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

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Abstract

We exploit the staggered introduction of CPA Mobility provisions in the United States to study the effects of spatial licensing requirements on the labor market for accounting professionals. Specifically, we examine whether the removal of licensing-induced geographic barriers affects CPA wages and employment levels, as well as the pricing and quality of professional services. We find that, subsequent to the adoption of CPA Mobility provisions, wages of accounting professionals decrease, whereas employment levels are unaffected. The documented wage effect stems from smaller CPA firms, is more pronounced for CPAs holding senior positions, and persists over time. We also find that service prices decline and that this effect is concentrated in local CPA firms. Moreover, we document that the increased wage and price pressure is not associated with deteriorating service quality. Collectively, our results suggest that the removal of occupational licensing barriers has sizable effects on labor supply and service prices. Our findings inform the current regulatory debate on occupational licensing.

Keywords: Occupational Licensing, CPA Licensure, CPA Mobility, Labor Market Outcomes, Auditing

JEL Classification: D45, J20, K20, L51, M41, M42

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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, thereby allowing incumbents to extract rents (Friedman, 1962; Stigler, 1971; Maurizi, 1974; Rottenberg, 1980).[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

[1] In Section 1 of the Online Appendix, we provide a review of the labor economics literature on occupational licensing regulation.

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-differences (DiD) research design, to compare labor market outcomes between states that adopt CPA Mobility and states that have not (yet) adopted the policy.

We expect that, after CPA Mobility adoption, out-of-state CPAs may decide to enter opening states as they find it less costly to offer their services across state lines. This, in turn, increases service supply in opening states and thus reduces CPA wages and service prices. We further expect the competitive effects of CPA Mobility to accrue to (and derive from) small local CPA firms because, prior to CPA Mobility, large CPA firms, such as the Big 4 firms, could already circumvent regional barriers by leveraging on their national networks. We note that, for the competitive effects of CPA Mobility to occur, the physical relocation of out-of-state CPAs to the opening states is not necessary. This is because CPAs, unlike other

[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 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.

professionals such as healthcare providers, offer highly tradable services that do not require "face-to-face" provision. Moreover, even the mere *threat of entry* of out-of-state CPAs would suffice for wages and service prices to decline.

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 all 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%, that is, more than half of the 1.8% pre-treatment average annual real wage growth rate. We regard the magnitude of the estimated wage effect as economically meaningful, especially considering 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. This finding is in line with our conjecture that the actual migration of our-of-state CPAs to opening states is not a necessary condition for the increase in service supply to obtain.

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. 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, including the share of board members working in small local CPA firms, which reinforces the view that CPA Mobility constitutes a prominent shift for smaller CPA firms.

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, based on the assumption that markets for small and large CPA firms are *de facto* segregated (e.g., Cook et al., 2020), 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 since large CPA firms are likely unaffected by CPA Mobility adoption. 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 furthermore indicate that the effects stem 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 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 addition, this dataset does not include the wages paid by large audit-service providers (e.g., Big 4 firms), thereby allowing us to focus on small local CPA firms. Using this sample, we find larger 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. These findings are in line with the reported wage declines and the 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. 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. CPAs facing enhanced wage or fee pressure might in fact 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 substandard professional service cases based on disciplinary action announcements by the AICPA. Second, we construct a dataset of pension plan deficient filer cases (i.e., substandard 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. Furthermore, by focusing on small local CPA firms, our study sheds light on the labor market dynamics of this under-investigated segment of the audit market.

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. 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.

2. Institutional Background

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. This was the case even after most states passed the 150-hour rule, effectively harmonizing entry requirements for CPAs (Barrios, 2019). 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.[3] 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, 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

[3] 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.

their own constituents) and reduce their fee revenue generation opportunities.[4] 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 adoption.

In Table 1, Panel A, we report both the enactment, as well as the effective, dates for each state adopting CPA Mobility during our sample period.[5] 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).[6] This mitigates the concern that the policy adoption

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[4] 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.

[5] 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). The results of these tests yield qualitatively similar inferences.

[6] 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

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 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. 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 allays the concern that adoption timings may be driven by local labor market shocks.

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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, Panel A).

3. Empirical Analysis

3.1. Identification

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. Based on a simple model of spatial licensing requirements, which we discuss in Section 2 of the Online Appendix, we expect that the introduction of CPA Mobility leads to a reduction in wages. To capture the effect of the policy, we estimate model specifications of the following form:

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

In this model, $y_{s,t}$ is the respective state-year CPA mean wage or employment level.[7] The policy indicator ($CPAMobility_{s,t-1}$) is switched on for states that adopt CPA Mobility provisions in the year following the adoption and thereafter. 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.[8]

The coefficient β captures the policy effect on wages or employment levels. To control for state-level time-invariant confounders and time-varying factors affecting the response variable of interest, we include state fixed effects (α_s) and year fixed effects (γ_t), respectively. 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

[7] In the analyses presented in Sections 3.6, 3.7, and 3.8, we also explore the effects on service pricing and quality as alternative dependent variables.
[8] 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).

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.[9] We provide detailed variable definitions in the Appendix.

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

3.2. 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 aggregated 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 the Appendix and in Section 3 of the Online Appendix, respectively. Table 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

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[9] In untabulated robustness tests, we also include further state-year-level controls for demographics: gender, minority, and marital status variables. The inclusion of demographic controls does not alter the tenor of our empirical findings.

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 (at the 5% level in Columns (1) to (5) and at the 10% level in Column (6)) 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 one-year decline of USD 541 subsequent to CPA Mobility adoption. In gauging and interpreting the economic magnitude of this effect, it is important to make several considerations. First, the estimated effect is economically meaningful since it represents more than half of the 1.8% ten-year pre-treatment average annual real wage growth rate. Second, the documented wage declines persist over time and reflect the overall size of the accounting profession. Assuming that wages would have continued to grow at the pre-treatment growth rate, our point estimate of 1.0% would entail discounted “forgone” wages over a five-year period of around USD 8,300 for the average CPA firm employee, or USD 3.25 billion when aggregated across all CPA firms.[10] Third, a limitation of our QCEW program data is that wages

[10] A detailed discussion of how we gauge the economic magnitude of the documented wage effect by computing counterfactual forgone wages is presented in Section 5 of the Online Appendix.

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.[11] The magnitude of our results should therefore be interpreted as a lower bound of the policy effect.

An important identifying assumption of our DiD design is that wage trends between treated states and control states would have moved 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, 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.[12] The evidence from this figure suggests that prior to CPA Mobility adoption: (i) treatment effect magnitudes are economically small and statistically indistinguishable from zero; and (ii) there is no evidence of differential wage trends across treated and control states over time. In contrast, treatment effects 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, our treatment effects become (slightly) stronger over time, with wage declines persisting at least up to four years after the time of adoption. This suggests that the documented effects are unlikely to be explained by potential reversals due to wages rebounding from temporary drops. This persistence is consistent with predictions from the intra-industry (or "cross-hauling") trade theory (e.g., Brander and Krugman, 1983) according

[11] We directly address this limitation in our analyses presented in Section 3.6 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 subset of states.

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

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.

Because the introduction of CPA Mobility provisions allows out-of-state CPAs to enter adopting states more easily, a potential concern with our DiD analysis is that control observations may be indirectly treated (i.e., a potential violation of the stable unit treatment value assumption (SUTVA)). To assess whether spillover effects from our control group may be driving our findings, we conduct a set of tests, which we discuss in Section 7 of the Online Appendix. We find that CPA Mobility adoption in neighbor states exert no effect on wages of CPA firm employee in opening states, which suggests that indirect control group effects are unlikely to drive our findings (Table OA-4 in the Online Appendix).

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.

3.3. 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 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. Based on the point estimate presented in Column (6), we can reject with 95% confidence that CPA Mobility leads to a change in employment exceeding 2.8%. The absence of a statistically significant effect of CPA Mobility on employment levels is consistent with the idea that an increase in service supply in opening states can occur even without physical relocation of CPAs. This is because the services offered by CPAs are highly tradable and, as such, can also be provided remotely.

