The London School of Economics and Political Science

Essays on Financial Gatekeeper Regulation

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A thesis submitted to the Department of Accounting of the
London School of Economics and Political Science
for the degree of Doctor of Philosophy

London
April 2020

Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case, the extent of any work carried out jointly by me and any other person is clearly identified in it).

I confirm that Chapter 1 is jointly co-authored with Stefano Cascino (LSE) and Ane Tamayo (LSE) and Chapter 2 is jointly co-authored with Zachary Kowaleski (Notre Dame) and Andrew Sutherland (MIT).

I declare that my thesis consists of 59,818 words.

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Acknowledgements

I am deeply indebted to my advisors, Stefano Cascino and Ane Tamayo, for their insightful guidance of this thesis and for their generous support throughout the PhD. I also thank my examination committee, Hans Christensen and Florin Vasvari for their generous comments. I am further indebted to faculty and fellow PhD students at the London School of Economics for many inspiring discussions and suggestions.

I am most grateful to my family for their encouragement and support. This journey would not have been possible without you.

I gratefully acknowledge generous financial support from the Economic and Social Research Council (ESRC) and the London School of Economics. Any remaining errors or omissions are my own responsibility.

Abstract

This thesis consists of three studies that investigate how regulation affects Certified Public Accountant (CPA) and financial adviser markets.

The first chapter, which is co-authored with Stefano Cascino and Ane Tamayo, investigates whether local occupational licensing regimes create geographical labor market barriers. To study this question, we investigate the labor market consequences of a regulatory change that extended the geographical scope of CPA licenses. Prior to the regulatory change, CPAs required a separate license for each U.S. state they wished to provide services to. With the regulatory change, CPAs can offer services across state lines holding a single CPA license. Our study reveals that local occupational licensing regimes create meaningful labor market barriers. Specifically, we find that CPA wages and service prices decline with the removal of licensing induced geographical barriers.

The second chapter, which is co-authored with Zachary Kowaleski and Andrew Sutherland, explores whether the content of exams financial advisers need to take to provide investment advice affects financial adviser misconduct. Specifically, we study differences in misconduct between advisers that are subject to exams with an emphasis on professional ethics and advisers subject to exams with an emphasis on technical material. Comparing two advisers within the same firm and year, we find that advisers subject to more ethics-related topics exhibit lower future misconduct rates.

The third chapter, which is solo authored, investigates the relation between firm licensing requirements and entrepreneurship in audit markets. Specifically, I study whether mandatory peer review—that is, CPAs having to monitor each other in an effort to promote service quality—affects CPA entrepreneurship. I find that CPA entrepreneurship declines and entrepreneur exit rates increase with the introduction of

mandatory peer review. Increases in exit rates, however, are not pronounced for low-quality service providers, but are concentrated among young female CPA entrepreneurs.

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1 Labor Market Effects of Spatial Licensing Requirements: Evidence from CPA Mobility

1.1 Introduction

Accounting professionals play a pivotal role in the production and auditing of financial information disclosed by firms. Yet, very little is known about how the supply of competent, qualified and independent accountants is determined in the labor market and how institutions shape the labor supply (Francis, 2011). In this paper, we shed light on these issues by examining the economic impact of *occupational licensing* regulations on the accounting profession.

Occupational licensing—that is, the requirement to hold a license for the provision of certain services—is widespread and regulates, along with accountants, a number of other professions including doctors, lawyers, and engineers. In fact, between 25% and 30% of the U.S. workforce is currently regulated through licensing (Kleiner and Krueger, 2010; Kleiner and Vorotnikov, 2017). The labor economics literature discusses the merits and demerits of occupational licensing. On the one hand, by imposing minimum quality standards, occupational licensing effectively protects the public from unqualified professionals, thereby preventing market failures (Akerlof, 1970; Leland, 1979). As such, licensing may increase welfare by reducing consumer uncertainty over the quality of licensed services, which in turn may drive up overall demand (Arrow, 1971; Shapiro, 1986). On the other hand, by constraining supply and increasing prices, licensing may mainly serve the interests of licensed professionals (Friedman, 1962; Stigler, 1971; Maurizi, 1974; Rottenberg, 1980). The origins of this latter view date back as far as the 18th century when, in *The Wealth of Nations*, Adam Smith argues that occupational regulations are by no means an assurance of quality, but rather a way to restrain competition, grant privileges, and allow for rents to be extracted by incumbents.[1]

One way in which licensing may impose barriers to entry is by constraining the geographic mobility of licensed individuals. In the United States, licensing requirements for Certified Public Accountants (CPAs), as well as for other professions, are primarily regulated at the state level (Kleiner and Vorotnikov, 2017). Therefore, licensees must obtain separate licenses for each state in which they provide services. The resulting barriers to geographic mobility may prevent licensees from competing for business across state lines potentially misallocating the provision of services and ultimately driving up their prices (Holen, 1965; Rottenberg, 1980; Kleiner, 2000).

In this paper, we empirically examine how these licensing-induced geographic barriers affect labor market outcomes. In particular, we study the effects of lifting spatial licensing requirements on wages and employment levels of CPAs, as well as their implications for service pricing and quality. To do so, we take advantage of the staggered adoption of CPA mobility provisions (henceforth, *CPA Mobility*) across U.S. states.

CPA Mobility constitutes the most significant change to CPA interstate license recognition according to the National Association of State Boards of Accountancy (NASBA), effectively allowing individual out-of-state CPAs to enter markets other than their home states without the need to notify boards, obtain reciprocal licenses, and pay related fees.[2] We exploit variation in state-level adoption dates, in a difference-in-

[1] Discussing the privileges of the guilds, Adam Smith (1776) states: "*It is to prevent this reduction of price, and consequently of wages and profit, by restraining that free competition which would most certainly occasion it, that all corporations, and the greater part of corporation laws, have been established. [...] and when any particular class of artificers or traders thought proper to act as a corporation without a charter, such adulterine guilds, as they were called, were not always disfranchised upon that account, but obliged to fine annually to the king for permission to exercise their usurped privileges*" (The Wealth of Nations, Book I, Chapter X, paragraph 72).

[2] State-level CPA Mobility provisions in the mid-2000s were based on the Uniform Accountancy Act (UAA) developed by the NASBA and the American Institute of Certified Public Accountants (AICPA). The NASBA and the AICPA introduced the UAA as a blueprint legislation which was subsequently

differences (DiD) research design, to compare labor market outcomes between states that adopt CPA Mobility and states that have not (yet) adopted the policy. This research design allows us to control for general time trends, as well as time-invariant state-level factors that may possibly correlate with state-level licensing requirements and labor market outcomes.

The first part of our empirical analysis explores the effects on wages and employment levels. This analysis is based on the Bureau of Labor Statistics' (BLS) Quarterly Census of Employment and Wages (QCEW) program data, which provide detailed industry-level information on employment and wages for employees of CPA firms. Using this dataset, we find that, subsequent to CPA Mobility, CPA firm employees experience an average wage decline of around 1.0%. The estimated magnitude is economically meaningful, especially when considering the size of the accounting profession and that wage declines persist over time. We further test whether CPA Mobility affects employment levels in CPA firms and find no evidence that this is the case.

A potential concern that we share with virtually any study investigating policy changes is that regulation does not occur in a vacuum (Leuz and Wysocki, 2016; Leuz, 2018). In our setting such a concern would arise if unobservable state-year factors affect both the adoption sequence of CPA Mobility provisions and labor market outcomes. The commonly-held belief by practitioners, however, suggests that the adoption sequence of CPA Mobility is mainly determined by the number of state-level authorities involved in the implementation process, more subtle factors such as personal ties between State Boards of Accountancy and national regulatory bodies, State Boards of Accountancy

adopted by all states. Prior to the adoption of CPA Mobility, states required temporary licenses for out-of-state CPAs in order to grant CPA practice privileges.

board composition, as well as state legislation session schedules (U.S. Department of Treasury's Advisory Committee on the Auditing Profession, 2008:VII:5). Nonetheless, to allay this concern, we investigate whether variables capturing the macroeconomic, entrepreneurial, political, and regulatory environment of the state over time, as well as characteristics of State Boards of Accountancy, can predict the adoption sequence. Interestingly, we find that the adoption sequence is primarily determined by state board characteristics.

To further alleviate concerns that local unobservable time-varying factors may be driving our results, we conduct a difference-in-difference-in-differences (DiDiD) analysis and a county-level analysis.

In our DiDiD tests, we rely on two *within-state* control samples, which allow us to difference out time-varying state-level factors. First, we estimate treatment effects for accounting professionals operating in small CPA firms relative to a control sample of accounting professionals operating in large CPA firms in the same state. Large CPA firms are likely unaffected by CPA Mobility adoption as they could already circumvent regional barriers by leveraging on their national networks and thus the competitive effects of CPA Mobility accrue to (and derive from) small local CPA firms. Second, given that prior studies identify legal professionals as a suitable control group for accounting professionals (e.g., Bloomfield et al., 2017), we estimate treatment effects for accounting professionals relative to a control sample of legal professionals in the same state. The results of our DiDiD tests are in line with those of our main analysis and indicate that the effects stem entirely from small local CPA firms.

In our county-level tests, we restrict the estimation sample to contiguous counties in different states to exploit regulatory discontinuities across state borders (Card and Krueger, 1997; Holmes, 2006; Dube et al., 2010), which allows us to control for heterogeneity in local economic conditions. The identifying assumption in this set of tests is that local time-varying conditions that could correlate with labor market outcomes and the adoption sequence are common along a state border. The results of our border-county tests closely mirror our state-level findings.

Besides CPA Mobility effects on wage *levels*, we also investigate the policy impact on wage *elasticities*. We find that the removal of geographic barriers stemming from CPA Mobility leads to wages becoming less sensitive to local economic conditions and to smaller wage differentials across states after all states adopt.

While our prior analyses based on QCEW program data allow us to examine policy effects on a near-census of *all* employees in CPA firms, ideally we would like to isolate labor market effects for accounting professionals holding a CPA license *only*. To this end, we utilize a proprietary dataset obtained from the American Institute of Certified Public Accountants (AICPA) *Management of an Accounting Practice* (MAP) survey, which includes detailed wage information for accounting professionals working in CPA firms by seniority rank, albeit for a smaller number of states. In this sample, we find wage declines of 3.4% subsequent to the introduction of CPA Mobility. Furthermore, we find that lifting spatial licensing restrictions significantly reduces wage *dispersion*, mostly because of reductions in the wages of top earners (i.e., accounting professionals holding senior positions in CPA firms). In addition, we find that billing rates decline, but we do not find any impact on the number of hours charged to clients. This finding is in line with the reported wage declines and absence of detectable effects on employment levels in our prior analyses.

To supplement our evidence on billing rate declines, we also investigate the effects of CPA Mobility on service prices using a novel dataset of private pension plan audits. In the United States, most private pension plans are subject to mandatory audits, which are typically performed by nation-wide as well as by local audit-service providers. Our dataset allows us to observe audit fee responses for this fairly homogenous service that is offered by both types of audit-service providers. We find that pension plan audit fees decrease by 1.7% on average. Further, we document that the reported effect is concentrated among local audit-service providers.

Finally, since proponents of occupational licensing argue that licensing restrictions are ultimately meant to preserve service quality (Leland, 1979), we conduct an additional set of analyses to assess whether CPA Mobility provisions lead to changes in the quality of the professional services CPAs provide. Entry-level licensing requirements do not substantially change during our sample period, but CPAs facing enhanced wage or fee pressure might minimize costs, resulting in the provision of lower-quality services. To empirically explore this possibility, we take a three-pronged approach. First, we construct a state-year panel of sub-standard professional service cases based on disciplinary action announcements by the AICPA. Second, we construct a dataset of pension plan deficient filer cases (i.e., sub-standard pension plan filings) to investigate whether the reported pension plan audit fee decreases are associated with declines in service quality. Third, we collect CPA firm license and disciplinary action data for the population of all CPA firms in Colorado to estimate disciplinary action probabilities. Collectively, the results of these tests do not support the view that relaxing geographic licensing requirements impairs service quality.

Our paper makes three distinct contributions. First, it adds to the nascent accounting literature examining the impact of regulation on the labor market for accounting professionals (Aobdia et al., 2017; Bloomfield et al., 2017; Barrios, 2019) and provides a direct response to Francis' (2011) call for research on "*the people who conduct audits*." While labor is considered to be the most decisive input to audit-production functions (e.g., Lee et al., 1999), surprisingly little is known about the underlying structure of the labor market for accounting professionals, including the potential impact of regulatory actions. Our study directly addresses this gap in the literature and complements the recent findings of Barrios (2019) who provides valuable insights on the effects of changes to the entry requirements for CPAs.

Second, we contribute to the labor economics literature that examines occupational licensing by showing that lifting spatial licensing requirements produces non-trivial wage and service pricing effects while preserving service quality. Geographic barriers are believed to be one of the most severe costs imposed by occupational licensing regulations (e.g., Kleiner, 2000), yet no compelling empirical evidence supports this claim. DePasquale and Stange (2016) investigate the impact of such restrictions on the labor market for nurse practitioners and find no evidence of wage effects, suggesting that the potential costs of geographic barriers are not severe. However, their results may hinge on the low tradability of healthcare services that naturally require "face-to-face" provision (Crino, 2010). In contrast, we focus on a profession providing highly-tradable services, for which geographic barriers may impose greater costs and for which we have, unlike in healthcare, reasonable service quality measures. As such, our findings complement those of DePasquale and Stange (2016) and suggest that licensing-induced geographic costs are especially relevant when services are highly tradable (Crino, 2010; Criscuolo and Garicano, 2010).

Third, our paper contributes to the recent regulatory debate on the potential costs resulting from state-level occupational licensing regulation (e.g., Kleiner and Vorotnikov, 2017). CPA Mobility constitutes a major change to the licensure of CPAs in the United States, which renders the policy inherently important to study. Furthermore, CPA Mobility is subject to an ongoing debate within the public accounting profession and among the profession's regulatory bodies. In fact, the most recent edition of the Uniform Accountancy Act (UAA) proposes further reforms regarding mobility provisions. More generally, interstate barriers have caught the attention of regulators beyond the accounting profession. The U.S. Department of the Treasury Office of Economic Policy, the Council of Economic Advisers, and the Department of Labor (2015) place the reduction of geographic licensing barriers among their top regulatory priorities. Similarly, the European Union has taken a number of steps to limit licensing barriers across its member countries and is considering further changes. In this respect, our empirical findings may guide policy makers in their recent regulatory efforts.

1.2 Prior Literature and Institutional Background

1.2.1 Prior Studies and Regulatory Debate

The study of occupational licensing has a long tradition in economics (Kleiner, 2000). Occupational licensing is the restriction of the provision of goods and services to individuals holding a license and is intended to protect the public interest. Public protection arguments for licensing rest on asymmetric information about service-provider quality (Shapiro, 1986). In such settings, occupational licensing takes the form of minimum quality standards (Leland, 1979) and aims at mitigating quality deterioration as proposed by Akerlof (1970). Leland (1979) acknowledges, however, that quality standards tend to be set higher than socially optimal when mandated by profit-maximizing self-regulated industries.

A large stream of the labor economics literature assesses this rent-seeking view of occupational licensing (Friedman, 1962; Stigler, 1971; Maurizi, 1974; Rottenberg, 1980). These studies generally argue that licensing mainly serves licensed professionals by creating barriers to entry. Licensed professionals may maximize producer rents: (i) by lowering the occupational licensing exam pass rates (Maurizi, 1974; Pagliero, 2013); (ii) by imposing both higher general and more specific education requirements (Kleiner and Kudrle, 2000; Barrios, 2019); and/or (iii) by creating geographic barriers (Holen, 1965; Kleiner et al., 1982; DePasquale and Stange, 2016).

The rent-seeking view has recently gained juridical and legislative traction. For instance, in 2015 the Supreme Court ruled that a state's occupational licensing boards, which are primarily composed of individuals active in the market, only have immunity from antitrust investigations if they are actively supervised by the state (see, *North Carolina State Board of Dental Examiners v. The Federal Trade Commission*).[3] The Supreme Court ruling has been accompanied by wider efforts to reform occupational licensing regulation, which highlights the timeliness of studying related policies. In the same year, the U.S. Department of the Treasury Office of Economic Policy, the Council of Economic Advisers, and the Department of Labor released a joint report proposing a roadmap to reform occupational licensing regulation, including the removal of geographic barriers.[4] Based on these proposals, 11 states are participating in a "Peer Learning Consortium" to identify best practices aimed at enhancing interstate license reciprocity and portability.[5] In a similar vein, Alexander Acosta, U.S. Labor Secretary, and Dennis Daugaard, Governor of South Dakota, have recently promoted regulatory efforts to reduce geographic licensing barriers (Wall Street Journal, 2018).

Despite its importance and timeliness, *interstate license recognition* has received limited attention in the academic literature. In a recent paper, Johnson and Kleiner (2017)

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[3] Prior to the Supreme Court decision, the North Carolina State Board of Dental Examiners had issued several cease-and-desist orders to non-dentists offering cosmetic dentistry services. These orders prompted non-dentists to stop offering cosmetic services and, ultimately, led to a complaint by the Federal Trade Commission alleging that such actions were anti-competitive and unlawful under the Federal Trade Commission Act.
[4] U.S. Department of the Treasury Office of Economic Policy, Council of Economic Advisers, and the Department of Labor (2015).
[5] See the National Conference of State Legislators (2017).

examine the demographic effects of occupational licensing and provide evidence of a negative association between licensing and migration patterns. DePasquale and Stange (2016) exploit the staggered introduction of the Nurse Licensure Compact (NLC), which allows nurse practitioners to provide services in states other than their state of licensure, to examine the effects of licensing on labor market outcomes. Despite an extensive set of tests, the authors do not find evidence that the NLC impacts labor market outcomes. The absence of an effect may indicate that the potential costs of geographic barriers are not as high as previously thought. However, an alternative explanation may hinge on the low tradability of healthcare services, which typically require "face-to-face" provision (Crino, 2010; Criscuolo and Garicano, 2010) and potentially relocation. In contrast, we focus on a profession providing highly-tradable services, for which licensing-induced geographic barriers may impose greater relative costs. Given that the provision of accounting services across states does not require relocation (and its associated costs), the removal of licensing-induced geographic barriers represents a relatively more substantial reduction in the costs of providing services to other states.[6]

In conclusion, despite the increasing regulatory interest and the ubiquity of licensing in labor markets, empirical evidence on licensing-induced geographic barriers is surprisingly scant. By focusing on a profession providing highly-tradable services, for which geographic barriers may impose greater costs, and for which we have—unlike in healthcare—reasonable (albeit imperfect) service quality measures, our study intends to fill this void. We believe this could be especially relevant to policy makers in the European Union who are considering regulatory actions to relax geographic licensing restrictions across its member countries.

[6] The fact that relocation is not necessary also differentiates our study from the immigration economics literature.

1.2.2 Institutional Background: Stylized Facts on CPA Mobility Adoptions

CPA Mobility provisions constitute the most significant change to CPA interstate license recognition according to the NASBA (2008). By effectively removing temporary licenses for individual CPAs engaged in interstate practice, CPA Mobility provisions introduce a "driver's license" model for CPAs.

Prior to the adoption of CPA Mobility, CPAs who intended to provide services in states other than their home states had to navigate through a patchwork system of notifications, fee models, and board application requirements in order to obtain temporary licenses. According to Art Berkowitz, a Wall Street Journal columnist and CPA, temporary practice privilege applications entailed, among others, the provision of copies of college transcripts and the payment of fees of up to USD 450 even for a single engagement.[7] Similarly, Scott Voynich, former chair of the AICPA Board of Directors and chair of the AICPA's Special Committee on Mobility, states that the main costs of obtaining temporary licenses stemmed from the lengthy applications and the associated fees. He points out that this process not only was burdensome for CPAs already engaged in interstate practice, but also deterred other CPAs from offering services to out-of-state clients. In his view, the problem related to interstate practice reached a tipping point due to increasing interstate business operations of CPA clients.

In a joint effort, the AICPA and the NASBA addressed these issues with the introduction of the Fifth Edition of the UAA. In particular, Section 23 stipulates that an out-of-state CPA with a license in good standing shall be granted the same privileges as resident license holders, effectively removing temporary license applications. The UAA,

[7] Art Berkowitz kept record of the issues related to temporary licenses on his blog *cpaoutofstatelicensing.blogspot.com*. We are indebted to him for taking the time to discuss with us the temporary license application process numerous times.

however, serves as a mere "evergreen" legislation and therefore is only binding for adopting states.

A natural question that arises is why State Boards of Accountancy would support the adoption of CPA Mobility provisions, which mainly benefit out-of-state CPAs (rather than their own constituents) and reduce their fee revenue generation opportunities.[8,9] First, as a matter of fact, State Boards of Accountancy were "encouraged" to support the adoption of CPA Mobility; The U.S. Department of Treasury's Advisory Committee on the Auditing Profession called for Congress to pass a federal provision if state boards failed to (voluntary) adopt the mobility provisions included in the UAA (U.S. Department of Treasury's Advisory Committee on the Auditing Profession, 2008:II:2). Second, not all CPA firms would lose out from CPA Mobility—that is, while small local CPA firms would be harmed from a regulation that increases competition, large CPA firms would not because they were already exposed to the competition of other (out-of-state) large CPA firms that could use their national networks to circumvent regional barriers before CPA Mobility.

[8] The question arises because the occupational licensing literature suggests that regulatory bodies do not necessarily maximize the overall welfare of the profession but, rather, maximize their own interests or those of their constituents (Maurizi, 1974; Leland, 1979; Shaked and Sutton, 1981). CPA Mobility provisions reduce the means by which State Boards of Accountancy can generate fees since the provisions effectively eliminate temporary individual licenses and the related fees. Moreover, the immediate benefits of CPA Mobility adoption by a State Board of Accountancy do not directly accrue to home-state CPAs but to out-of-state CPAs. Thus, it is unclear why states would adopt CPA Mobility provisions. It is worth noting, however, that this reasoning applies primarily to State Boards of Accountancy rather than to the NASBA. First, according to its bylaws, the NASBA primarily represents professional interests at the national level. Second, the NASBA is primarily financed through national CPA exam related program fees. Regarding this latter point, we hand collect data on the NASBA's revenue sources and find that the NASBA generates less than 4% of its revenues from state board membership fees during our sample period.

[9] For a detailed discussion of the political economy of trade policies, see Baldwin (1989). Considering CPA Mobility, the decision of a state to implement mobility provision shares characteristics of a prisoners' dilemma game: a uniform adoption of the system increases the overall mobility of CPAs in the country but an individual state adoption benefits out-of-state CPAs, who can enter the adopting state market, while the benefits for home-state CPAs depend on the adoption decision of other State Boards of Accountancy.

In Table 1.1, Panel A, we report both the enactment, as well as the effective, dates for each state adopting CPA Mobility during our sample period.[10] Our discussions with regulators suggest that important drivers of the adoption sequence are: (i) the number of state-level authorities involved in the implementation process; (ii) professional (and social) ties; and (iii) states' legislative schedules (U.S. Department of Treasury's Advisory Committee on the Auditing Profession, 2008:VII:5).[11] This mitigates the concern that the policy adoption correlates with factors driving labor market outcomes (e.g., Leuz and Wysocki, 2016; Karpoff and Wittry, 2018).

To further alleviate this potential concern, in Panel B, we examine whether the adoption sequence can be predicted using a host of variables capturing: (i) local CPA labor market macro factors (e.g., CPA wage and employment trends and differentials); (ii) factors related to the political economy of local CPA labor markets (e.g., demographic characteristics of State Boards of Accountancy); (iii) general state macro factors (e.g., state-level unemployment, GDP, entrepreneurial activity, etc.); and (iv) factors related to the general political economy of a state (e.g., the share of Democrats/Republicans in the House/Senate in a state, the number of bills introduced and enacted in a state, etc.). We find that a state's representation in the NASBA's "Mobility Task Force," which captures the proximity between the NASBA and the State Boards of Accountancy, predicts the

[10] In our main analyses, we exclude the states of Ohio and Virginia since their CPA Mobility adoption dates (respectively 1961 and 1999) precede our sample period (i.e., 2003-2017). In untabulated sensitivity tests, we assess the robustness of our main findings to the inclusion of these states (considering them as "always treated" throughout the sample period). Results from these tests yield qualitatively similar inferences.
[11] Samuel K. Cotterel, former Chair of the National Association of State Boards of Accountancy (NASBA), and David A. Castello, former President and Chief Executive Officer of the NASBA, suggest that legislative schedules might have played a role as to why not all states have adopted CPA Mobility provisions as of 2008. They propose this argument in a response to the U.S. Department of Treasury's Advisory Committee on the Audit Profession (U.S. Department of Treasury's Advisory Committee on the Auditing Profession, 2008:VII:5). To corroborate their argument, we collect historical legislative schedules from the Book of States Archives at the Council of State Governments. Historical legislative schedules show that only Montana, Nevada, North Dakota, Oregon, and Texas do not hold scheduled legislative sessions in even-numbered years during the CPA Mobility adoption period. Interestingly, none of these states adopts CPA Mobility in even-numbered years (Table 1.1, Panel A).

adoption sequence. In addition, we find that the share of board members working in small-sized local audit-service providers is associated with later adoption. This finding is in line with Colbert and Murray (2013) who argue that it is local CPA firms that perceive CPA Mobility as a source of increased competition, whereas large audit-service providers operating in nation-wide networks do not.[12] Most importantly, we find that general and local labor market conditions, as well as general political economy factors, do not predict the adoption sequence, which alleys the concern that adoption timings may be driven by local labor market shocks.

1.2.3 A Simple Model of Spatial Licensing Requirements

In this section, we present a simple model of spatial licensing requirements to provide the economic intuition behind our empirical tests. We start with the individual supply of an accounting professional (henceforth, "accountant" in this section). We then derive the aggregate labor supply and provide comparative statics. Finally, we introduce geographic barriers and derive our predictions.[13] The intuition of our model builds on Perloff (1980: 412) who argues that (local) licensing restricts quantity adjustments "*leaving only wage adjustments to clear the market*."

Let us assume an individual accountant i with the following utility function: $u_i(y_i) = wy_i - \gamma y_i^2$, where y_i is the number of labor units (e.g., hours) provided, w denotes wages, and $\gamma > 0$ is a cost parameter. We assume quadratic costs since the number of hours an accountant can provide to the market is limited. Taking the first order condition, individual labor supply for a given wage w is given by: $y_i = \frac{w}{2\gamma}$. The aggregate

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[12] We are indebted to Gary Colbert and Dennis Murray for kindly sharing their survey data on State Board of Accountancy board member characteristics.

[13] Our model setup can be adapted to assess the labor market outcomes of *initial* licensing requirements and yields predictions similar to those of Leland (1979).

supply, Y, can simply be written as the sum of individual supply y_i as follows: $Y = \sum_{i=1}^{N} y_i = N\frac{w}{2\gamma}$, where N denotes the number of accountants in the market.

Let us now assume an inelastic demand of quantity Y^*.[14] The market clearing wage can then be written as a function of the number of market participants, N: $w^* = \frac{Y^*}{N}2\gamma$.

Before introducing licensing costs, we provide some comparative statics to confirm that our model captures the intuition provided in Perloff (1980). We can see that $\frac{\partial w^*}{\partial Y} > 0$, i.e., supply is upward sloping, and $\frac{\partial w^*}{\partial N} < 0$, i.e., the wage level decreases as a function of the number of participants in the market. Since N is, for now, exogenous, an increase in N corresponds to shifting the supply curve to the right, i.e., the wage is lower when more accountants participate in the local market. Furthermore, we can see that $\frac{\partial^2 w^*}{\partial Y \partial N} < 0$, i.e., the aggregate supply curve is flatter (more elastic) when the number of accountants participating in the market increases. Graphically, we illustrate this in Figure 1.1, in which we show supply curves for two levels of market participation, N and N', with $N < N'$. In Figure 1.1, Panel A, we see that a market with N' accountants exhibits lower wages as well as a more elastic (flatter) supply curve *vis-à-vis* a market with N accountants. To visualize the supply elasticity effect of increasing N to N', let us assume an exogenous shock to demand, e.g., a new regulation requiring a larger number of accounting services that shifts the demand curve from Y^* to $Y^{*\prime}$. In Figure 1.1, Panel B, we see that the resulting change in wages is larger for a supply assuming N accountants in the market, i.e., $\Delta w^N > \Delta w^{N'}$.

[14] Our predictions with regard to wage responses and supply elasticities are not driven by the assumption of inelastic demand.

Next, we provide a framework for the potential effects of removing licensing-induced geographic barriers by introducing a fixed cost parameter (licensing cost, L). Let N^{In} denote the number of local accountants in state S and N^{Out} the number of out-of-state accountants (potentially providing services to state S). The total supply N in our local market S is given by: $N = N^{In} + pN^{Out}$.[15] This decomposition assumes that the number of accountants in the state, N^{In}, is fixed (in the short run) while a share, p, of out-of-state accountants providing labor to S may vary. The parameter p describes the proportion of out-of-state accountants providing labor to state S. Accordingly, let $0 \leq p \leq 1$.

To describe p, we illustrate the decision problem an out-of-state accountant j faces when considering to provide services to state S as follows:

1. The accountant j observes two wage offers: w_S when providing services to state S, and w_{Out} when providing services only in the home state;
2. Each accountant j also receives random draws of utility associated with providing services to either state S or the home state: $\{v_{j,S}, v_{j,Out}\}$;
3. Crucially, providing services to state S prior to the introduction of CPA Mobility provisions imposes a fixed licensing cost L.

Accountants outside of state S will provide services to state S if $\max\{w_S - L, 0\} + v_{j,S} > \max\{w_{Out}, 0\} + v_{j,Out}$. The introduction of CPA Mobility provisions effectively removes the fixed cost component L. We can think of L as a threshold level, which determines the share of accountants p who are willing to provide services to state S. We would expect that p increases as L decreases ($\frac{\partial p(L)}{\partial L} < 0$), i.e., the inequality

[15] Our decomposition effectively assumes that N^{In} and N^{Out} are substitutes. This assumption seems, in our context, reasonable since, to the best of our knowledge, there are no major state-level changes in the initial licensing criteria.

$\max\{w_S - L, 0\} + v_{j,S} > \max\{w_{Out}, 0\} + v_{j,Out}$ will be satisfied for a larger number of accountants for all $\Delta w = w_{In} - w_{Out}$, holding constant the utility draws. It follows that $N = N^{In} + p(L)N^{Out} < N^{In} + p(L')N^{Out} = N'$ if $L' < L$ for all Δw. As shown with the comparative statics and graphically in Figure 1.1, we expect that a reduction from L to L', i.e., the introduction of CPA Mobility provisions in state S, leads to an increase in N, which, in turn, results in lower wages in state S, as well as in a more elastic supply.

To summarize, based on our model, we expect that the introduction of CPA Mobility will lead to a reduction in wages. Further, we expect that wages of accountants become less sensitive to local economic conditions. In addition, assuming a fairly inelastic demand, we would not expect to observe sizable effects on quantities.

1.3 Research Design

To empirically examine whether the removal of licensing-induced geographic barriers affects labor market outcomes, we take advantage of the staggered introduction of CPA Mobility provisions across states. We exploit variation in adoption dates of CPA Mobility provisions in a generalized DiD research design, effectively comparing labor market outcomes in states that adopt CPA Mobility with those that have not (yet) adopted the policy. To capture the effect of the policy, we estimate model specifications of the following form:

$$y_{s,t} = \beta CPAMobility_{s,t-1} + \partial' X_{s,t-1} + \alpha_s + \gamma_t + \varepsilon_{s,t}. \tag{1}$$

In this model, $y_{s,t}$ is the respective state-year CPA mean wage or employment level.[16] The policy indicator ($CPAMobility_{s,t-1}$) is switched on for states that adopt CPA

[16] In the analyses presented in Sections 1.4.6, 1.4.7, and 1.4.8, we also explore the effects on service pricing and quality as alternative dependent variables.

Mobility provisions in the year following the adoption and thereafter.[17] The policy indicator is lagged by one year for two reasons: (i) to allow for time until the policy effect materializes; and (ii) to account for different adoption dates within a year.[18]

The coefficient β captures the policy effect on wages or employment levels. To control for state-level time-invariant confounders, we include state fixed effects (α_s). To control for time-varying factors affecting the response variable of interest, we include a set of year fixed effects (γ_t). Finally, we include a vector of state-year control variables ($X_{s,t-1}$) to account for state-year-specific potential confounders, such as differences in state-year macroeconomic conditions and migration patterns. Following prior studies (e.g., Dube et al., 2010; Autor et al., 2016), we proxy for state-year macroeconomic conditions using lagged unemployment rates ($Unemployment_{s,t-1}$) and real GDP per capita ($GDPPerCapita_{s,t-1}$). We also include lagged control variables for state-year specific migration patterns ($WithinImmigration_{s,t-1}$ and $AbroadImmigration_{s,t-1}$) to account for the effect of structural demographic changes.[19] We provide detailed variable definitions in Appendix 1.A.

To account for the grouped structure of our wage data—that is, our units of observations are average wages for the employed in a state-year—we estimate weighted least squares (WLS) regressions using annual employment share weights (e.g., Autor et

[17] The policy indicator, $CPAMobility_{s,t-1}$, effectively corresponds to the product of a state indicator variable and a post-adoption indicator variable. To enhance readability, we do not write out the product of the state indicator and the post-adoption indicator but, rather, refer to the product as $CPAMobility_{s,t-1}$. Since we include state and year fixed effects, we suppress the respective main effects.

[18] Rather than choosing an arbitrary cut-off month, we lag the policy indicator by one year. We assess the sensitivity of our findings to this design choice by examining pre-treatment trends (Figure 1.2).

[19] In untabulated robustness tests, we also include further state-year-level controls for demographics: gender, minority, and marital status variables. Prior literature (e.g., Kleiner et al., 2016) identifies demographic characteristics to be meaningful wage determinants in American Community Survey (ACS) micro-level regressions. While these variables exhibit explanatory power in micro-level regressions, variation at the state-year level is very modest and therefore they contribute only very little over our fixed effects structure. The inclusion of demographic controls does not alter the tenor of our empirical findings.

al., 2006). We cluster standard errors at the state level (e.g., Bertrand et al., 2004; Donald and Lang, 2007).

1.4 Empirical Analysis

1.4.1 State-Level Difference-in-Differences Analysis: CPA Mobility Wage Effects

In our first set of tests, we examine the effect of CPA Mobility provisions on state-level wages of CPA firm employees. We source the data for this analysis from the BLS Quarterly Census of Employment and Wages (QCEW) program. The QCEW program provides data on wages and employment levels disaggregated by industry and geographic units and is, to the best of our knowledge, the most comprehensive data source to explore CPA labor market effects.

Our state-year panel of CPA firm employee wages and employment levels, the *QCEW State-Level Sample*, covers the period from 2003 to 2017 and comprises 720 state-year observations. We provide detailed variable definitions and sample selection criteria in Appendices 1.A and 1.B, respectively. Table 1.2, Panel A presents descriptive statistics. On average, CPA firm employees earn USD 63,514 per year and employment levels are around 8,000 employees. When looking at regional patterns (unreported), state-level CPA firm employment is positively correlated with the total labor force (pairwise correlation above 0.90 across all years). Moreover, highly-populated states, such as California, Texas, New York, and Illinois show high levels of CPA firm employment.

To gauge the effect of CPA Mobility on wages, we estimate model (1). Our dependent variable is the natural logarithm of wages paid to CPA firm employees. In Panel B, we present different specifications of model (1) in which we individually and jointly account for macro-level factors (Columns (2), (3), and (6)), as well as migration patterns (Columns (4), (5), and (6)). All models include state and year fixed effects.

Across all model specifications, the negative coefficient estimate on our policy indicator ($CPAMobility_{s,t-1}$) is statistically significant and economically meaningful. The estimated decrease in average wages is similar in magnitude across alternative model specifications and fairly insensitive to the inclusion of macro- and migration-level control variables. Our most conservative estimate yields a coefficient of -0.010 (Column (6)), indicating average wage declines of 1.0% subsequent to the adoption of CPA Mobility. This percentage decrease implies that state average pre-treatment wages experience a USD 541 decrease subsequent to CPA Mobility adoption. In gauging and interpreting the economic magnitude of this effect, it is important to make several important considerations. First, the documented wage declines persist over time and reflect the overall size of the accounting profession. Second, a limitation of our QCEW program data is that wages and employment levels are averaged across *all* employees of CPA firms, including non-accounting professionals (e.g., administrative and IT staff), for whom the reduction of licensing-induced barriers may have smaller effects.[20] Lastly, our treatment estimates likely capture both CPA Mobility wage declines in adopting states (that face more competition) and potential wage increases in non-adopting states (that can offer services in adopting states).[21] The magnitude of our results should therefore be considered with these caveats in mind and interpreted as a lower bound of the policy effect.

[20] We directly address this limitation in our analyses presented in Section 1.4.6 in which we provide estimates based on more granular data (i.e., specific wages of accounting professionals *vis-à-vis* all CPA firm employees) for a sub-set of states. Moreover, to gauge the extent to which our data reflect actual CPA wages, we compare QCEW wage data in the most recent year of our sample to wage information provided by online job advertisement websites such as *roberhalf.com*, *glassdoor.com*, and *payscale.com*. We find no discernible differences. For instance, workers in New York CPA firms earn an average income of USD 110,539 based on our QCEW data which are in close proximity to estimates of New York CPA incomes of USD 103,200 provided by *payscale.com*.
[21] We examine potential SUTVA violation concerns in Section 1.4.3.

An important identifying assumption of our DiD design is that wage trends between treated states and control states would move in parallel absent the CPA Mobility treatment. Because counterfactual trends are not empirically observable, we test for differences in pre-treatment trends. Accordingly, we examine differences in wages across states that adopt CPA Mobility and states that have not (yet) adopted the policy by mapping out treatment effects in event-time. In Figure 1.2, Panel A, we map out these effects by replacing our policy indicator with separate event-time dummies, each marking a period relative to the policy announcement (*t=0*), and plot the estimated treatment effects.[22] The evidence from this figure suggests that, while prior to CPA Mobility adoption the treatment effects are statistically indistinguishable from zero, they experience a sharp decrease in the years following CPA Mobility adoption that persists over time. These results mitigate concerns that our prior findings could be driven by differences in pre-treatment trends. Importantly, this graphical evidence further suggests that the reported effects are not limited to the short run only. Rather, we observe wage declines persisting at least up to four years after the time of adoption. This persistence is consistent with predictions from the intra-industry (or "cross-hauling") trade theory (e.g., Brander and Krugman, 1983) according to which consumers are better off in the long run if the same quantity of services is offered by more (competing) firms even when the total number of suppliers in the overall economy stays constant.

Overall, in line with our predictions, the results of this analysis indicate that the removal of licensing-induced geographic barriers results in negative wage pressure.

[22] We omit the indicator for *t-1*, which serves as benchmark period.

1.4.2 State-Level Difference-in-Differences Analysis: CPA Mobility Employment Effects

In our previous tests, we assess the effect of CPA Mobility on state-year CPA wages. We now move to examine the effect of the policy on average employment levels in CPA firms. To explore the effects on employment levels, we estimate a version of model (1) in which our dependent variable is the natural logarithm of the number of employees working in CPA firms ($Log(Employment_{s,t}^{QCEW})$). In Table 1.2, Panel C, we present the results of this analysis based on our *QCEW State-Level Sample*.

Our coefficient estimates across all specifications (Columns (1) to (6)) do not suggest effects of CPA Mobility provisions on the number of employees in CPA firms. As the potential effects of CPA Mobility on employment may take longer to materialize, or CPA firms may anticipate the policy change and adjust employment levels, in untabulated tests we also explore the extent to which the timing of CPA Mobility provisions may affect employment. Evidence from these sensitivity tests indicates neither lagged nor anticipation effects.

1.4.3 Neighbor CPA Mobility Adoption Effects

The introduction of CPA Mobility provisions allows out-of-state CPAs to enter adopting states more easily. Thus, a potential concern with our DiD analysis is that control observations may be indirectly treated—that is, a potential violation of the stable unit treatment value assumption (SUTVA). While we share the SUTVA violation concern with virtually every study that examines the removal of trade barriers (e.g., Donaldson, 2015), we conduct a further set of tests to assess whether spillover effects from our control group may be driving our findings.

Following prior studies (e.g., Heider and Ljungqvist, 2015; Simintzi et al., 2015), we include *neighbor treatment effects* ($NeighborCPAMobility_{s,t-1}$) in our main model specification to capture potential spillover effects. The neighbor treatment is constructed as the employment-weighted treatment of all neighbor states.[23] In the odd-numbered columns of Table 1.2, Panel D, we augment our base model specification (model (1)) by including neighbor treatments as additional control variables. Controlling for neighbor treatments does not subsume the effect of each state's own treatment (coefficients on $CPAMobility_{s,t-1}$ remain negative and significant). In the even-numbered columns, we report coefficient estimates of model specifications in which we suppress a state's own treatment and, instead, include only neighbor treatments. For each individual state, neighbor treatments, which effectively serve as "pseudo-treatments," should not induce any effects. The coefficient estimates on neighbor treatments are not statistically significant, which suggests that indirect control group effects are unlikely to drive our findings.

1.4.4 Mitigating the Influence of State Time-Varying Factors

Unobservable state time-varying factors may pose a challenge to our identification strategy and bias our inferences if correlated with the timing of CPA Mobility adoption and labor market outcomes. To make an initial assessment of the robustness of our findings to omitted variable bias and evaluate the stability of our treatment effects, we implement the bounding methodology proposed by Oster (2019). The evidence from this analysis, which we discuss in Section 1.8.2, suggests that it is unlikely that our treatment effects are driven by omitted variables, as unobservables would need to be almost eight times (Δ = 7.864) as important as the observables to produce a treatment effect of zero (Table 1.OA-3 in Section 1.8). Nevertheless, we

[23] In untabulated tests, we repeat the analyses presented in Table 1.2, Panel D with neighbor treatment variables constructed as the Census Region average treatment, the Census District average treatment, unweighted neighbor treatment, first neighbor's treatment, as well as inverse distance weighted treatment. Estimates based on these alternative definitions of neighbor treatment closely mimic our reported results.

employ several other strategies to alleviate this potential concern, including: (i) a DiDiD analysis in which we use different *within-state* control groups; and (ii) a contiguous-county analysis in which we compare labor market outcomes across neighboring counties located in different states.

To alley the concern that the wage declines that we document in our state-level analysis may be due to unobservable local shocks, we employ a DiDiD research design, which allows us to compare labor market outcomes of CPA Mobility within each state using additional *within-state* control groups.

First, we argue that not all CPA firms lose out from CPA Mobility—that is, while small (local) CPA firms experience wage declines because of increased competition from other small CPA firms, large (national) CPA firms were already exposed to the competition of other (out-of-state) large CPA firms even before CPA Mobility given that these could already use their national networks to circumvent regional barriers. Accordingly, we design a set of tests in which we estimate the effects of CPA Mobility for small CPA firms, relative to control group of *large CPA firms* in the same state. To capture CPA firm size, we use state-level data from the Census Statistics of U.S. Business (SUSB) program, which allow us to observe wage (and employment) responses for accounting professionals employed in firms of different sizes. We discuss the construction of our *SUSB State-Level Sample* in Appendix 1.B.

To test whether wages of small CPA firms decline after CPA Mobility adoption relative to wages of large CPA firms, we use a DiDiD specification, which includes: (i) state × year fixed effects to control for unobservable state-year specific factors potentially correlated with both the adoption sequence and labor market outcomes; (ii) state × firm size fixed effects to account for CPA firm size heterogeneity across states; and (iii) firm size × year fixed effects to capture differences in wage and employment trends across large and small CPA firms.

We report the results of this analysis in Table 1.3, Panel A. We find that, following CPA Mobility adoption, wages in small CPA firms decline by 1.9% relative to wages in large CPA firms (Column (1)). This percentage decrease implies that, relative to large CPA firms in treated states, small CPA firms' average pre-treatment wages decrease by USD 793 subsequent to CPA Mobility adoption. In contrast, we do not observe statistically significant effects on total employment (Column (2)), average employment (Column (3)), and total number of CPA firms (Column (4)), which provides reassurance that our wage findings are not driven by CPA firm employees switching across firms of different sizes.

The key identifying assumptions of this analysis are that (i) large CPA firms are unaffected by the treatment and (ii) trends across large and small firms would have moved in parallel absent the regulatory intervention. To assess the validity of the first assumption, in Table 1.OA-2, Panel B, we report separate treatment effects for large firms and find that wages of large CPA firm employees are not affected by CPA Mobility, which provides reassurance on the suitability of this control group. We assess the validity of the second assumption by mapping out the treatment effects in event-time. In Figure 1.2, Panel B, we plot event-time coefficient estimates around CPA Mobility adoption dates and observe no statistically significant differences prior to the adoption time.[24] Reported wage declines accrue subsequent to CPA Mobility adoption only.

Next, following Bloomfield et al. (2017), we use *legal professionals* an additional within-state control group. We leverage on the industry disaggregated information

[24] Unfortunately, SUBS program data are unavailable prior to 2007, which limits the extent to which we can examine pre-treatment trends.

provided by our QCEW program data to collect information on legal professionals' wage and employment levels from 2003 to 2017 using the NAICS code 541110 "Offices of Lawyers."[25]

Conceptually, legal professionals are likely to be a suitable benchmark for accounting professionals as both professions require substantial investment in education and expert knowledge, and are subject to state-level licensing in the United States. Accordingly, we estimate a DiDiD model specification, which includes: (i) state × year fixed effects to control for state-year specific shocks potentially correlated with the adoption of CPA Mobility provisions and local labor market conditions; (ii) state × profession fixed effects to account for unobservable local differences between professions; and (iii) profession × year fixed effects to capture differences in national-level trends between professions.

In Table 1.3, Panel B, we report the results of this analysis. We find that, relative to legal professionals, CPAs experience a 0.9% decline in wages following CPA Mobility adoption (Column (1)), which is in close proximity to our state-level DiD estimates. This percentage decrease implies that, relative to legal professionals from treated states, accounting professionals' average wages decrease by USD 487 after CPA Mobility adoption. Moreover, in line with our previous findings, we do not observe statistically significant effects on total employment (Column (2)), average employment (Column (3)), and total number of firms (Column (4)).

We assess the suitability of legal professionals as a control group by estimating separate treatment effects for the wages of legal professionals only and find that these are not affected by CPA Mobility (Table 1.OA-2, Panel C). To gauge the validity of the

[25] In an additional set of tests, which we discuss in Section 1.8.3 and whose results are reported in Table 1.OA-4, we also take a synthetic control group approach (Abadie and Gardeazabal, 2003; Abadie et al., 2010) and use two "synthetic groups" of CPAs based on business professionals (other than accounting professionals) as alternative control samples.

parallel-trends assumption, we map out the reported effects in event-time. The graphical evidence in Figure 1.2, Panel C, suggests that treated and control units do not show statistically significant pre-treatment differences in wages.

Finally, in the spirit of the double-matched approach proposed by Bloomfield et al. (2017), we pair small and large CPA firms to small and large legal firms each sample year to form *quadruplets*. This research design effectively allows us to gauge wage and employment heterogeneity across small and large CPA firms, controlling for state time-varying factors. The results of this analysis, reported in Table 1.3, Panel C, are, once again, similar to our main findings.

To further address the concern that unobservable local factors may drive our results, we conduct an additional analysis in which we take advantage of more granular county-level wage- and employment-level data. We follow Dube et al. (2010) and construct a sample of contiguous counties located on different sides of a state-pair border (see Figure 1.3). Dube et al. (2010) argue that such border counties provide a powerful setting to assess policy effects on labor market outcomes. The basic argument for this test is that contiguous counties are subject to similar economic conditions that may correlate with policies and outcomes (Card and Krueger, 1997; Holmes, 2006; Dube et al., 2010). However, since these counties are located in different states, they differ in terms of adoption dates. To operationalize the idea outlined above, we estimate a model of the following form:

$$\begin{aligned} y_{c,b,s,t} = {} & \beta CPAMobility_{s,t-1} + \partial' X_{s,t-1} + \vartheta Unemployment_{c,t-1} \\ & + \alpha_c + \gamma_{b,t} + \varepsilon_{c,b,s,t}. \end{aligned} \tag{2}$$

In this model, $y_{c,b,s,t}$ is the respective county-year CPA mean wage or employment level. Each county belongs to a border segment denoted by the subscript *b* and a state denoted by the subscript *s*. The state in which a county is located determines the treatment timing. To control for county-level time-invariant confounders, we include county fixed effects (α_c). This design choice effectively allows us to control for time-invariant factors that may correlate with occupational licensing legislation and labor market outcomes. To control for time-varying factors along each border segment, we include a set of border-year fixed effects ($\gamma_{b,t}$). All other variables are as previously defined except for unemployment rates, which are available at the county-year-level through the BLS LAUS program. Detailed variable definitions are provided in Appendix 1.A.

