The Economist's Craft:
An Introduction to Research, Publishing, and Professional Development

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Dedication: “To my students and coauthors, who taught me most of what is in this book.”

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Preface

Purpose of the Book

When I moved to Tucson, Arizona in 1994 to teach at the University of Arizona, I started teaching a doctoral course on corporate finance, covering research about the way that firms raise capital, corporate governance, and related issues. In the 26 years since then, I have taught some variant of that course almost every year during my time on the faculties of three different universities. The students in these classes were almost always smart and hard-working, and the vast majority wanted a career as academics themselves. Yet, many did not succeed in that goal. Some were unable to complete their programs, some could not get academic jobs following graduation, and some were unable to publish their work once they became junior faculty.

There are a limited number of tenure-track positions in academia, so it is not possible for every entering doctoral student who wants an academic career to have one. To become a productive academic scholar, talent and a drive to succeed are necessary, but are not sufficient. For the majority of young scholars who do not succeed in becoming successful academics, the problem is not a lack of ability or effort. Instead, the problem is that they do not go about their task as graduate students, and then as junior faculty, in the best way. Being a professional scholar is something completely different from almost any other profession, and many people who want to become academics never figure out important aspects of the job.

In light of this observation, I started including short segments about how best to approach the task of becoming a successful academic into my doctoral course. I covered topics such as what to spend time on as a doctoral student, starting research programs, motivating papers, writing English prose, presenting research, and acquiring the human capital necessary for a successful career after finishing the dissertation. After a few years, I began to notice that the students were paying more attention during these short segments than they were in the rest of my lectures. Students appeared to be more interested in the advice I

had for them about the way they should approach their time in the program and their future careers than what I had to tell them about corporate finance.

Doctoral students are always justifiably nervous that they aren't making optimal use of their time in the program so are almost always very appreciative of whatever guidance they can get from faculty. Over the years, I found a demand for advice came not just from the students I was teaching, but also from students and younger faculty I met as I visited other universities around the world. In fact, the demand for advice is so great that many young scholars even turn to internet message boards, asking for the opinions of anonymous strangers who usually know as little as they do.

A second observation I have had over the years is that, perhaps because of a lack of good advice, many scholars, both doctoral students and also faculty members, constantly make the same mistakes. It is far too common that publicly-circulated papers contain incredibly long, mind-numbingly dull literature surveys; introductions that go on and on before they tell the reader what the point of the paper is and why the reader should bother to waste her time on it; data descriptions containing insufficient detail for a third party to replicate the results; tables that are unnecessary, badly labeled, or hard to understand; or overly dry prose written in passive voice that seems designed to put the reader to sleep. In addition, many scholars manage their time so badly when giving presentations that they don't get to the main results of the paper until the last five minutes of the talk. Their presentations are often poorly designed with slides that are incomprehensible, or even just unreadable due to fonts so small that participants sitting more than a few rows back cannot read them. Young faculty routinely mismanage their careers by not having a coherent research agenda, not getting their papers to journals, or not making connections with people in their own fields who teach at other universities. Sometimes they don't even bother to show up for seminars in their field at their own universities.

Writing papers, making presentations, and communicating with other scholars are basic parts of a professor's job. Yet, while there are books and courses on how to do almost anything in the world, there is very little written on how to be a successful academic. The irony is that as academics, we spend our lives teaching other people skills for all sorts of non-academic jobs. But rarely does anyone teach us how

to do ours. Usually what a scholar has learned about how to do her job is what she's been lucky enough to absorb from faculty advisors, friends, and colleagues; the rest she figures out on her own.

Doctoral programs in economics and related fields usually do a reasonably good job teaching the *science* involved in research, meaning that programs teach theory, econometrics, and the literatures in applied fields fairly well. Where they are lacking is the way that they teach the *craft* of research. Like other kinds of craftsmanship, the way to write a research paper can be thought of a craft, involving a combination of time-tested techniques, strategic thinking, imagination, ethics, and attention to details that are often overlooked.

The observation that doctoral students and young faculty members often do not learn the craftsmanship necessary to do their jobs well made me think that "someone should write a book explaining to academics how to do research and manage their careers." After teaching at five research-oriented departments and seeing the success and failure of my students and colleagues, I decided that I would try to be that someone. What you are about to read is my attempt.

The purpose of this book is to provide a guide for a scholar who wishes to pursue a career in academia. The primary focus is on research — how a scholar selects research topics, does the analysis, writes her papers, and publishes them—though the book marries research and publishing with professional development. Throughout the book, I encourage scholars to think of each paper as part of a larger research program. A scholar's goal should be to structure her research so that the profession learns more from her body of collected research than from the sum of the contributions of each individual paper.

When writing this book, I tried to make each chapter more or less self-contained, with each covering a different aspect of a scholar's job. The idea is that whenever a scholar stumbles across an aspect of her job about which she was uncertain, the book would contain a chapter that can help her address the issues she is confronting. So, for example, if a scholar is trying to write the introduction of a paper, she can turn to Chapter 5 and read my discussion about the way to write an effective introduction. Of if she is contemplating how to present her empirical results, she might look to Chapter 7, where I describe ways in which a scholar can present empirical results in a clear and compelling manner.

Since my background is in economics and finance, the book is most relevant to scholars working in those fields. For scholars in other fields that might be considered part of the broader economics ecosystem, such as accounting and some aspects of public policy and related social sciences, the book should be equally relevant. My hope is that much of what I say will be valuable to academics working in other fields, and also for non-academics who do research and try to publish it.

How the Book Came to Be

Much of the book explains the process of writing a paper from the idea stage through publication. It describes the transformation of an idea into a research project, then into a draft of a paper, then through the many revisions prior to submitting it to a journal, then through the further revisions that follow submission, and eventually to acceptance by the journal and publication. It also explains how a scholar should think of each paper as a part of a coherent research program that defines her career, and how she should manage that career to maximize her own welfare. At each step, I present techniques that a scholar can use to accomplish this step of the research project.

When writing this book, I tried to approach it as I would a research project, and to utilize the very techniques I was writing about. So, when I was starting out and wondering whether I should write it at all, I decided to follow the advice I would eventually include in Chapter 3. I gave myself some time to write an introduction and to see if I could make it something that I thought others would find interesting.

I began my attempt during a vacation, so that I could write without any work-related distractions. On a cruise down the Dalmatian Coast in the summer of 2017, I sat for days in the boat's lounge with a computer on my lap, trying to write something I liked. The other passengers often came up to me and asked me what I was doing. At first, I told them I was *working*, and they glared at me, because *working* on a cruise is considered a faux pas and is not supposed to be done on vacation, especially not in a public place where it would make the other passengers feel guilty. After a few days, I changed my answer to be that I was *writing,* and the other passengers were fine with that. Writing can be fun, so is an acceptable

thing to do on vacation. In fact, the sooner that a scholar believes that writing really is fun, the better her papers will be!

Two years later, I finally had something I liked, which was an early draft of Chapter 1. I showed it to a few people. As I mention in Chapter 13, one of the first people I showed it to was my former thesis advisor, Jim Poterba of MIT. In addition, my close friend and coauthor Ben Hermalin of Berkeley gave it a careful read, as did a few other asked coauthors, students, and colleagues. All of these people agreed that what I was doing was worthwhile and encouraged me to proceed. So, I nervously wrote a tentative outline and gave it a try.

I decided to write each chapter as a more or less self-contained essay on one aspect of being an academic. After writing an initial draft of a chapter, I used the approach discussed in Chapter 10, distributing it sequentially to students and colleagues rather than giving it to all of them at the same time, hoping to minimize duplication of the suggestions I would receive. I was fortunate to have Hyeik Kim, one of our excellent doctoral students, assigned as my research assistant for the academic year. Hyeik was always the first person who saw the initial draft of a chapter. She fixed many mistakes and gave me her opinion on the strengths and weaknesses of the chapter. She seemed to take great joy catching me violating one of my rules for English prose discussed in Chapter 8, such as over-using passive voice, or God forbid, using "this" as a noun rather than as a modifier. I dropped or completely rewrote several chapters before anyone else saw them because of Hyeik's reaction to them. I could never have completed the book without her.[1]

After Hyeik was finished with a chapter, I forwarded it to two former doctoral students, Murillo Campello and Shan Ge. Murillo and Shan each read every chapter carefully and offered invaluable detailed suggestions that substantially improved the text. After a while, my colleague Lu Zhang and my coauthor Tracy Wang joined in, each reading the entire draft while giving me detailed comments. Throughout this process, I also received feedback from a number of friends and colleagues on various

[1] Toward the end of the writing process, two other doctoral students, Dongxu Li and Rui Gong, also provided invaluable help improving the manuscript.

chapters. Ben Hermalin, Steve Kaplan, Mervyn King, Josh Lerner, Ron Masulis, and Jim Poterba were especially helpful.

After I had written about half of the book, around the beginning of 2020, I realized I had to find a publisher. I realized fairly quickly that the appropriate publisher for this book was likely to be a university press, since the target audience is academics. Similar to the journal review process described in Chapter 11, these presses all require that manuscripts be reviewed by several scholars. However, unlike the journal review process, it is normal when publishing a book to send the manuscript to multiple publishers at the same time. So, I sent the paper to some well-known university presses and received favorable feedback from a number of them. Eventually, four prestigious university presses agreed to send the manuscript to reviewers.

While all the editors I dealt with were very professional and helpful, one who stood out throughout this process was Peter Dougherty of Princeton University Press. I often teach my private equity class that the best venture capitalists begin to add value even before they sign a contract; it turns out that the same is true for the best editors. One Sunday morning at 8 AM, I received my first email from Peter, which contained substantive suggestions for ways I could improve my manuscript. Before even sending it to reviewers, he convinced me to change the title and to focus on economics-related disciplines (I had been trying to write a book focusing equally on all fields). When Peter called with an offer to publish the book at Princeton University Press, I accepted it immediately without hearing whether the other publishers would be willing to offer contracts.[2]

The reviews from Princeton University Press were incredibly valuable. They led me to rethink several sections of the book and to address a number of new topics. Yueran Ma, Steve Medema, Jonathan Wight, and John Cochrane were all particularly helpful. John Cochrane even wrote an amusing entry to his blog about the book, which can be seen at: https://johnhcochrane.blogspot.com/2020/04/weisbach-

[2] It was my wife Amy's suggestion to accept immediately. My natural inclination would have been to foolishly drag the process out longer than necessary. Sometimes, it is a good idea to listen to the suggestions of your spouse!

advice.html. The book would not have been the same without the reviewers' help, for which I am eternally grateful.

I would be remiss if I did not mention that there are other sources that provide useful advice for young scholars. A book that has a very similar title as mine is William Thomson's *A Guide for the Young Economist*. However, the overlap between the two books is surprisingly small. I would recommend that scholars look at Thomson's book if they have a question about an issue that I do not address here satisfactorily.

There are of course many wonderful books about how to write. William Zinsser's *On Writing Well* is excellent. In terms of writing about economics, the classic is Deirdre McCloskey's *The Rhetoric of Economics*. John Cochrane's *Writing Tips for Ph.D. Students* is a very good guide, and is not just about writing, but also pertains to many of the other topics covered in this book.

One last thing I would like to emphasize is that my recommendations in this book are a matter of opinion, not fact. What I say are my opinions about the best way to go about an academic career. My guess is that most readers will agree with much of what I say and disagree with me on a few points. There is more than one way to do research and more than one way to approach a career. Many academics have had excellent careers doing the opposite of what I recommend.

Nonetheless, many scholars do drift through their careers without thinking much about the issues I discuss. They go from project to project without much thought about how the work fits together into a research portfolio and whether any of it has the chance to be influential. These scholars usually think that when they have finally gotten a likelihood function to converge or proved a theorem that they have worked on for a long time, their work is done.

At this point, however, the work is only beginning. The way in which a paper is written up, presented to an audience, marketed to a journal, and packaged with other related work as part of a research portfolio, will determine the work's impact and the value it will add to the scholar's career. If scholars think about these issues, but decide that I am wrong about every point I make in the following pages, that is fine with me. These scholars will still be much better off having spent the time thinking

about these issues and the best ways that they can address them, so the book will have accomplished its purpose.

Chapter 1: Introduction – How Academic Research Gets Done

When one thinks of the way in which one does academic research, one might think of the famous 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 17th Century, although Fermat did not provide a proof.[3] For over 300 years, no one could prove that Fermat's Last Theorem was true, nor provide a counterexample to show it was false. It became one of the greatest unsolved problems in mathematics, or really in any field. There was a substantial financial reward for solving the problem in the form of the Wolfskehl Prize, donated by a German industrialist and amateur mathematician who himself tried and failed to solve the problem. For several centuries, Fermat's Last Theorem baffled many of the world's greatest mathematicians, including Euler and Hilbert, each of whom spent several years attempting to solve it. 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 was working on the problem. One could 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' 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 would lead whoever solved the problem to become an "academic celebrity." Many of us get our PhDs with the dream of making a discovery like Wiles' and gaining the kind of acclaim he achieved.

[3] 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' formal statement of the problem and presentation of his proof, see Wiles, A. (1995). "Modular elliptic curves and Fermat's last theorem". *Annals of Mathematics*, 141(3), 443-551.

However, the actual experience of the vast majority of researchers, even the most successful ones, is nothing like Wiles'. Not only are most researchers far less successful than Wiles, but the approach they take to research is much different. Indeed, what is relevant to most researchers about Wiles' 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, certainly not 300 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. Often, once a question is asked, answering it is 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 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 as he did. The brilliant part of the paper was the asking, not the answering.[4]

Second, unlike Wiles' experience, research in most fields is intensely collaborative. In the sciences, research usually centers around a laboratory or research group who work together on related problems. In the social sciences, the collaboration tends to be less structured, but no less important. Most papers are coauthored, and even sole authored papers have been 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.

[4] See Ronald Coase's "The Nature of the Firm," *Economica*, Vol. 4, pp. 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* (1960) Vol. 3, pp. 1-44.

Third, the discussion following Wiles' 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 around 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.[5] 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 published and, even if it does get published, will have little impact. Sometimes, a lack of an adequate explanation ends up being because the paper does not tell us anything particularly important. But often it is the result of a researcher failing to "put her best foot forward" doing a good job 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, to derive properties of estimators, to critique other people's work, and 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 the Economic Research (NBER) office in Cambridge, MA, 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 is good research and what is not, what are the important questions yet to be solved, and whether the seminar

[5] 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/authors, and male pronouns for readers. When I discuss doctoral programs and journal submissions, I have made advisors 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.

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 will 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 of course on a totally different scale than passing a qualifying exam.

Where many graduate programs struggle is by not providing young researchers 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 particular project will be important, how to handle data properly, how to write up one's results in an appropriate scientific, yet readable manner, and how to interpret one's 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 advisor, other faculty, and fellow students.

Young scholars often ask my advice on various aspects of the research process. Their questions tend to concern about 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 make investments in the human capital that would allow them to enjoy their jobs throughout their career.

Learning the economist's craft, the way to proceed in research and 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 careers 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 in some fields, basic research 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 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, in the US and especially in other countries, have decided that they should improve 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 been increasingly difficult to get papers published in journals that are considered top tier.

Growth in Academic Research

Many universities have cut back on the number of tenure-track faculty as a way of saving money, but others are trying to gain prestige by increasing their research presence. In the 30-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 mostly 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 left graduate school. Similar changes have occurred in economics departments and also in other related fields such as accounting.

In 1987, there was little research in the top journals outside the top 20 or 25 U.S. departments. Now, there are probably at least 100 U.S. departments that expect publications in top journals as a condition of earning tenure. Internationally, the growth has been even larger. In 1987, there were only two European finance departments that consistently produced top finance research (London Business School and INSEAD). Today, there are probably at least 10 or 15 departments with as many active researchers as London Business School and INSEAD had in 1987. In Asia, there really was not much serious finance research going on in 1987. Now there are at least three very good departments in Singapore, and four or five in both Hong Kong and in Seoul. In mainland China, growth in academic research activity has been so large that it is virtually impossible to keep track of all the good departments unless one lives 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 Ph.D. students graduated from the top ten U.S. departments. Now, the best students on the academic job market come from all over the world. European departments regularly place students at top five U.S. departments, and U.S. departments ranked outside the top 15 or 20 regularly produce extremely good students who land jobs at top departments. Students from Asian programs are getting better every year—it is only a matter of time before they are regularly placing at the top of the market like their European counterparts. 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, with researchers becoming increasingly specialized. The basic questions in any field remain the same, so people 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, it led to an important burst of research seeking to understand the causes of the crisis, the way new financial products can affect the economy and should be regulated, the way in which governments should intervene when financial crises ensue, whether banks should be allowed to be "too big to fail," and other 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. Backhouse and Cherrier (2017) point out that 10 of the previous 12 winners of the John Bates Clark medal, which is given to the top American economist under 40, mostly focus on empirical or applied work. This pattern is marked contrast to the early years of the John Bates Clark award, which was most often awarded for work in theoretical economics or theoretical econometrics.[6]

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 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, each worked in a number of different areas of finance.[7] When they were beginning their careers, academic finance was in its infancy 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 been able to make important contributions across the major

[6] For a list of recipients of the John Bates Clark medal, see https://www.aeaweb.org/about-aea/honors-awards/bates-clark.

[7] Economists will immediately recognize all these names. Noneconomist readers should be aware that of the individuals on this list, Fama, Merton, Miller, and Scholes all 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.

subfields. Most of us, however, specialize in one subfield or the other. In the generation after mine, scholars have become even more specialized, with a typical new PhD coming out of graduate school as a "macro-finance person," a "dynamic-contracting scholar,", or a "time series econometrician specializing in asset prices."

This specialization has, unfortunately, led to a situation in which scholars who do excellent work in one subfield of finance sometimes do not have 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 that finance is heading in the direction of many other fields, in which scholars in the same department cannot understand each other's work.

The Publication Process

In contrast to some other fields where the most important publications can be books or conference proceedings, in the economics-based fields, by far the most important method of disseminating research is through refereed journals. These journals differ substantially in both their quality and for the style of research that they tend to publish. Higher-ranked journals are much more presitigious, and many departments only promote faculty if they can publish in the few journals that they consider to be "top-tier". Consequently, the ability to publish in top journals is an important element of a 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 to be "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*, remain the same 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.[8]

In finance, we have the same three "top tier" journals as we had in 1987: *The Journal of Finance*, *Journal of Financial Economics*, and *Review of Financial 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*. Each of these journals publishes more papers than it 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 comparable ones from related fields.

Whatever the reason—a topic, not surprisingly, about which academics love to speculate—journal reputation is extraordinarily sticky. 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, there was an influx of Japanese and other companies that produced better cars, which in turn led American manufacturers to increase quality. This pattern regularly occurs in many different industries; its existence 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 to be "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.

[8] See Card, D., and Della Vigna, S. (2013) "Nine facts about top journals in economics," *Journal of Economic Literature*, 51(1), 144-161.

Changes in Academia and the Research Process

How has the research process been affected by the changes in the publication environment? One effect is that, since the contributions are narrower and more specialized, the pool of potential reviewers for a given paper tends to become smaller. Smaller pools of reviewers increase the potential for politics and the creation of cliques. In some subfields, these politics lead reviewers to be more positive if they seek to promote their subfield (benefit those in the clique); in others, where various turf wars rage, 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 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, then 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 depressing: an academic research career is becoming a more and more difficult way to earn a living. However, academia is a wonderful profession in which one can have a fantastic life. We can contribute to society in any number of ways, by educating good students, increasing humanity's body of knowledge, and providing insights that could improve public policy. Tenure provides us with the ability to present unpopular ideas without worrying about retribution from bosses. Our friends in the private sector often become 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 Ph.D. or faculty member survive and even thrive in this environment?

There are always factors that are out of one's control that affect one's success. But there is much that can be done to advance one's career, often in ways that might seem obvious, but often ignored by

young academics. It is somewhat ironic that in business schools, we spend much time teaching our MBA students how to improve their career prospects, but we spend little time thinking about our own.

Faculty often pursue haphazard research strategies. Some start too many papers, others not enough. Some essentially rewrite the same paper over and over, while others constantly start papers in many different subfields and never publish any of them. There are many other correctable mistakes that academics commonly make 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 refer to a production function as a way of characterizing the way a producer can convert inputs (materials, labor, capital, etc.) into outputs. Formalizing this production process helps economists to study firms, as well as the markets in which they operate.

But the notion of a production function is much more general, and is a useful way for academics to understand the way that 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 24 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 manage one's workload. Personally, I try to work really hard on one paper at a time, return it to my coauthors, and then

focus on another paper while the coauthors taking their turn editing the paper. That way, I can work diligently on a number of papers simultaneously.

Nonetheless, there is a limit on how many papers 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. It can be a serious mistake to commit to research projects that exceed that capacity. Some scholars constantly start new projects and work on many papers at once. The result is often that these scholars have frustrated coauthors and produce sloppy work, with many research projects never getting finished. 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 needs to set very specific goals, such as customer acquisition, development of beta versions of software, a date at which point the firm is profitable, etc. Such plans can be thought of as a route to become a successful firm in a specified (but narrow) sector of the economy. Entrepreneurs, even successful ones, do not always end up following the plan. Often, the plan ends up being overly ambitious; even if the firm makes good progress, it is 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 her or him to have a very good reason to depart from the original plan.

I see no reason why young academics shouldn't follow 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," showing what has been learned about the question, what hasn't, and perhaps the reasons 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 end up looking like the plan you made, but having such a plan is likely to make you happier with the output you produce.

I realize 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 young scholars to think systematically about where their research is going and how they are going to get there. Many young scholars proceed in a rather haphazard fashion, searching kind of randomly for whatever topics happen to occur to them. I know this problem well, because I was like that 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 Things

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. When a scholar starts a research project, there is a problem she does not know the answer to, and she comes up with a way in which she hopes will 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. She will go down blind alleys, having to retrace her steps when that approach does not work. She will actually have to write the paper, doing so in a way that others can understand what she did, why she did it, and what she found. She has to present it in seminars and deal with people questioning her analysis, often doing so with less than the usual social graces. 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 that one enjoys being questioned about the 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. More likely though, the author will 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 the paper is? And worse, because referees are anonymous in many fields, they often feel no constraints about being harsh (the 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 a temptation to throw up their hands and start another paper. After all, starting papers is fun but finishing them can be painful. **DO NOT GIVE INTO 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 spend a fair amount of effort explaining why the result is important and why a reader should care about it. The key to success in the face of negative criticism is persistence. She has to learn how to understand a paper's contribution and elucidate clearly in a manner to which 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. Further, after a paper has spent a few years as an unpublished paper, one's peers

will stop feeling an obligation to cite it. In other words, both the paper (which ends up having 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." This attitude is a mistake. There are many journals and your paper will have a much larger impact if it is published in a good one than not published at all, even if that journal is not considered top tier.[9] Many good papers never get published because the authors lack the persistence to see it through, or because they do not understand the paper's contribution and limitations, so 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 way that brilliant scholars can regularly behave like five-year olds. With the advent of social media and the web, every mistake one makes not only risks becoming 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 has seen. Or a prominent faculty member writes a paper that cannot be replicated, and the entire profession soon knows the story, leading the faculty member's reputation to be damaged.

When one is a faculty member or even a doctoral student, one must remember to be a professional scholar. The standards about anything related to one's job are much higher for professionals

[9] Not everyone agrees with me on this point. One of my favorite coauthors commented that: "Pushing a paper through in a below top-tier journal often takes a great amount of time as well. Yet papers on 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 we can put the time into a more promising project."

than for amateurs. This is true in all professions; it is fine for economics professors to go to a bar and karaoke out of tune, but for a professional singer, a tape of such activity could be harmful to her career.

As academics, we are on display all the time, whenever we discuss anything related to our specialty. If we produce a result and post it publicly, we have to make sure it is correct. We double and triple check the code before posting. Once it is online, it is there forever and people can (and will) find it. Everyone makes honest mistakes, but too many mistakes, even honest ones, mean that people will stop believing anything we do. If we blog or tweet, we try to do so intelligently. If we say things in social media that do not stand up to the standards of logic that one expects 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?[10]

So, why does one do academic research? 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. There will be drudgery, cleaning data answering referee reports, doing your social personal and professional duties.

You should also have a bit of that ambition and 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 impact, not to have someone remark at your funeral 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% of people on this planet, being told to "Go to your office and think up

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

something great" is a paralyzing and terrifying mandate. Most people need to be told what to do, told when to work and told it's ok not to work, 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 it — who love playing with the ideas, and who love the hard process of refining them, writing them, presenting them, and interacting with others about them — actually produce good work. You should only do academic scholarship if you love it.

Part 1: Topic Selection

Chapter 2: Selecting Research Topics

Before one can begin a research project, one must first find a topic to study, and a specific question within the topic to address. Coming up with good research questions is often the hardest and most important part of a researcher's job. A good research project should address an issue that is not completely understood by the existing literature, and one for which the researcher has a chance of contributing to. It also should be something that the researcher finds interesting and fun, and fits well into her personal research portfolio.

Scholars often ask me for advice about how to pick research topics. I usually answer that one should pick research projects in the context of her broader research goals. While there is no magic formula for picking research topics, there are general principles that can aid a researcher in picking projects that are likely to be successful. One can and should think systematically about the research projects she undertakes.

The choice of any research project should be predicated on a researcher's background, knowledge, and prior research history. The quality of a research project is individual-specific; what is a good project for one scholar might be a bad one for another one, even if they are in the same field. For a number of reasons, research projects are more valuable when they are part of a stream of related research. The overall importance of a series of projects tends to be greater when they complement each other.

It is important for every researcher to define his own identity in terms of the set of issues and methodological approaches that she wishes to explore over a relatively long period of time. These issues are known as a "research agenda" or "research program". Once a scholar has a moderately well-defined research agenda, she can pick individual projects that fit into this agenda. Occasionally, it makes sense to depart from one's research agenda if one comes across sufficiently high-quality research opportunity. But

as I will discuss in detail below, these departures should be the exception, and the majority of one's work should result from a coherent research agenda.

I think of the issue of picking research projects as a two-part process: First, a researcher needs to define her own research agenda, the long-term set of goals she wishes to accomplish with her research. Second, conditional on a research agenda, a researcher needs to select individual research projects. Each of these choices can be difficult, but these choices ultimately have a huge impact on one's career, so should be taken extremely seriously.

Why Specialize?

Most graduate programs, regardless of field, are designed to expose students to most of the important work in the field, including the major subfields. A good student will find many of the topics she learns to be interesting and is likely to have ideas as to how knowledge can be extended in a number of them. Many students do not know exactly what they want to specialize in, so begin research projects in a number of different areas. This process is the way that most students are introduced to research.

Nonetheless, for a number of reasons, it is usually advisable for researchers to specialize and focus on a relative narrow set of issues in their research. First of all, in any literature, there are usually many papers that have already been written. To make progress and to further advance the literature, one needs to know these papers extremely well. Moreover, in any literature, there are always issues that participants know about that are not easily learned by reading papers. There can be papers published in top journals that are not taken seriously because participants in the literature know that there is something wrong with them. Sometimes, there is an obvious approach that has flaws once it is examined more closely; a new researcher in the literature might find out the hard way by spending time on the approach and realizing that it does not work. The learning curve to do first-rate research in any literature is steep, and it is doubly hard to keep up when one works in multiple literatures.

Once one has done research that has been recognized as a contribution to a literature, it is often easier to make subsequent, related contributions. Often there is a natural "follow-on" paper to the first

one. Sometimes it turns out that the project that one was originally working on was interesting but not as interesting as a second idea the research had while working on the first one. Once a scholar has made an investment in the first paper, she has a comparative advantage in writing subsequent related papers.

In addition to the advantages in the production of new research by specialization, there are other advantages to research specialization that occur in the marketing, distribution and eventual impact of the research. Once a paper is written, an important task of a researcher is to market the paper to others in the field. It is not enough to do the research, a scholar must communicate to others what you have done and convince them that the work is important. She should present the paper at other universities and at conferences, and figure out how to get others doing related work to pay attention to it. The sad fact is that people pay more attention to the work of people who are well-known in a particular area than by others, even if the others are high quality researchers and their papers are good. All other things equal, work inside a scholar's area of specialty is likely to receive more attention and citations than work outside the scholar's main area.

Finally, the politics of academia favors those who specialize. At Ohio State I think we are fairly typical in that when we hire new faculty, usually we wish to hire someone in a particular area of need. We judge them by whether they have the potential to be a well-known scholar in that area. Of course, it is nice if they have additional work outside that area. But the most important thing is that the candidate has a body of related work to demonstrate to us that they are likely to become an important scholar in that area. So, if a candidate has a second paper related to their main paper, it would usually benefit them more than if they have a second paper that is in a completely different area.

When it is time to consider faculty for promotion and tenure, it again is to the candidate's advantage if he is at least somewhat specialized. An important part of the process is letters of evaluation from scholars outside the university. It is much easier to get strong letters from outside scholars if the work is relatively concentrated. When asked to write outside letters, letter-writers almost always prefer to be already familiar with the bulk of the candidate's work before sitting down to write a letter of evaluation, which is usually time-consuming and not compensated. It can be awkward if the letter-writer

says that he knows one paper but the remainder are far from his area of specialty so cannot comment on them. Since letter-writers are often besieged with requests for promotion letters, sometimes getting more than 10 requests in a given academic year, they are often unable or unwilling to spend much time learning much work that is new to them for the purpose of writing a tenure letter.

For all these reasons, I usually encourage young scholars to try to focus their research around a coherent research program, especially when they are starting out. It is much better to focus one's research around a big issue such as "The way that machine learning affects advertising," or "The causes and consequences of countries adopting the euro" than to write a series of unrelated papers. Research programs do not have to last forever; scholars' interests change over time and particular fields go in and out of fashion. Most of us have a number of different research programs over our careers, and sometimes find it enjoyable to work on projects outside our major research programs. But at any point in time, it is probably advisable to have most of your work concentrated around a group of somewhat related projects.

Be an Academic Hunter, not an Academic Farmer

A very popular guest speaker who regularly guest lectures in my class used to be a partner in one of the most famous venture capital firms in Silicon Valley, and now manages a local fund. This fund provided financing for some of the tech companies most readers probably deal with on a daily basis. One thing he always tells the class is that at his Silicon Valley firm (and his current firm as well), "We are hunters, not farmers". What he means by being a "hunter" is when their firms make decisions about which markets are likely to take off, they map out all the subsectors of that market, decide the characteristics of the firm they would in principle like in each subsector, and actively search for such a firm. Sometimes such a firm does not exist so they create it themselves. Once when his firm found two MIT PhD students who were working on a technology that fit what they thought would be valued by the market, they approached the students and convinced them to start a company which they (very profitably)

funded. Another time they tried unsuccessfully to convince existing firms to use an approach that they thought would revolutionize an industry, so created their own company to do so.[11]

The alternative approach, which is the more traditional approach to venture investing, is what my guest speaker calls "farming". This approach is more passive and means that the venture capitalists sorts through the thousands of business plans that happen to cross a venture capitalist's desk, and invests in the best ones. Many venture capitalists follow this approach and are successful. These venture capitalists do focus the types of companies in which they invest to certain industries and ages of companies (early stage or more mature companies). But they are less aggressive at seeking out companies that do not contact them, and are more likely to invest in whatever firms they happen to know about through personal contacts.

How does this discussion of venture capital relate to academic research? I think that since venture capital firms and academics both make risky investments in innovative projects, many of the same principles apply. An important difference between professional investors and academics, however, is that professional investors spend a lot of time thinking about the investment process and academics tend to follow a more haphazard approach.

Almost all academics are very smart and have learned the material taught in the courses they took as students. But they often have different strategies for coming up with research ideas. Some people get ideas from papers they read or hear at seminars. Perhaps there are mistakes in them that need fixing or extensions that could be done. Or they rely on lunch conversations with their colleagues or wait for friends to suggest projects they can coauthor. This approach might be called what my guest speaker would refer to as "farming". Many successful research careers have evolved through strategies like this.

The key distinction is that a hunter knows what she wants to accomplish, what questions she wants to address and what methods she wants to use. Hunters are proactive. In contrast, farmers are reactive. They are motivated by others' work, perhaps fixing mistakes or providing extensions. Or they

[11] This company went public around the time of this writing with a valuation well into the billions of dollars.

will join a bunch of unrelated projects with their friends and get publications that way. Sometimes, this distinction is not clearcut; for example, a conversation with a colleague or a reaction to an important paper can lead to a lifetime of work. I think, however, that it reflects an important distinction in the way that academics think about their research.

I believe that most of the most successful scholars have a well-defined research program usually centered around a specific question or research approach. It is not usually a response to others' research but instead a scholar's drive and search for answers that motivates the best research. The scholar will "hunt" for the truth wherever clues can be found and will usually pursue this research program with extreme vigor and single-mindedness. The research becomes a goal itself rather than a means to an end. In other words, the most successful researchers do not do research because they want to get promoted or become famous or increase their salary; they do research because they want to know the answer to the questions they ask.

Two Particularly Successful Research Programs

There are many successful academic researchers who have adopted such a "hunting" strategy. I will provide a short discussion of two particularly successful ones to highlight the importance of a targeted and specialized research strategy. Each could be described as "hunters" as they spent their career seeking out novel approaches to problems and rather than focusing on problems that happened to be available to them.

One such scholar is Daniel Kahneman. His book describing his work (*Thinking, Fast and Slow*) is one that every scholar should read, regardless of their field.[12] Kahneman is a psychologist who, together with his longtime collaborator, Amos Tversky, spent his career studying the way that humans make decisions. In an influential series of experiments, Kahneman and Tversky presented compelling evidence

[12] See Kahneman, Daniel, *Thinking, Fast and Slow*, Farrar, Strauss and Giroux, 2011. For an entertaining treatment of Kahneman and Tversky's lives and the relationship between the two, see Michael Lewis' *The Undoing Project*, which was published by Norton in 2016.

that human decision-making can be characterized by what they refer to as "prospect theory", which departs from rationality in predictable ways. This work has revolutionized both psychology and also economics, in which prior to their work, analysis always started from the presumption that individuals were rational.

The thing that struck me when I read Kahneman's book, aside from the brilliance and importance of the work itself, was the way that Kahneman and Tversky went about doing their work. They started working together after Tversky gave a guest lecture in Kahneman's class. What is surprising to many is that Kahneman absolutely hated Tversky's talk. The two of them ended up arguing about the issues in the lecture endlessly and eventually came up with experiments that enabled them to distinguish between their alternative views. The issues raised in these discussions, about the way individuals make decisions, became each of their life's work. Within this broad research agenda came many individual projects, which often involved other coauthors as well.

Kahneman and Tversky's research agenda came from their disagreements, both with each other, and also with the prevailing "rational" view of behavior. They wanted to know how and why people behave the way they do. What they did not do is to ask "What are the journals publishing nowadays?" "What issues are hot?" or "What issues can we get research funding for?" The impetus for the work was their desire to understand the question of how people make decisions, not the rewards that would ultimately accrue to its authors. Kahneman and Tversky started with a question, hunted for the answer to that question using approaches that were unconventional at the time, and were ultimately rewarded for it.

While Kahneman's research centered around one particular question, other very successful researchers have centered their research around particular methods or styles of answering questions. One such scholar whose work follows this pattern is Alan Krueger. Alan Krueger was a labor economist whose unique style of research led him to a number of important insights.

Traditionally, labor economists have relied on large publicly available databases to draw inferences about labor markets. Krueger sometimes used this approach but more often collected his own data, usually designed to address a particular question in a "causal" manner. Krueger and a group of like-

minded labor economists who often coauthor together have popularized a number of methods that allow for drawing causal inferences. The issue of how one makes causal inferences from real world data is a key issue in economics (and really all the social sciences). His research program was to use unique and often hand-collected data to draw causal inferences about some of the most important issues about labor markets.

Perhaps Krueger's most celebrated (and controversial) paper was coauthored with David Card and concerns the effect of the minimum wage.[13] Every economics textbook has historically taught that the minimum wage leads to lower employment because the higher wages lead firms to substitute away from low-priced workers. Card and Krueger challenged this view by analyzing what happened when, in 1992, New Jersey raised its minimum wage and neighboring Pennsylvania did not. They found, counter to the standard textbook prediction, that employment at fast food restaurants which pay minimum wage increased more in New Jersey than in Pennsylvania. This finding has generated an incredible amount of interest, and occurred because of Card and Krueger's strategy of actively seeking data that provide insights about an important question.[14]

An important methodological concern in economics is called "unobservables"; for example, if one is interested in the returns to getting more schooling, one cannot tell if the schooling *caused* better outcomes or if the better outcomes are associated with more schooling because more talented people go to school longer. To address this issue, Krueger, together with Orley Ashenfelter, gathered a sample of identical twins, whose DNA is exactly the same.[15] They actually went to a convention of twins, in a place called Twinsburg, Ohio and interviewed the twins themselves about their education, life history and earnings. With these data, they were able to provide estimates of the returns to schooling that are not

[13] See David Card and Alan Krueger (1994) "Minimum Wages and Employment: A Case Study of the Fast Food Industry in New Jersey and Pennsylvania," *American Economic Review*, 84, 772-793.
[14] Card and Krueger's findings are not universally accepted. For an alternative view, see David Newmark and William Wascher (2006) "Minimum Wages and Employment: A Review of Evidence from the New Minimum Wage Research," NBER Working Paper 12663, and David Newmark and William Wascher (2008) *Minimum Wages*, MIT Press.
[15] See Orley Ashenfelter and Alan Krueger (1994) "Estimates of the Economic Return to Schooling from a New Sample of Twins," *American Economic Review*, 84, 1157-1183.

subject to the unobservables bias. Like the previous example, Krueger's strategy of actively seeking new data from creative sources to identify causal answers to fundamental questions yielded a seminal contribution.

How to Develop A Research Agenda

Perhaps it is a bit unfair of me, when discussing the importance of a research agenda, to give examples of academics who are among the most successful in their fields. A reader might think that they were obviously brilliant and would have been successful no matter what strategy they pursued. But in academia, there are many brilliant people, and very few are as successful as Kahneman or Krueger. What distinguished them is that they knew what they wanted to learn from their research, they each developed a style of their own, and were able to make a substantial contribution to our knowledge.

Most young scholars enter graduate school with a vague idea of what they want to study but no particular research agenda in mind. Coming up with a coherent research agenda is possibly the most difficult task facing a young researcher, but might also be the most important. One's research agenda defines who she is as an academic. It is what a scholar spends the majority of her time working on and what others think when her name comes up. When starting an academic career, a scholar should spend substantial effort thinking about what she wants that agenda to be going forward, and how she will go about achieving it. In addition, as she gets older, she should always be asking herself these sorts of questions about her research agenda: What about it is still exciting? What is getting done over and over again (by you and others) and is becoming boring? Are there new areas I would like to explore and are they worth the upfront costs?

Personally, I entered an economics Ph.D. program with little knowledge of finance. I was an undergraduate mathematics major who had taken a lot of economics courses, and was a research assistant to a number of faculty in the mathematics, economics, and finance departments. What I did know about finance was through my work as a research assistant on work about option pricing, which is an area of finance that I ultimately chose *not* to focus my research on.

When I entered graduate school, I was interested in economics and had the skills to succeed at it, but did not really know what I wanted to study. Motivated by the 1980s takeover wave that was occurring while I was a graduate student, I wrote my dissertation about corporate governance. This work led to research in more mainstream corporate finance, and after I moved to the University of Illinois in the early 2000s, to an interest in private capital markets and the way they finance new companies and buyouts of existing companies. My research agenda evolved based on what I happened to be interested in at the time, which is probably fairly typical of many academics' careers.

How does one get started on a research agenda? The answer varies a lot by field; in some fields one is limited by the lab facilities in one's university or by the knowledge and interests of the faculty in the department. In those fields, a student might enter graduate school receiving funding from a particular professor so that she could do work in his lab doing research he directs. Or she could have such specialized interests that she knows with fairly high certainty which faculty member will be her advisor. But in most departments in the economics-oriented fields, the burden of finding a research program is on the student, or young faculty member. Sometimes one's research program mirrors that of one's thesis advisor or mentor. However, the mark of a successful scholar is independence; a scholar should try to have her own unique approach that is somewhat different from her advisors. Her goal is to make contribution for which she will be known individually.

I have always encouraged my doctoral students to take advantage of their unique knowledge and talents, and to pursue a research program that takes advantage of one's natural comparative advantage. To use a bad sports analogy, if one happens to be 7 feet tall, it is usually not such a good idea to play shortstop, since focusing on basketball would probably lead to a better outcome. Thinking about the topics of recent doctoral dissertations I have helped to advise, in most cases, the students have, with my encouragement, taken advantage of their own individual strengths. One student worked on Wall Street prior to graduate school and had a deep understanding of the way loans are securitized; he wrote a fine dissertation on this topic and had a successful research program focusing on securitization. Another student had the ability to pick up Bayesian statistics and related programming as easily as most of us learn

basic algebra; he eventually wrote a fine dissertation taking advantage of these skills. Most recently, a student came into the program with connections and knowledge of real estate funds and started a research program in that area. The point is that all of these students and many others have started their careers in good shape by finding a research agenda that matches their skills and interests.

However, one should not feel limited by what one knows when one enters graduate school. The real purpose of graduate school is for students to pick up new interests and skills that they did not have when they arrived on campus. Students should take advantage of opportunities to learn new areas and to develop new skills. A good strategy for deciding what to learn is to observe what the best young faculty are working on, and the skills they have using.

If you are one of those students (or faculty) who cannot seem to focus their research ideas around a central theme, I would encourage you to think about the following kinds of questions: Why did you enter the program in the first place? What about that field was so exciting that led you to go into it? Is it still exciting? Are there a number of unsolved issues that you can address? Ultimately, you want to find a niche where you can contribute something beyond what the profession already knows, preferably about something sufficiently important that the profession will care about what you do.

One approach to starting a research program is to think of a classic puzzle that the literature has been considering for a long time. While sometimes celebrated research comes from this approach (Fermat's Last Theorem), it often can be difficult to make progress on such topics. As John Cochrane puts it: "There are thousands of papers, they probably thought of any idea you might have, and you'll have to master them before you publish."[16] Cochrane also cautions against extensions of well-known papers for the mere purpose of generalizing them rather than drawing any new implications from them: "I'm going to add recursive preferences and estimate the Campbell-Cochrane model on Korean data."

Another approach is to focus one's research on large events that are going on in the world and their implications for economics or whatever field one is studying. As I write this paragraph, the world is

[16] The quote is from his review of this book for Princeton University Press.

in shutdown from the Covid-19 virus, and academics have been besieged by many papers inspired by the virus. These papers were put together remarkably quickly – some papers have been *published* in 2020 using data generated earlier in the same year.[17] Similar runs of papers occurred following the 2008 Financial Crisis, the dot com boom and bust, the breakup of the Soviet Union, etc. Some of these papers have had lasting impact but most have not. The trick when writing about the hot topic of the day is to try to focus on an aspect of it that will still be interest once the crisis is over and the world's attention is focused on something else.

While everyone has their own approach to picking research areas, I personally have found it useful to avoid the topics that that are the "hottest" at any particular point in time. I try to find topics that are of fundamental importance that the literature does not understand well, on which there are not too many people working at that time. For example, at the beginning of my career, I thought corporate governance fit this description. It is clearly an important issue, and I was one of the few economists focusing on corporate governance at that time. Since then, there has been a huge increase in the number of people working on corporate governance. While I am still interested in the topic, most of my research efforts have been focused elsewhere, although I do still write governance papers when I think I have a new approach to studying them. Recently, I have begun to focus my research on "private capital markets," which have become incredibly important in economy but for which the academic literature is still in its early stages. My bet is research that leads to a better understanding of these markets will yield important insights.

How to Pick Individual Research Projects

Selecting research projects to work on, especially at the beginning of one's career, can seem like a daunting task. A new research project has to have the potential to make a major contribution to the

[17] See for example the November 2020 issue of *Review of Corporate Finance Studies*, which is entirely devoted to papers about the Covid-19 crisis.

literature and to teach the profession something important that it did not previously know. This is a high bar, substantially higher than anything most young academics have previously cleared in their careers.

However, getting involved in new research projects can be one of the most fun parts of an academic's life. Coming up with new research projects to work on involves thinking about the big picture, what we know and what we don't know, and why the answers to particular questions are important. Most academics find thinking about these kinds of issues to be fascinating -- that is why we became academics in the first place.

Unfortunately, the beginning of a research project can also be incredibly frustrating. When one is searching for research ideas in a crowded field, others will be doing the same, so ideas that are obvious or easily executed tend to be competitive. Often potential projects that initially seem promising have to be dropped because they have already been done by others. In addition, often ideas that make sense turn out, on further investigation, to be infeasible. For example, they could require data that does not exist or is impossible for a researcher to access.

The best researchers seem to have a knack for finding influential research projects. They usually find questions that others find interesting and stimulate further work. But how does one develop skill at finding interesting projects? There is no magic formula for picking research projects. However, there are productive ways that one can approach the search for research projects.

A precondition to finding a good research project is a deep knowledge of the literature in which you want to work. A literature can be thought of like a conversation; to contribute productively to it, one needs to know what has been said previously. Without knowing in detail what has already been done, it is impossible to gauge the extent to which any potential contribution extends the literature. Specialization and a focus on a particular research program facilitates the acquisition of the knowledge necessary to understand the importance of any particular contribution.

Once a scholar has settled on a particular research agenda, she should try to take a step back and go through a process like the following: First, ask herself what big picture questions the literature tries to address. Make a list of all conceivable questions. Sometimes literatures become obsessed with one

particular question when an equally important one (or sometimes even a more important one) is unaddressed. Then go through what has been done already related to these questions. What do we know? What don't we know? What techniques have been used so far? Is there room for improvement? What could I add? When going through this process, she should always remember that the best papers always come about when the author does work about an issue that she thinks is important, and not when she works on things that she thinks others will be impressed by.

I recently had a bright doctoral student come to my office with an idea I liked. It was in an interesting area that was relatively new, and he had a general approach that could potentially yield new insights. But I thought that the particular question he suggested was not as important as other questions he might have asked. I suggested that he do what venture capitalists do, and make a "market map" of all potential topics in the area. I advised him to list all of the issues that one might research, all the papers that have addressed any of them, and see what new he could add. I am hopeful this approach will help him come up with an interesting dissertation topic.

It is important for every researcher to understand the nature of her potential sources of value added. Does she bring technical skills to a problem? New data? Or simply a more creative approach? A deeper understanding of the literature or the institutions you are studying? Often, especially for younger scholars, the most important thing they can bring is their time and energy. The value of time and energy should not be underestimated; senior faculty can often be pressed for time and skip steps or skimp on data work. A younger scholar can sometimes improve existing work by being more thorough and spending more time thinking about the underlying issues the research is studying.

One should also think about the style of work she is most comfortable doing. Sometimes academics write papers that reexamine others' work, sometimes using more advanced techniques. For example, the James Heckman, the 2000 Nobel Prize winner in Economics, together with Steven Durlauf, published a paper criticizing the analysis in Roland Fryer's well-known study of the effect of racial

differences on the use of excessive force by police.[18] Assuming that Durlauf and Heckman are correct, their analysis can materially affect the way we think about racial disparities in policing, which is currently one of the most important and controversial issues facing the United States. Methodological critiques such as Durlauf and Heckman's are an important part of scientific analysis, and help to ensure that the profession interprets research correctly.

Personally, this approach to research has no attraction for me. I much prefer examining new questions than finding mistakes that others have made. But others love this approach and have made successful careers using it. A scholar has to find an approach to research that works for her that is a good fit for her personality and skills. Otherwise, she won't enjoy doing the research, her heart won't be in it, and the research will tend to be of lower quality.

One final thought about selecting research projects is that scholars should always try to ask questions for which the answer actually matters, which often are not the cleanest or most straightforward to address. Steve Kaplan, a well-known scholar in my area, often tells his students that if one wants to study elephants, then study the whole elephant and not a pimple on the elephant's skin. What he means is that a researcher should try to understand all aspects of a problem. By this metaphor, he is implicitly critical of scholars who examine only the one aspect of a problem that is easiest to study, while ignoring larger but potentially messier issues. Taking on larger but potentially more complicated issues can lead in the long run to research that is more interesting and useful to readers.

[18] See Roland G. Fryer, Jr. (2019) "An Empirical Analysis of Racial Differences in Police Use of Force," *Journal of Political Economy*, Vol. 127, pp. 1210-1261 and Steven N. Durlauf and James J. Heckman (2020) "Comment on Roland Fryer's An Empirical Analysis of Racial Differences in Police Use of Force," *Journal of Political Economy*, Vol. 128, pp. 3998-4002.

Chapter 3: Strategic Issues in Constructing Research Portfolios

When I was a new assistant professor, I was fascinated by all areas of economics and finance. I worked on a lot of different topics, most of which were somewhat related to one another. But I never had a "plan" and never thought very much about my research portfolio, how it fit together and how it was perceived by others. Things have worked out fine for me, but looking back on things, I would have benefited if I had thought a little about how to optimize my research portfolio to maximize its impact on both the profession's knowledge, and also on my career.

This chapter will discuss factors that I probably should have thought more about when starting my career and deciding on which research project to pursue. I will first discuss how research is evaluated and the way that this evaluation process should affect the way research is conducted. Then I will discuss the way to think about the costs of a research project and why it is a mistake to start too many projects at once. Finally I will provide some guidelines for selecting projects to optimize the impact of a research portfolio, with special attention to issues such as the stages of projects one should work on at any point in time, the question of when to abandon projects, and the choice of coauthors.

How Research is Evaluated

Before one can understand the way to optimize one's research portfolio, it is important to think about the way in which research is evaluated. Both the methods of research and the way it is distributed varies from field to field. In the humanities, a scholar's value is largely determined based on the books that she publishes. In the sciences and engineering the amount of money a scholar brings in through research grants is extremely important. And in the social sciences, at least the economics-based ones, by far the most important factor in evaluating research is the scholar's refereed journal articles.

Universities try to have at least a pretense of common standards across areas. Consequently, they often claim that they value research in all fields based on its "impact", regardless of whether the research

is distributed through journals, books, or conference presentations. However, the way research is determined to be impactful varies across universities, across departments, and even across individuals evaluating it within a department.

Some departments determine a paper's impact solely by the journal in which it is published. In these departments, junior faculty are often told an explicit rule stating how many papers must be published, and in which journals, for them to achieve tenure. A journal's quality, however, is a very noisy measure of the quality of the papers the journal publishes. The refereeing process is imperfect and can be political. Sometimes bad papers are published in top-ranked journals, and important papers can be rejected by more than one journal before they are finally published.

In economics, there are many seminal papers, some of which led to Nobel Prizes for their authors, that were rejected by at least one journal.[19] Often these papers were rejected because the referee or editor did not see the paper's point, or was closed-minded about accepting new ideas. Nonetheless, all of these papers were eventually published somewhere and their contributions recognized. We have no way of knowing how many other brilliant papers were never published and their ideas lost.

For this reason, when assessing research quality, higher ranked schools tend to rely more on citations and careful reads of the paper themselves rather than relying on the reputation of the journal in which the work was published. In general, the better the department, the less it emphasizes counting papers and weighting by journal quality, and the more it tries to measure the paper's impact indendently of where it was published. The very best departments regularly deny tenure to scholars who publish many papers in top journals if none of these papers are judged to be sufficiently pathbreaking.