3.4. 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 6 of the Online Appendix, 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 that we use (i.e., our macro and migration control variables) to produce a treatment effect of zero (Table OA-3 in the Online Appendix). Nevertheless, we employ several other strategies to alleviate this potential concern, including: (i) a DiDiD analysis in which we use different *within-state* control groups (Section 3.4.1); (ii) a contiguous-county analysis in which we compare labor market outcomes across neighboring counties located in different states (Section 3.4.2); (iii) a further analysis in which we control for the potential confounding effect of neighbor states' CPA Mobility adoption (Section 7 of the Online Appendix); and (iv) a sensitivity test in which we control for another important regulation affecting CPA labor market, the 150-hour rule, which some states adopted during our sample period (Section 8 of the Online Appendix).

3.4.1. Difference-in-Difference-in-Differences Analysis

To allay 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 a control group of *large CPA firms* in the same state. In this analysis, we implicitly assume that the labor markets for small and large CPA firms are *de facto* segregated. Prior literature provides evidence consistent with product market segregation between small and large CPA firms (e.g., Cook et al., 2020).[13] Since labor is arguably the main input in audit production functions (e.g., Francis, 2011) and segregation on product markets (i.e., output markets) inherently calls for skill specialization, it is reasonable to assume that CPA labor markets (i.e., input markets) are segregated.[14]

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;[15] (ii) state × firm size fixed effects to

[13] In fact, prior research shows that: (i) CPA firms tend to specialize by client size (Doogar and Easley, 1998; Ferguson et al., 2018); (ii) large client firms match with large CPA firms (Gerakos and Syverson, 2015; Cook et al., 2020); and (iii) the resulting product market segregation between large and small CPA firms has increased over time (DeFond and Lennox, 2011; Duguay et al., 2020).

[14] Since recent audit research finds that small and large CPA firms require different skills from prospective employees (Hann et al., 2020) and labor economics research shows that differences in skill requirements are inversely related to job mobility (e.g., Gathmann and Schoenberg, 2010), labor market segregation between small and large CPA firm is likely to obtain.

[15] State × year fixed effects also allow us to control for state-year specific regulatory interventions affecting accounting professionals, such as the introduction of the 150-hour rule in some of our sample states.

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.[16]

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 Section 3 of the Online Appendix.

We report the results of this analysis in Table 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 experience a one-year wage decline of 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, we estimate separately the effect of CPA Mobility on employee wages in large CPA firms. We find statistically insignificant effects (Table OA-6, Panel B, in the Online Appendix), 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, Panel B, we plot event-time coefficient estimates around CPA Mobility adoption dates. Unfortunately,

[16] For instance, the introduction of the Sarbanes-Oxley Act increased the demand for services provided by large CPA firms (e.g., Duguay et al. 2020) and our fixed effects structure allows us to control for such labor demand shocks.

SUBS program data are unavailable prior to 2007, which limits the extent to which we can examine pre-treatment trends. With this caveat in mind, we observe a treatment effect for the years leading up to CPA Mobility adoption that is economically small and statistically indistinguishable from zero. Reported wage declines accrue subsequent to CPA Mobility adoption only.

Next, following Bloomfield et al. (2017), we use *legal professionals* as an additional within-state control group. We leverage on the industry-level information 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."[17]

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 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 experience a one-year wage decline of USD 487 after CPA Mobility adoption. Moreover, in line with our previous findings, we do not observe statistically

[17] In an additional set of tests, which we discuss in Section 9 of the Online Appendix and whose results are reported in Table OA-7 in the Online Appendix, 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.

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 OA-6, Panel C, in the Online Appendix,). To gauge the validity of the parallel-trends assumption, we map out the reported effects in event-time. The graphical evidence in Figure 1, Panel C, suggests that treated and control units do not exhibit economically meaningful and statistically significant pre-treatment differences in wages as coefficient estimates are fairly uniform with no apparent trend.

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 treatment effect on wages that we document in this analysis, which we report in Table 3, Panel C, Column (1), though only statistically significant at the 10% level, is very similar in magnitude to that we observe in our main tests.

3.4.2. County-Level Analysis of CPA Mobility Wage and Employment Effects

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 2). 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:

$$y_{c,b,s,t} = \beta CPAMobility_{s,t-1} + \partial' X_{s,t-1} + \vartheta Unemployment_{c,t-1} + \alpha_c + \gamma_{b,t} + \varepsilon_{c,b,s,t}. \quad (2)$$

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 and time-varying factors along each border segment, we include county fixed effects (α_c) and border × year fixed effects ($\gamma_{b,t}$), respectively. 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 the Appendix.

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 Section 3 of the Online Appendix. Table 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 2, Panel B). Furthermore, similar to the state-level analysis in Section 3.3, 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.

3.5. *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*. Based on our simple model of spatial licensing requirements (Section 2 of the Online Appendix), 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 these potential *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 Section 3 of the Online Appendix.

In Figure 3, we provide graphical evidence on the long-term effects of CPA Mobility on wage sensitivities. 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 sensitivities of legal professionals, the wage sensitivities 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 5, Panel A, presents the results of this analysis.

Consistent with the graphical evidence shown in Figure 3, we document a statistically significant decline in wage sensitivities 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 estimated effects for accounting and legal professionals is not statistically significant.

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 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.

3.6. *Within-CPA Firm Effects on Wages, Billing Rates, and Hours Charged*

Our analyses so far are based on QCEW program data, which are aggregated by industry. As discussed in Section 3.2, 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.

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 Section 3 of the Online Appendix.

In Table 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 one-year wage decline of USD 2,564. The coefficient estimate is, however, only statistically significant at the 10% level. 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 ($Log(HoursCharged_{s,w}^{MAP})$) is statistically 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 that 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 4 of the Online Appendix 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 unlikely 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). 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 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.[18] Our results on billing rates (Panel B, Column (2)) are consistent with the latter explanation.

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

[18] In Section 10 of the Online Appendix, we present an additional analysis in which we assess whether our findings hinge on concurrent changes in compensation structures and/or on CPAs adjusting their wage structures to receive preferable tax treatments in response to CPA Mobility adoption and find that this is not the case.

with the effect being more pronounced for accounting professionals holding a CPA license.[19] 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.

3.7. CPA Mobility Effects 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 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.[20] To this end, we collect private employee benefit

[19] However, the documented differential effect of CPA Mobility on billing rates between junior and senior accounting professional is only statistically significant at the 10% level (Table 6, Panel D, Column (1)).

[20] 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

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 Section 3 of the Online Appendix.

In Table 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 3.1 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 the Appendix. 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 type characteristics.

In Table 7, Panel B, we present the results of our analysis. The coefficient estimate (statistically significant at the 10% level) presented in Column (1) suggests that, relative to the pre-treatment period, pension plan audit fees decline by 1.7% subsequent to the introduction

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.

of CPA Mobility, which implies a one-year decline of USD 277. 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 experience a one-year decline of USD 359.

Overall, our results are 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, which is consistent with the evidence we document in our analysis comparing wage responses in large vs. small CPA firms.

3.8. CPA Mobility Effects 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 easing 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.[21] 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 data construction details for our *AICPA Misconduct Sample* are presented in Section 3 of the Online Appendix. In Table 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 3.7 are associated with pension plan audit-service quality deterioration. To investigate this

[21] Armitage and Moriarity (2016) examine AICPA disciplinary actions from 1980 to 2014.

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 Section 3 of the Online Appendix. In Table 8, Panel B, we present the results of this analysis, which are inconsistent with a negative effect of CPA Mobility on service quality. If anything, our results indicate statistically significant declines in EBSA deficient filer enforcement cases (Columns (3) and (7)). Also, based on our insignificant OLS point estimate on $CPAMobility_{s,t-1}$ (Column (2)), we can reject with 95% confidence that CPA Mobility leads to an increase in deficient filer enforcement cases that exceeds 0.5.

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 Section 3 of the Online Appendix.

An inherent limitation of relying on data from one state only is, however, that we cannot compare firm-level disciplinary action probabilities across states. Nevertheless, as the competitive effects of CPA Mobility should entirely accrue to (and derive from) small CPA firms, we design empirical tests in which we estimate service quality deterioration effects on small CPA firms using large CPA firms as a control group. 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.[22]

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 as well as related factors that may correlate with disciplinary action incidents—we document, again, no statistically significant effect on service quality. In Column (2), the coefficient on $CPAMobility_{t-1}^{Colorado} \times YoungCPAFirm_i$ of -0.002 is statistically insignificant. Based on the estimated treatment effect, we can reject with 95% confidence that CPA Mobility leads to an increase in the probability of a disciplinary action that exceeds 0.2 percentage points.

