IMMIGRATION FEDERALISM RENEWED: THE EFFECTS OF STATE AND LOCAL POLICIES ON THE LEGAL AND LABOR MARKET OUTCOMES OF THE U.S. IMMIGRANT POPULATION

By

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To my parents, whose continuous encouragement, support, and sacrifices allowed this manuscript to be possible,

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INTRODUCTION

The federal immigration system controls the lives of every individual entering the United States—it dictates when they can enter, when they must leave, and the process by which officials operate if they encounter individuals without legal status. Though federal laws and regulations once dominated immigration policy in the U.S., many policies are now enacted on the local level. This increased state and local involvement in immigration policy—known as "immigration federalism"—has become an important facet of immigration law and policy. This dissertation contributes to the existing literature in this area by examining the effects of immigration federalism at three different levels: the state, the county, and finally, the immigration court.

My first chapter focuses on the labor market impacts of the modern Official English movement on the limited English proficient (LEP) population in the United States. This movement, which originated in the early twentieth century, was revived in the 1980s. Thirty-two states currently have an Official English law. These laws require that all official government business be conducted in only English. I find that these laws are associated with a wage penalty for men residing in Official English law states. This wage penalty remains unexplained by individual characteristics and occupational representation in jobs that place a lesser importance on English skills. I find that these laws have a heterogeneous effect on LEP minorities: white LEP workers do not earn the same wage penalties that other LEP minorities do in Official English states, providing limited evidence for employment discrimination as one mechanism underlying the negative effects of Official English laws on LEP workers.

My second chapter provides a nationwide empirical analysis of the labor market impacts of 287(g) agreements. 287(g) agreements are made between representatives from local governments and U.S. Immigrations and Customs Enforcement (ICE) and allow ICE to deputize local officers to investigate the immigration status of individuals so that local officers may report those individuals to ICE. I find that these agreements are associated with large wage penalties for both male and female immigrant workers. After segmenting my sample by a proxy for legal status, I find that the negative effects of 287(g) agreements on wages are felt more acutely by potentially undocumented workers than their peers with legal status. Further, underlying heterogeneity in the breadth of these agreements can explain some of their negative effects on immigrant wages.

My final chapter examines a different type of local body: the immigration court. While immigration courts are a part of the federal immigration system, each court must autonomously manage the backlog of cases in their docket. This case backlog has resulted in a crisis of volume facing immigration courts and has nearly crippled the immigration system: at the end of 2017, there were 629,051 cases pending nationwide, or more than 2,500 cases per immigration judge. My analysis examines the impact that this case backlog has on both the duration of immigration cases and the final decision in each immigration case. My results provide evidence that a congestion effect is occurring in immigration courts: when case backlog becomes sufficiently large, the amount of time that it takes judges to reach a final decision in a case slows and the probability of success in a given case increases. Ultimately, my dissertation provides evidence that local immigration policies and procedures created under the immigration federalism framework can have a significant impact on the U.S. immigrant population.

CHAPTER ONE: THE OFFICIAL ENGLISH MOVEMENT AND MECHANISMS UNDERLYING THE EFFECTS ON LEP WORKER WAGES

I. Introduction

The United States has a long history of promoting a singular national English language. Theodore Roosevelt wrote in support of one language: “We have room for but one language here, and that is the English language, for we intend to see that the crucible turns our people out as Americans, of American nationality . . .” (Morrison and Roosevelt 1954). The influx of immigrants to the United States in recent decades prompted state legislatures to return to the ideal of a nation under a homogeneous language through state-level Official English policies. Yet, the effects of these modern laws on workers who do not speak English well or at all—and the mechanisms through which these workers can be affected—are unclear. In this paper, I examine the impact of Official English laws on limited English proficient (LEP) worker wages. After finding that these laws negatively impact LEP worker wages, I isolate the mechanisms behind the effects of these laws.

Language is one of the most important factors in immigrant assimilation. Immigrants who learn the native language of their host country achieve better labor market outcomes (Koussodji 1998; Dustmann and Van Soest 2002; Dustmann and Fabri 2003; Bleakley and Chin 2004; Chiswick and Miller 2007) and are quicker to integrate socially (Bleakley and Chin 2010; Tam and Page 2016). Yet there are more than 19.2 million adults in the United States who report that they do not speak English well or at all (Wilson 2014).

As immigration rates swelled throughout the 1980s, many state policymakers responded to this increase by proposing bills declaring English the official language of the state, with the stated intent of helping immigrants assimilate to the culture of the United States by learning the native language. Those in opposition to this Official English movement saw these laws as thinly veiled attempts to discriminate against workers who do not speak English well or at all—or even against foreign-born individuals as a whole, regardless of English ability (Liu 2014).

The Official English movement supports the declaration of English as the official government language of the United States and each individual state. Under Official English laws, all official government business—including public documents, records, and meetings—are conducted in English, and English only.[1] West Virginia became the thirty-second state to pass an Official English law on March 5, 2016 and debates over Pennsylvania's Official English bill are currently taking place in its legislature. National attempts to make English the official language of the United States are quickly gaining support, as there are over 90 co-sponsors of a proposed national Official English bill in the House of Representatives. If enacted, this bill would require all official functions of the U.S. government to be conducted in English and would require all candidates for naturalization of citizenship to undergo uniform testing of their English language ability.

Prior research shows that these laws hinder the labor market outcomes of limited English proficient workers. Zavodny (2000) finds that limited English proficient (LEP) male workers experience a wage penalty of 12 percent in states that have Official English laws, but does not find a significant effect of these laws on women's earnings. Mora and Saenz (1997) find an 8 percent decline in earnings for all LEP Hispanic workers in states

[1] Though providing ballots and other voting materials could be considered official government business, the Voting Rights Act of 1975 contains a provision that requires state governments to provide ballots and other information relating to the electoral process in minority languages if the language group is more than (1) 10,000 people or (2) is more than five percent of all voting age citizens.

with Official English laws. Federman et al. (2006) conclude that Official English policies harm LEP workers after finding that manicurist state licensing laws requiring a test in English only results in a significant wage penalty for Vietnamese immigrants. Official English laws decrease housing investments among Hispanic immigrants (Dávila et. al 2003) but moderately increase the literacy of certain foreign-born children (Lleras-Muney and Shertzer 2015).

Yet, past literature focusing on Official English laws stops short of determining the mechanism by which these laws affect the labor market outcomes of LEP individuals. Using four decades of Census and American Community Survey (ACS) data, I use a differences-in-differences-in-differences methodology that uses the differing state enactment dates of Official English laws. I find that Official English laws are associated with roughly a 3 percentage point decrease in wages for male workers. Using O*NET, a detailed dataset that provides detailed working conditions by occupation, I find these wage penalties remain unexplained by individual occupational characteristics and the importance of English in the individual's job. These wage penalties also unexplained by proxies for differences in individual productivity.

I ultimately conclude that the unexplained differences of the effects of these laws may be associated with wage discrimination against LEP minorities. While these laws negatively impact foreign individuals, the effects of these laws are concentrated among minority ethnicities. Additionally, I find limited evidence that white LEP workers do not experience the same wage penalties as minority ethnicities. In *United States v. Carolene Products*, the Court characterized historic Official English laws as discrimination against particular national minorities.[2] My results show that the characterizations of the 1938 Supreme Court have held over time, and provide a possible explanation for why these Official English laws are negatively impacting LEP worker wages.

II. Official English Movements: Past and Present

Though the United States is one of the most ethnically diverse nations in the world, it is historically one of the most homogeneous countries linguistically (Thernstrom 1980; Citrin et al. 1990; Patsiurko et al. 2012). Despite the lack of linguistic diversity, there exists continued support for establishing English as the official language of the United States. This support has ebbed and flowed with migration trends. After a surge of immigration in the early 1900s, twenty-one states passed laws to make English the official language of the state and required the teaching of only English in schools (Trasvina 1990). Concerns over immigrant assimilation prompted these laws, as immigrants were coming from a diverse set of nations and most did not know English. Bilingual education programs raised concerns that teachers were not properly conveying "American values" and were therefore slowing the assimilation process (Lleras-Muney and Schertzer 2015). These concerns were directed primarily towards German immigrants, who used the German language in German social clubs, newspapers, churches, and parochial schools. During the outbreak of World War I, distrust of German immigrants grew (Moser 2012) and Americans believed that the use of the German language signaled disloyalty towards the United States. Many states with large numbers of immigrants began adopting Official English laws that required instruction in public and private schools to be in English. However, these Official English laws did not survive judicial scrutiny. In 1923, the Supreme Court ruled in *Meyer v. Nebraska* that these

[2] United States v. Carolene Prods. Co., 304 U.S. 144, 152 n. 4 (1938).

Official English laws were unconstitutional because they prohibited the teaching of foreign languages in schools.

Soaring levels of immigration in the 1980s ushered in a new era of federal legislation promoting English as the official language of the United States. This culminated in the Bill Emerson Language Empowerment Act of 1996, which declared English the official language of the federal government and required all government business to be conducted in English. The bill was passed by the House of Representatives, but stalled in the Senate due to concerns of discrimination against immigrants and beliefs that the bill was unnecessary, as the United States had functioned without an official language for more than 200 years. Despite these concerns, a federal Official English bill is introduced into Congress almost every year, and the English Unity Act of 2017 has gained more supporters in the House of Representatives than any other proposed Official English bill since 1996.

Rather than wait for federal action, some states took the Official English movement into their own hands by adopting a statute or passing a Constitutional amendment declaring English the official state language. These laws differ from the Official English laws of the 1920s as they require the use of English only when the state government is acting in its official capacity. Modern Official English laws do not directly limit bilingual education, though some states have used their Official English law to restrict bilingual education for LEP students. Table 1 lists all thirty-two states that have passed Official English laws along with the date of adoption and form of law (whether the law was enacted by statute or constitutional amendment).

Figures 1 and 2 show the progression of these state laws over time.[3] Figure 3 depicts all states with some form of Official English law as of April 2016. States that have enacted Official English laws are geographically and politically diverse. This is consistent with evidence from the American National Election Study (NES), which shows that support for Official English laws is broad and is not based on partisanship, social class, or racial and ethnic hostility (Frendreis and Tatalovich 1997). The four states that receive the most immigrants—California, Texas, New York, and Florida—are also equally balanced: two (California and Florida) have official English laws while two (New York and Texas) do not. One study claims that these laws are adopted in part depending on the proportion of a state's population that is foreign-born and whether the state allows for direct initiatives to influence the adoption of language laws (Schildkraut 2001) but other scholars find that the size of a minority population has only an indirect effect on legislative policy decisions (Citrin et al. 1990; Preuhs 2005). Liu et al. (2014) finds that a state's foreign-born population only increases the likelihood of English-official legislation adoption when the issue of immigration is nationally salient.

Figure 1 also depicts the handful of states that passed Official English laws before 1980. There is evidence that the motives underlying these early laws were different than those passed during the Official English movement. For example, Louisiana's Official English law was passed in 1807, denouncing French as its official language so the state could join the Union. Illinois's law was passed in 1969 to replace its official language,

[3] Virginia clarified its official English law in 1996, by adopting a new statute stating, "[e]xcept as provided by law, no state agency or local government shall be required to provide and no state agency or local government shall be prohibited from providing any documents, information, literature or other written materials in any language other than English." The prior law had simply designated English as the official language of the state. For the bulk of my analysis, I treat Virginia's date of adoption as 1981, but I also perform robustness checks changing the date of adoption to 1996 and my results are not qualitatively affected.

which was then “American.” My analysis focuses only on Official English laws passed from 1980–2010. I examine Official English laws passed before 1980 in the Appendix.

III. Conceptual Framework

Despite claims that these laws are purely symbolic, Official English laws may affect the wages of workers with limited English ability. The direct effect of these laws would exclude LEP individuals from working in state governments. As the share of LEP workers in state government is likely to be slim, this direct effect is unlikely to be substantial. Yet, the indirect effects of Official English laws on both worker and employer behavior have the potential to be large.

Official English laws can impact a worker’s occupational choice. Without English ability, LEP workers may shift their occupational choices to occupations that do not emphasize knowledge of the language, as occupational choices are the “intervening activity” that links earnings to acquired country-specific human capital skills (Duncan 1961; Chiswick and Miller 2010). However, at the individual level, even if LEP workers select out of occupations requiring high levels of English language ability because of a lack of country-specific language skills, the supply of workers in each occupation should not differ on the basis of whether there are Official English laws in the state, as worker preferences should not shift based on Official English laws.

Similarly, while LEP workers may be less productive in occupations requiring the English language (Lazear 1999; Chiswick and Miller 2010), the productivity of LEP workers should not differ between states if there is no difference in the quality of LEP workers in states with or without Official English laws.[4] However, to the extent that

[4] Table 3 reports that LEP workers in states with and without Official English laws are similar on many observable characteristics, including age and years of education.

Official English laws limit the use of government services (such as driver's licenses and job licensing), LEP workers may be hindered from obtaining the necessary prerequisites for employment.[5] Controlling for differences in strength of enforcement of Official English laws can explain the effects of these laws on worker productivity.

Any remaining unexplained difference between LEP worker wages in Official English states may be attributable to employment discrimination (Oaxaca 1973; Blinder 1973). Official English laws have been linked to discrimination against foreign-born individuals by encouraging xenophobic rhetoric and prejudice (Califa 1989; Arington 1991) and by allowing to employers to create their own English-only policies (Davis 1997). However, according to the Equal Employment Opportunity Commission (EEOC), English-only policies are categorically unlawful and presumed to violate Title VII of the Civil Rights Act of 1964 by discriminating based on national origin unless the employer can demonstrate that English is necessary for the individual's job.[6] Still, employment discrimination may persist without a formal English-only policy in Official English states if employers choose to favor non-LEP workers systematically over LEP workers, or if employers systematically favor groups of LEP workers. If Official English laws reflect employer discrimination against LEP workers, I expect a decline over time in the labor

[5] For example, in states that enforce their Official English law such that they will not offer driver's license exams in any language other than English, an LEP worker's employment opportunities may be severely restricted in places without ample public transportation.

[6] *See* 29 C.F.R. § 1606.7(a) (2012); *see also* Garcia v. Spun Steak Co., 998 F.2d 1480, 1489 (9th Cir. 1993) ("The EEOC Guidelines provide that an employee meets the prima facie case in a disparate impact cause of action merely by proving the existence of the English-only policy."); Long v. First Union Corp., 894 F. Supp. 933, 940 (E.D. Va. 1995) (same). The Supreme Court has yet to rule on whether language-based discrimination should be considered a suspect class, but legal scholars have contended that this type of discrimination should be given strict scrutiny, or intermediate level scrutiny at the very least (Moran 1981; Califa 1989).

market outcomes of LEP workers in Official English states relative to LEP workers in non-Official English states that cannot be explained by observable differences.[7]

Finally, these laws may cause immigrants to resist the English language. The anti-immigrant rhetoric that often surrounds the Official English movement may encourage ethnic minorities to become more ethnocentric and resist acquiring English skills (Pérez 2014; Fouka 2015). These laws may also allow for feedback such that LEP workers choose lower paying jobs with less emphasis on the English language in anticipation of employer discrimination, migrate out of states that adopt Official English laws, or leave the labor force altogether. I discuss the potential for these effects giving my analysis in the concluding remarks of this chapter.

IV. Data and Methodology

A. Data

I use the 1980, 1990, and 2000 Census 1% samples along with a 1% sample of the 2009-2013 5-year American Community Survey (ACS).[8] This is the only large data set spanning the modern Official English movement that also asks participants questions regarding English proficiency.[9] I use this time period because it coincides with the modern Official English movement and because the Census first asked questions regarding English proficiency in 1980.

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[7] This conceptual framework squares with political science work regarding support for Official English laws. The two main explanations for these laws are economic conflict and cultural resentment (Moran 1987), where majority groups feel vulnerable and support Official English laws as a defensive reaction to this vulnerability (Citrin et al. 1990). As a result, the majority may discriminate against minority actors.

[8] I use data compiled by the Integrated Public Use Microdata Series (IPUMS). IPUMS contains the same underlying values as Census data, but harmonizes the data across years by assigning uniform codes to variables.

[9] Though the Current Population Survey (CPS) provides labor market information, it only twice asks individuals about English ability. The CPS included questions about English ability in the supplements administered in 1979 and 1989.

In each Census year, all households in the United States receive a short form Census, which asks basic demographic questions regarding each household member. Additionally, through the 2000 Census, one out of six households receives the long form Census, which asks more detailed labor market and migration questions. I use data from the long form Census, as it contains data on immigration status and important employment measures such as employment status and income.

The questions that were once asked in the long form Census are now asked in the ACS, which began in 2005. The ACS is a nationally representative sample that continually surveys randomly sampled addresses in every state, the District of Columbia, and Puerto Rico. Each year, the ACS samples nearly three million household unit addresses in the United States. No household will receive the survey more often than once every five years. I supplement my analysis of Census samples with the 5-year ACS spanning the years of 2009–2013. The 5-year ACS has the benefit of providing more reliable samples than 1-year ACS estimates of smaller populations, like those of limited English proficiency. I treat the 2009–2013 ACS as a proxy for the 2010 Census consistent with prior literature (Borjas 2015).

To obtain information on English proficiency, the both ACS and Census ask individuals how well they speak English. There are four responses to this question: very well, well, not well, and not at all. Census forms are in English but are also available in Spanish. Following Zavodny (2000), I classify those who report their ability to speak English as not well or not at all as having limited English proficiency. These measures are self-reported and could introduce non-random measurement error if, for example, individuals were overly confident about their English speaking abilities when answering the survey. But there is evidence that individual's self-reported measures of English ability are accurate: Kominski (1989) and Wilson (1999) find that self-reported measures of English ability are highly correlated with interviewer reports of English ability. Further, Kominski (1989) noted that reporting errors in this question were unlikely to change the final distribution of a two-category measure of English ability. I use a two-category measure of English ability. Therefore, it is unlikely that the estimated coefficients will be biased due to systematic measurement error. However, these coefficients could be attenuated due to random error, which would only bias my coefficients downward and result in my coefficients providing conservative estimates of the true effect of these laws on LEP workers.

To analyze the effects of Official English law enactments, I code each state law according to its date of adoption as shown in Table 1. I create indicator variables to show if a state has enacted an Official English Law in each year of data. For example, South Carolina, which enacted its law in 1987, would have an indicator variable for an Official English law equal to zero in 1980, but equal to one in 1990, 2000, and 2010.

My analysis uses measures of wages, hours, and weeks worked. The Census and ACS report earnings annually. The Census and ACS measure hours worked each week by asking the surveyed household member: "during the past 12 months, in the weeks worked, how many hours did you usually work each week?" The Census and ACS measure weeks worked each year by asking respondents how many weeks that they worked last year, including paid vacation, paid sick leave, and military service. The Census reports actual weeks worked, while the ACS reports weeks in broad ranges. For individuals in the ACS, I use the midpoint of the reported range as the number of weeks that individual worked. I construct hourly wage by dividing an individual's earnings by the amount of weeks worked multiplied by usual hours worked each week. I adjust all wages to 2010 dollars.

I create a variable for years of completed schooling.[10] I create indicator variables for marital status, race, veteran status, and if children are present in the individual's household. I also create independent indicator variables for racial and ethnic groups to allow for the mutual exclusivity of Hispanic individuals and white individuals. To examine the importance of an individual's occupation, I create indicator variables using the 2010 Standard Occupational Classification (SOC) major groups including transportation, construction, management, sales, and production. As my sample excludes those in the military, there are 22 different indicator variables for occupation in my analysis.

There is a substantial literature showing that time spent in the United States is positively related to the wage growth of foreign-born individuals relative to natives (Chiswick 1978; Chiswick 1986; Smith 2006; Lubotsky 2007; Borjas and Friedberg 2009), though this growth may be slowing in newer cohorts (Borjas 2015). To capture this heterogeneity of the foreign-born population, and to provide for direct comparison to prior literature (Zavodny 2000), I create categorical indicator variables for the years since entry to the United States for each foreign-born individual.[11]

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[10] The ACS provides a detailed report of the highest level of schooling completed. Each survey participant has an option to select the precise highest grade of schooling, but some are also reported in ranges. For example, individuals could report their highest level of schooling as grade 1 or grades 1, 2, 3, or 4. For those individuals that report their highest level of schooling as a range, I use the midpoint values of the ranges as their years of schooling. All individuals that spent more than four years in college and obtained a college degree were coded as obtaining 16 years of education.

[11] These categories are less than 5 years, less than 10 years, less than 15 years, less than 20 years, less than 30 years, and more than 30 years.

I restrict the sample to full-time, full-year workers who are not self-employed or working without pay and are not in the armed forces. I remove individuals who were born in Puerto Rico, Guam, and the U.S. Virgin Islands, or those born abroad with American parents. Therefore, my sample contains only citizens born in the United States, naturalized citizens, and non-citizens. I create a variable indicating foreign-born status, which includes both naturalized and non-citizens.

Table 2 provides the summary statistics of individuals by their residence in states with or without an Official English law. Individuals in states with an Official English law and those in states without such a law are strikingly similar: they both have roughly thirteen years of education and are roughly 40 years old on average. Those in states without an Official English law earn only 38 cents less per hour on average than individuals that reside in Official English states ($22.54 per hour compared to $22.16 per hour in 2010 dollars). There are also more total immigrants in states that have Official English laws. I address the potential for selective migration of LEP workers in subsection F.

Table 3 compares demographics of LEP individuals based on state of residency. LEP individuals in states with or without Official English laws are strikingly similar: they have almost identical average years of education and ethnic makeups. In this raw data, LEP workers that reside in states without official English laws earn 9 cents more per hour than LEP workers in states with such a law. Table 4 compares LEP individuals to the other workers in my sample. English proficient foreign-born individuals look more like natives than LEP workers: they are as equally educated as natives on average and also earn more per hour than natives. In contrast, LEP workers earn roughly one-half of the hourly wage of individuals who are proficient in English. Roughly half of the English proficient foreign-born individuals in my sample are white, and roughly one third of English proficient foreign-born individuals workers are Hispanic. Most of my sample is natives, but my sample also contains 67,324 LEP workers and 240,746 English proficient foreign-born individuals.

B. Methodology

I use a differences-in-differences-in-differences, also known as a triple difference, framework to examine the impact of Official English laws on the labor market outcomes of LEP workers. This specification has the advantage of eliminating different trends in the wages of LEP and non-LEP individuals over time. My main specification is:

$$\begin{aligned} lnW_{ikt} = \propto\ &+ LEP_{ikt}\beta_1 + \beta_2 state_k + \beta_3 year_t + \beta_4(LEP_{ikt} \times state_k) \\ &+ \beta_5(year_t \times state_k) + \beta_6(LEP_{ikt} \times year_t) + \beta_7(LEP_{ikt} \times OE_{kt}) \\ &+ X_{ikt}\lambda + \epsilon_{ikt} \end{aligned} \tag{1}$$

where *i* indexes individuals, *k* indexes state, and *t* indexes years. *W* represents the real hourly wage of an individual in 2010 dollars. LEP is equal to one if a worker *i* reports limited English proficiency, while *OE* equals one if the state in which the individual is located has adopted an Official English law in year *t*. *X* is a vector of demographic variables including veteran status, race, age, age squared divided by 100, years of education, children present, married, occupation, and whether the individual identifies as Hispanic. I cluster standard errors at the state level.

The three interaction terms in the model are between LEP status and state, between year and state, between LEP status and year. The triple difference term interacts LEP status and whether an Official English law was passed in the given state at the time.

β_7 is the coefficient of interest and represents the change in the LEP wage penalty between Official English and non-Official English states.

The benefit of this triple difference framework is that it requires few identifying assumptions to give β_7 a causal interpretation. The triple difference framework should not be affected by unobserved ability or other unobserved heterogeneity that can bias other estimates. For example, if English ability is correlated with other unobserved abilities that affect earnings, omitting these variables would bias the estimates. Triple difference estimates do not suffer the same bias if unobservable ability or other characteristics of LEP workers in states with or without Official English laws did not change during the studied period. Should there be any shock in the sample period that affects the wages of LEP workers in states that adopted official English laws differently than it affects the wages of LEP workers in other states, these identifying assumptions could be violated.[12] I address threats to these identifying assumptions in later portions of this Section.

C. Results

Table 5 reports the results of Equation 1 using Census and ACS data for men and women. Consistent with prior research, individuals who do not know English well or at all are at a large wage disadvantage.[13] I find that the there exists a 3.7 log point wage

[12] My analysis assumes that these laws are exogenous and are not influenced by factors such as the in-migration of LEP workers. Using the methodology of Liu (2014), I predict whether states adopted a law over the 1980–2010 time period controlling for the presence of voter initiatives, whether the state was in the south, whether the state voted for a democrat in the 1992 presidential election, and the change in LEP population over the preceding decade. The coefficient of change in the LEP population is not significant at the 10 percent level, lending support to the fact that these are exogenous laws unexplained by underlying factors such as inflows of LEP workers.
[13] I describe my results in log points, as the straightforward interpretation of continuous regressors as a percentage difference in semi-log equations does not hold in the case of estimated coefficients of dummy variables. (Halvorsen and Palmquist 1980). The proper representation of the marginal effect (g) of a dummy variable on the dependent variable is $g * 100$= [exp (C)-1]*100, where C represents the coefficient

penalty between male LEP workers in Official English as compared to LEP workers in non-Official English states. The 3.7 log point coefficient on β_7 is relatively large, as it expands the gap between male LEP workers and their English proficient counterparts by 20%. These results are consistent with—but smaller in magnitude than—past research on Official English laws: Zavodny (2000) finds an 8 percent wage gap for LEP male workers in Official English states during the years of 1980–1990. The smaller wage penalty for male LEP workers in Official English states could be explained by the productivity or quality of LEP workers in official English states improving over time. This smaller wage penalty could also signify that the more recent laws are not enforced as stringently as older laws. I examine enforcement and ties to productivity in Section VI of this Chapter. Given that these laws result in larger wage penalties for LEP male workers, I examine the potential occupation-based mechanisms behind these negative effects in Section V.

Consistent with past research, I find that the wage penalty for LEP women does not differ with the adoption of Official English laws. Table 5 shows that while female LEP workers in non-Official English states are at a wage disadvantage of roughly 10 percent compared to female non-LEP workers, female LEP workers residing in states with Official English laws do not experience any incremental penalty in wages over time. This could be due to female LEP worker's occupational distribution—for example, if they were already in jobs that required little English knowledge both before and after the Official English law was passed. I examine this possibility in Section V.

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from the equation. One log point represents the regression coefficient that has not undergone this transformation. However, for most of my coefficients, log points are roughly equivalent to marginal effects multiplied by 100. For example, a 3 log point change would be equivalent to a marginal effect of 0.03045, or 3 percent.

Though my analysis focuses on only wages as an outcome for LEP workers, I also examine the effects of these laws on other employment outcomes, such as probability of employment, in the Appendix. I find evidence that LEP women and men in Official English states work fewer weeks and hours than their counterparts in Official English states, which could be due to the shift of LEP workers in Official English states to part-time work. I also find evidence that LEP men in Official English states are less likely to be employed.

D. Placebo Tests

Though I find significant effects of Official English laws on LEP male workers, the treatment of residing in a state with an Official English law is not randomly assigned. However, the triple differences methodology that I employ estimates the average treatment effect on the treated as long as LEP worker trends are uncorrelated with the adoption of these laws.

To test this identifying assumption, I conduct a placebo test that examines whether the resulting effects are persistent to an arbitrary change in the date of enactment to a different time period as suggested in Bertrand and Duflo (2004).[14] To perform a placebo test, I adjust all laws that were passed in the 1990–2000 time frame in my sample to reflect that they were instead passed from 1980–1990. I then examine whether I find a similar effect of these laws by using Equation 1. I repeat this by adjusting all laws that were passed in the 1980–1990 time frame to reflect that they were instead passed from 2000–2010. Table 6 shows the results from this placebo test. I find that coefficient for β_7

[14] Ideally, I would be able to perform an event study, which would require measuring the years leading up to the passage of Official English laws. However, because the Census is conducted every decade, I am unable to observe Census data during the years immediately preceding passage of an Official English law.

with these placebo laws is statistically insignificant for all specifications for men and women, providing evidence of a causal effect of Official English laws on LEP wages.

E. Shifts in the LEP Worker Population

During the observed time period, many states that enacted Official English laws also had increasing limited English proficient populations. The influx of foreign individuals to many states could be underlying the observed negative impact of Official English laws on LEP wages. In my main specifications, I include state, year, and state-by-year fixed effects to control for any variation over time in each state. However, this does not specifically control for the underlying trends in LEP population growth. Controlling for the increased percentage of the LEP population in each state will capture any spillover effects that occur due to changes in the LEP population, such as spillovers to non-LEP workers who may complement or substitute LEP workers.