Academics often debate the way in which one should measure research quality, and usually do not agree on an answer. Nonetheless, it is important for any scholar to have some sense as to how

[19] For a list of some such papers and for some interesting discussion about their history and how rudely some seminal papers have been treated, see "How are the Mighty Fallen: Rejected Classic Articles by Leading Economists," by Joshua S. Gans and George B. Shephard, *Journal of Economic Perspectives*, (1994) Vol. 8, pp. 165-179. It is a good idea to keep Gans and Shephard's list of rejected papers handy, as it can help keep one's spirits up when your own paper gets rejected.

research quality/impact is measured at their university and at other universities for which they might someday work.

As economists we study the way individuals in the economy respond to incentives, and we should always try to respond to the incentives we face ourselves. If your department asks you to write a specified number of papers and publish them in particular journals, then you should try your best to meet the expectations that the department has laid out. But at the same time, you should always remember that the underlying goal is to produce scholarship that has a lasting influence on the way people think about important issues, and not let the short-term pressure to publish *n* papers in a few specified journals distract you from that goal. If the idea in your paper gets "legs" and it influences others' thinking, you will eventually be rewarded for writing it regardless of where it is published.

How Evaluation Impacts the Research Process

A young scholar has to, to some extent, cater to the whims of the refereeing process and try to do work that is accepted by the profession. However, I do not mean to imply that the *only* criterion one should have for measuring the quality of a potential research project should be whether the resulting paper has a chance at being published in a journal your university values highly. As discussed in Chapter 2, I encourage young scholars to be "hunters" and to set their own research agenda. Sometimes setting one's own agenda means doing work a little outside the mainstream. Unfortunately the most innovative work can be the most difficult to publish.

A scholar's goal should be to do innovative work *and* to publish it in a good place. When one thinks about starting a new research project, it should advance one's research agenda and have a strong likelihood the paper will be publishable in a journal valued by one's university. If the approach is somewhat different from the mainstream, it is extra important that the scholar go out of her way to explain exactly why she is using this approach. Especially important is that she is clear about the way that the paper relates to the mainstream literature and improves on it. Most importantly, for a paper to be impactful, it must explain what is learned from this paper that we did not know before.

The impact of any paper and therefore its value to the authors will be determined by the profession's belief about the importance of what is learned from the paper. Often academics complain that these beliefs are subjective and that the process *should* be more objective. I put *should* in italics because it is such a funny word to describe the process of evaluating research. Even though academics often make this complaint, there is almost no way that any evaluation of the importance of a particular research paper could possibly be anything but subjective. What is important to one reader will undoubtedly be trivial to another.

For the vast majority of papers, their impact will depend crucially on the authors' ability to explain the paper's contribution and why it is important.[20] Sometimes authors themselves do not really understand all the implications of their work and do not have a sense as to whether the profession will find their results interesting. The ability to understand a potential paper's contribution before starting to work on it is an important skill that tends to be learned over time.

An exercise I recommend to authors is before they start working on any research project, they should think about their best guess as to what the results are likely to be. Then they should try to write a three to four page introduction to the paper assuming that the results are as anticipated. The ability to write such an introduction is likely to be related to the author's understanding of the paper's contribution. If she can write an introduction easily and can convince her friends that the paper will be interesting assuming the results come out as expected, then the project is probably worth proceeding with. If, on the other hand, she has trouble writing an introduction that others find interesting before she does the analysis, she probably will also have difficulty motivating the paper after she does the analysis. Such a paper is unlikely to be influential, since readers will have trouble recognizing a paper's incremental contribution if the authors cannot explain clearly what it is.

[20] Exceptions are papers like the proof of Fermat's Last Theorem in which the contribution is obvious to anyone who reads it. If you can write papers like that one, my recommendation is that you stop wasting your time reading this book and get back to working on your research.

An issue an author should consider when she writes this first draft of an introduction is the extent to which a potential paper's interest would depend on what the results turn out to be. Often the best projects to start, especially for a graduate student trying to write a dissertation, are ones for which are interesting no matter what the results are. Papers that provide a "horse race" between alternative theories can be excellent dissertations, since no matter which theory "wins", the paper will provide a contribution to the literature. However, some papers are well-known precisely because of the way the results turn out. Card and Krueger (2000) is famous because they found that the minimum wage did *not* increase unemployment, which is in stark contrast to what is traditionally taught in microeconomics courses. If instead they had found an increase in unemployment, the paper would still have made a valuable contribution, but would not have been nearly as impactful.

The Costs of a Potential Research Project

The potential benefits of any research project must be weighed against the associated costs. These costs can be financial, if one runs a lab and must pay human subjects, or if the project requires data purchased from an outside vendor. However, most of the time, the main cost is the researcher's time. A researcher's time is the most valuable asset she owns and she must learn to be judicious when allocating it. There are only 24 hours in a day, and in addition to doing research, we must sleep, eat, teach classes, have relationships, exercise, etc.

For most scholars, the scarce resource is not *total time*, but *intense research time*. When writing a paper, most people have to be completely focused on that paper to the exclusion of everything else. Random hours here and there are not as valuable as blocks of time when a scholar can avoid distractions. These blocks of time are precious. For many scholars, the availability of such blocks of time is an important factor affecting their productivity.

Everyone works in different ways, but it is important for every researcher to know the way she works best. These work habits together with a scholar's other commitments lead to what I call *capacity*. By capacity, I mean the number of research projects that one can productively work on at any point in

time. This number can vary substantially across people. I have very successful academic friends who can work on 10 or 15 projects at once, and other equally successful ones who can only work on one or two at any point in time. One's capacity also varies over their career; my capacity today is actually higher than when I was younger, in large part because my coauthors now usually do the time-consuming data work that I used to do myself.

Every scholar should know his own capacity and not commit to more projects than that number. When one overcommits beyond her capacity, work quality tends to suffer as does one's relationships with coauthors, who can feel taken advantage of. A researcher's capacity determines the opportunity cost of any additional research project and should affect which projects a scholar should take. If one is far from reaching capacity, then she should make an effort to find things to work on, even if they are a bit speculative or depart a little from her main research program. However, if one is near or at one's capacity, it is important to be extremely selective about what one commits to work on. One should always find a way to make room for a project that is sufficiently pathbreaking. But at the same time she must beware of the costs of overcommitment because they can be substantial.

Structuring Your Research Portfolio

A theme of this book is that much of what academics often do haphazardly could be markedly improved with a little thought and planning. Nowhere is this idea more relevant than in the structuring of a research portfolio. A research portfolio should be a series of research projects that collectively teach the readers something important. Ideally, they should be related to one another, and the value of them together should be greater than the sum of their individual values.

There are a number of issues one should consider when constructing a coherent portfolio. Here are some of the important ones:

Projects at Different Stages. The research process can be a long, laborious, and sometimes frustrating one. It begins when a scholar and her coauthors have an idea. For a while they probably bounce the idea

off of one another to ensure that they understand the issues completely. If she and her coauthors decide to pursue the idea, the analysis itself could be relatively quick or take a long time, depending on the nature of the work involved. Then they have to write the paper, circulate it, get feedback, circulate it some more, and present it as many times as possible. At this point, she and her coauthors might be ready to submit to a journal. The review process varies from field to field, but can be slow. Papers that are accepted usually have gone through a number of rounds of revisions, with substantial changes required for each round. In addition, most papers have to be submitted to several journals before they are accepted, each of which can take a long time between submission and a decision. Finally, if she and her coauthors are fortunate enough to have the paper accepted for publication in a journal, there is usually a substantial lag prior to publication. The entire process from start to finish usually takes a number of years.

Because the research process is so long and involved, it is often a good idea to stagger projects and to try to always have some projects at different stages. There are a number of reasons why staggering is a good policy. First, different parts of a project require different skills; for example, initiating a project requires creativity while when finishing up the publishing process, perseverance and attention to detail becomes essential. Second, the custom in the economics-oriented fields is to present papers at other universities and conferences in draft or "working paper" form. Once the paper is far along in the publication process, it is less desirable to present the work at conferences or other universities, since any suggestions from discussants or the audience could be difficult for authors to incorporate. It is a good idea to try to always have some work at the right stage for presentation where it is a complete draft that is sufficiently polished to present in public, but not so polished that it is ready for publication. A scholar should avoid being in a position where she has to decline speaking opportunities because none of her papers is at the right stage.

Third, papers at different stages require different kinds of work. A former student recently told me that as she was revising her job market paper for publication, she could not do the data work required for too long. But she found it possible and enjoyable to complement the data work with reading and thinking about potential new projects.

Finally, it is a good idea to always be starting new projects or at least to be thinking about ideas for a new one. Being sufficiently overwhelmed with revisions that one does not have capacity to start a new project often leads scholars to avoid thinking about new ideas and new ways of approaching problems, which in itself is a problem as it leads one's creativity to become stale.

Keep spare capacity. When deciding whether to start a new project, it is important to understand the costs associated with the project and take them into account in your decision. Depending on the nature of the research, the project will take time and resources, and will involve a time commitment to the others involved with the project. In addition, an important but sometimes neglected cost of a new project is that starting a project today could potentially limit one's ability to start future projects. It is frustrating when one is so committed to existing projects that she cannot start any new ones. As academics, we should constantly be thinking about new developments in our field, how they relate to our own work, and what extensions are possible. If a new research project limits one's ability to act on subsequent ideas because the project causes her to approach her capacity, this is a real cost that should be taken into account when making decisions about investing in a new research project.

Abandon bad projects. The idea of preserving research capacity should affect one's attitude toward existing projects as well. All of us have some projects that we start that at the time seem very promising, but eventually become less interesting than we thought they might be. It is important to reevaluate projects in a timely manner, and if a project is not worth doing on a *going forward* basis, then it might make sense to abandon the project. Once a researcher has put substantial time and effort into a project, she can become subject to the "sunk cost" fallacy, in which individuals make decisions based on costs that have already been spent and therefore should not be relevant to current decisions. If a project does not appear to justify the *subsequent* time and effort, it probably does not make sense to continue it.

Sometimes, however, it can be difficult to abandon projects, especially when they are coauthored. If a coauthor is subject to the sunk cost bias, or if she does not have a high opportunity cost of time, that

coauthor could potentially want to continue the project when it might make sense for you to abandon it (or vice versa). These situations can be difficult and there are not any easy answers. My advice in this type of situation involving coauthors disagreeing is to remember the importance of relationships in academia (and elsewhere). Sometimes it is worth making sacrifices to maintain these relationships.

Coauthors

Most work today is coauthored; finding the right coauthors can in large part determine the success of one's research program. I have worked with many more coauthors in my career than most people, 55 as of the time of this writing. Each has played a large part in my career. I have been extremely fortunate, as the vast majority of my coauthors have been wonderful. They are smart, interesting, hard-working, and fun to be with. Equally importantly, I have found a way to work productively with them, and most of the time, our skills have proved to be complementary.

How does one find good coauthors? There are no hard and fast rules but there are a number of things one should consider. First of all, coauthors should have overlapping interests and complementary skills. The project has to be of interest to both scholars and fit into each of their research programs. In addition, each coauthor should be able to bring something to the table for the match to work well. If both coauthors good at programming but terrible writers, then their paper is likely to be well-programmed but poorly written.

Second, it is important that coauthors are compatible with one another on a number of relevant dimensions. Of course, coauthors have to get along personally and have the same worldview about your approach to research. But there are other factors that matter as well. Some people are night owls and work best between midnight and 4 AM. I am more of a morning person, so I would probably have difficulty coordinating with someone like that. In addition, the *tempo* at which one works can affect how well coauthors work together. I am very intense when I work and like to do things immediately. Most coauthors like this aspect of my work habits, but some others prefer to work more slowly and to take a long time to finish things.

Third, it is important to consider the age and reputation of the person. Older, more established scholars can be helpful in a number of ways: they often have better perspectives on the state of the literature and how one could contribute to it, and are usually better at navigating the publication process. However, there are costs to working with more established scholars. They tend to be busier than younger people and also have lower incentives to complete the work in a timely manner. Consequently, they can sometimes take longer to do their part of the work, and sometimes can shirk and not complete it at all. Senior people have different incentives regarding the journal submission process; junior faculty's careers depend on publishing in top-ranked journals quickly while senior faculty would like to publish influential work but the timing is less important. Sometimes there are conflicts where junior faculty want to rush papers to journals too soon, while senior faculty want to wait too long.

More importantly (and often unfairly), no matter who does the work, the more senior person usually receives disproportionate credit for the research. People tend to assume, often incorrectly, that the senior person is the one who had the idea originally. The cost of working with senior people is especially high if it is evident that the paper is part of the senior person's research agenda, in which case readers will surmise (perhaps incorrectly) that the senior person is the driving force behind the research. It is important for a young scholar to establish her own research identity, and too much work with senior people can make it difficult to do so.

Fourth, you should consider whether a potential coauthor is good at getting work done and completing projects. There are many scholars who are smart, nice people with good ideas, but get bored quickly and move onto other things rather than finishing existing projects. It can be dangerous to start too many projects with people like that. A young scholar's career can become tied to a coauthor's ability and willingness to put in the time to complete projects, and also to shepherd the projects through the often painful publication process. If a senior colleague is a person that enjoys starting papers and hates finishing them, the coauthored work can remain a working paper for a long time, possibly forever. Sometimes young faculty are denied tenure because their senior coauthors decide that they would rather take yoga or mountain climbing more seriously than publishing their coauthored papers.

Fifth, and perhaps most importantly, a scholar's career becomes tied to her coauthors' in a number of ways. If the coauthor is sloppy or commits an ethical violation, it can become a major problem. For example, suppose a coauthor "accidentally" types "2.5" instead of "1.5" as a t-statistic on the variable you are interested in learning about. If this result is deemed sufficiently important, an outsider is likely to try and fail to replicate your "significant" result. You will be equally blamed with your coauthor, even if you had no idea about the cause of the mistake. You still lose if outsiders cannot replicate a result of a different one of your coauthor's papers, even if you have nothing to do with that paper. People will infer that if he is sloppy on one paper, then possibly all of his papers are equally sloppy. They will infer that there is a decent chance that the result in your paper is wrong as well. I sometimes tell students that choosing a coauthor is a little like choosing a spouse, in that you end up suffering for all of the person's failings even if you have nothing to do with them.

Despite these concerns, there is a reason why coauthoring has become the dominant way to do research in many fields. Usually there is value to be added by combining forces as people tend to have different strengths. In addition, it is more fun to do research with someone else. Some of my coauthors are my closest friends, with the friendships growing as we went through the research process together.

One thing that has changed in recent years is the ease of working with people who live far away. With the ability to talk via *Skype* or *Zoom* and to store files in the cloud using *Dropbox*, it is trivial to coauthor with scholars at other universities. It wasn't always that way; not that long ago we shared files by literally mailing floppy disks to one another (by regular US mail, Fedex was too expensive at the time). Today though, it sometimes seems easier to get in touch with my coauthors in Asia or Australia than with my colleagues who live a few minutes from me in Columbus, Ohio. Distance should not be a deterrant to working together with scholars no matter where they live.

Don't let others push you into topics.

An awkward moment sometimes occurs when a friend suggests a potential coauthored project that does not fit into your existing research program. Or a senior colleague who you would like to impress

suggests an idea that seems reasonable but does not align well with your existing work. Or you could be close to your research capacity and be concerned that committing to a new project could negatively affect your ability to finish your existing ones. Often you like the idea and you think that person would be an excellent coauthor. It can seem like a great opportunity that you do not want to pass up. However, there are some issues you should think through before you agree to work with her on the paper.

The costs of starting a project with this potential coauthor are the same as discussed above. The project will take valuable time away from your main research agenda. How wedded are you to your current research program? Are you looking to expand your horizons and move to a different area, or are you better concentrating on your main research area? Can these two areas of research be connected by a future research project? How much capital would you gain by coauthoring, or lose if you offend a colleague by not coauthoring? How close are you to your research capacity? If you are close, then the opportunity cost of starting the project could be quite high. These are some of the issues you should think through before agreeing to start a new project with a friend or colleague.

Some people view coauthorship the same way as they view dinner parties, as a way of socializing and improving professional relationships. While there are relationship-building aspects to starting a coauthored paper, they should not be the primary motive for starting a research project. Research is not a social obligation. Research is what we do for a living. Academia is a competitive world, and there are many others searching for ideas related to the ones we are studying.

To compete, one must devote all of her resources toward projects that maximize the impact of one's research. Usually the best way to do is to pursue a portfolio of research projects that are related to one another and develop the scholar's reputation and one particular area. Advancing our own research program is sufficiently difficult enough absent outside constraints. If a friend's research interests coincide with your own, then it can be wonderful to work with one another. The key is the project; one should never sacrifice the best possible projects just to work with a particular coauthor, even if he is a good friend you really want to work with.

Part 2: Writing A Draft

Chapter 4: An Overview of Writing Academic Research Papers

Once you pick a topic, then the real work begins. Any research project involves much analysis, which could be doing an experiment, solving a model, estimating an equation or many other things. In this book, I will not focus on the technical details of the analysis. There are many books covering economic theory, econometrics, etc., far better than I could. My focus is on the craft of putting papers together and writing them in the most effective fashion. Craftsmanship is particularly important when writing papers because the choices that a scholar makes about how to write and present her analysis are often the difference between a paper being influential and it being ignored.

A common mistake that many scholars make is not to take the "write-up" seriously enough. Academics often say something like: "I've done all the work, now I just have to write it up." If you have made a contribution that is immediately obvious to all readers, such as proving Fermat's Last Theorem, this approach would be sensible. But for the vast majority of scholars, the importance of their research will not be immediately evident to a reader. An important part of the job in writing an academic paper is to explain why the research is important and why a reader should care about it.

Writing Research Papers in a Competitive Marketplace for Research

As I discussed in Chapter 1, the academic marketplace is becoming more competitive every year. Top journals have acceptance rates under 10%, small regional conferences have 200-300 submissions and usually take 8 to 10 papers, while large international conferences have over 2000 submissions and take between 100 and 150 papers. In addition, it is even harder to get accepted than these ratios would imply if you are not yet established and are not at a top-ranked university.

Editorial decisions at academic journals are usually made through what is known as a "peer review" process, which is discussed in detail in Chapter 11. Editors send papers to scholars doing related research, who then write referee reports on the paper. These reviewers are supposed to make a recommendation to the editor and also to make suggestions that help the authors improve the paper's quality. The referees are expected to be somewhat antagonistic and part of their job is to bring up alternative viewpoints. In addition to monitoring the paper's quality, referees help ensure that authors interpret their results appropriately and acknowledge the paper's limitations.

However, blind reviewing has become a joke in the internet age when a reviewer can google the paper's title and immediately see who the authors are. Unfortunately, reviewers sometimes can favor friends of theirs and famous people who they respect over people they do not know, even if they are supposed to be reviewing papers blindly. Consequently, if one is not a well-known scholar, it can be particularly difficult to get your papers onto conference programs and published by top journals.[21]

When you submit a paper to a journal or a conference, what can you do to maximize the chance of having your paper accepted? A good place to start is by thinking about the review process from the viewpoint of the people making the decisions about your work: the editors, referees, and conference organizers. The people running journals and conferences have their own incentives that should be understood by submitters. Editors want to increase the rankings and visibility of their journal, and also their own personal reputation. Consequently, the people running journals have incentives to pick high quality papers but also have incentives to favor higher profile authors, who bring visibility to the journal and will get cited more often. Conferences work similarly; each session organizer wants his session to be well attended and have intelligent discussions so they want to take the best papers, but also will tend to favor more established scholars to give the conference more visibility and credibility.

[21] Not everyone agrees with me on this point. In his review of this book, John Cochrane writes: "This is absolutely not true and you do a disservice passing it on. You leave out an important variable – quality. *Conditional on quality* – of research and of presentation (writing) – it is easier to get on programs if you are nobody from nowhere."

Editors and conference organizers often use what might be called a "Triage" approach to evaluation. Editors look at a paper quickly and decide whether to desk reject it. If they send it out for reviewing they select the referee(s). They will send papers they perceive to be higher quality to better referees. Higher quality referees benefit the paper's authors because they are likely to provide insightful comments and also tend to have a higher likelihood of recommending that the editor request a revision and eventually accept the paper for publication. Conference organizers often quickly go through the papers and eliminate most of the submissions, so that they can read the remaining ones more carefully and make the final selections. If a paper does not make it past these initial screenings, it will never be read carefully and given a serious chance.

This type of review process means that significant decisions about one's research are made based on very quick reads of the paper. Therefore, it is extremely important that one's paper makes a good first impression. No matter what is in the paper, if it does not make a good first impression, it will be rejected a lot. Since the abstract and introduction are the parts of the paper most likely to be read by someone doing a cursory scan of a paper, these parts of the paper will have a large impact on its ability to get on conference programs and to be accepted for publication in a good journal.

A common reaction to such cursory reviews is to bemoan the fact that the process is not "fair" and all authors are not treated equally. Some academics feel there is some underlying true "quality" of every research paper and that any process that does not do its very best to uncover that true quality is unfair and evidence of poorly designed and political system of evaluating research. I am sympathetic to this viewpoint, and we all feel that somehow the system should have a better way of measuring research quality and treat all authors equally. However, while the peer review system does have problems, there does not seem to be a better one available. As Churchill famously said about democracy, peer review is the worst way to evaluate research other than all the other potential ways one could use to evaluate research.[22] The costs inherent in doing evaluations of research mean that any system of evaluation will

[22] See Churchill, W., and Langworth, R.M (2008) *Churchill by Himself: The Life, Times, and Opinions of Winston Churchill in his Own Words*, Ebury.

necessarily be imperfect. Researchers must accept that the existing system for evaluating research is what it is, and it is not going to change any time soon. Academics have to learn to navigate the current system of peer review if they are to have a successful career.

Since research is evaluated in an imperfect manner, it is important to present one's work in a way that is appealing to the decision-makers. Presentation of one's work involves writing well, but it also means emphasizing what readers are likely to think is interesting, organizing the work in a fashion that makes sense, interpreting the results appropriately, and citing the prior literature correctly but not excessively. Perhaps most importantly, a successful author must do a good job of highlighting the work's significance and what is learned from it.

Research and the way it is written up

A useful way to approach writing academic papers is not to think about "research" and "write-up" as separate activities. A write-up is the end product of what a researcher does. Rather than saying that "the write-up describes your research", I prefer to say that "the write-up *is* your research".

Many of the same people who are completely obsessive about their analysis are cavalier about the way that they write their paper. For example, scholars who prove theorems in their papers are usually incredibly careful to ensure that the theorem is stated correctly, and that the conclusions do in fact follow logically from the assumptions they make. However, many of these same scholars think nothing of describing the results in paragraphs that go on for 2 pages, using run-on sentences containing three or four unrelated ideas, without giving much thought to providing a coherent explanation of the results. Or they might not provide any explanation for the results at all, figuring that any decent scholar would be able to figure out the theorem's importance by himself. This type of scholar, while often very smart, often feels underappreciated and tends to complain about the review system not treating him fairly.

Scholars should think of their research the way that an artist would view his art, or a musician would view his music. An artist cares about not only the subject matter of his painting and the colors and brushstrokes he uses, but also about the way that the picture is framed and how and where it is hung in a

particular room. An artist will care about these other factors because they affect how people view their creations. Similarly, musicians care about how their music is presented, what the performers wear, and what goes into the music video. A musician cares about these things because they affect people's opinions of his music. The same is true for research; research that is not presented well does not attract attention, does not influence other researchers so does not get cited, and consequently has low impact.

A particularly sore point of mine concerns careless errors commonly referred to as "typos". People often excuse these mistakes by saying that they are "just typos". My view is that typos are errors and should be treated just like any other error. When I was an undergraduate, I submitted a paper I coauthored with one of my professors to a journal. The referee responded that there were enough typos in the paper that he could not be confident the paper was correct given the lack of professionalism exhibited in the write up. He rejected the paper for this reason (and perhaps others as well). The referee was correct to reject our paper for this reason. We are professionals and research is one of the most important things we do. We need to treat the *entire* paper as if it were our life's work, because it really is!

Structuring a Research Paper

When one starts writing a paper, one must proceed from the assumption that the work will be read in the manner in which academic articles actually are read, and not how we would like them to be read. Some scholars think that academic papers are read like novels, in that readers will take them to the beach and go through them from beginning to end over the course of a week or two. Many novels keep the reader in suspense and only let the reader know what the outcomes of the important plot twists in the last chapter, or even sometimes the very last page.

Such an approach can make for a fun novel, but it is not a good way to write an academic paper. An editor giving an initial screen to a paper written this way would not know what the point of the paper is, and most likely would desk reject the paper. In an academic paper, a reader should be able to tell easily what the paper's question is, what the methods are, and what the paper concludes from a quick glance. The author should present the central fact and the logic of the paper clearly so that the reader understands

easily what you actually did in the paper. She should try to make these points in as transparent a manner as possible and not to hide anything from a reader.

An author's goal should be to get as many people as possible to read her paper as soon she makes it publicly available. To do so, it is important to write the paper in a way that leads other scholars to spend their valuable time reading the paper. There are always many new papers being circulated, leading to a lot of competition for readers' time. For example, I receive a number of emails every day with lists of new papers that usually contain the papers' titles, authors names and abstracts, as well as a link to websites where I can download the papers. I also receive emails from many conferences, including ones I do not attend personally, with links to the papers that will be presented. In addition, because I am old fashioned, I receive hard copies of the leading journals in my field via regular mail.

Given this deluge of papers I have access to every day, I only have time and energy to read a few of them aside from the ones that I have to read because they are directly relevant to my own work, are written by a close friend or student, or because I am a reviewer for a journal/grant, etc. Consequently, I scan the emails and journals quickly and then decide which ones I should read more carefully. I only download and read the ones that seem especially interesting. The process is quite similar to the triage approach taken by conference organizers and journal editors. A first pass tells the reader which papers to look at more carefully, and then the reader will look more carefully at the ones that seem interesting.

Once one realizes that most academic readers will follow this kind of process, the immediate implication is that authors should write their papers so that a cursory glance will lead the paper to seem appealing and accessible. Consequently, inside a paper, not all words are created equally. The words that make it to the email lists and the words that readers will see on a quick read of the paper are far more important than the others. A useful analogy is with land prices in cities. Every city has some areas that are much more valuable than other areas. For example, in New York City, apartments in the buildings overlooking Central Park in Manhattan are many times more expensive than similar sized apartments in the poor areas of the outer boroughs. These apartments overlooking Central Park are desirable and scarce, so prices get bid up so high that only movie stars and investment bankers can afford to live there.

Analogously, in an academic paper, space in the abstract and introduction is far more valuable than any other space in the paper. Because of both the way work is evaluated and also because of the way that it is read, the abstract and introduction will be read many times more than space in the body of the paper. Words in the abstract and the introduction will be read more often than words in the body of the paper and many more times than those in internet appendices. For this reason, space in the abstract and introduction is more more valuable than space elsewhere in the paper.

Consider a typical paper that is presented in a departmental seminar and how it is distributed. A personal goal of mine is to spend at least some time reading every paper that is presented in our weekly seminar. But the amount of time will vary across papers depending on my initial screening. I always read the abstract and sometimes the introduction right away when it is emailed to the department. If I find the paper interesting and have time, I will read the rest of the paper or at least the parts of it I think is most important. The amount of time I spend on a given paper ends up being a function of how interesting the paper seems to me on my initial read.

My guess is that most academics follow a similar practice to my own. If they do, then every time a paper gets presented in a seminar, most participants will read the abstract and some of the introduction. Some readers will go through the body of the paper and the conclusions, a few will spend time on the technical details of how the authors cleaned their data or provide their theorems, and almost no one will read the appendices. Authors should realize that readers will follow this process when they decide how to structure their papers.

Another fact to remember is that most readers will not vary the amount of time they spend on a paper as a function of a paper's length. If a 60 page paper seems interesting, I will try to read some of it, but probably won't spend that much more time on the 60 page paper than I would on an equally interesting 25 page paper. This phenomenon potentially could seem odd to authors; after all, every reader spends longer reading a 1000 page novel than a 200 page novel. But remember, most readers do not read academic papers completely. They read the abstract, some of the introduction and skim the rest of the paper looking for the interesting parts, perhaps a key table or the authors' interpretations of a result they

find puzzling. The details in the body of the paper will usually only be read by scholars doing closely related work and doctoral students seeking to learn about the area.

Authors should understand that their paper will be read this way and should write it accordingly. They should put the parts of the paper that they want readers to read in the places where they will actually be read. Parts of the paper that are necessary but perhaps not so interesting such as proofs, details of how data were cleaned, etc., can go in places where can readers find them but not where they otherwise spend time. **The authors' goal should be to make the paper as short and readable as possible, subject to the constraint that the paper contains all of the information that is necessary.**

What does an author hope to accomplish when writing a research paper?

When an author sits down to write a paper, it often is with dread, especially if the author does not like to write. After doing the "fun" part of the research, she now has to "write it up". As usually happens when people do not like doing something, the quality of papers written by people who have this type of attitude is often low.

It is helpful if one can learn to like writing. If one likes to write, usually the quality ends up being higher. Of course, the causality of this relation is not clear, good writers tend to like to write more than bad writers do. But good writing can be learned, and every academic, regardless of her field, should make an effort to become a better writer.

One way to start writing better is with the right attitude and objective. Instead of "writing it up" being your goal, have smaller, more manageable goals in mind when writing the paper. These are 5 goals that almost all academic papers should achieve:

1) Say what the paper's point is and convince readers it is interesting and novel.
2) Convince readers the analysis is correct and address alternative explanations.
3) Give credit to others.
4) Provide details of what you do.
5) Draw appropriate interpretations.

When one starts writing a paper, have these goals in mind. Think about how each can be accomplished in the most interesting and succinct manner. If a paper accomplishes these goals, it is likely to be successful.

1) Say what the paper's point is and convince readers it is interesting and novel.

This goal is perhaps the most important yet sometimes overlooked aspect of a paper's writeup. An author who spends her life working on a particular topic is normally fascinated with the topic, and the paper's contribution to it is likely to be obvious to her. But the reason why a paper is interesting to the author is not necessarily obvious to others, especially a reader who does not specialize in the paper's topic. Remember that conference organizers are typically tasked with sorting through 200 submissions and selecting 8 to be presented, and that most research-active faculty receive emails containing 40 or 50 different papers every day, and download at most one or two of these to read. If the contribution does not stand out immediately, there is a good chance that a paper will be overlooked.

A summary of the paper's novel contribution should be clearly stated in the abstract and the beginning of the introduction. The burden is always on the author to describe exactly what she has done in the paper, what the results are, and how she interprets them. She also must explain why he should care about the paper, not make the reader to figure it out for himself.

In providing this explanation, the author has to make some assumptions about the background of the person who is reading the paper. If the reader is a specialist in the paper's subfield, it is possible that no motivation is necessary. However, the more general is the audience that will be reading the paper, the more background information that an author should include. This background information is often already known by specialists but if an author wishes for all potential readers to understand the paper's contribution and why it is interesting, the author might want to include it in the paper anyway.

How should an author decide on how broad an audience to target? There is no easy answer to this question, but a lot depends on which journal the author is hoping to publish the paper in. For example, in economics, there are some extremely prestigious "general interest" journals such as *American*

Economic Review and *Journal of Political Economy*. If an author would like to target one of these journals, then the paper should be written in a manner that any professional economist can understand. In contrast, if an author is planning on publishing the paper in a more specialized journal, then it could be reasonable to omit some background information and assume a higher level of knowledge from readers.

My attitude with my own papers is to err on the side of giving a little extra background information than is absolutely necessary to ensure that all potential readers understand the paper's contribution and why they should care about it. The cost is relatively low because the additional explanations often take only a few sentences. Being accessible to a wider audience can add to the paper's potential readership and increase its impact. In the social sciences and business schools in particular, we often try to influence practitioners and/or policy makers in addition to academics. A surprisingly large number of people are interested in research about the social sciences; if authors make an effort to explain why people should care about the work and to present the analysis in a clear and understandable manner, the number of people who will read a paper can increase dramatically.

2) Convince readers the analysis is correct and address alternative explanations.

Before an author goes through the details of the analysis, it is important to give the reader a sense for what is coming. The author should explain in an intuitive manner why she does what she does, what goes into the analysis, if there are any "tricks" the author employs, etc. Papers using unique data or an interesting experiment can be great, and the author should emphasize exactly what is unique in a manner that all readers will understand even if they don't spend a lot of time going through the paper in detail. In a theoretical paper, the author should clearly explain the key assumptions and the ideas underlying the proofs. This discussion should be sufficient so that a reader can pretty much tell where the paper is going and what the results are going to be. If the reader stops after this discussion for some reason, he should understand the analysis at a sufficiently high level that he will have an opinion on the paper and can describe it reasonably well to his friends.

An important part of the discussion at this point is that the author should acknowledge the "standard" view, i.e., what the profession knew prior to her paper. The purpose of a research paper is to change people's priors. This discussion should say exactly how the author feels that the paper will change reader's priors and why. Part of this explanation should be a discussion of the alternative interpretations of the author's evidence and the extent to which these interpretations are valid.

3) Give credit to others.

A surprisingly tricky aspect of writing academic paper is which papers to cite, how to cite them, and where in your paper you should cite them. It is incumbent on an author to acknowledge all relevant prior work and to be honest about her paper's novelty relative to prior work. However, it is *not* necessary nor appropriate to spend so much time on prior work that a reader cannot tell what the contribution of the author's paper's is. A common error, especially among young authors, is to confuse readers about their own paper's contribution by describing prior work in too much detail before explaining their own paper's contribution.

It sometimes can be awkward when referencing work that you do not completely agree with, or that your results contradict. An author must point out the differences between her work and the prior literature, and let readers understand which approaches lead to which results. However, it is advisable to do so in a polite and scientific way. Authors can be sensitive when their work is criticized; it is their life's work and a negative critique of their work can have a huge impact on their careers. Academics who are normally completely sensible and rational can become extremely emotional and defensive when their work is challenged. Sometimes when discussing prior work, an author has to be extremely careful to elucidate the differences between the papers, as well as the reasons why different approaches affect the papers' conclusions, in as inoffensive a manner as possible.

4) Provide details of what you do.

The body of an academic paper describes the research in detail. The rule is that someone who does not know the authors and has nothing but access to the paper and the existing literature should be able to replicate every result in the paper. Every way that the data has been cleaned must be entirely documented, every element of the experimental design written down, every assumption of a model made explicit, proofs completely explained, and all other relevant details included in the discussion. If a doctoral student on the other side of the globe cannot follow the analysis completely and find the same results as are reported in the paper, then the author has not done a sufficiently good job at describing her work.

However, just because a discussion has to be complete, it does not have to be boring. Sometimes authors feel there is a tradeoff between having a readable paper and a complete one that covers all the necessary details. Most of the time, however, it is possible to have both. The key is to remember that different parts of the paper will be read by different people. Parts that will be read by many people, such as the abstract, introduction, and conclusions, should contain the main ideas but be light on details. The body of the paper should contain as many details as necessary for the paper to be replicated.

There is a limiting factor on the number of details in the body of the paper, which is that it still has to be readable. If the body of the paper stops being readable because the problem is extremely complicated or aspects of the data collection or algorithms are sufficiently complicated, a good option is an internet appendix. When using an internet appendix, it often is a good idea to explain the point of the appendix in the text and keep the details in the appendix. So for example, if a proof has an interesting idea, an author might explain the idea in the text and refer to the internet appendix. The requirement for description of research is that the proverbial doctoral student on the other side of the globe has can replicate the work using publicly available information. But it is ok to require the student to download an internet appendix to supplement the published version of your paper when she replicates the work.

Sometimes people try to replicate the work but cannot do so. When work cannot be replicated, most of the time there is an honest mistake somewhere. Many published papers contain errors. Sometimes published equations are wrong but more often authors forget to describe some step they took when

constructing their data, accidentally deleted important code, or made some other honest mistake. Alternatively, it could be that the person trying to replicate the results is doing something wrong. In any event, it is always important for the author to understand why the paper could not be replicated.

When authors are contacted by scholars who want to understand how they obtained particular results in their papers, they are ethically bound to work to help resolve the inconsistency. However, helping people replicate work is not just an ethical issue, but a practical one. If a scholar tries and fails to replicate a study, it is in the interest of the original author to take the time to explain what the person is doing wrong, or to find the error in the original paper. If the author is rude or unhelpful, and the scholar who cannot replicate it circulates a paper saying that a well-known paper is wrong, the original author's reputation is likely to suffer. Being known for producing results that cannot be replicated can be extremely damaging to the author's career.

A example of a paper that famously was not able to be replicated was Carmen Reinhart and Kenneth Rogoff's "Growth in a Time of Debt". This paper tackles the incredibly important question of whether government debt inhibits economic growth, and presents evidence suggesting that extremely high debt to GDP ratios (over 90%) inhibits a country's growth. This paper was highly cited and used in policy debates to highlight the costs of government debt. However, three other scholars, Thomas Herndon, Michael Ash, and Robert Pollin, were unable to replicate the Reinhart/Rogoff findings, and claimed to have found a number of errors in the original analysis. The incident was highly publicized and embarrassing to the authors of the original paper, who would clearly have been better off had they been more careful double-checking their analysis before publishing their paper.[23]

5) Draw appropriate conclusions.

[23] See Carmen M. Reinhart and Kenneth S. Rogoff (2010) "Growth in a Time of Debt," *American Economic Review Papers and Proceedings*, Vol. 100, pp 573-578, and Thomas Herndon, Michael Ash, and Robert Pollin (2014) "Does High Public Debt Consistently Stifle Economic Growth? A Critique of Reinhart and Rogoff," *Cambridge Journal of Economics*, Vol. 38, pp. 257-279.

In some fields, especially mathematics and the sciences, the conclusions one draws from the analysis tend to be factual and indisputable. However, in the social sciences, they are more often subject to interpretation. Some social scientists (like me) enjoy this aspect of our field because we think it makes it more interesting and exciting. However, ambiguity can create difficulties when drawing inferences from economic analysis.

Different people can and do interpret the same statistical results in different ways. When drawing conclusions from data that can be interpreted in multiple ways, an author should strive to present all possible interpretations of their data. She should explain how one could conceivably distinguish them, even if the author does not or cannot do this work in the current paper. However, it is also an author's responsibility to say, if possible, which interpretation she feels is mostly likely to be true and why. It can be a delicate balancing act to give all plausible explanations appropriate credence while simultaneously explaining to the reader why she thinks one particular interpretation is the most likely one. Remember that the goal of a paper is to influence readers' priors. If an author does not let the readers know what she feels the results really mean, then their priors are unlikely to change very much.

Chapter 5: The Title, Abstract, and Introduction

When a paper is posted online, all a potential reader usually sees is the title and abstract. Then, if she looks at the actual paper, she most often reads only the introduction. A paper's title, abstract, and introduction have a large influence on the number of people who read it, and therefore can materially affect its ultimate impact on the profession. For this reason, I devote this chapter to discussing the way an author picks a title, and writes her abstract and introduction.

The Title

A paper's title is the first thing a reader sees about the paper, and his response to the title often determines whether the reader looks at any more of the paper. It is the first and most important advertisement for a paper since a good title can motivate the reader to spend time with the paper. However, coming up with an appropriate title for a research paper can sometimes be tricky.

When choosing a title, there are several main approaches. My preferred one, and the most commonly used, is to make the title as descriptive about the paper as possible. The idea is that after seeing the title, a reader should have a pretty good idea about what the paper says. Sometimes a descriptive title is a question, usually addressed by the analysis, but other times is a few words that describe the paper's contribution.

An example of an effective use of a question title is Shiller's famous "excess volatility" paper entitled: "Do Stock Prices Move Too Much to be Justified by Subsequent Changes in Dividends?"[24] After reading that title, no one could possibly have any doubts as to what is in the paper (and after reading the title, we all somehow knew that Shiller would come to a "yes" answer to his question). My job market paper used a "short description" title, that in my biased opinion, served its purpose very well. After circulating early versions with several other (longer) titles, I ended up calling the paper "Outside

[24] See Shiller, Robert J. (1981), "Do Stock Prices Move Too Much to be Justified by Subsequent Changes in Dividends?" *American Economic Review*, Vol. 71, pp. 421-436.

Directors and CEO Turnover."[25] Not surprisingly, after reading the title, most readers correctly assumed that the paper was about the way in which outside directors affect CEO turnover. Perhaps in small part because of the title, the paper has done very well, with over 5,900 Google Scholar cites at the time of this writing.

A second approach to choosing titles is to have it be something cute, sometimes containing an allusion to a famous saying. These titles can sometimes be effective; Leamer's "Let's take the Con out of Econometrics" has been an influential paper, probably in part because of the memorable title.[26] My impression is that these titles, while attracting attention, sometimes leave the reader wondering what the paper is actually about. I tried this approach and came up with one of my less successful titles, a reference to what I thought was a famous example from Merton Miller's AFA presidential address. In his address, Miller argues that the costs of financial distress are orders of magnitude smaller than the tax benefits of debt, humorously saying that the tradeoff between the two "looks suspiciously like the recipe for the fabled horse and rabbit stew – one horse and one rabbit." When I wrote a paper studying this tradeoff, I foolishly suggested that we call it: "Horses and Rabbits? Tradeoff Theory and Capital Structure." I convinced my coauthors that most readers would see the reference right away. Of course, almost everyone who saw our paper had no idea about what the title referred to, and instead of being perceived as clever, most people thought it was confusing.[27]

Andrei Shleifer likes to follow the practice of Alfred Hitchcock, the great director of suspense movies, and use one-word titles.[28] This approach works well for Andrei because he is a superstar, and

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[25] See Weisbach, Michael S. (1988), "Outside Directors and CEO Turnover," *Journal of Financial Economics*, 20, pp. 431-460.
[26] See Leamer, Edward E. (1983), "Let's take the Con out of Econometrics," *American Economic Review*, Vol. 73, pp. 31-43.
[27] See Miller, Merton H. (1977) "Debt and Taxes", *The Journal of Finance*, 32, pp. 261-275, and Nengjiu Ju, Robert Parrino, Allen M. Poteshman and Michael S. Weisbach (2005), "Horses and Rabbits? Trade-Off Theory and Optimal Capital Structure," *Journal of Financial and Quantitative Analysis*, 40, pp. 259-281.
[28] See Shleifer, A., & Vishny, R. W. (1993) "Corruption," *Quarterly Journal of economics*, *108*(3), 599-617, Djankov, S., La Porta, R., Lopez-de-Silanes, F., & Shleifer, A. (2003) "Courts," *Quarterly Journal of Economics*, *118*(2), 453-517, Barberis, N., Shleifer, A., & Wurgler, J. (2005) "Comovement," *Journal of Financial Economics*, *75*(2), 283-317, and Bordalo, P., Coffman, K., Gennaioli, N., & Shleifer, A. (2016) "Stereotypes," *Quarterly Journal of Economics*, *131*(4), 1753-1794.

many economists want to know about what he is working on, so the one word title becomes an effective "teaser". When economists see the one-word titles on his papers, they become intrigued, wonder what the paper is about, and then read it to find out. However, my guess is that for most of us, a one-word title would confuse potential readers and lead them to skip over our papers.

Some of the most successful titles catch a reader's attention in a way that gets him thinking about the issues and gets him to want to read more. George Akerlof's classic paper developing the idea of adverse selection has a particularly intriguing title: "The Market for 'Lemons': Quality Uncertainty and the Market Mechanism." When a reader sees the paper for the first time, he probably wonders if "lemons" refers to the fruit, and does not immediately see why do we care about such a market. Once we start the paper and understand what kind of lemons Akerlof is discussing, most of us start to think about the worst car we ever purchased. We are then fascinated by the resulting analysis the way in which asymmetric information about such a car could affect used car markets. A paper as brilliant as this one would have done just fine with a boring title, but the clever one he used made the paper that much more special.[29]

Different scholars have different philosophies about how to pick titles. One of my coauthors remarked that she really likes short titles (although longer than one word). Another one suggested using a catchy title with a more descriptive subtitle, as I tried to do with this book. The important thing is the goal when picking a title is to convince readers to read more. Coming up with a title that accomplishes that goal is something worth spending time on, because a good title can meaningfully change a paper's readership and its eventual impact.

One word of caution is that authors, especially young scholars beginning their careers, can have a tendency to make their titles a little too "cute". It is a bad sign if readers think that a scholar spent more time thinking of a clever title than they did in solving their model or in coming up with the correct econometric specification.

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[29] On the other hand, I still find it astonishing that such a paper managed to get rejected a few times before the *Quarterly Journal of Economics* accepted it for publication. Could the title have been a culprit? Perhaps a clueless referee thought the paper was somehow about fruit? See George A. Akerlof (1970) "The Market for 'Lemons': Quality Uncertainty and the Market Mechanism," *Quarterly Journal of Economics*, 84, 488-500.

The Abstract

The abstract is a short summary that is included at the beginning of every paper. Historically, when scholars only read hard copies of papers, abstracts were less important since anyone reading them had a copy of the paper in front of them as well. However, today there are many places where abstracts show up prominently and affect whether a potential reader downloads the paper and reads it in detail. For example, the National Bureau of Economic Research (NBER) and Social Science Research Network (SSRN) send regular emails containing abstracts of new working papers, with links to the actual papers. I am probably typical in that I do regularly read these emails, but usually download only one or two papers each week. The papers I download are the ones for which the abstracts make the papers sound particularly interesting. So, the way the authors write the abstracts does materially affect the likelihood that I, and undoubtedly other readers as well, download their papers.

Abstracts can vary in length. Many authors, including myself, have begun to make them longer so that the abstract can contain more details about what is in the paper. Sometimes publicly circulated papers can have abstracts of more than 200 words and encompass two or three paragraphs. The practice of longer abstracts is fine for early drafts that are sent for comments and are presented in seminars. However, journals usually limit abstracts to about 100 words and can be strict about these limits. Some even won't let a paper be submitted with an abstract of more than 100 words; the computer actually counts the words and won't let authors complete the submission until the abstract is short enough. Given this limit, an author has to be judicious with what she puts into the abstract.

How does an author decide on what to put in the limited space available in an abstract? She should start with the understanding that an abstract is just an advertisement for the paper. Therefore, she should focus her attention on the aspects of the paper that will entice readers to read more of the paper. The way to interest readers is not by cramming as many facts as possible into the available space, but by getting them interested in the issues the paper raises and persuading them that the paper has something important to say.

I usually start an abstract with a sentence explaining the question a paper asks and a reminder to the reader of why it is interesting. For example, an important issue in corporate finance is payout policy. The big puzzle, even after many years of research, is that firms still pay dividends despite the tax disadvantage when there are more tax-efficient ways to pay cash out to shareholders, such as stock repurchases. Most readers of a corporate finance paper will understand this issue very well, but some might not and some might have forgotten about it. So, mentioning the tax disadvantage of dividends or the "dividend puzzle" in the first sentence of a paper with a reason for why firms do pay dividends is a good way to remind a reader who doesn't think about dividends regularly why the paper is addressing an interesting issue.

Then I would spend the next two or three sentences saying what is "cool" about the paper and what the results are. If the paper has a clever identification strategy or new, interesting data, say so. Focus on the most important findings and skip over the secondary results, unless they are of an importance comparable to the main result. In a theoretical paper, describe the idea in a sentence or two but skip modeling details unless they are the crux of the paper's contribution.

If the methods or data used in a paper are standard, there is no need to waste space in the abstract saying what they are. The same goes for robustness checks. If there is a commonly-raised alternative explanation that can be ruled out because of an interesting test, it should be mentioned. But there is no need to waste space in an abstract for the discussion of the now standard set of robustness tests that are in every empirical paper.

Finally, authors should finish the abstract with a sentence or two saying what the paper means, and what its main implications are. It is not enough to say that y and x are positively correlated; the author should explicitly state what theories this correlation is consistent with, and also if there are any other implications of this correlation. Often, I read abstracts where the authors are so focused on including as many details of the paper as possible into the 100 word limit that they forget to tell the reader why they should care about the paper in the first place. Authors always have to remember they are competing in the market for ideas, and success in that market means having results that change people's priors about

important issues. The end of the abstract is a good place for authors to explain to a reader what he might learn if he spends time going through their paper.

The abstract is an advertisement for the paper. The author should start with the motivation and question. Then she should say what the main results are. Then say what the results mean and why we should care about them. All other details are not necessary to include in an abstract and can be left to the paper itself.

The Introduction

Aside from the title and the abstract, an academic paper's introduction is the part of it that gets read by far the most often. If a researcher wants to understand the basic results in a paper to decide whether the paper is relevant for her current research, or if a student wants to know the paper's results for an upcoming exam, usually reading the introduction is sufficient. While not the most important part of the paper – that is the details of the paper's contribution described in the body of the paper – it is usually the most difficult to write. It is the part of the paper that I and many other authors spend many hours agonizing over before we circulate a paper.

The reason why authors spend so much time on the introduction is that of all the paper's sections, the introduction can make the largest impression on a reader, and can influence the paper's publication and eventual impact the most. I often rewrite a paper's introduction a number of times before I let even my coauthors see it, and then rewrite it again with my coauthors' help multiple times before the paper gets circulated publicly. In the Dropbox folders that I have for my papers, there are many drafts of introductions, with names like "Intro – Mike – Try 7". Often these drafts led to an email from a coauthor explaining that this one was ok, but probably we can do better. So there ended up being a "Try 8" and a "Try 9" in the next few days. Eventually my coauthors and I reach an agreement on what the introduction should look like, but it usually takes longer and more rounds than any other part of the paper.

Why is the introduction to a research paper so difficult to write, and so important to write well? Introductions have to cover a lot of material, while at the same time being easy to read and to understand.

In addition, they should accomplish so much in a minimum of space. The introduction and the abstract are the first and often the only parts of the paper that readers look at. They are an author's best shot at getting a potential reader interested in the paper. Consequently, an author should try to make the introduction a snapshot of everything that she thinks is important in the paper.

An important principle about writing introductions is that the author should assume that the reader will spend the same amount of time on it regardless of its length. Therefore, anything that is included in the introduction takes away reading time from something else. The introduction is not meant to be a mini-version of the paper. Not every twist and turn has to appear there.

When writing an introduction, always remember the expression "Less is More". Say what is necessary as concisely as possible in a readable and interesting way, and in a way that emphasizes what is unique and insightful about your work. Given how many things ought to be in the introduction, it is therefore important for authors to realize that anything other than the things necessary to be in the introduction *should not be there*, and should be moved into the body of the paper.

Here is a brief list of the things that an author should hope to accomplish in her introduction:

1) *Grab the reader's attention.*
2) *Say what question you are asking.*
3) *Say what approach you use.*
4) *Say what the results are.*
5) *Say how you interpret the results.*
6) *Discuss other implications of the results.*
7) *Provide an outline of the paper, which can be a formal outline or just a brief summary of each of the paper's sections.*

These seven tasks are a lot to cover in four or five pages, which a typical reader will skim in only a minute or two! Therefore, it is important to leave everything but these seven points out of the introduction. Including much other than these points usually ensures that readers miss the point of the paper they are reading, and do not appreciate what it has to offer. The two most common culprits that are mistakenly included in introductions are long discussions of others' work, and detailed presentations of the methods used in the paper.

I will discuss each of the seven parts of an introduction in turn, explaining how an author might accomplish each one in the most effective manner possible.

1) Grab the reader's attention.

This part of an introduction is possibly the most important one, yet is sometimes neglected by authors, especially inexperienced ones. Many authors start their papers by saying exactly what they do in their papers, rather than giving the reader a reason for *why* they are doing what they are doing. If it is obvious to potential readers why a contribution is important, then it is fine to ignore the motivation. For example, if your research contains a way to make microprocessors work faster or to design bridges to hold up better in hurricanes, then motivation is probably not that important because the importance of these contributions is likely to be obvious to most readers. However, as I have argued above, this type of paper is the exception, and most papers must explain why they are important to compete for readers' attention. In a competitive marketplace for ideas, it is incumbent on the author to give potential readers a reason to spend time on your paper, otherwise they won't.

