Occupational Licensing and Labor Market Fluidity*

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Abstract

We show that occupational licensing has significant negative effects on labor market fluidity, defined as cross-occupation mobility, and positive effects on wage growth. Using a balanced panel of workers constructed from Current Population Survey (CPS) and Survey of Income and Program Participation (SIPP) data, we analyze the link between occupational licensing and labor market outcomes. We find that occupational licensing represents a barrier to entry for both non-employed workers and employed ones. The effect is more prominent for employed workers than those entering from non-employment, because the opportunity cost of acquiring a license is much higher for employed individuals. We also find that average wage growth is higher for licensed workers than non-licensed workers. Workers who have just switched into an occupation that requires licensing also experience higher wage growth on average than their non-licensed counterparts whether they have switched in from other occupations or from unemployment. We find significant heterogeneity in the licensing effect across different occupation groups. These results hold across various data sources, time spans, and indicators of being licensed. Finally, we see that during the COVID-19 pandemic the effect of licensing on occupational transitions is similar that of to the pre-pandemic period, but the effect on wage growth disappears.

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1 Introduction

Over one-fifth of all U.S. workers are employed in licensed occupations—jobs that require a government license. This number was only 5% about 70 years ago (Kleiner and Krueger (2013) and Cunningham (2019)). The increase in the number of licensed occupations and workers was generally motivated as a consumer protection measure. It can benefit public by providing better services and reducing potential injury at work. However, licensing may also represent a public choice that focuses on monopoly and rent capture. Therefore, for workers who are trying to enter a profession or change professions, increased licensing may become a barrier that restricts geographical and occupational worker mobility. It can also increase consumer prices, decrease worker welfare, and ultimately impair economic growth (Chetty (2009), Johnson and Kleiner (2020), and Kleiner and Soltas (2019)).

We use public individual-level survey data from the Current Population Survey (CPS) and the Survey of Income and Program Participation (SIPP), as well as a newly constructed data set on licensing requirements, the Occupational Licensing Law Research Project (OLLRP), to investigate the relationship between occupational licensing and labor market fluidity, defined as cross-occupation movement. The OLLRP contains a detailed range of occupational licensing requirements for all of the universally licensed occupations across every state from the 1980s to 2020.[1] We employ this detailed long panel dataset on licensing requirements to document the trends in licensing across different states and occupations. Our paper is one of the first to examine the effects of licensing on occupational switching rates, to examine the movement out of unemployment to licensed and unlicensed occupations, and to document the wage growth associated with switching and licensing. The focus of our analysis is to examine the relationship between licensing and labor market fluidity. Davis and Haltiwanger (2014) argue that reduced fluidity has harmful consequences for productivity, real wages, and employment. Lower fluidity among licensed workers could be harmful for individual labor market outcomes as well as for the aggregate economy. Figure 1 shows that over the past two decades, although the licensing rate (the fraction of the workforce that is licensed) has trended up steadily, the occupational mobility or switching rate has declined. This suggests that there may be a link between workers' licensing status and occupational switching decisions. Using the CPS to focus on occupation-level data, we find that occupations with higher licensing shares are experiencing relatively lower churn rates.[2] Figure 2 shows the

[1] A universally licensed occupation is one that requires a license in every state in the U.S. The full list of these occupations is presented in Appendix A.

[2] Licensing share is defined by the share of the total working population that holds a government-issued

relationship between the licensing share and the switching-out rate, while Figure 3 shows the relationship between the licensing share and the switching-in rate. The negative correlation between the share of workers who are licensed and the occupational churn rate suggests that licensing may have a negative effect on individuals' labor market transition decisions.

Much of the previous literature investigates the relationship between occupational licensing and labor market outcomes. Kleiner and Krueger (2013) and Kleiner and Vorotnikov (2017) show that occupational licensing is associated with higher average wages and has implications for wage inequality and income distribution. Kleiner (2000) shows that occupational licensing reduces labor supply while increasing labor prices. Wiswall (2007) focuses on the market for teachers and finds that licensing reduces the supply of teachers and the quality of teaching, while increasing the average length of teaching careers. These papers focus generally on cross-sectional employment outcomes rather than worker employment dynamics. Recent papers have expanded this area of research. Gittleman, Klee and Kleiner (2018) investigate the effect of licensing on the probability of being employed as well as of receiving employer-sponsored health insurance. Johnson and Kleiner (2020) show the impact of occupational licensing on reducing interstate migration rates. Another set of studies has focused on the declining trend in labor market dynamism. For example, Hyatt (2015) shows that the rate at which workers switch jobs has been trending down over the last few decades. Hiring and job creation rates have also been declining. Moscarini and Vella (2008) use the CPS to document the declining trend in occupational switching frequency, while Xu (2022) investigates how much of this trend is due to increases in occupational switching costs (such as occupational licensing). This paper connects these two literatures. We focus on worker employment dynamics and investigate how licensing affects occupation switching probabilities as well as wage changes associated with occupational switches. This connection will help us investigate, through an endogenous career-choice dynamic structural framework, the impact of licensing on workers' lifetime welfare changes, as well as the overall welfare effects of occupational licensing, which are also examined by Kleiner and Soltas (2019).

One of our contributions is a detailed analysis of the Occupational Licensing Law Research Project (OLLRP) data set, which establishes new historical data patterns and trends in occupational licensing. Specifically, we measure the degree to which occupational licensing costs have changed (increased) for workers over time and across different states and occupations. Our second contribution is to examine the licensing effect on the dynamic aspects of workers' labor market outcomes. We study the relationship between licensing, occupational

license in a given occupation.

switching, and the wage changes associated with these transitions. We compare the switching patterns and wage changes of workers who are licensed with those of workers who are not. Our findings show that occupational licensure has significant negative effects on occupational mobility, especially for workers who switch into a licensed occupation. Specifically, we find that workers who are licensed are 8 percentage points less likely than other workers who are not licensed to have just switched into their occupation. After controlling for observable heterogeneity as well as the state fixed effects, time fixed effects, and the occupational fixed effects from both the source and destination occupation, we find that those switching into a licensed occupation experience higher wage gains (a 7 percentage point higher growth rate, or an additional \$2.8k[3] annual wage income for average workers[4]) than those switching into a non-licensed occupation. Workers who are currently licensed experience higher wage growth next year, regardless of whether they stay in the same occupation (on average, 6 percentage points higher in log wage changes or \$2.1k more annually than non-licensed workers) or switch occupations (on average, 4 percentage points higher or \$1.2k annually more than non-licensed workers). Furthermore, we find that licensing presents less of a barrier to entry for the unemployed than it does for employed workers, possibly because of differences in the opportunity cost of time to meet government requirements between the two groups. We use various strategies – including evaluating the parameter stability due to omitted variable bias, following Altonji, Elder and Taber (2005) and Oster (2019) – as well as different datasets to confirm the robustness of the licensing effect on worker mobility and wage growth. Finally, we do a back-of-the envelope calculation and find that licensing can account for at least 7.7% of the total decline in occupational mobility over the past two decades. These results suggest that in addition to the static general equilibrium welfare effects that Kleiner and Soltas (2019) suggest, licensing also has significant lifetime welfare effects for workers through impacting their dynamic occupational career choices.

The paper proceeds as follows. In section 2, we present a simple dynamic model of occupational choice, which we use to investigate how licensing may affect occupational switching decisions. This model guides our empirical analysis. In section 3, we introduce the various data sources that we use in our analysis, and we provide details on the sample selection and data cleaning processes. Section 4 outlines our empirical analysis and baseline results, and we perform several robustness analyses in section 5. We then focus on the COVID-19

[3] All dollar amounts in this paper are in 2000 real dollars.

[4] The licensing dollar effect is calculated using the group mean wage multiplied by the marginal licensing effect within the group. In this case, the group consists of workers who have just switched into their current occupation.

pandemic period in section 6 to explore the licensing effect on workers occupational mobility and wages. We summarize and conclude in section 7.

2 Theoretical Model

In this section, we build a simple recursive dynamic discrete-choice model, and we discuss how licensing may affect workers' occupation choices and switching decisions through the lens of the model. We then discuss how licensing can affect workers' lifetime welfare through its dynamic impacts on occupational choices.

Consider a world in which over the course of their careers, workers choose a sequence of occupations in order to maximize their expected lifetime discounted utility. Workers receive utility from consumption and non-pecuniary preference shocks for each occupational choice. The discounted stream of utility being maximized by the worker is

$$\sum_{a=1}^{A} \beta^{a-1} \Big[u(c_{ija}) + \zeta_{ija} \Big],$$

where the subscripts denote worker i in occupation j at age a, and A is the age at retirement. Workers receive occupation-specific preference shocks ζ_{ija} for every occupation j at the beginning of each time period and then decide whether they want to stay in their current occupation or switch to another one. A worker's consumption while employed at a particular job can be written as

$$c_{ija} = w_{ija} - Cost_{ioja};$$

w_{ija} denotes worker i's wage in occupation j at age a. The $Cost_{ioja}$ term represents the costs incurred at age a for a worker in occupation j who has switched from occupation o in the previous period $(a-1)$. We consider the following switching cost term:

$$Cost_{ioja} = \begin{cases} (\kappa_1 + \kappa_3 w_{ioa-1})\mathbf{1}_{o \to j_L} + (\kappa_2 + \kappa_4 w_{ioa-1})\mathbf{1}_{o \to j_N} & \text{if } o \neq j. \\ 0 & \text{if } o = j \end{cases}$$

The switching cost is zero if the worker does not switch occupations in the next period $(o = j)$. When $o \neq j$, the switching cost differs depending on whether the destination occupation is licensed (subscript L) or non-licensed (N). The entrance fees for licensed and non-licensed occupations are captured by κ_1 and κ_2, respectively; κ_3 and κ_4 take values

between 0 and 1 and are multiplied by the current wage to capture the opportunity costs of switching to licensed and non-licensed occupations. We describe a worker's state as a vector x of the state variables (other than their current occupation) that workers take into account when they make their occupational decisions for the next period. The vector x may include age, previous wage w_{ioa-1}, tenure, occupational skills, and so on. We then write the worker's problem recursively as

$$W(x, o, \zeta) = \max_{j \in J}(V(x, o, j) + \zeta^j),$$

where J is the set of possible occupational choices and

$$V(x, o, j) = u(x, o, j) + \beta \sum_{x'} p(x'|x, j)\mathbb{E}_{\zeta'} W(x', j, \zeta').$$

Note that the first two arguments of V denote the worker's current state, x, and previous occupation, o, while the third argument denotes the worker's occupation choice for the current period, j. For simplicity, we assume linear utility, so $u(x, o, j) = w(x, j) - Cost(x, o, j)$. We also assume ζ^j is drawn from a type I extreme value distribution. In this case, following Rust (1987), the probability of choosing occupation j in state x is

$$q(x, o, j) = \frac{\exp(V(x, o, j))}{\sum_{k \in J} \exp(V(x, o, k))}.$$

This allows us to integrate out over the preference shock, and it greatly simplifies the solution to the worker's problem.

Depending on the choices of state variables and wage structure, the analytical solution to the problem described above may not be feasible. However, one can compute workers' value V by value function iteration starting from age $a = A$ and working backwards. It is then feasible to take the model to the appropriate data and estimate the relevant parameters.[5] In this paper, we do not intend to perform full structural estimation. Instead, we use the model to simply illustrate the mechanisms of how licensing affects workers' occupation choices and welfare. We make several assumptions to simplify the model and derive analytical solutions for illustrative purposes, but the key mechanisms hold in the full model as well. The first assumption is that there are only three occupational choices: a licensed occupation (L), a non-licensed occupation (N), and unemployment (U).[6] Second, we assume there is a single

[5] See Xu (2022) for a similar analysis with full structural estimation.

[6] We follow Traiberman (2019) and treat non-employment as a voluntary choice of workers. Doing so greatly simplifies the occupational choice problem, as we interpret workers going to unemployment as the result of a very positive preference shock for unemployment or very negative preference shocks for all the

market wage for each choice, where w_L denotes the wage for the licensed occupation, w_N denotes the wage for the non-licensed occupation, and b denotes unemployment income. We do not consider the general equilibrium effect on wages, and we abstract away from endogenous human capital or skill accumulation so there is no wage growth, though this assumption can easily be relaxed. Our final assumption is that workers' state contains only their past occupation and age. Workers are ex-ante homogeneous before sorting into their first occupation. We will discuss how we can relax these assumptions and what the implications are for each.

Given the above simplifications, we are now ready to solve the worker's occupational switching decision for the last period before retirement. When $a = A$,[7] we can write all of the choice specific worker values as

$$\begin{aligned}
&V(A,L,L) = w_L, && V(A,L,N) = w_N, && V(A,L,U) = b \\
&V(A,N,N) = w_N, && V(A,N,L) = w_L - \kappa_1 - \kappa_3 w_N, && V(A,N,U) = b \\
&V(A,U,L) = w_L - \kappa_1, && V(A,U,N) = w_N, && V(A,U,U) = b.
\end{aligned}$$

Note that in the last period, there is no continuation value for workers, so workers consider only the wage (and non-pecuniary shock) that comes with the occupation they choose and the switching costs associated with it. Without loss of generality, we normalize the non-licensed switching cost parameters κ_2 and κ_4 to be equal to zero. This means that κ_1 and κ_3 should be interpreted as the additional costs for entering the licensed occupation relative to those of the non-licensed occupation. We also assume workers who move to unemployment do not pay any costs, and unemployed workers entering licensed occupations need to pay only the entry cost, not the opportunity cost, since workers do not lose unemployment insurance even when they are training for a new occupation. We are now ready to study how licensing affects worker entry decisions in this simple model. We focus first on workers who are currently employed. The probability of a non-licensed worker switching to a licensed occupation in the last period is

other potential occupations.

[7] Workers retire after $a = A$. We do not model the retirement problem, as it is beyond the scope of this paper.

$$
\begin{aligned}
Prob_{N\to L} &= \frac{\exp(V(A,N,L))}{\exp(V(A,N,L))+\exp(V(A,N,N))+\exp(V(A,N,U))} \\
&= \frac{1}{1+\exp((1+\kappa_3)w_N+\kappa_1-w_L)+\exp(b+\kappa_1+\kappa_3 w_N-w_L)}.
\end{aligned}
$$

Similarly, we get the probability of licensed workers entering a non-licensed occupation:

$$
\begin{aligned}
Prob_{L\to N} &= \frac{\exp(V(A,L,N))}{\exp(V(A,L,N))+\exp(V(A,L,L))+\exp(V(A,L,U))} \\
&= \frac{1}{1+\exp(w_L-w_N)+\exp((b-w_N)}.
\end{aligned}
$$

Note that the switching cost parameters κ_1 and κ_3 (which are a function of the relevant licensing policies) are crucial in determining the probability of worker entry into licensed occupations. When κ_1 and/or κ_3 increases—that is, when the licensing cost increases (or when an occupation becomes licensed)—the probability of worker entry declines. Furthermore, when the average wages for licensed and non-licensed occupations are similar ($w_L \approx w_N$), the entry rate into the licensed occupation is always smaller than the one into the non-licensed occupation ($Prob_{N\to L} < Prob_{L\to N}$).

