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. Using a balanced panel of workers constructed from the CPS and SIPP data, we analyze the link between occupational licensing and labor market outcomes. We find that workers with a government-issued occupational license experience churn rates significantly lower than those of non-licensed workers. Specifically, licensed workers are 24% less likely to switch occupations and 3% less likely to become unemployed in the following year. Moreover, occupational licensing represents barriers to entry for both non-employed workers and employed ones. The effect is more prominent for employed workers relative to those entering from non-employment, because the opportunity cost of acquiring a license is much higher for employed individuals. Lastly, we find that average wage growth is higher for licensed workers than non-licensed workers, whether they stay in the same occupation in the next year or switch occupations. 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. Overall, licensing could account for almost 8% of the total decline in monthly occupational mobility over the past two decades.

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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)). The increase in the number of licensed occupations and workers was generally motivated as a consumer protection measure. However, for workers who are trying to enter or change a profession, increased licensing may also result in barriers such as restricted geographical and occupational worker mobility; decreased worker welfare; increased consumer prices; and ultimately, impaired economic growth (Chetty (2009), Johnson and Kleiner (2017), 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[1] occupations across every state from the 1980s to 2016. 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. One of our key interests in this paper is 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 while the licensing rate (the fraction of the workforce that is licensed) has trended up steadily, the occupational mobility or switching rate has declined over the past two decades. This suggests that there may be a link between workers' licensing status and occupational switching decisions. Using the CPS to focus on occupational-level data, we find that occupations with higher licensing shares are experiencing relatively lower churn rates.[2] Figure 2 shows the relationship between the licensing share and the switching-out rate, while Figure 3 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] The licensing share in an occupation is defined by the share of the total working population that holds a government-issued license in a given occupation.

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 (2017) 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 (2019) 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, similar to 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 relationship between licensing, occupational switching, and the wage changes associated with these transitions. Using propensity score matching (PSM), we compare the switching patterns and wage changes of those who are licensed with those of workers who are not. The matching criteria are based on a series of

observable worker characteristics as well as the occupations' skill requirements, which are determined by applying principle component analysis to O*NET data on the occupation task-skill mix. Before applying PSM, we first employ a coarsened exact matching (CEM) strategy to ensure that our sample is balanced. The preliminary findings show that occupational licensure has significant negative effects on occupational mobility when switching both into and out of licensed occupations. Specifically, we find that workers who are licensed are 23.6% (9.7 percentage points) less likely to switch to another occupation next year and 3% (0.5pp) less likely to become unemployed. Workers who are licensed are 24.1% (9.6pp) less likely than other workers who are not licensed to have just switched into their occupation. After controlling for observable heterogeneity, we find that those switching into a licensed occupation experience higher wage gains (5.4 percentage point higher growth rate, or an additional $1,834[3] 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, 6.3 percentage points higher in log wage changes or $2,208 more than non-licensed workers) or switch occupations (on average, 3.8 percentage points higher or $1,251 more than non-licensed workers). Furthermore, we find that compared with employed workers, licensing presents less of a barrier to entry for the unemployed, 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 (2017), 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 suggested that in addition to static general equilibrium welfare effects that Kleiner and Soltas (2019) suggests, 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

[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.

data cleaning steps. Section 4 outlines our empirical analysis, and section 5 contains the baseline results. Section 6 focuses on the heterogeneous effects of licensing across different occupation groups, and we summarize and conclude in section 8.

2 Theoretical Model

In this section, we build a simple recursive dynamic discrete-choice model and use it to discuss how licensing may affect workers' occupation choices and switching decisions. We then discuss how one can extend this model and use it to estimate the lifetime welfare effects of occupational licensing.

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

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

where the subscripts denote worker i in occupation j at age a, and A is the age at retirement. Workers receive job-specific preference shocks ζ_{ija}, with ρ representing the relative importance between the monetary and nonpecuniary components of worker utility. A worker's consumption from a particular job can be written as

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

where 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 can then specify the switching cost term as

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

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 consists of two parts that differ depending on whether the destination occupation is licensed (subscript L) or non-licensed (subscript N).

κ_1 and κ_2 capture the entrance fees for licensed and non-licensed occupations, 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

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

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

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

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

The problem described above can then be solved recursively by starting from age $a = A$ and working backwards. Depending on the choice of state variables and wage structure, solving the general problem can be complicated and is beyond the scope of this section.[5] For illustrative purposes, we simplify the problem using the following assumptions. First, we assume 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 market wage for each occupation and we assume $w_L > w_N > b$, where b denotes unemployment income.

[5] See Xu (2019) 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, so we interpret workers going to unemployment as workers receiving a very positive preference shock for unemployment or very negative preference shocks for all the potential occupations.

This means that in the example, we do not consider the wage effects of factors such as idiosyncratic tenure or skill accumulation. Our final assumption is that a worker's state contains only their past occupation and age. Later, 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. When $a = A$,[7] we can write all of the possible worker values as

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

Recall that in this value function, the first element denotes the worker's state (in the current case, this is just age). The second and third elements denote the worker's previous and current occupation. Note that in the last period, there is no continuation value for workers so workers consider only the wages (and nonpecuniary shock) that come with the occupation they choose and the switching costs associated with it. Furthermore, without loss of generality, we have normalized the 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 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 investigate 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

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

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

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

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

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—the probability of worker entry declines. Furthermore, if we assume that the average wages for licensed and non-licensed occupations are similar ($w_L \approx w_N$), then 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), 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, which in turn increases incentives to obtain the license. 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:

$$
\frac{dProb_{N\to L}}{d\kappa_3} = \frac{1}{\rho} Prob^2_{N\to L} \left[e^{\frac{1}{\rho}((1+\kappa_3)w_N + \kappa_1 - w_L(\kappa_3))} + e^{\frac{1}{\rho}(b+\kappa_1+\kappa_3 w_N - w_L(\kappa_3))} \right] (\frac{dw_L}{d\kappa_3} - w_N).
$$

Notice that the expression takes the same sign as the last term, 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, because the marginal benefit for younger workers is much larger because of their longer career prospects. Finally, we see that licensing entrance fees (κ_1) always decrease the probability of workers' getting licensed.

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 for working and therefore "voluntarily" moving into unemployment. We compare the probability of switching into unemployment for non-licensed and licensed workers:

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

Note that in the case that 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. These are implications that we can directly test in the data.

This simple model allows us to establish some intuition about how licensing will affect worker welfare and occupation entry and exit. However there are some key limitations relative to what we will look at in the data. First, having only two occupations (and non-employment) implies that we cannot say much about how licensing affects flows between non-licensed or licensed occupations. In this example, every job-to-job transition into L is also an exit out of N. Relaxing this assumption by having an arbitrary number of occupations would allow workers to flow between different licensed or non-licensed occupations. This would allow us to gain intuition separately for the effects of licensing on exit and entry probabilities within and between classes of occupations, which are what we investigate in our empirical section. We could also relax our second assumption and allow wages to depend on individual idiosyncratic characteristics. This would allow the model to better fit wage patterns in the data and also gain more intuition on how licensing affects wages. Our simple example focuses only on the mean wage differentials between licensed and non-licensed occupations and how this affects worker flows. In our empirical analysis, we also investigate how licensing affects worker 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 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.[8]

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. No nationally representative survey in the United States has 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.

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 whether he or she 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 the first two questions.[9]

The SIPP also has information on occupational licensing in two separate panels: 2008 and

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

[9] We use an alternative definition and define a respondent who answers yes to all three questions as a "licensed" worker, and the results from this analysis are qualitatively and quantitatively very similar to our baseline analysis. The results are available from the authors.

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 answers "Federal government, state government, or local government" to the third question, then we regard him or her 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, "Has...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 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 being licensed.[10] 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, they both provide qualitative evidence of occupational licensing effects on labor market outcomes that are consistent with each other. We do not use panel 2014 of the SIPP in this paper since only one panel of data is currently available. However, since the licensing-related questions are included in the core data, it will be a rich and valuable source for future work on occupational licensing.

[10] 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.

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 (Flood et al. (2015)) for 2015 through 2018. 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. It is also recorded monthly, so we can observe how licensing affects monthly occupational transitions. The CPS also has two major drawbacks: First, the questions about licenses and certification are asked only in months 1 and 5 for each individual, with values in other months being imputed. This imputation could 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 suggested 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. To be consistent in our analysis, 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 are required. We impute data in two ways. First, we keep worker observations from months 4 and 8 so we have the most reliable data on wages. We then use workers em-

ployment status information to modify the spurious licensing status. In the second method, we keep observations from months 1 and 5 and impute wage values using data from months 4 and 8. 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.

3.2 SIPP Sample Selection

The SIPP collects information on up to two jobs for each individual. We first 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 where 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, we 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 based on whether an occupation is universally licensed. The list of universally licensed occupations is provided in the Appendix A. This non-strict definition allows us to conduct some analysis 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 in Appendix C, we show the results.

3.3 Supplementary Datasets

The third dataset we employ is Occupational Licensing Law Research Project (OLLRP). OLLRP is newly constructed detailed data set on occupational licensing and requirements for all universally licensed occupations across states and time, 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. While the data are very detailed, the dataset is not yet complete for all occupations, and so we cannot present a complete aggregate analysis using it. However, what we have so far shows clear evidence regarding trends in occupational licensing requirements. Based on the completed data, we see that both the licensing fees and renewal costs have increased for almost all occupations. The national average increases in licensing fees range from $0 for occupations such as truck drivers to over $300 for occupations such as funeral directors.[11] Licensing requirement changes are not universal across states. Dentists see an increase in licensing costs of $950 in Alabama but no change in Arizona. In Table 2 present 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 shows 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. Many other types of requirements are available in this dataset such as an experience or exam requirements. 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.

Besides the newly constructed licensing data, we also use O*NET in our regression analysis. Our primary interest is in the effect of licensing on employment dynamics and outcomes. One challenge is to separate the licensing effects from other potential confounding factors. For example, workers who are licensed may tend to be more educated. Both the SIPP and CPS record detailed information on individual characteristics. This information enables us to carefully control for worker heterogeneity, which might be correlated with licensing status. Furthermore, we want to control for occupational characteristics. In our analysis, we use O*NET to separate the effect of working in the occupation requirements from that of licensing requirements. O*NET, also known as the Occupational Information Network, is a database describing occupations in terms of skill or knowledge requirements and worker practices. We use all measures of occupational requirements from the following panels: knowledge,

[11] To recap, all dollar amounts in the paper are in USD deflated to constant 2000 dollars.

abilities, skills, and work activities. We reduce this large set of occupational requirements to three dimensions by applying principal component analysis (PCA). We then follow Lise and Robin (2017) and recover the cognitive, manual, and interpersonal skill requirements for each occupation. For each occupation, we map the skill requirements acquired from ONET into the occupations we observe in the SIPP and CPS.

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 figures 4 through 7. An examination of the figures shows that licensed workers experience far less churn than non-licensed workers. The rate at which people switch into licensed occupations is lower (Figure 4 shows workers who switched in from a different occupation, and Figure 6 shows workers who switched in from unemployment), as is the rate at which people switch out (Figure 5 shows workers switching to a different occupation, and Figure 7 shows workers switching into unemployment.). This result holds even 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 who are working in universally licensed occupations (regardless of licensing attainment status) experience far less churn than other occupations (see figures C.1 through C.4 in Appendix C). However, this difference in switching rates between licensed and not 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 this analysis is to determine whether occupational mobility is different for workers who are licensed and those who are not and to measure the specific effect of licensing on mobility. To do this, we use data from both the CPS and SIPP as described above. Our

baseline results are from the CPS data. In the CPS, we use monthly data on individual occupation, employment status, demographics, and other characteristics. These data allow us to construct measures of mobility at the individual and occupational level. Our baseline strategy is to construct measures of mobility into and out of each occupation and determine the link between whether those individuals are licensed and the probability of switching.

Our first specification, examines the probability of switching out of an occupation, conditional on that occupations being licensed or not. Let an individual's sequence of occupation choices be $\{\ldots, O_{it-1}, O_{it}, O_{it+1}, \ldots\}$. The switching out measure is $Y_{it} = 1$ if $O_{it} \neq O_{it+1}$ and 0 otherwise. The basic regression is then the following linear probability model:

$$Y_{it} = \beta_0 + \beta_1 L_{it} + \varepsilon_{it}, \tag{1}$$

where $L_{it} = 1$ if the individual is licensed in period t, and $L_{it} = 0$ otherwise. The parameter of interest, β_1, will tell us the conditional probability of a worker who is licensed, switching occupations versus that of a worker who is not licensed.