For our border-county analysis, we source data from the BLS QCEW program. We identify contiguous counties located on different sides of border segments using the BLS County Adjacency Files. We provide detailed data construction and sample selection information in Appendix 1.B. Table 1.4, Panel A presents the descriptive statistics for our *QCEW Border-County Sample*.

In Panel B, we present our DiD estimates. In Columns (1) to (4), we show CPA Mobility effects on wages, whereas in Columns (5) to (8), we show effects on employment levels. Despite the extensive fixed effect structure used in these tests, estimates of the policy impact on wages remain statistically significant across all specifications. Most importantly, despite the differences in research design, coefficient magnitudes are very close to those documented in our main analysis (Table 1.2, Panel B). Furthermore, similar to the state-level analysis in Section 4.2, we do not find an effect on employment levels.

In conclusion, the evidence from our border-county analysis mitigates concerns that the results of our state-level analysis are driven by unobservable local macroeconomic conditions.

1.4.5 CPA Mobility and Wage Sensitivities to Local Economic Conditions

In this section, we explore whether the removal of licensing-induced geographic barriers extend beyond wage *levels* and also affect *elasticities*. We expect that wage sensitivities to local economic conditions and wage differentials to become smaller over time as a result of the increased CPA labor market integration brought about by CPA Mobility.

To empirically gauge the *long term* effects of CPA Mobility, we separately estimate wage sensitivities to local economic conditions for accounting and legal professionals over the period before the first of our sample states adopts CPA Mobility (i.e., 2002-2005) and after the last of our sample states adopts CPA Mobility (i.e., 2014-2017). Following prior studies (e.g., Mian and Sufi, 2014), we conduct this analysis at the Metropolitan Statistical Area (MSA) level, which allows us to gather more granular information on changes in GDP per capita—our proxy for changes in local economic conditions—and wage data for both accounting and legal professionals. The data construction details for our *QCEW MSA-Level Sample* are presented in Appendix 1.B.

In Figure 1.4, we provide graphical evidence on the long term effects CPA Mobility on wage elasticities. In Panel A, we plot wage sensitivities for our treatment group (accounting professionals), whereas in Panel B we show wage sensitivities for our control group (legal professionals) both before (left-hand side plots) and after (right-hand side plots) CPA Mobility adoption. These plots suggest that, relative to the wage elasticities of legal professionals, the wage elasticities of accounting professionals decline over time.

We then complement our graphical evidence with a formal regression analysis in which we estimate wage sensitivities to local economic conditions separately for accounting and legal professionals before the first of our sample states adopts CPA

Mobility and after the last of our sample states adopts CPA Mobility. Table 1.5, Panel A, presents the results of this analysis. Consistent with the graphical evidence shown in Figure 1.4, we document a statistically significant decline in wage elasticities for accounting professionals (negative and significant coefficient on $PostAdoption_t \times \Delta Log\left(Wage_{m,t}^{QCEW}\right)$ in Column (1)), but not for legal professionals (Column (2)). However, the difference in coefficients is not significantly different across the accounting and legal professional partitions.

Next, we explore whether (cross-sectional) wage volatility and (cross-sectional) wage dispersion decline with the introduction of CPA Mobility. To operationalize these two constructs, we calculate, for each year, standard deviations and interquartile ranges for $\Delta Log(GDPperCapita_{m,t})$ and $Log(GDPperCapita_{m,t})$ for both groups of professionals, respectively. We then examine how wage dispersion and wage volatility vary before and after the adoption of CPA Mobility. We present the respective point estimates in Table 1.5, Panels B and C. Although based on very small samples of 16 observations (2 professions × (4 years before + 4 years after Mobility adoptions)), these tests suggest that both wage dispersion and wage volatility decrease with the introduction of CPA Mobility.

1.4.6 CPA Mobility Effects Within CPA Firms

Our analyses so far are based on QCEW program data, which are disaggregated by industry. As discussed in Section 1.4.1, the QCEW program data do not allow us to distinguish between accounting professionals and other staff employed in CPA firms. Because of this data limitation, a potential concern with our previous analyses is that the policy effect that we document may be underestimating the "true" effect.

Ideally, we would want to obtain data on the individuals actually targeted by the policy change (i.e., accounting professionals holding a CPA license within CPA firms). To this end, we hand-collect data from the survey response sheets of the AICPA's Management of an Accounting Practice (MAP) survey. The AICPA MAP survey is a biennial survey of CPA firms and serves as a benchmarking tool for participating firms. Participating firms receive a report which provides them with state-level mean performance metrics, permitting thereby state-peer performance comparisons.

We obtain these confidential state-level reports from the AICPA. While the survey is not explicitly designed to allow for comparisons over time, part of it includes wage information for CPAs, which is collected and presented consistently over time. The MAP survey also provides us with an opportunity to collect information on billing rates charged by CPA firms, as well as the number of hours charged to clients. A detailed presentation of our *AICPA MAP Survey Sample* is provided in Appendix 1.B.

In Table 1.6, Panel A, we present descriptive statistics for the *AICPA MAP Survey Sample*. The mean wage of USD 85,039 in this sample is considerably higher than the mean wage in the *QCEW State-Level Sample*. This discrepancy is partly due to disclosure restrictions; we are able to obtain survey reports at the state-year level only for highly populated states in which CPAs, on average, earn higher wages. In addition, the higher mean wage is consistent with limiting observations to accounting professionals only (i.e., excluding other CPA firm staff members who presumably earn less).

In Panel B, we replicate our analyses based on the *QCEW State-Level Sample* with AICPA MAP survey data and document similar findings. We find that, relative to state average pre-treatment levels, wages decrease by 3.4% subsequent to CPA Mobility adoption (Column (1)), which imply a USD 2,564 wage decrease. In Column (2) we document a decline in billing rates ($BillingRate_{s,w}^{MAP}$), whereas in Column (3) we show that the effect on hours charged ($Log(HoursCharged^{MAP}_{s,w})$) is insignificant. The combined finding on billing rates and hours charged is consistent with the wage and employment-level effects that we document in our *QCEW State-Level Sample* analyses.

Quite interestingly, the coefficient magnitude on the policy indicator in the wage regression is substantially higher than our prior estimates based on QCEW program data. This could be due to our prior results underestimating the true wage effect of CPA Mobility because QCEW program data do not allow us to estimate effects for accounting professionals *only*, while the *AICPA MAP Survey Sample* does. Importantly, there are also differences in the population of CPA firms forming the aggregated wage statistics we use in our analyses. In particular, while wages paid by large audit-service providers (e.g., Big 4 firms) are included in our *QCEW State-Level Sample*, these are not part of our *AICPA MAP Survey Sample* (see Section 1.8.1 for details on how we identify whether Big 4 firms are part of our samples). Unlike small-sized local audit-service providers (e.g., non-Big 4 firms), Big 4 firms operate through national networks and hence bypass interstate licensing restrictions. Therefore, Big 4 firms are less likely to be affected by the introduction of CPA mobility provisions.

Next, we test for differences in policy effects conditional on accounting professional *seniority*. In Panel C, we examine the effect of CPA mobility on wage dispersion, measured as *logratios*, which we compute as the natural logarithm of the ratio of wages across different seniority levels (e.g., natural logarithm of senior-level wages to junior-level wages).[26] Our results show that the effect of CPA Mobility on wages is stronger for high-seniority personnel (Columns (1) and (2)). This result partly reflects the fact that more senior accounting professionals within CPA firms, because of their longer

[26] Logratios are commonly used in labor economics studies as a measure of wage dispersion (e.g., Autor et al., 2008; 2016).

tenure, are more likely to hold a CPA license. Also, the stronger effect on wages for more senior CPAs is consistent with their compensation entailing a higher proportion of variable pay, which is typically more responsive to shocks.[27] Our results on billing rates (Panel B, Column (2)) are consistent with the latter.

In Panels D and E, we focus on billing rates and hours charged, respectively. We find declines in billing rates only relative to junior accounting professionals, which is consistent with the effect being more pronounced for accounting professionals holding a CPA license. Lastly, our results for hours charged (Panel E) do not provide any evidence for differential policy impact across seniority levels.

Collectively, our results show that CPA Mobility effects are more pronounced for senior professionals, which suggests that wages become more homogenous after the policy adoption.

1.4.7 CPA Mobility Effect on Service Prices

Prior literature argues that licensing-induced geographic barriers prevent licensees from competing across state lines and, ultimately, drive up service prices. To explore whether wage declines reported in our prior analyses are accompanied by declines in service prices, we investigate the effects of CPA Mobility on audit fees. In addition, we examine whether such effects, if any, differ between *national audit firms*, which

[27] There are two potential concerns with this analysis: (i) there may be concurrent changes in compensation structures and (ii) CPAs may adjust wage structures to receive preferable tax treatments for their total compensation packages to compensate for wage decreases in pre-tax compensation. The QCEW program data and the AICPA MAP survey report total wages as opposed to limiting wages to, for instance, fixed wage components, which allays the first concern. To address the second concern, we examine whether including state-level income tax rates alters our results and find this not to be the case (results are untabulated for brevity). We focus on income tax rates since the vast majority of CPA firms are organized as either S Corporations, Sole Proprietorships, or Partnerships. To assess the distribution of legal structures of CPA firms in the United States, we use Census CBP program data that reports the number of establishments by industry classification, state, year, legal form, and size class. Our (untabulated) results reveal that less than 10% of all CPA firms are organized as Corporations, for which profits are taxed at the company level. We graphically show the shares of CPA firm legal structures for different (BLS-defined) size classes in Figure 1.OA-1.

operate in nation-wide networks, and *local audit firms*, which are typically smaller and tend to operate on a more local basis. Colbert and Murray (2013) point out that, while audit firms operating more locally regard CPA Mobility as a source of increased competition, CPA Mobility should not affect national audit firms whose (national) networks already allowed them to circumvent the licensing-induced barriers removed by the policy adoption.

To investigate the effect of CPA Mobility on service prices and potential differences between national and local audit firms, we require a standardized service provided by both types of audit firms. Hence, we focus on limited scope pension plan audits, which are fairly homogenous in terms of engagement complexity (AICPA, 2018). Moreover, unlike mandatory financial statement audits that are mainly provided by national audit firms, these services are provided by both national and local firms.[28] To this end, we collect private employee benefit plan files available from the Employee Benefit Security Administration (EBSA) of the Department of Labor. In the United States, most private employee benefit plans are subject to mandatory audits according to Section 103(a)(3) of the Employee Retirement Income Security Act (ERISA). These private pension plan audits are provided by both audit-service providers operating in nation-wide networks, as well as by small-sized local audit-service providers. We define national audit firms using Statista's list of "national accounting firms." The data construction details for our *Private Pension Plan Audit Sample* are provided in Appendix 1.B.

[28] Based on Audit Analytics data, the average (fee-weighted) Big 4 market share in the mandatory financial statement audit segment amounts to 65% (90%). This, in turn, highlights that the mandatory financial statement audit market segment is not a suitable setting for our analysis since it is mainly dominated by large audit-service providers operating in nation-wide networks. Nonetheless, we construct a sample based on Audit Analytics data and estimate our generalized DiD model augmented with control variables frequently used in studies on audit fee determinants (e.g., DeFond and Zhang, 2014). As expected, given the dominance of Big 4 firms in the mandatory financial statement audit segment, we do not find an effect of CPA Mobility provisions on fees.

In Table 1.7, Panel A, we present the descriptive statistics for our *Private Pension Plan Audit Sample*. The mean (median) plan-level audit fees in our sample amount to around USD 17,243 (USD 12,000). The average national audit firm market share is 30% in our sample, which is considerably lower than their market share in the mandatory financial statement audit market segment.

To examine the effects of CPA Mobility on pension plan audit fees, we estimate a version of the generalized DiD model presented in Section 1.3 augmented with the control variables proposed by Cullinan (1997), who investigates the determinants of pension plan audit fees. We provide detailed variable definitions for these control variables in Appendix 1.A. In addition, we include: (i) state × audit firm type fixed effects to account for audit firm type heterogeneity (local vs. national) across states; and (ii) audit firm type × year fixed effects to control for time-varying audit firm characteristics.

In Table 1.7, Panel B, we present the results of our analysis. The estimate presented in Column (1) suggests that, relative to the pre-treatment period, pension plan audit fees decline by 1.7% subsequent to the introduction of CPA Mobility, which implies a USD 277 decrease. This result is consistent with our prior findings suggesting both wage and billing rate declines. In Column (2), we investigate whether the reported decline in pension plan audit fees varies conditional on the type of audit firm. Given that national audit firms operate in nation-wide networks and have licensed personnel in every state, the average effect reported in Column (1) is likely driven by local audit firms. In line with our expectations, the coefficient magnitude is more negative for pension plans audited by local audit firms, suggesting fee declines of 2.2% on average relative to pre-treatment levels (Column (2)), which implies that audit fees on average decrease by USD 359.

Overall, our analysis investigating the effect of CPA Mobility on service prices is in line with the view that licensing-induced geographic barriers prevent licensees from competing across state lines and, ultimately, drive up service prices. In addition, we find that service price declines are only observable for local audit-service providers.

1.4.8 CPA Mobility Effect on Service Quality

Since the *raison d'être* for occupations to be organized through licensing regulations is to ensure minimum quality standards (Leland, 1979), in our last set of tests we assess whether the removal of geographic barriers affects service quality. Along with increased wage and fee pressure, the provisions of CPA Mobility may induce quality deterioration in the services provided by accounting professionals.

A number of reasons suggest that such service quality deterioration should not obtain. First, we do not observe substantial changes in the initial licensing requirements during our sample period. Second, CPA Mobility includes a "no escape" provision, which gives adopting states direct jurisdiction over out-of-state CPAs providing in-state services. Third, as pointed out by Lynch and McDonnell (2008), the removal of notification or application requirements should free up resources that State Boards of Accountancy could allocate to enforcement. Fourth, UAA provisions effectively require CPAs engaging in cross-border service provision to have "substantially equivalent" qualifications.

Notwithstanding the abovementioned reasons that speak against service quality deterioration, we take a three-pronged approach to explore this possibility in our last series of tests. First, we obtain data on AICPA misconduct cases from Jack Armitage and Shane Moriarity and examine if the frequency of misconduct cases changes after CPA mobility adoption. Armitage and Moriarity (2016) examine AICPA disciplinary actions from 1980 to 2014. AICPA misconduct cases provide a direct measure for the adherence to professional standards of CPAs. The AICPA's enforcement process is designed to identify and sanction, if necessary, substandard professional services by either admonishment, suspension of membership, or termination of the membership. AICPA membership is automatically terminated when a member is convicted of a crime, or a CPA license is suspended or revoked by the issuing jurisdiction of the license. Since the AICPA is the largest CPA association in the United States, AICPA misconduct cases provide a suitable sample for assessing professional standard adherence for a large number of CPAs. The dataset construction details for our *AICPA Misconduct Dataset* are presented in Appendix 1.B. In Table 1.8, Panel A, we present the results of this analysis. Since our dependent variable is the *count* of misconduct cases per state-year and AICPA misconducts are a low-frequency events (Armitage and Moriarity, 2013), we report coefficient estimates based on Poisson regression models in addition to ordinary least squares (OLS) estimates. We find no evidence suggesting that the introduction of CPA Mobility is associated with deteriorating service quality. If anything, our results suggest an increase in service quality (i.e., a decline in misconduct cases).

Second, we investigate whether the declines in pension plan audit fees reported in Section 1.4.7 are associated with pension plan audit-service quality deterioration. To investigate this possibility, we construct a sample of EBSA deficient filer enforcement cases using EBSA data. Deficient filers are plans that do not adhere to ERISA's Form 5500 annual reporting requirements and, therefore, provide a suitable sample to investigate potential pension plan audit-service quality effects. The sample construction details for our *EBSA Deficient Filer Sample* are presented in Appendix 1.B. In Table 1.8, Panel B, we present the results of this analysis, which are inconsistent with a negative effect of CPA Mobility on service quality.

Third, following Vetter (2020), we collect CPA firm license and disciplinary action data for the population of CPA firms in the state of Colorado whose State Board of Accountancy makes these data accessible for all its CPA firms. Combining disciplinary action and CPA firm license data allows us to estimate firm-level disciplinary action *probabilities*—as opposed to incident *counts*—which helps to address the concern that our previous service quality findings based on AICPA data could be driven by lack of statistical power. The sample construction details for our *CPA Firm Disciplinary Action Sample* are presented in Appendix 1.B.

An inherent limitation of relying on data from one state only is, however, that we cannot compare firm-level disciplinary action probabilities across states.[29] Nevertheless, as the competitive effects of CPA Mobility should entirely accrue to (and derive from) small CPA firms—as large CPA firms could already circumvent licensing barriers because of their national networks—we design empirical tests in which we estimate service quality deterioration effects on small CPA firms using large CPA firms as a control group. Moreover, because CPA firm-level data on size are not available, we assume that younger CPA firms are on average smaller than older CPA firms and operationalize CPA firm size by using age, which we gather via tracking entries to and exits from the profession.[30]

In Panel C, we present the results of our tests assessing whether younger CPA firms experience a change in disciplinary actions compared to older CPA firms subsequent to the adoption of CPA Mobility provision in the state of Colorado. Using within state-year variation only—that is, holding constant state-level oversight regimes

[29] Relative comparisons are crucial for our quality analysis as State Boards of Accountancy frequently remove disciplinary action incidents after some years, which results in mechanical increases in (observed) disciplinary action incidents over time.
[30] We gauge the validity of this assumption by collecting additional data from the Census Business Dynamics Statistics program, which provides employment data disaggregated by firm age for professional services firms (Census Sector Code 70 "Professional Services"). Using firm age and firm size data based on the Census definition of firm age categories, we find a strong positive correlation between firm age and firm size. This strong positive correlation provides reassurance that CPA firm age is a sensible proxy for CPA firm size.

as well as related factors that may correlate with disciplinary action incidents—we document, again, no effect on service quality.

In sum, across the different empirical approaches described above, we find no evidence suggesting that the introduction of CPA Mobility is associated with deteriorating service quality. We acknowledge, however, that we cannot observe service quality directly, but rather capture "extreme cases" of poor quality.[31]

1.5 Conclusion

In this paper, we explore the effects of removing spatial occupational licensing restrictions on the labor market for accounting professionals by exploiting the staggered introduction of CPA Mobility provisions in the United States. We document substantial wage declines subsequent to CPA Mobility. We find these effects to be persistent over time, to stem from small local CPA firms, and to be more pronounced for accounting professionals holding more senior positions. Furthermore, our analysis of service prices reveals sizable audit fee declines, which are only observable for small-sized local CPA firms. The increased wage and audit fee pressure does not seem to be accompanied by deteriorating service quality, however.

Our study caters to the current regulatory debate on the potential costs resulting from spatial occupational licensing restrictions. Our findings may inform the ongoing debate within the public accounting profession and among the profession's regulatory bodies on the desirability of further reforms regarding mobility provisions. More generally, they may be relevant to a broader audience considering reforms in a variety of occupations subject to licensing. For example, our results may prove helpful in guiding

[31] In addition, we cannot trace back the exact timing of the misconduct leading to AICPA or EBSA investigations. We address this shortcoming by re-estimating all models by lagging our policy indicators to allow for later manifestations of lower quality detection and find similar results (untabulated).

regulatory efforts to reform the licensing requirements for legal professionals as recently proposed by Winston and Karpilow (2016).

1.6 Appendix 1.A: Variable Definitions

Variable	*Definition*
$CPAWageDifferential_s$	The pre-treatment difference between wages paid to accounting professionals in state s relative to the national average (Source: QCEW variable "avg_annual_pay"). We calculate $CPAWageDifferential_s$ in 2005, that is, before the first of our sample states adopts CPA Mobility.
$CPAEmploymentDifferential_s$	The pre-treatment difference employed accounting professionals in state s relative to the national average (Source: QCEW variable "annual_avg_emplvl"). We calculate $CPAEmploymentDifferential_s$ in 2005, that is, before the first of our sample states adopts CPA Mobility.
$CPAWageTrend_s$	Five-year accounting professional wage trends (Source: QCEW variable "avg_annual_pay"). Trends are calculated from 2000 to 2005, that is, before the first of our sample states adopts CPA Mobility.
$CPAEmploymentTrend_s$	Five-year accounting professional employment trends (Source: QCEW variable "avg_annual_pay"). Trends are calculated from 2000 to 2005, that is, before the first of our sample states adopts CPA Mobility.
$CPABoardMembers_s$	The number of CPA members in public practice on the State Board of Accountancy in state s relative to the number of total board members (Source: Survey data of Colbert and Murray (2013)).
$LocalCPABoardMembers_s$	The number of CPA members in public practice working in local (non-national) CPA firms relative to $CPABoardMembers_s$ (Source: Survey data of Colbert and Murray (2013)).
$MobilityTaskForce_s$	An indicator variable equal to one if state s has a representative in the NASBA's "Mobility Task Force," and zero otherwise (Source: Hand collection from NASBA's Annual Reports).
$FundingAutonomy_s$	An indicator variable equal to one if the State Board of Accountancy in state s has funding autonomy (Source: Hand collection from State Board of Accountancy bylaws, state legislation, and survey data of Colbert and Murray (2013)).
$Unemployment_{s,t}$	Unemployment rate for state s in year t defined as total unemployment divided by the total labor force in state s in year t (Source: BLS LAUS).
$GDPPerCapita_{s,t}$	Real GDP per Capita in state state s in year t (Source: Bureau of Economic Analysis (BEA)).
$FirmBirth_{s,t}$	The net number of new establishments in state s in year t (Source: Census Business Dynamics Statistics (BDS).
$JobBirth_{s,t}$	The net number of new jobs created in state s in year t (Source: Census BDS).
$SenateDemocrats_{s,t}$	The share of democrats in the State Senate in state s in year t (Source: Hand collection from the Book of States Archive at the Council of State Governments).
$HouseDemocrats_{s,t}$	The share of democrats in the State House or Assembly in state s in year t (Source: Hand collection from the Book of States Archive at the Council of State Governments).
$BillsIntroduced_{s,t}$	The number of bills introduced in state s in year t (Source: Hand collection from the Book of States Archive at the Council of State Governments).
$BillsEnacted_{s,t}$	The number of bills enacted in state s in year t (Source: Hand collection from the Book of States Archive at the Council of State Governments).

Appendix 1.A (continued)

$Wage_{s,t}^{QCEW}$	State-year annual wage mean in state *s* in year *t* (Source: QCEW variable name "avg_annual_pay").
$Log(Wage_{s,t}^{QCEW})$	Natural logarithm of $Wage_{s,t}^{QCEW}$.
$Employment_{s,t}^{QCEW}$	Employment level for state *s* in year *t* (Source: QCEW variable name "annual_avg_emplvl").
$Log(Employment_{s,t}^{QCEW})$	Natural logarithm of $Employment_{s,t}^{QCEW}$.
$CPAMobility_{s,t}$	An indicator variable switched on the year CPA Mobility becomes effective in state *s* and thereafter, and zero otherwise. Effective dates for each state are reported in Table 1.1.
$WithinImmigration_{s,t}$	The ratio of American Community Survey (ACS) respondents in state *s* in year *t* indicating they moved to state *s* from another state within the United States (Source: ACS Public Use Microdata Samples).
$AbroadImmigration_{s,t}$	The ratio of ACS respondents state *s* in year *t* indicating they moved to state *s* from abroad (Source: ACS Public Use Microdata Samples).
$NeighborCPAMobility_{s,t-1}$	Employment weighted neighbor CPA Mobility adoption of state *s*.
$Wage_{s,t}^{SUSB}$	State-year annual average wage in state s, firm size category *j*, and year *t* (Source: SUSB).
$Log(Wage_{s,t}^{SUSB})$	Natural logarithm of $Wage_{s,j,t}^{SUSB}$.
$Employment_{s,t}^{SUSB}$	Employment level in state *s*, firm size category *j*, and year *t* (Source: SUSB).
$Log(Employment_{s,t}^{SUSB})$	Natural logarithm of $Employment_{s,j,t}^{SUSB}$.
$AvgEmployment_{s,t}^{SUSB}$	Average employment per establishment in state s, firm size category *j*, and year *t* calculated as $Employment_{s,j,t}^{SUSB}$ divided by $Firms_{s,j,t}^{SUSB}$(Source: SUSB).
$Log(AvgEmployment_{s,t}^{SUSB})$	The natural logarithm of $AvgEmployment_{s,j,t}^{SUSB}$.
$Firms_{s,t}^{SUSB}$	The number of establishments in state *s*, firm size category *j*, and year *t* (Source: SUSB).
$Log(Firms_{s,t}^{SUSB})$	The natural logarithm of $Firms_{s,t}^{SUSB}$.
$Small_j$	An indicator variable equal to one for firms with less than 20 employees, and zero otherwise.
CPA_o	An indicator variable equal to one for CPA firms, and zero otherwise (NAICS code 541211).
$Wage_{c,b,s,t}^{QCEW}$	County-year annual wage mean in county *c* at border *b* located in state *s* in year *t* (Source: QCEW variable name "avg_annual_pay").
$Log(Wage_{c,b,s,t}^{QCEW})$	Natural logarithm of $Wage_{c,b,s,t}^{QCEW}$.
$Employment_{c,b,s,t}^{QCEW}$	Employment level in county *c* at border *b* located in state *s* in year *t* (Source: QCEW variable name "annual_avg_emplvl").
$Log(Employment_{c,b,s,t}^{QCEW})$	Natural logarithm of $Employment_{c,b,s,t}^{QCEW}$.
$Unemployment_{c,b,s,t}$	Unemployment rate for county *c* at border *b* located in state *s* in year *t* defined as total unemployment divided by the total labor force in county *c* at border *b* located in state *s* in year *t* (Source: BLS LAUS).

Appendix 1.A (continued)

Variable	*Definition*
$Wage^{QCEW}_{m,t}$	MSA-year average wage in MSA *m* and year *t* (Source: QCEW).
$Log(Wage^{QCEW}_{m,t})$	Natural logarithm of $Wage^{QCEW}_{m,t}$.
$\Delta Log(Wage^{QCEW}_{m,t})$	First difference of $Log(Wage^{QCEW}_{m,t})$.
$GDPperCapita_{\,m,t}$	MSA-year average GDP per capita (Source: BEA).
$Log(GDPperCapita_{\,m,t})$	Natural logarithm of $GDPperCapita_{\,m,t}$.
$\Delta Log(GDPperCapita_{\,m,t})$	First difference of $Log(GDPperCapita_{\,m,t})$.
$PostAdoption_t$	An indicator variable equal to one after the last of our sample states adopts CPA Mobility provisions, and zero otherwise.
$\sigma\left(\Delta Log\left(Wage^{QCEW}_{m,o,t}\right)\right)_{o,t}$	The standard deviation of $\Delta Log\left(Wage^{QCEW}_{m,o,t}\right)$ calculated across MSAs for industry *o* in year *t*.
$IQR\left(\Delta Log\left(Wage^{QCEW}_{m,o,t}\right)\right)_{o,t}$	The interquartile range of $\Delta Log\left(Wage^{QCEW}_{m,o,t}\right)$ calculated across MSAs for industry *o* in year *t*.
$\sigma\left(Log\left(Wage^{QCEW}_{m,o,t}\right)\right)_{o,t}$	The standard deviation of $Log\left(Wage^{QCEW}_{m,o,t}\right)$ calculated across MSAs for industry *o* in year *t*.
$IQR\left(Log\left(Wage^{QCEW}_{m,o,t}\right)\right)_{o,t}$	The interquartile range of $Log\left(Wage^{QCEW}_{m,o,t}\right)$ calculated across MSAs for industry *o* in year *t*.
$Wage^{MAP}_{s,w}$	Survey-year average annual wage over all positions in state *s* in survey-year *w*.
$Log(Wage^{MAP}_{s,w})$	Natural logarithm of $Wage^{MAP}_{s,w}$.
$WageSenior^{MAP}_{s,w}$	Survey-year average annual wage for senior-level positions in state *s* in survey-year *w*.
$WageMid^{MAP}_{s,w}$	Survey-year average annual wage for mid-level positions in state *s* in survey-year *w*.
$WageJunior^{MAP}_{s,w}$	Survey-year average annual wage for junior-level positions in state *s* in survey-year *w*.
$BillingRate^{MAP}_{s,w}$	Survey-year average hourly billing rate for all positions in state *s* in survey-year *w*.
$BillingRateSenior^{MAP}_{s,w}$	Survey-year average hourly billing rate for senior-level positions in state *s* in survey-year *w*.
$BillingRateMid^{MAP}_{s,w}$	Survey-year average hourly billing rate for mid-level positions in state *s* in survey-year *w*.
$BillingRateJunior^{MAP}_{s,w}$	Survey-year average hourly billing rate for junior-level positions in state *s* in survey-year *w*.
$HoursCharged^{MAP}_{s,w}$	Survey-year average hours charged for all positions in state *s* in survey-year *w*.
$Log(HoursCharged^{MAP}_{s,w})$	Natural logarithm of $HoursCharged^{MAP}_{s,w}$.
$HoursChargedSenior^{MAP}_{s,w}$	Survey-year average hours charged for senior-level positions in state *s* in survey-year *w*.
$HoursChargedMid^{MAP}_{s,w}$	Survey-year average hours charged for mid-level positions in state *s* in survey-year *w*.
$HoursChargedJunior^{MAP}_{s,w}$	Survey-year average hours charged for juniors-level positions in state *s* in survey-year *w*.

Appendix 1.A (continued)

Variable	*Definition*
$CPAMobility_{s,w}^{MAP}$	Due to the biennial structure of the AICPA MAP Survey we have to align the effective dates with the survey-years. We move effective dates to the next year a survey-year is available. For instance, CPA Mobility became effective in Texas in 2007. We code $CPAMobility_{s,w}^{MAP}$ as equal to one for Texas in 2008 and thereafter, and zero otherwise.
$AuditFees_{p,s,t}$	Audit Fees (Source: Form 5500 Schedule C).
$Log(AuditFees_{p,s,t})$	Natural logarithm of $AuditFees_{p,s,t}$.
$Log(Assets_{p,s,t})$	Natural logarithm of total plan assets (Source: Form 5500 Schedule H).
$Contributions_{p,s,t}$	Total contributions divided by the total number of plan assets (Source: Form 5500 Schedule H).
$Hardtoaudit_{p,s,t}$	Assets invested in joint ventures and real estate divided by total plan assets (Source: Form 5500 Schedule H).
$InvestmentFees_{p,s,t}$	Investment management fees divided by total plan assets (Source: Form 5500 Schedule H).
$Income_{p,s,t}$	Plan income divided by total plan assets (Source: Form 5500 Schedule H).
$CPAMobility_{a,s,t-1}^{LocalAuditFirm}$	An indicator variable equal to one if $CPAMobility_{s,t}$ is switched on and the pension plan is audited by a local audit firm, and zero otherwise. We define national audit firms as firms that are not listed in Statista's list of top-10 audit firms.
$CPAMobility_{a,s,t-1}^{NationalAuditFirm}$	An indicator variable equal to one if $CPAMobility_{s,t}$ is switched on and the pension plan is audited by a national audit firm, and zero otherwise. We define national audit firms as firms that are listed in Statista's list of top-10 audit firms.
$Cases_{s,t}^{AM}$	Number of AICPA misconduct cases in state *s* in year *t.*
$WeightedCases_{s,t}^{AM}$	Number of AICPA misconduct cases weighted by severity in state *s* in year *t*.
$Cases_{s,t}^{EBSA}$	Number of EBSA Deficient Filer enforcement cases in state *s* in year *t.*
$WeightedCases_{s,t}^{EBSA}$	Number of EBSA Deficient Filer enforcement cases weighted by severity in state *s* in year *t.*
$DisciplinaryAction_{i,t}$	An indicator variable equal to one if CPA firm *i* is subject to a disciplinary action in year *t* (Source: Collection from Colorado's State Board of Accountancy following the approach of Vetter (2020).
$YoungFirm_{i}$	An indicator variable equal to one if firm *i*'s age is below the median firm age in 2007, and zero otherwise.

1.7 Appendix 1.B: Data and Samples

1.B.1 QCEW State-Level Dataset

We obtain state-year data from the BLS Quarterly Employment and Wage Statistics (QCEW) Annual Average Files (BLS QCEW Aggregation Level Code 58) based on the North American Industry Classification System (NAICS) Code 541211 ("Offices of Certified Public Accountants").[32] The NAICS-based disaggregation allows us to identify wages and employment in firms that fall under a definition that follows the UAA's definition of CPA firms almost verbatim. QCEW data are based on unemployment insurance filings that every establishment is required to file for purposes of calculating payroll taxes related to unemployment insurance. Since 98% of all workers in the United States are covered by unemployment insurance, the QCEW program constitutes a near-census of employment and wages (Dube et al., 2010). We restrict QCEW state-level data to privately-owned establishments (QCEW Ownership Code 5). We obtain data for all states (and the District of Columbia) adopting CPA Mobility within our sample period from 2003 to 2017.

We merge wage and employment QCEW program data with information on state-level macroeconomic conditions. In particular, we obtain information on unemployment rates from the BLS Local Area Unemployment Statistics (LAUS) program and information on real GDP per capita from the Bureau of Economic Analysis' Regional Economic Accounts program. Our immigration controls are based on the American Community Survey (ACS) Public Use Microdata Sample (PUMS). We merge these data sources for the years from 2003 to 2017. Our final *QCEW State-Level Sample* comprises 720 state-year observations. All variables used in the *QCEW State-Level Sample* are

[32] For a recent study using this data source to examine audit markets, see Duguay et al. (2020).

denoted by the superscript *QCEW*. Variables are further denoted by the subscript s and t, where s indicates the respective state and t the year.

We further augment this dataset with data for NAICS Code 541110 (“Offices of Lawyers”) following the steps we outline above.

1.B.2 SUSB State-Level Dataset

We obtain state-year data from the Census Statistics of U.S. Business (SUSB) program based on the NAICS Code 541211 (“Offices of Certified Public Accountants”). The NAICS-based disaggregation allows us to identify wages and employment in firms that fall under a definition that follows the UAA’s definition of CPA firms almost verbatim. We collect data for the aggregate firm size categories “<20 employees” and “20-99 employees.” We first obtain data for all states (and the District of Columbia) adopting CPA Mobility within our sample period for the years from 2007 to 2015, that is, the entire period for which SUSB data disaggregated by six-digit NAICS Codes are available. Then, we require availability of wage and employment data throughout the sample period for both firm size categories and merge these data with information on both state-level macro conditions and migrations patterns (as outlined in Section 1.B.1). Based on these sample selection criteria, our *SUSB State-Level Sample* comprises 369 state-year observations for each size category. All variables used in the *SUSB State-Level Sample* are denoted by the superscript *SUSB*. Variables are further denoted by the subscript s and t, where s indicates the respective state and t the year.

We further augment this dataset with data for NAICS Code 541110 (“Offices of Lawyers”) following the steps we outline above.

1.B.3 QCEW Boarder-County Dataset

We obtain county-year data from the BLS QCEW Annual Average Files (BLS QCEW Aggregation Level Code 78) based on the NAICS Code 541211 ("Offices of Certified Public Accountants"). These data cover all counties located in states (and the District of Columbia) adopting CPA Mobility within our sample period from 2003 to 2017. We restrict QCEW county-level data to privately-owned establishments (QCEW Ownership Code 5). We further restrict QCEW county-level data to contiguous counties located in different states (border counties). Border counties are identified using the Census Bureau's County Adjacency File. We follow Dube et al. (2010) and require availability of data for each county for the entire period from 2003 to 2017. In our QCEW state-level dataset, we do not have to impose such restrictions as data do not fall under BLS confidentiality and are disclosed for all states (and the District of Columbia). Since QCEW county-level data provide a significantly more detailed geographic disaggregation allowing for easier identification of the firms, employees, or self-employed, we face such disclosure restrictions in this sample.[33] Finally, we restrict QCEW county-level data to border segments, where the two states forming the segment exhibit different effective policy implementation dates.

Imposing the data availability screens outlined above, we construct a county-year panel of wage and employment information. We merge this panel with county-level unemployment rates to control for county-level time-varying macroeconomic conditions that may affect the outcome of interest. Unemployment rates are obtained from the BLS LAUS files. To identify individual border segments, we also merge this dataset with border segment information provided by Thomas Holmes.[34] Thomas Holmes provides

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[33] Detailed information on BLS confidentiality regulation and according disclosures are outlined at: https://www.bls.gov/bls/confidentiality.htm.

[34] The files are provided at: http://users.econ.umn.edu/~holmes/data/BorderData.html.

numerical identifiers for each border segment. A border segment is defined as the shared border between two states. Finally, we restrict the data to border segments with different treatment timings for the states forming the border segment. Our final *QCEW Border-County Sample* comprises 3,285 county-year observations. All variables used in the *QCEW Border-County Sample* are denoted by the superscript *QCEW*. Variables are further denoted by the subscripts c and b, where c indicates the respective county and b the border. Furthermore, subscript s denotes the state in which each county is located and t denotes the respective year.

1.B.4 QCWE MSA-Level Dataset

We obtain annual Metropolitan Statistical Area (MSA) data from the BLS QCEW Annual Average Files (BLS QCEW Aggregation Level Code 48) based on the NAICS Code 541211 ("Offices of Certified Public Accountants") and NAICS Code 541110 ("Offices of Lawyers"). These data cover all MSAs. We restrict QCEW MSA-level data to privately-owned establishments (QCEW Ownership Code 5). Further, we collect data from 2002 to 2017 to obtain four-year estimation samples for the period before the first of our sample states adopts CPA Mobility (i.e., 2002-2005) and after the last of our sample states adopts CPA Mobility (i.e., 2014-2017). We then merge these data with MSA-level information on GDP per capita obtained from the BLS LAUS program. We further require MSA-industry-level data availability for at least five years. This yields our *QCEW MSA-Level Sample* comprising 2,536 observations. All variables used in the *QCEW MSA-Level Sample* are denoted by the superscript *QCEW*. Variables are further denoted by the subscript p, m, and t, where p indicates the industry, m the MSA, and t the year.

1.B.5 AICPA MAP Survey Dataset

This dataset is based on the biennial American Institute of CPAs (AICPA) Management of an Accounting Practice (MAP) Survey. We obtain all available state-level reports from the AICPA for the years, 2002, 2004, 2006, 2008, 2010, 2012, and 2014. We hand-collect wage information for each state for which we have at least 5 survey waves available. We require data availability for wages, billing rates, and hours charged for all positions (senior-level, mid-level, and junior-level). We merge these data with CPA Mobility adoption dates. To account for the biennial structure of the survey, we move each effective policy implementation date to the respective next available survey year for cases where the implementation year and survey waves are not aligned. This procedure leads to our *AICPA MAP Survey Sample* which entails 129 observations. All variables used in the *AICPA MAP Survey Sample* are denoted by the superscript *MAP*. Variables are further denoted by subscripts *s* and *w*, where *s* indicates the respective state and *w* the survey-year.

1.B.6 Private Pension Plan Audit Dataset

This dataset is based on private pension plan data we hand-collect from the Employee Benefit Security Administration (EBSA) of the Department of Labor.[35] We individually download every single file from EBSA. From these files, we select Form 5500, Schedule C, and Schedule H. Schedule H contains plan-level financial information as well as the plan auditor. We identify plan auditors based on Employer Identification Numbers (EIN) provided on Schedule H. We then obtain audit fee information from Schedule C. Schedule C contains fee information for all service providers providing services to the respective pension plan. We merge the information from Schedules H and

[35] Private pension plan data are available at: https://www.dol.gov/agencies/ebsa/about-ebsa/our-activities/public-disclosure/foia/form-5500-datasets.

C using a combination of EBSA filing identifiers and EINs to obtain plan-level audit fees. We merge by EIN numbers in addition to filing identifiers since pension plans have multiple service providers. Finally, we merge these data with Form 5500 data using EBSA filing identifiers to obtain plan-level information on plan administrators, which we require to assign our policy intervention variable. Finally, we limited the sample to plans that are subject to mandatory audits, that is, plans with 100 or more participants, and restrict the sample to "limited scope" audits to hold the underlying audit service constant. This procedure yields our *Private Pension Plan Audit Sample*. Variables based on this dataset are denoted by subscripts p, s, and t, where p indicates the plan, s the state the plan is located in, and t the year.

1.B.7 AICPA Misconduct Dataset

This dataset is based on AICPA misconduct cases as identified in Armitage and Moriarity (2016). We augment this information with hand-collected data from the AICPA website and, for earlier years, AICPA misconduct notifications from *The CPA Letter*. Misconduct notifications are part of the "Disciplinary Actions" sections of *The CPA Letter*. *The CPA Letter* issues for the years from 2003 to 2008 are available through the archives of the University of Mississippi. All variables in the *AICPA Misconduct Sample* are denoted by the superscript AM. Variables are further denoted by subscripts s and t, where s indicates the respective state and t the year.

1.B.8 EBSA Deficient Filer Dataset

This dataset is based on EBSA Enforcement Data provided by the Department of Labor.[36] The original dataset consists of closed cases that resulted in penalty assessments

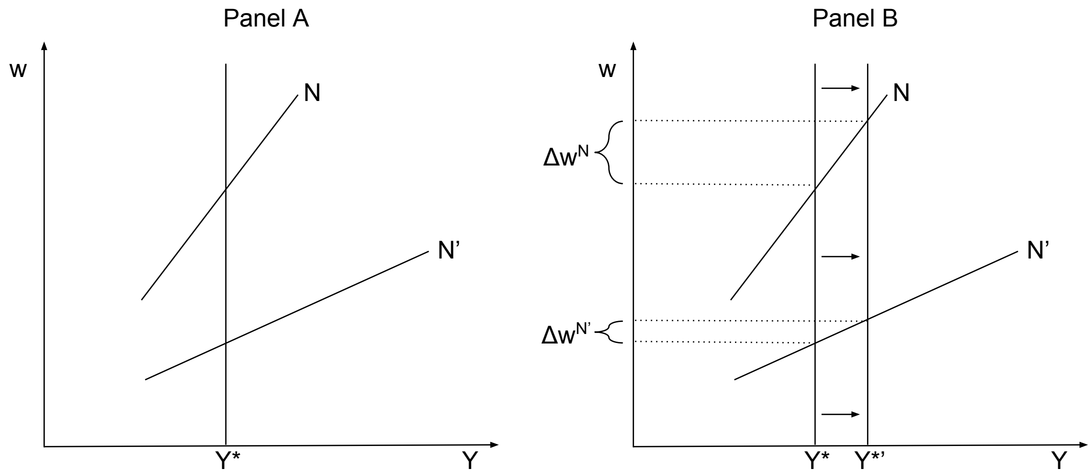
[36] The original EBSA enforcement files are available at: https://enforcedata.dol.gov/views/data_catalogs.php.

by EBSA since 2000. These data provide information on EBSA's enforcement programs to enforce ERISA's Form 5500 Annual Return/Report filing requirement focusing on deficient filers, late filers, and non-filers. We restrict the original data to cover the years from 2003 to 2015. This yields our *EBSA Deficient Filer Sample.* All variables in the *EBSA Deficient Filer Sample* are denoted by the superscript *EBSA*. Variables are further denoted by subscripts *s* and *t*, where *s* indicates the respective state and *t* the year.

1.B.9 CPA Firm Disciplinary Action Dataset

This dataset is based on CPA firm license data collected from the Colorado State Board of Accountancy (available at: https://www.colorado.gov/pacific/dora/Accountancy). We collect data on all CPA firm licenses along with disciplinary action filings from this website. CPA firm license data include information on the firm license issue and expiration dates, alongside information on addresses, as well as data on disciplinary actions brought forward against each CPA firm. We use data on firm license issue and exit dates to construct a panel of active CPA firms in Colorado during the period from 2003 to 2015. We require CPA firms to be active at least one year prior to Colorado's CPA Mobility adoption in 2007. We then merge 247 disciplinary action incidents occurring in Colorado during our sample period with this CPA firm panel. This yields our *CPA Firm Disciplinary Action Sample.* All variables in the *CPA Firm Disciplinary Action Sample* are denoted by the superscript *DA*. Variables are further denoted by subscripts *i* and *t*, where *i* indicates CPA firm *i* and *t* the year.

Figure 1.1: Aggregate Supply and Supply Elasticity



This figure shows the supply curves for two (exogenous) numbers of accounting professionals in the market, N and N', where $N < N'$, based on our simple model presented in Section 1.2.3. In Panel A, we see that a market with N' accountants exhibits lower wages as well as a more elastic (flatter) supply curve *vis-à-vis* a market with N accountants. In Panel B, we visualize a supply elasticity effect of increasing N to N'. An exogenous shock to demand from Y^* to $Y^{*\prime}$ results in a larger change in wages when assuming a supply of N accountants in the market, i.e., $\Delta w^N > \Delta w^{N'}$.

