Philosophical Adventures

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I had the lovely opportunity of being introduced by Alison Wylie, to whom I owe a large thank you, and thank you especially to Anne Jacobson, and the whole program committee, for this chance to share a bit of my life and career with you. This invitation charged that I was to give an "autobiographical sort" of talk. Specifically, it required the speaker to provide "an intellectual autobiography, with perhaps some account of the way in which [she] was shaped by or shaped the profession, how the profession seems to have changed over the years, etc. The lecturer might reflect on the people and issues that led [her] into philosophy and provide a personal perspective on the state of the field today." I tried to stick pretty closely to this mandate.

Over the course of my career, which is now—although I find this astounding—over thirty years long, I have had the great pleasure of seeing my primary field of research grow and establish itself as a serious field of thought and activity in philosophy. When I was in graduate school at Princeton in the early 1980s, I was told, and I quote, "there is no such thing as Philosophy of Biology. You can't write a dissertation on that." And John Beatty wrote that same year: "In the world of academic specialties and subspecialties, philosophy of biology certainly counts as a self-respecting, if not otherwise respected, field of study."[1] It is impossible to imagine anyone saying that now! Five years later, I was also told that feminist philosophy of science was hopeless, that there were no good cases of male bias in science worth discussing, and that since science was self-correcting, those sorts of bias couldn't have any long-term significance. But I eventually published a case study dramatically showing the falsity of those statements, a book that won both scientific and philosophical prizes. This afternoon, I'll tell you about a few things that happened along the way, and chat about a couple of people I have had the great pleasure to have known. But let's begin at the beginning.

I've had the enormous privilege of having been mentored, ever since I was an undergraduate student at University of Colorado at Boulder, by wonderful advisors who thought I was a serious person, starting with writer Juliet Wittman, my women's studies teacher (and, later, finalist for the National Book Award). She gave me confidence and rare experience by choosing me as the assistant director and musical director for her Theater in Prison project at the Colorado Women's State Penitentiary for the Colorado Humanities Project, which I participated in, for weekly visits, three hours drive from Boulder, for four years. Handling and directing inmates—in prison for forgery, pimping, drug offences, assault, and murder—for theater and musical productions is not the ordinary thing for an undergraduate woman, but it certainly gave me confidence later, in encountering folks who wanted to tell me what I could and could not do!

My first philosophical mentor was that wonderful mensch at the philosophy department in Boulder, the late Gary Stahl. He actually *tricked* me into becoming a philosopher. I was raised in a very scientific household. My dad's job was to be a pure mathematician at the math department at Bell Laboratories in Murray Hill, NJ. Both his sister and brother had Ph.D.s in engineering from Purdue, but he was their family's theoretician, having worked as a theoretical physicist at the Institute for Advanced Study with both Einstein and Robert Oppenheimer, back when the Institute had twelve people, after having worked on the Manhattan project when he was very young, with Wigner's group out at the University of Chicago. Even though his Ph.D. was in theoretical physics, he then was informally trained by his friends to be a mathematician, when he was recruited by Bell Labs further north in New Jersey, where he became noted for Lloyd's Theorem (or Algorithm), an early and foundational result in information theory, used in coding, and also widely used in computer science, engineering, and operations in printing. He met my mom at a club for the ancient game of "Go," and she was a mathematician on technical staff at Bell Labs, too, and was pursuing a license to fly small airplanes. She also drove a ragtop, bright red, classic MG. She was forced to quit as a mathematician at the Labs when she became pregnant with my older brother, but she remained a technophile at heart. My mom got us lots of educational toys, and she also took us regularly on lots of educational trips, to the museums in New York City, to natural history museums, planetariums, zoos, and parks, wherever we traveled. We were a family of nerds, and we argued about things at the kitchen table like: Does water boil faster if it starts with cold or lukewarm water? And what's the advantage of a mercury versus an electric thermometer, like the one our daddy built, etc. You can only imagine. My mother also bought us the entire Time/Life series of nature books. These were basically simple encyclopedia books with lots of pictures and diagrams for kids on topics such as reptiles, mammals, and North America. But when I was seven years old and the book on evolution arrived, I claimed it for myself, and read it again and again for the next many years. I actually broke the spine on the book; it started by breaking the cardboard cover, and then fell apart completely. I had to carry it carefully up to my room—and meanwhile I was waiting to study evolution in school—and had to wait until tenth grade biology!

