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Doctoral Dissertations, 2011-2026 Sociology

2025

THREE PAPERS ON STRATIFICATION AND CULTURE IN SCIENCE

Cook, Gavin Gillespie

Princeton, NJ : Princeton University

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THREE PAPERS ON STRATIFICATION AND CULTURE IN SCIENCE

Volume I

Gavin G. Cook

A DISSERTATION
PRESENTED TO THE FACULTY
OF PRINCETON UNIVERSITY
IN CANDIDACY FOR THE DEGREE

OF DOCTOR OF PHILOSOPHY

RECOMMENDED FOR ACCEPTANCE BY

THE DEPARTMENT OF SOCIOLOGY

Adviser: Yu Xie

January 2025

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ABSTRACT

This dissertation investigates a selection of unanswered questions from the sociology of science's heroic age and issues of contemporary interest to scholars of science. Chapter 1 investigates the hard-soft distinction. We first reevaluate and extend the Mertonian theory of 'hardness' as scientific consensus with new theoretical advances from ethnographic and quantitative studies of science and methodological advances in data processing. We then present 14 metrics that collectively summarize field-level publication, citation, communication, and collaboration norms, and we use these metrics to compare 9 fields of study to one another. We find that the hard-soft spectrum exists and mirrors popular and academic understandings of the hard-soft distinction. We suggest that theories of inter-field differences may enrich future work in science studies. Chapter 2 presents a methodological toolkit for investigating bias and inequality in the sciences using sociology as a test case. We create a metric for tracking bias in publication that we term the citation-adjusted publication premium, and we use it to show that the works of alumnae of prestigious sociology PhD programs are over-published but under-cited in sociology's three top journals. We additionally find that sociology's two house journals, the American Journal of Sociology and Social Forces, are biased in favor of PhD alumnae of the University of Chicago and the University of North Carolina – Chapel Hill respectively. Both types of bias diminish over time except for the bias in favor of University of Chicago PhD alumnae in the American Journal of Sociology. Chapter 3 applies the methodological and theoretical techniques from Chapter 1 and Chapter 2 to investigate bias against Chinese American scientists in sociology, economics, chemistry, and physics. We find that male Chinese American scientists suffer from a citation penalty and must attain more citations than their non-Chinese male peers to attain equivalent positions where female Chinese American scientists instead enjoy a citation bonus relative to their non-Chinese female peers. Taken together, the three chapters of this dissertation form a research agenda for analyzing the intersection of field-level and national cultures across the sciences and for uncovering evidence of inequality and favoritism in any of science's main institutions.

ACKNOWLEDGEMENTS

As I finish my thesis, I realize that I am in the debt of many. It is trite to say so in science and even triter to say so in science studies, but, to quote Newton, I stand on the shoulders of giants. The crazy thing about being a PhD student is that many of these giants can actually teach you - in person and in the flesh! I am very grateful for the efforts and input of many people, and I hope that you will indulge me in allowing me to thank the giants that have reared me.

I will begin by thanking my committee.

Firstly, Miguel Centeno is the reason I am a sociologist. I took The Western Way of War 10 years ago as a senior at Princeton, and that class set me on my current path. Miguel: You ferried me toward graduate school and then helped ferry me through graduate school, and you taught me volumes about how to be a scholar along the way. I am grateful beyond words for this.

In addition to being a first-rate mentor, Dalton Conley showed me by example that a sociologist could be more than an amateur in the biological sciences. This gift, given freely and unconsciously, has opened new doors for me in optimizing my health and charting a course through academia.

The joy and curiosity with which Brandon approaches the scientific craft is infectious. While I have learned a great deal about the measures and methods of social science from Brandon, I suspect that the lesson about the role of emotions in science is the more important of these lessons, and a handful of books that I read in the last month of writing this dissertation have only confirmed. Thank you, Brandon, for showing me the role of play and joy in social science.

Finally, I thank Yu Xie for his generosity and close tutelage. Professor Xie: others have written about your intelligence, and that is true, but the quality of yours that I have always admired the most is your cognitive endurance. It is the quality of yours that I most ardently seek to emulate.

David Leheny advised me on a number of independent research projects as an undergraduate, and I am grateful for his tutelage, particularly in his emphasis on the importance of clear writing!

I remember going to Michael Gordin's office as a senior at Princeton on the verge of graduation with a nascent but unclear interest in the formal study of science. Thank you, Prof. Gordin, for stoking and feeding that interest over the years and for constantly pushing me to think of how to study science in ways beyond those of the sociologists of science.

I thank Peter Lighte for encouraging me to take the leap to graduate school and for his guidance along the way.

I thank the wonderful staff of the Princeton Sociology Department, namely Amanda Rowe, Donna DeFrancisco, and Eric Altman, for everything. Yan Bennett and Phillip Rush, both of whom I am glad I can count as personal friends, were instrumental sources of intellectual inspiration during this journey.

There are a few scientists outside the academy that I wish to thank as well.

I thank Alex Kikel for providing the best example I have seen thus far of a scientist operating outside the institutions that currently govern science. The way that you work has

pushed me to rethink core assumptions that most minds in my field maintain regarding what science truly is and how it should function.

I had thought that nutrition science was a dead-end field of zero interest for many years. Chris Masterjohn showed me that I was wrong about this. Chris, your work has taught me the value of energy metabolism, and your example has shown me a few major quirks of the biomedical world and new dimensions of how biomedical research actually happens.

I would not be a scientist without my friends. I am grateful for the friendship of many people, but, in the interests of space, I will only cite friends who had a direct influence on my intellectual growth. In more or less randomized order:

Levon Avanesyan: you made my years on campus incredible and taught me more than you know about culture, tradition, and probability.

Sean King: You are a very quick wit, and while I do think science would be better with you in it, you have taught me much about how knowledge production can happen outside of science, and you have always encouraged me to think behind the US, particularly about inequality and discrimination in economic activity beyond the US.

Brendan Ma: You could have been a first-rate academic in another life - and may still be one in this life! I am privileged to count you as a mental companion.

Mike Kosk: I appreciate discussing both culture and data with you, and I thank you for sharing your insights on Slavic America.

Zach Horton: Thank you for teaching me about the beauty of faith and the richness of Catholic thought and for showing me the magic of a living tradition lived authentically.

John Maldonado: Thank you for teaching me so much about politics and the internet.

Max Simchowitz: I have greatly enjoyed our conversations about navigating the academy and biology.

Allan Jabri: I had a blast talking about PhD dreams in NoLiTa in 2016, and I thank you for sharing both your excitement about the potential of computing and the process of figuring out how to make computing better and also your more sober assessments about the state of CompSci.

Parijat Chakrabarti: I am grateful for your framing of sociology in South Asia and East Africa, and I have learned a great deal from the way you approach living as a scientist.

Audrey Tseng: Your insights on the nature of families and culture have proven very useful in a few of these papers.

Jacob Simon: Visiting you in San Francisco helped me fall in love with software at a very opportune time, and I have learned so much from your views on technology and society.

Eric Hagstrom: You have taught me a great deal about media and other cultures, and the few months we were roommates were fun, productive, and full of insight. Thank you. It is a mark of our friendship that the many, many photos you sent me of you in Disneyland on weeknights never riled my envy but instead only sparked joy.

Ian McKeachie: thank you for introducing me to rich traditions outside of the academy.

Yegor Chekmarev: I have learned so much from you about writing, politics, and media, and I am very grateful to have counted you as a constant companion in both of my stints at Princeton.

Finally, I wish to thank Brendan Kutler. Brendan - I thank you for your inspiration and continual guidance. I also thank the entire Kutler family.

Above all, I wish to thank my family, and I thank my family from the bottom of my heart. In this space alone, I cannot do justice to the many gifts that my family has given me. To my mother and father: It is an enormous privilege to be your son. I would not be where I am without you, and I hope to live up to and build upon your name into the future. I thank my brother for his support, for pushing me to think beyond the academy, and for his company as a constant and first-rate intellectual partner. I am grateful to count you as a conversation partner and for teaching me what it means to be a man.

For those who have supported me thus far: please know that this is just the beginning.

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INTRODUCTION

The heroic age of the sociology of science began after World War 2 and petered out in the 1980s. It was defined by the work of Stephen and Jonathan Cole (Cole 1979; Cole, Gary, and Cole 1988), Scott Long (Long 1978; Long and McGinnis 1981), and, most famously, Harriet Zuckerman and Robert Merton (Zuckerman 1977; Merton 1974). The sociology of science of the latter half of the 20th century was methodologically defined by a mixture of qualitative and quantitative investigation. The modern research communities that study science in a sociological context owe their origins to disciplines outside sociology, and they can be clustered into an explicitly qualitative camp defined by ethnographers of science (Latour and Woolgar 1986) and an explicitly quantitative camp that is known as the science of science (Fortunato et al. 2018). The increasing specialization of both camps has led to many valuable insights, but many of the old questions of the sociologists of science remain frustratingly unanswered. This dissertation revisits some of these quiescent debates with new advances in theory, data availability, and methodology. This work also benefits from the simple fact that science has ballooned in the 30-odd years since the heroic age of the sociology of science informally ended, and this additional time provides additional resolution to questions that long dogged sociologists of science. Chapter 1 and Chapter 2 innovate upon nascent strands of thought regarding inter-field differences and elitism in sociology, and Chapter 3 uses some of the methodological advances of the first 2 chapters to examine bias against Chinese American scientists.

Chapter 1 examines the hard-soft distinction. This topic was once a central debate in the study of science and the sociology of science in particular, and it was investigated by Merton and Zuckerman (Merton 1974; Zuckerman and Merton 1971) and Stephen Cole (1983). The paucity of data available to sociologists of science in the mid-1980s rendered it prohibitively difficult to

conclusively resolve debates on scientific hardness, and very little work has been authored on this topic in the past 3 decades. We revisit this debate with new data and recent theoretical advances in the sociology of consensus. Expanding Mertonian theories of consensus in science, we operationalize the hard-soft distinction by creating 12 metrics that measure consensus by quantifying the composite elements of four dimensions of scientific practice: publication, citation, communication, and collaboration. We broadly find that, compared to scientists in the stereotypically softer sciences, scientists in the harder sciences publish more often and in shorter formats; cite more recent literature and cite work more equitably; use more specialized vocabulary; and collaborate with more peers and more internationally. We also find that papers in the harder sciences are more likely to be written in English and to use first-person pronouns than those in the soft sciences. Overall, we suggest that the hard-soft distinction is real. Each of our 14 metrics clearly sorts the sciences into the order predicted by the hard-soft theory of science.

Chapter 2 focuses more intently on one branch of science. Where the doyennes of the sociology of science labored intensely on questions in the hard sciences, they generally overlooked the social sciences and devoted almost no attention to sociology itself. We propose to investigate sociology using the field's own methods and theories. We begin from a handful of under-cited but insightful papers on bias in sociology from the 1950s to the 1970s (Shamblin 1970; Yoels 1971), and we then turn to the branch of economics wherein economists use the methods of their own field to study economics itself. We term this subfield the economics of economics. Key findings in the economics of economics suggest that publications in economics' top 5 journals are instrumental to professional advancement (Heckman and Moktan 2020), but the publication process in the journals may unfortunately be warped by two biases: a nepotistic favoritism for PhD alumnae of a journal's host institution that economists term home bias (Card

and DellaVigna 2013) and a potential favoritism for alumnae of prestigious institutions in general that we term prestige bias (Krueger and Wu 2000; Hilmer and Hilmer 2007).

Sociologists have long wondered if there is home bias in sociology 2 house journals, namely the University of Chicago-affiliated American Journal of Sociology and the University of North Carolina – Chapel Hill (UNC-CH)-affiliated Social Forces. We ask if there is home bias in these two journals and prestige bias in sociology's top three journals, which are the American Journal of Sociology (AJS), the American Sociological Review (ASR), and Social Forces, and if this bias has changed from the 1970s to the present. To do so, we invent a metric that we term the citation-adjusted publication premium, which is a combination of a cohort- and journal-specific measure of relative citation and cohort- and journal-specific measure of relative publication success. Prestige bias as measured by the citation-adjusted publication premium in favor of sociology's top programs has declined since the 1970s, but the bias against less-prestigious programs has remained stagnant or increased since the 1970s. Home bias in favor of UNC-CH alumnae in Social Forces has improved greatly since the 1970s, but home bias in favor of UChicago alumnae in the AJS increased dramatically from the 2000s to the 2010s. The methodological toolkit presented in this chapter may be used investigate bias in any discipline at any level of resolution.

Chapter 3 uses the methodological toolkit from Chapter 2 to analyze bias against Chinese scientists in sociology, economics, chemistry, and physics. Where Chapter 2 used these methods to analyze bias towards academics with specific PhD backgrounds, we show how these methods may also be used to investigate bias against academics of specific ethnic backgrounds. There is a paucity of work on discrimination by ethnicity in academia and on Asian scientists in general. We specifically focus on Chinese American scientists because the unique history of Chinese

America, which stretches back to the 1800s, has allowed for the accumulation of many negative stereotypes against the Chinese diaspora in particular (Lew-Williams 2018) and because many scientists allege that the Trump administration's China Project, which sought to investigate corrupt ties between Chinese American scientists and Chinese institutions, has created an atmosphere of fear (Lee and Li 2023; Xie et al. 2023).

Using exact matching methods, we condition on a wide range of covariates include prestige of PhD, decade of PhD, and professorial rank, and we use the citation multiple outlined in Chapter 2 to create an index of relative citation. We find that male Chinese American scientists suffer from a persistent citation penalty that varies by scientific hardness and prestige of department. This Chinese penalty for Chinese males is greatest in softer sciences and in departments ranked in the top 50 – 100. Female Chinese scientists, by contrast, benefit from a Chinese bonus relative to their non-Chinese female peers that is strongest in lower-prestige departments in the soft sciences. Read in the context of Chapter 1, Chapter 3 is not only an investigation of bias in the sciences but an exploration of the field-level cultures of four scientific disciplines. We show that the Chineseness of a field is a reliable metric scientific hardness, and we ask why the hard sciences have been so receptive to scholars of Chinese descent.

Taken together, this dissertation advances a methodological toolkit for investigating inter-field differences in scientific practice and inequality either within or between fields and posits that the culture of science – broadly construed – may be investigated quantitatively.

Science Hard and Soft: An Empirical Approach to Evaluating Scientific Consensus with Big Data

Abstract

Though the terms “soft science” and “hard science” have long been used in both popular and academic discourse, many scholars of science studies argue that the hard–soft distinction does not exist. While the two most active fields of science studies, the science of science and the ethnography of science, have largely ignored questions of inter-field difference, the Mertonian school of the sociology of science explored how science varies between fields at length. We return to this age-old question with new data, theoretical approaches, and computational techniques. We first conduct a survey of the educated and general public to determine general perceptions of the hard–soft hierarchy in the sciences. We then merge contemporary advances in science studies with classical work in the sociology of science to create 14 metrics that explore the practice of science using big data. We find that the hard–soft spectrum exists, and public understandings of the spectrum comport almost exactly with empirical measures of hardness. We conclude by illustrating how a focus on inter-field variation in scientific practice can enrich current work on science studies.

Keywords: Big data, science of science, sociology of science, hard science, soft science

INTRODUCTION

It is well accepted both in public and in academic circles that some fields of science are "soft" while others are "hard." In the public imagination, a hard science may be defined by mathematization, experimental methods, or perceived rigor. In academic contexts, "hardness" has been used to refer to the degree of objectivity (Storer 1967) or the rate at which knowledge accumulates (Price 1970) in a science. The terms "hard science" and "soft science" first entered the English vernacular in the 1960s, and their use has increased year after year ever since. When the terms were first used in a sociological context, it was assumed that the audience would readily understand their meaning (Storer 1967). More recently, however, this assumption about their accepted meanings has been challenged (Shapin 2022; Knorr-Cetina 1981).

Academic confusion over the import or lack thereof of the hard–soft distinction is not due to any abatement in the voluminous academic work in the many scholarly communities that comprise science studies.[1] The past four decades have seen a remarkable outpouring of innovative work on the sciences, much of which has taken place within two schools of scholarship. The first of these two schools, which we shall term the ethnography of science, applies methods that have historically been used to study shamans of indigenous cultures to instead study the shamans of the modern West, namely scientists (Latour and Woolgar 1986, 29). By stripping the mystique of prestige from science, the ethnographers of science conclude that scientific facts are partially constructed through social processes (Latour and Woolgar 1986); a scientists' preconceptions can influence every stage of analysis that leads to an ostensibly

[1] These include the history of science, the sociology of science, the economics of science, and, as we outline above, the two newest fields: the ethnography of science, often referred to as "science and technology studies," and the science of science.

objective final product (Dumit 2004). For example, ethnographers of science have shown that scientists value graphs for their ability to lend an air of respectability to scientific output but that the production of graphs may be warped by, among other things, the vagaries of software packages, field-specific norms, and individual scientists' errors (Lynch 1985; Lynch and Woolgar 1990; Coopmans et al. 2014). Even things that may seem inconsequential to the practicing scientist, such as the structure of a given scientific organization, can profoundly shape the types of knowledge that an organization can produce (Vertesi 2015; Vertesi 2020). The central conceits of this school are that science may not be objective as scientists would like to think and that this lack of objectivity is only possible to see with an emphasis on the mundane realities of scientific practice.

The second burgeoning school calls itself the science of science (Fortunato et al. 2018). The scientists of science, many of whom are former physicists or computer scientists, have harnessed advances in statistical methods and computational power to realize the dreams of mid-century informaticians and bibliometricians by mining databases of tens of millions of scientific papers (Wang and Barabási 2021; Azoulay et al. 2018). To pick one example, the scientists of science have advanced theories of how scientific teams function. They have shown that science is now a group profession; most research in most fields is now performed by teams instead of solo scholars, and the work produced by teams is cited more than that of solo authors (Wuchty, Jones, and Uzzi 2007). The benefits that come with collaboration, however, are remarkably heterogenous. As there is a citation premium for papers published by teams, so too is there a citation premium for teams of authors from diverse disciplinary backgrounds and with both male and female authors (Zheng, Li, and Wang 2022). However, while work from smaller teams may not be cited as highly, smaller teams tend to produce more original work (Wu, Wang, and Evans

2019). These and other findings in this field have been made possible by the use of big data, and they form but one part of a larger corpus of new work in the rapidly evolving discipline of computational social science (Salganik 2018).

Nonetheless, while sociologists of science have left the question of science hard and soft unanswered, ethnographers of science and scientists of science have left it unasked. In this paper, we bridge the methodological focus on big data in the science of science with the theoretical emphasis on scientific process in the ethnography of science to re-examine perhaps the most significant unsolved theoretical problem in the sociology of science: is scientific hardness a sociologically meaningful concept? We do so by first conducting a survey of the general and educated public to establish how non-scientists view this concept. We then create 14 new metrics that use big data to capture the variation in the practice of science by the hardness–softness dimension. We draw upon a dataset of more than 90 million papers spanning 1990 to 2020 from the Microsoft Academic Graph bibliometric database. These metrics measure the four main pillars of the scientific process: publication, citation, communication, and collaboration. Our results support Harriet Zuckerman and Robert K. Merton's (1971) original insights that consensus varies across the sciences in a manner that corresponds very closely to what most call "hardness" and "softness."

We show, firstly, that scientific hardness exists. Our metrics sort the sciences into a predictable hierarchy that is mirrored almost exactly in the hierarchy generated by our survey respondents. We also demonstrate that the relative softness or hardness of a science has profound consequences for norms of publication, citation, communication, and collaboration. We also offer two additional metrics that are suggestive of broader differences in objectivity, generalizability, and internationalization between the sciences. We conclude by presenting how

an emphasis on inter-field differences may enrich future work in the ethnography of science and the science of science.

TOWARD A NEW SOCIOLOGY OF SCIENCE

The ethnographers of science have shown by "thick description" how science works at the microscopic level, and the scientists of science have shown with big data how science works at the macroscopic level. Although the two schools have different research foci, scholars in both traditions have studied science under a premise that, for simplicity's sake, we will refer to as the "implicit assumption of scientific homogeneity" (IASH). It is easiest to illustrate this assumption by pointing to a countervailing trend: instead of comparing science across different fields, both ethnographers of science and scientists of science almost exclusively compare science to non-scientific domains. In the process of demystifying the uniqueness of science, both the ethnographers of science and the scientists of science have revealed the remarkable commonalities between science and non-science; for example, scientists of science have analyzed commonalities between citations in private industry and citations in scientific journal articles (Mukherjee et al. 2017), as well as between scientific papers, films, and paintings (Candia et al. 2019; Liu et al. 2021). But if they have ever found evidence of variation between the sciences, they have not done so intentionally.[2] In conducting such a volume of often ground-breaking research on the parallels between science and non-science, both schools implicitly assume that scientists in different fields of science behave more or less the same.

[2] A few papers have unintentionally presented evidence of inter-field differences without discussion of the implications of their results. Wuchty et al. (2007) and Fortunato et al. (2018), for example, both show that team size has increased over time but that papers in the engineering sciences are authored by larger teams than in the social sciences.

This was not always the case. In the early 1960s to the 1980s, the pioneers of the sociology of science, notably including Robert K. Merton, were interested in scientific hardness on the premise that hardness had consequences for the elements of science with which they were most concerned: the cultural norms and reward systems of science (Shapin 2022; Cole and Cole 1967, Zuckerman 1977). Merton and Zuckerman (1972) demonstrated that, in more codified (i.e., harder) fields, papers became obsolete earlier, and young scientists rose to prominence earlier than in less codified fields. Zuckerman and Merton (1971) and also Hargens (1988) found that journals in the "hardest" fields had rejection rates as low as 11 percent whereas those in the "softest" fields were as high as 83 percent. This, moreover, was found to be entirely independent from issues of space shortages in journals, a factor often credited with causing such variance. Zuckerman and Merton, along with the vast majority of their colleagues during this age of the sociology of science, did not subscribe to the IASH. It was clear to Zuckerman and Merton that scientific hardness was real and thus correlated with sociologically meaningful differences in the behavior of scientists across disciplines.

While most sociologists of science in this period viewed hardness as a meaningful sociological concept, a select few did not. Stephen Cole, a longtime collaborator of Zuckerman and Merton, tested their theory of codification with empirical rigor and proposed a wide battery of metrics that were intended to uncover a "hierarchy of the sciences" (Cole 1983). Cole found little evidence of variance across fields. Since then, the question has been left unanswered. Indeed, as early as 1988, Hargens lamented that most work in science studies had "tended to emphasize similarities across scholarly fields rather than differences between them" and cited early work in the then-nascent fields of the ethnography of science and the science of science to justify his claim (Hargens 1988, 139).

When the sociologists of science studied the hard–soft distinction in earnest, they generally framed the distinction as a spectrum of between-field variance in the degree of agreement over key principles. This agreement has been variously referred to as consensus, consolidation, and codification, and this spectrum places the high-consensus natural sciences at one end and the low-consensus social sciences at the other. The intellectual origin of this sociological insight was Thomas Kuhn's concept of the scientific paradigm.[3] Initially described in *The Structure of Scientific Revolutions* (Kuhn 1996), a Kuhnian paradigm is a crystallized and agreed-upon set of principles that emerges in the wake of a scientific revolution. It allows for what Kuhn calls "normal science": the steady and reliable accumulation of results in an agreed-upon theoretical framework. Kuhn's work does not discuss inter-field differences, noting only that the social sciences were pre-paradigmatic at the time in which he wrote; the initial inspiration for his theory of the scientific paradigm was ironically sparked when he witnessed debates among social scientists (Kuhn 1996, 21).

Merton and Zuckerman's concept of codification[4] is similar to the Kuhnian paradigm in that it takes within-field consensus as its starting point, but, unlike Kuhn's paradigm, codification was explicitly designed for the study of inter-field differences. Codification "refers to the consolidation of empirical knowledge into succinct and interdependent theoretical formulations," and Merton and Zuckerman note that "the various sciences and specialties within them differ in the extent to which they are codified" (Merton and Zuckerman 1972, 507). Because Merton and

[3] While Kuhn was originally a philosopher, many sociologists of science have adapted or otherwise responded to his work. Kuhn's work is thus a key component of the canon of the sociology of science's period of work on the hard–soft distinction.

[4] While Merton and Zuckerman do not explicitly label their work as a response to Kuhn, Kuhn's influence on their work is evident; the essay cites Kuhn's *The Structure of Scientific Revolutions* two separate times and mentions Kuhn by name a total of 15 times. Merton and Zuckerman name their version of Kuhn's paradigm theory "codification."

Zuckerman's explanation of codification comprises only three paragraphs in a deliberately "exploratory" essay[5] (Merton and Zuckerman 1972, 559), the theory must be both interpolated and then extrapolated before it is possible to derive metrics from it.

We will begin with interpolation. The essence of both Kuhn's theory of scientific paradigms and Merton and Zuckerman's theory of codification is scientific consensus. Merton, Zuckerman, and Kuhn all assert that the degree to which scientists agree on the core tenets of their fields has important consequences for the practice of science, and they also argue that the extent of this consensus varies by field. We proceed from this insight to ask how consensus affects scientific practice across four universals of science: publication, citation, communication, and collaboration. Specifically, we predict that consensus manifests in the publication process as greater brevity and speed to publication (i.e., a greater preponderance of journal articles and conference proceedings relative to books and book chapters), in citation patterns as speed of accumulation and speed of obsolescence (i.e., how recent citations are, on average) and equality of distribution, in communication as increased rates of usage of specialized vocabulary, and as a holistic increase in all types of collaboration (i.e., more multiple-author, inter-institutional, and international research teams). We derive 12 metrics from these four dimensions and propose an additional two metrics from other theoretical sources for a total of 14 metrics. These metrics sort the sciences into a regular and predictable hierarchy that mirrors understandings of the hard–soft distinction among the general public.

[5] The essay mostly explores how age influences the reward structure of the sciences.

The Sociological Consequences of the Hardness of Science

Publication

Publication is a core component of scientific practice (Fyfe et al. 2022). The written form is the primary product of any scientific discovery (Vertesi 2020, 155), and all scientists in all fields are expected to "publish or perish." Publications allow knowledge to accumulate by recording new findings in an immutable and easily circulated form at the time of discovery. This helps properly assign recognition and credit for work and build community. Because publication is so universal, we can compare differences in the publication process across the sciences to find evidence of variation in scientific practice by hardness. Though scientists define themselves primarily by performing field-specific research, they all spend a significant amount of time reading and writing (Latour and Woolgar 1986, 49). In the 1980s, Latour and Woolgar stressed the importance of analyzing formal scholarly communications over the then in vogue "invisible colleges" because "almost without exception, every discussion and brief exchange observed in the laboratory centered around one or more items in the published literature" (Latour and Woolgar 1986, 52).

Though all scientists publish their work, the forms these publications take may vary by the degree of consensus in a given field. History, for example, is famously a "book discipline," and historians have debated the advantages and disadvantages of this book-centricity for their field (Putnam 2015; Cronon 2012). Even in fields that are not as book-dominant as history, books serve as implicit manifestos for new fields of study (Lewenstein 2007).[6] The greater word count of books creates more room for exposition, and classic books in certain fields, such Darwin's *On the Origin of the Species*, have long served as the canonical explanations of the

[6] This may explain the relative abundance of books in computer science.

principles undergirding certain scientific paradigms (Kuhn 1996, 10). Dashun Wang and Albert-László Barabási, two contemporary scientists of science with physics backgrounds, note in passing that "in some branches of the social sciences and humanities, books are the primary form of scholarship," while "computer scientists choose conference proceedings rather than journals as their primary venue to communicate their advances" because of the field's "rapidly developing nature" (Wang and Barabási 2021, 9). Wang and Barabási remark upon differences in primary publication type across fields to stress that it may be difficult to compare productivity across fields but stop short of either clarifying why this difference may exist or exploring the implications of this remarkable difference. In this study, we hypothesize that harder sciences publish in formats that reach publication faster and empirically test this hypothesis.

Sociologists of science have fruitfully investigated inter-field variation in the publication process. Regarding differences in publication format, Kuhn suggests that a shift from books to articles defines the transition from pre-paradigmatic science to normal science (Kuhn 1962, 20).[7] We can investigate this by comparing the percentage of publications that are books, book chapters, journal articles, or conference papers across fields. We also ask if there is variation in publishing patterns between the sciences within a given format. Do different fields of study produce different types of journal articles? In their investigation of journal rejection rates, Merton and Zuckerman note that "articles in journals of physics are ordinarily short, typically running to only a few pages of print" (Zuckerman and Merton 1971, 79). In the same article, they suggest that differences in acceptance rates between journals from different fields may "reflect

[7] Anthony Biglan, a psychologist who worked on this issue in roughly the same period as Zuckerman and Merton, similarly suggests: "Compared to scholars in soft or nonparadigmatic areas, those in hard or paradigmatic areas publish fewer monographs and more journal articles" (Biglan 1973a, 211).

differences in the extent of agreement on standards of scholarship in the various disciplines" (Zuckerman and Merton 1971, 77). We extend their insight and hypothesize that differing levels of consensus (and thus hardness) result in variations in journal article page length. Ellison (2002, Table 1) presents the mean page length for articles in journals in a range of fields to show a holistic increase in journal article length across all fields but does not comment on differences in page length between fields. We draw from a much wider sample of journals over a longer time frame, and we additionally perform our calculations at the field level.

We hypothesize that consensus is reflected in the realm of publication in speed to print and brevity, measured in terms of three metrics: proportion of books, proportion of conference papers, and journal article page length. We first suggest, following Kuhn and then Wang and Barabási, that authors in harder, higher consensus fields publish their work in shorter formats that reach publication in a shorter time. By calculating the proportion of books and the proportion of conference papers published in given fields, our results confirm that the softer sciences are book-dominant where the harder sciences are conference-paper-dominant. We also present supplementary analyses on proportions of book chapters and journal articles by field. We then ask if these between-format differences also hold true for within-format comparisons. To do so, we analyze journal articles, the most common publication type in all of the sciences, in isolation and ask if journal article page length in the hard sciences is shorter than that of the soft sciences. In summary, we derive the following testable hypotheses regarding the variation of publication practices across fields:

Hypothesis 1: Sciences with high consensus do not require authors to extensively cite past findings or explain their theoretical approaches in great detail because this knowledge is shared

among and agreed upon by most scientists in the field. Consequently, scientific hardness manifests in the publication process as speed to print and brevity.

- Hypothesis 1a: Proportion of books. A smaller proportion of work is published as books in the hard sciences than in than the soft sciences.
- Hypothesis 1b: Proportion of conference papers. A larger proportion of work is published in the form of conference proceedings in the hard sciences than in the soft sciences.
- Hypothesis 1c: Journal article page length. Journal articles in the hard sciences are shorter than journal articles in the soft sciences.

Citation

If publication is the first universal of science, citation is the second. Soon after the establishment of the first scientific journals, it became customary to cite one's sources to advance an argument (Porter 1964). Citations are also the primary currency of science, the units of recognition on whose basis evaluations are made and rewards are disbursed. The number of citations a paper earns is viewed as a proxy for its impact, and the total number of citations a scholar garners is commonly used to evaluate their importance (Moed 2005). Because citation is universal, we can study variance in the practice of citation between the sciences. According to the Mertonian school of the sociology of science, differing levels of scientific hardness should lead to differences in citation behavior. If there is a high degree of consensus, knowledge can accumulate more quickly and so the harder sciences move past canonical papers more quickly. This will generally manifest in citation of more recent papers, a greater capacity for academics to achieve high citation counts at a younger age, and a more equitable distribution of citations in the harder sciences.

The quantitative study of science has long been interested in citation patterns, with much attention paid to how citations age, the inequality of citations, and the age at which scholars are cited. Quantitative work on science conducted in the 1920s found that citations can be modeled with half-life decay curves (Lotka 1926), and an extensive research tradition surrounding this question developed in the intervening century in the quantitative branch of the sociology of science (MacRae 1969; Price 1970) and later in the science of science (Radicchi et al. 2008; Yin and Wang 2017). One strand of this literature concerns the age of references cited by papers, but the majority of these studies focus on single fields (Matricciani 1991; Gupta 1997; Sinatra et al. 2015). The handful of works that do explore inter-field differences in citation practices were conducted in the age of the sociology of science's exploration of the hard–soft distinction, and they show that publications in the harder sciences cite, on average, more recent publications than those in the softer sciences (Burton and Kebler 1960; Cozzens 1985). While these results hold true for articles published before the personal computing revolution, the advent of online journals has radically altered citation behavior across the sciences (Evans 2008). This transformation has fueled a larger research agenda in the science of science that has revealed a rise in the inequality of citations across the sciences (Nielsen and Anderson 2021). If citations are a form of currency, then science is profoundly and increasingly unequal. Of all papers published in five leading scientific journals in 1990,[8] the most highly cited 1 percent in each collected around 17 percent of citations in 2010 (Barabási, Song, and Wang 2012). In terms of the Gini coefficient of

[8] The five journals are *Science*, *Nature*, the *New England Journal of Medicine*, *Proceedings of the National Academy of Sciences of the USA*, and *Cell*.

inequality, the distribution of citations across all of the sciences is less equal than the distribution of wealth across between all people on the planet (Nielsen and Andersen 2021).[9]

As inequality has elicited investigation, so has the related question of age and age structure. It is often said that "science is a young man's game" (Merton and Zuckerman 1972, 513) or that science progresses "one funeral at a time" (Azoulay, Fons-Rosen, and Graff Zivin 2019). The scientists of science have found that folk maxims of scientific success may not hold true; whether science is a game dominated by the old or the young varies greatly with the historical epoch in which scientists work (Jones and Weinberg 2011). They have also shown that when accounting for productivity, which tends to wane as scientists age, a scientist's most cited paper may be published at any point in the course of his or her career (Sinatra et al. 2016). While the scientists of science have revealed a great deal about how scientists cite the work of others at a macroscopic scale only visible with the benefit of big data, they have so far devoted very little attention to how these patterns may vary systematically by field.

Sociologists of science have long explored how variations in consensus drive inter-field differences in citation practices.[10] One of the first metrics ever used to explore citation age, the immediacy index, was invented by Price (1963) to study inter-field differences. The immediacy index refers to the percentage of references in an article that were published within five years of the article's publication. In a survey of immediacy indices across fields, Price found that papers in the harder sciences cited more recent work than papers in the softer sciences and observed that the "hierarchy" of his results comports "very well with what we intuit as hard science, soft

[9] In 2015, the Gini coefficient of citations across all of the sciences was 0.70. This is greater than the Gini inequality coefficient for all individuals on Earth, which was estimated in 2015 to be 0.65 (Hellebrandt and Mauro 2015).
[10] It is also worth noting that there is a small literature in the management sciences on this topic. See Pfeffer 1993 for an example.

science, and nonscience as we descend the scale" (Price 1970, 12). Zuckerman and Merton tabulated immediacy indices for additional journals and similarly noted that "the social sciences are intermediate to the physical sciences and the humanities in degree of codification" (Merton and Zuckerman 1972, 509).

Merton's theory of the Matthew Effect (Merton 1968, 1988), which states that successful scientists accrue additional advantages compared to less eminent scientists for the same unit of output, was originally conceived as specific to science but has since been revisited by scientists of science and has motivated research in sociology, management science, and education studies (Bol, de Vaan, and van de Rijt 2018; Azoulay et al. 2014; Powell and Snellman 2004; Stanovich 2009). In this long tradition of studying scientific inequality, some sociologists of science have specifically investigated inequality of citations. Cole examined the distribution of citations across journals but did not find evidence that the Gini coefficient of citations varied across the sciences (Cole 1983). A team of scientists of science have unintentionally found preliminary evidence that the hard sciences are more egalitarian than the soft sciences, but, because their work was conducted under the IASH, they simply presented this difference without comment or explanation (Pan et al. 2018).

As inequality has elicited investigation, so has the related question of age and age structure. The sociologists of science made age structure a key focus of their research (Zuckerman and Merton 1971; Cole 1979), and Merton and Zuckerman's theory of codification was conceived in part to track how the role of age varies across the sciences. This was difficult to track with the data available to Merton and Zuckerman at the time in which they wrote, but they presented tentative findings that showed differences in the intensity of gerontocracy between fields. Even in the 1960s, the social science professoriate was notably older than the

professoriate in the natural sciences (Merton and Zuckerman 1972). This may be because consensus enables the objective and transparent evaluation of the work of young scientists. In the softer sciences, a lack of consensus inflates the importance of prestige, and "the personal and social attributes of scientists are more likely to influence the visibility of their ideas and the reception accorded them" (Merton and Zuckerman 1972, 516). Following this, Merton and Zuckerman predicted that "work by younger scientists who, on the average, are less widely known in the field, will have less chance of being noticed in the less codified sciences" (Merton and Zuckerman 1972, 516). They were unable to test this hypothesis because they never had access to big data bibliometric databases that cover scientists' entire careers and even entire fields of study. We now fortunately have the data to do so.

We examine Merton and Zuckerman's hypothesis that younger scientists are more easily recognized in more codified fields with a simple test: how far into a scholar's career does he or she publish his or her most cited paper? We can answer this by comparing the average age of publication for the most cited works of scholars between fields. If it is true that the more consolidated sciences incorporate work from younger scholars more quickly than do the less consolidated sciences, we would expect to see that scholars publish their most cited works when they are relatively older in the soft sciences and relatively younger in the hard sciences. We additionally return to unanswered questions of inequality in citation patterns by field and the age of citations by field. Overall, we propose three metrics to measure inter-field differences in citation behavior: average age of citation, Gini coefficient of citation inequality, and average age at publication of most cited paper.

In more codified sciences, papers are generally evaluated more objectively,[11] so accomplished researchers do not enjoy as much of a "halo" for future work (Thorndike 1920). Such a halo effect creates inequality (Thorndike 1936, Mulford et al. 1998), and the weakening of the halo effect in the harder sciences means that they are generally more equitable than the softer sciences. It is also the case that consensus allows normal science to proceed relatively unencumbered by factional strife and fosters the rapid accumulation of knowledge (Kuhn 1962). The end result of both of these factors is a general trend toward immediacy in all areas of citation behavior in the hardest sciences. In the harder and more codified sciences, papers cite more recent literature, implying that knowledge accumulates more quickly in sciences with high levels of consensus. Additionally, citation patterns are more equitable in the harder sciences, suggesting that hardness is correlated with an attenuation of the halo effect. Finally, the weakening of the halo effect in harder sciences allows for more objective evaluation of papers. This allows rising stars to rise faster, which means that authors in the harder sciences publish their most cited papers at younger ages.

Hypothesis 2: Scientific hardness manifests in citation behavior as an attenuation of the impact of researcher prestige on a paper's success and as an increase in the rate of knowledge accumulation and obsolescence. Consequently, scientific hardness manifests in the publication process as greater speed of accumulation and speed of obsolescence of citations, and greater equality of distribution.

[11] There is an extensive literature on prestige hierarchies in anthropology. While this is beyond the scope of this paper, see Cheng et al. 2013 and Henrich and Gil-White 2001.

- Hypothesis 2a: Average age of citation. Due to greater consensus, there is less need to cite old literature in the hard sciences. Papers in the harder sciences cite more recent literature.
- Hypothesis 2b: Gini coefficient of citation inequality. Because the harder sciences move past canonical papers more quickly than the softer sciences, citation distributions in the harder sciences are more equitable.
- Hypothesis 2c: Average age at publication of most cited paper. Because accomplished researchers in the hard sciences do not benefit as much from their prestige as scientists in the soft sciences, rising stars rise faster in the hard sciences. Scientists in the hard sciences publish their most cited papers at younger ages.

Communication

It has been well understood that scientists tend to use esoteric jargon (McAllister 1955). Jargon commonly refers to "special words or expressions that are used by a particular profession or group of people, and are difficult for others to understand,"[12] and the usage of jargon in science is intimately related to the history of specialization in the sciences. As medicine became professionalized in modern Europe, medical vocabulary specialized in tandem. Physicians in medieval Europe used the Latin vocabulary of Celsus and Galen, but after the 1950s physicians began to use creative hybrids of Greek and Latin to create neologisms that reflected advances in medical practice (Wulff 2004). This practice is mirrored in all of the biological sciences, including evolutionary biology (Mayr 1978), and the rest of the natural sciences (Dörries 2002).

[12] See https://www.oxfordlearnersdictionaries.com/definition/american_english/jargon.

The development of specialized vocabulary is one of the hallmarks of the professionalization process (Wilensky 1964, 148), and consensus surrounding specialized vocabulary is essential for normal science (Kuhn 1962). The greater consolidation of the hard sciences may be reflected in the language of the hard sciences with frequent usage of field-specific jargon.

Although previous research has not interrogated the written language of science very closely, it has analyzed the visual language of science via scientific graphs. Latour suggests that the main purpose of the laboratory is to render "anything and everything … transformed into inscriptions" (Latour 1990, 22). Inscriptions, such as graphs, are "immutable mobiles" that can be freely transported between scientists while retaining their content, and they are central to the practice of science (Latour 1990, 26).[13] Before Latour, however, an early scientist of science, William Cleveland, suggested that tabulating the proportion of journal article space devoted to graphs would rank the sciences in terms of hardness (Cleveland 1984). This metric, termed Fractional Graph Area (FGA), reveals clear and predictable inter-field differences in the practice of science that followed general understandings of the hard–soft distinction: papers in the natural sciences are the most graph-heavy whereas papers in the social sciences are graph-sparse but text-heavy. While graphs may be necessary for certain types of science, they are not sufficient by themselves. Graphs require an accompanying specialized vocabulary: "no scientific discipline exists without first inventing a visual and written language which allows it to break with its confusing past" (Latour 1990, 36). Graphs are of particular use to science because they are "mobile, flat, reproducible, still, and of varying scales," and they can be "reshuffled and recombined" (Latour 1990, 45).

