

A Field Guide to Economics:
A Young Scholar's Introduction to Research, Publishing, and Professional Development.

Michael S. Weisbach

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Chapter 1. Why do Academic Research

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.¹ 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. If we were able to do something like this, then we would have made a substantial contribution to knowledge and have become an "academic celebrity" at the same time. Many of us get our PhDs, with the dream of making a discovery like Wiles' and gaining the kind of acclaim he achieved.

¹ Fermat famously wrote in the margin of a book: "I have a truly marvelous demonstration of this proposition which this margin is too narrow to contain." For a formal statement of the problem and a discussion of the 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 far 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 the question is asked in the right way, 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. The 1991 Nobel Prize in Economics, which Coase received, was in large part because he had the foresight to be the first person to ask such an important question.²

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.

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

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

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

My own PhD is in economics. 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) every evening, where I hung out with some of the best faculty and doctoral students from both Harvard and MIT, mixing in a very informal atmosphere. 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 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,

³ For ease of exposition I will use feminine pronouns when referencing researchers and male pronouns for readers throughout the book. Occasionally when referring to a particularly clueless trait of authors I switch and use male pronouns, since the most clueless authors are usually men.

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 how to pick research projects that will have lasting impact, to communicate why a particular project will be important, to handle data properly, to write up one's results in an appropriate scientific, yet readable manner, and to interpret one's results in a way that others will find reasonable. These skills are, for the most part, gained in an apprenticeship type relationship with a student's advisor, other faculty, and fellow students. I have been fortunate enough to learn them from an incredibly diligent thesis advisor, Jim Poterba, the stimulating environment of the NBER, as well as over thirty years subsequent experience of being a research faculty member of a number of different universities.

People often ask my advice on various aspects of the research process. I am asked questions such as how to pick an appropriate topic, how to find coauthors, how and when to write up results, how to write readable and interesting prose (in English), how to structure academic papers, how to present results, how to interpret results, when to cite other scholars, and, most frequently, about all aspects of the publication process. These skills are as important to success as an academic as the ability to solve difficult problems. They are usually learned from advisors, colleagues, personal experience, and sometimes nowadays, even from interactions with strangers on websites. Learning how to do research has always been a random, word-of-mouth, process that some scholars learn better than others. I have had tenured faculty at major research institutions tell me that they never learned some of what I would consider to be basic research skills, and that they very much wished they learned these skills much earlier in their careers. My view is that there is no reason why this research process should be word of mouth. It can and should be written down.

My hope for this book is that it will explain exactly how one goes about a research project, and structures a career around a structure of such projects. There are any number of good books about how to

do econometrics and how to solve various kinds of models. But for perhaps the most important skills involved in doing research, we rely on word of mouth and learning by doing. How does one decide on which topics to work, whom to work with on them, how to interpret and present results, how to explain these results to others in prose and also in presentations, and how to publish the work in prestigious outlets? I hope to provide some guidance on these and related issues in this book.

I am an economist who specializes in corporate finance, the economics of the firm, and related topics, so most of my examples will be from these fields. However, the source of the problem I am hoping to address, the haphazard way in which research skills are taught, is present in all fields. I suspect that the solutions I present for these problems will be applicable in these fields as well.

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 to improve their reputations. To do so, they are trying to have a higher research profile, 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, as well as the development of fields, has lead faculty to become increasingly specialized. Third, there has not been a commensurate growth in the number of top-level journals, so it has been increasingly difficult to get papers published in journals that are considered top tier.

Growth in Academic Research

While many universities have cut back on the number of tenure-track faculty as a way of saving money, other universities are trying to gain prestige by increasing their research presence. In the 30-plus years since I left graduate school, the number of universities expecting their faculty to publish in top outlets has increased dramatically. In my field, finance, there was little research in the top journals outside the top 20 or 25 U.S. departments in 1987. 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 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 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 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 departments ranked outside the top 15 or 20 produce, not infrequently, 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. Today there are many more active researchers in the world than when I began my career and that number is growing at an accelerating rate. While this observation is about finance, it likely to be true of many other fields as well.

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. Approximately forty years ago, the tools of game theory opened up a number of new lines of inquiry in economics. Recently, the availability of immense amounts of data and the computing tools to work with such data have revolutionized many fields. Artificial intelligence and genomics are two of the hottest fields because of the rapid advances that have been made in them recently. Universities have responded by devoting substantial resources toward expanding their faculties in these areas.

These examples of “quantum jumps” are, 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, 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.

In my field, 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.⁴ 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 researchers, such as Andrei Shleifer, Jeremy Stein, and Robert Vishny, have also been able to make important contributions across the major subfields. Most of us,

⁴ 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 long enough. Jensen’s Prize likely hopefully be awarded at some point in the near future.

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 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 our field is heading in the direction of many other fields, in which people in the same department cannot understand each other’s work.

The Publication Process

How has the publication process changed over time? While the number of journals has grown with the size of the profession, the ones considered to be “top-tier” have not changed. In finance, we have the same three “top tier” journals as we had when I entered the profession in 1987: *The Journal of Finance*, *Journal of Financial Economics*, and *Review of Financial Studies*. 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.⁵ 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.

In other fields the situation is similar. 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

⁵ See <http://jfe.rochester.edu/turn.pdf> for statistics and Card, D., & Della Vigna, S. (2013). “Nine facts about top journals in economics”. *Journal of Economic Literature*, 51(1), 144-161. These authors focus about the evolution of competitiveness in the top economics journal over time but the same trends are occurring in the top journals in other fields as well.

expect junior faculty to publish at least some of their work in the top general-interest journals if they are to earn tenure. 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*.

Whatever the reason—a topic, not surprisingly, about which academics love to speculate—journal reputation is extraordinarily sticky. Irrespective of the questionable editorial decisions top journals not infrequently make and the terrible service they often provide authors, 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.⁶

Changes in Academia and the Research Process

How has the research process been affected by these changes? 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 negative. Overall, the academic research world has become increasingly competitive, as 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 suspect 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

⁶ 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 a “top” journal 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.

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, providing insights that could improve public policy, or develop interventions or products that aid world. Tenure provides us with the ability to write 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.

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 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; 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, and it is 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 usually that they have frustrated coauthors, 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 and these perfectionists often 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. But 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, 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 to 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 one starts a research project, there is a problem she does not know the answer to, and she comes up with a way that she hopes will answer it. Sometimes she will get interesting results and learn something; sometimes 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 their your 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, ultimately tenure and promotion 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 is a mistake; there are many journals out there and your paper will have a much larger impact if it is published in a good one, even if that journal is not considered top tier.⁷ 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

⁷ Not everyone agrees 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 journals 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."

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 before long, the entire profession knows about it and the faculty member's reputation is damaged.

When one is a faculty member or even a doctoral student, one must remember that one is a professional scholar. The standards about anything related to one's job are much higher for professionals than for amateurs. This is true in all professions; it is fine for you and me 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 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.

The Goal of this Book

In my 30 plus years as an academic, I feel like I have done almost everything there is to do (except being an academic administrator, my colleagues know me well enough to keep me far away from

those jobs). I have been on the tenure track at four major universities, visited a fifth for a year, and have given talks at countless others. I have been an editor at one of the top journals in my field (the *Review of Financial Studies*), been associate editor for the other top journals in my field and several others, and refereed many papers (averaging over 25 per year for a while). I regularly supervise doctoral students, usually being on at least one thesis committee every year. I have gotten to know faculty from universities all over the world, have followed their careers, and have observed the factors that have led some to have successful research careers and others to have more difficulty establishing themselves.

As I have gotten more grey hair, younger faculty often come to me for advice as to how to manage their careers, how to develop a research program, how to write papers, present their results and navigate the publication process. This book represents the culmination of my thinking. In it, I present my thoughts on how one should pursue research, from picking a research program and projects through the writing up of the analysis to the publication process. On the way, I will try to provide advice that will be useful to young scholars in structuring their research careers.⁸

I do want to emphasize that much of what I will say is my own opinion, and there is not necessarily a consensus on the optimal way to do research. I expect that most readers will agree with the majority of what I say, and vehemently disagree with some of it. If everyone agreed with everything that I say, then I am not addressing important enough topics. There is much about academic research where different people can have different viewpoints and both be successful. I believe, however, that thinking about what I say below, even if one disagrees with it, will help scholars do better research.

⁸ When proofreading a draft of this chapter, a former student pointed out that I would have been a much more successful researcher if I had followed my own advice!

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 his personal research portfolio.

