

# DISCUSSION PAPER SERIES

DP16845  
(v. 2)

## **The Purpose of a Finance Professor**

Alex Edmans

**FINANCIAL ECONOMICS**

**CEPR**

# The Purpose of a Finance Professor

*Alex Edmans*

Discussion Paper DP16845  
First Published 28 December 2021  
This Revision 31 December 2021

Centre for Economic Policy Research  
33 Great Sutton Street, London EC1V 0DX, UK  
Tel: +44 (0)20 7183 8801  
[www.cepr.org](http://www.cepr.org)

This Discussion Paper is issued under the auspices of the Centre's research programmes:

- Financial Economics

Any opinions expressed here are those of the author(s) and not those of the Centre for Economic Policy Research. Research disseminated by CEPR may include views on policy, but the Centre itself takes no institutional policy positions.

The Centre for Economic Policy Research was established in 1983 as an educational charity, to promote independent analysis and public discussion of open economies and the relations among them. It is pluralist and non-partisan, bringing economic research to bear on the analysis of medium- and long-run policy questions.

These Discussion Papers often represent preliminary or incomplete work, circulated to encourage discussion and comment. Citation and use of such a paper should take account of its provisional character.

Copyright: Alex Edmans

# The Purpose of a Finance Professor

## Abstract

The academic finance profession has the potential to be uniquely purposeful due to four characteristics – the freedom to take risks and work on what we're passionate about, the loyalty to our profession rather than just our institution, the collaborative nature of the creation and dissemination of knowledge, and the magnitude of our potential impact. However, what the profession currently values, and its current social norms, are significant barriers to the fulfilment of this potential. This article highlights the special features of our profession that we often take for granted and ignore, and proposes ideas to make it not only more impactful and relevant, but also more collegial and fun.

JEL Classification: A11, A13, A14, A22, A23, G00

Keywords: Purpose, Citizenship, collegiality, Research, teaching, impact, Innovation

Alex Edmans - aedmans@london.edu  
*London Business School and CEPR*

### Acknowledgements

This article is based on my keynote speech at the 2021 Financial Management Association Annual Meeting. I am grateful to the FMA and, in particular, the VP-Program Michelle Lowry, for inviting me to give the talk, and to Utpal Bhattacharya, the Executive Editor of Financial Management, for his interest in publishing a written version. For helpful comments, I thank Kenneth Ahern, Jack Bao, Nick Barberis, Taylor Begley, Jonathan Berk, James Choi, John Cochrane, Vivian Fang, Jill Grennan, John Griffin, Moqi Groen-Xu, Laurie Hodrick, Cliff Holderness, Wei Jiang, Tim Johnson, Marcin Kacperczyk, Ralph Koijen, Cami Kuhnen, Michelle Lowry, Dimitris Papanikolaou, Luke Taylor, Ed Van Wesep, Toni Whited, and David Yermack.

# The Purpose of a Finance Professor\*

Alex Edmans<sup>a</sup>

*London Business School, CEPR, and ECGI*

Current draft: December 31, 2021

## Abstract

The academic finance profession has the potential to be uniquely purposeful due to four characteristics – the freedom to take risks and work on what we’re passionate about, the loyalty to our profession rather than just our institution, the collaborative nature of the creation and dissemination of knowledge, and the magnitude of our potential impact. However, what the profession currently values, and its current social norms, are significant barriers to the fulfilment of this potential. This article highlights the special features of our profession that we often take for granted and ignore, and proposes ideas to make it not only more impactful and relevant, but also more collegial and fun.

Keywords: Purpose, citizenship, collegiality, research, teaching, impact, innovation

JEL codes: A11, A13, A14, A22, A23, G00

---

\* This article is based on my keynote speech at the 2021 Financial Management Association Annual Meeting. I am grateful to the FMA and, in particular, the VP-Program Michelle Lowry, for inviting me to give the talk, and to Utpal Bhattacharya, the Executive Editor of *Financial Management*, for his interest in publishing a written version. For helpful comments, I thank Kenneth Ahern, Jack Bao, Nick Barberis, Taylor Begley, Jonathan Berk, James Choi, John Cochrane, Vivian Fang, Jill Grennan, John Griffin, Moqi Groen-Xu, Laurie Hodrick, Cliff Holderness, Wei Jiang, Tim Johnson, Marcin Kacperczyk, Ralph Koijen, Cami Kuhnen, Michelle Lowry, Dimitris Papanikolaou, Luke Taylor, Ed Van Wesep, Toni Whited, and David Yermack.

<sup>a</sup> Email: [aedmans@london.edu](mailto:aedmans@london.edu), London Business School, Regent’s Park, London NW1 4SA.

## 1. Introduction

I'm grateful to you for even starting to read an article with such a title. Titles like this jar with many readers, and for good reason. The author typically writes as if he's made it – found the elusive meaning of life that no-one else has, and knows the one best way to pursue a vocation that he'll now dictate to the uneducated masses.

That's not my reason for writing. So why this topic? I was honored to be invited to give the keynote address at the 2021 Financial Management Association Annual Meeting. A keynote is a great opportunity to publicize your research, but I realized that many audience members might just not be interested in my research – they may work in other areas. And, as I looked at the highly intimidating list of past keynote speakers, I realized my research is, quite frankly, not as good as my predecessors'. Some have won Nobel Prizes; most have been Presidents of the American Finance Association (“AFA”).

And that's perhaps my only advantage. Giving a talk about purpose preaches well from the pulpit when you've made it to the very top and can afford to invest in citizenship, but seems unrealistic when you're still climbing the ladder. It's easy to wax lyrical about taking risks with your research if you've never had a paper rejected in your career, as is the case for at least one past keynote. I've been in this profession for fewer than 15 years and been a full professor for under 10. So, many things I said in that keynote were issues I often struggle with myself, wrestling with the purpose of this profession when I've had another paper rejected or am dealing with an apparent injustice (where the other party is always wrong, of course). I was saying them to myself as much as to the audience.

I'm very grateful to *Financial Management* for inviting me to write up that talk. Doing so has allowed me to go above and beyond that speech, by incorporating the comments from many colleagues in the profession who saw the talk or read an early draft of this article. I thank all of them for their input – but none of them should be blamed for the output.

There are many generic talks on purposeful careers that aim to apply across all professions; I've given such a talk myself as a Gresham College public lecture (Edmans (2019)). This article will thus focus on four dimensions specific to our profession. Some features may apply to other academic disciplines beyond finance, and a few even to non-academic professions. However, the focus of this article is the purpose of a finance professor.<sup>1</sup>

The first unique dimension is the freedom to work on whatever topic we're passionate about. Ours is a profession of hard knocks, with multiple rejections and harsh discussions, but if we're working on what truly excites us, we're willing to pick ourselves up and persevere. It also means that the years invested in a paper were well spent, even if you're disappointed by the eventual publication outcome. We also have the freedom to work on open, unexplored questions, even combining insights and methodologies from outside of finance.

But many researchers don't leverage that freedom. Common advice is to research what's hot, even though this may lead to marginal contributions in a crowded area. Moreover, the paper-counting nature way in which we're evaluated encourages us to work on popular fields, to maximize the probability of a top publication, rather than taking risks and trying something new.

The second dimension is the significant amount of luck in our profession. A publication may depend more on the referee draw rather than the paper's inherent quality; a chance encounter leads to a productive coauthorship; bad luck means that someone else is working on the same idea. Of course, skill, creativity, and hard work matter, but we underplay the importance of luck and overplay the signal contained in a single publication, leading to path dependency. A lucky publication as a PhD student significantly improves your job market outcome; quickly publishing your job market paper

---

<sup>1</sup> Similarly, this goal of this essay is not to provide every potential suggestion on how to improve the profession. There are already excellent articles on topics ranging from the editorial process (e.g. Spiegel (2012), Welch (2012), Hirshleifer (2015), and Hirshleifer, and Harvey (2020) to data mining (Harvey (2017)). This article focuses on improvements related to the purpose of our profession (while recognizing that there is some overlap with these other areas).

can lead to a move “up”. We draw substantial inferences from a person’s institution – indeed, we may decide not to talk to someone at a conference because of the school on their nametag – so it affects future outcomes.

Path dependency now extends to our PhD students. To get into a good program, you’re advised to have a masters, undergrad RA experience, and even a predoc. As with the publication process, we’re excessively conservative and go for safety rather than innovation. Someone with non-academic experience, or who only discovered their passion for research later in life, may have diverse skills useful for innovative work.

The third dimension is the non-rivalrous nature of our profession. In an M&A deal, only one acquirer can win the auction; one investor’s trading profit is another’s loss. In contrast, journal space is not finite, particularly given the move to online journals, and our loyalty is to our profession not our institution – we trust the recommendations of “rival” schools when deciding who to tenure at ours. We can thus view other academics as our colleagues in the creation and dissemination of knowledge – providing feedback on their papers and sharing their work.

However, some faculty have the “pie-splitting mentality” that the profession is a fixed pie, so the only way to increase their slice is to reduce others’ – by trashing them on anonymous message boards or failing to acknowledge their research. But reducing our colleagues’ slice doesn’t mean more for us – instead, the pie will be taken by companies and consultancies, who are increasingly releasing sloppy research that claims what practitioners want to hear and thus boosts their image. The general public, which includes donors, are increasingly skeptical about the value of academic research, believing we can’t agree on anything. While there are legitimate disagreements (e.g. different methodologies reaching different results), we focus too much on them rather than highlighting the substantial areas of academic consensus, leaving a big open goal for consultancies to shoot into.

The final dimension is the potential impact of our work. All professors create and disseminate knowledge, but we do so in *finance* – which affects how companies invest and fund themselves, how citizens save for the future, how agents share risks, and how resources are allocated to their best use. Yet the profession places very little weight on either teaching or impact outside academia in tenure or hiring decisions. And most teaching ratings reward entertainment and popularity rather than challenging and stretching students.

I'll use a number of examples from my own career to illustrate my points. This is not to suggest that my career is the right way of doing things, nor that we can extrapolate from it to form general principles, but instead to make certain points more concrete. Some are instances when I've stumbled upon purposeful behavior, but this is not to imply that I always do things the right way. Many things I do, I get wrong, but there's greater pedagogical and aspirational value in giving examples of what works than what doesn't.

