NORTHWESTERN UNIVERSITY

Matthew Effect in Market Competition: Causal Effect of Status and Implications for Strategy

A DISSERTATION

SUBMITTED TO THE GRADUATE SCHOOL

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS

for the degree

DOCTORAL OF PHILOSOPHY

Field of Management and Organizations

By

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EVANSTON, ILLINOIS

September 2020

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Abstract

In this dissertation, I combine quasi-experiments and computational tools with large-scale data in new ways to address questions that revolve around the Matthew Effect of status. My dissertation is a collection of four empirical papers on status at both the organizational and the individual levels. I employ two distinct empirical settings that include both large public corporations and the market for professional labor in academia. First, a long line of research on status has focused on product market competition. My dissertation departs from that by studying the market for input factors, and the labor market in particular. Additionally, past research has focused on how status can bring unearned benefit and cumulative advantage to actors. I am to advance this line of research by looking at how we might challenge and limit the Matthew effect of status, which has far reaching implications for the persistence of inequality. Empirically speaking, I aim to identify the causal effect of status in ways that prior research has been unable, by exploiting artificial breakpoints in status hierarchies produced by influential third parties like the media.

In the first chapter, I provide empirical evidence that one factor shaping firms' employment-related corporate social responsibility (CSR) is organizational status, this is due to benefits of employee engagement that come with both organizational status and CSR. To causally identify the effect of organizational status, I use a regression discontinuity design (RDD) in the context of Fortune 1000 rankings. Specifically, I argue that the construction and evolution of the ranking makes the 500th rank position an artificial breakpoint in status when quality follows a smooth distribution near the cut-off. Implementing RDD in this context, I find that firms just failing to make it into the high-status Fortune 500 category have significantly

better performance in employment-related social responsibility, as compared to firms just in the high-status category. One implication of this study is to establish the labor market benefit of organizational status.

In the second chapter, I argue that actors may manage the implications of status by managing their information environments proactively. Specifically, as information asymmetry is a key enabling condition for the Matthew Effect of status, I argue that enabling (or disabling) information asymmetry may be one way whereby actors may manage the consequences stemming from their status positions. Implementing RDD in the Fortune 1000 context, I test this hypothesis by investigating companies' financial information environment and how they communicate their earnings information.

In the third chapter, I study how actors respond to multiple status hierarchies in the context of academia, where past research has theorized a poignant paradox between intra-professional status and public status for scientists. Combining large scale data with an instrumental variable estimator, my findings suggest that public status conferred by the media increases scientists' commitment to the topics covered by the media. And the effect is more pronounced among scientists who are of relatively low intra-professional status. My findings also have implications for the broader research on the use of non-pecuniary incentives in motivating professionals and scientific development.

Despite much organization effort that aims to remedy for inequality, the persistence of racial inequality has sparked recent scholarship on the unintended consequences of these efforts. In the fourth chapter, I theorize a paradox of diversity promotion whereby minorities are more likely to be allocated to positions that help embody diversity for the organizations. This may

have inadvertent effects on minorities' career development. I investigate this paradox in the context of academia by constructing rich personnel data for faculty from a public university. The results demonstrate that efforts to increase the representation of minorities leads to an increased prevalence of hiring minority assistant professors as joint appointments. This outcome is important as we further find that joint appointments overexpose new faculty members to a set of risks that can negatively affect their research productivity and career advancement. Together, the results extend research on allocative sources of inequality and highlight an unintended cost of diversity efforts related to the hiring process.

Acknowledgements

I am deeply indebted to the members of my dissertation committee, Brayden King, Ned Smith, Brian Uzzi, Jillian Chown, and Jeannette Colyvas. I am grateful to Brayden King for his generous and outstanding advising. Brayden's research and advising has deeply shaped my approach and identity as a researcher. I truly appreciate his kindness, patience, and encouragement to me. If I were to become a mentor down the road, it is his model that I wish to follow. I have been fortunate to have Ned Smith as my mentor. It is a tremendous privilege to have the opportunities to work closely with Ned on research projects and teaching. I owe my growth as a scholar to his patience, brilliance, and inspirations. I would also like to give special thanks to Brian Uzzi for his guidance, encouragement, and inspirations. To work with Brian is a dream come true for me and it has also provided me with the opportunities to learn from an incredible scientist. I am also indebted to Jillian Chown for her generous help and job market advice, and to Jeannette Colyvas for her generosity and insightful guidance.

My dissertation has benefited from the discussions at the Alliance for Research on Corporate Sustainability Conference (2019); Strategic Management Society Annual Meeting (2018); Wharton People and Organization Conference (2017, 2018); Academy of Management Annual Meeting (2017, 2018); International Conference on Computational Social Science (2018); European Group for Organizational Studies (2017).

Finally, I would like to thank my friends from the Management and Organizations department and Northwestern Institute for Complex Organizations (NICO): Luke, Julia, Daniel, Kaylene, Andrea, Grace, Shane, Hui, Haochi, Lu, and Yang.

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Introduction

In an era of complex and radical technological and social changes, organizations and decision makers face an increasing need to understand the way complex social structures affect their competitive environments and outcomes. To that end, this dissertation aims to study (1) how social structures—such as organizations and individuals' status positions in a market—affect strategies and behaviors, and (2) how the increasing relevance of an ever-growing range of stakeholders and audiences amplifies and interacts with the effects of status hierarchies. These overarching inquiries have naturally led me to conduct my dissertation research on status at both the organizational and the individual level in two distinct empirical settings that include large public corporations and the market for professional labor in science.

Guided by these overarching questions, my dissertation is a collection of four empirical papers that leverage a combination of econometric and computational tools to address new questions that aim to advance the long line of research on status. Specifically, I collect large-scale data and employ quasi-experiment designs (e.g. regression discontinuity; instrumental variable regression) to describe and identify new patterns and mechanisms pertinent to my research interests. In addition to using public datasets in new and creative ways, this dissertation also analyzes several primary datasets. In this way, this dissertation combines sociological insights on status with the availability of new empirical tools to address questions related to organizational strategies and professional behaviors in the contexts of labor market, financial market, and science and academia. In what follows I briefly introduce the chapters and highlight several intended contributions to the literature on status and discuss broader implications for research related to strategy in a variety of contexts.

Chapter Abstracts

In "*Benefits in the Labor Market*" (**Chapter 1**), I provide empirical evidence that one factor shaping firms' employment-related corporate social responsibility (CSR) is organizational status, due to benefits of employee engagement that come with both status and CSR. To causally identify the effect of organizational status on an organization's commitment to socially responsible behaviors, I utilize a regression discontinuity design (RDD) in the context of Fortune 1000 rankings. I argue that the construction and evolution of the ranking renders the 500th rank position an artificial breakpoint in status when quality follows a smooth distribution near the cut-off. Implementing RDD in this context, I find that firms just failing to make it into the high-status Fortune 500 category have significantly better performance in employment-related social responsibility, as compared to firms just in the high-status category. An important implication of the finding is that corporate social responsibility provides non-pecuniary value to employees that constitutes a substitute for organizational status, and firms may invest in employment-related social responsibility in order to offset their status disadvantage relative to higher status competitors.

In "*Matthew Curse*" (**Chapter 2**), I draw on the growing insights of the downside of having status and propose that organizational status affects firms' information environment by increasing the scrutiny from financial analysts—a key audience for public companies (Davis 2009). As status leads to presumptions of better quality, I further propose that high-status companies may seek to manage heightened expectations from financial analysts via strategic communication. Given extensive regulatory constraints in this context, companies have limited discretion in *what* they communicate. I test my hypothesis by investigating their communication

styles—i.e. *how* they communicate earnings information. To that end, I use a similar research design as the previous chapter which exploits a regression discontinuity design (RDD) in the Fortune 500/1000 context. Around the 500th cut-off, I find that companies that just make it into Fortune500 have significantly greater amount of analyst attention. Analysis carried out with earnings guidance data finds that status affects the level of ambiguity in their earnings guidance (e.g. point estimate, interval estimate, open intervals) contingent on the valence of the earnings information. The key implication of the finding is that companies adjust their communication styles to manage audience expectations corresponding to their status positions. While prior work has treated the information environment as an important contingency for status effect, this chapter points to a novel observation that companies may take strategic actions to enable or prevent the implications of status by affecting their information environment proactively.

In "*Public Status and Professionals*" (**Chapter 3**), I study whether and how actors negotiate the space among multiple status hierarchies. Prior work on status tends to focus on the dynamics within one status hierarchy (Sauder, Lynn and Podolny 2012), while individuals and firms are faced with a growing multiplicity of stakeholders who may enact multiple status hierarchies that are different yet fateful at the same time. I address this question in the context of the academia, where there is a poignant paradox between intra-professional status and public status for scientists (Abbott 1981). Combining large scale data on scientists' research outputs and their respective media attention from 2010 to 2015 with an instrumental variable estimator, my findings suggest that public status conferred by the media increases scientists' commitment to the topics covered by the media. And the effect is more pronounced among scientists who are of relatively low intra-professional status as measured by widely used proxies such as institution

prestige and cumulative impact. As media attention tends to follow immediately after the time of publication, my research design leverages the potential that homerun papers by other scientists (measured as the top ten most cited papers per year), may coincide with a focal scientist's paper at random; this can ultimately result in an underreporting of the focal scientist's research due to a co-occurrence with an unpredictable homerun paper that monopolized media coverage. As I find that public status affects scientists' commitment to previous topics, my findings also have implications for the broader research on the use of non-pecuniary incentives in motivating professionals and scientific development.

In "*Paradox of Promoting Diversity in Higher Education*" (**Chapter 4**), I study organizational efforts to dismantle the cumulative advantage of status related to race at the individual level. Drawing on research on allocative inequality in the workplace, we theorize a paradox of diversity promotion as a novel mechanism for unintended consequences of organizational efforts to increase racial diversity. We choose to investigate this paradox in the context of higher education. Constructing rich personnel data for a university, the study demonstrates that efforts to increase the representation of underrepresented minorities in faculty positions leads to an increased prevalence of hiring minority assistant professors as joint appointments. This outcome is important as we further find that joint appointments overexpose new faculty members to risks that can negatively affect their research productivity and career advancement. Together, the results extend research on allocative sources of inequality and highlight an unintended cost of diversity efforts related to the hiring process. As higher education organizations are leaders in the efforts to achieve diversity, studying this context offers a strong test for the idea that efforts to increase the representation of minorities may paradoxically

handicap the career advancement for the very individuals who embody diversity for the organizations.

CHAPTER 1. Effect of Organizational Status on Employment-Related Corporate Social Responsibility: Evidence from a Regression Discontinuity Approach [1]

ABSTRACT

This chapter provides empirical evidence that one factor shaping firms' employment-related corporate social responsibility (CSR) is organizational status, due to benefits of employee engagement that come with both status and CSR. To causally identify the effect of organizational status on an organization's commitment to socially responsible behaviors, I utilize a regression discontinuity design (RDD) in the context of Fortune 1000 rankings. I argue that the construction and evolution of the ranking renders the 500th rank position an artificial breakpoint in status when quality follows a smooth distribution near the cut-off. Implementing RDD in this context, I find that firms just failing to make it into the high-status Fortune 500 category have significantly better performance in employment-related social responsibility, as compared to firms just in the high-status category. An important implication of the finding is that corporate social responsibility provides non-pecuniary value to employees that constitutes a substitute for organizational status, and firms may invest in employment-related social responsibility in order to offset their status disadvantage relative to higher status competitors.

[1] This chapter is under revision in collaboration with Edward B. Smith and Brayden G. King.

INTRODUCTION

Given the cost, why do companies increasingly engage in corporate social responsibility (CSR)? Existing research provides mixed evidence as to the financial returns associated with engaging in socially responsible behavior, suggesting that the factors associated with improved social responsibility have as much to do with the social environment of the workplace as they do with profitability (e.g., Turban and Greening 1997; Barnett and Salomon 2006; King 2008; McDonnell and King 2013; Flammer 2015a). In other words, being socially responsible may create non-pecuniary benefits for employees who want to work for a company or organization that is known for doing good things (Bode, Singh and Rogan 2015; Flammer and Luo 2017), making it easier for such organizations to recruit, retain, and reward high-value employees.

In this respect, having a reputation for being socially responsible may create similar employment-related benefits as those associated with possessing positive organizational status (e.g. Podolny 1993; Bidwell *et al.* 2015; Rider and Tan 2015). According to Podolny's (1993) status-based model of market competition, because employees value status, high-status organizations should face lower costs to recruit, employ, and retain employees, compared to lower status competitors. The resulting cost advantages—coupled with an increased willingness to pay for high-status goods and services—create a human capital advantage for high-status organizations, as talented employees may even forgo pecuniary benefits in exchange for high-status affiliation (Phillips 2001; Bidwell *et al.* 2015; Rider and Tan 2015).

Inasmuch as organizational status and CSR are both associated with non-pecuniary employment benefits, we argue that organizational status may affect firms' investment in CSR. More specifically, we theorize that when deprived of status advantages, low-status organizations

must find ways to compete with high-status organizations for the best employees. Building a reputation for being socially responsible may offset differences in status and make a lower status organization more appealing than it otherwise would be. We draw on recent research on the relationship between CSR and employee governance (e.g., Turban and Greening 1997; Stewart *et al.* 2011; Doh, Stumpf and Tymon 2011; Bode et al. 2015; Carnahan, Kryscynski and Olson 2017; Flammer and Luo 2017; Gupta, Briscoe and Hambrick 2017) to propose that low-status organizations may therefore attempt to mitigate their status-based disadvantage by engaging in employment-related CSR behavior. As non-pecuniary benefits such as CSR become increasingly important as a lever in employee engagement, we seek to investigate whether the benefits of organizational status can be substituted for other non-pecuniary benefits.

Additionally, this paper also seeks to accelerate research on the *causal* effects of organizational status on firm behavior. Despite the ubiquity of status research over the last three decades, identifying the causal effects of status in the real world poses considerable empirical challenges. To start, it is difficult to differentiate the effects of status from quality, especially in cases where quality is uncertain and/or difficult to observe or quantify. Recent research utilizing new sources of data and innovative methods have made significant headway in this regard, but none that we are aware of have focused on identifying the causal effects of status in labor markets specifically (e.g., Simcoe and Waguespack 2011; Azoulay, Stuart and Wang 2013; Kim and King 2014; Malter 2014). The reasons for this relate to a second challenge associated with status research: the risk of reverse causality. In labor and employment-related research, in particular, hiring has long been proposed to be a conduit whereby status can flow from one organization to another (Podolny and Phillips 1996; Roberts, Khaire and Rider 2011; Rider and Tan 2015). One must therefore be cautious when interpreting the relationship between

organizational status and the ability to hire employees of a given quality and cost. For example, does organizational status beget high-quality employees, or do high-quality employees contribute to elevated organizational status? While it may be easy to say that both effects are true, it is far harder to isolate causality in one direction or the other.

To causally identify the effect of organizational status on employment-related CSR, we utilize a regression discontinuity design (RDD) in the context of Fortune 500 and 1000 rankings. Specifically, we treat the rank positions from 1 to 1000 as a continuous assignment variable and the Fortune 500 cut-off as an arbitrary diagnostic threshold. We argue that the evolution of the ranking, as well as its sole reliance on revenue as ranking criteria, renders the 500th rank position an artificial breakpoint. Empirically speaking, this breakpoint is useful as it creates a discontinuity in status—i.e., regardless of one's specific rank, being in the Fortune 500 is status enhancing compared to being in the Fortune 1000 only—at a precise location where quality differences can be assumed to follow an otherwise smooth distribution. To put it differently, there is no reason to believe ex ante that the 499th largest firm (by revenue) is notably different in underlying quality as compared to the 501st. Nevertheless, these two firms will vary in their status, as only one can claim membership in the coveted Fortune 500.[2] Implementing this research design, we find that firms just failing to make it into the Fortune 500 have significantly better reputations in employment-related CSR—which should logically reflect investment in employment-related CSR activities—as compared to firms just in the Fortune 500.

[2] Theoretically speaking, belonging to the Fortune 500 is akin to membership in a high status group or a group of actors that are differentiated by a characteristic that signals is attributed higher status in society (Berger, Cohen, and Zelditch 1972).

In what follows we introduce the theoretical reasons for labor market benefits of organizational status and develop our hypothesis regarding the relationship between organizational status and employment-related CSR. We then describe the empirical context where we implement RDD and our specific estimation strategies. Moving forward, we present our main estimation results and a series of robustness tests that address a number of alternative explanations. Finally, we consider the broader theoretical and practical implications of our findings in the discussion section.

THEORY AND HYPOTHESIS DEVELOPMENT

Labor market benefits of organizational status

While the effect of organizational status on labor market advantage calls for further empirical investigation (Rider and Tan 2015), there are a wide range of theoretical reasons to speculate a positive link between organizational status and employees' willingness to work. To start, existing research on status highlights two mechanisms whereby advantages of status arise (Correll *et al.* 2017; Sharkey and Kovács 2017). The first mechanism relates to how status may affect inferences of underlying quality (Podolny 1993; Salganik, Dodds and Watts 2006; Simcoe and Waguespack 2011; Azoulay *et al.* 2013; Kim and King 2014; Kovács and Sharkey 2014; McDonnell and King 2018). When decision-makers face difficulties with verifying quality, they may rely on signals from social standings such as status to form their evaluations. Consistent with Merton's depiction of the "Matthew Effect" (Merton 1968), a large body of empirical research has demonstrated that status leads to presumptions of higher quality (e.g. Salganik *et al.* 2006; Simcoe and Waguespack 2011; Azoulay *et al.* 2013; Kim and King 2014; Kovács and

Sharkey 2014; McDonnell and King 2018), which then prompts evaluators to allocate more resources to high-status actors (e.g. Berger and Webster 2006; Podolny 1993; Correll *et al.* 2011; Sauder, Lynn and Podolny 2012; Bothner et al 2011). Recent research highlights an additional mechanism whereby benefits of status arise. As Correll and authors (2017) point out, decision makers are often inter-dependent, and their success may depend on inferring the likely reactions of other decision-makers rather than the inference of underlying quality (Ridgeway and Correll 2006; Malter 2014; Correll *et al.* 2017; Sharkey and Kovács 2017). As the problem of third-order inference dominates judgements of quality when inter-dependence is high, decision makers tend to act on prevailing status beliefs in favor of high-status actors. Examples of status benefits in such decision contexts include conspicuous consumption (Veblen 1899; Malter 2014), gift giving (Sharkey and Kovács 2017), stock market reaction (Smith *et al.* 2016), and market intermediaries (Zuckerman *et al.* 2003).

The circumstances that are conducive to both mechanisms of status advantages are relevant in labor market contexts. Just as high-status affiliations can provide signals of superior human capital (Merton 1968; Rider and Tan 2015; Smith 2019), being affiliated with high-status organizations, via employment, can lead to better career opportunities in the future (Merton 1968; Podolny 1993; Podolny 2001; Phillips 2001; Bidwell *et al.* 2015; Rider and Tan 2015). First, hiring decisions involve significant information asymmetries and uncertainty (Spence 1973). As the quality of an applicant is difficult to determine ex ante, the status of the applicant's prior employer can serve as an important signal which influences the employer's evaluation of a candidate (e.g., Podolny 1993; Rider and Tan 2015). Additionally, a considerable fraction of hiring decisions are made through labor market intermediaries and involve third-order inference (Zuckerman *et al.* 2003; Bidwell and Fernandez-Mateo 2010; Fernandez-Mateo and King 2011;

Fernandez-Mateo and Fernandez 2016). In these mediated labor markets, the intermediaries are shown to act on conventional status beliefs more than the clients themselves do, as they seek to infer their clients' preferences (Zuckerman *et al.* 2003; Fernandez-Mateo and King 2011) and alleviate accountability in the event of failures (Uzzi and Lancaster 2004; Jensen 2006; Correll *et al.* 2017). Moreover, as organizations may attempt to gain status by hiring the former employees of high-status organizations, such decisions further amplify the benefits of working for a high-status organization (Podolny and Phillips 1996; Roberts *et al.* 2011; Bidwell *et al.* 2015; Rider and Tan 2015).

In light of these benefits of organizational status in the context of labor markets—specifically hiring and employee retention—we theorize that employees should have a greater willingness to work for high-status organizations relative to lower status organizations (see Phillips 2001; Bidwell *et al.* 2015; Rider and Tan 2015).

Non-pecuniary incentives and CSR

A growing literature warns that pecuniary incentives alone may be ineffective and, in some circumstances, may even work counter to organizations' goals (e.g., Akerlof and Kranton 2005; Bergstresser and Philippon 2005; Burns and Kedia 2006; Nickerson and Zenger 2008; Larkin, Pierce and Gino 2012; Frank and Obloj 2014; Larkin 2014; Hur and Nordgren 2016; Flammer and Luo 2017). Such unintended consequences associated with the use of pecuniary incentives include attentional fixation on compensation instead above and beyond work-related tasks (Hur and Nordgren 2016; Coff 1997; Flammer and Luo 2017), increases in employee gaming and unethical behavior (Bergstresser and Philippon 2005; Burns and Kedia 2006; Frank and Obloj

2014; Larkin 2014), decreases in employee satisfaction (Zenger 1992; Pfeffer and Langton 1993; Nickerson and Zenger 2008; Larkin *et al.* 2012; Lazear 1989).

The limitations of financial incentives have sparked discussions that emphasize the importance of employees' preferences for relational incentives and non-pecuniary benefits (e.g., Flammer 2015b; Campbell *et al.* 2012; Bode *et al.* 2015; Flammer and Luo 2017; Rider and Tan 2015; Carnahan *et al.* 2017; Burbano 2016; Gubler, Larkin and Pierce 2018). Because investment in non-pecuniary benefits such as CSR initiatives may require more firm-specific commitment and tend to be less imitable thereafter, it is also argued that non-pecuniary incentives may provide companies with more sustainable competitive advantage when it comes to employee governance (Flammer and Luo 2017). In particular, a growing literature points to the role of CSR as a lever in employee engagement (Turban and Greening 1997; Brammer *et al.* 2007; Doh *et al.* 2011; Stewart *et al.* 2011; Bode *et al.* 2015; Carnahan *et al.* 2017; Gupta *et al.* 2017; Flammer and Luo 2017). For example, Gubler and coauthors documented a significant increase in worker productivity brought by corporate wellness programs, which is an integral part of employment-related CSR (Gubler *et al.* 2018).

These findings suggest that companies may engage in more employment-related CSR when they are facing more challenges in employee management. This is particularly relevant to low status firms, which do not have the same advantages as high status companies in attracting, recruiting, or retaining employees. Lacking the labor market benefits of high status companies, low status companies may then seek to offset this competitive disadvantage by investing in improving employment conditions. Employment-related CSR involves embracing positive employment practices, such as human capital development, while also avoiding negative labor practices, such as employing sweatshop labor. Maximizing the good while minimizing negative

employment practices sends a signal to employees that the company cares about employee welfare and is willing to incur costs in order to "do the right thing" for its employees. Importantly, having a reputation for good employment-related CSR is not without costs. Emphasizing and investing in employee welfare creates additional labor costs in its supply chain. Therefore, building a reputation as a good employer requires real investment on the firm's part. For this reason, we believe that firms with incentives to offer non-pecuniary benefits to their employees will be more likely to seek to build a reputation in employment-related CSR,and this will be especially true for firms that lack sufficient status. Accordingly, we propose our hypothesis as follows.

Hypothesis: Ceterus Paribas, Low-status companies will engage in more employment-related CSR than high-status companies.

EMPIRICAL CONTEXT

The context where we test our hypothesis derives from the Fortune 500/1000 ranking. In several respects, the unique history of the Fortune 500/1000 ranking provides a promising empirical setting where we can apply a sharp regression discontinuity design (RDD) to examine the causal effect of organizational status on employment-related CSR. In 1955, the Fortune magazine began to rank all US public companies by yearly revenue and to publish the list of the 500 largest companies as the US business elites. In its earlier years, the Fortune 500 ranking consisted almost entirely of manufacturing companies. With the rise of the service sector overtime, Fortune started to publish a separate "Service 500" ranking in addition to the traditional "Manufacturing 500" in the 1970's. In 1995, Fortune redefined the ranking to be the Fortune

1000 ranking that listed the top 1000 US companies by their revenue sizes regardless of sectoral distinction (Deile 2003).

First, we argue that Fortune confers a high-status distinction on companies ranked within the Fortune 500 category. Although nominally a ranking of the largest companies, the Fortune 500 has become an important status marker in the business community. The reason of this relates to the important role that third-party information intermediaries play in the construction of status beliefs, to which decision-makers tend to conform when they seek to better coordinate with others (Correll *et al.* 2017; Sharkey and Kovács 2017). While status beliefs can emerge from local interactions (Ridgeway 1991; Ridgeway *et al.* 1998; Webster and Hysom 1998; Troyer 2003; Ridgeway 2006), an integral premise for status beliefs to become actionable and fateful is the formation of common knowledge where everyone knows that everyone shares the belief (Centola, Willer, and Macy 2005; Zuckerman 2010; Correll *et al.* 2017). In other words, publicity is key for the status beliefs to be consequential, and prominent third parties such as the Fortune magazine may have significant impact on the construction of status beliefs as they produce common knowledge of status hierarchies using influential rankings and prizes (Sauder 2006; Lynn, Podolny, and Tao 2009; Sauder *et al.* 2012; Espeland and Sauder 2016; Correll *et al.* 2017; Sharkey and Kovács 2017; Bowers and Prato 2018). The Fortune ranking is one of the first business rankings in history, and the Fortune magazine has made immense efforts to publicize the Fortune 500 such that the category itself has gained a profuse amount of publicity and status over the last half century. As organizations derive part of their identity through membership in categories (Zuckerman 1999; Smith 2011; Sharkey 2014), being a member of the Fortune 500 confers a categorical distinction that is similar in function to that which social psychologists initially described as a "status characteristic" (Berger et al. 1972). If an

organization is ranked, it implicitly belongs to a high-status group and this serves as a distinguishing characteristic that elevates it above other organizations (Sauder *et al.* 2012; Sharkey 2014; Bowers and Prato 2018).

Second, we argue that there exists a discontinuity in status conferred on companies that are just above and below the 500th position (i.e., just in versus just out of the coveted Fortune 500) while revenue (i.e., the underlying statistic determining the Fortune ranking) follows an otherwise smooth distribution around this threshold. As research on rankings has demonstrated, rankings create an artificial distinction between organizations, amplified by the publicity that a ranking receives, that cannot be fully reduced to quality differences (Espeland and Stevens 1998; Carruthers and Stinchcombe 1999; Espeland and Sauder 2007; Sauder and Espeland 2009; Espeland and Sauder 2016; Correll *et al.* 2017). As Merton (1968) notes in his observation of "the 41st chair", rankings generate a discontinuity in status around the endpoint of membership in the ranking, despite a smooth and continuous distribution of quality around the artificial endpoint. This pertains to the 500th position in the Fortune ranking in particular. As the Fortune magazine only began to extend the ranking to Fortune 1000 forty years after Fortune 500 had been established, it is likely that the Fortune 500 category continued to garner more publicity and prestige even with the presence of a longer list. As Figure 1 illustrates, the number of media references to the Fortune 500 remains about four times higher than that of Fortune 1000 after 1995[3]. Among other hundredth rank positions, the Fortune 100 has even lower publicity than the Fortune 1000, while others seem largely negligible. This is consistent with the anecdotal observation that the Fortune 500 used to be publicized as one category of prestigious companies

[3] Source: Business Source Premier

while their respective rank positions mattered less, such that Fortune 500 became a meaningful category imbued with status relative to other potential Fortune rankings (Rosch 1978). It follows that as the public's attention focuses on membership in the Fortune 500, the 500th position marks an artificial endpoint of status. In this way, the distinction in status around the Fortune 500 threshold is disproportionate to that of underlying quality between those who are just above and just below the 500th threshold. Therefore, we can employ a sharp RDD with the rank positions in Fortune 1000 as the assignment variable and the 500th position as the diagnostic threshold[4].

[Insert Figure 1 here]

We further contend that companies are unlikely to control and manipulate their rank positions with precision in the Fortune 500/1000 ranking. Because the Fortune ranking is determined by counting the top 500/1000 US companies according to company revenues alone, where a company ends up depends on its relative standing across the full spectrum of companies and industries in a given fiscal year. Precise strategic manipulation is therefore unlikely as it entails detailed knowledge of all other companies' performance before their financial data become available. Furthermore, Fortune's simple and straightforward ranking method leaves little room for manipulation on behalf of companies or Fortune itself. Based on these reasons, we argue that RDD is well suited for the research setting.