Young scholars often ask me for advice about how to pick research topics. They are usually bright and hard-working, with a strong desire to have a career devoted to scholarship. 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 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 at a high level. 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 you work 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 was interesting but not as interesting as a second idea the research had had while working on the first one. Once she 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 paper is 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 uncompensated. 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. Research programs do not have to last forever; one's 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 particular theme of 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 they found two MIT PhD students who were working on a technology that fit what they thought the market would require, so 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.⁹

⁹ This company they created is now a “unicorn”, meaning that it has more than a \$1billion valuation.

The alternative approach, which is the more traditional approach to venture investing, is what my speaker calls “farming”. This approach is more passive and means the venture capitalists sorts through thousands of business plans that 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 limit the types of companies in which they invest but are less aggressive at seeking out companies 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. One 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 well trained. 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 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 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 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 but 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.¹⁰ 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 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

¹⁰ 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 2017.

working together after Tversky gave a guest lecture in Kahneman's class. The surprising thing was that Kahneman absolutely hated Tversky's talk. They argued 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 life's work for each them. 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?" As is common with much great work, 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 style 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 has led him 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 is coauthored with David Card and concerns the effect of the minimum wage.¹¹ 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 find that, counter to the textbooks' prediction, employment at fast food restaurants that pay minimum wage increased more in New Jersey than in Pennsylvania. This finding has probably 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.

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.¹² 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 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 Your 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

¹¹ 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.

¹² 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.

were obviously brilliant and would have been successful not matter what strategy they pursued. But in academia, there are many brilliant people, and very few are nearly 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 one spends the majority of your time working on and what others think when one's name comes up. When starting an academic career, one 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 one gets older, one should always be redefining one's 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 one would like to explore and are they worth the upfront costs?

Personally, I entered an economics Ph.D. program with little knowledge of finance. What I did know was about capital markets and options, which is the area of finance that I ultimately chose *not* to focus my research on. Although my current coauthors probably will not believe it, at the time I was very good at math, programming, and data analysis. 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 Illinois in the early 2000s, to an interest in private equity. 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 your department. But in most departments, 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 successful scholar is one's independence; a good policy is to try to have a unique approach and to make your individual mark on the profession.

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, probably focusing on basketball would 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 picked 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.

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.

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

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. Often potential projects that initially seem promising have to be dropped because either they have already been done or cannot be done. 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. In addition, often ideas that makes 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. Absent this knowledge, 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 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?

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 a little second order and 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. 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 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 issues.

One should also think about the style of work she is most comfortable with. There are famous academics who have made a career of reexamining others' work, sometimes using more advanced techniques. 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.

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 somewhere on the elephant's body. 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 in the long run to more influential research.

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 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 your 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, scholars are valued based on books they publish, in the sciences and engineering the amount of money scholars bring 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 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 it is distributed through journals, books, or conference presentations.

However, what research is considered to be “impactful” varies across universities, across departments and even across individuals measuring it within a particular department. Some universities judge research solely by the journal in which it is published.¹³ 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 sometimes political. Sometimes bad papers are published in top journals and important papers are rejected.

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.¹⁴ Often these papers were rejected because the referee or editor did not see the paper’s point, or were 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, higher ranked schools tend to rely more on citations and careful reads of the paper themselves rather than the place the work was published when they assess research quality. 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 research quality/impact is measured at their university and at other universities for which they might someday work.

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

¹³ In the humanities, the corresponding approach would be to judge research by the publisher of the scholar’s books. In some fields, research quality is measured mostly by the amount of research money brought in to fund the research.

¹⁴ 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.

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 one go out of one’s way to explain exactly why one is using this approach and how it relates to the mainstream literature and improves on it. Most importantly, a paper must explain what, at the end of the day, 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 what is important could possibly be anything but subjective. What is important to one reader will undoubtedly be trivial to another.

For the vast majority of papers, its impact will depend crucially on the authors’ ability to explain the paper’s contribution and why it is important.¹⁵ 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 understanding a potential paper’s contribution before you start working on it is an important skill that tends to be learned over time.

¹⁵ 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.

One 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. This type of papers tends not to be influential, since readers usually have trouble recognizing a paper's incremental contribution if the authors cannot explain clearly what it is.

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, must pay human subjects, or requires data purchased from an outside vendor. However, in most fields, 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. Research is an activity that requires total immersion of oneself into a project. I find that for myself, and I think for most others as well, 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 block out 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, and encompass a number of rounds, with substantial revisions 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 most 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, and audiences generally prefer to see newer work. 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. One should avoid being in a position where one has to decline speaking opportunities because none of one’s 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 revises her job market paper for publication, she cannot do the data work required for too long. But she finds 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 are not relevant to their 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 sacrificing something 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, you should have overlapping interests and complementary skills. The project has to be of interest to both of you and fit into each of your research programs. In addition, you each should be able to bring something to the table for the match to work well. If you are both good at programming but terrible writers, then your paper is likely to be well-programmed but poorly written.

Second, it is important that you are compatible with each other 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. Overall, if you are a young scholar, working with senior people is often a good idea, but ideally this work should not comprise the majority of your research. It is important to establish your 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. Your career becomes tied to their ability and willingness to put in the time to complete projects, and also to shepherd the projects through the often painful publication process. 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. Or the senior colleague could be a person that enjoys starting papers and hates finishing them, so the coauthored work stays a working paper for a long time, sometimes forever.

Fifth, and perhaps most importantly, your career becomes tied to your coauthors' in a number of ways. If your coauthor is sloppy or commits an ethical violation, it becomes a major problem for you. 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.

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 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 an exciting 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 one's impact. Advancing our own research program is sufficiently difficult enough absent outside constraints. If a friend's research interests coincide with your own, then it would be wonderful if you can work together. The key is the project; one should never sacrifice the best possible projects just to work with a particular coauthor.

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 details of the analysis researchers do. In economics there are many books covering economic theory, econometrics, etc., far better than I could. In other fields I am not an expert but there are many sources available for most technical issues. The focus on this book is different: it is on the way that researchers can optimize their careers and how they can best structure their research efforts to put their proverbial best foot forward. Researchers in all fields make common mistakes when they write their papers. In this chapter, I will discuss some of these mistakes, how to avoid them and to improve one's papers and to increase their impact.

One most common mistake that scholars often 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 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 university.

Most academic reviews are done through what is known as a “peer review” process. Editors send papers to scholars doing related research, who then write referee reports on the paper. These reports 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. The idea is that 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. No matter what anyone says, reviewers definitely favor friends of theirs and famous people who they respect more than 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 is really difficult to get your papers onto conference programs and published by top journals.

When you submit a paper to a journal or a conference, what can you do to maximize your chances 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 incentives themselves 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.

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, and if they send it out for reviewing, the identity of 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 get published 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. The costs inherent in doing evaluations of research mean that any system of evaluation will 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 manage 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 appeals to the decision-makers. Presentation of one's work involves writing well, but it also means emphasizing what readers 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, and not 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 it is hung. 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 we exhibited in the write up. He rejected the paper for this reason (and perhaps others as well). The referee was correct. 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 often 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's goal is 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. To do so, it is important to consider the way in which they are distributed. I personally 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 a website where I can download the pdf of the entire paper. I also receive emails from many conferences, including ones I do not attend personally, including the programs links to the papers. In addition, I am old fashioned, so 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 and relevant. The process is quite similar to the triage approach taken by conference organizers and journal editors. A first pass tells a 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. Consequently, inside a paper, not all words are created equally. The ones that make it to the email lists and 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 by far the 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 body of the paper will be read much more often than words in the appendix and especially internet appendices, so consequently are that much more valuable.

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 long reading a 1000 page novel than a 200 page novel. But remember, most readers do not read the complete paper. 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 write it accordingly. They should put the parts of the paper that they want reader 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 find them but not where they otherwise will look. 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. 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 explain to a reader why he should care about the paper, not on 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 could appreciate. 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. If a paper is written in a manner that is not too technical and is well-motivated, the number of people who are potentially interested in the 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 as to 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 what 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 say what the key assumptions are and what the idea underlying the proofs is. 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 these priors should change 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 that paper’s contribution 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. It is incumbent on an author to point out differences between her work and the prior literature, and to 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 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. The proverbial doctoral student on the other side of the globe has to be able to 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 it.

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.

5) Draw appropriate interpretations.

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 of us social scientists like this aspect of our field because we think it makes it

more interesting and exciting. However, ambiguity can create difficulties when drawing inferences from analysis.

Different people can and do interpret the same data 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 possible explanations appropriate credence while simultaneously explaining to the reader why she thinks one particular interpretation is the most likely one. Yet, the goal of a paper is the influence readers' priors. If an author does not let the reader know what she feels the results really mean, then priors are unlikely to change very much.