To control for the effect of the change in the limited-English-proficient population share, I use the following equation:

$$\begin{gathered} lnW_{ikt} = \propto + LEP_{ikt}\beta_1 + \beta_2 state_k + \beta_3 year_t + \beta_4(LEP_{ikt} \times state_k) \\ +\beta_5(year_t \times state_k) + \beta_6(LEP_{ikt} \times year_t) + \beta_7(LEP_{ikt} \times OE_{kt}) \\ +\delta_1(\%\Delta\mathrm{Pop}_{kt} \times LEP_{ikt}) + \mathrm{X}_{ikt}\lambda + \epsilon_{ikt} \end{gathered} \tag{2}$$

where $\%\Delta\mathrm{Pop}_{kt}$ is the percentage change between observation years in the fraction of the state population that does not speak English or does not speak English well. I derive this variable from the 1980, 1990, 2000 and 2010 census data. This variable takes the value of 0 for all observations within the 1980 census sample. Interacting this variable with the indicator variable for LEP status of each individual will allow me to compute the effect of the percentage change in the LEP population share on earnings for LEP workers. Comparing β_7 with the values that I obtained from the main specification in Equation 1 will allow me to analyze whether the change in the LEP population has an effect on LEP worker wages in states with Official English laws. Further, the coefficient of interest, δ_1, will allow me to determine whether there is a effect of the increasing LEP population share on LEP status.

Table 7 depicts the results from Equation 2. I find that my main results hold: LEP male workers in Official English states experience a wage penalty of roughly 3 log points relative to others while women do not experience a significant wage penalty. The coefficient on the percentage change of the LEP population is positive in Table 7, which shows that an increase in the LEP population positively affects all male wages. My main coefficient of interest, δ_1, is not significant for either men or women, providing evidence that there is no confounding effect of LEP population shifts in my main specification. Given this analysis, my main results are not sensitive to shifting demographics of the LEP worker population and potential spillover effects that may occur due to inflows of LEP workers.

F. Selective Migration

One of the identifying assumptions of the triple difference framework is that any shock during the sample period that affects the wages of LEP workers in states that adopted official English laws does not differently affect the wages of LEP workers in other states. One such shock could be selective migration, or inflows of LEP workers to states that have not enacted Official English laws to avoid the perceived negative impacts of these laws. Selective migration is particularly problematic for my estimates if the most skilled LEP workers are choosing to leave Official English states in favor of states without such laws.

The Census asks specific questions regarding migration, namely where the individual lived 5 years ago. To partially control for selective migration, I drop all individuals who did not live in the same state when surveyed for the Census and five years prior to the Census survey. As immigrants who are more educated and have better skills are more likely to move to a different city (Bartel and Koch 1991), removing these individuals may also remove individuals that have greater unobservable skills in the labor market. Among my sample, 14 percent of non-LEP individuals moved to a new state in the past 5 years while 9 percent of LEP individuals that did not immigrate to the United States within the past 5 years moved to a new state. When dropping these individuals from the sample, I find similar results for wage outcomes for both men and women. Table 8 shows that LEP men incur roughly a 4 log point wage penalty in Official English states, while LEP women do not incur a significant wage premium or penalty.

In addition to restricting the sample to non-movers, I also test for migration response. Using an individual's residence five years ago, I examine whether an individual residing in a state with an Official English law was more likely to have moved to a non-Official English state. To do this, I use a probit model to test the likelihood of movement with this restricted sample. The dependent variable in this equation is the equal to one if the individual moved from an Official English state to a state without an Official English law. The independent variables are the same as those in Equation 1. Table 9 shows the results from this probit equation. I find that individuals are not more likely to move from an Official English law state to a state without an Official English law, as the coefficient on β_7 is insignificant and close to 0. Therefore, I am confident that my results are not driven by selective migration of individuals moving away from Official English states after these laws are passed.

G. Heterogeneity of Official English Laws

Official English laws vary in form. These laws appear as either statutes or constitutional amendments. It is both theoretically ambiguous and empirically unknown whether constitutional amendments or statutes carry more force in practice. While constitutional amendments require more support to pass, they also require more support to overturn. However, they generally do not provide the level of detail that is present in a statute. Official English statutes generally enumerate specific details or exceptions, but require less support to pass and overturn. Further, broad language such as that usually found in constitutional amendments may be interpreted broadly by courts, while clear statutes that detail the effect of the law may be subject to a more restrained interpretation. Notably, though Georgia already has an Official English statute, legislators in Georgia recently announced plans to lobby for an Official English Constitutional amendment because they believe that this amendment would be more difficult to overturn should the political sentiment for Official English in the state shift.

Table 10 shows that states with statutes and those with constitutional amendments are quite similar along a variety of metrics, including average years of education, age, and gender. The two differ greatly, however, when comparing the proportion of foreign-born individuals and the proportion of Hispanic individuals. There exists a greater proportion of foreign-born (22 percent) and Hispanic individuals (18 percent) in states with Official English constitutional amendments than in states with Official English statutes (2 percent and 6 percent, respectively). Though this disparity exists, it does not follow that there is also a similar gap when considering LEP individuals: LEP individuals make up 4 percent of the workforce in states with Official English statutes and just 6 percent of the workforce in states with Official English constitutional amendments.

I test whether my results are sensitive to the form of these laws to determine other potential explanations for the differences in enforcement of these laws. To test whether statutes or constitutional amendments have differing impacts on LEP worker wages, I modify Equation 1 to include the form (statute or constitutional amendment) of the law. To directly compare these forms, I use the following specification:

$$\begin{gathered} lnW_{ikt} = \propto +LEP_{ikt}\beta_1 + \beta_2 state_k + \beta_3 year_t + \beta_4(LEP_{ikt} \times state_k) \\ +\beta_5(year_t \times state_k) + \beta_6(LEP_{ikt} \times year_t) + \beta_7(LEP_{ikt} \times statute_{kt}) \\ + \beta_8(LEP_{ikt} \times constitution_{kt}) + \mathrm{X}_{ikt}\lambda + \epsilon_{ikt} \end{gathered} \tag{3}$$

where the variable definitions are identical to Equation 1 except for the inclusion of a variable, *statute*, indicating that the state had an Official English statute in effect and *constitution*, indicating that the state had an Official English constitutional amendment in effect. This specification allows me to use states without any Official English law as the control group. β_7 and β_8 are the coefficients of interest in this equation, and they represent the change in the LEP wage penalty between states with Official English statutes and non-Official English states and the change in the LEP wage penalty between states with Official English Constitutional amendments and non-Official English states.

Table 11 shows the results of Equation 3. I find that while the form of the law does not affect the wages of female LEP workers, it greatly impacts those of male LEP workers. I find a 9 log point wage penalty associated with being an LEP male worker in a state that has enacted an Official English statute instead of a Constitutional amendment. I do not find any effect of the form of each law on LEP women's wages. These large effects could be due to the level of detail present in statutes that allow for greater enforcement of the law as compared to Constitutional amendments that usually allow for broad language. These large effects could also be due to other qualities that states with Official English statutes have in common, such as region or large immigrant populations, but my identification strategy addresses these qualities by controlling for state fixed effects.

These results suggest that the large negative impact of these laws is not driven by California, which boasts the highest LEP population of any state. California, as shown in Table 1, adopted an Official English law through a Constitutional amendment rather than a statute. Yet, more negative effects on LEP worker wages are found in states that adopted statutes, which alleviates concern that California is driving my results.[15] However, California does have a voter initiative, which I next examine.

Official English laws also may be adopted through a voter initiative, which allows the public to vote on whether it wants its state to adopt an Official English law. Not unique to either Constitutional amendments or statutes, voter initiatives have the unique ability of testing the voting public's approval of legislative policies, which could examine the general public sentiment towards LEP workers. Voter initiatives to make English the official language of the state have appeared on nine statewide ballots since the beginning of the Official English movement. Surprisingly, all nine such initiatives have passed. Table 1 indicates whether the Official English law in place in a state was adopted by statute or constitutional amendment, and if adopted through voter initiative, what percentage of the voters approved the initiative.

[15] I also drop California from the sample and find no marked change in my results from Equation 1.

Though little evidence exists whether statutes or Constitutional amendments should be stronger in effect, there is theoretical and empirical evidence showing laws that are enacted by voter initiative are more divisive and are more restrictive on the underlying minority population (Arington 1991; Matsusaka 1992). The fact that many of the most recent Official English laws have been passed by voter initiative highlights the importance of divisiveness in this decision. Additionally, it may be in the best interest of a voting politician to disguise their ideologies by not committing to either side and instead allowing voter initiatives to occur (Alesina and Cukierman 1990). Table 1 indicates if the Official English law in each state was passed by voter initiative, and by what margin the initiative won. Many of the most recent initiatives passed by margins of more than 75 percent.

I modify Equation 1 to capture the difference in states with voter initiatives to the following:

$$\begin{aligned} lnW_{ikt} = \propto\ &+ LEP_{ikt}\beta_1 + \beta_2 state_k + \beta_3 year_t + \beta_4(LEP_{ikt} \times state_k) \\ &+ \beta_5(year_t \times state_k) + \beta_6(LEP_{ikt} \times year_t) + \beta_7(LEP_{ikt} \times VI_{kt}) \\ &+ \beta_8(LEP_{ikt} \times NoVI_{kt}) + X_{ikt}\lambda + \epsilon_{ikt} \end{aligned} \tag{4}$$

where the variable definitions are identical to Equation 1 except for the inclusion of a variable, *VI*, which indicates that the state had an Official English voter initiative in effect, and *NoVI*, which indicated that the state had an Official English law that was not passed via voter initiative. States without Official English laws serve as the control group for this specification. The coefficient of interest, β_7, represents the change in the LEP wage penalty in states that have enacted a voter initiative.

Table 12 sheds light on the differences between states that have enacted their Official English laws through voter initiatives and those that have not. There is a far greater percentage of Hispanic individuals in voter initiative states than other states (22 percent compared to 3 percent) and the population of voter initiative states is 26 percent foreign born compared with only 6 percent of foreign-born individuals in other states. Table 13 shows the results of Equation 4. I find that LEP men in states with voter initiatives earn a wage penalty of 3.5 log points. This coefficient is significant at the 5 percent level. Consistent with prior results, I find no effect for women. These findings are robust even when I exclude California residents from my sample, again providing evidence that California is not driving my main results. These results show that states with Official English voter initiatives are having a sizeable impact on the wages of LEP workers, and not all Official English laws have the same impact on LEP workers.

V. Occupational Representation in Official English States

Though I find an impact of Official English laws on male LEP worker wages, it is unclear through which mechanism these effects are occurring. I supplement my analysis with detailed data on occupational requirements to determine if Official English laws affect occupational distribution. If I do not find evidence of worker behavior changes in response to these laws, other mechanisms—such as differences in productivity or employer discrimination—will underlie my results.

Past work has examined the differences in immigrant earnings among occupational classifications (Kossoudji 1998; Chiswick et al. 2005). These studies do not take into account the substantial variation in job requirements among these classifications. For example, the occupational category "managers" encompasses both Chief Executive Officers and cafeteria directors, which have differing levels of English level requirements. I use the Occupational Information Network (O*NET), a detailed dictionary of job descriptors for 798 occupations to capture this variation in English language requirements across and within occupational classifications.

Researchers have used O*NET to examine immigrant workers' specialization and job matching, but have not examined how legal changes affect occupational matching. Peri and Sparber (2009) use O*NET measures of physical tasks and language ability requirements and find that immigrants and native-born workers specialize in jobs with different tasks. Chiswick and Miller (2010) use O*NET language requirement measures coupled with the 2000 Census to show that there is an earnings premium for workers that match their language skills with job requirements. This section extends this work by examining occupation matching not only by occupational language requirements, but also within the context of legal changes regarding language. This Chapter is the first to examine how occupational choice is affected when Official English laws are adopted.

A. Data

O*NET is a comprehensive data source on job characteristics and worker attributes within occupations. O*NET is administered by the Department of Labor as a replacement for the Dictionary of Occupational Titles (DOT). O*NET provides ratings of occupational characteristics and skill requirements, including communication and interpersonal contact requirements. Since the inception of the O*NET 1.0 in 1998, O*NET has been extended and updated based on input from job analysts and workers. I use O*NET Version 21.3, which was released in May 2017.

O*NET contains two questions of particular relevance to this analysis: "how important is knowledge of the English language to the performance of your current job?" and "how important is communicating with others outside of your organization to the performance of your current job?”. I use both of these questions to determine the importance of communication in English to individual’s occupations. O*NET rates the importance of both speaking English and communicating with the public on a five-point scale: (1) not important, (2) somewhat important, (3) important, (4) very important, and (5) extremely important.

To examine how pay varies with job attributes, I merge O*NET with Census and ACS data. O*NET identifies 798 occupations using detailed Standard Occupational Classification (SOC) codes, while the Census and ACS identify workers based on Census occupation codes, which is comprised of 533 occupations. To bridge the gap between these two occupation codes, I created a crosswalk between O*NET and Census occupations using the methodology of Hirsch and Schumacher (2012). There is a one to one match from O*NET occupation codes to Census occupation codes for 491 occupations. Many of the remaining involve mapping two or more O*NET occupations to the Census category. To map these occupations onto the Census, I weight the O*NET descriptor scores of each O*NET occupation using the employment from the corresponding year reported in the Bureau of Labor Statistic’s Occupational Employment Statistics (OES) as weights. If the OES employment is unavailable, I equally weight the O*NET descriptor scores. The SOC also contains codes that encompass “all other” categories, such as “sales and related workers, all others.” O*NET does not have ratings for these occupation codes. Therefore, I assign O*NET values based on average ratings (using employment weights) among similar occupations.

The relevant O*NET variables for my analysis are “speaking,” or how important speaking English is in the occupation, and the importance of communicating with persons outside of the individual's organization. On average, LEP workers are represented in occupations that have lower importance ratings for both speaking English and communicating with the public. The average importance of speaking English score for LEP worker occupations is 3.12 on a scale of 1–5, with a standard deviation of 0.44. This average importance is slightly lower than the mean score of English proficient workers, who have a mean importance score of 3.54 and a standard deviation of 0.45. The importance scores of communicating with the public tell a similar story: the average for LEP workers is 2.76 with a standard deviation of 0.64, while the average for English proficient workers is 3.28 with a standard deviation of 0.69. These statistics accord with Chiswick and Miller's findings (2010) that LEP workers are likely to be in occupations that place a lower importance on speaking English or communicating with the public. I next determine if these differences are also apparent in states with or without Official English laws.

B. Are LEP Workers Less Represented in Occupations that Require English Skills in Official English States?

I next test whether workers alter their occupational preferences based on the presence of an Official English law in their state of residence. To determine how occupational sorting and official English laws impact occupational representation, I modify Equation 1 to the following structure:

$$Z_{ikt} = \propto\ + LEP_{ikt}\beta_1 + \beta_2 state_k + \beta_3 year_t + \beta_4(LEP_{ikt} \times state_k) + \beta_5(year_t \times state_k) + \beta_6(LEP_{ikt} \times year_t) + \beta_7(LEP_{ikt} \times OE_{kt}) + X_{ikt}\lambda + \epsilon_{ikt} \tag{5}$$

where the dependent variable, Z_{ikt}, represents either how important speaking English or communicating with the public is in the individual's occupation. The independent variables remain the same as in Equation 1. β_7 is the variable of interest and represents the change in the importance of speaking English or communicating with the public for LEP individuals in Official English states.

Table 14 reports the results from Equation 5. My results show that, consistent with my hypothesis, LEP workers are not, over time, less represented in jobs that require English based on whether their state of residence has an Official English law. For both men and women, the coefficient of interest, β_7, is not statistically significant when considering both the importance of speaking English or the importance of communicating with others as the dependent variable. Hispanic men and women are likely to be less represented in jobs that place high levels of importance on speaking English or communicating with others. LEP men and women are neither more nor less likely to be represented in occupations with high importance on the English language.

Despite this lack of differences in representation in occupations that place a high level of importance on the English language, there still exists a wage penalty over time for LEP workers in Official English states. I examine whether this wage penalty persists when controlling for occupation characteristics. To do this, I modify Equation 1 to the following:

$$\begin{gathered} lnW_{ikt} = \propto \; + \delta\, Z_i + LEP_{ikt}\beta_1 + \beta_2 state_k + \beta_3 year_t \\ + \beta_4(LEP_{ikt} \times state_k) + \beta_5(year_t \times state_k) + \beta_6(LEP_{ikt} \times year_t) \\ + \beta_7(LEP_{ikt} \times OE_{kt}) + \mathrm{X}_{ikt}\lambda + \epsilon_{ikt} \end{gathered} \tag{6}$$

where Z_i represents either how important speaking English or communicating with the public is in the individual's occupation. The independent variables remain the same as in Equation 1. β_7 is the coefficient of interest and represents the change in the LEP wage penalty between Official English and non-Official English states after controlling for the importance of communication in the individual's occupation.

Table 15 shows the results of Equation 6. I find that the wage gap persists for LEP men in Official English states when controlling for occupational level characteristics. Workers in occupations with high importance levels of speaking English or communicating with others earn significant wage premiums. Men in jobs that place a high importance on speaking English earn 12.5 log points more, while women in high earn 16.5 log points more than those in jobs that place a lesser importance on speaking English. However, this does not explain the wage gap over time for LEP men. Even when controlling for these occupational characteristics, male LEP workers still experience a wage penalty of roughly 3.4 to 3.7 log points. Consistent with my main results, women do not experience any wage penalty. Therefore we must consider other qualities, other than the importance of the English language in individual's occupations, to explain the wage gap. I next turn to worker productivity.

VI. LEP Worker Productivity in Official English States

Official English laws could also affect LEP worker wages if these laws caused a decrease in productivity of LEP workers. However, worker productivity should not vary depending on whether the state of residence has adopted an Official English law, given that the quality of LEP workers is not markedly different between states. There is a possibility for these laws to affect the productivity of workers given that they can directly impede the ability of LEP workers to obtain necessary services, such as driver's licenses. As stronger enforcement of these laws will lead to greater restrictions on government services that LEP workers may rely on, I proxy for worker productivity and enforcement by looking at states that only allow driver's license exams in English.

Though all modern state Official English laws stem from the same Official English movement, they are heterogeneous in level of enforcement. For example, though Kentucky's Official English law declares English the official language of the state and requires all government workers to conduct government business in English, Kentucky's Department of Labor employees were placed in mandatory Spanish language training to "effectively communicate" with workers. This suggests that Kentucky's Official English law may be only symbolic and have no legal force. Yet, even symbolic laws may lead to negative consequences if they encourage discrimination against LEP workers (Califa 1989). This section uses proxies for enforcement to determine whether stronger laws have a greater impact on LEP worker wages or if the presence of the law, regardless of enforcement, impacts LEP worker wages.

To determine how enforcement measures after laws are enacted may impact LEP workers, I look to states that allow driver's license tests to be taken in only English. I modify my main specification to interact the LEP coefficient with a dummy variable equal to one if a state only allows a driver's license test in English. There are currently eight states with driver's license tests in English only: Hawaii, Arizona, Wyoming, South Dakota, Kansas, Oklahoma, New Hampshire, and Maine. Maine is the only one of those that does not have a corresponding Official English law, and I therefore exclude Maine from my analysis in this section.

To examine the impacts of driver's license exams on LEP worker's wages, I modify Equation 1 to the following:

$$\begin{aligned} lnW_{ikt} = \propto\ &+ LEP_{ikt}\beta_1 + \beta_2 state_k + \beta_3 year_t + \beta_4(LEP_{ikt} \times state_{ikt}) \\ &+ \beta_5(year_t \times state_{ikt}) + \beta_6(LEP_i \times year_t) \\ &+ \beta_7(LEP_{ikt} \times OED_{kt}) + \mathrm{X}_{it}\lambda + \epsilon_{ikt} \end{aligned} \tag{7}$$

where the variable definitions are identical to Equation 1 except for the inclusion of a variable, *OED*, which indicates that the state had a law that allowed driver's license exams in English only. States without Official English laws that allow for driver's license exams in other languages serve as the control group for this specification. The coefficient of interest is β_7, which represents the change in the LEP wage penalty in states with driver's license exams in English only.

Table 16 shows that there are 71,672 individuals in my sample that live in states that have enacted driver's license tests in English only, and these individuals look similar to those in states offering driver's license tests in other languages. A greater proportion of foreign-born individuals (11 percent) live in states with tests offered in other languages than those with tests offered in English only, where the foreign-born population makes up 8 percent of individuals. There are also more LEP individuals in states that offer these driver's license exams in other languages.

Table 17 shows the results of Equation 7 and shows that LEP workers wages are unaffected by whether states with driver's license exams in English only. The impact on men's wages is negative, but insignificant at the 10 percent level, while the impact on LEP women is positive but insignificant at the 10 percent level. Therefore, greater enforcement of Official English laws, which would lead to less access to important government services—in this case driver's licenses—does not appear to have an impact on the productivity of LEP workers. Yet, Official English laws continue to have an effect on LEP worker wages. The next section discusses an alternative mechanism—employment discrimination—that could account for the unexplained effects of Official English laws on LEP workers.

VII. Employment Discrimination

Thus far, there exist differences in LEP worker wages in Official English states that remain unexplained by observable characteristics, including occupational choice and worker productivity. This suggests that employers may be discriminating against LEP workers, or foreign-born workers generally. In the employer discrimination model of Becker (1957), some employers are unwilling to hire minority workers at the majority wage because they derive disutility from doing so. This can result from distaste for a specific subset of the population or could be due to profit-focused motives, which could be driven by observations that workers become more productive when they share a common language or business culture (Aslund et. al 2014). To determine whether employers are motivated by profit-driven motives or by taste-based discrimination, I first look at whether these laws operate to impact all foreign-born minorities, which should not occur if the employer does not exhibit taste-based discrimination. Next, if profit-driven motives are occurring, all LEP workers should be treated equally. I examine whether the effects of these laws disadvantage particular LEP minority groups and find that a taste-based discrimination story is likely.

There are profit-motivated reasons for why employers would discriminate against LEP workers. For example, there is evidence that workers are more productive if they share a language with their managers (Lazear 1999), and managers could therefore reward English proficient workers with higher wages compared to their LEP counterparts, especially in states with Official English laws due to the emphasis on the English language. If managers are risk averse, or acquiring information about applicants is costly, managers can experience more efficient selection processes by focusing on workers with a similar background to their own due to less noise in productivity signals from applicants (Fang and Moro 2011). This would cause managers to hire fewer LEP workers, especially in Official English states where the productivity signals from LEP workers may carry more noise due to the salience of the importance of the English language. However, under these assumptions, all LEP workers should be treated equally. If white LEP individuals do not suffer the same labor market consequences in Official English states as do LEP individuals of other races, something other than LEP status is working to disadvantage LEP workers.

To test whether this is occurring in my sample, I restrict my sample to just one ethnic group (Hispanic, Asian, and White) to compare results between LEP and English proficient workers using Equation 1. The coefficient of interest remains β_7, but here this coefficient represents the change in LEP worker wages in Official English states over time relative to workers of their same minority group. Table 18 shows that male LEP Hispanic workers in Official English states experience a 6 log point wage penalty in Official English states. LEP Hispanic women in Official English states are neither better nor worse off over time, but LEP Hispanic women do experience a 9 log point wage penalty that is significant at the 10 percent level. These results show that these laws are strongly affecting LEP workers of Hispanic ethnicity.

Table 19 reports that Asian LEP workers in states with Official English laws experience an 11 log point wage penalty. Like LEP Hispanic women, LEP Asian women in Official English states are neither better nor worse off than their ethnic counterparts. Given these results, it is not foreign-born status that is driving the negative impacts of Official English laws on LEP workers, but LEP status. Table 20 shows that being a white

LEP worker does not translate to a wage penalty relative to other white workers in Official English states. Though being a white LEP worker is disadvantageous, there is no significant effects over time for white LEP workers residing in Official English states. This result could be driven by the relatively few LEP workers in my sample. However, my results do provide limited evidence for the existence of employment discrimination in Official English states.

Therefore, though these laws do not operate to produce discrimination against specific ethnicities as a whole, they may allow employers to discriminate against non-white LEP workers. The mechanism underlying these Official English laws may be employer discrimination that does not exhibit a distaste for all foreign-born individuals but rather for LEP minority workers. The differential treatment of white LEP individuals shows that this discrimination is unlikely to be attributed to profit-maximizing behavior.

VIII. Conclusion

This chapter has shown that modern Official English laws are associated with a decline in male LEP wages. The contribution of this chapter to the existing literature on Official English laws shows that these negative effects on male LEP wages cannot be explained by occupational representation or individual characteristics. I conclude that one likely mechanism behind the effects of these laws is employer discrimination, focused specifically on minority LEP workers rather than on all foreign-born individuals.

Rather than focus on the change in actual English knowledge of LEP individuals, my analysis examines the employment outcomes of workers. My analysis still allows for the unintended consequences of these laws to be another driver behind negative LEP worker outcomes. These laws can encourage the ethnocentrism of LEP individuals, and through this mechanism, workers may turn to networks to obtain employment that would not require the use of the English language. Through these networks, or through an LEP worker's own observations, there may be a feedback mechanism such that workers in Official English states anticipate employer discrimination in jobs that place an importance on the English language and therefore they choose other jobs through this mechanism. Future research is needed to determine whether these unintended consequences are occurring and the magnitude of their effects.

Further these Official English laws may coincide with the passage of other immigration-related measures, and these effects could capture anti-immigrant sentiment more generally. In this way, Official English laws are only one manifestation of nativism and the defenses to vulnerability observed in Official English states. My next Chapter looks at another immigration-related measure to determine how laws focused on crime, rather than language, can impact immigrant worker labor market outcomes.