DATA AND SAMPLE

[4] It is also worth noting that the Fortune500 cut-off is the mid-point in the complete Fortune ranking, which alleviates concerns about nuances to companies that appear at the bottom of a ranking (Lewis and Chad 2018).

Our analyses are based on data from 1995 to 2013 and were obtained from the Fortune magazine, Compustat, and MSCI ESG STATS KLD (KLD) databases. We begin with 1995 because this is the year that Fortune magazine began to publish the full Fortune 1000 ranking, meaning 1995 is the earliest year for which our estimation method can be used. Next, we measure employment-related CSR using the number of strengths in the employment relations dimension of the KLD data (e.g. Flammer and Luo 2017). The KLD data provide ratings of corporate social performance across a variety of dimensions including community, corporate governance, diversity, employment relations, environment, human rights, and product quality. As we are interested in employment-related CSR specifically, we focus on the employment relations dimension of the KLD data, which covers specific areas including union relations, cash profit sharing, employee involvement, employee health and safety, supply chain labor standards, human capital development, labor management, and controversial sourcing, among others. Appendix 3 includes a full list of categories and definitions (MSCI 2015). The KLD database provides the most comprehensive coverage of firms' CSR performance that extends back to 1995 when the Fortune 1000 ranking first became available. The extensive coverage is particularly important for our empirical strategy, as the RDD identification requires having dense distribution of observations in a local area near a focal threshold. KLD is the most widely used database for academic research related to CSR. Our use of KLD, however, differs from the majority of that existing research. Rather than assuming the KLD data to represent objective measures of employee responsibility, we treat the data as a proxy measure of the amount of effort (and consequently the cost) expended by a company to improve its reputation in that area of employee-related CSR. We assume that increases in a company's employee-related CSR reputation can function to improve its attractiveness to current and potential employees.

The KLD database does have limitations. For example, several researchers have expressed concerns about how well KLD ratings reflect CSR performance and caution against the practice of aggregating KLD scores (Mattingly and Berman 2006; Chatterji, Levine and Toffel, 2009; Kacperczyk 2009; Flammer and Luo 2017). Accordingly, we derive our main measure of employee-related CSR without aggregation and use the number of strengths from the employment relations dimension only. Importantly, our results are robust to alternative measures that have been used in prior research, including the sum of the number of strengths in employment relations and workforce diversity dimensions (Flammer and Luo 2017), and the net scores that subtract the number of concerns from the number of strengths in the employment relations dimension (e.g., Chin, Hambrick, and Treviño 2013). We further examine the specific subcategories in the employment-related dimension in the robustness check section.

We further include a number of control variables including industry fixed effects, year fixed effects, company return on assets (ROA, lagged), sales growth, book-leverage ratio, cash ratio, earnings-per-share, and the natural log of employment size. We obtained this information from the Compustat North America Fundamentals Annual database. After merging the Fortune ranking, KLD social ratings, and the Compustat data, the final dataset on which we draw for our analysis contains 12,225 firm-year observations from 1995 to 2013.

ESTIMATION STRATEGY

As the treatment of organizational status—here, being part of the fortune 500 or not—follows a deterministic and discontinuous function of rank positions in the Fortune 1000 ranking with the 500^{th} position as the diagnostic threshold, we implement a sharp regression discontinuity design

to estimate the effect of firm status on employment-related CSR, measured using the number of strengths in the employment dimension of KLD.

Parametric estimation

To estimate the size of the discontinuity at the Fortune 500 threshold, we first use a standard parametric method of regression discontinuity analysis that includes flexible functional forms of the assignment variable (e.g., Imbens and Lemieux 2008). Following convention, we pool the data from 1995 to 2013 and use robust clustered standard errors on the firm level to account for within-firm correlation of the errors overtime (Lee and Lemieux 2010). We specify the following model for firm i with Fortune rank position $rank_i$ in year t:

$$Employment\ CSR_i = \alpha_0 + \alpha_1 Fortune500_i + \alpha_2(rank_i - 500) + \alpha_3\ (rank_i - 500)^2 + \alpha_4\ (rank_i - 500)^3 + \alpha_5 Fortune500_i \times (rank_i - 500) + \alpha_6 Fortune500_i \times (rank_i - 500)^2 + \alpha_7 Fortune500_i \times (rank_i - 500)^3 + \alpha_t + \alpha_{ind} + \delta X'_i + \varepsilon_i$$

In this model, the dependent variable $Employment\ CSR_i$ measures firm i's employment-related CSR in year t using the number of strengths in the employment dimension of KLD. The variable $Fortune500_i$ is an indicator that firm i is ranked within Fortune 500 (from Fortune 1st to Fortune 500th). The variable $rank_i$ equals the companies' rank positions in the Fortune 1000 ranking, and $(rank_i - 500)$ measures the distance between the rank positions and the 500th threshold from either side of the cut-off. By subtracting the threshold of 500 from the rank positions, the assignment variable is normalized which renders α_1 the average causal effect in

RDD (Angrist and Pischke 2008, p. 193). To account for distances far away from the threshold, we include a number of polynomial functions of the normalized assignment variable ($rank_i - 500$). The interaction between the $Fortune500_i$ treatment indicator variable and the polynomials are also included to allow more flexibility and different trends on different sides of the Fortune500 threshold. Our choice of the order of polynomials is guided by the Bayesian Information Criterion (BIC) to alleviate concerns about overfitting when we include higher order polynomials of the assignment variable (Lee and Lemieux 2010). The BIC penalizes model complexity as its value increases with error variance and the number of parameters. When we select models according to BIC, the model that minimizes BIC value is preferred. For the main specification, we compared the models when we include polynomials that take on different orders from one to five, and the minimum BIC value is obtained when the 3rd order polynomial is included.

Additionally, we include a year fixed effect α_{t} and an industry fixed effect α_{ind}. In some specifications, we further include a set of covariates X'_i which include lagged ROA, sales growth, book-leverage ratio, cash ratio, EPS, and employment size (log). As we measure firm i's employment-related CSR using the number of strengths in the employment dimension of KLD data in the main specification, we estimate this model by a negative binomial regression estimator with robust standard errors clustered on firms.

Local linear estimation

We also estimated a non-parametric local linear regression without making assumptions about functional forms as a complement to the parametric estimation (Lee and Lemieux 2010). In this

estimation, we restrict our analysis sample to a smaller sample around Fortune500 discontinuity. Inherent to the choice of bandwidth is a trade-off between bias and precision. While using observations close to the cut-off reduces bias from influences by data points far-away from the cut-off, having more observations yields more precise estimates (Lee and Lemieux 2010). Following convention our choices of bandwidth were informed by our data. Specifically, we selected the pilot bandwidth of our discontinuity sample using a data-driven mean squared error optimal bandwidth which allows different bandwidths on different sides of the cut-off (MSE-TWO optimal bandwidth) (Calonico *et al.* 2017). The pilot bandwidth we obtained using this method includes 347 rank positions to the left of the Fortune500 threshold, and 298 rank positions to the right of the Fortune500 threshold. In addition to this pilot bandwidth, we report consistent results using a handful of alternative data-driven bandwidth selection methods in the robustness check section (Imbens and Kalyanaraman 2012; Calonico, Cattaneo, and Titiunik 2014; Calonico *et al*. 2017).

To account for industry and year fixed effects and within firm correlations of errors overtime in this estimation, we first fit a negative binomial model of the number of strengths in employment-related CSR on industry fixed effect and year fixed effect with standard errors clustered on the firm level. We then took the deviance residuals of this estimation, $resid\_emps_i$ as the dependent variable in our local linear estimation, where we used a triangular kernel that allows the weight of observations to decay with increasing distance from the threshold.

RESULTS

Table 1 summarizes the results of the main parametric estimation. The results support our hypothesis: the negative effect of Fortune 500 status on $Employment\ CSR_i$ is statistically significant at 5% level ($\hat{\beta} = -0.299, SE = 0.148$). All the models include the 3rd order polynomials of the assignment variable *Fortune-rank*, as well as the interaction terms of the Fortune500 indicator variable and the polynomials. Models (1)-(3) all control for industry fixed effects and year fixed effects and cluster the robust standard errors at the firm level. Model (2) adds covariates including lagged ROA, sales growth, book-leverage ratio, and model (3) further adds cash ratio, EPS, and log (employment size). Importantly, the coefficients and significance levels remain highly consistent as additional control variables are included from model (1) through model (3). Specifically, the point estimate in Model (2) suggests that Fortune500 status decreases the expected value of log ($Employment\ CSR_i$) by 30 percent.

[Insert Table 1 here]

Table 2 reports the result of the non-parametric local linear estimation, which are consistent in supporting our hypothesis. We found that Fortune500 companies have significantly lower $resid\_emps_i$ ($\hat{\beta} = -0.081, SE = 0.037$) controlling for industry, year fixed effects, and within firm correlation of errors overtime. The result confirms that companies just in the high-status category, Fortune 500, have lower performance in $Employment\ CSR_i$.

[Insert Table 2 here]

In addition, the relationship between employment-related CSR and the rank positions in Fortune 1000 is plotted to show the existence of a discontinuity. Figure 2 plots the relationship between $resid\_emps_i$ and *Fortune-rank* within the aforementioned MSE-TWO optimal discontinuity bandwidth near the Fortune500 threshold. We specified 5 bins on both sides of the Fortune500 threshold, and the diamond dots quantify the mean $resid\_emps_i$ within bins using triangular kernel. Each diamond dot quantifies the average of about 720 observations. The dotted line bars indicate the 95% confidence intervals (Calonico *et al.* 2017). As the figure shows, there is a visible discontinuity in $resid\_emps_i$ around the Fortune500 threshold, with companies just fail to make the Fortune 500 category attaining higher employment-related CSR[5].

[Insert Figure 2 here]

ROBUSTNESS TESTS

Testing for manipulation of the assignment variable

We assessed the robustness of our results in several ways. First, we tested for the continuity of the assignment variable. In our empirical context, the assignment variable *Fortune-rank* should in theory follow a continuous distribution as every rank position typically corresponds to one firm in each year. But due to the limited coverage of the KLD dataset, our final analysis sample is affected by missing data to some extent, which warrants further inspection into the continuity of the assignment variable. Specifically, missing data may constitute a threat to the validity of

[5] Additionally, figure A1 (Appendix 1) fits a locally weighted scatterplot smoothing, a non-parametric smoother on the full analysis sample. We specified 20 bins on both sides of the cut-off using bin width of 25 rank positions. Using this alternative method of visualization, we can also observe a discontinuity in $resid\_emps_i$ around the Fortune500 threshold.

our RDD estimation if they are systematically related to whether a company is a Fortune500 company, such that the assignment variable is *not* smoothly distributed around the Fortune500 threshold. We argue that this is unlikely the case, as the inclusion to the KLD social ratings data is based on the S&P500 index, the Domini 400 Social Index and (or) the top 1,000 and 3,000 U.S. companies by market capitalization instead of the Fortune ranking (MSCI 2015).

To directly check for evidence of discontinuity of the assignment variable *Fortune-rank* around the Fortune500 threshold, we implemented a standard test on the continuity in the distribution of the assignment variable *Fortune-rank*. Specifically, we implement the McCrary density test to investigate if there exists a significant difference in the density of observations above and below the Fortune500 threshold in our analysis sample (McCrary 2008). The test estimate is -0.014 with a standard error of 0.07, indicating that there is no significant difference in the density of observations around the Fortune 500th cut-off. Figure A2 (Appendix 2) visualizes the McCrary density test result.

As discussed previously, companies are unlikely to be able to manipulate the assignment variable *Fortune-rank* with precision in this empirical context. Nevertheless, to further consider possible manipulation of the assignment variable, we next examined whether there exist any pre-exiting differences in the covariates across the Fortune500 threshold. Table 3 compares the covariates of interest in the main parametric estimation on either side of the Fortune500 threshold, controlling for linear trends in the distance from the Fortune500 threshold. Model (5) in table 3 reports the comparison in the aforementioned MSE-TWO discontinuity sample. As the p-values indicate, none of the covariates have statistically significant differences on either side of the Fortune500 threshold within the MSE-TWO bandwidth. Model (6) in table 3 further reports

consistent results using an alternative discontinuity sample selected using cross validation (Calonico *et al.* 2014).

[Insert Table 3 here]

Bandwidth sensitivity

It is further important to assess whether our local linear estimation results are sensitive to bandwidth selection decisions. In addition to the MSE-TWO optimal bandwidth, we employed several widely used alternative data-driven bandwidth selection algorithms including IK (Imbens and Kalyanaraman 2012), CCT (Calonico *et al.* 2014), and cross validation (Calonico *et al.* 2014). Table 4 reports the non-parametric local linear estimation results using these alternative bandwidths. As the estimates in table 4 indicate, our result remains consistent as we use different bandwidths through model (7) to model (9).

[Insert Table 4 here]

Other placebo thresholds

We also tested our assumption that Fortune 500 is indeed a discontinuous threshold—related to an important status categorization—in the otherwise smoothly distributed Fortune 1000 ranking. To rest this assumption, we investigated whether any additional discontinuities are observable at other potential cut-off points. Specifically, we used the same parametric RD method as in our main specification and the same number of observations to test whether there are alternative discontinuity cut-offs at the Fortune 100, Fortune 200, Fortune 300, Fortune 400, Fortune 600,

Fortune 700, Fortune 800, and Fortune 900 levels. Figure 4 plots the coefficients and the 95% confidence intervals of the estimation using these thresholds. As Figure 4 demonstrates, no statistically significant effects are found at any other potential cut-off points. This provides further support that the 500th position marks a discontinuity in status in the otherwise smooth Fortune 1000 ranking.

[Insert Figure 4 here]

Non-pecuniary subcategories in employment-related CSR

We propose that organizational status affects companies' engagement in employment-related CSR because employees derive non-pecuniary benefits from both employers' status and their investments in CSR. In this respect, we expect firm status and employment-related CSR investment to act as substitutes. While the number of strengths in the employment-relations dimension provides an overall assessment of the companies' performance in employment-related CSR, this measure may also reflect employment CSR efforts that are also pecuniary. To disentangle the effects of non-pecuniary versus pecuniary incentives, we investigated specific subcategories relevant to employment-related CSR.

We first divided all the relevant subcategories of employment related CSR (Appendix 4) into pecuniary and non-pecuniary groupings. We classify the subcategories cash profit sharing, retirement benefit strengths, and employee stock ownership as pecuniary, while the other subcategories including employee health and safety, union relations, human capital development, supply chain labor standards, human capital-other strengths, employee relations, professional development, and work-life benefits as non-pecuniary. Each subcategory is a binary variable indicating if the company has a strength in that area in a given year. We then measured whether

the company provides pecuniary employee benefits and non-pecuniary employee benefits by taking the union of the subcategories in each group respectively. We then ran the same specification of model (3) in our main parametric estimation on the pecuniary benefits and non-pecuniary benefits indicators. Model (10) and (11) in table 5 reports the logistic regression results respectively. As model (10) indicates, we do not find statistically significant difference for companies just above and below the Fortune 500 threshold with respect to pecuniary benefits ($\hat{\beta} = -0.241, SE = 0.272$). In stark contrast, model (11) indicates that companies just failing to make the Fortune 500 threshold are significantly better in terms of non-pecuniary benefits than companies just in Fortune 500 at 1% significance level ($\hat{\beta} = -0.815, SE = 0.261$). In model (12), we further exclude supply chain labor standards from the non-pecuniary benefits indicator, to rule out supply chain bargaining power, and find results consistent with model (11) ($\hat{\beta} = -0.753, SE = 0.261$). The results provide strong support for our contention that non-pecuniary benefits underlie the effect of organizational status on employment CSR.

[Insert Table 5 here]

Other stakeholder categories

We have examined the effect of Fortune500 status on employment-related CSR. It is also pertinent to examine whether organizational status affects companies' engagement with CSR initiatives that focus on other stakeholder categories including environment, community, and consumers. To investigate this question, we employ the same estimation strategy as model (3) in the main parametric estimation, replacing the dependent variable to be KLD strengths related to

environment, consumers and community in models (13) to (15) respectively. As the results in table 6 indicate, we do not find statistically significant effects on other stakeholder categories at the 5% level. This result is consistent with Flammer and Luo (2017), which further accentuates the role of employment-related CSR in employee governance.

[Insert Table 6 here]

Alternative measures of employment-related CSR

Finally, we further examined our hypothesis using two alternative measures of employment-related CSR; including the net scores in the employment dimension of KLD and the sum of the number of strengths in the employment relation and diversity dimensions in KLD (Flammer and Luo 2017). Results are presented in Appendix 3. To ensure comparability, we implemented the same specification of model (3) of our main parametric estimation. Models (16) and (17) of table A1 report the estimates respectively and show consistent results with the estimates using the number of strengths in the employment relations dimension as our main measure of employment-related CSR.

DISCUSSION AND CONCLUSION

This paper provides empirical evidence that one factor shaping firms' investments in employment-related CSR is organizational status. Lacking the non-pecuniary employment benefits associated with status, low-status firms need to invest more in other intangible resources that provide psychological and social benefits to employees. Having a reputation for CSR is one

such asset. Inasmuch as a firm develops a reputation for being a good, "virtuous" employer, employees are likely to experience more commitment to the company (e.g., Flammer and Luo 2017). If companies fall outside of a status category, like the Fortune 500, they must do something to give themselves a competitive advantage in the labor market. We contend that investing in CSR is one possible strategy for creating a distinctive reputation that would help offset a firm's lack of status.

To empirically identify the effect of status on companies' engagement in employment CSR, we draw on sociological insights about the role that information intermediaries like the Fortune magazine play in the construction of status (e.g., Espeland and Sauder 2007; Lynn, Podolny and Tao 2009; Zuckerman 2010; Correll *et al.* 2017) to argue that the Fortune 500 cut-off brings discontinuous difference in organizational status disproportionate to differences in underlying quality near the cut-off. Using both parametric and non-parametric RDD estimation strategies, we found that firms just making it into the high-status Fortune 500 category have significantly lower performance in the employment relations dimension of KLD ratings. Though ratings may be susceptible to status biases, the potential bias is likely making our estimates conservative as extant status research shows that status leads to upward biases in favor of high-status actors in evaluation (Salganik *et al.* 2006; Simcoe and Waguespack 2011; Azoulay *et al.* 2013; Kim and King 2014; Kovács and Sharkey 2014; McDonnell and King 2018). The findings remain robust to a number of data-driven bandwidth choices and alternative measures of employment-related CSR. Our analysis of the non-pecuniary sub-categories in the employment relations dimension further suggests that considerations about non-pecuniary employee benefits underlie firms' efforts to improve reputation in employment-related CSR when they cannot provide status-based non-pecuniary benefits to employees. Importantly, in support of our

contention that Fortune 500 marks a discontinuity in the otherwise smoothly distributed Fortune ranking, we do not find significant difference in employment CSR around the placebo cut-offs at any other hundredth rank positions. Despite the important contributions we seek to make, this approach is not without limitations. While our theoretical proposition pertains to firms concerned with employee governance in general, we implement RDD in a local area near the Fortune 500 cut-off in the Fortune ranking where unambiguously there is a status difference. Although RDD is arguably "the sharpest tool for causal identification with observational data" (Flammer 2015a), an inherent weakness of this quasi-experimental design is limited generalizability. Future research may examine if the proposition holds in other contexts comprised of companies with different sizes or forms of ownership, and may also consider other strategic factor markets to advance the understanding of the strategic implications of organizational status in factor market competition.

Our study provides a richer window into the motivations behind CSR investments. An important implication of the analysis is that firms invest in employment-based CSR in order to offset their status disadvantage relative to higher status competitors. But beyond the effects of status competition, our analysis points to a more general insight about companies' CSR behavior: firms invest in CSR partly as a means to provide nonpecuniary benefits to employees as part of their employee governance strategy. Therefore, we should expect that firms will invest in CSR in situations when they need to develop a human capital advantage relative to competitors. This would apply in situations other than just status-based competition, including in industries in which competition for human capital is intense and in companies that have suffered a reputational crisis that has damaged their appeal to employees. One contribution of our paper, then, is to provide causal evidence of this strategic motivation for investing in CSR.

A related contribution is to suggest a strategic implication for companies in response to their status positions in factor market competition. Extant research on organizational status tends to focus on the impacts that status positions have on companies' market outcomes (e.g., Podolny 1993; Stuart *et al.*1999; Uzzi and Lancaster 2004; Roberts *et al.* 2011; Bothner, Kim and Smith 2012; Malter 2014; Smith 2019). While this literature significantly advances our understanding of the important role that social positions play in market success, the question remains as to how companies might address specific implications of status positions. This question is particularly important for actors that have relatively lower status who comprise the majority of a population, as the value of status as a positional good tends to be a function of the scarcity of high-status positions (Hirsch 1977; Podolny 2005; Sorenson 2014). Insofar as high status leads to favorable biases in evaluation of quality and third-order inference (Correll *et al.* 2017; Sharkey and Kovács 2017), the biases from status may result in a self-confirming cycle that's vicious for the low status actors in competitive environments (e.g., Merton 1968; Podolny 1993). Trapped outside the virtuous cycle where high-status companies reside, how might companies with relatively low status cope with the implications of their status positions? Our finding suggests one possible solution readily at the companies' disposal in the factor market of labor. We find that employment-related CSR may serve as a substitute for organizational status in providing non-pecuniary benefits that employees value. Lower status organizations can seek to offset their status disadvantage in the competition for labor by investing in employee benefits, as employees derive important non-pecuniary benefits from both affiliation with high-status organizations and employment-related CSR initiatives.

This paper further contributes to the literature on Podolny's status-based model of market competition in two aspects. First, this paper seeks to provide empirical evidence for the labor

market advantage of organizational status. The factor market advantages work in tandem with increased product market acceptance to result in the status-based profitability in market competition (Podolny 1993). While past empirical works have documented the status-based product market advantages in a variety of industries (e.g., Benjamin and Podolny 1999; Stuart, Hoang, and Hybels 1999; Uzzi and Lancaster 2004; Jensen 2006; Robert *et al.* 2011), empirical research has only recently started to examine the status-based cost benefits in factor markets (Bidwell *et al.* 2015; Rider and Tan 2015).

Furthermore, research on the labor market benefits of organizational status to date focuses on the trade-off between status and pecuniary benefits (Podolny 2001; Phillips 2001; Bidwell *et al.* 2015; Rider and Tan 2015). Yet growing research points to the limitations of pecuniary incentives (e.g., Larkin *et al.* 2012; Rider and Tan 2015; Flammer and Luo 2017). Research on employee governance has also highlighted the vital role of relational and non-pecuniary benefits in engaging and managing employees (e.g., Turban and Greening 1997; Brammer *et al.* 2007; Bode *et al.* 2015; Gupta *et al.* 2017). It thus calls for insights into the relationship between organizational status and other coveted non-pecuniary benefits to advance our understanding about the labor market implications of organizational status. Our finding suggests that high-status organizations also face lower demands for non-pecuniary benefits in the form of employment-related CSR to attract, retain and engage employees. To the best of our knowledge, this is the first attempt to empirically document the trade-off between having organizational status and the deployment of other forms of non-pecuniary incentives in employee governance.

CHAPTER 2. Matthew Curse: Heightened Expectations from Organizational Status and Strategic Communication

ABSTRACT

This chapter draws on growing research on the downside of having status and proposes that organizational status affects firms' information environment by increasing the scrutiny from financial analysts—a key audience for public companies. As status leads to presumptions of better quality, I further propose that high-status companies may seek to manage heightened expectations from financial analysts via strategic communication. Given extensive regulatory constraints in this context, companies have limited discretion in *what* they communicate. I test my hypothesis by investigating their communication styles—i.e. *how* they communicate earnings information. To that end, I use a regression discontinuity design (RDD) in the Fortune 500/1000 context. Around the 500th cut-off, I find that companies that just make it into Fortune500 have significantly greater amount of analyst attention. Analysis carried out with earnings guidance data finds that status affects the level of ambiguity in their earnings guidance (e.g. point estimate, interval estimate, open intervals) contingent on the valence of the earnings information. The key insight of the finding is that high-status companies may adjust their communication styles to manage inflated audience expectations corresponding to their status positions.

INTRODUCTION

Existing research on organizational status has substantially advanced our understandings about the important role of social and structural determinants of firm success in market competition. Consistent with the Matthew effect conceptualization, a long line of research has found that status may be associated with a myriad of unearned benefits in a variety of contexts (e.g., Podolny 1993; Podolny and Phillips 1996; Uzzi and Lancaster 2004; Roberts et al., 2011; Kim and King 2014; Rider and Tan 2015). However, by emphasizing the impacts of status on organizations, prior research on organizational status has inadvertently treated organizations as relatively passive actors in the process of evaluation, whereas a large body of research argues that organizations are strategic actors that engage in strategic behaviors to manage audience expectations (e.g., Gioia and Chittipeddi 1991; Elsbach, Sutton, and Principe 1998; Zajac and Westphal 1995; Wade, Porac, and Pollock 1997; Fiss and Zajac 2006; King 2008; Graffin, Carpenter and Boivie 2011; McDonnell and King 2013; Smith and Chae 2016). Admittedly, the cumulative advantage of status may not necessitate strategic actions on the part of high-status organizations as unearned benefits accrue.

However, recent research has pointed to more nuances and limits to the Matthew Effect of status (e.g. Rhee and Haunschild 2006; Malmendier and Tate 2009; Bothner, Kim, and Smith 2012; Hahl and Zuckerman 2014; Kovacs and Sharkey 2014; King and Carberry 2017; McDonnell and King 2018). Importantly, status may become a liability when it gives rise to heightened attention and inflated expectations about quality. First, inflated attention and scrutiny by external audiences may function as a constraining mechanism that limits high-status actors from reaping the unearned benefits that come with status (Wade et al. 2006; Fombrun 1996). Ascent in status may also produce inflated expectations and distort the composition of relevant

audiences which increases the difficulty to meet audience expectations (Kovacs and Sharkey 2014; Malmendier and Tate 2009). For example, Kovacs and Sharkey (2014) found that award-winning books experience a decrease in ratings as the award attracts people who would otherwise not be interested in the book to read and rate it. In this case, the composition of audiences changes unfavorably because of heightened attention and expectations due to a status boost. These risks of status are further compounded as high-status actors may experience harsher punitive measures when they fail to meet the audience expectations (Rhee and Haunschild 2006; McDonnell and King 2018; King and Carberry 2018). Specifically, McDonnell and King (2018) found that as high-status companies are expected to act more ethically, they experience harsher punishment when they are found guilty of discriminatory employment practices. While status leads to expectations of better quality, this line of research reveals that inflated audience expectations may constitute substantial challenges for the possessors of status.

In light of these findings that point to the negative implications of status, this chapter aims to advance research on whether and how might organizations respond to status implications with strategic actions to manage audience expectations. Existing research has found that companies seek to manage audience expectations through a variety of strategic behaviors that include strategic choice of names (Smith and Chae 2016), releasing noises to their information environments when appointing new CEOs (Graffin, Carpenter and Boivie 2011), and engaging in positive impression management when targeted by social movements (King 2008; McDonnell and King 2013). These findings have substantially improved our understanding of companies' strategic actions to manage audience expectations and their information environment, especially for events that are salient yet relatively rare. As I aim to identify the effect of organizational status on companies' strategic actions to manage their audience expectations, the ideal empirical

context involves salient events that are frequently occurring, so that I may obtain longitudinal data to disentangle the effect of status on companies' strategic behaviors.

To accomplish this aim, I study the context of company issued earnings guidance in the post Regulation Fair Disclosure period. Company issued earnings guidance refers to the management forecast about future earnings that companies disclose voluntarily to guide the market estimates about their future performance. With the rise of the shareholder value and the financialization of the economy in the past thirty years (Davis 2009; Davis and Kim 2015), Wall Street has become one of the most salient audiences to companies. Publicly traded companies are highly concerned about meeting analyst estimates when they report quarterly earnings. Many companies attempt to manage market expectations so that they have higher likelihood of meeting the prevailing market estimates by issuing guidance (management forecasts) about future earnings (Matsumoto 2002; Brown and Caylor 2005; Graham, Harvey, and Rajgopal 2005; Baik and Jiang 2006; Cotter, Tuna, and Wysocki 2006). Company issued guidance is a voluntary form of disclosure and tends to be a long-term strategy that involves the commitment from the highest level of management (Gibbons, Richardson and Waterhouse 1990; Brochet, Faurel and McVay 2011). Therefore, this context provides a promising empirical setting to examine the effect of organizational status on companies' strategic behaviors to manage their information environment corresponding to their status positions.

In what follows, I will develop the hypotheses with respect to the effect of status on companies' information environment and their strategic behaviors in the context of earnings guidance. Second, I will introduce the data and empirical strategy, followed by the estimation results and a number of robustness tests. Finally, I will discuss the findings and their implications for the broader literature on organizational status.

