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Occupational Licensing and Accountant Quality: Evidence from the 150-Hour Rule

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Abstract

I examine the effects of mandatory occupational licensure on the quality of Certified Public Accountants (CPAs) using the staggered state-level adoption of the 150-hour Rule (the Rule). Although the Rule reduces the number of entrants into the profession, an analysis of labor market outcomes shows that accountants subject to the Rule are more likely to be employed at a Big 4 public accounting firm and specialize in taxation. However, accountants subject to the Rule have the same likelihood of promotion, the same duration until promotion, and exit public accounting at faster rates than their non-Rule counterparts. Moreover, Rule accountants earn a wage premium relative to non-Rule accountants. These findings suggest that restrictive licensing laws reduced the supply of new CPAs and increased rents to the profession without drastically improving quality in the labor market.

Keywords: The 150-Hour Rule, Occupational Licensure, CPA Licensure, Screening, Human Capital, Labor Market Outcomes, Hazard Rate Model.
JEL Classification Numbers: D45, I21, J2, K2, L51, M4.

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1 Introduction

The quality of corporate financial reporting is of central importance to capital market participants, regulators, and scholars. The demand for audits (as well as regulation of accounting and auditing standards) arises from information asymmetries that exist between management and financial statement users. Without independent audits, management could exploit its information advantage and misrepresent the firm's underlying economic performance. As a result, the regulation of financial reporting and auditing services has been one of the most widely examined topics in the accounting literature.[1] However, the quality of independent audits is a function of not only the quality of auditing standards established by regulators and the quality of procedures established by accounting firms based on these standards, but also the quality and expertise of the accountants responsible for the audits.

Given the vital role that auditors play in assuring financial reporting quality, the emergence of occupational licensing requirements (e.g., the CPA exam, educational requirements, and experience requirements) for those conducting audits (i.e., Certified Public Accountants or CPAs) is not surprising.[2] It is somewhat surprising, however, that there is a paucity of evidence on the effects of licensing requirements on the quality and preparation of those who select into the accounting profession. In this paper, I take a first step to fill this void by examining the effect changes in the restrictiveness of licensing requirements of CPAs have on the supply and quality of those who enter the accounting profession.

The minimum educational requirement for CPA licensure has historically been 120 semester hours of college course work, typically completed in four years. Approximately four decades ago, the accounting profession began debating the advantages of implementing a 150-semester-hour requirement for licensure. The stated objective of this requirement was to enhance the quality of work performed by CPAs by bringing better CPAs into the profes-

[1] For reviews, see DeFond and Zhang (2014) and Francis (2011).

[2] The rationale for such regulation is that it helps avoid negative third-party effects that may result from incompetent practitioners. For example, the licensing of CPAs is rationalized, in part, as protecting investors who must rely on the accuracy of financial information produced and verified by accountants who are neither selected by, nor accountable to investors.

sion (Elam (1996)). The 150-hour requirement (hereafter, the Rule) has now been adopted and is in force in all 54 U.S. jurisdictions. My analysis relies on the Rule's staggered adoption to provide insight into the effects of increases in licensing restrictiveness on the individuals who prepare and audit financial statements.

The relation between licensing rules and outcomes is not without significant tension. Proponents of professional licensure justify these laws as a means to protect the public against incompetent, unprepared, or irresponsible practitioners (Kleiner (2000); Leland (1979)). From this public-interest view of licensing asserts that administrative procedures regulate the quality of labor in the market. The regulator screens entrants to the profession and bars those whose skills or character traits suggest a tendency toward low-quality output, thus raising the lower tail of the quality distribution and providing a minimum guaranteed level of quality and safety to consumers. This public-interest perspective, the Rule, with its additional 30 credit hours of education, arguably serves as a screening mechanism to separate high-ability CPAs from low-ability CPAs through their willingness to acquire the additional education. Thus, if the public-interest view motivates the Rule's implementation, the quality of individuals in the profession should be raised via a reduction in the supply of candidates.[3]

On the other hand, critics of professional licensure argue that licensing merely secures higher rents for those in the occupation, by raising prices and, in particular, harming consumers who may not be able to afford their preferred level of service. This capture/private-interest perspective views licensing as mainly a rent-seeking barrier to entry introduced by current members of the profession to limit the supply of new entrants and extract monopoly rents (Friedman (1962); Stigler (1971); Maurizi (1974)). This view of licensing predicts that the Rule reduces the number of entrants while maintaining the average candidate quality or even deteriorating the quality if the incremental time cost of the Rule leads to high-ability students switching to other disciplines because of their higher opportunity cost of time. Moreover, in addition to the null quality effects, this hypothesis predicts an increase in rents

[3]The Rule's required additional education could have a human capital effect with individuals investing in more schooling irrespective of its supply effects.

for grandfathered CPAs after the Rule's adoption.

The main obstacle in studying CPAs' labor market outcomes is a lack of available data on individuals' employment and education histories. Available empirical measures of aggregate job-to-job and cross-industry transitions in the labor literature are typically constructed using census data.[4] Yet, using census data for accounting professionals stretches the limits of the surveys given their low coverage of accounting professionals and their inability to disentangle CPAs from bookkeepers. Moreover, previous studies in accounting rely on National Association of State Boards of Accountancy (NASBA) data to examine the supply effects of the Rule (Jacob and Murray (2006)). While these studies find reductions in the number of candidates sitting for the exam, they do not provide direct evidence of the Rule's quality effects given the limitations of the data. I circumvent this challenge by constructing a new, comprehensive panel dataset of career paths for more than 10,000 CPAs from 11 states who post their resumes on a major professional networking website. My sample spans the past four decades and provides a unique overview of the individual CPAs' employment and educational histories that allows me to examine differences in the labor market outcomes of CPAs who are subject to the Rule against those who are not. Specifically, I measure average tenure, number of positions, time until promotion, and time in public accounting for each individual. These measures allow for explicit tests of the quality of CPAs on the full sample of CPAs as well as on a matched sample where CPAs subject to the Rule are matched based on graduation year and gender to non-Rule CPAs.

I begin my analysis by using an extensive panel dataset of first-time CPA test takers from NASBA for the years 1984–2004 to reexamine changes in the supply of CPA candidates. Using a difference-in-differences specification, I find a 15% reduction in the number of candidates taking the exam for the first time following the Rule's enactment. Yet, the decrease does not come solely from the low end of the distribution but also from the upper tail. High type candidates' higher opportunity cost of time leads to fewer taking the exam

[4]Labor studies typically use the Current Population Survey (CPS), the Survey of Income and Program Participation (SIPP), or the Panel Study of Income Dynamics (PSID).

after the Rule. Thus, while the Rule increases the marginal cost of becoming a CPA, it is unclear to what extent the quality of the labor market changed given the reduction in both high and low types. As a result of this ambiguity, I use individuals' labor market outcomes to examine the Rule's long-term quality effects.

Descriptively I find that the Rule had an effect on the employment focus of CPAs, such that Rule CPAs are comparatively more likely to be employed at a Big 4 public accounting firm, are more likely to specialize, and are more likely to have more graduate degrees. With respect to quality effects, I find no significant difference in the time until promotion between the two groups. Furthermore, I find that CPAs subject to the Rule spend a larger percentage of their careers in public accounting although they tend to exit public accounting at higher rates than their non-Rule counterparts. Five years into their careers 62% of the Rule cohort have left public accounting as compared to only 59% of the non-Rule cohort. Assuming that promotions and tenure length are measures of CPA quality, the lack of a significant difference between Rule and non-Rule individuals in promotions and tenures casts doubt on the Rule's screening role and the public-interest motivation for the Rule. The breadth of options with which the requirement can be fulfilled (e.g., a master's degree or separate courses) likely allow some low-ability CPAs to pass the screening via non-degree programs and thereby diminishes the screening value of the Rule.

The null effect on quality does not necessarily support the capture/private-interest view of licensing. To provide evidence of rent extraction, I use data from the current population survey to examine the Rule's effect on wages. I examine whether accounting professionals in states that adopted the Rule collect rents above what can be explain by their education. Using a difference-in-differences specification, I document a 9% earnings premium for CPAs in Rule states relative to equally educated CPAs in non-Rule states. Collectively, I document that while the Rule decreased supply, it did not lead to an increase in average quality while still increasing rents to the profession. This is consistent with my inferences of the dominant role that the capture/private-interest view played in the adoption and implementation of the

Rule.

I conduct an array of robustness and sensitivity tests to validate my inferences. In particular, I address whether noise in the classification of job titles drives the promotion results, by focusing only on those individuals who become partners and testing for differences in their time until promotion to partnership. Using the sample of CPA partners, I also find a insignificant difference in the time until promotion between the Rule and non-Rule cohorts. This reinforces earlier findings of insignificant differences in long-term performance.

I also evaluate whether noise in the overall resume data can explain my quality findings that would make a null result more likely. To test the impact of data quality, I examine differences between CPAs with and without a master's degree. Prior literature has documented a significant difference in outcomes for master's degree holders (Arrow (1973); Spence (1973); Card (1999); Dupray (2001)). When I rerun my tests on master's versus non-master's degree holders, I find that master's degree holders spend less time in each position, have more jobs, and are promoted at faster rates. This finding alleviates concerns about power in the tests and further confirms the benefits of a graduate degree.

My study is subject to several caveats. The long-term labor market outcome measures in this study rely on individuals being promoted based on some quality differential. Thus, my tests are limited to the extent that promotions in firms do not incorporate quality. Additionally, inferences based on resume data are subject to concerns about selection and accuracy given the voluntary nature of the profiles and the reporting errors or biases that may be present in the resumes. The pervasive use of the website by individuals for credible networking, as well as job-search purposes, however, provides some assurance as to the integrity of the data posted. Moreover, unlike lying on a resume, which only a prospective employer will see and cannot easily verify, lying on one's profile is publicly visible. This public accountability makes it more difficult for individuals to make false claims about their employment and is thus distinct from traditional resume data. Yet, despite such potential limitations, the data allow me to document some of the first large-scale evidence on the

long-term labor market outcomes of CPAs.

This paper makes several contributions to the literature. First, it provides a comprehensive examination of the Rule's effect on the accounting labor market as well as an evaluation of licensing's effect on CPAs' quality. Despite the Rule being in effect in all 54 U.S. jurisdictions, to date there has been no long-run analysis of the Rule's effect on the individuals entering the accounting profession. While previous studies have mostly been normative in their assessments of the benefits of the Rule, I provide a positive cost-benefit analysis of the Rule's impact. Second, this paper provides a foundation for more research that connects labor economics to the accounting literature, by utilizing insights from personnel economics and human capital to improve our understanding of disclosure production and audit quality. For example, Francis (2011) suggests that "audits are of higher quality when undertaken by competent people." Yet, most extant research examines audit-firm or office-level characteristics and, due to a lack of data, implicitly assumes that there is little within-firm heterogeneity . Moreover, while the debate about the regulation of disclosure and auditing continues to be contentious, it is critical that we consider the individuals who implement these standards in order to fully understand the full effects of the regulated standards.

Finally, this paper contributes to the broader literature on occupational licensing and education. Occupational licensing regulation affects nearly 30% of the U.S. labor force, a larger proportion of workers than are in unions or covered by minimum wage laws (Kleiner and Krueger (2010)). Given its breadth, occupational licensing has begun to be scrutinized. A recent Wall Street Journal article highlighted the fact that the Obama administration budgeted $15 million to study the costs and benefits of occupational licensing on the U.S. labor market(Litan (2015)). The Rule's supply restriction, its increase in rents, and the lack of a quality effect further cast doubt on the effectiveness of increases in licensing restrictions in the labor market. Additionally, the stringency of mandatory educational requirements for licensing has also begun to be called into question in professions such as medicine and law. For example, the cost of law school has been framed as a major impediment to getting

underrepresented minorities into law (Rodriguez and Estreicher (2013)). Moreover, there have been recent calls to reduce the required years of law school from three to two years (Estreicher (2012)). In medicine, the relative costs and benefits of a fourth year of medical school have also been called into question given the scarcity of new medical students and the increasing demand for medical services in the near future (Emanuel and Fuchs (2012)). Using the Rule, this paper sheds light on the reasonableness of reducing the number of mandated school years for other professions to increase access to the profession while still maintaining quality.

The remainder of the paper is structured as follows. Section 2 provides the Rule's institutional background and the economic framework used to analyze individuals' labor market outcomes. Section 3 presents my data sources and sample selection procedure, and describes the data. Section 4 presents the empirical analysis and provides a series of sensitivity tests and robustness checks. Section 5 concludes.

2 Institutional Background and Economic Framework

2.1 Licensing of Accountants and the 150-Hour Rule

Occupational licensing regulation specifies the requirements a practitioner must fulfill in order to be permitted to perform certain services. Such regulation currently governs more than one thousand occupations (Brinegar (2006)) or nearly thirty percent of the U.S. workforce. Additionally, over the past several decades, both the number of occupations and the percentage of the workforce covered by such regulations have increased dramatically (Kleiner and Krueger (2013)). These, mostly state-level, regulations directly affect both blue- and white-collar workers.

