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LICENSURE AS A CREDENTIAL: EVIDENCE ON UNEMPLOYMENT PROTECTION AND INEQUALITY FROM THE GREAT RECESSION AND COVID-19

Peter Q. Blair
Bobby W. Chung

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Licensure as a Credential: Evidence on Unemployment Protection and Inequality from the Great Recession and COVID-19
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ABSTRACT

Recent research shows that education shields workers from job loss in recessions. We test whether occupational licensing, another widespread credential, provides similar unemployment protection during recessions. Using individual-level licensing data and supplemental state-occupation measures, we study both the Great Recession and the COVID-19 recession. Licensed workers experienced a 0.8-1 percentage point (27%) smaller increase in unemployment than comparable unlicensed workers in both downturns. Ignoring these unemployment effects understates inequality between licensed and unlicensed workers by 3–7%. Our findings suggest that, like education, licensing not only raises wages but also protects jobs, thereby amplifying inequality during economic downturns.

Peter Q. Blair
Harvard University
Graduate School of Education
Department of Economics
and Stanford University (Hoover Institution)
and also NBER
peter_blair@gse.harvard.edu

Bobby W. Chung
University of South Florida
bobbywchung@usf.edu

1 Introduction

Occupational licensing is among the most pervasive labor market regulations in modern economies. In the United States and the European Union, roughly one in four workers is subject to laws that make it illegal to work for pay without a license (Gittleman et al., 2018; Koumenta and Pagliero, 2019). Importantly, licensing shares key features with educational credentials. Licenses often include formal training requirements, reflecting the human capital rationale of education (Becker, 1964; Gittleman et al., 2018; Koumenta and Pagliero, 2019), and they frequently screen workers based on criminal history, akin to the signaling role of degrees (Spence, 1973; Blair and Chung, Forthcoming). Recent evidence by Beuermann et al. (2024) shows that education not only raises wages but also protects workers from job loss during recessions. Motivated by this finding, we ask whether occupational licensing provides similar protection during economic downturns. If so, licensing may not only boost wages but also serve as a shield against unemployment risk during recessions, with important implications for growing earnings inequality between licensed and unlicensed workers.

Economists have long considered the institution of occupational licensure to be a contributing factor to inequality (Smith, 1776; Friedman and Kuznets, 1945; Friedman, 1962; Leland, 1979; Maurizi, 1974; Shapiro, 1986).[1] The empirical evidence on the inequality caused by licensing, however, focuses on inequality in wages between licensed and unlicensed workers, mirroring the traditional focus on wages when studying inequality by education levels (Kleiner and Krueger, 2013; Pizzola and Tabarrok, 2017; Autor, 2014; Katz and Murphy, 1992). What is largely missing from the literature is an understanding of how licensing contributes to inequality by impacting the likelihood that a worker is employed (Kukaev and Timmons, 2024). Studying the extent to which licensing can

[1] For example, in *The Wealth of Nations*, Adam Smith, argued: "The policy of Europe, by obstructing the free circulation of labour and stock both from employment to employment, and from place to place, occasions in some cases a very inconvenient inequality in the whole of the advantages and disadvantages of their different employments."

cause inequality by creating a disparity in unemployment between licensed and unlicensed workers is important because we know from recent work on black-white inequality that ignoring differences in unemployment can result in underestimating both the level and time evolution of inequality between groups (Bayer and Charles, 2018).

In this paper, we provide the first estimates of the impact of occupational licensing on unemployment during recessions. We find that licensed workers experienced a 1 percentage point (27%) smaller increase in unemployment than unlicensed workers during both recessions, suggesting that our findings are not driven by the peculiarities of a given recession. Moreover, we show that the differential job protection experienced by licensed workers during recessions is not offset by a negative compensating earnings differential but is rather an augmenting ammenity that varies positively with the higher wages that licensed workers enjoy (Rosen, 1974; Mortensen, 2003; Lang and Majumdar, 2004; Sorkin, 2018). While the literature to-date has focused on wage inequality caused by licensing, we show that occupational licensing laws generate an additional source of inequality by reducing the extent to which licensed workers experience unemployment during times of economic uncertainty. Using a back-of-the-envelope calculation, we find that the inequality between licensed and unlicensed workers is 3%-7% higher than one would think based solely on the licensing wage premium.

To test the shielding effect of licensing during recessions, we leverage cross-state variation in licensing laws to compare the difference in unemployment between licensed and unlicensed workers in the same industry during each month in the two years before and after the COVID-19 recession and the Great Recession. In each recession, we have over 3 million worker-month observations. Our key outcome of interest is whether an individual reports being unemployed. We code an individual as being licensed if the individual reports having a government-issued occupational license that is required by their current job (if employed) or required by their previous job (if unemployed). Our key parameter of interest is the coefficient on the interaction term between the indicator for whether an individual has a license and an indicator for whether the observation comes from the period that follows the onset of the recession. The parameter measures how much occupational licensing differentially shields licensed workers from unemployment as compared to unlicensed workers during the recession as compared to non-recessionary times.[2] We show that our results are similar when we use the state-occupation definition of licensure, which mitigates against measurement error in individual self-reported licensure.

The key identifying assumptions for us to causally interpret our parameter measuring the shielding effect of licensing are: 1) the unemployment gap between unlicensed and licensed workers would have evolved similarly in the post-recession period as it had in the pre-recession period, had the recession not occurred, 2) the timing of the recession is uncorrelated with other treatments that could have shielded licensed workers from unemployment as compared to their unlicensed peers, and 3) our measure of pre-labor market ability is a valid proxy for controlling for selection. We test the first assumption by running a placebo test in which we split the two-year pre-period in half and re-estimate our model. We find no shielding effect for occupational licensing during this placebo recession. The second assumption seems plausible, given that the recession is a nationwide shock, whereas the licensing variation occurs at the state level. To test our third assumption, we show that our measure of average worker ability at the state-occupational level is economically meaningful for predicting wages, unemployment, license status, and college attainment, and our preferred treatment effects of licensing on unemployment during the recession are obtained by the specifications that control for pre-market ability.

Quantitatively, we find that licensing shields workers from a recession-induced increase in the unemployment rate of 0.82 percentage points (p.p.) during COVID-19 and 1.11 p.p. during the Great Recession. These effects are meaningful. To avoid a 1 p.p. increase in the state unemployment rate, Borgschulte and Martorell (2018) estimate that

[2] We control for a pre-labor market measure of worker ability, education, potential experience, demographics, regional time trends, interactions between worker ability, education, and union status, and the recession indicator, and fixed effects for industry, survey month, and state.

workers are willing to give up 1.5%-2% in earnings. Moreover, the 1 p.p. lower increase in unemployment represents 27% to 37% of the increase in the unemployment rate among unlicensed workers during the COVID-19 and the Great Recession. Occupational licensing has the strongest shielding effect from recession-induced unemployment in places where labor demand was hardest hit by the recession, as measured using industry-level Bartik shocks (Great Recession), and states that mandated lockdowns (COVID-19). In both recessions, licensing shields workers from layoffs and not voluntary quits, ruling out the possibility that the reduced job loss is caused by labor supply responses. Both pieces of evidence suggest are consistent with a labor demand story that supports employment for licensed workers during recession.

We find no evidence for a negative compensating differential, which economic theory would suggest (Rosen, 1974, 1986); the licensing earnings premium does not fall in the presence of the lower unemployment risk face by licensed workers as compared to unlicensed workers — ruling out a scenario in which licensed workers and firms agree to trade off lower unemployment for lower earnings.[3] A back-of-the-envelope calculation shows that overlooking the additional job-loss protection licensed workers receive during recessions would understate inequality between licensed and unlicensed workers by 3% to 7%—a magnitude comparable to the welfare gains estimated by Finkelstein et al. (2024) from the mortality impacts of the Great Recession.

To check the robustness of our main results, we conducted a series of auxiliary tests. We find that our results are not driven by a single industry, but are similar across all industries. The relative selection of workers into licensed occupations does not change in recession years as compared to non-recession years. Moreover, selection on unobservables would need to be implausibly large to explain our findings (Altonji et al., 2005; Oster, 2019).

[3]The reduction in job loss for licensed workers during recessions that we document does not appear to come at the expense of greater job loss for unlicensed workers in the same state-industry pair.

2 Data and Empirical Strategy

2.1 Employment and Licensing Data

The data used in the study are drawn from the monthly Current Population Survey (CPS). The CPS is a nationally representative survey of US workers with rich labor market and demographic information, including whether an individual is employed or unemployed. Equally important for our study, in 2015 the CPS became the first nationally representative survey to continuously record whether an individual has an occupational license. Prior to the CPS, a special module of the 2008 SIPP recorded a single cross-section mapping out which workers were licensed Gittleman et al. (2018).[4] One further advantage of using the CPS data is that it allows us to measure the licensure status of workers whether they are employed or unemployed. In the CPS, employed workers are asked if their current job requires a license and unemployed workers are asked if their previous job required a license. Having a measure of licensure for both employed and unemployed workers makes it possible for us to explore how licensure changes the probability of unemployment during recessions as compared to normal economic times.

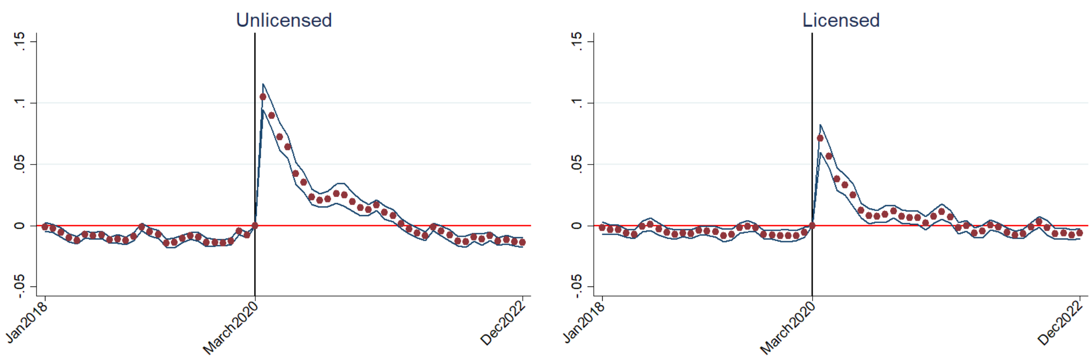
Our estimation sample consists of individuals between 18 and 65 who are in the labor force, excluding armed forces and unpaid family workers. For our analysis of the impact of licensing on unemployment during the COVID-19 recession, we use licensing and unemployment data from the CPS covering January 2018 to December 2022. For our analysis of the impact of licensing on unemployment during the Great Recession, we use unemployment data from the CPS data covering January 2006 to December 2010, and the licensing data from the 2008 SIPP and two other data sources. The monthly nature of the CPS, and the two-year pre-period and two-year post-period are useful for implementing our event study difference-in-differences research design.

Because we will use the Great Recession as a test of the external validity of our results

[4] There were also surveys conducted by Gallop and Westat those provided a snapshot of the prevalence of occupational licensing Kleiner and Krueger (2010, 2013).

from the COVID-19 recession, we focus on first describing the data for the COVID-19 portion of the study and defer describing the data for the Great Recession to Section 4. In Figure 1, we plot the unemployment rate for licensed and unlicensed workers in the two-year window before and after the COVID-19 recession relative to its value in the month just before COVID-19 hits. Unemployment spikes for licensed and unlicensed workers at the onset of the recession; however, it spikes more for unlicensed workers (12 p.p.) than for licensed workers (7 p.p.).

Figure 1: Event Study for Unemployment Rate of Worker during COVID-19



Data: IPUMS Monthly Current Population Survey (2018 - 2022). *Note:* Sample includes individuals between 18 and 65 who are in the labor force. We plot the probability of unemployment for unlicensed workers and licensed workers separately, conditional on basic characteristics (age, race, gender, education), for each time period in the two-year window around the COVID-19 recession. 'Licensed' refers to workers who possess a government-issued occupational license required by current/previous job. Sample weights apply. Standard errors are clustered at the state level.

In Table 1 we report the average unemployment rate separately for licensed and unlicensed workers before COVID-19 and after COVID-19. We also calculate the difference in the unemployment rate between licensed and unlicensed workers in each time period and the difference in this difference, which measures the extent to which licensed workers experience less job loss than unlicensed workers during the recession. Likewise, we report pre-COVID-19 and post-COVID-19 means for the characteristics of workers, e.g., sex, age, race, college, separately for licensed workers and unlicensed workers. We also measure the difference in the means for each period and the difference in this difference, which measures how much selection into licensing changed during the recession. In the pre-COVID-19 period, we find that licensed workers are less likely to be unemployed by

2.84 p.p.; they also appear to be selected on each of the individual characteristics. Likewise, in the post-COVID-19 period we also find that licensed workers are less likely to be unemployed than unlicensed workers by 3.79 p.p. and they are also selected on each of the individual characteristics.

When we examine the difference-in-differences measure, licensed workers are on average 0.96 p.p. less likely to experience job loss during the recession than their unlicensed peers despite both categories of workers being more likely to be unemployed during the recession. Examining the difference-in-differences for the nine individual worker characteristics, we find that they are each economically small, and eight of the nine are statistically indistinguishable from zero. For example, we find that the gap in the fraction of workers with a bachelor's degree who are licensed versus those who are unlicensed drops by 0.26 p.p., relative to the pre-COVID-19 value. The difference-in-difference here represents less than 1% of the pre-COVID-19 mean of 33.5% of unlicensed workers with bachelor's degrees and is, moreover, statistically indistinguishable from zero. The one characteristic that has a statistically significant difference-in-difference is union membership. Licensed workers are 0.29 p.p. more likely to be union members (than unlicensed workers) during the recession than before, which represents an increase in unionization of 1% (26.8% of licensed workers are union members before COVID-19). The summary statistics in Table 1 make it intriguing to consider whether occupational licensing protects licensed workers from job loss during recessions above and beyond what could be explained by selection on observable worker characteristics.

Table 1: Average Unemployment and Demographics by License Status Before and After COVID-19 Recession

	Pre-COVID-19			Post-COVID-19			
	Unlicensed	Licensed	Difference	Unlicensed	Licensed	Difference	Diff-in-Diff
Outcome							
unemployed	0.043	0.015	-0.0284***	0.065	0.027	-0.0379***	-0.00956***
			(0.000893)			(0.00127)	(0.00102)
Individual characteristics							
female	0.452	0.555	0.103***	0.449	0.559	0.109***	0.00619
			(0.00377)			(0.00350)	(0.00377)
age	40.206	43.174	2.968***	40.272	43.250	2.978***	0.0105
			(0.0982)			(0.117)	(0.0728)
black	0.133	0.112	-0.0216***	0.134	0.113	-0.0209***	0.000714
			(0.00594)			(0.00568)	(0.00241)
Hispanic	0.199	0.112	-0.0864***	0.205	0.119	-0.0860***	0.000357
			(0.0147)			(0.0140)	(0.00298)
Asian	0.066	0.056	- 0.0103***	0.069	0.059	-0.00970***	0.000646
			(0.00268)			(0.00282)	(0.00184)
union membership	0.242	0.268	0.0259***	0.235	0.264	0.0288***	0.00290**
			(0.00209)			(0.00264)	(0.00115)
college	0.335	0.569	0.234***	0.358	0.590	0.231***	-0.00260
			(0.00653)			(0.00575)	(0.00373)
govt	0.108	0.248	0.140***	0.108	0.249	0.140***	0.000272
			(0.00596)			(0.00617)	(0.00322)
self employed	0.084	0.119	0.0346***	0.088	0.125	0.0363***	0.00177
			(0.00443)			(0.00516)	(0.00207)
Observations	1,196,258	290,593		1,229,125	303,485		

Data: Monthly CPS (Jan 2018 to Dec 2022)

Note: 'Licensed' refers to individuals who require a government-issued credential to work in the current job (the previous job if unemployed). 'Pre-COVID-19' refers the period before March 2022. Sample weight apples. Standard errors are clustered at the state level in testing differences.

2.2 Empirical Strategy

To test the hypothesis that individuals with occupational licenses experience less job loss during recessions than their unlicensed peers, ideally one would randomize the occupational license attainment of individuals before a recession and measure whether the gap in unemployment between licensed and unlicensed workers changes during the recession.[5] There are two challenges to implementing the ideal experiment. First, it is nearly impossible to forecast the timing of a recession. Second, randomly assigning licenses raises ethical concerns given the body of work showing that licensee workers earn a wage premium compared to their unlicensed peers (Kleiner and Krueger, 2010; Timmons and Thornton, 2010; Kleiner and Krueger, 2013; Pizzola and Tabarrok, 2017; Kleiner and Soltas, 2023; Blair and Chung, Forthcoming). Because we cannot foresee recessions or randomly assign occupational licenses to individuals, we make progress on the question animating this paper by leveraging a natural experiment.

Since occupation definitions are national, the choice of individual states to disagree on whether an occupation is licensed creates a natural experiment in which there is plausibly exogenous variation in licensing across states.[6] For example, an individual remodeling a bathroom in Massachusetts is required to have a license but an individual performing the same task in New Hampshire, a neighboring state, is not required to have a license (Blair and Fisher, 2022). We pair this across-state and within-state variation in licensing with variation in the timing of when the NBER declares that the economy is in recession to test whether licensing shields workers from job loss during recessions. Since the NBER's recession dating is retrospective, it is hard for workers to contemporaneously sort into occupations on account of the NBER's future designation of the recession period. The COVID-19 recession, in particular, was unexpected because it was driven by a global

[5]By construction, individuals with an occupational license would be legally permitted to work for pay in the occupation where they are licensed whereas those without could not be legally employed for pay in any occupation that requires a license.

[6]The variation in licensing laws across states functions effectively as an instrument for whether an individual in the occupation reports being licensed.

health shock rather than by a steady deterioration in macroeconomic conditions.