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.[23]

4. 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

[22] We gauge the validity of this assumption by collecting additional data from the Census Business Dynamics Statistics program, which provides employment data aggregated 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.

[23] 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).

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

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Appendix: 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, and zero otherwise (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 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).
$BillsIntroduced_{s,t}$	Natural logarithm of one plus 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).
$BillsEnacted_{s,t}$	Natural logarithm of one plus 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).

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Appendix (*continued*)

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Variable	*Definition*
$Wage^{QCEW}_{s,t}$	State-year annual wage mean in state *s* in year *t* (Source: QCEW variable name "avg_annual_pay").
$Log(Wage^{QCEW}_{s,t})$	Natural logarithm of $Wage^{QCEW}_{s,t}$.
$Employment^{QCEW}_{s,t}$	Employment level for state *s* in year *t* (Source: QCEW variable name "annual_avg_emplvl").
$Log(Employment^{QCEW}_{s,t})$	Natural logarithm of $Employment^{QCEW}_{s,t}$.
$CPAMobility_{s,t}$	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.
$WithinImmigration_{s,t}$	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).
$Wage^{SUSB}_{s,t}$	State-year annual average wage in state s, firm size category *j*, and year *t* (Source: SUSB).
$Log(Wage^{SUSB}_{s,t})$	Natural logarithm of $Wage^{SUSB}_{s,j,t}$.
$Employment^{SUSB}_{s,t}$	Employment level in state *s*, firm size category *j*, and year *t* (Source: SUSB).
$Log(Employment^{SUSB}_{s,t})$	Natural logarithm of $Employment^{SUSB}_{s,j,t}$.
$AvgEmployment^{SUSB}_{s,t}$	Average employment per establishment in state s, firm size category *j*, and year *t* calculated as $Employment^{SUSB}_{s,j,t}$ divided by $Firms^{SUSB}_{s,j,t}$(Source: SUSB).
$Log(AvgEmployment^{SUSB}_{s,t})$	The natural logarithm of $AvgEmployment^{SUSB}_{s,j,t}$.
$Firms^{SUSB}_{s,t}$	The number of establishments in state *s*, firm size category *j*, and year *t* (Source: SUSB).
$Log(Firms^{SUSB}_{s,t})$	The natural logarithm of $Firms^{SUSB}_{s,t}$.
$Small_j$	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).
$Wage^{QCEW}_{c,b,s,t}$	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").
$Log(Wage^{QCEW}_{c,b,s,t})$	Natural logarithm of $Wage^{QCEW}_{c,b,s,t}$.
$Employment^{QCEW}_{c,b,s,t}$	Employment level in county *c* at border *b* located in state *s* in year *t* (Source: QCEW variable name "annual_avg_emplvl").
$Log(Employment^{QCEW}_{c,b,s,t})$	Natural logarithm of $Employment^{QCEW}_{c,b,s,t}$.
$Unemployment_{c,b,s,t}$	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).

(continued)

Appendix (*continued*)

(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 employment-weighted 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 employment-weighted 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 employment-weighted 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 employment-weighted 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*.

(continued)

Appendix (*continued*)

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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 $AuditFees_{p,s,t}$.
$Contributions_{p,s,t}$	Total contributions divided by the total number of plan assets (Source: Form 5500 Schedule H).
$Income_{p,s,t}$	Plan income divided by total 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).
$Log(Assets_{p,s,t})$	Natural logarithm of 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).
$Participants_{pst}$	Plan participants divided by total plan assets (Source: Form 5500).
$CPAMobility_{a,s,t-1}^{LocalAuditFirm}$	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)).
$YoungCPAFirm_{i}$	An indicator variable equal to one if firm *i*'s entry year is above the median firm entry year in 2007, and zero otherwise.

Figure 1: CPA Mobility Wage Effects in Event-Time

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

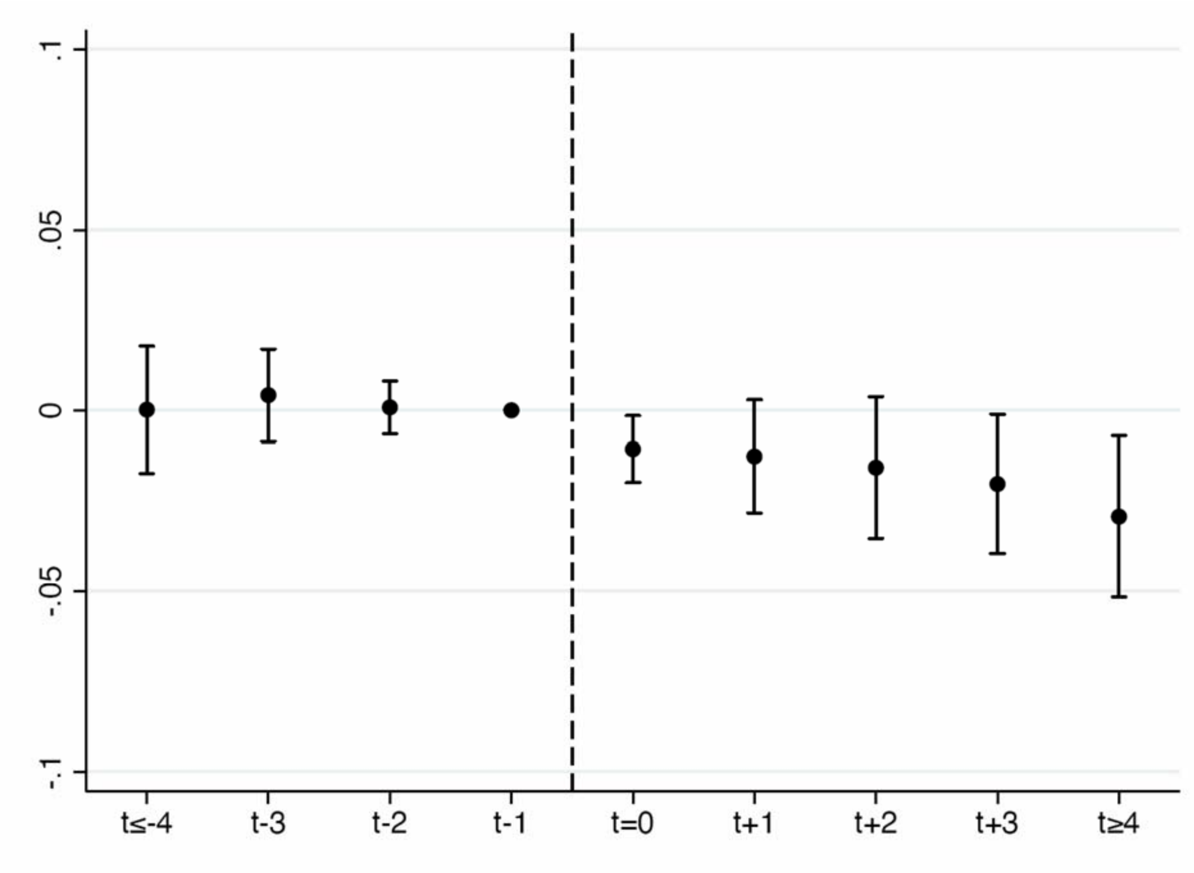


Figure 1 (*continued*)