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FIGURES AND TABLES

Figure 1: States with an Official English Law as of 1980

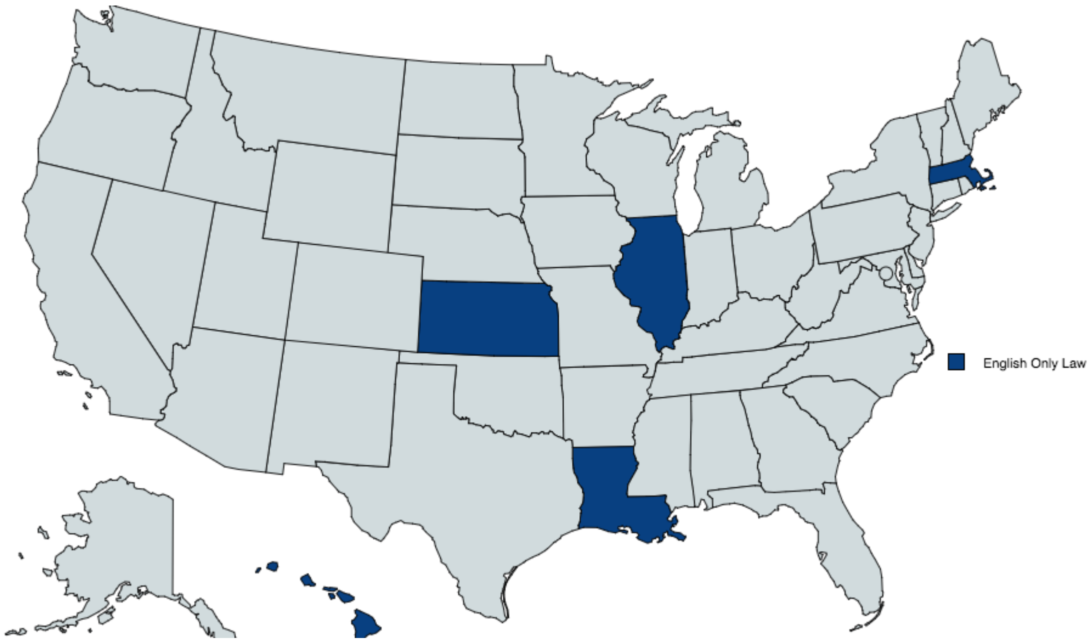

Figure 2: States with an Official English Law as of 2000

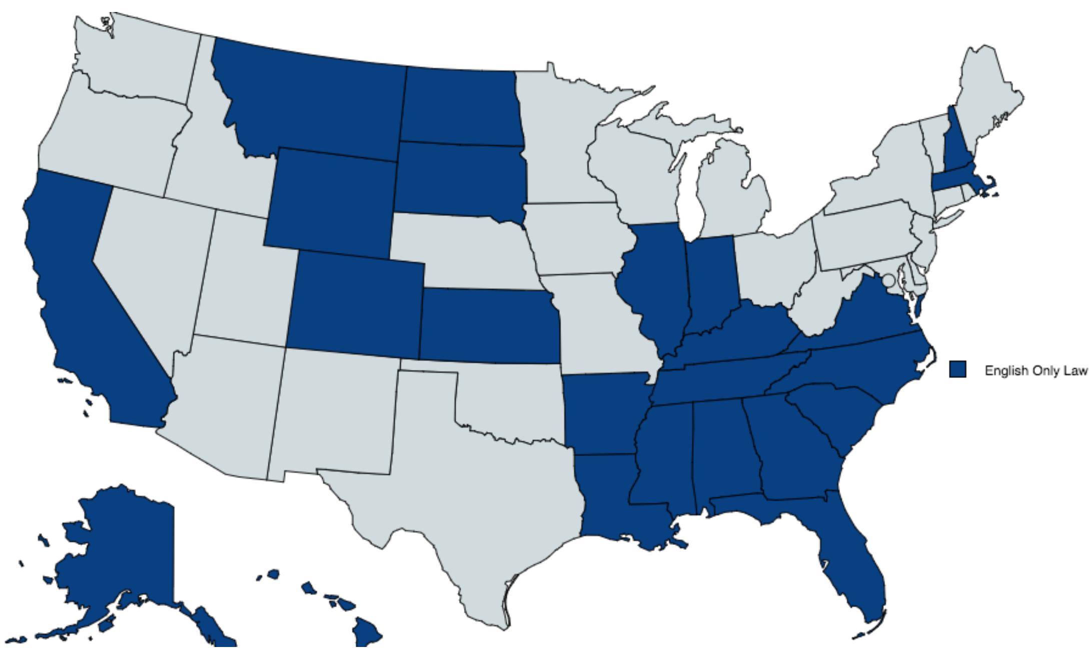

Figure 3: States with an Official English Law as of April 2016, by Form

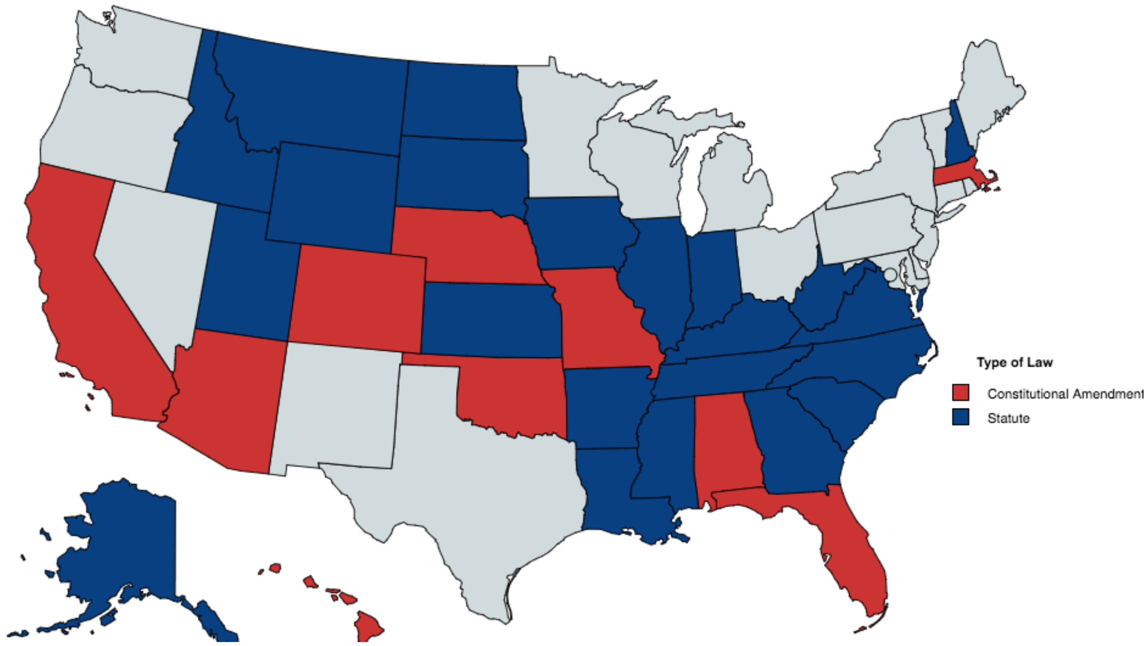

Table 1: List of All States with Official English Laws

State	Year of Adoption	Form of Law	Popular Voter Initiative, Percentage in Favor
Alabama	1990	Constitutional Amendment	Yes, 90%
Alaska	1998	Statute	Yes, 69%
Arizona	2006	Constitutional Amendment	Yes, 74%
Arkansas	1987	Statute	No
California	1986	Constitutional Amendment	Yes, 73%
Colorado	1988	Constitutional Amendment	Yes, 61%
Florida	1988	Constitutional Amendment	Yes, 84%
Georgia	1996	Statute	No
Hawaii	1978	Constitutional Amendment	No
Idaho	2007	Statute	No
Illinois	1969	Statute	No
Indiana	1984	Statute	No
Iowa	2002	Statute	No
Kansas	2007	Statute	No
Kentucky	1984	Statute	No
Louisiana	1807	Statute	No
Massachusetts	1975	Constitutional Amendment	No
Mississippi	1987	Statute	No
Missouri	2008	Constitutional Amendment	Yes, 86%
Montana	1995	Statute	No
Nebraska	1920	Constitutional Amendment	Yes, 63%
New Hampshire	1995	Statute	No
North Carolina	1987	Statute	No
North Dakota	1987	Statute	No
Oklahoma	2010	Constitutional Amendment	Yes, 76%
South Carolina	1987	Statute	No
South Dakota	1995	Statute	No
Tennessee	1984	Statute	No
Utah	2000	Statute	Yes, 67%
Virginia	1981,1996	Statute	No
West Virginia	2016	Statute	No
Wyoming	1996	Statute	No

Table 2: Summary Statistics by Presence of Official English Law in State of Residence, 1980–2010

Variable	Official English Law	No Official English Law
Male	0.59	0.61
Age	40.17	39.68
Married	0.61	0.64
Years of Education	13.35	13.22
Hispanic	0.11	0.08
Black	0.12	0.09
Asian	0.05	0.03
Disabled	0.01	0.02
Limited English Proficient	0.04	0.02
Foreign Born	0.14	0.10
Wage (in 2010 dollars)	22.54	22.16
N	1,277,402	1,642,934

Note: Data shown is comprised of the 1% Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Weighted using sample weights. Individuals are considered limited English proficient if they reported that they speak English "not-well" or "not at all." Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Those born in Guam, Puerto Rico, or the U.S. Virgin Islands were excluded from this analysis, as were citizens born abroad.

Table 3: Summary Statistics by Presence of Official English Law in State of Residence, LEP Workers Only

Variable	Official English Law	No Official English Law
Male	0.68	0.68
Age	38.46	38.85
Married	0.55	0.57
Years of Education	9.44	9.45
Hispanic	0.75	0.72
Black	0.03	0.03
Asian	0.05	0.06
Wage (in 2010 dollars)	13.03	13.12
N	37,668	29,656

Note: Data shown is comprised of the 1% Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Weighted using sample weights. Individuals are considered limited English proficient if they reported that they speak English "not-well" or "not at all." Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Those born in Guam, Puerto Rico, or the U.S. Virgin Islands were excluded from this analysis, as were citizens born abroad.

Table 4: Summary Statistics by English Proficiency

Variable	Limited English Proficient Immigrant	English Proficient Immigrant	Native
Male	0.68	0.62	0.60
Age	38.66	39.99	39.92
Married	0.56	0.64	0.62
Years of Education	9.11	13.43	13.37
Hispanic	0.72	0.32	0.07
Black	0.02	0.10	0.10
Asian	0.16	0.17	0.00
Disabled	0.00	0.00	0.01
Wage (in 2010 dollars)	12.44	23.51	22.50
	67,324	240,746	2,612,266

Note: Data shown is comprised of the 1% Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Weighted using sample weights. Individuals are considered limited English proficient if they reported that they speak English “not-well” or “not at all.” I restrict the sample to those working full time, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Those born in Guam, Puerto Rico, or the U.S. Virgin Islands were excluded from this analysis, as were citizens born abroad.

Table 5: Triple Difference Estimates of Official English Laws, 1980–2010

	Men	Women
Age	0.0627***	0.0544***
	(0.0003)	(0.0003)
Age Squared / 100	-0.0604***	-0.0562***
	(0.0004)	(0.0004)
Years of Education	0.0709***	0.0853***
	(0.0002)	(0.00027)
Hispanic	-0.0964***	-0.0513***
	(0.0022)	(0.0025)
Asian	-0.0731***	-0.0010
	(0.0053)	(0.0054)
LEP	-0.2055***	-0.1020***
	(0.0333)	(0.0341)
OE Law × LEP	-0.0369**	-0.0008
	(0.0149)	(0.0194)
Constant	0.1734***	-0.1720***
	(0.0354)	(0.0376)
Observations	1,744,056	1,163,228
R-squared	0.3133	0.3145

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of the 1% Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009-2013 American Community Survey. Sample weights are used. Sample is restricted to full-time and full-year workers earning between \$1.50 and \$300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Additional controls include occupation, marital status, race, Hispanic status, children present, year of entry, and veteran status. Individuals are considered limited English proficient (LEP) if they reported that they speak English "not-well" or "not at all." Sample is restricted to workers earning between \$1.50 and \$300 per hour who are not self-employed. "OE Law" indicates the presence of an Official English law in state *k* at time *t*. Table also shows robust standard errors clustered at the state level.

Table 6: Triple Difference Estimates of Official English Laws, Placebo Tests

	1990s Law Passed in 1980		1980s Law Passed in 2000s	
	Men	**Women**	**Men**	**Women**
Age	0.0664***	0.0551***	0.0595***	0.0558***
	(0.0004)	(0.0005)	(0.0004)	(0.0004)
Age Squared / 100	-0.0643***	-0.0589***	-0.0574***	-0.0589***
	(0.0005)	(0.0006)	(0.0005)	(0.0006)
Years of Education	0.0629***	0.0747***	0.0791***	0.0922***
	(0.0003)	(0.0004)	(0.0003)	(0.0003)
Hispanic	-0.0795***	-0.0333***	-0.0806***	-0.0602***
	(0.0031)	(0.0036)	(0.0027)	(0.0028)
Asian	0.0261***	0.0433***	0.0606***	0.0291***
	(0.0088)	(0.0090)	(0.0071)	(0.0068)
LEP	-0.1908***	-0.0659	-0.2152***	-0.1411***
	(0.0420)	(0.0508)	(0.0447)	(0.0405)
OE Law × LEP	0.0038	-0.0041	0.0087	-0.0311
	(0.0202)	(0.0252)	(0.0202)	(0.0252)
Constant	0.3368***	0.2277***	0.3368***	0.2277***
	(0.0437)	(0.0525)	(0.0150)	(0.0525)
Observations	783,902	459,010	960,154	704,218
R-squared	0.2848	0.2463	0.3319	0.2463

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of the 1% Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample weights are used. Sample is restricted to full-time and full-year workers earning between \$1.50 and \$300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Additional controls include occupation, marital status, race, Hispanic status, children present, year of entry, and veteran status. Individuals are considered limited English proficient (LEP) if they reported that they speak English "not-well" or "not at all." Sample is restricted to workers earning between \$1.50 and \$300 per hour who are not self-employed. "OE Law" indicates the presence of an Official English law in state *k* at time *t*. Table also shows robust standard errors clustered at the state level.

Table 7: The Effects of Changes in the LEP Population on Wages

	Men	**Women**
Age	0.0627***	0.0544***
	(0.0003)	(0.0003)
Age Squared / 100	-0.0604***	-0.0562***
	(0.0004)	(0.0004)
Years of Education	0.0709***	0.0853***
	(0.0002)	(0.0003)
Hispanic	-0.0964***	-0.0513***
	(0.0022)	(0.0025)
Asian	-0.1002***	-0.0141***
	(0.0034)	(0.0034)
% Change in LEP Population	0.0059***	0.0054
	(6.7501)	(5.9691)
% Change in LEP Population × LEP	0.0032	0.0053
	(0.0059)	(0.0072)
LEP	-0.2069***	-0.1040***
	(0.0334)	(0.0342)
OE Law × LEP	-0.0348**	0.0019
	(0.0149)	(0.0194)
Constant	0.1694***	-0.1980***
	(0.0340)	(0.0357)
Observations	1,744,056	1,163,228
R-squared	0.3133	0.3140

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Sample weights are used. Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample restricted to those working full time, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Sample restricted to workers earning between $1.50 and $300 per hour. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, race, Hispanic status, children present, year of entry, and veteran status. Individuals are considered limited English proficient if they reported that they speak English "not-well" or "not at all." The change in the LEP population was measured between each Census year from 1980–2010 using Census data. "OE Law" indicates the presence of an Official English law in state *k* at time *t*. Table also shows robust standard errors clustered at the state level.

Table 8: Triple Difference Estimates Without Migrants

	Men	Women
Age	0.0617***	0.0549***
	(0.0003)	(0.0003)
Age Squared / 100	-0.0610***	-0.0512***
	(0.0002)	(0.0003)
Years of Education	0.0756***	0.0853***
	(0.0002)	(0.0003)
Hispanic	-0.0965***	-0.0521***
	(0.0022)	(0.0024)
Asian	-0.0987***	-0.0121***
	(0.0033)	(0.0034)
LEP	-0.2012***	-0.1029***
	(0.0333)	(0.0342)
OE Law × LEP	-0.0430**	0.0019
	(0.0149)	(0.0194)
Constant	0.1710***	-0.1720***
	(0.0352)	(0.0372)
Observations	1,395,291	940,290
R-squared	0.3323	0.3120

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample weights are used. Sample restricted to those working full time, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Sample restricted to workers earning between $1.50 and $300 per hour. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, race, children present, year of entry, and veteran status. Individuals are considered limited English proficient (LEP) if they reported that they speak English “not-well” or “not at all.” “OE Law” indicates the presence of an Official English law in state *k* at time *t*. Individuals that moved states in the past five years were excluded from the sample.

Table 9: Probit Estimates, Probability of Moving to an Official English State

	Men	**Women**
Age	-0.0046**	-0.1123***
	(0.0018)	(0.0023)
Age Squared / 100	-0.0357***	-0.0260***
	(0.0002)	(0.0028)
Years of Education	0.0346***	0.0271***
	(0.0111)	(0.0015)
Hispanic	-0.0431***	-0.0737***
	(0.0022)	(0.0024)
Asian	-0.0163***	-0.0469***
	(0.0033)	(0.0034)
LEP	-0.2434	0.1429***
	(0.1866)	(0.0342)
OE Law × LEP	0.0055	-0.0019
	(0.3749)	(0.4608)
Constant	-1.1710***	-0.1237***
	(0.0374)	(0.0372)
Observations	1,744,056	1,163,228

***p<0.01; **p<0.05; *p<0.10. The dependent variable is equal to 1 if the individual moved from a state with an Official English law to a state without an Official English law. Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample weights are used. Sample restricted to those working full time, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Sample restricted to workers earning between $1.50 and $300 per hour. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, race, children present, year of entry, and veteran status. Individuals are considered limited English proficient (LEP) if they reported that they speak English "not-well" or "not at all." "OE Law" indicates the presence of an Official English law in state *k* at time *t*. Individuals that lived in the same state five years ago at the time of survey were excluded from the sample.

Table 10: Summary Statistics: Official English Laws by Form

Variable	Statute	Constitutional Amendment
Male	0.57	0.58
Age	40.19	40.25
Married	0.61	0.63
Years of Education	13.20	13.40
Hispanic	0.06	0.18
Black	0.17	0.10
Asian	0.02	0.07
Disabled	0.01	0.01
Limited English Proficient	0.04	0.06
Foreign Born	0.02	0.22
Wage (in 2010 dollars)	20.16	23.71
N	323,699	571,810

Note: Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009-2013 American Community Survey. Weighted using sample weights. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Only states with an Official English law are included in this sample.

Table 11: Triple Difference Estimates, Form of Official English Laws

	Men	Women
Age	0.0627***	0.0544***
	(0.0003)	(0.0003)
Age Squared / 100	-0.0604***	-0.0562***
	(0.0004)	(0.0004)
Years of Education	0.0709***	0.0853***
	(0.0002)	(0.0003)
Hispanic	-0.0963***	-0.0513***
	(0.0022)	(0.0025)
Asian	-0.1002***	-0.0141***
	(0.0034)	(0.0034)
LEP	-0.2057***	-0.1032***
	(0.0333)	(0.0341)
Statute × LEP	-0.0933***	0.0274
	(0.0325)	(0.0377)
Constitutional Amendment × LEP	0.0095	-0.0017
	(0.0118)	(0.0157)
Constant	0.1734***	-0.1721***
	(0.0354)	(0.0376)
Observations	1,744,056	1,163,228
R-squared	0.3133	0.3140

***p<0.01; **p<0.05; *p<0.10. The dependent variable the natural log of wages in 2010 dollars. Sample weights are used. Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009-2013 American Community Survey. Sample restricted to those working full time, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Additional controls include occupation, marital status, race, Hispanic status, children present, year of entry, and veteran status. Individuals are considered limited English proficient (LEP) if they reported that they speak English "not-well" or "not at all." "Statute" indicates the presence of an Official English law in the form of a statute in state *k* at time *t*. "Constitution" indicates the presence of an Official English law in the form of a constitutional amendment in state *k* at time *t*.

Table 12: Summary Statistics: Voter Initiatives

Variable	Voter Initiative	No Voter Initiative
Male	0.59	0.58
Age	40.13	40.32
Married	0.57	0.63
Years of Education	13.37	13.28
Hispanic	0.22	0.03
Black	0.09	0.16
Asian	0.08	0.02
Disabled	0.01	0.01
Limited English Proficient	0.07	0.02
Foreign Born	0.26	0.06
Wage (in 2010 dollars)	24.01	20.84
N	447,191	448,288

Note: Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009-2013 American Community Survey. Weighted using sample weights. Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are no self-employed. Age was restricted to those between the ages of 18 and 65. Only individuals living in state with Official English laws are included.

Table 13: Triple Difference Estimates, Voter Initiatives

	Men	Women
Age	0.0633***	0.0546***
	(0.0003)	(0.0003)
Age Squared / 100	-0.0609***	-0.0563***
	(0.0004)	(0.0004)
Years of Education	0.0705***	0.0851***
	(0.0002)	(0.0003)
Hispanic	-0.0835***	-0.0562***
	(0.0020)	(0.0022)
Asian	-0.0725***	-0.0005
	(0.0052)	(0.0051)
LEP	-0.2021***	-0.1031***
	(0.0329)	(0.0347)
Voter Initiative × LEP	-0.0353**	-0.0117
	(0.0149)	(0.0198)
Non-Voter Initiative × LEP	0.0519***	0.0549***
	(0.0064)	(0.0076)
Constant	0.1613***	-0.1410***
	(0.0344)	(0.0371)
Observations	1,744,056	1,163,228
R-squared	0.3133	0.3140

***p<0.01; **p<0.05; *p<0.10. The dependent variable the natural log of wages in 2010 dollars. Data showr is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample weights are used. Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Additional controls include occupation, marital status, race Hispanic status, children present, year of entry, and veteran status. Individuals are considered limited Englist proficient (LEP) if they reported that they speak English "not-well" or "not at all." "Voter Initiative" indicates the presence of an Official English law that was passed via voter initiative in state *k* at time *t*. "Non-Voter Initiative" indicates whether a state had an Official English law but no voter initiative at time *t*.

Table 14: Occupational Representation in Official English States

	Men	Women	Men	Women
	Importance of Speaking English	**Importance of Speaking English**	**Importance of Communicating with Others**	**Importance of Communicating with Others**
Age	0.0137***	0.0043***	0.0160***	0.0073***
	(0.0002)	(0.0002)	(0.0004)	(0.0004)
Age Squared / 100	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Years of Education	0.0699***	0.0625***	0.0757***	0.0624***
	(0.0001)	(0.0002)	(0.0002)	(0.0003)
Hispanic	-0.0080***	-0.0257***	-0.0193***	-0.0426***
	(0.0016)	(0.0017)	(0.0025)	(0.0030)
Asian	-0.0065	-0.0108	-0.0117	-0.0147
	(0.0020)	(0.0023)	(0.0033)	(0.0037)
LEP	0.0018	-0.0254	0.0480	-0.0084
	(0.0230)	(0.0313)	(0.0318)	(0.0442)
OE Law × LEP	-0.0001	0.0042	-0.0101	0.0091
	(0.0091)	(0.0132)	(0.0127)	(0.0183)
Constant	1.7437***	2.0492***	1.3875***	1.7689***
	(0.0241)	(0.0331)	(0.0340)	(0.0476)
Observations	1,744,056	1,163,228	1,744,056	1,163,228
R-squared	0.3401	0.2960	0.2259	0.1897

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the importance of speaking English or the importance of communicating with others in each individual's occupation. Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey merged with O*NET Version 21.3. Sample weights are used. Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Additional controls include occupation marital status, race, Hispanic status, children present, year of entry, and veteran status. Individuals are considered limited English proficient (LEP) if they reported that they speak English "not-well" or "not at all." The dependent variables, "Speak" and "Communicating with Others," indicate the importance of speaking or communicating with others in the individual's occupation. These importance levels are reported in O*NET and represent a score on a 5-point scale for each occupation. For more information about the scores, see Section V.A.

Table 15: LEP and Occupational Characteristics in Official English States

	Men	Women	Men	Women
Age	0.0610***	0.0536***	0.0619***	0.0537***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Age Squared / 100	-0.0000***	-0.0000***	-0.0000***	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Years of Education	0.0621***	0.0751***	0.0673***	0.0797***
	(0.0002)	(0.0003)	(0.0002)	(0.0003)
Hispanic	-0.0954***	-0.0470***	-0.0955***	-0.0474***
	(0.0022)	(0.0025)	(0.0022)	(0.0025)
Asian	-0.0921***	0.0026	-0.0944***	-0.0007
	(0.0033)	(0.0034)	(0.0033)	(0.0034)
Speak	0.1250***	0.1648***		
	(0.0012)	(0.0014)		
Communicate			0.0478***	0.0897***
			(0.0007)	(0.0008)
LEP	-0.2065***	-0.1022***	-0.2086***	-0.1057***
	(0.0328)	(0.0328)	(0.0332)	(0.0337)
OE Law × LEP	-0.0342**	0.0018	-0.0368**	0.0021
	(0.0149)	(0.0194)	(0.0156)	(0.0204)
Constant	-0.6410***	-0.5067***	0.8760***	-0.3277***
	(0.0344)	(0.0355)	(0.0347)	(0.0363)
Observations	1,744,056	1,163,228	1,744,056	1,163,228
R-squared	0.3188	0.3239	0.3155	0.3226

***p<0.01; **p<0.05; *p<0.10. The dependent variable is natural log of an individual's wage in 2010 dollars. Data shown is comprised of a 5% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009-2013 American Community Survey merged with O*NET Version 21.3. Sample weights are used. Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Additional controls include occupation, marital status, race, Hispanic status, children present, year of entry, and veteran status. Individuals are considered limited English proficient (LEP) if they reported that they speak English "not-well" or "not at all." Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. OE Law" indicates the presence of an Official English law in state *k* at time *t*. "Speak" and "Communicating with Others" indicate the importance of speaking or communicating with others in the individual's occupation. These importance levels are reported in O*NET and represent a score on a 5-point scale for each occupation. For more information about the scores, see Section V.A.

Table 16: Summary Statistics: Driver's License Tests in Only English

Variable	**Tests Offered in English Only**	**Tests Offered in Other Languages**
Male	0.61	0.58
Age	40.97	40.21
Married	0.64	0.60
Years of Education	13.67	13.32
Hispanic	0.07	0.13
Black	0.09	0.16
Asian	0.08	0.02
Disabled	0.01	0.13
Limited English Proficient	0.02	0.04
Foreign Born	0.08	0.11
Wage (in 2010 dollars)	23.05	22.42
N	71,672	2,848,913

Note: Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Weighted using sample weights. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Only states with an Official English law are included in this sample.

Table 17: Triple Difference Estimates, English Only Drivers' License Exams

	Men	Women
Age	0.0627***	0.0544***
	(0.0003)	(0.0003)
Age Squared / 100	-0.0604***	-0.0562***
	(0.0004)	(0.0004)
Years of Education	0.0707***	0.0854***
	(0.0002)	(0.0003)
Hispanic	-0.0843***	-0.0555***
	(0.0021)	(0.0023)
Asian	-0.0731***	0.0000
	(0.0053)	(0.0052)
LEP	-0.2050***	-0.1057***
	(0.0332)	(0.0340)
EO Driver's License Exam		
× LEP	-0.0121	0.1175
	(0.0683)	(0.0820)
Constant	0.1565***	-0.1741***
	(0.0348)	(0.0367)
Observations	1,744,056	1,163,228
R-squared	0.3132	0.3140

***p<0.01; **p<0.05; *p<0.10. The dependent variable the natural log of wages in 2010 dollars. Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample weights are used. Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, race, children present, year of entry, and veteran status. Individuals are considered limited English proficient (LEP) if they reported that they speak English "not-well" or "not at all." "EO Driver's License Exam" indicates whether a state allowed their exam to obtain drivers' license laws in only English in state *k* at time *t*.

Table 18: Triple Difference Estimates on Wages, Hispanic Only

Variable	Men ln(wages)	Women ln(wages)
Age	0.0557***	0.0512***
	(0.0010)	(0.0012)
Age Squared / 100	-0.0570***	-0.0544***
	(0.0012)	(0.0016)
Years of Education	0.0385***	0.0513***
	(0.0005)	(0.0007)
LEP	-0.2389***	-0.0964*
	(0.0608)	(0.0585)
OE Law × LEP	-0.0621***	0.0144
	(0.0177)	(0.0519)
Constant	0.7418***	0.2933***
	(0.0910)	(0.0911)
Observations	147,780	87,369
R-squared	0.3060	0.3154

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Sample weights are used. Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample restricted to those working full time, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, race, children present, year of entry, and veteran status. Individuals are considered limited English proficient if they reported that they speak English “not-well” or “not at all.” The change in the LEP population was measured between each Census year from 1980–2010 using Census data. Only Hispanic individuals were included in the sample.

Table 19: Triple Difference Estimates on Wages, Asian Only

Variable	Men ln(wages)	Women ln(wages)
Age	0.0638***	0.0482***
	(0.0022)	(0.0021)
Age Squared / 100	-0.0570***	-0.0544***
	(0.0012)	(0.0016)
Years of Education	0.0814***	0.0744***
	(0.0012)	(0.0013)
LEP	-0.1453*	0.0843
	(0.0791)	(0.1004)
OE Law × LEP	-0.1099**	-0.0512
	(0.0550)	(0.0504)
Constant	-0.1394	-0.3050
	(0.1394)	(0.2192)
Observations	55,119	43,063
R-squared	0.3267	0.3501

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Sample weights are used. Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample restricted to those working full time, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, race, children present, year of entry, and veteran status. Individuals are considered limited English proficient if they reported that they speak English "not-well" or "not at all." The change in the LEP population was measured between each Census year from 1980–2010 using Census data. Only Asian individuals were included in the sample.

Table 20: Triple Difference Estimates on Wages, Whites Only

Variable	Men ln(wages)	Women ln(wages)
Age	0.0644***	0.0556***
	(0.0003)	(0.0004)
Age Squared / 100	-0.0604***	-0.0562***
	(0.0012)	(0.0016)
Years of Education	0.0749***	0.0883***
	(0.0002)	(0.0003)
LEP	-0.1568***	-0.0886**
	(0.0408)	(0.0447)
OE Law × LEP	-0.0275	0.0122
	(0.0175)	(0.0240)
Constant	-0.0192	-0.2575***
	(0.0424)	(0.0473)
Observations	1,485,026	940,358
R-squared	0.3014	0.3095

***p<0.01; **p<0.05; *p<0.10.. The dependent variable is the natural log of wages in 2010 dollars. Sample weights are used. Data shown is comprised of a 1% sample of the Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample restricted to those working full time, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, race, children present, year of entry, and veteran status. Individuals are considered limited English proficient if they reported that they speak English "not-well" or "not at all." The change in the LEP population was measured between each Census year from 1980–2010 using Census data. Only White individuals were included in this sample.

APPENDIX

A. Official English Laws Pre-1980

My analysis focuses only on Official English laws that were passed between the years of 1980 until 2010, during the modern Official English movement. However, I find that Official English laws passed before 1980 that are still current have similar effects. Table 3A shows that my results are robust to the inclusion of these laws: LEP men experience a 3.7 log point wage penalty in Official English states while women do not. Therefore, even though these earlier laws were often passed with different motives (recall the “American” to “English” change in Illinois) my results do not change. For the bulk of my analysis, I only focus on states with Official English laws coinciding with the Official English movement from 1980 until 2010, though I also perform robustness checks to ensure that my results do not change when adding in current laws that existed before the modern Official English movement.

B. The Effects of Official English Laws on Other Labor Market Outcomes

Wages may not be the only labor market outcome that Official English laws affect. Other labor outcomes are important, as they assess whether LEP worker are being excluded from the labor market or are matched to jobs that are part-time. I test to see if these laws also affect the usual hours worked in a week, weeks worked in a year, and the probability of employment. To do so, I keep all of the restrictions of my previous analysis except for the restrictions that the individual must be working and the individual must be working full time.

Table 4A reports the results of modifying the dependent variable of Equation 1 to the natural log of usual hours and usual weeks worked on all workers, not just those who are full time. The impact of Official English laws on male LEP worker hours and weeks is negative and statistically significant at the 1% level, and therefore these laws are associated with not only with a reduction in wages, but a reduction in hours and weeks worked for men in Official English states. I find, unlike when I simply looked at wages, that Official English laws negatively affect women's hours and weeks, and the results are significant at the 1% level. Therefore, these laws are working to hinder the employment opportunities of both male and female LEP workers, not just their wages.

These laws can also impact an LEP worker's chances of getting hired in the first place. To determine the effects of these laws on the employment of LEP workers, I use a probit model where the dependent variable, *emp* indicates whether the individual is currently employed. I use the same vector *X* of demographic variables as in Equation 1, and all other variable definitions remain the same. Table 5A shows that I find that LEP men in Official English law states have a 17 percent less likelihood of being employed simply because they reside in Official English states, but find no effect on LEP women's chances of employment. This change in employment could be due to LEP workers exiting the labor market or employers choosing to not hire these individuals. Consistent with my findings on LEP worker wages, these results show that Official English laws disadvantage LEP male workers.