Chapter 5: The Title, Abstract, and Introduction

When a paper is posted online, all a reader usually sees is the title and abstract. Then if the reader looks at the actual paper, he most often reads only the introduction. A paper's title, abstract, and introduction have a large impact on the number of people who read it, and therefore can materially affect its ultimate impact on the profession. For this reason, I will spend some time 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 can be thought as 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 as possible about the paper. 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?"¹⁶ 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 will come to a "yes" answer to his question). My job market paper used a "short description" title, in my biased opinion, very well. After circulating early versions with

¹⁶ 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.

several other (longer) titles, I ended up calling the paper “Outside Directors and CEO Turnover.”¹⁷ 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 5700 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 (“DB or not DB”). 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 refers to, and instead of being perceived as clever, most people thought it was confusing.¹⁸

Andrei Shleifer likes to follow the practice of Alfred Hitchcock, the great director of suspense movies, and use one-word titles.¹⁹ This approach works for Andrei because he is a superstar, and many economists want to know about what he is working on. 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

¹⁷ See Weisbach, Michael S. (1988), “Outside Directors and CEO Turnover,” *Journal of Financial Economics*, 20, pp. 431-460.

¹⁸ 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.

¹⁹ 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.

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 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.²⁰

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, like I did 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.

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

²⁰ 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.

example, NBER and 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 tend to 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. 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 the 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 ancillary 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 what one might learn if a potential reader 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 they 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

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 so to decide whether to read the rest of it, or if 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, provided the introduction is well-written. 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", which is often followed by a note from a coauthor explaining that this one was ok but probably we can do better. 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 summarized in a paragraph.

When writing an introduction, always remember the expression “Less is More”. Say what is necessary as concisely as is 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 what 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 results are.*
- 5) *Say how you interpret the results.*
- 6) *Discuss other implications of results.*
- 7) *Provide an outline of 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 potentially will read a paper on a topic he previously did not find particularly interesting.

One approach that seems to work in economics is to point out a divergence between 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 that traditionally, corporate finance has focused on domestic rather than international considerations. In one of my better-known papers on the capital structures 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.

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" becomes a lot more meaningful to a reader who is aware that the existence of sticky prices underlies much of traditional Keynesian economics. So, documenting that prices are stick 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*.²¹ I have worked in both these subfields, and always make a point to cite the classic works, in part to give credit where credit goes, but also to remind readers of these issues’ fundamental importance to our field over a long period of time.

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. A common criticism is that “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 subtle than he previously had thought, that there is an important issue in the world that the literature has forgotten, or that there is a gap in the literature somewhere important. Once a reader realizes that his understanding of the issue you raise is imperfect, he is much more likely to spend time understanding 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.

²¹ 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.

When I attend seminars or read 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, and 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 they should be deferred to the body of the paper (or 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. Write a description of the methods you use 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 unique ways, 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 corporate finance paper, it would be ok to say that the sample consists of 1000 publicly traded U.S. between 2000-2010 but not say at this point exactly how these firms are chosen, where the data come from etc. 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 explain in detail in the introduction about the different laws, the way that the sample was constructed using hand-collected the necessary data, and other details specific to this paper that a reader will be interested in.

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 in the social sciences 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 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 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.

For example, many empirical papers in the social sciences 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 warranted. Overly strong interpretations can be a deliberate strategic move to attract attention to a paper. In addition, authors sometimes interpret results too strongly when they do not accept that there are plausible alternatives that are also consistent with the author's results. Authors do sometimes overinterpret their results; if they do so too often 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 so they are not overinterpreted or underinterpreted. A good rule is that an author should always make it clear what the author thinks the results mean, even if she includes a number of caveats and alternative interpretations that are possible. But the author has to be honest about the extent to which the results distinguish the author's favored interpretation from other possible ones.

6) Discuss other implications of results.

The authors' goal 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 go and 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 prominently. Some readers will not be interested in your main questions but will be interested in the additional ideas you present. If you point these additional ideas out in the introduction, these readers might value the paper 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 is traditional in a number of fields 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.

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 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 of your paper 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. There are two main ways in which authors confuse readers by including extraneous information in the introduction: 1) Including 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) Including 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 often spent 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 now know it well and also an appropriate courtesy to the authors of those other papers.²²

But 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 now. In fact, sometimes by this point the author has not told me what the point of his paper is because he is spending so much time describing others' work. I often never figure out the paper's point and usually come away from this kind of paper thinking more of the paper's literature than of the paper itself.

It is extremely important to explain your paper's point 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

²² It has become common to move the literature survey to the end of the introduction rather than in a separate section. This approach is sensible if the relevant literature is important but does not require its own section. If an author includes the literature survey at the end of the introduction, then everything I say in this chapter is relevant for writing the first part of the introduction that comes prior to the literature survey.

know the background necessary to understand it, without confusing a reader with too much information about others' work prior to discussion your own?

The difficulty of introducing your paper quickly, while explaining the ideas that the paper builds on and giving credit to those authors without making the introduction long and cumbersome, is one reason why writing introductions can be so difficult. One way that sometimes works is to explain the ideas and main results of the prior literature quickly and hopefully in a clear manner. The details of which author did what can be left to the body of the paper. Sometimes it is useful to have a footnote with lists of relevant papers, perhaps sorted into categories for different "types" of papers. But it is usually a good idea 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 your paper builds on one particular paper or is written in response to a paper if you think the authors are wrong or misinterpreted something. In this case, you should start the paper with a discussion of the other paper and how you improve or critique it. If your paper extends a long literature, it often is a good idea to cite the source of the literature. For example, if your paper is about whether firms should maximize profits to the exclusion of other factors, then it would be almost a requirement to start your discussion with Milton Friedman's classic arguments and an appropriate reference.²³ But if you discuss every paper related to this question before you explain the point of your paper, then many readers will never make it to the discussion of your own paper.

In summary, there are many issues to consider when writing an introduction. An introduction should be a summary of your paper and also an advertisement for it. You have to work hard to make the introduction short and readable, while explaining the important points you have to make in the entire paper. 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.

²³ 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 an 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 it is appropriate for the paper the author is writing. 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?

A minor thing that authors could do that would make their papers a bit more on point and readable is to use more descriptive names for the 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”, call the section something like: “Estimates of the way that Minimum Wage Laws Affect Employment” (substituting a description of your paper’s results of course). If there are two main results in the paper, break the “Results” section into two and use titles that indicate to the reader what is presented in each.

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 little unimportant things. When

one adds them up and pays attention to each of them, a paper can become markedly more readable and therefore will be more influential.

Since academic papers are organized into sections and there are issues that authors should be concerned with about 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. 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. Second, it is to give credit to other authors for the work that they have done and to acknowledge their contributions.

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 not to connect the papers to one another or to the author's paper. The goal can seem to be 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 is that one is writing for professionals in one's field, not for undergraduates or the general public.²⁴ Given that the target reader is a professional, it may not be necessary to review the literature at all. If it is necessary to review the 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 it 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 the theory section, so that a separate section 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 is potentially useful anyway if the reader wants to know about the subfield in which you are writing.

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?

In other words, your literature review section should focus on 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

²⁴ 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.

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).”

An important issue in a literature survey is what to cite, what to leave out, and how much time to spend on each. There are no hard and fast rules. But 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, one 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 one’s work often builds on one’s 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. 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 a serious scholar. When I am an 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 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 obviously 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 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 in fact 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 one 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. An author should structure her paper so that such a reader 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. 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. Often it is relatively simple when explained by itself but confusing in the context of a complicated model. Once you explain 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 they were thought of by the authors unimportant. Readers 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 the impact of papers 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 constructed the variables? What are the main patterns are in the data? How are your data are different from that present 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, recent work in economics 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.²⁵ 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 a good job covering the basic facts about boards. What is their size? What are the backgrounds of 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.

²⁵ 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.

One common problem is that authors can be 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 it seems likely that they are errors in a database. 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.²⁶ In other words, 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; either 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 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 increasingly requiring 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.

²⁶ For example of a number of well-known studies that could not be replicated, see Kewei Hou, Chen Xue and Lu Zhang (2018) “Replicating Anomalies” *Review of Financial Studies*.

The Conclusion

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 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 "more is less" 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" 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 machine learning tools to aid in their corporate governance, in particular, the way they select directors.²⁷ 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 it 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 of the ideas in the paper to other questions. It doesn't really matter if these thoughts are completely thought out. Readers like to finish papers with some additional ideas that they can think about,

²⁷ See Isil Erel, Léa Stern, Chanhao Tan, and Michael S. Weisbach, (2020) "Selecting Directors Using Machine Learning," NBER Working Paper 24435

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 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.

