Driver's License Reforms and Job Accessibility among Undocumented Immigrants

Heepyung Cho *

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Abstract

I analyze how allowing undocumented immigrants to legally obtain driver's licenses shifts commuting patterns, increases job accessibility, and improves labor market outcomes. Using state- and nativity-level variation in reforms, I show that granting driving privileges to the undocumented increases vehicle ownership and the probability of car commute by 3 percentage points. This improvement in accessibility leads to a 1 percentage point increase in the employment rate. The employment effects are larger in low-accessibility localities, which are more rural and entail longer commuting times. Undocumented immigrants exhibit stronger positive employments effects in more car-dependent occupations, shifting away from less car-dependent occupations.

Keywords: immigration policy, spatial mismatch, commuting, undocumented immigrants
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*Associate Fellow, Korea Institute of Public Finance. Email:heepyungcho@gmail.com Address: 336 Sicheong-daero, Sejong 20147, South Korea. I would like to give special thanks to David Albouy, Mark Borgschulte, Alex Bartik and Rebecca Thornton, Stephen Ross and seminar audiences at the University of Illinois, Urban Economics Association and Southern Economic Association for helpful discussion and comments.

1 Introduction

Labor market efficiency depends critically on effectively matching workers with firms. As cities have been decentralized and specialization has increased, *spatial mismatch* has been hypothesized as a potential barrier preventing workers from finding employment at firms and in occupations that best fit their abilities. As first highlighted by Kain (1968),[1] spatial mismatch occurs when workers are spatially disconnected from jobs as a result of limited residential mobility or a scarcity of local jobs. Although spatial mismatch can play an important and growing role in the labor market, evidence for its empirical effects on labor market outcomes and the effectiveness of policies designed to address it remains scant. Previous studies often lack exogenous variation of job accessibility, which is subject to the problem of reverse causality between accessibility and labor market outcomes.

In this paper I analyze the causal effects of improved job accessibility using recent policy reforms: granting driver's licenses to undocumented immigrants. Prior to the reforms, many undocumented immigrants were unable to drive legally because of restrictions on driver's licenses, which exacerbated the spatial mismatch they experienced. Beginning especially in 2013, a wave of state-level reforms removed legal barriers to obtaining licenses, granting undocumented immigrants significantly greater mobility and independence. To fix these ideas, I first present a simple search-and-matching model that illustrates the employment effects of reduced commuting costs resulting from license reforms. Then, using the policy variation in event-study, difference-in-differences, and triple difference models, I identify the impacts of the license reforms on vehicle ownership and commuting patterns. I then examine the consequences for spatial mismatch and other labor market outcomes.

The analysis in this paper depends crucially on identifying undocumented immigrants from survey data, which does not usually reflect such sensitive information. To do so, I use an imputation method to identify undocumented immigrants in survey data, developed by Borjas (2017). I first identify documented immigrants among the foreign born in the datasets using characteristics such as citizenship status or year of US entry. The residual group of all the other foreign born individuals is then classified as potentially undocumented immigrants.[2]

I find that driver's license reforms resulted in economically meaningfully improvements in labor market outcomes for undocumented workers, which appears to result primarily from improved accessibility. I first show significant increases in vehicle ownership and the probability of car commute for undocumented immigrants. The vehicle ownership rate and probability of commuting by car increase by 2 to 3 percentage points. This result confirms that the license reforms had large first-order policy effects on undocumented immigrants, improving their accessibility through increases in vehicle ownership. I then show that granting driver's licenses to undocumented immigrants increases their employment rate by 1 percentage point.

[1] Kain (1968) argues that blacks living in segregated inner cities have poor labor market outcomes as a result of poor accessibility to suburban areas with decentralized jobs.

[2] I conduct several robustness checks, such as identifying the undocumented population using Mexican-born high school dropouts, and find that the results are largely unchanged by alternative methods.

There are two main mechanisms underlying the commuting and employment effects of issuing driver's licenses to the undocumented: spatial frictions and the level of car-dependency associated with a given occupation. First, the effects of license reforms are examined by reference to area-level accessibility for undocumented immigrants, which is measured by the difference in the mean transit time between undocumented and documented immigrants. I find that the positive impacts of license reforms are large in localities where undocumented immigrants suffer from poorer accessibility with longer transit times than documented workers experience. These localities tend to be rural areas and smaller cities, where jobs are scarce and public transportation is limited.

Second, the effects of license reforms are broken down by how much each occupation requires workers to drive. The measure of occupation-related car-dependency is calculated using the percentage of documented workers commuting by car for each occupation. For undocumented immigrants, the positive employment and commuting effects are concentrated in more car-dependent occupations, such as agriculture, food services, manufacturing, or construction. These occupations tend to be more male-dominated and higher paying for undocumented immigrants. In contrast, I find negative employment effects in low car-dependent occupations, such as childcare or housekeeping. This implies that undocumented workers shifts from low car-dependent to high car-dependent occupations. Unlike the undocumented, documented immigrants exhibit more negative employment effects in high car-dependent occupations, suggesting that undocumented and documented workers are generally substitutes in labor markets.[3]

This paper makes the following contributions to the existing literature. First, it adds to the spatial mismatch literature using a novel policy variation that can directly improve employment accessibility for minorities. There is a voluminous body of literature on the spatial mismatch hypothesis, such as Brueckner and Zenou (2003), Hellerstein, Neumark, and McInerney (2008), and Andersson et al. (2018).[4] Also, Ong (2002), Raphael and Rice (2002), Cervero, Sandoval, and Landis (2002), Gurley and Bruce (2005), and Baum (2009) focus specifically on "transportation mismatch," analyzing the relationship between vehicle ownership and labor market outcomes.[5] An empirical challenge to identifying a causal effect of spatial or transportation mismatch on employment outcomes has emerged, however, because of reverse causality or omitted variable bias (Glaeser 1996). Unlike previous papers, many of which use primarily instrumental variables to address these problems, I analyze an exogenous policy reform that can directly improve job accessibility. More-

[3]It is possible that there exists an alternative explanation for the positive employment effects of license reforms, for example that driver's licenses serve as forms of identification. However, most driver's licenses for undocumented immigrants cannot be used for identification or verification purposes for employment eligibility. Also, it is unlikely that having a better form of identification will induce large changes in the occupation choices of undocumented immigrants.

[4]For literature surveys, see Ihlanfeldt and Sjoquist (1998), Houston (2005), and Gobillon et al. (2007).

[5]Ong (2002), Cervero, Sandoval, and Landis (2002), Gurley and Bruce (2005), and Baum (2009) find that vehicle ownership improves labor market outcomes for welfare recipients. Raphael and Rice (2002) find similar effects on a more general population using gasoline taxes and state insurance premiums as instruments. Gautier and Zenou (2010) provide a theoretical model to explain how the lack of vehicles for minorities leads to poor labor market outcomes.

over, I show empirically that the effects of issuing driver's licenses is stronger in places characterized by lower accessibility, connecting the traditional spatial mismatch literature with the transportation mismatch literature that emphasizes the role of private vehicles. Furthermore, I examine shifts in occupational choice induced by improved job accessibility, which has not been fully explored in prior works on spatial (or transportation) mismatch.[6]

The second contribution involves analyzing the policy effects of granting driver's licenses to undocumented immigrants. In particular, I show the large impacts of license reforms on vehicle ownership and means of commuting in this population. Other works have examined the effects of driver's license policy targeting undocumented immigrants. Cáceres and Jameson (2015) study how restricting undocumented immigrants from obtaining driver's licenses in the earlier periods affected insurance premiums. Lueders, Hainmueller, and Lawrence (2017) focus on the impact of license reform on accidents in California. In a recent paper that was released while this paper was in progress, Amuedo-Dorantes, Arenas-Arroyo, and Sevilla (2020) examine the effects of license reforms on labor market outcomes for likely undocumented immigrants in a panel-data design that is similar to the one used in this paper. My paper differs substantially from this other work, however, additionally analyzing first-order effects on vehicle ownership and showing the spatial and occupation mechanisms behind the positive employment effects on undocumented immigrants.[7]

The paper proceeds as follows. In Section 2, I provide a conceptual framework for the effects of license reforms. In Section 3, I sketch the timeline of license reforms. In Section 4, I describe the data and introduce descriptive statistics. In Section 5, I outline empirical strategies used in the analysis. In Section 6, I discuss the effects of license reforms on vehicle ownership. In Section 7, I analyze how license reforms affect commuting and employment outcomes. Sections 8 and 9 focus on the two mechanisms underlying the license reforms: spatial and occupation mechanisms. In Section 10, I use the Current Population Survey (CPS) instead of the American Community Survey (ACS). I conclude in Section 11.

2 Conceptual Framework

To fix ideas, I present a simple search-and-matching model illustrating the effects of allowing undocumented immigrants to obtain driver's licenses on their employment outcomes. The model is a slightly simplified and modified version of Gautier and Zenou (2010), who find that minorities

[6] One exception is Boustan and Margo (2009), who find that blacks in central cities substitute towards postal work in response to the suburbanization of jobs in the 1960s and 1970s.

[7] Also, they define likely undocumented as the low-skilled non-citizen Hispanics who have stayed in the U.S. longer than five years, while I define undocumented immigrants using the methodology introduced by Borjas (2017), which is explained in detail in Section 4. In Table A5 and Table A6, I also run the analysis restricting the samples to low-educated Hispanics. In addition, the paper's previous version (Amuedo-Dorantes, Arenas-Arroyo, and Sevilla 2018) used primarily the Annual Social and Economic Supplement (ASEC) of the CPS (2013-2017), focusing primarily on employment outcomes for likely undocumented immigrants. In this paper, I use primarily the Decennial Census and ACS (2000-2018), which collect information on vehicle ownership and commuting. In Table A20, I also use the CPS instead of the ACS.

exhibit lower car ownership because of lower initial wealth, eventually leading to adverse labor market outcomes. I extend the previous model by incorporating undocumented workers' risks of being deported into their commuting costs.

Here, I briefly summarize the conceptual framework shown in Appendix A. When undocumented immigrants are allowed to legally obtain driver's licenses, this will reduce the deportation risk of commuting by car embedded in their commuting costs. If the risk is sufficiently reduced, then undocumented workers will switch from public transportation to car. This transition to car commuting will be larger in rural localities where private vehicles are more effective means of commute and in occupations that offer higher wages to workers with private vehicles (car-dependent occupations). Because of the reduced commuting costs from the license reforms, undocumented workers will expand their maximum search (commuting) distance, increasing the probability that undocumented workers are being matched to firms. This eventually increases employment rate of undocumented immigrants. Also, I show that the positive employment effects will be larger for the low-skilled (under the assumption of diminishing returns on search) who were not searching far enough away before the reform.

3 Timeline of License Reforms

By 1954, all of US drivers were required to obtain valid driver's licenses issued by their state of residence. The documents required to obtain licenses varied by state, but most states asked only for some form of identification and proof of residency which they could provide with documents, such as power bills or rent contracts, without requiring Social Security Numbers (SSNs) or documented presence in the United States. Thus, many states placed few restrictions on undocumented immigrants seeking driver's licenses.

Nevertheless, starting in the early 1990s, a number of states began imposing restrictions on the issuance of driver's licenses. There are two ways to make it difficult for undocumented immigrants to obtain driver's licenses (Cáceres and Jameson 2015). The direct way is requiring proof of documented presence, such as US birth certificate, a permanent residence card, or a US visa. The other indirect way is to require a valid SSN, which is available only to persons with a legal presence in the United States. If a state implements at least one of the above measures, it is considered to be a state that does not allow undocumented immigrants to obtain driver's licenses. By the end of the 1990s, most states had enacted some form of such restrictions regarding SSNs. In addition, the September 11 attacks of 2001 induced most states to further introduce documented presence requirements, which directly prevented undocumented immigrants from legally obtaining driver's licenses.

We have seen a change in the trend more recently, however, especially starting from 2013. Some states began removing the restrictions and to explicitly allow undocumented immigrants to obtain driver's licenses. As of 2018, 12 states (in addition to the District of Columbia) allowed

undocumented immigrants to obtain driver's licenses or driving permits. Immigrants in those states are not required to provide their SSNs or proof of legal status in the US; these requirements have been replaced by other alternative documents, such as Individual Taxpayer Identification Numbers (ITINs) or foreign birth certificates. The list of states that have changed their driver's license laws favoring undocumented immigrants during the sample periods (2000-2018) is shown in Table 1.[8] The "start license" column refers to the first year when undocumented immigrants were able to obtain driver's licenses through elimination of documented presence requirements. Except for New Mexico, Tennessee and Maine, most of the license reforms have been put into effect after 2014 during our sample period. Tennessee and Maine again stopped issuing licenses to undocumented immigrants after a several years.

In addition to the state-level reforms, two federal laws have had significant impacts on license policies in individual states (Schroeder et al. 2015). The Real ID Act, enacted in 2005, set guidelines for driver's licenses; every state was authorized to issue driver's licenses to undocumented immigrants, but only if those licenses are distinguishable from the standard licenses. Thus, most driver's licenses that are issued to undocumented immigrants nowadays cannot serve for identification, with exceptions in Washington and New Mexico. Second, Deferred Action for Childhood Arrivals (DACA), which was implemented in 2012, has allowed young undocumented immigrants, who were brought to the United States in their early ages, to get work permits and driver's licenses. However, certain age and education requirements (having earned at least a high school degree) with high application fees (495 dollars) may still restrict many young undocumented immigrants from obtaining valid driver's licenses.[9]

The license reforms targeting undocumented immigrants stirred extensive debate. Most of debates focused on non-economic issues (Cáceres and Jameson 2015); proponents of license reforms often argue that issuing driver's licenses to undocumented immigrants will enhance public safety and strengthen civil rights, while opponents worry about encouraging the "illegal presence" of undocumented immigrants. The license reforms are mostly enacted by state legislatures or governors, which are driven primarily by their political agendas. For example, Democrats in California felt compelled to introduce a requirement for documented presence in 1993, under threat by State Republicans to worsen other privileges enjoyed by immigrants. Since then, there have been continuous efforts to issue driver's licenses to undocumented immigrants, a goal has been finally put into effect through Assembly Bill 60 in 2013 (Silva 2015).

[8]For a full history of license policies targeting undocumented immigrants between 1994 and 2018, refer to Table A1.

[9]I specifically look at the younger age groups (18-24 years old) to obtain the results reported in Table A13 to see the interaction of DACA with state-level license reforms.

4 Data and Descriptive Statistics

In the main analysis, I use the Decennial Census (2000) and the American Community Survey (2001 to 2018) from the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al. 2018). I restrict my samples to the working age population (ages 18-54 years) in the main analysis, classifying them to (potentially) undocumented immigrants, documented immigrants, and natives.[10] I drop samples from Tennessee and Maine, which have stopped issuing licenses to undocumented immigrants again after issuing them when the license reforms were passed, although dropping these states does not change the main results reported in this paper.

To identify undocumented immigrants from the Census and the ACS, I closely follow the methodology introduced by Borjas (2017) and Borjas and Cassidy (2019). A foreign-born person is classified as a documented immigrant if she meets any of the following conditions:

1. Arrived before 1981 (Immigration Reform and Control Act of 1986)
2. Has citizenship
3. Has Social Security income
4. Is a veteran or is currently in the armed forces
5. Works in the government sector
6. Was born in Cuba (all Cuban immigrants were granted refugee status)
7. Works in an occupation that requires some form of licensing[11]
8. Is likely to have H-1B Visa (H1B Filter)[12]
9. Is an individual whose spouse or parents meet any one of the above conditions[13]

[10] Immigrants are defined as foreign-born persons who are not US citizens by birth. Natives are US-born or born abroad to US citizen parents.

[11] Borjas does not explicitly identify occupations that require licensing in his paper. Here, I use the list of universally licensed occupations from Gittleman, Klee, and Kleiner (2018). Also, I additionally consider 20 occupations as occupations requiring licensing (such as nursing and psychiatry), where majority of workers are licensed according to the CPS (the definition of being licensed is taken from Kleiner and Soltas (2018)). Using alternative sets of occupations does not change the main results reported in this paper.

[12] According to Borjas and Cassidy (2019), a foreign-born individual is considered to have a H-1B visa if she satisfies all of the following conditions: (1) works in an occupation that commonly employs H-1B visa holders (computer and information system managers, computer and mathematical occupations, architecture and engineering occupations, and post-secondary teachers) (2) has lived in the United States for six years or less, and (3) has earned at least a BA degree. Applying this H-1B filter reduces Type-1 error for identifying high-skilled undocumented immigrants. Again, this does not have significant impacts on the results reported in this paper.

[13] Borjas's original criterion is "a person whose spouse meet any of the above conditions," which excludes parents. However, most documented immigrants are able to have their children stay legally in the US using dependent visas or invitations. Again, changing this criterion does not have any significant effects on the results discussed in this paper.

The residual group of all other foreign-born respondents is classified as potentially undocumented immigrants.[14] Borjas (2017) shows that the estimates derived using his methodology are highly comparable to those of the Pew Institute (Passel and Cohn 2014), generating very similar counts and labor supply characteristics of undocumented immigrants.

Of course, the above methodology is not a perfect way of identifying undocumented immigrants. It is possible that the survey data are not accurate as a result of misreporting (intentionally or non-intentionally) or refusals. In addition, this methodology might identify undocumented immigrants too "liberally," potentially generating more Type-1 errors for identifying undocumented immigrants. That is, there is a chance that some immigrants with valid visas are classified as undocumented immigrants (such as international students with F-Visas or high-skilled workers with H-1B Visas), which would overestimate the undocumented population in the analysis.[15]

Nevertheless, the goal of this paper is not to accurately estimate the number undocumented population; rather its goal is to determine whether the license reforms have any impact on the undocumented population. In the framework of difference-in-differences or triple difference analyses that this paper uses, over-counting undocumented immigrants would lead only to a reduction in the coefficients of license reforms. Thus, the coefficients here need to be interpreted as lower bounds of the policy effects, because the "potentially undocumented group" in the analysis is actually a mixture of undocumented immigrants and (a small number of) documented immigrants who are not directly affected by the license reforms. In addition, I find that the share of undocumented immigrants estimated using the above methodology is very similar to the share that is calculated by the Department of Homeland Security (Figure A1). Finally, restricting my samples to low-educated Hispanics do not alter the main results, which further validates the methodology used in this paper (Section 7.2 and Table A5).

Table 2 presents the summary statistics for the samples. About 6 percent (using Census/ACS weights) of the samples are identified as undocumented immigrants in the sample period (2000-2018). I find that undocumented immigrants are more male and younger than documented immigrants and natives. In addition, undocumented immigrants exhibit significantly lower educational attainment, with 44 percent being high school dropouts. Undocumented immigrants exhibit lower vehicle ownership rates than documented immigrants and natives. Immigrants overall experience generally longer transit times (27 minutes one way) than natives (24 minutes one way). Finally, undocumented immigrants experience worse labor market outcomes than documented immigrants and natives, with lower employment rates and wage income.

[14] In the subsequent analysis, I will refer to this group as "undocumented immigrants."

[15] I explicitly test for the possibility of large measurement errors by further restricting my samples to low-educated Hispanics and reporting the results in Table A5 .

5 Empirical Strategy

Throughout the paper, I estimate primarily the following equation for outcome variable y_{ist}, separately for undocumented immigrants, documented immigrants, and natives:

$$y_{ist} = \beta D_{st} + X_{ist}\Gamma + \psi_s + \theta_t + Z_s \cdot \theta_t + \epsilon_{ist} \tag{1}$$

where the subscripts refer to individual (i), state (s), and year (t). The key explanatory variable is D_{st}, which is an indicator that equals 1 if state s grants licenses to undocumented immigrants in year t. Outcome variables, y_{ist}, are indicators for vehicle ownership and employment, respectively. I additionally include fixed effects for individual characteristics (age, sex, education, years in US, race, regions of birth, English skills, and veteran status, all interacted with Hispanic status), X_{ist}. I also control for the baseline state-level characteristics, Z_s, which are the share of immigrants with college degrees (2000-2002) and the average state partisanship (2000-2018), interacted with the year fixed effects, θ_t.[16] Finally, I include state fixed effects, ψ_s, and year fixed effects, θ_t. Thus, β is a difference-in-differences estimator for the effects of license reforms on the outcome variables.

Also, I consider an event-study version of equation 1, where the indicator variable, D_{st}, is replaced by event-year indicators, τ_{st}. Event years are defined as the calendar year minus the first year after the license reforms were put into effect. More formally, I estimate

$$y_{ist} = \sum_{d=-5, d\neq -1}^{4} \tau_{st}^{d} \cdot \beta_d + X_{ist}\Gamma + \psi_s + \theta_t + Z_s \cdot \theta_t + \epsilon_{ist} \tag{2}$$

where the event windows are four years prior to the reform and three years after the reform.[17] The event-study framework has the advantage of revealing pre-trends and post-trends surrounding the reforms, which can substantiate the common trends assumption of the above difference-in-differences estimators.