And I was so looking forward to studying evolution in college, and by that time I'd decided that I wanted to be either a veterinarian or a marine biologist, so as a freshman, I enrolled in Biology 101 at the good Canadian school, Queen's University in Kingston Ontario, when I was seventeen years old. We had to meet with an academic advisor there, before attending class, and when I did, I was told that he had dis-enrolled me from Bio 101, and put me into Geography 110, and had also changed my major from biology to geography. "Why?" I asked. He told me that Bio 101 was a "pretty tough course," and he "didn't think I could handle it." "Many girls are better off taking a geography major," he said; so I was dis-enrolled from the beginning biology course and enrolled in geography. To say I was disappointed is just a whisper of my emotions, but I felt helpless in the face of this bureaucracy.

At that time, I had also signed up for an introductory philosophy course, which I attended for the first three or four classes and then dropped. As a Lloyd, I was weaned on questions of empirical evidence and reasoning, all of which turned out to be very counterproductive in my philosophy class (really an analytic metaphysics and epistemology class), which concerned how we knew the table was there. Coming from a scientific background, I admit that I acquired a bad prejudice about philosophers then and there, which was that they didn't ever arrive at any knowledge or useful advance. And then I did *so* badly on the first classroom assignment that it was demonstrated that I had *absolutely no knack* for philosophy. So in my first year of college, philosophy and I got an official divorce.

I did drop out of college after that year of being a geographer, and went back to school at University of Colorado at Boulder later, where they *allowed* me to study biology, and where I was picked up by the scruff of the neck by Gary Stahl, when I was taking his fascinating honors course called, simply, "Human Nature." At that time I was a premed and a political science major, and thus needed to find out about human nature because every political system assumes a basic human nature of some sort or another. He invited me to do a further Independent study, and I leapt at the chance to read more John Dewey with him (my personal favorite, and I did not know at the time that he was a philosopher). I read Jane Austin, Rousseau, Goethe, and so on, under his guidance and conversation, as well as studying Western intellectual history and honors Enlightenment and nineteenth-century European history with David Gross, a left-leaning historian who bridged the gaps between the Enlightenment, Kant, Fichte, Hegel, Marx, Dewey, James, and some twentieth-century thinkers I was enjoying. Gary then suggested that I might want to structure my own major, where he could be my supervisor, and historian David Gross and Horst Mewes, the political theorist I'd studied with, could be on my committee, all of which sounded great to me.

And *then* I discovered that Gary was a *philosopher*!!!! A philosopher! *He* didn't argue about tables! (I only found out decades later that Gary Stahl was a Deweyian Kantian Ethicist). And then imagine my shock when he suggested that I should go on to grad school in philosophy, rather than going to med school. A philosopher? Did he really think that I belonged in such an unempirical field? Under his guidance, I did end up happily taking several courses from the Boulder philosophy department, and acquired a couple of further mentors along the way, including the kind and generous Wes Morriston, who taught me Husserl, and Heidegger's *Sein und Zeit*, among other things.

But then I read Thomas Kuhn and Paul Feyerabend in modern philosophy class, and was very inspired by this new field, the history and philosophy of science. Gary told me that I could study this field in grad school, and advised me to apply to the National Science Foundation (NSF) for a fellowship. I wrote a personal statement for the NSF application. Gary told me to run it by a professor related to the field, who told me it was awful, hopeless. But I thought *he* was awful, so I mailed it anyway and, luckily, was offered the national NSF Fellowship, which supported me to go anywhere that admitted me. I also got an award from the philosophy department at Boulder, which gave me a collection of Nietszche's works.

So that's how I was *tricked* into going into philosophy by a Kantian philosopher. Isn't there something wrong with that?

But it turns out Gary was right, and the universe soon demonstrated that I have a *totally cosmic* connection with philosophy. You laugh now . . . but I was traveling with several philosophers from Italy through Switzerland on the way to a meeting in Salzburg where I was presenting my first paper. We stopped in San Moritz, a small town in the Swiss Alps, and, being a keen hiker, I had a yen to do some hiking on the beautiful ring of peaks that surround the small town. I recruited my supervisor, Bas van Fraassen, for this adventure, and got a hold of one of those painted maps—you know, the ones painted to show the main roads and the post office, and dirt roads in different shades of tan and the peaks surrounding the town with the dotted lines showing the main hiking trails? I studied the map, and there was one very promising looping trail that caught my eye, among dozens, on the side of a peak where I really wanted to go, although it was rather remote. I then called out the directions while Bas was driving. The roads and trails had no names, and there were likewise no signs or names for any of the peaks or trailheads or turns. I requested that he drive further and further down this particular dirt road. Finally, I said, "park somewhere around here, I think the trail starts here." He parked the car, and we noticed that we got out next to an isolated, small, white cottage. When we investigated, we found it had a bronze plaque. The plaque said that this was the house where Nietszche had drafted the entirety of his book, *Also Sprach Zarathustra*. It also said that Nietszche had hiked up this very loop trail every day during the book's composition. I got a bad case of the goosebumps. So, you see, the universe (and not just Gary) says that it was *fate* that I would become a philosopher!