Chapter 7. Reporting Empirical Work

Much research, especially 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 project, the researcher 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 (or to referee); the reader knows that the author does not report information he would like to know about while at the same time the paper goes on and on about things he doesn’t care about.²⁸

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 correct econometrics and unbiased estimates of standard errors 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

²⁸ 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.

the tests 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 sound quite simple and students who read nothing but the paper often think that the research process is far easier than it actually is.

Sometimes research is as straightforward as such published papers make it appear to be, but more often it is much more haphazard. 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 seen (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 a later chapter, 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.²⁹ 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.

²⁹ 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.

A scholar reading the paper for the first time would have no way of knowing the multiple drafts, what the paper said previously or which tests are not included. We tried to present the results in the order in which they make sense intellectually; the order in which we actually did the tests did not affect this decision. When writing a draft of a paper, every author should take a step back, think about the most coherent way to present our analysis, and write it that way. Whether that presentation coincides with the history of how she actually did the analysis is not relevant in this decision.

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.

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 reports should be picked with the goal of persuading a skeptical reader that the findings are correct and the conclusions are appropriate. However, these conclusions are conditional on the choices the author made throughout the research process. Readers will wonder whether the conclusions are sensitive to the choices made. It is the author's job to persuade such readers that in the paper 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 your experimental design matters for the paper's conclusions.

With this objective 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 one could report. Some 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 (or sometimes cannot even remember) exactly what they did.

Nowadays, since such an important component of data analysis involves coding, 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.

Authors should remember that it is *their* interest that the descriptions in the paper and code are clear so that others can replicate their findings easily. If the paper cannot be replicated, all sorts of insults and accusations can go back and forth, which can lead to embarrassment or worse for the author of the original paper. Many cases in which there is a failure to replicate could be avoided if the author put more effort into documenting exactly how the work was done.

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 to conduct an empirical study concerning the sample construction, the treatment of outliers, the empirical specification, and choice of which results to report in the paper and online. Readers often wonder exactly 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, the author will want to err on the side of being too thorough with robustness checks, so as to leave no doubt in the reader’s mind. However, 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 your 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 amount of time on the paper. An author should think about the issue of what to report by trying 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, and to keep the other elements that are necessary to include such as robustness checks, proofs, etc., 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.³⁰ Along the way, he provides numerous facts about leveraged buyouts, such

³⁰ See Steven Kaplan (1989) "The effect of management buyouts on operating performance and value," *Journal of Financial Economics*, Vol. 24 pp. 217-254.

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 of them. By using his space wisely and reporting as many novel facts as possible that were of interest to readers, Kaplan was able to make his paper more impactful.

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 represents a security’s systematic 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 as most readers are likely to find this section of the paper 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. A reader will only spend a fixed time on the 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 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 that the author has to cater to. 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 learning the paper's literature or thinking of working in the area. Finally, there will (hopefully) be a few readers that read your paper extremely carefully and go through every table in detail. These readers are potentially working on a related paper and could want to understand everything in the paper. Possibly one of them could try to replicate the results. 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 paper consists of the author trying to convince readers that this result is

correct and not spurious for one reason or another and discussing the result's implications. For such a paper, I think that a sensible approach is to separate the "main" results from the robustness tests. In other words, present the estimates that you think best characterize the data. Then, perhaps in a separate subsection, go through the litany of potential objections to this test. Some results are likely to be sufficiently specialized that they are of little interest to most readers can go in 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 important 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 the order in which she presents her results. For most papers, an author will want to present a number of alternative approaches. Does she present the estimates in levels, or in first differences? Which control variables should be included? 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 effect first, and then go on for several pages about why the first set of results are not correct.

These authors would eventually get around to reporting the results with fixed effects, in what they believe to be the preferred specification later in the paper. 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. Authors often use a 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 but loses 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.³¹ I would encourage young scholars to read this book carefully and to think seriously about adding it to their libraries. Tufte's book contains numerous

³¹ See Edward Tufte, *The Visual Display of Quantitative Information*, published by Graphics Press in 2001.

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.³² 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, 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.

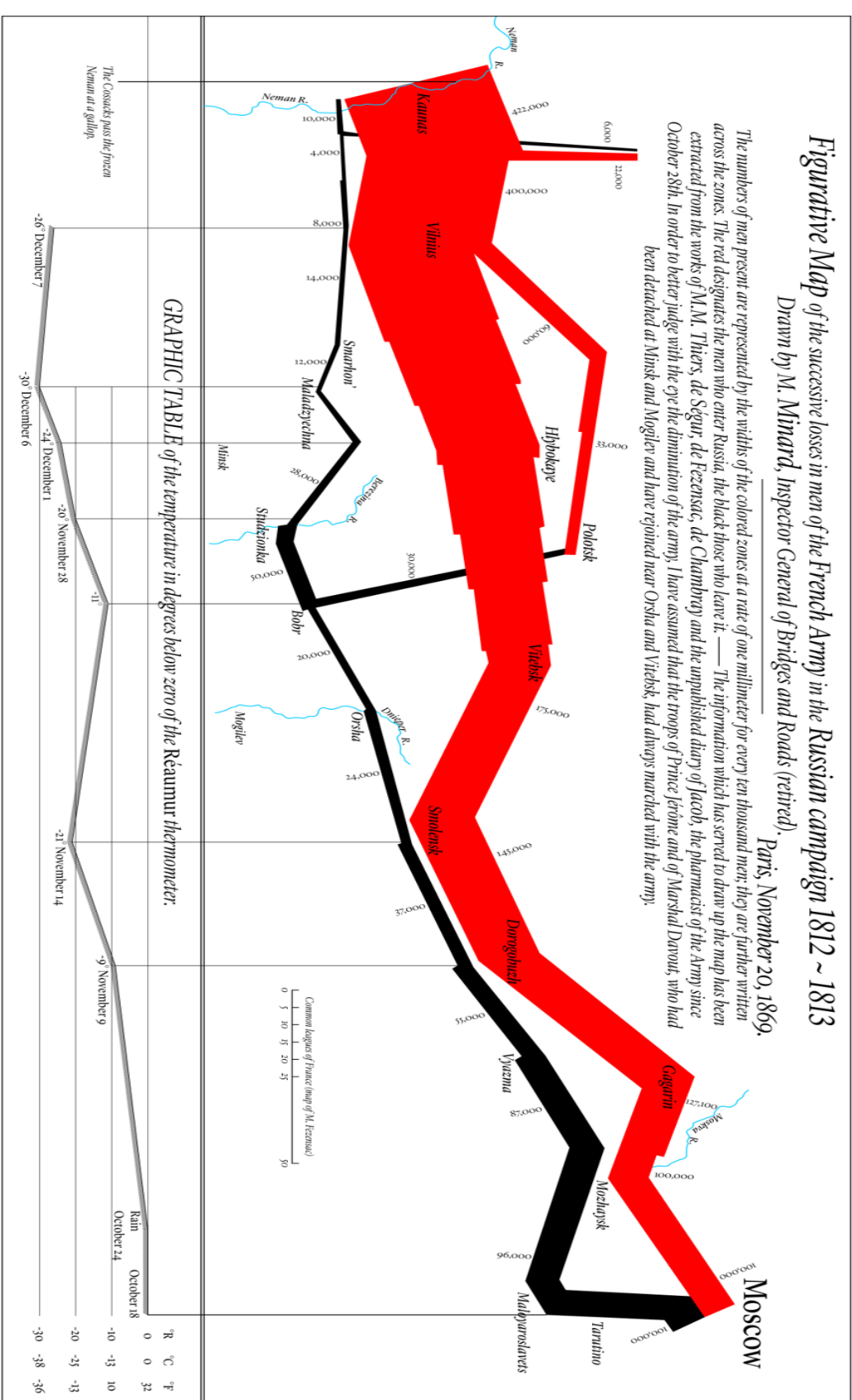
Minard’s graph illustrates, in one picture, many aspects of the story. The graph is superimposed on a map of Russia so it is possible to see the routes Napoleon took both in and out of Russia. The red line (tan in the original illustration) shows the route he took to Moscow while the black line represents 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 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 during the retreat at the bottom of the graph, which highlights the extreme cold that the troops faced from the

³² 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 to supplement his discussion of the 1812 invasion of Russia.

Russian winter. The Minard graph is celebrated 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.³³ The appendices provide texture about the details of the ownership structure, or 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.

How to interpret results

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

A common mistake that authors make is that they focus too much on statistical significance and not on the magnitudes of their estimates. For some reason, papers often make it seem that if a coefficient has a p-value of .05, it is different from zero, and if the p-value is .06, then the coefficient is not different

³³ 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.

from zero. In each of these two cases, it is more likely than not that the coefficient is not zero, but there is some small chance the coefficient equals zero. In addition, there is always a lot of noise when measuring standard errors, so reported p-values can be misleading.