[13] Historians of science have also suggested that scientists use graphs to serve to emphasize the objectivity of their results (Gross, Harmon, and Reidy 2002).

These advantages apply equally to scientific jargon, for a specialized neologism in science represents the condensation of a single concept in linguistic form. An emphasis on scientific vocabulary was central to Kuhn's theory of scientific paradigms. Paradigms are crystallized in specialized vocabulary, and the expansion of a given paradigm is mirrored in the increasing size and complexity of its jargon. Kuhn notes that the development of a paradigm "ordinarily calls for the construction of elaborate equipment, the development of an esoteric vocabulary and skills, and a refinement of concepts that increasingly lessens their resemblance to their usual common-sense prototypes" (Kuhn 1962, 64). Contention over shared vocabulary is a key process in disputes between competing paradigms, and Merton and Zuckerman's theory of codification may be fruitfully extended to incorporate a focus on vocabulary as well. Merton and Zuckerman contend that "codification refers to the consolidation of empirical knowledge into succinct and interdependent theoretical formulations," but they do not delineate the form that these "theoretical formulations" ultimately take. The most "succinct" form a theory may take is that of a single, specialized word, and Merton elsewhere suggests that the highest form of scientific reward, above even the Nobel Prize, is for a law or a finding to be named after a given scientist (Merton 1974, 298). Some of the theories that were rewarded with eponymic names, such as Darwinism or Newtonian physics, define particular paradigms in their respective fields. While earlier sociologists of science may not have theorized in detail about how the usage of scientific vocabulary varies across the sciences, their work nonetheless invites us to do so.

Besides creating differences in the usage of jargon, hardness may also affect the use of language in other ways. We suggest that high levels of consensus allow scientists in the hard sciences to adopt single-word neologisms whereas researchers in the soft sciences are forced to create multi-word phrases from pairs of less technically specific words. This hypothesis emerges

naturally from a close reading of articles across the sciences. Using a recent edition of *Science* as an example, we can compare the titles of two articles from a hard and a soft science: "Zygotic genome activation by the totipotency pioneer factor Nr5a2" (Gassler et al. 2022) for biology and "A causal test of the strength of weak ties" (Rajkumar et al. 2022) for the social sciences. The biology title is a relatively straightforward summary of the results of the paper, and it makes ample usage of Greco-Latin constructions. The social scientific article, by contrast, uses two-word phrases, such as "causal test" and "weak ties." While the phrase "weak ties" is immediately recognized by the sociologists as a reference to a particular social scientific concept (Granovetter 1973), the words "weak" and "tie" in isolation are very common. "Zygotic" and "totipotency," by comparison, are rarely encountered outside of specialized discourse.

We posit that the hard sciences feature significantly more jargon than the soft sciences. Consensus creates the conditions for jargon; if scientists agree on the core principles of their field, then they are more able to craft specialized language to refer to the concepts that follow from these principles. We chart this by analyzing the average word frequency of abstracts across the sciences. We additionally predict that the hard sciences use single word neologisms where the soft sciences use multi-word phrases wrought from common words. We test this by showing variation in the percentage of non-repeated words and average sentence length in abstracts across the sciences.

Hypothesis 3: Hard sciences feature significantly more jargon than the soft sciences. More specifically, the hard sciences use single-word neologisms whereas the soft sciences use phrasal combinations of otherwise common words.

- Hypothesis 3a: Average word frequency. Greater consensus means that scholars in the hard sciences are able to create elaborate systems of specialized language. Therefore, the average word frequency in a hard science abstract will be significantly lower than the average word frequency in a soft science abstract.
- Hypothesis 3b: Average sentence length. Because the soft sciences use compound words, sentences in the hard sciences are longer than those in the harder sciences.
- Hypothesis 3c: Percentage of non-repeated words. Because the soft sciences use compound words, more non-repeated words are used in the abstracts of papers in the soft sciences than in those of the hard sciences.

Collaboration

Collaboration is not necessarily a scientific universal, but, in the 21st century, it may as well be. The days of the lone scientist are over, and science is now primarily a team-based pursuit. A supermajority of papers are published with co-authors (Nielsen and Andersen 2021; Wang and Barabási 2021). Even when scientists publish alone, they necessarily share their findings immediately with colleagues, for science is a social process (Latour and Woolgar 1986).

Both ethnographers of science and scientists of science have explored the role of teams in science (Vertesi 2020), with the latter group having specifically explored trends in collaboration across the sciences. Collaboration between teams from different institutions and different nations is on the rise, and papers published by inter-institutional and international teams tend to be more highly cited (Jones, Wuchty, and Uzzi 2008; Adams 2013). Team size is one of the few examples for which the scientists of science have examined cross-field differences, but they have only done so unintentionally. A handful of papers have shown that team size varies by hardness, but

they only do so at the level of the macro-field. One study presents a comparison of how team size has changed over time for three macro-fields: science and engineering, social sciences, and arts and humanities (Fortunato et al. 2018, Figure 4), indirectly finding that hardness correlates with team size. Using the same three-macro-field framework, another study demonstrates how inter-university collaboration varies but only does so to stress "the generality of these patterns across fields" (Jones, Wuchty, and Uzzi 2008, 1259). While there is promising evidence of variation in collaboration behavior across the sciences, pervious research has only hinted at the sociological implications of hardness for collaboration.

The sociologists of science suggested that hardness dramatically affects collaboration. In the same article in which they outline codification, Merton and Zuckerman (1972, 546–547) present the proportions of papers co-authored from 1900 to 1959 in three macro-fields – the natural sciences, the social sciences, and the humanities. They find that codification is correlated positively with scientific collaboration, though they do not explore why this may be so. Their pioneering work has been extended with empirics by other studies. The first concerted empirical effort was Lodahl and Gordon's (1972) test of Kuhn's theory of the scientific paradigm.[14] Lodahl and Gordon began with a survey of professors in top graduate departments in seven fields: sociology, political science, economics, psychology, biology, chemistry, and physics. After first asking each respondent to rank the paradigm development of his or her field in comparison to that of the others, Lodahl and Gordon then gathered information about the number of teaching assistants and research assistants that the respondents employed and graduate students they advised. Finally, they recorded each respondent's degree of satisfaction with each of these

[14] Though Lodahl and Gordon 1972 was not a formal test of Merton and Zuckerman's theory of codification, the paper was heavily informed by Merton and Zuckerman's previous work comparing science hard and soft, such as Zuckerman and Merton 1971.

quantities. They found that professors in the softer sciences had fewer students and research assistants and, moreover, were more likely to not want any more students or research assistants. More paradigmatic sciences allow for easier collaboration at all levels of scientific practice; in the absence of a shared paradigmatic foundation, collaboration in the softer sciences can be more difficult and less rewarding. Consensus standardizes the scientific training process and fosters a shared discursive and cognitive environment for all scientists in the field. It may be said that consensus strengthens the development of field-specific scientific cultures that, like other types of culture, foster exchange and understanding even across institutional, regional, and perhaps even national or linguistic boundaries.

We return to the question of how hardness affects scientific collaboration with a new emphasis on the practice of science. Instead of asking scientists about their subjective views about collaboration, we directly observe their collaboration practices at scale using big data. We propose three metrics to chart collaboration by field: average number of authors per paper, average number of institutions per paper, and average number of nations per paper.

Hypothesis 4: Collaboration of all types is more frequent in the hard sciences than in the soft sciences.

- Hypothesis 4a: Average number of authors per paper. Collaboration is more common in the hard sciences than in the soft sciences.
- Hypothesis 4b: Average number of institutions per paper. Collaboration between authors from different institutions is more common in the hard sciences than in the soft sciences.

- Hypothesis 4c: Average number of nations per paper. Collaboration between authors from different nations is more common in the hard sciences than in the soft sciences.

DATA

We first conducted two separate rounds of the same survey. The survey text was the same for each round. The first round was dispensed via Amazon MTurk and included 1,201 respondents, and there were no restrictions placed on educational attainment for this sample. The second round was fielded through a knowledge panel that was limited to respondents with master's degrees to roughly approximate the education levels of Gordon and Lodahl's 1972 survey of the professoriate and included 516 respondents. The survey elicited a total of 1,716 responses.

After assessing public sentiment on scientific hardness, we then tabulate output for our metrics across each of the nine fields in our survey. To test our hypotheses about differences in the practice of science by hardness, we draw upon data in the same nine fields from the Microsoft Academic Graph (MAG), a leading large-scale bibliometric database (Sinha et al. 2015). For this part of the research, we analyze more than 50 million of the 180 million papers in the MAG database (see Table S1 in the Supplementary Information for details). We then use cutting-edge computational tools for the analysis of big data to tabulate output for our 14 metrics from the papers in the database.

While our metrics may be conceptually simple, they are computationally complex, and they require computationally expensive data transformations. Because we analyze data at a scale rarely seen in the social sciences, we employ computational tools that are also seldomly used in the social sciences. The simplicity of our metrics is theoretically motivated. We aim to show that the variation in the practice of science across fields is visible from a bird's eye view.

RESULTS

We present the results from our two survey rounds followed by the output from our 14 metrics. Our survey respondents rank the sciences into a very clear and familiar hierarchy with sociology as the softest science and physics as the hardest. Our metrics then rank the sciences into an almost identical hierarchy. The remarkable consonance between, in Salganik's framework (Salganik 2018), the ranking that emerges from the "custommade" survey and the ranking that emerges naturally from the "readymade" big data suggests, firstly, that the IASH is erroneous because the hard–soft distinction is real and, secondly, that public understanding of the hard–soft distinction more or less accurately reflects the empirical reality of this distinction.

Survey

Almost 2,000 individuals responded to our survey across the two survey rounds. We first asked respondents to rank nine sciences from soft to hard: sociology, political science, economics, psychology, machine learning, computer science, biology, chemistry, and physics.[15] We solicited responses using ordinal rankings and then with conjoint comparisons. The results from the survey were the same regardless of how we asked the question or to whom we asked it; both the more educated and less educated samples sorted the sciences identically into a familiar hierarchy with social sciences (sociology, political science, economics, psychology) at one end, computational sciences (machine learning, computer science) in the middle, and the natural

[15] We added computer science and machine learning to Gordon and Lodahl's original seven fields because the fields are new and, given that most of the modern world is directly or indirectly reliant on algorithms powered by advances in deep learning, extremely consequential.

sciences (biology, chemistry, physics) at the other. Because conjoint questions are so unambiguous, they generally yield more accurate responses than other survey methods (Horiuchi et al. 2021). We use the results from the conjoint portion of the survey as the independent variable when presenting the results for each of our metrics. Figure 1 presents the ranking of the sciences from hard to soft as revealed in conjoint comparisons.

Figure 1 About Here

Publication

Figure 2 About Here

Figure 3 About Here

Figure 4 About Here

We begin with publication, the primary universal of scientific practice. We hypothesized earlier that scientific hardness manifests in the publication process as speed to publication and brevity, in part because authors in high-consensus sciences do not need to reiterate shared precepts or background information (Hypothesis 1). Our results support this hypothesis. First, we show that the primary publication format varies by hardness. Figure 2 shows the proportion of books by field (as a comparison, Figure S1 shows books and book chapters), and Figure 3 shows the proportion of conference papers by field (Hypotheses 1a and 1b). We see a clear and unmistakable trend that will repeat itself for the remainder of our metrics: the social sciences, typically regarded as soft, present at one end, and the natural sciences, which are generally seen as hard, are at the other. The softest and least codified sciences are far more book-heavy than the hard sciences, confirming Hypothesis 1a. While political science may be an outlier with 11

percent of its publications in the form of books, roughly 1.5 percent of publications in sociology, 1.6 percent of publications in psychology, and 1.1 percent of publications in economics are books. Only roughly one third of 1 percent of publications in physics or chemistry are books. Books, then, are three to five times more common in the soft sciences as compared to the hard sciences.

Conference papers, by contrast, are either insignificant or crucial to certain sciences. For most of the sciences in our sample, including all of the social sciences as well as biology and chemistry, less than 1 percent of total publications are published as conference proceedings. In contrast, 53 percent of publications in machine learning and 50.5 percent of publications in computer science are conference proceedings. The only science for which conference proceedings are not either less than 1 percent or a majority of all publications is physics, for which conference proceedings are 6.6 percent of all publications. The computational sciences, as Wang and Barabási (2021) predicted, are unique in being conference disciplines, though conferences are of some importance to physicists. Advances in the hardest sciences are published either as a mixture of mostly as journal articles (see Figure S2 for a plot of journal article proportions by field). These results fail to confirm Hypothesis 1b but do suggest that scientists in the harder sciences generally publish work in shorter formats.

These profound between-format differences translate to within-format differences. Figure 4 shows the average journal article page length for our nine fields. We see that articles in the natural sciences are roughly seven pages long on average, which comports with what Zuckerman and Merton observed: articles in the social sciences are significantly longer (Zuckerman and Merton 1971, 79). Economics, with an average of 14 pages per article, is something of an outlier, but articles in sociology and political are on average 12 and 11 pages long, respectively. Journal

articles in the softest sciences, then, are roughly 1.5 times to twice as long as articles in the hardest sciences, confirming Hypothesis 1c.

Citation

Figure 5 About Here

Figure 6 About Here

Figure 7 About Here

The patterns that emerged from our metrics of publication practices across the sciences emerge again in citation behavior across the sciences. We hypothesize that scientific hardness manifests in citation behavior as a reduction of the halo effect and as an increased pace of knowledge accumulation (Hypothesis 2). Contrary to the IASH, we see very real variation in citation behavior along the hard–soft distinction that supports this hypothesis: for each of our metrics of citation behavior, the hard sciences move quickly, and the soft sciences move slowly.

Looking at average age of citation (Figure 5), while the computational sciences are again outliers, the average age of a citation in a sociology publication is 12.5 years, 25 percent older than the average age of a citation in a publication in the natural sciences, which is roughly 10 years. Canonical work in the softer sciences becomes obsolete at a slower rate than in the hard sciences, and knowledge in the soft sciences accumulates less quickly than in the hard sciences, confirming Hypothesis 2a. This follows from theories of consensus. In fields where most scholars agree on core principles, it is not necessary to cite old papers repeatedly. While Cole

(1983) suggests that differences in citation age may be due to the humanities citing very old works as primary sources, none of the fields we survey draw extensively upon original historical sources.[16] The stark differences we see in average citation age across the sciences, then, are primarily attributable to hardness and consensus.

For the Gini coefficient of inequality for the distribution of citations, we see that the soft sciences are extremely unequal where the hard sciences are slightly more egalitarian (Figure 6), confirming Hypothesis 2b. Citations in sociology are distributed with a Gini coefficient of 0.86, and citations in chemistry, the most equal science in our sample, are distributed with a Gini coefficient of 0.66. While the scientists of science have shown that science is far from equal (Nielsen and Andersen 2021), their focus on science in the aggregate has obscured tremendous inter-field heterogeneity.

Like citation inequality, the age structure of the sciences follows a predictable hard–soft gradient (Figure 7), confirming Hypothesis 2c. The most cited paper of a physicist's career is published, on average, 10.5 years into his or her career. We define the start of the career as the year in which an author first publishes a publication. The corresponding figure for a sociologist is almost 13 years into the career. Additional controls would almost certainly yield larger differences. The maxim that science is a young scholar's game may be true with a slight caveat: hard science is a young scholar's game, but soft science is an older scholar's game. Rising stars may be able to rise more quickly in fields with more consensus because the criteria for success

[16] Historical sociology may be an outlier in this regard in that it does draw upon primary sources, but historical sociology is a relatively small subfield in sociology, and, even if we exclude sociology from our results, the trend still holds.

are more clear and more objective, but a full discussion of the relationship between prestige and hardness is beyond the scope of this paper.

Communication

Figure 8 About Here

Figure 9 About Here

Figure 10 About Here

We interrogate variation in language usage across the scientific fields in our sample to examine how hardness influences scientific practice. The internal consensus regarding conceptual frameworks in the hard sciences fosters the development of elaborate systems of specialized language. We hypothesize that hardness and consensus lead to increased rates of jargon usage (Hypothesis 3), and we again uncover evidence that supports our hypothesis and contradicts the IASH.

Figure 8 presents the average word frequency in abstracts across our nine fields. To briefly explain word frequency, the word "the" has a word frequency of 0.054, which indicates that it comprises 5.4 percent of all words used in English-language texts. Returning to the two example titles from *Science* that we used earlier, the average word frequency for "Zygotic genome activation by the totipotency pioneer factor" is 0.000011 or 1.1×10^{-5}, and the average word frequency for "A causal test of the strength of weak ties" is 0.000058 or 5.8×10^{-5}. In other words, the average word frequency in the latter is five times greater than that in the former. A fuller illustration of this may be found in Table S2 in the Supplementary Information. Plotting the average word frequency in abstracts across the nine fields in our sample reveals a clear cline

of declining word frequency between the social sciences, the computational sciences, and the natural sciences. The average word frequency in a political science abstract is twice that of a chemistry abstract. This is broadly reflective of the unique history of specialized vocabulary in the natural sciences, but it is significant that this trend also holds for the computational sciences, which are medium-consensus and do not study natural processes. Put another way, the fact that the average word frequency decreases from the social to the computational sciences, neither of which employ the Greco-Latin neologisms that are so common in the biological sciences, means that we cannot attribute the unique rarity of vocabulary in the natural sciences to path dependence.[17] Jargon intensity, then, is more a product of hardness and consensus than of history.

As a corollary to the general relationship between scientific hardness and jargon usage, we more specifically hypothesized that professional vocabulary takes the form of single-word neologisms in the hard sciences and multi-word phrases built from common words in the social sciences (Hypotheses 3b and 3c). If this is the case, we would expect to see minor differences in average sentence length and the percentage of non-repeated words in abstracts between fields. This is exactly the case. Figure 9 shows that the average sentence in an abstract in the social sciences is about two words longer than that in the natural sciences. The small variation in average sentence length between fields is not reflective of major stylistic differences between fields[18] but is instead most likely driven by the prevalence of phrasal vocabulary in the soft

[17] For another example of this, we also see that the average word frequency in a physics abstract is very similar to that in a biology abstract. Physics is a natural science, but it does not inherit the Greco-Latin neologisms from the history of medicine that are so common in biology. This then implies that the relative rarity of words in physics is more attributable to consensus and not path dependence.

[18] For an example of the magnitude of changes in sentence length that would be expected from stylistic differences, see Gross, Harmon, and Reidy (2002). The average sentence length in an English-language scientific journal in the 19th century decreased from 41.46 words in 1800 to 31.69 words in 1900 (Gross, Harmon, and Reidy 2002, 124, Table 6.3).

sciences. Similarly, as shown in Figure 10, the percentage of non-repeated words varies only slightly between the sciences for the same reason. Therefore, Hypotheses 3b and 3c are confirmed.

Collaboration

Figure 11 About Here

Figure 12 About Here

Figure 13 About Here

Consensus directly facilitates and eases collaboration. As we proposed in Hypothesis 4, the relationship between scientific hardness and collaboration is clear and unmistakable. As Figures 11, 12, and 13 show, consensus is correlated with a holistic increase in collaboration across personal, institutional, and national boundaries. On average, papers in the natural sciences have between three and four times as many authors as do those in the social sciences, confirming Hypothesis 4a. Inter-institutional and international collaboration are also more common in the hard sciences than in the soft sciences, confirming Hypotheses 4b and 4c. There are a few exceptions to these trends. Psychology manifests more as a social science in terms of international collaboration but as more of a natural science in terms of institutional collaboration. This reflects psychology's liminal position between the biological and social sciences.

Additional Metrics

Figure 14 About Here

Figure 15 About Here

We finish our presentation of results with a brief sketch of two additional metrics: the proportion of papers that are published in English by field and the usage of first-person pronouns in paper abstracts.

The first of these metrics, the proportion of papers that are published in English, may reveal the generalizability of a field's subject matter. This metric is unique among our others in that it does not address the practice of science but instead reflects differences in field-specific subject matter and research foci. The language of science is now English, but that has not always been the case (Woolston and Osório 2019). In the 19th century, science was conducted in a wide range of predominantly European languages, and many scientists were forced to read publications in multiple languages to follow the bleeding edge of science (Gordin 2015). In the 21st century, however, the most famous and widely read generalist journals, such as *Science, Cell*, and *Nature,* are published exclusively in English. Though the unique dominance of English as the international language of science is historically unprecedented, some sciences are more English-dominant than others. Figure 14 presents the proportion of papers that are published in English by field. We see that the hardest sciences are almost entirely conducted in English whereas some soft sciences retain some elements of science's multilingual past. Sociology is relatively less English-dominant, with only two-thirds of publications in the field published in English. Chemistry is an exception in the natural sciences, at roughly 85 percent of publications in English, but most other natural sciences are almost entirely conducted in English in our sample. While not all local journals are indexed by the MAG (or other leading bibliometric databases such as the Web of Science, see Alperin 2014), this alone cannot explain the remarkable relationship between hardness and English.

The generalizability of a science's subject matter, however, might explain this relationship. Social scientists study specific places, peoples, or cultures, and knowledge of one society may not generalize to another. As evidence of this, we see that a large segment of sociological research remains resolutely Francophonic (Bénatouïl 1999). The laws of physics, on the other hand, are unchanged by geography or culture. There may be many other factors that drive English dominance in some fields, and we suggest that generalizability may be one of them.

The second of these metrics, the proportion of abstracts that utilize any first-person pronoun, may illuminate how scientists respond to the widespread public understanding of the hard–soft hierarchy. Figure 15 graphs the proportion of abstracts that utilize any first-person pronoun, revealing a surprising pattern: abstracts in the purportedly objective hard sciences are roughly twice as likely to use a first-person pronoun than abstracts in the soft sciences. This conflicts with a famous early result in the sociology of science. In the first academic paper to use the terms "hard science" and "soft science," Storer suggested that the hard sciences are less personal than the soft sciences. To prove this, he showed how citations in the hard sciences used only a first initial whereas citations in the soft sciences used the full name (Storer 1967; Shapin 2022).

This outcome may reflect that scientists in the soft sciences feel a need to perform their status as scientists by preferentially using objective language. Historians of science who specialize in the rhetoric of science have suggested that "the general decline of personal pronouns and corresponding rise of passive voice" in scientific articles from the 17th century to the 19th centuries occurred as a result of scientists presenting "science as an objective enterprise" (Gross, Harmon, and Reidy 2002, 231). Though we do so tentatively, we suggest that as men

respond to feeling emasculated by "doing gender" and performing masculinity (West and Zimmerman 1987), scientists who feel they are at the bottom of a scientific hierarchy may "do science" by using objective language, intentionally or otherwise.

These two metrics offer tentative directions for future work on the hard–soft distinction. We have focused our main analysis on tracing the second-order consequences of scientific hardness and consensus in the practice of science. The first of these additional metrics moves upstream from consequences to causes. The essential nature of the things that a science studies can have wide-ranging consequences for how that science is later practiced. The second additional metric moves from second-order consequences to tertiary consequences. Social scientists may react to public understanding of the hard–soft distinction with behavior that reinforces that distinction.

CONCLUSION

For almost as long as science has existed, people have ranked and compared the sciences to one another (Comte 1896). The most commonly used framework for doing so is the hard–soft distinction: the natural sciences are typically seen as hard, and the social sciences are said to be soft. From the 1960s to the late 1980s, sociologists of science debated whether or not the hard–soft distinction had consequences for the social organization of science. The two frameworks used most often to guide empirical work on the subject were Merton and Zuckerman's theory of codification and Kuhn's theory of the scientific paradigm. This debate was left unresolved. More recent work in science studies, conducted mostly by ethnographers of science and scientists of science, has so far overlooked this debate and is instead conducted under the IASH. The results of this study, however, strongly indicate that this assumption is erroneous.

We return to the unresolved debate regarding the hard–soft distinction with the benefit of theoretical and methodological advances in science studies. We borrow the ethnography of science's focus on the practice of science and the science of science's use of big data to explore how the practice of science varies across nine fields. We argue, following Merton and Zuckerman, that hardness manifests in the practice of science as consensus. We create metrics that capture the consequences of consensus in four categories: consensus manifests in the publication process as greater speed and brevity, in citation patterns as greater speed of accumulation/obsolescence and equality of distribution, in scientific communication as more jargon usage, and in collaboration patterns as increased levels of cooperation across all boundaries. These metrics cover almost all elements of the practice of science to some extent. Our metrics sort the sciences into a hierarchy of science hard and soft with physics at one end and sociology at the other.

We show, in short, that Zuckerman and Merton were right and that the hard–soft distinction is real: the sciences differ in their levels of consensus. We also show that the hierarchy of the sciences that emerges from our metrics matches the hierarchy of the sciences that emerges from a survey of almost 2,000 respondents. This means that public understanding of the hard–soft distinction is more or less accurate. We do not argue that hard science is necessarily better or more valuable than soft science, as we believe in the value of sociology as practicing sociologists ourselves. We simply show that the hard–soft distinction exists and that it has very real consequences for the practice of science.

The sciences are very different from one another, and our metrics offer a new theoretical lens that we hope will enrich future work in science studies. As a brief example, we have shown that collaboration rates vary greatly across the sciences. Future ethnographers may explore what

factors besides consensus allow for such collaboration. Does the frequency of collaboration perhaps encourage or discourage certain minds from entering certain fields? Indeed, it seems that there is a paradoxically inverse correlation between studying people and interacting with people; sociologists study people but do not frequently collaborate with one another where physicists do not study living organisms, let alone people, but almost always publish in large teams. Relatedly, while scientists of science have shown which types of collaboration are most beneficial across all of the sciences, they may additionally examine which specific subfields benefit most from these many types of collaboration. If, as Wu, Wang, and Evans (2017) argue, science is advanced by large teams but "disrupted" by small teams, does the innovation premium accorded to small teams vary by field?

This study is not without its limitations. While our metrics reliably and accurately sort the sciences into a hierarchy that is familiar to the general public, they do not do so perfectly. Much of the inconsistency between our metrics and the predictions from our hypotheses may be attributed primarily to field-level idiosyncrasies. Reviewing Figure 4, which presents journal article page length by field, we see that economics is an outlier in terms of page length; while there is a general correlation between increasing softness and page length, the average economics article is two pages longer on average than the average journal article in sociology, the softest science in our study. It is well known inside and outside of economics that economists publish lengthy journal articles and that the length of these articles has only increased in recent years (Ellison 2002; Card and DellaVigna 2013). We have also discussed how psychology presents more like a natural science on certain metrics and more like a social science on others.

More significantly, however, these imperfections may be attributed to the existence of other dimensions that may be used to characterize the sciences beyond the simple spectrum of

science hard and soft. Another framework, tellingly formulated in the era of research into the hard–soft binary, adopts a three-dimensional system for classifying sciences: whether the science is pure or applied, whether the science concerns living things or not, and finally, and perhaps tautologically, whether a science is hard or not (Biglan 1973a; 1973b). Where hardness implies a hierarchy of independent and even competing sciences, other frameworks used to compare sciences, famously including Comte's 19th-century ranking, instead suggest that the sciences are linked to one another and posit that advancement in fields higher on the chain of science are conditional on advances in more foundational fields (Comte 1896). E. O. Wilson provocatively suggests that science is like the labyrinth of Grecian legend, and an "Ariadne's thread" of "connecting causal explanations" should allow one to traverse from the social sciences to "back through the behavioral sciences to biology, chemistry, and finally physics" (Wilson 1999, 73). Regardless of whether every stage of this hierarchy feeds into the stage above it, Wilson's and Comte's work suggests that the sciences may be productively divided into applied fields and foundational fields. Two of the fields in our work, namely computer science and machine learning, may be best understood as applied in relation to pure mathematics but foundational in relation to engineering and the social science, and this may help explain why computer science and machine learning do not fit as neatly into the hard–soft binary, viewed through the lens of scientific practice, as the other fields in our sample.

Some have turned the hard–soft distinction on its head and suggested that the social sciences, which study subjects "orders of magnitude more complex and multifaceted" than those of the natural sciences, should instead be seen as the true bearers of "hard" science (Shermer 2007). Because they are so complex, the things that social scientists study, like social networks,

are often very difficult to quantify (Watts 2007; Lazer et al. 2009),[19] and quantifiability (ease of quantification) could be another dimension in an expanded conception of scientific hardness. This relates also to the scale of the objects that a field studies and, as we discussed in relation to Figure 14, to the generalizability of a field's findings. Investigating these other dimensions of science, however, necessitates looking past the practice of science and instead examining either the objects of study of various scientific fields or the cognitive fabric of the knowledge that scientists have created to study these objects. They are beyond the scope of this paper but certainly not beyond the scope of future work. While much pioneering work has been done on how science behaves in the aggregate, science, as we have shown, is far from homogenous. It is now time for sociologists to lead the way in investigating heterogeneity between the sciences.

[19] Social phenomena are also very multi-dimensional. The Fragile Families Fragile Families and Child Wellbeing Study (now the Future of Families and Child Wellbeing Study) contains more than 1,600 variables (Salganik et al. 2020).

FIGURES

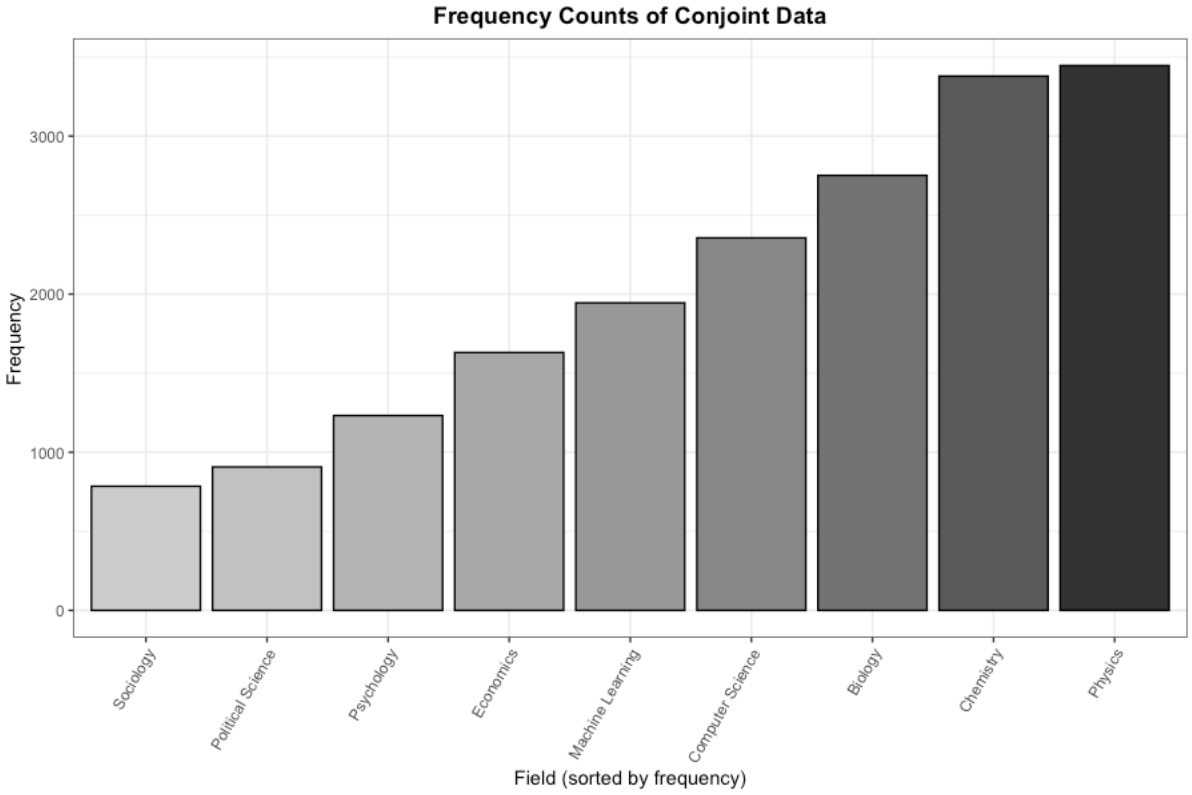


Figure 1: Survey Results
Survey results show that the public sorts the sciences into a very clear hierarchy. Using the total counts of every time a science was ranked as harder than another in conjoint comparisons, we construct a hierarchy of the sciences from soft to hard. Physics is the hardest science, and sociology is the softest.

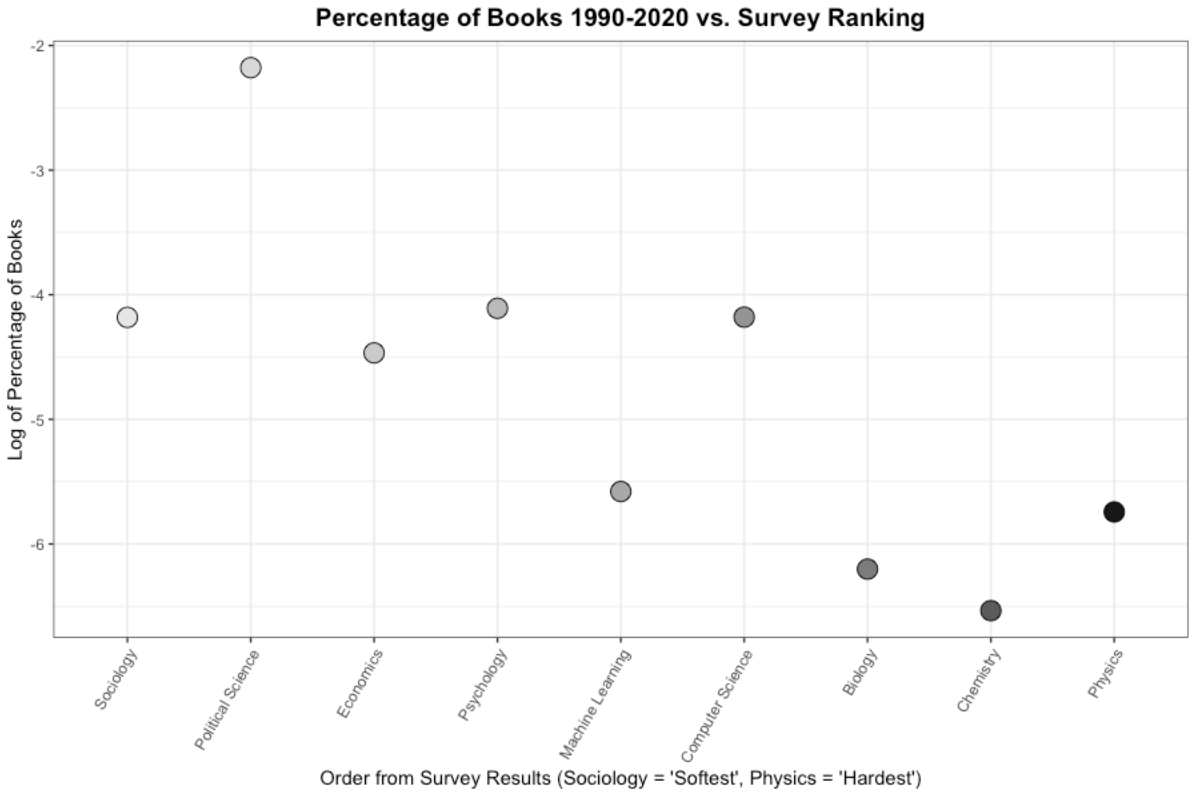


Figure 2: Log percentage of total publications in each field that are books
Graphing the proportion of publications that are books across the nine fields reveals a strong hierarchy. The logged percentage of books is plotted on the Y axis. Results show that hard sciences, on average, feature more books than soft sciences. The trend holds true even though political science is an outlier.

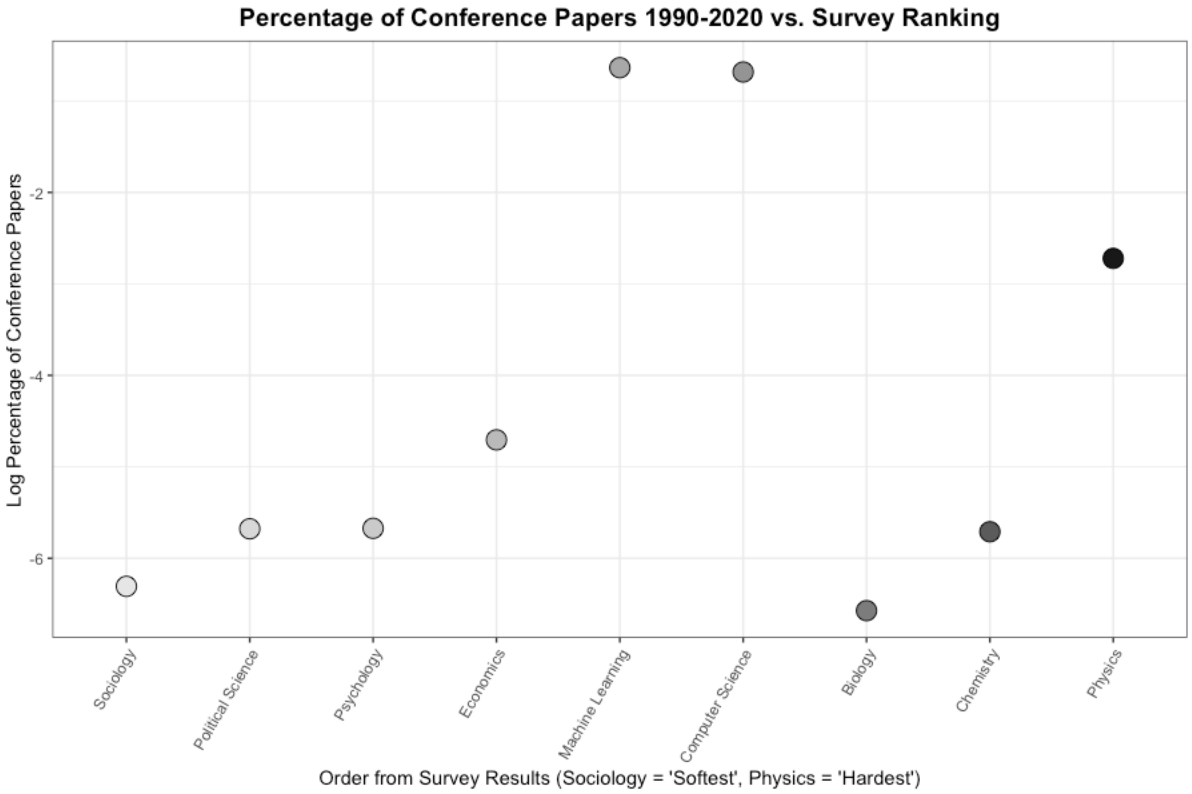


Figure 3: Log percentage of total publications in each field that are conference papers
Graphing the proportion of publications that are as conference papers across the nine fields reveals a similar hierarchy to that in Figure 2. Results show that hard sciences, on average, feature more conference papers than soft sciences. The computational sciences are unique among all other sciences in that they are conference paper-dominant.

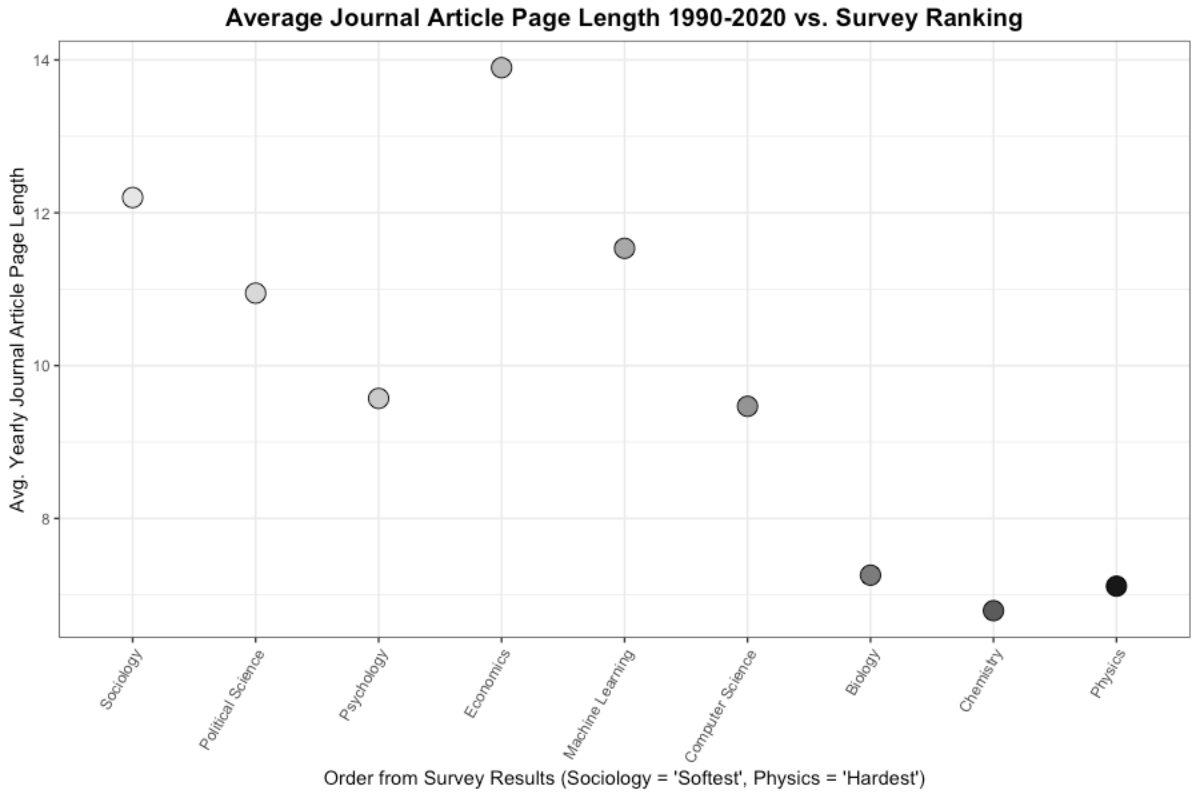


Figure 4: Journal article page length by field

Journal article page length results show that articles in the soft sciences are longer than articles in the hard sciences. Sociology articles are just above 12 pages on average where articles in physics are almost half as long at only seven pages on average. Economics is an outlier, though economists and non-economists alike have commented on how the field-specific culture of economics tends to favor longer journal articles (Ellison 2002; Card and DellaVigna 2013).