The problem becomes more complex if we consider the effect of occupational licensing in general equilibrium. Generally, licensing costs are introduced as a consumer protection measure. Suppose service or product quality is tied to worker ability. Then, if the training requirements to obtain a license increase (κ_3 goes up, and one has to forgo more income to train and obtain a license), the average quality of the service or product may grow as lower-ability workers choose not to enter the occupation, thereby increasing the demand and wages (w_L) for that occupation's output and so increasing incentives to obtain the license in turn. The overall effect of switching costs on the probability of workers' moving into a licensed occupation then depends on a comparison between the marginal costs and marginal benefits of obtaining the license:

$$
\frac{dProb_{N\to L}}{d\kappa_3} = \frac{\Big[\exp((1+\kappa_3)w_N+\kappa_1-w_L(\kappa_3))+\exp(b+\kappa_1+\kappa_3 w_N-w_L(\kappa_3))\Big](\frac{dw_L}{d\kappa_3}-w_N)}{\Big(1+\exp((1+\kappa_3)w_N+\kappa_1-w_L)+\exp(b+\kappa_1+\kappa_3 w_N-w_L)\Big)^2}.
$$

Notice that the expression takes the same sign as the last term of the numerator, so the sign of $(\frac{dw_L}{d\kappa_3}-w_N)$ determines whether workers are more or less likely to switch into licensed

occupations when the training cost increases. If the forgone income from training is outweighed by the increased wage premium of licensing, then workers will be more willing to switch. The empirical work on how licensing affects worker entry probabilities will inform us on the relative values of these marginal costs and benefits. The simple model above also has an interesting implication for how age interacts with licensing and switching costs. Younger workers will have greater incentives than older workers to pay licensing costs, since the marginal benefit for younger workers is due largely to their longer career prospects compared with those of older workers. Finally, we see that licensing entrance fees (κ_1) always decrease the probability of workers' entering licensed occupations.

We then investigate how licensing affects worker job security. One caveat is that in our model, workers voluntarily switch into unemployment. However, we can interpret this as the probability of workers getting large negative shocks to preferences for working and therefore "voluntarily" moving into unemployment. We compare the probability of switching into unemployment for non-licensed and licensed workers:

$$\begin{aligned}
Prob_{N\to U} &= \frac{\exp(V(A,N,U))}{\exp(V(A,N,U)) + \exp(V(A,N,N)) + \exp(V(A,N,L))} \\
&= \frac{1}{1 + \exp(w_L - \kappa_3 w_N - \kappa_1 - b) + \exp(w_N - b)} \\
Prob_{L\to U} &= \frac{\exp(V(A,L,U))}{exp(V(A,L,U)) + \exp(V(A,L,N)) + \exp(V(A,L,L))} \\
&= \frac{1}{1 + \exp(w_N - b) + \exp(w_L - b)}.
\end{aligned}$$

Note that when κ_3 and κ_1 are greater than zero, we always have $Prob_{N\to U} > Prob_{L\to U}$. This is true regardless of κ_3's effect on w_L. The intuition is that to switch to unemployment, licensed workers have to have much larger negative shocks than non-licensed workers. In other words, the model implies that licensed workers always have better job security, since the relative value of unemployment is always lower.

We then turn our attention to worker welfare. For simplicity, we assume workers live only two periods ($A = 2$). Workers are initially sorted into different occupations after observing their preference shocks in the beginning of the first period ($a = 1$). We use $\bar{V}$ to denote workers present discounted lifetime value, so we have

$$\bar{V}(x) = \int W(x,\zeta) g(\zeta) d\zeta.$$

Recall that ζ follows a type one extreme value distribution. In our simplified case, x includes only age and current occupation. We assume everyone enters period one unemployed. An unemployed worker's expected lifetime welfare at the beginning of period one is

$$\begin{aligned}\bar{V}(x) =& \gamma + \log\Big[\sum_{j\in J}\exp(V(x,j))\Big] \qquad (x=(a,o), \quad \text{where } \ a=1 \ \& \ o=U)\\ =& \gamma + \log\Bigg[\exp\Big(b+\gamma+\log\big(exp(b)+exp(w_n)+exp(w_l-\kappa_1)\big)\Big)\\ &+\exp\Big(w_N+\gamma+\log\big(\exp(w_N)+\exp(w_L-\kappa_1-\kappa_3 W_N)+\exp(b)\big)\Big)\\ &+\exp\Big(w_L+\gamma+\log\big(\exp(w_L)+\exp(w_N)+\exp(b)\big)\Big)\Bigg].\end{aligned}$$

Note that $V(x,j)$ is the choice specific value for workers, and γ is the Euler constant. Holding wages constant, when licensing requirements become more strict (κ_1 and/or κ_3 increases), we see that lifetime expected welfare decreases ($\bar{V}(x)$ is decreasing in both κ_1 and κ_3). However, if the training costs improve the quality of services provided by licensed workers, thereby raising the wages of licensed workers relative to those of unlicensed workers (w_L increasing in κ_3), the welfare effect of increasing training costs is ambiguous. If $\frac{dw_L}{d\kappa_3} \geq w_N$, then increases in training costs increase expected welfare. This also holds for values of $\frac{dw_L}{d\kappa_3}$ slightly less than w_N.[8] For values of $\frac{dw_L}{d\kappa_3}$ sufficiently less than w_N, expected welfare is always decreasing in training costs.

Changes in licensing costs affect workers' expected lifetime welfare even when wages are held constant. This is because workers react to licensing changes by increasingly or decreasingly sorting into or out of licensed occupations. To avoid high licensing costs, workers may choose not to move to a licensed occupation, even though it would be their preferred occupation if the licensing costs were low. This hinders the sorting of workers into their desired occupations and therefore affects welfare. Since workers, especially young ones, may switch occupations many times during the course of their career in order to find the position they fit the best (Xu (2022)),[9] it is important to take this dynamic decision into account

[8] Let the second, third and fourth terms of $\bar{V}(x)$ be Λ, Γ and Δ respectively. We then have

$$\frac{d\bar{V}}{d\kappa_3} = \frac{1}{\Lambda+\Gamma+\Delta}\left(\frac{\Lambda e^{w_L-\kappa_1}}{e^b+e^{w_N}+e^{w_L-\kappa_1}}\frac{dw_L}{d\kappa_3} + \frac{\Gamma e^{w_L-\kappa_1-\kappa_3 w_N}(\frac{dw_L}{d\kappa_3}-w_N)}{e^b+e^{w_N}+e^{w_L-\kappa_1-\kappa_3 W_N}} + \frac{\Delta(e^b+e^{w_N}+2e^{w_L})}{e^b+e^{w_N}+e^{w_L}}\frac{dw_L}{d\kappa_3}\right).$$

Note that the first and the third term in the bracket are always positive. The second term can be positive or negative depending on the sign of $(\frac{dw_L}{d\kappa_3}-w_N)$. It is therefore ambiguous whether $\frac{d\bar{V}}{d\kappa_3}$ is positive or negative.

[9] Over 40% of high school graduates transition between white and blue collar occupations more than once

when investigating the effect of licensing on lifetime welfare.

This simple model allows us to establish some intuition about how licensing affects worker welfare and occupation entry and exit. The model is simplified in many ways. First, the model has only two occupations, as well as unemployment. This limits our ability to study how licensing affects flows between licensed occupations. Second, workers are ex-ante homogeneous, and the occupation decision is dependent only on their age and preference shocks. Relaxing these assumptions will allow the model to better fit wage patterns in the data and also gain more intuition on how licensing affects wages. For example, if wages are increasing with tenure, we would see workers with longer tenure being less likely to switch to licensed occupations, since the forfeited wages (due to time spent on training, represented by κ_3 in our model) are significantly higher than those of workers with shorter tenure. Our simple model focuses only on the mean wage differentials between licensed and non-licensed occupations and how licensing affects worker welfare through dynamic career decisions. In our empirical analysis, we also investigate how licensing affects wages. The general model outlined at the beginning of this section has both of these features and could be taken directly to the data. One could also expand our model into a general equilibrium framework similar to that of Traiberman (2019) to measure the potential benefits of licensing (wage growth, higher product or service quality) and costs of licensing (higher output prices and switching costs, labor misallocation, decreased output) and evaluate the lifetime welfare effects on workers.[10]

3 Data

In this section, we describe the data we use in our analysis, as well as the data cleaning and sample selection procedures. Until recently, data on occupational licensing have been limited, and nationally representative survey in the United States asked questions about occupational licensing (Kleiner and Soltas (2019)). However, this situation has recently been dramatically improved, thanks to new questions in both the Current Population Survey and the Survey of Income and Program Participation.[11]

between the ages of 18 and 28. Many workers make occupation transitions within one year of entering the labor force; with the average time until the first occupation switch is roughly 1.5 years (Gorry, Gorry and Trachter (2014))

[10] We will leave to future work the theory and estimation of the general equilibrium effects of licensing.

[11] More periodic and specialized datasets such as Baccalaureate and Beyond (B&B 2008) and the Beginning Postsecondary Students also include questions about licensing. See Bryson and Kleiner (2019) for a detailed list of surveys in the United States since 2010 that have added questions on occupational regulation.

Starting in 2015, the CPS has included three new questions directly related to occupational licensing. First, workers who are 16 years old and over are asked whether they have a professional certification or state- or industry-level license, not including business licenses. If the answer to this first question is yes, then workers are further asked if their professional, state, or industry license was issued by the federal, state, or local government. These two questions were asked in every interview in 2015, but from 2016 on, these questions have been asked only in the first and the fifth interviews. The third question asks whether a worker has a government-issued certification or license (provided the worker answered yes to the first two questions) or is currently unemployed but has previously worked. The question asks whether the respondent's government-issued professional, state, or industry license was required for his or her job. Unemployed respondents who worked in the past were asked about their last job. This third question was added in 2016 and is asked only in the first and the fifth interviews. To match the U.S. government's definition of occupational licensing, in our baseline analysis, we define a licensed worker as a respondent who answers yes to all three questions.[12]

The SIPP also has information on occupational licensing in two separate panels: 2008 and 2014. The 2008 SIPP panel has 16 waves from May 2008 through November 2013. The 13th wave, collected from September to December of 2012, contains a "professional certifications, licenses, and educational certificates" topical module, which contains licensing information that can be linked with core interview data in the same wave. This questionnaire is more detailed than what is found in the CPS. To construct an occupational license indicator, we use three key questions that are comparable to the ones in the CPS. The first question is, "Do you have a professional or state or industry certificate?" The second question is, "Is (the) certification or license required for current or most recent job?" The third question is, "Who awarded this certificate or license?" If a respondent answers yes to the first two questions and "Federal government, state government, or local government" to the third question, then we regard the respondent as licensed. The most recent SIPP panel (2014) also includes questions about licensing in the core data rather than the topical module. Respondents who are 18 years old and over and whose educational attainment is high school graduate or higher are asked whether they have earned a professional certification or license. Those who answered yes to the previous question are further asked, "Is the certification or license issued by the federal, state or local government?" These two questions in the 2014

[12]We use an alternative definition and define a respondent who answers yes to the first two questions as a licensed worker, and the results from this analysis are qualitatively and quantitatively very similar to those of our baseline analysis. The results are available in the Appendix.

SIPP are essentially the same as the first two questions in the CPS, though the 2014 SIPP doesn't ask if the license is required for the respondent's job.

In this paper, we use both the CPS and SIPP for our analysis. We use the CPS to deliver our baseline results, using the licensing definition above. We then use the SIPP as a robustness check to strengthen and verify our results and analysis. Our analysis with the SIPP starts with waves 12, 13, and 14 of the 2008 panel, since they contain the most reliable definition of licensure linked to employment dynamics. We then use an additional 20 years of data from the other SIPP panels with an alternative definition of licensure.[13] The results for this analysis are shown in Appendix C. Note that because of the differences in the survey design, the definition of licensing and the timing of the interviews, the results from the CPS and SIPP are not directly quantitatively comparable. However, both surveys provide qualitative evidence of occupational licensing effects on labor market outcomes, and their results are consistent with each other.

3.1 CPS Sample Selection

In this section, we describe how we select and clean our CPS sample. As mentioned in the previous section, we use the three newly added survey questions to construct the licensing attainment indicator, which is considered to be the most reliable licensing indicator in the literature. We use data from the IPUMS CPS (Ruggles et al. (2020)) for 2016 through 2022. We keep respondents from age 20 to 65 who are not enrolled in school; not out of the labor force or retired; not in the military; not disabled; and not family workers, self-employed, or unpaid workers.

The CPS has several advantages for our analysis. It is a nationally representative survey and has a relatively large sample size. The panel structure in the CPS allows us to follow the same workers for up to a year and four months (with a gap of eight months in the middle), allowing us to analyse the relationship between workers licensing status and their labor market dynamics. It is also recorded monthly, so we can observe how licensing affects workers' monthly occupational transitions. On the other hand, the CPS also has some major drawbacks: First, the questions about licenses and certification are asked only in months 1 and 5 for each individual; the values in other months are imputed. This imputation could

[13] Other than in the wave 13 topical module, there is no information in the SIPP about an individual's licensing status. In this analysis, we use a much more relaxed definition as the licensing indicator. We provide more details about this analysis in the SIPP sample selection subsection.

be problematic – for example, people who were not licensed in month 1 but later acquired a license and switched to a different occupation in month 4 should have their licensing status changed between month 1 and 4, but the imputation often misses this change. Using the IPUMS imputed licensing status indicator, we measure the rate of change for worker monthly licensing status. Table 1 presents these results for first round interviewers (interview months 1 to 4, Panel A) and second round interviewers (interview months 5 to 8, Panel C). It is clear from the table that the licensing status change rate is significantly higher between the interview months and imputed months (M1-M2 and M5-M6) than between two imputed months (M2-M3, M3-M4, M6-M7, and M7-M8). This high rate in licensing status changes in month 1 and 5 suggests that there might be errors in the original imputation process. Moreover, wage data are available only for the Outgoing Rotation Groups (i.e., only in months 4 and 8). This limitation leads us to restrict our wage-related analysis to an annual rather than monthly frequency. For consistency's sake, our baseline results are all in annual frequencies, but in the robustness section, we show monthly frequency results for worker occupational mobility.

The most reliable licensing indicator is provided in months 1 and 5, while wage data are available for months 4 and 8. Both variables are at the center of our analysis, and this discrepancy in the timing of data collection means that some adjustments to the existing imputation may be required. We first keep worker observations from months 4 and 8, so we have the most reliable data on wages but less reliable data on licensing switching status. We then use this sample as our baseline analysis. To make sure our results are not dampened by the potential coding error in licensing status indicator, we then use workers' employment status information to modify the spurious licensing status and re-estimate all of the baseline results.[14] Lastly, we we keep observations from months 1 and 5 and impute wage values using data from months 4 and 8. We keep the last two analyses as a robustness check to make sure that our results are not sensitive to potential coding issues in the original data.