When I read papers, I try to focus mainly on the coefficients themselves rather than their statistical significance level. What is their magnitude? Are they large enough to matter? 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 question about the size of the coefficients are and what they imply about the world.

In my mind (but not everyone's), it is even perfectly ok to discuss the magnitude of statistically *insignificant* coefficients while at the same time ignoring significant ones. In a well-specified equation, every estimated coefficient represents an unbiased estimate of that coefficient. Suppose the best estimate of a coefficient you are interested in is 2 with t statistic of 1. Then even though we cannot reject the hypothesis 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 that readers are likely to be interested in? It is misleading to think that it is more "conservative" and more "scholarly" to entirely ignore these interpretations of the estimates.

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. Often authors draw strong conclusions when the results confirm the author's prior view. Others are more conservative and interpret the results more narrowly. These authors 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 occurred in the economics literature 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.³⁴ The idea of these papers test is that 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 present 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 do not draw strong conclusions 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 was 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

³⁴ 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.

results. Nonetheless, in 2013, Robert Shiller was awarded the Nobel Prize for this paper and his follow-up work questioning the efficient markets hypothesis, and emphasizing 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 compete with the accepted ideas, with the reward being status in the profession for the scholar whose ideas are 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 and public speakers, regardless of their field.³⁵

Sometimes, scholars take the view that as pure academics, our work should speak for itself and there should be no need to explain it. This view, which is often stated 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?

Illustrating that sometimes writing does not matter very succinctly is a paper by John Conway and Alexander Soifer, which is supposedly the shortest mathematics paper ever written.³⁶ The original submission was entitled:

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

³⁵ 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 were recognized despite his poor presentation skills.

³⁶ 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!³⁷

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,” I think most editors are happy when referees reject badly written papers, even if the papers have other merits. 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.

But 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

³⁷ The referee’s note was “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.”

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, and it develops a new kind of “specification test”.³⁸ 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.³⁹

As graduate students, we were amused to discover that in 1973, five years prior to the publication of Hausman’s paper in 1978, there was another paper, also published in *Econometrica*, that proposed essentially the same test as Hausman’s.⁴⁰ We did not understand why this paper, by a econometrician named Wu, did not scoop Hausman’s. Given Wu’s paper was published five years before Hausman’s, why was Hausman’s paper considered so important? Why was it 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 papers, which are never Shakespearian). 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

³⁸ 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 essentially just presented a chi-squared test to evaluate 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 our econometrics paper. See Jerry A. Hausman, (1978) “Specification Tests in Econometrics,” *Econometrica*, Vol. 46, pp. 1251-1271.

³⁹ The existence an estimator that is consistent regardless of this assumption begs the question of why we care about this assumption in the first place. Why not just stick with the estimator that is known to be consistent if one exists?

⁴⁰ See De-Min Wu, (1973) “Alternative Tests of Independence Between Stochastic Regressors and Disturbances,” *Econometrica*, Vol. 41, pp. 733-750.

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 1,173.⁴¹ 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, so if Hausman had never written his paper, Wu's would have had much less impact. Today the test is commonly referred to as a "Hausman-Wu" test, or sometimes a "Wu-Hausman" test. It is likely that benefitted from Hausman's paper 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 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

⁴¹ 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.

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.

In contrast, many academics dread the “write-up” of the paper. This dread is especially common for scholars for whom English is not their native language, because writing academic papers is doubly difficult for them. These academics 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 analysis on it 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 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. However, 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 the prominent scholar's papers again. The differences 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 one makes an effort to read well-written 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 other academics' writing, and consequently become more boring and less innovative. Reading nonacademics can ground academics in the real world, and tends to lead them to use language that will be easier for nonspecialists to relate with. 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 that we all enjoy. 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 my favorite writer about the American Civil War, Bruce Catton. 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.⁴² After reading a Catton book, I feel my prose improves, although perhaps that is an illusion.⁴³

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.⁴⁴

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 could 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 a paper should be written in as reader-friendly 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

⁴² 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*.

⁴³ There are of course many other historical writers who also would provide inspiration for anyone writing about the social sciences. A very incompletely list might include Barbara Tuchman, David McCollough, Ron Chernow, Stephen Ambrose, Winston Churchill and many others.

⁴⁴ If one googles “style of writing”, one gets a classification of writing styles into 4 categories: expository, persuasive, descriptive, and narrative. Apparently, the standard view is that academic writing falls into the “persuasive” category, which to me seems to be completely wrong. Academic research does not try to persuade the reader of anything; it tries to evaluate data honestly and draw reasonable inferences from it.

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. Most successful academics, however, do not have Bruce Catton's flair, yet can still produce well-written, easy to understand papers.

One who did have such flair was Adam Smith. Not only did he start the field of economics with his *Wealth of Nations* start the field of economics, but he did it with a flair equaled by few economists since. For example, I love his famous description of corporate governance:

“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.”

This section of the *Wealth of Nations* is most famous because it is the origin of the expression “other people's money”, as well as the economics literatures on agency theory and corporate governance. It is 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”.⁴⁵

Beginning to Write a Paper

A simple way to think about presenting 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.⁴⁶

⁴⁵ No student has ever taken me up on that challenge.

⁴⁶ 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.

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 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 picking 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 seem to be

correcting in students' and colleagues' papers regularly for many years. 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 often 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 should be 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.⁴⁷

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. And I wonder when I see an author that does not proofread her paper, her life’s work, what else she messed up in her analysis?

Most writers are aware to 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, an author might write something 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*.

⁴⁷ When I was writing this last sentence, I almost committed the sin I was discussing. I thought about just saying “this” because I was feeling lazy and couldn’t think of the noun I wanted to use after “this”. It is always easier to just say “this” than to think of an appropriate noun to describe what you really mean by it.

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 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 annoy their readers.

Advice for Non-Native Speakers

To me, perhaps the most unfair element in academia is that English has become its official language. 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 in the world that is written in a field. However, it 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. To me, this is kind of amazing, since I 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 if you are French, German, Korean, Chinese, or Turkish, you are 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 doubly hard on her 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 come off as charming. However, written English is expected to be correct, and there is 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 and are increasingly in the majority in many fields (including my own).

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 they don't go through enough revisions and the papers end up getting circulated in poor shape, or worse yet, do not get circulated at all. 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.

It is a good idea for all young scholars, but especially those whose native language is not English, to seek out as much help as possible. A good practice is to find people you get along within your 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 you do for your career. If you can make it better by an investment in a professional editor, then hiring the editor is likely to be well worth the cost of doing so.

Chapter 9: Making Presentations

Our work as academics involves disseminating our research results, 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 young scholar learn how to present her work in a way that communicates her motivation, methods, and conclusions in a persuasive and entertaining manner.

One thing that some scholars do not realize is that the best way to do a presentation is not necessarily to create an oral version of the paper. Some authors essentially read their papers when doing presentations of it. While these scholars are fine with this approach, these presentations often do not do the best 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 document that represents a complete representation 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 change 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 in 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 familiar with the literature can appreciate what she has done and how it contributes to ongoing debates. In addition, she must explain how the analysis 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, as well as any other 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 she proceeds with the presentation by following the paper without much additional planning, an author 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.

The author should consider her presentation to be the opportunity to have a conversation with the local faculty (when part of a workshop visit). 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 when the author tries to explain to the audience why they should care about the paper, what the paper's approach is, and what we 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."⁴⁸

An author should have some idea how long she would like to spend on each part of her presentation. Before any talk, I think an author should have some idea of approximately what time she 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

⁴⁸ See Jesse Shapiro: "How to Give an Applied Micro Talk," available at <https://www.brown.edu/Research/Shapiro/>.

try to 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 what happens in 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 do not have time to ask the author questions about things they do not understand about the findings, so leave the seminar confused about what the author was trying to say.

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 we are expected to go to departmental seminars regardless of the topic. In addition, we all listen to many presentations at conferences and from 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. The presentations can be in an area we are interested in, but sometimes they are not. 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.

Many 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 nonetheless 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 do so is by convincing them that you have something interesting and important to say. What is important to academics varies from field to field; a scholar should know what is valued in their field sufficiently to make their paper appealing.

As an economist, I find that reminding scholars about reality usually gets people interested in the topic. 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 say. Numbers can really help; often people outside a particular field won't realize the quantitative importance of a topic until the speaker provides them with evidence. For this reason, when I talk about private capital markets to an audience of nonspecialists, I sometimes find it useful to remind them of the enormous amount of capital currently in these markets and the importance they play in the economy.