## **2. What is Purpose?**

A big chunk of my current work is about corporate purpose. In Edmans (2020) I explain how a company's purpose has two unique features. First, a purpose is about serving wider society, not just maximizing profits. Second, even though a purpose is genuinely altruistic, purposeful behavior may unexpectedly lead to a company becoming more profitable in the long-term – employees may become more motivated, customers more loyal, and governments less likely to pass restrictive regulation.

The same two principles apply to a professor's purpose. A “strategic professor” will focus exclusively on advancing her own academic standing. This might involve creating some public goods, but only for instrumental reasons – where she can, at least roughly, forecast an ultimate benefit to herself. A “purposeful professor” is primarily inspired by serving society, and will create public goods for intrinsic reasons. She's comfortable that not every single decision she takes will improve her

career, even in the long term. But adopting a purposeful approach across all her decisions may, in aggregate, lead to even more career success than a strategic one, because altruistic actions may lead to unexpected benefits. (I'll give a few examples in this essay).

So what exactly *is* a professor's purpose? I'll use definitions coined by two professors who I've learned a lot from on this topic. One is Andrew Metrick, who I was lucky to overlap with for a year at Wharton. Andrew was a legendary teacher (as well as a top researcher), winning best teacher awards not only at Wharton but across the whole of UPenn. The student body was crestfallen when he announced his departure to Yale, and invited him to give a "last lecture". Andrew spoke on "How to Have a Successful and Meaningful Career" (Metrick, 2008). Most of the talk was on careers in industry, given his audience, but he touched on a professor's purpose, defining it as "the creation and dissemination of knowledge". He explained that one of his early passions was chess, a pastime I also enjoy and respect. But chess doesn't really contribute to something greater than yourself, so he chose to become a finance professor instead.

The second is Laurie Hodrick who, similar to Andrew, won best teacher awards not only at Columbia Business School but also across the whole of Columbia University. Word-for-word, her "Ten Questions" interview in the *Financial Times* (Hodrick, 2011) is the single most transformative article I've ever read. The first question was "What does it mean to be a professor?" Laurie replied "I have come to realize that, rather than being a title or job description, for me being a professor is a way of life. Being a professor is not something I do, it is who I am. It is being driven by intellectual curiosity to create, translate and disseminate ideas. I am equally a professor when working at Columbia or when working as an investment banker.<sup>2</sup> I am still honored and thrilled when I step into an elevator and someone says "Good morning, professor."

---

<sup>2</sup> Laurie spent two years as Global Head of Alternative Investment Strategies at Deutsche Bank.

Andrew's and Laurie's descriptions span very similar ground. The intersection contains the words "create" and "disseminate", so for the purpose of this article I'll – following Andrew – define a professor's purpose as "the creation and dissemination of knowledge."

### **3. Freedom**

#### *3.1 Passion*

I attended the 2006 FMA Doctoral Tutorial in Salt Lake City as a third-year PhD student. This kicked off with a panel of distinguished scholars either presenting a field of research, or giving their advice on how to succeed in the profession. One, whom I'll call Joe, spoke about how to choose a research topic. He suggested we focus on one area, so we can be known for it when up for tenure. Even post-tenure, we should continue the same agenda as there are economies of scale, allowing us to rack up more publications. We'll then be seen as a world expert on the topic – perhaps invited to write a Handbook chapter.

And how to choose that topic? You should base it on two factors. One is what your skills are. If you're a master of math, perhaps go into behavioral theory. With non-standard utility functions, the typical concavity requirements to show a global optimum aren't always satisfied, so you need more sophisticated techniques. The second is what's hot at the moment. Look at what's getting published and cited; see what subjects journals are running Special Issues on; take the most recent crop of job market stars and study what they were writing about.

The next person got up and said "I completely disagree with Joe. You need to work on what you're passionate about." He stressed that academia is a tough gig, a profession of many hard knocks. The top journals accept fewer than 10% of papers – and before even submitting to a journal, your paper might be savaged at a seminar or destroyed by a discussant. And the more innovative your paper, the

less likely it is to be appreciated, at least initially.<sup>3</sup> So you'll need to pick yourself up from multiple rejections and criticisms, and only if you're passionate about the topic will you persevere – continue to present and submit your paper, and do that extra analysis (having already worked on the paper for many years) to address people's concerns.

But even though passion helps you succeed by encouraging perseverance, that wasn't the main reason he advised us to research what we were passionate about. He said we should do so because *it's fun*. (As I'll stress in Section 3.2, fun does not mean self-indulgence or working on trivialities). What we work on is our life, or at least the working part of it. Many of us could earn far more money in industry, so a key reason for selecting academia is freedom. But many people are too afraid to enjoy this freedom, which defeats the point of our career choice. And pursuing your passion might lead you to switch direction, perhaps quite drastically, as your research interests change over time. Why be constrained once-and-for-all by what you wrote your job market paper on?

That person was Jonathan Berk – someone who's written seminal papers across a number of areas as well as a leading textbook. I recognize correlation isn't causation, but this is a researcher whose pursuit of passion doesn't seem to have been at the expense of success.

Jonathan's words have stuck with me ever since. And, as I spent more time in the profession, I saw the typical advice you hear doesn't always work, at least if your objective is to create knowledge rather than just crank out publications. The idea of researching a booming area doesn't make sense if markets are efficient – if a topic is hot, everyone else knows it's hot, making it a crowded field. You might still get your paper published, but it's unlikely to be innovative. Now, many people (including me) don't believe markets are fully efficient, but then you have a worse problem – you might end up working in a bubble. We over-extrapolate from past successes, not realizing the “time-to-build” in writing a paper is long, and by the time you're written your paper, the bubble may have already burst.

---

<sup>3</sup> See Dai et al. (2021) for empirical evidence supporting this point.

During my first year as a PhD student at MIT, almost every seminar flew above my head. In the second year (2004-5), I could understand at least a little bit of each talk. That year, most of the job market stars were working in behavioral finance, as that was the really hot topic. The Odean dataset was now widely available; Smith-Breeden Prizes had been handed out to behavioral asset pricing papers every year since 1999 (starting from Daniel, Hirshleifer, and Subrahmanyam (1998)), and behavioral corporate was starting to emerge with papers such as Malmendier and Tate (2005). But by the time I was on the market in 2007, the interest in behavioral had declined and there were few job market papers on the topic – before it later enjoyed a resurgence as researchers discovered new questions and new data. Beyond this one anecdote, Dai et al. (2021) show that novel topics quickly become saturated and thus reshuffle.

After my job market was over, I had a few months left at MIT. My job market paper and two others in my packet were on corporate theory, so it would have been rational to focus on this field, but I had an idea for an empirical paper on corporate social responsibility (CSR). Remembering Jonathan's advice, I pursued it – I cared about whether employee-friendly companies perform better. At the time, CSR was a non-existent field and so it was a very difficult paper to publish; the first journal said it was a “clear reject ... there is simply nothing novel, creative, or important in this paper”, and the second that it belonged in a practitioner journal. Edmans (2011a) is now my most-cited paper, through sheer luck because the field ended up booming in a way I could never have predicted.

Being skilled at a topic is far from a sufficient condition either, because skills are in abundant supply in our profession. I was strong at math during my economics undergrad – but as I started my PhD I realized how distinctly average my ability was. Some of my classmates already had PhDs in math or physics, or were undergrad majors in more quantitative subjects like engineering. I then thought I might have unique institutional knowledge as I'd worked as an investment banker in Morgan Stanley's Financial Institutions Group, so hoped to specialize in empirical banking. But anyone in a

PhD program can quickly learn institutional detail. Except in rare cases, your technical skills or institutional knowledge are unlikely to be unique.

I don't mean to portray the standard advice as a straw man. No-one says you should choose a topic by simply looking at the intersection of your skills and what's in-demand, paying no attention to what you care about. But when people ask what you care about, they'll ask "what are you *interested* in?" An academic's CV lists her research *interests*. When someone tells you about their latest research during a seminar office visit, you'll say it "sounds *interesting*" – even if you don't care for it. But Jonathan's advice wasn't to work on what you're interested in – he said "passionate". There's a big difference between being interested in something and being passionate about it. If you're merely interested in a topic, you'll spend a couple of years writing your initial draft, but might not stick with it after a few rejections. And the time you spend researching it will be pleasant, but it won't be fun.

In Section 4 I'll discuss how there's significant randomness in the profession. Whether you get your paper published depends on which referee happens to be chosen, not just its inherent quality. We have less control over where our paper ends up published than we think, but we can control whether we had fun writing it. In some sense, the most important referee is you – whether you thought it was worth you spending three, five, perhaps ten years of your life on a paper.

In July 2012 I had to submit my tenure packet at Wharton – a great school where I was lucky to spend six years. Back then, we not only had to submit it electronically, but also print out every paper we'd ever written and give them to the department chair's assistant. As I walked to her office, I ran into my colleague John Zhu who asked me what I was carrying, and I told him. He quipped that this must be the first time I'd held my life's work in two hands. I never realized that before, because I was so focused on the admin elements of submitting my packet that I was blind to the big picture, but John was right. After John wished me luck, I took a moment before resuming my walk. I looked through all the papers I was holding, arranged chronologically as was the requirement. They started with the

paper on soccer and the stock market I wrote during my PhD, then my job market paper on blockholders, then my CEO compensation theories with Xavier Gabaix, then my CSR research, and finally my studies on the real effects of financial markets. I realized I didn't write these papers to get tenure – to get approval from some committee containing many people I barely knew, based on anonymous letters – but because I was passionate about these topics. I cared about how companies are governed, whether socially responsible firms perform better, whether financial market trading is a sideshow or affects the real economy – and I loved soccer. By then, I'd received many negative signals about my tenure chances and might have to uproot to a different city, perhaps a different country. But I was able to hand in that packet and be fully relaxed about it, even though I'd spend the next four months mired in uncertainty – because I was proud of what I'd discovered during my five years at Wharton and four years at MIT.

As will be a common theme throughout this essay, I'm not advocating going from one extreme to the other. The decision does matter, and I was truly grateful when it ended up being positive. But the uncertainty of that external assessment was attenuated by my internal satisfaction with what I'd worked on.

### *3.2 Significance*

Pursuing passion and fun does not mean self-indulgence – devoting our life to trivialities that have little significance on the world. Instead, what's typically most fun, and inspires the most passion, is work that connects with something greater than yourself. That's why Andrew Metrick turned down a career in chess, even though he found it inherently interesting.