The plausible exogeneity of the state variation in licensing laws alone may not be enough, however, for our natural experiment to yield causal estimates. We must overcome endogeneity due to ability bias. Selection into licensed occupations within state, for example, could introduce ability bias, as shown in the Blair and Chung (2021) model of statistical discrimination and occupational licensing. An occupation's market share also decreases when it is licensed by the state, which is further reason to believe that licensing laws could induce selection by screening out low-ability workers (Blair and Chung, 2019). Furthermore, baseline differences in worker ability could also shape how workers experience an economic shock independently of occupational license. For our natural experiment to yield valid causal estimates, we therefore also require a measure of worker ability at the state-occupation level to account for nonrandom selection within state into licensed occupations and differential shocks to unemployment by ability during the recession.

Table 2: Measures of Pre-Labor market Ability by Licensure Status

	unlicensed	licensed	licensed-unlicensed
Math	0.4805 (0.0019)	0.5558 (0.0042)	0.0753 (0.0046)***
English	0.6694 (0.0017)	0.7623 (0.0036)	0.0929 (0.0040)***
Science	0.4669 (0.0018)	0.5735 (0.0042)	0.1066 (0.0046)***
N	71,831	13,699	

Source: Survey of Income and Program Participation (Panel 2008)

Note: Sample includes employed individuals who are between 18 and 65. Following Gittleman et al. (2018), individuals who have missing/imputed information in license/union/wage questions, and wage outliers (hourly wage below $5 or above $100) are dropped. 'License' refers to individuals who report having a government-issued license and is required by the job.

Table 3: Ability Predicts Licensure, Unemployment, Wages, & Educational Attainment

	(1)	(2)	(3)	(4)	(5)	(6)
Y = license						
Math	0.0447***	0.0195***				
	(0.00724)	(0.00583)				
English			0.0629***	0.0338***		
			(0.0119)	(0.0105)		
Science					0.0654***	0.0385***
					(0.00733)	(0.00656)
Constant	0.188***	0.0414***	0.190***	0.0449***	0.188***	0.0445***
	(0.00494)	(0.00433)	(0.00366)	(0.00426)	(0.00485)	(0.00436)
Observations	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461
Y = unemployed						
Math	-0.0102***	-0.00493***				
	(0.000834)	(0.000612)				
English			-0.0105***	-0.00516***		
			(0.00127)	(0.000893)		
Science					-0.0109***	-0.00537***
					(0.000998)	(0.000750)
Constant	0.0486***	0.0933***	0.0482***	0.0931***	0.0486***	0.0932***
	(0.00174)	(0.00280)	(0.00154)	(0.00271)	(0.00171)	(0.00277)
Observations	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461
Y = log(wage)						
Math	0.0840***	0.0547***				
	(0.00928)	(0.00655)				
English			0.0736***	0.0495***		
			(0.00788)	(0.00563)		
Science					0.0919***	0.0629***
					(0.00942)	(0.00678)
Constant	2.869***	2.581***	2.869***	2.582***	2.870***	2.583***
	(0.0142)	(0.0126)	(0.0156)	(0.0130)	(0.0145)	(0.0126)
Observations	373,982	373,982	373,982	373,982	373,982	3,019,461
Y = college						
Math	0.148***	0.126***				
	(0.00972)	(0.00766)				
English			0.131***	0.103***		
			(0.0135)	(0.0100)		
Science					0.154***	0.130***
					(0.0105)	(0.00837)
Constant	0.393***	0.312***	0.397***	0.317***	0.393***	0.314***
	(0.00740)	(0.0194)	(0.00972)	(0.0223)	(0.00765)	(0.0198)
Observations	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461
Individual Characteristics		X		X		X

Source: Survey of Income and Program Participation (Panel 2008); Current Population Survey (2018-2022).

Note: This table assesses the predictive power of the three SIPP ability measures (ever taken advanced English, math, and advanced science) on license attainment, college attainment, wages, and employment. The CPS monthly sample includes individuals who are between 18-65 and in the labor force. For the wage analysis, we drop the wage outliers and missing observations. Individual controls include age, gender, race, and control for government worker and self-employment. Sample weights apply.

We follow the literature and take the fraction of workers in a state-occupation cell who report having taken advanced math, science or English courses in high school as proxy measures of average worker ability at the state-occupation level (Blair and Chung, Forthcoming). The data come from the topical module of the SIPP in 2012 and are advantageous for our use for three reasons. First, because the survey also asks workers if they have an occupational license, we can test whether licensed workers are on average more "able" than unlicensed workers. As reported in Table 2, licensed workers are more likely to have taken advanced math (7.5 p.p.), science (10.7 p.p.), and English (9.3 p.p.) courses in high school than their unlicensed peers. Second, the common occupation classification code enables us to link the external ability measures in SIPP to our main household CPS data and quantify the usefulness of our ability measure by exploring its correlations with educational attainment and labor market outcomes. As reported in Table 3, a one-unit increase in our ability proxy predicts a 2-4 p.p. increase in the probability that a worker is licensed, a 0.5 p.p. reduction in unemployment, wages that are 5-6 p.p. higher, and a 12 p.p. increase in bachelor's degree attainment. Third, because our ability measures come from data that precede the COVID-19 pandemic, they are predetermined, which rules out reverse causality. In our preferred empirical specification, we will include all three measures of worker ability as control variables.

In our baseline specification, the outcome 'Y_{istd}' measures whether worker 'i' living in state 's,' working in industry 'd' reports being unemployed at time 't'. We regress the unemployment indicator on an indicator variable '$license_{ist}$' that equals one if the worker reports having a state-issued license that is required for the worker's current job (if the worker is employed, or for the worker's previous job if the worker is unemployed). In the regression, we further include an interaction term between the worker's license status and an indicator '*post*' that equals one for all observations from time periods following the onset of the recession being studied. The coefficient on the interaction between license $\times$ post is our coefficient of interest.

To test whether our main effect is driven by selection on ability or differences in other observable features, in the regression, we also include control variables for average worker ability at the state-occupation level '$a_{s,p}$' and its interaction with the '*post*' recession indicator, worker demographics 'X_{ist},' and fixed effects for the worker's state of residence 'θ_s' and occupation 'θ_p'.

The formal regression is:

$$\begin{aligned} Y_{istd} = &\beta_0 + \beta_1 license_{ist} + \beta_2 post + \beta_3 license_{ist} \times post \\ &+ a_{s,p} + a_{s,p} \times post + \Gamma X_{ist} + \theta_s + \theta_p + e_{istp}. \end{aligned} \tag{1}$$

β_1 represents the baseline unemployment gap between the licensed and the unlicensed. In various specification checks, we also test the sensitivity by controlling for the interaction effect of recession with education/union, regional trends, and industry fixed effect. The coefficient β_3 on the interaction term '$license_{ist} \times post$, our parameter of interest, measures how the gap in unemployment between licensed and unlicensed workers changes after the recession as compared to the value of the unemployment gap prior to the recession. If this coefficient is negative, occupation licensing is "shielding" licensed workers from increases in unemployment during the recession (as compared to their unlicensed peers) by an amount equal to the magnitude of the coefficient of interest. For example, if the estimated treatment effect from the model were -0.00957, this would suggest that licensing shields workers from a 0.96 p.p. increase in unemployment. We require three assumptions to hold for us to interpret β_3 as a causal parameter. First, we need to assume that in the absence of the recession the unemployment gap between unlicensed and licensed workers would have evolved similarly in the post period to its path in the pre-period. Second, we need to assume that the timing of the recession is orthogonal to other treatments that could have shielded licensed workers from unemployment compared to their unlicensed peers. Third, we need that our measure of pre-labor market ability is a valid proxy for controlling for selection.

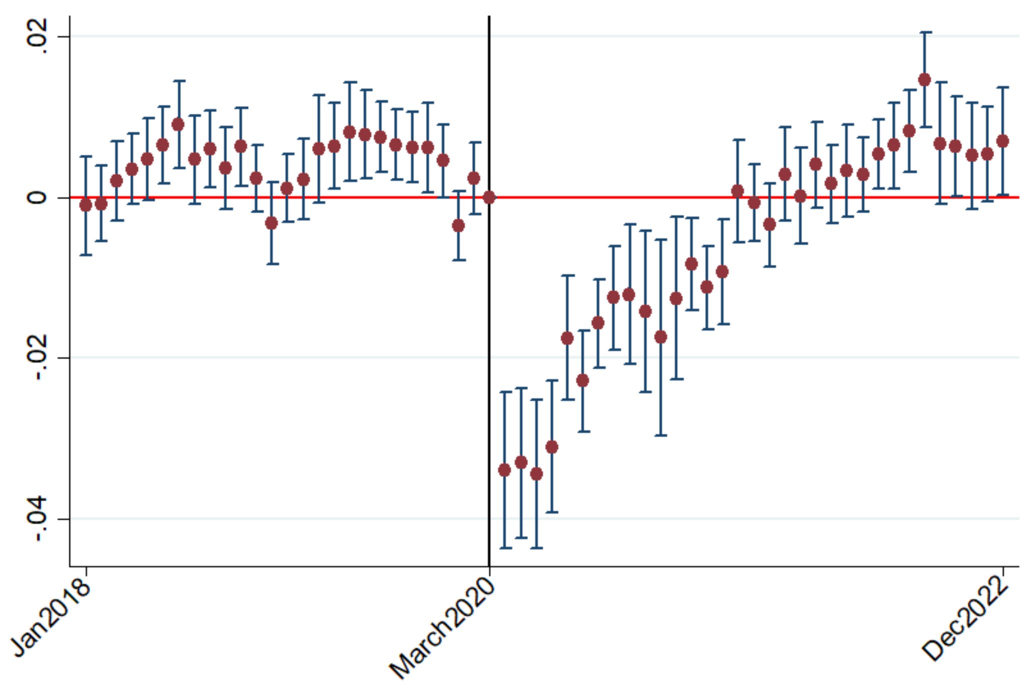
3 Results from COVID-19 Recession

We begin our analysis by presenting the variation in the data transparently using an event study inspired by the difference-in-differences approach in Equation 1:

$$
\begin{aligned}
Y_{istp} = & \alpha_0 + \alpha_1 license_{ist} + \sum_{\tau \neq -1} \alpha_\tau \times \mathbb{1}(\tau = t - t^*) \times license_{ist} \\
& + a_{s,p} + a_{s,p} \times post + \Gamma X_{ist} + \theta_s + \theta_t + \theta_p + e_{istp},
\end{aligned}
\tag{2}
$$

where the outcome remains the unemployment status of a worker and the parameters of interest are the α_τ, which capture the average unemployment difference between licensed and unlicensed workers in time 'τ,' relative to the event month immediately preceding the recession $\tau = -1$, i.e., March 2020 (where t^* equals April 2020).

Figure 2: Licensing Workers Shielded from job loss during COVID-19 Recession



Data: IPUMS Monthly Current Population Survey (2018 - 2022).

Note: Sample includes individuals between 18 and 65 who are in the labor force. The panel shows the difference between the unlicensed and the licensed in unemployment with a 95 percent confidence interval, conditional on basic characteristics (age, race, gender, education). 'Licensed' refers to workers who possess a government-issued occupational license required by current/previous job. Sample weights apply. Standard errors are clustered at the state level.

We present the result of the event study in Figure 2, where the y-axis measures the average difference in the probability of unemployment between licensed workers and unlicensed workers in a given period of time, relative to its value in $\tau = -1$. We call this difference the relative unemployment gap. In the period before the recession, we did not see substantial differences in the relative unemployment gap. In fact, the unemployment gap bounces around between zero and one percentage point, and all confidence intervals overlap. In contrast, when the COVID-19 recession hits in March 2020, we see an immediate and statistically significant decrease in the unemployment rate of licensed workers compared to that of unlicensed workers. Licensed workers are 4 percentage points less likely to be unemployed than their unlicensed peers. Over the next 16 months licensed workers continue to be differentially shielded from the increase in unemployment from COVID-19. Following the 16-month mark, we return to the pre-recession baseline.

In Table 4, we report the results of our analysis in which we estimate the difference in the average unemployment rate between licensed and unlicensed workers in the two years after the COVID-19 recession compared to its value in the two years before the recession, using the difference-in-difference regression from Equation 1. The analysis allows us to quantify the average effect of occupational licenses in shielding licensed workers from recession-induced job loss in the two years following the onset of the recession. In column (1) of Table 4 we report results from a model that only includes control variables for worker demographics and state-fixed effects. In column (2) we enrich the model to include industry fixed effects. The model in column (3) includes our measure of pre-labor market ability and an interaction between ability and the *post* recession indicator. By comparing the model in column (3), to the models in columns (1) and (2), we can discern how much of the shielding effect of licenses during recessions is due to differences in ability.

Based on the first specification in column (1), we find that licensing shields workers from a 0.96 p.p. increase in unemployment. The result is statistically significant at the

1% level. Adding industry fixed effects increases the magnitude of the shielding effect of licensing slightly to 1.01 p.p., without altering the level of significance at the 1% level. Relative to the model with industry fixed effects, the model in which we control for ability exhibits a statistically significant shielding effect of 0.93 p.p., which is roughly 7% smaller. Had we not controlled for differences in ability, our estimate of the shielding effect of occupational licensing would have been subject to omitted variable bias.

To measure the shielding effect of licensing that is independent of the fact that licensed workers are on average more educated than unlicensed workers and that workers with more education are shielded from unemployment during COVID-19 as in Beuermann et al. (2024), in column (4), we add an interaction between an indicator for whether worker i completed a four-year college degree and the '*post*' recession indicator. In column (5), we add an interaction between the worker union status and the '*post*' recession indicator. Recall from the summary statistics that union status was the only observable for which there was a statistically significant difference in the pre- and post-COVID-19 worker attributes. In column (6), we control for differential time trends in unemployment by region prior to the recession. In column (7) we replace the industry fixed effects with occupation fixed effects, which allows for a finer comparison of workers in the same occupation across states that differ in licensing laws. In column (8) we drop observations from all universally licensed occupations since these occupations are licensed in all states and therefore do not contribute any identifying variation. Dropping universally licensed occupations also tests whether the shielding effect of licensing during COVID-19 was driven by an increase in demand for medical professionals since many professions in the medical field are universally licensed, e.g., physicians and nurses.

Table 4: Licensure Shields Workers from Unemployment during COVID-19 Recession

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
license $\times$ post	-0.00957***	-0.0101***	-0.00930***	-0.00722***	-0.00682***	-0.00749***	-0.00772***	-0.00819***
	(0.00106)	(0.00105)	(0.000961)	(0.000822)	(0.000814)	(0.000815)	(0.000848)	(0.00111)
license	-0.0162***	-0.0112***	-0.0112***	-0.0124***	-0.0126***	-0.0124***	-0.0103***	-0.00802***
	(0.000624)	(0.000758)	(0.000720)	(0.000680)	(0.000674)	(0.000656)	(0.000829)	(0.000955)
post	0.0227***	0.0226***	0.0284***	0.0295***	0.0297***	0.0799***	0.0799***	0.0824***
	(0.00203)	(0.00202)	(0.00335)	(0.00338)	(0.00342)	(0.00559)	(0.00555)	(0.00573)
Observations	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461	2,652,796
R-squared	0.018	0.022	0.023	0.023	0.023	0.027	0.084	0.088
Ind FE		X	X	X	X	X		
Ability			X	X	X	X	X	X
College $\times$ post				X	X	X	X	X
Union $\times$ post					X	X	X	X
Regional trend						X	X	X
Occ. FE							X	X
Sample				All workers				Drop universal Licensed occs.

Data: IPUMS Monthly Current Population Survey (2018 - 2022).

Note: Dependent variable in all regressions is an unemployment indicator. Sample includes individuals between 18 and 65 who are in the labor force. 'License' refers to individuals who possess a government-issued occupational license required by a job. *Post* refers to the post-COVID-19 period after March 2020. All regressions control for demographic characteristics (age, race and ethnicity, gender), a college indicator, a union indicator, a public sector indicator, a self-employed indicator, an indicator of whether the license is required by the job, an indicator of possessing a professional certification, and state fixed effects. The 22 universal defined by Johnson and Kleiner (2020) include elementary/secondary school teacher, lawyer, barber/cosmetologist, real estate broker/agent, electrician, insurance agent, pharmacist, EMT/paramedic,real estate appraiser/assessor, pest control worker, chiropractor, nurse (RN/LPN), physician, social worker, occupational and physical therapist, psychologist, dental hygienist, dentist, physician assistant, veterinarian, optometrist, and podiatrist. Sample weights apply. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Among the checks that we perform in columns (4) to (8) of Table 4, adding an interaction between education reduces our estimated coefficient of the shielding effect of licensing the most. In particular, we find that the shielding effect of licensing drops from 0.93 p.p. to 0.72 p.p., or by 23% when we add the education recession interaction. The point estimate remains statistically significant at the 1% level. When we add controls for unionization, regional time trends, occupation fixed effects, and drop universally licensed occupations in column (8), we estimate a shielding effect of 0.82 p.p., which is also statistically significant at the 1% level. While controlling education is important, our results suggest that doing this alone may cause one to understate the extent to which occupational licenses shield workers from job loss during recessions.

We establish the economic importance of the shielding effect of occupational licensing that we estimate by comparing our estimate of 0.82 p.p. to three benchmarks. First, compared to the standard deviation of unemployment in the post- and pre-COVID-19 time periods, we find that the shielding effect of licensing is 0.5 to 0.66 standard deviations (respectively). Second, in the absence of the shielding effect of occupational licensing, the average unemployment for licensed workers would have been 3.52% rather than 2.75% during the two years of the COVID-19 pandemic. Third, the shielding effect of licensing represents one fifth of the gap in the post-COVID-19 unemployment rate between unlicensed and licensed workers or 29% of the pre-pandemic gap in unemployment rate (see Table 1 for the gaps in unemployment rates).

