Two Tales of Academic Collaboration

Edward F. Aboufadel

Grand Valley State University

Follow this and additional works at: http://scholarworks.gvsu.edu/gvr

Recommended Citation
Available at: http://scholarworks.gvsu.edu/gvr/vol18/iss1/5

This Article is brought to you for free and open access by ScholarWorks@GVSU. It has been accepted for inclusion in Grand Valley Review by an authorized administrator of ScholarWorks@GVSU. For more information, please contact scholarworks@gvsu.edu.
Since beginning my career as a professor six years ago, I have been attracted to experiences of academic collaboration. Lessons can be learned from two collaborations that I have been a part of, for in them we can see reasons why someone would want to get involved in this type of project, and also reasons why someone might prefer to “go it alone.” I view one of the collaborations as successful, while the other one was not, although a question to consider is how one measures whether or not a collaboration is successful.

There are a number of reasons why participating in a collaboration has been attractive to me. First and foremost, it affords me the opportunity to learn about an area of mathematics or mathematics education that I might not have expertise in. In some cases, a collaboration is an interdisciplinary affair, combining mathematics with another area such as economics, geology, or history. Also, there are social aspects of collaboration that I find appealing. For my taste,
too much of the work on my doctoral dissertation was done in isolation.

Although learning new ideas is important, I have found that as a young faculty member, an overriding goal of all of my professional activities has been to publish papers. After six years working in graduate school—in an environment whose primary value can be summarized as “Do Research! Get Published!”—it is not hard to see why publishing has been my principal goal. Reflecting on my collaborations, I often felt that the chief measure of whether or not the project was successful was if I was able to add something to my vita. I’m not proud of this attitude, particularly now that I am a little older, but it is something to be noticed in the two tales of collaboration that I will spin below.

Fool’s Gold

The beginnings of this collaboration can be found as I started graduate studies in mathematics at Rutgers University in August 1986. After earning a bachelor’s degree in mathematics from Michigan State University, I went on to graduate school with the idea of becoming a professor, but I felt a lot of anxiety about that choice. Looking back a decade, I now see what is dear: I had an equal if not greater interest in teaching collegiate mathematics than in high-powered research. After three semesters at Rutgers, I determined that I wanted to take at least one course in mathematics education. With the recommendation of a faculty member, I approached a professor, to whom I will give the pseudonym “Dr. Aurum,” about an independent study on the work of George Polya, a noted researcher in mathematics and mathematics education (in retrospect, a role model for me). Dr. Aurum agreed, and I enrolled for an independent study course for Spring semester 1988.

The course began with a study of Polya’s idea that we should strive to teach “insightful knowledge, [which] is a more ambitious
Two Tales of Academic Collaboration

am than mechanical knowledge” (Polya, 1984). Insightful knowledge, in this sense, can occur through what is known as “discovery learning,” which places analogy and plausible argument above two ideals that mathematicians strive for: precise definitions and rigorous demonstrations. The goal of this type of learning is summarized by Polya as follows: “A mathematician who has checked the details of a demonstration step by step, and has found each step in order, may still be dissatisfied. He needs something more to satisfy himself than the correctness of each detail. He wants to understand the demonstration” (Polya, 1954). Polya, in various writings, suggested methods that can be used to reach this sort of understanding.

About the middle of the semester, Dr. Aurum could sense that many of the ideas of Polya were not sinking in, and he suggested an exercise. Would it be possible to apply Polya’s methods to help me better understand some graduate level mathematics that I was finding difficult? There was a course in the area of algebraic topology where I could solve many of the problems, but had little idea of what it all meant. I went home that day and began working on two problems that I had already solved in that course, but this time I tried to be very aware of what I was doing and why I was doing it. At the end of the evening, I still didn’t understand.

The next time I met with Dr. Aurum, he interviewed me about my work. I would say things like, “This symbol here is called a chain,” and he would ask questions like, “Why is it called a chain?” and “If you were going to invent a mathematical object called a chain, what would you create?” As we conducted this conversation, I began to grasp the meaning behind the symbols. Technical mathematical terms such as cycle and boundary operator began to make sense. Suddenly, algebraic topology wasn’t so mysterious and intimidating. And neither was Polya. At the end of the hour, Dr. Aurum asked me that I write down a transcript of the meeting to the best of my ability, and I did so. This one wonderful interview with him taught me a lot about mathematics education.
Later that semester, Dr. Aurum suggested that our meeting could be the basis for a good paper in mathematics education on the topic of applying Polya’s idea to graduate level mathematics. At the time I was pleasantly shocked at the suggestion, but also wary, since I never had anything published before, and, of course, the purpose of being at Rutgers was to write a dissertation in mathematics and earn my Ph.D. As a result, I did not pursue his suggestion that year, but kept all my notes with the thought that I would return to it sometime in the future.