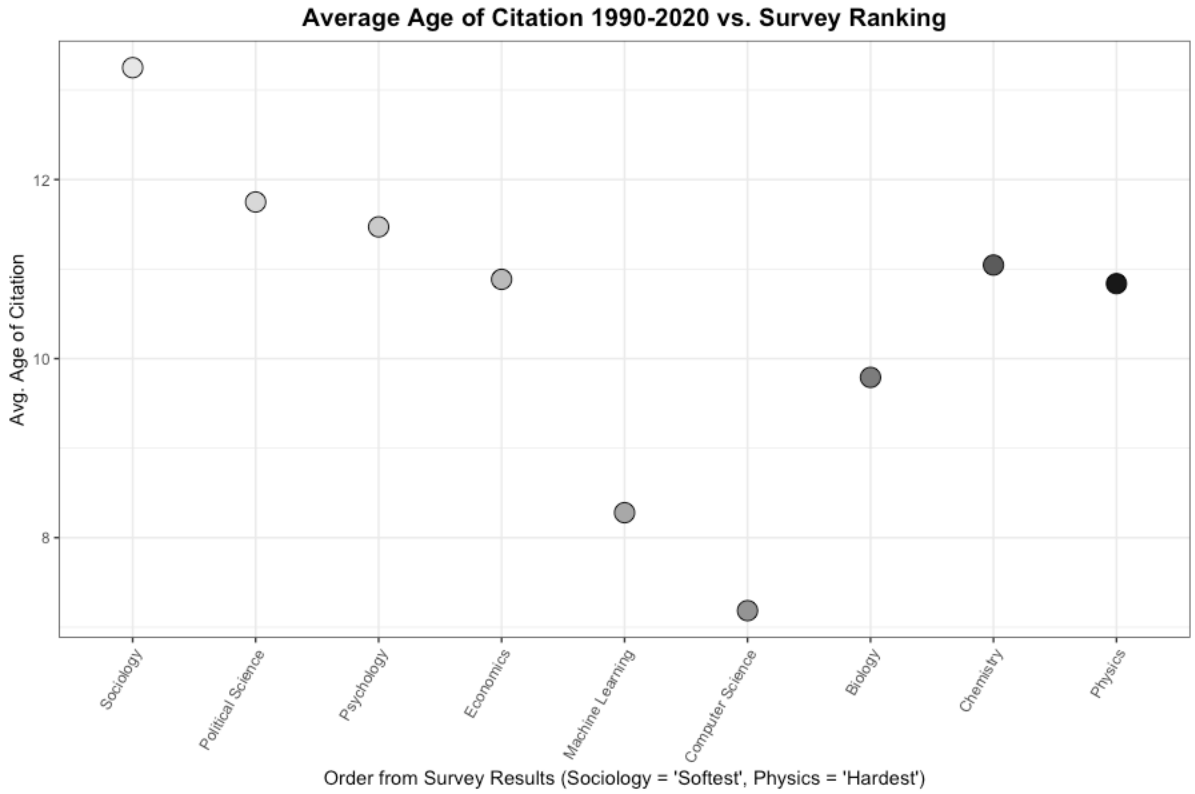


Figure 5: Average age of citation by field

The average age of citation of a given paper is calculated by subtracting the average of the years of publication of a paper's references from a paper's year of publication. The average work cited in a sociology paper is, on average, 12.5 years older than the paper itself. In general, papers in the soft sciences cite older work than papers in the hard sciences. This suggests that knowledge accumulates more slowly in the soft sciences.

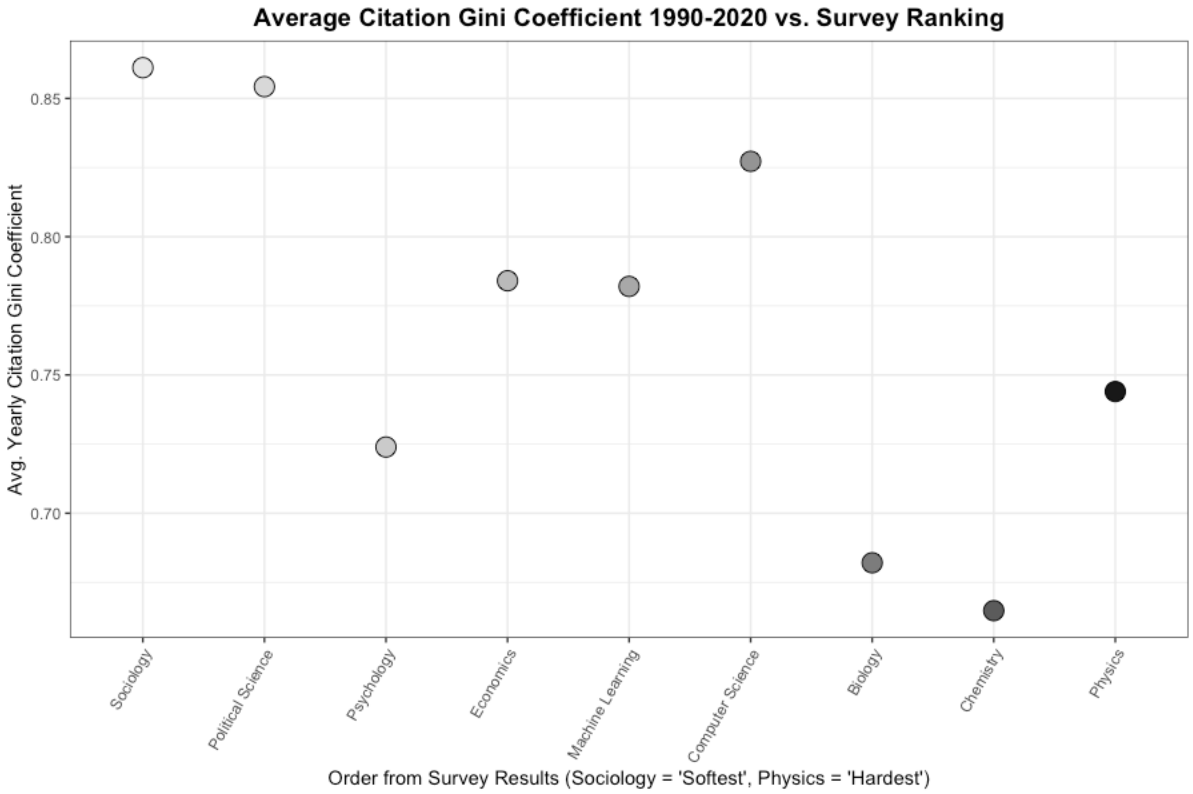


Figure 6: Citation Gini by field
We plot the average yearly Gini coefficient of citations per paper for each field in our sample. We see that the distribution of citations is less equitable in the soft sciences than in the hard sciences. The soft sciences are more dominated by canonical papers that attract a larger number of citations than the hard sciences.

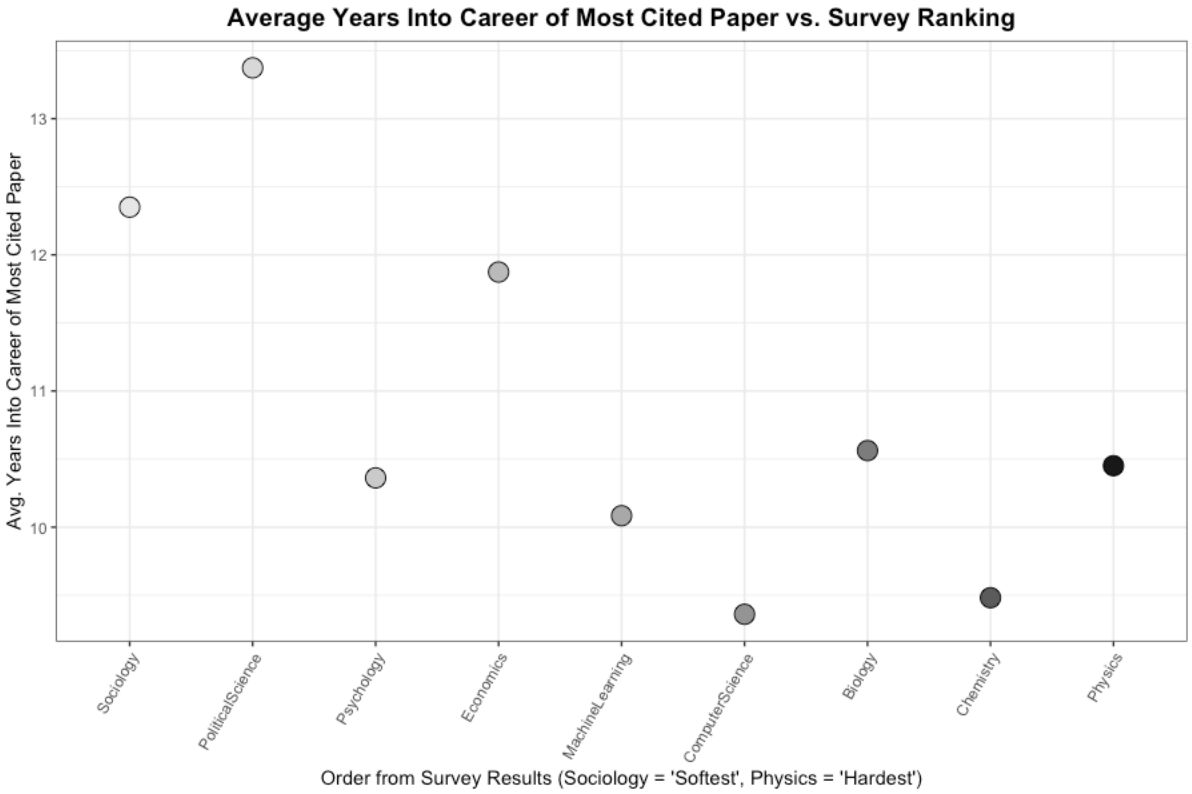


Figure 7: Years into career of most cited paper by field

As further evidence that the hard sciences move faster than the soft sciences, we see that scientists in the hard sciences publish their most-cited articles earlier in their career than scientists in soft sciences. A physicist's most-cited article is published at roughly ten and a half years into his or her career where a political scientist publishes his or her most-cited work three years later.

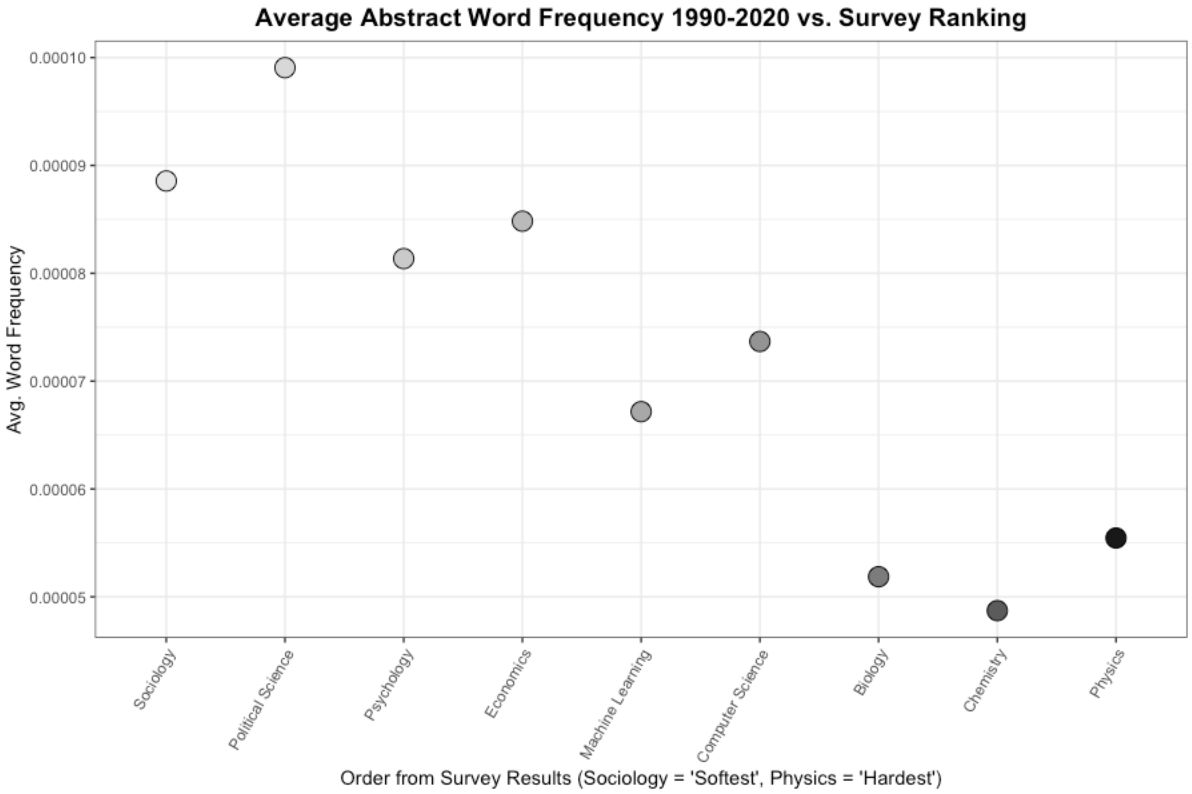


Figure 8: Abstract word frequency by field
Abstracts in the softer sciences feature more common words than abstracts in the harder sciences. Word frequency in this graph is defined as the percentage of all English words that are a given word. For example, "test" is 0.000155% of all English words and appears on this chart as 0.000155 (see Table S2 in the Supplementary Information for details). Because word frequency and word difficulty are highly correlated, this implies that the softer sciences use simpler words.

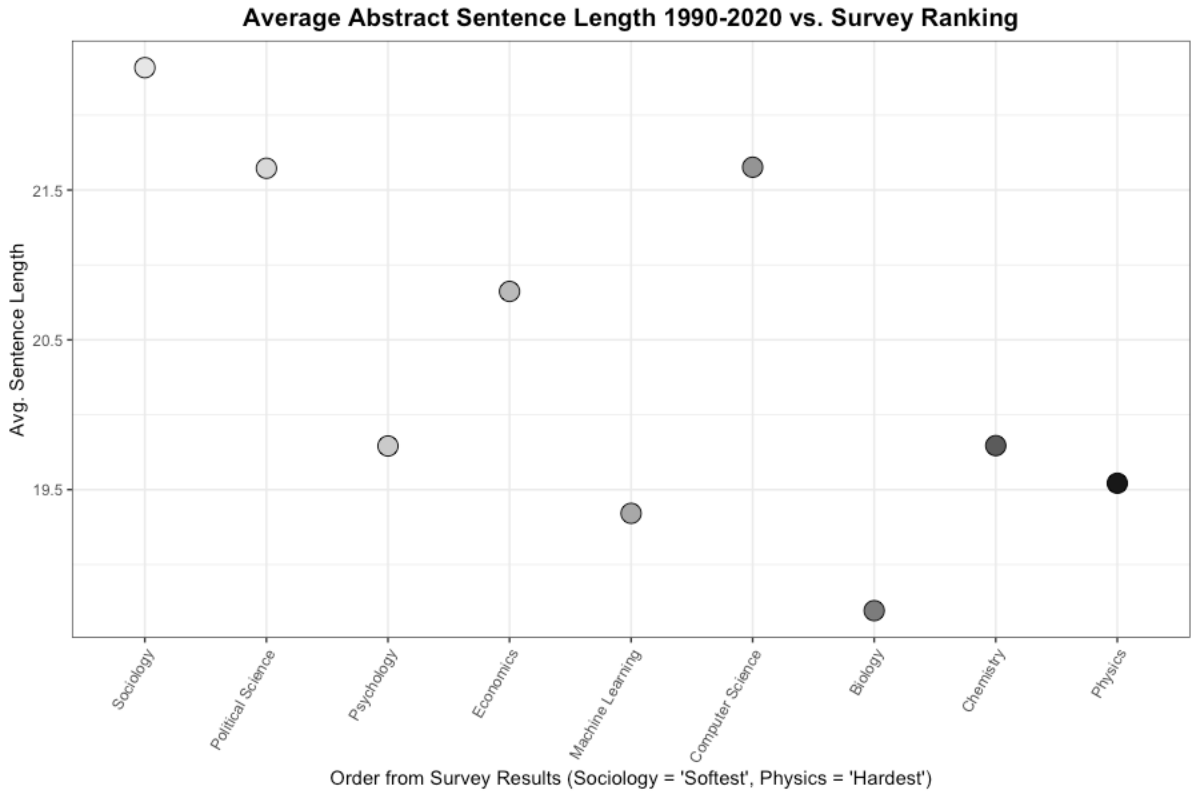


Figure 9: Average abstract sentence length by field
Abstracts in the softer sciences are composed of longer sentences than abstracts in the harder sciences. The relatively small differences in average sentence length between fields suggests that this is driven by not by variations in style but by field-level preferences for single-word neologisms or multi-word phrases.

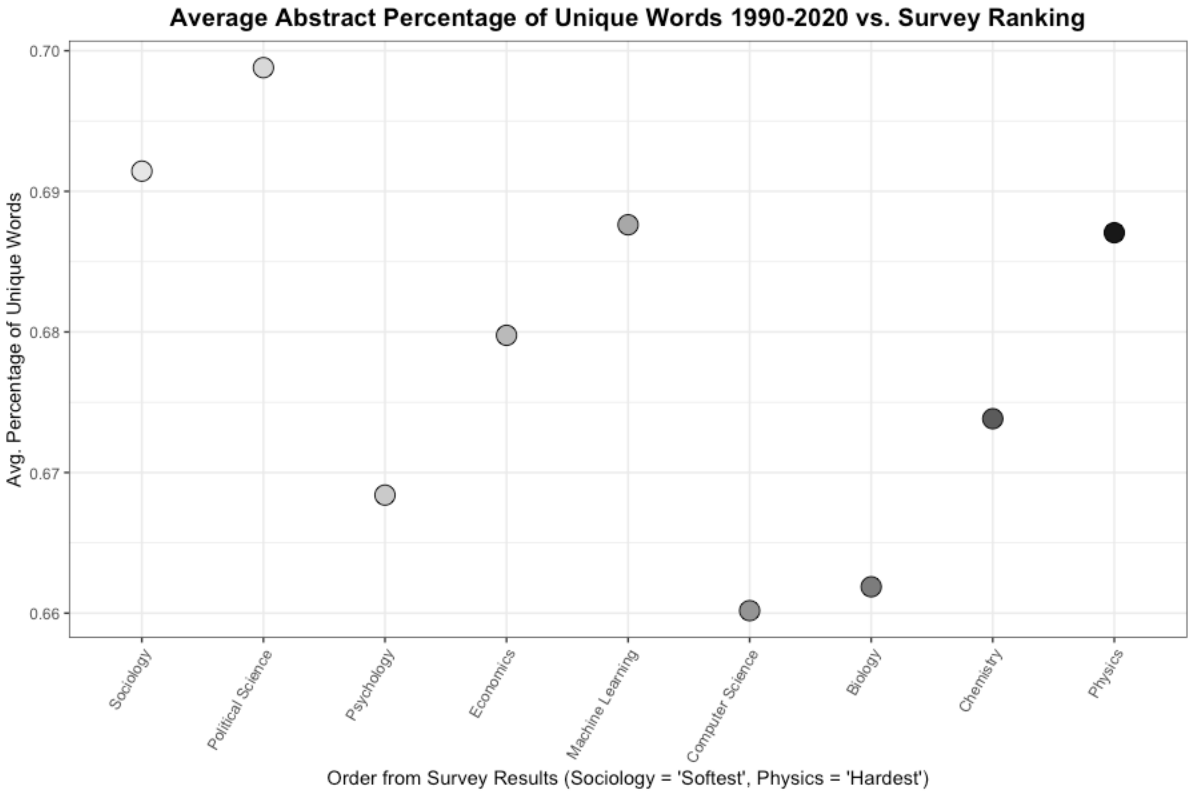


Figure 10: Percentage of non-repeated words in abstracts by field
Abstracts in soft sciences feature more unique words than abstracts in hard sciences. The difference in this metric between fields, however, is small. It is more likely to be driven by differences in the usage of single-word Greek-influenced neologisms versus multi-word phrases than stylistic differences.

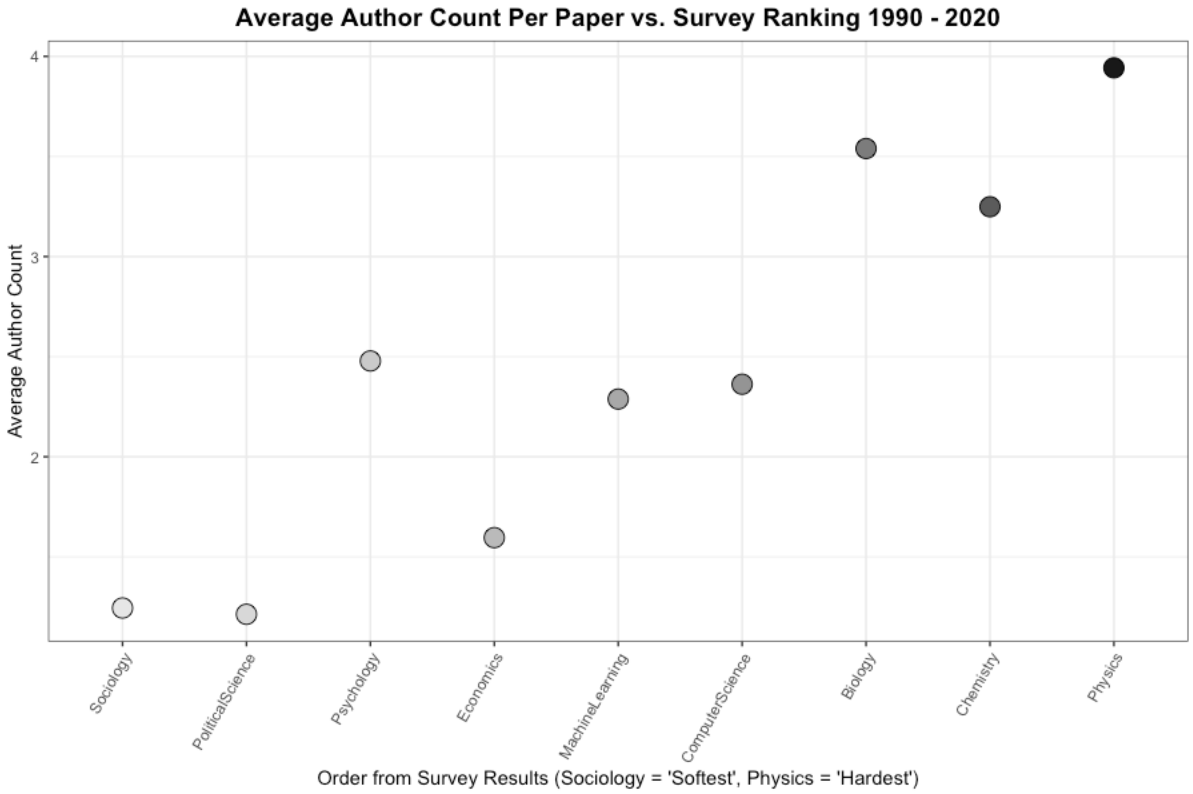


Figure 11: Average author count per paper by field
Harder sciences, which are more codified, foster more collaboration. Whereas papers in the least codified of the social sciences are, on average, penned by only one author, papers in the natural sciences feature as many as four authors per paper on average.

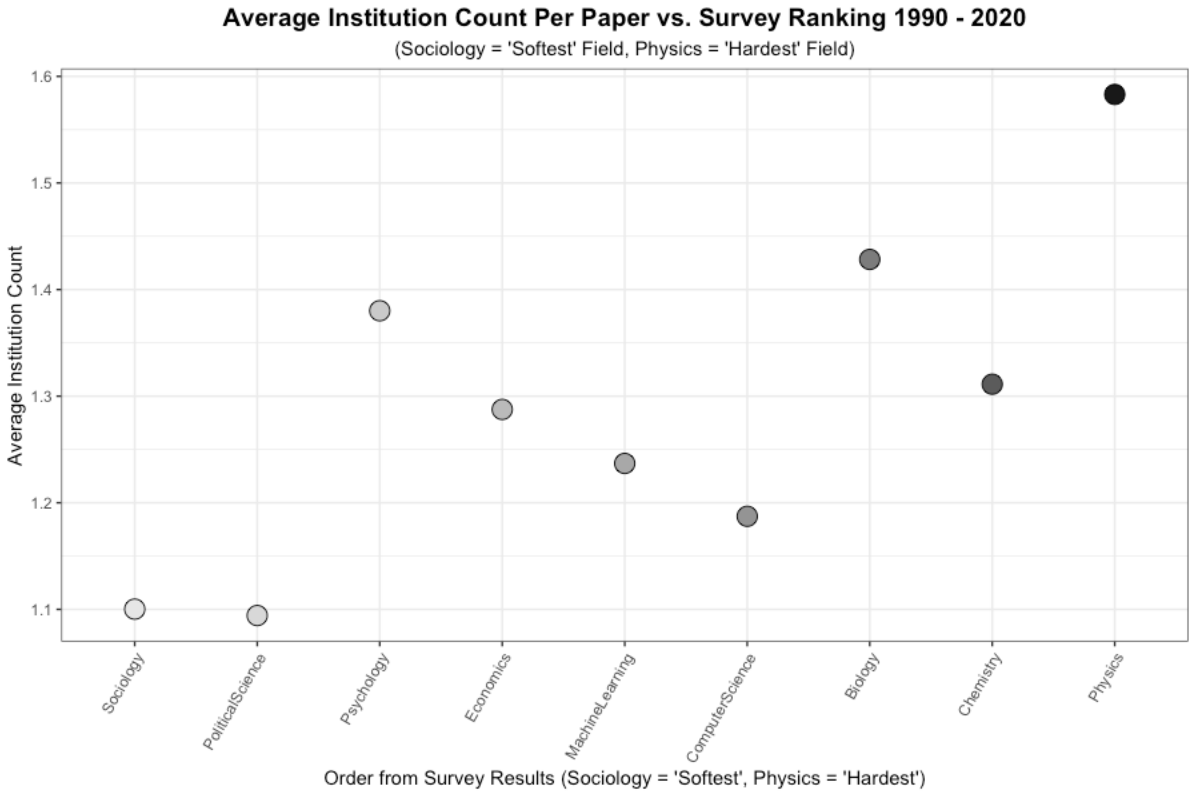


Figure 12: Average institution count per paper by field

Codification allows for collaboration across institutional as well as personal boundaries. Papers in the hardest, which is to the say the most codified, sciences are more likely to feature scientists from multiple institutions.

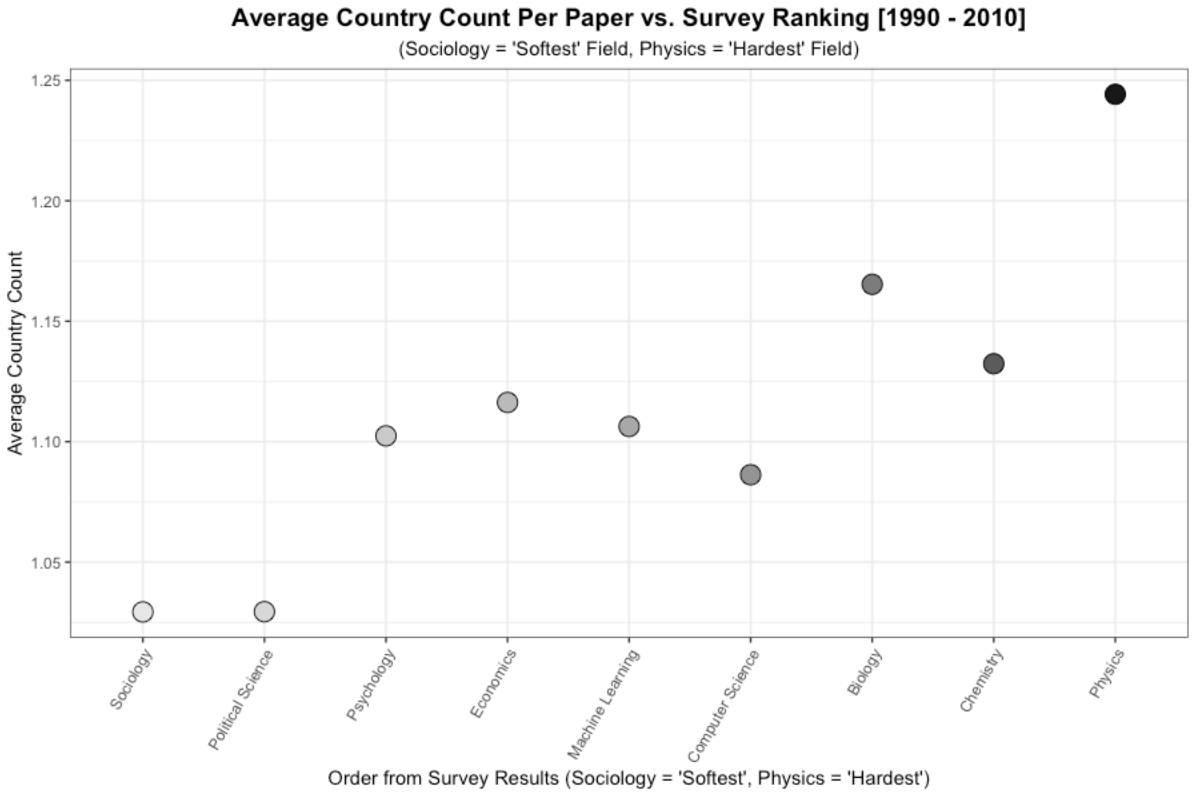


Figure 13: Average country count per paper by field

Finally, in addition to personal and institutional boundaries, codification allows for collaboration across national borders. The social sciences are extremely local and rarely involve international collaboration. The natural sciences, by contrast, foster international collaboration at significantly higher rates than the soft sciences.

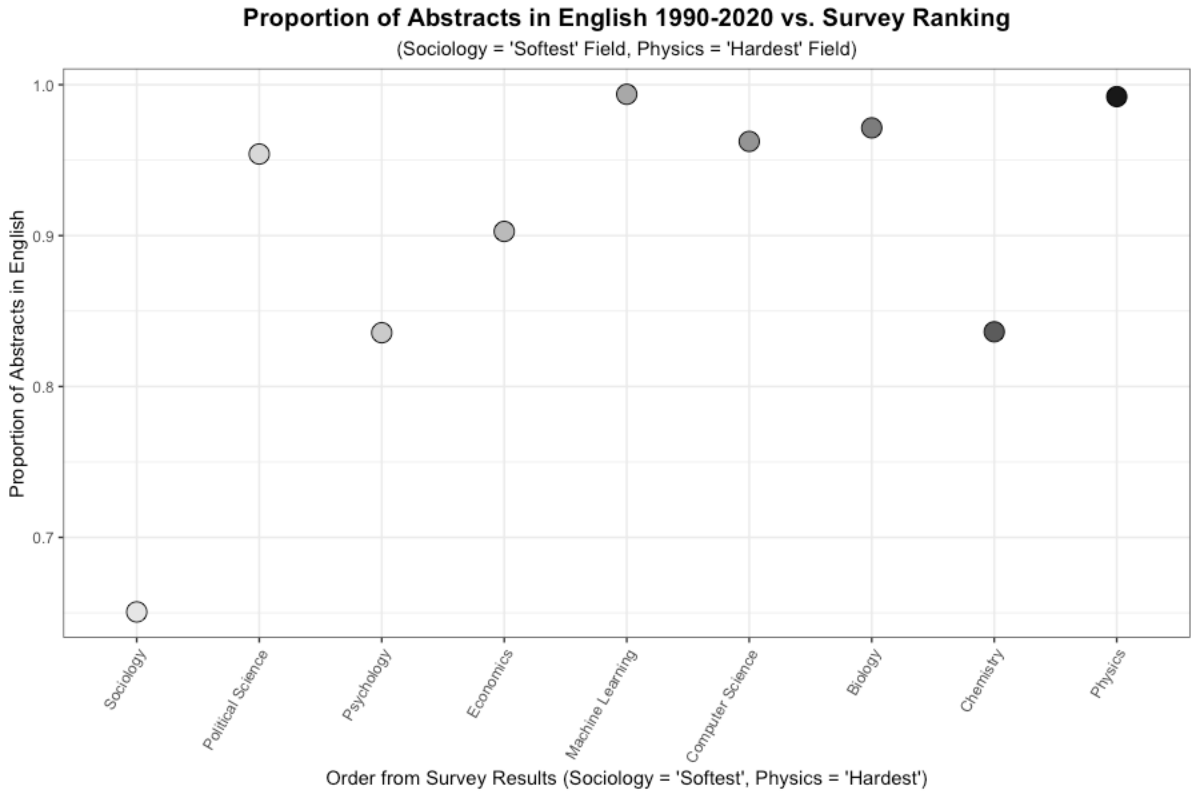


Figure 14: Proportion of abstracts in English by field

Generally speaking, the harder sciences are more English-dominant. Roughly 65% of sociology abstracts are in English where almost 100% of papers in the natural sciences feature English-language abstracts. Chemistry is an outlier in this regard, for a large portion of chemistry papers are penned in languages besides English. Softer sciences tend to have language- and region-specific communities that are often quite different from one another. See French sociology for an example. This may imply that the subject matter of the hard sciences is more generalizable than that of the soft sciences.

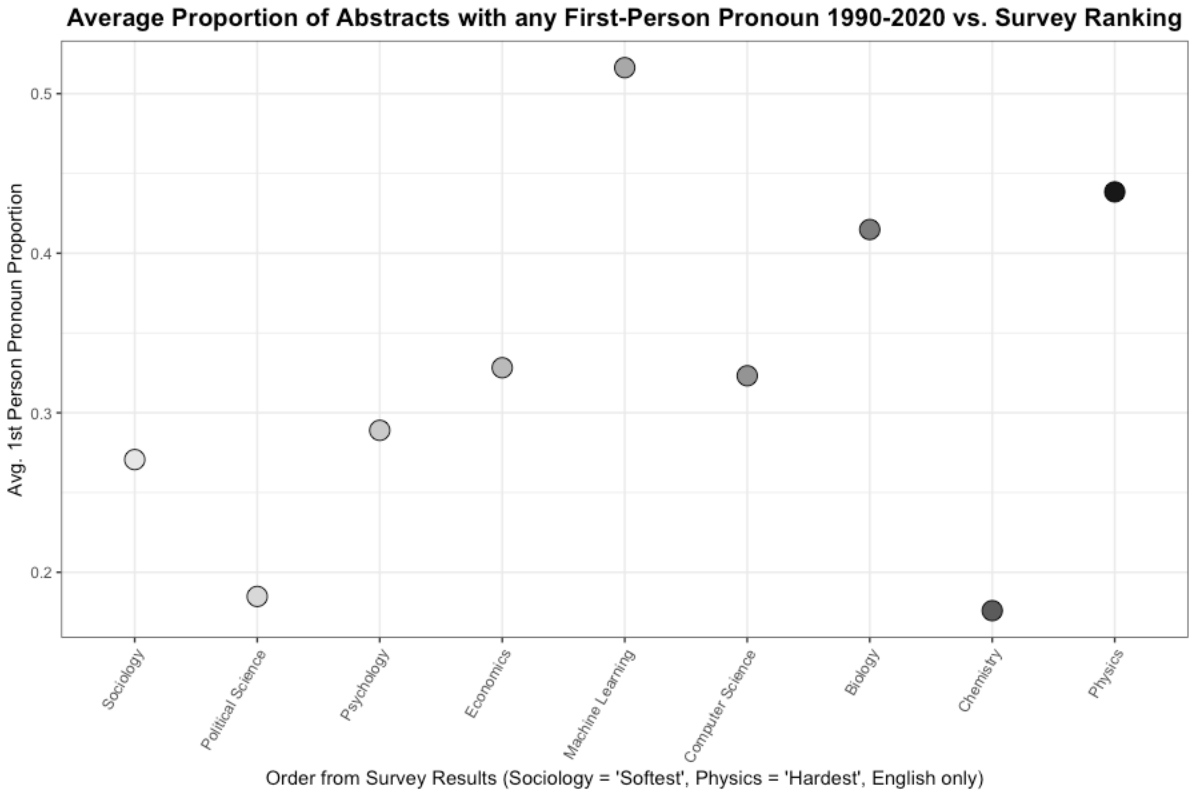


Figure 15: Abstract pronoun proportion by field

Abstracts in the softer sciences are less likely to feature first-person pronouns than abstracts in the harder sciences. Where less than 20% of papers in political science use any first-person pronoun, almost 45% of papers in physics use a first-person pronoun. This suggests that softer sciences may use impersonal language in an attempt to appear impersonal and more scientific.

Supplementary Information
Tables

	Unrestricted Sample	1990-2020 Sample	Total Abstracts	English Abstracts	English Abstracts 1990-2020
Sociology	3,033,107	2,307,298	2,135,519	1,395,886	1,266,718
Political Science	1,145,615	935,409	1,664,045	1,590,405	1,509,950
Economics	2,758,209	2,209,110	2,523,043	2,291,659	2,081,480
Psychology	4,157,400	3,133,042	3,730,811	3,165,478	2,780,965
Machine Learning	843,814	822,824	840,599	835,197	816,834
Computer Science	10,547,583	9,530,106	7,159,192	6,891,467	6,574,357
Biology	13,596,187	10,112,717	10,131,416	9,793,122	8,171,636
Chemistry	13,933,103	9,439,223	11,112,031	9,236,933	7,313,968
Physics	6,814,625	5,043,371	4,924,658	4,857,208	3,924,764
TOTAL	56,829,643	43,533,100	44,221,314	40,057,355	34,440,672

Table S1: Number of papers per field for the Microsoft Academic Graph data

The above table presents a list of the number of papers in the Microsoft Academic Graph database for each of our nine fields. Beginning with an unrestricted sample covering all years in the database, we restrict our analysis to 1990 to 2020. We perform an additional filtering step for the abstracts. Firstly, not every paper has an abstract, so the total number of abstracts is lower than that of the unrestricted sample. We then filter the abstracts for English papers published between 1990 and 2020. In total, we use 43.5 million papers for the non-textual analyses and 34.4 million papers for the textual analyses.

Word	Zygotic	Genome	Activation	Totipotency	Pioneer	Factor	MEAN
Frequency [as % of all English words]	4.07 x 10-8	5.01 x 10-6	6.76 x 10-6	0.0 [extremely rare]	8.13 x 10-6	4.68 x 10-5	1.11 x 10-5

Word	Causal	Test	Strength	Weak	Ties	MEAN
Frequency [as % of all English words]	2.14 x 10-6	1.55 x 10-4	6.31 x 10-5	4.57 x 10-5	2.24 x 10-5	5.77 x 10-5

Table S2: Average word frequency of words in two titles

To illustrate how we calculate the average word frequency for abstracts, we can use two tiles from a recent issue of *Science* that represent different ends of the hard–soft spectrum: "Zygotic genome activation by the totipotency pioneer factor Nr5a2" (Gassler et al. 2022) for biology, a hard science, and "A causal test of the strength of weak ties" (Rajkumar et al. 2022) for a softer, social science. We first find the word frequency of each word in the text sample. In the above example, the word "zygotic" has a word frequency of 4.07 x 10-8 percent, which means that 4.07 x 10-8 percent of all English words are "zygotic." We then take the average word frequency of all of the words in the text sample.

