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Introduction—How Academic Research Gets Done

WHEN WE THINK about the way in which we do academic research, we might think of the mathematician Andrew Wiles, who won the Abel Prize in 2016 for proving Fermat's Last Theorem. This "theorem" was originally stated by Pierre de Fermat in the seventeenth century, although Fermat did not provide a proof.[1] For over three hundred years, no one could prove that Fermat's Last Theorem was true, nor provide a counterexample to show it was false. Fermat's Last Theorem would baffle many of the world's greatest mathematicians—including Leonhard Euler and David Hilbert, each of whom spent several years attempting to solve it—and it became one of the greatest unsolved problems in mathematics, or really in any field. A German industrialist and amateur mathematician who himself had tried and failed to solve the problem established the Wolfskehl Prize at the end of the nineteenth century—a substantial financial reward to be given to the scholar who solved the problem. A century later, Wiles, who was a professor at Princeton at the time, worked on proving Fermat's Last Theorem in total secrecy for a number of years, letting only his wife know that he

1. Fermat famously wrote in the margin of a book: "I have a truly marvelous demonstration of this proposition which this margin is too narrow to contain." For Wiles's formal statement of the problem and presentation of his proof, see A. Wiles, "Modular Elliptic Curves and Fermat's Last Theorem," *Annals of Mathematics* 141(3, 1995): 443–551.

was working on the problem. One can only imagine what the conversations were like at lunch when his colleagues or students asked Wiles about his research. When Wiles finally announced the solution in 1993, it was generally considered one of the greatest mathematical discoveries of all time.

Those of us who become academic researchers in any field look to Wiles's discovery as the "Holy Grail" of what we would like to achieve with our scholarship. We all would love to solve a famous problem that was formulated by someone else, especially one that many others have unsuccessfully attempted to solve. Such an accomplishment would represent a substantial contribution to knowledge, and the scholar who solved the problem would become an "academic celebrity." Many of us get our PhD with the dream of making a discovery like Wiles's and gaining a similar kind of acclaim.

However, the actual experience of the vast majority of researchers, even the most successful ones, is nothing like Wiles's. Not only are most researchers far less successful than Wiles, but the approach they take to research is very different. Indeed, what is relevant to most researchers about Wiles's remarkable discovery is that it illustrates what most research is not. There are important differences between his experience and the approach most of us have to take to become successful researchers. At least three such differences are worth highlighting.

First, most problems we solve were not stated by someone else, and certainly not three hundred years ago by someone as famous as Fermat. Most of the time, at least half of the battle is coming up with the right questions to ask and the right way to ask them. In fact, once a question is asked, answering it is often quite straightforward. In 1937, Ronald Coase asked a question no one had asked before: "What determines the boundaries of the firm?" His paper asking this question led to the development of the field of organizational economics. Coase earned the 1991 Nobel Prize in Economics in large part because he had the foresight to be the first person to ask such an important question. Coase also proposed reasons for the boundaries of firms, but his explanation was fairly straightforward. Once the question was asked, many people would have come to the same conclusion he did. The brilliant part of Coase's paper was the asking, not the answering.[2]

Second, unlike Wiles's experience, research in most fields is intensely collaborative. In the sciences, research is usually centered on a laboratory or a research group working together on related problems. In the social sciences, the collaboration tends to be less structured but is no less important. Most papers are coauthored, and even sole-authored papers go through many rounds of revision based on discussions with colleagues before they are published. In most fields, it is very rare for someone working alone in secret to come up with an important discovery.

Third, the discussion following Wiles's discovery was about whether his proof was in fact correct, since there was no question about the importance of the problem he was trying to solve. However, the discussion about most academic papers usually centers on the nature of the contribution, the questions the paper asks, and the limitations of the analysis. Frequently the most important question in an academic seminar, and the one for which the author most often does not have a good answer, is "Why do we care about this paper?"