Little did I dream of it when I was laboring in eighteen-hour days in grad school at Princeton, gleefully studying Spinoza, Locke, Hume, and Aristotle, that when you become a faculty member, you gain all sorts of "privileges," like membership on the animal care and use committee, when you get to decide how to kindly euthanize "pinkies," which are tiny bald baby mice used to feed giant bullfrogs—not really what I had dreamt of—but you *also* gain the duties to attend graduation ceremonies fully decked out in your doctoral robes.

Imagine me then, thirty-three years old when I got tenure at Berkeley in 1990, and attending my first graduation ceremony as a tenured professor at the university. I was in the back dressing area, where you pick up your robes from the bookstore, and the robes are always wrapped up in a plastic square, from which it is impossible to dress yourself and figure out which way the robes and the doctoral hood go—that's why you need aid from the bookstore helpers! On this occasion, the helpers from the bookstore were all young women, wearing two-inch by one-and-a-half-foot-long bright yellow ribbons saying "Bookstore" on their chests. (An important detail.)

So I was walking over to pick up my Princeton robes, and a middle-aged, pleasant-looking man came up to me and handed me his plastic wrapped square of robes—Do you know how, if someone hands you something, you automatically reach to receive it?—and so I naturally reached out and took them! As you would have, too!

And he said, "help me put these on."

I actually don't *know* how the hood thing goes, and I said, "I'm sorry, I don't know how . . . I think you want one of the bookstore people. They're the ones with the big yellow ribbons that say 'Bookstore'."

And I handed the robes back.

But *he* was mad at *me* . . .

It was a moment where he had never imagined that I *could* be a professor.

But it wasn't the first, or last, time that I had an experience like this in the academy. And even though we still have a long way to go, I've been *extraordinarily* lucky for all the support I've had as a female philosopher. For example, Bas van Fraassen was the best graduate supervisor ever. He was just this year given the very first Hempel Award by the Philosophy of Science Association, not only because he was responsible for revising and reviving empiricism for the late twentieth and early twenty-first centuries, but also for his "generosity, egalitarianism and kindness that he brought to the philosophy of science (including, especially, his mentoring of graduate students and junior scholars in philosophy of science)." That would include me; let me tell you a story.

I was deeply inspired in my first year of grad school in spring 1981 by a course I was taking in the history department, with Gerry Geison and Martin Rudwick on the Darwinian revolution. It was through my detailed research on Darwin's Origin and all of his published correspondence in this course that I came up with a paper to give to the brand new professor in the philosophy department, Bas van Fraassen. In the paper, I tried to articulate my idea that Darwin seemed to propose what I called "model outlines" in his theory of natural selection, and that much of his writing and especially his evidence could be made sense of if we related it to this idea of models. I argued that this approach made much more sense than the logical positivists' axiomatic view expressed in Michael Ruse's fascinating four-year-old book, *The Darwinian Revolution*, which we were reading in seminar. Bas then invited me out to lunch to discuss my paper—I had worried that he was about to dress me down like others hostile to my approaches at Princeton had—but he instead said that my paper on Darwin's model outlines was publishable with a bit of work, and he also mentioned gently that I might want to take a look at his new book, *The Scientific Image*, and what it has to say about models and "model types."

I bought the book and was so thrilled to find that, lo and behold, *here* was a theory of models—ready made—that I could use to articulate my idea about models in Darwin. So I wrote up the paper in terms of the "semantic view" of theories, and it appeared in *Philosophy of Science* two years later. I also gave a related analysis of population genetics in a second *Philosophy of Science* paper in my second year. Bas's gentle approach and kind and generous hand meant everything to my success. But what I theorized and learned from my early studies about the way Darwin confirmed his models here had real legs. Just now, my outline of how evolutionary models are confirmed is being used as the framework for evolutionary biologist David Sloan Wilson's *Evolution Institute* project involving thirty biologists working on evolutionary mismatch, or when old traits adapted to one environment are maladapted to or unfit in a new one.[2]

In my third year of grad school, I presented my ideas on theory structure and models in my first talk, at a conference in Salzburg, which many philosophers of biology were attending. I was very nervous giving this first talk, but apparently not as nervous as Bas, who I learned later had covered his face with his hands when I started my presentation. He later commented to me that he had never seen such an aggressive audience during the discussion at a philosophy talk. But it being my first presentation, I didn't see things that way at all.