The way that an author can grab a reader's attention varies from field to field, but ultimately, it comes down to persuading the reader of the importance of the issue being discussed. Feedback received from colleagues and seminar participants can help authors know what parts of the paper readers find most interesting. Highlighting these parts in the introduction is likely to pique the interest of new readers. In addition, including numbers that make the paper's importance evident to readers is a great way to begin a paper. If a potential reader sees that a question is quantitatively important, he is more likely to spend his valuable time reading the paper.

One approach that I sometimes find effective is to point out a difference between the assumptions of the academic literature and the real world. Recently I coauthored a book chapter on the way that multinational firms make financial decisions. My coauthors and I started the chapter with some facts about the preponderance of multinational firms in the world today and noted that despite the large number of multinational firms, most of the academic corporate finance literature has focused on the issues facing

domestic rather than multinational firms. In one of my better-known papers on the capital structures of private equity portfolio firms, my coauthors and I started the article by pointing out that practitioners and academics view the same issues very differently from one another. We used these different approaches to thinking about the issue to motivate a model of private equity capital structures.[30]

Another way to grab a reader's attention is to put the issue being discussed into a larger context. A paper documenting that prices are "sticky" is a lot more meaningful to a reader who is aware that the existence of sticky prices underlies much of traditional Keynesian economics. Consequently, documenting that prices are sticky could be an important factor differentiating Keynesian models from New Classical ones. Placing a paper in the context of an important literature sometimes convinces a reader that your paper is worth reading, especially if the reader is a fan of that literature.

Some papers contribute to classic literatures that had their origins with the "masters" who originated or revolutionized our fields. If your paper falls into this category, it is worth mentioning the classic work. For example, the question of the way a board of directors monitors management dates to a section in Adam Smith's *Wealth of Nations,* and the issue of how corporations manage liquidity was originally raised in Keynes' *General Theory*.[31] I have worked in both these subfields, and always make a point to cite the classic works prominently when they are relevant. I do so in part to give credit where credit should go, but also to remind readers of these issues' fundamental importance to debates that have lasted for many years.

Often, the key issue in motivating a research paper, especially in the social sciences, is that readers often think they understand whatever subject your paper is about prior to reading the paper. In other words, people react to a paper by saying "we knew that already." An author's goal in the first part of the introduction is to convince the reader otherwise, that something he thinks he understands is more

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[30] See Isil Erel, Yeejin Jang, and Michael S. Weisbach, "The Corporate Finance of Multinational Firms," forthcoming in *Multinational Corporations in a Global Economy*, edited by F. Foley, J. Hines and D. Wessel, Brookings Institution, and Ulf Axelson, Per Strömberg, and Michael S. Weisbach, "Why are Buyouts Leveraged? The Financial Structure of Private Equity Firms," *The Journal of Finance,* Vol. 64 (August 2009), pp. 1549-1582.

[31] See Adam Smith, 1776, *An Inquiry into the Nature and Causes of the Wealth of Nations* (Modern Library) p. 700, and John M. Keynes, 1936, *The General Theory of Employment, Interest and Money* (Palgrave Macmillan), p. 196.

subtle than he previously had thought, that there is an important issue in the world that the literature has forgotten, or that there is an important gap in the literature. Once a reader realizes that his understanding of the issue you raise is imperfect, he is much more likely to spend time trying to understand it better by reading your paper.

The attempt to get the reader's attention should be at the very beginning of the paper. There is no point in getting into the bones of what the paper does before you convince the reader that your paper is worth reading. But the "attention-grabbing" part of the introduction should be relatively short. A good approach is to use the first paragraph to explain to the reader why the issue is important. Then start explaining how you address that issue in the second paragraph, certainly no later than the third paragraph.

2) Say what question you are asking.

When I attend seminars or read research papers, I sometimes feel like I am playing an iconic TV game show called *Jeopardy*. On this show the host gives the contestants an expression or a name and the contestants have to come up with a question for which what he told them is the correct answer. In other words, the show's premise is to start with the answer and then quiz contestants on whether they can think of an appropriate question for that answer. The academic version of this show is when authors look at some interesting data or do some analysis that somehow seems plausible, but does not tell the reader what the analysis means, or why the reader should be interested in it. The reader or seminar participants feel like they are playing a game of *Jeopardy*, in which they have to figure out for themselves what question the author is asking. These papers can be frustrating to read or to referee. When they are presented in seminars, authors often lose control of the room, since everyone in the room thinks the author's goal is something different.

It is important to avoid playing *Jeopardy* with readers in this fashion. Once an author convinces a reader that the overall issue she is studying is interesting, she should narrow down that issue to a specific question or two that she will address in the paper. It is usually a good idea to be very explicit about the question your paper is asking. Some authors literally make the paper's title the question that is addressed,

while others raise the issue explicitly in the second or third paragraph. Regardless of the way the author raises the question, it is important that the reader understands what specific issue or question will be addressed in the paper. Readers should not have to wait to have the question explained to them; it should be done as quickly as possible, preferably by the end of the first page.

A benefit of raising the question explicitly in the beginning of paper is that the coauthors become aware of it themselves and can focus their energies around the particular issue that the paper raises with no misunderstandings among them. One might think that the question being asked in the paper is so obvious that no reasonable author would ever spend all the time and effort to do a research project without knowing it. In fact, authors proceed with research projects without a specific question in mind surprisingly frequently. Sometimes authors get so caught up in the details of the modeling or the data work that they forget exactly what they want to learn from the analysis. Other times, coauthors go a year or two before realizing that each of them wants to focus the paper around a different question. Writing the question explicitly in the beginning of the paper lays everything bare and helps to ensure that there are no such misunderstandings.

3) *Say what approach you use.*

Once you have convinced the reader that the overall issue you are studying is interesting and have told the reader exactly which question you will be asking, the next step is to explain how you will answer the question. In the introduction, space is extremely valuable, so it is important that authors think carefully about the correct amount of detail to include in this description. The purpose of the description in the introduction is to explain the paper's contribution to a reader who is skimming the introduction quickly. Therefore, details necessary to replicate the paper but not to understand the paper's point should not be included in the introduction. Instead these details should be deferred to the body of the paper (or to an appendix).

The introduction should focus on whatever makes the paper novel, and why this novelty leads to a unique contribution. Remember that your goal when writing the introduction is to make your paper

stand out from the many others that editors and conference organizers will see. When describing the methods you use, try to do so in a way that will give the reader a sense as to what is novel and interesting about your paper's approach. If, for example, you hand-collect novel data or run an experiment with a unique design, then it is worth emphasizing the way these data are collected. If on the other hand, you use standard data in an unusual manner, then focus your discussion more on your estimation approach or whatever is different about your paper.

The goal should be to have a description of your paper that people can read and understand on a quick pass through the paper. The general principle is "Less is More". The introduction should contain a paragraph or two that a typical reader could read and come away with an understanding of what your paper does, and what is special about it. All details necessary for replication but not a general understanding of the paper's contribution should not be included in the introduction. For example, in a typical empirical corporate finance paper, it is appropriate to say in the introduction that, for example, the sample consists of 1000 publicly traded U.S. companies between 2000 and 2010. But including in the introduction details on exactly how these firms are chosen, what filters are applied to the data, or other similar details would waste valuable introduction space that could be used on something else.

On the other hand, in the famous Card-Krueger minimum wage paper discussed in Chapter 2, the trick in the paper is to compare wages in fast food restaurants in New Jersey and Pennsylvania where the minimum wage laws are different but labor markets are similar in other ways. Consequently, Card and Krueger detail the different laws in the introduction, explain the way that the sample was hand-collected, as well as other details specific to this paper that are likely to matter to a reader.

4) Say what results are.

Once you tell the reader what the question and general approach of the paper are, you should go through the paper's results and summarize them. Here it is important to include details but to be selective so as to keep the introduction relatively short and readable. A good strategy is to follow the structure of the paper. So, if there is a formal model, explain how it works in a paragraph or two. Then if you estimate

the model, say briefly how you estimate it and what the results are. Often papers consist of a series of related tests that build on each other; at this point in the introduction you should go through the main tests, explain how they work, and discuss the results of each one.

If the paper is empirical, it is important to discuss the most important estimates you find. The introduction is a good place to highlight to the reader exactly which results you think are most important and why. Empirical papers often provide estimates of many different variables, so one of your goals when you write the paper is to focus the reader's attention on the ones you think are most relevant. Don't just say whether the estimates are positive or negative. Be sure to give the actual estimates, discuss their magnitudes and give the reader a sense as to whether the effect is large enough to be meaningful. By discussing these results in the introduction, you are telling the reader where you think he should focus his energies when (if) he reads the rest of the paper.

Readers, however, don't like to be told what to focus their energies on. It is their prerogative to try to find holes in your analysis. Academics love the give and take involved with critiquing new research. A good strategy for authors is to anticipate the objections that will be made and to try to respond to them in advance. With almost every paper, there are usually one or two objections that come up from most readers and during most presentations. For these objections, think long and hard about the best response, and include this response in a prominent place in the paper. If the objection is sufficiently important, the author should probably place a short version of the response in the introduction and a longer version in the body of the paper.

Many empirical papers struggle with the notion of causality. It is usually easy to document that two variables constructed from real-world data are correlated with one another. What is much more difficult is to draw inferences that one variable caused the other to move, and not vice versa, and not that some third unobservable variable causes both of them to move. Consequently, many objections to empirical papers stem from the difficulty of inferring causality. Often in these types of papers it behooves the authors to discuss in the introduction of the paper the extent to which causality can be inferred and the methods the authors have used to address the issue.

5) Say how you interpret the results.

Along with a discussion of what the results *are*, it is also important to say what you think the results *mean*. What theories are they consistent with and what theories do they cast doubt on? How robust are the results, and to what extent are there important caveats to your interpretation?

Some authors interpret their results more strongly than is warranted. Overly strong interpretations can be a deliberate strategic move to attract attention to a paper. In addition, authors sometimes do not accept that there are plausible alternatives that are also consistent with the author's results, leading them to place a higher weight on their favored explanation. However, if authors consistently overinterpret their results, the cost is that people will take their papers less seriously in the future.

An equally egregious sin that some authors commit is underinterpreting their results. Some authors are so cautious about interpreting the results that their papers become a list of facts and statistical findings without giving the reader any sense as to what the results mean. Readers tend to get bored of these kinds of papers and find it hard to understand why they should be interested in the paper.

It is sometimes hard to know exactly how hard to push one's results, to find that fine line between overinterpretation and underinterpretation. A good rule is that an author should always make it clear what she thinks the results mean, even if she includes a number of caveats and discussions of plausible alternative interpretations. However, the author also has to be honest about the extent to which the results distinguish the author's favored interpretation from the alternatives, and not to oversell her work.

6) Discuss other implications of results.

The authors' task in the introduction is to provide a short summary of what is in the paper, as well as what she thinks the results mean and what they add to our knowledge. The goal should be to persuade readers, and especially reviewers, that the paper is worth spending time on. After reading the introduction, the reader should want to learn more details about the analysis by reading the rest of the paper. Therefore, if there are ancillary predictions of the model you present, or implications of your empirical work other

than the ones you mainly focus on, you want to point them out. Some readers will not be interested in your central questions but will be interested in the additional ideas you present. If you raise these additional ideas in the introduction, these readers might value the paper more highly. Alternatively, if you wait until the end of the paper to make these points, these readers may never see them, as they may never get that far.

7) *Provide an outline of paper. This can be a formal outline or it can just mean going through results in order.*

It has become traditional to end the introduction with a paragraph that starts with a sentence like: "The remainder of the paper proceeds as follows:" This sentence is followed by one sentence describing each section of the paper. Some journals require such paragraphs while others discourage them. I have had papers that are almost accepted where the editor has told me to add a paragraph along these lines, and have had other papers at the same stage that included such a paragraph and the editor has told me to take it out.

These types of paragraphs are almost never read and rarely add anything of value to the paper. If an author has a choice in the matter, it is usually a good idea to skip them. A better solution is to structure your introduction around the paper's organization, and to integrate the outline of the paper into the discussion of the paper's content. So, for example, when discussing the formal model, one could say something like: "In section 3, I present a model of … in which agents are risk averse but principals are risk neutral". Then when discussing the next section, start with a similar sentence, so by the time you reach the end of the introduction, the "outline paragraph" becomes superfluous.

Common mistakes authors make when writing their introduction.

The introduction of your paper should be a summary that is written in a way that encourages potential readers to want to read the entire paper. The focus should be on *your* paper and its contribution and things that distract from that discussion should be minimized. Authors have a tendency to confuse

readers by including extraneous information in the introduction in two ways: 1) Providing too much technical detail about your paper that is not necessary to understand the paper's point, and 2) Spending too much time on others' papers before explaining your paper's point.

1) Providing too much technical detail about your paper that is not necessary to understand the paper's point.

Many authors are justifiably proud of the effort they put into their papers. They have understood up to date methods and perhaps modified these methods to suit the question they are asking. These authors sometimes go into laborious detail in the introduction as to how they did all the work in their paper. However, authors who use elaborate and novel approaches can spend far too much space in their introduction discussing their methods.

If the paper asks an applied question and the paper's methods are a means to an end rather than an end in themselves, then readers can get confused about the paper's point if too many details are in the introduction. If, on the other hand, the point of the paper is to develop new methods rather than to use them in an application, then the methods should be the main focus of the introduction. Authors should shy away from including too many equations and formalisms in the introduction; it is usually better to explain in words how the method works. The formal discussion can come later. Space in the introduction is precious, and should not be wasted on details that can be covered in depth later on.

2) Spending too much time on others' papers before explaining your paper's point.

It is always tricky to know how and where to cite others' work. Academics are always required to give credit to those who did related work. Citing the relevant literature is a service to readers who do not know it well and also an appropriate courtesy to the authors of those other papers.

A common error is to cite the previous literature in too much detail and too early in your own paper. I often read papers on areas I'm interested in but haven't worked in myself, and find the authors starting the paper with a discussion of interesting-sounding work. By the time I get to page 3 or 4, I have

read some ideas I like, but can't quite understand the point of the paper I'm reading. Too often the author has not told me what the point of her own paper is by page 3 or 4 because she is spending so much time describing others' work. Sometimes I never figure out the paper's point and come away from the paper thinking more of the paper's literature than of the paper itself.

An author's primary goal in her introduction is to explain the point of the paper, and she should make this explanation right away. Doing so usually means deferring discussion of others' work until later on. But what if your paper builds on others' work? Do you ignore the other papers? How do you simultaneously explain your paper to a reader who needs to know the background necessary to understand it, without confusing a reader with too much information about others' work prior to discussion your own?

One way is to explain the ideas and main results of the prior literature quickly and clearly. The details of which author did what can be left to the body of the paper. Alternatively, it is useful to include a footnote with lists of relevant papers, perhaps sorted into categories for different "types" of papers. But it is usually good to avoid citing others' work in too much detail before getting into your own paper's idea and results.

There are exceptions to this rule. Sometimes a paper is an extension of one particular paper, or is written in response to a paper whose authors have made an error or have misinterpreted something. In this case, the author should start the paper with a discussion of the other paper and the issues about it that the author wishes to address. The example I use in my doctoral class is Shleifer and Vishny's "Large Shareholders and Corporate Control." This paper builds on Grossman and Hart's "Takeover Bids, the Free-Rider Problem and the Theory of the Corporation", so the Shleifer and Vishny paper begins by providing a short description of the Grossman and Hart model. The paper's introduction then gives a

lucid discussion of important role of large shareholders in the economy, before describing the model of the way that they can affect takeover markets that the paper develops.[32]

If the author's paper extends a long literature, she should focus on the source of the literature and leave the details for later. For example, if the paper is about whether firms should maximize profits to the exclusion of other factors such as concerns for the environment and the firm's workers, then it would be almost a requirement to start the discussion with Milton Friedman's classic arguments and an appropriate reference.[33] But if the author discusses every paper related to this question before she explains the point of her paper, then many readers will understand the point of the author's paper.

In summary, there are many issues to consider when writing an introduction. An introduction should be a summary of the paper and also an advertisement for it. Perhaps the most important and neglected role of the introduction is to explain to readers why they should care about the issues discussed in the paper and the particular question that it addresses. The author has to work hard to make the introduction short and readable, while explaining the paper's important points. To ensure that the introduction is short and readable, yet provides a sufficient summary of the paper, it is necessary to defer much important information such as literature surveys and technical details to the body of the paper.

[32] See Andrei Shleifer and Robert W. Vishny (1986) "Large Shareholders and Corporate Control," *Journal of Political Economy*, 94 (3), 461-488, and Sanford J. Grossman and Oliver D. Hart (1980) "Takeover Bids, the Free-Rider Problem, and the Theory of the Corporation," *Bell Journal of Economics*, 11(1), 42-64.
[33] See Milton Friedman, "The Social Responsibility of Business is to Increase its Profits," *New York Times Magazine*, September 13, 1970.

Chapter 6: The Body of the Paper: The Literature Review, Theory, Data Description, and Conclusion Sections

Most academic writing is far too formulaic for my taste. In almost every academic paper, Section 1 is always called "Introduction". Section 2 is usually the obligatory "Literature Review". If there is a formal model, there will be a "Theory" section or for a empirical paper, "Hypothesis Development". Next comes one called "Data Description", then "Empirical Specification", "Results" and finally "Conclusion". I don't object to this way of organizing a paper; my problem is when authors always use it without thinking about whether this way of organizing a paper rather than an alternative is appropriate for the paper the author is writing. Before writing a draft, authors should ask themselves the following kinds of questions: Is the formal model really necessary? If so, could it be in the appendix? How much literature is there to review? Could it be integrated into the rest of the text or does it require a separate section? Should the results be organized into one section or two? Should the empirical specification be its own section that goes prior to the results, or should it be integrated in to the discussion of the results?

An example of a slight deviation from this formulaic model that authors could easily do that would make things a bit easier on readers is to use more descriptive names for the paper's sections. For example, in a recent working paper, instead of calling a section "Literature Review," I call it "Prior Work Measuring Risk and Return of Private Equity Funds". Instead of calling a section "Results", an author think about calling the section something like: "Estimates of the way that Minimum Wage Laws Affect Employment" (substituting a description of the paper's results of course).

Authors should always try to think of ways that they can make their papers more thoughtful and user friendly. The organization into sections and titles for each is one way that they can do so. The organization of a paper could seem fairly unimportant to a young scholar relative to the paper's contribution, leading them not to think much about it and to follow the standard approach mentioned above. But one should always remember that a paper is the sum of many seemingly unimportant things.

When an author pays attention to each of them, her paper will become more readable and will ultimately become more influential.

Since academic papers are organized into sections and there are issues about which authors should be concerned in each type of section, I will discuss each in turn. The issues involved with the presentation of empirical results are sufficiently important that I will skip them in this chapter and devote all of Chapter 7 to discussing them.

Literature Review

One of the most misunderstood sections of an academic paper is the literature review section. A description of the prior literature is important because it places a paper in the context of what is already known, and in terms of the questions that the literature has historically tried to address. However, authors frequently do not put enough time and effort into this part of the paper. As a result, the literature review is often boring and badly written, and consequently skipped over by readers (other than to see if they themselves are cited). Authors who don't make sufficient effort to make the literature review readable, interesting and informative are missing an opportunity to increase the impact of their papers.

The literature review section has two main goals: First, it has to bring a reader up to speed with what has been done already so that he can better understand your paper's contribution. In doing so, it is sometimes effective if the author explains how her paper fits into this literature and exactly what it adds that is new. Second, the literature review gives credit to other authors for the work that they have done and to acknowledge their contributions. Giving appropriate credit to others is part of the scientific process, and an author who regularly fails to do so can end up damaging her reputation.

With these two goals in mind, an author should spend some time thinking about the way to make the literature review as useful as possible to the reader. Many authors think that it is always better to cite more papers, and automatically include a section that spends one or two sentences on any paper that is conceivably related to her paper. When written this way, literature review sections tend not to have much structure and do not connect the papers to one another or to the author's paper. These types of literature

reviews often seem motivated to give the appearance of being thorough, rather than to explain to a reader where the paper's contribution fits into the existing literature.

Before starting a literature review, an author should ask herself some questions about the nature of the prior literature and its relation to her paper. A key assumption one usually makes when writing up results for the first time in a scientific paper is that one is writing for professionals in one's field, not for undergraduates or the general public.[34] Given that the target reader is a professional, it may not even be necessary to review the literature at all. But if the author does decide to include a literature, what papers should be covered and in what depth? How should she organize the discussion? And where should it go in the paper; does the literature review require a separate section or could the discussion be integrated into another part of the paper? Often the relevant literature can be sufficiently surveyed at the end of the introduction or in another section, so that a separate section surveying the literature is not necessary.

Assuming that you decide that you want to include a literature review section, your goal should be to make it a self-contained document that can be read as background reading by a scholar wishing to learn about a subspecialty. Instead of just writing a perfunctory background section before getting to the results, think about the literature review as something interesting and important in its own right. If a reader is not that interested in the particular results you present in your paper, the literature review you write might be useful anyway if the reader wants to learn about the subfield in which you are working.

Many authors write the literature review as if they are trying to give equal time to all papers and not to offend anyone. I think a better way to approach the section is to start by explaining what the main issues/questions are that the literature has addressed. How has the literature addressed them? What are the main results the literature has found and the pitfalls that it has to overcome along the way? What are the main questions left to be addressed? How does the current paper fit into all of this, and how is it different from the most related extant work?

[34] I don't mean to imply that one *shouldn't* write up one's results for different audiences. I am a big fan of writing about research so that nonspecialists can understand it. Rather, I am saying that when one writes the original article for publication in an academic journal, one should assume that readers are professionals in the field.

In other words, your literature review section should be organized around the *issues* in the literature rather than the *papers.* Of course you still need to discuss all the relevant papers, but do so in the context of the issues they address. A useful approach often is to mention prior work in "groups" based on the approach adopted in each paper. For example: "Some authors obtain identification using weather as an instrument (see xxx). These papers typically find that … Other authors have obtained their identification through regulatory changes adopted in 19xx (see xxx)." This way of discussing the literature can be helpful to a reader because it provides a perspective on the reasons why authors adopted various approaches, the effect of different approaches on results, and potentially indicates which work is most relevant for understanding the current paper. Another useful approach is to group prior work based on their findings, for example, "some papers find that A is positively correlated with B (see xxx), while others make the opposite conclusion (see xxx)."

Perhaps the most difficult issue an author faces when writing a literature survey is deciding what to cite, what to leave out, and how much time to spend on each. There are no hard and fast rules. But she must remember that not all papers are created equal. Make sure to discuss the seminal papers that started the literature in much more detail than the later, more marginal contributions. A reader should be able to get a sense as to what the main issues are in a literature without being overwhelmed by references to too many papers.

Authors do get offended if they are not cited and think they should have been. When deciding whether to cite a paper, try to put yourself in the shoes of the paper's author and ask yourself if you would be offended if it were your paper that were not cited. A good idea is to give the benefit of the doubt to authors and add a citation if it is a close call. However, an author can take this practice too far. Sometimes, however, authors cite every paper that is even tangentially related, leading their papers to become cumbersome and difficult to read. If there are a lot of related (but not too related) papers, one strategy is to add a footnote with a list of somewhat related papers that does not describe any of them in detail. Such footnotes can acknowledge related papers' contributions without affecting the readability of your paper.

It is important to be professional when deciding which papers to cite and how to cite them. Self-citations are appropriate in many circumstances, since a scholar's work often builds on her prior work. But many authors go overboard and cite themselves to an extreme, often to the exclusion of others' equally relevant work. Such a practice comes across as self-serving and unprofessional, and can create unnecessary tension with the authors of the other papers, who are likely to end up refereeing the paper or discussing it at conferences. In addition, authors are more likely to cite work of more established, senior people to the exclusion of younger, less well-known scholars. Young scholars, especially those who are not well "connected," justifiably complain that their papers do not get cited nearly as often as they would if they were written by a more famous and influential author.

Scholars also tend to over cite work by their friends and advisors, and also by the editors of journals in which they wish to publish their paper. This type of behavior can end up making authors look petty and not like serious scholars. When I am a thesis advisor, I often tell my students to drop some of the references to my work that they include in their papers. When I was a journal editor, I did the same. While it can seem like a good idea to add references to the work of people you wish to impress, this practice is obvious to readers and makes a bad impression on them. In general, the best policy about citations is to do your best to ignore who the authors are, and to decide whether to cite or not to cite based on the work itself rather than the identity of the authors.

Theory

The best way to write a theory section of a paper depends on the paper's purpose and what the author hopes to accomplish with the theory section. For some papers, the purpose of the paper is to convey a new idea, which can be done through a formal model or through verbal arguments. For others, the paper is mainly empirical and the model is included to motivate the empirics. Some papers are a combination of the two, in which the model is presented and is calibrated, or the model parameters estimated structurally. The way an author writes the theory section should vary substantially depending on which of these categories the paper falls into.

If the purpose of the paper is to convey a new idea or modeling approach, then the theory section is the paper's key section. Much more detail of the theory should be included in the text if the authors think of the paper as a theory paper than if it is mainly an empirical paper with a theory presented for motivation. Even if the paper is pure theory, it is nonetheless important to think carefully about which details to include, which to omit, and how to describe the analysis to a reader.

Sometimes the main goal of a paper is to prove a hard theorem, to derive an asymptotic distribution of an estimator, or to suggest an easier or more straightforward proof to a well-known theorem. In this case, then the proof itself would of course be a main part of the body of the paper. For more applied papers, however, the main point of the paper is not normally the proofs. Readers clearly want to know that all the propositions are true but usually do not care a lot about how the proofs work. The proofs are, for most papers, essential for the theory to be correct, but not something that needs to be highlighted in the text. Often in this situation, it is best to present the proofs in an appendix, possibly an online one. In an appendix, proofs won't distract the majority of readers from the paper's main message but are still available for the minority of readers interested in seeing them. An advantage of online appendices rather than ones published in journals is that there are no length restrictions on online appendices. If the proofs are in an online appendix, the author can provide interested readers every detail without skipping any steps.

If the author's reason for writing the paper is more applied, then I would encourage authors to minimize the quantity of technical details of the theory in the main text. Remember the general principle that most readers will spend the same amount of time on a paper regardless of its length. If a faculty member has a free half hour before the paper is presented in a seminar, then that is all the time he will spend on the paper. Time that a reader spends wading through technical details is time that he won't spend understanding what you think that we learn from the paper's analysis. For this reason, an author should structure her paper so that readers will spend his time where she wants him to spend it.

When writing a theory section, a good idea is to start simple and then add complications later on. It is great if authors can start with a real world example that illustrates the model's idea. If the behavior

of a real firm follows the behavior of the model, then the model will appear to be more relevant to readers. Starting off by explaining what happened to a particular firm allows readers to relate to the model, and understand that the model is not just algebra, but a characterization of an important phenomenon.

Authors often think that presenting the most complicated version of their model will impress readers when in fact it is more likely to confuse them. Most models are built on one idea, and then add complications to this idea. So when describing a model, start with the main idea. The idea often can be fairly simple when explained by itself but confusing in the context of a complicated model. Once the author explains the main idea and the mechanics of what is driving the model's action, readers will be much more willing to delve into the model's details, even if these details would seem byzantine in the absence of an understanding of the model's idea.

Data Description

A necessary part of any empirical paper is a discussion of the data used in it. Many authors treat the write up of the data description as just that, a necessary part of the paper that they have to write. Not surprisingly, many data sections read as if the authors thought them to be unimportant. Readers of the data section can easily tell that such authors put minimal effort into writing it. Since so many data sections are written without much care, readers tend to assume that the data description will not be important or interesting, and often skip over this section when they read the paper.

One way to improve papers' impact is for authors to make all sections of their papers as interesting and innovative as possible. The data section is no exception. By putting some thought and effort to it, authors can make the description of their data as a positive element of their paper that provides value to readers.

The way an author should write a data description depends a lot on the nature of the data. If it is standard data, then the data section can be shorter. In this case, the focus should be on letting readers know exactly how you structured the database. Which observations are included and which omitted (and

why)? How did you construct the variables? What are the main patterns in the data? How are your data different from that presented by other authors?

One thing that has happened in many fields is that new, very large datasets have become available, often because of the revolution in information technology. For example, some interesting recent work has been based on reviews on the Amazon website or on job histories from LinkedIn. These data can be interesting in their own right. But are they credible? To what extent are they subject to self-selection? Over time, these types of databases will become increasingly important in the social sciences; describing them and their limitations will make data sections less formulaic and hopefully more innovative in the future.

To the extent that your data are new and different from what is in the literature, then the data section can take on an importance of its own. Academic papers tend to be written around tests of hypotheses coming from theoretical arguments. Readers, however sometimes are not particularly interested in the hypotheses themselves, but instead are interested in the paper's topic. They want to learn more about the topic, and might be interested in doing their own related work. For these readers, the data section could be the most important section in your paper. If the facts they want to know are easily accessible, then your paper can be useful to them even if they are not particularly interested in the hypotheses you happen to be testing.

For example, my dissertation was about boards of directors and their role in corporate governance. The three published papers that came out of the dissertation were some of the earliest papers in the economics/finance literature on the topic.[35] When I was writing the dissertation, I was most interested in the hypotheses I was testing: which directors monitor the most, how directors are chosen, and what their effect is on corporate performance. But when I was writing the papers, I was advised to do

[35] See Michael S. Weisbach (1988), "Outside Directors and CEO Turnover," *Journal of Financial Economics*, Vol. 20, pp. 431-460, Benjamin E. Hermalin and Michael S. Weisbach (1988) "The Determinants of Board Composition," *RAND Journal of Economics*, Vol. 19, pp. 589-605, and Benjamin E. Hermalin and Michael S. Weisbach (1991) "The Effects of Board Composition and Direct Incentives on Firm Performance," *Financial Management*, Vol. 20, pp. 101-112.

a good job covering the basic facts about boards. How large are boards and what are their compositions in terms of "insiders" and "outsiders"? What are the backgrounds of the directors? How long do directors and CEOs serve? How often CEOs and directors have prior relationships? Many of the cites my papers have received have been from authors interested in these facts rather than the hypotheses I tested. The data descriptions in my papers apparently have been very useful for readers, and consequently ended up helping my career greatly.

In addition to its own inherent interest, the data description also provides the important role in the scientific process. A thorough description allows others to know exactly what you did in your analysis. The rules of academic research are that all data must be described in sufficient detail so that a stranger with nothing but your paper and knowledge of the field should be able to replicate your work. Consequently, it is important to describe every step you take in detail, even those that are "standard" in your subfield.

An author should write the data section for a potential reader who is a graduate student on the other side of the world and wishes to replicate her analysis. If the writing isn't clear or if details are left out, this graduate student will email the author, usually long after she forgets exactly how the data was constructed. It is extremely helpful to have a detailed documentation in a data appendix that can clear up misunderstandings.

A common problem is that authors are insufficiently clear in their descriptions of the data cleaning process. For example, authors sometimes forget to describe how they dealt with observations that are such outliers that they think are likely to be data entry errors. Were potentially faulty observations dropped or were they winsorized? How did authors decide which observations were likely to be errors? There is often not a single "correct" procedure for dealing with these kinds of issues, but the authors have to make clear to any reader exactly how and why they chose the procedure they used.

Efforts to replicate papers fail surprisingly often.[36] Scholars who try to replicate well-known papers frequently find different results from that reported by the authors. Usually there is an innocent explanation for the discrepancy. Perhaps the author forgot to document something she did, the database changed between the time the author wrote the original paper and the replicator downloaded it, or the replicator made a mistake in their analysis. Nonetheless, when an outsider is unable to replicate published results, it casts doubt on the paper and is embarrassing and professionally costly to the paper's authors.

Authors should always remember that it is in their own interest that anyone who tries to replicate their study can do it. If someone tries and fails to replicate your study, it becomes your problem *even if the reason why they failed to replicate is their own mistake.* People talk, and your paper will become "suspect." In this age of the internet, someone might post that something is wrong with your paper, and once something like that is posted, it is there forever.

Therefore, it is an author's interest to make sure that one's data description is 100% accurate. When possible, you should post the data online; if you are not allowed to post the raw data then post the code. Journals are starting to require authors to do so anyway. But even if it is not required, it is in authors' interests to be as transparent as possible to avoid misunderstandings. A little extra care in describing one's data cleaning when writing the paper can save much heartache later on.

The Conclusion[37]

At the end of a paper, authors sometimes don't know what to say. They have made their main points relatively quickly in the introduction and in more detail in the body of the paper. By the time they

[36] Campbell Harvey has an excellent discussion of the issues involved with replication in his Presidential Address to the American Finance Association. An example of a well-known area of work for which many results could not be replicated is a huge literature in finance documenting "anomalies". Anomalies are patterns in the data that could, if true, lead to profitable trading strategies that are inconsistent with the efficient markets hypothesis. See Campbell R. Harvey (2017), "The Scientific Outlook in Financial Economics," *The Journal of Finance*, Vol. 72, pp. 1399-1440, and Kewei Hou, Chen Xue and Lu Zhang (2018) "Replicating Anomalies" *Review of Financial* Studies, Vol. 33, pp. 2019-2133.

[37] After the description of the data normally comes a discussion of the empirical methods and results. I have devoted the entire next chapter to the issue of reporting empirical results. For this reasin, I skip to a discussion the *Conclusion* section here.

have gotten to the end, it seems a bit silly to repeat everything for a third time. So what should authors put into the "Conclusion"?

The answer to the question of what goes in a conclusion depends a lot on the authors, and whether they have anything more to say. The principle of "less is more" applies here; if the author does not have anything else to say, it is perfectly fine to just have a short summary of the paper, taking up two or three paragraphs. In that case, I would recommend calling the last section "Summary" rather than "Conclusion", since "Conclusion," at least to me, implies there is some broader message beyond merely repeating what was said earlier.

I have ended papers with a short "Summary" and no one has ever objected. But I prefer to use the last section of the paper to think about broader issues related to those the paper addresses. For example, I have a recent paper that proposes a way that firms could use machine learning tools to aid in their corporate governance, in particular, the way they select directors.[38] At the end of the paper, my coauthors and I decided to discuss the way the paper relates to the broader literature about how and why algorithms can sometimes do better than humans at making decisions. We felt that by finishing the paper this way, it gave readers a sense that rather than our findings being a curiosity, they are an application of a larger and important idea.

The conclusion section is the place where one can be a bit speculative. The authors should tell the readers what they really think the results mean, subject to appropriate caveats. They should talk about ways to apply the ideas in the paper to other questions. It doesn't really matter if these thoughts are completely fleshed out. Readers like to finish papers with some additional ideas that they can think about, and are inclined to give authors more leeway for speculative ideas mentioned in the conclusion than for the main ideas developed in the body of the paper.

I like to end the conclusion with a paragraph talking about potential future research ideas. The purpose of an academic paper is to extend our knowledge. But usually a given paper is only one step in

[38] See Isil Erel, Léa Stern, Chanhao Tan, and Michael S. Weisbach, (2020) "Selecting Directors Using Machine Learning," NBER Working Paper 24435.

the learning process. By discussing subsequent work, authors can remind readers that this paper is part of a growing and exciting literature. Although they can rarely act on the suggestions for future research, readers do appreciate hearing them. Such suggestions, even if they are somewhat speculative, can be a positive and forward-looking way to end a paper that highlights the importance of the paper's contribution.

Chapter 7. Reporting Empirical Work

Much research in the social sciences involves analysis of data. These data often come from the real world but sometimes are from experiments or simulations. In a typical research project, the scholar looks at many numbers, does all kinds of tests, shows the results to friends, and then does more tests. When she finally gets around to writing the paper there are many more "results" than could possibly be included in one paper. The author must decide what numbers to report, what to omit, how to report them, where to report them, and how to describe them in the text. There are many publicly-circulated papers that report too many results, but at the same time manage to omit the ones that the reader most often wants to see. These papers can be infuriating to read since the author does not report information the reader would like to know about while at the same time the paper goes on and on about things he doesn't care about.

Sometimes the fault lies not with the author, but with the editorial process. Referees and editors can make the paper *less* reader-friendly and useful by forcing authors to include many pointless robustness checks and caveats in the interpretation of the results. It can be incredibly frustrating when referees and editors force one to include so many additional tests that one's paper becomes overly long and hard for a reader to wade through. Editors and referees too often think they are being diligent and thorough in asking for many additional tests that actually add little to the analysis. In doing so, these editors and referees manage to make papers less reader friendly and impactful while driving the authors crazy at the same time.

The decisions an author makes about how to report results can play a large part in determining a paper's impact. However, the way to go about making these decisions is rarely discussed in classes. We do spend a lot of time in our doctoral programs teaching about appropriate statistical techniques for analyzing data, and our journals regularly publish new methodological advances related to these techniques. In seminars we argue endlessly about issues such as clustering of standard errors and the

validity of instruments. Yet, papers with valid instruments and appropriately clustered standard error estimates get rejected all the time because the authors report their results in ways that readers do not find useful.

A Paper's History and How it is Written

A misleading aspect of journal articles can be the description of the process by which the research was done. Journal articles tend to give the impression that the work was done in the order presented, that the results presented in the published version of the paper were *all* that were done, and that the research process followed the logic discussed in the paper. The author starts with a question, details the three or four main steps she took in the analysis, then presents the results. It can make the research process sound quite simple, leading students who read nothing but the paper to think that doing research is far easier than it actually is.

Most of the time, the research process is much more haphazard than published papers make it out to be. Authors normally do far more work on any research project than is ultimately published, and drafts are usually rewritten and restructured a number of times prior to being publicly available. After circulating the paper and getting feedback, authors rewrite the paper, often making major changes, prior to submission for publication in a journal. The review process itself often leads to even more changes. I've received (and written) referee reports saying something like: "Tables 1 through 4 are terrible but Tables 5 and 6 could be interesting if the analysis were completely redone. Perhaps the journals could think about publishing a paper oriented around the results in Tables 5 and 6 with the following focus…" Then when the paper comes back to the journal, the paper might follow the approach laid out in the report and be almost a completely new paper. However, a reader will never know the history and might in fact think that the published version is similar to the authors' first draft.

I will discuss the review process in detail in Chapter 11, but the point here is that a paper's history can be long and sometimes convoluted. However, the author does not need to tell the entire history of how the research progressed. One of my coauthors once remarked to me that the paper we finally

published after a number of rejections had been "three different papers" along the way.[39] In various drafts, we had changed the emphasis of the writing, the data we used, the hypotheses we tested, the methods we used, and even added a coauthor along the way.

When we wrote the final draft of that paper, we did *not* include anything about the paper's history, the tests we performed in the prior drafts, and the interpretations and implications of those tests. A scholar reading the published version of the paper would have no way of knowing about the multiple drafts, what the earlier versions said, or which results did not make it into in the final version. My coauthors and I tried to present the results in the order in which they make sense intellectually; the order in which we actually did the tests was not relevant for this decision.

When writing a draft of a paper, every author should take a step back, think about the most coherent way to present her analysis, and write it that way. Whether that presentation coincides with the history of how the author actually did the analysis is not relevant, and should not affect the way that she presents her results in the final version of the paper.

How to Write up Empirical Results

Suppose you have finished the analysis on your research project, and it is time to write a draft. You try to write the introduction but get stuck when it comes time to describe the empirical results because you aren't quite sure what they will look like. Which results should you report? Where do you present them, and how do you organize the paper? How do you optimally make use of tables or figures, and how should you structure each to do so? Sometimes it is obvious how one should organize a paper's results. Other times, authors can go through multiple revisions before converging to a structure that they are happy with.

[39] If you don't believe me, the original version is actually still available online as a working paper. Compare "What Determines the Structure of Corporate Debt Issues?" by Brandon Julio, Woojin Kim and Michael S. Weisbach, (NBER Working Paper 13706) with "Macroeconomic Conditions and Capital Raising," by Isil Erel, Brandon Julio, Woojin Kim and Michael S. Weisbach (2012), *Review of Financial Studies*, Vol. 25, pp. 341-376.

In most research projects, the author has a fair amount of discretion over which results to report. She cannot publish every single test she has done and must decide which ones to include in the draft and which ones to omit. Often it is not obvious exactly how to make these choices.

Suppose an author is writing a paper in which she has a hypothesis that comes from a theoretical model, and her research consists of a test of that hypothesis. For example, she could be studying a model in which political factors related to the uncertainty about the current or next government affect the cost of capital of particular kinds of firms. She gathers a sample of firms and finds a setting in which she can measure both the political uncertainty and cost of capital for that particular set of firms. She then comes up with a way to identify the relation causally and then does the estimation. Through this process, she makes several choices: what sample to use, how to measure the variables of interest, how to do the estimation, etc. At the end of the day, she wants to come to some conclusions about what the data are telling her about the validity of the theory she is testing.

As she decides what to include in the empirical section of her paper, her goal should always be to give a fair assessment of the empirical findings and what they mean. The results she does report should be selected with the goal of persuading a skeptical reader that the empirical results are robust and the inferences she draws from them are appropriate. However, these inferences are conditional on the choices the author made throughout the research process. Readers will wonder whether the paper's conclusions are sensitive to the choices made. It is the author's job to persuade such readers that she has given an honest accounting of what she did, what factors are likely to influence the interpretation she makes, and the extent to which the choices she made in her experimental design matters for the paper's conclusions.

With these objectives in mind, an author should spend some time thinking about the optimal way to report her results. For any project, there are likely to be any number of possible things that she could choose to report. Some of these things are absolutely necessary, some are optional, and others are to be avoided altogether. I will discuss each type in turn.

What does an author have to report?

In the "absolutely necessary" category are all results needed to satisfy two particularly important concerns that apply to almost every empirical paper in any field: replication and robustness. A writeup must include sufficient detail so that a stranger can replicate the analysis using only the information provided by the author. Replication is an important part of the scientific process, and there has recently been much controversy in the social sciences about papers that cannot be replicated. When other scholars try to replicate published work, they often end up with different results from that in the original paper. Often the failures have to do with careless reporting in the original paper; the authors did not say (and sometimes cannot remember) exactly what they did.

Nowadays, since coding is such an important component of data analysis, a number of journals are requiring authors to share their code publicly. This practice is a good one and I encourage authors to do post their code publicly regardless of whether doing so is required by the journal. When sharing code, authors should make efforts to keep their code "clean" and well-documented so that an outsider can easily understand it. Providing user-friendly code so that a paper can be replicated easily is in the interest of authors, as the consequences they face if others try and fail to replicate their findings can be substantial.

The second crucial element in a professional reporting of empirical results is a serious discussion of their robustness. An author has to make many choices when conducting an empirical study. These choices concern the sample construction, the treatment of outliers, the empirical specification, and which results to report. Readers naturally wonder how these choices affect the paper's conclusions. When going through any empirical paper, skeptical readers are likely to have the following sorts of questions: Had the authors used a different approach would they have gotten a different answer? When the author eliminated observations that she thought were typographic errors in the database, did she actually do something more sinister (maybe by accident), and create a spurious relation in the data? Is the paper's conclusion a location or period-specific result or is it more general? Is the statistical approach appropriate, and would have alternative ways of doing the estimation had led to different implications?

The author's job is to convince a reader that she has addressed all these issues and has given a fair accounting of them in the write up. Sometimes, it is a good idea to err on the side of being a bit too

thorough with robustness checks, so as to leave no doubt in the reader's mind. It is important to be careful when writing up these tests to emphasize what is really going on in the data, and to do so in a way that a reader who is not interested in the robustness checks can easily skip them.

What does an author want to report?

Ensuring replicability and robustness is part of convincing a reader that there is nothing wrong with your analysis. But a good paper has to be more than correct. There has to be something interesting and unique about a paper that will attract attention from potential readers. Hopefully, there are a number of such things.

An author should start from the assumption that most readers will spend a fixed, relatively short, amount of time on the paper. She should try to structure the writeup to maximize the interest of such a reader. The way to do so is to focus the most prominent parts of the paper around the issues that the target reader will find most interesting. The other elements that are necessary to include such as robustness checks, proofs, etc., should be put in less prominent parts of the paper, either separate sections or appendices.

Often a paper will contain much more useful information than is relevant for testing the specific hypotheses considered by the paper. A good empirical paper can provide institutional background and background facts that will be useful to others. For example, private capital markets have become extremely important in the economy, yet they have just become a major area of academic research in the last few years. Academics have been relatively slow to understand these markets because data on private markets are usually private and unavailable to academics for research purposes. In addition, these markets function using institutions that are different from those traditionally studied in academia. Consequently, the early scholars studying these markets such as Paul Gompers, Josh Lerner, and Steve Kaplan have had a number of papers whose impact comes in part because they laid out the institutional environment of this market well, and provided many facts about it that others would find useful.

These authors went out of their way to provide the facts that would be useful to readers, regardless of whether those facts were relevant for the hypotheses they were testing the paper. For example, Steve Kaplan's job market paper examines the hypothesis that firms increase in value when they undergo a leveraged buyout.[40] Along the way, he provides numerous facts about leveraged buyouts, such as their ownership structures pre- and post-buyout, their capital structures and the incentives that are provided to managers in the buyout. While these facts are not necessarily crucial for testing his main hypotheses, they are interesting by themselves, and his paper often gets cited because it includes these facts. Kaplan was able to make his paper more impactful by using his space wisely and reporting as many novel facts as possible that were of interest to readers.

What does an author not want to report?

With most research projects, certain steps are more or less standard in the literature. Readers have seen something similar to these steps many times and are likely to have done them personally as well. For example, in finance, we often estimate what we call "beta," which is a commonly used measure of a security's risk. If a finance professor estimates beta in a standard fashion, she must document exactly how she did the estimation so that an outsider can replicate her results. However, there is no need to spend much time discussing the estimation because most readers are likely to find such a discussion to be a little boring and will tend to skip over it. In this situation, it might make sense to include sufficient detail for replication but to keep this discussion short, and to try to focus the readers' attention on the parts they will find more interesting.

There is a substantial opportunity cost to everything an author discusses in detail in her paper. If the author occupies his attention on something mundane that he has seen before, he not only will be bored but will spend less time on the material the author wants him to focus on. An author's goal is to have her

[40] See Steven Kaplan (1989) "The effect of management buyouts on operating performance and value," *Journal of Financial Economics*, Vol. 24 pp. 217-254.

paper be known for the parts of it that are novel and interesting. Any space spent on well-known things will distract readers' attention from those parts. Papers do sometimes get rejected because reviewers never get to the interesting part of the paper. In papers that do get published, their impact can be lowered if it is not evident to readers exactly what the paper's new contribution is.

Where to Report Results

When an author is thinking about reporting empirical results, the goal is to maximize the usefulness of the paper to each possible reader. What makes this process complicated is that there are likely to be many different types of readers to whom the author wishes to cater. The vast majority of readers are casual readers, who will glance through the abstract and introduction prior to the paper's seminar presentation or when it comes out in a journal or an online blog. These readers might look at a table or two but will not spend more than 5 or 10 minutes on the paper regardless of how long it is. Other readers are willing to dive more into the details of the paper; they could be making an effort to learn the paper's literature and thinking about working in the area in the future. Finally, there will (hopefully) be a few readers that read the paper extremely carefully and go through every table in detail. These readers are potentially doing related work and could want to understand everything in the paper. The challenge an author faces is to write her paper in a manner that appeals to each type of reader.

How does an author balance the interests of different kinds of readers and make her paper interesting to all of them? It is a bit of an art, and authors do not always get the balance right. Sometimes I read papers that go on and on with so many details that I have trouble finding the important and interesting sections. Other times, papers skip over so much that a reader working on a related paper has to contact the author to find out what exactly she did. Sometimes the author will fail to report or hide obvious robustness tests, leading others to question the paper's conclusions. Reaching a "happy medium" is sometimes difficult.

One factor that can help readers sort through a paper to the degree they wish is if a paper has a careful and transparent organization. Most empirical papers have a "main" finding, sometimes two or

three of them. The rest of the empirical tests are designed to convince readers that this result is correct and not spurious for one reason or another. For such a paper, I think that a sensible approach is to separate the "main" results from the robustness tests. The author should get to the main table quickly, and make it as easy to understand as possible.

After the author presents the main result, perhaps in a separate subsection, she can go through the litany of potential objections it. Some results that are likely to be sufficiently specialized that they are of little interest to most readers can go into an online appendix. A casual reader is likely to read the section about the main result and skip some of the robustness tests, while a more interested reader will be more likely to read the discussions of all of them. The key thing for the author to do is to make it easy for readers to figure out what is the main test and what is robustness.

An important decision authors have to make concerns which results she presents, and the order in which she presents them. For most papers, an author has estimated a number of alternative approaches and can only include a small fraction of them in the paper. Should she present the estimates in levels, or in first differences? Which control variables should she include? Which sample period? For most empirical papers, the number of potential variants in specification can go on and on. Usually, there is not a "correct" answer to all of these questions. Instead, the author should try to give the reader a sense as to the factors to which the result is robust and the factors which can change it.

One approach to organizing a paper authors sometimes use is what I call the "mystery novel" form of organization. In a "mystery novel paper", the author presents results using a seemingly plausible approach. She then would explain to the reader what is wrong with this approach. Over the course of the paper, she changes the specification, adds and subtracts variables from the equation, and presents alternative models. By the end of the paper, the "mystery" is solved and the author presents what she argues to be the "correct" specification, usually in Table 9 or 10.

Some people like these kinds of papers, but I personally find them infuriating to read. When I read an academic paper, I want to know quickly what the paper's arguments are and what its conclusions are. For example, if the author thinks fixed effects belong in a specification, then she should say why they

belong upfront and put them into the main specification. Some authors, however, might present results without fixed effects first, and then go on for several pages about why the first set of results are not correct because the fixed effects should in fact be there. These authors would eventually get around to reporting the results with fixed effects. However, a reader who is pressed for time could easily miss the main point of the paper and look at the wrong table when trying to understand the results.

How to report results

In addition to *what* results to report, and *where* to report them, an author has to decide *how* she wants to report her results. In other words, should the results be presented in a table or a figure, and how should each be constructed? The basic issue concerns the way that data should best be presented to a reader to convey the author's message in the most compelling manner. Unfortunately, authors often use an overly formulaic approach to presenting data. In economics, the most common approach consists of a table or two of means and medians, followed by regressions with the dependent variable at the top and the independent variables listed vertically along the left-hand margin. While there is nothing wrong with this way of presenting data – I often do it that way myself – authors often use this approach out of laziness rather than any belief that this is the optimal approach.

Real world data is usually complicated and multidimensional. To perform statistical analysis on it, a researcher must collapse it to a manageable level, usually focusing on one or two variables that can be analyzed statistically. In doing so, much interesting information can be lost. Statistics sometimes does a good job of conveying the data's basic message at the cost of losing some of its texture. When presenting results, authors should attempt to structure their presentations to ensure that they convey as much of what is interesting to readers as is possible. Often graphical analyses can help to convey such information in a convenient manner.

A classic book that describes innovative ways of displaying data is *The Visual Display of Quantitative Information*, by Edward Tufte.[41] I would encourage all scholars to read this book carefully and to think seriously about adding it to their libraries. Tufte's book contains numerous examples of graphs and charts that have been structured to fit data and display its texture in ways that could not be done using standard formulaic approaches.

The graph that Tufte claims was "the best statistical graphic ever drawn" is the illustration that tells the story of Napoleon's 1812 invasion of Russia, which was created by the French civil engineer Charles Minard in 1869. I have copied an English translation of the graph into Figure 1.

