

Exporting CPAs:
The Effects of the 150 Hour Requirement on CPA Candidates
and Cross-State Mobility of Candidates

Joshua G. Rosett*

Claremont McKenna College

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Abstract: This paper investigates the effects of the 150 Hour Rule for Certified Public Accountant licensure on both the pipeline of candidates sitting for the CPA exam and the opportunity to avoid the extra hours of study by moving to another state. Using data from 1990-2019, almost the complete history of effective dates for the Rule across all jurisdictions other than Florida, the results show that early in this time period the Rule both substantially decreases the number of students sitting for the exam in states putting the Rule into effect, but also increases a measure of net exports of candidates to other states. As the time period progresses and the opportunities to move to non-Rule states decline, the impacts of Rule on both candidates and moving diminish. Late in the time period the Rule has no statistically significant impact on either.

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1. Introduction and background

The 150 Hour Rule (henceforth “Rule”) is a provision adopted by states essentially requiring an extra year of coursework in addition to a typical undergraduate degree as a requirement for obtaining a Certified Public Accountant (CPA) license in the state.¹ The precursor discussions leading to the Rule go back at least to the 1960s, and it was implemented across all U.S. states, territories and Washington D.C. over the three-decade period from the mid-1980s to the mid-2010s. Over that entire period of time there has been a debate about the merits and adverse consequences of the Rule. Over just the couple of years the accounting profession has undergone a sea change with respect to the Rule, and it is now apparent that many states will reverse it. This paper seeks to provide some evidence on the impacts of the Rule and related factors which may be of use to decision-makers in the current debate about the way forward.

Amid much broader concern about the complexity of accounting measurement and the adequacy of standards setting and standards themselves, early debate about changes in accounting education and licensure requirements goes back at least to the early 1960s.² In the first issue of the first volume of the *Journal of Accounting Research*, Trueblood (1963) reviewed the history of previous reports on standards for education and experience leading to CPA licensure, and made a set of recommendations for education going forward based on the perceived needs for effectiveness of the profession. These include 1. a focus on how to learn to be a professional accountant rather than to just learn the rules; 2. additional focus on quantitative skills; 3. additional focus on liberal arts; 4. differentiation for general business education to focus on basic principles of accounting; and 5. a wide range of business subjects. (p. 90) Trueblood also argued that the CPA exam content should be broadened to recognize the broader educational aims, and that educators should lead the process of broadening the education and requirements, though he

¹ “States” include the 50 U.S. States, U.S. territories of Guam, Puerto Rico, and the Virgin Islands, as well as Washington, D.C. All of these jurisdictions have adopted the 150 Hour Rule. In this paper I exclude the territories from the analysis due to missing data issues.

² Zeff (2003a, 2003b) provides an extensive review and commentary on early accounting standards setting bodies from before the Securities Acts of the 1930s to the early 2000s, the challenges they faced, and the evolution of the profession along with methods of establishing standards.

recognized that satisfying expanded internship or experience requirements were problematic at the time due the structure of the profession.

Following this, the first commonly cited implementation of the Rule dates back almost a half century to when in 1979 Florida passed legislation mandating in effect an extra year of higher education for those pursuing the CPA.³ The legislation took effect in 1983. After a period of consideration and a favorable vote of the membership of the American Institute of Certified Public Accountants (AICPA), the AICPA and the National Association of State Boards of Accountancy (NASBA) endorsed the Rule and recommended that the individual state boards of accountancy work with state legislatures to adopt it. The recommendation cited the need for more broadly based education and provided guidelines on other requirements beyond the 150 hours, including a broader based curriculum with more focus on principles in accounting, and coursework and experience aimed at other competencies. (AECC, 1990; Schroeder and Franz, 2004)

There was a lag of several years following the NASBA/AICPA recommendation before additional states adopted the Rule, but all other states, Washington, D.C., Guam, and the Virgin Islands implemented the Rule between 1993 and 2015. Thirteen states put the Rule into effect in the 1990s, 14 in 2000, 15 between 2001 and 2009, and the final six by 2015. Colorado was the last state to implement the Rule, with an effective date of July 1, 2015. Generally, there was a substantial delay, ranging from 3 to 15 years, between adoptions and effective dates for the Rule.

Higher education followed suit, for example with the University of Illinois' "Project Discovery" starting in the early 1990s and aimed at providing a more principles-based understanding of the role of accounting in the economy compared to a relatively rules-based approach focusing on the technical aspects of

³ Recent studies use December 31, 2001 as the effective date for Hawaii, making 2002 the treatment year. However, we found legislative information from 1978 specifying at least 30 hours of education in addition to the undergraduate program, and at least one other study (Boone and Coe (2002, p. 256)) indicates that Hawaii adopted the Rule in 1977 and made it effective in 1978. However, later legislation indicates December 31, 2001, and I follow the precedent of recent papers using this as the effective date.

accounting provided by authoritative guidance. However, the CPA exam itself did not change substantially until 2004, when the number of testing periods increased from two to four each year, the on-paper exam was replaced with computerized testing, and the content was expanded to include more of the broad subject area content envisioned earlier.

Even from the early days of the Rule there were concerns about its effects on the pipeline, stock, and quality of CPAs. In an article titled “Where Have All the CPAs Gone?” in *CFO* magazine, Frieswick (2000) documents the decreasing overall numbers of CPA candidates from 1990 to 1999, though noting increased pay over that period attributable to reduced supply. In a survey of student opinions on the Rule, Bierstaker et al. (2004) document considerable resistance to it. More recent articles note the effects of the Rule decreasing supply, but also note relatively low pay. Two recent examples from the *Wall Street Journal* include “Why No One’s Going Into Accounting” (Ellis and Overberg, 2023) and “Why Graduates Aren’t Hot on Accounting Careers: Low Starting Pay, Onerous Testing” (Mutoh, 2023). In effect, there was both the cost of additional schooling and the opportunity cost of lost time working which would discourage some from entering the profession. Articles in the professional journals and organizations have similarly weighed in on these issues over time, but there was substantial resistance to reversing the Rule until recently.⁴

Just within the past couple of years, the profession appears to have come to the conclusion that the Rule should be modified to provide alternative pathways to the CPA license. In September 2024 the NASBA and AICPA released a call for comment on a potential new pathway to the CPA license called the “Competency-Based Experience Pathway,” along with an Exposure Draft of the pathway dated September 12, 2024.⁵ In effect, this pathway provides an alternative to the 150 Hour requirement,

⁴Within the past year, while calling for change particularly in experiential learning, Barry Melancon defended the history of the Rule. (Iacone, 7/11/24) Later in 2024, accountingTODAY author Daniel called Melancon “The most important man in accounting.” (Hood, 12/24)

⁵ <https://nasba.org/blog/2024/09/11/aicpa-and-nasba-seek-input-on-proposed-additional-pathway-to-cpa-licensure/>, and exposure draft attached at <https://nasba.org/wp-content/uploads/2024/09/CPA-Experience-Pathway-Exposure-Draft.pdf>.

allowing a 120 hours pathway with additional work requirements substituting for the extra course hours, though it leaves in place the option to satisfy the requirement by taking 150 hours. Following this, since late 2024 several states have enacted or contemplated enacting legislation removing the Rule.⁶

Although the individual states all adopted the basic requirement of 150 hours as one requirement for licensure, there is substantial variation in other requirements such work experience, proof of ethics training, whether candidates could sit for the exam having completed 120 hour of education and complete the additional 30 before licensure (the 120/150 model), alternative pathways such as a master's degree for those not completing the mandated coursework during their undergraduate program, and specific curricular requirements in addition to the 150 hours overall. These variations were generally present at the original adoption dates across states but often states amended the requirements after the original effective dates.