This regression is potentially problematic. First,individuals who are licensed may be more or less likely to switch out of those occupations for reasons besides licensing. For example, licensed workers may tend to be in higher-skill occupations. If switching occupations leads to relative losses of human capital, workers in occupations that require more human capital or occupation-specific skills may be less likely to switch out. We control for these potential differences in the characteristics of occupations by including a set of occupation-specific skill requirements as independent variables in the linear probability model. In particular, for each occupation j, we use principal component analysis and detailed data on occupational skill requirements from O*NET to construct a three-dimensional skill requirement vector $S_j = (C_j, I_j, M_j)$: cognitive skill (C_j), interpersonal skill (I_j), and manual skill (M_j).[12] Furthermore, we include in our baseline analysis a set of individual observable characteristics, occupation fixed effects, occupation employment shares, and occupation state fixed effects interactions. [13] Moreover the treatment of being licensed versus not licensed is not

[12]The skills are constructed over 440 occupation categories. Here, we include skill levels rather than the full set of occupation fixed effects because our sample size is greatly reduced after applying the CEM and PSM matching methods.

[13]Occupational fixed effects and the state-occupation fixed effects that are included in the analysis include 17 coarsely defined occupation groups: 1. Executive, Administrative, and Managerial Occupation; 2. Management Related Occupations; 3. Professional Specialty Occupations; 4. Technicians and Related Support Occupations; 5. Sales Occupations; 6. Administrative Support Occupations; 7. Housekeeping and Cleaning Occupations; 8. Protective Service Occupations; 9. Other Service Occupations; 10. Farm Operators and Managers; 11. Other Agricultural and Related Occupations; 12. Mechanics and Repairers; 13. Construc-

random. To get the proper treatment effect of licensing, 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 groups (non-licensed workers).[14] We match individual characteristics[15] on the probability of being licensed, then use the generated propensity scores as weights in the linear probability regression model. Our baseline specification is then

$$Y_{it} = \beta_0 + \beta_1 L_{it} + X_{it}\Gamma + \beta_2 O_{it} \times S_{it} + \theta_t + \varepsilon_{it}. \tag{2}$$

As before, L_{it} is the indicator of a worker's being licensed, and β_1 is a measure of the effect of licensing on the probability of switching occupations in the next period. X_{it} is a vector of individual characteristics in period t, including age, education, gender, race, marriage status, union status, log wage, the skill vector for the individual's occupation in period t, and the occupational employment shares. O_{it} denotes the occupational fixed effects[16] and S_{it} denotes the state fixed effects. θ_t is a set of year and month fixed effects. For this regression model , we restrict the sample to individuals who were employed in both periods t and $t+1$.

One of the key goals of using matching methods is to prune observations from the data so that the remaining data provide a better balance between the treated and control groups. Propensity score matching, however, does not guarantee any level of imbalance reduction and may even increase imbalance and model dependence (Iacus, King and Porro (2012)). One typical way of addressing the imbalance issue is to check the estimation results and, if needed, repeatedly re-calibrate the method, which can be time consuming and less reliable. In this paper, we apply a coarsened exact matching method and restrict the data to the common support every time before we apply the propensity score matching.[17] This approach ensures balance in the data and improves the reliability of our results.

We also analyze how licensing relates to the probability of switching into an occupation. Since licensing represents a barrier to entry, one might expect that being licensed will drive down switching rates for licensed occupations, but by how much? To answer this question

tion Trades; 14. Extractive Occupations; 15. Precision Production Occupations; 16. Machine Operators, Assemblers, and Inspectors; 17. Transportation and Material Moving Occupations

[14] We recognize that PSM does not fully deal with the biased estimation coming from unobservable variables. We address this issue in Section 7.

[15] The characteristics included in the matching step are: age, gender, race, education, marriage and union status, income level, occupational skills, year, and 17 coarsely defined occupational fixed effects.

[16] We use the 17 occupation groups defined previously in the paper.

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

we use a modified version of the first linear probability model. Here, the dependent variable Y_{it-1} is an indicator that equals 1 if the worker switched into his or her current occupation between the previous and current periods and equals 0 otherwise. We employ the same strategy as above, where we control for the individual's current characteristics, his or her current occupation's skill requirements, occupational and states fixed effects, and weight. We use the same matching strategy as above:

$$Y_{it-1} = \beta_0 + \beta_1 L_{it} + X_{it}\Gamma + \beta_2 O_{it} \times S_{it} + \theta_t + \varepsilon_{it}. \tag{3}$$

This regression gives us the probability of switching into an occupation conditional on individual and occupation characteristics. The interpretation of the β_1 parameter in this regression is the effect of being licensed now on the probability of having just switched into current occupation from the previous period. Similar to the above specification, the sample for this regression is all individuals who were employed in both period $t-1$ and period t. One natural question regarding this analysis is whether we are mixing the licensing effect with the impact of limited employment opportunities. For example, if we find that workers are less likely to switch occupations and become a lawyer (a universally licensed occupation), is it because licensing makes it difficult to switch into that job, or is it because there simply are not a lot of job opening opportunities to become a lawyer? To address this, we include the occupational employment share[18] to absorb the effect of limited opportunity on the switching rate.

We are also interested in how licensing may affect mobility for unemployed workers—that is, the probability that an individual switches from unemployment into an occupation or vice versa, conditional on the occupation being licensed. To do this, we run the same two regressions as above, but instead of Y_{it} being 1 for anyone who had a job-to-job switch, we set it to 1 for those who switch from unemployment to employment and vice versa for Y_{it-1}. The sample for the former is all individuals who were employed or unemployed in the second period and employed in the first period. For the latter, it is all those employed or unemployed in the first period and employed in the second period.

Compared with those who are employed, is licensing more or less of a barrier for individuals who are unemployed? To investigate this, we estimate the following linear probability model using the subsample of workers who are working in this period but have switched into current occupation from either unemployment or a different occupation (i.e., we are

[18] We use the 2010 Census occupation classification, the same classification we use in constructing the indicator for occupation switching.

excluding occupational stayers from t-1 to t).

$$Y_{it-1} = \beta_0 + \beta_1 L_{it} + X_{it}\Gamma + \beta_2 O_{it} \times S_{it} + \theta_t + \varepsilon_{it}. \tag{4}$$

This model is identical to the previous one but has a different sample and interpretation. Here, $Y_{it-1} = 1$ if the worker switched from unemployment in the last period into their current occupation and 0 if he or she switched from another occupation into the current occupation. The coefficient on L_{it} can then be interpreted as the difference in the probability for licensed versus non-licensed occupations that a worker who switched into his or her current occupation came from unemployment. If $\beta_1 > 0$, then workers who have just switched into a licensed occupation are more likely than workers who just switched into a non-licensed occupation to have come from unemployment, suggesting that licensing is less of a barrier for the unemployed than for the employed.

Finally, we look at the effect of licensing on wage changes for job switchers. There is a significant literature on how occupational licensing affects wages (see Gittleman and Kleiner (2016) for a recent example) but far less work on how it affects changes in wages for those who stay in or switch between occupations. Our strategy is to look at how changes in wages for stayers and switchers relate to licensing.

We first calculate raw economy-wide average weekly wages for licensed workers versus non-licensed workers, without any controls. Figure 8 confirms the previous literature that states that licensed workers on average enjoy a wage premium compared with non-licensed workers. This wage difference widens as age increases. Figure 9 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. Figure 10 shows the result for workers who stay in the same occupation during a year, while Figure 11 shows the result for occupation switchers. We see in Figure 10 that when workers stay in the same occupation, licensed workers have lower wage growth rates relative to 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 lead workers without licenses to

have 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 separately identify these confounding factors from the direct effect of licensing, we use the following specification:

$$\Delta w_{it} = \beta_0 + \beta_1 L_{it} + X_{it}\Gamma + \beta_2 O_{it} \times S_{it} + \theta_t + \varepsilon_{it}, \tag{5}$$

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 differences 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 between two periods, we control for both the new occupation and their old occupational skill levels, as well as both their new and old occupational employment share. Furthermore, to look at how licensing affects wages for those switching into licensed versus non-licensed jobs, we shift the timing of the regression and estimate the following model

$$\Delta w_{it-1} = \beta_0 + \beta_1 L_{it} + X_{it}\Gamma + \beta_2 O_{it} \times S_{it} + \theta_t + \varepsilon_{it}. \tag{6}$$

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

5 Baseline Results

Our baseline results on occupational mobility are summarized in tables 3 to 7. For each regression model, column A shows the results without any controls; Column B shows the results with basic controls for observable individual characteristics; Column C further adds the occupational skill levels as well as the occupational employment share controls; Column D adds in state-occupation and time fixed effects; Column E is the same specification as column D but weighted using the weights acquired from the CEM and PSM strategy. From this point forward, and Column E's results are referred to as our preferred results.

5.1 Switching Out

Table 3 shows the results from estimating equation 2. Recall that the dependent variable here is an indicator for whether an individual switched out of his or her current occupation in the next period. The results across all five specifications show that individuals who are currently licensed are much less likely to switch out of an occupation than those who are are not licensed. The regression sample includes individuals who are working in both month 4 and month 8. Our baseline analysis here focuses on annual switching results, but we also show monthly results in the robustness section. The results show that the estimates of β_1 are biased away from zero when we do not include any individual or occupational controls other than licensing status. Without any controls, the estimate implies that compared with workers who are not licensed, being licensed is associated with workers about 15.8 percentage points less likely to switch out of their occupation. Gradually adding in controls, we see this effect of licensing on the occupational switching rate decline. With the full set of controls, this effect reduces to a 8.9 percentage point difference. However, even with the full set of controls, the distributional balance of measured covariates between the treated and non-treated group is not guaranteed. Therefore, in the last column, we consider only the controlled workers who are matched to treated workers, resulting in a much smaller sample size. We find that the licensed workers are 9.7 percentage points less likely to switch occupations than other non-licensed workers who are otherwise very similar in all controlled observables. This represents a 23.63% reduction in the average occupational exit rate (the average switching rate within the sample is 41.6%). Note that all of these estimates are highly statistically significant. The negative coefficient on the licensing indicator suggests that there is something inherent about being licensed outside of their occupations' skill requirements that reduces worker mobility out of those occupations. The direction of the bias suggests that workers who self-select into licensed jobs tend to already be less mobile than the average worker. Looking at the rest of the coefficients, we find that non-white workers and workers with no college degree tend to switch out of occupations with a higher probability. Being married, older, in a union, and having a higher income decreases the probability of switching out. Females tend to switch less than males in general, though this difference becomes statistically insignificant once we control for sample matching balance. This finding suggests that gender has no direct correlation with occupational switching rates. Occupation characteristics play a large role in predicting occupation switching rates. Working in a job that requires a high level of interpersonal skills dramatically reduces the probability of switching to another occupation, whereas working in a job that requires high levels of cognitive skill increases that probability.

Manual skill, however, does not have a significant relationship with occupational mobility. Lastly, the higher an occupation's total employment share, the less likely a worker is going to switch out of that occupation.

5.2 Switching In

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 as well as 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 effect of licensing occupation entry rates, we estimate equation 3 and show the results in Table 4. As in the switching-out case, 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. Recall that our sample selection is such that we are looking 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. Similar to the previous switching out analysis, we look first at the effects of licensing without taking into account any observable controls (column A). The estimate implies a worker in a licensed occupation is 17.3 percentage points less likely than those in unlicensed occupations to have switched into that job from another occupation. The direction of bias that arises from ignoring other effects is the same as in the switching out analysis for similar reasons: those who self-select into licensed jobs have overall lower occupational mobility, which biases the estimate of β_1 downward and away from zero. We then gradually and column-by-column add relevant observable controls into our regression and finally add the PSM weights in column E. Our baseline analysis shows that licensed workers are 9.6 percentage points less likely than workers who are not licensed to have just switched into their current occupation. This represents a 24.08% of reduction relative to the average occupational entry rate. As in the switching-out specification, we

find that individuals who are older, in unions, or married are less likely to have recently switched into their current job. Having a higher income in this period is correlated with a lower probability of having switched in the last period, since higher incomes are probably associated with a tighter labor market. The cognitive skill requirements of the current job are significantly positively related to the switching-in rates while the interpersonal skill requirement shows the opposite relationship. One possible explanation is that job markets that rely on relationships are relatively more rigid, while job markets that rely more on merit or intelligence are more fluid. Another possible explanation is that cognitive skills are more general and transferable across firms and occupations, while interpersonal skills are more often embodied in the specific culture of a firm or occupation and so represent less transferable, firm-specific skills. Lastly, workers in occupations with a higher employment share are less likely to have recently been switched into.

5.3 Switching In and Out of Unemployment

We have established that occupational licensing has significant effects for workers switching between occupations for employed individuals. In this section, we investigate how licensing affects labor market transitions in and out of unemployment. Table 5 and Table 6 show the results for those switching into or out of unemployment. The two tables replicate the first two exercises, with the exceptions that we now include workers who are unemployed preceding or following the employment observation, and the dependent variables are indicators of switching from any occupation into unemployment (J2N) and switching from unemployment into any occupation (N2J). In our analysis, we include only unemployed workers and discard workers who are not in the labor force, since non-participation could be due to many reasons that are not labor market-related.