For those of you who do not know the story of Napoleon's invasion of Russia, please stop working on your papers for a few days and read *War and Peace,* or a history of the Napoleonic Wars. This invasion is one of the great tales in recorded history and every educated person should know the story.[42] On June 24, 1812, Napoleon invaded Russia with 422,000 men (and 180,000 horses). After fighting a number of battles including a particularly large and bloody battle at Borodino, he was able to enter Moscow on September 14, 1812. Finding the city without food and other resources, Napoleon and his troops retreated back to France, fighting the particularly cold winter and the Russians along the way. Of the original force, only 10,000 soldiers, less than 5 percent of the army's original force, were able to make it out of Russia.

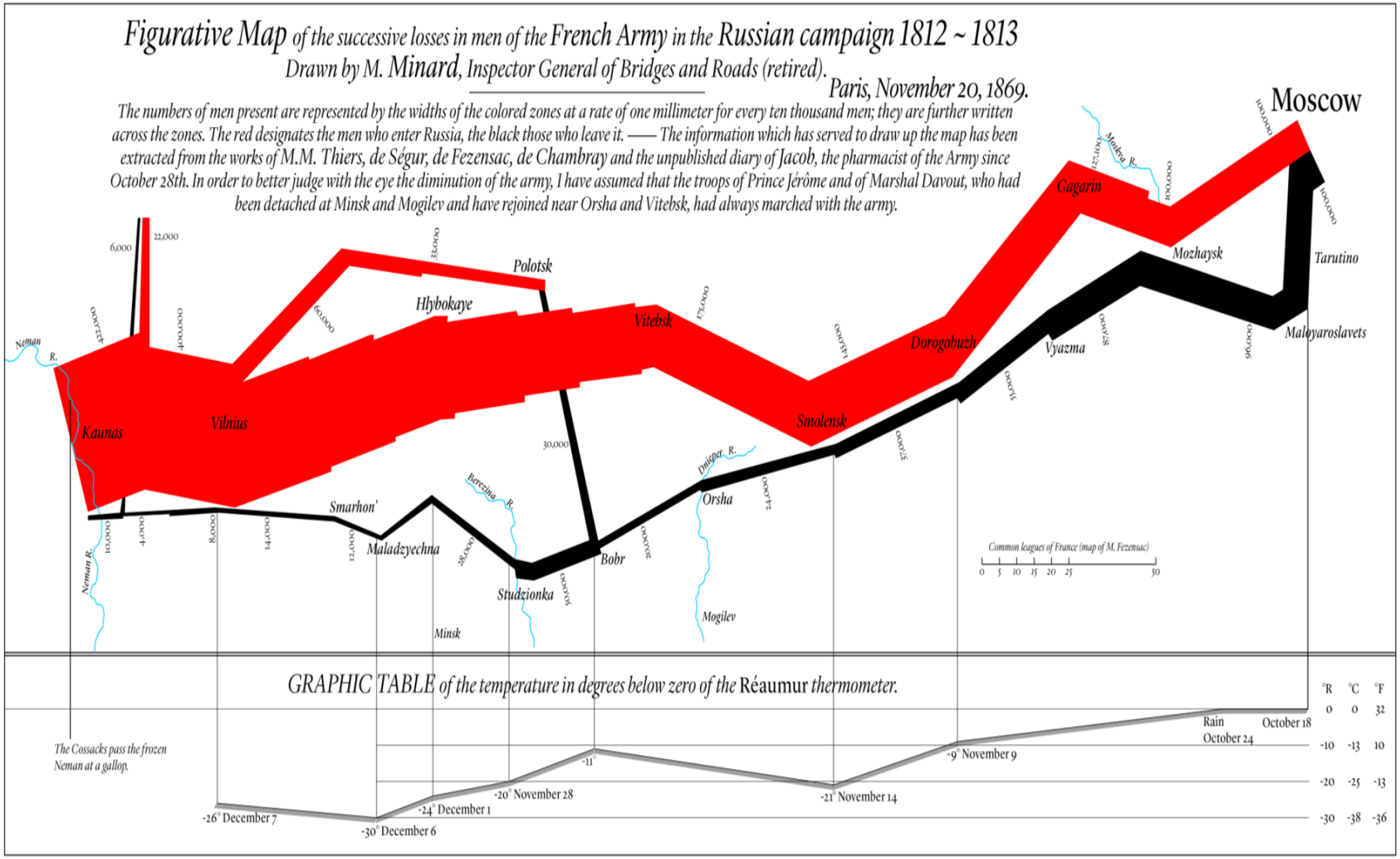
Minard's graph illustrates, in one picture, many aspects of the story. The graph is superimposed on a map of Russia so it is possible to see the routes Napoleon took both in and out of Russia. The red line (tan in the original illustration) shows the route he took to Moscow while the black line represents the more southerly route out. The width of the line is proportional to the number of men Napoleon had at any point in time, and major cities/battles (Smolensk, Moscow) are illustrated as well. Decreases in the lines' width mark points where Napoleon lost men. For example, the black line gets noticeably thinner when a

[41] See Edward Tufte, *The Visual Display of Quantitative Information*, published by Graphics Press in 2001.
[42] A good source is Andrew Roberts biography *Napoleon: A Life*, published by Penguin in 2014. It is not surprising that Roberts uses the Minard graph as part of his discussion of the 1812 invasion of Russia.

number of French soldiers died when the army (barely) crossed the Berezina River under fire. Finally, and perhaps most illustrative, Minard includes the temperatures Napoleon faced while retreating at the bottom of the graph, which highlights the extreme cold that the troops faced during the Russian winter. The Minard graph is still celebrated today because it does an extraordinarily good job of illustrating so many aspects of Napoleon's disastrous campaign in one picture.

Minard's graph is special in part because he designed it to fit what he wanted to illustrate. As such, he was able to capture the geography of the campaign, the enormous troop losses, and the locations of the losses, as well as the extreme temperatures faced by the troops. I find it telling that this graph was

Figure 1



Modern redrawing of Charles Joseph Minard's figurative map of the 1812 French invasion of Russia, including a table of temperatures converting degrees Réaumur to degrees Fahrenheit and Celsius

made in 1869, many years prior to the invention of Excel. Perhaps because Minard was not able to rely on standard packages, he invented something far superior to what most people would have done today!

While most researchers will not come up with something as innovative as Minard did, they can improve their papers substantially by thinking carefully about the problem they are trying to address, and structuring their presentation of the empirical analysis accordingly. Sometimes, sophisticated graphs are unnecessary but detailed verbal descriptions can play that role.

For example, my former colleagues Harry and Linda DeAngelo are very good at supplementing their papers with appendices containing short case studies describing each observation in their sample (when they are working with relatively small samples). Their work is often about corporate structure, such as the ownership structure or the way that firms negotiate with unions.[43] The appendices provide texture about exactly who owns the company's securities and their relationship with the company, as well as the timing and specifics of the union negotiations. I always found the appendices to their papers to be particularly interesting reading, much more so than most academic papers. While probably not a requirement for publication, the extra flavor the DeAngelos bring to their work through these extra description makes it more meaningful to me, and probably to others as well.

Interpreting results

Finally, after an author presents her work, she must provide an interpretation for the readers. What do the results mean? What are their implications for important theories? Are there other potential implications of the work, for example, for public policy?

Authors normally begin their analysis in an empirical paper with a question, often a set of alternative theoretical possibilities that the empirical work can help distinguish. After reporting the results, an author should go back to the original question and explain to a reader what the analysis has

[43] See for example Harry and Linda DeAngelo, "Managerial Ownership of Voting Rights: A Study of Corporations with Dual Classes of Common Stock," *Journal of Financial Economics*, 1985, Vol. 14, pp. 33-69 and "Union Negotiations and Corporate Policy: A Study of Labor Concessions in the Domestic Steel Industry in the 1980s," *Journal of Financial Economics*, 1991, Vol. 30, pp. 3-43.

taught them about it. Ideally, there should be a "story" of what she thinks the data are trying to say through the lens of the paper's statistical tests. An author should be honest about the extent to which her results rule out alternative stories, and the extent to which readers should think of them as somewhat suggestive rather than conclusive.

A common mistake that authors make when interpreting results is focusing too much on statistical significance and not on the magnitudes of their estimates. Empirical papers often are written so that when a coefficient has a p-value of .05, it is almost definitely different from zero, but if the p-value is .06, then the coefficient is almost definitely not different from zero. However, in each of these two cases, the appropriate interpretation is that it is more likely than not that the coefficient is not zero, but there is some small chance the coefficient equals zero. Too much is made in research in the applied social sciences about the difference between "significant" and "insignificant" results.[44]

When I read papers, I try to focus mainly on the coefficients themselves rather than their statistical significance level. What are the actual estimates? Are they large enough to matter? If we change the independent variable by a reasonable amount, say one standard deviation, how much do the estimates imply about the change in the dependent variable. These are the questions authors should be focusing on and readers should think about in the vast majority of empirical papers. Instead, academics tend to obsess about whether the coefficients are statistically significantly different from zero, ignoring the more interesting and important issue of the size of the coefficients, as well as what they imply about the issue that the paper is addressing.

In my mind (but not everyone's), it is even perfectly fine to discuss the magnitude of statistically *insignificant* coefficients while at the same time ignoring significant ones. In a well-specified equation, every estimate represents an unbiased estimate of a coefficient. Suppose the best estimate of a coefficient you are interested in is 2.0 and it has a t-statistic of 1. Then even though we cannot reject the hypothesis

[44] In addition, there is always a lot of noise when measuring standard errors, so reported p-values are often misleading.

that the coefficient equals zero at conventional statistical levels, it is still more likely to be 2 than 0 and certainly more likely to be 2 than -2. Why not say so in the paper if this coefficient is one number that readers are likely to be interested in? It is incredibly misleading and almost dishonest to act as if it is more "conservative" and more "scholarly" to ignore the information contained in estimates that are not statistically significant at conventional levels.

Authors differ in their approaches to interpreting results. Some authors are fairly aggressive when they interpret results, and like to draw strong conclusions from it about major theories. These authors tend to be particularly aggressive in their interpretations when the results confirm the author's prior view. Others are more conservative and interpret the results more narrowly. These more conservative authors like to give credence to all potential theories, sometimes even ones that are *a priori* implausible to most readers.

A famous example of two papers using differing approaches to interpretation occurred in the early 1980s. In 1981, one paper was published by Leroy and Porter, and another by Shiller (mentioned in the previous chapter) that developed what are known as "variance bounds" tests.[45] The idea that these papers test is one of the important theories in finance and economics called the "Efficient Markets" theory. This theory states, in layman's terms, that the movements of stocks and other securities is only a function of rational expectations of future payoffs, which for stocks come in the form of dividends. The alternative to the efficient markets theory was most famously posited by Keynes, who argued that investor psychology, or what he called "animal spirits" can determine stock prices in addition to fundamentals.

The Leroy/Porter and Shiller papers both presented similar statistical tests that document that the variance of observed stock prices is too large to be justified by subsequent changes in dividends, as would be predicted by the simplest form of efficient markets theory. But the papers differ in their interpretations: Leroy/Porter are relatively cautious, focus on the statistical analysis, and did not draw strong conclusions

[45] See Stephen LeRoy and Richard Porter, "The Present Value Relation: Tests based on Implied Variance Bounds," *Econometrica*, 1981, Vol. 49, pp. 555-574, and Robert Shiller, "Do Stock Prices Move Too Much to be Justified by Subsequent Changes in Dividends?" *American Economic Review*, 1981, Vol. 71, pp. 421-436.

about the implications of their work for the theory of efficient markets. In contrast, Shiller viewed his tests as important evidence against efficient market theory.

Economists like to argue about which paper had the appropriate interpretation of these tests. Many have criticized Shiller for overinterpreting his results and ignoring other possible explanations. For example, *expected* returns varied over the sample used in Shiller's paper, which could lead to patterns similar to the ones he documented, even if the efficient markets theory were true. The literature, sparked in large part by these two papers, has gone back and forth in its interpretation of the patterns these papers document.

There sometimes is a payoff to authors to interpreting their results strongly. In part because he interpreted his results strongly, Shiller was able to build a persuasive case not just of the statistical correctness of his tests, but also of the important big picture implications of his results concerning the way in which capital markets work. Today, not all academics agree with Shiller's interpretation of his results. Nonetheless, in 2013, Robert Shiller was awarded the Nobel Prize for this paper and his follow-up work questioning the efficient markets hypothesis, which emphasized the importance of investor psychology in capital markets.

The appropriate way to interpret results with multiple possible explanations is something academics argue over constantly. Authors often are asked to "tone down" their interpretations by referees and editors, who want papers to come across more scientifically and less argumentatively. My own view is somewhat in the middle. I think it is important that any author give credence to all possible explanations of her results. However, I also feel that an author's responsibility is to let the reader know what she thinks the results really mean and what is the most plausible explanation for them.

Chapter 8: Writing Prose for Academic Articles

As I discussed earlier, academia can be thought of as a marketplace in which researchers compete over ideas. The competition occurs through our articles and seminar presentations. Scholars introduce new ideas that compete with the accepted ideas, with the reward being status in the profession for the scholar whose ideas turn out to be the most influential. In most fields, this competition occurs in English, so the more facile a researcher is with the English language, the more successful she is likely to be. It is not a coincidence that the most successful academics tend to be excellent writers, public speakers, and teachers, regardless of their field.[46]

Sometimes, scholars take the view that as academics, our work should speak for itself and there should be no need to explain it. This view, which is often loudly expressed when academics complain about their work not being appreciated, often at bars late at night or on internet message boards, espouses that smart readers should understand research without much need for explanation. Can scholarship be judged "apolitically", based solely on its merits? To what extent does writing matter and can it be separated from the contributions it describes?

A very succinct illustration that sometimes writing does not matter is a paper by John Conway and Alexander Soifer, supposedly the shortest mathematics paper ever written.[47] The original submission was entitled:

"Can $n^2 + 1$ unit equilateral triangles cover an equilateral triangle of side $> n$, say $n + \varepsilon$?"

[46] There are exceptions. One Nobel Prize winning economist is completely incoherent when speaking publicly. I have attended three or four of his seminars over the years, and each time regretted being there after the first 5 minutes of the talk. This economist can get away with being such a poor speaker because his contributions were so important that they have become celebrated in spite of his poor presentation skills.

[47] See Conway, John H. and Alexander Soifer, 2005, "Covering a Triangle with Triangles," *American Mathematical Monthly*, Vol. 112, p. 78. For an entertaining discussion of this paper and other related issues, see Alexander Soifer, 2010, "Building a Bridge III: from Problems of Mathematical Olympiads to Open Problems of Mathematics," *Mathematics Competitions*, Vol. 23, pp. 27-38.

The body of the paper consisted only of the words *"n^2 + 2 can"* together with two diagrams presenting the way in which the example is constructed. However, without the consent of the authors, the editors of the *American Mathematical Monthly* moved the authors' suggested title to the body of the paper and added a different title, so the published version is slightly longer than the authors intended. Nonetheless, the authors' point is that to answer the question they raise, the picture speaks for itself and no explanation is necessary.

The Conway and Soifer paper, however, is not the shortest academic paper ever published. In 1974, the *Journal of Applied Behavioral Analysis* published a paper by Dennis Upper, which had the title *"The Unsuccessful Self-Treatment of a Case of 'Writer's Block' "*. The body of the paper contained no words, but there was a humorous note from a referee at the bottom of the page: "I have studied this manuscript very carefully with lemon juice and X-rays and have not detected a single flaw in either design or writing style. I suggest it be published without revision. Clearly it is the most concise manuscript I have ever seen – yet contains sufficient detail for other investigators to replicate Dr. Upper's failure. In comparison with other manuscripts I get from you containing all that complicated detail, this one was a pleasure to examine. Surely we can find a place for this paper in the Journal – perhaps on the edge of a blank page."

The Importance of Language in Academic Articles

These amusing examples aside, for the vast majority of academic articles, their impact depends crucially on the way in which they are written. It goes without saying that the English in a paper must be "correct". When I review papers, my tolerance for grammatical mistakes and typos has declined over the years, and consider too many typographic mistakes to be grounds for rejection, regardless of the paper's content. While some would disagree and say that papers should be judged on "merits" rather than "presentation," most editors are happy when referees reject badly written papers, even if the papers have other desirable qualities. Authors are presumed to be professionals; grammatical mistakes and typos

indicate a lack of professionalism and care about their work that is probably indicative of other aspects of the work.

There is much more to having a well-written paper than simply having correct grammar and no typos. A well-written paper has to explain why the question it asks is interesting, what the paper's contribution is, why a reader should care about this contribution, and what its implications are for our understanding of larger issues. And it should accomplish these tasks in a style that the reader finds easy to go through.

I learned the importance of having a paper that is easy to read and explains its point carefully when I was a graduate student. One of the most popular papers among MIT economics students in the 1980s was an *Econometrica* paper by Jerry Hausman, a professor of ours, that develops a new kind of "specification test".[48] This test is a way of examining the underlying assumptions of a model (such as whether independent variables are correlated with residuals) by comparing the coefficients estimated under the assumption of no misspecification to those of an estimator that is consistent regardless of this assumption.

As graduate students, we were amused to discover that in 1973, five years prior to the publication of Hausman's paper , there was another paper, also published in *Econometrica*, that proposed essentially the same test as Hausman's.[49] We did not understand why this paper, by a econometrician named Wu, did not scoop Hausman's. Given that Wu's paper was published five years before Hausman's, why was Hausman's paper considered so important? Why was it even published at all?

One day, perplexed by this issue, I sat down and read Wu's paper. The contrast between the two papers could not be more evident. Hausman's paper is beautifully written (by the standards of statistics

[48] Yes, at MIT, papers were popular or unpopular among the students, as we spent endless hours discussing the merits and faults of different research papers. It says something about the mentality of MIT economics students, a number of whom would become world-renown economists, that we could take such a liking to a paper that whose contribution was something as dry as a chi-squared test that evaluates the fit of a model. Of course, this "popularity" might have something to do with the (probably incorrect) belief we had that including such a test was a necessary requirement for students to pass their econometrics paper. See Jerry A. Hausman, (1978) "Specification Tests in Econometrics," *Econometrica*, Vol. 46, pp. 1251-1271.

[49] See De-Min Wu, (1973) "Alternative Tests of Independence Between Stochastic Regressors and Disturbances," *Econometrica*, Vol. 41, pp. 733-750.

papers, which are never Shakespearean). Hausman's paper explains why specification tests are important and lays out the details of the test as simply as is possible. Wu's, on the other hand, presents a lot of equations without really saying what they mean. As a nonspecialist in econometrics, I'm sure if I read Wu's paper without knowing about Hausman's first, I would not have grasped its importance.

Despite the fact that Wu's paper was published five years earlier, Hausman's paper has had much more impact than Wu's. At the time of this writing, Hausman's paper has 18,342 cites on Google scholar while Wu's paper has 1,173.[50] In addition, a number of the cites to Wu's paper were from people like me who know about Wu's paper only because of Hausman's. Consequently, if Hausman had never written his paper, Wu's would have had less impact than it actually did. Today the test is commonly referred to as a "Hausman-Wu" test, or sometimes a "Wu-Hausman" test. It is likely that Wu's paper benefitted from Hausman's being published, even though at the time he probably felt that his work should have precluded the publication of such a similar paper.

How to Improve One's Writing

A reader might be thinking at this point that she already knows that writing is important, but what she does not know is how to make her writing better. Unfortunately, there is no magic formula for improving one's writing. Writing is one of those skills that does not come naturally to everyone. However, it can improve with effort, and it is a good idea for scholars in all fields to put effort into continually improving their writing skills. Most universities have writing centers designed to help students write better, and there are many books devoted to writing improvement. I encourage young scholars to seek help in improving their writing from the many sources they are likely to have available to them.

I frequently observe that people who write well tend to like writing, while people who write poorly often think of it as a chore that they hate to do. This observation occurs in part because people

[50] The difference in citations could also reflect that Hausman is a famous MIT professor and Wu taught at Kansas. No one ever said that academia was fair.

usually like to do things they are good at. But it also occurs because people who like writing spend more time on their prose. They tend to reread their papers many times to ensure that the text is as readable as possible. Good writers often think that finding the best way to explain or motivate an issue in a paper is an issue as important as the way they do the statistical analysis, or any other major component of the research project.

Academics who dread writing papers commonly refer to the writing of their papers as the "write-up." This dread is especially common for scholars for whom English is not their native language, because writing academic papers is doubly difficult for them. In fact, the use of the phrase "write-up" undermines the importance of the writing of the paper. It makes it sound perfunctory rather than an essential part of the research process.

Academics who do not like to write often think of research as separate from the write-up. Only when they have finished much of the analysis do they force themselves to write a draft. They also tend not to proofread the draft as carefully as they should. They almost never put in the effort revising the prose to make it as readable as possible. As can be seen with the Wu-Hausman example, having prose that is easy to read rather than merely "correct" can make a huge difference in a paper's impact.

I think that instead of viewing the "research" and "write-up" as separate things, it is more productive to view the write-up as part of the research itself. A scholar who prides herself in doing the very best job in structuring her experiment and performing the statistical work should also pride herself on putting together the most coherent draft. Putting together a draft that is easy to read is every bit as much a part of the research process as anything else that a researcher does.

When I was a doctoral student, my advisor, Jim Poterba, suggested that I spend some time studying the work of scholars whose writing I like and whose papers I wanted to emulate. Jim suggested I use Marty Feldstein and Larry Summers, two very prominent economists who write well. So I read some of their papers. Then I read my own job market paper. Then I read theirs again. Not surprisingly, there was a huge difference in the quality of the writing, both in terms of the way the paper was structured and the quality of the prose. This exercise made clear to me exactly where my writing was lacking and how it

could be improved. In the end, my job market paper, as well as my subsequent papers, got much better through this process.

I often suggest to my students that they do a similar exercise. I encourage them to consider the work of a prominent scholar doing research somewhat related to their own. They should pick someone who they think communicates well, and also who does interesting research. The student should then read some of the prominent scholar's papers carefully. Then they should read their own papers. Then repeat the process, reading both sets of papers a few more times. The differences between their papers and the prominent scholar's are almost always obvious to the students. They notice the differences in the writing quality, but also in the depth of the analysis and the importance of the issues being discussed. The process can be painful, but it usually leads students to produce much better papers.

Another thing that can help an author produce better written papers is to read a lot outside of one's field, especially high quality writing by nonacademics. We all get stimuli from the world around us, and unconsciously copy things that we are exposed to. Writing works in a similar manner. We tend to mimic the stylistic patterns of what we read. So if a scholar makes an effort to read well-written non-academic books, she will be more likely to write well-written articles herself.

Academic writing tends to be overly dry and full of jargon. If all one reads is the writing of other academics, then one's writing will tend to become like most academics' writing, i.e., formulaic and not particularly exciting to read. Reading nonacademic writing leads scholars to use more interesting prose that nonspecialists can understand and that specialists enjoy reading.

There are many places where one can look for nonacademic reading that is both interesting and will help one improve one's writing skills. Some of one's reading should be articles about politics or current events. But a significant component should also be serious, well-written books on subjects that one is interested in. Fiction and nonfiction work equally well.

Personally I enjoy reading books about history, and find that doing so helps my academic writing. There are many nonacademic historians that write well about fascinating topics. Since both economics and history study the way humans interact with one another, I find well-written history to be a natural

place to find inspiration for economics research. When I become frustrated with my own writing, I sometimes reread some books by Bruce Catton, my favorite writer about the American Civil War. I find that Catton has an entertaining yet elegant way of describing dramatic events and a skill for turning a phrase that we all wish we could emulate.[51] After reading a Catton book, I feel my prose improves, although perhaps that is an illusion.[52]

There are also many well-written books about economics by nonacademics. These books can provide interesting perspectives on our field and examples on how to write about it effectively. One author who is particularly good about writing about economics is Sylvia Nasar. Her biography of John Nash, *A Beautiful Mind*, is a classic and was made into a popular movie. Another of her books, *Grand Pursuit: The Story of Economic Genius,* tells the reader about both the personal lives and the contributions of some of the most important economists over the past 200 years. Both books provide excellent examples of how to write about economics in a clear and entertaining fashion.

A graduate student should invest in writing just as she would in math, computer programming, data, modeling skill, and institutional knowledge. Whenever she reads an academic paper, in addition to understanding its scientific contribution, she should think about what makes the paepr easier or more difficult for her to read, and what aspects of its writing and presentation makes it a more or less valuable contribution to our literature.

There are many books and articles available that discuss how one can write better, a graduate students should make a point of investing her time in at least some of them. A few particularly good sources for young economists are Cochrane's *Writing Tips for PhD students*, Zinsser's *On Writing Well*, Strunk and White's *The Elements of Style*, and McCloskey's *The Rhetoric of Economics*.

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[51] For example: "A certain combination of incompetence and indifference can cause almost as much suffering as the most acute malevolence." Bruce Catton, *A Stillness at Appomattox*. Or: "His soldiers and the country might have been better off if Burnside had been more of a quitter, but that was one defect which he lacked." Bruce Catton, *Glory Road.*

[52] There are of course many other historical writers who also would provide inspiration for anyone writing about the social sciences. A very incomplete list might include Barbara Tuchman, David McCollough, Ron Chernow, Stephen Ambrose, Winston Churchill and many others.

What Style of Writing should one use in an Academic Article?

Whenever one writes something, one must decide on the *style* of writing to use. By *style*, I mean the way that the author structures her sentences and paragraphs, the way she organizes the paper, the language she uses, etc. Note that this use of the word *style* is different from the way it is typically used by English teachers. If one Googles "style of writing", one sees a classification of writing styles into four categories: expository, persuasive, descriptive, and narrative. Apparently, the standard view is that academic writing falls into the "persuasive" category, which seems to me to be completely wrong. Academic research is not intended to persuade the reader of anything; it attempts to evaluate data honestly and draw reasonable inferences from it.

An academic article should have a certain degree of formality to it. For example, I would never use a contraction in an academic article, even though I use them all the time when I speak (and when I write this book). But even though an academic article is a formal document, an author has a large degree of discretion over the style of writing she can use. To see why, take a look at some of the papers that have been presented in your seminar series lately. Probably some of them are relatively "stuffy" and formal, with a lot of big, flowery words, long sentences, and paragraphs that go on forever. Others might be mostly equations without too many words explaining what the equations mean. Many of the papers are likely to be full of jargon and references to unexplained prior papers that readers are presumed by the author to know well, but which only a specialist in a particular subfield really understands.

As a reader can probably tell, I'm not a big fan of any of these writing styles. I think that an author should try to write her paper in as reader-friendly a manner as possible. Most readers find that papers are easier to read if they are well-organized, made up of simple, short sentences and paragraphs, and make their point as succinctly and clearly as possible. It is wonderful if the author has exceptional command of the English language and can use elegant and entertaining prose like Bruce Catton or Sylvia Nasar. Most successful academics, however, do not have such flair, yet can still produce well-written, easy to understand papers.

One economist who wrote incredibly elegantly was Adam Smith. Not only did he start the field of economics with his *Wealth of Nations*, but he did it with a literary flair equaled by few economists since. For example, I love his characterization of the way in which corporations are governed:

> "The Directors of [joint stock] companies, however, being the managers of other people's money rather than their own, it cannot be expected that they should watch over it with the same anxious vigilance [as owners]… Negligence and profusion, therefore, must always prevail, more or less, in the management of the affairs of such a company."

These sentences of *Wealth of Nations* are famous because they are the origin of the economics literatures on agency theory and corporate governance, as well as the expression "other people's money." They are also beautifully written, using language that one doesn't see in academic papers today. I always make a point of reading it aloud in class, and facetiously tell the students that anyone using the expression "negligence and profusion" in one of their papers will automatically get an "A".[53]

Beginning to Write a Paper

A simple way to think about the discussion of ideas and empirical results is to ask oneself: "How would I like to have the issues presented to me if I were a nonspecialist interested in the topic?" The answer to this question usually involves a minimum of jargon, simple, no fancy language, as well as short sentences and paragraphs. The author should explain exactly what she does, why she does it, and what the results mean. She should try to minimize the number of footnotes and make sure they are really ancillary and not essential to understanding the paper. She should avoid fancy sounding but content free expressions like "to wit" as much as possible.[54]

After I write a few pages, I always go back and reread my prose carefully, usually at least two or three times. I ask myself if any of my sentences can be split into two. If a sentence can be split, it is usually a good idea to do so. The same goes for paragraphs. Each paragraph should only contain one main

[53] No student has ever taken me up on that challenge.
[54] I sometimes fight with some of my coauthors who insist on using the phrase "to wit" in our papers. Alas, I don't always win these fights and the phrase can be found in a few places in some of my work. I still don't know what the expression means.

idea. If a paragraph can be divided into two smaller paragraphs where each has a coherent idea, probably two smaller paragraphs is preferable. Academics love to repeat themselves; try to eliminate as much repetition as possible and make the paper as streamlined and as straightforward as possible. Shortening and simplifying text can do a surprising amount to make a paper easier to read and understand.

Think about who your target reader is and exactly how much he knows. Did you use acronyms that he might not understand? When one is "into" a subject, it is easy to forget that others are just thinking about it for the first time, and might miss an acronym or two you use. Does the target reader have enough background to understand your analysis? If you are in doubt about how much background to provide, try to err on the side of providing a little extra explanation. Specialized readers who see a few extra sentences of background usually don't mind going through things that they already know. But a reader who is a bit less specialized can be turned off if you assume background knowledge he doesn't have. Remember that such a reader is a possible reviewer of your paper for a journal or grant. The last thing you want to happen is to turn off a reviewer and have them recommend that the paper be sent to a more specialized journal.

The big thing to remember is that when deciding on the style of writing to use, an author should try to make the paper accessible to the largest number of readers. Some authors seem to think that by presuming a lot of background information that some readers do not have, they are making their article more "elite". I find this attitude to be naïve. Presuming too much knowledge and not explaining what you are doing in detail is a good way to get your paper rejected, and to lower its ultimate impact if it does happen to get published.

Common Mistakes in Academic Prose

Having read many papers in my 30 plus years as an academic, I have developed a taste as to what I like and what I don't like in academic prose. There are some particular things that I'm inclined to correct in students' and colleagues' papers. Most are stylistic, so are not "incorrect", but do lead to papers that are less reader-friendly. Here are some of my favorites:

Active versus Passive Voice. Every American teenager is taught in High School English that one should use "active" rather than "passive" voice. A sentence in active voice has both a noun and a verb, while passive voice leaves out the noun and describes what has been done without saying who did it. For example, the sentence "I drove the car to the office" is in active voice, while "The car was driven to the office." is passive voice. Active voice is generally considered preferable, because it is more descriptive; in this example, it makes clear who is driving the car. Sentences written in active voice usually have a sharper, more "lively" feel to them than those in passive voice.

However, many academics who learn this lesson in high school immediately forget it when they write academic papers. So, they say "The data were gathered," not "I gathered the data" or "The following interpretations are evaluated" rather than "I evaluate the following interpretations". Some people think that passive voice sounds more scientific and formal. Perhaps it does, but it also makes prose less easy to read, and gives it a clumsy and sterile feel. It also is less descriptive. If a paper says "Equation (1) was estimated" rather than "We estimated Equation (1)", the paper leaves as a mystery who did the estimation. It is usually the authors, but perhaps not; sometimes equations are estimated by other people and then reported by the authors. Why leave doubt in anyone's mind about who did the work?

Passive voice is not incorrect and it is OK to use it from time to time. But if one always makes an effort to use active voice as much as possible, one's papers will generally be easier on the readers who one is trying to attract to one's papers.

"This". One of my pet peeves about writing is the use of the word "this" to refer to a general idea, an argument, or virtually anything else the author has in mind. I constantly correct students and coauthors who use "this" in this manner, sometimes multiple times in the same paragraph. My view is that using "this" to refer to an idea one has just described is symptomatic of laziness. An author can't quite think of what to say when describing his idea so he says "this". It doesn't require any thought and the reader can usually (but not always) figure out what the author means.

Simply put, “this” is not a noun. It is a modifier. So do not use it as a noun, even though many people do. To keep it simple, make sure that whenever you use the word “this”, there is a noun after it. If you want to use “this” as a noun, think about what you actually mean by “this” and add the appropriate word. Try to treat this rule as a hard and fast one, and if you don’t allow yourself to violate it, your writing will improve.

Sentence Fragments and Run-on Sentences. A sentence fragment is a collection of words that together do not form a complete sentence. Authors should avoid using them, and the appearance of a sentence fragment in a paper indicates to me that the author did not care enough to proofread his paper. Many readers wonder when they see an author that does not proofread her paper, her life’s work, what else she has messed up in her analysis?

Most writers are aware that they should avoid sentence fragments, but I do see them surprisingly often in papers that have been publicly circulated. Sentence fragments occur when authors are careless, often involving subordinate clauses. For example, I too often read things like: *“I cluster the standard errors. Because I was concerned that the errors could not be independent.”* In this example, the italicized words after the period are a sentence fragment, not a full sentence. The two parts should be combined into one sentence: *“I cluster the standard errors because I was concerned that the errors could not be independent.”* The existence of sentence fragments in publicly circulated, and sometimes published papers highlights the importance of proofreading papers carefully, and paying attention to mistakes pointed out electronically by programs like *Microsoft Word* and *Grammarly*.

More difficult to identify are “run on sentences”. While English teachers have standard definitions of run on sentences, I prefer to classify them in more practical terms. Each sentence should make one and only one point. Academics love to describe complicated ideas. In doing so, they often try to cram as much as possible into each sentence, so their sentences end up being long and complicated. Such sentences become hard to read, for a reader will forget what the author was saying at the beginning of the

sentence. It is much easier for a reader if the author splits the long sentence into two or three smaller sentences.

Sometimes run-on sentences occur when authors separate ideas by commas (which is called a "comma splice"). This practice can lead to long, confusing sentences. It is usually better to separate two related ideas into clauses that are separated by a semicolon. Personally, however, I think the best practice is just to separate the ideas into separate sentences. Readers usually find paragraphs easier to go through if the points are made in short, easily digestible chunks, with sentences taking up at most one or two lines of text.

Use Present Tense. A somewhat awkward decision authors must make when writing a paper concerns the *tense* one should use. In other words, should you say: "I *estimate* the following equation.", or "I *estimated* the following equation."? By the time you write a draft, you have already estimated the equation, so it might seem natural to use past tense. Alternatively, what you are really trying to say is that if one estimates that equation, no matter when one does it, one will get the results you report. In other words, the results you are reporting are, you hope, timeless. For timeless results, present tense seems appropriate.

For this reason, I was taught to use present tense when writing academic articles. I always teach students to use present tense and write in present tense in my own papers. But if one prefers using past tense, there is nothing wrong with that approach. The key thing is to be consistent throughout the paper. Some authors unintentionally switch back and forth between past and present tense, which can be very annoying to their readers.

Advice for Non-Native Speakers

English has become the official language of academia. All the important journals I know of are published in English, major conferences are almost always in English, and many universities in non-English speaking countries teach in English. It is efficient that all work is in one language, so that

everyone can read everything that is written in a field. However, having everything in English does impose the additional requirement on non-native speakers that they conduct their professional business in a foreign language.

Being able to conduct professional business in a foreign language goes well beyond fluency. Almost all foreign students who come to the United States to study are fluent. I can almost always figure out what they are saying and they seem to understand me without any problems. I find the fact that our international students all speak English so well to be incredibly impressive, since I personally am completely hopeless in any foreign language other than FORTRAN. But merely being able to be understood in English, while certainly sufficient to live in an English speaking country, is not good enough for an academic.

The standards for academic writing do not vary with the nationality of the author. A reader should not be able to tell whether an author is a native speaker by reading her paper. Regardless of whether a scholar is French, German, Korean, Chinese, or Turkish, she is expected to write English as well as an American or Englishman who has studied her entire life to be able to write well in English. Therefore, if English is not a scholar's native language, she has to work extremely hard on her English writing skills to meet this expectation.

Writing English is probably more difficult than speaking English for non-native English speakers. If a listener can figure out what a speaker is saying, they often will give a non-native speaker the benefit of the doubt if their English is not perfect; sometimes certain "mistakes" can even come off as charming when spoken. However, written English is expected to be correct, and readers have little tolerance for grammatical errors. Nonetheless, it is possible for non-native speakers to become excellent writers in English. Non-native speakers have always been prominent in the academic world currently make up the majority of scholars in many fields.

If you are a non-native speaker, it is important to remember that you can't let yourself be afraid to write in English. Some international students have told me that they are so afraid to circulate a paper in English that they procrastinate writing until the very last minute. Waiting so long means that their papers

do not go through enough revisions. When these scholars finally get around to circulating their papers, the papers can be insufficiently polished and fail to impress readers. If you are unsure about your ability to write in English, then it is crucial that you start writing early, go through many drafts, and continually improve your work.

A good practice for non-native speakers who wish to have academic careers is to try to read non-academic, high-quality English writing as much as possible. Non-native speakers should to try to spend their free time talking in English, preferably with native speakers. English should not just be a language that scholars know, it should be a language that scholars are as comfortable writing and speaking in as their native language.

It is a good idea for all young scholars, especially those whose native language is not English, to seek out as much help as possible. A good practice is to find people they get along within their field and to spend time reading each others' work carefully, paying special attention to writing. Often native speakers can be talked into helping with language-related issues.

But beyond getting help from one's friends, it is a good idea to consult a professional editor. Producing research is what a scholar has chosen to do for her career. If she can make her research better by an investment in a professional editor, then hiring the editor is likely to be well worth its cost.

Chapter 9: Making Presentations

Our work as academics involves communicating research results, which often means presenting papers to all sorts of audiences, and in all sorts of formats. Many of the people whom one is trying to influence with the work will not read the paper, but instead will see the author or one of her coauthors present it in a seminar or a conference. These presentations can have a large impact on people's impression of papers, and also of their authors. Consequently, it is crucial that a scholar learns how to present her work in a way that communicates her motivation, methods, and conclusions in a persuasive and entertaining manner.

Unlike in other fields in which a research seminar can cover an entire topic, the norm in the economics-based disciplines is that a seminar focuses entirely on one of the presenter's papers. However, the best way for a scholar to structure a seminar presentation is not necessarily to create an oral version of the paper. Some authors insist that no matter what, they will discuss every result in the paper in the same order as in the text. These authors sometimes essentially read their papers when doing presentations. Not surprisingly, these presentations often do not do a good job of conveying the paper's message to the audience.

Papers and presentations are fundamentally different from one another, and are designed for different tasks. A paper is a formal characterization of the research. It must motivate and explain the analysis, and also contain sufficient detail that a stranger can replicate the results. Once published, the paper will be online for eternity, and will constitute the public record of the research.

In contrast, a presentation is more like an extended advertisement for the paper. It is a "one time" event that can and should change when an author presents her work to different audiences. During the talk, an author has a fixed amount of time to present her work. She can be interrupted with questions that can lead to lively discussions that sometimes veer far from the questions addressed by the paper.

People attending a paper presentation realize that the speaker faces these time constraints. Consequently, their expectations from a presentation are therefore different from those from the paper itself. When attending a presentation, a scholar's goal is to understand the paper's main point and its implications, and to form an opinion on whether the paper is correct and important. He does not expect to see every detail of the paper, and knows that he can always look at the paper itself for documentation of the detailed steps the author went through to reach the conclusions discussed in the presentation.

Planning a Presentation

In any presentation, there are a number of things that an author hopes to accomplish. She has to convince the audience that they should care about the issue and pay attention during the rest of the talk. Most research papers are part of a larger literature that some of the audience will be familiar with, but others will not. The author must give enough background so that the people attending the talk who are not familiar with the literature can appreciate what she has done and how it contributes to ongoing debates. In addition, she must explain how the work was done and address any methodological issues that people in the audience are likely to be concerned about. Finally, the author must explain why she interprets the results the way she does, and discuss other interesting implications of her analysis. And she has to do all of these things in an entertaining manner, and finish in the allotted amount of time.

Accomplishing these tasks in front of a skeptical and sometimes hostile audience can be more difficult than many young scholars expect it to be. If an author proceeds with the presentation by following the paper without much additional planning, she can sometimes get in trouble and not do a good job conveying the paper's message to the audience. Instead, an author should plan the presentation separately from the paper. Sometimes the presentation can directly follow the paper, but other times it should take a life of its own. Some factors that can lead authors to use a presentation that deviates from the paper are time limitations, complications in the analysis that can be simplified or omitted in the presentation (but not the paper), and a desire to emphasize something in the presentation that is different from the focus of the paper.

When she presents a paper at another university, the author should consider her presentation to be the opportunity to have a conversation with the local faculty. It is not just a one-sided, one-size-fits-all show. The goal of a presenter is not (only) to wow the audience, but to get real feedback on the paper that will help improve the analysis. To increase the likelihood of receiving useful suggestions, it can be a good idea to open up to and expand on issues the local faculty are experts on.

Time Management

Presentations are almost always scheduled for a fixed time period. In economics and finance, the custom is that seminars are about an hour and a half, lunch talks take an hour, and presentations at conferences are usually only 15 or 20 minutes. During seminars, participants can interrupt speakers with questions, while in conferences, they usually have to wait until the speaker is finished to ask.

It is important to keep time considerations in mind when planning a talk. Authors should try to adjust the amount of material they cover in a presentation for the amount of time allotted to them. Some authors, when given a twenty minute time window at a conference, attempt to use the same presentation as they would for a ninety minute seminar and just talk faster. This strategy is almost always a bad idea; a better approach is to focus the talk on the main results and to cut material that is not absolutely essential.

Just like space in written papers is not uniformly valuable, an author's time during a presentation varies in its importance. The most important time is during the first 5 or 10 minutes of the talk. During this relatively short period of time, the author should try to explain to the audience why they should care about the paper, what the paper's approach is, and what they should learn from the analysis. If the author does not do a good job convincing the audience that her research is worthwhile at the beginning of the talk, as Jesse Shapiro puts it, "the talk is over, and you just don't know it."[55]

An author should go into a talk with an expectation about the amount of time she would like to spend on each part of her presentation. Before the talk, she should write down the approximate time she

[55] See Jesse Shapiro: "How to Give an Applied Micro Talk," available at https://www.brown.edu/Research/Shapiro/.

hopes to be at when finishing each part. For example, if the talk is an hour and a half, probably 20 to 25 minutes would be a good amount to spend on the motivation and summary of results. Then the author should plan approximately how long she wants to spend on each subsequent section.

When I give a talk, I decide in advance which results are most important and which can be skipped. If the talk is going on time, then I can go through all the prepared slides. However, if things take longer than expected, then I can skip all but the most important results to ensure that I can spend sufficient time on each of them.

No matter how much a speaker is delayed by discussion in the early part of the seminar, it is important to get through all the important results before the last few minutes of the talk. By the end of the talk, people stop paying attention and start thinking about lunch, their next meeting, the class they have coming up later in the day, or any number of other things. If the main results are presented in the last five minutes of the talk, seminar participants will not have time to understand the issues in the paper. And they won't be able to ask the author questions about things they do not understand about the findings, even if they are still paying attention by that time. If a seminar speaker does not get to the most important results in the paper early enough, the seminar is likely to be received poorly, even by scholars who are predisposed to like the paper.

Motivating a Presentation

When preparing a presentation, it is useful to approach the issue from the perspective of a listener. As scholars, we attend many presentations, from outside speakers, our colleagues, and our students. My department is similar to most in that all faculty and doctoral students are expected to go to departmental seminars regardless of whether we are interested in the topic. In addition, we all listen to many presentations at conferences we attend, and from our students. Some of the time we go because we want to go hear what the speaker has to say, but often we attend because we have to. Our minds will tend to wander if the speaker does not give us a reason to pay attention, especially if the talk is in an area far from one's own interests.

Most people subconsciously decide in the first few minutes of a presentation whether they should pay attention for the remainder of the talk. If a listener is really interested in a talk, he can become very engaged, trying to follow everything the speaker says and getting as much out of it as possible. However, listeners often "tune out". They become bored in the beginning of the talk, miss something important the speaker says early on, and then spend the rest of the time watching the clock, sneaking peeks at their smartphones, and thinking about anything but what the speaker is talking about. Occasionally these bored participants will ask a question or two, but mostly they count the minutes until the time the talk ends.

One should approach one's own talks with the hope of keeping as many listeners engaged as is possible, and minimizing the number who tune out. The key is the beginning of the talk. It is incredibly important to get the audience interested during the first few minutes. The way to pique the audience's interest is by convincing them that you have something interesting and important to say. What is important to academics varies from field to field so a scholar should know what is valued in their field sufficiently to make their paper appealing.

Since my work is in applied economics, I usually try to convince listeners that my research is interesting not just to academics, but also is something that aids in our understanding of an important real-world issue. If I convince the audience that a particular issue is really important to our understanding of the real economy but not that extensively studied, then academic economists usually come to the conclusion that they should pay attention to what I am going to say. Numbers help a lot; often scholars outside a particular field won't realize the quantitative importance of a topic until the speaker provides them with evidence. For example, when I motivate a talk about private capital markets to an audience of nonspecialists, telling them of the enormous amount of capital currently in these markets and the importance they play in the economy almost always does a good job of piquing their interest.

A second option is to relate the results to classic literatures in the field. In economics, a good idea is to start with Adam Smith, John Maynard Keynes, or Milton Friedman, and explain how the research in the paper builds on a question one of them originally raised. The audience will most probably appreciate the reference and realize that the speaker is talking about an important issue.

A third way of motivating one's analysis is to point out a gap in an important literature and to explain how the paper fills that gap. If listeners care about that literature, they are likely to pay attention. If not, they are likely to go back to their phones and check their email or read about whether their favorite team won or lost last night.

Know the audience

There are a number of considerations related to the specific circumstances of the talk that can affect the planning of a particular presentation. Who is the audience? Is it a seminar, a conference presentation, or a PhD class? Is the speaker being considered for a job? What is the audience's background and level of technical sophistication? Will there be a discussant? Will the audience ask questions along the way or will the speaker be able to present the paper uninterrupted? What kinds of issues does the author expect the audience or discussant to raise?

The audience's background will affect the amount of background material the speaker has to cover and the way that she can discuss issues raised in the paper. The general rule is that the more specialized an audience, the less background information is necessary. The National Bureau of Economic Research puts on prestigious conferences in most subspecialties of economics where the participants are top scholars working in the area; in these conferences, authors can get right into the meat of their talk with little motivation. In departmental seminars, there will be people from all subfields, and it is important to include enough background information and motivation so that non-specialists can appreciate the work's importance. In a classroom situation, it is sometimes necessary to include very basic material before getting to the more sophisticated and interesting things. In finance and some other fields, we sometimes have the opportunity to explain our research to practitioners doing work related to our research, and don't normally need much institutional background information in these talks. However, with practitioners, it is usually a good idea to explain some things academics could be assumed to know well (like how to interpret regression coefficients).

If a presentation is a "job seminar", there will be a somewhat different audience than if it is a regular seminar. Faculty who are not interested in the subject will feel like they should show up and try to understand the paper. Sometimes deans and faculty from other departments attend the talk as well. These faculty will be as interested in the speaker's presentation skills and personality as they will be in the research she is presenting. When a job is on the line, faculty are concerned about how the speaker would be as a teacher and colleague if they hired her. Consequently, in job talks, it is particularly important to motivate the analysis with nonspecialists in mind, and to make sure the presentation comes across as both professional and entertaining.

The Role of Slides

Central to any presentation today is a set of slides (sometimes called a "deck"). An author will plan her presentation around the slides she uses, and follow them throughout the talk. A high quality slide deck can be a valuable aid when doing a presentation. There are a number of ways in which a high-quality slide deck can improve an author's presentation.

The most important role of a slide deck is to provide a visual aid for the talk. A speaker can put equations, summaries of results, figures, and other elements of the research on her slides, so that the audience can see them easily. In addition, an author can include bullet points containing logical arguments, or even cartoons or pictures that will entertain the audience. If the audience does not follow everything the speaker is saying, often the slides can help them understand her points. They can also remind the seminar participants where the author is in the presentation, and contain basic information the audience should be aware of. If it is a theoretical paper, the slides can contain the equations being discussed together with variable definitions and any other information that will help understand the model. In an empirical paper, it is a good idea for the slides to contain details about the sample period, sample size, estimation procedure, etc. Having this information on the screen can help the speaker avoid questions about basic information that interrupt the flow of the presentation, and consequently leave more time for discussion of substantive issues.

A good slide deck can help the presenter focus the audience's attention where she wants it to be. The slide that is being presented tells the audience what they shoud be focusing on at that moment, so can help a speaker maintain control of the discussion. Keeping control in an academic seminar can be more difficult than one might think. Discussions of academic work can move far from the issues that the speaker wants to focus on, or even the paper being presented. A trick that can help to maintain control of the room during a seminar is to look to the slides during a break in the discussion, and to move on to the next one. The audience is likely to look at the board, see the next slide, and move its focus to whatever is said on it. Which is exactly where the speaker wants it to be!

An author should construct the slides with the goal of keeping the audience's attention on the issues she wishes to emphasize in the talk, and not on tangential issues that might come up. For example, it is common in economics to estimate equations including many explanatory variables, often too many to show clearly on any one slide. Authors should present the complete equations including all variables and their coefficients in the paper. However, in the slide deck, it can be a good idea to include only the "main" variables of interest that are relevant to the hypothesis the author is interested in testing. This way, the speaker can focus the audience's attention on the issues the speaker wants to emphasize, and not other variables that may be interesting to some, but not relevant to the author's hypothesis.

A slide deck can provide a useful takeaway from the presentation. Like most faculty, I distribute my slides to students for all the classes I teach. The slides provide a guide for the students, since the slides tell them what aspects of the material that I think are most important and will cover in class. With research papers, slide decks can be useful documents -- a practitioner who works in one of the areas I do research on has told me that he much prefers learning about my research by reading my slides than by reading the papers themselves. Some faculty even post presentations of their papers online since the slides are an easy-to-read summary of what the author wants to say.

Constructing a Slide Deck

An author should construct a slide deck with these goals in mind. The slides should display the paper's results, provide guidance for the audience, focus people's attention on the issues the authors want to cover, and become a document that people can take away from the presentation that covers the author's main points in a straightforward manner. These objectives should be what an author thinks about when constructing their slide deck.

However, many authors try to do too much with their slides. One common mistake author make is to use the slides as a way of demonstrating their skill at *Powerpoint*, or whatever program the author is using. We have all been to presentations where pictures and graphs go flying across the screen in all different ways, and come away amazed by the author's *Powerpoint* skills. These presentations can be quite entertaining. The problem with these kinds of presentations, however, is that afterwards, the people who attended the presentation often end up talking about the slides, and not the content of the talk. If the audience focuses their attention on the slides themselves rather than the subject being discussed, then the slides have become too much of a distraction. Slides are supposed to help the audience understand the author's ideas, not to become themselves the subject of the audience's attention.

Another common problem is that authors try to cover too much material on each slide. Too much material on each slide leads the audience to spend too much time reading the slides rather than listening to what the speaker has to say. Or worse, they could struggle and not be able to read what is on the slide because the author used too small a font. If the audience cannot read a slide, they will interrupt the speaker asking what the slide says, which can disrupt the flow of her presentation. In this case, a slide deck that is intended to keep the audience focused where the speaker wants it to be, can become a distraction that leads the audience's attention to wander.

The small font size problem might sound trivial. However, it is extremely common in presentations for the people in the back of the room not to be able to read the slides, especially when tables are copied directly onto them. If you are unsure about a slide deck, take it to an empty classroom and put it on the screen. Go to the back of the room and see how small the words and numbers are. If it is at all difficult to read them, then you have a problem. In this case, it is important to make the font larger

and the slide more readable. If necessary, delete some of the numbers from the table and get rid of some of the writing on it. Having slides that people can easily read is a notion that no one would disagree with, but making sure that *one's own* slides are readable is something that not everyone actually does.