The burden of any researcher is to explain why the question she is asking is important and why she did what she did to answer it.[3] Most importantly, she should explain why the results tell us something we want to know, or should want to know, about the world around us. The ability of a researcher to provide such explanations can, and often does, determine the success of a particular research project. A paper that fails to explain why its contribution is important will have trouble getting

2. See Ronald Coase's "The Nature of the Firm," *Economica* 4(16, 1937): 386–405. Coase's Nobel Prize also was awarded for his other seminal contributions, especially "The Problem of Social Cost," *Journal of Law and Economics* 3(1960): 1–44.

3. For ease of exposition, I have tried to be consistent with my use of pronouns throughout the book. I use feminine pronouns when referencing researchers and authors, and male pronouns for readers. When I discuss doctoral programs and journal submissions, I have made advisers male and editors female. I have made these choices for consistency and not to make any statements about the empirical distribution of genders in the profession.

published, and even if it does get published, it will have little impact. Sometimes a researcher lacks an adequate explanation because the paper does not tell us anything particularly important. But often a researcher lacks a good explanation because she failed to "put her best foot forward" in explaining to a reader why he should care about the paper's results.

When I was a doctoral student, I was the beneficiary of the spectacularly good training provided by the MIT Economics Department. In my classes, I learned how to solve models, derive properties of estimators, and critique other people's work, as well as many other useful skills. What I did not learn in class was how actually to do research. That I learned by going to the National Bureau of Economic Research (NBER) office in Cambridge, Massachusetts, every evening, where I hung out with some of the best faculty and doctoral students from both Harvard and MIT. We spent hours and hours talking about what was good research and what was not, what we thought were the important questions yet to be solved, and whether the seminar presentation we heard that day made any sense. We also read each other's papers carefully and helped one another become successful scholars.

One thing I have observed over the years is that most graduate programs tend to prepare students for problems like the one Andrew Wiles solved, not the ones they are much more likely to deal with in their future careers. Traditional classes in graduate programs teach students to solve problems that have been posed for them, which is what they have to do to pass their qualifying exams. Solving a well-known question is what Wiles did when he solved Fermat's Last Theorem, although the challenge, of course, was on a totally different scale than passing a qualifying exam.

Where many graduate programs struggle is by not providing young researchers with the experiences and insights that are necessary to be successful researchers. They do not, for the most part, teach students the *craft* of being a scholar. In particular, they do not teach students how to pick research projects that will have lasting impact, how to communicate why a project will be important, how to handle data properly, how to write up results in an appropriately scientific yet readable manner, and how to interpret results in a way that others will find reasonable. Most scholars learn these skills as doctoral students in an apprenticeship-type relationship with their thesis adviser, from other faculty, and from fellow students.

Young scholars often ask my advice on various aspects of the research process. Their questions tend to arise from the craft rather than the science of economics. Young scholars want to know how they should pick research topics, find coauthors, write readable and interesting prose (in English), structure academic papers, present and interpret results, and cite other scholars. Most frequently, they have questions about all aspects of the publication process. In addition, academics of all ages do not think enough about their own professional development and do not invest in the human capital that would allow them to enjoy their jobs throughout their career.

Learning the economist's craft—how to do research and how to proceed in career development—has historically been a random, word-of-mouth process. Some scholars are fortunate enough to have someone to teach them the craft of the profession, while others go their entire career without figuring it out. There is no reason why something this important must be communicated in a haphazard manner by word of mouth. It can and should be written down.

The State of Academic Research

Before getting into the particulars of how to do research, it is important to understand the market in which we work and how it has affected research. While basic research in some fields is done by the corporate and government sectors, in most fields it tends to be dominated by universities. Universities reward faculty in large part based on their research, so faculty have substantial incentives to do research and publish their findings in the most prestigious outlets possible.

The academic marketplace can be summarized by three main trends: First, there has been substantial growth in academic research globally. Many universities, both in the United States and, especially, in other countries, have decided that they should improve their research reputation and are strongly encouraging their faculty to become more active scholars. Second, this growth has led to more competition among faculty for research ideas. This competition, in combination with the maturing of most fields of study, has led faculty to become increasingly specialized. Third, the growth in the number of top-level journals has not matched that of research-active faculty, so it has become increasingly difficult to publish a paper in a "top-tier" journal.

