The Impact of Occupational Licensing on Earnings and Employment: Evidence from State-Level Policy Changes

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Abstract

This paper studies the short- and long-run impact of occupational licensing on labor market outcomes in the United States. I compile new data from contemporary and historical legislative documentation that records all state-level policy changes for over 200 licensed occupations. Using this data, I implement an event study design that exploits within-occupation variation in the timing of licensing statutes across states to trace out the dynamic response of earnings and employment to policy changes. I find consistent evidence across several independent employer and household surveys that the typical licensing statute adopted during the past half-century increased worker earnings, but had null or weakly positive effects on employment. Twenty-five years after licensing statutes were adopted, cumulative wage growth in treated state-occupation cells exceeded that of untreated controls by 4 to 7%. Over the same time period, my results rule out an average disemployment effect greater than -5%. The data show much larger decreases in employment, however, among occupations that have little potential to cause serious harm. In cases where the consumer protection rationale for licensing is more plausible, I find simultaneous increases in both earnings and employment following the adoption of licensing requirements.

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1 Introduction

Over 30 million workers in the United States – roughly a fifth of the total labor force – are required to obtain an occupational license before they may legally work or advertise services to the public. The number of jobs covered by state licensing statutes has risen substantially over the past half-century and now includes hundreds of distinct occupations, the majority of which require a license in some, but not all, states.[1] Growing awareness of these discrepancies, together with limited empirical evidence on the costs and benefits of licensing, have made this method of labor market regulation an increasingly prominent public policy issue (Council of Economic Advisers, 2015).[2]

Understanding how occupational licensing impacts earnings and employment in regulated occupations is essential to an evaluation of the trade-offs at the core of ongoing policy debates. On the one hand, licensing may raise barriers to entry, restricting the supply of labor and increasing the market power of incumbent workers (Friedman & Kuznets, 1945; Stigler, 1971). On the other hand, licensing may also increase demand for professional services by resolving informational asymmetries in markets where worker quality is otherwise difficult to observe and consumer health or safety is at risk (Arrow, 1963; Leland, 1979). While both views predict that occupational licensing requirements will increase worker earnings, total employment in regulated occupations could rise or fall depending on the relative importance of these competing supply and demand channels.

Two important challenges, however, have hindered empirical research on occupational licensing. The first is the difficultly of assembling reliable data on the timing of state statutes. As a result, most researchers have either focused on a narrow set of occupations or adopted designs comparing licensed to unlicensed workers at a single point in time.[3] Second, because any number of political or economic factors could plausibly affect both regulation and labor market outcomes, the causal effect of occupational licensing is difficult to discern. For instance, larger or more successful professional associations may be more influential when lobbying state legislators for favorable treatment. Moreover, the growth of licensing over time might correlate with other changes in state-level institutions or economic fundamentals that are difficult to net out without tracking both outcomes and policy changes for a large number of occupations over time.

In this paper I study the short- and long-run impact of occupational licensing on earnings and employment using newly-compiled data on the timing of major regulation events for over 200 unique occupations in all fifty states. This allows me to better exploit the rich cross-sectional and

[1] These occupations include art therapists, dietitians, electricians, interior designers, locksmiths, massage therapists, plumbers, private investigators, radiologic technologists, and security guards, among many others. Occupations that require a license in every state include architects, attorneys, commercial drivers, emergency medical technicians, dentists, insurance agents, nursing home administrators, physicians, public school teachers, and registered nurses.

[2] In addition to the attention the topic has received from the White House, a number of states have recently taken steps toward regulatory reform by increasing recognition of out-of-state licenses, harmonizing credentialing requirements, or deregulating occupations entirely. See DeAntonio et al. (2017) and Kilmer (2019) for a discussion of these reforms.

[3] The latter approach was introduced by Kleiner and Krueger (2010; 2013), who collected the first custom survey data on occupational licensing. Following their seminal work, questions related to licensing and certification have been added to both the Current Population Survey and the Survey of Income and Program Participation. Unfortunately, these questions were introduced too recently to capture meaningful changes in the prevalence of licensing within states and occupations over time, limiting designs using these datasets to cross-sectional comparisons.

longitudinal variation in state laws covering the most economically significant occupations licensed during the past half-century.[4] Importantly, my data are the first to link every policy to both a current statute or regulation, as well as to the full text of the underlying legislation that enacted, amended, or replaced any relevant laws. Relative to existing data on occupational licensing, my approach offers a complete panel of legal changes for many occupations, the ability to differentiate between licensing and weaker methods of occupational regulation, and greater historical accuracy. I match my policy data to several independent employer and household surveys and report findings based on labor market outcomes observed between 1980 and 2018.

My analysis exploits the fact that state legislatures typically pass licensing requirements for the same occupation at very different points in time, if ever. I adopt a difference-in-differences design that uses this staggered treatment timing to contrast changes in earnings and employment within detailed occupation categories across states.[5] Since the impact of licensing may vary over time, I estimate an event study specification that traces out the full cumulative response of outcomes relative to the year of the policy change. As in other difference-in-differences designs, causal identification requires that earnings and employment in licensing states would have followed the trend observed for the same occupation in control states if the licensing requirement had not been enacted. To lend credibility to this assumption, my analysis flexibly controls for alternative methods of occupational regulation, heterogeneity in regional labor markets, and common state-level trends that may be correlated with the adoption of licensing statutes.

I first document that licensing has a clear positive effect on average hourly wages that increases with treatment duration when pooling across all occupations and policy changes. Twenty-five years after the adoption of a licensing requirement, cumulative wage growth in licensing states exceeded that of untreated controls by 5% on average. The magnitude of this intention-to-treat estimate is economically significant and shows that licensing does indeed have a meaningful impact on the labor market. In the long-run, the licensing wage premium is roughly equivalent in size to the wage differential that would be expected as a result of increasing the average educational attainment of all workers in the occupation by six months (Card, 1999). It is considerably smaller, however, than the 15% wage premium commonly attributed to union membership (Hirsch, 2004). In contrast to its notable long-run effects, licensing has little impact on average earnings in the short-run, most likely due to the grandfathering of incumbent workers (Han & Kleiner, 2017).

Despite experiencing a significant increase in long-run earnings, I find no evidence that employment in the typical licensed occupation fell relative to control states. My results show that adopting a licensing requirement had no effect on the number of workers employed in the short-run, and a weakly positive – but often statistically insignificant – impact in the long-run. After twenty-five years, point estimates range from 2 to 7% across various samples and rule out a long-run decrease

[4] My estimation sample includes the near universe of licensed occupations with a precise definition in the Standard Occupational Classification system. These six-digit codes cover approximately two-thirds of all licensed workers.

[5] Fewer than a third of the occupations I study are currently licensed in all fifty states. Since there are no untreated units for these "universally-licensed" occupations, relative treatment timing is the only source of identifying variation. Variation for the remaining occupations comes from both relative treatment timing and comparisons between licensing and non-licensing states (Goodman-Bacon, 2019).

in employment greater than -5% at conventional levels of statistical significance. I do find some evidence, however, suggesting that licensing may have reduced the share of self-employed workers over time. Nonetheless, the absence of large disemployment effects stands in sharp contrast to the view that the average occupational licensing statute increases earnings primarily through a reduction in the extensive margin of labor supply.

One potential explanation for these findings is that licensing simply induces higher-skilled workers to sort into the occupation without affecting net employment. Observable changes in the stock of human capital and demographic composition of workers, however, explains only a small fraction of the estimated wage premium. Although I find that licensing requirements have the expected effect of raising average educational attainment, this impact is modest and explains at most 15% of the long-run change in wages. Rather, exploring heterogeneity in earnings and employment effects, I find evidence that is more consistent with licensing increasing demand for certain occupations. The data show that when the risk of consumer harm is serious, both earnings and employment increase following the adoption of licensing statutes. Where the consumer protection rationale for licensing is less plausible, the supply channel appears to dominate, increasing earnings, but significantly reducing the number of workers employed in the occupation.

This paper makes several contributions to a growing empirical literature on the labor market effects of occupational licensing. First, my analysis addresses an important trade-off between internal and external validity by analyzing a broad range of licensing policies. For the most part, other studies exploiting quasi-experimental variation in the timing of licensing statutes have focused on specific occupations. These include dental assistants (Xia, 2020), massage therapists (Thornton & Timmons, 2013), midwives (Anderson et al., 2020), nurses (Law & Marks, 2017), and opticians (Timmons & Mills, 2018), as well as a handful of professions licensed during the early twentieth-century (Han & Kleiner, 2017; Law & Kim, 2005; Law & Marks, 2009). While these studies have led to important insights, it is difficult to draw strong conclusions about the average impact of licensing requirements from these policy settings alone. My results, by contrast, provide more general evidence on the labor market effects of occupational licensing and an assessment of heterogeneity in earnings and employment responses across various occupation groups.

Second, I show that occupational licensing consistently raises earnings, despite no apparent decrease in employment for the typical occupation in my sample. Like this paper, a large number of studies have documented a substantial licensing wage premium, often using cross-sectional variation in self-reported license attainment from household survey data (Blair & Chung, 2018; Gittleman et al., 2018; Ingram, 2019; Kleiner & Kreuger, 2013). Comparing otherwise similar workers with and without a license, these studies have estimated wage effects ranging from 4 to 18%, which are larger on average than those I find. Often, this wage differential has been interpreted as indirect evidence that licensing restricts the supply of occupational practitioners (Council of Economic Advisers, 2015). Direct evidence on employment effects, however, is more limited and the literature has yet to reach a consensus on the size or sign of these impacts.

In a recent paper, Kleiner and Soltas (2020) document that licensing increases wages by 15%

while reducing employment by as much as 30% using cross-sectional variation in the share of workers reporting a license by state and occupation. In a related approach, Blair and Chung (2019) also find evidence of disemployment effects on the order of 20 to 30%. In contrast to these studies, I adopt a panel design that tracks earnings and employment over time and measures state-level policy changes directly. The difference in our empirical strategies offers one potential explanation for why my results – especially for employment – differ from those above. Further, my sample includes a number of occupations such as emergency medical technicians that became licensed in every state relatively recently. Estimates based on current data do not use variation from this type of occupation, which could also contribute to the differences in our findings.

The previous work most closely related to my own is Redbird (2017), who also uses state-level policy variation for many occupations to identify the impact of occupational licensing on labor market outcomes. Unlike this study, Redbird's analysis found no effect of licensing on wages and a negative impact on educational attainment. By contrast, I find robust positive effects for both. Like Redbird, I find that the average occupational licensing statute may have had a weakly positive impact on employment, though my point estimates are considerably smaller. Taken together, Redbird interprets her results as evidence that licensing increases labor supply by drawing new workers into the occupation, whereas my findings are more consistent with increases in labor demand offsetting any negative impact of higher entry costs on overall participation. Although our research designs differ on a number of dimensions, the discrepancies in our findings are most likely explained by important differences in our policy data, which I outline in my companion paper, Carollo (2020).

Finally, in addition to providing new evidence on the direct effects of occupational licensing on earnings and employment, my results also contribute to a broader literature studying the role of labor market frictions in the United States. Given the prevalence of occupational licensing requirements across sectors and skill levels, several recent papers have cited licensing as a potential factor contributing to the declining U.S. employment-to-population ratio (Abraham & Kearney, 2020; Austin et al., 2018), as well as falling occupational and geographic mobility (Barrero et al., 2020; Johnson & Kleiner, 2020; Kleiner & Xu, 2020). The macroeconomic implications of occupational licensing, however, remain largely unexplored. While a comprehensive general equilibrium analysis is beyond the scope of this paper, my results suggests that the impact of licensing on aggregate employment may be less pronounced than is commonly assumed.

The rest of this paper is structured as follows. Section 2 provides key details of the institutional background related to my research design and discusses the hypothesized effects of licensing on the labor market in greater detail. Section 3 gives an overview of the data, including how my policy and outcome variables are constructed. Section 4 discusses identification and introduces my empirical specification. Section 5 presents my main findings on earnings and employment. Section 6 assesses potential mechanisms and explores heterogeneous treatment effects. Section 7 concludes.

2 Institutional Background

This section provides an overview of occupational regulation in the United States and discusses the main channels through which licensing may impact the labor market. Occupational regulation is a complex institution that involves multiple levels of government and at least three distinct regulatory approaches. Although this paper focuses on occupational licensing – the most prevalent and restrictive method of regulation – occupations licensed by some states at one point in time may be subject to weaker forms of regulation elsewhere. These policy alternatives generate additional variation in treatment status that I leverage in my empirical analysis. Moreover, because licensing statutes usually contain broad exemptions for incumbent workers, policy changes may have little immediate effect, implying that a long-run analysis is necessary to fully characterize the impact of licensing on employment and wages.

2.1 Occupational Regulation

There are three general methods of occupational regulation that require workers to obtain government approval before they may legally practice or advertise their services to the public: licensing, certification, and registration (Shimberg, 1980).[6] Most occupational licensing and regulation occurs at the state level, though a small number of occupations are regulated by federal agencies, individual municipalities, or multiple levels of government simultaneously (Kleiner & Krueger, 2013). This paper focuses on state and federal policies, which cover the vast majority of licensed jobs and workers.[7] Because states often take different approaches to regulating the same occupation, understanding precisely how these policies differ is necessary for both the coding of my legal variables and ultimately the research design.

Methods of Regulation. Licensing, or formally "right-to-practice," is the most restrictive form of occupational regulation. Policymakers adopting licensing statutes define a set of tasks that fall within the occupation's scope of practice and make performing this type of work without meeting predetermined competency standards a criminal offense.[8] The specific qualifications necessary to obtain a license vary by occupation and state, but applicants are usually required to demonstrate

[6] Occupational licensing is distinct from business licensing, such as a license to operate a dry cleaning shop or restaurant. Whereas a business license attaches to an establishment, an occupational license is a credential that individual workers must obtain to demonstrate that they are legally qualified for their job.

[7] Appendix Table A1 displays summary statistics on the source and characteristics of worker credentials using data from the Survey of Income and Program Participation, the only large household survey that reports detailed information on how workers obtained their credentials. Less than one percent of the population holds a license or certification issued at the local level, and these account for only 2.3% of government-issued credentials overall. Omitting municipal licenses is therefore unlikely to have a significant impact on my results.

[8] Unlicensed practice is punishable by severe fines or even incarceration. While there is little data on enforcement actions, the majority of licensing boards are overseen by members of the regulated occupation itself, and have strong incentives to restrict unlicensed practice (Allensworth, 2017). Licensing boards and professional organizations are also known to actively defend their scope of practice, including from encroachment by other occupations (Kleiner, 2016). In *North Carolina State Board of Dental Examiners v. Federal Trade Commission*, for example, the U.S. Supreme Court ruled that the dental board violated federal antitrust law by attempting to prevent non-dentists from providing teeth whitening services.

a minimum level of education or experience and pass an entry examination. Many licenses require periodic renewal, which may be accompanied by continuing education requirements.

Certification and registration are less restrictive alternatives to licensing. In certification statutes, the government establishes a "right-to-title" for workers who have obtained a credential. Only certified workers may legally use the reserved occupation titles in connection with their practice.[9] Importantly, however, certification does not place limitations on the tasks that uncertified workers may perform, provided that they do not advertise these services using legally-protected terms. For that reason, state certification is sometimes referred to as "voluntary licensure" (Law & Marks, 2013). Finally, registration requirements are the weakest method of occupational regulation, under which workers must only file their name and qualifications with a government agency before beginning their practice. While registration is mandatory, workers are not required to demonstrate a minimum level of competency, though proof of bonding and insurance or criminal background checks may still be required.[10]

Implementation and Grandfathering. When occupational licensing policies are initially implemented, the minimum training and examination requirements they adopt may not apply to incumbent workers. Instead, licensing statutes usually include "grandfathering" provisions, allowing applicants who can demonstrate that they were working in the occupation before the law was approved to qualify for a license under relaxed standards (Han & Kleiner, 2017). Although complete exemptions from competency requirements are rare in modern statutes, licensing examinations are frequently waived if the incumbent otherwise qualifies by education or experience. Similarly, workers who do not meet the new educational standards may be allowed to substitute their work experience or continue practicing conditional on passing the examination. New workers, by contrast, must comply with all requirements of the law as soon as it becomes effective.

Grandfathering provisions imply that licensing requirements will have a greater impact on the flow of entrants than the stock of incumbents in the occupation. Because few workers are immediately forced to exit, changes in total labor supply depend on the entry decisions of new workers and may take several years to detect in aggregate. Likewise, average earnings will reflect the both the composition of workers through the stock of human capital, as well as any changes in the supply and demand for labor over time. As a result, the labor market may take many years to converge to a new steady-state, implying that the causal effect of licensing on earnings and employment may differ significantly in the short- and long-run. My empirical strategy therefore tracks outcomes up to twenty-five years after policy changes and places minimal structure on the evolution of the

[9] Unlike the credentials and educational certificates issued by professional organizations (Deming et al., 2016), state certification is a direct method of labor market regulation with standards set and enforced by a government agency. Title protection statutes can also be quite expansive, limiting the use of unmodified occupation titles such as "accountant," "interior designer," or "psychologist" to workers with a credential approved by state regulators.

[10] Governments may also indirectly regulate occupations through deceptive trade practice acts, limitations on eligibility for public insurance reimbursement, or minor training requirements such as food safety courses that do not require formal government approval to work. Because these methods of regulation are not occupation-specific and are widely considered to be less restrictive than even registration requirements (Hemphill & Carpenter, 2016), I do not consider them in this paper.

treatment effects to account for these stock-flow dynamics.

2.2 Potential Impacts on the Labor Market

As discussed by Kleiner and Soltas (2020), theory unambiguously predicts that earnings will rise following the adoption of licensing statutes, but the total number of workers employed in the occupation may rise or fall depending on the extent to which licensing shifts the relative supply and demand for labor. By mandating that workers demonstrate some minimum level of occupation-specific human capital, licensing requirements increase the cost of entry for new workers, which is expected to decrease labor supply on the extensive margin.[11] In a frictionless labor market with complete information, supply effects dominate, average earnings rise, and incumbents covered by grandfathering provisions benefit from the decrease in competition at the expense of entrants and consumers (Friedman & Kuznets, 1945; Friedman, 1962; Stigler, 1971).

In markets with incomplete information, however, the negative effect of higher barriers to entry on labor supply may be offset by an increase in demand for the occupation's services, especially when consumer health and safety is at risk or employers are liable for harm caused by unqualified workers.[12] By establishing minimum qualifications for professional practice, licensing addresses the problem of asymmetric information by providing a credible signal of worker competency and increasing the average level of human capital in the occupation (Arrow, 1963; Ackerlof, 1970; Leland, 1979). To the extent that consumers are willing to pay for these benefits, rising demand further increases earnings and dampens (or potentially reverses) any disemployment effects.

Even in markets where asymmetric information is not a concern, licensing requirements could impact earnings and employment through various institutional channels. For health care occupations in particular, the growth of licensing over time may reflect the specialization of labor and the emergence of new professions and technologies rather than rent-seeking by incumbent practitioners in established professions (Law & Marks, 2005; Lin, 2011). If the tasks preformed by emerging occupations overlap with the scope of practice reserved for existing occupations, licensing statutes may clarify potential regulatory issues and grant workers legal recognition as a distinct profession (Nunn & Scheffler, 2019). In some cases, licensure may even be a requirement for services to qualify for insurance reimbursement (Roederer, 1980). If this is the case, licensing is expected to increase employment while earnings may rise or fall as new workers enter the occupation.

3 Data

In this section, I document the construction of my legal variables and describe the earnings and employment data used in the analysis. My policy data covers all fifty states and 250 unique occu-

[11] This view assumes that the standards set by regulators are at least as stringent as the minimum qualifications that would otherwise prevail in the market. Naturally, licensing will be less of a barrier to entry if it simply formalizes credentialing requirements that were already demanded by most employers (Redbird, 2017).

[12] As shown in Appendix Figure A1, the prevalence of occupational licensing is strongly correlated with the potential harm from worker mistakes. While individual licensing requirements often appear arbitrary, it is clear that licensed occupations differ significantly from unlicensed occupations on average.

pations, of which 139 can be directly matched to a statistical code in the Standard Occupational Classification system. These occupations cover 180 detailed SOC codes (the near universe of occupations regulated at this level of aggregation), of which, 111 were licensed in fewer than fifty states by 1980 and potentially contribute identifying variation for my empirical analysis.[13] The remaining occupations in the policy data are too small to exploit when estimating treatment effects, but are used to construct the control variables used in certain specifications.

This project is not the first attempting to assemble data on the enactment of occupational licensing policies (Redbird, 2017). My data are unique, however, on two important dimensions. First, I adopt a more detailed coding of specific legal provisions that allows me to differentiate between licensing requirements and less stringent methods of occupational regulation. Second, in addition to linking each policy to a current statute or regulation, I perform a comprehensive search of state session laws to gather the original text of all relevant historical legislation.[14] Because statutes are frequently amended – or even replaced entirely – accurately determining when the regulatory status of a particular occupation changed is extremely difficult without this step.[15] My approach allows me to observe superseded legislation, track exactly how each amendment modified the law, and construct a complete history of policy changes for each state and occupation.