Figure 1.2: CPA Mobility Wage Effect in Event-Time

Panel A: CPA Mobility Effect on Wages in Event-Time (All CPA Firms)

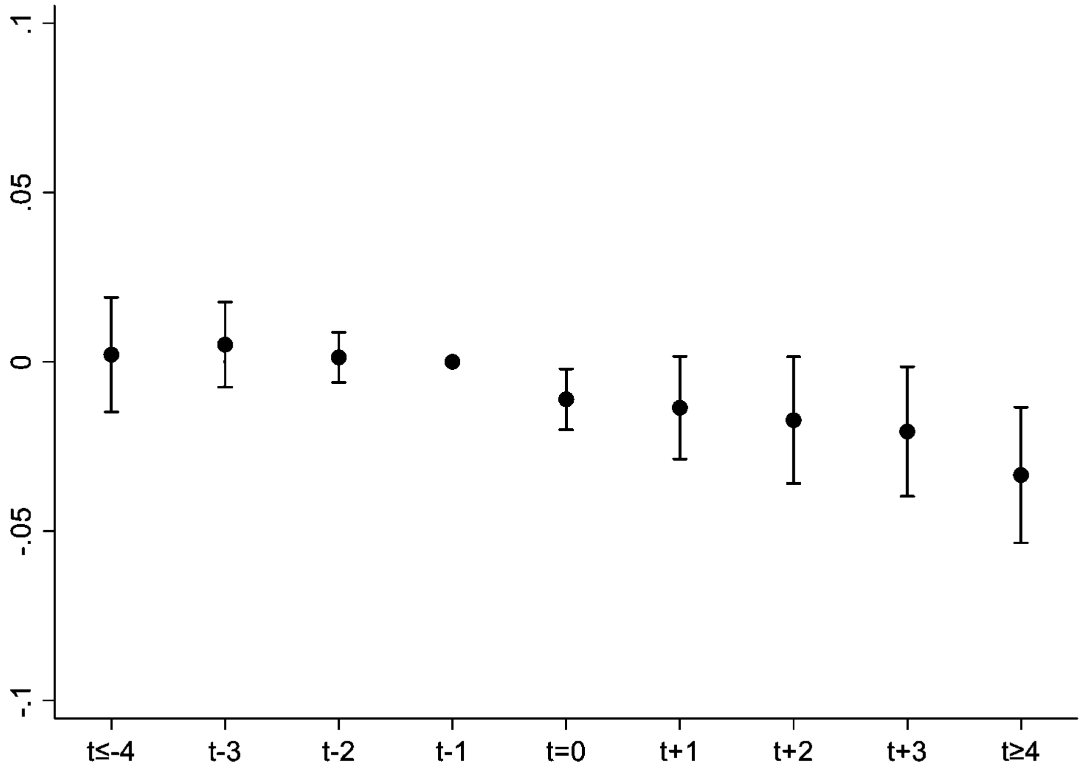


Figure 1.2 (continued)

Panel B: CPA Mobility Effect on Wages in Event-Time (Small vs. Large CPA Firms)

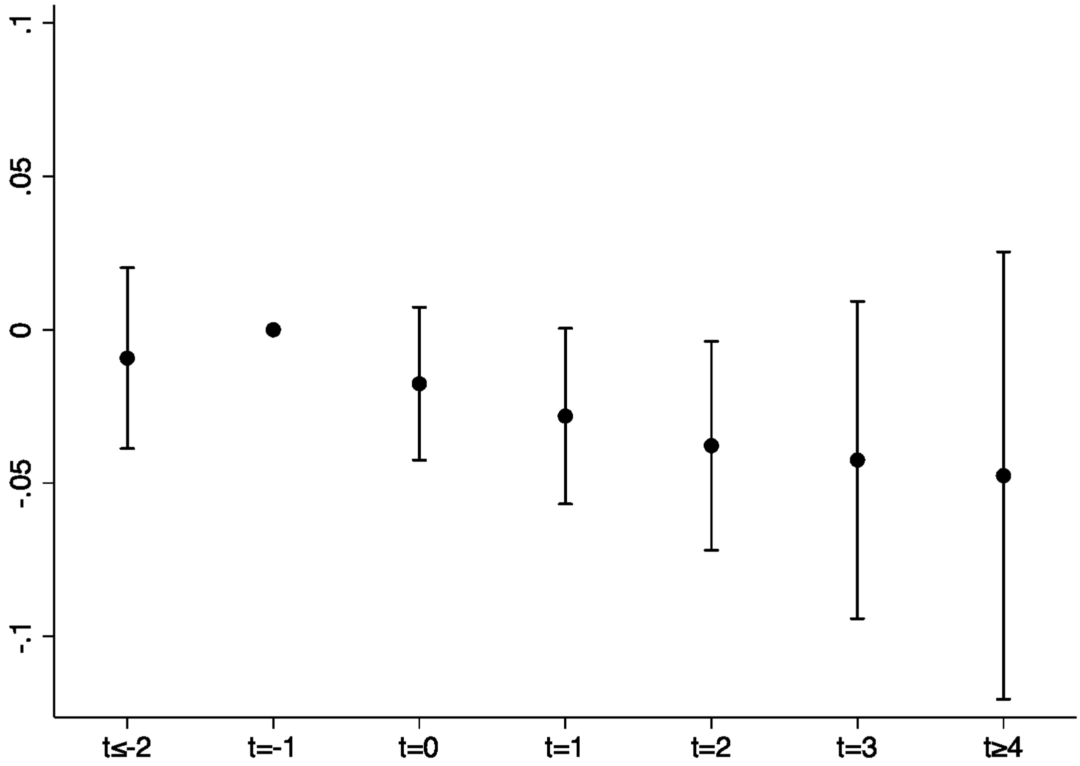
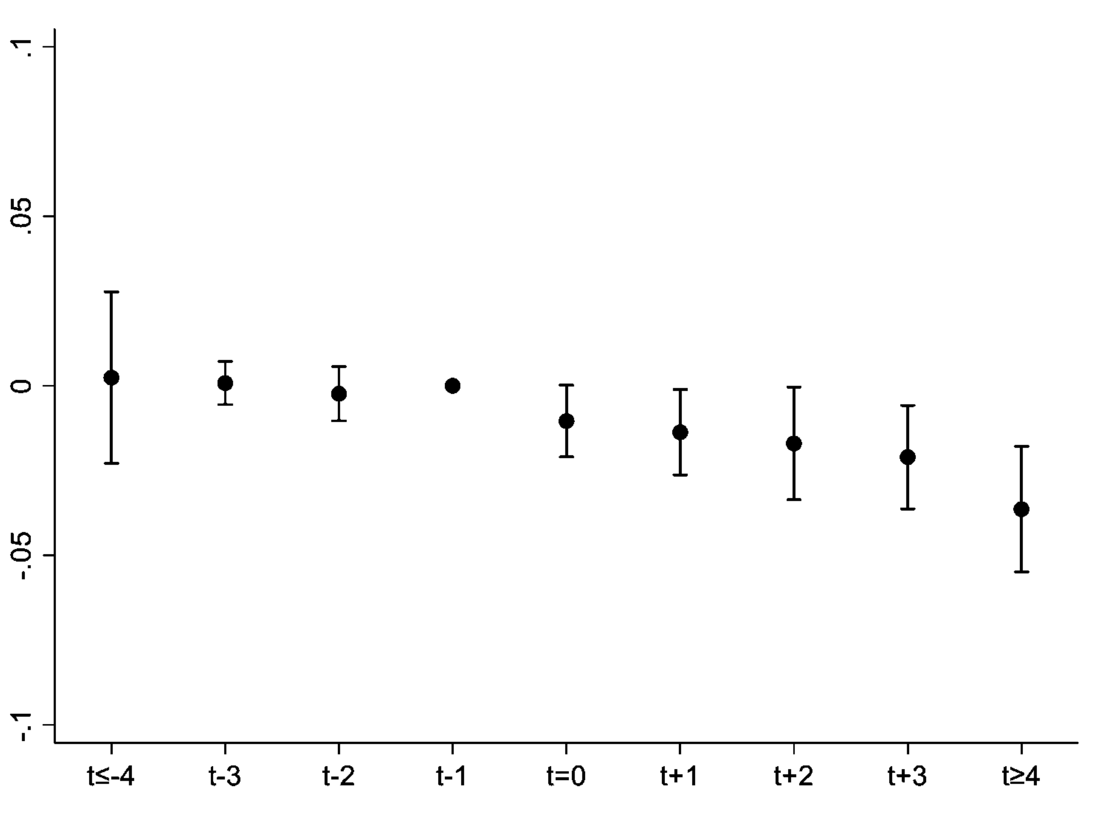


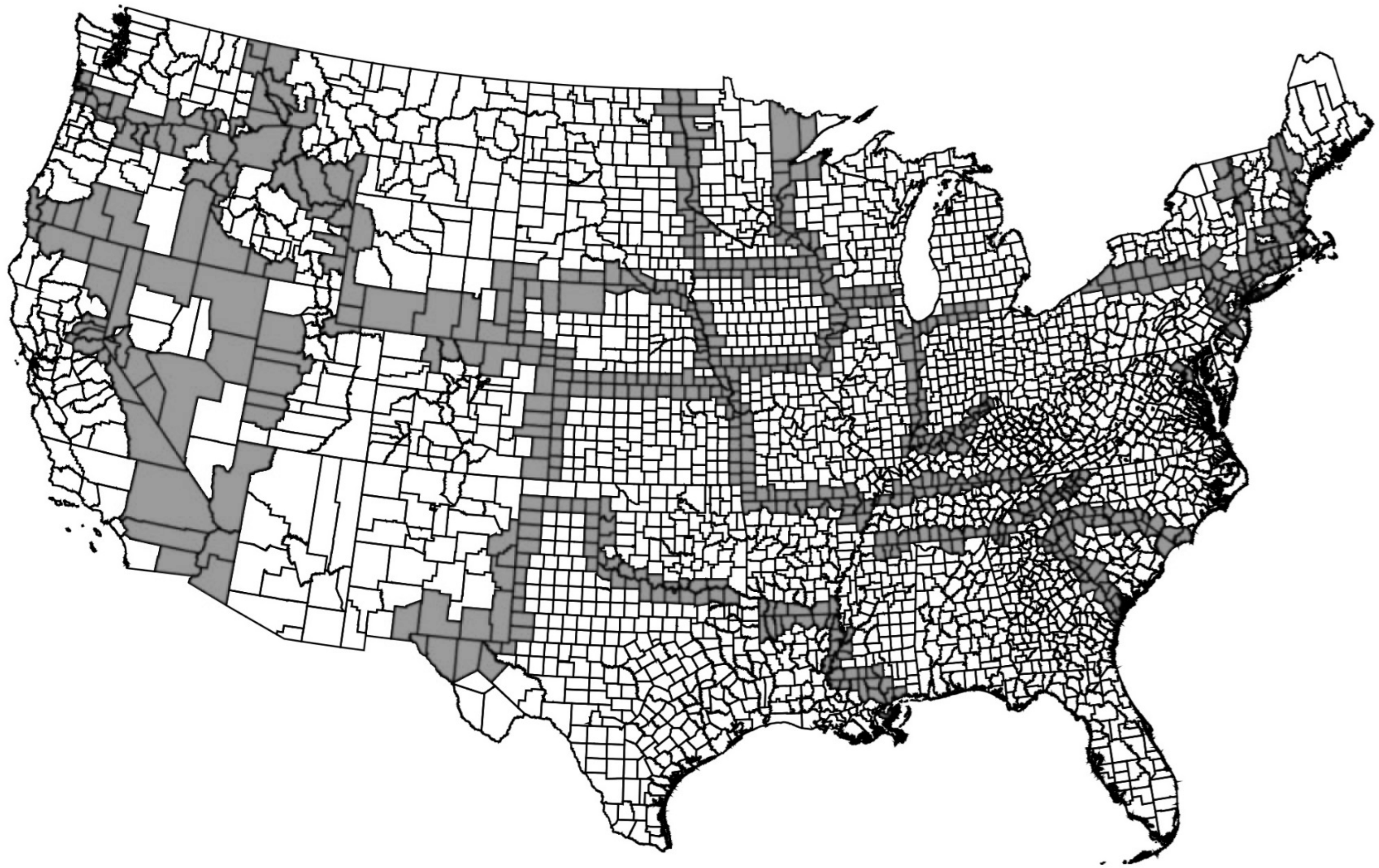
Figure 1.2 (continued)

Panel C: CPA Mobility Effects on Wages in Event-Time (Accounting Professionals vs. Legal Professionals)



This figure reports the coefficients of ordinary least squares (OLS) regressions, which we use to investigate CPA Mobility effects on wages in event-time. This analysis is based on our *QCEW State-Level Sample* (Panels A and C) and *SUSB State-Level Sample* (Panel B). In Panel A, we estimate $Log(Wage^{QCEW}_{s,t}) = \beta CPAMobility_{s,t-1} + \partial' X_{s,t-1} + \alpha_s + \gamma_t + \varepsilon_{s,t}$, but replace the policy indicator variable with separate event-time dummies, each marking a period relative to the policy announcement (*t=0*). We omit the indicator for *t-1*, which serves as benchmark period and include a set of state-year control variables ($X_{s,t-1}$). In Panel B, we show event-time CPA Mobility effects on wages for accounting professionals in small CPA firms relative to wages for accounting professionals in large CPA firms. Formally, we estimate $Log(Wage^{SUSB}_{s,j,t}) = \beta CPAMobility_{s,t-1} \times Small_j + \alpha_{s,j} + \gamma_{s,t} + \gamma_{j,t} + \varepsilon_{p,s,t}$, but replace the policy indicator variable with separate event-time dummies, each marking a period relative to the policy announcement (*t=0*). We omit the indicator for *t-1*, which serves as benchmark period. In Panel C, we show the event-time CPA Mobility effects on wages for accounting professionals relative to legal professionals. Formally, we estimate $Log(Wage^{QCEW}_{s,o,t}) = \beta CPAMobility_{s,t-1} \times CPA_o + \alpha_{s,o} + \gamma_{s,t} + \gamma_{o,t} + \varepsilon_{o,s,t}$, but replace the policy indicator variable with separate event-time dummies, each marking a period relative to the policy announcement (*t=0*). We omit the indicator for *t-1*, which serves as benchmark period. Vertical bands represent 95% confidence intervals for the point estimates in each event-time period and are calculated based on standard errors clustered at the state level.

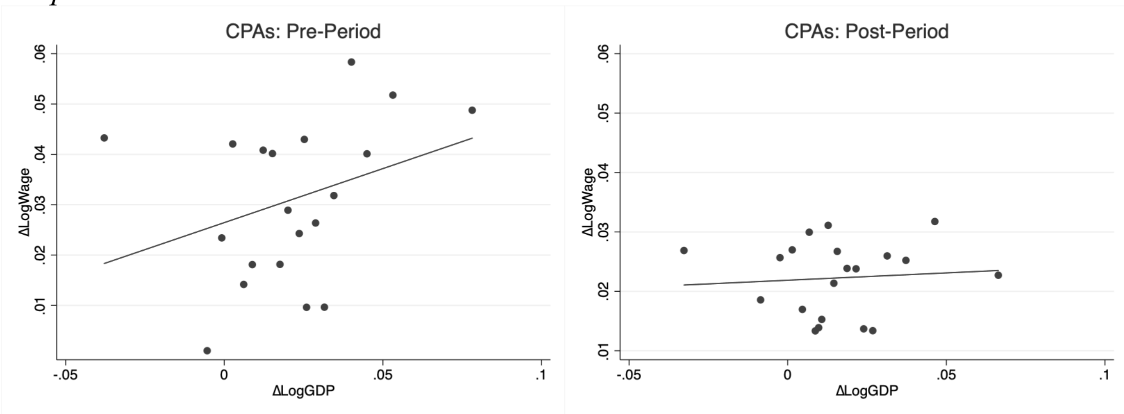
Figure 1.3: Border-Counties with Non-Overlapping Treatment Dates



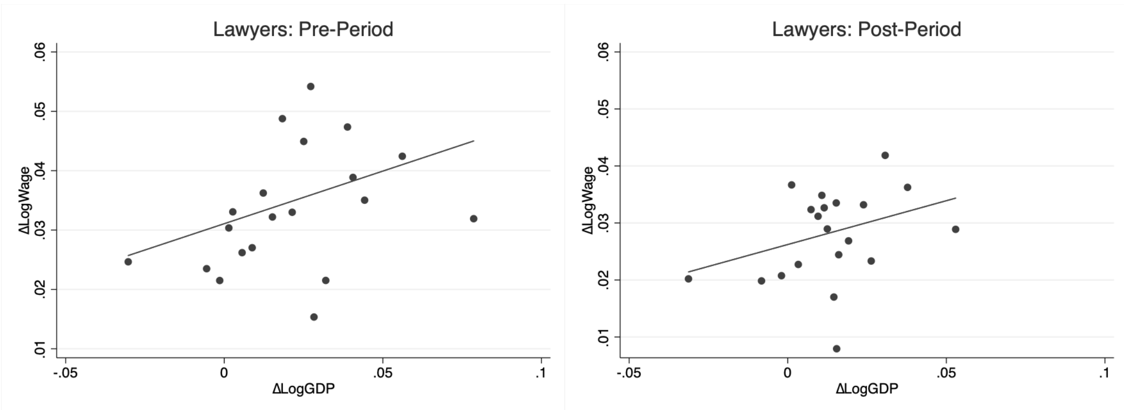
This figure shows contiguous counties located at border segments with non-overlapping treatment dates of the states forming the border segment.

Figure 1.4: CPA Mobility and Wage Sensitivities to Local Economic Conditions

Panel A: Wage Sensitivities of CPAs Before the First and After the Last CPA Mobility Adoption



Panel B: Wage Sensitivities of Legal Professionals Before the First and After the Last CPA Mobility Adoption



This figure plots the relation between changes in CPA wages and changes in GDP at the MSA-level for the period before the first of our sample states adopts CPA Mobility (i.e., 2002-2005) and after the last of our sample states adopts CPA Mobility (i.e., 2014-2017). In Panel A, we plot the relation between changes in CPA wages and changes in GDP while in Panel B, we plot the relation between changes in lawyer wages and changes in GDP.

Table 1.1: CPA Mobility Adoption Sequence and Adoption Determinants

Panel A: CPA Mobility Adoption Dates

State #	State	Effective Date	Enactment Date
1	Wisconsin	Apr-06	Apr-06
2	Tennessee	Apr-07	Apr-07
3	Texas	Jun-07	Jun-07
4	Indiana	Jul-07	May-07
5	Rhode Island	Jul-07	Jul-07
6	Maine	Sep-07	Jun-07
7	Louisiana	Dec-07	Dec-07
8	Illinois	Jan-08	Aug-07
9	Minnesota	Apr-08	Apr-08
10	Missouri	Apr-08	Jan-08
11	Connecticut	May-08	May-08
12	New Mexico	May-08	Feb-08
13	Utah	May-08	Mar-08
14	Michigan	Jun-08	Jun-08
15	South Carolina	Jun-08	Jun-08
16	Washington	Jun-08	Mar-08
17	West Virginia	Jun-08	Mar-08
18	Idaho	Jul-08	Mar-08
19	Kentucky	Jul-08	Apr-08
20	Colorado	Aug-08	May-08
21	Delaware	Aug-08	Aug-08
22	Arizona	Sep-08	Jun-08
23	Pennsylvania	Sep-08	Jul-08
24	Maryland	Oct-08	May-08
25	Oklahoma	Apr-09	Apr-09
26	Oregon	Jun-09	Jun-09
27	Arkansas	Jul-09	Feb-09
28	Florida	Jul-09	May-09
29	Georgia	Jul-09	Jun-08
30	Iowa	Jul-09	Apr-08
31	Mississippi	Jul-09	Mar-08
32	Nevada	Jul-09	Apr-09
33	New Hampshire	Jul-09	Jun-09
34	New Jersey	Jul-09	Jul-08
35	North Carolina	Jul-09	Jul-09
36	South Dakota	Jul-09	Mar-09
37	Vermont	Jul-09	May-09
38	Wyoming	Jul-09	Mar-09
39	North Dakota	Aug-09	Apr-08
40	Alabama	Oct-09	May-09
41	Montana	Oct-09	Apr-09
42	Kansas	Nov-09	Mar-09
43	Nebraska	Sep-10	Feb-09
44	Alaska	Jan-11	Apr-10
45	Massachusetts	Jun-11	Jan-10
46	New York	Nov-11	Sep-11
47	District of Columbia	Oct-12	Oct-12
48	California	Jul-13	Sep-12

Table 1.1 (continued)

Panel B: Determinants of CPA Mobility Adoption

	Dependent variable: $CPAMobilityAdoption$				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
CPA Macro Factors:					
$CPAWageDifferential_s$	0.731				0.350
	(0.474)				(0.494)
$CPAEmploymentDifferential_s$	0.942				0.832
	(0.129)				(0.175)
$CPAWageTrend_s$	0.367				0.130
	(0.809)				(0.447)
$CPAEmploymentTrend_s$	0.247				0.324
	(0.286)				(0.547)
CPA Political Economy:					
$CPABoardMembers_s$		0.678			0.599
		(0.418)			(0.656)
$LocalCPABoardMembers_s$		0.530			0.337**
		(0.235)			(0.166)
$MobilityTaskForce_s$		2.432**			2.356*
		(0.900)			(1.066)
$FundingAutonomy_s$		0.878			0.677
		(0.285)			(0.244)
General Macro Factors:					
$Unemployment_{s,t-1}$			0.842		0.848
			(0.153)		(0.133)
$GDPperCapita_{s,t-1}$			1.000		1.000
			(0.000)		(0.000)
$FirmBirth_{s,t-1}$			2.612		0.000
			(27.536)		(0.004)
$JobBirth_{s,t-1}$			0.914		0.847
			(0.121)		(0.143)
General Political Economy:					
$SenateDemocrats_{s,t-1}$				0.756	0.312
				(1.163)	(0.604)
$HouseDemocrats_{s,t-1}$				0.386	1.215
				(0.723)	(2.720)
$BillsIntroduced_{s,t-1}$				1.174	1.193
				(0.116)	(0.178)
$BillsEnacted_{s,t-1}$				1.080	1.248
				(0.174)	(0.329)
Observations	272	272	272	272	272
Pseudo R^2	0.005	0.016	0.005	0.016	0.046

This table reports CPA Mobility adoption dates as well as our analysis of adoption date determinants. Panel A reports the enactment and effective dates of CPA Mobility provisions obtained from the AICPA and the NASBA. States and the District of Columbia are ordered by effective dates. We present enactment and effective dates for all states adopting CPA Mobility provisions during our sample period from 2003 to 2017. Panel B reports the results of a Cox discrete time proportional hazard model analyzing the hazard of a state adopting CPA Mobility. We report hazard ratios and (in parentheses) standard errors. States are excluded from the sample after they adopt CPA Mobility. Detailed definitions of all variables are presented in Appendix 1.A. Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

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Table 1.2: State-Level Mobility Effect on Wages

Panel A: Descriptive Statistics for the QCEW State-Level Sample

	Obs.	Mean	S.D.	P1	P25	P50	P75	P99
$Wage_{s,t}^{QCEW}$	720	63,514	17,755	36,795	51,009	60,390	71,830	123,469
$Log(Wage_{s,t}^{QCEW})$	720	11.025	0.257	10.513	10.840	11.009	11.182	11.724
$Employment_{s,t}^{QCEW}$	720	7,984	10,203	552	1,669	4,480	8,577	48,325
$Log(Employment_{s,t}^{QCEW})$	720	8.366	1.116	6.314	7.419	8.407	9.057	10.786
$Unemployment_{s,t-1}$	720	6.065	1.995	2.900	4.600	5.650	7.200	11.300
$GDPPerCapita_{s,t-1}$	720	51,928	20,415	33,395	42,373	47,637	55,519	175,653
$WithinImmigration_{s,t-1}$	720	0.027	0.012	0.011	0.020	0.025	0.032	0.082
$AbroadImmigration_{s,t-1}$	720	0.004	0.002	0.001	0.003	0.004	0.006	0.014

Panel B: CPA Mobility Effect on Wages

	Dependent variable: $Log(Wage_{s,t}^{QCEW})$					
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)
$CPAMobility_{s,t-1}$	-0.011**	-0.011**	-0.011**	-0.011**	-0.012**	-0.010*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
<u>Macro Controls:</u>						
$Unemployment_{s,t-1}$		-0.000				0.003
		(0.002)				(0.002)
$GDPperCapita_{s,t-1}$			0.000**			0.000**
			(0.000)			(0.000)
<u>Migration Controls:</u>						
$WithinImmigration_s$				0.873		0.443
				(1.106)		(1.085)
$AbroadImmigration$					2.413	0.415
					(3.319)	(2.989)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	720	720	720	720	720	720
Adj. R^2	0.988	0.988	0.988	0.988	0.988	0.988

Table 1.2 (continued)

Panel C: CPA Mobility Effect on Employment:

	Dependent variable: $Log(Employment_{s,t}^{QCEW})$					
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)
$CPAMobility_{s,t-1}$	-0.005	-0.004	0.004	-0.004	-0.006	0.001
	(0.015)	(0.014)	(0.015)	(0.015)	(0.014)	(0.014)
Macro Controls:						
$Unemployment_{s,t-1}$		-0.017***				-0.010**
		(0.006)				(0.005)
$GDPperCapita_{s,t-1}$			0.000***			0.000**
			(0.000)			(0.000)
Migration Controls:						
$WithinImmigration_s$				1.707		-0.354
				(2.057)		(1.584)
$AbroadImmigration$					8.171	6.277
					(4.991)	(4.012)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	720	720	720	720	720	720
Adj. R^2	0.997	0.997	0.997	0.997	0.997	0.998

Table 1.2 (continued)

Panel D: Neighbor CPA Mobility Effect

	Dependent variable: $Log(Wage^{QCEW}_{s,t})$							
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$CPAMobility_{s,t-1}$	-0.010**		-0.009*		-0.010**		-0.009*	
	(0.004)		(0.005)		(0.004)		(0.005)	
$NeighborCPAMobility_{s,t-1}$	-0.011	-0.012	-0.012	-0.014	-0.011	-0.012	-0.012	-0.014
	(0.013)	(0.014)	(0.013)	(0.013)	(0.013)	(0.014)	(0.013)	(0.013)
Macro Controls	No	No	Yes	Yes	No	No	Yes	Yes
Migration Controls	No	No	No	No	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	720	720	720	720	720	720	720	720
Adj. R^2	0.988	0.987	0.988	0.988	0.988	0.987	0.988	0.988

This table presents the results of our state-level difference-in-differences (DiD) analysis of CPA Mobility effects on CPA wages and employment, which is based on the *QCEW State-Level Sample*. Panel A presents summary statistics for all variables. Detailed definitions for all variables and sample selection criteria are presented in Appendices 1.A and 1.B, respectively. Panel B documents the effect of CPA Mobility on wages. The reported coefficients and (in parentheses) standard errors are obtained from weighted least squares (WLS) regressions of $Log(Wage^{QCEW}_{s,t})$ on $CPAMobility_{s,t-1}$ and control variables, as indicated in each column. Regressions are weighted by state-year employment shares. State-year employment shares are defined as $Employment^{QCEW}_{s,t}$ divided by the sum of all $Employment^{QCEW}_{s,t}$ in year *t.* Panel C documents the effect of CPA Mobility on employment. The reported coefficients and (in parentheses) standard errors are obtained from ordinary least squares (OLS) regressions of $Log(Employment^{QCEW}_{s,t})$ on $CPAMobility_{s,t-1}$ and control variables, as indicated in each column. Panel D presents results of the analysis that examines the effect of regional CPA Mobility adoption patterns on wages. $NeighborCPAMobility_{s,t-1}$ is defined as the average treatment variable of neighbors weighed by the number of employees. Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.3: Difference-in-Difference-in-Differences Analysis of CPA Mobility Effects on Wages

Panel A: Within-State Control Group – Firm Size

	Dependent variables:			
	$Log(Wage_{s,j,t}^{SUSB})$	$Log(Employment_{s,j,t}^{SUSB})$	$Log(AvgEmployment_{s,j,t}^{SUSB})$	$Log(Firms_{s,j,t}^{SUSB})$
Independent variables:	(1)	(2)	(3)	(4)
$CPAMobility_{s,t-1} \times Small_j$	-0.019**	-0.015	0.012	-0.016
	(0.008)	(0.027)	(0.014)	(0.027)
State × Year FE	Yes	Yes	Yes	Yes
State × Firm Size FE	Yes	Yes	Yes	Yes
Firm Size × Year FE	Yes	Yes	Yes	Yes
Obs.	738	738	738	738
Adj. R^2	0.997	0.992	0.997	0.997

Panel B: Within-State Control Group – Legal Professionals

	Dependent variables:			
	$Log(Wage_{s,o,t}^{QCEW})$	$Log(Employment_{s,o,t}^{QCEW})$	$Log(AvgEmployment_{s,o,t}^{QCEW})$	$Log(Firms_{s,o,t}^{QCEW})$
Independent variables:	(1)	(2)	(3)	(4)
$CPAMobility_{s,t-1} \times CPA_o$	-0.009**	-0.016	0.003	-0.004
	(0.004)	(0.015)	(0.014)	(0.006)
State × Year FE	Yes	Yes	Yes	Yes
State × Profession FE	Yes	Yes	Yes	Yes
Profession × Year FE	Yes	Yes	Yes	Yes
Obs.	1,440	1,440	1,440	1,440
Adj. R^2	0.991	0.998	0.975	0.999

Table 1.3 (continued)

Panel C: Within-State Control Group – Firm Size and Legal Professionals

	Dependent variables:			
	$Log(Wage^{SUSB}_{s,j,o,t})$	$Log(Employment^{SUSB}_{s,j,o,t})$	$Log(AvgEmployment^{SUSB}_{s,j,o,t})$	$Log(Firms^{SUSB}_{s,j,o,t})$
Independent variables:	(1)	(2)	(3)	(4)
$CPAMobility_{s,t-1} \times Small_j \times CPA_o$	-0.014*	-0.020	0.016	-0.027
	(0.007)	(0.036)	(0.017)	(0.035)
State × Year × Firm Size FE	Yes	Yes	Yes	Yes
State × Year × Profession FE	Yes	Yes	Yes	Yes
State × Firm Size × Profession FE	Yes	Yes	Yes	Yes
Firm Size × Profession × Year FE	Yes	Yes	Yes	Yes
Obs.	1,476	1,476	1,476	1,476
Adj. R^2	0.998	0.999	0.999	0.997

This table presents the results of our state-level difference-in-difference-in-differences (DiDiD) analysis of CPA Mobility effects in which we use within-state control groups. Test results presented in Panels A and C (Panel B) are based on our *SUSB State-Level Sample* (*QCEW State-Level Sample*). Detailed definitions for all variables and sample selection criteria are presented in Appendices 1.A and 1.B, respectively. We present summary statistics for each control group in Table 1.OA-1. In Panel A, the reported coefficients and (in parentheses) standard errors are obtained from weighted least squares (WLS) regressions (Columns (1) and (3)) and OLS regressions (Columns (2) and (4)) of the respective dependent variable on $CPAMobility_{s,t-1} \times Small_j$ and control variables, as indicated in each column. The regression reported in Column (1) is weighted by state-year employment shares. State-year employment shares are defined as $Employment^{QCEW}_{s,t}$ divided by the sum of all $Employment^{QCEW}_{s,t}$ in year *t*. The regression reported in Column (3) is weighted by state-year firm shares. State-year firm shares are defined as $Firms^{SUSB}_{s,j,t}$ divided by the sum of all $Firms^{SUSB}_{s,j,t}$ in year *t*. In Panel B, the reported coefficients and (in parentheses) standard errors are obtained from weighted least squares (WLS) regressions (Columns (1) and (3)) and OLS regressions (Columns (2) and (4)) of the respective dependent variable on $CPAMobility_{s,t-1} \times CPA_o$ and control variables, as indicated in each column. The regression reported in Column (1) is weighted by state-year employment shares. The regression reported in Column (3) is weighted by state-year firm shares. In Panel C, the reported coefficients and (in parentheses) standard errors are obtained from weighted least squares (WLS) regressions (Columns (1) and (3)) and ordinary least squares (OLS) regressions (Columns (2) and (4)) of the respective dependent variable on $CPAMobility_{s,t-1} \times Small_j \times CPA_o$ and control variables, as indicated in each column. The regression reported in Column (1) is weighted by state-year employment shares. The regression reported in Column (3) is weighted by state-year firm shares. Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.4: Border-County Analysis

Panel A: Descriptive Statistics for the QCEW Border-County Sample

	Obs.	Mean	Std. Dev.	P1	P25	P50	P75	P99
$Wage^{QCEW}_{c,b,s,t}$	3,285	51,563	18,593	22,415	38,724	48,123	60,785	113,276
$Log(Wage)^{QCEW}_{c,b,s,t}$	3,285	10.791	0.343	10.017	10.564	10.782	11.015	11.638
$Employment^{QCEW}_{c,b,s,t}$	3,285	565	2,274	9	40	91	314	11,914
$Log(Employment^{QCEW}_{c,b,s,t})$	3,285	4.800	1.483	2.197	3.689	4.511	5.749	9.385
$Unemployment_{c,b,s,t-1}$	3,285	6.424	2.565	2.600	4.600	5.900	7.700	14.700
$GDPPerCapita_{s,t-1}$	3,285	50,800	12,424	33,616	43,962	48,534	56,847	74,031
$WithinImmigration_{s,t-1}$	3,285	0.023	0.009	0.011	0.016	0.022	0.029	0.054
$AbroadImmigration_{s,t-1}$	3,285	0.004	0.002	0.002	0.003	0.004	0.005	0.008

Panel B: CPA Mobility Effects on Wages and Employment

	Dependent variables:							
	$Log(Wage^{QCEW}_{c,b,s,t})$				$Log(Employment^{QCEW}_{c,b,s,t})$			
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$CPAMobility_{s,t-1}$	-0.015**	-0.014**	-0.013***	-0.014**	0.007	0.009	0.012	0.011
	(0.007)	(0.007)	(0.005)	(0.006)	(0.017)	(0.016)	(0.016)	(0.016)
Macro Controls	No	Yes	No	Yes	No	Yes	No	Yes
Migration Controls	No	No	Yes	Yes	No	No	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	3,285	3,285	3,285	3,285	3,285	3,285	3,285	3,285
Adj. R^2	0.981	0.981	0.983	0.983	0.989	0.989	0.989	0.989

This table presents summary statistics and the results of our border-county analysis, which is based on the *QCEW Border-County Sample*. Detailed definitions for all variables and sample selection criteria are presented in Appendices 1.A and 1.B, respectively. Panel A shows the descriptive statistics for all variables used in our border-county analysis. In Panel B, reported coefficients and (in parentheses) standard errors are from weighted least squares (WLS) regressions of $Log(Wage^{QCEW}_{c,b,s,t})$ and ordinary least squares (OLS) regressions of $Log(Employment^{QCEW}_{c,b,s,t})$ on $CPAMobility_{s,t-1}$ and control variables, as indicated in each column. Regressions in Columns (1) to (4) are weighted by county-year employment shares. Employment shares are defined as $Employment^{QCEW}_{c,b,s,t}$ divided by the sum of all $Employment^{QCEW}_{c,b,s,t}$ in year *t*. Macro Controls includes both

$Unemployment_{s,t-1}$, as well as $GDPperCapita_{s,t-1}$. Migration Controls includes both $WithinImmigration_{s,t-1}$, as well as $AbroadImmigration_{s,t-1}$. Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.5: CPA Mobility Effect on Wage Sensitivities to Local Economic Conditions

Panel A: CPA Mobility and Wage Sensitivities to Local Economic Conditions

	Dependent variable: $\Delta Log(Wage^{QCEW}_{m,t})$	
	Accounting Professionals	Legal Professionals
Independent variables:	(1)	(2)
$PostAdoption_t$	-0.005	-0.004**
	(0.004)	(0.002)
$\Delta Log(GDPperCapita_{m,t})$	0.214*	0.177***
	(0.118)	(0.063)
$PostAdoption_t \times \Delta Log(GDPperCapita_{m,t})$	-0.189*	-0.023
	(0.102)	(0.090)
Test for Difference in: $PostAdoption_t \times \Delta Log(GDPperCapita_{m,t})$		
χ^2-test [p-value]: $CPAs = Lawyers$	[0.215]	
Obs.	1,524	1,012
Adj. R^2	0.023	0.028

Panel B: CPA Mobility and Wage Volatility

	Dependent variables:	
	$\sigma\left(\Delta Log(Wage^{QCEW}_{m,o,t})\right)_{o,t}$	$IQR\left(\Delta Log(Wage^{QCEW}_{m,o,t})\right)_{o,t}$
Independent variables:	(1)	(2)
$PostAdoption_t$	-0.006**	0.000
	(0.002)	(0.005)
CPA_o	0.013**	0.018**
	(0.006)	(0.006)
$PostAdoption_t \times CPA_o$	-0.013*	-0.017**
	(0.006)	(0.007)
Obs.	16	16
Adj. R^2	0.581	0.437

Panel C: CPA Mobility and Wage Convergence

	Dependent variables:	
	$\sigma\left(Log(Wage^{QCEW}_{m,o,t})\right)_{o,t}$	$IQR\left(Log(Wage^{QCEW}_{m,o,t})\right)_{o,t}$
Independent variables:	(1)	(2)
$PostAdoption_t$	0.024***	0.129***
	(0.004)	(0.014)
CPA_o	0.008**	0.040
	(0.003)	(0.030)
$PostAdoption_t \times CPA_o$	-0.036***	-0.146***
	(0.005)	(0.032)
Obs.	16	16
Adj. R^2	0.796	0.683

This table presents the results of our analysis assessing the effects of CPA Mobility on wage sensitivities to local economic conditions, wage (growth) volatility, and wage convergence. Test results are based on our *QCEW MSA-Level Sample*. Detailed definitions for all variables and sample selection criteria are presented in Appendices 1.A and 1.B, respectively. We present summary statistics in Table 1.OA-1, Panel D. Panel A documents wage sensitivities for CPAs and Lawyers for the period before the first of our sample states adopts CPA Mobility (i.e., 2002-2005) and after the last of our sample states adopts CPA Mobility (i.e., 2014-2017). The reported coefficients and (in parentheses) standard errors are from weighted least squares (WLS) regressions of $\Delta Log\left(Wage_{m,t}^{QCEW}\right)$ on the interaction term $PostAdoption_t \times \Delta Log(GDPperCapita_{m,t})$, as well as control variables, as indicated in each column. Regressions are weighted by MSA-year employment shares. Standard errors are clustered at the MSA level. We report the p-value from a χ^2-test for the difference in the interaction term across the accounting professionals and legal professionals partitions. Panel B documents the effect of CPA Mobility on wage growth volatility. The reported coefficients and (in parentheses) standard errors are from ordinary least squares (OLS) regressions of $\sigma\left(\Delta Log\left(Wage_{m,o,t}^{QCEW}\right)\right)_{o,t}$ or $IQR\left(\Delta Log\left(Wage_{m,o,t}^{QCEW}\right)\right)_{o,t}$ on the interaction term $PostAdoption_t \times CPA_o$ and control variables, as indicated in each column. We report robust standard errors. Panel C documents the effect of CPA Mobility on wage convergence. The reported coefficients and (in parentheses) standard errors are from ordinary least squares (OLS) regressions of $\sigma\left(Log\left(Wage_{m,o,t}^{QCEW}\right)\right)_{o,t}$ or $IQR\left(Log\left(Wage_{m,o,t}^{QCEW}\right)\right)_{o,t}$ on the interaction term $CPA_o \times PostAdoption_t$ and control variables, as indicated in each column. We report robust standard errors. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.6: CPA Mobility and Wage Dispersion

Panel A: Descriptive Statistics for the AICPA MAP Survey Sample

	Obs.	Mean	Std. Dev.	P25	P50	P75
$Wage_{s,w}^{MAP}$	129	85,039	11,983	75,767	83,824	92,937
$Log(Wage_{s,w}^{MAP})$	129	11.341	0.139	11.235	11.336	11.440
$WageSenior_{s,w}^{MAP}$	129	173,252	29,374	154,058	170,241	186,174
$WageMid_{s,w}^{MAP}$	129	77,994	14,752	66,114	76,591	87,769
$WageJunior_{s,w}^{MAP}$	129	47,798	5,637	43,057	48,233	51,175
$BillingRate_{s,w}^{MAP}$	129	129	20	114	128	141
$BillingRateSenior_{s,w}^{MAP}$	129	172	24	155	172	185
$BillingRateMid_{s,w}^{MAP}$	129	139	27	118	137	158
$BillingRateJunior_{s,w}^{MAP}$	129	94	13	85	95	103
$HoursCharged_{s,w}^{MAP}$	129	1,422	64	1,381	1,421	1,464
$Log(HoursCharged_{s,w}^{MAP})$	129	7.259	0.045	7.231	7.259	7.289
$HoursChargedSenior_{s,w}^{MAP}$	129	1,288	95	1,228	1,289	1,350
$HoursChargedMid_{s,w}^{MAP}$	129	1,422	86	1,377	1,423	1,472
$HoursChargedJunior_{s,w}^{MAP}$	129	1,491	70	1,438	1,497	1,541

Table 1.6 (continued)

Panel B: Effects on Wages, Billing Rates, and Hours Charged

	Dependent variables:		
	$Log(Wage_{s,w}^{MAP})$	$BillingRate_{s,w}^{MAP}$	$Log(HoursCharged_{s,w}^{MAP})$
Independent variables:	(1)	(2)	(3)
$CPAMobility_{s,w}^{MAP}$	-0.034*	-5.188***	0.001
	(0.019)	(1.300)	(0.008)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Obs.	129	129	129
Adj. R^2	0.848	0.928	0.616

Panel C: Differential Effects on Compensation

	Dependent variables:		
	$Log\left(\frac{WageSenior_{s,w}^{MAP}}{WageJunior_{s,w}^{MAP}}\right)$	$Log\left(\frac{WageSenior_{s,w}^{MAP}}{WageMid_{s,w}^{MAP}}\right)$	$Log\left(\frac{WageMid_{s,w}^{MAP}}{WageJunior_{s,w}^{MAP}}\right)$
Independent variables:	(1)	(2)	(3)
$CPAMobility_{s,w}^{MAP}$	-0.059**	-0.074**	0.015
	(0.028)	(0.033)	(0.028)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Obs.	129	129	129
Adj. R^2	0.407	0.619	0.525

Table 1.6 (continued)

Panel D: Differential Effects on Billing Rates

	Dependent variables:		
	$Log\left(\frac{BillingRateSenior_{s,w}^{MAP}}{BillingRateJunior_{s,w}^{MAP}}\right)$	$Log\left(\frac{BillingRateSenior_{s,w}^{MAP}}{BillingRateMid_{s,w}^{MAP}}\right)$	$Log\left(\frac{BillingRateMid_{s,w}^{MAP}}{BillingRateJunior_{s,w}^{MAP}}\right)$
Independent variables:	(1)	(2)	(3)
$CPAMobility_{s,w}^{MAP}$	-0.037*	-0.011	-0.026**
	(0.018)	(0.020)	(0.011)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Obs.	129	129	129
Adj. R^2	0.519	0.711	0.636

Panel E: Differential Effects on Hours Charged

	Dependent variables:		
	$Log\left(\frac{HoursChargedSenior_{s,w}^{MAP}}{HoursChargedJunior_{s,w}^{MAP}}\right)$	$Log\left(\frac{HoursChargedSenior_{s,w}^{MAP}}{HoursChargedMid_{s,w}^{MAP}}\right)$	$Log\left(\frac{HoursChargedMid_{s,w}^{MAP}}{HoursChargedJunior_{s,w}^{MAP}}\right)$
Independent variables:	(1)	(2)	(3)
$CPAMobility_{s,w}^{MAP}$	0.024	0.033	-0.009
	(0.015)	(0.019)	(0.011)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Obs.	129	129	129
Adj. R^2	0.483	0.369	0.237

This table presents our analyses based on the *AICPA MAP Survey Sample*. Detailed definitions for all variables and sample selection criteria are presented in Appendices 1.A and 1.B, respectively. Panel A presents the summary statistics of all variable used in this analysis. In Panel B, we present estimates of our analysis examining the effect of CPA Mobility on wages, billing rates, and hours charged. In Panel C, we examine the differential effect of CPA Mobility across seniority levels on wages. In Panel D, we examine the differential effect of CPA Mobility across seniority levels on billing rates. In Panel E, we examine the differential effect of CPA Mobility across seniority levels on hours charged. Reported coefficients and (in parentheses) standard errors are from weighted least squares regressions. Regressions are weighted by the number of responding firms in state *s* in survey-year *w*. Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.7: CPA Mobility Effect on Service Prices

Panel A: Descriptive Statistics for the Private Pension Plan Audit Sample

	Obs.	Mean	S.D.	P1	P25	P50	P75	P99
$AuditFees_{p,s,t}$	30,501	17,243	17,332	4,590	8,088	12,000	19,304	96,706
$Log(AuditFees_{p,s,t})$	30,501	9.493	0.658	8.432	8.998	9.393	9.868	11.479
$NationalFirm_{p,s,t}$	30,501	0.277	0.447	0.000	0.000	0.000	1.000	1.000
$Contributions_{p,s,t}$	30,501	0.050	0.206	-0.935	-0.012	0.068	0.148	0.555
$Income_{p,s,t}$	30,501	0.000	0.001	0.000	0.000	0.000	0.000	0.002
$Hardtoaudit_{p,s,t}$	30,501	0.007	0.034	0.000	0.000	0.000	0.000	0.212
$Log(Assets_{p,s,t})$	30,501	0.273	1.459	0.000	0.038	0.077	0.126	5.656
$InvestmentFees_{p,s,t}$	30,501	17.090	1.790	13.249	15.831	16.939	18.256	21.736
$Participants_{p,s,t}$	30,501	0.002	0.003	0.000	0.000	0.001	0.003	0.013

Panel B: CPA Mobility Effect on Pension Plan Audit Fees

	Dependent variable: $Log(AuditFees_{p,a,s,t})$	
Independent variables:	(1)	(2)
$CPAMobility_{s,t-1}$	-0.017*	
	(0.010)	
$CPAMobility^{LocalAuditFirm}_{a,s,t-1}$		-0.022**
		(0.010)
$CPAMobility^{NationalAuditFirm}_{a,s,t-1}$		-0.009
		(0.032)
$Contributions_{p,s,t}$	0.051***	0.051***
	(0.005)	(0.005)
$Income_{p,s,t}$	-0.158***	-0.157***
	(0.031)	(0.031)
$Hardtoaudit_{p,s,t}$	0.480**	0.453*
	(0.226)	(0.225)
$Log(Assets_{p,s,t})$	0.137***	0.136***
	(0.005)	(0.005)
$InvestmentFees_{p,s,t}$	11.851***	11.448***
	(2.967)	(3.083)
$Participants_{p,s,t}$	16.553**	16.027**
	(6.646)	(6.472)
Test for Difference in $CPAMobility_{s,t-1}$		
F-test [p-value]: $LocalAuditFirm = NationalAuditFirm$		[0.092]
State × Audit Firm Type FE	Yes	Yes
Year FE	Yes	No
Audit Firm Type × Year FE	No	Yes
Obs.	30,501	30,501
Adj. R^2	0.544	0.547

This table presents the results of our analysis assessing the effect of CPA Mobility on service prices. Panel A presents summary statistics. Detailed definitions for all variables and sample selection criteria are presented in Appendices 1.A and 1.B, respectively. Continuous variables are winsorized at the 1st and 99th percentile. Panel B documents the effect of CPA Mobility on pension plan audits fees. The reported coefficients and (in parentheses) standard errors are from ordinary least squares (OLS) regressions of $Log(AuditFees_{p,s,t})$ on $CPAMobility_{s,t-1}$, or $CPAMobility^{LocalFirm}_{s,t-1}$ and $CPAMobility^{NationalFirm}_{s,t-1}$, and control variables, as indicated in each column. We report the p-value from an F-test for the difference between the coefficients on $CPAMobility^{LocalFirm}_{s,t-1}$ and $CPAMobility^{NationalFirm}_{s,t-1}$. Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.8: CPA Mobility and Service Quality

Panel A: CPA Mobility and AICPA Misconduct Cases

	Dependent variables:							
	$Cases_{s,t}^{AM}$				$WeightedCases_{s,t}^{AM}$			
	OLS	OLS	Poisson	Poisson	OLS	OLS	Poisson	Poisson
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$CPAMobility_{s,t-1}$	-1.320***	-0.071	-0.764**	0.024	-3.434***	-0.450	-0.790***	-0.028
	(0.482)	(0.347)	(0.305)	(0.221)	(1.168)	(0.817)	(0.288)	(0.242)
State FE	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	585	585	585	585	585	585	585	585
Adj. R^2 / Pseudo R^2	0.008	0.588	0.026	0.419	0.007	0.611	0.029	0.505

Panel B: CPA Mobility and EBSA Deficient Filer Enforcement Cases

	Dependent variables:							
	$Cases_{s,t}^{EBSA}$				$WeightedCases_{s,t}^{EBSA}$			
	OLS	OLS	Poisson	Poisson	OLS	OLS	Poisson	Poisson
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$CPAMobility_{s,t-1}$	-6.150	-1.139	-0.948***	-0.029	-9.336	-1.165	-0.913***	-0.005
	(4.599)	(0.834)	(0.308)	(0.063)	(6.926)	(1.053)	(0.308)	(0.079)
State FE	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	624	624	624	624	624	624	624	624
Adj. R^2 / Pseudo R^2	0.110	0.666	0.188	0.694	0.109	0.640	0.207	0.740

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Table 1.8 (continued)

Panel C: CPA Mobility and Disciplinary Actions

	Dependent variable: $DisciplinaryAction_{i,t}^{DA}$	
Independent variables:	(1)	(2)
$CPAMobility_{t-1}^{Colorado} \times YoungCPAFirm_i$	-0.002	-0.002
	(0.002)	(0.002)
Firm Age FE	Yes	No
Firm FE	No	Yes
Year FE	Yes	Yes
Obs.	13,401	13,401
Adj. R^2	0.006	0.012

This table presents the results of our analysis assessing the effect of CPA Mobility on service quality. Detailed definitions for all variables and sample selection criteria are presented in Appendices 1.A and 1.B, respectively. In Panel A, we report coefficient estimates and standard errors (in parentheses) from ordinary least squares (OLS) regressions as well as coefficient estimates and standard errors (in parentheses) from Poisson regressions of AICPA Misconduct Cases on $CPAMobility_{s,t-1}$ and control variables, as indicated in each column. Standard errors are clustered at the state level. In Panel B, we report coefficient estimates and standard errors (in parentheses) from ordinary least squares (OLS) regressions as well as coefficient estimates and standard errors (in parentheses) from Poisson regressions of EBSA Deficient Filer Enforcement Cases on $CPAMobility_{s,t-1}$ and control variables, as indicated in each column. Standard errors are clustered at the state level. In Panel C, we report coefficients estimates and standard errors (in parentheses) from ordinary least squares (OLS) regressions of Disciplinary Action Incidents on $CPAMobility_{Colorado,i,t-1} \times YoungCPAFirm_i$ and control variables, as indicated in each column. Standard errors are clustered at the CPA firm level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

1.8 Online Appendix

1.8.1 Big 4 Firm Sample Representation

In this section, we provide details on our triangulation strategy through which we assess whether Big 4 firms are part of our *QCEW State-Level Sample* and/or our *AICPA MAP Survey Sample* as discussed in Section 1.4.6.

First, we assess whether Big 4 firms are part of our *QCEW State-Level Sample*. Our *QCEW State-Level Sample* is based on data disaggregated by industry. The QCEW program assigns industries based on questionnaires.[37] While these questionnaires are not accessible, which prevents us from directly identifying the industry assignment of Big 4 firms used by government programs, we triangulate the industry assignment of Big 4 firms using Census County Business Pattern (CBP) program data. These data provide establishment counts at the ZIP code level (for different size classes) and utilize the same industry classification as our QCEW program data. We use CBP data to identify ZIP codes in which we observe only small CPA firm establishments (10 employees or less) and one large CPA firm establishment. We then conduct searches of CPA firm licenses for the respective ZIP code using the CPA license lookup function of the State Board of Accountancy the respective ZIP code belongs to.