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

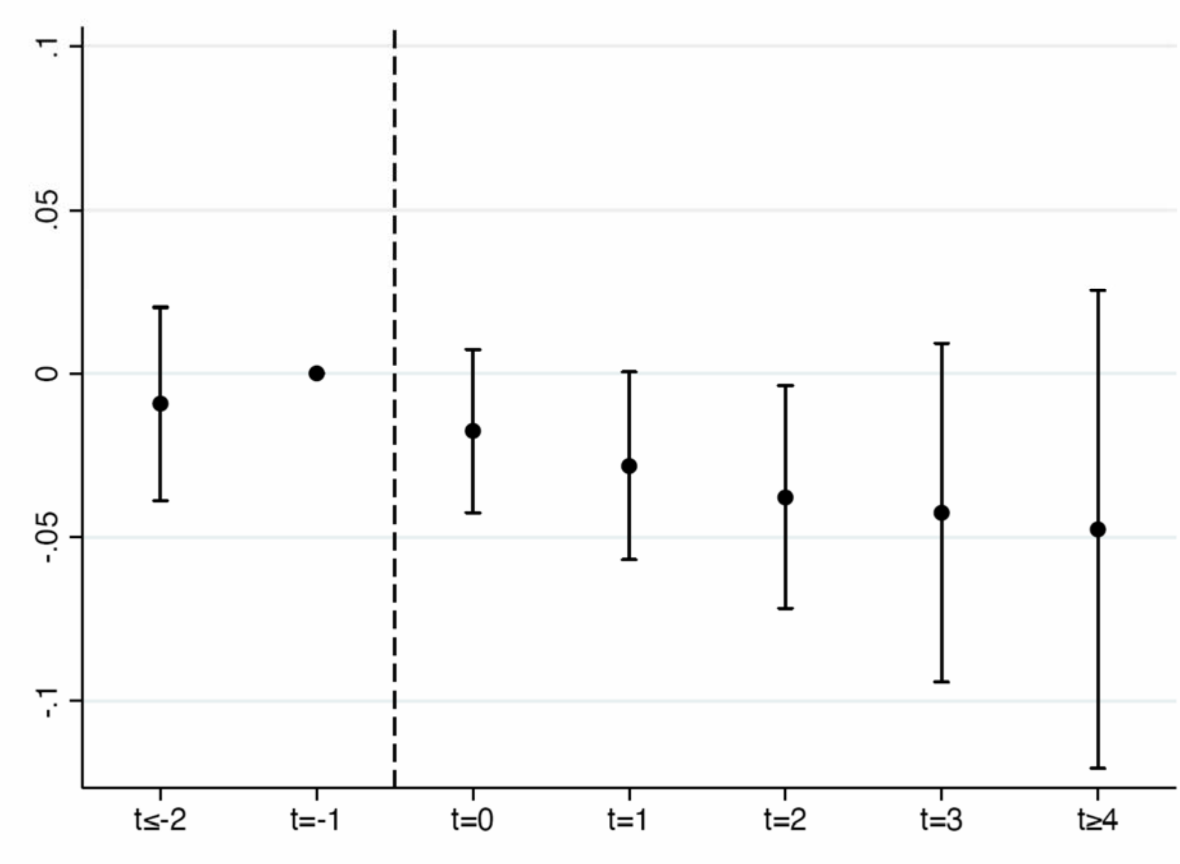
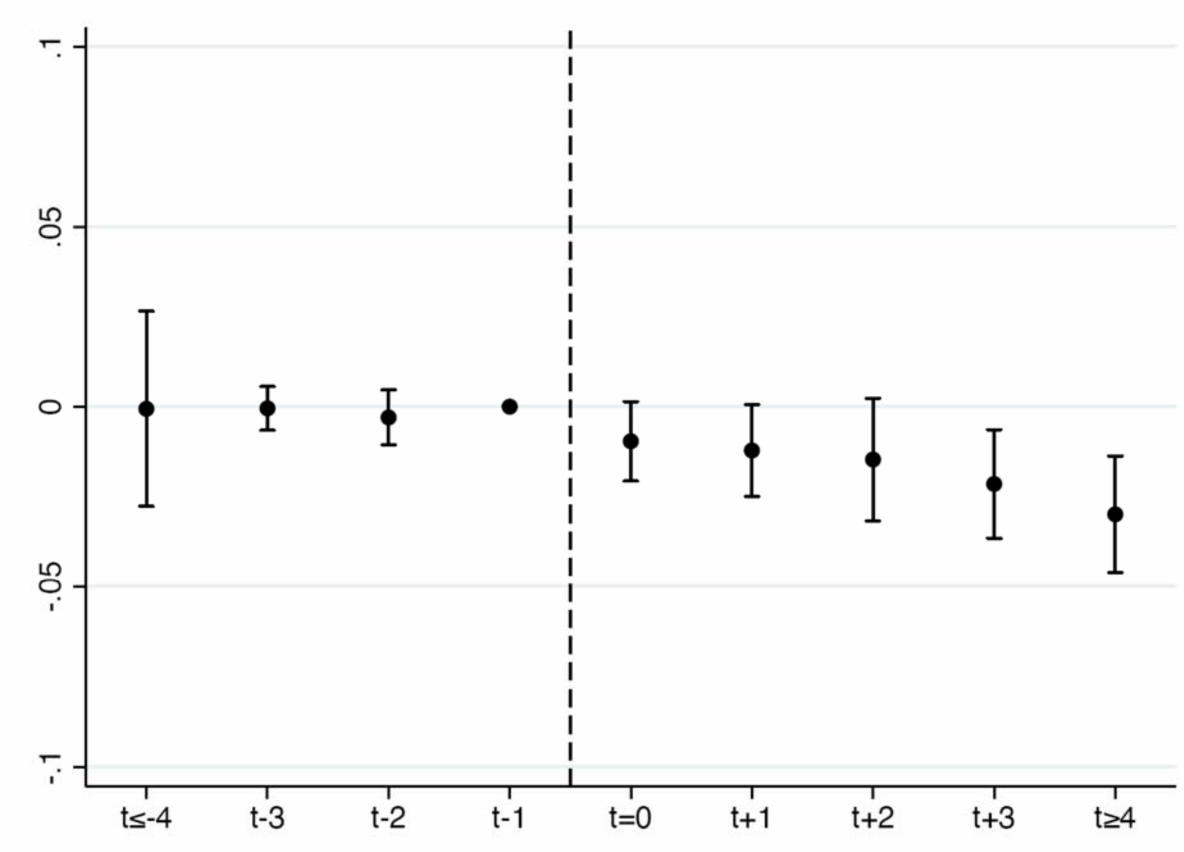


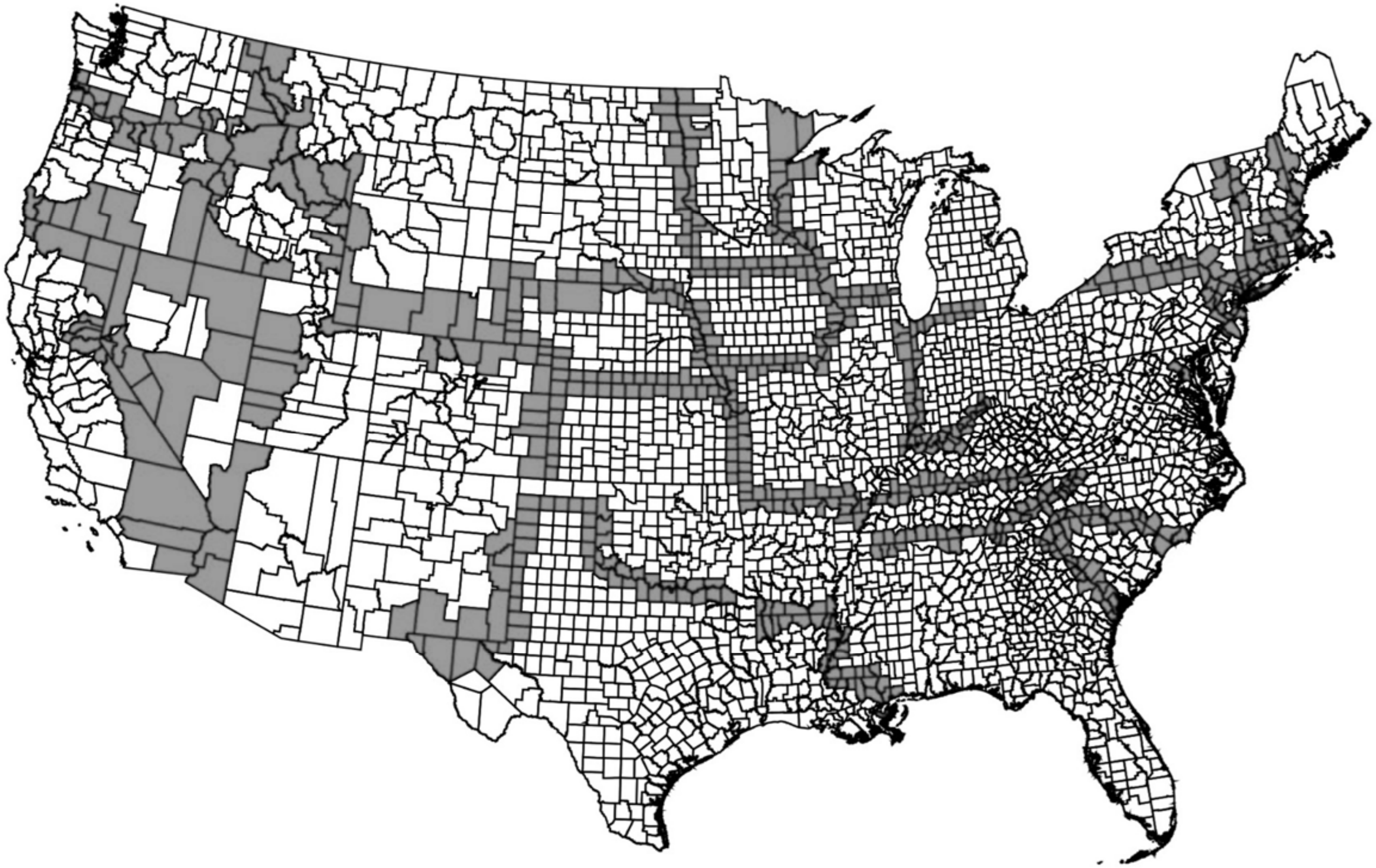
Figure 1 (*continued*)