3.1 Occupational Licensing Legislation

I compile data on occupational licensing and regulation from state and federal statutes, regulations, and sessions laws, then use this information to construct a balanced panel that assigns a treatment status to each state, occupation, and year cell between 1950 and 2018. A more in-depth discussion of data collection and validation can be found in my companion paper, Carollo (2020).

Compiling Statutes and Regulations. I first assemble a list of regulated occupations from CareerOneStop, an online database that aggregates information for job-seekers under a grant from the U.S. Department of Labor. I supplement this list with additional policy reports compiled by the Institute for Justice (Carpenter et al., 2017), the National Conference of State Legislatures (2017) and, whenever possible, state agencies and professional organizations.

To classify the method of regulation currently in effect following the definitions introduced in Section 2.1, I preform a search of statutes and administrative regulations for each occupation using

[13]The number of SOC codes covered is larger than the number of unique regulated occupations because some regulatory definitions, such as physicians, cover multiple statistical codes. I currently lack policy data for a small number of occupations that could potentially provide additional variation for my analysis. Together, these cover less than 15% of workers employed in licensed six-digit occupations.

[14]Session laws compile changes to statutes made during each legislative session. They include all new laws as well as amendments to existing code. On the extensive margin that I study, nearly all occupational licensing requirements are specified in statutory law. The small number that were adopted or overturned through administrative or case law are handled on a case-by-case basis with the aid of secondary sources. An entire state-occupation pair is excluded from the sample if the date of relevant policy changes cannot be determined.

[15]For example, in the current version of the Arizona Nurse Practice Act available through the LexisNexis legal database, the earliest historical reference to the licensing of registered nurses is to a law adopted during the 1995 legislative session. In fact, Arizona adopted voluntary certification for registered nurses in 1921 and mandatory licensing in 1952. These early statutes are easily located in Arizona's session laws and agree with the timing documented by secondary sources (Monheit 1982; White, 1983).

the LexisNexis legal database. I gather the complete text of the law and repeat the search for all states to confirm that I capture all jurisdictions regulating the occupation. Next, I hand code variables that describe the scope of the regulation by examining how the occupation is defined, what actions are considered unlawful, and whether the law contains any major exemptions. Specifically, I note whether it is unlawful to perform any tasks without a credential, whether any occupation titles or modifiers are protected, and if the law requires a demonstration of competency, which I define to be any mandatory education, training, experience, or examination requirements.[16]

I track the history of policy changes for each occupation by collecting the original text of relevant session laws from HeinOnline, which provides a fully-digitized collection of laws passed during nearly every legislative session in the history of United States. I search for references to each occupation in these documents, identify the first law (if any) regulating the occupation in every state, and record the same set of variables as above. If the policy enacted by the first law differs from the current method of regulation, I chronologically search subsequent amendments and gather any additional policy changes until I locate the legislation that enacted the current method of regulation. I note both the enactment and statutory effect dates of these laws to produce a sequence of legislative events that characterize how the policy changed over time.

Sample Construction. To link the policy data to outcomes observed in employer and household surveys, I assign each regulated occupation a numerical identifier based on a refinement of the Standard Occupational Classification (SOC) system. The SOC defines over 800 occupation categories covering all civilian jobs and is the most detailed level at which the federal government collects wage and employment data. It is based on a six-digit classification system that groups occupations into major, minor, and detailed categories with similar work content. At the detailed level, these codes are sufficiently precise that the statistical and regulatory definitions of many occupations are nearly equivalent. For occupations that nest entirely within a six-digit code, I create a new eight-digit identifier to preserve the hierarchical structure of the SOC.

I use the underlying event data to create an annual panel recording whether each state-occupation cell is unregulated, licensed, certified, or registered in each year between 1950 and 2018. When an occupation is regulated at both the state and federal level, I assume that the federal law is binding unless the state law imposes more stringent requirements. The length of the regulation panel is chosen to reflect the possibility that licensing has long-run impacts on labor market outcomes. Because events occurring before the beginning of my outcome samples (typically in 1980) may still affect outcomes, assigning treatment and control groups in this setting requires observing each cell's regulatory status at least twenty-five years before the beginning of the outcome window, which is the length of the event window I consider.

[16] For the vast majority of observations, there is a clear distinction between practice and title requirements. In the small number of cases where the restriction is ambiguous, I choose the strongest implied requirement based on my reading of the law.

3.2 Earnings and Employment

Estimating the impact of occupational licensing on the labor market requires data on earnings and employment for detailed occupation categories over a relatively long time horizon. I therefore draw on three distinct datasets, the Current Population Survey, the Census and American Community Survey, and the Occupational Employment Statistics program to construct my estimation samples. Alone, each of these datasets have certain limitations that make it challenging to study the dynamic effects of licensing statutes. I show, however, that despite differences in sample coverage and aggregation, they produce remarkably similar treatment effect estimates. The consistency of my main findings across samples provides strong evidence that data quality concerns specific to particular surveys are not a major source of bias.

Current Population Survey. My primary source of data on earnings and employment are the Current Population Survey (CPS) outgoing rotation group extracts from 1983 to 2018 (Flood et al., 2018). These samples provide information on hourly wages for approximately 150,000 unique workers per year. The CPS is a relatively small survey, but it is the largest dataset where the occupational affiliation of workers is observed annually prior to 1999. Unless otherwise noted, the sample is limited to employed civilian adults between the ages of 16 and 64, excluding unpaid family workers. Wages are measured using the straight-time hourly wage for workers paid by the hour and usual weekly earnings divided by usual weekly hours otherwise. Observations with imputed earnings, hours, or occupation data are omitted from any analysis using these variables.[17] Although the CPS does not collect earnings information for the self-employed, I retain these observations when calculating employment counts.

Occupation codes in the CPS are based on Census classifications, which are revised every decade. To ensure that definitions and coverage remain comparable throughout the sample, I use a slightly aggregated version of the coding system developed by Dorn (2009) that extends the original balanced panel of occupations to incorporate additional revisions implemented after 2010. As described in Appendix B, these aggregations reduce the number of consistently-identifiable occupations from 330 to 310, but mainly affect unregulated production jobs. Data coded using Census classifications prior to 1980 cannot be fully accommodated in this system without substantial loss in detail, preventing the use of earlier data in balanced samples. I construct the main CPS estimation sample using earnings weights to collapse hourly wages to annual averages by state-occupation cell, the level at which treatment is assigned. Annual employment counts and worker characteristics are derived from the full monthly extracts.[18]

[17] As shown in Appendix Figure B1, imputation rates are significantly higher for wages than occupations, and have been increasing over time. A potential concern is that treatment may directly affect the imputation rate through changes in worker non-response. In Appendix Table B3 and Appendix Table B4 I replicate my main estimates with the imputation rate as the outcome variable and show that licensing is not related to changes in the share of imputed observations.

[18] Excluding imputed occupations results in an underestimate of total employment. I adjust for this bias using an approach similar to Cengiz et al. (2019) by first computing employment shares using non-imputed data, then scaling up the shares by total state employment to generate counts.

Census and American Community Survey. Due to its small sample size, a concern when using the CPS data is that some state-occupation cells may be extremely sparse. For that reason, I also use data from the decennial Census from 1980 to 2000 and the American Community Survey from 2001 to 2017 (Ruggles et al., 2019). These datasets are considerably larger, but present a trade-off between a reduction in cross-sectional sampling error on the one hand and potential data frequency issues related to the lack of annual data prior to 2000 on the other. As in the CPS, I limit the sample to employed civilian adults and aggregate the data to a balanced state-occupation panel using the same set of 310 consistent occupational codes discussed above. Although the CPS is my preferred source of data on hourly wages, the ACS-Census has the advantage of also recording total earned income, which is useful to verify that the main results are not driven by the omission of self-employment earnings.

Occupational Employment Statistics. Finally, I use data from the Occupational Employment Statistics (OES) program from 1999 to 2018. The OES is an employer-based survey that reports annual estimates of total employment and hourly wages for over 800 detailed Standard Occupational Classification codes.[19] Surveyed establishments are drawn from the universe of firms covered by state unemployment insurance programs, which make up over 95% of public and private-sector employment, excluding certain agricultural industries and the unincorporated self-employed. Respondents are asked to provide employment counts for their establishment by occupation and wage bracket, which are then used to estimate the distribution of wages by occupation and state. The public-use data are released as three-year moving averages, and are derived from 1.1 million establishment-level surveys covering an average of 57% of all U.S. workers.

The OES data permits an analysis of the effect of regulation on labor market outcomes using much finer occupation categories and larger underlying sample sizes than are currently available in any household survey. Further, because the data are based on employer job classifications rather than an individual's self-reported occupation, measurement error resulting from the misclassification of occupations is less of a concern (Abraham & Spletzer, 2010). However, because the data are not available prior to 1999, fewer policy changes provide identifying variation and the relatively short outcome window makes it more difficult to interpret long-run treatment effects, which I show are crucial for understanding the full impact of occupational licensing on the labor market.

4 Research Design

My research design exploits the rich policy variation resulting from differences in the timing of regulatory changes within occupations across states. To estimate the impact of occupational licensing on long-run labor market outcomes, I adopt an event study framework, which generalizes the

[19] I harmonize revisions to the Standard Occupational Classification system adopted in 2010 by aggregating a small number of occupations that were split or combined using a crosswalk described in Appendix B. This results in a balanced set of 788 six-digit occupation codes with consistent longitudinal coverage.

canonical difference-in-differences estimator to a setting with time-varying treatment effects.[20] This specification traces out the cumulative response of earnings and employment to licensing events within narrowly-defined state-by-occupation cells relative to changes for the same occupation in states that never licensed the occupation or adopted licensure at a different point in time.

Because licensing statutes do not necessarily apply to all workers within a statistical occupation code, my results should be interpreted as intention-to-treat effects that measure changes in outcomes at the cell level in response to an increase in licensing coverage. Further, since my design compares outcomes within occupation categories over time, occupations with no variation in treatment timing or policy differences during the sample window do not provide identifying variation. My results are therefore not applicable to occupations such as dental hygienists, physicians, or hairdressers that were already licensed in all states prior to 1980. Appendix Table A2 displays summary statistics for a subset of the licensed occupations that provide identifying variation in my analysis.

4.1 Identification

The underlying variation in my data is illustrated by Figure 1, which plots the date and type of regulation events for a subset of fifty licensed occupations. Importantly, initial licensing dates for the same occupation often differ by decades across states. This implies that it is feasible to estimate relatively long-run treatment effects by comparing early to late adopters, even for the minority of occupations that are eventually licensed in all states. The figure also highlights the importance of controlling for alternative approaches to regulation. At any point in time, an occupation may be licensed by some states, but certified or registered in others and each state-occupation pair may experience multiple changes to its regulation status during the sample window. Many current licensing statutes were preceded by certification or registration requirements, and a small number of regulations were repealed or overturned in court.[21]

Identifying the causal effect of licensing on earnings and employment requires that within-occupation variation in the timing of policy changes is exogenous to potential outcomes. Because licensing is a policy choice made by state governments, however, the adoption of regulations is unlikely to be completely random (Law & Kim, 2005; Stigler, 1971). To assess possible threats to identification, I analyze the political and economic factors associated with the timing of licensing policies in an event history analysis. Table 1 displays the results of a conditional logistic regression

[20] The canonical difference-in-differences estimator simply contrasts pre- and post-treatment periods for each unit, which requires the assumption that treatment effects are realized immediately and remain constant over time. As discussed in Section 2.1, this assumption is unlikely to hold in my setting. In staggered treatment designs with time-varying treatment effects, difference-in-differences estimates confound long-treated observations for treated units with untreated controls, potentially resulting in biased estimates. In some cases, the bias can be severe enough that point estimates may be negative even when the underlying treatment effects are uniformly positive (de Chaisemartin & D'Haultfoeuille, 2020; Goodman-Bacon, 2019; Borusyak & Jaravel, 2018).

[21] Few units, however, experience more than two events, and the maximum observed in the data is four. Alabama adopted a state certification policy for interior designers in 1982, which was replaced with a licensing requirement in 2001. In 2004 the practice act was declared unconstitutional by a state court. After the ruling was upheld by the Alabama Supreme Court in 2007, the legislature adopted a revised certification act in 2010 (Thornton & Timmons, 2015).

with time-varying hazard rates for each occupation. Unsurprisingly, the partisan composition of the state government is strongly associated with the timing of policy changes, though there is also evidence that policies are adopted earlier in states with lower aggregate employment-to-population ratios and possibly less union coverage. The average wage and total employment in the three-digit occupation code do not appear to influence the timing of legislation.

The correlation between the adoption of licensing policies and time-varying state characteristics raises concerns that states with a greater propensity to regulate occupations may differ along other unobserved dimensions as well. Fortunately, because I study many regulated occupations (and not all occupations are regulated) my design is able to flexibly control for these potential confounds. In addition to the standard two-way unit and time fixed effects, my preferred empirical specification also introduces state-year fixed effects that vary across six major occupation groups. These terms absorb any aggregate labor market shocks or institutional differences that have a common effect on all occupations. Allowing the fixed effects to vary by major occupation group further nets out any state-specific trends affecting similar licensed and unlicensed occupations – such as the polarization of local labor markets (Autor & Dorn, 2013) – that may have occurred contemporaneously with the expansion of licensing requirements.

4.2 Treatment Measures

Earnings and employment are observed at different levels of occupational aggregation in my samples, which may or may not coincide with the coverage of licensing statutes. I therefore construct treatment measures first at the six-digit level, then use employment weights to map these variables to three-digit Census codes.

Treatment Events. The policy changes that provide identifying variation in this paper occur at the level of a state s and a detailed six-digit occupation code k. In each calendar year t, a state-occupation cell is either unregulated, certified, licensed, or potentially subject to a weaker form of regulation such as a registration requirement. Treatment events are identified by a set of indicators e^l_{kst} which are equal to one if unit ks experiences a policy change of type l in year t and zero otherwise. Although the adoption of licensing regulations are the main events of interest in this paper, I control for other policy changes throughout the analysis.

To increase statistical power, I treat all licensing events as equivalent, regardless of whether or not weaker regulations were already in place at the time the law was passed. Repeals, while uncommon, are modeled as a separate type of event to avoid placing any symmetry restrictions on the effect of adopting and repealing a policy. My baseline specification therefore includes four types of events: certification, licensing, repeal, and a single residual category capturing all other occupation-specific regulations. The treatment variables are constructed from these event indicators by summing them over time,

$$d^l_{kst} = \sum_{r \leq t} e^l_{ksr}. \tag{1}$$

For the vast majority of state-occupation pairs that experience at most a single policy change of each type, d^l_{kst} is equivalent to the familiar post-treatment indicator of the canonical difference-in-differences specification. As in Autor et al. (2006), however, the indicators for multiple polices may be on at the same time if the unit has experienced more than one treatment event by time t. Since repeals are captured by a separate variable, all treatment sequences are non-decreasing, which implies that d^l_{kst} may be greater than one if a policy is adopted, repealed, and later re-adopted. In practice, these cases are extremely rare, and although I include them in the sample, dropping these cells has no effect on the results.

Aggregation. With the exception of the Occupational Employment Statistics sample, outcomes are observed only for occupation categories defined by the Census, which are typically less detailed than the six-digit level where treatment is assigned. Census occupational classifications may therefore contain both regulated and unregulated suboccupations, as well as units whose treatment timing differs. However, because the 310 consistent occupation codes used in this project are direct aggregations of the Standard Occupational Classification, it is possible to estimate the share of each three-digit code j covered by its six-digit components k using the OES data. These estimates can then be used to construct an employment-weighed average of d^l_{kst},

$$D^l_{jst} = \sum_k \hat{\pi}_{jk} d^l_{kst}, \tag{2}$$

where $\hat{\pi}_{jk}$ is an estimate of the share of j's total national employment in suboccupation k as of 2000, the approximate midpoint of the samples using these aggregated codes.[22]

Equation 2 defines the treatment variables used in the main analysis, which map policy changes for detailed occupations to the balanced occupational classification system used in the CPS and ACS-Census samples. Although the identifying variation is limited to events that affect an entire six-digit occupation code, at this level of aggregation, these units may be grouped together with other detailed codes containing regulations that are even more narrowly defined (Kleiner & Soltas, 2020). Since it is not possible to assign employment share weights to regulations below the six-digit level, I control for them instead using a separate set of variables that count the number of these "minor" policies by three-digit occupation. I include in this set any six-digit occupations that cover less than 20% of the corresponding three-digit category to avoid identifying from events that have very little bite at the level outcomes are observed.[23]

[22]The Occupational Employment Statistics program does not report employment data for all six-digit occupations prior to 2004, so employment for some occupations must be estimated. To do this, I use data from 2004 to 2016, where the panel is fully balanced, and log-linearly interpolate the missing employment data prior to calculating shares. I use national estimates due to concerns about the endogenous effect of regulation on employment at the state level. A time-invariant measure is also preferred so the weights do not vary with treatment timing. All within-occupation variation in Equation 2 therefore comes from policy changes rather than differences in employment shares across states or over time.

[23]A small number of occupations do not cover an entire six-digit code, but reasonably partition one, for example, water and wastewater treatment plant operators or school and public librarians. I assign these suboccupations a weight of 50% prior to aggregation rather than exclude them. Appendix Figure B2 plots the final distribution of aggregation weights $\hat{\pi}_{jk}$ for the regulated six-digit occupations included in the data. In Section 5.4 I show that the

4.3 Event Study Specifications

My baseline empirical specification takes the standard two-way state and time fixed effects design that would be appropriate for a case study of a single occupation and stacks across all occupations in the data to estimate the average effect of occupational licensing statutes on labor market outcomes. Specifically, I estimate a distributed lag regression of the form,

$$Y_{jst} = \alpha_{js} + \sum_{l \in \mathcal{L}} \sum_{\tau \in \mathcal{T}} \gamma_{\tau}^{l} D_{js,t-\tau}^{l} + X_{jst}' \Lambda + \Omega_{jst} + \delta_{jt} + \epsilon_{jst} \tag{3}$$

where Y_{jst} denotes an outcome for occupation j in state s at time t. The unit fixed effects α_{js} absorb initial level differences in occupation-specific outcomes across states, and the time fixed effects δ_{jt} control for occupation-specific time trends common to all states. The vector X_{jst} includes additional controls that vary by state and occupation, and Ω_{jst} denotes an additional set of distributed lags that count the number of minor regulations in effect by three-digit occupation.

The key explanatory variables are a set of leads and lags of the treatment variables $D_{js,t-\tau}^{l}$ (the inner summation) which span an event window $[\underline{\tau}, \overline{\tau}]$. The coefficients on these variables identify the contemporaneous effect of a policy change occurring τ years relative to time t. I include a full set of these indicators for each type of policy change (the outer summation), which is a specification similar to the multiple event study design studied by Sandler and Sandler (2014). The coefficients of interest are event study estimates $\hat{\beta}_{\tau}^{l}$, which trace out the cumulative response of the outcome to the policy over time. These are recovered by taking the running sum of the contemporaneous policy responses,

$$\hat{\beta}_{\tau}^{l} = \sum_{\rho \leq \tau} \hat{\gamma}_{\rho}^{l} \tag{4}$$

which I normalize by the period prior to treatment. Recovering the event study coefficients from a distributed lag model in this way produces point estimates and standard errors that are numerically identical to estimating the event study directly with a sequence of relative time indicators (Schmidheiny & Siegloch, 2020). The distributed lag model is preferred in my setting because it is less cumbersome to implement with non-binary treatment variables and recurring events.[24] Given the small sample sizes in some surveys, I also report simple averages of the event study coefficients along the lines of the estimation strategy proposed by Borusyak and Jaravel (2018).

My baseline specification implicitly allows any states not regulating an occupation at time t to contribute toward the estimation of counterfactuals for that occupation. This is undesirable if labor market trends display spatial heterogeneity (Dube et al., 2010). Moreover, the results presented in Section 4.1 suggest that certain time-varying state characteristics may be correlated with the adoption of licensing policies. To guard against potential bias from these sources, my preferred

main results are similar when using alternative thresholds or methods of aggregation.

[24] Appendix Figure A2 provides a graphical illustration of the relationship between contemporaneous and cumulative treatment effects in a setting where units experience multiple treatment events.

empirical specification allows the occupation-year fixed effects to vary by census division d and introduces state-year fixed effects for six broad occupation categories g,

$$Y_{jst} = \alpha_{js} + \sum_{l \in \mathcal{L}} \sum_{\tau \in \mathcal{T}} \gamma_\tau^l D_{js,t-\tau}^l + X_{jst}'\Lambda + \Omega_{jst} + \delta_{djt} + \phi_{gst} + \epsilon_{jst}. \tag{5}$$

The fixed effects δ_{djt} address concerns related to spatial heterogeneity by limiting comparisons to the same occupation in geographically proximate states. The ϕ_{gst} terms further control for any state-level trends common to similar groups of occupations regardless of their regulation status, as in a triple-differences design.

