs. We almost immediately became a mathematical odd couple, with different skills and approaches but with great mutual admiration (or, at least, mine for him). For many years, all our papers were done jointly. Then one day he said, "I don't want to be married any more; I want to be promiscuous." (He had, in the meantime, actually married.)

After getting over the shock of being mathematically jilted (we remain collaborators and close friends), I realized that his decision was good for both of us and also started collaborating with many people. I had already begun to seek out biologists to work with and was actively collaborating with Karen Sigvardt and Thelma Williams (and Bard) on the mathematics of Central Pattern Generators (CPGs), networks of the nervous system that govern rhythmic motor activity like walking and swimming. That collaboration had arisen from what then seemed to be another step in my professional random walk: Avis Cohen, a biologist working on lamprey swimming, convinced me that the expertise I had in working with oscillating chemical reactions should somehow be useful to people working in CPGs, so I should write a tutorial chapter on oscillators for a book on motor control that she was helping to edit.

Avis, who is about my age and was not as senior academically, became my next major mentor and teacher. The chapter was to be about 60 pages, and I refused to write it unless I knew more about the field, so I could focus on what just might be relevant. She sent me a carton of biological papers, which I tried to pick my way through. (I had a fellowship that year, and I felt a bit crazier than usual.) At the beginning, I understood roughly every third word, including "and" and "the." She didn't hang up on me when I called to ask the meaning of words (this was pre-email and we were not at the same university) and even sent me follow-up papers. Best of all, she refused to accept the first couple of stumbling attempts that I made at this chapter, making it clear that it *would not do* to write in mathese to a set of bio readers. She was the one who first tutored me in how to begin to cross the cultural barrier between math and biology, and I learned that it is a continuous process.

There was another happy accident associated with attempting to learn the literature of CPGs and write for that audience: I realized that work I had recently done with Bard on a completely different

subject was relevant to an open question about lamprey coordination. So I actually had something to write about! I met Karen and Thelma at a biology meeting in which they were presenting lamprey work and I was giving a poster on work with Bard about this. I recall them sniffing around my poster late in the session. (I was a wallflower for the first part, as everyone went to see their friends' posters first, and no one but Avis knew me at all.) The reaction was polite but suspicious: what does math have to do with anything? We soon warmed up to one another, and a long and happy collaboration ensued. One of the proudest moments in my professional career was when biologists Karen and Thelma presented a math poster at the annual meeting of the Society for Neurosciences by the four of us (including Bard), explaining features of lamprey swimming. This work also led to collaboration with biologist Eve Marder, who was my mentor in learning about the importance of biophysical detail to network dynamics.

I was very lucky to find such a hospitable group of biologists to draw me into thinking about biological issues. I still find that it is almost impossible to fight one's way into a biological subfield; outsiders tend to be ignored, and mathematicians are *way* outside. But I have found many instances (fast increasing) of biologists who have come across some of my work, seen the potential relevance, and asked me to help in some way with their own work. This has been especially true in my most recent set of obsessions, about rhythms in the nervous system associated with sensory processing and cognition. I now have many collaborations with experimentalists that shape the questions and answers of my work.

The random aspect of my training shaped another part of my career: a desire to help people work at the interface between mathematics and biological applications, without having to do a random walk. I now spend quite a large fraction of my time working with students and younger colleagues. There is still no road map at this frontier (at least in some areas, such as neuroscience), only communities of people to help each other muddle forward. But these communities make it so much more likely that students will get the mentors they need to find their own voices and make their own contributions.