In the accounting profession, the CPA license entitles an individual to audit firm financial statements and attest to their compliance with generally accepted accounting principles

(Jacob and Murray (2006)).[5] Non-holders are not legally permitted to undertake this activity. Currently, state boards of accountancy have put in place educational, experience, ethics, and national examination requirements that must be satisfied in order for accountants to practice legally in state and local jurisdictions as CPAs. While all CPA applicants are required to pass the national CPA examination set by the American Institute of Certified Public Accountants (AICPA), the Rule required that these applicants to complete 150 semester hours of additional education prior to obtaining their license.

The AICPA spent the past century wrestling with ways in which to elevate the prestige of the profession through higher education (Van Wyhe (1994)).[6] Yet, it was not until the 1980s that the AICPA finally institutionalized an extended educational curriculum for licensure, driven by an increase in congressional scrutiny over several prominent audit failures.

The savings and loan crisis of the 1980s gave way to a series of congressional hearings regarding the role of auditors in the crisis. The hearings examined how several prominent public companies, ranging from the Penn Square Bank in Oklahoma to E.S.M. Securities in Florida, failed so soon after receiving clean audit opinions (Berg (1988)). The threat of congressional scrutiny, with regard to new federal regulation on the accounting profession, led the AICPA in the mid-1980s to implement a set of reforms in the name of "self-regulation" (Madison and Meonske (1991)). One of the main tenets of these reforms was to require CPA candidates and AICPA members to have 150-semester hours of college education prior to receiving membership (Committee (1986)). In 1988, at its annual meeting in New York City, 84% of the AICPA's voting members backed the proposal, effective for the year 2000. The AICPA asserted that the requirement was meant to "improve the overall quality of work performed by CPAs" and "ensure the quality of future audits" by improving the quality of audit staff and those entering the profession (AICPA (2003)).

[5]These individuals also enjoy various privileges before the Internal Revenue Service.

[6]As early as 1937, the governing council of the American Institute of Accountants (AIA), superseded by the AICPA, publicly stated that the "highest practicable standards of preliminary education similar to those effective in other professions, such as law or medicine" should be required for the accounting profession (Allen and Woodland (2006)).

While the AICPA required the Rule for membership, the state boards of accountancy would have to adopt the Rule in order for it to be legally required for licensure. While states like Florida and Hawaii adopted the Rule as early as 1979, the majority of the state boards began passing the Rule only after the AICPA's action. Relying on the AICPA's assertions, state boards of accountancy began adopting the requirement as a condition for CPA licensure in the mid-1990s and early 2000s. In the year 2000 alone, 14 states adopted the Rule (see Table 1 for details on the Rule's years of adoption and enactment).

Yet, even before the adoption of the Rule, most jurisdictions already specified a minimum number of hours in business and accounting. Moreover, most states did not change these detailed requirements with the adoption of the Rule.[7] The Rule, which required 150 semester hours rather than a master's degree, was worded in order to provide flexibility to colleges and universities in designing their programs.[8] Specifically, this freedom was granted in order to allow four-year colleges, which do not have the authority to grant master's degrees, the ability to offer programs that could meet the Rule's requirement (Jacob and Murray (2006)).[9] As a result, candidates for the CPA exam could accumulate the additional hours of education through courses associated with a graduate degree (an MBA with an accounting concentration or a master's in accounting), courses from another upper-level undergraduate option (a second major), or courses from no specified program of study at all.[10] As of July 2017, the Rule has been enacted in all 54 U.S. licensing jurisdictions, with the states of New Hampshire, California, and Vermont beginning enforcement in 2014 and Colorado in 2015.

[7]The AICPA pushed for the extra 30 credit hours to be composed of more general liberal arts courses as well as general business courses rather than pure accounting ones (Collins (1989)).

[8]In this regard, the Rule has been criticized for allowing CPA candidates to meet the requirements for licensure with no more hours in business and accounting than what was already required under the old regulations.

[9]The political economy of the Rule can be seen in the case of Oklahoma where the original bill that required graduate courses to fulfill the Rule was not passed after lobbying by four-year universities. The bill eventually passed when the wording was changed to 30 additional hours of higher-level education.

[10]See Online Appendix A for a list of the current educational requirements by state.

2.2 An Economic Framework for the Rule

This section provides a discussion of previous theoretical and empirical work on occupational licensing in order to motivate my analysis of the Rule's effect on the supply of labor into the profession and its quality. Despite the fact that occupational licensing covers 30% of the U.S. workforce, its effects on the supply and quality of professionals is not without tension (Kleiner and Krueger (2013)).

The traditional motivation for occupational licensing stems from the public-interest view of licensing, which asserts that licensing is needed in cases of imperfect information to protect consumers (Shapiro (1986)). Theoretical work along this line claims that credence goods such as attestation demand regulation on the basis of quality certification (Leland (1979) and Shapiro (1986)). In the absence of regulation, consumers, it is alleged, will tend to drift to the low-price, low-quality alternative. This perspective suggests that administrative procedures from regulators (i.e., state boards) regulate the supply of labor in the market. The regulator screens new entrants to the profession and bars those whose skills or character traits suggest low-quality outputs (Gittleman and Kleiner (2013)).The imposition of licensing in such markets may in effect shift the quality-adjusted demand curve upward, improving consumer welfare and increasing the supply of such services by ensuring that only high-ability individuals perform the tasks (Adams III et al. (2003)). In the case of the Rule, the willingness of the individuals to undertake the additional 30 hours of course work should be correlated with increased ability and high quality (Spence (1973)). Moreover, the extra year of education required under the Rule can also be viewed as an additional investment by individuals in their human capital that will lead to increases in the quality of these individuals irrespective of the restrictions on entry (Becker (1993)). Thus, the public-interest view predicts that the supply of CPAs will be lowered by the Rule, but that this decline in supply will represent a decrease in low types and as a result increase the average quality of individuals in the market.

In contrast to the public-interest motive, a capture/private-interest motive for licensing

has been suggested by a large stream of literature in regulatory economics, whereby changes in licensing requirements are introduced by current members of the profession to limit the supply of new entrants and extract monopoly rents (Friedman (1962); Stigler (1971); Maurizi (1974)).[11] On the one hand, it predicts that the Rule's additional 30-credit hour requirement will increase the marginal cost of becoming a CPA and reduce the number of new CPAs as in the public-interest case, but, on the other hand, it holds that the average quality of candidates in the market will remain unchanged or even be reduced.[12] For example, the variety of ways in which to meet the 30-credit-hour requirement of the Rule could make it ineffective in filtering out low types as the requirement may not be less costly to high types. The approximate one-year increase in the necessary time to complete the Rule's educational requirement could potentially lead to adverse selection by high-ability potential CPAs. Their relatively higher opportunity cost of time could force them into substitute fields as a result of the additional year (Akerlof (1970)). This in turn would lead to a decrease in the overall quality of CPAs after the implementation of the Rule. Along this line of reasoning, Lee et al. (1999) analytically incorporate auditors' education and audit effort as joint inputs of audit quality in a Dye (1993) and Dye (1995) model to evaluate the effects of the Rule on the audit market. They show that the audit fees are higher, making pre-Rule CPAs better off and audit clients worse off as a result of the compositional supply changes to the Rule. Finally, the capture/private-interest view predicts that the reduction in supply will lead to increases in rents by the profession. By restricting the supply of CPAs, the Rule will allow the profession to increase their rents (Kleiner (2006) ; Kleiner and Krueger (2013)). As a result, the capture/private-interest view will be supported if we observe reductions in supply and increases in rents that are not accompanied by increases in quality.

[11] The use of licensing raises the entry costs into a profession by imposing a fee as well as training requirements, which shift the short-run and long-run supply curves upward and left (Maurizi (1974)).

[12] Several studies have provided empirical evidence on the supply reducing effects of occupational licensure in other occupations (Shepard (1978) and Carroll and Gaston (1981)).

3 Data

3.1 Samples

This section introduces the data from both the business networking website and my other sources. My primary empirical analysis requires (i) candidate data from the CPA exam, (ii) education and employment histories for a sample of CPAs, and (iii) wage data on individuals in accounting. My analysis of CPA quality relies on data from a leading professional networking website that includes the self-reported resume of each user. My supply and rent extraction tests rely on data from NASBA and the Current Population Survey.

Business Networking Website Data The professional networking website (hereafter referred to as the "website") serves as the world's largest online professional networking and recruiting site. The website began as a networking site for technology and financial industry employees and has grown tremendously ever since. It now covers various industries and has members at all levels of experience, i.e., from college students to senior executives. As of 2014, the website includes executives from all Fortune 500 companies as members. Additionally, its members represent a vast array of age groups: 46% of members are between the ages of 25 and 44 while 35% are between the ages of 45 and 64. With respect to the accounting profession, the website lists over 656,000 CPAs in the continental United States, roughly 60% of the number of individuals estimated by the Bureau of Labor Statistics to be in the occupational category of accounting (of Labor Statistics (2018)). Given the breadth of coverage, the site serves as an effective data source for information on CPAs, including their education and career outcomes.

For the purposes of the present study, computational restrictions on the data collection process limit my study to a sample of individuals drawn from 11 prominent states. The 11 states were chosen by their relative importance in terms of the number of accountants, their contribution to the national GDP, and their relative timing in the enactment of the Rule. Table 2 provides a descriptive overview of the characteristics of the 11 states analyzed in the

study.

I begin the construction of my sample by searching for individuals who either self-report "CPA" or "Certified Public Accountant" on their profiles. The website search is restricted neither by geographical proximity nor by the personal connections of the account used to search. On a state-by-state basis, I draw individuals who entered the labor market (i.e., obtained their CPA) around the enactment of the Rule.[13] To understand the sample size considerations, I conducted several power calculation tests. The goal of a power test is to identify sample sizes required to detect a pre-specified treatment effect (minimum detectable effect) at specified levels of power and statistical significance. In my case, as is consistent with common practice, I consider sample sizes for a specified power of 0.8 and a statistical significance of 0.05.[14] The results of the power test support the following: if I can obtain 1200-1500 individuals into both the treatment and control groups, I will likely be able to detect an effect that represents between a 10–25% change from the baseline rate of the controls. The large minimum detectable effect also takes into account the potential for cross-over (i.e., the possibility that I misclassify treated Rule CPAs as controls) of approximately 15%. As a result, I collect an initial sample of 2,500 individuals per state to ensure sufficient power in my tests.

Based on the selected profiles, I collect workers' information, focusing in particular on the career path of each individual. For each position, I observe the job title held by the individual, the start and end dates for each job title, and the company name. The job titles and descriptions for a given position allow me to classify jobs based on seniority in order to decipher promotions versus lateral changes for individuals. I determine the chronological order of the positions using their arrangement on the profile page, and assign to each worker a unique identifier. I also collect data on the user's gender and current location. I perform

[13]The collection process requires that I screen the profiles afterwards to make sure I capture CPAs and not individuals that list CPA in descriptions of work-related projects such as accounting software in an IT firm.

[14]The power calculations require estimates of variances for treated and control samples. In experimental settings, these are obtained from pre-existing data or acquired from a pilot study. Since I did not have either when I was planning the paper, I made additional assumptions on reasonable estimates for these values.

additional data cleaning by using an individual's unique ID to remove duplicate individuals that may arise due to the automated collection of the profiles. I reshape the data in the resume, which is reported at each job level to a panel that lists an individual's name and work information in each period.

A key issue that I face is multiple overlapping job spells; that is, some individuals may list several occupations over the same period. While I track all occupations for all the individuals in the sample, I need to make some assumptions when conducting the analysis of career transitions. In particular, I limit my analysis to individuals who list a maximum of two simultaneous occupations in a year. These individuals account for more than 90% of my original sample. In order to deal with missing spells or holes in resumes, I classify an individual as unemployed if there is a one- or two-year-long time gap between job (or education) spells and, on the other hand, classify a spell as having missing information if the time gap between two spells is longer than two years.

A critical variable for labor market outcomes is an individual's age. As age is not explicitly included in the resume, I determine an individual's age in a given period under the following rule: (i) age at high school graduation is 18 plus any years of prior full-time employment, (ii) otherwise, age for starting a college degree is 18 plus any years of prior full-time employment, (iii) otherwise, age at completing an Associates Degree is 20 plus any years of prior full-time employment, (iv) otherwise, age for completing a Bachelor's Degree is 22 plus any years of prior full-time employment. I consider 3-year college degrees (typically from international institutions) to be equivalent to bachelor's degrees. In addition, given the sample considerations, I place associates degrees with high school graduates. Finally, I distinguish between the following graduate degrees: masters, juris doctor (JD), masters of business administration (MBA), master's of accounting, and PhD. I assume that missing education signals a high school graduate or less. Missing education also implies missing age since I infer an individual's age based on education dates. In such cases, instead of age, I match years since first work experience.