Table 5 shows how licensing affects the probability of switching out of employment into unemployment. As before, the coefficient on the licensing indicator is negative and significant, though it is smaller in magnitude. We find, after including our controls, that being in a licensed occupation is associated with a 0.5 percentage point decrease in the probability of subsequently switching into unemployment. Similarly, in Table 6, we see a decrease of 0.5 percentage points in the probability of having switched in the last period from unemployment into the current occupation if the current occupation requires an license. These results, as in the first two specifications, support the hypothesis that licensing creates barriers to entry, incentives to stay in that occupation, and potential buffers against unemployment through

potential monopoly rents and incumbent advantages. Similar to the probability of job-to-job switching, we find that older workers are less likely to switch into or out of unemployment, as are married individuals. Cognitive skills are negatively correlated with the probability of moving to unemployment and moving in from unemployment. The intuition is straightforward: jobs that require a high level of cognitive skill embody difficult-to-replace human capital, and so workers may be less likely to be laid off. Also, as shown in the previous analysis, cognitive skills are more general, and so workers who are laid off from cognitive jobs may be more able to find new jobs instead of falling into unemployment. It is also less likely for a worker to switch directly from unemployment into a highly cognitively demanding job. Interpersonal skill is strongly positively correlated with the probability of switching in from unemployment, while manual skills do not have significant effects on the probabilities of workers switching into or coming out of unemployment.

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? Table 7 shows the results from the regression in equation 4. Recall that in equation 4, we restrict our sample to workers who are working today, and either not working ($Y_{it-1} = 0$) or working at a different occupation ($Y_{it-1} = 1$) in the previous period. We find that the coefficient on the licensing indicator is negative and significant, implying that workers who have switched into a licensed occupation are 4.4 percentage points 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 non-licensed jobs is lower than it is for the employed. We also find that of switchers, coming from a different occupation (rather than from unemployment) is less likely for non-white workers and more likely for married workers. It is also more likely that those entering jobs requiring high cognitive skills and low interpersonal skills switch from other occupations rather than from unemployment. The coefficient on licensing status further confirms our hypothesis that licensing is a barrier to entry both financially and in terms of time costs.

5.4 Licensing and Wage Growth

In this section, we investigate the effect of licensing on wage changes 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 non-licensed workers. 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 are measuring the change in wages associated with labor market transitions at an annual level. All results in this section use the matched/corrected sample discussed earlier; that is, we apply the CEM matching and weight all regressions by the PSM weights in order to estimate the treatment effect of licensing in a balanced matched sample of treated and non-treated groups. Table 8 shows the results for our wage growth analysis using estimating equations 5 and 6. The first column shows the results for workers staying in their occupation between two consecutive years. After matching workers who are similar in observable characteristics and job skill requirements, we find that on average, workers who are licensed have a 6.3 percentage point higher log wage growth than workers who are not licensed. This represents about a $2,208 wage increase.[19] This result is in line with the licensing literature, which suggests that occupational licensing attainment has positive effects on wages. We also see that older workers and workers who are employed at jobs with higher skill requirements experience higher wage growth rates, whereas on average, females, non-whites, and workers without college degrees have lower wage growth rates. Finally we find that, on average, higher current wages are associated with lower wage growth rates.

The second column in table 8 shows the results for workers who switch out of their current occupation into a new occupation. With the full set of controls as well as applying propensity score matching, 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 gain 3.8 percentage log points in wages, or $1,251 annually on average. 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 tend to have higher unobserved ability and therefore higher

[19] As mentioned earlier, all dollar amounts in the paper are in 2000 real dollars. The mean annual income of this stayer sample is $35,046. Workers who are making the average income and staying in a licensed occupation see a $2,208 extra annual wage gain compared with workers who stay in a non-licensed occupation (6.3%*35,046 = 2,208).

wage growth. In section 7, 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, we also find 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. Switching out of high cognitive and interpersonal skill occupations increases the relative gains from switching out, while coming from a manual skill-intensive job doesn't make a significant difference. However, all skills are positively correlated with switching wage gain, with cognitive skills having the strongest correlation.

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 8 shows that switching into a licensed occupation is associated with higher wage gains relative to an observationally similar worker switching into an occupation with no licensing requirements. Specifically, relative to the gain from switching into an unlicensed job, switching into a licensed occupation is associated with a 5.4 percentage point boost in log wage growth, or $1,834 more annually. This is consistent with the licensing literature: licensing has a positive effect on wages. This finding also supports papers that estimate the switching costs associated with occupation mobility (Xu (2019) 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 before female, non-college-educated, and non-white workers gain less when switching into an occupation and that older, married, and union workers gain more. Having high skill levels always has a positive impact on worker wage growth. This is true for both the source occupation and the destination occupation, though the effect of the skill requirements for the destination occupation is much stronger. As before, cognitive skill has the strongest impact on wage growth.

Finally, in the fourth column, we look at the effect of entering a licensed occupation from unemployment. The sample here is all individuals who switch from unemployment to employment between $t-1$ and t. Since the switch is from unemployment, the wage gain is not so much from the change in wages, as in the previous three exercises, but rather the entrance wage received in the new job. The wage variable used in these estimates is the log weekly wage. Column four shows that there is a strong and significant effect of licensing:in log wages, workers who switch from unemployment to licensed jobs gain 0.114 points (starting wages) more than those switching into non-licensed occupations, controlling for individual and job characteristics. This is consistent with the findings in the literature (For example,

Kleiner and Krueger (2013)). We see that older workers, union workers and married workers tend to have higher entry wages from unemployment, whereas female, non-college workers, and non-white workers tend on average to have lower entry wages. The higher the job's skill requirements, the higher the average entry wage. Among the skill requirements, cognitive skills are the most important in terms of the starting wage coming from unemployment.

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. 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.

6 Occupational Heterogeneity in Licensing Effects

In this section, we examine how the effect of licensing differs across occupations. In the previous section we see 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 12 to 15 provide some evidence on heterogeneity in occupational mobility. Using figure 13 as an example, the gray bars show the monthly switching-out rate for 25 different universally licensed occupations, while the blue (leftmost) 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 workers see monthly switching-out rates of almost 8%. Similarly, we see great heterogeneity in the rate of switching out to unemployment (15), switching in from other occupations (12), and switching in from unemployment (14). 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 occu-

pations. We answer this question using an strategy similar to our baseline analysis. Here, we focus on one occupation at a time instead of pooling all occupations together, similar to Han and Kleiner (2016) and Johnson and Kleiner (2017). However, our strategy deviates from theirs in that we do not use the universally licensed occupations as an indicator for licensing. Instead, we use the individual-level licensing attainment that is the same as in our baseline analysis. We then restrict our sample to one (coarsely defined) occupation[20] group at a time. Table 11 shows the results for this analysis.

As shown in Table 11, the results for "managerial and professional" occupations are very 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 qualitative similarities 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, licensed workers who stay in the same occupation between two years see 3 percentage points in additional wage gains compared with workers who are not licensed. However, for Operators, Fabricators and Laborers, being licensed leads to an additional 8 percentage points in annual wage gains.

7 Robustness Checks

One potential concern about the results in our baseline analysis is that they may be subject to selection bias. Workers who are licensed are possibly more/less skilled and therefore have higher/lower wage growth and are more/less likely to switch occupations. We address this potential problem with three different exercises. First, in section 7.1, we follow the strategy introduced by Altonji, Elder and Taber (2005) and Oster (2017) and attempt to place on our estimates bounds that account for selection on unobservables. Second, in section 7.2, we use an instrumental variable strategy to correct for potential bias in our estimation. Third, in section 7.3 we attempt to directly tackle the selection problem by following the two step

[20]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 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%.

selection correction strategy introduced in Heckman (1979). Furthermore, to make sure our results are not specific to our particular sample or data, we repeat our primary analysis in section 7.4 using different samples from the CPS as well as a totally different data set, the SIPP.

7.1 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, several recent papers (Altonji, Elder and Taber (2005), Oster (2017)) 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 (2017)), 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 such 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 where 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 12 to 14 show the results of this analysis. The first four columns of Table 12 show the estimated treatment effect of licensing on the switching-out rate with the gradual

inclusion of various sets of observed controls. The estimated treatment effect with no controls (-0.104) is slightly higher in absolute value than the estimate with all the controls (-0.097), implying that the uncontrolled regression was (slightly) biased away from zero. The inclusion of all the controls substantially increases the R^2 from 0.02 to 0.09. Columns five through eight show the same analysis for the switching-in rate, with similar results: the parameter estimate becomes slightly smaller in absolute value, whereas the R^2 increases significantly.

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 (2017), who suggests an R_{max} of 1.3 times the R^2 from the fully controlled regression. We also include estimates using several intermediate values of R_{max} (0.5 and 0.7). 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 12, 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 $\delta = 1$ such 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 occupation in the next period is -0.094. If we keep the assumption on the importance of unobservables but increase $R_{max} = 0.5$, the lower bound of the licensing effect becomes much smaller in absolute terms at -0.039. When R_{max} increases to 0.7, the effect of licensing becomes -0.001, which is not too different from zero. This is reassuring that the licensing effect on workers' occupation switching fades away only when we assume the most stringent conditions – the unobservables are at least as important as the observables, and together they can explain 70% of data variation. Under reasonable assumptions where unobservables are typically not more important than the observables ($\delta \leq 1$)[21], the negative impact of licensing on workers' switching remains. Similarly, we see that licensing has strong and significant barrier effects on worker entry even under the strictest assumptions about unobservables, though the effect becomes relatively small at -0.006 under this extremely strict assumption. We can also interpret these same results from a different angle. For example, suppose we assume that we can explain 50% of the data variation in worker occupation

[21] 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)).

switching rates. Under this assumption, the licensing effect on workers who switch in or out of their current occupation will disappear only when the unobservables are at least 0.5 times more important than the observables ($\delta = 1.5$ in both switching out and switching in analysis).

Tables 13 and 14 show the same robustness exercise on wage changes for workers who stay and switch occupations, respectively. First, Table 13 focuses on the occupational stayers. As opposed to the results presented in the switching analysis (Table 12), 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 $R_{max} = 0.7$, the upper bound for the effect of licensing on wage changes from staying in an occupation is 0.077, which is significantly larger than the original estimate of 0.063. This general result is true across all of the assumptions on R_{max} and δ in Table 13. This implies that the wage growth results for stayers represent a lower bound on the treatment effect of licensing. If we assume that by combining the observables and unobservables we can explain 70% of the 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 7 times more important than the observables ($\delta = -6.9$) to make the wage effect of licensing become zero. We can interpret the results in Table 14 in a similar fashion. On the left panel we focus on the wage growth effect of licensing for workers who have switched out of their occupations. We see that omitting observables biased the estimates away from zero, since including more observables makes the estimates of licensing effects smaller. Under the assumption that the unobservables are as important as the observables in explaining switcher wage growth and the assumption that together they explain 70% of the total wage growth variation, the lower bound of the licensing effect on workers wage growth is 0.019. On the right panel of Table 14, we present the results for the licensing effect on wage growth when workers switch into their occupation. Similar to the results in Table 13, we see that including more observable controls in fact strengthened our estimates of the licensing effect. This implies that omitting unobservables has biased our estimates toward zero. When they are taken into consideration, and when (for example) unobservables are of the same importance as the observables and $R_{max} = 0.7$, the upper bound of the licensing effect on worker wage growth is 0.068. Our baseline estimation (0.054) 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.

7.2 Instrumental Variable Approach

In previous subsection, we evaluated the impact of omitted variables on estimated licensing effects by putting bounds over the estimated coefficients under various assumptions. In this section, we use an instrumental variable strategy to correct for the potential bias in our estimated licensing effects.

We begin by rewriting our baseline model as follows:

$$y_{it} = \beta_0 + \beta_1 L_{it} + X_{it}\Gamma + \varepsilon_{it}, \tag{7}$$

where L_1 denotes the licensing indicator, and ε is potentially correlated with the licensing status and therefore may bias our estimates of β_1. y_{it} denotes the key variable of interest, such as log wage growth or the individual switching indicator. X_{it} includes the following observables and fixed effects: age; gender; race; education; union status; marriage status; log income level; occupational fixed effects using detailed census 2010 codes; as well as state, year, and month fixed effects. We now introduce our instrumental variable, $\%L_{OS}$, which denotes the share of workers who are licensed in the worker's particular occupation and state group. The idea behind our instrument is that, using wage growth rates as an example, the share of licensed workers within each occupation and state group may affect the probability of workers' becoming licensed,[22] but it should not have a direct impact on individual-level wage growth except through that worker's licensing status. We estimate two new sets of results: the above model using OLS, and the same model using 2SLS, showing the results in table 15.[23] We see the coefficients and standard errors of the estimated licensing effects for five separate analyses. From column one to column five, we investigate the licensing effects on log wage growth for workers who stay in the occupation; log wage growth for workers

[22]The variation in the share of licensing attainment within an occupation across state groups may represent cross-state heterogeneity in difficulty level when acquiring a license, or it may show heterogeneous average levels of worker preferences over licensing across different occupations and states, all of which will likely affect each individual's licensing decision.