DEVELOPMENT OF HYPOTHESES

The rise of the shareholder value in the US economy in the past thirty years has produced profound changes in what public companies are primarily organized to accomplish (Davis 2009; Davis and Kim 2015). The maximization of shareholder value has become the first and foremost commitment for public companies, and the financial analysts who evaluate and publish regular forecasts about companies' financial performance have become one of the most salient external audiences that public companies need to satisfy (e.g. Zuckerman 1999). In the post Regulation FD period, public companies are required to report quarterly performance information to the public regularly. Because analysts have considerable impact on the financial market's reactions to public companies, meeting the analyst estimates has become a vital task and challenge facing managers at these public companies (Davis 2009; Davis and Kim 2015). Failure to meet the analyst estimates when they report earnings warrants punitive stock market reactions.

Insofar as status may function as an important structural cue that affect audience evaluation, it is pertinent to consider the role of organizational status in the context of the financial market. As noted previously, status may lead to the presumption of better quality and heightened audience expectations, attracting more audience attention at the same time. A key mechanism underpinning Merton's conceptualization of the Matthew Effect is the increase in the amount of attention and recognition that flows to works by high-status scientists (Merton 1968). According to Podolny (1993), one cost-saving benefit of organizational status arises because high status induces greater attention and publicity that reduces companies' costs associated with advertising. Recent research provides empirical evidence that status leads to an increase in audience attention (e.g. Merton 1968; Podolny 1993; Simcoe and Waguespack 2010; Azoulay, Stuart and Wang 2012; Kovacs and Sharkey 2014). Consistent with this line of literature, I

propose that companies are faced with different information environments corresponding to their status positions when it comes to audience attention in the financial market. First, I hypothesize that there exists a positive relation between status and the amount of analyst attention.

Hypothesis 1. *High-status companies receive more analyst attention.*

In addition to attracting greater analyst attention, organizational status may also lead to upward biases in market expectations. Using experiment and clever research designs, the recent literature on status has demonstrated that status leads to the presumption of better quality and favorable biases in audience evaluation (Kim and King 2014; Salganik et al. 2006). Despite a myriad of benefits that follow favorable audience expectations, heightened expectations from the financial analysts may be far from ideal for the managers of public companies. Specifically, optimistic expectations from the financial analysts are pertinent to the potential of upward biases in analysts' forecasts, inadvertently resulting in greater difficulties to meet prevailing market estimates from the companies' perspective. Such problems have been noted in previous literature. For example, Malmendier and Tate (2009) have found that companies may underperform after their CEOs win prestigious awards, in part due to inflated market expectations. Furthermore, past research also finds that status may induce harsher punishment in the wake of failures to meet expectations (Rhee and Haunschild 2006; McDonnell and King 2018). Put together, the cumulative advantage of status appears to put high-status companies at a particularly precarious position with respect to analyst estimates in the financial market. With increase in status, companies are faced with both inflated expectations and amplified pressures to meet these higher standards.

Faced with threats from the environment, companies are strategic actors that manage their information environment and audience expectations proactively (e.g. Wade, Porac, and Pollock 1997; Fiss and Zajac 2006; King 2008; Graffin, Carpenter and Boivie 2011; McDonnell and King 2013; Smith and Chae 2016). While existing research suggests that companies issue earnings guidance to manage market expectations, what might high-status companies do to manage the status-induced inflation in market expectations? In this context, companies are highly constrained. Because financial information contains considerable economic value, voluntary disclosure of information is highly regulated to ensure level ground for all investors. Furthermore, although one possible option is to artificially lower their guidance to adjust the market expectations, past research suggests that supplying inaccurate information may undermine the credibility of a company's guidance and handicap the long-term strategy of company issued guidance (Brochet, Faurel and McVay 2011).

While companies are constrained in *what* may communicate to a large extent, they may have some discretion over *how* they communicate certain information. I propose that one way whereby companies may manage the implications of status is to exploit ambiguity or precision in their communication of guidance. As contexts of uncertainty and information asymmetry tends to be conducive to the Matthew effect, information asymmetry is a key enabling condition for the effect of status (e.g. Podolny 1993; Kim and King 2014; Correll et al. 2017). By affecting the level of information asymmetry in one's information environment, companies may counteract the unfavorable implications from status. In this specific context, the market is predisposed to react more positively (less negatively) to positive (negative) guidance coming from high-status companies than low-status ones. To the extent that high-status companies may attempt to counteract the inflated expectations from prevailing market estimates, I propose that high-status

companies, relative to low-status ones, may adjust the level of ambiguity in their communication to manage market expectations.

In the context of earnings guidance, the level of precision of guidance is precisely one nuance that companies have at their disposal. Specifically, they may choose to issue guidance with four distinct levels of precision: from the most precise level of point estimate to intervals, open intervals and qualitative statement at the most ambiguous end. A body of research in accounting suggests that the level of ambiguity associated with communication affects the audience's reaction to the information and their subsequent expectations of the organization (Budescu, Weinberg and Wallsten, 1988; Budescu, Kuhn, Krammer and Johnson, 2002; Du and Budescu, 2005; Baginski et al. 2011). In light of these findings, I examine whether high-status companies strategically exploit communicative precision or ambiguity in their guidance to manage audience expectations.

Importantly, it is pertinent to consider the valence of the underlying information. Past research in finance demonstrates that companies may attempt to communicate positive or negative information in different ways because of subtle yet important heterogeneity in market reaction to the underlying news (Dellavigna and Pollet 2009; Cohen, Lou and Malloy 2017). While communicative precision is generally favored in the financial market (Du and Budescu, 2005; Baginski et al., 2011), a stream of literature in finance suggests heterogeneous effect of communicative precision on market reactions contingent on the valence of the underlying information. First, positive news communicated with ambiguity may be received with more optimism, especially when the ambiguous information is communicated using an open interval. As open intervals specify a minimum while setting no limits to the upward possibilities, the market may perceive greater potential for positive news (Du and Budescu, 2005; Baginski et al.,

2011). By contrast, existing research finds that using open interval that sets a maximum to communicate bad news tends to be perceived with greater pessimism (Baginski et al., 2011). In other words, relative to precise communication, ambiguous communication tends to amplify the market reaction to both positive and negative information. Therefore, to contain inflated market expectations, high-status companies may communicate good news with higher precision and bad news with higher ambiguity. Accordingly, I propose the following set of hypotheses that high-status companies tend to be more precise in guidance when communicating good news (H2a); and high-status companies are more likely to use ambiguity in guidance when communicating bad news (H2b).

Hypothesis 2a*. With bad news, high-status companies are more ambiguous than low-status companies in the way they communicate guidance.*

Hypothesis 2b*. With good news, high-status companies are more precise than low-status companies in the way they communicate guidance.*

EMPIRICAL STRATEGY

Estimation strategy

To identify the effect of status on companies' information environment and strategic communication poses considerable empirical challenges. For example, it is possible that unobserved differences in the management's capabilities may account for both differences in status and strategic behaviors to manage audience expectations. Also, companies that are more successful at managing audience expectations may gain more recognition and ascend in status thereafter. To address this empirical issue, I implement a regression discontinuity design in the

context of Fortune 500 and 1000 ranking. Specifically, Tian, Smith and King (2020) have proposed that the Fortune 500th position is an artificial breakpoint in the otherwise smoothly distributed Fortune ranking due to the evolution of the ranking. As Fortune magazine has extended great efforts to construct and publicize the Fortune 500 as a category of American corporate elite since the 1950s, the 500th position constitutes an exogenous breakpoint in organizational status, whereas quality follows continuous distribution around it. Implementing RDD in this context, I test whether companies that are just above the Fortune500 cut-off (high-status) have different information environments relative to their counterparts just below it (low-status) and whether they communicate with more ambiguity or precision depending on the underlying information.

Data and Variables

This paper combines data from the Fortune magazine and the I/B/E/S database to measure companies' rank positions in the Fortune 1000 ranking, analyst attention and company issued guidance. As I focus on the period following the Regulation Fair Disclosure which regulates communication in the financial market, the sampling period ranges from 2000 to 2015.

The key independent variable $Fortune500$ measures organizational status by a dichotomous variable that equals 1 if a company is ranked within Fortune 500 in a given year, and zero otherwise. To test the first hypothesis regarding organizations' information environment, the dependent variable $Estimates$ measures the number of analyst earnings estimates for 30 day forecast period. To test the second set of hypotheses, the dependent variable $Precision$ measures the precision of the earnings guidance with a binary variable that equals 1 if

the guidance is communicated with a point estimate and 0 if it is communicated with a range or open interval. Both dependent variables are derived from the I/B/E/S data that cover analyst estimates and company issued guidance.

Additionally, including control variables in RDD may increase the precision of the estimation. Specifically, I include industry fixed effect, year or year-month fixed effect. As company issued guidance is a voluntary strategy that may affect companies' information environment in terms of analyst attention, I control for a binary variable as to whether a company issues earnings guidance in the estimations pertaining to the first hypothesis. To test the second set of hypotheses, I control for several additional variables. First, companies may communicate forecasts for a number of different measures such as earnings per share, sales, returns on asset, returns on equity etc. They may further involve varied periods including annual, semiannual, and quarterly forecasts. Therefore, I control for measure and periodicity fixed effects.

Lastly, it is important for my analysis to measure the valence of the underlying information. The I/B/E/S database categorizes the guidance into four categories, including "shortfall", "beat consensus", "match consensus" and "management guidance". Among them, "shortfall" refers to negative news in that the guidance is lower than the prevailing market estimates, whereas "beat consensus" refers to positive news such that the management forecast is more optimistic than the prevailing market estimate. Both "Match consensus" and "management guidance" comprise of neutral information as the managements either have the same forecast as the prevailing market estimates or do not specify whether their forecasts exceed the prevailing estimates. Therefore, I control for the valence of information using a categorical variable that comprise of $PositiveInfo$, $NeutralInfo$, and $NegativeInfo$.

ESTIMATION AND RESULTS

Following Tian et al. (2020), I implement a sharp regression discontinuity design in the context of Fortune 500 and 1000 ranking to test the effect of organizational status on companies' information environment and communication. Specifically, I treat the Fortune rank as the continuous distribution of assignment variable, and the Fortune 500 position as the diagnostic threshold which determines the treatment of organizational status. I use the parametric estimation as the main estimation strategy, while I demonstrate confirmatory results using non-parametric estimation and discontinuity bandwidths.

To begin with, to test the effect of organizational status on companies' information environment, I first adopt standard parametric method of RDD by using pooled cross sections of data and including flexible functional forms of the assignment variable (Imbens and Lemieux 2008). To account for within-firm correlation of errors overtime, I use robust clustered standard errors on the firm level (Lee and Lemieux 2010). The estimation is specified as follows,

$$
\begin{aligned}
Estimates_{it} = {} & \alpha_0 + \alpha_1 Fortune500_{it} + \alpha_2 (rank_{it} - 500) \\
& + \alpha_3 \, (rank_{it} - 500)^2 + \alpha_4 \, (rank_{it} - 500)^3 + \; + \alpha_5 \, (rank_{it} - 500)^4 \\
& + \alpha_6 Fortune500_{it} \times (rank_{it} - 500) + \alpha_7 Fortune500_{it} \times (rank_{it} - 500)^2 \\
& + \alpha_8 Fortune500_{it} \times (rank_{it} - 500)^3 + \alpha_9 Fortune500_{it} \times (rank_{it} - 500)^4 \\
& + \alpha_{\mathrm{t}} + \; \alpha_{\mathrm{ind}} + \; \delta X'_{it}
\end{aligned}
$$

whereby the dependent variable is $Estimates_{it}$, which measures the number of analyst estimation and the independent variable is $Fortune500_{it}$ which indicates whether the company is ranked within the Fortune 500 category. To control for the distance from the 500th cut-off, I include the polynomials of the assignment variable $(rank_{it} - 500)$ and their interaction with the $Fortune500_{it}$ indicator variable, which allows for different trends above and below the 500th threshold. Specifically, I use the Bayesian Information Criterion (BIC) as the model selection criterion to select the order of polynomials. Up to the 4th level of polynomials are included as the BIC is minimized in this specification. Lastly, I control for industry fixed effect, year or year-month fixed effect, and whether the company issues earnings guidance. As the dependent variable is a count variable, the model is estimated using a Poisson estimator.

Table 7 presents the results of the parametric estimation. Model (1) controls for industry and year fixed effects and provides a baseline estimate. Specifically, the coefficient in model (1) suggests that for companies that just cross the Fortune500 threshold from below, the log of the expected number of analyst estimates increases by 0.139. The result is substantively significant, as the difference constitutes approximately 23% of the mean number of analyst earnings estimates in the Fortune 1000 sample. Model (2) further controls for whether the company issues earnings guidance. Consistent with the literature, issuing earnings guidance has a significant and positive association with analyst earnings estimates. Models (3) and (4) add finer controls for period by controlling the year-month fixed effect and provide confirmatory results. Importantly, the magnitude of the coefficients of Fortune500 remain highly consistent across models (1) through (4), providing supports for the validity of the research design.

[Insert Table 7 here]

Furthermore, I use nonparametric estimation to relax the functional form assumptions to complement the parametric method (Lee and Lemieux 2010). At the same time, I also consider several different discontinuity samples around the Fortune 500 threshold. While the previous parametric estimation uses the full sample to obtain higher precision in the estimate, a possible trade-off is the biases that stem from using observations too far away from the 500^{th} threshold. To determine the specific bandwidths of the discontinuity samples, I use several data-driven bandwidth selection methods (Calonico et al. 2017), including one common mean squared error (MSE)-optimal bandwidth selector (MSERD), one common coverage error rate (CER)-optimal bandwidth selector (CERRD), and two different mean squared error (MSE)-optimal bandwidth selector (MSETWO). In particular, the first two bandwidth selectors MSERD and CERRD specify the same sizes above/below the threshold, while the third selector MSETWO allows more flexibility by using two different bandwidth sizes above/below the thresholds. Table 8 presents the results of the non-parametric estimation using these three different bandwidths with triangular kernel. The dependent variable $Estimates(residuals)$ measures the number of analyst earnings estimates net of industry and year-month fixed effects, and whether the company issues guidance. It is obtained from a Poisson regression with clustered standard errors on the firms. As the estimates in table 8 demonstrate, there remain a significant and positive effect of Fortune 500 status on the number of analyst earnings estimates.

[Insert Table 8 here]

Additionally, I conduct an important falsification test with respect to alternative cut-offs. As I argue that Fortune 500 marks a discontinuity in organizational status, I test if there are significant differences in companies' information environment around alternative thresholds at every hundredth rank position. Specifically, I implement the same parametric estimation model as specified in model (4) of table 7 with the same number of observations. Consistent with the analyses in table 7, the order of polynomials is selected according to the minimization of BIC. Figure 5 shows the coefficients and the 95% confidence intervals of the estimates at hundredth rank positions. As the results demonstrate, there is no significant difference in the number of analyst earnings estimates around alternative cut-offs. This suggests that Fortune 500 is a discontinuous threshold that produces status distinctions in the otherwise smoothly distributed Fortune ranking.

[Insert Figure 5 here]

Next, I test the second set of hypotheses related to the precision of communication using a standard parametric estimation method. Similar as the previous specification, I use pooled cross sections of the data and use clustered standard errors at the firm level to account for within-firm correlation of errors overtime. Using BIC to select the order of polynomials of the assignment variable, I include up to the fourth order of polynomials to account for the distance from the Fortune 500^{th} threshold. The estimation is specified as follows.

$$Precision_{it} = \alpha_0 + \alpha_1 Fortune500_{it} + \alpha_2 PositiveInfo_{it} + \alpha_3 NeutralInfo_{it} + \alpha_4 Fortune500_{it} \times PositiveInfo_{it} + \alpha_5 Fortune500_{it} \times NeutralInfo_{it} + \alpha_6 (rank_{it} - 500) + \alpha_7 (rank_{it} - 500)^2 + \alpha_8 (rank_{it} - 500)^3 + \alpha_9 (rank_{it} - 500)^4 + \alpha_{10} Fortune500_{it} \times (rank_{it} - 500) + \alpha_{11} Fortune500_{it} \times (rank_{it} - 500)^2 + \alpha_{12} Fortune500_{it} \times (rank_{it} - 500)^3 + \alpha_{13} Fortune500_{it} \times (rank_{it} - 500)^4 + \alpha_{\mathrm{t}} + \alpha_{\mathrm{ind}} + \delta X'_{it}$$

In this specification, I include the interaction terms between $Fortune500_{it}$ and the valence of the underlying information, which is measured as a categorical variable comprised of three categories including positive information, neutral information, and negative information. Specifically, α_1 is the average treatment effect of Fortune500 status on guidance precision when the underlying information is negative (H2a) and α_4 is the treatment effect of Fortune500 status on guidance precision when the underlying information is positive (H2b). In addition to the polynomials of the assignment variable, I control for industry fixed effect, year-month fixed effect, measure fixed effect, and periodicity fixed effect.

[Insert Table 9 here]

Table 9 presents the results of this estimation. Model (1) in this table refers to the estimation based on the full sample. As the estimates of $Fortune500$ show, when the underlying information contains negative surprise, companies that just make it into Fortune500 are significantly more ambiguous in their communication of guidance relative to those just fail to

make it, offering significant support for H2a. By contrast, the estimates of $Fortune500\#PositiveInfo$ suggest that Fortune500 companies use more precision in their communication of positive surprise. The result is marginally significant, which offers suggestive evidence in support for H2b. Additionally, the estimate of $PositiveInfo$ pertains to the effect of the valence of the underlying information among non-Fortune500 companies. The insignificant result suggests that low status companies, relative to their high-status counterparts, are less likely to adjust the precision of their communication depending on the valence of the underlying information. This observation is consistent with my theoretical argument that high-status companies may find it more challenging to manage audience expectations in this context. Model (2) presents the estimation results based on a discontinuity sample closer to the Fortune500th threshold, by restricting the bandwidth to 200 rank positions on both sides of the threshold. It provides confirmatory support for H2a alone, suggesting that high-status companies are significantly more ambiguous in their communication of negative surprises to the financial market.

DISCUSSION AND CONCLUSION

To sum up, I propose that organizational status may function as a source of constraint for high-status actors in the context of the financial market. First, I argue that high-status organizations are faced with different information environments relative to low-status organizations. The difference involves both greater amount of attention and inflated expectation as status tends to lead to upward biases in evaluation. Importantly, while prior work has treated the information environment as an important contingency for status effect, this chapter provides

empirical evidence that points to a novel nuance that information environment differs according to status positions. Furthermore, I find suggestive evidence that companies may take strategic actions to enable (or disable) the implications of status by adjusting the ambiguity of their communication to proactively affect their information environment. Beyond the data presented in this chapter, my future step involves analyzing the role of organizational status in communicative ambiguity with large-scale textual data from companies' earnings conference call transcripts.

This chapter adds to the growing line of research that studies status as a source of constraints for status actors (e.g. Malmendier and Tate 2009; Kovac and Sharkey 2014; Bothner et al. 2012; McDonnell and King 2018). For example, past research has considered the downsides of status due to scrutiny from external audiences (Wade et al. 2006), complacency (Bothner et al. 2012), and harsher punitive measures in the wake of failures to meet audience expectations (McDonnell and King 2018; King and Carberry 2018). This chapter highlights an additional mechanism whereby status may produce unfavorable implications. Specifically, I argue that heightened expectations due to status-related biases may work to the disadvantage of high-status actors in contexts wherein companies seek to lower market expectations to beat prevailing market estimates in the future. In stark contrast to the Matthew effect conceptualization, status may become a liability when inflated expectations stemming from upward biases are unfavorable.

While this mechanism of status constraint is generalizable to a proliferation of contexts, this chapter also seeks to highlight the importance of the present context in the financial market. According to Podolny (1993), status advantages arise from two main sources: customer acceptance and cost of input factors (Podolny 1993). In the past thirty years, there has been a rapid rise of the shareholder value in the US market. As Davis (2009) noted, the rise of

shareholder value has led to fundamental shifts in the economy as companies became organized for shareholder value maximization. In this context, Podolny's model has tremendous implications as both customer acceptance and cost of input factors determine the bottom line. However, with the financialization of the economy, the financial market and analysts have also become one of the most important audiences for public companies (Davis and Kim 2015). Meeting quarterly analyst estimates is one of the most salient goals of managers at public companies (Davis and Kim 2015). As existing organizational status research tend to focus on the customers or input factor providers as the key audiences, this chapter aims to contribute to the organizational status literature by illuminating the implications of status with respect to the rise of the financial analysts as a key audience for public companies.

The third contribution of this chapter is to shed light on the effect of status on the strategic behaviors of status actors (Bothner, Kim, and Smith 2012). Existing research highlights the significant role of status in shaping audience evaluations. As external audiences face difficulties with verifying quality and predicting future performance, they tend to rely on signals from an organization's status to form expectations about quality (Merton 1968; Podolny 1993). This mechanism underlying the Matthew Effect of status has attracted a large body of empirical research. Using clever research designs, a growing body of empirical research has established the causal evidence for the effect of status on audience evaluation (e.g. Kim and King 2014; Simcoe and Waguespack 2010; Azoulay, Stuart and Wang 2012), causing audiences to allocate more resources and unearned benefits to those actors occupying prominent status positions (Correll et al., 2017; Sauder, Lynn and Podolny 2012). While it is important to study how status affects evaluation by external audiences, a growing body of research emphasizes the effect of status on its possessors' behaviors and strategy (e.g. Bothner, Kim and Smith 2012; Phillips and

Zuckerman 2001). This chapter aims to make headway in this direction by highlighting how company may strategically manage audience expectations in response to the implications of their status positions.

CHAPTER 3. Effect of Extraprofessional Status on Professionals: Evidence from A Quasi-experiment Design

ABSTRACT

This chapter studies whether and how actors navigate multiple status hierarchies. Prior work on status tends to focus on the dynamics within one status hierarchy, while individuals and firms are faced with a growing multiplicity of stakeholders that may enact multiple status hierarchies that are fateful at the same time. I study this question in the context of academia, where there is a poignant paradox between intra-professional status and public status for scientists. Combining large scale data on scientists' research outputs and their respective media attention from 2010 to 2015 with an instrumental variable estimator, my findings suggest that public status conferred by the media increases scientists' commitment to the topics covered by the media in the future. Additionally, the effect is more pronounced among scientists who are of relatively low intra-professional status as measured by widely used proxies such as institution prestige and cumulative impact. As I find that public status affects scientists' commitment to status-conferring topics, my findings also have implications for the broader research on the use of non-pecuniary incentives in motivating scientists and professionals.

INTRODUCTION

Due to the increasing complexity in social and technological changes, organizations and decision makers are faced with greater needs to coordinate with an ever-expanding range of stakeholders and audiences (King and Soule 2007; Kaplan 2019). As audiences may become relevant agents with the authority to shape the definition of symbolic value (Bourdieu 1993), a growing multitude of stakeholders may have crucial implications for how multiple status hierarchies evolve and interact to affect actors' behaviors in a field. Importantly, past research has found that distinct audiences tend to define what is worthy of status on different basis (White and White 1993; Cattani, Ferriani, and Allison 2014). While multiple status hierarchies may be complementary to one another (Bothner, Kim, and Lee 2012), they may also differ and conflict (Abbott 1981). For example, research in the context of the cultural market has pointed to the insight that different types of audiences may generate dynamics that challenge the self-perpetuation of cumulative advantage that accrues to core actors defined by one type of audience (Cattani et al. 2014). Therefore, the growing relevance of multiple stakeholders surrounding organizations and individual decision-makers necessitates the question as to how status actors may navigate multiple status hierarchies that are misaligned. Strikingly, existing research on the behavioral implications of status focuses almost exclusively on investigations within one status hierarchy (Sauder, Lynn and Podolny 2012). This chapter seeks to advance this line of literature with empirical investigation into actors' behaviors in the face of multiple status hierarchies.

Prior research has highlighted two prominent types of audiences that may have the relevant authority to produce status hierarchies: peer audiences and third-party arbiters. First, peer audiences are members in the same field and may confer status through interactions and

deference (Gould 2002). A long line of research has examined status that emerges and evolves through interactions among peers in intra-industry contexts (e.g. Podolny 1993; Uzzi and Lancaster 2004; Bothner et al. 2012; Rider and Tan 2015; Rider and Negro 2015; Tan and Rider 2017). By contrast, a recent line of status research has started to emphasize the role of external audiences and third-party arbiters (e.g. Sauder 2006; Espeland and Sauder 2007). Without direct ties with actors within a field, influential external arbiters may purportedly become important status-defining sources by synthesizing information and providing independent judgments (e.g. Sauder 2006; Espeland and Sauder 2007; Rao 1994; White and White 1993; Cattani et al. 2014; Cattani et al. 2017). One key source of the impacts of third parties is publicity (Correll et al. 2017). For example, existing research argues that rankings and prizes conferred by third parties such as prominent media outlets can become fateful despite actors' disagreements about the underlying standards used in the rankings (Espeland and Sauder 2007; Sauder and Espeland 2009; Espeland and Sauder 2016; Correll et al. 2017). Consistent with this insight is the empirical observation that prominent rankings based on pre-existing deference structures may produce independent and profound impacts (Espeland and Sauder 2016). Inasmuch as peer and external audiences use different basis to allocate status and symbolic value (White and White 1993; Cattani et al. 2014; Sauder et al. 2012), status actors in the field are subject to the implications of different status hierarchies.

The present paper examines how actors navigate different status hierarchies in a context of professional labor conducive to the impacts of both peer and external audiences. Specifically, I propose that the context of academia constitutes a promising setting to allow empirical investigation into this question. Abbott's (1981) work on status strain points to a poignant

paradox between intra-professional status conferred by peers and extraprofessional status conferred by external audiences such as the public in the contexts of professional labor. This paradox is likely amplified when it comes to the context of academia. As Merton (1973) illuminated, one of the key norms in the institution of science is that scientists are rewarded primarily by the recognition from their peers. To the extent that scientists are primarily concerned with intra-professional status conferred by peer recognition, studying the role of extraprofessional status granted by external audiences in this context offers a strong test for the pertinence of the impact of alternative status hierarchies.

Using large-scale data and an instrumental variable estimator, I test if extraprofessional status conferred by external audiences has causal impact on scientists' future commitment to topics conducive to extraprofessional status. Drawing on Abbott's (1981) insight that public status arises from contact with the public wherein the media may provide an effective avenue (Abbott 1981; 1988), I focus on the mass media as the external audience that confers public status, a key instance of extraprofessional status. As media recognition tends to follow immediately after the time of publication, the IV-2SLS estimator leverages the potential that homerun papers by other scientists (measured as the top ten most cited papers per year) may coincide with a focal scientist's paper at random. This can ultimately result in an underreporting of the focal scientist's research due to a co-occurrence with an unpredictable homerun paper that monopolized media coverage. Combining research output and media coverage data from 2010 to 2015, I find that media recognition significantly increases scientists' future commitment to continuing research on the topics of the focal paper covered by the media. Furthermore, I point to heterogeneous effect according to the scientists' intra-professional status using widely adopted

proxies for peer recognition in this context. Relative to the subsample of scientists with high intra-professional status, the commitment to public status is markedly more pronounced among scientists with low intra-professional status.

In what follows I will first introduce the pertinent theories and literature to develop the research questions regarding scientists' commitment to extraprofessional status and how it interacts with intra-professional status. Next, I will introduce the empirical strategy, data, and estimation results. Before concluding the chapter, I will discuss the implications of the findings for several streams of literature, such as the roles of non-pecuniary incentives among professional labor, status implications for innovation, and impetus for change and challenges to the cumulative status of status.

THEORY AND HYPOTHESES

Merton's (1968; 1973) seminal works on the sociology of science highlight the importance of intra-professional status in the context of academia. Not only is intra-professional status self-perpetuating due to the mechanism of Matthew Effect (Merton 1968), but the pursuit for intra-professional status arising from peer recognition is also one of the key tenets that define the institution of science according to Merton (1973). By contrast, Abbott's (1981; 1988) works on the sociology and history of professions propose a more nuanced and dynamic account of status by accentuating the distinct impacts of intra-professional and extraprofessional status in the context of academia and other professions alike. In particular, he points to an inherent paradox between the sources conducive to these two distinct status hierarchies. While intra-

professional status stems from professional purity that arises from the exclusion of non-professional engagements, the basis of extraprofessional and public status for professionals comes from the effective demonstration of contact with professional disorders to the public (Abbott 1981).