Another common problem in presentations is the number of slides an author tries to cover in a single talk. As I discussed in Chapter 7, most papers have several "main" results and other results that are more ancillary, that deal with potential objections or special cases. In that chapter, I encouraged authors to focus their writing on the main results and to move as much as possible of the other material to appendices. In presentations, the same principle applies even more so. Presenters have only a fixed amount of time and have to allocate it wisely to communicate their material effectively. Since almost every paper contains far more results than could be effectively discussed in a short period of time, it is important to focus on presenting only the most important ones.

Objections that are commonly made to the results should be addressed head on, but ones that come up occasionally can usually be omitted and discussed only if someone in the audience raises them. Some scholars include links in the presentation that will take them to optional slides that address the most far-flung of objections. I think this practice is a good idea, but more for the presenter's state of mind than anything else -- it is extremely rare that presenters actually have to click on those links during a presentation.

One issue that scholars, especially young ones, worry about too much is the software package used to create the slides. As I write this paragraph, the most fashionable package is called *Latex*, although in a few years, it will probably be something else. It is fine if one likes *Latex* – it is a very good package, especially for equations. The point, though, is that one shouldn't feel that she *has* to use *Latex* rather than *Powerpoint* or any other program that the author is comfortable with. The purpose of slides is to help the speaker convince people that her research is interesting and important, not to be something of independent interest. The goal should be to keep the slides simple and use them to support the talk, and not to have them become a topic of discussion themselves.

Keys to a Good Presentation – Answering Questions and Keeping Control

The quality of one's slides is only one of many factors that determines how well a presentation goes. The largest factor, of course, is the quality of the underlying analysis. But by the time one gets ready for the presentation, the paper is what it is. The author has to think about what would make the best impression given the paper she is going to present.

There are some things that an author should think about when preparing and delivering her presentation. She should talk clearly, loudly, and to the audience. Acoustics are surprisingly bad in some seminar rooms; our department unfortunately have a fan in ours that can make it difficult to hear the speaker and questions from the other side of the room. If the speaker talks in a quiet voice and the audience has to try hard to understand what she is saying, it is easier for the audience to tune out. In addition, some speakers, especially when they are nervous, tend to read from their slides and not talk to the audience. This practice leads to boring, lifeless talks. An author should make an effort to talk directly to the audience, to make eye contact with them, and to explain the issues in clear, easy to understand words.

Even if the speaker is an outsider, and much younger and less well-known than some senior faculty in the audience, she should do whatever she can to let them know she is in charge. One easy thing a speaker can do is to erase the blackboard and close the doors to ensure there is no extraneous noise from the hallway. I have attended many seminars at various universities where the board has advertisements for the finance club or even calculus problems from the last time the classroom was used. In those cases, the speaker often felt like a guest and didn't think it appropriate to erase the blackboard, leaving extraneous material on the board to distract the audience when she wanted their attention on her paper. When a speaker visits another university, she is a guest for the majority of the trip. But during the seminar, it is her show, and she should work hard to remain in charge throughout the duration of her talk.

A good idea is to start the talk with a short sentence about something personal, light, a small planned joke. A funny joke can make the audience feel relaxed. When I'm visiting another university, I

like to talk about the place, the hospitality they have shown me, or even tell a funny (but not too embarrassing) story about one of their faculty.

Giving talks, like many things, improves with practice. The first time an author presents a seminar, she is usually nervous and can be somewhat robotic in her answers. Over time, she will get more comfortable in front of a seminar room, better at explaining things to an audience, and more able to answer questions well. Therefore, it is a good idea for all of us, especially young scholars who are new to the profession, to give as many talks in front of as many different audiences as is possible.

There are a number of excellent speaking coaches that are available for hire. If a young scholar is too timid in front of an audience or feels that she needs help for any reason, hiring one can be a valuable investment. The ability to talk comfortably in front of an audience is one of the most important skills an academic has; investments in improving this skill are usually worthwhile.

It is important to use one's time in a presentation wisely, since time is always limited and audiences can tune out quickly if a speaker does not get to the point quickly. A common mistake is to waste time on literature surveys, especially in the beginning of a talk when time is most valuable. Sometimes it is necessary to spend some time on the prior literature since part of the motivation for one's paper could come from someone else's research. But a speaker must be really careful to focus the discussion around her work, and not the earlier papers. Seminars can go off track quickly if they end up devolving into discussions of a controversial paper that someone else wrote a few years ago. These discussions distract from the author's message, and make it extremely difficult for the author to get the audience to focus on her contribution rather than the previous paper.

One of the key metrics by which academics rate seminar presentations is the way in which speakers answer questions. The answers that a speaker gives can convey the depth of her knowledge, the way she thinks about problems, and the extent to which the research is robust to reasonable alternative assumptions or research designs. They also convey much about the speaker herself: how open-minded she is, what kind of colleague she might be, and how she would do in front of a classroom.

If one does not know the answer to a question, it is fine to say so. The key thing is to appear thoughtful; the audience will want to hear that the speaker has thought deeply about the issues in her paper even if she does not know the answer to every question. Sometimes speakers don't appear to be taking questions seriously which can lead them to come across as arrogant. By not answering questions respectfully and clearly, authors can turn successful presentations into unsuccessful ones.

In any research project, there are many choices one must make along the way. By the time an author is doing a presentation of her work, she will have made many such choices. People will question the choices she made, sometimes aggressively. Most of the time when an author is questioned about methodological issues, she will have thought about the issues they raised in much more detail than the people asking about them. So when they do ask, the author should clearly explain why she approached the issue in the manner she did. There is no need to be bashful or defensive. By the time the author is presenting the work she should have thought about various possible specifications and decided on the appropriate one. Most of the time, it is fairly straightforward to explain to the audience why she made the choices she made throughout the research process.

A good way to answer to a question about possible alternative methodological choices would be for an author to discuss what the results would be if she used the alternative approach suggested in the question, how sensitive the results are likely to be to the choices she made during the research process, and why she thought the approach that she did use was superior to the alternative. If she gives these answers in a confident, scholarly tone, it can help to show that the author is on top of the issues they raised.

Problems speakers have in answering questions frequently come from not listening carefully to the questions that are asked. Sometimes someone in the audience will ask a simple explanatory question, the author does not understand exactly what is being asked, assume it was the one question she dreaded, and give an overly defensive answer. If a speaker is not sure exactly what is being asked, it is perfectly fine to ask the person to repeat or rephrase the question. A good practice is after the speaker has answered a difficult question, to ask the questioner if she actually answered the question he asked. Another good

practice is to defer questions that are more appropriate for later in the presentation. But if one does that, then the speaker has to be sure to answer the question at some point, otherwise the questioner could feel that the speaker is ignoring him. When I defer a question, I always make a point of answering it eventually. I also try to look at the person who asked the original question at the time I do answer it and direct my answer to him, to be sure he realizes that I did get to his question.

Sometimes in seminar presentations one or two people try to dominate the discussion and ask a series of aggressive questions. The person could feel offended by something in the paper, or just have a different point of view from that expressed by the author. Regardless of the reason, when a speaker is continually interrupted by belligerent questioners, it can be a trying experience. One approach is to say "I only have x minutes left, I'd really love to discuss this issue with you. Maybe we can continue this conversation after the talk." Or, "xxx is definitely an important and interesting topic. But let me try to move on with my talk and show you the model. Hopefully, xxx will become more clear as I explain what I am doing in more detail."

The important thing is to keep in control of the talk. The goal should always be to avoid turning the seminar into a fight. Therefore, the author should answer questions calmly and politely no matter how aggressive the questioner's tone. If possible, she should defer questions and suggest that she talk privately with the questioner about the troublesome issue after the talk. However, regardless of what the speaker does, some academics are just obnoxious, and like to take out their frustrations on seminar speakers.

In any academic's career, there are always a number of memorable seminars, often involving questionable behavior by audience participants. One of my friends managed to offend his dean in his second year as an assistant professor, and the dean responded by being rude and insulting to my friend. The dean didn't really listen to anything my friend said, and seemed to get much joy out of tormenting a new assistant professor. My friend, despite an excellent research record, ended up leaving that school and had a successful career elsewhere.

I once presented in front of a famous faculty member, who is a friend of mine, but is well-known for liking to hear himself talk. He went on and on about various things during the talk, making it difficult

for me to get in a word during my own seminar. I don't think he had any particular problems with my paper, he just was in a mood where he felt like talking. I'm sure he didn't think he was going out his way to cause me difficulties, that is just the way he always is. However, even though he wasn't trying to be difficult, he made the afternoon unpleasant for me, and probably led his colleagues to get less out of my paper than they otherwise would have.

Discussing Other People's Papers at Conferences

At conferences, paper presentations are often followed by a discussant, who typically has 10 or 15 minutes to give her perspective on the paper. Young scholars often do not know what to say in their discussions the first few times they are asked to discuss papers at conferences. Discussing a paper in a high-profile conference is often not an easy task -- a discussant should try to give a fair assessment of the paper while not offending the author too much and entertaining the audience at the same time. How should one go about preparing such a discussion?

When given a few minutes to discuss a paper in a public forum, a scholar should have a few goals in mind. She is supposed to be an expert in the field who has spent time studying the paper, so the audience looks to her to help understand whether a paper is correct, what we learn from it, and where it fits into the larger literature. In addition, she should try to help the author make the paper better, and to frame her criticisms in a way that does not belittle or embarrass the author.

A common approach is what I refer to as a "formulaic discussion". In a formulaic discussion, the discussant spends a few slides summarizing the paper's findings. Then she tells the audience it is a "great paper" and suggests that they all read the paper.[56] The discussant concludes the discussion by pointing out some suggestions for the authors, usually minor things the audience doesn't care about. For example, the

[56] Sometimes the same person who gave this positive discussion will subsequently referee the paper for a journal. It is not unheard of for the same person, after proclaiming the paper to be "great" in public, to tell the editor privately that it is kind of stupid, and recommend rejection.

discussant might suggest that the author could use three stage least squares instead of two stage least squares, or that she could have clustered the standard errors differently.

As a reader can probably tell, I'm not a fan of formulaic discussions. The summary of the paper, which can take half of the discussant's time, usually repeats what the author just said in her presentation, so is a waste of time for the audience. Compliments discussant make to authors often come across as phony. And the suggestions, while sometimes helpful to the author, are often of no interest to anyone else in the room.

Rather than following a formula, I recommend that discussants try to give a short talk that the audience will be interested in. They should present their view on the issues addressed by the paper, explain how the paper fits into the larger literature, and give an honest assessment of the paper's incremental contribution. An approach that works well for me is to spend the first few minutes of the discussion talking about the issue the paper addresses. I start as if I am teaching a class or explaining to a student what the literature is about. Then I discuss the paper's results and what about them is a new contribution and what confirms what others have already found. There is no need to present the paper again; the authors almost always do a decent job explaining what they did. Instead, I focus my discussion on what I think we learn from the paper, what it adds to the literature, where it falls short, and what its implications are. The important thing is to give the audience my perspective on the issues in the paper and not just to repeat what the author said.

I was recently asked to discuss a paper on "infrastructure funds". Before I talked at all about the paper, I explained what these funds were, gave a few real-world examples of their investments, and explained how they worked. I told the audience about my friend who used to run one of the funds, and what my friend said about the way they worked. Then I brought up the paper's results, and compared the paper's perspective to my friend's. The discussion went very well, as the audience seemed to appreciate what I said and told me they learned something from the discussion.

Discussants often wonder about the extent to which they should be critical of a paper they do not like. If a discussant is negative, most of the audience will forget about the discussion relatively quickly,

but the author will remember it for the rest of her life. However, a paper's faults are often obvious to the audience, and a discussant that fails to point them out can look foolish. A sensible strategy is to point out the paper's drawbacks, but to do so in as nice a way as possible. If there is something the authors can redeem from the paper, try to give them a gentle push in the appropriate direction. The goal when discussing a bad paper is to point out the paper's flaws to the audience in a way that does not embarrass the author or get her too upset with you.

After the discussant makes her points, the author has a chance to respond. Authors often use this opportunity to give long, detailed diatribes about why the discussant is wrong. The author might in fact be correct and the discussant wrong, but it doesn't matter. By this point no one is listening. The attendees have made up their mind about the paper, and are anxious to go to the coffee break or to hear the next paper. One wants to fight back after a negative discussion, but the problem is that the audience is rarely listening after the discussant is finished. For this reason, I think authors should always thank the discussant, to point out any errors that the discussant made quickly, and leave whatever time that is available for audience suggestions, which sometimes are very useful.

However, the most important response to any discussion, especially a negative one, is to spend time digesting the discussant's comments and revising the paper based on them. If he was negative, there is usually a reason why. Perhaps he didn't understand what the author did or ignored a key element of her analysis. If so, then it is likely that the author didn't explain what she did very clearly, or included this explanation in a place where the discussant didn't notice it. Or it could be that the discussant's objections are more serious than the author presumed. No matter what the discussant's reaction is, authors should always make an effort to understand why it occurred, and to revise the paper with the goal of receiving a better response the next time she presents the paper.

Chapter 10: Distributing, Revising, and Publicizing Research

Once an author has completed an initial draft of a paper, she must decide what comes next. How should she distribute the paper? Whom should she give it to? In what order? When should she present it to her colleagues? When should she put the paper online? When should she submit to conferences and journals? And which conferences and which journals, in which order?

The answers to these questions are often not obvious to young scholars, who tend to be somewhat haphazard in the way they choose to disseminate their work. But the process of distributing papers to the public can materially affect a paper's impact. It affects who reads the paper and the particular version that each reads. The distribution process also affects the feedback an author receives throughout the process of revising the paper. Ultimately, this feedback can be a major factor affecting the quality of the paper that is eventually published. The way research is circulated, like everything else in the research process, should be thought through and done in a systematic fashion.

Soliciting Feedback on New Papers

Many papers end up being very different when they are published from when they were first written. The changes, which usually (but not always) improve the paper, typically occur because of feedback the authors received from friends and colleagues. Most successful academics are good at utilizing the suggestions they receive to improve the analysis and impact of their papers. However, to take advantage of feedback on one's papers, one first has to receive it, hopefully at a time when the feedback is most useful.

When thinking about the comments one can expect to receive on any paper, an author should remember that there are a number of different types of readers. Hopefully, there will be a few people who will provide detailed thoughts on a paper if the author asks them nicely. These people most often are close friends, colleagues, and students, as well as scholars who work on related topics.

However, most people who read a paper will do so only once. Some will give the author feedback on her paper, which could be detailed comments, but more likely will be just a general reaction. Scholars who give the author any feedback at all, though, are the minority. The vast majority of readers will just look over the paper and try to understand the paper's goals, general approach, and results. People who are willing to give an author useful feedback on a paper are a valuable resource and should be treated as such.

Another thing an author should remember is that readers' opinions about a paper tend to be correlated with one another. As authors, we try to anticipate people's reactions: what we think they will be interested in, what they will be bored with, what potential problems readers will think are serious, and which ones they think are not a big deal. Sometimes, these predictions turn out to be correct, more often they are not. Invariably, there are one or two issues with any paper I write that I didn't think were that important when I write the first draft, but that almost every reader brings up. Usually, these are concerns people had about my analysis, although occasionally they are things that people like about my paper that people felt I did not emphasize enough.

When an author distributes papers, her goal is to learn what the common reaction is to one's paper without exhausting all of the feedback she is likely to get. It is inefficient to send a paper to everyone you know and have all of your friends make the same suggestions to you. Instead, it is preferable to send papers out sequentially. Send it first to one or two close friends who are likely to read it and give comments. Then make revisions that address the important issues they raise, and send to a few more people. Continue the process, addressing each set of comments as you receive them, and keep sending to other people until you finally have sent the paper to everyone who might be interested. This approach takes longer than sending the paper out all at once, but leads to much more efficient use of the feedback you are likely to receive.

I recently read an interesting paper, which I was given a copy of early in the paper's life, prior to the authors receiving much feedback from others. The paper had two major sets of results. The ones that the authors reported first seemed fine to me; I thought they were undoubtedly correct but not particularly surprising. The second set of results, however, were quite novel because they use a new technique to

address questions that the literature had not previously addressed. I wrote an email to the authors telling them my reaction and encouraging them to deemphasize the first set of results and focus more on the second. If the authors agree with me, they should try to make this change before sending it to too many others. That way, their next set of comments will likely address different issues from mine, and would be more valuable to the authors than if the next person to read the paper had merely repeated what I had already told them.

Because of examples like this one, I always encourage my coauthors to send our papers out for feedback sequentially. Before we let anyone else see the paper, my coauthors and I usually go back and forth about how to write the paper. We normally argue a little about how to structure the paper but ultimately converge to a version that we think is as good as we can make it. After my coauthors and I agree, I usually ask my research assistant to give the paper a careful read. At Ohio State, we are fortunate to have excellent doctoral students assigned as research assistants; my research assistant normally does not just find typos and awkward sentences, but makes substantive comments that sometimes lead my coauthors and me to rethink some of our analysis. When my coauthors and I have digested these comments, I try to give the paper to one or two friends of mine who I think are likely to read the paper carefully. My coauthors and I incorporate those comments, and repeat this process as often as it seems useful. I feel that it is important that a paper goes through several rounds of revision before I am willing to present it in a public forum, let alone put it online to make it easily available to anyone in the world.

The cost of this sequential distribution process is that it takes time. Authors generally want to distribute papers as quickly as possible for a number of reasons. First of all, research is a competitive marketplace, so the sooner one's paper gets out, the better the claim one has that the paper is the first to do what it does. Second, authors often have human capital reasons to speed up the research process as much as possible. Doctoral students' job market prospects are improved if they have established research that appears likely to be published soon, junior faculty come up for tenure at pre-specified points in time, and even senior faculty have reviews that depend on their ability to produce important research in a timely manner. Third, and perhaps most important, our research is something we put time and energy into

and hopefully are proud of. We all enjoy sharing our research with friends and discussing it with them as soon as we can.

These reasons to speed up the distribution process are important. But authors should remember that most readers will only read the paper once. If there are mistakes in the version of the paper that is distributed, if it emphasizes the wrong thing, if the writing is mediocre, or if it does not contain some interesting results that will be in subsequent versions, then the author risks not making her best impression. The consequence of circulating too soon is that readers would think less of the paper than they would if the author waited a little longer.

This discussion highlights the importance of having a network of friends and colleagues who are willing to give one honest, constructive, and fast feedback on one's papers. A scholar should work throughout her career to develop these relationships. One way to develop working relationships with scholars in the same area is to reciprocate and to provide useful feedback to others on their papers. When given a paper to read, a scholar should make a serious effort to give authors detailed, constructive comments whenever possible. This effort is likely to end up being more than a nice gesture to a friend -- it is an investment that can yield huge benefits when the friend responds by helping the scholar make her own work better.

Presenting Papers

After an author circulates her paper privately among her friends and uses the suggestions she receives to make the paper as strong as possible, she should try to publicize her paper more broadly. By this point, there should be no logical errors in the analysis, the writing should be high quality, the paper well-motivated, and obvious alternative explanations addressed. The paper should be something that the author is proud of and wants to share with the entire profession. Hopefully, the profession will appreciate it and recognize its brilliance!

There are a number of reasons why an author should spend time and effort publicizing her work. First, the way that research has an impact is through its influence on others. If the profession does not

know about a paper, it is hard for a paper to become influential. Second, a strong paper will increase people's impression of the author herself, which can help her reputation in the profession. Finally, when more people see the work, the author will receive more feedback, and she will have more chances to improve the paper.

Publicizing work outside one's close circle of friends should continue to use the sequential process discussed above. The author should first present the paper to groups that are most likely to give useful feedback. Then she should revise the paper incorporating the feedback, and continue to present the paper as often as possible. In addition, she should make other efforts through social media and personal contacts to draw attention to her work.

Most departments have a weekly "Brown Bag" workshops over lunch, mostly for internal speakers.[57] These tend to be informal events in which people present relatively early versions of their papers. When I revise my own papers, I have found Brown Bags to be extremely useful. Regardless of what people say about the paper during the talk, I find that spending time with the paper preparing the talk to be particularly valuable. Thinking about the structure of a presentation, what is important, what to emphasize, and what are likely to be objections, leads me to rethink my papers in ways I hadn't thought of when I was writing them. In addition, my colleagues can be counted on to bring up the toughest objections to any paper I have. After addressing my colleagues' concerns, the ones I hear at other universities and from referees usually seem easy. Because my papers tend to improve so much during Brown Bag workshops, I always present new papers in one before taking them on the road.

After discussing my paper in an internal workshop, I try to visit other universities and present it in their seminars. I am fairly well-known in the profession, so I usually have more invitations that I have time to accept. For people just starting their careers, arranging outside talks can be more difficult. However, there are ways that a less established scholar can generate opportunities to present her work at

[57] The term "Brown Bag" comes from the days in which participants would actually bring their own lunches, usually in brown bags. I haven't seen an actual brown bag at a "Brown Bag" workshop in many years. In my department, we serve pizza at our Brown Bag workshops. I'm sure our doctoral students have no idea where this strange term comes from.

other universities. Keeping in contact with a network of close friends helps, and letting such friends know when one has a paper to present can lead to invitations. If a scholar arranges to be in the city of an outside university for some other reason, then schools will often invite the scholar to present in their seminar series, since it does not involve any cost to the school. One strategy for scheduling presentations, therefore, is to visit universities for other reasons, and then try to arrange talks while one is there.

Internationally, invitations tend to go to people who have visited the area before. For example, Chinese universities are much more likely to invite someone who is a regular visitor to Asia than someone who does not travel internationally very often. When one begins to travel overseas and meet international scholars in their home countries, subsequent opportunities to visit again often materialize fairly quickly.

Another place where scholars present their research is at conferences. In recent years, there has been a large increase in the number of conferences in most fields. Unfortunately, despite the increase in the number of conferences, it has at the same time gotten more difficult to get on programs at high-quality conferences because the number of submissions for each conference has grown even more rapidly than the number of conferences. For example, in finance, large conferences run by the major associations have acceptance rates of around 10% or lower. Even smaller conferences run by individual departments get at least 200-300 submissions, despite typically taking only 8 to 10 papers. Given that conference organizers usually prefer having papers from well-known scholars and people they know personally, getting on programs can be particularly difficult for someone who is not established and does not know the conference organizers to have their papers accepted.

I encourage young scholars to submit their papers to conferences regularly despite the long odds. A sensible strategy is to send the papers to a number of different conferences with the hope that they make it onto one or two programs. Acceptances for more specialized conferences can be a bit easier since submissions are limited to papers on a specific topic. Conferences focused on one topic can be more valuable to authors than general interest ones anyway, since the papers are usually related to one another and their authors more able to give valuable feedback on each other's papers.

A good policy is to attend the major conferences in one's field whenever possible, even if one's papers are not on the program. Conferences are a good way to make connections and hear interesting papers. However, some people go overboard and attend too many conferences. They can take a lot of time and can be expensive. It is important to remember that a scholar's most valuable resource is her time – some young scholars forget to guard their time sufficiently and spend too much of it traveling to too many conferences.

Mass-Mailing Research

An easy thing to help publicize one's research is to send copies of the work to people who work in the area. Even before I post a paper and make it publicly available, I email a copy of the paper to the people who do the most related work. I include a short note that summarizes the paper's contribution and let the person know that I would appreciate hearing any reactions they have to the paper. After sending papers out this way, I am likely to receive suggestions on how to improve my paper. The people to whom I send my paper also will be more likely to remember it when they revise their own papers, and to invite me to present it if they organize a conference related to the paper's topic.

I am sufficiently established that I usually know the people to whom I send my papers fairly well. But it is a good idea to write to people doing related work even if one is just starting out in the profession and does not know the people personally. An author should not be shy about making sure that the leading scholars in one's area know about her work. If a young scholar explains who she is and what her paper does, people doing related work will most likely appreciate hearing about the paper. In the worst case and they don't care about your paper, they will delete it quickly, and nothing is lost by sending the email. The key things to remember are: first, an author should not send the paper out until she is absolutely sure it is ready; and second, when the paper is ready, she should not be bashful about approaching people and telling them about her paper.

Publicizing Research Online

When one presents a paper at another university, the faculty and students at that university can see the paper. However, until someone posts the paper online, the exposure is still somewhat limited. If major issues come up in the seminar and the author wishes to revise the paper substantially, it is unlikely that copies of the earlier version containing the mistake will appear a few years later. Even after public presentations at universities, a paper is not *that* public until it appears online.

After a paper is online, however, it is really in the public domain. People from everywhere in the world can download copies and keep the pdf files. If there are mistakes in the analysis or if the author changes her mind about the appropriate interpretation of her results, copies of the original version can pop up in the future. These versions can haunt the author if, as sometimes happens, readers bring up a problem in an older version that the author had by that time addressed.

A cost of having a paper online is that others working in related areas can potentially steal some of the ideas in the paper, and include them in their own papers. I personally have never had this problem, but know of cases where other faculty have. A number of faculty are reluctant to post their papers online until they are almost published for fear that someone will steal their ideas.

Sometimes, when a paper is online, an author receives feedback that can be unpleasant. For example, someone could write to an author claiming that he could not replicate the results that were posted online, or that there was a mistake in a derivation or the data work. If the criticism is correct, then it is extremely important for the author to fix the mistakes and repost a new version as soon as possible. However, such criticism is often misplaced, and can occur because of a misunderstanding or an error on the part of the person writing to the author.

Regardless of whether the author thinks the criticism is correct, it is important for her to engage with the person who contacted her. That person *believes* he is correct, and will not be happy if he is ignored. Therefore, any author who is contacted about a potential mistake in her paper must immediately decide whether the criticism is correct, and then explain what is going on to the the person who contacted her. If the author ignores the issue, the person could go public with the criticism, which would likely be embarrassing to the author and damaging to her reputation.

For these reasons, an author should think hard about when she is ready to allow her paper to appear online. Personally, I usually wait until the second or third draft before I am willing to post my papers publicly. If I present an early draft at another university, I often request that the department distributes the paper to their faculty via email rather than post it publicly on their website to limit the extent to which my papers are publicly circulated before they are ready. Some faculty go farther than I do and insist that early versions of paper presented in seminars be distributed as hard copies rather than by email, to ensure that no one forwards them outside the university.

After I present a paper a few times and am confident that it won't change much in the near future, I do post the paper online. And when I post it, I go all out, and try to have it available in as many places as possible. Our department has its own working paper series, which contains copies of new papers from our faculty. I always enter my papers into this series when they are sufficiently mature to be online. The papers in our departmental working paper series are automatically posted on a public website called "Social Science Research Network", which is a widely used network of working papers from all areas of the social sciences. In addition, since I am a Research Associate of the National Bureau of Economic Research, I enter my papers into their working paper series as well.

Other Ways to Publicize Research

In addition to posting online, authors should take advantage of other opportunities they have to publicize their research. One such opportunity is when visitors come through a scholar's university. Most departments have seminar series and faculty are normally given the chance to have appointments to meet with the speakers during the days they are on campus. Faculty, especially the younger ones, should always take advantage of these meetings, and if possible, use them to solicit feedback on their work.

Another way to publicize research, and also to network, is through social media. I am not active on Twitter or have a blog, but some of my colleagues post their papers as well as discussions of them on these media. I am active on Facebook and it is amazing how I feel like I have made connections with faculty at other universities by reading their posts every day. A number of faculty post regularly on

Twitter, and have been able to use this platform as a way to publicize their research, and also to network with other scholars around the globe. Social media is becoming increasingly important in all aspects of our lives, and I strongly encourage young faculty to make an investment in it to increase their visibility and connections within the profession.

One place where one should *not* publicize one's papers (too much) is during someone else's seminar, or when discussing someone else's seminars. It is extremely annoying to speakers, and to other audience members, when someone asks a long question in a seminar that is basically a summary of their most recent paper or when a discussant more or less ignores the paper he is supposed to discuss and presents his own paper. During someone else's talk, the time belongs to them, and it is rude to distract from their work unless the issue brought up is extremely pertinent.

Having an Up to Date Website

One place where I immediately post any paper mature enough to be online is my own website. I *always* keep my website up to date with the most recent versions of my papers. When I revise a paper and have a new version that I am willing to post publicly, I update the website very quickly. No more than 5 or 10 minutes after the update is finished, the new version will be online and the old version will not be downloadable anymore. Why should I let people download old versions of my papers when there is a new version available?

Websites have become a necessity for all academics in the 21st Century, including both faculty and graduate students. I do not understand why some people do not have one, or let theirs become out of date. Once a website is up and running, it takes no time to update it. Academics regularly look at each other's websites, and there is no excuse for letting them see outdated information when it is so easy to keep the information up to date.

Not having an up to date website can be costly. A few years ago, we had an unfilled position in my department. I brought up a name of someone I thought would be a good person for us to hire. This person probably would have accepted an offer from us and would have loved the job. But when I told her

name to my colleagues who then Googled it, they didn't find a website. The discussion then moved on to other scholars and we ended up hiring one of them for the position.

When to Submit to a Journal

One issue that young scholars often wonder about is the appropriate time to submit a paper to a journal. Some submit early drafts before the paper has been sufficiently polished. Usually, these submissions are quickly rejected. Others wait so long to submit that by the time the editor receives it, their paper is no longer novel and is rejected for that reason. How does one decide on the correct time to submit a paper to a journal?

The issue is complicated by the custom in most fields that a given paper cannot be submitted to two journals at the same time, and also that once rejected, a paper cannot be resubmitted to the same journal again.[58] In addition, in most fields, there are only a small number of journals that are considered top tier, and among these top tier journals, not all are appropriate for a given paper. For example, in economics, one of the top journals, *Econometrica*, historically has mostly published papers that break new methodological ground. So, if an author writes an applied paper that is not methodologically sophisticated, she probably would not want to waste her time submitting to *Econometrica*.

One should approach a submissions as a continuation of the revision process. When an author writes a new manuscript, she first sends it out to close friends, and revises it based on these comments. When the paper is as good as she can make it, she presents it in an internal workshop and revises it again. She then presents it at outside workshops and revises based on those comments. Along the way, she sends to scholars working in the area and receives even more comments. At every step, she revises the paper to be as good as she can make it. Eventually, she runs out of useful suggestions. Only when she reaches this point is the paper ready for submission to a journal.

[58] An exception to this rule is law, where most journals are student-run law reviews. For law review submissions (but not submissions to peer-reviewed law journals), simultaneous submission to multiple journals is allowed.

There are some factors that can cause her to speed up or slow down this process. One is the extent to which the issue she is studying is crowded and competitive. When there are others working on similar questions, scholars should do whatever they can to speed up the revision process, since the first paper to be submitted usually has a higher likelihood of being accepted for publication than the ones submitted later on. Of course, if the author speeds up the revision too much and the paper looks "early" to editors and reviewers, it could be rejected for that reason. Some academics working in competitive fields tend to submit papers too soon and get upset when reviewers are not sympathetic.

There are also career-related reasons to try to speed up the submission process. Scholars coming up for tenure soon naturally want to submit their work as quickly as possible. However, they should remember that editors will not take their tenure decision into account when handling their papers. So these scholars could potentially be hurt in the long run if they rush the revision process, submit too soon, and have a paper rejected that might have had a better chance if the authors had polished it more prior to the initial submission.

Journal Submission Strategies

Once a paper is ready for submission, a scholar has to decide on the journal to which she should submit it. This choice is actually more difficult than one might think. Journals differ from each other on a number of dimensions. Within a large field like economics, some journals are general interest (*American Economic Review*) and others focus on a particular subject (*Journal of Labor Economics*). While all are refereed, the review process they use can vary. Some use a more bureaucratic process involving multiple referees and associate editors while others rely almost solely on the opinions of one individual referee. Some editors read papers carefully, give detailed comments to authors, and overrule referees when they disagree. Others simply forward the referee reports to authors and follow the referees' suggestions, regardless of what they are.

These different processes lead authors to have very different experiences dealing with different journals. Some are relatively straightforward, having just to satisfy one referee. Others involve a complicated process of placating multiple referees and editors, who sometimes disagree with one another.

There are a number of factors that should affect one's strategy of journal choice. First and foremost is the journal's prestige. In every field, there are a small number of journals that are considered "top tier". These journals usually have the highest "impact factors",[59] and publish the most well-known papers. They are also valued extremely highly by universities when making promotion decisions. Many departments (including my own) focus promotion decisions mainly on a faculty member's publications in journals they consider to be top tier.

However, not every paper belongs in a top tier journal. Top tier journals try to be "general interest", and to publish papers that are interesting to the majority of the profession. Some papers are more specialized, and even if they are well-executed, these specialized papers ask questions that are too narrow for the top-tier journals. Submitting a very specialized paper to a general interest journal usually ends up wasting everyone's time: the author's, the editor's, and the referees'.

The time element can be significant. Some economics journals can take over a year to respond to authors. Waiting a year to hear a response from a journal can be costly, especially if the odds are low that the journal will request a revision. During that year, others can write competitive papers, the authors can get interested in other topics, and tenure clocks keep ticking away. A number of my classmates from graduate school sent their dissertations to extremely prestigious general interest economics journals like *Journal of Political Economy* or *Econometrica*. While some had their papers accepted, more ended up losing a year or more before submitting it to the more specialized journal that ultimately accepted the paper. Some of my classmates had papers that had some appeal for general interest journals, so it made sense for them to try the general interest journals. But others, who wrote high quality but specialized dissertations, should probably have submitted their papers to more appropriate outlets. In part because

[59] The impact factor of an academic journal is an index that reflects the yearly average number of citations received by articles published in the last two years in a given journal. It is a commonly used measure of a journal's quality.

they submitted to the higher ranked journal and consequently lost valuable time, and some of the papers from my classmates' dissertations never ended up getting published.

The most significant cost of a submission is the time it takes.[60] The cost of delaying publication by waiting for a journal to respond is meaningful and should not be underestimated. If a paper is rejected after a long time, then when the paper is finally rejected, the author has to spend time revising it rather than working on new research. In addition, there is an emotional cost to getting rejected after a year of waiting. A scholar never can publish in a top journal if one does not try from time to time. But I recommend only trying top journals for papers that one feels has a real shot at getting accepted, because the cost of waiting a long time for a rejection can be substantial.

It is important to consider the fit of the journal to a paper when deciding where to submit. Has the journal recently published papers of a similar style to the one being considered? Given that editorial boards change, do the *current* editors of a particular journal like the style of paper being submitted? For example, many economists have been openly hostile to behavioral research. Fortunately, this attitude is changing, but for a long time it was pointless to submit behavioral paper to journal with anti-behavioral editors who would not take the papers seriously. Having an editor who takes an interest in a paper and guides the paper through the review process is wonderful. Sometimes having an editor like that makes it worth submitting to a slightly less prestigious journal.

Finally, an author should remember that the journal submission process can be an author's last chance to improve a paper. The revision process can be stressful and tiring, but at the end of it, most papers end up better. Even if a paper is rejected, the suggestions an author receives from one journal can help her improve her paper so that it has a better chance at being published in another journal.

When a paper finally is ready, an author should not waste time and submit it to a journal quickly. Then she should incorporate whatever feedback she receives into the draft as soon as possible, regardless

[60] Most journals do have submission fees. But the majority are relatively low. The journal that has historically had the highest submission fees is *Journal of Financial Economics*, which at the time of this writing, has a fee of $1000 for nonsubscribers. While this fee sounds high, the editors of that journal have always argued that compared to the impact of a publication in *JFE* on a scholar's human capital, even their relatively high fees are trivial.

of whether that journal is interested in publishing it. If the paper keeps improving throughout the revision process, it should eventually be published in a good outlet.

Chapter 11: The Journal Review Process

If one is at a cocktail party full of academics and wanders around listening to random conversations, it is likely that sooner or later, one will overhear some discussion of the review process at a journal. Invariably the journal would be referred to by its initials (*JPE, AER*, etc.), and the discussion would involve some outrageously unfair thing that the editor (and referees) did. In the opinion of the people talking, the editor probably either played favorites and accepted a mediocre paper by someone they don't like, or rejected a wonderful paper by one of the people participating in the discussion. However, no matter how much people complain about the review process, it usually works reasonably well. Most of the time referee reports are reasonable assessments of papers' quality and editors' decisions make sense given the quality and objectives of the journal.

Academics are obsessed with the way journals review papers, the procedures used by the review process, the politics involved in it, and perhaps most importantly, which of their friends' papers have been accepted by the top journals. And they have a reason to be so concerned. The way in which journals decide on the papers to publish is an opaque, sometimes inefficient system, and is an incredibly important element of their professional lives. The ability to navigate the review process well has always been a necessary ingredient of a successful academic career.

Every academic spends an inordinate amount of effort on the review process in one way or another. Much of one's career is spent preparing one's papers for submission, revising them in response to referees' comments, and writing reports on other people's papers. Together, these activities take up a large fraction of our time not spent in the classroom. Often the "reward" for a successful career is to be asked to be an editor oneself, which entails overseeing the review process, handling appeals, and, often, many of the journal's management details such as the choice of associate editors, journal policies, etc. Even aside from the amount of time one spends on one's own papers, interacting with journals in one way or another can take at least a few days of every *month* for an active researcher.

Consequently, it makes sense to think a bit about the review process, and the way in which journal submissions are evaluated. An understanding of the way that review process works can aid a young scholar in maximizing their chances of having their work published in the best possible journals.

I will discuss the way a submission works from beginning to end, trying to emphasize differences between the practices that different journals use. At each step I will try to advise authors on things they can do to improve their chances of getting their papers accepted.[61]

Preparing the Paper

As emphasized in the last chapter, papers that are submitted to journals should have gone through many revisions and be at the point where authors cannot think of anything substantive to improve them. However, some scholars can take revising too far. I know assistant professors who waste incredible amounts of time making meaningless changes to papers because they are nervous about submitting their papers to journals. For example, some spend valuable time programming the computer so that whenever they submit a paper to a journal, it reformats the paper so it looks like a copy of the journal to which they are submitting, with the same fonts, paginations, etc. This type of effort does nothing to improve the paper's analysis or likelihood of acceptance. Or they rerun every test five times, then make a small change and rerun them again a few more times.

Although it is good to redo the analysis from time to time to ensure there are no mistakes, after a while one hits a point at which the return is outweighed by the cost in time and delay. Before submitting a paper, one must make sure that the analysis is correct. But making minor changes solely for the sake of making changes accomplishes nothing useful, and is unlikely to affect the opinions of editors and referees.

There are some things that do matter and can substantially affect the response an author receives from a journal. The writing, especially in the abstract and introduction, is incredibly important. The

[61] My colleague René Stulz has written a very good set of "Tips for Authors", which is available online at: http://jfe.rochester.edu/tips.htm.

author should make sure that anyone reading the first few pages knows exactly what the paper does, why it matters, and what the results and potential objections to them are. As I stressed in Chapter 5, the introduction is the most important part of the paper since for many readers, it is the only part of the paper they will actually read. Referees, although they are supposed to go through the entire paper carefully, sometimes do not make it past the introduction, especially if they are not impressed when they read it. Therefore, an author, especially an inexperienced one, should spend a lot of time polishing a paper's introduction before submitting it to a journal.

The other thing an author should think a lot about before submitting to a journal is a paper's length. It is *much* easier to publish a paper with 25 pages of text and seven tables than one with 40 pages of text and 14 tables (appendices don't count). Editors often claim that the reason for the preference for short papers is that journal space is scarce, but I'm not sure that is correct. When I was an editor at *Review of Financial Studies*, the publisher, Oxford University Press, didn't place any limitations on the number of pages in each issue. I think the truth was that Oxford University Press actually liked to have longer issues for their readers.

I have no idea whether other journals actually have page limits or just say they do. My theory on why it is so much more difficult to publish longer papers is that they tend to be more painful to review than shorter papers, so referees are more likely to be negative. Editors, too, prefer shorter papers since it makes their job easier if papers are shorter and readers tend to find them easier to go through.

Regardless of the reason, it is definitely easier to publish shorter papers than longer ones. Therefore, prior to submission, an author should go through the paper and chop anything that isn't essential. Nonessential things include tests of alternative hypotheses that most readers don't really care about, generalizations of models that add complications but no new ideas, and redundancies in the paper's prose. Much of this "nonessential" material can be described in the text and moved to an appendix, possibly an online one. The key factor in determining whether material should be included in the main text or in an appendix is whether a typical reader would find it interesting or superfluous. If a finding in a paper does not advance its key points and is likely to be considered by most readers to be not essential,

then the author should think about moving it to an appendix. If the referee turns out to be one of the few people that really cares about the finding, the author can always move it back into the main text.

A few years ago, one of my students had a job market paper that I really loved.[62] When her job market was over, she spent some time incorporating the feedback she received from the schools she visited, and tried to publish it. I was sure that referees would think the paper was very good and that it would be accepted by one of the "top three" finance journals. Unfortunately, to my surprise her paper was rejected by all three. After these rejections, my student asked me to look at the paper again and to help her decide what to do next. When I read the version she had been submitting, I realized that she had diligently incorporated every suggestion she received from her job market seminars, like students all over the world are taught to do. But in doing so, her paper grew from 30 readable and interesting pages to 45 boring ones, which was probably why the referees recommended rejection at the top journals. I helped her chop whatever was not absolutely necessary from the paper and shorten it to under 30 pages. Her paper was accepted at the next journal she submitted it to.

This story highlights the importance of taking advice from one's advisor, even after finishing the PhD. I wish my former student had asked me to look at her paper prior to her initial submissions. The fact that the paper was unnecessarily long is the sort of thing that is obvious to someone who has been rejected many times over the years, but was not to a newly-minted PhD. Decisions about journal submissions, including when to submit, where to submit and how to revise papers prior to submission are the sorts of things for which it is usually a good idea to consult with a thesis advisor or other experienced scholars.

Forming A Submission Strategy

The rule in most fields of academia is that a paper can only be submitted to one journal at a time. The review process is very costly in terms of editors' and referees' time, so journals will not pay these

[62] I still do. I teach it in my doctoral class every year.

costs unless they have the option to publish the paper if they like it.[63] In addition, the paper cannot be resubmitted to that journal if it is rejected. Some authors try to make small changes to the paper, call it a new paper and try the same journal again. This strategy is almost always a bad one, since editors usually notice such submissions, and reject them quickly while lowering their opinion of the authors.

Given that an author can only submit to one journal at a time, how does she decide which journal to try first? Why not always try the best journal in the field? It does, in many circumstances, make sense to try the top journals first. There is no stigma to getting a rejection, so long as the paper is reasonably well executed. The major cost to a submission, aside from the emotional cost of rejection, is the time it takes.

However, many scholars substantially underestimate the value of that time. In economics, a journal can sometimes take more than year to provide an editorial response to the authors. During that year, others will continue to work on competitive papers, which can lead the author's paper to be considered less novel. In addition, while the author is waiting for a response, the paper still affects an author's capacity to do new work. Having an unpublished paper sitting at a journal still takes some of the author's energy. While a paper is sitting at a journal, authors often spend valuable time thinking about changes one could make to the paper, what the referees' reactions are likely to be, etc.

I think that an author should try to form a realistic expectation of whether a paper has a substantial chance at getting accepted to a journal before submitting it. Forming these expectations can be difficult because doing so requires accepting the limitations of one's papers. If an author thinks a paper of hers has broad appeal and she has received positive feedback on it in presentations and from advisors and colleagues, then she should probably try a top tier journal. Publications in top journals are very valuable, especially for junior faculty who will be up for tenure soon. But she should be aware of the time and energy involved with the submission process. If a paper is having problems at the top journals and a lower

[63] Since law reviews are edited by students, the opportunity cost of reviewers' time is low. Perhaps for this reason, law reviews are willing to consider manuscripts for publication even if they are also being considered by other law reviews. If their paper is accepted by more than one law review, the authors can choose the journal in which the paper is published.

ranked journal is likely to take it without too much effort, then it might make sense to publish it there and move on to the next paper.

The Initial Submission

Most journals today use a program called *Editorial Express* or some similar software that enables the submitter to upload the paper and other relevant information directly to the journal's website. These programs make the journal's job easier as it computerizes all the information they require, and includes a link to a payment system that processes the submission fee.

The one important decision an author must sometimes make at the time of the initial submission is the choice of an editor who handles the paper, since some journals let authors suggest an editor at that time. These journals do not guarantee the editor choice, but my experience is that well over half the time, the suggested editor does handle the paper.[64] Most journals have multiple editors who have different tastes in research from one another, so the choice of the editor who handles the paper is extremely important.

When given the ability to suggest an editor, an author should suggest the editor who she believes will be sympathetic to the paper and its message. This person is usually the person whose research is closest to the paper's. However, sometimes an author is concerned that the editor whose research is closest to the paper could be predisposed against the paper, in which case that editor should be avoided. Such a predisposition against the paper could occur if the editor has well-known views contrary to those expressed in the paper, has a student with a paper competitive to the author's, or a number of other reasons. Another reason for not choosing the editor whose research is closest to one's paper would be if a different editor has expressed an interest the paper, making that second editor a sensible person to suggest.

[64] When journals do not use the editor the author suggests, it is usually because the managing editor wants to even out the workload among editors, or if there is a conflict that prevents the requested editor from handling the submission.

When I was an editor, I almost never read the cover letters accompanying the submission. I skipped straight to the paper and read it quickly as I was deciding who to ask to referee it. However, other current and former editors have told me that they do read the cover letters carefully, and use the summaries that authors provide in them to guide their choice of referees. The cover letter can provide the author with an opportunity to grab the editor's attention, to put the paper into context, and to explain to the editor why the paper is a good fit for the journal.

What the Journal does when it gets a New Submission

When a new paper is submitted to a journal, usually the first person who sees it is someone on the staff who makes sure that the paper meets the requirements of the journal. For example, some journals have maximum lengths or other restrictions such as the paper having a minimum font size.[65] Assuming the paper meets the specifications and the submission fee is paid, the paper is looked over briefly by the editor in charge of assigning submissions, usually called the "managing editor". This person can reject the paper at this point if it is obviously not suitable for the journal (a "desk rejection"). More often, the managing editor will assign the paper to one of the journal's other editors to handle the paper, or she could decide to handle it herself.

At this point, the process differs somewhat from journal to journal. At *Review of Financial Studies*, we had a post-doc who read every submitted paper and suggested several names of possible reviewers for each paper to the editor who was handling it. Other journals assign each paper to an associate editor, who gives the editor an opinion on the paper and suggests potential referees. Eventually, the paper hits the editor's desk. The editor makes the call about whether the paper should be refereed or desk rejected, and if it is to be refereed, who to ask to review it. Personally, I hated to desk reject papers and rarely did so. Probably I could have saved referees and authors' time if I desk rejected more often, but

[65] At *Review of Financial Studies*, we adopted a minimum font size because one author submitted 4 or 5 papers every year that always seemed to be close to 100 pages long in a very small font. Needless to say, none of the editors looked forward to handling this person's papers.

I hated the expressions I would see on desk-rejected authors' faces when they saw me subsequently at conferences. Other editors, however, do regularly desk reject papers that are extremely unlikely to be published, which is probably a good policy. Some journals, such as *The Quarterly Journal of Economics* desk reject over half of the papers that are submitted.[66]

If the paper is not desk rejected, the editor must decide how many and which referees to use. Most journals are moving to a policy of having multiple referees on each paper, although some still use one referee most of the time. Editors normally choose referees who do research related to the paper, since they are better able to assess the paper's contribution, especially in terms of what the paper adds to the existing literature. If there are multiple referees, editors try to pick people with diverse backgrounds and skills. For example, if a paper has both theoretical and empirical work, an editor might ask one scholar who is better at theory to be one referee, with the other being someone who specializes in empirical work. Or she might ask a more senior scholar to write one report and a more junior one to write a second (younger referees frequently are the ones to go through the entire paper more carefully, and to give detailed suggestions to the authors).

Editors view referees, especially the ones they trust most, as valuable resources. For this reason, they tend to only use their favorite referees on papers they think have a real shot at getting accepted. What this means, unfortunately, is that more senior and well-known researchers are much more likely to get referees more trusted by editors. These trusted referees tend to be senior scholars who have better perspectives on research. They usually are less likely than younger scholars to reject papers for minor methodological issues, and more likely to focus on what is learned from a paper than on what is wrong with it.

Referee Reports

[66] But not my papers. Every time I have submitted a paper to *QJE*, the editor has sent it to referees and then rejected it.

After an editor decides on which referees to use for a submission, she sends them email requests asking them to referee the paper. The email will contain a copy of the paper, a deadline by which the editor would like to receive the paper and details about any (small) payment that will go to the referee if he completes the report on time. Traditionally, papers were "double-blind" refereed, meaning that neither the referee nor the authors knew the identity of the other. Today, however, blind refereeing has become somewhat of a joke because referees can almost always find out the authors' names by googling the paper's title. Some journals have nonetheless kept the façade of double-blind refereeing, while others have gone to a single-blind system, in which referees are told the authors' names, but the authors do not know who the referees are.

Research-active scholars generally are asked to referee far more often than they would like. They agree to write referee reports as a professional obligation and as a favor to the editor, not because they want to do the work. If a scholar wishes to publish her own work in a particular journal, she is generally expected to referee for that journal when asked, unless there are mitigating circumstances. But writing referee reports is painful, takes a lot of time, and has trivial compensation. For most academics, refereeing papers ranks with grading exams and attending faculty meetings as the aspects of our jobs that we try hardest to avoid.

That being said, when reviewers reluctantly agree to referee a paper, most try to do a good job.[67] Reviewers want to help the editor make better decisions and the author improve her paper. So they read the paper carefully and try to provide useful suggestions to the authors. But reviewers get grumpy when they are writing their third or fourth report in a busy month. They really don't like it if the paper they are reviewing is hard to read and to understand, if there are obvious mistakes or omissions of arguments, or if there is much extraneous material included. Authors' efforts to make their papers more readable do affect

[67] A very good paper about the refereeing process and the way a scholar should go about writing a report is: Jonathan Berk, Campbell Harvey and David Hirshleifer (2017), "How to Write an Effective Referee Report and Improve the Scientific Review Process," *Journal of Economic Perspectives*, Vol. 31, Number 1, pp. 231-244.

referees' reactions to them and can substantially increase the likelihood that their paper will be accepted for publication.

A referee provides the editor with advice about the way she should handle the paper. Does the paper have the potential to make a sufficiently large contribution to the literature to warrant publication in the journal? What do the authors have to do to the paper to make it publishable? Are there any ways the authors could improve their analysis that the editor should encourage them to pursue? The answers to these questions, especially the first one, can vary from journal to journal. Very often, a paper whose contribution is not large enough for the *American Economic Review* would make a nice addition to a more specialized journal.

Referees convey their advice fairly bluntly in a private note to the editor, and also usually repeat this advice in the report itself for the authors to see. Sometimes referees sugar coat the report a little if it is negative. For example, if a referee tells the editor: "This paper is really awful," he might say in the report the authors see: "In my opinion, the incremental contribution is too small to warrant publication in this journal." However, not all referees are so nice, and too often include unnecessarily hurtful words when they write negative reports.

In addition to the note to the editor, the referee writes a formal report that is forwarded to the authors. A referee report usually starts by providing a summary of the paper. It then gives an overall evaluation and recommendation regarding the paper's publication. Referee reports also contain suggestions to help the authors improve their papers. The quality and quantity of suggestions that referees provide vary tremendously. Sometimes referees provide detailed comments taking five or six single-spaced pages that can be extremely valuable to authors when they revise their papers. Other referees only write a paragraph or two, and focus their comments on the big picture, ignoring most details.