That time was four years later, as I had just finished my dissertation. By May 1992, I had my Ph.D. in hand and had accepted a position as an assistant professor at Southern Connecticut State University, in New Haven. With a free summer ahead of me, I went back to Dr. Aurum to see if he was still interested in writing a paper with me on that fateful conversation. (This wasn’t totally out of the blue, since I had kept in touch with him and also worked in his research center for a while after the independent study.) He was quite enthusiastic about the idea, and suggested that I write an initial draft that summer. I did, and we began our collaboration.

We met at the end of the summer, before I moved to New Haven, and Dr. Aurum had some excellent ideas for the paper. My first draft was basically the transcript that I had written four years before, and he suggested that we needed a significant introduction to explain the ideas of Polya. He promised to work on that. Also needed was some annotation of the transcript, since it gave the impression, at first reading, that we were not qualified to say anything professional about algebraic topology. We determined to continue working on the paper, and to communicate with each other by phone, and by e-mail.

Over the next two years, I learned a few hard lessons about academic collaboration. Communicating with Dr. Aurum turned out to be difficult. E-mails I sent would yield replies such as, “Going out of the country for six weeks. Will respond when I get back.”
Two Tales of Academic Collaboration

Dr. Aurum was doing a lot of traveling, and, it turned out, not a lot of writing. Between August 1992 and May 1994, we had only three significant conversations about the paper. In each case, we made some progress with it, and each of us promised to continue working on the writing, but in each case, I was doing my part, but Dr. Aurum was not doing his. At one point, I actually got a little rude with him, writing him an e-mail in December 1993 with the heading, "ENOUGH!" It did get his attention, because I soon received a phone call from him, and he sounded a bit wounded.

Nevertheless, two years seemed like an awful long time to write this manuscript.

Meanwhile, my attempts to publish my dissertation and prove new results had been fruitless. I was getting rather frustrated with the whole "Do Research! Get Published!" paradigm, and with Dr. Aurum. The final straw for the Polya paper came in May 1994, when an e-mail from Dr. Aurum indicated that he had misplaced his copy of the most recent version of "Polya and Algebraic Topology." He couldn't find it on his computer, either, so he requested that I send him a hard copy. He also mentioned that he was headed for Germany (again) soon. With my last ration of hope, I sent a copy of the article to an address in Germany that he gave me, along with a cover letter explaining what I thought still needed to be done. I wondered if he would write back that summer.

During this time, a few other interesting things were happening in my professional life. That May, I wrote a different paper about collegiate mathematics education that I sent to a journal called PRIMUS. Two weeks later, I received a letter saying that they wanted to publish that manuscript. The editor wrote that this was exactly the type of paper they were looking for. Also, I had been accepted to be a participant at two different NSF (National Science Foundation) conferences that summer. Besides giving me a broader view of academia, attending these conferences would lead, indirectly, to
three publications for me, which was very important to me at the time.

By December 1994, I had a few publications on my vita, and I was feeling much more confident about my professional life. Also, I decided to seek a new position at another school (which turned out to be GVSU). Finally, not having heard from Dr. Aurum since he left for Germany, I decided to forget about our paper. I didn't think it was worth expending any more energy. I had too many other things to do. Apparently, Dr. Aurum did, too. He never wrote back.

If you measure it in terms of “did I get a paper published?” then this first academic collaboration was a failure. A lesson to learn from all of this is to choose your collaborators carefully. I believe that participants in a project like this should have similar goals, whether it is to publish an article or just to learn something new, and they should also possess a similar level of commitment. Although I appreciate the ideas I came to understand better by writing my part of the paper—ideas that I use today when teaching mathematics—clearly my goal for this project was to advance myself professionally. Just as clearly, Dr. Aurum was successful enough already that he had other things on his mind.

**Simon Says**

This second story concerns one of the NSF conferences that I attended during summer 1994 at Rensselaer Polytechnic Institute (RPI) in Troy, New York. The conference focused on how to work with differential equations, in the classroom and in research, using a software program called Maple. Maple can do many mathematical tasks symbolically, rather than numerically (for example, on a calculator, \( \int \) is computed as 0.75, while Maple computes \( \int \)). I had just begun learning about this software at work and, since my dissertation's cover page...
Two Tales of Academic Collaboration

I attended graduate school at Rensselaer Polytechnic Institute (RPI). My dissertation was in differential equations, I was motivated to discover more.

The activity of the conference consisted of conversations, both one-on-one and in groups, on different topics, and a significant amount of time in a computer lab solving problems using Maple. Many of the conversations turned out to be effective therapy for some of worries about my career. One of the professors at RPI helped me clarify my thoughts about research, and it turned out that a number of faculty did not seem quite happy where they were working, either. Also, I discovered that I was more knowledgeable and progressive about collegiate mathematics education than I thought. I started to view my career a bit differently.