Event Window Trimming. I estimate event study coefficients over an event window beginning 10 years prior to treatment and ending 25 years after treatment. Treated units are dropped after 25 years have elapsed without an event to prevent long-treated units from re-entering the sample as controls. This is because once licensed, regulation may increase on the intensive margin (i.e. more stringent entry requirements or changes to scope of practice), making long-treated units poor controls for recent adopters. I keep all observations prior to treatment but interact the unit fixed effects with an event window indicator. This allows units that are eventually treated to serve as controls more than 10 years before their treatment date, but excludes these distant periods from estimation of the unit fixed effects, which may bias estimates if the parallel trends assumption is violated in years long before the event occurs (Autor et al., 2006).

Weighting and Standard Errors. In regressions using state-occupation aggregated data, I weight observations by cell-level employment counts. Weights are derived from the person-level survey weights, and are computed to correspond to the universe of the outcome variable, i.e. wage and salary workers in the CPS wage regressions and all workers including the self-employed for employment regressions. Unless otherwise noted, standard errors are clustered at the state level to allow for arbitrary correlation in the error terms both within state-by-occupation cells over time and between different occupations in the same state.

5 Main Results

In this section, I study the average impact of occupational licensing statutes on earnings and employment using data from three independent labor force surveys. These estimates measure the change in outcomes within state-occupation cells in response to the adoption of licensing legislation for six-digit occupations with state-level policy differences during the sample. I find consistent evidence that licensing increases average hourly wages by 4-7% after being in effect for twenty-five years, but no evidence of a contemporaneous decline in employment. The data rule out long-run disemployment effects greater than 5% and reject a pure regulatory capture interpretation of licensing for the typical occupation in my sample.

5.1 Evidence from the Occupational Employment Statistics

I begin with a graphical preview of my main findings by estimating the baseline two-way fixed effect event study specification using data from the Occupational Employment Statistics sample. Unlike the other datasets used in this paper, the OES reports average hourly wages and employment counts by detailed six-digit occupation, so these initial estimates are free from any potential biases resulting from the aggregation of occupation codes. Moreover, because the data are constructed from large underlying samples and are based on employer occupational classifications rather than worker self-response, the outcome variables are less susceptible to measurement error than those obtained from household surveys. Although there are limitations to using the OES data alone – in particular the short panel and lack of data for the self-employed – the remaining analysis is largely consistent with the pattern shown in these figures.[25]

Figure 2 displays estimates from the baseline two-way fixed effect specification, where all coefficients are expressed relative to the year prior to treatment. Panel A shows that licensing has a clear positive impact on average hourly wages. As expected given that licensing requirements are not immediately binding for all workers, the wage premium appears only gradually and is not statistically significant at the 5% level for more than a decade after the policy change. By contrast, there is no evidence of differential wage trends prior to policy adoption. Panel B displays estimates of the impact of licensing on employment. Unlike for wages, there is some evidence that employment in treated states was already declining prior to regulation. After treatment, however, there is no evidence that the number of workers employed in the occupation fell. If anything, the declining trend observed prior to the treatment date reverses, though the standard errors are too large to rule out a uniform null effect on employment during the outcome window.

Plotting the full set of event study coefficients is useful to visually assess treatment dynamics, but the point estimates are imprecise, particularly for longer leads and lags. To increase statistical precision and summarize the estimates, I also report simple averages of the coefficients and their corresponding standard errors. This washes out higher frequency variation in the cumulative response function to focus on longer-run dynamics. Taken at face value, the long-run estimate for the impact of licensing on wages is 4.4 log points (s.e. 1.5). This is an economically significant effect that is roughly equivalent in magnitude to increasing the average educational attainment of all workers in the cell by six months (Card, 1999). My intention-to-treat effects are smaller, however, than most estimates of the licensing wage premium reported in the literature, which typically fall between 4 and 10 percent when comparing licensed to unlicensed workers in cross-sectional survey data (Ingram, 2019; Gittleman et al., 2018).

[25] Long-run treatment effects in this sample are identified only through a comparison of units treated before the beginning of the sample to late adopters and never-treated controls. This raises concerns that the estimated cumulative response function may reflect the changing composition of events rather than the average dynamic treatment effect. Appendix Figure A6 reports the results of an alternative specification that matches treated units to untreated controls and balances the sample in event time (Cengiz et al., 2019). This design fixes the composition of events so the full cumulative response function is identified from the same set of units. While balancing the sample prevents the estimation of long-run effects, short and medium-run estimates are similar in the balanced and unbalanced samples.

5.2 Impact of Licensing on Earnings

My main estimates use data from the Current Population Survey from 1983 to 2018, which roughly doubles the length of the panel relative to the Occupational Employment Statistics sample. This enhances the credibility of my long-run estimates and allows for a richer set of controls, but comes at the cost of less detailed occupation codes and smaller underlying sample sizes. To ensure that a sufficient number of observations are used to estimate each coefficient, I replace the annual leads and lags of the treatment variables with three-year leads and lags, then report nine-year averages of the binned event study coefficients, as described above. Despite major differences in the samples, I find that the CPS estimates are remarkably similar to those obtained from the OES, providing strong evidence that licensing has a meaningful and plausibly causal impact on earnings.

Baseline Estimates. Table 2 presents my main earnings results in specifications with progressive policy and spatial controls. Column one reports a specification that includes leads and lags of licensing policies only. In this specification, states adopting certification, registration, or minor regulations form part of the control group, along with states that do not regulate the occupation at all. Introducing policy controls in column two increases the magnitude of the medium and long-run estimates somewhat, implying that weaker methods of regulation also have some impact on worker outcomes. More importantly, the estimates in column two are extremely similar to those found in the equivalent regression reported in Figure 2, with an estimated long-run wage premium of 4.2% (s.e. 2.2%). This is reassuring and shows that the aggregation of treatment to the three-digit level is unlikely to be a major source of bias. Column three adds state-year fixed effects specific to six broad occupational categories, with little impact on the estimates.

Columns four to six add separate occupation by year fixed effects for each of the nine census divisions. These sweep out variation between divisions and restrict the identifying variation to differential timing in policy adoption among a set of geographically proximate states. If the adoption of licensing polices are correlated with unobserved spatial heterogeneity, these comparisons form better control groups than allowing any untreated state to form part of the counterfactual (Allegretto et al., 2017). Comparing outcomes within census divisions increases the estimated impact of licensing on wages over time, but the underlying pattern of dynamics is qualitatively similar. As in the baseline specification, controlling for common time effects by state and occupation group has no effect on the estimates. Column six, which is my preferred specification, implies a long-run wage premium of 6.9% (s.e. 2.1%) due to licensing.

Across specifications, there is clear evidence that licensing increases the average hourly wages of workers in regulated occupations. Although they are never statistically significant, the pooled pre-treatment effects are uniformly positive in these regressions, and roughly comparable in magnitude to the short-run treatment effects. Since the pre-treatment effects are also expressed relative to the leave-out period one to three years prior to treatment, these estimates imply that wages were *declining* slightly on average before licensing statutes were adopted. The consistency of this finding across increasingly saturated specifications could be evidence that policy changes are anticipated, which is plausible given that legislation may be introduced in several legislative sessions before it is ultimately passed. Appendix Figure A3 plots the full set of event study estimates for each specification and shows that any potential decrease in wages before licensing occurs is quickly reversed in the post-treatment period.

Alternative Earnings Measures. One concern when estimating the impact of licensing on earnings using data from either the CPS or the OES is that neither include workers who are self-employed. If the impact of licensing on self-employment income differs from its effect on wage and salary income, or if licensing selectively shifts workers into or out of self-employment, the earnings estimates presented so far may be biased.[26] For that reason, I also report earnings estimates using data from Census and American Community Survey, which includes measures of wage and salary income, as well as total earned income from all sources. While this is an advantage of the Census data, measuring hourly compensation is less straightforward than in the CPS. Specifically, hourly wages (and earnings) must be imputed based on annual income, weeks worked at all jobs in the previous year, and usual weekly hours. Because this introduces greater potential for measurement error, I report estimates for the impact of licensing on both hourly and annual income, through the latter also captures changes in the intensive margin of labor supply.

Table 3 reports the impact of licensing on the Census earnings variables. All regressions include separate occupation by year fixed effects for each census division, with my preferred triple-difference specification reported in the even-numbered columns. Overall, the Census results show the same pattern of treatment dynamics as found above, though the estimates are more comparable in magnitude to those obtained from the baseline two-way fixed effect specification in the CPS and OES samples. Importantly, however, within the Census sample, the effect of licensing on earnings is similar regardless of whether or not business income is measured. I find a long-run impact of licensing on annual wage and salary income of 4.3% (s.e. 2.0%) compared to an impact on total earned income of 4.4% (s.e. 2.1%). This results alleviates concern that that failure to measure self-employment income biases the results of the previous section, and provides further evidence that licensing has a positive impact on earnings in another independent sample of workers.

Robustness to Sample Definitions. Figure 3 displays the results of a number of robustness checks for my preferred CPS earnings estimates. Specification one drops public-sector jobs, which I define to be any occupation where 50% or more of workers are employed by the local, state, or federal government. In practice, the largest licensed occupations excluded from the sample under this definition are law enforcement officers, water and wastewater treatment plant operators, and speech-language pathologists (most of whom are employed in public schools). The second specification includes additional controls for the number of contiguous states licensing the same occupation. This is potentially a concern if licensing has geographic spillovers or the presence of

[26] Appendix Figure A4 plots event study estimates for the share of workers who are self-employed in the CPS. Although the estimates are not statistically significant in my preferred specification, there is some evidence licensing has a negative effect on self-employment as large as 10-20% relative to the mean self-employment rate in licensed occupations.

nearby unregulated markets affects the return to licensing. The third specification drops 1994 and 1995 from the sample due to a lack of reliable wage imputation flags. Specification four drops policies that were repealed and later re-enacted. Specification five allows long-treated units to re-enter the sample as controls and specification six reports estimates that are not employment-weighted. The remaining rows drop each of the nine census divisions separately.

None of these tests raise serious concerns about the robustness of the wage estimates, though allowing units that have been licensed for twenty-five years or more to serve as controls lowers the estimated wage premium somewhat and results in a statistically significant pre-treatment effect. This finding is consistent with graphical evidence shown in Appendix Figure A3, which implies that wage growth may not have returned to trend in licensing states by the end of the event window. If wages continue growing in early-adopting states more than twenty-five years after treatment, including these observations in the set of potential controls biases estimates of the counterfactual trends for late-treated units upward, resulting in smaller estimated treatment effects overall. Even under the assumption that these comparisons are valid, the results show an economically significant increase in average hourly wages following the adoption of licensing statutes.

5.3 Impact of Licensing on Employment

Turning now to my main results on employment, I again begin with data from the Current Population Survey. I then replicate my findings in the Census and American Community Survey to address concerns that small sample sizes are driving the results. In both datasets I find that the impact of licensing on employment is weakly positive, implying that the licensing wage premium is not driven by a reduction in the extensive margin of labor supply.

Baseline Estimates. Table 4 presents my main employment results and shows that licensing has a uniformly positive impact on the number of workers employed in the occupation as a share of the state's total population. Comparing the estimates in column two to the equivalent Occupational Employment Statistics specification, I find a larger positive effect of licensing on employment in the CPS that is statistically significant at the 1% level in the medium to long-run. This specification implies that employment per capita increased by 7% (s.e. 2.5%) on average two decades after the occupation became licensed. Restricting the identifying variation to comparisons within census division has little impact on this finding. The magnitude of my two-way fixed effect estimates are comparable to those reported by Redbird (2017), who found that licensing increases an occupation's share of total state labor hours between 5% and 10% after twenty years.

In contrast to my findings for earnings, however, netting out common state by occupation time trends reduces the magnitude of the employment effects substantially and renders them statistically insignificant in both the baseline and census division specifications. A significant share of the increase in occupational employment observed following the adoption of licensing statutes therefore appears to have been driven by broad changes in the composition of the state's labor market rather than the causal effect of occupational licensing. Nonetheless, even in the most saturated specification, there is no evidence of large disemployment effects in either the short or long-run.[27] My preferred specification in column six implies a moderate increase in employment of 4.4% (s.e. 3.4%) after twenty years, ruling out a long-run decrease in employment of more than 2.5% at conventional levels of statistical significance.

ACS-Census Replication. The small sample size of the CPS is potentially a concern when estimating employment changes, as many state by occupation cells contain few observations. I therefore replicate my employment analysis using data from the Census and American Community Survey, where such sparsity is less of an issue. The results are shown in Appendix Table A3 and are extremely similar to my preferred CPS estimates. As above, the point estimates are uniformly positive and statistically significant in the two-way fixed effect specifications, but substantially smaller and insignificant in the triple-difference specifications. In the Census sample, my preferred specification rules out a long-run decrease in employment greater than 5.4%. Interestingly, the Census results also display a decrease in employment *before* licensing takes effects of 2-4%, which is comparable to the pattern observed in the Occupational Employment Statistics.

Robustness to Sample Definitions. Figure 4 displays sample definition robustness checks for my preferred CPS employment results. These are equivalent to the alternative specifications estimated for earnings, with the exception of specification three, which estimates employment effects in log levels rather than log shares. As with the wage results, assumptions about the validity of long-treated units as potential controls appear to be the most consequential for the estimates. Allowing units licensed more than twenty-five years to serve as controls for late adopters results in a negative, but statistically insignificant employment effect of about -6%. As discussed above, this estimate is only valid under the relatively strong assumption that treatment effects have been fully realized after twenty-five years, which Appendix Figure A5 suggests is not the case. By contrast, dropping either of two census divisions – East North Central or East South Central – results in positive effects that are marginally significant at the 95% level.

5.4 Robustness to Treatment Aggregation

Appendix Table A4 and Appendix Table A5 show the robustness of my preferred triple-difference results to alternative methods of aggregating the treatment variables from the six-digit to three-digit level. In addition to measuring sub-occupation employment shares in 2000, I also report measures that use 1980 and 2015 as the year of measurement. Because the Occupational Employment Statistics data is only available beginning in 1999, I log-linearly interpolate 1980 employment shares based on observed employment trends from 1999 to 2018. The 2015 shares use data directly from the OES without additional adjustments. For each of these three samples, I also report estimates that use

[27] Appendix Figure A5 displays the full set of underlying event study coefficients. In the census divisions specifications, there is a small dip in employment immediately following the enactment date of licensing. Given the imprecision of the estimates, it is difficult to interpret this effect, but it could reflect some incumbents who do not meet grandfathering criteria being forced out of the occupation in the short-run.

all six-digit events as well as specifications excluding suboccupations covering less than 20% or 50% of their three-digit superset.[28] Throughout these tables, the underlying pattern and magnitude of the effects are similar to my baseline estimates.

6 Heterogeneity and Mechanisms

The baseline estimates discussed in the previous section represent an average across all occupations in the data. This section assesses potential mechanisms for my findings and explores heterogeneous responses to occupational licensing requirements. I show that although licensing increases average educational attainment, observable changes in worker composition explain only a small share of the licensing wage premium. Next, I allow the effects of licensing to vary across different groups of occupations. As in the main analysis, I find that licensing is consistently associated with higher earnings, but has no discernible impact on employment for most subgroups. An important exception are occupations that pose a low risk of consumer harm, for which the evidence that licensing functions primarily as a barrier to entry is stronger. In cases where the consumer protection rationale for licensing is plausible, I find simultaneous increases in earnings and employment.

6.1 Mandated Investment and Worker Sorting

Occupational licensing statutes establish uniform minimum qualifications that workers must meet to legally enter the occupation. How much these mandatory investments in human capital raise the cost of entry, however, depends on how burdensome the statutory requirements are relative to the prevailing distribution of human capital in the market. In this section, I show that licensing increases average educational attainment by one to two months in the long-run, with larger effects for occupations that typically require at least an associate's degree. Controlling for this increase in the stock of human capital, as well as changes in the demographic composition of workers, explains at most 15% of the wage premium due to licensing. This finding rules out worker sorting as the primary mechanism through which licensing impacts the labor market.

Educational Attainment. To study the impact of occupational licensing requirements on human capital, I use data from the Current Population Survey and estimate my event study design with average years of completed education by state-occupation cell as the dependent variable.[29] I present both average estimates using the full sample and separate event studies for jobs that typically require at least an associate's degree and those that require a high school education or

[28] As another check, I computed aggregation weights using a leave-out method. Because there are few cases where a single state has a large influence on sub-occupation employment share estimates, the results were nearly identical to those reported in these tables.

[29] The Current Population Survey changed its coding of educational attainment from a definition based on years of completed education to highest degree attained in 1992. I follow the replication materials from Acemoglu & Autor (2011) to impute years of education following the revision. In addition, I note that vocational training is not well measured in the Census definition of education, which may bias the estimated impact of licensing on educational attainment downward.

less based on information from O*Net Online, the successor of the Dictionary of Occupation Titles. Table 5 shows that licensing has a uniformly positive impact on educational attainment, through these estimates are highly imprecise and only statistically significant in the long-run. My preferred specification implies that licensing increased average educational attainment across all occupation by 0.12 years (s.e. 0.07), or approximately a month and a half.

The table also shows that the increase in educational attainment is slightly larger for jobs that usually require workers to attend some college or more, with average educational attainment increasing by two months in the long-run. Because years of education are measured in discrete intervals, increasing educational attainment likely reflects shifting some workers from high school to associate's degrees and from bachelor's to master's degrees. Assuming that each takes an additional two years to complete, an alternative interpretation of the magnitude of this coefficient is that licensing increased the share of workers with associate's or master's degrees by 10% relative to non-licensing states. For comparison, the average increase in educational attainment I find is approximately one-third as large as that reported by Kleiner and Soltas (2020), who find licensing raises mean education by 0.4 years when proxying for policy differences using the recent addition of self-reported license attainment to the CPS questionnaire.[30]

Does Sorting Explain the Wage Premium? One potential explanation for the finding that licensing increases earnings without reducing employment is that wage premium simply reflects changes in the composition of workers in the occupation. Appendix Table A6 suggests that this is not the case. In the table, I present my preferred earnings specification with progressive controls for worker characteristics using data from the Current Population Survey. After controlling for increases in average educational attainment and the potential experience of workers, I still find a long-run wage premium of 6% (s.e. 1.9%) relative to my baseline estimate of 6.9%, a reduction consistent with the small increase in average educational attainment discussed above. Additional controls for the demographic composition of the occupation, union coverage, and the share of workers who are self-employed has little effect on the estimates. While this analysis does not account for selection on unobservables, it suggests that worker sorting alone does not explain the wage premium.

6.2 Heterogeneous Impacts of Licensing Requirements

Licensing policies apply to an incredibly diverse set of occupations. To assess the extent to which my main results mask important heterogeneity in earnings and employment responses, I allow the impact of licensing to vary based on the characteristics of the occupation.

By Difficulty of Entry. Panel A of Table 6 reports the results of estimating my preferred event study specification separately by the level of preparation needed to enter the occupation based on data from O*Net. The first group includes jobs with low to moderate entry requirements. These

[30] Appendix Figure A7 displays the impact of licensing on the average age of workers. The results show that licensing potentially delays entry for jobs typically requiring some college education or more, but these estimates are extremely imprecise.

usually require at most an associate's degree, apprenticeship, or vocational training. The second group includes higher-skilled jobs that usually require at least a bachelor's degree. I find that that the licensing wage premium is larger from occupations in the former group, with a long-run wage premium of 7.5% (s.e. 2.8%), compared to a statistically insignificant 3.8% (s.e. 5.3%) for the latter. I find no evidence of a decline in employment for either group.

By Occupation Group. To assess the extent to which the results are driven by the licensing of high-skilled services, I next split the sample into the highly-licensed professional specialty occupations based on the Dorn (2009) occupational classification and all other occupations in the sample. The former contains many health care occupations, as well as social service jobs such as psychologists and social workers. In Panel B of Table 6, I find a substantial long-run wage premium of 17.7% (s.e. 6.5%) for professional specialty occupations, though this estimate is highly imprecise owing to the smaller sample share of these occupations. This estimate is large relative to my baseline estimates, but it is comparable in magnitude to the average wage premium found by Kleiner and Krueger (2013) using custom survey data. All other occupations experience a smaller average wage premium of 6.6% (s.e. 2.8%), closer to my main estimates. Again, however, neither group shows any evidence of disemployment effects.

By Consequence of Error. The main justification for licensing is that it protects consumers from potential harm caused by unqualified workers. To measure these risks, I use the O*Net Online variable "consequence of error," which ranks how serious the result would be if a worker were to make a mistake that was not easily correctable. I then estimate my preferred event study specification separately for licensed occupations above and below the overall median on this scale. Although the prevalence of licensing correlates strongly with consequence of error, the possibility that it increases demand by providing a signal of quality is more plausible for some occupations than others. Licensed occupations that appear to pose little risk to consumers include barbers, cosmetologists, florists, dispensing opticians, interior designers, and massage therapists. However, even many occupations with a high potential for harm – including crane operators, electricians, medical assistants, pharmacy technicians, radiation therapists, and stationary engineers – are still licensed by fewer than fifty states.

