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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 occupational licensing on the quality of Certified Public Accountants (CPAs). I exploit the staggered adoption of the 150-hour rule, which increases the educational requirements for a CPA license. The analysis shows that the rule decreases the number of entrants into the profession, reducing both low- and high-quality candidates. Labor market proxies for quality find no difference between 150-hour rule CPAs and the rest. Moreover, rule CPAs exit public accounting at similar rates and have comparable writing quality to their non-rule counterparts. Overall, these findings are consistent with the theoretical argument that increases in licensing requirements restrict the supply of entrants and do little to improve quality in the labor market.

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A data appendix is available at http://www.nber.org/data-appendix/w29318

1. Introduction

Certified public accountants (CPAs) play a central role in assuring the accuracy and completeness of public companies' financial reports. For this reason, understanding how the audit profession can attract and select high-quality CPA candidates is essential (DeFond and Zhang 2014). Licensing requirements, which typically require some minimum years of education, are a commonly used method for enhancing quality in a profession (Kleiner (2000); Leland (1979)). Thus, the emergence of occupational licensing requirements (e.g., the CPA exam and educational and experience requirements) for auditors is not surprising.[1] This approach, however, has been harshly criticized by Friedman (1962), who viewed licensing standards as a way for professions to restrict entry and extract rents, with little to no improvement in quality. In this study, I use a change in the educational requirement for the licensing of CPAs to examine these two alternative theories.

Historically, the minimum educational requirement for CPA licensure was 120 semester hours of college coursework, usually completed in four years. Approximately four decades ago, the accounting profession began considering a requirement of 150 semester hours (the rule) under the assumption that this change would enhance CPAs training and attract better candidates (Elam (1996)).[2] The first state to mandate the 150-hour requirement was Florida in 1983. By 2016, all 54 U.S. jurisdictions had done so. The staggered nature of the rule's introduction provides a unique opportunity to study the effect of increased educational requirements on the supply of CPAs and individuals' labor market outcomes.

[1] The licensing of CPAs is justified, 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. Licensing is meant to help avoid negative third-party effects that may result from incompetent practitioners.

[2] 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)).

I use an extensive panel dataset of first-time CPA test takers at the university level from the National Association of State Boards of Accountancy (NASBA) to conduct the supply analysis. In a difference-in-differences specification, I find that the rule leads to a 15% reduction in the number of first-time candidates taking the exam. The reduction in supply is consistent with prior studies that have solely examined the supply effects at the state level (Boone and Coe (2002), Raghunandan, Read, and Brown (2003), Jacob and Murray (2006), and Briggs and He (2011)).

However, the post-rule decrease in supply does not necessarily signal a quality improvement. For instance, reducing the number of low-type candidates (those who fail all four sections of the exam in a sitting) would not necessarily increase the overall quality in the labor market. To begin with, low-type individuals would have failed the exam even before the rule and would thus not have entered the market. By contrast, a reduction in the number of high-type candidates (those who pass all four sections in a sitting) would mean fewer high-quality individuals are entering the profession. Indeed, when I examine the compositional change in the types of test-takers, I find that the decrease in test-takers does not come solely from a reduction in the number of low-ability candidates, but also a reduction in the number of high-ability candidates.[3] The fact that the reduction comes from both groups of candidates renders inference on quality that relies solely on pass rates inconclusive.[4]

To overcome the ambiguity of the supply test results, I turn to two sets of individuals' long-run outcomes. First, motivated by the labor literature I focus on two labor market outcomes (time to promotion and tenure at the firm) that are of interest from a policy evaluation perspective. Additionally, these measures flow naturally from the human capital arguments used by those that

[3] The extra year of education appears to also be costly for high-ability candidates, potentially due to their higher opportunity cost of time, which leads to fewer of them taking the exam.

[4] Like every previous study that uses NASBA data, I am not able to observe exam scores but rather whether individuals passed the 70% requirement on each of the four sections of the exam. If I had exam scores, this would help tease out the quality of the passers before and after the rule's implementation.

promoted the rule.[5] Secondly, outcomes tied explicitly to the stated motives for the rule's enactment as argued by advocates (time in public accounting and communication skills). These two sets of outcomes allow for a comprehensive evaluation of the rule's quality effect on the profession.[6]

To empirically assess these two sets of outcomes, I construct a unique and new comprehensive panel dataset of career paths for more than 23,000 CPAs from 11 states who post their résumés on a major professional networking website. The sample spans the past four decades and provides a unique overview of the individual CPAs' employment and educational histories.

The first two labor market outcomes I utilize to assess CPAs' individual quality —time to promotion and tenure at a firm—are well-established proxies for employee quality in the labor economics literature (Topel (1991); Baker, Gibbs and, Holmstrom (1994); Neal (1999); Gibbs and Hendricks (2004); Gibbs, Ierulli, and Milgrom (2002); Devaro and Waldman (2012)).[7] Furthermore, I demonstrate that both measures are positively and significantly correlated with various audit outcomes at the state level. These measures should reflect more closely the granularity of individual ability, as reflected in their labor market outcomes, than the audit outcomes and can thus increase the power in tests to capture the rule's potential effect.

Despite the tests' increased power, the labor market tests fail to find any significant improvement in career outcomes; specifically, there is no significant difference in the time to promotion for rule individuals. Comparing a subset of rule audit partners to nonrule audit partners, I

[5] Proponents of the rule referenced the Commission on Professional Accounting Education (CPAE) report in 1983, which stated that post-baccalaureate education would "attract higher-caliber students ... enhance their professional awareness and contribute to their commitment to the profession," with results including improvement in … staff retention, and staff advancement (CPAE, p. 25).

[6] While it may seem natural to use audit outcomes to directly measure the rule's quality effects, these audit measures capture various attributes related to the firms' operating environments, managerial incentives, and legal liability, all of which may prevent them from capturing changes in the individual auditor's quality and therefore the rule's effect.

[7] An individual accountant's quality captures the factors that make the individual productive both within the firm at his or her specific job and outside the firm. These factors can include education and training, innate ability, motivation, and fit at the firm.

again fail to find any difference in their time to achieving partnership at the firm. Moreover, there is also no significant difference between rule and nonrule individuals' tenure at firms. This result continues to hold when I focus on auditors' tenure at large multinational accounting firms.[8]

The null result above could be driven by the breadth of options through which the educational requirement can be fulfilled (e.g., a master's degree or separate courses). This allows low-ability CPAs to opt for less rigorous nondegree programs, thus rendering the additional education requirement moot.[9] At the same time, the increased time needed to satisfy the requirement may create incentives for high-quality candidates to pursue other careers. Finally, to address the potential concern that data quality issues drive the labor market outcome results in the résumé data, I further validate the measures by examining differences between master's and nonmaster's degree holders, finding that the former are promoted faster. This finding makes it less likely that measurement error in the data is driving the null effect.

Next, I assess the extent to which the rule increases the retention of CPAs in public accounting and increases the oral and written communication skills of new CPAs as advocated by its supporters (Elam (1996)). Using the novel resume data, I find that CPAs subject to the rule are no more likely to exit or stay in public accounting than their nonrule counterparts. Moreover, rule CPAs exit public accounting on a comparable timeline to nonruled CPAs. I then turn to individuals' written communication skills using elements from their resumes. Individuals' online resumes include a job description for each position, which I use to measure individuals' writing abilities. When descriptions are provided, I find that rule individuals' descriptions are slightly shorter and of equal textual

[8] The literature documents that Big N audit firms produce better audits (DeFond and Zhang 2014). They should also provide more desirable employment, at least as long as an auditor stays at an audit firm and does not move to become CFO of a publicly traded firm. Thus, the longer the time spent at these firms by individuals, the better the quality of audits in the market.

[9] Additionally, the potential dilution of curriculums by universities to expand educational programs to five years could have reduced any potential human capital effects from the rule.

complexity to those of the nonrule descriptions. Overall, the tests fail to detect substantial evidence that the rule produces CPAs with better career outcomes, nor does it substantially change CPAs' professional commitment or improve their written communication skills.

This study is subject to several important caveats. The individual quality measures are based on résumé data that may contain measurement error or be biased due to the voluntary nature of the reporting. However, the extensive use of these résumés for networking and job searches assures their integrity. Moreover, the labor market proxies rely on competitive labor markets and standards in firm promotions. Despite the measures' theoretical appeal, the absence of a quality effect in the tests should be interpreted with these caveats in mind. That said, the positive relation between career outcomes and audit outcomes should alleviate some of the concerns that measurement error drives the results. Additionally, the absence of an effect may not signal a lack of an effect, as other potential improvements in the profession may not be reflected in this analysis. For this reason, the results should be viewed as a basis for further examination of the general role of licensing in accounting.

This paper makes several contributions to the literature. First, it comprehensively examines the rule's effect on the accounting labor market. While this study is not the first to examine the rule's effects, prior studies have mainly focused on the number of test-takers and exam pass rate at the state level.[10] Using a stricter difference in difference design at the university level, I extend prior studies by documenting that the reduction in supply does not manifest in solely fewer low-types but also fewer high-types.

A closer study to mine, Clikeman, Schwartz, and Lathan (2001) use a survey to assess the behavioral views of 400 CPA candidates' professional commitment, ethical orientation, and professionalism. While they fail to find that the rule affected students' views, they call on future

[10] These studies include Boone and Coe (2002), Raghunandan, Read, and Brown (2003), Jacob and Murray (2006), and Briggs and He (2011).

work to study the long-term effects. Thus, I substantiate the short-run behavioral expectations with actual long-run outcomes of individuals, using a larger, more general archival sample of CPAs. Furthermore, the evaluation of the rule serves as a starting point for future research to further examine the role of licensing in accounting (the impacts of licensing on various demographic groups, for example).

Second, the paper's novel data and measures illustrate a promising approach for capturing differences in the quality of individual auditors and CPAs. While audit quality is considered a function of both the audit process and personnel (Francis (2011)), data availability has limited the research distinguishing between process and personnel effects.[11] Moreover, studies that examine the role of auditor human capital rely on indirect aggregate measures from the general population (Beck et al. (2017). These aggregate measures, however, make inferences on the role of auditors' human capital difficult, as they could also reflect the human capital of clients' employees (Call et al., (2017)). Thus, the use of CPAs' résumés and the labor market measures of individual career outcomes in the paper offer a path forward to separate the effects of auditor human capital from that of the firm's employees.[12]

Finally, the paper contributes to the broader economics literature on occupational licensing. Occupational licensing affects nearly 30% of the U.S. labor force (Kleiner and Krueger (2010)). Given its prevalence, the cost and benefits of occupational licensing have become the subject of major debate.[13] The rule's supply reductions combined with the lack of a quality effect cast doubt on the benefits of licensing restrictions in U.S. labor markets. The relative merits of increases in

[11] Such studies include: DeAngelo (1981); Ferguson et al. (2003); Carey and Simnett (2006); Francis and Yu 2009; Choi et al. (2010); Ghosh and Moon (2005).

[12] Additionally, the paper's descriptive evidence on career outcomes is compelling, as it should help stimulate future research. For example, the rate of individuals entering the Big-N seems to have changed over time, which prompts questions about how the emergence of alternative career options for CPAs could impact the audit market.

[13] In 2015 the Obama administration budgeted $15 million to study the costs and benefits of occupational licensing on the U.S. labor market (Litan (2015)).

mandatory educational requirements for other highly skilled licensed professions such as law and medicine have also come under criticism.[14] The rule serves as an ideal setting for providing insights on the costs and benefits of changes in mandatory general education in other high-skilled professions.

The paper proceeds as follows. Section 2 provides the rule's institutional background and the economic framework. Section 3 presents the data sources and sample selection procedure and describes the data. Section 4 presents the empirical analysis and results. Section 5 concludes.