To give an example, we start with the CBP data and search for ZIP Codes that have fewer than 10 CPA firms, of which there is one large firm (more than 500 employees) and, other than that, only small CPA firms (fewer than 20 employees). One of these ZIP Codes is "44133" in Ohio. This ZIP Code shows six CPA firms, of which five have fewer than 20 employees and one has 500 to 999 employees. We take this ZIP Code to the Ohio State Board of Accountancy and use its "License Lookup Function" to search for all CPA firms in this ZIP Code. The search result is shown below:

[37] For detailed information on the BLS industry assignments, see: https://www.bls.gov/cew/cewover.htm #Coverage.

License Search [back]

Select a Board	Accountancy Board
Select a Profession	FIRM.Firm - Financial Reporting (AICPA Peer Review)
Business Name/DBA	
-or- License Number	.
-or- Name (Last, First)	,
City, State Zip	Ohio 44113
County	- DISPLAY ALL -
Status	- DISPLAY ALL -
	Search Reset

Name	Type	City	State	Credential	Credential Status
COZZA & STEUER	COMPANY	CLEVELAND	OH	FIRM.44113001-PR	OUT OF BUSINESS
HARRIS, CHARLES E & ASSOCIATES INC	COMPANY	CLEVELAND	OH	FIRM.44113009-PR	ACTIVE
CSATARY, GEORGE CPA	COMPANY	CLEVELAND	OH	FIRM.44113072-PR	ACTIVE
FRANCK, J & CO	COMPANY	CLEVELAND	OH	FIRM.44113078-APR	OUT OF BUSINESS
VAZQUEZ, JOSE A CPA	COMPANY	CLEVELAND	OH	FIRM.44113084-TC	ACTIVE
HAWKINS, EDWARD C & CO LTD	COMPANY	CLEVELAND	OH	FIRM.44114042-APR	ACTIVE
ERNST & YOUNG LLP	COMPANY	CLEVELAND	OH	FIRM.44115001-APR	ACTIVE
HARVARD GROUP INC (THE)	COMPANY	CLEVELAND	OH	FIRM.44120020-PR	ACTIVE

We web search for each of these firms, which suggests that all firms in this ZIP Code are indeed small-sized local audit-service providers, the exception being Ernst and Young. Ernst and Young is likely to be the one firm in this ZIP Code with 500 to 999 employees. Specifically, Ernst & Young's "E&Y Tower" is located in this ZIP Code.

Second, we assess whether Big 4 firms are part of our *AICPA MAP Survey Sample*. The AICPA MAP Survey is distributed among firms of the AICPA Private Companies Practice Section (PCPS). We search available PCPS membership lists for Big 4 firms and do not find decisive membership information suggesting that the Big 4 are part of the AICPA MAP Survey.

Taken together, our triangulations suggest that Big 4 firms are included in our *QCEW State-Level Sample*, but are not included in our *AICPA MAP Survey Sample*.

1.8.2 Treatment Effect Stability

While our different fixed effects structures that we employ in the tests presented in Tables 1.3 and 1.4 already alleviate, to a great extent, a potential omitted variable bias in our empirical analysis, in this section we implement the bounding methodology proposed by Oster (2019) to assess the stability of our treatment effects and evaluate their robustness to omitted variable bias. Nonetheless, to further allay potential omitted variable concerns, we follow the bounding methodology developed by Oster (2019) to assess the stability of our treatment effects and evaluate their robustness to omitted variable bias. Specifically, we re-estimate our main model specification (Table 1.2, Panel B, Column (6)) with and without macro and migration control variables. We then assume a value for R^{max} (the R^2 from a hypothetical regression of the outcome on treatment and both observed and unobserved control variables) and, based on this assumption, calculate the value of delta (the relative degree of selection on observed and unobserved control variables) for which the treatment effect would be zero. Delta is a function of R^{max} and the change in the coefficient on $CPAMobility_{s,t-1}$ and R^2 as the control variables are included in the regression. Following the most conservative approach proposed by Oster (2019), we set R^{max} equal to 2 multiplied by the *within* R^2 of a regression that includes all controls (we calculate delta based on the within R^2 following Breuer et al. (2018) as our objective is to gauge the role of unmodelled (unobservable) state-year factors (following your suggestion)). We present the results of this analysis in Table 1.OA-3. Our delta of 7.864 suggests that the unobservables would need to be almost eight times as important as the observables to produce a treatment effect of zero. The magnitude of this delta value indicates that our treatment effect is unlikely to be driven by unobservable factors alone.

1.8.3 Within-State Synthetic Control Group

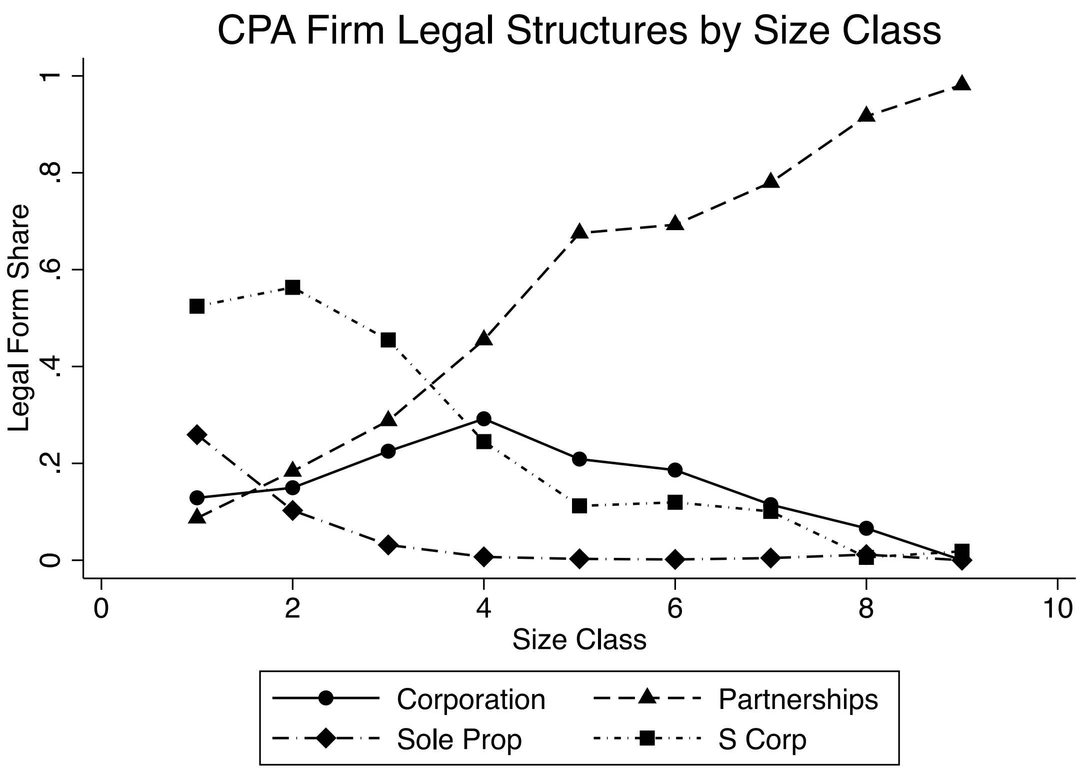
In our DiDiD tests, we also use "synthetic" control groups of CPAs based on other business professionals. This synthetic control group approach (Abadie and Gardeazabal, 2003; Abadie et al., 2010) offers a data driven method for choosing controls groups to use in (individual) treatment case studies. In particular, for each state that receives a policy treatment, the synthetic control is the weighted average of untreated states (or other potential "donor" groups) that best matches the treated state trends prior to the policy intervention. In our setting, in which all states are eventually treated and treatment dates are clustered in time, we lack untreated (donor) groups *within* the accounting profession. To overcome this issue, we separately estimate synthetic control group weights for each state drawing donor units from the NAICS top-code 54 "Professional, Scientific, and Technical Services." We restrict the donor group to "Professional, Scientific, and Technical Services" to ensure that we draw control units from industries that provide comparable services and to keep the computational requirements within feasible bounds.

Besides defining a pool of potential donors, the synthetic control approach requires us to specify periods over which trends between treated and (potential) control units are matched. We define these periods in two different ways. First, we define equal periods across all states, that is, we match trends from 2003 to 2006. This approach assures a comparable matching algorithm across states but, for some states, does not utilize all available pre-treatment years. Second, we also rely on a different approach, in which we match trends from 2003 until one year prior to a state's CPA Mobility adoption.

In Table 1.OA-4, Panels A and B, we report the sample weights for each donor industry. We tabulate the mean and median weights calculated across all sample states for each six-digit donor industry sorted by mean weights (from highest to lowest). Panel A reports the weights obtained from the approach imposing equal matching periods across states. Panel B reports the weights based on the approach using state-specific matching periods. We use the sample weights obtained from our two synthetic control approaches to calculate (weighted average) synthetic CPA state-years. Panel C presents summary statistics for both synthetic control group samples—that is "Synthetic CPA 1" and

"Synthetic CPA 2". Using these two synthetic control groups, we estimate DiDiD models similar to the model with use in our tests using legal professionals. The results of this analysis are presented in Panel D. We observe statistically significant and economically meaningful declines in wages subsequent to the introduction of CPA Mobility provisions, which range from 1.1% to 1.3%. We also investigate potential effects on employment levels and find no evidence suggestive of meaningful effects of CPA Mobility. Overall, these results are in line with the ones from our baseline specification.

Figure 1.OA-1: CPA Firm Legal Structures



This figure shows CPA firm (NAICS Code: 541211 - "Offices of Certified Public Accountants") legal structures for different size classes defined by the Bureau of Labor Statistics (BLS). We plot the share of establishments relative to all establishments for each legal form provided by the BLS Census County Business Pattern (CBP) files. Establishment count information is derived from BLS CBP state-level files.

Table 1.OA-1: Summary Statistics for Additional Estimation Samples

Panel A: Descriptive Statistics for the SUSB State-Level Sample

	Obs.	*Mean*	*Std. Dev.*	*P1*	*P25*	*P50*	*P75*	*P99*
				SUSB Small CPA Firm Sample				
$Wage_{s,t}^{SUSB}$	369	45,151	6,230	32,835	40,516	44,165	49,836	59,123
$Log(Wage_{s,t}^{SUSB})$	369	10.708	0.138	10.399	10.609	10.696	10.816	10.987
$Employment_{s,t}^{SUSB}$	369	4,126	4,359	493	1,441	2,951	4,795	23,063
$Log(Employment_{s,t}^{SUSB})$	369	7.914	0.903	6.201	7.273	7.990	8.475	10.046
$AvgEmployment_{s,t}^{SUSB}$	369	3.850	0.428	2.972	3.534	3.836	4.195	4.760
$Log(AvgEmployment_{s,t}^{SUSB})$	369	1.342	0.113	1.089	1.262	1.344	1.434	1.560
$Firms_{s,t}^{SUSB}$	369	1,142	1,300	108	368	785	1,340	6,759
$Log(Firms_{s,t}^{SUSB})$	369	6.573	0.965	4.682	5.908	6.666	7.200	8.819
				SUSB Large CPA Firm Sample				
$Wage_{s,t}^{SUSB}$	369	67,219	10,867	49,735	58,733	65,839	73,770	96,160
$Log(Wage_{s,t}^{SUSB})$	369	11.103	0.158	10.814	10.981	11.095	11.209	11.474
$Employment_{s,t}^{SUSB}$	369	1,637	1,653	185	537	1,141	1,979	8,480
$Log(Employment_{s,t}^{SUSB})$	369	6.997	0.903	5.220	6.286	7.040	7.590	9.045
$AvgEmployment_{s,t}^{SUSB}$	369	25.641	5.236	14.385	21.667	25.903	29.538	37.765
$Log(AvgEmployment_{s,t}^{SUSB})$	369	3.222	0.215	2.666	3.076	3.254	3.386	3.631
$Firms_{s,t}^{SUSB}$	369	63	58	6	21	45	78	309
$Log(Firms_{s,t}^{SUSB})$	369	3.775	0.882	1.792	3.045	3.807	4.357	5.733

Table 1.OA-1 (continued)

Panel B: Descriptive Statistics for QCEW Law Firm State-Level Sample

	Obs.	*Mean*	*Std. Dev.*	*P1*	*P25*	*P50*	*P75*	*P99*
	QCEW Legal Professionals Sample (All Sizes)							
$Wage_{s,t}^{QCEW}$	720	70,644	20,824	39,387	56,336	67,312	80,642	145,049
$Log(Wage_{s,t}^{QCEW})$	720	11.128	0.269	10.581	10.939	11.117	11.298	11.885
$Employment_{s,t}^{QCEW}$	720	20,808	27,469	1,302	5,007	12,869	21,308	127,198
$Log(Employment_{s,t}^{QCEW})$	720	9.309	1.136	7.172	8.519	9.463	9.966	11.754
$AvgEmployment_{s,t}^{QCEW}$	720	5.813	2.903	2.863	4.636	5.350	6.013	23.253
$Log(AvgEmployment_{s,t}^{QCEW})$	720	1.697	0.313	1.052	1.534	1.677	1.794	3.146
$Firms_{s,t}^{QCEW}$	720	3,418	4,171	315	904	2,262	3,571	20,044
$Log(Firms_{s,t}^{QCEW})$	720	7.612	1.011	5.753	6.806	7.724	8.181	9.906

Table 1.OA-1 (continued)

Panel C: Descriptive Statistics for the SUSB State-Level Law Firm Sample

	Obs.	*Mean*	*Std. Dev.*	*P1*	*P25*	*P50*	*P75*	*P99*
				SUSB Small Law Firm Sample				
$Wage_{s,t}^{SUSB}$	369	54,045	9,458	36,300	46,367	53,395	60,844	76,030
$Log(Wage_{s,t}^{SUSB})$	369	10.882	0.177	10.500	10.744	10.885	11.016	11.239
$Employment_{s,t}^{SUSB}$	369	11,025	12,233	1,224	3,580	7,290	12,332	59,212
$Log(Employment_{s,t}^{SUSB})$	369	8.864	0.925	7.110	8.183	8.894	9.420	10.989
$AvgEmployment_{s,t}^{SUSB}$	369	3.197	0.307	2.586	3.028	3.183	3.359	4.481
$Log(AvgEmployment_{s,t}^{SUSB})$	369	1.158	0.092	0.950	1.108	1.158	1.212	1.500
$Firms_{s,t}^{SUSB}$	369	3,542	4,046	299	1,280	2,290	3,771	19,570
$Log(Firms_{s,t}^{SUSB})$	369	7.707	0.954	5.700	7.155	7.736	8.235	9.882
				SUSB Small Law Firm Sample				
$Wage_{s,t}^{SUSB}$	369	80,795	13,794	51,829	71,947	80,874	90,111	113,238
$Log(Wage_{s,t}^{SUSB})$	369	11.285	0.175	10.856	11.184	11.301	11.409	11.637
$Employment_{s,t}^{SUSB}$	369	5,163	6,272	95	1,306	3,164	5,065	29,605
$Log(Employment_{s,t}^{SUSB})$	369	7.997	1.092	4.554	7.175	8.060	8.530	10.296
$AvgEmployment_{s,t}^{SUSB}$	369	26.180	3.412	18.213	24.036	26.241	28.600	33.884
$Log(AvgEmployment_{s,t}^{SUSB})$	369	3.256	0.136	2.902	3.180	3.267	3.353	3.523
$Firms_{s,t}^{SUSB}$	369	189	224	5	58	119	177	1,107
$Log(Firms_{s,t}^{SUSB})$	369	4.741	1.020	1.609	4.060	4.779	5.176	7.009

Table 1.OA-1 (continued)

Panel D: Descriptive Statistics for the QCEW MSA-Level Sample

	Obs.	*Mean*	*Std. Dev.*	*P1*	*P25*	*P50*	*P75*	*P99*
				Accounting Professionals				
$Wage^{QCEW}_{m,t}$	1,524	54,411	16,022	27,540	42,816	52,266	63,282	105,115
$Log(Wage^{QCEW}_{p,m,t})$	1,524	10.862	0.289	10.223	10.665	10.864	11.055	11.563
$\Delta Log(Wage^{QCEW}_{p,m,t})$	1,524	0.029	0.050	-0.112	0.006	0.028	0.053	0.149
				Legal Professionals				
$Wage^{QCEW}_{p,m,t}$	1,012	62,708	20,669	27,859	47,366	60,491	74,952	122,153
$Log(Wage^{QCEW}_{p,m,t})$	1,012	10.993	0.331	10.235	10.766	11.010	11.225	11.713
$\Delta Log(Wage^{QCEW}_{p,m,t})$	1,012	0.030	0.057	-0.153	0.004	0.028	0.053	0.217
				MSA GDP				
$GDPperCapita_{m,t}$	2,536	42,958	13,884	20,368	33,230	41,077	49,587	87,536
$Log(GDPperCapita_{m,t})$	2,536	10.624	0.291	9.922	10.411	10.623	10.811	11.380
$\Delta Log(GDPperCapita_{m,t})$	2,536	0.014	0.030	-0.072	0.000	0.014	0.029	0.093

This table presents the summary statistics for additional estimation samples. Detailed definitions for all variables and sample selection criteria are presented in Appendices 1.A and 1.B, respectively.

Table 1.OA-2: CPA Mobility Effect on Wages for Small CPA Firms, Large CPA Firms, and Legal Professionals

Panel A: CPA Mobility Effects on Wages for Small CPA Firms

	Dependent variables: $Log(Wage_{s,t}^{SUSB})$			
Independent variables:	(1)	(2)	(3)	(4)
$CPAMobility_{s,t-1}$	-0.008**	-0.010**	-0.008**	-0.008**
	(0.003)	(0.004)	(0.003)	(0.004)
Macro Controls	No	Yes	No	Yes
Migration Controls	No	No	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Obs.	369	369	369	369
Adj. R^2	0.981	0.982	0.982	0.982

Panel B: CPA Mobility Effects on Wages for Large CPA Firms

	Dependent variables: $Log(Wage_{s,t}^{SUSB})$			
Independent variables:	(1)	(2)	(3)	(4)
$CPAMobility_{s,t-1}$	0.010	0.007	0.009	0.006
	(0.008)	(0.006)	(0.009)	(0.007)
Macro Controls	No	Yes	No	Yes
Migration Controls	No	No	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Obs.	369	369	369	369
Adj. R^2	0.921	0.927	0.920	0.927

Table 1.OA-2 (continued)

Panel C: CPA Mobility Effects on Wages for Legal Professionals

	Dependent variables: $Log(Wage_{s,t}^{SUSB})$			
Independent variables:	(1)	(2)	(3)	(4)
$CPAMobility_{s,t-1}$	-0.005	-0.002	-0.005	-0.003
	(0.006)	(0.004)	(0.006)	(0.003)
Macro Controls	No	Yes	No	Yes
Migration Controls	No	No	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Obs.	720	720	720	720
Adj. R^2	0.986	0.988	0.989	0.999

This table presents the results of our analysis examining CPA Mobility effects on wages for the subsamples of small CPA firms, large CPA firms, and legal professionals. Tests results are based on the *SUSB State-Level Sample* (Panels A and B) and *QCEW State-Level Sample* (Panel C). Detailed definitions for all variables and sample selection criteria are presented in Appendices 1.A and 1.B of the paper, respectively. In Panel A, we report coefficient estimates and (in parentheses) standard errors from weighted least squares (WLS) regressions of $Log(Wage_{s,t}^{SUSB})$ on $CPAMobility_{s,t-1}$ and control variables, as indicated in each column restricting the estimation sample to include small CPA firms only. Regressions are weighted by state-year employment shares. State-year employment shares are defined as $Employment_{s,t}^{SUSB}$ divided by the sum of all $Employment_{s,t}^{SUSB}$ in year t. Panel B documents the effect of CPA Mobility on wages restricting the estimation to include large CPA firms only. In Panel C, we report coefficient estimates and (in parentheses) standard errors from weighted least squares (WLS) regressions of $Log(Wage_{s,t}^{SUSB})$ on $CPAMobility_{s,t-1}$ and control variables, as indicated in each column for the subsample of legal professionals. Regressions are weighted by state-year employment shares. State-year employment shares are defined as $Employment_{s,t}^{SUSB}$ divided by the sum of all $Employment_{s,t}^{SUSB}$ in year t. Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.OA-3: Treatment Effect Stability

$\beta_{Uncontrolled}$	-0.011
$R^2_{Uncontrolled}$	0.010
$\beta_{Controlled}$	-0.010
$R^2_{Controlled}$	0.049
Δ	7.864

This table presents an estimate of the value of Delta (Δ), the relative degree of selection on observed and unobserved control variables for which the treatment effect would be zero, following the methodology developed by Oster (2019). The table reports the coefficient on $CPAMobility_{s,t-1}$ and the within R^2 from the estimation of our main model specification (Table 1.2, Panel B) with ($\beta_{Controlled}$, $R^2_{Controlled}$) and without ($\beta_{Uncontrolled}$, $R^2_{Uncontrolled}$) macro and migration control variables. Following the methodology proposed by Oster (2019) we set R^{max} (the R^2 from a hypothetical regression of the outcome on treatment and both observed and unobserved control variables) equal to 2.0 multiplied by the R^2 of the regression that includes all control variables (i.e., the controlled regression).

Table 1.OA-4: Within-State Synthetic Control Groups

Panel A: Synthetic Control Weights Calculated until 2005

NAICS Code	NAICS Description	Mean Weight	Median Weight
541110	Offices of Lawyers	0.374	0.415
541330	Engineering Services	0.177	0.042
541940	Veterinary Services	0.066	0.038
541511	Custom Computer Programming Services	0.051	0.016
541611	Administrative Management and General Management Consulting Services	0.022	0.012
541512	Computer System Design Services	0.020	0.014
541820	Public Relation Agencies	0.015	0.007
541219	Other Accounting Services	0.013	0.012
541910	Marketing Research and Public Opinion Polling	0.013	0.009
541380	Testing Laboratories	0.013	0.010
541513	Computer Facilities Management Services	0.012	0.007
541213	Tax Preparation Services	0.012	0.007
541310	Architectural Services	0.011	0.011
541810	Advertising Agencies	0.011	0.010
541613	Marketing Consulting Services	0.010	0.009
541890	Other Services related to Marketing	0.010	0.007
541214	Payroll Services	0.010	0.009
541690	Other Scientific and Technical Consulting Services	0.010	0.009
541618	Other Management Consulting Services	0.009	0.009
541612	Human Resource Consulting Services	0.009	0.009
541519	Other Computer Related Services	0.009	0.009
541620	Environmental Consulting Services	0.009	0.008
541370	Survey and Mapping Services	0.009	0.007
541840	Media Representatives	0.009	0.007
541614	Process, Physical Distribution, and Logistics Consulting Services	0.009	0.008
541720	Research and Development in the Social Sciences and Humanities	0.008	0.007
541990	All Other Professional, Scientific and Technical Services	0.008	0.007
541430	Graphic Design Services	0.008	0.008
541191	Title Abstract and Settlement Offices	0.008	0.008
541860	Direct Mail Advertising	0.007	0.007
541320	Landscape Architectural Services	0.007	0.007
541850	Building Inspection Services	0.007	0.007
541410	Interior Design Services	0.007	0.006
541360	Geophysical Surveying and Mapping Services	0.007	0.006
541350	Building Inspection Services	0.007	0.005
541830	Media Buying Agencies	0.006	0.006
541921	Photography Studios, Portrait	0.006	0.005
541340	Drafting Services	0.006	0.006
541199	All Other Legal Services	0.006	0.005
541420	Landscape Architectural Services	0.006	0.006
541870	Advertising Material Distribution Services	0.006	0.006
541930	Translation and Interpretation Services	0.006	0.005
541922	Commercial Photography	0.006	0.005
541490	Other Specialized Design Services	0.005	0.005

Table 1.OA-4 (continued)

Panel B: Synthetic Control Weights Calculated until State-Specific Treatment Date

NAICS Code	NAICS Description	Mean Weight	Median Weight
541110	Offices of Lawyers	0.368	0.402
541330	Engineering Services	0.176	0.043
541940	Veterinary Services	0.070	0.040
541511	Custom Computer Programming Services	0.049	0.015
541611	Administrative Management and General Management Consulting Services	0.025	0.013
541512	Computer System Design Services	0.022	0.014
541820	Public Relation Agencies	0.018	0.007
541380	Testing Laboratories	0.013	0.010
541513	Computer Facilities Management Services	0.013	0.008
541219	Other Accounting Services	0.013	0.012
541910	Marketing Research and Public Opinion Polling	0.013	0.009
541310	Architectural Services	0.011	0.011
541213	Tax Preparation Services	0.011	0.007
541810	Advertising Agencies	0.010	0.010
541890	Other Services related to Marketing	0.010	0.007
541613	Marketing Consulting Services	0.010	0.009
541690	Other Scientific and Technical Consulting Services	0.010	0.009
541618	Other Management Consulting Services	0.010	0.009
541214	Payroll Services	0.009	0.008
541612	Human Resource Consulting Services	0.009	0.008
541620	Environmental Consulting Services	0.009	0.008
541614	Process, Physical Distribution, and Logistics Consulting Services	0.009	0.008
541519	Other Computer Related Services	0.009	0.009
541370	Survey and Mapping Services	0.009	0.007
541990	All Other Professional, Scientific and Technical Services	0.008	0.007
541840	Media Representatives	0.008	0.008
541720	Research and Development in the Social Sciences and Humanities	0.008	0.007
541430	Graphic Design Services	0.008	0.007
541191	Title Abstract and Settlement Offices	0.008	0.008
541860	Geophysical Surveying and Mapping Services	0.007	0.007
541320	Landscape Architectural Services	0.007	0.006
541850	Building Inspection Services	0.007	0.007
541410	Interior Design Services	0.007	0.006
541360	Geophysical Surveying and Mapping Services	0.006	0.006
541199	All Other Legal Services	0.006	0.005
541921	Photography Studios, Portrait	0.006	0.005
541350	Building Inspection Services	0.006	0.005
541830	Media Buying Agencies	0.006	0.006
541340	Drafting Services	0.006	0.005
541420	Landscape Architectural Services	0.006	0.006
541930	Translation and Interpretation Services	0.006	0.005
541870	Advertising Material Distribution Services	0.006	0.006
541922	Commercial Photography	0.005	0.005
541490	Other Specialized Design Services	0.005	0.005

Table 1.OA-4 (continued)

Panel C: Descriptive Statistics for QCEW State-Level Synthetic Control Group Samples

	Obs.	Mean	Std. Dev.	P1	P25	P50	P75	P99
	Synthetic CPA 1 (Calculating Weights until 2005)							
$Wage^{QCEW}_{s,t}$	720	66,124	16,990	39,236	53,690	63,537	74,545	122,405
$Log(Wage^{QCEW}_{s,t})$	720	11.069	0.244	10.577	10.891	11.059	11.219	11.715
$Employment^{QCEW}_{s,t}$	720	13,904	17,440	966	3,099	8,228	16,337	77,849
$Log(Employment^{QCEW}_{s,t})$	720	8.928	1.121	6.874	8.039	9.015	9.701	11.263
	Synthetic CPA 2 (Calculating Weights until State-Specific Treatment Date)							
$Wage^{QCEW}_{s,t}$	720	66,426	17,429	39,279	53,778	63,670	74,752	128,605
$Log(Wage^{QCEW}_{s,t})$	720	11.073	0.247	10.578	10.893	11.061	11.222	11.765
$Employment^{QCEW}_{s,t}$	720	13,981	17,692	938	3,075	7,721	16,351	78,039
$Log(Employment^{QCEW}_{s,t})$	720	8.927	1.122	6.843	8.031	8.952	9.702	11.265

Panel D: CPA Mobility Effects on Wages and Employment

	Dependent variables:			
	Synthetic CPA 1		*Synthetic CPA 2*	
	$Log(Wage^{QCEW}_{s,o,t})$	$Log(Employment^{QCEW}_{s,o,t})$	$Log(Wage^{QCEW}_{s,o,t})$	$Log(Employment^{QCEW}_{s,o,t})$
Independent variables:	(1)	(2)	(3)	(4)
$CPAMobility_{s,t-1} \times CPA_o$	-0.013***	-0.008	-0.011**	-0.007
	(0.004)	(0.016)	(0.005)	(0.016)
State × Profession FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Profession × Year FE	Yes	Yes	Yes	Yes
Obs.	1,440	1,440	1,440	1,440
Adj. R^2	0.991	0.998	0.991	0.998

This table presents the results of our difference-in-difference-in-differences (DiDiD) analysis examining CPA Mobility effects on wages and employment using two within-state synthetic control groups. Detailed definitions for all variables and sample selection criteria are presented in Appendices 1.A and 1.B of the main paper, respectively. Panel A reports the mean and median weights (sorted by mean weights) assigned to each donor industry based on a synthetic control approach that matches wage and employment trends over the period from 2003 to 2005, that is, the sample period before the first of our sample states adopts CPA Mobility provisions. Panel B reports the mean and median weights (sorted by mean weights) assigned to each donor industry based on a on a synthetic control approach that matches wage and employment trends over the period from 2003 until a state adopts CPA Mobility provisions. Panel C reports summary statistics for both synthetic control groups. We form synthetic control groups by calculating average state-year wage and employment levels using the weights reported in Panel A (*Synthetic CPA 1*) and the weights reported in Panel B (*Synthetic CPA 2*). In Panel D, we report coefficient estimates and (in parentheses) standard errors are from weighted least squares (WLS) regressions of $Log(Wage_{s,o,t}^{QCEW})$ and ordinary least squares (OLS) regressions of $Log(Employment_{s,o,t}^{QCEW})$ on $CPAMobility_{s,t-1}{\times}CPA_{o}$ and control variables, as indicated in each column. Regressions in Columns (1) to (3) are weighted using employment shares. Employment shares are defined as $Employment_{s,o,t}^{QCEW}$ divided by the sum of all $Employment_{s,o,t}^{QCEW}$ in year *t.* Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

2 Can Ethics be Taught? Evidence from Securities Exams and Investment Adviser Misconduct

2.1 Introduction

Financial markets and institutions are shaped by responses to corporate scandals and financial crises. One way this happens is through the design and enforcement of regulation. However, scandals and crises also lead to calls for changes in how market participants are qualified, particularly in ethics, professional conduct, and fiduciary duties (Piper et al.,1993). For example, in his Presidential address to the American Financial Association, Zingales (2015) says:

> We should not relegate our prescriptive analysis to separate, poorly attended ethics courses, validating the implicit assumption that social norms are a matter of interest only for the less bright students. Several social norms are crucial to the flourishing of a market economy. We should teach them in our regular classes, at the very least emphasizing how violating these norms has a negative effect on reputation.

Similar calls for training in the classroom and on the job followed Enron's failure and the Great Recession (Koehn, 2005; Arbogast et al., 2018).

Others question the effectiveness and desirability of professional conduct training (Drucker, 1981): "business ethics courses are seen to have been created largely for the sake of appearances and from the imperative of initiating some form of responsive action" (McDonald and Donleavy, 1995: 842-843). Another line of criticism acknowledges that, while rules can be taught, beliefs about acceptable conduct guide behavior and these beliefs are primarily formed outside the classroom. Then, individuals may participate in ethics education, but their participation is feigned. Additionally, professional conduct training can be difficult to tailor to the specialized and often ambiguous nature of daily work.

Claims surrounding the consequences of ethics and professional conduct training have not been investigated empirically. A key barrier has been that researchers do not observe the training that individuals receive or how this training affects their behavior. In this paper, we study a change in the Series 66 exam, which individuals pass before becoming licensed investment advisers.[38] The exam, administered by the Financial Industry Regulatory Authority (FINRA) and designed by the North American Securities Administrators Association (NASAA), comprises two sections. One section focuses on allowable forms of compensation and disclosure requirements ("rules") and prohibitions of unethical business practices (commonly referenced in the securities industry as "ethics", a convention we follow throughout our paper). A second section covers capital market theory, investment vehicle characteristics, ratios, and financial reporting ("technical material").

Starting January 1, 2010, the exam weighted technical and rules/ethics questions equally (50% each), whereas prior, rules/ethics questions received an 80% weight while technical questions received a 20% weight. NASAA altered the content weights "based on responses to (a) survey indicating that dually licensed individuals should have enhanced testing in…(technical) areas" (Cole-Frieman and Mallon, 2010). Meanwhile, the exam's cost, length, and time allotted remained constant, as did the qualification received by those passing the exam.

We leverage several features of the exam change and the investment adviser setting to shed light on the consequences of professional conduct training. First, individuals must master a significant amount of rules- and ethics-related material before becoming investment advisers. A popular study guide advises individuals to spend 75-

[38] In this paper, we refer to licensed investment advisers as "investment advisers" or "advisers". We refer to other securities industry employees without this license as "registered representatives" or "representatives".

100 hours over 4-8 weeks preparing for the exam (Cohen, 2018). After, these advisers provide advice to, typically, unsophisticated investors who rely on their adviser's qualifications and adherence to professional standards. When violations of these standards occur (henceforth, "misconduct"), we observe the date, employee and employer identity, and a description of the incident. Misconduct incidents commonly involve misrepresentation, unauthorized activity, omission of key facts, and excessive trading to generate commissions, rather than mere formalities or violations of obscure rules.

Second, we observe the exact date that advisers pass each securities exam. Rather than develop our own definition of rules and ethics training, we exploit the large reduction in rules and ethics coverage as defined by NASAA from the old to the new exam version. We simply assume that advisers passing the old exam had more rules and ethics training than those passing the new exam.

Third, other common securities exams (including the co-requisite Series 7) did not undergo any change in content around 2010; moreover, individuals working for the same firm-location often take the same exams but at different times. This variation aids our identification strategy by providing a group of advisers with the same employer and qualifications, but different rules and ethics training required to achieve those qualifications. Together, these features help us develop credible evidence on an important but largely unexplored research question.

To study adviser behavior, we compare the change in an adviser's misconduct after they pass the Series 66, across those who took the old versus new exam. We omit those passing the exam in the window surrounding the change enactment, to mitigate selection concerns surrounding strategic exam registration. We include individual fixed effects to account for time-invariant features that could affect behavior, such as an individual's upbringing, gender, and formative career experiences (e.g., Oyer, 2008; Shue, 2013; Egan et al., 2018; Clifford et al., 2019; Law and Zuo, 2019).

We find significantly less misconduct among those passing the more rules- and ethics-focused exam. Our estimates suggest that taking the old exam is associated with a one-quarter reduction in advisers' 0.86% average annual propensity to commit misconduct. Our results are economically meaningful, yet not too large as to be implausible considering the individual nature of advising work and related research linking individual characteristics to misconduct. For example, the exam change is slightly less important than gender (Egan et al., 2018) and much less important than prior misconduct (Egan et al., 2019) for explaining new misconduct.

To this point, we do not discern between selection and treatment explanations for the misconduct differences. In terms of selection, the results could reflect unobservable differences between pre- and post-2010 Series 66 passers. For example, working or sitting for qualification exams during the crisis could have a sustained effect on one's view of appropriate interactions with investors. Related, one's proclivity for misconduct could be correlated with their ability to master technical material, which the new exam more heavily weights. Then, differences in misconduct may not result from the change in content per se, but from unobservable differences in individuals capable of passing the old and new exam.

We investigate selection explanations in several ways. First, we continue to find our results if we limit our sample to individuals with recession experience or passing any qualification exam (Series 66 or other) before 2010. Second, we compare the misconduct of one individual to another who entered the industry at the same time as them, through separate year fixed effects for each cohort, and find the same results. Third, in Figure 2.1 we compare old and new Series 66 passers in event time. If our results stem from selection on unobservables, then we expect to find differences in pre-exam misconduct across these groups. However, the differences are confined to the post-exam period. Together, these findings reduce concerns that our evidence of higher misconduct among new exam passers is explained by selection.

Because the exam change reduced coverage of both rules and ethics-based questions, we then investigate why the change appears to affect adviser behavior. One compliance-based interpretation is that advisers passing the new exam are more likely to engage in misconduct simply because they are less aware of the rules. A second, not mutually exclusive explanation is that the exam's focus on ethics alters individuals' perceptions of right and wrong conduct. Our objective is not to fully attribute our main findings to either compliance or ethics-based explanations. Indeed, both classes of explanations could be valid, and both are relevant to understanding the consequences of qualification exams in financial markets (Warren et al., 2014). Rather, we aim to establish whether there is some role for ethics in explaining the differences in misconduct across old and new exam passers.

We conduct a textual analysis of 64,972 misconduct descriptions, and identify 18,754 incidents involving theft, fraud, and deceit. For these obvious offenses, we presume the exam's reduction in rules coverage was inconsequential, as even industry outsiders would recognize that the adviser engaged in wrongdoing. If our main results were solely explained by compliance, we should find no difference in obvious misconduct between passers of the old and new exam. However, we find a significant difference comparable to that in our main results. Further, we find the misconduct differences across passers of the old and new exam persist for at least three years, which we would not expect if advisers merely memorize rules rather than draw more fundamental lessons about acceptable conduct from the ethics portion of the exam. In sum, this evidence suggests that our main results cannot be explained by compliance alone, and that the exam change altered advisers' perceptions of acceptable conduct.

In terms of individual characteristics, those passing the exam without prior misconduct appear to respond most to the amount of rules and ethics material covered on their exam. And, the behavior of the least experienced advisers is most sensitive to the extent of rules and ethics testing. These results are consistent with the exam playing a "priming" role, where early exposure to rules and ethics material prepares the individual to behave appropriately later (Cohn and Maréchal, 2016). As for firm characteristics, we find the exam's coverage to be less pertinent to those advisers working at firms where misconduct is prevalent. Thus, the contagion of misconduct behavior appears to limit the effectiveness of training in preventing transgressions (Dimmock et al., 2018; Easley and O'Hara, 2019).

Our final set of tests examines how advisers respond to workplace ethics scandals. To illustrate, consider the Wells Fargo account fraud that became widely known in 2016. While the fraud was contained in the consumer banking division, a number of Wells Fargo investment advisers noted the deterioration in the firm's culture as their reason for leaving to work for another employer (Flitter and Cowley, 2019). We study turnover among all Wells Fargo advisers, and find those passing the old exam are most likely to leave after the scandal broke. Because the Wells Fargo scandal did not relate to investment advisers or rules covered on their qualification exam, these results reinforce how the exam change altered advisers' perception of acceptable conduct, and not just their awareness of the rules.

While Wells Fargo provides an appealing case study, we extend our analysis to the full sample, and study turnover at firms subject to major penalties or company-wide increases in professional violations. We find a similar turnover pattern in this sample, indicating that advisers with more rules and ethics training are less likely to tolerate bad behavior at their firm, and seek employment elsewhere. Building on this, we find that departures of advisers passing the old exam predicts scandals at their former employer next year.

We make three contributions. First, to our knowledge our paper is the first archival study of the effects of rules and ethics training on professional conduct in financial markets. The large literature on investor protection focuses on the design and consequences of regulation, disclosure laws, and governance mechanisms (Campbell et al., 2011; Dimmock and Gerken, 2012; Hail et al., 2018; Charoenwong et al., 2019). A lack of data on individual qualifications and behavior has prevented researchers from investigating what role, if any, ethics training might play. Our results support the view that ethics training plays an important role in constraining fraud and influencing employee-firm matching. In this way, our results complement work studying financial literacy and financial education for *consumers* (see Lusardi and Mitchell, 2014, for a review).

Second, we add to a growing body of research concerned with understanding the causes of adviser and representative misconduct (Dimmock and Gerken, 2012; Dimmock et al., 2018; Egan et al., 2018; 2019; Parsons et al., 2018; Clifford et al., 2019). One impetus for this work is that misconduct affects household saving and stock market participation (Guiso et al., 2008; Gurun et al., 2017).[39]

Third, our results contribute to research on professional labor markets, and licensing in particular. Professional conduct education has long been part of licensing not only for advisers, but also accountants, lawyers, and other non-financial occupations

[39] Our paper also adds to the recent literature on individual misconduct within firms (Soltes, 2018; Stubben and Welch, 2018; Heese and Cavazos, 2019), and auditors' oversight of this misconduct (Cook et al., 2019).

including physicians.[40] In this respect, the study perhaps most related to ours is Clifford and Gerken (2019), who examine how labor mobility provisions in the securities industry affect individuals' decisions about which qualifications to acquire, and how these qualifications relate to their fee model, assets under management, and customer complaints. In line with our findings, they conclude that an adviser's acquisition of professional licenses represents an important investment in their human capital.

2.2 Setting

2.2.1 Investment Advisers

Investment advisers guide investors engaging capital markets. All investment advisers must register and file certain forms with the SEC under the Investment Advisers Act of 1940, even if their firm's size exempts them from SEC oversight as described in Charoenwong et al (2019).[41] Both the SEC and FINRA, a self-regulatory enforcement agency tasked with protecting investors in the US securities industry, disclose adviser- and firm-specific information on their websites. FINRA's BrokerCheck website notes that "all individuals registered to sell securities or provide investment advice are required to disclose customer complaints and arbitrations, regulatory actions, employment terminations, bankruptcy filings, and criminal or judicial proceedings." This information can be submitted by an individual, their employer, or the regulator.

[40] For example, 35 state accounting boards require individuals to achieve at least a 90% score on a 40 question ethics exam before receiving the CPA designation. Forty-eight states require prospective lawyers to pass a 60 question ethics exam before being admitted to the bar (the average required score across these states is 81%). The American Board of Physician Specialties requires members to take an ethics course every eight years.

[41] Congress exempted investment adviser firms with less than $25 million from SEC oversight under the Securities Investment Promotion Act of 1996 and increased this threshold to $100 million under the Dodd-Frank Act of 2010.

2.2.2 *Investment Adviser Licensing Exam*

Although some representatives happen to provide advice that is incidental to their fundamental business, investment advisers provide fee-based advice. Acknowledging this explicit advisory relationship, regulators set a higher standard of conduct as well as additional licensing requirements for investment advisers relative to registered representatives.[42] Specific to licensing, these advisers must pass either the Series 65 or 66 exam to provide fee-based advice, though neither exam is independently sufficient: individuals must pass the Series 63 with the 65, or the Series 7 with the 66.[43] Individuals tend to sit for these exams early in their career, though some sit for exams later to upgrade their qualifications. While NASAA develops the 63, 65, and 66 exams, FINRA administers the related licensing for these, and other, industry exams.

Both the Series 65 and 66 exams cover two broad areas: rules/ethics (specifically, "Laws, Regulations, and Guidelines, including Prohibition on Unethical Business Practices") and technical material ("Economic Factors and Business Information, Investment Vehicle Characteristics, and Client Investment Recommendations and Strategies") (NASAA, 2011). Rules/ethics material covers allowable forms of compensation, disclosure requirements, and various aspects of an adviser's fiduciary duty to investment clients. Technical material covers capital market theory, investment vehicle characteristics, ratios, and financial reporting.

While delineating between rules and ethics topics is not always straightforward, the exam categorizes questions in separate categories. NASAA does not disclose exam

[42] Whereas investment advisers must meet a fiduciary standard of conduct, other representatives are bound by a suitability standard during the study period, or more recently by Regulation Best Interest (SEC, 2019).
[43] Whether an individual takes the combined Series 66 exam or the Series 63 and Series 65 exam is primarily determined by an employer's registration status. The Series 66 exam effectively combines the Series 63 and Series 65 exams but requires individuals to take the Series 7 exam. The industry provides alternative paths since the (co-requisite) Series 7 exam can only be taken by individuals with FINRA sponsorship, i.e., employees of FINRA members.

questions, but we have collected several from a popular Series 66 study guide to illustrate the categorization (Mometrix, 2019):

<u>Example Rules Questions:</u>

Describe the registration process.

Describe the obligation to ensure that client security transactions are handled and recorded accurately.

Describe allowable forms of compensation for investment advisers.

Describe the investment adviser's responsibility to disclose the source of any third-party recommendations and reports.

Describe the circumstances in which an investment adviser is permitted to maintain custody of its clients' assets.

<u>Example Ethics Questions:</u>

Discuss the ethical and fiduciary responsibility of advisers regarding the charging of commissions.

Describe the obligation to consider a client's investment objectives when making recommendations.

Describe the conditions that must be met in order for an advisor to ethically take custody of a client's funds.

Describe the fiduciary responsibilities of investment advisers.

Discuss the act of committing fraud by omission.

<u>Example Technical Questions:</u>

Briefly describe modern portfolio theory.

Discuss current ratios and describe what they are useful in measuring.

Define capital gains and describe how capital gains are taxed.

Define S corporations, and describe their usefulness to investors.

Describe the weak form of the efficient market hypothesis.

Define value stocks, and describe how portfolio managers determine if a stock is a value stock.

Define inflation-adjusted return and name the index used to help calculate it.

Calculate the beta for XYZ Company using the following details:

- Risk-Free Rate of Return = 2%
- XYZ Company Rate of Return = 5%
- S&P 500 Index Rate of Return = 7%

Our tests study the 2010 change in the Series 66, announced in September 2009.[44] Prior to January 1, 2010 the exam contained 100 questions, with 80% of the questions covering rules and ethics and 20% covering technical material. Starting January 1, 2010 the composition of the exam was altered such that rules/ethics questions and technical material were equally-weighted. The change was motivated by a desire to increase testing in "Economic Factors and Business Information, Investment Vehicle Characteristics and Client Investment Recommendations and Strategies" (Cole-Frieman and Mallon, 2010). At the same time, the Series 63 and 65 underwent similar, albeit smaller changes, with the rules/ethics section weights decreasing from 50% to 45%.[45] None of the other major securities exams (e.g., the Series 6, 7, or 24) were affected. The Series 66 changed again in July 2016, with the rules/ethics weight falling from 50% to 45% to match the Series 63 and 65.

[44] The earliest reference we can find to the exam change was a blog post on September 16, 2009 (Walks, 2009).

[45] The changes received much interest from study guide websites and investment adviser discussion forums. For example, one blog dedicated to the exam stated "If you're one of those people who need deadline pressure in order to actually start studying for the Series 65/66 exams, here you go: the Series 65 and 66 are changing starting January 1st. Yikes! For example, the 45 questions on business practices/ethics is being reduced to 40 on the Series 65. The 80/20 split is changing to 50/50 on the Series 66" (Walks, 2009).

Following the 2010 change, the Series 66 exam length (100 questions), time permitted (150 minutes), and cost (roughly $130) remained. However, the minimum passing grade increased from 71% to 75%. The minimum passing grade for the Series 63 (65) increased from 70% to 72% (68% to 72%). Thus, while all three exams experienced similar changes in required passing grades, the reduction in rules/ethics content was much greater for the Series 66 (30%) than for the 63 and 65 (5%).[46]

2.3 Data, Summary Statistics, and Research Design

2.3.1 Data

In January 2018, we accessed BrokerCheck's database of adviser and representative records. The database contains all registered advisers and representatives currently employed in the US securities industry at brokerage firms, as well as registered advisers and representatives employed up to ten years prior. Thus, following other work using this data (Egan et al., 2018; 2019; Honigsberg and Jacob, 2018; Law and Zuo, 2019) we study a ten year period, spanning 2007-2017. Each individual's record contains information about their current employment, previous employment, exams passed (including the type and date), as well as disclosures of customer complaints, arbitrations, regulatory actions, employment terminations bankruptcy filings, and any civil or criminal proceeding involving them. FINRA does not report failed exam attempts or exam scores. Using these disclosures, we classify misconduct incidents as those fitting into six categories as described in Egan et al. (2019): Civil-Final, Criminal-Final Disposition, Customer Dispute-Award/Judgment, Customer Dispute-Settled, Employment Separation

[46] And, to the extent that the higher passing grade and more technical training result in more qualified individuals becoming advisers, it would work against us finding an increase in misconduct for those passing the new Series 66.

after Allegations, and Regulatory-Final. Appendix 2.B contains an example report from an individual in our sample.

Table 2.1 describes our sample construction. We start with 8,838,880 individual-firm observations from BrokerCheck from the years 2007-2017, for which we have a full record of advisers and representatives. We then adjust this initial sample in several ways.

First, to reduce concerns about advisers selecting into the old or new exam, we eliminate those passing the Series 66 in the months surrounding January 2010. Figure 2.2 shows an elevated number of Series 66 passers in November and December 2009, followed by a sharp reversal in January 2010 and February 2010. We study the number of exams passed each month in a regression framework and find significant evidence of bunching around January 2010 (see Appendix 2.C). Based on this evidence, our misconduct tests eliminate those advisers passing the Series 66 from October 2009 (the month after the exam change was announced) to March 2010. As nearly half of advisers pass the Series 66 in their first year in the industry, we see little remaining concern about strategic selection into the old or new exam. Nevertheless, we verify that our inferences are the same if we include every adviser or drop those who passed the exam within six months or even a year of January 2010.

Second, we omit observations from those passing the Series 66 after July 2016, when the exam weights (slightly) changed again as discussed in Section 2.2.2. Last, we omit the year of each adviser's Series 66 exam, because we collapse our data at the individual-firm-year level and it is ambiguous whether any misconduct occurred before or after the exam during such years. The remaining 8,500,453 observations form the sample for our misconduct analyses, described below.