Table 1A: Example of State Official English Constitutional Amendments

State	Amendment Language
Alabama ALABAMA CONSTITUTION, AMENDMENT 509	English is the official language of the state of Alabama. The legislature shall enforce this amendment by appropriate legislation. The legislature and officials of the state of Alabama shall take all steps necessary to insure that the role of English as the common language of the state of Alabama is preserved and enhanced. The legislature shall make no law which diminishes or ignores the role of English as the common language of the state of Alabama. Any person who is a resident of or doing business in the state of Alabama shall have standing to sue the state of Alabama to enforce this amendment, and the courts of record of the state of Alabama shall have jurisdiction to hear cases brought to enforce this provision. The legislature may provide reasonable and appropriate limitations on the time and manner of suits brought under this amendment.
Florida FLORIDA CONSTITUTION, ARTICLE II, SECTION 9 (1988)	(a) English is the official language of the state of Florida. (b) The legislature shall have the power to enforce this section by appropriate legislation.
Missouri MISSOURI CONSTITUION, ARTICLE II, SECTION 34 (2008)	That English shall be the language of all official proceedings in this state. Official proceedings shall be limited to any meeting of a public governmental body at which any public business is discussed, decided, or public policy formulated, whether such meeting is conducted in person or by means of communication equipment, including, but not limited to, conference call, video conference, Internet chat, or Internet message board. The term “official proceeding” shall not include an informal gathering of members of a public governmental body for ministerial or social purposes, but the term shall include a public vote of all or a majority of the members of a public governmental body, by electronic communication or any other means, conducted in lieu of holding an official proceeding with the members of the public governmental body gathered at one location in order to conduct public business.
Oklahoma OKLAHOMA CONSTITUTION, ARTICLE XXX (2010)	As English is the common and unifying language of the State of Oklahoma, all official actions of the state shall be conducted in the English language, except as required by federal law. No person shall have a cause of action against an agency or political subdivision of this state for failure to provide any official government actions in any language other than English. Nothing in this Article shall be construed to diminish or impair the use, study, development, or encouragement of any Native American language in any context or for any purpose. The legislature shall have the power to implement, enforce and determine the proper application of this Article by appropriate legislation.

Table 2A: Example of State Official English Statutes

State	Statute Language
Georgia; **OFFICIAL GEORGIA CODE § 50-3-100: OFFICIAL LANGUAGE (1986)**	(a) The English language is designated as the official language of the State of Georgia. The official language shall be the language used for each public record, as defined in Code Section 50-18-70, and each public meeting, as defined in Code Section 50-14-1, and for official Acts of the State of Georgia, including those governmental documents, records, meetings, actions, or policies which are enforceable with the full weight and authority of the State of Georgia. (b) This Code section shall not be construed in any way to deny a person's rights under the Constitution of Georgia or the Constitution of the United States or any laws, statutes, or regulations of the United States or of the State of Georgia as a result of that person's inability to communicate in the official language. (c) State agencies, counties, municipal corporations, and political subdivisions of this state are authorized to use or to print official documents and forms in languages other than the official language, at the discretion of their governing authorities. Documents filed or recorded with a state agency or with the clerk of a county, municipal corporation, or political subdivision must be in the official language or, if the original document is in a language other than the official language, an English translation of the document must be simultaneously filed. (d) The provisions of subsection (a) of this Code section shall not apply: (1) When in conflict with federal law; (2) When the public safety, health, or justice require the use of other languages; (3) To instruction designed to teach the speaking, reading, or writing of foreign languages;(4) To instruction designed to aid students with limited English proficiency in their transition and integration into the education system of the state; and (5) To the promotion of international commerce, tourism, sporting events, or cultural events.
Iowa **IOWA SF 165 (2002)**	1. The general assembly of the state of Iowa finds and declares the following: a.) The state of Iowa is comprised of individuals from different ethnic, cultural, and linguistic backgrounds. The state of Iowa encourages the assimilation of Iowans into Iowa's rich culture. b.) Throughout the history of Iowa and of the United States, the common thread binding individuals of differing backgrounds together has been the English language. c.) Among the powers reserved to each state is the power to establish the English language as the official language of the state, and otherwise to promote the English language within the state, subject to the prohibitions enumerated in the Constitution of the United States and in laws of the state. 2. In order to encourage every citizen of this state to become more proficient in the English language, thereby facilitating participation in the economic, political, and cultural activities of this state and of the United States, the English language is hereby declared to be the official language of the state of Iowa. 3. Except as otherwise provided for in subsections 4 and 5, the English language shall be the language of government in Iowa. All official documents, regulations, orders, transactions, proceedings, programs, meetings, publications, or actions taken or issued, which are conducted or regulated by, or on behalf of, or representing the state and all of its political subdivisions shall be in the English language.

For the purposes of this section, “official action” means any action taken by the government in Iowa or by an authorized officer or agent of the government in Iowa that does any of the following: a.) Binds the government, b.) Is required by law,.c.) Is otherwise subject to scrutiny by either the press or the public.

4. This section shall not apply to: a.) The teaching of languages, b.) Requirements under the federal Individuals with Disabilities Education Act, c.) Actions, documents, or policies necessary for trade, tourism, or commerce, d.) Actions or documents that protect the public health and safety, e.) Actions or documents that facilitate activities pertaining to compiling any census of populations, f.) Actions or documents that protect the rights of victims of crimes or criminal defendants, g.) Use of proper names, terms of art, or phrases from languages other than English, h.) Any language usage required by or necessary to secure the rights guaranteed by the Constitution and laws of the United States of America or the Constitution of the State of Iowa.

Table 3A: Triple Difference Estimates Including All Official English Laws 1920–2010

	Men	Women
Age	0.0627***	0.0544***
	(0.0003)	(0.000331)
Age Squared / 100	-0.0604***	-0.0562***
	(0.0004)	(0.0004)
Years of Education	0.0707***	0.0854***
	(0.0002)	(0.000265)
Hispanic	-0.0843***	-0.0555***
	(0.0022)	(0.00248)
Asian	-0.0731***	-0.0010
	(0.0034)	(0.00520)
LEP	-0.2018***	-0.1050***
	(0.0333)	(0.0341)
OE Law × LEP	-0.0367**	-0.0009
	(0.0149)	(0.0194)
Constant	0.1735***	-0.1700***
	(0.0353)	(0.0375)
Observations	1,744,056	1,163,228
R-squared	0.3132	0.3140

***p<0.01; **p<0.05; *p<0.10. The dependent variable the natural log of wages in 2010 dollars. Data shown is comprised of the 1% Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample weights are used. Sample is restricted to full-time and full-year workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Additional controls include occupation, marital status, race, children present, year of entry, and veteran status. Individuals are considered limited English proficient (LEP) if they reported that they speak English "not-well" or "not at all." Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. "OE Law" indicates the presence of an Official English law in state *k* at time *t*.

Table 4A: Triple Difference Estimates on the Natural Log of Usual Hours Worked and Weeks Worked

	Men ln(hours)	**Women ln(hours)**	**Men ln(weeks)**	**Women ln(weeks)**
Age	0.0011***	0.0014***	0.0047***	0.0041***
	(0.0000)	(0.0000)	(0.0001)	(0.0001)
Age Squared / 100	-0.0011***	-0.0015***	-0.0058***	-0.0047***
	(0.0000)	(0.0000)	(0.0001)	(0.0001)
Years of Education	0.0004***	-0.0007***	0.0057***	0.0062***
	(0.0000)	(0.0000)	(0.0001)	(0.0001)
Hispanic	-0.0010***	-0.0014***	-0.0167***	-0.0083***
	(0.0001)	(0.0002)	(0.0006)	(0.0006)
Asian	-0.0020***	-0.0006	-0.0170***	0.0035**
	(0.0003)	(0.0004)	(0.0015)	(0.0014)
LEP	0.0005	-0.0034	0.0118	-0.0090
	(0.0024)	(0.0031)	(0.0097)	(0.0068)
OE Law × LEP	-0.0035***	-0.0034**	-0.0161***	-0.0133***
	(0.0012)	(0.0017)	(0.0038)	(0.0042)
Constant	3.9116***	3.9143***	3.6095***	3.5749***
	(0.0025)	(0.0034)	(0.0103)	(0.0077)
Observations	1,844,477	1,223,764	1,844,477	1,223,764
R-squared	0.0535	0.0409	0.0442	0.0417

***p<0.01; **p<0.05; *p<0.10. The dependent variable is indicated as the natural log of the usual hours worked, and the natural log of weeks worked, respectively. Data shown is comprised of the 1% Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample weights are used. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, race, children present, year of entry, and veteran status. Individuals are considered limited English proficient (LEP) if they reported that they speak English "not-well" or "not at all." Sample is restricted to workers earning between \$1.50 and \$300 per hour who are not self-employed. "OE Law" indicates the presence of an Official English law in state *k* at time *t*.

Table 5A: Probit, Employment

	Men Pr(emp)	**Women** Pr(emp)
Age	0.0675***	0.0739***
	(0.0012)	(0.0014)
Age Squared / 100	-0.0824***	-0.0845***
	(0.0015)	(0.0018)
Years of Education	0.0500***	0.0508***
	(0.0008)	(0.0010)
Hispanic	-0.0987***	-0.1050***
	(0.0083)	(0.0093)
Asian	-0.0489**	-0.0409*
	(0.0210)	(0.0224)
LEP	-0.0560	0.0508
	(0.1880)	(0.1677)
OE Law × LEP	-0.1713***	-0.0514
	(0.0560)	(0.0715)
Constant	0.0597	-0.2749***
	(0.0457)	(0.0622)
Observations	1,844,477	1,223,764

***p<0.01; **p<0.05; *p<0.10. The dependent variable is equal to one if the individual is employed. Data shown is comprised of the 1% Census during the years 1980, 1990, and 2000 and a 1% sample of the 5-year 2009–2013 American Community Survey. Sample weights are used. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, race, children present, year of entry, and veteran status. Individuals are considered limited English proficient (LEP) if they reported that they speak English "not-well" or "not at all." "OE Law" indicates the presence of an Official English law in state *k* at time *t*.

CHAPTER TWO: LOCAL ENFORCEMENT OF FEDERAL IMMIGRATION LAWS: A STUDY OF THE 287(G) PROGRAM

I. Introduction

Recently, representatives from many local jurisdictions around the United States negotiated agreements with representatives of U.S. Immigrations and Customs Enforcement (ICE) to delegate enforcement of federal immigration laws to the local jurisdiction. Upon signing on the dotted line, these jurisdictions allowed ICE to deputize local officers to investigate the immigration status of individuals and turn immigration law violators over to ICE. These agreements, known as 287(g) agreements after the statute that enables them, were introduced after September 11, 2001 and gained popularity amidst increased immigration enforcement nationwide. Though the Obama administration allowed the 287(g) program to expire in 2012, the Trump administration has revived these agreements. In a campaign speech, now-President Donald Trump promised, "we will expand and revitalize the popular 287(g) partnerships, which will help to identify hundreds of thousands of deportable aliens in local jails that we don't even know about" (Lee and Kessler 2016). The administration has adhered to its promise to expand the 287(g) program: there are currently 59 local jurisdictions in 17 states with 287(g) agreements, and more are planning to ink agreements with ICE in the near future.

287(g) agreements are one tool implemented in light of shifting "immigration federalism," (Schuck 2007; Huntington 2008; Chacón 2012) in which federal immigration power is decentralized to local governments. By empowering local law enforcement agents to enforce immigration laws, 287(g) agreements allow localities to take ownership of what was once exclusively federal. Immigration federalism has allowed jurisdictions to choose to enforce—or not enforce—immigration laws. 287(g) agreements are the polar opposite of sanctuary cities, another type of immigration federalism, which explicitly do not allow jurisdictions to engage in 287(g) agreements. This chapter examines how immigration federalism aimed at the enforcement of immigration laws can affect the wages of immigrant workers.

As more jurisdictions continue to implement 287(g) agreements, the effects of these policies on immigrant wages nationwide remain unknown. The majority of literature on 287(g) agreements is limited to case studies in specific areas such as North Carolina (Weismann 2009) and Davidson County, TN (Armenta 2012; Kee 2012) and focus solely on the impacts of these agreements on crime rates (Wong 2012). Other scholars have found that the 287(g) program is adversely impacting the Latino community and undermining its social cohesion (Lacayo 2010), as well as creating an insecure environment for all immigrants (Lopez and Minushkin 2008; Quereshi 2010). Yet, little work has examined the impact of these agreements on immigrant labor markets. Studies that examine the impacts of 287(g) agreements on labor outcomes have been limited to the agricultural industry (Kostandini 2013), or have only looked at the effects within industries with large levels of immigrant workers (Pham and Van 2012).

My analysis extends the current literature by first providing a nationwide analysis of 287(g) agreements enacted during the 2000–2012 period. I use Census and ACS data to exploit the differences in enactment dates of 287(g) agreements in the United States. Using a differences-in-differences-in-differences model identifying differences in county, year, and immigrant status, I find that the impacts of these agreements are large and negative on immigrant worker wages when the individual resides in a jurisdiction with a 287(g) agreement. To determine whether these results are driven by undocumented individuals in my sample, I use the methodology of Borjas (2017) to identify potentially undocumented individuals. I find that potentially undocumented individuals experience significant wage penalties in excess of the magnitude that potentially documented individuals experience in 287(g) jurisdictions. I also find that the effects of these agreements are not limited to wages: immigrants in 287(g) jurisdictions work fewer hours than their peers in non-287(g) jurisdictions.

To determine whether underlying heterogeneity is driving the results of 287(g) agreements as a whole, I examine the differences in type, strength, and breadth of agreements. I find that agreements allowing for broader enforcement of the laws, such as task agreements and those that issue more detainers for low-level crimes like traffic violations, lead to larger wage penalties for immigrants residing in those jurisdictions. In addition, jurisdictions with 287(g) agreements permitting the issuance of more immigrant detainers also have large and negative effects on immigrant wages. As 287(g) agreements are introduced with renewed vigor, my results show that while all agreements disadvantage immigrant workers, it is the broader 287(g) agreements that are driving these negative results.

II. Background

To provide for greater enforcement of immigration laws, the federal government began signing 287(g) agreements in the early 2000s with local law enforcement agencies. Under these agreements, local law enforcement officers are trained to act as immigration officers in the course of their daily activities. Though they are local enforcement agents, immigration officers in 287(g) jurisdictions have been shown to act as extensions of the federal government rather than independent agents (Armenta 2012). Local enforcement officers under 287(g) programs are empowered by the federal government to arrest any individual that the officer believes to be in violation of immigration law. Officers may also issue ICE detainers to hold offenders before being transported to an ICE detention center.

These agreements derive their authority from Section 287(g) of the Immigration and Nationality Act, a broad statute that allows the federal government to delegate immigration enforcement powers to state and local officers.[16] This statute was adopted in 1996 as part of the Illegal Immigration Reform and Responsibility Act (IIRIRA) changes to the Immigration and Nationality Act (INA), and has since provided the statutory authorization for partnerships between local governments and ICE.

The statute itself authorizes the attorney general to enter into written agreements with state and local officials empowering them to "perform the function of an immigration officer in relation to the investigation, apprehension, or detention of aliens in the United States . . . ." Under these written agreements, state and local officials must receive training in federal enforcement and must be supervised by federal officials. The statute itself provides little more than a broad framework for 287(g) agreements with states, but the details of each agreement are fleshed out during negotiations with individual localities in a Memorandum of Agreement (MOA), which defines the scope and limitations of the delegation of authority from the federal government to the local government.

[16] INA § 287(g)(1) reads: "Notwithstanding section 1342 of title 31, United States Code, the Attorney General may enter into a written agreement with a State, or any political subdivision of a State, pursuant to which an officer or employee of the State or subdivision, who is determined by the Attorney General to be qualified to perform a function of an immigration officer in relation to the investigation, apprehension or detention of aliens in the United States (including the transportation of such aliens across State line s to detention centers), may carry out such function at the expense of the State or political subdivision and to extent consistent with State and local law."

The first jurisdictions to enter into a 287(g) agreement with ICE were Florida in 2002 and Alabama in 2003.[17] Early agreements, including the one signed by Los Angeles County in 2005, were focused on identifying "individuals who pose a threat to border security" (GAO 2009). However, beginning with Mecklenburg County, North Carolina in 2006, 287(g) agreements started to become less focused on those committing serious crimes and more focused on civil immigration violations. Sheriff Jim Pendergraph testified before the House of Representatives that a benefit of the 287(g) program was that it permitted his officers to identify a large number of civil immigration law violators.[18] Pendergraph soon became the head of ICE's Office of State and Local Coordination (OSLC), and Mecklenburg County's model spread to other jurisdictions.

Under Pendergraph, 54 other jurisdictions across the U.S. included similar language to the Mecklenburg agreement, which allowed for broader and less targeted enforcement of immigration laws (Capps et al. 2011). These new agreements allowed for officers to apprehend all unauthorized immigrants rather than inquiring regarding someone's immigration status after they committed specific crimes. Notably, the limiting language on ICE's website, which described the program as aimed at "violent crimes" was removed after the Mecklenburg agreement was signed. This limiting language also stated that the program was "not designed to allow state and local agencies to perform random street operations" and was "not designed to impact issues such as excessive

[17] Salt Lake City entered into negotiations with ICE in 1999, but decided against signing a 287(g) agreement, reportedly due to concerns about racial profiling.

[18] Testimony of Jim Pendergraph, Sheriff of Mecklenburg County, North Carolina, "Empowering Local Law Enforcement to Combat Illegal Immigration," before the House Committee on Government Reform, Subcommittee on Criminal Justice, Drug Policy and Human Resources, 109th Cong., 2nd sess., August 25, 2006.

occupancy and day labor activities." When this language was removed from ICE's website no other limiting language on the scope of 287(g) agreements took its place.

Without explicit limits on authority of 287(g) agreements, these programs began to focus less on serious criminal offenders and more on small violations. For example, a study conducted by the American Civil Liberties Union (ACLU) of North Carolina found that 83 percent of those detained through the 287(g) program in Gaston County, North Carolina in 2008 were charged with traffic violations (Weissman et al. 2009). In Davidson County, Tennessee, the 287(g) agreement resulted in more than half of foreign-born arrestees being processed for removal. Of those processed for removal, 88 percent of individuals were processed only for civil immigration violations (Kee 2012). These statistics reveal a program far from the targeted enforcement that was envisioned upon the adoption of 287(g) agreements.

As of December 2011, there were 67 jurisdictions with 287(g) agreements in place. Table 1 shows the local jurisdiction and enforcement agency under the agreements as well as the date and type of agreement. There are three different types or "models" of 287(g) programs. The jail model allows officials to screen for immigration status and issue detainers when booking individuals into jails on non-immigration charges, while the task force model allows state and local officials to screen for status and issue detainers in the field during policing operations. ICE also prescribed a hybrid model that would allow local governments to implement both the task force model and the jail model. Over time, the 287(g) program has grown in its impact on the immigrant community: between October 2005 and October 2008, 287(g) officers identified over 80,000 noncitizens for potential removal. In the two years that followed, 287(g) officers identified more than 100,000 identified noncitizens for potential removal (Parrado 2012).

As the 287(g) program expanded, it drew criticism regarding uneven application of its policies and rising costs amidst few benefits.[19] A 2009 U.S. Government Accountability Office Report found that the program lacked a documented program objective, did not provide clear and consistent mechanisms of supervision, and provided unclear protocols to local governments (GAO 2009). The 287(g) program also placed a strain on local funding. Local agencies shoulder the bulk of costs associated with the program, and often these costs exceed the measured benefits. The Police Foundation concluded that the 287(g) program's costs far outweighed the benefits of participation, particularly when a state or local law enforcement agency's focus of a 287(g) program is broader than serious criminal offenders (Kashu 2009). Recently, the Sherriff of Harris County, Texas cited the program's staggering costs as a key factor in his decision to end his county's 287(g) agreement (Pinkerton and Smith 2017). Over the course of the program, Congress steadily increased the program's funding: from $15.5 million in federal funds in 2007 to $68 million in 2010. Yet, states bear the burden of the costs in these programs, which can place a strain on already limited local resources. A study on Prince William County, Virginia's participation in the program found that it would cost the county $6.4 million in the first year and $26 million over five years. To combat this cost, the county raised their property taxes (Singer et al. 2009).

[19] Salt Lake City, Utah, ultimately decided to not sign a 287(g) agreement due to concerns that the program promoted racial profiling (Capps et al. 2011).

Concerns over cost and limited oversight, along with criticisms over controversial 287(g) programs, such as those led by Sheriff Joseph Arpaio,[20] led the Obama administration to decide in 2012 that it would not renew any of its agreements with state and local law enforcement agencies operating 287(g) programs. After these agreements expired in June 2013, the 287(g) program ended and most policymakers believed that it would not resurface (Rhodan 2017). However, the Trump administration has revived the 287(g) program. Following an executive order that prescribed expansion of the 287(g) program, the Trump administration oversaw additional 287(g) agreements, including 18 new agreements with sheriff's offices in Texas. The administration also expanded the scope of the 287(g) program to prioritize aliens who have been convicted or charged with any criminal offense or, in the judgment of an immigration officer, "pose a risk to public safety or national security." All new agreements are jail models. As more local governments consider adoption of 287(g) agreements, this chapter provides analysis of how these agreements affect immigrant worker wages, and sheds light on how these effects are shared between legal and undocumented workers.

III. Conceptual Framework

When a jurisdiction signs a 287(g) agreement with ICE, the jurisdiction signals that it seeks to increase the enforcement of federal immigration laws through the utilization of local officers. These agreements can have a direct effect on the immigrant workforce by increasing the probability that individuals are deported. A greater likelihood of deportation can lower wages for undocumented workers (Rivera-Batiz

[20] In March 2009, the Department of Justice announced that it had opened an investigation into the practices of Sheriff Joseph Arpaio of Maricopa County, Arizona. Investigators found that Arpaio and his deputies engaged in "patterns or practices of discriminatory police practices," such as "unconstitutional searches and seizures" and "national- origin discrimination," including failure to provide meaningful access to services for persons of limited English proficiency.

1999; Pena 2010a; Pena 2010b; Hotchkiss and Quispe-Agnoli 2013; Borjas 2017) or cause stress, which can negatively affect an individual's health, and wages in turn (Cavazos-Rehg et al. 1999; Berk and Schur 2011). While the direct effects of these laws will target undocumented immigrants, the spillover effects to other immigrants have the potential to be large (Massey et al. 2002; Gentsch and Massey 2011; Quiroga et al. 2014).

An increased likelihood of deportation can impact undocumented workers' willingness to accept lower wages because they perceive that they have limited power in the labor market. Employers who possess monopsonistic market labor market power know that undocumented employees will not report wage violations or grievances to the necessary authorities for fears of encountering law enforcement. Rather than maximizing their wages, undocumented workers will be focused on minimizing detection or encounters with ICE (Kossoudji and Clark 2002). This focus will be more pronounced in jurisdictions that have adopted 287(g) agreements, as the number of individuals enforcing immigration laws increases drastically in 287(g) jurisdictions. While these jobs are not necessarily all lower paying, workers will make tradeoffs that are inconsistent with wage-maximizing behavior to achieve these ends. As a result, workers may also sort into marginal occupations, or occupations in declining industries that offer comparatively lower pay (Rivera-Batiz 1999). These effects perpetuate wage differences between immigrants and non-immigrants over time.

287(g) agreements may have effects on the supply of undocumented workers. Workers may choose to work fewer hours to minimize the possibility of detection, and some workers may exit the labor force altogether. Workers may also choose to move out of jurisdictions with 287(g) agreements in favor of those that do not have 287(g) agreements. I test whether migrants are driving my results in Section V.

The impacts of these agreements can also be felt on the demand side if employers alter their demand for undocumented workers after their jurisdiction adopts a 287(g) agreement. This could occur if employers want to minimize interactions with local law enforcement officers for fear of detection if they are employing undocumented workers. Knowingly hiring undocumented workers is illegal and punishable by civil and criminal penalties. Employers, wishing to avoid these penalties may substitute legal workers, whether they are foreign-born or not. Further, firms wishing to evade employer sanctions for hiring undocumented workers but still wishing to employ these workers can also shift from direct hiring to labor subcontracting. There is evidence that this subcontracting is associated with less bargaining power, resulting in lower wages (Phillips and Massey 1999; Aguilera and Massey 2004). Overall, the adoption of 287(g) agreements may increase the salience of immigration status, leading to more negative consequences for the immigrant workforce.

There is evidence that the negative effects of a greater likelihood of deportation is not limited to undocumented workers, but the effects will be felt for all immigrants. Increases in U.S. immigration enforcement have undermined the labor market position of immigrants to the United States, regardless of legal status (Massey et al. 2002; Gentsch and Massey 2011). Though in theory legal immigrant workers have full labor rights, many immigrant workers are concentrated in industries where the workers around them do not. There is evidence that legal immigrants, particularly Hispanic legal immigrants, compete in labor markets where most workers lack labor rights and labor bargaining power, resulting in lower wages (Rivera-Batiz 1999; Gentsch and Massey 2011). Community spillover effects exist for immigration benefits or penalties conferred to one segment of a population. Quiroga et al. (2014) find that respondents to a survey indicate a high degree of negative impact in their lives of immigration enforcement activity. This response is persistent even for minorities who are U.S.-born, and does not significantly differ between U.S.-born minorities and their foreign-born counterparts. There are many ways that an increased risk of deportation can affect the wages of legal immigrants, including through occupational sorting, increases in stress, and allowing employers to exert monopsonistic market power over their wages. However, it is likely that undocumented workers will experience an increase in the likelihood of deportation more acutely (Orrenius and Zavodny 2014), and this effect will be larger in 287(g) jurisdictions.

There is also a possibility that immigrant employment outcomes will benefit from 287(g) agreements if employers substitute legal immigrants for undocumented workers. If immigrants generally are willing to work for lower wages, employers in localities with 287(g) agreements may shift their preferences towards legal immigrants rather than undocumented workers to minimize the potential of detection. This substitution will depend on the supply of legal immigrants in the given jurisdiction as compared to undocumented workers and the elasticity of substitution between the two types of workers.

The underlying scope of the agreement may also have a differing effect on the immigrant workforce. 287(g) agreements that are broadly enforced, like task force agreements that allow officers to ask anyone their immigration status or agreements that allow individuals who commit traffic violations to be detained by ICE, can cause more acute effects than those that are targeted towards specific types of crimes because they will affect more people and increase the probability of ICE encounters. Further, the strength of the enforcement of the agreement matters: if a jurisdiction signs an agreement but does not in effect inquire about immigration status or allow ICE to detain individuals, we would expect that the labor market consequences of these effects to be less severe. I examine the effects of the underlying heterogeneity in 287(g) agreements in Section V.

IV. Data and Methodology

To examine the effects of 287(g) agreements on the immigrant labor force, I use data from the Census and American Community Survey (ACS). I use a 1% sample of the 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey, as these years span the implementation and adoption of most 287(g) agreements in the United States. This time frame also captures the effects of these agreements before the Obama administration decided not to renew the agreements in 2013. These datasets also contain geographic identifiers beyond the state-level, such as county, which identify smaller jurisdictions that have implemented these 287(g) agreements.

In each Census year, all households in the United States receive a short form Census, which asks basic demographic questions regarding each household member. Additionally, through the 2000 Census, one out of six households received the long form Census, which asks more detailed labor market and migration questions. I use data from the long form Census, as it contains data on immigration status and important employment measures such as employment status and income.[21]

To analyze labor market outcomes, I use measures of wages, hours, and weeks worked. The ACS reports earnings annually and hours worked each week by asking the surveyed household member: "during the past 12 months, in the weeks worked, how many hours did you usually work each week?" The Census and ACS measure weeks worked each year by asking respondents how many weeks that they worked last year, including paid vacation, paid sick leave, and military service. The Census reports actual weeks worked, while the ACS reports weeks in broad ranges. For individuals in the ACS, I use the midpoint of the reported range as the number of weeks that individual worked. I construct hourly wage by dividing an individual's earnings by the amount of weeks worked multiplied by usual hours worked each week. I adjust all wages to 2010 dollars. Consistent with prior literature on immigrant worker wages, I limit my analysis to full-year and full-time workers, though I do examine the effects of 287(g) agreements on hours and wages without this restriction.[22]

I create a variable for years of completed schooling.[23] I create indicator variables for marital status, race, veteran status, and if children are present in the individual's household. I also create an indicator variable for Hispanic individuals. Finally, I create

[21] The American Community Survey (ACS) supplemented the long-form Census in the early 2000s and replaced the long-form Census in 2010. The ACS is a nationally representative sample that continually surveys randomly sampled addresses in every state, the District of Columbia, and Puerto Rico. Each year, the ACS samples nearly three million household unit addresses in the United States. No household will receive the survey more often than once every five years.

[22] Full-year workers are those that worked 50 or more weeks this year and full-time workers are workers that report more than 35 usual hours worked per week.

[23] The ACS provides a detailed report of the highest level of schooling completed. Each survey participant has an option to select the precise highest grade of schooling, but some are also reported in ranges. For example, individuals could report their highest level of schooling as grade 1 or grades 1, 2, 3, or 4. For those individuals that report their highest level of schooling as a range, I use the midpoint values of the ranges as their years of schooling. All individuals that spent more than four years in college and obtained a college degree were coded as obtaining 16 years of education.

indicator variables using the 2012 North American Industry Classification System (NAICS) Major Groups for 15 different industry categories including construction, manufacturing, and transportation.