Most referees feel that when they are going to recommend acceptance of a paper eventually, they want to work with the author to make the paper as strong as possible prior to publication. When they are going to reject a paper, they still want to help the authors improve it, but are unlikely to devote as much time to doing so. Therefore, when recommending that the author(s) be invited to resubmit a revised

version, referees usually try to be extremely thorough and make whatever suggestions they think will improve the paper. When recommending rejection, they tend to write shorter reports that explain why the paper is not suitable for this particular journal but don't spend as much time making helpful suggestions.

When the Editor Gets the Paper Back from the Referees

Although it sometimes takes longer than the editor (and the authors) would like, the referees eventually send their reports to the editor.[68] At this point, the editor must decide how to handle the paper: she can reject the paper, ask for a revision or, in rare circumstances, accept the paper as is. Most of the time the decision is fairly easy. At good journals, the majority of submitted papers are clear rejections. Editors can usually tell that a submission is a clear rejection after a quick read of the paper and referee reports. The analysis in these rejected papers is not usually wrong, in that the authors made errors in their proofs or used inappropriate statistical techniques. The reason why most papers are rejected is that they do not have a sufficient incremental contribution to warrant publication in the journal considering them. Journals compete with one another to have the most impactful papers, so editors try to publish papers that will increase their journal's reputation. Most rejections occur because editors and referees judge the papers to be ordinary, and not likely to be sufficiently impactful to help the journal's reputation.

When an editor rejects a paper, she normally writes a short note to the authors and attaches copies of the referee reports. Some editors reject papers using "form letters" that are the same for all rejected papers. When I was an editor, I preferred to personalize my rejection letters. Having been rejected many times myself, I felt that I should explain in a few sentences exactly why the paper's contribution was insufficient to justify publication. Authors were still upset when they received my rejection letters, but I

[68] When things go well, it should only take referees a month or two to return their reports. However, sometimes referees can be very slow, and papers sometimes sit on slow referees' desks for six months or even a year. Occasionally, an editor will have to give up on a particularly delinquent referee, so makes a decision on the basis of the reports received, or has to solicit another report.

hope most of them felt that they were treated fairly and received useful feedback from the submission process.

The editor's job becomes harder for papers that are close calls, or on papers that the journal is likely to publish. On a paper that the editor is thinking seriously about publishing, she normally will carefully read the paper and referee reports. The referee reports provide guidance but the final decision on a paper belongs to the editor. A good editor will sometimes overrule referees in both directions. Sometimes she will reject a paper that the referees like and sometimes she will invite a resubmission on a paper the referees recommend rejecting.

Editors take a variety of approaches to the resubmission process. Some are not willing to invite resubmission unless they are fairly certain the paper will eventually be accepted for publication. Others are lenient on the first round and reject a lot of papers after they are resubmitted the second or third time.[69] Still others like to give authors a second chance, so tell a fair number of them that their paper is a "reject and resubmit." In this case, the paper is rejected, but unlike with most rejections, the authors have the right to revise the paper substantially and resubmit it as a new paper.

When requesting a revision, the editor writes a letter to the authors, explaining exactly why the current version is not acceptable and the direction in which the editor would like the authors to take the paper. Often these instructions are very detailed. For example, the editor could tell an author that she should forget about the first set of tests presented in Tables 3-5, and expand the second set of tests discussed later in the paper. Or she could tell the authors to increase her sample size dramatically, change the econometrics completely, come up with a valid instrument, or any number of different things. Revision requests vary dramatically in the amount of work involved – some are simple and can be completed in a few days, while others involve essentially writing a new paper and can take over a year.

[69] This experience can be extremely upsetting for the authors, especially when they are just starting out in the profession. A former student had one of the top economics journals reject his job market paper on the *fourth* resubmission. He was so upset he left academia and has had a successful career in the money management industry.

When I was an editor, I wanted to avoid rejections at later rounds, so I was very selective in terms of the papers for which I asked authors to revise their papers for potential publication. I very rarely rejected a paper after the first round, so virtually all of the papers for which I requested revisions were ultimately published. That being said, each time I wrote a letter requesting a revision, I did not know if the paper would ultimately be accepted. Hence, I was always firm, and stated clearly what the authors had to do before the paper could be accepted. I further emphasized that I had the right to reject the paper in the future. However, the authors I dealt with almost always turned out to be extremely diligent, and they usually succeeded in doing what I asked them to do.

After the Author Receives the Editor's Letter and Referee Reports

Today, authors hear news from journals through emails with subject-lines of something like "decision on your submission". When an author sees that subject on an email, her heart starts beating faster and she braces herself; most of the time the news she hears will be upsetting. The vast majority of submissions to good journals are rejected. No matter how long one has been in the profession, getting rejected is still not easy. Indeed, as a former classmate of mine once told me: "The day you cease caring about a rejection is the day you should hang it up."[70] Even when the news is good and the response is a "revise and resubmit," reading the reports can be painful. The reports tend to focus on what the referees do not like about the paper and therefore can be unpleasant to read. Few scholars enjoy reading criticism of their work, even when the criticism is intended to be constructive.

The thing one has to remember when reading the email from the editor is that it (almost) always seems worse than it actually is. If the paper is rejected, there will probably be something useful in the reports, and the author can try submitting to another journal with an improved paper. If the editor gives the author the option to resubmit her paper, it is good news even if the letter includes a lot of negative verbiage about the paper's analysis. The author gets advice on what she can do to her paper to move it

[70] Perhaps this classmate took his own advice to heart – he became a dean last year!

towards publication, and taking the paper in this direction will probably improve the paper irrespective of the publication process. The trick one needs to learn is to be able to take negative feedback in a positive manner, and to use it to improve the work. Criticism is an integral part of academic life and the ability to engage with unpleasant feedback and use it in a positive manner is a skill that is necessary for academic success.

Rejections

Top journals in most fields now reject over 90% of submissions. When authors receive these rejections, they can be very painful. Authors put their heart and soul into the papers, and most of the time truly believe that the papers deserve to be published. When the papers are rejected by editors and referees, often in what can appear to the authors to be a cavalier manner, it can be hard to take. Yet, the ability to take this bad news in a productive manner has become a necessary skill for a successful academic.

Why do so many papers get rejected? The simple answer is that there are many more submissions than a journal can publish. But that answer begs the question of why so many papers are submitted to journals at which they will be rejected. Some rejected papers had a real shot at getting accepted. But the majority are fairly obvious calls to the editors; *The Quarterly Journal of Economics* desk rejects over half their submissions within a few hours of the submission because it is obvious to the editors that these papers are not appropriate for their journal. Other journals are not as aggressive about desk rejections, but their editors still know before they send papers to referees which ones have a realistic shot at getting accepted and which do not.

One reason why so many clearly unacceptable papers are submitted has to do with the incentive structures that authors face. There is a very large payoff to having a paper appear in a top journal, while the cost of trying is low. Submission fees are usually trivial and often paid by grants or research budgets; if one is lucky, only a few months are lost. So why not try? Perhaps the paper will get a sympathetic referee and/or editor? This logic leads most papers produced by research active faculty to be submitted to at least one or two top-tier journals before the authors try a more specialized journal. The puzzle to me is

why, when an author submits to a top tier journal thinking it is not likely that her paper will be accepted, she is so often upset when the paper is rejected.

A number of rejections occur because authors have inflated views of their own work. The same scholar who is a trusted resource for colleagues and editors can think his own work is far more important than it actually is. Persistence and a high level of confidence are some of the qualities that help people become successful scholars. However, the tendency of scholars to overvalue their own work can lead them to submit to higher ranked journals than are warranted by their papers' contributions.

What to do when One Receives a Rejection

When an author receives a rejection, her first reaction will be to be upset with the review process and complain about it to anyone who will listen. It can be extremely disturbing to hear that others do not like the paper one has been working on for years. The first reaction of many is to contact the editor and to ask for a better explanation for the rejection than is in his letter. From the author's perspective, the editor has made a mistake. It is natural for the author to think that the editor might realize her mistake and change her mind about the decision. However, no matter how much she wants to, an author should not contact the editor (except through the formal appeal process I discuss below). Complaining will not change the editorial decision but might lead the editor to lower her opinion of the author.

After a rejection, usually the best thing for an author to do is to spend a few weeks dissecting the rejection by herself and with her coauthors, close friends, and advisors. A rejection is one of those times when close friends can be extremely valuable. Sometimes it is useful for an author to take out her frustrations by screaming at her close friends (in private), and letting the friends scream at her when their papers get rejected. But in public, and especially when communicating with senior colleagues and editors, she should do her best to remain calm, cool, and collected. She should try to come across as a professional. Her colleagues will know from their own experiences how difficult it is to be rejected, so if she reacts professionally, their opinion of her will increase.

Although referees do make mistakes, typically there is some validity to their points. Perhaps one section of the paper isn't as clearly written as the author thought, a given result less surprising than the paper makes it out to be in light of other work, or some omitted variables could also explain the paper's findings. Give the referees a fair hearing—even if you think they didn't do so when they read your paper carefully. Keep in mind too that in matters of opinion (*e.g.*, the paper is too long, the introduction is hard to follow, etc.), it is possible that the referees' opinions could be shared by other readers. In short, if a referee has done a diligent job, there is likely to be something in the report that can be usefully incorporated into the paper.

Of course, unlike when a paper is a "revise and resubmit," there is no requirement to address each point. If the author thinks the referees' points are stupid, she can ignore them. But she will be ignoring them at her own risk. There is some chance the same referee will be asked to review the paper from the next journal she tries.[71] The referee will not be happy if, having spent spent a lot of time writing a report to help the authors, and the authors appear to be ignoring his suggestions.

More likely, however, is that the negative report is symptomatic of something that is wrong with the paper. A referee's objections to something in the paper probably means that something is wrong, either with the formal analysis or the way that the author explained and interpreted the results. If the referee misinterpreted things, then the author should revise the paper with the goal of making sure that the next referee won't react in a similar fashion. Otherwise, a new referee is likely to reject the paper for the same reason as the first one.

One way to view negative referee reports is as a reaction to the choices an author made in constructing her paper. Which specifications did she report and which did she leave out? Which order did

[71] Some referees have a personal "no double jeopardy rule"—they won't review a paper a second time from another journal. However, if a referee tells the editor that is the reason why she or he won't do a review, the editor often asks to see the report submitted to the other journal. In this situation, I once made the mistake of sending the earlier report to the editor, who promptly sent it to the authors and rejected the paper on the basis of my report. It turned out that one of the authors was the department chair of a department in which I had a job seminar shortly after he received this report on his paper. When I was visiting his department, he improperly confronted me and told me that he thought I was the referee who rejected him twice. I lied and said I wasn't, but I don't think he believed me. I didn't get offered the job.

she put them in? How did she interpret the results? The list of decisions an author must make is endless. Referee reports can help authors make better decisions about how to write their papers. Authors sometimes have opinions about their own work that differ from others' on the issues that are most important, the parts of the paper that are most interesting, and the appropriate interpretation of the results. A negative referee report is a chance to rethink the answers to these questions and to use others' opinions to guide the structure of the paper. Hopefully the result of this process is to make the paper more interesting to future readers.

Revise and Resubmit

When an editor likes a paper, she rarely accepts it outright. The referees almost always have some suggestions and editors often have ways that they think the paper could be improved from their own readings. Hence, the editor normally doesn't wish to publish the paper exactly as it was submitted, but believes that, with significant work, a revised version will be acceptable for publication. In this case, the editor returns the paper to the authors and invites them to resubmit the paper after they revise it.

These outcomes are typically referred to as "revise and resubmits" or "R&Rs." With an R&R, editors are often very explicit about the way they want the paper to be modified. The editor says exactly how she thinks the authors should restructure the paper, what referees' points she thinks are most important, and what she thinks are the appropriate inferences for the authors to draw from their analysis. With this type of editorial letter, the editor is making a contract with the authors: if the author does what she asks, then the paper will be accepted for publication.

Alternatively, the editor could be more vague in the R&R, saying something like: "Your paper would be quite nice if you can solve this problem (these problems) with the paper. I have no idea if what I am asking is possible, but I would be willing to consider a resubmission if you can." Implicit in the second response is that the authors shouldn't bother resubmitting unless they can solve the problem(s) the editor raised in his letter. With this type of letter, the issue is often a fundamental one that may not be possible for the authors to address. For example, the editor could ask the author to solve a model via a

closed-form solution rather than through a simulation, or to come up with a better instrument for their dependent variable.

The goal in the editor's letter is to give the authors a direction to push the paper so that the revision will be publishable. I personally think of an editor's R&R letter as being essentially a contract: if the author does what has been asked, the editor will eventually accept the paper for publication. That being said, not every editor views revision requests as contracts. Editors can ask for substantial revisions and, after the authors faithfully do what is asked, reject the paper anyway. In fairness to those editors, they are often explicit that they do not view R&R as a contract and make clear that what they are asking for is necessary but not sufficient the paper to be publishable. Also in fairness to those editors, as with any contract, authors and editors can disagree over whether the terms were met and the authors actually performed what was asked of them.

R&Rs vary a lot in terms of the amount of work they ask the authors to do. Sometimes they are quite easy and be completed in a few days. For example, the editor might ask the authors to expand their discussion of her favorite alternative interpretation of her results, add a few extra tests, or cut out a section that seems superfluous to her. Alternatively, the editor could ask for essentially a new paper, with a totally different focus and methods. Sometimes editors refer to these decisions as "reject and resubmits." In a reject and resubmit, the editor is telling the authors that she likes something about the paper, maybe the data, the question the authors asked, or the overall methodological approach. However, the current version is so far from being acceptable that the editor does not wish to make any implicit promises and the revised paper would have to be sufficiently different from the original so as to be considered a new paper. Between these two extremes is a typical R&R, in which the authors have to rethink a number of parts of their existing analysis and perform some new work.

When an author receives an R&R, she should be happy — she has a shot at getting a revision of her paper published in the journal considering it. But it is only a shot. Papers often do get rejected at the second, third, and even fourth round. It is important to respond to every point that the referees and editors

make in their reports and letters. Even more important, the authors must be able to understand what is bothering the referees and to address the underlying cause of their concerns.

Appeals

Sometimes, however, referees do make serious mistakes. Refeees are human like the rest of us: they sometimes miss key assumptions or explanations, they can get the impression that the authors were doing X when they actually were doing Y or, occasionally, they do not devote enough time to understanding the paper. Editors realize that the review process is imperfect and want to be fair to authors.

For this reason, most journals have set up an appeals process. This process differs from journal to journal, but usually involves the author writing a letter explaining why the decision is incorrect and the editor sending the paper to an associate editor or other senior person in the profession to review the decision. Decisions do, on occasion, get overturned in appeal. At *Review of Financial Studies*, we found that the acceptance rate for appeals was surprisingly similar to the acceptance rate for initial submissions.[72]

That being said, I recommend appealing decisions only when there is a clear and substantive mistake in the referee's analysis. Some authors regularly appeal rejections. Editors know who these people are and find their constant appeals to be annoying. But editors do want authors to appeal decisions when a paper they reject a paper because of an error by the referees. Editors spend a lot of time and effort in their work for the journal and want very much to make the correct calls in their publication decisions.

Personally, I have appealed only one rejection in my career. In the paper, my coauthors and I had what we thought was an airtight identification strategy. But the referee rejected the paper because the referee misunderstood what we did, and argued in the report that the identification we used was problematic. I first ranted to my old friends for a few days (I get really upset after rejections—I don't

[72] Probably the reason for this pattern is that authors only appealed rejections that were close calls.

know anyone who takes them with total equanimity). I then wrote a letter that explained, as clearly as I could, exactly what we did and why the identification strategy in the paper was valid. I let my coauthors tone down the letter because it was too strongly worded (I was pretty angry). We then sent the letter to the journal, and the editor immediately forwarded it to the referee. To his/her credit, the referee immediately realized the mistake, and understood why the identification strategy in the paper was correct. Not too long after the initial appeal, we converged to a version that the referee thought was acceptable, and the paper was eventually published in that journal.

Starting a Revision – The "Response Document"

Where should authors begin when they receive a revise and resubmit? How should they go about in revising their paper? Where to begin? A few years ago, when we had a revise and resubmit on one of our joint papers, Isil Erel convinced me to organize the revision around the cover letter to be included with the revised document, which I always refer to as the "response document." I liked the approach so much that I now use it for all my papers.

The response document for a resubmission contains a detailed summary of the changes the authors have made since the original submission. In addition, it can contain other information, such as polite explanations of why a certain approach was used and why the referee's comments are or are not relevant. It can even contain results that the authors do not want to include in the paper itself. A good response document makes editors' and referees' jobs much easier and can sometimes persuade them that the author has done what they asked without them spending a lot of time on the revised paper. The response document enables the editors and referees to skip over the parts of the paper for which they had no issues in the previous round. They can focus their attention on the way in which the authors responded to what they asked, which is available to them in the response document in an easy to read format.

Some authors wait until the revision is complete before writing the response document, which is how I used to do it. However, Isil persuaded me that it makes more sense to start this document right after

receiving the editor's letter. The response document provides a way to structure the revision process in a very productive way.

Here is what I do: I first copy the editor's letter and referee reports into a file and put it in italics (omitting trival bits, like "Dear Mike"). I then go through the report in detail with my coauthors. When we agree on the way in which we will address each point, we make a note on the response document of how we will do it (often in a bright color). When we make the changes to the paper, we delete the colored note and replace it with a short description of what we changed in language appropriate to give to editors and referees. When all the colored text is gone from the response document, that means we have basically completed the revision. I then add a short introduction to the document explaining exactly how it is structured and the response is finished.

There are several advantages of this approach to revising the paper. First and perhaps most important, it gets all coauthors on the same page as everyone sees clearly what the team has agreed to do. Second, it ensures that every point the referee makes is addressed; it is easy to forget some of the points if there are several long reports and an editor's letter which all contain issues an author must address in the revision. And finally, by doing the response document during the revision process and not afterward, the document is complete as soon as the paper is done, so we can resubmit immediately.

The Substance of the Revision

In a typical R&R, there will be one or two referee reports, each of which can be 5 or 6 single spaced pages long and full of suggestions. Sometimes the ideas overlap, but they do disagree with one another from time to time, which can make satisfying both of them a bit challenging. A good editor will try to give guidance as to which of the referees' points he thinks must be followed, which he views as optional, and which he thinks the author should disregard. In addition, there can be some suggestions on how to revise the paper in the editor's letter, and possibly some from an associate editor as well. The author will typically find most of the suggestions to be useful, but some will probably be pointless and others completely wrong.

How does an author think about responding to a R&R? Before submitting the paper, she probably struggled to cut out as much extraneous material as possible. Does she really want to add a bunch of new material that the referees care about, but will make her paper longer and cause most readers' eyes to glaze over? At the same time, it is nearly always a fatal strategy to appear nonresponsive to referees and editors who spent a lot of time making suggestions and have the right to reject the paper. Responding carefully to every point the referees make while keeping the paper relatively short and readable can seem like an impossible task.

There are a few principles one should have when revising a paper for a journal. First, the authors must address *every* point made in the referee report. Not addressing a point that a referee thinks is important can upset the referees and lead them to recommend rejection on a paper they otherwise like. Second, authors should strive to use the comments as a chance to improve the paper, and not just to jump through hoops to satisfy the referees. However, if the authors add a lot of material to the paper in response to every point the referee makes, there is a decent chance that the referee will then read the new version of the paper, decide that it is too long and boring, and not like it for that reason. Third and perhaps most important, authors should always remember that their name is on the paper for all eternity. After the paper is published, the referees will always be the "anonymous referees" and soon will probably forget the details of their reviews (or if they even reviewed that paper at all). The authors, not the referees, will be the one who gets credit and blame for anything in the published version, regardless of whether it was the referees' suggestion to include it.

How does one go about this seemingly impossible task, adding material without making the paper longer, addressing pointless comments without implying to a reader that you think specious arguments are correct, and satisfying multiple referees who want you to push the paper in different directions? A sensible way of approaching this problem is to remember that although the authors must address every comment in the referee reports and editor's letter, they have a lot of freedom about where they address each point and the way in which they do so.

When I revise papers to address referees' concerns, my rule is that I insist that my coauthors and I only put things in the paper that we truly believe ourselves will improve it. If a referee makes suggestions that improve our paper, we will, of course, incorporate them. Especially important to include in the paper are replies to concerns that appear to be common to a number of readers and have been brought up in seminars. If we do include additional robustness checks at this point, we try to follow the strategy outlined in Chapter 7 and put them in a separate subsection to make it easy for readers to skip if they don't share the referee's concern. If a referee suggests that we do something that addresses a concern that seems minor, it might go in an appendix (possibly an online one). In that case, we would usually mention in the text or in a footnote that the test is available in the appendix, perhaps together with a short description of the results.

For suggestions that seem to us to be specific to one referee and are unlikely to matter to most readers, my coauthors and I will often reply to it directly in the response document and not put anything in the paper. My response documents frequently contain a number of tables intended to alleviate referees' concerns. Because space in the actual paper is precious, I often include tests requested by the referee that might not be of interest to a general reader in the response document. Hopefully, when the referee sees that the additional control he sought is not significantly different from zero, he will be persuaded that these new tests don't warrant space in the paper.

If my coauthors and I put an additional test in the appendix or response document, we always tell the editor and referees that we would be happy to move it to the document itself if they would like. Editors and referees appreciate when authors make sensible choices over where to put additional analyses and offer to move them at the editors' discretion. But editors rarely take authors up on this offer. Over my career, I have offered to move additional tests requested by the referee to the main text many times when I have revised papers. Not once has an editor or referee taken me up on the offer and asked that I move something from the response document or an appendix to the body of the paper.

In a typical R&R, the referees and editor basically like what the authors are doing: the question, data, methods, etc. However, they have a problem with one aspect of the authors' approach – it could

relate to the author's statistics, modeling, interpretation or pretty much anything. The referees or editor will usually try to suggest a specific response to this issue, perhaps a statistical test or a different way of modeling the problem. Sometimes authors, who have usually thought about the issues for much longer than the referees and editors, have a reason not to include the particular remedy that was suggested. In this situation, it is perfectly fine to explain in the response document why the authors don't want to do what the referee suggested. But if the authors don't do what is suggested by the editors and referees, they should include a clear explanation of why they don't want to. In addition, they should discuss, politely and deferentially, why a given concern of the referees or editor isn't really an issue or how an alternative test the authors perform addresses it. The key is that the authors understand exactly what is bothering the referees and address the point head on so that referees know the authors are taking their concern seriously.

Most of the time, if the authors are thoughtful in their use of appendices and response documents, it is possible to address the concerns of the referees and editors in ways that do improve the paper. However, sometimes referees are insistent. One time, a referee decided that my coauthors and I had to include a particular calculation that I thought made absolutely no sense. My coauthors and I went back and forth with the journal a few times and tried to explain why we didn't want to include a nonsensical calculation into our paper. Eventually, it became clear to me that the referee was not going to change his mind, the editor wouldn't overrule him, and the paper wasn't going to get published unless we included it. I held my nose a bit and put the calculation in the paper (my coauthors didn't care by this point, they just wanted the paper published). But I made sure to add a footnote "thanking" the referee for suggesting it; I wanted readers to understand that it wasn't our decision to include the nonsensical calculation in the paper.

After Resubmitting to the Journal

Once the paper is returned to the journal, the process repeats itself. Unless the revisions are trivial, the editor will send the paper back to the referees, who write new reports. Usually the referees are the same but occasionally there is a new referee if one of the original ones cannot do the report, or if the

editor wants an additional opinion. The editor considers these reports as in the first round, then sends the authors a second decision letter. The authors edit the paper in response to the second set of comments in the same manner as the first time, write a new response document, and eventually submit a new revision back to the journal.

This process repeats itself until the paper is either accepted or rejected. At each round, the authors do not have to resubmit the paper to the same journal, but it is usually advisable to do so. Occasionally authors will decide that the revised paper is so much better than the original that they can try to submit it to a higher ranked journal rather than resubmit to the original one. The idea is that they can always resubmit to the original journal if they get rejected by the higher ranking one. Although there is nothing unethical with this strategy, I am reluctant to recommend it. The author could draw the same referees as from the original journal, who will not be happy at the authors' disregard for the work they did in the earlier reports. Additionally, there is the risk of burning bridges with the original journal's editor, who will undoubtedly realize that another journal realized the benefits of his and his referees' efforts to help one improve the paper. For these reasons, I have always resubmitted my papers to one journal so long as I have the option to, and have never tried a second journal in the middle of this process.

Ideally, the authors and referees converge to a version that is acceptable to both and eventually the editor can accept the paper. Sometimes, however, things do not go so well and papers get rejected at later rounds. Revising a paper and then getting rejected can be quite upsetting to authors. One of my closest friends had his job market paper rejected from a top economics journal in the second round. It had taken several years of his time between waiting for the journal to respond to both submissions, and for him to revise the paper in response to the original reports. The reason the journal ultimately gave him for the rejection was that the paper was not interesting enough for the journal. How could it possibly have been interesting enough for them to ask him to spend so much time revising it if it wasn't interesting enough to publish after he did all the work? My friend has had an extremely successful career, so that setback wasn't the end of him, but that experience was needlessly painful for him and should have been handled better by the editor.

Once the Paper is Accepted

Once a paper is accepted for the journal, the uncertainty is resolved and the stress reduced. But there is still work to be done. Most journals send their papers to copy editors, who send the authors a marked-up copy of the paper. These copy editors are usually excellent writers but not specialists in the subject of the paper. In their zeal to improve the paper's prose, these copy editors sometimes accidentally change the meaning of something they edit. Consequently, it is important to go through the copy editor's comments carefully and not just "accept all" of the suggested changes.

The rule for copy editors is that formatting changes are required but all other changes are optional. Each journal has its own style: references must be done in some very specific way; sections enumerated in particular ways; and so forth. Authors must comply with these edits by the copy editors and usually have no reason not to do so. English changes designed to improve the paper's prose are optional. However, unless one is an experienced writer, it is usually a good idea to accept whatever changes are suggested unless they change the meaning of the sentence.

After copyediting, in the last stage before publication, authors are sent page proofs. Journals usually will wait six months to a year to get the authors page proofs and then want a response in 48 hours. This deadline can be ignored; journals will always publish the paper if it has been accepted by the editor. The worst thing that can happen to an author at this stage is that the paper appears in a later issue of the journal than it otherwise would.

In the olden days before word processors, journals used to have to manually typeset papers before they were published. So, authors had to go through every word and number in their paper to ensure that there were no typos or other errors. But today, any errors in words or numbers are usually the authors' fault and were there prior to the final submission. They can still be corrected at this stage but publishers get upset if authors try to make too many changes at the page-proof stage.

One thing important to be careful about when going through page proofs is the formatting of the tables. For some reason, journals seem to want to publish papers in which the numbers in a column are

not underneath the column headings. Or the entries in the table can be right justified so that all of the numbers are in the rightmost part of the cell and look very strange to a reader. Or the headers could be hard to read for one reason or another, either they are in too small a font or the line that is supposed to be beneath them actually goes through them. I have had these problems and have had to send proofs back to the journals multiple times before they are formatted in an acceptable fashion.

The rule here is to be tough and not approve a paper until it is perfect. An author should never approve a page proof until it is 100% correct. There is no need to compromise. The paper will have the authors' names on it forever and the person laying out the tables for the journal might be working for a different company a year after the paper is published. There is clearly a misalignment of incentives; it is in the authors' interest —and the journal's too for that matter — for authors to be persistent and to make sure everything is right.

Part 4: Being a Successful Academic

Chapter 12: How to be a Productive Doctoral Student

When I entered graduate school, my father, who had a PhD in organic chemistry, explained to me the most important difference between doctoral programs and other graduate programs. He told me that my friends who went to medical school, law school, or business school (for a non-doctoral degree) would work at least as hard as I will. They will stress out over their exams and have sleepless nights worrying about them, just like I will. However, virtually all of my friends who enter these programs will graduate. And they will know exactly when the graduation will be with sufficient certainty that their parents can reserve hotel rooms for the graduation ceremony on the day they enter the programs.

In doctoral programs, students' progress is much more uncertain. Many students end up leaving doctoral programs, both voluntarily, if students change their mind about their chosen career, and involuntarily, if they do not pass their exams or cannot write acceptable dissertations.[73] Of the students who do graduate, the time it takes to complete the degree can vary tremendously. Both the average time to completion and its variation across students has increased in recent years. When my father received his PhD in the 1950s and I received mine in the 1980s, most students finished their degrees in four years. Now, in economics and related fields, five years is the norm and many students stay six or seven years. In short, when a student enters a PhD program, there is much uncertainty about whether she will eventually receive her PhD, and if she does receive it, how long the process will take.

Why are PhD programs so different from other programs? What makes students' progress in these programs so uncertain? Is there anything that faculty and students could do to improve the process?

[73] One study of economics departments' doctoral programs finds an attrition rate of 26.5% in the first two years of the program. See Wendy A. Stock, T. Aldrich Finegan, and John J. Siegfried (2006) "Attrition in Economics Ph.D. Programs," *American Economic Review*, 96, pp. 458-466.

I think the answers to these questions lie in the expectations of doctoral students that are different from those in other graduate programs. Most graduate programs, such as law school or business school, are essentially extensions of undergraduate education. Students take a series of classes, some of which can be difficult. To graduate from these programs, all one needs to do is to pass their classes. Students who are accepted to good programs are usually excellent students, so the vast majority of the time, they do pass their classes. In some programs such as medical schools or dental schools there are clinical aspects to the education, which can be extremely time consuming and difficult. But it is rare for a student to flunk out of programs because of their performance in rotations. In the end, most students graduate on time.

In contrast, in a PhD program, the goal is to teach students how to do research. The main graduation requirement is a dissertation, which is an original piece of research that meaningfully moves the frontier of our knowledge. The ability to write a good dissertation requires skills that are not easy to identify. Sometimes, the students who could always solve any problem handed to them on an exam and received nothing but "A's" throughout their lives are unable to come up with a research idea. Other students, some of whom are marginal admits to the doctoral programs they entered and barely pass their qualifying exams, turn out to be the best researchers. The most important attribute of successful doctoral students is a knack for selecting interesting problems and solving them in creative ways. Since success at academic scholarship requires a different set of skills than success at taking classes, it is difficult for faculty or even a student herself to know whether she will be a natural fit for academic research.

It is a good idea for prospective students to spend some time understanding exactly what they are getting themselves into when they begin a PhD program. Academic research in the economics-based disciplines tends to be much more abstract than many outside academia suspect. Entering students often think that in a PhD program, they will do research about how to predict GDP, or to become better at picking stocks. Sometimes PhD students are surprised that empirical academic research tends to analyze systematic datasets rather than do case studies, and that theoretical research uses mathematical models to

convey ideas rather than verbal arguments, emphasizing generality and abstraction more than specific institutional details.

One way that a prospective PhD student can learn whether academic research is right for her is to get involved in it before she starts graduate school. I would encourage anyone thinking about getting a PhD to work with their undergraduate or Master's faculty on their research. If they write a thesis as an undergraduate or Masters student, they should try to publish it. Another option is to do what is becoming known as a "pre-doc", which is a job doing research prior to entering graduate school. There is an increasing number of such positions that have become available. Some schools hire recent college graduates as research assistants for their faculty, many of whom end up going on to get PhDs. In addition, there are jobs available at government agencies and other places that do economic research. Such positions help prospective students learn what is involved with academic research, and also make them more attractive to doctoral programs.

Approaching Life as a PhD Student

Part of the problem many students have during doctoral programs is that they do not approach their time as a PhD student correctly. PhD students almost always have been excellent students throughout their entire lives, otherwise they would not have had either the inclination to enter a PhD program or the grades to get in. Entering students often think the PhD Program is a continuation of their earlier studies, albeit at a higher level. In a certain sense, this view is correct – PhD programs do contain a number of required classes that can be quite difficult.

However, in an important way, PhD programs are fundamentally different from other programs. The underlying goal of any PhD program is to help the students become serious scholars in their area of inquiry. The way a student establishes herself is by writing a dissertation that makes a substantive contribution to the academic literature. A good dissertation will extend the literature sufficiently that other scholars working in her area will want to find out what she did in the paper. Ideally, the dissertation will provide a launching pad for a career of meaningful scholarship.

Given that the objective in pursuing a PhD, to establish oneself as a researcher, is different from the objectives of other programs, a student should approach it differently. When a student enters a doctoral program, she should keep the ultimate goal of producing research in mind from day one. She should work to develop skills, to acquire a knowledge of the literature, and to come up with ideas that can lead to a successful research program.

With these goals in mind, I have a set of rules that doctoral students should follow throughout their years in the program. These rules consist of both things that students should try to do, and also the things that they should avoid. I will first go through the things that doctoral students should avoid. These are summarized in the following list of "Don'ts" for PhD students.

The "Don'ts" of being a Ph.D. Student

1) Don't be too concerned with your grades. When you apply for a job, nobody will ever ask for them.
2) Don't be competitive with your fellow students. Your ultimate placement depends on the quality of your work on a national scale, not on the ranking relative to your fellow students in your department. If you work well with your fellow students, they will be your future colleagues, coauthors and lifelong friends.
3) Don't be too intimidated to ask questions of faculty. We are being paid to answer your questions, so ask them!
4) Don't go "into hiding." Always keep a high profile in your department, even if things aren't going particularly well.
5) Don't be restricted to one area, or the specialties of your advisor. Most faculty are broad enough to supervise the work you want to write, and not just what they want to write.
6) Don't ever think that the first draft of a paper will be "good enough". It won't. Most faculty papers go through multiple revisions before they are ready to be submitted to a journal; yours will probably go through between 5 and 10 major revisions before you are ready to send it out on the job market.
7) Don't think of research as a game. You are trying to teach the profession something meaningful, not just to "get out". The same goes for the publication process; the best papers are written to learn something important, not just to get published.
8) Don't write papers that are interesting only to academics. Try to explain the actual economy, it's more fun and interesting that way.
9) Don't write papers for the purpose of illustrating a technique. Find an interesting question and use the techniques appropriate for the question.
10) Don't write papers that are relevant today but are unlikely to be of interest in the future.

"Don'ts" 1-2: Don't Worry about Grades and or be Competitive with Fellow Students.

Until a student enters a PhD program, her grades are almost a sufficient statistic for her academic success. They determine her GPA, strongly influence the academic honors she earns, and are a major factor in her job placements and admissions to subsequent programs. The emphasis on grades can lead to a zero-sum game mentality and competitiveness between students. After all, the same number of students are chosen for Phi Beta Kappa, law review, and other prestigious positions each year. If a student helps her friend succeed, that means fewer honors that are available for her.

When the student enters a PhD program, she naturally assumes that the same rules apply. But they don't. Of course, it is always better to get an "A" than a "B", and PhD program directors normally do an annual review of the students to ensure that each of them has done reasonably well in their courses. However, beyond these reviews, grades in PhD programs really don't matter very much. Unlike when one graduates from most other programs, job applicants do not provide their grades to potential employers. When a school is thinking of hiring a newly-minted PhD from another school for their faculty, it is not just that the hiring committee doesn't care what grade she got in econometrics, but they don't even know what it is as it is not on the application materials. Grades are considered to be irrelevant by hiring committees at universities, and also by private sector firms and government agencies considering hiring newly-minted PhDs.

Since grades don't really matter, PhD students should not obsess about them. Instead, PhD students should approach every course as a researcher rather than as a student. Students should always be thinking about things that they learn in the course that can be applied to a research project. These things could be a research area or problem to be addressed, a technical skill that helps them address a problem they have been working on, or an idea or way of thinking that could be further explored that could be applied to other settings. Classes should be thought of as a means to helping students do better research rather than as an end in themselves.

Equally important to the way that a PhD student should approach her classes is the way she should approach her classmates. Growing up in a zero-sum environment in which a classmate's

achievement can takes away from a student's success, can lead to students thinking of each other as competitors. This kind of thinking in doctoral programs can be extremely harmful. Most successful students learn at least as much from their classmates as they do from their professors.

A helpful, cooperative environment in which students have long discussions about research, read each other's papers, and encourage each other through the inevitable setbacks that PhD students experience, can be as important to a student's success as the faculty's research prowess. I've always felt that one of the secrets of the success of the PhD program of the MIT economics department is that students always seem to help one another and write papers together. One of my closest friends, Ben Hermalin, was a year behind me, and specialized in contract theory. I lost track of the number of times he read the main essay of my dissertation, an empirical study about boards of directors. By the time I presented it on the job market, he was practically a coauthor. This experience led us to coauthor a paper on boards, which led to another one, and eventually a research program that has continued throughout our careers. Over the past 30 years, we have published seven coauthored papers and edited a book together about boards of directors and other aspects of corporate governance. I think of this research as an outgrowth of the working relationship we formed as doctoral students.

The dynamics between students is especially important in smaller doctoral programs, such as the ones we have in business schools. I've been on the faculty of four business schools, all of which have small but successful doctoral programs. To my surprise, I have found that at all of these schools, the most important determinant of student success has been, not the qualifications of the students when they entered the programs, but whether the students in a cohort like and cooperate with one another. At each of the schools in which I have taught, there have been years when the students socialize with one another outside of school and help one another. In those years, the students are usually happy during the program, write high quality dissertations, and eventually get jobs at good universities. In other years, students don't like one another, stay away from each other as much as possible, and struggle to complete the program. I always tell first-year doctoral students that I can't tell them whom to like and to socialize with. But if they

do happen to get along with one another and help each other through the program, they will all be happier and do better in the program.

*"Don'ts" 3-6: How **Not** to Interact with Faculty*

Many PhD students think that they should approach their time writing their dissertations in the following manner: The students think that they should retreat into their apartments or some corner of the library and spend most of their time there polishing their paper. To impress a faculty member, they think that they should make the paper an extension of one of the professor's papers, and conclude the paper by providing evidence that support's this person's favorite theory. While writing the paper, the students don't ask any questions of the faculty member or make any effort to get to know him, but do work hard making the paper perfect. Then, after not seeing the professor for a long time, they hand him their paper, expecting him to tell them that the paper is brilliant.

Unfortunately, what usually happens when a student adopts this approach is that the faculty member doesn't like the paper, gives tough feedback, and the student ends up being very upset. Sometimes these students recover and write good dissertations, but other times they drop out of the program and find jobs outside academia. What is particularly sad about this situation is that it is entirely preventable. Had these students approached their interactions with faculty in a different way, the writing process would have had a much happier outcome.

Faculty are paid to teach students. They understand that most students don't have the experience or knowledge to put together a research paper that extends the forefront of our knowledge without a lot of help. Faculty know that for the vast majority of PhD students, it takes many conversations with their advisors and classmates, as well as many unsuccessful attempts, before the students can write a high-quality dissertation. Most (but not all) faculty view these conversations as one of the best parts of their jobs, since they love to spend time chatting with PhD students about their research and helping students make it better.

But the PhD students usually have to make an effort to initiate a dialogue with the faculty members with whom they wish to work. Sometimes students, especially those from countries in which the culture is for faculty to be distant from students, find these conversations difficult. Such students most likely have never had to seek faculty help for them to succeed. Their natural tendency is to "go into hiding" until they have something that they think faculty will really like.

The problem with this approach is that most academic research, with a few exceptions such as Andrew Wiles' proving of Fermat's Last Theorem (see Chapter 1), is not conducted in this manner. By cutting themselves off from contact with advisors and classmates, these students that "go into hiding" are depriving themselves of the very resource they need to write a good dissertation. Regular conversations with faculty and other students are, for the vast majority of PhD students, a necessary ingredient of a successful research paper. Each student should try to find some faculty with whom they are comfortable talking, and make sure these faculty think a research project is promising before spending too much time on it.

Another problem with the way in which some doctoral students interact with faculty is that students can misinterpret a faculty member's feedback on the students' research. It is sometimes more difficult than one might think to understand exactly what individual faculty mean by their comments. At a university where I used to teach, I had a colleague with a very negative disposition. He hated most ideas, and if he said one was "OK", that was high praise. A different faculty member at the same university would describe almost any idea a student proposed as "Great". It was sometimes hard for the doctoral students to realize that receiving "OK" from the first person is higher praise than receiving "Great" from the second!

A mistaken belief that many students have is that faculty are only interested in helping students working in the subfield where they work themselves. My experience, both as a student and as a faculty member, is that this view couldn't be farther from the truth. My dissertation was about the role of boards of directors in corporate governance. My advisor, Jim Poterba, has published papers in virtually every

area of economics except the one on which I wrote my dissertation, corporate governance. However, when I asked him for help with my research, he could not have been more helpful.

When I have been a thesis advisor, some of my most successful students have written dissertations in subfields in which I have never worked myself. I found supervising these dissertations to be particularly interesting and rewarding. In several cases I ended up working together with the students in their chosen areas following their graduation. These students' dissertations that started in areas outside my own research interest ended up leading to successful research programs for my students, and also for me.

One final misperception that students sometimes have concerns their expectations about the reactions that first drafts of their work will receive from faculty. These students have gotten "A's" their entire lives and given faculty papers that the students are very proud of. The faculty often read the papers and tell the students that the papers are not very good, sometimes in a not-so-polite manner. This reaction can be devastating to the students.

PhD students should always approach their work with low expectations about the reception it will receive from the faculty. If a faculty member tells them that the paper is "Not That Terrible", the student needs to learn to take that admittedly depressing feedback as good news. Maybe the next draft will be "OK". The one after that might be "Pretty Decent", and after that, it will be "Getting There". A paper usually doesn't become "Really Good" until the sixth or seventh draft. That is just the way it is in PhD programs; if a student has trouble dealing with that kind of feedback, she should probably go into another line of business!

"Don'ts" 7-10: The Kind of Research I Don't Encourage

The final set of "don'ts" concern the type of research that I think that young scholars should avoid, and the way that they should pick topics. Obviously, the choice of research topics is a matter of opinion, and there are excellent dissertations written on many different subjects. My concern is that there is a natural tendency for PhD students to gravitate to topics that do not serve them well in the long run.

Students are anxious to impress their advisors and fellow students, and they see what topics are currently being published in the top journals. Sometimes students end up choosing topics because they seem fashionable and not because the students have a deep interest in the subject, or because the subjects are fundamentally important.

A research idea often can take on interesting dynamics. An idea that is novel when first proposed can get boring pretty quickly after a few papers have been written about it. Suppose a scholar develops a model with 2 firms that illustrates a new idea of hers. Then someone else will undoubtedly write down a similar model with 3 firms, even if the inclusion of the third firm doesn't add anything interesting to the analysis. Eventually, it will become an arbitrary *n* firms. Then someone adds taxes, someone else does it in continuous time, and on and on. These things can take a life of their own, especially if someone thinks there is a mistake somewhere in the analysis, and the original author gets upset and responds. All of the follow up papers usually contain basically the same idea, just differ in details. It becomes a game in which participants try to "win" and to show off their skills, rather than to explain anything about the world.

Rather than play these games, my preferred approach to research, which I encourage my students to follow, is to explain something interesting about the world that non-academics care about. I try not to fight with other academics about details of their papers. My rule for research topics, especially for PhD dissertations, is that the author should be able to explain how any research project she undertakes answers an important question in a way that intelligent non-academics care about. I find that this rule grounds academics in reality, and leads to more relevant research. Academics who find this rule to be constraining should remember that research topics that non-academics are concerned with tend to be important ones. Papers on topics that are interesting to non-academics tend to appeal to academics as well, so are likely to lead to successful outcomes in the job market and publication process.

I should emphasize that this view of research is not universally accepted among academics. I have a former colleague and good friend who always told doctoral students to write "technique-oriented theses". He thought that students should pick topics that show off their skills. By doing so the students would impress other academics and have successful careers. This friend is not alone in disagreeing with

me on this point. Many prominent faculty push students toward dissertations in which they attack the work of other scholars, or focus their research around technical issues of interest only to academics.

However, I don't write myself, and don't encourage my students to write, papers where the main aim is to show off skills, to attack other papers, or to make methodological contributions. I would never write a paper for the purpose of illustrating a technique – the first order of business for me is to study interesting problems, and it is the problems themselves that determine whether a research project is worth tackling. Of course, I always try to use the appropriate methods for whatever problem I am studying, to not hold back when I disagree with other scholars, and to make clear if something I have done is a methodological improvement that someone else can use. But the main point of my papers (and my students' papers) is never the interactions with the other scholars; it is always the extent to which I can shed light on the problem I am studying.

A final "don't" concerns the time horizon of the topics on which students choose to work. Every student writes one and only one dissertation unless they get a second PhD. Ideally, the dissertation should help her reputation for the remainder of her career. Consequently, when picking dissertation topics, it is preferable if the topic is one that people are likely to care about in the future. While it is impossible to know for sure what topics people will care about in the future, it is clear that some topics will have much shorter half-lives than others. Even if many people care about a topic when a student is in school, it does not do her much good later in her career if people aren't interested in it at that point.

For example, when I was a PhD student in the 1980s, there was much interest in the Tax Reform Act of 1986. This act was one of the largest changes ever made to the US tax code, substantially lowering both corporate and individual rates while raising capital gains rates. It was very controversial at the time, and to some extent, still is today. There were many papers about it in the 1980s, and I presume there were dissertations written about the effects that it would likely have on the economy. But today, 1986 is viewed as ancient history by current doctoral students, most of whom were not even born at that time. My guess is that the vast majority of economics PhD students today have not even heard of the Tax Reform Act of 1986, let alone would spend valuable time reading old papers that speculate on its effects might be. If one

had written a dissertation in the 1980s about the Tax Reform Act of 1986, it is likely that most scholars today who do not specialize in tax policy would have forgotten about it.

The "Do's" of being a PhD Student

1) Do develop a comparative advantage. This can be a unique data set, institutional knowledge, and/or an econometric or modeling technique. Try to use this comparative advantage to develop a reputation for being the best scholar addressing certain kinds of questions.
2) Do try to apply ideas you learn in one course to another. Often approaches that are standard in one field can be used productively in other fields.
3) Do incorporate your own experiences/knowledge into your research. Sometimes students start with a comparative advantage in one area given their backgrounds.
4) Do out and get all of the data you can. Don't limit yourself to the particular sample that happens to be at hand. Be imaginative about different types of data sources. Sometimes unusual kinds of data can lead to the most interesting projects.
5) Do make sure you are proactive but polite in your dealings with faculty. Remember that they want to help you out but are very busy themselves.
6) Do pick an advisor whose scholarship you admire, and whom you like personally. No matter how good a scholar he is, if you don't get along with him, your life will be miserable.
7) Do bring something on paper to every meeting with a current or potential advisor. It doesn't need to be perfect, and can only be a paragraph long, but gives him/her something to react to. If you have done some analysis or modeling, email it advance to get a more informed reaction.
8) Do be strategic in sharing drafts with your advisor—if you overwhelm him/her with requests, the quality of feedback will decline.
9) Do take advantage of the resources you have access to. This especially includes getting to know all of the people who come through town on the seminar series (not just from your department).
10) Do get involved in research as early as possible. The sooner you discover what research is all about, the more likely your ultimate success.
11) Do present early and often—every lunch brownbag and mini-workshop you can get invited to.
12) Do act professionally in all of your dealings with students, faculty, and administration.
13) Do work hard. Work really hard. But remember to enjoy yourself. Your time in PhD program can be one of the happiest times in your life!

"Do's" 1-4: Developing a Research Program

By the time a doctoral student is about to write a dissertation, she has done well in many different classes. She has acquired a substantial knowledge of the literature and can solve (almost) any problem that is handed to her on an exam. However, in the economics-oriented fields, a dissertation topic is not handed to her. Getting started on a dissertation is likely to be the hardest part of her PhD program. Ideally, the dissertation will turn into a research program that will continue long after the student leaves graduate school and change the way that others think about an important issue. Coming up with a dissertation topic that can turn into a successful research program is often a daunting task, which not every young scholar is able to complete. It amounts to creating a personal brand for which a scholar will be known for throughout her career.

There is no magic formula for developing such a personal brand. If there were, academic research would be much easier than it actually is. I have spent much time discussing the issues involved with developing research programs in Chapters 2 and 3, so I will not repeat myself here. But I do have some suggestions that seem particularly relevant to doctoral students in economics, and even the broader social sciences, that I outline in "Do's" 1-4.

The first concerns developing a comparative advantage. Graduate school is a time when, although it doesn't seem so, students actually have a lot of time on their hands. Most students spend an amazing amount of time worrying about exams, gossiping about faculty, and wasting time in other ways. When I was a doctoral student, many of my classmates and I found the time to become addicted to computer games, to play a lot of tennis and softball, to attend concerts from the Boston Symphony Orchestra, to go skiing as often as possible, to learn to folk dance, and to be involved with many other activities. Most of us still managed to take four or five classes in our first few years, to attend many seminars, to graduate in four years, and to have successful careers subsequently. The key is to spend a lot of time working, and to be very intense when one is working. But it is a perfectly fine, and in fact can increase productively, to take some time off and to enjoy other activities as well.

Students should try to use their time as a doctoral student investing in as many kinds of human capital as possible. For a young scholar, valuable human capital includes not just a knowledge of the literature, but also technical skills that go beyond what is taught in classes and known by most scholars in the student's subfield. In addition, knowledge of the relevant economic institutions and available databases about a subfield is important human capital that a scholar must acquire. A surprisingly large number of students go about writing dissertations without the relevant institutional knowledge of their area. These students often fail to ask the most interesting questions, approach issues incorrectly from a methodological perspective, and ultimately end up doing less impactful research than they could have.

Both technical skills and institutional knowledge are much easier to acquire when one is a doctoral student and has free time, than after she becomes a faculty member. Faculty have to teach classes, to attend faculty meetings, to help with recruiting and to do many other things in addition to their research. If a research project requires an upfront investment that requires a substantial amount of time, doctoral students actually have an advantage over faculty. Projects that more senior scholars are reluctant to undertake because they require an investment in a time-consuming activity such as data work or programming, can make excellent choices for dissertation topics.

Of the corporate finance scholars in my rough age cohort who have "grown up" with me in the profession, a number of the most successful ones acquired valuable specialized human capital in a particular subfield when they were doctoral students or young faculty. Each of them used this capital to write a series of papers that are still among the most important papers in the area in which they work. For example, my former colleague Kevin Murphy wrote his dissertation on executive compensation before it was a commonly studied field, and his early work set the tone of that literature for many years. Jay Ritter had a similar influence on the literature on Initial Public Offerings. As doctoral students, both Kevin Murphy and Jay Ritter became experts on the institutional features in their area of specialty, gathered the best available databases, and consequently were able to write some of the most important papers on these topics. This model, acquiring a competitive advantage through a deep knowledge of the institutions, data,

and economics of a particular market while in graduate school, is not particular to corporate finance – it is a recipe for success in most other fields as well.

Another advantage that doctoral students have over faculty is that they are taking up-to-date courses in a number of different subjects. In contrast, faculty struggle to keep up with what is going on in their own field and cannot hope to understand the developments in other fields at a deep level. Applying the approaches commonly used in one field to another one is a productive way that doctoral students can contribute to a literature.

Before he was a public figure, Paul Krugman followed this model of applying advances in one field to another as the bases for his early work. This work consisted of developing the "New Trade Theory" by taking ideas about increasing returns to scale that were becoming popular in Industrial Organization, and applying them to international trade.[74] The outcome of this effort was a revolution in the field of international trade that eventually led to Krugman's Nobel Prize.

In my field, corporate finance, much of the progress that has occurred during recent years has involved taking identification much more seriously using both reduced form and structural approaches. Many of the reduced form approaches were popularized by Joshua Angrist and other labor economists, and the structural approaches by industrial organization economists such as Ariel Pakes.[75] Identification is a serious problem in corporate finance research, just as it is in Labor and Industrial Organization. Therefore, it is not surprising that progress was made in corporate finance by applying approaches that were popular in those fields.

