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LAB REPORT

Lessons from a Multi-Year Collaboration between Nanoscience and Philosophy of Science

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In this chapter, I describe a successful ongoing collaboration between Dr. Jill Millstone, a nanochemist, and myself, a philosopher of science. Jill[1] runs a laboratory that creates new architectures of noble-metal nanomaterials and researches their fundamental properties and characteristics. I am involved in a research program focusing on the role of scale in material behavior and the varieties of inter-theory relations in the physical sciences. Our collaboration began in 2011 at the University of Pittsburgh, where Jill was then an Assistant Professor of Chemistry and I was a graduate student in the Department of History and Philosophy of Science. It has continued through Jill's promotion to Associate Professor and my graduation and first years on the tenure track. Our collaboration has taken many forms, beginning with me, as a student, coming to Jill's office hours and encompassing my participation in her weekly lab meetings, Jill's service on my dissertation committee, our embarking on experimental joint outreach projects, and our co-authorship of essays for both scientific and philosophical audiences.

My aim in documenting this collaboration is not to present an instance of qualitative research on the phenomenon of collaboration nor a philosophical argument for collaboration as a preferred methodology in philosophy of science. Rather, what follows is a personal narrative of my collaboration with a chemistry laboratory as a graduate student in the history and philosophy of science—a discussion of how this unusual undertaking has informed my research career and of the set of lessons I have carried forward into other collaborations. My hope is that highlighting both the successes and failures of this collaboration will provide insight for other philosophers of science aiming to begin and sustain collaborations with scientists, and perhaps also for scientists aiming to collaborate with philosophers. Additionally, I hope that by presenting an account of a collaboration between laboratory scientists and a philosopher of science, I can introduce a complementary narrative to projects that have embedded sociologists, anthropologists, historians, and other researchers in science and technology studies (STS) in laboratories. The targets of the study and research methods I employed are distinct from those of laboratory-embedded humanists from other disciplines, such as Erik Fisher, Bruno Latour and Steve Woolgar, and Sharon Traweek.

Originating the Collaboration

I met Jill when I was finishing my Ph.D. coursework in philosophy of science and was beginning to focus on the set of research questions that would comprise my dissertation. My main research interest was in the philosophy of chemistry, and my chemistry classes had led me to a series of questions about how chemists make sense of electronic behavior in metallic compounds too complex to be described by quantum mechanics. This domain posed some interesting challenges for philosophical theories of modeling, and, moreover, I just liked thinking about metals.[2]

My graduate program and my dissertation director, who was not in my department, were both supportive of students taking classes in science departments after basic in-house course requirements were met. This support was extremely helpful in developing my collaboration, as it allowed me to take the classes that led me to Jill's classroom. So here is a quick first piece of advice: mentors should encourage graduate students to take classes outside their home departments when students have specific interests that cannot be accommodated within a department. At many institutions, administrative barriers can interfere with sharing students across departments, so the support of mentors in encouraging student interests and assisting with registration hurdles can change the course of research careers. It certainly did mine.

While studying the Fall 2011 course catalog, I came across a class titled "Atoms, Molecules, and Materials," which focused on nanomaterials—a class of materials that encounter modeling challenges similar to the ones that excited me about the metallic compounds I had studied. I could not register without permission from the instructor, so I sent a request to register along with some basic background information about my experience and interests.

I received a very brief reply asking for a meeting. As a graduate student with little experience of communicating with professors outside my discipline, I read the brevity of the email as curtness. I know now that different disciplines have different norms for electronic communication, and I even discuss expectations for email communication when I'm setting up new collaborations. Here is a bit of highly anecdotal reporting: natural scientists rarely write emails longer than about a paragraph, and they find the multiple-long-paragraph structure of many humanists' emails alienating. Some people tend to use bold, italic, and bullet points to highlight information, while others find it pedantic; these preferences have vague overlaps with disciplinary boundaries between the social and natural sciences. Pleasantries are optional and generally seen as a distraction, especially for faculty on the tenure track. Emails are more likely to get responses when they close with a direct request, either for information or a meeting, especially if the request comes with a (reasonable) deadline.