The results shown in Panel C of Table 6 are consistent with licensing increasing the demand for services when the consequences of worker error are high, but functioning primarily as a barrier to entry when these risks are low. For occupations below the median consequence of error, I find a statistically insignificant long-run increase in wages of 4.7% (s.e. 3.9%), but a 24.8% (s.e. 8.3%) decrease in employment. Employment is also increasing for these occupations prior to regulation, which provides some evidence in favor of the regulatory capture view of licensing for these low-risk occupations. By contrast, for occupations above the median consequence of error, I find simultaneous increases in earnings and employment of of 7.3% (s.e. 2.3%) and 9.1% (s.e. 3.5%) respectively, which is more consistent with the view that licensing addresses a market failure. As most licensing policies in my data were adopted for occupations with relatively high potential for harm, these results provide some evidence reconciling my findings with negative labor supply estimates in the literature (Kleiner & Soltas, 2020; Blair & Chung, 2019).

6.3 Additional Heterogeneity Results

Given the significant heterogeneity in the characteristics of licensed occupations, an ideal analysis would estimate a separate set of dynamic treatment effects for each policy. Unfortunately, small sample sizes in the CPS and the short OES panel prevent this type of analysis for most occupations. To provide some occupation-specific evidence along these lines, this section first exploits the precise cross-sectional wage and employment estimates from the OES to assess the distribution of earnings and employment differences by state and occupation. Next, I present separate event studies for a small number of individual industries that are highly exposed to occupational licenses using data on precise employment counts derived from administrative records. Finally, I show that the increase in average hourly wages documented in the main analysis is driven by gains in the lower tail of the earnings distribution, consistent with theoretical predictions.

Cross-Sectional Estimates. The large underlying samples used to produce wage and employment estimates for the Occupational Employment Statistics survey make this data uniquely suited to a cross-sectional analysis of differences in outcomes across states for individual occupations. For each licensed six-digit occupation in my sample with policy variation of as 2015, I use the OES data to compute the difference in log average hourly wages and employment per capita between states that license the occupation and those where it is entirely unregulated. Although these comparisons should not be interpreted as causal, they are useful to assess how earnings and employment differences in the raw data are related to the estimates from my main empirical design.

Appendix Figure A8 shows that hourly wages are higher in licensing states for two-thirds of the occupations in the sample. This pattern is reversed for employment, however, with point estimates more likely to be negative in licensing states. Pooling across all occupations, a simple employment-weighted OLS regression with occupation fixed effects implies that licensing is associated with 5% (s.e. 2.3%) higher hourly wages and 3.4% (s.e. 2.9%) higher employment on average.[31] These estimates are comparable in sign and magnitude to the long-run impact of licensing identified in Section 5.1. Subject to the caveat that the OES does not capture self-employed workers, there is little evidence that licensing decreases employment on average, though this may clearly be the case for certain occupations. The high likelihood of finding statistically significant negative (or positive) employment effects for a single occupation also cautions against drawing strong conclusions about the average impact of occupational licensing from studies limited to a single policy setting.

Industry Case Studies. In the United States, data derived from administrative records such as the Quarterly Census of Employment and Wages (QCEW) report exact employment counts,

[31] Unweighted estimates imply that licensing is associated with an increase in wages of 3.7% (s.e. 1.1%) and a decrease in employment of -3.8% (s.e. 2.9%), which is consistent with the distribution of occupation-specific differences shown in the figure.

and are therefore not subject to the sampling error that could potentially affect my estimates. While the data unfortunately do not contain information on the occupational affiliation of workers, in some cases, industry definitions are sufficiently detailed that a six-digit NAICS code is highly likely to be affected by a specific occupational license. For example, the industry "interior design services" (NAICS 541510) is clearly associated with the occupation "interior designer." Industry-level administrative data can therefore be used to estimate the impact of certain occupational licenses on precise state employment counts for wage and salary workers. Although these estimates reflect the total impact of the policy on the employment of both licensed and unlicensed workers in the industry, if licensing causes an economically meaningful reduction in employment, this analysis should detect those effects.

Using data from the QCEW for 1990 to 2018, I report case study estimates for six industries that meet two sample selection criteria. First, the industry must be highly exposed to a single occupational license.[32] Second, a sufficient number of policy changes must have occurred within the sample window to identify the cumulative response of employment to licensing over the full -10 to +25 year event window. For each of these industries, I estimate a separate regression with state and census division by year fixed effects using the timing of the relevant occupational licensing policies. The results are shown in Appendix Figure A9. Analyzing each industry separately results in a substantial loss in power, but as in the main analysis, the employment estimates are concentrated around zero. The only exception is that registration of locksmiths appears to have reduced employment, though this effect is driven by California's statute alone, which the Institute for Justice ranks as the third *least* burdensome locksmith regulation in the United States (Carpenter et al., 2017).

Distribution of Wages. Throughout this paper, my analysis of earnings has focused on the impact of occupational licensing requirements on average hourly wages. Licensing, however, may not have a uniform impact across the wage distribution. By raising entry costs, the model of Shapiro (1986) predicts that licensing raises the price of lower-quality services while simultaneously decreasing the price of higher-quality services, implying larger wage effects at the bottom of the earnings distribution. I test this hypothesis by estimating my baseline event study specification using data from from the Occupational Employment Statistics, where the dependent variable is now the log of hourly wages at the 10th, 25th, 50th, 75th and 90th percentiles within the state by occupation cell. In Appendix Table A7, I find that increasing average hourly wages are indeed driven by gains for workers at or below median earnings, including a moderate short-run wage premium. Consistent with theoretical predictions, there is no effect on the 90th percentile of hourly wages.[33]

[32] This rules out, for example, "Offices of physical, occupational, and speech therapists, and audiologists" (NAICS 621340), which is affected by the regulation of at least nine distinct occupation categories.

[33] Even at the 90th percentile, fewer than 1% of wages are top-coded for the occupations in my sample, so this finding is not explained by the truncation of the wage distribution in the data.

7 Conclusion

Economic theory predicts that occupational licensing requirements will increase worker earnings, but total employment could rise or fall depending on the extent to which regulation influences the demand for professional services. Consistent with these predictions, I find that licensing increased earnings by 4 to 7% across all occupations in my sample. This wage premium, however, did not coincide with a significant decline in employment relative to non-licensing states. Rather, employment effects appear to depend on the type of occupation being licensed, in particular how severe worker errors might be. When the risk to consumers is high, I find that licensing increases employment, which is consistent with the view that regulation addresses a market failure. When the risk is low, employment falls, which suggests that licensing primarily increases barriers to entry instead. The robustness of these findings across datasets, time periods, and levels of occupational aggregation supports a causal interpretation of these estimates

Although understanding the impact of occupational licensing on earnings and employment is central to ongoing policy debates, it is important to stress that this paper does not attempt to fully characterize the costs and benefits of licensing. Even without negatively affecting total employment, licensing may impose costs on workers by reducing geographic mobility (Johnson & Kleiner, 2020) or job switching (Kleiner & Xu, 2020). Likewise, an assessment of consumer benefits would require either a clearly-defined measure of service quality (Anderson et. al, 2020; Farronato et al., 2020), or a structural model of the labor and product markets (Kleiner & Soltas, 2020). Further, this paper looks only at the extensive margin of regulation. Conditional on becoming licensed, variation in specific requirements – such as banning workers with a criminal history – may matter a great deal for labor market outcomes (Blair & Chung, 2018). At a minimum, my finding of both a wage premium and null or positive employment effects across a broad range of occupations shows that the potential for licensing to increase labor demand should be taken seriously.

My results, together with the new data I compile in this paper, suggest several topics for future research. First, my findings depart substantially from those obtained using cross-sectional survey data on occupational licenses. Understanding why these alternative approaches arrive at different conclusions will be crucial for reconciling my findings with the previous literature. Second, what explains the growth in occupational licensing over time and its variation across states? Traditionally, the public choice view has interpreted the diffusion of regulation as evidence of rent-seeking behavior. At least in the health care sector, however, new license categories might also have been created in response to the increasing specialization of workers and the evolution of their scope of practice over time. Finally, the data used here should also be useful for analyzing the general equilibrium effects of occupational licensing and its macroeconomic implications.

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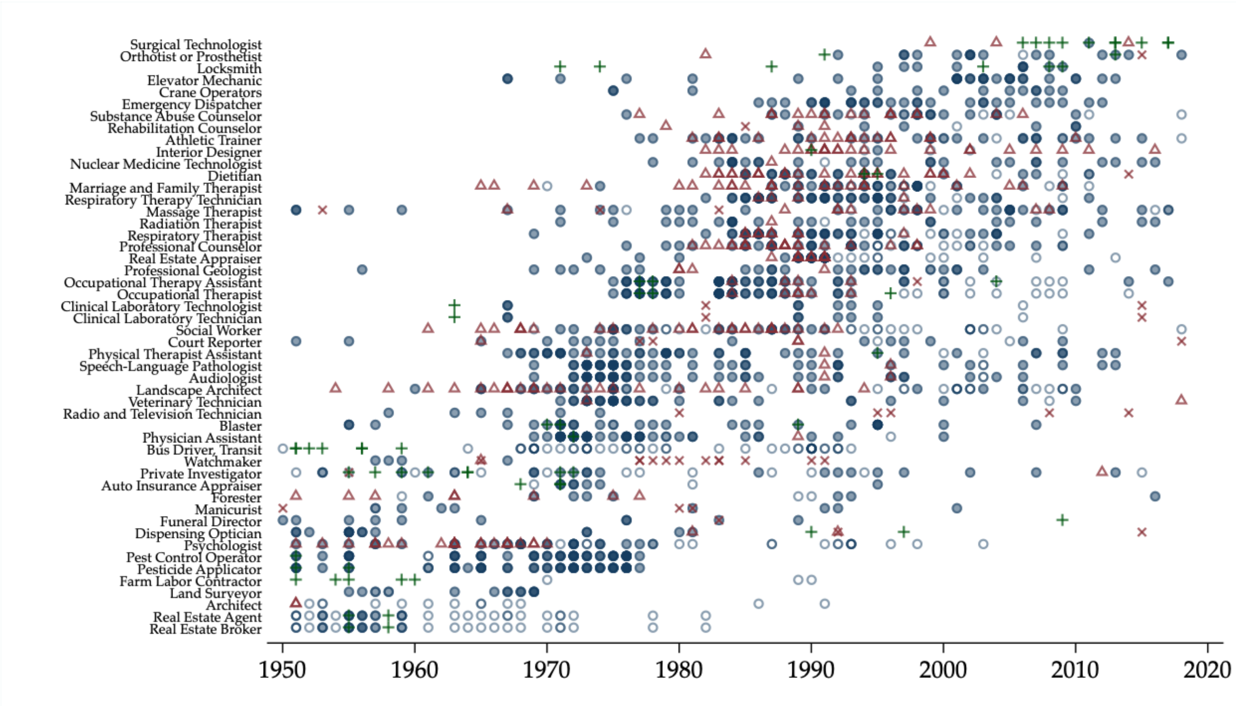
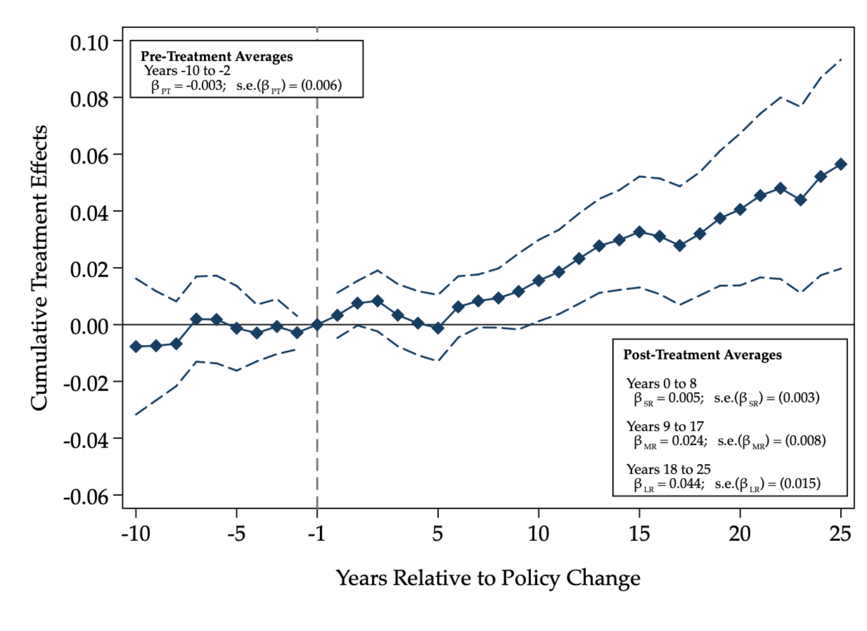
Figure 1: Licensing, Certification, and Registration Events by Detailed Occupation



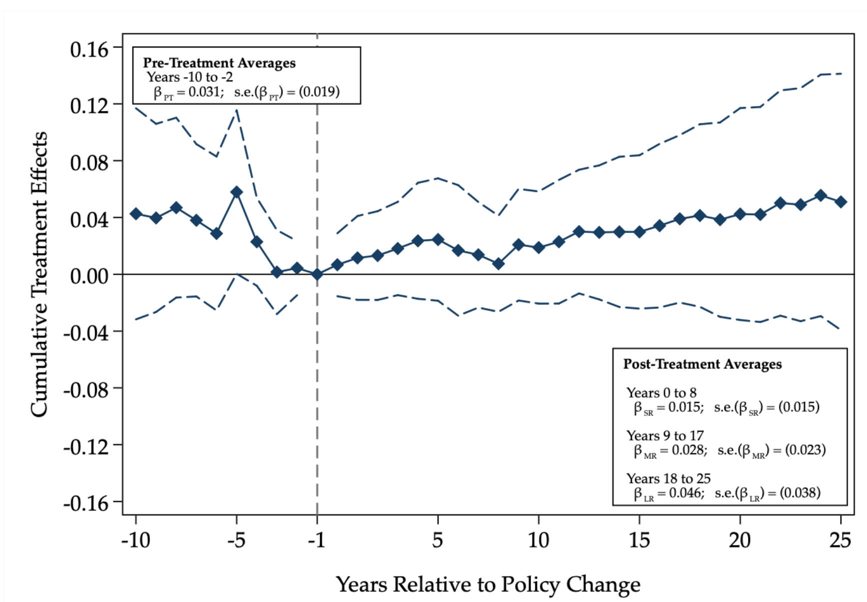
Notes: This figure plots the date and type policy changes for a subset of the larger six-digit occupations experiencing ten or more regulation events after 1950. The data are obtained from an analysis of state session laws. Solid blue circles mark the licensing of a previously unregulated occupation and open blue circles the licensing of an occupation that was previously registered or certified. Red triangles denote the certification of a previously unregulated occupation. Red crosses indicate that a regulation was repealed, and green plus signs show the adoption of a regulation other than state licensing or certification. Markers are darker when multiple states adopted the policy simultaneously.

Figure 2: Dynamic Response of Hourly Wages and Employment to Licensing Events (Occupational Employment Statistics State Panel 1999-2018)

(a) Log Average Hourly Wage

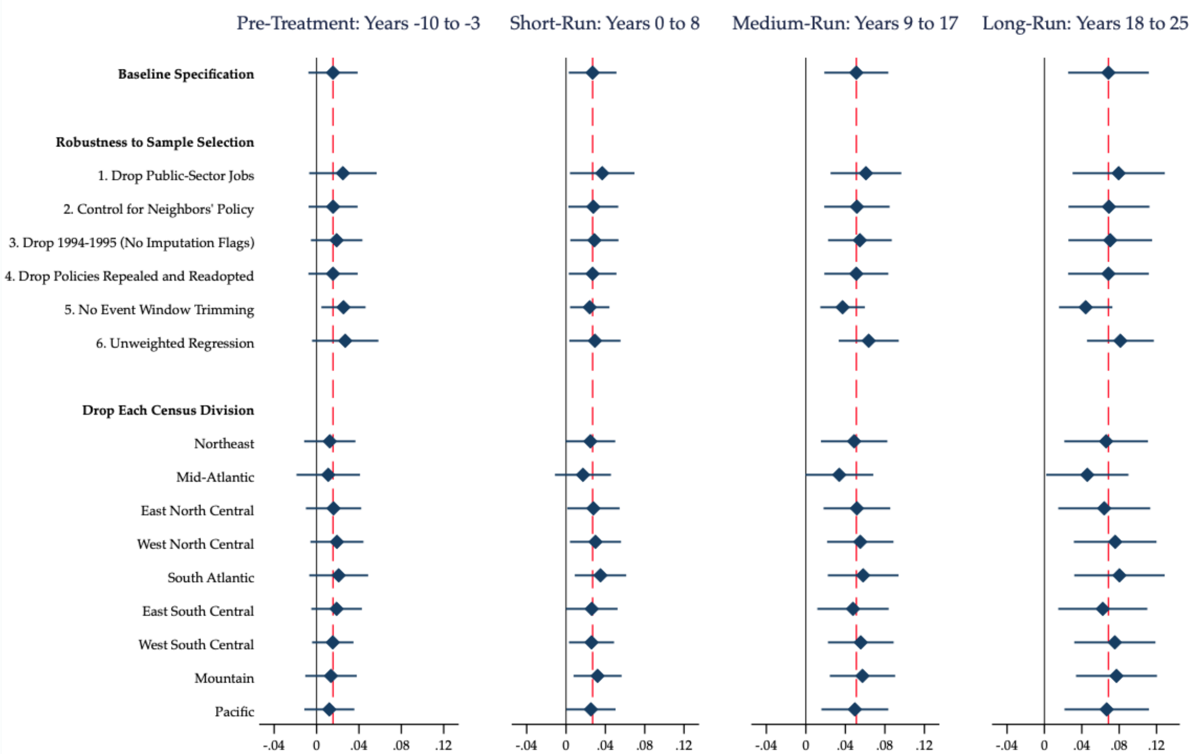


(b) Log Employment-to-Population Ratio



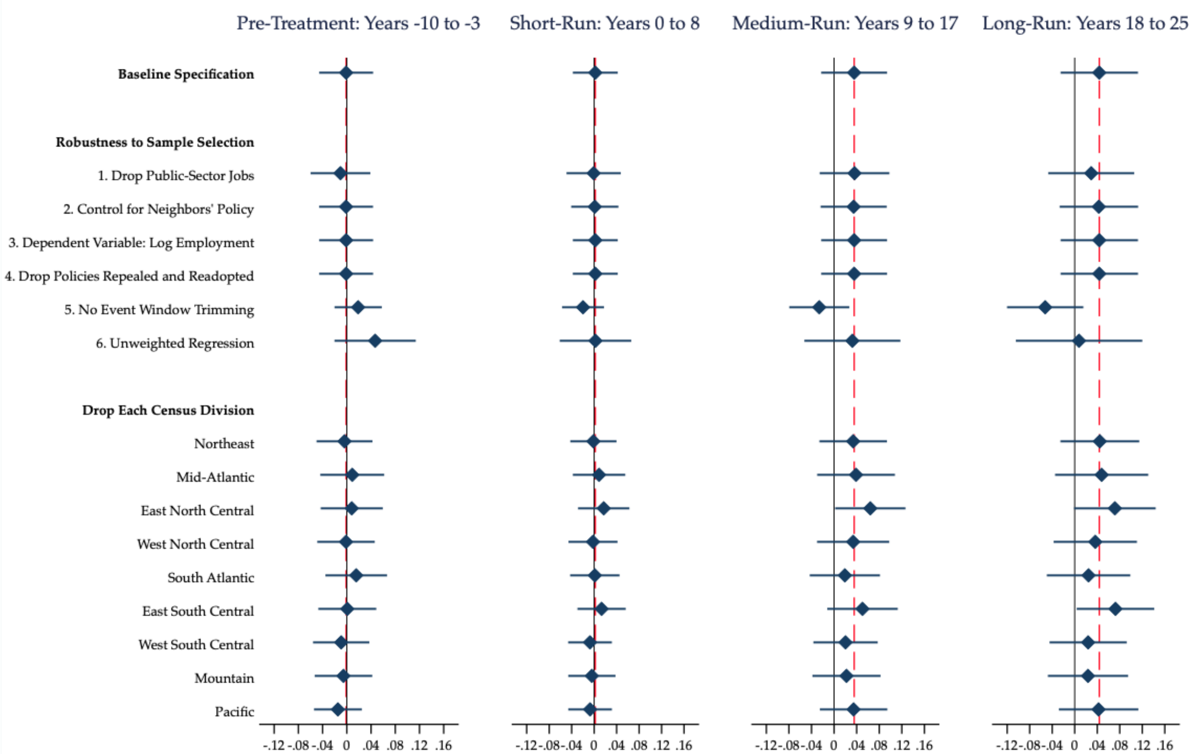
Notes: This figure displays event study estimates for the effect of licensing on wages and employment using data from the Occupational Employment Statistics program. In this sample, treatment and outcome variables are observed at the six-digit level. Both regressions include state-by-occupation and occupation-by-year fixed effects, control for regulations other than licensing, and are employment-weighted. All coefficients are normalized relative to the year prior to the statutory effective date of the law. Dashed lines denote 95% confidence intervals for the point estimates based on standard errors clustered by state. Each panel also reports binned averages of the event study coefficients and the standard errors associated with these estimates.