Finally, in order for a profile to be included in the sample, I require that it contain educational information, such as university attended, degree obtained, and/or graduation year. In addition to the education, I require it to have a complete career history with job titles and dates. Appendix A provides a descriptive table on the sample selection data requirements and changes in the sample. In the end I am left with a sample of over 10,000 CPAs with clean work experience and 8,793 individuals with complete education.

I believe that the extensive amount of collected data, and descriptive evidence suggest that my sample represents a reasonably accurate collection of CPAs. That said, despite my efforts to collect, clean, and validate the data, it is unlikely that I have accurately identified all individuals and their backgrounds for all of my sample. This is the result of three different issues: (1) individuals not registering their resumes on the professional networking website, (2) individuals potentially either omitting or incorrectly listing information on their profiles (preventing me from accurately pairing them to years, identifying prior work experience, or capturing their education and training), and (3) errors in my collection and parsing of these profiles. With respect to reporting biases and errors, inferences based on resume data in general are subject to these concerns given the voluntary nature of the profiles. Yet, the pervasive use of the website by individuals for credible networking, as well as job search purposes, provides some assurance of the integrity of the posted data. Moreover, unlike lying on a resume, which only a prospective employer will see and cannot easily verify, lying on one's profile is publicly visible. This public accountability makes it more difficult for individuals to make false claims about their employment and is distinct from traditional resume data. Despite efforts to reduce these risks, my inferences should be interpreted with these caveats in mind.

Additional Data Sources My CPA supply analysis relies on data from NASBA. NASBA provides data on the number of first-time candidates, repeat candidates, and total candidates for each of the exam periods by jurisdiction from 1984–2014. At the university level, I obtain data on the total number of first-time test takers as well as the number of

individuals who pass all four sections in one sitting and the number of individuals who fail all four sections.

Data for my tests on CPAs' rent extraction comes from the CPS. The CPS has gathered employment and demographic data from 57,000 households on a yearly basis since 1988 to represent the nation as a whole. Employment information for individuals participating in the CPS is based on their occupation and industry. Thus, I select individuals who listed Occupation Code 023, Accountants and Auditors, and Industry Code 089, Professional Services.[15] Additionally, those who did not report positive earnings were dropped from the sample, resulting in a final sample of 6,994 accounting-related individuals. Finally, since data is collected over various years, earnings are converted to 2010 dollars, using the Bureau of Labor Statistics implicit price deflator.

3.2 Labor Market Outcome Variables

In order to examine quality differences between Rule and non-Rule CPAs, I construct non-monetary proxies of labor market outcomes from labor economics and include proxies that where proposed by advocates and critics of the Rule.[16] I describe and motivate below the measures of long-term labor market outcomes, which I build and use in my tests.

Number of Firms & Tenure I use the job titles and individual firm names to obtain an accurate count of the individual's positions during his or her career. The career histories allow me to measure the individuals' tenure at each firm as well as the total number of jobs held. My examination of tenure at firms and turnover follows from economic theory and arguments put forth by supporters of the Rule. Advocates of the Rule justified its implementation as a mechanism to reduce turnover at firms. Moreover, if the Rule had a human capital effect, such as individuals taking additional classes, recent literature in human cap-

[15]It is unlikely that my sample solely includes CPAs. However, my selection criteria are as finely tuned as the census data permits with respect to pinpointing certain professions. It is also not unreasonable to believe that non-CPAs in the sample who work in close proximity to CPAs are also indirectly affected by the occupational regulation of CPAs.

[16]These outcomes were advocated by industry leaders such as J.Michael Cook in his 1996 article "150-Hour Education Programs: A Practitioner's Perspective on Useful Research Possibilities."

ital has proposed that increases in human capital may lead to lower employment turnover. Acemoglu and Pischke (1999) model increases in general human capital as increasing firm productivity through firm-specific skills via complementarities in production. In their framework, any increase in human capital by the Rule would lead to mobility constraints. As a result, I investigate whether the Rule's additional education requirement worked to improve matches between individual CPAs and firms. If it had such an effect, as claimed by it advocates, one would then see a lower number of firms, all else equal, and longer tenures for individuals at the firms.

Time till Promotion The length of time it takes individuals to be promoted should serve as an attractive measure of ability in the absence of compensation. My focus on time till promotion stems from the view that promotions serve to screen employees up the organization and can thus be thought of as a screening mechanism in the internal labor market of the firm. During the pre-promotion period the employee is trying to signal her type with regard to her ability to perform higher-order tasks. As such, the timing until selection for promotion can be seen as part of the screen, as it is being used by the firm to decipher an individual's type. Thus, time to promotion is a function of the underlying labor pool quality, which means that promotion times should be shorter for high quality labor pools and longer for low-quality labor pools.

If the Rule effectively screen high types, one would expect Rule individuals to reach higher-level positions quicker than non-Rule individuals.[17] In order to determine time until promotion, I rely on job titles in the constructed database. The use of job titles stems from the inability to observe wages or a systematic classification of job types with respect to seniority/prestige. Specifically, I classify the job titles in the dataset into a three-tier hierarchy meant to represent the job's level of seniority. Then I take the job titles in the

[17]I believe it is helpful to distinguish between the likelihood of promotion and my duration (time till promotion) measure. In the case of likelihood of promotion, a relative performance evaluation would lead to no change in the expected likelihood of promotion if all individuals were subject to the Rule and the firm needed to promote someone. By contrast, my focus on time till promotion does not rely on the likelihood of promotion as I examine only individuals who get promoted in order to observe changes in the time until promotion.

dataset and match them based on similarity scores to the titles in a dictionary of titles with seniority/prestige classifications from the Department of Labor as well as several online job search engines. The seniority levels are meant to capture jobs that entail a higher level of responsibility and wage rate. Their use allows me to disentangle promotions from lateral moves.[18]

Time in Public Accounting My focus on the time individuals spend in public accounting stems from its prominent use as a motivating factor for the Rule's passage (Cook (1996)). One of the main themes in arguments for the Rule's enactment was its supposed ability to increase the retention of CPAs in public accounting. It was suggested that by screening higher-type CPAs the profession would be able to stop the exit of CPAs from audit firms to clients. Moreover, even if the Rule were to lead to human capital accumulation that makes Rule CPAs' outside options more lucrative, their exit would lead to lower audit quality. Using the detailed career history of individuals, I measure the length of time individuals spend in public accounting. I construct this measure using a combination of individuals' job titles and the name of the firms in which they are employed to determine the specific year in which they exit public accounting for the private sector.

3.3 Descriptive Statistics

3.3.1 Sample Descriptives: NASBA

Descriptive statistics for the NASBA sample are reported in Table 3. The table reports sample means and medians for the number of first-time candidates at the university level for the years 1984–2004. In addition to the total number of test takers, the table includes the number of candidates passing (Passed All) and failing (Passed None) all four sections of the CPA exam. Descriptive statistics for the Rule and non-Rule subsamples are also provided. The average number of candidates per sitting is 20 at the university level with 3.5

[18] Appendix B provides a sample of the job titles that fall into each of the three tiers as well as descriptive statistics on relative rank of seniority jobs in individuals' careers and average tenure in each rank.

on average passing all four sections of the exam and 10.9 on average failing all four sections. A comparison of the Rule and non-Rule subsamples indicates a decrease in the total number of candidates sitting for the exam. The average number of test takers drops from 21.7 in the non-Rule period to 15 in the Rule period. While this decline is reflected in the Passed None number (which drops from 11 to on average 7), it is also evident in the Passed All number (which drops from 3.75 to 2.87 on average). This decline in the Passed All number will be formally analyzed when I examine the treatment effect of the Rule on the supply of CPAs below.

3.3.2 Sample Descriptives: CPA Sample

In Table 4, Panel A, I provide descriptive statistics for my CPA sample of 8,793 CPAs along the lines of demographics, career outcomes, and educational background. The sample is skewed on gender with 61% of the sample being male. On average, individuals have 5.3 jobs during their career, averaging 4 years per job with 63% of the sample being employed at a Big 4 public accounting firm at one point in their career. Twenty-one percent of the sample has worked in taxation, and the mean graduation year in the sample is 1997. More than 50% of the sample have master's degrees with 25% of those degrees being specifically in accounting. In addition to the sample averages, I also report changes in the means between the non-Rule and Rule cohorts. Looking at the full sample, Rule individuals graduate 13 years later than non-Rule individuals, have more master's degrees, have lower average tenure per job (4 years less on average), and are more likely to be employed at a Big 4 firm.

In Panel B, I take into account the differences in age and gender between the two cohorts documented in Panel A, by matching Rule CPAs to non-Rule CPAs based on age and gender. The number of Rule CPAs is lower as I require each Rule CPA to have at least one matched non-Rule CPA. When I do match, the significance on the differences in means between the two cohorts diminishes. The matched sample, in Panel B, shows that Rule individuals tend to be in the Big 4 more often (62% vs. 68%), more likely to specialize in tax, and have more

accounting master's degrees while having no significant difference in the number of jobs held. By matching on graduation year, I control for the effects of the economic conditions that prevail when the individuals enter the labor force as well as the individual's age given that age is a function of graduation year.

3.3.3 Sample Descriptives: CPS

Descriptive statistics for the CPS sample data are presented in Table 5. The table provides a skeletal profile of accountants engaged in professional services over the years of 1985–2015. The individuals are on average 38 years of age, with approximately 16 years of education. The sample is predominantly white 90% and male 57%. As can be expected, average earnings of $47,684 are above the U.S. population average. More than 68% of the sample is married. Finally, 63% of the sample works in states with the Rule requirement.

4 Results

The results discussion is split into four sections. First, I estimate the impact of the Rule on the supply of CPAs by focusing on how the Rule impacts the lower and upper ends of the quality distribution of candidates. Second, I evaluate the Rule's effects on the quality of CPAs, using long-term labor market measures. Third, I measure the extent of rent extraction on the part of the profession as a result of the Rule's enactment. Finally, I provide several sensitivity and robustness tests.

4.1 The Rule's Effect on Supply

I measure the impact of the Rule on the supply of CPAs using a difference-in-differences framework. Previous studies examining the supply effects of the Rule have found reductions in the number of candidates sitting for the exam (Jacob and Murray (2006)). These reductions do not provide direct evidence of the Rule's quality effects, given limitations of

the data and their focus on pass rates. For example, decreases in the number of low types sitting for the exam do not translate into increases in quality as these individuals would have failed the exam even without the Rule. On the other hand, decreases in the number of high types would suggest a deterioration in quality. This view motivates my focus on where the reductions in the number of candidates are coming from in the distribution of candidates taking the exam.

The staggered adoption of the Rule provides me with non-Rule counterfactuals from both the time series (i.e. before and after the Rule) and in the cross section (i.e. states that have yet to adopt the Rule in a given year). I analyze the marginal effect of the Rule on the total number of test takers, the number of test takers who pass all four sections in a sitting, and the number of test takers who fail all four sections, using the following fixed-effect specification:

$$\begin{aligned} Y_{u,m,y} &= \beta_1(Rule_{u,m,y}) + \beta_2(Year\_Before\_Adoption_{u,m,y}) + \beta_3(May\_Sitting_{u,m,y}) + (1) \\ & \quad Year\_FE + University\_FE + University\_FE * Year + \epsilon_{it}, \end{aligned}$$

where $Y_{u,m,y}$ is either the log number of candidates, the log number of candidates passing all four sections, or the log number failing all four sections in university u in sitting m and year y. $Year\_FE$ is a vector of year identifiers that take the value of one when the observation is for year y and zero otherwise. The year fixed effects are used to control for macroeconomic factors and shocks that may affect all universities in a given year. $University\_FE$ is a vector of university identifiers that take the value of one when the observation is from university u and zero otherwise. The university fixed effects are introduced to control for shocks in the educational quality at the university level. By using university observations, I am able to reduce the noise that aggregation at the state level may cause, thereby increasing the power of my tests. $University\_FE * Year$ is a university-specific linear time trend that allows each university to have its own time trend with respect to the outcome measure. $Year\_Before\_150$ is an indicator variable set to one the year before the Rule is implemented

and is used to capture any run-up in the supply. $May\_Sitting_{u,m,y}$ is an indicator variable set to one for sittings of the exam in the month of May in order to pick up differentials in testing in May relative to November. Finally, *Rule* is a binary indicator variable that takes the value of one if the jurisdiction implements the Rule in that year and zero otherwise. Thus, β_1 provides the marginal change in the number of test takers driven by the Rule's implementation.

Table 6 reports the results of the three specifications. In Model 1, I find that the Rule reduces the number of test takers by roughly 15% after controlling for year, university fixed effects, and university-specific time trends in the number of test-takers. Consistent with an anticipation of the implementation of the Rule, the year before the Rule goes into effect is associated with a 21% increase in the number of test takers. The *May Sitting* identifier controls for the fact that 8% fewer candidates take the exam in May. While a reduction in the overall number of test takers has been used as evidence of an increase in the quality of candidates, I use Models 2 and 3 to examine what part of the distribution of candidate quality the Rule's supply reduction affects.