Figures

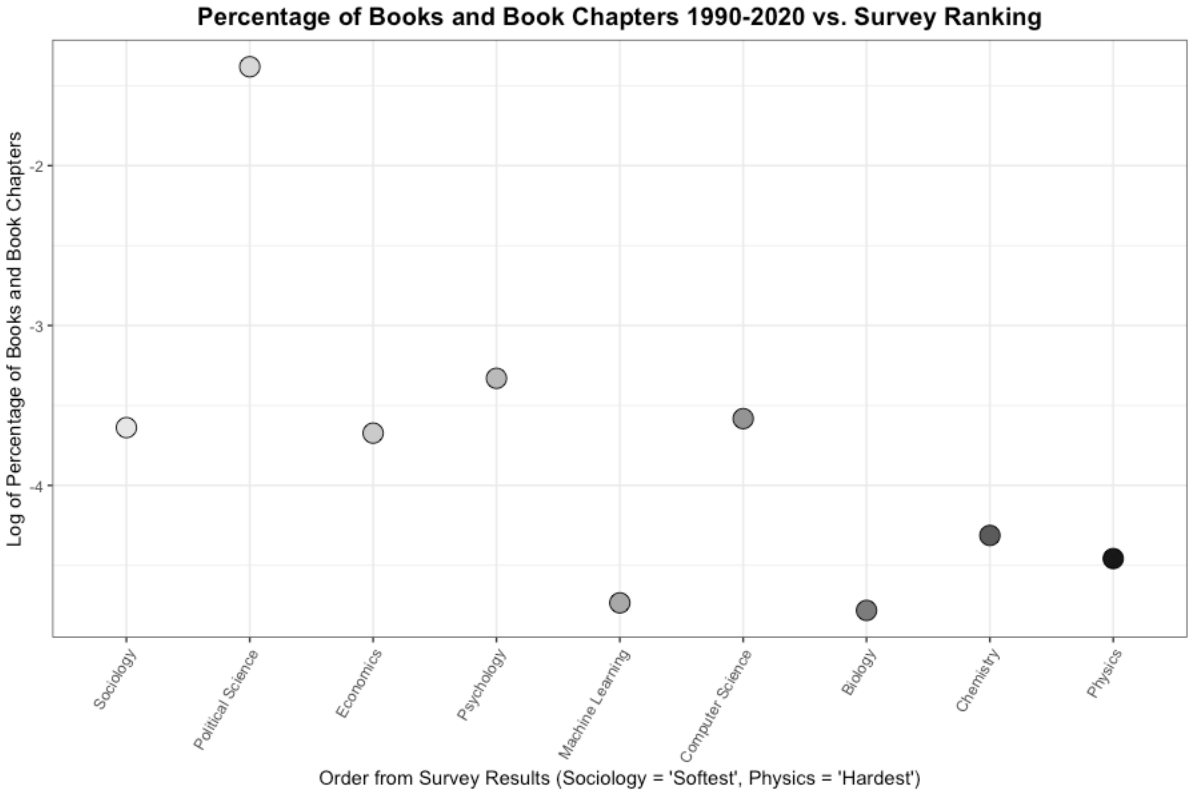


Figure S1: Log percentage of total publications in each field that are books or book chapters
An addendum to Figure 2. The hierarchy that emerges when plotting the percentage of publications in each field that are books is robust to the inclusion or exclusion of book chapters.

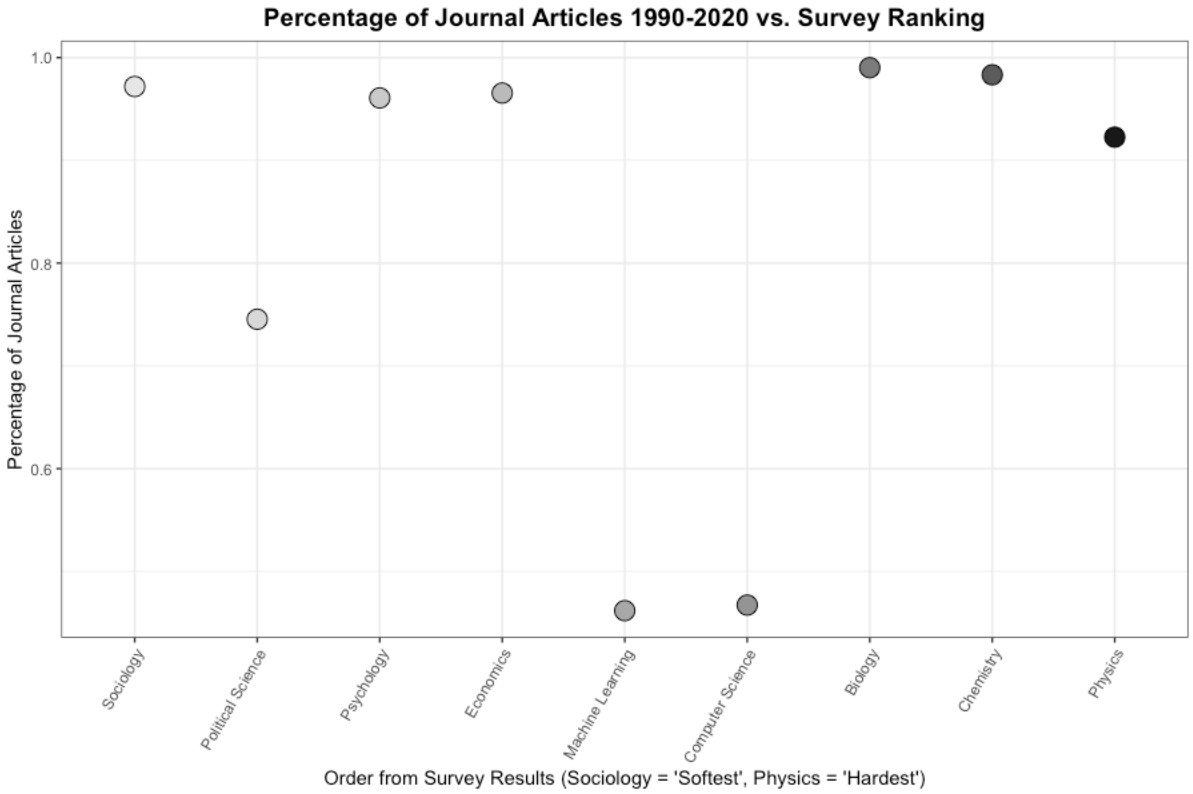


Figure S2: Percentage of total publications in each field that are journal articles
An addendum to Figure 3. Biology and chemistry are dominated almost entirely by journal articles, and, as shown in Figure 3, physics is a journal discipline with a significant percentage of conference papers. Taken together, this figure and Figures 2 and 3 show that harder sciences tend to be conducted in a mixture of journal articles and conference proceedings where softer sciences are reliant on journal articles and books.

Towards a Sociology of Sociology: Inequality, Elitism, and Prestige in the Sociological Enterprise From 1970 to the Present

ABSTRACT

There is a science of science and an informal economics of economics, but there is no sociology of sociology. We turn the central findings and theoretical lenses of the sociological tradition of the study of stratification inward on sociology itself to investigate how sociology has changed since the 1970s. We link two bibliometric databases to trace diachronic relationships between PhD training and publication outcomes, both of which are understudied in the science of science and sociology of science. All of sociology's top 3 journals remained biased against alum of less prestigious PhD programs, and the house bias of the American Journal of Sociology in favor PhD alumnae of UChicago has intensified.

INTRODUCTION

In the words of one economist, "navel-gazing has been a favorite activity of economists (and other academics) for at least sixty years" (Hamermesh 2013, 162). Sociologists are no stranger to looking inward at their own field, but they often avoid gazing too intently at the ugly imperfections of the sociological establishment.

This is not surprising. The first and most fundamental theory of the sociology of science might be termed the Imperfect Science Hypothesis. Science is ostensibly meritocratic and objective, but, as any scientist can attest and as many sociologists of science have argued, scientists are only human, and humans, as all humans know, are imperfect. Science is, therefore, as imperfect as the scientists that practice it (Merton 1974; Latour and Woolgar 1986; Xie and Shauman 2009). This assumption undergirds the entire enterprise of the sociology of science, and almost all empirical work on the topic, both qualitative and quantitative, has proven this assumption to be true.

A famous but fraught example of science's imperfections may be found in the relationship between PhD prestige, journal publications, and job placement. It is broadly assumed by most academics that journal articles, ideally highly-cited and in prestigious journals, are the common currency of academic advancement and determine job placement. Sociologists of science have long argued that this is not the case; instead, they contend the prestige of one's PhD-granting institution and advisor determine an academic's professional success (Long 1978; Long et al. 1979; Allison and Long 1990). Indeed, some have even argued that the presumed causal relationship between productivity and profession may actually run in reverse, with the prestige of an academic's professional context determining productivity (Long and McGinnis 1981). This is a scathing refutation of the Mertonian norm of science as a 'disinterested search

for truth' (Merton 1974, 323). This is on its own is distressing enough, but what if the publication process itself were biased? The question of publication bias has attracted very little attention, and it is not hard to see why. It does not behoove any professional to bite the hand that feeds him or her, and it was very difficult until very recently to attain access to the data that would make the tentative evaluation of bias in journals possible.

The question of bias in journals is particularly relevant for sociologists because the distribution of prestige accorded to sociology's major journals is extraordinarily unequal. While sociologists may not agree on the horizontal boundaries of the field's core and frontier, they certainly agree upon the general outlines of how the field stratifies vertically: the AJS, ASR, and Social Forces are at the top of sociology, and journals become more specific and less prestigious from there on down. A publication in any of sociology's top 3 journals can be seen as an argument-ender between feuding camps of professors from different subfields in hiring committees. Because of their primacy in the hiring process, publications in sociology's top 3 journals are a valuable but understudied source of data for studying the sociological enterprise. Even in the 1990s, sociologists had observed that the competition to publish in top journals grew more intense year after year, making "journals the unequivocal courts of first instance for professional success" (Abbott 1999, 190).

If those 3 journals form the narrow peak of sociology, the field's wide base is the dissertation. Dissertations are read by very few and are generally not seen as prestigious. They are a perfunctory exercise that every sociologist must complete and may also turn into journal or monograph-length publications. From base to peak, then: every academic sociologist must complete a dissertation, but not every academic sociologist publishes journals, for some attrit from the academy, and far fewer academic sociologists ever publish in sociology's top 3 journals.

To understand who makes it to sociology's peak, we must understand how the peak of sociology relates to the base. This was not possible until very recently. To do so, we link a database containing every sociology thesis published since 1970 to another bibliometric database that contains almost every journal article published in sociology. This allows us to explore how PhD prestige may influence publication outcomes. While we do not claim to have comprehensively summarized every element of American sociological establishment, our data provides a robust overview of elitism in the American sociology.

There is very little empirical inquiry in sociology on the sociological enterprise itself, and we argue that sociology is overdue for a temperature-taking. Following the scientists of science (Fortunato et al. 2018) and a tradition that we name the economics of economics (Heckman and Moktan 2020), we will turn one of our field's grandest traditions, specifically the sociological study of inequality, on sociology itself. We are not the first team to do so. There has been a scattered flurry of insightful sociological work on sociology itself over the past few decades, particularly in the 1970s (Shamblin 1970; Yoels 1971). This pioneering work unfortunately never condensed into a subfield with legs of its own the way it has in economics or other social sciences. While this is unfortunate, it is not surprising because studying sociology poses a few major difficulties. Chief among these difficulties is that sociology is a notoriously nebulous field with no formal core, and it is has only grown more diffuse since the field first became large enough to serve as a legitimate object of study fifty years ago. Sociology is so low in consensus that Thomas Kuhn's theory of the scientific paradigm, arguably the most well-known theory in all of science studies, was inspired in part by watching sociologists bicker at the Institute of Advanced Study (Kuhn 2012 [1962], 21). Thirty years after Kuhn's inspiration, Cole summarized the state of sociology in the 90s with a single, pithy sentence: "There seems to be no

sociological work that the great majority of the community will regard as both true and important" (Cole 1994, 134). In the core-frontier framework of Cole (1983, 1992), it is very difficult to pinpoint what the core of sociology contains, and it is more difficult still to delineate where the boundaries of the frontiers of sociology lie. Early and more recent empirical explorations of this topic have confirmed that sociology is among the lowest-consensus fields of study in the academy (Merton 1974; Cook and Xie 2023).

We devote special focus to elitism in the form of a preference for alumni of high-ranking PhD programs. There are certainly other vectors of bias in academia, notably along race and gender lines: the authors of journal articles are overwhelmingly male (Son and Bell 2022), as are the editors of journals, and male editors tend to self-publish in their own journals at higher rates than do women (Liu et al. 2023). Additionally, women are less likely to receive credit in the form of authorship on a paper for their work (Ross et al. 2022), and, in physics, papers authored by women are cited less (Teich et al. 2022). While gendered and racialized bias operate in sociology as well, critiques of elitism occupy a special place in the sociology of science and in the nascent sociology of sociology (Shamblin 1970; Long et al. 1979; Allison and Long 1990). We aim to revive the theoretical focus on prestige that was emblematic of these two traditions. While there was a series of pioneering works on elitism published in the 2010s (Khan 2012; Khan 2013; Sherman 2017; Cousin et al. 2018; Keister et al. 2022), the sociological literature on elitism is much thinner than the literatures on other vectors of inequality. It is also conspicuous that elitism is understudied in sociology despite the continuing prevalence of elitism in the field, which we will also show with our analyses. Though sociology has come tremendously far in addressing issues of race and gender, we argue that sociology has yet to substantively contend with its nagging history of elitism.

Our analysis comprises two parts. We first analyze the interaction between prestige and publication outcomes in sociology's three leading journals: the AJS, the ASR, and Social Forces. Drawing upon methods and theory from the of sociological study of inequality, we find that articles penned by alumnae of prestigious PhD programs are over-published but under-cited in top sociology journals and this bias has been more or less constant for decades. This bias in favor of prestige implies a halo effect (Thorndike 1920) of prestige on publication outcomes and is a clear violation of meritocratic norms. We then examine one of the testiest topics of water-cooler discussion in sociology departments across America: does the close connection between the UChicago sociology department and the AJS invite nepotism? We find that house bias towards UNC-CH alum in Social Forces, a situation roughly analogous to that of UChicago and the AJS, was strong in the past but has since dwindled to almost nothing in the present. By contrast, house bias favoring UChicago PhD alum at the AJS has only increased in the past 2 decades. Sociology, then, is unfortunately unequal, which has profound consequences for a field that purports to study inequality. More broadly, we hope to make a case for the sociology of sociology and invite others to analyze sociology with the methods of the sociologist.

LITERATURE REVIEW

WHAT PUBLICATION BIAS IS AND IS NOT

The terminology surrounding biases and effects in the social sciences is inherently confusing. A central issue regarding the definitions of common types of bias is that their phrasing rarely indicates the direction of the action or activity that may be biased. Publication bias is a first-rate example of this phenomenon. In most cases, publication bias is generally understood to refer to a bias regarding what types of results are submitted for publication (van Aert, Wicherts,

and van Assen 2019; Leichsenring et al. 2017; Franco, Malhotra, and Simonovits 2014). Publication bias understood thusly is often diagnosed with funnel plots, where published effect sizes are plotted in tandem with measures of precision (Mavridis and Salanti 2014). Asymmetry in funnel plots is taken to indicate publication bias, and these plots are frequently used in meta-analyses in biomedical research (Sterne et al. 2011). Sociologists have used other statistical tests to examine the extent of this framing of publication in our own field (Gerber and Malhotra 2008).

Still more confusing is that many definitions of publication bias do not differentiate between bias in authors submitting papers for publication and in journal editors' reception of these publications (Eastbrook et al. 1991; Song, Hooper, and Loke 2013). These discrete stages are instead bundled together and treated as one. The almost universal disdain for null findings that is reported across the publication bias literature is then treated as stemming from either element. We focus on not publication submission bias but on publication reception bias.

REFLEXIVE META-FIELDS AND THE ECONOMICS OF ECONOMICS

The landscape of academia is peppered with a mixture of both formally and informally defined subfields that train the methods of a given field on the field itself. We term these recursive meta-fields. Before we propose a sociology of sociology, we will first summarize extant work recursive meta-fields. The most famous of these is the science of science (Fortunato et al. 2018), which purports to use the scientific method to science but informally amounts to the quantitative study of science at scale. The scientists of science have produced pioneering work on the influence of team structure on science (Jones, Wuchty, and Uzzi 2008; Adams 2013), the lifecycle of citations (Yin and Wang 2017), and productivity across the career (Liu et al. 2021).

While the science of science is a formal subfield, the other recursive meta-fields are decidedly less formal. There is a very loose school of work that may be termed the psychology of psychology (Blasi et al. 2022), and there is historiography, which is essentially a history of history (Cheng 2012). We must look elsewhere, then, for a model from which to build a sociology of sociology.

Economics is the only social science broad enough to foster an active if informally defined reflexive meta-field of its own. Though the authors who publish in its pages do not describe their work with the term, the Journal of Economic Literature is dedicated to what may be termed an economics of economics. The sheer size and relative cohesion of the recursive meta-field of economics makes it a very well-defined and stable departure point for our sociology of sociology. While sociologists have devoted very little empirical attention to sociology's top journals, economists have analyzed publishing trends in their field's most prestigious journals in great detail. Economics, a larger field by far than sociology, has five top journals to sociology's three. As is the case for publications in sociology's top three journals, a publication in a top five economics journal is crucial to professional advancement in economics. Publishing in the top five journals, however, has only grown more difficult. Top 5 economics journal acceptance rates from dropped from 15% in 1976 to 6% in 2012 as annual submission rates doubled from 1990 to 2011 (Card and DellaVigna 2013).

Any bias in this process would taint the putative meritocracy of economics, and, at first blush, it appears that there is bias in this process. Of particular concern is that the world economics has grown more nepotistic in general; the central node in a network representation of economics coauthorship comprised 15% of all connections in the 1970s but 40% of all connections in the 2000s (Goyal et al. 2006). This nepotism is especially evident in top five

journals, which show favoritism to scholars who are connected to the members of their editorial boards (Heckman and Moktan 2020, 422). One survey of connections between journal editors and the scholars who publish in said journals estimates that 43% of the 1,620 articles in 4 of the top 5 journals published between 2000 and 20006 were authored by a scholar with a connection to a member of the editorial board. Specifically, the former doctoral students of editors wrote 15% of the articles and former colleagues a staggering 29% of articles in these journals (Colussi 2018). While some economics have argued that nepotism may be used to identify high-impact papers more efficiently (Laband and Piette 1994), far more economists have instead explored how nepotism warps the metaphorical marketplace of ideas in economics. Given the relative dearth of work on this subject in our field, it is fitting to start with what economists have written about theirs.

HOME BIAS IN ECONOMICS

If publication patterns in top economics journals bear some resemblance to the publication patterns of top sociology journals, it is worth exploring some the explanations economists offer for why their top journals have malfunctioned. One major category of explanations involves affiliation-based nepotism, and economists generally refer to this type of bias as home bias. Unfortunately, economists refer to many other types of bias as home bias.

The specific meaning of the term ‘home bias’ in particular can vary independently across two distinct dimensions. Firstly, home bias can refer to two distinct directions of actions. The term applies to active actions, such as investors choosing which countries to invest in (Dlugosch, Horn, and Wang 2023), and also receptive actions, such as an editor showing favoritism to submissions to a journal from academics with the same PhD affiliation. Secondly, home bias can

refer to either regionally-oriented or institutionally-oriented actions, such as national leaders with an ethnic ties directing resources to their region of origin (Bommer, Dreher, and Perez-Alvarez 2022) and potentially the AJS favoring affiliates of UChicago. We focus on the definition of home bias as referring to institutionally-specific, receptive actions.

There are two direct analogues to the close association between the AJS and UChicago in economics. The first analogue is The Journal of Political Economy, which, like the AJS, is affiliated with UChicago, and a UChicago affiliates are over-represented in the pages of the journal: a full 14.3 percent of published authors in the JPE are affiliated with UChicago (Heckman and Moktan 2020). The second of these analogues, the Quarterly Journal of Economics, is closely affiliated with Harvard but not as closely as the JPE is with UChicago. A full 33 percent of authors in the QJE are affiliated with Harvard and/or MIT (Heckman and Moktan 2020). This is troubling given that it is now the top journal in the field, defined in this case as the journal with the highest median citation count per article (Card and DellaVigna 2013). Publication count, however, is not meaningful in isolation and requires that we know either the origin or outcomes of publications to evaluate claims of putative bias. In other words, to use raw publication counts to investigate journal-level bias, we require a control either in the form of a pre-hoc quantity to serve as denominator or post-hoc quantity to serve as numerator.

Economists of economics have use article-level citation rates as a proxy for article quality and as a form of post-hoc control for this exact purpose. If there were bias in Harvard journals, for example, we would expect articles from Harvard affiliates to underperform in the pages of Harvard house journals with underperformance defined imperfectly as low citation. Economists of economics have found that there does not seem to a bias in favor of publications from Harvard affiliates because in Harvard journals, as of 2011, Harvard faculty were the most-cited group of

faculty in the top 5 economics journals (Ellison 2011). There may even be a countervailing bias against authors from UChicago at the UChicago-affiliated JPE, whose articles are cited more in the JPE than those published by the faculty of other schools (Oswald 2008).

This work, while very valuable, does have a few shortcomings. The first, which is no fault of the economists, is that most studies of nepotism and publication with a few exceptions in the economics of economics have tended to treat institutional affiliation at time of publication and not PhD origin as the main vector of nepotism. This is because PhD origin had to be manually tabulated from CVs until recently. The second shortcoming is that no economists have created index-based approaches a la Xie and Greenman (2009) to compare the separate but related signals of publication count by institution and mean citation count by institution.

PRESTIGE BIAS

With the evidence for nepotism in economics established, we may also ask if there are other sources of bias that skew economic scholarship. Nepotism is relational and directed, but there may be a more general and transferable source of unfair advantage conferred by prestige. Economics have explored the impact of prestige on productivity and career outcomes.
Some have defined prestige as network centrality, and they have shown that more well-connected advisors lead to better career outcomes (Rose and Shekhar 2023). Others have defined it as rank of graduate institution (García-Suaza, Otero, and Winkelmann 2020). Graduates of top economics programs find full-time professorial employment at higher rates are more satisfied with their academic than their peers of less prestigious origins (Siegfriend and Stock 1999). Students who graduate from prestigious PhD programs attain better jobs than their peers, but there is an independent effect on professional success of being advised by a highly-cited advisor

even in a highly-ranked department (Hilmer and Hilmer 2007). The effect of an eminent advisor can be enormous. Though the definition of 'leading economists' was left vague, a 2000 paper found that the benefit of a 'leading economist' writing a recommendation letter led to a prestige increase of 60 points on the USNWR rankings for a young economist's first professorial job (Krueger and Wu 2000).

PhD prestige also dramatically influences publication success in economics. A landmark paper from 2001 found 28% of publications in top 30 economics journals were published by authors from 12 US-based universities, all of which were highly ranked (Hodgson and Rothman 2001). A replication of the paper found that this concentration was more pronounced for higher-ranked journals; graduates of the top 5 economics PhD programs publish 45.5% of the articles in the top economics 5 journals (Ailstleitner, Kapeller, and Kronberger 2023, Figure 7). The favor shown to graduates of top departments is rendered more suspicious because the median graduate of certain top departments does not appear to perform as well as those from some stand-out lower-ranked departments, including Carnegie Mellon (Conley and Önder 2014).

If elite economics in general is this nepotistic and if a handful of elite journals with institutional ties show specific favoritism to other members of their host institutions, then it begs the question: are other social sciences similarly biased? The short answer is that some evidence suggests that politics is no better that economics (Reingewertz and Lutmar 2017). We may then ask how sociology fares in comparison to its cousins in the social sciences.

A NASCENT SOCIOLOGY OF SOCIOLOGY

Where there is an economics of economics, there is barely a sociology of sociology. There is no dearth of sociological thought on science, and the sociology of science is a very rich subfield, but

the most eminent sociologists of science have ironically rarely studied their own fiefdoms. Though Long has published primarily in sociological venues for his entire career, his famous series of ASR papers arguing that the main determinants of professional success are not productivity but the prestige of one's job placement and PhD origin focused exclusively on the natural sciences (Long 1978; Long et al. 1979; Allison and Long 1990). Zuckerman's work on Nobel laureates similarly focused on scholars in the harder sciences (Zuckerman 1972; Zuckerman 1977). Zuckerman and Merton do discuss the humanities and social sciences in passing in Zuckerman and Merton (1971), and Stephen Cole does the same in Cole (1983), but these works are inherently comparative and seek to understand the hard-soft distinction. The sociology of sociology never earned the intensive focus of the sociology of science's commanding stars, and it was never a high-prestige pursuit.

The nascent beginnings of a sociology of sociology may be found in a handful of papers from the 1970s that presented evidence of elitism and nepotism in the sociological establishment. Many suggested that the main source of bias in the sociology publishing were related to editorship bias. Though the ASR has never been as closely linked to one institution in the manner of the AJS, it has historically been yoked closely to a small handful of very elite institutions. Four-fifths of the 1963-1965 ASR editorial board was from sociology's then-top five departments (Shamblin 1970). From 1948 to 1968, PhD alumnae of only 3 institutions, namely UChicago, Harvard, and Columbia, commanded 61.2 percent of the positions on the ASR editorial board (Yoels 1971). During this period of the ASR's history, editors-in-chief from one of three aforementioned departments tended to recruit alumnae of their PhD-granting department to the editorial board at disproportionate rates, and it was not uncommon for a full 33 to 37% of the editorial board to be composed of the editors' PhD peers. A quick survey of the ASR editorial

board proves that this is the case: while the editors of the 2024 editorial board all hail from the University of Massachusetts-Amherst, the full editorial board features scholars from universities across the United States and even from most major regions of the world, and the rankings of these universities vary dramatically.

While there is some contemporary work on the sociology of sociology, there is very little, and it does not address bias in sociology's top journals. The closest equivalent to The Journal of Economic Literature in sociology is The American Sociologist, but the 2 journals differ greatly in their impact and prestige. The Journal of Economic Literature's impact factor is 11.5 as of 2024, and it serves as an informal repository for review articles of economics as well as a forum for articles on the state of economics. There is, then, an incentive to publish in the journal, and many of the biggest names in economics have published economics of economics articles in its pages. This is not the case for The American Sociologist, which has an impact factor of 0.9 as of 2023. While the impact factor is an imperfect metric, articles in the journal are not very visible to sociology as a profession and do not garner much attention even though many of the sociology of sociology articles it publishes are of very high quality.

Much modern research on stratification in sociology has been conducted with a relational, network-oriented focus. It is generally the case in sociology that elite PhD programs overproduce graduate students who then must seek tenure track professorial posts at less prestigious universities (Baldi 1994). Some sociologists have followed the implications of this finding and adopted a framing of prestige defined as "centrality within hiring networks" (Val Burris 2004, 239; Hevenstone 2008). This view has impressive explanatory power and is able to explain a very large amount of the variance between departmental rankings, but it relies on outcomes to define prestige. Prestige is at least as much about public perception as objective

outcome, and it may be more fitting to use measures that directly capture how sociologists view their peers, such as the US News and World Report rankings, and some work has addressed prestige in the modern sociological establishment. PhD prestige has immense influence on the prestige of an academic's first professorial job (Baldi 1995).

EVIDENCE OF BIAS IN SOCIOLOGY'S TOP JOURNALS ACROSS TIME

Sociologists have investigated bias in top sociology journals from the discipline's earliest decades to the 1970s, but they have not done so since. For a sense of how these trends have changed over time, we can compare the overrepresentation of UChicago PhD alum in the pages of the AJS from the journal's earliest days to the present. The AJS was seen as an informal operation, and, perhaps as a consequence of its informality, it was also widely regarded as a dominion of UChicago PhD alumnae (Abbott 1999). The nature of competition suggests that more competitive markets permit less room for bias to muddy results (Hong and Kacperczyk 2010), and the number of sociologists has increased dramatically since the 1900s. We can then assume that bias in sociology's top journals was mechanistically more pronounced in the early half of the 1900s, and we can take the bias of the AJS shown towards UChicago graduates and alumnae of top institutions, which is to say the home bias of the AJS and the prestige bias of the AJS and other top journals, as a baseline for evaluating the bias of sociology's major journals in the present.

There are two sources for the PhD origins of authors in sociology's top journals from before the period that coverage that we study, which is 1970 to the present. The first of these sources on early bias in the AJS covers 1900 – 1920. Table 2 in Abbott (1999, 90) records the PhD origins of contributors to the AJS for the first three full decades of the journal's history. The

table is summarized below with authors from Columbia, the second most-represented school, included for completeness.

Table 1: PhD origins of AJS contributors by decade from 1900 to 1920 as count

Decade	Total authors	UChicago PhD count	Columbia PhD count
1900	131	29	7
1910	164	24	18
1920	136	30	30

Table 2: PhD origins of AJS contributors by decade from 1900 to 1920 as %

Decade	Total authors	UChicago PhD count	Columbia PhD count
1900	131	22.1%	5.3%
1910	164	14.6%	10.98%
1920	136	22.05%	22.05%

Overall, we see that alumnae from UChicago published between 14.6% and 22% of the articles in the AJS from the 1900s to the 1920s.

The second historical datapoint on bias in sociology's top journals is from 1945 a study of ASR submissions, which found that submissions from elite institutions both private and public were more likely to be accepted than those from less-prestigious institutions (Goodrich 1945). The author of this piece unfortunately used then-current affiliation instead of PhD origin, which means that it is not directly comparable to Table 1 and Table 2. Regardless, this still provides evidence of prestige bias in sociology's past. The work also showed that universities from the mid-Atlantic submitted 45 articles with 21 articles accepted, and those from the eastern portion of the Midwest submitted 35 with 24 articles accepted. Sociology departments in these two regions occupy the commanding heights of elite programs in sociology. Universities from the American South, a region with a comparative dearth of strong sociology programs, submitted 7 articles with 5 articles accepted, and those from outside the US submitted 7 with 6 articles

accepted (Goodrich 1945). We see, then, that authors from regions of higher average prestige submit many times more articles but enjoy a far lower rate of article acceptance than authors from regions of lower average prestige. The sheer difference in submission volume, however, means that authors from high prestige regions have more than three times the total article count in the ASR. While a wider, more diffuse version of home bias that we might term regional bias may also skew results in favor of authors from the Midwest and the northeast, we can read this as additional albeit inconclusive evidence of prestige bias.

A third datapoint on bias in sociology publishing covers the early 1960s. In an effort objectively to rank the performance of sociology departments, Knudsen and Vaughan (1969) tabulated the count of articles published in the AJS, ASR, and Social Forces by PhD-granting department for recent sociology PhDs who graduated between 1960 and 1964 and for then-current faculty. UChicago PhD alumnae were the most-published in the AJS by far compared to PhD students from other departments with 133% of the AJS publications of the Columbia, the second most-represented PhD origin in the AJS, and UChicago faculty were the most-published in the AJS by a similar margin compared with the second-ranked University of Michigan. (Knudsen and Vaughan 1969, Tables 1 and 3). Knudsen and Vaughan unfortunately use a points-based system to weight different types of publications across publishing venues. While this system is interesting, it does not allow for the recovery of raw article counts, and it is hence impossible to compare these results to those tabulated by Abbott.

We now have a rough baseline for later comparison. Regarding home bias, we know that in the 1920s, roughly 20% of articles in the AJS were published by PhD alumnae of UChicago. This home bias had diminished significantly in the 1960s, during which UChicago PhD alumnae published 1/3 more articles than alumnae of the second-most published PhD origin. Regarding

prestige bias, we know that all journals published far more work from sociologists of prestigious PhD origins, though we do not have a measure of the discrepancy in publishing success between prestigious and less-prestigious PhD origins.

DATA AND METHODS

DATA

We are the beneficiaries of access to data that previous scholars in this field did not enjoy. Large bibliometric databases, such as the Web of Science and Scopus, have brought big data to the study of science and transformed the quantitative study of knowledge production.

We draw on a combination of databases with overlapping but distinct bits of information and link them. We specifically use the Microsoft Academic Graph, which contains information on more than 150 million papers and their authors; the ProQuest Dissertations & Theses database, which provides access to the full text of almost every dissertation published in the US and many published abroad since 1970 with associated metadata; and Scopus, an index of journals that provides abstracts for most articles. We focus on PhDs from the United States, and we do so for a few reasons. Sociology in the United States is notably parochial in terms of both disciplinary and national focii. While there are some scholars who have earned doctorates in other fields, they are rare. Similarly, while there are some scholars from outside the US who publish in top American journals, notably a contingent of Dutch scholars, the vast majority of those who publish in American sociology journals earned their doctorates in sociology from American universities and publish almost exclusively on the United States. The content of American sociology tends to revolve around the United States.

We link the ProQuest Dissertations & Theses database to Microsoft Academic Graph using a combination of string matching and vectorized representations of words via word embedding methods. We specifically use GloVe (Pennington, Socher, and Manning 2014). We supplement the MAG records with more detailed journal-level information on articles in the AJS, ASR, and Social Forces from Scopus. This gives us a comprehensive view of all the publications of every scholar who has published in the AJS, ASR, or Social Forces from the 1970s to the present.

We first present a selection of descriptive results to motivate our later work. Figure 1 shows the cumulative percentage of articles published by PhD-granting sociology departments arranged in order of article count in top journals. It is an application of the Lorenz curve to the social sciences with articles in top journals swapped for wealth and PhD-granting departments swapped for households or individuals (Lorenz 1905). We see that the inequality of each journal as expressed by the relative monopolization of journal publications by top institutions varies dramatically by journal. The AJS is the least equal journal among sociology's top 3 journals. The ASR is slightly more egalitarian than the AJS, and Social Forces is the most equal of all, but this equality is only relative, for the distribution of articles in these journals to the alumnae of top PhD-granting institutions is far more stratified than even the most inegalitarian countries in the world.

METHODS

After linking our databases, we tabulate a series of index-based metrics to control for relative bias inspired by Greenman and Xie (2008). Before formulating these metrics, we must decide how to quantify bias. The central question that emerges from the extant literature in sociology

and economics on both prestige bias and home bias in journal publishing is where in the publishing pipeline to look for bias.

Stage of pipeline	1: Pre-submission covariates	2: Decision	3: Publication	4: Post-publication citation count
Metric of interest	Many, but one possible metric is: Total size of PhD cohort	Editorial Decision rates	Publication count	Citation count
Import	Pre-submission covariates may serve as additional controls for other metrics	A proxy for how the editorial board and reviewers evaluate the pre-publication quality of a paper or set of papers	Because any publication in a top journal is valuable academic currency in and of itself, raw publication count can be a valuable signal of either editorial bias or PhD quality	A proxy for how other scientists view the quality of a paper or set of papers

Table 3: An overview of the publication pipeline and metrics that may be used for evaluating prestige bias and home bias

Each stage of the publishing pipeline offers a new metric that may be used to examine bias. We offer a summary of these metrics and their potential significance in Table 3. Some of these metrics are already proxies of article quality. Acceptance rates are a pre-hoc proxy for quality, and citation counts are a post-hoc proxy for quality. More specifically, acceptance rates loosely map to quality as defined by the editors of a given journal, and citation rates are a very imperfect measure of quality as evaluated by a field or subfield. We may then combine metrics from various stages of the pipeline to examine how papers move from submission to publication in more detail.

Combined Metric	Source Metrics	Import
Acceptance rate by PhD origin in a given journal	1: Pre-submission covariates [size of cohort] + 2: Decision	A confounded measure of both PhD student quality and potentially prestige or nepotistic bias; requires sensitive data
% of PhD alumnae from a given PhD origin with a publication in a given journal	1: Pre-submission covariates [size of cohort] + 3: Publication	A confounded measure of both PhD student quality and potentially prestige or nepotistic bias
Relationship between publication count and citation count for given PhD origin in a given journal	3: Publication + 4: Post-publication citation count	Combines publication count, a strong professional signal, with citation count, a signal of quality with some professional importance, to examine editorial judgement post-hoc
Relationship between editorial acceptance rate and citation by PhD origin	1: Pre-submission covariates [size of cohort] + 2: Decision + 4: Post-publication citation count	Combines pre-hoc and post-hoc judgements of quality; would be a very robust measure of editorial bias but requires sensitive data

Table 4: Examples of combined metrics for evaluating prestige bias and home bias in the publication process

Table 4 provides examples of some of these metrics. We would ideally have access to the relationship between editorial acceptance rate and citation by PhD origin, but this requires very sensitive data that most journals would not provide. We focus on the relationship between publication decisions and post-publication citation count because it relies on easily accessible information on articles and offers a reliable way to scan for bias in editorial decisions.

After deciding upon which stage of the publication process to direct our attention, we may now tabulate indices of publication and citation. We must first, however, contend with the primary difficulty of comparing citation counts, which is that they follow a power law

distribution (Clauset et al. 2009; Peterson et al. 2010; Brzezinksi 2015). Processes that follow a power law distribution are distinguished by extreme outliers. While the arithmetic mean is serviceable for normal distributions, the arithmetic mean of a power law yields a very biased estimate skewed towards higher values. These problems only grow more pronounced when analyzing subgroups of a power law distribution, such as the average citation per PhD-granting institution of articles in a given journal. A possible solution may be found in the geometric mean. For citation counts specifically, the geometric mean is more reliable than the arithmetic mean or percentile for comparing citation counts between nations (Thelwall 2016). We adapt the spirit of this approach to compare citation counts between articles written by alumnae of different PhD-granting institutions. The geometric mean has a very long history of usage in the social sciences (Galton 1879; Goodman 2017). While the geometric mean is more mathematically sound in theory than the median for comparing citation counts, the geometric mean and the median behave very similarly in practice. Other scholars (Card and DellaVigna 2013) have used medians to compare citation counts. We perform the analysis with both methods, and our analyses are robust to this specification (see Supplemental Information).

The geometric mean is found by taking the arithmetic mean of a logged sequence of numbers and then exponentiating the result:

$$Geometric\ Mean = \exp\left(\frac{1}{n}\sum_{i=1}^{n}\ln(x_i)\right)$$

A second major difficulty that arises when working with citation counts is that citation counts vary between journals and across time. To control for this, we propose an index that we

term the citation multiple. The metric answers a simple question: How do we establish whether articles from PhD alumnae of a certain school over- or under-perform relative to articles from alumnae of other institutions? We control for periodicity and journal effects by taking the geometric mean of the citations earned for all articles in a given journal in a given decade. We then match each article to its journal- and decade-specific mean and divide the citations said article has earned by the journal- and decade-specific geometric mean of citation. This gives us a measure of relative citation count. We then take the geometric mean of citation multiple by institution. This yields a robust measure of how a given institution's progeny compare to others. An institution-level citation multiple of 1 indicates that PhDs from said institution publish articles that perform as well as the average articles in top sociology journals. An institution-level citation multiple of 2 would suggest that PhDs from said institution publish articles that perform twice as well as what would be expected.

Over- or under-citation, however, is only one component of the putative bias that we seek to uncover. If publishing in the journals is the central currency by which young academics either advance or falter in the academy, then the binary outcome of publishing in sociology's top journals or not is what matters. We must then create a similar index to investigate over- or under-publication by PhD-granting institution. Because publication counts do not follow a power law distribution, calculating publication overrepresentation is much more straightforward and does not require geometric means. The simple arithmetic mean is more than adequate for comparing publication counts between institutions.

We now have indices for over- or under-citation and over- or under-publication. While we can use these indices in isolation, we can combine them to arrive at a single, readily interpretable metric that summarizes both aspects at once. By dividing the publication multiple by the citation

multiple, we can plot over-publication and under-citation simultaneously. Given that we expect PhD alum from prestigious universities to be both over-published and under-cited in sociology's top journals, this metric is ideal for our purposes, and we term it the *citation-adjusted publication premium.* We use this metric to explore two specific manifestations of elitism in sociology: a general bias in favor of prestigious PhDs from prestigious universities across all journals that we term *prestige bias* and a more specific bias in some journals towards PhDs from affiliate departments that we term *nepotistic bias*.

RESULTS

We begin by presenting our results for prestige bias, the most general form of bias. We first segment the prestige of PhD origins as quantified by the US News and World Report into three band: Top 1-15, Top 15-50, and Top 50-100. Economists of economists have used similar approaches to group schools by prestige (Siegfried and Stock 1999).

The relative positions of sociology departments have changed dramatically over the past five decades. To account for this, we have collated a list of history USNWR rankings and match every PhD graduate to the prestige of his or her department at year of graduation[20].

[Figure 2 here]

Figure 2 shows the change in prestige bias over time for the AJS, the ASR, and Social Forces. We see immediately that the citation-adjusted publication premiums for all three prestige bands are very different remain separate except for only one of 15 possible journal-decade

[20] For years with missing data, we interpolate USNWR rankings by taking the average of years on either side of the rankings lacuna.

combinations: in the AJS in the 1970s, the citation-adjusted publication premium of Top 10-50 departments is lower than that of Top 50-100 departments for the only time in our data. Apart from this lone exception, Top 10 departments enjoy a strong and pronounced advantage citation-adjusted publication premium that peaks in the 1990s and 2000s and declines in the 2010s. This decline in the 2010s is most pronounced in the AJS, but the ultimate result of this decline yields a citation-adjusted publication premium of 1.34, which is composed of a publication multiple of 2.00 and a citation multiple of 1.49. In other words, top 10 schools published papers twice as often as would be expected, but their work was cited only 1.5 times more than the work of all other schools. The bias towards top 50+ institutions, however, remains almost unchanged in the AJS into the 2010s. PhD alumnae from top 50+ institutions publish 0.38 papers for every 1 paper published on average, but their papers are cited 1.91 times more often than average. This publication multiple of 0.38 and citation multiple of 1.91 yield a citation-adjusted publication premium of 0.20. It is of interest that the ASR maintains, on average, the most intense prestige bias in favor of top 10 institutions, who enjoy a citation-adjusted publication premium of 2.01 in the ASR in the 2010s, and against top 50+ institutions, who suffer from a rock-bottom citation-adjusted publication premium of 0.18 in the ASR in the 2010s. The least prestige-biased journal is Social Forces.