A second option is to relate the results to classic literatures in the field. In economics, starting a talk with Adam Smith or John Maynard Keynes and explain how the research builds on a question one of them originally raised tends to wake people up and to realize that the speaker is talking about a first order question. 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. In economics, an organization called the National Bureau of Economic Research puts on prestigious conferences in most subspecialties where the audience consists of 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 substantial 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, we sometimes have the opportunity to explain our research to practitioners, who don't normally need much institutional background information. 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, and 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 about what the author has done with the research such as the sample period, sample size, estimation procedure, etc. Having this information on the screen can be valuable to the speaker, as it avoids questions about basic information that interrupt the flow of the presentation, and leaves more time for discussion of more substantive issues.

An equally important role of slides to information provision is that slides help the presenter focus the audience’s attention where she wants it to be. A well-designed slide deck can help a speaker maintain control of the room, which 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. One way of maintaining control of the room 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 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. I, like most faculty nowadays, 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 I assign that I think is 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.

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 can 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 audience members focus 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, and use too many slides, sometimes having one for every result in the paper and every possible objection that someone could come up with. Too much material on each slide leads the audience to spend too much time reading the slides rather than listening to what the author 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, so will interrupt the speaker asking what the slide says. 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 sounds 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 try it out. 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. Do what you can to make the slide more readable and the font larger. 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 something 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. 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 if someone in the audience raises them. Some people 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.

One issue that scholars, especially young ones, can worry about is the software package used to create the slides. As I write this paragraph, the most fashionable package is called *Latex*, although by the time this book is published, 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 they *have* to use *Latex* rather than *Powerpoint* or any other program that the author is comfortable with. One junior faculty member told me that she was concerned that people would think worse of her if she didn't use *Latex*, presumably because it would mean she is out of date. Personally, I can't imagine why these things should matter. Remember, a speaker's goal is to convince people that her research is interesting and important. So if people are talking about her slides and not her analysis, she has lost the game. An author should try 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 set. 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; we 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 one's own 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 is a speaker can do 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 random calculus problems from the last time the classroom was used. In those cases, the speaker felt like a guest and didn't think it is appropriate to erase them, so the audience had to spend the whole talk staring at irrelevant material that is on the blackboard. 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 has to remain in charge of all aspects of the talk, including the room, the tenor of the discussion, and the extent to which she is able to get through her talk without interruptions.

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. Moving around rather than standing still can keep the audience's attention from wandering. A speaker should try to talk like she is talking to someone using simple language. For example, rather than saying that x and y are positively correlated, she should say when x goes up, y also goes up. Finally, a fun way to make the talk more personal is to use the audience as examples. If one is giving a talk about an entrepreneurial firm, the speaker can pick someone from the audience, often the department chair or a senior faculty member, and say something like: "Suppose Joe (the department chair) has started a firm and wants to..."

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 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 well worth the cost.

It is important to use one's time 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 paper. 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 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 about the issues in her paper deeply 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 in a presentation, 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 explain why she did what 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 why she decided to structure the analysis the way she did.

If possible, an author should discuss what the results would be if she used alternative approaches, how sensitive the results are likely to be to various choices she made along the way, and why she thought the approach that she did use was superior to the alternative they are suggesting. 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.

Often the problems in answering questions come from not listening carefully to the questions that are asked. Sometimes, the person will ask a simple explanatory question, and the author will not understand exactly what is being asked, assume it was the one question she dreaded, and give an overly defensive answer. If one 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 and to look at the person who asked the original question and direct my answer to him.

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 offline.” 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 be to avoid turning the seminar into a fight to the extent possible, so the author should answer questions calmly and politely no matter how aggressive the questioner’s tone. If possible, defer questions and suggest that you 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 who likes to hear himself talk. He went on and on about various things during the talk, making it difficult for me to get a word in 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. Unfortunately, many faculties have one or two professors who go overboard with the way they probe a speaker’s analysis, causing unnecessary problems for seminar speakers.

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, while 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.⁴⁹ 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 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. 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 is the audience will be interested in. They should present their view on the issues addressed by the paper,

⁴⁹ Sometimes the same person who gave this discussion will subsequently referee the paper for a good journal. After proclaiming it “great” in public, she will tell the editor privately that it is kind of stupid, and recommend rejection.

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 contribution and where it fits into the literature. 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 reasonable approach 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. At this time, authors often go into long, detailed diatribes about why the discussant is wrong, and list all the things they have done to address her concerns. Everything the author says might be correct but it doesn't matter. By this point no one is listening. The attendees normally have made up their mind about the paper, and are

anxious to go to the coffee break or to hear the next paper. Even after a negative discussion, I think it is better to thank the discussant, and to just mention one or two things the discussant has said that were misleading or incorrect. One wants to fight back after a negative discussion, but the problem is that the audience is rarely listening by this point. For this reason, it is a much better strategy to thank the discussant, to point out any errors quickly, and to try to do a better job of anticipating the discussant's criticisms when revising 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 read. 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, often 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 and want to learn the

paper well. These scholars might have a competitive paper, or could be thinking about doing research that extends the paper.

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 just give the author 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 what the paper's goals, general approach, and results are. 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 they won't think are 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 potential problems with my analysis, although occasionally they are things that people like about my paper that people felt I did not emphasize enough.

The goal when an author distributes papers is to learn what the common reaction is to one's paper without exhausting all of the feedback one 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 mostly like to read it and give comments. Then to make revisions that address the important issues raised in them, and send to a few more people. Continue the process, addressing one set of comments and 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.⁵⁰

I recently read an interesting paper, which I was given a copy of early in the paper's life before the authors received 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 be more valuable to the authors than if the comments had merely repeated what I had already told the authors.

Because of examples like my experience with this paper, I always send my 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 RAs; my RA normally will not just find typos and awkward sentences, but will make 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 people 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 go 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.

⁵⁰ This process can bog down if one waits for feedback from a particular person and that person doesn't get to the paper for a long time. It is important to adjust for such delays and not to let them slow down the revision process too much.

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, the authors often have human capital reasons to speed up the research process as much as possible. Doctoral students' job markets are improved if they have established research that appears likely to be published soon, junior faculty come up for tenure at fixed 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 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 do so is to reciprocate and to provide useful feedback to others. 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 One's 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 of course 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.⁵¹ 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. Particularly helpful is the time I spend before the talk doing the preparation. 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 writing them. In addition, my colleagues can be

⁵¹ 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. Even the faculty who are on the keto diet so bring their own lunches don't use brown bags. I'm sure our doctoral students have no idea where this strange term comes from.

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 normally have more invitations than 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 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 schools are much more likely to invite someone they know likes to visit China than someone who never travels. 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 rapidly. In my field, 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 very difficult for someone who is not established and does not know the conference organizers to have their papers accepted.

I encourage young scholars, despite the long odds, to submit their papers to conferences. The optimal 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 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 travel to too many conferences.

Mass-Mailing One's Research

An easy thing to do to publicize one's research is to send copies of the work to people who do related work. Even before I post a paper publicly, 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 valuable 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 an event on the paper's topic.

I am sufficiently established that I usually know the people I am writing to 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 and make sure that the leading scholars in one's area know about the 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 One's 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, regardless of whether the author has updated the paper. 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 and haunt the author when new readers bring up an problem 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 have heard from others who have. A number of faculty are extremely 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, this type of criticism is often misplaced, and could 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 can be furious 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 or worse to the author.

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 online. If I present an early draft at another university, I often request that they distribute 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 drafts be distributed as hard copies rather than by email to ensure that no one forwards that draft 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, and I enter papers that are ready to be online in this series. 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. I am a Research Associate of an organization called the National Bureau of Economic Research, and enter all my papers into their working paper series. 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 also advertise my papers whenever possible on other people's blogs, especially one on corporate governance that is put out by friends of mine who teach at Harvard Law School. Once a paper is online, I do whatever I can to maximize the number of people who see it.

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 the paper 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 academics in the 21st Century. I do not understand why some academics 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 hire for us. This person probably would have accepted an offer from us and would have loved the job. But when my colleagues googled her name, they didn't find her website, and then forgot about my suggestion. We ultimately hired someone else for the position.

When to Submit to a Journal

One issue that young scholars often wonder about is the appropriate time when they should 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 customs 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.⁵² 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 a paper that uses standard methods to address an important question, she probably would not waste her time submitting to *Econometrica*.

My view is that the right way to think about journal submissions is as a continuation of the revision process. When an author writes a new manuscript, she first sends it out to close friends, and revises 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.

There are some factors that can cause her to speed up or slow down this process. One is the novelty of the work, and 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.