Insofar as both peer recognition and public support carry important implications for the professionals and professions (Abbott 1981; 1988), this paradox begets a poignant conflict between two consequential status hierarchies that require scientists to navigate. From the perspective of professional purity, engagement with the public constitutes one source of impurity which undermines scientists' intra-professional status. On the contrary, it is nevertheless important to acquire public support for professions to maintain jurisdictional control over areas of research (Abbott 1988). While the latter concern pertains to a wholistic perspective of the professions, professional institutions may in turn seek to mobilize scientists to engage with the public to enlist general support for science (Peters et al. 2008; Colyvas 2007). Importantly, recent research that surveys scientists offers confirmatory evidence that this inherent paradox is manifested in scientists' lived experiences and perception. When it comes to the engagement with the public, approximately half of the scientists consider it highly important to themselves and to their institutions, whereas a substantially smaller proportion of scientists report that their colleagues and peers appreciate engagement with the public (Rose et al. 2020).

The paradox between intra-professional and public status leads to competing demands for scientists. How might scientists navigate these competing status hierarchies? When it comes to conflicting demands from different constituents, a natural decision that arises is the allocation of commitment to different constituents (Kanter 1968; Selznick 1957; Phillips et al. 2011; King

2015). Commitment may come in a variety of forms. Aligned with prior research that studies commitment as manifested in product or service offerings (Phillips et al. 2013), I focus on the research outputs that scientists produce which are systematically observable. Specifically, I consider a form of commitment pertaining to continued engagement with the same research topics, which is akin to an instance of continuance commitment enabled by costly investments (Kanter 1968). To the extent that some scientists may become predominantly concerned with extraprofessional status, this focus on scientists with continued research productivity also ensures that I study the group of scientists who experience the influences of both intra-professional and extraprofessional status.

While it is prevalently believed in prior research that scientists are concerned first and foremost with intra-professional status due to peer recognition (Merton 1973), the present study draws on the insights and evidence of competing status hierarchies in the context of academia (Abbott 1981; Sauder et al. 2012; Rose et al. 2020) to study whether scientists demonstrate commitment to extra-professional status. Additionally, research related to audience-level variations point to the insight that alternative audience and status hierarchies may have greater impacts on actors who are on the periphery according to one focal status hierarchy (Bourdieu 1993; Cattani and Ferriani 2008; Cattani et al. 2014). Accordingly, I propose two hypotheses as follows:

Hypothesis 1. *All else being equal, scientists tend to commit to research topics conducive to public status.*

Hypothesis 2*. Relative to scientists with high intra-professional status, the impact of public status on scientists' commitment to status-conferring topics is more pronounced among those with low intra-professional status.*

ESTIMATION AND RESULTS

Empirical Strategy

Empirically speaking, to investigate the impact of different status hierarchies on actors' behaviors poses substantial challenges related to causal identification. First, status is a meaningful sociological construct to the extent that it has independent effect net of underlying quality (Sauder et al. 2012), which is difficult to isolate in many empirical settings involving information asymmetry (Simcoe and Waguespack 2010; Kim and King 2014). Research on status dynamics also emphasizes an intricate relation between status and underlying quality (Podolny and Phillips 1996; Sharkey 2014). A stream of empirical research has adopted novel and clever research designs and contexts to advance this line of scholarship (e.g. Salganik et al. 2006; Simcoe and Waguespack 2010; Roberts et al. 2011; Kim and King 2014; Azoulay et al. 2014).

However, to study status dynamics that pertain to multiple status hierarchies poses further challenges. When it comes to isolating the effect of status conferred by external audiences from that granted by peers, the empirical challenge is compounded. For example, despite substantial differences in the basis for ascribing status according to different status hierarchies, there may remain considerable overlap in the realized status orders. It is especially plausible that external

audiences may draw partially on information from peer audiences who purportedly have the expertise to gauge underlying quality (Espeland and Sauder 2016; Cattani et al. 2014). In the present context, it is also possible that extraprofessional status gauged via media recognition may be endogenous to intra-professional status as media attention tends to accrue to prominent scientists and prestigious institutions (Peters et al. 2008; Yang et al. 2020).

To isolate the independent effect of public status on scientists, I employ an instrumental variable estimation that leverages the limited supply of media coverage relative to the large number of research outputs being published at any given time. In this sense, the underlying process to gain public status via media recognition is akin to a vacancy chain process (White 1970) whereby the likelihood of gaining media recognition for one focal paper is linked to other research outputs published around the same time. Furthermore, it is plausible that scientists do not have precise control over when their papers are published. Nor can they possibly foresee when other research outputs across all fields will be published to strategically manipulate their publication dates accordingly. Therefore, concurring homerun papers may constitute a source of exogenous shock to the likelihood of gaining media recognition. Exploiting this possibility for exogeneity in the process underpinning media recognition of science, I implement an IV-2SLS estimator with new data that enable my inquiries into the effect of public status on scientists' commitment to status-conferring topics.

Data and Sample

The present study employs new data that provide comprehensive coverage on research outputs and media recognition of them. Combining the media coverage data from Altmetrics[6] with the research outputs data from Microsoft Academic Graph (MAG) (K.Wang et al. 2019) from 2010 to 2015, I obtained a sample of around 9 million observations. As my instrumental variable estimator is derived from the competition for media recognition among research outputs that are published around the same time, it is important that I use the precise dates of the publications. Specifically, the distribution of the publication dates in the full sample suggests issues with imprecision. Figures A2 and A3 in the appendix show the histograms of days in a month and months in a year respectively. As the figures demonstrate, both January and the first day of every month are significantly overrepresented in this full sample, while other days of a month and other months of a year follow smooth and even distribution.

[Figure A2 is about here]

[Figure A3 is about here]

It is plausible that the MAG database rounds unknown date to 1st day of every month and unknown month to January. To ensure that I use data on research outputs whose publication dates are known and precise, I exclude all observations from January or 1st day of every month in

[6] The author thanks Altmetrics for granting the API access (www.altmetric.com).

the following analysis. Therefore, my main analysis sample consists of about 3.5 million observations. While this exclusion leads to a significant reduction in the number of observations and precision of estimation, it is nevertheless important for the instrumental variable in my research design. Additionally, I show consistent results using the full sample in the appendix, which confirms that my findings are not conditional on sample selection with respect to having precise publication dates.

Variables

First, the dependent variable pertains to scientists' commitment to the research topics of the papers that are conducive to public status. I measure topic commitment by comparing the similarity between the sets of research topics in the focal paper and those of their future research in the next five years. Using machine learning, the MAG database assigns topics associated with every research output, with varied levels of specificity. The highest level of topics is similar in concept as academic fields such as "engineering", "psychology", and "sociology". Here, I use the topics at the next level of specificity with examples such as "gender inequality" and "social movement". Specifically, following existing research that measures the similarity between scientists' research topics overtime (e.g. Wang, Jones and Wang 2019), I use the Jaccard similarity coefficient to measure the dependent variable $Topic\ Commitment$. With two sets of topics—one pertaining to the topics of the focal paper and the other pertaining to the research topics in one's future works in the following five years—the Jaccard coefficient is calculated by the size of the intersection divided by the size of the union between these two sets of topics. Higher the coefficient, the more similar are the two sets of topics and the more committed is the author to the topics of the focal paper.

Next, according to Abbott's (1981; 1988) conceptualization, public status among scientists arises from effective contact with the public, whereby media coverage of one's research plays an important role. The Altmetrics database collects information about mainstream media mentions associated with research outputs. I use a recent sampling period from 2010 to 2015 after the advent of the Altmetrics when the coverage is relatively comprehensive. Specifically, I rely on two sets of information including the number of mainstream media mentions and the dates of the media mentions. I construct the independent variable measure $Media\ Recognition$ by first counting the total number of mainstream media mentions of the focal paper. In the main analysis, I use the logarithm transformation of the total number of media coverage ($Log(Media\_mention)$). For tests of robustness, I also use a binary measure of whether a paper has obtained any media recognition ($Media\_mention01$). In the rare event that media mentions may take place before publication dates, I also use a measure of the logarithm of the number of media coverage excluding the number of media mentions prior to publication dates ($Log(Media\_post)$) in a robustness test.

The instrumental variable $Homerun\ Paper\ Concurrence$ is a binary variable that indicates whether a focal paper is published around the same time of a homerun paper that monopolizes media attention. Specifically, I define a homerun paper as the top 10 highest cited papers per year across all fields. Furthermore, the publication dates are obtained from the MAG database, whereas the media coverage dates come from the Altmetrics database. Figure 6 presents the distribution of the dates of media mentions over 26 weeks after a paper's publication date. As the figure shows, media coverage is significantly concentrated immediately after the publication of a paper. Therefore, I contend that the competition for media coverage is most

pronounced when the homerun paper is published around the same time as the focal paper. Specifically, I consider a homerun paper concurrent if it is published within one week before and after the focal paper's publication. In the main analysis sample with precise publication date, about 13% of the research outputs experience a concurrent homerun paper according to this definition.

[Figure 6 is about here]

In terms of intra-professional status, my current analysis considers two widely used proxies for peer recognition in the context of academia: a scientist's cumulative impact and the prestige of the institution. Specifically, scientists' cumulative impact is measured by the total number of citations that one accumulates overtime. While it is an imperfect proxy for quality, it is widely agreed upon that cumulative citation reflects peer recognition which gives rise to intra-professional status. Additionally, institutional prestige is measured by the US News Best Global University Rankings. Beyond this basic measure, I aim to measure prestige at the department level in the next step. Measuring prestige at the department level will also allow me to isolate other factors at the institution level. Specifically, research that operationalizes intra-professional status in the context of academia highlights the role of a department's position in the hiring networks in the making of department prestige (Burris 2004; Bothner, Smith and White 2010; Abbott 2001). I aim to incorporate this operationalization of intra-professional status in the next step.

Lastly, I measure a set of control variables related to the focal paper, which include field fixed effect, year fixed effect, the cumulative impact of the first author and the last author at the time of the publication, the average institutional prestige of the team, and author fixed effect in selected models.

Estimation Results

Implementing an instrumental variable regression that exploits exogenous processes underlying the mechanism of public status obtained via media recognition, I aim to provide empirical evidence as to whether public status has causal impact on scientists' commitment to the underlying topics in the focal papers that attracted media recognition. But to start, I begin the analysis by testing for a correlation between media recognition to the focal paper and authors' commitment to the underlying topics in the following 5 years. Table 10 presents significant correlational evidence. Specifically, the OLS model controls for variables related to the focal paper including the field and year fixed effects, the first author and the last author's cumulative impact and the average institutional prestige of the team, and shows a significantly positive association between media recognition and last authors' commitment to the underlying topics. The Author Fixed Effect model further controls for last author fixed effect which accounts for time-invariant omitted variables at the author level and demonstrates confirmatory evidence.

[Table 10 is about here]

In terms of the main analysis, table 11 presents the analysis with $Log(Media\_mention)$ as the measure for the independent variable media recognition. The estimation controls for field fixed effect, year fixed effect, the first author and the last author's cumulative impact, and the average prestige of their affiliated institutions. In the present estimation, I focus on the last author's future commitment and use robust standard errors clustered on the last authors. As I will address later, I find consistent results with the focus on first authors. Model (1) of table 11 shows the results of the first-stage estimation wherein media recognition is the dependent variable predicted by the instrumental variable $Homerun\ Paper\ Concurrence$. Model (2) of table 11 presents the two-stage-least-squared (2SLS) estimate of the effect of $Log(Media\_mention)$ on the dependent variable $Topic\ Commitment$.

[Table 11 is about here]

Consistent with my observation that publications need to compete for media recognition at any given time, the first-stage estimation reveals a significant negative impact of a concurrent homerun paper on a focal paper's likelihood of obtaining media recognition. Importantly, the model's F-statistic (F = 60.24) is significantly greater than the Staiger and Stock (Staiger and Stock 1994) rule of 10, which suggests a strong first-stage relation and alleviates concerns about pathologies related to weak instrument. Furthermore, the estimates related to first author and last author's impact and institution rank are consistent with existing research that predicts media coverage on science (Peters et al. 2008; Yang et al. 2020). Model (2) presents the 2SLS estimates

and demonstrates a significant effect of media recognition on the last authors' commitment to the topics in the focal papers that gain media recognition, providing strong evidence in support of the first hypothesis.

Before turning to the second hypothesis, I test the robustness of the results with a few steps. Specifically, table 12 replicates the findings while media recognition is measured using a binary variable $Media\_mention01$. Table 13 points to confirmatory findings using $Log(Media\_post)$ as the measure for media recognition, wherein I exclude media mentions to a paper prior to the publication dates. Table 14 presents consistent results when I examine the first authors' future commitment instead. Finally, table A2 in the appendix presents the analysis using the full sample that includes January and first day of every month. The first-stage relation is exceeding high, which is likely due in part to the imprecision of dates. Nevertheless, the results point to confirmatory findings when the full sample is employed, assuaging concerns about sample selection.

[Table 12 is about here]

[Table 13 is about here]

[Table 14 is about here]

Next, I test the second hypothesis on the heterogeneous effect of extraprofessional status with respect to intra-professional status. The present analysis employs two widely used proxies for intra-professional status gauged by peer recognition, including the scientists' cumulative impact and the prestige of one's institution. Using a median split according to these two proxies, I categorize the authors into two buckets of high and low intra-professional status subsamples wherein I conduct the instrumental variable analysis similar as the main estimation models. Figure 7 plots the coefficients and the 95% confidence intervals of the estimates within subsamples. Specifically, as both Figure 7A & 7B demonstrate, the effect of public status on scientists' commitment is statistically significant among the low intra-professional status subsamples and insignificant among the high intra-professional status subsamples. Additionally, Figure A4 in the appendix demonstrates consistent patterns using data from the full sample. This finding provides strong support for the second hypothesis that the effect of extraprofessional status is more pronounced among scientists with low intra-professional status rather than high intra-professional status.

[Figure 7 is about here]

DISCUSSION AND CONCLUSION

This paper aims to advance the research on the behavioral implications of multiple status hierarchies that can be simultaneously fateful yet misaligned—an important question that necessitates academic inquiry as actors and organizations are faced with a growing multitude of stakeholders and audiences (King and Soule 2007). Drawing on prior research that highlights the

distinction between status hierarchies conferred by peer audiences and third-party arbiters (Sauder et al. 2012), I study the effect of extraprofessional status on professionals' behaviors and how it interacts with intra-professional status in the context of the academia. Implementing an instrumental variable estimator, I find that extraprofessional status conferred by media recognition significantly increases scientists' commitment to the research topics underlying the publications covered by the media. This effect is more pronounced among scientists with low intra-professional status relative to their high-status counterparts.

Beyond the data presented in this chapter, I seek to advance this research with future steps. For example, I aim to construct additional proxies for intra-professional status following existing research that considers the labor market and a department's position in the hiring network as a key source of intra-professional status in the academic context (Abbott 2001; Burris 2004). Second, Chown (2020) theorizes and measures jurisdictional dominance to account for the varying degree of jurisdictional control that professions may have over tasks. By analogy, I aim to incorporate this construct and examine the heterogeneous effect of public status contingent on the jurisdictional dominance of topics in different fields.

This paper complements research on the use of non-pecuniary incentives among scientists and professionals. When it comes to incentivizing scientists and professionals to pursue certain research directions or tasks, existing research suggests that the effectiveness of pecuniary incentives is highly limited (Myers 2019; Chown 2020). Whereas a large body of research points to the importance of non-pecuniary incentives for scientists (Merton 1968; 1973; Stern 2004), the exclusive role of intra-professional status makes intervention difficult from a policy perspective. As this paper provides empirical evidence for the role of public status on scientists' choices of

research direction, I suggest that public status may function as a non-pecuniary incentive that is more amenable to intervention from policy-makers' perspective.

Additionally, this paper provides insights into mechanisms that may challenge the cumulative advantage of status by highlighting the role of alternative status hierarchies due to audience-level variation (Cattani et al. 2014). Importantly, I argue the present context provides a case for a strong test for the effect of external audiences, as scientists are concerned primarily with intra-professional status (Merton 1973), which may be highly misaligned with extraprofessional status (Abbott 1981). Therefore, the behavioral impacts of status hierarchies produced by external audiences may be amplified in many other contexts where the finding is generalizable.

Lastly, my findings carry far-reaching implications for science at the aggregate level. The Matthew effect conceptualization implies that scientists with low intra-professional status are often deprived of resources relative to their high-status counterparts (Merton 1968). By contrast, research on the sociology of knowledge and creativity emphasizes the role of periphery members as a key source of innovation (Merton 1972; 1973; Cattani and Ferriani 2008). Therefore, while periphery members may be predisposed to creative ideas, they may be inadvertently constrained in terms of resources due to the mechanism of Matthew effect. In this paper, I highlight the effect of alternative status hierarchies which may function as an impetus for change to the cumulative advantage of one status hierarchy that may ultimately affect the allocation of resources.

CHAPTER 4. Stretched Thin? The Paradox of Promoting Diversity in Higher Education [7]

ABSTRACT

This chapter examines organizational efforts to dismantle the cumulative advantage of status related to race at the individual level. Drawing on research on allocative inequality in the workplace, we theorize a paradox of diversity promotion as a novel mechanism for unintended consequences of organizational efforts to increase racial diversity. We choose to investigate this paradox in the context of higher education. Constructing rich personnel data for a university, the study demonstrates that efforts to increase the representation of underrepresented minorities in faculty positions leads to an increased prevalence of hiring minority assistant professors as joint appointments. This outcome is important as we further find that joint appointments overexpose new faculty members to risks that can negatively affect their research productivity and career advancement. Together, the results extend research on allocative sources of inequality and highlight an unintended cost of diversity efforts related to the hiring process. As higher education organizations are leaders in the efforts to achieve diversity, studying this context offers a strong test for the idea that efforts to increase the representation of minorities may paradoxically handicap the career advancement for the very individuals who embody diversity for the organizations.

[7] Note: This chapter is under revision in collaboration with Edward B. Smith.

INTRODUCTION

Beginning in the 1960s policymakers and organizations alike have extended significant effort to reduce racial disparities in labor markets. In addition to legal reform, attempted remedies at the organizational level have typically included dedicating resources and developing programs to promote equal opportunity and facilitate career development for underrepresented racial minorities. These programs include the rise of merit-based pay systems (Castilla 2008; Castilla and Benard 2010) and diversity training (Kalev 2009) as well as the adoption and diffusion of affirmative action policies (Kalev, Dobbin, and Kelly 2006) and Equal Employment Opportunity Commission (EEOC) guidelines. The successes of these efforts notwithstanding, nearly every indicator of economic status continues to suggest that significant racial inequality persists in the U.S. labor market. Over the past decade, African Americans, in particular, remained twice as likely to be unemployed compared to whites.[8] In addition, the average full-time African American male makes 70 cents for every dollar his white counterpart earns.[9]

The endurance of inequality in the face of efforts to resolve it has not only called into question the efficacy of past diversity management efforts, but has also sparked discussion as to how various practices meant to reduce inequality may in fact *increase* existing disparities in organizational and labor market contexts (e.g. Castilla 2008; Castilla and Benard 2010; Chan and Anteby 2016; Dobbin 2009; Kalev et al. 2006; Dobbin, Schrage, and Kalev 2015; Briscoe and Kellogg 2011). For a prominent example, Dobbin and Kalev (2016; see also Paluck and Green 2009) found the three most common organizational diversity interventions—mandatory diversity

[8] Bureau of Labor Statistics, Current Population Survey
[9] US Census. As in research on the gender wage gap (Bertrand and Hallock, 2001; Fernandez-Mateo, 2009), this difference is attributable to a combination of discrimination as well as differences in sorting, whereby African Americans selected and/or are sorted into lower paying jobs.

training, job tests, and grievance systems—often give rise to the opposite outcome. In a similar vein, in their study of merit-based pay systems Castilla and Benard (2010) found that such programs can unintentionally exacerbate pay differences between employees in majority and minority groups. Using an experimental design, the authors demonstrated that individuals tend to favor male employees over equally qualified female employees (in assigning monetary rewards) when they believe their organizations to be more meritocratic. Findings such as these point to a host of subtle and unintended mechanisms that can undermine the goals of equal opportunity initiatives and stand in the way promoting diversity (see also Fernandez-Mateo and King 2011; Lewis 2013; Smith and Gaughan 2016). At face value, merit-based pay programs and mandatory diversity training seem like obvious steps towards creating fairer, more inclusive workplaces. And yet as each of these studies demonstrate, when it comes to battling inequality, what seems obvious is not always effective.

The present paper aims to accelerate research on how organizational practices meant to promote equality may in fact undermine it. Importantly, our paper departs from this prior research that studies the unintended consequences of diversity management efforts as rooted primarily in actors' agency and biases (Dobbin and Kalev 2016; Castilla and Benard 2010; Briscoe and Kellogg 2011). Instead, we highlight the role of formalized personnel structures as a novel mechanism underlying unintended consequences of organizational efforts to remedy inequality. Drawing on recent research on subtle workplace segregation (Chan and Anteby 2016), we examine a case of within-job segregation driven by organizational desire for diversity, whereby underrepresented minorities are disproportionately allocated to roles and positions that embody diversity for the organizations while inadvertently undermining individual careers. In this sense, we highlight a paradox in diversity management in which the organizational efforts to

promote inclusion at the organizational level masks allocative inequality that disadvantages individuals of underrepresented minorities and ultimately exacerbates exclusion.

To accomplish this goal, we examine a case of the paradox in diversity management whereby organizational desire for diversity begets subtle within-job segregation in markets for professional labor. Our specific research setting is higher education, though our findings may readily generalize to other social and organizational contexts. In short, we demonstrate that organizational efforts to increase the representation of racial minorities can overexpose those minority group members to a set of unique risks which negatively affect their career advancement. Using comprehensive administrative data from a large (R1) U.S. public university from 1990 to 2016, we find that African American assistant professors are approximately four times more likely to be jointly appointed—that is, being formally appointed in two different academic departments simultaneously—as compared to their white colleagues. More specifically, we find that the decision to jointly appoint African American assistant professors is motivated in part by efforts to increase the amount of racial diversity within departments. Such is a worthy goal, to be sure. However, our results also demonstrate that independent of race, being jointly appointed at the assistant professor level is associated with lower research productivity and poorer career outcomes. Ceteris paribus, jointly appointed assistant professors experience a marked decline in their productivity and face evaluative hurdles not experienced by their singly appointed peers. The end result is that jointly appointed assistant professors—among whom African Americans are disproportionately represented—receive smaller year-over-year raises and face lower likelihoods of promotion to tenure. More striking still, our results demonstrate that the lower likelihood of promotion to tenure among African American assistant professors is explained in part by the increased prevalence of joint appointments among that group of faculty.

In what follows we develop four sets of hypotheses regarding (1) the relationship between underrepresented minorities and joint appointment in higher education, (2) the "upstream" antecedents of that relationship, (3) the "downstream" consequences of it and finally (4) two unique mechanisms underlying the “downstream” consequences, one pertaining to the effects of joint appointment on productivity and the other on evaluation. We next describe our data, measurement strategies, and empirical approach, and present our results in the order of our hypotheses, beginning with basic frequency tabulations and preceding on to identify the consequences of joint appointment among African American assistant professors in particular. Following the mechanism tests regarding research productivity and evaluation penalty, we explore a handful of alternative explanations and engage in a series of tests to assess the robustness of our results. In the discussion section we consider the broader theoretical and practical implications of our findings for work related to diversity management and organization theory.

THEORY AND HYPOTHESES

The Paradox of Promoting Diversity

On the heels of the civil rights movement, organizations of all kinds have increasingly faced legal and normative pressures to recruit and retain diverse workforces. While a small number of companies have adopted special recruitment programs and attempt to seek out gender and ethnic minorities earlier in the career cycle (e.g. Dobbin et al. 2015; Thomas 2004; Diversity Inc 2010), many organizational practices aim to increase the visible representation of demographic minorities in a short time frame (Rider et al. 2016). Nevertheless, when it comes to achieving diversity goals in the short-term, the preexisting underrepresentation of demographic

minorities constitutes a significant source of operational strain conducive to task and workplace segregation (Chan and Anteby 2016). Specifically, diversity efforts that focus on the visible representation of minorities generate tasks and roles that cannot be “filled by interchangeable incumbents” (Graen 1976:1224). Rather, only demographic minorities can *visibly* embody diversity for the organizations, resulting in an acute mismatch between the growing demand for minorities to fill immediate positions related to diversity and—paradoxically—the insufficient supply of minorities which begets these demands in the first place. This operational strain due to demographic mismatch is consistent with the observation that the achievement of managerial diversity is affected by the preexisting level of diversity in the labor pools (Cohen, Broschak, and Haveman 1998; Shenhav and Haberfeld 1992).

Importantly, the literature on workplace segregation has noted that when employer preferences for a group of employees meet demographic imbalance, individuals of different demographic groups may be allocated to different jobs and tasks, inadvertently working to the advantage of one group over another (e.g. Reskin and Roos 1990; Reskin 1991; Reskin 1993; Fernandez and Sosa 2005; Chan and Anteby 2016). Chan and Anteby's (2016) ethnographic account of the Transportation Security Administration (TSA) offers a prominent example. In their analysis, the authors describe how the gender-sensitive procedure whereby advanced screening of passengers (i.e., "pat downs") must be performed by same-gender TSA agents leads to gender-based inequality among TSA agents. Due to a mismatch between the percentage of female travelers and female TSA agents, female TSA agents are disproportionately called upon to perform advanced screening, raising both the physical and psychological demands of their work relative to their male counterparts. In a similar vein, minorities may be disproportionately channeled to diversity-related tasks and roles because of the mismatch between growing task

matching preference for minorities and limited supply of minority individuals in the workforce. For example, in one news report focusing on issues of race in higher education, an African American faculty member noted that minority faculty are not only more likely to sit on more committees, but also indicated that committee participation "feels more like an obligation [for minority faculty] than a choice." This "obligation" stems from the fact that minorities are often looked to to share alternative, underrepresented perspectives. Despite the importance of hearing that perspective, the persistent underrepresentation of minorities may create a situation whereby individual minority faculty members are called on more often and more intensely (see Chan and Anteby 2016) as compared to their non-minority counterparts.

The forces conducive to differential allocation of demographic minorities are further compounded by processes of diversity management that notably involve decentralization and ambiguity (Kalev et al. 2006). Taking a closer inspection on how diversity is articulated and organized within organizations, recent ethnographic research reveals that obscurity and symbolic commitment are prevalent in efforts to promote diversity (Berrey 2015; Ahmed 2012; Randolph 2012; Thomas 2018). Whereas a long line of organizational and sociological literature suggests that decoupling between formal diversity goals and specific practices is possible when organizations respond to regulatory and normative pressures (Dobbin et al. 1988; Edelman and Petterson 1999; Sutton and Dobbin 1996; Westphal and Park 2020), it is likely aggravated when such efforts are obscured and decentralized (Kalev et al. 2006; Edelman 1990; Meyer and Rowan 1977; Edelman et al. 2011). Consistent with this line of theory, ethnographic research reveals that it is common for diversity management efforts to be focused on symbolic commitments to diversity norms, which may further increase the demand for tasks with an explicit matching preference for minority individuals as they can increase the *visibility* of diversity for the

organizations (Ahmed 2012; Berrey 2015; Thomas 2018). Put together, organizations face considerable operational strains in their efforts to achieve diversity, which may rationalize within-job segregation whereby minority individuals are disproportionately requested to spend more time on tasks and roles that increase diversity for the organizations.

As minority individuals may disproportionately perform diversity-related activities, of particular importance is the question whether tasks and roles pertaining to diversity management entail equal levels of quality relative to others. Specifically, tasks and roles differ markedly from one another in terms of desirability for a variety of reasons (e.g. Cohen 2013; Chan and Anteby 2016; Babcock et al. 2017). One of the critical dimensions whereby tasks differ is their implications for promotion in the future. In general, tasks that are more relevant to performance evaluation can be perceived as more promotable (Babcock et al. 2017). As diversity-related practices are often carried out with decentralized efforts without specific specialists and accountability, it is likely that diversity becomes everyone's additional yet secondary responsibility (e.g. Kalev et al. 2006; Ahmed 2012; Thomas 2018). Under this circumstance, diversity-related tasks and role are rarely central to one's job prescription and performance evaluation. Rather, to perform diversity tasks is akin to engaging in positive extra-role behaviors that employees voluntarily exert beyond what is formally prescribed in their job expectations (Katz 1964; Van Dyne and LePine 1998). Insofar as organizations tend to evaluate employees based on their primary and in-role job requirements rather than extra-role behaviors, diversity-related tasks have less bearing on one's performance evaluation and constitute activities with low promotability.