3.1 Recession Intensity

We build on our analysis by exploiting spatial variation in the intensity of the COVID-19 recession to test whether occupational licensing protects the licensed worker more strongly in places that were more severely hit by the recession. In particular, we compare the shielding effect of licensing during the recession in places that imposed a mandatory lockdown to states that did not. In the beginning (March and early April) of COVID-

19, 42 states plus DC implemented a statewide lockdown order, while the rest of the eight did not. The eight states include Arkansas, Iowa, Nebraska, North Dakota, South Dakota, Oklahoma, Utah, and Wyoming. Although both sets of states experienced a common labor supply shock from COVID-19, states that imposed a lockdown experienced a more severe negative labor demand shock due to government order. For example, Alexander and Karger (2023) show that stay-at-home orders decreased individual mobility and spending. We leverage this unanticipated (one-time) policy response with the COVID-19 timing and generate state-by-time variation in the intensity of shock.

To test whether licensing differentially protected licensed workers from job loss during COVID-19-induced recession in states that experienced a larger decline in labor demand, we augment Equation 1 with a triple differences design, comparing the shielding effect of licensing in states with and without lockdown orders. We estimate the following regression:

$$
\begin{aligned}
Y_{istp} = &\delta_0 + \delta_1 license_{ist} + \delta_2 license_{ist} \times post \\
&+ \delta_3 lockdown_s \times post + \delta_4 license_{ist} \times lockdown_s \times post \\
&+ a_{s,p} + a_{s,p} \times post + \Gamma X_{ist} + \theta_s + \theta_t + \theta_p + e_{istp},
\end{aligned}
\tag{3}
$$

where $lockdown_s \times post$ is an interaction between an indicator for states that imposed a lockdown and the *post* indicator that equals one or all time periods following the onset of the recession. Our coefficient of interest is δ_4 which measures whether licensing differentially shielded licensed workers from unemployment during COVID-19 in states that implemented a lockdown compared to states that did not impose a lockdown. The coefficient δ_3 is an estimate of the labor demand shock associated with lockdowns — specifically, it measures how much unemployment increased for workers in states that issued lockdowns. Positive values $\delta_3 > 0$ indicate that places with lockdowns experienced higher unemployment during the COVID-19 pandemic than places without lockdowns.

A comparison of δ_4 to δ_3 provides a useful benchmark for quantifying the magnitude of the shielding effect of occupational licensing during the recession relative to the negative labor demand shock of the lockdown.

The identification of δ_4 as a causal parameter relies on the idiosyncratic timing of COVID-19 and the emergency reaction of the state governments to issue lockdown orders in response to COVID-19. We also assume that the unanticipated variation in lockdown decision by state is orthogonal to an individual's licensing decision; therefore, pre-lockdown selection is unlikely to bias the causal interpretation of δ_4. Pre-pandemic sorting by workers is further accounted for by the coefficient δ_2 on the base term $license_{ist}$, which allows for the natural level of unemployment to be different between licensed and unlicensed workers before the pandemic — consistent with the finding in Kukaev and Timmons (2024) that licensed workers experience lower levels of unemployment and shorter unemployment spells during non-recessionary times. One might still worry that post-lockdown sorting of workers into licensing could generate our results by reverse causality: individuals could choose to get a license because of the recession. Although this type of sorting is possible in theory, fulfilling all requirements is not instantaneous in practice – especially during a pandemic that resulted in the closure of most services. Another potential threat to identification is the endogeneity of lockdown decision to the state of the local economy.

We assess the possibility of post-lockdown sorting with two approaches. In Figure A1 of the appendix, we first test whether the proportion of people who report a license changes around the time of the COVID-19 pandemic. There is no significant change or trend in individuals' license attainment before and after the shock. In Figure A2 of the appendix, we perform a second set of tests, regressing observable characteristics of individuals, e.g., race, age, educational attainment, on the interaction between license status and event time dummies. We do not observe significant changes in the observable characteristics of licensed workers as a result of the pandemic. Both tests suggest that reverse causality is unlikely to drive any results that we find. In Table 5, we test the orthogonality between the state unemployment rate in 2018 and 2019 and the state lockdown decision in 2020. We also include the percent of workers in the state who have a license or professional certification, are self-employed, the mean hourly wage, and the racial composition of the state. In both 2018 and 2019, we do not find significant predictive power of these variables on whether a state adopts a stay-at-home order in 2020.

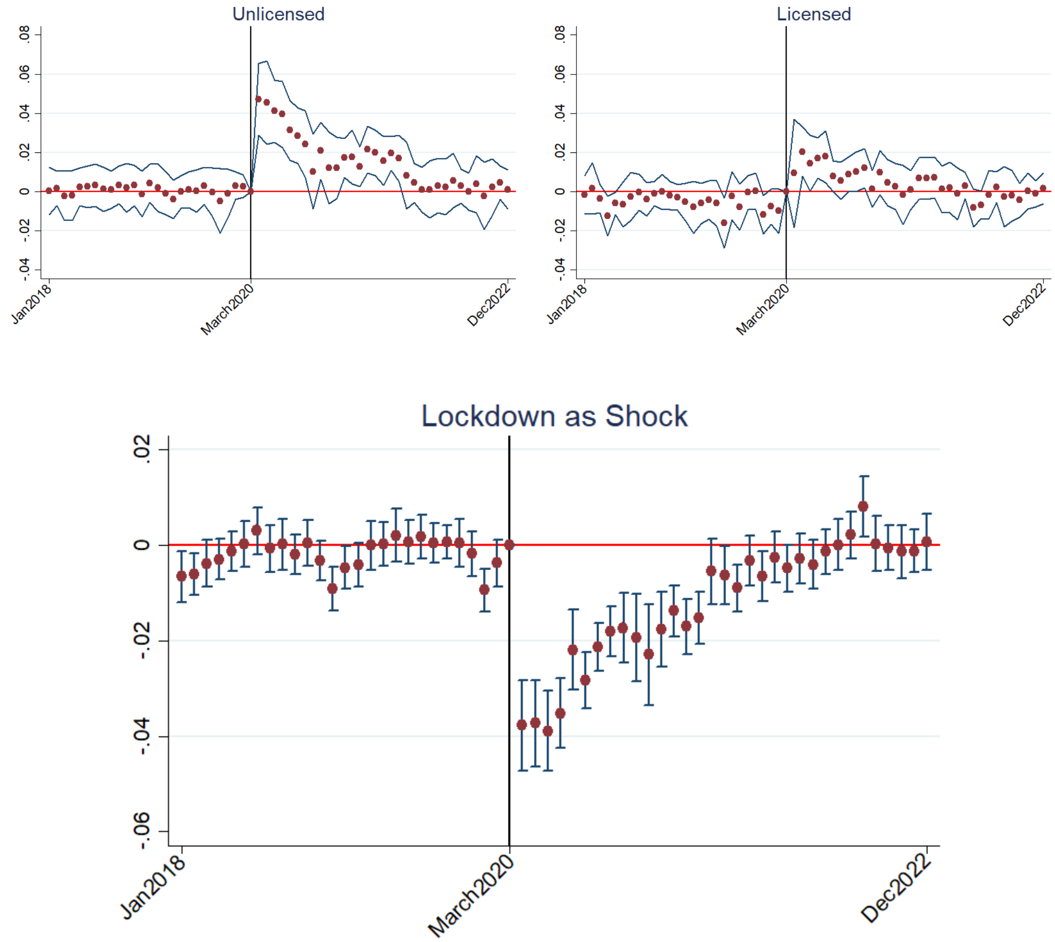
Table 5: Balancing Test: Predictability of Lockdown decision

	(1) 2018	(2) 2019
State percentage:		
license	0.00710	-0.0256
	(0.0303)	(0.0271)
cert	0.0860	0.0446
	(0.0835)	(0.0880)
unemployed	0.0916	0.0935
	(0.0700)	(0.0674)
black	0.00151	0.00357
	(0.00690)	(0.00687)
hispanic	0.00326	0.00297
	(0.00565)	(0.00559)
hourly wage	0.00566	0.00225
	(0.00407)	(0.00443)
self-employment	-0.0337	-0.00884
	(0.0355)	(0.0378)
F-stat	1.76	1.31
p-value for joint significance	0.1197	0.27
Number of states	51	51
R-squared	0.223	0.175

Note: Dependent variable equals 1 if a state implements statewide stay-home order in March/April 2020. The explanatory variables are the state average (with sample weight) of the corresponding characteristics in a particular pre-COVID-19 year. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

The upper panel of Figure 3 is an event study of the difference in the probability of unemployment of licensed (and unlicensed) workers between states that issued lockdowns and states that did in the two-year window around the COVID-19 recession. Unlicensed workers in states that issued lockdowns experience a sharp 5 p.p. spike in unemploy-

Figure 3: Lockdown Policy to Proxy for Recession Intensity



Data: IPUMS Monthly Current Population Survey (2018 - 2022).

Note: The first panel compares the unemployment pattern by plotting the time dummies between unlicensed and licensed workers, conditional on basic characteristics (age, race, gender, education). The second panel shows the difference between the unlicensed and the licensed with a 95 percent confidence interval. 'Licensed' refers to workers who possess a government-issued occupational license required by current/previous job. Sample weights apply. Standard errors are clustered at the state level.

ment compared to their peers in states without lockdowns. Licensed workers in states with lockdowns, by contrast, experience a less pronounced 2 p.p. increase in relative unemployment. In the lower panel of Figure 3, we present the results of an event study in which we plot the coefficient on the triple interaction between license $\times$ lockdown $\times$ post over time. We find that in states with lockdowns, which are the states that experience the largest increase in unemployment during the recession, that licensed workers are shielded from a 3 p.p. increase in unemployment at the onset of the COVID-19 recession and that this shielding eventually fades out after 15 months.

Table 6: Job Shielding Effect of Licensing during COVID-19 recession strongest in states imposing Lockdowns

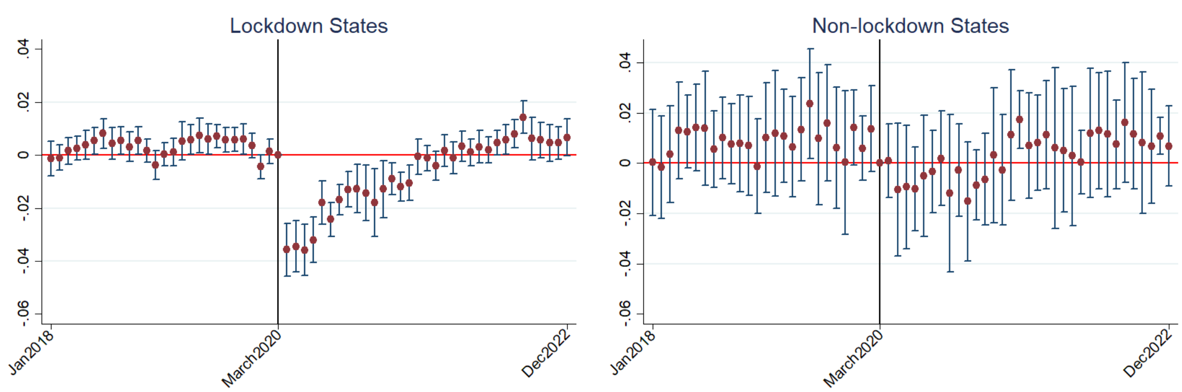
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
license × lockdown × post	-0.0111***	-0.0104***	-0.00910***	-0.00947***	-0.00947***	-0.00943***	-0.00892***	-0.00783***
	(0.00256)	(0.00235)	(0.00224)	(0.00221)	(0.00221)	(0.00221)	(0.00225)	(0.00214)
license × post	0.000328	-0.000829	-0.00116	0.00162	0.00162	0.00170	0.000966	-0.000880
	(0.00243)	(0.00220)	(0.00213)	(0.00209)	(0.00209)	(0.00210)	(0.00215)	(0.00209)
license	-0.0163***	-0.0113***	-0.0113***	-0.0126***	-0.0126***	-0.0127***	-0.0106***	-0.00807***
	(0.000629)	(0.000764)	(0.000727)	(0.000691)	(0.000691)	(0.000673)	(0.000854)	(0.000972)
lockdown × post	0.0152***	0.0151***	0.0221***	0.0201***	0.0201***	0.0195***	0.0187***	0.0194***
	(0.00281)	(0.00279)	(0.00386)	(0.00367)	(0.00367)	(0.00345)	(0.00352)	(0.00357)
Observations	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461	2,652,796
R-squared	0.028	0.032	0.032	0.032	0.032	0.032	0.089	0.092
Ind FE		X	X	X	X	X		
Ability			X	X	X	X	X	X
College × lockdown × post				X	X	X	X	X
Union × lockdown × post					X	X	X	X
Regional trend						X	X	X
Occ FE							X	X
Sample	All workers							Drop universal licensed occs.

Data: IPUMS Monthly Current Population Survey (2018 - 2022).

Note: Dependent variable in all regressions is an unemployment indicator. The sample includes individuals aged 18 to 65 years who are in the labor force. 'License' refers to individuals who possess a government-issued occupational license required by a job. All regressions control for demographic characteristics (age, race, and ethnicity, gender), a college indicator, a union indicator, a public sector indicator, a self-employed indicator, an indicator of whether the license is required by the job, an indicator of possessing a professional certification, and state and month fixed effects. Sample weights apply. Standard errors are clustered at the state level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

In Table 6, we report the results that we obtain by estimating Equation 3 with varying levels of control variables, following the structure of Table 4. Our coefficient of interest comes from the interaction license $\times$ lockdown $\times$ post, which measures the extent to which occupational licenses differentially shielded workers from job loss in states that implemented lockdowns at the onset of COVID-19. States that imposed lockdowns experienced an unemployment rate that was 1.5 to 2 p.p. higher (see the coefficient on lockdown $\times$ post). Indeed, the lockdown pinpoints variation in the intensity of the recession. Across the eight specifications in Table 6 we find that occupational licensing mutes between 0.78 to 1.1 p.p. of the increase in unemployment due to the lockdown. Although we know that unemployment also increased in states that did not impose a lockdown, we did not find a shielding effect of licensing from recession-induced job loss in states without a lockdown. The coefficient on the interaction license $\times$ post is not only statistically insignificant across all specifications, it is an order of magnitude smaller than coefficient on the triple interaction license $\times$ lockdown $\times$ post.

Figure 4: Shielding Effect in Lockdown vs Non-lockdown States



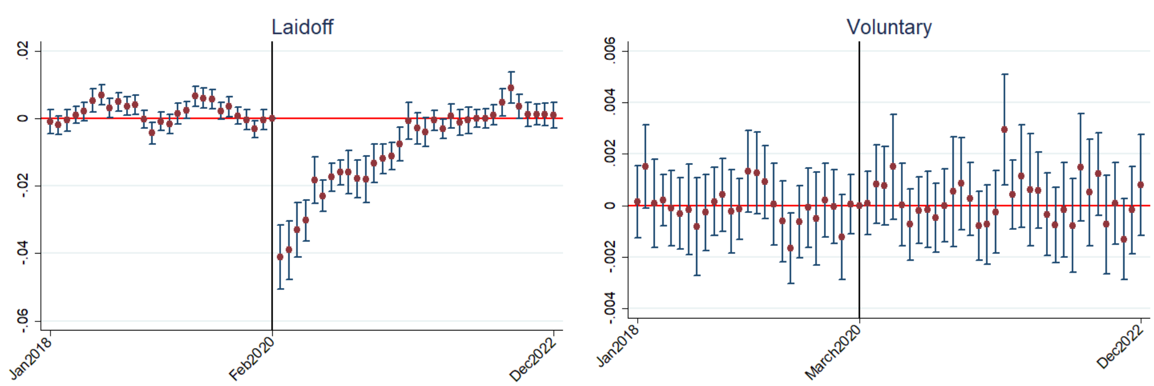
Data: IPUMS Monthly Current Population Survey (2018 - 2022).

Note: Dependent variable is an unemployment indicator. The sample includes individuals between 18 and 65 who are in the labor force. This figure compares the time dummies of the shielding effect of licensing in lockdown states (left) and non-lockdown states (right) with a 95% confidence interval. Sample weights apply. Standard errors are clustered at the state level.

The event study in Figure 4 in which we split our sample into states with lockdowns and non-lockdown states and estimate the difference in unemployment between licensed and unlicensed workers following the approach in Equation 2 confirms the findings from our triple difference regression model. This pattern of results is consistent with the hypothesis that licensing shields workers from job loss due to negative labor demand shocks, but not against common labor supply shocks.

We now directly test the hypothesis that licensing shields against recession-induced job loss due to negative labor demand shocks but not labor supply shocks. In the data, we observe whether an individual is unemployed due to a layoff, which we consider to be more closely related to a negative labor demand shock, or a voluntary quit, which we consider to be more closely related to a labor supply decision. In Figure 5 we estimate our event study of changes in the relative unemployment gap separately by layoffs and voluntary quits, using the triple difference approach. Here we find that licensing shields workers from job loss due to layoffs in states that implement lockdown, but there is no impact of licensing on voluntary quits.