The ACS contains both state and county identifiers for each year. The ACS data also contains city identifiers, though this is not available for all cities in the United States and is limited, generally, to the largest metropolitan areas. For the handful of cities that signed 287(g) agreements alone, I match those cities found in the ACS data to the entire county. I treat a locality as having a 287(g) agreement if there is a 287(g) agreement in place at the time of the observation. For example, Los Angeles County, California signed a 287(g) agreement in 2005 and ended its participation in the program in 2012. I treat Los Angeles as having a 287(g) agreement for all years between 2005 and 2012. During my sample period, there are a few 287(g) programs that ended before the Obama administration officially ended the program, including Alabama and Massachusetts. Alabama signed a 287(g) agreement in 2003 and ended its participation in the program in 2010. Therefore, Alabama is treated as not having an agreement in the years before 2003 and the years after 2010.

I restrict my sample to workers who are not self-employed or working without pay and are not in the armed forces. I remove individuals who were born in Puerto Rico, Guam, and the U.S. Virgin islands, or those born abroad with American parents. For those born abroad, I create an indicator variable to show that they are an immigrant to the United States.

Table 2 shows the summary statistics for my sample by presence of 287(g) agreement in the jurisdiction where the individual lives. There are substantially more individuals in jurisdictions without a 287(g) agreement: just over 20 percent of individuals live in a jurisdiction with a 287(g) agreement. Despite the differences in total numbers, the two groups are strikingly similar, from their average age and years of education to their wages and marital status. Individuals in 287(g) jurisdictions are more likely to be foreign-born and Hispanic, so there is a potential for reverse causality as these agreements could be implemented in response to increasing immigrant populations. Table 3 shows the summary statistics by immigrant status. The plurality of immigrants (45%) are Hispanic, followed by Asian (24%) and individuals of multiple or other races (22%). On average, immigrants in my sample earn less than native-born workers and have fewer years of education. Given this, I next determine whether the presence of 287(g) agreements ameliorates or exacerbates this wage gap.

Variation in the timing of 287(g) agreements provides a quasi-experiment to analyze the impact of these agreements on immigrant labor markets. To capture the change in wages for immigrant individuals based on 287(g) agreements, I use a difference-in-difference-in-differences (triple difference) framework, which takes the form of the following semi-logarithmic equation:

$$\begin{aligned} lnW_{ikt} = \propto\ + imm_{ikt}\beta_1 + \beta_2 county_k + \beta_3 year_t + \beta_4(county_k \times year_t) \\ + \beta_5(imm_{ikt} \times county_k) + \beta_6(imm_{ikt} \times year_t) + \beta_7(imm_{ikt} \times agree_{kt}) \\ + X_{ikt}\lambda + \epsilon_{ikt} \end{aligned} \tag{1}$$

where *i* indexes individuals, *k* indexes county, and *t* indexes years. *W* represents the real hourly wage of an individual in 2010 dollars. Imm is equal to one if a worker *i* was not born in the United States, while *agree* equals one if the county has a 287(g) agreement in place. *X* is a vector of demographic variables including veteran status, race, age, age squared divided by 100, years of education, children present, married, and industry. I cluster all standard errors at the county level. β_7 is the coefficient of interest and represents the difference in earnings of immigrant workers in states that adopted a 287(g) agreement over time. The identifying assumption of my model is that in the absence of the reform, the wages of immigrant workers in jurisdictions with 287(g) agreements would have evolved similarly to wages of immigrant workers in jurisdictions without such agreements. I test this assumption in Section IV using a placebo test. This identifying assumption may also be violated if there exists competition between immigrants and natives for jobs such that potential spillover effects between immigrants and natives occur.

V. Results

A. All Immigrant Workers

I find that the adoption of a 287(g) agreement in a jurisdiction has a large and negative impact on immigrant wages. Table 4 shows that my coefficient of interest, β_7, is large and statistically significant at the 1 percent level. This coefficient shows that male immigrant workers living in a jurisdiction with a 287(g) agreement earn roughly a 4.8 log point wage penalty relative to immigrants living in jurisdictions without 287(g) agreements. This result is large and statistically significant at the 1 percent level. I also find that male immigrants earn large wage penalties as compared to native-born individuals in my sample, consistent with decades of research confirming that foreign-born individuals earn less on average than their native-born counterparts (Chiswick 1978; Borjas 1987; Friedberg 2000; Borjas and Friedberg 2009).

My main result holds true for women as well. Female immigrant workers living in jurisdictions with 287(g) agreements in my sample earn roughly a 4.8 log point wage penalty compared to immigrants living in jurisdictions without such agreements. This coefficient is also statistically significant at the 1 percent level. My findings that 287(g) agreements affect women's hourly wages at an almost equal magnitude to men's contradict studies showing that legal interventions have little effect on immigrant women's wages (Zavodny 2000; Orrenius and Zavodny 2014). My results suggest that both male and female immigrant worker wages are affected by increased immigration enforcement performed by local authorities.

These results show the impact of 287(g) agreements on all foreign-born individuals and capture the effects on both legal immigrants and undocumented immigrants. However, to the extent that these agreements have more of a direct effect on undocumented workers, I would expect the effects on undocumented workers to be greater. I next test whether these effects are felt more heavily in individuals who may be undocumented.

B. Undocumented Workers

The effect of 287(g) agreements may be more pronounced for undocumented workers, as undocumented individuals are immigrants that could be deported by ICE. Therefore, they will be directly impacted by the possibility of an ICE detainer if an individual's immigrant status is in question. Undocumented workers are those that entered the United States without inspection or were admitted temporarily and stayed past the date they were required to leave. The wages of undocumented immigrants are responsive to legal changes: a grant of amnesty had a positive effect on the wages of undocumented individuals by raising their real wage by 3 percent (Kaushal 2006), while more restrictive worker policies led to a decrease in the wages of unauthorized Mexican workers (Orrenius and Zavodny 2015).

However, it is difficult to measure the effects of policy changes on undocumented workers because of the lack of data associated with undocumented status. Asking undocumented workers to report their status for a government survey would lead to massive underreporting due to fears of governments having information on individuals' immigrant status. Yet, the undocumented population is an important—and sizeable—part of the immigrant population in the United States. After Warren and Passel (1987) first developed an algorithm to estimate the size of the undocumented population, government entities have attempted to identify the size of the undocumented population. DHS's most recent estimates indicate that the current undocumented population totals roughly 11 million individuals in the United States. Though the labor market impacts of these individuals are thought to be substantial, quantifying these impacts has proven difficult due to the lack of identification of undocumented individuals in data sets.

To aid in the identification of undocumented workers, the Pew Research Center extended the work of Warren and Passel (1987) to the CPS's 2012–13 Annual Socioeconomic and Economic Supplements. These supplements contain a variable constructed by the Pew Research Center that indicated if a foreign-born individual is "likely authorized" or "likely unauthorized." After observing these micro-level files, Borjas (2017) extended the classification system to flag undocumented individuals in both the CPS and the 2011–2012 American Community Surveys. Using this strategy, he confirms results of the Pew Research Center and provides a meaningful way to indicate undocumented status in large, publicly available datasets. With these identifiers, Borjas

(2017) confirms that the earnings of undocumented workers are far below those of legal immigrant and native workers.

To examine how 287(g) agreements impact undocumented worker wages, I create a potentially undocumented status identifier using the criteria of Borjas (2017). Under this framework, a foreign-born individual is classified as a legal immigrant if any of the following conditions hold: (1) the person arrived before 1980; (2) the person is a citizen; (3) the person receives Social Security benefits, SSI, Medicaid, Medicare, or Military Insurance; (4) the person is a veteran or is currently in the armed forces; (5) the person works in the government sector; (6) the person resides in public housing or receives rental subsidies; (7) the person was born in Cuba; (8) the person's occupation requires some form of licensing; (9) that person's spouse is a legal immigrant or citizen. After imposing these qualifications for legal individuals, any other foreign-born individual is considered potentially undocumented.

I follow these criteria to create an undocumented indicator variable in my sample. Table 5 provides the summary statistics for undocumented workers. Undocumented individuals in my sample make up 5.6 percent of the total population, which is similar to the 6.8 percent of the population that Borjas (2017) found when using the 2011–2012 ACS.[24] The undocumented population is comprised mostly of Hispanic individuals, with 59 percent of undocumented individuals reporting that they are Hispanic. Undocumented individuals are also younger and more likely to be male than their legalized counterparts. Additionally, the wages of undocumented individuals are considerably lower than those of legalized individuals.

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[24] When isolating my sample to just the 2011–2012 ACS, I find the same results as Borjas (2017): 6.8 percent of the population in my sample is undocumented.

To determine the impact of 287(g) agreements on the potentially undocumented population, I modify equation (1) to the following:

$$\begin{gathered} lnW_{ikt} = \propto \; + undoc_{ikt}\beta_1 + legal_{ikt}\beta_2 \; + \beta_3 county_k + \beta_4 year_t \\ + \beta_5(undoc_{ikt} \times county_k) + \beta_6(year_t \times county_k) \\ + \beta_7(undoc_{ikt} \times year_t) + \beta_8(legal_{ikt} \times county_k) + \beta_9(legal_{ikt} \times year_t) \\ + \beta_{10}(undoc_{ikt} \times agree_{kt}) + \beta_{11}(legal_{ikt} \times agree_{kt}) \\ + X_{ikt}\lambda + \epsilon_{ikt} \end{gathered} \tag{2}$$

where *W* represents the real hourly wage of an individual in 2010 dollars. $Undoc$ is equal to one if a worker *i* is classified as potentially undocumented using the methodology of Borjas (2017). $Legal$ is equal to one if a worker *i* is classified as potentially legalized using the methodology of Borjas (2017). *Agree* equals one if the locality has adopted a 287(g) agreement. The definitions of all other variables are identical to those in Equation 1. β_{10} and β_{11} are my coefficients of interest. β_{10} represents the difference in earnings of potentially undocumented workers relative to natives in states that adopted a 287(g) agreement over time. β_{11} represents the same for potentially legal workers.

Table 6 reports the results of Equation 2. I find that both male and female potentially undocumented workers in jurisdictions with 287(g) agreements incur a larger wage penalty than those living in jurisdictions without 287(g) agreements. Male potentially undocumented workers earn a wage penalty of 9.8 log points in states with 287(g) agreements, while female potentially undocumented workers earn a wage penalty of 14.1 log points. However, this effect is not constrained to only potentially undocumented workers. Male potentially legal workers earn a wage penalty of 5.9 log points, while female potentially legal workers earn a 5.4 log point wage penalty. The magnitude of the wage penalty for potentially legal workers in 287(g) agreements is slightly less than for undocumented workers, lending some support that these effects are felt more acutely if individuals are undocumented. The coefficient on potentially undocumented workers, β_1, is 9.8 log points for men and 14.2 log points for women, while the coefficient on potentially legal workers, β_2, is 1.6 log points for men. The coefficient on potentially legal workers for women is statistically insignificant and has a magnitude of 0.6 log points for women. These results confirm past literature that potentially undocumented workers earn less than other immigrant workers (Rivera-Batiz 1999; Borjas 2017), but both groups are disadvantaged as compared to other workers (Borjas and Friedberg 2009).

My results show that potentially undocumented workers shoulder more negative effects from 287(g) agreements. I formally test the equivalence of these coefficients and find that these coefficients are statistically significantly different. Yet, the mechanisms that underlie these effects are unclear. Section V addresses the differences between 287(g) agreements—both in form and effect—to determine whether there exists heterogeneity in the effects of 287(g) agreements on immigrant worker wages.

C. Other Employment Effects

These agreements may also have the effect of shifting both undocumented and documented workers away from employment or into jobs with fewer hours. Employers may not want to hire immigrant workers in 287(g) agreement jurisdictions due to worries about ICE encounters, or immigrant workers may be selecting out of the work force to decrease detection or minimize unpleasant interactions with local enforcement officers. To determine whether these effects are present in my sample, I use my entire sample, without restrictions for number of hours worked or employment status. I modify the dependent variable from Equation 1 to the natural log of hours by each individual.

Table 7 shows the effects of 287(g) agreements on the number of hours worked for both immigrant and undocumented individuals. I find negative effects for the interaction between presence of a 287(g) agreement and immigrant status for both men and women, though the effect for male immigrant workers is stronger. Male immigrants in jurisdictions with 287(g) agreements work 1.3 percent fewer hours than their peers in jurisdictions without these agreements. Similarly, female immigrants work 0.9 percent fewer hours when residing in jurisdictions with 287(g) agreements. Though these results are small in magnitude, they are statistically significant at the 1 percent level. These results provide evidence that foreign-born individuals are not only disadvantaged by lower wages in 287(g) agreement jurisdictions but also will work fewer hours in response to these agreements.

Table 8 shows the effects of 287(g) agreements on the probability of employment for immigrant individuals. I adapt Equation 1 to a probit framework and assign the dependent variable to be one if the individual is employed at time *t*. Using this model, the coefficient on the interaction between immigrant status and the presence of a 287(g) agreement is positive and statistically significant. This result provides evidence that individuals do not select out of the work force in response to 287(g) agreements. Instead, foreign-born individuals may be more likely to work in jurisdictions with a 287(g) agreement. However, this result may be driven by employers substituting legal immigrants for undocumented workers, as potentially legal immigrants are more plentiful in my sample and could be replacing potentially undocumented workers in their jobs should undocumented workers leave these jobs.

D. Placebo Test

One of the assumptions of a triple difference model is that there are common trends in both the treated and untreated groups in a sample, or that the wages of both groups would grow in a similar manner but for the presence of a 287(g) agreement. I use a placebo test to examine whether there is a violation of the common trends assumption in my sample. To do so, I re-estimate Equation 1 by assuming that the 287(g) agreement was signed into law three years earlier than it was in each jurisdiction. Table 9 depicts the results of this falsification test. I find that, with these earlier enactment dates, my coefficient of interest, β_7, is not statistically significant and is close to zero. This suggests no violation of the common trends assumption in my sample and provides evidence that there is not a difference in time trends in the pre-treatment period between jurisdictions that have adopted these agreements and jurisdictions that have not.

E. Migration Response

Immigrants may also respond to these agreements by moving from jurisdictions with 287(g) agreements to jurisdictions without these agreements. This migration response may violate the assumptions of my triple-difference model if this migration shifts the quality of immigrants in treated or untreated states. For example, if high-earning immigrants leave 287(g) jurisdictions but low-earning immigrants do not, my model will not accurately capture the differences between the treated and treatment group as a result of the adoption of a 287(g) agreement. To test whether this is occurring in my model, I identify individuals who have moved by a question in the ACS that asks if an individual has changed residence in the past year. In the 2000 Census, the relevant question asks if the individual has moved in the past five years. I consider individuals who moved at all—whether intra- or inter-state—as migrants. I include those who moved intra-state as a migrant because both the ACS and Census do not identify the county from which the individual moved, and I am unable to determine whether individuals who move intra-state are moving into or out of locations with 287(g) agreements. I eliminate all individuals that have moved from my sample.

Table 10 reports the results of Equation 1 on my non-migrant sample. I find that there is no marked change in my results when eliminating individuals that have moved. The coefficient of interest, β_7 is negative and statistically significant, with a magnitude that is comparable to, but slightly large than, the results from Table 4. These results provide evidence of a limited migration response in my sample. These results are consistent with those in Parrado (2012), which found that Mexican workers did not have a migration response to a limited set of 287(g) agreements. As immigrants who are more educated and have better skills are more likely to move to a different city (Bartel and Koch 1991), these results without migrants provide support that the quality of individuals in jurisdictions with and without 287(g) agreements is not shifting in response to these agreements.

VI. Heterogeneity of 287(g) Agreements

The effects of 287(g) agreements may be affected by underlying heterogeneity in each agreement's text. As outrage over the scope—and increased visibility—of the program prompted concerns, and ultimately a temporary halt on the program, this section answers whether agreements with stronger enforcement mechanisms or a broader scope caused more harm than other agreements. The results of this section will inform the scope and strength of MOAs as they are adopted in the future.

A. Data

To examine the underlying heterogeneity present in 287(g) agreements, I use a dataset from ICE. This dataset derives from the Transactional Records Access Clearinghouse (TRAC), a data research organization, and was obtained from ICE using the Freedom of Information Act (FOIA).[25] The data contain individual records on each recorded I-247 detainer or notice request prepared by ICE,[26] including the month and year when ICE prepared the I-247 form, the state, county-facility detainer sent, whether the detention facility was federal, state, or county, and whether ICE assumed custody after the detainer was issued. For each detainer, the data contain both information on the agency that sent the request and demographic information, such as country of citizenship for each individual. The data also contain the individual's criminal history, categorized by both the most serious criminal conviction and whether that conviction falls under Level 1, Level 2, or Level 3 of the prioritization for 287(g) enforcement. TRAC used ICE's guidance to group recorded offense codes into these three categories.

B. Type of Program

I first investigate whether any of the three types of 287(g) agreements are driving the negative effects for immigrant wages in 287(g) jurisdictions. There exist three types of agreements: the jail enforcement model, the task force model, and the hybrid model, which is a combination of the two. These types of agreements could have differential

[25] This data was made available to me through Vanderbilt Law School's subscription to TRAC's data warehouse.
[26] Notice requests only ask that the law enforcement agency notify ICE of a pending release during the time the individual is otherwise in custody, and do not request that the person be detained beyond the point at which he or she would otherwise be released.

effects on immigrant outcomes because they allow law enforcement officers to engage with suspected immigration law violators in a different manner. As the task enforcement model allows officers to enforce immigration laws in the field when they are policing, while the jail model allows officers to only enforce these laws in the context of a jail, I predict that the task force model will have a stronger negative effect on immigrant worker wages.

To test this hypothesis, I use the following semi-logarithmic equation to test the differences among program types:

$$\begin{gathered} lnW_{ikt} = \propto + imm_{ikt}\beta_1 + \beta_2 county_k + \beta_3 year_t \\ + \beta_4(imm_{ikt} \times county_k) + \beta_5(year_t \times county_k) + \beta_6(imm_{ikt} \times year_t) \\ + \beta_7(imm_{ikt} \times task_{kt}) + \beta_8(imm_{ikt} \times jail_{kt}) \\ + \beta_9(imm_{ikt} \times hybrid_{kt}) + X_{ikt}\lambda + \epsilon_{ikt} \end{gathered} \tag{3}$$

where the dependent variable is the real hourly wage of an individual in 2010 dollars. The variables *task*, *jail*, and *hybrid*, represent the type of 287(g) agreement that each locality had at time *t.* I code these in accordance with Table 1. The definitions of all other variables are identical to those in Equation 1.

When examining the effects of the heterogeneity of programs, I find large and significant negative effects of task force and jail enforcement models on immigrant wages. Table 11 reports these results. The magnitude of the coefficient for task force models is larger than that of jail enforcement models. Male immigrants residing in a 287(g) jurisdiction with jail enforcement model earn 7.7 log points less than individuals residing in a state without a 287(g) agreement, while male immigrants in a jurisdiction with a task force agreement earn 10 fewer log points. These results are similar for female immigrants, with those with a jail enforcement model earning a 7.8 log point wage penalty and those with a task force jurisdictions earning a wage penalty of 11.7 log points. I find limited evidence of a negative effect of hybrid models for men, and no significant effect of these hybrid models on immigrant women wages. Overall, my results comport with criticisms of the broad nature of task force agreements, specifically. These agreements allow state and local officials to screen for status and issue detainers in the field during policing operations, essentially enforcing immigration laws on the street as they are doing their usual tasks. Therefore, it is no surprise that these effects will be felt more acutely as these agreements allow more action to be taken by police against individuals that they suspect violated federal immigration laws.

C. Strength of Program

Not all 287(g) agreements are created equal, and this heterogeneity is evident beyond the classification of the type of agreement. The MOAs in each agreement may also vary significantly. Some allow local officers trained through the program to interrogate any individual that they encounter, while others restrict an officer's ability to interrogate immigration status. Some 287(g) agreements also allow ICE to detain individuals and eventually assume custody of them. The bounds of each 287(g) agreement are important for determining what effects these agreements will have, both on the ultimate effects of the program and also the labor market effects of the agreement beyond the program. Strong levels of enforcement will magnify the effects of 287(g) agreements on immigrant worker wages, as they will increase the probability of interactions with ICE and can encourage workers to accept a wage below their true productivity. These effects may be particularly acute for undocumented workers, who have a higher fear of deportation (Arbona et al. 2010). If a 287(g) agreement is not strong, or is rarely enforced, it may not have the same effects as agreements that are enforced. However, if it is the presence of the agreement itself, and not enforcement, that is causing effects on the immigrant population, the strength of the agreement will not matter.

To assess the strength of the effects of each 287(g) agreement, I use a variable within the TRAC data that indicates whether ICE actually assumed custody of the individual. This variable is important because it tracks whether the local jurisdiction complied with ICE's detainer request and identifies whether the program has a strong level of enforcement. For each year in my sample, I compute the percentage of individuals that were taken into custody by ICE after a detainer request was issued. The mean of this proportion is 0.58 for my sample, so for any jurisdiction that has a proportion equal to or above 0.58, I characterize them as having a strong agreement, because they have a stronger effect by allowing ICE to take a greater proportion of individuals into custody after a detainer is issued. For any jurisdiction with a proportion below the mean value, I characterize them as having a low-strength agreement.

For the vast majority of jurisdictions, the number of individuals taken into ICE custody increased over time after the jurisdiction signed a 287(g) agreement. As an example of the program's evolution over time, Figure 1 shows the number of individuals that were taken into custody by ICE in one of the largest 287(g) jurisdictions, Los Angeles County California. Before Los Angeles County adopted its 287(g) agreement in 2005, there were very few individuals taken into custody by ICE. After 2005, the number of individuals taken into custody by ICE dramatically increased over time.

To assess the strength of the agreement's impact on workers, I modify Equation 1 to include a variable that captures the strength of the program:

$$
\begin{aligned}
lnW_{ikt} = \propto \ &+ imm_{ikt}\beta_1 + \beta_2 county_k + \beta_3 year_t + \beta_4(imm_{ikt} \times county_k) \\
&+ \beta_5(year_t \times county_k) + \beta_6(imm_{ikt} \times year_t) \\
&+ \beta_7(imm_{ikt} \times highstrength_{kt}) \\
&+ \beta_8(imm_{ikt} \times lowstrength_{kt}) + X_{ikt}\lambda + \epsilon_{ikt}
\end{aligned} \tag{4}
$$

where *highstrength* equals one if the individual lives in a jurisdiction with a 287(g) agreement where ICE takes a percentage of individuals into custody that is above or equal to the mean, and *lowstrength* equals one if the individual lives in a jurisdiction with a 287(g) agreement where ICE takes a percentage of individuals into custody that is below the mean. The definitions of all other variables are identical to those in Equation 1.

Table 12 reports the results of Equation 4. I find that both high and low strength agreements have strong negative effects on immigrant worker wages, with male immigrants in jurisdictions with high strength 287(g) agreements earning a 7.6 log point wage penalty and male immigrants in jurisdictions with low strength 287(g) agreements earning a 4.8 log point wage penalty. These effects are similar in magnitude, and are statistically different at the 1 percent level. The effect of high and low strength agreements on female workers is almost equivalent, with female immigrant workers earning 6.2 log point wage penalty in jurisdictions with high-strength agreements and female immigrant workers earning a 7.1 log point wage penalty in jurisdictions with low-strength agreements. I formally test these effects and find that they are not statistically different. Therefore, there is evidence that male immigrant wages differ based on the strength of each 287(g) agreement, but there is little evidence that this difference exists for female immigrant wages.

However, when examining the impact of these agreements on potentially undocumented workers, there is a separation between the effect of high-strength agreements and low-strength agreements on male undocumented worker wages. Table 13 reports these results. Male immigrants in jurisdictions with high strength 287(g) agreements earn an additional 7.7 log point wage penalty and male immigrants in jurisdictions with low strength 287(g) agreements earn an additional 4.6 log point wage penalty. Though similar in magnitude, these effects are statistically different at the 5 percent level. The effect of high and low strength agreements on female workers is again unclear, with female workers earning a 5.1 log point wage penalty for high-strength agreements and female workers earning a 9.1 log point wage penalty for low-strength agreements.

These results show that the effects of these agreements differ based on the actual strength of the agreement if the individual is male and undocumented. For both undocumented immigrants and immigrants in general, the effects are felt simply by having the agreement in place, rather than the strength of the agreement itself. This provides evidence that the inherent nature of the agreement, rather than the strength of the agreement, is driving the effects of these agreements on immigrant worker wages.

D. Targeting

One of the stated purposes of the 287(g) program is to identify immigrants that are committing serious crimes in order to initiate deportation proceedings. Many of these agreements reportedly targeted all immigrants, not just serious criminal offenders (Capps et. al 2011). To refocus the program, the Obama administration released a 2009 template to prioritize the identification and removal of dangerous criminals, rather than use the 287(g) agreements to identify all immigration law violators. These “targeted” models became more widespread in later years of the 287(g) program. However, recent 287(g) agreements under the Trump administration have shifted to broad forms of enforcement of 287(g) agreements, using template agreements that do not distinguish between immigrants based on seriousness of crime. This section explores whether 287(g) programs are more likely to target low-level criminal offenders or individuals who have not been charged with committing a crime and how this targeting affects immigrant worker wages.

ICE classifies criminal offenses in accordance with the National Crime Information Center’s (NCIC) three seriousness levels. The most serious (Level 1) covers what ICE considers to be “aggregated felonies”, including murder, manslaughter, rape, robbery, and kidnapping. Level 2 offenses include minor drug offenses and burglary, larceny, fraud, and money laundering. Level 3 offenses are misdemeanors, including petty civil offenses.

I determine whether these agreements that target all individuals, and not just those charged with serious crimes, have differential impacts on labor market outcomes of immigrant workers. The effects of these agreements could vary based on scope, as the broader these agreements are, the more people they will impact. This is especially true as individuals with no convictions can still fall under the purview of the agreement and end up detained by ICE.

To classify whether 287(g) agreements are broad or targeted, I use TRAC data on ICE detainers. For each jurisdiction with a 287(g) agreement, I examine the crimes charged for each individual that ICE detained. The TRAC data also classifies whether this crime falls under Level 1, Level 2, Level 3, or no conviction. I first determine the proportion of individuals that ICE detained in the jurisdiction with no conviction or with a Level 3 (misdemeanor) crime as compared to other crimes. The mean of this proportion is 0.54 for my sample, so for any jurisdiction that has a proportion equal to or above 0.54, I characterize them as having a broad agreement, because they detain more low-level offenders and individuals without convictions than any other jurisdiction. For any jurisdiction with a proportion below the mean value, I characterize them as a targeted jurisdiction.

To test whether the scope of the 287(g) program has an impact on immigrant wages, I modify Equation 1 to the following equation:

$$\begin{aligned} lnW_{ikt} = \propto\ + imm_{ikt}\beta_1 + \beta_2 county_k + \beta_3 year_t + \beta_4(imm_{ikt}\ \times\ county_k) \\ + \beta_5(year_t \times\ county_k) + \beta_6(imm_{ikt}\ \times\ year_t) + \ \beta_7(imm_{ikt}\ \times\ targeted_{kt}) \\ + \beta_8(imm_{ikt}\ \times\ broad_{kt}) \\ + \mathrm{X}_{ikt}\lambda +\ \epsilon_{ikt} \end{aligned} \tag{5}$$

where *broad* equals one if the individual lives in a jurisdiction with a 287(g) agreement where the proportion of individuals that ICE detains with no convictions or Category 3 convictions is higher than the mean, and *targeted* equals one if the individual lives in a jurisdiction with a 287(g) agreement where the proportion of individuals that ICE detains with no convictions or Category 3 convictions is below the mean. The definitions of all other variables are identical to those in Equation 1. β_7 and β_8 are the coefficients of interest and represent the change in immigrant wages if the individual lives in a jurisdiction with a targeted or broad 287(g) agreement, respectively.

Table 14 reports the results of Equation 5. I find that only those agreements with a broad scope have an effect on immigrant worker wages, while those with a targeted scope have little effect on immigrant worker wages. Male immigrant workers in jurisdictions with broad 287(g) agreements suffer a 2.9 log point wage penalty compared to their counterparts in jurisdictions without these agreements. Male immigrant workers in states with targeted agreements, however, do not have wages that differ significantly from male immigrant workers in jurisdictions without a 287(g) agreement. These results hold for female workers as well: female immigrant workers in jurisdictions with broad 287(g) agreements earn a 2.8 log point wage penalty, while the effects of a targeted program have no significant impact on female immigrant worker wages. These results provide support for the theory that targeted agreements, which only focus on those committing serious crimes, do not negatively impact the immigrant workforce.

When looking at the effect on potentially undocumented workers, I find similar results: broad scope agreements have a marked negative effect on undocumented worker wages. Table 15 reports that potentially undocumented male workers in states with broad agreements earn a wage penalty of 6.3 log points, but those in jurisdictions with targeted agreements do not have wages that differ significantly from male immigrant workers in jurisdictions without a 287(g) agreement. Female potentially undocumented workers in jurisdictions with 287(g) agreements earn a wage penalty of 9.4 log points. Again, the effects of residing in a county with a targeted scope program are insignificant. This follows previous literature focusing on undocumented worker wages: to the extent that broad agreements make undocumented workers fear deportation—because they could be confronted by ICE for no reason at all, or for a minor traffic violation—they will be more likely to accept lower wages (Rivera-Batiz 1999; Pena 2010a; Pena 2010b; Hotchkiss and Quispe-Agnoli 2013; Borjas 2017). As more broad continue to be signed in the modern era of 287(g) agreements, these will likely have a greater impact on both undocumented and legal immigrant wages.