I was nervous about meeting Jill. I wanted to leave a good impression of myself and of philosophy of science. I consulted with colleagues about how to prepare, and I read her webpage and a few of her papers. Despite my best efforts, the meeting was awkward. It was scheduled in a conference room in a building I did not know well, with someone I had never met, and I was not prepared for how much that novelty would affect my composure. I stumbled and mumbled, started more sentences than I finished, and occasionally talked over Jill rudely.

I know now that there is a lot about initiating a collaboration that is inherently awkward, and I have learned to embrace the awkwardness, but at the time I was sure I had failed some kind of test. Jill was clearly very busy, did not want her time wasted, and was not at all sure whether talking to a philosopher of science would be a good use of her time. In order to try to convince her that I wasn't wasting her time, I explained my background in chemistry and stumbled through a muddy introduction to philosophy of science. It ended up working, less because of me than because of Jill: she had a background in English as well as chemistry and took an interest in some of the technical vocabulary in philosophy of science. Our discussion of the word "epistemology" sold her on letting me into her course.

Advice for Initiating Collaborations

While our conversation ended up convincing Jill to let me into her class, other cues suggested to her that I was a viable potential discussant. For instance, my dissertation research was supported by a fellowship from the National Science Foundation, which indicated that I had a working understanding of scientific content and that we shared institutional infrastructure. Credentials such as funding from scientific agencies or authorship of scientific talks or articles can signal to a potential collaborator that you are a member of the same epistemic community, as can doing your homework on their research and professional profile before you meet. Scientists, like many Humanists, find common ground with each other over shared subdisciplines, recognition of journal names and affiliations, attendance at the same conferences, and support from the same funding agencies. By learning about and engaging with these institutions, you can indicate your merit as a collaborator.

In a similar vein, in my many experiences explaining my research to scientists and initiating collaborations since my initial meeting with Jill, I have developed an elevator-pitch overview of what philosophy of science is, with the intention of demonstrating its value to scientific research. This cuts off the most common misconceptions of my work, namely, that I am an ethicist, that I am anti-science, or that my research employs the Continental tradition in philosophy, as many STS researchers' work does. My spiel usually starts with the explanation that philosophy of science is about how scientific knowledge works: what allows researchers to trust the results of an experiment, how different scientific theories interact, what is considered the ultimate goal of scientific research. I find it works best to give a playful example, such as pointing out that if the goal of science is to say how the world really is, it would not be unreasonable to have a scientific discipline devoted to counting the number of blades of grass on my front lawn.

While my collaboration with Jill began within a student–teacher dynamic, most of the collaborations I have built with scientists have begun between colleagues. These typically start with a brief, in-person exchange at a meeting or event where faculty from multiple departments have gathered, or, less frequently, with a targeted email. In my current position, science departments have approached me about sharing my research in their weekly seminars, partly as an inexpensive way of filling their talk schedule. These have led to collaborative discussions, grant proposals, and shared mentoring of students.

The first of these talks took place because I met a materials engineer at a university event. We shared research interests and arranged a one-on-one meeting to discuss research, whereupon he invited me to speak at his department's seminar. In subsequent meetings with scientists across campus, I was then able to let them know I had a seminar prepared, which has led to a few more talks, many more meetings, the development of a grant proposal, and a few extra science students in my classes. In giving talks to scientific audiences, I typically take a cue from science talks I've attended and, rather than sustaining a philosophical argument through 45 minutes, spend my time describing two or three of my research projects and explaining why these research results are relevant to their work. I also always use slides, and I try to make the slides more visual than they would be for a conference talk in philosophy.