2. Institutional Background and Economic Framework

2.1. Licensing of Accountants and the 150-Hour Rule

Occupational licensing specifies the requirements that must be fulfilled to be permitted to perform certain services. It governs more than 1,000 occupations (Brinegar (2006)) or nearly 30% of the U.S. workforce. Over the past several decades, both the number of occupations and the percentage of the workforce covered by licensing have increased dramatically (Kleiner and Krueger (2013)).

In accounting, a CPA license entitles its holder to audit the financial statements of public companies and attest to their compliance with generally accepted accounting principles (Jacob and Murray (2006)).[15] Only CPAs can legally do this. Currently, educational, experiential, ethical, and national-examination requirements, instituted and overseen by state boards of accountancy, must be satisfied for accountants to practice legally as CPAs. While all applicants must pass the national CPA examination, set by the American Institute of Certified Public Accountants (AICPA), the rule

[14] In law, for example, the high cost of law school, relative to the expected benefits, have led to recent calls to reduce the required years of a law degree from three to two years (Estreicher (2012)). The high costs of a three-year law degree have been linked to the lack of underrepresented minorities in that profession (Rodriguez and Estreicher (2013)) In the case of medicine, the scarcity of new medical students and the high cost of their schooling has led to recent calls to reduce medical school from four to three years (Emanuel and Fuchs (2012)).

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

required that applicants complete 30 semester hours of additional education prior to obtaining their license.

The threat of congressional scrutiny regarding new federal regulation on the accounting profession led the AICPA in the mid-1980s to implement reforms in the name of "self-regulation" (Madison and Meonske (1991)).[16] One of the main reforms was to require new 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. While the AICPA required the rule, the state boards of accountancy had to adopt it for it to be legally required for licensure. States like Florida did so as early as 1983. However, most state boards began passing it only after the AICPA's action. In the year 2000 alone, 14 states did so.[17]

Even before the adoption, most jurisdictions specified a minimum number of hours of coursework in business and accounting.[18] Most states did not change these requirements with the adoption of the rule.[19] This freedom was granted 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)).[20] 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-

[16] The savings and loan crisis of the 1980s led 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 soon after receiving clean audit opinions (Berg (1988)).
[17] See Appendix 1 for details on the rule's years of adoption and enactment.
[18] The AICPA pushed for the extra 30 credit hours to be composed of more liberal-arts and general-business courses, rather than pure accounting ones (Collins (1989)).
[19] The rule was worded to provide flexibility to colleges and universities in designing their programs. In this regard, it has been criticized for allowing CPA candidates to be licensed with no more hours in business and accounting than were required previously.
[20] The political economy of the rule can be seen in 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 allow 30 additional hours of higher-level education.

level undergraduate option (a second major), or courses in a nondegree program. As of July 2018, the rule has been enacted in all 54 U.S. licensing jurisdictions, with New Hampshire, California, and Vermont beginning enforcement in 2014 and Colorado in 2015.

2.2. An Economic Framework for Analyzing the Rule

Though occupational licensing covers 30% of the U.S. workforce, its effects are still intensely debated (Kleiner and Krueger (2013)). Traditional theories assert that licensing protects consumers in markets with asymmetric information (Shapiro (1986)). Theoretical work claims that credence goods, such as attestation, demand regulation to protect uninformed consumers (Leland (1979) and Shapiro (1986)). Licensing is thus seen as an administrative means by which regulators (i.e., state boards) control the labor supply. The regulator uses licensing to maintain a minimum level of human capital, which in turn ensures a certain level of quality.[21] The imposition of licensing may, in effect, shift the quality-adjusted demand curve upward, improving consumer welfare and increasing the supply of high-quality services by ensuring the competency of practitioners (Adams III. et al. (2003)).

The rule's requirement for an extra year of education can be viewed as an additional investment by individuals in their human capital that will lead to increases in their competence as auditors (Becker (1962), Becker (1993)). For example, additional courses in writing done to fulfill the rule would potentially increase individuals' communication skills. Additionally, as education may be less costly for high-type individuals, the willingness to undertake the additional 30 semester hours of coursework should also be correlated with high ability and increase the number of high-quality CPAs (Spence (1973)). Overall, these theories predict that the rule will lower the overall supply of CPAs and increase their average quality.

[21] This could either be through increasing the human capital of individuals through educational requirements or screening new entrants to bar those whose skills or character traits suggest low quality (Gittleman and Kleiner (2013)).

In contrast, a significant stream of literature in regulatory economics describes licensing requirements as a means for members of a profession to discourage new entrants and extract monopoly rents irrespective of quality effects (Friedman (1962); Stigler (1971); Maurizi (1974); Shepard (1978); Carroll and Gaston (1981)). These theories predict that the rule's additional 30-credit hour requirement will increase the marginal cost of becoming a CPA, reducing the number of new CPAs while not changing the candidates' average quality. Moreover, the temporal component of the rule—the approximate increase of one year to complete the educational requirement—could lead to adverse selection. High-ability candidates, who have a higher opportunity cost of time, might be motivated to pursue other jobs (Akerlof (1970)).[22] This would, in turn, lead to a decrease in the average quality of CPAs after the rule's implementation.[23] As a result, we should observe reductions in supply with no accompanying increases in quality.[24]

While both licensing theories predict average decreases in supply, the public interest view would suggest that these effects would be accompanied by increased quality (i.e., better career outcomes and higher communication skills). In contrast, Friedman's view would suggest no increase in quality. To determine which of these two views is more likely to be accurate, I examine the rule's effect on the supply of CPAs and quality effects in the accounting market, using individuals' career outcomes.

3. Data

[22] If the rule's main impact is that high-quality candidates must forego a year of paid work and they have higher pay potential, then the rule would actually be costlier for them.

[23] 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 grandfathered CPAs better off and audit clients worse off, as a result of the rule's compositional supply changes.

[24] There are additional theories of the private benefits of licensing that predict that licensing requirements will lead to an increase in rents extracted by those already in the profession. By restricting the supply of CPAs, the rule allows grandfathered CPAs to increase their wages, as they need not make the additional investment (Kleiner (2006); Kleiner and Krueger (2013)). However, this prediction requires access to CPA wages which are currently unavailable.

The empirical analysis requires data on (i) CPA exam outcomes and (ii) CPA education and employment histories. The data on CPA exam outcomes comes from NASBA, while data on individuals' education and employment histories come from self-reported resumes on a leading professional networking website.

3.1. Samples

NASBA: The CPA supply analysis relies on data from NASBA. NASBA provides data on the number of first-time candidates by exam period and jurisdiction and covers 1984–2014. In contrast to previous studies that rely on aggregate state-level data. I compile data on the CPA exam at the university level; NASBA provides data on the total number of first-time test-takers and the number of individuals who pass all four sections in one sitting and the number who fail all four sections in a sitting.

Business Networking Website Data: I use the world's largest online professional networking and recruiting site "LinkedIn" to construct the labor market tests sample. The website began as a networking site for technology and financial industry employees and has grown tremendously since. It covers a wide range of industries and has members at all experience levels, from college students to senior executives. As of 2014, membership includes executives from all of the Fortune 500 companies. Additionally, membership spans all age groups: 46% of members are between the ages of 25 and 44, while 35% are between 45 and 64. The website lists over 656,000 CPAs in the continental United States, which is roughly 60% of the number of individuals in the occupational category of "accounting," as estimated by the Bureau of Labor Statistics (Bureau of Labor Statistics (2018)). The breadth of coverage suggests that the website is a comprehensive data source for information on CPAs.

Computational restrictions on the data collection process, limit the sample to individuals drawn

from 11 prominent states. The states are chosen based on their relative importance regarding the number of accountants, their contribution to the national GDP, and their relative timing in enacting the rule. Appendix 2 provides an overview of the characteristics of the states analyzed.

I begin the dataset construction by searching for members who self-report "CPA" or "Certified Public Accountant" in their profiles.[25] For each state, I draw individuals who entered the labor market (i.e., obtained their CPA) around the rule's enactment. To estimate the appropriate sample size, I perform several power calculations to identify sample sizes required to detect a pre-specified treatment effect (minimum detectable effect) at specified power levels and statistical significance. I consider sample sizes for a specified power of 0.8 and a statistical significance of 0.05. The power test results indicate that if I can obtain 1,200–1,500 individuals in both the treatment and control groups, I can likely detect an effect representing a 10%–25% change from the baseline rate of the control group. The large minimum detectable effect also considers crossover (i.e., the possibility that I misclassify treated rule CPAs as controls) of approximately 15%. Thus, to ensure sufficient power in the tests, I collect an initial sample of 2,500 individuals per state.

Using the selected profiles, I collect workers' information, focusing on the career history of each person. I collect the job title, job description, start and end dates for the job, and company name for each position. The titles and descriptions for a given position allow me to classify jobs based on seniority, to distinguish promotions versus lateral changes. I determine the chronological order of the positions using their arrangement and start dates on the profile page. I also collect data on the user's gender and current location. To ensure data quality, I clean the data by using an individual's unique identifier to remove duplicates that may arise due to the automated collection of the profiles. This procedure allows me to transform résumé data into a panel dataset with an individual's

[25] The website search is neither restricted by geographical proximity nor by the personal connections of the account used to search.

employment history over time.[26]

A common issue with résumé-based data is that some individuals may list more than two positions simultaneously. Therefore, I limit the analysis to individuals who list a maximum of two simultaneous jobs in a year to reduce any problems from unclear résumés. The resulting set of résumés accounts for more than 90% of the original sample. To deal with missing job spells or holes in the résumé, I classify an individual as unemployed if there is a one- or two-year time gap between job (or education) spells.

To estimate an individual's age, I assume that individuals complete their bachelor's degrees at the age of 22. I consider three-year college degrees (typically from international institutions) to be equivalent to bachelor's degrees. Subsequently, I add the number of years passed since the graduation date. In the case of a profile missing the year of undergraduate graduation, I infer an individual's age based on the first year of their first job, also setting the age to 22.[27]

Finally, for a profile to be included in the sample, I require that it contain information on the university attended, degree obtained, graduation year, or a combination of these. These details allow me to distinguish between the following graduate degrees: master's degree, juris doctor (JD), master of business administration (MBA), master of accounting, and doctorate. I also require a complete career history with job titles and dates. Appendix 2 provides a descriptive table on the data requirements and changes in the sample. The resulting sample contains over 17,592 CPAs with data on work experience and 17,085 individuals with complete educational data.[28]

The sample represents an accurate collection of CPAs. Despite efforts to collect, clean, and validate the data, I may not identify all individuals who should be included in the sample. This is

[26] See Appendix 3 for an example of how a profile is transformed into machine readable data.
[27] The use of graduation dates to determine age leads me to indirectly control for an individuals' age when I control for the year the individual entered the labor market, using cohort fixed effects in my specifications.
[28] Changes in the sample of individuals by state given the data requirements is presented in Appendix 4.

due to three different issues: (1) some individuals do not register their résumés on the professional networking website, (2) some may omit or inaccurately list information on their profile (preventing me from accurately pairing them to years, identifying work experience, or capturing their education and training), and (3) I may have made errors in my collection and parsing of the profiles. Inferences based on résumé data in general face these concerns, given the voluntary nature of the profiles. Nevertheless, the pervasive use of the website by individuals for both credible networking and job searching is indicative of the data's integrity. Moreover, unlike false claims on a résumé, which only a prospective employer can see and cannot easily verify, the public nature of one's profile and its accompanying public accountability discourages individuals from lying about their employment. This distinguishes my setting from traditional résumé studies. Nevertheless, the inferences should be interpreted with the above caveats in mind.