The purpose of this paper is to provide evidence on the impact of the Rule on the pipeline of CPA candidates. This paper adds to this literature in several ways. First, it estimates the effects of the Rule on the numbers of candidates sitting for the CPA exam as part of the licensure process, extending previous results by looking at a longer history of the Rule. Other studies primarily use data from years before the significant changes to the exam in 2004. Second, it adds the effects of the Rule on cross-state mobility of students attending college and sitting for the exam in different states. Moving provides an opportunity to avoid the extra course hours. Third and fourth, it makes and tests predictions for the pattern over time of both the Rule and mobility effects. Finally, it looks at the role of additional curricular requirements on the Rule's pipeline and mobility effects.

The main findings are, first, that consistent with previous results, when the Rule is enacted by a state it substantially reduces exam candidates in that state. Second, the Rule is associated with increased net

⁶ Several websites track state updates, including : <https://www.cpacredits.com/resources/tracking-changes-to-the-150-rule-state-by-state/> and <https://www.cfodive.com/news/broadening-cpa-licensure-paths-marching-beyond-150-hour-rule-accounting-talent-shortage/745282/> .

exports of candidates. Third, for both of these results there is evidence that the effects of the Rule diminish as the fraction of the country using the Rule increases over time. Finally, the results do not show an additional effect due to curricular requirements. Although the requirements work as expected in regressions excluding Rule enactment effects, when these are included along with year and state fixed effects the requirements no longer explain candidates or exports.

The remainder of this paper included a literature review in Section 2, methods in Section 3, data description in Section 4, main results in Section 5, and discussion of results and conclusions are in Section 6.

2. Literature survey

There are several strands of literature investigating the Rule. These include, among other things, its labor market effects as a barrier to entry in terms of the flow of new CPAs, on the stock of CPAs, on pass rates on the CPA exam, on the quality of services provided by CPAs, on CPA pay, on audit fees.

In a theory paper treating the Rule as a barrier to entry and examining a range of outcomes, Lee et al. (1999) model the effects of the Rule for CPAs grandfathered in from pre-adoption rules, CPAs earning the license after the Rule, and audit clients in terms of fees and audit quality. They predict that “Compared to the situation without the Rule, audit fees are higher, the pre-Rule CPA candidates are better off, and the audit clients are worse off under the Rule.” (p. 223) The outcome for post-Rule CPAs is more complex and depends on candidate wealth, but overall they predict the Rule discourages entry and replaces more educated and higher quality candidates who would have entered the market absent the Rule with less

educated and lower quality candidates who earned the license prior to the Rule. Several papers empirically test these or closely related hypotheses.⁷

Several papers examine the impact of the Rule and related factors on candidates for the CPA exam. Coe and Boone (2002) use data from the AICPA on CPA certifications and comparing the 29 states which had adopted the Rule by 2000 compared to those adopting 2001 or later to estimate the impact of adoption reductions in certifications, while controlling for the overall reduction in candidates during the 1990s. They conclude that, by the year 2000, the impact of the Rule was a decrease of 11.38%, and in that year the Rule explains 38% of the overall decline associated with the 1990s, while 62% is due to other causes.⁸

Jacob and Murray (2006) estimate the decline in numbers of candidates, successful candidates, and pass rates using NASBA data from 1992 to 2004 winsorized at 5%. Overall, they conclude that there are 28% fewer successful first-time candidates relative to five years before the Rule effective dates. They control for cross-state migration induced by the Rule using Census data on overall migration between 1995 and 2000 as a proxy, expecting that this proxy is correlated with candidate relocation (p. 165). A contribution of this paper is to directly examine the cross-state migration using NASBA data rather than it as a control from aggregate population data.

Carpenter and Stephenson (2006) examine the effects of adoption of the Rule using NASBA data on first-time candidates from 1985 to 2002. They measure candidates as the log of number of candidates, and estimate the difference between levels from two years before to two years after adoption, allowing for an

⁷ It is worth noting In a setting similar to the Rule, Angrist and Guryan (2004), examine the effects of teacher certification testing on the quality of teachers. Similar to the hypotheses in Lee (1998), they posit that the costs associated with testing may lower the average ability of teachers hired. They find that at least there is no evidence that testing raised quality.

⁸ They exclude Hawaii and Florida from their sample due to earlier adoption and effective dates. They also exclude the territories. It is worth noting that, unlike some other papers, they show Hawaii as enacting the rule in 1977, with an effective year on 1978, five years before Florida enacted the rule in 1983. (p. 256)

increase in candidates in the year before adoption. Controlling for state GDP, population, and the number of *Fortune* 500 firms in the state, they find a decrease of 60.5% from before to after adoption of the Rule.

Gramling and Rosman (2009) document a decline in candidates due to the Rules between 1991 and 2002, but also look at the effect of allowing candidates to sit for the exam upon completion of 120 hours and then obtain the additional 30 hours as a requirement for licensure (the 120/150 approach adopted by many states over time). They find an average decline associated with enactment of the Rule between 1991 to and 2002 of 26.3% for states allowing 120/150 and 31.8% for 150 states. However, the difference in decline rates is not statistically significant. They also examine passing rates, finding again that although passing rates decline in both groups, there is no statistical difference between the decline rates. Their methods rely on the ratio of candidates from before to after adoption.

As noted above, an integral part of the 1988 recommendations for the Rule were that accounting education should evolve from a curriculum mostly focused on the rules of measurement for constructing financial statements to a broader based with more attention to understanding principles, problem solving, and broader attention to cultural, behavioral and ethical issues. (AECC, 1990) A notable early example of implementing these changes was Project Discovery at the University of Illinois (Urbana-Champaign) in the early 1990s. Stone and Shelley (1997) examine the impact of Project Discovery based on a sample of students from Project Discovery compared to students in the co-existing traditional curriculum. They find that, controlling for selection into Project Discovery, overall there is not a significant difference between outcomes on the CPA exam between students in Project Discovery and those in a more traditional program. Consistent with this, Dresnack and Strieter (2006), in a survey of CPAs, find that practitioners do not see benefits from the additional year of education.

Grant et al. (2002) examine the marginal impact of the additional year of schooling on CPA exam success using data on over 100,000 exam candidates from 1996-1998. They find that the extra hours of coursework are inefficient relative to other inputs for exam success and could be replaced with two-thirds

of an exam review course. In addition, looking at the marginal rates of mathematics aptitude against other inputs to passing exam sections, they find that the increase to 150 hours for states requiring 150 hours to sit for the exam is “equivalent to a four percentile increase in mathematics aptitude.” They point out that, despite the Rule’s rationale reflecting the need for broader education for CPAs, the exam continued at that time to focus mainly on narrower accounting skills. They further point out that this could be the reason for states to adopt the 120/150 model. They further discuss the aims of testing a broader set of skills in the then upcoming 2004 change of exam format, pointing out that structuring the exam to reflect the broader skills should increase the correlation between the extra work and exam success. This would be consistent with the original rationale for the Rule. It is worth noting that finding the extra education does not improve performance on the exam, but that this does not show that the extra education is not relevant for performance on the job once the license is obtained. In addition, the impacts of the extra hours on reducing exam candidates, selection of who takes the exam, and cross-state flows of candidates are not addressed. The results in this paper help to address some of these related issues.