VII. Conclusion

This chapter examines a policy that engages with two important aspects of modern immigration law: immigration federalism, or the increasing delegation of the federal immigration authority to local authorities, and the increasing overlap between immigration law and criminal law. 287(g) agreements empower local agents to not only enforce federal immigration laws, but also encourage them to do so in the context of criminal enforcement. This chapter contributes to the existing literature on immigration enforcement by providing evidence that 287(g) agreements are associated with approximately a 5 percent wage penalty for both male and female immigrant workers. These penalties are even larger effect for those who can be classified as potentially undocumented under the methodology of Borjas (2017), with potentially undocumented men earning a 6 percent wage penalty in places with 287(g) agreements and potentially undocumented women earning a 9 percent wage penalty. As more 287(g) agreements are signed between ICE and local law enforcement agencies and local continue to be signed in 2017 and beyond, the impacts on the immigrant community itself—included undocumented workers—should be considered.

This chapter also contributes to the existing literature on immigration enforcement by examining the effects of underlying heterogeneity in the local enforcement immigration of federal immigration laws. Targeted programs that focus on only individuals who commit crimes beyond low-level misdemeanors had no effect on the wages of immigrants in those jurisdictions. However, broad programs that allow for the detainment of any individual, even for low-level misdemeanors or no criminal violation at all, have large impacts on immigrant wages. These negative wage effects are not confined to potentially undocumented workers, which provides evidence that the immigrant community as a whole is harmed by these broad partnerships. When negotiating these agreements, local law enforcement can limit the impact of these laws on the immigrant population as a whole if they limit the program's scope, rather than engaging in widespread local enforcement of federal immigration laws.

More broadly, these results provide evidence that local enforcement of immigration law has an impact on the labor market outcomes of immigrant workers. To the extent that local jurisdictions seek to discourage the negative effects that are incurred by immigrant workers in 287(g) jurisdictions, they could consider employing other local policies that fit within the framework of immigration federalism, such as sanctuary cities, the inverse of 287(g) programs. This chapter shows that the effects of enforcement through immigration federalism can be felt widely in immigrant labor markets and is not confined to just those workers who may be undocumented.

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FIGURES AND TABLES

Table 1: List of All Active 287(g) Agreements, as of December 2011

State / County / City	Law Enforcement Agency	Model	Date Signed
Alabama	Alabama Department of Public Safety	Task Force	09/10/2003
Arizona	Arizona Department of Corrections	Jail &Task Force	09/16/2005
Arizona	Arizona Department of Public Safety	Jail & Task Force	04/15/2007
Mesa, Arizona	City of Mesa Police Department	Jail & Task Force	11/19/2009
Phoenix, Arizona	City of Phoenix Police Department	Jail & Task Force	03/10/2008
Florence, Arizona	Florence Police Department	Task Force	10/21/2009
Maricopa County, Arizona	Maricopa County Sherriff's Office	Jail & Task Force	02/07/2007
Pima County, Arizona (Tuscon)	Pima County Sheriff's Office	Jail & Task Force	03/10/2008
Pinal County, Arizona	Pinal County Sheriff's Office	Jail & Task Force	03/10/2008
Yavapai County, Arizona	Yavapai County Sheriff's Office	Jail & Task Force	03/10/2008
Benton County, Arkansas (Bentonville)	Benton County Sheriff's Office	Jail & Task Force	09/26/2007
Springdale, Arkansas	City of Springdale Police Department	Task Force	09/26/2007
Rogers, Arkansas	Rogers Police Department	Task Force	09/25/2007
Washington County, Arkansas	Washington County Sheriff's Office	Jail & Task Force	09/26/2007
Los Angeles County, California (Los Angeles)	Los Angeles County, Sheriff's Office	Jail Enforcement	02/01/2005
Orange County, California (Anaheim)	Orange County Sheriff's Office	Jail Enforcement	11/02/2006
Riverside County, California	Riverside County Sheriff's Office	Jail Enforcement	04/28/2006
San Bernardino County, California	San Bernardino County Sheriff's	Jail Enforcement	11/19/2005

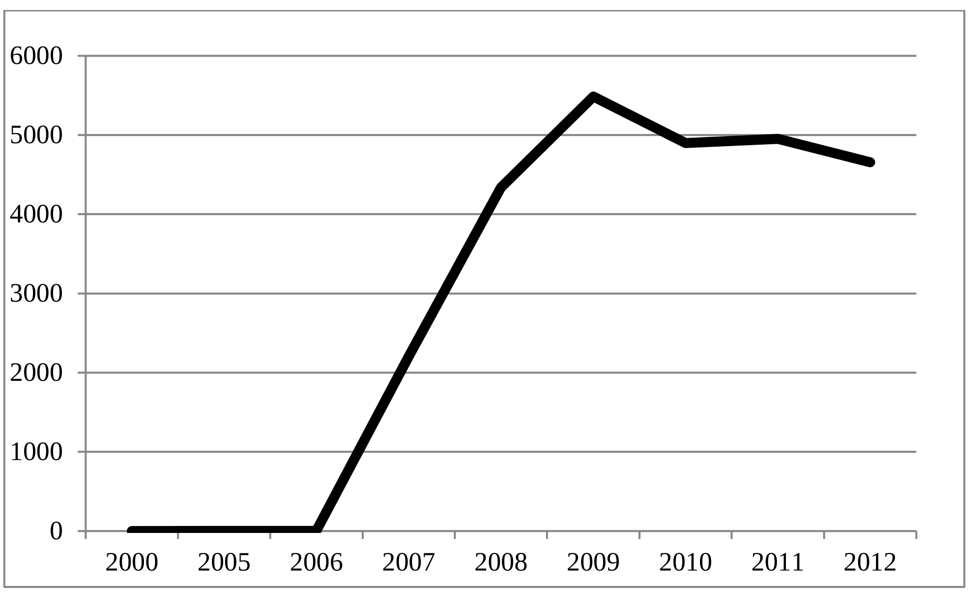
	Office		
Colorado	Colorado Department of Public Safety	Task Force	03/29/2007
El Paso County, Colorado	El Paso County Sheriff's Office	Jail Enforcement	05/17/2007
Danbury, Connecticut	City of Danbury Police Department	Task Force	10/15/2009
Delaware	Delaware Department of Corrections	Jail Enforcement	10/15/2009
Bay County, Florida (Panama City)	Bay County Sheriff's Office	Task Force	06/15/2008
Collier County, Florida (Naples)	Collier County, Sheriff's Office	Jail & Task Force	08/06/2007
Florida	Florida Department of Law Enforcement	Task Force	07/02/2002
Jacksonville, Florida	Jacksonville Sheriff's Office	Jail Enforcement	07/08/2008
Georgia	Cobb County Sheriff's Office	Jail Enforcement	02/13/2007
Georgia	Georgia Department of Public Safety	Task Force	07/27/2007
Gwinnet County, Georgia	Gwinnet County Sheriff's Office	Jail Enforcement	10/15/2009
Hall County, Georgia	Hall County Sheriff's Office	Jail & Task Force	02/29/2008
Whitfield County, Georgia	Whitfield County Sheriff's Office	Jail Enforcement	02/04/2008
Frederick County, Maryland	Frederick County Sheriff's	Jail & Task Force	02/06/2008
Minnesota	Minnesota Department of Public Safety	Task Force	09/22/2008
Missouri	Missouri State Highway Control	Task Force	06/25/2008
Las Vegas, Nevada	Las Vegas Metropolitan Police Department	Jail Enforcement	09/08/2008
Hudson City, New Hampshire	Hudson City Police Department	Task Force	05/05/2007
Hudson County, New Jersey	Hudson County Department of Corrections	Jail & Task Force	08/11/2008
Monmouth County, New Jersey	Monmouth County Sheriff's Office	Jail Enforcement	10/15/2009
New Mexico	New Mexico	Jail Enforcement	09/17/2007

	Department of Corrections		
Alamance County, North Carolina	Alamance County Sheriff's Office	Jail Enforcement	01/10/2007
Cabarrus County, North Carolina	Cabarrus County Sheriff's Office	Jail Enforcement	08/02/2007
Durham, North Carolina	Durham Police Department	Task Force	02/01/2008
Gaston County, North Carolina	Gaston County Police Department	Jail Enforcement	02/22/2007
Guilford County, North Carolina	Guilford County Sheriff's Office	Task Force	10/15/2009
Henderson County, North Carolina	Henderson County Sheriff's Office	Jail Enforcement	06/25/2008
Mecklenburg County, North Carolina (Charlotte)	Mecklenburg County Sheriff's Office	Jail Enforcement	02/27/2006
Butler County, Ohio	Butler County Sheriff's Office	Jail & Task Force	06/25/2008
Tulsa County, Oklahoma (Tulsa)	Tulsa County Sheriff's Office	Jail & Task Force	08/06/2007
Rhode Island	Rhode Island State Police	Task Force	10/15/2009
Beaufort County, South Carolina	Beaufort County Sheriff's Office	Task Force	06/25/2008
Charleston County. South Carolina (Charleston)	Charleston County Sheriff's Department	Jail Enforcement	11/09/2009
York County, South Carolina	York County Sheriff's Office	Jail Enforcement	10/16/2007
Davidson County, Tennessee (Nashville)	Davidson County Sheriff's Office	Jail Enforcement	02/21/2007
Tennessee	Tennessee Department of Safety	Task Force	06/25/2008
Carrollton, Texas	Carrollton Police Department	Jail Enforcement	08/12/2008
Farmers Branch, Texas	Farmers Branch Police Department	Task Force	07/08/2008
Harris County, Texas (Houston)	Harris County Sheriff's Office	Jail Enforcement	07/20/2008
Washington County, Utah	Washington County Sheriff's Office	Jail Enforcement	09/22/2008

Weber County, Utah	Weber County Sheriff's Office	Jail Enforcement	09/22/2008
Herndon, Virginia	Herndon Police Department	Task Force	03/21/2007
Loudoun County, Virginia	Loudoun County Sheriff's Office	Task Force	06/25/2008
Manassas Park, Virginia	Manassas Park Police Department	Task Force	03/10/2008
Manassas, Virginia	Manassas Police Department	Task Force	03/05/2008
Prince William County, Virginia	Prince William County Police Department	Task Force	02/26/2008
Prince William County, Virginia	Prince William County Sheriff's Office	Task Force	02/26/2008
Prince William-Manassas, Virginia	Prince William-Manassas Regional Jail	Jail Enforcement	07/09/2007
Rockingham County, Virginia	Rockingham County Sheriff's Office	Jail & Task Force	04/25/2007
Shenandoah, Virginia	Shenandoah County Sheriff's Office	Jail & Task Force	05/10/2007

Source: Capps et al. (2011)

Figure 1: Number of Individuals, ICE Assumed Custody: Los Angeles County, 2000–2012



Source: Transactional Records Access Clearinghouse (TRAC) Data, Tracking Immigration and Customs Enforcement Detainers. Last Accessed November 9, 2017.

Table 2: Summary Statistics by Presence of 287(g) Agreement in Jurisdiction of Residence

Variable	287(g) Agreement	No 287(g) Agreement
Male	0.57	0.59
Age	41.99	41.50
Married	0.55	0.59
Years of Education	13.55	13.58
Hispanic	0.20	0.11
White	0.73	0.78
Black	0.12	0.11
Asian	0.06	0.04
Other Race	0.09	0.07
Foreign Born	0.23	0.15
Wage (in 2010 dollars)	24.04	24.50
N	397,021	1,839,335

Note: Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Weighted using sample weights. Sample restricted to those working full-time and full-year. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Those born in Guam, Puerto Rico, or the U.S. Virgin Islands were excluded from this analysis, as were citizens born abroad.

Table 3: Summary Statistics by Immigrant Status

Variable	Immigrant	Native to U.S.
Male	0.63	0.57
Age	40.57	41.75
Married	0.60	0.59
Years of Education	12.76	13.72
Hispanic	0.45	0.07
White	0.46	0.83
Black	0.08	0.11
Asian	0.24	0.01
Other Race	0.22	0.04
Wage (in 2010 dollars)	22.48	24.82
N	316,605	1,919,751

Note: Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Weighted using sample weights. Sample restricted to those working full-time and full-year. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Those born in Guam, Puerto Rico, or the U.S. Virgin Islands were excluded from this analysis, as were citizens born abroad.

Table 4: Triple Difference Estimates, 287(g) Agreements 2000–2012

	Men **ln(wage)**	**Women** **ln(wage)**
Age	0.0579***	0.0526***
	(0.0007)	(0.0008)
Age Squared / 100	-0.0544***	-0.0512***
	(0.0009)	(0.0009)
Years of Education	0.0863***	0.1044***
	(0.0006)	(0.0007)
Hispanic	-0.0950***	-0.0514***
	(0.0051)	(0.0055)
Black	-0.1703***	-0.0858***
	(0.0046)	(0.0042)
Asian	-0.0811***	-0.0120
	(0.0076)	(0.0082)
Other Race	-0.0408***	-0.0318***
	(0.0059)	(0.0068)
Immigrant	-0.0338***	-0.0291***
	(0.0041)	(0.0041)
Immigrant × 287(g) Agreement	-0.0484***	-0.0479***
	(0.0106)	(0.0125)
Constant	0.3824***	0.1205***
	(0.0082)	(0.0090)
Observations	1,288,237	950,119
R-squared	0.3342	0.3089

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the log of wages in 2010 dollars. Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Sample weights are used. Sample restricted to those working full-time and full-year. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, children present, and veteran status. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. "287(g) Agreement" indicates the presence of a 287(g) agreement in state *k* at time *t*.

Table 5: Summary Statistics by Immigrant Legal Status

Variable	Legal Immigrant	Potentially Undocumented Immigrant
Male	0.58	0.71
Age	44.35	35.61
Married	0.66	0.52
Years of Education	13.47	11.81
White	0.46	0.46
Hispanic	0.34	0.59
Black	0.07	0.07
Asian	0.29	0.19
Other Race	0.28	0.28
Wage (in 2010 dollars)	25.88	18.02
N	191,784	124,821

Note: Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Weighted using sample weights. Sample restricted to those working full-time and full-year. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. Age was restricted to those between the ages of 18 and 65. Those born in Guam, Puerto Rico, or the U.S. Virgin Islands were excluded from this analysis, as were citizens born abroad. “Potentially Undocumented” indicates whether the individual is classified as an undocumented worker according to Borjas (2017). For more information about this classification, see Section IV.

Table 6: Triple Difference Estimates of the Impact of 287(g) Agreements by Potential Legal Status

	Men **ln(wage)**	**Women** **ln(wage)**
Age	0.0575***	0.0524***
	(0.0007)	(0.0008)
Age Squared / 100	-0.0542***	-0.0512***
	(0.0009)	(0.0009)
Years of Education	0.0864***	0.1039***
	(0.0006)	(0.0007)
Hispanic	-0.0821***	-0.0417***
	(0.0051)	(0.0055)
Black	-0.1680***	-0.0858***
	(0.0046)	(0.0042)
Asian	-0.0882***	-0.0202**
	(0.0075)	(0.0081)
Other Race	-0.0435***	-0.0349***
	(0.0059)	(0.0068)
Potentially Undocumented	-0.0980***	-0.1421***
	(0.0067)	(0.0086)
Potentially Legal	-0.0167***	-0.0064
	(0.0060)	(0.0066)
Potentially Undocumented × 287(g) Agreement	-0.0681***	-0.0894***
	(0.0117)	(0.0173)
Potentially Legal × 287(g) Agreement	-0.0586***	-0.0543***
	(0.0116)	(0.0123)
Constant	0.4461***	0.1856***
	(0.0082)	(0.0090)
Observations	1,288,237	950,119
R-squared	0.3373	0.3154

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Sample weights are used. Sample restricted to those working full-time and full-year, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, children present, and veteran status. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. "287(g) Agreement" indicates the presence of a 287(g) agreement in state *k* at time *t*. "Potentially Undocumented" indicates whether the individual is classified as an undocumented worker according to Borjas (2017). For more information about this classification, see Section IV.

Table 7: Triple Difference Estimates of the Impact of 287(g) Agreements on Immigrant Worker Hours

	Men **ln(hours)**	**Women** **ln(hours)**
Age	0.0043***	0.0040***
	(0.0001)	(0.0001)
Age Squared / 100	-0.0051***	-0.0043***
	(0.0001)	(0.0001)
Years of Education	0.0062***	0.0075***
	(0.0001)	(0.0001)
Hispanic	-0.0164***	-0.0080***
	(0.0009)	(0.0008)
Black	-0.0144***	0.0026
	(0.0022)	(0.0021)
Asian	0.0043***	0.0040***
	(0.0001)	(0.0001)
Other Race	-0.0487***	-0.0293***
	(0.0029)	(0.0033)
Immigrant	-0.0314***	-0.0312***
	(0.0048)	(0.0049)
Immigrant × 287(g) Agreement	-0.0130***	-0.0098***
	(0.0016)	(0.0016)
Constant	3.6499***	3.5727***
	(0.0083)	(0.0075)
Observations	1,342,983	1,109,614
R-squared	0.0511	0.0466

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Sample weights are used. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, children present, and veteran status. Sample is restricted to workers earning between \$1.50 and \$300 per hour who are not self-employed. "287(g) Agreement" indicates the presence of a 287(g) agreement in state *k* at time *t*.

Table 8: Probit Estimates of the Impact of 287(g) Agreements on Immigrant Worker Employment

	Men Pr(emp)	**Women** Pr(emp)
Age	0.0391***	0.0544***
	(0.0013)	(0.0015)
Age Squared / 100	-0.0472***	-0.0597***
	(0.0014)	(0.0017)
Years of Education	0.0422***	0.0505***
	(0.0011)	(0.0013)
Hispanic	-0.1599***	-0.1332***
	(0.0104)	(0.0117)
Black	-0.2658***	-0.2460***
	(0.0089)	(0.0086)
Asian	-0.2490***	-0.1856***
	(0.0141)	(0.0157)
Other Race	-0.0726***	-0.0815***
	(0.0114)	(0.0137)
Immigrant	-0.0790***	-0.0828***
	(0.0094)	(0.0110)
Immigrant × 287 (g) Agreement	0.2372***	0.1497***
	(0.0181)	(0.0208)
Constant	0.2373***	-0.2749***
	(0.0295)	(0.0347)
Observations	1,545,884	1,330,166

***p<0.01; **p<0.05; *p<0.10. The dependent variable is equal to one if the individual is employed. Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Sample weights are used. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, children present, and veteran status. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. "287(g) Agreement" indicates the presence of a 287(g) agreement in state *k* at time *t*.

Table 9: Triple Difference Estimates, Placebo Test

	Men **ln(wage)**	**Women** **ln(wage)**
Age	0.0577***	0.0526***
	(0.0007)	(0.0008)
Age Squared / 100	-0.0543***	-0.0512***
	(0.0009)	(0.0009)
Years of Education	0.0862***	0.1044***
	(0.0006)	(0.0007)
Hispanic	-0.0988***	-0.0548***
	(0.0051)	(0.0055)
Black	-0.1725***	-0.0884***
	(0.0046)	(0.0042)
Asian	-0.0807***	-0.0100
	(0.0076)	(0.0081)
Other Race	-0.0383***	-0.0301***
	(0.0059)	(0.0068)
Immigrant	0.0261*	0.0078
	(0.0150)	(0.0176)
Immigrant × 287(g) Agreement	-0.0158	0.0151
	(0.0251)	(0.0286)
Constant	0.3846***	0.1187***
	(0.0315)	(0.0369)
Observations	910,398	682,979
R-squared	0.3441	0.3192

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Sample weights are used. Sample restricted to individuals who have not moved in the past five years and those working full-time and full-year, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, children present, and veteran status. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. "287(g) Agreement" indicates the presence of a 287(g) agreement in state *k* at time *t*–3.

Table 10: Triple Difference Estimates Without Migrants

	Men ln(wage)	**Women** ln(wage)
Age	0.0583***	0.0532***
	(0.0004)	(0.0005)
Age Squared / 100	-0.0553***	-0.0516***
	(0.0005)	(0.0006)
Years of Education	0.0898***	0.1086***
	(0.0004)	(0.0004)
Hispanic	-0.1176***	-0.0776***
	(0.0031)	(0.0033)
Black	-0.1783***	-0.0787***
	(0.0028)	(0.0026)
Asian	-0.0964***	-0.0062
	(0.0046)	(0.0049)
Other Race	-0.0530***	-0.0332***
	(0.0037)	(0.0041)
Immigrant	-0.0221***	-0.0109***
	(0.0041)	(0.0041)
Immigrant × 287(g) Agreement	-0.0535***	-0.0590***
	(0.0189)	(0.0150)
Constant	0.4855***	0.2470***
	(0.0315)	(0.0369)
Observations	1,288,237	950,119
R-squared	0.3441	0.3192

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Sample weights are used. Sample restricted to individuals who have not moved in the past five years (2000 Census) or moved in the past year (ACS) and those working full time and full-year, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Sample restricted to those who have not moved in the past year inter- or intra-state. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, children present, and veteran status. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. “287(g) Agreement” indicates the presence of a 287(g) agreement in state *k* at time *t*.

Table 11: Triple Difference Estimates of Agreement Type

	Men **ln(wage)**	**Women** **ln(wage)**
Age	0.0580***	0.0511***
	(0.0001)	(0.0001)
Age Squared / 100	-0.0548***	-0.0496***
	(0.0001)	(0.0001)
Years of Education	0.0868***	0.1043***
	(0.0009)	(0.0011)
Hispanic	-0.1117***	-0.0732***
	(0.0008)	(0.0008)
Black	-0.1693***	-0.0668***
	(0.0007)	(0.0006)
Asian	-0.0748***	-0.0293**
	(0.0012)	(0.0013)
Other Race	-0.0525***	-0.0160
	(0.0009)	(0.0010)
Immigrant × Jail 287(g) Agreement	-0.0775***	-0.0408***
	(0.0040)	(0.0046)
Immigrant × Task Force 287(g) Agreement	-0.1006***	-0.1177***
	(0.0024)	(0.0028)
Immigrant × Hybrid 287(g) Agreement	-0.0115*	0.0078
	(0.0052)	(0.0058)
Constant	0.1920**	0.2609***
	(0.0089)	(0.0094)
Observations	1,288,237	950,119
R-squared	0.3401	0.3204

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Sample weights are used. Sample restricted to those working full-time and full-year, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, children present, and veteran status. Sample is restricted to workers earning between \$1.50 and \$300 per hour who are not self-employed. "287(g) Agreement" indicates the presence of a 287(g) agreement in state *k* at time *t*. Jail, task force, and hybrid agreements are described in detail in Section II.

Table 12: Triple Difference Estimates of the Impact of Agreement Strength

	Men **ln(wage)**	**Women** **ln(wage)**
Age	0.0577***	0.0526***
	(0.0007)	(0.0008)
Age Squared / 100	-0.0543***	-0.0512***
	(0.0009)	(0.0009)
Years of Education	0.0869***	0.1048***
	(0.0006)	(0.0007)
Hispanic	-0.0887***	-0.0468***
	(0.0051)	(0.0055)
Black	-0.1670***	-0.0845***
	(0.0046)	(0.0042)
Asian	-0.0837***	-0.0128
	(0.0075)	(0.0081)
Other Race	-0.0443***	-0.0361***
	(0.0059)	(0.0068)
Immigrant	-0.0533***	-0.0525***
	(0.0050)	(0.0057)
Immigrant × High Strength Agreement	-0.0768***	-0.0621***
	(0.0122)	(0.0139)
Immigrant × Low Strength Agreement	-0.0484***	-0.0716***
	(0.0116)	(0.0142)
Constant	0.4106***	0.1454***
	(0.0082)	(0.0090)
Observations	1,288,237	950,119
R-squared	0.3378	0.3160

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Sample weights are used. Sample restricted to those working full-time and full-year, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, children present, and veteran status. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. "High Strength Agreement" and "Low Strength Agreement" indicate the presence of a 287(g) agreement that resulted in a percentage of individuals taken into custody by ICE that is above or below the median in state *k* at time *t*.

Table 13: Triple Difference Estimates of the Impact of Agreement Strength on the Potentially Undocumented Population

	Men **ln(wage)**	**Women** **ln(wage)**
Age	0.0571***	0.0522***
	(0.0007)	(0.0008)
Age Squared / 100	-0.0540***	-0.0511***
	(0.0009)	(0.0009)
Years of Education	0.0866***	0.1042***
	(0.0006)	(0.0007)
Hispanic	-0.0824***	-0.0422***
	(0.0051)	(0.0055)
Black	-0.1670***	-0.0842***
	(0.0046)	(0.0042)
Asian	-0.0803***	-0.0088
	(0.0075)	(0.0081)
Other Race	-0.0418***	-0.0325***
	(0.0059)	(0.0067)
Potentially Undocumented	-0.0145	-0.0573***
	(0.0089)	(0.0108)
Potentially Undocumented× High Strength Agreement	-0.0770***	-0.0511***
	(0.0155)	(0.0142)
Potentially Undocumented × Low Strength Agreement	-0.0468***	-0.0910***
	(0.0154)	(0.0229)
Constant	0.0577***	0.0527***
	(0.0004)	(0.0004)
Observations	1,288,237	950,119
R-squared	0.3377	0.3161

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Sample weights are used. Sample restricted to those working full-time and full-year, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Additional controls include industry, marital status, children present, and veteran status. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. “High Strength Agreement” and “Low Strength Agreement” indicate the presence of a 287(g) agreement that resulted in a percentage of individuals taken into custody by ICE that is above or below the mean in state *k* at time *t*. “Potentially Undocumented” indicates whether the individual is classified as an undocumented worker according to Borjas (2017). For more information about this classification, see Section IV.

Table 14: Triple Difference Estimates of the Effects of Scope

	Men **ln(wage)**	**Women** **ln(wage)**
Age	0.0577***	0.0525***
	(0.0007)	(0.0008)
Age Squared / 100	-0.0543***	-0.0510***
	(0.0009)	(0.0009)
Years of Education	0.0867***	0.1047***
	(0.0006)	(0.0007)
Hispanic	-0.0916***	-0.0513***
	(0.0051)	(0.0055)
Black	-0.1685***	-0.0850***
	(0.0046)	(0.0042)
Asian	-0.0847***	-0.0144*
	(0.0075)	(0.0082)
Other Race	-0.0443***	-0.0349***
	(0.0059)	(0.0068)
Immigrant	-0.0630***	-0.0637***
	(0.0048)	(0.0055)
Targeted Scope × Immigrant	-0.0217	-0.01973
	(0.0565)	(0.0647)
Broad Scope × Immigrant	-0.0295***	-0.0282**
	(0.0112)	(0.0133)
Constant	0.4133***	0.1472***
	(0.0082)	(0.0090)
Observations	1,288,237	950,119
R-squared	0.3376	0.3157

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Sample weights are used. Sample restricted to those working full-time and full-year, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, children present, and veteran status. Sample is restricted to workers earning between \$1.50 and \$300 per hour who are not self-employed. "Broad Scope" and "Targeted Scope" indicate the targeted nature of each 287(g) agreement that. If the proportion of individuals who committed no crime or committed a low-level crime under ICE detainer request in state *k* at time *t* is greater than the mean, observations are classified as "Broad Scope," while "Targeted Scope" captures observations under the mean, as these agreements mostly target individuals who commit crimes that are considered to be a priority. For more discussion of these labels, see Section V.

Table 15: Triple Difference Estimates of the Effects of Scope on Potentially Undocumented Immigrants

	Men **ln(wage)**	**Women** **ln(wage)**
Age	0.0577***	0.0525***
	(0.0007)	(0.0008)
Age Squared / 100	-0.0543***	-0.0510***
	(0.0009)	(0.0009)
Years of Education	0.0867***	0.1046***
	(0.0006)	(0.0007)
Hispanic	-0.0915***	-0.0511***
	(0.0051)	(0.0055)
Black	-0.1687***	-0.0852***
	(0.0046)	(0.0042)
Asian	-0.0847***	-0.0148*
	(0.0075)	(0.0082)
Other Race	-0.0441***	-0.0350***
	(0.0059)	(0.0068)
Potentially Undocumented	-0.0631***	-0.0633***
	(0.0047)	(0.0053)
Targeted Scope × Potentially Undocumented	-0.0445	-0.0322
	(0.0702)	(0.0972)
Broad Scope × Potentially Undocumented	-0.0637***	-0.0949***
	(0.0143)	(0.0221)
Constant	0.4136***	0.1490***
	(0.0082)	(0.0090)
Observations	1,288,237	950,119
R-squared	0.3334	0.3084

***p<0.01; **p<0.05; *p<0.10. The dependent variable is the natural log of wages in 2010 dollars. Data shown is comprised of the 1% 2000 Census and a 1% sample of both the 3-year 2005–2007 and 5-year 2008–2012 American Community Survey. Sample weights are used. Sample restricted to those working full-time and full-year, with usual hours greater than 35 hours per week and weeks worked per year greater than 50. Age was restricted to those between the ages of 18 and 65. Additional controls include industry, marital status, children present, and veteran status. Sample is restricted to workers earning between $1.50 and $300 per hour who are not self-employed. "Broad Scope" and "Targeted Scope" indicate the targeted nature of each 287(g) agreement that. If the proportion of individuals who committed no crime or committed a low-level crime under ICE detainer request in state *k* at time *t* is greater than the mean, observations are classified as "Broad Scope," while "Targeted Scope" captures observations under the mean, as these agreements mostly target individuals who commit crimes that are considered to be a priority. For more discussion of these labels, see Section V. " Potentially Undocumented" indicates whether the individual is classified as an undocumented worker according to Borjas (2017). For more information about this classification, see Section IV.