We provide a decomposition of the citation multiple and publication multiple plots used to create the plot of the citation-adjusted publication premium in the supplementary information (Figures S1 – S2). We additionally provide a diachronic view of prestige bias where we present prestige as a continuous variable instead of the discretized variant we use in the analyses above (Figures S3 – S5). Doing so changes the focus of our analyses but does not affect our results.

HOUSE BIAS

We have seen that, in the aggregate, the citation-adjusted publication premium is inversely related with departmental prestige. To examine house bias, we shift our focus slightly from prestige bias among departments of all prestige levels to house bias among a subset of the most elite sociology departments. We do so because prestige bias is distributed unevenly throughout all schools, albeit very unevenly, but house bias is a nepostic backdoor that offers a single department a backdoor route to publish in a single journal and necessarily offers a much weaker signal.

We shift our focus from the ranking of all schools based on prestige to the top 10 schools as defined not by prestige but instead by publication count in sociology's top 3 journals. The main dependent variable of interest for prestige bias is departmental prestige, but the main dependent variable interest for house bias is publishing quantity. If we assume that the schools with house bias backdoor connections publish the most in the journals to which they are connected, then we should compare schools directly on the basis of publication count.

[Figure 3 here]

Where prestige bias fell in the AJS in the 2010s, we see the opposite trend for house bias. Figure 3 shows that the favoritism given to UChicago PhD alumnae in the pages of the AJS as measured by the citation-adjusted publication premium declined from the 1970s to the 1990s, but it then increased dramatically and reached a peak of 4.45 in 2010. This citation-adjusted publication premium is composed of a publication multiple of 2.62 and a citation multiple of 0.22. This means, then, that UChicago PhD alumnae published 2.62 times more papers than PhD

alumnae from the other top 9 institutions but that the papers of UChicago PhD alumnae were cited only 22% as often. By contrast, the average citation-adjusted publication premium for the other nine departments in the top ten most-published departments remained roughly constant. A handful of schools are outliers with citation-adjusted publication premiums above that of UChicago in some decades, but, in the 2010s, UChicago rockets to first place.

[Figure 4 here]

In contrast to the AJS, house bias in Social Forces as measured by the citation-adjusted publication premium (Figure 4) declined dramatically from a high of almost 3.5 in the 1970s to 1.5 in the 2010s. UNC-CH alumnae have not been ranked first on this metric since the 1980s. In relative terms, UNC-CH has been the third school as ranked by citation-adjusted publication premium from the 1980s to the 2010s.

Overall, we see differing pictures of the progression and relative rates of bias in each journal. In the AJS, prestige bias declines while house bias increases dramatically. In Social Forces, both types of bias decline over time. The affiliation of the ASR with the American Sociological Association instead of any particular institution means that it cannot support house bias, but it is the most sharply prestige-biased of any of sociology's top 3 journals.

The primary limitation of our work thus far is that graduates of different departments publish proportionally more often in different subfields and that these subfields have sharply differing rates of average citation. This limitation has two parts, neither of which have yet been empirically verified.

Regarding average citations, it is a common contention among sociologists that different genres of sociology are cited at varying rates, and this is certainly true. Generally speaking, papers that with softer methodological or topical foci are cited less, and papers that read more like those from the hard sciences are cited more. As Cook (2024a) shows, scholars in the harder fields, who publish shorter papers at higher frequencies than do scholars in softer fields. While empirically proving so is beyond the scope of this paper, sociological work with high citation counts likely attracts cross-disciplinary citations from harder fields. The differential in paper volume between the hard sciences and soft sciences means that the easiest way to garner more ciations is to write a paper that scholars in a hard science will cite. A quick glance at highly cited articles in sociology suggests this is the case. The most cited article ever published in the American Journal of Sociology is *The Strength of Weak Ties* (Granovetter 1973), which is cited extensively for work on networks in all fields. Granovetter (1973) has almost 75,000 citations, and the majority of these citations are from fields outside of sociology. For additional evidence of this, see the Supplementary Material (Figure S6). Regarding differences in genre by department, it is widely known, for example, that sociologists from UChicago and Berkeley are known for producing qualitative work on atypical topics (Abbott 1999, Burawoy 2021). Many intuit that this work garners far fewer citations than demography-adjacent sociology, economic sociology, or computational sociology, and this is indeed the case (Figure S7).

Given these two points regarding citation by topic and differences in topic by department, one might think that the house bias shown to UChicago in the AJS can be explained by UChicago PhDs focusing on under-cited genres of sociology. To address this, we control for topic by creating a citation multiple that is topic-specific as well as decade- and journal-specific.

Our results are robust to this specification, though controlling for topic does change the magnitude of some of our results and diminishes most types of bias globally (Figures S8-S10).

There are three main limitations of our topic modeling approach. The first is methodological, and it is that most topic models allow each document to belong to more than one topic. Attaining the most accurate model possible required that we do the same. The second issue is more substantive, and it is that the topics in our topic model do not capture many of the aspects of sociological inquiry that are of the most interest to sociologists. There is no topic that easily summarizes the offbeat sociology for which UChicago PhD alumnae are famous, and there is similarly no single topic in our model for more mainstream sociological approaches to the quantitative study of inequality. Future work could explore using classification-based approaches both supervised and unsupervised to develop a finer-grained model that more accurately reflects the collective and currently implicit understanding of sociology's many genres.

The final limitation of using topic models in this manner is that it may not be substantively useful. Any publication in a top sociology journal is professionally significant, and that graduates of top-tier sociology programs publish preferentially in lowly-cited topics is an important signal. Controlling for topic diminishes this signal. It may be that graduates of prestigious sociology departments game publication in top journals by knowingly or unknowingly working on lowly-cited topics that are still of sociological importance. More broadly, why do the top PhDs of sociology work on these topics? More work is needed to understand what drives sociologists who were trained at the most eminent departments to study what they study.

Overall, we see that there are serious and deep-seated issues in the journal ecosystem of the sociological enterprise. As a final check on the health of sociology's top journals, we may ask

at how many years into their academic careers sociologists publish in these journals, and we again turn to economics for an analogy with our own field. The brutal competition for space in the top five journals has rendered publishing in them a young scholar's game. Established scholars are more likely to use pre-print venues to circulate their work than brush elbows with plucky graduate students or untenured faculty in the narrow bottlenecks to top journals (Ellison 2011). We find that this is also true for sociology. The age structure of sociology journals is similarly skewed towards the young; the mean academic age of publication in sociology's top 3 journals is, depending on the journal, 7 to 9 years after acquiring a PhD (Figure 5). Sociology's top journals are not forae for sharing knowledge; they instead collectively serve as a "screening device" (Heckman and Moktan 2020, 462) for young scholars. This does not mean that high-quality and/or foundational papers do not end up in sociology's top journals. We suggest that the processes that ultimately enable useful knowledge to accumulate in journals could be optimized.

DISCUSSION

Overall, we have seen that publishing in sociology is an elitist venture. Prestige and nepotism both tilt the scales of landing a paper in one of sociology's top 3 journals. Many sociologists have wondered if there is home bias between the two house journals in sociology, namely the UChicago-affiliated AJS and the UNC-CH-affiliated Social Forces. This is unfortunately the case. The works of UChicago PhD alumnae are over-published but under-cited in the AJS, and this favoritism has only increased over time. While the works of UNC-CH PhD alumnae PhD were once over-published but under-cited in Social Forces, this bias has diminished to essentially zero over time. More broadly, the works of scholars with PhDs from prestigious institutions are similarly over-published and under-cited in the pages of all of sociology's top 3. The good news

is that these biases have generally shrank over time, though the home bias shown to UChicago PhD alumnae has grown stronger in the 2010s. The bad news is that the inequalities in the sociological establishment seem deep-seated and resistant to change.

Our work has both methodological and substantive contributions to the study of science in sociology and beyond. The first is that we have developed a methodological toolkit for tracking affiliation- or prestige-based publication bias that can be readily deployed to other disciplines. A key strength of our index-based approach is that does not rely on proprietary data that must be provided by individual journals. Comparing metrics of this type, such as article acceptance rates, may require prohibitively time-intensive negotiation with the staff of different journals, and gaining more sensitive types of data, such as fuller metadata on the authors that submit to prestigious journals or any data on the peer review process, is even harder. The core of our methodological approach draws from numbers that may be tabulated from any bibliometric database: publication count and citation count. While we group by authors by their PhD origin, and this relies on a proprietary database, our methodological approach may be used to track other types of bias, such as bias favoring certain institutions of employment for professors or even gender- or ethnicity-based bias. We use an extension of this toolkit in Cook (2024c) to examine bias by gender and ethnicity for Chinese professors in the United States.

The substantive implications may be grouped into practical and theoretical concerns. The practical concern is that the sociological establishment has failed to live up to some of its most dearly cherished ideals. Sociology is elitist. Many sociologists would likely balk at the implication that sociology is stratified so intensely, but that does not change that it is so. Bias towards elites is not inherently bad. The competition for sociological talent and the wide array of sociological genres means that we cannot realistically expect a world where the prestige of one's

PhD has zero influence on publishing outcomes. Sociologists may ask themselves what degree of elitism is acceptable or even beneficial for sociology. More broadly, what are the core values of sociology? Is there any consensus on what a sociologist should believe or do? Sociology is a remarkably pluralistic and low-consensus field. The main virtue of this pluralism is that sociology offers an extraordinarily diverse array of paths to professional fulfillment and niches for sociologists to fill, but its main vice is that there is little to no explicit consensus in sociology on the boundaries of the sociological enterprise or what constitutes good or bad sociology. Because of this confusion over core goals and evaluator criteria, allocating the spoils of professional success in sociology can be frustratingly difficult. To ensure that new generations of sociologists continue to enter sociology, it may be useful for sociologists to seriously consider how to explicate the evaluative standards of the field, even if only partially.

Journal publishing in sociology is deeply flawed, but, unfortunately, this problem is unfortunately not all endemic nor specific to sociology. We have no intention of singling out sociology for punishment, for many of the journals that collectively compose the main knowledge preservation mechanism of science are similarly broken. To name a few salient ways in which science is not as scientific as it would like to be: peer review can be cartelized and gamed, psychology and biology are riven by existentially-threatening replication crises, and fraud is a constant companion in modern science (Ritchie 2020). It is little wonder that some fields have moved away from the journal system entirely. Preprint cultures powered by arXiv and similar preprint-hosting websites have emerged in many fields, and computer scientists have made advancement in their field contingent on publishing papers in prestigious conference. Indeed, computer science is the world's first conference-dominant discipline (Cook 2024a), but none of these solutions really work all that well. The problem is deeper than most would like to

admit. It is, in short: once knowledge is produced, how can it be transmitted, preserved, and disseminated to ensure that knowledge accumulates as efficiently and as effectively as possible? Scientists have not yet solved this wicked problem. It would bode well for the future of science if they did.

FIGURES

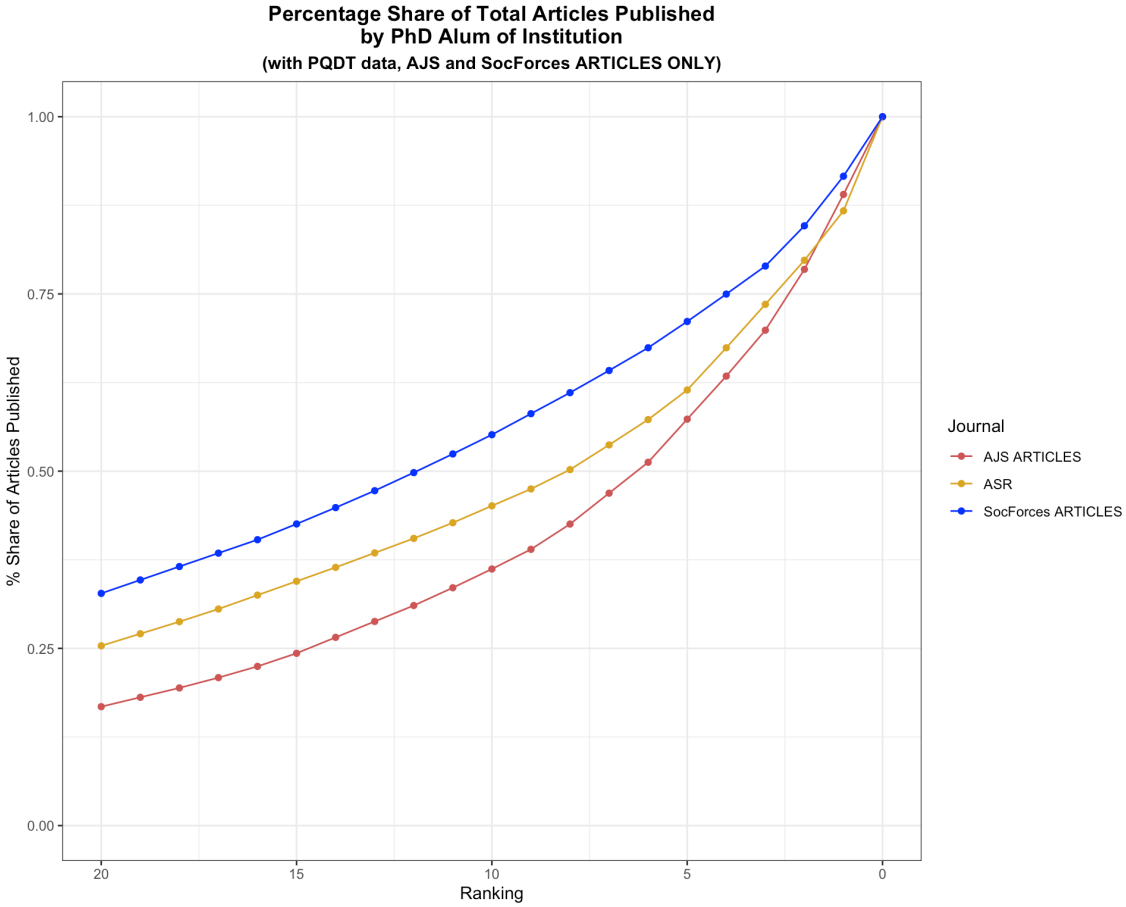


Figure 1: Lorenz curves of publication count by journal
Prestigious PhD institutions publish an outsize proportion of the total papers in sociology's top 3 journals. The top 20 institutions by publication in each of the 3 journals author roughly 75 percent of the papers published in each journal. These patterns vary slightly by journal. The AJS is the least equal, the ASR is slightly more equal, and Social Forces is slightly more equal still.

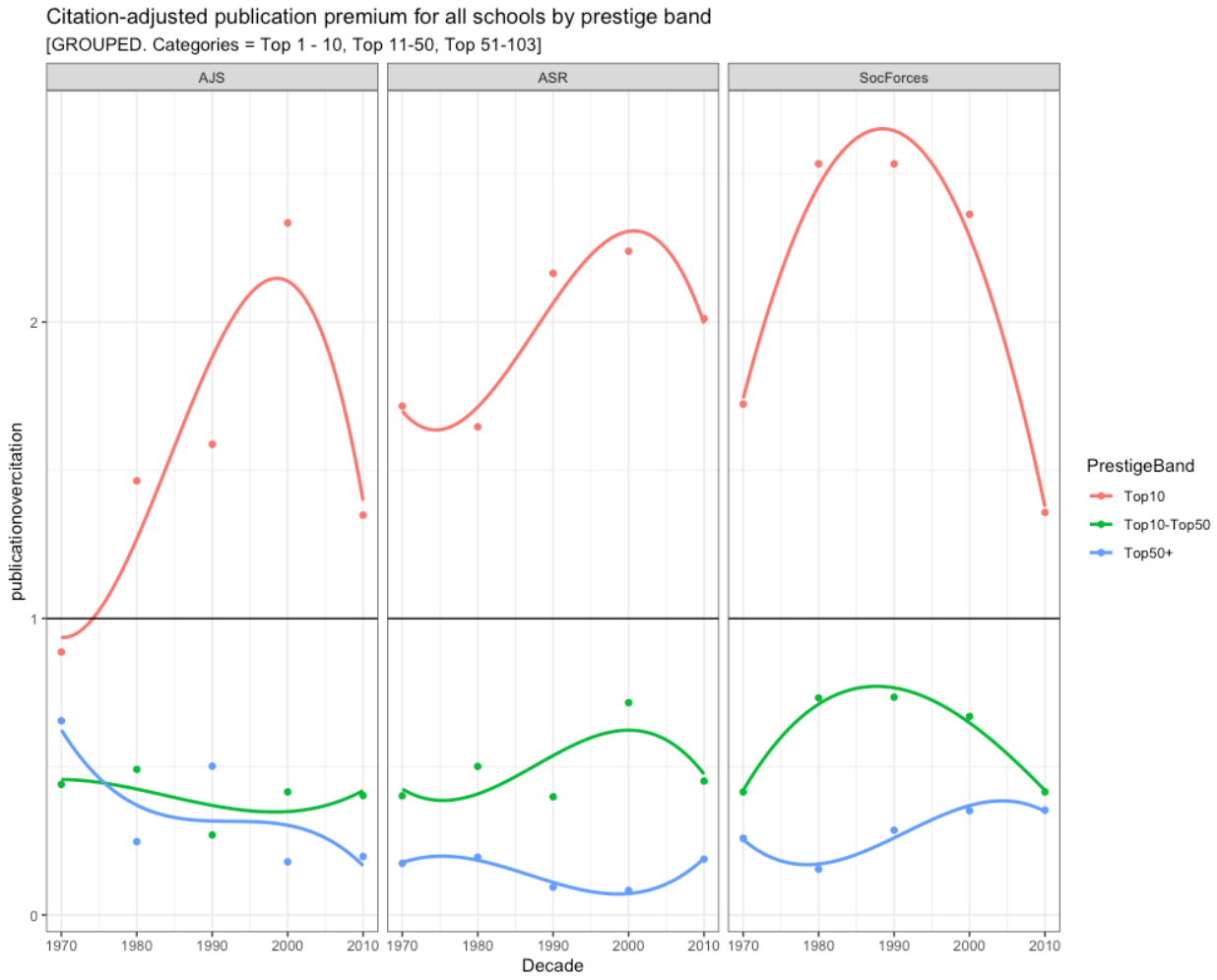


Figure 2: Prestige bias in sociology's top journals

We segment PhD origins into 3 buckets: top 10, top 11-50, and top 51 and beyond. With our original metric, the citation-adjusted publication premium, we find that papers from authors with prestigious origins are over-published but undercited in all of sociology's top 3 journals. The bias towards authors from less-prestigious PhDs is mostly constant over time, and the premium accorded to top PhDs improves over time but remains pronounced. The ASR is the most prestige-biased journal and discriminates the most against authors from less-prestigious PhDs.

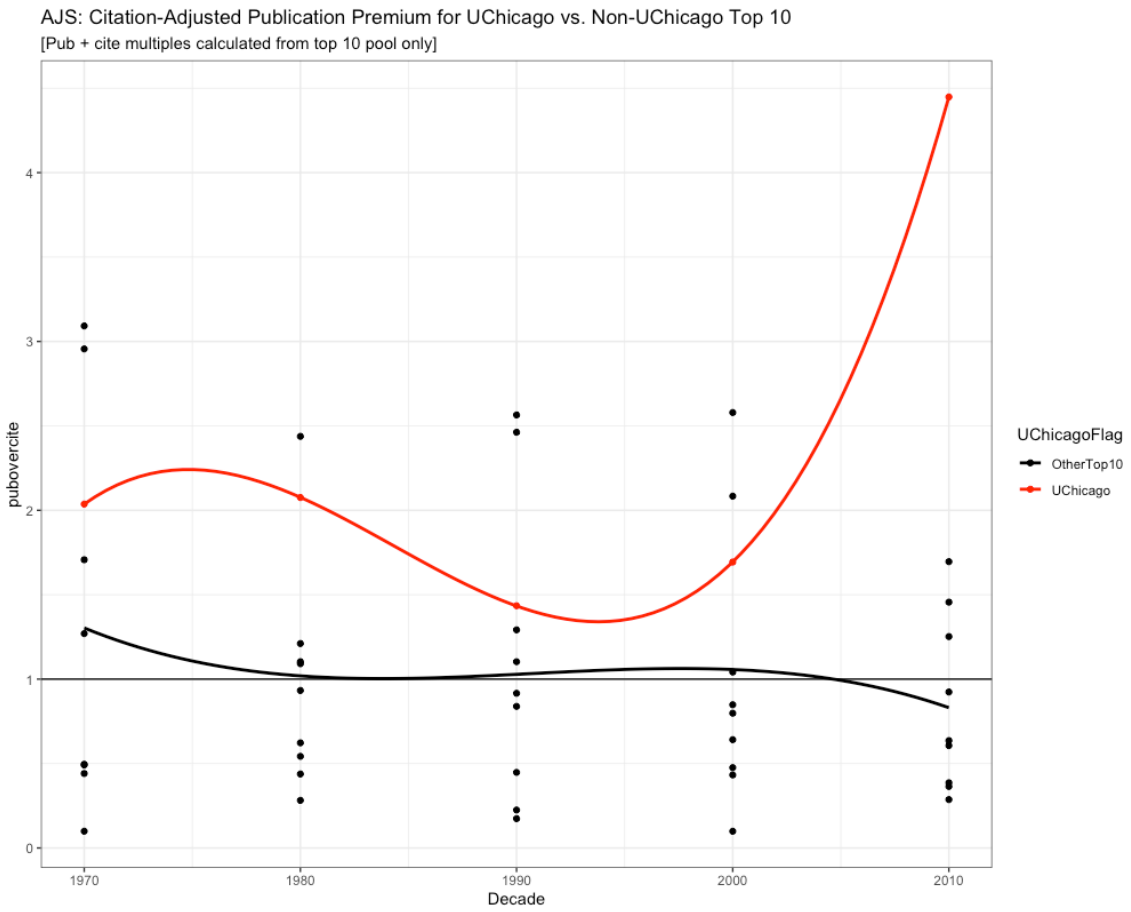


Figure 3: House bias in the AJS

We focus on the American Journal of Sociology in isolation. To uncover favoritism that the editors of the AJS may show to UChicago PhD alum, we compare sociology's top 10 programs by publication count in the top 3 journals to one another. We see that AJS alumnae are over-published but under-cited in the AJS far more than the alumnae of any other top sociology program. We also see that this favoritism increases greatly after the year 2000 into the 2010s.

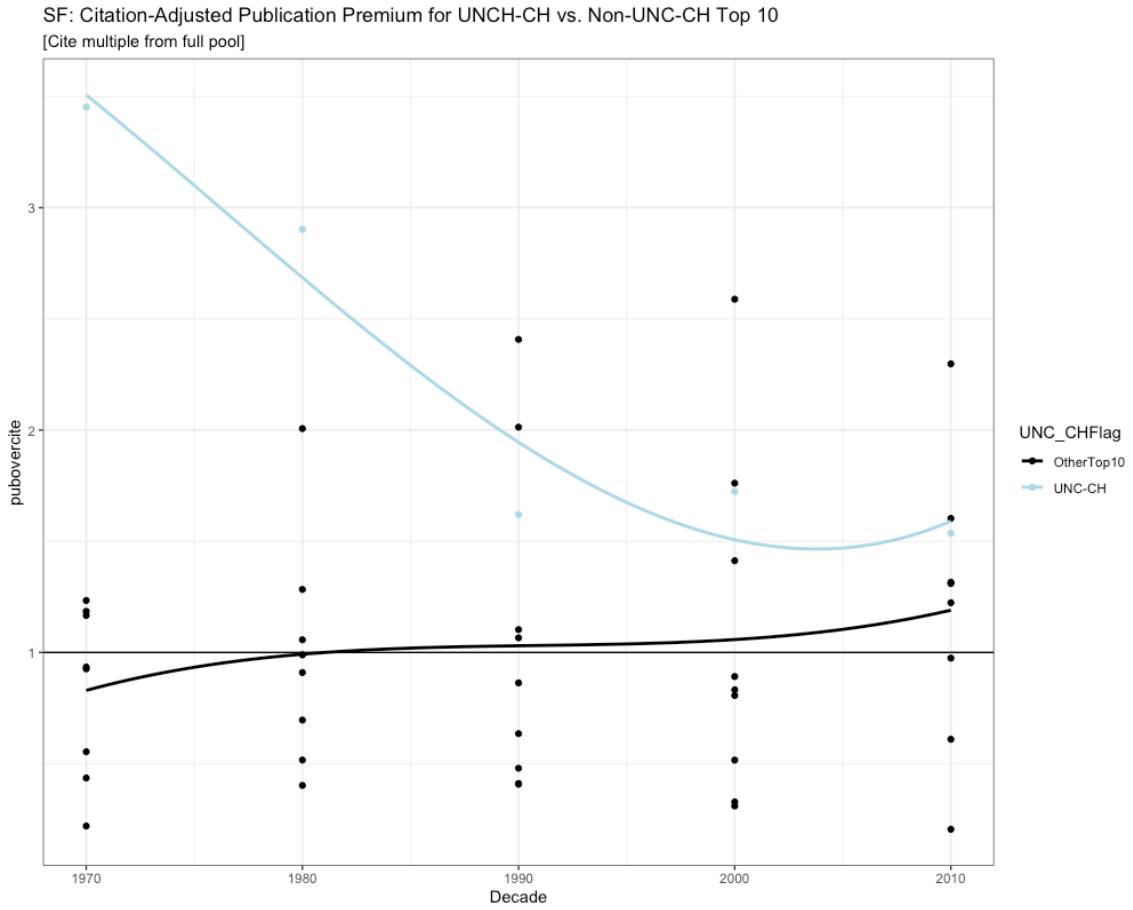


Figure 4: House bias in Social Forces

We focus on Social Forces, the other major house journal in sociology, in isolation. As with our analysis of the AJS and UChicago, we compare sociology's top 10 programs by publication count and search for any favoritism that may have been shown to UNC-CH alumnae. While alum of UNC-CH may have been over-published but under-cited in the past, any potential favoritism has greatly decreased over time.

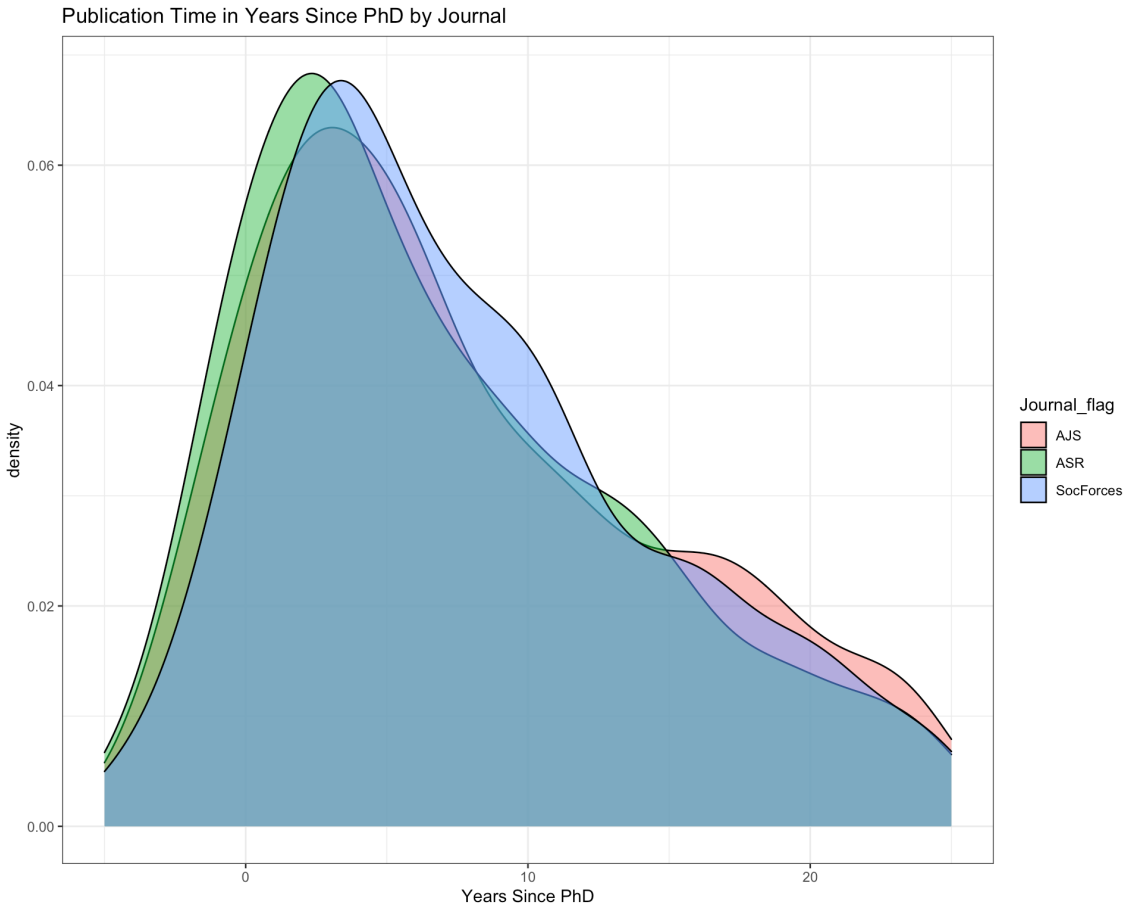


Figure 5: Age Structure of Publishing in Elite Sociology Journals
Sociologists publish in sociology's most elite journals, on average, within 10 years of obtaining their PhDs. This suggests that the main purpose of publishing in elite journals is not to advance sociological knowledge. Instead, as economists have found for economics, the main purpose of publishing in an elite sociology journal is likely to secure professional advancement.