[14]We leave the details of our imputation strategy to appendix B. Panel B and panel D in table 1 show our imputation results: after the imputation process, the licensing status change rate is uniformly distributed across all the interview months – this reassures us that our imputation improves the reliability of the licensing indicators. However, the imputation reduced the licensing status changing rate in almost all months, and this reduction may be problematic. For this reason, we use the original licensing indicator in the data as our baseline, and use only the imputed licensing indicator as a robustness analysis. Fortunately, the results are similar between the two specifications, as one can see from the comparison between the baseline results and the robustness results in the appendix.

3.2 SIPP Sample Selection

The SIPP collects information on up to two jobs for each individual. We define an individual's primary job as the job at which he or she works the most hours. If information on hours is not provided, then the primary job will be defined as the one from which he or she receives the highest monthly payment or highest hourly rate. Our analysis using the SIPP is based on a worker's primary job. Similar to the CPS cleaning criteria, our criteria drop individuals who are younger than 20 or older than 65. In addition, we drop individuals who are members of the armed forces, disabled, family workers, unpaid workers or enrolled students. Furthermore, because of the well-known seam bias in the SIPP, we keep individuals only in their interview month. Therefore, our sample is at a four-month frequency.

As mentioned earlier, we use two different licensing indicators in our analysis. The first licensing indicator is constructed using the 13th topical module from the 2008 SIPP panel. We use the three questions provided in the topical module to construct the indicator; if an individual has a professional license that is issued by the government and required for the job, then we say he or she is licensed. This is comparable to the licensing indicator we construct using the CPS. We also include the core data from panels 12 to 14 for employment dynamics analysis. The second licensing indicator we construct is not as strict as the first one. This licensing indicator is defined according to whether an occupation is universally licensed. The list of universally licensed occupations is provided in appendix A. This non-strict definition allows us to conduct some analyses using the long SIPP panels and large sample size, despite the lack of direct licensing questions in those years. In the paper, we use all available data from the SIPP from 1991 to 2013 and this second definition of licensure as a robustness check, and we show the results in Appendix C.

3.3 Supplementary Datasets

The third dataset we employ is the Occupational Licensing Law Research Project (OLLRP). The OLLRP is a newly constructed detailed dataset on occupational licensing and requirements for all universally licensed occupations across states and time and is available at the Minnesota Population Center. The requirements recorded include monetary costs such as initial licensing fees and renewal costs, as well as time costs such as required hours of training and education or experience requirements. Although the currently available information from the database, does not yet allow for a complete aggregate analysis, we can see clear

evidence regarding trends in occupational licensing requirements. From the data, we see that both licensing fees and renewal costs have increased for almost all occupations. The cost of initial licensing fee increases on average by $70 across all occupations and states, and the cost of licensing renewal also increased on average by $62 across all occupations and states.[15] The licensing requirement changes are not universal across occupations or states. The national average increases in licensing fees range from $0 for occupations such as truck drivers to over $300 for occupations such as attorneys. Dentists see an increase in licensing costs of $950 in Alabama but no change in Arizona. Table 2 presents a few more summary statistics on licensing requirement changes. The first two columns show the education requirements of an occupation. The third and fourth columns show the initial cost of licensing, which is the initial amount that must be paid to receive a license. The fifth and sixth columns show the renewal cost, which is the amount charged to renew a license. We see that the education requirements for many occupations have increased. For example, there were no education requirements on average for school counselors back in 1995, but in 2015, most school counselors needed to have a bachelor's degree. Similarly, architects were required to have an associate's degree on average in 1995, but in 2015, they needed to have a bachelor's degree or post-graduate degree. Some occupations, like veterinarian, do not see an increase in education requirements for a license, but the costs for getting a initial licence and renewal become much higher. Many other types of requirements are also available in this dataset, such as an experience or exam requirements, and these also show an increasing trend.[16] Despite the significant heterogeneity across occupations and states, the overall increase in licensing requirements is very clear. This is one of the key motivations for our analysis.

3.4 Switching Rates for Licensed and Non-licensed Workers

First, we calculate the basic economy-wide switching rates for each occupation in the SIPP. We divide all workers into licensed workers and non-licensed workers. We then calculate the average worker mobility rates in each group and present the data in Figure 4. An examination of the figure shows that licensed workers experience far less churn than non-licensed workers. The rate at which people switch into licensed occupations is lower (panel (a) shows workers who switched in from a different occupation, and panel (c) shows workers who switched in from unemployment), as is the rate at which people switch out (panel (b) shows workers

[15] These averages simply take each occupation state pair as an observation and are not weighted by occupational employment. All dollar amounts in the paper are in USD deflated to constant 2000 dollars.

[16] Results for other licensing requirements for various occupations are available upon request.

switching to a different occupation, and panel (d) shows workers switching into unemployment.). This result holds when we use different licensing indicators. We use universally licensed occupations as the licensing indicator and the SIPP from 1990 to 2013, which shows that workers in universally licensed occupations (regardless of licensing attainment status) experience far less churn than workers in other occupations (see figure C.1 in appendix C). However, this difference in switching rates between licensed and non-licensed workers may be due to a number of factors not directly related to licensing. If licensing is somehow correlated with other characteristics of the occupation that are themselves correlated with switching rates, or if the people who are licensed tend to be older or more educated and therefore less inclined to switch, we may incorrectly associate lower occupational mobility with licensing, when in fact no causal relationship exists. In the following sections, we detail our empirical strategy to identify these possibly confounding factors separately from the direct effect of licensing on mobility and wages.

4 Empirical Analysis

A primary goal of our paper is to study whether occupational mobility is different for workers who are licensed and those who are not, and to measure the specific effects of licensing on mobility as well as workers' wage growth rate. Our baseline results are based on analysis using ordinary least squares (OLS). We then discuss our instrumental variable (IV) approach, along with various other robustness estimates.

4.1 Baseline Results

4.1.1 Occupational Switching

We first investigate how licensing influences worker occupational switching dynamics. Much of the discussion around occupational licensing concerns the costs of entry, which can be framed as either a barrier that increases incumbent rents or a filter that improves the quality of service. Either way, jobs that require workers to be licensed appear to have less entry and less exit. We show in Figure 3 that the raw relationship between an occupation's licensing share and its entry rate is negative, meaning workers are less likely switch into a licensed occupation. To further investigate the impact of licensing on occupation entry and exit rates, we estimate two different linear probability models. Let an individual's sequence

of occupation choices be $\{\ldots, O_{it-1}, O_{it}, O_{it+1}, \ldots\}$, and define the switching indicator as $Y_{it} = 1$ if $O_{it} \neq O_{it+1}$ and 0 otherwise. Thus, $Y_{it} = 1$ indicates a worker switching *out of* their t-period job, while $Y_{it-1} = 1$ indicates a worker switching *into* their t-period job.

Since licensing represents a barrier to entry, one might expect that it will drive down the rate at which workers switch into licensed occupations, but by how much? To answer this question, we estimate the following linear probability model:

$$Y_{it-1} = \beta_0 + \beta_1 L_{it} + X_{it-1}\Gamma + O_{it} + S_{it} + \theta_t + \varepsilon_{it}., \tag{1}$$

where Y_{it-1} indicates switching into the current occupation in period t; L_{it} is the indicator of a worker's being licensed in period t, so β_1 is a measure of the effect of being licensed now on the probability of having just switched into the current occupation; X_{it-1} is a vector of individual characteristics in period $t-1$, including age, education, gender, race, marriage status, union status and log wage;[17] O_{it} denotes occupational fixed effects;[18] S_{it} denotes state fixed effects; and θ_t is a set of year and month fixed effects. The sample for this regression is all individuals who were employed in both period $t-1$ and t.

To estimate the effect of licensing on the probability of switching out of an occupation, we use a very similar model:

$$Y_{it} = \beta_0 + \beta_1 L_{it} + X_{it}\Gamma + O_{it} + S_{it} + \theta_t + \varepsilon_{it}, \tag{2}$$

where now we control for period t worker characteristics and use Y_{it} as the dependent variable. This means β_1 is a measure of the effect of licensing on the probability of switching out of the current occupation next period. Here, we restrict the sample to individuals who were employed in both periods t and $t+1$.

Columns one and two of table 3 show the results from both models. In column one, we find that conditional on being employed in both periods, the probability that the worker switched *into* the occupation in period t from a different occupation in $t-1$ (as opposed to having the same occupation in both periods) is much lower if the worker's current (new) occupation requires licensing. Specifically, the results show that the estimate of β_1 is negative and statistically significant at 1% level. Being in a licensed occupation means that it is 8%

[17] Union status is potentially a result of being licensed. We check this potential "bad control" problem pointed out in Angrist and Pischke (2009) by redoing our estimation without union status. The results are essentially unchanged.

[18] We use 440 occupation groups as defined in the IPUMS CPS, which are similar to the 2010 census codes.

less likely that a worker has just switched into his or her current occupation. Recall that our sample selection looks at workers from months 4 and 8 of the CPS, so the switching-in rate is the annual switching rate. However, this may include workers who have gone through multiple jobs in between these two periods, or those who have gone through one or more unemployment spells. We do not take a stand on whether this annual occupational status change represents the true annual occupational switching rate. Instead, our focus is simply looking at workers who are working in both months across two consecutive years and investigating the impact of licensing on worker occupational entry probabilities.

Similarly, in column two we show how licensing relates to the probability of switching *out of* the current occupation next period. After controlling for worker characteristics and fixed effects, we see that workers in a licensed occupation have a 5.2% lower probability of switching occupations next period than non-licensed workers. Note that many factors, such as worker ability, may be correlated with worker licensing status as well as occupational switching decisions, raising concerns about omitted variable bias. We address this concern in the next sections, following methods from Oster (2019). We also find that older workers and white workers tend to switch occupations with a lower probability. Being married, being in a union, and having a higher income also decreases the probability of switching. Occupational and state effects play a large role in predicting occupation switching rates. Adding the fixed effects in our specification increases the R^2 ten fold and decreases the coefficients of interest four fold.

4.1.2 Switching In and Out Of Unemployment

We are also interested in how licensing may affect job security. Are licensed workers more or less likely to become unemployed next period? Furthermore, we want to study the effect of licensing on the probability that an individual switches from unemployment into employment. To answer these two questions, we run regressions similar to 2 and 1 with two main differences. First, we now include workers who are unemployed preceding or following the employment observation.[19] Second, the dependent variables are now indicators of switching from working into unemployment (Y_{it}) and switching from unemployment into any occupation (Y_{jt-1}).[20]

[19] Our definition of "unemployed" excludes those who are not in the labor force, since non-participation could be due to many reasons that are not related to the labor market.

[20] Note that when studying the switching in probability from unemployment, the controls X_{it-1} do not include income in period $t-1$, since we do not observe unemployment earnings.

Column 3 of table 3 shows how licensing affects the probability of switching out of employment into unemployment. The coefficient on the licensing indicator is negative and significant, which suggests that licensing may have a positive impact on job security on average, though the effect is small in magnitude, decreasing the probability of becoming unemployed next period by 0.6%. This result suggests that licensing may not have a strong impact on worker job security. We then look at how likely it is that unemployed workers find work in an occupation that requires licensing. Column 4 shows a decrease of 0.5 percentage points in the probability of having switched from unemployment into an occupation that requires a license. These results, as in the first two specifications, support the hypothesis that licensing creates barriers to entry and incentives to stay in that occupation. As with the probability of job-to-job switching, we find that older, white, married, and union workers are less likely to switch into or out of unemployment.

Lastly, we focus on workers who have just moved into a new occupation, either from unemployment or from a different occupation. It seems clear that licensing represents a relative barrier to entry for both unemployed and employed workers. Both groups switch into occupations requiring licensing with a lower probability than other occupations. However, this raises the question of whether licensing represents a greater or lesser barrier for the unemployed versus the employed. Compared with an employed individual, is it more or less difficult for an unemployed person to switch into a licensed occupation than a non-licensed one? Column 5 in Table 3 shows the results from the regression in equation 1 where we restrict our sample to workers who are working today and switched in either from unemployment ($Y_{it-1} = 0$) or from a different occupation ($Y_{it-1} = 1$) in the previous period. In other words, we do not include workers who stayed in the same occupation in both years. We find that the coefficient on the licensing indicator is negative and significant at the 1% level, implying that workers who have switched into a licensed occupation are 2% less likely to have been in a different occupation than previously unemployed. One possible interpretation is that a large part of the barrier to entry for licensed jobs is the time costs of satisfying the training or experience requirements to obtain the license. Since the opportunity cost of time for unemployed workers is likely lower than it is for employed workers, the time component of the barrier is less important, and so the cost of entry relative to that of non-licensed jobs is lower than it is for the employed. The coefficient on licensing status further confirms our hypothesis that licensing is a barrier to entry both financially and in terms of time costs.

4.1.3 Licensing and Wage Growth

We now turn our focus to the effect of licensing on worker wage changes. We study this effect when a worker stays in, switches into, or switches out of a licensed occupation. Many papers in the literature have shown that licensed workers tend to have higher wage levels than those of non-licensed workers (see Gittleman and Kleiner (2016) for a recent example), but there is far less work on how licensing affects wage growth, especially for occupational switchers. Our focus here is to examine the effects of licensing status on wage growth dynamics. Since wage data are available only in the outgoing rotation group (months 4 and 8) in the CPS, we will be measuring the change in wages associated with labor market transitions at an annual level.

We first calculate raw economy-wide average weekly wages for licensed workers versus non-licensed workers, without any controls. Figure 5 is in line with the previous literature that argues that compared with non-licensed workers, licensed workers on average enjoy a wage premium. This wage difference widens as age increases. Figure 6 shows that the average wage gap between licensed workers and non-licensed workers increases as workers grow older. Note that this is a cross-sectional analysis rather than a longitudinal life-cycle analysis, so the wider wage gap for older workers may be due to cohort effects rather than age. To further compare wage growth rates for licensed workers and non-licensed workers, we separate workers into two groups: occupation stayers and occupation switchers. Panel (a) in Figure 7 shows the result for workers who stay in the same occupation during a year, while Panel (b) of Figure 7 shows the result for occupation switchers. We see in Panel (a) of Figure 7 that when workers stay in the same occupation, licensed workers have lower wage growth rates relative to those of workers who are not licensed. However, many factors may contribute to wage growth rates, so a simple summary of the data may confound licensing effects with other factors. One plausible explanation for licensed workers having on average lower wage growth rates is the age composition effect – workers without licenses are on average younger. Since younger workers have wage growth rates that are generally higher than those of older workers, this may result in workers without licenses having higher than average wage growth rates. On the other hand, the occupational switcher exercise shows the opposite: licensed workers have higher wage growth rates even when they switch out of their occupation in the next period. As mentioned earlier, all of these exercises are simply summaries of the data – many factors that are directly or indirectly related to licensing could contribute to the differences in wage growth and levels. If licensed workers are systematically older, more experienced, or more educated, they may enjoy higher wages and wage growth

rates. To separate these confounding factors from the direct effect of licensing, we use the following specification:

$$\Delta w_{it} = \beta_0 + \beta_1 L_{it} + X_{it}\Gamma + O_{it} + O_{it+1} + S_{it} + \theta_t + \varepsilon_{it}, \tag{3}$$

where $\Delta w_{it} = \log(W_{it+1}) - \log(W_{it})$ is the difference in log wages. Here, β_1 can be interpreted as the difference in wage growth if the worker is licensed versus not licensed. We estimate the regression separately for workers who stay in the same occupation and for workers who switch occupations between two periods. For workers who switch occupations, we control for both the old and new occupational fixed effects (O_{it} and O_{it+1}). Stayers have $O_{it} = O_{it+1}$ by definition, so we include only one set of occupational fixed effects. To look at how licensing affects wages for those switching into licensed versus non-licensed jobs, we shift the timing of the regression and additionally estimate the following model:

$$\Delta w_{it-1} = \beta_0 + \beta_1 L_{it} + X_{it-1}\Gamma + O_{it} + O_{it-1} + S_{it} + \theta_t + \varepsilon_{it}. \tag{4}$$

Here, β_1 is the gain in log wage for those switching into licensed occupations relative to that for those switching into non-licensed occupations.