THE GROWTH IN ACADEMIC RESEARCH

Many universities have cut back on the number of their tenure-track faculty as a way of saving money, but others are trying to gain prestige by increasing their research presence. In the thirty-plus years since I left graduate school in 1987, the number of universities expecting their faculty to publish in top outlets has increased dramatically. While my PhD is from an economics department, I have focused my research on financial economics, a subfield of economics that is mostly taught in business schools. I have observed a number of changes in the structure of finance academia since I left graduate school. Similar changes have occurred in economics departments and also in related fields such as accounting.

In 1987, little research was published in the top journals that came from outside the top twenty or twenty-five US departments. Now there are probably at least one hundred US departments that require publication in top journals as a condition of earning tenure. Internationally, the growth in this expectation has been even larger. In 1987, only two European finance departments consistently produced top finance research, London Business School and INSEAD. Today there are probably at least ten or fifteen departments with as many active researchers as London Business School and INSEAD had in 1987. In Asia, little serious finance research was going on in 1987. Now there are at least three very good departments in Singapore and four or five in both Hong Kong and Seoul. In mainland China, academic research activity has grown so much that it is virtually impossible to keep track of all the good departments unless you live there.

Growth in doctoral programs has mirrored the increase in high-quality departments. In the 1980s and 1990s, with rare exceptions, most of the best finance PhD students graduated from the top ten US departments. Now the best students on the academic job market come from all over the world. European departments regularly place students at the top five US departments, and US departments ranked outside the top fifteen or twenty regularly produce extremely good students who land jobs at top departments. Students from Asian programs are getting better every year, and it is only a matter of time before, like their European counterparts, they are regularly placing at the top of the market. As a result of this growth in doctoral programs, there are many more active researchers in the world today than when I began my career, and that number is growing at an accelerating rate.

SPECIALIZATION IN RESEARCH

What about the problems that are being studied? In most fields, contributions tend to become narrower and narrower over time as researchers become increasingly specialized. The basic questions in any field remain the same, so researchers discover the most fundamental contributions first, then refine them over time.

Occasionally, there is a seminal event, research breakthrough, or technological innovation that spurs new research. In my field, one such event was the Financial Crisis of 2008. While catastrophic for the world economy, the crisis led to an important burst of research seeking to understand its causes, the effect of new financial products on the economy and how they should be regulated, potential government interventions during a financial crisis, whether banks should be allowed to be "too big to fail," and similar issues.

Recently, the availability of immense amounts of data and the computing tools to work with such data have revolutionized many fields. Much recent research in many fields of economics has been based on newly available large databases, dramatically increased computing speed, and new approaches to data analysis, such as machine learning. These developments have pushed economics and related fields toward applied empirical work. Roger Backhouse and Béatrice Cherrier point out that ten of the previous twelve winners of the John Bates Clark Award, which is given to the top American economist under age forty, focus on empirical or applied work.[4] This pattern is in marked contrast to the early years of the award, when it most often recognized work in theoretical economics or theoretical econometrics.[5]

These examples of "quantum jumps," however, are more the exception than the rule. The general rule is that academic fields tend to become more narrow and more specialized over time. In some fields, such as math, biology, psychology, and economics, the subfields have essentially become fields of their own, with the faculty becoming so specialized that there is sometimes little interaction across subfields.

For example, in finance most of the leading lights of the generation previous to mine, such as Fischer Black, Gene Fama, Mike Jensen, Bob Merton, Merton Miller, Steve Ross, and Myron Scholes, worked in a number of different areas of finance.[6] Academic finance was in its infancy when they were beginning their careers, and all of these individuals made important contributions across the main subfields of finance. In my generation, a few of the very best finance researchers, such as Andrei Shleifer, Jeremy Stein, and Robert Vishny, have also made important contributions across the major subfields. Most of us, however, specialize in one subfield or another. In the generation after mine, scholars have become even more specialized: a typical new PhD comes out of graduate school as a "macro-finance person," a "dynamic-contracting scholar," or a "time series econometrician specializing in asset prices."