It is possible that the difference-in-differences estimators are biased upward by unobserved demand shocks or pre-existing trends that are correlated with the license reforms. Thus, I also consider the triple difference framework for undocumented and documented immigrant samples, using natives as an additional control:

$$y_{inst} = \beta D_{st} + X_{inst}\Gamma + \psi_{ns} + \theta_{nt} + \omega_{st} + \epsilon_{inst} \tag{3}$$

where n refers to nativity. Here, I include a full set of two-way fixed effects: nativity by state, ψ_{ns},

[16] For the average state partisanship, I use the number of years (out of 19 years) with Democratic controls of state senates, houses, and governors (data taken from https://www.ncsl.org/research/about-state-legislatures/partisan-composition.aspx). I show in Table A12 that excluding these baseline state-level characteristics do not alter my main results.

[17] I bin-up the endpoints (-5 and 4, not reported in Figure 1), which are event years outside of the event windows.

nativity by year, θ_{nt}, and state by year, ω_{st}.

There is a chance that natives are not valid controls for undocumented immigrants because they are substantially different from one another. Therefore, I reweight natives samples to make the distribution of their personal characteristics similar to those of the undocumented samples (DiNardo, Fortin, and Lemieux 1996). More specifically, I multiply the Census weights by the inverse probability of being an undocumented immigrant ($\frac{1-\hat{p}}{\hat{p}}$). The probability of being an undocumented immigrant, $\hat{p}$ is predicted from a regression with fully saturated age, sex, education, and Hispanic origin indicators.[18]

The fixed effects in the triple difference specification are expected to capture most of the pre-existing trends and unobserved state-level shocks that could bias the difference-in-differences coefficients. However, if there are any positive (or negative) spillovers of license reforms on the "reweighted natives," the triple difference estimators would likely to underestimate (or overestimate) the true effects of the license reforms on undocumented immigrants or documented immigrants. Still, this can be used to compare the heterogeneous effects of the reforms on undocumented and documented immigrants.

6 License Reforms and Vehicle Ownership

The likely first-order impact of allowing undocumented immigrants to obtain driver's licenses is that they will often be driving their own vehicles. Consequently, it is expected that the vehicle ownership rate for undocumented immigrants will rise, which can be tested directly using ACS data. I analyze the effects of license reforms on vehicle ownership by undocumented immigrants, documented immigrants, and natives using equations 1 and 3 and report the results in Table 3. The outcome variables are the number of vehicles in households and an indicator variable for vehicle ownership (which equals 1 if a household owns at least one vehicle).

Although the vehicle data in the ACS are available at the household level, I run individual-level regressions because I can fully control for individual characteristics such as age and sex, which will enhance precision. To correct for double-counting of households when using individual-level regressions (when the number of adults in a household is n, the household appears n times in the sample), I divide the individual Census or ACS personal weights by the number of times each household appears in the samples. Also, I flexibly control for the number of household members in several age groups: 0-17, 18-24 and 25+ years.

Table 3 summarizes the results pertaining to vehicle ownership by undocumented immigrants (columns 1-2), documented immigrants (columns 3-4), and natives (column 5). The results reported in columns 1, 3 and 5 are estimated using the difference-in-differences specification from equation 1. For columns 2 and 4, I use the triple difference specification from equation 3, with "reweighted

[18]Refer to Table A2 for the distributions of the probability of being undocumented for undocumented immigrants and natives.

natives" as an additional control. The results reported in column 1 of Panel A shows that license reforms have significant impacts on the number of vehicles available to undocumented households, increasing that number by 0.05. This is equivalent to a 3.5 percent increase in the number of vehicles relative to the mean (of the pre-reform periods). I show similar positive effects on undocumented immigrants in column 2, when using reweighted natives as an additional control. In contrast, I do not find any significant effects on documented immigrants or natives.

To obtain the results reported in Panel B of Table 3, I use an indicator for having at least one vehicle in a household as an outcome variable. Analogous to the results for the number of vehicles reported in Panel A, I observe statistically significant effects on vehicle ownership rates regardless of the specifications; license reform increases the probability of having at least one vehicle per household by 2.5 percentage points (see columns 1 and 2). Nevertheless, reading columns 3-5, we see that there is no significant impact on documented immigrants or natives. Summarizing the results, license reforms have substantial first-order effects on vehicle ownership by undocumented immigrants, which are the targeted group of the reforms. This significant rise in vehicle ownership can potentially alter how undocumented workers commute and increase their employment rates by improving job accessibility.

7 License Reforms, Commuting, and Employment

7.1 Effects on Commuting and Employment

In this section, I analyze the effects of issuing driver's licenses to undocumented immigrants on commuting and employment outcomes. I estimate equations 1 (difference-in-differences) and 3 (triple difference) separately for undocumented immigrants and documented immigrants.[19] Table 4 summarizes the commuting and employment effects of license reforms, where the outcome variables are indicators for commuting by car (Panel A) and being employed (Panel B).[20] Thus, the table shows the coefficients of license reforms, β, estimated using the linear probability model.

Figure 1 plots the event-study figures using equation 2, where I show the effects of license reforms on commuting and employment by event year. The event windows range from -4 to 3, where I "bin up" the event years outside of the event windows. Ten out of twelve reformed states started the policy in the later years (2014-2016), so I have relatively longer pre-periods but shorter post-periods in the ACS samples (2000-2018).[21]

I first focus on the effects of license reforms on the means of commuting using the difference-in-differences and triple difference specifications and report the results in Panel A of Table 4. In general, I show that there is a significant shift to car commuting for undocumented immigrants,

[19] In Table A3, I report the results for natives.

[20] For the purposes of this study, the term "car" includes automobiles, trucks, vans and motorcycles.

[21] The event-study analysis using balanced panel with longer event windows -9 to 3 (dropping unbalanced states and years) is reported in Figure A3, and they are virtually identical to Figure 1, which uses all samples.

but not for documented immigrants. For undocumented immigrants, license reforms are associated with an approximately 3 percentage points increase in the probability of commuting by car (see columns 1 and 2 of Panel A). This rise in car commuting is consistent with the increase in vehicle ownership rate reported in Table 3.[22] In contrast, I do not find any statistically significant effects on documented immigrants (see columns 4 and 5). To obtain the results reported in column 3, I further restrict the samples to employed individuals and show similar result for commuting as using all the samples, although the result should be interpreted with caution as this may introduce the possibility of selection bias into the sample.

Panel A of Figure 1 illustrates the dynamics of the license reforms effects on the probability of commuting by car. I show that the probability of car commuting by undocumented immigrants increases significantly immediately after the reforms (black line), with little evidence of pre-trends. The probability of commuting by car increases over 3 percentage points after 3 years of the reforms (+3), which is consistent with the coefficients reported in Table 4. On the other hand, This increase in car-commuting is not shown among documented immigrants (gray line).

It is not surprising that issuing driver's licenses to undocumented immigrants increases the probability that they will commute by car. Prior to the license reforms, many undocumented immigrants simply drove illegally without valid driver's licenses. The costs of driving were potentially high because of risk of being fined or deported in situations such as being stopped by the police or being involved in an accident. The other option for the undocumented was obtaining driver's licenses from other states that do not require a documented presence, paying the high costs of traveling to another state and preparing for false proof of residency. Thus, having valid driver's licenses in states where undocumented immigrants live will greatly reduce the risks and costs associated with driving, stimulating undocumented immigrants to commute by private vehicles more frequently.

I next focus on the effects of license reform on the employment rate. The results reported in Panel B of Table 4 show statistically significant and positive employment effects of license reforms on undocumented immigrants using both the difference-in-differences and triple difference specifications. Allowing undocumented immigrants to obtain driver's licenses leads to a 1.1 to 0.8 percentage point increase in the employment rate (see columns 1 and 2). This employment increase is largely driven by an increase in new wage-employment, and there is suggestive evidence of undocumented workers shifting from self-employment to wage-employment (Table A2). In contrast, I find a small statistically insignificant 0.2 to 0 percentage point decrease in the employment rate for their documented counterparts (columns 4 and 5).

The event-study results for employment effects are depicted in Panel B of Figure 1. I show that employment among undocumented immigrants starts to increase after a year (+1) of license reforms (black line). This suggests that it takes some time for undocumented workers to find new employment opportunities. Employment rate increases by more than 1 percentage point after two

[22] I additionally find that the effects on car commuting are driven entirely by driving alone, not carpooling (Table A4).

years of reforms, which is again in accordance with the results reported in Table 4. On the other hand, documented immigrants do not exhibit such a pattern, where the coefficients become steadily negative after the reform (gray line).

The positive employment effect of license reform on undocumented immigrants is consistent with the theoretical prediction highlighted in Section 2. Having valid driver's licenses will lower car commuting costs for undocumented immigrants, increasing the number and range of jobs to which they have access and can accept. Many empirical studies on transportation (automobile) mismatch also find that owning private vehicles leads to better labor market outcomes (Raphael and Rice 2002; Ong 2002; Cervero, Sandoval, and Landis 2002; Gurley and Bruce 2005; Baum 2009). If I assume that the employment increase for undocumented immigrant stems solely from the rise in the vehicle ownership, then having a vehicle in a household increases employment opportunity by 30 to 50 percent (comparable to the magnitude by Baum (2009)).[23]

Any effects on documented immigrants are spillovers as they are not directly affected by license reforms that target undocumented immigrants. Undocumented immigrants and documented immigrants are closer substitutes for one another than natives are with either, reflecting their similarity in skill levels, work abilities, cultures, and labor market characteristics. Therefore, any increase in the labor supply of undocumented immigrants generated by license reforms can result in more negative employment effects on documented immigrants than on natives (Borjas, Grogger, and Hanson (2008), Ottaviano and Peri (2012) and many others show that substitutability between immigrants is higher than substitutability between immigrants and natives). Consistent with this prediction, the results in Table 4 and Figure 1 suggest decreases in employment of documented immigrants following license reform, although the estimates are small and insignificant.[24]

7.2 Robustness of Commuting and Employment Results

I consider alternative sample restrictions and regression specifications to see whether the commuting and employment coefficients in Section 7.1 are robust. I first examine the possibility that the residual methodology of identifying undocumented immigrants (Borjas 2017) that is used in this paper causes large measurement errors. As stated in Section 4, this method might identify undocumented immigrants too liberally; the undocumented group in my analysis is a mixture of actual undocumented immigrants and (a small number of) documented immigrants. To address this problem I further restrict the samples to low-educated (high school or lower) Hispanics, the proportion of whom are undocumented immigrants is significantly high. This reduces Type-1 error, where I mistakenly classify documented immigrants in the undocumented group. I find that the commuting and employment results are largely unchanged by this additional sample restriction,

[23]This is calculated by dividing the coefficients reported in column 1 (column 2) of Panel A in Table 4 by those reported in column 1 (column 2) of Panel B in Table 3.

[24]For natives, on the other hand, I can rule out negative employment effects larger than 0.04 percentage points (Table A3).

which validates the methodology for identifying undocumented immigrants that is used in this paper (Table A5). Moreover, I analyze the effects only on non-citizen Mexican-born high school dropouts, which is often used as the proxy group of undocumented immigrants in the previous literature (Steele, Murnane, and Willett 2010; Passel, Cohn, and Gonzalez-Barrera 2013; Orrenius and Zavodny 2015). Again, the results pertaining to commuting and employment are largely unchanged from this alternative method of identifying undocumented immigrants (Table A6).

It is possible that the positive employment results reported in Table 4 are driven mostly by California, which experienced economic booms in recent years. To address this concern, I estimate equations 1 and 3 while excluding California, and the coefficients are largely unchanged (Table A7). Moreover, I additionally control for tightening of the license policies in the earlier years, either through SSN or documented presence requirements. I find that including such policy variables does not change the main effects of the recent license reforms (Table A8). In Section 10, I extend this analysis and focus in particular on the effects of the driver's license restrictions during the earlier periods (before 2000) by using the CPS. I also find that the employment and commuting effects of license reforms remain unchanged even after controlling for state-level immigration enforcement policies, such as E-verify or Secure Communities (Table A9).[25] Finally, I run the logit model instead of the linear probability model of equation 1. The general pattern of the effects on commuting and employment is unaltered when using this alternative model specification (Table A10).

Moreover, I use propensity-score matching to examine the validity of the difference-in-differences specification (equation 1). More specifically I use a large set of state-level characteristics in 2000, such as the share of foreign born and the employment rate, to predict the passage of license reforms after 2000. In the logit prediction, I consider two alternative specifications: using all possible state-level variables (the "kitchen sink" method) and using only the variables selected from LASSO.[26] The predicted probability (Figure A4) up to the power of 3 is then interacted with time fixed effects. The matching results are shown to be analytically identical to the basic difference-in-differences estimates (Table A12).

It is also possible that the treatment effects of license reforms on young undocumented immigrants (aged 18-24 years, included in the main analysis) will be different, especially for those who are eligible for DACA. DACA, which was implemented in 2012, has allowed certain young undocumented immigrants to obtain work permits and driver's licenses in every state.[27] I find, however, that there is no significant impact on commuting and employment outcomes of younger

[25]Data on immigration enforcement policies are taken from https://www.urban.org/features/state-immigration-policy-resource.

[26]The list of variables used in the prediction are: number of times with Democrats majority in senates, number of times with Democrats majority in houses, number of times with Democrat governors, log of state population, share foreign born, employment rate, share collage graduates and car ownership rate. The Lasso procedure selects the number of times with Democrats majority in senates and share foreign born as predictors of license reforms (Table A11).

[27]Some states, such as Arizona and Nebraska insisted that they would not allow the DACA recipients to apply for driver's licenses, but their decisions were eventually over-turned by the Supreme Court.

undocumented immigrants, regardless of DACA eligibility. It is possible that the effects of issuing licenses on younger undocumented immigrants are small because of college enrollment or their limited financial ability to purchase cars (Table A13).

I additionally check whether other labor market outcomes for undocumented immigrants, such as wages and hours, correspond with increases in employment from license reforms. When using all undocumented immigrant samples (including zero wage earners) in the regressions, I find large positive effects on wages. I also show similar increases in hours for undocumented immigrants. The effects on wages and hours for undocumented immigrants is consistent with the positive employment effects (extensive margin effects) that are reported in Table 4. When restricting the samples to employed workers, I find statistically insignificant but negative wage effects on both undocumented and documented immigrants. This possibly reflects increases in labor supply of undocumented workers induced by license reforms (Table A14).[28]

Another concern associated with analyzing the employment effects of license reforms is the possibility of migratory response. If the reforms lead to a selective in-migration response on the part of documented immigrants, then the possible negative effects on the employment rate of documented immigrants might have resulted from the increase in the population of documented immigrants. Thus, I examine the effects of license reforms on the out-migration and in-migration rates of undocumented and documented immigrants. I find no evidence of migratory response of documented immigrants, ruling out the possibility that the employment effects on documented immigrants stem from selective in-migration. For undocumented immigrants, I find small and marginally significant increases in both in-migration and out-migration rates, suggesting a rise in mobility of the undocumented following license reforms (Table A15).

8 License Reform Mechanisms: Spatial Frictions

8.1 Rural vs. Urban

In the previous section, I show that state-level license reforms improve accessibility to work for undocumented immigrants, leading to positive effects on car commuting and employment. In this section, I additionally use within-state variations to empirically identify spatial mechanisms underlying the license reforms. If there is any spatial aspect of license reforms, undocumented immigrants living in localities with restricted accessibility should be affected more by the reforms.

I first start the spatial analysis by considering how the effects of license reforms vary across urban and rural areas. As highlighted by Gurley and Bruce (2005) and Baum (2009), it is important to make this urban/rural distinction because urban and rural areas feature significantly different

[28]It is entirely possible that license reforms also increase labor demand for undocumented workers because of reduced spatial mismatch (Gobillon et al. 2007). Also, I find in Section 9 that undocumented workers are shifting to more car-dependent occupations that generally pay higher wages, canceling out the negative effects on wages of the increase in the labor supply.

commuting environments. In particular, rural undocumented immigrants can suffer from especially restricted accessibility because their jobs are often located far away from their homes, with limited access to transportation. As shown in Table 5, rural undocumented immigrants experienced a 4-percentage-point lower employment rate than urban undocumented immigrants in the pre-reform period (on the other hand, the difference between rural and urban documented immigrants is only 1 percentage point). Thus, it is possible that the effects of improving accessibility through license reforms will be stronger for undocumented immigrants in rural areas with lower accessibility.

To show the differences between the effects of license reforms on urban and rural areas, I estimate the main difference-in-differences equation 1, additionally interacting the policy variable, D, with the indicators for urban and rural status.[29] Also, I include time fixed effects interacted with urban/rural status in the regression equation to allow for divergent trends in urban and rural places.

The estimation results reported in Table 5 suggest that the effects on commuting and employment for undocumented immigrants are much stronger in rural areas than in urban areas. As indicated by the results reported in Panel A, I find that the probability of car commuting in rural areas increases by 4.6 percentage points, which is almost twice as large an increase as in urban areas. The differences between employment effects are seen to be even larger in Panel B; the undocumented immigrant employment rate in rural areas increases by 2.4 percentage points, which is three times larger an increase than in urban counterparts. Again, the results are consistent with the theoretical prediction posited in Section 2, which finds that the effects of granting driver's licenses are bigger in rural localities where private vehicles offer more effective way of commuting than public transportation.

8.2 Effects by Commuting-Zone-level Accessibility

I next analyze how the effects of license reforms vary by commuting-zone-level accessibility for undocumented immigrants. Commuting zones represent local labor markets in the United States, encompassing both metropolitan and non-metropolitan areas.[30] According to the spatial mismatch hypothesis, the effects of license reforms should be stronger on undocumented immigrants living in commuting zones where they have less accessibility.

To identify this spatial mechanism empirically, I first generate a commuting-zone-level accessibility measure for undocumented immigrants. I define this measure as the differences between the transit times of undocumented and documented immigrants, controlling for observable attributes. The measure is motivated by the traditional urban economics literature that uses transit time as

[29] I use the crosswalk between the Public Use Micro Area (PUMA) and urban/rural status provided by Marble/Geocorr 14k (Missouri Census Data Center 2018). The individual observations in the samples are split into two (urban and rural), and their weights are multiplied by the fractions of the population in the PUMA that belongs to urban and rural blocks.

[30] I use the 1990 version of 741 commuting zones from Autor, Dorn, and Hanson (2013). They provide the fractions of the population in the PUMA that belongs to each commuting zone. The individual observations in the samples are split into one per commuting zone, and their weights are multiplied by this fraction.

an indicator of spatial mismatch (Holzer 1991; Houston 2005). I refine this using the *difference* between the mean transit times of undocumented and documented immigrants in each commuting zone, where the transit time of documented immigrants serves as a baseline. That is, if undocumented immigrants experience significantly longer transit times than their documented counterparts in a certain commuting zone (conditional on observable characteristics and occupations), they are likely to have less accessibility there.

More formally, I estimate the following regression equation using undocumented and documented immigrant workers in the baseline years of 2000-2011:

$$\sqrt{f_{incot}} = \phi_{nc} + X_{inct}\Gamma_n + \rho_{no} + \theta_t + \epsilon_{inct} \tag{4}$$

where subscript n denotes the documentation status of immigrants ($n = 0$ when individual i is an undocumented immigrant and $n = 1$ when individual i is a documented immigrant), c denotes commuting zone, and o denotes occupation. The outcome variable, $\sqrt{f_{incot}}$, is the square root of transit time (f), following Albouy and Lue (2015).[31] The commuting time differential relative to the national average is $\hat{f_{nc}} = 2\hat{\phi_{nc}}/\overline{\sqrt{f}}$, where $\hat{\phi_{nc}}$ is the estimated commuting zone fixed effects, and $\overline{\sqrt{f}}$ is the average of square-root commuting time.

The measure of commuting-zone-level accessibility is then defined as the difference between the estimated commuting time differentials for undocumented and documented immigrants, $\hat{f_{0c}} - \hat{f_{1c}}$. In other words, this is the percentage difference between the transit times of undocumented and documented immigrants in each commuting zone, controlling for observable characteristics (X), occupation fixed effects (ρ_{no}) and time fixed effects (ψ_t).

Figure 2 depicts differences between the commuting times of undocumented and documented immigrants for each commuting zone. The negative slope of the fitted line suggests that the transit time gap tends to be wider (indicating less accessibility for undocumented immigrants) in commuting zones where documented immigrants experience shorter transit times. In Kennewick, for example, documented immigrants experience 40 percent shorter commuting times than the national average for immigrants, but undocumented immigrants experience 20 percent longer transit times than their documented counterparts. These commuting zones tend to be smaller and more rural, with heavy reliance on owned vehicles because of the scarcity of jobs and limited public transportation options. On the other hand, documented immigrants in larger cities such as Chicago experience almost 20 percent longer commute times than the national average, and the undocumented and documented experience similar transit times there.