3.2. Descriptive Statistics

3.2.1. NASBA Sample

Descriptive statistics for the NASBA sample are reported in Table 1. The table reports the mean and median number of first-time CPA candidates at the university level from 1984–2004. The table also includes the number of candidates passing all (Pass All) and failing all (Pass None) four sections of the CPA exam and their percentages in brackets.[29] Descriptive statistics are provided for the rule and nonrule subsamples. The average number of candidates per sitting is 20 at the university level. Out of these, 3.5 individuals or 16% of test-takers, on average, pass all four sections of the exam, and 10.9 individuals or 56% of test-takers, on average, fail all four.

[29] I am forced to use the Pass All and Pass None as proxies for candidate type as the NASBA data is at the university level. The data contains the number of individuals at the university that passed all four sections, some of the four sections, and none of the four sections. The Pass All measure is also an inadequate measure of quality, as it only reflects obtaining more than 70% on a section. I do not have the actual exam scores. Moreover, I am not given any demographics or individual information about the test takers. All these limitations further motivate the use of labor-market outcome proxies and sample.

Comparing the rule and nonrule subsamples indicates a decrease in the number of candidates sitting for the exam. The average number of test-takers drops from 21 in the nonruled period to 15 in the rule period (a 30% decrease). While the decline is reflected in the Pass None number, which drops from 11, on average, to seven (a 36% decrease), it is also evident in the Pass All number, which drops from 3.75 to 2.87 individuals, on average (a 23% decrease).

Finally, the percentage of Pass All increases in post-rule periods (going from 16% to 17%). This increase comes from the large decrease in the Pass None, which is contained in the denominator (total test-takers), as the number of high-types who take the exam also declines in post-rule periods. The decline in the number of Pass All candidates seems pertinent to quality, as it signals that high-types also found the rule costly. I formally analyze this drop when I examine the treatment effect of the rule on the supply of CPAs below.

3.2.2. Professional Networking CPA Sample

Table 2 Panel A provides descriptive statistics for the CPA sample, consisting of 17,592 CPAs; these statistics show demographics, career outcomes, and educational background. Sixty percent of the sample is male. On average, individuals have 5.3 jobs during their careers, averaging four years per job. I find that 38% of them are employed at a Big four public accounting firm at some point; 18% have worked in taxation. Concerning time in public accounting, 55% of the sample have gone to work outside of public accounting, having spent, on average, seven years in public accounting. The mean graduation year is 1997. More than 47% of the sample have master's degrees, with 23% being in accounting.

In addition to sample averages, Panel A also reports differences in the means between the nonrule and rule subsamples. A direct comparison of rule to nonrule individuals presents a challenge, given the vast differences in graduation year between the two subsamples (with rule individuals

graduating on average 14 years later than their nonrule counterparts). Thus, the differences between the two groups comingle differences in age, business environments, and any possible effect of the rule. To address this issue, I implement coarsened exact matching (CEM).

In Panel B, I use CEM to match individuals based on gender and the year they enter the labor market.[30] The CEM algorithm creates a set of strata with the same coarsened values of the continuous matching variable (year entered the market). The number of rule CPAs decreases in the panel as CEM restricts the matched data to areas of common empirical support by trimming unmatched observations from both the treated and control samples. As a result of the matching procedure, the differences in means between the two groups' graduation years are eliminated.[31] Moreover, Panel B indicates that differences between the matched rule individuals to control individuals are no longer significant; the two groups have no significant differences in the number of jobs held. At the same time, rule individuals continue to have significantly more degrees than nonruled individuals.

By matching on the year of labor market entry, I control for both the effects of the economic conditions that prevail when individuals enter the labor force and an individual's age, given that their age is a function of their graduation year.[32] As a result of the difference documented above and the benefits of the matched sample, I use the matched sample for most of the labor market tests. The matching is implemented by including the CEM weights in the multivariable regressions when using

[30] In essence, this enables me to compare, say, a male in Miami (rule individual), who began his first job in 2000, with a male in Atlanta (nonrule), who also started in that year. An individual's age is being indirectly controlled by using the year of graduation (market entry) to match individuals.

[31] The CEM procedure returns matching weights that balance the distribution of the matching variables in the treated and control samples. I use these weights when comparing the means of the two groups in the descriptive table and when I run the matched sample analysis.

[32] Note that the rule mechanically forces an individual to delay market entry for one year (the additional 30-credit requirement). As a result, the comparison is to examine an individual who enters the market with an extra year of schooling versus those who enter the market without the extra year. The additional year and extra age should be attributable to the rule and considered part of the rule's effect.

the matched sample. Moreover, I use cohort fixed effects in the analysis to further control for the year an individual enters the labor market.[33]

4. Results

The results discussion is split into four sections. First, using the NASBA sample, I estimate the impact of the rule on the supply of CPAs focusing on its impact on the supply of low- and high-quality candidates. Second, using the resume sample, I assess the quality effects of the rule on individual CPA's general career outcomes. Third, I evaluate the rule's effect on professional commitment and individuals' written communication skills. Finally, I provide a validation test of the networking sample data.

4.1. The Rule's Effect on Supply

I measure the rule's supply effect using a difference-in-differences framework on the NASBA sample of first-time test takers. The analysis is at the university level and covers the period of 1984–2004. The staggered adoption of the rule allows me to construct nonrule counterfactuals by using variation from the time series (i.e., before and after the rule) and the cross-section of states (i.e., states that have yet to adopt the rule in a given year). I focus on the total number of test-takers, the number of test-takers who pass all four sections in one sitting, and the number of test-takers who fail all four sections in a sitting, using the following fixed-effect specification.

$$Y_{u,m,v} = \beta_1\left(Rule_{u,m,y}\right) + \beta_2\left(Year\ Before\ Rule_{u,m,y}\right) + \beta_3\left(May\ Sitting_{u,m,y}\right) + Year\ FE + University\ FE + University\ FE * Year + c_{it}, \quad (1)$$

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 takes the value of one when the observation is for year y

[33] This allows for more precision in estimating the effects of the rule on the labor market outcomes by using only within cohort variation.

and zero otherwise. The year fixed effects control macroeconomic factors and shocks that may affect all universities in a given year. $University\ FE$ is a vector of university identifiers that takes the value of one when the observation is from university u and zero otherwise. The university fixed effects are used to control for shocks in educational quality at the university level. The university-level data reduces aggregation noise in state level studies, thereby increasing the power of the test. $University\ FE * Year$ is a university-specific linear time trend that allows each university to have its own linear time trend. $Year\ Before\ Rule$ is an indicator variable set to one the year before the rule is implemented and it captures any surge in supply immediately preceding the rule's enactment. $May\ Sitting_{u,m,y}$ is an indicator variable set to one for sittings of the exam in May. Finally, $Rule_{u,m,y}$ is a binary indicator variable that takes the value of one in jurisdiction years in which the rule is in effect and zero otherwise. Thus, β_1 provides the marginal change in the number of test-takers driven by the rule.

Table 3 reports the results for the three different specifications. Model 1 indicates that the rule reduces the number of test-takers by roughly 15% after controlling for year, university fixed effects, and university-specific time trends. Consistent with anticipation of the rule's implementation, the year before the rule takes effect is associated with a 21% increase in the number of test-takers. The May Sitting identifier controls for the fact that fewer candidates take the exam in May. In Figure 1, Panel A, I plot the rule indicator in event time to show the dynamics of the treatment effect of the rule on the supply. Consistent with the regression results, states implementing the rule see a persistent reduction in the number of test-takers for up to three years after the rule's implementation. The documented reduction in test takes is consistent with prior studies which examined the supply effects of the rule using cross-sectional variation and aggregate state measures of the number of candidates sitting for the exam (Boone and Coe (2002); Raghunandan, Read, and Brown (2003); Allen and

Woodland (2006); Jacob and Murray (2006); Briggs and He (2011). These reductions, however, do not provide direct evidence of the rule's quality effects, given the limitations in the data and their focus on average pass rates.

While a reduction in the overall number of test-takers has been used as evidence of an increase in quality, understanding the type of candidates that are not sitting for the exam after enacting the rule is necessary to determine the potential quality effects. For example, decreases in the number of low types sitting for the exam need not translate into increases in quality. Even in the absence of the rule, all these individuals would have failed the exam and not entered the CPA labor market. On the other hand, decreases in the number of high types would suggest a deterioration in quality, as these individuals would have become CPAs, absent the rule. This view motivates my focus on reductions in the number of candidates by type.

Models 2 and 3 in Table 3 examine which part of the distribution of candidate quality the rule's supply reduction comes from. Model 2 examines the number of high types: candidates who pass all sections in one sitting (Pass All). Model 2 indicates that the rule leads to a 10% reduction in the number of high-type test takers. This reduction is inconsistent with the theories suggesting no change (or an increase) in the number of high types following implementation. The additional year of education appears costly to high-ability candidates, potentially due to their higher opportunity cost of time, leading to fewer of them taking the exam. Model 3 also indicates a 14% reduction in the number of low types (Pass None). This effect is not significantly different from the one in Model 2, as an F-test fails to find a significant difference between the two coefficients. Model 3 estimates also confirm an increase in the number of low-type candidates taking the exam in the year before the rule and a reduction in the number of candidates in May sittings.

The documented results on high and low types are further illustrated in event time in Figure 1 Panel B.[34] Graphically, one observes a similar decrease in the number of both high and low type candidates after the rule's enactment over time. The reduction in the number of low types is not necessarily related to an increase in the quality of CPAs in the labor market, as these individuals would have failed the exam, even absent the rule, and would not have entered the CPA labor market.[35] The reductions in *both* high- and low-type candidates and the fact that candidates failing the exam cannot enter the market in the first place necessitates an examination of additional individual outcomes, such as actual labor market outcomes, to determine the rule's quality effects.

4.2. The Rule's Effect on Career Outcomes

In this section, I focus on the rule's effect on measures of individual quality using labor market outcomes. Specifically, I use an individual's time to promotion and tenure at a firm as proxies for individual quality. Accounting individuals' quality is defined as the set of factors that make an individual productive both within the firm at their specific job and outside the firm. These factors include education, training, motivation, and innate ability. The unobservable nature and high dimensionality of this construct make it challenging to quantify. My approach to measuring accountant quality is instead based on the long-term labor market outcomes of individuals. Specifically, if the rule had a meaningful impact on individual quality, this effect should eventually materialize in the career success of rule individuals, as compared to their nonrule peers—including how long it takes for them to receive promotions and their tenure/retention at a given firm. [36]

[34] In Appendix 5, I provide an alternative graphical representation of changes in the trend of high and low candidates around the rule's adoption using a regression event time approach. The inferences remain the same.

[35] It is this very fact that makes dividing the number of test takers that passed by the total a misleading measure of quality. The reduction in this percentage is driven by both changes in the numerator and the denominator, where changes in the number of low types (reductions) may make it appear we have a higher percentage of high types in the market, when there were fewer than before.

[36] One way to measure accountant quality would be to gain access to time-series data on the internal performance evaluations of CPAs at audit firms. These assessments contain, for example, the supervisors' assessments of an individual's quality, such as assessments of productivity, professionalism, motivation, etc. Such proxies, however, are

The labor market proxies of individual quality are motivated by empirical and theoretical literature in labor economics (Topel (1991); Baker, Gibbs and, Holmstrom (1994); Gibbs, Ierulli, and Milgrom (2002)). Moreover, proponents of the rule implied that these measures would be affected as a result of the rule (CPAE (1983)). There are two primary reasons why these proxies are likely to capture the quality of individuals in the accounting profession.