⁵² 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 also human capital 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 do not take their tenure decision into account when handling their papers. So they could 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. 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*) or 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",⁵³ and publish the most well-known papers. They are also valued extremely highly by universities when making promotion decisions; many

⁵³ 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.

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 very well-executed, 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 schools 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 sending 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 perfectly good but specialized dissertations, should probably have submitted their papers to more appropriate outlets. In part because they submitted to the higher ranked journal and consequently lost valuable time, and some of the papers never ended up getting published.

The most significant cost of a submission is the time it takes.⁵⁴ 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

⁵⁴ 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.

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, as 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 a long time, many economists were hostile to behavioral research and it was pointless to submit a behavioral paper to a journal with an anti-behavioral editor that would not take the paper 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 is 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 can be 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 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 almost certain 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 process, it usually works pretty 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 our time with the review process in one way or another. When one starts, preparing one's papers for submission, revising them in response to referees' comments, and writing reports on other people's papers, all take up a large fraction of our time not spent in the classroom. When one becomes more senior, editing journals, handling appeals, and addressing issues involved with the management of journals such as the choice of editors and associate editors, policies toward data disclosure, etc., take even more. Even aside from the amount of time one spends on her own papers, interacting with journals in one way or another can take a few days of every *month* for a research active scholar.

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 review process aids 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.⁵⁵

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 young scholars can take revising too far. I know assistant professors who waste an incredible amount 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 5 times, then make a small change and rerun them again a few more times. It is good to redo the analysis from time to time to ensure there are no mistakes, but after a while there is no point to rerunning things to convince oneself that one is being productive. These young scholars become so afraid to send the paper off that they continue to come up with an endless number of pointless things to do prior to submission.

There is a time when one must send the paper off to face the editors and referees. 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.

⁵⁵ 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>.

There are some things that do matter, that can substantially affect the response an author receives from a journal. The writing, especially in the abstract and introduction, is incredibly important. The 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 it is the only part that will be looked at by many readers. Referees, although they are supposed to read the entire paper carefully, can end up being some of those who never make it past a paper's introduction if they are not impressed by 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 7 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 didn't place any limitations on the number of pages in each issue; I think the truth was that our publisher, 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, potentially an online one. The key issue in determining whether a particular piece of analysis should be

included in the main text or in an appendix is whether a typical reader would find it interesting or distracting. If most readers find it distracting, the author should move it to an appendix. If the referee turns out to be one of the few people that really care, the author can always move back to the text.

A few years ago, one of my students had a job market paper that I really loved.⁵⁶ 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, as any good student would. 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.

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 costs unless they have the option to publish the paper if they like it.⁵⁷ 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.

⁵⁶ I still do. I teach it in my doctoral class every year.

⁵⁷ Since law reviews are edited by students, the opportunity cost of reviewers’ time is low. 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.

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, and the major cost to a submission, aside from the emotional cost of rejection, is the time it takes.

However, many scholars substantially underestimate the cost of their time. In economics, a journal submission can sometimes take more than year for a response. During that year, others will continue to work on competitive papers that can subsume the results in the paper, and 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 takes emotional energy, and the author is likely to be spending time thinking about changes she could make to the paper, what the referees are likely to ask for. Sometimes the time reflecting on a paper while it is at a journal can help the author put the results into a better perspective, but often it is wasted while the author worries about who the referee will be and what he will say.

I think that an author should try to form a realistic expectation of whether a paper has a real shot at a journal before submitting it. Doing so can be difficult as it requires accepting the limitations of one's papers. If an author thinks a paper of hers is very good and she has received positive feedback on it in presentations, then she should definitely try a top journal. Publications in top journals are very valuable, especially for junior faculty who will be up for tenure soon. But be aware of the time and energy involved with the submission process. If it is having problems at the top journals and a lower ranked journal is likely to take it without too much effort, then perhaps it makes 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 when she submits 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 the time of submission. 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.⁵⁸ 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. 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 competitive paper to the author's, or a number of other reasons. Another reason for not choosing the editor whose research is closest to that in the paper would be if a different editor has shown an interest in the issues the paper addresses, so would be a sensible person to suggest.

When I was editor, I almost never read the cover letters accompanying the submission. I skipped straight to the paper and read it quickly when I selected the referees. However, other 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 does 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.

⁵⁸ 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.

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.⁵⁹ 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. More often, he will assign the paper to one of the journal’s other editors to handle the paper, or he could decide to handle it himself.

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. When the paper hits the editor’s desk, he can desk reject the paper or send it to referees. 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 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* have policies of desk rejecting over half of the papers that are submitted.⁶⁰

Assuming the editor decides to send the paper out for refereeing, he 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 choose referees who do research related to the paper, so understand the paper’s contribution, as well as what the paper adds to the literature relative

⁵⁹ At *Review of Financial Studies*, we adopted a minimum font size because one author submitted 4 or 5 papers every year that were close to 100 pages long in a very small font. Needless to say, none of the editors looked forward to handling his papers.

⁶⁰ But not my papers. Every time I have submitted a paper to *QJE*, the editor has sent it to referees and then rejected it.

to what already is known. If there are multiple referees, editors try to pick people with diverse backgrounds and skills. For example, if a paper has both theory and empirical work, an editor might ask one scholar who is better at theory to be one referee, and also another to be someone who specializes in empirical work. Or he might ask a more senior referee to provide perspective on the issues the paper addresses together with a younger referee who has the energy 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 scholars are much more likely to get referees more trusted by editors. These trusted referees tend to be more senior scholars who tend to have better perspectives on research, and also to be more lenient towards authors. While there are exceptions, more senior referees are less likely 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.

It is definitely true that more senior scholars have advantages in publishing their papers over more junior people. One reason is that they are more likely to get established and sympathetic referees. Junior and less well-known scholars can have a harder time publishing their work than more senior, well-known scholars, even if they submit similar quality papers.

Referee Reports

After an editor decides on which referees to use for a submission, he 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 since 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' name, 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, they try to do a good job.⁶¹ Reviewers want to help the editor make better decisions and the author improve her paper. So, they read the paper carefully and try to help them out. 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 referees' reactions to them and can substantially increase the likelihood that they will be accepted for publication.

A referee report consists of two main parts. First, the referee gives the editor advice about how he 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.

⁶¹ 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 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.

The second part of the referee report consists of advice to the paper’s authors. The referee first summarizes the paper, and gives an overall evaluation and recommendation regarding the paper’s publication. Referees are also expected to make suggestions to help the authors improve their papers. The quality and quantity of suggestions that referees provide vary widely. Sometimes referees provide detailed comments taking 5 or 6 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 it as good as possible prior to publication. When they are going to reject a paper, they still want to help the authors improve it, but the suggestions they give the authors are less important from the referee’s perspective. The paper won’t be published in that journal and the authors might ignore them anyway. Consequently, on revision requests, referees usually try to be extremely thorough, while on rejections, their reports can be more brief.

When the Editor Gets the Paper Back from the Referees

Eventually, all the referees return their reports to the editor.⁶² At this point, the editor must decide how to handle the paper, whether to reject the paper, to ask for a revision or, in rare circumstances, to

⁶² 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.

accept the paper as is. Most of the time the decision is fairly easy. At a good journal, the majority of papers submitted are clear rejections. An editor can tell that a paper 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 there are errors in the proofs or that the authors used inappropriate statistical techniques. Rather, for most rejected papers the referees and editors decide that the scope of the analysis and the paper's incremental contribution is not large enough. Journals compete with one another to have the most impactful papers, and editors try to publish papers that will increase their journal's reputation. Most rejections occur because editors and referees judge papers to be ordinary, and not likely to be sufficiently impactful to help the journal's reputation.

When an editor rejects a paper, he will write a short note to the authors and attach copies of the referee reports. Some editors reject papers using "form letters" that are the same for all rejected papers. When I was 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 hopefully, 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 papers that the journal is likely to publish. He must decide to reject the paper, to invite a resubmission, and on rare occasions, accept the paper essentially "as is". On a paper that the editor is thinking seriously about publishing, he should give the paper and referee reports a careful read himself. 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 he will reject a paper that referees like and sometimes he will invite a resubmission on a paper the referees recommend rejecting.

Different editors have different views about resubmissions. Some are lenient on the first round and reject a lot of papers after they are resubmitted.⁶³ 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 he could tell the author she has to dramatically increase her sample size, 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.

When I was editor, I wanted to avoid rejections at later rounds, so 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. I always used tough language in them, stating clearly what the authors had to do before the paper could be accepted, and emphasized that I had the right to reject the paper in the future. But the authors I dealt with 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

⁶³ 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.

Today, authors hear news from journals via an email with a “subject” 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 will hear will be upsetting. The vast majority of submissions to good journals are rejected. After 30 years of submitting papers to journals, getting rejected is still not easy. 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, despite the fact that these same reports often end up being extremely helpful.