Table 4 shows the results from estimating equations 3 and 4. The first column shows the results for workers staying in their occupation between two consecutive years. After controlling for workers' observable characteristics as well as occupation, state, and time fixed effects, we find that on average, workers who are licensed have 5.5 percentage points higher log wage growth than that of workers who are not licensed. This represents a $1,760 wage increase.[21] This result is in line with the existing literature that suggests that occupational licensing attainment has positive effects on wages. We also see that older workers, white workers, married workers, workers with college degree, and workers in a union have higher wage growth rates. Finally, we find that on average, female workers have lower wage growth rates than those of male workers.

The second column in Table 4 shows the results for workers who switch out of their current occupation into a new occupation. We find that licensing is associated with an increase in wage gains for those switching out of their current occupation. Specifically, relative to those switching out of a non-licensed occupation, those who switch out of a licensed occupation

[21] As mentioned earlier, all dollar amounts in the paper are in 2000 real dollars. The mean annual real income of this stayer sample is $32k. Workers who are making the average income and staying in a licensed occupation see an additional $1,760 in annual wage gain, compared with workers who stay in a non-licensed occupation (0.055*32k= 1,760).

see a 3.9 percentage point higher change in wages. This may reflect some human capital accumulation due to license-related training or movement into a higher demand industry such as high-tech programming, from a licensed occupation. It may also come from workers' licensing status being correlated with some unobserved personal characteristics: workers who are licensed also to have higher unobserved ability and therefore higher wage growth. In section 5, we will address this issue by using several methods to evaluate the impact of and correct for omitted variable and selection bias. Similar to previous results, these results show that female workers, non-white workers, and non-college-educated workers tend to have lower wage growth when switching occupations, whereas older workers, union workers, and married workers gain more.

We can also look at the effect of switching into a licensed occupation on wage gains. Similar to column one and two, the third column of Table 4 shows that switching into a licensed occupation is associated with higher (6.8 percentage points) wage gains compared with those of switching into an occupation with no licensing requirements. This finding supports papers that estimate the switching costs associated with occupation mobility (Xu (2022) and Traiberman (2019)). If occupational licensing represents a hurdle for workers who switch, then workers have to expect a larger wage increase in order to compensate for the licensing-incurred switching costs. Furthermore, we find that as was the case before, female, non-college-educated, and non-white workers gain less when switching into an occupation, and older, married, and union workers gain more. Note again that we control for occupational fixed effects in both the source occupation and the destination occupation, so we are not confounding occupation-specific effects with the licensing effect.

Finally, we look at the effect of entering a licensed occupation from unemployment. The sample in this case is all individuals who switch from unemployment to employment between $t-1$ and t. The dependent variable is the log (weekly) wage received at the new job rather than change in log wage. Column four shows that there is a strong and significant effect of licensing. Workers who switch from unemployment to licensed jobs gain starting wages that are 14 percentage points higher than those of workers switching into non-licensed occupations. This is consistent with the findings in the literature (for example, those in Kleiner and Krueger (2013)). We see that older workers, college educated workers, and married workers tend to have higher entry wages from unemployment, whereas female and non-white workers tend on average to have lower entry wages.

To sum up, we show that licensing has strong positive effects on wage growth. The effect exists whether a worker stays in the same job, moves out of or into a licensed job, or

has newly joined a licensed job from unemployment. However, these coefficients should be interpreted with caution. What we established in these sections are correlations rather than causal relationships. Furthermore, the interpretations between specifications are different. For example, for workers who stay in a job (column one), the positive coefficient on licensing represents purely the positive effect of licensing on wage growth. On the other hand, the positive coefficient on licensing for job switchers in column three combines both the wage growth effect for licensed workers and the barrier effect of entering a licensed occupation. Lastly, our analysis in this section may be subject to selection bias. Worker mobility decisions are endogenous. Workers who decided to stay in an occupation may have been affected by licensing differently than workers who switched out of their occupation, had they decided to stay. We address this in the robustness section, using a two-stage Heckman correction method.

4.2 Instrumental Variable Approach

The effects we have presented so far are correlations rather than causal relationship. In this section we address this concern by using an instrumental variable strategy. All of the regression models in section 4.1 take the following form:

$$y_{i\tau} = \beta_0 + \beta_1 L_{it} + X_{i\tau}\Gamma + O_{it} + S_{it} + \theta_t + \varepsilon_{it},$$

where τ is either t or $t-1$ and $y_{i\tau}$ denotes the key variable of interest such as log wage growth or the individual switching indicator. The potential identification threat in our baseline approach above is that there may be factors in ε_{it} that are correlated with both the licensing status and the dependent variable, biasing our estimates of β_1. We attempt to identify the causal relationship between licensing and our outcomes of interest using two different instruments for licensing status. Our primary instrument is a residualized measure of location-occupation-specific licensing shares: $\%L_{OS}$. This is the share of workers who are licensed in the worker's particular occupation and state group after removing state and occupation fixed effects. The idea behind this first instrument is that, using wage growth rates as an example, the variation in residualized shares of licensed workers within each occupation and state group may affect the probability of workers becoming licensed,[22] but

[22] The variation in the share of licensing attainment within an occupation across state groups may represent cross-state heterogeneity in the difficulty of acquiring a license, which will likely affect each individual's licensing decision.

it should not have a direct impact on individual-level wage growth except through that worker's licensing status. We also construct a second instrument as a robustness check using a strategy closely related tothe one in Kleiner and Soltas (2019). Firstly, we residualize the licensing shares within each occupation and state pair as in the first method. We then create an indicator for whether that residual share is more than one standard deviation away from zero, either above or below. The intuition is similar to Kleiner and Soltas (2019) in that large differences in licensing shares in an occupation likely reflect policy variation.

We re-estimate all of the baseline models in section 4.1 using each of our two instruments and show the results in tables 5 and 6. Table 5 shows the IV analysis for occupational switching dynamics. As a comparison, the first row shows the OLS results (and standard errors) that were presented in the previous section as a comparison. The second row shows the results using our primary instrument for licensing status (residualized licensing share). The third row shows the results using our second instrument (the indicator for large deviations from the mean residualized licensing share). In column one, we see that using either instrumental variable decreases the estimated causal effect of licensing on switching into an occupation. This suggests that factors such as unobserved individual ability have biased our OLS estimates away from zero. However, as is consistent with what we find in the baseline analysis, we see that licensing still has a significant negative effect on the probability of switching in to an occupation. Workers are about 5 or 6 percentage points less likely to have just switched into an occupation if that occupation requires licensing. In column two, we see that after including either of the instrument variables, the effect of licensing on the probability of switching out of the current occupation becomes much smaller and not statistically different from zero. This suggests that the licensing effects we found in the baseline results are driven largely by unobservables correlated with licensing. We do see that on average, licensed workers are less likely switch out of their occupation, but the effect is small and insignificant. Similarly, we find that including the IVs reduces the licensing effect on both the probability of switching to unemployment and the probability of switching in from unemployment. This suggest that licensing does not have a strong and significant impact on the labor market flows in and out of unemployment. However, the last column shows that using our IVs increases the effect of licensing on whether a newly licensed worker is more likely to be coming from unemployment or a different occupation. This result suggests that acquiring a licensing can be time-consuming, so it is more of a hurdle (higher opportunity costs) for an employed worker to become licensed than for unemployed workers.

We then turn our focus to the causal effect of licensing on wage changes. Table 6 shows the

results across our four specifications. The first column shows the effect on wage growth for occupation stayers. Controlling for the endogeniety of licensing with either IV significantly increases our estimates of the causal effect of licensing on wage changes for stayers. We see that licensing increases workers' annual log wage growth by 8 to 9 percentage points if they stay in their occupation. Similarly, we see new hires from unemployment also enjoy a licensing premium. The effects with IVs are slightly smaller than the OLS results. We see that on average, licensed workers have an initial log wage that is 13 percentage points higher than that of non-licensed workers. We also see a strengthened licensing effect after including the IVs for switchers who have just switched into a licensed occupation, which is in line with the switching cost intuition: workers switching to a licensed occupation require more compensation in wages, owing to the high licensing costs in fees as well as in time consumed. Lastly, we see that the point estimates of the effects of licensing remain similar but become insignificant when we use our IVs to consider workers who switch out of their current licensed occupation.

Overall, the results from our IV strategy are consistent with the results from our baseline analysis, and the direction of the apparent bias is consistent with our intuition. So far, our results support the conclusion that licensing has statistically and economically significant negative effects on worker mobility and positive effects on wage growth. In section 5, we will use a parameter-bound strategy to further examine the robustness of our results.

4.3 Occupational Heterogeneity in Licensing Effects

In this section, we examine how the effect of licensing differs across occupations. The analysis above shows that on average, workers in licensed occupations have less occupational mobility. However, these differences in occupational mobility, and thus the effects of licensing on mobility, may vary across occupations because of variation in task composition. Figures 8 to 11 provide some evidence on heterogeneity in occupational mobility. For example, the gray bars in Figure 9 show the monthly switching-out rate for 25 different universally licensed occupations, while the blue (left-most) bar shows the average switching rate for all other occupations. The dash-dotted line shows the average occupational switching rate across all occupations, including the universally licensed and non-licensed ones. It is clear from the graph that almost all universally licensed occupations have switching rates lower than those in the average non-licensed occupations, but we see great heterogeneity across these universally licensed occupations. While some universally licensed occupations have a

monthly switching rate of less than 2%, some occupations, such as pest control, see monthly switching-out rates of almost 8%. Similarly, we see great heterogeneity in the rate of switching out to unemployment (11), switching in from other occupations (8), and switching in from unemployment (10). Given this large heterogeneity in occupational mobility across licensed occupations, the next question that we want to address is whether the mobility and wage growth effects of licensing are also heterogeneous across these occupations. We answer this question using a strategy similar to the one in our baseline analysis. Here, we focus on one occupation at a time instead of pooling all occupations together, similar to the approaches of Han and Kleiner (2021) and Johnson and Kleiner (2020). However, our strategy deviates from theirs in that we do not use the universally licensed occupations as an indicator for licensing. Instead, we use individual-level licensing attainment, as in our baseline analysis. We then restrict our sample to one (coarsely defined) occupation group at a time.[23] Table 9 shows the results for this analysis.

As shown in Table 9, the results for managerial and professional occupations are similar to the economy average results we presented in the previous section. This is because this occupation group represents the largest proportion of workers (34% of total workers). On the other hand, the results for farming, forestry, and fishing occupations (column four) are mostly statistically insignificant because of the very small sample size (1% of total workers). While we see across occupations that licensing has a negative effect on worker mobility and a positive effect on worker wage growth, the magnitudes of the effects significantly differ across groups. For example, in technical, sales and administration support, compared with non-licensed workers, licensed workers who stay in the same occupation between two years see 3 percentage points in additional wage gains. However, for operators, fabricators and laborers, being licensed leads to an additional 8 percentage points in annual wage gains.

5 Robustness Checks

In the previous section, we showed licensing effects on workers' occupational mobility as well as wage changes. One potential threat to our estimates is that our results may be subject to omitted variable bias. In section 5.1, we follow the strategy introduced by Altonji, Elder and

[23]There is large variation in occupational licensing attainment rates across these coarsely defined occupation groups. For example, in managerial and professional occupations, more than 30% of workers are licensed. This is much higher than our pooled sample's average licensing rate of 19.87%. On the other hand, the licensing attainment rate in the farming, forestry, and fishing occupation group is only 6.21%.

Taber (2005) and Oster (2019) and attempt to place on our estimates bounds that account for selection on unobservables. Then, in section 5.2, we address the potential sample selection problem to account for the endogenous occupational mobility when we focus on incumbent workers in our wage growth analysis. Finally, in section 5.3 we describe the exercises of our analysis using different licensing indicator, using a different imputation strategy, and applying the principal score matching method before regressions. The exercises are to make sure that our results are not specific to our particular sample or data in the baseline analysis. The results for these robustness exercises are in the appendix.

5.1 Parameter Stability and Selection on Unobservables

A common method of evaluating the impact of omitted variable bias is to check whether the parameter estimates of interest are robust (stable) to the inclusion of control variables. The idea behind this approach is that the bias arising from omitting observable controls may be informative about the bias arising from omitting the full set of observed and unobserved variables. While this method is intuitive, recent papers (Altonji, Elder and Taber (2005), Oster (2019)) show that simply observing coefficient stability as controls are added is not enough to conclude that omitted variable bias is negligible. In this section, we apply the more robust methods developed in these two papers to our primary analyses (closely following Oster (2019)), allowing us to place bounds on the bias that may be driving our estimated treatment effects.

The method makes several assumptions. Consider a basic version of our regression analysis above:

$$Y = \beta_0 + \beta_1 L + X\Gamma + W_2 + \varepsilon,$$

where X is the vector of all observed controls, L is the treatment indicator, and W_2 is unobserved. Define $W_1 \equiv X\Gamma$. The first assumption is about the relationship between the selection relationships. Define the coefficient of proportionality δ so that $\delta \frac{\sigma_{1L}}{\sigma_1^2} = \frac{\sigma_{2L}}{\sigma_2^2}$, where $\sigma_{iL} \equiv Cov(W_i, L)$ and $\sigma_i^2 \equiv Var(W_i)$. This relationship will always hold for some delta. The method first assumes that $\delta = 1$; that is, the importance of selection on observables equals that of selection on unobservables. We can then examine the relative degree of selection on unobservables (the value of δ) that would result in the true treatment effect being zero. Second, define R_{max} as the R^2 resulting from the theoretical regression in which we did happen to observe W_2 along with X and L. Using different potential values of R_{max} (the true value of which is unknown), we can measure the influence of omitted variable bias by

placing bounds on our estimated treatment effects.

Tables 10 to 12 show the results of this analysis. The first three columns of Table 10 show the estimated treatment effect of licensing on the switching-in rate with the gradual inclusion of various sets of observed controls. The estimated treatment effect with no controls (-0.200) is significantly higher in absolute value than the estimate with all the controls (-0.080), implying that the uncontrolled regression was biased away from zero. The inclusion of all the controls substantially increases the R^2 from 0.03 to 0.12. Columns 4 through 6 show the same analysis for the switching-out rate, with similar results: the parameter estimate becomes significantly smaller in absolute value, whereas the R^2 increases sixfold.