First, promotions provide key incentives (Lazear (1992), McCue (1996)) and help screen for ability (Medoff and Abraham (1980); Medoff and Abraham (1981); Bernhardt and Scoones (1993); Gibbs (2008)), especially in hierarchical white-collar firms. Accounting firms are known for requiring young professionals to work long hours and for instituting up-or-out promotion systems.[37] The purpose of these arrangements is to identify capable, diligent professionals who will likely become partners in the future (Barlevy and Neal (2018)).[38] In fact, variation in the time to promotion has been shown to relate to individual ability and quality.[39] Specifically, Baker, Gibbs, and Holmstrom (1994) document that fast-track promotions identify individuals of higher ability who are more likely to have successful careers.[40]

A concern with using the time to promotion as a proxy for individual quality is that it may fail to capture variation in quality in the presence of a fixed promotion timeline.[41] While this concern would be pertinent in some organizations (e.g., government or state-sponsored organizations) or if

rarely available from accounting firms. Moreover, they are not without limitations, as supervisors' may not observe all the relevant dimensions (e.g., ability) and their ratings are subject to behavioral biases.

[37] Barlevy and Neal (2018) model the longstanding differences between the labor market for professional services and other markets for well-educated workers.

[38] Firms can identify more professionals who can function effectively as partners when they require new associates to perform more tasks. And when they replace experienced associates with new workers, they gain opportunities to identify talented professionals who will have long careers as partners.

[39] If the purpose of a promotion is to sort employees by ability, then the best performance measure for promotion decisions would be the one that is most correlated with ability, rather than effort. My reliance on time to promotion is based on changes in timing reflecting changes in the makeup of individual ability.

[40] This finding has been replicated in various firms, industries, and countries (Chan (1996); Seltzer and Merrett (2000); Treble, et. al. (2001); Gibbs and Hendricks (2004); DeVaro and Waldman (2012)).

[41] Descriptive evidence in the data shows that there is wide heterogeneity in time to promotion for similar positions, even within the same firm.

one were to examine only within-firm promotions, this concern is unlikely to be a first-order issue in my setting. First, in my setting, there is an active labor market for talent. If talented individuals are not being promoted, they can find lucrative opportunities elsewhere. Second, I define time to promotion with respect to both within- and between-firm promotions. As a result, the proxy will capture an individual's quality, even if a given firm has a fixed timeline for promotion, as it will be reflected in outside opportunities. Nevertheless, I acknowledge that the proxies are not perfect and maybe correlated with other factors, such as monitoring, risk, and the accuracy of the supervisor's assessment. I take care in the empirical specification to control for these other determinants of labor market outcomes when analyzing the rule's effect on promotion speed.

Second, employees spend a significant fraction of their careers with the same employer (Doeringer and Piore (1971); Gibbs et al. (2002)). Most job search models feature employees searching for the right match with an employer, and this match determines tenure and the likelihood of promotion (Doeringer and Piore (1971); Topel (1991); Topel and Ward (1994); Neal (1999)). Consistent with the theoretical arguments, empirical studies find that increases in human capital improve match quality and lower employee turnover.[42] Additionally, from an informational perspective, firms tend to have better information about their worker's quality than the external labor market, since they observe their work directly and in detail over time. Moreover, firms have an incentive to strategically use this private information advantage, including retaining those it observes to be higher quality and vice versa for those with lower quality. This interaction leads to a positive association between quality and tenure (Gibbons and Walden (1999)). Finally, lower turnover

[42] For example, Acemoglu and Pischke (1999) argue that an increase in human capital improves firm productivity and leads to the acquisition of firm-specific skills and longer tenure.

(longer tenure at the firm) at firms can lead to ex-post higher productivity and quality by lowering the adjustment costs related to hiring and separation for the firm.[43]

A potential limitation of tenure at the firm as a measure of quality is that the CPA labor market is distinct from the general labor market because it contains employee moves between audit firms and private clients. This could imply that individuals who are unable to obtain more interesting jobs outside the firm would stay. Thus, longer tenure at the firm could reflect lower quality. To assess this possibility, in an untabulated analysis, I show that there is no relation between a short tenure at the firm and the likelihood of obtaining a higher seniority position in the private sector after leaving public accounting. This analysis assuages concerns as to this potential view. However, the firm tenure measure results should be interpreted with the prior caveat in mind.

4.2.1. Time to Promotion Analysis

I begin by performing a duration analysis to more closely look at the time elapsed before promotion. I use the CPA profiles, which each display start and end dates for each position, and calculate the time spent at each.[44] I then classify these positions by seniority by matching all job titles (based on similarity scores) to the titles in the Department of Labor's seniority/prestige classifications (as well as several online job search engines). The seniority levels capture variation in responsibility (and wage rates) for the jobs in my sample.[45] This classification allows me to distinguish between promotions and lateral job changes.[46] Thus, I can identify promotions using the profiles even if an individual takes a more senior position at a different firm. These external

[43] To complement the theoretical motivation for using labor market outcomes as proxies for individuals' quality, I empirically correlate the two measures with several proxies of audit quality in Appendix 6. The overall positive association between the labor-market and audit-quality measures suggests that the labor market measures capture some attributes of individual CPA quality and audit quality.

[44] 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.

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

[46] Sample titles and descriptions of classified positions are provided in Appendix 7.

promotions could occur if a firm has only a fixed number of open slots and an oversupply of qualified individuals. In this case, an active labor market will allow individuals to move to other firms.[47]

To examine differences in time to promotion, I use a Cox proportional hazard model. The Cox model is a semi-parametric method for analyzing the effects of different covariates on the hazard function (Cox (1972) and Wooldridge (2010)).[48] Specifically, I estimate the following model.

$$Number\ of\ Years_i = \beta_1 Rule_i + \beta_2 Male_i + Cohort\ FE + State\ FE, \qquad (2)$$

where *Number of Years*$_i$ is the number of years until individual *i* is promoted, *Rule*$_i$ is an indicator of the individual being subject to the rule, *Male*$_i$ 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 *State_FE* are state fixed effects to capture state economic conditions.[49]

I begin to explore the rule's effect on time to promotion in Figure 2, which plots the Kaplan-Meier survival estimates for rule and nonrule cohorts with respect to promotions. I provide graphs for both the full sample (Panels A and C) and the matched sample (Panels B and D). Promotions to level two jobs are displayed in Panels A and B, while promotions to level three are in Panels C and D. The y-axis gives the percentage of the cohorts yet to be promoted while the x-axis traces the number of years. When comparing the rule (dashed line) to the nonrule cohort in the full sample, the rule-cohort appears to be promoted to level two and three jobs in a shorter time span. In contrast, when employing the matched sample, the two groups exhibit a similar pattern in promotion timing. Thus, it appears that, while over time, the time to promotion has decreased (as shown in the full

[47] Thus, active labor markets allow promotion speed to vary, even if a firm may have a fixed promotion schedule and allow the proxy to reflect the quality of these individuals.
[48] Appendix 8 examines mean differences for matched sample of rule and nonrule individuals along the lines of average tenure at a position and time to promotion.
[49] A reported hazard rate of one would indicate no difference and a zero effect.

sample results), once I control for gender and the point in time when individuals first entered the labor market, the rule does not incrementally increase the speed of promotion.

In Table 4, I use regressions to examine differences between rule and nonrule individuals in the average time to promotion to each of the seniority levels. The analysis is performed on a sample of rule individuals matched to nonrule individuals on gender and the year they entered the labor market. In Panel A, I estimate a Cox model on the time to promotion on the matched sample. This approach allows me to control for time effects and measure and isolate the difference between the two groups more accurately. The results for level-two seniority positions are displayed in Column 1, while the level-three seniority results are in Column 2. I include state fixed effects in each specification to capture economic conditions in the state and control for time effects and age using cohort fixed effects. In each specification, the hazard rates (slope coefficients) for the rule in both promotion levels (Columns 1 and 2) are close to one and are statistically insignificant.[50] This implies that the rule did not affect time to promotion.

While the overall analysis of time to promotion shows neither a statistical nor economic difference between rule and nonrule individuals, it can be argued that this may be driven by noise in the seniority classification scheme used in the networking website sample. To assuage this concern, in Columns 3 to 5, I re-estimate the Cox model on a sample of individuals who become public accounting partners. This subsample allows for a cleaner test of promotion outcomes. The career seniority is more comparable for the two groups (they both consist of individuals that eventually become partners in their firms) and the titles are standardized within accounting firms. Moreover, it allows me to focus on individuals specifically related to public accounting.

[50] A descriptive analysis of the estimate's confidence intervals and point estimates indicates that at most the rule cohort would be promoted at most 95 days for level 2 seniority positions. This descriptive analysis is consistent with the recommendations of Cready et. al. (2019).

The analysis again finds no significant difference in the time to partner between the two groups. Additionally, I re-estimate the model after partitioning the sample into Big N and non-Big N in Columns 4 and 5.[51] Consistent with the main results, I find no effect of the rule on time to partner in either of the samples. Overall, time to promotion and time to partner is not significantly different between rule and nonrule individuals.

4.2.2. Tenure at the Firm

Next, I examine the rule's effect on firm-employee match quality. To do this, I regress firm tenure and the number of firms an individual has worked in over his or her career on the rule indicator and several determinants of firm match quality. To isolate the rule's effect, I control for the individual's gender and whether they began their career at a Big N firm. *Began Career at Big N* is an indicator that captures differences in careers that an initial Big N placement could cause.[52] I control for age and the total number of years an individual was employed through cohort fixed effects.[53] Cohort fixed effects are set to one in the year an individual entered the job market (i.e., the year that an individual begins his or her first full-time job after college). I control for variation in state economic characteristics by using state fixed effects.

$$Outcome = \beta_1 Rule_i + \beta_2 Male_i + \beta_2 Began\ Career\ at\ BigN_i + Cohort\ FE + State\ FE + c_i. \quad (3)$$

I estimate the above model as both ordinary least squares and as a negative binomial regression. The negative binomial regression accounts for the count nature when the outcome measure is the

[51] This subsample partition of partners is done to further ease concerns that rigidity and differences in business models in Big N partnerships would reduce the ability of time to promotion to capture quality differences and lead to a null result.

[52] I add *Began Career at Big N* since descriptive statistics show a general trend in accounting toward starting one's career at a Big N firm, and I want to untangle that effect from the rule's effect.

[53] When I control for the year the individual entered the labor market (cohort fixed effects), I am technically also controlling for individuals' ages, as these are a function of their graduation year. (I assume an age of 22, as this is a typical age for college graduation).

Number of Firms. If the rule influenced mobility between firms, we should expect to see a significant coefficient β_1.

Table 4, Panel B, reports the results of the firm-tenure tests. Column 1 indicates that the rule had no significant incremental effect on the average firm tenure. Column 2 shows that the same result holds when the log of the number of firms is used as an outcome variable. Column 3 reports the negative binomial regression results and confirms that the rule does not incrementally explain tenure at the firm. Descriptively, males tend on average to have longer tenures at firms and work in 4% fewer firms over their careers. Individuals starting their careers at a Big N firm tend to work in 25% fewer firms over their careers, all else constant.

I then focus on auditors, examining the time spent at Big N firms by individuals in the sample in columns (4) and (5). The literature documents that Big N firms produce higher quality audits (DeFond and Zhang 2014). They should also provide more desirable employment, at least as long as an auditor stays at an audit firm and does not move to become CFO of a publicly-traded firm. Moreover, the efficiencies of lower turnover should help maintain better quality audits in the market due to less time being spent training new hirers given high turnovers. In column (4), I run a Cox hazard model on the tenure at a Big N firm, while in column (5), I estimate an OLS with the log tenure at a Big N as my outcome variable. Again, I fail to find an economic or statistical difference in the rule individual's tenure at a Big N firm. Model 5 allows me to state that tenure at the Big N did not increase more than 0.002% for rule individuals as a result of the rule with 95% confidence. Overall, these results, along with the time to promotion results, suggest that the rule failed to substantially change the career outcomes of individuals entering the accounting profession.