Instead, I thought to myself, "if I could just explain my ideas about models and model outlines or model types in evolutionary theory in a *different way*, then the folks would see the good sense in my analysis."

Now, of course, everyone talks about models when discussing evolutionary biology (just as Bill Wimsatt, Jim Griesemer, John Beatty, Ron Giere, and Paul Thompson always did), and they can't even imagine a time when a large group of philosophers of science and biology would go *all out* to deny the sense in *that*! We now have an entire academic society devoted to the history, philosophy, and social studies of biology, and if you look at the program of its biennial meetings, you will be overwhelmed by model talk.

But I would also go on to study biology in grad school as well as the history and philosophy of science, on an exchange program. I was thus enrolled as a grad student in geneticist and evolutionary theorist Richard Lewontin's laboratory (he goes by Dick), up at Harvard, while working on my philosophy dissertation.

The highlight was a graduate seminar co-taught by Lewontin, the late paleontologist and evolutionary theorist Stephen Jay Gould, and a developmental biologist, the late Pere Alberch, and it focused on challenges and problems in evolutionary biology and evolutionary theory. There I learned that Gould was working on a problem called species selection, where he was attempting to generalize natural selection to the species level, treating species as if they were individuals in a natural selection process. But as I worked on my dissertation analyzing the structure of evolutionary theory, I came to think that he was wrong about his analysis of species selection, which he had just published with another paleontologist, Elisabeth Vrba.

As a student in the seminar, I went in to Gould's office at the Museum of Comparative Zoology repeatedly over the semester, and then after the semester was over, and then whenever I visited Cambridge, which was as often as I possibly could, since I got so much out of talking with Lewontin and Gould, and the people hanging around them—their students, their post-docs, their visitors. I would visit Ernst Mayr, as well, who had his office upstairs in the Museum of Comparative Zoology, and ran a little coffee study group for philosophers of biology and biologists, and I would argue with him in his office about group selection. It was all incredibly stimulating. And I just bugged and bugged the very tolerant Gould about this species selection problem. Some days I went away thinking I'd convinced him, but I would come back the next day and discover that he still had doubts about my analysis. Finally, after a train ride he'd suggested to Yale and back to see Vrba, and a challenge to me while riding on the train back to explain some gastropods, we finally agreed! And so he proposed writing up this theory and the gastropod example, which is how I came to write two papers with Gould.

Steve Gould was a man of many colors, and was both appreciated and criticized by his colleagues, but I found him to be *wonderful* company—walking across the quad with him to get ice cream was always an adventure, he always had some fascinating story to tell about the trees, the architecture, the wind, the grass.

He was just brilliant company. So it was in the middle of writing up the first of these papers when I told Steve about an example and analysis I was developing, about the female orgasm, as a case of bias in science. I knew he would love it because it seemed to be a case of a non-adaptation. He said, "write it up for me, and I'll write a column on it." So my work appeared in his Natural History column in 1987. I was very intrigued to see what reaction it evoked, so that I could prepare for it in my work in advance of publication, and in fact I did devote a chapter of my book to their reactions to his presentation.[3] The disadvantage was that people saw the example as Steve's, and not mine, years later, when I did finally publish my book. Nevertheless, I felt that the advance publication by Steve was a real advantage, as it flushed out a number of objections that I didn't think of.

Some years later I was back at Harvard, on sabbatical, and Steve Gould had finally finished his big book, *The Structure of Evolutionary Theory* (which he would tease me about, saying that he liked the title of my first book so much, that he had stolen it), and he and I were in the midst of writing a third article, this one critiquing the behavior ecologists' understanding of exaptation, when he declined and suddenly died. That paper is in my filing cabinet, and I simply can't look at it, even though the argument is a good one.