The creation of knowledge involves working on important, unexplored questions. Doing so has great overlap with passion – you're driven to continue exploring a question, despite numerous setbacks, if the answer really matters. Discovering something that's novel, rather than a minor

extension, is exciting. While there's some overlap with what the profession rewards, the overlap is less than it should be. The safe route to publishing is to make a minor contribution to a popular literature. For such a literature, you can see the formula that published papers have followed and copy it. Moreover, there are many potential referees working in the area so they'll view the topic as important. One of my coauthors refers to this publication strategy as "read a paper, write a paper"; another as "move from A to A prime".

Journals have strong incentives to publish papers in popular fields, given the attention to impact factors. Even if the contribution is minor, new research in that field (Paper 2) will still cite the paper (Paper 1) in their literature review, as part of a long list of papers. This is true even if the Paper 1 had no influence on Paper 2 – Paper 2 would have still been written even if Paper 1 didn't exist. If, in contrast, Paper 1 developed a new methodology that inspired Paper 2, that still counts as only one citation. Impact factors measure relatedness more than contribution.

This is particularly the case since journals are primarily assessed on their two-year impact factor. *Two years!* This fails to reward research with long-term impact. Some innovative papers may be before their time. The first fintech paper I'm aware of is Yermack (2017); only citations to it by papers published in 2018 and 2019 count to its journal's impact factor. But aside from the *Review of Financial Studies*' special issue in July 2019, most fintech papers weren't published until 2020 and beyond. Dai et al. (2021) find that more conventional papers are significantly more likely to be published in a top-three finance journal, yet garner fewer long-term citations. They conclude that "a paper's measured differentiation from previous work is not as prized by our profession's top outlets despite the academic community's stated goal of pursuing original research." They also show that novelty, in terms of deviating from standard finance subfields, significantly reduces publication prospects even though it's associated with higher citations.

If the purpose of a professor is to create knowledge, not just publish papers, this requires bravery in choosing research topics and going after novel questions, even if the field is nascent or nonexistent today. Doing so means that what we devote our lives to, spend five years working on, stay up past midnight perfecting, has significance.

Some have caveated that this approach is too idealistic. Given the bang-bang nature of tenure outcomes, people need to be pragmatic and maximize their tenure chances. They do so by choosing research topics that maximize the probability of publication in journals in their institution's A list, even if at the expense of knowledge creation. But that's only the optimal strategy because of how we decide tenure. Many schools and letter writers simply count the number of top publications, placing much less weight on a paper's significance or the researcher's contribution. Being a glorified research assistant on a multiple-authored paper in a top-three journal counts as 1; having a sole-authored paper on an innovative topic in a journal just below counts as near-0 in some schools.

Journal quality certainly matters – higher-ranked journals typically publish better papers. But while reasonable people can disagree whether journals just below should count for 0.6, 0.3 or something else (for those taking a “counting” approach), the optimal weight is almost certainly not zero. The binary way in which we treat different journals has several negative consequences. If we're out after three strikes, authors go for safety over innovation, either in choosing a conservative research topic or coauthoring with senior faculty on their agenda. It increases the role of luck, since if you don't get a favorable referee draw at an A-journal, your paper counts for almost nothing. It also means that schools effectively outsource the tenure decision to anonymous referees who they never chose, rather than reading the paper for themselves.

There's an active literature in corporate finance on how a focus on short-term signals, such as quarterly earnings, leads companies to take myopic actions. In myopia models, one solution is to gather qualitative information about the firm's fundamental value rather than just relying on freely-

available quantitative measures (e.g. Edmans (2009, 2011b)). Such information acquisition is costly, but worth it for important decisions such as whether to fire a CEO. Applied to academia, for a decision as important as whether to show an assistant professor the door, we should gather qualitative information on “input” factors scholars can control (e.g. innovativeness and significance) rather than relying exclusively on “output” factors significantly affected by randomness (e.g. journal publication). In short, we should read papers more.

Again, we shouldn’t go too far to the other extreme. Completely ignoring quantitative measures such as publications and citations, with the justification “we read the papers”, opens the door to decision being based on cronyism or taste – personal like or dislike for the candidate or type of research. An objectively weak candidate may be tenured based on claims that the papers are high-quality; a process that involves purely subjective criteria may be particularly biased against minorities. Just like evaluating companies, the optimal approach involves a combination of hard and soft information. Similarly, the significance of a topic is a matter of opinion, particularly for a nascent subject. Many elder statesmen initially dismissed behavioral finance as studying trivial curiosa that aren’t important enough to affect asset prices or corporate decisions. However, the innovativeness of a topic might be less subjective – people might agree that that a paper takes a new direction even if they disagree on how important that new direction is.

Note that the reliance on short-term quantitative signals pervades throughout the profession, not just evaluating tenure. Rankings of finance departments count the number of publications in certain journals, paying no attention to the importance or impact of those publications. This in turn may explain why schools are so risk-averse in their tenure, hiring, and even PhD admission decisions (see Section 4.4).

### 3.3 *Coauthorships*

Another freedom we often take for granted is that we can coauthor with whoever we like, without being limited by our institution – in a company, you’re forced to work with your colleagues. It might be tempting to choose a big name to increase the impact of your paper (Brogaard et al. (2020) find that author fame has a positive causal effect on citations), even if they do little work or are unpleasant to work with. Sometimes there’s pressure to coauthor with a senior colleague in our department for political reasons. But we have the freedom to choose coauthors who challenge us as iron sharpens iron, possess complementary skills that we can learn while simultaneously passing on our own expertise, and have fun working and celebrating success with.

## 4. **Luck and Path Dependency**

### 4.1 *The importance of luck*

When I started my PhD, the upperclassmen told me how fewer than 10% of submissions to a top journal are accepted. I was shocked at the low odds, but they reassured me the tenure process takes this into account – at good schools, 4-5 top publications might be enough.

With successes being so lumpy, there’s a significant amount of luck in the profession. If you get a favorable referee draw, you could already be a quarter of a way towards tenure. Welch (2012) found that, for conference reviews, the referee-specific component of a paper’s score is twice as large as a paper’s common component. Loosely speaking, which referee is chosen is twice as important as its actual quality.

The effect of luck is compounded by path dependency. If you get a paper accepted early on, further successes likely follow. If an assistant professor at a top school gets her job market paper published quickly, the profession thinks she deserved her placement and sees her as a star. She’ll receive seminars and conference invitations, and editors and referees will look upon her work

favorably. But if she strikes out in the first couple of years, people write her off as having been overrated by her letter writers. And successes can make future publications not only more likely but also more impactful, given the effect of author fame documented by Brogaard et al. (2020).

What this all means is those who are in privileged positions are there at least partly due to luck – and one of these people is me. Because luck affects not only whether your paper gets published, but whether you even write it in the first place. At the start of the second year at MIT, you take an empirical asset pricing course. The regular professor was on sabbatical, so it was taught by Tim Johnson, visiting from LBS. Normally the course is focused on neoclassical asset pricing, but Tim included Hirshleifer and Shumway's (2003) paper on weather and stock returns in the first week's reading list. During the lecture, Tim discussed the broader literature on sentiment and asset prices and explained how the most convincing papers had a strong *a priori* hypothesis – they used variables with a powerful effect on sentiment that's correlated across a country. Soccer results immediately came to mind.

After the class, I emailed Tim to ask if this might be a potential research idea. Tim wrote back approving of the topic, with the caveat that he's not on top of the behavioral finance literature so didn't know if someone was already working on it. So he reached out to Nick Barberis, whom he knew from his time as a Chicago PhD student. Nick also said the idea was new, so I pursued it, found positive results, and posted a draft on SSRN.

When you work on a paper, what's the worst thing that can happen? That someone else is doing so. Diego Garcia and Oyvind Norli, then at Dartmouth, were independently working on the same idea. During Nick's seminar at Dartmouth, Diego told him about their paper and so Nick informed them of mine. Diego and Oyvind would have clearly won any "fight" – I hadn't even passed my qualifying exams and had limited time to work on it, and had no experience of the publication process. But instead they reached out and graciously invited me to combine our papers. And the worst thing

that could have happened turned out to be the best thing, as I learned a ton from Diego and Oyvind – how thorough you need to be with empirical research (particularly on a controversial topic like ours), how to write a paper, and how to respond to a referee.<sup>4</sup>

The paper was published by the *Journal of Finance* as Edmans et al. (2007). Yes, I benefited from the alphabetical ordering studied by Brogaard et al. (2020) – something that’s been an undeserved windfall to me throughout my career. I don’t want to belabor the point, but it’s worth putting in one place all the luck I had with that paper. I was lucky Tim was teaching the PhD class and knew Nick; lucky two professors were working on the same paper (even though it would normally be a death-knell to a PhD student); lucky Nick gave a seminar at Dartmouth and found out about both papers; lucky Diego and Oyvind invited me to join forces; lucky to have a supportive Editor, Associate Editor, and referee for a potentially controversial paper; and lucky my name was first so that I got equal credit despite being a PhD student.

I had luck on the next paper as well. Shortly after the soccer paper was conditionally accepted, the AFA annual meeting took place. I was lucky it happened to be in Boston, so I could go along. I saw Cliff Holderness present his paper, later published as Holderness (2009), which showed how prevalent blockholders are in the US – but also how small their stakes were. I recalled the blockholder theory papers I’d learned in Stew Myers’ PhD corporate finance class. These papers were about governance through “voice”/intervention, but the small stakes Cliff found would likely make intervention difficult. This gave me the idea to write a theory of how blockholders can govern even if they can’t intervene, i.e. only engage in “exit”, which became my job market paper (Edmans, 2009).

These stories aren’t meant to be a self-indulgent autobiography, but to stress how I’m here largely due to luck. If I played that same game 100 times starting with those same initial conditions, 99 of

---

<sup>4</sup> I didn’t even know that you needed to write a response document, and if I did, I’d have probably told the referee why s/he was wrong.

those times I wouldn't have the honor of giving the FMA keynote. Other scholars with strong records may also have benefited at least in part from good fortune. Again I don't want to go to the other extreme and suggest it's all due to luck. Skill, creativity, hard work, and resilience to setbacks all play significant roles – and even if you get lucky, you have to make something of it. But luck plays far more of a role than we think. In the words of Taleb (2001), we're "fooled by randomness". Our profession has so few tangible measures of success that we overweight the signal contained in a single publication or someone's institution. Your job market outcome may be thanks to a lucky publication as a PhD student, or your job market paper happening to be close to the research interests of someone on the hiring committee for that year. This overweighting leads to path dependency and means the effects of luck are long-lasting, as evidenced by the rarity of upward mobility in our profession.