I initiated most, but not all, of my present collaborations. I prepare for a first meeting by setting aside time to learn about my potential collaborator's research. I find specific points of contact between their research interests and mine that will help me to explain my research in terms they both understand and care about. I try to familiarize myself with as many pieces of jargon ahead of time as I can, so that I don't wind up slowing down the conversation and making myself look ignorant by asking for vocabulary clarifications. In initiating collaborations, it is often more important to convey that you know what your potential collaborator is talking about than it is to convey that you know what you are talking about.

For instance, I recently met with a traffic engineer who is interested in whether philosophy of science can help his department to prepare for the societal changes that will accompany the coming self-driving vehicle revolution. My research is well outside this area, and I don't expect to get closer to it any time soon. But I find the subject interesting and, in general, I make a habit of taking meetings with new potential collaborators, even if I am not sure what will come of it. In prepping for the meeting, I learned that in his world "ITS" stands for "intelligent transportation system," which encompasses everything from self-driving cars and subways that send text alerts about schedule changes to automated trucking weigh stations for trucks on the highways. I did not do any preparation to determine what is out there in philosophy of science on the specifics of the particular kind of futurist questions he had. We ended up talking for more than an hour about the potential for problems such as socioeconomic stratification and job loss associated with automated transportation, as well as benefits like empowerment and improved mobility for the presently immobile. Throughout, I was able to draw on my knowledge of value-laden science, intersectionality, and the history of unethical scientific enterprises in order to contribute to the conversation. As a result of our conversation, he agreed to give an address at an upcoming conference on socially engaged philosophy of science, as a way of advertising the set of problems he is concerned with to a wider philosophical audience.

Together, these anecdotes suggest a need, in the initiation of collaborations, for a type of flexibility that is sometimes uncharacteristic of philosophers. To borrow from a source thoroughly outside the analytic tradition, it is useful to think of these strategies as a way of attaining the Zen principle of "beginner's mind" by taking oneself out of the typical patterns of expectation and inferential paths common within the discipline. I prefer this analogy to the economic metaphors of trading zones and exchanges of ideas that are usually associated with interdisciplinary research.

Collaboration as a Student

Returning to the collaboration with Jill, recall that it began in her classroom. It was an upper-level undergraduate course, but I put more time into it than I did most of my graduate classes that term. The class was deeply interesting; eventually, the subject matter would become the central scientific focus of my research. During class, I got to know Jill better by participating actively and attending office hours, both to ask clarification questions and to test out various epistemological inquiries I had about nanomaterials. I'd tried this technique with other professors of previous science classes with little success. While most of them thought the questions I had were thought-provoking, my queries did not ultimately impact their research, and so were not a productive way for them to spend their time.

For instance, before taking Jill's class, I had taken a class with another chemist who worked on nanoscience, and I had asked him conceptual questions about the materials we studied. He was kind and engaged during his responses, but my questions never excited his curiosity. As an educator, he was invested in helping me learn, so he would think through a conceptual question and try to provide an answer, or indicate what factors would affect his answer, but he never took the questions and ran with them.

There are two lessons here. First, there will be more unsuccessful attempts at collaboration than successful ones. That is expected. Second, if you can find questions that are impactful to scientists, they are more likely to spend time thinking about them with you. Luckily, questions that are impactful to scientists are, in general, better questions to ask in philosophy of science anyway, so this strategy is effective—if you can figure out how to find impactful questions.

Unlike my previous attempts, asking Jill conceptual questions about nanoscience turned out to be fascinating and productive for both of us. Some of this has to do with the nature of nanoscience, some with Jill, and some with me. Nanoscience is a young and developing discipline in which the ways of conceptualizing various material properties and behaviors are not yet deeply entrenched in the scientific community. So when I asked whether individual nanoparticles are molecules or not, the question did not have a clear answer—and the absence of a clear answer was interesting both scientifically and philosophically. It led Jill to conceptualize nanomaterials as occupying a neither-fish-nor-fowl space between molecules and crystals, which explained why tools from both molecular and crystal theory could be used to predict certain nanoscale material behaviors.