[23]We cannot use our baseline results from the previous section as a valid comparison to the 2SLS results, since our baseline analysis uses a CEM and PSM matched restricted sample, while here we are using all individuals in our sample. The disadvantage in using all individuals is that we do not achieve similarities in the covariates over observables between the treated and non-treated groups. On the other hand, the advantage in using all individual data is that we have a much larger sample size, which allows us to include detailed occupation and state fixed effects as opposed to including only a coarse occupation groups fixed effect, as in our baseline analysis.

who switch out in the next period; log wage growth for workers who have just switched in this period (from a different occupation); an indicator for switching occupations next period; and an indicator for having switched occupations from the previous period. We show our OLS results in row one and 2SLS results in row two. The F-statistics in stage one is shown in row three which suggest that we can reject that the coefficients on the instruments in the first stage regression are equal to zero. Stock-Yogo weak IV tests critical values also shows no evidence that our instrument is weak (Stock and Yogo (2002)).

In column one, we see that using our instrumental variable increases the effect of licensing on occupational stayers' wage growth. This suggests that the unobservables have biased our estimates toward zero and correcting it will strengthen estimated licensing effects on wages. This result is consistent with our previous results in Table 13, which show that after taking into account the omitted unobservables in the analysis, the effect of licensing on wages becomes stronger. Column two shows the wage growth effect of licensing on workers who switch out of their current occupation the following year. The OLS result shows a positive and significant licensing coefficient (0.023) that is slightly less than our baseline estimate. The 2SLS result is larger, but not statistically significant. This result is consistent also with our results in the previous subsection (Table 14, left panel)– omitted variables bias our results away from zero. Hence, if the unobservable variables contribute more to the variation in switcher wage growth, the effect of licensing may go toward zero. Column three shows the licensing effect on wage changes for workers who have just switched into their current occupation. We see that the licensing effect on wage growth increases from 0.047 to 0.054 after including the instrumental variable and that the estimates are statistically significant at a confidence level of 95%. This increase in the estimated licensing effect implies that any omitted variable bias here was biasing our estimates toward zero, which is again consistent with the results shown in Table 14, where our baseline estimates represent a lower bound on licensing effects on wage growth.

In columns four and five, we then turn to investigate how licensing affects worker-level occupational switching decisions. In both columns we see negative and statistically significant coefficients on licensing and the licensing effect's becoming smaller (in absolute value) once the instrument is included. This result suggests that the omitted variables bias our estimates of worker switching probabilities away from zero and including them would make the licensing effect smaller – which is consistent with the results in Table 12. The magnitude in this analysis is much smaller compared with the ones in Table 12, which could be due to sample selection differences, since our baseline results are restricted to PSM matched

workers. However, the results in this section confirm our baseline results that licensing has significant negative effects on workers' occupational switching decisions, both when switching in and switching out. Licensing also shows strong wage effect on workers' wage growth when they stay in a licensed occupation or switch into a licensed occupation.

Overall, the results from our IV strategy are consistent with the results from our parameter-bound strategy in the previous section. Our 2SLS results support the conclusion from our baseline estimates that licensing has statistically and economically significant negative effects on worker mobility and positive effects on wage growth.

7.3 Heckman Selection Correction

In this section, we attempt to address the selection issue following the two step correction procedure introduced in Heckman (1979). Consider our baseline model but simplify it to $Y = (1 - D)Y_0^* + DY_1^*$, where

$$\begin{aligned} Y_0^* =& X\beta_0 + \varepsilon_0 \\ Y_1^* =& X\beta_1 + \varepsilon_1, \end{aligned}$$

and

$$D = 1\{Z\gamma - u > 0\}.$$

Y is the variable of interest, such as log wage growth. D is an indicator of whether a worker is licensed. Y_1^* denotes the variable of interest for licensed individuals, and Y_0^* denotes the same variable for non-licensed individuals. We are interested in the average treatment effect: $ATE(X) = X(\beta_1 - \beta_0)$. The concern is that the error terms ε_0, ε_1, and u are probably not independently distributed, so estimates of β will be biased. For example, if workers who are non-licensed have higher (lower) values of ε_0 relative to licensed worker values of ε_1, then the OLS estimator for $\beta_1 - \beta_0$ will underestimate (overestimate) the true licensing effect. The direction of the bias is not clear ex ante, since it is possible that workers with high unobservable ability (for example) will join licensed occupations, but at the same time, it is possible that workers with very high ability may choose not to join the licensing queue.[24] We follow the two step procedure proposed by Heckman. In the first stage, we use a probit model to predict the probability of $D = 1\{Z\gamma - u > 0\}$. Z includes the same variables as X. We then calculate and include the inverse mills ratio $\hat{\lambda} = \phi(Z\hat{\gamma})/\Phi(Z\hat{\gamma})$ in the second

[24] This is similar to Robinson (1989)'s argument on union member wage premium estimation.

stage regression.[25]

Using this two step procedure, we first check the effect of licensing on log wage growth. For workers who stay in the same occupation, the selection-corrected estimate for the treatment effect of licensing is 0.087. For workers who switch out of their current occupation the following period, the treatment effect of licensing is 0.078. These estimates are a little larger than our baseline estimates, which suggests that endogenous selection in our baseline sample and regression likely biased our result towards zero. However, since in our baseline analysis we are restricted to only propensity score-matched observations, the results may not be directly comparable. Lastly, we look at the licensing effect on occupational mobility. The average treatment effect for licensing is -0.144, which is again much larger than our baseline analysis in Table 3. The results using two step Heckman correction strategy are all in line with our baseline results (at least qualitatively), which validates the strong and significant licensing effects we find in the previous section.

7.4 Different Data and Sample Selection Criteria

In this section, we investigate the same questions as in the baseline analysis, but we use different data samples and sources. Recall that our baseline results look at the effect of licensing on annual labor market transitions, using low-frequency data from the CPS (tables 3 to 7). 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 differently from our baseline analysis. 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 modified the worker licensing status using worker labor market data. In this section, we employ the second imputation procedure by keeping workers in months 1 and 5 while imputing the wage data. The results are shown in Table 9. 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

[25]Since we do not have any exclusion restrictions in the first stage, identification is generally achieved through functional form assumptions - in particular, the nonlinearity of the probit model relative to the second stage regression.

[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 not available month to month.

are significant and negative as 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 our second imputation method giving us very similar results to our baseline method (bottom panel of Table 9). This strengthens our conclusions about how occupational licensing affects labor market transitions, especially because in month 1 and month 5, the licensing indicators are the most credible. The results here further validate our imputation methods and baseline results.

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. After data cleaning,[27] we have 82,000 observations left in the sample. Out of these observations, only 22,000 have a valid value for licensing status. This is because the licensing-related questions are provided only in the topical module in wave 13. When matching these workers from the topical module to the core data, we have only their licensing status in this one interview month. Furthermore, many workers who are interviewed in the core questions in wave 13 are not in the topical module and therefore do not have values for licensing status. Some of the 22,000 workers who have valid licensing statuses do not have values for labor market transitions. Dropping these workers brings the sample of workers who have both a valid switching status as well as a licensing indicator down to about 19,500. We see that roughly 4.6% of these workers switch occupations in four months, which is about 900 observations for switchers. Applying matching techniques results in a loss of additional observations. So, for example, when we are looking at wage changes when switching into an occupation that requires licensing, the sample size drops to just over 200 (row 8 in Table 10). This makes some of our results statistically insignificant. However, regardless of the small sample size and using different data with different time horizons, our results from this analysis are in line with our baseline results using the CPS. Licensing reduces worker mobility going in and out of licensed occupations from both other occupations and non-employment.

[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. Finally, we exclude 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.

Relative to getting hired from non-employment, workers are less likely to get hired from other occupations 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.

Finally, we separately attempt to overcome the small sample issue by performing some analysis using more than 20 years of data from the SIPP. Here, 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 all panels of the SIPP starting from 1990 to 2013. 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.

8 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. Both the number of licensed occupations and the magnitude of the licensing requirements have 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 24% less likely to switch occupations and 3% less likely to become unemployed in the following year. Furthermore, licensed workers have wage growth rates that are 6.3 percentage points (pp) higher on average than other workers who stay in the same occupation next year and wage growth rates that are 3.8pp higher than other workers who switch occupations. Compared with occupations without licensing requirements, occupational licensing represents a barrier to entry for both unemployed workers (1.2% lower entrance rate) and workers who enter from other occupations (24.1% lower entrance rate). Occupational licensing represents a

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

larger barrier to entry if a worker is switching in from other occupations relative to switchers from non-employment. The barrier effect of licensing on labor flows may partly explain the decline in occupational mobility over recent decades (Xu (2019)). These results raise further questions about the effects of licensing: Does licensing affect the quality of goods and services, and by what mechanism? 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? We are currently pursuing answers to these questions by using a structural model that allows us 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

Note: Data source: CPS IPUMS.

Table 2: Changes in Occupational Licensing Requirements

Occupation	Education (yrs)		Initial Cost		Renewal Cost	
	'95	'13	'95	%Δ ('13)	'95	%Δ ('13)
Engineer	3.7	4.0	$124	55%	$46	101%
Land Surveyor	1.6	4.1	$82	42%	$86	24%
Psychologist	5.8	6.0	$263	33%	$169	56%
Nurse	2.0	2.0	$36	124%	$26	142%
Teacher	2.3	3.7	$19	177%	$16	188%
Veterinarian	6.0	6.0	$23	512%	$23	468%
Total (Mean)	3.22	4.9	$101	116%	$83	106%

Note: Years of Education: 2 is High School, 4 is Assoc., 6 is Bachelor's, 8 is Post-Grad.

Table 3: Occupational Switching (Job to Job, out)

	Switching Out (J2J)				
	A	B	C	D	E
Licensed (α_1)	-0.158***	-0.143***	-0.112***	-0.089***	-0.097***
Age		-0.002***	-0.002***	-0.002***	-0.001***
Female		-0.013***	-0.009**	-0.008**	-0.002
Non-White		0.007*	0.009**	0.012***	0.033***
No college		0.011***	0.030***	0.004	0.019***
Union Coverage		-0.070***	-0.045***	-0.032***	-0.024***
Married		-0.014***	-0.016***	-0.017***	-0.018***
Weekly log income		-0.004***	-0.024***	-0.028***	-0.016***
Cognitive			0.452***	0.322***	0.195***
Manual			-0.199***	-0.011	-0.031
Interpersonal			-0.281***	-0.208***	-0.263***
Occupation EmpShare			-4.404***	-5.667***	-5.592***
$\mathbb{E}[Y]$					0.416
% Effect of α_1					-23.63%
Fixed Effects	No	No	No	Yes	Yes
CEM & PSM Weighted	No	No	No	No	Yes
Observations	130,290	130,106	129,790	129,773	66,502

Note: Data Source: CPS IPUMS. Seventeen coarsely defined occupation groups are used in the occupation-state fixed effect: 1. Executive, Administrative, and Managerial Occupations; 2. Management Related Occupations; 3. Professional Specialty Occupations; 4. Technicians and Related Support Occupations; 5. Sales Occupations; 6. Administrative Support Occupations; 7. Housekeeping and Cleaning Occupations; 8. Protective Servce Occupations; 9. Other Service Occupations; 10. Farm Operators and Managers; 11. Other Agricultural and Related Occupations; 12. Mechanics and Repairers; 13. Construction Trades; 14. Extractive Occupations; 15. Precision Production Occupations; 16. Machine Operators, Assemblers, and Inspectors; 17. Transportation and Material Moving Occupations. The coefficients represent the effects on annually occupational switching rate. The occupation classification used in transition rate is from IPUMS harmonized 2010 census code (440 categories). Fixed effects include occupation, year, month, and occupation state interaction.

Table 4: Occupational Switching (Job to Job, in)

	Switching In (J2J)				
	A	B	C	D	E
Licensed (α_1)	-0.173***	-0.160***	-0.128***	-0.105***	-0.096***
Age		-0.002***	-0.002***	-0.002***	-0.001***
Female		-0.012***	-0.005	-0.009***	-0.019***
Non-White		0.004	0.006	0.011**	0.010*
No college		0.003	0.016***	0.002	0.006
Union Coverage		-0.065***	-0.044***	-0.029***	-0.015***
Married		-0.013***	-0.015***	-0.017***	-0.015***
Weekly log income		-0.006***	-0.022**	-0.027***	-0.026***
Cognitive			0.441***	0.326***	0.255***
Manual			-0.184***	-0.063****	-0.049
Interpersonal			-0.304***	-0.264***	-0.350***
Occupation EmpShare			-3.612***	-4.677***	-4.591***
$\mathbb{E}[Y]$					0.399
% Effect of α_1					-24.08%
Fixed Effects	No	No	No	Yes	Yes
CEM & PSM Weighted	No	No	No	No	Yes
Observations	130,305	130,110	129,674	129,658	63,437

Note: Data Source: CPS IPUMS. Seventeen coarsely defined occupation groups are used in the occupation-state fixed effects. See table 3 for details. The coefficients represent the effects on annually occupational switching rate. The occupation classification used in transition rate is from IPUMS harmonized 2010 census code (440 categories). Fixed effects include year, month, and occupation state interaction fixed effects.