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 $Log(Wage_{s,t}^{QCEW}) = \beta CPAMobility_{s,t-1} + \partial' X_{s,t-1} + \alpha_s + \gamma_t + \varepsilon_{s,t}$, 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 $Log(Wage_{s,j,t}^{SUSB}) = \beta CPAMobility_{s,t-1} \times Small_j + \alpha_{s,j} + \gamma_{s,t} + \gamma_{j,t} + \varepsilon_{p,s,t}$, 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 $Log(Wage_{s,o,t}^{QCEW}) = \beta CPAMobility_{s,t-1} \times CPA_o + \alpha_{s,o} + \gamma_{s,t} + \gamma_{o,t} + \varepsilon_{o,s,t}$, 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. The vertical dashed line indicates the occurrence of the treatment. 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.

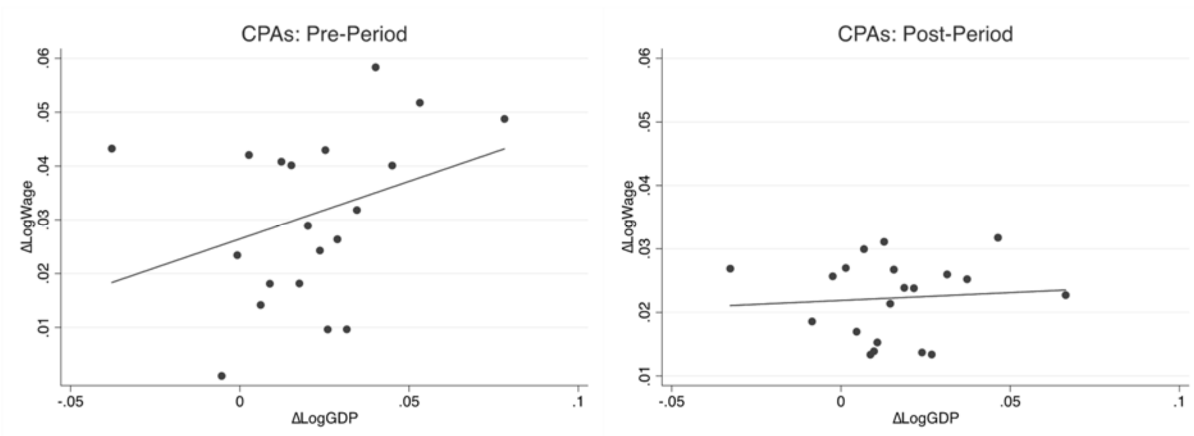
Figure 2: Border Counties with Non-Overlapping Treatment Dates



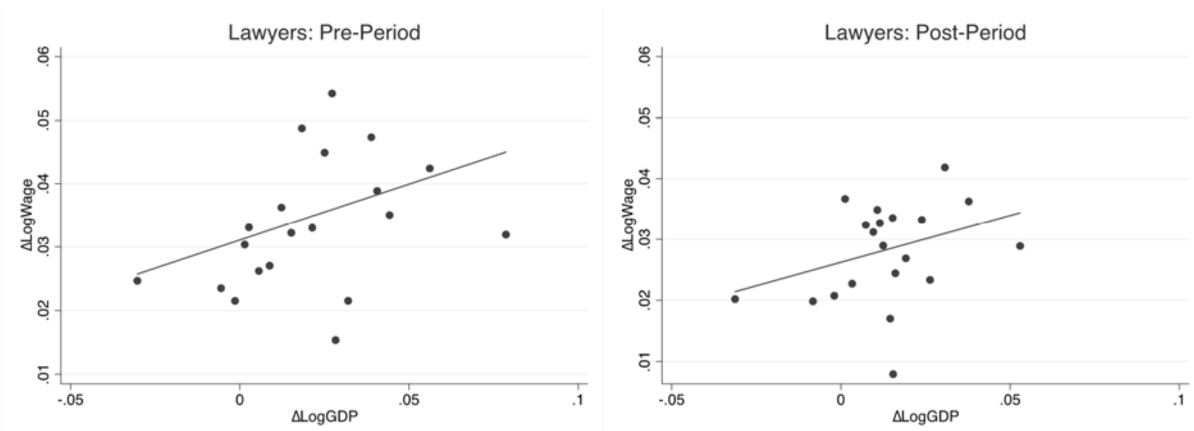
This figure shows contiguous counties located at border segments with non-overlapping treatment dates of the states forming the border segment.

Figure 3: 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: CPA Mobility Adoption Dates 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 (*continued*)

Panel B: Determinants of CPA Mobility Adoption

	Dependent variable: $CPAMobilityAdoption$				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
<u>*CPA Macro Factors:*</u>					
$CPAWageDifferential_s$	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)
<u>*CPA Political Economy:*</u>					
$CPABoardMembers_s$		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)
<u>*General Macro Factors:*</u>					
$Unemployment_{s,t-1}$			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)
<u>*General Political Economy:*</u>					
$SenateDemocrats_{s,t-1}$				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 the Appendix. Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2: State-Level Difference-in-Differences Analysis of CPA Mobility Effects on Wages and Employment

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

	Obs.	Mean	Std. Dev.	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 Effects 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>						
$WithinImmigration_{s,t}$				0.873		0.443
				(1.106)		(1.085)
$AbroadImmigration_{s,i}$					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 2 (*continued*)

Panel C: CPA Mobility Effects 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:						
$WithinImmigration_{s,t}$				1.707		-0.354
				(2.057)		(1.584)
$AbroadImmigration_{s,t}$					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

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 the Appendix and in Section 3 of the Online Appendix, 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 $Log(Wage_{s,t}^{QCEW})$ 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 $Employment_{s,t}^{QCEW}$ divided by the sum of all $Employment_{s,t}^{QCEW}$ 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 $Log(Employment_{s,t}^{QCEW})$ on $CPAMobility_{s,t-1}$ and control variables, as indicated in each column. Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3: Difference-in-Difference-in-Differences Analysis of CPA Mobility Effects on Wages, Employment, and Firms

Panel A: Within-State Control Group - Firm Size

	Dependent variables:			
	$Log(Wage^{SUSB}_{s,j,t})$	$Log(Employment^{SUSB}_{s,j,t})$	$Log(AvgEmployment^{SUSB}_{s,j,t})$	$Log(Firms^{SUSB}_{s,j,t})$
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:			
	$Log(Wage^{QCEW}_{s,o,t})$	$Log(Employment^{QCEW}_{s,o,t})$	$Log(AvgEmployment^{QCEW}_{s,o,t})$	$Log(Firms^{QCEW}_{s,o,t})$
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 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 the Appendix and in Section 3 of the Online Appendix, respectively. We present summary statistics for each control group in Table OA-1 in the Online Appendix. 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 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 the Appendix and in Section 3 of the Online Appendix, 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 5: CPA Mobility and 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)
$\Delta Log\left(GDPperCapita_{m,t}\right)$	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:	
	$\sigma\left(\Delta Log\left(Wage^{QCEW}_{m,o,t}\right)\right)_{o,t}$	$IQR\left(\Delta Log\left(Wage^{QCEW}_{m,o,t}\right)\right)_{o,t}$
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:	
	$\sigma\left(Log\left(Wage^{QCEW}_{m,o,t}\right)\right)_{o,t}$	$IQR\left(Log\left(Wage^{QCEW}_{m,o,t}\right)\right)_{o,t}$
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 the Appendix and in Section 3 of the Online Appendix, respectively. We present summary statistics in Table OA-1, Panel D, in the Online Appendix. 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 $\Delta Log\left(Wage^{QCEW}_{m,t}\right)$ 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 6: Within-CPA Firm Effects on Wages, Billing Rates, and Hours Charged