This exchange marked the first stage of our collaboration. Even while I was interacting with Jill as an instructor, she saw my research questions as valuable and interesting, and she wanted to work with me to solve them, rather than simply seeing them as someone else's interests. With the other professor, the questions were always mine, and once he had provided as much information as he could, he dropped them. Jill held on to them, and in doing so gave me my first taste of what it was like to work with a scientist, rather than just read and write about science. It was thrilling, and it motivated me to think more deeply about the problems the questions were raising. This early interaction became the inspiration for my research career: I had found a subject area that sparked my curiosity, and a set of questions that an actual scientist cared about and wanted to solve with me. Jill's participation stoked the flames of my dissertation.

Residency in Jill's Lab

At the end of the semester, I met with Jill again to ask how I could continue to work with her. Coming from a fairly traditional philosophical background in terms of research-relationship structures, the only way I had interacted with professors outside the classroom was in one-on-one meetings, in reading groups, and at talks and conferences. Coming from a fairly traditional scientific background, Jill's go-to answer was to have me come sit in on research meetings with her lab, a group comprised of 2–4 undergraduates, 4–8 graduate students, and 1–2 postdoctoral students, which met weekly to discuss progress on the lab's various research projects and hear an extended presentation from one member. Presentations were assigned on a rotating basis, and Jill added me to the rotation. It would never have occurred to me that this would be a way of engaging with scientists, and it quickly became a formative experience.

There is no generalizable lesson here for my role in this development other than that sometimes you can get really lucky, but it is worth mentioning that if you can get yourself embedded in a research lab, you should. In addition to attending weekly meetings, I was invited to open house events and added to the lab calendar and email list. I learned who worked where in the lab and came to know what the various instruments did. I was eventually given the keycode to work in the offices. After a while, I was listed on the lab website as their "Resident Philosopher."

Participating in the life of the lab changed the way I thought about the reasoning processes behind scientific research. It allowed me to witness the inherently collaborative nature of scientific research in a way that no amount of reading about the social construction of scientific knowledge, or reading published scientific papers, could. It gave me immediate access to expert assistance in understanding the details of experimental setups and characterization techniques, significantly decreasing the amount of time it took me to get up to speed on the mechanics of a piece of theory or experiment. Most importantly, though, it let me see how much science doesn't get published: not only the failed experiments, but the figures that are painstakingly drawn and then discarded when they don't land with the lab audience, the follow-up trials to confirm or disconfirm a suspicion about a particular synthetic pathway, the spirited debates about what theoretical model best captures and explains an observed pattern, and even the semantic questions about how to name a new nanoscale architecture. These are all topics that are the subjects of papers and research programs in STS disciplines, but no amount of reading even detailed descriptions of the activity behind a publication can substitute for witnessing and participating in it. My research is not about the activity of laboratory life; it is about scientific reasoning. However, I could not write about scientific reasoning the way I do without having spent years witnessing it in action.

My first few times at lab meetings I acted as a non-participant observer, taking careful notes not only about the content of the presentations but about the way members of the lab interacted and how they approached their research questions. This was not a fruitful approach, for two reasons. First, I did not have the social science background to enact this observation in a systematic or insightful way and, second, I did not have the chemical background to follow many of the discussions in the lab meetings.

Realizing this, I changed tactics and, like the philosophers of old, started asking questions. I asked clarification questions about the mechanics of instruments and experimental protocols. I asked why a particular result led to the need for further experiment. I asked what parts of a diagram were to scale, and what parts were merely schematic. The collaborative, constructive atmosphere of the lab meetings boosted my confidence to ask varieties of questions that I could not have had answered by the publication record, and the ability to seek answers to these questions generated unique insights into the nature of scientific reasoning.

Most of my questions centered on the assumptions and inferences behind a particular piece of scientific reasoning, or about why a researcher was thinking about a problem in a particular way. These became known in the lab as "Julia questions," and other members of the group started asking them as well. These kinds of questions became a hallmark of my collaboration with the lab. They led to a number of the research projects in my dissertation, as well as to short essays in scientific journals and refinements in experimental protocols. I still recall fondly the day about three years into the collaboration when, during a lab meeting, one student asked another a question about how to understand part of a diagram. I couldn't help but feel proud when the student's response began, "That seems like an epistemological issue."