Figure 3: Robustness of Hourly Wage Estimates to Alternative Sample Definitions (State-Occupation Aggregated CPS Panel 1983-2018)



Notes: This figure assesses the robustness of the main hourly wage event study estimates to alternative sample selection decisions. All regressions include occupation by state, occupation by census division and year, and occupation group by state and year fixed effects, which corresponds to the specification estimated in column six of Table 2. The top row of the figure replicates this estimate for reference. Horizontal bands denote 95% confidence intervals with standard errors clustered at the state level. Specification 1 drops any occupation where 50% or more of workers are employed in the public sector. Specification 2 controls for polices adopted by neighboring states. Specification 3 drops years 1994 and 1995, for which there are no reliable flags for earnings imputations. Specification 4 drops cases where regulations were repealed and later readopted. Specification 5 allows long-treated units to remain in the sample as controls. Specification 6 reports a regression that is not employment-weighted. The remaining rows drop each of the nine census divisions separately and estimate the regression with data from the remaining eight.

Figure 4: Robustness of Employment Estimates to Alternative Sample Definitions (State-Occupation Aggregated CPS Panel 1983-2018)



Notes: This figure assesses the robustness of the main employment event study estimates to alternative sample selection decisions. All regressions include occupation by state, occupation by census division and year, and occupation group by state and year fixed effects, which corresponds to the specification estimated in column six of Table 4. The top row of the figure replicates this estimate for reference. Horizontal bands denote 95% confidence intervals with standard errors clustered at the state level. Specification 1 drops any occupation where 50% or more of workers are employed in the public sector. Specification 2 controls for polices adopted by neighboring states. Specification 3 uses log employment rather than log employment per-capita as the dependent variable. Specification 4 drops cases where regulations were repealed and later readopted. Specification 5 allows long-treated units to remain in the sample as controls. Specification 6 reports a regression that is not employment-weighted. The remaining rows drop each of the nine census divisions separately and estimate the regression with data from the remaining eight.

Table 1: Political and Economic Factors Affecting the Timing of State Licensing (Occupational Licensing Legislation Adopted 1975-2010)

	Dependent Variable: Indicator for Licensing Statute					
	(1)	(2)	(3)	(4)	(5)	(6)
A. Partisan Control of State Government: Legislature x Govenor						
Republican × Democrat	1.043	1.044	1.031	1.032	1.027	1.045
	(0.098)	(0.098)	(0.099)	(0.099)	(0.099)	(0.085)
Democrat × Republican	1.242**	1.237**	1.216**	1.212**	1.207**	1.239***
	(0.115)	(0.115)	(0.113)	(0.113)	(0.112)	(0.098)
Democrat × Democrat	1.501***	1.499***	1.446***	1.445***	1.445***	1.451***
	(0.132)	(0.131)	(0.129)	(0.129)	(0.129)	(0.109)
B. State Economic Conditions						
Log Employment-to-Population Ratio			0.862**	0.865**	0.863**	0.868***
			(0.051)	(0.052)	(0.052)	(0.045)
Log Gross State Product			1.075	1.069	1.064	1.006
			(0.182)	(0.181)	(0.181)	(0.146)
Urbanization Rate			1.029	1.030	1.031	1.039
			(0.042)	(0.042)	(0.042)	(0.036)
Union Coverage			0.964	0.964	0.960	0.915***
			(0.036)	(0.036)	(0.036)	(0.030)
C. Occupation-Specific Characteristics						
Log Average Hourly Wage		1.025		1.033	1.032	1.028
		(0.031)		(0.044)	(0.044)	(0.036)
Log Employment per capita		0.855		0.875	0.867	0.909
		(0.088)		(0.091)	(0.091)	(0.086)
Previously Certified or Registered					1.166	1.296**
					(0.153)	(0.143)
Neighboring States Licensing (%)					0.893	1.021
					(0.143)	(0.138)
P-values for Joint Significance						
Partisan Control of State Government	0.000	0.000	0.000	0.000	0.000	0.000
State Economic Conditions			0.094	0.111	0.092	0.001
Occupation-Specific Chracteristics		0.296		0.390	0.445	0.142
Number of Events	1,206	1,206	1,206	1,206	1,206	1,656
Sample Observations	25,777	25,777	25,777	25,777	25,777	39,268
Occupation Sample	6-digit	6-digit	6-digit	6-digit	6-digit	All

Notes: This table reports the results of a discrete-time hazard model estimating the factors correlated with the timing of state licensing statutes. Each column is produced from a separate conditional logistic regression that absorbs occupation-specific hazard rates. All estimates are expressed as the change in the log-odds of adopting a policy in a given year conditional on not having licensed the occupation in the past. State-level variables and occupation-specific wages and employment are reported as the impact of a one standard deviation increase in the variable. Alaska and Hawaii are omitted because they do not border any other states. Nebraska is omitted because it has a unicameral legislature. Occupational wages and employment per capita are measured at the three-digit level by linearly interpolating data between census years prior to 2000. Data on the timing of policy adoption is compiled through an analysis of state session laws. The remaining data sources are as follows: partisan composition of state governments (Klarner, 2013); total state employment (Quarterly Census of Employment and Wages); state population, urbanization, occupational wages and employment (Census Bureau); gross state product (Bureau of Economic Analysis); union membership (Hirsch & Macpherson, 2019). Significance levels are indicated by *** 1%; ** 5%; and * 10%.

Table 2: Dynamic Response of Average Hourly Wages to Licensing Events (State-Occupation Aggregated CPS Panel 1983-2018)

	Dependent Variable: Log Average Hourly Wage					
Cumulative Treatment Effects	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Treatment Effect	0.010	0.011	0.012	0.012	0.015	0.016
$\hat{\beta}_{PT}$: Years -10 to -3	(0.014)	(0.015)	(0.012)	(0.013)	(0.013)	(0.012)
Short-Run Effect	0.010	0.011	0.016	0.020*	0.022*	0.027**
$\hat{\beta}_{SR}$: Years 0 to 8	(0.010)	(0.011)	(0.014)	(0.011)	(0.012)	(0.012)
Medium-Run Effect	0.037**	0.039**	0.038**	0.044***	0.047***	0.051***
$\hat{\beta}_{MR}$: Years 9 to 17	(0.014)	(0.015)	(0.017)	(0.014)	(0.016)	(0.016)
Long-Run Effect	0.039**	0.042*	0.042*	0.064***	0.069***	0.069***
$\hat{\beta}_{LR}$: Years 18 to 25	(0.018)	(0.022)	(0.022)	(0.018)	(0.021)	(0.021)
Number of Events	1,166	1,166	1,166	1,109	1,109	1,109
Sample Observations	348,249	348,249	348,249	339,660	339,660	339,660
Total Worker Observations	21,745,118	21,745,118	21,745,118	21,618,794	21,618,794	21,618,794
Occupation-Year FE	✓	✓	✓	✓	✓	✓
Occupation-State FE	✓	✓	✓	✓	✓	✓
Policy Controls		✓	✓		✓	✓
Occupation Group-State-Year FE			✓			✓
Occupation-Division-Year FE				✓	✓	✓

Notes: This table reports binned averages of event study estimates for the effect of licensing on hourly wages. The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. Treatment events occur at the six-digit level and are aggregated using the procedure explained in the text. These regressions use variation from all available licensing events in the data, including those with overlapping event windows and units treated before the outcome window. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation-year cell. All estimates are expressed relative to the leave-out category of 1 to 3 years prior to treatment. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

Table 3: Robustness of Earnings Estimates to Alternative Income Measures (State-Occupation Aggregated Census 1980-2000 and ACS 2001-2017)

	Log Hourly Wage/Salary Income		Log Annual Wage/Salary Income		Log Total Hourly Earnings		Log Total Annual Earned Income	
Cumulative Treatment Effects	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-Treatment Effect	-0.011	-0.009	-0.007	0.000	-0.005	-0.001	-0.001	0.011
$\hat{\beta}_{PT}$: Years -10 to -3	(0.011)	(0.011)	(0.010)	(0.009)	(0.013)	(0.012)	(0.009)	(0.009)
Short-Run Effect	0.000	0.000	0.003	0.008	-0.002	-0.003	0.003	0.009
$\hat{\beta}_{SR}$: Years 0 to 8	(0.011)	(0.011)	(0.010)	(0.010)	(0.012)	(0.013)	(0.010)	(0.010)
Medium-Run Effect	0.023*	0.024*	0.025*	0.030*	0.019	0.020	0.027*	0.033**
$\hat{\beta}_{MR}$: Years 9 to 17	(0.012)	(0.014)	(0.013)	(0.015)	(0.013)	(0.016)	(0.014)	(0.016)
Long-Run Effect	0.038**	0.034*	0.043**	0.043**	0.035*	0.030	0.043**	0.044**
$\hat{\beta}_{LR}$: Years 18 to 25	(0.018)	(0.018)	(0.020)	(0.020)	(0.019)	(0.020)	(0.021)	(0.021)
Number of Events	1,062	1,062	1,062	1,062	1,065	1,065	1,065	1,065
Sample Observations	235,737	235,737	235,737	235,737	238,426	238,426	238,439	238,439
Total Worker Observations	26,991,334	26,991,334	26,991,334	26,991,334	26,997,863	26,997,863	26,997,881	26,997,881
Occupation-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Occupation-Division-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Policy Controls	✓	✓	✓	✓	✓	✓	✓	✓
Occupation Group-State-Year FE		✓		✓		✓		✓

Notes: This table reports binned averages of event study estimates for the effect of licensing on annual earnings. The estimation sample is an aggregated state-occupation panel constructed from the 1980-2000 Census and 2001-2017 American Community Survey. Treatment events occur at the six-digit level and are aggregated using the procedure explained in the text. These regressions use variation from all available licensing events in the data, including those with overlapping event windows and units treated before the outcome window. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation-year cell. All estimates are expressed relative to the leave-out category of 1 to 3 years prior to treatment. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

Table 4: Dynamic Response of Occupational Employment to Licensing Events (State-Occupation Aggregated CPS Panel 1983-2018)

	Dependent Variable: Log Employment-to-Population Ratio					
Cumulative Treatment Effects	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Treatment Effect	0.005	0.013	0.008	-0.007	0.000	-0.001
$\hat{\beta}_{PT}$: Years -10 to -3	(0.017)	(0.017)	(0.020)	(0.018)	(0.020)	(0.022)
Short-Run Effect	0.005	0.011	0.002	-0.007	0.003	0.002
$\hat{\beta}_{SR}$: Years 0 to 8	(0.015)	(0.014)	(0.013)	(0.018)	(0.019)	(0.020)
Medium-Run Effect	0.043*	0.055***	0.022	0.031	0.054*	0.036
$\hat{\beta}_{MR}$: Years 9 to 17	(0.025)	(0.020)	(0.021)	(0.024)	(0.028)	(0.029)
Long-Run Effect	0.053*	0.070***	0.027	0.043	0.075**	0.044
$\hat{\beta}_{LR}$: Years 18 to 25	(0.028)	(0.025)	(0.032)	(0.030)	(0.034)	(0.034)
Number of Events	1,189	1,189	1,189	1,145	1,145	1,145
Sample Observations	410,831	410,831	410,831	406,459	406,459	406,459
Total Worker Observations	22,060,862	22,060,862	22,060,862	21,982,018	21,982,018	21,982,018
Occupation-Year FE	✓	✓	✓	✓	✓	✓
Occupation-State FE	✓	✓	✓	✓	✓	✓
Policy Controls		✓	✓		✓	✓
Occupation Group-State-Year FE			✓			✓
Occupation-Division-Year FE				✓	✓	✓

Notes: This table reports binned averages of event study estimates for the effect of licensing on employment by occupation. The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. Treatment events occur at the six-digit level and are aggregated using the procedure explained in the text. These regressions use variation from all available licensing events in the data, including those with overlapping event windows and units treated before the outcome window. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation-year cell. All estimates are expressed relative to the leave-out category of 1 to 3 years prior to treatment. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

Table 5: Dynamic Response of Average Educational Attainment to Licensing Events (State-Occupation Aggregated CPS Panel 1983-2018)

	Dependent Variable: Average Years of Education					
Cumulative Treatment Effects	Full Sample (1)	Some College or More (2)	High School or Less (3)	Full Sample (4)	Some College or More (5)	High School or Less (6)
Pre-Treatment Effect	0.032	0.021	0.049	0.044	0.012	0.078
$\hat{\beta}_{PT}$: Years -10 to -3	(0.038)	(0.053)	(0.050)	(0.037)	(0.055)	(0.048)
Short-Run Effect	0.017	0.035	-0.001	0.039	0.028	0.063
$\hat{\beta}_{SR}$: Years 0 to 8	(0.038)	(0.049)	(0.047)	(0.040)	(0.044)	(0.058)
Medium-Run Effect	0.082	0.112	0.060	0.079	0.109*	0.082
$\hat{\beta}_{MR}$: Years 9 to 17	(0.054)	(0.068)	(0.070)	(0.055)	(0.064)	(0.073)
Long-Run Effect	0.137**	0.169**	0.122	0.122*	0.181**	0.140
$\hat{\beta}_{LR}$: Years 18 to 25	(0.068)	(0.079)	(0.092)	(0.065)	(0.079)	(0.085)
Number of Events	1,145	678	467	1,145	678	467
Sample Observations	406,459	149,756	255,712	406,459	149,756	255,712
Total Worker Observations	21,982,018	8,658,441	13,323,577	21,982,018	8,658,441	13,323,577
Occupation-Division-Year FE	✓	✓	✓	✓	✓	✓
Occupation-State FE	✓	✓	✓	✓	✓	✓
Policy Controls	✓	✓	✓	✓	✓	✓
Occupation Group-State-Year FE				✓	✓	✓

Notes: This table reports binned averages of event study estimates for the effect of licensing on average educational attainment by occupation. Columns one and four use the full sample. The remaining columns split the sample into occupations typically requiring some college education or more versus a high school degree or less based on data from O*Net. The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. Treatment events occur at the six-digit level and are aggregated using the procedure explained in the text. These regressions use variation from all available licensing events in the data, including those with overlapping event windows and units treated before the outcome window. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation-year cell. All estimates are expressed relative to the leave-out category of 1 to 3 years prior to treatment. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

Table 6: Heterogeneous Impact of Licensing on Average Hourly Wages and Employment by Occupation Characteristics (State-Occupation Aggregated CPS Panel 1983-2018)

	Log Average Hourly Wage				Log Employment-to-Population Ratio			
	Pre-Treatment (1)	Short-Run (2)	Medium-Run (3)	Long-Run (4)	Pre-Treatment (5)	Short-Run (6)	Medium-Run (7)	Long-Run (8)
A. Level of Education, Experience, or Training Required								
Little to Medium Preparation	0.020	0.033*	0.054***	0.075***	-0.004	0.004	0.052*	0.049
	(0.018)	(0.020)	(0.020)	(0.028)	(0.027)	(0.028)	(0.030)	(0.039)
Considerable or Extensive Preparation	0.013	0.015	0.035	0.038	0.004	-0.008	-0.010	0.005
	(0.015)	(0.016)	(0.037)	(0.053)	(0.041)	(0.033)	(0.056)	(0.070)
B. Occupation Group								
Professional Specialty Occupations	-0.008	0.042**	0.117***	0.177***	0.017	0.018	0.060	0.097
	(0.010)	(0.019)	(0.043)	(0.065)	(0.039)	(0.037)	(0.076)	(0.112)
All Other Occupations	0.024	0.036*	0.052**	0.066**	-0.017	-0.003	0.035	0.032
	(0.020)	(0.020)	(0.020)	(0.028)	(0.027)	(0.029)	(0.034)	(0.039)
C. Consequence of Error								
Below Median Seriousness	0.015	0.004	0.044	0.047	-0.122**	-0.143**	-0.184**	-0.248***
	(0.024)	(0.035)	(0.039)	(0.046)	(0.050)	(0.059)	(0.071)	(0.083)
Above Median Seriousness	0.011	0.028**	0.050***	0.073***	0.016	0.025	0.065**	0.091**
	(0.012)	(0.012)	(0.017)	(0.023)	(0.023)	(0.022)	(0.030)	(0.035)

Notes: This table reports binned averages of event study estimates for the effect of licensing on hourly wages. The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. Treatment events occur at the six-digit level and are aggregated using the procedure explained in the text. These regressions use variation from all available licensing events in the data, including those with overlapping event windows and units treated before the outcome window. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation-year cell. All estimates are expressed relative to the leave-out category of 1 to 3 years prior to treatment. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

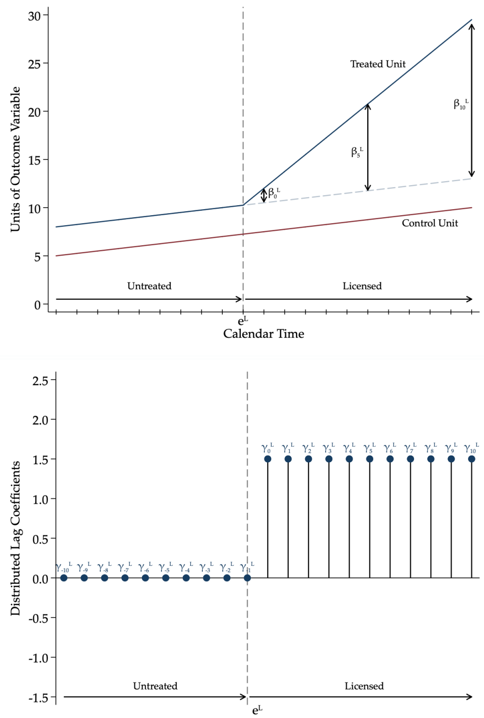
Appendix A: Additional Tables and Figures

Appendix Figure A1: Share of Workers Licensed by Consequence of Error

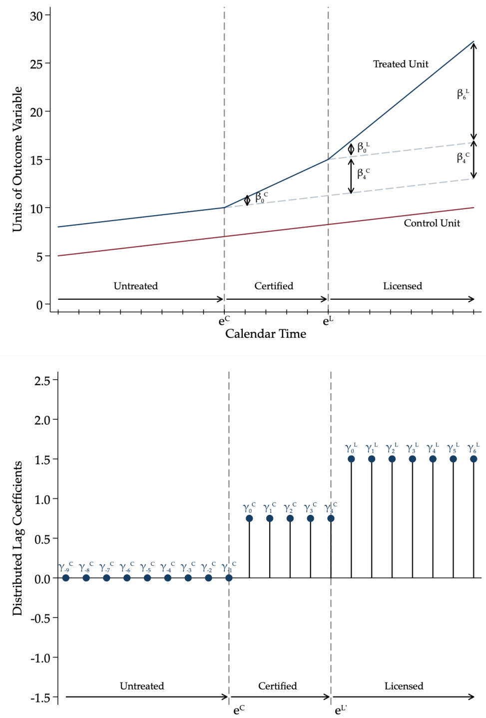
Notes: This figure uses data from the 2015-2018 Current Population Survey and O*Net Online. It displays a binned scatterplot of the relationship between the share of workers in the CPS reporting that they hold an occupational license against the potential severity of a worker error. The consequence of error measure is taken from O*Net, and measures on a one to five scale, "How serious would the result usually be if the worker made a mistake that was not readily correctable?"