Meanwhile, I was also learning a lot about a huge issue from Lewontin, Ernst Mayr, E. O. Wilson, and many other biologists and philosophers—namely, the general levels or units of selection problem: On what biological levels does natural selection occur? There had been big fights about this topic in the evolution literature since the 1960s and 1970s, and the issue was custom-made for philosophers because it is so conceptual and oriented towards definitions. I was totally immersed in the debates when I went to Oxford University in 1989, along with a new set of ideas and analyses to try out on the biologists, and I got a chance to talk with some of the British leaders in modern evolutionary thinking, including some leading theoreticians in sociobiology, kin selection, population genetics, and game theory, such as John Maynard Smith, John Krebs, and Richard Dawkins. One of the most exciting was William Hamilton, who goes by Bill, and who had invented kin selection, a reduced form of population genetics that he used to solve the age-old problem of the bee hive—How could bee hives work, when all those worker bees are sterile? So Hamilton pioneered this alternative genetics based on relatedness of the worker bees to the queen, which was also widely applied to other animal behavior, in a revolution in the field, and in sociobiology. The Oxford zoology department was ground zero for the application of kin selection to animal behavior, and I talked to a number of biologists there.

More specifically, I had been studying a dispute in genetics between those who thought that Hamilton's kin selection stood alone, and those who thought that kin selection was a version or type of group selection, which, as the name says, characterizes groups as a level on which selection acts. Every biologist I had spoken with at the Oxford department was against group selection and declared that kin selection was viable but group selection was *not*, and that they were *totally different things*. I did not agree; I thought that kin selection was a type of group selection, only it used kin groups, as I had just written in my new 1988 book.[4]

And I had scheduled a tea date with Bill Hamilton to ask him a crucial question about this, and had come loaded for bear. When we finally got a chance to sit down with our milky tea, I asked him the key question: Do you think kin selection is a type of group selection? I completely expected him to say "no," as I had heard all the other zoologists in the Oxford department of zoology tell me on my visit, who claimed to be following Hamilton. So I was ready to pounce on Hamilton when he said "no." But he said, "of course. I just came back from India, where we were studying ants that are not related, in their huge nests. Kin selection is *definitely* a form of group selection." I deflated like the blowup snowmen you see on people's lawns. I couldn't believe it! There went all my arguments. So we went on to talk about these new ants, which fascinated Hamilton. And he invited me up to his office and dug out a reprint of a 1975 paper—a decade and a half old, but neglected, alas—in which he had written that kin selection was a version of group selection. (And I thought, you should let the guys downstairs know about this!!) And I gave him a copy of my new book, *The Structure and Confirmation of Evolutionary Theory*.

Then I was shocked the next morning at eleven o'clock in the tea room, when Bill Hamilton approached me, and said, "I stayed up all night reading your book." And we then had a very nice chat about it, and he invited me to lunch at New College, and this led to many more nice chats over the years. So it came to pass that I was watching Bill give a talk with some very colorful slides five years later, and I asked him if I might use one of the slides for the paperback cover of the reprinting of my first book for Princeton University Press. He then airmailed me more than a dozen and a half slides from the talk (about selection in host/parasite systems), accompanied by a letter in which he offered two separate detailed and ranked lists with comments. One was a ranking according to the aesthetic value of the slides, and the other was a list according to their theoretical interest. I'm not telling you which I used on the cover, which I brought in for show and tell, but I will reveal that it is an inside joke.

But let's go back and think about all those visits to the Harvard biologists in the 1980s and 1990s. I would stay with different, generous friends, having spent all my money on the plane fare from California. I don't remember exactly how this happened, but for several years, while they still had the big townhouse on Beacon Hill, Tom Kuhn (Thomas S. Kuhn) and his wife Jehanne invited me to stay with them when I came to study at Harvard, and I accepted. It was fantastic. They lived up a steep Beacon Hill street from the subway line, called the T station—I would take the T into Harvard square and trudge over to Lewontin's lab at the Museum of Comparative Zoology and back. Dick always would make a desk available to me when I came to town, an incredibly generous move that I didn't appreciate enough at the time. And after a day of picking every head that I could get a hold of, and having one or more intense sessions with Dick on some puzzle that I had, I would head back to the T station and trudge up that steep hill in time for dinner at Tom and Jehanne Kuhn's house. The thing is, they were gourmet cooks, in the Italian style, and would shop for fresh ingredients every day, fresh bread, fresh vegetables, fresh meat. And then would cook some subtle, Italian, yummy dish that would take at least an hour to make. I remember the fresh red pepper puree very vividly, and the fresh Italian sausage. Incredible care went into these meals, and into the choices of wine, which was liberally applied.