2.3.2 *Summary Statistics*

Table 2.2, Panel A provides summary statistics for the individual-firm-year observations studied in our misconduct analyses. In a typical individual-firm-year, 0.76% of individuals have a misconduct incident, while 0.22% have an obvious misconduct incident involving fraud, deceit, or theft (further described below). Nearly 8% of individuals have a prior incident on their record. For those with a Series 66 qualification, 0.86% have a misconduct incident while 6.8% have a prior incident. Seventeen percent of advisers exit their employer each year. The typical individual has 13 years of experience. Thirty-seven percent (66%; 15%) of the individuals have passed the Series 6 (Series 7; Series 24). As for the Series 63, (65), 72% (20%) have the qualification. In 20% of the observations, the individual has already passed the Series 66, while 16% have passed the pre-2010 version. Thirty-one percent of individuals have attained qualifications other than those involving these six major exams.

Panel B reports statistics for investment adviser characteristics, measured at the date they pass their Series 66. As of the exam pass date, the average individual has 4.72 years of experience, while 46% are taking the exam during their first year in the industry. Four percent of advisers already have a misconduct record from their pre-exam work as a representative.

2.3.3 *Research Design*

We study individual misconduct using the following linear probability specification:

$$y_{ijt} = \beta_1 \times S66_{it} + \beta_2 \times EthicsS66_i \times S66_{it} + \alpha_i + \alpha_{jct} + \gamma \times Controls_{ijt} + \varepsilon_{ijt}. \quad (1)$$

Our specification follows that of Egan et al. (2018, 2019). The unit of observation is individual-firm-year, where i indexes individuals, j indexes firms, t indexes years, and c indexes cities. Occasionally, individuals change employers during the year, and in such cases we have more than one observation per individual-year. The dependent variable y_{ijt} is an indicator for whether individual i has a misconduct incident at firm j in year t. $S66_{it}$ is an indicator for whether individual i has passed the Series 66 as of year t. $EthicsS66_{it}$ is an indicator for whether individual i passes the pre-2010 exam, which contained more rules and ethics material.

The main effect for $EthicsS66_{it}$ is absorbed by our inclusion of individual fixed effects (α_i), which account for time-invariant individual characteristics having a sustained effect on behavior. We also include fixed effects for each firm-city-year (α_{jct}). In doing so, we effectively compare the incidence of misconduct among individuals working for the same firm in the same location. This prevents across-firm differences in internal controls, risk taking, strategy, culture, or regulatory oversight from contaminating our analysis. The city-year dimension of the fixed effect accounts for city-specific drivers of misconduct including investor demographics, the state of the economy, as well as the strictness of regulatory enforcement. Following Egan et al. (2019) we include controls for (log) years of experience, having passed the Series 6, 7, 24, 63, 65, or other qualification exams, and an indicator for whether the individual has ever been disciplined for misconduct prior to the current year. We cluster our standard errors by firm. Clustering instead by individual or individual and firm does not affect our inferences.

2.4 Results

2.4.1 Misconduct and Exam Coverage

Table 2.3 presents the results from estimating equation (1). In Column (1) we begin with a relaxed version of equation (1) with only controls and individual and year fixed effects, and augment the fixed effects in subsequent columns. We find the annual propensity to commit misconduct is 0.162% lower among those passing the old exam covering more rules and ethics material. Considering the average annual likelihood of misconduct for Series 66 qualified advisers is 0.86%, this represents nearly a one-fifth difference in new misconduct rates. The signs on our control variables (not tabulated for brevity) are consistent with prior work (e.g., Egan et al., 2018; 2019). Individuals with the Series 7 are more likely to be involved in misconduct incidents. This is natural because such individuals have more responsibility and interact in greater depth with investment clients who file many of the misconduct complaints. Individuals with a history of misconduct are more likely to commit misconduct again, as are those with more experience (who tend to have more clients and more responsibility).

Column (2) introduces fixed effects for each firm, and finds a similar result.[47] To mitigate concerns that time-varying firm heterogeneity explain these initial results, Column (3) adds a firm-year fixed effect. Our results remain. In Column (4) we estimate our fully saturated equation (1). We continue to find a significant difference related to the exam change, now accounting for approximately one-fourth of the average misconduct level for Series 66 qualified advisers.

To benchmark this result, consider that Egan et al. (2018) use a very similar sample period and research design to ours to study differences in misconduct between males and females. In their strictest specification (comparable to our Column (4)), they

[47] The number of observations declines as we add stricter fixed effects because singletons are dropped.

find females are roughly one-third less likely to commit misconduct as males. Furthermore, Egan et al. (2019) show that new misconduct is five times more likely for individuals with a history of misconduct. Therefore, the exam change effect size appears both important and plausible.

Finally, Column (5) adds a Series 66 dimension to our firm-year-city fixed effect. In this way, we are comparing individuals in the same year with the same qualifications, employer and location. Our results remain, although given the within-fixed effect variation required by this approach (we lose nearly half of our adviser observations, mostly from small cities and branches) we do not continue with this specification.

2.4.2 Why Does Misconduct Vary with Exam Coverage?

In this section, we investigate why adviser misconduct varies with the Series 66 exam coverage. Under a treatment explanation, an adviser's conduct is informed in part by the amount of rules and ethics training they have undertaken. By contrast, under a selection explanation, individuals passing the old and new Series 66 are fundamentally different, and therefore their long run propensity for committing misconduct is different. As an example, working or sitting for qualification exams during the financial crisis could affect one's view of appropriate interactions with investors. Or, individuals' predisposition for misconduct behavior may be correlated with their ability to master technical material, which the new exam more heavily weights.[48]

We investigate these selection explanations in four ways. First, we add year fixed effects for each cohort to our main specification by interacting indicators for each calendar year with indicators for each cohort year. Thus, each year we effectively

[48] Yet another selection explanation relates to individuals strategically timing their exam around the change. However, recall from Section 2.3.1 that we eliminate advisers passing the Series 66 between the change announcement date and several months after enactment, suggesting this particular selection explanation is unlikely.

compare the misconduct of one adviser to another who entered the profession in the same year.[49] One drawback of this approach is that because advisers typically take exams early in their career, some of the exam type variation we are interested in gets absorbed, making it harder to find results. Despite this, Column (1) of Table 2.4 shows that we continue to find less misconduct among those advisers passing the old Series 66.

Second, we construct a sample around more comparable cohorts. Specifically, we evaluate the sensitivity of our results to limiting our sample of advisers and representatives to those passing a securities exam (either the Series 66 or some other exam) before 2010 (Column (2)), and those with pre-2010 work experience (Column (3)).[50] Our results remain; moreover the coefficient on $S66\ x\ Ethics\ S66$ is similar to our baseline results (Table 2.3). Column (4) eliminates advisers who passed the Series 66 outside of the 2008-2011 period, such that our identification comes from advisers passing the exam during the same narrow window (though we continue to omit the October 2009-March 2010 passers). Again, our results remain.

Third, we perform a placebo test. We study the timing of individuals' Series 7 exam, which did not undergo any content change around 2010. Although nearly all Series 66 passers also passed the precursor Series 7 exam, only a third of those that pass Series 7 go on to pass the Series 66. This provides a relevant setting to examine confounding cohort effects. If our main results come from factors affecting securities exam passers around 2010 rather than the change in Series 66 coverage, then we expect to find differences in misconduct among those passing the Series 7 before versus after 2010. Column (5) reports no such difference.

[49] Also note that we control for experience in equation (1), and that adding polynomial experience controls or fixed effects for each experience level does not affect our results.
[50] Restricting the Post 2010 Series 66 sample to advisers with recession work experience produces the same results.

Fourth, we study advisers' misconduct in event time around their obtaining the Series 66 qualification. Specifically, we estimate a version of equation (1), in which we replace the treatment indicator by event time dummies for years t-1, t=0, t+1, t+2, and >=t+3 (t-2 is the holdout).[51] The sample is limited to the subset of advisers passing the Series 66. Point estimates on our event time dummy variables can be interpreted as the event time differences in misconduct propensities between individuals taking the old versus new exam. If advisers passing the old and new exam differ in some fundamental way, then we would expect pre-exam differences in their misconduct. However, Figure 2.1 reveals no such differences.

The foregoing analysis suggests the reduction in the Series 66 rules and ethics coverage had a direct effect on advisers' conduct. We now study whether this only relates to advisers' awareness of the rules (e.g., compliance), or also their beliefs about appropriate conduct (e.g., ethics). Of course, both types of explanations could apply, and fully distinguishing between them is not possible in our setting. Instead, our objective is to establish whether ethics appears to play *some role* in generating our findings, by examining adviser behavior in greater detail.

We perform a textual analysis of our misconduct incident descriptions, and flag those involving fraud, theft, and deceit. For example, we flag incidents containing variations of the following terms and their synonyms: deception, embezzle, fabricate, fake, falsify, forgery, impersonate, lie, misappropriate, misrepresent, omission, omit, and steal. Of the 64,972 misconduct incidents in our sample window, only 18,754 get flagged. We refer to these incidents as *Obvious Misconduct*. Such misconduct seems more likely to result from an adviser's lapse in ethical judgment than their ignorance about specific

[51] As we use lagged control variables, our sample begins in 2007, and the exam changed in 2010, t-2 is the earliest date we can use.

securities industry rules. In other words, we assume that even individuals outside the securities industry without knowledge of its rules would find something inherently wrong with the adviser's conduct.

Table 2.5 presents the results of estimating equation (1) using an indicator for *Obvious Misconduct* as the dependent variable. We find those advisers passing the old exam are 14.6% less likely to engage in obvious misconduct, compared to advisers at the same firm location passing the new exam. This suggests that interpretations based on compliance or rules awareness cannot fully explain our results. Reinforcing this inference, Figure 2.1 shows that the misconduct differences between old and new exam passers persist, statistically and economically, for at least three years. Interpretations based on rules awareness would predict event time decay in our coefficients, as individuals forget specific rules covered by the exam and learn others more pertinent to their daily work. By contrast, under an ethics-based interpretation, misconduct differences between old and new exam passers persist, because individuals draw more lasting lessons from ethics material.

2.4.3 How do Individual and Firm Characteristics Relate to Exam Coverage and Misconduct?

Our next tests study how the characteristics of the individual or their employer affect the relation between exam content and misconduct. For individual characteristics, we consider whether they had a misconduct record (*Prior Misconduct at S66*), as well as their experience in the securities industry (*Yrs Exp at S66*), before passing the Series 66. For firm characteristics, we measure the percent of other advisers and representatives at the firm with misconduct that year (*Firm Misconduct*). We also assess firm size according to whether the firm (branch, defined as a firm-city combination) has 500 (10) or fewer advisers and representatives. Our tests augment equation (1) with interactions for these individual and firm variables.

In these analyses, we tabulate only the coefficients for $S66\ x\ Ethics\ S66$ and its interaction with individual (Table 2.6) or firm (Table 2.7) characteristics; however, our regression includes all two-way and main effects not subsumed by our fixed effects, as well as our controls from equation (1). Column (1) of Table 2.6 shows a significantly positive coefficient on the triple interaction term for prior misconduct, indicating that the rules/ethics content of the Series 66 is less relevant to those with a misconduct record before the exam. Column (2) studies the length of each adviser's experience when they passed the exam. We find the exam content is most relevant to those who are new to the profession: $S66\ x\ Ethics\ S66$ is most negative and significant for those passing the exam with two or fewer years of experience. The triple interaction for those with three years of experience is less negative and only marginally significant. We find no effect for those with four or more years of experience. Overall, our analysis of individual characteristics indicates that the effects of rules and ethics training on behavior depends on when the adviser passes the exam. Those already engaging in misconduct, or having spent several years working in the securities industry, respond least or not at all. This result echoes one respondent to a Wall Street Journal recruiter survey who said "If you're not ethical by the time you're 27, no classroom experience is going to make a difference" (Alsop, 2007).

Table 2.7 studies firm characteristics. In Column (1) we find the rules and ethics coverage is less consequential for advisers working at firms where misconduct is more widespread (the interaction with *Firm Misconduct* is positive). Economically, doubling the prevalence of misconduct at a firm reduces the misconduct difference between old and new exam passers by nearly half. Column (2) studies firm size, and finds no effect of the exam change for advisers working at small firms (*S66* x *Ethics S66* + *S66* x *Ethics*

S66 x *Small Firm* is statistically indistinguishable from zero). Column (3) repeats this test for small branches. Again, we find less of an effect of the exam change for advisers at small branches (although the t-statistic for the triple interaction is only 1.47).

2.4.4 Advisers' Response to Ethical Scandals

How does the extent of an adviser's rules and ethics training affect their willingness to remain with an employer violating professional standards and drawing attention for its behavior? A salient example of one such employer in our sample is Wells Fargo, one of the largest financial institutions and adviser employers in the US. Starting in 2011, Wells Fargo branch employees began creating fake savings, checking, and credit card accounts without client authorization. The extent of the fraud became widely known in 2016, when the Consumer Financial Protection Bureau (CFPB) revealed that thousands of employees opened over two million fake accounts. Resulting fines and sanctions totaled $185 million, while settlements have exceeded $3 billion.

The fraud had repercussions beyond the consumer banking division of Wells Fargo. A New York Times article describes the reaction of Melissa Kinnard, a former Wells Fargo investment adviser: "Frustrated by what she saw as the bank's culture, Ms. Kinnard quit in January" (Flitter and Cowley, 2019).[52] Incidentally, Kinnard passed the old Series 66 exam, and has no reported misconduct during her 33-year career in the securities industry.

Table 2.8 studies all Wells Fargo advisers and representatives, and investigates whether the propensity to remain at the firm after the scandal broke relates to the Series 66 coverage. Each year, we measure an indicator for whether the individual exits Wells Fargo. We model the exit indicator as a function of time, our Series 66 variables, controls,

[52] Interestingly, in April 2017 Wells Fargo CEO Tim Scott announced an initiative to rehire 1,000 employees who were wrongfully terminated or had quit in protest of fraud (Keller, 2017).

polynomials for years of experience, and city x year fixed effects. While the CFPB announcement, Senate Banking Committee Hearing, fine announcement, and share price drop occurred in fall 2016, reports of an aggressive sales culture at the bank appeared before. In 2015, the City of Los Angeles sued Wells Fargo “for pressuring employees of its retail bank to commit fraudulent acts, such as opening customer accounts without their approval” (Rudegeair, 2015). Later that year, the Office of the Comptroller of the Currency and the San Francisco Federal Reserve created probes of their own (Glazer, 2015). Ultimately, the CFPB built upon these investigations in 2016 as later revealed in FOIA documents (CFPB, 2018). We therefore experiment with different event windows and samples.

Column (1) shows that starting in 2015, those advisers passing the old Series 66 exam are 2.8% more likely than those passing the old exam to exit. This represents a meaningful margin above the 12.9% average exit rate for this sample. Column (2) repeats the test with a sample beginning in 2013 instead of 2012, and finds similar results. Columns (3) and (4) use the year 2016 as the beginning of the post-fraud revelation period, and again finds advisers with the old Series 66 qualification are more likely to leave Wells Fargo.

We then use this case study to motivate an analysis of employee turnover following evidence of a marked shift in the behavior of their colleagues in our entire sample. The first of the four misconduct measures we study is *Misconduct Shock*, based on the percent of a firm’s advisers and representatives involved in a misconduct incident that year. The second is *Misconduct Ever Shock*, based on the percent of a firm’s advisers and representatives with a misconduct incident from a prior year on their record. Thus, the first measure considers the flow of new misconduct, while the second considers the misconduct history of individuals currently working at the firm. Our third (fourth) measure is *Penalty Amount Shock* (*Penalty Number Shock*) which uses the firm's dollar amount of (number of incidents with) damages granted, sanctions or settlements per individual.

The analysis proceeds in four steps. First, for each firm-year we model the four misconduct measures described above as a function of firm size (equal to the log individual count) and firm and year fixed effects. The firm and year fixed effects help us detect deviations in misconduct relative to the firm's long run average and the industry as a whole. We focus on within-firm deviations rather than levels, because firm misconduct culture differs and we presume each individual matched to their firm knowing something about its culture. Second, we extract the residual from the step one regression as our proxy for changes in the firm's misconduct culture. Third, we create an indicator for residuals above the 95th percentile for the sample. Fourth, we model individual exits from the firm as a function of our Series 66 variables and their interaction with this indicator, polynomials for years experience, as well as the controls and fixed effects from equation (1). To ensure the individual was not involved in the scandal, we omit individuals with a misconduct incident on their record that year.

We present the results in Table 2.9, Column (1) shows that at firms experiencing a spike in misconduct that year, advisers with more rules and ethics training are 2.7% more likely to leave. This represents about one-sixth of the average turnover rate for advisers in this sample. Column (2) shows a negative but insignificant coefficient on *Misconduct Ever Shock*. Thus, a spike in new misconduct incidents (Column (1)) appears more likely to trigger turnover in our old Series 66 passers than a rise in the number of advisers with a misconduct history (say, due to hiring advisers with such histories or turnover among those without them).

In terms of financial penalties, both the dollar amount of penalties and the number of incidents involving payment predict turnover for old Series 66 passers, but only the latter are statistically significant at conventional levels. Overall, our results suggest that rules and ethics training affects employer-employee matching through advisers' willingness to remain at firms experiencing scandals.

Our final tests examine an implication of these turnover results: departures by advisers with more ethics training predict future scandals. Based on our Table 2.9 findings, we examine whether advisers prefer to leave firms *before* a scandal breaks. For example, firms may hire individuals with misconduct records, fail to punish transgressions, underinvest in controls that protect investors, or pursue more aggressive sales strategies. Advisers with more ethics training may respond by leaving, before such developments manifest in misconduct.

We study individual exits from firms as a function of these firms' future misconduct, using the lead (year $t+1$) value of our firm misconduct indicators from Table 2.9. As before, we use equation (1) and omit individuals engaging in misconduct themselves that year.

Table 2.10 presents the results. Column (1) shows that, compared to advisers passing the new exam, advisers passing the old exam are 6.5% more likely to leave firms with major scandals and misconduct on the horizon. Notably, the coefficient for *S66* x *Ethics S66* x *Misconduct* $Shock_{t+1}$ is more than double the analogous coefficient based on the contemporaneous misconduct shock from Column (1) of Table 2.9, Columns (2) and (3) also find larger coefficients than the analogous columns in Table 2.9, while Column (4) finds a smaller, although still significant, coefficient.

Overall, our results are consistent with 1) advisers observing signals of future misconduct at their firm, and 2) advisers with more ethics training being just as likely to leave before the signals manifest in misconduct as after. Then, departures of certain types of advisers can reveal the firm's future misconduct. Of course, we cannot observe the exact circumstances under which departures occur. Rather than resignations, departures of ethics-trained advisers may be involuntary, perhaps because the individual refuses to participate in aggressive sales practices, or underperforms in firms where such practices are embraced. However, such departures would also predict future misconduct at the firm.

2.5 Conclusion

We study a 2010 change in the Series 66 exam, which qualifies individuals as investment advisers. The exam shifted emphasis from rules and ethics to technical topics. We use this shift to proxy for the extent of advisers' rules and ethics training, and study their conduct and labor market activity through their career. Comparing two advisers at the same firm location, with the same qualifications, in the same year, we find those with more rules and ethics training are one-fourth less likely to commit misconduct. The misconduct differences are best explained by the exam content change having a direct effect on adviser behavior, instead of unobservable differences between old and new exam cohorts. While both compliance and ethics-based interpretations for our misconduct results may be valid, our analysis of obvious offenses suggests the exam influences perceptions of right and wrong, and not only awareness of specific rules.

We find the exam change was less consequential for those engaging in misconduct before their exam, or working for firms where misconduct is common. As such, prior infractions and contagion of misconduct behavior appears to reduce the effectiveness of the exam at preventing transgressions. Finally, we show when a firm is experiencing a spike in misconduct and financial sanctions, those advisers with more rules and ethics training are more likely to leave. Such departures also predict future misconduct and sanctions.

Overall, our results can be understood through the lens of Becker's model of crime (1968, 1992). In this model, "many people are constrained by moral and ethical considerations, and did not commit crimes even when they were profitable and there was no danger of detection… The amount of crime is determined not only by the rationality and preferences of would-be criminals, but also by the economic and social environment created by… opportunities for employment, schooling, and training programs." (Becker, 1992: 41-42). In our context, ethics training can affect an individual's behavior by increasing the value of their reputation, as well as the psychological costs of committing misconduct. But such effects will be moderated by the employer's culture, which affects the stigma of offenses, as well as the individual's beliefs about appropriate conduct.

While we cannot evaluate all of the tradeoffs behind adviser training, our results are relevant to discussions and analyses of investment adviser misconduct. More importantly, to our knowledge we present the first large sample evidence of rules and ethics training affecting the conduct and labor market activity of individuals in the financial industry.

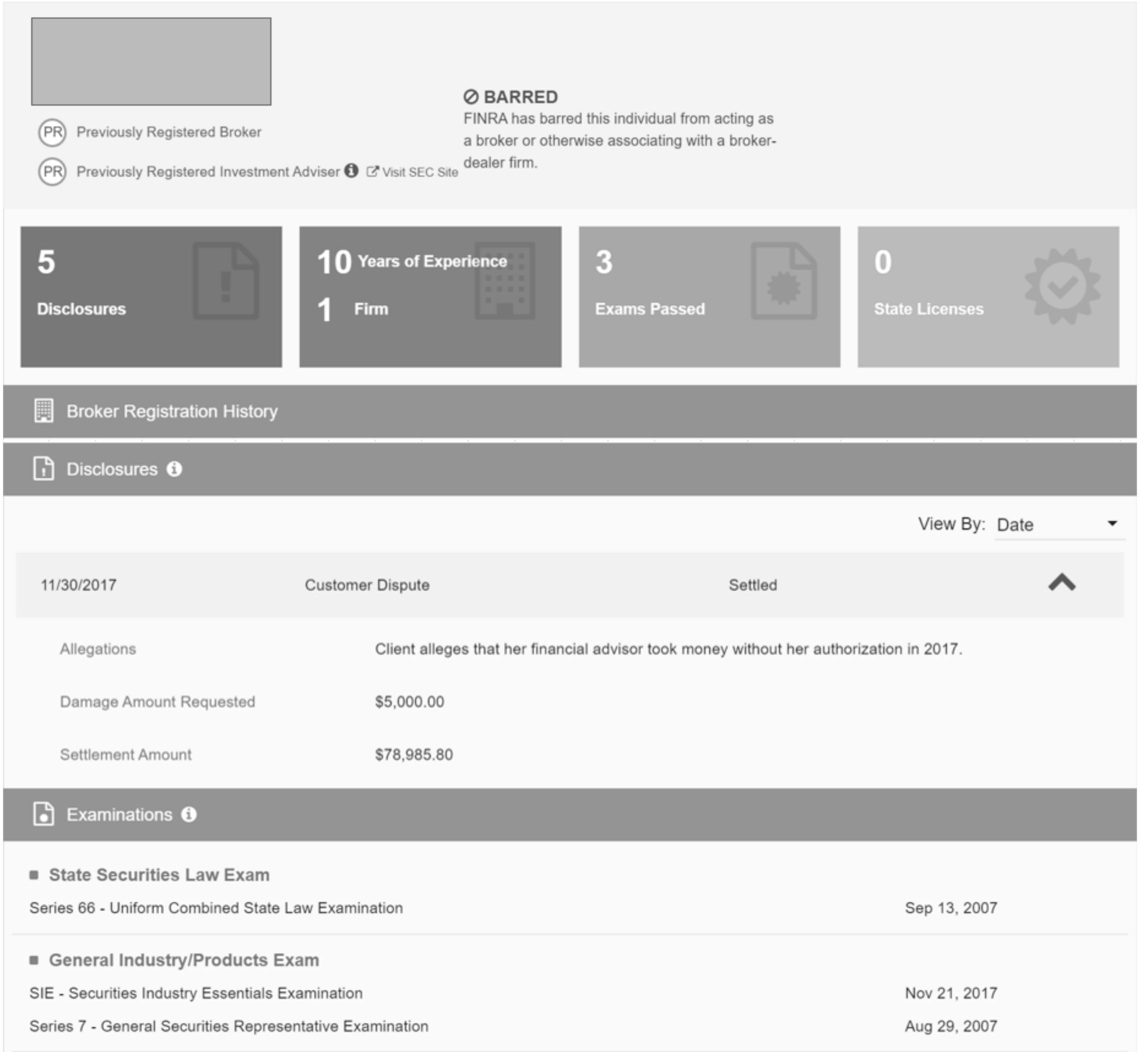
2.6 Appendix 2.A: Variable Definitions

Variable	*Definition*
Misconduct	An indicator equal to one for individuals involved in a misconduct incident at the firm that year, and zero otherwise. Following Egan et al. (2019), misconduct incidents include the following categories: Civil-Final, Criminal-Final Disposition, Customer Dispute-Award/Judgment, Customer Dispute-Settled, Employment Separation after Allegations, and Regulatory-Final.
Obvious Misconduct	An indicator equal to one for individuals involved in an obvious misconduct incident at the firm that year, and zero otherwise. From our original set of misconduct incidents, we use textual analysis to classify obvious cases as those involving fraud, theft, or deception as described in the text.
Exit Firm	An indicator equal to one for individuals who leave their employer that year, and zero otherwise.
Log # Exams	The natural logarithm of the number of securities exams passed that month.
Series 66	An indicator equal to one for individual-years after the individual has passed the Series 66, and zero otherwise.
Ethics 66	An indicator equal to one for advisers who pass the Series 66 before January 1, 2010, and zero otherwise. The variable is recorded as zero until the individual passes the Series 66.
New Placebo Series 7	An indicator equal to one for individuals who pass the Series 7 on or after January 1, 2010, and zero otherwise. The variable is recorded as zero until the individual passes the Series 7.
Prior Misconduct at S66	An indicator equal to one for advisers who had a misconduct incident on their record when they passed the Series 66 exam.
Yrs Exp at S66	A series of indicator variables each equal to one for advisers with various years of experience in the securities industry when they passed the Series 66 exam, and zero otherwise.
Firm Misconduct	The percent of advisers and representatives at the firm with a misconduct incident that year. We measure this for each individual-year observation by omitting the individual themselves from the average calculation, to avoid a mechanical relation.
Small Firm	An indicator equal to one for firms with 500 or fewer advisers and representatives, and zero otherwise.
Small Branch	An indicator equal to one for firm-city pairs with 10 or fewer advisers and representatives, and zero otherwise.
Misconduct Shock	An indicator equal to one for firm-years with an abnormal level of new misconduct that year. We classify an abnormal firm-year as one whose residual from a regression with size controls and year and firm fixed effects is above the 95^{th} percentile.
Misconduct Ever Shock	An indicator equal to one for firm-years with an abnormal percent of individuals with a misconduct history that year. We classify an abnormal firm-year as one whose residual from a regression with size controls and year and firm fixed effects is above the 95^{th} percentile.
Penalty Amount Shock	An indicator equal to one for firm-years with an abnormal level of damages granted, sanctions, and settlements per individual that year. We classify an abnormal firm-year as one whose residual from a regression with size controls and year and firm fixed effects is above the 95^{th} percentile.

Appendix 2.A (continued)

Variable	*Definition*
Penalty Number Shock	An indicator equal to one for firm-years with an abnormal percent of individuals attracting damages, sanctions, and settlements that year. We classify an abnormal firm-year as one whose residual from a regression with size controls and year and firm fixed effects is above the 95th percentile.

2.7 Appendix 2.B: Example Investment Adviser Record on BrokerCheck



PR Previously Registered Broker

PR Previously Registered Investment Adviser Visit SEC Site

⊘ BARRED

FINRA has barred this individual from acting as a broker or otherwise associating with a broker-dealer firm.

5 Disclosures	10 Years of Experience 1 Firm	3 Exams Passed	0 State Licenses

Broker Registration History

Disclosures

View By: Date

11/30/2017	Customer Dispute	Settled

Allegations	Client alleges that her financial advisor took money without her authorization in 2017.
Damage Amount Requested	$5,000.00
Settlement Amount	$78,985.80

Examinations

State Securities Law Exam

Series 66 - Uniform Combined State Law Examination	Sep 13, 2007

General Industry/Products Exam

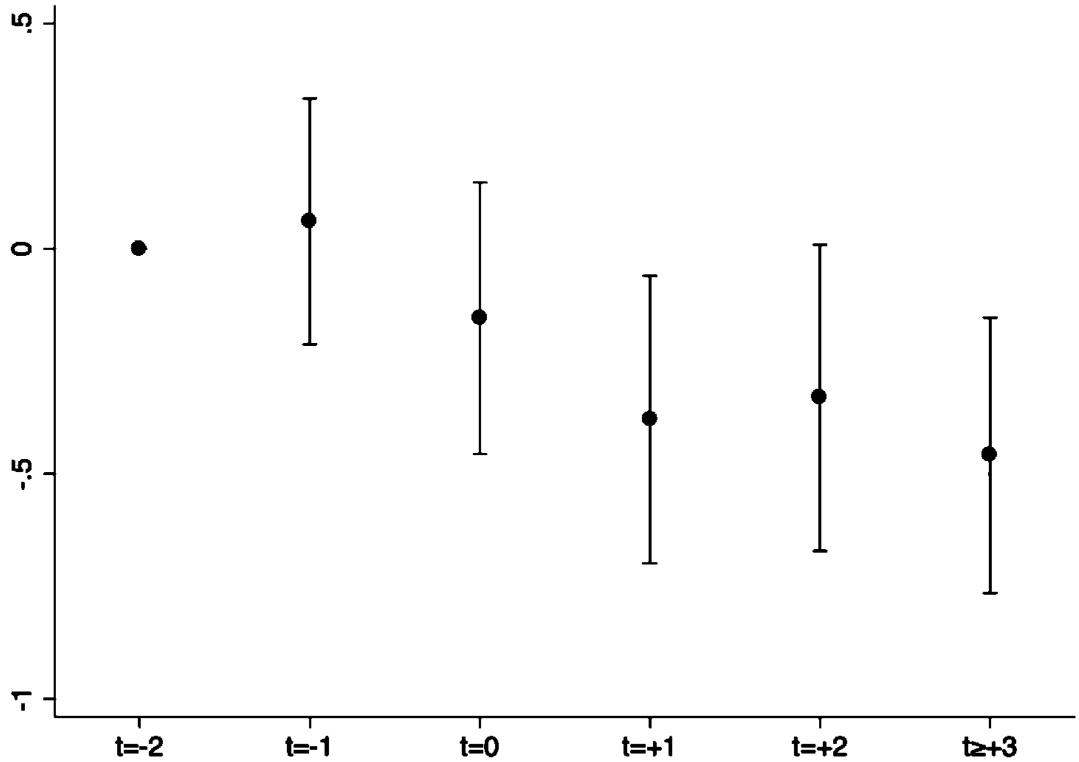
SIE - Securities Industry Essentials Examination	Nov 21, 2017
Series 7 - General Securities Representative Examination	Aug 29, 2007

2.8 Appendix 2.C: Exam Bunching

	(1) Log # Exams	(2) Log # Exams
Oct 2009 x S66	0.015	-0.010
	[0.43]	[-0.21]
Nov 2009 x S66	0.215***	0.322***
	[6.11]	[6.78]
Dec 2009 x S66	0.464***	0.388***
	[13.16]	[8.17]
Jan 2010 x S66	-0.440***	-0.295***
	[-12.48]	[-6.22]
Feb 2010 x S66	-0.203***	-0.091*
	[-5.75]	[-1.91]
Mar 2010 x S66	-0.133***	-0.054
	[-3.78]	[-1.13]
Adj R-Sq.	0.954	0.960
N	336	144
Cluster by Month-Year	Yes	Yes
Sample Years	2008-2011	2008-2011
Sample Exams	All	S63, S65, S66
Month-S66 FEs	Yes	Yes
Month-Year FEs	Yes	Yes
Exam Type FEs	Yes	Yes

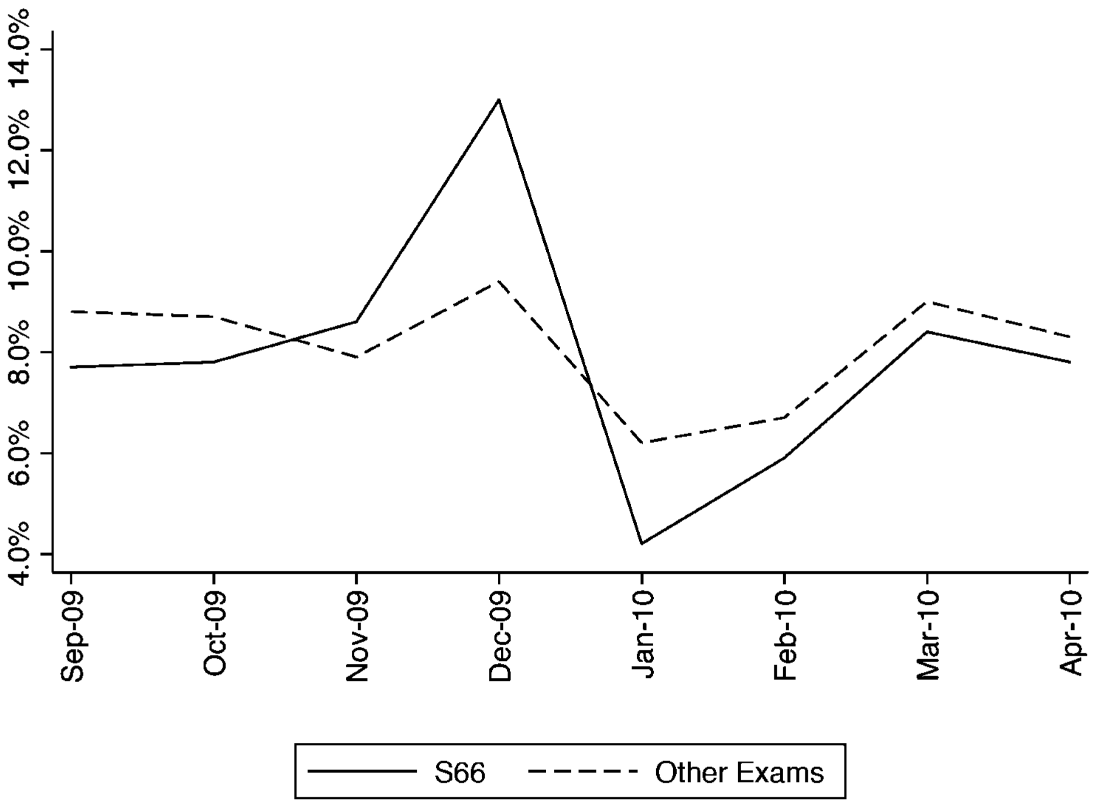
This table models the number of exams passed as a function of time. The dependent variable is the log number of exams passed. The unit of observation is exam type-month-year. The sample in Column (1) contains observations from the Series 6, 7, 24, 63, 65, and 66. The sample in Column (2)n contains only observations from the Series 63, 65, and 66, which all experienced a similar change in minimum passing grade around January 2010. Reported below the coefficients are t-statistics calculated with standard errors clustered at the month-year level. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See Appendix 2.A for variables definitions.

Figure 2.1: Series 66 Exam Type and Misconduct in Event Time



This figure plots event year coefficients and confidence intervals obtained from estimating the following model: $y_{ijt} = \sum_{t-2}^{t\geq 3} \beta \times Ethics66_i \times S66_{it} + S66_{it} + \alpha_i + \alpha_{jt} + \gamma \times Controls_{it} + \varepsilon_{ijt}$. The X-axis labels the event year(s) for each coefficient marking an event year relative to the investment adviser's exam at t=0. We omit the indicator for t-2, which serves as the benchmark period. Vertical bands represent 90% confidence intervals for the point estimates in each event year, and are calculated based on standard errors clustered at the firm level. We drop event-time observations prior to t-2 to ensure common support for pre- and post-Series 66 exam passers.

Figure 2.2: Exams Passed by Exam Type between September 2009 and April 2010



This figure presents the share of exam passers by month and exam type. Each month, we divide the number of exam passers by the number of exam passers for the calendar year ("Month Shares").

Table 2.1: Sample Construction Misconduct Analysis

Firm-Adviser-Year observations from 2007-2017	8,838,880
Less Observations from:	
Advisers who pass the S66 between October 2009 and March 2010	(73,159)
Advisers who pass the S66 after July 2016	(90,626)
Year of Adviser's S66 exam	(174,642)
Final Sample for Table 2.3 to 2.6 misconduct analyses	8,500,453

This table describes our sample construction.

Table 2.2: Summary Statistics

Panel A: Individual Firm-Year Variables

	Mean	Std. Dev.	25%	50%	75%	N
Misconduct (%)	0.764	8.709	0.000	0.000	0.000	8,500,453
Egregious Misconduct (%)	0.221	4.692	0.000	0.000	0.000	8,500,453
Misconduct Ever (%)	7.879	26.941	0.000	0.000	0.000	8,500,453
Exit Firm	0.167	0.373	0.000	0.000	0.000	8,500,453
Years Experience	12.955	9.587	5.000	11.000	19.000	8,500,453
S6	0.371	0.483	0.000	0.000	1.000	8,500,453
S7	0.655	0.475	0.000	1.000	1.000	8,500,453
S24	0.146	0.353	0.000	0.000	0.000	8,500,453
S63	0.723	0.447	0.000	1.000	1.000	8,500,453
S65	0.202	0.401	0.000	0.000	0.000	8,500,453
S66	0.200	0.400	0.000	0.000	0.000	8,500,453
Ethics S66	0.162	0.369	0.000	0.000	0.000	8,500,453
Other Exam	0.308	0.462	0.000	0.000	1.000	8,500,453

Panel B: Series 66 Exam Passer Characteristics at Series 66 Date

	Mean	Std. Dev.	25%	50%	75%	N
Years Experience	4.718	6.187	1.000	2.000	6.000	263,924
Misconduct Ever	0.040	0.196	0.000	0.000	0.000	263,924

This table summarizes the individual-firm-year and exam passer variables in our sample. In Panel B, we measure characteristics of only Series 66 passers, at the time they passed their exam.

Table 2.3: Exam Coverage and Adviser Misconduct

	(1) Misconduct	(2) Misconduct	(3) Misconduct	(4) Misconduct	(5) Misconduct
S66	0.210***	0.217***	0.113**	0.054	
	[4.53]	[4.70]	[2.33]	[1.07]	
S66 x Ethics S66	-0.162*	-0.168*	-0.197**	-0.238***	-0.201**
	[-1.82]	[-1.92]	[-2.38]	[-2.88]	[-2.24]
Adj R-Sq.	0.166	0.169	0.178	0.214	0.219
N	8,423,524	8,421,628	8,379,914	7,851,574	7,630,507
Controls	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	No	No	No
Individual FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	No	Yes	No	No	No
Firm-Year FEs	No	No	Yes	No	No
Firm-Year-City FEs	No	No	No	Yes	No
Firm-Year-City-S66 FEs	No	No	No	No	Yes

This table models individual misconduct as a function of exam coverage using equation (1). The unit of observation is individual-firm-year. The dependent variable is *Misconduct*, the flow of new misconduct for the individual during the year. The regression sample is defined in Table 2.1. Reported below the coefficients are t-statistics calculated with standard errors clustered at the firm level. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See Appendix 2.A for variables definitions.

Table 2.4: Exam Coverage and Adviser Misconduct—Robustness Analysis

	(1) Misconduct	(2) Misconduct	(3) Misconduct	(4) Misconduct	(5) Misconduct
	Full Sample	Has Pre-2010 Exam	Has Recession Experience	Series 66 2008-2011	Full Sample
S66	0.004	0.069	0.070	0.095	
	[0.07]	[1.31]	[1.34]	[1.11]	
S66 x Ethics S66	-0.155*	-0.214**	-0.214**	-0.332***	
	[-1.82]	[-2.40]	[-2.40]	[-3.27]	
S7 x New Placebo S7					-0.039
					[-0.37]
Adj R-Sq.	0.214	0.226	0.225	0.213	0.217
N	7,851,535	6,574,094	6,638,732	6,449,919	7,696,320
Controls	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes
Firm-Year-City FEs	Yes	Yes	Yes	Yes	Yes
Cohort-Year-Year FEs	Yes	No	No	No	No

This table models individual misconduct as a function of exam coverage using equation (1). The unit of observation is individual-firm-year. The dependent variable is *Misconduct*, the flow of new misconduct for the individual during the year. The regression sample is defined in Table 2.1, and further restricted as labeled in the column headers. Reported below the coefficients are t-statistics calculated with standard errors clustered at the firm level. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See Appendix 2.A for variables definitions.

Table 2.5: Exam Coverage and Obvious Misconduct

	(1) Obvious Misconduct
S66	0.023
	[1.20]
S66 x Ethics S66	-0.146***
	[-3.60]
Adj R-Sq.	0.147
N	7,851,574
Controls	Yes
Individual FEs	Yes
Firm-Year-City FEs	Yes

This table models obvious misconduct as a function of exam coverage using equation (1). The unit of observation is individual-firm-year. The dependent variable is *Obvious Misconduct*, the flow of new misconduct involving fraud, theft, or deception for the individual during the year. The regression sample is defined in Table 2.1. Reported below the coefficients are t-statistics calculated with standard errors clustered at the firm level. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See Appendix 2.A for variables definitions.

Table 2.6: Individual Characteristics

	(1) Misconduct	(2) Misconduct
S66 x Ethics S66	-0.399***	
	[-5.12]	
S66 x Ethics S66 x Prior Misconduct at S66	0.036***	
	[3.76]	
S66 x Ethics S66 x <=2 Yrs Exp at S66		-0.589***
		[-5.09]
S66 x Ethics S66 x 3 Yrs Exp at S66		-0.411
		[-1.53]
S66 x Ethics S66 x 4 Yrs Exp at S66		-0.095
		[-0.46]
S66 x Ethics S66 x >=5 Yrs Exp at S66		-0.071
		[-0.56]
Adj R-Sq.	0.214	0.214
N	7,851,574	7,851,574
Controls, Main and Two-Way Effects	Yes	Yes
Individual Fes	Yes	Yes
Firm-Year-City FEs	Yes	Yes

This table models individual misconduct as a function of exam coverage and individual characteristics using equation (1). The unit of observation is individual-firm-year. The dependent variable is *Misconduct*, the flow of new misconduct for the individual during the year. *Prior Misconduct at S66* is an indicator for whether the individual had a misconduct record at the time they passed the Series 66. *Yrs Exp at S66* is an indicator for various levels of adviser years of experience at the time they passed the Series 66. The regression sample is defined in Table 2.1. Reported below the coefficients are t-statistics calculated with standard errors clustered at the firm level. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See Appendix 2.A for variables definitions.

Table 2.7: Firm Characteristics

	(1) Misconduct	(2) Misconduct	(3) Misconduct
S66 x Ethics S66	-0.324*** [-3.73]	-0.286*** [-3.48]	-0.259*** [-3.15]
S66 x Ethics S66 x Firm Misconduct	0.204*** [4.15]		
S66 x Ethics S66 x Small Firm		0.330*** [3.82]	
S66 x Ethics S66 x Small Branch			0.142 [1.57]
Adj R-Sq.	0.215	0.214	0.214
N	7,851,574	7,851,574	7,851,574
Controls, Main and Two-Way Effects	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes
Firm-Year-City FEs	Yes	Yes	Yes

This table models individual misconduct as a function of exam coverage and firm characteristics using equation (1). The unit of observation is individual-firm-year. The dependent variable is *Misconduct*, the flow of new misconduct for the individual during the year. *Firm Misconduct* is the percent of advisers and representatives at the firm with a misconduct incident on their record before that year. *Small Firm* (*Small Branch*) is an indicator for firms (firm-city combinations) with fewer than 500 (10) advisers and representatives. The regression sample is defined in Table 2.1. Reported below the coefficients are t-statistics calculated with standard errors clustered at the firm level. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See Appendix 2.A for variables definitions.

Table 2.8: Exam Coverage and Adviser's Response to Ethical Scandals: Wells Fargo

	(1) Exit Firm	(2) Exit Firm	(3) Exit Firm	(4) Exit Firm
	Year>2011	Year>2012	Year>2011	Year>2012
S66 x Ethics S66 x Year>=2015	0.028***	0.031***		
	[3.18]	[3.39]		
S66 x Ethics S66 x Year>=2016			0.040***	0.042***
			[4.87]	[5.22]
Adj R-Sq.	0.052	0.047	0.052	0.047
N	200,074	167,077	200,074	167,077
Controls and Main Effects	Yes	Yes	Yes	Yes
Controls for Polynomials of Experience	Yes	Yes	Yes	Yes
City-Year FEs	Yes	Yes	Yes	Yes

This table models individual turnover as a function of exam coverage. The unit of observation is individual-firm-year. The dependent variable is *Exit Firm*, an indicator equal to one if the individual leaves the firm that year. The sample is limited to individuals employed by Wells Fargo and the years labeled at the top of the column. Reported below the coefficients are t-statistics calculated with standard errors clustered at the city level. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See Appendix 2.A for variables definitions.

Table 2.9: Exam Coverage and Adviser's Response to Ethical Scandals: Full Sample

	(1) Exit Firm	(2) Exit Firm	(3) Exit Firm	(4) Exit Firm
S66 x Ethics S66	-0.026***	-0.025***	-0.029***	-0.029***
	[-3.48]	[-3.29]	[-3.79]	[-3.77]
S66 x Ethics S66 x Misconduct Shock	0.027***			
	[2.94]			
S66 x Ethics S66 x Misconduct Ever Shock		0.022		
		[1.21]		
S66 x Ethics S66 x Penalty Amount Shock			0.009	
			[1.58]	
S66 x Ethics S66 x Penalty Number Shock				0.051**
				[2.28]
Adj R-Sq.	0.359	0.359	0.359	0.359
N	7,787,627	7,787,627	7,787,627	7,787,627
Controls and Main Effects	Yes	Yes	Yes	Yes
Controls for Polynomials of Experience	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes
Firm-Year-City FEs	Yes	Yes	Yes	Yes

This table models individual turnover as a function of exam coverage. The unit of observation is individual-firm-year. The dependent variable is *Exit Firm*, an indicator equal to one if the individual leaves the firm that year. *Misconduct Shock* (*Misconduct Ever Shock*) is an indicator equal to one for firm-years whose abnormal misconduct that year (the percent of advisers and representatives with a misconduct history) is above the 95th percentile. *Penalty Amount Shock* (*Penalty Number Shock*) is an indicator equal to one for firm-years whose damages granted, sanctions, and settlements per individual (percent of advisers and representatives attracting damages, sanctions, and settlements) is above the 95th percentile. Reported below the coefficients are t-statistics calculated with standard errors clustered at the firm level. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See Appendix 2.A for variables definitions.

Table 2.10: Exam Coverage, Adviser Exits, and Future Ethical Scandals

	(1) Exit Firm	(2) Exit Firm	(3) Exit Firm	(4) Exit Firm
S66 x Ethics S66	-0.030***	-0.025***	-0.031***	-0.030***
	[-3.80]	[-3.31]	[-4.06]	[-3.84]
S66 x Ethics S66 x Misconduct Shock$_{t+1}$	0.065***			
	[4.09]			
S66 x Ethics S66 x Misconduct Ever Shock$_{t+1}$		0.024***		
		[2.92]		
S66 x Ethics S66 x Penalty Amount Shock$_{t+1}$			0.013***	
			[3.07]	
S66 x Ethics S66 x Penalty Number Shock$_{t+1}$				0.034***
				[3.75]
Adj R-Sq.	0.359	0.359	0.359	0.359
N	7,787,627	7,787,627	7,787,627	7,787,627
Controls and Main Effects	Yes	Yes	Yes	Yes
Controls for Polynomials of Experience	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes
Firm-Year-City FEs	Yes	Yes	Yes	Yes

This table models individual turnover as a function of exam coverage. The unit of observation is individual-firm-year. The dependent variable is *Exit Firm*, an indicator equal to one if the individual leaves the firm that year. *Misconduct Shock* (*Misconduct Ever Shock*) is an indicator equal to one for firm-years whose abnormal misconduct *next year* (the percent of advisers and representatives with a misconduct history *next year*) is above the 95th percentile. *Penalty Amount Shock* (*Penalty Number Shock*) is an indicator equal to one for firm-years whose damages granted, sanctions, and settlements per individual *next year* (percent of advisers and representatives attracting damages, sanctions, and settlements *next year*) is above the 95th percentile. Reported below the coefficients are t-statistics calculated with standard errors clustered at the firm level. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See Appendix 2.A for variables definitions.