In other words, job expectations are defined differently for minorities who are disproportionately allocated to roles and tasks that embody diversity. Relative to others who

occupy the same organizational positions, minorities are essentially sorted to jobs with lower quality (Kalleberg 2011) as their jobs put greater weights on the expectations for extra-role behaviors that have lower performance and promotion implications. Inasmuch as employees have the same number of hours at work, greater expectations for extra-role behaviors also constitute significant time intrusion that distracts employees from performing high promotability activities. Therefore, this subtle kind of within-job segregation may have farther-reaching career implications, which necessitates academic investigation into its downstream impacts on minorities' careers and unintended consequences on workplace inequality.

The Higher Education Context

The higher education context provides a promising test case that enables empirical inquiry into the paradox of diversity promotion. Higher education organizations demonstrate great desire for diversity while similar mechanisms of demographic mismatch and decentralization of diversity management efforts bring substantial operational strains to their attempts to achieve diversity. To start, as Dobbin and colleagues (2011) have pointed out, "organizational culture" and pro-diversity norms at the industry level both play a "critical role in companies' decisions to promote diversity practices." Due in part to the liberal ideological leanings of academia (e.g, Gross 2013), it is unsurprising that higher education is a context in which diversity promotion has long been an important topic of concern (e.g. Berrey 2011; Ahmed 2012; O'Meara, Culpepper and Templeton 2020; Babcock et al. 2017; O'Meara et al. 2017; Smith and Rand 2016; Smith 2015). And yet despite this long-held concern, the descriptive data in Table 1 reveal a stark mismatch between the racial demographics of the professorship and the country at large. Notably, while underrepresented minorities—i.e., African

American and Hispanic, specifically—make up 31% of the United States population, respectively, they account for a mere 10% of the tenured and tenure track faculty at degree-granting postsecondary institutions (IPEDS 2017).

Although a number of factors may ultimately be responsible for the underrepresentation of racial minorities in higher education, existing research has argued that one proximate cause of the racial mismatch may stem from the relative scarcity of underrepresented minorities receiving doctoral degrees (Cooney 2015; Griffin 2016; Yared 2016). According to data from the National Center for Education Statistics, whites and Asians account for more than 85% of the doctoral degrees awarded to US citizens, despite making up only 65% of the population as a whole.[10] Our aim in the present paper is not to diagnose the underlying cause of this supply problem but instead highlight the constraint it can pose for academic institutions striving to create more diversity among their faculty. Simply put, if practices are not put in place to increase the (real or perceived) supply of minority candidates for faculty positions, no amount of diversity management procedures can fully remedy the mismatches revealed in Table 13.

Short of an immediate and effective solution to the supply problem—or any other cause of the underrepresentation of minorities in academia, for that matter—how might universities cope with their inability to achieve their diversity goals? Drawing on research on the operational strains due to task-worker matching (e.g. Chan and Anteby 2016), we have previously discussed that one likely solution—though "solution" may be the wrong word here—is the disproportionate allocation of underrepresented demographic minorities to tasks and organizational positions that

[10] It is worth noting that if there is an *oversupply* of PhD holders relative to positions requiring a PhD, then the *relative* scarcity of minority PhD recipients is not sufficient to demonstrate a "pipeline" problem. This important nuance is beyond the scope of the present paper.

help increase diversity. When it comes to recruiting and maintaining a more diverse faculty, one instantiation of this allocative mechanism is to rely disproportionately on joint appointments whereby individual faculty members are simultaneously appointed in more than one academic department to hire minorities.

The practice of joint appointment is prevalently deployed among universities in the U.S. Specifically, we searched for news and university guidelines related to joint appointment for 50 randomly selected R1 universities that included both public and private institutions. Among them, 90% have noted the use of joint appointment and over 70% have recent announcements of cluster hires that may lead to joint appointments. There are several different pathways that can lead a faculty member to be jointly appointed. For example, at the individual level joint appointments can be designed around individuals for personal or professional reasons. At the department level joint appointments can enable additional hiring by stretching a department's resources if and when a second department expresses an interest in a candidate and willingness to share the costs in hiring that candidate. In this way, joint appointments may also result from efforts to retain high-profile faculty, especially at more senior ranks. Finally, at the school or university level, joint appointments are a common component of "cluster hires" and often result from university-wide strategic initiatives designed to increase the amount of interdisciplinary research on campus.[11]

The absence of known means to achieve diversity-related goals put universities and administrators under the circumstances of mission ambiguity (March and Olsen 1976) which

[11] This list of reasons for joint appointments is not meant to be exhaustive. For our purposes here, however, the many different pathways to joint appointment do suggest that the individuals selected for jointly appointed positions may be different than those selected for singly appointed positions. This complicates our analysis in various ways. We address these issues in detail in the paper's empirical section.

may spark opportunistic use of tactics that use unexpected events to reach existing goals (Miner 1987). In terms of maintaining a diverse workforce, specifically, jointly appointing underrepresented minorities is a useful tactic in that it offers a relatively simple way to increase the *appearance* of diversity. At the department level, a jointly appointed minority faculty member appears on two different departmental registers (and websites), increasing the visible diversity of both. The result is thus similar in both theory and practice to the long standing observation in organizational sociology whereby firms adopt certain practices and policies to appear more legitimate (e.g., Meyer and Rowan 1977; Zajac and Westphal 2004; Westphal and Graebner 2010; Smith and Chae 2016; Westphal and Park 2020). From the university's point of view, jointly appointing demographic minorities may allow them to take advantage of cluster or joint hire slots to meet the goal of increasing workforce diversity in addition to research interdisciplinarity (Berrey 2015). Whereas joint appointment can be opportunistically developed around individuals as idiosyncratic jobs (Minor 1985), the potential for universities to systematically deploy this practice further constitutes an enabling condition for within-job segregation (Chan and Anteby 2016). Accordingly, we hypothesize:

> *(H1) Under-represented minorities are more likely to be jointly appointed than non-minorities.*

Our first hypothesis stems in part from the fact that academic departments often maintain their own internal diversity goals, in addition to the goals of the university as a whole. If departments intentionally utilize joint appointments to achieve diversity goals, then it should be the case that joint appointments of underrepresented minorities are most likely to occur at times

when and in departments where the *lack of diversity* is most salient. This is consistent with the theoretical prediction that practice adoption is affected by the functional needs for the underlying practices (Tolbert and Zucker 1983; Dobbin et al. 2011). Dobbin and colleagues (2011) argue that organizations have greater functional needs for diversity practices in the presence of greater regulatory pressures and in the absence of workforce diversity. In this case, departments of the same university experience similar regulatory pressures, while those that notably lack diversity in the workforce will have markedly greater functional needs for practices that create diversity. Thus, we further propose:

> *(H2a) Academic departments having no underrepresented minorities are more likely to jointly appoint an underrepresented minority faculty member.*

and,

> *(H2b) Academic departments having fewer underrepresented minorities are more likely to jointly appoint an underrepresented minority faculty member.*

Before moving to our third set of hypotheses, it is worth noting that prior organizational research has illustrated patterns opposite to those described in hypotheses 2a and 2b; namely that organizations with more minorities are more likely to adopt diversity practices and advocate hiring additional minorities (e.g., Bobo and Kluegel, 1993; Dobbin et al., 2011; Steeh and Krysan, 1996). Furthermore, it is possible that minority job candidates may be more attracted to and more likely to accept employment offers from organizations that are already diverse. We do not disagree with this prior work. Given our opposing expectation, H2a and H2b can be read as conservative tests of our argument.

Whereas our second set of hypotheses speak to an antecedent, or "upstream" cause of the first hypothesis—specifically, that the functional need for diversity in an academic department may account for between-race differences in the likelihood of being jointly appointed—our next two sets of hypotheses focus on two potential "downstream" consequences. In short, we expect that jointly appointed faculty experience unique costs that singly appointed faculty do not.

These additional costs come in two forms at least. First, jointly appointed faculty may be subject to greater demands on their time—that is, they may be "spread thin" by additional meetings, seminars, student advising, service, and so on—which can negatively impact their productivity as both teachers and research scholars (e.g., O'Meara et al. 2017; Chan and Anteby 2016; Babcock et al. 2017; Leahey, Beckman, and Stanko 2017).[12] Research on task assignment in the higher education context documents a widely agreed upon consensus as to how tasks rank by promotability (Babcock et al. 2017). In research-oriented universities, committee works and services to the university are widely regarded as tasks of low promotability whereas conducting research is presumed to be highly promotable (Babcock et al. 2017; O'Meara et al. 2017). To the extent that committee work and services tend to be organized at the department level, jointly appointed faculties experience systematically more time commitments to low promotability tasks as they juggle service demands from multiple departments. As a result of the disproportionate exposure to time-consuming service activities, jointly appointed faculty have systematically fewer hours to spend on academic research which may lead to lower research productivity. In addition to the opportunity cost of time, performing these service tasks may have negative

[12] In an additional analysis described later in the text, we investigate the causal effect of joint appointment on research productivity directly. Because this analysis utilizes a smaller, random sample of faculty, we opt to distinguish it from our main hypothesis tests.

externalities as faculty may experience perception of inequity and reduced job satisfaction (O'Meara et al. 2019; Chan and Anteby 2016; Babcock et al. 2017). Even more generally, as academic departments differ in norms and expectations involving teaching and service, jointly appointed faculty face the unique burden of satisfying the expectations of two or more differing contingencies. As knowledge work can be exceedingly demanding (Michel 2011), the general burdens of joint appointment may constitute extra hurdles that handicap one's research productivity.

Second, jointly appointed faculties are likely to experience evaluation penalties irrespective of research productivity. To start, as an important component of an individual's identity derives from the group or groups with which the individual is affiliated, jointly appointed faculty—being not *of* any single department despite being *in* two or more—may come to have more ambiguous identities as compared to their singly appointed peers (cf., Tajfel and Turner 1999). Prior research has pointed to a plethora of consequences associated with such identity ambiguity, including increased psychological strain for the individual having an ambiguous identity (cf., Swann 1983; Polzer, Milton, and Swann 2002), and greater difficulty among third parties when evaluating an individual with an ambiguous identity (e.g., Zuckerman 1999; Smith 2011). The latter consequence stems from the fact that evaluation typically involves comparison with a known reference set. When it comes to evaluating jointly appointed faculty, it may be difficult for a set of evaluators—i.e., colleagues and promotion committees—to determine the "right" reference set for evaluation.[13] As prior research has shown, this difficulty

[13] As in the case of productivity, H3a and H3b test the evaluation-based mechanism only indirectly. In a subsequent set of analyses we explore this mechanism in greater depth by comparing the consequences of being jointly appointed in two or more intellectual similar departments versus intellectual dissimilar. If it is indeed difficult to

often results in negatively biased evaluations.

We further propose that the evaluation penalties associated with joint appointments are intensified by the micro-level processes underlying performance evaluation. To start, the costs associated with joint appointments due to the identity problem can be exacerbated by the fact that individual departments are less likely to be held accountable for their evaluation decisions of joint appointments compared to regular appointments. Whereas accountability arises when decision makers need to justify their decisions to relevant constituents (Tetlock 1999; Castilla 2008), the evaluation of joint appointments is distinct in that decision-making is diffused among multiple departments which obscures the specific decision makers. Therefore, from the perspectives of the participating departments, accountability is less likely to be triggered in the evaluation of joint appointments as decisions are less attributable to individual departments. Inasmuch as accountability has been found to be key to counteracting managerial biases in evaluation (Kalev et al. 2006; Castilla 2008; Kalev 2014), departments will be less likely to restrain evaluation biases due to the identity problem when it comes to evaluating jointly appointed faculty.

Additionally, joint appointment has further implications for the micro processes underpinning evaluation through its effect on networks. Performance evaluation often requires rich and idiosyncratic information (Kalev 2014) which flows in part through networks. In particular, studying the role of managerial network influence in performance evaluation, Castilla (2011) documents the distinct effects of both manager-employee and manager-manager homophily on evaluation, which is consistent with the insight that information flow can be

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evaluate a jointly appointed faculty member, this difficulty should be greatest when the evaluating departments are further from one another in intellectual space.

enhanced through direct contact as well as echo chamber (Castilla 2011; Burt 2000, 2001). In this case, jointly appointed faculties—by virtue of belonging to more than one department—not only have less access to direct contact with evaluators from each individual department, but are also less likely to benefit from an echo effect insofar as their evaluators come from multiple departments rather than one closed group (Burt 2000, 2001).

Importantly, it is also possible that individuals who are jointly appointed in multiple academic departments are more likely to conduct research across disciplinary boundaries. The impact of such research notwithstanding, Leahey, Beckman and Stanco (2017) found that conducting interdisciplinary research may inadvertently stifle one's research productivity and negatively impact evaluation outcomes due to difficulties specific to both the production and evaluation of interdisciplinary research (Leahey et al. 2017). In other words, being jointly appointed may also be associated with systematic differences in the work contents of one's primary tasks in this specific case. As our theoretical conceptualization of the paradox of diversity promotion implicitly holds constant the contents of one's primary tasks, we take caution to control for this important yet unique mechanism specific to this empirical context in the following analyses.

Taken together, the additional costs of jointly appointing untenured assistant professors lend to the following two sets of hypotheses:

(H3a) Ceteris paribus, jointly appointed (untenured) faculty will be evaluated more poorly than their singly appointed peers as manifested in annual salary.

and,

(H3b) Ceteris paribus, jointly appointed (untenured) faculty will be less likely to

be promoted to tenure.

More specifically, the two consequences of being jointly appointed—the first related to productivity and the second to evaluation—also lead to the following set of specific hypotheses regarding the productivity and evaluation penalties underlying the costs of joint appointment. Specifically, as jointly appointed faculty are spread thin, we propose that they experience lower research productivity after being jointly appointed as compared to singly appointed counterparts. The reduced productivity notwithstanding, joint appointment is associated with additional penalties specific to evaluation due to its implications for one's identity as well as micro-level processes of performance evaluation. Furthermore, if the consequences of being jointly appointed stem in part from the difficulties associated with evaluating jointly appointed faculty, then we expect that these consequences should be greatest in cases where the jointly appointed faculty member is in a disciplinary department where evaluators are less equipped to overcome those difficulties. Therefore, we hypothesize:

(H4a) Ceteris paribus, jointly appointed (untenured) faculty experience lower productivity than their singly appointed peers.

(H4b) Jointly appointed (untenured) faculty will be evaluated more poorly irrespective of research productivity.

(H4c) Jointly appointed (untenured) faculty will be evaluated more poorly in

disciplinary departments than in interdisciplinary departments.

Unlike our first two sets of hypotheses which involve race specifically, H3 and H4 are notably race independent. We expect joint appointments to create additional costs for all faculty, and all untenured faculty in particular, regardless of race. Before turning to discuss our data and analytic approach, we hasten to point out that as long as our first hypothesis is supported, our third and fourth set of hypotheses carry significant race-related implications. If underrepresented minorities are indeed more likely to be jointly appointed compared to their non-minority counterparts, and if jointly appointed faculty face professional costs not experienced by their singly appointed colleagues, then underrepresented minorities by default are at greater risk of experiencing certain professional consequences than others due to disproportionate allocation to positions that carry negative implications for productivity and evaluation.

METHODOLOGY AND RESULTS

Data and Sample

We test our hypotheses using comprehensive administrative employment records from a large (R1) U.S. public university, with which we combined extensive first-hand data on the university faculty employees' demographic information, publication records, and career mobility. Constructing a unique and comprehensive set of data on the demographic, career, and publications information for faculty employees of the focal university, we treat this context as a revelatory case that reveals novel insights into what are previously unexamined (Yin 1994; Colyvas 2007). Despite inherent limitations on generalizability, revelatory cases are useful to examine social phenomena which are unknown or understudied by previous scholarship (Glaser

and Strauss 1967; Rivera 2017). Additionally, as we seek to examine how allocative mechanisms motivated by diversity efforts affect the under-represented minority individuals' career outcomes, focusing on one university helps to hold constant the mechanisms pertaining to evaluation at the university level (Petersen and Saporta 2004).

While our data do not directly quantify the implications of joint appointments at other higher education organizations, our manual search for anecdotal evidence confirms the prevalence of joint appointment at other U.S. universities. As previously mentioned, among a random sample of fifty R1 universities, nine in ten note the possible use of joint appointments on their websites and seven in ten have recent announcements of cluster hires which often involve the use of joint appointments. Combined with past findings that American higher education organizations tend to resemble one another in their organizational structures and practices (see Dey, Milem and Berger 1997), these anecdotes suggest that our findings may have broader implications that are generalizable to other higher education organizations beyond our immediate context.

We began with the university personnel data which extend from 1990 to 2016 and include the following variables: individual name, academic department, position or rank, year, and annual salary. As we are interested in the career development of academic researchers, in particular, we restricted the sample of university employees to include only tenured and tenure-track faculty, or individuals who have worked at the positions of Assistant Professor, Associate Professor, and/or Full Professor. We further restricted the sample to the university's main campus and followed convention to exclude records from the university's medical and dental schools as both schools follow notably different personnel policies relative to the remainder of university schools and departments (e.g. IPEDS 2017). The final university personnel dataset on which we

drew for our further data collection and analyses was an unbalanced panel containing 51,633 person-year observations, and the individuals were appointed to 83 distinct academic departments.

In addition to the university personnel records, we also manually coded individual's demographics, fields of study and publication records using a combination of sources including departmental webpages, personal webpages, Microsoft Academic Graph, Google Scholar, Scopus, and various other online resources. To conduct relevant robustness tests, we also coded individuals' PhD granting institution and professional mobility both before and after employment at the focal university for a subsample of individuals. Because of the extensive use of departmental and personal websites in the academic context, we were able to code the key demographic variables for 95% of the individuals in our university personnel dataset. As our analyses depend on substantial efforts of data collection and coding, we discuss in detail how we collected these additional data and measured the key variables in the next section.

Variables and Measures

Joint appointment: We used administrative records at the individual level to determine whether a person was jointly appointed. Specifically, we coded joint appointment as a binary variable equal to one if an individual was *formally* affiliated with two or more academic departments in the same year, and zero otherwise. Only formal joint appointments appear in the university's administrative record. Informal and/or "courtesy" appointments are not included in our data. We manually verified a random sample of the cases marked as indicating joint appointment using university webpages and individual CVs.

Demographic variables: We manually coded the demographic variables with respect to

gender and race using public data made available on personal and department websites. Specifically, there is growing consensus in sociology that race is a socially constructed characteristic (American Sociological Association 2003; Saperstein and Penner 2012) that encompasses different dimensions (e.g., Espiritu 1993; Frederickson 2002; Saperstein and Penner 2012) and malleability on both macro and individual levels (e.g., Nobles 2000; DaCosta 2007; Saperstein and Penner 2012; Nix and Qian 2015; Liebler et al. 2017). The extant literature highlights two distinct dimensions of race that both carry important implications for individuals' life experiences pertaining to self-identification and perception by the others (Espiritu 1993; Frederickson 2002; Saperstein and Penner 2012). Manifesting this multi-dimensional nature, empirical measures of race fall in two broad categories that are either self-selected or observer-selected (e.g. Telles and Lim 1998; National Research Council 2004; Saperstein 2006; Nix and Qian 2015). Specifically, growing research in social cognition suggests that observer-selected racial classification may have as much influence on people's life experiences as self-identification, as social cognition research documents an automatic process for observers to engage in racial classification when encountering faces (Telles 2002; Saperstein 2006; Brown, Hitlin and Elder 2007; Campbell and Troyer 2007; Saperstein and Penner 2012).

Inasmuch as departments and organizations alike may seek recognition for their diversity-related achievements, we adopt an observer selection approach that involves racial classification by perceivers. Recent research using the observer selected measure has relied on either visual information (e.g. Rider et al. 2016) or names (e.g. AlShebli 2018). We follow the former convention and code the demographic variables on gender and race visually because of evidence for the automatic association of racial classification with facial perception (Telles 2002; Saperstein 2006; Brown, Hitlin and Elder 2007; Campbell and Troyer 2007; Fu et al. 2014).

Before moving on to our specific measures, we hasten to note that empirical attempts at racial classification usually involve intricacies for both observer-selected and self-selected measures, in part due to the socially constructed nature of the construct (Saperstein 2006; Nix and Qian 2015; Liebler et al. 2017). Specifically, shifts in subjects' social and economic status (Saperstein 2006; Saperstein and Penner 2012) and the observers' motives for dominance (e.g. Krosch et al. 2013; Kteily et al. 2014; Krosch and Amodio 2014) are two examples of the factors that may affect observer-selected racial classification.

While our empirical context holds constant subjects' employer and consequently their related social and economic status, we took a wary approach to derive the observer selected measure of race. Specifically, we derived our main measure with an ethnically diverse group of human observers, and an alternative measure with a machine learning method that tests the robustness of our main results to this alternative measurement method in the appendix. To start, for every individual in the sample, two independent raters manually coded race and gender using information made public on individuals' personal and department websites. The raters relied on photos predominantly, while also referring to names and relevant diversity experiences[14]. For the first two rounds, the raters agreed in their judgments for 94.5% of the time, yielding substantial interrater reliability (Cohen's $\kappa = 0.84$)[15]. A third independent rater conducted a third round of coding when the first two raters disagreed. We then derived our main observer-selected measure of race based on majority agreement when at least two rounds reached agreement in the classification. Due to the low representation of some minority groups, as well as several unique

[14] They referred to the racial classification adopted by the U.S. census bureau, Population Estimates Program (PEP) while adding a category "unknown" if they were unable to find the individuals or to code with confidence.
[15] The calculation excludes cases that raters did not specify a racial category and indicated "unknown". Interrater reliability remains good (Agreement = 89%; Cohen's $\kappa = 0.715$) when such cases are included.

features associated with Asian faculty, in particular, we constrained our primary analyses to examine differences between African American and white faculty specifically.[16]

African American Joint Appointment: To test our second set of hypotheses we created a department-level variable equal to one if a department jointly appointed one or more African American tenure-track faculty members in a given year, and zero if the department did not jointly appoint any African American tenure-track faculty members in that year.

Starting Salary: To control for any initial differences that affected the university's salary decisions at the time of hiring, we included a measure of starting salary in all the regression analyses involving salary (Rider et al. 2016). We measured one's starting salary using one's annual salary amount in the first year. For ease of interpretation, we standardized the starting salary amount within one's starting year in the following regression analyses.

Promotion: For all individuals who first appeared in the data as assistant professors, we used the administrative personnel records to code whether they were promoted to the level of associate professor with tenure. Naturally, a limitation of the university's administrative data is that we do not have a direct measure indicating whether a lack of promotion was voluntary or involuntary. To mitigate the resulting measurement liability, we coded the year in which an un-promoted faculty member left the university *and* the positions and school to those individuals moved immediately upon leaving the university. We then eliminated cases where a faculty member left the university (1) more than two years prior to his or her scheduled tenure review and/or (2) for a higher-status institution. Although this coding is imperfect, we assume that an assistant professor's exit from the data is indicative of voluntary exit when his or her subsequent

[16] Compared to all other minority groups, Asian faculty are overrepresented relative to their percentage in the overall U.S. population. Our results are not conditional on excluding other races from the analyses.

job was at a more prestigious institution than the focal institution (according to rankings in US News and World Report). When this was not the case, we assume that an assistant professor's exit was involuntary. Each of these adjustments should raise the likelihood that what is coded to be a failed promotion is indeed that. As we show in the appendix (Table A4), our results are consistent when we instead code *all* pre-tenure exists from the data as indicating failed promotions.

Annual Salary: We measured one's annual salary amount using direct reports from the university administrative records. While annual salary increases can depend on several factors—including basic cost-of-living increases—we assume that the size of an individual's raise relative to other individuals in comparable positions reflect at least in part an evaluation of a focal individual's performance. Accordingly, we treat annual salary as an important, albeit imperfect measure of how an individual was evaluated for her performance over the prior year. In Appendix (Table A4), we take the natural log of annual salary and show that our results remain robust to measuring salary with log transformation.

Research Performance: We measured research productivity and performance using data from the Microsoft Academic Graph (MAG) database[17] which provides the most extensive coverage on research outputs to date. Due to various difficulties with author name disambiguation, we conducted a cautious matching that involved two steps. We began with an automatic matching process using individuals' last names, the initials of their first names, and institutional affiliation with the focal university. For individuals that we did not identify one and

[17] As a robustness test, we also have productivity measure for a subsample of individuals using Scopus data, which yielded qualitatively consistent results as the full sample analyses based on the MAG data.

unique match in the MAG database, we conducted manual check relying on information including authors' webpages, Google Scholar, CV, department and field of study.

Specifically, we measured two sets of indicators of research performance pertaining to productivity and citation impact. First, we followed the literature convention and measured research productivity using article counts (e.g. Leahey et al. 2017). We first derived our productivity measure by counting the total number of all research outputs that individuals accumulate per year in the MAG database, which include journal articles, books, book chapters and conference proceedings. Among them, we further differentiate the types of research outputs by specifying the number of journal articles and the number of books produced per year. Second, we measured the total citations at the author-year level as a proxy for the impact of their research outputs. Albeit an imperfect measure, the number of citations is prevalently used as a proxy for impact and quality gauged via peer reception and provides a standardized measure across disparate disciplines (e.g. Uzzi et al. 2013; Leahey et al. 2017).

Research Interdisciplinarity: Additionally, individuals who are jointly appointed in multiple academic departments may be more likely to conduct research across disciplinary boundaries. To account for the possibility that jointly appointed individuals may experience more productivity penalties due to greater engagement with interdisciplinary research that is difficult to produce and evaluate (Leahey et al. 2017), it is important to control for the level of interdisciplinarity of the research outputs. Following Leahey and colleagues (2017), we adopted the Porter and coauthors' metric (Porter et al. 2007) to measure interdisciplinarity on the article level, which was further aggregated to an author-year level measure of interdisciplinarity (Leahey et al. 2017).

Department Interdisciplinarity: We coded the interdisciplinarity level of each of the academic departments in our data according to the homogeneity versus heterogeneity of its faculty's most advanced degrees (Abbott 2001). Specifically, to measure departmental interdisciplinarity, we gathered information on the PhD majors of twenty randomly selected current tenured and tenure track faculty in each pertinent academic department of the focal university[18]. We then measured the diversity of faculty PhD majors in each department using the Normalized Shannon Entropy index (Eagle, Macy, and Claxton 2010). The smaller the measure, the smaller the diversity of faculty PhD majors and the more disciplinary the department (Abbott 2001). We list departments according to the measure in the appendix Table A6. The measure reveals strong differences between fields such as Philosophy and Economics—where all faculty have degrees from either Philosophy or Economics departments, respectively—compared to departments such as Organizational Studies and Biologic and Materials Sciences—where faculty hold degrees from many different fields.

Estimation Strategy and Results

We began our analyses by examining whether African Americans were more likely to be jointly appointed, as compared to their white colleagues (*H1*). Table 16 includes basic tabulations of the number and proportion of jointly appointed faculty by race and rank and reveals stark differences. Among all faculty ranks, 26% of African American faculty were jointly appointed compared to 10% of white faculty. This difference is even more striking among untenured assistant professors; whereas 25% of African American assistant professors were

[18] The total number of departments involved is slightly smaller than that of the full sample due to department evolution overtime and the exclusion of administrative programs.

jointly appointed within our sample period, the frequency of joint appointment among white assistant professors was less than 6%.

To account for other factors that may affect the likelihood of joint appointment, we used a standard logistic regression framework. Specifically, we estimated a series of logistic regressions of the following form,

$$\log\left(\frac{p_{it}}{1-p_{it}}\right) = \delta D_i + \beta X_i + \phi_t + \lambda_d + \varepsilon_{it}$$

where p_{it} is the probability of individual i being jointly appointed in year t, and D_i is an indicator variable equal to one if individual i is African American and zero otherwise. Thus, the coefficient δ on the indicator variable measures the increase in the log odds of joint appointment in a given year, holding constant other variables in the model. X_i is an indicator variable equal to one if individual i is female and zero if male. We additionally included a year fixed effect, ϕ_t, and department fixed effect, λ_d, to account for temporal and department-level differences in the propensity to use joint appointments. Results are presented in Table 17 and offer strong support for our first hypothesis. Among all professors (model 1), African American professors are approximately 2.08 times more likely to be jointly appointed than white professors, holding constant gender, time, and the department(s) in which an individual is appointed. Among assistant professors (model 2), specifically, African Americans are nearly 3.48 times more likely [=exp(1.248)] to be jointly appointed than their white colleagues.