SUPPLEMENTARY FIGURES

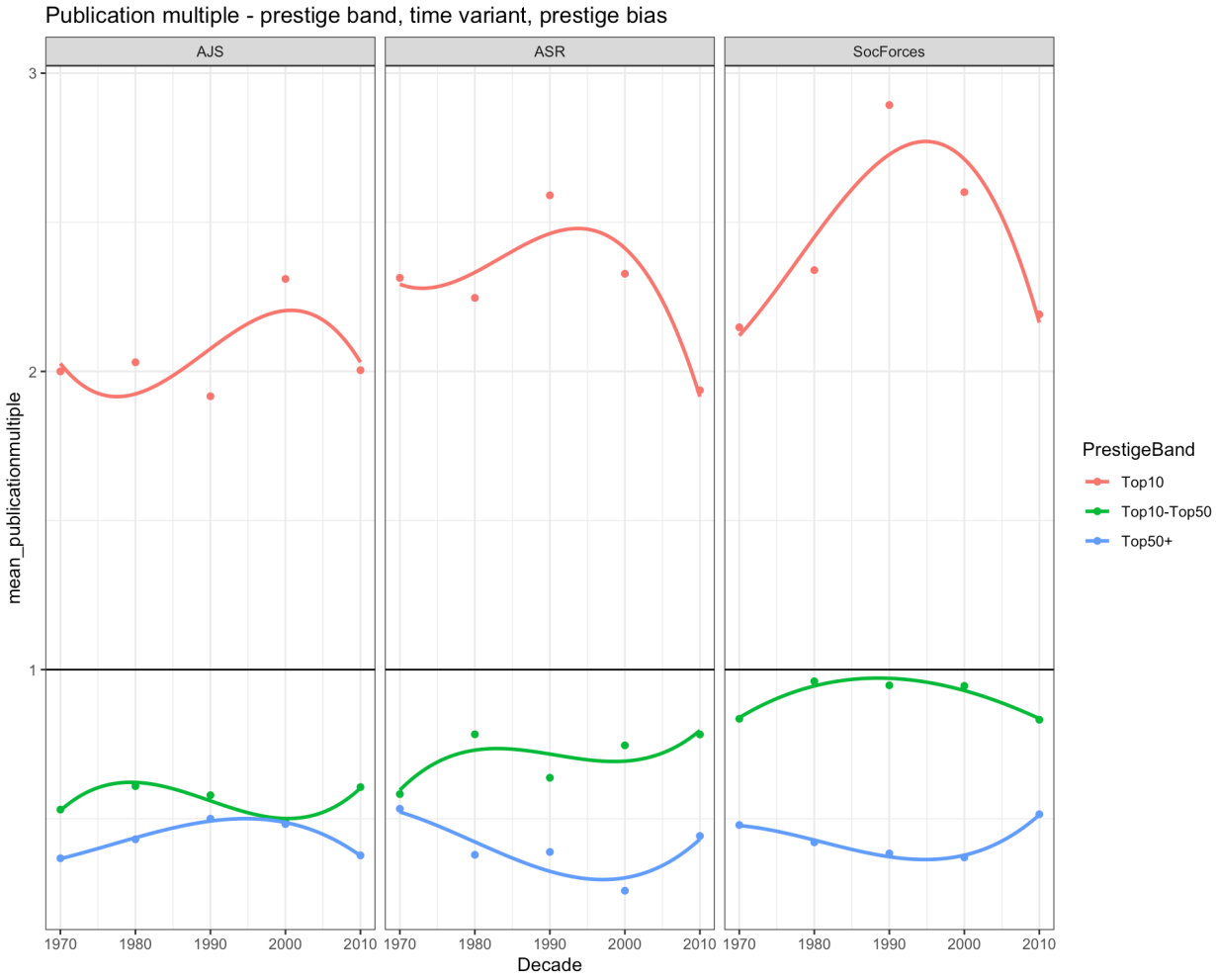


Figure S1: Publication multiple in isolation for prestige bias

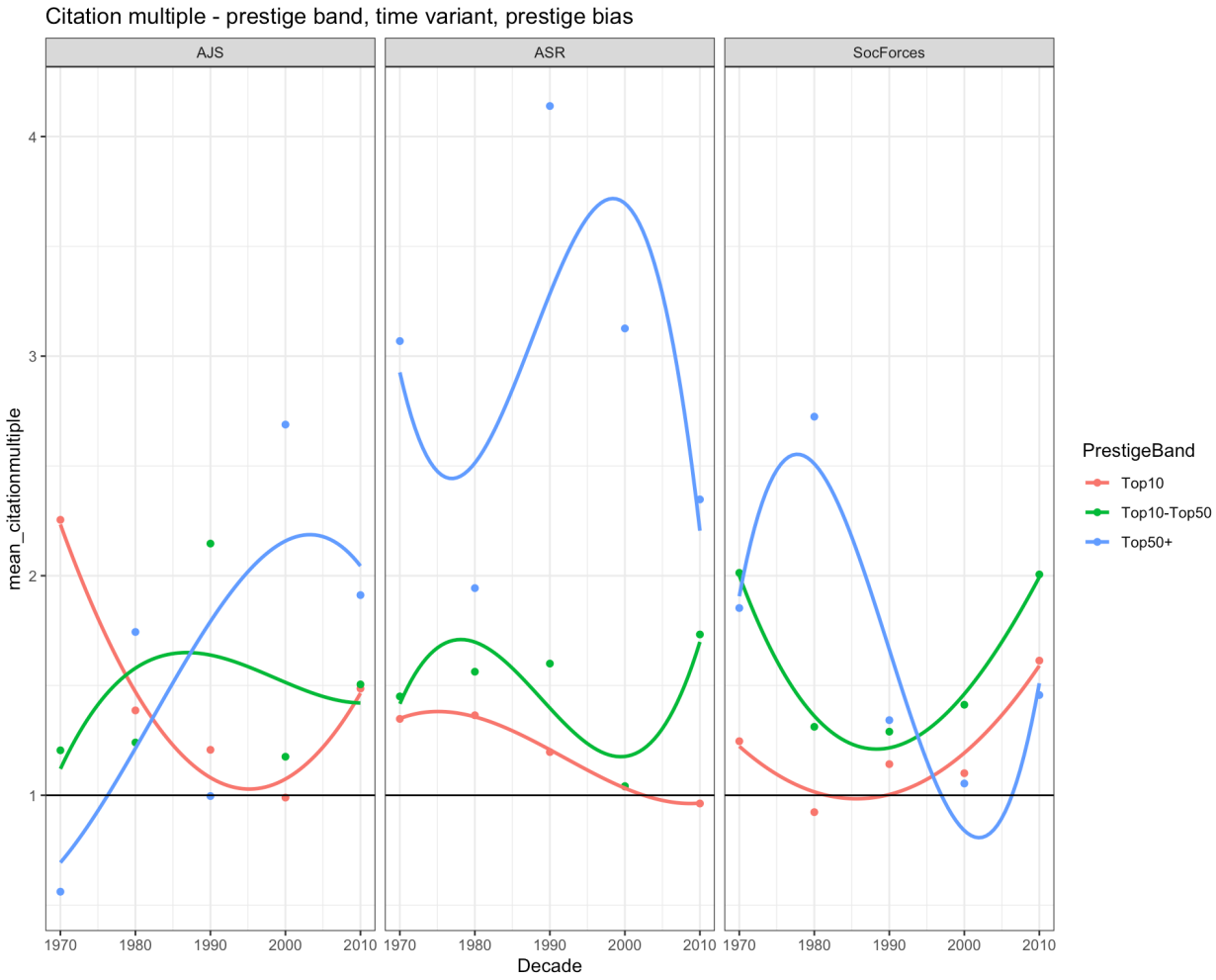


Figure S2: Citation multiple in isolation for prestige bias

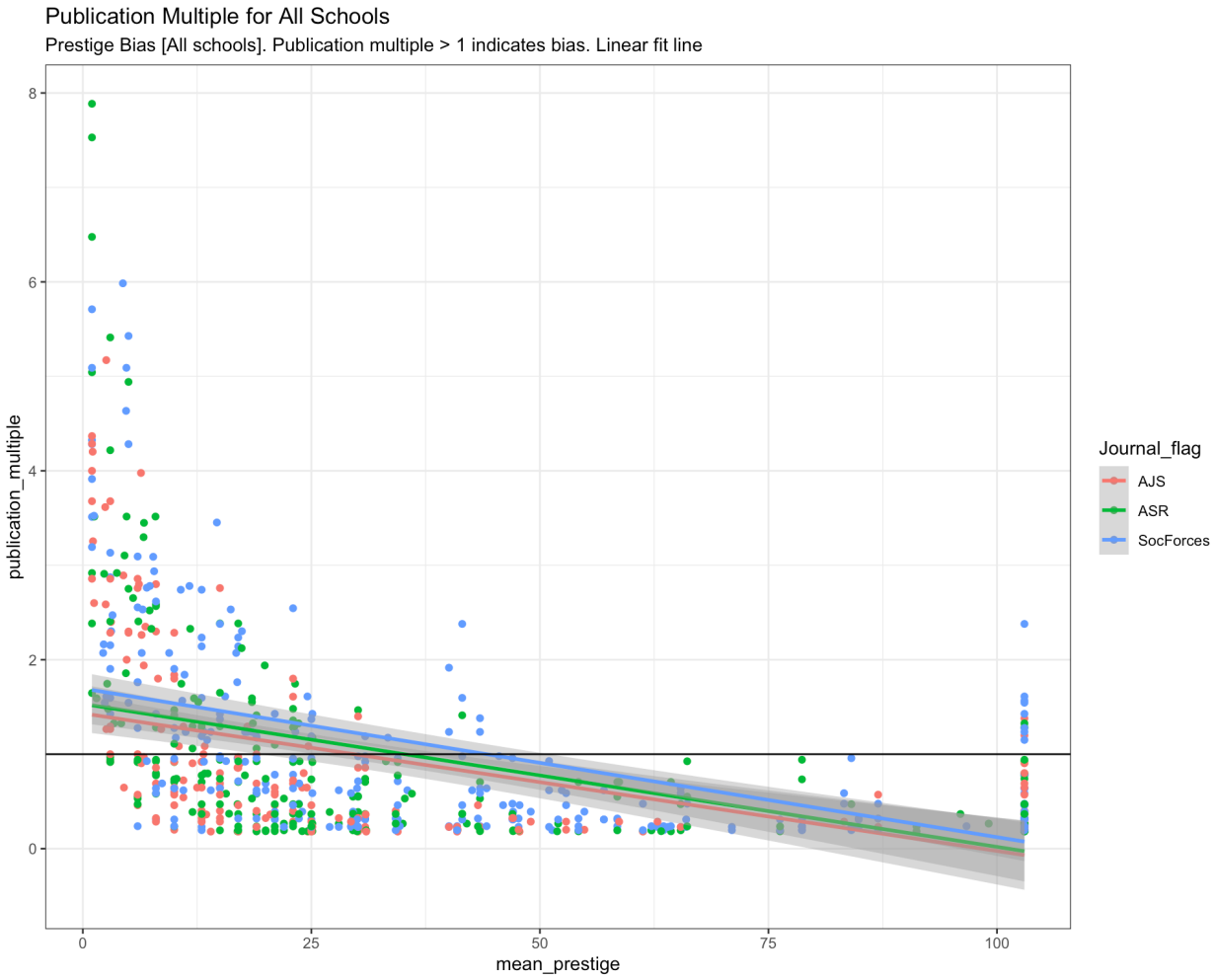


Figure S3: Publication multiple for prestige bias; time invariant

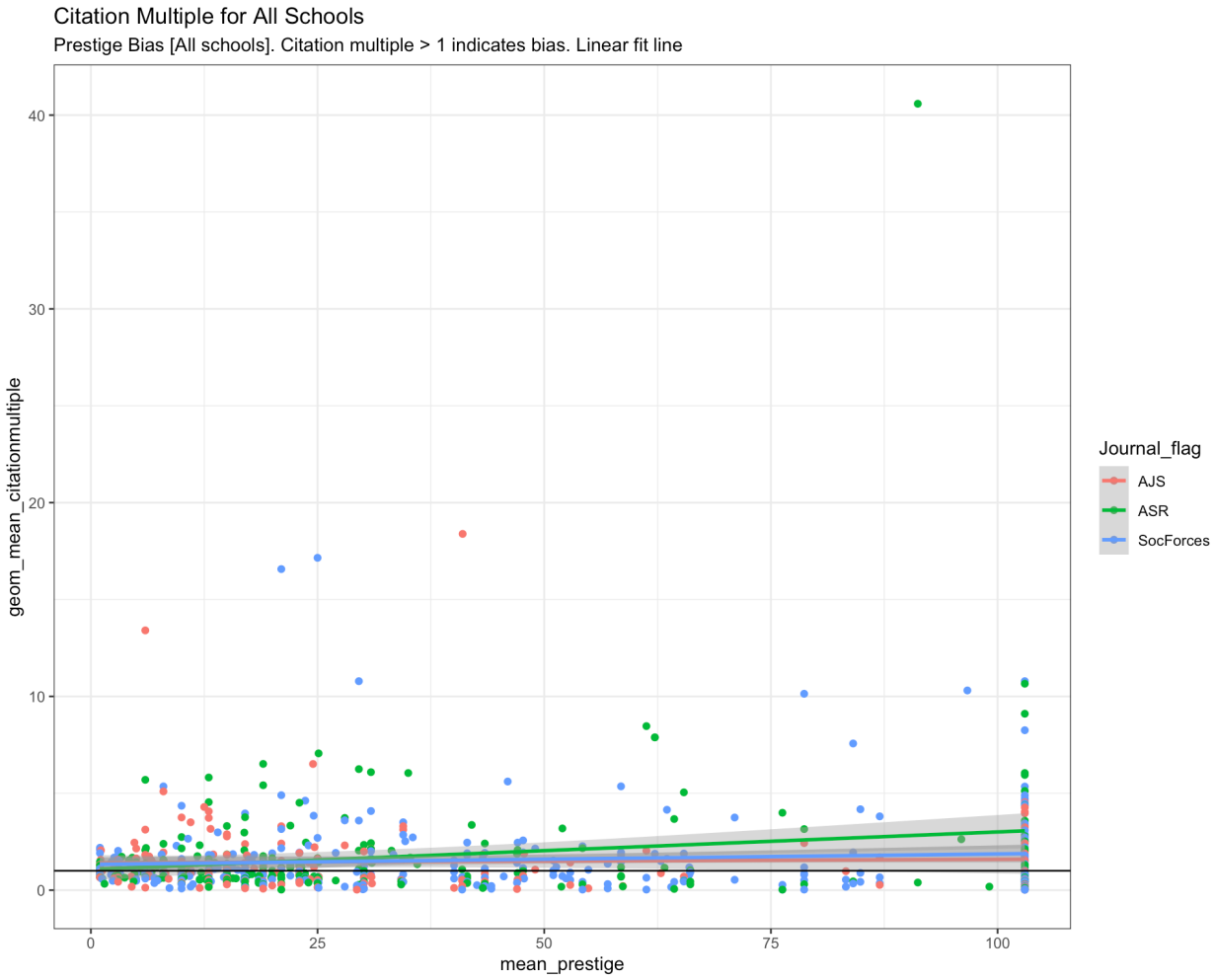


Figure S4: Citation multiple for prestige bias; time invariant

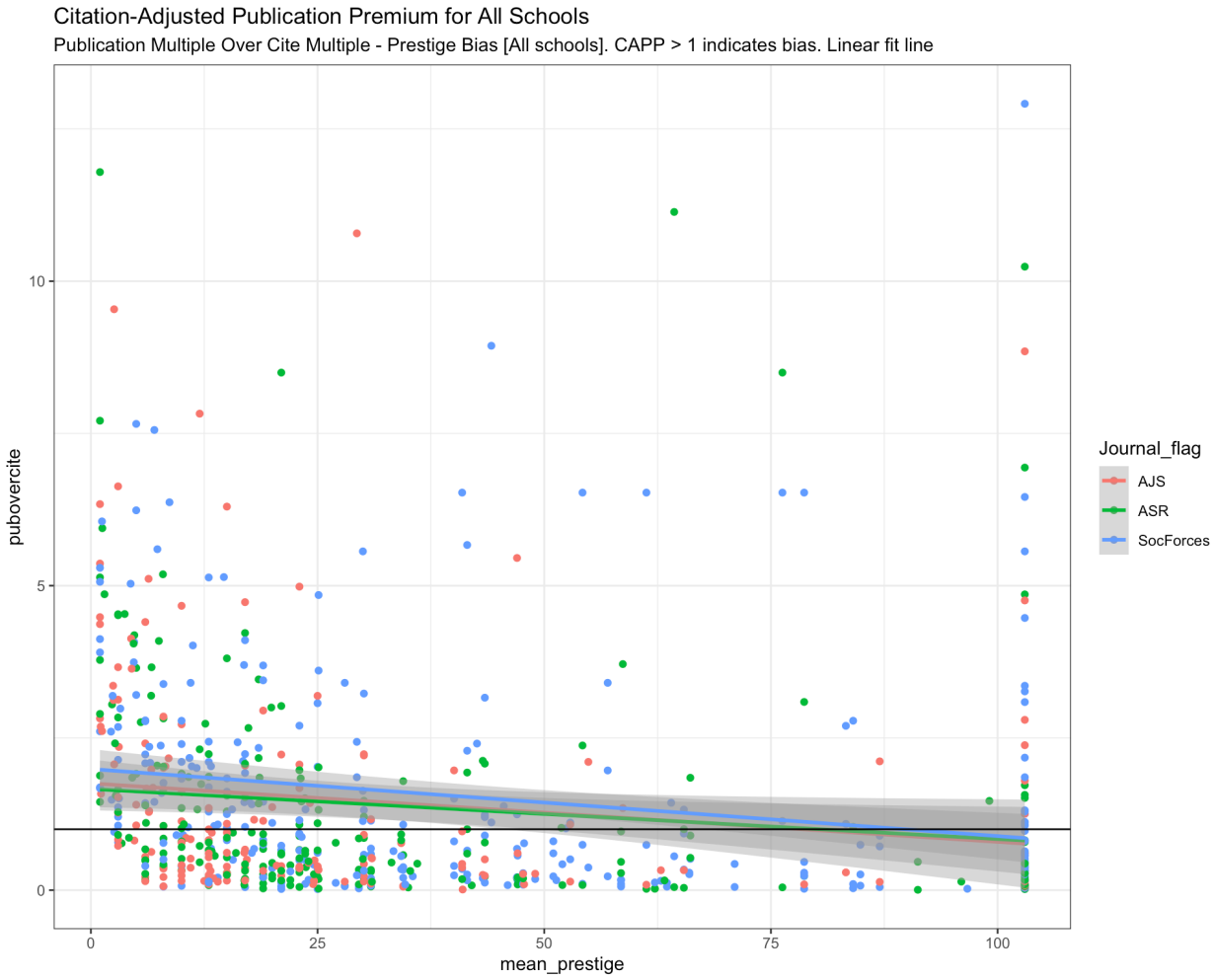


Figure S5: Citation-adjusted publication premium for prestige bias; time invariant

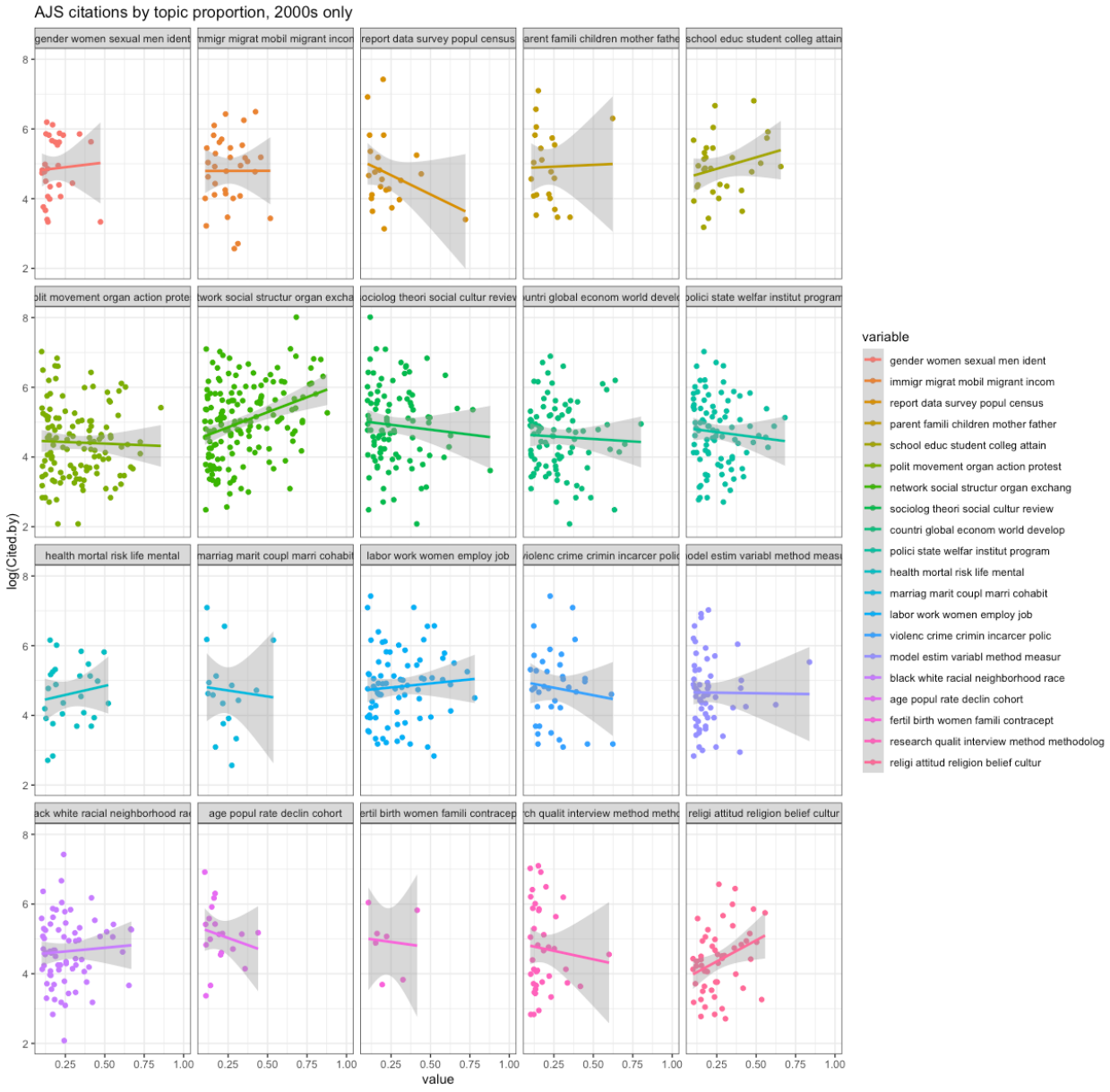


Figure S6: Incoming citations by topic proportion

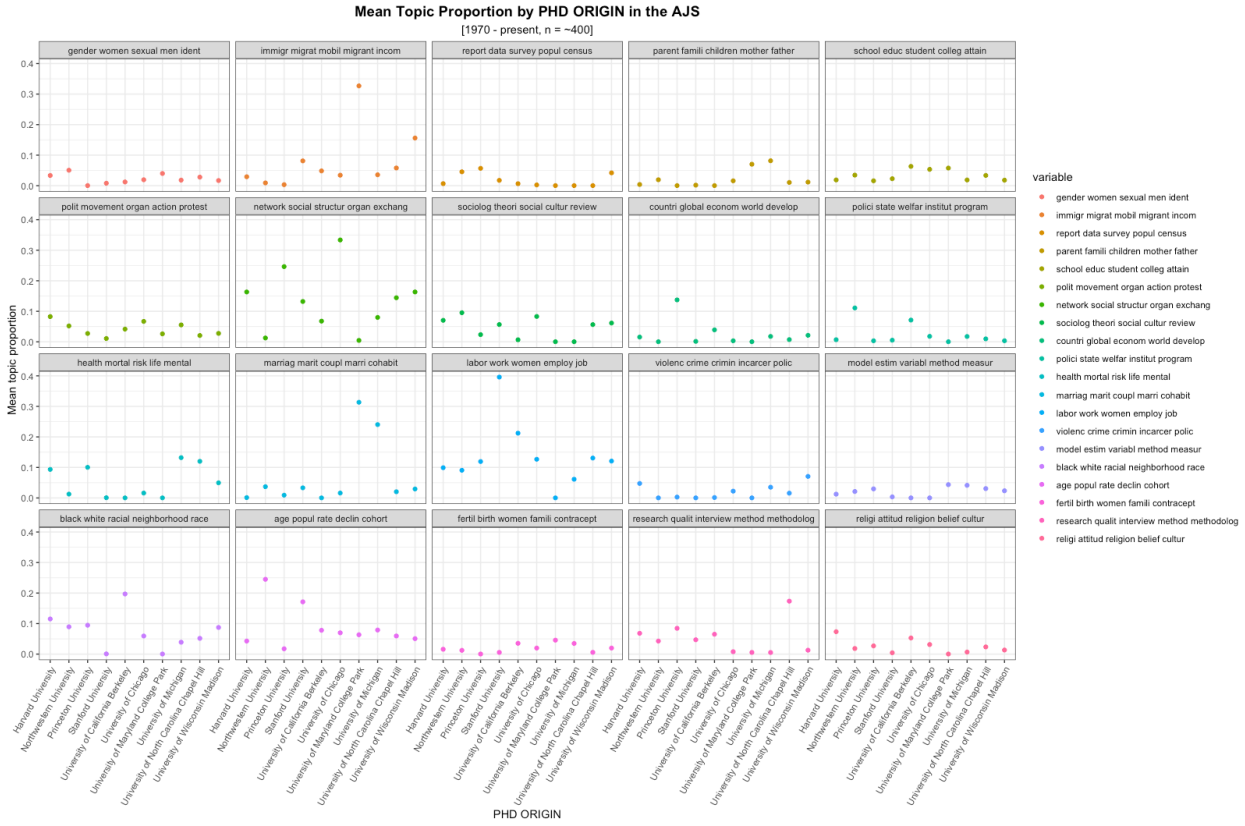


Figure S7: Mean topic proportion by PhD origin

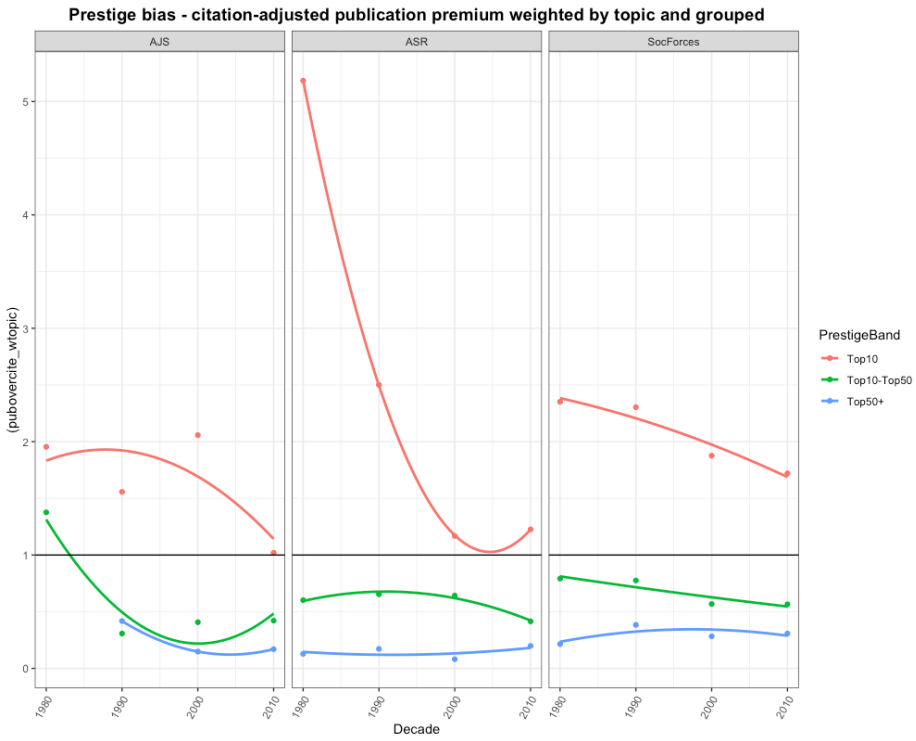


Figure S8: Prestige bias weighted by topic

A replication of Figure 3 with a topic-specific citation multiple and publication multiple. This suggests that our work is robust to PhD alum from particular departments specializing preferentially in different topics.

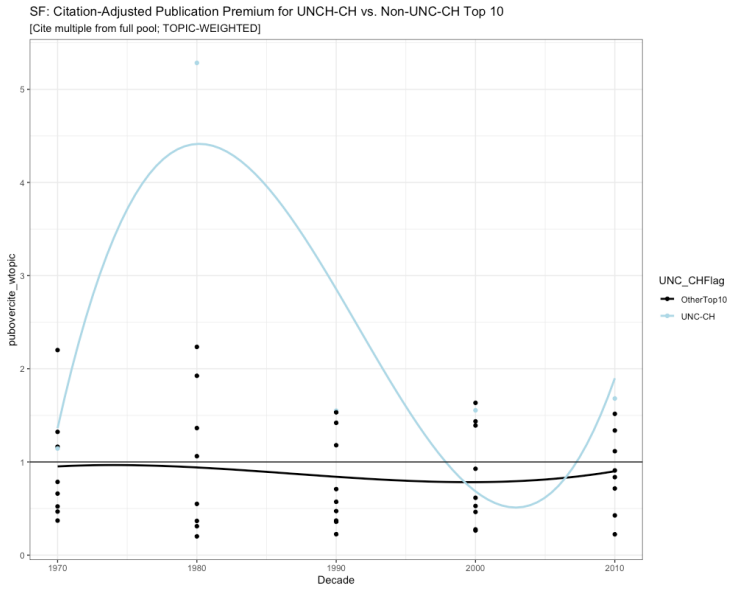


Figure S9: House bias in SF weighted by topic

A replication of Figure 4 with a topic-specific citation multiple and publication multiple. This suggests that our work is robust to PhD alum from particular departments specializing preferentially in different topics.

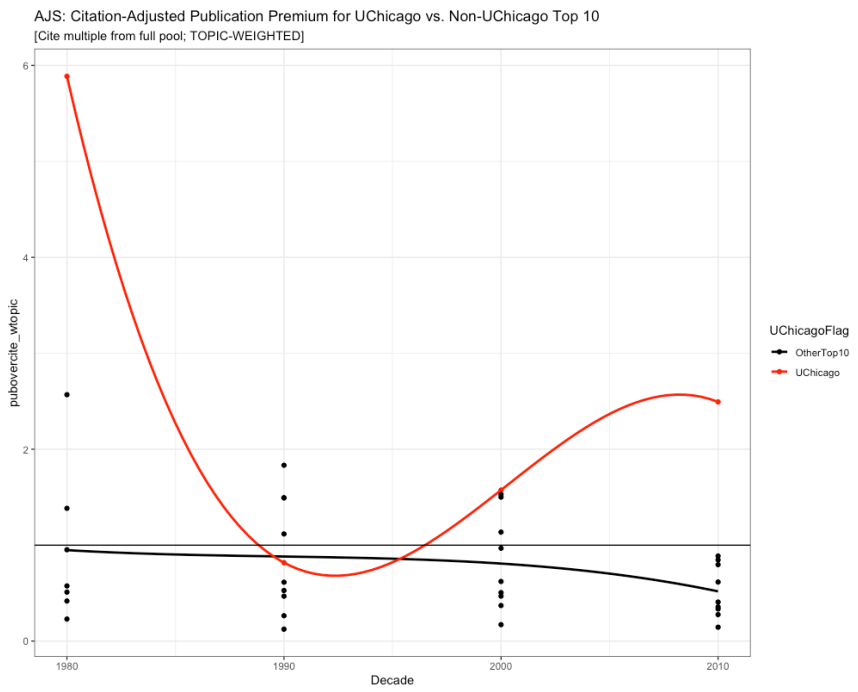


Figure S10: House bias in the AJS weighted by topic

A replication of Figure 5 with a topic-specific citation multiple and publication multiple. This suggests that our work is robust to PhD alum from particular departments specializing preferentially in different topics.

Bimodal Bias against Chinese Scientists in the American Academy: Penalties for Men, Bonuses for Women

ABSTRACT

Chinese scientists working in the United States have contributed greatly to their institutions and the scientific enterprise in general, but many Chinese scientists allege that they are the victims of prejudice. Given rampant anti-Asian discrimination in other realms of the American life, notably the relative paucity of East Asians in leadership positions despite the preponderance of East Asians in entry- and mid-level roles, and the recent targeting of Chinese scientists, we investigate whether these claims of discrimination have merit. We first show that female Chinese scientists comprise a much higher percentage of the female professoriate than the equivalent figure than do male Chinese scientists the male professoriate. Using an exact matching approach, we then find that male Chinese scientists suffer from a citation penalty but that female Chinese scientists enjoy a persistent citation bonus. Female Chinese scientists garner fewer citations on average than non-Chinese women of equivalent standing where male Chinese scientists require more citations than their non-Chinese counterparts. Our findings suggest that bias is best understood through an interactive lens.

INTRODUCTION

The first incarnation of what would one day evolve into modern science emerged in Western Europe following The Enlightenment. Many have argued that science was one of the ultimate products of a 1000-year process that began with the Catholic church banning consanguineous marriage, led to the weakening of clan-based institutions, and ultimately resulted in the psychological transformation of Europe into a continent of individuals unmoored from extended families (Merton 1938, Henrich 2021). While the particular lineage of the science that is practiced around the world does not have its origins in East Asia, East Asian cultures have long nurtured traditions of detailed instrumentation and empirically-oriented observation of the natural world that many have referred to as either protoscience or simply science (Needham and Wang 1988). The strong presence of an autochthonous variant of scientific or prescientific reasoning made the cultures of East Asia fertile soil for the transplantation of Western science. As a single glance at the economies of technologically advanced economics of East Asia shows, European science was indeed transplanted to East Asia, and it has obviously flourished in the region, though neither science's transplantation nor eventual flourishing happened immediately. At first contact, both the fruits and techniques of science were violently shunned by elites in imperial China and imperial Japan, but they were then enthusiastically adopted hundreds of years later by new generations of the elite castes of the fledgling nation-states of both cultures beginning with post-imperial Japan and followed soon by nationalist China (Elman 2005). The nations of East Asia are now global leaders in industrial and academic science.

While the fruits of science are often attributed to the nations to which scientists trace their descent and can confer great prestige on a nation, as was the case in the 'space race' of the 1960s between the United States and the Soviet Union (Hardesty, Eisman, and Khrushchev 2007),

nation states by themselves cannot perform the daily labor of science. The frontlines of science are manned by scientists, who are necessarily human, and scientists of East Asian descent contribute greatly to knowledge production both in East Asia and abroad (Mary Paul 2022). The influence of East Asian scientific migrants is particularly strong in the United States. A brief glance at American luminaries in the natural sciences shows this to be the case: Fei-Fei Li, a heavyweight in artificial intelligence whose work has garnered more than 250,000 citations; MIT's Feng Zhang, one of the pioneers of CRISPR gene editing technology (Cong et al. 2013); and Michio Kaku, a quantum physicist and scientific communicator. There are, however, far fewer East Asian household names in the upper echelons of science than one would expect from the remarkable preponderance of East Asians in the lower and middle rungs of the scientific hierarchy. It is a fact that the conduct of science, particularly the natural sciences, in the United States is either reliant or partially dependent on the labor of East Asians. A remarkable 88,512 Chinese students earned their PhDs in the United States from 2001 to 2020, comprising almost a quarter of all PhDs earned by international students, and many of the other top countries of origin for American PhD student are also in East Asia and include South Korea (25,994 students) and Taiwan (12,648 students) (National Science Foundation 2022). While there is no ready-made list of the scientists that form the commanding heights of science, the lack of East Asian household names in science mirrors the under-representations of East Asians in leadership positions in corporate America (Lu 2020).

While the situation of East Asian scientists in the United States is not sanguine, that of Chinese scientists is particularly dire. This is partially because Chinese Americans are tied with Japanese Americans for the longest history of continuous residence in the United States of any Asian American group, and there has been ample time for painful stereotypes about the Chinese

to emerge in America and for violence to be done to Chinese America (Lew-Williams 2018). Chinese scientists are surely impacted by negative stereotypes of the Chinese as, for example, dishonest and robotic (Bui 2022). More recently, however, geopolitical tensions between the United States and China have unfortunately hampered the careers of both Chinese and Chinese American scientists in the United States. The Trump Administration's China Project has had terrible consequences for Chinese scientists in the US. The China Project and other initiatives have uncovered many cases of proven subterfuge, including but not limited to the non-Chinese Charles Lieber, who was convicted for violating the terms of NIH and DOD contracts by secretly accepting funds from the Chinese Thousand Talents Program (Office of Public Affairs 2020); Zheng Zaosong, who was arrested at Boston's Logan airport with 21 vials of biological material en route to China (Office of Public Affairs 2020); and Linwei Leon Ding, who worked for Google and tried to illegally transfer code on artificial intelligence from Google to companies in China (Office of Public Affairs 2024). Many Chinese scientists, both Chinese and non-Chinese, allege that the boundaries of what constitutes illegitimate versus legitimate exchange with China are overly capacious and that the China Project has created an atmosphere of fear (Mervis 2022; Lee and Li 2023; Xie et al. 2023). Both Chinese and non-Chinese scientists mourn the painful consequences for Chinese scientists who were falsely accused of wrongdoing and forced to spend years clearing their names, such as Feng Franklin Tao and Gang Chen, and also for the Chinese scientists who may have done no wrong but live under the threat of investigation and the specter of anti-Chinese sentiment (Mervis 2022; Mervis 2023; Mervis 2024).

While it would be convenient to pin the blame for the anti-Chinese sentiment in American science on the Trump administration and right-leaning Americans, there are many other initiatives beyond the China Project that similarly create barriers, psychic or bureaucratic

or otherwise, to American professors working with Chinese nationals. MIT in particular has adopted an official stance of wariness toward collaboration with certain institutions in China. MIT's China Strategy Group[21] has recommended that the university not accept visitors or postdoctoral fellows who are affiliated with the seven military-adjacent universities that are colloquially the 'Seven Sons of National Defense' (国防七子, *guo fang qi zi*) and has cautioned faculty against participating "in 'talent recruitment' [sic] programs that are designed to transfer technology to China," such as the "thousand talents" program (MIT China Strategy Group 2022). While MIT places additional scrutiny on any collaborations with China, Saudia Arabia, and Russia (MIT Global Support Resources, n.d.), only China has sparked the convening of a country-specific strategy group. The predictable result of these policies is that research exchange between the US and China has slowed since 2019, and many Chinese scientists have even left the United States (Zhu et al. 2021; Quinn 2023). Regardless of whether one thinks the increased scrutiny on Chinese scientists is justified or not, that this scrutiny exists at all creates an urgent need for foundational research on the plight of Chinese scientists in the United States.

As Chinese scientists in general suffer under anti-Chinese discrimination, so do female scientists suffer under various forms of gendered discrimination. Many researchers in race and gender have adopted an 'intersectional' framework that posits that race and gender are independent vectors of oppression that either add or multiply in the 'multiply-burdened' to produce far more dire outcomes than either vector in isolation (Crenshaw 1989). Chinese women are formally considered 'women of color' by many, and this framework would lead us to assume that Chinese women suffer from more discrimination than do Chinese men. Because the

[21] The group was chaired by a high-ranking provost of international affairs and likely has considerably influence. Its affiliation with the MIT name and official MIT insignia alone conveys significant visibility and prestige.

predictions from intersectionality and similar theories of discrimination have not been tested in the context of Chinese American scientists, we will additionally investigate how anti-Chinese bias varies with gender.

To understand the presence and potential extent of bias against Chinese scientists in the United States by gender, we rely upon a unique dataset composed of bespoke and found data. We first collated a proprietary database of the CVs of all professors in the top 100 departments in the United States in four fields: sociology, economics, physics, and chemistry. This database includes information about the Chineseness and gender of every professor it covers. We then link this database to Microsoft Academic Graph, a large-scale bibliometric database with extensive coverage of both authors and publications (Sinha et al. 2015). We begin by providing summary statistics of Chinese scientists in our four fields of interest across three tiers of institutional prestige as defined by positioning on the US News and World Report (hereafter USNWR) rankings: High (top 1-15), Medium (top 15 - 50), and Low (top 50 - 100). Following many decades of sociological inquiry on inequality and stratification, we aim to detect bias by investigating how the relationship between performance and compensation varies across race in the American academy. As have many other scholars since the founding of scientometrics (or what is now called 'the science of science') as a distinct field (Fortunato et al. 2018), we use citation counts as a flawed but generally reliable proxy for article quality. Overall, we find that there is a Chinese penalty for Chinese men but a Chinese bonus for Chinese women. This bonus emerges both in the distribution of placements and in the requirements necessary to attain a placement. If we quantify the requirements necessary to attain a placement as the gender-, cohort-specific, and university ranking-specific citation count necessary to attain a job, we find female Chinese scholars enjoy a citation advantage relative to non-Chinese women where

Chinese men suffer from a citation disadvantage relative to non-Chinese men. Our findings are at odds with predominant theories of stratification and disadvantage, including intersectionality theory, and do not lend support to any popular paradigms of inequality.

LITERATURE REVIEW

ASIAN AMERICANS AND RACE IN AMERICA

The unique racial context of the United States provides the context for our investigations into bias against Chinese scientists. There are 5 official racial categories on the United States Census: White, Black, Native American/Alaska Native, Asian, and Native American/Other Pacific Islander. There is also the non-racial ethnic category of Hispanic. The interracial relations of most groups in this hierarchy are summarized clearly by theories of racial inequality in the United States, which argue that America is marred by an ugly history of 'systemic racism' and that there is a putative racial hierarchy in America with Whites at the top and African Americans at the bottom. Many volumes have been authored on this topic (Kluegel 1990; Paige and Witty 2010; Rowley and Wright 2011; Gans 2012; Misir 2022; Banaji, Fiske, and Massey 2022). Though their plights are discussed less often, the pre-Columbian denizens of North America are assumed to join African Americans at the bottoms of this hierarchy, and the relatively admixed progeny of Iberian, African, and pre-Columbian Central and South Americans who are collectively called Hispanic Americans occupy a position close to the bottom (Massey 2014; Michener and Brower 2020).

This is not, however, an open and shut case. These categories are far from perfect, and the Hispanic category is an excellent case of why this is so. Hispanic America cannot be easily

slotted into the America's presumed White-Black racial system because 'Hispanics' are an admixed population with a broad range of skin tones (Perreira and Telles 2014), and even the Hispanic groups that are sometimes considered White-adjacent, notably Cuban-Americans, present with a wide array of skin tones (Portes 1984). Recent work has shown that the advent of Latinx-Americans has perturbed long-standing dynamics between White and Black America (Abascal 2015, 2020). Scholars of ethnicity contend that the 'Hispanic' label is less a reflection of demographic reality than a resultant of the political jousting between activists, businessmen, and unelected administrators (Mora 2014).

If Hispanic America's position in the American racial hierarchy is complicated, that of Asian-Americans is yet more complex and additionally precarious. Their position is complex is because, for one, the category of 'Asian-American' is remarkably capacious. While it is unrealistic to expect or demand that census categories offer a realistic mapping of ethnic realities, the census category of AAPI (Asian American Pacific Islander) is in a league of its own. It includes almost every human east of the historic boundaries of Europe as demarcated by Bosphorus strait and the Ural mountains. This mega-region ranges from Turkey to Tuvalu and beyond to the Hawaiian archipelago, and it encompasses roughly 60 percent of the world's population. As one might expect, the groups that form the AAPI supergroup of almost 5 billion people have alternately enjoyed and suffered diverging destinies in the United States. The AAPI census category is a very big tent that includes the Chinese and Indian American communities, who are, on average, are at the top of America's income distribution and far wealthier on average than Whites, and the impoverished Cambodian and Hmong communities, who cluster at the bottom end of the income distribution (Sakamoto, Goyette, and Kim 2009). The position of Asian Americans in America's racial hierarchy is precarious, then, because many Asian

Americans are successful. Asian Americans are often called a 'model minority,' and scholars of Asian America have long discussed the impact and import of what they have termed the 'model minority myth' (Sakamoto, Goyette, and Kim 2009; Sakamoto, Takei, and Woo 2012; Lee and Zhou 2015).

There is a third and insidious reason that the census category of 'Asian' is so complicated: many Americans assume that is it is shorthand for 'East Asian' and, more specifically, 'Chinese' (Lee and Ramakrishnan 2020; Goh and McCue 2021). While the remarkable heterogeneity of Asian America means that the experiences of no single Asian American group can be seen as representative of other Asian American groups, Chinese Americans have a very long history in America, and the uniqueness of the Chinese American experience is at odds with their automatic elision as prototypically 'Asian' by non-Asian Americans. The Chinese Americans are the largest constituent of Asian Americans in the United States, and they are tied with Japanese Americans for duration of residence in the United States (Lew-Williams 2018). The combination of numbers and time has created many opportunities for American history to be unkind to the Chinese American community. The Chinese Exclusion Act is the most dramatic example of discrimination against the Chinese people, but it is far from the only one. The first Chinese immigrant to the United States arrived in California in the 1840s, and many other Chinese migrants followed suit. The first Chinese Americans worked in gold mines and then on railroads in back-breaking conditions (Lew-Williams 2018, Chang 2019). Most Chinese migrants to California were men, and they were seen as robotic, soulless, automaton-like workers who were pushed to inhuman extremes by their bosses (Bui 2022). The Chinese in San Francisco were maligned as sources of vice, including but not limited to prostitution, gambling, and opium, and diseases ranging from leprosy to syphilis (Trauner 1978). While the association of foreigners

with disease has occurred since time immemorial and may even have an evolutionary basis, the COVID-19 pandemic unfortunately showed America that the alignment of the Chinese people with disease in the anglophone world endures to the present (Cook, Huang, and Xie 2021). Many medical professionals worried that the ills of the Chinese interacted to form vicious cycles of vice; for example, it was a common fear that opium smoking would increase the sexual desires of White men and cause them to father miscegenated offspring with Chinese women (Ahmad 2000). Concerns over the influence of the Chinese on America led to the Chinese Exclusion Act of 1882. This anti-Chinese sentiment continued into the 20th century. Explicitly anti-Chinese tirades, including the infamous *Ways that are Dark* (Townsend 1933), continued to be favorably received in the United States into the 20th century.

INTER-ASIAN DIFFERENCES

Whenever Chinese Americans are compared to other Asian American groups, Chinese America's uniqueness is made immediate. It is easiest to begin by comparing Chinese Americans to the American diaspora communities of China's regional neighbors: Korean Americans, Japanese Americans, and potentially Vietnamese Americans. For a striking example of how China is often singled out as uniquely bad among other East Asian cultures, *Ways that are Dark*, which occupies a notorious place in history as perhaps the most Sinophobic book ever written, senselessly denigrated the Chinese people but heaped praise on the Japanese people (Townsend 1933). This anecdotally suggests that the anti-Chinese racism may be more intense and qualitatively different than anti-Asian racism, and many lines of empirical evidence buttress this conclusion. A 2007 survey in California of self-reported discrimination reports that a higher percentage of Chinese Americans than Japanese Americans reported experiencing discrimination

(Gee et al. 2009). During the COVID-19 pandemic, Trump infamously labeled COVID-19 the ‘China Virus,’ and anti-Chinese sentiment on Twitter, now simply X, spiked dramatically in the wake of COVID-19’s arrival on American shores (Huang, Cook, and Xie 2021). While the COVID-19 pandemic is not useful for directly comparing anti-Chinese sentiment with discrimination directed against other East Asian Americans, surveys of Chinese Americans after COVID-19 show consistent evidence of Chinese Americans self-reporting discrimination at higher rates than other East Asian groups (Hahm et al. 2021; McGarity-Palmer et al. 2024). Most relevantly, Chinese restaurants reported a sharper decline in patronage during the COVID-19 pandemic than other comparable restaurants, and this effect was larger in communities that voted for Trump in 2020 (Huang et al. 2023)

Wealth is another lens of comparison. We may contrast Chinese America with the only Asian American group with a higher average yearly income than Chinese Americans: Indian Americans. While, in the aggregate, Chinese Americans are wealthier on average by far than White Americans, Chinese Americans are underrepresented at the extremes of wealth, and there is a suspicious dearth of Chinese Americans in the highest rungs of power. Many call the mechanism that prevents Chinese Americans from rising to C-suite positions in corporate America the 'bamboo ceiling,' an appropriation of the 'glass ceiling' that is purported to hold back women in corporate America (Lu, Nisbet, and Morris 2020). The dearth of Chinese executives becomes most evident when Chinese Americans are compared Indian Americans, who famously dominate the C-suite leadership of America's flagship technological corporations. To name a few high-profile examples, the CEOS of Microsoft, Alphabet (the parent company of Google), Adobe, YouTube, and IBM are all ethnically Indian (FT Online 2023). There is little work comparing the behaviors and achievements of Chinese Americans and Indian Americans directly,

but there is a growing body of work that compares East Asians in America to South Asians in America.

Some explanations for the striking differences in leadership representation between East and South Asians in America include the ability of South Asians (and relative inability of East Asians) to share resources endogenously and to form connections preferentially with coethnic alters (Lu 2022). It is puzzling, however, that South Asians are rated as more ethnocentric than East Asians in some contexts (Yousaf et al. 2022). Both Lu (2022) and Yousaf et al. (2022) use an instrument called the Intercultural Willingness to Communicate (IWTC) to measure ethnocentrism, but where Lu found that East Asian students were less willing to communicate with non-East Asian students at elite American business and law schools, Yousaf et al. found that Pakistani college students in Pakistan were less willing to communicate interculturally than Chinese college students in China. Regardless of ethnocentricity, South Asians are consistently rated as more assertive than East Asians, which might explain their relative success over East Asians (Lu, Nisbet, and Morris 2020; Lu, Nisbet, and Morris 2022). There is a stark difference between the intensity of in-group cohesion and propensity to out-group networking, and the paucity of assertiveness among East Asians could partially explain why East Asians are not as skilled at networking with outgroup members as other minority groups. The placements of both groups on the Stereotype Content Model supports this: while Americans sort Indian Americans into the 'Ingroup/Allies cluster,' defined by high competence and high warmth, American slot East Asians into the stereotypically high competence but low warmth 'Competent but not Nice cluster' (Lee and Fiske 2006).

Despite its widespread usage, The Stereotype Content Model only captures 2 dimensions of stereotypes and fails to capture many important dimensions of intergroup conflict. By many

measures, South Asians suffer from more discrimination than East Asians. Loosely paralleling a growing body of work on colorism in the African American community (Monk 2015), some extant evidence suggests that darker-skinned Asian Americans suffer from a proportionally higher discriminatory burden than lighter-skinned Asian Americans (Ryabov 2016a, 2016b). As with the situation of Chinese Americans after COVID-19, many South Asian Americans were stereotyped as 'terrorists' in the wake of the September 11th attacks (Cainkar 2018). Americans additionally tend to prefer Chinese respondents over Indian respondents in classic tests of bias. Americans would generally prefer to live next to a Chinese family over an Indian family and would rather their children marry people of Chinese descent than Indian descent (Lu, Nisbet, and Norris 2020). Finally, a recent wave of extremely virulent anti-Indian sentiment has swept across the English-speaking internet with hotspots in the United States, the UK, and Canada. The intensity of the ongoing anti-Indian outburst is worse than the peaks of anti-Chinese sentiment that appeared on Twitter during the COVID-19 pandemic (Huang, Cook, and Xie 2021), and while it is recent enough that there is currently no academic work on it, the Indian press has covered it extensively (ET Online 2024; Jain 2024; TOI World Desk 2024). Despite this racial animus, Indian Americans have attained a level of success that is paradoxical when seen through the lens of 'colorism' and the American racial hierarchy. Taken together, this evidence implies that outright racial hatred is likely not responsible for the any putative bias against Chinese Americans. We must instead look to finer-grained sources of bias and prejudice.