In the computer lab, our task, once we learned the basics of Maple, was to work in groups of two or three to develop a project that could be used in a differential equations course. I was partnered with Simon Tavener of Penn State University, a mathematician originally from England. Simon was acquainted with a friend of mine who was a graduate student at Penn State at that time. My fretting about research was matched (and, in some odd way, placated) by Simon's worrying about his upcoming tenure decision. Conversations with him made me begin to recognize that I would not be happy working at a big research institution.

During the week, the two of us developed a project called "The Leaky Tank." Normally in a differential equations course, students are exposed to what are called "mixing problems." For example, you have a tank partially filled with salty water, and there are two pipes attached to the tank. More water, with a different level of salt than the water in the tank initially, is pumped into the tank through one of the pipes, and the second pipe is attached to a drain in the tank where the salty water can flow out. Over time, the salinity of the water in the tank will change, and differential equations can be used to model this situation. The ideas in this problem can be ap-
plied to the study of pollution in the Great Lakes, or of carbon
dioxide in the atmosphere.

In our project, the tank in question had a leak—in effect a sec-
ond drain—and we wanted to determine the rate at which the salty
water was coming out of the leak. We imagined that the tank was
buried so that we could not directly measure this rate, but we could
measure the saltiness of the water coming into and going out of the
tank. The analysis of this situation led to some messy equations,
which, it turned out, Maple could handle much more quickly than
we could, which was the whole point. By the end of the week, we
had developed a nice project that was distributed to the rest of the
participants.

Early that autumn, I received a phone call from Simon, who
called to suggest that our project could be the basis for a good paper
in mathematics education on the topic of applying Maple to the
problem of leaky tanks. At the time I was pleasantly shocked at the
suggestion, but also wary, since the troubles with Dr. Aurum were
still in progress. I told Simon about Dr. Aurum and my concerns,
and he assured me that things would be different this time. For
one, Simon was going to take the lead in the writing. Secondly,
Simon liked e-mail, and further, he was planning to be in New En-
gland later that fall, and would stop by New Haven to work with
me on the manuscript. Impressed that he would take this sort of
initiative, I agreed to the collaboration.

Events unfolded pretty much as Simon and I discussed in that
initial phone call. Simon started with the Maple computer files that
we created at RPI and began writing the paper. I wrote some of the
introduction in Connecticut. We communicated by e-mail. By
January 1995, the manuscript was done and sent off to the C-ODE-
E journal. While we were waiting to hear the verdict, Simon was
granted tenure at Penn State. A few months later, the paper was
accepted for publication, and during my first semester at GVSU,
...carbon

...a second

...the salty

...tank was

...we could

...some of the

...the paper

..."The

...Tank"

...(Aboufadel

...&

...Tavener,

...1996),

...appared.

...Needless
to say, I was

...pleased.

...Right at

...the time

...we received

...the acceptance

...letter, Simon

...called to

...chat and
to tell me that he was taking a year's sabbatical in

...England. This time I was not concerned about a

...collaborator

...flying to Europe.

...Advice

...and Consent

...These two experiences of academic collaboration differed in outcome, but there is another critical distinction. Simon and I functioned as equals in our work together, as we were both tenure-track professors who attended the same workshop as participants, and we shared certain short-term career goals. On the other hand, before we began writing the paper in 1992, Dr. Aurum was at times both my professor and my boss. Though I appreciated all that I learned from him about mathematics education, we definitely had different motivations. As a full professor, Dr. Aurum did not share my urgency about publishing, and it seemed the only way to complete the manuscript was to follow him to universities around the world, which I was not, of course, in a position to do.

...There are other lessons to learn from these two experiences. First of all, I don't think it is a good idea to get too involved in the "publish or perish" mindset when entering an academic collaboration. In both of these examples, the best part of the experience for me was the learning or problem solving, whether it was algebraic topology or leaky tanks. There is something about the tenure process, though, that can make one lose sight of the intellectual accomplishments of a collaborative activity. It is like the bartender who measures whether or not he has done a good job by the size of his tips.

...If, after this sort of investigation, one is interested in publishing a paper, make sure your colleague is, too. Start up a conversation to get a sense of the motivation level of your potential colleague. Find
out what writing experience that person may have. Don't be afraid to ask questions, because, after all, writing a paper requires your consent.

The most important lesson, though, is that it should be clear early on what the responsibility of each participant will be when it comes to writing the manuscript. It is not enough to say, "We'll work on it." Explore how you will work on it, and how long each of you expects the writing to take. If you figure it can be done in three weeks, and your colleague is reluctant to commit to anything less than a year, then maybe you should reconsider the decision to write together. Ultimately, it is nice to work with someone who has a similar working style, and it is a good idea to investigate each other's working traits before a lot of time is invested in a manuscript.

Since arriving at GVSU, I have been a part of a few new collaborations, all of which have gone relatively well, including research with undergraduates. I now draw upon my early experiences as a way to make these newer adventures productive and worthwhile. I also now know some questions to ask the next time a colleague says he is flying to Europe.

References