4. R. E. Backhouse and B. Cherrier, "The Age of the Applied Economist: The Transformation of Economics since the 1970s," *History of Political Economy* 49(2017): 1–33.

5. For the American Economic Assocation's list of recipients of the John Bates Clark Medal, see https://www.aeaweb.org/about-aea/honors-awards/bates-clark.

6. Economists will immediately recognize these names. Non-economist readers should be aware that of the individuals on this list, Fama, Merton, Miller, and Scholes are recipients of the Nobel Prize in Economics. Black and Ross tragically passed away before they received the prize but undoubtedly would have received it at some point had they lived longer. Jensen's prize will hopefully be awarded at some point in the near future.

Owing to this specialization, however, scholars who do excellent work in one subfield of finance sometimes lack a basic level of competence in other related subfields. For example, people who are strong in macro-finance often fail to keep up with new empirical results related to investments, nor are they fluent in behavioral research, even though each of these subfields has important things to say about the determinants of asset prices. I fear that finance is heading in the direction of many other fields that find themselves populated by scholars in the same department who cannot understand each other's work.

THE PUBLICATION PROCESS

In contrast to some other fields in which the most important publications can be books or conference proceedings, by far the most important method of disseminating research in the economics-based fields is through refereed journals. These journals differ substantially in both their quality and the style of research that they tend to publish. Higher-ranked journals are much more prestigious, and many departments promote only faculty who publish in the few journals that they consider top-tier. Consequently, the ability to publish in top journals is an important element of an economics scholar's success.

Since I entered the profession in 1987, the number of journals has grown with the size of the profession, but the ones considered top-tier have not changed. In economics, the top general-interest journals in 1987—*Journal of Political Economy*, *American Economic Review*, *Quarterly Journal of Economics*, *Review of Economic Studies*, and *Econometrica*—retain that status today. While more specialized "field" journals have grown in both quantity and quality since 1987, most research-oriented economics departments expect junior faculty to publish at least some of their work in the top general-interest journals if they are to earn tenure.[7]

In finance, we have the same three top-tier journals as we had in 1987: *Journal of Finance*, *Journal of Financial Economics*, and *Review of Financial*

7. See D. Card and S. DellaVigna, "Nine Facts about Top Journals in Economics," *Journal of Economic Literature* 51(1, 2013): 144–61.

Studies. Similarly, in accounting the same three journals dominate the field today as in 1987: *Journal of Accounting and Economics*, *Journal of Accounting Research*, and *Accounting Review*. These journals usually publish more papers per year than they used to, but not nearly enough to compensate for the increasing number of scholars in the field. Most of the top departments expect the majority of their faculty's research to be published in these journals or in comparable ones from related fields.

Whatever the reason, journal reputation is extraordinarily sticky (a topic, not surprisingly, about which academics love to speculate). Top journals not infrequently make questionable editorial decisions and sometimes provide terrible service to authors. Nonetheless, it is virtually impossible for a new journal or a lower-ranked journal, even if such a journal provides excellent editorial service and publishes first-rate papers, to break into the top tier in the eyes of tenure committees and university administrators.

As an economist, I am depressed by the failure of market forces to ensure quality in our own industry, in contrast to both the principles of economics and the experiences of real-world industries. For example, when American automobile manufacturers produced mediocre cars that had terrible gas mileage in the 1970s, a subsequent influx of better cars produced by Japanese competitors prompted American manufacturers to increase quality. This pattern regularly occurs in many different industries and is one of the hallmarks of a successful free-market economy. But in academia, a journal can regularly take more than a year to get back to authors and still be considered top-tier by universities. Faculty will continue to submit their top papers to such journals regardless of the poor service they receive, and the journal will feel little market pressure to improve its service to authors.