The passage of the Sarbanes-Oxley act in 2002 and other events from that period likely increased the demand for CPAs at the same time that a large proportion of states adopted the Rule. Allen and Woodland (2010) hypothesize that the potential for reduction in supply and simultaneous increase in demand should lead to higher audit fees in adopting states relative to others. They examine this effect as well as measures of audit quality, finding that fees rose 4.5% due to adoption, and clients paid 4.8% more in states with the Rule. Yet their results do not indicate an improvement in audit quality corresponding to the higher fees.

In a survey of accounting majors, Bierstaker et al. (2004) summarize the objectives of the Rule. They find that students’ objectives for themselves were aligned with the rationales for the Rule, but that students were not in favor of the Rule. They believed that pay should be higher given the cost of extra education and foregone income, but believe that CPA firms would not increase salaries. This discouraged students from pursuing the CPA, especially coming from state schools. The findings in this study complement their conclusions in that discouraged students might move to avoid the Rule.

In a more recent paper, Barrios (2022a) examines a sample of 17,592 CPAs from 11 states selected to represent a significant proportion of the CPA population, with roughly 37% from CPAs in Rule states, for candidates sitting for the exam before the change in format in 2004. The data include NASBA data on pass rates from the students' schools, along with information of numbers of jobs and job tenure, and other variables for each CPA from the "*Business Networking Website*." Using a differences-in-differences specification comparing the log number of candidates at school level (rather than the sample candidates) in the year/month before to year/month after adoption and between those with and without the Rule, he finds a 21% increase in the year before adoption of, and a decrease of 15% in the year after, consistent with the Rule reducing numbers of candidates while creating an anticipation effect for those attempting to pass the exam before the Rule goes into effect. However, tests using the individuals sample do not find a difference between Rule and non-Rule individuals in time to promotion, tenure at firm, likelihood of leaving public accounting, or subsequent job placement seniority. He concludes that the Rule decreases entry into the profession of both low and high ability candidates, but that there is not evidence that the Rule increases the overall quality of those entering the profession.

In a separate working paper using NASBA data on 43 adopters to 2004 and 10 who had not yet adopted as of 2004, Barrios (2022b) uses NASBA data candidates to demonstrate biases when using two-way fixed effects differences-in-differences traditional models to other methods including Goodman-Bacon (2019) Callaway and Sant'Anna (2021) and comparing it to other methods using staggered DID. The paper shows the effects of several sources of bias in using this traditional method, though typically not sufficient to change the statistical significance of the estimates. He recommends using several other methods as diagnostic checks. This paper provides both staggered event study results and results based on Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020).

In a paper using data from the entire history of the Rule, Sutherland et al. (2024) estimate the differential impact on minorities on the flow of new CPAs based on changes in the existing stock using data on CPA profiles from the NASBA. They show that the flow into the profession slowed in response to the Rule,

and that minority entry in the profession lags behind overall entry. Their sample construction captures the endpoint of the licensure process based on new CPAs entering the profession. The results presented in the paper complement Sutherland et al. by looking at the pipeline rather than its output.

The main source of data for this paper is the NASBA “Candidate Performance” books discussed below. These each include extensive commentary on the previous year’s data. Several mention both an apparent effect of both nonresidents from other states and foreigners becoming candidates in states without the Rule. (NASBA, 2002, p. 4; and 2004, p.4.)

Finally, a recent paper related to this subject by Cascino et al. (2021) addresses CPA cross-state mobility, but this is directed at the ability of existing CPAs to practice in states other than the state of their original license. Like other aspects of the profession discussed above, the rules for this vary by state, although CPA Mobility provisions of the Uniform Accountancy Act in the early 2000s provided a template for states to adopt. They find that increased mobility decreased wages and service prices but did not affect employment levels. The results are consistent with increased competition due to lowering the barriers to cross-state mobility for existing CPA license holders. The change in existing CPA Mobility may also affect the choices of students considering where to take the exam and obtain a CPA license due to the common restriction that those licensed in non-Rule state generally were not eligible to practice in Rule states. In principle, this seems likely to have the same effect on students’ choices over time as the increase in Rule states. I do not address this issue in this paper.

3. Hypotheses

As noted in the introduction, the purpose of this paper is to provide evidence on the impact of the Rule on the pipeline of CPA candidates. It seeks to add to the literature by addressing five hypotheses.

First, it estimates the effects of the Rule on the numbers of candidates sitting for the CPA exam as part of the licensure process. As a barrier to entry, enacting the Rule should reduce candidates taking the exam. Although past work looks at the effects of portions of the Rule's history or using data on new CPAs rather than the pipeline, to the best of my knowledge this is the first to examine the nearly the entire history of the pipeline from using NASBA data on exam candidates.

Second, it estimates the effects of the Rule on students attending college in states with the Rule but then moving to other states to take the exam. The Rule serves as a barrier to entry in a given state, but the possibility of moving presents an opportunity to avoid the Rule. Since candidates can move in either direction, the effect is on net exports. Moving is a means of avoiding the Rule, leading to a prediction of increased net export associated with enactment of the Rule.

However, as a progressively larger portion of the country enacts the Rule, this opportunity diminishes. Hence, third, I expect that early in the time period the Rule will be associated with net exports, but over time the effect of the Rule on candidates moving to other states should decline. In the extreme cases, at the start of time period studied there would be many available states to move to without the Rule, but at the end of the time there would be no opportunity for students to move from where they studied to a state without the Rule.

The opportunity to move also impacts the effect of the Rule on candidates who stay in the state where they went to school (own-state candidates). Absent the opportunity to move, there would be some potential candidates who opt out of the CPA pathway due to the additional costs relative to benefits, as predicted by Lee et al. (1999). Another group might have become CPA candidates despite the higher costs even if there was no opportunity to move. But given the opportunity to move, they did so due to the relative improvement in costs vs. benefits. As the possibility of moving to states without the Rule vanished, these students would be more likely to stay in the state where they attended school. Hence, fourth, I expect to observe attenuation of the Rule effect of own-state candidates over time.

Fifth, the paper examines one aspect of the additional requirements which vary across states. These requirements vary between requiring a major focusing on accounting courses, to additional sets of mandated courses within accounting, to those spread across distinct areas focusing on the more technical aspects of accounting, business and economics, and other areas. As an additional barrier, more stringent curricular requirements are expected also to decrease candidates and enhance exports.

4. Data sources and descriptive statistics

The primary data source for this study is the National Association of State Boards of Accountancy's (NASBA) annual series of "Candidate Performance" reports containing summary statistics and other information about CPA exam performance as a component of the requirements for those seeking the CPA license. These publications report data from 1985 forward. For this study, we recorded data for several variables from 1985 to 2019.⁹ Due to limitations on some other sources, we used data from 1990-2019 in this study, effectively covering the time period of all Rule effective dates other than Florida. In general, the NASBA reports several slices of the pool of those sitting for the CPA exam in each jurisdiction (including states, territories and Washington, D.C.) as "candidates," including those taking exam sections for the first time, those repeating, and the total, pass rates across these slices, demographic information on candidates, type of program (undergraduate vs. master's), and other information. Although there is considerable cross-state variation in some licensing requirements, all candidates in a given testing period take the same exam and are commonly graded. There is substantial variation over time on which variables the NASBA reports, as well as the frequency of reporting. From 1985 to 2003 the exam was

⁹ We have collected and compiled data for all years from the "CPA Candidate Performance on the Uniform CPA Examination" editions containing data from 1985 through 2019. The titles up to the 2005 edition reflect data for the previous year, while those with titles 2006 and later are titled corresponding to the year of data presented. Data for calendar year 2005 come from a version with no year in the title of the report, and a subtitle reading "Revised Edition: Report and Commentary on the 2005 CPA Examinations." Additional data for 2006-2012 were collected from the digital versions of the "Uniform CPA Examination: Candidate Performance" editions.

given twice a year, in May and November, and the reports summarize each testing window separately. From 2004 forward the format, frequency and content of the exam changed substantially. These changes included moving the test from paper to digital, substantially changing the content of the exam, and introducing four testing periods per year rather than two. Starting in that same year, the NASBA reports changed to report only total annual figures rather than twice per year. The change in exam format coincided with a steep decline in candidates in 2004 compared to 2003. This is attributed to reluctance to take the new test in the first few years after the changes due to uncertainty about its difficulty, in addition to implementation problems in 2004. The NASBA did not provide figures on candidates passing the 4th section of the exam in 2004 and 2005, but data for other years in fact shows a substantial decrease in pass rates for several years starting in 2004. Results in all estimation procedures reported below include year fixed effects.