I then interact the estimated measure of accessibility for undocumented immigrants, $\hat{f_{0c}} - \hat{f_{1c}}$, and the transit time differentials of documented immigrants , $\hat{f_{1c}}$, with the license reform indicator (D_{it}) in equation 1:

[31] According to Albouy and Lue (2015), the square root of commuting time fits the data better than most power transformations and accommodates reports of zero commuting time.

$$y_{icst} = \beta D_{st} + \gamma_1\{D_{st} \times (\hat{f_{0c}} - \hat{f_{1c}})\} + \gamma_2\{D_{st} \times \hat{f_{1c}}\} + X_{icst}\Gamma + \psi_s + \theta_t + Z_s \cdot \theta_t + \omega_c + \epsilon_{icst}. \quad (5)$$

where outcome variables y are car commuting and employed, respectively. Commuting-zone fixed effects, ω_c, are also included in the regression equation. The estimated coefficients are reported in Table 6, separately for undocumented immigrants (columns 1 through 3) and documented immigrants (columns 4 through 6).

The results reported in columns 1 through 3 show that the effects of license reforms on undocumented immigrants are stronger in commuting zones where the undocumented experience longer transit times than the documented. In column 1, I report the estimated coefficients of including only commuting-zone fixed effects. The results reported in column 2 show that the coefficients of commuting-zone-level accessibility measures (the transit time gap between undocumented and documented immigrants) interacted with license reforms, $\hat{\gamma_1}$, are large and statistically significant for all the outcome variables. The results reported in Panel A show that a 10-percent-point increase in the transit time gap between undocumented and documented immigrants is associated with a 1.35-percentage-point increase in the effects of license reforms on car commuting. The results reported in Panel B show that a 10-percentage-point increase in the transit time gap is associated with 0.57-percentage-point increase in the employment effects of license reforms, which is half of the mean employment effect. Adding the interaction term for documented immigrants' transit times (the baseline) in column 3 does not significantly change the coefficients of the accessibility measure.

Columns 4 through 6 display the results pertaining to documented immigrants. In general, documented immigrants do not exhibit the same pattern as undocumented immigrants; all the coefficients of the accessibility measure (the transit time gap) are statistically insignificant. Although it is imprecisely estimated, the accessibility coefficient is negative for documented immigrants when the outcome variable is employment (Panel B), suggesting a certain degree of substitutability between the undocumented and documented within commuting zones.

It is possible that the commuting-zone-level accessibility index for undocumented immigrants (indicating differences between the transit times of undocumented and documented workers) is subject to measurement errors, especially in smaller commuting zones with small number of immigrant populations. To check whether the above results are robust to possible measurement errors, I rerun the analysis but restrict my sample to immigrants in larger commuting zones (commuting zones with working-age populations above 100,000 and 500,000). Even after these sample restrictions, I find that the positive employment effects on undocumented immigrants are still stronger in commuting zones where undocumented workers experience longer transit times than documented workers (Table A16).

In Figure 3, I plot the estimated effects of license reforms on undocumented immigrants by

commuting zone using the coefficients reported in column 3 of Table 6. The positive effects on car commuting and employment are stronger in restricted-accessibility localities for undocumented immigrants where there are wider transit time gaps between undocumented and documented workers. These commuting zones with less accessibility tend to be smaller and more rural, which is consistent with the findings of the previous analysis using the simple urban/rural distinction.

8.3 Effects by Individual Transit Time

I now analyze the effects of license reforms by an individual-level measure of accessibility — individual transit time. As undocumented immigrants are granted privileges to drive, it is possible that there is a change in the individual transit times of undocumented immigrants. Because they enjoy easier access to workplaces using their own vehicles, it is likely that they will experience reduced transit times, especially if they experienced restricted accessibility with longer transit time.

To analyze the effects on transit time by reference to varying levels of individual accessibility, I use unconditional quantile regressions (Firpo, Fortin, and Lemieux 2009) in addition to simple OLS specification.[32] That is, I show the effects of the license reforms on multiple quantiles of transit times. In particular, the dependent variable y_{ist} in equation 1 is replaced with the re-centered influence function (RIF):

$$RIF(y; q_\tau) = q_\tau + \frac{\tau - 1\{y \leq q_\tau\}}{f_Y(q_\tau)} \tag{6}$$

where τ refers to quantiles (0.05 0.1 ... 0.95), q_τ is the value of the square root of transit time at quantile q. $f_Y(q_\tau)$ is the density of y at quantile τ, estimated using the Epanechnikov kernel with a bandwidth of 0.5.[33] As in the previous analysis, I estimate the regression equation separately for undocumented immigrants and documented immigrants.

Figure 4 illustrates the unconditional quantile effects of the license reforms on transit times. The OLS coefficients for undocumented immigrants (the black dotted horizontal line) are negative, but statistically insignificant. Nevertheless, I find an interesting pattern for undocumented immigrants when the effects are decomposed by quantile (the black solid line). There are statistically significant and larger negative effects on undocumented workers, for whom the quantiles of commuting times are larger than 0.5 (20 minutes or more, oneway). Also, the negative effects on transit times become larger as the quantile of transit time increases. For example, undocumented workers with

[32] It is important to distinguish the difference between conditional quantile regression (CQR) and unconditional quantile regression (UQR). CQR considers the effects by quantile, where the distribution is conditional on covariates, posing challenges to interpretation. For its part, UQR focuses on estimating the effects of the absolute distribution of an outcome variable that is not conditioned on covariates. Thus, in this particular example of the effects of license reforms on transit time, it makes more sense to look at the effects on unconditional quantiles of transit time. For more detailed explanations on unconditional quantile regression, refer to Firpo, Fortin, and Lemieux (2009) and Porter (2015).

[33] I also estimate using alternative bandwidths and kernel density functions (Gaussian), and the main results are essentially unaltered.

40 minunites of commuting times experience around a 0.5 minute ($= \sqrt{0.28}$) decrease in commuting time as a result of the license reforms. On the other hand, I do not find such a pattern for documented workers (the gray dotted line). Thus, the license reforms are effective tools for improving accessibility, especially for undocumented immigrants with restricted accessibility (longer transit times), who are particularly vulnerable to the problem of spatial mismatch.

9 License Reform Mechanisms: Occupation-Related Car-Dependency

9.1 Effects by Occupation-Related Car-Dependency

The second mechanism underlying the effects of license reforms is based on industry and occupation. Facilitating vehicle ownership can be a more effective tool for increasing employment among undocumented immigrants in certain industries or occupations where workers are more required to drive their own vehicles. That is, having driver's licenses will reduce transportation costs for undocumented workers to a greater extent in occupations that are more dependent on their owning cars.[34]

Before analyzing the occupation mechanism, I first document the basic facts about industry of undocumented immigrants. Compared to documented workers, undocumented workers are heavily concentrated in agriculture, construction, administrative/support, and accommodation/food services (Figure A6). The increase in car commuting and employment among undocumented immigrants from license reforms are mostly driven by agriculture, manufacturing and accommodation/food services industries. When focusing on within-industry effects of license reforms (controlling for industry fixed effects), construction also exhibit significant employment effects from license reforms (Figure A7).[35]

It is entirely possible that the sizable effects on the accommodation/food, agriculture, manufacturing, or construction industries may simply reflect the fact that a large share of undocumented immigrants are working in them (as shown in Figure A6). It is also possible, however, that occupa-

[34]For anecdotal evidence on importance of driver's licenses on certain jobs, refer to the following article. Selmuels, Alana. 2016. "No Driver's License, No Job." *The Atlantic*. June 2016. https://www.theatlantic.com/business/archive/2016/06/no-drivers-license-no-job/486653/.

[35]There are two ways to conduct industry-level analysis: with and without industry-level controls in the regression. First, I simply estimate the difference-in-differences regression specification (equation 1), where the outcome variables are employed (or commuting by car) in two-digit NAICS industry d. This specification accounts for workers who change their industries after license reforms. It is possible, however, that growth or decline of certain industries in reformed states are correlated with the timing of license reforms, biasing the estimates calculated without industry-level fixed effects. Alternatively, I consider within-industry employment effects, estimating equation 1 separately by industry d. To rescale the effects by industry size, I multiply the estimated coefficients, $\hat{\beta}_d$, by s_d, where s_d is the employment share of industry d. This specification can fully control industry-by-state characteristics and industry-specific trends. However, this model is based on the stronger assumption that license reforms are not affecting the composition of industries for undocumented and documented immigrants because the estimation is done within each industry.

tions in these industries depend more heavily on car commuting than those in other industries. For example, being able to drive can benefit workers in the accommodation/food industry, who often have break times built into their working hours. They can enjoy other activities or return home to relax during break hours, using their cars (Gobillon et al. 2007). Agriculture, manufacturing and construction workers may rely heavily on cars as well because the workplaces in these industries are often located in remote places that are not easily reachable by public transportation. Also, these workers sometimes need to operate agriculture or construction vehicles, which makes having driver's licenses a necessity for them.

To empirically test the occupation channel of license reforms, I first create a measure of occupation-level car-dependency. This is measure essentially calculates the probability of car commuting for each occupation, controlling for personal characteristics (such as age and education), place of residence, and place of work. More formally, I estimate the following equation using employed documented immigrants in the pre-reform period (2000-2011)[36]

$$(CommuteCar)_{iorwt} = \phi_o + X_{iorwt}\Gamma + \psi_t + \omega_{rw} + \epsilon_{iorwt} \tag{7}$$

where i denotes individuals; o denotes occupations (450 occupation categories); r denotes place-of-residence Public Use Microdata Area (PUMA); w denotes place-of-work PUMA. X is again the vector of personal characteristics; ψ_t is the year fixed effects. I additionally control for ω_{rw}, which is the place-or-residence PUMA interacted with place-of-work PUMA. The dependent variable, $CommuteCar$, is an indicator variable that is equal to 1 if a documented worker is commuting by car and 0 otherwise. The estimated occupation fixed effect, $\hat{\phi}_o$ is the probability of car commuting for occupation o, representing its dependency on car commuting.

The X-axis of Figure 5 depicts the estimated probability of car commuting for major occupations ($\hat{\phi}_o$ in equation 7), in differentials relative to the national average of documented immigrants. For the Y-axis, $\hat{\phi}_o$ is obtained by additionally controlling for the log of individual wages in equation 7. The 45-degree line shows that the values of $\hat{\phi}_o$ with and without the wage control are almost identical. Next, I rank occupations according to the estimated probability, assigning them to quartiles. Childcare workers (quantile 1), maids and housekeeping cleaners (quantile 1) and janitors, and building cleaners (quantile 3) are examples of low car-dependent occupations. Agriculture workers (quantile 6), construction laborers (quantile 7), and production workers (quantile 8) are examples of high car-dependent occupations. For a more detailed list of occupations and the corresponding measures of car-dependency, refer to Table A17.

Figure A8 shows how occupation-related car-dependency is associated with gender and wages. Again, all the measures here are constructed from documented immigrants. The results depicted

[36]I use documented immigrants here because they are more similar to undocumented immigrants than to natives. As robustness checks, I use natives to estimate occupation-related car-dependency and the main results presented here do not change significantly.

in Panel A indicates that occupations with larger share of males tend to be more heavily car-dependent occupations. In Panels B and C, I show the relationship between the average wages in various occupations and their car-dependency, separately low-skilled and high-skilled occupations.[37] In low-skilled occupations where undocumented workers usually work, higher-paying occupations tend to be more heavily car-dependent. I find the opposite pattern for high-skilled occupations, where higher-paying jobs are inclined to be less heavily car-dependent.

I then find the effects of license reforms on commuting and employment by quartile of occupation-related car-dependency. More specifically, I estimate

$$y_{ist}^{q} = \beta^{q} D_{st} + X_{ist}\Gamma + \psi_s + \theta_t + Z_s \cdot \theta_t + \epsilon_{ist} \tag{8}$$

where $y^q = \{CommuteCar^q, Employed^q\}$ are the open-ended occupation bins for car-dependency with quantile $q = 1, 2...10$. For example, $CommuteCar^q$ is the indicator variable for commuting by car and being employed in occupation with car-dependency that is less than or equal to that in the q quantile of car-dependency. The process is similar for $Employed^q$.

Figure 6 plots the effects of license reforms by quantile of occupation-related car-dependency, $\hat{\beta}_q$. The outcome variables are commuting by car (for undocumented immigrants) and being employed (for undocumented immigrants and documented immigrants). Unlike the commuting effects, the employment effects of license reforms on undocumented immigrants are clearly larger in occupations with higher car-dependency. The employment effects on undocumented immigrants are negative (employment decreases by 0.7 percentage points) and statistically significant in low car-dependent occupations with $q \leq 3$, such as childcare and housekeeping. On the other hand, I find stronger positive effects in higher car-dependent occupations, such as waiters and agriculture workers, which raises the total employment rate among undocumented immigrants increase by 1.1 percentage points. The results also suggest that undocumented workers shift from less car-dependent to more car-dependent occupations. These car-dependent jobs tend to be high-paying for low-skilled occupations, as shown in Figure A8.

The pattern of larger employment effects in car-dependent occupations on undocumented immigrants also appears in within-occupation effects of license reforms.[38] As in the case of Figure 6, I find negative within-occupation employment effects in low car-dependent occupations ($q \leq 3$). The employment effects steadily increase and become positive, however, as the quantile of occupation-related car-dependency increases. This further supports the occupation mechanism of the license reforms, although the results here should be interpreted cautiously in light of potential selection

[37] Occupations are low-skilled when the share of workers with high school degrees or lower educational attainment is smaller than 0.5. The remaining occupations are high-skilled.

[38] I estimate the basic difference-in-differences estimating equation (equation 1) with occupation fixed effects, restricting the samples to individuals in occupations with car-dependency that is less than or equal to q quantile. Again, this specification is based on the restrictive assumption that occupation compositions are not altered by the reforms.

and composition effects (Figure A9).

Unlike undocumented immigrants, documented immigrants do not exhibit the pattern whereby high car-dependent occupations mean greater employment effects on workers. Rather, I find that the effects tend to be more negative in high car-dependent occupations. This is again consistent with the previous findings that show a degree of substitutability between undocumented and documented immigrants in labor markets.

9.2 Heterogeneity

I now analyze the heterogeneous commuting and employment effects of license reforms by occupation-related car-dependency, estimating equation 8 (as in Figure 6) by subgroup. In Panels A and B of Figure 7, the effects of license reforms are broken down by sex. Both male and female undocumented immigrants experience a higher likelihood of car commuting, although the effect size is generally larger for males. There is an even starker difference in the employment effects, where the employment rate among male undocumented immigrants increases by 1.4 percentage points, which is much larger than the 0.5-percentage-point increase for females.[39] This difference between the genders is not surprising given that car-dependent occupations are male-dominant (as shown in Figure A8), and the effects of license reforms are stronger in more heavily car-dependent occupations. In fact, female undocumented immigrants exhibit more negative employment effects in low car-dependent occupations, while male undocumented immigrants show more positive employment effects in high car-dependent occupations.

In addition to the differences between the occupations in which male and female undocumented immigrants usually work, there are potential alternative explanations for the gender difference in the effects of license reforms. First, male undocumented immigrants are significantly more likely to be employed and to commute by car in the first place than their female counterparts. The mean employment rate among male undocumented immigrants in the pre-reform period is 0.82, which is much larger than the female employment rate of 0.51, Also, men generally travel farther than women when commuting, even after conditioning on transportation modes (Crane 2007). Thus, having access to owned vehicles will induce men to commute (or search) farther, leading to more employment opportunities.

The results depicted in Panels C and D of Figure 7 show the effects of license reforms by educational attainment: high school dropouts (Panel C) and high school graduates or above (Panel D).[40] For both low-skilled and high-skilled undocumented immigrants, the positive employment effects are mostly driven largely by more heavily car-dependent occupations. The effects on employment among low-skilled undocumented immigrants are larger, however, than those on the high-skilled.

[39] In Table A18, I show that the employment effects on undocumented immigrants are driven in particular by men in married-couple family households. I also find that this leads to a significant reduction in wife-only-employed households for undocumented immigrants (see Table A19 for the results).

[40] As shown in Table 2, almost half of undocumented immigrants do not have high school degrees.

This result is in line with the theoretical prediction posited in Section 2. Low-skilled undocumented immigrants are more likely to be vulnerable to the problem of spatial mismatch, not searching for jobs widely enough because they are less likely to be offered jobs and more likely to be offered lower wages. Thus, if there are diminishing returns on searching, the employment benefits of reduced transportation costs should be larger for low-skilled job seekers who could not search widely enough prior to the reforms.[41]

10 Evidence from Current Population Survey

In this section, I briefly discuss the impacts of driver's license laws using the Basic Monthly Samples of CPS (Flood et al. 2018) instead of the ACS used in the previous analysis. Although the CPS includes fewer variables and smaller samples than the ACS,[42] it has the advantage of reporting monthly instead of yearly data. Also, the panel structure of the CPS (with the same household being interviewed eight times during each 16-month interview period) makes it possible to analyze the effects on employment flows in addition to the employment stocks.

Using the CPS, I first analyze the effects of license reforms on employment stocks and flows. The effects on employment stocks broadly follow the patterns that I report in Table 4 using the ACS. I also find that the positive effect on employment stock of undocumented immigrant is entirely driven by increase in employment-to-employment (EE) transition or decrease in nonemployment-to-nonemployment transition (NN). It is difficult to detect any small effects on documented immigrants because of larger standard errors when using the CPS, but there is suggestive evidence that the possible negative effects on documented workers are driven by an increase in employment-to-nonemployment transition (Table A20).

Next, I focus on the effects of "restricting" undocumented immigrants from legally obtaining driver's license from 1994 (refer to Table A1 for the years of changes in the laws for each state). I find that the effects of restricting access to driver's licenses on employment is minimal, with all of the coefficients being insignificant. There are several possible reasons why placing restrictions on driver's license application may have limited impacts on undocumented immigrants. Undocumented immigrants who have already obtained valid driver's licenses can usually drive legally until the expiration date (or beyond the expiration date in states that have lenient renewal policies), even after tightening the license policy. Secondly, imposing (only) SSN requirements on license applications does not mean that all undocumented immigrants cannot obtain driver's licenses because the SSN

[41] An alternative explanation is that there are more high-skilled immigrants who are actually documented immigrants but are classified as potentially undocumented immigrants in the analysis.

[42] Also, compared with Census/ACS data, the CPS Basic Monthly Samples tend to over-estimate the undocumented population when utilizing the methodology introduced by Borjas (2017) because information on Social Security income is missing. Finally, unlike with Census/ACS data, non-response bias can be more problematic in the CPS because the survey is not mandatory. The non-response rate associated with the CPS has recently been growing rapidly. Thus, it is possible that undocumented immigrants are increasingly refusing to respond the survey entirely, or selectively refusing sensitive questions about their citizenship and years of immigration.

requirements were implemented differently across states. For example, some states required SSNs only for applicants who already have them, while other states asked them for for all applicants (Table A21).[43]

11 Conclusion

The existence of spatial mismatch among minorities and its importance in the labor market have been discussed extensively in multiple fields of studies. Empirical evidence indicating how spatial mismatch affects labor market outcomes remains scarce, however, because it is difficult to find credible and informative natural experiments. In this paper, I seek to fill this gap by exploring a policy that improves job accessibility, granting driving privileges to undocumented immigrants. In particular, I analyze the impacts of the policy on vehicle ownership as well as the commuting and employment outcomes for undocumented immigrants.

There are three primary findings. First, the license reforms significantly improve job accessibility of potentially undocumented immigrants, in turn increasing vehicle ownership and employment rates. Second, the effects of license reforms are stronger in localities with restricted job accessibility and in occupations that are more heavily car-dependent. Third, there is suggestive evidence of substitution between undocumented and documented workers in labor markets resulting from license reforms.

Many states are currently engaging extensive debate over the question of issuing driver's licenses to undocumented immigrants, a question that is directly related to the research question that motivates this paper. Moreover, this paper addresses the broader issue of the spatial mismatch problem among minorities, providing policy implications for improving their labor market outcomes. For example, some states have stopped suspending driver's licenses for unpaid parking tickets, which can have significant impacts on accessibility and labor market outcomes, especially for minorities.

Of course, to examine the overall welfare costs and benefits of license reforms, it is necessary to examine other outcomes such as crime and accident rates and administrative costs for issuing licenses. Also, the long-term effects of the reforms need to be explored in future work when sufficient post-reform outcomes can be observed. Yet, the evidence here suggests that minorities with driver's licenses not only drive more frequently, but also work more of the time when they own vehicles.

[43] I additionally analyze the impact of imposing only a documented presence requirement (instead of an SSN-or-documented-presence requirement). Detailed information on state driver's license requirements regarding documented presence or SSN requirements are obtained from the series "Overview State Driver's License Requirements" issued by the National Immigration Law Center (2002-2009), generously provided to the author by Richard Irwin. I find that the employment effects of imposing only documented presence requirements are also insignificant, consistent with the results reported in Table A21.