Figure 5: Licensing Reduces Job Loss due to Layoffs in COVID-19



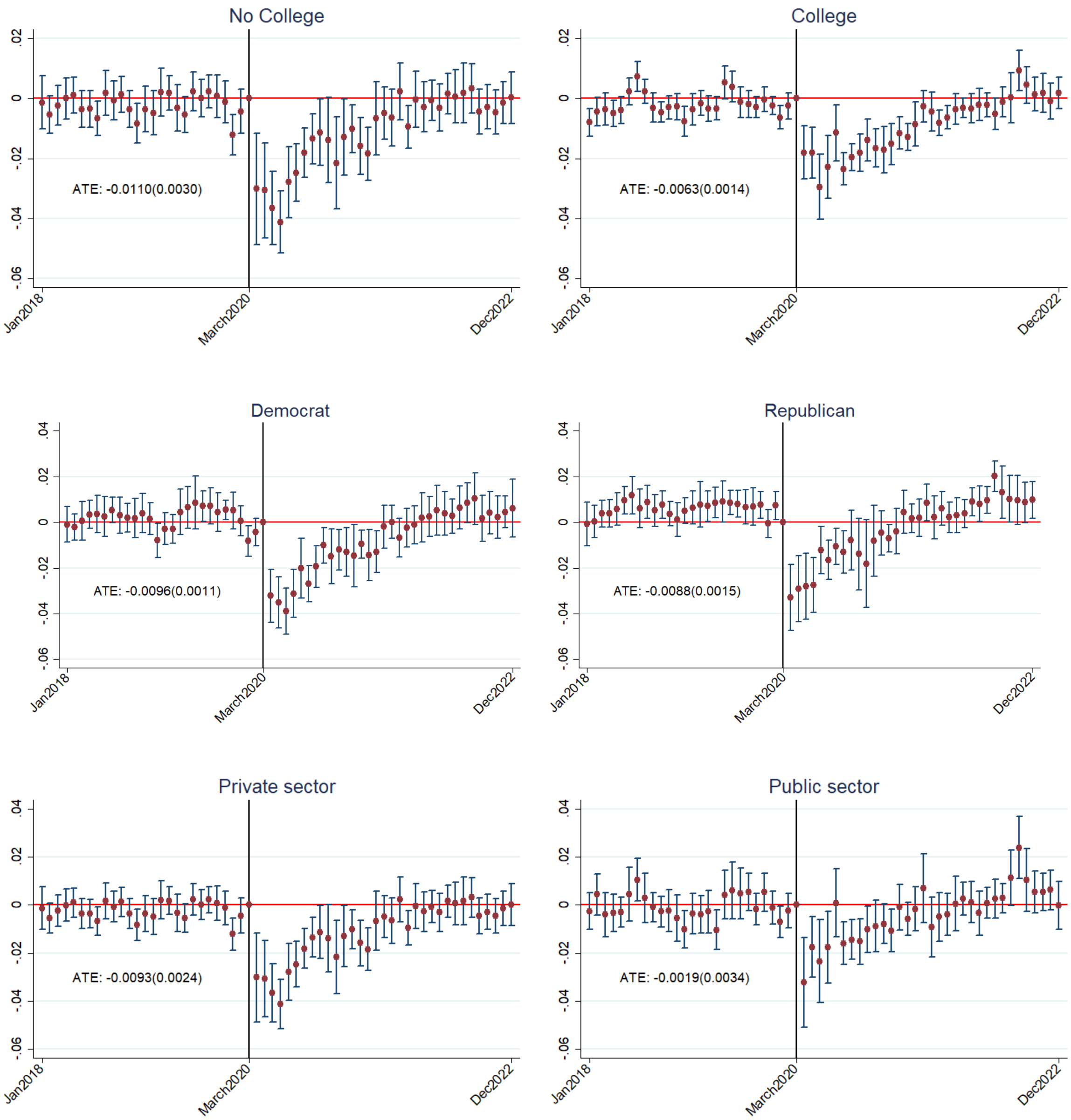
Data: IPUMS Monthly Current Population Survey (2018 - 2022).

Note: Sample includes individuals between 18 and 65 who are in the labor force. This figure compares the time dummies of the shielding effect of licensing on laidoff (left) and voluntary separation (right) with a 95% confidence interval, using the lockdown triple-differences full specification. Sample weights apply. Standard errors are clustered at the state level.

Across many dimensions of heterogeneity, we find that licensed workers experience less job loss during recessions than their unlicensed peers. For example, licensed workers who are college educated and licensed workers without college degrees both experience less job loss during recessions compared to their unlicensed peers, as shown in Figure 6. This result holds notwithstanding the research showing that workers without college degrees experience more downward mobility (Autor, 2014; Blair et al., 2021). In both red and blue states, we document a similar impact of licensing on protecting workers from recession-induced job loss, as shown in Figure 6.[7] The only exception to the homogeneity of the treatment effects is that licensing appears to provide stronger protection from recession-induced job loss for workers in the private sector than for workers employed by the government.

[7] We define the political affiliation of a state based on the 2016 presidential election. Since all Democrat states implemented lockdown during Covid, the triple-differences approach in Equation 3 does not have variation if we split the sample by political affiliation of a state. We therefore present the event study dummies using the DID setup in this sub-analysis.

Figure 6: Heterogeneity by Education and Industry

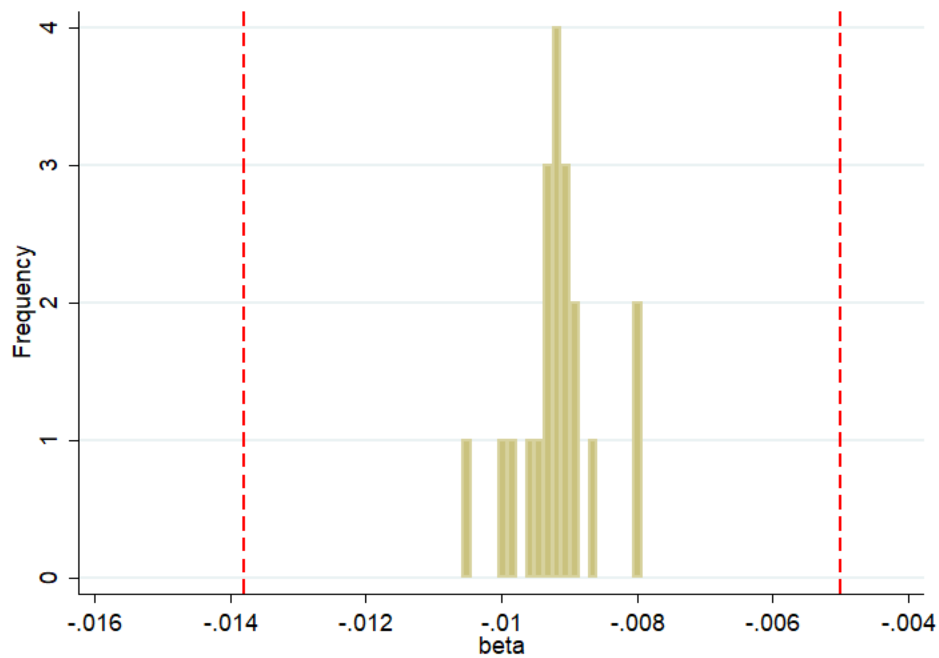


Data: IPUMS Monthly Current Population Survey (2018 - 2022).

Note: Dependent variable in all regressions is an unemployment indicator. Sample includes individuals between 18 and 65 who are in the labor force. This figure includes event study plots (full specification) of the COVID-19 shock using different sub-samples. We use the lockdown triple-differences specification for college/no-college and public/private sub-sample. Since all Democrat states implemented lockdown during Covid, the triple-differences approach in Equation 3 does not have variation if we split the sample by political affiliation of a state. The plots present the event study dummies using the DID setup in this sub-analysis. The shielding pattern of licensing is homogeneous across education (upper panel) and state political status (middle panel). The lower panel shows that the shielding is more apparent for private workers.

The job shielding effect of licensing appears to be consistent across industries. Notably, we show in Figure 7 that dropping all observations from any one of the 20 industries and re-estimating the model does not yield substantial departures from the average treatment effect that we obtain from using all industries. In fact, all 20 industry permutations fall within the 95% confidence interval of the main treatment effect using the lockdown shock[8]

Figure 7: Job Shielding Impact of Licensing during COVID-19 Similar Across Industries



Note: This figure plots the distribution of the twenty estimates on the shielding effect of licensing by dropping the 20 industries one at a time using lockdown shock (comparable to column 6 of Table 6). The red line marks the 95% confidence interval of the main estimate. The raw coefficients in the 20 iterations are all significant at 1% level.

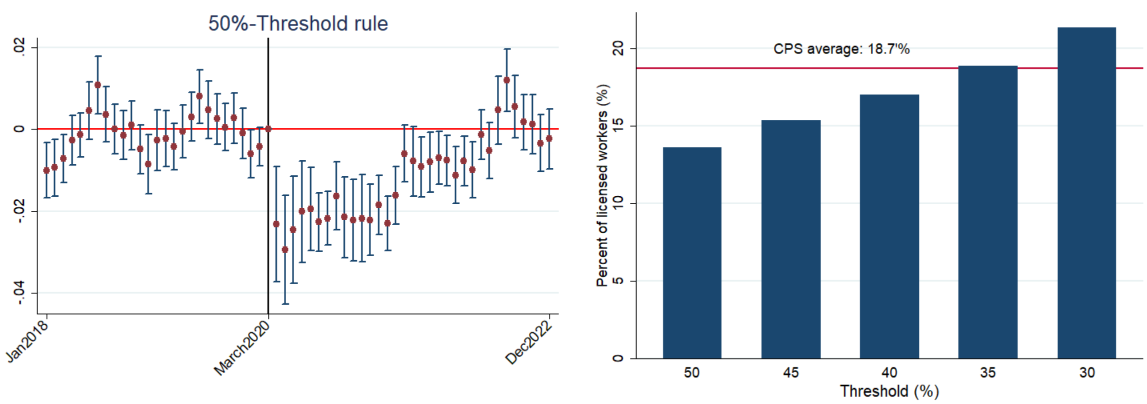
3.2 Alternative Measures of Licensing

The license attainment variable that we employ is self-reported in the CPS and may be susceptible to measurement error (Kleiner and Vorotnikov, 2017). In this subsection, we adopt a threshold rule as an alternative way to define license requirements at the occupation-by-state level (Blair and Chung, 2019). For each 6-digit occupation (defined

[8]We also reproduce the permutation exercise to the main DID estimate in Figure A3.

by the Standard Occupational Classification (SOC)) in each state, we tabulate (with sample weights) the proportion of workers who report requiring a government-issued license. The tabulation sample is limited to the pre-COVID-19 period to limit the sorting caused by the COVID-19 recession. We then define a state-occupation cell as licensed if more than x% of workers in that state-occupation cell report requiring a license to work. The treatment variable is then an intent-to-treat measure, assigning the license status to a worker based on the other workers' response in the same state-occupation cell.

Figure 8: Sensitivity Check: 50%-Threshold Rule to Define Licensure



Data: IPUMS Monthly Current Population Survey (2018 - 2022).

Note: The left panel plots the event study graph of the shielding effect using the 50%-threshold (define a state-occupation as licensed if the pre-Covid percent of licensed workers exceeds 50%). The dependent variable is an unemployment indicator. The Sample includes individuals between 18 and 65 who are in the labor force. The regression uses the lockdown triple-differences full specification. The right panel compares the tabulated average of licensed workers using the corresponding threshold with the raw sample average (18.7%). The 50% rule gives the most conservative threshold.

In the left panel of Figure 8, we plot an event study graph of the relative unemployment gap between licensed workers and unlicensed workers, the 50% threshold, which is a common standard in the literature. Qualitatively, the picture looks similar to what we found when we use the individual self-reported licensing variables in Figure 2. Before the onset of COVID-19, the relative unemployment gap between licensed and unlicensed workers bounced around zero before dropping immediately in the aftermath of COVID-19. In the right panel of Figure 8, we plot the fraction of licensed workers that we obtain by assigning an individual's license status using the outcome of a threshold rule for state-occupation licensing and compare it to the average licensing rate from the individual self-reports in the CPS which is 18.7%. Our comparison suggests that the 50% rule might underestimate the license attainments, while the 30% rule might overstate it.

Table 7: Results Consistent across Licensing Thresholds

	(1) 0.5	(2) 0.45	(3) 0.4	(4) 0.35	(5) 0.3
license*lockdown*post	-0.0102***	-0.00961***	-0.00975***	-0.00792***	-0.00793***
	(0.00234)	(0.00214)	(0.00186)	(0.00205)	(0.00191)
license*post	0.00372	0.00368*	0.00352**	0.00266	0.00265
	(0.00230)	(0.00202)	(0.00171)	(0.00201)	(0.00181)
license	-0.00402***	-0.00395***	-0.00435***	-0.00504***	-0.00604***
	(0.000672)	(0.000662)	(0.000623)	(0.000733)	(0.000684)
lockdown*post	0.0203***	0.0204***	0.0206***	0.0205***	0.0207***
	(0.00345)	(0.00345)	(0.00344)	(0.00350)	(0.00357)
Observations	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461
R-squared	0.031	0.031	0.031	0.031	0.031

Data: IPUMS Monthly Current Population Survey (2018 - 2022).

Note: Dependent variable in all regressions is an unemployment indicator. The sample includes individuals between 18 and 65 who are in the labor force. 'License' is defined using the corresponding pre-shock threshold at the state-occupation level. All regressions control for demographic characteristics (age, race and ethnicity, gender), a college indicator, a union indicator, a public sector indicator, a self-employed indicator, an indicator of whether the license is required by the job, an indicator of possessing a professional certification, industry, state, and month fixed effects, and the additional controls in the full model. Sample weights apply. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

In Table A1, we present results that probe the sensitivity of our core finding to the choice of the licensing threshold. For licensing thresholds that vary from 50% to 30% in increments of 5 p.p, we estimate the specification from column 6 in Table 6, where we replace the individual license attainment with an indicator for whether an individual in a given state-occupation is licensed as determined by the threshold rule. The regression specification includes controls for education, ability, and union status, as well as their interactions with the 'post' variable to allow for the differential impacts of these variables before and after the recession. In the models, we use industry fixed effects to guard against the potential that the occupation fixed effect would be colinear with the state-occupation definition. We find that licensing shielded workers from increases in unemployment, but only in states that experienced a negative labor demand shock, as measured by the introduction of a lockdown. The shielding effect that we estimate ranges from 0.79 p.p. using the least stringent licensing threshold of 30% to 1.02 p.p. using the most stringent threshold of 50%. For comparison, when we used the individual licensing attainment, we measured a shielding effect of 0.94 p.p. (column 6 in Table 6). The measurement error from the use of individual license self-reports leads to an underestimate of the treatment effect by approximately 6%.

4 Generalizing the Results to the Great Recession

Is our finding that occupational licensing protects workers from job loss during a recession a general result? Or is it a finding unique to the COVID-19-induced recession? We tested the generalizability of our findings by applying our research design to the Great Recession using monthly employment and demographic data from the CPS. In total we have 3.9 million worker-month observations in the two years before and after the Great Recession, i.e., January 2006 to December 2010. Because continuous data on self-reported licensing were sparse before 2015, we construct a state-by-occupation measure of licensing that is contemporaneous with the Great Recession by combining data from three sources: 1) a topical module on occupational licensing from the 2008 Survey of Income and Program Participation (Gittleman et al., 2018), 2) data from the American Bar Association (ABA) on licensing statutes that have restrictions on felons (Blair and Chung, Forthcoming), and 3) data on occupations that are universally licensed (Kleiner et al., 1982; Johnson and Kleiner, 2020).

The 2008 SIPP data are the first nationally representative household survey that contains individual license attainment. However, the licensing data were only collected during its Wave 13 in 2012/2013. We follow the threshold rule of 50% used in the literature to define a state-occupation as licensed if more than 50% of individuals report requiring a license to work in the corresponding state-occupation cell. Defining a state-occupation measure of licensing from the SIPP data permits us to combine it with the ABA licensing data and the data on universally licensed occupations which are both reported at the state-occupation level.[9] We code a worker as licensed if the worker is working in a state where the occupation is coded as requiring a license in any of the three data sets that we have assembled. We find that 29% of the individuals in the CPS (2006-2010) sample are coded as licensed, using this definition. Because one of the three data sources, i.e., 2008 SIPP, comes from a time period that follows the Great Recession, we were concerned that our estimate of the fraction of licensed workers could be subjected to measurement error. To test this, we compared our fraction of licensed workers with the fraction of licensed workers estimated by Kleiner and Krueger (2013) in 2008 using a Gallup survey. They find that 29% of the workers are licensed, which is similar to what we find within a few decimal points.

In Table 8 we report the means before the Great Recession and after the Great Recession of our outcome of interest, the unemployment rate separately for licensed and unlicensed workers. We also calculate the difference in the unemployment rate between licensed and unlicensed workers in each time period and the difference in this difference, which measures the extent to which licensed workers experience less job loss than unlicensed workers during the recession. Likewise, we report pre-Great Recession and post-Great Recession means for the characteristics of workers, e.g., sex, age, race, college, separately for licensed workers and unlicensed workers. We also measure the difference in the means for each period and the difference in this difference, which measures how much selection into licensing changed during the recession. In the pre-Great Recession period, we find that licensed workers are less likely to be unemployed by 1.94 p.p. and they also appear to be selected on each of the individual characteristics. Likewise, in the post-Great Recession period we also find that licensed workers are less likely to be unemployed than unlicensed workers by 3.61 p.p. and they are also selected on seven of the nine individual characteristics.

[9] We use the 6-digit SOC code to define an occupation.