#### *4.2 The implications of the importance of luck*

What this means is those fortunate enough to be in privileged positions are indeed there – at least partly – because we've been fortunate. We thus have the responsibility to share this fortune with the profession through creating public goods. For the most elite group of scholars, the best public good they can create is their own research, and they should focus on that. But for others, tenure frees us from having to focus exclusively on our own papers and allows us to create other public goods. This may involve mentoring PhD students, serving on boards of journals or professional organizations, running conferences or summer schools, or providing comments on others' papers. Now for most types of public goods, there's an instrumental benefit. Being a journal editor or President of a professional association is prestigious; running a summer school on your research field will help that field grow. Thus, even a strategic professor will create these public goods because she estimates the benefit exceeds the cost. But a purposeful professor will do so for intrinsic reasons – to contribute to the profession – and any boost to her own reputation will be a by-product.

The importance of luck and path dependency also means that those with thinner CVs, or at institutions written off as lower-tier, may be just as capable as those at top schools. They may have suffered bad luck, or chosen a department due to dual-career issues or other geographic preferences. But our profession puts so much weight on someone's institution. Why is it that every conference nametag contains not only your name but also your institution? At an industry conference, that makes sense. People do different jobs, and so knowing a stranger's employer helps you decide whether to strike up a conversation. If someone has Houlihan Lokey on their badge, then someone interested in restructuring will approach him. Fidelity signals you're in public markets investing, and KKR you're in private equity.

But in our profession, *everyone does the same thing* – we're all finance professors. Now we don't exactly do the same thing – some of us work in macro-finance, others corporate governance theory, others still behavioral asset pricing. But then, why don't conference nametags say our research field? That would lead to far more interesting conversations at conferences, where people strike up dialogs based on their shared interests – sorry, their shared *passions* – rather than whether we judge another person as worthy of our time based on their school.

It's even worse at arguably the two most prestigious conferences in our profession. At the NBER, you start every meeting with everyone introducing themselves by their name and institution. Knowing someone's name is useful as you may have never met them before. Perhaps you've read their papers and now you can put a face to them, and initiate a conversation later. But why is their institution relevant? Will you go up to someone during the coffee break and say "Hi, professor at University X, I'm a professor at University Y?" The main effect is that others start judging whether you deserve to be in an NBER meeting, or are a gatecrasher. At the AFA, students are branded with "STUDENT"

on their badge, as if to say “don’t waste your time talking to me; you’re better off sniffing out a big shot”.<sup>5</sup>

The school clearly contains *some* information about a person’s quality, and conversations with more knowledgeable folks are more stimulating. I’m sure we could write a model of limited attention in which it’s rational to talk to someone at a conference based on their institution, but as a profession we’ve taken it to the extreme. In legal parlance, the prejudicial value of the school outweighs its probative value. If we write-off another scholar – a colleague in the same profession – based on their institution, we’re judging on a variable that’s significantly affected by noise. This in turn starts a vicious cycle – being repeatedly treated like a second-class citizen can discourage researchers from attending conferences, which reduces their exposure and further perpetuates the path dependency.<sup>6</sup>

And it goes beyond the nametag. Why do conference programs list the institutions of the authors and discussants – why aren’t the titles of the session and papers sufficient? For conferences with multiple parallel sessions, this practice may cause us to attend sessions based on the presenters’ schools rather than whether we’re passionate about the research being discussed. Now the quality of the talks, not just the topics, affects how stimulating the session will be. But the relevant factor is the person, not their institution. Some discussants outside the elite schools consistently give excellent

---

<sup>5</sup> Similarly, some seminar schedules contain not only the name of the person you’re meeting in an office visit but also their rank (Assistant, Associate, or Full Professor). The latter is unnecessary.

<sup>6</sup> Alternatively, one might argue that there’s relevant non-quality information contained in the nametag. Perhaps if someone’s at school X, we might ask “do you know Y? We did our PhDs together.” But if you really want to learn the school for these reasons, you can ask it after starting a conversation – by which time you can’t run away if the answer isn’t elite enough to justify your time. A second justification is that the institution may have been important in pre-internet days where you needed a simple way to search for a colleague you met briefly at a conference but forgot their name. Even if you remembered the topic of their research, there was no directory where you could look up all macro asset pricers, but you could delve into a faculty directory. However, it may be a relic of the past that’s less important now. A third justification is to advertise the institution. For example, an institution that has succeeded in hiring active researchers might enjoy an improved reputation from those researchers attending conferences and being clearly identified with that institution. However, there may be other ways in which institutions can signal their progress, and this benefit needs to be weighed against the costs.

comments; others in the magic circle always twist a discussion into an informercial for their own papers. But this reputation formation is hindered if we overweight the institution.<sup>7</sup>

How much deadweight loss is created in our profession through judging others based on their institution? How many research conversations between people with the same passions have been lost, and how many great papers would these conversations have led to? How many interesting articles have we never read, despite being published in top journals, or never seen, despite being accepted to great conferences, because of the authors' school? We'll never know about the past – but we can fix this for the future.

Organizations are responding. At least two schools have used blind recruiting processes where faculty read job market papers that omit the author's name. Of course, there's significant information contained in reference letters and CVs, but these are read after the job market paper to avoid first impression bias. Such experimentation is to be encouraged. Some experiments may fail, as there turn out to be costs of removing the institution that I've failed to anticipate. But, we won't know until we try, and if any profession should be open to innovation, it's ours. I'd encourage future conferences to think about removing institutions from nametags and conference programs, and replace the former with either research interests or nothing at all.<sup>8</sup>

As with everything in this article, I'm saying this to myself as much as I'm saying it to everyone else, as I'm also prone to prejudice. On my office wall is a quote from the book "The One Minute Manager", which reads "Everyone is a potential winner. Some people are disguised as losers. Don't let their appearances deceive you." This quote is quite blunt, and I absolutely don't mean to suggest that faculty at different institutions, MBA students who struggle with the material I'm teaching, or

---

<sup>7</sup> Again, there may be benefits of listing the institution in a conference program. In the absence of institutions, attendees may default to attending the presentation by a distinguished senior scholar rather than a new assistant professor at a good school. However, organizers should discuss the costs and benefits, rather than defaulting to include the institution.

<sup>8</sup> Note that other forces are moving in the opposite direction. The move to single-blind reviews for some journals and conferences means the first thing a referee sees is the author's name and institution, even before she's read even the abstract.

journalists from less famous newspapers who want an interview, are “losers”. Instead, this quote is to remind me that if I write someone off through my own prejudice, then the biggest loser – the person who loses out the most – is me.

### *4.3 The importance and irrelevance of luck on the job market*

When I went on the job market, I was neurotic about where I’d end up. This was when your “type” is revealed, and you’d learn whether all those years slogging away in the PhD program, while your college friends were bringing home the bacon in industry, were worth it.

But Dimitris Papanikolaou, also on the market from MIT Finance in the same year, pointed out that our eventual placement didn’t really matter, at least from a purpose standpoint. All it affected was pride. We write the same papers, go to the same conferences, and teach the same material, regardless of what school we’re at. And he was right. If the real reason I’d chosen academia over industry was because I loved research and teaching – because I wanted my purpose to be the creation and dissemination of knowledge – rather than to introduce myself at cocktail parties as a Harvard Business School professor, the outcome would make little difference. So, paradoxically, luck is both important (our job market outcome depends far more on luck than we think) and irrelevant (our job market outcome has far less effect on life satisfaction than we think).

Yet again, I’m not advocating the opposite extreme. Your institution does matter somewhat. You might have smarter colleagues, a bigger research budget, superior PhD students, a smaller teaching load, and a stronger seminar series. But it matters far less than we think. In investment banking, if you’re not at the bulge-bracket banks, you’re not in the room when the biggest deals are being negotiated. In our profession, we all have a seat at a table. Anyone can go to any conference<sup>9</sup>, sit in

---

<sup>9</sup> There are some conferences that are invitation-only, due to extensive audience discussion of each paper. Examples include the NBER and the Finance Theory Group.

any session – even by a Nobel Laureate – and ask questions in the Q&A. We can collaborate with coauthors from any institution, particularly in the days of Zoom. In Spring 2020, Murray Frank, Vojislav Maksimovic, and Gordon Phillips launched a Virtual Corporate Finance seminar series that anyone can attend, and others there are similar series around other themes. Anyone can submit to any journal, while novelists can't send a manuscript to a publisher – editors only accept submissions from agents, who play a screening role. The main reason the school matters is if, as a profession, we've decided it does – if we judge others by their institution or if I, as a journal editor, am prejudiced against authors because of it. This not only leads to wrong decisions ex post, but also adds a huge amount of ex ante stress to job market and tenure outcomes which are already significantly driven by luck.

#### *4.4 The barriers to entry in PhD programs*

Judging happens not only once we're assistant professors, but also when we're PhD students – our institution affects whether we get job market interviews or are invited to doctoral conferences. It occurs even before that – applicants to a PhD program are evaluated on where they studied before and who they've worked with. Nowadays, the barriers to entry are huge. Most PhD applicants already have a Masters; many have undergrad or predoc RA experience where the fame of the professor matters much more than the tasks undertaken. PhD students now take six years to graduate, since we now expect a large pipeline and even a publication, making the pathway to a finance professor even more financially costly. Both the premium placed on RA experience with a famous professor and the financial resources needed for a PhD may alienate students from developing countries or less affluent backgrounds. It's becoming an arms race.

Of course, some of this prior training is useful. And, institutions do carry some informational value, particularly when sifting through lots of applications. But if a precondition for getting into a

PhD is years of research experience plus a Masters, we're going to exclude many people who didn't find their passion until later in life – they didn't know to get RA positions during their undergrad. We make the path dependency even worse.

I never wanted to be a finance professor. I loved my job at Morgan Stanley's Investment Banking Division, but had never lived outside the UK and wanted to travel. I had a Fulbright Scholarship to the US but it wouldn't fund an MBA. There were no Masters in Finance back then, so my only option was to do a PhD in finance and drop out after two years with a Masters. I took a Summer Associate position at the end of my first year, hoping to get a full-time offer for after my Masters. I moved into Fixed Income Sales and Trading to give me experience of another side of industry, and I enjoyed it much more. During the weekends, I wrote my first paper – a theory of inside debt I later completed at Wharton with a PhD student and published as Edmans and Liu (2011). Morgan Stanley gave me an offer, but since I had the inside debt and soccer papers, I thought the PhD might only take one year more than the Masters, so I decided to complete it before returning to Wall Street.