I always encourage students to take advantage of their backgrounds and knowledge, and to try to apply them to their research. Every student enters graduate school with their own experiences, knowledge and skills that can provide a competitive edge in their research. Sometimes the competitive edge comes

[74] See Krugman, Paul R., (1979) "Increasing Returns, Monopolistic Competition, and International Trade," *Journal of International Economics* 9, pp. 469-479.
[75] See Joshua D. Angrist and Jörn-Steffan Pischke (2009) *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton University Press, and Berry, S.T., Levinsohn, J., Pakes, A. (1995). "Automobile prices in market equilibrium" *Econometrica* 63(4), 841–890.

from someone entering school with a unique set of technical skills, while other times it has to do with institutional knowledge. For an example of institutional knowledge being particularly useful, a former student worked at Goldman Sachs prior to entering graduate school, and while there, became an expert in the "structured products" that helped lead to the 2008 Financial Crisis. He wrote an excellent dissertation on the topic, which proved to be the start of a very successful career.[76]

Another thing that students should always consider is the use of nonstandard sources of data. Sometimes the best insights into important problems come from different kinds of data from that which has been standard in a particular subfield. Some influential papers in labor economics have taken advantage of unique data sources that have allowed them to intriguing findings that appear to be clear evidence of discrimination in the workplace. Claudia Goldin and Cecelia Rouse study the way in which the introduction of "blind" auditions affected the hiring practices of symphony orchestras. They find that when auditions are blind, more women are chosen than when judges can see who is playing, strongly suggesting there was sex discrimination prior to the introduction of this practice. Marianne Bertrand and Sendhil Mullanaithan created their own database by sending fake resumes with names that are either commonly associated with African Americans (Lakisha, Jamal) or whites (Emily, Greg). They find that the resumes with the White-sounding names were more likely to receive calls than the resumes with African-American names. Since the resumes were constructed to be essentially identical in other respects, the obvious implication is that the hiring firms treat applications differently depending on the likely race of the applicant.[77]

I sometimes try to utilize unique data sources in my own papers. I have had fun studying the letters written from a large institutional investor to companies as part of their activism programs. I also

[76] See Taylor D. Nadauld and Shane M. Sherlund (2013) "The Impact of Securitization on the Expansion of Subprime Credit," *Journal of Financial Economics*, 107, 454-476.
[77] See Claudia Goldin and Cecelia Rouse (2000) "Orchestrating Impartiality: The impact of 'Blind' Auditions on Female Musicians," *American Economic Review*, 90(4), 715-741, and Marianne Bertrand and Sendhil Mullanaithan (2004) "Are Emily and Greg more Employable than Lakisha and Jamal? A Field Experiment on Labor Market Descrimination," *American Economic Review*, 94 (4), 991-1013.

was able to analyze the actual minutes of board meetings to study the way boards actually function. In both cases, being able to access private data led my coauthors and me to interesting research projects.[78]

Some of my favorite finance papers are based on data from the market for used aircraft, on the width of train tracks in the nineteenth century, and on weather-related delays in maritime shipping in the eighteenth century. My guess is that most readers who do not know these excellent papers would not have guessed that these topics would be the subject of influential *finance* papers. Perhaps not surprisingly, all three of these papers were written by doctoral students as part of their dissertations. Creativity is an attribute that doctoral students have in abundance, and they often have success when they use it in their research![79]

"Do's" 5-8: Interacting with Faculty

The quality of a doctoral student's relationship with the faculty in her area is one of the most important factors determining the student's success. How should a doctoral student foster a productive relationship with faculty? Are there any particular things that a student should think about doing? How is the relationship with faculty different as a doctoral student than it is as an undergraduate or Master's student?

One might think that it is obvious that a doctoral student should work hard building relationships with faculty. But sometimes students don't make the effort. Many students have spent their entire lives doing well in their classes more or less on their own, by paying attention in class, learning the material by

[78] See Willard T. Carleton, James M. Nelson and Michael S. Weisbach (1998) "The Influence of Institutions on Corporate Governance through Private Negotiations: Evidence from TIAA-CREF," *The Journal of Finance*, 53, 1335-1362 and Miriam Schwartz-Ziv and Michael S. Weisbach (2013) "What do Boards Really do? Evidence from Minutes of Board Meetings," *Journal of Financial Economics*, 108 349-366.

[79] The papers referenced in this paragraph are: Todd Pulvino (1998) "Do Asset Fire Sales Exist? An Empirical Investigation of Commercial Aircraft Transactions," *The Journal of Finance*, 53, 939-978; Efraim Benmelech, (2007) "Asset Salability and Debt Maturity: Evidence from 19th Century American Railroads," *Review of Financial Studies*; and Peter Koudijs (2016) "The Boats That Did Not Sail: Asset Price Volatility in a Natural Experiment," *The Journal of Finance*, 71, 1185-1226.

themselves, and doing a good job with the assignments. These students naturally assume that the same approach will work well in a doctoral program. The problem is that it usually doesn't.

The goal of a doctoral student is to become a serious scholar, which normally only occurs through an apprentice-type relationship with the faculty. It can be a problem if students do not establish good relationships with faculty and do not talk with them regularly. These students can drift aimlessly from topic to topic and never hit upon one that makes an impact on the literature. Most often, the students who have trouble contributing to the literature are the same students that don't spend much time talking with faculty.

To establish a good relationship with the faculty, a student should be somewhat proactive (but polite) in the way she deals with them. Students should remind faculty from time to time that they do exist, and are interested in studying a particular area. They should read the papers before relevant seminars and ask good questions during them. In addition, they should make sure to keep the faculty who do related work to their own aware of their work and to talk with them regularly about it. Faculty always like it when doctoral students are full of ideas and have a genuine interest in the subject they are studying.

It is also a good idea for the students to quickly go through the faculty's related research before approaching them. For example, their questions about the data may already be answered in detail in the faculty member's papers. It can be a waste of the faculty's time if students don't bother to read the faculty member's related paper before approaching him.

Students should also do high-quality RA work to build a good reputation among faculty. In programs where students are not required to do RA work, students can also volunteer to help faculty. A former student tells me that in her job as a faculty member, she has spent a lot of time helping two students who volunteered to assist her with teaching (paid), and research (almost no pay).

Most faculty really do enjoy helping doctoral students. A close relationship with their students is something that many faculty treasure. However, faculty are usually very busy, and there are other students who would like to interact with them. Unless a student makes the effort, she might not get to know the faculty she is interested in working with.

The Thesis Advisor

Almost as important to a doctoral student as the subject of the dissertation thesis topic is the choice of an advisor. For every doctoral student, there is one, or sometimes two, faculty who are deemed "advisors", and are formally responsible for the student's progress. Ideally, the advisor will guide her through the process of writing a dissertation, help her navigate the job market, and be her friend and a valuable resource for the rest of her career. In the hard sciences, a student often is linked to a particular advisor from the beginning of the program because the advisor is the one who provides the student's funding. However, in economics and its related fields, students can choose whomever they want, usually after the second year in the program. How should a doctoral student approach this decision?

Students often enter doctoral programs with a strong sense of the professor with whom they would like to work. They are interested in a particular subfield and enroll in a particular doctoral program hoping to work with the person there who is an expert in that subject. Sometimes, this approach can work out well. But not always. The student could fail to impress this professor. Or the professor could become a dean, move to a different university or to the private or government sectors. Or the student could realize that she is not really interested in the area she thought she would work in, but that a different area is fascinating.[80]

Consequently, I always recommend that students try to enter programs that have a number of potential thesis advisors who are experts in different areas of research. Regardless of what area a student thinks she is interested in studying, or whether the student thinks that she would be interested in working with a particular professor, she should try to get to know all the subfields in which her department teaches classes, and all of the faculty she can. Many times, students discover that the work that attracted her to the

[80] This last scenario describes my experience as a graduate student. When I entered graduate school, I thought I would do research on derivatives pricing, since I had been an RA on the topic and my undergraduate professor told me that it was a good field to study. I ended up doing a dissertation on corporate governance. Since then, I have worked on many different topics, none of which have anything to do with derivatives pricing.

subject and led her to go to graduate school is no longer state of the art, or is not in an area where significant work is currently going on.

An important factor that students usually don't know much about when they start their doctoral program is the personality of a potential advisor, and their approach to supervising students' dissertations. Some faculty are warm and like to socialize with students, while others are cold and tend to keep their distance. Some spend a lot of time talking to students at every stage of a research project, while others don't want to talk much with the students until after the students have completed a draft. Some like to work with students with great technical skills, while others don't care as much about technical ability as creativity. These, and other differences in the way that faculty approach advising, can have a big impact on a doctoral student's experience with a particular advisor.

Students should work hard to find a faculty member with whom they have a good fit in terms of both the type of research he does and the style of advising he uses. The advisor/advisee relationship is a long-term one. It is extremely important for the doctoral student's career (and her mental health) that the relationship is a productive one.

One thing that students often worry too much when picking an advisor is what might be called academic politics. Some students think that it is extremely important that they choose the most famous person in the department, or choose the person who they think is best connected to help her get a good job after she graduates. It is true that the most famous faculty tend to be among the better scholars in a department, so are likely to be good choices as advisor. But not always. Sometimes the famous person is so busy that he is unwilling to spend much time helping students. Or the famous person's style is one with which the student is not comfortable. Or the famous advisor has so many students that he is unable to give each one sufficient attention.

In any of these situations, an alternative choice might lead the student to write a better dissertation and to be happier while writing it. Ultimately, the only thing that really affects a student's career is the quality of the dissertation she writes. **Therefore, a student should ask the faculty member who the student believes can help her write the best dissertation to be her advisor.** Period.

Everything else should be secondary. Students who keep this choice as simple as possible and pick the person who can help them the most are usually the ones who are most satisfied with their choice several years later.

After a student selects an advisor, the real fun begins. And yes, it is fun! Research amounts to discovering something new about the world. Then proving it, first to oneself and then to the world. If one has the DNA of a scholar, the process can be exhilarating.

The advisor's role is to guide a student through the process of writing her dissertation. A student should approach the relationship somewhat strategically to maximize the value that the advisor can add to it. It is a good idea to have regular meetings between a student and her advisor, and a student should try to keep in regular contact with her advisor. When I am a student's main advisor, I like to meet with the students pretty often, usually every week or two, with informal discussions more often than that. However, other faculty have different styles than mine, and prefer meeting less frequently.

Prior to a meeting with one's advisor, it is a good idea for the student to prepare something in writing to focus the discussion. If it is short, the student can just bring it to the meeting. But if it is longer and will require some time for the advisor to digest, the student should send it to the advisor in advance so that he can read it and think about it.

A doctoral student should try not to waste an advisor's time. Some students give faculty drafts of new papers every few days. These papers usually aren't well thought out and have problems that the students should have been aware of. Students should realize that professors' time and energy are limited and should think about how to utilize them efficiently. Besides sharing drafts, if a student overwhelms him with questions to the point that the questions become annoying, the quality of the answers the student receives will decline. Running ideas by graduate student friends before approaching professors about them can be a good policy, especially for students who have many ideas and want to weed out the less promising ones.

"Do's" 9-13: Being a Successful PhD Student

Much of a student's success in a doctoral program comes from her attitude and the way she goes about life as a doctoral student. No program is perfect, and in every PhD program, there is always a lot of complaining. No matter how much quality control departments impose, some courses always end up being badly taught. Some departments emphasize certain fields over others, which can be a problem for students interested in the neglected fields. Placements are often disappointing. Faculty tend to get out of date in their research, sometimes do not care enough about the doctoral students, and fight with one another. The list of things that doctoral students complain about is endless.

However, the most successful students tend to be the ones who look past a program's deficiencies and take advantage of the resources that *are* available to them. Most universities have some areas in which they are strong; a student should learn those areas well regardless of what they want to specialize in. For example, if a school has strong econometricians, students should take a few extra econometrics courses; if it is good at theory, then she should take a lot of theory courses; and if there is a lab for doing economic experiments, she should learn some experimental economics. In addition, if the university has a strong law school, accounting department, or statistics department, it might make sense for a student to think about research issues that take advantage of complementarities between those fields and economics.

Looking beyond the resources offered by the university, students should try to take advantage of other resources they have access to. In most fields, there are practitioners and government officials doing work in the area who can help a student understand institutional details of markets and sometimes can provide useful data. These non-academics are often delighted to help bright graduate students, but many students don't make efforts to get to know them. When practitioners working in areas related to students' interest come to campus to give talks to MBA students and undergraduates, doctoral students should attend and introduce themselves to the speakers if there is an opportunity. Doctoral students should read newspapers, websites and magazines for material related to their research, and should not be bashful about cold-calling people who might help them with their work.

Every student has her own experiences, skills and connections, which can be helpful in writing a dissertation. In fact, sometimes something in one student's background can help another student with her

research. For example, my job market paper was an empirical study of corporate governance, but was not based on a publicly available database. Instead, the data on boards of directors that I used in that paper were hand-collected by one of my classmates, Jim Dana, when he was an undergraduate. Jim and his coauthors were kind enough to share them with me. If I had not known Jim, or if he and his coauthors were less generous, my career would likely have gone in a completely different direction.

Another thing that helped me when writing my dissertation was that I had been doing research for a while before I began. I worked as both an RA and a coauthor with a number of faculty in economics, finance and mathematics while I was an undergraduate. During my first few years as a doctoral student, I wrote several pretty decent papers on taxation that I should have published, but unfortunately did not. By the time I started working on my dissertation, I knew at least a little about the academic research process.

I encourage all students to get involved in research as early as possible. Research is a skill with a steep learning curve. The more involved a student is in research early on, the higher on the curve she will be when it comes time to write a dissertation. In addition, if a student is able to publish her early work, these publications will be a large boon for her when she looks for a job. Even if there is not tangible publication, she will learn details about the literature, data, and modeling necessary to work in a particular area. More importantly, she will begin to understand how frustrating, and ultimately rewarding academic research can be. This experience will be extremely valuable to her when she starts writing her dissertation.

Once a student starts doing research on her own, she should strive to present it as often as possible. There are always a number of places where a doctoral student can present her work. Most departments have regular seminars and brown bag workshops. In addition, students can sometimes present in classes, and they can organize their own mini-brown bags where they present their work to each other.

Students' presentations improve over time for a number of reasons. First, during the presentations, students hear tough questions about their analysis and can address the concerns as they revise their papers. Second, with practice students will discover the best way to present a particular piece

of work. And third, when students present more, they will become more comfortable talking to a group about their work. They will respond better to questions they have not heard before, and they won't be as intimidated when a faculty member they wish to impress doesn't like their work. Presenting research can be hard work and stressful, but practicing as often as possible will help students develop a skill that is necessary to succeed in academia (and elsewhere).

Graduate school is a transition period between being a student and being a faculty member. As one proceeds through the program, a student should try to act more like a faculty member. To the extent that they get involved with undergraduate teaching, they should realize that they will be held to the same professional standards of conduct as faculty. For example, doctoral students should behave professionally around undergraduates and should be especially careful that any personal relationships with the students they teach are appropriate. Graduate students should even think a bit more about how they dress. If students would like faculty to think of them as peers who they can recommend to their friends at other universities, it can't hurt if the students dress like a professor does, especially at public events like conferences.

An important part of the transition is to learn to think of faculty as peers. An easy first step in this transition process is to address faculty by their first names. More important is the way students approach their interactions with faculty. Some students think that the best strategy when talking to a professor is to nod yes, and to agree with anything that they say, even if the student disagrees. But academia is about the back and forth exchange of ideas. Faculty are most impressed with students who disagree with them from time to time, since it is these disagreements that help both faculty and students sharpen their ideas.

Successful doctoral students work extremely hard, usually harder than they ever have before in their lives. Any student entering a doctoral program should do so with the expectation that it will not be easy, and that they will struggle to keep up with the workload. When I was a doctoral student, my rule was that unless I had something better to do, I would work at the NBER every evening and every weekend. When I went there, I could always find the same group of friends working on their dissertations.

All of my friends who were at the NBER every evening have gone on to very successful careers, and some of them ended up becoming the top economists of my generation.

But graduate school can be fun too. Looking back, it was one of the best times of my life. I was taught by famous scholars, learned many fascinating ideas, and met other students from all over the world. I loved listening to the high-level debates, over both academic ideas and their implementation in the real world. And despite the workload, there was time to enjoy life away from the university as well. If one decides to go to graduate school for a PhD, one should expect to have an intense but ultimately rewarding experience.

Racial and Gender Related Issues.

Throughout this book, I have discussed a number of potential pitfalls that a scholar can face when pursuing an academic career. Some of them are inevitable given the inherent difficulties in producing new knowledge, combined with the competition arising from the desirability of tenure-track positions at research-oriented universities. But there are a number of in principle, solvable issues that would both make academia a more attractive place to work and enable us to do a better job for our students and society. Particularly problematic are issues connected to gender and race. Many of us, perhaps naïvely, believed that by the 21st Century, we would be past these issues. However, in academia, like so many other sectors of the economy, gender and racial problems still occur and show no signs of going away. Doctoral students, regardless of their gender or race, should be aware of these issues and should be encouraged to make efforts to minimize their impact.

Underrepresentation of Women and Minorities.

It is evident from even a casual glance at the data that women, Blacks, and Hispanics are substantially underrepresented in academia, particularly in the economics-oriented fields. In 2016, women made up just 31% of the assistant professors and 15% of the full professors in U.S. Economics Departments. In the same survey, only 8.1% of assistant professors and 4% of full professors were Black

or Hispanic, despite those groups making up about 30% of the U.S. population. The situation is even worse on finance faculties, with very few women and minorities on the faculties of most departments. Understanding why there is such underrepresentation of women and minorities is an important ongoing topic of research.[81]

The underrepresentation of women and minorities among faculty members in economics and finance has substantial negative consequences. Faculty become role models for students, especially impressionable undergraduates. Students tend to go into fields in which they can relate to the faculty. If there are faculty that "look like them", then students will be more likely to enter that field. So, to encourage young women and minority students to go into economics, it is important to have women and minority faculty as role models.

As social scientists, we produce research that is used to influence public policy; some of us even end up working in the government helping to make policy decisions. Since these policy decisions affect all of society, it is particularly important that all groups are represented. In fact, there is evidence that economic policy advice does vary systematically with the gender and race of the economist providing it.[82] Consequently, for the economics profession to help guide public policy in a way that maximizes the welfare of all citizens, it is important that the advice is from a group of scholars who reflect the population as a whole.

Hurdles Facing Scholars

I have supervised many female doctoral students' dissertations, have had a large number of female coauthors, and am in a department with much better than average female representation for a

[81] The statistics in this paragraph are from Amanda Bayer and Cecilia E. Rouse (2016), "Diversity in the Economics Profession: A New Attack on an Old Problem," *Journal of Economic Perspectives*, 30, 221-242. Bayer and Rouse also provide a nice discussion of the potential reasons why there is such underrepresentation of women and minorities in the economics profession.

[82] See Ann Mari May, Mary G McGarvey, and Robert Whaples (2014) "Are Disagreements Among Male and Female Economics Marginal at Best? A Survey of AEA Members and their Views on Economics and Economic Policy." *Contemporary Economic Policy*, 32 (1), 111-32.

finance department.[83] I have observed that unfortunately there are obstacles facing female students and faculty that simply do not exist for men. I think that it is appropriate to discuss some of them at the end of this chapter so that female doctoral students have some sense as to what awaits them as they advance in the profession.

There have been a number of studies that have documented unsettling patterns about women economists. Female economists appear to be given a harder time than male economists both by their students and by editors – in studies that try to hold quality constant, women tend to receive lower teaching evaluations and are less likely to receive favorable responses from journals. Women are tenured at lower rates than men, *even holding research records constant*. The effect continues at higher levels with women less likely to be promoted to full professors and awarded endowed positions.[84]

One particular problem that female academics face is that, even in liberated academic couples where each partner tries to share in care responsibilities, women end up doing more than half of the work involved with raising children. This fact was illustrated vividly during the 2020 Covid-19 lockdown – male economists who were stuck at home increased their output, but female economists, who ended up with extra child care responsibilities, saw their productivity decline.[85] Universities have tried to adjust their standards for time spent in child care by granting extensions of tenure clocks for childbirth. But as so often happens, universities' good intentions appear to have made things worse. Male faculty, who also receive extensions for child births, turn out to be the primary beneficiaries of this policy while female faculty are actually disadvantaged by it.[86]

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[83] I have not had comparable experiences serving on the thesis committees of any Black or Hispanic doctoral students, nor have I had many Black or Hispanic colleagues whose experiences I can share. For this reason, I will focus the discussion in this subsection on the hurdles facing female scholars, although I'm sure that much of what is said here applies to minority scholars as well.

[84] See Friederike Mengel, Jan Sauermann, and Ulf Zölitz (2017) "Gender Bias in Teaching Evaluations," IZA Institute of Labor Economics Discussion Paper; David Card, Stefano DellaVigna, Patricia Funk, and Nagore Iriberri (2018) "Are Referees and Editors in Economics Gender Neutral," Working Paper; and Mila Getmansky Sherman and Heather Tookes (2020) "Female Representation in the Academic Finance Profession," Working Paper.

[85] See Noriko Amano-Patiño, Elisa Faraglia, Chryssi Giannitsarou, and Zeina Hasna (2020) "Who is Doing New Research in the Time of COVID-19? Not the Female Economists," Working Paper.

[86] See Heather Antecol, Kelly Bedard and Jenna Stearns (2018) "Equal but Inequitable: Who Benefits from Gender-Neutral Tenure Clock Stopping Policies," *American Economic Review*, 108(9), 2420-41.

While there are undoubtedly many reasons for these obstacles faced by female scholars, part of the reason is that there is a sexist culture that permeates much of the profession. This culture is vividly illustrated in a Berkeley undergraduate thesis by Alice Wu.[87] She performs an internet "scrape" of the postings on a popular website, Economics Job Market Rumors (EJMR), to create a database of the postings. Wu examines how female economists are treated relative to male economists on this website to examine the different way women and men are viewed by members of the profession. By looking at an anonymous forum, what she observes is likely to reflect the unfiltered opinions of the posters (who hopefully are not too reflective of the broader population).

Wu finds that holding the prominence of the person fixed, women scholars tend to be discussed much more than men. But the discussion of women tends to focus on their appearance and personal life, while the discussion of the men centers around their research. I have spoken to some young scholars who have had criticisms of their appearance or their personal life discussed on this website, and it was extremely upsetting for them. But the larger concern is that the focus on appearance and personal lives reflects a lack of seriousness given to the women's work. At least in the mind of EJMR posters, the scholarship of female scholars takes a backstage to other aspects of their lives, which ought to be irrelevant to their professional success.

Reading the previous few pages must be upsetting to female scholars, especially those who are just starting out. However, it is possible to succeed despite these barriers; more and more young women scholars have been making a mark on the profession in recent years. There is an active network of female academics who make an effort to provide role models and mentorship for younger female scholars. They have set up a number of conferences where presenting authors have to be untenured women. These conferences provide opportunities for women scholars and potentially offset some of the hurdles faced by female scholars.

[87] See Alice Wu (2018) "Gendered Language on the Economics Job Rumors Forum," *American Economic Review*, 108, 175-179; and Alice Wu (2020) "Gender Bias in Rumors among Professionals: An Identity-based Interpretation," *Review of Economics and Statistics*, forthcoming.

The one attribute I have noticed of successful women academics is that they are all *tough*. What I mean is that although these successful women are generally nice, polite people, they have their own points of view and do not back down from them when challenged. Especially when they are challenged by men. I have always thought that an unfortunate aspect of many cultures is that women are sometimes taught that they should be deferential to men. If you grow up in such a culture, it is important to disabuse yourself of this idea if you wish to be a successful academic. Academia is a place where we earn our reputation by having our *own* ideas, and by standing up for them.

Discrimination Against Asians.

Unlike Blacks and Hispanics, Asians are not underrepresented in academia. There are many scholars from Asia in our doctoral programs, as well as a number of US-born Asian-Americans. Perhaps because Asians seem to be well represented, some people incorrectly believe that there is not any discrimination against Asians.

There are not as many studies documenting discrimination against Asians as there are about gender discrimination. However, many of my Asian friends in academia believe that holding quality constant, it is true that it is harder for Asians than whites to: i) get into doctoral programs, ii) receive tenure track offers, iii) publish their papers, iv) get promoted to associate and full professor. With the caveat that I have not seen formal studies documenting these patterns, I suspect that my Asian friends are correct, and it is harder for Asians to succeed in academia than it is for whites who do comparable work. For some reason, while academia is so outspoken against discrimination against other minority groups, today it is perfectly fine to hold Asians to higher standards.

I do not understand why such discrimination against Asians persists and is considered acceptable by much of the academic world. There does not appear to be any easy way of addressing the problem. Perhaps the first step is that this discrimination should be acknowledged and stigmatized. Nowadays, it is unacceptable for scholars to makes sexist or racist comments. But people are not called out when they make a joke about or promote stereotypes about Asians. Hopefully, discrimination against Asians will be

condemned to the same extent as discrimination against other groups, and will become less prevalent in the future.

Chapter 13: How to be a Diligent Thesis Advisor

The most important professional relationships doctoral students have during graduate school are usually with the members of their dissertation committee, the chair of which is known as the "Advisor." Advisors teach their students how to do research and also how to earn a living as academics. Ideally, advisors will be lifelong friends to their students and help to guide their careers for a long time after graduate school.[88] However, many students are not as fortunate as I was and do not have a supportive relationship with their advisors. Often such a bad relationship is the student's fault, if she does not work hard enough or doesn't listen to what the advisor says. Sometimes, though, the fault is with the advisor. He may have unrealistic expectations or simply not care or know enough to help students in a productive manner.

Since I talked in Chapter 12 about the way in which doctoral students should approach their relationships with their advisors, I thought it appropriate to consider the relationship from the advisor's perspective. How should faculty view their responsibilities as thesis advisor? Is the opportunity to supervise doctoral students' dissertations an optional perk of being a professor, something to be consumed by faculty who enjoy it and avoided by faculty who don't? Or is it an important element of the job of every scholar in a research university? How should a faculty member set his expectations for his doctoral students? How often should he meet with them, and how should he structure the meetings? What else can he do to help his students succeed?

Why Advise Doctoral Students?

Before I discuss *how* one advises doctoral students, I will first address the issue of *why* one would bother to do it at all. After all, the only instructional responsibility normally specified in a faculty contract

[88] After over 30 years as a faculty member, when I started to write this book, one of my first calls was to my thesis advisor from graduate school. If he had told me that writing the book was a bad idea, I would have never have completed it.

is the number of courses he must teach every year. In addition, universities are pretty clear when they do their reviews that a faculty member's research is extremely important to his standing. However, supervision of doctoral students' dissertations is somewhat amorphous. It is sometimes mentioned in a faculty member's annual review. However, most departments treat advising doctoral students as kind of an extra, sort of like being on a departmental committee, helping with a student club, or meeting with an alumni group.

Yet, advising a doctoral student represents an enormous commitment of a faculty member's time and energy, going well beyond the work involved with being on any faculty committee.[89] To do the job correctly, the advisor must have many discussions with the students about research, career planning, and many other topics. He must spend a lot of time providing the student with detailed suggestions on numerous drafts of her papers.

So why do faculty members agree to advise doctoral students? Some do not, and avoid being on thesis committees, and especially avoid being the main advisor. However, most faculty are eager to help doctoral students and are honored to be asked to serve on such committees, especially if they are the main advisor.

There are some explicit benefits from advising doctoral students. Departments put substantial resources into their doctoral programs and do appreciate it when faculty members put effort into supervising their dissertations. It is prestigious for an advisor when a student places on the faculty of a good university, especially when the student subsequently goes on to have a successful career publishing in the academic journals.

However, most of the benefits from advising students are not so explicit. Advising doctoral students can be a particularly rewarding form of teaching. Most academics choose their profession at least in part because they enjoy teaching. But teaching the same class every year can get boring. In contrast to a faculty member's regular teaching, which usually consists of going through the same material over and

[89] Possible exceptions are particularly onerous committees involving selection of a new dean or president, or ones that investigate ethical violations.

over, each doctoral student's dissertation is on a new topic. These dissertations are usually state of the art studies about issues that the advisor is interested in. Consequently, advising doctoral students can be a form of instruction that can be both more interesting and more rewarding than a faculty member's usual teaching.

Faculty can enjoy productive relationships with their doctoral students for many years after they graduate. If the student ends up working in academia, the advisor will run into her at conferences, and the two are likely to continue to do research on related topics. Many of my coauthors are former doctoral students of mine, and a number of them have become very close personal friends.

A faculty member also can learn a lot from supervising dissertations. As I discussed in earlier chapters, getting "up to speed" on a topic at a level at which one can do state of the art research is quite difficult and time consuming. For this reason, many faculty, once they get established in a narrow subfield, never end up doing any research outside that subfield for their entire careers. Being an advisor on a dissertation is one way for a faculty member to expand his horizons and to understand a subject on a much deeper level.

A number of times I have advised students on theses about subjects I was interested in, but of which I had only a cursory knowledge. After helping the students with their work on these topics, I learned enough about the related issues that I ended up coauthoring papers with them. As a result, I found myself doing research in areas in which I had not previously considered working. This model ended up leading to my work on stock repurchases, cash management, public equity offerings, securitization, and insurance.[90] I would likely not have pursued these projects had I not had the opportunity of advising these excellent students on their dissertations.

[90] See Clifford P. Stephens and Michael S. Weisbach (1998) "Actual Share Reacquisitions in Open Market Repurchase Programs," *The Journal of Finance*, Vol. 53, pp. 313-334; Heitor Almeida, Murillo Campello, and Michael S. Weisbach (2004), "The Cash Flow Sensitivity of Cash," *The Journal of Finance*, 59, pp. 1777-1804, Woojin Kim and Michael S. Weisbach (2008) "Motivations for Public Equity Offers: An International Perspective," *Journal of Financial Economics*, Vol. 87, pp. 281-307; Taylor D. Nadauld and Michael S. Weisbach (2012) "Did Securitization Affect the Cost of Corporate Debt?" *Journal of Financial Economics*, Vol. 105, pp. 332-352; and Shan Ge and Michael S. Weisbach (2020) "The Role of Financial Conditions in Portfolio Choices: The Case of Insurers," *Journal of Financial Economics*, forthcoming.

What are the Main Goals/Issues involving in Supervising Doctoral Students?

Advising a doctoral student is a form of teaching that is very different from usual classroom instruction. When teaching a class, the professor's goal normally is to convey specific material to the students. If the students demonstrate that they learn the material, they do well in the class, and if they fail to demonstrate that they have learned it, they do poorly.

In contrast, when advising a doctoral student, the goal is much broader. A successful advisor will teach his doctoral students to become scholars who are capable of doing world-class research on their own. The "deliverable" a student must have to graduate is a dissertation, and an advisor usually ends up helping the student a lot with its contents. But the real objective of doctoral student instruction is to teach the students how to think like a scholar, how to go about coming up with a creative research program, and how to execute and to present the research according to the standards of the profession. The dissertation-writing process is a hands-on introduction to research that students can replicate throughout their careers.

In addition to teaching the students how to do research, faculty must also judge the quality of the students' work. They are expected to uphold, and hopefully to raise, the departmental standards defining what makes up an acceptable dissertation. Sometimes an advisor must make the difficult decision to tell a doctoral student that her work isn't good enough. But he should strive to make these times the exception and not the rule. An advisor's goal should be to work with his students and to help them exceed the departmental standards, so that as many as possible can graduate.

One of my pet peeves is that some faculty like to think that they have higher standards than everyone else, and make it difficult for doctoral students to get through the program. The irony is that these faculty, while talking to the world about how they have such high standards for the students, often themselves produce many papers that they cannot publish. If these faculty held their own work to the same standards that they expect from their students', their careers would likely have taken a different trajectory.

Professional Ethics

Another goal of doctoral programs is to teach students about professional ethics and appropriate behavior. Ethical behavior for an academic goes beyond the self-interested incentives to establish a good reputation for oneself. The American Economic Association recognizes this idea in its recent Code of Professional Conduct (https://www.aeaweb.org/about-aea/code-of-conduct) that requires "integrity," including honesty, care, transparency, and the "disinterested" assessment of ideas.

Ethical behavior should be a part of all aspects of an academic's professional life. In research, it means that we are expected to search for the truth, and not just do whatever leads to publications in top journals if it comes at the expense of the truth. The publication process is imperfect and there are always situations in which scholars can massage their search for truth in ways that increase the likelihood of publication. Authors have been known to tilt their interpretations towards editors' favorite theories, work on topics that are fashionable rather than what is important, and selectively trim samples and pick specifications in ways that lead to statistical significance. Consistent with this last notion, papers that are published are much likely to report t-statistics slightly larger than 2 than slightly below 2, which is the opposite of what ought to be true in the absence of reporting and publication biases.[91]

Appropriate ethical behavior is also an important element of the way we behave in the non-research aspects of our jobs. Perhaps most importantly, it concerns the way we treat each other in our professional lives. As I discussed in Chapter 12, sexism and racism is too prevalent in academia. But there is more to treating people well than simply not being sexist or racist. Many academics are rude to

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[91] See Daniele Fanelli, (2012). "Negative results are disappearing from most disciplines and countries," *Scientometrics*, 90, 891-904, Campbell R. Harvey (2018) "The scientific outlook in financial economics," *The Journal of Finance*, 72(4), 1399-1440, Isaiah Andrews and Maximillian Kasy (2019) "Identification of and Correction for Publication Bias," *American Economic Review*, 109(8), 2766-2794 and Christina Blanco-Perez, and Abel Brodeur, (2020) "Publication Bias and Editoral Statement on Negative Findings," *The Economic Journal*, 130, 1226-1247.

colleagues, students, and staff, don't respond to emails, don't read students' or colleagues' papers, and don't even show up for departmental seminars and gatherings. When faculty are easy-going and pleasant to be around, everyone is happier, students learn better, and it is easier to hire and retain faculty and staff.

Learning ethical behavior goes hand in hand with learning to be a good colleague. Ethics and colleagueship are notoriously difficult to teach, as there are no formulas or set rules to follow. I believe that the key to teaching ethics and colleagueship is to set a good example for the students, and to talk to them regularly about how to go about all aspects of the job of being a professor. One thing that I am very proud of is that, to my knowledge, none of my former doctoral students have ever been involved with an ethical scandal, and the vast majority of the ones still in academia are beloved by their colleagues.

As academics, we are granted incredible benefits by society. We can teach what we are interested in, have high social status, good pay, and perhaps most importantly, indefinite tenure. It is important to teach our doctoral students not to take these things for granted. They should be made to realize that academics are not entitled to these benefits, and should work hard to pay some of this good will back. We do so by teaching well, doing research nonacademics that find interesting, and providing nonacademics with positive externalies (other than football and basketball) when they visit our campuses.

Motivating Doctoral Students

Perhaps the most important factor determining a doctoral student's success is her motivation. One might think that motivating students to do first-rate research would never be difficult since that is what they went to graduate school to learn to do. But sometimes students stop caring about their work. In some fields, students can become "professional PhD students" who stop working seriously on research and don't make progress toward graduation. They can stay on campus for seven or eight years or sometimes even longer as doctoral students, since they enjoy life on campus and the financial package they receive as a teaching assistant is sufficient to live on.

In economics and its related fields, there are usually good jobs available for students if they write high-quality dissertations and present themselves well on the job market. Nonetheless, it can still be

difficult to motivate some doctoral students. They can become depressed and unproductive if their advisor doesn't like their ideas, if they decide that academic research isn't all it is cracked up to be, or if they are procrastinators who always find reasons to postpone finishing their work. The job market does motivate students to some extent. Furthermore, departments can use both carrots (financial aid) and sticks (threats to be dropped from the program), to provide incentives for students to finish.

Relying on these incentives, however, is a second-best approach. The most successful students are always the ones who are self-motivated and don't need explicit incentives to motivate them to do research. They do research because they love it, not because of any carrots or sticks they receive from their research success. The best students are fascinated by new problems that they can tackle, and will write a good dissertation irrespective of any explicit incentives they receive.

The Effect of Strong Explicit Incentives on Doctoral Students

The job of a thesis advisor is to uphold high standards, to push students really hard, and still make it so students enjoy going to the office. Unfortunately, when faculty set the bar too high and threaten to kick students out if they don't meet it, there can be a number of negative consequences for the program. If students are afraid of flunking out of the program, they do put in more hours. They also become extremely nervous and try to tailor their research to whatever they think the faculty will be most willing to approve (which might be different from what faculty will actually approve).

One of the most important things that often is lost when a student faces explicit pressure is the student's creativity. For a research paper to be influential, it has to address a new question or use a novel approach to address an existing one. The ability to think of these new questions and approaches is where creativity plays a key role.[92] Pressure, especially when combined with negative feedback on ideas that are a little "different," can lead to research that is sterile and not particularly innovative.

[92] Identifying the students who have the "creative gene" and will end up loving research is actually quite difficult. Admissions committees to doctoral programs struggle to identify which of the potential applicants will do best, because it is very hard to observe applicants' creativity and curiosity, which are the most important ingredients of successful scholars.

Earlier in my career, when I was on the faculty of a university that has faculty known for being tough on their students, I became a good friend of one particular student who was struggling to find a thesis topic. Every few months, he would come to me with a new idea. I almost always liked his ideas and encouraged him to write his dissertation about one of them. Unfortunately, some of my colleagues were never enthusiastic about his ideas, and my friend always ignored my advice and did not pursue the ideas he told me about. He was worried about being able to complete his PhD if he worked on an idea that some faculty were not excited about. In the end, he asked a very senior professor to work with him on a fairly boring but unobjectionable topic.

Several of my friend's ideas eventually did get published in top journals. Unfortunately, the publications had other people's names on them! What happened was that scholars at other universities had ideas similar to my friend's. However, unlike my friend, these scholars pursued the ideas and published the results. My friend did graduate but never did publish his dissertation. Despite such an inauspicious start, he nonetheless has had a very successful career. But it would have been even more successful if he had a bit more backbone when he was a graduate student. In this student's case, the pressure to satisfy the idiosyncratic tastes of his professors led him to a far less interesting dissertation than he was capable of.

Another problem with programs in which students are constantly worried about being dropped is that the students become much less likely to do joint work and tend not to help one another with their dissertations. Students can feel that if they help their classmates do better, it makes them look worse in the eyes of the faculty. These students think that if they help their classmates, it could end up hurting their own chances to graduate. Consequently, they don't coauthor with one another and support one another with their research. Strong explicit incentives, especially those coming from the possibility of not graduating, can lead to an overly competitive environment, leaving all of the students worse off.

Fostering Self-Motivation

The best dissertations do not come from students being worried about getting kicked out of their programs. They emerge when students come up with creative ways to address questions to which the students would like to know the answer. These students spend a lot of time thinking about the state of the literature, what questions have yet to be addressed, and innovative ways of answering them. The motivation for this work is that the students are fascinated by the questions and want to challenge themselves to provide a satisfactory answer. The best students work hard not because of any incentives provided by the departments, but because they love research and want to be the best scholars that they can be.

The extent to which doctoral students are self-motivated, however, can vary tremendously depending on the environment the students are in. Almost all doctoral students are smart and are enthusiastic when they begin their programs. Occasionally a student is so talented and motivated that she will succeed in producing good work regardless of her surroundings. But for most students, the drive to produce first-rate research comes from seeing others produce it and wanting to become part of the exciting scholarly environment around them. The environment students are exposed to will determine how motivated they are and, consequently, the quality of their dissertations.

Faculty should therefore think about the environment students are exposed to and ways in which they can make it as stimulating as possible. Each faculty member remembers what motivated them when they were doctoral students, and naturally tries to emulate the good parts of their student experience and avoid the bad parts. As I discussed in Chapter 1, I was very fortunate to be a student at MIT in the 1980s and also to have a desk at the NBER, where I got to know a number of the Harvard students and faculty as well. The incredible research environment surrounding me when I was a student was a huge factor in whatever success I have had since then. When I have worked with students throughout my career, I have tried to make the environment my students face as much like Cambridge, Massachusetts, as possible.

Expectations of Students

In my first week as a doctoral student, I attended my first seminar, where Dale Jorgenson presented an applied econometrics paper. After the seminar, Peter Diamond, who years later would be awarded the 2010 Nobel Prize in economics, introduced himself to me and asked me what I thought of the talk. The only honest answer I could have given would have been: "I didn't understand a word of it." However, being a nervous new student, I didn't want to admit not understanding the paper, so I told him that I thought the talk was very interesting.

Of course, Peter knew that I was a first-year student who had not yet taken econometrics, so he probably surmised that I didn't understand the paper. But he wanted me to know that as a doctoral student, I was expected to be an active participant in the intellectual life of the department. Whenever I attended seminars as a doctoral student, I was supposed to read the papers beforehand, to think about the papers that were presented, and to have thoughtful opinions about them. The minute I stepped onto campus, I was treated as somewhat of a peer. That being said, I still had to pass my classes and exams. I worked extremely hard to pass the exams, even though hardly anyone actually failed them and was asked to leave the program. My motivation for working so hard was that I was surrounded by many brilliant people, and I wanted to fit in and earn their respect.

I was highly influenced by the quality of the work of the other students in the program. Everyone in our program was one of the best students in his or her university before coming to MIT. The very best students in this highly select group were just incredible. A number of the papers we read in our macroeconomics course were by Greg Mankiw, who at the time was still a doctoral student in our program. Andrei Shleifer was a year ahead of me, and before he left graduate school, he had multiple papers appear in *Journal of Political Economy* and a number of papers in other top journals as well. Two of his *JPE* papers were even the lead article.[93] For most of us "normal" students, who for our entire lives

[93] The majority of tenured faculty at top twenty departments go their entire careers without having a lead article in one of the top general-interest economics journals such as *JPE*. Andrei's lead articles from his graduate school days were: B. Douglas Bernheim, Andrei Shleifer, and Lawrence H. Summers, "The Strategic Bequest Motive," *Journal of Political Economy*, (1985) Vol. 93, pp. 1045-1076, and Andrei Shleifer and Robert W. Vishny, (1986) "Large Shareholders and Corporate Control," *Journal of Political Economy*, Vol. 94, pp. 461-488.

were the top student in whatever program we were in, it was quite a revelation. We came to understand that to be one of the better students at MIT, we had to become one of the top scholars in our field. And we had role models who showed us how to become exactly that.

I let my own doctoral students know that my expectations for them are very high. I expect them to do more than just show up for seminars – I expect them to form opinions on the work, participate in the discussion during the seminars, and be part of the culture that makes life exciting for the other students. I strongly encourage my students to read each others' work and to help them make it better. Whenever possible, I have older students and former students explain their work to the younger students, so that the younger students see that it is possible for people just like them to produce excellent scholarship. The goal is to convey to students that faculty expect their work to be world-class, but also that it is possible for the students to produce such world-class work. Bright students who observe that other students are meeting these high expectations will work harder, and strive to fulfill them themselves.

Ideas Matter – Rank Doesn't

Perhaps the most important thing a professor can teach a doctoral student is that research should stand on its own, and the student should judge the quality of a paper independently of its authorship. Regardless of whether a paper is written by a Nobel Prize winner or another doctoral student, the paper's analysis should receive the same scrutiny.

Many students spend their entire lives thinking that their job as students is to repeat everything their professors say, and to be deferential to their professors' ideas. The deferential approach to learning is perhaps a good way to understand material that is cut and dried like mathematics. However, it is not a productive way to understand state of the art research in a subjective area like economics, where it is acceptable for different scholars to have different worldviews. An important part of the job of a thesis advisor is to disabuse students of the reverence for faculty and for published work that they have been previously taught.

I always thought one of the great things about the NBER was that people sat around the table and argued about economic ideas. It didn't matter whether it was at lunch, late at night, or in the conference room after a seminar, there was always discussion about economics. The opinions of the more famous senior people did get more weight in the discussion, but only because their opinions were more credible than those of the PhD students. At the end of the day, what mattered was not the identity of the person saying something, but whether what was said was correct.

At the NBER, doctoral students would regularly discuss recent research and often came to the conclusion that the work was wrong or misguided. When the students reached this conclusion, it did not matter if the work was done by one of their professors or friends. The students were not taught to worship at the feet of their professors. Rather, they were encouraged to challenge the professor's ideas.

Great research often consists of challenging the prevailing worldview. Doctoral students must learn that the research of the most famous scholars of the day, even if it is by their own professors, is not always correct. If students wish to write influential dissertations, they have to get used to the idea of challenging the prevailing viewpoint early in the program, even if their professors are the ones who created it. Most of the time, when students do so, they will not be correct. But once in a while, there will be cracks in the arguments of the leading scholars that will lead to fruitful new research.

The Value of Informality – Professors are Human Too

I always felt that the informal mixing of students and faculty at the NBER helped create the atmosphere in which students were willing to challenge faculty's work. Faculty and students would go to lunch together, play tennis with each other, and socialize in various ways. Students would always call faculty by their first names. Students would realize that professors, even the most famous ones who taught at Harvard and MIT, were just people like they were. Professors had good days and bad days; sometimes they wrote excellent papers, and other times they wrote awful ones.

Accepting professors as individuals who make mistakes leads students to go the next step and recognize that their papers are not perfect. If the papers of Harvard and MIT professors aren't perfect,

then the ones from faculty of other universities aren't either. Doctoral students' natural inclinations should be to look skeptically at published papers. They need to learn that if they look hard enough at established research, they might find flaws in it. The students can then figure out how to fix these flaws, and eventually make a meaningful contribution to the literature.

An advantage of an informal atmosphere is that students become much more comfortable talking with faculty, asking questions, and bouncing ideas off of them. Students' creativity does not come from their being scared. It comes from their recognizing that there is much that the literature does not know and thinking of new ways to advance knowledge.

When I work with doctoral students, I always strive to create an atmosphere as similar to the NBER as possible. I spend time talking with them about non-research related topics, hoping to make them comfortable in the office and to think of me as a normal human being. I try to go to lunch with the students I work with regularly, usually every week or two. At these lunches, we typically don't talk much about research, but instead enjoy ourselves with good food and conversation. When the students get back to the office, they hopefully will be a bit happier and realize that the faculty are there to help them rather than to scare them. Ultimately, I believe that an atmosphere where students can feel comfortable and respected helps the students to become excellent scholars.

Students Should Work Together

Another objective of graduate school is for students to begin to work with one another and to form relationships that can hopefully last throughout their careers. Advisors should realize the importance of these relationships to the students and do what they can to encourage students to develop them. Of course, whether students develop relationships with one another depends mostly on the students -- if they like one another, they will become friends and if they don't like one another, they won't. But there are things an advisor can do to create a positive atmosphere in which students' relationships with one another can thrive.

Perhaps the most important thing an advisor can do is to base recommendations on students' absolute performance and not their performance relative to one another. Any rankings in letters of recommendation should be about the quality of the students' work relative to historical standards and to the overall market for new economists. They should *not* be about students' standing relative to their classmates. If students think that they will be hurt if another student in their year does well, they will tend to think of each other as rivals rather than friends, and won't help each other with their work. Academia *is* very competitive, but the real competition is from the outside market and the historical standards that universities have applied when evaluating their faculty. If multiple students stand out on the national market, then they should all be able to do well on the job market. If a student is the top student from a weak class, then being the top student is unlikely to help her very much.

I try to do whatever I can to encourage students to work together. I have started research projects with several students as coauthors, which gets the students thinking of each other as teammates. Research, even on sole-authored dissertations, is very much a team effort and if students view it that way, they will do better in the long run. When I am working with several doctoral students at the same time, I like to have "research group" meetings, in which students take turns presenting their work to one another. When they work well, I try to say as little as possible and to let the students make suggestions for each other's work. Ideally, these sessions tend to encourage the students to work together and can even lead to coauthored work between them.

Know and Understand the Students

Finally, advisors should make sure that they know their students reasonably well. It might seem obvious that advisors ought to know their students, but so many do not. Many are not aware of the basic facts of their student's lives. Where are they from (in more detail than just that they are from "China" or "Russia")? Do they have siblings? Do they get along with their parents? Are they in a relationship or did

one just end? Do they have other interesting skills/hobbies?[94] Are there any health issues that they must deal with? Students really appreciate when their advisors make an effort to try to get to know them better. And any additional information that advisors have about students can help them do a better job supervising students' dissertations and providing guidance on the job market.

Equally important to knowing students personally is understanding their abilities. Some students are great at formal modeling, some have excellent institutional knowledge and others are exceptional at handling databases. I always try to steer students into topics that show off the skills that the student is best at. What matters in the end is the contribution to our knowledge from the student's dissertation. It is a lot easier for a student to make a contribution to the literature if what she attempts to accomplish takes advantage of her skill set.

Understanding students' lives and their skills allows advisors to do better jobs of both setting expectations and helping students achieve them. Some students need to be helped through the dissertation one step at a time. These students should meet with their advisors regularly, showing them one test after another, and rewriting each section of a paper many times before going onto the next one. Other students are more independent, and can be left alone for a few months to write a draft by themselves.

A good advisor will try to set expectations differently for each student based on their abilities. Faculty should realize that advice is not "one size fits all". What is reasonable for faculty to ask a top student to do might overwhelm an average student. The goal is to tailor each student's workload to their ability, so that each student who enters the program can eventually leave with a high-quality dissertation.

[94] I have one former student who was a champion figure skater and another who was an All-American rugby player before entering graduate school. Graduate students are often very interesting people, but many faculty never find out.

Chapter 14: Managing an Academic Career

When I was deciding what to do after graduating college, one of the things that led me toward pursuing a PhD in economics was the number of potential employment options that were available after I graduated. I thought that I might enjoy being an academic, but wasn't sure if I would be able to get a good academic job. After all, when my father received his PhD, there weren't many academic jobs available so he took a nonacademic offer. Nonetheless, he went on to a very interesting career in the private sector. One of the appealing aspects of studying economics was that if academics didn't work out, I might be able to follow a similar path and have an equally rewarding career.

The truth is that, even though my goal was to be an academic, I had no idea which way my career would proceed at the time I entered graduate school,. At the time, I did not know if I would flunk out of the doctoral program, decide I don't like economics, or find a private sector job that would be more interesting and lucrative than one in academia.

Throughout my career, like everyone else, I had to find my way through my professional career amidst considerable uncertainty. This uncertainty was multi-dimensional – it concerned my own interests and abilities, the demand for economists both inside and outside academia, the way in which the field will develop, and many other things. It was the kind of uncertainty that John Kay and Mervyn King refer to as "Radical Uncertainty", meaning that its sources are sufficiently unclear that it is impossible to model or hedge.[95] How should one think about managing one's career in the presence of all of such radical uncertainty? What can one do to prepare for the inevitable setbacks one will encounter? How can one best make smart career decisions in an uncertain world?

General Principles

[95] See John Kay and Mervyn King (2020) *Radical Uncertainty: Decision-Making Beyond the Numbers*, Norton.

I think that management of an academic career, or really any career, comes down to two principles. The first principle is that, regardless of the stage of her career, a scholar should always spend time thinking about what realistic options are for new directions that her career could take. When first starting out, these options might include different possible fields to study and different types of employers that she could work for. Later on, the options could be other schools/firms that might hire her away from her current job. Finally, when she is established inside an organization, these opportunities are likely to consist of other roles one could take on, and other outside opportunities that could be available.

The second principle is that once these opportunities are identified, one should make an effort to acquire the human capital necessary to pursue them. By human capital, I mean whatever resume, skills, and connections will enable one to move her career in this direction. In other words, when one sees a potential opportunity, one should actively pursue it by acquiring whatever capital is necessary. This point probably seems obvious to many readers, but is often ignored by people who expect that opportunities to appear magically without any effort on their part.