Changes in Academia and the Research Process

How has the research process been affected by the changes in the publication environment? One effect of contributions becoming narrower and more specialized is a shrinking pool of potential reviewers for papers. Smaller pools of reviewers increase the potential for politics and the creation of cliques. In some subfields, reviewers seeking to promote their subfield (to the benefit of those in the clique) tend to be more positive; in fields where various turf wars are raging, reviewers tend to be overly negative. Overall, the academic research world has become increasingly competitive, since there are more and more scholars pursuing narrower and narrower research topics, and all are competing for space in the same journals. There is every reason to think that it will become even more competitive in the future.

If you are reading this book, you probably are an academic or are considering becoming one. Therefore, you probably find this discussion disquieting, if not outright depressing. In some ways, it is certainly depressing: an academic research career is becoming a more and more difficult way to earn a living. However, academia remains a wonderful profession in which you can have a fantastic life. Scholars can contribute to society in any number of ways—educating good students, increasing humanity's body of knowledge, providing insights that can improve public policy. Tenure enables us to present unpopular ideas without worrying about retribution from bosses. Our friends in the private sector are often jealous of our academic freedom to express such unpopular opinions publicly.

Responding to the Competitive Environment

How should the increasingly competitive nature of the academic labor market affect our behavior? In other words, how does a newly minted PhD or faculty member survive and even thrive in this environment?

There are always factors that are out of your control that affect your success. But there is much that can be done to advance your career, often in ways that might seem obvious but are ignored by young academics. It is somewhat ironic that in business schools we spend considerable time teaching our MBA students how to improve their career prospects but little time thinking about our own.

Faculty often pursue haphazard research strategies. Some start too many papers, others start too few. Some essentially rewrite the same paper over and over, while others constantly start papers in many different subfields and never publish any of them. Academics make many other correctable mistakes when managing their research career. While my experience is mostly with business school faculty and economists, I am confident that the same issues affect faculty in all fields.

Helping young academics survive the pressure they face and put their best foot forward when doing research is the overarching theme of this book. Here are a few principles that I will touch on throughout the book that are likely to help young scholars develop a successful research portfolio.

1. UNDERSTAND YOUR PRODUCTION FUNCTION

Economists characterize the way a producer can convert inputs (materials, labor, capital, and so on) into outputs as a "production function." Formalizing this production process helps economists study firms, as well as the markets in which firms operate.

But the notion of a production function is also much more general, and a useful way for academics to understand how they go about doing research themselves. We each have a certain set of skills that allow us to contribute usefully to research projects. Some of us work well by ourselves, while others prefer being part of a team. Some people are very creative and come up with novel ideas, while others are better at performing analyses suggested by others.

Perhaps the most important aspect of an academic production function that academics misunderstand is the notion of capacity. There are only twenty-four hours in a day, and most of us like to spend some of them enjoying life outside of work. Moreover, research is an intense activity; it is hard to focus on more than one or two things with sufficient intensity at any time. There are tricks to managing your workload. Personally, I try to work hard on one paper at a time; I then return it to my coauthors and focus next on another paper while the coauthors take their turn editing the paper. That way I can work diligently on a number of papers simultaneously.

Nonetheless, there is a limit to the number of papers that any of us can work on at any point in time. This number varies across individuals, but each of us has a "capacity." I believe that it is important for people to know their own capacity, because committing to research projects that exceed your capacity can be a serious mistake. Some scholars constantly start new projects and work on many papers at once, but too often they frustrate their coauthors, produce sloppy work, and never finish many of their research projects. Of course, the opposite is true as well: some scholars are such perfectionists that they never start anything that they don't think will win them a Nobel Prize. Usually, such projects never arrive, but these perfectionists nonetheless like to boast of having higher standards than other people despite their lack of production.