Knowing it would be my last chance to travel, I spent my second summer as a volunteer teacher of English in Saraburi, Thailand. I chose rural Thailand because the absence of tourists meant that barely anyone could speak English, making volunteers particularly important. My surroundings meant there was no internet where I stayed, nor anything for a non-local to do. So, once the school day was over, I wrote a theory paper of capital structure which ended up being Edmans (2011b).

I returned to MIT for my third year and went straight on the (industry) job market. I'd chosen Morgan Stanley for my fixed income internship since I'd previously worked with them in investment banking. But I learned that, in fixed income, Lehman was the market leader so I signed with them. I defended my dissertation and told my advisors I was graduating. One advisor, Dirk Jenter, caveated that I was leaving academia before learning what my outcome would have been. He advised me to

stay for a fourth year, write a job market paper and see how I fared. If I didn't get the placement I wanted, "Lehman will always be around". Famous last words.

Lehman graciously deferred the offer, and I started my job market paper after seeing Cliff Holderness's presentation at the 2006 AFA. But the more important effect of that extra year was that it made me realize how much I enjoyed academia. I gave my first conference presentations in Spring 2006 and really enjoyed this part of the profession.<sup>10</sup> I'd started delivering lectures as a Teaching Assistant and felt in the zone doing them. I loved writing my job market paper, getting lost for long hours with just pencil and paper, and exploring something deeply. So I decided I was going "all in" to academia – there was no cutoff school below which I'd join Lehman. Research and teaching had become my passion.

I'd have never gotten into a top PhD program if I applied today. Even if I sneaked through, I'd have been nudged out for doing an internship in my first summer, and booted out for doing volunteer work in my second. Many leading scholars have similarly told me how they decided on academia much later in life. If the profession is going to be truly serious about diversity, this extends beyond the important aspects of diversity in gender and ethnicity to diversity in background. As with evaluating researchers by counting publications, our approach to recruiting PhD students is extremely risk-averse. Applicants who've been finance or economics RAs from when they've been undergrads might be the safest bet. But some right-tail outcomes might arise from researchers with a broad set of skills. Behavioral finance was catalyzed by insights from psychology; nowadays, promising topics such as machine learning and corporate culture similarly bring in ideas from outside finance. Our current preference for career RAs recruits candidates who are used to working on other people's ideas, rather than their first experience of research being to take the risk of exploring their own.

---

<sup>10</sup> My coauthor presented at the 2005 EFA in Moscow. We'd submitted the soccer paper to other conferences before it was accepted by the *JF* and it was at those conferences I presented in Spring 2006.

## 5. Non-Rivalry

At Morgan Stanley, many interactions with other investment banks were zero-sum. Most mandates are awarded to only one bank, so if you win a pitch, no-one else can. On the trading floor, if you make a profit on a transaction, your counterparty loses.<sup>11</sup>

But in academia, many things aren't zero-sum – the pie is not fixed. Journal space is not finite, so someone else publishing doesn't reduce the space for you. The *Review of Financial Studies* was created in 1988 because there were more top papers than top journal slots, and expanded from bi-monthly to monthly in 2009 as the supply of great research grew. At the *Review of Finance*, we've recently increased our issue size by over 50%. The move towards online journals removes any constraints associated with limits on the size of a physical issue. At a conference, there are limited presentation slots, but supply adjusts to accommodate increasing demand; many professors believe there are now too many conferences. Collaborating on a paper is similarly not zero-sum – if a paper has three authors, each one gets more than a third of the credit (indeed, there are some schools that simply count the number of publications without adjusting for coauthorship; Google Scholar and other citation databases similarly make no adjustment). And unlike in other disciplines, we don't need to argue with our coauthors over who contributed the most and thus should be the first author.

Certainly, there are rivalries in some elements of our profession. Only one person can win the Fischer Black Prize or be the AFA President in a given year. But, for most of us, these honors are out of reach – and of little consequence if our purpose is the creation and dissemination of knowledge (there are many influential scholars with neither honor). There are also rivalries between authors working on the same topic, but they can often be overcome if approached with a non-rivalrous

---

<sup>11</sup> Of course, many aspects of investment banking are positive-sum, such as advising on a value-creating transaction. The above discussion surrounds which bank is hired to give the advice. Similarly, trading increases price efficiency, which can have real effects. The above discussion focuses on trading profits and losses.

mindset. In Section 4 I discussed how Edmans, Garcia, and Norli (2007) came about through a merger of two similar papers. Sometimes mergers are impossible – it’s difficult to go on the market with a paper co-authored by senior faculty. When I presented the first draft of my job market paper (Edmans, 2009) at the MIT PhD student lunch, I was despondent to see the MIT external seminar that afternoon was another model of governance through exit by two leading theorists (Admati and Pfleiderer, 2009). But we kept abreast of each other’s papers, ensured we maintained our different focuses rather than steal each other’s turf, and cited each other fairly – and both ended up getting published. In contrast, copying someone else’s idea or failing to give others credit leads to win-lose outcomes.

Ours is a unique vocation in that our main loyalty is to our profession, not the institution. People at other universities are our colleagues, not our competitors. We decide on who to give tenure to based on what our competitors say about the candidate. How crazy is that? Would a law firm decide which associates to promote to partner based on their rivals’ assessment of them? This process is so standard that we follow it without stopping to think how nonsensical it would be in the private sector. Yet it’s not crazy in our field, since our main affiliation is to the profession.

At a conference, we ask someone at a “competing” institution to discuss of a paper. How crazy is that? Would an asset management conference have one company present its responsible investment strategy and a rival then comment on that strategy’s effectiveness? Journal editors decide which papers to accept by scholars at competitor universities based on advice by referees also at different institutions. Would an M&A acquirer outsource its decision of which investment bank to hire to another advisor which, for some reason, was not pitching for the deal?

That the profession’s pie is not fixed provides us with tremendous opportunities to grow it by contributing to the creation and dissemination of knowledge across our profession as a whole – by viewing other members as our colleagues in the advancement of knowledge.

### *5.1 Collaborating to create knowledge*

So how do we do that? Starting with the creation of knowledge, perhaps the biggest thing is to provide comments on each other's papers. I was lucky to have had PhD classmates who I did that with. Because the soccer paper allowed me to go to conferences, I met many faculty who later sent me their papers for feedback. I was flattered that they were reaching out to a PhD student for comments and tried my best to help.

When Diego, Oyvind, and I merged our papers, I wrote to Nick thanking him for his role in it. He replied saying it was a pleasure, and kindly inviting me to send him the first version of our joint paper as he'd be happy to provide comments – and he indeed did. I later told Tim Johnson I had a corporate theory paper on inside debt, but there were few corporate theorists in MIT Finance. So, he advised me to send it to his LBS colleague Denis Gromb. That summer, I presented the model in Gerzensee, and Denis came up to me afterwards with a full mark-up of my paper (which he'd read in advance) and spent an hour giving me comments.

Having benefited from others' input on my papers, this was one of my key criteria for choosing which school to join on the job market. During one of my post-offer visits, I asked someone whether the faculty provide comments on each other's papers. He said no-one does that – but no one does that at any school because people are too busy with their own research, so it shouldn't factor into my decision. I was surprised, and explained how Nick and Denis had helped me even though I wasn't at their schools. He replied “Ah, but Nick and Denis are special. Most people in the profession aren't like them.”

I went away from that meeting impressed by his honesty but depressed by his message. But, as a theorist, I understood the concept of multiple equilibria. You can have competitive equilibria and you can have cooperative equilibria. And, unlike models where a sunspot decides which equilibrium is selected, here you might be able to affect the equilibrium. Deviating to “cooperate” when everyone

else “defects” may not be a best response if your only objective is your own utility, but if your purpose is the creation of knowledge, it may well be. (And, unexpectedly, others may start cooperating too, so you end up benefiting even though that wasn’t your motivation). I tried to provide the best possible comments on every paper people sent me, and soon formed relationships where we’d send each other our papers for feedback. Perhaps I gave more comments than I received myself, but I wasn’t keeping track. I genuinely enjoyed reading others’ new findings and providing input into them.

These relationships are ones we can proactively form ourselves. Why is it that, until the pandemic, nearly the only way to obtain feedback was to fly around the world, at great financial and environmental cost? Why does our ability to receive suggestions hinge on whether our paper gets accepted at a conference (which Welch (2012) showed largely depends on luck), whether we’re invited to give a seminar (which depends on our institution and perhaps luck from knowing a faculty member there), and our research budget and ability to travel (a particular barrier to non-US authors)? Why will we only give feedback in the form of a public discussion or in a seminar, where we get something in return by appearing smart in front of our colleagues?

I was lucky to form most of my relationships implicitly. I never sat down with someone and said “let’s have an arrangement where we send each other our papers for feedback”; I’d just provide comments when people sent me papers and the relationship naturally evolved. But, there’s no reason why such a relationship can’t form deliberately – and, why limit it to two people? At Wharton, I established a corporate finance discussion group for junior finance, accounting, and economics faculty to present embryonic work. The guidelines were that you had to attend every session, even if you were not presenting, and much bigger organizations like the Finance Theory Group have similar rules. When I moved to LBS, some colleagues and I set up an LBS/LSE corporate finance discussion group with an extra rule – you had to read the paper in advance and prepare your comments. Thus, when the author presented, it wasn’t a standard seminar where the goal of each slide was to explain

its contents to the audience, but instead to prompt the audience to provide their comments on the aspect of the paper contained on that slide.

In today's virtual world, such hubs can be formed across the globe – we're not limited by our institution. We can read each other's papers and then provide comments over a Zoom call, if it's a hassle to write an email; or we can give virtual presentations to each other to save reading effort. The challenge with physical conferences is you need scale for them to run, but the online world shatters the minimum efficient scale. A hub can be established even around a niche area.

In addition to informal comments, we contribute to the creation of knowledge through our formal role as referees. I started refereeing as a PhD student, and quickly these requests exceeded 40 per year, in part due to working in different fields. Senior colleagues said this was my fault and I should do a good job rather than aiming for perfection – not only is the time spent directly costly, but being too diligent means that editors will keep coming back to you.