Table 5: Switching out to Unemployment

	Switching Out (J2N)				
	A	B	C	D	E
Licensed (α_1)	-0.027***	-0.008***	-0.005***	-0.004***	-0.005**
Age		-0.003***	-0.002***	-0.003***	-0.001***
Female		-0.005***	-0.007**	-0.010***	-0.002
Non-White		0.022***	0.020***	0.021***	0.023***
No college		0.003	0.002	0.003	0.006*
Married		-0.078***	-0.075***	-0.072***	-0.037***
Weekly log income		-0.029***	-0.030***	-0.031***	-0.019***
Cognitive			-0.060***	-0.062***	-0.022**
Manual			-0.001	-0.000	-0.011
Interpersonal			0.021**	0.023**	0.013
Occupation EmpShare			0.141	0.148	0.218
$\mathbb{E}[Y]$					0.184
% Effect of α_1					-2.98%
Fixed Effects	No	No	No	Yes	Yes
CEM & PSM Weighted	No	No	No	No	Yes
Observations	252,228	251,813	251,532	251,217	74,928

Note: Data Source: CPS IPUMS. Seventeen coarsely defined occupation groups are used in the occupation-state fixed effects. See table 3 for details.The coefficients represent the effects on annually occupational switching rate. The occupation classification used in transition rate is from IPUMS harmonized 2010 census code (440 categories). Fixed effects include year, month, and occupation state interaction fixed effects.

Table 6: Switching In from Unemployment

	Switching In (N2J)				
	A	B	C	D	E
Licensed (α_1)	-0.024***	-0.003	-0.003	-0.001	-0.005**
Age		-0.006***	-0.006***	-0.006***	-0.005***
Female		0.005**	-0.001	-0.002	-0.024***
Non-White		0.025***	0.023***	0.025***	0.016***
No college		0.023	0.011***	0.002	0.045***
Married		-0.074***	-0.072***	-0.071***	-0.056***
Cognitive			-0.096***	-0.116***	-0.093***
Manual			-0.023**	-0.002	-0.002
Interpersonal			0.006	0.012*	0.084***
Occupation EmpShare			-0.102	0.084	0.832***
$\mathbb{E}[Y]$					0.455
% Effect of α_1					-1.150%
Fixed Effects	No	No	No	Yes	Yes
CEM & PSM Weighted	No	No	No	No	Yes
Observations	246,845	246,845	246,307	246,307	115,852

Note: Data Source: CPS IPUMS. Seventeen coarsely defined occupation groups are used in the occupation-state fixed effects. See Table 3 for details. The coefficients represent the effects on annually occupational switching rate. The occupation classification used in transition rate is from IPUMS harmonized 2010 census code (440 categories). Fixed effects include year, month, and occupation state interaction fixed effects.

Table 7: The Relative Effect of Licensing on the Unemployed vs. the Employed

	Switching In (J2J vs N2J)				
	A	B	C	D	E
Licensed (α_1)	-0.067***	-0.087***	-0.073***	-0.045***	-0.044**
Age		0.004***	0.004***	0.005***	0.005***
Female		0.004*	0.008**	0.008***	0.014
Non-White		-0.018***	-0.017***	-0.020***	-0.019***
No college		0.004*	0.016***	0.009	0.008**
Married		0.055***	0.053***	0.063***	0.061***
Weekly log income		0.037***	0.026***	0.029***	0.018***
Cognitive			0.254***	0.134***	0.126***
Manual			-0.082***	0.021*	-0.007
Interpersonal			-0.139***	-0.063***	-0.065***
Occupation EmpShare			-1.696***	-1.466***	-1.327
$\mathbb{E}[Y]$					0.319
% Effect of α_1					-13.96%
Fixed Effects	No	No	No	Yes	Yes
CEM & PSM Weighted	No	No	No	No	Yes
Observations	179,059	178,778	178,345	178,345	74,824

Note: Data Source: CPS IPUMS. Seventeen coarsely defined occupation groups are used in the occupation-state fixed effects. See Table 3 for details. The coefficients represent the effects on annually occupational switching rate. The occupation classification used in transition rate is from IPUMS harmonized 2010 census code (440 categories). Fixed effects include year, month, and occupation state interaction fixed effects.

Table 8: Wage Growth Effects of Licensing

DepVar: $\Delta \log w$	J2J, stay	J2J, out	J2J, in	N2J, in
Licensed (α_1)	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***
Non-white	-0.032***	-0.040***	-0.051***	-0.055**
No college	-0.127***	-0.121***	-0.153***	-0.259***
Union Coverage	0.041***	0.056***	0.073***	0.144***
Married	0.005**	0.032***	0.026**	0.030***
Weekly Income	-0.588***	-0.613***	-0.633***[1]	-
Cognitive-old	0.707***	0.149***	0.179***	-
Manual-old	0.135***	0.074	0.067*	-
Interpersonal-old	0.293***	0.083**	0.088**	-
Cognitive-new	-	0.781***	0.614***	1.263***
Manual-new	-	0.090***	0.068	0.133***
Interpersonal-new	-	0.335***	0.174***	0.480***
Occ EmpShare - old	-0.609	-1.851***	-0.143***	-
Occ EmpShare - new	-	-1.055**	0.283	-0.955***
Annual $\bar{w}$	\$35,046	\$32,910	\$33,971	-
Licensing Effect	\$2,208	\$1,251	\$1,834	-
Fixed Effects	Yes	Yes	Yes	Yes
Matched	Yes	Yes	Yes	Yes
Observations	42,369	27,449	21,989	52,641

[1] This represents the weekly income from the job before switching occupation.

Note: Data Source: CPS IPUMS. Seventeen coarsely defined occupation groups are used in the occupation-state fixed effects. See table 3 for details. The coefficients represent the effects on annually occupational switching rate. The occupation classification used in transition rate is from IPUMS harmonized 2010 census code (440 categories). Fixed effects include year, month, and occupation state interaction fixed effects.

Table 9: Baseline Analysis Using Alternative Months in CPS

	CPS Monthly Results		
	$\widehat{\alpha_1}$	$\mathbb{E}[Y]$	% Effect
Probability of Switching Out (J2J)	-0.017	0.041	-46.34%
	(.005)		
Probability of Switching In (J2J)	-0.018	0.039	-46.73%
	(.003)		
Probability of Switching Out to Unemployment (J2N)	-0.002	0.014	-32.25%
	(.001)		
Probability of Switching In from Unemployment (N2J)	-0.001	0.007	-20.73%
	(.000)		
Prob of Switching In from J vs N (N2J vs J2J)	-0.017	0.106	-16.04%
	(.007)		
	CPS Month 1 and 5 Annual Results		
Probability of Switching Out (J2J)	-0.099	0.435	-22.87%
	(.003)		
Probability of Switching In (J2J)	-0.105	0.419	-24.89%
	(.004)		
Probability of Switching Out to Unemployment (J2N)	-0.007	0.459	-1.56%
	(.002)		
Probability of Switching In from Unemployment (N2J)	-0.042	0.512	-8.29%
	(.002)		
Prob of Switching In from J vs N (N2J vs J2J)	-0.047	0.291	-16.01%
	(.003)		

Note: Data Source: CPS IPUMS. All results above include occupational fixed effects, year fixed effects, union fixed effects, occupation and state interaction fixed effects, CEM matching, and PSM weights.

Table 10: Percentage Effect of Licensing Using SIPP 2008

	SIPP Panel 2008		
	$\widehat{\alpha_1}$	$\mathbb{E}[Y]$	% Effect
Probability of Switching Out (J2J)	-0.011	0.036	-32.06%
	(.005)		
Probability of Switching In (J2J)	-0.011	0.043	-26.57%
	(.005)		
Probability of Switching Out to Unemployment (J2N)	-0.004	0.022	-18.48%
	(.004)		
Probability of Switching In from Unemployment (N2J)	-0.012	0.028	-44.96%
	(.004)		
Prob of Switching In from J vs N (N2J vs J2J)	-0.102	0.681	-14.93%
	(.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)		

Note: The data is from waves 12, 13, and 14 of SIPP panel 2008. The indicator of licensing is from the topical module of SIPP wave 13. All results above include occupational fixed effects, year fixed effects, CEM matching, and PSM weights. Wage in this analysis represents workers' monthly wages, and the switching rate is in four-month frequency.

Table 11: 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***

Note: All results above include all controls and PSM matching weights. 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 12: Control Sensitivity Treatment Effect Bounds – Switching Rate

	Switch Out Rate				Switch In Rate			
	1	2	3	4	5	6	7	8
Variable of Interest								
Licensed	-0.104	-0.102	-0.098	-0.097	-0.103	-0.098	-0.096	-0.096
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Controls								
Control 1		*	*	*		*	*	*
Control 2			*	*			*	*
Control 3				*				*
Bounds and Deltas								
$R_{max} = \tilde{R} \times 1.3$				$(-0.097, -0.094), \delta = 20.3$				$(-0.096, -0.094), \delta = 19.5$
$R_{max} = 0.5$				$(-0.097, -0.039), \delta = 1.5$				$(-0.096, -0.044), \delta = 1.5$
$R_{max} = 0.7$				$(-0.097, -0.001), \delta = 1.0$				$(-0.096, -0.006), \delta = 1.1$
R^2	0.02	0.02	0.06	0.09 ($\tilde{R}$)	0.02	0.03	0.05	0.10 ($\tilde{R}$)
Observations	66,542	66,542	66,542	66,542	63,482	63,482	63,482	63,482

Note: All results above include CEM matching and PSM weights. Control 1 include: age, gender, education, race, marriage status, union status, income level. Control 2 include: occupational skill levels, occupational employment share. Control 3 include: year effects, month effects, state and occupation effects. Column 4 of the left panel is the same as the first row of column E of Table 3. Column 4 of the right panel is the same as the first row of column E of Table 4.

Table 13: Control Sensitivity and Treatment Effect Bounds – Stayer Wage Growth

	Stayers ΔW			
	1	2	3	4
Variable of Interest				
Licensed	0.054	0.071	0.062	0.063
	(0.006)	(0.005)	(0.005)	(0.005)
Controls				
Control 1		*	*	*
Control 2			*	*
Control 3				*
Bounds and Deltas				
$R_{max} = \tilde{R} \times 1.3$				$(0.063, 0.067), \delta = -28.9$
$R_{max} = 0.5$				$(0.063, 0.070), \delta = -14.1$
$R_{max} = 0.7$				$(0.063, 0.077), \delta = -6.9$
R^2	0.002	0.25	0.28	0.31 ($\tilde{R}$)
Observations	42,414	42,414	42,414	42,414

Note: All results above include CEM matching and PSM weights. Control 1 include: age, gender, education, race, union status, marriage status, income level. Control 2 include: occupational skill levels, occupational employment share. Control 3 include: year effects, month effects, state and occupation effects. Column 4 of the table is the same as column of Table 8.

Table 14: Control Sensitivity and Treatment Effect Bounds – Switching Wage Change

	Switch Out ΔW				Switch In ΔW			
	1	2	3	4	5	6	7	8
Variable of Interest								
Licensed	0.052	0.054	0.052	0.038	0.043	0.047	0.046	0.054
	(0.008)	(0.007)	(0.007)	(0.007)	(0.009)	(0.008)	(0.008)	(0.008)
Controls								
Control 1		*	*	*		*	*	*
Control 2			*	*			*	*
Control 3				*				*
Bounds and Deltas								
$R_{max} = \tilde{R} \times 1.3$				$(0.034, 0.038), \delta = 7.6$				$(0.054, 0.057), \delta = -23.6$
$R_{max} = 0.5$				$(0.029, 0.038), \delta = 4.0$				$(0.054, 0.060), \delta = -615.0$
$R_{max} = 0.7$				$(0.019, 0.038), \delta = 1.9$				$(0.054, 0.068), \delta = -6.7$
R^2	0.002	0.236	0.246	0.317 ($\tilde{R}$)	0.001	0.262	0.272	0.338 ($\tilde{R}$)
Observations	27,586	27,586	27,586	27,586	22,082	22,082	22,082	22,082

Note: All results above include CEM matching and PSM weights. Control 1 include: age, gender, education, race, marriage status, union status, income level. Control 2 include: source occupational skill levels, source occupational employment share. Control 3 include: destination occupational skill levels and occupational employment share, year effects, month effects, state and occupation effects. Column 4 of the left panel is the same as column 2 (J2J, out) of Table 8. Column 4 of the right panel is the same as column 3 (J2J, in) of Table 8.