Panel A: Descriptive Statistics for the AICPA MAP Survey Sample

	Obs.	Mean	Std. Dev.	P1	P25	P50	P75	P99
$Wage_{s,w}^{MAP}$	129	85,039	11,983	63,541	75,767	83,824	92,937	116,007
$Log(Wage_{s,w}^{MAP})$	129	11.341	0.139	11.059	11.235	11.336	11.440	11.661
$WageSenior_{s,w}^{MAP}$	129	173,252	29,374	116,041	154,058	170,241	186,174	250,012
$WageMid_{s,w}^{MAP}$	129	77,994	14,752	52,215	66,114	76,591	87,769	117,835
$WageJunior_{s,w}^{MAP}$	129	47,798	5,637	37,445	43,057	48,233	51,175	61,639
$BillingRate_{s,w}^{MAP}$	129	129	20	92	114	128	141	181
$BillingRateSenior_{s,w}^{MAP}$	129	172	24	130	155	172	185	250
$BillingRateMid_{s,w}^{MAP}$	129	139	27	95	118	137	158	213
$BillingRateJunior_{s,w}^{MAP}$	129	94	13	66	85	95	103	126
$HoursCharged_{s,w}^{MAP}$	129	1,422	64	1,284	1,381	1,421	1,464	1,587
$Log(HoursCharged_{s,w}^{MAP})$	129	7.259	0.045	7.158	7.231	7.259	7.289	7.370
$HoursChargedSenior_{s,w}^{MAP}$	129	1,288	95	1,056	1,228	1,289	1,350	1,484
$HoursChargedMid_{s,w}^{MAP}$	129	1,422	86	1,244	1,377	1,423	1,472	1,621
$HoursChargedJunior_{s,w}^{MAP}$	129	1,491	70	1,329	1,438	1,497	1,541	1,647

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

Table 6 (*continued*)

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

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

Table 6 (*continued*)

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 the results of our analyses based on the *AICPA MAP Survey Sample*. Detailed definitions for all variables and sample selection criteria are presented in the Appendix and in Section 3 of the Online Appendix, respectively. Panel A presents the summary statistics of all variable used in this analysis. In Panel B, we report coefficient 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 (WLS) 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 7: CPA Mobility Effects on Service Prices

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

	Obs.	Mean	Std. Dev.	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 Effects 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)	
$CPAMobility_{a,s,t-1}^{LocalAuditFirm}$		-0.022**
		(0.010)
$CPAMobility_{a,s,t-1}^{NationalAuditFirm}$		-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 the Appendix and in Section 3 of the Online Appendix. 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 $CPAMobility_{s,t-1}^{LocalFirm}$ and $CPAMobility_{s,t-1}^{NationalFirm}$, and control variables, as indicated in each column. We report the p-value from an F-test for the difference between the coefficients on $CPAMobility_{s,t-1}^{LocalFirm}$ and $CPAMobility_{s,t-1}^{NationalFirm}$. Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table 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

Table 8 (*continued*)

Panel C: CPA Mobility and Disciplinary Actions in the State of Colorado

	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 the Appendix and in Section 3 of the Online Appendix. 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 in the state of Colorado on $CPAMobility_{Colorado,i,t-1} \times YoungCPAFirm_i$ and control variables, as indicated in each column. Data on disciplinary action incidence are collected from the Colorado State Board of Accountancy. Standard errors are clustered at the CPA firm level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Online Appendix for

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

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Table of Contents

1. Occupational Licensing 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 origins of the rent-seeking 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] The rent-seeking view has recently gained juridical and legislative traction. For instance, in 2015 the Supreme Court ruled that a state's occupational

[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).

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*).[2] 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.[3] 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.[4] In a similar vein, Alexander Acosta, former U.S. Labor Secretary, and Dennis Daugaard, former Governor of South Dakota, 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 (2020) 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

[2] 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.
[3] U.S. Department of the Treasury Office of Economic Policy, Council of Economic Advisers, and the Department of Labor (2015).
[4] See the National Conference of State Legislators (2017).

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.[5]

2. A Simple Model of Spatial Licensing Requirements

We develop 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"). We then derive the aggregate labor supply and provide comparative statics. Finally, we introduce geographic barriers and derive our predictions.[6] 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 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.

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

Let us now assume an inelastic demand of quantity Y^*.[7] 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 OA-1 in which we show supply curves for two levels of market participation, N and N', with $N < N'$. In Figure OA-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 OA-1, Panel B, we see that the resulting change in wages is larger for a supply assuming N accountants in the market, i.e., $\Delta w^N > \Delta w^{N'}$.

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:

[7] Our predictions about wage responses and supply elasticities are not contingent on the assumption of inelastic demand.

$N = N^{In} + pN^{Out}$.[8] 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 $\max\{w_S - L, 0\} + v_{j,S} > \max\{w_{Out}, 0\} + v_{j,Out}$ 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 OA-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.

[8] 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.

3. Data and Samples

3.1. 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"). The NAICS-based aggregation 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. To gauge the extent to which our QCEW 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.

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 *QCEW State-Level Sample* comprises 720 state-year observations. All variables used in the *QCEW State-Level Sample* are 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.

3.2. 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 aggregation 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 aggregated 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 3.1). 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.

3.3. QCEW Border-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 aggregation allowing for easier identification of the firms or employees, we face such disclosure restrictions in this sample.[9]

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.[10] Thomas Holmes provides 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

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

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

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.

3.4. QCEW 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.

3.5. 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.

3.6. Private Pension Plan Audit Dataset

This dataset is based on private pension plan data we collect from the Employee Benefit Security Administration (EBSA) of the Department of Labor.[11] We first obtain all files 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 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 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.

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

3.7. *AICPA Misconduct Dataset*

This dataset is based on AICPA misconduct cases as identified in Armitage and Moriarity (2016). 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.

3.8. *EBSA Deficient Filer Dataset*

This dataset is based on EBSA Enforcement Data provided by the Department of Labor.[12] The original dataset consists of closed cases that resulted in penalty assessments 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 deficient filers for 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.

3.9. *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 and 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 have entered at least one year prior to Colorado's CPA Mobility adoption. We then merge

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

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.

4. 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 3.6 of the paper.

First, we assess whether Big 4 firms are part of our *QCEW State-Level Sample*. Our *QCEW State-Level Sample* is based on data aggregated by industry. The QCEW program assigns industries based on questionnaires.[13] 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

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

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:

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*.

5. Gauging the Economic Magnitude of CPA Mobility Effects on Wages

To better calibrate the economic magnitude of the 1.0% estimated wage decline that we document in our most conservative model specification (Table 2, Panel B, Column (6) of the paper), we conduct a calibration exercise and compare our estimated effect with the 1.8% ten-year pre-treatment average growth rate in real wages. Thus, the magnitude of the wage decline that we document (i.e., 1.0%) represents more than half of the long-term growth path (i.e., 1.8%), which we interpret as a meaningful economic effect.

Furthermore, we provide an even more intuitive calibration of the magnitude of our documented wage effect by calculating counterfactual "forgone" wages—that is, wages that accounting professionals would have earned absent the policy intervention. To do so, we calculate the ten-year average real wage growth rate for accounting professionals up until 2005—that is, in the period before any of our sample states adopts CPA Mobility provisions—and assume that wages would have continued to follow this long-term growth path (i.e., 1.8% per year). Specifically, we first extrapolate (counterfactual) wages using this assumed wage growth rate over a five-year horizon, we then calculate wage differentials for each year, and finally we discount yearly wage differentials back to 2005. Based on this calibration exercise, whose details are presented in Table OA-2, our point estimate of 1.0% would entail discounted forgone wages over a five-year horizon of around USD 8,290 for the average individual, or USD 3.25 billion when aggregated across all CPA firms.

6. Treatment Effect Stability

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. Specifically, we re-estimate our main model specification (Table 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. We present the results of this analysis in Table 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.