In Model 2, I examine the number of high types. I find that the Rule's implementation leads to a reduction of 10% in the number of high type test takers. This reduction is inconsistent with the public-interest view that would suggest no change or even an increase in the number of high types after the Rule's implementation. Model 3 confirms that there is also a reduction in the number of low types (by 14%). Model 3 also confirms the run-up in the number of low-type candidates taking the exam the year before the Rule goes into effect and the lower number of candidates in the May sittings. This reduction in the number of low types is not necessarily related to an increase in quality as these individuals would have failed the exam even if the Rule were not in place. Thus, reductions in both high and low types of candidates necessitate an examination of actual labor market outcomes in order to decipher quality effects.

4.2 The Rule's Effect on Quality: Long-term Labor Market Outcomes

4.2.1 Firm Match and Average Time in Job Position

I begin my analysis of the Rule's effects on long-term labor market outcomes by examining the match quality between CPAs and firms' using differences in average job turnover and firm retention. In Table 7, I examine the average individual tenure at each position for both Rule and non-Rule individuals. I use a student t-test and a Wilcoxon rank-sum test to determine the significance of differences. In Panel A, I report the differences between the two cohorts, using the full sample, for their first 5 jobs. In the case of the full sample, results point to Rule individuals spending an average of 3 years less at their first and second jobs as compared to their non-Rule counterparts. Even at their fifth position, there is a significant difference of a year and a half less for the Rule cohort. Yet, their is a timing difference with respect to when these cohorts enter the labor market; therefore, in Panel B, I control for timing differences by using a matched sample based on age and gender. When I match the cohorts, I find no significant difference in the average tenure at each position except for the third job where Rule individuals spend on average .84 years more. Overall, controlling for the year in which individuals enter the market (age), the results point to no difference in job commitment as measured by the average tenure at each position.

I go on to test more formally the Rule's effect on firm-employee match quality by regressing the number of firms an individual has worked in over their career on the Rule and several determinants of firm commitment. The rationale behind the test is that better firm-employee matches lead to a lower number of firms over the career of the individual and thus lower firm turnover. To isolate the Rule's effect, I control for the gender of the individual and whether the individual began at a Big 4 firm. The inclusion of the $Began\_at\_Big4$ in the model is meant to capture differences in career tracks that initial Big 4 placements may cause.[19] I

[19] I add $Began\_at\_Big4$ since descriptive statistics show a general trend in accounting toward starting one's career at a Big 4 firm and I want to untangle that effect from the Rule's implementation effect.

control for the individual's age by using cohort fixed effects and control for variation in state economic characteristics by using state fixed effects.

$$\begin{aligned} Log\_Num\_Firms_i &= \beta_1(Rule_i) + \beta_2(Male_i) + \beta_3(Began\_at\_Big4_i) + \\ &\quad Cohort\_FE + State\_FE + \epsilon_i \end{aligned} \tag{2}$$

I run the above model via ordinary least squares as well as a negative binomial regression in order account for the the count nature of the outcome measure ($Numberof Firms$). If the Rule had an effect on individuals' mobility between firms we should expect to see a significant effect on β_1. Table 8 reports the results of the firm-tenure test. While unconditionally it seems that Rule individuals stay longer at firms (i.e., work in fewer firms), when I control for the age/cohort of individuals and the economic environment of the state (Model 3) the Rule has no incremental effect on firm tenure. Model 4 reports the results of the negative binomial regression and again confirms that the Rule does not incrementally explain individuals' firm tenure. Interestingly, males work in 3% more firms even though starting one's career at a Big 4 firm leads to working in 2% fewer firms over one's career.

4.2.2 Duration Analysis

While tenure at a firm and position may not fully reflect an individual's ability, one's time until promotion may be more informative of performance and quality. I investigate the determinants of the time elapsed until an individual is either promoted, in the first case, or exits public accounting, in the second case. To achieve this goal, I perform a duration analysis to determine unconditionally, as well as conditionally on a set of covariates, the likelihood of an individual being promoted or exiting public accounting over time.

In order to perform the duration analysis, I use the CPA profiles to obtain the start and end dates at each position and calculate the time spent at the position.[20] I then classify

[20]This is outflow sampling, which implies that my tests are free of censoring concerns, which are one of the most prevalent issues in duration analysis.

these positions with respect to their seniority in an organization in order to perform the promotion tests. Specifically, to construct the seniority ranking, I take all job titles in the dataset and match them based on similarity scores to the titles in a dictionary of titles with seniority/prestige classifications from the Department of Labor and several online job search engines. The seniority levels are meant to capture variation in the levels of responsibility and wage rates for the jobs in my sample.[21] Their use allows me to disentangle individuals' promotions from lateral moves.[22]

I formally examine differences in time till promotion and time in public accounting using a Cox proportional hazard model. The Cox model is a semi-parametric method for analyzing the effects of different covariates on the hazard function.[23] To examine the duration of the individuals at their position, I model the following specification:

$$NumberofYears_i = \beta_1(Rule_i) + \beta_2(Male_i) + Cohort\_FE + State\_FE \tag{3}$$

where $NumberofYears_i$ is either the number of years until individual i is promoted or the number of years in public accounting before exiting, $Rule_i$ is an indicator of the individual being subject to the Rule, $Male$ is an indicator variable set to one if the individual is male and zero otherwise, $Cohort\_FE$ are set to one in the year the individual entered the job market and capture the individual's age, and $State\_FE$ are state fixed effects to capture state economic conditions. The hazard ratio is β_1 is .

Time till Promotion I begin my analysis of promotion timing by examining differences in the average time till promotion to each of the seniority levels between Rule and non-Rule individuals in Table 9. I examine the significance parametrically using a student t-test and non-parametrically using a Wilcoxon rank sum test. The table provides averages for the full sample (Panel A) and a matched sample based on age and gender (Panel B). While Panel A

[21]The use of job levels stems from my inability to observe wages or a systematic classification of job types with respect to seniority/ prestige in the website.

[22]Sample titles and descriptives of classified positions are provided in Appendix B.

[23]More information on the Cox model can be obtained from Cox (1972) and Wooldridge (2010).

shows a significant difference in the time till promotion between the two groups, when age and gender are controlled for in the matching, the average difference in promotion timing disappears. As a result, I go on to examine the time till promotion, using survival analysis and hazard rates.

In Table 10, I tabulate the results of the more formal duration analysis of the promotions on the full sample. In Panel A, I plot the Kaplan–Meier survival estimates for the two cohorts with respect to promotions. The graph of promotions to level two seniority positions is displayed on the left while promotions to level-three positions are displayed on the right. The y-axis gives the percentage of the cohorts yet to be promoted while the x-axis traces the number of years. Comparing the Rule (dashed line) to the non-Rule cohort, the Rule cohort is promoted to level-two and level-three seniority positions in a shorter time span. Yet, as the previous test showed, the timing of when these individuals entered the market plays a big role in their outcomes, which these estimates fail to incorporate.

In Panel B, I run cox models on the duration to promotions, which allow me to control for time effects and gain a more accurate measure of the difference between the two groups. The results for the level-two seniority positions are on the left while the level-three seniority results are on the right. The four models display the effects on the hazard rate of the Rule controlling for time effects, using cohort fixed effects, and state fixed effects. When just the $Post\_150$ variable is included, Model 1, shows that the rate of promotion for the Rule cohort increases by 85% for level-two promotions and 43% for level-three promotions. These results do not change when I control for the gender of the individuals. Yet, when I control for their age and the year in which they enter the labor market via cohort and state fixed effects, the hazard rates decrease for both promotion levels and become statistically insignificant. The rate of promotion decreases to almost no difference for level-two seniority positions and a statistically insignificant 10% for level-three seniority promotions. Overall, the results of the duration analysis of promotions signal no significant difference in the promotion rate

between the two cohorts once I control for the year in which they enter the market.[24] Thus, while the Rule does not have an effect on time till promotion, it seems that over the years the relative time to promotion decreases in accounting.

Time in Public Accounting I examine the Rule's effect on the time spent in public accounting by looking at individuals' exit rates from public accounting. Many advocates of the Rule claimed it would help retain CPAs and stem the exit to private industry (Anderson, 1988). A natural way to test this claim would be to examine the difference in exit rates from public accounting between the Rule and non-Rule counterparts. In Table 11, I report descriptive statistics on the number of years and number of positions spent in public accounting by individuals. In Panel A, I report the results for the full sample while Panel B reports the results for the matched sample. Looking at the full sample, one sees that the Rule cohort spends a larger percentage of their career in public accounting (52% vs. 41%), yet they also tend to have less experience (12 years on average). As a result, I use the matched sample on age and gender to examine the effects and find no significant difference in the percentage of careers in public accounting while finding that the Rule cohort has a lower number of positions in public accounting.

In Table 12, I provide a formal duration analysis of the Rule's effects on the time spent in public accounting. Panel A graphs the Kaplan–Meier survival estimates for the full sample (left) and the matched sample (right). When the full sample is used, it appears that the Rule cohort exits public accounting at a faster rate. This result is diminished when the matched sample is used. In Panel B, where the failure function, i.e., the percentage of the cohort that has left public accounting, is listed by the number of working years, one sees that 10 years out, 40% of the non-Rule cohort has exited, compared to 64% of the Rule cohort. In Panel C, I run Cox models on both samples, controlling for the effects of gender, age and timing of one's entry into the labor market, and state fixed effects. The results for the full

[24]The same result is obtained if I used the matched sample for the duration analysis as seen in AppendixC. The matching makes the difference in the timing between the two groups close to zero for promotions to both level-two and three-seniority positions.

sample point to a 25% increase in the exit rate from public accounting for the Rule cohort. In the matched sample, this increase in the exit rate drops to 18% and becomes statistically insignificant. Overall, the results of the analysis of duration in public accounting point to the Rule leading to a slight decrease in the time spent in public accounting. These results point to the Rule not effectively increasing individuals' commitment to public accounting as many proponents claimed it would do. Moreover, their exit suggests that the quality of work in the profession could have decreased given this increase of accountants leaving public accounting.

4.3 The Rule's Effect on Earnings

A reduction in supply and no change in the average quality of CPAs is consistent with a rent extraction motive of the capture/private-interest view. The documented relation above could also be driven by the presence of both adverse selection and positive screening changing the distribution. Thus, in order to truly examine the capture/private-interest view one needs wage data to test whether earnings increased solely because of the Rule's supply reduction. In this section I examine the Rule's effect on accountants' earnings.

Models of the determinants of workers' earnings have a long history in labor economics (Mincer (1958) and Card (1999)). The most common specification, derived from Mincer (1974), specifies that one's log earnings is a linear combination of explanatory variables such as age, gender, education, and a random error term. In order to capture the effect of the Rule on earnings, I implement a Mincer equation and include an indicator variable for the presence of the Rule in a given state and year. If the Rule had any rent-extraction effects, the indicator should be positive and significant whereas if the screening and human capital effects dominated the Rule's implementation the increase in wages should be explained solely through the schooling variable rather than the Rule indicator. I regress log earnings of accountants on the various determinants. In addition to the Rule indicator, I follow previous studies and include age, age squared, race, education and marital status as determinants of

wages. Formally, I estimate the following model:

$$\begin{aligned} Log\_E_i &= \beta_1 Age_i + \beta_2 Age^2 + \beta_3 Male_i + \beta_4 White_i + \beta_5 Education_i + \\ &\beta_6 Married_i + \beta_7 Rule_i + Year\_Fixed\_Effects + State\_Fixed\_Effects + \epsilon_i. \end{aligned} \tag{4}$$

Table 13 reports the result of the earnings regressions.[25] In each model, consistent with previous studies age is positively associated with earnings but at a decreasing rate as indicated by the negative coefficient on age squared. *Education* is measured as the number of years of schooling. It is also positive and significantly associated with earnings, each year of schooling is associated with a 10% increase in one's earnings. In Model 2, I introduce the Rule and find that it is incrementally significant and positively associated with earnings. In Model 3, I control for both year and state fixed effects and find that the Rule is associated with a 9% increase in earnings. This 9% premium is above what is explained by an individual's years of schooling and as such represents rents extracted by the Rule's implementation rather than any human capital accumulation. If the Rule had only a human capital effect we would not observe any incremental premium and hence the result supports a capture/private-interest motive behind the Rule.

4.4 Robustness Tests

Time till Partner While the duration analysis of time until promotion for the two cohorts showed no evidence of a statistical or economic difference between Rule and non-Rule cohorts, some may claim that the result is driven by noise in the seniority classification scheme. As a result, I limit my sample to public accounting partners and re-run the Cox model for individuals who ultimately become partners. The use of this subsample, in which the career progression is more comparable for the two cohorts, allows for a cleaner test of outcomes. Table 14 provides the hazard rate for the Rule and non-Rule cohorts. I find that while

[25]Results are robust to using non-linear years of schooling fixed effects rather than the linear number of school years.

unconditionally there is an increase in the speed to the partnership, when I control for the year in which individuals enter the market and age using cohort fixed effects and state fixed effects, there is no significant difference in the rates to partnership between the two cohorts. Additionally, some may view that the Big 4's rigid promotion structure would lead to lower power. As a result, I run the partner test on a sample of Big 4 partners and a sample of non-Big 4 partners and continue to find no effect.