But I was grateful that editors trusted me to advise them whether to accept a paper – a decision that would affect not only the journal's reputation but also whether the authors step as much as one-quarter of the way to tenure. I enjoyed seeing how papers evolved after incorporating my comments, and was thankful to play a role in the creation of knowledge. I never expected any instrumental benefit. But the *Review of Financial Studies* suddenly introduced a Distinguished Referee award in 2009, at the end of my second year as an assistant professor, and I was a recipient; two years later I was invited to become an associate editor. And, junior faculty at other schools informed me that editors told their departmental colleagues over lunch that they appreciated my reports, something I never expected to become public. Through the process itself, I learned a ton – how to pick apart a paper, highlight its contribution, and tease out the implications for a general audience – that I could apply to my own papers and, more recently, my editorial work. I could have never predicted these, but the power of purpose is that actions undertaken for intrinsic reasons can bear unexpected fruit.

Another way we can contribute to knowledge is to answer requests for help. As finance professors, we're all familiar with the concept of arbitrage – things of equal value trade at equal prices. But the key to citizenship is to provide gifts of unequal value, that are worth far more to the recipient than they cost the gifter. When I was a PhD student, I'd sometimes email professors with questions about their dataset or analysis that weren't covered in the paper, or ask if they knew of follow-up work that had expanded on it in a particular direction. I was grateful for all the responses I received – they may have taken them five minutes to write, but saved me five hours.

Morgan Stanley was a company with potentially huge rivalry, yet it was routine for analysts to help each other. When someone needed sample slides on how to deal with flowback in cross-border M&A, they'd email the analyst mailing list in case any of us had done something similar. In our profession, we have Statalist for coding queries, but no list or message board if someone has a question on a dataset, or wants to know if there's a paper that shows a particular result. We have message boards for gossip and slander, as I'll discuss in Section 5.3; why don't we have ones that catalyze the creation and dissemination of knowledge?

None of the above is to suggest that we're obligated to respond to every email, nor that we have the right to expect responses to our own messages. It's our responsibility to compose our emails so that the recipient wishes to respond to them – for example, explaining that we've already looked at X, Y, and Z but still couldn't find the answer. But it does mean that, if the sender has done his homework, a response may be a gift of unequal value.

Similarly, we shouldn't expect everyone to provide comments on papers that we send them, particularly if they're way outside our research field. I hope my earlier suggestion to provide comments on each other's papers doesn't lead to the other extreme, where people send around papers randomly and bear a grudge if they don't get a response. While many two-way relationships can be

created, it's the recipient's prerogative to decide whether to enter into a relationship with us. They may have other professional or personal commitments that we're not aware of.

## 5.2 *Collaborating to disseminate knowledge*

The second way to grow the profession's pie is to share others' research – mention it to others working in similar fields, invite it to a conference, or posting it on Twitter or LinkedIn so that it's seen by practitioners. Doing so has instrumental benefits – the beneficiary is grateful and may return the favor later.<sup>12</sup> Thus, a strategic professor will disseminate others' research, but only that of big names, hoping for reciprocity – thus perpetuating the path dependency. But a purposeful professor will do so because the research is interesting and, in some cases, because it's a gift of unequal value.

How did I end up at Gerzensee to receive Denis Gromb's comments? My advisor Dirk Jenter saw a working paper version of Sundaram and Yermack (2007), an empirical study of inside debt. I sent the authors my theory paper on the same topic and both graciously responded. David Yermack asked if I'd sent the paper to a journal; I replied that it had been rejected by one. He told me not to be discouraged as many of his most innovative papers had to be sent to multiple journals. While most professors know that rejection is part and parcel of the profession, I was quite devastated to have been rejected and so this was a gift of unequal value to a green PhD student. A few months later, David invited me to present my work in a Focus Session he was organizing in Gerzensee that summer. There were so many other people he could have invited to bolster his career, but invited me to help publicize a paper that might not otherwise see the light because I was a PhD student.

Another way of disseminating knowledge is to let authors know about relevant papers. I've written a number of survey papers, and my coauthors and I send drafts to people in the literature to ensure

---

<sup>12</sup> Citing a paper has other instrumental benefits – it may provide evidence to justify your modeling assumption or empirical specification. In addition, citing relevant papers is necessary to avoid plagiarism.

we're referencing everyone accurately and haven't missed relevant research. There are some authors who always respond by telling me to cite several of their other papers, even though none are relevant, with no comments beyond that. But others will alert me to papers by other scholars that I've missed, perhaps alongside a suggested correction to how I've cited them.

Some researchers have public platforms, for example on Twitter and LinkedIn, and thus the ability to disseminate knowledge to a wider audience. A number will only share their own papers, or newspaper articles. But sharing the writings of others is a great way to grow the profession's pie.

### 5.3 *Shrinking the pie*

Even though our profession is characterized by non-rivalry, we sometimes act as if we have huge rivalry – as if making others lose automatically causes us to win. The worst example is the anonymous trashing of our colleagues on internet forums. Such forums are the cancer of our profession. They lambast others' work, their character, and their personal lives, sometimes involving sexist or racist comments. No other profession to my knowledge has such forums. Our students, our executive education clients, our university administrators, those who donate to our school, and companies who sponsor conferences and best paper prizes would be aghast if they saw this side of our profession.

Our profession is enhanced by criticism and challenge, but only within a framework of accountability. A discussion is public, so if a discussant is below the belt, her own reputation is harmed. A referee report is anonymous to the authors, but visible to the Editor. But message forums allow people to post false allegations with zero consequences. Doing so is like urinating in a swimming pool. It can be done without anyone noticing it's you, spreads easily, and can't be easily taken back. The damage only subsides once everything is sufficiently diluted, which takes years.

Now one might claim it's OK to read these forums, as long as you don't post on them, but doing so contributes to advertising revenue. Is a platform where our colleagues get vilified something we

really want to support? We might instead argue that reading them is a way to obtain information about the profession. But by trying to “rationalize” it this way, we’re telling ourselves “rational lies”. Ours is a profession characterized by evidence. We can’t publish a conclusion without it being based on research, which has been vetted diligently by a review team. These forums allow people to post claims without any substantiation; while we might think we’ll apply the appropriate discounting, once you’ve heard an allegation, it’s difficult to unhear it even if false. There are far more reliable channels to keep abreast of what’s going on.<sup>13</sup> And viewing such forums pollutes the reader with endless negativity or – worse – misinforms our decisions (e.g. we might not submit to a journal if we believe it’s biased, or approach a professor if he’s been labeled a predator). If you step into a swimming pool in which others have urinated, you get contaminated yourself.

A second way in which we practice rivalry is through Schadenfreude. If someone’s paper is destroyed in a discussion, overturned, or retracted, we’re secretly delighted – particularly if it’s a high-profile person. We think reducing their slice leaves more of the pie for us. But the entire pie shrinks if outsiders view academic research as irreplicable.

Third, we can shrink the pie through being territorial about our own agenda. If we work in (say) ESG, we want to position ourselves as the only academic expert in ESG. We refuse to disseminate others’ work; in contrast, we may trash it. But reducing our colleagues’ slice of the pie doesn’t mean more for us – instead, the pie will be grabbed by others outside our profession. Companies, consultancies, and think tanks are all putting out their own research. While some practitioner papers are informative, most don’t aim to be a scientific analysis of the data but to support the organization’s pre-existing position or yield a conclusion that the public likes and thus boosts its image. These organizations may not have the expertise or incentives to produce high-quality research, but they

---

<sup>13</sup> If, indeed, there may be relevant information that is best shared through an online platform (such as job market or conference information), then such online platforms should focus exclusively on that, rather than allowing for trashing.

certainly have the resources to market it. If we don't promote the research of our profession as a whole because we're only focused on tooting our own horn, this leaves the rest of the pie up for grabs – and it will be grabbed, by companies and consultancies.

A lion can't catch a springbok in a race. So the lion waits for springboks to fight with each other and then attacks. I don't want to be apocalyptic, but our entire profession is under attack. Corporations are now developing their own executive education programs. The move to online education means that whatever teaching requirements we retain can be met by fewer people, so the profession is likely significantly over-capacity. The public – which includes both users and funders of academic research – don't recognize its value, both because it gets crowded out by practitioner studies that exploit their confirmation bias, and also because the petty fights we get into with each other give the impression that no-one agrees and so there's nothing to learn from academia (see also Schoar, 2020). Of course, healthy dissent is to be encouraged if people reach different conclusions – but, each year, our profession pushes out the frontier of knowledge and we agree much more than we disagree. For example, the consensus from a broad stream of research is that share buybacks are much less value-destructive than politicians and talking heads claim them to be – but academia is having very little impact on this debate and so unwarranted regulation is a real possibility.

If we don't stand up for our own profession or, worse, actively try to destroy it, we ourselves will lose in the long-term. The schools of the future may be teaching schools. Indeed, such schools are already starting to crop up, with few research requirements aside from developing case studies. If research is irrelevant, why should students or donors pay for it?

Despite these threats, the scenario is far from a doomsday one. We have the power to help our colleagues create knowledge and to disseminate their research. Our time is a scarce resource, and we face our own creation and dissemination pressures. But the beauty of gifts of unequal value is that public goods can be created at little cost to ourselves – seemingly small actions make a big difference.

The main barrier isn't so much time, but a mindset shift – from a zero-sum mentality to thinking proactively about how we can advance our profession.

One such gift is kindness. This doesn't mean telling white lies – if we're asked to discuss a paper with serious flaws, it's our duty to point them out. Not to do so is unkind to the authors, otherwise they send a flawed paper to a journal, as well as being a disservice to the conference. But even harsh criticisms can be given in a constructive way<sup>14</sup> – yet sometimes the discussant thinks he can boost his reputation by slamming the (usually junior) author, like a bully who beats up little kids in the school playground. And like in the playground, it's tempting to weigh in ourselves – to be the thermometer that reflects the temperature around us – but we have the power to be a thermostat and affect the temperature. In the Q&A following discussions that went overboard, I've seen audience members politely, but firmly, explain why the main criticism is invalid – to defend those unable to defend themselves.<sup>15</sup>

Other gifts of unequal value involve small acts of awareness. After a discussion, it's tempting to respond to at least the main criticism, but I've seen authors remark that the session is running late and forgo the opportunity to respond so other papers get their full time. Presenters and discussants have voluntarily shortened their own talks for the same reason. The effect of these actions goes far beyond providing a few extra minutes to the later papers. They change the temperature of a conference from

---

<sup>14</sup> I once saw a discussant point out a serious flaw in the empirical methodology of a junior author's paper at the NBER, where some think there are extra brownie points from being harsh. But rather than doing so, the discussant said "I'm sure you only did this because a referee asked you to do so. If the referee is in the audience, shame on you." As a result, the comment ended up being focused on the appropriateness of the methodology, rather than a personal attack on the author's competence.