So at the end of dinner, Tom would carry my wine glass up to his study, sit me down on the sofa, and the session would begin. Many of you might know that at the end of his famous book, *The Structure of Scientific Revolutions*, Kuhn sketches an evolutionary picture of the growth of scientific knowledge and scientific change. In his later life, he pursued that picture. He would ask me questions about biology, how evolution works, how natural selection works, did I think this, did I think that, how did natural selection compare to the growth of scientific knowledge, what were the subtleties of natural selection, and so on, and on, while I was drinking more wine, and subsiding into the couch. Tom had a strong, even booming, voice, which was good for keeping you awake, but eventually, not even that worked, and I would just subside. He would give up for the evening, and allow me to go to bed, all to be renewed for the next round tomorrow. So it was really an excellent learning environment: the cognitive scientists say now, that if you have to repeat or rephrase what you've just learned, and tell it to someone else, then that reinforces your learning. So it would go *in* at Lewontin's lab, and Mayr's office, and Gould's office, and then *out* at Tom Kuhn's house! But it was pure, glorious, fatigue! I was so fantastically lucky to have known them all.

I was also lucky enough to meet Helen Longino when I was moments out of graduate school. Well, actually, a year out, at a conference at Hamilton College on feminist case studies of science. I had met Evelyn Fox Keller the previous year at the Philosophy of Science Association joint meetings with the History of Science Society, and she asked me whether I knew of any cases of sexism in science. At first I said no, but later I realized that I did know of one. I had only stumbled upon the case of the female orgasm because my friend at graduate school, a visiting professor at Princeton from Australia named Libby Prior, asked me, over a few peach daiquiris late one summer evening, "What is the evolutionary function of female orgasm?" I told her I didn't know, but naively said that I would go look it up in the library. I did that, assuming that I would find the adaptive function of female orgasm, and got a big surprise! Not only were there several authors writing on the subject, who disagreed with one another—so, no simple answer—but nearly all the accounts that I read were totally incompatible with what I already knew about the sexology evidence! And in a male-centered or sexist way, too. They simply assumed that women responded the same way to sexual intercourse that men do, namely, with easily attained orgasm. But as many of you know—can I say most??—this isn't a fair account of female sexuality. Only a small minority of women reliably attain orgasm with sexual intercourse—most women attain it only sometimes, and a full third of women either rarely or never have orgasm with intercourse. So all these theories that said that orgasm was women's *reward* for having frequent intercourse with the men, were contradicted by the sex evidence, but vast multitudes of biologists and anthropologists believed—and taught—these accounts nonetheless. This is male-centered science *par excellence*. So I told Evelyn Keller that I *did* know of an example of sex bias or sexism in science. So she got me invited to this small conference about feminist science critique, where I met a bunch of really neat women, including Helen Longino, whose analysis I really liked, and I proceeded to pester her throughout her career.

A few years later, Alison Wylie, whom I had never met before, dropped into my office one day at the philosophy department at Berkeley, asking about what I was working on. We got into an intense discussion of androcentric bias in science, and she introduced me to two other Berkeley faculty members in the anthropology department who had worked with Alison as pioneers in feminist archaeology. They posed the challenge to their fellow archaeologists: reanalyze your data using sex or gender as a category. The results were collected in a series of essays published in a co-edited book with some stunning news for the field. Specifically, gender was an important human category when examining archaeological data, and sometimes changed the entire interpretation of the fundamentals of human cultures, including division of labor, diets, where who lived, what they did for a living, what they ate, and so on. This really influenced my thinking about sex bias and science. I knew that my little case where I found such sex bias in evolutionary accounts of female orgasm was a real eye-opener, but these cases where taking sex and gender into account changed the entire outlook on a civilization really knocked my socks off. Steve Gould had already published his piece about my research, but I had not published my own article yet. Talking with Alison about how to pose the argument, and what exact arguments to make, along with thinking about Helen Longino's analysis of objectivity and standards in science, basically determined my approach to these questions. When I was invited to speak at a symposium on gender and science at the Pacific APA meeting in 1992, I took the opportunity to give a talk about this and related research, and turned to both Helen Longino and Alison Wylie for aid in constructing that talk, and later with my book.