Table 8: Average Unemployment and Demographics by License Status before and after the Great Recession

	Pre-Great Recession			Post-Great Recession			
	Unlicensed	Licensed	Diff	Unlicensed	Licensed	Diff	Diff-in-Diff
Outcome							
unemployed	0.051	0.031	-0.0194***	0.092	0.056	-0.0361***	-0.0167***
			(0.00101)			(0.00162)	(0.00150)
Individual Characteristics							
female	0.432	0.538	0.106***	0.433	0.546	0.113***	0.00667***
			(0.0102)			(0.00964)	(0.00243)
age	39.511	41.247	1.737***	39.901	41.562	1.661***	-0.0752
			(0.173)			(0.207)	(0.0923)
black	0.116	0.116	-0.000289	0.117	0.116	-0.00113	-0.000839
			(0.00435)			(0.00425)	(0.00136)
hispanic	0.155	0.110	-0.0442***	0.161	0.117	-0.0444***	-0.000132
			(0.00856)			(0.00904)	(0.00192)
asian	0.046	0.045	-0.000984	0.048	0.047	-0.000916	-0.000068
			(0.00259)			(0.00216)	(0.00117)
union membership	0.240	0.256	0.0163***	0.230	0.252	0.0220***	0.00574***
			(0.00284)			(0.00257)	(0.00106)
college	0.250	0.435	0.186***	0.259	0.447	0.188***	0.00262
			(0.00765)			(0.00790)	(0.00329)
govt	0.117	0.201	0.00931***	0.118	0.206	0.0878***	0.00387**
			(0.0110)			(0.0116)	(0.00184)
self employed	0.102	0.111	0.00862***	0.098	0.103	0.00548*	-0.00383**
			(0.00289)			(0.00281)	(0.00156)
Observations	1137463	442252		1685915	663237		

Data: Monthly CPS (Jan 2018 to Dec 2022)

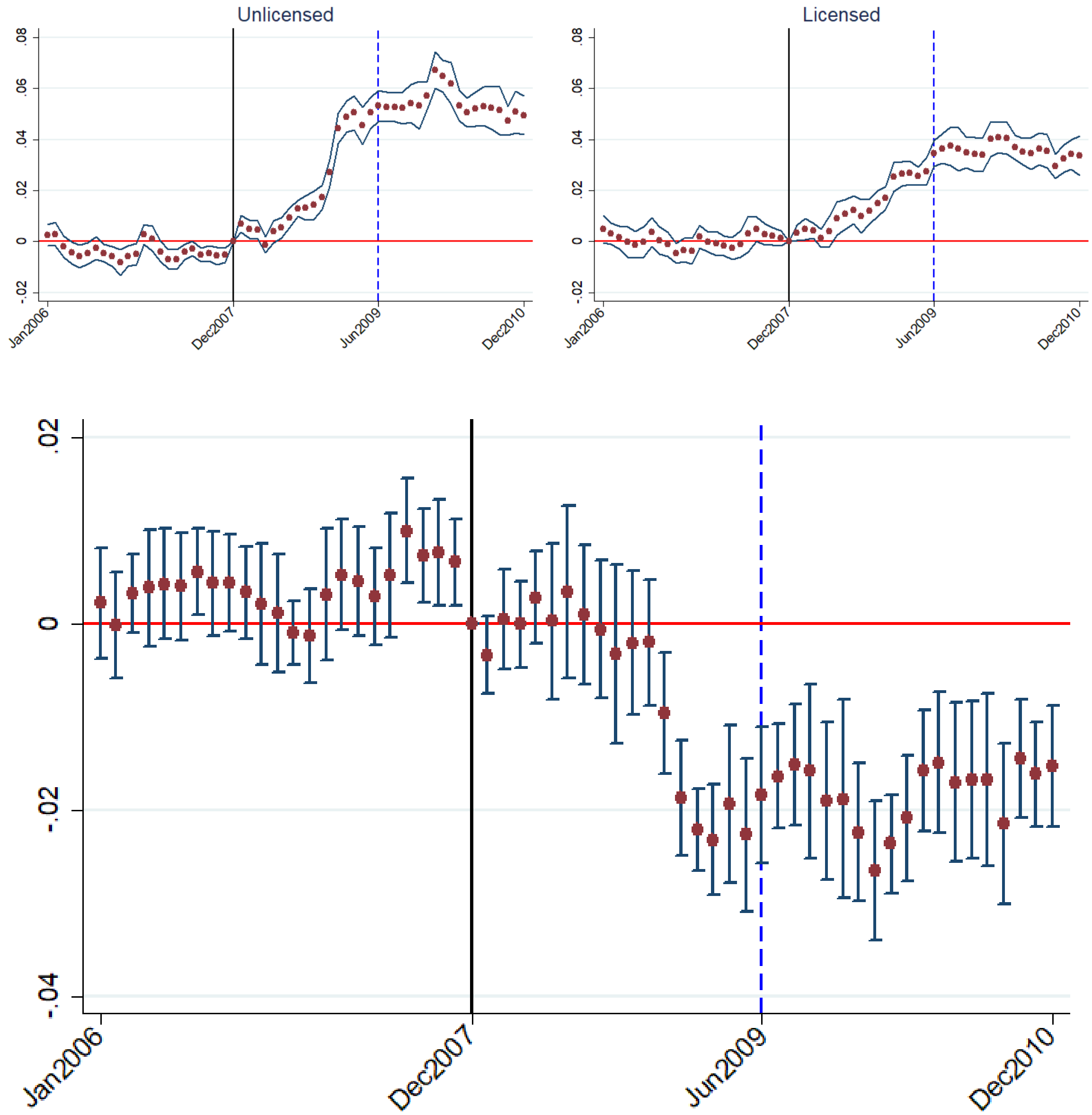
Note: 'Licensed' refers to individuals who work in a licensed state-occupation defined using the data described in Section **??**. 'Pre-Recession' refers to the period before Dec 2007. Sample weight apples. Standard errors are clustered at the state level in testing differences.

When we examine the difference in the difference measure, licensed workers are on average 1.67 p.p. less likely to experience job loss during the recession than their unlicensed peers despite both categories of workers being more likely to be unemployed during the recession. Examining the difference in the difference for the nine individual characteristics, we find that they are each economically small and that five of the nine are statistically indistinguishable from zero. In the four cases where the difference-in-differences for the individual characteristics are statistically significant, i.e., female, union membership, government employee and self-employed, the magnitudes of the difference-in-differences are economically small — ranging from 3.8% to 1.5% of the pre-Great Recession mean for both licensed and unlicensed workers. As was the case with the summary statistics for the window around the COVID-19 recession, the summary statistics for the Great Recession make it intriguing to consider whether occupational licensing protects licensed workers from job loss during recessions above and beyond what could be explained by selection on observable worker characteristics.

In Figure 9 we use an event study to illustrate the difference in the unemployment rate before and after the Great Recession for unlicensed workers and unlicensed workers – using the month before the Great Recession as a benchmark. Both licensed and unlicensed individuals experienced a gradual increase in unemployment starting in December 2007. In the latter part of 2008, we observe a more rapid increase in unemployment for unlicensed workers than for licensed workers. This period coincides with the failure of Lehman Brothers in September 2008, which accelerated the financial crisis during the Great Recession. The event study in the lower panel of Figure 9 quantifies the difference in the unemployment rate between licensed and unlicensed workers before and after the Great Recession illustrates these dynamics.

In Table 9, we report the results of our analysis in which we estimate the difference in the average unemployment rate between licensed and unlicensed workers in the two years after the Great Recession compared to its value in the two years prior to the Great

Figure 9: Descriptive Pattern in 2008 Recession



Data: IPUMS Monthly Current Population Survey (2006 - 2010).

Note: Sample includes individuals between 18 and 65 who are in the labor force. The upper panel compares the unemployment pattern by plotting the time dummies between unlicensed and licensed workers, conditional on basic characteristics (age, race, gender, education). The lower panel shows the difference between the unlicensed and the licensed with a 95 percent confidence interval. We define 'licensed' using external data sources described in Section **??**. Sample weights apply. Standard errors are clustered at the state level.

Recession, using the difference-in-difference regression from Equation 1 and the state-occupation-level measure of licensing. The analysis allows us to quantify the average effect of occupational licenses in shielding licensed workers from recession-induced job loss in the two years following the onset of the recession. Since the license variable is

Table 9: Licensure Shields Workers from Unemployment during the Great Recession

	(1)	(2)	(3)	(4)	(5)	(6)
license × post	-0.0165***	-0.0167***	-0.0124***	-0.0121***	-0.0122***	-0.0111***
	(0.00145)	(0.00142)	(0.00120)	(0.00115)	(0.00120)	(0.00135)
license	-0.00722***	0.000994	-0.00159	-0.00182*	-0.00173	-0.00135
	(0.00103)	(0.00114)	(0.00107)	(0.00105)	(0.00107)	(0.00145)
post	0.0419***	0.0422***	0.0481***	0.0487***	0.0142***	0.0128***
	(0.00303)	(0.00309)	(0.00367)	(0.00373)	(0.00173)	(0.00174)
Observations	3,928,867	3,928,867	3,928,867	3,928,867	3,928,867	3,496,543
R-squared	0.031	0.038	0.038	0.039	0.041	0.040
Ind FE		X	X	X	X	X
College x recession			X	X	X	X
Union x recession				X	X	X
Regional trend					X	X
Sample	All workers					No universal licenses

Data: IPUMS Monthly Current Population Survey (2006 - 2010).

Note: Dependent variable in all regressions is an unemployment indicator. The sample includes individuals between 18 and 65 who are in the labor force. 'Recession' refers to the period after the mid-point of the recession (Dec 2007). 'License' is defined using the 50% threshold at the state-occupation level pooled from SIPP Panel 2008, the universal licensed professions (Johnson and Kleiner, 2020), and the felony license data. All regressions control for demographic characteristics (age, race and ethnicity, gender), a college indicator, a union indicator, a public sector indicator, a self-employed indicator, an indicator of whether the license is required by the job, an indicator of possessing a professional certification, state fixed effects. Sample weights apply. Standard errors are clustered at the state level.

defined at the state-occupation level, this limits the variation to include occupation fixed effect and the state-occupation ability proxies.

In column (1) of Table 9, we report results from a model that only includes control variables for worker demographics and state fixed effects. In column (2) we enrich the model to include industry fixed effects. In column (3) we add an interaction between an indicator for whether worker i completed a four-year college degree and the '*post*' recession indicator. In column (4), we include an interaction between worker union status and the recession indicator. In column (5) we control for differences in regional time trends. In column (6), we drop observations from all universally licensed occupations.

Based on the model specification in column (1), we find that licensing shields workers from a 1.65 p.p. increase in unemployment. The result is statistically significant at the 1% level. Adding industry fixed effects increases the magnitude of the shielding effect of licensing slightly to 1.67 p.p., without altering the level of significance at the 1% level. Relative to the model with industry fixed effects, the model in which we control for ability

– as measured by college degree attainment in column (2) – exhibits a statistically significant shielding effect of 1.24 p.p., which is roughly 26% smaller.[10] Had we not controlled for differences in education, our estimate of the shielding effect of occupational licensing would have been subject to a substantial amount of omitted variable bias.

Among the checks that we perform in columns (4) to (6) of Table 9, dropping the observations from the universally licensed occupations reduces our estimate of the shielding effect of licensing the most, reducing it from 1.24 p.p. to 1.11 p.p. Even then, the estimate of our coefficient of interest remains statistically significant at the 1% level. The shielding effect of licensing of 1.11 p.p. represents 31% of the gap in the post-Great Recession unemployment rate between unlicensed and licensed workers or 56% of the pre-pandemic gap in the unemployment rate. Compared to what we found with the COVID-19 recession, the shielding effect of licensing during the Great Recession is of a similar magnitude, i.e. 1.11 p.p. versus 0.82 p.p.

4.1 Heterogeneity by Industry

In Table B3 and Table B4, we report estimates of the impact of licensing on unemployment during the COVID-19 recession and the Great Recession separately for each of the 20 industries in the data. The goal of this exercise is to measure heterogeneity in the impacts of licensing by industry and to assess whether these impacts appear generalizable across industries and across recessions.

Across both recessions, the majority of industry-level point estimates are negative. During COVID-19, 14 of 20 industries have negative coefficients; during the Great Recession, 13 of 20 industries do. Of these, 10 industries have negative coefficients in both downturns: Health, Public Administration, Retail, Education, Construction, Professional, Manufacturing, Arts, Administrative, and Wholesale. During COVID-19, 5 of the 14 neg-

[10] In our main results we do not employ the three ability measures (math, English, science) since they are tabulated using the survey after 2008.

ative coefficients are significant at the 5% level (Health, Public Administration, Education, Retail, and Transportation). During the Great Recession, 7 of the 13 negative coefficients are significant at the 5% level (Public Administration, Professional Services, Wholesale, Retail, Arts, Health, and Real Estate). By contrast, none of the positive coefficients in either recession is statistically significant.

Taken together, these patterns indicate that the protective effect of licensing is both widespread across industries and robust across recessions, despite the very different origins of the two downturns. For example, licensed workers in the Health industry during COVID-19 were 0.95 percentage points less likely to be unemployed than their unlicensed peers, while licensed workers in the Real Estate industry during the Great Recession were 1.36 percentage points less likely to be unemployed than their unlicensed peers. These results suggest that occupational licensing provides meaningful unemployment protection in a broad range of sectors and across very different types of recessions.

4.2 Recession Intensity using Industry Bartik Shocks

From our analysis of the COVID-19-induced recession, we found that occupational licensing shielded workers from job loss in places hardest hit by the recession. Was this also the case during the Great Recession? We follow Hershbein and Kahn (2018) in using Bartik shocks as a source of plausibly exogenous variation in exposure to negative labor demand shocks during the Great Recession. The Bartik shocks provide simulated unemployment changes in a Metropolitan Statistical Area (MSA) during the Great Recession by projecting national shocks to unemployment by industry (during the recession) onto MSAs using the MSA industry shares from 2004 and 2005 — a few years before the Great Recession.

We regress an indicator for whether an individual 'i' in MSA 'c' living in state 's' at time 't' working in occupation 'p' and industry 'd' is unemployed (Y_{icstpd}) on an indicator for whether the individual workers in a licensed occupation and a triple interaction between the license indicator, a post-recession indicator and the value of the simulated employment shock to the MSA measured by '$Bartik'_c$. The exact empirical specification that we run is:

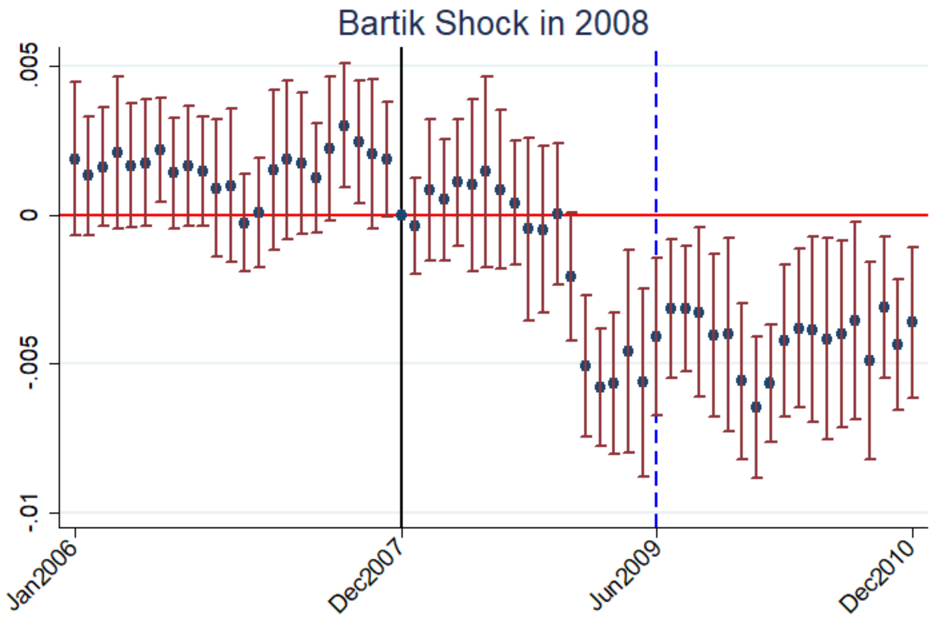
$$\begin{aligned} Y_{icstd} =& \beta_0 + \beta_1 license_{ist} + \beta_2 Bartik_c + \beta_3 Bartik_c \times post \\ &+ \beta_4 license_{ist} \times Bartik_c \times post + \Gamma X_{ist} + \theta_s + \theta_t + \theta_d + e_{icstd}, \end{aligned} \tag{4}$$

where the control variables are the same as in Equation 1. Our coefficient of interest, β_4, which comes from the triple interaction, measures the average difference in the shielding effect of occupational licenses during the Great Recession between an MSA predicted to be in the 90th versus the 10th percentile of the recessionary unemployment shock. Negative values of β_4 imply that licensing offers stronger protection from job loss for licensed workers in MSAs that are hardest hit by the Great Recession.

In Figure 10 we plot event study estimates of β_4 over time relative to its value in December 2007, which marked the beginning of the Great Recession. Qualitatively, we see that prior to the Great Recession there was no difference in the relative unemployment gap between licensed and unlicensed workers as a function of how hard an MSA is predicted to be during the Great Recession. In the months after Lehman Brothers failed, we see the emergence of a gap between places that are predicted to be hit harder by the Great Recession in how insulated licensed workers are from job losses as compared to unlicensed workers.

In Table 10, we present estimates of the average value of β_3 for increasingly rich implementations of Equation 4. To begin with, the Bartik shock captures a meaningful variation in the intensity of the Great Recession. A one-unit increase in the Bartik shock predicts an increase in the unemployment rate of 1.65 p.p. to 2.27 p.p. during the Great Recession. Occupational licensing, however, dampens the increase in the unemployment rate by an average of 0.30 p.p. to 0.44 p.p. for licensed workers in MSAs in the 90th percentile of

Figure 10: Event Study of Shielding Effect - 2008 Bartik Shock



Data: IPUMS Monthly Current Population Survey (2006 - 2010).

Note: This figure plots the time dummies of the shielding effect using Bartik exposure to measure shock intensity (Hershbein and Kahn, 2018). The dependent variable in all regressions is an unemployment indicator. The sample includes individuals between 18 and 65 who are in the labor force. Sample weights apply. Standard errors are clustered at the state level.