Table 15: OLS vs 2SLS

	Δw (stayer)	Δw (switch out)	Δw (switch in)	Switch out	Switch in
β_1 (OLS)	0.045	0.023	0.047	-0.034	-0.062
	(0.006)	(0.008)	(0.008)	(0.004)	(0.004)
β_1 (2SLS)	0.082	0.031	0.054	-0.023	-0.055
	(0.018)	(0.020)	(0.022)	(0.011)	(0.011)
(Cragg-Donald Wald Fstat)	7811.7	8348.1	7720.7	1.7e+04	1.6e+04

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

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

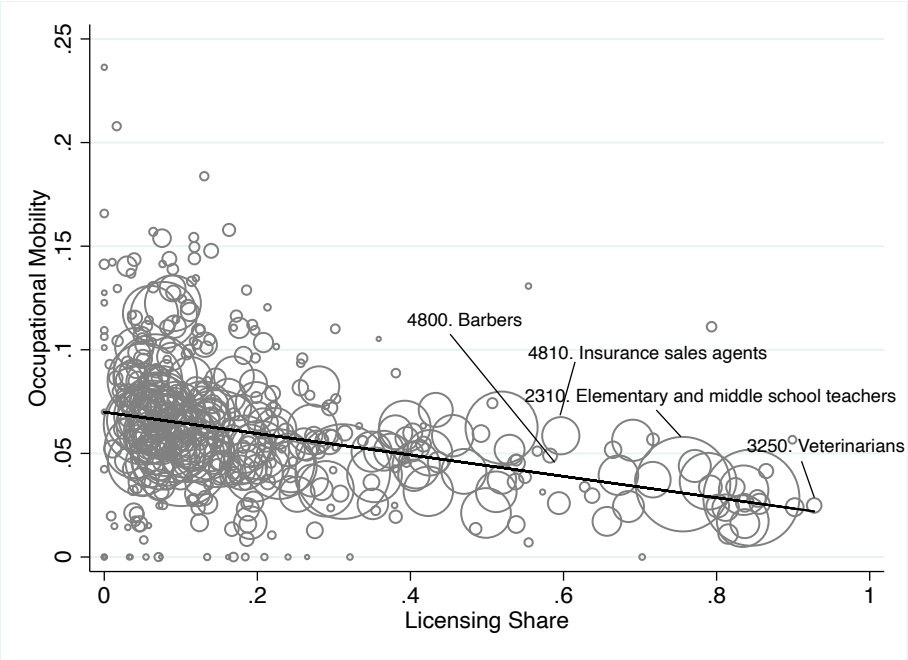


Figure 3: **Licensing Share vs. Switching In Rate**

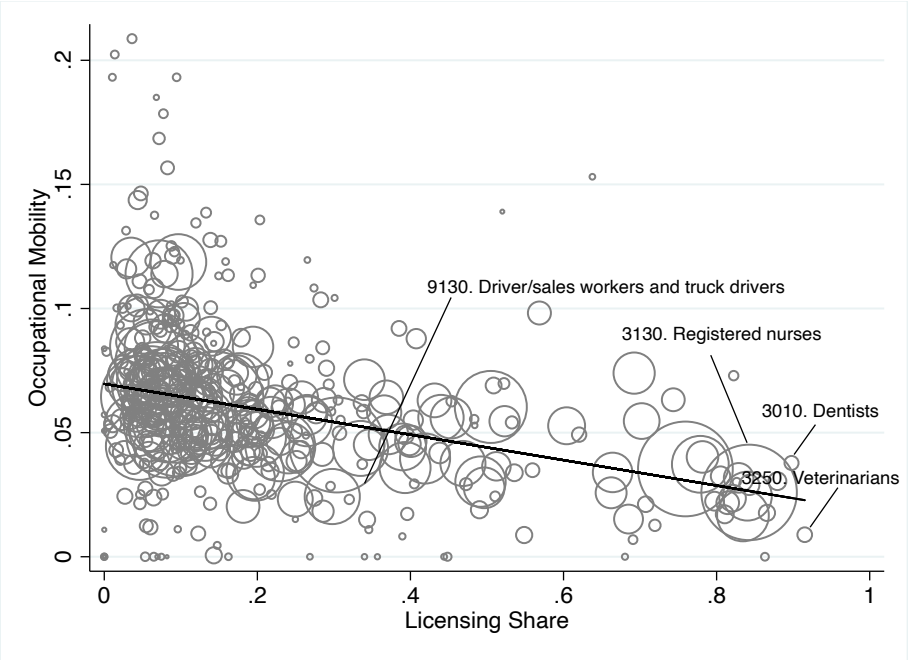


Figure 4: **Average Switching In Rate**

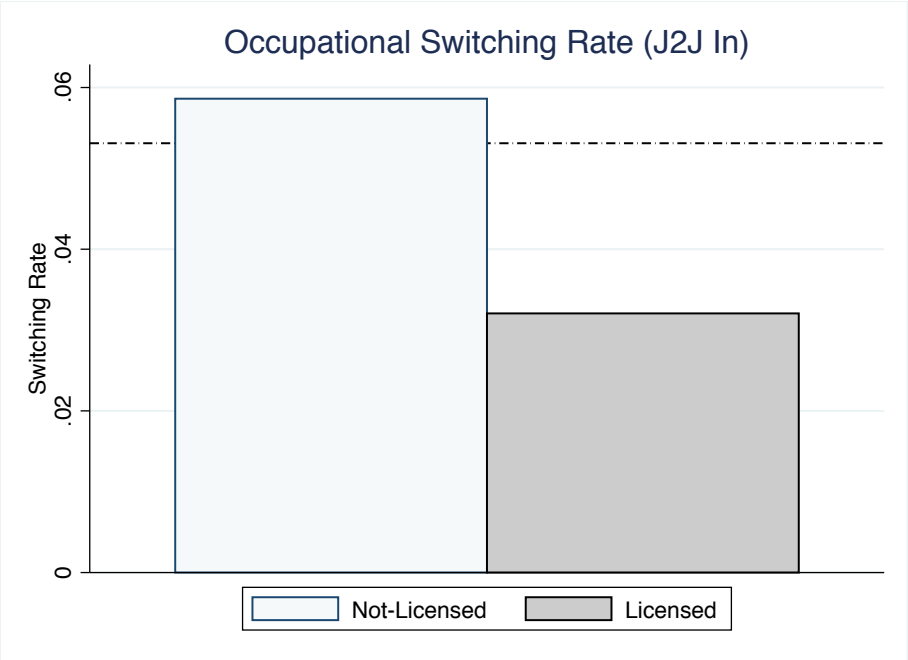


Figure 5: **Average Switching Out Rate**

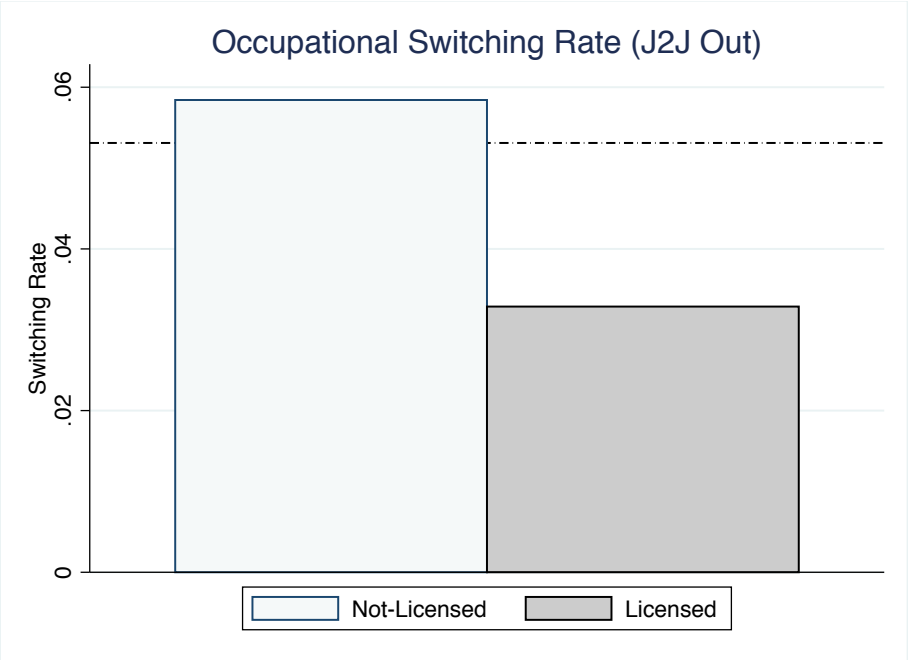


Figure 6: **Average Switching In Rate (From U)**

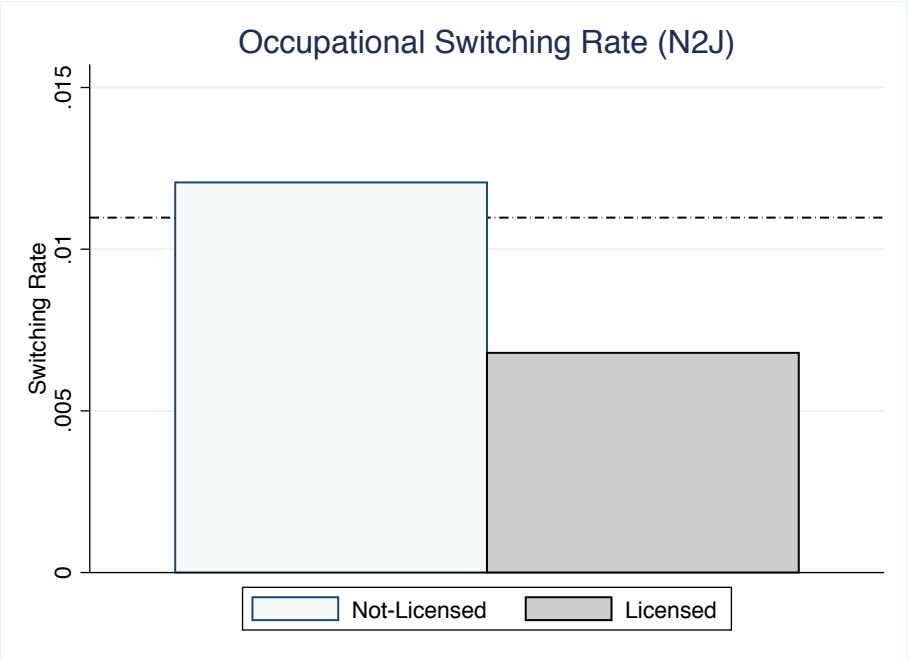


Figure 7: **Average Switching Out Rate (To U)**

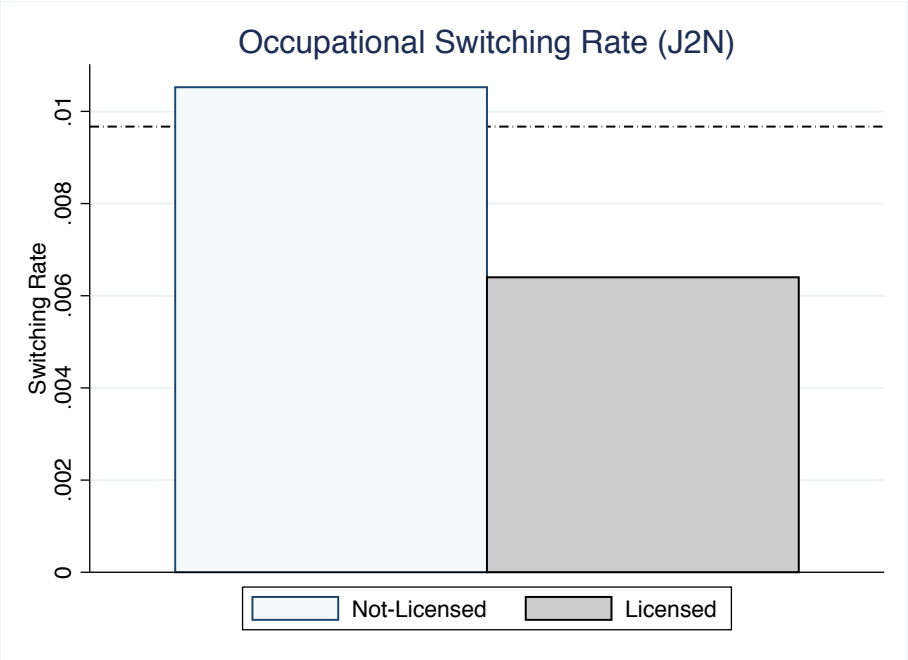


Figure 8: **Average Weekly Wages**

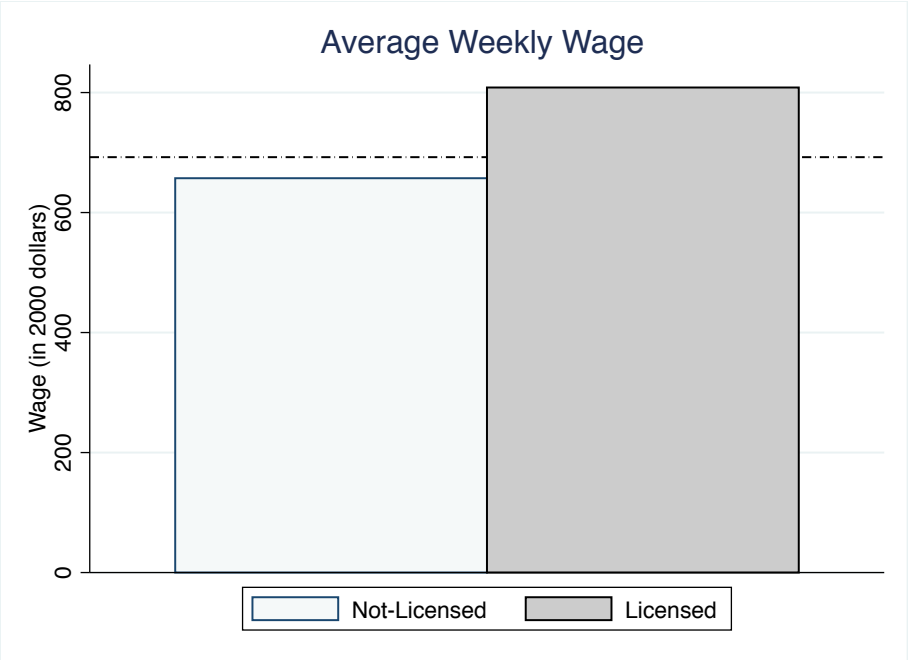


Figure 9: **Average Weekly Wage Differences by Age**

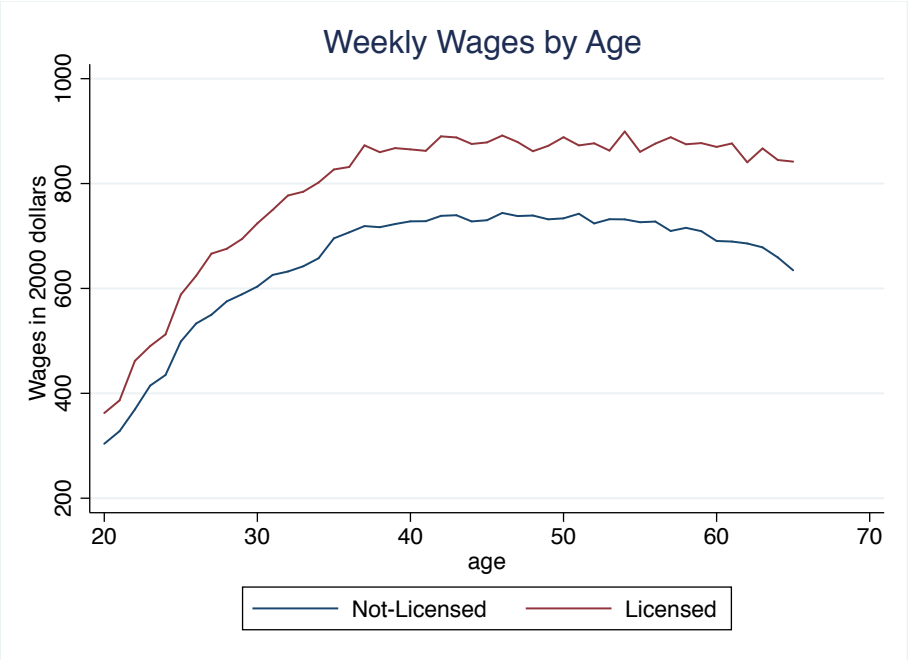


Figure 10: **Wage growth rate for stayers**

Figure 11: **Wage growth rate for switchers**

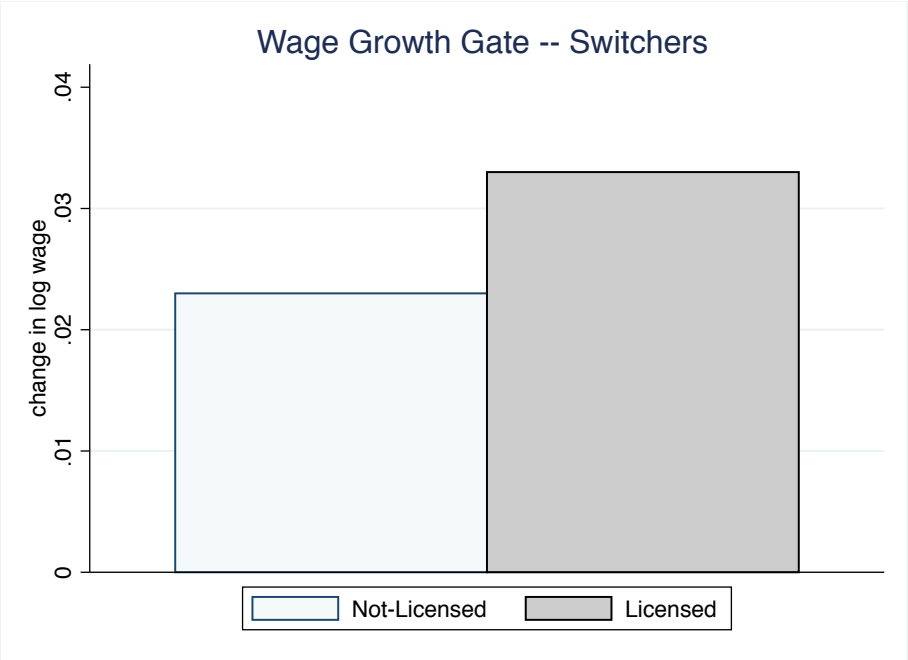


Figure 12: **Average Switching In Rate by Occupations**

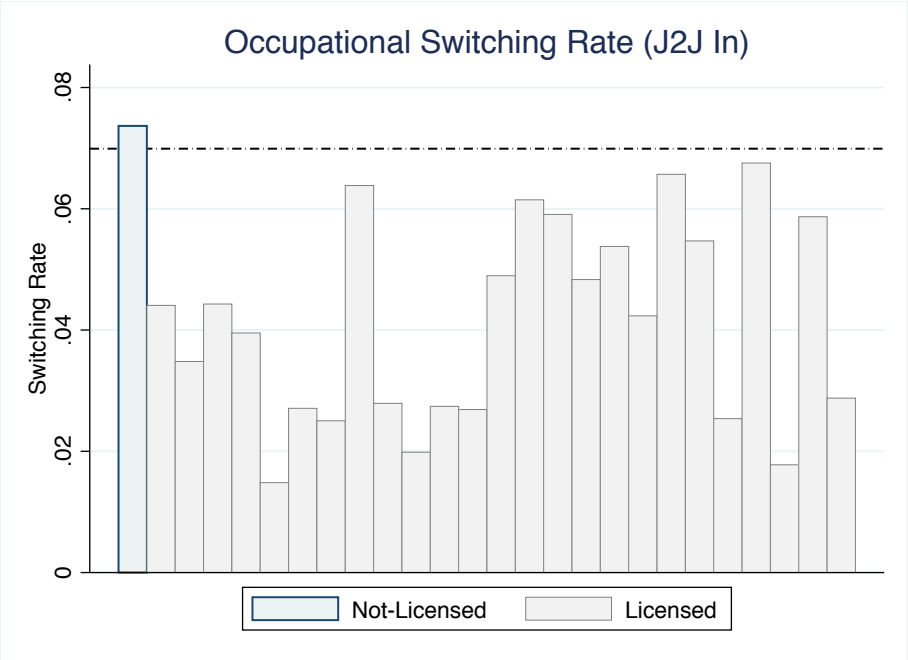