Master's Degrees vs. Non-Master's Degrees I also evaluate whether my findings can be driven by noise in the resume data. Noisy data would lead to a null result, driven by a lack of power. To test the validity of the data, I examine differences between master's degree CPAs and non-master's CPAs. The benefits of a master's degree are well documented in the literature in labor economics (Arrow (1973); Spence (1973); Card (1999); Dupray (2001)). The concept of private returns to a college degree, including a master's degree, is drawn from human capital theory, which states that the earned income of individuals is a function of labor productivity, derived from investments in education (Becker (1993)). With regard to benefits, researchers note that trends in college enrollment generally mirror trends in the college earnings premium (i.e., the gap in earnings between college and high school graduates) (Becker (1993); Ellwood et al. (2000)).

If my results were driven by noise in the resume data, I would not expect to find a difference between these individuals. In Table 15, I rerun my tests on master's versus non-master's degree holders by matching individuals by age and gender. I find that individuals with master's degrees are more likely to be employed at Big 4 accounting firms and specialize in taxation. Additionally, they spend less time at each position, have more jobs, and are promoted at quicker rates. The promotion results are consistent with prior work on the value of a master's degree. In Table 15, Panel C, I examine whether the Rule affected the quality of master's degree holders and find that master's degrees are not significantly better off after the Rule, as measured by decreases in the time till promotion. These findings help alleviate issues of noise in the data and further confirm the benefits associated with a graduate degree.

5 Conclusion

In this paper, I empirically examine the effects of the 150-Hour Rule on accountants' labor market outcomes. While virtually the entire country now requires an extra year of education for CPAs, there is scant evidence on the long-run impacts of this policy change. Moreover, there is a paucity of evidence on the general effects of licensing requirements on the quality and preparation of those who select into the accounting profession. Competing theoretical views on the nature of licensing and disparity in the implementation of the Rule generate significant tension as to the outcomes. For example, the Rule's reduction in the supply of test takers seems to have come from reductions in both high types (those who always pass) as well as low types (those who never pass), thus requiring an examination of the actual labor market outcomes to decipher the quality effects.

In a departure from previous studies, which examine only CPA pass rates, I utilize a comprehensive sample of employment and educational histories for licensed CPAs from a large online professional networking site to assess the long-term labor market impacts of the Rule on career outcomes. In particular, I compare the career paths of individuals' who are subject to the Rule with those who are not subject to it along the dimensions of average number of firms, tenure, number of positions, time to promotion, time in public accounting, and earnings.

I find that Rule individuals are more likely to be employed at a Big 4 public accounting firm, are more likely to specialize in taxation, and are more likely to have more graduate degrees. Yet, I find no significant difference in their time until promotion as compared to their non-Rule counterparts. Further, I find that while individuals subject to the Rule spend a larger percentage of their career in public accounting, they exit public accounting at quicker rates than their non-Rule counterparts. Finally, I document an increase in the earnings of accountants associated with the Rule's enactment that cannot be explained by an increase in education, thus providing evidence of rent-extraction from the Rule's implementation.

This paper makes several contributions to both the profession and the academic liter-

ature. First, it is the first study to provide large sample evidence of the makeup of the accounting profession and the long-term labor market effects of the Rule by utilizing novel career and educational data from a large professional networking site. Second, my results lays a foundation for research on the effects of human capital on financial reporting. Francis (2011) suggests that "audits are of higher quality when undertaken by competent people." Yet, he goes on to mention how "the fact remains that we know very little about the people who conduct audits" (Francis (2011)). In this paper, I provide a novel approach using resume data to develop measures of human capital, i.e., for auditors, which in turn can be used to directly examine the effects of human capital on the disclosure of information. Finally, this paper contributes to the broader literature on occupational licensing and education. The Rule's supply restriction, its increase in rents, and the lack of a quality effect further cast doubt on the effectiveness of using increases in licensing restrictions in the labor market to increase quality. Moreover, using the Rule, the present paper sheds light on the current debate in law and medicine on the reasonableness of reducing the number of mandated school years to increase access to their respective professions while still maintaining quality.

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Table 1: 150-Hour Rule Jurisdiction Adoption List

Jurisdiction	Enactment Year	Effective Date	Adoption Rank	Jurisdiction	Enactment Year	Effective Date	Adoption Rank
Florida	1979	8/1/83	1	**West Virginia**	1989	2/15/00	27
Tennessee	1987	4/14/93	2	**Guam**	1994	6/1/00	28
Utah	1981	7/1/94	3	**Idaho**	1993	7/1/00	29
Alabama	1989	1/1/95	4	**New Jersey**	1995	7/1/00	30
Mississippi	1990	2/1/95	5	**Washington**	1995	7/1/00	31
Louisiana	1990	12/31/96	6	**Hawaii**	1977	12/31/00	32
Kansas	1990	6/30/97	7	**Alaska**	1991	1/1/01	33
Montana	1989	7/1/97	8	**Illinois**	1991	1/1/01	34
South Carolina	1991	7/1/97	9	**Iowa**	1992	1/1/01	35
Texas	1989	8/31/97	10	**Nevada**	1993	1/1/01	36
Arkansas	1990	1/1/98	11	**North Carolina**	1997	1/1/01	37
Georgia	1991	1/1/98	12	**Wisconsin**	1996	1/1/01	38
Nebraska	1991	1/1/98	13	**Massachusetts**	1998	7/1/02	39
South Dakota	1992	1/1/98	14	**Maine**	1997	5/1/03	40
Missouri	1993	6/30/99	15	**Michigan**	1998	7/1/03	41
Maryland	1993	7/1/99	16	**Oklahoma**	1998	7/1/03	42
Rhode Island	1992	7/1/99	17	**Arizona**	1999	6/30/04	43
Connecticut	1992	1/1/00	18	**New Mexico**	1999	7/1/04	44
Indiana	1992	1/1/00	19	**Minnesota**	2000	6/1/06	45
Kentucky	1990	1/1/00	20	**Virginia**	1999	7/1/06	46
North Dakota	1993	1/1/00	21	**New York**	1998	8/1/09	47
Ohio	1992	1/1/00	22	**Pennsylvania**	1996	1/1/11	48
Oregon	1997	1/1/00	23	**Delaware**	2010	8/1/12	49
Puerto Rico	1994	1/1/00	24	**California**	2009	1/1/14	50
Wyoming	1993	1/1/00	25	**New Hampshire**	N/A	7/1/14	51
District of Colombia	1995	1/2/00	26	**Vermont**	N/A	7/1/14	52
				Colorado	1992	7/1/15	53

This table reports the year in which each jurisdiction enacted the Rule (Enactment Year) as well as the date on which the Rule became effective (Effective Date). Additionally, the order of adoption is reported in the Adoption Rank column.

Table 2: Professional Networking Sample States' Descriptives

State	Effective Date	Initial Profiles	Clean Profiles	16-yr. Avg. Num of Accountants	Rank of State	16-yr. Avg. % Contribution of State to U.S. GDP	Rank of State
Florida	8/1/83	2,500	2,264	61,523	4	5.13%	4
Texas	8/31/97	2,500	2,278	77,239	3	7.99%	2
Georgia	1/1/98	2,500	2,285	26,824	12	2.88%	10
Connecticut	1/1/00	2,500	2,292	15,836	23	1.57%	23
Pennsylvania	1/1/00	2,500	2,313	43,044	6	3.93%	6
Illinois	1/1/01	2,500	2,288	46,916	5	4.61%	5
Massachusetts	7/1/02	2,500	2,303	29,146	11	2.63%	13
Michigan	7/1/03	2,500	2,288	30,426	10	2.95%	9
New York	8/1/09	2,500	2,412	86,478	2	7.77%	3
California	1/1/14	2,500	2,338	114,859	1	13.13%	1
Colorado	7/1/15	2,500	2,292	22,872	13	1.74%	21

This table provides the adoption dates of the 150-Hour Rule for the eleven selected states. Additionally, descriptive data is provided with regard to the average number of accountants in the state, the state's rank in terms of accountants, the state's 16-year average contribution to national Gross Domestic Product as well as the rank of each state in each of the categories.

Table 3: University-Level CPA Exam Descriptive Statistics

	Full Sample Mean / Median	Non-Rule Mean / Median	Rule Mean / Median
Number of Candidates	20.41 (12.00)	21.73 (13.00)	15.15 (10.00)
Passed All	3.57 (2.00)	3.75 (2.00)	2.87 (1.01)
Passed None	10.97 (7.00)	11.77 (7.00)	7.79 (5.00)
Observations	18,875	15,095	3,780

This table presents the descriptive statistics on the number of candidates taking the CPA exam at the university level from 1984 to 2004. Number of Candidates is the number of first-time test takers in the specific sitting. Passed All is the number of first-time test takers who passed all four sections of the exam in the sitting. Passed None is the number of firs-time test takers who failed all four sections of the exam. The observations have been split between the pre- and post-periods of states implementing the Rule. Observations from states without observations in either the pre-period or post-period have been deleted.

Table 4: CPA Sample: Career and Education Descriptives

Panel A: Full Sample

	Full Sample				Non-Rule				Rule				Difference in Means		Non-Parametric
	Obs.	Mean	Median	Std. Deviation	Obs.	Mean	Median	Std. Deviation	Obs.	Mean	Median	Std. Deviation	Rule-Non	t-test	Rank-sum test
Male	8,793	0.610	1	0.488	5,593	0.632	1	0.482	3,200	0.571	1	0.495	-0.061***	(-5.649)	[-5.680***]
Number of Jobs	8,793	5.296	5	2.958	5,593	5.885	5	3.101	3,200	4.267	4	2.358	-1.618***	(-27.522)	[-25.937***]
Years per Job	8,793	4.350	3.083	4.467	5,593	5.167	3.633	5.092	3,200	2.922	2.250	2.516	-2.244***	(-27.599)	[-34.628***]
Big 4	8,793	0.637	1	0.481	5,593	0.619	1	0.486	3,200	0.667	1	0.471	0.048***	(4.529)	[4.488***]
Tax	8,793	0.212	0	0.409	5,593	0.201	0	0.401	3,200	0.231	0	0.422	0.030***	(3.319)	[3.363***]
Year Graduated	8,793	1997.915	1999	11.193	5,593	1992.880	1993	10.061	3,200	2006.714	2008	6.754	13.834***	(76.910)	[57.677***]
Number of Degrees	8,793	1.546	1	0.638	5,593	1.516	1	0.625	3,200	1.600	2	0.656	0.084***	(5.891)	[6.000***]
Master's Degree	8,793	0.504	1	0.500	5,593	0.476	0	0.499	3,200	0.553	1	0.497	0.078***	(7.052)	[7.024***]
Non-Accounting Master's	8,793	0.259	0	0.438	5,593	0.285	0	0.452	3,200	0.213	0	0.410	-0.072***	(-7.643)	[-7.420***]
Accounting Master's	8,793	0.245	0	0.430	5,593	0.190	0	0.393	3,200	0.340	0	0.474	0.150***	(15.161)	[15.724***]

Panel B: Rule Matched Sample Based on Age & Gender

	Full Sample				Non-Rule				Rule				Difference in Means		Non-Parametric
	Obs.	Mean	Median	Std. Deviation	Obs.	Mean	Median	Std. Deviation	Obs.	Mean	Median	Std. Deviation	Rule-Non	t-test	Rank-sum test
Male	5,818	0.576	1	0.494	2,909	0.576	1	0.494	2,909	0.576	0	0.494	0	(0)	[0]
Number of Positions	5,818	4.387	4	2.329	2,909	4.412	4	2.313	2,909	4.362	4	2.345	-0.050	(-0.497)	[-1.675]
Years Per Job	5,818	3.017	2.306	2.614	2,909	3.001	2.259	2.657	2,909	3.033	2.347	2.570	0.033	(0.293)	[3.171***]
Big 4	5,818	0.656	1	0.475	2,909	0.627	1	0.484	2,909	0.684	1	0.465	0.057***	(2.829)	[4.608***]
Tax	5,818	0.196	0	0.397	2,909	0.160	0	0.366	2,909	0.232	0	0.422	0.072***	(4.175)	[6.940***]
Year Graduated	5,818	2005.802	2007	6.395	2,909	2005.802	2007	6.395	2,909	2005.802	2007	6.395	0	(0)	[0]
Number of Degrees	5,818	1.529	1	0.637	2,909	1.455	1	0.606	2,909	1.604	2	0.657	0.149***	(5.432)	[9.239***]
Master's Degree	5,818	0.490	0	0.500	2,909	0.423	0	0.494	2,909	0.557	1	0.497	0.134***	(6.272)	[10.201***]
Non-Accounting Master	5,818	0.203	0	0.403	2,909	0.193	0	0.395	2,909	0.214	0	0.410	0.021	(1.206)	[1.987**]
Accounting Master	5,818	0.287	0	0.452	2,909	0.230	0	0.421	2,909	0.343	0	0.475	0.113***	(5.756)	[9.508***]