The thing one has to remember when one looks at 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 another journal with an improved paper. If the editor gives the author the option to resubmit her paper, it is good news even if it is accompanied by a lot of negative verbiage about the analysis. The author has a direction to take the paper that has a shot at being a publication, and probably will improve the paper as well. 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.

Rejections

Most of the time, when one submits a paper to a journal, it is rejected. Top journals in most fields reject over 90% of submissions today. These rejections are usually very painful for the authors. Their papers represent their life’s work, and the authors put their heart and soul into the papers. To have them rejected in a seemingly cavalier manner by editors and referees that don’t seem to take them seriously 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 the paper is not appropriate. Other journals are not as aggressive about desk rejections, but their editors still know before they send papers to referees which 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 and low costs to trying. Submission fees are usually trivial and often paid by grants or research budgets, and the few months wait is inconsequential. 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 prior to submitting to a more specialized journal. The puzzle to me is why, when an author submits to a top tier journal not thinking it likely their papers will be accepted, she is so often up upset when the paper is rejected.

Authors tend to have inflated views of their own work. The same scholar who is a trusted resource for colleagues or editors who wants objective opinion on a research-related topic, can think his own work is far more important than it actually is. Perhaps persistence and overconfidence are some of the qualities that make people into successful scholars. The tendency of scholars to overvalue their own work can lead them to submit to higher ranked journals that are warranted by their papers.

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 upsetting to hear that others do not like the paper one has been working on for years. The first reaction of many is to call or email the editor and to ask for a better explanation for the rejection than is in his letter. It would be great if the editor would change his mind about the decision. However, no matter how much she wants to, an author should not call or email the editor. If she does, the editor will not change his decision, but might lower his opinion of her.

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, and let 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.

When dissecting the report, it is important to focus on the extent to which the referee's points are correct, and what in the report can be usefully incorporated into the paper. Unlike when a paper is a "revise and resubmit", there is no requirement to address each point. If the author thinks the 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. The referee will not be happy if he spent a lot of time writing a report to help the authors, and the authors ignored his suggestions and submitted the same paper to another journal.

More likely, however, is that the negative report is symptomatic of something that is wrong with the paper. If one referee had objected to something about the paper, there is likely to be something lacking in the way the authors explained. What led to the referee's faulty interpretation of 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 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 these types of questions. Authors sometimes have opinions about their work that differ from others' about 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, which hopefully can make it more attractive to future readers.

Revise and Resubmit

When an editor likes a paper, he rarely accepts the paper 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. The term that has become accepted for this type of reply from a journal is "revise and resubmit." On a revise and resubmit, editors can be very explicit about the way they want the paper to be modified, saying exactly how they think the authors should restructure, what referees' points they think are most important, and what he thinks are the appropriate inferences for the author to draw from her analysis. Alternatively, the editor is sometimes vague, saying something like: "Your paper would be quite nice if you can solve this problem. I have no idea if you can solve it 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 the editor raised in his letter.

In either case, 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. Ideally, an editor's letter on a revise and resubmit is essentially an implicit contract in which if the author does what is expected of her, the editor will eventually accept the paper for publication. Unfortunately, some editors don't view the process as an implicit contract, and can sometimes ask for substantial revisions and then reject the papers anyway.

Revise and resubmits 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 his favorite alternative interpretation of her results, add a few extra tests, or cut a section that seems superfluous to him. 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 he 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 revise and resubmit, in which the authors have to rethink a number of parts of their existing analysis and perform some new work. These revisions can be sufficiently extensive that they can take up to a year for authors to complete.

When an author receives a revise and resubmit, 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 difficulties.

Appeals

Sometimes, however, referees do make mistakes. They are human beings, have biases and particular viewpoints, and writing referee reports usually does not come high on their list of priorities. Editors realize that the process is imperfect, and they really do want to be fair to authors and at the same time publish the best possible papers. 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 get overturned by this appeal process. At *Review of Financial Studies*, we found that the acceptance rate for appeals was surprisingly similar to the acceptance rate for initial submissions.⁶⁴

That being said, I would recommend only appealing decisions when there is a clear mistake in the referee's analysis. Some authors regularly appeal rejections. Editors know who these people are and find

⁶⁴ Probably the reason for this puzzling pattern is that authors only appealed rejections that were close calls.

their constant appeals annoying. But editors do want authors to appeal decisions that are based on incorrect reasoning. They really do want to make the right calls on their publication decisions.

I have only appealed 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 he/she misunderstood what we did and thought the identification we used was problematic. I first screamed at my old friends for a few days (I get really upset after rejections as everyone else does). 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 since it was probably too strongly worded (I was pretty mad). We then sent the letter to the journal. The editor sent it to the referee, and to his/her credit, the referee immediately realized the mistake. After that, we fairly quickly converged to a version that the referee thought was acceptable and it 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 “response document” we write when resubmitting our paper. I liked the approach so well 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 they were fine with. They can focus on the way in which the authors responded to what they asked, which is available to them in an easy to read format in the response document

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 (sometimes omitting the verbiage/summary at the beginning but including all the substantive comments). 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 (full sentences, no slang or jokes, etc.). 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 an author must respond to. 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 revise and resubmit, 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. In addition, there can be some suggestions 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 respond to a revise and resubmit? 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 more boring to most readers? However, she does not want to appear nonresponsive to referees and editors who spent a lot of time making suggestions and have the right to reject her 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 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 appearing to a reader like you think they are legitimate, and satisfying referees who want you to push the paper in different directions? The trick is that although the authors must address every comment in the referee reports and editor’s letter, the authors have a lot of freedom about where they address each point and the way in which they do so. Authors should take advantage of space in appendices and the response document to supplement the actual

document. The idea is that authors should strive to make the revision an opportunity for genuine improvement to the paper and not just satisfying referees for the purpose of publication.

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 believe ourselves will improve it. If a referee makes suggestions that improve our paper, we, of course, will incorporate them and put them in the paper. Especially important to include in the paper are replies to concerns that appear to be common to a number of readers and are 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 particular to one referee and are unlikely to matter to most readers, my coauthors and I will often reply directly in the response document. My response documents frequently contain a number of tables intended to alleviate referees' concerns. Since space in the actual paper is sufficiently valuable, it often makes additional tests that might not be of interest to a general reader in the response document, where a referee hopefully be persuaded that a concern of his is not all that serious.

If we 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. I think 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 the more than 30 years I have been a professor, I have to this point published 58 papers, the vast majority of which were refereed. I have made this offer to referees and editors many times over the years – not once has an editor or referee ask that I move something from the response document or an appendix to the body of the paper.

In a typical revise and resubmit, 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 why the concern raised by the referees isn't really a problem 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 going to be insistent, 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 it 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 the calculation; I wanted readers to understand that it wasn't our decision to include it 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 processes these reports like on the first round, and 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 a higher ranked journal rather than resubmit to the original one. They can always resubmit to the original journal if they get rejected by the higher ranking one. There is nothing wrong with this strategy. On rare occasions it works and the paper is published in the higher ranked journal. Personally though, 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. Recently I had a referee tell me at the second round that the paper is much improved from the initial submission that he could finally figure out what my coauthors and I were doing. But after doing so, he realized that he didn't like what we did, so he rejected the paper. Needless to say, I wasn't very happy to get this report.

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 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,

but never did publish his job market paper. By the time this saga was over, others had written papers that were sufficiently similar to his that he decided his paper wasn't worth publishing anymore.

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, and will want the references done in a particular way. For example, some will want section headers in bold, others in italics, etc. Authors must comply with these kinds of suggestions from the copy editors and usually have no reason not to. But English changes designed to improve the paper's prose are strictly optional. I usually give the copy editor the benefit of the doubt and so I don't care, I do it his way. But I never give in if I prefer the prose the way I wrote it to the copy editor's version. It is my paper, not hers. No editor will unaccept the paper he previously accepted because of a disagreement between me and the copy editors.

After copyediting, the last stage before publication, authors are sent page proofs. Journals usually will wait 6 months to a year to get the authors page proofs and then want a response in 48 hours. This deadline can be ignored – editors do not unaccept papers because of missed page proof deadlines. 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, which does not have any negative consequences for authors whatsoever.

In the olden days before word processors, journals used to have to literally retype papers before they were published. So, authors had to go through every word and number in their paper to ensure that there are no typos. 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 this point.

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 papers back to the journals 3 or 4 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 always ask to have the journal send a page proof back 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 from now. There is clearly a misalignment of incentives, and is in the authors' (and journal's) interest for authors to be persistent and to get things right.