Figure 13: **Average Switching Out Rate by Occupations**

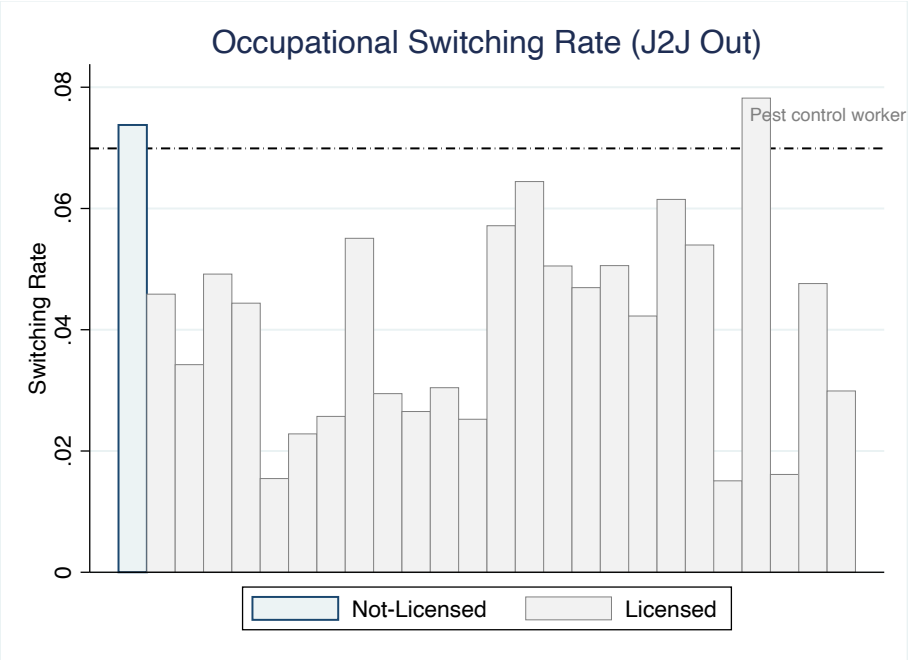


Figure 14: **Average Switching In (From U) Rate by Occupations**

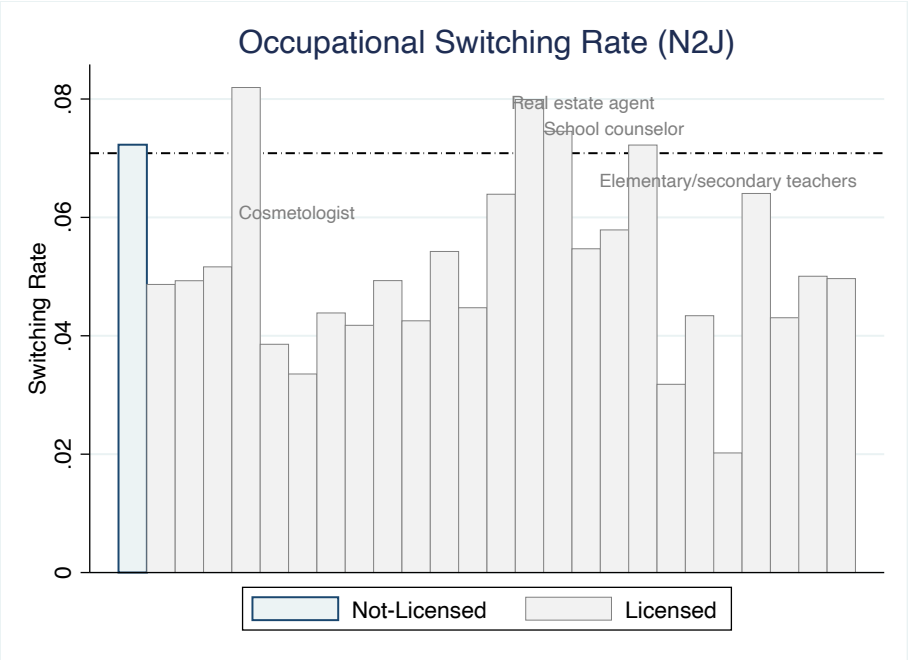
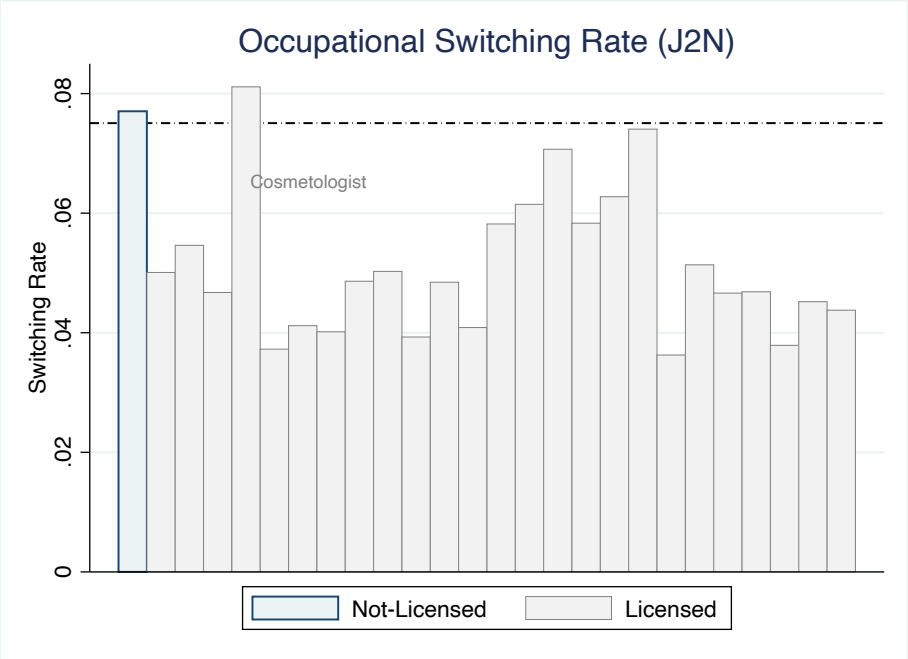


Figure 15: **Average Switching Out Rate (to U) by Occupations**



Appendix

A Universally Licensed Occupations

The following occupations are universally licensed (licensed in all 50 states and the District of Columbia) in the U.S. 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 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 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[29] 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 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 this licensing data and the panel structure of the CPS and have done what we can to make sure that the imputed licensing status data is 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 and Table **??**. After imputation, the licensing status change rate shows much more balance between interview months and imputation months. Tables **??** and **??** 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

[29]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 this type 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

In this section, we 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.