<sup>15</sup> In addition, everyone has the right to defend themselves, but the hierarchies in our profession mean we're expected to bow down to criticism from seniors. I once saw a discussion that bordered on bullying from a chaired professor at a top school. The author responded by acknowledging that many of his points were fully justified, and said "my bad, that's on me". But she bravely pushed back on some other points – she'd worked on the paper for years and knew the data much more deeply. Standing up to bullies isn't defensive, but disseminates knowledge by clarifying to the audience what they can and can't take away from your paper.

being about marketing your own research to listening to others'. This then inspires the audience to think about how they can be a thermostat themselves.

Some acts of awareness involve talking to a conference attendee who's alone at a conference cocktail party. I don't want this to come across as an "adopt an introvert" project, but to highlight how such awareness can have an outsized effect. During the second evening of the Utah Winter Finance conference, there's no scheduled dinner to allow professors to form their own groups. One group noticed I knew virtually no-one as I was a PhD student. They kindly invited me to their dinner even though it meant they now had too many people for the car and we had to take a taxi. When the bill came, a professor remarked that I won't have a research budget and should pay less than everyone else.

In the third summer of my PhD, I was grateful to be invited to the NBER Summer Institute. I was at the dinner the evening before the conference. A social psychologist studying human behavior would have a field day at NBER dinners. When it's time to move from the drinks to the dinner, no-one sits down. They wait for the big shots to seat themselves first and then they rush to sit at the same table. Then, when you're at the table, you hang on their every word and laugh as audibly as you can at their attempted jokes.

There was an assistant professor at my table. She should have talked to everyone else at the table instead of a PhD student; she wasn't even sitting next to me so had no obligation to do so. Yet she struck up a lengthy conversation, showed genuine interest in my research, and looked at my website after the conference and dropped me a note saying she found my capital structure paper interesting (at the time, it had "limits to arbitrage" in the title). This was Wei Jiang, and we ended up writing two papers together, one on limits to arbitrage. At the time, she couldn't have calculated that there'd be any instrumental benefit to such a conversation. But, at the risk of repeating myself yet again, actions taken for intrinsic reasons might end up benefiting us unexpectedly.

## 6. Impact

The first deal I did at Morgan Stanley was selling the consumer finance division of Abbey National, a UK bank, to GE Capital. The sense of accomplishment was fantastic. The next day, the transaction hit the front page of the *Financial Times*. It praised Abbey National for obtaining a good price and, more broadly, giving credibility to its overall disposal strategy.

But the next day, the *Financial Times* had completely different headlines. I realized I'd spent 7 months of my life working on a single company's problems at a single point in time. Of course, the long-term consequences of that transaction would be far beyond a single day, but the transaction still only affected two companies, Abbey and GE.

Even in investment banking, I got exposure to the research done by Morgan Stanley's economists. I read a note on the implications of greater female labor force participation for economic growth, and thought how engaging it would be to have a job studying companies or economies as a whole, not just one client. And when I got to MIT, and started reading academic research, I realized that not only was the perspective broader than a single company, but also timeless. It wasn't about forecasting economic growth for that year, but uncovering relationships that might hold in general – not only at the time the researchers wrote their paper but also in the future.

We're lucky to work in a profession with incredible impact. Our research can be read by thousands of people, and we teach hundreds of students each year who then practice it and pass it onto their colleagues. The knowledge we create and disseminate affects how citizens save and invest for the future, how managers run companies, and how policymakers set regulations. Our research and teaching really matter.

But I need to rephrase that last sentence. Our research and teaching *have the potential* to really matter. At the moment, they don't, due to what our profession rewards and values. Two aspects are particularly underweighted: teaching and engagement.

### 6.1 Teaching

Teaching has very little effect on professional success, as long as it doesn't fall below a rather low bar. When I joined Wharton, there was a half-day orientation for new assistant professors. I asked the Deputy Dean how much weight is put on teaching in tenure decisions. He replied "Zero". If I was initially surprised, I was shocked a few moments later when he clarified "Zero or negative". He explained that, if you did too well in teaching, faculty would assume you're not serious about research – as if one is inevitably at the expense of each other.

This is sad. Our salaries are primarily paid by current students through their tuition fees and alumni via their donations. Yet our evaluation system ignores our impact on our students. It would be crazy for a professional services firm to put zero weight on what clients think of a consultant when making promotion decisions. The *Journal of Finance* is the most-cited finance journal, with a 2-year impact factor of 7.5. If we get a paper into our profession's most elite outlet, on average it will be cited by 4 other papers published each year. Yet how many students do we teach each year? 200, 300, maybe more? This isn't to say that research isn't important, and I recognize I'm comparing apples and oranges because one student isn't equivalent to one citation – but it's to say that teaching is very important. Our profession is about the creation and dissemination of knowledge, yet we pay no attention to how we disseminate information to a key stakeholder group. Just as path dependency exists for us, it also exists for our students<sup>16</sup> – a little more investment in the classroom can help them get a great job, which then sets them on a quite different trajectory.

---

<sup>16</sup> See Oyer (2008) for evidence on this point for investment bankers.

Moreover, there are significant synergies between teaching and research. Adding new material to courses can spark research ideas that take many years to publish (potentially after a tenure decision) given the time-to-build; many pioneers in FinTech started teaching it before writing papers. Condensing complex ideas into simple language for students forces you to understand them deeply. Such a skill is transferable to writing papers and giving seminars that are of interest to a general academic audience rather than just those in your niche.

The problem is serious. It can't be solved by simply increasing the weight we put on teaching ratings for tenure or hiring. Most schools' teaching ratings don't actually measure teaching, but customer satisfaction. Scores of "the quality of the professor" gauge popularity and entertainment; "how did the course meet its stated objectives?" or "how clearly did the faculty explain the course concepts?" reward easy courses with non-ambitious objectives and easy-to-explain material, and "how respectfully did the professor treat students?" penalizes instructors who tell students they've given a wrong answer, or hold them to account for consistent lateness or smartphone usage. In contrast, there are few ratings or awards that capture how much we challenge and stretch students.<sup>17</sup>

The problem has become markedly worse over my 15 years in the profession. Business *schools*, which should focus on education and view students as the product<sup>18</sup>, are becoming *business* schools, focused on customer service and viewing students as the client. One reason might be schools' growing concern with MBA program rankings, which are significantly influenced by student satisfaction. A second could be the increasing staff-to-faculty ratio<sup>19</sup>, which in turn raises administrators' influence on evaluation processes. Staff may have different objectives than faculty, such as maximizing response rates for course evaluations rather than their accuracy. As a result, some schools ask students

---

<sup>17</sup> One exception is the "Tough, but we'll thank you in five years" award at Wharton.

<sup>18</sup> The word "product" is not meant to dehumanize students. Instead, it is to highlight how one of a school's primary responsibilities is for students to graduate with much more human capital (broadly defined) than they started.

<sup>19</sup> This in turn may have a range of causes, such as rising concerns for student satisfaction, or increasing compliance requirements.

to fill in evaluations at the end of the final class. Given recency bias, there are a huge number of ways to game the system – make the final class particularly entertaining, or turn it into a revision lecture and say “this will be on the exam” so students think the course was useful (of course, defining “usefulness” in relation to the exam rather than real life). At other schools, the ratings are after the final exam, punishing instructors who give a challenging exam.

A strategic professor shouldn't want to change the system, particularly since it's easy to game. But a purposeful professor will be deeply concerned with the system. Our profession has developed many optimal contracting models where agent actions have long-lasting effects. None of those theories predict rewarding only the most short-term measure of performance available – an evaluation immediately at the end of the final class. Alternative, or at least complementary, approaches might involve students evaluating core professors at the end of their degree (to gauge whether the core class helped with electives), or all courses several years after graduation to assess whether they had long-lasting impact. The criteria themselves could be supplemented – ratings of the academic research content of an elective will encourage dissemination of our profession's primary output. However, no matter how much we improve the evaluation questions, quantitative scores will remain an incomplete measure. Just as how our evaluation of research should go beyond paper- and cite-counting to reading papers, those assessing teaching should read students' qualitative comments rather than just the scores.

## 6.2 *Engagement*

Students aren't the only constituency we share knowledge with. But our coverage in the media, our impact on policy and practice, writing practitioner summaries of our papers, responding to SEC consultations, or contributing to public debates on the most important finance issues of the day, similarly has almost no effect on professional success. This is crazy – not only does doing so directly

influence the specific topic we're contributing to, but also elevates the impact of our profession more generally. In fact, such activities can reduce our academic standing if others think that, if research can be covered by a newspaper it can't be highbrow, just as an indie musician might turn her nose up at an artist who's gone mainstream.

Writing textbooks might be met with mild nods of approval, particularly if they're for PhD students. But anything for a general audience is sometimes seen as selling out, even though the economics of writing books are unattractive (unless you write something fluffy to be sold in airport bookstores). Yet trade books are an excellent way of disseminating academic research to the public. In other business fields, such as organizational behavior and strategy, books are highly respected and seen as a natural next step after becoming an expert in a field. But for finance, the sign that you're an authority is that you write a survey paper for a Handbook or the *Journal of Economic Literature*. We see the pinnacle of dissemination as writing solely for other academics; other fields view it as influencing the practice of business.

Similarly, the relevance of the topics we're writing academic papers about has little or negative effect on success. One of my former senior colleagues told me that a "proper" theorist shouldn't do empirical work as he should be pure, and that citations and media coverage are a negative measure because pure theory should be unintelligible to the unwashed proletariat.

### 6.3 *Expanding the information set*

With all these things, I'm again not recommending a complete 180. Rigorous research must be front and center of what we teach our students, what we communicate to the media, and what we advise practitioners. There's a high personal payoff to telling practitioners what they want to hear even if not based on evidence, as they might invite you to keynote at conferences and give you lucrative consulting projects; to exaggerating your findings to the media, as they're more likely to

share it; or to writing a one-sided book that quotes research selectively as it's more likely to be a bestseller. And I fully understand the importance of basic research that doesn't have any immediate applications – this is the research that can only get done in academia. Thus, reasonable people can disagree on whether the optimal weight we put on teaching and external impact is 10%, 20%, or something else. But I'm pretty sure it isn't zero.