Though most of the work on bias against Chinese Americans focuses on corporate America, we ask if this is true for the halls of the ivory tower as well as the offices of Fortune 500 companies. While corporate America and academic America are procedurally similar in that they offer cognitively demanding careers and proactively select highly educated talent, their end

goals are very different. Large companies seek to maximize shareholder value, and academic institutions seek to, depending on who you ask, produce new knowledge and/or maximize prestige. While there are many parallels between the two, we will leave the question of how the differing ends of corporate and academic America might affect the reception of Asian-Americans in each for future work. Because firms in many technologically focused industries frequently publish in academic forae (Chang 2014, Krieger et al. 2021, Camerani, Rotolo, and Grassano 2018), however, there may be more interoperability between work in the two realms than previously considered. Scholars at Google have published more than 10,000 papers since 1998 (Google Research 2024), and the most highly cited paper in artificial intelligence is a 2017 paper from researchers at Google describing transformers, one of the most foundational concepts of the field (Gibney 2024). Understanding bias against Chinese scientists in the academy may have additional applications to understanding bias against Chinese scientists in industry.

THE SUCCESS OF CHINESE AMERICA: CULTURE OR SELECTIVITY?

The origins of the remarkable successes of Chinese America are a matter of long-standing debate in the social sciences. Sociologists have offered two main explanations. The first is the 'hyper-selectivity' hypothesis (Lee and Zhou 2015), which argues that Asian-American migrants are often from high-status backgrounds and that this selection is the primary cause of the successes of Asian America. The evidence for this hypothesis is mixed. The Chinese migrants to California in the 1800s were generally from the lower classes, and, despite their meager backgrounds, Chinese and Japanese Americans had equaled or exceeded the educational

attainment of White Americans by 1930[22] (Hirschman and Wong 1986; Tian 2023). Asian Americans additionally reached economic equality with White Americans by the 1970s (Hirschman and Wong 1984). The second explanation may be termed the cultural hypothesis or the overachievement hypothesis. It posits that Asian-American success in education is due to non-cognitive factors, particularly a culture-wide emphasis on academic success (Hsin and Xie 2014). Many Asian-American cultures also link effort and achievement very explicitly (Hsin and Xie 2014). Other cultures in the United States do not do so as explicitly, and some scholars have suggested that the many cultures of African-America view racism as blocking the link between academic effort and academic success and instead attribute racial achievement gaps to structural factors (Bañales et al. 2020). It is one thing if a child's parents reward the child in the family for academic achievement and another if the child's broader community rewards the child as well; in the case of Chinese Americans, both are true (Fuligni 2001; Lee and Zhou 2015). Present in both frameworks is the idea that Asian Americans may feel a need, whether rationally or irrationally, to out-compete their non-Asian alters in the job market due to perceived discrimination. As Asian-American families spend more on education than White parents (Tian 2023), so may Asian Americans feel a need to acquire advanced educational credentials to compete on a job market in which they are both "presumed competent" and the targets of discrimination (Lee et al. 2024).

[22] It merits mentioning that Hirschman and Wong, who were staunch opponents of the cultural hypothesis, found that Chinese Americans were doing better than most White Americans on some measures of academic success in the 1930s, a full thirty years before the beginning of mass migration and fifty years after the Chinese Exclusion Act.

RACE, SEX, AND INTERSECTIONALITY

Gender and racial stereotypes do not exist in isolation. Many racial stereotypes are implicitly gendered, and people of African ancestry are seen as more modally masculine than Whites where those of East Asian ancestry are seen as more feminine on average than Whites (Galinksy, Hall, and Cuddy 2013). The most famous framework for exploring the intersection of race and gender is fittingly termed intersectionality. First outlined by Kimberle Crenshaw, the framework argues that non-White women are "multiply-burdened" and urges scholars to "contrast the multidimenstionality of Black women's experience with the single-axis analysis" of White women and Black men (Crenshaw 1989, 139). The verbiage Crenshaw employs brings to mind linear algebraic framings of matrices and vectors and implies that racism and sexism are independent vectors of oppression. Crenshaw adds that "the intersectional experience is greater than the sum of racism and sexism," and while this means that racism and sexism cannot be summed directly as 'racism + sexism', it implies that their relationship is fundamentally additive (Crenshaw 1989, 140). One way of interpreting this is that overall oppression burden of racism and sexism is the sum of racism and sexism plus an additional factor or the product of race multiplied by sexism. The full explication of Crenshaw's framing of is: "To bring this back to a non-metaphorical level, I am suggesting that Black women can experience discrimination in ways that are both similar to and different from those experienced by white women and Black men. Black women sometimes experience discrimination in ways similar to white women's experiences; sometimes they share very similar experiences with Black men. Yet often they experience double-discrimination - the combined effects of practices which discriminate on the basis of race, and on the basis of sex. And sometimes, they experience discrimination as Black women - not the sum of race and sex discrimination, but as Black women" (Crenshaw 1989,

149). Whether the oppression that Black women is experience is an additive blend of their experiences as women and non-White or a wholly unique third option, the end result is that Black women are ultimately more oppressed than White women or Black men. Others have operationalized this approach along these lines and have argued either for the uniqueness of the Black or non-White female experience (Ghavami and Peplau 2013; Billups et al. 2022) or that racism and sexism can be summed (Pogrebna et al. 2024). Some explicitly argue for an "additive model" (Liu and Wong 2018) of discrimination or a "double jeopardy" or "double disadvantage" hypothesis (Beal 2008; Denise 2012; Denise 2014). All contend that Black women experience far more oppression than White women or Black men individually. Crenshaw does not mention the plight of Asian or Hispanic Americans of any stripe, but others have applied Crenshaw's work to these groups (Ghavami and Peplau 2013; Rosette et al. 2016).

A weakness of the literature on the diverging destines of Asian-American groups is that it generally does not differentiate between men and women. Given the wide variance in stereotypes attributed to Asian American men and Asian American women, it would stand to reason that American culture would receive and perceive Asian Americans of both sexes differently. Intersectionality would predict that the bamboo ceiling and glass ceiling interact in some way, which would lead to Asian women suffering from harsher professional biases than Chinese men. Extant research in the intersectionality framework on the experiences of Asian women in the US and the UK argues that the two ceilings do, in fact, interact and that Asian women are burdened by both racism and sexism (Lane, Tribe, and Hui 2011; Ching et al. 2018; Forbes, Yang, and Lim 2023). A recent view by Lee, Goyette, Song, and Xie suggests that there a 'double disadvantage' for Asian American women in advancing to leadership roles (Lee et al. 2024). Some evidence suggests that the increasing femininity attributed to Asians in general stacks additively with the

femininity that Asian women possess by virtue of their gender, and this doubled femininity renders them less suitable than non-Chinese women in male-typical roles in the eyes of other Americans (Alt et al. 2024). This mirrors some parts of the 'double disadvantage' hypothesis. We might call this the 'doubled femininity' hypothesis, and it would lead us to predict that Asian women suffer more than non-Asian women in fields that are seen as prototypically masculine, such as physics (Gonsalves, Danielsson, and Pettersson 2016).

There is a large body of work about the plight of female scientists. For a fuller overview of this topic, see Xie and Shauman (2005). It suffices to note for our purposes that many have argued that women in science face barriers that their male counterparts do not and that these barriers create intense stratification. Female scientists may publish similar amounts of papers and are professionally rewarded at similar rates for these publications, but they drop out of academia far more often than do men and so publish fewer total papers over careers than do men (Huang et al. 2020). This higher drop-out rate is understandable, for science can be hostile to women. Some teams have found that women in science are less likely to be rewarded with authorship because their work is not recognized (Ross et al. 2022). In some universities, professorial recruitment procedures may even prevent women from finding success (Nielsen 2016).

STEREOTYPES AS A POSSIBLE MECHANISM FOR ANTI-CHINESE BIAS

We have established that simple racial animus is likely not to blame for the bias against Chinese scholars, for non-Chinese Americans view Chinese America relatively favorably. What, then, might drive potential bias against Chinese American scholars? One possible set of mechanisms for this bias may be found in the bevy of stereotypes that Chinese Americans face.

Due to the length of contact between Chinese America and non-Chinese America, there is a relatively rich set of cultural templates in America that may be used to talk and think about Chinese Americans. In the verbiage of the Stereotype Content Model, East Asians are generally seen as high in competence but low in warmth, which generally leads to resentment and envy (Lin et al. 2005). This is a serviceable heuristic, but science is a complex pursuit, and the combination of warmth and competence is not adequate to describe how America views Chinese America. Though empirical evidence on many of the more common but granular stereotypes of Chinese Americans is lacking, they merit explanation because the bulk of these stereotypes are immediately relevant to professional advancement in the sciences.

CHINESE AS ROBOTS AND CHEATS

Competition, particularly scholarly competition, is another major arena of stereotypes of the Chines people. Are Chinese Americans ruthless businessmen whose children 'ruin' school districts (Warikoo 2022), as elite Whites in of the suburbs San Francisco, who pull their children out of public schools when Asian-Americans enroll in large numbers, seem to assume today (Boustan, Cai, and Tseng 2024)? If the Chinese in the United States are seen as needlessly competitive automatons, a stereotype that has a particularly strong valence in academic settings, then they may encounter discrimination when applying to educational institutions. Whether or not this is the case and, more specifically, whether or not affirmative action has negatively impacted the fates of Chinese Americans in college admissions has been litigated extensively over the past decade and resulted in the 2023 decision banning affirmative action in the United States (Arcidiacono, Kinsler, and Ransom 2023). A series of foundational works in the sociology of science (Long 1978; Long, Allison, and McGinnis 1979) argues that one of the best predictors

of professional success in the academy is admission to a prestigious PhD program. If Chinese Americans are excluded from PhD admissions, the downstream consequences that would follow from this discrimination would include a resulting underrepresentation of Chinese scholars in the professoriate. There is no evidence for the underrepresentation of Chinese PhD students in PhD programs, though there is evidence of possible overrepresentation (National Science Foundation 2022). A full exploration of this question is beyond the scope of this paper.

If it is bad that the Chinese are competitive, stereotypes about the ways in which the Chinese compete with non-Chinese may worsen how non-Chinese Americans perceive this competitiveness. A neighboring stereotype to the competitive Chinese is that of the Chinese as cunning, cutthroat, and dishonest. This image thrived throughout the 20th century in anti-Chinese pamphlets and in popular media and was most directly embodied in Fu Manchu, a prototypical Chinese villain seen as an avatar of the Chinese threat to the American people (Frayling 2014). While there is little direct academic work on how the world perceives the purported honesty of the Chinese people, there is some work on honesty of Chinese people in China as measured by coin-flip test (Hugh-Jones 2016) or reporting lost wallets as stolen (Cohn et al. 2019; Yang et al. 2023), and theoretical summaries of extant evidence have so far concluded that honesty operates differently across cultural context without comparing absolute honesty (Blum 2007).

Any stereotype of Chinese scientists either in the West or in China as inveterately dishonest would likely have profound and dramatic effects on the careers of Chinese scientists. This is because science prizes itself as impartial, and the singular deadly sin of science is fraud. Merton did not list honesty as one of the four core values of science, and while this remission should be seen as one of his greatest errors, the Mertonian norm of disinterestedness could be read as an imperfect analogue for honesty (Merton 1974). The main values of American

scientists in the latter half of the 20th century in their words were “objectivity, honesty, integrity” (Hajek, Paul, and Ten Hagen 2024), and the same broadly true of science before the 1900s (Engberts 2022) and into the present (Shapin 2008). English-language discourse on the honesty of Chinese scientists has been tainted by discriminatory overtones. As recently as 2006, a news article in *Science* described China as a 'scientific Wild East' with "an unprecedented number of researchers... accused of cheating - from fudging resumes to fabricating data - to gain fame or plum positions" (Xin 2006). Whether these attitudes have changed in the intervening 20 years is an open question. Recent work has focused on the propensity of the incentive structure of Chinese science at best fails to punish fraud and at worst incentivizes it (Lei and Zhang 2018). It is hard to predict how stereotypes of Chinese scientists in China might carry over to those about Chinese scientists in America, but the nature of discrimination means that stereotypes for one group of Chinese scientists likely apply to all Chinese scientists.

It is similarly difficult to predict how stereotypes of dishonesty both scientific and otherwise would affect the careers of Chinese American scientists. As a first pass, we might predict that the careers of Chinese scientists would suffer the most in fields with the most problems with fraud. To establish which fields are most prone to fraud, it is possible to look to replication or reproducibility rates by field, but this is not a very useful metric because a vanishingly small percentage of published papers are replication studies[23]. Additionally, replication rates do not vary much across fields (Gordon et al. 2020). The finding that only 39% of psychology studies replicated famously began the ‘replication crisis’ in psychology in 2015 (Open Science Collaboration 2015), but the corresponding figure for cancer biology is 37%, which is almost identical (Errington et al. 2021). Finally, a paper’s lack of ability to replicate

[23] 0.1% of papers in economics are replication studies (Mueller-Langer et al. 2019).

does not necessarily imply fraud. Retractions, however, are a stronger but still imperfect but signal of fraud, and we may indirectly operationalize the prevalence of fraud by comparing retraction rates across field. Data on this is unfortunately outdated but suggests that rate of retractions is 0.3% in economics, 0.1% in sociology, and as high as 0.2% in some physics subfields and 0.4% in some chemistry subfields (Grieneisen and Zhang 2012, Table S1). We would then predict that if stereotypes of dishonesty hampered the careers of Chinese American scholars, the influence of these stereotypes would be strongest in the natural sciences.

STEREOTYPES OF CREATIVITY: ASIANS AS ROBOTS

If this stereotype of Asians as robotic is taken to the extreme, the work of Chinese scholars will be seen as derivative and unoriginal regardless of how original it might be. The ideal situation to test this hypothesis would be a more rigorous variation of the classic audit study where rotating panels of professors could be asked to evaluate the work of hypothetical White, Black, and Asian scholars. The work of each scholar would also rotate, and every panel would be exposed to a different configuration of race and scholarship. Though unobservables would muddy any causal inferences that might be drawn from the result, we would then be able to partially isolate how race affects the perception of scholarship. A prototypical small-scale natural experiment of this sort was described in Lu (2024). The violinist Joyce Koh related how the classical music critic Tom Deacon described the exact same recording as "beautiful" when he thought it was performed by a White woman and "faceless... neat as a pin but utterly flaccid" when he thought it was performed by an Asian woman (Koh 2021). This might apply to subfields in the social sciences. Work on how sociologists view specific subfields in sociology is scarce, and what does exist is almost exclusively devoted to the anxiety that sociology undergraduates

feel towards learning statistical methods (Williams et al. 2008; Ralston 2020). We are concerned with how professors might view the work of other academics. Given the stereotype that Asians excel at math, one could imagine that methods-related work might be seen as ground-breaking if authored by a non-Chinese scholar but derivative and rote if authored by a Chinese scholar.

This devaluation effect might apply between as well as within disciplines. The anecdote of Deacon judging the same piece differently based on perceived race of performer is certainly damning, but it is from the intensely subjective art world. To extend Cook (2024a), the first work in this dissertation, it is possible to think of the arts as an extremely soft science where there is no consensus and no shared understanding of essential truth. This would then mean that perception and prestige signals form the main basis for evaluating excellence in art. If we assume that the arts are soft sciences and also that the anecdote of anti-Asian racism in classical music applies universally to all art, both of which are very grand assumptions, it would imply that perception matters less in the soft sciences than in the arts and less in the hard sciences than the soft. Based on this anecdote, if we see strong bias against Asians in the arts, we would expect to see some bias against Asians in the soft sciences but less bias in the hard sciences.

Other scholars have provided ample evidence that Asians are stereotyped as not creative (Lu 2023). A quick glance at China's scientific productivity over the past decade should put these stereotypes to rest, but many have argued that productivity and creativity are not the same thing (Simonton 1997). There is less direct work on the question of how creative Chinese scientists are in fact. Work on creativity in Chinese cultural contexts has largely analyzed the putative creativity of Chinese people in predominantly Chinese societies (Fan 2014; Cheung and Yue 2019; Wu and Albanese 2014) and has generally avoided comparisons between greater China and the non-Chinese world with very exceptions, including Tang et al. (2021) and Tang and

Kaufman (2015). Recent meta-analyses have shown that the gap in creativity between 'Western and Eastern cultures,' which currently favors the West, has narrowed in younger cohorts (Barth and Stadtmann 2024). It would be possible to create a rough measure of creativity and originality, perhaps following Foster, Rzhetsky, and Evans (2015), and then use it quantify, if roughly, the creativity of Chinese scientists relative to other ethnic groups and then calculate the returns to creativity by race in the vein of Hofstra et al. (2020). It would also be possible to see if the stereotypes of Chinese Americans as unoriginal and math-savvy interact by comparing the returns on specialization in relatively quantitative subfields, such as casual inference in sociology, by ethnicity. A direct test of creativity following these lines is not possible with our current data, but we may instead ask in which fields creativity is the most prized. Creativity may potentially be reframed as a trait only tangentially or even orthogonally related to the empirical value of scientific work, but this is not a very useful reframing, for creativity has long been prized across the hard and soft sciences (Rothenberg 1979; Simonton 2004). Instead, calling a fellow scientist's work 'uncreative' might be a way to cast aspersion without addressing the import or validity of the work's conclusions. It is, in other words, a serious charge as well as an insult that is acceptable in scientific circles, which means that we would expect to see stereotypes of non-creativity to carry the most weight in sciences with lower degrees of internal consensus, which is to say the softer sciences. If the creativity hypothesis is true, we would expect to see a bias against Chinese scholars in the soft sciences.

SEX, GENDER, AND ATTRACTION

Sex and gender matter as well, for there are a number of gender-specific Chinese stereotypes. Are Chinese women submissive and passive, as they were commonly portrayed in

the late 1990s (Uchida 1998; Prasso 2006), or are they sexual predators out to ruin the lives of young White boys (Matsubara 2003)? Are Chinese men effete, creepy, feckless, and weird, or are they a threat to White women as Chinese women are to White men (Lee 2017)? Of particular interest is that stereotypes regarding race and gender interact for East Asians. East Asian faces are generally viewed as more feminine than similarly-gendered faces of other races (Stephen et al. 2018), and East Asian women are viewed as prototypically feminine (Alt et al. 2024). This broad attribution of femininity may have field-specific consequences for sorting Chinese scholars preferentially into more 'feminine' fields. While there is evidence on how the 'hard' sciences are viewed as masculine (Gonsalves, Danielsson, Pettersson 2016), there is none on how or if the soft sciences are viewed as feminine.

ATTRACTIVENESS AND GENDERED STEREOTYPES OF ASIAN AMERICA

Even the gender-based stereotypes of the Chinese in America that are not directly relevant to science, such as that of Chinese men as unattractive, may influence professional success via the 'halo effect' (Thorndike 1920). If Chinese women are seen as attractive and Chinese men as sexually undesirable, then the careers of both groups might diverge. It could be the case that the perceived attractiveness of Asian women and perceived unattractiveness of Asian men influences their respective placements on the academic job market. There is a bevy of evidence that beauty is associated with better professional outcomes in general (Hellyer et al. 2023; Bortnikoa, Havranek, and Irsova 2024), though this evidence also indicates that beauty and productivity are correlated; that students rated as good-looking by teachers earn better grades (Mehic 2022); and even that the attractiveness of a PhD student leads to more prestigious first academic job (Liu, Lu, and Veenstra 2022; Hale, Reveg, and Rubinstein 2023; Alkusari, Gupta,

and Etcoff 2024). If attraction is as important to professional success as some research suggests, than the stereotypes of Asian men as less desirable are not only psychologically damaging but also potentially career-hampering.

East Asian men suffer from a particularly intense stereotype burden. They are stereotypically seen as physically unattractive, sexually inadequate, socially awkward, and feminine, and they are aware that others seem them thusly, which has profound negative consequences for the mental health of Asian-American men (Iwamoto, Liao, and Liu 2010; Wong et al. 2012). Chinese men are consistently seen as less masculine and less athletic than White men, who are in turn seen as less masculine and athletic than Black men (Wong, Horn, and Chen 2013). On the surface, this contradicts the dictates of intersectionality; should not East Asian women suffer more than East Asian men by virtue of their being women? Scholars of East Asian masculinity have offered a few theoretical approaches that may this apparent tension. The Subordinate Male Target Hypothesis argues that low-status men are the target of oppression from high-status males, and, because many scholars see non-White men occupying lower status positions than White men, this model predicts that Asian men might experience more discrimination than Asian women. (Yoo, Steger, and Lee 2010; Buhl et al. 2024). If this hypothesis were true, we would predict that Asian men would suffer from more than Asian women on the whole but particularly in male-dominated fields.

MEASURES AND METHODS

DATA

We use a combination of CV data and large bibliometric databases to examine bias against Chinese American scholars using a metrics-based approach. Inspired by the methodology used to compare racial groups and college admission likelihood in Espenshade and Radford (2009) and the index-based approach to stratification in Greenman and Xie (2008), we construct an index comparing the productivity of Chinese and non-Chinese scholars. We specifically compare both citations and papers published. We find that, on average, Chinese scholars must publish more and be cited more often to attain positions similar to those of their non-Chinese peers, but this is only true on average and in the aggregate. We then stratify our analyses on gender, prestige of host institution, and field.

DATA

We use a combination of CV data and large bibliometric databases to examine bias against Chinese American scholars using a metrics-based approach. We begin with a database of transcribed information from CVs from the professoriate of top 100 departments across four fields: sociology, economics, chemistry, and physics. This provides information on everything that one could gleamed from a *curriculum vitae*: education history, professorial rank, employment history, and more. We then link this proprietary database to the Microsoft Academic Graph (Sinha et al. 2015) database, which contains comprehensive information on published papers, including paper-level citations. To link the two datasets, we use a combination of rule-based methods to ensure, for example, that the years of an author's published papers and PhD

attainment align, and we word vectorization techniques to disambiguate authors with the same or similar names that study different topics.

The end result of the data linkage process provides a comprehensive view of every professor in the top 100 departments across four fields: sociology, economics, physics, and chemistry. For every professor in our sample, we have information on Chineseness, gender, professorial status (assistant, associate, or full professor), PhD origin, and complete publishing record including all citations for all papers.

PRESTIGE

We segment professorial prestige into 3 bands: top 15, top 15-50, and top 50+. These bands roughly mirror how academics discuss the relative prestige of different graduate programs. For a sample of how this 3-way split would read in real terms to sociologists, a 3-way prestige split would capture the difference between Harvard (#2), UMichigan-Ann Arbor (#2), and the University of Chicago (#7) vs. Pennsylvania State (#23), UMaryland-College Park(#24), and the University of Arizona (#26) vs. University of Florida (#64), Bowling Green State University (#79), and Case Western Reserve University (#84) (USNWR 2024). There may be a possible confounding of field-level prestige with university-level prestige. In sociology, for example, Yale University is ranked #18, but Yale University is considered one of the most prestigious Ivy League universities along with Harvard and Princeton. This is beyond the scope of this paper.

INDEX-BASED APPROACH

The main independent variable of interest is citation count. We are interested in the presence of citation disparities between Chinese and non-Chinese professors of comparable status. Following the overachievement hypothesis, we ask: does a Chinese academic need to garner more citations to his or her work to attain a job that a non-Chinese academic could attain with lesser qualifications? This is, however, not a straightforward question because of the nature of citation counts. Citation counts follow a power law distribution[24] (Peterson, Pressé, and Dill 2010), and many methodological challenges arise when performing basic operations both within and between power distributions. We first assume that the citation counts of each field are independently distributed power law distributions. Following Greenman and Xie (2008) and the second paper in this dissertation (Cook 2024), we construct a cohort- and field-specific measure of relative citation that we term the citation multiple. While our results are robust to comparing the citations of Chinese and non-Chinese professors multiplicatively and additively, ie. via the citation multiple and then without correction, the results from the citation multiple are much more straightforward to interpret, and we present these in the main text.

While citation count, which is a proxy for article quality, is quantitatively different across fields, other measures that we might use to capture the quality of any given publication instead different qualitatively across fields and would render inter-field comparisons impossible. The most important dimension of publication success that we have not explored is publication prestige, or the relative prestige of a journal in which any given paper is published, but most methods of quantifying publication prestige are not portable between disciplines. Economists are very familiar with the 'Tyranny of the Top Five' (Heckman and Moktan 2020), wherein any

[24] Many other aspects of academic output similarly follow a power law distribution. One of the earliest findings in the sociology of science was that author-level productivity, as defined by total production of papers, may be modeled with a power law distribution (Lotka 1926).

publication in economics' top five journals carries outsize weight. Sociologists suffer under the tyranny of sociology's top 3 journals, which are the American Journal of Sociology, the American Sociological Review, and Social Forces (Abbott 1999). While sociology and economics have broadly similar publishing cultures and, more specifically, very similar incentive structures for high-impact publishing,[25] the publishing cultures of the natural sciences are very different. We use an index-based approach to measuring citations for a similar reason: the number of papers published per year in each field varies so widely between the hard and soft sciences that a direct comparison of the citations that each paper garners is useless, but this difference is purely quantitative and may be easily resolved with normalization techniques. We might also focus on productivity and paper count, but the generally held consensus in the science of science is that publication quality is more important than publication quantity for securing a prestigious job (Kaur et al. 2015; Forthman et al. 2020). Because our main outcome of interest is professorial prestige, we focus primarily on citation count, which is the most robust proxy for article that is also fungible between fields. We will address questions of productivity and publication prestige in future work.

We aim to advance debates about representation by comparing the relationship between professional productivity and professional success across ethnicity in the academy. In doing so, we benefit from the uniquely quantifiable nature of science. Where previous work has focused on the representation of East Asians in leadership positions in large corporations (Lu et al. 2020) and the relationship between GPA and club leadership in business schools and law schools (Lu 2022), the outcomes in these examples are not coupled as tightly to their inputs as in the case of

[25] It is also the case in sociology and economics that publications in a top journal are more impactful for one's career if they are solo-authored instead of authored in a team. This is not the case in physics, where large teams are the norm.

the professoriate allotting jobs on the basis of papers. While science does not always live up to its ideals of objective meritocracy (Merton 1974), many of the inputs that are involved in calculations of job allotment and prestige are public. These inputs mostly relate to publishing: how many papers an academic has published, how prestige the venues of these papers are, and how many citations these papers have garnered are all important components of the job application process. Because the stated ideals of science mean that scientists must at least pay lip service to these standards of objectivity in hiring decisions, any deviation from these standards should invite suspicion. We compare Chinese to non-Chinese women following other work on gendered bias in academia (Tinkler et al. 2019)

RESULTS

PRESTIGE BY CHINESENESS AND GENDER

We begin by segmenting our data by Chineseness[26] and gender and first present basic counts of professors by Chineseness and gender across the 3 prestige bands[27]. Though purely descriptive, this information may be of interest to scholars of gender and science and technology studies on its own terms. First, we have prepared tables showing the proportions of Chinese vs. non-Chinese (Table 1) and then male vs. female professors (Table 2) regardless of prestige for our 4 fields of interest. We additionally provide a table of distribution of professorial head count by prestige band in the supplementary information (Table S1).

[26] We recognize that 'Chineseness' is an unwieldy term but stress that it is the most straightforward way of describing the state of one's being Chinese or not Chinese.
[27] Because racial prejudice does not discriminate between Chinese people raised in America and Chinese people from China, we treat all academics in our sample with Chinese ancestry as Chinese.

[Table 1 here]

[Table 2 here]

We find that Chineseness and maleness scale almost linearly with the hardness of a science. Chemistry, the sole exception to this rule, is less hard than physics but has a higher proportion of Chinese professors than physics. The proportion of males by field, however, increases in perfect lockstep with scientific hardness. Some scholars of science contend that the hard sciences are seen as masculine (Gonsalves, Danielsson, Pettersson 2016). They are indeed masculine if we take the preponderance of males in chemistry and physics to indicate masculinity. While a full exploration of this finding goes beyond but the scope of this paper, it is sufficient to note that comparing Chinesess and maleness of professoriates across fields could be used as indicators of scientific hardness.

This descriptive finding invites us to consider why and how Chinese scholars choose their fields of study. There are exogenous and endogenous factors that presumably sort Chinese scholars into some fields over others. Following the 'doubled femininity' hypothesis (Alt et al. 2024), we might predict that Chinese women preferentially sort into more feminine-dominated fields. This is not the case. In fact, the exact opposite is true. Chinese women represent a much higher proportion of female scholars in the hard sciences than in the soft. There may also be an endogenous set of cultural forces that influences the course of Chinese Americans through academia. While there is a lesser-known Chinese cultural premium placed on earning money and having a stable life (Steele and Lynch 2012; Li and Hu 2022), it is very widely understood that

Chinese parents have high expectations for their children's educational success regardless of familial wealth (Li and Xie 2020). Education, which is mostly studying extant knowledge, and science, which involves producing new knowledge, are very different pursuits but do take place at the same institutions. This overlap might generally drive East Asians to preferentially become professors at higher rates than non-Chinese people. This does seem to be the case; Chinese-Americans are less than 1% of the United States but comprise between 4% to 11% of the professoriate for our fields (Table 1). The specific contours of this cultural preference, particularly the emphasis on educational success and on high-income jobs, may separately interact to drive Chinese scholars to select careers more well-renumerated fields. This is the case for undergraduates; East Asians in general (Xie and Goyette 2003) Chinese women are more likely to select undergraduate majors that lead to highly-paid professions than White women (Song and Glick 2004). This trend continues into post-graduate education, where Chinese students are far more likely to attend graduate programs in the hard sciences or in engineering that in the social sciences, and this necessarily means that fewer Chinese and Chinese-American scholars become social scientists (National Science Foundation 2022). We see that this trend extends into professorial careers upon obtaining a doctorate degree; Chinese scholars gravitate to harder sciences over the soft.

We have seen that certain fields skew male and have a higher proportion of Chinese scholars, and we have also seen that Chineseness and maleness are very tightly correlated. We now may additionally ask if there is any relationship between the proportion of Chinese females and Chinese males in our fields of interest.

[Figure 1 here]

We find that Chinese females enjoy a large and consistent gender-specific proportional advantage in terms of head count and placement over Chinese males. While the advantage enjoyed by Chinese women varies by prestige band, and there are a handful of field-prestige band combinations wherein Chinese females are scarce relative to Chinese males, there is no field where, on average, Chinese females are less represented than Chinese males. There are, for example, proportionally almost 3 times as many Chinese female as there are Chinese males in economics. This aligns with the work of Song and Glick (2004), who found that Chinese female undergraduates preferentially select jobs that lead to highly-renumerated jobs, for what field of study is more obviously associated with money than economics? There are proportionally 1.28 times as many Chinese females as there are Chinese males in chemistry, and the corresponding figure is 1.69 for chemistry. These findings directly contradict the 'doubled femininity' hypothesis. In sociology, the softest and most 'feminine' field in our work, the advantage of Chinese females over Chinese males is present but weakest, and there are proportionally the fewest Chinese females overall in sociology. For a more detailed view of these figures, see Supplementary Figures S1 – S4.

MATCHING RESULTS

COVARIATES AND MATCHING

We match on professorial rank, PhD prestige, decade of PhD, and gender. We use exact matching to match on all observable covariates. Because of this, the covariates are perfectly balanced by default, though our results are robust to other matching methods (See Figure S5).

Our results are additionally robust to specifying the Chinese penalty as a difference or as a ratio, and we will present both sets of results for ease of interpretation.

Figure 2 illustrates the bias against Chinese scholars in each of the fields under analysis by calculating the gap in citations needed to attain a given position between Chinese and non-Chinese scholars as a ratio, and Figure 3 calculates the same figure as a difference. Both figures show a consistent citation penalty against Chinese men in the academy and a consistent citation bonus for Chinese women in the academy. We invert the Y-axis for both figures to clearly show the direction of the bias against Chinese scholars.

[Figure 2 here]

[Figure 3 here]

A few general trends emerge. Both Chinese males and females enjoy a citation bonus relative to non-Chinese scholars in chemistry, but this is the only field for which is the case. In all other fields, penalties against Chinese men are the rule. On the whole, Chinese men are treated more equitably in the harder sciences, which also happen to be more male-dominated and more proportionally Chinese. Chinese women suffer from a slight penalty in top 15 departments in both economics and sociology, but these are the only two combinations of prestige band and discipline where Chinese women suffer a penalty, and, even then, the citation penalty they suffer is almost negligible. The fields that are the most hostile to Chinese males, namely sociology and economics, are also the most hostile to Chinese females, but top 50 – top 100 programs in sociology bestow an inordinate advantage upon Chinese female scholars. Where Chinese male scholars in top 50 – top 100 programs in sociology must garner almost 10 times more citations than their cohort- and gender-matched non-Chinese peers, Chinese females in the same positions

require only 0.02 times as many citations as their cohort- and gender-matched non-Chinese peers.

A minor but notable secondary finding is that the direction of bias across prestige bands varies by field. Chinese men suffer the most within-discipline bias in top 50-100 programs in sociology and economics relative to top programs in both fields but suffer more in top 15 programs in physics than in less prestigious physics departments. Additionally, Chinese females do not enjoy a bonus and instead suffer a very mild penalty in both top 15 sociology or top 15 economics departments, though the magnitude of this penalty is so small that it is almost not worth mentioning.

We may consider the specifics of the differential gap between Chinese male citation penalty and the Chinese female citation bonus. Figure 4 shows the ratio of the Chinese female bonus to the Chinese male penalty, and Figure 5 presents this result as a difference. The only field in which Chinese men enjoy a bonus relative to Chinese women in is chemistry, and the magnitude of this advantage is vanishingly small. In all other fields, Chinese females enjoy a sizable and consistent advantage over Chinese males, but the magnitude of this ethnicity-specific relative advantage shrinks in tandem with scientific hardness.

[Figure 4 here]

[Figure 5 here]

HYPOTHESES VERIFIED OR DISCARDED

A few of the hypotheses for bias against Chinese professors emerged readily from our summary of the literature on this topic, and we will discuss how have they have fared in light of our results in turn.

We begin with the hypotheses that were discarded. Firstly, we see that the intersectional hypothesis, which predicts that non-White women suffer from a greater discriminatory burden than do non-White men, is false. Chinese women enjoy a clear and distinct premium both in terms of overall representation in the professoriate and in qualifications needed to attain academic employment over Chinese men. This premium is dramatic and constant. Secondly, the fraud hypothesis, which predicts that Chinese scholars may be discriminated against most intensely in fields with higher proportions of retractions if Chinese scholars are seen as dishonest, is false. Fields with the most retractions feature the highest proportion of Chinese scholars and show the least bias to Chinese scholars. If there were underlying racism in American universities against Chinese scholars in fraud-ridden fields, it would likely emerge in at least one of the two domains. We see no evidence of the bias predicted by the fraud hypothesis in either of our metrics of interest and in fact see the opposite.

Finally, the subordinate male target hypothesis, which predicts that non-White men fare worse than non-White women in some contexts because they are preferentially oppressed by White men, is partially true in that Chinese men fare worse than Chinese women, but the subordinate male target hypothesis additionally requires that White men do the oppressing. There is no evidence that this is the case. The subordinate male target hypothesis would predict that non-White men would suffer unduly in male-dominated fields, and the truth is precisely the opposite: Chinese men suffer most in more female fields where Chinese women enjoy a weak

premium in male-dominated fields and a stronger premium depending on prestige band in more female fields.

This leaves three hypotheses as potentially true. While we do not have the data at hand to test any of the following, they are at least not rejected, and we leave any specific inquiry to future work. The attractiveness hypothesis would predict that Chinese women, who are seen as more attractive by non-Chinese alters, enjoy a premium relative to non-Chinese men. The data supports this conclusion, but to conclusively prove the attractiveness hypothesis would require much finer-grained tests of how individual-level attraction mediates the link between publication record and professional success. Secondly, the creativity hypothesis is also potentially viable, but we again stress that we have not specifically tested its predictions. A more intensive test of the creativity hypothesis would require much more extensive approaches to measuring scientific novelty, but if we frame creativity an ancillary quality that carries undue importance in the soft sciences, then we see just what the creativity hypothesis predicts: Chinese scientists suffer the least bias in the highest-consensus - which is to say the hardest – sciences, all of which have relatively clear standards and evaluatory criteria.

The final hypothesis that emerges from the literature is the ethnocentrism hypothesis, and it contends that East Asians are more ethnocentric than other groups and network primarily among themselves (Lu 2022). This hypothesis would prediction that the ostensibly ethnocentric Chinese would thrive in more Chinese-dominated fields, and it is not immediately disproven because Chinese scholars indeed suffer less bias in more Chinese-dominated fields. Another point supporting the ethnocentrism hypothesis is that Chinese males have been found to be more ethnocentric and generally less willing to network with non-coethnics than Chinese females in observational settings (Yousaf et al. 2022), and Chinese males suffer less discrimination in fields

with more Chinese males. As discussed above, however, this hypothesis is not immediately tenable because people of Chinese descent are not the most ethnocentric group in the world.

DISCUSSION

After matching on a wide range of covariates, we find that Chinese women enjoy a two-pronged advantage over Chinese men in the academy. Conditional on gender, there is a higher proportion of Chinese women than Chinese men in all 4 fields. The magnitude of this gender gap is dramatic; there are 3 times as many Chinese women relative to non-Chinese women as there are Chinese men relative to non-Chinese men in top 15 programs in chemistry. In other words, Chinese women enjoy a placement advantage relative to Chinese men.

This difference in representation is dramatic but strictly observational. We then use a battery of matching methods to control for observed covariates. Controlling for decade of PhD, professorial rank, gender, prestige of current job, and prestige of PhD origin, we compare Chinese to non-Chinese professors using a measure of relative citation that we term the citation multiple. This allows us to ask how much more cited a Chinese scholar must be relative to a non-Chinese scholar of an almost identical background to attain the same job.

We find that Chinese males, on average, suffer from a Chinese penalty relative to their non-Chinese peers. This penalty is as high as 9.42 for top 50 – 100 departments in sociology, which is to say that Chinese males must be cited 9.42 times as much as their non-Chinese peers in top 50 – top 100 departments in sociology, and as low as 0.1 in top 15 departments in chemistry. Chemistry is the only field in which Chinese males enjoy a persistent advantage relative to non-Chinese males. The situation is the exact inverse for Chinese females; relative to

non-Chinese females, Chinese female academics enjoy a citation bonus in all fields and all prestige bands except for top 15 departments in sociology and economics. By directly comparing the ratios of gender-specific Chinese female advantage to gender-specific Chinese male disadvantage, we find that the only field in which Chinese females do not enjoy an overwhelming advantage relative to Chinese men is chemistry.

Our results suggest that common approaches to understanding stratification in the United States along racial and gender liens are not always accurate. Specifically, the priors of intersectionality, perhaps the leading theory of gender- and race-based inequality, predict that different vectors of oppression can be summed and compound upon one another to create increased oppression for the "multiply burdened." Our work presents a third possibility: non-White women of a given race may enjoy an advantage relative to their male co-ethnics, and sex, commonly seen as an independent source of oppression, may ameliorate bias in some cases. While it is certainly grandiose to do so on the basis of one study, sociologists would likely formulate models of social systems with more predictive and explanatory power if they constructed these models from the bottom-up reality of how different groups behave in isolation and in tandem as opposed to beginning from stylized presumptions of how oppression flows from top to bottom. For similar findings in this vein, we see how the income gap between Black women and White women is almost non-existent where the income gap between Black men and White men is very large (Chetty et al. 2020). The hypotheses that were not immediately disproven by our work, which include the ethnocentrism hypothesis, creativity hypothesis, and the attractiveness hypothesis, are not mainstream explanations for inequality or stratification. The one thing these 3 hypotheses have in common, however, is that they are bottom-up theories of stratification. With the possible exception of the attractiveness hypothesis, which does integrate

very neatly with intersectionality and the rich literatures on the study of race in America, these 3 hypotheses make predictions about how a group will be received based on the characteristics of the group itself. These bottom-up approaches are also more easily integrated into other methodological traditions, such as simulation via agent-based models and experimental approaches in either surveys or lab settings. A more interactional approach to discrimination may yield more actionable models with actionable insights for marginalized groups.

Our work is not without limitations. We have not, for example, examined bias against non-Chinese East Asians or South Asians. We additionally either do not have the data or do not have the space to conclusively test the remaining hypotheses for bias against East Asians in the academy, which are the attractiveness hypothesis, the creativity hypothesis, and the ethnocentricity hypothesis. We leave a fuller exploration of these themes for future work.

A few additional conclusions specific to scholars of science emerge from our work. For sociologists of science, our results suggest that the underlying parameter of scientific hardness is useful for investigating inequality in the sciences. We see that the degree of bias shown to Chinese scientists in science varies dramatically with scientific hardness, and we also see that ethnicity and gender are related intimately to hardness.

We find that the hard sciences are more receptive to Chinese scholars than the soft sciences. These results are particularly striking because, by almost all extant measures, the hard sciences are more likely to be openly anti-Chinese than the soft sciences. Professors in the hard sciences tend to be more conservative than those in the soft sciences (Gross and Fosse 2012). Chinese professors in the natural sciences, engineering, and computer science, all of which are generally 'hard' sciences, are about 1.5 times more fearful of being investigated by the Trump DOJ and of being targeted by anti-Asian violence than Chinese academics in the social sciences

(Xie et al. 2023, Tables S10 and S11). Despite these fears, our work shows that there are proportionally more Chinese scientists in the hard sciences than the soft sciences, and Chinese scientists are also treated more equitably in the hard sciences than the soft as measured by the citation multiple. Presumably, the pull of the hard sciences outweighs the push of their anti-Asian elements, but this does not explain the relative lack of bias shown to Asians in the natural sciences. What, then, is going on? We do not have the data at hand necessary to test why this might be so, but a few obvious hypotheses merit discussion. The hard sciences may be more meritocratic than the soft sciences because the increased consensus in the hard sciences over the soft renders evaluation in the hard sciences more transparent and straightforward (Cook 2024a). This would presumably diminish the role of non-cognitive factors like assertiveness in success in the hard sciences, and if Lu (2022) is correct in arguing that assertiveness partially explains the advantage of South Asians over East Asians, then East Asians might preferentially filter into fields where they feel less disadvantaged.