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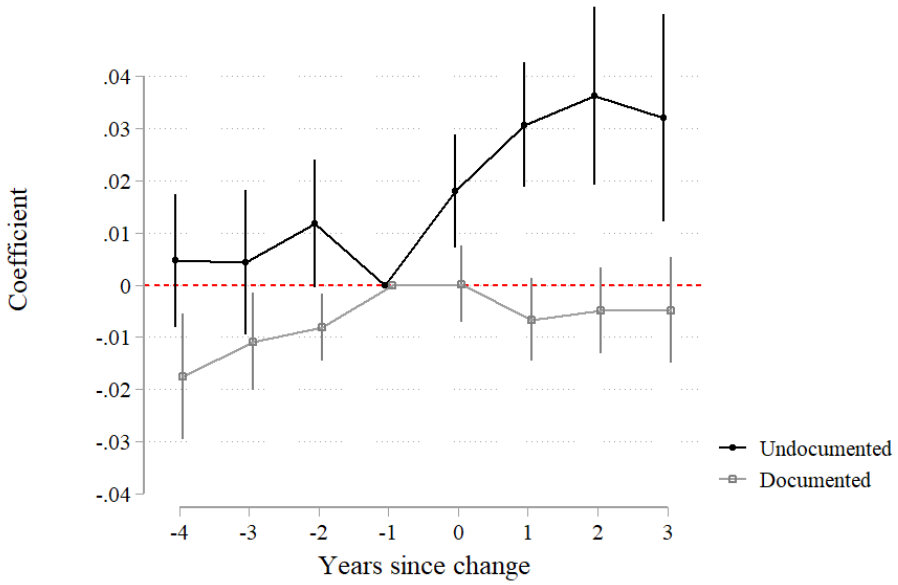
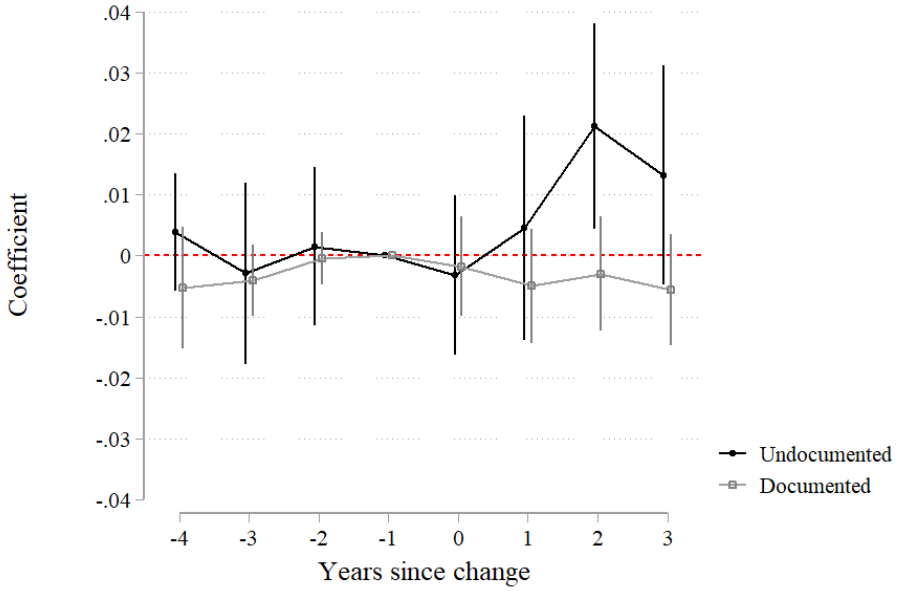
Figures and Tables

Figure 1: Event-Study Analysis of License Reforms

(a) Employed and Commuting by Car

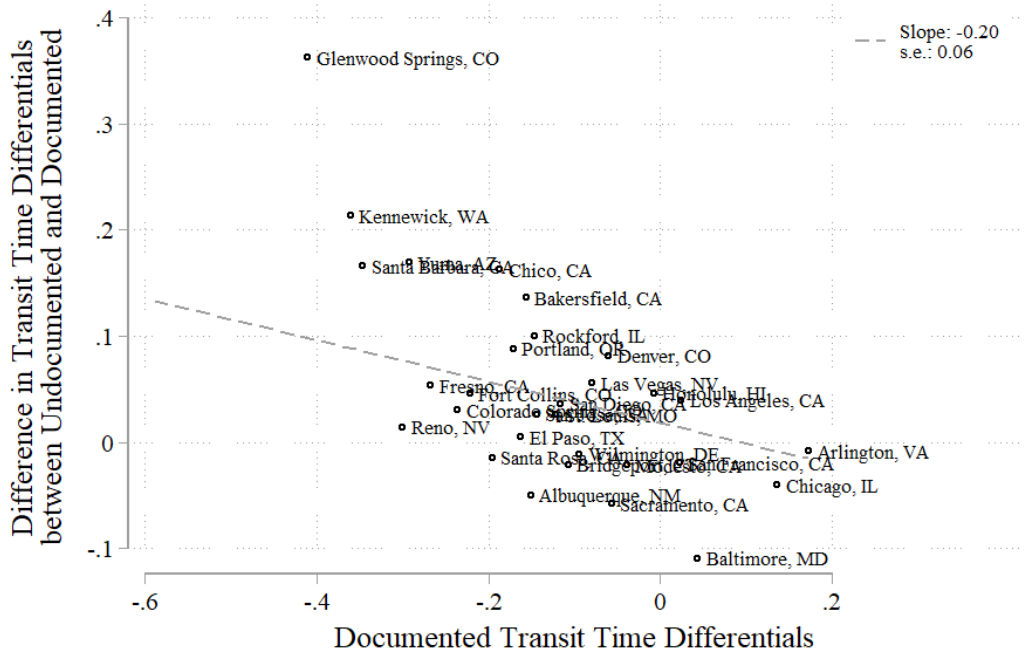


(b) Employed



Notes: Figures plot the event-study coefficients, τ^d, from equation 2. I run separate regressions for undocumented immigrants (black) and documented immigrants (gray). Standard errors are clustered by state. Vertical lines represent 95 percent confidence intervals. The event windows are -4 (4 years before the reforms) and 3 (3 years after the reform), and I bin-up the endpoints (-5 and 4, not reported). The outcome variables are indicators for being employed/commuting by car (a) and being employed (b).

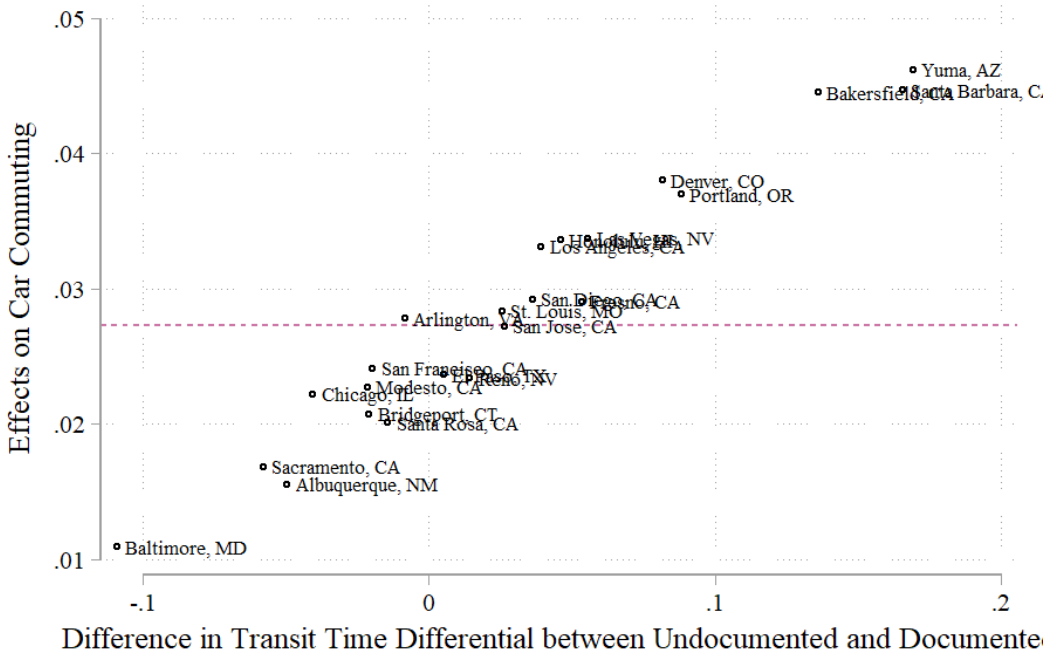
Figure 2: Difference between Commuting Times of Undocumented and Documented Immigrants



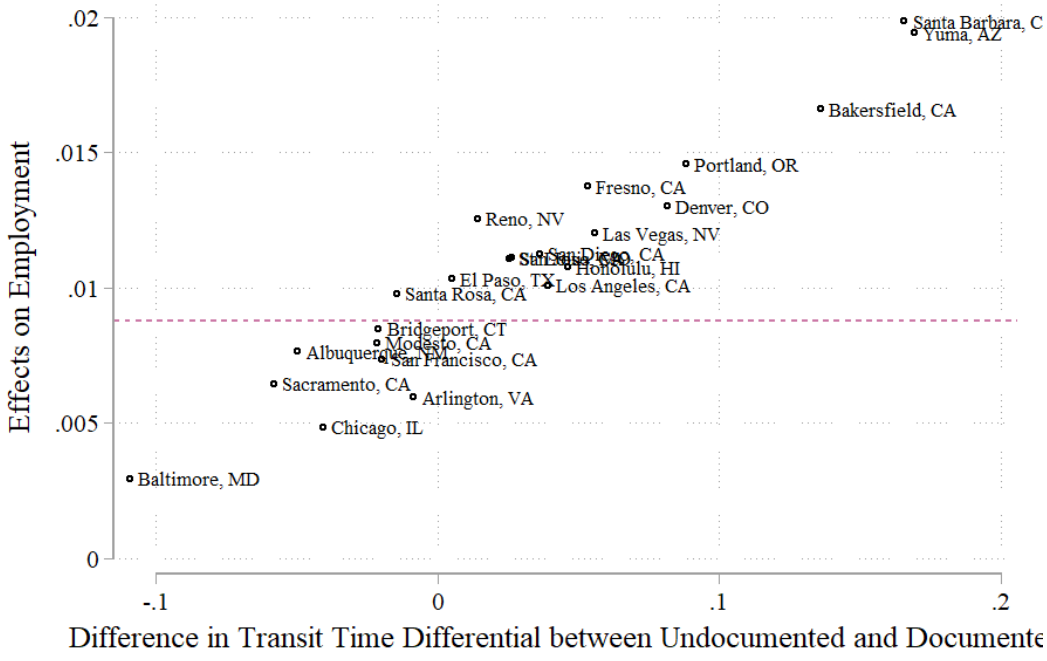
Notes: This figure depicts the commuting-zone-level commuting time differentials of undocumented and documented immigrants for 2000-2011. The commuting-time differentials are de-meaned transit time, one for each commuting zone, obtained from a regression of the square root of commuting time, controlling for worker characteristics and occupations. The Y-axis represents the measure of accessibility for undocumented immigrants, which is the difference between the commuting times differentials of undocumented and documented workers ($\hat{f_{0c}} - \hat{f_{1c}}$) in equation 4. The X-axis represents transit time differentials of documented immigrants ($\hat{f_{1c}}$) in equation 4. The dots represent commuting zones in states that have passed license reforms, with more than population of undocumented immigrants above 5,000. I use the largest city in each commuting zone as a label. The fitted line (dashed line) is obtained using commuting-zone populations as weights.

Figure 3: Effects of License Reforms by Commuting-Zone-level Accessibility for Undocumented Immigrants

(a) Employed and Commuting by Car

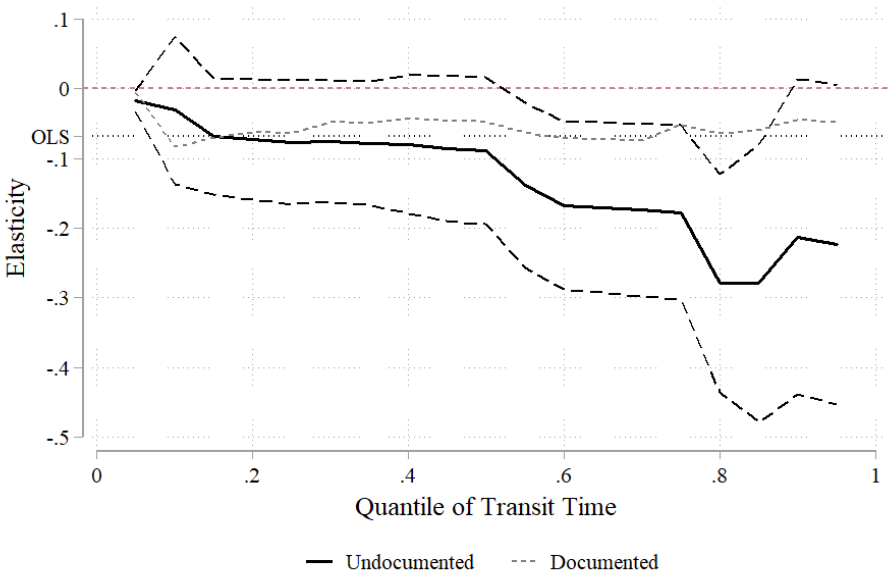


(b) Employed



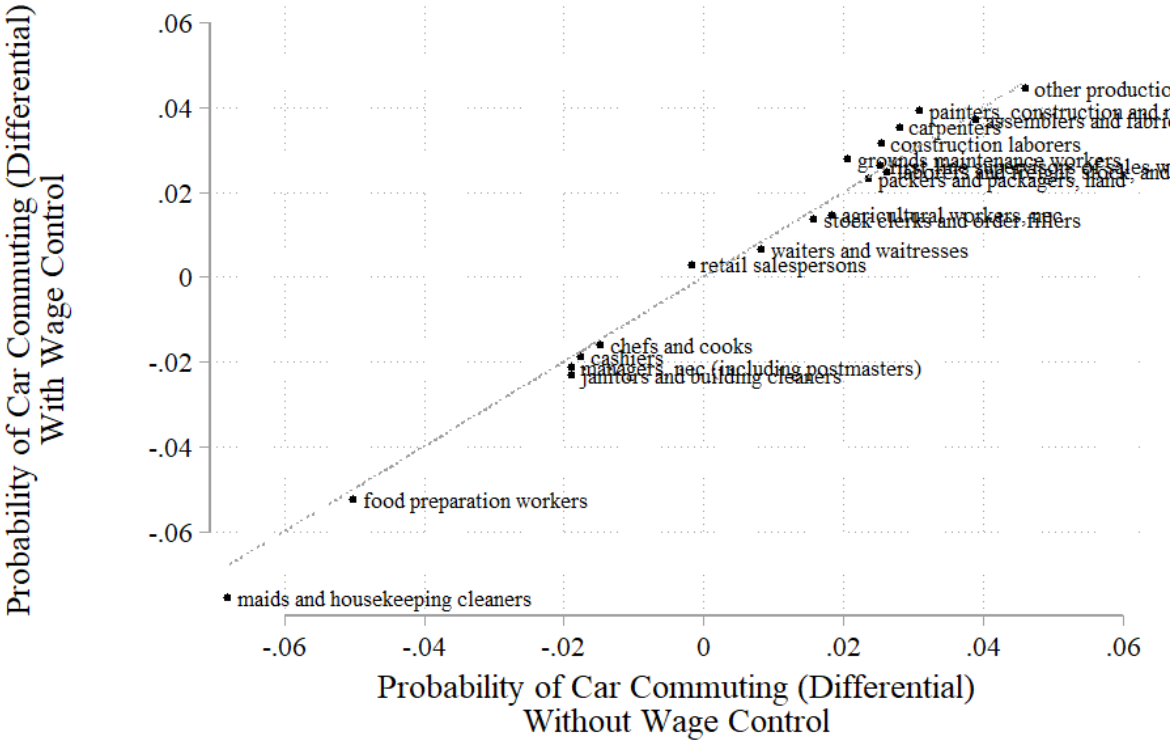
Notes: These figures depict the estimated commuting effects (a) and employment effects (b) of license reforms by commuting zone, using results reported in column 3 of Table 6. The X-axis represents the measure of accessibility for undocumented immigrants, which measures the differences between commuting time differentials of undocumented immigrants and documented immigrants ($\hat{f_{0c}} - \hat{f_{1c}}$) in equation 4. I use the largest city in each commuting zone as a label.

Figure 4: Unconditional Effects on Transit Time of Undocumented Immigrants



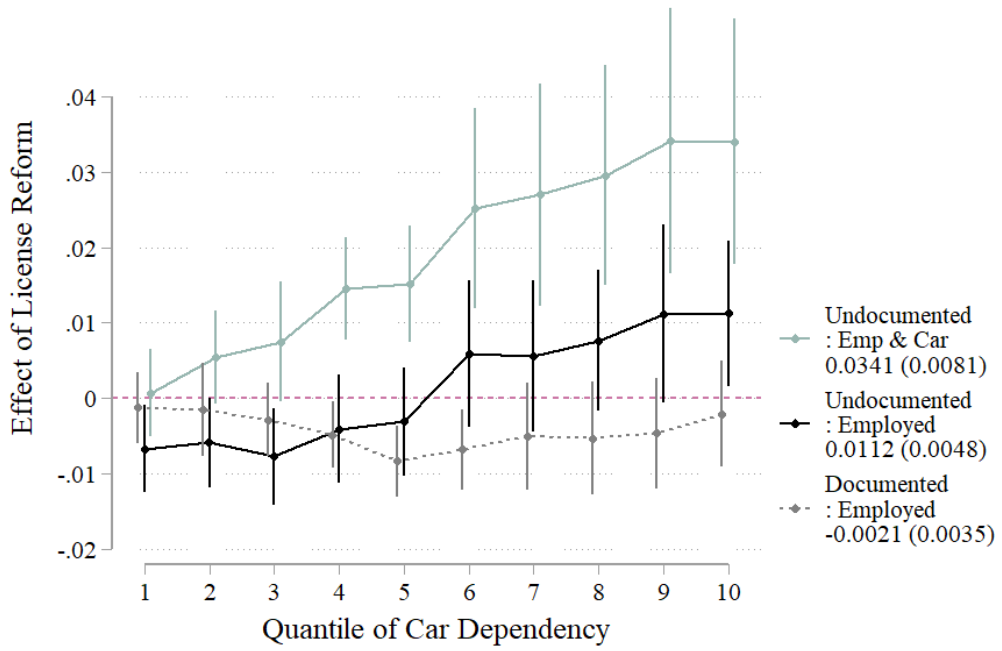
Notes: The above figure plots the coefficients of license reforms by (unconditional) quantile of transit time. The outcome variable is the recentered influence function (RIF) of the square root of transit time, shown in Equation 6. The density of y at each quantile is estimated using the Epanechnikov kernel with a bandwidth of 0.5. Black and gray lines represent the coefficients of undocumented and documented immigrants, respectively. The horizontal dotted lines represents OLS coefficients for undocumented immigrants. Robust standard errors are clustered by state. The dashed lines represent 95 percent confidence intervals for undocumented immigrants.

Figure 5: Car-Dependency by Occupation



Notes: This figure plots the probability of car commuting (differentials relative to the national average for documented immigrants) for each occupation using documented immigrant samples from 2000 to 2011. The X-axis represents the differentials without wage controls ($\hat{\phi_0}$ in equation 7, preferred specification), and the Y-axis represents the estimated differentials while controlling for the log of individual wages. The line in the graph is a 45-degree line. I show only occupations with large numbers of undocumented workers.

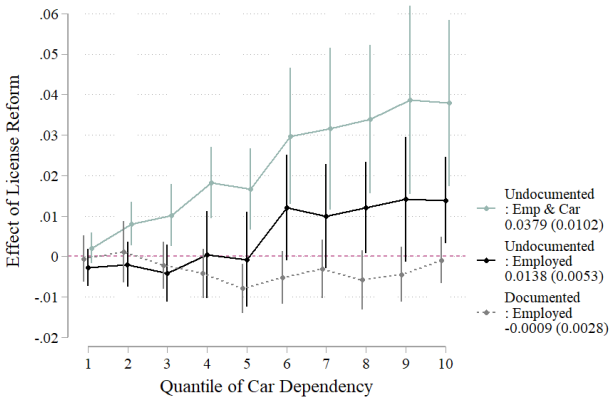
Figure 6: Effects of License Reforms by Occupation-Related Car-Dependency



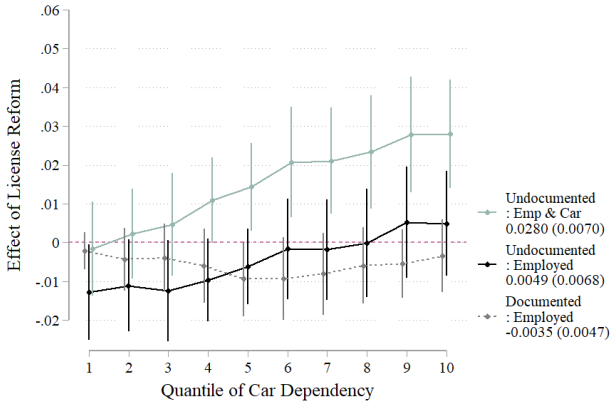
Notes: The above figures depict the effects of license reforms by quantile of occupation-related car dependency. Standard errors are clustered by state. The vertical lines represent 95% confidence intervals. I estimate 8, where the outcome variables are employed (or commuting by car) in occupations where car-dependency is less than or equal to the q quantile. The numbers in the legend show the coefficients and standard errors (in parentheses) of each outcome variables when $q = 10$, representing the effects on the entire population.