This table provides descriptive statistics on the demographics, career outcomes, and educational choices for the sample of certified public accountants drawn from the professional networking website. Apart from providing the number of observations, sample average, median, and standard deviation for each variable in the full sample, I also provide descriptive statistics for the subsample of Rule and non-Rule individuals. Differences in means between the Rule and non-Rule samples are also provided. The significance of the difference is evaluated parametrically using student t-tests, reported in parentheses, and non-parametrically, using the Wilcoxon rank-sum test, reported in brackets. Panel A provides the descriptives for the full sample while Panel B provides descriptives for a matched sample, where Rule individuals are matched to Non-Rule individuals based on age/cohort-year and gender. The variables reported are the following: $Male$ is an indicator variable set to one if the individual is male and zero otherwise; $Numberof Positions$ is a count of the number of positions that the individuals have held in their career, as reported in their profile; $YearsperJob$ is the length of time spent at each position for each individual; $Big4$ is an indicator variable set to one if the individual has worked at an international accounting firm such as Deloitte, PWC, E&Y, KPMG, or Arthur Andersen; Tax is an indicator variable set to one if the individual has worked in the area of tax as designated by his position or firm; Year Graduated, is the year in which the individual received his degree before entering the labor market; Number of Degrees, is the total number of degrees above the high school degree that the individual reports; $Master'sDegree$ is an indicator variable set to one if the individual reports any postgraduate degree; $Non-AccountingMasters$ is an indicator variable set to one if the individual reports a non-accounting-specific master's such as an MBA; $AccountingMasters$ is an indicator variable set to one if the individual reports an accounting specific master's such as MACC or MST. The final three columns of each panel provide differences in means between the groups as well as the significance of the difference using student t-tests and nonparametric Wilcoxon rank-sum tests. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: CPS Descriptive Statistics

	Obs.	Mean	Std.Dev	25%	Median	75%
Earnings	6,996	47,684	60,727	18,000	34,000	57,000
Age	6,996	38.8	12.5	28	37	47
White	6,996	.90	.299	1	1	1
Male	6,996	.57	.495	0	1	1
Married	6,996	.68	.466	0	1	1
Years of Schooling	6,996	15.9	1.48	16	16	16
Rule	6,996	.64	.481	0	1	1

This table presents the descriptive statistics on the earnings and demographics of accounting professionals from the CPS. The table provides the number of observations, sample average, standard deviation, 25^{th}percentile, median, and 75^{th}percentile for each variable. The variables reported are the following: *Earnings* is the annual wage and salary reported by an individual converted to 2009 real dollars; *Age* is the age in years of the individual at the time of the survey; $White$ is an indicator variable set to one if the individual identifies as white caucasian; $Male$ is an indicator variable set to one if the individual is male and zero otherwise; $Married$ is an variable set to one if the individual is married and zero otherwise; $YearsofSchooling$ is the number of years an individual has been in school; *Rule* is an indicator variable that is set to one if an individual is in a Rule state while the Rule is being implemented and zero otherwise.

Table 6: The Rule's Effect on the Supply of CPAs

	(1) Num Cand β / SE	(2) Pass All β / SE	(3) Pass None β / SE
Rule	-0.151**	-0.106**	-0.147**
	(0.058)	(0.043)	(0.067)
Year Before Adoption	0.213***	0.005	0.279***
	(0.041)	(0.026)	(0.048)
May Sitting	-0.086***	-0.092***	-0.049**
	(0.029)	(0.026)	(0.023)
Year Fixed Effect	Yes	Yes	Yes
University Fixed Effect	Yes	Yes	Yes
University Specific Time Trend	Yes	Yes	Yes
Observations	25,868	25,868	25,868
Adj. R^2	0.465	0.580	0.345

This table analyzes the effects of the Rule on the number and distribution of candidates taking the exam. The dependent variables are: the log number of candidates (Model 1), the log number of test takers passing all four sections of the exam (Model 2) and the log number of test takers failing all four sections of the exam (Model 3). The observations are measured at the university level. The variable of interest is *Rule*, an indicator variable set to one in the years of the Rule's implementation and zero otherwise. I control for any run-up in the exam by using $Year\_Before\_Adoption$, which is an indicator variable equal to one the year before adoption of the Rule and zero otherwise. Additionally, I control for the month of the sitting by using $May\_Sitting$, which is an indicator variable set to one if the sitting is in May and zero otherwise. Finally each model is run with year and university fixed effects in order to control for unobservable invariant variation within years and universities as well as with university-specific time trends. Significance levels are indicated by: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 7: Average Time at Each Position

Panel A: Average Tenure per Position for Full Sample

	Full Sample				Non-Rule				Rule				Difference in Means		
	Obs.	**Mean**	**Median**	**Std. Dev.**	**Obs**	**Mean**	**Median**	**Std. Dev.**	**Obs.**	**Mean**	**Median**	**Std. Dev.**	**Rule-Non**	**t-test**	**Rank-sum**
1st Position Tenure	8,792	4.720	3.000	5.430	5,593	5.453	3.333	6.184	3,199	3.438	2.417	3.405	-2.015***	(-19.704)	[-20.012***]
2nd Position Tenure	8,260	3.997	2.500	4.826	5,323	4.779	3.000	5.526	2,937	2.579	1.917	2.655	-2.200***	(-24.387)	[-23.308***]
3rd Position Tenure	7,354	3.636	2.250	4.148	4,909	4.243	2.750	4.678	2,445	2.418	1.833	2.365	-1.826***	(-22.229)	[-20.013***]
4th Position Tenure	6,167	3.414	2.167	3.733	4,345	3.885	2.583	4.142	1,822	2.290	1.750	2.112	-1.595***	(-19.946)	[-17.413***]
5th Position Tenure	4,849	3.178	2.083	3.323	3,590	3.550	2.333	3.586	1,259	2.116	1.500	2.082	-1.434***	(-17.103)	[-15.470***]

Panel B: Average Tenure per Position for Matched Sample Based on Age and Gender

	Full Sample				Non-Rule				Rule				Difference in Means		
	Obs.	**Mean**	**Median**	**Std. Dev.**	**Obs.**	**Mean**	**Median**	**Std. Dev.**	**Obs**	**Mean**	**Median**	**Std. Dev.**	**Rule-Non**	**t-test**	**Rank-sum**
1st Position Tenure	5,818	3.695	2.250	4.136	2,909	3.859	2.167	4.718	2,909	3.531	2.500	3.450	-0.328*	(-1.925)	[-1.754*]
2nd Position Tenure	5,322	2.584	1.917	2.750	2,622	2.501	1.833	2.795	2,700	2.665	2.000	2.705	0.164	(1.353)	[4.787***]
3rd Position Tenure	4,549	2.359	1.833	2.329	2,260	2.248	1.667	2.292	2,289	2.468	1.917	2.361	0.220**	(1.979)	[5.300***]
4th Position Tenure	3,527	2.400	2.000	2.110	1,804	2.468	2.083	2.110	1,723	2.330	1.833	2.109	-0.138	(-1.235)	[-3.600***]
5th Position Tenure	2,538	2.204	1.833	2.021	1,342	2.254	1.917	1.950	1,196	2.147	1.542	2.097	-0.107	(-0.838)	[-3.367***]

This table reports the average and median tenures at the first five positions for individuals in my sample. Apart from providing the full sample descriptives, I also provide descriptive statistics for the Rule and non-Rule samples. The last three columns report the differences in means between the Rule and the non-Rule groups. The student t-test is reported in parentheses while the non-parametric Wilcoxon rank-sum test is reported in brackets. In Panel A, the tenure per position for the full sample of individuals and for the Rule and non-Rule samples are reported. In Panel B, tenure is reported for a matched sample of Rule individuals matched based on age and gender to non-Rule individuals.

Table 8: The Rule's Effect on Firm Tenure

	(1) Log_Num_Firms β / SE	(2) Log_Num_Firms β / SE	(3) Log_Num_Firms β / SE	(4) Num_Firms β / SE
Rule	-0.327***	-0.325***	0.013	-0.001
	(0.012)	(0.012)	(0.020)	(0.018)
Male		0.042***	0.031***	0.033***
		(0.012)	(0.012)	(0.010)
Begin Career at Big 4		0.002	-0.022*	-0.017*
		(0.012)	(0.011)	(0.010)
Cohort Fixed Effects	No	No	Yes	Yes
State Fixed Effects	No	No	Yes	Yes
Observations	9,865	9,865	9,865	9,865
Adj. R^2 / Pseudo R^2	0.066	0.067	0.183	0.048

This table analyzes the effects of the Rule on firm matching. I regress the log number of firms an individual has worked on an indicator for an individual being exposed to the Rule, Gender, as well as whether they began their career at a Big 4 public accounting firm. Models 1–3 are OLS regressions while Model 4 reports a negative binomial regression to account for the count nature of the outcome variable. Models 3 and 4 contain cohort and state fixed effects. Significance levels are indicated by: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 9: Average Duration at Each Seniority Position

Panel A: Full Sample

	Full Sample				Non-Rule				Rule				Difference in Means		Non-Parametric
	Obs.	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Deviation	Rule-Non	t-test	Rank-sum test
Low Seniority	4,351	3.856	2.750	4.039	2,320	4.239	3.000	4.677	2031	3..419	2.583	3.101	-0.819***	(-6.885)	[-5.316***]
Medium Seniority	3,621	8.426	6.750	6.874	2,073	10.567	9.167	7.460	1548	5.559	4.375	4.654	-5.008***	(-24.782)	[-22.870***]
High Seniority	1,862	11.819	7.583	12.832	1,420	13.842	9.750	13.700	442	5.320	3.042	5.948	-8.522***	(-18.499)	[-14.916***]

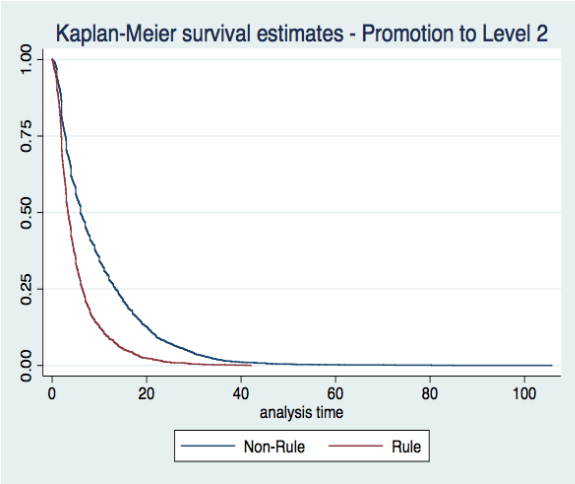
Panel B: Matched Sample

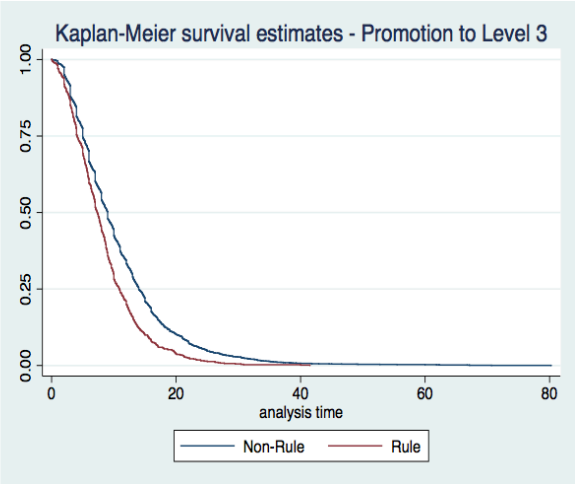
	Full Sample				Non-Rule				Rule				Difference in Means		Non-Parametric
	Obs.	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Dev.	Rule-Non	t-test	Rank-sum test
Low Seniority	3,598	3.444	2.750	2.998	1,799	3.407	2.833	2.857	1,799	3.481	2.667	3.133	0.074	(0.735)	[0.066]
Medium Seniority	2,910	5.647	4.417	4.630	1,455	5.623	4.167	4.651	1,455	5.671	4.583	4.611	0.048	(0.278)	[0.733]
High Seniority	840	5.426	3.000	6.175	420	5.448	2.833	6.354	420	5.404	3.083	5.999	-0.043	(-0.101)	[0.519]

This table provides descriptives on the time till promotion for level-one, level-two, and level-three seniority positions. Apart from providing the sample average, median, and standard deviation for each in the full sample, I also provided descriptives for the Rule and non-Rule sub-samples. The difference in means between the Rule and non-Rule samples are also provided. The significance of the differences are evaluated in the final three columns of each panel parametrically using student t-tests, reported in parentheses, and non-parametrically using the Wilcoxon rank-sum test, reported in brackets. In Panel A, the descriptives are provided for the full sample. Panel B incorporates differences between the subsamples and provides descriptives for a matched sample, where Rule individuals are matched based on age and gender to non-Rule individuals. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Duration Analysis: Promotions

Panel A: Percentage of Individuals yet to be Promoted





This panel displays the two Kaplan–Meier Survival estimates for promotion to level-two and level-three seniority positions. The percentage of the cohort that remains in the initial level-one seniority position is graphed on the y-axis, while the number of years elapsed is graphed on the x-axis. The figure on the left is for promotions from level one to level two while the one on the right is for promotions to level-three seniority positions. The solid blue line plots the estimate for the Rule cohort while the dashed line plots the estimate for their non-Rule counterparts.