Table 10: The Shielding of Licensing in 2008 - Bartik Shock

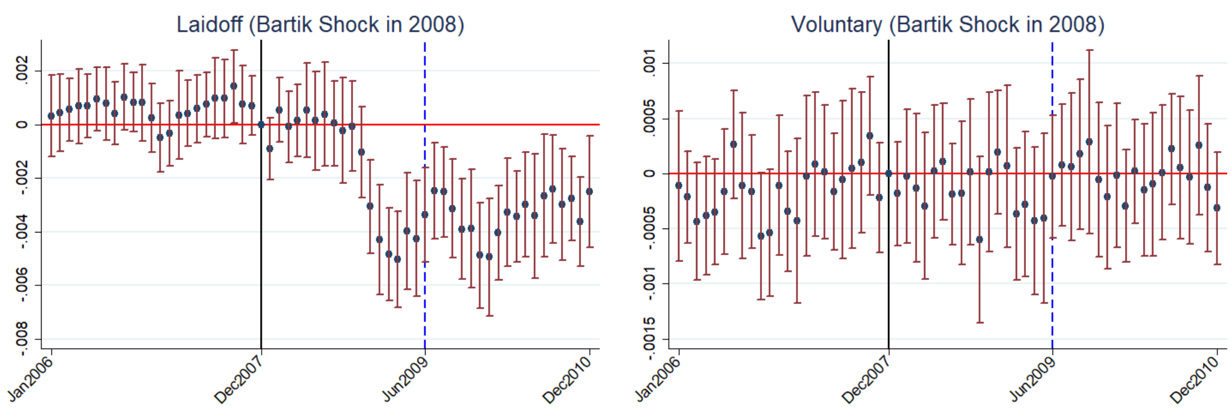
	(1)	(2)	(3)	(4)	(5)	(6)
lic. × Bartik × post	-0.00436***	-0.00441***	-0.00327***	-0.00317***	-0.00318***	-0.00297***
	(0.000363)	(0.000355)	(0.000320)	(0.000311)	(0.000331)	(0.000386)
license (lic.)	-0.00723***	0.000635	-0.00197*	-0.00219**	-0.00217**	-0.00216
	(0.000960)	(0.00113)	(0.00109)	(0.00109)	(0.00108)	(0.00147)
Bartik × post	0.0214***	0.0222***	0.0218***	0.0215***	0.0165***	0.0177***
	(0.00498)	(0.00512)	(0.00485)	(0.00495)	(0.00347)	(0.00341)
Observations	2,673,125	2,673,125	2,673,125	2,673,125	2,673,125	2,376,333
R-squared	0.034	0.041	0.042	0.042	0.042	0.042
Ind FE		X	X	X	X	X
College × Bartik × post			X	X	X	X
Union × Bartik × post				X	X	X
Regional trend					X	X
Sample			All workers			No universal licenses

Data: IPUMS Monthly Current Population Survey (2006 - 2010).

Note: Dependent variable in all regressions is an unemployment indicator. The sample includes individuals between 18 and 65 who are in the labor force. 'Shock' refers to the Bartik measure employed by Hershbein and Kahn (2018). 'License' is defined using the 50% threshold at the state-occupation level pooled from SIPP Panel 2008, the universal licensed professions (Johnson and Kleiner, 2020), and the felony license data. All regressions control for demographic characteristics (age, race and ethnicity, gender), a college indicator, a union indicator, a public sector indicator, a self-employed indicator, an indicator of whether the license is required by the job, an indicator of possessing a professional certification, state fixed effects. Sample weights apply. Standard errors are clustered at the state level.

the Bartik shock distribution when compared to their peers in MSAs in the 10 percentile. As was the case with COVID-19 results, controlling for educational attainment reduces the omitted variable bias of β_4 the most (by 25%). In our most stringent specification, we estimate that the magnitude of the shielding effect of occupational licensing is larger by 0.30 p.p. (or 18%) in MSAs that are hardest hit by negative labor demand shocks during the Great Recession – mirroring the lockdown results from the COVID-19 analysis.

Figure 11: Licensing Shields worker from layoffs during Great Recession



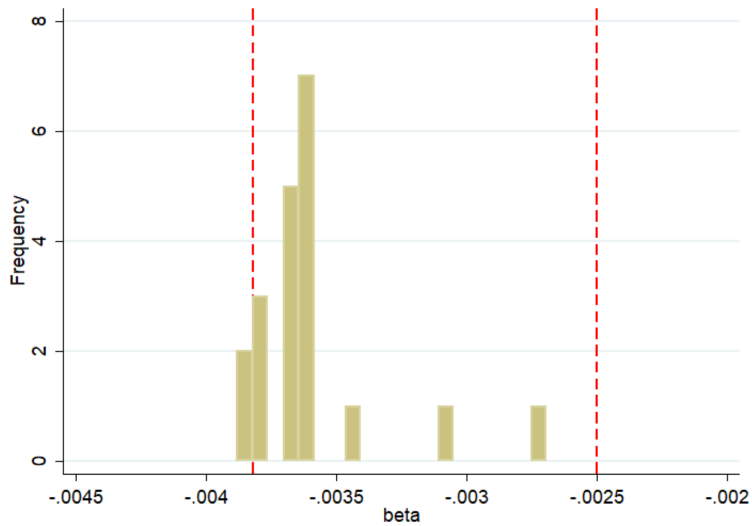
Data: IPUMS Monthly Current Population Survey (2006 - 2010).

Note: This figure compares the shielding effect on laidoff (upper-left), voluntary separation (upper-right), hourly wage (bottom-left), and hours worked (bottom-right). The event study regressions use the Bartik full specification. Sapmle weights apply. Standard errors are clustered at the state level.

Consistent with the evidence that licensing shields workers from unemployment during recessions most strongly in places where labor demand shocks are largest, in the top panel of Figure C2, we find that involuntary separations from layoffs explain the pattern that we observed in the data. By contrast, there is no change in unemployment due to voluntary separations during the recession.

The shielding effect of licensing during the Great Recession appears to be consistent across industries. Notably, we show in Figure 12 that dropping all observations from any one of the 20 industries and re-estimating the model does not yield substantial departures from the average treatment effect that we obtain from using all industries. Eighteen of the 20 estimated treatment effects fall within the 95% confidence interval that we obtain when we use all industries in the Bartik specification. The two iterations outside the confidence level in fact give a larger negative estimate than the estimate in column 5 of Table 10.[11].

Figure 12: Job Shielding of Licensing during the Great Recession Similar Across Industries



Note: This figure plots the distribution of the twenty Bartik estimates on the shielding effect of licensing by dropping the 20 industries one at a time using the regression from column 5 of Table 10. The red line marks the 95% confidence interval of the main estimate. Eighteen of the 20 estimated treatment effects fall within the 95% confidence interval that we obtain when we use all industries in the Bartik specification. Two of the iterations are significantly larger than the main estimate.

Overall, we find that occupational licensing protects licensed workers from job loss during the Great Recession, as it did during COVID-19. During both recessions, we find that occupational licensing provided the strongest protection against job loss due to layoffs and for workers in places that were hardest hit by negative labor demand shocks during the recession. Moreover, the magnitude of the job shielding impacts is similar over the 2-year period that we study.

5 Selection on Unobservables and Placebo Tests

In this section, we probe our results along three dimensions. First, we use the method in Altonji et al. (2005) and Oster (2019) to measure how large selection on unobservables relative to selection on observables would need to be in order to overturn our results.

[11]We also reproduce the permutation exercise using the DID estimation in Figure B1 in the appendix. Nineteen of the 20 DID estimates are within the confidence interval of the main DID estimate

Second, we exploit the two years of pre-recession data to conduct a placebo test to baseline whether our headline findings could have been generated from spurious correlations in the data. Third, we measure whether the shielding impacts of occupational licensing persist or dissipate overtime – extending our post-period from 24 months to 51 months after the Great Recession and to 33 months after the COVID-19 recession.

We follow the generalized approach developed by Oster (2019) in computing the implied ratio (δ) of the importance of selection on unobservables relative to selection on observables. The larger the ratio, the less likely our estimate of the shielding effect of occupational licensing during recessions is driven by omitted unobservables. Using the R^2 of our saturated model as the baseline (the regression model from column 6 of Table 4 for the COVID-19 recession and the regression model from column 5 in Table 9 for the Great Recession), in Table 11 we present the values of δ under different assumptions of about the maximum explanatory power, i.e., R^2_{max}, of a regression that includes both the variables that we observe and the omitted observables.

When $R^2_{max} = 1.1 \times R^2$, we assume that the omitted unobservables play a limited role that only explains 10% more of the residual variation of unemployment in the saturated model. The implied ratio of the shielding estimate for the 2008 recession is 3.268, meaning selection on unobservables needs to be about three times more important than selection on observables to nullify the shielding estimate. The implied ratio for the COVID-19 recession is even higher at 5.404. When $R^2_{max} = 1.3 \times R^2$, which is the recommended benchmark by Oster (2019), the implied ratio for COVID-19 and the Great Recession drops to 2.045 and 1.136, respectively. Since both ratios are above 1, selection on unobservables would have to be more important than selection on observables to entirely explain away our findings — an unlikely scenario given the guidance in that $\delta > 1$ implies implausibly large selection on unobservables (Altonji et al., 2005). When we further extend to an even more stringent standard $R^2_{max} = 1.5 \times R^2$ than the suggested benchmark, the value of δ for the COVID-19 recession remains above 1. Although the ratio for the 2008 recession drops to 0.687, it is 20% higher than the implied ratio of the license wage premium obtained in Kleiner and Krueger (2013) – a seminal result in the literature.

Table 11: Assessing Selection on Unobservables to Nullify the Shielding Estimate

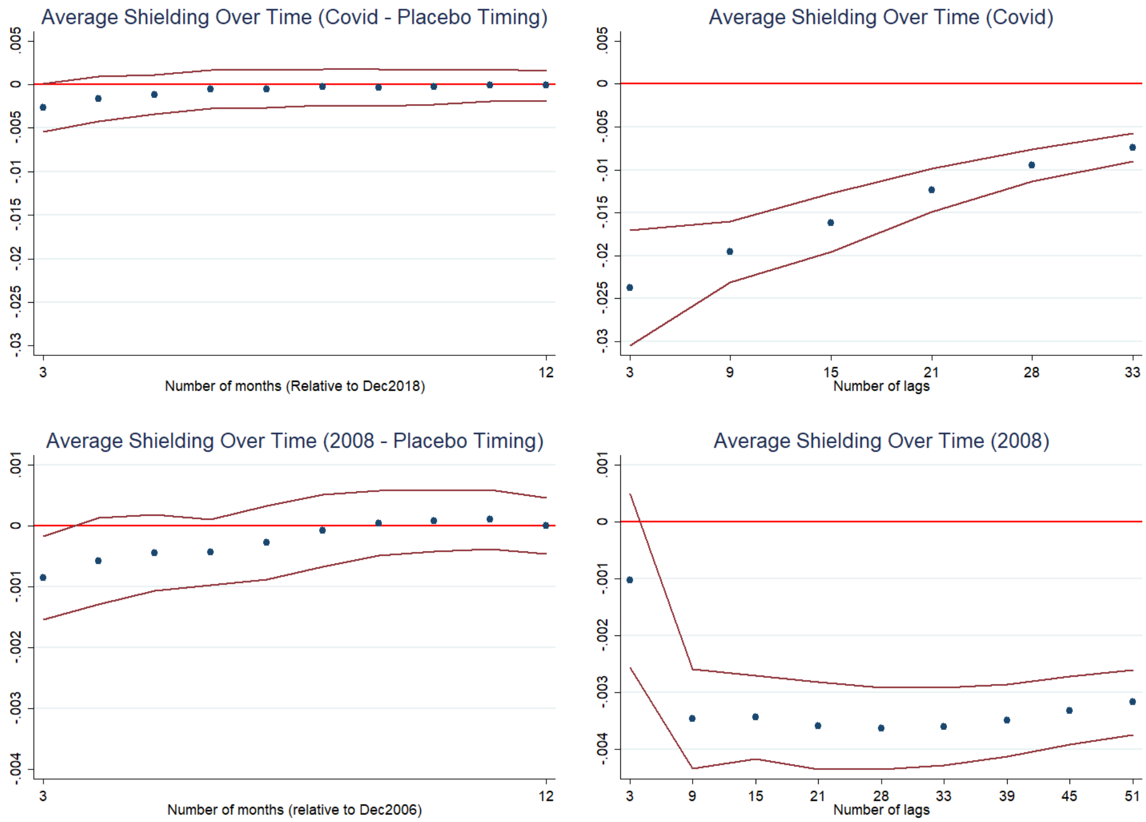
$R^2_{max} =$	$1.1 \times R^2$	$1.2 \times R^2$	$1.3 \times R^2$	$1.4 \times R^2$	$1.5 \times R^2$
COVID-19 Recession (δ)	5.404	2.967	2.045	1.560	1.261
Great Recession (δ)	3.268	1.685	1.136	0.856	0.687

Note: The numbers represent the implied ratios of selection on unobservables relative to selection on observables to completely explain away the shielding effect of licensing, under different assumptions of the explanatory power of a full model (R^2_{max}) (Altonji et al., 2005; Oster, 2019). The bigger the ratio, the less likely the shielding estimate is solely driven by omitted unobservables.

We complement our selection on unobservables test with a set of placebo tests. For the placebo tests, we split the 2 year pre-recession period in half and estimate the shielding effect of licensing assuming that the fictitious recession occurs in the second half of the actual pre-recession period. For our placebo analysis, we focus on estimating the triple difference models in which we exploit the intensity of the recessions using the lockdown variation from COVID-19 (Equation 3) and the Bartik variation from the Great Recession (Equation 4). In Figure 13 we report an estimate of the average shielding effect of licensing from placebo recessions averaged over months post-recession in increments of one month starting with 3 months post-recession going up to 12 months post-recession. For comparison purposes, we also report estimates of the average shielding effect of licensing using the actual recessions starting with 3 months post-recession going out as far post-recession as we possibly can, i.e., 51 months for the Great Recession and 33 months for the COVID-19 recession.

When we use the placebo recession date, we find very small and statistically insignificant treatment effects for both the COVID-19 recession and the Great Recession for the entire time from 3 months to 12 months post the fictitious recession. By contrast, when we use the factual recession dates, we find comparatively larger and statistically significant estimates of the job shielding effect of licensing that started immediately for the COVID-19 recession and at the 9-month mark for the Great Recession. Comparing the persistence

Figure 13: Placebo Effects (Left) and Persistence Effects (Right)



Note: The upper panel of the figure tracks the average shielding effect of licenses for workers with licenses in states that imposed lockdowns for COVID-19 for the placebo recession date (left) and the actual date of the COVID-19 Recession (right). The lower panel of the figure tracks the average shielding effect of the 2008 Bartik shock using the placebo recessions (left) and the actual date of the Great Recession (right).

of the job shielding effects of licensing from the factual recession dates, we find that the effect gradually fades out for the COVID-19 recession but is persistent for up to 51 months following the Great Recession. The difference in persistence mirrors the short-lived nature of the COVID-19 recession compared to the relatively protracted nature of the Great Recession (Rothstein, 2020; Chetty et al., 2024).

6 Spillovers from Licensed to Unlicensed workers

Dodini (2023) documents significant negative spillover of licensing on wage and employment of unlicensed workers. To measure the interaction between possible negative spillover and economic shocks, we construct state-industry unemployment rates in each month separately for all workers, licensed workers, and unlicensed workers. If we find that the state-industry unemployment rate for unlicensed workers increases more during the recession for unlicensed workers in state-industry pairs with higher levels of pre-recession licensed workers, this would be indirect evidence that the job shielding of licensed workers during a recession comes at the expense of job losses for unlicensed workers. Formally, for both the COVID-19-induced recession and the Great Recession and COVID-19, we run the following state-industry level difference-in-differences regression:

$$\begin{aligned} U_{sd,m} =& \beta_0 + \beta_1 LicenseExposure_{sd} + \beta_2 post \\ & + \beta_3 LicenseExposure_{sd} \times post + X_{sd,m}\Gamma + \theta_d + \theta_s + e_{sd,m}, \end{aligned} \tag{5}$$

where the outcome $U_{sd,m}$ is the unemployment rate of a state-industry sd in month m, 'post' is an indicator variable equal to one for all months after the onset of the recession, and the variable "LicenseExposure" is a standardized measure of the percent of licensed workers in that state-industry tabulated using the CPS in 2006 for the Great Recession and using the CPS data from 2018 for COVID-19. The control variables $X_{n,m}$ are the tabulated time-varying state-industry averages of worker characteristics. Sample weights apply to all tabulated variables and standard errors are clustered at the state level. Our coefficient of interest, β_3, measures the difference during the recession compared to before the recession in how much the industry-state unemployment rate changes for a one standard deviation increase in state-industry exposure to occupational licensing.

If job protection for licensed workers comes at the expense of higher job loss for unlicensed workers, then we would expect to see $\beta_3 > 0$ for the model in which our outcome is the state-industry unemployment rate for unlicensed workers. If instead $\beta < 0$ for the model in which our outcome is the state-industry unemployment rate for unlicensed workers, then the protection of the job experienced by licensed workers is not a zero-sum game. In Table 12 for both COVID-19 and the Great Recession, we report estimates of β_3 when our result is the state-industry unemployment rate for all workers (column 1),

Table 12: State-Industry License Exposure and Aggregate Effect

Worker Sample	All	Unlicensed	Licensed
COVID-19			
LicenseExposure*COVID-19	-0.00168	-0.00146	-0.00410***
	(0.00115)	(0.00119)	(0.00137)
LicenseExposure	0.00571***	0.00672***	0.00346*
	(0.00198)	(0.00210)	(0.00196)
COVID-19	0.0182***	0.0186***	0.0173***
	(0.00162)	(0.00169)	(0.00223)
Pre-COVID-19 Mean State-industry Unemployment	0.0371	0.0404	0.0185
Observations	58,631	58,411	48,560
R-squared	0.122	0.112	0.037
Great Recession			
LicenseExposure*Recession	-0.00332***	-0.00244**	-0.00179
	(0.000912)	(0.00111)	(0.00140)
LicenseExposure	0.00355**	0.00300*	0.00249
	(0.00169)	(0.00154)	(0.00207)
Recession	0.0313***	0.0329***	0.0251***
	(0.00194)	(0.00210)	(0.00188)
Pre-Recession Mean State-industry Unemployment	0.0427	0.0449	0.0352
Observations	58,983	58,568	53,585
R-squared	0.187	0.170	0.074

Data: IPUMS Monthly Current Population Survey.