The outcome measures used here are constructed from information from the NASBA “Candidate Performance” series. These measures include *Candidates*, a measure of total candidates taking the exam in a given State and time period; *FT_Candidates*, the number taking a section of the exam for the first time in a given testing period; and *Students*, the total number of students sitting for the exam from a given State’s schools in a given period. Students may attend school in one state but sit for the CPA exam in a different state, so *Candidates* and *Students* need not be the same.

Candidates is constructed as follows: from 2004 forward, *Candidates* are as reported. In statistical tests for the effect of the Rule on *Candidates*, from 1990-2003 it is defined as *Candidates* from the May testing period plus *FT_Candidates* from November to avoid double-counting relative to the measure used from 2004 forward. The underlying NASBA data on Candidates are compiled from several sources, including the individual state boards of accountancy, which provide data based on what students self-report on a questionnaire accompanying the exam and the American Institute for CPAs, which creates and grades the exams.

The *Students* measure is not used directly in statistical tests, but rather as an input for the variable *Candidates / Students*, an estimate of the ratio of those taking the test in a given state to those taking the test (but not necessarily in that state) who attended school in that state. Candidates can attend school in one state and then take the exam and obtain the CPA license in a different state. Hence the *Candidates / Students* ratio is an indicator of cross-state mobility, in effect measuring net trade of candidates across states. In general, information on *Students* under-represents *Candidates* for several reasons. First, a main source of information on school attended is voluntary self-reporting by students on the questionnaire accompanying the exam. Second, school level data reported by the NASBA excludes schools supplying fewer than five candidates in a given time period. However, in the statistical tests, I am concerned with changes in the ratio of *Candidates* to *Students* in response to the Rule and other variables rather than the level. It seems unlikely that the self-reporting rate regarding school is correlated with implementation of the Rule or other variables used below. For 2004 forward, *Students* is measured using the sum of the individuals from each school in the state taking the exam (in any state). For 1990-2004 there is an additional issue for measurement in that the NASBA reports do not comprehensively report first-time candidates at the school level. So, unlike the *Candidates* variable, it is not possible to sum the May total and November first time students. To accommodate this issue, the ratio is measured as the *sum* of *Candidates* from May and November divided by the *sum* of *Students* from May and November. In addition, in the main statistical tests using *Candidates to Students*, both year and state are absorbed. It is possible that some students pursue the CPA path through college and then to pursue an alternative career and do not to take the exam. However, that choice would affect neither the numerator nor the denominator of the *Candidates/Students* ratio because both are observed only for those who take the exam.

For two other independent variables, the 150 hour effective date in each state and the course hours required for a license in each state, we collected information from a variety of sources including directly contacting the individual state boards, examining information from state legislative documents containing

details of licensure requirements for the states, and other sources such as additional NASBA and AICPA publications.

The treatment date variable, *150 Effective Year*, represents the first full year when licensure requires 150 course hours of study.¹⁰ The effective dates (including month and day) we collected agree with those reported by other researchers, including Barrios (2022a) and Sutherland et al. (2024). However, in roughly half of states, the effective date occurs after at least the first testing period within the year. Because students could satisfy the requirements for a license within that year but before the Rule takes effect, or at least be grandfathered in on the basis of taking some portion of the exam, I use the following year as the treatment year for these states. For this, I chose to make the adjustment for effective dates in June or later, as this would have been after at least one testing period both before and after the 2004 changes to the exam. Table 3, discussed below in the Results section, compares the basic event study result using the treatment year as in other studies to the adjusted treatment year used here.

A variable used in this study and not examined elsewhere is the number of course hours specified across different areas of study specified by each state as a requirement for obtaining the CPA license. This is a requirement in addition to the basic 150 hours of study under the Rule. For example, California, with among the most extensive requirements, currently requires four groups of courses: 24 semester units of “Core Accounting Subjects,” 24 of “Core Business-Related Subjects,” 20 of “Accounting Study,” and 10 hours of “Ethics Education.” Each of those includes requirements for course titles to show they conform to group definitions. But these requirements vary broadly across states and over time. We collected information for all states based on both post-Rule and pre-Rule requirements from several sources including correspondence with state board of accountancy, state board websites, state legislative websites,

¹⁰ Recent studies use December 31, 2001 as the effective date for Hawaii, making 2002 the treatment year. We found legislative information from 1978 specifying at least 30 hours of education in addition to the undergraduate program, and at least one other study (Boone and Coe (2002, p. 256)) indicates that Hawaii adopted the Rule in 1977 and made it effective in 1978. However, later legislation indicates December 31, 2001, and I follow the precedent of recent papers using this as the effective date.

and a series of compilations of requirements issued jointly by the AICPA and NASBA (11 editions from 1985 to 2006). Given the broad variety of these requirements, we simplified the information into a single variable, *Course Hours*, as the total course hours across groups of courses shown as required to obtain a license in each state. Thirteen states did not specify a number of course hours required for the CPA for years before the Rule, but rather a baccalaureate degree in accounting at an accredited school.¹¹ For these, we impute the pre-Rule *Course Hours* as the average for other pre-Rule states in the effective year for the state missing a value. Although some states updated the requirements after the effective date of the Rule, compiling a complete panel of all such updates across all sample years proved prohibitive, and our measure reflects the requirements as soon after the effective date as we were able to identify them. In principle, *Course Hours* is another form of barrier to entry and should be negatively correlated with *Candidates*. To the extent that additional requirements fit within the curriculum for an accounting major at a given school this may not be a binding constraint, but it seems likely that it is often an increased burden.¹²

Two additional variables are used as controls. First, a variable summarizing state/year total *Employees* from Bureau of the Census data. This is effectively a measure of size, and as a baseline the expected elasticity could reasonably be one. Second, the total value of publicly traded firms in the state/year was compiled from Compustat data, adjusting state back to 2004 based on data provided by Gao (2023). In principle this affects demand for CPAs to audit statements and is expected to have a positive impact on *Candidates*.

Similar to other research in this area, for the analysis below all continuous variables are transformed to the log value. First, the raw data distributions for several variables are substantially skewed, and the log

¹¹ These are Alabama, D.C., Iowa, Kansas, Louisiana, Michigan, Minnesota, Mississippi, Nebraska, Nevada, South Dakota, Utah, and West Virginia.

¹² It is a significant source of difficulty for students at my college.

transformations are more symmetrical. Second, the log transformation allows the interpretation of coefficients as elasticities.