4.3. Professional Commitment and Writing Skills

The labor market outcomes examined in the prior section are of first-order importance when

considering the effects of licensing on quality in a profession. However, proponents for the rule asserted that it was also meant to increase CPAs' professional commitment and develop their communication skills. Thus, in this section, I focus on two outcomes that are linked to the motivation for the rule's passage: CPAs' commitment to public accounting and improvements in CPAs' communication skills.

4.3.1. Time in Public Accounting

CPAs commitment to public accounting served as a prominent issue when the rule was being contemplated by the AICPA (Cook (1996)). Specifically, many advocates of the rule claimed it would help retain CPAs and stem the exit to private industry by screening in more dedicated CPAs (Anderson, 1988). However, the extra year of business education could also result in more CPAs leaving the accounting profession to more general business career paths, as the rule requirements would also expose them to more general business courses. Thus, a natural test of these claims would be to examine the exit rates from public accounting for the rule-cohort and their nonrule counterparts.

To empirically examine individual's time in public accounting, I use detailed career histories to measure if and when individuals exit public accounting, and the length of time individuals spend in public accounting. To construct the measures, I use 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. Figure 4 graphs the percent of individuals remaining in public accounting over time for the two groups using Kaplan-Meier survival curves. Panel A is estimated using the full sample of CPAs, while Panel B utilizes the matched sample. While the rule cohort seems to exit public accounting earlier in their careers unconditionally, once I control for the year they entered the market using the matched sample, there is no discernable difference in the exit rates. The lack of a difference in timing is further supported in Figure 5, where I graph the percentage

difference in the likelihood of exiting public accounting for rule cohorts relative to nonrule cohorts around the adoption of the rule by each state. Again, even individuals three years after adopting the rule exhibit no difference in exit likelihood relative to matched nonrule individuals.

In Table 5, I formally examine the relation between the rule and individual's time in public accounting using linear probability and hazard models like those used in the previous section. In Panel A, I provide the estimates of the rule's effect on individuals' likelihood of exiting public accounting (the extensive margin) in column one and the time to exit (intensive margin) in columns 2 and 3.[54] The estimates from the linear probability models show no significant effect on the exit likelihood from public accounting. Examining the economic significance of the estimated effect, the -0.02 rule coefficient in column 1 implies that the probability of exiting public accounting for the rule group is 2% lower relative to the nonrule group. As the nonrule group's probability of exiting is 0.47, the 95% confidence interval on the coefficient indicates rule group's likelihood of exiting ranges from a reduction of 0.03% to an increase of 0.01%. The tight bounds imply a negligible economic effect.

Turning to the intensive margin and time to exit in columns 2-3, I again fail to find any significant difference in time to exit, either using a hazard model (column 2) or an OLS with log time in public accounting as the outcome (column 3). In addition to a lack of statistical difference, the economic significance of the estimates is also negligible. For example, the coefficient in the Cox regression is 0.99, which is close to 1 and insignificant.[55] The coefficient on the rule of 0.04 from the OLS in column 3 translates into a potential estimated effect of the rule on time in public accounting of anywhere between 71 days less in public accounting to 217 days more in public accounting for

[54] Each specification is estimated on the matched sample and includes controls for the gender of the individuals as well as Cohort (year of graduation) and state fixed effects.
[55] The lower bound of CI is 0.89 and the upper bound of CI is 1.09, which is again close to 1.

rule individuals. Overall, the evidence implies that the rule had negligible effects on individuals' time in public accounting.

Despite the lack of an effect on individuals time in public accounting, it is plausible that the general education needed to meet the rule's requirements could have led to an exodus from public accounting into higher seniority general business positions. In Panel B, I examine this potential effect by determining whether, post rule, the positions taken by those that exited public accounting were higher seniority using linear probability models. Each specification includes controls for gender and the seniority of the last position before leaving public accounting. I vary cohort and state fixed effects in each of the columns. Each specification fails to document a statistical difference in the likelihood of obtaining a higher seniority position in the private sector for rule individuals. To put the null effect into perspective, the -0.04 coefficient in column 2 implies the probability of having a position of higher seniority post public accounting for the rule group would economically decrease by 4% relative to the nonrule group or a change in probability between -0.01 to 0.001% for the rule group. The tight and small bounds imply a negligible effect.[56]

4.3.2. Written Communication Skills

The final set of tests focus on the rule's effect on CPAs' communication skills as many proponents for the rule argued that it would increase the communication skills of accountants (Bandy (1990), Becker (1993)). Moreover, from a human capital perspective, the additional courses in writing would potentially increase individuals' communication skills (Becker (1962)).[57]

To empirically test the writing skills of individuals, I use the job descriptions provided on their profiles to obtain quantitative measures of writing and communication skills. I focus on the

[56] This is calculated by comparing the probability of having a position of higher seniority post public accounting for the nonrule group of 0.11 and using the confidence interval of the coefficient to calculate the potential interval of the impact.
[57] The additional investment by individuals in their human capital can be seen as leading to an increase in competence.

length and readability of individuals' job descriptions.[58] To examine readability, I utilize two of the most cited measures in the literature, the Fog Index, and the Flesch Reading Ease Score (Loughran and McDonald 2014). The Fog Index is defined as the linear combination of average sentence length and the proportion of complex words (words with more than two syllables). The Flesch Reading Ease Score uses the same components as Fog, except that it uses an explicit count of syllables (Loughran and McDonald 2016). The Fog Index and Flesch Reading Ease Scores are scaled combinations of two factors that produce a numeric grade level estimate.

Figure 6 demonstrates the rule's effect on writing skills. It plots the percentage change in writing length and the complexity of individuals' writing for treated cohorts relative to non-treated cohorts.[59] Panel A plots the percentage change in the number of words per sentence in individuals' job descriptions. Panel B graphs the percentage change in the number of sentences in the job descriptions. Panel C plots the percentage change in the Fog-index of the job descriptions, and Panel D plots the percentage change in the Flesch reading ease score. All four panels show relatively little difference in the communication skills between rule and nonrule cohorts. In fact, the descriptive results in Panels C and D suggest that the rule had the opposite of the intended effect: rule individuals' writing has become somewhat more challenging to understand.

I formally investigate the rule's effect in Table 6. In Panel A, I provide the estimates of the rule's effect on individuals' likelihood of posting a description for at least one disclosed position (the extensive margin). I estimate the rule's effect via the following linear regression:

$$Outcome = \beta_1 Rule_i + \beta_2 Male_i + \beta_2 Masters_i + \beta_2 Began\ Career\ at\ BigN_i + Cohort\ FE + State\ FE + c_i. \quad (4)$$

[58] Descriptive statistics for the various written communication proxies used are provided in Appendix Table 9.
[59] The treated cohorts are defined relative to the year in which the rule is implemented. The estimates are obtained by regressing the writing proxies on indicators for the cohort relative to the initial rule cohort and includes state fixed effects and year of graduation fixed effects.

where the outcome is the log number of words per sentence (column 1), log number of sentences (column 2), the Fog-index (column 3), and the Flesch Score (4). The variable of interest rule is an indicator for an individual subject to the rule. *Male* is an indicator variable that is one if the individual is a male and zero otherwise. *Master* is an indicator for an individual having a master's degree. *Total Job* is a count of the number of positions that the individuals have held in their career, as reported in their profile. Each specification includes state and cohort (year of graduation) fixed effects.

The estimates from the models show no statistical difference in the number of words or in the number of sentences used to describe rule individuals' job descriptions. To gauge the economic magnitude of the estimates, consider the rule's coefficient of 0.01 in column 1. The estimated bound on this coefficient translates into rule individuals' descriptions having anywhere between 1.8 to at most two extra words per sentence than nonruled individuals.[60] Regarding the number of sentences used, the coefficient of -0.05 in column 2 translates into an effect bound anywhere between a third of a sentence less to at most half a sentence more.

Finally, in columns three and four, I estimate models examining individuals' writing complexity. There are no statistical differences in either the Fog-Index or the Flesch Score in rule individual's descriptions. Economically, these estimates are also very close to zero. For example, the upper bound on the rule's estimated effect in column 3 implies at most a four-percentage point increase in the fog-index of rule relative to nonrule individuals, less than a half a point increase in the index (i.e., less than half a year of education).[61] Overall, the rule seems to have negligible effects on the written communication skills of individuals in the profession.

[60] I use the two-standard error confidence interval of the coefficient to obtain the potential bound of the effect. I then multiply this time the mean of words per sentence for the nonrule group (34.07). This gives a lower bound of -1.86 words per sentence and an upper bound of 2.54 more words per sentence.

[61] In Appendix 10 I implement a supplemental analysis using various measures of writing tone of the individuals' job descriptions, and I again fail to find any significant difference between rule and nonruled individuals' writing.

4.4. Robustness Test: Master's versus Nonmaster's Degrees

The benefits of a master's degree are well documented in the literature in labor economics (Mincer (1958); Arrow (1973); Spence (1973); Card (1999); Dupray (2001)).[62] I examine differences between master's degree CPAs and nonmaster's CPAs to pick up variations in time to promotion using the sample of CPAs obtained from the website. The extent of noise in the resume data can be identified through analyzing master's degree holders; the presence of noise would lead to a null result, driven by a lack of power. Stated simply, if the résumé data contains noise or the time to promotion is a lousy proxy for quality, I would not expect to find a difference between these individuals.

Table 7 re-estimates the previous tests on CPAs with master's degree holders versus without the degree, matching individuals in the two groups by year of graduation and gender. In Panel A, I find that individuals with master's degrees are more likely to be employed at Big N firms and specialize in taxation. Additionally, they spend less time at each position, have more jobs, and are promoted more quickly. The promotion results are consistent with prior work on the value of a master's degree.

In Table 7 Panel B, I re-estimate the Cox hazard model on the masters' sample and find that degree holders tend to be promoted faster. In Columns 3 and 6, I examine whether the rule affected the promotion speed for these degree holders. They are not significantly better off after the rule, measured by decreases in time to promotion. These findings help alleviate noise issues in the résumé data and further confirm the ability of the proxy (time to promotion) to capture expected differences in individual quality.

5. Conclusion

[62] 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); Ellwood et al. (2000)).

While all U.S. jurisdictions now require the equivalent of an extra year of education for CPAs, there is little evidence for the long-run effects of this policy. In this paper, I empirically test the long-run effects on the accounting labor market of requiring an extra year of general business education for accountants. Consistent with prior studies, I find a 15% reduction in the number of first-time candidates taking the CPA exam following the rule's enactment. However, unlike prior studies, I find that the reduction is for both low-ability and high-ability candidates, raising questions about the rule's overall quality impact.

When I compare the labor market outcomes of rule individuals to a matched sample of individuals who are not subject to the rule, there is no economic or statistical difference in outcomes. Rule individuals have similar promotion timing, writing quality and spend a similar amount of time in public accounting. Overall, these results question the effectiveness of incremental licensing requirements as tools to attract better candidates to the accounting and auditing profession.