2. PROCEED WITH A PLAN

In business schools, we teach young entrepreneurs to start with a well thought out business plan for their new enterprises. Such a plan involves setting very specific goals, such as customer acquisition, development of beta versions of software, and a date by which the firm will be profitable. Such plans can be thought of as a route to becoming a successful firm in a specified (but narrow) sector of the economy. Entrepreneurs, even successful ones, do not always end up following the plan. Sometimes the plan is found to be overly ambitious; even if the firm makes good progress, it is often not as rapid as the entrepreneur had hoped for. And sometimes the plan turns out to be somewhat misguided and the firm has to shift its focus to be profitable. Nonetheless, having a business plan is important, principally because it forces the entrepreneur to keep his focus on the end goal and requires him to have a very good reason to depart from the original plan.

I see no reason why young academics shouldn't take a similar approach. Suppose you are a doctoral student who finished your exams and now needs to write a dissertation, or a young assistant professor looking to establish a research reputation, or even a full professor looking to remain active in research. Why not follow the same process as a new entrepreneur? Decide where your interests lie and what big-picture question you want to address. Then make a "market map" that shows what has been learned about the question, what remains unknown, and, perhaps, why some questions have not been addressed yet. Through this process, you will hopefully hit on a research idea or two. Decide what you are likely to learn from the idea and make sure it is sufficiently important to be worth your time. Then set a timetable for when you think you will be able to complete drafts of each research project, and try your best to keep to this timetable. Your ultimate output might not look like the plan you made, but having such a plan is likely to make you happier with the output you do produce.

I realize that approaching research in such a systematic fashion probably sounds simpler than it will prove to be in practice. My point is not to make the research process seem easy or formulaic. Rather, my intent is to get scholars to think systematically about where their research is going and how they are going to get it there. Many young scholars proceed in a rather random and haphazard fashion, looking only at whatever topics happen to occur to them. I know this problem well, because I did the same during my first few years as a faculty member. Once I defined my areas of research more tightly and focused on becoming one of the main participants in these areas, I became a much more productive scholar.

3. FINISH PROJECTS

The vast majority of academics enter the profession because they love to learn. We all did well in school and were fascinated by problems for which we did not know the answers. Solving them was a lot of fun. Research came naturally to us because we loved solving new problems and developing new ideas.

Starting research projects epitomizes what we love about academia. A scholar starts a research project because she is thinking about a problem she does not know the answer to and she hopes to come up with a way to answer it. Sometimes she will get interesting results and learn something; other times the analysis just makes the question murkier. But at some point in the analysis, she learns whatever she is going to learn from the question.

At this point, research stops being fun and starts being work. The scholar will go down blind alleys, then have to retrace her steps when that approach does not work. She will have to get around to writing the paper, hopefully, in a way that helps others understand what she did, why she did it, and what she found. She will have to present the paper in seminars and deal with people—some of them lacking the usual social graces—who question her analysis. She may have to wait as long as a year to hear about her submission to a journal, only to get two short referee reports of limited value and a terse note from the editor saying the paper might be reconsidered for publication if she is responsive to the referees.

Usually an author pretends to enjoy being questioned about her paper's basic premises as well as every step of its logic. Sometimes she really does enjoy the criticism, and sometimes it actually is helpful. The author is more likely, however, to publicly thank the critics politely but secretly want to strangle them. How can they not understand the point of what is in the paper, and why won't they just shut up and realize how brilliant it is? And worse, because referees are anonymous in many fields, they often feel no constraints about being harsh toward the paper they are reviewing (whose author becomes the dog they get to kick as they steam about the referee reports they just got on their own papers).

Young scholars can feel tempted to throw up their hands and start another paper. After all, starting papers is fun, but finishing them can be painful. *Do not give in to this temptation.* An author has to understand why people have responded negatively to her work, even if she thinks they are horribly misguided in doing so. Unless the author has proved Fermat's Last Theorem, she will have to expend a fair amount of effort explaining why her result is important and why a reader should care about it. The key to success in the face of negative criticism is persistence. The author has to learn how to understand and elucidate her paper's contribution so clearly that others will find it difficult to object.