Figure 7: Heterogeneous Effects of License Reforms by Occupation-Related Car-Dependency

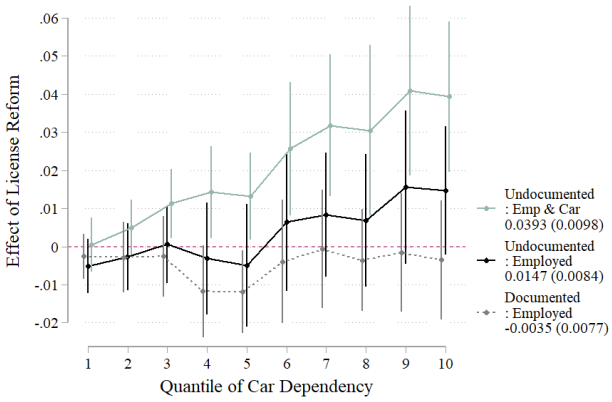
(a) Male



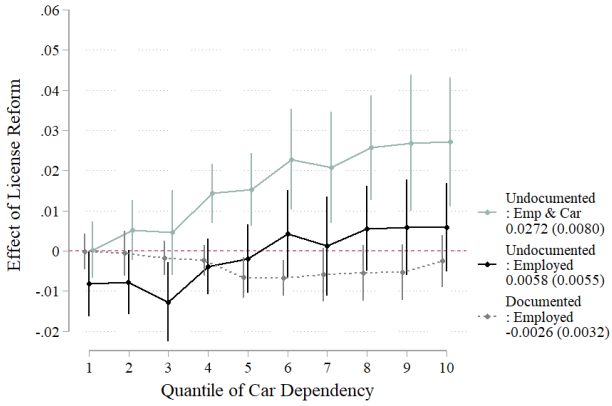
(b) Female



(c) High School Dropouts



(d) High School or Higher



Notes: The above figures depict the effects of license reforms by quantile of occupation-related car dependency, separately by subgroup. Standard errors are clustered by state. The vertical lines represent 95% confidence intervals. In all figures, I estimate equation 8, where the outcome variables are being employed (or commute by car) in occupations with car-dependency less than or equal to the q quantile. The numbers in the legends show the coefficients and standard errors (in parentheses) when $q = 10$.

Table 1: States that Enacted Driver's License Reforms Targeting Undocumented Immigrants (2000-2018)

State	Start License
Tennessee*	2001
New Mexico	2003
Maine*	2005
District of Columbia	2014
Illinois	2014
Maryland	2014
Nevada	2014
Vermont	2014
California	2015
Colorado	2015
Connecticut	2015
Delaware	2016
Hawaii	2016

Notes: The data are taken from http://www.ncsl.org/research/immigration/states-offering-driver-s-licenses-to-immigrants.aspx and Tirado-Alcaraz (2016). Start License indicates the first year when undocumented immigrants are allowed to obtain driver's licenses by removal of documented presence requirements. If the reforms are enacted during the second half of the year (Washington, Illinois, Colorado, Delaware), start years are assumed to be the next year of the reform. In the analysis, I drop Tennessee and Maine (starred) from the samples, as those states stopped issuing licenses to undocumented immigrants a few years after the license reforms were passed.

Table 2: Summary Statistics (ACS, 2000-2018)

	Undocumented Immigrants		Documented Immigrants		Natives	
	Mean	SD	Mean	SD	Mean	SD
ACS and Census (2000-2018)						
Female	0.44		0.52		0.50	
Age	33.9	9.4	38.5	9.5	35.9	10.8
Married	0.42		0.63		0.45	
Number of Children	1.07	1.33	1.25	1.30	0.85	1.15
Education						
Less than High School	0.44		0.21		0.10	
High School Graduates	0.25		0.22		0.29	
Some College	0.15		0.24		0.35	
Bachelor's Degree or Higher	0.17		0.33		0.27	
Number of Vehicles (Per Person)	0.47	0.36	0.59	0.37	0.75	0.44
Number of Vehicles (Per Household)	1.51	1.07	1.85	1.07	1.95	1.10
Percent with Car in Households	0.85		0.92		0.94	
Employed	0.69		0.74		0.74	
Wage Employed	0.64		0.69		0.70	
Self Employed	0.05		0.05		0.04	
Employed and Commute by Car	0.52		0.59		0.64	
Employed and Commute without Car	0.16		0.12		0.09	
Transit Time (Minutes)	26.6	22.4	27.0	23.0	23.7	22.0
Usual Hours of Work (per week)	39.5	10.7	40.3	11.5	40.0	12.0
Weeks Worked (per year)	46.2	11.6	47.5	10.0	47.3	10.3
Wage Income	34,088	43,361	52,709	59,358	50,135	54,880
Observations	1,350,666		3,002,615		25,658,862	

Notes: In the table above I report descriptive statistics from the Census (2000) and the American Community Survey (2001-2018). I restrict the samples to individuals aged 18-54 years. I classify the samples into potentially undocumented immigrants, documented immigrants, and natives. Refer to Section 4 for the classification of undocumented and documented immigrants. Sample statistics are weighted by the applicable Census or ACS weights. Wage income is reported in 2018 dollars.

Table 3: Effects of License Reforms on Vehicle Ownership

	Undocumented Immigrants		Documented Immigrants		Natives
	(1)	(2)	(3)	(4)	(5)
	Panel A: Number of Vehicles in Households				
License Reform	0.052**	0.056***	0.017	-0.002	0.000
	(0.025)	(0.019)	(0.013)	(0.008)	(0.007)
Mean	1.48	1.48	1.82	1.82	1.93
	Panel B: Having a Vehicle in Households				
License Reform	0.021***	0.025***	0.003	0.006	0.000
	(0.008)	(0.008)	(0.003)	(0.004)	(0.002)
Mean	0.83	0.83	0.91	0.91	0.93
State Fixed Effects	Y		Y		Y
Time Fixed Effects	Y		Y		Y
State x Time Fixed Effects		Y		Y	
State x Natives Fixed Effects		Y		Y	
Natives x Time Fixed Effects		Y		Y	

Notes: Robust standard errors are clustered by state and shown in parenthesis. The sample includes potentially undocumented immigrants (columns 1-2), documented immigrants (columns 3-4) and natives (column 5) in the Decennial Census (2000) and the American Community Survey (2001-2018). Refer to Section 4 for classification of undocumented and documented immigrants. The regressions are weighted by the applicable Census and ACS weights divided by the number of times each household appears in the samples. The means of outcome variables are calculated by excluding reformed states in the post-reform periods. Outcome variables used for Panels A and B are the number of vehicles in households and an indicator of having at least one vehicle in households, respectively. To obtain the results reported in columns 1, 3 and 5, I estimate equation 1 with state and time fixed effects. I additionally control for the baseline state-level characteristics (the share of immigrants with college degrees in 2000-2002 and the average state partisanship: see Section 5) interacted with year fixed effects. In columns 2 and 4, I estimate equation 3 with state-by-time, state-by-reweighted- natives and reweighted-natives-by-time fixed effects. Refer to Section 4 for the construction of reweighted native samples.

Table 4: Effects of License Reforms on Commuting and Employment

	Undocumented Immigrants			Documented Immigrants		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Employed	All	All	Employed
	Panel A: Employed and Commuting by Car					
License Reform	0.0341***	0.0248***	0.0359***	0.0041	-0.0010	0.0038
	(0.0081)	(0.0050)	(0.0100)	(0.0033)	(0.0034)	(0.0052)
Mean	0.51	0.51	0.74	0.59	0.59	0.80
	Panel B: Employed					
License Reform	0.0112**	0.0081*		-0.0021	-0.0001	
	(0.0048)	(0.0044)		(0.0035)	(0.0042)	
Mean	0.69	0.69		0.74	0.74	
State Fixed Effects	Y		Y	Y		Y
Time Fixed Effects	Y		Y	Y		Y
State x Time Fixed Effects		Y			Y	
State x Natives Fixed Effects		Y			Y	
Natives x Time Fixed Effects		Y			Y	

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants (columns 1-3) and documented immigrants (columns 4-6) in the Decennial Census (2000) and the American Community Survey (2001-2018). I further restrict the samples to employed individuals to obtain the results reported in columns 3 and 6. The regressions are weighted by the applicable Census, ACS weights. Outcome variables for Panels A and B are indicators for being employed and commuting by car, and being employed, respectively. The means of outcome variables are calculated while excluding reformed states in post-reform periods. To obtain the results reported in columns 1, 3, 4, and 6, I estimate equation 1 with state and time fixed effects. I additionally control for the baseline state-level characteristics (the share of immigrants with college degrees in 2000-2002 and the average state partisanship: see Section 5) interacted with year fixed effects. To obtain the results reported in columns 2 and 5, I estimate equation 3 with state-by-time, state-by-reweighted-natives and reweighted-natives-by-time fixed effects.

Table 5: Effects of License Reforms by Urban and Rural Area

	Undocumented		Documented	
	(1)	(2)	(3)	(4)
	All	Employed	All	Employed
Panel A: Employed and Commuting by Car				
License Reform × Rural	0.046***	0.040**	0.001	0.002
	(0.014)	(0.017)	(0.016)	(0.022)
License Reform × Urban	0.026***	0.028***	0.005	0.003
	(0.008)	(0.009)	(0.003)	(0.005)
Mean: Rural	0.55	0.83	0.64	0.88
Mean: Urban	0.50	0.73	0.59	0.79
Panel B: Employed				
License Reform × Rural	0.024**		-0.002	
	(0.009)		(0.005)	
License Reform × Urban	0.007		-0.001	
	(0.005)		(0.004)	
Mean: Rural	0.65		0.73	
Mean: Urban	0.69		0.74	
State Fixed Effects	Y	Y	Y	Y
Time × Urban Status Fixed Effects	Y	Y	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants (columns 1-2) and documented immigrants (column 3-4) in the Decennial Census (2000) and the American Community Survey (2005-2018). The ACS does not report information about the PUMA where respondents live for 2001-2004 data. The individual observations in the samples are split into two sets (urban and rural), and their Census/ACS weights are multiplied by the fraction of the population in a PUMA that belongs to urban and rural blocks, respectively. The crosswalk between the PUMA (Public Use Micro Area) and urban/rural status is provided by Marble/Geocorr 14k (Missouri Census Data Center 2018). To obtain the results reported in all columns, I estimate equation 1, interacting license reforms with urban/rural status and adding urban status by year fixed effects. The outcome variables are being employed and commuting by car (Panel A), and being employed (Panel B). For columns 2 and 4, I restrict the samples are restricted to employed respondents.

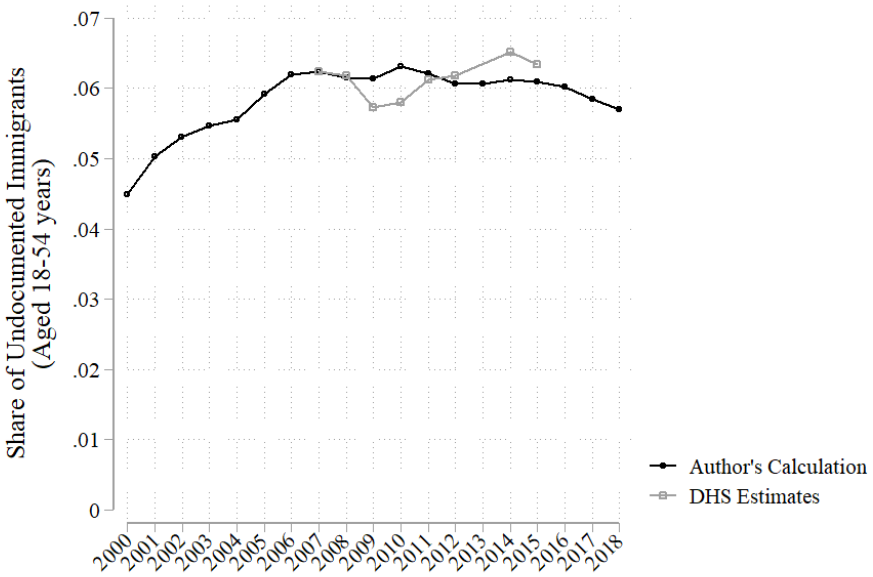
Table 6: Effects of License Reforms by Commuting-Zone-level Accessibility

	Undocumented			Documented		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Employed and Commuting by Car						
License Reforms	0.027***	0.026***	0.026***	0.004	0.004	0.004
	(0.008)	(0.007)	(0.007)	(0.003)	(0.004)	(0.004)
× Transit Time Gap		0.135***	0.152**		0.054	0.052
		(0.045)	(0.070)		(0.052)	(0.054)
× Transit Time of Documented			0.018			-0.002
			(0.039)			(0.016)
Panel B: Employed						
License Reforms	0.009*	0.008*	0.009*	-0.001	-0.001	-0.001
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
× Transit Time Gap		0.057***	0.047***		-0.028	-0.032
		(0.020)	(0.016)		(0.023)	(0.031)
× Transit Time of Documented			-0.011			-0.005
			(0.022)			(0.014)
State Fixed Effects	Y	Y	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y
CZ Fixed Effects	Y	Y	Y	Y	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants (columns 1-3) and documented immigrants (columns 4-6) in the Decennial Census (2000) and the American Community Survey (2005-2018). ACS data from 2001 through 2004 are omitted because the Public Use Microdata Area (PUMA) variable is missing. The individual observations in the samples are split into one per commuting zone, and their Census/ACS weights are multiplied by this fraction of the population in the PUMA that belongs to each commuting zone (Autor, Dorn, and Hanson 2013). I estimate equation 4, where the outcome variables are commuting by car (Panel A) and being employed (Panel B). For all columns, I control for commuting-zone fixed effects, state fixed effects, time fixed effects, and baseline state-level characteristics interacted with time fixed effects. To obtain the results reported in columns 2 and 5, I interact the commuting-zone-level accessibility measure for undocumented immigrants ($\hat{f_{0c}} - \hat{f_{1c}}$) with license reforms. The accessibility measure is calculated using the difference between undocumented and documented workers' transit times, while controlling for observable characteristics and occupations. To obtain the results reported in columns 3 and 6, I additionally interact transit-time differentials of documented immigrants in each commuting zone ($\hat{f_{1c}}$) with license reforms.

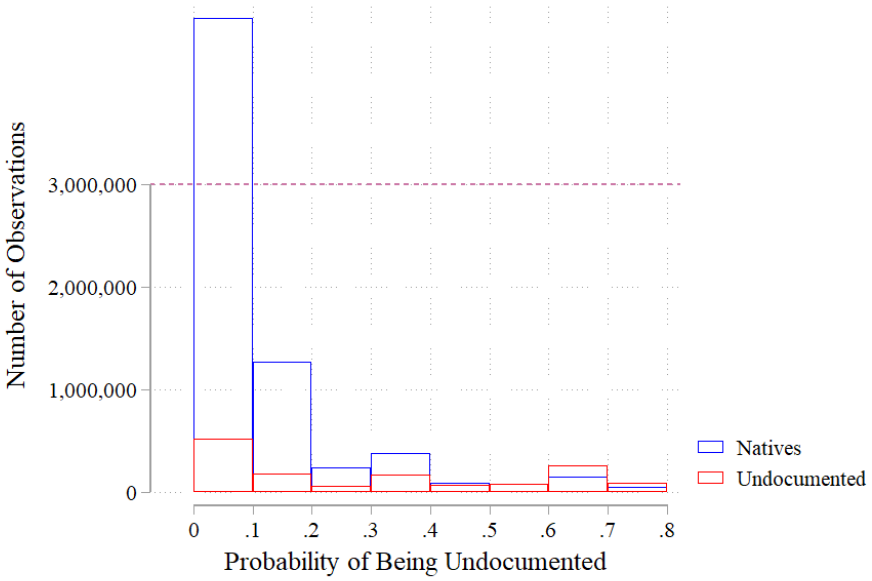
Appndix Tables and Figures

Figure A1: Share of Undocumented Immigrants Estimated by Author and DHS (Aged 18-54 years)



Notes: The black line represents the share of undocumented immigrants in the working age population (aged 18-54 years), which is estimated by the author using the Census/ACS. Refer to Section 4 for details regarding methodology for identifying undocumented immigrants. The gray line represents the same statistic as before, but is estimated by the Department of Homeland Security (https://www.dhs.gov/immigration-statistics/population-estimates/unauthorized-resident).

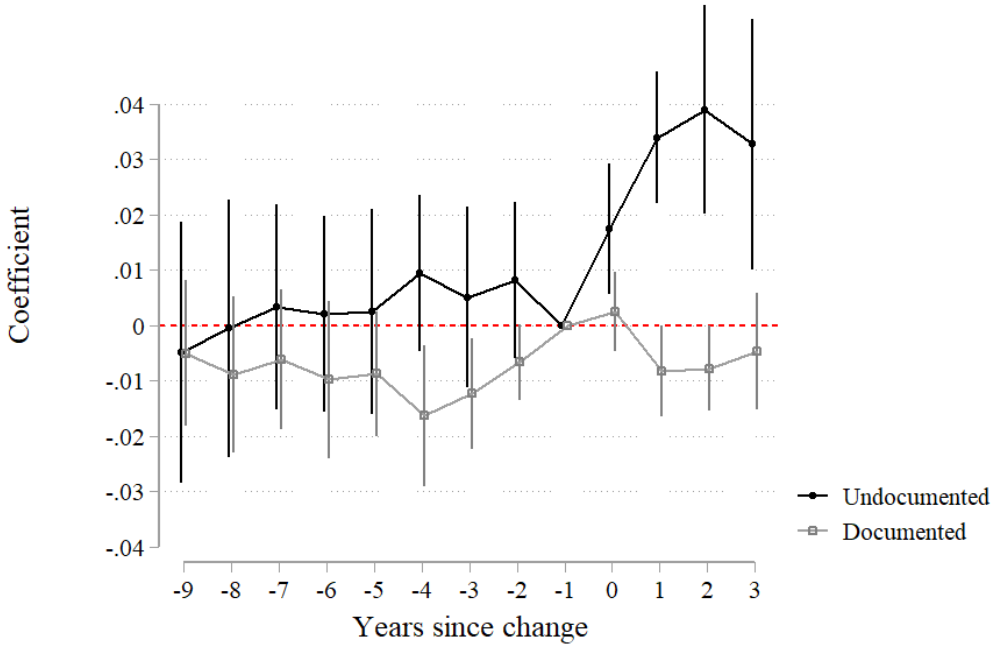
Figure A2: Distribution of Predicted Probability of Being an Undocumented Immigrant



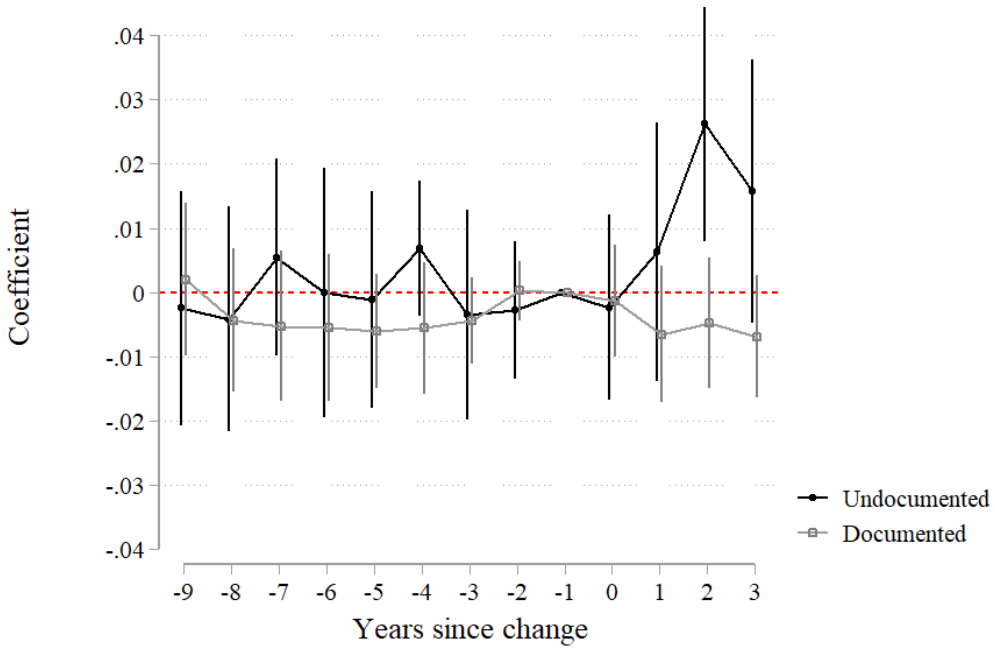
Notes: The probability of being an undocumented immigrant, $\hat{p}$ is predicted from a regression with fully saturated age groups (18-24, 25-29, 30-34, 35-39, 40-44, 45-49 and 50-54 years of age), sex (male and female), education (high school dropouts, high school graduates, some college, and BA or beyond) and Hispanic origin indicators. Blue and red bars represent natives and undocumented immigrants, respectively.