While it was never our primary intent, these questions occasionally contributed to experimental design. The biggest tangible contribution I made to the advance of a particular experiment came from a relatively innocuous question about how the experimenter was thinking about the material he was trying to make. A graduate student was building an experiment to test some of the mechanical properties of silver nanorods, that is, how they respond to stresses and strains. He had developed a complex protocol to enact the test, and during a lab meeting presentation, he reported some difficulty in determining the force needed to bend a rod. While other members of the lab were asking questions and offering suggestions about changing the protocol, I asked about how the student was modeling the mechanical forces in the experiment: what theories or material parameters he was relying on in order to determine the threshold forces that he needed to get out of the chemical interactions between the coatings. In particular, I was interested in what I saw as a mismatch between two pieces of the experiment. The student was drawing from two competing theories of matter—continuum and molecular mechanics—to develop the protocol, and it turned out that this was affecting his ability to construct a model for measuring the bend of the rod.

The effectiveness of continuum mechanics is a particularly thorny problem for philosophers of the physical sciences, and the problem had never seemed so vivid as it did here in the middle of a lab meeting, when a totally new material was being developed and modeled by a continuum mechanical model—and it wasn't behaving the way the theories said it should. This problem became a preoccupation of my research, even as it changed the direction of the experiment. The question I posed to the student was about whether continuum mechanics even applied to the nanorods in this experiment. My concern was that, because continuum mechanics assumes uniform bulk behavior and ignores surface interactions, and because the nanorods' behavior was, like many nanomaterials, disproportionately influenced by the behavior of its surfaces, the theory would fail to describe the predominant behavior of the nanorods. This question reframed the entire experiment and forced a re-evaluation of the whole protocol. Here, it turned out, was philosophy of science having an immediate and tangible impact on a particular piece of research, above and beyond affecting the general tenor of discussions in the lab.

Like a lot of real science, too, the next chapter in the experiment's history was something other than a celebratory triumph that ended with a high-profile publication and a revolution in research: while the reformulation helped to advance the experiment, the protocol still did not produce a reliable bend in the rods that reached Jill's standards for publication. Additional external pressures affected the student's research activity and the experiment continues to lie "dormant," to use Jill's word, until the right student or the right funding or the right theoretical motivation arises to pursue the protocol further. In this respect, the situation is not so different from philosophy, when articles can sit in tucked-away folders for years, awaiting reduced teaching loads, the right publication venue, or the missing piece of an argument.

An important upshot of this story is that, because the experiment never made its way into the publication record, I would not have encountered it if the publication record were my only access to scientific research. This experiment has become something of a touchstone for me, because in it are three of the central tenets of my research: that the materially different role of surfaces in nanomaterials impacts how we characterize, understand, explain, and manipulate those materials; that scale plays an explanatory role in the properties and behaviors of nanomaterials; and that constructing theories in philosophy of science using primarily well-tested and successful pieces of science (described after the fact in the publication record) has led to a variety of oversights among philosophers about the nature of scientific reasoning. For present purposes, this story is evidence of the unique benefits conferred by a field philosophy approach.

Likewise, even though it never made it to publication, this experiment is an instance of philosophy of science materially impacting the course of scientific research. My question led the experimenter to change his plans for refining the experiment by revealing an avenue of investigation that the rest of the lab had not considered. Jill and I have talked about this incident a number of times and she believes—and I am inclined to agree—that the lab would likely have reached a similar place of revisiting the computations that led to the protocol's specifics even if I had not been in the room, but that it would not have originated from an epistemic concern about the exportation of information between atomic and continuum theories. Chemists use mismatched theories all the time because it works; telling the story of why it works is a job for philosophers. In this case, though, the assumption that it would work broke down because of the scale of the materials in the experiment, which generated puzzles for myself and the chemists that have since impacted the shape of both our research programs.