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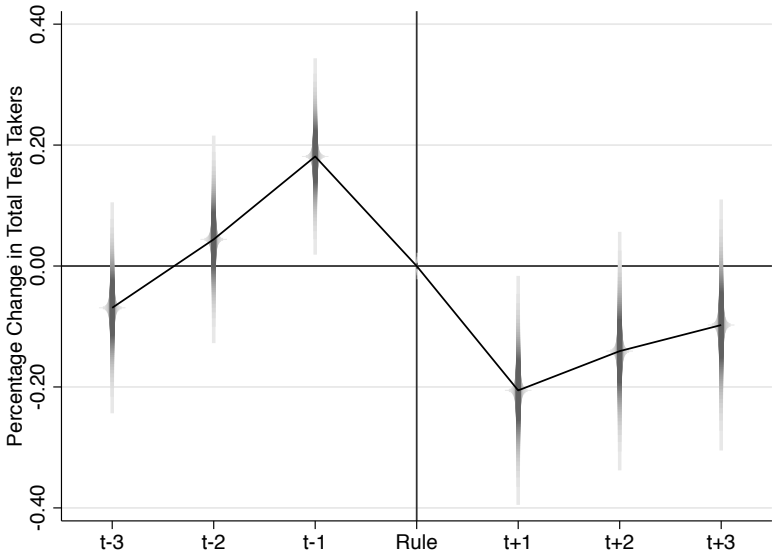
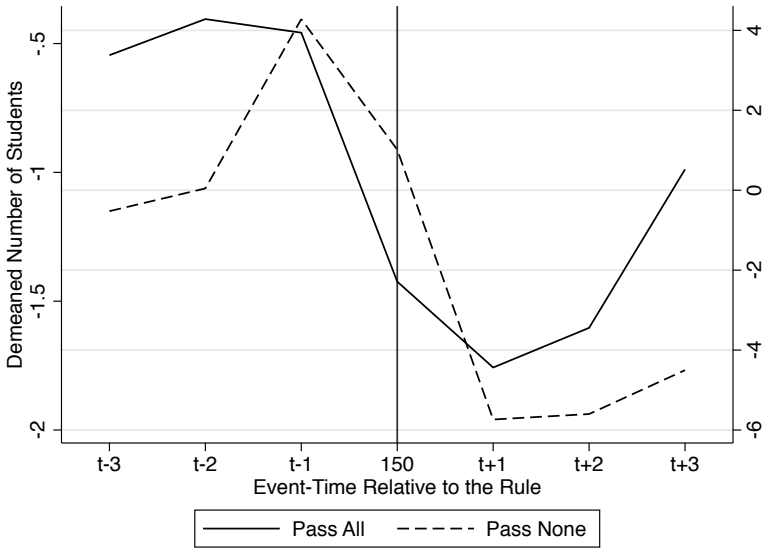
Figure 1: Rule's Effect on the Supply of CPAs



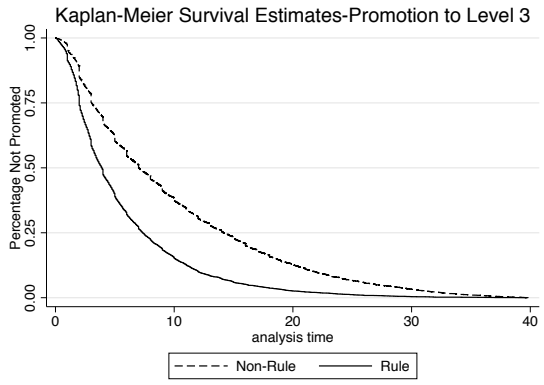
(a) Rule Supply Effects in Event Time



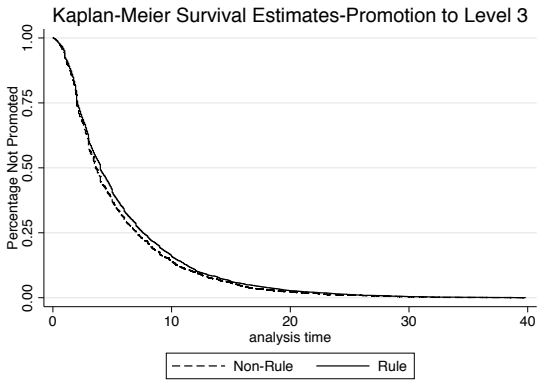
(b) Num of Pass All and Pass None in Event Time

The figure above graphs the trend of the supply of CPAs over the event-time relative to the Rule. The sample consists of observations at the university level from test sittings from the years 1984 to 2004. Panel (a) graphs the estimated impact on the number of candidates taking the CPA exam for years before, during, and after the implementation of the Rule. The estimates are obtained by regressing the log number of test-takers on five-year lags and leads of the Rule year, with state and year fixed effects as well as unique state-linear trends. They are plotted with 95-99% confidence intervals. Panel (b) graphs the demeaned number of candidates taking the CPA exam in event-time relative to the Rule year. Passed All is the number of first-time test takers who passed all four sections of the exam in a sitting by each university. Passed None is the number of first-time test takers who fail all four parts of the exam in a sitting by each university. The demeaned number of students is obtained by 1) demeaning the university observations by the average number of candidates in the sample period for each university and then summing the demeaned number for each university by year for each of the groups (pass all or pass none) for all the universities over event time.

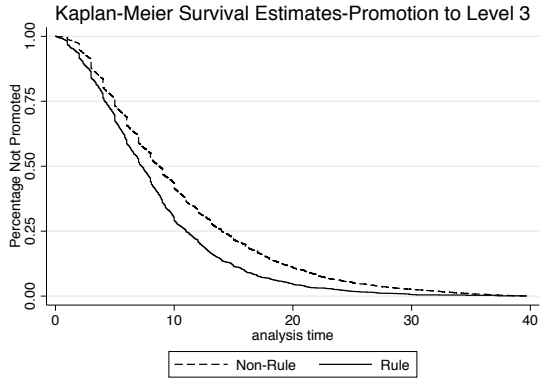
Figure 2: Percentage of Individuals Yet to Be Promoted



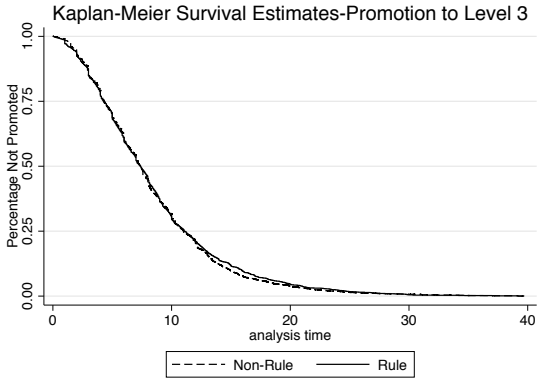
(a) Level 2 (Full Sample)



(b) Level 2 (Matched Sample)



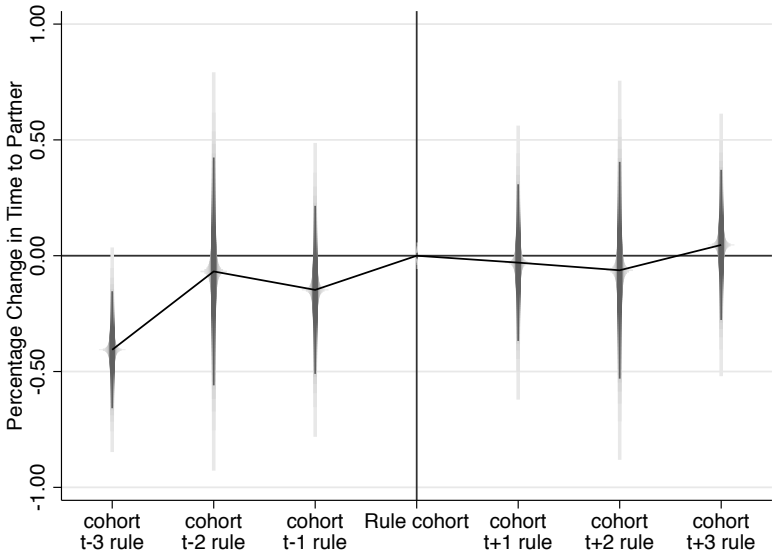
(c) Level 3 (Full Sample)



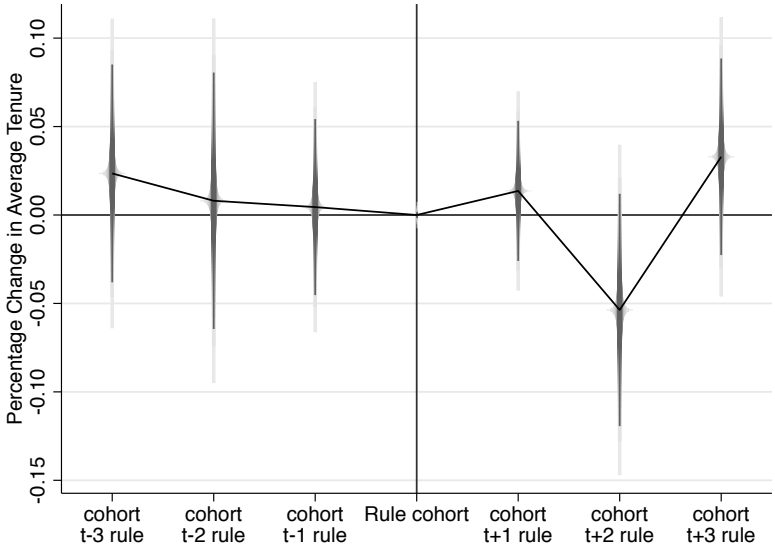
(d) Level 3 (Matched Sample)

This figure displays the Kaplan-Meier Survival estimates for promotion to level two (panels a and b) and three (panels c and d) positions for both the raw full sample (panels a and c) and a matched sample (panels b and d). The samples consist of observations of CPAs from the networking website. The matched sample entered is matched by gender, and the year they enter the job market. The percentage of the cohort that remains in the initial seniority level is graphed on the y-axis while the years passed since entered the position is graphed on the x-axis. The red dashed line graphs the estimate for the Rule cohort while the blue line graphs the estimate for their non-Rule counterparts. CEM weights are used in the matched sample.

Figure 3: Rule's Effect on the Time to Partner and Firm Tenure - Event Graphs



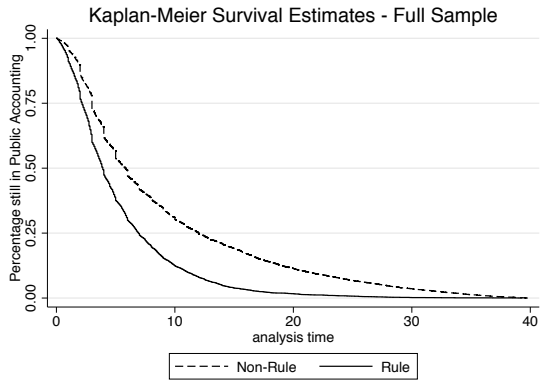
(a) Time to Partner Over Event Time



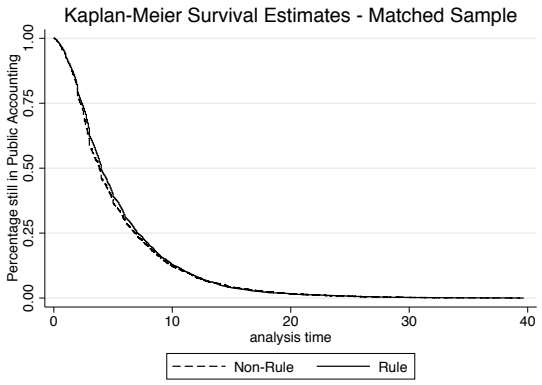
(b) Average Firm Tenure Over Event Time

The figure examines the effects of the rule on individuals' average time to partner and their overall time at each firm. The sample consists of matched observations of CPAs from the networking website. The matched sample is matched by gender, and the year they entered the job market. Panel (a) graphs the percentage difference in average the average time to partner for treated cohorts relative to non-treated cohorts. The treated cohorts are defined relative to the year in which the rule is implemented. The estimates are obtained by regressing the log time to partner on event-time indicators, with state and cohort fixed effects. Panel (b) graphs the percentage difference in the average tenure at the firm. The estimates are obtained by regressing the log time of the individual's tenure at a particular firm on event-time treated cohort indicators, with state and cohort fixed effects. The treated cohorts indicators are defined relative to the year in which the rule is implemented. In both panels, the vertical red line signals the impact of the rule on the first-year cohort. The standard errors, in both panels, are estimated by clustering at the state level. All regressions are done using CEM weights to implement the matching.