Table 1 presents summary statistics for the main variables. In all cases there are 1,530 observations representing the states plus Washington, D.C. for the years 1990-2019. It is worth noting that there are a few extreme values in *Log Candidates/Students*. Excluding observations with values larger than 2 does not affect the results presented below, and noted in Section 5.3.

Table 2 shows correlations among the main variables. It is worth noting that the high correlation between *Log Course Hours* and *Log Value Public Firms*. In a simple OLS regression of *Log Candidates* on *Log Course*, *Log Candidates/Students*, and *Log Value Public Firms*, all three variables have the expected sign effects and are significant at the 10% level or better. However, as seen below, including state and year effects reverses the sign on *Log Value Public Firms*, and adding the treatment effects makes both highly correlated variables insignificant.

5. Results

This section summarizes the main results for the effects of the Rule on numbers of candidates sitting for the exam, *Log Candidates*, and net exports of students, *Log Candidates / Students*. These effects are estimated both using an event study staggered differences-in-differences (DID) approach as outlined in Clarke and Tapia-Schythe (2021), and stacked DID methods enforcing for each cohort of states enacting the Rule that the control group excludes states previously enacting the Rule.¹³ Using the categorization of event study methods noted in Miller (2023), because all states make the Rule effective by the end of the

¹³ The staggered results are estimated using the Stata “eventdd” implementation provided by provided by Clarke & Schythe (2020). The stacked results are estimated using the Stata “stackdid” implementation provided by Triplett (2024).

time period studied, the effects are estimated using a timing-based event study, and not based on differences between treated and never-treated states.

5.1 Main effects using event studies

All specifications absorb year fixed effects due to control for unobserved factors. For the event study results in Tables 3 and 4, the general specification is

$$y_{st} = \alpha + \sum_n \beta_n (Lead\ n)_{st} + \sum_m \beta_m (Lag\ m)_{st} + \mu_s + \delta_t + X'_{st} \Gamma + \varepsilon_{st} \quad (1)$$

where y is either *Log Candidates* or *Log Candidates/Students*, μ_s are state fixed effects, δ_t are year fixed effects, X is a vector of time/state varying controls, and ε is the error term. The *Lead* and *Lag* terms represent the difference in time from the effective year (time 0, noted as *150 Effective Year* in the tables) of the Rule. The interpretation of coefficient estimates for Lead and Lag β_i effects can be converted to percent changes in y relative to the base period computed as $e^{\beta_i} - 1$. The coefficient estimates on the X 's are elasticities. *Lead 4* and *Lag 4* are estimated as the average of all larger leads or lags, respectively.

Similar to Sutherland (2024), I exclude *Lead 3* as the base period, so it is unreported in the tables. As observed in other papers and the data here also show to some extent, candidates adjust exam timing in anticipation of the Rule. Given the length of time it often takes to pass all four sections of the exam, it is reasonable that the anticipation effects would start several years before the effective date.¹⁴ Although the adjustment of the Rule effective year I propose in this paper yields less significant anticipation effects, I retain the timing used elsewhere.

¹⁴ Comments and in many editions of the NASBA Candidates refer to the fact that second time test takers generally do better than first. Several recent editions track repeat candidates over several years, showing that it is common for candidates to take at least two years to pass all sections.

Table 3 presents results for *Log Candidates* regressions. All specifications include year and state fixed effects and are clustered by state. Specification 1 includes the continuous variables but excludes the Rule event effects. In this specification *Log Course Hours* has the predicted sign is highly statistically significant. It also has a substantial impact on *Candidates*, as the elasticity implies that an additional three semester-hour course requirement lowers *Candidates* by almost 2%. As is standard, the table shows the significance for *Log Employees* relative to a null hypothesis of zero, but a more relevant comparison is to an elasticity of 1. Given the standard error, this hypothesis is not rejected. *Log Value Public Firms* does not have the predicted effect and is marginally statistically significant. As noted above, in an OLS regression without the year and state effects, it works as expected and is statistically significant. However, the inclusion state and year effects such as in Specification 4 makes the variance inflation factors very large. In addition, it seems likely that there are correlated omitted factors, such as cost of living, which may bias the results. I include this variable in specification 4 and similarly in Table 4 only for completeness in reporting. Excluding this variable alone does not change the significance of any of the main results in any specification including it in Tables 3 and 4.

Specification 2 shows the Rule effective *Lead* and *Lag* effects excluding the continuous variables. It shows increasing values in the lead years, consistent with anticipation effects, though statistically insignificant. The effects starting in the *150 Effective Year* and following lags are large and significant declines. For example, the coefficients in the *150 Effective Year* and *Lag 1* represent decreases of roughly 35% and 46%, respectively, in *Candidates* relative to three years before the Rule takes effect. Figure 1 shows the event period effects relative to year -3, as well as the 95% confidence intervals for each event year. In Specification 2, the effective year is adjusted forward one year for states with effective months in June or later, as described above.

Specification 3 replaces the effective year with the year typically used in other studies, not taking the effective month into account. Comparing this to Specification 2, now the anticipation effect at *Lead 1* is statistically significant and predicts a 23% increase in *Candidates* relative to two years earlier. However,

the effect in the *150 Hour Effective* year now becomes a much smaller and statistically insignificant decline. Figure 2 shows this difference visually. These results are consistent with the idea that states with mid-year effective dates include an increase in candidates in the first half of the year, as candidates rush to complete exam sections before the cutoff date, but with an off-setting reduction after the Rule is effective later in the year. This presents a trade-off between the two possible effective year choices. In the remainder of this paper, I adopt the adjusted year, making *Lead 1* the split effects year and *150 Effective Year* more clearly representing a full year of candidates after the Rule goes into effect.

Specification 4 adds back the three state/year continuous independent variables. In this case, only *Log Employees* is statistically significant compared to a null hypothesis of no effect, showing a size effect with an elasticity of 0.83. However, again a more relevant comparison is to an elasticity of 1, as might be expected if the number of candidates increases unit proportionally with employed population. That hypothesis cannot be rejected given the standard error. The results for *Log Course Hours* should be interpreted in light of the inclusion of the Rule lag and lead effects along with both state and year fixed effects. Due to the construction of *Log Course Hours* as changing contemporaneously with the Rule effective date, these sets of dummies effectively absorb nearly all within state potentially explanatory variation for *Log Course Hours*, and including it has almost no marginal impact relative to the event effects.

Table 4 presents results for the hypothesis that the Rule causes increased net exports of students from states implementing the Rule. As noted above, the dependent variable measures to ratio of candidates sitting in a given state/year to the number of students who studied at schools in that state but sit for the exam in any state. Given that students might move either to or from that state given where they went to school, the measure captures the net exports of students who studied in a given state to take the exam in a different state. All specifications include state and year effects and are clustered by state. As in the previous table, Specification 1 excludes the Rule event effects. Here, only *Log Course Hours* is statistically significantly different from a null of no effect. The coefficient estimate implies that an

additional requirement of a three semester-hour course predicts roughly a 1.3% increase in net exports. The point estimate for *Log Employees* is much lower than in the corresponding specification in Table 3, but here it is not clear that the expected value might be 1, as it is more likely that the course hours requirement in the state where the student studied is the only relevant factor. The NASBA data do not offer the opportunity to track where students took the exam relative to where they studied.

Specification 2 excludes the continuous variables and adds the Rule event effects. The lead values are insignificantly different from no effect. Starting in the *150 Effective Year* they become highly statistically significant in the expected direction. The effects in the *150 Effective Year* and *Lag 1* translate to expected reductions (increases in the measure of net export) of roughly 0.29 and 0.39, respectively, or a little more than slightly less to slightly more than half a standard deviation of the dependent variable.