Another very strong inspiration to me, ever since I was a graduate student, and up through the 2010 Philosophy of Science Association meeting, where she gave a truly magnificent presidential lecture, is Nancy Cartwright. I must confess that as a graduate student I was extremely intimidated by her, and for several years later, as well. I remember attending a talk of hers and being amazed that she would actually have the boldness to give a talk discussing the various ways that *she* had tackled a problem. I only really got over that, and we became very close, after we jointly initiated the Bay Area Philosophy of Science Study Group, or BAPS, when I moved to Berkeley. Our group alternated meeting at my house outside Berkeley and hers outside Palo Alto, and faculty and grad students from all around the bay area, from Santa Cruz to Davis and San Francisco, participated in our group. We had the pleasure of including many students who would later go on to great success, including Eric Schwitzgebel, now at University of California at Riverside, Jordi Cat, now at Indiana's history and philosophy of science department, and Hasok Chang, now heading the history and philosophy of science department at Cambridge. I often stayed overnight at Nancy's house outside Stanford after these late meetings, and would wake up and enjoy cooking eggs for Sir Stuart Hampshire, Nancy's husband, in the mornings. He was a dry and brilliant wit even first thing in the morning. What a treat!

You know, I lived in California for thirteen years, first teaching for four years at the University of California at San Diego, and then for nine years at Berkeley. Perhaps you know that Californians have mastered the art of living "house poor," or spending nearly all of their money on their house, so they don't have money to spend on anything else, like normal things that people in, say, the midwest, would buy. This was brought home forcefully to me one day teaching at Berkeley. I had been teaching a course on science and society, a big class with one hundred twenty students, only a few of whom spoke up in class, but some of them would occasionally come up to talk with me after class. There was one particular older student, a well-dressed woman who was very smart and sometimes asked really good questions, but was generally quiet. One day, she brought up a newspaper and showed me an article in it on a topic we had just discussed, and we chatted, and she offered for me to keep the paper that she had delivered. I thought to myself, "Wow, a student who can afford to have the *New York Times* delivered to her house!" At the end of the semester she came to my office and asked me if I would please come to her house for lunch, to chat with her husband, whom she thought would be interested in my thoughts. Turns out she had registered as a student under a false name. Her real name is Ann Getty, wife of billionaire Gordon Getty. And I was impressed with the paper delivery! I had the honor of being Gordon's dinner partner at his sixtieth birthday bash, a black tie party chock full of stars, political, academic, sports, and the movie kind as well. Wearing a black velvet hat with a short veil, I was doing just fine with Peter Coyote until Gordon barged in and told him that I was a philosophy professor at Berkeley, at which point he vanished in a puff of smoke.

I couldn't possibly have imagined the intellectual or social adventure I've been on since embarking on my philosophical career. Fortunately, Gary Stahl and my beloved late dad did get to see some of this take place. I did get to talk to my daddy after he had gone to the store to buy extra copies of the *New York Times* the day my work on orgasm was all over the front page of the Science Section, which led to being flown to New York and a turn on television with Barbara Walters and the *View*. I'll tell you something—she's one very smart woman, and the only one on that set who grasped the key points of the book. That whole scene led to a reference on Saturday Night Live, and later to other comedy and cartoon strips—Tom Tomorrow, Cracked, etc.

But on a much more serious note, I followed my own methodological research and recommendations from my 2005 book, which led to my own empirical, scientific work with a top neurophysiologist, Kim Wallen (Emory), which has been published in some leading scientific journals. Last year, some of it was written up—and was later the editor's pick—on the front page of CNN's website. The reason is that we have made a key discovery that may unlock the anatomical secret of why some women have orgasm with intercourse and most do not—it's the distance between the clitoris and the [surrogate measurement for the] vaginal opening that is tightly *correlated* with whether a woman *has orgasm with intercourse or not*. This genital distance is so closely correlated with orgasmic likelihood with (unassisted, no-hands) intercourse that we can actually *predict* whether a women will have orgasm with intercourse or not, based on this single anatomical measure.[5] Because orgasmic capacity has been a topic of much speculation—e.g., about women's responsibilities for orgasmic achievement (i.e., the woman is not "relaxed" enough, the woman's psychology is "too immature," the woman's psychological traits are not appropriately lined up for orgasm in a variety of ways, she is "too uptight," "too religious," etc., etc.), it is particularly significant that the apparent responsibility for orgasmic capacity seems to be largely anatomical. It is neither the woman's nor the man's "fault" that orgasm is not usually occurring with intercourse; it appears to be due to primarily the anatomy of the woman's genitals.[6] This is a discovery with significant social consequences. We have been approached by another research team and are currently pursuing further confirmation of the effect in a larger population using MRI measurements of the genitals. Who could have imagined any of it, when I was told there was no such field as philosophy of biology? I'm sure that now there is no such field as "philosophically guided empirical sex research"! But that's one of the things I do now!