The results from the robustness exercise are contained in the bottom panels of all three tables. The first exercise assumes that $\delta = 1$, or that the correlation between the treatment and unobservables is of the same direction and magnitude as that between the treatment and observables. We can then calculate what the estimated treatment effect would be under different assumptions about the relative variance of W_2, represented by R_{max}. We follow Oster (2019), who suggests an R_{max} of 1.3 times the R^2 from the fully controlled regression. The resulting bounds on β_1 are shown in brackets, with one bound being the value of β_1 under the value of R_{max} shown to the left, and the other bound being the original estimate. Examining Table 10, we see that controlling for selection on unobservables decreases the magnitude of the treatment effect, since our previous estimates were driven down (in absolute value), by the inclusion of the observed controls. For example, under the assumption that $\delta = 1$ so that the unobservables are just as important as the observables, and under the assumption of $R_{max} = \tilde{R} \times 1.3$, the lower bound for the effect of licensing on the probability of workers' switching occupations in the next period is -0.005. It is reassuring that though the licensing effect on workers' occupation switching decreases when we assume more stringent conditions – the unobservables are at least as important as the observables, and together they can explain 1.3 times of the data variation from the original specification – the effect is still negative on mobility. In other words, under reasonable assumptions in which unobservables are typically not more important than the observables ($\delta \leq 1$),[24] the negative impact of licensing on workers' switching remains. On the other hand, we see that the licensing effect on the switching-out rates becomes smaller or even positive once we impose the same stringent condition. Specifically, if $R^2 = 1.3\tilde{R}$, then the effects on the switching rate from the unobservables have to be smaller than 0.67 of the included observable effects

[24]The reason is that researchers focus their data collection efforts (or their choice of regression controls) typically on the controls they believe ex ante are the most important (Angrist and Pischke (2010)).

for licensing to have negative effects on the switching-out rate. This is consistent with our IV results. When including the instrumental variables, the effect of licensing on having switched in persists, but the effect on switching out fades. This suggests that licensing can be a barrier for outsiders to switch into the occupation, but it doesn't have significant effect on the switching-out for licensed workers.

Tables 11 and 12 show the same robustness exercise on wage changes for workers who stay and switch occupations, respectively. As opposed to the results presented in the switching analysis (table 10), in table 11 the parameter estimates from the wage analysis are biased toward zero when we ignore all the observable controls. Therefore, in this exercise, controlling for selection on unobservables in this manner actually increases the magnitude of the estimated treatment effect, since our previous estimates were driven up (in absolute value) by the inclusion of the observed controls. For example, under the assumption of $R_{max} = 1.3\tilde{R}$, the upper bound for the effect of licensing on wage changes from staying in an occupation is 0.099, which is significantly larger than the original estimate of 0.055. This implies that the wage growth results for stayers represents a lower bound on the treatment effect of licensing. If we assume that by combining the observables and unobservables we can explain 1.3 times the original wage growth variation for stayers, the unobservables would need to have an effect on wages opposite to that of the observables, and they would need to be almost 3 times more important than the observables ($\delta = -2.7$) to make the wage effect of licensing become zero.

We can interpret the results in Table 12 in a similar fashion. On the left panel we focus on the wage growth effect of licensing for workers who have switched into their occupations. We see that omitting observables biased the estimates towards zero, since including more observables makes the estimates of licensing effects bigger. Under the assumptions that the unobservables are as important as the observables in explaining switcher wage growth and that together they explain 1.3 times of the original wage growth variation, the upper bound of the licensing effect on workers' wage growth is 0.096. Only when the unobservables are almost 4 times ($\delta = -3.7$) more important than the included observables, and have the opposite effect on wage growth than the observables does, the licensing effect fade to zero. Similarly, in the right panel of Table 12, we present the results for the licensing effect on wage growth when workers switch out of their occupation. Including more observable controls strengthens our estimates of the licensing effect. This implies that omitting unobservables has biased the estimates toward zero. When they are taken into consideration, and when (for example) unobservables are of the same importance as the observables and $R_{max} = 1.3\tilde{R}$, the upper

bound of the licensing effect on worker wage growth is 0.075. Our baseline estimation (0.039) is a lower bound of the licensing effect. These results reassure us that, in general, our results on the licensing effect are robust even when omitted variables are taken into consideration and that many of our estimates actually represent lower bounds on the treatment effects.

To sum up, our results suggest that licensing represents a barrier for workers who want to enter an occupation. It reduces the probability of new entrants; however, compared with their non-licensed counterparts, licensed workers enjoy a wage growth premium. This is the case whether the workers are occupation stayers or occupation switchers.

5.2 Selection Correction

In this section, we attempt to address the potential selection issue in the wage analysis from section 4.2. Consider equations 3 and 4 rewritten with the following simplified notation:

$$\Delta w_i = X_i\beta + \varepsilon_i,$$

where Δw_i represents the wage growth of worker i, and $X_i\beta$ summarizes all the controls and fixed effects in the baseline model, including the licensing indicator. Our analysis runs this regression separately for occupation stayers and switchers. However if the decision to stay in an occupation is endogenous and correlated with the expected change in wage, then our estimates of β may be biased for both the stayer sample and the switcher sample. To think about this more formally, let u_i be a selection variable such that we observe Δw_i only if $u_i = 1$ and

$$\begin{aligned} u_i^* &= Z_i\gamma + \xi_i \\ u_i &= 1 \quad \text{if} \quad u_i^* \geq 0 \\ &= 0 \quad \text{if} \quad u_i^* < 0. \end{aligned}$$

Workers choose to stay in their occupation (and thus we only observe them in the sample)only if $u_i^* \geq 0$ (or $\xi_i \geq -Z_i\gamma$). We don't see the counterfactual wage gain the occupation switchers would have received if they had stayed. Similarly, in the regressions with the sample of occupation switchers, we do not see the counterfactual wage growth the stayers would get if they had stayed switched occupations. The direction of the bias is not clear ex ante, though it seems intuitive that licensed workers with lower expected wage gains may choose to leave, leading to an overestimate of the effect of licensing on wage gains for stayers.

To correct this potential bias, we follow the two step procedure proposed by Heckman (1979). In the first stage, we use a probit model to estimate the probability of staying ($u_i^* = Z_i\gamma + \xi_i$); Z_i includes the same variables as X_i, as well as information on worker families that provides additional identification for the correction procedure.[25] The intuition behind the exclusion restriction is that these family conditions may affect workers' decision to stay or switch out of or into an occupation, but they should not have any impact on how their individual wage changes relate to their licensing status. We then calculate and include the inverse Mills ratio $\hat{\lambda} = \phi(Z\hat{\gamma})/\Phi(Z\hat{\gamma})$ in the second stage regression.

Using this procedure, we first check the effect of licensing on log wage growth for stayers. We find that for workers who stay in the same occupation, the selection-corrected estimate for the effect of licensing is 0.035 (significant at the 10% level). Compare this with the uncorrected estimate of 0.055 in column 1 of table 4. This suggests that selection on stayers has biased the licensing effect away from zero. Workers whose wage growth tends to be increased more by licensing status are more likely to stay in their occupation, while those who tend to have lower growth switch out of the occupation. Correcting for this selection issue reduces the licensing effect, though it is still positive and significant. For workers who switch out of their current occupation in the following period, the licensing effect after selection correction becomes not significantly different from zero. This is in line with our IV results and suggests that the licensing effect we find in our baseline OLS analysis (0.039) is due to selection or omitted variable bias. We then check the effect of licensing on switchers who have just become licensed. The estimate after selection correction is 0.051 (significant at the 5% level). This is slightly lower than our baseline estimate of 0.068, again suggesting that the selection issue biases our estimate away from zero. Those who benefit more from licensing tend to choose to switch into licensed occupations. Overall, the estimated effects of licensing become smaller after selection correction, but the results using this two step Heckman correction strategy are all consistent with our main results. This validates our broad finding in the previous sections that licensing has significant positive effects for those who stay in their occupation and those who have just become licensed.

[25] The additional variables include number of children and an indicator if the youngest child in the family is younger than five.

5.3 Other Data Samples and Propensity Score Matching

In this section, we investigate the licensing effects as in the baseline analysis with different data samples and datasets as robustness checks. Recall that our baseline results look at the effect of licensing on annual labor market transitions, using low-frequency data from the CPS (Table 3). Our first exercise in this section is to use the same sample from the CPS but look instead at the effect of licensing on monthly transitions.[26] We then show how the results differ when we impute data in two ways as described in Section 3.1. The CPS has the most reliable licensing indicators in months 1 and 5 and reliable wage data in months 4 and 8. In the baseline results, we kept data from months 4 and 8, and we do not modify the licensing status in the data. As a robustness check, we first modify the worker licensing status using workers' labor market transition data and repeat the baseline analysis. We then keep workers in months 1 and 5 (and therefore not imputing licensing status) while imputing the wage data.

The results are shown in Table 7. The top panel of the table shows the results when estimating at the monthly level, using our baseline imputation/cleaning method. The bottom panel shows the results when using our alternate cleaning method. Notice that when using monthly data, the marginal effects of licensing on the probability of switching between occupations are significant and negative, as was the case before, but with smaller coefficients. The effect on workers moving in and out of unemployment is even smaller. Given that a worker is a new hire in this period, if the worker is now licensed, he or she is more likely to have come from unemployment than from another job. All results are in line with our baseline results. In addition, the results are robust to the two different imputation methods, with these imputation methods giving us very similar results to those of our baseline method (bottom panel of table 7). This strengthens our conclusions about how occupational licensing affects labor market transitions.

We then use SIPP waves 12, 13, and 14, as well as module 13 in panel 2008, and apply the same analysis. The advantage of using this sample is that the SIPP has wage data for every wave in the sample, and module 13 has more reliable licensing indicators that are comparable to the ones we use in the CPS. However, this analysis suffers from a relatively smaller sample size.[27] This makes some of our results statistically insignificant. However, regardless of the

[26] We cannot run the same wage change analysis using monthly data, since wage information is provided only in the CPS outgoing rotation group and is not available month to month.

[27] We drop people who are older than 65 or younger than 20, who are family workers, unpaid workers, or enrolled students. We also exclude workers who are disabled or in the armed forces. We then exclude

small sample size and our use of data with different time horizons, our results from this analysis are in line with our baseline results using the CPS (table 8). Licensing reduces worker mobility going in and out of licensed occupations from both other occupations and non-employment. Workers are less likely to get hired from other occupations than from non-employment if the destination occupation requires a license. The wage growth effect is higher if one stays in a licensed occupation. Because of the small sample size, results for wage changes upon switching are not statistically significant.

We separately attempt to overcome the small sample issue by performing some analyses using more than 20 years of data from the CPS. In this case, we use employment in a universally licensed occupation as an indicator for licensing.[28] Thus, for these results, we are trading a precise indicator of licensing to gain a (much) larger sample size. The particular analysis we perform is testing whether the rate of worker transitions through an occupation that is universally licensed (regardless of actual attainment rate) is different from the rate for other occupations. Because of the nature of this indicator (essentially, all it requires is the individual's occupation code), we are able to use CPS data from 1994 to 2019. We present all the results from this analysis in appendix C, but the key conclusion supports our baseline finding that occupational licensing has a significant negative effect on these labor market transition rates.

Lastly, we implement a propensity score matching strategy (PSM) that not only enables matching at the mean but also balances the distribution of observed characteristics across treatment (licensed workers) and control group (non-licensed workers) in order to estimate the treatment effect of licensing. We match individual characteristics on the probability of being licensed, then use the generated propensity scores as weights in the linear probability regression model.[29] Propensity score matching, however, does not guarantee any level of imbalance reduction and may even increase imbalances and model dependence (Iacus, King and Porro (2012)). We apply a coarsened exact matching method and restrict the data to the common support every time before we apply the propensity score matching.[30] This

workers who show up in the sample only once, since our analysis is about the effect of licensing on labor market dynamics, which requires that we see the same worker at least twice. We further restrict our sample to observations that have a valid value for licensing status (licensing-related questions are provided only in the topical module in wave 13) and valid labor market transition records. Altogether, this leaves us with small samples and less statistically significant results.

[28]The set of universally licensed occupations is presented in appendix A.

[29]The characteristics included in the matching step are age, gender, race, education, marriage, and union status, income level, and occupational fixed effects.

[30]CEM is a monotonic imbalance reducing matching method, in which the balance between the treated and controled group is chosen ex ante (Blackwell et al. (2009))

approach ensures balance in the data and improves the reliability of our results. However, after applying CEM and PSM, our matched sample is significantly smaller than the original baseline sample (the sample is reduced by almost 50%), which indicates that the treatment group and control group may have significant differences and PSM pruned a large part of our baseline sample. However, the results shown in appendix C are in line with our baseline results, which offers our analysis more support.

6 Occupational Licensing during the COVID-19 Pandemic

The COVID-19 pandemic has undeniably affected almost every person. Part of this impact has been through its influence on labor markets. In this section, we revisit our baseline analysis and see if the effects of licensing on labor market transitions and wages have changed during the pandemic.

We first look at workers occupational switching patterns. Table 13 mimics the analysis in table 5 but is restricted to the pandemic period of 2020-2022. We see that licensing represents a barrier to entry during the pandemic, as it did during the pre-COVID period. In the first column, we see licensing has a statistically significant negative impact on the probability of workers switching into occupations, similar in magnitude to that of the pre-COVID period. As in the pre-COVID period, the effect of licensing on the probability of switching out of an occupation and switching in and out of unemployment is not statistically different than zero. Licensing does present a larger barrier to entry for employed workers than for unemployed workers, with slightly larger point estimates than those of the pre-COVID period. This again suggests that the time costs of obtaining licenses mean working agents face higher opportunity costs than unemployed agents. Overall, we do not see the effects of licensing on workers occupational transitions change too much after the pandemic hits the economy.

This is not the case for the effect on wages. In table 14, we revisit the effect of licensing on wage growth, replicating the analysis in Table 6 for the period 2020-2022. In the first row, we see that the baseline OLS results are largely similar between the COVID pre-COVID periods. Licensed workers are associated with higher wage growth whether they stay in, switch in or out of, or enter occupations. However, once we control for endogeneity with our IV strategy, we see that the causal effect of licensing on wage growth (or entry wages) decreases in magnitude and significance – essentially becoming zero. Before 2020,

licensed workers who stayed in their jobs enjoyed 8 to 9 percentage points higher wage growth than unlicensed workers. However, in the middle of pandemic (using our primary IV), this advantage entirely disappears, with an insignificant point estimate of -1.4%. The gain from switching into licensed occupations similarly disappears, going from 9% to an insignificant 0.6%. The only estimate that survives is the relative gain for unemployed workers of moving into a licensed occupation, versus that of moving into an unlicensed occupation. All of the results with the second IV become statistically insignificant during this period.