Figure 4: Percentage of Individuals Remaining in Public Accounting Over Time



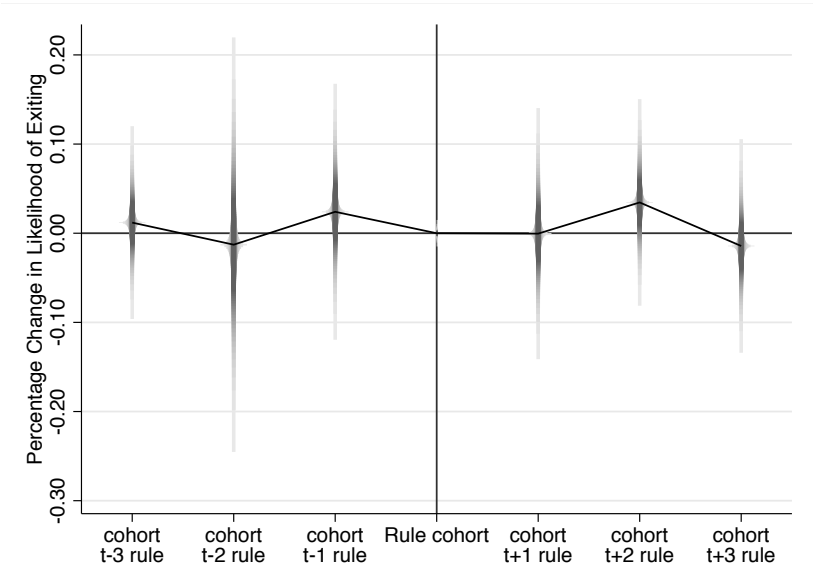
(a) Full Sample



(b) Matched Sample

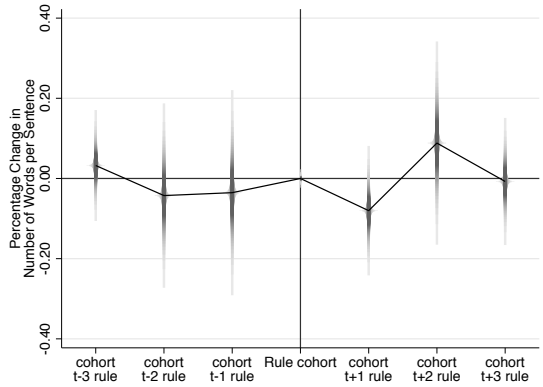
This panel displays the two Kaplan -Meier Survival estimates for CPAs staying in public accounting for both the full sample (panel a) and a matched sample (panel b). The full sample consists of observations of CPAs from the networking website. The matched sample is matched by gender, and the year they entered the job market. The percentage of the cohort that remains in public accounting is graphed on the y-axis while the years past since the start of an individual's career in public accounting is graphed on the x-axis. The dashed red line plots the estimate for the Rule cohort while the blue line plots the estimate for their non-Rule counterparts. CEM weights are used in the matched sample.

Figure 5: Rule's effect on the Likelihood of Exiting Public Accounting Over Event Time

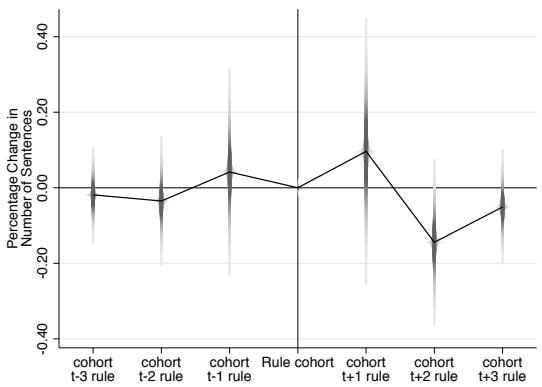


The figure graphs the percentage difference in the likelihood of exiting public accounting for treated cohorts relative to nontreated cohorts. The sample consists of observations of CPAs from the networking website. The treated cohorts are defined relative to the year in which the rule is implemented. The estimates are obtained by regressing an indicator for an individual exiting public accounting on the event-time dummies, with state and cohort fixed effects. The regression is weighted by CEM weights. The standard errors in both panels are estimated by clustering at the state level.

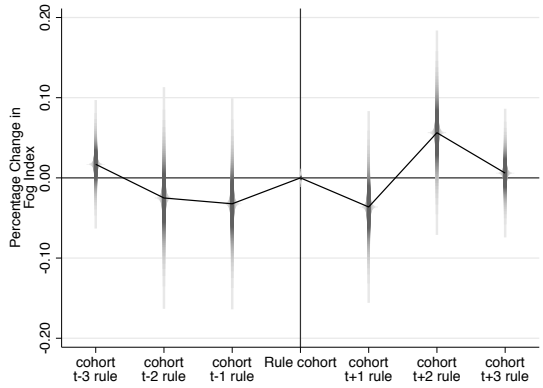
Figure 6: Rule's Effect on CPAs Writing Skills - Event Graphs



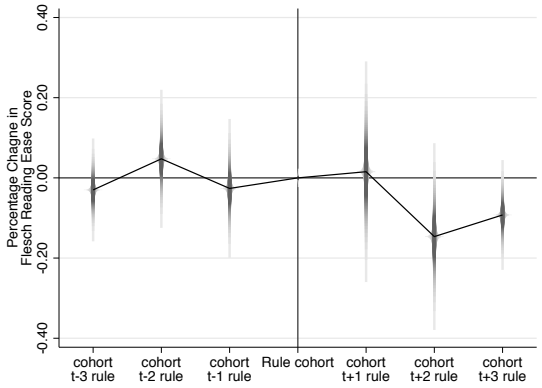
(a) Number of Words per Sentence



(b) Number of Sentences



(c) Fog Index



(d) Reading Ease Score

This figure examines the effects of the rule on the writing skills of CPAs. The analysis focuses on the percentage change in writing length and complexity for treated cohorts relative to non-treated cohorts. The treated cohorts are defined relative to the year in which the rule is implemented. The sample consists of job descriptions of all the CPAs from the networking website. Panel (a) plots the percentage change in the number of words per sentence in an individual's job descriptions. Panel (b) graphs the percentage change in the number of sentences in the job descriptions. Panel (c) plots the percentage change in the Fog index of the job descriptions and panel (d) plots the percentage change in the Flesch reading ease score. Definitions for the Fog index and Flesch score are found in the variable description appendix. All estimates are obtained by regressing the log outcome on the event-time cohort dummies, with state and cohort fixed effects. Standard errors are clustered by state.

Table 1: University-Level CPA Exam Descriptive Statistics

	Full Sample Mean / Median	Non-Rule Mean / Median	Rule Mean / Median
Graduate Degree	0.18 (0.00)	0.15 (0.00)	0.30 (0.00)
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)
Perc All	0.16 (0.14)	0.16 (0.13)	0.17 (0.14)
Passed None	10.97 (7.00)	11.77 (7.00)	7.79 (5.00)
Perc None	0.56 (0.57)	0.57 (0.57)	0.54 (0.54)
Observations	18875	15095	3780

This table presents the descriptive statistics on the number of candidates taking the CPA exam. The sample consists of observations at university level from test sittings from the years 1984 to 2004. Number of Candidates is the number of first-time test takers in the specific sitting from the university. Passed All is the number of first-time test takers who passed all four sections of the exam in a sitting. Passed None is the number of first-time test takers who fail all four sections of the exam in a sitting. The observations have been split between the pre- and post-periods of states implementing the Rule. The average percentages for Passed All and None are reported in brackets. Observations from states without observations in either the pre-period or post-period have been omitted from the descriptive table.

Table 2: CPA Sample: Career and Education Descriptives

Panel A: Full Sample

	Full Sample				Non-Rule				Rule				Diff in Means
	Obs.	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Dev.	Rule-Non
Male	17592	0.60	1.00	0.49	11013	0.62	1.00	0.49	6579	0.56	1.00	0.50	-0.056***
Nubmer of Positions	17592	5.29	5.00	2.69	11013	5.73	5.00	2.82	6579	4.54	4.00	2.26	-1.190***
Average Years per Job	17592	4.37	3.25	3.73	11013	5.18	3.83	4.17	6579	3.03	2.42	2.26	-2.155***
Big N	17592	0.38	0.00	0.49	11013	0.38	0.00	0.49	6579	0.37	0.00	0.48	-0.011
Tax	17592	0.18	0.00	0.39	11013	0.17	0.00	0.38	6579	0.20	0.00	0.40	0.023***
Year Graduated	17592	1996.89	1997	11.03	11013	1991.56	1992	9.54	6579	2005.81	2007	6.76	14.248***
Number of Degress	17592	1.63	2.00	0.77	11013	1.59	1.00	0.77	6579	1.69	2.00	0.75	0.100***
Master's Degree	17592	0.47	0.00	0.50	11013	0.41	0.00	0.49	6579	0.56	1.00	0.50	0.147***
Accounting Master's	17592	0.23	0.00	0.42	11013	0.16	0.00	0.36	6579	0.36	0.00	0.48	0.199***
Non-Accounting Master's	17592	0.24	0.00	0.43	11013	0.26	0.00	0.44	6579	0.20	0.00	0.40	-0.052***
Exit Pub ACC	17592	0.55	1.00	0.50	11013	0.60	1.00	0.49	6579	0.47	0.00	0.50	-0.124***
Time to Exit Pub ACC	9657	7.02	4.50	7.04	6555	8.04	5.00	7.78	3102	4.88	3.58	4.43	-3.165***
Higher Seniority Post ACC	17592	0.14	0.00	0.35	11013	0.16	0.00	0.36	6579	0.11	0.00	0.32	-0.042***

Panel B: Rule Matched Sample Based on Age & Gender

	Full Sample				Non-Rule				Rule				Diff in Means
	Obs.	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Dev.	Rule-Non
Male	15209	0.58	1.00	0.49	8917	0.56	1.00	0.50	6292	0.56	1.00	0.50	-0.003
Nubmer of Positions	15209	5.19	5.00	2.57	8917	4.59	4.00	2.27	6292	4.54	4.00	2.24	-0.053
Average Years per Job	15209	3.94	3.08	3.08	8917	2.91	2.37	2.16	6292	3.11	2.48	2.24	0.201
Big N	15209	0.38	0.00	0.49	8917	0.39	0.00	0.49	6292	0.38	0.00	0.48	-0.011
Tax	15209	0.18	0.00	0.39	8917	0.20	0.00	0.40	6292	0.19	0.00	0.40	-0.010
Year Graduated	15209	1999.13	1999	8.94	8917	2005.65	2007	6.68	6292	2005.54	2006	6.69	-0.117
Number of Degress	15209	1.62	2.00	0.76	8917	1.61	2.00	0.75	6292	1.69	2.00	0.75	0.073*
Master's Degree	15209	0.48	0.00	0.50	8917	0.49	0.00	0.50	6292	0.56	1.00	0.50	0.069
Accounting Master's	15209	0.24	0.00	0.43	8917	0.29	0.00	0.45	6292	0.35	0.00	0.48	0.066
Non-Accounting Master's	15209	0.24	0.00	0.43	8917	0.21	0.00	0.40	6292	0.21	0.00	0.41	0.003
Exit Pub ACC	15209	0.54	1.00	0.50	8917	0.51	1.00	0.50	6292	0.47	0.00	0.50	-0.033
Time to Exit Pub ACC	8274	6.31	4.25	5.79	5294	4.82	3.42	4.31	2980	4.98	3.75	4.34	0.161
Higher Seniority Post ACC	15209	0.14	0.00	0.34	8917	0.13	0.00	0.33	6292	0.11	0.00	0.32	-0.016