Specification 3 includes both the continuous variables and the event effects. Similar to Table 3, *Log Course Hours* becomes insignificant. The event effects are similar to Specification 2 with the exception of in the *150 Effective Year*, which is somewhat smaller and becomes insignificant, though it is significant in subsequent lags.

5.2 Additional results

The results above are re-estimated using stacked DID excluding states with previous effective dates for each cohort for two purposes. First, the event study method above is staggered, so inclusion of already-treated states in comparison groups for later cohorts may induce bias. Second, in principle as the fraction of the country with the Rule increases, it might be expected that both dependent variables would become less sensitive to the Rule, as posited above. Table 5 presents results for both the entire time period and for a rolling window of subperiods. As in the previous tables, all results are estimated absorbing state and year effects and clustered by state.

A caveat on these results is that a general condition for use of this method is parallel trends leading up to the treatment. In F-tests based on all leads and lags across all cohorts, parallel trends is strongly rejected for both *Log Candidates* or *Log Candidates / Students*. However, parallel trends over a period of up to three decades seems likely over-restrictive. When cohorts are examined separately using only *Lead 1* to *Lead 3* for pre-treatment (similar to the results shown Tables 3 and 4), four of twelve cohorts for *Log Candidates* reject. Three of these are the first three cohorts in the 1990s, and in all three cases the pre-treatment effect is small fraction (4% to 10%) of the post-treatment effect. The fourth case is New York State, which was the sole member of its cohort in 2010. In that case, the pre-treatment effect is estimated as -0.94, with a standard error of 0.04, which is the only case of a significant pre-treatment effect with the sign opposite to what might be expected if there is an anticipation effect causing students to accelerate test-taking. The treatment effect is small and insignificant. Hence New York appears to be unique. No *Log Candidates / Students* pre-treatment effects are significant, and all are small relative to the treatment effect when restricting to *Lead 1* to *Lead 3*.

Given this caveat, overall the results in Table 5 are consistent with predictions. For the entire time period seen in the first row, the results of the Rule effects do not contradict those from Tables 3 and 4. Excluding previously treated cohorts appears to increase the overall effect magnitudes, though the overall treatment effect is not directly comparable to the five post-treatment effects in the earlier tables. The results are consistent with Barrios' (2022b) conclusion, which is that for the early years using this data, there are differences in magnitudes estimated by alternative methods, but that these differences do not yield alternative conclusions.

Table remaining rows in Table 5 report the results for rolling nine-year windows. These include four years of data both before and after the year shown on the table, similar to the lag and lead timing in the previous tables. For both sets of results, the pattern over time shows statistically significant effects from the earliest years though 2006, and then declining with the exception of 2010 for *Log Candidates / Students*. The effect magnitudes are once again fairly large in the early years, though they grow during the earliest years,

which is not expected. Both sets of effects begin to shrink in the early to mid-teens. The exception to the pattern is again centered on the effective year for New York State, which is the only state in that year's cohort. The late years should be viewed with caution as there are few states remaining in those windows.

5.3 Robustness checks

I performed several checks due to issues with the data, and briefly report on them here.

First, the pre-Rule *Course Hours* data were imputed for 13 states where the requirements were effectively a baccalaureate in accounting or equivalent. Substituting an alternative assumption of no change in the requirement due to the Rule, or simply excluding those states does not substantively alter the results for *Log Course Hours*.

As suggested by Barrios (2022b), the stacked differences-in-differences results in Table 5 were re-estimated using the Stata “csdid” routine provided by the authors of Callaway and Sant’Anna (2021) / Sant’Anna and Zhao (2020). This routine implements a doubly robust DID, identifying average treatment effects on the treated “even if either (but not both) the propensity score model or the outcome regression models are misspecified.” (Sant’Anna and Zhao, 2020) The results are again substantively the same as those presented above.

There are a few suspect data points from the NASBA “Candidates” books numbers in the later years of the sample for Maine and New Hampshire. These cause large outliers particularly in *Log Candidates / Students*. None of the options of dropping these states, winsorizing the variable at the 95th percentile, or omitting the particular observations substantively changes the magnitudes or significance of estimates.

6. Discussion and conclusions

Collectively the results presented in the previous section are consistent with the main hypothesis except for the effects of *Log Course Hours* when included with the Rule effects along with state and year fixed effects. Over the entire time period studied, when the Rule becomes effective in a state the number of exam candidates immediately and substantially declines, and this effect persists for at least several years. At the same time, the ratio of candidates in a state falls relative to those who studied in that state and took the exam, suggesting that there was a net outflow of students to other states upon the effective date of the Rule. This also persists for at least several years. Over time, as the number of remaining non-Rule states declines, limiting the value of moving, both of these effects diminish. By the end of the sample period, both apparently have no further impact.

Several caveats apply to this interpretation and are next steps for investigation. First, the early sample period increases in both effects for the first few states enacting the Rule in the mid-1990s is a puzzle.

Second, further investigation of other potential drivers which might explain why, for example, a state like New York (effective in 2010) appears to differ significantly in behavior might be useful in understanding the role of export. One possibility is that states strategically chose when to adopt. It is notable, for example, that California (2014) and New York were both near the end of the sample period. In effect, both may have benefitted from candidates moving to their states, and by the time they made the Rule effective there was little remaining impact.

Third, this paper does not look at success on the test, but considering some of the arguments in Lee (1999), it could be that moving and test performance could be related. Students who judge themselves less likely to do well might be more likely at the margin to move and avoid the costs of the extra schooling, resulting in a difference in exam outcomes both where they studied and where they tested.

Fourth, the under-representation of *Students* due to NASBA not reporting for schools with fewer than five candidates does not appear to be a substantial issue based on the reported numbers comparing years with more or fewer schools reporting. However, it seems likely that schools dropping from the reported data are correlated with the Rule. Further examination of the effects of the Rule on higher education could be accomplished by assembling a panel of schools over time.

Overall the evidence appears consistent with the main predictions of the paper.

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Table 1: Main variables summary statistics

	Obs	Mean	SD	Min	Max
<i>Log Candidates</i>	1,530	6.74	1.12	3.43	9.68
<i>Log Candidates / Students</i>	1,530	0.16	0.65	-2.04	3.87
<i>Log Course Hours</i>	1,530	3.73	0.37	2.71	4.42
<i>Log Employees (from Census)</i>	1,530	14.11	1.04	11.79	16.56
<i>Log Value Public Firms</i>	1,530	12.21	2.08	4.22	17.06

Notes:

- All variables are at state/year level for 1990-2019. (States includes Washington, D.C.)
- *Log Candidates* is log of CPA license candidates sitting for CPA exam reported from NASBA sources. For 1990-2003 it is sum of May candidates and November first time candidates. For 2004-19 it annual as reported.
- *Log Candidates / Students* is log of candidates divided by students sitting for exam in any state, who studied at schools in the state for reported candidates. Number of students is from NASBA sources.
- *Log Course Hours* is log of the number of semester hours specified for CPA licensure.
- *Log Employees* is log of all employees reported in by the Census Bureau.
- *Log Value of Public Firms* is log of the sum of total assets from Compustat.