Ultimately, an academic has to publish her papers. In academia, little weight is given to unpublished work. Even in fields like economics and finance, where unpublished but circulating working papers can have influence, promotion and tenure committees want to see the "certification of quality" that comes with publication. And after a paper has remained unpublished for a few years, the author's peers will stop feeling an obligation to cite it. In other words, both the paper (which takes on a life of its own) and the author benefit substantially from publication.

Sometimes authors refuse to publish a paper if they cannot get it accepted in one of the journals considered top-tier. I believe this attitude is a mistake. There are many journals, and a paper will have a much larger impact if it is published in a good journal, even one that is not considered top-tier, than if it is not published at all.[8] Many good papers are never published because the authors lack the persistence to see it through, or because they do not understand the paper's contribution and limitations and try to market the paper in an inappropriate manner.

4. BE PROFESSIONAL IN YOUR INTERPERSONAL RELATIONSHIPS

For reasons that I do not understand, a very close friend of mine has gone into administration and now is a vice provost at one of the top universities in the country. He always tells me that the biggest surprise he finds in his job is the immature behavior of brilliant scholars, who regularly act like five-year-olds. With the advent of social media and the internet, every mistake you make risks becoming not only widely known but unforgettable. (Like most great innovations, Google is both a blessing and a curse.) Every few months there seems to be a new scandal that people discuss over the internet. For example, one big name might accuse another of stealing his idea while they are socializing, and before long there is a nasty email trail that everyone in the profession

8. Not everyone agrees with me on this point. One of my favorite coauthors commented: "Pushing a paper through in a below top-tier journal often takes a great amount of time as well. Yet papers in non-top-tier journals do not count towards tenure, and do not attract quality citations. In many places, publishing on lower-tier jounrals is even considered a bad signal about the author. So we often wonder whether it is worth the time or [whether] we can put the time into a more promising project."

has seen. Or a prominent faculty member writes a paper that cannot be replicated, the entire profession soon knows the story, and the faculty member's reputation is damaged.

A faculty member or even a doctoral student must always remember to be a professional scholar. As in all professions, the standards about anything related to one's job are much higher for professionals than for amateurs. It is fine for economics professors to go to a bar and karaoke out of tune, but a tape of a professional singer engaged in such activity could be harmful to her career.

As academics, we are on display all the time, especially when we discuss anything related to our specialty. If we produce a result and post it publicly, we have to make sure it is correct, double- and triple-checking the code before posting. Once it is online, it is there forever and people can (and will) find it. Everyone makes honest mistakes, but if we make too many mistakes, even honest ones, people will stop believing anything we do. If we blog or tweet, we try to do so intelligently. If we say things through social media that do not stand up to the standards of logic that we expect in an academic dialogue, we shouldn't expect people to take us seriously when we try to contribute to more serious discussions in other settings.

Why Do Academic Research?

So why do you do academic research?[9] Why embark on this path in life? There is only one really good answer to this question: *because you love it*. You love playing with new ideas, understanding things you didn't understand before, learning something new about the world, but also communicating that to other people, teaching them the new idea, shepherding new ideas to their place in the world. Yes, it will take some ambition, some working the system. And there will be drudgery—cleaning data, answering referee reports, doing your social personal and professional duties.

9. This subsection is copied (with minor edits) from the review of this book that John Cochrane wrote for Princeton University Press.

As an ambitious academic, you should also enjoy the accolades of people reading and following your work. You should not write just to publish papers; you should write to have people read them, cite them, think about them, and change the way they think. You want to make an impact beyond having someone remark at your funeral on how long your vita was.

If you love it, academic research is liberating. You have time to pursue ideas of your own choosing. But if you are like 99 percent of people on this planet, being told to "go to your office and think up something great" is a paralyzing and terrifying mandate. Most people need to be told what to do, told when to work, told when it's okay not to work, and told what to think about. Most people need daily pats on the back and the incentives that business is good at providing.

Only people who love academic scholarship—who love playing with ideas and who love the hard process of refining them, writing them, presenting them, and interacting with others about them—actually produce good work. Do you love academic scholarship? Then you should do it.

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