7. CPA Mobility Neighbor 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.[14] In the odd-numbered columns of Table

[14] In untabulated tests, we repeat the analyses presented in Table OA-4 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.

OA-4 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, though only statistically significant at the 10% level when macro and migration controls are included (Columns (3) and (7))). 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.

8. CPA Mobility Effects on Wages and the 150-Hour Rule Adoption

The 150-hour rule effectively harmonized the educational requirements to enter the public accounting profession across the United States. The 150-hour rule played an important role in paving the way for the removal of geographic barriers through the adoption of CPA Mobility provisions as roughly 70% of states adopted it before the start of our sample period. The remaining 30% of states, however, adopted the 150-hour rule during our sample period. Barrios (2019) shows that the 150-hour rule decreased the local supply of new CPAs, likely because candidates chose to abstain from a CPA career to start working earlier. Therefore, a potential concern is that the effect of CPA Mobility that we document could be confounded by the adoption of the 150-hour rule. To investigate this possibility, we conduct a further test in which we augment our main model specification (Table 2, Panel B, Column (6) of the paper) by additionally controlling for the adoption of the 150-hour rule. In Table OA-5, we present a comparison between our base model (Column (1)) and the 150-hour rule augmented model (Column (2)). The augmented model yields a positive, but statistically insignificant, coefficient on the 150-hour rule indicator. Most importantly, the inclusion of the 150-hour rule indicator leads to an estimate of the CPA Mobility effect which is virtually identical to the one of the base model, both in terms of magnitude and statistical significance. The evidence emerging from this additional test provides reassurance that the CPA Mobility wage effect that we document is unlikely affected by the adoption of the 150-hour rule.

9. Within-State Synthetic Control Group

We conduct supplemental DiDiD tests in which 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 states' 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" within that state. 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 a state's CPA Mobility adoption.

In Table OA-7, 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 1.0% to 1.1%. 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.

10. CPA Firm Legal Structure and CPA Mobility Effects by Seniority Rank

There are two potential concerns with our analysis of CPA Mobility effects by seniority rank, which we present in Section 3.6 of the paper: (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 the inclusion of state-level income tax rates affects our results and find that this is not the case (results are untabulated for brevity). We focus on income tax rates since most 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, which reports the number of establishments by industry classification, state, year, legal form, and size class. We find that that less than 10% of all CPA firms are organized as Corporations whose profits are taxed at the company level (untabulated). We graphically show the shares of CPA firm legal structures for different (BLS-defined) size classes in Figure OA-2.

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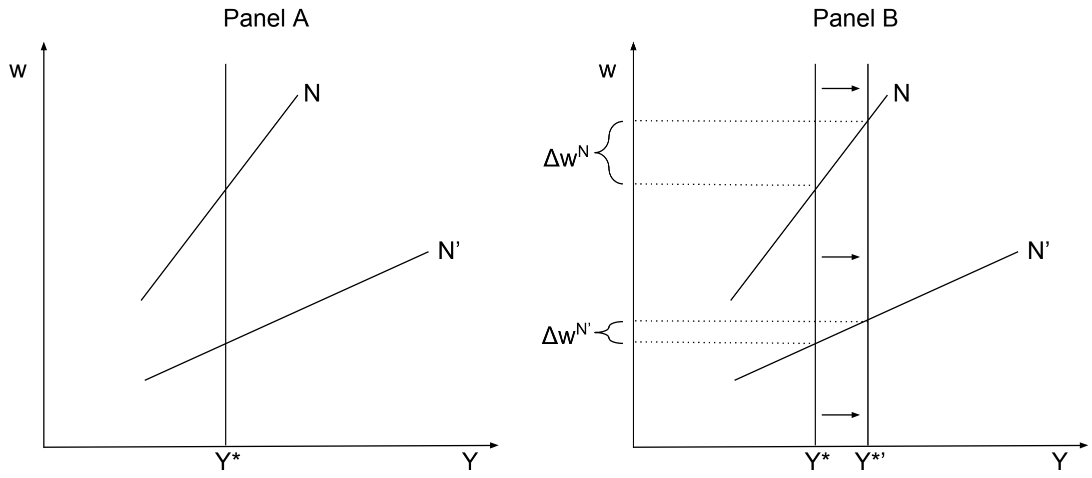
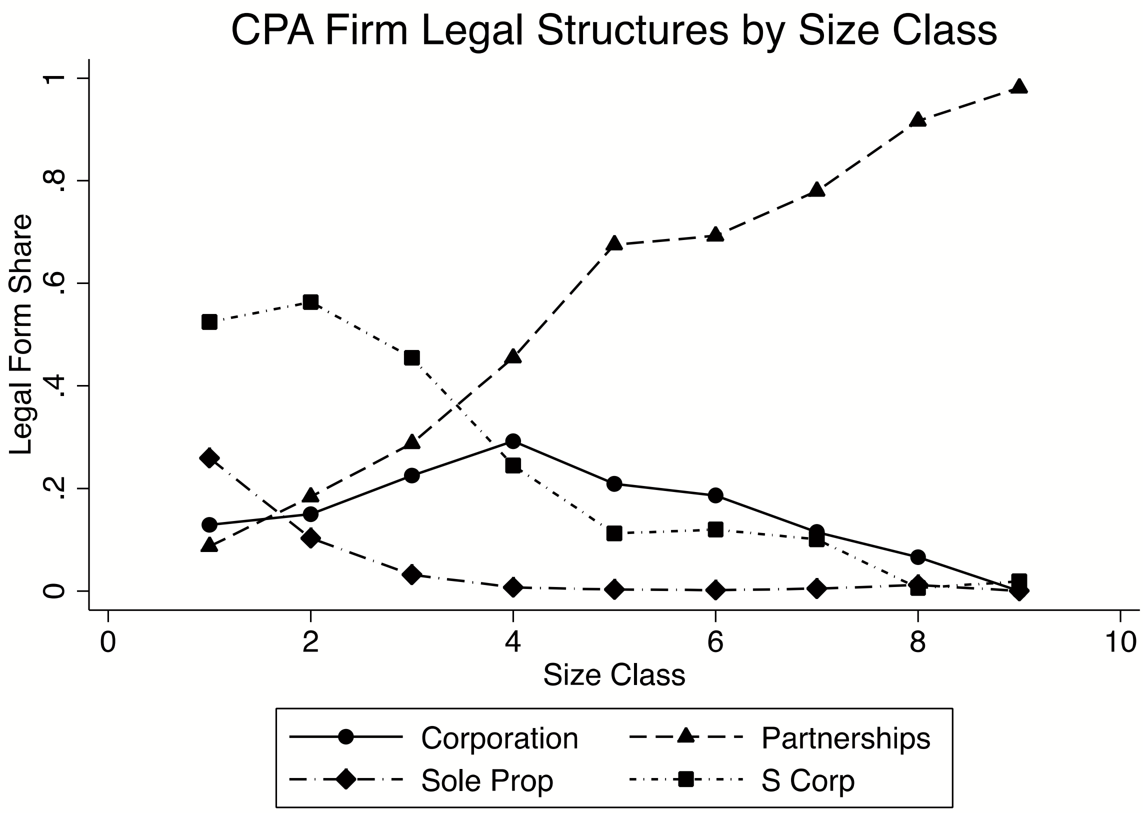
Figure OA-1: Aggregate Supply and Supply Elasticity



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 2. 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 OA-2: 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 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 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 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 Large 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 OA-1 (*continued*)

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

	Obs.	*Mean*	*Std. Dev.*	*P1*	*P25*	*P50*	*P75*	*P99*
				Accounting Professionals				
$Wage_{m,t}^{QCEW}$	1,524	54,411	16,022	27,540	42,816	52,266	63,282	105,115
$Log(Wage_{p,m,t}^{QCEW})$	1,524	10.862	0.289	10.223	10.665	10.864	11.055	11.563
$\Delta Log(Wage_{p,m,t}^{QCEW})$	1,524	0.029	0.050	-0.112	0.006	0.028	0.053	0.149
				Legal Professionals				
$Wage_{p,m,t}^{QCEW}$	1,012	62,708	20,669	27,859	47,366	60,491	74,952	122,153
$Log(Wage_{p,m,t}^{QCEW})$	1,012	10.993	0.331	10.235	10.766	11.010	11.225	11.713
$\Delta Log(Wage_{p,m,t}^{QCEW})$	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 the paper's Appendix and in Section 3 of the Online Appendix, respectively.