Appendix Figure A2: Hypothetical Outcome Paths and Dynamic Treatment Effects With Multiple Policy Changes

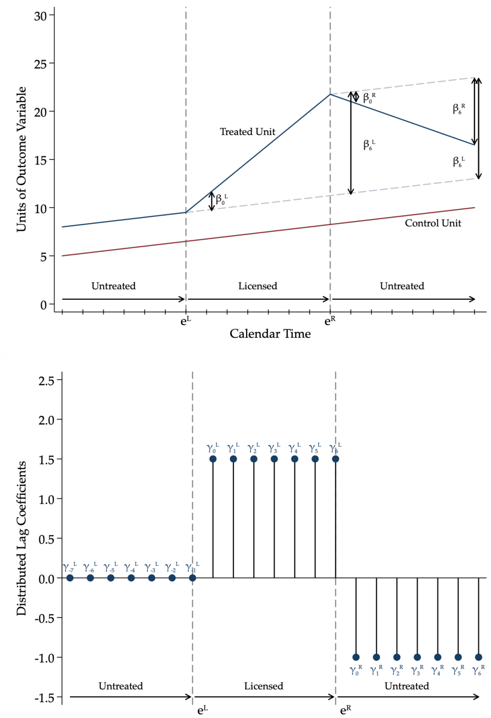
(a) Licensing Policy Only



(b) Certification Precedes Licensing



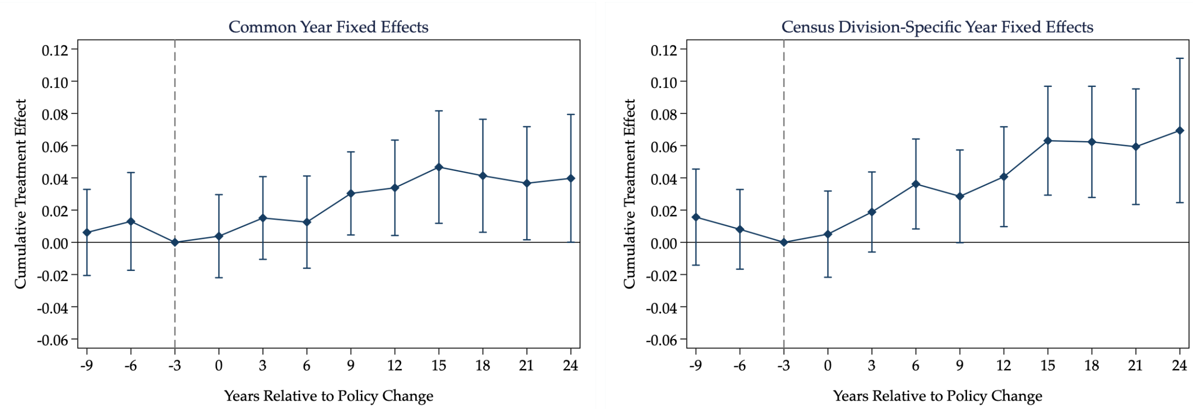
(c) Licensing Policy Repealed



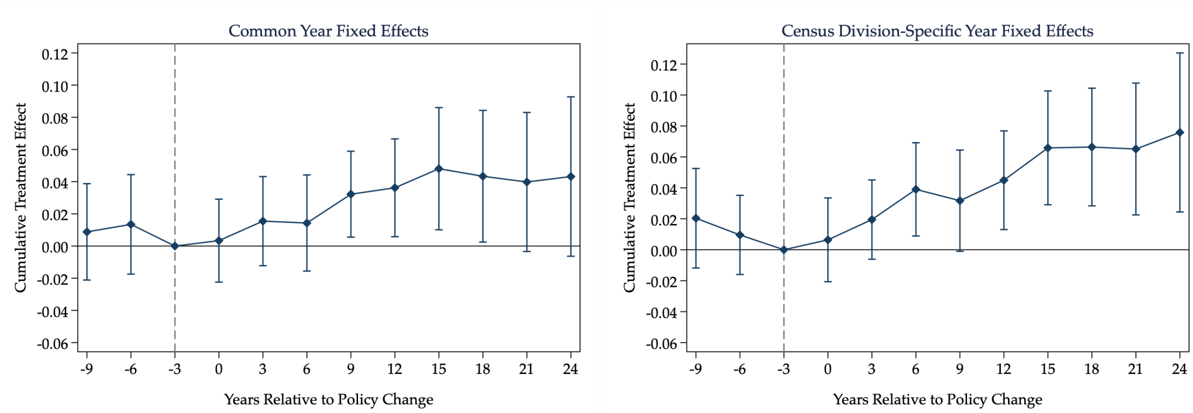
Notes: This figure displays three hypothetical outcome paths and the relationship between contemporaneous (distributed lag) and cumulative (event study) coefficients when units may experience multiple policy changes. For simplicity, the example assumes that the parallel trends assumption for treatment and control units is satisfied. Note that all past distributed lag coefficients (γ's) must be accumulated to explain the difference between treated and control units in levels (β's).

Appendix Figure A3: Dynamic Response of Hourly Wages to Licensing Events (Current Population Survey State Panel 1983-2018)

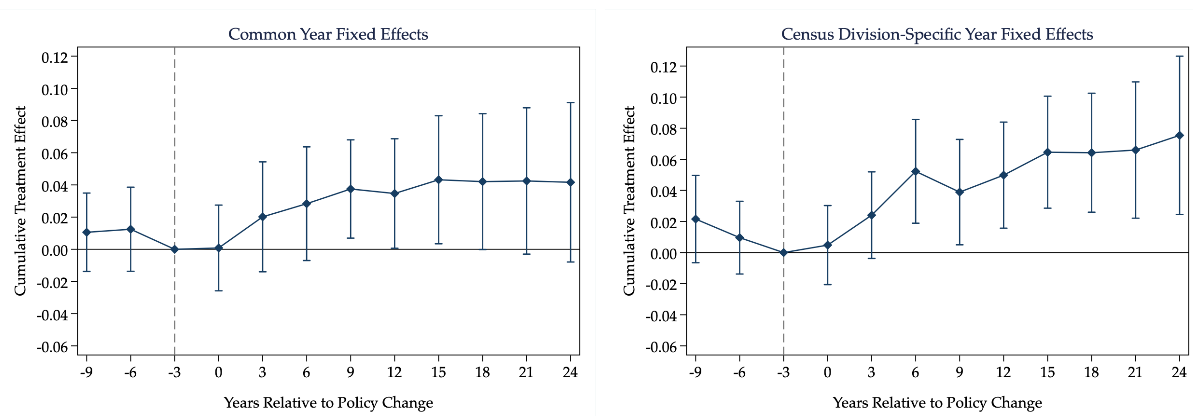
(a) Two-Way Fixed Effects, Licensing Only



(b) Two-Way Fixed Effects and Policy Controls

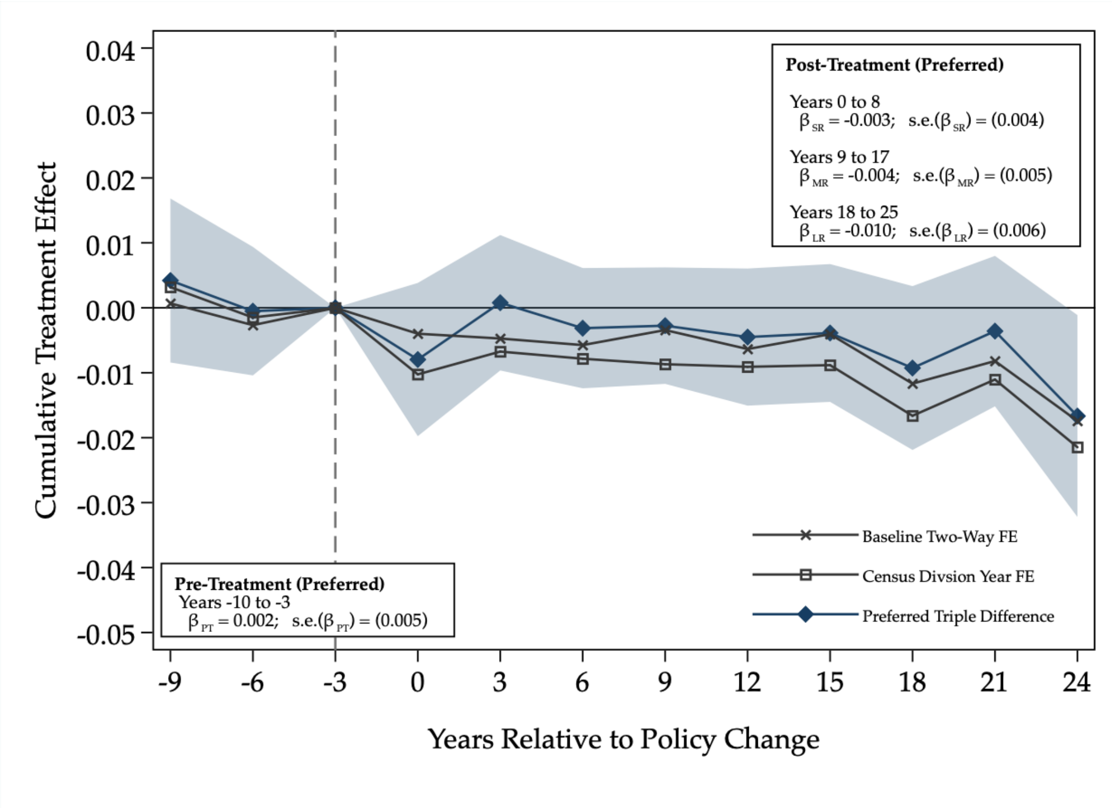


(c) Two-Way Fixed Effects, State by Occupation Group Trends, and Policy Controls



Notes: This figure displays the underlying event study coefficients used to construct the averages reported in Table 2. The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. All coefficients are normalized relative to the period one to three years prior to treatment. Vertical bands denote 95% confidence intervals based on standard errors clustered by state.

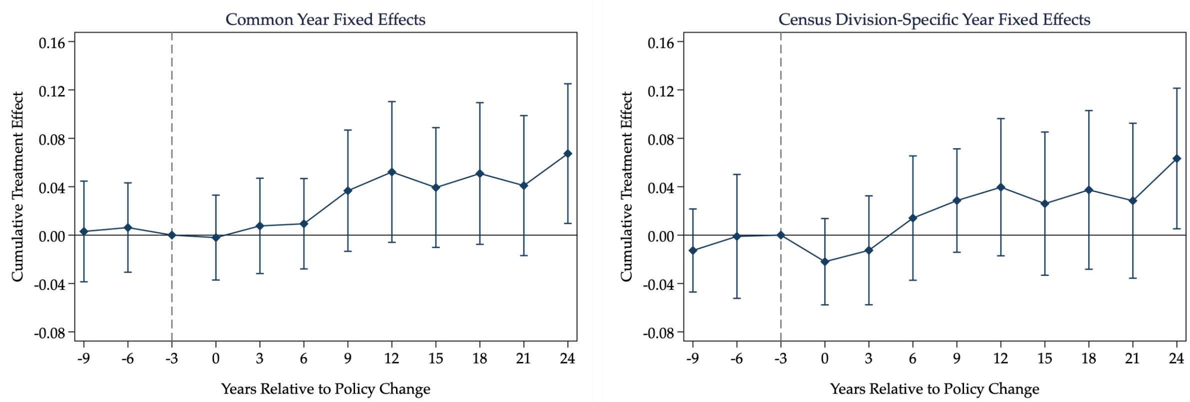
Appendix Figure A4: Effect of Licensing Events on the Self-Employment Rate (Current Population Survey State Panel 1983-2018)



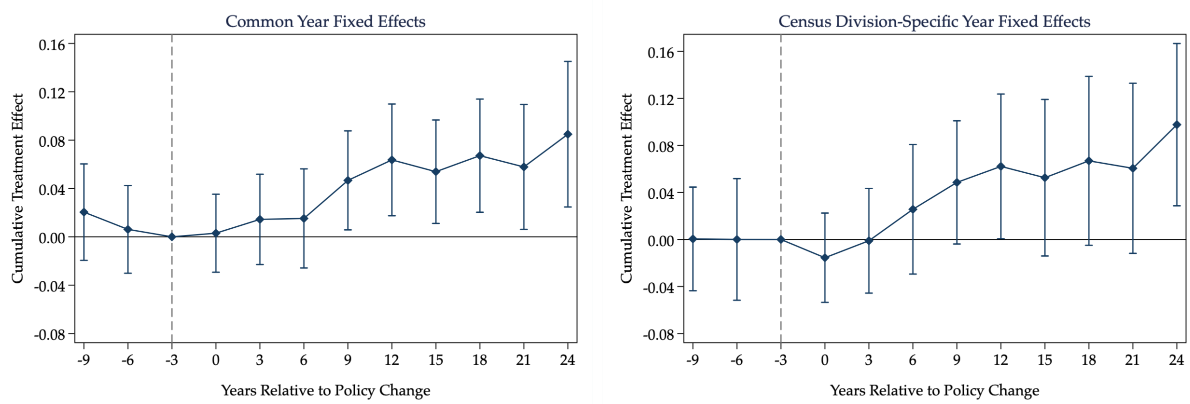
Notes: This figure plots event study estimates for the impact of licensing on the share of workers who are self-employed. The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. All coefficients are normalized relative to the period one to three years prior to treatment. The preferred specification includes state-year fixed effects specific to six broad occupation groups in addition to Census-division specific occupation-year effects. The shaded area denotes the 95% confidence interval based on standard errors clustered by state for the preferred specification.

Appendix Figure A5: Dynamic Response of Employment to Licensing Events (Current Population Survey State Panel 1983-2018)

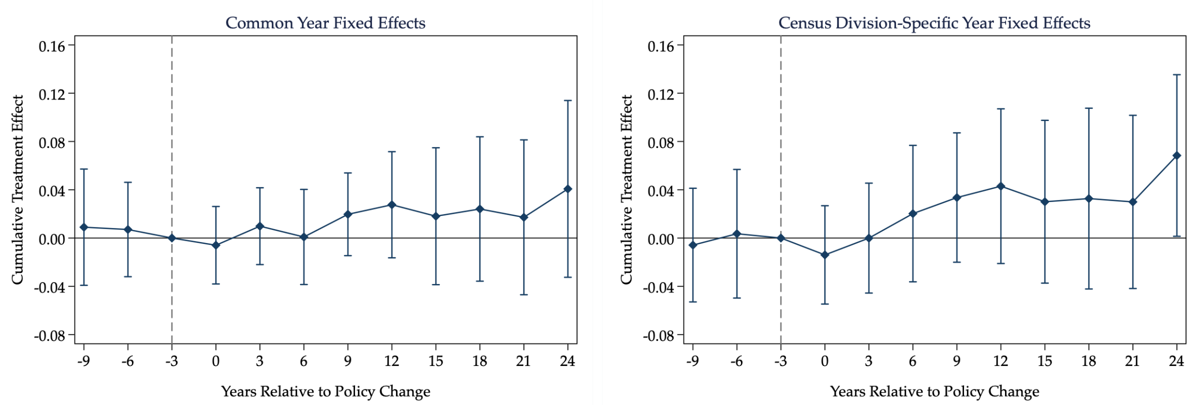
(a) Two-Way Fixed Effects, Licensing Only



(b) Two-Way Fixed Effects and Policy Controls



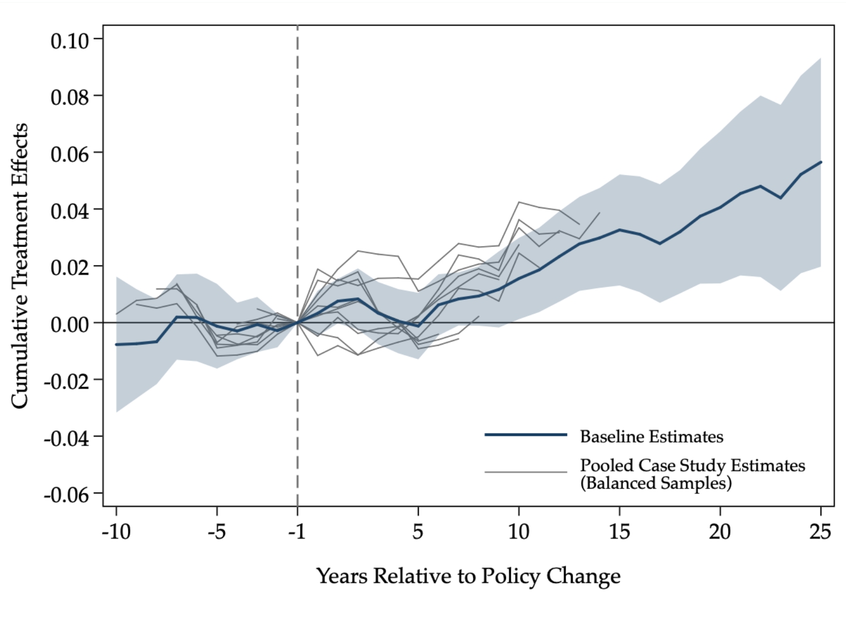
(c) Two-Way Fixed Effects, State by Occupation Group Trends, and Policy Controls



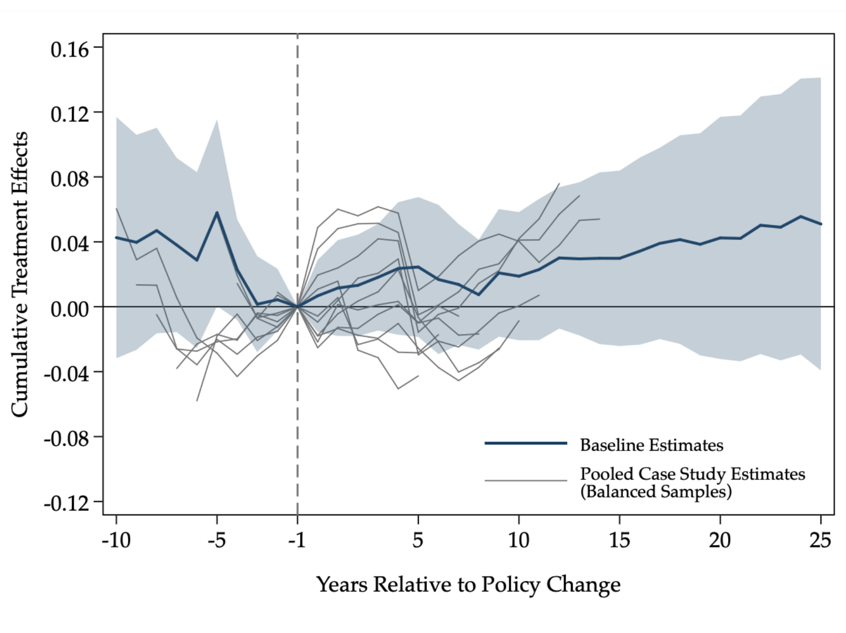
Notes: This figure displays the underlying event study coefficients used to construct the averages reported in Table 4. The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. All coefficients are normalized relative to the period one to three years prior to treatment. Vertical bands denote 95% confidence intervals based on standard errors clustered by state.

Appendix Figure A6: Pooled Case Study Estimates of Wage and Employment Effects (Occupational Employment Statistics State Panel 1999-2018)

(a) Log Average Hourly Wage



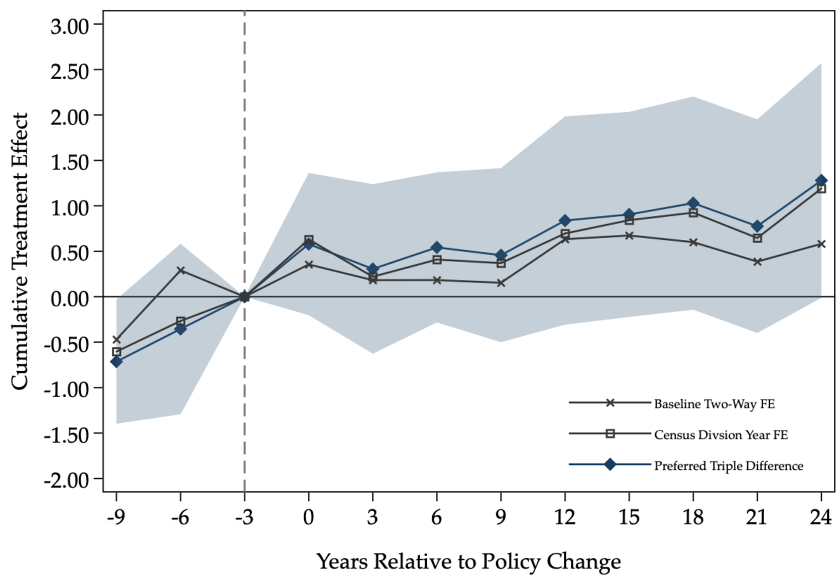
(b) Log Employment-to-Population Ratio



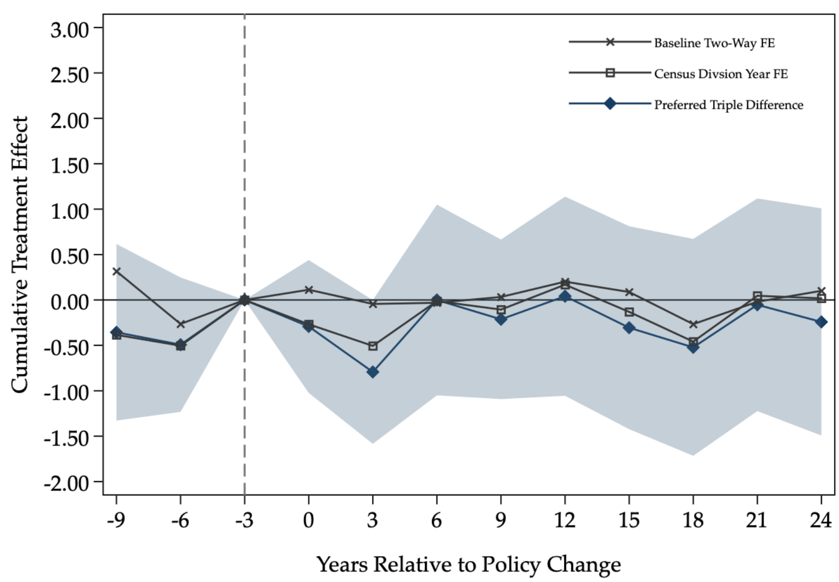
Notes: This figure displays pooled case study estimates for the effect of licensing on wages and employment using data from the Occupational Employment Statistics program. In this sample, observations for treated units within a -10 to 25 year window around each treatment event are matched to all clean control states, which include states that do not regulate the occupation or are observed more than ten years prior to regulation. The estimates are obtained from a version of the baseline event study regression that pools the data and estimates separate fixed effects for each matched treatment-control set to obtain an average across these "case-studies." Grey lines denote estimates from samples that have also been balanced in event time, so that each coefficient is estimated using data from the same set of events.