Table 2: Correlations among main variables

	<i>Log Candidates</i>	<i>Log Candidates / Students</i>	<i>Log Employees (from Census)</i>	<i>Log Course Hours</i>	<i>Log Value Public Firms</i>
<i>Log Candidates</i>	1.00				
<i>Log Candidates / Students</i>	0.31	1.00			
<i>Log Course Hours</i>	0.06	-0.30	1.00		
<i>Log Employees (from Census)</i>	0.85	-0.06	0.33	1.00	
<i>Log Value Public Firms</i>	0.70	-0.13	0.30	0.82	1.00

Table 3: Event study regressions of *Log Candidates* on 150 hour effective date events and continuous variables

	Specification 1	Specification 2	Specification 3	Specification 4
<i>Log Course Hours</i>	-0.600*** (0.156)			-0.010 (0.160)
<i>Log Employees (from Census)</i>	0.813** (0.326)			0.893*** (0.316)
<i>Log Value Public Firm</i>	-0.126* (0.071)			-0.103* (0.056)
<i>Lead 4</i>		0.034 (0.075)	0.069 (0.072)	0.026 (0.072)
<i>Lead 2</i>		0.064 (0.041)	0.069* (0.039)	0.063 (0.040)
<i>Lead 1</i>		0.087 (0.112)	0.207*** (0.070)	0.089 (0.109)
<i>150 Effective Year</i>		-0.437*** (0.113)	-0.164 (0.115)	-0.431*** (0.140)
<i>Lag 1</i>		-0.623*** (0.130)	-0.511*** (0.126)	-0.616*** (0.149)
<i>Lag 2</i>		-0.662*** (0.147)	-0.618*** (0.146)	-0.653*** (0.165)
<i>Lag 3</i>		-0.673*** (0.143)	-0.664*** (0.162)	-0.663*** (0.162)
<i>Lag 4</i>		-0.769*** (0.189)	-0.740*** (0.190)	-0.742*** (0.199)
<i>Constant</i>	-0.951 (4.442)	7.166*** (0.101)	7.137*** (0.104)	-4.144 (4.435)
State Fixed Effects Absorbed	Yes	Yes	Yes	Yes
Year Fixed Effects Absorbed	Yes	Yes	Yes	Yes
Observations	1,530	1,530	1,530	1,530
R-squared	0.884	0.896	0.897	0.889
Adjusted R-squared	0.877	0.890	0.890	0.893
Within R-squared	0.100	0.194	0.199	0.220

Notes:

- Baseline set at *Lead 3* (omitted). *Lead 4* represents average effect of all leads 4 and earlier. *Lag 4* represents average effect of all lags 4 and after.
- Significance levels for two-tailed tests 0.1, 0.05, and 0.01 represented by *, **, and *** respectively.
- Standard errors clustered by state in all specifications.

Table 4: Event study regressions of *Log Candidates / Students* on 150 hour effective date events and continuous variables

	Specification 1	Specification 2	Specification 3
<i>Log Course Hours</i>	-0.586** (0.188)		-0.169 (0.216)
<i>Log Employees (from Census)</i>	0.366 (0.424)		0.376 (0.421)
<i>Log Value Public Firm</i>	-0.089 (0.063)		-0.070 (0.059)
<i>Lead 4</i>		0.108 (0.073)	0.113 (0.073)
<i>Lead 2</i>		0.000 (0.048)	-0.001 (0.048)
<i>Lead 1</i>		-0.149 (0.108)	-0.150 (0.107)
<i>150 Effective Year</i>		-0.340*** (0.124)	-0.266 (0.164)
<i>Lag 1</i>		-0.500*** (0.149)	-0.424** (0.186)
<i>Lag 2</i>		-0.596*** (0.169)	-0.519** (0.202)
<i>Lag 3</i>		-0.605*** (0.172)	-0.528** (0.206)
<i>Lag 4</i>		-0.728*** (0.207)	-0.638*** (0.229)
<i>Constant</i>	-1.72 (6.00)	0.547*** (0.110)	-3.324 (6.049)
State Fixed Effects Absorbed	Yes	Yes	Yes
Year Fixed Effects Absorbed	Yes	Yes	Yes
Observations	1,530	1,530	1,530
R-squared	0.589	0.618	0.622
Adjusted R-squared	0.567	0.595	0.599
Within R-squared	0.067	0.132	0.141

Notes:

- Baseline set at *Lead 3* (omitted). *Lead 4* represents average effect of all leads 4 and earlier. *Lag 4* represents average effect of all lags 4 and after.
- Significance levels for two-tailed tests 0.1, 0.05, and 0.01 represented by *, **, and *** respectively.
- Standard errors clustered by state in all specifications.

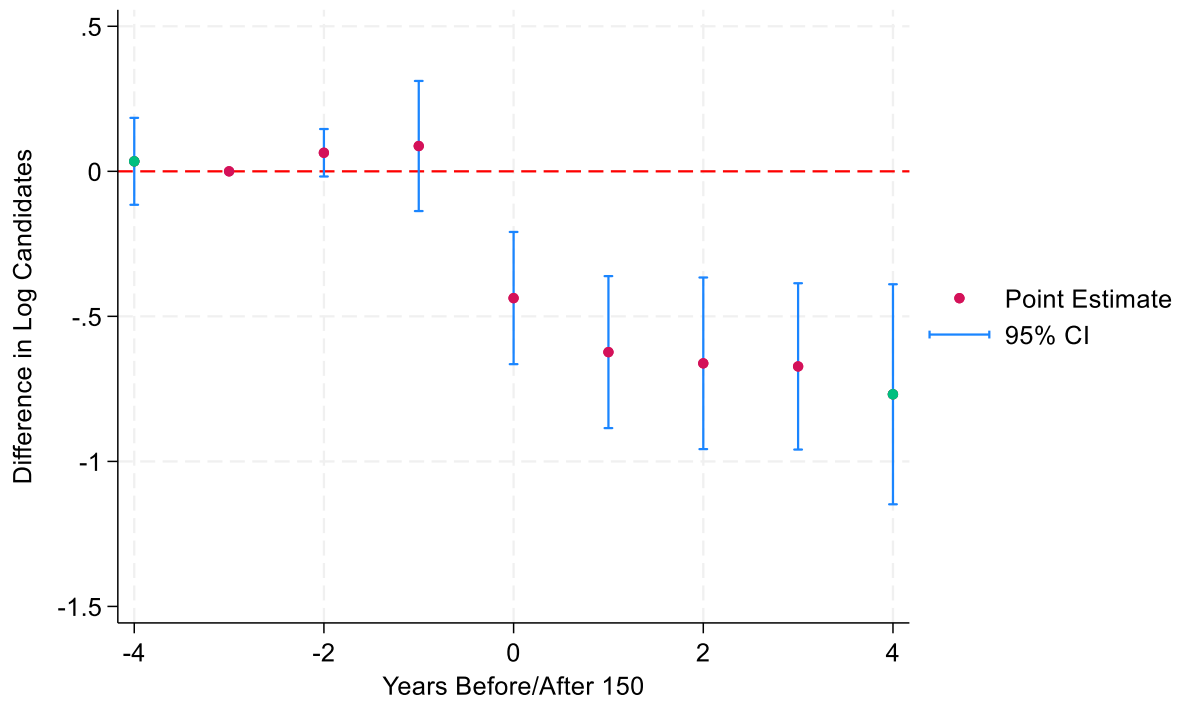
Table 5: Stacked difference in differences results

