Essays on Gender, Immigration, and Labor Markets

BY

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THESIS
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This thesis is dedicated to my grandmother, Ghamar Shabahang, whose love has always been a grounding force in my life. She raised me as her own and I couldn't have asked for anyone better. This thesis is also dedicated to the memory of my grandfather, Amir Ghahremanlou, who dreamed of this day and called me a doctor at age 6.

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and support. He brought to life the friendships I had only read about in storybooks.

SSA

CONTRIBUTION OF AUTHORS

The third chapter of this dissertation is a collaboration with Benjamin Feigenberg and Darren Lubotsky. Authors are listed alphabetically. I contributed to all areas of the project, including data collection, statistical analysis, and writing.

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LIST OF ABBREVIATIONS

ACS	American Community Survey
CBP	County Business Patterns
DHS	Department of Homeland Security
E-Verify	Electronic Employment Eligibility Verification System
FY	Fiscal Year
GDP	Gross Domestic Product
GSS	General Social Survey
ICE	Immigration and Customs Enforcement
INS	Immigration and Naturalization Service
IPUMS	Integrated Public Use Microsamples
LAWA	Legal Arizona Workers Act
NORC	National Opinion Research Center
OMB	Office of Management and Budget
PUMA	Public Use Microdata Area
QWI	Quarterly Workforce Indicators
UI	Unemployment Insurance

LIST OF ABBREVIATIONS

U.S.	United States
USCIS	United States Citizenship and Immigration Services

SUMMARY

I have often observed that labor markets are experienced differently by people with different characteristics. Within the United States (U.S.) labor market, gender and immigration status, seemingly, serve as serious impediments for some, and benefits for others due to society's attitudes toward gender roles and immigration-related legislation. Inspired by these observations, my research has focused on studying the impacts U.S. labor market conditions have on gender attitudes and, the impacts of immigration legislation on U.S. labor markets.

The first chapter of this dissertation focuses on data between 1997 and 2016, and examines the evolution of attitudes toward women working and women's emotional suitability for politics. Since 1977, the prevalence of traditional attitudes has significantly declined by 37 and 36 percentage points, respectively. However, in 2016, 25 percent of survey respondents still believed that women should tend home and 17 percent believed that women are not emotionally suitable for politics.

These traditional attitudes may affect women and families in myriad ways, including the options women can choose to pursue, such as education attainment, labor supply, age at which they marry, and age at which they have their first child, to name a few. Therefore, a clear understanding of how traditional attitudes on gender roles evolve is valuable.

To shed light on this evolution, I present a descriptive analysis of traditional gender-role attitudes on both women working and their emotional suitability for politics nationally and at the state level. I show that despite the significant decline in the prevalence of traditional attitudes nationally, there is substantial variation in the rate of decline across states over time. Furthermore, to investigate whether the aggregate decline in the prevalence of traditional attitudes is driven entirely by cohort replacement - the replacement of older by younger birth cohorts who have different life experiences and beliefs - or is, at least, partly a product of people changing their views over time, I partially decompose the share of traditional attitudes by birth cohort, age, and time period. I find evidence consistent with both processes of cohort replacement and people changing their minds.

Moreover, I investigate whether the aggregate evolution of traditional attitudes is uniform across sub-populations or if it is driven by larger changes among one sub-population defined on the basis of gender, education level, and marital status. I show that the prevalence of traditional attitudes toward women working and their emotional suitability for politics is marginally smaller among women versus men. I also show that the prevalence of traditional attitudes decreases in higher education levels, and increases with marriage.

The second chapter examines the effects of U.S. state-level labor demand changes on the share of those same traditional attitudes. Per Chapter 1, traditional gender-role attitudes, where women are viewed as homemakers and men as breadwinners, have declined substantially over time. Although, many contributing factors have been previously studied, I examine the contribution of labor demand shifts to these attitude changes. I document that positive labor demand shocks, measured as Bartik shocks, lower the prevalence of traditional attitudes toward women working but find no statistically significant effect on traditional attitudes toward women's emotional suitability for politics. Also, despite finding no evidence of heterogeneous effects of Bartik shocks, I find suggestive evidence that own-group Bartik shocks, defined along gender and education dimensions, are possibly more relevant measures than the overall labor demand shocks, especially among men with less than a high school diploma and men with at least a baccalaureate.

The third chapter of this dissertation (co-authored with Benjamin Feigenberg and Darren Lubotsky) estimates the impact of state-level "E-Verify" legislation that mandates employment eligibility verification for private-sector workers. We document declines in formal sector employment and employment turnover after mandate passage, with effects concentrated among those likeliest to be work-ineligible. Using newly available data, we show that larger firms are far more likely to comply with mandates. Heterogeneity in adher-

ence leads to substantial within-state employment spillovers from larger to smaller firms, as well as a reduction in the number of large firms. We find no evidence that work-ineligible populations relocate or that native-born workers' labor market outcomes improve in response to mandates.