3 Mandatory Peer Review and CPA Entrepreneurship

3.1 Introduction

I study whether mandatory peer review affects CPA entrepreneurship—that is, CPAs' decisions to start, continue, or cease operating their own CPA firms. In an effort to promote audit quality, CPA firms have to be reviewed by other CPA firms to meet CPA firm licensing requirements. Proponents of this peer review system argue that it allows to leverage industry expertise. Critics, on the other hand, contend that peer review lacks the necessary independence that effective oversight requires. While peer review is the main oversight mechanism for CPA firms without public clients, little is known about its consequences.

I study the consequences of mandatory peer review in the context of CPA entrepreneurs for two main reasons. First, this provides a setting in which litigation costs and reputational capital at risk—factors that prior literature has identified to shape the audit environment—are less severe. Focusing on a setting in which these factors are hardly present provides an opportunity to empirically isolate potential effects of mandatory peer review (Vanstraelen and Schelleman, 2017). Second, there is a wider and open debate whether imposing licensing requirements—such as mandatory peer review—fosters or hampers entrepreneurial activity.

On the one hand, mandatory peer review may foster CPA entrepreneurship. Current CPA firm licensing requirements prescribe peer review in an effort to maintain and promote service quality. Theory posits that such licensing requirements can provide minimum quality standards, reduce quality uncertainty, and, ultimately, prevent market failures (Akerlof, 1970; Leland, 1979). Recent entrepreneurship research argues that this may particularly benefit entrepreneurs, who lack alternative means—such as reputation—to credibly signal their quality (Albert et al., 2019).[53]

On the other hand, mandatory peer review may hamper CPA entrepreneurship. Requiring participation in peer review programs to meet CPA licensing standards entails compliance costs and entrepreneurs may be particularly sensitive to such costs, presumably because they face acute resource constraints. Regulators are increasingly concerned that compliance costs present barriers to entrepreneurship, negatively affecting the rate of entrepreneurial activity. In fact, the U.S. Department of the Treasury Office of Economic Policy, the Council of Economic Advisers, and the Department of Labor (2015) place potential adverse effects of licensing-induced compliance costs among their top reasons why licensing regimes may necessitate immediate reform efforts.

To empirically study the effects of mandatory peer review on CPA entrepreneurship, I construct a novel dataset based on CPA (firm) licenses, which allows to observe CPA entrepreneurs,[54] and exploit the state-level staggered introduction of peer review mandates in a difference-in-differences (DiD) design. This research design allows to control for general time trends as well as time-invariant state-level factors that may possibly also correlate with CPA entrepreneurship. A potential concern with this research design that is shared with virtually any study investigating policy interventions is that regulation does not occur in a vacuum (Ball, 1980; Leuz and Wysocki, 2016; Karpoff and

[53] In addition, based on research in the sociology of occupations, Albert et al. (2019) argue that licensing may not only help to overcome information asymmetry problems within an occupation but also by boosting the legitimacy of an occupation relative other occupations offering substitute services. In the context of tax preparers, the authors argue (and provide some cross-sectional evidence consistent with the idea) that licensing provides a signal to consumers that tax preparers are more legitimate than providers of substitute services, e.g., tax software providers.

[54] In order to start a CPA firm, an individual must hold a CPA license. For CPAs, licensing does not only apply at the individual level but also at the firm level and both of these licenses can be observed through license verification tools. Crucially, some states also provide CPA firm ownership data which allow to infer founding manager-owners, i.e., CPA entrepreneurs. Thus, CPA licenses allow to observe actual (prospective) CPA entrepreneurs (at least in the short run). Throughout this paper, I follow recent literature (as well as Census definitions) and define an entrepreneur as “someone who launches a business” (e.g., Levine and Rubinstein, 2017).

Wittry, 2018; Leuz, 2018). To alleviate such concerns, I explore differences in the characteristics of early- and late-adopting states. Comparisons suggest that the adoption timing is partly driven by the organizational structures of State Boards of Accountancy, which mitigates related concerns and provides some insights into the political economy of audit market regulation.

Turning to my main analysis, I first explore whether the *announcement* of peer review mandates affects the probability of individual CPAs to become CPA entrepreneurs. To do so, I estimate DiD models comparing the probabilities before and after a peer review mandate announcement relative to a control group. The estimates show that CPA entrepreneurship declines with the announcement of peer review mandates. The estimates are statistically significant and economically meaningful. Compared to an unconditional CPA entrepreneurship rate of 0.7%, the most conservative estimates suggest that the likelihood of an individual to start a CPA firm declines by around 14%. The validity of these estimates critically assumes that trends in CPA entrepreneurship rates would move in parallel absent any regulatory intervention. Evidence suggests that probabilities do not differ before the announcement date but gradually decline with the announcement of peer review mandates, which provides some assurance that the main identifying assumption is not violated.

In the next step of my analysis, I compare exit rates of CPA entrepreneurs in the treatment group to exit rates of CPA entrepreneurs in the control group and find that exit rates double with the introduction of mandatory peer review. Mapping out exit rates in event time shows that these increases occur with the introduction of mandatory peer review but not before.

The findings, thus far, indicate that CPA entrepreneurship declines with the introduction of mandatory peer review, which is in line with research investigating the effects of entry costs on firm formation (e.g., Djankov et al., 2002; Klapper et al., 2006; Branstetter et al., 2011). However, a remaining question is whether the reported entry and exit patterns are consistent with mandatory peer review promoting screening on service quality. While the findings do not support the idea that mandatory peer review sparks entrepreneurial activity by boosting the legitimacy of the profession (Albert et al., 2019), the results may still be in line with the (stated) policy objective if mandatory peer review facilitates screening out CPA entrepreneurs that provide low-quality services.

To investigate whether mandatory peer review facilitates active screening on quality, I explore potential heterogenous exit effects based on CPA entrepreneur (pre-treatment) service quality. Specifically, I conduct tests akin to DeFond and Lennox (2011), who investigate audit firm exits around the introduction of Public Company Accounting Oversight Board (PCAOB) inspections. Following the conceptual arguments by DeFond and Lennox (2011), I argue that if mandatory peer review facilitated active screening on quality, we would expect that providers of low-quality services predominantly account for observed exits, since, at the margin, it is more costly for these entrepreneurs to meet more stringent regulatory requirements. Cross-sectional tests comparing changes in exit rates for CPA entrepreneurs subject to disciplinary actions prior to the introduction of mandatory peer review—that is, CPA entrepreneurs fined by State Boards of Accountancy for substandard service provision—to those that are not do not suggest that providers of low-quality services exit the market at higher rates with the introduction of mandatory peer review.

I proceed by exploring which CPA entrepreneurs account for the stark increase in exit rates. Prior literature proposes that increases in licensing requirements may overly affect historically underrepresented groups and suggests different mechanisms through which this may occur. Specifically, Law and Marks (2009; 2017) offer conceptual arguments that an increase in licensing requirements may either harm historically underrepresented groups as it gives rise to discriminatory practices—such as "group favoritism"—or as these groups find it more costly to meet licensing standards. To test whether this is the case and, if so, through which mechanisms, I provide several additional cross-sectional tests.

I first explore whether exit rates are pronounced for (relatively) underrepresented groups.[55] Estimates show that increases in exit rates are pronounced for female but not for non-white CPA entrepreneurs. These results are consistent with the idea that increases in licensing requirements reduce the presence of historically underrepresented groups because they find it more costly to meet these requirements but are not consistent with group favoritism. If mandatory peer review gave rise to group favoritism, we would expect to observe similar exit patterns for non-white CPA entrepreneurs (Egan et al., 2018).[56]

To further substantiate which mechanisms may be at play, I explore differences in exit rates by gender and age and find that exits are most pronounced for young female CPA entrepreneurs. There are two main explanations for this pattern.[57] First, there may be differences in revenues between young female and male CPA entrepreneurs. Gurley-

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[55] A related literature investigates the effects of licensing on minority labor participation (see, for instance, Federman et al., 2006; Law and Marks, 2007; 2012; 2017; Klein et al., 2012; Blair and Chung, 2018a; 2018b). Thus far, this literature arrives at mixed conclusions. One of the reasons for the mixed evidence might be that it is challenging to identify actual licensees within the (aggregated and anonymized Census) data prior literature primarily uses (see the discussion in Klein et al., 2012; Law and Marks, 2012; as well as Gittleman et al., 2018). My approach that directly builds on licensing data and uses variation in regulatory requirements (for licensees) may provide an alternative that helps to overcome some of the data limitations this literature faces.

[56] Recent work by Egan et al. (2018) shows that women face harsher career outcomes than men subsequent to (the same) low-quality service provision. Egan et al. (2018) attribute this "gender punishment gap" to group favoritism. Following their conceptual arguments, I explore potential differences in policy responses for both female (relative to male) and white (relative to non-white) CPA entrepreneurs. The rationale for this test is that if the observed differences in exit rates across gender were due to group favoritism, we would expect to observe similar exit patterns for non-white CPA entrepreneurs.

[57] Prior literature has also proposed other explanations such as differences in (re-)financing availability (e.g., Lee et al., 1999). Such explanations, however, seem less likely in light of the observed differences in exit rates across gender *and* age.

Calvez et al. (2009) report that young female entrepreneurs allocate less time to working in their firms than male entrepreneurs due to childcare. The (related) second explanation is rooted in the literature on household production and proposes that women show greater income elasticities than men due to a larger number of substitution margins—such as childcare (e.g., Mincer, 1962; Becker, 1965; Heckman, 2015). Additional tests show that the increase in exit rates among young female (but not young male) CPA entrepreneurs is even more pronounced in areas with limited childcare availability, which hints at the latter explanation.

I provide several additional tests. First, I use Census data on CPA sole proprietors and show that the *stock* of CPA sole proprietors declines with the introduction of mandatory peer review, while revenues accruing to CPA sole proprietors remaining in the market stay constant.[58] Second, I match CPA entrepreneur license data with firm incorporation (and dissolution) documents and show that some CPA entrepreneurs do not only cease to hold CPA firm licenses but dissolve their firms altogether. Similar to prior findings, increases in firm dissolutions are pronounced for female CPA entrepreneurs but not for non-white CPA entrepreneurs. Third, I collect CPA entrepreneur entity filing histories and provide evidence alleviating concerns about concurrent changes in CPA firm ownership or firm relocation around the introduction of mandatory peer review.

This paper makes three distinct contributions. First, this paper provides novel evidence on the audit market consequences of mandatory peer review. Prior literature provides some evidence on the relation between peer review and audit quality (e.g., Hilary

[58] As I outline in Appendix 3.B.1. and 3.B.2., the ownership information required to infer CPA entrepreneurs in combination with information on disciplinary actions is only available for two states, i.e., Colorado and Texas. While these states are among the largest in terms of the number of CPAs (Barrios, 2019), there may be concerns that my findings do not carry over to other states. My additional Census-based estimates mitigate such concerns.

and Lennox, 2005; Casterella et al., 2009; Anantharaman, 2012).[59] I add to this literature by showing that mandatory peer review hampers CPA entrepreneurship. In addition, the findings do not suggest that CPA entrepreneurs subject to disciplinary actions exit the market at higher rates, which questions to what extent mandatory peer review facilitates active screening on service quality. The findings may even hint at (unintended) effects on participation outweighing (intended) quality screening effects.

Second, this paper contributes to a nascent literature at the intersection of accounting and labor economics (e.g., Aobdia et al., 2017; Bloomfield et al., 2017; Barrios, 2019; Cascino et al., 2020). This literature investigates the audit labor market composition and how institutions shape the audit supply. Aobdia et al. (2017) provide evidence on the composition of the audit workforce. Bloomfield et al. (2017) assess the effects of regulatory harmonization on cross-border migration of accounting professionals. Cascino et al. (2020) provide the first evidence on the labor market effects of moving from state- to national-level CPA licensing regimes. Barrios (2019) investigates the labor market consequences of increasing the licensing requirements for individual CPAs. My paper adds to this literature by showing that the design of CPA licensing and oversight regimes may also have meaningful effects on CPA entrepreneurship and, thus, on accounting professionals' labor participation choices.

Third, this paper is among the first to provide evidence on the relation between licensing regulation and entrepreneurship. Prior empirical work exploring this relation, thus far, provides primarily cross-sectional and inconclusive evidence (e.g., Rostam-Afschar, 2014; Albert et al., 2019). Part of the reason for the inconclusive findings may

[59] Recent literature exploring the consequences of statutory PCAOB oversight primarily finds positive effects (e.g., Gipper et al., 2019). Since the audit firms studied in this literature (often) transition from peer review to statutory PCAOB oversight (Loehlein, 2016), this evidence can be interpreted as PCAOB inspections yielding desirable effects vis-à-vis peer review. However, this literature does not (and does not intend to) provide direct evidence on the consequences of maintaining (or mandating) peer review as an oversight mechanism.

be data limitations or lack of plausibly exogenous variation in licensing requirements.[60] My findings suggest that increases in licensing requirements primarily hamper entrepreneurship and may even harm historically underrepresented groups within regulated professions. In this respect, my findings provide empirical support for the conjecture by Haltiwanger (2015) that increases in licensing regulation may have contributed to the substantial decline in U.S. entrepreneurial activity and may inform current regulatory debates on licensing reform efforts.

The remainder of this paper unfolds as follows. Section 3.2 introduces the institutional setting. Section 3.3 discusses the research design. Section 3.4 describes the main data sources and provides comprehensive descriptive statistics. Section 3.5 presents the empirical analyses. Section 3.6 concludes.

3.2 Institutional Background

Peer review has historically been the main way of monitoring quality among CPA firms that provide mandated financial statement audits, i.e., public client audits (Anantharaman, 2012; Loehlein, 2016). Under the peer review system, CPA firms select another CPA firm to review its engagements and internal controls. Peer review started in the 1970s as a response to a wave of audit failures (see Loehlein, 2016, for a review). The resulting self-regulated peer review system, however, has been subject to substantial critique. This critique is primarily based on descriptive and anecdotal evidence. For instance, most peer reviews result in unmodified reports (Hilary and Lennox, 2005), and most audit failures involve peer-reviewed firms (Fogarty, 1996). Others argue that peer review may not be effective due to the general lack of independence among reviewers

[60] Studying entrepreneurship is generally challenging due to the absence of readily available data on both prospective entrepreneurs, which can be essentially anyone, and actual entrepreneurs (Kerr et al., 2017). Leveraging licensing data provides one alternative that may help to overcome some of these data limitations.

and reviewees (DeFond, 2010). In line with this idea, Anantharaman (2012) as well as Lennox and Pitman (2010) provide evidence that CPA firms select reviewers issuing favorable reports.[61]

Other empirical research, in contrast, attributes positive effects to peer review. For instance, Casterella et al. (2009) provide cross-sectional evidence based on malpractice claims alleging auditor negligence. They find that some (adverse) peer review findings positively correlate with audit failures and conclude that their results are "encouraging and supportive of the effectiveness of a self-regulatory peer-review regime" (Casterella et al., 2009: 732).[62]

Ultimately, the creation of the PCAOB ended self-regulated oversight for public client audit engagements. Empirical work investigating the transition from self-regulated peer review to statutory PCAOB inspections documents positive effects and concludes that the introduction of the PCAOB yields desirable outcomes.[63] Despite this implicit failure of self-regulated peer review vis-à-vis statutory PCAOB inspections (Casterella et

[61] Theory predicts such opinion shopping. For instance, Tirole (1986) shows that peer review is, in general, biased towards positive reported outcomes and, when allowing for side payments, not effective in detecting low quality. Interestingly, another stream of analytical work (e.g., Baliga and Sjostrom, 2001) shows that peer monitoring may result in "predative" reviews, i.e., peer reviewers might issue unfavorable peer review reports to curb competition. Anantharaman (2012) provides evidence consistent with this notion. She finds that reviewers operating in the same (client) industry as well as geographically closer reviewers are less likely to issue favorable peer reviews. She concludes that this result is due to greater reviewee-specific expertise. The finding, however, is also consistent with the Baliga and Sjostrom (2001) model, in which close competitors have incentives to issue less favorable reports to challenge a competitor's standing. Due to data availability constraints and the focus on the relation between peer review mandates and CPA entrepreneurship, differentiating between these two possibilities is beyond the scope of this paper.

[62] Other studies investigate the link between peer review and perceived audit quality. For instance, Hilary and Lennox (2005) report positive correlations between clean (adverse) peer review reports and client gains (losses).

[63] For instance, DeFond and Lennox (2011) provide evidence suggesting that, at the margin, low-quality CPA firms leave the public client audit market with the introduction of the PCAOB. Similarly, studies investigating the effects of statutory PCAOB inspections suggest that this statutory regime yields desirable effects (e.g., Aobdia, 2017; Aobdia and Shroff, 2017; DeFond and Lennox, 2017; Gipper et al., 2019). Some of these studies use DiD approaches, in which the control group consists of auditor-client pairs not (yet) subject to PCAOB inspections. Since public client auditors were subject to peer review prior to introduction of PCAOB inspections, this stream of research can be interpreted as providing evidence of statutory oversight being more effective than self-regulated peer review. However, this line of research does not (and does not intend to) directly speak to the consequences of maintaining or mandating peer review.

al., 2009), peer review remains the main oversight mechanism for CPA firms without public clients and has been significantly extended in scope. In fact, peer review became mandatory for CPA firms regardless of their client portfolios. Over time, U.S. states introduced peer review as a necessary condition for holding a CPA firm license, resulting in mandatory peer review for CPA firms.[64]

In Table 3.1, I show the mandatory peer review adoption dates for all states adopting mandatory peer review as of 2007.[65] In addition, I tabulate State Board of Accountancy characteristics in Panel B. I provide mean estimates of these board characteristics together with *t*-tests comparing the mean characteristics of states that adopted peer review 2007 (early adopters) and states switching to mandatory peer review after 2007 (late adopters). The mean comparisons of State Board of Accountancy characteristics reveal that the board size of State Boards of Accountancy differs between early and late adopting states. Comparing compositional differences related to professional association, i.e., state-specific shares of CPAs vis-à-vis public (non-CPA) members, does not suggest substantial differences, however. Next, I compare the share of board members with ties to the American Institute of Certified Public Accountants (AICPA). The motivation for this comparison is that the AICPA is the main proponent of peer review and also the professional organization providing the largest peer review program. Therefore, one might argue that the (self-regulated) AICPA pursues its own

[64] Prior to peer review mandates enrolment in peer review programs was only required for AICPA member firms. To this end, part of my identification strategy using state-level mandates is akin to the approach in Barrios (2019), who investigates the effects of the 150-hour rule on CPA labor market outcomes. Similar to the peer review requirement, the 150-hour requirement was part of (voluntary) AICPA membership requirements (Barrios, 2019: 7-8) and only became mandatory for all (prospective) CPAs with the passage of state-level mandates. Note that the possibility to voluntarily enroll in a peer review program works against finding potential effects but helps in separating effects of licensing requirements, i.e., mandatory requirements, from potential certification, i.e., voluntary commitment, effects. For a detailed discussion of the differences between licensing and certification, see Kleiner (2000), Klein et al. (2012) and Law and Marks (2012).

[65] I collect these adoption dates from AICPA oversight reports as documented in Table 3.1. Data on earlier adoption dates is, unfortunately, not available (in a verifiable way).

interests in rolling out mandatory peer review to increase both its reach within the profession as well as its revenue base (Stigler, 1971; Maurizi, 1975; Posner, 1975; Peltzman 1976; Shaked and Sutton, 1981). However, while early adopting states show higher shares of AICPA-related board members, differences between early and late adopters are not statistically significant at conventional levels. Interestingly, we observe statistically significant differences when comparing states in which State Boards of Accountancy have funding autonomy with those states in which they do not. Funding autonomy means that state boards autonomously decide how to allocate budgets (including collected fees). The introduction of mandatory peer review increases the cost of license renewals (as well as the number of licensees subject to peer review, i.e., the potential fee base) and part of these fees are directly passed on to State Boards of Accountancy. Hence, State Boards of Accountancy might have stronger incentives to mandate peer review when they have funding autonomy (e.g., Maurizi, 1974). Funding autonomy, however, is also determined by the organizational structure of State Boards of Accountancy. For instance, the California State Board of Accountancy does not have funding autonomy since it is subordinate to the California Department of Consumer Affairs. Anecdotal evidence suggests that states in which State Boards of Accountancies are subordinate to other governmental bodies tend to take longer to pass any regulation.[66]

3.3 Research Design

To assess the relation between peer review mandates and entrepreneurship, I take advantage of the staggered introduction of mandatory peer review across states over time (see Table 3.1). I exploit variation in the announcement and adoption dates in a

[66] I thank two sources at the National Association of State Boards of Accountancy (NASBA) for pointing this out.

generalized DiD research design, effectively comparing individual-(entrepreneur-)level outcomes in states that announce or mandate peer review with those that already announced or implemented peer review mandates.

More specifically, to assess the relation between peer review and the probability of an individual CPA to start a CPA firm, I compare the probabilities of an individual to start a CPA firm in states adopting mandatory peer review with the probabilities of an individual to start a CPA firm in states that already adopted mandatory peer review over time. Formally, I estimate model specifications of the following form:

$$\mathbf{1}\{CPAStartUp\}_{i,t} = \beta PeerReviewAnnouncement_{s,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}. \qquad (1)$$

In this model, $\mathbf{1}\{CPAStartUp\}_{i,t}$ is an indicator variable that is switched on in the year *t* an individual CPA *i* starts a CPA firm. The policy announcement indicator $PeerReviewAnnouncement_{s,t}$ is switched on for states that announce (and later implement) peer review in the year of the policy announcement and thereafter.[67] To control for time-invariant factors, I include individual-level fixed effects (α_i). Furthermore, to control for time-varying common confounders, I include year fixed effects (γ_t) in this baseline specification.

To complement my analysis of potential effects of peer review on the probabilities to start a CPA firm, I assess the relation between peer review mandate implementation and the probability that a CPA entrepreneur exits the market. Formally, I estimate models of the following form:

[67] The policy indicator, $PeerReviewAnnouncement_{s,t}$, effectively corresponds to the product of an individual-level indicator variable (which is nested within a state-level indicator variable) and a post-adoption indicator variable. To enhance readability, I do not write out the interaction of the individual-level indicator and the post-adoption indicator but, rather, refer to this interaction as $PeerReviewAnnouncement_{s,t}$. Since I include individual and year fixed effects, I suppress the respective main effects.

$$\mathbf{1}\{CPAEntrepeneurExit\}_{i,j,t} = \beta PeerReview_{s,t} + \alpha_i + \gamma_t + \varepsilon_{i,j,t}. \quad (2)$$

In this model, $\mathbf{1}\{CPAEntrepeneurExit\}_{i,j,t}$ is an indicator variable that is switched on in the year t an individual CPA i exits the market. The policy indicator $PeerReview_{s,t}$ is switched on for states implementing peer review in year t and thereafter. To control for time-invariant factors, I include individual-(entrepreneur-)level fixed effects (α_i). Furthermore, to control for time-varying common confounders, I include year fixed effects (γ_t) in this baseline specification.

To further assess potential heterogenous responses to peer review mandates, I estimate models of the following form:

$$\begin{aligned}\mathbf{1}\{CPAEntrepreneurExit\}_{i,j,t} &= \beta PeerReview_{s,t} \times Attribute_i^a + \alpha_i \\ &\quad + \gamma_{a,t} + \partial_{s,t} + \varepsilon_{i,j,t}.\end{aligned} \quad (3)$$

In this model, all variables are defined as in model (2). In addition, allowing the policy variable to vary by individual-(entrepreneur-)level characteristics a (i.e., $\beta PeerReview_{s,t} \times Attribute_i^a$) allows to further saturate the baseline fixed effects structure. Specifically, I include state-year fixed effects ($\partial_{s,t}$) to control for any (unobservable) state-year confounders, as well as attribute-year fixed effects ($\gamma_{a,t}$) to control for potential differences in trends for individuals with attribute a.[68]

Throughout my main analysis investigating the relation between peer review mandates and CPA entrepreneurship, I estimate linear probability models, i.e., I estimate all of the above models using ordinary least squares (OLS). I estimate the models using

[68] This design effectively corresponds to a DiDiD design. As before, I only write out the (interacted) policy indicator of interest. All main effects and two-way interactions are subsumed by the fixed effect structure.

OLS despite the binary outcome variable since it allows for a comprehensive fixed effect structure. I cluster standard errors at the individual (entrepreneur) level.[69]

3.4 Individual CPAs, CPA Entrepreneurs, and Disciplinary Actions

Given that there is, to the best of my knowledge, no evidence on the individual-level characteristics of CPA entrepreneurs and only limited evidence on the characteristics of entrepreneurs providing complex professional services, I first provide detailed summary statistics together with a comprehensive discussion of the individual characteristics of CPA entrepreneurs. Furthermore, since this paper is the first to utilize detailed individual-level data on disciplinary actions, I also provide descriptive evidence on the characteristics of CPAs subject to disciplinary actions.[70]

In Table 3.2, I report the summary statistics for my *Individual CPA Sample* as well as my *CPA Entrepreneur Sample*. The (raw) data underlying these samples comprise all individual CPA licenses and CPA firm licenses in Colorado and Texas. I provide detailed variable definitions together with comprehensive sample construction steps in Appendices 3.A and 3.B, respectively.

Panel A reports the summary statistics for my (cross-sectional) *Individual CPA Sample*. Overall, this sample comprises 76,366 individual CPAs. The mean issue year of a CPA license, i.e., the year an individual CPA license is granted, is 1999. The mean exit year, i.e., the year an individual CPA license expires, is 2019. Accordingly, the mean age

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[69] Due to the group-level assignment of the treatment one might be inclined to cluster standard errors at this group level. Given data availability constraints and the small number of groups, i.e., two states, such clustering choice is not feasible in the present setting (e.g., Bertrand et al., 2004; Donald and Lang, 2007). In my later cross-sectional tests, in which I allow the policy indicator to vary conditional on the location of CPA entrepreneurs, I assess the robustness of my inferences to this research design choice. Additional (untabulated) tests, in which I cluster standard errors at the ZIP code level, yield similar confidence bands and, hence, similar inferences.

[70] Prior research uses aggregated AICPA fine data but does not compare individuals receiving fines with those that do not since the latter group is not observable in these settings (e.g., Armitage and Moriarity, 2016; Cascino et al., 2020).

of a license is 20 years. Furthermore, lower license expiration year percentiles show that expired individual CPA licenses are also included in the samples, which provides comfort that my data collection process is not restricted to collecting active licenses only.[71] I outline this data collection process in Appendix 3.B.1. and further provide an illustrated guide on how to collect CPA license information in Section 3.9.1.

In terms of demographic characteristics, about 42% of individual CPAs in my sample are female and the share of non-white CPAs amounts to 14%. These numbers are comparable to prior studies using Census micro-level data (e.g., Barrios, 2019), which provides some comfort in terms of the representativeness of my estimation samples. Furthermore, 4.1% percent of individual CPAs have disciplinary action records. Disciplinary actions capture violations of professional standards (including substandard audit service provisions) and serve as the main proxy for audit quality in this paper.[72]

Panel B presents the summary statistics for my *CPA Entrepreneur Sample*, which is a subset of the *Individual CPA Sample* as outlined in Appendix 3.B.2. In terms of individual-(entrepreneur-)level characteristics, CPA entrepreneurs, at the mean, tend to have licenses that are issued earlier, and show higher license ages when compared to the *Individual CPA Sample*. Further, the share of female CPA entrepreneurs (31%) is considerably lower than the share of women in the *Individual CPA Sample*, while there are no major differences when comparing the share of non-white CPAs across samples. Interestingly, the share of CPA entrepreneurs with disciplinary action records is higher

[71] To provide an additional test whether my data collection process yields all individual CPA (and CPA firm) licenses, I exploit the fact that license numbers are assigned sequentially, i.e., early-issued licenses have lower license numbers than late-issued licenses. I sort (cross-sectional) data on daily issue dates and calculate increments in license numbers. In the raw data, more than 95% of these increments have a value of one, which provides further assurance that the data collection approaches outlined in Appendix 3.B.1 and B.2. as well as in Section 3.9.1 yield license information for all individual CPAs and CPA firms.

[72] I use disciplinary action incidents to proxy for quality for two main reasons. First, disciplinary action incidents are a discrete and actual (not perceived) quality metric, which mitigates concerns about measurement error (DeFond and Zhang, 2014). Second, measuring audit quality based on engagement (or client) characteristics is not feasible in this setting due to data availability constraints and, even if it was, would lack appropriate benchmark (quality) models.

(5.3%) than the respective share for all individual CPAs (4.1%). Similarly, the number of CPA entrepreneurs with multiple fine records is higher than what the population mean indicates.

I next assess the individual-level characteristics of CPA entrepreneurs, i.e., linking the (cross-sectional) samples shown in Table 3.2, Panel A and Panel B. To do so, I estimate logistic regressions, for which the response variable, $CPAStartUp_{i,t}$, is an indicator variable equal to one in the year an individual CPA starts a CPA firm. To capture some of the dynamics of the decision to start a CPA firm, I convert (cross-sectional) license data to a panel using the issue year of an individual CPA license as the year of sample entry and the license expiration year as the year of sample exit. In addition, I drop an individual CPA from the estimation sample after this individual CPA starts a CPA firm. Thus, this analysis is akin to estimating hazard models, in which the hazard event is the formation of a CPA firm by an individual CPA.

Table 3.3, Panel A shows the results of the analysis exploring the individual-level characteristics of CPA entrepreneurs. The estimates show that baseline probabilities to start a CPA firm in any given year are around 1%. In terms of demographic characteristics, female CPAs are two thirds as likely as male CPAs to start a CPA firm. This result is consistent with the notion that females, on average, show higher risk aversion (e.g., Bertrand, 2011), and that risk aversion may negatively correlate with entrepreneurship (e.g., Levine and Rubinstein, 2017; 2018). Non-white individual CPAs, on the other hand, are about 1.3-times more likely than white CPAs to start a CPA firm. This result is consistent with recent research showing that (non-white) immigrants show higher probabilities of becoming entrepreneurs (e.g., Kerr and Kerr, 2017). An alternative explanation, however, pertains to discrimination against non-white CPAs during hiring processes. Prior research argues that there may be discrimination against non-white professionals during audit firm hiring processes based on comparisons between the share of minorities in accounting firms and the share of minorities in accounting degree programs (Glover et al., 2000; Moyes et al., 2000; Madsen, 2013).[73,74] While the estimates are consistent with prior work in the accounting literature, recent work in economics shows seemingly different associations. Specifically, Levine and Rubinstein (2017) show cross-sectional evidence suggesting that entrepreneurs tend to be white. However, they further show that entrepreneurs are more likely to come from high-earning families. Thus, the seemingly different findings may simply be due to selection into industries (and entrepreneurship) based on capital availability, which may also correlate with ethnicity or race (Levine and Rubinstein, 2017).

I also explore differences in probabilities to start a CPA firm across license age cohorts. The relationship between age and entrepreneurship is ultimately an empirical question. On the one hand, models such as Lucas (1978) suggest that (time-invariant) ability is the main dimension along which individuals select into entrepreneurship. Under this view, age (or cohort effects) should not matter. On the other hand, older individuals may have had more time to accumulate the experience and wealth necessary to become an entrepreneur. Yet another alternative pertains to risk tolerance being concentrated among younger individuals. My estimates are consistent with this latter explanation and

[73] Recent research models entrepreneurship as an individual's decision to either start formal employment or becoming an entrepreneur (e.g., Levine and Rubinstein, 2018). The higher likelihood of non-white CPAs to become an entrepreneur is consistent with discrimination in hiring processes (e.g., Bertrand and Mullainathan, 2004), since such discrimination lowers the relative (option) value of formal employment for minorities and, ceteris paribus, increases the probability to become an entrepreneur. However, while the results are consistent with this notion, I do not directly test whether discrimination may be at play in audit firm hiring. Thus, this paper does not directly speak to this question and the respective correlations should be interpreted with great care.

[74] A recent stream of literature assesses the relation between occupational licensing and minority labor market participation (e.g., Law and Marks, 2009; 2017; Blair and Chung, 2018a; 2018b). Thus far, this literature reaches mixed conclusions. My approach differs from this stream of literature in that I study individuals, who *already* hold a license.

in line with recent work by Bernstein et al. (2019), who show that entrepreneurial responsiveness to local demand shocks is primarily driven by younger individuals.

Furthermore, individual CPAs subject to disciplinary actions exhibit higher likelihoods of becoming CPA entrepreneurs. I report the respective estimates in Columns 7 and 8. Point estimates suggests a positive relation between disciplinary action incidents and entrepreneurship. This finding is consistent with prior literature proposing that "illicit" individuals tend to be more likely to become entrepreneurs (Levine and Rubinstein, 2017).[75]

A natural follow-up given the data at hand relates to the individual-level characteristics of CPAs subject to disciplinary actions. In Panel B, I investigate the demographic characteristics of these CPAs. Consistent with recent research exploring misconduct in the financial adviser industry (e.g., Egan et al., 2018; 2019), I find that female CPAS are about two-thirds as likely as male CPAs to have disciplinary action record, while there are no noticeable differences when comparing likelihoods between white and non-white CPAs. Lastly, I explore the role of license age. The estimates suggest an inverted U-shaped pattern, suggesting that mid-career CPAs exhibit comparably higher probabilities of being subject to disciplinary actions.

3.5 Peer Review Mandates and CPA Entrepreneurship

3.5.1 Peer Review Mandates and CPA Entrepreneurship: Baseline

In this section, I explore the relation between mandatory peer review and CPA entrepreneurship. To do so, I first assess whether peer review mandate announcements affect CPA entrepreneurship by estimating model (1). The dependent variable in this

[75] I provide a more detailed discussion as well as additional estimates regarding the relation between disciplinary action incidents and entrepreneurship in Section 3.9.2. Note that this correlation highlights that CPA entrepreneurs are a powerful setting to explore whether peer review mandates facilitate screening on quality.

model is an indicator set to one in the year an individual CPA starts a CPA firm. I estimate model (1) based on the *Individual CPA Sample*, which I describe in detail in Appendix 3.B.1.

Table 3.4 shows the results of this analysis. Across all model specifications, the negative coefficient estimate on the policy announcement indicator ($PeerReviewAnnouncement_{s,t}$) is statistically significant and economically meaningful. Compared to the unconditional probability of an individual to start a CPA firm of 0.7%, we observe an approximately 50% decline in the probability to start a CPA firm subsequent to peer review mandate announcements (based on estimating model specifications saturated with individual-level fixed effects). Coefficient magnitudes are stable across (cross-sectionally) saturated model specifications. It is noteworthy, however, that the point estimates on the policy announcement indicator as well as the R-Squareds increase with the introduction of individual fixed effects, which highlights the role of individual-level attributes (Oster, 2019). Overall, the estimates suggest that the anticipation of peer review mandates deters entrepreneurship.

The validity of the estimates presented in Table 3.4 hinges on the assumption that trends in probabilities to start a CPA firm between CPAs in the treatment and control group would have moved in parallel absent a policy announcement. To gauge the validity of this assumption, I provide graphical evidence. Figure 3.1 plots the point estimates obtained from estimating a version of model (1), in which I suppress the policy announcement indicator and include event-time dummies instead. Figure 3.1 shows that there are no statistically significant differences in probabilities to start a CPA firm between treated and control individuals before the announcement date. Beginning with the announcement date, however, we observe a decline in the probabilities to start CPA firms for individual CPAs in the treatment group. Overall, the plotted point estimates provide some comfort that the main assumption of the DiD estimator is not violated.

To further substantiate the findings on the relation between peer review mandates and CPA entrepreneurship, I next assess whether the actual implementation of peer review mandates affects exit probabilities of CPA entrepreneurs already in the market, i.e., CPA firms founded prior to the announcement and implementation of peer review mandates. To do so, I estimate model (2) based on the *CPA Entrepreneur Sample*, which I construct as outlined in Appendix 3.B.2. Table 3.5 shows the results of this analysis. Across all model specifications, the positive coefficient estimates on the policy indicator ($PeerReview_{s,t}$) are statistically significant and economically meaningful. Compared to the unconditional probability to exit the market of 1.3%, we observe stark increases in exit probabilities up to around 8% with the introduction of peer review mandates (based on model specifications saturated with entrepreneur fixed effects). As before, the inclusion of entrepreneur fixed effects results in meaningful changes in both coefficient magnitudes as well as R-Squareds, which highlights the importance of entrepreneur-level characteristics (Oster, 2019). I explicitly explore the role of these entrepreneur-level characteristics in my cross-sectional analyses in Section 5.2.

Before moving to these cross-sectional tests, I map out CPA entrepreneur exit probabilities in event-time to assess the validity of the generalized DiD estimator. In Figure 3.2, I plot event time indicator point estimates around the policy intervention. Figure 3.2 shows a sharp increase in exit probabilities with the introduction of peer review mandates, while there are no statistically significant differences between entrepreneurs in the treatment and control group prior to the policy intervention. This graphical evidence provides comfort that the main assumption underlying the DiD estimator, i.e., the parallel trends assumption, is not violated.

Overall, my analyses suggest that entrepreneurial activity declines with the introduction (and announcement) of peer review mandates.

3.5.2 Peer Review Mandates and CPA Entrepreneurship: Cross-Sectional Analyses

My empirical analysis, thus far, suggests that entrepreneurial activity declines with the introduction of peer review mandates. This finding is in line with prior literature assessing the relation between entry regulation and entrepreneurship (e.g., Djankov et al., 2002; Branstetter et al., 2011), and inconsistent with the argument that increases in licensing requirements spark entrepreneurial activity or facilitate the survival of entrepreneur firms (Albert et al., 2019). However, a remaining question is whether the reported entry and exit patterns are consistent with peer review facilitating screening on quality.[76] Djankov et al. (2002) argue that (costly) regulation may serve the public if it facilitates screening out providers of low-quality services.

To investigate whether mandatory peer review facilitates such active screening on quality, I first explore potential heterogenous effects based on CPA entrepreneur (pre-treatment) service quality. Specifically, I conduct tests akin to DeFond and Lennox (2011), who investigate auditor exits around the introduction of statutory PCAOB inspections. Following the conceptual arguments by DeFond and Lennox (2011), I argue that if peer review mandates provided an effective oversight mechanism, we would expect that providers of low-quality services predominantly account for observed exits, since, at

[76] Mandatory peer review increases the fixed costs of operating a CPA firm. Such increases in fixed costs are akin to raising entry costs, which prior literature identifies to hamper firm formation (e.g., Bertrand and Kramarz, 2002; Klapper et al., 2006; Mullainathan and Schnabel, 2010). Furthermore, when fixed costs increase, the number of firms in a market may decline when holding constant demand (e.g., Tirole, 1988). Thus, increases in exits and declines in entry alone are not sufficient to view increases in licensing requirements as consistent with screening on quality. Note, however, that the above argument assumes constant demand, which seems hard to reconcile with the idea that peer review mandates reduce quality uncertainty. If peer review mandates reduced quality uncertainty, demand may actually increase (e.g., Arrow, 1971; Shapiro, 1986). Rather than relying on this theoretical argument alone, I explicitly test whether peer review mandates are associated with exit patterns that are consistent (or inconsistent) with a reduction in quality uncertainty.

the margin, it is more costly for these firms to meet CPA firm licensing requirements under the mandatory peer review regime.

To operationalize this idea, I estimate model (3). In this model, I allow the policy intervention indicator to vary conditional on the presence of past (pre-treatment) disciplinary actions. As discussed in Section 4, I use the past disciplinary action incidents as a proxy for (pre-treatment) audit service quality. Disciplinary action incidents capture substandard professional service provision, such as violations of professional standards, and provide a binary measure of actual (not perceived) quality, which mitigates potential concerns pertaining to measurement error (DeFond and Zhang, 2014).

Table 3.6 reports the results of my analysis of the relation between peer review mandates and CPA entrepreneur exits conditional on (pre-treatment) service quality provision. The estimates do not indicate any statistically significant differences in exit rate changes between high- and low-quality service providers. Of course, one concern that comes to mind is that the absence of statistically significant estimates might be due to a lack of power. Binary disciplinary action incidents are (relatively) low-frequency events, a concern that I share with most studies relying on binary quality proxies (DeFond and Zhang, 2014). However, when we compare the *economic* significance (across specifications) with prior estimates (and even if we assumed *statistical* significance), we observe that even the largest (but insignificant) point estimates account for just a fraction of the increase in exit rates subsequent to the adoption of peer review mandates. Overall, these estimates are not consistent with the notion that peer review mandates facilitate screening on quality.

The natural question that follows in light of the stark increase in exit rates, which does not seem to vary conditional on (pre-treatment) service quality, is which CPA entrepreneurs account for the observed exits. The licensing literature provides several predictions in this regard. Specifically, this literature posits that licensing requirements may overly harm historically underrepresented groups and further suggests different mechanisms through which this may occur. For instance, Law and Marks (2009; 2017) propose that the design of licensing regimes may either harm historically underrepresented groups as it gives rise to discriminatory practices ("group favoritism") or as these groups find it more costly to meet increasing licensing requirements. To test whether peer review mandates overly affect historically underrepresented groups and, if so, through which mechanism(s), I provide several additional cross-sectional tests, in which I allow the policy response to vary conditional on entrepreneur-level demographic as well as local characteristics.

I first explore whether exit rates are pronounced for (relatively) underrepresented groups. Since CPA entrepreneurs (and, to a lesser extent, individual CPAs) are predominantly male and white, I explore changes in exit rates for female (relative to male) as well as for non-white (relative to white) CPA entrepreneurs in a DiDiD design.

I present the results of this analysis in Table 3.7. In Panel A, I present DiDiD estimates in which I allow the policy response to vary by gender. We observe statistically significant increases in exit rates for female CPA entrepreneurs. Point estimates suggest relative increases in exit rates of 2.3%, which is economically meaningful when compared to an unconditional base (exit) rate of 1.3%. In Panel B, I show the results of estimating a version of model (3), in which the policy indicator varies for white and non-white CPA entrepreneurs. The DiDiD estimates do not suggest meaningful differences in exit rate changes with the introduction of peer review mandates between white and non-white CPA entrepreneurs. Overall, these results are consistent with the idea that increasing licensing requirements overly affects some historically underrepresented groups because they find it more costly to meet licensing requirements but are not consistent with the idea that licensing requirements harm these groups by giving rise to group favoritism. If peer review mandates gave rise to group favoritism, we would expect to observe increases in exit rates for non-white CPA entrepreneurs (Egan et al., 2018), which we do not.

To substantiate which mechanism may account for the pronounced increase in exit rates for female CPA entrepreneurs, I further estimate models investigating the policy response by gender and age. I show the results of this analysis, in which I estimate model (1) for subsamples comprising CPA entrepreneurs of different age and gender in Table 3.8. I provide a graphical counterpart to these estimates in Figure 3.3. Several interesting patterns emerge. First, it is noteworthy that exit rates increase with the introduction of mandatory peer review across all subsamples. However, there is considerable variation in coefficient magnitudes when comparing exit rate changes across subsamples. In terms of age, we observe a U-shaped pattern, i.e., the youngest as well as the oldest CPA entrepreneurs exit at higher rates. This pattern is consistent with younger CPA entrepreneurs potentially having smaller revenue bases, as well as peer review mandates facilitating natural CPA entrepreneur exits, such as retirements. However, the U-shaped pattern is less pronounced for male CPA entrepreneurs. Specifically, for the youngest CPA entrepreneurs, we observe that exits of female CPA entrepreneurs increase more than exits of male CPA entrepreneurs around the introduction of mandatory peer review.

There are two main explanations for this finding. First, there may be revenue differences between young female and young male CPA entrepreneurs.[77] For instance, Gurley-Calvez et al. (2009) report that female entrepreneurs, on average, allocate less

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[77] Egan et al. (2018) do not find empirical support indicating that women show lower revenues (or productivity) than men in the financial adviser industry. Nonetheless, I entertain this possibility since it is inherently challenging to measure such differences at the individual level and it is not clear whether the absence of such differences in one industry carry over to other industries.

time to working in their firms than male entrepreneurs, primarily due to childcare.[78] This negative relation between time spent on the job and childcare might suggest potential differences in revenues (assuming similar hourly payment between female and male CPA entrepreneurs) but also hints at the second potential explanation, which is rooted in the household production literature (e.g., Mincer, 1962; Becker, 1965; Heckman, 2015). This literature proposes that females show greater income elasticities—that is, for a negative income shock (of the same magnitude) women may be more likely than men to exit entrepreneurship, even when income *levels* are similar. This arises as women are argued to have, on average, a larger number of substitution margins, such as childcare (Mincer, 1962; Becker, 1965; Heckman, 2015).

It is empirically challenging to separate these two interrelated explanations due to a lack of data on CPA entrepreneur revenues (or income). Nonetheless, I present further cross-sectional analyses, in which I restrict the estimation samples to young female entrepreneurs or young male entrepreneurs and allow the policy response to vary conditional on local childcare availability. To operationalize local childcare availability, I construct a dataset based on childcare facility licenses.[79] I outline the respective data collection process as well as the variable construction steps in Appendix 3.B.3. and present variable definitions in Appendix 3.A.

Table 3.9 shows the results of estimating a version of model (3), in which I interact the policy variable with an indicator that is switched on for CPA entrepreneurs located in areas with low childcare availability ($ChildcareDesert_z$). I estimate this version of model (3) for samples comprising only young female (Panel A) or only young male CPA

[78] In her 2014 presidential address to the American Economic Association, Goldin (2014) reports descriptive evidence closely in line with these patterns and states that "children are the main contributors to women's labor supply changes" (Goldin, 2014: 1111).
[79] The approach broadly builds on Malik and Hamm (2017).

entrepreneurs (Panel B). The point estimates on the interacted policy intervention indicator suggest that increases in exit rates among young female CPA entrepreneurs are pronounced in areas with limited childcare availability, while we do not observe such pronounced increases in exit rates for young male CPA entrepreneurs. These findings hint at the idea that differences in income elasticities may account for some of the heterogeneity in exit rates. However, I acknowledge that in the absence of individual-level CPA entrepreneur revenue or time-use data it is inherently difficult to separate the two potential mechanisms.

There are other potential concerns. One concern relates to the geographical partitioning. It might be that the childcare availability proxy simply captures differences between urban and rural areas. For instance, rural areas may have fewer childcare facilities and, at the same time, CPA entrepreneurs in rural areas have smaller revenue bases. I address this concern in multiple ways. First, the fixed effect structure should capture most of these (time varying) differences, as I include separate year fixed effects for areas with limited childcare availability. Second, the proxies are based on scaled childcare availability, i.e., I account for differences in the size of the local population (under the age of five). Third, I provide graphical evidence investigating the overlap between urban areas and low childcare availability. I present this graphical evidence in Figure 3.4. The graphical evidence does not indicate perfect overlap between rural areas and limited childcare availability. For instance, Panel B suggests multiple areas with limited childcare availability in the Denver area.

To summarize, the cross-sectional tests assessing the relation between peer review mandates and CPA entrepreneurship conditional on and across multiple entrepreneur-level attributes provide first micro-level evidence on the audit market consequences of mandatory peer review. The estimates do not suggest that mandatory peer review facilitates active screening on quality—that is, I do not find pronounced exit rate changes for CPA entrepreneurs subject to disciplinary actions. Exit rates are, however, pronounced for female CPA entrepreneurs. These findings may hint at some potential unintended consequences of mandatory peer review—that is, (unintended) effects on participation may outweigh (intended) screening effects. As for the mechanism through which such effects on participation may arise, the findings point towards differences in income elasticities between female and male CPA entrepreneurs. These latter estimates, however, rely on an indirect approach due to the absence of data on CPA entrepreneur income (or revenues) and ought to be interpreted accordingly.

3.5.3 Peer Review Mandates and CPA Entrepreneurship: Census NES Estimates

The analysis, thus far, suggests that the introduction of mandatory peer review drives (prospective) entrepreneurs out of the market. Further tests reveal substantial cross-sectional variation in exit effects. While the samples constructed from CPA license information allow to assess such heterogenous responses, these data can only be collected for two states, i.e., Colorado and Texas.[80] This limited (geographical) coverage may raise concerns about the external validity of the findings. To mitigate such potential concerns, I complement the individual-level estimates with estimates based on Census data. Specifically, I construct a dataset based on data provided by the Census Non-Employer Statistics (NES) program. The NES provides the most comprehensive data on sole proprietors disaggregated by detailed industry codes and U.S. states, which allows to construct a state-year panel of CPA sole proprietors. I use these data to construct my *Census NES Sample*, for which I provide detailed sample construction steps in Appendix

[80] While CPA license data can be collected for most U.S. states, my empirical analyses investigating the relation between mandatory peer review and CPA entrepreneurship require data on firm ownership. I require firm ownership data to identify founding manager-owners—that is, CPA entrepreneurs—following the approach outlined in Appendix 3.B.2 as well as Section 3.9.1.