To test hypotheses 2a and 2b, and better understand the "upstream" antecedents of the decision to jointly appoint African American faculty, we used a similar logistic regression

framework to estimate the likelihood that an academic department jointly appoints an African American professor as a function of the current racial profile of the hiring department (*H2*). Needless to say, our data do not include information on job offers that were turned down by a candidate. We are only able to observe successful joint appointments. Our models take the following form:

$$\log\left(\frac{p_{dt}}{1-p_{dt}}\right) = \delta D_d + \beta AA_d + \alpha \boldsymbol{X}_d + \phi_t + \lambda_d + \varepsilon_{dt}$$

where p_{dt} is the probability of department d jointly appointing an African American faculty member in year t. D_d is a binary variable equal to one if department d had zero African Americans in the previous year and zero otherwise. AA_d is a count of the number of African Americans in the department in the previous year. Thus, the coefficients δ and β measure the increase in the log odds of departments jointly appointing African American faculty members given the presence and number of African American faculty in the department over the prior year. In all specifications, we control for temporally variant department-level covariates, $\boldsymbol{X}_d$, including the size of the department measured by the total number of tenured and tenure track faculty and whether the department hired any relevant faculty that year. Finally, we include fixed effects at the level of the year, ϕ_t, and department, λ_d.

The models included in Table 18 offer support for hypotheses 2a and 2b. Model 3 reveals a significant, negative relationship between the number of African Americans in the department and likelihood of jointly appointing an African American across all ranks (β=-0.345, $p < 0.01$). Model 4 narrows the dependent variable by considering assistant professors only. The results

highlight a consistently negative relationship between the number of African Americans in the department and the likelihood of jointly appointing an African American assistant professor (δ=-0.434, $p < 0.05$), while suggesting a positive relationship between having no African Americans and the likelihood of jointly appointing an African American assistant professor (δ=1.505, $p < 0.05$), Ceteris paribus, departments having one additional African American in a given year are approximately one and a half times less likely to jointly appoint an African American assistant professor in a subsequent year. To reiterate, these results may be conservative estimates of the relationship between the demographic makeup of a department and the subsequent likelihood of jointly appointing an African American faculty member. As we discussed in the prior section of the paper, this is due to the fact that we also expect a countervailing effect whereby African American candidates may be more (less) likely to accept employment offers from departments having more (fewer or no) African Americans to begin with. It is not possible to independently control for this effect. Thus, our results are net of any tendency for African Americans to be more likely to join academic departments where other African Americans are already employed.

We next turned to examine the "downstream" consequences of being jointly appointed (as an assistant professor specifically) by analyzing individuals' career outcomes in terms of salary raises (H3a) and promotion likelihoods (H3b). First, as we are interested in the implications of joint appointment for one's earnings, we use standard OLS regression with the amount of annual salary as the dependent variable. In particular, we include starting salary as a covariate to control for factors that also influenced salary decisions at the time of hiring (Rider et al. 2018). We report consistent results with salary amount using logarithm transformation in appendix (Table A3). Second, we adopt logistic regression to examine whether joint

appointments affect one's likelihood of being promoted to the rank of associate professor with tenure. While past research has also modeled promotion outcomes using an exponential event history framework (e.g., Rider et al. 2018), our context in the academia is distinct in that the majority of assistant professors are evaluated for promotion after a similar duration, which renders a considerable number of ties in the time to promotion decisions. Therefore, we use a discrete time event history framework with logistic regression (Allison 1982; Rider et al. 2018) as our main method to model promotion decisions, and we account for right censoring by restricting the sample to individuals who were hired prior to the year of 2010. Specifically, the models used to test hypotheses 3a and 3b are as follows,

$$y_{it} = \alpha J_i + \delta D_i + \gamma J_i D_i + \beta \boldsymbol{X}_{it} + \phi_t + \lambda_d + \varepsilon_{it}$$

$$\log\left(\frac{p_{it}}{1 - p_{it}}\right) = \alpha J_i + \delta D_i + \gamma J_i D_i + \beta \boldsymbol{X}_i + \phi_t + \lambda_d + \varepsilon_{it}$$

where y_{it} is the amount of salary in year t and p_{it} is the probability that individual i gets promoted to associate professor with tenure in year t. J_i is an indicator variable that equals one if the individual is jointly appointed and zero otherwise. D_i indicates whether individual i is African American. $\boldsymbol{X}_{it}$ is a vector of control variables measured at the individual level that may include gender, starting salary, research productivity, research impact, and research interdisciplinarity. The models in Table 19 predict an individual's annual salary, and those in Table 20 predict an individual's likelihood of promotion to associate professor with tenure. As in prior models, all specifications include a year fixed effect, ϕ_t, and department-level fixed effect, λ_d.

Tables 19 and 20 offer strong support for hypothesis three. To start, the coefficient on African American in model 5 is insignificant, indicating that the raises experienced by African Americans are on average no different in magnitude from those experienced by their white colleagues. Model 6 adds the indicator variable, joint appointment, and confirms hypothesis 3a: jointly appointed assistant professors receive $920.7 less in their annual salary on average compared to singly appointed colleagues. This amounts to approximately 35% of the median annual raise in the sample. Models 8 and 9 in Table 20 examine the promotion likelihood among untenured assistant professors. In contrast to the prior findings involving salary, model 8 indicates that African American assistant professors do indeed face a marginally significant lower likelihood of promotion, on average, as compared to white assistant professors (δ=-0.317, $p < 0.10$). Model 9 confirms hypothesis 3b: jointly appointed assistant professors are significantly less likely to be promoted to tenure compared to their singly appointed colleagues (δ=-0.691, $p < 0.01$).

Importantly, the results in model 9 may speak to mediation; that is, that the negative relationship between African American and promotion likelihood (model 8) may in fact be explained by the greater percentage of African American assistant professors who are jointly appointed. Conversely, it is possible that the lower likelihood of promotion for joint appointments is due to lower conditional probability of promotion for African Americans who happen to be over-represented in joint appointment positions. To address this concern and test for mediation, we next test whether jointly appointed assistant professors indeed experience lower likelihood of promotion than singly appointed assistants holding constant their racial categories. Accordingly, models 11 and 12 in table 21 examine if the conditional probability of

promotion for joint appointments is lower than conventional appointments within African American and white subsamples respectively, controlling for individuals' gender, school and year fixed effects[19]. The results indicate significantly lower promotion likelihood for jointly appointed assistant professors within both African American and white subsamples. The result suggests that the disproportionate allocation of African Americans to positions of joint appointments account for the negative relationship between African American status and promotion likelihood. Table 22 presents consistent results of subsample analysis on annual salary.

Next, we highlight an additional set of analyses examining the proposed mechanisms underpinning the penalties associated with joint appointment as indicated by the fourth set of hypotheses. To recap, we argued that two different mechanisms are likely responsible for the career consequences associated with being jointly appointed. First, jointly appointed faculty may suffer from a decline in their productivity as a result of their having more administrative and advising responsibilities. Second, irrespective of the productivity decline, it may be more difficult to evaluate jointly appointed faculty than singly appointed faculty due to an identity problem compounded by specific micro-level processes underlying the evaluation of joint appointments.

To assess the first mechanism regarding productivity as implied by H4a, we attempted to gather comprehensive data on the pre- and post-hire publication records of all individuals in our dataset as previously introduced. We used a dynamic difference-in-differences estimation

[19] Due to the decrease in the sample size, we assign each department to one of eight academic super-categories, or "schools" and control for school fixed effect. Table A8 in the appendix lists the super-categories (also see Abbott 2001).

approach where we control for author and year fixed effects and include an indicator variable for post-appointment years. In particular, the post-appointment year for the jointly appointed assistant professors is the year when they began with joint appointments. As the majority of the joint appointments started at hiring, we specify the hired year as the post-appointment year for individuals with conventional appointments. We measure two dependent variables of productivity: (1) the total number of all research outputs and (2) the total number of journal articles and books that individuals have every year. As discussed, to account for the quality and the interdisciplinary nature of the works, we further control for the total number of citations and the level of interdisciplinarity (Leahey et al. 2017) at the author-year level. Importantly our sample includes the publication records for faculty who left the university. Models 15 and 16 in Table 23 contain the results of this analysis. Controlling for individual and year fixed effects, the citation impact as well as the interdisciplinarity of one's work, the rate of publication post-hire is notably lower among jointly appointed assistant professors compared to their singly appointed colleagues in terms of all research outputs (δ= -0.133, $p < 0.05$) and the number of journal articles and books specifically (δ= -0.130, $p < 0.01$), offering strong support for H4a.

To test the second mechanism, which relates to the difficulty of evaluating singly versus jointly appointed faculty, we gathered an additional set of data and conducted two additional analyses to test H4b and H4c specifically. First, we investigate whether the career outcomes differ by joint or conventional appointment status when individuals' research productivity and performance are accounted for. Table 24 shows the results on promotion and salary outcomes respectively and suggest a subtle difference in how the evaluative penalty for joint appointment plays out in different career outcomes. In particular, model 17 presents the results on annual

salary, and does not find significant difference in salary by appointment types after controlling for individuals' research productivity and performance. By contrast, model 18 presents the results related to promotion likelihood. In support for H4b, conditional on one's research productivity, impact, work interdisciplinarity, gender, race, as well as department and year fixed effects, the promotion likelihood for joint appointments remains significantly lower than conventional appointments (δ= -0.681, $p < 0.05$).

Second, H4c proposes that the evaluation penalties specific to joint appointments would be greater in a disciplinary department than an interdisciplinary department. Tables 25 and 26 point to the differential effects of joint appointment by department disciplinarity according to our measure. The models use a median split and the results of models 19 and 20 indicate that for promotion, jointly appointed assistant professors face greater consequences when their appointments are in disciplinary departments, holding constant our measures of research productivity and performance. Table 26 contains the results of a similar sets of analyses on salary outcomes. The results confirm that the salary-related consequences of being jointly appointed are more pertinent in disciplinary departments, though the result is only marginally significant statistically.

Our data reveal that jointly appointed assistant professors are disproportionately penalized even when our measures of research productivity and performance are accounted for. Also, the penalties for joint appointment are greater in disciplinary departments according to our measure, when the internal and relevant external evaluators are plausibly less equipped with assessing individuals with multiple appointments. Additionally, both analyses suggest that the evaluative penalties are more pronounced for promotion consequences than salary outcomes. Net

of the productivity penalties, these results speak to penalties specific to the evaluation mechanism that we proposed to underpin the career consequences associated with joint appointments.

Put together, the results of our empirical analyses thus far form a compelling, if distressing narrative. Academic departments having few or no African American faculty members display a higher likelihood of jointly appointing African American faculty (at the assistant professor level specifically). This is problematic in light of additional evidence demonstrating that being jointly appointed leads to clear, meaningful risks among younger faculty. Our results demonstrate two directly. Untenured faculty who are jointly appointed in two or more departments experience lower salaries and lower likelihoods of promotion to tenure. If African Americans are being jointly appointed for the sake of promoting diversity, the outcome is alarming: the desire to promote diversity may ultimately undermine it.

ROBUSTNESS TESTS & ALTERNATIVE EXPLANATIONS

Despite offering support for each of our hypotheses, and highlighting two distinct mechanisms underlying those hypotheses, the analyses presented thus far are not without certain limitations. We highlight several of these limitations and describe various attempts to adjust for them in this section.

To begin, it is possible that our effects may be due to unobserved initial differences between jointly appointed and singly appointed individuals. For example, if jointly appointed faculty were to have systematically lower initial (pre-hire) quality compared to singly appointed faculty, then underlying quality differences, and not differences in appointment status per se, may account for differing employment outcomes. Likewise, if departments are more likely to

accept a jointly appointed candidate who they are not enthusiastic about (because the alternative is to hire nobody), then jointly appointed faculty may be left vulnerable to negative evaluations. Finally, if jointly versus singly appointed faculty do indeed differ in their quality, it is possible that jointly appointed faculty may have lower capacity to negotiate salary increases and promotion outcomes.

We took several different steps to mitigate these concerns. First, we used US News & World Report ranking data to compare jointly appointed and singly appointed faculty with respect to their PhD (or highest degree) granting institutions. The results of this analysis, which are shown visually in Figures 8(A) and 8(B), reveal no significant differences between jointly and singly appointed faculty. Needless to say, this analysis is imperfect for several reasons, not the least of which is the fact that university-wide rankings do not perfectly correlate with quality at the department level. Next, we compared the pre-hire publication records of jointly appointed faculty with a matched sample of singly appointed faculty. Results of this analysis are included in Table 27. Model 26 uses fixed effects at the level of the school, or "super-category" from the prior analyses. Model 27 uses the more stringent department level fixed effects employed throughout. In both cases, the data show no difference between jointly appointed and singly appointed faculty with respect to their pre-hire research productivity. Third and finally, we replicated our main analyses by restricting our set of jointly appointed faculty to include only those who were jointly appointed at least one year *after* joining the university (as singly appointed faculty). In other words, we compared singly appointed faculty to a matched set of other singly appointed faculty *after the point at which individuals in the latter set took on a joint appointment*. Consistent with our prior findings, Table 28 (model 28) confirms that joint appointments are equally consequential among this restricted set: assistant professors who

remained singly appointed are approximately 3 times more likely to be promoted, on average, compared to those who became jointly appointed.

Lastly, as a fraction of African American assistant professors who were jointly appointed were affiliated with African American Studies and one other discipline, we further addressed the possible concern that the negative effect of joint appointment on one's career outcomes may simply reflect lower raises and promotion likelihoods in the African American Studies department. Models 32 and 33 in Table 29 reveal this is not the case. Model 33, in particular, demonstrates that the African American Studies department does not differ from other departments, on average, in their propensity to promote assistant professors to tenure. Most importantly, both models confirm that the consequences of being jointly appointed are not confined to the African American Studies department.

DISCUSSION

Organizations striving to increase diversity are facing a conundrum. On the one hand, many organizations are increasingly committed to promoting diversity and addressing the long-standing underrepresentation of certain minority groups amongst their workforces. On the other hand, underrepresented minorities may continue to be underrepresented in part as a function of their underrepresentation at earlier stages of the career cycle. Needless to say, the most effective long-term solution facing these organizations is to encourage underrepresented minorities to consider careers in their respective organizations and sectors and support those individuals to ensure their continued participation. By contrast, when it comes to achieving diversity goals in the short-term, organizations are facing a significant operational strain due to the mismatch between employer preference for demographic minorities who can embody diversity and the

preexisting underrepresentation of minorities to begin with.

Drawing on sociological and organizational research on the effect of task and demographic mismatch on workplace segregation (e.g. Chan and Anteby 2016; Reskin 1993), we propose that underrepresented minorities may be disproportionately assigned to tasks and positions meant to increase diversity as organizations seek to achieve diversity goals in the short-term. In the context of diversity promotion, this distortion in task allocation is often coupled with decentralized efforts and complex micro-processes that lower the promotability of diversity-related tasks (Kalev 2014; Babcock et al. 2017; Castilla 2011), compounding the negative career consequences for underrepresented minorities who are spread thin. We examine one instantiation of this paradox in the context of higher education. Specifically, we hypothesized and presented evidence showing that academic departments may attempt to manage their diversity targets by using joint appointments that allow underrepresented minorities to be formally affiliated with multiple departments, which in effect increases diversity and inclusion. Nevertheless, the consequences of this practice are severe. Joint appointments can overexpose early stage faculty, and racial minorities who are overrepresented in this type of appointments in particular, to a set of unique risks related to both research productivity and performance evaluation that undermine their career development overtime.

In this important respect, we theorize the unintended consequences of organizational efforts to promote diversity as a novel mechanism that generates allocative inequality. In doing so, our paper first contributes to the line of research on the *allocative mechanism* of discrimination (Petersen and Saporta 2004), whereby minorities are disproportionately sorted into less desirable positions and careers that have lower pay and lower rates of promotion (e.g. Anderson and Tomaskovic-Devey 1995; Reskin and McBrier 2000; Kmec 2005; Rosenfeld

1992; Baldi and McBrier 1997; Barnett, Baron, and Stuart 2000; Petersen, Saporta, and Seidel 2000; Elvira and Zatzick 2002; Fernandez and Sosa 2005; Fernandez and Fernandez-Mateo 2006; Chan and Anteby 2016; Rider et al 2016; Ferguson and Koning 2018). In the context of higher education, we can only speculate on the level of intentionality behind this sorting. While it may be unsurprising that joint appointments (at the junior level, specifically) negatively affect productivity, evaluation, and the likelihood of promotion, we believe it would be an overstatement to suggest that universities differentially target minorities for such positions *because* of the consequences associated with them. Instead, our data suggest an alternative mechanism: that departments utilize joint appointments not only as a way to extend resources, but also to work towards diversity-related goals.

As our results further demonstrate, we identify an important and novel conduit by which racial disparities can persist in organizations due to unintended consequences of organizational practices meant to remedy inequality. Drawing on the example of joint appointment as a formal personnel practice affected by racial considerations, our findings are consistent with prior research which argues that formalized organizational structures involving race and gender considerations may function as sources of biases and inequality (e.g. Kmec 2005; Bielby 2012; Kalev 2014; DiPrete 1989). Importantly, we point to the subtle and novel insight that organizational structures that increase the *inclusion* of minorities in the short-term may mask job and evaluation characteristics that ultimately result in *exclusion* overtime, inadvertently intensifying the "pipeline" problem alluded to earlier. Taking an organizational approach, our findings also complement the broader literature on the impact of within-firm processes on workforce demographic composition (e.g. Kalev et al. 2006; Petersen and Saporta 2004; Fernandez and Fernandez-Mateo 2006).

A third contribution of our paper is to complement research on the role of work contents as a within-job inequality-generating mechanism. Concurring with Barley and Kunda's (2001) call on inequality researchers to "bring the work back in", a growing line of research has considered within-job inequality that pertains to segregation at the task level, wherein minorities may be disproportionately allocated to devalued tasks within organizations (e.g. Babcock et al. 2017; O'Meara et al. 2017; Williams and Dempsey 2014). In particular, Chan and Anteby (2016) find that the implication of task segregation extends beyond the sorting of minorities to devalued tasks. Our paper adds to this important insight by providing a nuanced account as to how organizationally valued practices related to diversity become devalued in individuals' performance evaluation. Importantly, our specific arguments take into consideration the intricate interplay between formal work structures and actors' agency and accountability (Kalev 2014; Castilla 2011). In this respect, our research also highlights the importance of using a nuanced approach that considers the contingencies rooted in actors' agency when it comes to studying formalized structures and their implications for inequality (see Kalev 2014; Kalev et al. 2006).

Our findings also carry important theoretical and practical implications for diversity management. For starters, a lack of awareness among decision-makers may contribute to a lack of accountability for the inequality consequences of their decisions (Kalev 2014; Brooks and Purdie-Vaughns 2007). This may be relevant especially as our analyses highlight unintended consequences that take time to emerge. In this case, one practical implication for elevating accountability is to provide decision-makers with comprehensive data that reveal the longer-term effects of their personnel decisions. More importantly, our analyses on how organizationally valued diversity tasks and roles become devalued in individual performance evaluation points to insights that echo prior research on the importance of transparency and managerial accountability

in diversity management (Castilla 2008; Kalev et al. 2006).

Another pertinent implication concerns whether these diversity practices have positive effects on other minority individuals who are earlier in the career cycle (Bertrand et al. 2019). As prior research has argued, demographic composition can be self-reinforcing (e.g. Stainback and Tomaskovic-Devey 2012; Kanter 1977), wherein early entrants may function as internal advocates for future inclusion of the underrepresented minorities (Dobbin et al.2011; Dobbin and Kalev 2019). Nevertheless, Bertrand and colleagues (2019) find scarce support for a spill-over effect on younger women due to an exogenous increase in female board membership brought by the regulatory mandate of gender quota on corporate boards in Norway. By analogy, do joint appointments among minority faculty help to address the "pipeline" problem by increasing enrollments of minority students? In a supplemental analysis (not reported here, but available from the authors) we address this question using data on student enrollments at undergraduate, masters, and doctoral levels. The results of this additional analysis point to a weak but positive relationship between jointly appointing minority faculty in a given department and the subsequent enrollments of undergraduate minority students in that department. There were no observable effects among students at masters or doctoral levels. This is not, and should not, be a case of the ends justifying the means. Rather, if future research were to find stronger evidence that the disproportionate use of joint appointments among minority faculty has a direct, causal effect on increasing the enrollments of minority students, universities would be wise to put in place practices meant to alleviate the individual-level consequences that we have presented here.

More broadly, our findings also offer a meaningful example of how unintended consequences of organizational practices can sometimes work counter to the very outcomes they are meant to achieve. As early as 1936, Robert K. Merton called for “a systematic treatment” of

unanticipated consequences of purposive actions (1936: 894). A number of researchers in sociology and organizational theory alike have since taken up this challenge (e.g. Castilla and Benard 2010; Chan 2016; McDonnell, King, and Soule 2015; Tilcsik 2010; Turco 2012; Cattani 2005; Berrey, Nelson and Nielsen 2017; Kalev 2019). For example, McDonnell et al. (2015) demonstrated how defensive posturing among a set of organizations that are chronically targeted by social movement activists can unintentionally increase corporate receptivity to future activist challenges. Although our empirical context differs notably from this prior work, the overarching theme of the research is the same. In our case, efforts to promote racial diversity at the group level can lead to a series of unintended consequences that result in inferior career outcomes among minorities.

Lastly, we discuss the enabling conditions of our findings that can be generalizable to a proliferation of contexts. Pertaining to the upstream of joint appointment, one primary condition is the growing organizational desire for the rapid achievement of diversity despite important operational strains due to the persistent underrepresentation of minorities in higher education. Naturally, the organizational desire for diversity is not comparable across all industries and organizational contexts. As external pressures in an organization's legal and normative environments constitute major sources of motivation for diversity efforts (e.g., Dobbin et al. 2011; Kalev 2009), we point to the prevalence of such enabling conditions in a myriad of organizational contexts. To begin with, many organizations are faced with regulatory mandates to pursue diversity. A prominent instance is the mandated use of quota related to demographic representation. For example, companies are subject to regulations such as gender quota on corporate boards that directly make specific diversity goals mandatory for organizations in Norway (Bertrand et al. 2019; Ahern and Dittmar 2012). More broadly, regulatory pressures

have been found to motivate diversity promotion among various organizations with heightened legal awareness (Kalev 2009), including organizations in the public sector (Wilson, Roscigno, and Huffman 2013; Steinberg et al. 1990), private companies under legal oversight such as government contractors (Kalev et al 2006) and companies that have experiences with discrimination charges or affirmative action reviews in the past (Bielby 2012; Kalev and Dobbin 2006). In addition to law enforcement, some companies and organizations, relative to others, are faced with tremendous normative pressures to achieve diversity. In particular, such normative pressures tend to be more pronounced in industries with a pro-diversity culture (Dobbin et al. 2011) and in contexts with greater visibility that involve high-profile organizations as well as salient organizational positions such as corporate boards (Chang et al. 2019; Chu and Davis 2016; King 2008; Gardberg and Fombrun 2006). Insofar as the perpetuation of the underrepresentation of demographic minorities is pervasive in many industry and organizational contexts, the conditions conducive to joint appointment as a case of paradox of diversity efforts may generalize to a variety of organizations, especially those under greater legal and normative scrutiny.

Another important enabling condition for the generalization of our findings pertains to organizational contexts that involve career ladders wherein early conditions affects one's future career attainments (e.g. DiPrete and Soule 1988; Rosenbaum 1979; Steinberg et al. 1990). Specifically, our analysis on the downstream consequences of joint appointment points to unintended yet grave implications among untenured assistant professors for their future career outcomes in terms of earnings and career milestones such as promotion to tenure. Our findings may readily generalize to organizational careers organized with hierarchical ladders whereby one's initial conditions have profound impacts on long-term career trajectories and the

advancement through promotion milestones. This is relevant across many professions (Briscoe and Kellogg 2011), white collar jobs, and large organizations with internal labor market (Steinberg et al. 1990). For a prominent example among professional labor, Briscoe and Kellogg (2011) analyze a unique longitudinal dataset on professional careers in a large law firm and reveal a strong initial assignment effect on lawyers' career trajectories. Specifically, they document that being allocated to different initial conditions at random may well account for later career outcomes such as performance pay and attrition among a group of lawyers. Additionally, while our setting does not require making competition salient, the conditions that enable our findings may be further magnified in other organizational contexts with career ladders that involve a sequence of competitions in the form of tournament mobility (e.g. Rosenbaum 1979; Ehrenberg and Bognanno 1990).

CONCLUSION

The inclusion of underrepresented minorities is an important goal of many organizational leaders and policy-makers. As evidence of this, consider that the amount of minority group representation has long been a key measure of managerial performance (e.g. Dobbin, Kim, and Kalev 2011; Dobbin et al. 2015). In addition, policy-makers in several countries have gone as far as to mandate compulsory quotas to ensure the increase of minority group representation (Bertrand et al. 2014; Pande and Ford 2009). In spite of the many successes of such practices and policies, however, our findings indicate that an increase in the desire for greater representation of minority groups at the aggregate level does not necessarily translate to greater advancement among individual minority group members. It is thus critical to go beyond aggregate level metrics when assessing the efficacy of diversity management efforts. In the context of academia,

the quest for increased representation of minority faculty has positively affected the demand for jointly appointing minorities such that individuals can become "stretched too thin." Outside of academia, as organizations become "flatter" and work continues to shift towards project-based initiatives, it is not difficult to imagine parallel processes in other organizational contexts. Universities and organizations must be attuned to the kinds of unintended effects we have documented here and manage them in ways that do not undercut their goals.

Conclusion

Put together, this dissertation aims to contribute to the literature on status with new evidence and insights. Specifically, I discuss below several intended contributions.

Challenges to Cumulative Advantage

Following Merton's (1968) seminal work that conceptualized the Matthew Effect of status, a large body of research has demonstrated that status can lead to cumulative advantage as unearned benefits flow to those who occupy high-status positions. Examples of the unearned benefits that accrue to both high-status individuals and organizations alike come in a variety of forms, such as greater recognition (Simcoe and Waguespack 2010; Azoulay, Stuart and Wang 2012), favorable biases (Kim and King 2014), higher pricing power (Podolny and Phillips 1996; Uzzi and Lancaster 2004; Roberts et al., 2011), access to capital (Stuart, Hoang, and Hybels 1999), and lower costs associated with labor (Phillips 2001).

In contrast to the many benefits that status may generate for its possessors, a growing body of literature has begun to consider the mechanisms whereby the cumulative advantage of status may be limited (Rhee and Haunschild 2006; Malmendier and Tate 2009; Bothner, Kim, and Smith 2012; Hahl and Zuckerman 2014; Kovacs and Sharkey 2014; McDonnell and King 2018; Jensen 2008; Jensen and Kim 2015). My dissertation draws inspiration from this line of research and aims to advance our understanding as to how the cumulative advantage of status can be challenged. The self-perpetuating property of status has grave consequences when it comes to aggravating inequality. By studying the limits and challenges to the cumulative

advantage of status, this dissertation also aims to provide broader implications for efforts to dismantle inequality.

Specifically, chapter 2 argues that actors may manage the implications of status by managing their information environments proactively. An important enabling condition for Matthew effect of status is information asymmetry (e.g. Kim and King 2014; Correll et al. 2017). In this chapter, I argue that enabling (or disabling) information asymmetry in one's information environment may be one way whereby organizations may manage the consequences stemming from their status positions. Furthermore, chapter 3 considers the role of alternative status hierarchies conferred by different audience groups. By studying the effect of public status and its interaction with intra-professional status in the context of science and academia, this chapter aims to shed light on the possibility to consider alternative status hierarchies as a mechanism whereby cumulative advantage within one hierarchy may be limited. Lastly, chapter 4 explicitly examines organizational efforts meant to remedy inequality arising from diffuse status characteristics such as race in the context of academia. The findings highlight a paradox whereby efforts to combat inequality may unintendedly result in subtle workplace segregation. The finding speaks to the intricacies when it comes to dismantling cumulative advantage of status.

The Role of Third Parties in the Making of Status

A growing body of research on the determination of status has pointed to the role of third-party arbiters (e.g. Rao 1994; Sauder 2006; Baum and Oliver 1991; Correll et al. 2017; Cattani et al. 2017). Consistent with this line of research, I study status distinctions conferred by third parties and the media in chapters 1, 2, and 3. However, my dissertation departs from prior research on third parties by taking a step back to consider the specific processes underlying the

construction of status by third parties. Specifically, sociological research on rankings reveals that the production of rankings involves an inherent process of commensuration that generates artificial distinctions which cannot be fully reduced to differences in underlying quality (Espeland and Stevens 1998; Carruthers and Stinchcombe 1999; Espeland and Sauder 2007). As Merton (1968) notes in his observation of "the 41st chair", one salient artificial distinction that rankings generate is the discontinuity in status around the endpoint of rankings and prizes. Despite smooth and continuous distribution of quality around the artificial endpoint, rankings and prizes have inevitably limited supply of positions. In a similar vein, because media attention is timely and limited in supply, only selected events can make it into the media despite many other important events happening around the same time.