Figure A3: Event-Study Analysis of License Reforms with Balanced Samples

(a) Employed and Commuting by Car

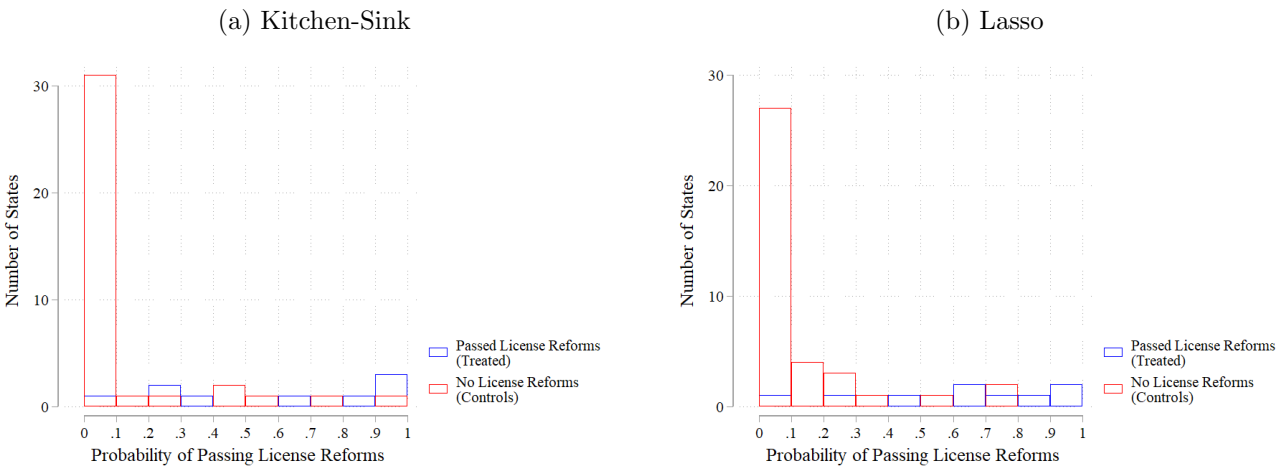


(b) Employed



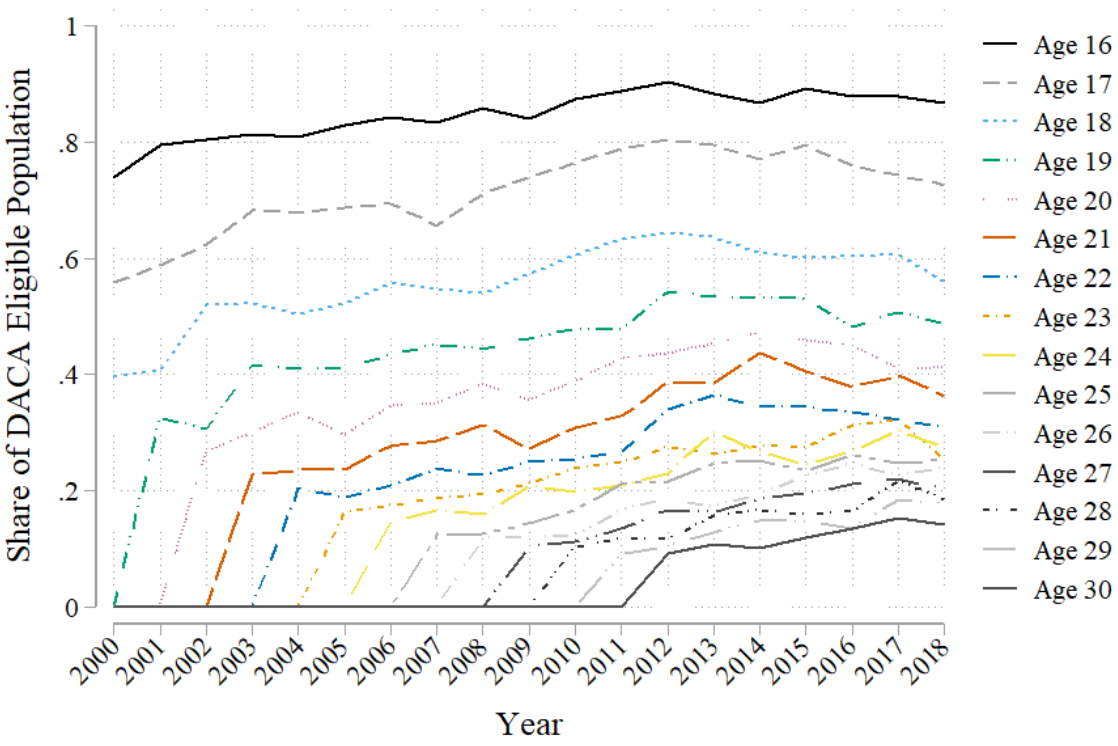
Notes: The above figures plot the event-study coefficients, τ^d, from equation 2. I run separate regressions for undocumented immigrants (black) and documented immigrants (gray). Standard errors are clustered by state. Vertical lines represent 95 percent confidence intervals. The event windows range from -9 to 3, dropping unbalanced states (states with of less than nine years of pre-periods or of three years post-periods) and unbalanced years (event years outside of the event window). The outcome variables are indicators for being employed/commuting by car (a) and being employed (b).

Figure A4: Propensity Scores of Passing License Reforms



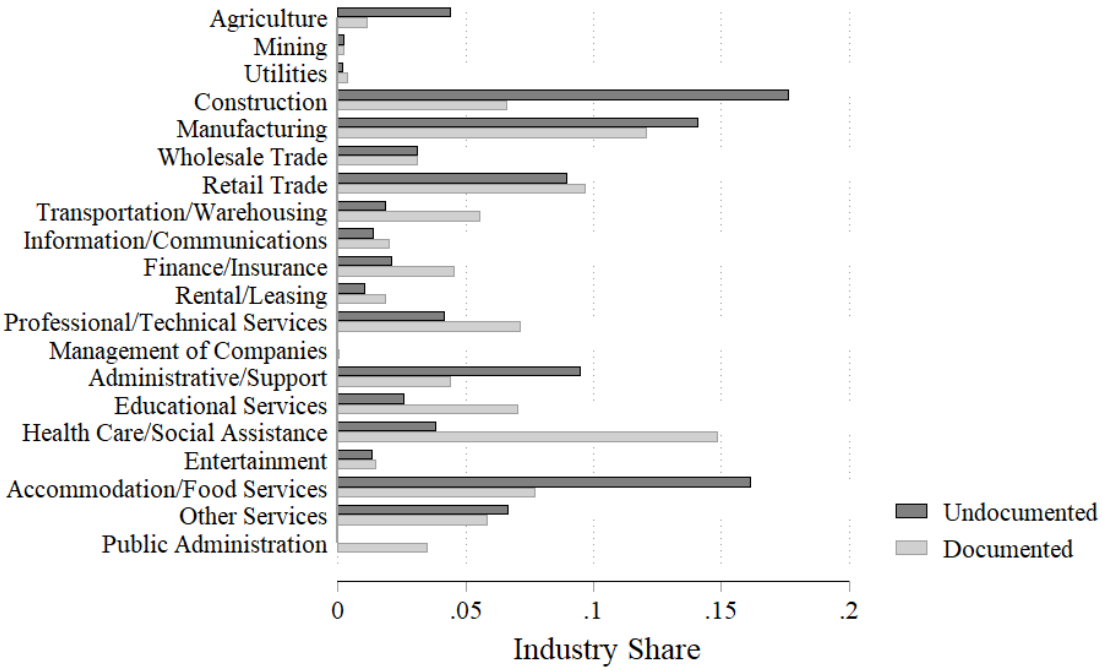
Notes: These figures plot the distribution of the propensity scores that have implemented the license reforms. The blue bars represent treated states (have implemented license reforms), whereas the red bars represent untreated states (have not implemented license reforms). (a) is derived using the kitchen sink method to predict license reforms based on the following state-level characteristics in 2000: share of construction/manufacturing/documented immigrants/undocumented immigrants/high school dropouts/college graduates/females in labor force, probability of commuting by car, log of state population, and party in control of state legislature (Democratic, Republican, or Split). (b) uses the variables selected from the LASSO procedure: party in control and share of undocumented immigrants.

Figure A5: Share of DACA-Eligible Undocumented Immigrants by Age Group



Notes: The lines represent the share of DACA-eligible population by age group. The shares are calculated using the applicable Census/ACS weights. The DACA-eligible population is defined as undocumented immigrants who are: (1) born after 1981, (2) came to the US before age 16 years, and (3) have completed high school/GED or currently enrolled in school.

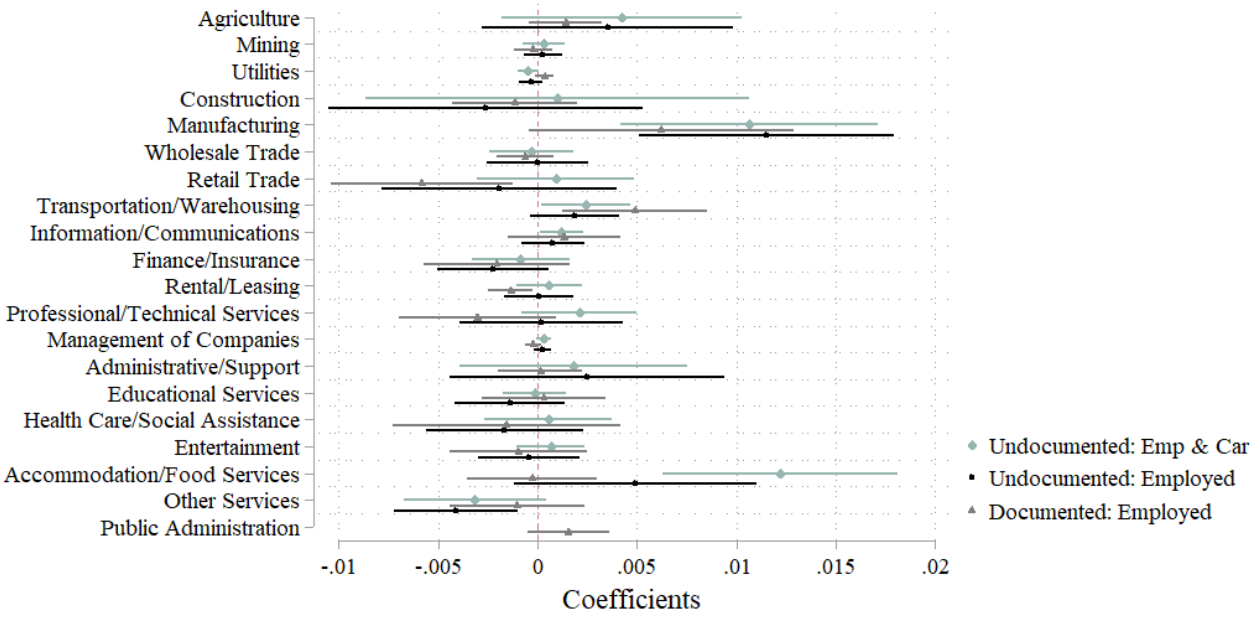
Figure A6: Industry Share of Undocumented and Documented Immigrants



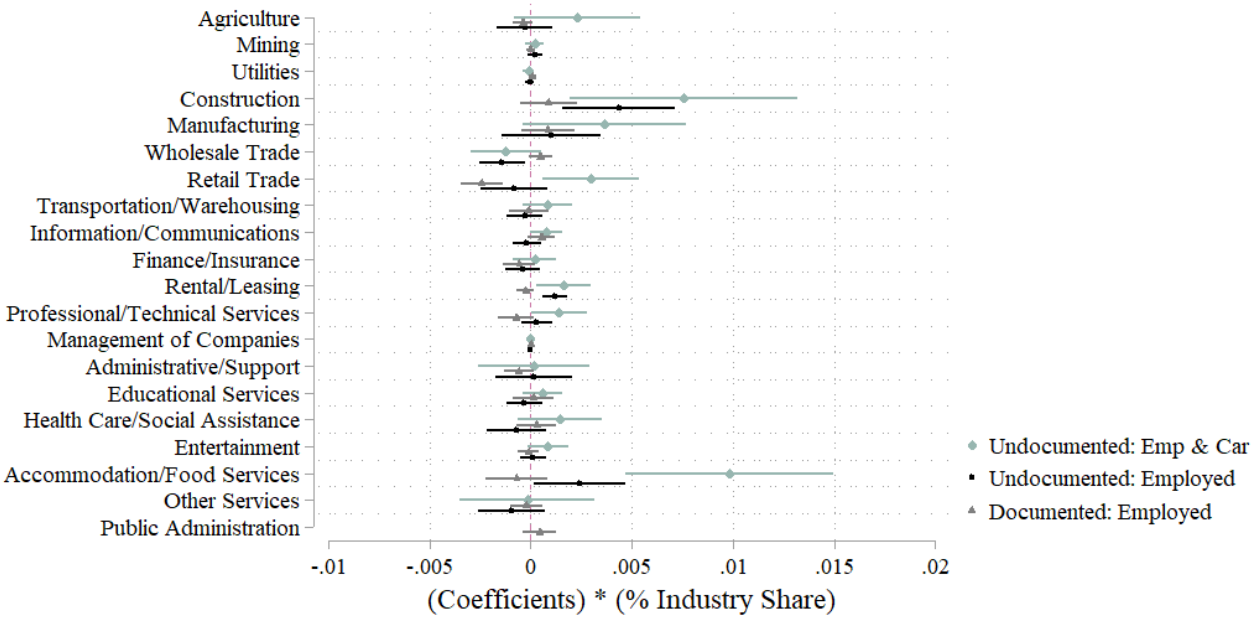
Notes: Industry shares are calculated separately for undocumented immigrants (black) and documented immigrants (gray) using the Census (2000) and the American Community Survey (2001-2018). The list of industries are taken from the North American Industry Classification System (NAICS).

Figure A7: Effects of License Reforms by Industry

(a) Effects on Industry Employment



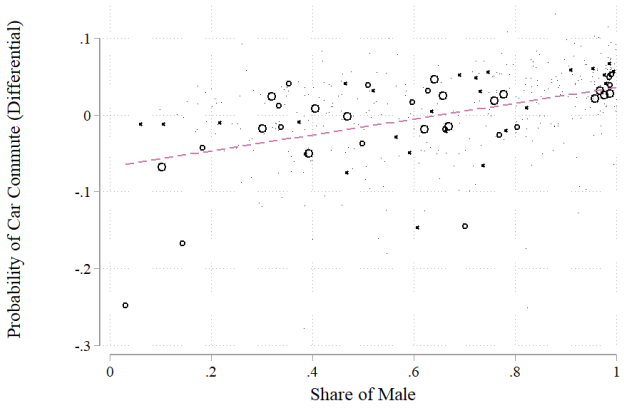
(b) Effects on Employment Within Industry (with industry fixed effects)



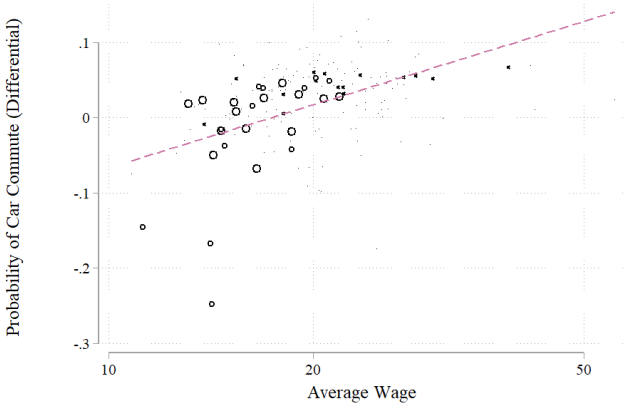
Notes: Robust standard errors are clustered by state. The lines are 95 percent confidence intervals. Panel (a) plots β in equation 1 using all the samples, where the outcome variables are: undocumented immigrants commuting by car and employed in industry d (blue/diamond), undocumented immigrants employed in industry d (black/circle), and documented immigrants employed in industry d (gray/triangle). Panel B plots $s_d \cdot \hat{\beta}$, where s_d is the share of industry d by nativity (Figure A6), and $\hat{\beta}$ is estimated from equation 1 for each industry d. The outcome variables for Panel (b) are: undocumented immigrants commuting by car and being employed (blue/diamond), undocumented immigrants being employed (black/circle), and documented immigrants being employed (gray/triangle).

Figure A8: Occupation-Related Car-Dependency, Gender, and Wages

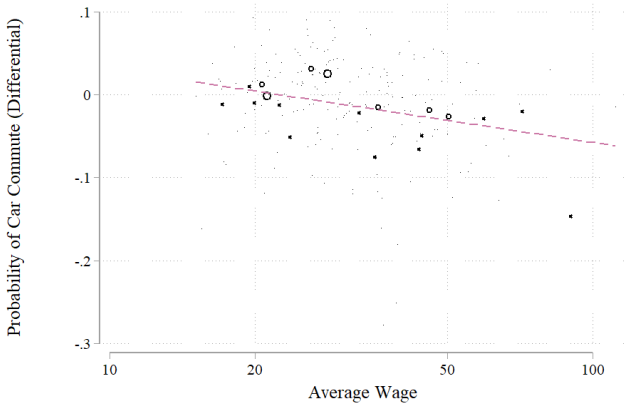
(a) Share Males



(b) Wages: Low-Skilled Occupations

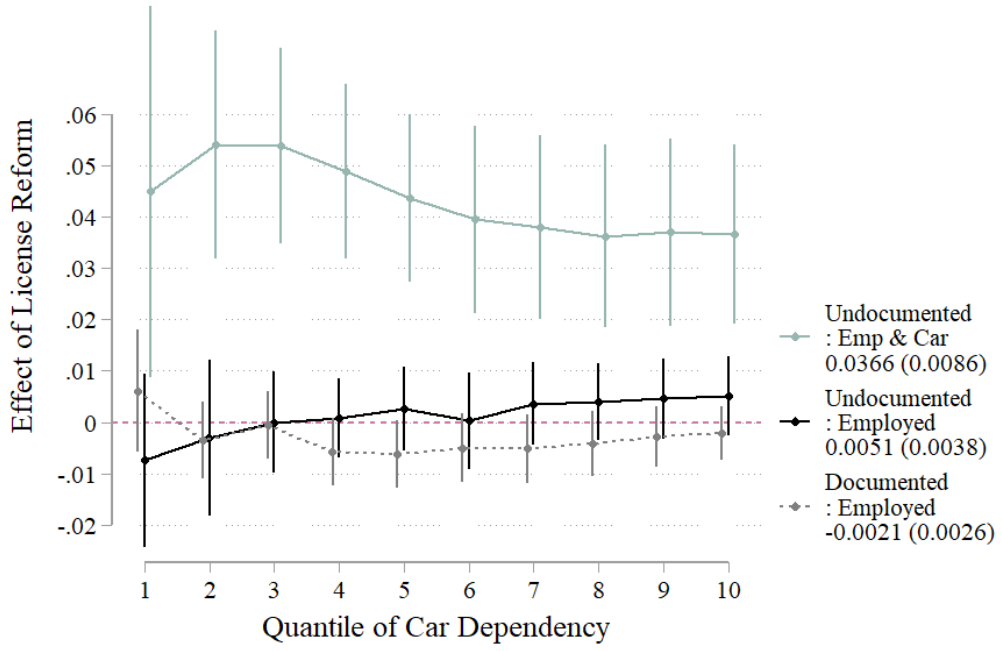


(c) Wages: High-Skilled Occupations



Notes: Occupation-level variables are constructed using documented workers (2000-2011). Occupations are low-skilled if the share of workers with high school degrees or lower is more than 0.5. The remaining are high-skilled occupations. The fitted line is estimated using the number of documented workers in each occupation as a weight.