This table presents the 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. 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 the year they enter the labor market and gender. The sample average for Rule and Non-Rule individuals in the matched sample are weighted by CEM weights. The variables reported are the following: Male is an indicator variable set to one if the individual is male and zero otherwise; Number of Positions is a count of the number of positions that the individuals have held in their career, as reported in their profile; Years per Job is the length of time spent at each position for each individual; Big N 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's Degree is an indicator variable set to one if the individual reports any postgraduate degree; Non-Accounting Masters is an indicator variable set to one if the individual reports a non-accounting-specific master's such as an MBA; Accounting Masters is an indicator variable set to one if the individual reports an accounting specific master's such as MACC or MST. Exit Pub ACC is the number of unique firms an individual has worked in during their career. Time to Exit Pub ACC is the number of years till an individual exits public accounting. Higher Seniority Post ACC is an indicator set to one if an individual gets a higher seniority position after exiting 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. Significance levels are indicated by: * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 3: The Rule's Effect on the Supply of CPAs

	(1) Log Total Cand	(2) Pass All	(3) Pass None
Post Rule	-0.15**	-0.11**	-0.15**
	(-2.21)	(-2.48)	(-2.21)
Year Before Rule	0.28***	0.01	0.28***
	(5.78)	(0.21)	(5.78)
May Sitting	-0.05**	-0.09***	-0.05**
	(-2.16)	(-3.55)	(-2.16)
Observations	25,333	25,768	25,333
Adjusted R^2	0.285	0.543	0.285
Year Fixed Effects	Yes	Yes	Yes
University Fixed Effects	Yes	Yes	Yes
University Specific Time Trend	Yes	Yes	Yes

The sample consists of observations of first-time test takers at the university level for test sittings from the years 1984 to 2004. The dependent variables are: the log number of candidates (Column 1), the log number of test takers passing all four sections of the exam (Column 2) and the log number of test takers failing all four sections of the exam (Column 3). The variable of interest Rule, is an indicator variable set to one for state years in which the Rule is in effect and zero otherwise. Year Before Adoption, controls for any run-up in the exam and is an indicator variable equal to one in the year before the Rule is implemented 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 includes year and university fixed effects to control for unobservable invariant variation within years and universities and include university specific time trends. An F-test is conducted on the statistical difference between the coefficients on Rule in the Pass All and Pass None specifications. Significance levels are indicated by: * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 4: The Rule's Effect on Career Outcomes

Panel A: The Effect of the Rule on Time to Promotion

	Time until Promotion		Time to Partner		
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Level 2	Level 3	Full Sample	Big N	Non-Big N
Rule	0.93	1.05	1.02	3.08	0.97
	(-1.59)	(0.73)	(0.06)	(0.62)	(-0.11)
Male	1.17***	1.21***	1.22	0.92	1.22
	(7.39)	(5.91)	(1.50)	(-0.06)	(1.42)
Observations	9533	4,532	396	43	653
Sample	Matched	Matched	Matched	Matched	Matched
Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
LR Chi2	1028	333.5	97.24	0.665	95.40

Panel B: The Rule's Effect on Firm Tenure

	Firm Tenure			Tenure at Big N	
	(1)	(2)	(3)	(4)	(5)
	Log Avg Tenure	Log Num Firms	Num Firms	Time in Big N	Log Time Big N
Rule	-0.02	0.03	0.02	0.91	0.01
	(-1.07)	(1.58)	(0.64)	(-1.50)	(0.16)
Male	-0.02	-0.04**	-0.04	1.01	-0.00
	(-1.14)	(-2.08)	(-1.22)	(0.43)	(-0.15)
Bign_First	0.07***	-0.25***	-0.21***		
	(4.48)	(-13.16)	(-6.26)		
Observations	15,206	15,206	15,206	5,847	5,847
Sample	Matched	Matched	Matched	Matched	Matched
Cohort FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.231	0.153	NA	NA	0.079
LR Chi2	NA	NA	NA	386.2	NA

The sample consists of observations of CPAs from the networking website. Panel A examines time to promotion. The panel reports the estimates from Cox proportional hazard models on the effect of the rule on time to promotion in Columns 1 - 2 and the time to partner in Columns 3 - 5. The variable of interest Rule is an indicator for an individual subject to the rule. Male, is an indicator variable that is one if the individual is a male and zero otherwise. The dependent variables are: the number of years to promotion to level-2 seniority (Column 1) and the number of years to promotion to level-3 seniority positions (Column 2). A promotion is defined as a move to a job title with greater seniority either within the firm or at another firm. The dependent variable becomes the number of year to promotion to partner for an individual in Columns 3 - 5. The time to partner model is estimated separately on Big N and non-Big N partner samples in Columns 4 and 5. In Panel B, the effects of the rule on firm matching is analyzed. Column (1) to (3) include the full observations of firm tenure's, and Column (4) to (5) include the sub-sample of the individuals' at Big N tenure. The dependent variables are: Log Avg Tenure (Column 1) which is an individual's log average tenure at the various firms they have worked in, Log Firms (Column 2) which is the log number of firms, and Num Firms (Column 3) which is the number of unique firms an individual has worked in during their career. The variable of interest is Rule which is an indicator for an individual being subject to the rule. Gender, is an indicator variable that is one if the individual is a male and zero otherwise. Big N First is an indicator variable that is one for individuals whose first employment is at a Big N public accounting firm and zero otherwise. Each model includes state fixed effects to control for time-invariant state economic effects. Finally, cohort fixed effects are used to control for the year individuals entered the market. Columns 1 and 2 are estimated using OLS, Column 3 is estimated as a negative binomial regression to take into account the count nature of the dependent variable Number of Firms, Column 4 is estimated as a Cox proportional hazard regression, and Column 5 is estimated using OLS. In the case of Cox Hazard models, the coefficients are exponentiated for ease of interpretation, and Z statistics are reported in parentheses. In the case of OLS models, I report the coefficient and t-statistics in parentheses. Standard errors are clustered at the individual level. All regressions are weighted by CEM weights. Significance levels are indicated by: * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 5: The Rule's Effect on Time in Public Accounting

Panel A: Likelihood of Leaving Public Accounting

VARIABLES	(1) Pr(Exit Pub Acc)	(2) Time	(3) Log Time
Rule	-0.02	0.99	0.04
	(-0.81)	(-0.29)	(1.01)
Male	0.02	0.97	0.01
	(0.97)	(-1.15)	(0.22)
Observations	15,209	8274	8274
Sample	Matched	Matched	Matched
Cohort Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Chi2	NA	1008	NA
Adjusted R^2	0.035	NA	0.132

Panel B: Post Public Accounting Placement Seniority

VARIABLES	(1) Post Pub Acc Seniority	(2) Post Pub Acc Seniority
Rule	-0.01	-0.04
	(-0.87)	(-1.60)
Male	0.06***	0.06**
	(2.59)	(2.58)
Seniority at the Last Accounting Position	-0.41***	-0.41***
	(-17.12)	(-17.06)
Observations	6,487	6,487
Adjusted R^2	0.215	0.22
Sample	Matched	Matched
Cohort Fixed Effects	No	Yes
State Fixed Effects	Yes	Yes

This table reports the Rule's effect on time in public accounting. The sample consists of observations of CPAs from the networking website. The sample is matched on the gender and year they enter the job market. Panel A reports the likelihood that an individual leaves public accounting. Column (1) includes all the matched observations. Column (2) to (3) include the subsample of the individuals that has ever exited public accounting. The variable of interest Rule is an indicator for an individual subject to the Rule. Male, is an indicator variable that is one if the individual is a male and zero otherwise. The dependent variables are: an indicator for an individual ever exiting public accounting in Column (1). The dependent variable becomes the log of the number of years till exiting public accounting in Columns (2) and (3). Columns (1) are estimated using a linear probability model. Column (2) is estimated as a Cox proportional hazard regression. Columns (3) is estimated using OLS. In Panel B, the post public accounting placements are analyzed using the linear probability model. The dependent variable is an indicator that equals one if an individual gets a higher seniority position after exiting public accounting. Seniority Last Pub Acc Position is the seniority level of the position before the individual exits public accounting. In the case of Cox Hazard models, the coefficients are exponentiated for ease of interpretation, and Z statistics are reported in parentheses. In the case of OLS and linear probability models, I report the coefficient and t-statistics in parentheses. Standard errors are clustered at the individual level. All regressions are weighted by CEM weights. Significance levels are indicated by: * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 6: The Rule's Effect on Writing Length and Complexity

VARIABLES	(1) Log Words Per Sentence	(2) Log Num of Sentences	(3) Fog	(4) Flesch
Rule	0.01	-0.05	-0.17	-0.83
	(0.31)	(-1.62)	(-0.30)	(-1.35)
Male	0.02	0.02	-0.16	0.53
	(1.14)	(1.12)	(-0.52)	(1.56)
Total Jobs	-0.01**	0.00	-0.16***	0.20***
	(-2.42)	(0.05)	(-3.99)	(3.54)
Observations	22,478	22,478	22,478	22,478
Adjusted R^2	0.035	0.021	0.026	0.019
Sample	Full	Full	Full	Full
Cohort Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes

This table reports the Rule's effect on writing quantity and quality using the profiles for the sample of CPAs from the networking website. The writing length and complexity of job descriptions are analyzed. The sample consists of all the reported job descriptions of CPAs from the networking website. The dependent variables measure the length of the job descriptions in column (1) and (2): log number of words in each sentence of a job description (Column 1), the log number of sentences in a job description (Column 2). The dependent variables measure the complexity of the writing in the job descriptions in column (3) to (4): the Fog index for the descriptions of the positions (Column 3) and the Flesch readability ease measure (Column 4). All the columns are estimated using OLS. I report the coefficient and t-statistics in parentheses. Standard errors are clustered at the individual level. Significance levels are indicated by: * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 7: Master's vs. Undergraduate Degree Analysis

Panel A: Descriptives and Average Tenure per Position for Master's vs. Undergraduate Degree Holders

	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 N	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]				

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**	1.139*	1.185**	1.185**	1.169*
	(3.02)	(2.97)	(2.21)	(3.21)	(3.20)	(2.45)
Male		1.029	1.027		1.010	1.007
		(0.66)	(0.62)		(0.18)	(0.12)
Rule			1.076			1.033
			(0.83)			(0.37)
Masters × Rule			0.991			1.043
			(-0.10)			(0.37)
Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
LR Chi2	617.02***	617.45***	618.25***	155.01***	155.04***	155.55***
N	2,331	2,331	2331	1,497	1,497	1497

The sample consists of observations of CPAs from the networking website. Panel A reports the descriptive statistics on the demographics, career outcomes, and educational choices for the sample of masters and undergraduate 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 masters and undergraduate individuals. In the second sub-panel the average and median tenure at the first five positions for individuals with just an undergraduate or a master's degree are reported. The final column reports differences in means. The significance of the difference is judged using a student t-test. Panel B reports estimates from a Cox proportional hazard model on the effects of the Master's degree on the time to promotion. The dependant variables are: the number of year to promotion to level-two seniority (Models 1, 2, and 3) and the number of years to promotion to level-three seniority positions (Models 4, 5, and 6). The variable of interest Master is an indicator for an individual having a master's degree. Male, is an indicator variable that is one if the individual is a male and zero otherwise. Rule is an indicator for an individual being exposed to the Rule. Master*Rule is an indicator variable that is one for Rule CPAs who have a master's degree. All models include state fixed effects and cohort fixed effects. The coefficients are exponentiated for ease of interpretation and Z statistics are reported in parentheses. Standard errors are clustered at the individual level. Significance levels are indicated by: * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.