CHAPTER THREE: BACKLOGS AND BORROWED TIME: THE IMPACT OF CASE BACKLOG ON THE IMMIGRATION COURT SYSTEM

I. Introduction

Case backlog has plagued the U.S. immigration court system for almost a decade. This backlog coincides with a time of immense delay in the immigration court system, with some courts scheduling hearings more than seven years in the future. Other than the inconvenience of delay, commentators have hypothesized that backlog can also disadvantage the likelihood of success of the underlying cases, as immigration court proceedings often turn on evidence that could become stale over time. As immigration enforcement has increased over the past decade, the inflows to immigration courts have also steadily increased, leaving immigration court backlog at an all-time high. At the end of the 2017 fiscal year, there were over 629,000 cases pending in immigration courts nationwide. This backlog has continued to increase in the early months of 2018. In this chapter, I use the variation in case backlog in each immigration court to determine how backlogs affects the duration of each case and the outcome of each case decided by immigration judges.

Immigration judges presiding over immigration cases work under fast-paced, high-pressure, and culturally-charged conditions. Due to this pressure, immigration judges have higher burnout rates than hospital workers and prison wardens.[27] Immigration judges must juggle hundreds, if not thousands of cases, at once. The infrastructure to support these judges has been lacking—with small staffs and limited

[27] Lustig et al. (2008) conducted a survey of 96 immigration judges that indicated that immigration judges were suffering significant symptoms of secondary traumatic stress and reported more burnout than any other professional group that the Copenhagen Burnout Inventory (CBI) has investigated, including busy physicians and prison wardens.

resources, immigration judges are acutely aware of the pressures that backlog creates in their courtrooms.

However, judicial backlog is not unique to immigration courts. Supreme Court Justice Samuel Alito stated that one of the greatest problems facing the judiciary was the “crushing” workload faced by courts of appeals.[28] Though there has been extensive discussion in legal literature on backlog in the federal courts, this discussion, until recently, has been qualitative and has focused on the judicial decision-making process in times of high caseloads (Richman and Reynolds 1996; Gulati and McCauliff 1998). It has since evolved to focus on two outputs: the number of case completions and the outcomes of the underlying cases.

A growing law and economics literature examines caseload effects on judicial output. Studies have found conflicting evidence of caseload on case completions. Beenstock and Haitovsky (2004) study the workload of judges in Israel and find that there is a positive correlation between cases completed and caseload pressure, in line with literature finding that higher caseloads leads to higher case completion rates due to pressure that incentivizes judges to speed up case resolutions (Luskin and Luskin 1986; Rosales-Lopez 2008; Dimitrova-Grajzl et al. 2012). However, other studies suggest that an increase in caseload leads to a congestion effect in courts, which slows down the court’s productivity and thwarts case resolution (Buscaglia and Ulen 1997; Murrell 2001). This chapter will determine whether the incentivization effect or congestion effect is stronger in U.S. immigration courts.

[28] Interview by David F. Levi with Samuel A. Alito, Assoc. Justice, Supreme Court of the United States, in Durham, North Carolina (Sept. 15, 2010); Huang (2011).

Studies also examine the effects of judicial decision making on the ultimate outcome of individual cases. In addition to studies that focus on immigration court cases (Ramji-Nogales et al. 2007; Keith and Holmes 2009; Chen et al. 2016; Ryo 2016), studies on the broader judiciary suggest that individual judge or court characteristics have some influence on the outcomes of cases (Anderson et al. 1999; Revesz 1999; Sunstein et al. 2004).[29] This literature pushes back on the ideal that individual cases are decided based on merit rather than extraneous factors, such as the biases of adjudicators. Backlog can be one of these extraneous factors (Huang 2011), but only a few papers have examined this factor in the immigration court context. Chen et al. (2016) find that U.S. immigration judges are less likely to approve an individual claiming asylum when they approved the previous claimant—thus, exhibiting gambler's fallacy—when case backlog is high. Norris (2018) studies refugee appeals courts in Canada and finds that a 10 percent higher workload reduces the consistency of judicial decision making, an effect that remains persistent even with controls for case outcomes. This chapter adds to the existing literature examining case backlog in courts by providing an analysis of the effects of backlog on case duration and case outcome in U.S. immigration courts.

I use a restricted-access dataset derived from Department of Homeland Security (DHS) records that captures all four million juvenile cases decided in immigration courts between 2003 and 2013. Juvenile cases provide a unique opportunity to analyze the effects of case backlog in immigration courts because, until 2014, they were not subject to changes in priority in the immigration court system, which continuously shifted the order of priority of adult cases, particularly when those individuals had been convicted of

[29] There is also a substantial literature that focuses on the effects of extraneous factors on judicial decision making, such as the proximity of the hearing to the judge's lunch time (Danzinger, Levav, and Avnaim-Pesso 2011) and the outcome of the home team's football game (Eren and Mocan 2016).

a crime. Studying only juvenile cases also allows me to disentagle the effects of case duration in immigration courts by focusing on the exogenous change in the backlog of adult cases.

I find, consistent with evidence on congestion effects in other courts, that immigration court backlog is correlated with longer case durations, even when controlling for specific court- and case-level factors. I find that when adult case backlog increases, juvenile case duration also increases in immigration courts by roughly three percent. This congestion effect persists regardless of the success of an individual's claim or the individual's representation status. However, I do find this effect is heterogeneous by year, suggesting a non-linear relationship between case backlog and case duration. I find that the relationship between case duration and backlog shifts when overall caseload becomes sufficiently large, suggesting that courts operate efficiently until they reach their breaking point. I find this phenomenon also holds when examining the probability of success in an immigration court case.

As backlog intensifies, courts are noting the realities of this delay, including the substantial costs to the immigration system as a whole. The Department of Homeland Security (DHS) spent approximately $2 billion on immigration detention in 2014, at an average of $5.46 million per day, or $161 per detainee per day (Ryo 2016). Mounting costs of case delay—including the cost to continue to hold individuals in detention—are straining the immigration system. My analysis shows that case backlog in immigration courts is associated with an increase in case duration, providing a congestion effect that can be costly to the immigration system as a whole.

II. Background

The immigration court system is an important component of immigration enforcement in the United States. Individuals charged with violating immigration law are able to present their cases in these administrative courts, which operate under the Department of Justice's Executive Offices of Immigration Review (EOIR). There are approximately 250 immigration judges currently serving in immigration courts. Immigration judges (IJs) oversee the fifty-eight immigration courts in the United States, where they hear evidence and determine the credibility of each application for relief. IJs are separate from the federal judiciary and are employees of EOIR.

Immigration courts are administrative bodies that are trial-like in nature. For many immigrants, immigration courts are the first and last opportunity that they have to present evidence in support of their case. Additionally, the highly deferential standard provided to immigration courts on appeal heightens the need for an effective and complete development of the factual record in immigration courts in support of any claims for relief. Immigration judges work with small staffs, and their staff and court hours are set exogenously. The size of each immigration court varies as well: in 2015, for example, Fishkill, New York had one full-time employee while the largest court, Los Angeles, California, had approximately 85 full-time employees.

Immigrants enter the immigration court system when they receive a Notice to Appear (NTA), a charging document issued by DHS alleging a violation of U.S. immigration law. DHS files this NTA with the closest immigration court to the respondent's location, unless it believes another location is more appropriate. When an immigrant receives an NTA, he is summoned to an immigration court to appear before an immigration judge in removal proceedings. While these removal proceedings are pending, Immigrations and Customs Enforcement (ICE) may detain respondents, or respondents may be released on bond on conditional parole.[30]

Court administrators randomly assign cases in immigration proceedings to judges, without regard to the underlying merit of each case (Ramji-Nogales et al. 2007). Usually an immigration hearing has many parts, and immigration judges have broad discretion to determine how long a particular case will last and how many hearings are necessary to rule on the individual's claim. The first, called a master calendar hearing, allows the IJ to ensure that the respondent understands the charges brought against him and gives him the opportunity to concede removability. If the respondent's removability is still in question, the IJ sets the schedule for future merit hearings until a decision is made on the individual's removability. At this merits hearing, the IJ hears testimony and reviews evidence regarding the individual's removability and any claims for relief on behalf of the respondent. Based on the record, the IJ then must determine whether the respondent is removable, or whether any of the respondent's claims for relief are meritorious. Judges issue oral or written decisions at the conclusion of these immigration court proceedings. If the individual is deemed removable by the judge, the individual is subject to removal from the United States per the judge's order. The respondent then may appeal this judgment to the Board of Immigration Appeals (BIA), which is also under the purview of EOIR.

Immigration judges hear a mixture of adult and juvenile proceedings, as there is no special juvenile immigration court. Juvenile proceedings continue just as adult

[30] The Immigration and Nationality Act requires DHS to detain certain aliens such as those deemed inadmissible for certain criminal convictions or terrorist activity. *See* 8 U.S.C. 1226(c).

proceedings do in immigration courts, except judges are instructed to pay special attention to unrepresented juveniles in immigration proceedings. In juvenile cases, immigration judges are encouraged to provide a list of pro bono services to the juvenile and are allowed to make reasonable accommodations for juvenile immigrants.[31] Further, for juveniles without representation, judges are asked to consider making a brief opening statement at the beginning of each proceeding to explain the purpose and the nature of proceedings and to introduce the parties and discuss each person's role in the proceeding (EOIR 2007). Other than these slight procedural modifications, there is no marked difference between adult and juvenile proceedings in immigration courts.

Immigration judges workloads are massive, and they are acutely aware of the consequences of their work. Judge Dana Leigh Marks, President of the National Association of Immigration Judges, said that immigration judges are "doing death penalty cases in a traffic court setting," as the judges are "working at light speed, and yet the stakes for the people who are before the courts can be a risk to their very life" (Yang 2017). Studies on immigration court judge decision making have found disparities in judge asylum grant rates both across and within individual immigration courts (Ramji-Nogales et al. 2007; Eagly and Shafer 2015) but most have attributed these disparities to individual judge characteristics, rather than the underlying backlog pressures facing these courts.

Immigration judges are aware of backlog pressure and make necessary tradeoffs to adequately manage this backlog. Yet, immigration judges, by the very nature of their posts, are required to act on each case regardless of the pile of cases that they see before

[31] Reasonable modifications including allowing juveniles to bring pillows or toys, sit with an adult companion, or permitting juveniles to testify next to a trusted adult or friend (EOIR 2007).

them. And—unlike in civil courts— backlog in immigration courts does little to encourage settlements outside of court as immigrant cases are, for the most part, handled in the immigration court system itself.[32]

Figure 1 shows the immigration court backlog through the end of the 2017 fiscal year. The number of pending cases steadily increased year over year from 2008 until 2017. The backlog in immigration courts reached historic heights in 2017, when immigration courts had more than 629,000 cases pending. Yet, this backlog is not homogeneous among immigration courts, as cases within each immigration court originate in the place of immigration charge and are rarely transferred. In Arlington, Virginia—highly regarded as one of the nation's most expedient immigration court—eight judges share responsibility for 30,000 cases, with some scheduling hearings more than seven years into the future. Smaller immigration courts shoulder a limited portion of the backlog: Oakdale, Louisiana had just 151 cases pending in 2017.

As President Obama increased immigration enforcement during his presidency, Congress tightened spending that resulted in a hiring freeze from 2011 to 2014 of immigration judges. Figure 2 shows the number of immigration judges hearing cases in immigration courts from 2003 until 2013. The number of immigration judges fluctuated slightly over this decade, but did not grow by a signification amount: the immigration judiciary has only expanded from a low of 207 immigration judges in 2007 to a high of 239 judges in 2011. The immigration judiciary continues to face staffing issues: 40

[32] Juvenile immigrants do have several types of available immigration relief that they can claim through the immigration system, including asylum, special immigrant juvenile status, U-visas, and T-visas. However, other than asylum, these types of relief must be resolved outside of the immigration court system. The numbers of individuals receiving this type of relief is also minute in comparison to the number of individuals presenting their case in immigration courts as Congress sets caps for these special visas. For example, no more than 6,000 SIJS visas were approved each year by Congress in 2011–2015.

percent of immigration judges nationwide in 2016 were eligible for retirement. This shortage in staffing persists even though Congress has allocated funds for the immigration court system to hire new judges. Recently, the Washington Post reported that the Department of Justice is "aiming to slash the massive immigration court backlog in half by 2020" through the addition of judges, technology, and refusing to tolerate repeated delays in deportation cases (Sacchetti 2017). However, it is unlikely that relief in the form of new judges will arrive quickly, as the GAO found that it took an average of 742 days to hire new immigration judges from 2011 through August 2016 (GAO 2017). Figure 3 shows the mounting backlog for each immigration judge on average. Over my sample period, the average backlog of each immigration judge nearly doubled, from 777 cases per judge each year in 2003 to 1516 cases per judge in 2013. To compare to Article III courts during this time period, United States District Court judges heard a high of only 546 cases per judge in 2013. The structure of immigration courts and the volume of cases that they incur distinguish these courts from U.S. federal courts. This chapter examines whether immigration courts' response to case backlog is also unique.

III. Conceptual Framework

The impact of case backlog on immigration court output in immigration courts remains both theoretically and empirically ambiguous. Studies on civil court activity find that increased backlog can either increase court output (Luskin and Luskin 1986; Rosales-Lopez 2008; Dimitrova-Grajzl 2012) by incentivizing judges to avoid backlog in light of reputational concerns or a distaste for backlog, or decrease output (Buscaglia and Ulen 1997; Murrell 2001; Mitsopoulos and Pelagadis 2007) by congesting courtrooms and court resources. In the immigration court context, it is unclear whether the incentivization or congestion effect will be dominant. Immigration judges may be motivated by reputational concerns and a distaste for backlog, or increased backlog could congest the limited resources that are currently allocated to immigration courts.

The model of judicial decision making of Cooter (1983) and Posner (1993) assumes that judges are subject to the basics of utility theory: judges are rational and, just like everyone else, maximize their utility. Judges derive utility from leisure, minimizing effort required to arrive at a decision in each case. Beenstock and Haitovsky (2004) add that judges derive utility from disposing of their caseload. Large backloads bring judges disutility, as large backlogs can provide a signal to other judges and superiors of low effort or underperformance. Reputational concerns, such as possibilities for promotion or recognition, encourage judges to minimize backlog. Notably, in U.S. Federal District courts, a Civil Justice Reform Act Report is released semiannually, which tallies all motions pending more than six months and civil cases pending for more than three years before each judge. These reports signal the manner in which each individual judge handles her court. There is no equivalent reporting mechanism in immigration courts, as cases pending are reported on the court-level only.

Beenstock and Haitovsky (2004) find support for an incentivization effect in courts. They hypothesize that when an individual judge's backlog increases, case completions will also increase. In the trial-level immigration court context, a quicker disposal of cases could result from judges ruling on the merits of the case earlier in the trial period or finding that the prosecution did not meet its burden in proving that the alien was removable and closing the case. This case completion increase, ceteris paribus, will lead to shorter case durations, especially in the immigration court context, where there is a large inflow of cases to each judge and immigration court. In the immigration court context, if judges respond to an incentivization effect, they will speed their decisions by ruling quickly on cases rather than waiting to schedule additional hearings. Immigration judges may also be less motivated to diminish their backlog for reputational or prestige reasons because, unlike in the Article III court system, there are fewer direct routes to promotion in the immigration court system, and there exists little recordkeeping on individual judge statistics.

In immigration courts, there is also a possibility that a congestion effect will also be present. This congestion effect is caused by an overwhelming number of cases into a court system, causing the functions of the court to diminish and judicial efficiency to decrease (Buscaglia and Ulen 1997; Murrell 2001; Mitsopoulos and Pelagadis 2007). A congestion effect may also be due to an expenditure of resources in the court system that causes more individuals to file cases: if backlog pressure causes judges to speed up their time to case resolution, more litigants may file their claims in the system, creating a supply-side congestion effect (Priest 1989; Buscaglia and Ulen 1997). Finally, a congestion effect may be present in immigration courts if, as Jonski and Mankowski (2014) find, judges feel less pressure as backlog decreases and expand the time that they take to complete cases.

There also exists unobserved immigration court-level heterogeneity that may be correlated with the caseload of a court. As individuals are sent to the immigration court nearest to the area in which they were given a notice to appear, the caseload of a given court will depend on factors including the size of the undocumented immigrant population in a given area.[33] Additionally, there may be reverse causality from case duration and judicial staffing: the Department of Justice could hire judges to help reduce the strain on the immigration court system. I address these potential problems in Section V.

The effect of backlog on grant rates for relief from removal in immigration proceedings is also ambiguous. First, it is possible that judicial decision making is not affected by case backlog at all. Yet, recent empirical literature on judicial decision making suggests that time pressures can affect the eventual outcome of a case before a judge (Huang 2011; Dimitrova-Grajzl et al. 2014, 2015; Chen et al. 2016; Norris 2018). If immigration judges are rational actors and make trade-offs taking time and caseload pressure into account, backlog should affect at least the marginal cases in their court.

Successful immigration relief applications often include detailed evidence and testimony, and speeding up the process could impinge necessary evidence from becoming a part of the record. Given this need for more detailed evidence and testimony, successful applications often take longer to hear. Therefore, in the context of significant time pressure, it may be less costly for a judge to reject an individual's case as it requires less time in the courtroom, and often, less time for the judge and their staff to craft an opinion. Further, psychology literature found that individuals gave negative evidence significantly more weight in decision making when making decisions that were subject to time pressure (Wright 1974). To the extent that immigration judges are subject to these

[33] Immigration courts are an ideal court to study in this context as little forum shopping, or selection of the court by the litigant, takes place in the immigration court system. As individuals are sent to the closest immigration court to where their NTA was issued, individuals cannot select the court that resolves cases more expeditiously. In court systems where forum shopping can occur, the demand for court services will be endogenous to case duration (Murrell 2001).

cognitive biases, these judges may be more likely to rule against a case if they are under time pressure.

However, if backlog increases, this increase could also lead to an increase in successful applications. Rather than being rejected relief outright, individuals with long case durations have the ability to amass more evidence and witnesses to testify on their behalf over the delay. Often immigration cases turn on evidence regarding country conditions, and these delays could lead to favorable changes in country conditions. Further, as individuals are awaiting their time in immigration court, they could also increase their human and social capital, another factor that immigration court judges often take into account when determining whether individuals may remain in the United States. These positive effects would lead to more successful applications for relief given an increase in case backlog. Successful immigration relief applications also rely on other factors, such as attorney skill, strength of claim, and individual immigration judge leniency (Ramji-Nogales et. al 2007, 2010; Eagly and Shafer 2015). While many of these factors are unobservable, I use individual case characteristics along with court- and year-fixed effects to control for the underlying heterogeneity in case outcomes.

IV. Data

To examine the impact of backlog on judicial decision making in immigration courts, I use a proprietary dataset that contains over five million juvenile immigration removal cases decided between 1950 and October 2016. The Transaction Records Access Clearinghouse (TRAC) obtained this dataset pursuant to a FOIA request from the Executive Office for Immigration Review (EOIR), the division of the Department of

Justice that retains jurisdiction over immigration court proceedings.[34] I am able to access this data through my appointment as a TRAC Fellow. The names of the respondents, as well as any other personal identifiers, are removed from DHS records before the information is released to TRAC.

The TRAC dataset is ideal for my analysis not only because it captures all juvenile cases decided in the immigration court system, but also because it is rich in information that is otherwise unavailable elsewhere, including court outcomes and demographic characteristics of respondents. EOIR employees extracted this data from EOIR's CASE database system, which tracks the workloads of immigration courts. One limitation of this dataset is that it was recorded purely for EOIR internal recordkeeping and was not intended for individuals to engage with it for data analysis. Therefore, EOIR does not impute values for certain variables and there are missing values for many variables in the data. Throughout my analysis, I create indicator variables for missing values for each variable that I use. There also may be a rate of error in the reporting by EOIR, as these data were used for in-house reporting only. As long as the measurement error is random for a given variable, my estimates will only suffer from attenuation bias and will be biased towards zero. If this bias occurs, my coefficients will be conservative estimates of the true effect. I address specific issues with measurement error for key variables in my analysis, such as with case duration, below.[35]

[34] TRAC is a data gathering and research nonprofit organization that operates out of Syracuse University.
[35] The TRAC data is compiled into many different large files with different information regarding charge data, individual judges, attorneys, and hearing schedules. I merge these datasets across individual case numbers (labeled idncase in the files). Each individual has one case number, even if they are associated with the same family. In each case, each different proceeding also has its own proceeding identification. However, I merge on case numbers as a whole, and assign any judge information and attorney information to the entire case.

The TRAC data contain case-level data regarding demographic characteristics of the individual and identifies the judge, attorney status, and the court in which the decision took place. The data contain demographic characteristics such as whether the individual is detained, crimes charged against the individual, and the individual's country of origin.[36] The data also contain case-level detail such as the type of case, number of grounds for removal charged, number of applications for relief filed, and whether the individual was present during the hearing when the decision was rendered.

There are a variety of different types of cases that arrive before an immigration judge, including removal cases, asylum cases, and withholding of removal cases. Over 98 percent of cases in the TRAC data from 2003 until 2013 are removal cases. As the merits and procedures of different case types may result in heterogeneous effects on case duration and the likelihood of relief, I drop cases other than removal cases from my sample. In removal cases, I code a decision as a denial of relief if the individual was denied any relief sought or the judge otherwise rules that an individual is subject to removal by authorities. In my sample, denial includes voluntary departure orders, where an immigration court judge sustains the charges against the individual and issues an order for voluntary departure.

I code all grants of relief and termination of proceedings as relief granted. These outcomes include actual grants of relief, in which the immigration judge finds that the individual is entitled to relief from removal and may remain in the United States. There are exist a variety of circumstances in which a case may be closed. These include cases in which the government attorney prosecuting the individual's case exercises prosecutorial

[36] The data also has fields to record information regarding the language spoken during the trial, gender, and birthdate. However these fields are rarely recorded in the data.

discretion and drops the request for a removal order, or when the immigration judge terminates proceedings after finding that DHS has not established that the individual is removable. Under these closures, the individual is permitted to stay in the United States. There are a few types of resolutions to specific cases that do not fall squarely in the relief granted or denied dichotomy. An example of this is when the case is transferred to a different court. I drop these cases from my sample. I remove from my sample cases in which the final decision is missing.

I create an indicator variable, in absentia, indicating whether the individual was present at the hearing in which the decision was rendered. The data also contain indicators for whether the individual is detained at the time of hearing, was detained but released from custody, or was never detained since court proceedings began. I create an indicator variable equal to one if an individual was ever detained. The TRAC data also contain a variable that tracks whether an attorney was present in the case at any time. I use this to create an indicator variable for attorney presence in a case. The TRAC data also include whether the individual was charged with a crime other than being illegally present in the United States. I create an indicator variable indicated that the individual committed a crime.

Finally, the data contain the date when the case was opened and the date of the final decision. I use these dates to calculate the duration of each individual's case in immigration court by counting the days elapsed from the date of the case opening to the date of the final decision.[37] This information at the individual case level provides a more accurate measure of individual case duration across courts than previous studies of case

[37] I find that for 56 individuals (less than one-hundredth of one percent of my sample), the TRAC data reports a negative case duration. This could be due to an incorrect recording of the date. I remove these individuals from my sample.

duration, which relied on the ratio of cases that are postponed or remain unresolved at the end of the year to the total number of cases introduced during the year (Mitsopoulos and Pelagadis 2007).

To obtain data on the number of cases pending, I use data from the TRAC Immigration supplements available online.[38] These supplements include the number of cases pending at the conclusion of each fiscal year (September 30) at each immigration court. I restrict the case-level data from 2003 until the end of 2013. Restricting my sample to these dates avoids the confounding effects from one of the largest immigration reform acts in history, the Immigration Reform and Immigrant Responsibility Act of 1996 and potential short-term effects of 9/11. Further, limiting cases to only those before 2014 avoids potential confounding effects of both a large hiring of immigration judges and a surge of immigrant children to the borders of the United States in the summer of 2014.

To obtain an exogenous measure of backlog pressure, I construct a measure of the percentage change in adult immigration cases pending. I use the total amount of cases pending and subtract the total number of juvenile cases resolved in that year to arrive at the total number of adult cases pending. To obtain the percentage change in adult immigration cases pending, I subtract the number of cases pending last year in a given court from the cases pending this year and divide this number by the number of cases pending last year. Using the percentage change in adult cases allows for an exogenous measure of backlog pressure that is not tied to the individual juvenile case at issue. Using the percentage change in total cases or juvenile cases only would be endogenous, as

[38] These are available at TRAC's Immigration Backlog tool, available at http://trac.syr.edu/phptools/immigration/court_backlog/.

longer juvenile case duration could cause an increase in juvenile case backlog. Using the percentage change in adult cases pending only avoids this endogeneity.

Table 1 shows summary statistics for the entire case sample. I find, nationwide, and over the entire sample period, that there are an average of 8,533 cases pending in the court that the individual case is located. The average case duration from opening to completion is just under on year, or 364.5 days. Less than half (35 percent) of individuals have an attorney at any point during their immigration court case. One-fifth of individuals are in absentia, or fail to show to a court date when a decision is rendered. 21.8 percent of individuals in my sample have successful applications for relief to remain in the United States, and the remainder (78.2 percent) of individuals are denied relief and are subsequently deemed removable from the United States. 16 percent of sampled individuals have committed a crime other than being present illegally in the United States, yet almost 60 percent of individuals have been detained at one point during their case.

Table 2 shows the summary statistics over time. Consistent with other evidence regarding immigration court backlog, the average number of cases pending across cases in my sample increased sharply from 2003–2010 levels, from under 8,000 cases to over 11,000 cases in 2011–2013. As this increase occurred, the average case duration increased slightly over time, from 375 days in 2003–2010 to 441 days in 2011–2013. The number of individuals with attorneys also increased over time, with a low of 32 percent of individuals represented in 2003–2006 to a high of 47 percent of individuals represented in 2011–2013. My sample also depicts evidence of increased immigration enforcement: the number of individuals detained has steadily increased from 43 percent in 2003–2006 to 72 percent in 2011–2013. Overall, the changes that I observe in my data over time comport with anecdotal evidence of increased backlogs and increased immigration enforcement. I next determine how these changes over time have affected case duration in immigration courts.

Table 3 depicts summary statistics by attorney presence. Attorneys in immigration court cases are likely better equipped to present specific evidence in an individual's case, or could strategically delay the individual's case. Attorneys may also be hired in cases that are substantially more complicated than others. Therefore, it is unsurprising that individuals with attorneys have a substantially longer case duration, with a case duration of over 721 days as compared to 164 days for those without an attorney. In the immigration court context, individuals may decide to hire an attorney only if they anticipate that they cannot adequately represent their own claims to an immigration court judge. Individuals with attorneys are also more likely to obtain relief from removal: over half of all individuals with attorneys are granted relief in immigration courts, while only 5 percent of pro se individuals are granted relief. I examine the effects of the presence of an attorney in my analysis of both case duration and the probability of a successful case.

V. Examining the Effects of Case Backlog on Case Duration

I first examine whether each court's backlog is associated with an effect on case duration. To examine this, I use a fixed effects model, adapted from Dimitrova-Grajzl et al. (2012) to include court and time fixed effects:

$$\ln(d_{ikt}) = \propto + \beta_1 C_{kt} + G_k + Y_t + \mathrm{X}_i\, \lambda + \mathrm{Z}_i \gamma + \epsilon_{ikt} \qquad (1)$$

where k= 1, 2 . . . ., N identifies the court, t= 1, 2, ..., T identifies the time period, and i= 1, 2 . . . ., N identifies the individual. The dependent variable, d, indicates case duration, measured in days, and C indicates the percentage change in the number of adult cases pending in each court from the prior year to the current year. X_i represents a vector of individual demographic characteristics including detained status (if the individual was ever detained or has never been detained), if the individual was convicted of a crime other than residing in the United States illegally, and country of origin. Z_i represents a vector of case-level characteristics including attorney presence, in absentia status, and case outcome. The model above includes a set of court fixed effects G_k and year fixed effects Y_t. Including these effects controls for unobserved heterogeneity in courts over each year, including the number of judges appointed. Murrell (2001) shows that estimators derived from models studying court performance without court and year fixed effects can be biased and recommends including, at the minimum, court fixed effects. I include robust standard errors clustered at the court level.

Table 4 shows the result of Equation 2. I find that backlog is positively associated with case duration, as an increase of 1 percent in case backlog from the prior year results in roughly a 4 percent increase in case duration without controlling for whether the individual has an attorney or the ultimate outcome of the individual's case. This result signals that a congestion effect is occurring in U.S. immigration courts, as case duration is positively related to the increase in the number of cases pending.