Appendix Figure A7: Dynamic Response of Average Worker Age to Licensing Events (Current Population Survey State Panel 1983-2018)

(a) Some College or More

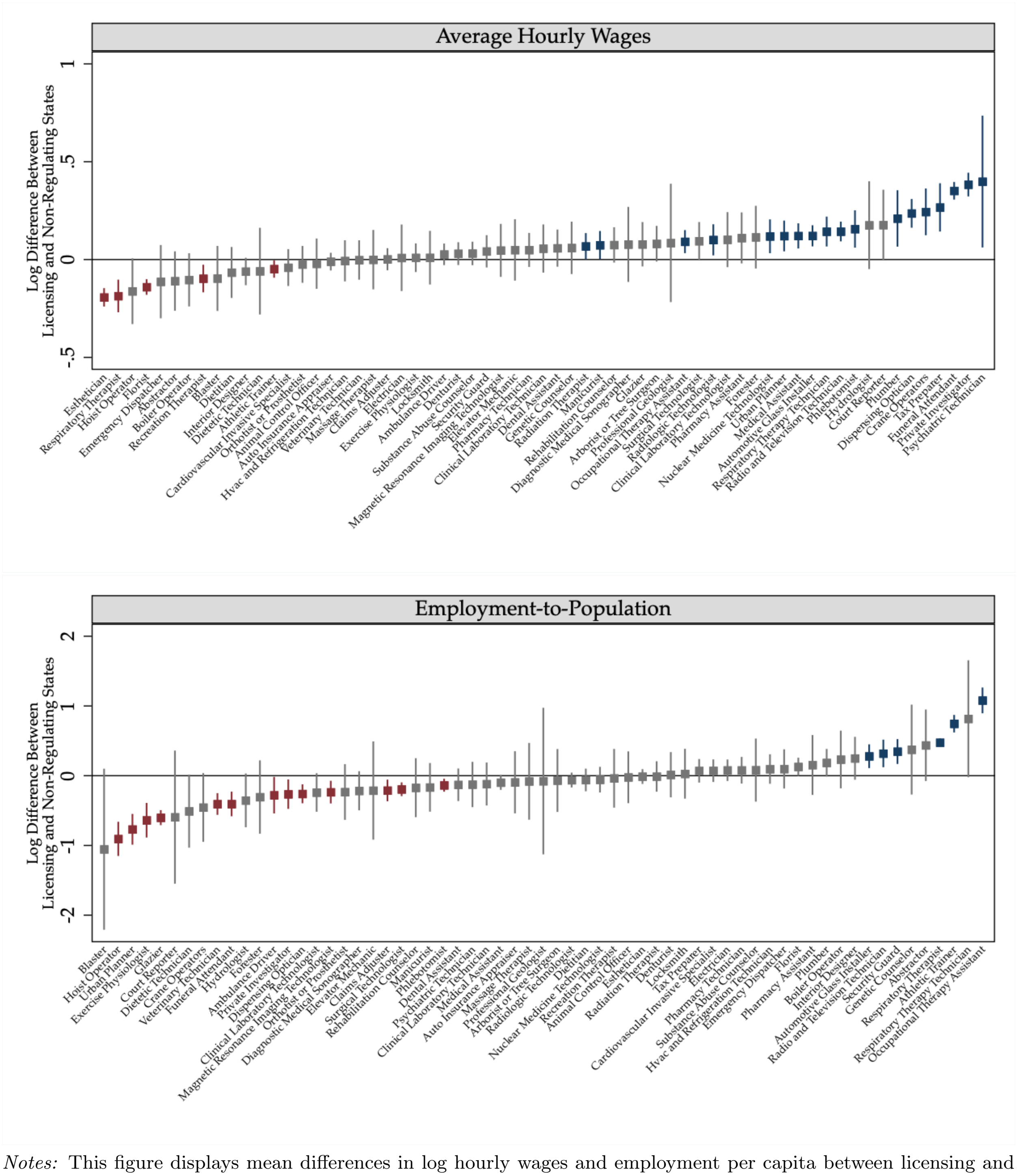


(b) High School or Less



Notes: This figure reports event study estimates for the effect of licensing on the average age of workers. The upper panel reports results for jobs that typically require some college or more, while the bottom panel reports results for jobs typically requiring a high school education or less based on data from O*Net. The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. Treatment events occur at the six-digit level and are aggregated using the procedure described in the text. All coefficients are normalized relative to the period one to three years prior to treatment. The two-way fixed effect regression contains occupation-state and occupation-year fixed effects specific to each of the nine Census divisions. The preferred specification additionally includes state-year fixed effects specific to six broad occupation groups. The shaded area denotes 95% confidence intervals based on standard errors clustered by state for the preferred specification.

Appendix Figure A8: Cross-Sectional Differences in Earnings and Employment (Occupational Employment Statistics State Sample 2015)



Notes: This figure displays mean differences in log hourly wages and employment per capita between licensing and non-regulating states using data from the 2015 Occupational Employment Statistics sample.

Appendix Figure A9: Dynamic Response of Industry Employment to Licensing Events (Quarterly Census of Employment and Wages 1990-2018)

Notes: This figure displays event study estimates for the effect of licensing, certification, and registration requirements on industry-level employment using data from the Quarterly Census of Employment and Wages. Treatment events and outcome variables are observed at the level of six-digit NAICS codes. The dependent variable is the log of industry employment per-capita. All regressions estimate the impact of licensing, certification, and registration policies simultaneously, include state and census division by year fixed effects, and are weighted by total employment in each state-year cell. All coefficients are normalized relative to the two years prior to treatment. Dashed lines denote 95% confidence intervals based on standard errors clustered by state.

Appendix Table A1: Source and Characteristics of Worker Credentials (Survey of Income and Program Participation 2008 Panel, Wave 13)

	Organization Issuing Professional Certification or License				
Panel A: Credential Attainment	All Sources (1)	Federal Government (2)	State Government (3)	Local Government (4)	Private Organization (5)
Share of Respondents Holding Credential	25.92	1.38	16.57	0.60	7.22
Number of Respondents Holding Credential	6,196	329	3,954	144	1,722
Total Observations	23,906	23,906	23,906	23,906	23,906
Panel B: Share of Workers Reporting Requirements to Obtain Credential					
Coursework or Training	92.48	92.46	93.06	83.72	91.96
Examination or Skill Demonstration	91.69	91.67	92.41	78.71	91.14
Continuing Education or Testing	68.01	66.81	72.45	48.42	60.03
Any Demonstration of Competency	97.48	97.59	97.60	89.60	97.82
Panel C: Share of Workers Reporting Reason for Holding Credential					
Required for Current Job	75.52	69.42	81.32	76.70	63.53
Mainly Obtained for Work-Related Reason	95.90	93.96	96.65	92.46	94.83
Mainly Obtained for Personal Interest	4.10	6.04	3.35	7.54	5.17

Notes: This table uses data from the professional licensing, certification, and educational certificates topical module conducted during Wave 13 of the 2008 Survey of Income and Program Participation panel. Interviews were administered between August and November 2012. The sample is limited to adults between the ages of 16 and 64 who participated in the topical module and whose responses are self-reported. Private organizations include professional associations, businesses or non-profits, industry groups, and all other non-governmental sources. Any demonstration of competency indicates that at least one of the listed requirements is needed to obtain or maintain the worker's credential. Averages are computed using the sampling weights provided in the SIPP.

Appendix Table A2: Summary Statistics for a Subset of Licensed Occupations with Recent Policy Changes (State Legislation and Occupational Employment Statistics as of 2015)

	States Regulating (1)	States Licensing (2)	Median Year of Licensing (3)	Average Education Level (4)	Average Hourly Wage (5)	Total Employment (6)
Tractor-Trailer Truck Driver	50	50	1977	High School	20.43	1,678,280
Nursing Assistant	50	50	1990	High School	12.89	1,420,570
Certified Public Accountant	50	46	1956	Bachelor's	36.19	1,226,910
Security Guard	33	27	1984	High School	13.68	1,097,660
Plumber	45	37	1950	High School	26.49	391,680
Claims Adjuster	34	34	1958	Some College	30.91	271,600
Emergency Medical Technician	50	50	1975	Some College	17.04	236,890
Physical Therapist	50	50	1959	Master's	41.25	209,690
Radiologic Technologist	41	41	1987	Some College	28.13	195,590
Bus Driver, Transit	50	50	1977	High School	19.31	168,620
Clinical Laboratory Technologist	10	10	1978	Bachelor's	29.74	162,950
Clinical Laboratory Technician	10	10	1978	Bachelor's	19.91	157,610
Speech-Language Pathologist	50	50	1976	Master's	36.97	131,450
Professional Counselor	50	45	1993	Bachelor's	21.67	128,200
Respiratory Therapist	49	49	1992	Some College	28.67	120,330
Phlebotomist	4	4	2001	High School	15.76	118,160
Occupational Therapist	50	50	1984	Bachelor's	39.27	114,660
Physician Assistant	50	50	1973	Bachelor's	47.73	98,470
Veterinary Technician	37	37	1976	Some College	16.00	95,790
Architect	50	50	1933	Bachelor's	39.83	93,720
Massage Therapist	45	42	1996	High School	20.76	92,090
Substance Abuse Counselor	40	33	1998	Bachelor's	20.64	87,090
Manicurist	49	49	1935	High School	11.36	83,840
Dispensing Optician	23	21	1955	Some College	17.70	73,520
Diagnostic Medical Sonographer	3	3	2012	Bachelor's	34.08	61,250
Dietitian	47	27	1992	Bachelor's	28.08	59,740
Psychiatric Technician	5	5	1970	Some College	17.44	58,450
Abstractor	10	7	1936	High School	23.96	54,620
Interior Designer	25	3	1997	Bachelor's	26.69	51,050
Glazier	1	1	2000	High School	21.84	44,230
Funeral Director	50	49	1936	Bachelor's	29.19	42,500
Occupational Therapy Assistant	49	49	1985	Some College	28.05	35,460
Magnetic Resonance Imaging Technologist	3	3	2013	Some College	32.86	33,460
Marriage and Family Therapist	50	44	1996	Master's	25.73	32,070
Professional Geologist	31	29	1994	Master's	50.83	31,800
Private Investigator	45	43	1970	Some College	25.41	30,460
Dietetic Technician	4	2	1989	High School	14.03	28,950
Landscape Architect	50	46	1987	Bachelor's	32.98	19,820
Nuclear Medicine Technologist	37	37	1994	Some College	36.06	19,740
Recreation Therapist	6	4	2006	Bachelor's	22.98	17,880
Locksmith	15	9	2006	High School	19.84	17,800
Automotive Glass Installer	1	1	2001	High School	16.93	17,160
Radiation Therapist	37	37	1990	Some College	40.61	16,930
Animal Control Officer	7	7	1992	High School	16.98	13,180
Audiologist	50	50	1976	Professional	37.22	12,070
Forester	15	11	1974	Bachelor's	29.16	8,590
Orthotist or Prosthetist	16	14	2003	Bachelor's	33.63	7,100
Exercise Physiologist	1	1	1996	Bachelor's	23.91	6,620
Hearing Aid Fitter	50	50	1972	Some College	25.41	5,920
Genetic Counselor	20	17	2013	Master's	35.85	2,520

Notes: This table provides summary statistics for a subset of fifty major occupations that experienced at least one policy change after 1980 (i.e. those that potentially contribute identifying variation in the main analysis). Columns one to three use data collected from state statutes, regulations, and historical session laws. Column four uses data on educational requirements from O*Net Online, the successor of the Dictionary of Occupation Titles. Columns five and six use national data from the Occupational Employment Statistics program.

Appendix Table A3: Dynamic Response of Occupational Employment to Licensing Events (State-Occupation Aggregated Census 1980-2000 and ACS 2001-2017)

	Dependent Variable: Log Employment-to-Population Ratio					
Cumulative Treatment Effects	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Treatment Effect	0.026	0.021	0.033*	0.040**	0.038**	0.041**
$\hat{\beta}_{PT}$: Years -10 to -3	(0.017)	(0.016)	(0.017)	(0.017)	(0.017)	(0.019)
Short-Run Effect	0.011	0.011	0.005	0.016	0.016	0.011
$\hat{\beta}_{SR}$: Years 0 to 8	(0.013)	(0.015)	(0.014)	(0.021)	(0.022)	(0.025)
Medium-Run Effect	0.048***	0.045***	0.012	0.040*	0.042*	0.015
$\hat{\beta}_{MR}$: Years 9 to 17	(0.015)	(0.015)	(0.018)	(0.021)	(0.022)	(0.026)
Long-Run Effect	0.062***	0.061***	0.019	0.054*	0.053*	0.017
$\hat{\beta}_{LR}$: Years 18 to 25	(0.022)	(0.021)	(0.027)	(0.029)	(0.031)	(0.036)
Number of Events	1,103	1,103	1,103	1,066	1,066	1,066
Sample Observations	243,006	243,006	243,006	241,744	241,744	241,744
Total Worker Observations	27,096,327	27,096,327	27,096,327	27,002,162	27,002,162	27,002,162
Occupation-Year FE	✓	✓	✓	✓	✓	✓
Occupation-State FE	✓	✓	✓	✓	✓	✓
Policy Controls		✓	✓		✓	✓
Occupation Group-State-Year FE			✓			✓
Occupation-Division-Year FE				✓	✓	✓

Notes: This table reports binned averages of event study estimates for the effect of licensing on employment by occupation.The estimation sample is an aggregated state-occupation panel constructed from the 1980-2000 Census and 2001-2017 American Community Survey. Treatment events occur at the six-digit level and are aggregated using the procedure explained in the text. These regressions use variation from all available licensing events in the data, including those with overlapping event windows and units treated before the outcome window. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation-year cell. All estimates are expressed relative to the leave-out category of 1 to 3 years prior to treatment. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

Appendix Table A4: Robustness of Preferred Earnings Estimates to Alternative Treatment Aggregation Decisions (State-Occupation Aggregated CPS Panel 1983-2018)

	Dependent Variable: Log Average Hourly Wage								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pre-Treatment Effect	0.016	0.012	0.021**	0.010	0.016	0.019*	0.010	0.015	0.019*
$\hat{\beta}_{PT}$: Years -10 to -3	(0.012)	(0.012)	(0.010)	(0.012)	(0.012)	(0.010)	(0.012)	(0.012)	(0.010)
Short-Run Effect	0.026**	0.029**	0.033***	0.020	0.027**	0.033***	0.021	0.027**	0.031***
$\hat{\beta}_{SR}$: Years 0 to 8	(0.013)	(0.012)	(0.011)	(0.012)	(0.012)	(0.011)	(0.013)	(0.012)	(0.012)
Medium-Run Effect	0.053***	0.060***	0.068***	0.040**	0.051***	0.067***	0.041**	0.051***	0.065***
$\hat{\beta}_{MR}$: Years 9 to 17	(0.017)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)	(0.016)	(0.017)
Long-Run Effect	0.069***	0.081***	0.093***	0.050**	0.069***	0.092***	0.052**	0.069***	0.091***
$\hat{\beta}_{LR}$: Years 18 to 26	(0.023)	(0.021)	(0.020)	(0.022)	(0.021)	(0.020)	(0.023)	(0.022)	(0.020)
Number of Events	1,360	1,056	867	1,428	1,109	807	1,427	1,149	817
Sample Observations	337,513	340,621	343,345	334,466	339,660	344,742	334,392	340,658	344,689
Total Worker Observations	19,480,896	21,618,555	21,943,168	19,396,662	21,618,794	21,958,198	19,380,075	21,672,842	21,957,894
Occupation-Division-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Occupation-State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Policy Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Occupation Group-State-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year of Estimated Employment Share	1980	1980	1980	2000	2000	2000	2015	2015	2015
Coverage Threshold for Event Sample	None	20%	50%	None	20%	50%	None	20%	50%

Notes: This table replicates my preferred event study specification for hourly wages using alternative parameters for treatment aggregation (Equation 2). The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. Treatment events occur at the six-digit level and are aggregated using the procedure explained in the text under the assumptions shown in the bottom rows of the table. Column five repeats the specification presented in the main text for reference. These regressions use variation from all available licensing events in the data, including those with overlapping event windows and units treated before the outcome window. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation-year cell. All estimates are expressed relative to the leave-out category of 1 to 3 years prior to treatment. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

Appendix Table A5: Robustness of Preferred Employment Estimates to Alternative Treatment Aggregation Decisions (State-Occupation Aggregated CPS Panel 1983-2018)

	Dependent Variable: Log Employment-to-Population Ratio								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pre-Treatment Effect	-0.012	-0.013	-0.013	0.001	-0.001	-0.012	0.003	0.002	-0.013
$\hat{\beta}_{PT}$: Years -10 to -3	(0.023)	(0.024)	(0.028)	(0.024)	(0.022)	(0.027)	(0.023)	(0.022)	(0.026)
Short-Run Effect	-0.009	-0.005	0.004	-0.003	0.002	0.006	-0.003	0.002	0.003
$\hat{\beta}_{SR}$: Years 0 to 8	(0.019)	(0.020)	(0.024)	(0.021)	(0.020)	(0.024)	(0.021)	(0.020)	(0.024)
Medium-Run Effect	0.023	0.026	0.037	0.026	0.036	0.043	0.025	0.035	0.038
$\hat{\beta}_{MR}$: Years 9 to 17	(0.027)	(0.028)	(0.029)	(0.032)	(0.029)	(0.028)	(0.032)	(0.030)	(0.029)
Long-Run Effect	0.039	0.040	0.056	0.035	0.044	0.061	0.032	0.040	0.053
$\hat{\beta}_{LR}$: Years 18 to 26	(0.036)	(0.034)	(0.040)	(0.042)	(0.034)	(0.038)	(0.040)	(0.035)	(0.039)
Number of Events	1,394	1,089	900	1,462	1,145	842	1,461	1,185	852
Sample Observations	404,599	408,839	411,731	400,691	406,459	413,565	400,617	407,468	413,476
Total Worker Observations	19,845,802	21,993,173	22,317,632	19,757,374	21,982,018	22,333,803	19,740,787	22,036,206	22,333,422
Occupation-Division-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Occupation-State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Policy Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Occupation Group-State-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year of Estimated Employment Share	1980	1980	1980	2000	2000	2000	2015	2015	2015
Coverage Threshold for Event Sample	None	20%	50%	None	20%	50%	None	20%	50%

Notes: This table replicates my preferred event study specification for employment using alternative parameters for treatment aggregation (Equation 2). The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. Treatment events occur at the six-digit level and are aggregated using the procedure explained in the text under the assumptions shown in the bottom rows of the table. Column five repeats the specification presented in the main text for reference. These regressions use variation from all available licensing events in the data, including those with overlapping event windows and units treated before the outcome window. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation-year cell. All estimates are expressed relative to the leave-out category of 1 to 3 years prior to treatment. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

Appendix Table A6: Impact of Licensing on Hourly Wages with Demographic and Compositional Controls (State-Occupation Aggregated CPS Panel 1983-2018)

	Dependent Variable: Log Average Hourly Wage					
Cumulative Treatment Effects	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Treatment Effect	0.017	0.016	0.015	0.017	0.017	0.016
$\hat{\beta}_{PT}$: Years -10 to -3	(0.014)	(0.013)	(0.013)	(0.012)	(0.012)	(0.011)
Short-Run Effect	0.020*	0.020*	0.021**	0.024**	0.024**	0.024**
$\hat{\beta}_{SR}$: Years 0 to 8	(0.011)	(0.011)	(0.010)	(0.011)	(0.010)	(0.009)
Medium-Run Effect	0.041***	0.041***	0.044***	0.046***	0.044***	0.045***
$\hat{\beta}_{MR}$: Years 9 to 17	(0.015)	(0.015)	(0.015)	(0.014)	(0.013)	(0.012)
Long-Run Effect	0.059***	0.059***	0.063***	0.060***	0.058***	0.058***
$\hat{\beta}_{LR}$: Years 18 to 25	(0.019)	(0.019)	(0.019)	(0.019)	(0.017)	(0.017)
Number of Events	1,125	1,125	1,123	1,109	1,109	1,107
Sample Observations	339,660	339,660	338,640	339,660	339,660	338,640
Total Worker Observations	21,618,794	21,618,794	21,611,000	21,618,794	21,618,794	21,611,000
Occupation-Division-Year FE	✓	✓	✓	✓	✓	✓
Occupation-State FE	✓	✓	✓	✓	✓	✓
Education and Potential Experience	✓	✓	✓	✓	✓	✓
Demographic Composition		✓	✓		✓	✓
Class of Worker and Union Coverage			✓			✓
Occupation Group-State-Year FE				✓	✓	✓

Notes: This table reports binned averages of event study estimates for the effect of licensing on hourly wages. The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. Treatment events occur at the six-digit level and are aggregated using the procedure explained in the text. These regressions use variation from all available licensing events in the data, including those with overlapping event windows and units treated before the outcome window. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation-year cell. All estimates are expressed relative to the leave-out category of 1 to 3 years prior to treatment. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

Appendix Table A7: Response of Hourly Wage Distribution to Licensing Events (Occupational Employment Statistics State Panel 1999-2018)

	Dependent Variable: Log Hourly Wage at Percentile				
Cumulative Treatment Effects	(1)	(2)	(3)	(4)	(5)
Pre-Treatment Effect	0.002	0.006	-0.000	-0.003	-0.008
$\hat{\beta}_{PT}$: Years -10 to -2	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)
Short-Run Effect	0.011**	0.016***	0.010*	0.001	-0.007
$\hat{\beta}_{SR}$: Years 0 to 8	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Medium-Run Effect	0.040***	0.044***	0.037***	0.016**	-0.005
$\hat{\beta}_{MR}$: Years 9 to 17	(0.012)	(0.012)	(0.011)	(0.008)	(0.011)
Long-Run Effect	0.054***	0.058***	0.062***	0.041***	0.012
$\hat{\beta}_{LR}$: Years 18 to 25	(0.019)	(0.018)	(0.022)	(0.015)	(0.016)
Wage Percentile	10th	25th	50th	75th	90th
Number of Events	1,429	1,429	1,429	1,429	1,429
Sample Observations	530,908	530,908	530,908	530,908	530,908
Occupation-Year FE	✓	✓	✓	✓	✓
Occupation-State FE	✓	✓	✓	✓	✓
Policy Controls	✓	✓	✓	✓	✓

Notes: This table displays event study estimates for the impact of licensing on the distribution of houlry wages using data from the Occupational Employment Statistics program. In this sample, treatment and outcome variables are observed at the six-digit level. All coefficients are normalized relative to the year prior to the statutory effective date of the law. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation-year cell. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

Appendix B: Data Appendix

This appendix provides additional details about the earnings and employment data used in this project. Section one describes how revisions to the various occupational coding systems used by government agencies are harmonized to create occupation groups that are consistently identifiable over time. Section two documents the processing of my outcome variables.