Window Center Year	<i>Log Candidates</i>		<i>Log Candidates / Students</i>	
	<u>Rule Effect</u>	<u>Robust Standard Error</u>	<u>Rule Effect</u>	<u>Robust Standard Error</u>
All Years	-0.814***	0.201	-0.661***	0.225
1993	-0.451***	0.074	-0.159**	0.062
1994	-0.481***	0.063	-0.200***	0.069
1995	-0.571***	0.073	-0.294**	0.111
1996	-0.626***	0.074	-0.335***	0.120
1997	-0.765***	0.090	-0.455***	0.126
1998	-0.847***	0.108	-0.528***	0.131
1999	-0.864***	0.116	-0.547***	0.129
2000	-0.853***	0.121	-0.543***	0.133
2001	-0.836***	0.122	-0.528***	0.126
2002	-0.780***	0.127	-0.512***	0.136
2003	-0.727***	0.121	-0.488***	0.133
2004	-0.605***	0.132	-0.479***	0.153
2005	-0.515***	0.160	-0.458**	0.159
2006	-0.406**	0.168	-0.346**	0.142
2007	-0.145	0.158	-0.110	0.111
2008	-0.071	0.164	0.118	0.104
2009	-0.117	0.146	0.071	0.100
2010	0.147	0.129	0.341**	0.131
2011	0.117	0.193	0.214	0.241
2012	0.083	0.223	0.175	0.269
2013	0.036	0.268	0.104	0.312
2014	0.026	0.381	0.066	0.418
2015	-0.040	0.494	0.024	0.526

Notes:

- Significance levels for two-tailed tests 0.1, 0.05, and 0.01 represented by *, **, and *** respectively.
- Standard errors clustered by state in all specifications.
- Years represent the mid-point of a 9 year window for each row after the first. The earliest year is estimated with one fewer lead year due to the earliest data year of 1990.

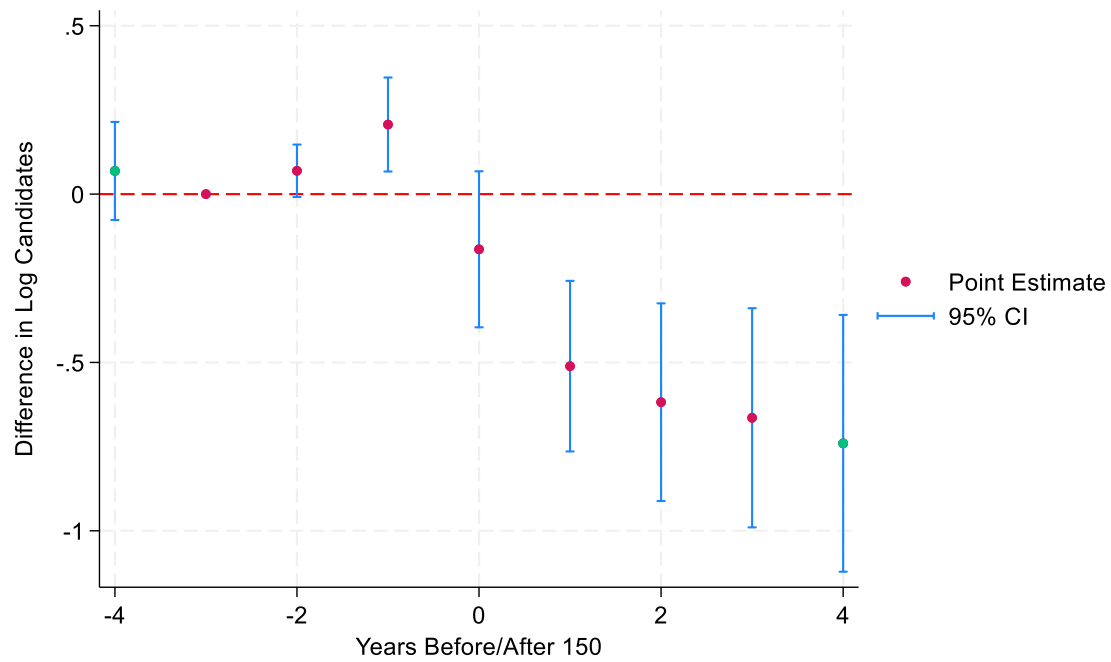
Figure 1 for Table 3, Specification 2



Notes:

- Baseline set at *Lead 3* (omitted). *Lead 4* represents average effect of all leads 4 and earlier. *Lag 4* represents average effect of all lags 4 and after.
- Standard errors clustered by state in all specifications.

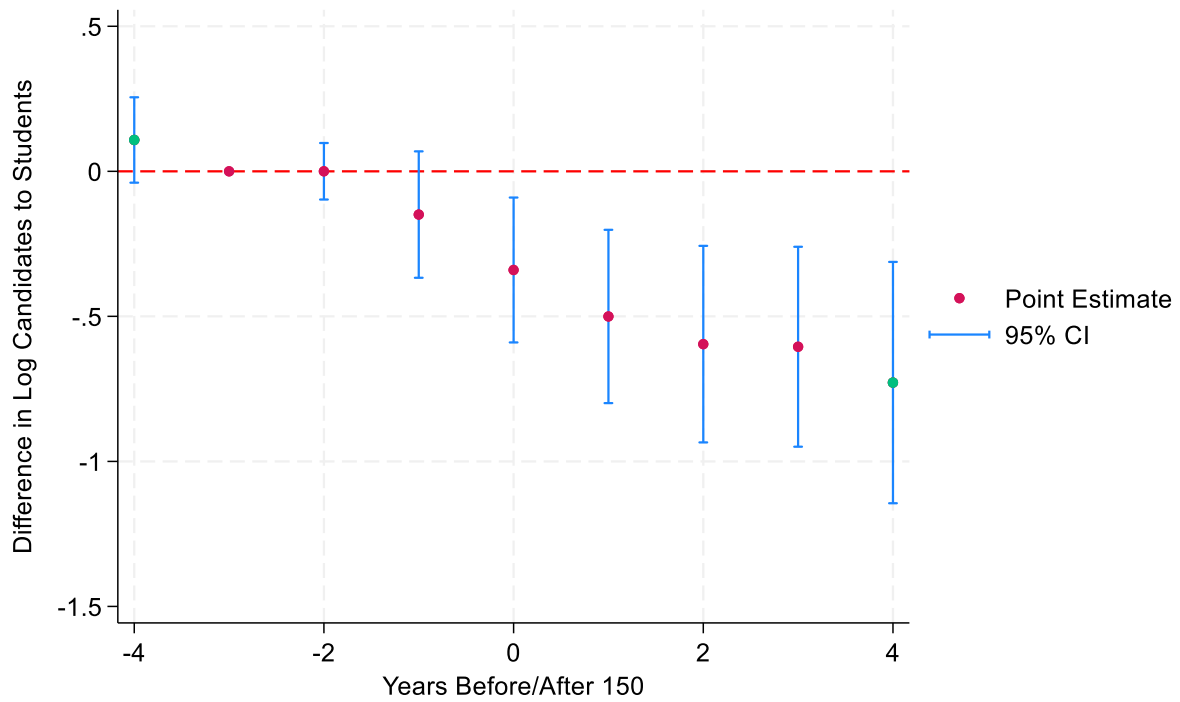
Figure 2 for Table 3, Specification 3



Notes:

- Baseline set at *Lead 3* (omitted). *Lead 4* represents average effect of all leads 4 and earlier. *Lag 4* represents average effect of all lags 4 and after.
- Standard errors clustered by state in all specifications.

Figure 3 for Table 4, Specification 2



Notes:

- Baseline set at *Lead 3* (omitted). *Lead 4* represents average effect of all leads 4 and earlier. *Lag 4* represents average effect of all lags 4 and after.
- Standard errors clustered by state in all specifications.