7 Conclusion

The number of workers in the United States who are licensed has been rising since the 1950s and was greater than 20% in 2019 (Cunningham (2019)). The magnitude of the licensing requirements has been increasing across most U.S. states. Using public data from the CPS and SIPP, we show that occupational licensing has a strong and negative effect on worker labor market flows but is associated with higher wage growth, whether a worker is staying in a licensed occupation or switching into a licensed occupation. Specifically, compared with other workers, licensed workers in our findings are 5.2 percentage point less likely to switch occupations and 0.6 percentage points less likely to become unemployed in the following year. Furthermore, licensed workers have wage growth rates that are 5.5 percentage points (pp) higher on average than those of other workers who stay in the same occupation next year and 3.9pp higher than those of other workers who switch occupations. Compared with occupations without licensing requirements, licensed occupations are more difficult to enter for both unemployed workers (0.5pp lower entrance rate) and workers who enter from other occupations (8pp lower entrance rate). Occupational licensing represents a larger barrier to entry if a worker is switching in from other occupations than if he or she is switching in from non-employment. The barrier effect of licensing on labor flows may partly explain the decline in occupational mobility over recent decades (Xu (2022)). These results raise further questions about the effects of licensing: Does licensing affect the quality of goods and services, and by what mechanism (Kleiner (2022))? How does licensing affect the welfare of workers outside of those licensed occupations relative to incumbent workers? What are the aggregate and distributional implications for heterogeneous changes in licensing coverage and entry costs? These are questions worth pursuing in future research to study how changes in occupation-specific entry costs affect economic welfare and the income distribution.

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Table 1: Monthly Licensing Status Changing Rate – before and after Imputation

Panels		Months		
Panel A:		M1-M2	M2-M3	M3-M4
Before correction	Licensing status changing rate (%)	52	32	28
Panel B:		M5-M6	M6-M7	M7-M8
Before correction	Licensing status changing rate (%)	69	48	40
Panel C:		M1-M2	M2-M3	M3-M4
After correction	Licensing status changing rate (%)	32	34	32
Panel D:		M5-M6	M6-M7	M7-M8
After correction	Licensing status changing rate (%)	39	40	38

Data source: CPS IPUMS. years 2016-2019

Table 2: Changes in Occupational Licensing Requirements

Occupation	Education (yrs)		Initial Cost		Renewal Cost	
	'95	'15	'95	%Δ ('15)	'95	%Δ ('15)
School Counselor	0.3	6.0	$15	296%	$18	243%
Architect	4.0	7.1	$161	-8%	$100	24%
Engineer	3.7	4.0	$124	55%	$46	101%
Physician Assistant	3.6	6.4	$72	123%	$62	99%
Real Estate Appraiser	2.2	4.0	$126	92%	$120	106%
Physical Therapist	4.4	5.9	$127	2%	$77	17%
Dentist	5.5	6.5	$175	93%	$163	99%
Psychologist	5.4	6.0	$205	57%	$140	77%
Teacher	2.0	4.0	$15	391%	$13	356%
Veterinarian	6.0	6.0	$24	494%	$27	435%
Total (Mean)	3.0	4.3	$92	107%	$78	115%

Note: Data source: OLLRP. Years of Education: 2 is High School, 4 is Assoc., 6 is Bachelor's, 8 is Post-Grad. All dollar values are in 2000 constant dollar values.

Table 3: Occupational Switching and Licensing

Variables	Switch In	Switch Out	J to N	N to J	J2J vs. N2J
Licensed	-0.080***	-0.052***	-0.006**	-0.005*	-0.022***
Age	-0.002***	-0.002***	-0.003***	-0.005***	0.004***
Female	-0.006	-0.006	-0.005***	-0.005***	0.003
White	-0.025***	-0.024***	-0.021***	-0.028***	0.021***
College	-0.002	0.000	-0.001	0.013***	-0.014**
Married	-0.013***	-0.010***	-0.058***	-0.054***	0.048***
Union	-0.037***	-0.037***	-0.027	-0.022***	0.012***
Logincome	-0.025***	-0.023***	-0.027***	-0.032***	0.023***
Observations	89,606	89,606	195,408	190,381	144,174
R^2	0.118	0.119	0.426	0.451	0.360
Fixed Effects	Yes	Yes	Yes	Yes	Yes

Data Source: CPS IPUMS. The coefficients represent the effects on annual occupational switching rates. The occupation classification used to calculate the switching rates is the IPUMS harmonized 2010 census code (440 categories). Fixed effects include occupation, state, year, and month.

Table 4: Wage Growth Effects of Licensing

Variables	Δw(stay)	Δw(switch out)	Δw(switch in)	w(new hires)
Licensed	0.055***	0.039***	0.068**	0.139***
Age	0.002***	0.002***	0.003***	0.006***
Female	-0.138***	-0.134***	-0.134***	-0.223***
White	0.022***	0.022***	0.022***	0.021***
College	0.131***	0.136***	0.170***	0.270***
Married	0.011*	0.048***	0.042***	0.064***
Union	0.087***	0.074***	0.101***	-
Logincome	-0.607***	-0.664***	-0.666***	-
Observations	46,120	43,367	43,367	100,731
R^2	0.323	0.369	0.375	0.367
Fixed Effects	Yes	Yes	Yes	Yes

Data Source: CPS IPUMS. The coefficients represent the effects on annual occupational switching rates. The occupation classification used to calculate the switching rates is the IPUMS harmonized 2010 census code (440 categories). Fixed effects include occupation, state, year, and month.

Table 5: OLS vs IV: Switching Rates

	Switch in	Switch out	J to U	U to J	J2J vs.U2J
β_1 (OLS)	-0.080	-0.052	-0.006	-0.005	-0.022
	(0.006)	(0.006)	(0.003)	(0.003)	(0.004)
β_1 (IV1)	-0.051	-0.011	0.006	0.006	-0.019
	(0.019)	(0.019)	(0.010)	(0.011)	(0.011)
β_1 (IV2)	-0.066	-0.012	-0.006	0.015	-0.031
	(0.023)	(0.023)	(0.013)	(0.013)	(0.016)
(Obs)	89,606	89,606	195,408	190,381	144,174

Data Source: CPS IPUMS.

Table 6: OLS vs IV: Wage Changes

	Δw (stayer)	Δw (switch out)	Δw (switch in)	New wage
β_1 (OLS)	0.055	0.039	0.068	0.139
	(0.008)	(0.010)	(0.010)	(0.008)
β_1 (IV1)	0.095	0.051	0.089	0.129
	(0.026)	(0.038)	(0.039)	(0.024)
β_1 (IV2)	0.083	0.038	0.102	0.128
	(0.034)	(0.050)	(0.046)	(0.030)
(Obs)	46,120	43,367	43,367	100,731

Data Source: CPS IPUMS.

Table 7: Baseline Analysis Using Alternative Months in CPS

Monthly Results	β_1	
Probability of Switching Out (J2J)	-0.017	
	(.005)	
Probability of Switching In (J2J)	-0.018	
	(.003)	
Probability of Switching Out to Unemployment (J2N)	-0.002	
	(.001)	
Probability of Switching In from Unemployment (N2J)	-0.001	
	(.000)	
Prob of Switching In from J vs N (N2J vs J2J)	-0.017	
	(.007)	
Annual Results	**Impute Wages**	**Impute Licensing**
Probability of Switching Out (J2J)	-0.069	-0.056
	(.003)	(.005)
Probability of Switching In (J2J)	-0.085	-0.070
	(.004)	(.005)
Probability of Switching Out to Unemployment (J2N)	-0.007	-0.013
	(.002)	(.003)
Probability of Switching In from Unemployment (N2J)	-0.004	-0.006
	(.002)	(.003)
Prob of Switching In from J vs N (N2J vs J2J)	-0.047	-0.025
	(.003)	(.003)

Data Source: CPS IPUMS.

Table 8: Percentage Effect of Licensing Using SIPP 2008

	SIPP Panel 2008
Probability of Switching Out (J2J)	-0.011
	(.005)
Probability of Switching In (J2J)	-0.011
	(.005)
Probability of Switching Out to Unemployment (J2N)	-0.004
	(.004)
Probability of Switching In from Unemployment (N2J)	-0.012
	(.004)
Prob of Switching In from J vs N (N2J vs J2J)	-0.102
	(.049)
Wage Growth for Stayer	0.019
	(.008)
Wage Growth for Switcher (out)	-0.0306
	(.165)
Wage Growth for Switcher (in)	0.146
	(.115)
Wage Growth for New Hire (N2J)	-0.236
	(.153)

Data Source: Waves 12, 13, and 14 of SIPP panel 2008. The indicator of licensing is from the topical module in SIPP wave 13. Wage in this analysis represents workers' monthly wages, and the switching rate is defined at the four-month frequency.

Table 9: Heterogeneous Licensing Effects across Occupations

	CPS Annual Results					
	occ1	occ2	occ3	occ4	occ5	occ6
Prob of Switching Out (J2J)	-0.104***	-0.062***	-0.96***	0.186*	-0.078***	-0.097***
Prob of Switching In (J2J)	-0.083***	-0.116***	-0.109***	0.082	-0.091***	-0.074***
Prob of Switching Out to N (J2N)	-0.006*	0.003	-0.013**	-0.031	-0.022**	-0.006
Prob of Switching In from N (N2J)	0.018***	0.015**	-0.045***	0.013*	0.013*	-0.018*
Switching In from J vs N	-0.047***	-0.036***	-0.022***	0.109**	-0.047***	-0.077***
Wage Growth for Stayer	0.060***	0.030**	0.070***	0.088*	0.035**	0.081***
Wage Growth for Switcher (out)	0.033***	-0.014	0.087***	0.458***	0.65***	0.042
Wage Growth for Switcher (in)	0.040***	0.024	0.093***	-0.175	0.032	0.133***
Wage Growth for New Hire (N2J)	0.097***	0.027*	0.105***	-0.077	0.129***	0.181***

Data Source: CPS IPUMS. The six occupations are 1. Managerial and Professional; 2. Technical, Sales and Administration Support; 3. Service Occupations; 4. Farming, Forestry, and Fishing; 5. Precision Production, Craft and Repair; 6. Operators, Fabricators and Laborers.

Table 10: Control Sensitivity Treatment Effect Bounds – Switching Rate

	Switch In Rate			Switch Out Rate		
	1	2	3	4	5	6
Variable of Interest						
Licensed	-0.200	-0.187	-0.080	-0.189	-0.173	-0.052
	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.006)
Controls						
Control 1		*	*		*	*
Control 2			*			*
Bounds and Deltas						
β			$(-0.097, -0.005)$			$(-0.052, 0.031)$
δ			1.05			0.67
R^2	0.03	0.03	$0.12(\tilde{R})$	0.02	0.03	$0.12\ (\tilde{R})$
Observations	89,745	89,607	89,606	89,745	89,607	89,606

Data Source: CPS IPUMS. Control 1 include: age, gender, education, race, marriage status, union status, income level. Control 2 include: year effects, month effects, state and occupation effects. Column 3 of the left panel is the same as the first row of column one of Table 3. Column 3 of the right panel is the same as the first row of column two of Table 3. The bound and deltas results are under the assumption that $R^2 = \tilde{R} * 1.3$.

Table 11: Control Sensitivity and Treatment Effect Bounds – Stayer Wage Growth

	Stayers ΔW		
	1	2	3
Variable of Interest			
Licensed	-0.006	0.065	0.055
	(0.007)	(0.008)	(0.008)
Controls			
Control 1		*	*
Control 2			*
Bounds and Deltas			
β			$(0.055, 0.099)$
δ			-2.7
R^2	0.00	0.24	0.32 ($\tilde{R}$)
Observations	46,135	46,135	46,120

Data Source: CPS IPUMS. Control 1 include: age, gender, education, race, marriage status, union status, income level. Control 2 include: year effects, month effects, state and occupation effects. Column 3 of the table is the same as the first row column 1 of Table 4. The bound and deltas results are under the assumption that $R^2 = \tilde{R} * 1.3$.

Table 12: Control Sensitivity and Treatment Effect Bounds – Switching Wage Change

	Switch In ΔW			Switch Out ΔW		
	1	2	3	4	5	6
Variable of Interest						
Licensed	0.006	0.058	0.068	-0.041	0.034	0.039
	(0.011)	(0.010	(0.010)	(0.011)	(0.009)	(0.010)
Controls						
Control 1		*	*		*	*
Control 2			*			*
Bounds and Deltas						
β			$(0.068, 0.096)$			$(0.039, 0.075)$
δ			-3.74			-1.36
R^2	0.00	0.29	$0.38(\tilde{R})$	0.00	0.28	$0.37\ (\tilde{R})$
Observations	43,377	43,377	43,367	43,377	43,377	43,367

Data Source: CPS IPUMS. Control 1 include: age, gender, education, race, marriage status, union status, income level. Control 2 include: year effects, month effects, state and occupation effects. Column 3 of the left panel is the same as column 3 of Table 4. Column 3 of the right panel is the same as column 2 of Table 4. The bound and deltas results are under the assumption that $R^2 = \tilde{R} * 1.3$.

Table 13: OLS vs IV: Switching Rates (2020-2022)

	Switch in	Switch out	J to U	U to J	J2J vs.U2J
β_1 (OLS)	-0.057	-0.048	-0.008	-0.024	-0.022
	(0.008)	(0.008)	(0.005)	(0.005)	(0.007)
β_1 (IV1)	-0.058	-0.017	0.005	-0.003	-0.038
	(0.015)	(0.027)	(0.015)	(0.015)	(0.021)
β_1 (IV2)	-0.057	-0.062	-0.011	0.020	-0.065
	(0.027)	(0.031)	(0.019)	(0.019)	(0.026)
(Obs)	47,676	47,676	107,298	112,388	72,519

Data Source: CPS IPUMS.

Table 14: OLS vs IV: Wage Changes (2020-2022)

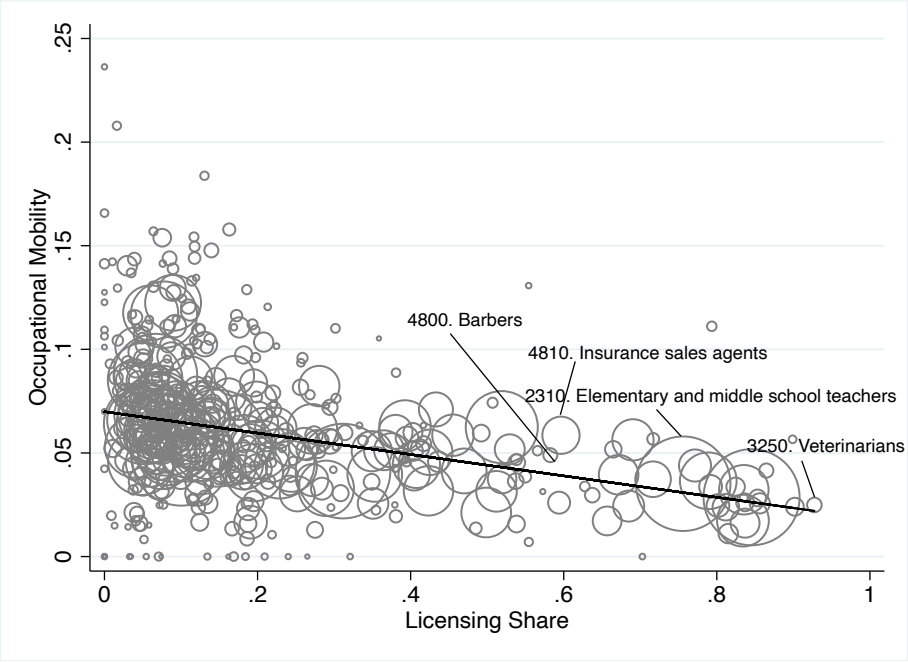
	Δw (stayer)	Δw (switch out)	Δw (switch in)	New wage
β_1 (OLS)	0.059	0.064	0.060	0.140
	(0.013)	(0.015)	(0.014)	(0.012)
β_1 (IV1)	-0.014	0.051	0.006	0.126
	(0.046)	(0.044)	(0.052)	(0.040)
β_1 (IV2)	0.061	0.038	0.063	0.083
	(0.052)	(0.054)	(0.067)	(0.052)
(Obs)	18,758	16,788	16,788	36,297

Data Source: CPS IPUMS.