In this dissertation, I point to the key insight that the mismatch between the limited supply of third-party attention and a continuous distribution of candidates competing for attention generates artificial breakpoints in status hierarchies produced by third parties such as the media. Drawing on this insight, this dissertation employs quasi-experiment designs such as regression discontinuity designs and instrumental variable regressions in the context of prominent business ranking and media attention to professionals in chapters 1, 2, and 3. Importantly, there is a prevalence in the inherent mismatch between the limited supply of attention from influential third-parties and the continuous distribution of candidates. Therefore, the insight to exploit artificial breakpoints in status stemming from this mismatch may provide promising empirical avenues to investigate the causal effect of status produced by third-party arbiters in a proliferation of contexts.

Empirically speaking, a pertinent contribution of this dissertation is to identify the causal effect of status in ways that prior research has been unable. Underlying the Matthew Effect is a causal assumption that status, net of underlying quality, causes positive evaluative biases and material benefits to those who occupy high-status positions. In recent years, a handful of empirical studies have sought to investigate this causal assumption in contexts ranging from citations to sports tournaments and wine pricing (Simcoe and Waguespack 2010; Roberts et al. 2011; Azoulay, Stuart and Wang 2012; Kim and King 2014; Malter 2014). Despite substantial advances, it remains a significant empirical challenge to establish the causal effect of status in organizational and market contexts. For example, while existing research suggests that the effect of organizational status is particularly pronounced in highly uncertain environments, and in contexts where quality is difficult to verify, it is in these same environments where researchers are also most susceptible to information asymmetries and omitted variable bias (Simcoe and Waguespack 2010). Likewise, in labor market contexts, reverse causality is particularly difficult to address as status may come from affiliations with better employees (Rider and Tan 2015). Given the empirical challenges, this dissertation seeks to provide additional evidence for the causal effect of status in the contexts of labor market for large organizations and professional labor, as well as the factor market for finance with a combination of large-scale data and methods such as regression discontinuity (Chapter 1 & 2) and instrumental variable regression (Chapter 3).

Labor Market Implications of Status

Furthermore, my dissertation differs from prior work because its focus is on the factor market competition for labor and finance. Podolny (1993) conceptualized the benefits of

organizational status in market competition that arise from both increased customer acceptance in product market competition as well lowered costs in factor market competition. Considerable scholarship has examined the role of organizational status in product market competition, while the empirical evidence with respect to factor market competition remains relatively scarce (Rider and Tan 2015). Specifically, chapter 1 is among the first to establish causal evidence for the effect of status in the factor market competition for labor. In chapters 3 and 4, I focus on the labor market for professionals in science and academia. Chapter 3 examines public status and its interaction with intra-professional status as an important non-pecuniary incentive for scientists. Chapter 4 examines the efforts to dismantle inequality stemming from cumulative advantage of demographic status in academia.

Tables and Figures

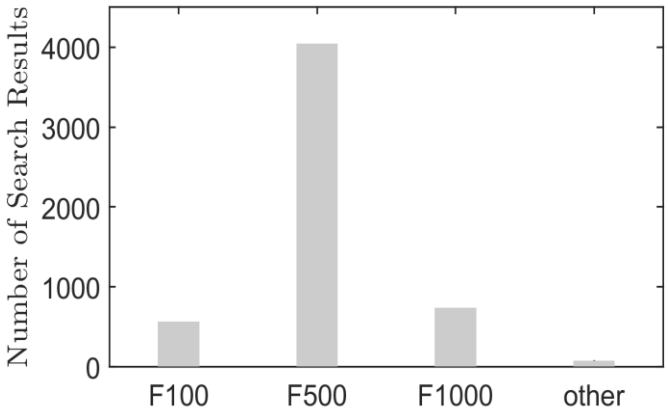


Figure 1. Number of Media References

Table 1. Parametric Estimation Results

VARIABLES	(1) Number of Strengths	(2) Number of Strengths	(3) Number of Strengths
FT500	-0.281**	-0.299**	-0.298**
	(0.139)	(0.148)	(0.149)
Lagged ROA		0.992***	1.069***
		(0.318)	(0.318)
Sales growth		0.0673	0.0735
		(0.0833)	(0.0950)
Book-leverage ratio		-0.408*	-0.426**
		(0.217)	(0.216)
Cash ratio			-0.0295
			(0.432)
EPS			-0.0102
			(0.00747)
Employment size (log)			0.0244
			(0.0621)
Constant	-0.0680	-0.128	-0.138
	(0.153)	(0.180)	(0.235)
Observations	12,225	9,451	9,451
Industry Fixed Effect	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes
Polynomials (3^{rd})	Yes	Yes	Yes
FT500*Polynomials (3^{rd})	Yes	Yes	Yes

Notes: Robust standard errors (in parentheses) clustered at the company level.
*** p<0.01, ** p<0.05, * p<0.1

Table 2. Nonparametric Local Linear Estimation Result
(Bandwidth: MSE-TWO)

Variables	(4) $resid\_emps_i$
Fortune500	-0.081**
	(0.037)
Observations	7,959
Kernel Type	Triangular
Bandwidth size	[Left 347.2 Right 297.6]

Note: *** p<0.01, ** p<0.05, * p<0.1

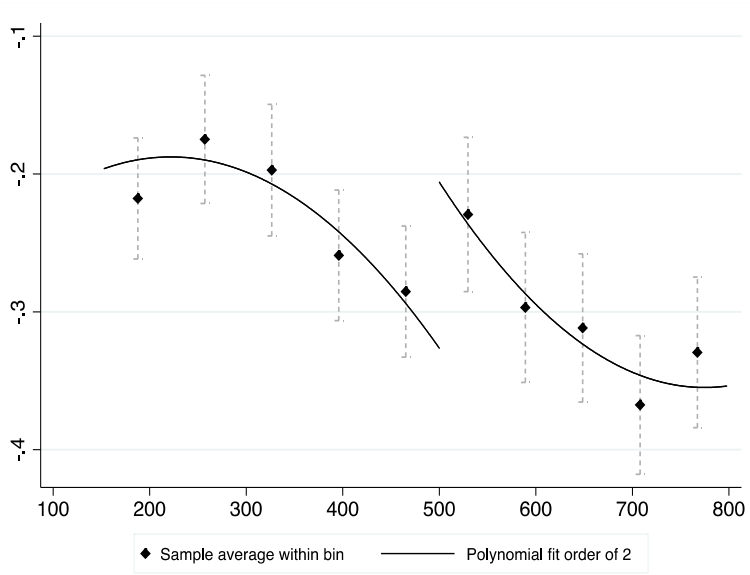


Figure 2. RD Plot in MSE-TWO Bandwidth

Notes: diamond dots are the mean $resid\_emps_i$ within bins (each diamond quantifies about 720 observations); number of bins is 5 on both sides of the 500th cut-off; dotted line bars indicate the 95% confidence intervals; bandwidth type is MSE-TWO; kernel type is triangular.

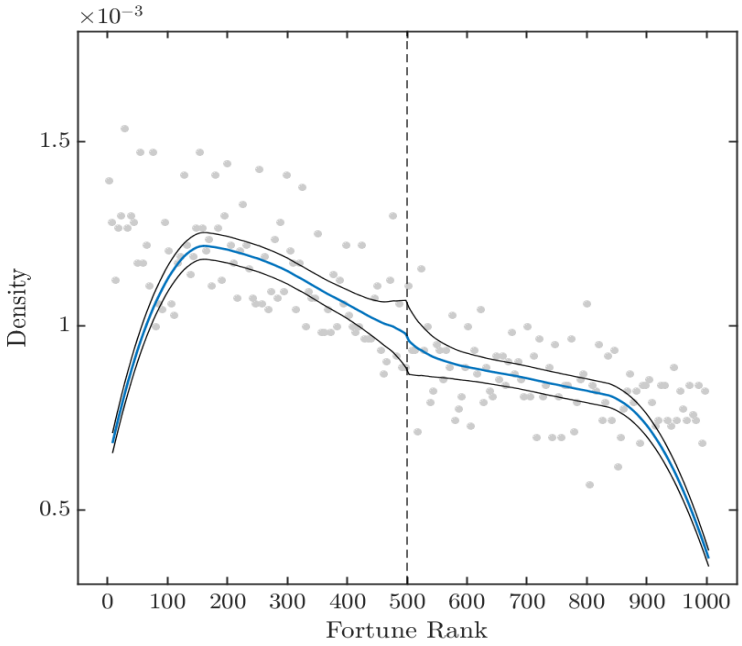


Figure 3. Visualization of the McCrary (2008) Density Test

Table 3: Covariate Comparison around Fortune500
(controlling for distance from the cut-off)

Selected Covariates	MSE-TWO Bandwidth (5) p-value	Cross Validation Bandwidth (6) p-value
Lagged ROA	0.764	0.701
Sales Growth	0.444	0.383
Earnings Per Share	0.611	0.133
Cash ratio	0.293	0.380
Employment size (log)	0.428	0.282
Book-leverage ratio	0.879	0.987
Bandwidth size	[347.2 297.6]	[199.8 199.8]

Notes: First column reports p-values calculated using bandwidth selected by MSETWO that allows for different bandwidths above/below the cut-off. Second column reports p-values calculated using bandwidth selected by cross validation. All comparisons are conducted controlling for the distance between the rank positions and the Fortune500 cut-off with standard errors clustered on the firm level.

Table 4. Different Bandwidths

VARIABLES	(7)	(8)	(9)
Fortune500	-0.101** (0.046)	-0.098** (0.048)	-0.073** (0.034)
Observations	4,766	4,307	8,874
Kernel Type	Triangular	Triangular	Triangular
Bandwidth Type	Cross-validation	CCT	IK

Note: *** p<0.01, ** p<0.05, * p<0.1

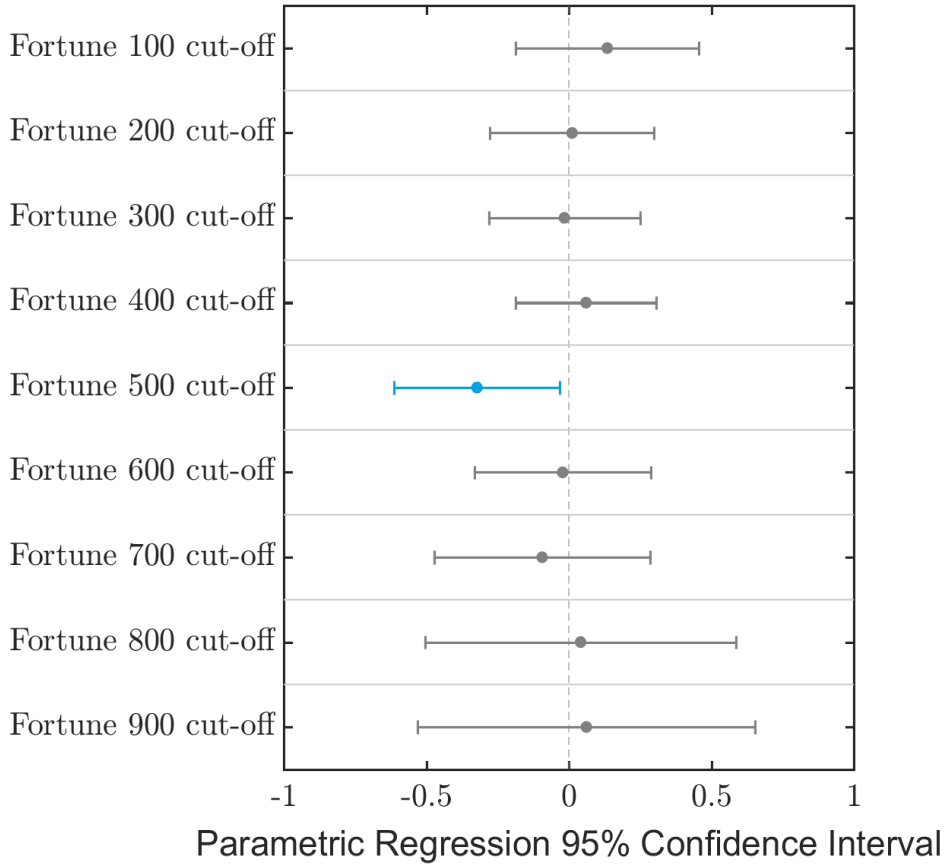


Figure 4. Other Placebo Cut-offs

Table 5. Sub-categories in Employment-related CSR

VARIABLES	(10) Pecuniary sub-categories	(11) Nonpecuniary sub-categories	(12) Nonpecuniary sub-categories (w/o supply_chain_standard)
FT500	-0.241	-0.815***	-0.753***
	(0.272)	(0.261)	(0.261)
Lagged ROA	2.309***	0.224	0.215
	(0.688)	(0.554)	(0.557)
Sales growth	0.426**	-0.0711	-0.0819
	(0.178)	(0.175)	(0.174)
Book-leverage ratio	-1.488***	-0.276	-0.233
	(0.419)	(0.366)	(0.372)
Employment size (log)	-0.0179	0.135	0.106
	(0.110)	(0.106)	(0.108)
Cash ratio	-0.243	-0.816	-0.838
	(0.746)	(0.751)	(0.760)
EPS	0.000517	0.0135	0.0130
	(0.0144)	(0.0159)	(0.0159)
Constant	0.598	-1.823***	-1.686***
	(0.436)	(0.398)	(0.407)
Observations	10,116	11,483	11,434
Industry Fixed Effect	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes
FT500*Polynomials	Yes	Yes	Yes

Notes: Robust standard errors (in parentheses) clustered at the company level.
*** p<0.01, ** p<0.05, * p<0.1

Table 6. Other Stakeholder Categories

VARIABLES	(13) Environment	(14) Consumers	(15) Community
FT500	0.228	0.0961	0.187
	(0.239)	(0.247)	(0.229)
Lagged ROA	-1.167**	-0.701	-0.300
	(0.471)	(0.488)	(0.450)
Sales growth	-0.0999	-0.0477	-0.0572
	(0.145)	(0.157)	(0.144)
Employment size (log)	0.00427	-0.116	0.0241
	(0.0898)	(0.0839)	(0.0867)
Book-leverage ratio	1.218***	1.076***	1.400***
	(0.308)	(0.315)	(0.315)
Cash ratio	0.375	0.883	-0.366
	(0.613)	(0.625)	(0.630)
EPS	-0.000725	-0.0200	0.000841
	(0.0134)	(0.0138)	(0.0129)
Constant	0.596	0.939	-2.012***
	(1.164)	(1.244)	(0.339)
Observations	10,749	10,685	10,660
Industry Fixed Effect	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes
FT500*Polynomials	Yes	Yes	Yes

Notes: Robust standard errors (in parentheses) clustered at the company level.
*** p<0.01, ** p<0.05, * p<0.1

Table 7. Parametric Estimates on Number of Estimates

	(1)	(2)	(3)	(4)
VARIABLES	Estimates	Estimates	Estimates	Estimates
Fortune500	0.139**	0.139**	0.135**	0.134**
	(0.0681)	(0.0682)	(0.0679)	(0.0680)
Company Issued Guidance		0.0794***		0.0845***
		(0.0218)		(0.0214)
Constant	0.866***	0.852***	0.939***	0.935***
	(0.100)	(0.102)	(0.186)	(0.183)
Observations	39,919	39,919	39,919	39,919
Polynomials	Yes	Yes	Yes	Yes
FT500*Polynomials	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Year_month FE	No	No	Yes	Yes

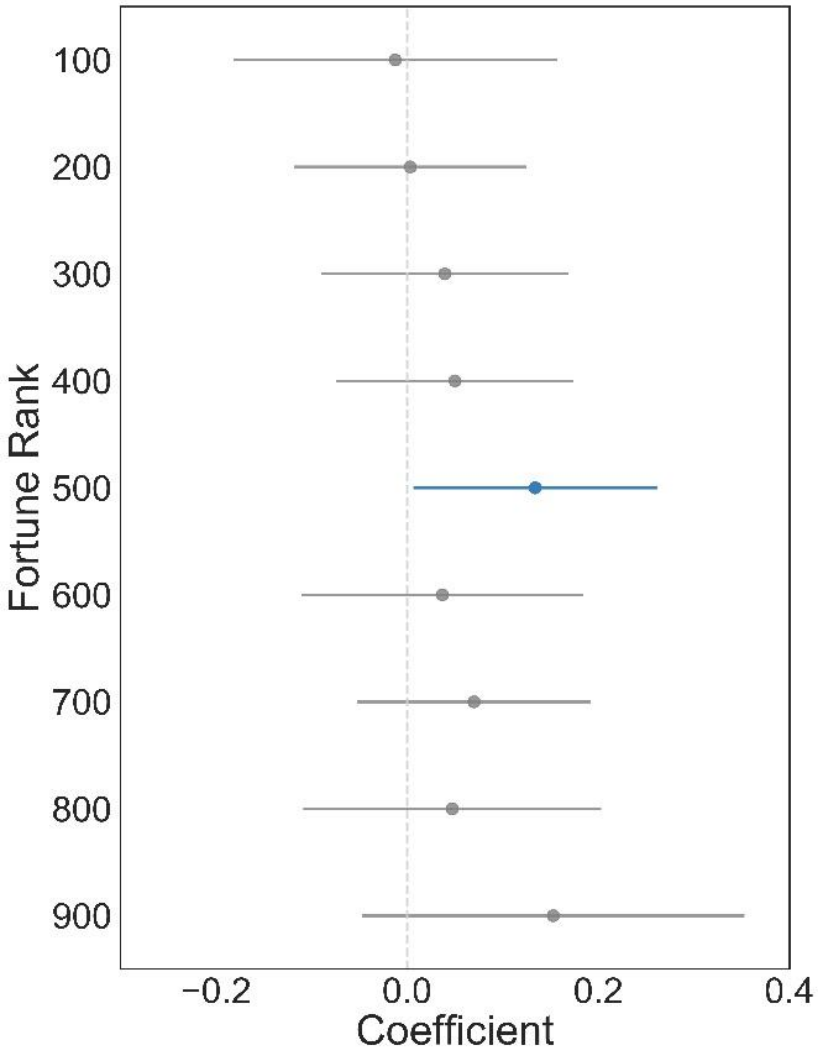
Note: Robust standard errors clustered on ncusip in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8. Nonparametric Estimates and Different Bandwidths

	(1)	(2)	(3)
VARIABLES	Estimates (Residuals)	Estimates (Residuals)	Estimates (Residuals)
Fortune500	0.3993**	0.6071**	0.3014**
	(0.1618)	(0.2186)	(0.1437)
Effective Observations	6,872	4,033	11,013
Kernel Type	Triangular	Triangular	Triangular
Bandwidth Type	MSERD	CERRD	MSETWO

Note: *** p<0.01, ** p<0.05, * p<0.1

Figure 5. Alternative Thresholds
(Number of estimates)



Notes: Parametric estimation; order of polynomials selected using BIC.

Table 9. Precision of Company Issued Guidance

VARIABLES	(1) Full_sample Precision	(2) Discontinuity_sample Precision
Fortune500	-0.0557**	-0.0771**
	(0.0266)	(0.0379)
PositiveInfo	-0.00118	-0.00285
	(0.00738)	(0.0108)
NeutralInfo	0.0991***	0.0870***
	(0.00763)	(0.0104)
FT500#PositiveInfo	0.0192*	0.0148
	(0.0108)	(0.0160)
FT500#NeutralInfo	0.0131	0.0119
	(0.0111)	(0.0152)
Constant	0.0683*	0.0544
	(0.0387)	(0.0558)
Observations	111,669	44,194
R-squared	0.273	0.275
Polynomials	Yes	Yes
FT500*Polynomials	Yes	Yes
Industry FE	Yes	Yes
Year_month FE	Yes	Yes
Measure FE	Yes	Yes
Periodicity FE	Yes	Yes

Note: Robust standard errors clustered on ncusip in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 6. Media Recognition Time from Publication Dates (Precise Date Sample)

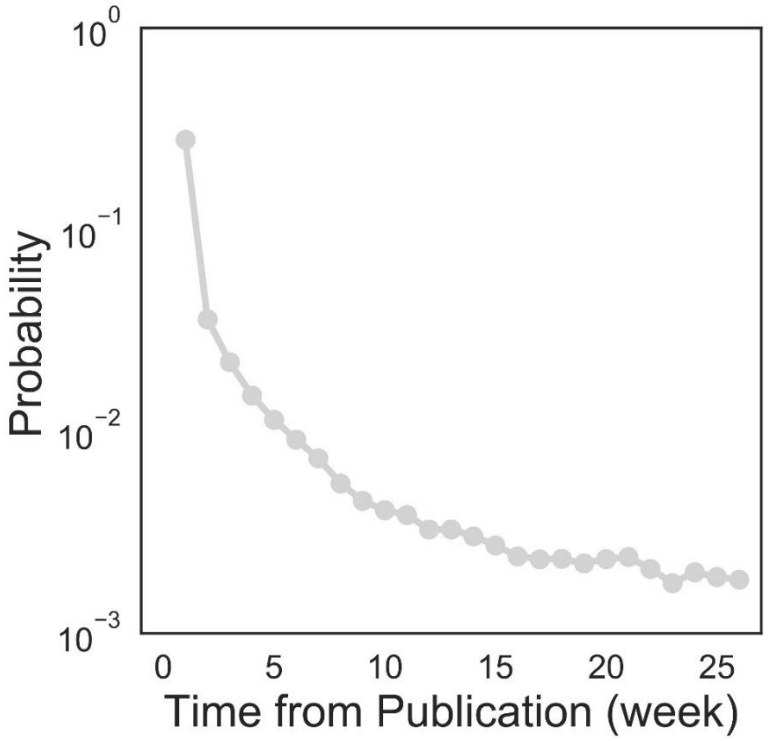


Table 10. OLS and Fixed Effects Models

VARIABLES	(1) OLS	(2) Author Fixed Effects
Media Recognition	0.00227***	0.00376***
	(0.000323)	(0.000366)
First Author Impact	-0.0111***	0.000649***
	(3.81e-05)	(0.000103)
Last Author Impact	0.00110***	
	(3.82e-05)	
Average Institutional Prestige (reverse coding)	8.48e-05***	-5.91e-05***
	(1.31e-05)	(1.73e-05)
Constant	0.0539***	0.0666***
	(0.00169)	(0.00370)
Observations	3,547,891	3,596,611
R-squared	0.055	0.850
Field Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Last Author Fixed Effects		Yes

Robust standard errors clustered on last authors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11. Instrumental Variable Estimation on Topic Commitment

Variables	First-stage (1) Media Recognition	2SLS (2) Topic Commitment
Homerun Paper Concurrence	-0.0027***	
	(0.00035)	
Media Recognition		0.3298***
		(0.0933)
First Author Impact	0.0051***	-0.0128***
	(0.0001)	(0.0005)
Last Author Impact	0.0049***	-0.00049
	(0.0001)	(0.00046)
Average Institutional Prestige (reverse coding)	-0.00049***	0.00025***
	(0.00003)	(0.00005)
Year Fixed Effect	Yes	Yes
Field Fixed Effect	Yes	Yes
Cragg-Donald Wald F Statistic	53.96	53.96
Kleibergen-Paap Wald F Statistic	60.24	60.24
Number of Observations	3,547,891	3,547,891

*Notes: ***p<0.01, **p<0.05, *p<0.1; Robust standard errors clustered on the last authors; Media recognition is measured by logarithm of the total number of media mentions; Precise date sample; Analysis focused on last authors.*

Table 12. Robustness Test

(Binary variable of media recognition)

Variables	First-stage (1) Media Recognition	2SLS (2) Topic Commitment
Homerun Paper Concurrence	-0.0024***	
	(0.00024)	
Media Recognition		0.3668***
		(0.0992)
First Author Impact	0.0036***	-0.0124***
	(0.0001)	(0.0004)
Last Author Impact	0.0035***	-0.0002
	(0.00005)	(0.0004)
Average Institutional Prestige (reverse coding)	-0.00035***	0.0002***
	(0.00002)	(0.00004)
Year Fixed Effect	Yes	Yes
Field Fixed Effect	Yes	Yes
Cragg-Donald Wald F Statistic	93.79	93.79
Kleibergen-Paap Wald F Statistic	104.39	104.39
Number of Observations	3,547,891	3,547,891

*Notes: ***p<0.01, **p<0.05, *p<0.1; Robust standard errors clustered on the last authors; Media recognition is measured by a binary variable; Precise date sample; Analysis focused on last authors.*

Table 13. Robustness Test
(Excluding media coverage before publication dates)

Variables	First-stage (1) Media Recognition	2SLS (2) Topic Commitment
Homerun Paper Concurrence	-0.0018***	
	(0.0003)	
Media Recognition		0.4843***
		(0.1488)
First Author Impact	0.0043***	-0.0132***
	(0.00009)	(0.0006)
Last Author Impact	0.0038***	-0.0007
	(0.00006)	(0.0006)
Average Institutional Prestige	-0.0004***	0.0003***
(reverse coding)	(0.00003)	(0.00007)
Year Fixed Effect	Yes	Yes
Field Fixed Effect	Yes	Yes
Cragg-Donald Wald F Statistic	29.13	29.13
Kleibergen-Paap Wald F Statistic	32.39	32.39
Number of Observations	3,547,891	3,547,891

*Notes: ***p<0.01, **p<0.05, *p<0.1; Robust standard errors clustered on the last authors; Media recognition excludes media mentions before publication dates; Precise date sample; Analysis focused on last authors.*

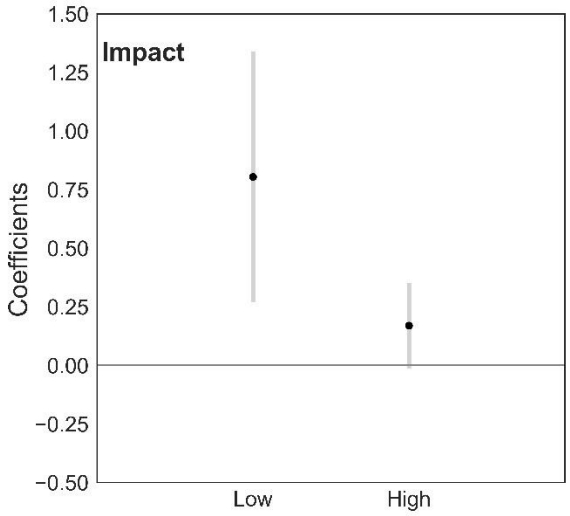
Table 14. Robustness Test
(Analysis on first authors)

Variables	First-stage (1) Media Recognition	2SLS (2) Topic Commitment
Homerun Paper Concurrence	-0.0027***	
	(0.00035)	
Media Recognition		0.3458***
		(0.0962)
First Author Impact	0.0051***	-0.0135***
	(0.0001)	(0.0005)
Last Author Impact	0.0049***	-0.0004
	(0.0001)	(0.00047)
Average Institutional Prestige	-0.00050***	0.00033***
(reverse coding)	(0.00003)	(0.00005)
Year Fixed Effect	Yes	Yes
Field Fixed Effect	Yes	Yes
Cragg-Donald Wald F Statistic	51.84	51.84
Kleibergen-Paap Wald F Statistic	57.69	57.69
Number of Observations	3,549,085	3,549,085

*Notes: ***p<0.01, **p<0.05, *p<0.1; Robust standard errors clustered on the first authors; Media recognition is measured by logarithm of the total number of media mentions; Precise date sample; Analysis focused on first authors.*

Figure 7. Heterogeneous Treatment Effect, Median Split (Precise date sample)

A. Cumulative Impact



B. Institution Prestige

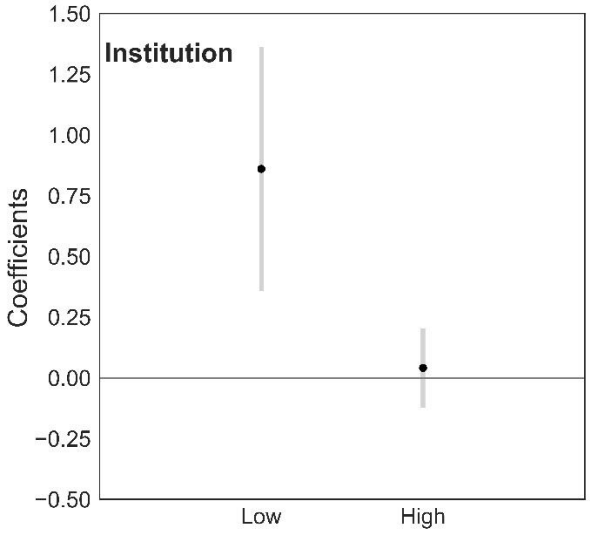


Table 15. Percentage Distribution of Instructional Faculty in Degree-granting Postsecondary Institutions[20]

	White		African American		Hispanic		Asian/Pacific Islander	
	Male	Female	Male	Female	Male	Female	Male	Female
Assistant	35%	38%	3%	4%	3%	3%	7%	6%
Associate	42%	35%	3%	3%	3%	2%	7%	5%
Full	55%	27%	2 %	2%	2%	1%	7%	3%
Total	41%	35%	3%	3%	3%	2%	6%	4%

Note: Percentage distribution of full-time, tenured or tenure track instructional faculty in degree-granting postsecondary institutions, by academic rank, selected race/ethnicity, and sex: Fall 2016

Detail may not sum to 100 percent because data on some racial/ethnic groups are not shown.

