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The Process of Doing Mathematics

By

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I have been actively involved for the last year as a mentor to several students under the New York City Louis Stokes Alliance for Minority Participation program. This experience forced me to think about the best approach to teaching undergraduates with a limited knowledge of mathematics how to actually do mathematics. I hope that my ideas on this matter may prove useful to someone who is acting as a mentor. I also hope to encourage teachers to become mentors by highlighting one of the less obvious benefits of mentoring.

I began with four students in the summer of 2006. By the spring of 2007, two of my students had moved on to schools outside of New York City. My students started with a background that included three semesters of calculus and a course in linear algebra. From the outset, I had two goals in mind. First, I wanted to teach my students some mathematics that would be useful no matter what careers they decided to pursue. Second, I wanted my students to gain the experience of actually doing mathematics, which in my mind meant that they should do some original research.

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With these aims in mind, I had to first of all decide what subject to study. My background is in non-commutative algebra. For a variety of reasons, I decided that this area was not appropriate for an undergraduate research project. There seems to me to be at least two approaches that one could adopt. Choose a subject that requires no specialized knowledge, but which is rich in open and challenging problems that could be solved without resorting to any complex machinery. On the other hand, one could spend some time teaching the students some specialized knowledge, and then give them problems that could be solved with the techniques that they had learned, perhaps supplemented by further theory according to the circumstances. I rejected the first approach because in my mind it was inconsistent with the first goal I stated above. To pursue the second approach, I finally decided to teach my students some number theory. Number theory has a rich history, is an active research area, and, most importantly for my immediate aims, has motivated the development of much of the abstract machinery which pervades modern mathematics

I spent the summer of 2006 teaching my students elementary number theory, roughly the equivalent of a junior-senior level undergraduate course in number theory. However, I adopted the point of view that in teaching number theory, I would make maximal use of the basic structures of abstract algebra: Groups, rings, fields, and vector spaces. This was consistent with

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my first goal. After the initial period of teaching my students elementary arithmetic, I had to now focus on my second goal, which was to get my students to do original research. Not being a professional number theorist, I was not conversant with the open problems, even the elementary ones. Frankly, I did not want to simply assign problems that were already well-known, on the assumption that such problems were already receiving plenty of attention. I decided to bring to bear my experience in non-commutative algebra, and look for problems in arithmetic that had analogues in algebra. Having done some work in the theory of central simple algebras, it was natural that I would look for the matrix analogues of results in elementary number theory. This is hardly a novel approach, but it turned out to be remarkably fruitful. For example, one of my students is currently investigating the analogies between Euler's totient (also called phi) function and row equivalence of matrices while another is looking at the analogues of Fermat's sum of two squares theorem over modular rings of integers.

How does one initiate a student into the process of doing research? The first thing that I had to overcome was the natural tendency of my students to turn to a textbook for the answer to a problem that they could not solve. More fundamentally, I wanted to encourage my students to embrace the unknown as an opportunity for discovery. My constant refrain is that at some point,

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no book, or paper, or person will have the answer to the question that one is asking. This fear of the unknown is primarily a psychological barrier, and it takes some time to overcome.

My next goal was to inculcate the notion that the most difficult part of doing mathematics is not in proving theorems, but rather in coming up with theorems that are worthy of being proved. For the novice in mathematics, the notion of proof and the techniques of proof seem to be of paramount importance. This is not entirely wrong, but it risks a fundamental misunderstanding of the nature of mathematics. To focus on proofs is to take the theorems to be proved as given. But in the practice of doing research, discovering the theorem to be proved comes before one can even begin to undertake the search for a proof.

How does one make new discoveries? I have spoken to other mathematicians about this topic, and it seems that the process of discovery is as individualized as one's fingerprints. So whatever I write about this topic is necessarily biased and reflective of my own temperament and abilities. I advise my students to pursue analogies and to examine many examples. The analogies one pursues will change as one's knowledge increases. However, I have been influenced by my advisor, Ravindra Kulkarni, to think in terms of three fundamental categories in mathematics: Number, space, and symmetry. The key is to appreciate the subtle and symbiotic relationship

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between these categories. This is not the place to elaborate on these themes, but the idea is to pursue analogies that follow from these basic categories.

The analysis of examples is of fundamental importance, and underappreciated by students. How often have you observed that a student can state a theorem and repeat its proof verbatim, but is completely lost when asked what the theorem implies about a basic example? A good example gives meaning to a theorem. Better yet, the examination of many examples is a fruitful way to make new discoveries. Of course, the discoveries that one makes will reflect one's talents. But the examination of many examples is a critical step in the process of discovery, and ought not to be bypassed. This is inductive reasoning. Students find it very difficult at first because it takes a lot of hard work. I encourage my students to engage in this process, and to persevere even if it doesn't yield immediate results. The process of doing research involves hard work and tenacity as well as creativity, and I want my students to understand and accept this.

In closing, I want to comment about some of the benefits of mentoring for the mentor. I teach at a community college, and as a result, have a heavy teaching load. The free time that I have for research is precious. Why should I devote any of that free time to mentoring students? Each teacher will have to find their own answer to this question, but my experience has taught me that

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mentoring can be personally rewarding and at the same time can further one's own research. The personal rewards are obvious, and need no elaboration from me. I think that it is the benefit to one's research that tends to be overlooked when one is considering whether or not to become a mentor. If one is actively engaged in research, why take the time to mentor someone who is unprepared to understand or contribute to the work that you are doing? I would argue that mentoring such a student forces you to look very carefully at basic questions. These questions have been the stimulus for original research for millennia. The time that one spends contemplating such questions can only benefit one's research program. This benefit may seem tenuous at best, but I am convinced that if a researcher brings to bear his or her creativity in the examination of such problems, then they will discover new avenues of investigation that they might not otherwise pursue or even be aware of.

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