In this chapter, I will explain how these principles can be used to suggest active career-management strategies a scholar could think about at each stage of her career. I will proceed sequentially, starting with the issues involved with career planning affecting a graduate student, then a junior faculty member, and finally a senior faculty member. I explain how one can apply these two general principals to make better decisions at each stage of an academic's career.

The overriding theme of this chapter is embodied in the saying (which we have on the wall of our family's summer home in Northern Michigan): "You are the CEO of your own life!" Everyone has to take responsibility for their own career and make sure it is interesting and fulfilling. If something goes wrong and she doesn't enjoy her job, it is her responsibility to take charge, to address the problems, and to try very hard not to blame others for them. Too many people are unhappy because they spend their lives complaining about their jobs and not doing what they can to improve their situations.

Keeping Options Open as a Graduate Student

I have talked throughout this book about the advantages of specialization and the development of a comparative advantage. Such specialization is necessary to make a meaningful contribution to a literature. But sometimes students get so engrossed in their own literature that they forget about everything else. In addition to developing a specialty, graduate school is a time when students should develop a knowledge of the major subfields of their area of study, and most importantly, acquire skills that can be applied to different kinds of problems.

A path that graduate students commonly follow is to "fall into" a topic, that sometimes but not always is related to research a faculty member of theirs is interested in. Perhaps the student is a research assistant for the professor or takes a class from her, and she ends up working on a related topic. Or the student could develop an interest in the topic on her own without help from the faculty. Once a student has a topic that fascinates her, she can end up immersing herself into it throughout her time in the program.

It is wonderful when a student can come up with a good thesis topic and spends most of her time thinking about it. However, it can be a problem if the thesis is all the student ends up knowing anything about after she finishes graduate school. If the research works out well, then the thesis could help the student get an academic job and a start on a productive research program. But sometimes things do not go as planned. The student can fail to get an academic job, or the research can hit a dead end, either before or after publication of the dissertation. Eventually, regardless of their initial success, most research programs reach the point of diminishing returns, with subsequent papers becoming less interesting and impactful over time. To be productive throughout her career, a scholar must be able to address new issues in addition to the ones she started working on initially. These new issues can require a different set of skills than required for her dissertation. Graduate school is the time to gain a broad set of skills to be able to address new topics throughout her career.

In addition, such a broad set of skills can, in itself, make a doctoral student much more attractive to employers. Private sector employers, and sometimes government sectors ones as well, often don't care at all about a student's dissertation and whether it will be published. Instead, they want to hire people who

can understand problems and apply the appropriate tools to solve them. In addition, private and government sector employers place high weights on the ability to write well and to give clear presentations. If a doctoral student makes an effort to become a good writer and public speaker, these skills will benefit her regardless of who is her future employer.

The ability to handle large databases well and an understanding of modern methods of statistical analysis has become incredibly important for most social scientists in the economics-based fields. One of the things that has changed over the 30 or so years I have been an academic has been the profession's increased focus on empirical work. The information economy has vastly increased both the availability of data and the computing power to analyze them. The standards for empirical work have increased dramatically over the past 30 years, and statistical approaches such as Bayesian MCMC analysis and machine learning algorithms are now regularly used in empirical papers by not particularly sophisticated non-econometricians (like me). There is every reason to think that the trend will continue. In my opinion, anyone getting a PhD in the social sciences today would be crazy not to do their best to get up to date on these and other empirical methods. If she does not, she will find herself hopelessly out of date in a few years when even more sophisticated techniques become commonplace.

One might think that it is obvious that students should learn empirical skills regardless of their specialty, especially if their future as an academic is uncertain. But many students don't. I often meet recent graduates who tell me that they are theorists so don't need to know how to deal with data. Or that they generate their own data running economic experiments, so don't have to understand the problems inherent in working with data generated in the real world.

I still am amused when I think of a sign posted on the bulletin board when I was a graduate student by a now world-renowned economist after he passed his second-year exams. It said: "For Sale, a copy of Theil. Only opened twice. A must for every serious economist, I no longer need mine!" [Theil was the most popular econometrics textbook at the time.] This scholar has had a tremendously successful research career since then that, not surprisingly, did not involve empirical work. But if he had had bad

luck publishing his work and wanted a private sector job, he might have had more trouble than he would if he had taken empirical work more seriously.

When planning their careers, graduate students should spend most of their time focusing on the dissertation they have to write, but they should be at the same time acquiring the skills that will help them pursue future opportunities. These opportunities consist of potential research programs on topics the students haven't yet thought about, and also job opportunities both inside and outside of academia that the skills could potentially help the student acquire. Students should remember that most scholars' research tends to become more applied over their careers. It is common for scholars to do more theoretical work soon after leaving graduate school and more empirical and policy-oriented work later on. Consequently, it is particularly important to acquire empirical skills regardless of the field one wants to pursue in her disseratation.

The Rookie Job Market

Perhaps the most talked about and time-consuming event on the economics calendar is the job market for newly-minted PhDs, often called "rookies". Every year at the annual "Allied Social Sciences Association" meeting, doctoral students and recent graduates interview with universities, firms, and government agencies for jobs. After these interviews, the students have fly outs in January and February. Offers are made throughout this period, usually in a very strategic manner, and the market clears by the middle of March.

Most departments do a reasonably good job of preparing their students for the job market (but not necessarily for life as a junior faculty member after the market is over). A number of faculty have written documents about the job market and how students should navigate it. Some have been kind enough to post these documents online so students outside their own department can benefit from them.[96] I will not

[96] See Alexander Butler and Timothy Crack (2019), "A Rookie's Guide to the Academic Job Market in Finance: The Labor Market for Lemons," (https://ssrn.com/abstract=3433785); John Cawley (2018) "A Guide (and Advice) for Economists on the US Junior Academic Job Market," (http://www.aeaweb.org/content/file?id=869); Robert Hall and Monika Piazzesi (2001). "Job Market Advice for Stanford Economics

describe the details of the market and the standard advice that is given to most students since readers can find this information in these publicly available documents. However, I will discuss a few points that I think are particularly important and which I always emphasize to our students when they go on the market.

The Importance of Advisors. During the job market, it is particularly important for a student to have a good relationship with her advisor, in which the student feels that she can be open with him about important issues in her life. A student's advisor should be both the one who guides her through the job market process, and also her primary cheerleader who helps sell her work to the profession. Some students are surprisingly not open to their advisors, which can make it hard for advisors to help their students. There are some issues that the student might not want to tell the market but still should be honest with her advisor about. For example, a student might want to keep the identity and locational preferences of her partner secret from schools considering hiring her. Or she may have a medical condition that could potentially affect her work. Or she may not wish to have her sexual preferences become publicly known. If the advisor is aware of all the facts, he can help the student come up with an appropriate strategy for the issue and the way to address it (or not to address it) with potential employers.

Advisors can sometimes can convey useful information credibly to the market. For example, many schools in the U.S. are located in small towns. For the majority of doctoral students, small towns are a minus, since most students prefer living in larger cities. Therefore, it can be hard for schools in small towns to hire faculty. Students are told by their advisors to express interest in all jobs until they have an offer. However, many students are likely to decline a potential job offer in a small town if opportunities in larger cities become available. Some students, however, are genuinely interested in living in a small town, perhaps because they have small children, and value the low house prices and ease of life in a small town.

Students," https://web.stanford.edu/~niederle/Advice_Bob_Hall.pdf; David Laibson, "Tips for Job Market," https://economics.harvard.edu/files/economics/files/jobmarketadvicehandout.doc.pdf; Peter Iliev (2016). "Finance Job Market Advice," *Available at SSRN 2779200*; and Eric Zwick, "How I Learned to Stop Worrying and Love the Job Market," http://www.ericzwick.com/public_goods/love_the_market.pdf.

For these students, it can help the student's case if the advisor lets schools in small towns know that these students would really find their location to be appealing. An advisor can sometimes communicate information to schools that are considering hiring a student in a much more credible manner than if the student tries to communicate the same information herself.

Deadlines Matter. In the job market, there are a number of deadlines that students face. A particularly important one is the date by which applications must be uploaded. In most U.S. departments, faculty start reviewing files the week when they return from the Thanksgiving holiday in early December. Therefore, students' applications must be complete by this point in time. I have attended a number of faculty meetings over the years where we evaluated candidates from other schools to decide who we would interview. Usually we defer considering applications that are not complete until a second meeting, which is usually planned for the following week. Unfortunately for the applicants, we often end up filling our interview schedule with the applications that were complete at the time of the original meeting, so the second meeting never happens. Students whose applications are not complete when we have our original meeting always end up having much lower chances of getting an interview.

A complete application involves letters of recommendation from faculty, so late letters from faculty can be very harmful to the students. For this reason, our department (and many others) has a staff member who is responsible for bugging the faculty to complete their letters of recommendation, and uploading them with the students' applications. Students have to realize that the staff are human and that it takes time to upload the letters. Therefore, the students have to get their portion of the applications finished in sufficient time so that the staff can upload the letters on time, and the application becomes complete before the Thanksgiving holiday.

One time, when I was teaching at a different university, there was a student who was spending a year in his home country working at the central bank. He had an excellent job market paper and a number of schools would certainly have been interested in interviewing him. But it was mid-December before he decided to go on the market and asked me (and the other faculty on his committee) to write him letters of

recommendation. As a result, he only got one or two interviews and did not receive any fly out invitations. This student ended up staying in his job in his country's Central Bank. I always thought this outcome was a sad one. The student ended up with a good job, but most likely would have had a number of other options if he had gotten his applications in a few weeks earlier.

Cast a Wide Net. Students often go into the job market with preconceived ideas of the job they will end up taking. Perhaps it is that they want to live in one part of the country, or only want to consider academic opportunities. For example, I once had a student who only wanted to consider jobs in Chicago. I pointed out that there weren't so many universities in Chicago and that two of the ones that are located there, University of Chicago and Northwestern University, were top-ranked departments that were unlikely to be interested in hiring her. I eventually convinced her to apply to universities in other large cities and fortunately she was able to find a job at one of them.

I always strongly encourage students to submit as many applications as possible, for both academic and non-academic opportunities in the U.S. and also in other countries. Students have to understand that things on the market often don't go as expected. Places that students think will be interested in them often are not. But sometimes students end up with jobs at their dream school, even if that job seemed like a remote possibility at the beginning of the job market process. In addition, people can end up in parts of the world they never really thought about, but turn out to enjoy living there. At Ohio State, our finance department has a number of European faculty who never envisaged living in Columbus, Ohio, but now love living here. I spent most of my life in the Northeast and Midwest of the U.S. and never thought much about living in the western part of the country. But when the University of Arizona offered me a job in 1994, I ended up spending five wonderful years in Tucson.

An increasing number of students have been taking "post-docs" prior to starting on tenure track positions. Some of them, such as the ones offered by the NBER, are designated as post-docs, while other students do a post-doc by taking "visiting faculty" positions and going on the market a few years later. These visiting faculty positions sometimes involve a fair amount of teaching, but they can be at fairly

highly ranked department, and provide recent graduates a way to make new connections and make progress on their research. Post-docs can be a smart career move since they allow new PhDs to make progress on their research before starting their tenures. If a new PhD is able to publish some work during her time as post-doc, she can potentially end up with a much better job than she would have gotten directly from graduate school.

Don't Ignore Non-Academic Opportunities. Most students who enter PhD programs plan on teaching at a university. But there are many excellent opportunities in government agencies, think tanks, consulting firms, financial firms, and industry. I personally have not worked in any nonacademic organizations, so cannot give much advice on how to manage careers for PhDs who take nonacademic routes. I have observed, however, that most of the people who got PhDs and did not go into academia have always seemed very happy with their decision. I always strongly encourage students going on the job market to apply to as many nonacademic positions as they can, and to consider them seriously if they become realistic options.

An important consideration in any decision about whether to accept a job offer is the extent to which one can move from the job in the future. It is often easier to move from academia to industry than from industry back to academia. Unless one has a position in which one can publish articles in a nonacademic job, which can be possible for example, at government jobs, it can be extremely difficult to move to a tenure track position in academia. Universities tend to prefer freshly-minted graduates to people who have been out for a few years.

In some fields, however, it is possible to move from the private sector to academia. My brother, David Weisbach, is a tax lawyer who started his career at a prestigious tax-oriented law firm. At nights, while doing the demanding job of a young associate, David was able to write an excellent paper, and take it on the law school market. After a short stint at Georgetown, he has taught at Chicago Law School for many years and has become one of the leading tax law professors in the country.

In the End, it is Your Decision. An advisor's job is to help students get as many opportunities as possible. Students should be sure to have many frank discussions with their advisors about career prospects and to seek their opinions about which are the best opportunities. In addition to the academic ranking of the department, a new PhD should consider the location of the job and the quality of the personal interactions with the people she met during the hiring process in her decision about which job to accept. The organization's culture and the extent to which it is supportive of new hires is particularly important and should also be an important factor in her decision.

But when making the final decision, students should always remember the saying: "You are the CEO of your own life." Students must figure out what the right job for them is, regardless of what others advise her to do.

Life as an Assistant Professor

Assistant Professors who worked extremely hard as doctoral students are often surprised to discover that their workload only increases when they become faculty members. Their research expectations remain the same or increase. But in addition, assistant professors also have to teach classes, go to faculty meetings, referee papers, help doctoral students with their research, be involved with faculty recruiting, and take on all the other responsibilities of a faculty member. These increased responsibilities coincide with the time that they move into a new house or apartment, find their way around a new city, and if they have a family, help their family members adjust to the new environment.

With so much to do, time passes quickly and assistant professors begin to feel pressure. This pressure is especially high if their job market paper gets rejected by a journal or two, or their teaching evaluations are below average. The new responsibilities, together with high research expectations, can be overwhelming. What should young faculty do to maintain progress on their research in the face of so many responsibilities? What priorities should they set when they go about doing their job? How and when should they think about pursuing outside opportunities?

Young faculty realize pretty quickly that the most precious resource they have is their time. All aspects of research always end up taking much more time than planned, affecting the time necessary to complete drafts of new papers and to publish old ones. Teaching is also time-consuming as faculty have to prepare for class, answer questions in their offices, make up tests, and assign grades. Before they realize it, their first year as a faculty member can seem to disappear soon after they arrive.

Be Organized and Proceed with a Plan. When one starts a job as an assistant professor, one should remember the basic principles outlined at the beginning of this chapter. First, she should think about all of her options for the future; and two, one should acquire the capital necessary to pursue these options. Of course, when a newly-minted PhD joins a faculty of a university, her number one option is to receive tenure at the university which hired her. But she should realize that not everyone gets tenure at their first job, and the majority of successful academics move at least once or twice in their careers. Therefore, a second option a new assistant professor should keep in mind when she joins a university is to move to a different university. And a third option, the feasibility of which varies across fields, is to move out of academia to the private or government sectors. In finance, there are usually firms looking to hire recent PhDs, especially people who specialize in capital markets research,. However, in other areas of economics, such as pure theory, the private sector market is more limited.

Keeping these options in mind, young faculty should have a plan to acquire the necessary capital to make them realistic possibilities. If the capital necessary were substantially different for each of these options, faculty would face a difficult choice over which kind of capital to acquire. Fortunately, the factors that help get junior faculty get promoted internally and have outside options are largely the same. The most important factor is the impact of a scholar's research. Teaching also matters a lot. In addition, a scholar has to demonstrate that she can be a good colleague who is likely to provide high quality service in the future.

I have already discussed the research process in detail in earlier chapters. But one thing I haven't focused on is the time element, and the way it can affect the way a faculty member goes about her research. Some faculty are such perfectionists that they never actually finish anything. However, there

does comes a point at which a scholar must reach a stopping point on each paper and send it off to a journal. A good approach is to set deadlines for oneself during each part of the research process. I set them for myself for most things that I do, and try really hard to finish tasks by the deadline, if not sooner. I don't always meet those deadlines, but simply having one can be a useful motivator.

Something like the following would be an admittedly optimistic set of deadlines for a new faculty member. A typical dissertation contains three essays. The first is normally the job market paper and should be very polished by the time they graduate. She should try to submit this paper to a journal even before she physically moves to the location of her new job, certainly before the school year begins for her at that job. Hopefully the second essay is in decent shape and won't require that much work prior to submission – she should try to have that one submitted sometime in the fall of her first year out (assuming her fall teaching isn't too onerous).

The third essay is often a bit more preliminary at the time of graduation, so it might take a bit more time before that paper is ready for submission. Nonetheless, in addition to the first two essays being submitted, the scholar should try to have the third paper in shape by the end of her first year as a faculty member. In addition, she should push to have a new paper written in her first year. Often it works well if a new faculty can establish coauthoring relationships early during her time tenure at a new place. It is great if she can produce a paper with her new colleagues fairly early in her time at the new university, since a coauthoring relationship with a new colleague can lead to a productive partnership that can last a long time.

This schedule is intentionally somewhat aggressive. Most junior faculty, even the ones who eventually receive tenure, do not get their papers to journals as fast as I suggest. This schedule should represent a goal that a new faculty member strives to achieve. However, when pushing herself to produce papers and submit them, she always has to be mindful not to submit the paper before it is ready, using the criteria for submission I laid out in Chapter 11. And before reaching that point, she should put the paper through the revision process I outlined in Chapter 10.

Getting the papers written as a student into shape and off to journals so quickly is a lot of work. But it is essential to start one's career well, and to work hard to publish one's dissertation as soon as possible. Early publications will enhance a scholar's ability to move on the seasoned market if she chooses to go that route. In addition, submitting the papers written in graduate school shortly after starting a first job frees up time and energy, making it easier to focus on new research by the time she reaches her second or third year on the faculty. A good start on the publishing front can be an incredible boon to one's career.

In addition to research, the other main component of promotion decisions is teaching. And no matter what people say, teaching really does matter in these decisions. When a university grants tenure to someone, they are giving her a lifetime contract. If the person's teaching is problematic, then the university will be extremely reluctant to grant such a lifetime contract. It is worth making an effort to improve one's teaching prior to coming up for tenure, and to take advantage of "Teaching and Learning" centers that are available on most campuses. Improved teaching can improve a faculty member's standing with her colleagues, and also her job satisfication, because it is much more fun to teach well than to teach poorly.

The Seasoned Market for Junior Faculty.

Young faculty often ask me how seasoned market works.[97] While the rookie market is fairly organized and there is a wealth of fairly standard advice that is publicly available, the seasoned market is more of a mystery. Young faculty often hear gossip about other people getting outside, and wonder how they can get this kind of attention themselves. How does this market work? What should people do to be considered for faculty positions by other universities? Should they contact schools themselves, and if so,

[97] The term "seasoned market" is somewhat of a misnomer, since it is the candidates, not the market, who are seasoned. The market itself is not particularly seasoned, and as I discuss below, is much less of an organized market than the "rookie market".

how and when should they do so? Is the human capital necessary for a scholar to move to another university different from what she needs to be promoted internally?

In contrast to the rookie market, the seasoned market works mostly on personal relationships. For example, if there is a new graduate from a top-ranked department with an interesting paper in an area in which our department is focusing our efforts on the rookie market, the odds are pretty high that we will find out about the person and consider interviewing her. But usually we won't consider someone on the seasoned market unless one of our faculty brings up the name. Most of the time, the faculty member who brings up the name knows the potential hire personally, or if they don't, they were given the name by a third party whom they trust. Occasionally, when our department wants to hire in an area in which we are short, we will spend some time going through the websites of other departments trying to come up with names of people who might be a good fit. In addition, like most departments we receive a number of unsolicited applications from faculty at other schools, and we do sometimes contact these candidates for an interview or seminar.

I think of the seasoned junior market as almost two separate markets. Some people move to new schools a year or two out of graduate school, and are given full tenure clocks. This type of hire is sometimes referred to as a "rookie substitute." Departments consider hiring a person a year or two out of school similarly to hiring a rookie – they get paid the same and take the same slot that a rookie would take. Departments can consider candidates as rookie substitutes at different points in the year – a particularly active time for this segment of the market is right after the rookie market closes, when schools whose rookie offers are turned down look for alternatives. From the school's perspective, scholars a few years out of graduate school can be good hires since they usually have had a good start publishing (otherwise they wouldn't be looked at), and they have a track record of teaching and colleagueship.

When a scholar has been out of graduate school for more than two or three years, universities considering hiring her are often prohibited from giving her a full tenure clock.[98] In that case, any offer the

[98] The rules saying who can and who cannot be offered a full clock vary across universities and even within universities, where they are not always applied consistently over time and across departments.

school makes to her would be of a different category, often called a "senior junior" offer. To receive this type of offer, the person should already have established herself to some extent. I have always felt that by making such an offer, the university is telling the person that her record is essentially on track to receiving tenure at that school. Presumably, if a person hired with a senior junior offer continues to produce research at the same pace, and is a good teacher and colleague in her new department, she should have a tenurable record when she does come up.

I believe that most of the profession shares my view about senior junior offers, in that when making these offers, the school is making an implicit promise about her future tenure decision. Consequently, the bar for getting hired with a shorter clock is higher than for rookies or rookie substitutes since schools only wish to hire senior juniors that have a strong enough record to make tenure with high probability. Empirically, it is true that scholars who are hired midway through their time as a junior faculty do have a higher tenure rates than rookies hired straight out of school, although there are some that get turned down.

Some junior faculty do get tenured offers from other schools before they are tenured at their own school. This practice is especially prevalent at the very top departments, which are always trying to lure away star junior faculty from one another. However, at most departments it is more common to make offers to people in which they start as junior faculty and come up for tenure in a year or two. Deans are generally more comfortable with this type of offer than a senior offer. An untenured offer in which the person goes up for tenure in a year or two gives the school the right to deny tenure if the person turns out to be a horrible colleague or teacher (this rarely happens). The bar for getting an offer prior to tenure is normally lower than for tenured offers, so scholars who are willing to move without tenure are likely to have more options than scholars who insist on having tenure the moment they arrive on campus.

Managing the Seasoned Market. Moving to another university is one of the three options for a young faculty member, the other two being getting promoted at one's own school and moving to a job outside of academia. Since a young faculty member does not know what will happen in the future at her home institution, it is prudent for her to develop whatever capital she can to maximize the possibilities

from the other two options. Regarding the external academic market, a young scholar's goal is to make herself attractive to other schools and to generate interest so that she has the option of moving if she wants to.

Most junior faculty would love to be approached by other schools about a potential move. Even if they are happy at their current school, being approached by a competitor is a positive signal about one's value. Such offers can improve the faculty member's standing in their own institutions since an outside offer from a peer department establishes to the deans that one's research is valued by the profession. Sometimes (but not always) universities will even match outside offers financially.

However, the optimal strategy for encouraging these approaches is not obvious. Unless one is "on the market", usually it makes sense for a scholar not to solicit offers actively, but to wait until she is approached (although dropping hints from time to time to schools that might be attractive is sometimes a good idea).

Generating interest without actively soliciting it can be somewhat tricky. One thing that can help a lot is to be well-known in the profession. If a scholar has more friends and her papers are recognized as being important, she will have the better chances of receiving interest from other schools. Going to conferences, presenting work as often as possible, and getting to know people from other schools can help to increase a scholar's visibility. This kind of visibility can be thought of as a type of human capital that is especially important for generating outside interest. To a lesser degree, it can also help with internal promotions as it can lead one's work to be more well-known and impactful.

One thing that every scholar should do is to establish a relationship with any university that they have a connection to for personal reasons. For example, if one is from a particular city, then she should make an effort to meet the faculty of the universities located in that city. This issue is especially important for non-U.S. faculty – regardless of whether they want to move back to their own country at that point in time, they should make efforts to get to know the faculty at the universities in their home countries. When the faculty member visits her family, she can also visit the universities nearby. Eventually if she wants to leave the university where she teaches, the ones in her hometown may become natural fits.

However, visibility and connections only help to a degree. The most important factor in generating outside interest is a scholar's productivity. Interest from other schools normally comes to faculty who are doing well at their home institutions. Faculty who receive outside offers tend to be those who publish, are good teachers, and likable colleagues. And yes, other schools do spend the effort to find out about teaching and colleagueship before approaching someone. Being a good teacher and colleague not only benefits a faculty member at her home institution, but also helps her external market value.

Going on the Seasoned Market. Sometimes a faculty member is looking to leave a university and wants to stay in academia. She could have had a personality conflict with someone in the department, have received bad signals about her tenure prospects, be denied tenure, or just want to live in another part of the world.

Unlike being on the rookie market, it is often a good idea to target schools selectively rather than sending out a mass mailing when looking on the secondary market. The exception is if a faculty member is denied tenure, in which case there is no reason not to write to many schools. If the faculty member does not *have* to leave, she can be somewhat more selective than when she was on the rookie market. In addition, the element of fit is much more important as the person gets more senior; she should place more emphasis on where she wants to live and on being around colleagues that she knows and likes than when she is on the rookie market. In addition, she should assume that her own school will find out that she contacted other departments, so if she contacts too many and does not leave, it can get awkward around her own department.

On the seasoned market, even more than the rookie market, it can be very helpful if someone else, usually someone more senior, initiates the contact with the potential university. This person can be the student's advisor, a senior colleague, a coauthor or another respected scholar who knows the person reasonably well and can vouch for them. The third party can credibly convey what the person is like, why they want to leave their current university, and why the particular school being contacted is a good fit. For example, a third party can say: "So and so is a great researcher and teacher, but is single and hates living in a little town in the Midwest of the US. She would love to live in a cosmopolitan international city like

yours, and would fit in very well with your group." If a department is looking to hire faculty and hears this statement from someone they trust, their faculty might very well contact the person. If, on the other hand, they read this statement in a cover letter from someone they don't know, it sounds a lot less credible and is less likely to generate a positive response.

At some point in a scholar's career, it is prudent to go on the market to explore outside options. However, it is a really bad idea to solicit an offer from a department unless you would seriously consider moving to that university. Making an offer requires a lot of effort, and departments are not happy if they think that someone feigned interest in an offer just to get a raise at her home institution.

A sensible time to consider moving to another university is at the time before one comes up for tenure or slightly before. Since tenure is never certain, schools will understand if their offer is turned down in the event that the person does get tenure. If things go badly and she does not get tenure, it is nice for her to have an option to move right away, rather than do a full search the following year. While the standard faculty contract contains a "terminal year" after the tenure decision so that people who are denied can find another job, it can be awkward to hang around a department that just denied your promotion. For this reason, many scholars who are denied tenure prefer to leave right away rather than stay another year at the university that decided not to grant them tenure.

Promotion Decision in Universities

Probably the most stressful and consequential times in an academic's career is during her reviews. In a typical U.S. university contract, aside from annual reviews that provide feedback and can affect salaries, there are three main reviews during an academic's career. The first normally occurs during the third or fourth year as an assistant professor. There may or may not be outside letters at this review but aside from that, it follows the same process as tenure reviews. There is usually a departmental committee that votes on the case, the department head makes a recommendation, a college committee makes yet another recommendation, and the dean decides what the college should recommend to the

university. Eventually someone high in the university administration, usually the provost, makes the final call, often with the assistance of a university-level promotion committee.

Most universities rarely let people go at this early review. Instead they use it as a way to give detailed feedback and to provide junior faculty for an opportunity to improve in areas of weaknesses. Importantly, universities use this review as an opportunity to advise some faculty that they are unlikely to be granted tenure in the future, and that it would be wise for them to start looking for another job. If a faculty member who has little chance of being promoted finds another job prior to the tenure review, it saves the senior faculty from a painful review process and allows the junior faculty member a chance to move on in her career without having been denied tenure.

The tenure review can be the most important review of a scholar's career, since passing it gives the person a lifetime contract. Tenure reviews usually occur during the sixth year and follow the same process except that schools require evaluation letters from senior scholars in the profession. Most universities allow the candidates to suggest some names and the department to suggest others. If a large number of the people that are asked refuse to write letters, it is usually considered as a bad signal about the candidate since it is likely that a number of the refusals occurred because the people who were asked thought the case was weak.

Finally, at most universities, there is a third review, from associate to full professor. This review does not occur on a fixed timetable but usually occurs about five years after a scholar receives tenure. Most departments I know will only encourage people to have their case considered for full professor if the faculty think it likely that they will support the case. Since tenured associate professors have the right to remain at that rank for the rest of their careers, there is no reason to consider cases that are likely to be difficult. Associate professors do have the right to have their cases for full professor considered even when they are discouraged from doing so by the department. Occasionally these cases succeed, but more often they do not.

This structure is fairly common in most U.S. state universities. At U.S. private universities, clocks tend to be a bit longer. At some, associate professors can be untenured, although there is still a serious

review with outside letters at the associate professor stage. In other countries, the review structure can differ substantially, and in some countries faculty have essentially lifetime contracts starting the day in they are hired.

How to maximize one's chances. While the promotion process is well-understood by most faculty, what is not understood is what a faculty member can do to put her best foot forward and maximize her chances. By the time of the promotion decision, she has done her best in publishing and teaching. She has to put together her packet, provide names of letter-writers to the department, and most importantly, remain calm around the department during this very stressful and sometimes awkward period.

One thing that actually makes a difference is the person's attitude. It does help to be a good colleague – to be helpful, cheerful, and active in departmental events. Faculty doing tenure evaluations try their best to be fair and ignore personal feelings, but there is no doubt that, holding all other things equal, well-liked faculty are more likely to be promoted than less well-liked faculty.

Promotions can be very difficult for everyone involved, including the people doing the evaluation. People up for promotion often can figure out when the senior faculty are meeting to discuss their case. Junior faculty should be aware that, at these meetings, the first thing that is always said is a reminder that everyone is obliged to keep quiet about everything said in the meeting, and especially not to say anything to the candidate. Therefore, after the meeting, there are always awkward moments in which the senior faculty really want to tell the candidate that her case went well, but aren't allowed to. Sometimes the candidate incorrectly infers from the lack of information from a friend who normally tells her everything that there is a problem with her case, when in fact everything in the promotion meeting went very well.

The Research Statement. One thing that many junior faculty do not spend enough time on is the research summary that must be included with the packet. Often when I read these documents, they seem like the candidates just copied the abstracts from their papers, put them one after another in a file, and

called it done. This kind of research document doesn't necessarily do any harm to the candidate but it doesn't really help her either. If I am writing an outside letter or am on a college committee evaluating a candidate from another department and want to read a paper's abstract, I can just look at the paper.

What I want to know, especially when I am evaluating someone whose work I don't already know well, is more of an overview of the candidate's work and how she views her scholarship. How does she think? What is the theme that links her papers with one another? What do we learn from her papers together as a group beyond what we would understand from reading them individually? Ultimately, the most important issue in promotion decisions is what we have learned from a candidate's work that we would not have learned had the person gone into another line of business.

I always encourage junior faculty to put together a research document designed to answer these questions. A candidate for promotion should explain in her research document why she chose her research program, what her general approach is to addressing questions related to this program, what she views as her most important contribution, and where her research is likely to go in the future. She should focus the discussion in her research document on issues rather than journal publications. The research document shouldn't say: "This paper got into *Journal of Political Economy*, which is a top-ranked journal". Instead, it should say how the paper affects the way in which the profession thinks about an important topic and how it influenced the opinions of other scholars. A good way to do so would be to document that the papers are regularly cited by other scholars and have made it on reading lists at other universities.

An outside reader can look at the resume to see where the papers are published. The research document should explain what is in the papers and why a reader should care about them. It should make a case that her research has taught us important lessons already, and that there is more to come in the future.

Outside Letter Writers. The other thing that a candidate has to include is a list of potential letter-writers. Most departments pick some of the names from the candidate's list and some additional names who are not on the list. Many junior faculty obsess over which names to include on their list, and don't really know how to think about which names to include. Is it more important to select big names, people

from top universities, or people who are likely to say good things? Is it ok if you know them? What if they are a coauthor or thesis advisor?

When proposing the names of potential letter-writers, it is useful to consider the letter-writing process both from the perspectives of both potential letter-writers and the faculty on the promotion committee who will be reading them. Senior faculty at other universities who are good choices for writing outside letters are scarce, and can be overwhelmed with requests, sometimes receiving 10 or 15 requests for outside letters in a single year. Many senior scholars are not viewed as good choices for outside letters because they have reputations for always being late, having strong opinions and idiosyncratic tastes, not being active in research any more, or always coming across as overly negative. Consequently, senior scholars who do not have these characteristics can be besieged with requests.

Letter-writers receive no compensation for writing the letters, usually only get a formal letter thanking them, and are normally not even told about the outcome of the case they spent time on. In addition, even though schools try their best to preserve secrecy, the candidates sometimes do find out the identities of the letter writers and what is said in the letters. Some schools have to respond to Freedom of Information Act requests; at others, staff or faculty on the committee have been known to tell candidates details about the letters, especially if they are negative.

In other words, when someone is asked to write a letter evaluating a tenure case, she is being asked to spend a day or two of her time working hard for no compensation, with a chance that some nice person will hate her forever if she says anything negative in the letter. And she can be asked 10 or 15 times to write such letters in a given academic year. The dilemma for scholars who are asked to write letters is that if they don't agree to write the letter, then that action would send a negative signal about the candidate's quality. Since faculty really don't want to send negative signals about someone who deserves to be promoted, most faculty will write letters for candidates that they think have very strong records, but are likely to be more reluctant for weaker candidates. It is painful to write negative letters, and there can be ramifications of writing one if the university does not keep them secret. This tendency is, of course, known to department heads and people on tenure committees. Therefore, an inability to find senior

scholars with good reputations who agree to write for a candidate is commonly taken as a bad signal about the candidate's quality.

Once the letters are written, they go to the committees who must evaluate the candidate, usually at the departmental, college and university levels.[99] Within departments, the senior faculty know pretty well what the profession thinks of their junior colleagues, and the letters invariably confirm what the senior faculty already know. Where the letters are more important is at the college and university levels, where the candidates going up for promotion are usually not well-known to the people on the committee. The scholars on these committees are mostly from different fields from the candidate, so normally don't know much about the people writing the letters except their title and a few qualifications such as whether they edit a major journal. Since outside letters are mostly positive (remember, no one ever wants to write a negative letter), committees tend to focus more on the university the person is from and whether there is anything at all negative in the letter, rather than any detailed comments about the papers in the letters.

Therefore, from the point of view of the candidate, the following is what I think to be the optimal strategy for picking names of letter writers. First of all, pick people who can evaluate your work. Second, pick people who you know and seem to like you and your work. Third, pick people whose personalities are more easy-going and positive. Remember that there are some academics who think that "She's OK" is high praise. The people reading a letter from such a person will not know about this person's personality and could potentially view such a letter overly negatively.

Fourth, think about how distinguished the person looks from the perspective of a potential tenure committee member, especially one from another department. A senior scholar at a good university who hasn't published much lately is likely to look much better than an "up and comer" who has a less fancy title but is up to date about the literature you work in. In addition, even though a department may be very strong in the candidate's field, if the university is not well thought of in the field of the person reading the

[99] This policy varies across universities. Some do not allow most faculty members to read the outside letters, some do not have a college committee but allow all faculty to vote on the cases, and some (like Ohio State) do not have a university committee.

letter, even a positive letter will have less impact on his opinion of the case. Therefore, it is a good idea to select names of scholars from universities that are highly-ranked in many fields, not just the one in which the candidate works.

Fifth, avoid any appearance of conflicts, even if they are not severe enough to bother your department. Coauthors and advisors are out, but I would recommend even avoiding people who taught you in graduate school but weren't on your thesis committee. The impression you want to give to the tenure committee is that there are many excellent scholars who like your work. If you include people who those on the committee think might be biased about you and your work, then their inference might be that there aren't any unbiased scholars who would have been willing to write a strong letter.

If the Review Does Not Go Well

The Third or Fourth Year Assistant Professor Review. The main purpose of this review is to provide junior faculty with feedback on their performance in their first few years. Ideally, junior faculty can correct any deficiencies so that they can have a better shot at earning tenure a few years later. Under certain circumstances, however, faculty are not renewed, though nonrenewals at this stage are rare at most schools. Usually nonrenewals at the review prior to the tenure decision involve a faculty member that has done very little research or is exceptionally bad in the classroom.

Even though most faculty are renewed at this stage, the feedback they receive can be harsh and upsetting. Senior faculty try to give the person up for review an honest assessment of what they have to do to earn tenure, and to suggest specific changes that could help the person do a better job. Sometimes these suggestions are things that the person up for review knows already: "You have to publish your papers"; or "You have to improve your teaching ratings". But other times the feedback is more substantive. In our department we try to be as specific as possible. We tell the faculty member which of their new projects we consider to be most promising, and whether they are making any mistakes in the way they are going about their jobs. For example, they could be starting too many or too few new papers,

traveling too much or too little, or making correctable mistakes in the classroom such as pitching the class at too high a level or trying to cover too much material.

After receiving the feedback, it is important for faculty to be honest with themselves. Does the faculty member who had the review think she has a reasonable chance of getting tenure given the feedback she received? If so, does she want to spend her life at this particular university? Is she enjoying herself enough and being sufficiently productive that it makes sense to stay longer at the current job even if she doesn't expect to be promoted? Does she agree with the specific criticisms of her work that she was given?

These reviews are a natural time to reflect on one's career and to think about strategies for the future. Most of the time, even if the review is negative, the right thing to do is to stay at the school through the tenure review, assuming that one is relatively happy and productive there. But the person should be honest about what is said in the review. I know of several cases of junior faculty who misinterpreted negative reviews and bought big houses immediately after receiving them, only to lose money when they had to sell the houses a few years later following an unsuccessful tenure review.

Sometimes, however, following a slow start and a negative review, it can be stressful to stay at the place that provided such feedback. In this situation, a move to a new environment with a fresh clock can rejuvenate a career. At this stage, it would probably be a mistake to do a mass mailing of one's resume to many schools. If a junior faculty member does a mass mailing, then the job search becomes public. In this case it can be very awkward for her if she decides not to move, even though she has the option to stay at her current school for at least a few more years. For this reason, if a young scholar wants to move midway through her time as a junior faculty member, a sensible approach can be to contact departments that already know her and have shown an interest in the past. She should try to do so as quietly as possible to minimize gossip among her current colleagues.

The Tenure Review. Unlike the review in the third or fourth year, the tenure review is an up or out decision. If the university promotes someone, then she has a lifetime contract. If they do not, then she

has a year to find a new job. When someone is denied tenure, word spreads pretty quickly, and the profession knows that person will be looking for a new job. Consequently, the right strategy when denied tenure is to do a global search and to consider all possibilities.

One thing that can be difficult for people who are denied tenure is maintaining a reasonably professional relationship with their senior colleagues. Being denied tenure is extremely stressful and upsetting, especially when the person truly believes that she deserves to be promoted. The natural thing to do is to speculate on the people who were the drivers of the decision to deny tenure and to take it out on them. However, a scholar must try to do what is best for her career at this difficult time in her life, even if it means hiding her feelings about the decision from everyone except her closest friends. Some faculty complain publicly and loudly about tenure denials. Such complaining rarely serves any productive purpose. However, when other schools hear about it (and they surely will), it can make the person come across to other schools as a complainer who is likely to be a poor colleague.

There are two things that anyone who is denied tenure must realize. First, the vast majority of time, the reason for the denial is not personal but professional. The reason for the denial was most likely that the senior faculty did not like the research or thought that there weren't enough papers in journals ranked high enough. Or there could be some other issue regarding the scholar's research or teaching. Sometimes it has to do with service; some schools expect a minimum time on campus and associated service before they are ready to give a person a lifetime contract.

Second, regardless of the reason for the denial, it is extremely helpful to someone denied tenure to have their senior colleagues say good things about them. Whichever school is thinking of hiring the person in the future will almost certainly call someone at the school that denied her tenure and ask why she was denied. If the answer is: "She is a great colleague and teacher but didn't quite have enough to meet our standards," the person has a good chance of being considered seriously for a job at the other (usually lower-ranked) university. If the answer is: "She has published some papers but is an extremely difficult colleague, and some faculty thought there were problems with these papers even though they were published," her chances become much lower.

The Full Professor Review. The final promotion in a scholar's career, unless she is nominated for an endowed professorship or chair, is for full professor. The important thing to remember about full professor promotions is that they don't have to occur at a particular point in time. Some people remain associate professors their entire careers and live perfectly happy lives. But most faculty would like to become a full professor at some point, and most departments would like to eventually promote all of their research-active tenured faculty to full professor. Therefore, it is best if there is an agreement among each faculty member and her colleagues on what is required for promotion to full professor. Departments should not put people up for full professor unless they plan on promoting them, assuming the letters are fine and there are no negative surprises about teaching or ethical issues that come up in the review process. And it is usually a mistake for faculty members to push departments to promote them to full professor before the department is ready. Doing so can create unnecessary conflict and hard feelings. Most of the time, the promotion will occur in a few years anyway if the person waits.

Keeping Life Interesting After Tenure

One thing that always surprises me about academia is the number of tenured faculty who end up very bitter about their jobs. I have always thought that being a professor was one of the greatest jobs in the world. Being a tenured professor is the dream job of most people who enter graduate school. How can so many people who get their dream job end up so unhappy with it? Is there anything they could have done that would have led to their being happier at work?

Most of the time when a faculty member ends up bitter, something like the following has happened: The person publishes three or four papers related to his dissertation and gets tenure in his late 30s. He continues to write similar papers, but they appear in lower-ranked journals because his approach to research becomes less novel and outsiders begin to think of him as a bit dated. After a few rejections from top journals, he starts being critical of the profession for its taste in research, which is changing from the way it was when he was a graduate student. Meanwhile, in his own department and/or college, he has

a disagreement with a department chair. The department chair takes it out on him by giving him a bad teaching assignment or a bad office, or starts taking away resources from him (summer money, access to an RA, etc.). The person starts complaining about his situation to everyone he knows, and eventually the complaining gets back to the department chair.

If this faculty member is tenured when he is in his late 30s, he could become bitter by his 40s. By this time, the person probably does not have many options to move to another research university nearly as good as the one where he currently teaches. This person can remain on the faculty for 20 or 30 more years, over which time he finds it increasingly difficult to contribute in a productive manner.

The sad fact is that this situation is entirely preventable. Anyone who is smart enough to get a PhD and earn tenure is capable of being a valuable member of the faculty for his entire career. Academics are almost always bright, hard-working scholars at the time that they complete their degrees and earn tenure. They end up much happier if they find a niche, continue to do interesting work, and contribute to their universities and society throughout their lives. This interesting work can be new research, but it doesn't have to be. There are many other kinds of opportunities that can become important parts of an academic's professional life as well.

Academics should always be thinking of ways to expand their horizons and to branch out to do new things that will keep them busy. All people, especially academics, do best when continually given new challenges. But many faculty fall into the trap of teaching the same courses over and over again, and writing essentially the same paper over and over again. When they do so, they forget about the two principles laid out at the beginning of this chapter: Faculty should always be thinking about research and nonresearch opportunities that they are likely to have access to in the future. To make these opportunities realistic, faculty must invest throughout their careers in the capital to pursue the ones they find appealing.

Some faculty continue to focus on research throughout their careers. I fall into this category myself. I am somewhat of an outlier in that I have become more interested in research and in understanding financial markets as I have gotten older. The typical pattern, however, is the opposite; interest in research peaks in most academics' younger years and their research slows down as they focus

on other things later in life. Some faculty move into administration as they get older. There is always a shortage of good administrators in academia, and providing leadership through administrative positions is an excellent way to contribute to a university. Other faculty get more into teaching as they get older. They can spend time writing textbooks and teaching extra courses, sometimes for additional compensation. Some become "public intellectuals" and try to contribute to the public discourse in areas in which they have expertise. Consulting is yet another way to keep an academic's life interesting. Establishing oneself as an expert in a particular area that is demanded by the private sector or government can lead to a rewarding and profitable consulting business.

If a faculty member focuses her life on any of these (and other) options, she can avoid falling into a rut and not enjoying her professional life. But pursuing any of these options requires investment in the right kind of capital. To remain state of the art in research, she must work to keep up with the latest techniques and issues that others care about. She also has to be sure to prevent her research from becoming stale and to be open about letting her research evolve in new directions over time.

To become a successful administrator, she should demonstrate interest in whatever positions that come up and gain experience. These positions might be less visible but still important ones, such as being the assistant head of a small program the department is running or the advisor to a student group. Being good at these positions can allow her to become a department head or a dean later in her career.

The ability to earn money through extra teaching involves an upfront investment in developing a unique course and in marketing that course. Writing a textbook is a major endeavor that many people plan on doing "someday" but most never get around to writing. The same goes for writing newspaper columns about current events and attempting to become a public intellectual.

Finally, consulting opportunities usually don't appear magically. Academics who wish to establish a consulting business usually have to develop it over time through investments in the knowledge of an area practitioners are willing to pay for, and by cultivating relationships with industry that allow the academic to exploit this knowledge.

A scholar should learn from an early stage of her career that "She is the CEO of her own life." She will be happiest if she goes about her career with a purpose and not just allow it to drift toward whatever seems interesting at the time. She should always spend time thinking about what her options are likely to be in the future and what capital will be necessary to pursue the ones she finds attractive. Investments in human capital designed to make one's life more interesting in the future are likely to be well worth their cost.

Epilogue – Academic Success Beyond the PhD

As academics, we receive little training about the way in which we should prepare for our careers. We do learn much in graduate school about the *science* of scholarship, which for an economist consists mostly of learning economic theory, econometrics, and the literatures in the applied fields. However, much of what one needs to know to be a successful academic is typically learned in a haphazard, word of mouth fashion. For example, almost every economics graduate student knows how to maximize a likelihood function and understands the conditions necessary for an instrument to be valid. But perhaps surprisingly, an astonishingly high fraction of them do not know how to decide which results to report, how to present them in a way that a reader can easily tell what they have done, or how to explain what they mean to a reader who is not a specialist in the paper's subfield.

Much of what academics do can best be described as a *craft*. Like other kinds of craftsmanship, it is a combination of time-tested technique, strategic thinking, ethics, and imagination. These things can be learned, but tend to be acquired erratically and incoherently. What I have attempted to do in this book is to distill my own views of the craft of an academic working in the economics-based social sciences. While others will undoubtedly disagree with some of what I suggest, I am confident that the vast majority of readers will concur with the book's main thesis, i.e., that most scholars would be better off if they spent much more time thinking about the nuts and bolts of writing their papers, the way that these papers fit into a research portfolio, and the development of their careers.

I have made a number of recommendations throughout the book. All are designed to help scholars to produce more influential research and to have more satisfying careers. I thought it might be helpful to some readers if I summarized some of what I thought were the most important suggestions.

A (Baker's) Dozen Points to Remember

1) Be an "academic hunter." Take a question you want to know the answer to and look for approaches that will allow you to provide insights into it. Don't wait for topics to come to you; instead, seek out research questions on issues that fascinate you. Make a "market map" of all of the work that has been done around the issue you want to study, and use it to determine the way that is likely to provide the most novel insights about the topic.

2) Try to choose research projects that are related to one another and form a coherent research program. Your goal should be to develop a portfolio of papers that establish your reputation as one of the top scholars in an important subfield. Remember that there is a limit on how many papers any of us can work on at any point in time. The opportunity cost of starting a project can be substantial, especially if a scholar is near her capacity so that starting a project today limits her ability to start others in the future.

3) Everyone has unique knowledge and talents. Start research projects that take advantage of what you are good at. In addition, always be open to learning new areas and acquiring new skills. Be cognizant of what the best young faculty in your area are working on and the tools they are using, which can be a useful guide for the direction in which you would like to take your research.

4) Do not think about "research" and "write-up" as separate activities. The write-up *is* your research. A huge fraction of the time you spend on your paper should be on the writing and presentation of your analysis. The success or failure of a particular research project will depend not only on the modeling and empirical results, but also on how you motivate the work, explain those results to a reader, and interpret them appropriately.

5) Assume that your target reader will spend a fixed amount of time on your paper regardless of its length. Write the paper so that this reader spends her time focusing on what you want her to think about: the analysis *you* do and the implications of *your* work. Don't spend too much time on lengthy literature reviews, technical details that do not affect the paper's main idea, or discussions that readers will find confusing rather than illuminating. If you do, readers will become distracted and spend less time thinking about your paper's insights.

6) The introduction is the most important part of an academic paper because it is the only part that many readers will look at. Therefore, space in the introduction is precious and the opportunity cost of wasting it is high. An introduction should accomplish the following (and only the following): *i) Grab the reader's attention; ii) Say what question you are asking; iii) Say what*

approach you use; iv) Say what the results are; v) Say how you interpret the results; vi) Discuss other implications of the results; vii) Provide an outline of the paper.

7) The description of data and methods has to be sufficiently detailed so that a doctoral student on the other side of the globe can replicate your results. But it should be written in a sufficiently interesting prose so that a typical reader won't get too bored. Only put things into the main body of the paper that most readers will find interesting. Details required for replication but not an understanding of the paper's analysis should be in appendices.

8) The keys to a good seminar presentation are communication and control. You must convey the ideas of your paper to a typical seminar participant, who is not likely to be a specialist in your subfield. You also have to control the audience and not let them bog you down with clarifying questions or discussions that are unrelated to your paper. Be sure to get to the main points you want to make early in the seminar and *finish on time*. The purpose of the slide deck is to keep the audience's attention on the topics you want and to provide them with details. Keep the slides clean without too much information on each one and use a large, readable font. At the end of the talk, you want people talking about what they have learned from your paper. If instead they are talking about how cool your slides are or the argument you got into with one of the faculty attending the talk, you have probably missed an opportunity to increase your paper's impact.

9) It is inefficient to give your paper to everyone you know at the same time. Instead, send papers out sequentially. First, ask one or two close friends who you can trust to read your paper carefully. Then, make revisions based on their reactions and ask for feedback from a few more people. Continue the process, addressing comments as you receive them. Eventually, the feedback reaches diminishing returns and becomes less useful. Only at this point should you make a paper publicly available and post it online.

10) View journal submissions as a continuation of the revision process. Don't submit a paper to a journal until it is as strong as you are capable of making it. Be realistic about your choice of journal; not every good paper belongs in the *American Economic Review*. Take rejections productively and use them as a way to improve your paper. If the editor gives you an opportunity to revise the paper, take advantage of the opportunity. Address each point the referees and editors raise carefully, and explain how you do so in your revision document. Hopefully your paper will eventually be published in a good journal.

11) An important factor affecting a doctoral student's success is her attitude towards research and her relationships within the department. A student should think about research ideas from the day she arrives on campus, and start getting involved in research as soon as possible. A key element of a graduate student's success is her relationships with the faculty and also with her fellow students. The best doctoral students usually provide incredibly valuable help to other students on their research, which is one reason why in many departments, the highest-quality dissertations tend to cluster in particular years. Graduate school is *not* a zero-sum game in which success for one student comes at the expense of other students.

12) Always think about potential new things to do professionally, and acquire the human capital necessary to move your career in the direction that seems most exciting to you. These opportunities can be research related, potentially extending one's earlier work or moving into a promising new area. But they can also be a chance to move into academic administration, to write textbooks or policy pieces for the general public, or to apply your expertise in the private or government sectors. Academics should strive not to let their lives become boring, teaching the same classes over and over without new challenges.

13) Academia can be a wonderful career. But much more so than other professions, it is only what you make of it. Take advantage of the good parts and avoid the bad parts. Spend your life making a difference in your students' lives, learning about new things, studying interesting problems, becoming good friends with colleagues both at your university and elsewhere, and traveling the world visiting other universities and presenting your research. Avoid fighting with your colleagues and deans, complaining about the inevitable inequities and inefficiencies in your university, and letting the annoying students bother you too much. Never forget that you are the CEO of your own life. Make sure it is an interesting and enjoyable one!

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