Note: Dependent variable in all regressions is the state-occupation unemployment rate of the corresponding group of workers. The sample includes individuals between 18 and 65 who are in the labor force. 'LicenseExposure' is the tabulated percent of licensed workers in a state-industry using CPS in 2006 for Great Recession and 2018 for COVID-19, respectively. 'COVID-19' equals 1 for the sample months after March 2020. 'Recession equals 1 for the sample months after Dec 2007. All regressions control for average characteristics in Table 1, the average ability measures, state, and industry fixed effects. Sample weights apply. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

unlicensed workers (column 2) and licensed workers (column 3). Focusing on the results for unlicensed workers in column 2, we find that β_3 is negative in both COVID-19 and the Great Recession, which suggests that we do not find evidence that the protection of the jobs of licensed workers comes at the expense of unlicensed workers.

7 Inequality between Licensed and Unlicensed Workers

To ballpark the implications of our findings for inequality between licensed and unlicensed workers, we write a simple model of the indirect utility of a worker as a function of earnings and licensure status. During the recession, an individual with earnings $\omega_0(L)$ keeps his job with probability $p(L)$, where both earnings and the probability of keeping the job are a function of whether the occupation is licensed L.[12] Following the literature we assume earnings are a log-linear function of licensing, i.e. $log(\omega_0(L)) = \alpha + \psi L$ such that $\frac{\partial \omega_0}{\partial L} = \omega_0 \psi$, where ψ is the licensing earnings premium which consists of the impact of licensing on wages (Kleiner and Krueger, 2010, 2013; Gittleman et al., 2018) and on hours worked. We model the employment probability $p(L)$ as a linear function of licensing, and estimate the quantity $\frac{\partial p}{\partial L}$ from the data. With probability $1 - p(L)$, the worker is unemployed during the recession and earns a fraction of his regular wage: $\omega_0(1 - \delta)$, where $0 < \delta < 1$. We assume that δ is not a function of licensing, i.e., $\frac{\partial \delta}{\partial L} = 0$ since unemployment insurance is not a function of whether an individual works in a licensed occupation.

We further assume that the worker has an indirect utility function $V(\omega_0; \eta)$ that exhibits constant relative risk aversion η:

$$V(\omega_0) = \frac{\omega_0^{1-\eta} - 1}{1 - \eta} \implies -\omega_0 \frac{V''(\omega_0)}{V'(\omega_0)} = \eta. \tag{6}$$

Therefore, given the risk of unemployment (1-p), the worker's expected utility is:

$$E[U] = pV(\omega_0) + (1 - p)V(\omega_0(1 - \delta)), \tag{7}$$

where we have suppressed the L dependence of wages and the probability of employment to simplify the notation. To first order approximation when $\delta < 1$, the expected utility is:

$$E[U] \approx pV(\omega_0) + (1 - p)[V(\omega_0) - \delta\omega_0 V'(\omega_0)] = V(\omega_0) - \delta\omega_0(1 - p)V'(\omega_0). \tag{8}$$

[12] We assume that the earnings is net of the licensing cost, so that the functional dependence of the wage on licensure directly includes the cost of licensure.

With this linearization, it is clear that the worker's expected utility is decreasing in the probability of unemployment, $1-p$, and decreasing share of wage loss conditional on unemployment, δ. To measure how much licensing changes expected utility, we Taylor expand $E[U|L=1]$ about the point $L=0$ and show that the difference in expected utility between a licensed and unlicensed worker is approximately equal to marginal utility of licensing:

$$E[U|L=1] \approx E[U|L=0] + \left.\frac{dE[U]}{dL}\right|_{L=0} \times \Delta L \implies \left.\frac{dE[U]}{dL}\right|_{L=0} \approx E[U|L=1] - E[U|L=0]. \tag{9}$$

Differentiating the expected utility in Equation (8) with respect to L,[13] we obtain:

$$\begin{aligned}
\left.\frac{dE[U]}{dL}\right|_{L=0} &\approx V'(\omega_0)\frac{\partial \omega_0}{\partial L} - \frac{\partial \delta}{\partial L}\omega_0(1-p)V'(\omega_0) - \delta\frac{\partial \omega_0}{\partial L}(1-p)V'(\omega_0) && (10)\\
&+ \delta\omega_0\frac{\partial p}{\partial L}V'(\omega_0) - \delta\omega_0(1-p)V''(\omega_0)\frac{\partial \omega_0}{\partial L} && (11)\\
&\approx \underbrace{V'(\omega_0)\frac{\partial \omega_0}{\partial L}\left[1+(1-p)(\delta\eta-\delta)\right]}_{\text{Wage Effect}} + \underbrace{V'(\omega_0)\frac{\partial \omega_0}{\partial L}\left(\frac{\delta}{\psi}\frac{\partial p}{\partial L}\right)}_{\text{Unemployment Effect}} && (12)\\
& && (13)
\end{aligned}$$

The difference in expected utility for a worker on account of licensing arises either because licensing increases wages, the "wage effect" or because licensing changes the probability of getting laid-off during a recession, the "unemployment effect." The key parameters for quantifying the wage effect are known from the literature: $\eta = 1.19$ (Layard et al., 2008), $\delta = 0.218$ (Farber, 2011), $(1-p(L=0)) = 0.065$ (Table 1: COVID-19), $(1-p(L=0)) = 0.092$ (Table 8: Great Recession). Quantifying the unemployment effect relies on estimates of $\frac{\partial p}{\partial L}$, which are new to the literature and calculated in this paper. For the Great Recession we use: $\left.\frac{\partial p}{\partial L}\right|_{[L=0]} = 0.012$ (Table 9 column 6: Great Recession), which is based on the state-

[13] In practice our state-level measure of licensing, a binary variable, is constructed from a continuous measure — the fraction of workers in the occupation who are licensed — and then discretized using a threshold rule.

by-occupation variation in licensing using the 50% threshold from the literature (Blair and Chung, 2019). For the COVID-19 Recession $\frac{\partial p}{\partial L}\big|_{[L=0]} = 0.0142$ (Table A1: COVID-19) which measures the extent to which licensing shielded workers from unemployment in states that implemented lockdowns, and is also based on the state-by-occupation measure of licensing that uses the 50% threshold rule.

We must also calculate the licensing earnings premium ψ during the recession. Given our finding that licensed workers are less likely to be unemployed during recessions, it could be that licensed workers trade off lower relative unemployment risk for lower relative wages. A negative compensating differential of this type would cause the license premium to fall during recessions, in accordance with theoretical results and evidence of compensating differentials in the US labor market (Rosen, 1974, 1986; Sorkin, 2018).

We directly test the existence of a negative compensating differential. In Table 13, we present an estimate of the licensing earnings premium before and after a recession, following the approach in Equation (1), where our outcome is log earnings. The coefficient of interest is the interaction between license and recession, which captures how much the licensing earnings premium changes after a recession. For completeness, we also include regressions where we decompose the licensing earnings premium into the components due to the licensing wage premium and the licensing hours premium by regressing log(wages) and log(hours) on licensing and an interaction of licensing with a recession indicator following the approach in Equation (1).

We find that the licensing earnings premium before the COVID-19 recession was 4.61%.[14] In the aftermath of the COVID-19 Recession, the licensing earnings premium changes by an economically small and statistically insignificant 0.05 p.p., therefore we do not find evidence of a compensating earnings differential between licensed workers and unlicensed workers during the recession.[15] We find that the licensing earnings premium prior to the

[14] The licensing wage premium is 3.99% and contributes more to the licensing earnings premium than the licensing hours premium which is 0.61% and statistically insignificant.

[15] The absence of an overall compensating earnings differential masks the fact that the licensing wage premium goes down by 1.14 p.p. which is nearly enough to completely offset the increase in the licensing

Great Recession was 7.68%.[16] In the aftermath of the Great Recession, the licensing earning premium increases by a statistically significant 1.23 p.p., therefore, giving us a value $\psi_{GR} = 8.91$. Instead of a compensating differential, we find evidence of an augmenting differential, in the spirit of Mortensen (2003), that partially offsets the 2.1 p.p. reduction in earnings due to the recession.[17]

Table 13: Licensing Earnings Premium before and after recession

	COVID-19 Recession			Great Recession		
	log(income)	log(wage)	log(hours)	log(income)	log(wage)	log(hours)
license × recession	0.000527	-0.0114***	0.0121***	0.0123**	0.00409	0.00813**
	(0.00670)	(0.00410)	(0.00429)	(0.00487)	(0.00328)	(0.00314)
license	0.0461***	0.0399***	0.00614	0.0768***	0.0715***	0.00539*
	(0.00913)	(0.00829)	(0.00567)	(0.00722)	(0.00562)	(0.00290)
recession	-0.00770*	-0.00395	-0.00377	-0.0210***	0.00472*	-0.0257***
	(0.00427)	(0.00258)	(0.00367)	(0.00405)	(0.00257)	(0.00233)
Observations	350,371	350,555	350,371	448,583	448,812	448,583
R-squared	0.366	0.436	0.154	0.291	0.341	0.125

Data: IPUMS Monthly Current Population Survey.

Note: The sample includes individuals between 18 and 65 who are in the labor force. The 'license' variable in the Covid sample uses individual license attainment in CPS, while that in the 2008 sample uses the three external data mentioned in Section 4. 'Recession' indicator equals 1 for the sample months after March 2020 in column 1 to 3. 'Recession' indicator equals 1 for the sample months after Dec 2007 in column 4-6. All regressions run with the fully saturated model in the corresponding sample analysis. Sample weights apply. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

In Table 13, we report our values of the "wage effect" and the "unemployment effect" of licensing during the two recessions in units of marginal utility $V'(\omega_0)\frac{\partial \omega_0}{\partial L}$. For COVID-19 we find that the wage effect is 1.002 and the unemployment effect is 0.067. For the Great Recession, the wage effect is 1.001 and the unemployment effect is 0.027. Overall, we find that inequality between licensed and unlicensed workers is 2.9% to 6.7% larger than previously thought during the Great Recession and the COVID-19 Recession (respectively) on account of the fact that occupational licensing shields licensed workers from job loss relative to their unlicensed peers. For comparison, Finkelstein et al. (2024) find that the unemployment induced by the Great Recession increased life expectancy by 2.3%, which

hours premium of 1.21 p.p.

[16]It is primarily due to a licensing wage premium of 7.15%. The licensing hours premium is comparatively smaller 0.54% and contributes less to the earnings premium.

[17]The increase in licensing earnings premium following the Great Recession loads onto licensed workers experiencing a smaller decline in hours than unlicensed workers.

is the same order of magnitude of the estimates we find here, albeit for a different outcome.

Table 14: Decomposition of Inequality between licensed and unlicensed workers

	Wage Effect	**Unemployment Effect**
COVID-19 Recession	1.002	0.067
Great Recession	1.004	0.029

8 Conclusion

Occupational licensing affects a significant portion of the U.S. workforce and plays a central role in current policy discussions. This paper is the first to examine how occupational licensing interacts with economic recessions and its implications for inequality. We find that licensing provides licensed workers with protection from job loss during both the COVID-19 recession and the Great Recession. This "shielding effect" is a robust, industry-wide phenomenon driven by rehiring frictions created by licensing requirements during periods of reduced labor demand. Licensed workers are less likely to be laid off than their unlicensed peers, and this effect is not explained by voluntary quits.

Importantly, there is no negative wage differential accompanying this job protection, indicating that inequality between licensed and unlicensed workers widens during recessions. Our findings also show that the job-shielding effect of licensing persisted for several years after the Great Recession but diminished more quickly following the COVID-19 recession. This suggests that longer, more severe recessions may cause lasting shifts in labor market dynamics, increasing inequality by amplifying the demand for credentialed workers. This is consistent with the evidence that the Great Recession led to a sustained increase in the demand for workers with bachelor's degrees (Blair and Deming, 2020).

Moreover, the gap in unemployment rates between licensed and unlicensed workers heading into the COVID-19 recession reflects the long-lasting impact of the Great Recession in widening this disparity. While occupational licensing has traditionally been viewed as a barrier to entry that exacerbates inequality, our findings demonstrate that licensing also increases inequality by serving as a barrier to job exit, benefiting licensed workers more than their unlicensed counterparts during economic downturns.

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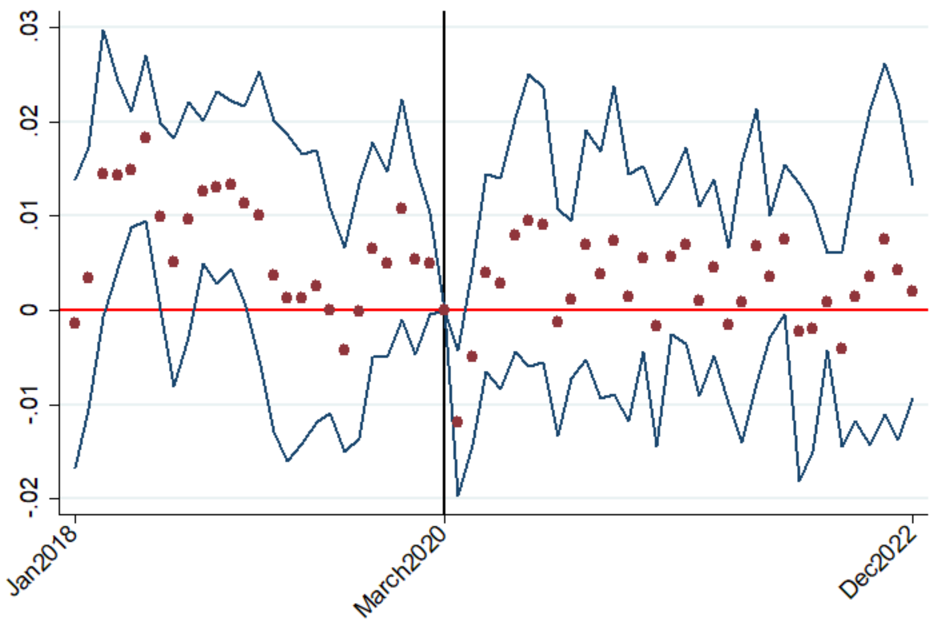
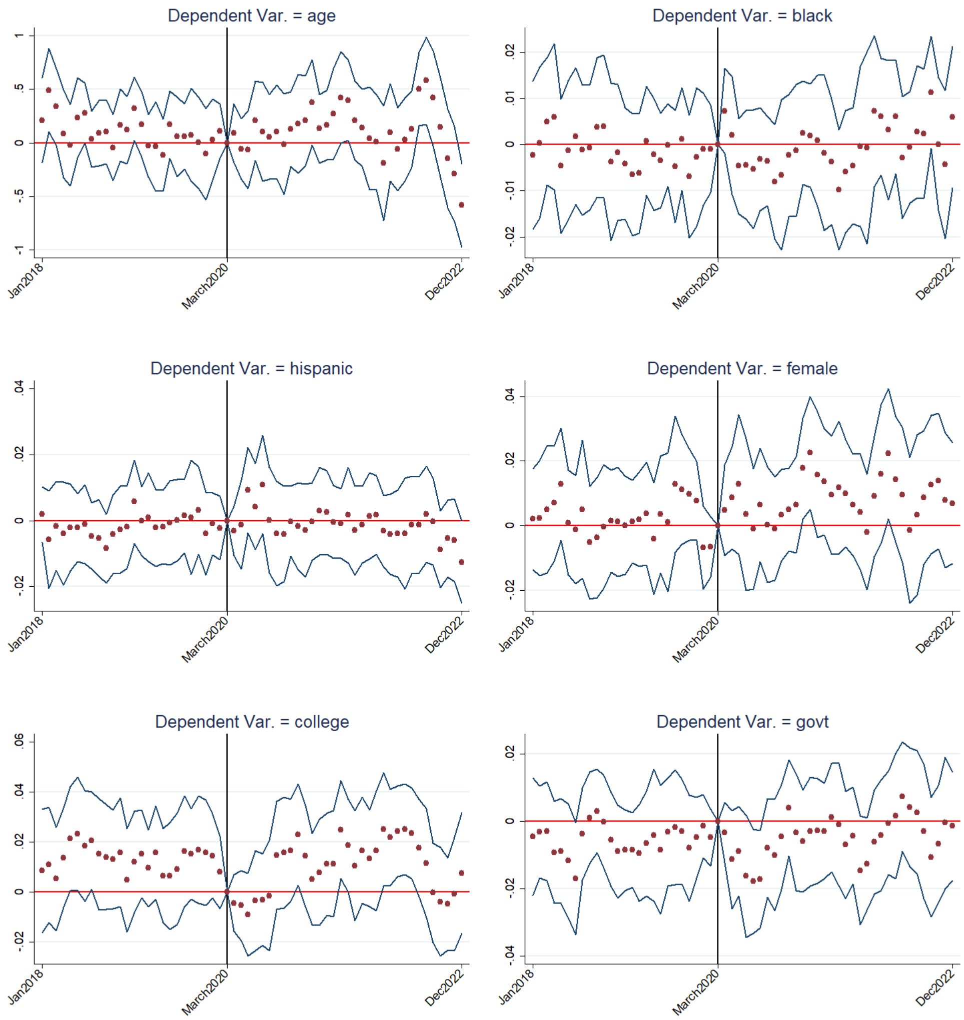
Appendix A: Additional Results (COVID-19 Recession)

Figure A1: Auxiliary Regression on License Status



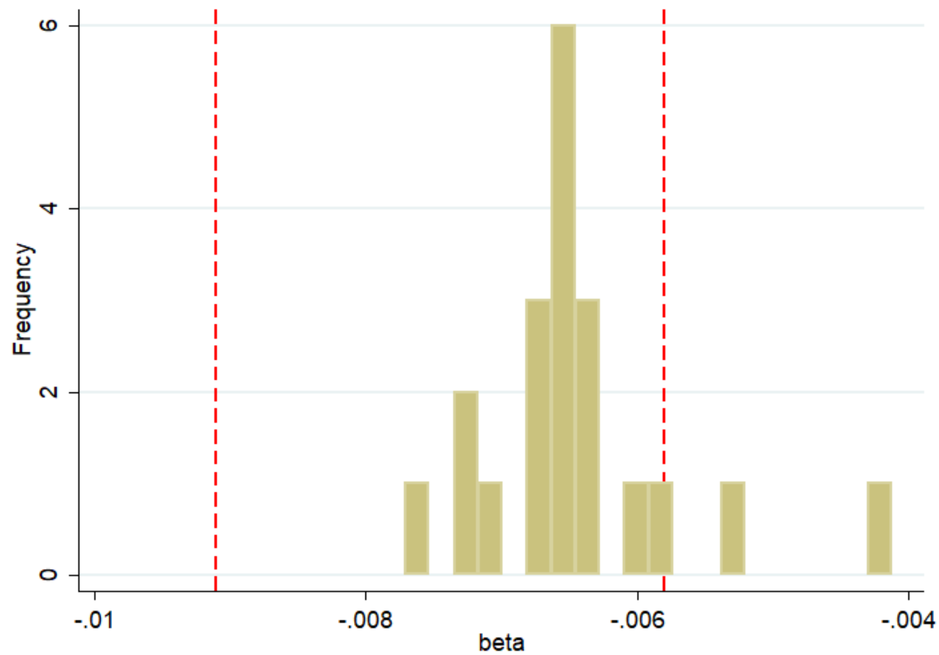
Note: This figure plots the event time dummies (with 95% confidence interval) on license status. License attainment of individuals before and after COVID-19 does not experience significant changes.