Figure 1: **Licensing Rate vs. Occupational Switching Rate**

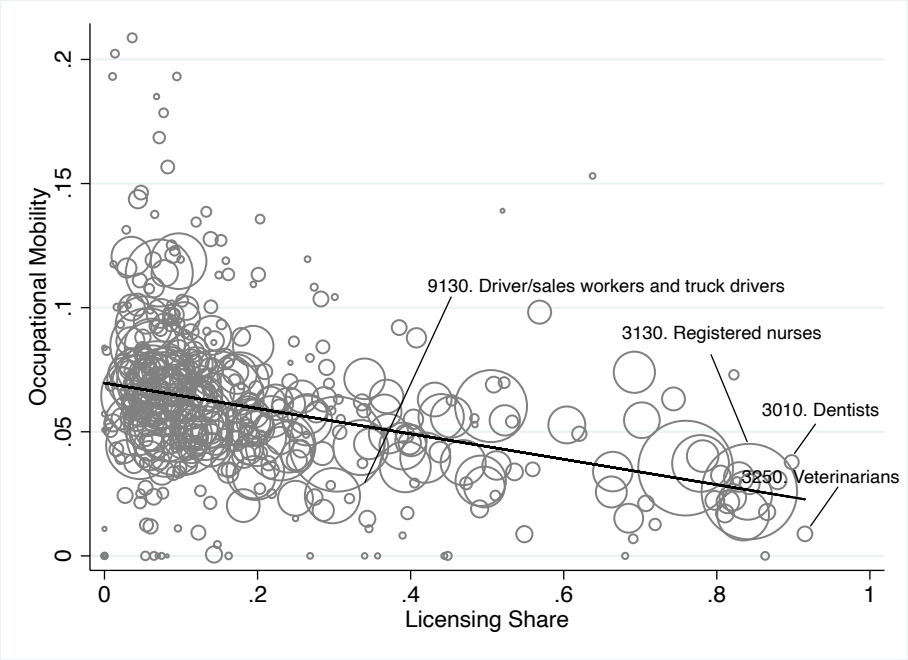
The occupational switching rate is from Xu (2022), and the licensing rate is from Kleiner and Krueger (2013)

Figure 2: **Licensing Share vs. Switching Out Rate**



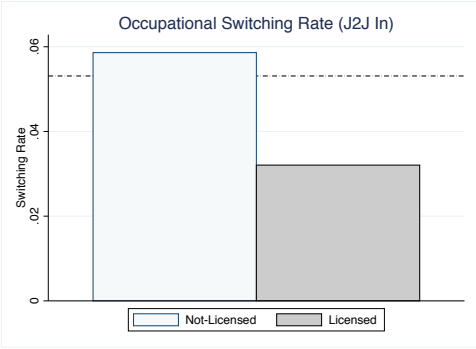
Data source: CPS IPUMS. The size of the circles represents the number of workers employed in that occupation.

Figure 3: **Licensing Share vs. Switching In Rate**

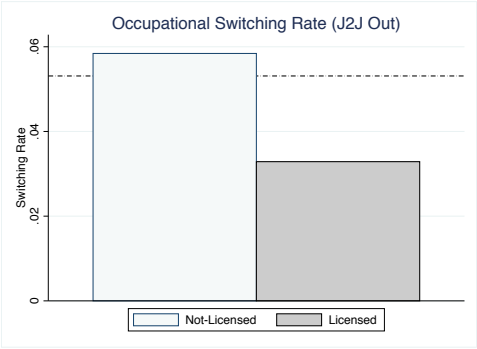


Data source: CPS IPUMS. The size of the circles represent the number of workers employed in that occupation.

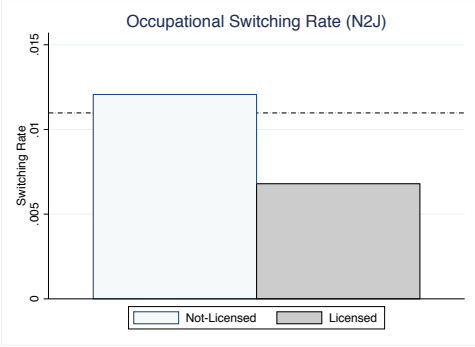
Figure 4: Average Occupational Switching Rates



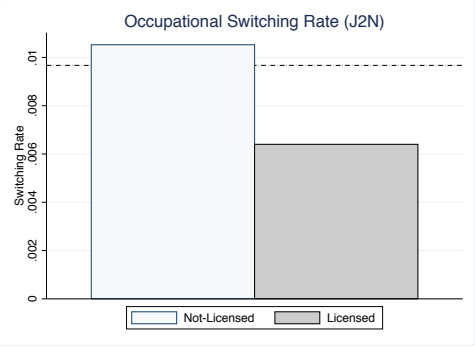
(a)



(b)



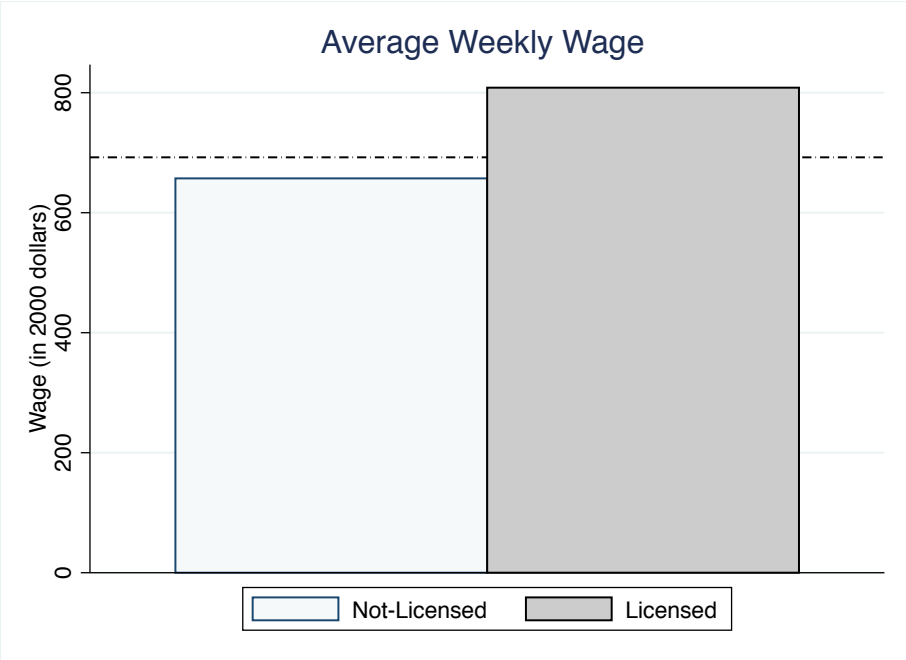
(c)



(d)

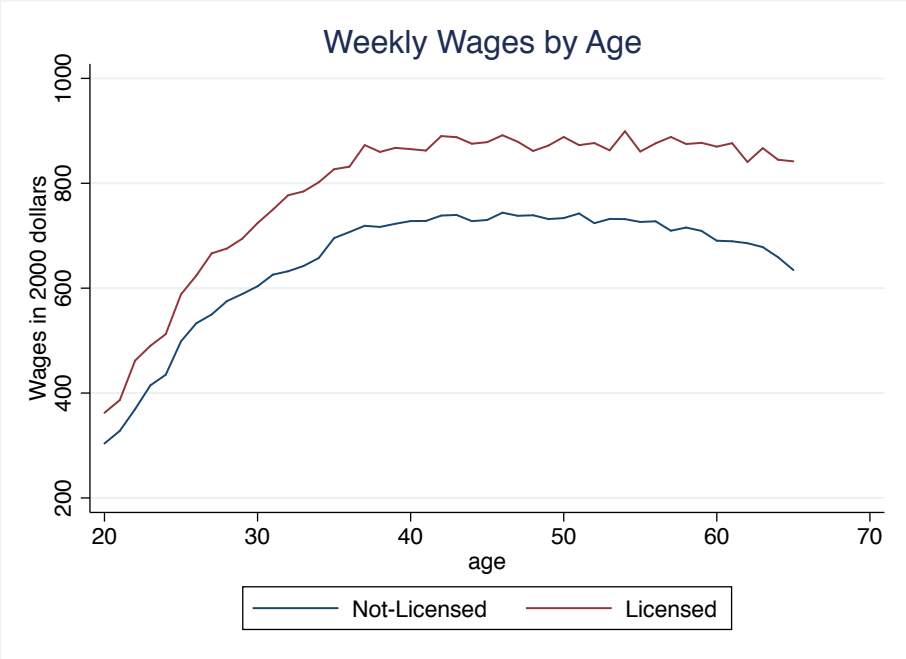
Notes: Data source: SIPP panel 2008, waves 12, 13 and 14.

Figure 5: **Average Weekly Wages**



Data source: CPS IPUMS, outgoing rotation group

Figure 6: **Average Weekly Wage Differences by Age**



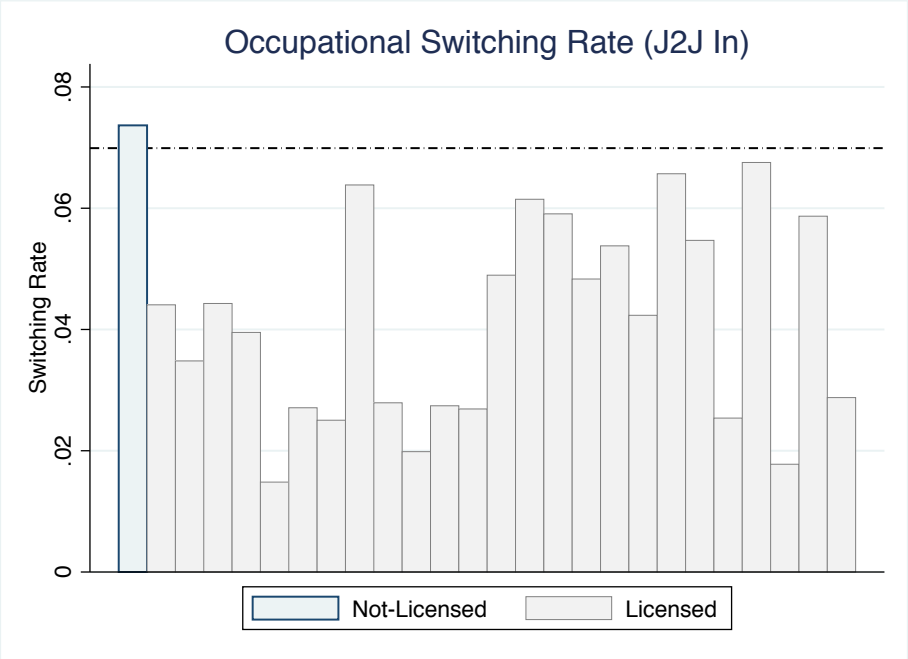
Data source: CPS IPUMS, outgoing rotation group

Figure 7: Wage Growth Rates

(a) (b)

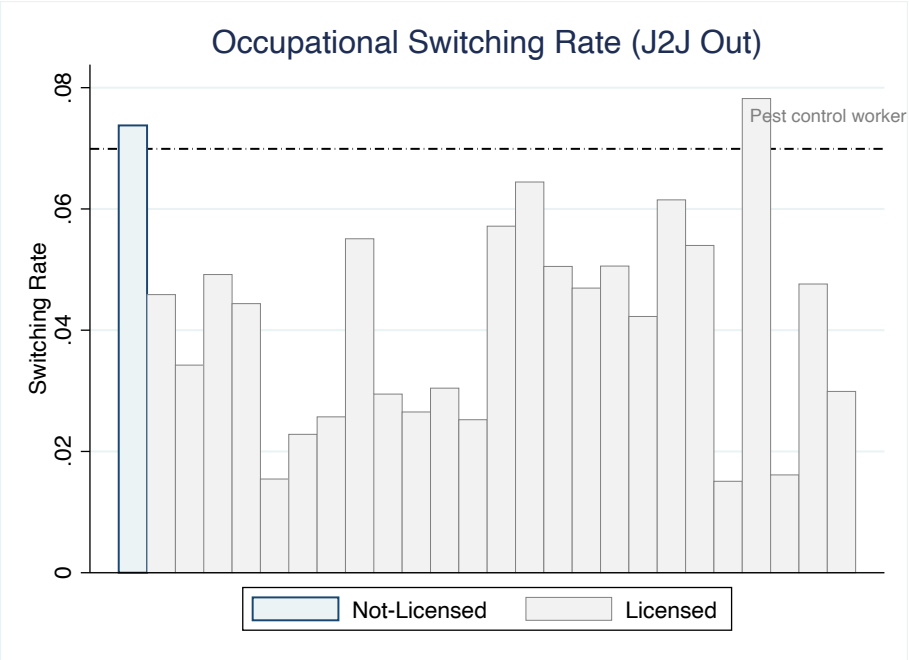
Notes: Data source: CPS IPUMS, outgoing rotation group.