During my residency in Jill's lab, the nanomechanics experiment was the most poignant moment of philosophical questions impacting both scientific and philosophical research. However, plenty of other questions, both philosophical and scientific, impacted both our research programs from that time. I was frequently surprised which of my questions were interesting to the lab members. This feedback had a significant impact on the kind of researcher I became and the kinds of philosophical problems I wanted to answer. It also shaped the way I think about what the relationships between philosophy and science, and between philosophers of science and scientists, should be.

Varieties of Collaboration

During the three years I spent in Jill's lab, we experimented with other modes of collaboration beyond my weekly participation in lab meetings. Some worked very well, resulting in new research activities or insightful conversations, or providing other benefits to one or both of our careers. Like the experiments in the lab, though, plenty of our experiments in collaboration did not bear fruit.

It worked well when I used my presentation time in the meetings to give overviews of a particular domain of history or philosophy of science that was relevant to the lab's research—for example, an overview of the arc of research in chemical bonding in inorganic materials from the early days of the quantum theory of chemical bonds to the present, or an overview of the realism debate, or of inter-theory relations in the physical sciences. It worked extremely well when Jill joined my dissertation committee as my external reader, and she became one of my primary mentors in the dissertation. Her expertise assured my philosophical readers that I was representing the science accurately, and her curiosity about conceptual questions often inspired new directions in my research. And it worked well when I worked with a couple of her graduate students to improve the broader impacts narratives in their grant applications, advising them on how to "zoom out" and think about the potential impacts of their research on human lives outside the lab, as well as helping them to outline their narratives.

It did not work well when I developed a qualitative-analysis style survey to determine the role of hypotheses in the experiments done in the lab. Even though I recruited social scientist colleagues to help me design and analyze the survey, the results did not tell me anything I could use in my research because I did not have the training to translate the data into elements of the kind of argument I knew how to make. One lesson I learned over and over was that contributing to the lab as a philosopher was distinct from contributing as an STS researcher. That said, it also did not work particularly well when I presented on my own research in lab meetings as I would to an audience of philosophers, since in those presentations I devote a lot of time to explaining the science, and the science was familiar to the lab. Finally, it did not work when Jill and I tried to build a wiki together to share information about experimental protocols such as the one described above. We had a hope that we could improve accessibility for procedural information that did not make it into the publication record, thereby broadening and diversifying the community of researchers able to perform experiments in nanoscience. The idea was a good one, we both still believe, but there were simply too many barriers to getting it off the ground, since neither of us were accomplished programmers or wiki editors.

There is a pattern behind these successes and failures. When things went badly, it was usually because one of us, either myself or the scientists, was trying to be something other than what we came into the collaboration to be: me, a philosopher, and Jill and the lab, nanochemists. The times I tried to employ methodology from the social sciences, I wound up with pages of scribbled observations or piles of survey data, neither of which I could transform into philosophical insight. The times Jill's lab tried to play the philosophical audience, their feedback was more about how I was introducing topics in nanoscience than about my philosophical arguments. When we got together and tried to be some combination of coders, textbook writers, and community organizers, even the best of intentions could not rescue our efforts from falling flat.

On the other hand, our successes came when we were true to our distinct disciplinary trainings and interests, and when we were able to recognize, through the lenses of those backgrounds, something of value to one or both parties being offered across the aisle. Philosophers of science are explicitly trained to seek out the epistemically and ontologically puzzling in pieces of scientific research, so I knew going in that Jill and her lab, by virtue of doing interesting science, had things to say that I wanted to hear. It was an interest in the philosophical puzzles of nanoscience that led me to her classroom in the first place, and I had no difficulty finding the import for my research in what the lab was doing.