Panel B: COX proportional Hazard Model for the Effect of the Rule on Promotions.

	Level-2 Seniority Promotions				Level-3 Seniority Promotions			
Rule	1.856***	1.843***	0.993	0.988	1.436***	1.435***	1.101*	1.100*
	(20.13)	(19.87)	(-0.18)	(-0.24)	(8.86)	(8.84)	(2.03)	(1.99)
Male		0.878***	1.000	1.001		0.993	1.051	1.044
		(-4.43)	(0.02)	(0.04)		(-0.19)	(1.39)	(1.18)
Cohort Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
State Fixed Effects	No	No	No	Yes	No	No	No	Yes
LR Chi2	380.21***	399.61***	1200.48***	1193.26***	73.21***	73.24***	196.91***	192.62***
N	4,963	4,963	4,963	4,963	3,693	3,693	3,693	3,693

This panel reports the Cox proportional hazard model estimates for the effect of the Rule on the time till promotion from level-one seniority positions to level-two and level-three seniority positions. I control for gender in Model 2 and controlling for a cohort effect in Model 3. Finally, Model 4 includes state fixed effects. The coefficients are exponentiated for ease of interpretation and Z statistics are reported in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Duration in Public Accounting

Panel A: Descriptive Statistics of Duration in Public Accounting

	Full Sample				Non-Rule			Rule			Difference in Means		
	Obs.	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**Rule-Non**	**t-test**	**Rank-sum**
Percentage Worked in Public Accounting	8,793	0.99	1.00	0.10	0.99	1.00	0.11	0.99	1.00	0.10	0.00	(0.50)	[0.49]
Number of Positions	8,793	5.28	5.00	2.94	5.86	5.00	3.08	4.26	4.00	2.35	-1.60***	(-27.38)	[-25.83***]
Number of Jobs in Public Accounting	8,793	1.76	1.00	1.09	1.79	1.00	1.14	1.71	1.00	1.00	-0.08****	(-3.57)	[-2.22**]
Percent of Jobs in Public Accounting	8,793	43.23	33.33	29.46	39.40	28.57	28.48	49.92	50.00	29.94	10.52***	(16.14)	[18.16***]
Total Years of Experience	8,793	18.88	16.67	13.07	23.44	21.83	13.35	10.93	9.58	7.67	-12.51***	(-55.82)	[-49.90***]
Total Years in Public Accounting	8,793	7.44	4.67	8.19	8.86	5.58	9.35	4.97	3.58	4.66	-3.89***	(-26.01)	[-22.04***]
Percent of Career in Public Accounting	8,793	45.31	36.70	32.73	41.02	30.77	31.82	52.80	47.33	32.94	11.77***	(16.32)	[16.86***]

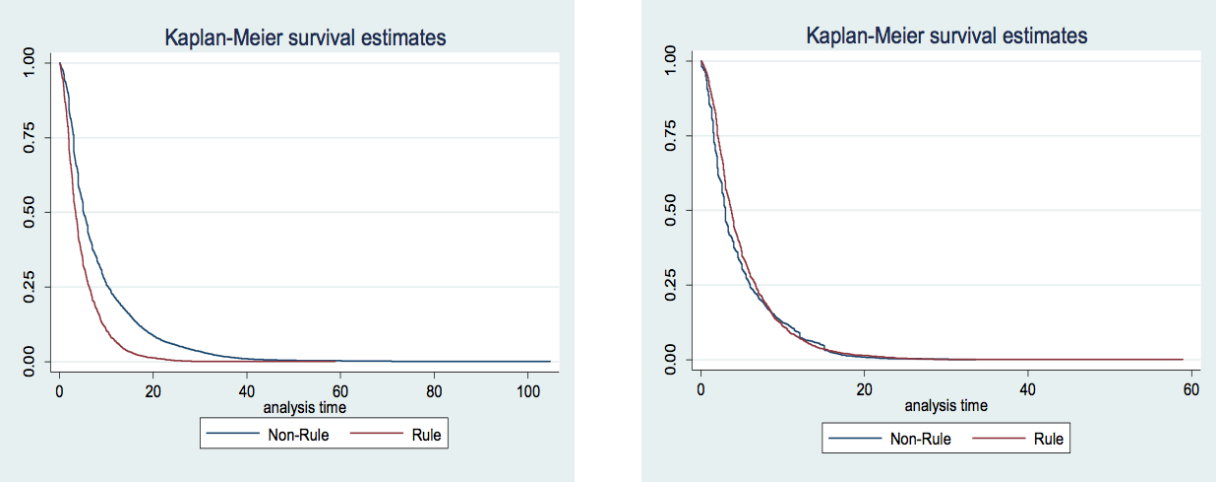
Panel B: Descriptive Statistics of Duration in Public Accounting: Matched Sample

	Full Sample				Non-Rule			Rule			Difference in Means		
	Obs.	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**Rule-Non**	**t-test**	**Rank-sum**
Percentage Worked in Public Accounting	5,818	0.99	1.00	0.08	1.00	1.00	0.06	0.99	1.00	0.10	-0.01	(-1.48)	[-2.54**]
Number of Positions	5,818	4.42	4.00	2.21	4.48	4.00	2.08	4.35	4.00	2.34	-0.13	(-1.32)	[-3.80***]
Number of Jobs in Public Accounting	5,818	1.81	1.00	1.04	1.86	2.00	1.06	1.75	1.00	1.02	-0.11**	(-2.47)	[-3.92***]
Percent of Jobs in Public Accounting	5,818	48.96	50.00	28.71	48.16	50.00	27.43	49.75	44.44	29.91	1.59	(1.28)	[1.06]
Total Years of Experience	5,818	11.35	9.75	7.37	11.30	9.75	7.21	11.39	9.92	7.53	0.08	(0.26)	[-0.04]
Total Years in Public Accounting	5,818	5.35	3.92	4.57	5.44	4.00	4.41	5.26	3.83	4.73	-0.18	(-0.90)	[-4.09***]
Percent of Career in Public Accounting	5,818	53.80	50.79	31.61	54.47	50.98	30.58	53.12	47.89	32.59	-1.36	(-0.99)	[-1.57]

This table reports the length of time spent in public accounting for the full sample of CPAs as well as for non-Rule and Rule cohorts. Panel A reports descriptive statistics for the full sample while Panel B reports descriptive statistics for a matched sample of Rule individuals matched to non-Rule individuals who are of the same gender and age. The mean, median, and standard deviation are reported for the following variables: Percentage worked in Public Accounting, is the percentage of the sample that has worked at least once in a CPA firm; Number of Positions, is the total number of jobs held by an individual as derived from the individual's career histories; Number of Jobs in Public Accounting is the number of positions that the individual has held in public accounting firms; Percentage of Jobs in Public Accounting is the percentage of total positions that an individual has held that are in public accounting firms; Total Years of Experience is the total number of years the individual has worked; Total Years in Public Accounting is the number of years the individual has spent in a public accounting firm; Percentage of Career in Public Accounting is the percentage of the individuals total years of experience that he spent in public accounting. The final three columns of each panel provide differences in means between the groups as well as the significance of the difference using student t-tests and nonparametric Wilcoxon rank-sum tests. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Duration Analysis:Time Spent in Public Accounting

Panel A: Percentage of Individuals Remaining in Public Accounting over Time



This panel displays the two Kaplan -Meier Survival estimates for CPAs in public accounting, the percentage of the cohort that remains in public accounting is graphed on the y-axis while the number of years elaspsed is graphed on the x-axis. The figure on the left is for the full sample while the one on the right is for the matched sample. The sold blue line plots the estimate for the Rule cohort while the dashed line plots the estimate for their non-Rule counterparts.

Panel B: Exit Rates from Public Accounting

	Full Sample						Matched Sample					
Time	Beg Total		Exit		Failure Function-Exit Rate		Beg Total		Exit		Failure Function-Exit Rate	
	Non-Rule	Rule	Non-Rule	Rule	Non-Rule	Rule	Non-Rule	Rule	Non-Rule	Rule	Non-Rule	Rule
1	5,268	2,874	279	349	0.0510	0.1103	2,741	2,694	405	230	0.0604	0.0830
2	4,867	2,376	552	544	0.1520	0.2823	2,363	2,284	600	465	0.2035	0.2386
3	4,213	1,817	720	524	0.2837	0.4480	2,066	1,770	505	506	0.3202	0.4095
5	3,036	1,135	1037	659	0.4734	0.6563	1,219	1,113	451	629	0.5900	0.6294
7	2,296	744	654	370	0.5930	0.7733	768	726	270	343	0.7385	0.7562
10	1,601	380	666	346	0.7148	0.8827	327	373	230	309	0.8888	0.8729
13	1,184	194	395	180	0.7871	0.9396	184	192	168	153	0.9375	0.9344
15	964	126	215	67	0.8264	0.9608	126	125	50	64	0.9575	0.9573
17	756	83	198	42	0.8626	0.9741	66	83	83	32	0.9779	0.9715
20	540	50	218	33	0.9025	0.9845	36	50	26	27	0.9879	0.983
25	324	17	214	33	0.9416	0.9949	19	17	14	26	0.9938	0.9944

Note: This panel displays the exit rates from public accounting for Rule and Non-Rule cohorts. The failure function is calculated over full data and evaluated at the indicated times; it is not calculated from the aggregate totals shown on the Beg Total column. The Time column is measured in years.

Panel C: Cox Proportional Hazard Model for the Effect of the Rule on Exit from Public Accounting

	Full Sample				Matched Sample			
Rule	1.801***	1.798***	1.038	1.147***	1.041	1.041	1.054*	1.531***
	(25.51)	(25.36)	(1.39)	(3.61)	(1.51)	(1.52)	(1.99)	(6.34)
Male		0.981	1.072**	1.073**		1.068*	1.118***	1.129***
		(-0.88)	(3.15)	(3.16)		(2.47)	(4.18)	(4.45)
Cohort Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
State Fixed Effects	No	No	No	Yes	No	No	No	Yes
LR Chi2	617.12***	617.90***	1949.07***	1943.76***	2.29	8.40**	1164.27***	1031.51***
N	8,683	8,683	8,683	8,683	5,777	5,777	5,777	5,777

This panel reports the Cox proportional hazard model estimates for the effect of the Rule on exit rates from public accounting for the full sample and matched sample. I control for gender in Model 2 and for a age/cohort fixed effects in Model 3. Finally, in Model 4 I include state fixed effects. The coefficients are exponentiated for ease of interpretation z statistics are reported in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: The Rule's Effect on Accountants' Earnings

	Model 1	Model 2	Model 3
Age	0.080***	0.080***	0.080***
	(0.006)	(0.006)	(0.006)
Age Squared	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
Male	0.372***	0.369***	0.363***
	(0.025)	(0.025)	(0.025)
White	0.022	0.034	0.066
	(0.033)	(0.033)	(0.036)
Education	0.105***	0.106***	0.103***
	(0.009)	(0.009)	(0.009)
Married	0.172***	0.176***	0.186***
	(0.025)	(0.025)	(0.025)
Rule		0.117***	0.098*
		(0.033)	(0.043)
Constant	6.486***	6.348***	6.236***
	(0.219)	(0.222)	(0.265)
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	No	No	Yes
Adj R-squared	0.161	0.162	0.168
F	22.947	22.795	13.031
N	6,994	6,994	6,994

This table reports the parameter estimates of log-earnings models. In each model the log earnings of accountants are regressed on various determinants of earnings such as age, gender, ethnicity, education and marital status. Model 1 reports the baseline model, while Model 2 introduces the Rule and year fixed effects. Finally, Model 3 reports the full model along with state fixed effects. Standard errors are reported in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 14: Duration Analysis: Time until Partner