Figure A9: Within-Occupation Effects of License Reforms by Occupation-Related Car-Dependency



Notes: The above figures depict the effects of license reforms by quantile of occupation-related car dependency. Standard errors are clustered by state. The vertical lines represent 95% confidence intervals. I estimate equation 1 with occupation fixed effects, restricting my samples to individuals who have occupations less than or equal to the q quantile, where the outcome variables are employed (or commuting by car). The numbers in the legend show the coefficients and standard errors (in parentheses) of each outcome variables when $q = 10$, representing the effects on the entire population.

Table A1: Timeline of Driver's License Policies

State	Stop Licenses	Start Licenses
Alabama	1996	
Alaska	1998	
Arizona	<1999	
Arkansas	1997	
California	1991	2015
Colorado	<1994	2015
Connecticut	<1993	2015
Delaware	1997	2016
District of Columbia	<1994	2014
Florida	<1990	
Georgia	<1994	
Hawaii	2001	2016
Idaho	1992	
Illinois	1990	2014
Indiana	2001	
Iowa	<1990	
Kansas	<1993	
Kentucky	<1994	
Louisiana	<1990	
Maine	1997	
Maryland	2001	2014
Massachusetts	<1994	
Michigan	1998	
Minnesota	2003	
Mississippi	1997	
Missouri	1991	
Montana	2000	
Nebraska	<1990	
Nevada	<1990	2014
New Hampshire	<1990	
New Jersey	<1994	
New Mexico	<1990	2003
New York	1995	
North Carolina	2006	
North Dakota	1999	
Ohio	1998	
Oklahoma	<1994	
Oregon	2008	
Pennsylvania	<1990	
Rhode Island	2004	
South Carolina	<1990	
South Dakota	<1990	
Tennessee	<1990	2001
Texas	1999	
Utah	1993	1999
Vermont	2003	2014
Virginia	<1990	
Washington	<1990	1993
West Virginia	2003	
Wisconsin	<1990	
Wyoming	<1990	

Notes: The data are primarily taken from Cáceres and Jameson (2015) and http://www.ncsl.org/research/immigration/states-offering-driver-s-licenses-to-immigrants.aspx. Start year indicates the first year when undocumented immigrants are allowed to obtain driver's licenses. Stop year indicates the first year when either the SSN or the documented presence restriction was put into effect. The data are not available for some years or states in the pre-2000 periods. In that case, I manually searched newspaper articles regarding the license restrictions in the missing states and years using Newsbank (https://www.newsbank.com/).

Table A2: Effects of License Reforms on Wage-Employment and Self-Employment

	Undocumented Immigrants			Documented Immigrants		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Employed	All	All	Employed
	Panel A: Wage-Employed					
License Reform	0.0139***	0.0100***	0.0045	-0.0030	0.0007	-0.0010
	(0.0049)	(0.0033)	(0.0028)	(0.0039)	(0.0042)	(0.0021)
Mean	0.64	0.64	0.93	0.68	0.68	0.93
	Panel B: Self-Employed					
License Reform	-0.0027	-0.0019	-0.0045	0.0009	-0.0008	0.0010
	(0.0021)	(0.0022)	(0.0028)	(0.0014)	(0.0009)	(0.0021)
Mean	0.05	0.05	0.07	0.05	0.05	0.07
State Fixed Effects	Y		Y	Y		Y
Time Fixed Effects	Y		Y	Y		Y
State x Time Fixed Effects		Y			Y	
State x Natives Fixed Effects		Y			Y	
Natives x Time Fixed Effects		Y			Y	

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants (columns 1-3) and documented immigrants (columns 4-6) in the Decennial Census (2000) and the American Community Survey (2001-2018). I further restrict the samples to employed individuals to obtain the results reported in columns 3 and 6. The regressions are weighted by the applicable Census, ACS weights. Outcome variables for Panels A and B are indicators for being wage-employed and self-employed, respectively. The means of outcome variables are calculated while excluding reformed states in post-reform periods. To obtain the results reported in columns 1, 3, 4, and 6, I estimate equation 1 with state and time fixed effects. I additionally control for the baseline state-level characteristics (the share of immigrants with college degrees in 2000-2002 and the average state partisanship: see Section 5) interacted with year fixed effects. To obtain the results reported in columns 2 and 5, I estimate equation 3 with state-by-time, state-by-reweighted-natives and reweighted-natives-by-time fixed effects.

Table A3: Effects of License Reforms Employment of Natives

Dependent Variable: Employed	All (1)	HS Dropouts & Hispanic (2)	P-Score Reweighted (3)
License Reform	0.0052*	0.0037	0.0057
	(0.0028)	(0.0139)	(0.0075)
State Fixed Effects	Y	Y	Y
Time Fixed Effects	Y	Y	Y

Notes: Robust standard errors are clustered by states and shown in parentheses. I restrict my samples to the native population in the Decennial Census (2000) and the American Community Survey (2001-2018). To obtain the results reported in column 2, I use native Hispanics with high school degrees or lower. To obtain the results reported in column 3, I use reweighted natives that resemble the characteristics of documented immigrants (refer to Section 5 for the construction of reweighted natives). Outcome variable is an indicator variable for being employed, respectively. To obtain the results reported in all columns, I estimate equation 1 with state and time fixed effects. I additionally control for the baseline state-level characteristics (the share of immigrants with college degrees in 2000-2002 and the average state partisanship: see Section 5) interacted with year fixed effects.

Table A4: Effects of License Reforms on Car Pooling

	Undocumented Immigrants		Documented Immigrants	
	(1)	(2)	(3)	(4)
Panel A: Commuting by Car and Driving Alone				
License Reform	0.0321***	0.0215***	0.0062*	-0.0017
	(0.0075)	(0.0053)	(0.0033)	(0.0031)
Mean	0.36	0.36	0.50	0.50
Panel B: Commuting by Car and Carpooling				
License Reform	0.0021	0.0034	-0.0023	0.0005
	(0.0075)	(0.0034)	(0.0017)	(0.0016)
Mean	0.15	0.15	0.09	0.09
State Fixed Effects	Y		Y	
Time Fixed Effects	Y		Y	
State x Time Fixed Effects		Y		Y
State x Natives Fixed Effects		Y		Y
Natives x Time Fixed Effects		Y		Y

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants (columns 1-2) and documented immigrants (columns 3-4) in the Decennial Census (2000) and the American Community Survey (2001-2018). The regressions are weighted by the applicable Census and ACS weights. Outcome variables for Panels A and B are indicators for driving alone and carpooling. To obtain the results reported in columns 1 and 3, I estimate equation 1 with state and time fixed effects. To obtain the results reported in columns 2 and 4, I estimate equation 3 with state-by-time, state-by-reweighted-natives and reweighted-natives-by-time fixed effects.

Table A5: Effects of License Reforms on Employment among Low Educated Hispanics

	Undocumented Immigrants			Documented Immigrants		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Employed	All	All	Employed
	Panel A: Employed and Commuting by Car					
License Reform	0.0360***	0.0335***	0.0361***	0.0031	-0.0025	-0.0026
	(0.0087)	(0.0064)	(0.0134)	(0.0058)	(0.0043)	(0.0048)
Mean	0.54	0.54	0.76	0.57	0.57	0.83
	Panel B: Employed					
License Reform	0.0108	0.0111*		0.0011	-0.0065	
	(0.0068)	(0.0058)		(0.0061)	(0.0057)	
Mean	0.71	0.71		0.68	0.68	
State Fixed Effects	Y		Y	Y		Y
Time Fixed Effects	Y		Y	Y		Y
State x Time Fixed Effects		Y			Y	
State x Natives Fixed Effects		Y			Y	
Natives x Time Fixed Effects		Y			Y	

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants (for columns 1-3) and documented immigrants (for columns 4-6) in the Decennial Census (2000) and the American Community Survey (2001-2018). I restrict my samples to the Hispanic population with high school degrees or lower. I further restrict the samples to employed individuals in columns 3 and 6. The regressions are weighted by the applicable Census and ACS weights. Outcome variables for Panels A and B are indicators for being employed and commuting by car, and being employed, respectively. The means of outcome variables are calculated while excluding reformed states in post-reform periods. In columns 1, 3, 4, and 6, I estimate equation 1 with state and time fixed effects. I additionally control for the baseline state-level characteristics (the share of immigrants with college degrees in 2000-2002 and the average state partisanship: see Section 5) interacted with year fixed effects. To obtain the results reported in columns 2 and 5, I estimate equation 3 with state-by-time, state- by-reweighted-natives and reweighted-natives-by-time fixed effects.

Table A6: Effects of License Reforms on Mexican-born High School Dropouts Non-Citizens

	(1) All	(2) Employed
Panel A: Employed and Commuting by Car		
License Reform	0.0229**	0.0221
	(0.0092)	(0.0134)
Mean	0.53	0.81
Panel B: Employed		
License Reform	0.0088	
	(0.0059)	
Mean	0.66	
State Fixed Effects	Y	Y
Time Fixed Effects	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. The samples are Mexican-born non-citizen. I further restrict the samples to employed individuals for columns 2. The regressions are weighted by the applicable Census and ACS weights. Outcome variables for Panels A, B, and C are indicators for being employed and commuting by car, being employed and commuting without car, and being employed, respectively. The means of outcome variables are calculated excluding reformed states in post-reform periods. To obtain the results reported in all columns, I estimate equation 1 with state and time fixed effects.

Table A7: Effects of License Reforms on Employment Excluding California

	Undocumented Immigrants			Documented Immigrants		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Employed	All	All	Employed
Panel A: Employed and Commuting by Car						
License Reform	0.0318***	0.0204**	0.0302***	0.0052	-0.0032	0.0078
	(0.0079)	(0.0087)	(0.0090)	(0.0036)	(0.0060)	(0.0066)
Mean	0.51	0.51	0.74	0.58	0.58	0.79
Panel B: Employed						
License Reform	0.0129**	0.0049		-0.0044	-0.0085**	
	(0.0057)	(0.0086)		(0.0041)	(0.0042)	
Mean	0.69	0.69		0.74	0.74	
State Fixed Effects	Y		Y	Y		Y
Time Fixed Effects	Y		Y	Y		Y
State x Time Fixed Effects		Y			Y	
State x Natives Fixed Effects		Y			Y	
Natives x Time Fixed Effects		Y			Y	

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants (for columns 1-3) and documented immigrants (for columns 4-6) in the Decennial Census (2000) and the American Community Survey (2001-2018). I exclude samples from California. I further restrict the samples to employed individuals in columns 3 and 6. The regressions are weighted by the applicable Census and ACS weights. Outcome variables for Panels A and B are indicators for being employed and commuting by car, and being employed, respectively. The means of outcome variables are calculated while excluding reformed states in post-reform periods. In columns 1, 3, 4, and 6, I estimate equation 1 with state and time fixed effects. I additionally control for the baseline state-level characteristics (the share of immigrants with college degrees in 2000-2002 and the average state partisanship: see Section 5) interacted with year fixed effects. To obtain the results reported in columns 2 and 5, I estimate equation 3 with state-by-time, state- by-reweighted-natives and reweighted-natives-by-time fixed effects.

Table A8: Effects of License Reforms on Employment while Controlling for Stop License

	Undocumented Immigrants			Documented Immigrants		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Employed	All	All	Employed
	Panel A: Employed and Commuting by Car					
License Reform	0.0332***	0.0248***	0.0351***	0.0035	-0.0009	0.0038
	(0.0079)	(0.0050)	(0.0097)	(0.0034)	(0.0034)	(0.0053)
	Panel B: Employed					
License Reform	0.0106**	0.0079*		-0.0028	-0.0003	
	(0.0048)	(0.0043)		(0.0032)	(0.0041)	
State Fixed Effects	Y		Y	Y		Y
Time Fixed Effects	Y		Y	Y		Y
State x Time Fixed Effects		Y			Y	
State x Natives Fixed Effects		Y			Y	
Natives x Time Fixed Effects		Y			Y	

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants (for columns 1-3) and documented immigrants (for columns 4-6) in the Decennial Census (2000) and the American Community Survey (2001-2018). I further restrict the samples to employed individuals for columns 3 and 6. The regressions are weighted by the applicable Census and ACS weights. I additionally control for an indicator variable, imposing SSN or documented presence requirements on driver's licenses. Outcome variables for Panels A and B are indicators for being employed and commuting by car, and being employed, respectively. In columns 1, 3, 4, and 6, I estimate equation 1 with state and time fixed effects. I additionally control for the baseline state-level characteristics (the share of immigrants with college degrees in 2000-2002 and the average state partisanship: see Section 5) interacted with year fixed effects. To obtain the results reported in columns 2 and 5, I estimate equation 3 with state-by-time, state- by-reweighted-natives and reweighted-natives-by-time fixed effects.

Table A9: Effects of License Reforms, Controlling for Other Immigration Enforcement Policies

	Undocumented Immigrants			Documented Immigrants		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Employed and Commuting by Car						
License Reform	0.034***	0.028***	0.028***	0.004	0.003	0.003
	(0.008)	(0.008)	(0.008)	(0.003)	(0.004)	(0.003)
E-Verify for some hires		-0.012**	-0.012*		-0.007*	-0.008*
		(0.006)	(0.006)		(0.004)	(0.004)
E-Verify for all hires		-0.029***	-0.029***		-0.014*	-0.014*
		(0.008)	(0.009)		(0.007)	(0.007)
Prohibition on E-Verify		0.006	0.006		-0.001	-0.001
		(0.005)	(0.005)		(0.003)	(0.003)
State had Secure Communities			0.002			-0.001
			(0.005)			(0.002)
Mean	0.51	0.51	0.51	0.59	0.59	0.59
Panel B: Employed						
License Reform	0.011**	0.009	0.009	-0.002	-0.004	-0.004
	(0.005)	(0.005)	(0.005)	(0.004)	(0.003)	(0.003)
E-Verify for some hires		-0.009*	-0.008*		-0.005	-0.005
		(0.005)	(0.005)		(0.004)	(0.004)
E-Verify for all hires		-0.021**	-0.021**		-0.016**	-0.016**
		(0.009)	(0.009)		(0.006)	(0.006)
Prohibition on E-Verify		-0.002	-0.002		0.001	0.001
		(0.005)	(0.005)		(0.003)	(0.003)
State had Secure Communities			0.002			-0.000
			(0.004)			(0.002)
Mean	0.69	0.69	0.69	0.74	0.74	0.74
State Fixed Effects	Y	Y	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants (for columns 1-3) and documented immigrants (for columns 4-6) in the Decennial Census (2000) and the American Community Survey (2001-2018). The regressions are weighted by the applicable Census and ACS weights. In columns 2 and 3, I additionally include indicator variables for the following immigration enforcement policies: employers mandated to use E-Verify for some hires, employers mandated to use E-Verify for all hires, prohibition on local E-Verify mandates, and Secure Communities activated in the state. Outcome variables for Panels A and B are indicators for being employed and commuting by car and being employed, respectively. I estimate equation 1 with state and time fixed effects. I additionally control for the baseline state-level characteristics (the share of immigrants with college degrees in 2000-2002 and the average state partisanship: see Section 5) interacted with year fixed effects.

Table A10: Effects of License Reforms: Logit Estimates

	(1) Undocumented	(2) Documented
Panel A: Employed and Commuting by Car		
License Reform	1.180***	1.019
	(0.046)	(0.016)
Panel B: Employed		
License Reform	1.064**	0.986
	(0.029)	(0.022)
State Fixed Effects	Y	Y
Time Fixed Effects	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. I estimate equation 1 using logit estimation. The regressions are weighted by the applicable Census and ACS weights. The reported coefficients are odds ratios. Outcome variables for Panels A and B are indicators for being employed and commuting by car, and being employed, respectively.

Table A11: Predicting License Reforms from State-level Characteristics

Dependent Variable: Passed License Reforms	(1) All Variables	(2) Selected from Lasso
Number of Times with Democrats Majority in Senates	10.72	9.67***
	(9.16)	(2.81)
Number of Times with Democrats Majority in Houses	5.09	
	(6.78)	
Number of Times with Democrat Governor	0.56	
	(3.67)	
Log(Population)	-0.21	
	(1.21)	
Share Foreign Born	22.00	13.89**
	(21.46)	(5.45)
Employment Rate	7.02	
	(42.14)	
Share College Graduates	-17.49	
	(13.85)	
Car Ownership Rate	24.94	
	(23.35)	
Constant	-32.89	-8.20***
	(34.54)	(1.83)
N	49	49
Pseudo R-squared	0.79	0.70

Notes: Robust standard errors are shown in parentheses. First three partisanship variables are calculated from the entire sample period (2000-2018). The rest of variables are from the pre-reform periods (2000-2002). Employment rate, share college graduates and car ownership rate are calculated using foreign-born population. Column 2 only uses variables that are selected from Lasso.

Table A12: Robustness Checks for Employment Effects

	Undocumented Immigrants					Documented Immigrants				
	(1) Diff-in -Diff	(2) Diff-in- -Diff	(3) Matching Kit-Sink	(4) Matching Lasso	(5) Matching Lasso	(6) Diff-in -Diff	(7) Diff-in- -Diff	(8) Matching Kit-Sink	(9) Matching Lasso	(10) Matching Lasso
Panel A: Employed and Commuting by Car										
License Reform	0.0237***	0.0341***	0.0358***	0.0340***	0.0329***	-0.0029	0.0041	0.0086**	0.0093**	0.0087**
	(0.0047)	(0.0081)	(0.0083)	(0.0077)	(0.0072)	(0.0028)	(0.0033)	(0.0035)	(0.0035)	(0.0037)
Panel B: Employed										
License Reform	0.0079**	0.0112**	0.0117	0.0112**	0.0112**	-0.0018	-0.0021	-0.0003	-0.0043	-0.0043
	(0.0030)	(0.0048)	(0.0075)	(0.0048)	(0.0049)	(0.0027)	(0.0035)	(0.0048)	(0.0042)	(0.0041)
Fixed Effects										
State	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Controls x Year		Y					Y			
p (1-3 Order) x Year			Y	Y	Y			Y	Y	Y
Excludes CA					Y					Y

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants (for columns 1-5) and documented immigrants (for columns 6-10) in the Decennial Census (2000) and the American Community Survey (2001-2018). The regressions are weighted by the applicable Census and ACS weights. Outcome variables for Panels A and B are indicators for being employed and commuting by car, and being employed, respectively. All columns estimate equation 1 with state and time fixed effects, but different sets of state-level controls. Columns 1 and 6 exclude baseline state-level controls (the share of immigrants with college degrees in 2000-2002 and the average state partisanship: see Section 5) interacted with year fixed effects. Columns 2 and 7 include the baseline state-level controls, interacted with year fixed effects (preferred specification used throughout the paper). Columns 3-5 and 8-10 instead interact p, p^2, and p^3 with year fixed effects, where p is the propensity to enact license reforms. The list of variables used to derive the predictions in columns 3 and 7 are: share of construction/manufacturing/documented immigrants/undocumented immigrants/high school dropouts/college graduates/females in labor force, the probability of commuting by car, the log of state population, and the party in control for state legislature (Democratic, Republican, or Split). Columns 4-5 and 9-10 use the Lasso procedure, which selects the party in control and the share of undocumented immigrants as predictors of license reforms. Columns 5 and 10 drop California which has the highest propensity to enact license reforms.

Table A13: Effects of License Reforms on Young Undocumented Adults (Aged 18-30 years)

	Aged 18-24 years		Aged 25-30 years
	(1)	(2)	(3)
	No DACA	DACA	All
Panel A: Employed and Commuting by Car			
License Reform	-0.002	0.007	0.038***
	(0.020)	(0.015)	(0.010)
Mean	0.36	0.41	0.51
Panel B: Employed			
License Reform	0.014	0.003	0.016*
	(0.012)	(0.009)	(0.009)
Mean	0.55	0.54	0.70
State Fixed Effects	Y	Y	Y
Time Fixed Effects	Y	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes young undocumented immigrants (aged 18-30 years) in the American Community Survey (2005-2018). I further stratify the samples into the DACA-ineligible population aged 18-24 years to obtain the results reported in column 1, DACA-eligible population aged 18-24 years in column 2, and the population aged 25-30 years to obtain the results reported in column 3. The regressions are weighted by the ACS weights. Outcome variables for Panels A and B are indicators for being employed and commuting by car, and being employed, respectively. For all columns, I estimate equation 1. The main eligibility requirements for DACA, which can be identified using the ACS, are the following: coming to the United States before their 16th birthday, born after 1981, and having completed high school/GED or being currently enrolled in school. Using these criteria, I separate the young undocumented sample (aged 18 to 24 years) into the DACA-eligible population and the DACA-ineligible population. I use the samples from the year 2005, when the share of the DACA-eligible population is non-zero for age groups 18-24 years (Figure A5).