For Jill and the chemists, though, being able to recognize the value of philosophy of science in general, and of my research in particular, was not something to which their backgrounds had predisposed them. As discussed above, Jill was naturally curious and collaborative about conceptual questions, so I was able to gain a foothold with her. For the rest of the lab, it was easier for them to see the value in "Julia questions" than to get interested in questions of reductionism and realism. While that fact would have surprised me in 2011, it seems obvious today, since the conceptual questions I asked about the lab's experiments did sometimes have direct bearing on how research was carried out—and even when they didn't, they were questions about a subject, namely, the lab's research projects, in which the other lab members were already invested.

Life as Colleagues: The Collaboration after Residency

The activities described so far took place during my residency in Jill's lab, which came to a close in the spring of 2015, once I left Pittsburgh. In the years that followed my departure, we have sustained a number of collaborative activities. I still regularly consult Jill about research, asking clarification questions about a new piece of science I'm studying or asking her opinion on an article or experiment. Jill and one of her graduate students participated in an interdisciplinary workshop I hosted in 2016. And Jill and I, along with that same graduate student, co-authored a short piece about conceptual analysis in nanoscience for a scientific audience (Bursten et al. 2016). We also developed an interview-style article for a collection of essays based on the 2016 workshop. We no longer share our weekly research progress at lab meetings, but I keep up with Jill's research more closely than with many of my philosophical colleagues, because it continues to be one of the biggest influences on my own research. We both expect to continue working together throughout our careers, and our collaboration has evolved from its genesis in Jill's classroom into a lasting colleagueship and friendship.

One of the challenges I face in this collaboration, and in a number of my other collaborations, is being in the uncomfortable position of being the one who benefits more from our work, and thus being the one who depends more on the continuation of the collaboration. There are certainly benefits for Jill of collaborating with me: above and beyond the conceptual insights that initially led her to work with me, having a philosopher in the lab boosted her interdisciplinary credentials and, as individuals, we are both useful partners for each other in talking through research and professional problems. However, if she had not worked with me, little would have changed about the direction of Jill's research, whereas my work with Jill has impacted nearly every step in my research career since I took up residency in her lab.

After leaving Pittsburgh, I sought collaborative residencies in other chemistry labs with little success. This disappointment could be due to a change in career stage: as a pre-tenure faculty member I don't have the time to go to weekly lab meetings, because their timing often conflicts with departmental duties. I'm also simply not done with the research that has come out of my collaboration with Jill's lab, so I feel less pressure to find a new residency. Additionally, the times I have met with chemists and other scientists since leaving Jill's lab have shown me just how rare it is to find someone as curious about conceptual questions as she is.

However, I have discovered and created other modes of collaboration in my current position. In addition to those discussed above, I have taken up a number of collaborations through pedagogical channels: I have served as an external committee member for Ph.D. students in biology and chemistry, developed an interdisciplinary course with members of the biology department at the University of Kentucky, and written about assignment design with a colleague in learning science. As a faculty member, too, I have more opportunities to interact with other departments around the university than I did as a graduate student, and my experiences collaborating with Jill have significantly improved my ability to communicate with my colleagues across campus.

As is the case with many successful interdisciplinary collaborations, the overall success of my collaboration with the Millstone lab was likely largely due to a host of particularities of personality, subject, and circumstance: the intellectual friendship between Jill and me; the interdisciplinarity and youth of nanoscience; the sheer timing of meeting a collaborator in graduate school, when my academic and personal lives allowed the time to go to extra meetings every week; and the good fortune of being in a graduate program that supported the collaboration. That said, I think there are some things that I, and that we, did well that generalize across collaborations between philosophers and scientists, and I have aimed to highlight these throughout this chapter.

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Notes

1 Dr. Millstone runs her lab on a first-name basis, and this chapter strives to convey the experience of working with her lab, so I will call her "Jill" throughout this piece.

2 One of the editors of this volume informed me this impulse makes me something of a geek, and I couldn't agree more. Having a keen and unflappable interest in chemistry has given me the patience and motivation I needed to learn the science in this collaboration.

Reference

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