B.1 Consistent Occupation Codes

Standard Occupational Classification. The Standard Occupational Classification (SOC) system defines over 800 occupation categories, and is the most detailed level at which the federal government reports wage and employment estimates. The data used in this project span two vintages of these codes, introduced first in 2000 and then revised in 2010. Although 90% of occupations are directly comparable across these systems, the rest must be aggregated to construct a balanced panel. In cases where existing codes were split or combined, the changes are harmonized by simply recombining the more detailed components. If instead, a new code was broken out from two or more existing ones, a single category is created out of all occupations with overlapping coverage.[1] This results in a balanced set of 788 non-military Standard Occupational Classification codes that are comparable in data coded using either the 2000 or 2010 systems. Aggregated codes and their original components are tabulated in Appendix Table B1.

Census Occupational Classifications. Census occupation codes are less detailed than the Standard Occupational Classification system, and are revised every decade. In addition, some microdata released through IPUMS further aggregates small occupations when disclosure requirements are not met, resulting in additional changes to occupation codes implemented in the 2005 and 2012 American Community Survey. To harmonize the Census classifications, I begin with the "occ1990dd" system developed by Dorn (2009), who provides a balanced panel of occupations valid in the 1980 to 2000 Census samples and the ACS from 2005 to 2009.[2] Extending this system to cover the period from 2010 to 2017 requires additional aggregations, which reduce the total number of non-military occupation codes from 330 to 310. As shown in Appendix Table B2, these aggregations mostly affect manufacturing jobs and therefore have little impact on the licensed occupations studied in this project.

B.2 Processing of Wage and Employment Data

Current Population Survey Outgoing Rotation Groups. I use data from the IPUMS Current Population Survey outgoing rotation group extracts from 1983 to 2018 (Flood et al., 2018). The sample is limited to adults ages 16 to 64 in the civilian labor force, excluding unpaid family workers. I process this sample following the replication materials provided by Acemoglu and Autor (2011), with the exception of minor differences in the construction of hourly wages.

[1] Changes resulting from the introduction of new occupation codes for transportation security screeners and solar photovoltaic installers are not completely harmonized. This is because doing so would require combining several occupations across major groups in a way that significantly affects the implied regulation coverage of jobs such as electricians and security guards. Instead, I assign these occupations to the main categories they were broken out from, which are "compliance officers" and "construction and related workers, all other," respectively. Since total employment in the new categories is extremely small relative to the original classifications they were split from, the impact on employment share estimates in these occupations is negligible.

[2] The occupation codes used in the 2001 to 2004 ACS provide slightly more detail than the 2005 classification, but can be directly aggregated to match the latter.

Usual hourly wages for workers paid by the hour are measured as the reported straight-time hourly wage, excluding overtime, tips, and commissions. For non-hourly workers, I construct the implied hourly wage by dividing weekly earnings by usual weekly hours. If usual weekly hours are missing or variable, actual hours worked in the previous week are used. Top coded earnings are multiplied by a factor of 1.5. Earnings are windsorized at half the federal minimum wage on the lower tail and the implied hourly wage corresponding to 1.5 times the maximum weekly earnings for a full-time hourly worker on the upper tail. Wages and earnings are adjusted to 2018 dollars using the CPI-U-RS.

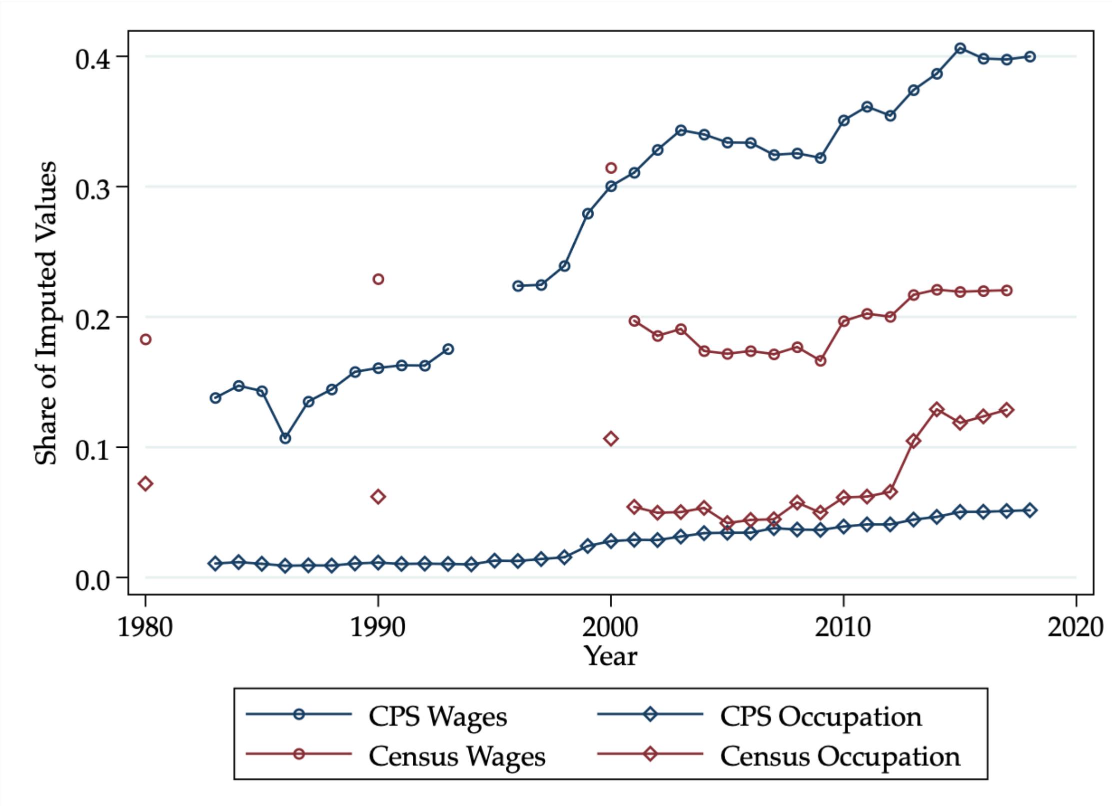
Observations with allocated earnings or occupations are excluded from all analysis using these variables. A flag for imputed occupations is consistently available throughout the CPS sample, but the method for identifying wage imputations varies by year. With the exception of 1989 to 1995, wage imputations are identified with positive allocation values for weekly earnings, hourly wages, or hours worked. Between 1989 and 1993, however, these flags are incomplete, and wage imputations are therefore identified as observations with positive edited earnings, wages, or hours, but missing unedited versions of these variables (Hirsch & Schumacher, 2004). As no method is known to identify wage imputations in 1994 and 1995, I retain all observations from these years in the main sample, but exclude these years in robustness checks.

Census and American Community Survey. I use data from the IPUMS Census extracts from 1980 to 2000 and the 2001-2017 American Community Survey (Ruggles et al., 2019). The sample is limited to civilian adults between the ages of 16 and 64 who are not living in group quarters. Demographic and education variables are processed following Acemoglu and Autor (2011). Hourly wages are constructed by dividing annual wage and salary income by the product of weeks worked and usual weekly hours. Top coded incomes are multiplied by a factor of 1.5, and implied hourly wages are truncated at 1.5 times the maximum feasible income for a full time worker given the top code. I then windsorize the bottom 1% of hourly wages in each year.

I also create a measure of hourly earnings that includes self-employment income by first applying the top coding rule to business income, then dividing the sum of wage and business income by hours of labor supply. Hourly earnings are windsorized below the first percentile of the hourly wage distribution for workers with positive wage and salary income, and below the first percentile of hourly earnings for those with business income only. The upper tail of hourly earnings is windsorized at the 99th percentile for workers with positive business income, and the adjusted top code of hourly wages otherwise. The hourly wage and earnings variables are therefore equivalent for workers with no business income. Observations with imputed occupation, wage, or earnings are excluded from any analysis that requires these variables.

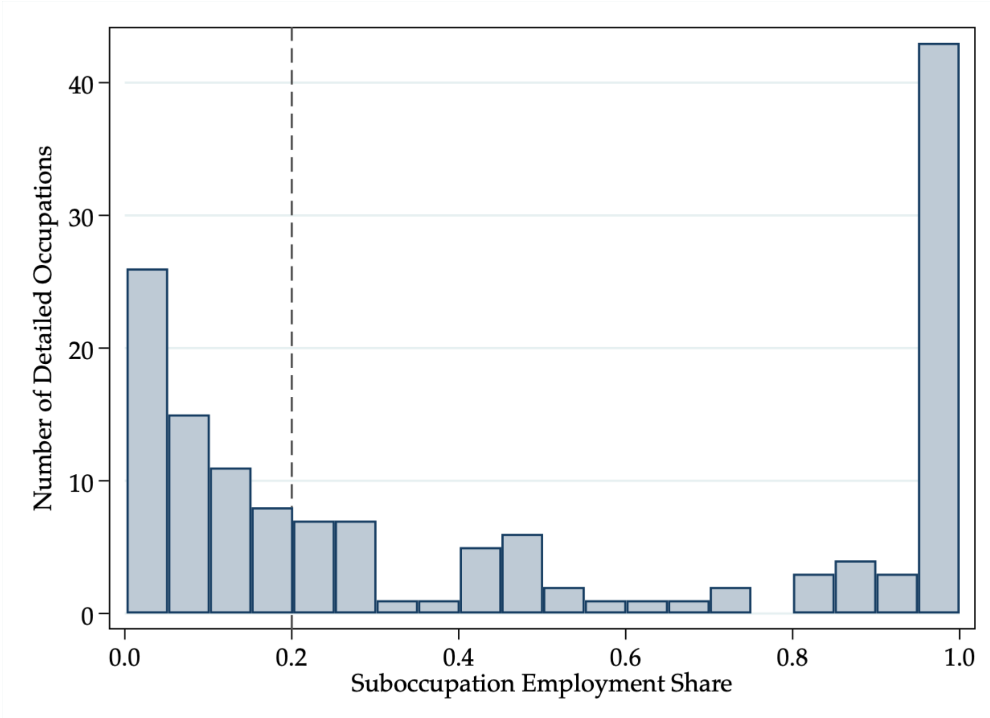
Occupational Employment Statistics. In addition to the individual-level surveys, I also use data from the Occupational Employment Statistics program, which provides wage and employment estimates by state and six-digit Standard Occupational Classification code for most occupations between 2000 and 2018. This data is based on three-year rolling averages of semi-annual business establishment surveys, and therefore excludes self-employed workers. In cases where only annual earnings are reported, hourly wages are constructed by dividing earnings by 2,080, which is the definition of full-time hours of labor supply used in the survey. Top coded wages are then multiplied by a factor of 1.5. Due to revisions to the Standard Occupational Classification system in 2010, I aggregate a small number of occupations to consistent definitions using the crosswalk described above in order to achieve a balanced panel. Hourly wages for aggregated occupations are constructed as employment-weighted averages across suboccupations.

Appendix Figure B1: Wage and Occupation Imputation Rates in the American Community Survey, Census, and Current Population Survey



Notes: This figure displays the share of observations in each of the main survey data sets used in the project that have imputed wage or occupation variables. These observations are dropped prior to any analysis using these variables.

Appendix Figure B2: Distribution of Employment Share Estimates for Regulated Suboccupations (Occupational Employment Statistics 2000)



Notes: This figure displays the distribution of weights, $\hat{\pi}_{jk}$ used to construct the aggregated policy variables described in Section 4.2. These weights measure the share of each consistent three-digit Census occupation's total national employment covered by its disaggregated six-digit components.

Appendix Table B1: Construction of Consistent Standard Occupational Classification Codes

	Original Components	
Aggregated Occupation Title	2000 SOC Codes	2010 SOC Codes
Farmers, Ranchers, and Other Agricultural Managers	11-9011, 11-9012	11-9013
Funeral Directors	11-9061	11-9061, 39-4031
Compliance Officers	13-1041	13-1041, 33-9093
Human Resources, Training, and Labor Relations Specialists, All Other	13-1071, 13-1079	13-1071, 13-1075
Business Operations, Marketing, and Public Relations Specialists	13-1121, 13-1199, 19-3021 27-3031	13-1121, 13-1161, 13-1199 27-3031
Computer Systems Analysts and Computer Scientists	15-1051, 15-1071, 15-1081	15-1121, 15-1122, 15-1134, 15-1142, 15-1143, 15-1152
Community and Social Service Specialists, All Other	21-1091, 21-1099	21-1091, 21-1094, 21-1099
Paralegals, Legal Assistants and Law Clerks	23-2011, 23-2092	23-1012, 23-2011
Special Education Teachers, Preschool, Kindergarten, and Elementary School	25-2041	25-2051, 25-2052
Teachers and Instructors, All Other	25-3099	25-2059, 25-3099
Registered Nurses	29-1111	29-1141, 29-1151, 29-1161 29-1171
Therapists, All Other	29-1129	29-1128, 29-1129
Radiologic Technologists and Technicians	29-2034	29-2034, 29-2035
Health Technologists and Technicians, All Other	29-2099	29-2057, 29-2092 , 29-2099
Healthcare Practitioners and Technical Workers, All Other	29-9099	29-9092, 29-9099
Nursing Aides, Orderlies, and Attendants	31-1012	31-1014, 31-1015
Healthcare Support Workers, All Other	31-9099	31-9097, 31-9099
Sales and Related Workers, All Other	41-9099	13-1131, 41-9099
Office and Administrative Support Workers, All Other	43-9199	43-3099, 43-9199
Construction and Related Workers, All Other	47-4099	47-2231, 47-4099
Installation, Maintenance, and Repair Workers, All Other	49-9099	49-9081, 49-9099
Printing and Binding Workers	51-5011, 51-5012, 51-5021 51-5023	51-5112, 51-5113
Photographic Process Workers and Processing Machine Operators	51-9131, 51-9132	51-9151
Production Workers, All Other	51-9199	51-3099, 51-9199

Notes: This table lists aggregations of the Standard Occupational Classification system that are made to bridge revisions to occupation definitions adopted in 2010 and create a balanced panel of codes with consistent longitudinal coverage. Codes that do not appear in this table are already comparable in the 2000 and 2010 SOC systems.

Appendix Table B2: Aggregations Extending the Occ1990dd Balanced Occupation Panel to Cover 2010 Census Codes

Aggregated Occupation Title	Occ1990dd Codes
Managers, administrators, and researchers, n.e.c.	13, 22, 37, 166
Health technologists and technicians, n.e.c.	208, 678
Farmers (managers, owners, and tenants)	473, 475
Masons, tilers, carpet, and structural metal workers	563, 597
Painters and paperhangers	579, 583
Model makers, patternmakers, and molding machine setters	645, 719
Other precision and craft workers	644, 684, 703, 706, 708, 709, 723, 724
Shoe and leather workers	669, 745
Printers, binders, and typesetters	679, 734, 736
Miscellaneous production workers	764, 779
Subway, streetcar, and other rail transportation workers	824, 825

Notes: This table lists aggregations of the occ1990dd classification system that are made to extend the balanced occupation panel to cover revisisons to Census occupation definitions adopted after 2010. No changes are necessary for codes that do not appear in this table.

Appendix Table B3: Impact of Licensing Events on Wage and Occupation Imputation Rate (State-Occupation Aggregated CPS Panel 1983-2018)

	Hourly Wage Imputation Rate			Occupation Imputation Rate		
Cumulative Treatment Effects	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Treatment Effect	-0.014	-0.010	-0.006	0.003	-0.001	-0.000
$\hat{\beta}_{PT}$: Years -10 to -3	(0.010)	(0.009)	(0.009)	(0.003)	(0.003)	(0.003)
Short-Run Effect	0.005	0.009	0.002	0.000	-0.002	-0.002
$\hat{\beta}_{SR}$: Years 0 to 8	(0.008)	(0.009)	(0.009)	(0.003)	(0.003)	(0.003)
Medium-Run Effect	0.009	0.007	-0.004	-0.001	-0.004	-0.005
$\hat{\beta}_{MR}$: Years 9 to 17	(0.012)	(0.011)	(0.010)	(0.004)	(0.003)	(0.005)
Long-Run Effect	0.005	0.003	-0.016	-0.005	-0.006	-0.006
$\hat{\beta}_{LR}$: Years 18 to 25	(0.015)	(0.012)	(0.010)	(0.005)	(0.005)	(0.007)
Number of Events	1,172	1,123	1,123	1,172	1,123	1,123
Sample Observations	369,929	362,919	362,919	370,024	363,028	363,028
Total Worker Observations	21,943,216	21,838,698	21,838,698	21,944,842	21,840,577	21,840,577
Occupation-Year FE	✓	✓	✓	✓	✓	✓
Occupation-State FE	✓	✓	✓	✓	✓	✓
Policy Controls	✓	✓	✓	✓	✓	✓
Occupation-Division-Year FE		✓	✓		✓	✓
Occupation Group-State-Year FE			✓			✓

Notes: This table replicates the main event study specifications with the imputation rate for hourly wages and occupation as the dependent variables. The estimation sample is an aggregated state-occupation panel constructed from the 1983-2018 Current Population Survey and a balanced panel of three-digit occupation codes. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation- year cell, which is constructed using sample-weighted annual occupational employment share estimates from the full monthly CPS scaled by total employment. All estimates are expressed relative to the leave-out category of 1 to 3 years prior to treatment. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

Appendix Table B4: Impact of Licensing Events on Earnings and Occupation Imputation Rate (State-Occupation Aggregated Census 1980-2000 and ACS 2001-2017)

	Earned Income Imputation Rate			Occupation Imputation Rate		
Cumulative Treatment Effects	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Treatment Effect	-0.004	-0.007	-0.005	-0.006	-0.002	-0.003
$\hat{\beta}_{PT}$: Years -10 to -3	(0.006)	(0.007)	(0.005)	(0.004)	(0.004)	(0.003)
Short-Run Effect	0.001	-0.002	-0.005	-0.002	0.003	0.002
$\hat{\beta}_{SR}$: Years 0 to 8	(0.004)	(0.006)	(0.005)	(0.003)	(0.003)	(0.002)
Medium-Run Effect	0.006	0.002	-0.005	-0.003	-0.001	-0.002
$\hat{\beta}_{MR}$: Years 9 to 17	(0.005)	(0.008)	(0.006)	(0.004)	(0.004)	(0.003)
Long-Run Effect	0.008	0.008	-0.002	-0.004	-0.001	-0.002
$\hat{\beta}_{LR}$: Years 18 to 25	(0.007)	(0.010)	(0.007)	(0.005)	(0.005)	(0.004)
Number of Events	1,103	1,072	1,072	1,103	1,072	1,072
Sample Observations	242,875	241,593	241,593	243,126	241,858	241,858
Total Worker Observations	27,140,075	27,046,126	27,046,126	27,140,337	27,046,426	27,046,426
Occupation-Year FE	✓	✓	✓	✓	✓	✓
Occupation-State FE	✓	✓	✓	✓	✓	✓
Policy Controls	✓	✓	✓	✓	✓	✓
Occupation-Division-Year FE		✓	✓		✓	✓
Occupation Group-State-Year FE			✓			✓

Notes: This table replicates the main event study specifications with the imputation rate for houlry wages and occupation as the dependent variables. The estimation sample is an aggregated state-occupation panel constructed from the 1980-2000 Census, and the 2001-2017 ACS and a balanced panel of three-digit occupation codes. Policy controls include additional distributed lags for six-digit registration and certification requirements, as well as occupation-specific controls for the number of minor (sub six-digit) regulations. The regressions are weighted by the number of workers in each state-occupation- year cell, which is constructed using sample-weighted annual occupational employment share estimates from the full monthly CPS scaled by total employment. All estimates are expressed relative to the leave-out category of 1 to 3 years prior to treatment. Standard errors are clustered by state. Significance levels are indicated by *** 1%; ** 5%; and * 10%.

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