And now I actually have a third career—and I was *tricked* into this other one, *too*, a matter of months after I published the orgasm book.

I played in a rock and roll band when I was an undergraduate. I played electric lead and slide guitar, well enough to have a few gigs, and so on. And it was an all-female band, too, a rarity in 1978. And by 2005, one of my band members from long ago in Boulder had been nagging me for several years that she wanted me to meet a friend of hers who was in the sciences, thinking we had much in common. So I finally met this friend of my bandmate, who turns out to be a fan of my philosophy work, including my old book on the structure of evolutionary theory, having gotten her undergrad degree in philosophy, and now she asks me, Would you please help us explain to congress the evidence in favor of the climate models and of climate change? I say, "I know nothing *about* climate change or climate models," and she says "come down to my office tomorrow and I'll show you a few things." So I go, and I see some amazing graphs, and data, and models, and she sets up a lunch with Tom Wigley, a chief consultant for Al Gore's movie, "An Inconvenient Truth," and trust me, this guy can be persuasive. I had investigated the evidence concerning *evolutionary* models in biology, and here were *climate* models that were being challenged, so I was *seduced* into taking on this project of investigating the evidence for and against the climate models.

I organized the first symposium on the philosophy of science of climate models at the Philosophy of Science Association meeting in 2008, and have been co-organizing with two climate scientists a session at the American Geophysical Union, which combines philosophers and climate scientists discussing how climate models are evaluated, confirmed, and interpreted, for the past four years (with another one to come in 2013), with hundreds of climate scientists in attendance every year at our session, that has hopefully built some new bridges.

And now I am co-editing, with philosopher of science and computer simulation Eric Winsberg, a sizable cutting-edge collection on the philosophical and conceptual issues in climate modeling. I'm having so much fun with this. I have Michael Mann, the lead author of the original so-called "hockey stick" paper—in which the temperatures are more or less stable, a bit up and down, for a thousand years, and then go steeply up since 1860, forming the blade of the hockey stick—writing an original, fresh new piece for our book, and some of the other top climate scientists are writing for it, too. It will be so stimulating, in this new field. Just my type of thing! And I just found out that one of my new articles on climate science methodology and philosophy, co-authored with a climate person, Vanessa J. Schweizer, is being cited in the *Fifth IPCC Report*, *AR5*, the big international climate report from the United Nations, which will come out in 2014.

I got a divorce from philosophy in 1974, when I first concluded that it was not for me. But Gary Stahl, the sly Kantian, and some political philosophers, and an intellectual historian, all convinced me that I might have something to do in the field, and when I got the NSF scholarship, I thought, here I go! However hard grad school turned out to be, I have been so extremely fortunate to have the very *best* of mentors and teachers, the very *best* of friends and opponents. And I can say now that philosophy didn't turn out to be anything *like* what I thought it was!

NOTES

1. Beatty, "What's Wrong with the Received View of Evolutionary Theory?," 397.
2. Lloyd, Wilson and Sober, "Evolutionary Mismatch," ms.
3. Lloyd, *The Case of the Female Orgasm*, chapter six.
4. Lloyd, *Structure*.
5. Wallen and Lloyd, "Sexual Arousal in Women."
6. Ibid.

BIBLIOGRAPHY

Beatty, John. "What's Wrong with the Received View of Evolutionary Theory?" PSA: *Proceedings of the Biennial Meeting of the PSA* 2 (1980): 397–426.

Lloyd, Elisabeth. *The Structure and Confirmation of Evolutionary Theory*. Greenwood Press, 1988; paperback edition, with new preface, Princeton University Press, 1994.

Lloyd, Elisabeth. *The Case of the Female Orgasm: Bias in the Science of Evolution*. Harvard University Press, 2005. Italian trans., 2006. (Vern and Bonnie Bullough Prize, Society for the Scientific Study of Sexuality (SSSS): "Best Book on Sexuality," 2006; Philosophy of Science Association Women's Caucus Prize, 2010.)

Lloyd, Elisabeth, David Sloan Wilson, and Elliott Sober. "Evolutionary Mismatch and What to Do About It: A Basic Tutorial," ms. in final draft, to submit to *Evolutionary Applications* (waiting for other articles for issue).

Wallen, Kim, and Elisabeth A. Lloyd. "Sexual Arousal in Women: Genital Anatomy and Orgasm in Intercourse." *Hormones and Behavior* 59, no. 5 (2011): 780–92.