I find that controlling for whether an individual has an attorney reduces the point estimate on case duration to 3.7 percent. Attorneys have a large and positive effect on case duration, as attorneys in immigration courts will likely be more knowledgeable with regards to immigration law than a pro se client. Attorneys have an ability to achieve advantageous delays in an immigration case, and can present more evidence that naturally makes a case duration longer. Literature on judicial decision making in immigration courts (Eagly and Shafer 2015; Ryo 2016) has noted the correlation between attorney representation and increased case duration. Eagly and Shafer (2015) attribute this correlation to the fact that attorneys often enter into cases in immigration courts later, as individuals often seek continuances to obtain counsel. Additionally, attorneys are far more represented in cases with claims for relief (Eagly and Shafer 2015). These claims require time to file the application with the immigration court and time to schedule a separate hearing on the merits of the application for relief. My results are congruent with these explanations regarding length of time and representation status.

I find my results are also robust to the inclusion of case outcome, as the inclusion of case resolution does not shift the magnitude of my main coefficient of interest. I find that relative to successful cases, denials of relief are negatively correlated with case duration. It is intuitive that this point estimate on denial of relief should be negative and large, as individuals in immigration court with denials of relief may not plead any form of relief. Therefore, it is likely that an immigration court judge will order them removed with a need for less evidence and less time. Across all three specifications, I find that the coefficient for individuals who have committed a crime have a positive correlation with case duration, while the coefficient on an individual who has been detained is correlated negatively with case duration.

Table 5 shows the results of Equation 1 by attorney status. I find that case durations are, for the most part, impacted in a similar way that is not tied to whether the individual has an attorney. However, the magnitude of the effect of case backlog on case duration is nearly doubled when considering an individual who does not have an attorney compared with one who does: case backlog is associated with a 2.3 percent increase in case duration with an attorney and just less than a 5 percent increase without an attorney.

To determine whether my results are driven by heterogeneous differences in my sample, I separate my sample into three different time periods. Table 6 shows the results of Equation 1 by time period. The coefficient of interest on the percentage change in cases pending switches signs over time, indicating a potential for a heterogeneous effect of case backlog on case duration. From 2003 until 2006, a one percent increase in the number of cases pending is associated with a 7 percent decrease in case duration. However, between 2007 and 2010, this effect becomes positive, with a one percent increase in the number of cases pending associated with a 1.6 percent increase in case duration. In 2011 through 2013, a one percent increase in the number of cases pending is associated with an 11.06 percent increase in case duration. These results could signify that the congestion effect increases in immigration courts as only as backlog becomes sufficiently large that it cripples the court's resources, and suggests that the relationship between case duration and backlog is non-linear. These results comport with the findings of Jonski and Mankowsi (2017) that a 'hockey-stick' production model captures the response of judges to increasing caseload: namely, that the judicial response to growing caseload differs between courts operating below and over their capacity (Gillespie 1976).

There is potential that my results are driven not by congestion, but by other factors that I cannot observe in the TRAC data including factors that are often left unrecorded by DHS, such as language at trial and the underlying merit of a case. The omission of these factors in my estimating equation would result in omitted variable bias, which would bias my results towards zero. By using court-level fixed effects along with court-by-year and year fixed effects, I capture underlying heterogeneity in each court-year so that my results are not biased due to differences among courts. It is also unlikely that my specification is subject to concerns of reverse causality. It is unlikely that immigration courts caused the increase in inflows of cases, as these increases were likely due to increased immigration enforcement without a similar increase in staff that could handle such increased enforcement.

My results do not examine the underlying quality of decision in immigration courts, but past literature has indicated a positive relationship between efficiency and quality of decisions in courts (Djankov et al. 2001, Buscaglia 2001). To examine the potential relationship between the judicial decision making process and case backlog, I next examine case outcomes in immigration courts.

VI. Examining Case Outcomes

When examining case outcomes, it is difficult to isolate the influence of docket pressure from other factors, namely the changes in the quality of cases brought before immigration courts. Huang (2011) notes that reversal rates in the appellate cases will naturally fall as caseloads rise if certain classes of weaker appeals are growing at faster rates than the rest of the docket. Similarly, if an increase in backlog ushers in less meritorious claims, it will be difficult to disentangle whether an increase in denials of relief is due to the increased backlog or due to the degradation of the average case's underlying merit.

While it is impossible to know the underlying merit of each case in my sample, Table 7 shows the demographics of cases during my sample period remain similar, though the share of countries has changed over time. Notably, the region of birth for individuals in my sample has shifted over time to include more individuals from Mexico and fewer individuals from the Caribbean, South America, and the Middle East. There exists roughly the same share of individuals from Europe, Asia, Central America, Africa, Oceania, and Canada. As immigration claims often turn on specific country conditions, the similarities between the demographics of individuals in distinct time periods in my sample may signal a homogenous distribution of case merit over time. However, to the extent that the distribution of underlying merit of each case in immigration courts has changed, I use court, year, and court-by-year fixed effects to control for the underlying heterogeneity over time.

To determine the effects of backlog on case outcomes, I use the following probit equation:

$$\Pr(S) = \propto + \beta_1 C_{kt} + G_k + Y_t + \mathrm{X}_i\,\lambda + \mathrm{Z}_i\gamma + \epsilon_{ikt} \tag{2}$$

where k= 1, 2 . . . ., N identifies the court, t= 1, 2, ..., T identifies the time period, and i= 1, 2 . . . ., N identifies the individual. The dependent variable, S, indicates success in an immigration court case and equals 1 if there is a grant of relief or termination of proceedings. C indicates the percentage change in the number of adult cases pending in each court from the prior year until the current year. X_i represents a vector of individual demographic characteristics such as detained status (if the individual was ever detained or has never been detained) and country of origin. Z_i represents a vector of case-level characteristics including attorney status and in absentia status. I also include court fixed effects G_k and year fixed effects Y_t.

Table 8 shows an increase in the percent change in cases pending also increases the probability of success for individuals in immigration courts when controlling for demographic and attorney effects. This effect could be attributed to the ability to develop the factual record in immigration cases that a longer case duration provides, which would lead to a greater chance of success for individuals. I also find that detained and in absentia individuals are less likely to obtain relief in immigration court cases, as are individuals who have committed a crime other than residing in the United States illegally. Table 9 shows that the effects of backlog on the probability of success hold when bifurcating my sample by attorney status. An increase in the percentage change in adult cases pending in an immigration court leads to an increase in the probability of success for juvenile individuals both with and without an attorney.

Table 10 shows the results of Equation 2 over time. I find, like case duration, there is significant heterogeneity over time when I separate my sample into three distinct time periods. I find that from 2003–2006, a one percent increase in the percentage change of adult cases pending is associated with a decrease in the probability of success for a given individual's case. This effect changes signs in 2006–2009, when a one percent increase in the in the percentage change of adult cases pending is associated with a slight percent increase in the probability of success for a given individual. This magnitude increases significantly in 2011–2013, when the same change is associated with a large increase in the probability of success for a given individual.

These results are consistent with my findings on the effects of case backlog on case duration. As I find that backlog increases case duration, individuals may have more time in immigration courts to present their cases and provide evidence that merits them a grant of relief from removal. Yet, I find that the relationship between backlog and the likelihood of success may also be non-linear, as the probability of success only dramatically increases when the overall backlog in the time period becomes sufficiently large. Ultimately, these results present a consistent story of the effects of backlog in the immigration court system.

VII. Conclusion

This chapter provides an empirical analysis of the effects of case backlog on the immigration court system and finds that a congestion effect is prevalent in immigration courts, as an increase in case backlog leads to roughly 3 percent increase in case duration days overall. However, when segmenting my sample by years, I find that this congestion effect is most prevalent in the later years in my sample when overall case backlog is the highest. My results suggest that immigration courts are ill prepared to handle the increase in cases that are flooding their courtrooms. However, I find that for respondents in immigration courts this congestion could be good news—an increase in backlog is associated with an increase in the likelihood of success in immigration courts. This chapter contributes to existing literature by providing an analysis of the effects of backlog on case duration and case outcome in U.S. immigration courts. This chapter ultimately suggests that while a congestion effect is dominant, there is evidence that a non-linear relationship between backlog and case-level outcomes exists.

My findings implicate economic concerns, as the delay in immigration courts is costly. The United States spends nearly $2 billion on immigrant detention each year, and this number has been steadily rising over the past decade. The ever-increasing wait times in immigration courts have forced circuit courts to debate the constitutionality of these extended detention periods. The results of my study implicate a recent circuit split regarding when detained aliens are allowed a bond hearing while their cases are pending in immigration courts. Section 1226(c) of the Immigration and Nationality Act (INA) allows for the mandatory detention of criminal aliens for a “reasonable amount of time” to complete the removal proceedings. Circuits are split as to what constitutes a “reasonable amount of time” amidst case backlogs that are taking years longer than anticipated. Many circuits have held that this delay is a violation of individual due process rights. My results show that this delay—and the concern of underlying justice that circuits are grappling with—is unlikely to be ameliorated by increased caseload pressure.

The impact of caseload pressure on successful grants of relief also has implications for the immigration court system’s deliverance of justice. If backlog pressure has an effect on the length of an individual’s case or—more critically—how the judge rules in the case, it is plausible that the same case would result in a different manner in a different court. As some immigration courts are now scheduling hearings that will not take place for many years, this examination comes at a critical time to determine how immigration judges are adjusting to this mounting strain and the effects that it has on delay and justice in the immigration court system.

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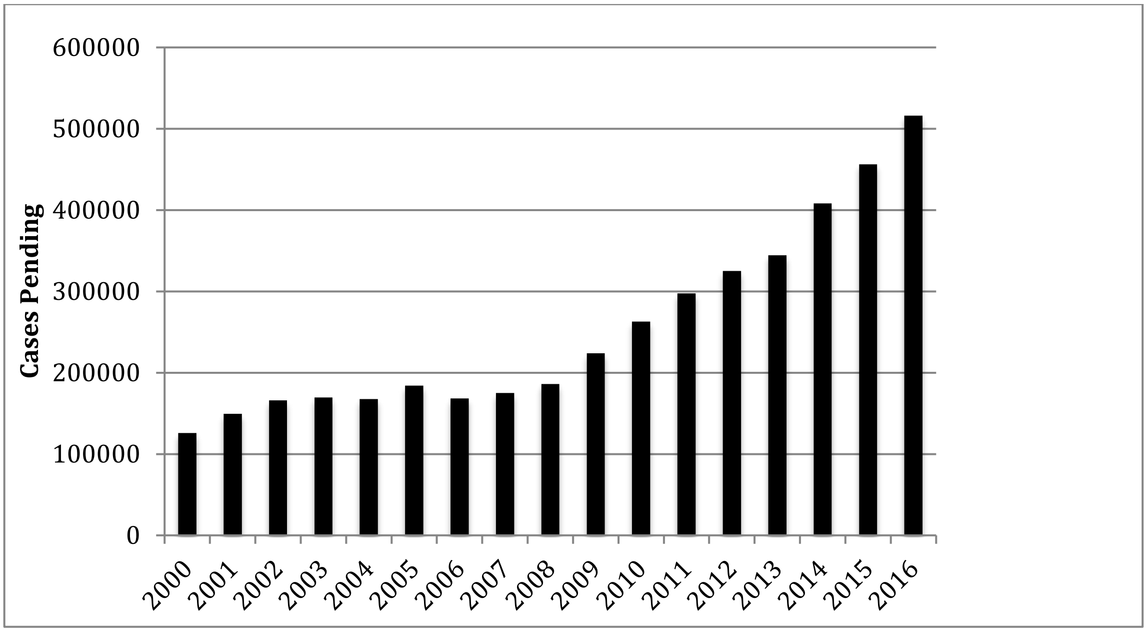
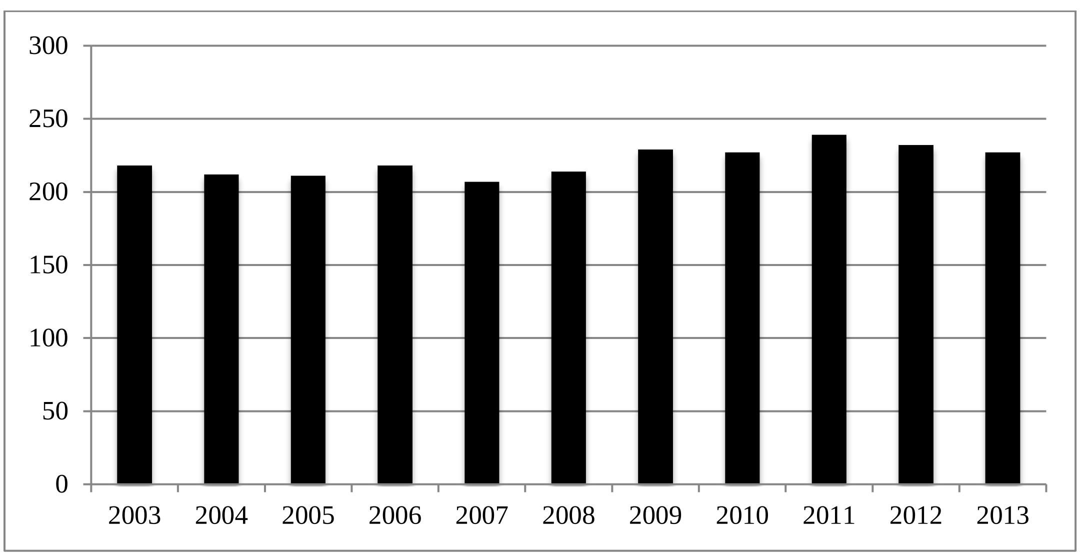
FIGURES AND TABLES

Figure 1: Backlog in U.S. Immigration Courts, 2000–2016



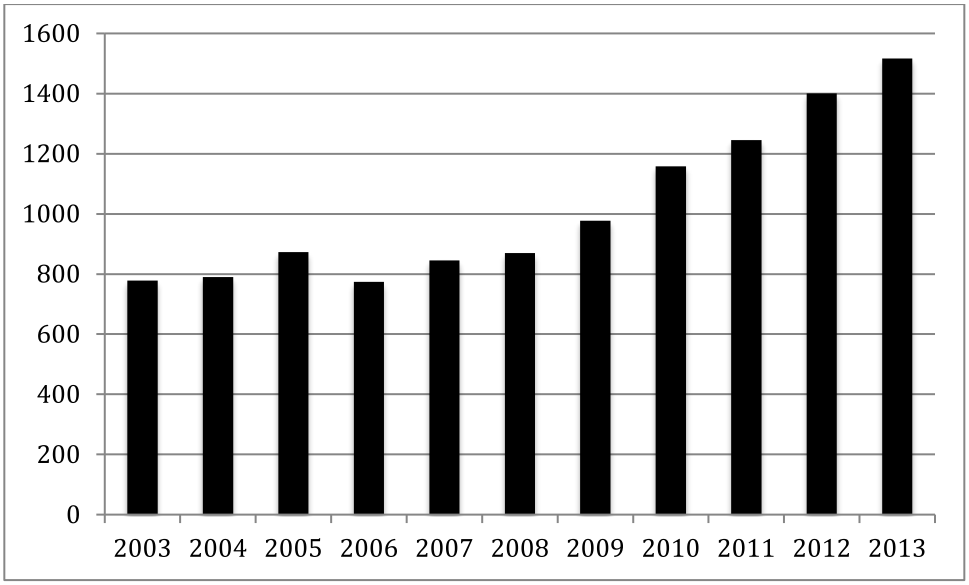
Source: Transactional Records Access Clearinghouse (TRAC) Immigration Backlog Tool.

Figure 2: Number of Immigration Judges Hearing Cases in U.S. Immigration Courts



Source: Transactional Records Access Clearinghouse (TRAC).

Figure 3: Number of Cases Per Judge, 2003–2013



Source: Transactional Records Access Clearinghouse (TRAC).

Table 1: Summary Statistics, All Cases 2003–2013

	Mean	Standard Deviation
Cases Pending	8533.21	11402.63
Case Duration	364.51	539.58
Attorney	35.92	0.48
No Attorney	64.08	0.48
In Absentia	20.56	0.35
Relief Granted	21.82	0.41
Relief Denied	78.12	0.41
Committed Crime	16.36	0.37
Ever Detained	58.78	0.49
Never Detained	41.22	0.49
N	2,284,432	

Note: Data shown is comprised of the DHS records of all juvenile cases in immigration courts during the years 2003–2013. The unit of observation is at the case-level. Data was compiled by the Transactional Records Access Clearinghouse (TRAC) through a Freedom of Information Act (FOIA) and was made available to me through my appointment as a TRAC Fellow. Data on cases pending in each immigration court was provided by TRAC's immigration tool. "Cases Pending" reflects the number of cases pending in each immigration court. "Case Duration" is measured by days elapsed between the individual's Notice to Appear and the conclusion of their case before an immigration judge. Individuals are recognized as "In Absentia" if they are not present at the hearing in which an immigration judge announces the conclusion of their case. "Committed Crime" reflects whether the individual was accused of committing a crime other than entering the United States illegally. All variables except "Cases Pending" and "Case Duration" are expressed as a percentage of the total sample.

Table 2: Summary Statistics, by Year

	2003–2006	2007–2010	2011–2013
Cases Pending	7914.12	7586.85	11090.13
Case Duration	375.58	364.48	441.36
Attorney	32.54	37.12	37.12
No Attorney	67.46	63.88	61.68
In Absentia	13.71	13.97	16.03
Relief Granted	18.88	23.03	23.50
Relief Denied	81.12	76.97	76.50
Committed Crime	15.52	16.59	17.15
Ever Detained	43.35	63.25	72.43
Never Detained	56.64	36.75	27.57
N	717,376	1,125,804	657,732

Note: Data shown is comprised of the DHS records of all juvenile cases in immigration courts during the years 2003–2013. Data was compiled by the Transactional Records Access Clearinghouse (TRAC) through a Freedom of Information Act (FOIA) and was made available to me through my appointment as a TRAC Fellow. Data on cases pending in each immigration court was provided by TRAC's immigration tool. "Case Duration" is measured by days elapsed between the individual's Notice to Appear and the conclusion of their case before an immigration judge. Individuals are recognized as "In Absentia" if they are not present at the hearing in which an immigration judge announces the conclusion of their case. "Committed Crime" reflects whether the individual was accused of committing a crime other than entering the United States illegally. All variables except "Cases Pending" and "Case Duration" are expressed as a percentage of the total sample.

Table 3: Summary Statistics by Attorney Presence

Variable	Attorney	No Attorney
Cases Pending	12806.34	6138.20
Case Duration	721.73	164.30
In Absentia	5.48	29.02
Relief Granted	51.76	5.03
Relief Denied	48.24	94.97
Committed Crime	15.34	16.93
Ever Detained	40.88	68.80
Never Detained	59.12	31.20
N	820,489	1,463,943

Note: Data shown is comprised of the DHS records of all juvenile cases in immigration courts during the years 2003–2013. Data was compiled by the Transactional Records Access Clearinghouse (TRAC) through a Freedom of Information Act (FOIA) and was made available to me through my appointment as a TRAC Fellow. Data on cases pending in each immigration court was provided by TRAC's immigration tool. "Case Duration" is measured by days elapsed between the individual's Notice to Appear and the conclusion of their case before an immigration judge. Individuals are recognized as "In Absentia" if they are not present at the hearing in which an immigration judge announces the conclusion of their case. "Committed Crime" reflects whether the individual was accused of committing a crime other than entering the United States illegally. All variables except "Cases Pending" and "Case Duration" are expressed as a percentage of the total sample.

Table 4: Fixed Effects Estimates, Case Duration

	ln(caseduration)	ln(caseduration)	ln(caseduration)
Percentage Change in Cases Pending × 100	0.0430***	0.0379***	0.0377***
	(0.0002)	(0.0001)	(0.0001)
Attorney		1.4947***	1.3396***
		(0.0020)	(0.0021)
In Absentia	-0.2962***	0.5051***	0.7010***
	(0.0026)	(0.0026)	(0.0026)
Relief Denied			-0.6251***
			(0.0024)
Crime Committed	0.1457***	0.1654***	0.1796***
	(0.0024)	(0.0022)	(0.0022)
Ever Detained	-1.1441***	-0.8209***	-0.6846***
	(0.0024)	(0.0022)	(0.0022)
Constant	5.8884***	4.8259***	5.2283***
	(0.0058)	(0.0054)	(0.0056)
Observations	2,284,432	2,284,432	2,284,432
R-squared	0.4479	0.5565	0.5694

***p<0.01; **p<0.05; *p<0.10. Dependent variable is the natural log of case duration. Data shown is comprised of the DHS records of all juvenile cases in immigration court. Data was compiled by the Transactional Records Access Clearinghouse (TRAC) through a Freedom of Information Act (FOIA) and was made available to me through my appointment as a TRAC Fellow. Data on cases pending in each immigration court was provided by TRAC's immigration tool. "Case Duration" is measured by days elapsed between the individual's Notice to Appear and the conclusion of the case before an immigration judge. "Relief Denied" represents a denial of relief in an immigration court that results in the individual's removal. This variable is described in detail in Part IV. Other control variables include country of origin, in absentia status, if the individual was ever detained, and whether a crime other than residing illegally within the United States was charged.

Table 5: Case Duration, By Attorney Status

	Attorney	No Attorney
Percentage Change in Cases Pending × 100	0.0231***	0.0554***
	(0.0002)	(0.0002)
In Absentia	0.2418***	0.7759***
	(0.0051)	(0.0035)
Relief Denied	-0.4734***	-0.9510***
	(0.0025)	(0.0049)
Crime Committed	-0.0044	0.2803***
	(0.0032)	(0.0027)
Ever Detained	-0.4542***	-0.7453***
	(0.0027)	(0.0034)
Constant	6.4832***	5.6823***
	(0.0060)	(0.0093)
Observations	820,489	1,463,943
R-Squared	0.3311	0.4346

***p<0.01; **p<0.05; *p<0.10. Dependent variable is the natural log of case duration. Data shown is comprised of the DHS records of all juvenile cases in immigration court. Data was compiled by the Transactional Records Access Clearinghouse (TRAC) through a Freedom of Information Act (FOIA) and was made available to me through my appointment as a TRAC Fellow. Data on cases pending in each immigration court was provided by TRAC's immigration tool. "Case Duration" is measured by days elapsed between the individual's Notice to Appear and the conclusion of the case before an immigration judge. "Relief Denied" represents a denial of relief in an immigration court that results in the individual's removal. This variable is described in detail in Part IV. Other control variables include country of origin, in absentia status, if the individual was ever detained, and whether a crime other than residing illegally within the United States was charged.

Table 6: Case Duration, By Year

	2003–2006	2007–2010	2011–2013
Percentage Change in Cases Pending × 100	-0.0721***	0.0016***	0.1106***
	(0.0013)	(0.0002)	(0.0046)
Attorney	1.2526***	1.4253***	1.1779***
	(0.0031)	(0.0034)	(0.0043)
In Absentia	0.4020***	0.7570***	1.1850***
	(0.0038)	(0.0046)	(0.0057)
Relief Denied	-0.6086***	-0.6027***	-0.6920***
	(0.0035)	(0.0039)	(0.0053)
Crime Committed	0.1294***	0.2617***	0.0723***
	(0.0033)	(0.0034)	(0.0047)
Ever Detained	-0.8217***	-0.6933***	-0.4984***
	(0.0033)	(0.0038)	(0.0047)
Constant	5.4480***	5.1187***	5.0608***
	(0.0076)	(0.0084)	(0.0120)
Observations	717,376	1,125,804	657,732
R-squared	0.5981	0.5902	0.5647

***p<0.01; **p<0.05; *p<0.10. Dependent variable is the natural log of case duration. Data shown is comprised of the DHS records of all juvenile cases in immigration co013. Data was compiled by the Transactional Records Access Clearinghouse (TRAC) through a Freedom of Information Act (FOIA) and was made available to me through my appointment as a TRAC Fellow. Data on cases pending in each immigration court was provided by TRAC's immigration tool. "Case Duration" is measured by days elapsed between the individual's Notice to Appear and the conclusion of the case before an immigration judge. "Relief Denied" represents a denial of relief in an immigration court that results in the individual's removal. This variable is described in detail in Part IV. Other control variables include country of origin, in absentia status, if the individual was ever detained, and whether a crime other than residing illegally within the United States was charged.

Table 7: Demographic Makeup of Respondent Demographics, By Year

	2003–2006	2007–2010	2011–2013
Region of Birth			
Mexico	28.54	46.24	47.98
Caribbean	19.08	11.76	13.31
Asia	10.40	9.91	9.45
West Europe	0.89	1.06	0.88
Eastern Europe	1.89	1.65	1.57
Middle East	3.16	2.00	1.54
South America	11.17	5.25	4.02
Central America	20.91	17.53	18.12
Africa	3.21	3.16	2.45
Oceania	0.32	0.39	0.34
Canada	0.43	0.38	0.38
Demographics			
Attorney	32.54	37.12	37.12
No Attorney	67.46	63.88	61.68
In Absentia	33.71	13.97	16.03
Relief Granted	18.88	23.03	23.50
Relief Denied	81.12	76.97	76.50
Committed Crime	15.52	16.59	17.15
Ever Detained	43.35	63.25	72.43
Never Detained	56.64	36.75	27.57
N	717,376	1,125,804	657,732

Note: Data shown is comprised of the DHS records of all juvenile cases in immigration courts during the years 2003–2013. Means reported. Data was compiled by the Transactional Records Access Clearinghouse (TRAC) through a Freedom of Information Act (FOIA) and was made available to me through my appointment as a TRAC Fellow. Data on cases pending in each immigration court was provided by TRAC's immigration tool.

Table 8: Probit Model, Success

	Pr(Success)
Percentage Change in Cases Pending	0.0221***
	(0.0002)
Attorney	1.1025***
	(0.0027)
In Absentia	-1.6505***
	(0.0056)
Crime Committed	-0.0955***
	(0.0033)
Ever Detained	-0.9496***
	(0.0027)
Constant	-0.5626***
	(0.0071)
Observations	2,284,432

***p<0.01; **p<0.05; *p<0.10. Data shown is comprised of the DHS records of all juvenile cases during the years 2003–2013. Data was compiled by the Transactional Records Access Clearinghouse (TRAC) through a Freedom of Information Act (FOIA) and was made available to me through my appointment as a TRAC Fellow. The dependent variable, probability of success, is defined by a grant of relief or termination of a case in immigration court. This variable is described in detail in Part IV. Data on cases pending in each immigration court was provided by TRAC's immigration tool. "Case Duration" is measured by days elapsed between the individual's Notice to Appear and the conclusion of the case before an immigration judge. Individuals are recognized as "In Absentia" if they are not present at the hearing in which an immigration judge announces the conclusion of their case. Other control variables include country of origin, if the individual was ever detained, and whether a crime other than residing illegally within the United States was charged.

Table 9: Probit Model, Success, By Attorney Status

	Attorney	No Attorney
Percentage Change in Cases Pending × 100	0.0163***	0.0132***
	(0.0002)	(0.0003)
In Absentia	-2.5789***	-1.8342***
	(0.0183)	(0.0067)
Crime Committed	-0.1083***	-0.0434***
	(0.0042)	(0.0057)
Ever Detained	-0.7276***	-1.4354***
	(0.0032)	(0.0053)
Constant	0.3586***	0.0386***
	(0.0078)	(0.0131)
Observations	820,489	1,463,943

***p<0.01; **p<0.05; *p<0.10. Data shown is comprised of the DHS records of all juvenile cases during the years 2003–2013. Data was compiled by the Transactional Records Access Clearinghouse (TRAC) through a Freedom of Information Act (FOIA) and was made available to me through my appointment as a TRAC Fellow. The dependent variable, probability of success, is defined by a grant of relief or termination of a case in immigration court. This variable is described in detail in Part IV. Data on cases pending in each immigration court was provided by TRAC's immigration tool. "Case Duration" is measured by days elapsed between the individual's Notice to Appear and the conclusion of the case before an immigration judge. Individuals are recognized as "In Absentia" if they are not present at the hearing in which an immigration judge announces the conclusion of their case. Other control variables include country of origin and whether a crime other than residing illegally within the United States was charged.

Table 10: Probit Model, Success, By Year

	2003–2006	2007–2010	2011–2013
Percentage Change in Cases Pending × 100	-0.2240***	0.0162***	0.3621***
	(0.0021)	(0.0006)	(0.0083)
Attorney	0.8225***	1.1137***	1.1913***
	(0.0045)	(0.0045)	(0.0064)
In Absentia	-0.0408***	-0.0344***	-0.2327***
	(0.0053)	(0.0054)	(0.0080)
Crime Committed	-1.6760***	-1.7435***	-2.0890***
	(0.0083)	(0.0102)	(0.0162)
Ever Detained	-0.7361***	-0.8386***	-0.7469***
	(0.0050)	(0.0049)	(0.0071)
Constant	-0.9555***	-0.5844***	-0.5074***
	(0.0219)	(0.0201)	(0.0277)
Observations	717,376	1,125,804	657,732

***p<0.01; **p<0.05; *p<0.10. Data shown is comprised of the DHS records of all juvenile cases during the years 2003–2013. Data was compiled by the Transactional Records Access Clearinghouse (TRAC) through a Freedom of Information Act (FOIA) and was made available to me through my appointment as a TRAC Fellow. The dependent variable, probability of success, is defined by a grant of relief or termination of a case in immigration court. This variable is described in detail in Part IV. Data on cases pending in each immigration court was provided by TRAC's immigration tool. "Case Duration" is measured by days elapsed between the individual's Notice to Appear and the conclusion of the case before an immigration judge. Individuals are recognized as "In Absentia" if they are not present at the hearing in which an immigration judge announces the conclusion of their case. Other control variables include country of origin and whether a crime other than residing illegally within the United States was charged.