Table OA-2: Counterfactual “Forgone” Wages due to CPA Mobility

$Year$	$WageProjection_{untreated}$	$WageProjection_{treated}$	$ForegoneWage$	$ForgoneWage_{2005}$
2006	63,408	62,785	623	597
2007	64,524	63,262	1,262	1,160
2008	65,659	63,743	1,917	1,690
2009	66,815	64,227	2,588	2,188
2010	67,991	64,715	3,276	2,655
			Per person:	**8,290**
			Total:	**3,251,420,889**

This table presents the calculation of counterfactual “forgone” wages in USD and is based on data obtained from the Quarterly Census of Employment and Wages (QCEW). The average wage for CPA firm employees in 2005 is USD 62,311. $WageProjection_{untreated}$ are extrapolated wages from 2005—that is, in the period before any of our sample states adopts CPA Mobility provisions—and assuming that wages would have continued to grow at 1.8% per year (i.e., the ten-year average real growth rate before 2005). $WageProjection_{treated}$ factors in the decline in wage growth of 1.0%, the most conservative estimate of the policy effect, which is based on our baseline specification (Table 2, Column (6) of the paper). $ForegoneWage$ is the difference between $WageProjection_{untreated}$ and $WageProjection_{treated}$, and $ForgoneWage_{2005}$ is $ForegoneWage$ discounted back to 2005 assuming a discount rate of 4.29%, which is the 10-year treasury rate in 2005. To compute the total foregone wages of accounting professionals, we multiply individual forgone wages by the national-level total employment in “Offices of Certified Public Accountants” in 2005 (i.e., 392,193 employees), which we obtain from the Census’ QCEW program data.

Table 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 2, Panel B of the paper) 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 OA-4: CPA Mobility Neighbor Effects

	Dependent variable: $Log(Wage_{s,t}^{QCEW})$							
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 results of the analysis that examines the effect of regional CPA Mobility adoption patterns on wages, which is based on the *QCEW State-Level Sample*. The reported coefficients and (in parentheses) standard errors are obtained from weighted least squares (WLS) regressions of $Log(Wage_{s,t}^{QCEW})$ on $CPAMobility_{s,t-1}$, $NeighborCPAMobility_{s,t-1}$, and control variables, as indicated in each column. $NeighborCPAMobility_{s,t-1}$ is defined as the average treatment variable of neighbors weighted by the number of employees. Detailed definitions for all other variables and sample selection criteria are presented in the paper's Appendix and in Section 3 of the Online Appendix, respectively. Regressions are weighted by state-year employment shares. State-year employment shares are defined as $Employment_{s,t}^{QCEW}$ divided by the sum of all $Employment_{s,t}^{QCEW}$ in year *t*. Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table OA-5: CPA Mobility Effects on Wages and the 150-Hour Rule Adoption

	Dependent variable: $Log(Wage^{QCEW}_{s,t})$	
Independent variables:	(1)	(2)
$CPAMobility_{s,t-1}$	-0.010*	-0.010**
	(0.005)	(0.005)
$150HourRule_{s,t-1}$		0.008
		(0.007)
Macro Controls:		
$Unemployment_{s,t-1}$	0.003	0.002
	(0.002)	(0.002)
$GDPperCapita_{s,t-1}$	0.000**	0.000*
	(0.000)	(0.000)
Migration Controls:		
$WithinImmigration_{s,t-1}$	0.443	0.512
	(1.085)	(1.082)
$AbroadImmigration_{s,t-1}$	0.415	0.228
	(2.989)	(2.885)
State FE	Yes	Yes
Year FE	Yes	Yes
Obs.	720	720
Adj. R^2	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 controlling for adoption of the 150-hour rule, which harmonized the educational requirements to enter the public accounting profession across the United States. $CPAMobility_{s,t-1}$ is defined as in the paper. $150HourRule_{s,t-1}$ is an indicator variable switched on the year the 150-hour rule becomes effective in state s and thereafter, and zero otherwise. Effective dates for each state are defined as in Barrios (2019). The reported coefficients and (in parentheses) standard errors are obtained from weighted least squares (WLS) regressions of $Log(Wage^{QCEW}_{s,t})$ on $CPAMobility_{s,t-1}$, $150HourRule_{s,t-1}$, 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.* Standard errors are clustered at the state level. ***, **, and * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

Table OA-6: CPA Mobility Effects 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**	-0.008**	-0.007*	-0.008**
	(0.004)	(0.004)	(0.004)	(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	0.982	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	0.011	0.012	0.013
	(0.009)	(0.007)	(0.009)	(0.008)
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	0.924	0.923	0.926

Table OA-6 (*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 the paper's Appendix and in Section 3 of the Online Appendix, 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 OA-7: Within-State Synthetic Control Groups

Panel A: Synthetic Control Weights Calculated until 2006

NAICS Code	NAICS Description	Mean Weight	Median Weight
541110	Offices of Lawyers	0.399	0.430
541330	Engineering Services	0.151	0.043
541940	Veterinary Services	0.065	0.040
541511	Custom Computer Programming Services	0.048	0.018
541512	Computer System Design Services	0.023	0.013
541611	Administrative Management and General Management Consulting Services	0.021	0.012
541820	Public Relation Agencies	0.015	0.007
541380	Testing Laboratories	0.013	0.010
541910	Marketing Research and Public Opinion Polling	0.013	0.009
541219	Other Accounting Services	0.013	0.012
541214	Payroll Services	0.013	0.009
541513	Computer Facilities Management Services	0.013	0.008
541213	Tax Preparation Services	0.012	0.008
541310	Architectural Services	0.011	0.011
541613	Marketing Consulting Services	0.011	0.009
541810	Advertising Agencies	0.010	0.010
541890	Other Services related to Marketing	0.010	0.007
541690	Other Scientific and Technical Consulting Services	0.010	0.009
541618	Other Management Consulting Services	0.010	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
541614	Process, Physical Distribution, and Logistics Consulting Services	0.009	0.008
541370	Survey and Mapping 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
541840	Media Representatives	0.008	0.007
541191	Title Abstract and Settlement Offices	0.008	0.008
541860	Direct Mail Advertising	0.007	0.007
541320	Landscape Architectural Services	0.007	0.006
541850	Building Inspection Services	0.007	0.007
541410	Interior Design Services	0.007	0.006
541830	Media Buying Agencies	0.007	0.005
541360	Geophysical Surveying and Mapping Services	0.007	0.006
541350	Building Inspection Services	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
541922	Commercial Photography	0.006	0.005
541870	Advertising Material Distribution Services	0.006	0.006
541930	Translation and Interpretation Services	0.006	0.005
541490	Other Specialized Design Services	0.005	0.005

Table OA-7: (*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	Direct Mail Advertising	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 OA-7 (*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 2006)					
$Wage_{s,t}^{QCEW}$	720	66,117	16,991	39,191	53,737	63,302	74,798	122,351
$Log(Wage_{s,t}^{QCEW})$	720	11.069	0.244	10.576	10.892	11.056	11.223	11.715
$Employment_{s,t}^{QCEW}$	720	13,816	17,247	974	3,108	8,367	16,475	77,662
$Log(Employment_{s,t}^{QCEW})$	720	8.927	1.115	6.881	8.042	9.032	9.710	11.260
			Synthetic CPA 2 (Calculating Weights until State-Specific Treatment Date)					
$Wage_{s,t}^{QCEW}$	720	66,423	17,433	39,168	53,778	63,670	74,752	128,605
$Log(Wage_{s,t}^{QCEW})$	720	11.072	0.247	10.576	10.893	11.061	11.222	11.765
$Employment_{s,t}^{QCEW}$	720	13,981	17,692	938	3,075	7,721	16,351	78,039
$Log(Employment_{s,t}^{QCEW})$	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*	
	$Log(Wage_{s,o,t}^{QCEW})$	$Log(Employment_{s,o,t}^{QCEW})$	$Log(Wage_{s,o,t}^{QCEW})$	$Log(Employment_{s,o,t}^{QCEW})$
Independent variables:	(1)	(2)	(3)	(4)
$CPAMobility_{s,t-1} \times CPA_o$	-0.010**	-0.005	-0.011**	-0.007
	(0.004)	(0.015)	(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 the paper's Appendix and in Section 3 of the Online Appendix, 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 2006, that is, the sample period until 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.