	Full Sample				Big 4				Non-Big 4			
Rule	1.350***	1.350***	0.904	1.218	2.038	1.742	1.681	4.057	1.354***	1.354***	0.899	1.162
	(3.32)	(3.32)	(-0.99)	(1.29)	(1.48)	(1.13)	(0.99)	(1.68)	(3.28)	(3.28)	(-1.02)	(0.95)
Male		0.999	1.115	1.107		0.584	0.605	0.513		1.013	1.123	1.119
		(-0.02)	(1.32)	(1.19)		(-1.60)	(-1.30)	(-1.27)		(0.16)	(1.36)	(1.27)
Cohort Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
State Fixed Effects	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
LR Chi2	10.37***	10.37***	87.94***	92.12***	1.85	4.23	4.27	5.60	10.12***	10.15***	85.73***	87.86****
N	803	803	803	803	52	52	52	52	751	751	751	751

This table reports the cox hazard model estimates for the effect of the Rule on the time until partner for the full sample. I control for gender in Model 2 and for cohort fixed effects in Model 3. Finally, in Model 4 I include state fixed effects. The coefficients are exponentiated for ease of interpretation. Z statistics are reported in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 15: Master's vs. Undergraduate Degree Analysis (Matched Sample)

Panel A: Descriptives and Average Tenure per Position for Master's vs. Undergraduate Degree holders (Matched Sample)

	Descriptives					Average Tenure at each Position		
	Total	**Undergrad**	**Master's**	**Diff-Mean**		**Undergrad**	**Master's**	**Diff_Mean**
Num of Jobs	4.976	5.011	4.942	-0.068	**Position 1**	4.460	4.060	-0.401***
	[2.740]	[2.768]	[2.711]	[-0.802]		[4.973]	[4.229]	[-2.787]
Avg Years per Job	3.872	4.017	3.727	-0.290**	**Position 2**	3.695	3.534	-0.161
	[3.838]	[4.059]	[3.598]	[-2.424]		[4.396]	[4.332]	[-1.146]
Big 4	0.646	0.639	0.653	0.014	**Position 3**	3.424	3.152	-0.273**
	[0.478]	[0.481]	[0.476]	[0.944]		[4.197]	[3.454]	[-2.069]
Tax Specialist	0.213	0.211	0.216	0.005	**Position 4**	3.083	2.903	-0.180
	[0.410]	[0.408]	[0.412]	[0.418]		[3.422]	[3.105]	[-1.450]
Grad Year	2000.420	2000.420	2000.420	0.000	**Position 5**	2.860	2.876	0.017
	[10.187]	[10.189]	[10.189]	[0.000]		[3.139]	[3.116]	[0.121]
Num Degrees	1.684	1.420	1.949	0.528***				
	[0.663]	[0.607]	[0.609]	[27.883]				

This panel reports the descriptives of the sample as well as the average and median tenure at the first five positions for individuals with just an undergraduate or master's degree in the sample. The final column reports differences in means. Significance levels are indicated by: * p < 0.10, ** p < 0.05, *** p < 0.01.

Panel B: Cox Proportional Hazard Model for the Effects of a Master's Degree on Promotion (Matched Sample)

	Level-2 Seniority Promotions		**Level-3 Seniority Promotions**	
Master's Degree	1.136**	1.134**	1.185**	1.185**
	(3.02)	(2.97)	(3.21)	(3.20)
Male		1.029		1.010
		(0.66)		(0.18)
Cohort Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
LR Chi2	617.02***	617.45***	155.01***	155.04***
N	2,331	2,331	1,497	1,497

This panel reports the Cox proportional hazard model estimates for the effect of Master's degree on the time until promotion from level-one positions to level-two and level-three seniority positions. I control for gender in the second column. All columns include state and cohort fixed effects. The coefficients are exponentiated for ease of interpretation and z statistics are reported in parentheses. All specifications control for state fixed effects. Significance levels are indicated by: * p < 0.10, ** p < 0.05, *** p < 0.01.

Panel C: Cox Proportional Hazard Model for the effects of the Rule and Master's Degree on time until promotion (Matched Sample)

	Level 2 Seniority Promotions				Level 3 Seniority Promotions			
Masters	1.068	1.074	1.134*	1.139*	1.140*	1.142*	1.174*	1.169*
	(1.14)	(1.24)	(2.17)	(2.21)	(2.10)	(2.13)	(2.57)	(2.45)
Rule	1.782***	1.777***	0.985	1.076	1.432***	1.430***	0.956	1.033
	(9.64)	(9.59)	(-0.23)	(0.83)	(4.43)	(4.41)	(-0.51)	(0.30)
Masters*Rule	1.062	1.059	1.005	0.991	1.018	1.015	1.048	1.043
	(0.73)	(0.68)	(0.06)	(-0.10)	(0.16)	(0.13)	(0.42)	(0.37)
Male		0.928	1.036	1.027		0.920	1.028	1.007
		(-1.77)	(0.82)	(0.62)		(-1.51)	(0.50)	(0.12)
Cohort Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
State Fixed Effects	No	No	No	Yes	No	No	No	Yes
LR Chi2	198.26***	201.37***	613.34***	618.25***	47.88***	50.14***	160.38***	155.55***
N	2331	2331	2331	2331	1497	1497	1497	1497

This table reports the cox proportional hazard model estimates for the effect of the Rule and a Master's degree on the time until promotion for a matched sample of CPAs with master's degrees. I interact the Masters variable with the Rule to obtain the differential effect of Master's degrees with the Rule have on the time til promotions to level-2 and level-3 seniority positions. I control for gender in Model 2 and add Cohort year fixed effects in Model 3. Finally, in Mode 4 I include state fixed effects. The coefficients are exponentiated for ease of interpretation. Z statistics are reported in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A Sample Selection and Screens

	Initial Sample Downloaded Resumes				Sample with Clean Work Experience				Reports Education			
State	**Non-Rule**	**Rule**	**Total**	**%**	**Non-Rule**	**Rule**	**Total**	**%**	**Non-Rule**	**Rule**	**Total**	**%**
CALIFORNIA	2,181	157	2,338	9.22	924	45	969	9.63	777	41	818	9.30
COLORADO	2,227	65	2,292	9.04	879	16	895	8.89	790	15	805	9.16
CONNECTICUT	1,118	1,169	2,287	9.02	530	475	1,005	9.99	469	432	901	10.25
FLORIDA	190	2,074	2,264	8.93	90	754	844	8.39	78	637	715	8.13
GEORGIA	926	1,359	2,285	9.01	380	465	845	8.40	333	411	744	8.46
ILLINOIS	1,146	1,142	2,288	9.03	436	349	785	7.80	385	305	690	7.85
MASSACHUSETTS	1,214	,1089	2,303	9.09	567	409	976	9.70	497	357	854	9.71
MICHIGAN	1,198	1,090	2,289	9.03	598	373	971	9.65	537	344	881	10.02
NEW YORK	1,914	498	2,412	9.52	876	146	1,022	10.16	764	120	884	10.05
PENNSYLVANIA	1,821	492	2,313	9.12	747	119	866	8.60	631	84	715	8.13
TEXAS	887	1391	2,278	8.99	368	518	886	8.80	332	454	786	8.94
Total	14,822	10,526	25,348	100	6,395	3,669	10,064	100	5,593	3,200	8,793	100

This appendix reports the sampling procedure for the sample of CPA resume. It displays how the sample is reduced given the requirements for each individual to report their work experience and educational histories.

B Seniority Position Classification Scheme

Panel A: List of Titles included in Each Seniority Level

Level-1: Low Seniority	%	Level-2: Medium Seniority	%	Level-3: High Seniority	%
Staff Accountant	11.35	Senior Accountant	3.49	Controller	11.95
Associate	3.66	Senior Auditor	2.38	Chief Financial Officer	6.36
Audit Associate	3.27	Manager	2.20	CFO	4.57
Auditor	3.26	Tax Manager	2.02	Assistant Controller	3.13
Accountant	3.17	Senior Associate	1.80	Corporate Controller	2.65
Tax Associate	2.68	Audit Manager	1.79	Partner	2.12
Internal Auditor	2.28	Senior Manager	1.75	President	1.93
Staff Auditor	2.10	Accounting Manager	1.70	Owner	1.67
Assurance Associate	1.85	Consultant	1.56	Vice President	1.48
Administrative Staff	1.61	Audit Senior	1.24	Financial Controller	0.88
Bookkeeper	1.15	Financial Analyst	1.10	VP Finance	0.62
Accounting Assistant	1.02	Director	0.99	Vice President of Finance	0.52
Tax Accountant	1.02	Senior Financial Analyst	0.95	Assistant Corporate Controller	0.49
Audit Staff	0.62	Director of Finance	0.82	CEO	0.47
Accounting Clerk	0.60	Finance Manager	0.76	Managing Partner	0.45

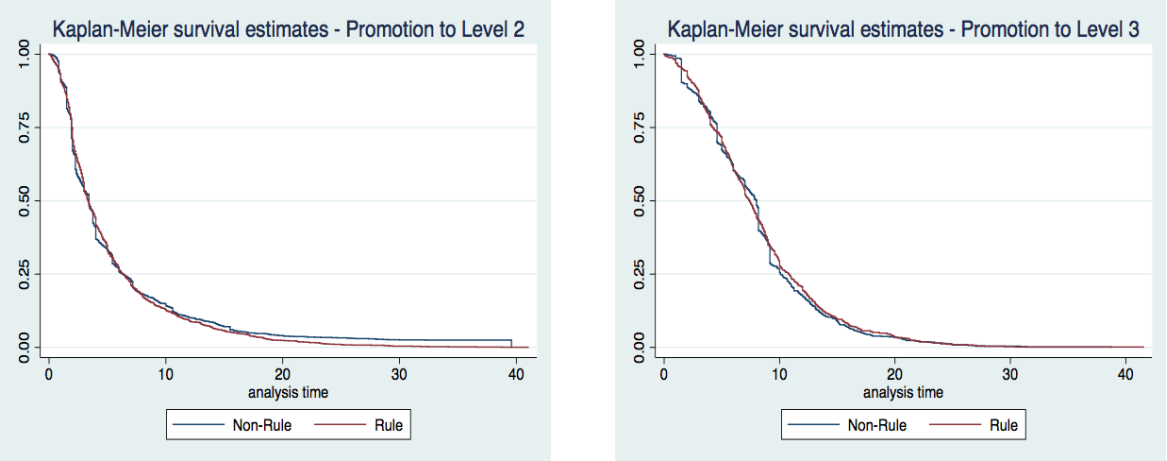
Panel B: Descriptives

Full Sample					
		Obs	Mean	Median	Std. Deviation
Low Seniority	**Time to Promotion**	7,167	3.058	0.833	6.270
	Job Order	7,167	2.252	2.000	1.776
Medium Seniority	**Time to Promotion**	23,493	9.061	6.583	9.347
	Job Order	23,493	3.965	3.000	2.673
High Seniority	**Time to Promotion**	10,995	15.533	13.750	12.117
	Job Order	10,995	5.206	5.000	2.916

This appendix provides examples of job titles that have been classified as belonging to one of the three seniority groups. High seniority positions contain jobs titles that make reference to top-level corporate officers in corporations or partners in public accounting firms. The medium seniority group contains job titles that refer to middle-management positions as well as senior positions at firms. Finally, the low seniority group contains job titles of entry-level positions at the firm. Panel A provides examples of titles that have been classified into each of the seniority levels. Panel B provides descriptive statistics on the average rank order position of titles in individuals careers as well as the average time spent in each level (for the high seniority it is the time individuals take to get to these positions).

C Duration Analysis: Promotions(Matched Sample)

Panel A: Percentage of Individuals yet to be Promoted



This panel displays the two Kaplan–Meier Survival estimates for promotion to level-two and level-three seniority positions. The percentage of the cohort that remains in the initial level-one seniority position is graphed on the y-axis, while the number of years elapsed is graphed on the x-axis. The figure on the left is for promotions from level one to level two while the one on the right is for promotions to level-three seniority positions. The solid blue line plots the estimate for the Rule cohort while the dashed line plots the estimate for their non-Rule counterparts.

Panel B: Cox Proportional Hazard Model for the Effect of the Rule on time till Promotion

	Level-2 Seniority Promotions				Level 3 Seniority Promotions			
Rule	1.051	1.035	1.032	1.034	0.971	0.967	0.947	1.018
	(1.41)	(0.96)	(0.87)	(1.49)	(-0.56)	(-0.63)	(-1.04)	(0.16)
Male		1.173***	1.285***	1.206***		0.805***	0.834***	0.892*
		(4.37)	(6.75)	(4.99)		(-4.02)	(-3.35)	(-2.00)
Cohort Fixed Effects	No	No	No	No	No	No	No	No
State Fixed Effects	No	No	No	Yes	No	No	No	Yes
LR Chi2	1.98	21.25***	321.87***	305.47***	0.31	16.20***	204.60***	155.02***
N	3209	3209	3209	3209	1467	1467	1467	1467

This panel reports the Cox proportional hazard model for the effect of the Rule on the time till promotion from level-one to level-two and level-three seniority positions on a matched sample based on age and gender. I control for gender in Models 2 and 3. Model 4 includes state fixed effects. The coefficients are exponentiated for ease of interpretation and Z statistics are reported in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.