Figure A2: Auxiliary Regressions on Sample Characteristics



Note: This figure plots the event time dummies interacted with licensing (with 95% confidence interval) on the corresponding individual characteristics. The difference of characteristics between licensed and unlicensed individuals before and after COVID-19 does not experience significant changes.

Figure A3: Sensitivity Check: Iteration of Dropping 20 Industries (COVID-19)



Note: This figure plots the distribution of the twenty estimates on the shielding effect of licensing by dropping the 20 industries one at a time. The left shows the full model using the basic DID (comparable to column 6 of Table 4).

Table A1: DID Results using Licensing Thresholds - Covid

	(1) 0.5	(2) 0.45	(3) 0.4	(4) 0.35	(5) 0.3
license*covid	-0.00399***	-0.00361***	-0.00405***	-0.00346***	-0.00351***
	(0.00141)	(0.00131)	(0.00127)	(0.00125)	(0.000999)
license	-0.00505***	-0.00488***	-0.00521***	-0.00575***	-0.00671***
	(0.000674)	(0.000657)	(0.000604)	(0.000740)	(0.000698)
covid	0.0796***	0.0797***	0.0798***	0.0798***	0.0799***
	(0.00559)	(0.00559)	(0.00558)	(0.00559)	(0.00561)
Observations	3,019,461	3,019,461	3,019,461	3,019,461	3,019,461
R-squared	0.027	0.027	0.027	0.027	0.027

Data: IPUMS Monthly Current Population Survey (2018 - 2022).

Note: Dependent variable in all regressions is an unemployment indicator. The sample includes individuals between 18 and 65 who are in the labor force. 'License' is defined using the corresponding pre-shock threshold at the state-occupation level. All regressions control for demographic characteristics (age, race and ethnicity, gender), a college indicator, a union indicator, a public sector indicator, a self-employed indicator, an indicator of whether the license is required by the job, an indicator of possessing a professional certification, industry, state, and month fixed effects, and the additional controls in the full model. Sample weights apply. Standard errors are clustered at the state level. *** p$<$0.01, ** p$<$0.05, * p$<$0.1.

Appendix B: Additional Results (2008 Recession)

Table B1: Summary Statistics for Great Recession Analysis - Alternative cutoff

	Pre-Recession			Post-Recession			
	(1)			(2)			
	Unlicensed	Licensed	Diff	Unlicensed	Licensed	Diff	Diff-in-Diff
Outcome							
unemployed	0.053	0.033	-0.0203***	0.101	0.061	-0.0400***	-0.0197***
			(0.000973)			(0.00162)	(0.00143)
Individual characteristics							
female	0.432	0.539	0.107***	0.434	0.547	0.114***	0.00718***
			(0.0100)			(0.00961)	(0.00223)
age	39.566	41.255	1.689***	39.950	41.644	1.693***	0.00472
			(0.184)			(0.199)	(0.0589)
black	0.117	0.116	-0.000844	0.117	0.117	-0.000741	0.000103
			(0.00443)			(0.00415)	(0.00148)
hispanic	0.155	0.112	-0.0435***	0.163	0.117	-0.0453***	-0.00185
			(0.00872)			(0.00895)	(0.00151)
asian	0.047	0.046	-0.000840	0.048	0.047	-0.00106	-0.000218
			(0.00250)			(0.00213)	(0.00109)
union membership	0.240	0.256	0.0167***	0.228	0.251	0.0232***	0.00656***
			(0.00274)			(0.00258)	(0.000898)
college	0.251	0.438	0.186***	0.260	0.448	0.188***	0.00143
			(0.00735)			(0.00824)	(0.00308)
govt	0.117	0.202	0.0847***	0.119	0.207	0.0881***	0.00335*
			(0.0110)			(0.0118)	(0.00171)
self employed	0.102	0.110	0.00862***	0.097	0.102	0.00518*	-0.00344**
			(0.00284)			(0.00297)	(0.00148)
Observations	1514868	589399		1308510	516090		

Data: Monthly CPS (Jan 2018 to Dec 2022)

Note: 'Licensed' refers to individuals who work in a licensed state-occupation defined using the data described in Section **??**. 'Pre-Recession' refers the period before Aug 2008. Sample weight applies. Standard errors are clustered at the state level in testing differences.

Table B2: Difference-in-differences Estimates - 2008 Recession (Alternative Cutoff)

	(1)	(2)	(3)	(4)	(5)	(6)
license × recession	-0.0196***	-0.0198***	-0.0150***	-0.0146***	-0.0146***	-0.0134***
	(0.00141)	(0.00140)	(0.00125)	(0.00121)	(0.00127)	(0.00146)
license	-0.00803***	0.000187	-0.00206**	-0.00226**	-0.00220**	-0.00177
	(0.000904)	(0.00105)	(0.00102)	(0.00101)	(0.00102)	(0.00133)
recession	0.0489***	0.0492***	0.0558***	0.0566***	0.0404***	0.0402***
	(0.00304)	(0.00312)	(0.00366)	(0.00372)	(0.00190)	(0.00192)
Observations	3,928,867	3,928,867	3,928,867	3,928,867	3,928,867	3,496,543
R-squared	0.033	0.040	0.041	0.041	0.042	0.041
Ind FE		X	X	X	X	X
College x recession			X	X	X	X
Union x recession				X	X	X
Regional trend					X	X
Sample			All workers			Drop universal licenses

Note: Recession refers to the period after the beginning of the recession (Aug 2008).

Table B3: COVID-19 Recession: Effect of Licensing on Unemployment by Industry

Industry	Beta	SE	t-stat
Health	-0.0095	0.0021	-4.566
Public Administration	-0.0066	0.0018	-3.662
Education	-0.0070	0.0020	-3.593
Retail	-0.0159	0.0045	-3.555
Transportation	-0.0108	0.0035	-3.039
Accommodation & Food	-0.0166	0.0106	-1.559
Construction	-0.0039	0.0038	-1.041
Professional	-0.0019	0.0019	-1.032
Manufacturing	-0.0030	0.0140	-0.211
Arts	-0.0022	0.0107	-0.204
Information	-0.0034	0.0170	-0.202
Administrative	-0.0010	0.0060	-0.165
Wholesale	-0.0012	0.0078	-0.151
Utility	-0.0001	0.0052	-0.026
Mining	0.0003	0.0212	0.014
Real Estate	0.0006	0.0047	0.121
Finance	0.0028	0.0031	0.910
Agriculture	0.0109	0.0089	1.235
Management	0.0723	0.0580	1.247
Other Services	0.0063	0.0044	1.422

Data: IPUMS Monthly CPS (2018–2022).

Notes: Entries report industry-specific estimates of the effect of licensing on an unemployment indicator during the COVID-19 recession. Coefficients (Beta) are from linear probability models; standard errors (SE) are clustered at the state level; the final column reports t-statistics. Sample includes labor-force participants ages 18–65. Controls include age, race/ethnicity, gender, college indicator, union indicator, public-sector indicator, self-employed indicator, an indicator for whether the license is required by the job, an indicator for professional certification, and state fixed effects. Negative coefficients indicate that licensed workers were less likely to be unemployed than unlicensed workers. None of the positive coefficients are statistically significant.

Table B4: Great Recession: Effect of Licensing on Unemployment by Industry

Industry	Beta	SE	t-stat
Professional	-0.0159	0.0029	-5.411
Public Administration	-0.0320	0.0062	-5.158
Wholesale	-0.0112	0.0048	-2.361
Retail	-0.0108	0.0047	-2.299
Arts	-0.0177	0.0077	-2.285
Health	-0.0050	0.0025	-2.032
Real Estate	-0.0136	0.0067	-2.024
Other Services	-0.0077	0.0044	-1.751
Education	-0.0043	0.0033	-1.293
Manufacturing	-0.0154	0.0125	-1.225
Administrative	-0.0134	0.0111	-1.210
Construction	-0.0067	0.0055	-1.204
Mining	-0.0058	0.0232	-0.252
Finance	0.0010	0.0039	0.257
Agriculture	0.0062	0.0158	0.391
Accommodation & Food	0.0035	0.0071	0.497
Information	0.0058	0.0104	0.557
Management	0.0486	0.0716	0.679
Utility	0.0105	0.0122	0.864
Transportation	0.0077	0.0060	1.291

Data: IPUMS Monthly CPS (2006–2010).

Notes: Entries report industry-specific estimates of the effect of licensing on an unemployment indicator during the Great Recession. Coefficients (Beta) are from linear probability models; standard errors (SE) are clustered at the state level; the final column reports t-statistics. Sample includes labor-force participants ages 18–65. Controls include age, race/ethnicity, gender, college indicator, union indicator, public-sector indicator, self-employed indicator, an indicator for whether the license is required by the job, an indicator for professional certification, and state fixed effects. Negative coefficients indicate that licensed workers were less likely to be unemployed than unlicensed workers. None of the positive coefficients are statistically significant.

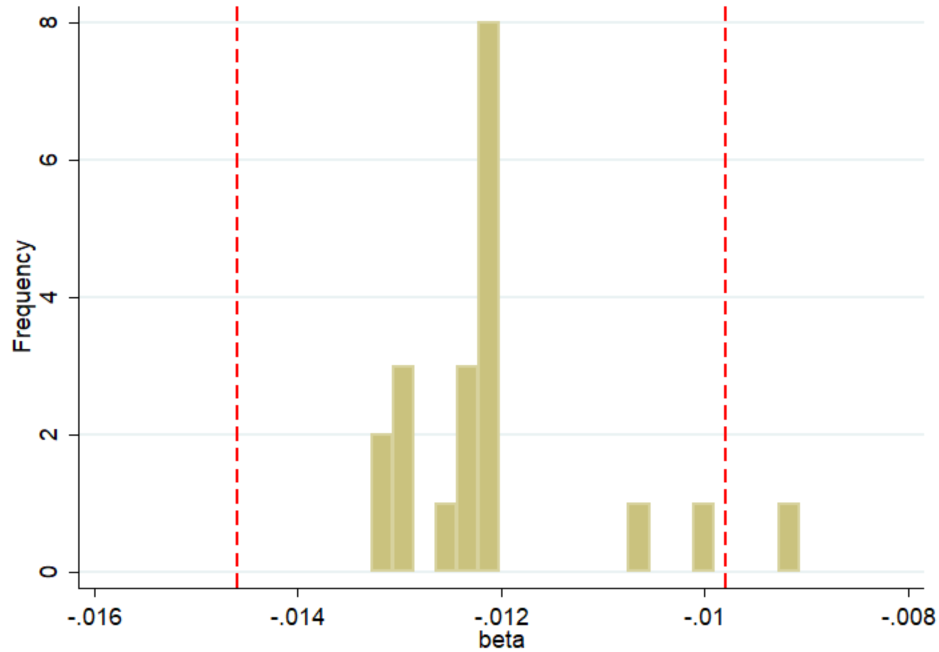
Table B5: Bartik Shock - Aug 2008 as Alternative cutoff

	(1)	(2)	(3)	(4)	(5)	(6)
license*shock	-0.00523***	-0.00529***	-0.00402***	-0.00390***	-0.00390***	-0.00374***
	(0.000383)	(0.000373)	(0.000359)	(0.000351)	(0.000368)	(0.000421)
license	-0.00788***	-1.56e-05	-0.00228**	-0.00249**	-0.00248**	-0.00230
	(0.000919)	(0.00112)	(0.00110)	(0.00111)	(0.00109)	(0.00141)
shock	0.0227***	0.0235***	0.0232***	0.0228***	0.0187***	0.0198***
	(0.00508)	(0.00523)	(0.00495)	(0.00506)	(0.00373)	(0.00374)
Observations	2,673,125	2,673,125	2,673,125	2,673,125	2,673,125	2,376,333
R-squared	0.035	0.041	0.042	0.042	0.042	0.042
Ind FE		X	X	X	X	X
College x shock			X	X	X	X
Union x shock				X	X	X
Regional trend					X	X
Sample			All workers			Drop universal licenses

Data: IPUMS Monthly Current Population Survey (2006 - 2010).

Note: Dependent variable in all regressions is an unemployment indicator. Sample includes individuals between 18 and 65 who are in the labor force. 'Shock' refers to the Bartik measure employed by Hershbein and Kahn (2018). 'License' is defined using the 50% threshold at the state-occupation level pooled from SIPP Panel 2008, the universal licensed professions (Johnson and Kleiner, 2020), and the felony license data. All regressions control for demographic characteristics (age, race and ethnicity, gender), a college indicator, a union indicator, a public sector indicator, a self-employed indicator, an indicator of whether the license is required by the job, an indicator of possessing a professional certification, state fixed effects. Sample weights apply. Standard errors are clustered at the state level.

Figure B1: Sensitivity Check: Iteration of Dropping 20 Industries (2008 Recession)



Note: This figure plots the distribution of the twenty estimates on the shielding effect of licensing by dropping the 20 industries one at a time and running the regression in column 5 of Table 9. The red line marks the 95% confidence interval of the main estimate. The raw coefficients in the 20 iterations are all significant at 1% level. Nineteen of the 20 DID estimates are inside the confidence interval of the main estimate.

Appendix C: Other Outcomes

Table C1: Covid - individual

VARIABLES	(1) lincome	(2) lwage	(3) lhurs
license_recession	-0.00716	-0.00931**	0.00228
	(0.00550)	(0.00401)	(0.00413)
license	0.155***	0.109***	0.0466***
	(0.00481)	(0.00316)	(0.00365)
recession	-0.00736	-0.00390	-0.00347
	(0.00452)	(0.00250)	(0.00379)
Constant	3.210***	-0.149	3.359***
	(0.184)	(0.126)	(0.109)
Observations	351,560	351,744	351,560
R-squared	0.371	0.440	0.155

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure C1: Event graph - Covid individual

Table C2: Covid - 5050

VARIABLES	(1) income	(2) lwage	(3) lhurs
license1_recession	0.000527	-0.0114***	0.0121***
	(0.00670)	(0.00410)	(0.00429)
license1	0.0461***	0.0399***	0.00614
	(0.00913)	(0.00829)	(0.00567)
recession	-0.00770*	-0.00395	-0.00377
	(0.00427)	(0.00258)	(0.00367)
Constant	3.228***	-0.142	3.370***
	(0.187)	(0.128)	(0.109)
Observations	350,371	350,555	350,371
R-squared	0.366	0.436	0.154

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C3: 2008

VARIABLES	(1) lincome	(2) lwage	(3) lhurs
license_recession	0.0123**	0.00409	0.00813**
	(0.00487)	(0.00328)	(0.00314)
license	0.0768***	0.0715***	0.00539*
	(0.00722)	(0.00562)	(0.00290)
recession	-0.0210***	0.00472*	-0.0257***
	(0.00405)	(0.00257)	(0.00233)
Constant	4.967***	1.339***	3.628***
	(0.0612)	(0.0448)	(0.0314)
Observations	448,583	448,812	448,583
R-squared	0.291	0.341	0.125

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure C2: Licensing Shields worker from layoffs during Great Recession

Data: IPUMS Monthly Current Population Survey (2006 - 2010).

Note: This figure compares the shielding effect on laidoff (upper-left), voluntary separation (upper-right), hourly wage (bottom-left), and hours worked (bottom-right). The event study regressions use the Bartik full specification. Sapmle weights apply. Standard errors are clustered at the state level.