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 figures 15 and 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 a similar empirical analysis as before, regressing worker switching status and switching-associated wage changes on observable worker and job characteristics. The results are shown in Tables C.1 to C.4. 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 and wage changes are remarkably similar. Switching in and out of licensed occupations is much less frequent than non-licensed occupations, whether it is transitions 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: **Occupation Switching Rate from J to J**

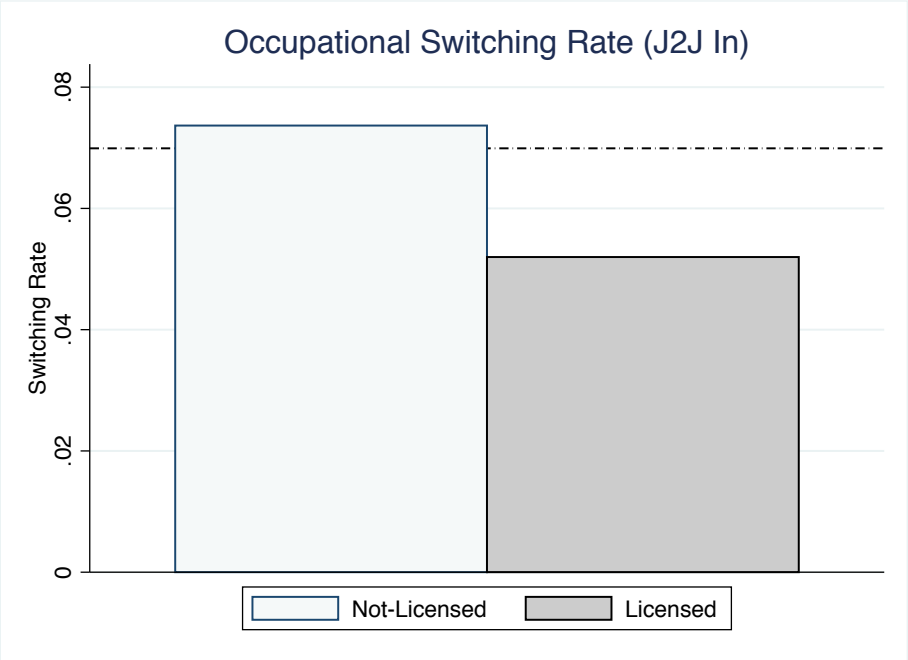


Figure C.2: **Occupation Switching Rate from J to J**

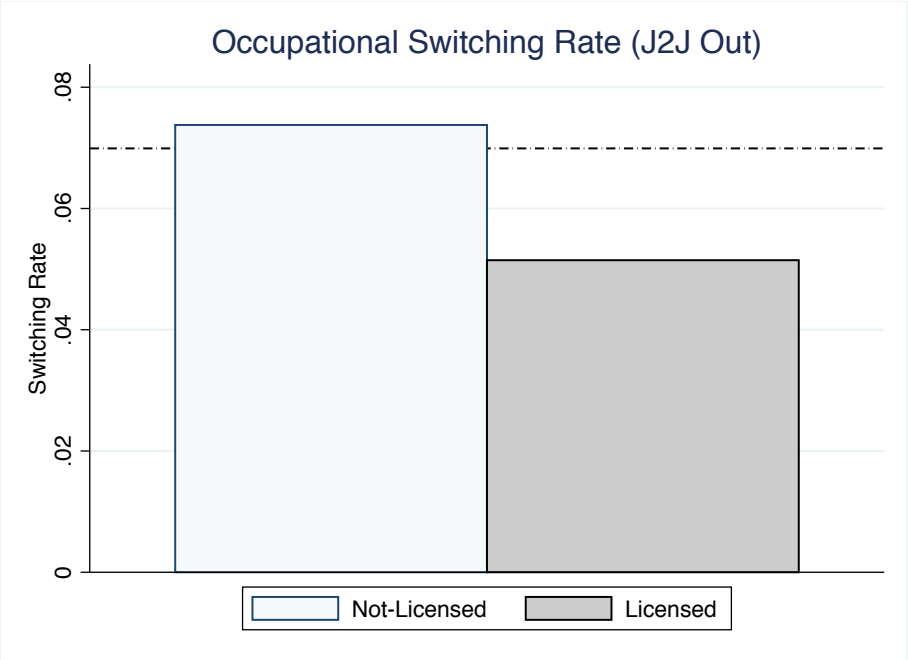


Figure C.3: **Occupation Switching Rate from U to J**

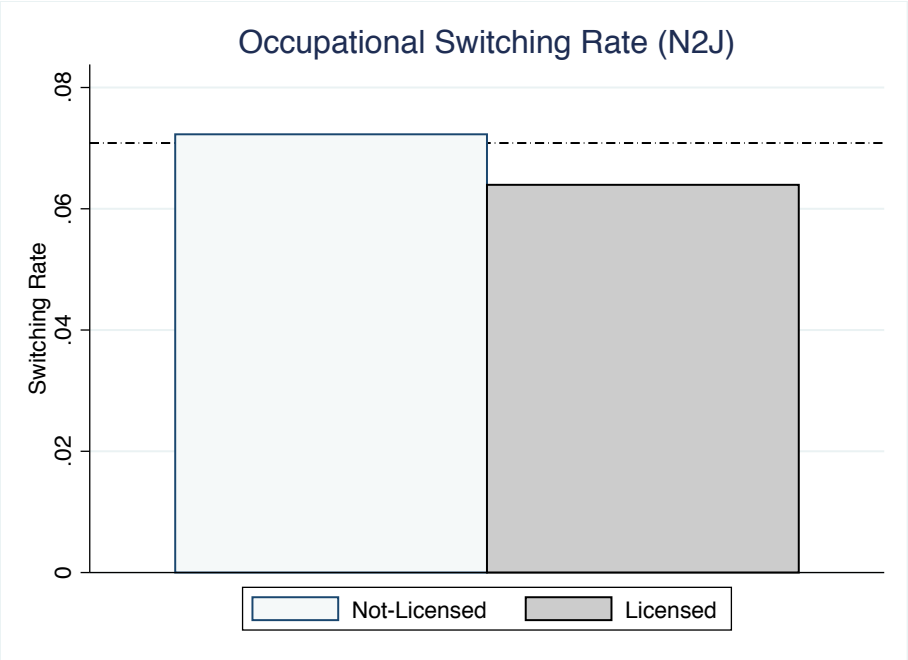


Figure C.4: **Occupation Switching Rate from J to U**

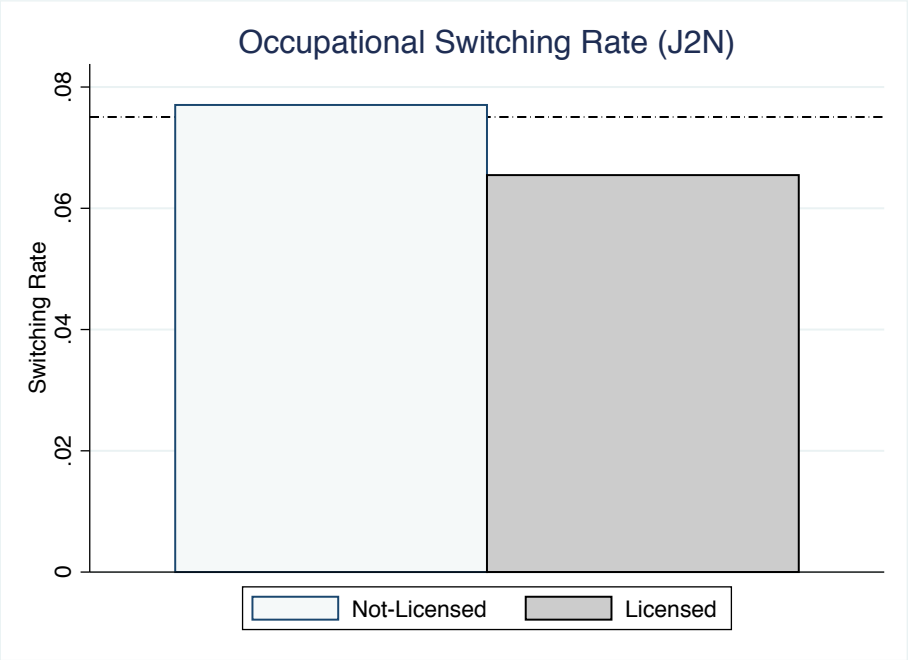


Table C.1: Occupational Switching (Job to Job)

	Switching Out (J2J)			Switching In (J2J)		
	1A	1B	1C	2A	2B	2C
Licensed (α_1)	-0.016***	-0.015***	-0.010***	-0.015***	-0.015***	-0.013***
Age	-0.002***	-0.002***	-0.001***	-0.002***	-0.002***	-0.002***
Female	-0.012***	-0.014***	-0.007***	-0.012**	-0.015**	-0.009***
Married	-0.020***	-0.020***	-0.020***	-0.020***	-0.020***	-0.023***
College	0.009***	0.010***	0.002*	0.010***	0.009***	0.000
Monthly Income	-0.010***	-0.010***	-0.008***	0.001***	0.001***	0.001***
Cognitive		-0.009**	0.023***		-0.090***	-0.028***
Manual		-0.022***	-0.038***		-0.001	-0.046***
Interpersonal		-0.020***	-0.020***		-0.065***	-0.039***
$\mathbb{E}[Y]$			0.053			0.058
% Effect of α_1			-18.7%			-22.3%
Matched	No	No	Yes	No	No	Yes
Observations	1,131,107	1,127,636	379,876	1,129,902	1,126,104	379,476

Table C.2: Switching In and Out of Unemployment

	Switching Out (J2N)			Switching In (N2J)		
	3A	3B	3C	4A	4B	4C
Licensed (α_1)	-0.004***	-0.006***	-0.004***	-0.003***	-0.005***	-0.004***
Age	-0.002***	-0.002***	-0.001***	-0.003***	-0.003***	-0.002***
Female	0.006***	0.003***	0.008***	0.006**	0.003**	0.007***
Married	-0.025***	-0.024***	-0.018***	-0.026***	-0.025***	-0.023***
College	-0.005***	-0.001***	-0.005*	0.007***	0.013***	0.009
Monthly Income	-0.005***	-0.005***	-0.004***	-0.007***	-0.006***	-0.008***
Cognitive	-	-0.048**	-0.018***	-	-0.067***	-0.034***
Manual	-	-0.013***	-0.030***	-	-0.018	-0.041***
Interpersonal	-	-0.000***	-0.029***	-	-0.005***	-0.030***
$\mathbb{E}[Y]$			0.068			0.067
% Effect of α_1			-5.96%			-5.94%
Matched	No	No	Yes	No	No	Yes
Observations	1,297,738	1,293,988	444,770	1,291,856	1,288,114	444,286

Table C.3: The Relative Effect of Licensing on the Unemployed vs. the Employed

	Switching In (N2J vs J2J)		
	5A	5B	5C
Licensed (α_1)	0.056***	0.053**	0.043***
Age	-0.002***	-0.002***	-0.002***
Female	0,051***	0.051***	0.048***
Married	-0.019***	-0.016***	0.004
College	0.021***	0.038***	0.039***
Monthly Income	-0.016***	-0.013***	-0.007***
Cognitive	-	-0.196***	-0.098***
Manual	-	0.026***	-0.062***
Interpersonal	-	0.014	-0.011
Matched	No	No	Yes
Observations	175,440	174,987	50,030

Table C.4: Wage Effects of Switching from Unemployment to Licensed vs. Non-Licensed Occupations

	J2J, out	J2J, in	N2J, in
Licensed (α_1)	-0.007***	0.004***	0.207***
Age	-0.001***	-0.001***	0.005***
Female	0.006***	0.006***	-0.037***
Married	-0.001	-0.006***	-0.012
College	0.003*	0.001	0.027***
Cognitive	-0.033***	0.004	1.699***
Manual	-0.022***	0.008	0.873***
Interpersonal	-0.019***	0.002	0.612***
$\mathbb{E}[Y]$	0.009	0.009	7.382
% Effect of α_1	-77.7%	47.3%	22.2%
Matched	Yes	Yes	Yes
Observations	367,016	367,528	26,306