Table 16. Number and Proportion of Jointly Appointed Faculty by Race and Rank (Human observer sample)

Panel A. Number of JA/Non-JA Faculty	**Full**		**Associate**		**Assistant**		**All Ranks**	
	JA	Non JA	JA	Non JA	JA	Non JA	JA	Non JA
African American	26	86	31	86	38	117	67	191
White	209	1683	152	1197	109	1671	359	3183

Panel B. Proportion of Joint Appointment	**Full**	**Associate**	**Assistant**	**All Ranks**
African American	0.23	0.26	0.25	0.26
White	0.11	0.11	0.06	0.10

[20] SOURCE: U.S. Department of Education, National Center for Education Statistics, Integrated Postsecondary Education Data System (IPEDS), IPEDS Spring 2017, Human Resources component. See Digest of Education Statistics 2017, table 315.20.

Table 17. Likelihood of Joint Appointment for Individual Faculty

VARIABLES	(1) All ranks	(2) Assistants Only
African American	0.733***	1.248***
	(0.201)	(0.288)
Female	0.116	0.0429
	(0.141)	(0.223)
Constant	6.950***	-2.430**
	(1.030)	(0.981)
Observations	42,975	7,885
Department Fixed Effect	Yes	Yes
Year Fixed Effect	Yes	Yes

Robust standard errors clustered on individuals in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 18. Functional Needs for Diversity and Joint Appointment of African American Faculty

VARIABLES	(3) All ranks	(4) Assistants Only
Total Number of AA Last Year	-0.345***	-0.434**
	(0.121)	(0.173)
Having Zero AA Last Year	0.412	1.505**
	(0.680)	(0.760)
Faculty Size Last Year	0.0347	0.100
	(0.0409)	(0.0624)
Total Number Hired	0.352***	
	(0.0849)	
Total Number of Assistants Hired		0.573***
		(0.124)
Constant	-3.791***	-4.465***
	(0.786)	(1.116)
Observations	581	323
Department Fixed Effect	Yes	Yes
Year Fixed Effect	Yes	Yes

Robust standard errors clustered on departments in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 19. Annual Salary

VARIABLES	(5)	(6)	(7)
African American	-56.17	64.77	-51.90
	(349.7)	(353.3)	(463.7)
Joint Appointment (JA)		-920.7***	-979.2***
		(272.7)	(285.7)
African American#JA			356.8
			(709.1)
Female	-260.8	-256.2	-256.6
	(251.6)	(251.4)	(251.3)
Starting_salary	31,403***	31,383***	31,378***
	(768.3)	(768.6)	(768.8)
Constant	69,462***	69,390***	69,404***
	(945.3)	(945.9)	(947.9)
Observations	8,636	8,636	8,636
R-squared	0.952	0.953	0.953
Department Fixed Effect	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes

Robust standard errors clustered on individuals in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 20. Likelihood of Promotion

VARIABLES	(8)	(9)	(10)
African American	-0.317*	-0.233	-0.0963
	(0.172)	(0.176)	(0.203)
Joint Appointment (JA)		-0.691***	-0.629***
		(0.203)	(0.217)
African American#JA			-0.461
			(0.409)
Female	-0.207*	-0.227**	-0.224**
	(0.111)	(0.112)	(0.112)
Constant	3.643***	3.664***	3.634***
	(0.581)	(0.587)	(0.588)
Observations	14,739	14,739	14,739
Department Fixed Effect	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes

Robust standard errors clustered on individuals in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 21. Subsample Analysis of Promotion Likelihood

VARIABLES	(11) AA	(12) White
Joint Appointment	-0.894**	-0.638***
	(0.439)	(0.187)
Female	-0.330	-0.125
	(0.425)	(0.113)
Constant	3.081***	2.976***
	(0.997)	(0.203)
Observations	1,402	13,755
School Fixed Effect	Yes	Yes
Year Fixed Effect	Yes	Yes

Robust standard errors clustered on individuals in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 22. Subsample Analysis of Annual Salary

VARIABLES	(13) AA	(14) White
Joint Appointment	-2,618***	-2,061***
	(852.2)	(345.1)
Female	-525.9	-524.5
	(852.7)	(344.3)
Starting_Salary	40,599***	38,697***
	(1,836)	(490.5)
Constant	78,276***	75,507***
	(1,341)	(440.2)
Observations	952	7,684
R-squared	0.912	0.942
School Fixed Effect	Yes	Yes
Year Fixed Effect	Yes	Yes

Robust standard errors clustered on individuals in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 23. Productivity Mechanism

VARIABLES	(15) Productivity_all	(16) Productivity_journal_book
Post_appointment	-0.133**	-0.130***
	(0.0547)	(0.0460)
Interdisciplinarity	-0.258*	0.199**
	(0.137)	(0.0901)
Log (citation)	0.314***	0.257***
	(0.0231)	(0.0208)
Author Fixed Effect	Yes	Yes
Year Fixed Effect	Yes	Yes
Constant	-0.0739	-0.437***
	(0.152)	(0.112)
Observations	4,429	4,427
R-squared	0.962	0.961

Robust standard errors clustered on individual faculty in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 24. Evaluation Mechanism (Salary and Promotion Outcomes)

VARIABLES	(17) Annual salary	(18) Promotion
African American	1,160*	0.479*
	(635.1)	(0.255)
Joint Appointment (JA)	-567.0	-0.681**
	(441.5)	(0.275)
Female	514.6	0.166
	(390.8)	(0.161)
Starting_salary	31,708***	
	(887.5)	
Productivity (log)	1,113***	0.708***
	(273.3)	(0.141)
Citation (log)	383.3***	0.457***
	(147.0)	(0.0731)
Work Interdisciplinarity	387.3	-0.756**
	(559.9)	(0.357)
Constant	64,193***	-1.998**
	(1,312)	(0.904)
Observations	4,962	9,968
R-squared	0.957	
Department Fixed Effect	Yes	Yes
Year Fixed Effect	Yes	Yes

Robust standard errors clustered on individuals in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 25. Promotion Likelihood by Department Interdisciplinarity, median split

VARIABLES	(19) Div_low	(20) Div_high
African American	0.554*	0.414
	(0.314)	(0.368)
Joint Appointment	-0.635**	-0.694
	(0.292)	(0.429)
Female	0.297	-0.0858
	(0.201)	(0.256)
Productivity (log)	0.901***	0.425**
	(0.193)	(0.210)
Citation (log)	0.411***	0.519***
	(0.0957)	(0.117)
Work Interdisciplinarity	-0.904*	-0.505
	(0.472)	(0.514)
Constant	-2.345**	-3.470***
	(1.086)	(1.159)
Observations	6,520	3,448
Department Fixed Effect	Yes	Yes
Year Fixed Effect	Yes	Yes

Robust standard errors clustered on individuals in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 26. Annual Salary by Department Interdisciplinarity, median split

VARIABLES	(21) Div_low	(22) Div_high
African American	1,398	423.6
	(1,003)	(635.4)
Joint Appointment	-1,132*	102.0
	(631.5)	(530.8)
Female	1,184**	-487.3
	(578.2)	(326.0)
Starting Salary	31,446***	30,962***
	(1,007)	(2,099)
Productivity (log)	1,280***	805.1***
	(372.9)	(267.8)
Citation (log)	532.6***	97.73
	(198.4)	(174.6)
Work Interdisciplinarity	201.4	1,364**
	(751.9)	(652.6)
Constant	61,937***	67,904***
	(1,657)	(1,605)
Observations	3,236	1,726
R-squared	0.953	0.966
Department Fixed Effect	Yes	Yes
Year Fixed Effect	Yes	Yes

Robust standard errors clustered on individuals in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 27. Starting Productivity

VARIABLES	(26) Starting Productivity	(27) Starting Productivity
African American (AA)	-0.427	-0.00589
	(0.407)	(0.440)
Joint Appointment (JA)	-0.264	-0.185
	(0.287)	(0.319)
AA # JA		-0.724
		(0.728)
Female	-0.403**	-0.386**
	(0.165)	(0.169)
Constant	0.627	0.705
	(0.476)	(0.510)
Observations	185	185
Department Fixed Effect	Yes	Yes
Year Fixed Effect	Yes	Yes

Robust standard errors clustered on ID in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 28. Promotion Likelihood, excluding joint appointments at hiring

VARIABLES	(28)	(29)
JA later	-1.074**	-0.818
	(0.474)	(0.549)
African American (AA)	-0.326	-0.252
	(0.279)	(0.295)
JA later#AA		-1.176
		(1.046)
Female	-0.111	-0.109
	(0.177)	(0.177)
Constant	-1.154	-1.143
	(0.969)	(0.968)
Observations	13,307	13,307
Dept Fixed Effect	Yes	Yes
Year Fixed Effect	Yes	Yes

Robust standard errors clustered on ID in parentheses
*** p<0.01, ** p<0.05, * p<0.1

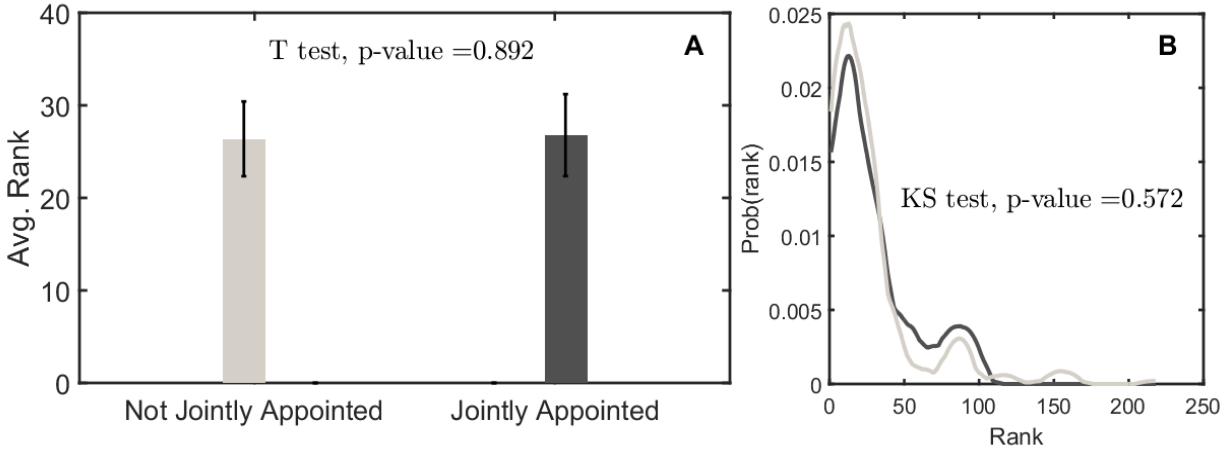
Table 29. African American Studies

VARIABLES	(32) Salary Raises	(33) Promotion Likelihood
Joint Appointment	-790.3***	-0.603***
	(101.1)	(0.230)
African American Studies	-264.6*	-0.166
	(153.2)	(0.305)
African American	-109.3	-0.482**
	(174.7)	(0.235)
Female	-99.96	-0.211
	(87.17)	(0.150)
Constant	2,441***	0.562***
	(109.5)	(0.134)
Observations	6,699	15,737
R-squared	0.103	
Year Fixed Effect	Yes	Yes

Robust standard errors clustered on ID in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 8. PhD Institution Rankings

(A) Average Rankings, t Test Result; (B) Kernel Density Distribution, KS Test Result



Appendices

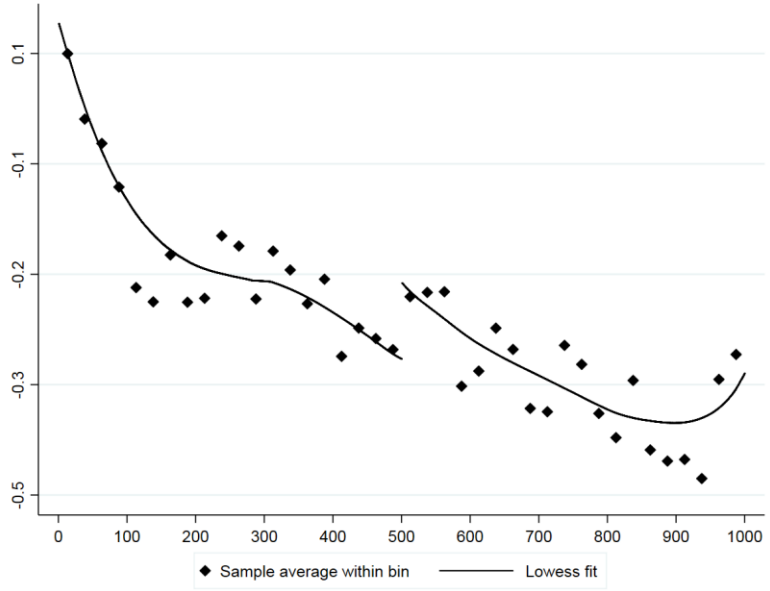


Figure A1. LOWESS Plot

Notes: diamond dots are the mean $resid\_emps_i$ within bins (each diamond quantifies about 235 observations); number of bins is 20 on both sides of the 500th cut-off; bandwidth is full sample.

Table A1. Alternative Measures of Employment-related CSR

VARIABLES	(16) Employment Net scores (OLS)	(17) Employment and Diversity Strengths (Negative binomial regression)
FT500	-0.252	-0.278
	(0.115)	(0.112)
Lagged ROA	1.650	0.226
	(0.253)	(0.196)
Sales Growth	0.0117	-0.00195
	(0.0706)	(0.0695)
Book-leverage ratio	-0.378	-0.0239
	(0.162)	(0.155)
Cash ratio	-0.707	0.103
	(0.306)	(0.303)
EPS	0.00667	0.00112
	(0.00611)	(0.00513)
Employment size (log)	-0.0826	0.0206
	(0.0421)	(0.0440)
Constant	1.593	-0.0365
	(0.163)	(0.165)
Observations	9,451	9,451
R-squared	0.396	
Industry Fixed Effect	Yes	Yes
Year Fixed Effect	Yes	Yes
Polynomials	Yes	Yes
FT500*Polynomials	Yes	Yes

Notes: Robust standard errors (in parentheses) clustered at the company level.

Figure A2. Histogram of Publication Day in Full Sample

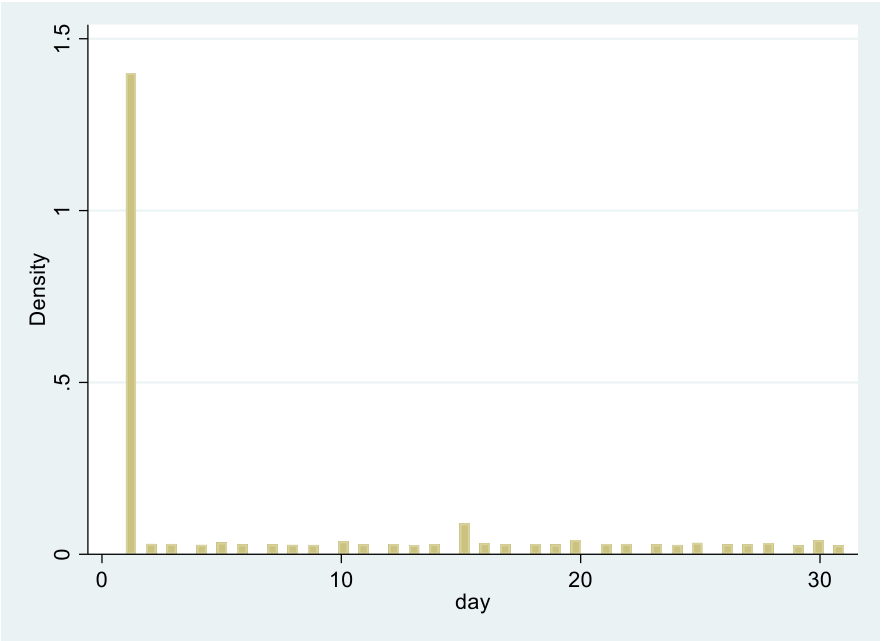


Figure A3. Histogram of Publication Month in Full Sample

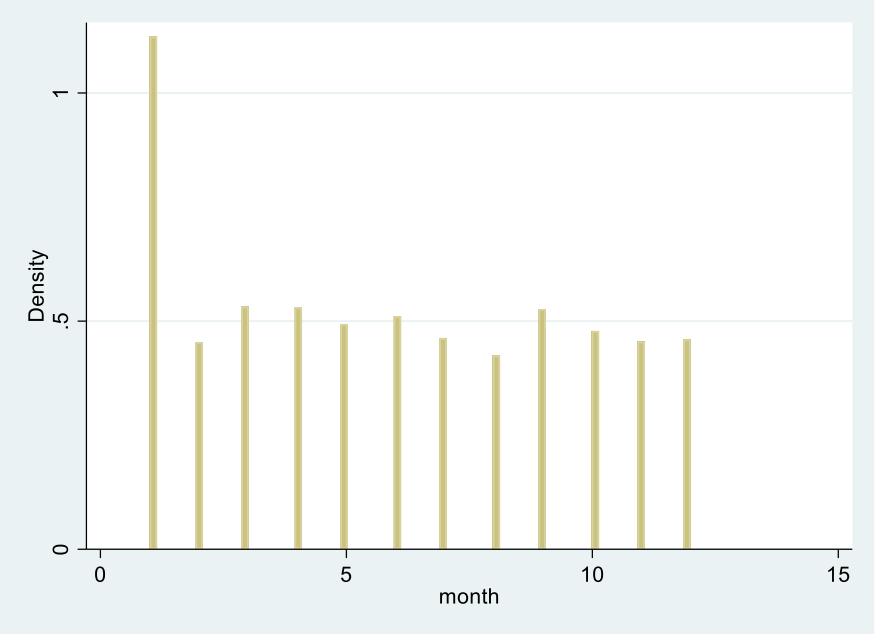


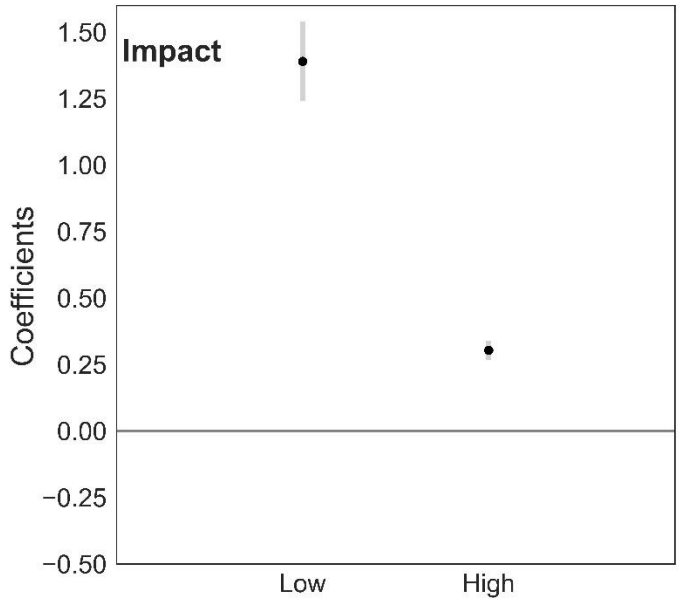
Table A2. Robustness Test
(Full sample)

Variables	First-stage (1) Media Recognition	2SLS (2) Topic Commitment
Homerun Paper Concurrence	-0.0062***	
	(0.00014)	
Media Recognition		0.5735***
		(0.0213)
First Author Impact	0.0039***	-0.0122***
	(0.00005)	(0.00009)
Last Author Impact	0.0036***	0.0004***
	(0.00003)	(0.00008)
Average Institutional Prestige (reverse coding)	-0.00039***	0.00028***
	(0.000015)	(0.000015)
Year Fixed Effect	Yes	Yes
Field Fixed Effect	Yes	Yes
Cragg-Donald Wald F Statistic	1802.67	1884.47
Kleibergen-Paap Wald F Statistic	1874.16	1587.57
Number of Observations	9,119,480	9,119,480

*Notes: ***p<0.01, **p<0.05, *p<0.1; Robust standard errors clustered on the last authors; Media recognition is measured by logarithm of the total number of media mentions; Full sample; Analysis focused on last authors.*

Figure A4. Heterogeneous Treatment Effect, Median Split (Full sample)

A. Cumulative Impact



B. Institution Prestige

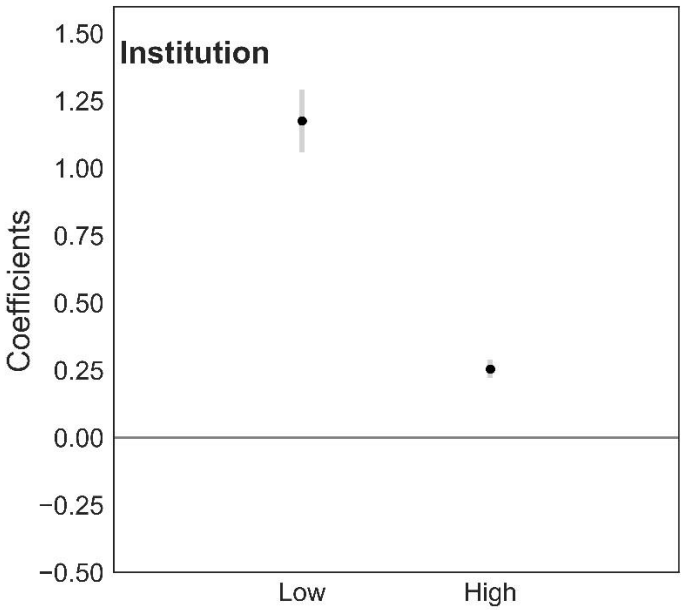


Table A3. Promotion Likelihood

	All Pre-tenure Exits		
VARIABLES	(1)	(2)	(3)
African American	-0.324*	-0.246	0.589**
	(0.171)	(0.175)	(0.266)
Joint Appointment (JA)		-0.684***	-0.721***
		(0.202)	(0.280)
African American#JA			-0.376
			(0.515)
Female	-0.202*	-0.218*	0.116
	(0.111)	(0.111)	(0.151)
Productivity (log)			1.287***
			(0.137)
Citation (log)			-0.0169
			(0.0727)
Work Interdisciplinary			-0.264
			(0.334)
Constant	3.536***	3.555***	-1.623*
	(0.589)	(0.594)	(0.887)
Observations	15,074	15,074	10,461
Department Fixed Effect	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes

Robust standard errors clustered on individuals in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A4. Salary Model

	Log (Salary)		
VARIABLES	(1)	(2)	(3)
African American	0.00127	0.00253	0.0128
	(0.00537)	(0.00543)	(0.00901)
Joint Appointment (JA)		-0.00964**	-0.00368
		(0.00476)	(0.00601)
African American#JA			0.0105
			(0.0150)
Female	-0.00724**	-0.00718**	0.000624
	(0.00350)	(0.00350)	(0.00369)
Starting_salary	0.201***	0.201***	0.225***
	(0.0124)	(0.0124)	(0.00821)
Productivity (log)			0.0195***
			(0.00266)
Citation (log)			0.000491
			(0.00156)
Work Interdisciplinary			0.0129**
			(0.00619)
Constant	10.92***	10.92***	10.88***
	(0.0156)	(0.0156)	(0.0148)
Observations	8,636	8,636	5,614
R-squared	0.952	0.952	0.962
Department Fixed Effect	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes

Robust standard errors clustered on individuals in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A5. African American Assistant Professor Joint Appointment Departments

SCHOOL OF PUBLIC POLICY
AMERICAN CULTURE
ANTHROPOLOGY
AFRICAN AMERICAN STUDIES
COMMUNICATION STUDIES
ECONOMICS
ENGLISH LANGUAGE & LIT
GERMANIC LANGUAGE & LIT
HISTORY
HISTORY OF ART
POLITICAL SCIENCE
PSYCHOLOGY
ROMANCE LANGUAGES & LIT
SCREEN ARTS & CULTURES
SOCIOLOGY
UG: ENVIRONMENT
UG: RESIDENTIAL COLLEGE
WOMEN'S STUDIES
SCH OF NAT RESOURCES & ENVIRON
SCHOOL OF EDUCATION
SCHOOL OF INFORMATION
HIGHER AND POSTSECONDARY EDUCATION
THEATRE AND DRAMA
URBAN PLANNING

Table A6. Departmental Interdisciplinarity

Department Name	Normalized Shannon Entropy of Faculty PhD Majors
ECONOMICS	0
PHILOSOPHY	0
SOCIOLOGY	0.2974722
POLITICAL SCIENCE	0.4397532
MATHEMATICS	0.4831882
LAW SCHOOL	0.4966964
ANTHROPOLOGY	0.5050337
HISTORY	0.5109641
PHYSICS	0.5109641
CLASSICAL STUDIES	0.5563309
MECHANICAL ENGINEERING	0.5563309
URBAN PLANNING	0.5931391
LINGUISTICS	0.6022446
ENGLISH LANGUAGE & LIT.	0.6273363
ELECTRICAL ENGR & COMPUTER SCI	0.6485456
BIOSTATISTICS DEPARTMENT	0.652815
CIVIL & ENVIRONMENTAL ENGR	0.664088
NAVAL ARCH & MARINE DEPT	0.6650099
STATISTICS	0.6706029
CENTER FOR POPULATION PLANNING	0.6903169
SCHOOL OF BUSINESS	0.6927221
GEOLOGICAL SCIENCES	0.7232908
CHEMISTRY	0.7269576
CHEMICAL ENGINEERING DEPARTMENT	0.7402395
COMMUNICATION STUDIES	0.754549
MATERIALS SCIENCE & ENGIN.	0.7609075
NUCLEAR ENGR & RADIOLOGICAL SCI	0.7637168
COMPARATIVE LITERATURE	0.786988
SCH OF PUBLIC POLICY	0.7889917
ASTRONOMY	0.7907428
INDUSTRIAL OPERATIONS	0.7974112
HEALTH BEHAVIOR & HEALTH ED DEPT	0.80687
AEROSPACE ENGINEERING	0.8181183
SCHOOL OF EDUCATION	0.8212884

PUBLIC HEALTH POLICY AND ADMIN	0.8382235
SCHOOL OF INFORMATION	0.8410777
SCHOOL OF SOCIAL WORK	0.8505561
AMERICAN CULTURE	0.8554676
GERMANIC LANGUAGE & LIT.	0.8592975
ROMANCE LANGUAGES & LIT	0.8694862
SLAVIC LANGUAGES & LIT.	0.8710777
PALEONTOLOGY MUSEUM	0.8856331
BIOPHYSICS	0.8906267
APPLIED PHYSICS	0.8931914
AFRICAN AMERICAN STUDIES	0.894922
HIGHER AND POSTSECONDARY EDUCATION	0.8989096
SCH OF NAT RESOURCES & ENVIRON	0.8995535
SCREEN ARTS & CULTURES	0.9005677
DEPARTMENT OF DANCE	0.9020738
BIOLOGY	0.9119554
ENVIRONMENTAL HEALTH SCIENCES	0.9211855
SOE-EDUCATIONAL STUDIES	0.9284318
JUDAIC STUDIES	0.9306282
THEATRE AND DRAMA	0.9306282
PSYCHOLOGY	0.9313461
BIOLOGIC AND MATERIALS SCIENCES	0.946412
HEALTH MANAGEMENT AND POLICY	0.9517962
SCHOOL OF MUSIC	0.9551923
NEAR EASTERN STUDIES	0.9578793
ORGANIZATIONAL STUDIES	0.9665385
KOREAN STUDIES PROGRAM	0.9684275
ASIAN LANGUAGES & CULTURES	0.9766147
WOMEN'S STUDIES	0.9766876
UG: ENVIRONMENT	0.9839669
UG: RESIDENTIAL COLLEGE	0.9849357
SCHOOL OF ART AND DESIGN	0.9934625
STUDY OF COMPLEX SYSTEMS	1

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