Table A14: Effects on Wages and Hours

	Undocumented Immigrants	Documented Immigrants
	(1)	(2)
Panel A: Asinh(Wage), Including Zero Wages		
License Reform	0.052**	-0.016
	(0.025)	(0.019)
Panel B: Log(Wage), Excluding Zero Wages		
License Reform	-0.018	-0.021*
	(0.015)	(0.011)
Panel C: Usual Hours of Work, Including Zero Hours		
License Reform	0.656***	0.005
	(0.221)	(0.162)
Panel D: Usual Hours of Work, Excluding Zero Hours		
License Reform	0.003	-0.021
	(0.162)	(0.096)
State Fixed Effects	Y	Y
Time Fixed Effects	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. Wages are calculated from the Census/ACS from 2000 through 2017 by diving wage income by (usual hours of work per week × weeks worked). Because the reference period of hours and wages in the Census/ACS is the previous calendar year (or previous 12 months for the ACS), I adjust the years accordingly. For Panel A, the dependent variable is the inverse hyperbolic sine of wages, which includes respondents who earn zero wages. For Panel B, the dependent variable is the logarithm of wages, which excludes zero wages. For Panel C, the dependent variable is usual hours of work per week, including respondents who have zero working hour. In Panel D, the dependent variable is usual hours of work, excluding respondents who have zero working hours. I estimate equation 1 to obtain the results reported in all columns.

Table A15: Migratory Response of License Reforms

	Undocumented Immigrants		Documented Immigrants	
	(1)	(2)	(3)	(4)
Panel A: In-Migrate to State				
License Reform	0.0033	0.0036**	-0.0002	0.0004
	(0.0030)	(0.0017)	(0.0016)	(0.0015)
Panel B: Out-Migrate from State				
License Reform	0.0043	0.0039*	0.0003	0.0005
	(0.0044)	(0.0022)	(0.0033)	(0.0014)
State Fixed Effects	Y		Y	
Time Fixed Effects	Y		Y	
State x Time Fixed Effects		Y		Y
State x Natives Fixed Effects		Y		Y
Natives x Time Fixed Effects		Y		Y

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes potentially undocumented immigrants (for columns 1-2), documented immigrants (for columns 3-4), and natives (for column 5) in the Decennial Census (2000) and the American Community Survey (2001-2018). To obtain the results reported in columns 1, 3, and 5, I estimate equation 1 with state and time fixed effects. I additionally control for the baseline state-level characteristics (the share of immigrants with college degrees in 2000-2002 and the average state partisanship: see Section 5) interacted with year fixed effects. To obtain the results reported in columns 2 and 4, I estimate equation 3 with state-by-time, state-by-reweighted-natives and reweighted-natives-by-time fixed effects. Refer to Section 4 for the construction of the reweighted native samples. I consider only domestic migration because it is impossible to know whether respondents in the ACS out-migrate to foreign countries.

Table A16: Effects of License Reforms in Large Commuting Zones by Commuting-Zone-level Accessibility

	Undocumented			Documented		
Dependent Variable: Employed	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Commuting Zone Population Aged 18-54 Years Greater than 100,000						
License Reforms	0.013**	0.012***	0.013***	0.001	0.001	0.001
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
× Transit Time Gap		0.038**	0.029***		-0.004	-0.008
		(0.014)	(0.011)		(0.015)	(0.016)
× Transit Time of Documented			-0.010			-0.005
			(0.010)			(0.005)
Panel B: Commuting Zone Population Aged 18-54 Years Grater than 500,000						
License Reforms	0.006	0.006	0.006	-0.001	-0.001	-0.001
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
× Transit Time Gap		0.041**	0.039**		-0.005	-0.009
		(0.016)	(0.016)		(0.017)	(0.018)
× Transit Time of Documented			-0.003			-0.005
			(0.005)			(0.006)
State Fixed Effects	Y	Y	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y
CZ Fixed Effects	Y	Y	Y	Y	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants (for columns 1-3) and documented immigrants (for columns 4-6) in the Decennial Census (2000) and the American Community Survey (2005-2018). ACS data from 2001 through 2004 are omitted because the Public Use Microdata Area (PUMA) variable is missing. The individual observations in the samples are split into one per commuting zone, and their Census/ACS weights are multiplied by this fraction of the population in the PUMA that belongs to each commuting zone (Autor, Dorn, and Hanson 2013). I estimate equation 4, where the outcome variable is being employed. To obtain the results reported in Panels A and B, I restrict my samples to immigrants living in commuting zones with populations aged 18-54 years greater than 100,000 and greater than 500,000 (in year 2000), respectively. For all columns, I control for commuting-zone fixed effects, state fixed effects, time fixed effects, and baseline state-level characteristics interacted with time fixed effects. To obtain the results reported in columns 2 and 5, I interact the commuting-zone-level accessibility measure for undocumented immigrants ($\hat{f_{0c}} - \hat{f_{1c}}$) with license reforms. The accessibility measure is calculated using the differences between undocumented and documented workers' transit times, while controlling for observable characteristics and occupations. To obtain the results reported in columns 3 and 6, I additionally interact transit time differentials of documented immigrants in each commuting zone ($\hat{f_{1c}}$) with license reforms.

Table A17: Probability of Car Commuting by Occupation

Occupation	Differential	Q	Share Male	Mean Wages
childcare workers	-.248	1	.03	14.2
personal care aides	-.167	1	.14	14.1
postsecondary teachers	-.164	1	.6	31.5
management analysts	-.147	1	.61	89.8
designers	-.075	1	.47	35.4
maids and housekeeping cleaners	-.068	1	.1	16.5
computer scientists and systems analysts/network systems analysts/web developers	-.065	1	.74	43.7
other teachers and instructors	-.051	2	.39	23.6
food preparation workers	-.05	2	.39	14.3
lawyers, and judges, magistrates, and other judicial workers	-.05	2	.53	65.6
computer programmers	-.046	2	.7	44
sewing machine operators	-.043	2	.18	18.6
nursing, psychiatric, and home health aides	-.042	2	.14	19.7
managers in marketing, advertising, and public relations	-.028	2	.56	59.5
computer and information systems managers	-.028	2	.76	57
software developers, applications and systems software	-.027	3	.77	50.4
sales representatives, wholesale and manufacturing	-.022	3	.66	32.8
hairdressers, hairstylists, and cosmetologists	-.021	3	.12	15.8
chief executives and legislators/public administration	-.02	3	.78	71.3
bookkeeping, accounting, and auditing clerks	-.019	3	.17	24.4
managers, nec (including postmasters)	-.019	3	.66	46.1
janitors and building cleaners	-.019	3	.62	18.6
cashiers	-.018	4	.3	14.7
accountants and auditors	-.016	4	.34	36.1
chefs and cooks	-.015	4	.67	15.9
secretaries and administrative assistants	-.012	4	.06	22.5
receptionists and information clerks	-.012	4	.11	17.1
real estate brokers and sales agents	-.01	5	.47	36
human resources, training, and labor relations specialists	-.01	5	.33	31.4
office clerks, general	-.01	5	.22	19.9
retail salespersons	-.002	5	.47	21.2
financial managers	.006	5	.46	57.3
teacher assistants	.007	5	.1	17.1
waiters and waitresses	.008	5	.41	15.4
security guards and gaming surveillance officers	.01	6	.82	19.4
customer service representatives	.012	6	.33	20.7
medical assistants and other healthcare support occupations, nec	.014	6	.17	18.3
stock clerks and order fillers	.016	6	.6	16.3
engineers, nec	.017	6	.84	48.8
general and operations managers	.018	6	.73	45.7
agricultural workers, nec	.018	6	.76	13.1
grounds maintenance workers	.021	6	.96	15.3
packers and packagers, hand	.024	6	.32	13.8
social workers	.025	7	.26	26.2
first-line supervisors of sales workers	.025	7	.66	28.3
construction laborers	.025	7	.98	20.7
laborers and freight, stock, and material movers, hand	.026	7	.78	16.9
first-line supervisors of office and administrative support workers	.027	7	.39	27.1
carpenters	.028	7	.99	21.9
taxi drivers and chauffeurs	.029	7	.95	16.1
shipping, receiving, and traffic clerks	.031	7	.73	18
food service and lodging managers	.031	8	.63	26.2
painters, construction and maintenance	.031	8	.97	19
personal appearance workers, nec	.032	8	.2	14.9
assemblers and fabricators, nec	.039	8	.51	16.9
inspectors, testers, sorters, samplers, and weighers	.041	8	.46	22.1
other production workers	.046	9	.64	18
driver/sales workers and truck drivers	.047	9	.97	21
automotive service technicians and mechanics	.049	9	.99	21.1
metal workers and plastic workers, nec	.049	9	.72	20.2
licensed practical and licensed vocational nurses	.049	9	.15	26.5
first-line supervisors of construction trades and extraction workers	.052	9	.98	30
electricians	.053	9	.98	27.2
first-line supervisors of production and operating workers	.056	9	.75	28.3
elementary and middle school teachers	.057	10	.22	30.5
welding, soldering, and brazing workers	.059	10	.91	20.8
industrial truck and tractor operators	.06	10	.95	20
physicians and surgeons	.067	10	.62	77.8
registered nurses	.072	10	.13	41.9

Notes: The list of occupation is taken from the IPUMS. The table is constructed using documented immigrants from year 2000-2011. Wages are in 2018 dollars. Refer to Section 9 for how to calculate car-dependency of each occupation.

Table A18: Effects of License Reforms by Household Type and Sex

	Married-Couple Family Household		Other Types of Household	
	(1)	(2)	(3)	(4)
	Male	Female	Male	Female
	Panel A: Employed and Commuting by Car			
License Reform	0.0366**	0.0376***	0.0400***	0.0116
	(0.0136)	(0.0090)	(0.0117)	(0.0087)
Mean	0.70	0.31	0.56	0.40
	Panel B: Employed			
License Reform	0.0167**	0.0107	0.0084	-0.0074
	(0.0063)	(0.0101)	(0.0079)	(0.0074)
Mean	0.86	0.43	0.79	0.61
State Fixed Effects	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. The sample includes undocumented immigrants aged 18-54 years in the Decennial Census (2000) and the American Community Survey (2001-2018). I restrict my samples to undocumented immigrants in married-couple family households for columns 1-2 and all other types of households for columns 3-4. The regressions are weighted by the applicable Census and ACS weights. Outcome variables for Panel A and B are indicators for being employed/commuting by car and being employed, respectively.

Table A19: Effects of License Reforms on Married-Couple Family Households

Husband:	Non-employed	Non-Employed	Employed	Employed
Wife:	Non-employed	Employed	Non-Employed	Employed
	(1)	(2)	(3)	(4)
License Reform	-0.0043	-0.0079***	0.0028	0.0094
	(0.0031)	(0.0022)	(0.0057)	(0.0063)
Mean	0.06	0.04	0.53	0.37
State Fixed Effects	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. The samples are restricted to married-couple family households for undocumented immigrants, which consist of one male husband and one female wife whose ages are between 18-54 years. The regression controls for personal characteristics of husbands and wives (the spouse of an undocumented immigrant is also considered an undocumented immigrant according to the framework of Borjas (2017)). The regressions are weighted by personal Census/ACS weights of husbands, although using household weights or personal weights of wives does not significantly alter the coefficients. The outcome variables are a household having a non-employed husband/a non-employed wife, a non-employed husband/a employed wife, a employed husband/a non-employed wife, and an employed husband/an employed wife.

Table A20: Effects of License Reforms on Employment Stocks and Transitions (CPS)

	Undocumented	Documented
	(1)	(2)
Panel A: Employed		
License Reform	0.0162*	-0.0012
	(0.0090)	(0.0057)
Mean	0.679	0.753
Panel B: Employed to Employed		
License Reform	0.0168*	-0.0015
	(0.0093)	(0.0062)
Mean	0.642	0.726
Panel C: Employed to Non-Employed		
License Reform	0.0003	0.0012
	(0.0021)	(0.0015)
Mean	0.040	0.028
Panel D: Non-Employed to Employed		
License Reform	-0.0005	0.0003
	(0.0017)	(0.0016)
Mean	0.038	0.029
Panel F: Non-Employed to Non-Employed		
License Reform	-0.0165*	-0.0000
Mean	(0.0082)	(0.0057)
	0.281	0.218
State Fixed Effects	Y	Y
Time Fixed Effects	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. Estimates are weighted by the CPS weights. The samples include undocumented immigrants (column 1) and documented immigrants (columns 2) in the Current Population Survey (2000-2018). For all columns, I use the difference-in-differences specification (equation 1). I additionally control for the baseline state level characteristics in 2000 (the probability of commuting by car and the share of the population with college degrees) interacted with year fixed effects. The outcome variables are indicators for being employed (Panel A), employed in the previous month and employed in the current month (Panel B), employed in the previous month and not employed in the current month (Panel C), not employed in the previous month and employed in the current month (Panel D), and not employed in both previous and current months (Panel E). For Panel A I use all month-in-samples of the CPS. For Panels B-F I drop samples from month-in-samples 1 and 5 as there is no pre-employment outcome. Samples without consistent age, sex, or education information across panels are dropped for Panels B-F.

Table A21: Effects of License Policies on Employment from 1994 through 2017 (CPS)

Dependent Variable: Employed	Undocumented		Documented	
	(1)	(2)	(3)	(4)
License Stop	0.0064	0.0078	0.0063	0.0065
	(0.0060)	(0.0061)	(0.0042)	(0.0041)
License Start		0.0153*		0.0024
		(0.0080)		(0.0053)
State Fixed Effects	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y

Notes: Robust standard errors are clustered by state and shown in parentheses. Estimates are weighted by the CPS weights. I estimate equation 1 with state and time fixed effects. I additionally control for the baseline state-level characteristics (the share of immigrants with college degrees and the average state partisanship: see Section 5) interacted with year fixed effects. The outcome variable is the indicator for being employed. "License Stop" refers to the indicator variable named "imposing SSN or documented presence requirements in license application process." "License Start" refers to the indicator variable named "removing documented presence requirements in license application process."

Appendix A: Conceptual Framework

Suppose that undocumented workers and firms are uniformly distributed over a circle, with circumference 1. The distance between a worker located in i and a firm located in j is $0 \leq x_{ij} \leq 0.5$. I assume that workers live forever, and time is continuous.

The matching function is defined as

$$M(u, v)$$

where u denotes the unemployment rate and v denotes the vacancy rate. It is assumed that $M()$ has constant returns to scale. The ratio between vacancies and unemployment is defined as $\theta \equiv \frac{v}{u}$, which is the tightness of the labor market. The contact rate of a worker is

$$\frac{M(u, v)}{u} = M(1, \frac{v}{u}) \equiv m(\theta) \tag{9}$$

At each moment in time, the probability an undocumented worker finds a job can be expressed as

$$p(\theta) = m(\theta) \times 2\hat{x} \tag{10}$$

where $m(\theta)$ is the contact rate and $\hat{x}$ is the maximum commuting distance (search) a worker is willing to accept. I multiply $\hat{x}$ by 2 because workers are searching both sides.

Assume that the separation rate (the rate at which jobs are being destroyed) is exogenously given by s. In the steady state, outflows into unemployment and inflows into unemployment are equal.

$$p(\theta)u = se \tag{11}$$

where $e = 1 - u$ is employment rate. Thus, I have the following steady-state relationship

$$e = \frac{2\hat{x}m(\theta)}{s + 2\hat{x}m(\theta)}. \tag{12}$$

The relationship between $\hat{x}$ and e is

$$\frac{\partial e}{\partial \hat{x}} > 0 \quad \frac{\partial^2 e}{\partial \hat{x}^2} < 0$$

The positive first derivative suggests that employment increases as the search area increases. The negative second derivative implies diminishing returns on search for employment outcomes.

Undocumented workers next need to choose the maximum commuting distance or area of search, $\hat{x}$. Because $\hat{x}$ is the distance that makes workers indifferent between being employed and remaining unemployed, we have the following relationship

$$V_u = V_e(\hat{x}, w) \tag{13}$$

where w denotes wages. For simplicity, I assume that w is exogenously given.

First, V_u is the asset value of an unemployed worker, which can be represented as

$$rV_u = z + m(\theta)\left[2\int_0^{\hat{x}} [V_e(x,w) - V_u]\, dx\right] \tag{14}$$

where $r \in (0,1)$ is the discount rate and z is the unemployment benefit that an unemployed worker receives. Thus, the discounted lifetime value of an unemployed worker is unemployment benefit (z) plus the total gains from being employed ($V_e(x,w) - V_u$) with maximum commuting distance $\hat{x}$ and matching probability $m(\theta)$.

V_e is the lifetime utility of an employed worker with wage w and commuting distance $\hat{x}$, which can be expressed as

$$rV_e(\hat{x},w) = w - C_k(\hat{x}) + s[V_u - V_e(\hat{x},w)] \tag{15}$$

where $C_k(\hat{x})$ is the total cost of commuting when using transportation method $k \in \{b,c\}$. There are two means of commuting: b denotes commuting by public transportation and c denotes commuting by private vehicle. The discounted lifetime utility of an employed worker is $w - C(\hat{x})$ plus the value of being unemployed ($V_u - V_e(\hat{x},w)$) with probability s.

The cost of commuting, $C_k(x)$, is

$$C_k(x) = d_k w t_k(x) \tag{16}$$

where $t_k(x)$ is commuting time using transportation method k.[44] Denote μ_k as the average speed of commuting, representing smoothness of travel. Then,

$$t_k(x) = \frac{x}{\mu_k}. \tag{17}$$

Also, $d_k \geq 1$ is the parameter that represents the risk of deportation or fines that undocumented workers' face from using transportation method k. If undocumented immigrants are allowed to legally obtain driver's licenses, this will lead to a reduction in the deportation risk of commuting by car, d_c.

If d_c is sufficiently reduced following license reforms such that $C_c < C_b$, undocumented workers will switch from using public transportation (b) to driving cars (c). The condition that includes undocumented workers to then shift their transportation method is

$$d_c < d_b\left(\frac{\mu_c}{\mu_b}\right)$$

It is noteworthy that whether they shift from b to c depends on $\frac{\mu_c}{\mu_b}$, which is the average speed

[44] I can further assume that there are some pecuniary costs involved in owning vehicles or taking public transportation, as in Gautier and Zenou (2010). Yet, the implication of this model does not change with the fixed pecuniary costs.

(smoothness) of cars relative to the average speed of public transportation. Thus, the effects of license reforms on commuting and employment (shown below) will likely be larger in localities where the difference in average speed (or commuting time) between public transportation (used more regularly by undocumented immigrants) and private vehicle (used more frequently by documented immigrants) is larger. These localities tend to be more rural, with limited public transportation—where jobs are located far from workers' homes.

From (5), (6), (7) , (8) and (9) I have

$$\frac{m(\theta)d_c w}{\mu_c}\hat{x}^2 + \frac{d_c w(r+s)}{\mu_c}\hat{x} - (w-z)(r+s) = 0 \tag{18}$$

There is one root $\hat{x}^*$ that is strictly positive,

$$\hat{x}^* = -\frac{d_c(r+s)}{2m(\theta)d_c} + \frac{\mu_c}{2m(\theta)wd_c} \times \sqrt{\left(\frac{d_c w(r+s)}{\mu_c}\right)^2 + 4\frac{m(\theta)d_c w(w-z)(r+s)}{\mu_c}}$$

Then, I derive the following relationship between d_c and e by plugging $\hat{x}^*$ into (4).

$$\frac{\partial e}{\partial d_c} < 0$$

That is, a reduction in the risk of being deported or being fined from car commute raises the employment rate of undocumented workers. Issuing driver's licenses to undocumented workers expands their maximum search (commuting) distance by reducing their commuting costs, which should increase the probability that undocumented workers are being matched to firms.

Suppose the effects of license reforms on employment are heterogeneous across varying wages, w. Undocumented workers cannot generally receive unemployment benefits, so I further assume $z = 0$. By differentiating $\frac{\partial e}{\partial d_c}$ by w, I have

$$\frac{\partial}{\partial w}\left(\frac{\partial e}{\partial d_c}\right) > 0$$

Therefore, the positive employment effects of reducing deportation risks will be larger for low-skilled undocumented individuals who have lower wages than high-skilled undocumented individuals. Given the diminishing returns on search, the reduction in transportation costs will more clearly benefit low-skilled workers who were not searching far enough away before the reform.

Finally, assume that firms offer two types of occupations. Occupation 1 has higher car-dependency, where workers with private vehicles (c) have higher wages than those without private vehicles (b), so $w_c^1 > w_b^2$. For example, agricultural workers with their own vehicles (or driver's license) can have higher wages because they have easier access to multiple farms and ability to operate agricultural vehicles. Occupation 2, on the other hand, has lower car-dependency, where its wage is not dependent on transportation method, so $w_b^1 = w_c^2$. I later show that more car-dependent occupations

tend to have higher wages than less car-dependent occupations (Figure 5), so $w_c^1 > w_c^2$. Then, it is clear that undocumented workers switch from occupation 2 to occupation 1 when the deportation risk of commuting by car, d_c is sufficiently reduced.