The weighting scheme matters beyond tenure. When candidates are up for election to director of a professional association, their biography (sent out with the slate of candidates and the basis on which the association members are expected to vote) focuses on their research contributions. Who we choose as our association representatives bears little relation to their teaching or engagement, suggesting we ourselves don't care about them. This is surprising since the actual duties of a director don't involve conducting or even evaluating research but producing public goods – which is arguably more correlated with teaching and external impact.

Those in senior positions, who evaluate junior colleagues and decide on tenure, have the power to change the system. Do we really want to be in a profession that put negligible weight on how we educate our students, how we inform practitioners, and how we contribute to the most pressing debates of our time? This is our profession, and we can decide what we value. Can those on the boards of our professional associations (including me) do more to improve our profession's external impact, rather than focusing exclusively on internal issues such as our conferences and journals, important as they are?

And those not yet tenured have the power to choose how much time we spend on teaching and engagement, *even if* our seniors put negligible weight on them. If we view our purpose as the creation and dissemination of knowledge, then these things really, really matter, regardless of whether they're extrinsically rewarded. We put effort into teaching because it's a privilege every year to meet hundreds of smart young people who will run companies, start businesses, and allocate capital. We

speak to the media because we want topical issues such as executive pay, dividends, and share buybacks to be portrayed fairly to the public rather than being subjected to one-sided bashing. We engage with policymakers because we care about the laws they pass and wish them to be based on evidence. None of these things are valued by the profession, nor are they financially compensated, but they may be some of the most rewarding activities we undertake. And, centering our worth in part on teaching and engagement rather than exclusively on publication or tenure reduces the anxiety resulting from the latter's significant dependence on luck.

My fourth year review at Wharton was very negative. I had 9 so-called A publications, but was told that my probability of tenure was so low that it was pointless even coming up; instead, I should focus on looking for another job. An hour later, an MBA student walked into my office because he had some finance questions. He saw I was upset and I told him what happened. He started consoling me and, mid-sentence, remarked offhand “wow, look at all your teaching awards”, nodding to my shelf. I lamented that they're all irrelevant because I was going to be fired. He rebutted that each award represents 200 students, so the awards collectively corresponded to over 1,000 students who I'd had an impact on.

The next day, I was teaching at the Faculty Showcase during Wharton Welcome Weekend. This is where hundreds of MBA admits check us out to decide whether to accept Wharton or another school. Even though I clearly wasn't going to be unprofessional, I feared I wouldn't be able to give my class wholeheartedly. How could I with full sincerity pump up a school that was treating me so badly? Of course, given my behavioral biases, I believed the negative signal I'd received was completely unjustified; I ignored the fact that tenure is a lifetime contract and a very high bar *should* be imposed. In my eyes, I was right and the collective opinion of over 20 senior faculty was wrong.

But I remembered I wasn't a member so much of my institution, but the profession – a profession whose purpose is, in part, to educate, challenge, and inspire MBA students. And, even as far as my

own school, I realized what a privilege it was to represent it at this event. I tried to delude myself that I could get my own back by not putting heart and soul into the class, and then some admits would go to other schools, but I'd have only been hurting myself. I recalled something my advisor Xavier Gabaix said when I told him I was leaving MIT for Lehman, giving a trivial slight I'd received at MIT as one of the justifications. He told me "never make any decision out of spite" – something that had applied to many situations since, and now applied to my present one. I had little power to affect my tenure decision, but I did have the power to live unoffended.

I delivered that lecture with the same passion and enthusiasm as I'd done every year. And it was I who benefited the most, not my employer or even the admits. The black cloud cleared from my head as the lecture reminded me why I chose this profession in the first place.

## **7. Conclusion**

This is a fantastic profession to be part of. We have the freedom to research bold, important questions we're passionate about – but we're sometimes afraid to leverage that freedom as we feel pressured to work on what's currently popular or where our expertise lies. We have tremendous power to be citizens to this profession, and our loyalty is indeed to the profession, not our institution – but some things we do are motivated by rivalry. The school we work at may not be that relevant – yet we judge members of our profession by it, contributing to our own stress when on the job market or coming up for tenure. We have the potential to make profound contributions to wider society – but the way we evaluate each other ignores such impact.

We have the power to change that. This article has proposed a number of suggestions for how the profession can be improved, which I'll summarize here.

*Decision-makers (e.g. tenure committees and hiring committees)*

- Be less risk-averse in hiring decisions; hire those with the potential to create knowledge, not just publish papers.
- Place less weight on quantitative measures and treat journal publications as less binary. Place more weight on qualitative measures, such as innovation, independence, impact, and importance. However, recognize that the optimal assessment involves a balance of hard and soft information, to avoid decisions being based predominantly on cronyism or taste.
- Evaluate the dissemination of knowledge, through both teaching and engagement. Ensure that teaching assessments reward education, rather than entertainment or popularity.
- Consider “blind” recruiting processes where faculty read papers that omit the author’s name and institution, and only see the CV and reference letters after submitting their comments on the paper.

#### *Conferences and professional bodies*

- Consider removing institutions from conference nametags, and replace them with research interests or nothing at all. Consider removing institutions from conference programs.
- Explore ways to improve the creation and dissemination of knowledge, such as hubs, forums, or mailing lists around a research area, or coordinating the profession’s response to a policy consultation.

#### *Members of the profession*

- Be bold in your choice of research topic. Pursue the questions you’re passionate about and view as important, rather than feeling pressure to work on a currently popular area.
- Recognize the significant amount of luck in this profession. Focus on inputs (what you choose to work on) rather than outputs (publications); celebrate what you can control (paper submissions) rather than what you can’t (paper acceptances).

- See yourselves as members of a profession, rather than an institution. While the “rank” of your school makes some difference, the effect is much less than commonly thought. A disappointing job market or tenure outcome has limited impact on your ability to create and disseminate knowledge.
- View colleagues in the profession as partners in the creation and dissemination of knowledge. Provide feedback on others’ papers and share others’ research, both to other academics and to the real world. Stand up for the value of academic research (over and above practitioner studies) for real-world questions.
- View dissemination of research to be an integral part of the purpose of a professor, through teaching or engagement with practitioners, media, or policymakers.
- Be comfortable with the fact that a purpose-driven approach to the profession may not maximize short-term success. However, it may lead to more success in the long-term, as well as greater life fulfilment.
- Never forget what caused you to join this profession to begin with – the freedom to work on what you want, with whom you want, when you want; the ability to discuss the latest research discoveries with global experts at conferences or seminars; the privilege of meeting hundreds of smart young students each year and teaching them skills they’ll use to manage companies or allocate capital; and the power to influence public opinion on the leading issues of the day. Keep this at the forefront of your mind through uncertainty or disappointment.

My ideas for making the profession more purposeful are only the musings of one person. Others may have quite different ideas. I hope this essay will not necessarily advance my suggestions, but encourage a discussion more generally about how our profession can become more impactful, relevant, collegial, and fun.

## References

- Admati, A. & Pfleiderer, P. (2009). The “Wall Street Walk” and shareholder activism: Exit as a form of voice. *Review of Financial Studies*, 22, 2645–2685.
- Brogaard, J., Engelberg, J., Eswar, S., & Van Wesp, E. (2020). On the causal effect of fame on citations. Working paper.
- Dai, R., Donohue, L., Drechsler, Q., & Jiang, W. (2021). Dissemination, publication, and impact of finance research: When novelty meets conventionality. Working paper.
- Daniel, K., Hirshleifer, D., & Subrahmanyam, A. (1998). Investor psychology and security market under- and overreactions. *Journal of Finance*, 53, 1839–1885.
- Edmans, A. (2009). Blockholder trading, market efficiency, and managerial myopia. *Journal of Finance*, 64, 2481–2513.
- Edmans, A. (2011a). Does the stock market fully value intangibles? Employee satisfaction and equity prices. *Journal of Financial Economics*, 101, 621–640.
- Edmans, A. (2011b). Short-term termination without deterring long-term investment: A theory of debt and buyouts. *Journal of Financial Economics*, 102, 81–101.
- Edmans, A. (2019). Finding purpose in your career. Gresham College public lecture, available at <https://youtu.be/kXVWAsiZaq0>.
- Edmans, A. (2020). *Grow the pie: How great companies deliver both purpose and profit*. Cambridge University Press.
- Edmans, A., Garcia, D., & Norli, O. (2007). Sports sentiment and stock returns. *Journal of Finance*, 62, 1967–1998.
- Edmans, A., & Liu, Q. (2011). Inside debt. *Review of Finance*, 15, 75–102.
- Harvey, C., & Hirshleifer, D. (2020): Up or out: Resetting norms for peer reviewed processes I the social sciences. Working paper.
- Harvey, C. (2017): The scientific outlook in financial economics. *Journal of Finance*, 72, 1399–1440.
- Hirshleifer, D., & Shumway, T. (2003). Good day sunshine: Stock returns and the weather. *Journal of Finance*, 58, 1009–1032.
- Hirshleifer, D. (2015). Cosmetic surgery in the academic review process. *Review of Financial Studies*, 28, 637–649.
- Hodrick, L. (2011). Ten questions – Laurie Hodrick. *Financial Times* April 12, 2011.

- Holderness, C. G. (2009). The myth of diffuse ownership in the United States. *Review of Financial Studies*, 22, 1377–1408.
- Malmendier, U., & Tate, G. (2005). CEO overconfidence and corporate investment. *Journal of Finance*, 60, 2661–2700.
- Metrick, A. (2008). How to have a successful and meaningful career. Talk at Wharton.
- Oyer, P. (2008). The making of an investment banker: Stock market shocks, career choice, and lifetime income. *Journal of Finance*, 63, 2601–2628.
- Schoar, A. (2020). Finance research for social good. Keynote speech, Western Finance Association Annual Meeting.
- Spiegel, M. (2012). Reviewing less – progressing more. *Review of Financial Studies*, 25, 1331–1338.
- Sundaram, R. K. & Yermack, D. L. (2007). Pay me later: Inside debt and its role in managerial compensation. *Journal of Finance*, 62, 1551–1588.
- Taleb, N. N. (2001). *Fooled by randomness: The hidden role of chance in life and in the markets*. Random House.
- Welch, I. (2012). Referee recommendations. *Review of Financial Studies*, 27, 2773–2804.
- Yermack, D. (2017). Corporate governance and blockchains. *Review of Finance*, 21, 7–31.