Figure 8: **Average Switching In Rate by Occupations**



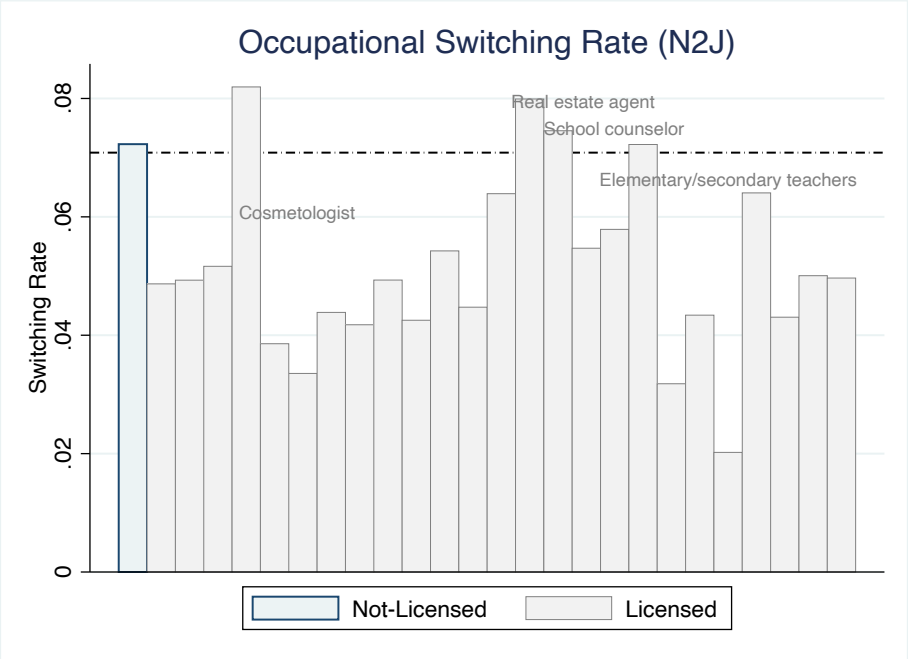
Data source: CPS IPUMS

Figure 9: **Average Switching Out Rate by Occupations**



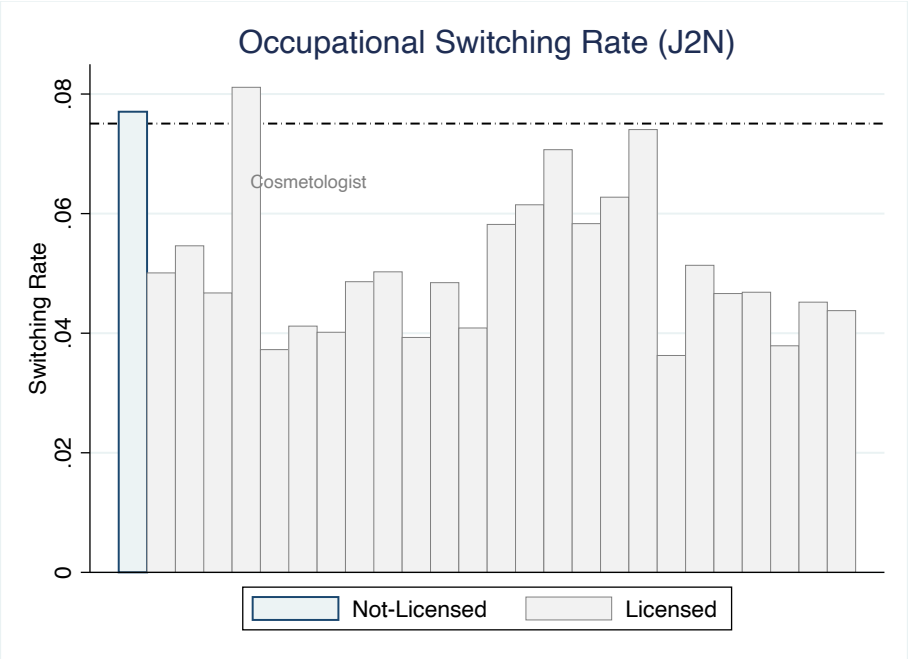
Data source: CPS IPUMS

Figure 10: **Average Switching In (From U) Rate by Occupations**



Data source: CPS IPUMS

Figure 11: **Average Switching Out Rate (to U) by Occupations**



Data source: CPS IPUMS

Appendix

A Universally Licensed Occupations

The following occupations are universally licensed in the U.S. (licensed in all 50 states and the District of Columbia). There are many occupations that are partially licensed – licensed in some states but not others. For example, security guards are licensed in 37 states, while bartenders are licensed in 13 states. Note that our main analysis uses individual-specific licensing data that do not rely on knowing these universally licensed occupations.

- Accountant/auditor; Architect (except landscape or naval); Barber; Bus driver (municipal); Chiropractor; Dental hygienist Cosmetologist; Dentist; Emergency medical technician; Engineer; Funeral director; Hearing aid dispenser; Insurance agent; Land surveyor; Insurance adjusters; Lawyer; Practical/vocational nurse; Medical and health service manager; Mortgage loan originator; Registered nurse; Nursing assistant; Occupational therapist; Occupational therapy assistant; Optometrist;Osteopath; Pesticide applicator; Pharmacist; Physical therapist; Physical therapy assistant; Physician assistant; Physician/Surgeon; Podiatrist; Psychologist; Real estate agent; Real estate broker; Real estate appraiser/assesor; School bus driver; School counselor; Securities; commodities and financial service agent; Social worker; Speech language pathologist; Truck driver; Veterinarian; Veterinarian technician/assistant; Teachers.

B CPS Imputation

We impute the CPS data in two ways. First, we keep worker observations from months 4 and 8 so that we have the most reliable data on wages. We then use workers' employment status information to modify the spurious licensing status. Specifically, we correct the imputed licensing status in the following four cases. The first two cases are when a worker's record shows a change in licensing status between month 1 and 4 or between month 5 and 8. If a worker is (is not) licensed in month 1/5 but is not (is) licensed in month 4/8, and he or she does not experience an occupation change (finely defined 2010 census code, 440 categories in IPUMS), industry change, employment status change, or class of work change, then we say the worker is still (is still not) licensed. We then modify the licensing indicator from 0 (1) to 1 (0). The third and fourth cases are when a worker's record shows that he or she

keeps the same licensing status from month 1 to 4 or from month 5 to 8. If a worker is recorded as licensed in both month 1/5 and 4/8 but has switched occupations[31] in between, the new occupation is not a universally licensed occupation (see the appendix for the list of universally licensed occupations), and furthermore his or her answer to the second question in the licensing questionnaire has changed from 1 to 0 between the two months, we then change the licensing status in 4/8 from 1 to 0. Similarly, if a worker is recorded as not licensed in months 4 and 8 but has switched into a universally licensed occupation, and the answer to the first question in the licensing questionnaire has changed from 0 to 1, we then change the licensing indicator from 0 to 1. Lastly, for workers who are not licensed in either months 1 or month 4 and have switched occupations between months 1 and 4, we further check if the worker's occupation in month 4 is the same as it is in month 5. If it is the same, and his or her licensing status in month 5 is "licensed", we then change the worker's licensing status in month 4 from 0 to 1. These modification steps are not perfect. For example, workers in universally licensed occupations haven't necessarily attained a license. However, we see these as first steps in utilizing these licensing data and the panel structure of the CPS and have done what we can to make sure that the imputed licensing status data are as reliable as possible. To verify the reliability of our exercise, we compare the switching rate in licensing status for interview months and imputation months, which is the same exercise as in Table 1, panel A and C. After imputation, the licensing status change rate shows much more balance between interview months and imputation months. Table 1, panel B and D show the results: after the imputation process, the licensing status change rate is uniformly distributed across all the interview months. This reassures us that our imputation improves the reliability of the licensing indicators.

In our second imputation method, we keep observations from months 1 and 5 and impute wage values using data from months 4 and 8. We keep workers in months 1 and 5 to ensure that we have the most reliable licensing indicator. However, we lose workers who are no longer in the sample in months 4 or 8 and those who have changed their labor market status. We also cannot account for potential high-frequency wage changes, so the wage values are not as reliable as when we employ the first imputation method above. In particular, we impute values only for workers who haven't experienced occupation, industry, employment, and class changes. The assumption we impose here is that if a worker hasn't changed his

[31] Here, occupations are defined in six coarse groups: (1) managerial and professional; (2) technical, sales and administration Support; (3) service occupations; (4) farming, forestry, and fishing; (5) precision production, craft and repair; (6) operators, fabricators and laborers. We consider these types of occupational switches to be significant career changes, which are more likely to be associated with licensing status change.

or her occupation and employment status within four months (month 1 to 4, or 5 to 8), his or her wage will stay the same. This imputation imposes very strong assumptions on wage observations, and these assumptions could bias our wage change analysis. However, we use this imputation method (imputed wages with reliable licensing indicators), together with the first method (reliable wages with imputed licensing indicators), to make sure our results are reasonably robust. Since imputing wages is likely to induce more error, we use the data from our first imputation in our baseline and are our preferred results.

C Alternative Licensing Indicator and Robustness Results

As mentioned in the baseline analysis, licensing is not a randomly assigned treatment, and workers who are licensed may be systematically different from workers who are not. In this section of robustness check, we firstly use the same licensing indicator as the baseline analysis, but restrict our analysis to the CEM and PSM matched sample. Specifically, we match people in all the observables included in our baseline regression on the probability of being licensed. We then use this balanced treated and untreated sample to get licensing effect on workers mobility and wage changes. The results are presented in table C.1 and table C.2. With the matched sample, we see that even though the sample size changed for much of the analysis, the effect of licensing is very similar to that in our baseline results. This offers us confidence that what we see in the baseline is a good measure of the licensing effect on workers' mobility and earnings.[32]

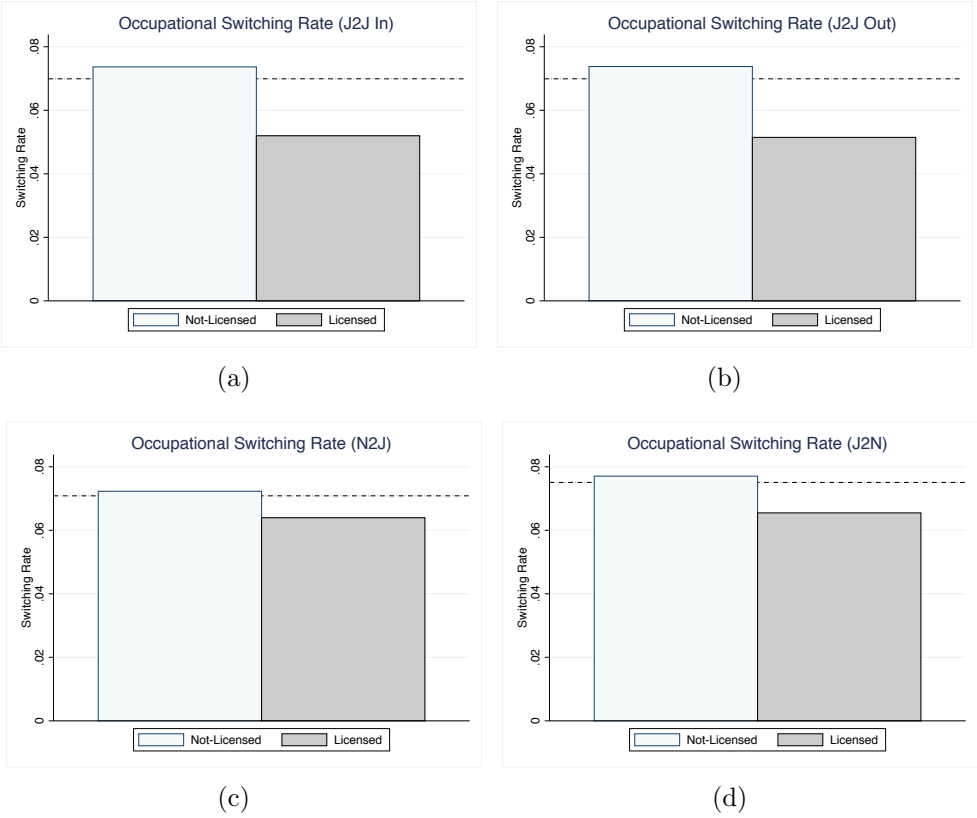
We then present results using a licensing indicator different than the one in our baseline analysis. Specifically, we use employment in a universally licensed occupation as an indicator of being licensed. As mentioned in the data cleaning section, this indicator has its drawbacks. It doesn't necessarily reflect the true licensing status of an individual (since individuals may be licensed in a non-universally-licensed occupation, or unlicensed in a universally licensed occupation). However, it also allows us to use a much longer panel of data (over 20 years) for our analysis. The results are therefore not quantitatively comparable to our baseline analysis, but can still be qualitatively compared with what we have already shown in the main body of the paper.

[32]With this CEM and PSM matched sample, we also performed IV analysis as well as the bounded robustness check. The results are again very similar to our results in the main body of the paper, and they are available upon request.

We first calculate the raw economy-wide switching rates for each occupation and compare the switching in and out rates between universally licensed occupations and non-universally-licensed occupations. We present the data in figure 11 and figure C.1. A brief glance across the figures will make it immediately clear that licensed occupations experience far less churn than other occupations. The rate at which people switch into licensed occupations is lower, and so is the rate at which people switch out. This result is in line with our baseline results using individual-level occupational licensing attainment as the licensing indicator.

We then perform an empirical analysis similar to the one before,, regressing worker switching status and switching-associated wage changes on observable worker and job characteristics. The results are shown in table C.3. A striking result from this analysis is that even when we are using a different licensing indicator, different data sets, and different time spans, the effects of licensing on transition patterns are remarkably similar. Switching in and out of licensed occupations is much less frequent than switching in and out of non-licensed occupations, regardless of whether the transition is through other jobs or from unemployment. Occupational licensing imposes a cost that is time-consuming for workers, so conditional on switching, it is more likely that an individual switches to a licensed occupation from unemployment than from other occupations.

Figure C.1: Average Occupational Switching Rates



Notes: Data source:SIPP, years 1990-2013

Table C.1: Occupational Switching and Licensing

Variables	Switch In	Switch Out	J to N	N to J	J2J vs. N2J
Licensed	-0.096***	-0.097***	-0.005**	-0.005*	-0.044***
Age	-0.001***	-0.001***	-0.001***	-0.005***	0.005***
Female	-0.019***	-0.002	-0.002	-0.024***	0.014
White	0.010*	-0.033***	-0.023***	-0.016***	0.019***
College	-0.006	-0.019***	-0.006***	-0.045***	-0.008**
Married	-0.015***	-0.018***	-0.037***	-0.056***	0.061***
Union	-0.015***	-0.024t***	-0.017	-0.022**	0.010***
Logincome	-0.026***	-0.016***	-0.019***	-0.012***	0.018***
Observations	63,437	66,502	74,928	115,852	74,824
Matched	Yes	Yes	Yes	Yes	Yes

Table C.2: Wage Growth Effects of Licensing

Variables	Δw(stay)	Δw(switch out)	Δw(switch in)	w(new hires)
Licensed	0.063***	0.038***	0.054**	0.114***
Age	0.002***	0.002***	0.002***	0.005***
Female	-0.148***	-0.151***	-0.151***	-0.249***
White	0.032***	0.040***	0.051***	0.055***
College	0.127***	0.121***	0.153***	0.259***
Married	0.005**	0.032***	0.026**	0.030***
Union	0.041***	0.056***	0.073***	0.144***
Logincome	-0.588***	-0.613***	-0.633***	-
Observations	42,369	27,449	21,989	52,641
Matched	Yes	Yes	Yes	Yes

Data Source: CPS IPUMS.

Table C.3: Occupational Switching and Licensing

Variables	Switch In	Switch Out	J to N	N to J	J2J vs. N2J
Licensed	-0.015***	-0.015***	-0.006**	-0.005*	-0.053***
Age	-0.002***	-0.002***	-0.003***	-0.002***	-0.002***
Female	-0.015**	-0.014***	0.003**	0.003***	0.005***
White	-0.025***	-0.024***	-0.021***	-0.028***	0.021***
College	0.009***	0.010**	-0.001***	0.013***	0.004**
Married	-0.020***	-0.010***	-0.058***	-0.054***	0.048***
Observations	1,126,104	1,127,636	1,293,988	1,288,114	174,987

Data Source: CPS IPUMS, years 1994-2018