3.B.4. To replicate and extend the analysis based on the *Census NES Sample*, I estimate an adjusted version of model (2). Specifically, since the units of observation are state-years, I replace entrepreneur fixed effects by state fixed effects. The policy indicator is coded according to the adoption years reported in Table 3.1 but lagged by one year.[81]

Table 3.10 shows the results of this state-level analyses based on the *Census NES Sample*. The findings are in line with both the reported decline in CPA entrepreneurship as well as the increase in CPA entrepreneur exits. As a consequence of increases in exit rates and declines in the formation of CPA firms, the *stock* of CPA sole proprietors declines with the introduction of mandatory peer review. Coefficient magnitudes on the policy indicator are statistically significant and suggest a decline of about 5% in the number of CPA sole proprietors. Furthermore, the Census NES program data allow to observe total revenues accruing to CPA sole proprietors in a state-year. If peer review mandates increased (perceived) quality, one might expect that revenues accruing to CPA entrepreneurs increase.[82] In Column 2, I present the results of my analysis exploring this possibility. Coefficient estimates on the policy intervention variable do not suggest increases revenues. Similarly, we do not observe that the average revenue per CPA entrepreneur (remaining in the market) changes with the introduction of peer review mandates (Column 3). I further provide graphical evidence in Figures 3.5 to assess the validity of the assumptions underlying the DiD estimation strategy. While there are no statistically significant differences between treated and not (yet) treated states, we do

[81] In addition to addressing potential concerns pertaining to the external validity of the findings, these additional tests exploiting the introduction of peer review mandates across *multiple* states also help to mitigate potential concerns regarding concurrent state-year factors that may correlate with both the policy timing and CPA entrepreneurship (Leuz and Wysocki, 2016; Leuz, 2018; Ruhm, 2018; Christensen, 2019). I lag the policy intervention indicator since the aggregate nature of Census data does not allow within year pinpointing. Such design choice is common in the economics literature using aggregated state-year data (e.g., Autor et al., 2006; 2008; 2016) as well as the accounting literature exploring the effects of state-year regulatory changes using aggregated state-year data on accounting professionals (e.g., Cascino et al., 2020).

[82] Note that one might expect revenues to increase if peer review mandates facilitated screening on quality and, thereby, reduced quality uncertainty (Arrow, 1971; Shapiro, 1986).

observe a decline in the (logged) number of CPA sole proprietors subsequent to the introduction of peer review mandates.

Overall, the estimates based on the *Census NES Sample* are consistent with the entry and exit patterns reported before. Furthermore, consistent with the absence of pronounced exit rates for low-quality CPA entrepreneurs, we do not observe increases in the demand CPA entrepreneurs face, as evidenced by the absence of increases total (and average) revenues.

3.5.4 Peer Review Mandates and CPA Entrepreneurship: Additional Tests

In this section, I discuss several additional tests, which I present in Section 3.9 to this paper. Specifically, I estimate a determinant model exploring the individual-level characteristics of CPA entrepreneurs subject to disciplinary actions, I explore whether my findings are robust to operationalizing CPA entrepreneur entry and exit dates to using firm incorporation and dissolution dates, and I assess whether CPA firms (in the sample) exhibit address or ownership changes around the passage of peer review mandates.

First, I explore the individual-level characteristics of CPA entrepreneurs subject to disciplinary actions. Prior literature argues that entrepreneurs may have distinct risk preferences (e.g., Levine and Rubinstein, 2017), and risk preferences may correlate with the likelihood of being subject to disciplinary actions. Thus, it is unclear whether the associations between individual-level characteristics and disciplinary action probabilities I report for individual CPAs (Table 3.3, Panel B) carry over to CPA entrepreneurs. Table 3.OA-1 shows the results of estimating a determinant model of disciplinary action incidents based on a sample restricted to CPA entrepreneurs. Overall, we observe similar correlations (in direction and magnitude) between demographic characteristics and disciplinary actions. We only observe minor differences when comparing disciplinary action rates across age groups. Disciplinary action incidents are pronounced for younger CPA entrepreneurs, although these differences are not statistically significant at conventional levels when including fixed effects. Nonetheless, I further explore this dimension and estimate linear probability models, in which I include recent disciplinary action incidents as a predictor for CPA entrepreneurship, while holding constant individual-level time-invariant characteristics, such as innate risk preferences. Table 3.OA-3 reports the results of this analysis. The results suggest that recent disciplinary actions are positively associated with CPA entrepreneurship. I view this pattern as consistent with disciplinary actions capturing a quality dimension, as I outline in Section 3.9.2.[83]

Second, I explore whether my findings are robust to operationalizing CPA firm entry and exit dates using incorporation and dissolution dates as opposed to CPA firm license issue and expiration dates.[84] To do so, I obtain incorporation filings for CPA firms in my *CPA Entrepreneur Sample* via the OpenCorporates project. I match CPA firms in my sample with the OpenCorporates entity profile dataset via the OpenCorporates Research API using CPA firm names. Then, I re-construct the *CPA Entrepreneur Sample* using incorporation and dissolution dates as opposed to CPA firm license issue and exit dates (see Section 3.9.3 for a more detailed discussion of the matching procedure as well as sample construction steps). Table 3.OA-4 shows the results of this analysis. Overall, using firm incorporation and dissolution dates yields very similar coefficient magnitudes

[83] Note that this positive correlation is consistent with disciplinary action incidents capturing quality as opposed to (time invariant) risk preferences. Recent disciplinary action incidents are available to the public—including potential employers. Thus, the decision to start a CPA firms may, in part, be explained by changes in employment opportunities.

[84] Prior research discusses the advantages and disadvantages of using incorporation (or register) data vis-à-vis other data sources to operationalize firm entries and exits (see, for instance, the response by Runst et. al., 2017 to Rostam-Afschar, 2014).

(and, inferences). Besides, these additional tests suggest that CPA firms cease to operate as distinct (entrepreneur) firms rather than simply dropping their CPA firm license.[85,86]

Third, I explore whether firms exhibit changes to entity statutes around the passage of peer review mandates. My classification of entrepreneurs relies on cross-sectional data on firm ownership. Thus, there might be a potential concern that I am misclassifying (some) CPA firms as CPA entrepreneur firms. Note that such concern would only be severe if potential misclassifications coincided with the introduction of mandatory peer review. Similarly, some of my cross-sectional tests rely on inferring an entrepreneur's location based on ZIP codes. If entrepreneurs moved to other locations in anticipation of peer review mandates, this may raise potential concerns about the validity of my (geographical) partitioning. To explore these possibilities, I collect data on entity filings indicating changes to entity ownership or entity addresses. I outline the respective data collection approach in Section 3.9.4. Using these data, I then assess the association between peer review mandates and incidents of entity ownership or address changes. The results of this analysis do not suggest that peer review mandates predict any such changes, which provides further comfort.

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[85] Note that CPA entrepreneurs must hold CPA firm licenses to offer audit services. Thus, my prior analyses using CPA firm license issue and expiration dates to operationalize CPA entrepreneur entry and exit may pick up the decision to offer audit services. While studying this decision is of first order importance when assessing market dynamics in the audit industry (e.g., Francis, 2011), my additional tests do not suggest that CPA entrepreneurs only cease to offer audit service.

[86] To obtain entity incorporation and dissolution dates, I rely on a (fuzzy) merge using CPA firm names. Fuzzy matching techniques are noisy and, therefore, I am not able to match all (incorporated) CPA firms. However, some (other) CPA firms may not have matching entries in the OpenCorporates entity profile dataset for the simple reason that these firms chose not to incorporate. To explore the potential role of such systematic differences, I further estimate models in which I allow the policy indicator to vary conditional on whether a CPA firm has an identifiable OpenCorporates entity profile entry. I present these estimates in Table 3.OA-4. The estimates suggest that increases in exit rates among CPA entrepreneurs without matching OpenCorporates entity profile entry are slightly pronounced for CPA entrepreneurs, although these differences are not statistically significant at conventional levels. Overall, I view these additional estimates as providing further assurance pertaining to the robustness of my main analyses, but I acknowledge that some of my estimates may also pick up differences in, say, firm size or revenues. I explicitly discuss this possibility as part of my cross-sectional analyses presented in Section 3.5.2.

3.6 Conclusion

This paper explores the relation between mandatory peer review and CPA entrepreneurship. I exploit the staggered introduction of peer review mandates and find that CPA entrepreneurship declines with the introduction of mandatory peer review. I further document substantial cross-sectional variation in CPA entrepreneur exit effects. Exits are pronounced for female CPA entrepreneurs, specifically for young female CPA entrepreneurs in areas with limited childcare availability, but not for CPA entrepreneurs subject to disciplinary actions. Overall, the paper provides first evidence on the audit market consequences of mandatory peer review and may question its effectiveness as an active quality screening mechanism. The findings may be of interest to audit regulators and may also contribute to the wider debate on licensing regulation and its effects on entrepreneurial activity.

This study is subject to several caveats. First, while leveraging licensing data helps to observe prospective entrepreneurs in the short run, my findings ought to be interpreted at the intensive margin (i.e., holding constant selection into the profession). Second, this study focuses on specific audit market participants: CPA entrepreneurs. While focusing on specific market participants offers a comparably high degree of internal validity (e.g., Christensen, 2019), future research may explore the long-run or market-wide effects of mandatory peer review, or the consequences of changes in audit market dynamism—that is, CPA firm entries and exits. Leveraging CPA license data provides one way to study entry and exit dynamics for the population of CPA firms as well as individual CPAs and may further provide an impetus for research investigating engagement partner characteristics, which seems timely in light of recent regulation requiring audit engagement partner disclosures.

3.7 Appendix 3.A: Variable Definitions

Variable	*Definition*
ΔGDP_s	Pre-treatment change in real GDP per capita in state *s* (Source: Bureau of Economic Analysis).
$\Delta Laborforce_s$	Pre-treatment change in the labor force in state *s* (Source: Bureau of Economic Analysis).
$\Delta Unemploymentrate_s$	Pre-treatment change in the unemployment rate in state *s* (Source: Census Local Area Unemployment Statistics).
$\Delta JobCreationRate_s$	Pre-treatment change in the job creation rate in state *s* (Source: Census Statistics of U.S. Business).
$CPAs_s$	The number of CPAs on the State Board of Accountancy's board in state *s* (Source: Colbert and Murray, 2013, and hand collection).
$PublicMember_s$	The number of public members on the State Board of Accountancy's board in state *s* (Source: Colbert and Murray, 2013, and hand collection).
$BoardSize_s$	The sum of CPAs and public members on the State Board of Accountancy's board in state *s* (Source: Colbert and Murray, 2013, and hand collection).
$CPAShare_s$	The number of CPAs on the State Board of Accountancy's board in state *s* scaled by the total number $BoardSize_s$ (Source: Colbert and Murray, 2013, and hand collection).
$NationalFirmShare_s$	The number of CPAs in national accounting firms on the State Board of Accountancy's board in state *s* scaled by the total number $BoardSize_s^{CM}$ (Source: Colbert and Murray, 2013, and hand collection).
$AICPAMemberShare_s$	The number of AICPA members on the State Board of Accountancy's board in state *s* scaled by the total number $BoardSize_s$ (Source: Colbert and Murray, 2013, and hand collection).
$FundingAutonomy_s$	An indicator variable set equal to one if the State Board of Accountancy's board in state *s* has funding autonomy (Source: Colbert and Murray, 2013, and hand collection).
$IssueYear_i$	The year the CPA license of individual CPA *i* is issued.
$ExpirationYear_i$	The year the CPA license of individual CPA *i* expires.
$LicenseAge_i$	$ExpirationYear_i$ minus $IssueYear_i$.
$Female_i$	An indicator variable set to one if individual CPA *i* is female. Since the gender of individual CPA *i* is not directly observable, I use names (first names) to predict individual *i*'s gender. I use a prediction model based on Sood and Laohaprapanon (2018).
$NonWhite_i$	An indicator variable set to one if individual CPA *i*'s race is not classified as "white." Since the race of individual CPA *i* is not directly observable, I use names (first name and surname) to predict individual *i*'s race. I use a prediction model based on Sood and Laohaprapanon (2018).
$DisciplinaryAction_i$	An indicator variable set to one if there is at least one disciplinary action incident associated with the CPA license of individual CPA *i*.
$MultipleDisciplinaryActions_i$	An indicator variable set to one if there is more than one disciplinary action incident associated with the CPA license of individual CPA *i*.

Appendix 3.A (continued)

Variable	*Definition*
$FirmDisciplinaryAction_{i,j}$	An indicator variable set to one if there is at least one fine associated with the CPA firm license *j* founded by individual CPA entrepreneur *i*.
$MultipleFirmDisciplinaryAction_{i,j}$	An indicator variable set to one if there is more than one fine associated with the CPA firm license *j* founded by individual CPA entrepreneur *i*.
$CPAStartUp_{i,t}$	An indicator variable set equal to one in the year *t* individual CPA *i* starts a CPA firm.
$AgeQuintile_i^q$	An indicator variable set to one if individual CPA *i* belongs to age quintile *q*. Age quintiles are defined based on license issue years.
$PeerReviewAnnouncement_{s,t}$	An indicator variable set to one in the year t, in which state *s* announces the introduction of mandatory peer review and thereafter. This indicator corresponds to a treat-times-post interaction in conventional difference-in-differences research designs. For my estimation sample, the indicator is switched on for all individual CPAs in Colorado in 2010 and thereafter.
$CPAEntrepreneurExit_{i,j,t}$	An indicator variable set to one in the year *t* CPA firm *j* of entrepreneur *i* exits. Exit years are coded according to the CPA firm license expiration year of firm *j* founded by individual CPA *i*.
$PeerReview_{s,t}$	An indicator variable set to one in the year t, in which state *s* implements mandatory peer review and thereafter. This indicator corresponds to a treat-times-post interaction in conventional difference-in-differences research designs. For my estimation sample, the indicator is switched on in the mandatory peer review dates reported in Table 3.1, Panel A.
$PastIndividualDisciplinaryAction_i$	An indicator variable set to one if individual CPA *i* has a fine record with the respective State Board of Accountancy prior to 2014.
$PastFirmDisciplinaryAction_j$	An indicator variable set to one if firm *j* founded by individual CPA entrepreneur *i* has a fine record with the respective State Board of Accountancy prior to 2014.
$ChildcareDesert_z$	An indicator variable taking the value of one if a ZIP code is classified as a childcare desert. A ZIP code is defined as a childcare desert if the ratio of the population of children under five to licensed childcare capacity in the respective ZIP code exceeds three or if there is no licensed childcare facility in the respective ZIP code. My partitioning follows Malik and Hamm (2017). I provide detailed data collection steps required to construct this variable in Appendix 3.B.3.
$CPASoleProp_{s,t}$	The number of CPA sole proprietors in state *s* in year *t*.
log ($CPASoleProp_{s,t}$)	The natural logarithm of $CPASoleProp_{s,t}$.
$CPASolePropRevenue_{s,t}$	The total revenues of all CPA sole proprietors in state *s* in year *t*.

Appendix 3.A (continued)

Variable	*Definition*
$\log(CPASolePropRevenue_{s,t})$	The natural logarithm of $CPASolePropRevenue_{s,t}$.
$AvgSolePropRevenue_{s,t}$	$CPASolePropRevenue_{s,t}$ divided by $CPASoleProp_{s,t}$.
$\log(AvgSolePropRevenue_{s,t})$	The natural logarithm of $AvgSolePropRevenue_{s,t}$.

3.8 Appendix 3.B: Data and Samples

3.B.1 Individual CPA Dataset

This dataset is based on collecting every available individual CPA license in Colorado and Texas. For both states, I employ a two-stage data collection approach. In the first step, I compile lists of all available individual CPA license numbers. Second, I search for each of these license numbers and collect all available individual-level information. For Colorado, I search the license verification function tool of the Department of Regulatory Agencies (available at: https://apps.colorado.gov/dora/licensing/Lookup/LicenseLookup.aspx). More specifically, in the first step, I search for all licenses by ZIP codes. I obtain a list of all Colorado ZIP codes from Census geography reference files.[87] These searches yield a list of all individual CPAs holding a Colorado CPA license (in each ZIP code). In the second step, I search for each individual CPA license and collect all available information associated with the respective individual CPA license. This information includes names (first and last), an address, the issue date of the license, the expiration date of the license, the status of the license, as well as any disciplinary actions associated with the license. I employ a conceptually similar data collection strategy for Texas. The license lookup function for individual CPAs in Texas is available at: http://www.tsbpa.state.tx.us/php/fpl/indlookup.php. The collection process yields one observation for each individual CPA holding a license in Colorado or Texas. Table 3.2, Panel A provides the summary statistics for this dataset. For my estimates exploring the characteristics of CPA entrepreneurs, I convert this dataset to a panel. To do so, I define the license issue year as the year an individual CPA enters the sample, and I define the license expiration year as the year an individual CPA exits the sample. I assume that a

[87] Census Geography reference files are available under: https://www.census.gov/geographies/reference-files/time-series/geo/relationship-files.html.

CPA is active in all years in between the entry and exit year. For most tests, I restrict this panel to CPA-years between 2010 and 2018.[88] These steps yield my *Individual CPA Sample*. In addition to the sample construction steps above, I provide a graphically illustrated step-by-step guide on how to collect CPA license information in Section 3.9.1.

3.B.2 CPA Entrepreneur Dataset

To identify CPA entrepreneurs, I complement my *Individual CPA Sample* with information on CPA firm licenses as well as information on manager-owners of each CPA firm. To collect information on all available firms in Colorado and Texas, I use an approach similar to the collection approach for individual CPA licenses (see Appendix 3.B.1.). Then, I collect information allowing to identify managing partners (i.e., manager-owners) of each CPA firm. For Colorado, each CPA firm license contains information on the manager-owner of the respective CPA firm. I collect this information (along with all other information available for this firm) and merge the individual-level license information with the respective firm using individual CPA license numbers. For Texas, I employ a similar approach. First, I collect all CPA firm license numbers as well as all available firm-level information. For Texas, the information on manager-owners is stored in the individual-level records (including the CPA firm license number). Hence, for Texas, I use CPA firm license numbers to merge CPA firm data with individual CPA license data. To identify founding manager-owners, i.e., CPA entrepreneurs, for both Colorado and Texas, I use the date (year) as of which a respective manager-owner assumes her role. I require this date to coincide with the founding year of the CPA firm. As before, this collection process yields one observation per CPA entrepreneur. I convert

[88] I slightly depart from this (time) restriction for my tests gauging the effects of peer review announcements and CPA entrepreneurship (i.e., Table 3.4). For these tests, I form the estimation sample around the peer review announcement year in Colorado (i.e., 2010) and include licenses active around 2006 to 2014. In addition, I require individual CPA licenses to be issued before the announcement in 2010.

these data to a panel by using the firm founding year (i.e., the CPA firm license issue year) as the year a CPA firm enters the sample and the CPA firm license expiration year as the year a firm exits the sample.[89] This approach yields my *CPA Entrepreneur Sample*. In addition to the sample construction steps above, I provide a graphically illustrated step-by-step guide on how to collect CPA license information in Section 3.9.1.

3.B.3 Constructing Childcare Availability Proxies

For constructing childcare availability proxies, I use three main data sources: childcare facility license search tools for Colorado, childcare facility license search tools for Texas, and population counts by age for both states.[90] For Colorado, I collect childcare facility licensing data from Colorado's Department of Human Services' Office of Early Childhood.[91] These data contain information on the location of each licensed childcare facility as well as information on the licensed capacity, i.e., the maximum number of children a facility may accommodate. I search the Colorado childcare license database by ZIP codes.[92] Then, I collect the information on each childcare facility by ZIP code. For Texas, I take a similar approach. Texas childcare facility license information can be collected via the license search function provided by the Texas Department of Health and Human Services.[93] Again, I collect information on all childcare facilities by ZIP Code.

Then, I use Census ZIP code files to create a "frame" of all ZIP codes in Colorado and Texas and merge childcare facility information with this ZIP code frame. This yields

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[89] I explicitly explore the robustness of my findings to this operationalization decision in Section 3.9.3.
[90] My procedure broadly builds on Malik and Hamm (2017).
[91] The Colorado childcare facility search function is available under: https://www.coloradoshines.com/search.
[92] A reference list of all Colorado (and Texas) ZIP codes can be obtained from Census Geography reference files, which are available under: https://www.census.gov/geographies/reference-files/time-series/geo/relationship-files.html.
[93] The Texas childcare facility search function is available under: https://www.dfps.state.tx.us/Child_Care/Search_Texas_Child_Care/default.asp.

a dataset containing the total number of licensed childcare capacity (and the total number of childcare providers) for each ZIP code. Note that the approach using a "frame" ensures that ZIP codes without any childcare providers, i.e., "true" zeros, are included in the final dataset.

In the next step, I obtain data on the population by age (as well as the land area) of each ZIP code (to calculate population densities). Data on the population by age is collected from the American Community Survey's (ACS) five-year files using Census API access points.[94] Specifically, I collect data on the total number of children under 3 (ACS variable name "B09001_003E"), children age 3 and 4 (ACS variable name "B09005_004E"), children age 5 (ACS variable name "B09005_005E"), as well as the total population (ACS variable name "B01001_001E").[95] Data on the (land) area of each ZIP code is available from Census geography files.[96] Finally, I merge this dataset with my *CPA Entrepreneur Sample* using ZIP codes. CPA entrepreneur ZIP codes are extracted from CPA firm license information, which I collect following the approach outlined in Appendix 3.B.2.

3.B.4 Census NES Dataset

This dataset is based on data provided by the Census Non-Employer Statistics (NES) program. The Census NES program provides the most comprehensive data on sole proprietors in the United States. The NES program provides these data disaggregated by U.S. state and industry (6-digit NAICS code). To identify CPA sole proprietors, I collect

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[94] The Census ACS API documentation is available under: https://api.census.gov/data/2017/acs/acs5/examples.html. I query the ACS API using Python scripts. The scripts are available from the author upon request.

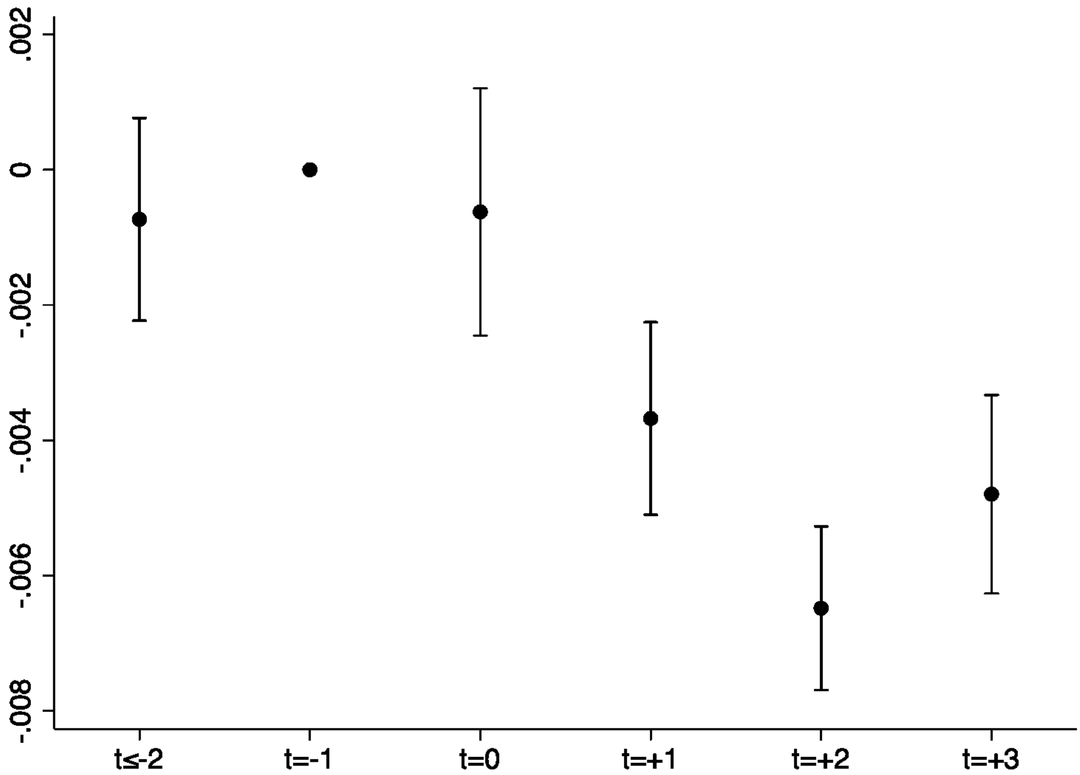
[95] A full list of all variables available via the Census ACS API is available under: https://api.census.gov/data/2017/acs/acs5/variables.html.

[96] These data can (best) be obtained via Census FTP access available under: ftp://ftp2.census.gov/geo/tiger/. Geography files further include the shapefiles required to map childcare deserts (e.g., Figure 1.3.4).

information for all sole proprietors assigned to the 6-digit-NAICS code 541211 "Offices of Certified Public Accountants." Prior research using data based on this industry classification to study audit markets includes Duguay et al. (2020) as well as Cascino et al. (2020). I collect these data for all states adopting mandatory peer review during my sample period. These sample construction steps yield my *Census NES Sample*.[97]

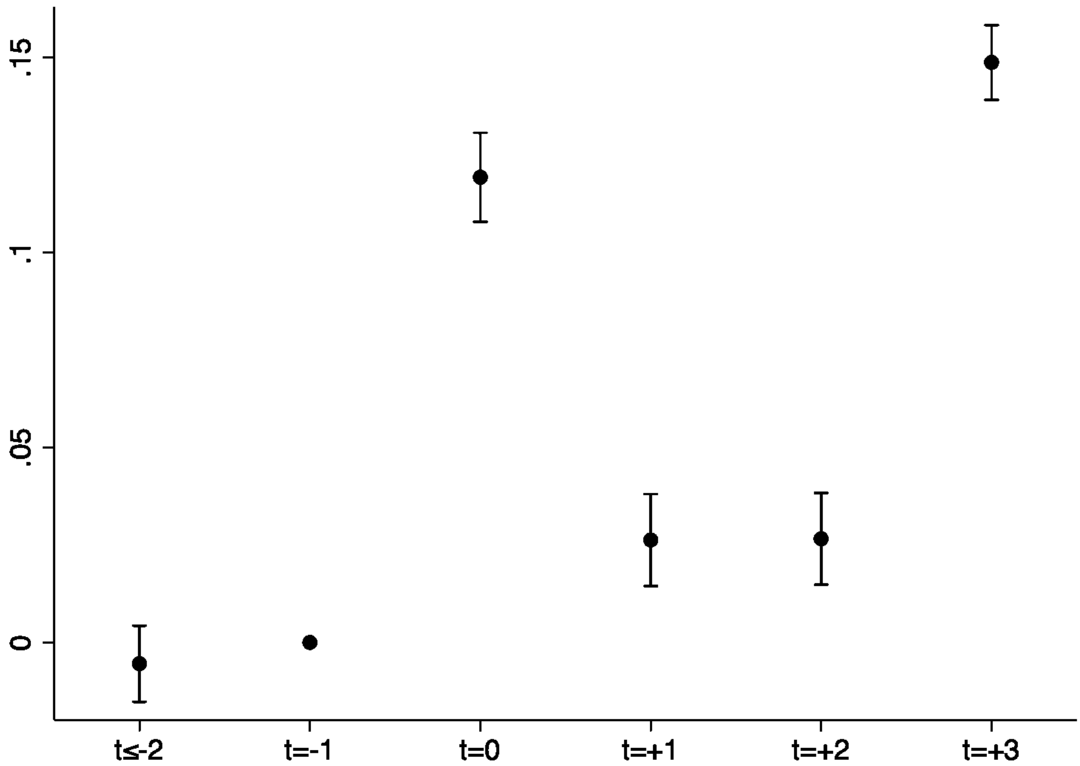
[97] I obtain data from the Census NES using the Census API. The full Census NES API documentation is available at: https://www.census.gov/data/developers/data-sets/cbp-nonemp-zbp/nonemp-api.html. Scripts querying the Census NES statistics to form my estimation sample are available from the author upon request.

Figure 3.1: Peer Review Announcement and CPA Entrepreneurship



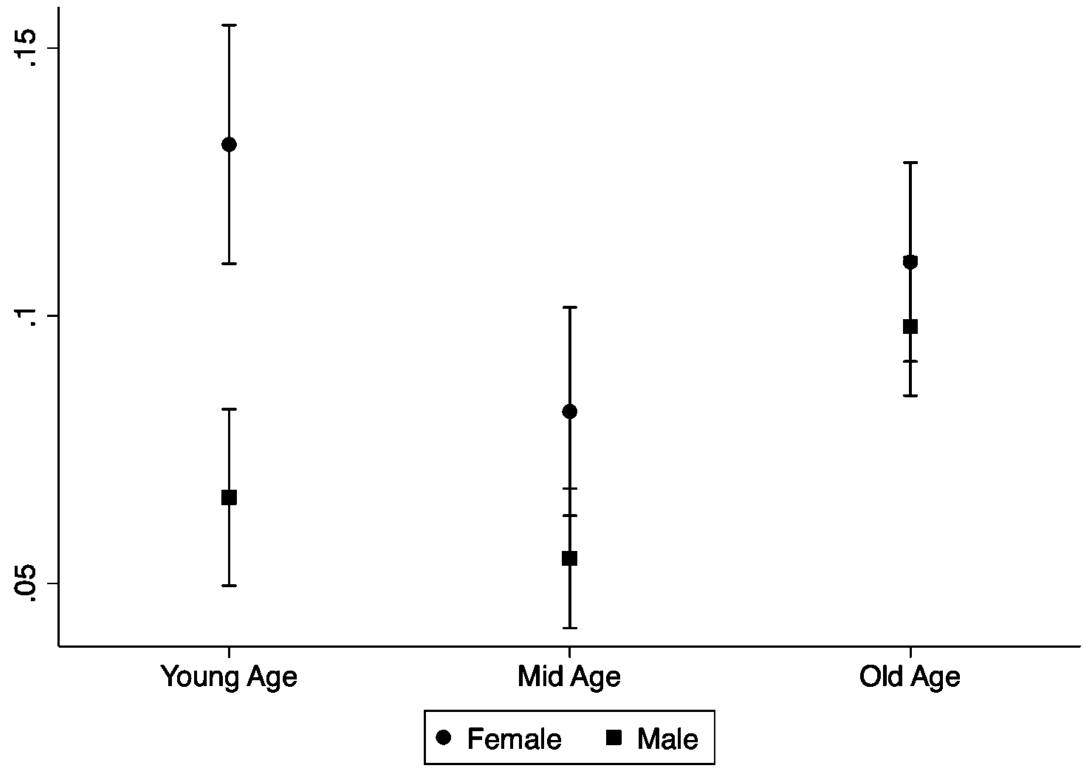
This figure reports the coefficients of ordinary least squares regressions investigating the effect of peer review mandate announcements on CPA entrepreneurship in event time. Formally, I estimate $\mathbf{1}\{CPAStartUp\}_{i,t} = \beta PeerReviewAnnouncement_{s,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ but replace the policy announcement indicator variable with separate event time dummies, each marking a period relative to the year prior to the policy announcement (t=-1). I omit the indicator for t-1, which serves as the benchmark period. Vertical bands represent 95% confidence intervals for the point estimates in each event time period and are calculated based on standard errors clustered at the individual level.

Figure 3.2: Peer Review and CPA Entrepreneur Exits



This figure reports the coefficients of ordinary least squares regressions investigating the effect of peer review mandates on CPA entrepreneur exits in event time. Formally, I estimate $\mathbf{1}\{CPAEntrepreneurExit\}_{i,j,t} = \beta PeerReview_{s,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ but replace the policy indicator variable with separate event time dummies, each marking a period relative to the year prior to the policy implementation (t=-1). I omit the indicator for t-1, which serves as the benchmark period. Vertical bands represent 95% confidence intervals for the point estimates in each event time period and are calculated based on standard errors clustered at the individual (entrepreneur) level.

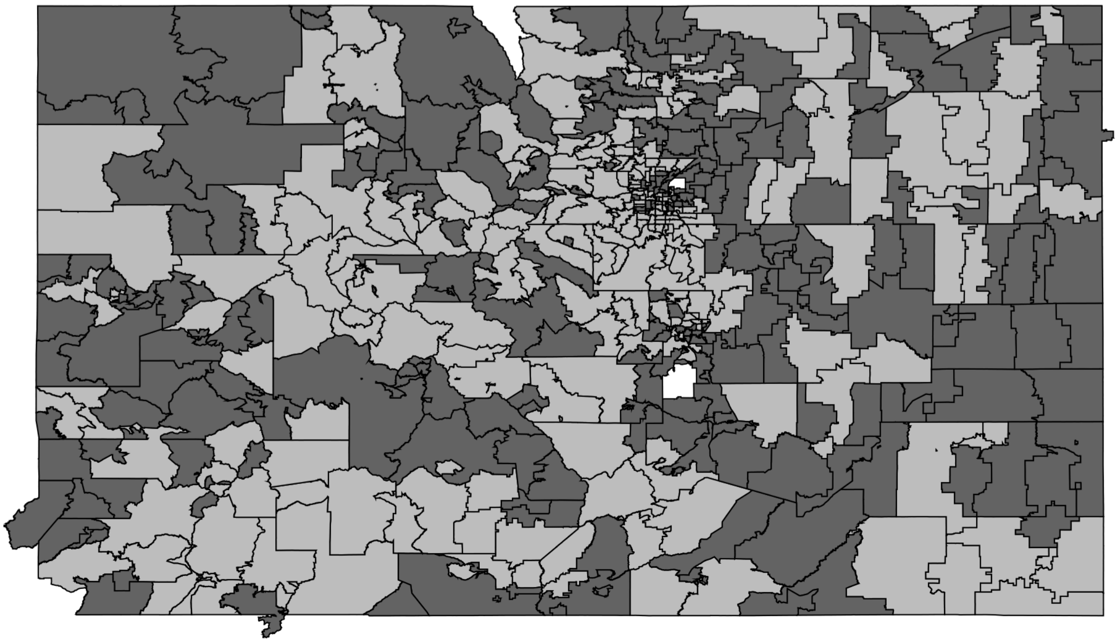
Figure 3.3: Peer Review and CPA Entrepreneur Exits by Gender and Age



This figure reports the coefficients of ordinary least squares regressions investigating the effect of peer review mandates on CPA entrepreneur exits across subsamples of gender and age terciles (as presented in Table 3.8). Formally, I estimate $\mathbf{1}\{CPAEntrepreneurExit\}_{i,j,t} = \beta PeerReview_{s,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$. Vertical bands represent 95% confidence intervals for each point estimate on the policy indicator and are calculated based on standard errors clustered at the individual (entrepreneur) level.

Figure 3.4: Childcare Availability in Colorado and Texas

Panel A: Childcare Deserts in Colorado



Panel B: Overlap between Childcare Deserts and Urban Areas in Colorado

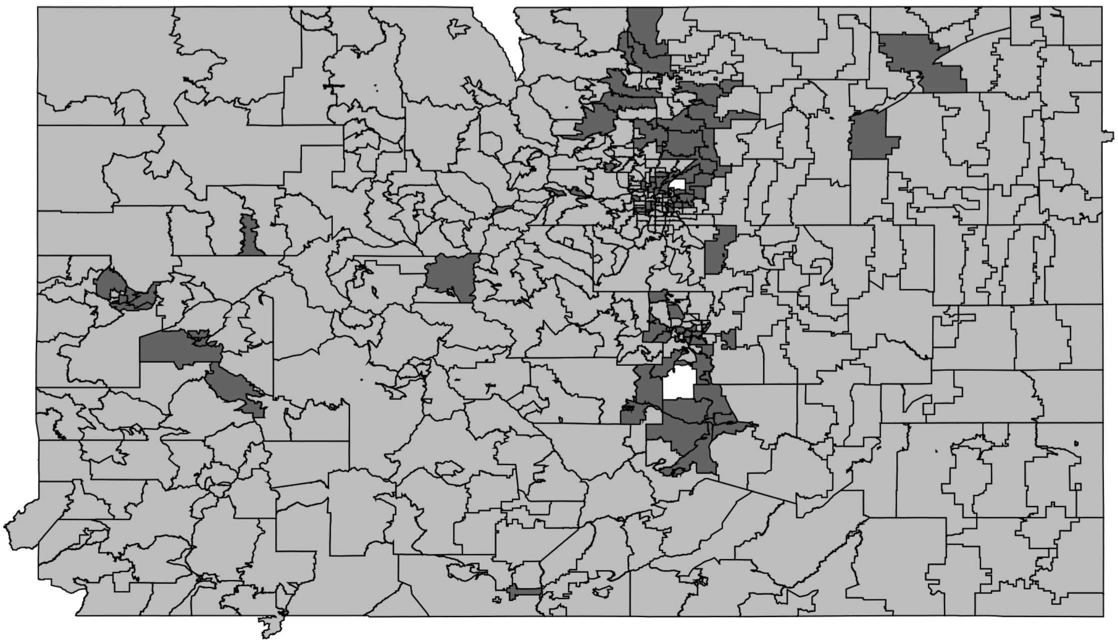


Figure 3.4 (continued)

Panel C: Childcare Deserts in Texas

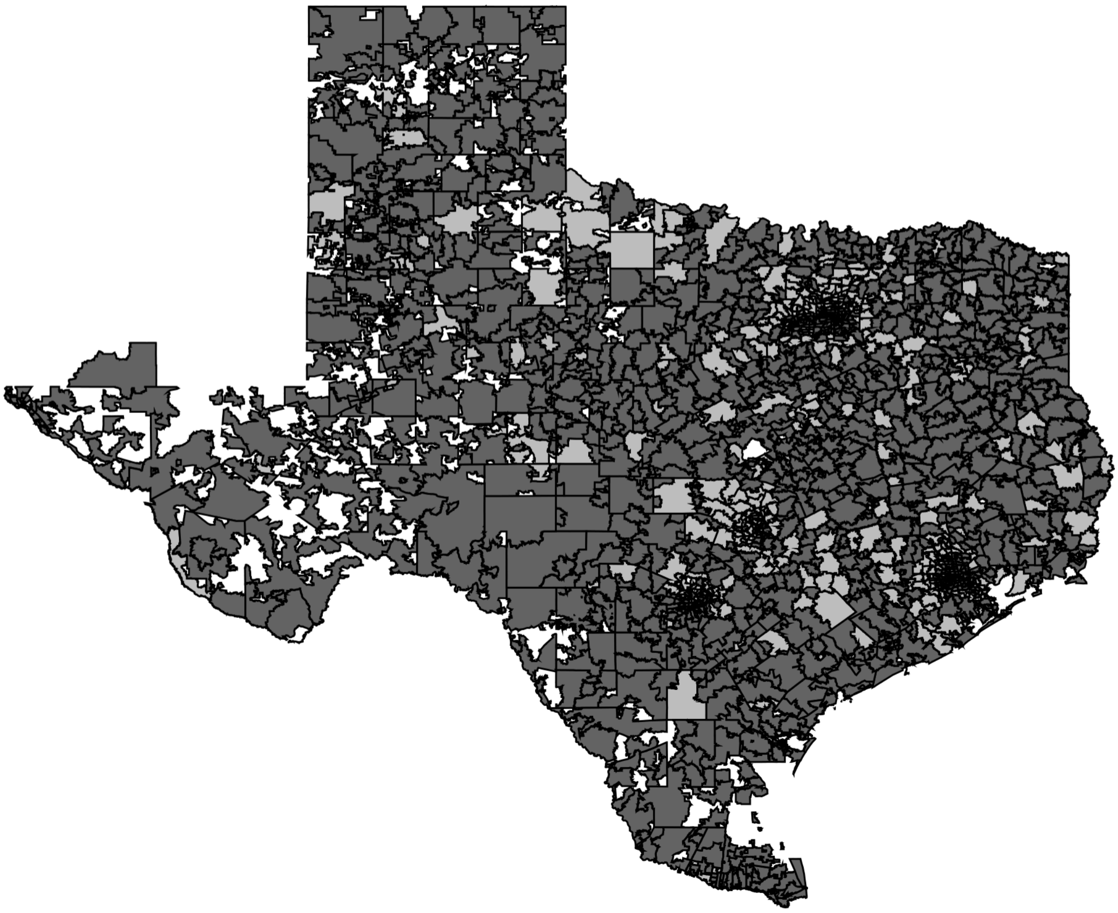
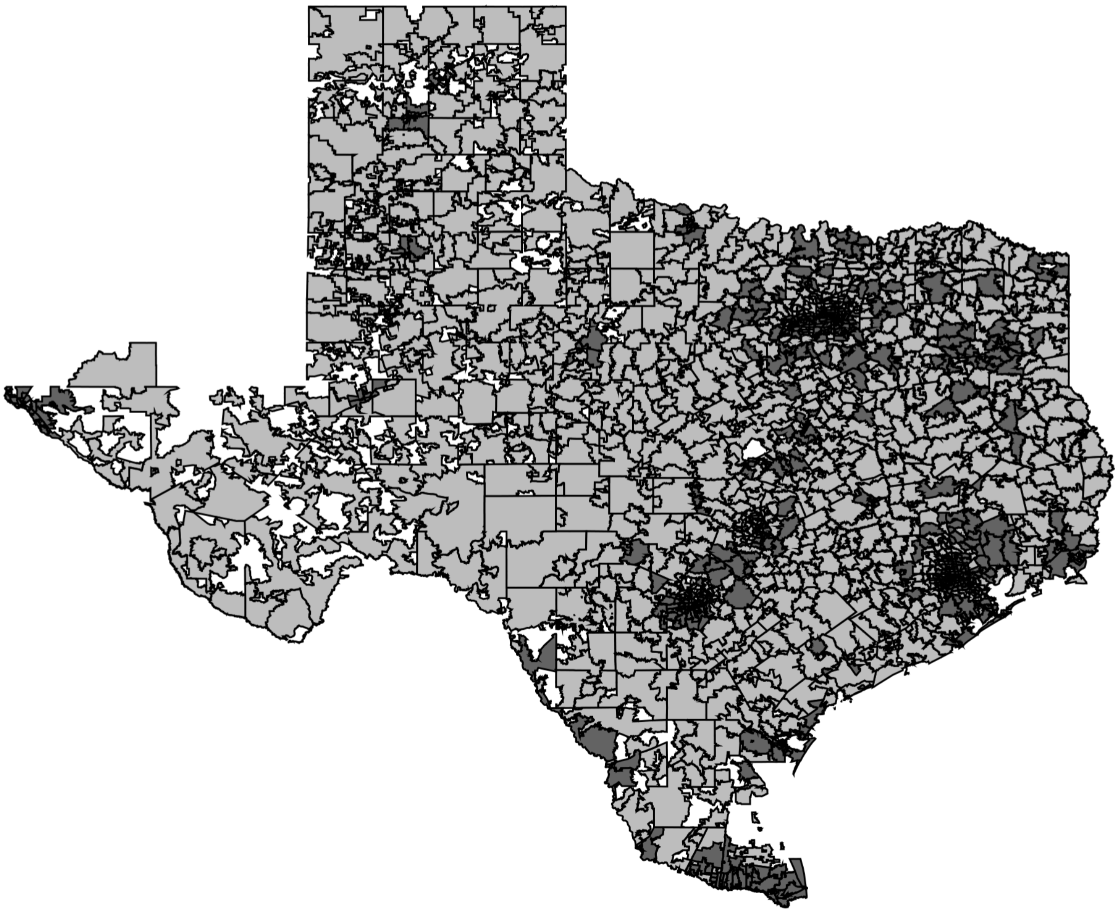


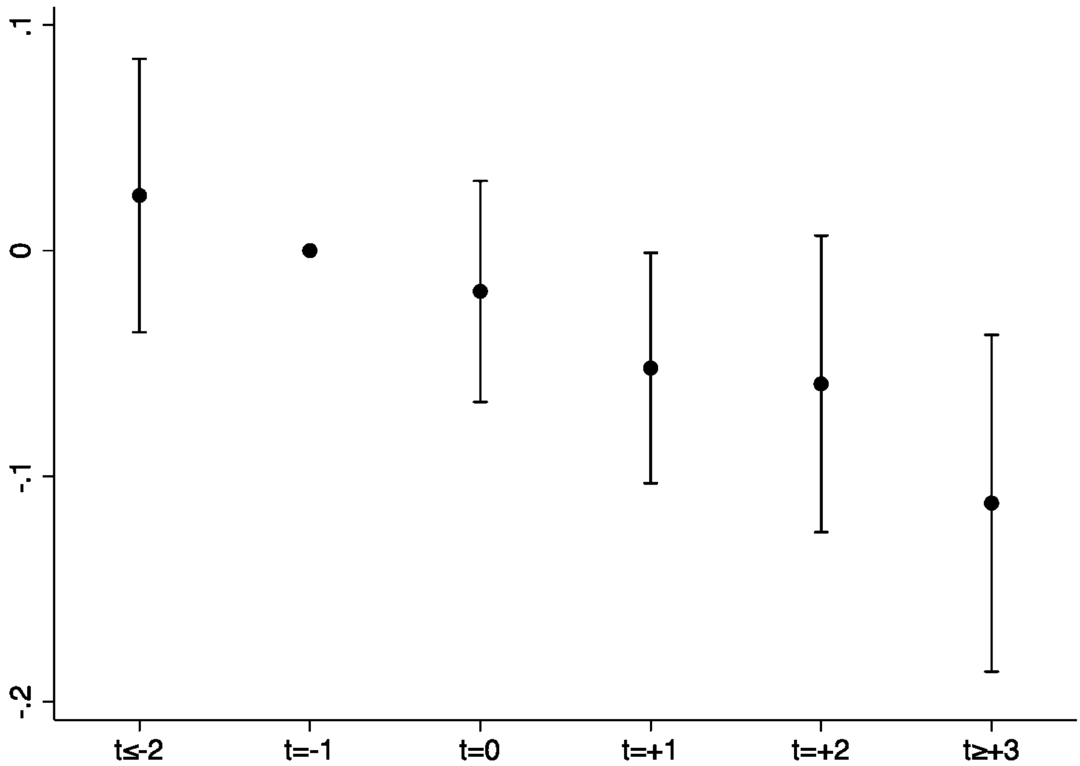
Figure 3.4 (continued)

Panel D: Overlap between Childcare Deserts and Urban Areas in Texas



This figure shows ZIP codes classified as childcare deserts as well as ZIP codes that are classified as both childcare deserts and urban areas. Following Malik and Hamm (2017), childcare deserts are defined as ZIP codes in which the share of children under the age of five to licensed childcare facility capacity is larger than three or areas without any childcare facility. Urban areas are defined as above median population density ZIP codes. Population density is defined as the ratio between the total population to (land) area. I provide detailed variable definitions in Appendix 3.A. The data used to calculate partitions (and create figures) are collected using the approach outlined in Appendix 3.B.3. Panel A shows ZIP codes classified as childcare deserts (darker shaded areas) in Colorado. Panel B shows Colorado ZIP codes classified as both childcare deserts and urban areas (dark shaded areas). Panel C shows ZIP codes classified as childcare deserts (darker shaded areas) in Texas. Panel D shows Texas ZIP codes classified as both childcare deserts and urban areas (dark shaded areas). Non-filled (white) areas denote ZIP codes with missing data.

Figure 3.5: Peer Review and CPA Sole Proprietors



This figure reports the coefficients of ordinary least squares regressions investigating the effect of peer review mandates on the number of CPA sole proprietors in event time. Formally, I estimate $\log(CPASoleProp_{s,t}) = \beta PeerReview_{s,t} + \alpha_s + \gamma_t + \varepsilon_{s,t}$ but replace the policy indicator variable with separate event time dummies, each marking a period relative to the year prior to the policy implementation (t=-1). I omit the indicator for t-1, which serves as the benchmark period. Vertical bands represent 95% confidence intervals for the point estimates in each event time period and are calculated based on robust standard errors.

Table 3.1: Mandatory Peer Review Adoption Dates

Panel A: Peer Review Mandatory Adoption Dates

Adoption Rank #	State	Adoption Year
1	Maryland	2008
2	Michigan	2008
3	California	2010
4	New York	2012
5	Illinois	2012
6	District of Columbia	2012
7	Colorado	2014
8	Florida	2015
9	Hawaii	2015
10	Delaware	2017

Panel B: State Characteristics of Early Adopting vs. Late Adopting States

$Variables$	$Mean_s^{EarlyAdopt}$	$Mean_s^{LateAdopt}$	$Mean_s^{EarlyAdopt} - Mean_s^{LateAdopt}$
ΔGDP_s	-0.039	-0.020	0.019
$\Delta Laborforce_s$	0.012	0.011	0.001
$\Delta Unemploymentrate_s$	0.994	1.197	-0.203
$\Delta JobCreationRate_s$	-0.267	-0.241	-0.026
$CPAs_s$	5.475	6.900	-1.425*
$PublicMember_s$	1.825	3.200	-1.375**
$BoardSize_s$	7.300	10.100	-2.800**
$CPAShare_s$	0.796	0.733	0.063
$NationalFirmShare_s$	0.114	0.119	-0.005
$AICPAMemberShare_s$	0.576	0.486	0.090
$FundingAutonomy_s$	0.700	0.400	0.300**

This table reports mandatory peer review adoption years and state-level characteristics of early and late adopting states. Panel A presents effective years for mandatory peer review, which I collect from the AICPA's "Annual Report on Oversight" (AICPA, 2006; 2008; 2010; 2011; 2013; 2015; 2017) for all states and the District of Columbia that adopted mandatory peer review as of 2006. All other U.S. states adopted mandatory peer review prior to 2006. In Panel B, I present state characteristics of early and late adopting states together with t-tests for differences in each characteristic. Early adopting states are defined as all states adopting mandatory peer review prior to 2006. Detailed definitions for all variables are presented in Appendix 3.A. ***, **, and * denotes statistical significance in mean differences at the 1%, 5%, and 10% level, respectively.