The other major implication of our work for scholars of science involves reversing the direction of our analysis. We have so far discussed what the reception of Chinese scientists in various sciences can tell us about Chinese scientists. While we have asked how underlying cultural parameters might guide Chinese scientists to preferentially select certain fields over others, we have devoted most of our time to analyzing how this informs us of the American treatment of Chinese Americans. We may instead ask what the reception of Chinese scientists in various sciences tells us about those sciences. Following Cook (2024a), we may fruitfully position 'Percentage of Chinese Scientists' as a marker of the hard-soft distinction, and we may do the same, albeit far less usefully, with 'Percentage of Women' as well. That ‘hardness’ may be quantified as a cline with sociology at one end and physics at the other and that the percentage of

Chinese scientists in a given field is very closely correlated with hardness implies that there may be a deeper link between scientific hardness and receptiveness to Chinese scholars or, more broadly, non-American scholars. If we assume every branch of science is a functionally identical to the other branches of science, as many scholars of science have done implicitly if not explicitly (Cook 2024a), then this finding is neither explicable nor important. If we instead assume, however, that every discipline maintains its own disciplinary culture, then this finding makes perfect sense.

TABLES

	Non-Chinese	Chinese	% Chinese
Sociology	1291	53	4%
Economics	1895	192	9%
Physics	3158	323	9%
Chemistry	2953	359	11%

Table 1: Chineseness by field

	Female	Male	% Male
Sociology	680	664	49%
Economics	257	1017	79%
Chemistry	540	1674	75%
Physics	336	2004	85%

Table 2: Maleness by field

Prestige Band	Sociology	Economics	Chemistry	Physics
Top 15	279 (24.9%)	222 (23.9%)	356 (21.7%)	388 (27.3%)
Top 15-50	513 (45.8%)	374 (40.3%)	573 (34.9%)	551 (33.8%)
Top 50+	328 (29.3%)	332 (35.8%)	715 (43.5%)	481 (33.9%)
Total	1120	928	1644	1420

Table 3: Professorial headcount by field and prestige band

FIGURES

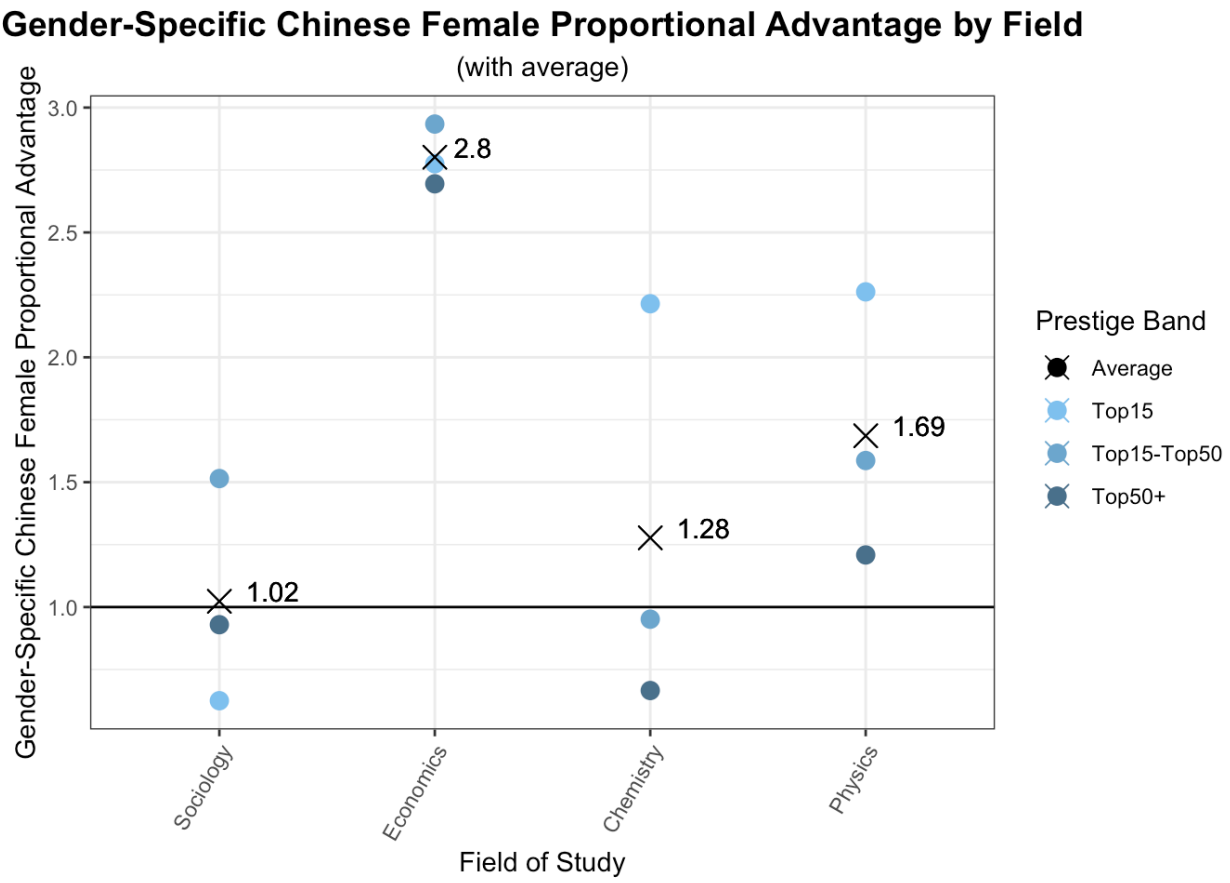


Figure 1: Gender-specific Chinese female proportional advantage by field

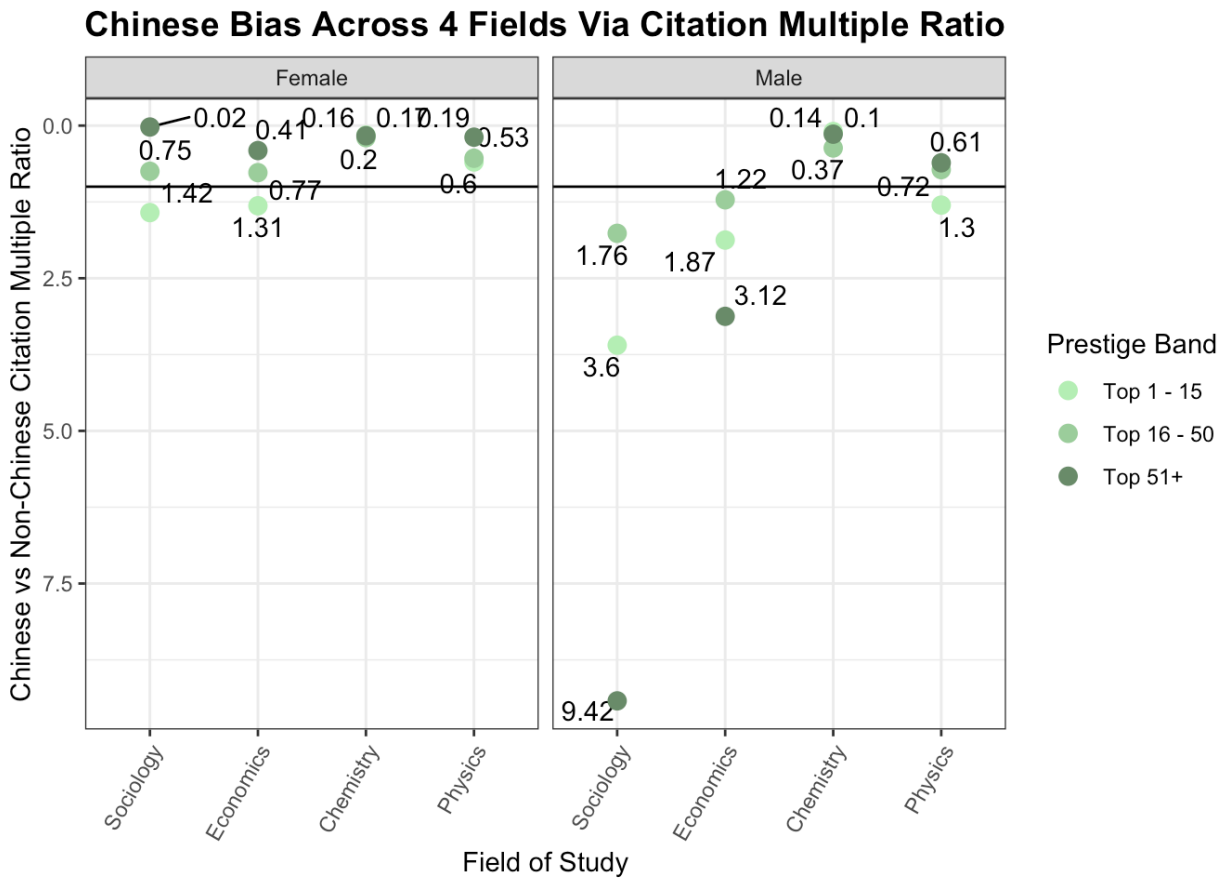


Figure 2: Chinese bias across 4 fields via ratio

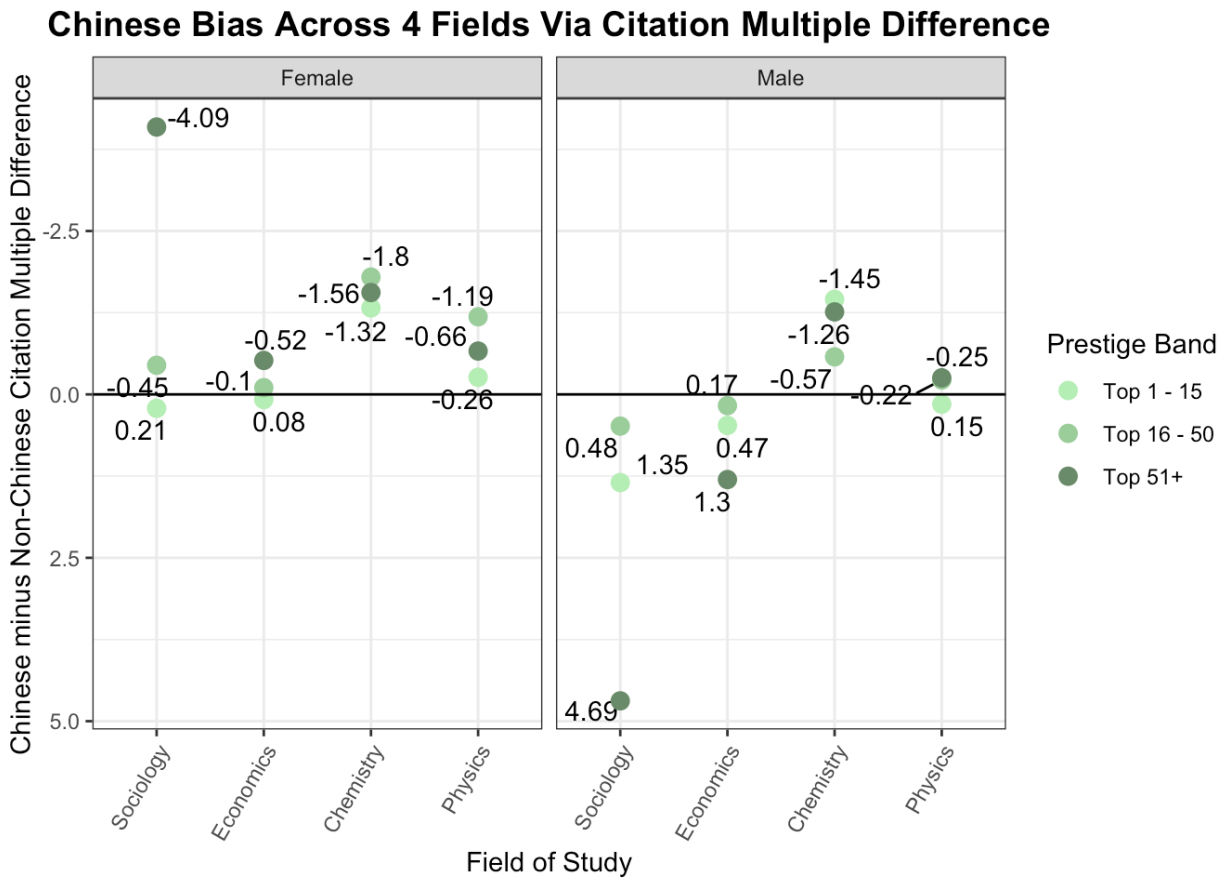


Figure 3: Chinese bias across 4 fields via difference

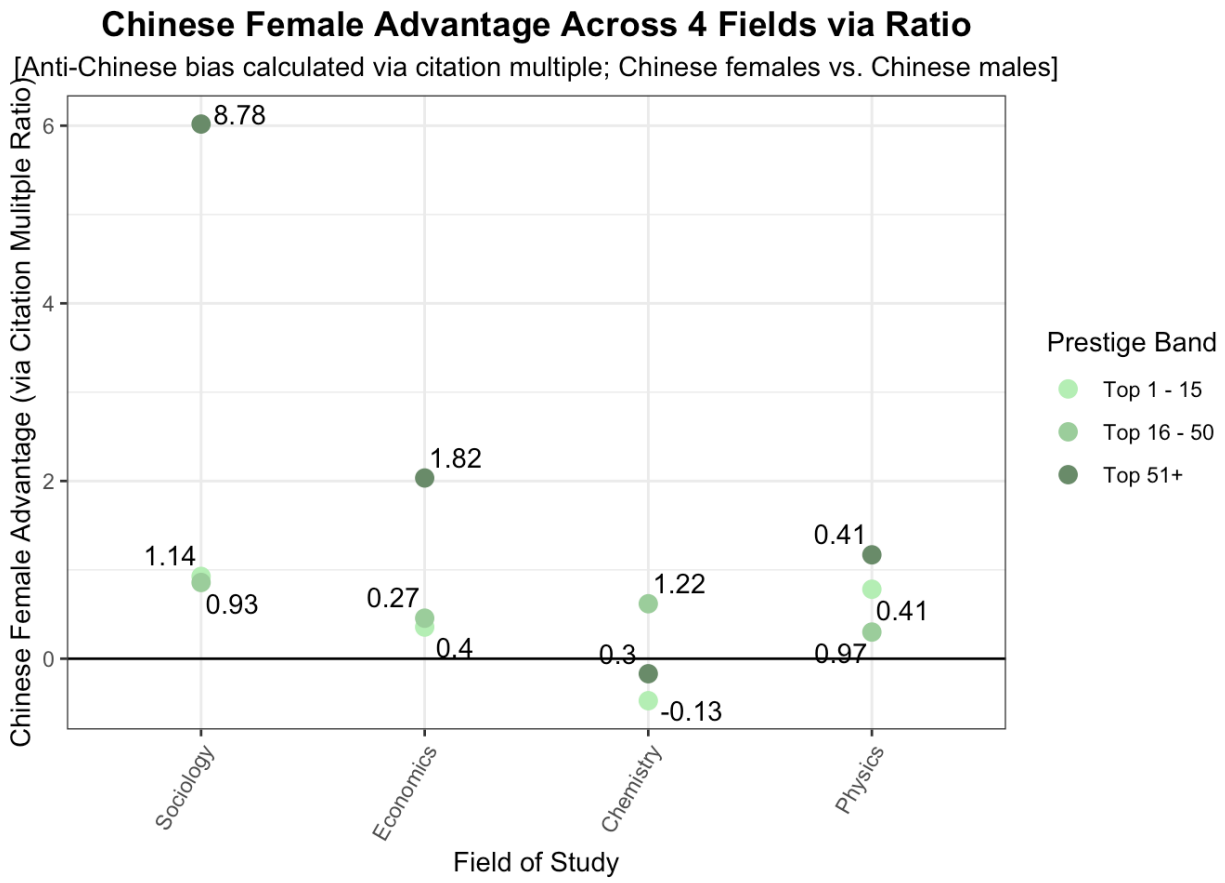


Figure 4: Chinese Female Advantage via ratio

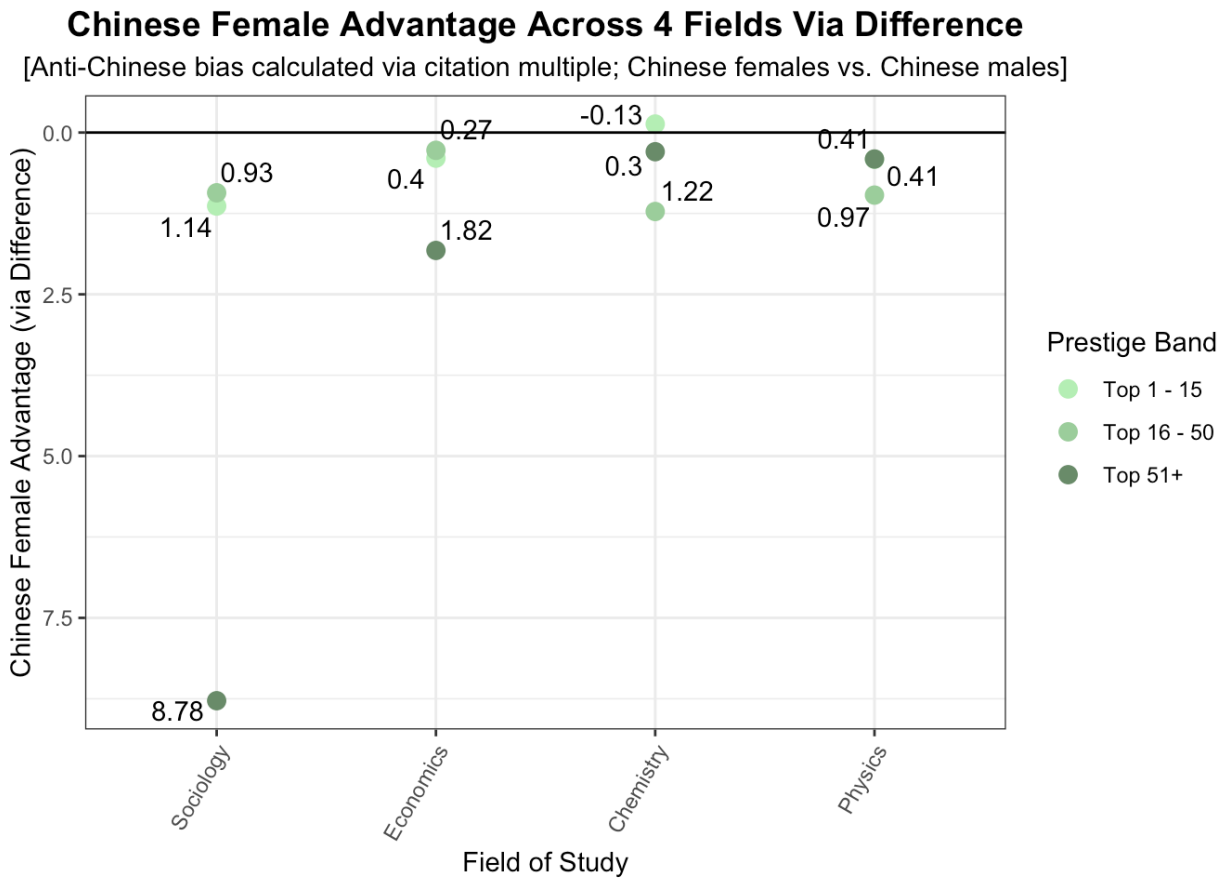


Figure 5: Chinese female advantage via difference

SUPPLEMENTARY FIGURES

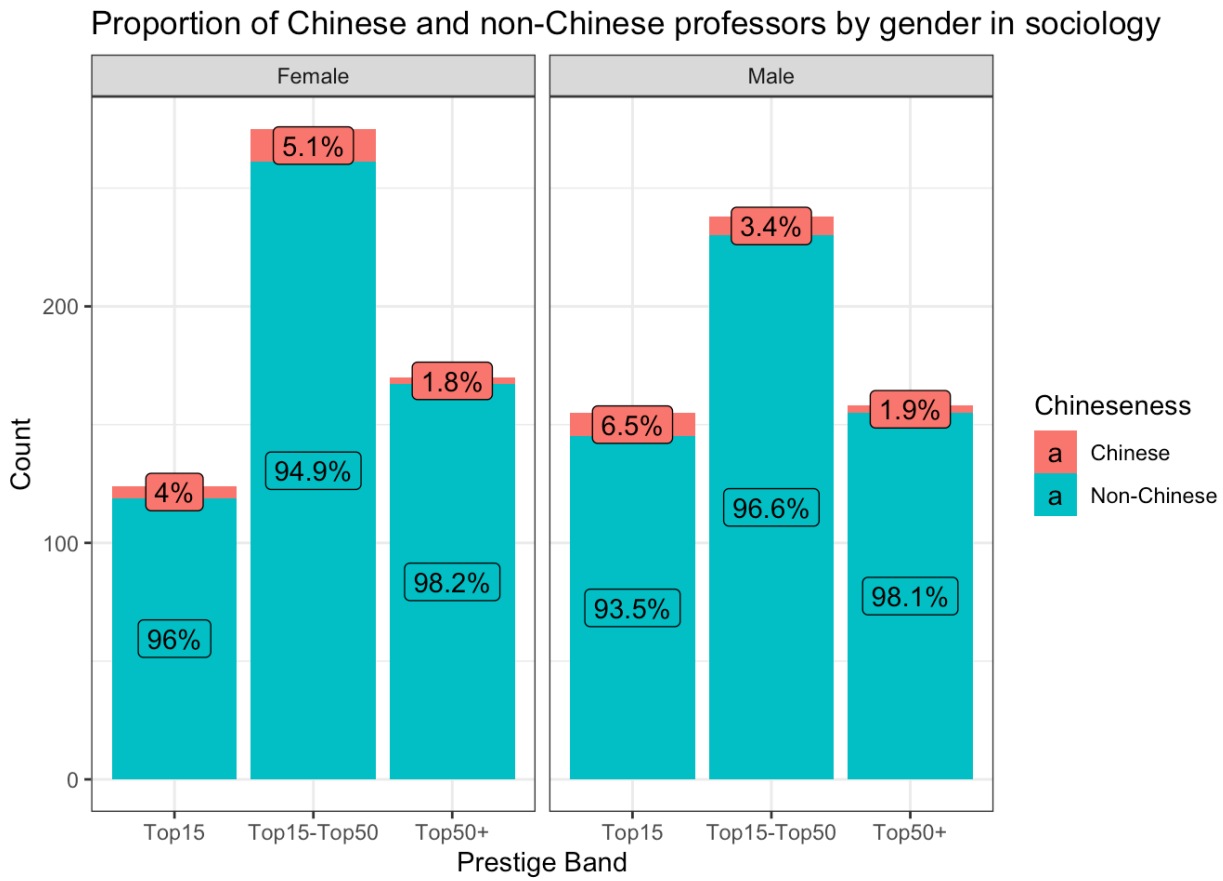


Figure S1: Proportion of Chinese and non-Chinese professors by gender in sociology

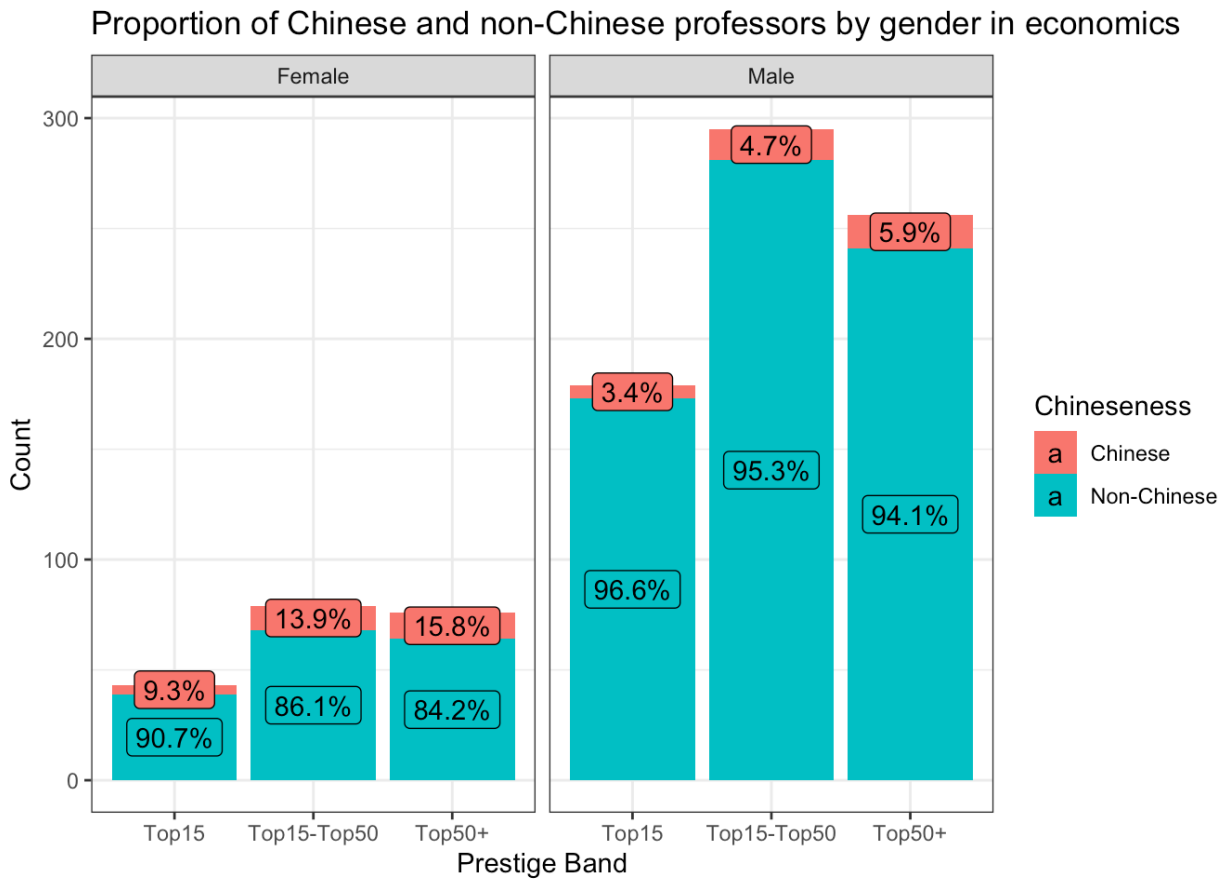


Figure S2: Proportion of Chinese and non-Chinese professors by gender in economics

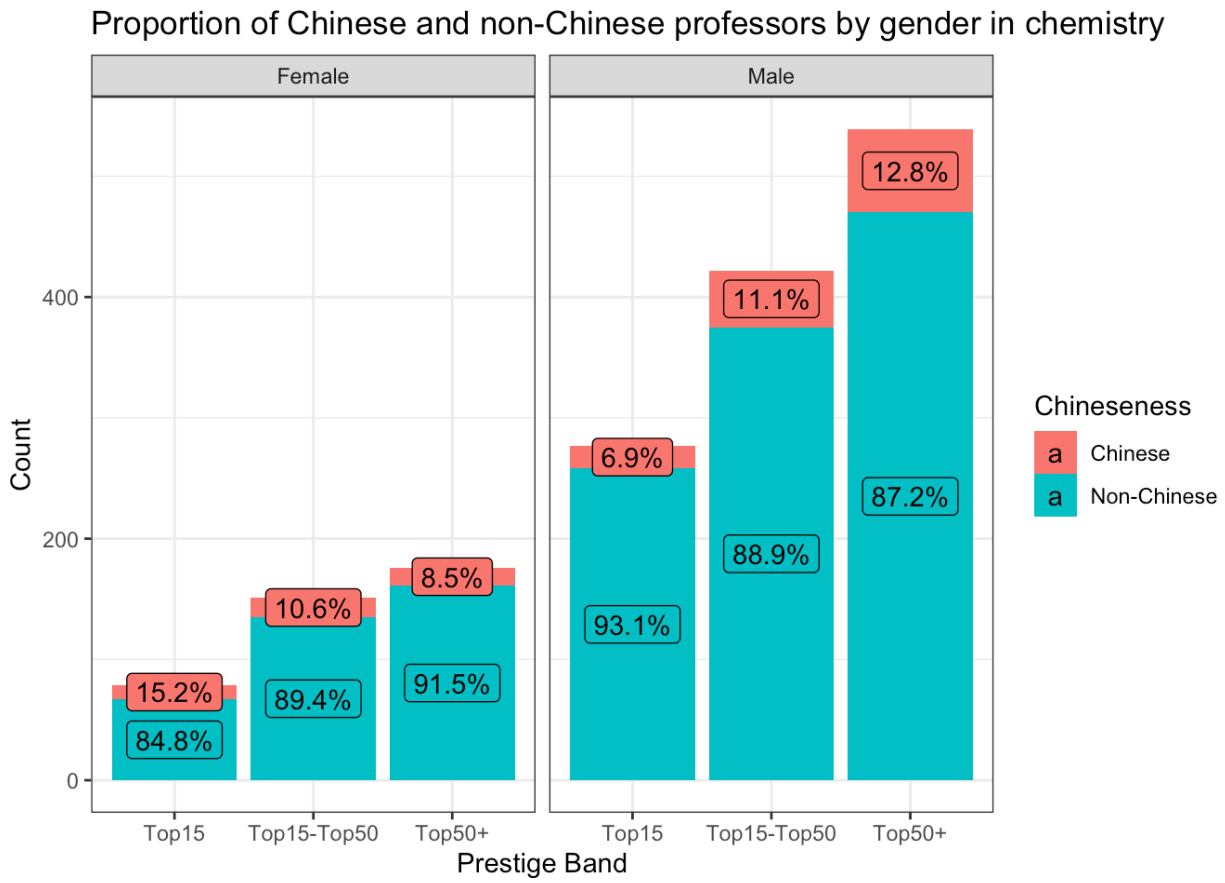


Figure S3: Proportion of Chinese and non-Chinese professors by gender in chemistry

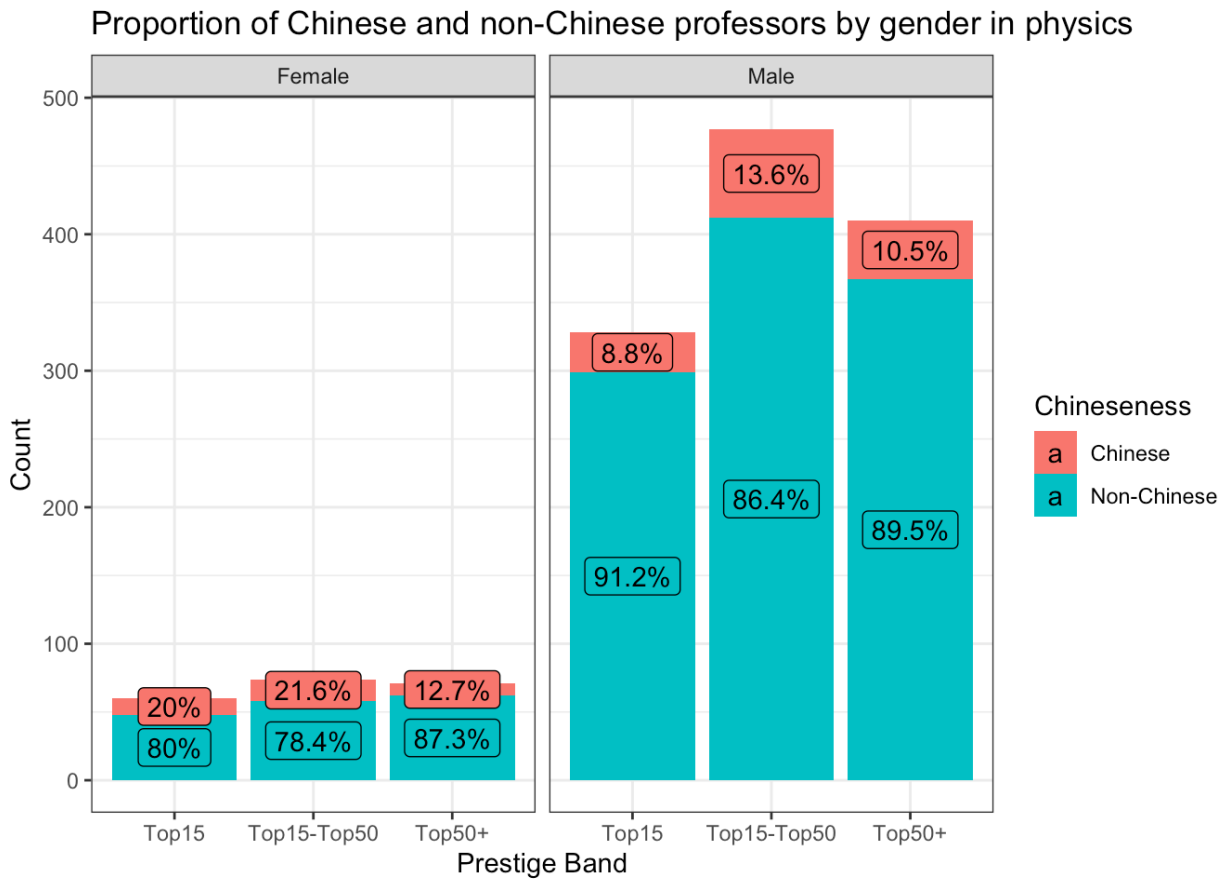


Figure S4: Proportion of Chinese and non-Chinese professors by gender in physics

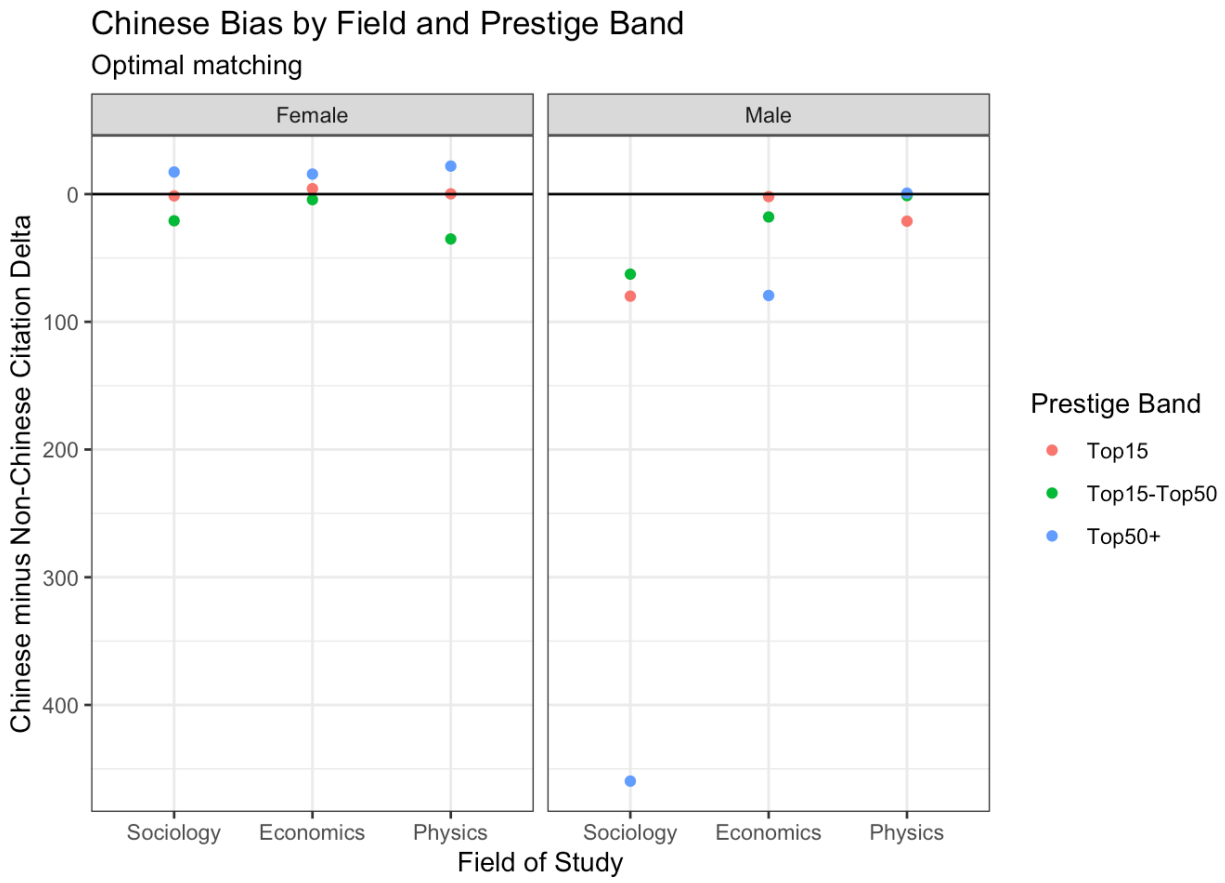


Figure S5: Robustness analyses – optimal matching

CONCLUSION

The papers in this dissertation seek to answer long-standing questions of interest to sociologists of science past and present. The first two papers revisit old debates on the relationship between the hard-soft distinction and consensus in science, a topic of spirited exchanges among the sociology of sciences until the 1980s, and stratification in sociology itself, an overlooked but exciting line of inquiry among lesser-known sociologists who worked from the 1960s to the 1980s. The third paper applies methodological and theoretical approaches from the first two papers to investigate a topic of urgent importance in the present, namely discrimination against Chinese scientists.

The elephant lurking in the room of this dissertation is the question of what science actually is. Is it a social construct with no grounding in empiricism? Is it a cultural construct? Is it a method for discovering essential and natural truths? I contend that it is all three. There are explicit and unalienable truths in the natural world, and science broadly construed is, so far, the best method we have for uncovering these truths. Many scholars of science do not discuss this aspect of science often, and Latour and Woolgar consciously dimmish the role of natural truth in their work, but it surely exists. Without reference to natural truth, scientists could not have invented electricity would not run through the device on which I am typing this, but science is not just a simple cataloguing of facts and figures.

Science is also a cultural construct that emerged in a very particular place and time, and it explicitly and implicitly serves as a vessel for the most salient features of the culture that birthed it. The emphasis on independence and objectivity in the many framings of the value system of modern science reflects the values Northern European Enlightenment (Merton 1974; Shapin 2008; Engeberts 2022; Hajek, Paul, and Ten Hagen 2024). In some readings, the notion that singular natural truths exist in the ether for humans to find is a transposed copy of the Christian

idea that there is one true God knowable to every human on the earth (Hooykaas 1972; Ferngren 2002; Numbers 2009).

On top of the enduring natural truths that lie at the core of science and the durable cultural construct that guides the discovery and accumulation of these truths is a set of social structures that scientists must navigate: universities, labs, conferences, and departmental politics. It is these ephemeral institutions that have been the focus of the ethnographers of science (Latour and Woolgar 1986; Vertesi 2015).

These three components operate on differing timescales. The deep truths of science emerge between billions and thousands of years, the cultural core of science is a few hundred years old, and the immediate social structures of science are reborn on the scale of years and decades. All three are equally important to the healthy conduct of science, and, as of this writing, there is no work in the sociology of science that presents a unified understanding of each of these three components and their many interactions.

An implicit thread throughout all three of these papers is the role of culture in science. Of the three components of science detailed above – natural truth, culture, and social structures – the least researched is culture. One may argue that the ethnographers of science have studied this in some depth, but another reading of their work is that it focuses primarily on the social construction of science (Knorr-Cetina 1981), which is not necessarily the same thing as studying the culture of science or studying science as a culture.

Further complicating this is that the cultures of science are layered. The cultural core of science rests at the heart of every discipline, but each discipline maintains its own subculture. All three chapters of this dissertation explored this topic in some depth; the first chapter compares indirect measures of culture across 9 fields, the second chapter explored one field at length, and

the third chapter of this dissertation compares disciplinary cultures to one another a set of metrics outlined in the second chapter. Yet another complication to understanding the cultural core of science is that the disciplinary cultures that compose science interact with national cultures outside of science, and the social institutions that collectively form the venues in which science is conducted are the main arenas for this interaction. This is not to say that only Europeans can practice science. It is immediately obvious that that is not true, and, in fact, the three papers in this dissertation provide direct evidence that non-Europeans can and do participate in science. It is to say that, as science continues to globalize, its cultural core will continue to be exposed to other folkways. Viewing science as a cultural construct may help scholars of science better understand how global science has evolved and will continue to evolve, and it may also help scientists and non-scientists alike how to fix science.

Fixing science may unfortunately be urgent. It is an open secret that science is in a state of disrepair, and it is mired by fraud, replication crises, and various forms of rent-seeking behavior (Ritchie 2020). It is anyone's guess as to how or even if science will ever be fixed, and it is difficult to feel any urgency regarding the grand task of fixing science when scientists continue to produce incredible gizmos and gadgets, including but certainly not limited to large language models and smartphones. If, however, science emerged in a particular cultural context and may be thought of as a cultural construct, as I and others have argued, then perhaps understanding the culture of science and the science of culture will play a part in healing science.

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