Table 3.2: Summary Statistics

Panel A: Summary Statistics for the Individual CPA Sample (Cross-Section)

Variable	#Licenses	Mean	Std. Dev.	P1	P25	P50	P75	P99
$IssueYear_i$	76,336	1999	14	1965	1988	1999	2011	2018
$ExpirationYear_i$	76,336	2019	1	2012	2019	2019	2019	2020
$LicenseAge_i$	76,336	20	14	1	8	20	31	53
$Female_i$	76,336	0.425	0.494	0.000	0.000	0.000	1.000	1.000
$NonWhite_i$	76,336	0.137	0.344	0.000	0.000	0.000	0.000	1.000
$DisciplinaryAction_i$	76,336	0.041	0.198	0.000	0.000	0.000	0.000	1.000
$MultipleDisciplinaryActions_i$	76,336	0.007	0.081	0.000	0.000	0.000	0.000	0.000

Panel B: Summary Statistics for the CPA Entrepreneur Sample (Cross-Section)

Variable	#Licenses	Mean	Std. Dev.	P1	P25	P50	P75	P99
$IssueYear_i$	7,249	1988	11	1964	1980	1987	1995	2011
$ExpirationYear_i$	7,249	2019	1	2015	2019	2019	2019	2020
$LicenseAge_i$	7,249	31	11	8	24	32	39	55
$Female_i$	7,249	0.312	0.463	0.000	0.000	0.000	1.000	1.000
$NonWhite_i$	7,249	0.141	0.314	0.000	0.000	0.000	0.000	1.000
$DisciplinaryAction_i$	7,249	0.053	0.216	0.000	0.000	0.000	0.000	1.000
$MultipleDisciplinaryActions_i$	7,249	0.018	0.091	0.000	0.000	0.000	0.000	0.000
$FirmDisciplinaryActions_{i,j}$	7,249	0.026	0.161	0.000	0.000	0.000	0.000	1.000
$MultipleFirmDisciplinaryActions_{i,j}$	7,249	0.002	0.042	0.000	0.000	0.000	0.000	0.000

This table provides the summary statistics for my (cross-sectional) *Individual CPA Sample* as well as my (cross-sectional) *CPA Entrepreneur Sample*. I provide detailed variable definitions as well as sample construction steps in Appendices 3.A and 3.B, respectively.

Table 3.3: Demographic Characteristics of CPA Entrepreneurs and CPAs Subject to Disciplinary Actions

Panel A: Demographic Characteristics of CPA Entrepreneurs

	Dependent variable: $CPAStartUp_{i,t}$							
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Female_i$	-0.400***	-0.393***						
	(0.036)	(0.036)						
$NonWhite_i$			0.256***	0.197***				
			(0.047)	(0.047)				
$AgeQuintile_i^2$					-0.428***	-0.439***		
					(0.056)	(0.056)		
$AgeQuintile_i^3$					-0.509***	-0.533***		
					(0.055)	(0.055)		
$AgeQuintile_i^4$					-0.715***	-0.762***		
					(0.053)	(0.053)		
$AgeQuintile_i^5$					-0.623***	-0.679***		
					(0.045)	(0.045)		
$IndividualFine_i$							0.501***	0.515***
							(0.070)	(0.070)
$Constant$	-4.799***	-4.574***	-4.990***	-4.751***	-4.502***	-4.222***	-5.071***	-4.861***
	(0.021)	(0.049)	(0.018)	(0.048)	(0.032)	(0.055)	(0.018)	(0.050)
Year FE	No	Yes	No	Yes	No	Yes	No	Yes
State FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	494,588	494,588	494,588	494,588	494,588	494,588	494,588	494,588
Pseudo R-Squared	0.003	0.009	0.001	0.001	0.006	0.013	0.001	0.006

Table 3.3 (continued)

Panel B: Demographic Characteristics of CPAs Subject to Disciplinary Actions

	Dependent variable: $DisciplinaryAction_{i,t}$					
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)
$Female_i$	-0.292***	-0.294***				
	(0.055)	(0.055)				
$NonWhite_i$			0.025	0.056		
			(0.079)	(0.080)		
$AgeQuintile_i^2$					0.713***	0.713***
					(0.112)	(0.112)
$AgeQuintile_i^3$					0.974***	0.971***
					(0.104)	(0.104)
$AgeQuintile_i^4$					1.010***	1.022***
					(0.099)	(0.100)
$AgeQuintile_i^5$					0.441***	0.463***
					(0.100)	(0.101)
$Constant$	-5.751***	-5.603***	-5.870***	-5.724***	-6.522***	-6.423***
	(0.033)	(0.075)	(0.029)	(0.073)	(0.086)	(0.111)
Year FE	No	Yes	No	Yes	No	Yes
State FE	No	Yes	No	Yes	No	Yes
Observations	494,588	494,588	494,588	494,588	494,588	494,588
Adjusted R-Squared	0.001	0.006	0.000	0.005	0.008	0.013

This table shows the results of the analyses assessing the individual characteristics of CPA entrepreneurs as well as the individual characteristics of CPAs subject to disciplinary actions, which are based on my *Individual CPA Sample* and estimated over a sample period covering 2010 to 2018. I provide detailed variable definitions together with sample construction steps in Appendices 3.A and 3.B, respectively. Panel A, reports odds ratios and (in parentheses) standard errors from logit regressions of $CPAStartup_{i,t}$ on predictors as denoted in each column. Panel B, reports odds ratios and (in parentheses) standard errors from logit regressions of $DisciplinaryAction_{i,t}$ on predictors as denoted in each column. Individual CPAs are removed from the estimation sample after starting a CPA firm. Standard errors are clustered at the individual CPA level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.4: Peer Review Mandates and CPA Entrepreneur Entries

	Dependent variable: $CPAStartUp_{i,t}$					
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)
$PeerReviewAnnouncement_{s,t}$		-0.001**	-0.003***	-0.003***	-0.003***	-0.003***
		(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
$Constant$	0.007***					
	(0.001)					
State-FE	No	Yes	No	No	No	No
Individual-FE	No	No	Yes	Yes	Yes	Yes
Year-FE	No	Yes	Yes	No	No	No
Female-Year-FE	No	No	No	Yes	No	Yes
NonWhite-Year-FE	No	No	No	No	Yes	Yes
Observations	386.337	386.337	386.337	386.337	386.337	386.337
Adjusted R-Squared	0.000	0.000	0.152	0.153	0.151	0.154

This table shows the results of my analysis assessing the relation between peer review mandate announcements and CPA entrepreneurship, which is based on my *Individual CPA Sample* and estimated over a sample period from 2006 to 2014. I further restrict the estimation sample to include only CPAs with licenses granted prior to the announcement of mandatory peer review. I provide detailed variable definitions together with sample construction steps in Appendices 3.A and 3.B, respectively. Reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $CPAStartup_{i,t}$ on $PeerReviewAnnouncement_{s,t}$ and controls as denoted in each column. $CPAStartup_{i,t}$ is an indicator variable equal to one in the year an individual CPA starts a CPA firm. Individuals are removed from the estimation sample after they start a CPA firm. Standard errors are clustered at the individual CPA level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.5: Peer Review Mandates and CPA Entrepreneur Exits

	Dependent variable: $CPAEntrepreneurExit_{i,j,t}$					
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)
$PeerReview_{s,t}$		0.027***	0.078***	0.078***	0.078***	0.077***
		(0.012)	(0.017)	(0.007)	(0.008)	(0.007)
$Constant$	0.013***					
	(0.002)					
State-FE	No	Yes	No	No	No	No
Entrepreneur-FE	No	No	Yes	Yes	Yes	Yes
Year-FE	No	Yes	Yes	No	No	No
Female-Year-FE	No	No	No	Yes	No	Yes
NonWhite-Year-FE	No	No	No	No	Yes	Yes
Observations	40,912	40,912	40,912	40,912	40,912	40,912
Adjusted R-Squared	0.000	0.013	0.136	0.136	0.136	0.136

This table presents the results of my analysis assessing the relation between peer review mandates and CPA entrepreneur exits, which is based on my *CPA Entrepreneur Sample* and estimated over a sample period from 2010 to 2018. I further restrict the estimation sample to include only CPA entrepreneur firms founded prior to the introduction of mandatory peer review. I provide detailed variable definitions together with sample construction steps in Appendices 3.A and 3.B, respectively. Reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $CPAEntrepreneurExit_{i,j,t}$ on $PeerReview_{s,t}$ and controls as denoted in each column. Entrepreneurs (firms) are removed from the estimation sample after they exit. Standard errors are clustered at the entrepreneur level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.6: Peer Review Mandates and CPA Entrepreneur Exits by Past Disciplinary Action Incidents

	Dependent variable: $CPAEntrepreneurExit_{i,j,t}$					
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)
$PeerReview_{s,t} \times PastIndividualDisciplinaryAction_i$	0.007	-0.001	0.001			
	(0.017)	(0.025)	(0.026)			
$PeerReview_{s,t} \times PastFirmDisciplinaryAction_j$				0.013	0.012	0.019
				(0.016)	(0.017)	(0.025)
State-FE	Yes	No	No	Yes	No	No
PastIndividualDisciplinaryAction-FE	Yes	No	No	No	No	No
PastFirmDisciplinaryAction-FE	No	No	No	Yes	No	No
State-PastIndividualDisciplinaryAction-FE	No	Yes	No	No	Yes	No
State-PastFirmDisciplinaryAction-FE	No	No	No	No	Yes	No
Entrepreneur-FE	No	No	Yes	No	No	Yes
State-Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
PastIndividualDisciplinaryAction-Year-FE	Yes	Yes	Yes	No	No	No
PastFirmDisciplinaryAction-Year-FE	No	No	No	Yes	Yes	Yes
Observations	40,912	40,912	40,912	40,912	40,912	40,912
Adjusted R-Squared	0.041	0.044	0.163	0.040	0.043	0.164

This table presents the results of my analysis assessing the relation between peer review mandates and CPA entrepreneur exits conditional on past disciplinary action incidents, which is based on my *CPA Entrepreneur Sample* and estimated over a sample period from 2010 to 2018. I further restrict the estimation sample to include only CPA entrepreneur firms founded prior to the introduction of mandatory peer review. I provide detailed variable definitions together with sample construction steps in Appendices 3.A and 3.B, respectively. Reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $CPAEntrepreneurExit_{i,j,t}$ on $PeerReview_{s,t} \times PastIndividualDisciplinaryAction_i$ or $PeerReview_{s,t} \times PastFirmDisciplinaryAction_j$ and controls as denoted in each column. Entrepreneurs (firms) are removed from the estimation sample after they exit. Standard errors are clustered at the entrepreneur level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.7: Peer Review Mandates and CPA Entrepreneur Exits by Gender and Race

Panel A: Peer Review Mandates and CPA Entrepreneur Exits by Gender

	Dependent variable: $CPAEntrepreneurExit_{i,j,t}$		
Independent variables:	(1)	(2)	(3)
$PeerReview_{s,t} \times Female_i$	0.023**	0.029**	0.032***
	(0.011)	(0.014)	(0.007)
State-FE	Yes	No	No
Female-FE	Yes	No	No
State-Female-FE	No	Yes	No
Entrepreneur-FE	No	No	Yes
State-Year-FE	Yes	Yes	Yes
Female-Year-FE	Yes	Yes	Yes
Observations	40,912	40,912	40,912
Adjusted R-Squared	0.042	0.042	0.164

Panel B: Peer Review Mandates and CPA Entrepreneur Exits by Race

	Dependent variable: $CPAEntrepreneurExit_{i,j,t}$		
Independent variables:	(1)	(2)	(3)
$PeerReview_{s,t} \times NonWhite_i$	-0.017	-0.020	-0.045
	(0.022)	(0.029)	(0.037)
State-FE	Yes	No	No
NonWhite-FE	Yes	No	No
State-NonWhite-FE	No	Yes	No
Entrepreneur-FE	No	No	Yes
State-Year-FE	Yes	Yes	Yes
NonWhite-Year-FE	Yes	Yes	Yes
Observations	40,912	40,912	40,912
Adjusted R-Squared	0.040	0.041	0.161

This table presents the results of my analysis assessing the relation between peer review mandates and CPA entrepreneur exits conditional on gender or race, which is based on my *CPA Entrepreneur Sample* and estimated over a sample period from 2010 to 2018. I further restrict the estimation sample to include only CPA entrepreneur firms founded prior to the introduction of mandatory peer review. I provide detailed variable definitions together with sample construction steps in Appendices 3.A and 3.B, respectively. In Panel A, reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $CPAEntrepreneurExit_{i,j,t}$ on $PeerReview_{s,t} \times Female_i$ and controls, as denoted in each column. In Panel B, reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $CPAEntrepreneurExit_{i,j,t}$ on $PeerReview_{s,t} \times NonWhite_i$ and controls, as denoted in each column. $CPAEntrepreneurExit_{i,j,t}$ is an indicator variable equal to one in the year a CPA entrepreneur (firm) exits. Entrepreneurs (firms) are removed from the estimation sample after they exit. Standard errors are clustered at the entrepreneur level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.8: Peer Review Mandates and CPA Entrepreneur Exits by Gender and Age

	Dependent variable: $CPAEntrepreneurExit_{i,j,t}$					
	Young Age		*Mid Age*		*Old Age*	
Independent variables:	*Female*	*Male*	*Female*	*Male*	*Female*	*Male*
$PeerReview_{s,t}$	0.132***	0.066***	0.082***	0.055***	0.110***	0.098***
	(0.012)	(0.009)	(0.009)	(0.006)	(0.009)	(0.007)
Test for Difference in $PeerReview_{s,t}^{Gender}$						
F-Test [p-value]*:*	[0.000]		[0.012]		[0.286]	
Entrepreneur-FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,844	8,404	4,484	9,310	5,170	10,266
Adjusted R-Squared	0.158	0.161	0.119	0.143	0.176	0.119

This table presents the results of my analysis assessing the relation between peer review mandates and CPA entrepreneur exits conditional on gender and age, which is based on my *CPA Entrepreneur Sample* and estimated over a sample period from 2010 to 2018. I further restrict the estimation sample to include only CPA entrepreneur firms founded prior to the introduction of mandatory peer review. *Young Age*, *Mid Age*, and *Old Age* refer to sample partitions based on age terciles. *Female* and *Male* refer to sample partitions based on gender indicators (as previously defined). I provide detailed variable definitions together with sample construction steps in Appendices 3.A and 3.B, respectively. Reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $CPAEntrepreneurExit_{i,j,t}$ on $PeerReview_{s,t}$ and controls, as denoted in each column. Entrepreneurs (firms) are removed from the estimation sample after they exit. Standard errors are clustered at the entrepreneur level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.9: Peer Review Mandates, CPA Entrepreneur Exits, and Childcare Availability

Panel A: Childcare Availability and Exits Among Young Female CPA Entrepreneurs

	Dependent variable: $CPAEntrepreneurExit_{i,j,z,t}$		
Independent Variables:	(1)	(2)	(3)
$PeerReview_{s,t} \times ChildcareDesert_z$	0.091***	0.097***	0.118***
	(0.022)	(0.032)	(0.027)
State-FE	Yes	No	No
ChildcareDesert-FE	Yes	No	No
State-ChildcareDesert-FE	No	Yes	No
Entrepreneur-FE	No	No	Yes
State-Year-FE	Yes	Yes	Yes
ChildcareDesert-Year-FE	Yes	Yes	Yes
Estimation Sample	Young Female	Young Female	Young Female
Observations	3,844	3,844	3,844
Adjusted R-Squared	0.064	0.066	0.215

Panel B: Childcare Availability and Exits Among Young Male CPA Entrepreneurs

	Dependent variable: $CPAEntrepreneurExit_{i,j,z,t}$		
Independent variables:	(1)	(2)	(3)
$PeerReview_{s,t} \times ChildcareDesert_z$	-0.014	0.009	-0.020
	(0.014)	(0.022)	(0.018)
State-FE	Yes	No	No
ChildcareDesert-FE	Yes	No	No
State-ChildcareDesert-FE	No	Yes	No
Entrepreneur-FE	No	No	Yes
State-Year-FE	Yes	Yes	Yes
ChildcareDesert-Year-FE	Yes	Yes	Yes
Estimation Sample	Young Male	Young Male	Young Male
Observations	8,404	8,404	8,404
Adjusted R-Squared	0.034	0.035	0.202

This table presents the results of my analysis assessing the relation between peer review mandates and CPA entrepreneur exits conditional on local childcare availability. Estimates are based on the *CPA Entrepreneur Sample* spanning a sample period from 2010 to 2018. I further restrict the estimation sample to include only CPA entrepreneur firms founded prior to the introduction of mandatory peer review. I provide detailed variable definitions together with sample construction steps in Appendices 3.A and 3.B, respectively. In addition, I provide a detailed guide on how to construct local childcare availability proxies in Section B.3. Reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $CPAEntrepreneurExit_{i,t}$ on $PeerReview_{s,t} \times ChildcareDesert_i$ and controls, as denoted in each column. $ChildcareDesert_i$ is an indicator variable taking the value of one, if CPA entrepreneur (firm) *i* (*j*) is located in a ZIP code *z* classified as having low childcare availability. In Panel A, I restrict the estimation sample to include young female CPA Entrepreneurs only (as previously defined in Table 8). In Panel B, I restrict the estimation sample to include young male CPA Entrepreneurs only (as previously defined in Table 8). CPA entrepreneurs (firms) are removed from the estimation sample after they exit. Standard errors are clustered at the entrepreneur level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.10: Peer Review Mandates and CPA Sole Proprietors

Panel A: Census NES Sample Summary Statistics

Variable	Obs.	Mean	Std. Dev.	P1	P25	P50	P75	P99
$CPASoleProp_{s,t}$	72	1,795	1,398	34	901	1,133	2,946	4,734
$CPASolePropRevenue_{s,t}$	72	109,775	97,932	2,761	44,336	60,122	167,860	341,508
$AvgSolePropRevenue_{s,t}$	72	61	15	45	52	55	69	121
$\log(CPASoleProp_{s,t})$	72	6.960	1.368	3.526	6.803	7.024	7.988	8.463
$\log(CPASolePropRevenue_{s,t})$	72	11.053	1.277	7.923	10.700	11.002	12.030	12.741
$\log\left(AvgSolePropRevenue_{s,t}\right)$	72	4.093	0.219	3.807	3.954	4.014	4.236	4.792

Panel B: Peer Review Mandates and CPA Sole Proprietors

	Dependent variables:		
	$\log(CPASoleProp_{s,t})$	$\log(CPASolePropRevenue_{s,t})$	$\log\left(AvgSolePropRevenue_{s,t}\right)$
Independent variables:	(1)	(2)	(3)
$PeerReview_{s,t-1}$	-0.051***	-0.047	0.003
	(0.021)	(0.038)	(0.029)
State-FE	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes
Observations	72	72	72
Adjusted R-Squared	0.998	0.993	0.862

This table presents summary statistics together with the results of my analysis assessing the relation between peer review mandates and CPA sole proprietors, which is based on my *Census NES Sample* covering contiguous U.S. states from 2007 to 2016. I provide detailed variable definitions together with sample construction steps in Appendices 3.A and 3.B, respectively. In Panel A, I provide summary statistics for my estimation sample. In Panel B, I provide the results of my regression analysis. Reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $\log(CPASoleProp_{s,t})$ (Column 1), $\log\left(CPASolePropRevenue_{s,t}\right)$ (Column 2), or $\log\left(AvgSolePropRevenue_{s,t}\right)$ (Column 3) on $PeerReview_{s,t-1}$ and controls, as denoted in each column. I report robust standard errors. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

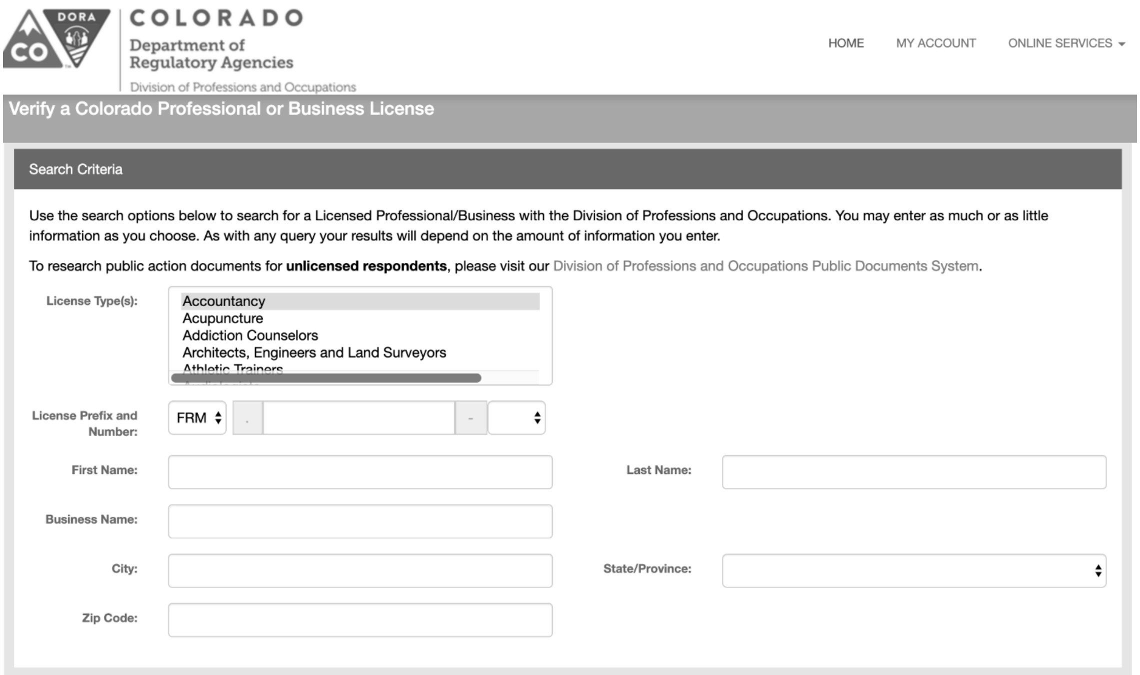
3.9 Online Appendix

3.9.1 An Illustrated Guide to Collecting CPA License Data

In this section, I provide a guide to collecting CPA license information using online license verification tools. In the following graphically illustrated example, I focus on collecting Colorado CPA firm licenses. The general approach, however, can be employed for collecting CPA licensing data for most states and for both firm- and individual-level licenses. Of course, any collection approach depends on the state-specific license verification tool, its search fields, search result limitations, etc. For most states, license verification tools are available via the website of the respective State Board of Accountancy. A list of State Board of Accountancy websites can be obtained from the National Association of State Boards of Accountancy (NASBA).[98]

Turning to the Colorado example, the CPA license verification tool is available via the website of Colorado's Department of Regulatory Agencies. The Colorado Department of Regulatory Agencies allows to verify licenses for most licensed firms (and professionals) holding a Colorado license. To collect Colorado CPA firm licenses, one first has to select "Accountancy." This limits the returned search results to licenses issued by Colorado's State Board of Accountancy. In addition, selecting a "License Prefix," allows to limit the returned results to only include CPA firm licenses or individual CPA licenses. In this example, I limit the returned search results to include CPA firm licenses only by selecting the license prefix "FRM." The following figure displays Colorado's license verification tool preset to search for CPA firms only:

[98] The NASBA further provides a license verification tool that allows to validate CPA licenses across multiple states. This tool is available at: https://cpaverify.org/about/. The NASBA's license verification tool allows to search for both individual and firm CPA licenses. While the possibility to search for licenses across jurisdictions is appealing, a downside is that NASBAs verification tool, for most states, provides less information for both individuals and firms than state-specific verification tools provide. Furthermore, the NASBA's tool limits the number of displayed results per search. Thus, the NASBA's verification tool provides an appealing approach for collecting information on specific individual CPAs or CPA firms but is of limited use only when attempting to collect information on the population of individual CPAs and / or CPA firms.



For most states, search functions only accept searches with at least one defined search parameter. One possible way to fulfil this requirement, while not searching for specific (and potentially unknown) individuals or firms, is searching by ZIP codes. ZIP codes provide a fine enough search criterion, i.e., using ZIP codes does not return too many search results, which could otherwise exceed the maximum number of results a license verification tool returns. At the same time, ZIP codes are a coarse criterion, which limits the number of distinct searches one has to perform to collect information on the population of either CPA firms or individual CPAs. Besides, lists of *all* ZIP codes (in a state) are comparably easy to obtain (e.g., via Census geography reference files). Thus, ZIP codes provide a search criterion, which allows the researcher to capture the population, while minimizing the number of searches returning zero results.

Turning back to the example, entering a ZIP code into the Colorado search mask yields a list of all CPA firms in the respective ZIP code. The following figure provides an example search result:

	Name	License Number	License Status	Contact Type	City	State	Zip Code
Detail	[redacted]	FRM[redacted]	Active	[redacted]			
Detail		FRM	Active				
Detail		FRM	Expired				
Detail		FRM	Expired				
Detail		FRM	Active				

The returned search result list includes the name of each CPA firm in the entered ZIP code, CPA firm license numbers, and some geographical information. To obtain more detailed information on a CPA firm, one has to go to the firm-specific license entry (by clicking on the "Detail" icon in the above example). The following figure shows such firm-specific license entry:

License Details

Lookup Detail View

Licensee Information

This serves as primary source verification of the license.*

**Primary source verification: License information provided by the Colorado Division of Professions and Occupations, established by 24-34-102 C.R.S.*

Name	Public Address
[redacted]	[redacted]

Credential Information

License Number	License Method	License Type	License Status	Original Issue Date	Effective Date	Expiration Date
FRM[redacted]		Public Accounting Firm	Active	[redacted]/2002	[redacted]/2017	[redacted]/2020

Supervision

Relationship	Supervisor/Supervisee	License	Start Date	Relationship Type
Supervised By	[redacted]	CPA[redacted]	[redacted]/2002	Responsible Individual

Board/Program Actions

Case Number	Public Action	Resolution	Effective Date	Completed Date
2012-[redacted]	CLS Stipulation	Stipulation	[redacted]/2013	[redacted]/2013

Firm-specific license entries provide information on the name, address, license number, as well as license issue and expiration dates. Furthermore, for Colorado, the firm-specific license entry contains information on the manager-owners as well as disciplinary action incidents. The information provided will, of course, differ by state and license verification tool. Most states provide information on names, addresses, license numbers, as well as license issue and expiration dates.

3.9.2 CPA Entrepreneurs and Disciplinary Actions

Part of the paper's findings may be interpreted as peer review mandates having unintended consequences as (unintended) effects on participation may outweigh (intended) screening on quality. For instance, female CPA entrepreneurs exit at higher rates than male CPA entrepreneurs with the introduction of mandatory peer review, while they, on average, show lower probabilities of being subject to disciplinary actions. This conjecture, thus far, builds on comparing disciplinary action incident rates between male and female CPAs for the population of individual CPAs in Colorado and Texas.

Prior literature (e.g., Levine and Rubinstein, 2017; 2018), however, argues that entrepreneurs may show distinct risk preferences, which may correlate with the likelihood of disciplinary action incidents. To the extent that risk preferences, or other unobservable factors, correlate with both the likelihood to become an entrepreneur as well as disciplinary action rates, it is not clear whether we observe similar differences in disciplinary action probabilities, say, across gender when holding constant the decision to become a CPA entrepreneur. Therefore, in this section, I separately explore the individual-level characteristics of CPA entrepreneurs subject to disciplinary actions. The analysis is akin to the analysis of individual CPAs subject to disciplinary actions discussed in Section 3.4 (Table 3.3, Panel B presents the respective estimates). The main difference compared to the prior analysis is that I limit the estimation sample to include CPA entrepreneurs only.

Table 3.OA-2 shows the results of this analysis. I show odds ratios from logit regressions of $DisciplinaryAction_{i,t}$, an indicator variable equal to one if CPA i is

subject to a disciplinary action in year *t*, on individual-level characteristics. Overall, the comparison between estimates based on the sample restricted to include CPA entrepreneurs only (Table 3.OA-2) and estimates based on all individual CPAs in my sample (Table 3.3, Panel B) does not suggest major differences. Female CPA entrepreneurs are less likely to be subject to disciplinary actions than male CPA entrepreneurs, while there are no meaningful differences comparing white to non-white CPA entrepreneurs. One difference when comparing entrepreneur estimates to individual CPA estimates is that younger CPA entrepreneurs are more likely to receive disciplinary actions. Even though these differences are not statistically significant at conventional levels when including state and year fixed effects (Column 6), I further explore this pattern. Specifically, I re-estimate a CPA entrepreneurship (prediction) model in which I include recent disciplinary action incidents as predictor variables.

I present the results of this analysis in Table 3.OA-3. Coefficient estimates from a linear probability model suggest that individual CPAs with recent disciplinary actions tend to be more likely to become CPA entrepreneurs than those individual CPAs without recent disciplinary actions. This relation also remains when holding constant (time-invariant) risk preferences. One explanation for this pattern might be that part of the choice to start a CPA firm is determined by the availability of other (employment) opportunities. Note that disciplinary action incidents are observable to potential employers via license verification tools (as I show in Section 3.9.1), and the presence of such disciplinary action incidents may decrease the chances of employment in audit firms. This notion is in line with recent research modeling the decision to become an entrepreneur in Roy models (e.g., Levine and Rubinstein, 2018). In these models, the decision to become an entrepreneur is determined by the value of other outside (employment) options. Publicly observable disciplinary action incidents may lower the chances of employment, which, ceteris paribus, increases the odds of an individual to become an entrepreneur by lowering the outside option value. Furthermore, note that, if such dynamics are at play and low(-quality)-type CPAs tend to select into entrepreneurship, CPA entrepreneurs provide a powerful setting for exploring whether mandatory peer review facilitates screening on quality.

3.9.3 Peer Review Mandates and CPA Entrepreneur Firm Dissolutions

My previous analyses leverage CPA license information and operationalize CPA entrepreneur (firm) entries and exits by CPA firm license issue and expiration dates. While this approach, in principal, allows to construct datasets effectively comprising the population of individual CPAs and CPA firms, there might be concerns that CPA firm license issue and expiration dates do not capture actual (entrepreneur) firm entries and exits. Firms must hold CPA firm licenses to provide audit services but may provide non-audit services without holding a CPA firm license. Thus, it could be that my prior analyses pick up firms ceasing to offer audit services, while continuing to operate as distinct (entrepreneur) firms. While, at a conceptual level, the decision to stop offering audit services is akin to an exit from the audit industry and, thus, of first order interest, I further explore whether CPA (entrepreneur) firms dissolve subsequent to the passage of peer review mandates.

To do so, I construct a dataset of CPA firm incorporation and dissolution dates. I obtain these data through the OpenCorporates project. OpenCorporates allows to search incorporation documents for all (incorporated) entities in the United States. I access incorporation and dissolution dates by searching OpenCorporates profiles for each CPA (entrepreneur) firm in my sample via the OpenCorporates API.[99,100] I use CPA firm names in this search process and, first, collect OpenCorporates identifiers. In the second step, I access each individual profile using these (matched) identifiers and collect the CPA (entrepreneur) firm profile data.[101]

I then repeat the main analyses of the relation between peer review mandates and CPA entrepreneur exits based on a sample of CPA firms, for which I can collect incorporation (and dissolution) data. For these firms, I use incorporation dates (not CPA firm license issue dates) and dissolution dates (not CPA firm license expiration dates) to create a CPA (entrepreneur) firm panel. Using this sample, I estimate the baseline exit model as well as models in which I allow the policy intervention indicator to vary conditional on entrepreneur-level demographic characteristics.

Table 3.OA-4 shows the results of the analysis that explores the relation between peer review mandates and CPA (entrepreneur) firm dissolutions. Throughout specifications, we observe that CPA (entrepreneur) firms tend to dissolve (not just drop CPA firm licenses). Table 3.OA-5 reports the results of the analysis exploring the relation between CPA (entrepreneur) firm dissolutions conditional on entrepreneur-level demographic characteristics. Again, this analysis closely resembles my estimates using CPA license issue and expiration dates, i.e., the increase in exit rates subsequent to peer review mandates is pronounced for female but not for non-white CPA entrepreneurs.

Of course, while some of the missing information on firm incorporation (and dissolution) dates might be due spelling errors in names (and other common challenges when conducting fuzzy merges based on names), other non-matched firms may truly not

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[99] I am indebted to OpenCorporates for providing full research API access.

[100] The OpenCorporates API documentation is available under: https://api.opencorporates.com/documentation/API-Reference.

[101] The query scripts for looping through profile identifiers and collecting profile information are available from the author upon request.

incorporate, and this incorporation decision may correlate with other factors, such as firm size.[102] To the extent that matches (or non-matches) indirectly capture firm size (and non-matches are concentrated for exiting firms), prior estimates may pick up such size differences. To explore this possibility, I further assess whether the relation between peer review mandates and CPA entrepreneur exits varies conditional on whether the respective CPA firm can be identified in the OpenCorporates database. Table 3.OA-6 reports the results of this analysis. Coefficient estimates are positive but insignificant at conventional levels.

Overall, these additional tests suggest that the main findings are robust to using firm incorporation and dissolution dates as an alternative way of operationalizing CPA (entrepreneur) firm entries and exits. In addition, the estimates suggest that CPA entrepreneurs do not only cease to offer audit services but stop operating as distinct (entrepreneur) firms altogether.

3.9.4 Peer Review Mandates and CPA Entrepreneur Firm Filings

I also explore whether firms exhibit changes in entity statutes around the passage of peer review mandates. My classification of entrepreneurs relies on cross-sectional data on firm ownership. Thus, there might be a potential concern that I am misclassifying (some) CPA firms as CPA entrepreneur firms. Note that such concern would only be severe if such misclassification coincided with the introduction of mandatory peer review. Similarly, some of my cross-sectional tests rely on inferring an entrepreneur's location based on ZIP codes. If entrepreneurs moved to other locations in anticipation of peer review mandates, this may raise potential concerns about the validity of my

[102] For instance, Cascino et al. (2020) show that there are differences in the choice of incorporation (type) across CPA firms of different size.

(geographical) partitioning. To explore these possibilities, I collect data on entity filings indicating changes in entity ownership or entity addresses.

Specifically, I collect firm filing histories for CPA (entrepreneur) firms in Colorado from Colorado's Secretary of State website. The Colorado Secretary of State website allows to search all business paper documents via its "business database search" function.[103] I search this online database for each CPA (entrepreneur) firm, which I can also identify in the OpenCorporates database (I introduce the OpenCorporates database as well as my matching approach for using this database to obtain CPA entrepreneur firm incorporation in dissolution dates in Section 3.9.3). For each of these CPA (entrepreneur) firms, I collect the full set of regulatory filings. I then identify all filing events pertaining to ownership changes by conducting string searches and define an indicator variable ($AgentChange_{i,j,t}$) that takes the value of one in the year a firm files a document relating to ownership changes. Similarly, I search for changes pertaining to entity address changes and code an according indicator variable ($AddressChange_{i,j,t}$).

Table 3.OA-7 presents the results of the analysis that investigates potential ownership (and address) changes subsequent to the introduction of mandatory peer review. Since the collection of filing histories is only feasible for Colorado CPA (entrepreneur) firms, I present estimates for my main cross-sectional tests based on individual-level demographic characteristics. Throughout specifications, point estimates do not suggest meaningful changes in ownership or addresses around the introduction of mandatory peer review.

[103] The business paper documents search function is available under: https://www.sos.state.co.us/biz/BusinessEntityCriteriaExt.do.

Table 3.OA-1: Additional Variable Definitions

Variable	*Definition*
$RecentDisciplinaryAction_{i,t}$	An indicator variable set equal to one if individual CPA *i* has a fine record with the respective State Board of Accountancy over the last three years, and zero otherwise. Past three years are defined relative to year *t*.
$NotMatchedOpenCorps_i$	An indicator variable set to one if CPA entrepreneur firm *j* is not identifiable via the OpenCorporates firm profile search function, and zero otherwise. I provide a detailed description of the search process as well as this classification in Section 3.9.3.
$AgentChange_{i,j,t}$	An indicator variable equal to one if CPA entrepreneur firm *j* files a document pertaining to changes in the agent status in year *t*, and zero otherwise.
$AddressChange_{i,j,t}$	An indicator variable equal to one if CPA entrepreneur firm *j* files a document pertaining to changes in the entity address in year *t*, and zero otherwise.

This table presents variable definitions for additional variables used in the analyses presented in Section 3.9. All other variables used in the analyses in Section 3.9 are defined in Appendix 3.A.

Table 3.OA-2: Demographic Characteristics of CPA Entrepreneurs Subject to Disciplinary Actions

	Dependent variable: $DisciplinaryAction_{i,t}$					
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)
$Female_i$	-0.250**	-0.318**				
	(0.112)	(0.153)				
$NonWhite_i$			-0.160	0.006		
			(0.247)	(0.248)		
$AgeQuintile_i^2$					-0.392**	-0.215
					(0.195)	(0.197)
$AgeQuintile_i^3$					-0.387**	-0.102
					(0.191)	(0.196)
$AgeQuintile_i^4$					-0.463**	-0.089
					(0.211)	(0.217)
$AgeQuintile_i^5$					-0.905***	-0.477*
					(0.236)	(0.244)
$Constant$	-5.402***	-5.028***	-5.464***	-5.121***	-5.093***	-4.982***
	(0.080)	(0.155)	(0.071)	(0.152)	(0.125)	(0.186)
Year FE	No	Yes	No	Yes	No	Yes
State FE	No	Yes	No	Yes	No	Yes
Observations	52,160	52,160	52,160	52,160	52,160	52,160
Adjusted R-Squared	0.001	0.047	0.000	0.045	0.006	0.047

This table shows the results of my analysis assessing the individual characteristics of CPA entrepreneurs subject to disciplinary actions, which is based on my *CPA Entrepreneur Sample*. I provide detailed variable definitions together with sample construction steps in Appendices 3.A and 3.B, respectively. Reported odds ratios and (in parentheses) standard errors are from logit regressions of $DisciplinaryAction_{i,t}$ on predictors as denoted in each column. Standard errors are clustered at the CPA entrepreneur level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.OA-3: Recent Disciplinary Actions and CPA Entrepreneurship

	Dependent variable: $CPAStartUp_{i,t}$					
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)
$RecentDisciplinaryAction_{i,t-1}$		0.008***	0.007***	0.008***	0.007***	0.008***
		(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
$Constant$	0.007***					
	(0.000)					
Individual-FE	No	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No	No	No
State-Year-FE	No	No	Yes	No	No	Yes
Gender-Year-FE	No	No	No	Yes	No	Yes
NonWhite-Year-FE	No	No	No	No	Yes	Yes
Observations	494,588	494,588	494,588	494,588	494,588	494,588
Adjusted R-Squared	0.000	0.150	0.151	0.150	0.150	0.151

This table shows the results of the analysis assessing the relation between recent disciplinary action incidents and CPA entrepreneurship, which is based on my *Individual CPA Sample* and estimated over a sample period covering 2010 to 2018. I provide detailed variable definitions together with sample construction steps in Appendices 3.A (as well as Table 3.OA-1) and 3.B, respectively. The table reports coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $CPAStartup_{i,t}$ on $RecentDisciplinaryAction_{i,t-1}$ and controls as denoted in each column. $CPAStartup_{i,t}$ is an indicator variable equal to one in the year an individual CPA starts a CPA firm. Individuals are removed from the estimation sample after they start a CPA firm. Standard errors are clustered at the individual CPA level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.OA-4: Peer Review Mandates and CPA Entrepreneur Firm Dissolutions

	Dependent variable: $CPAFirmDissolution_{i,j,t}$					
Independent variables:	(1)	(2)	(3)	(4)	(5)	(6)
$PeerReview_{s,t}$		-0.004	0.008***	0.008***	0.009***	0.008***
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$Constant$	0.005***					
	(0.001)					
State-FE	No	Yes	No	No	No	No
Entrepreneur-FE	No	No	Yes	Yes	Yes	Yes
Year-FE	No	Yes	Yes	No	No	No
Female-Year-FE	No	No	No	Yes	No	Yes
NonWhite-Year-FE	No	No	No	No	Yes	Yes
Observations	17,728	17,728	17,728	17,728	17,728	17,728
Adjusted R-Squared	0.000	0.002	0.142	0.143	0.142	0.143

This table presents the results of my analysis assessing the relation between peer review mandates and CPA entrepreneur firm dissolutions. The estimation sample is based on my *CPA Entrepreneur Sample* merged with entity filing information as outlined in Section 3.9.3. The sample period covers 2010 to 2018 and I restrict the estimation sample to only include CPA entrepreneur firms founded prior to the introduction mandatory peer review. I provide detailed variable definitions together with additional sample construction steps in Appendices 3.A (as well as Table 3.OA-1) and 3.B (as well as Section 3.9.3), respectively. Reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $CPAFirmDissolution_{i,j,t}$ on $PeerReview_{s,t}$ and controls as denoted in each column. $CPAFirmDissolution_{i,j,t}$ is an indicator variable equal to one in the year a CPA entrepreneur (firm) exits. Entrepreneurs (firms) are removed from the estimation sample after they dissolve. Standard errors are clustered at the entrepreneur level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.OA-5: Peer Review Mandates and CPA Entrepreneur Firm Dissolutions by Gender and Race

Panel A: Peer Review Mandates and CPA Entrepreneur Firm Dissolutions by Gender

	Dependent variable: $CPAFirmDissolution_{i,j,t}$		
Independent variables:	(1)	(2)	(3)
$PeerReview_{s,t} \times Female_i$	0.014**	0.015*	0.019***
	(0.005)	(0.008)	(0.005)
State-FE	Yes	No	No
Female-FE	Yes	No	No
State-Female-FE	No	Yes	No
Entrepreneur-FE	No	No	Yes
State-Year-FE	Yes	Yes	Yes
Female-Year-FE	Yes	Yes	Yes
Observations	17,728	17,728	17,728
Adjusted R-Squared	0.004	0.004	0.144

Panel B: Peer Review Mandates and CPA Entrepreneur Firm Dissolutions by Race

	Dependent variable: $CPAFirmDissolution_{i,j,t}$		
Independent variables:	(1)	(2)	(3)
$PeerReview_{s,t} \times NonWhite_i$	-0.019	-0.019	-0.038
	(0.019)	(0.029)	(0.020)
State-FE	Yes	No	No
NonWhite-FE	Yes	No	No
State-NonWhite-FE	No	Yes	No
Entrepreneur-FE	No	No	Yes
State-Year-FE	Yes	Yes	Yes
NonWhite-Year-FE	Yes	Yes	Yes
Observations	17,728	17,728	17,728
Adjusted R-Squared	0.050	0.050	0.156

This table presents the results of my analysis assessing the relation between peer review mandates and CPA entrepreneur firm dissolutions conditional on individual-level demographic characteristics. The estimation sample is based on my *CPA Entrepreneur Sample* merged with entity filing information as outlined in Section 3.9.3. The sample period covers 2010 to 2018 and I restrict the estimation sample to only include CPA entrepreneur firms founded prior to the introduction mandatory peer review. I provide detailed variable definitions together with additional sample construction steps in Appendices 3.A (as well as Table 3.OA-1) and 3.B (as well as Section 3.9.3), respectively. Reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $CPAFirmDissolution_{i,j,t}$ on $PeerReview_{s,t} \times Female_i$ or $PeerReview_{s,t} \times NonWhite_i$ and controls, as denoted in each column. $CPAFirmDissolution_{i,j,t}$ is an indicator variable equal to one in the year a CPA entrepreneur (firm) exits. Entrepreneurs (firms) are removed from the estimation sample after they exit. Standard errors are clustered at the entrepreneur level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.OA-6: Peer Review Mandates and CPA Entrepreneur Exits by Matching Result

	Dependent variable: $CPAEntrepreneurExit_{i,j,t}$		
Independent variables:	(1)	(2)	(3)
$PeerReview_{s,t} \times NotMatchedOpenCorps_i$	0.022	0.020	0.032
	(0.015)	(0.018)	(0.021)
State-FE	Yes	No	No
NotMatched-FE	Yes	No	No
State-NotMatched-FE	No	Yes	No
Entrepreneur-FE	No	No	Yes
State-Year-FE	Yes	Yes	Yes
NotMatched-Year-FE	Yes	Yes	Yes
Observations	40,912	40,912	40,912
Adjusted R-Squared	0.037	0.037	0.169

This table presents the results of my analysis assessing the relation between peer review mandates and CPA entrepreneur exits conditional on whether a CPA (entrepreneur) firm name can be matched against the OpenCorporates database. I outline this matching procedure in Section 3.9.3. $NotMatchedOpenCorps_i$ is an indicator equal to one, if a CPA (entrepreneur) firm does not have an identifiable profile entry in the OpenCorporates database. The presented analysis is based on my *CPA Entrepreneur Sample*. The sample period covers 2010 to 2018 and I restrict the estimation sample to only include CPA entrepreneur firms founded prior to the introduction mandatory peer review. I provide detailed variable definitions together with additional sample construction steps in Appendices 3.A (as well as Table 3.OA-1) and 3.B (as well as Section 3.9.3), respectively. Reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $CPAEntrepreneurExit_{i,j,t}$ on $PeerReview_{s,t} \times NotMatchedOpenCorps_i$ and controls, as denoted in each column. $CPAEntrepreneurExit_{i,j,t}$ is an indicator variable equal to one in the year a CPA entrepreneur exits. Entrepreneurs are removed from the estimation sample after they exit. Standard errors are clustered at the entrepreneur level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.OA-7: Peer Review Mandates and CPA Entrepreneur Firm Filings

Panel A: Peer Review Mandates and CPA Entrepreneur Firm Filings by Gender

	Dependent variables:			
	$AgentChange_{i,j,t}$		$AddressChange_{i,j,t}$	
Independent variables:	(1)	(2)	(3)	(4)
$PeerReview_{s,t} \times Female_i$	0.012	0.010	0.012	0.014
	(0.016)	(0.016)	(0.016)	(0.017)
Female-FE	Yes	No	Yes	No
Entrepreneur-FE	No	Yes	No	Yes
Year-FE	Yes	Yes	Yes	Yes
Observations	4,267	4,267	4,267	4,267
Adjusted R-Squared	0.000	0.039	0.000	0.046

Panel B: Peer Review Mandates and CPA Entrepreneur Firm Filings by Race

	Dependent variables:			
	$AgentChange_{i,j,t}$		$AddressChange_{i,j,t}$	
Independent variables:	(1)	(2)	(3)	(4)
$PeerReview_{s,t} \times NonWhite_i$	0.023	0.026	-0.019	-0.017
	(0.020)	(0.020)	(0.029)	(0.030)
NonWhite-FE	Yes	No	Yes	No
Entrepreneur-FE	No	Yes	No	Yes
Year-FE	Yes	Yes	Yes	Yes
Observations	4,267	4,267	4,267	4,267
Adjusted R-Squared	0.000	0.046	0.000	0.045

This table presents the results of the analysis assessing the relation between peer review mandates and CPA entrepreneur firm agent or address changes conditional on individual-level attributes. The estimation sample is based on my *CPA Entrepreneur Sample* but restricted to Colorado and augmented with CPA firm filing information as outlined in Section 3.9.4. The sample period covers 2010 to 2018 and I restrict the estimation sample to only include CPA entrepreneur firms founded prior to the introduction mandatory peer review. I provide detailed variable definitions together with additional sample construction steps in Appendices 3.A (as well as Table 3.OA-1) and 3.B (as well as Section 3.9.4), respectively. Reported coefficients and (in parentheses) standard errors are from ordinary least squares regressions of $AgentChange_{i,j,t}$ or $AddressChange_{i,j,t}$ on $PeerReview_{s,t} \times Female_i$ or $PeerReview_{s,t} \times NonWhite_i$ as well as controls, as denoted in each column. Standard errors are clustered at the entrepreneur level.. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

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