Thesis Supervisor: Steven Rivkin

Title: Department Head and Professor of Economics

1 FRAILTY THY NAME IS STILL WOMAN? EVIDENCE FROM THE EVOLUTION OF TRADITIONAL GENDER ATTITUDES

1.1 Traditional Gender Attitudes

Although gender inequality and the notion of "missing women" is more striking in developing countries[1], gender equality remains far from a reality in the United States (U.S.). In fact, there is an extensive body of work that studies gender differences in labor market outcomes in the U.S.[2]. Research has shown that differences in societal expectation about appropriate roles for men and women may prescribe different choices for women versus men, including choices about schooling, work, occupation, ages of marriage and childbearing, and ultimately women's earnings and authority both in and out of the home (Goldin, 2006; Buchmann, DiPrete, and McDaniel, 2008; Charles, Guryan, and Pan, 2018). A recent study finds that some wives try to avoid earning more than their husbands by adjusting their labor market participation when faced with such possibility (Bertrand, Kamenica, and

[1]See Esther Duflo 2012 for a review of literature on gender inequality in developing countries.

[2]See Blau, Kahn, 2017 for a survey of literature on gender wage gap, and Bertrand, 2011 for a review of gender differences in labor market outcomes and possible contributing factors.

Pan, 2015). Another study finds that in some marriages where wives end up earning more than their husbands, both spouses misrepresent their income on surveys to downplay wives' income and exaggerate husbands'(Murray-Close, and Heggeness, 2018).

The prevalence of traditional gender attitudes, attitudes that subscribe to traditional division of labor with men as breadwinners and women as homemakers, may affect labor market and social outcomes of women by influencing their own expectations, and the behavior of employers and schools. Examining the evolution of traditional gender attitudes and their prevalence over time is, therefore, essential to developing a deeper understanding of how these attitudes are formed.

Existing research on the determinants of attitudes toward gender roles have identified the introduction of contraceptive pills (Goldin and Katz, 2002), the AIDS epidemics (Fortin, 2015), childhood experiences and cultural backgrounds (Vella, 1994; Fernandez, Fogli, and Olivetti 2004; Farré and Vella, 2007; Fernandez and Fogli, 2009), and single-sex and co-ed schooling and college environments (Maccoby, 1990, 1998; Lee and Marks, 1990; Dasgupta and Asgari, 2004) among some of the causes of changes in attitudes toward women. I contribute to that body of work with a study of the effects of labor demand changes on attitudes.

This chapter examines the evolution of U.S. traditional gender attitudes, between 1977 and 2016, and lays the foundation for examining the U.S. state-level labor demand changes as determinants of traditional gender attitudes in the second chapter. The rest of the chapter proceeds as follows. Section 1.2 describes the gender attitudes data; section 1.3 examines the evolution of traditional gender attitudes by time, birth cohort, and age; section 1.4 inspects this evolution of traditional attitudes by gender, education, and marital status, and lastly, section 1.5 examines the evolution of traditional attitudes by state over time.

1.2 Data

The data to measure gender attitudes comes from the General Social Survey (GSS) (Smith et al., 2018). Although the GSS data is publicly available, the geographic information was obtained via a confidentially contract with the National Opinion Research Center (NORC), which administers the survey. From 1972 to 1994, the survey was mostly conducted annually with a target sample size of 1500 (Smith et al., 1972-2016). Since 1994, it has been conducted biennially in two samples, each with a target size of 1500. The GSS oversamples adults in small households, and since 2004, it also oversam-

ples non-responders[3]. The GSS is only representative at the national level and not the state level.

There are 27 states that are included in each of the five years of 1977, 1990, 2000, 2010, and 2016 used in this paper[4]. These states are Alabama, Arizona, California, Colorado, Connecticut, Florida, Georgia, Illinois, Indiana, Louisiana, Maryland, Massachusetts, Michigan, Minnesota, Missouri, New Jersey, New York, North Carolina, Ohio, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, Virginia, Washington, and Wisconsin.

The GSS collects information on demographics, social attitudes, and special interest topics. For the purposes of this analysis, I focus on two attitudinal questions that are directly about non-domestic roles of women[5]. In the first question, the survey participants are read the statement: "it is much

[3]Oversampling occurs because the GSS only interviews one adult per household and the probability of being selected for the interview declines in larger households. The GSS also subsamples no response cases in a two-stage subsampling design, and focuses resources on gaining responses from this subset.

[4]These years were chosen based on data availability and considerations for period length needed for measuring changes. To increase the number of observations for the empirical analysis, I pool 1978 with 1977 observations, 1986 with 1985, and 2014 with 2016 observations. It is unlikely that attitudes change much over a year or two-year period and therefore, this pooling should not affect the results.

[5]Knight and Brinton (2017) suggest that gender attitudes toward the public sphere of work and the private sphere of family should be distinguished. As such, I choose the only two questions that are not specific to private spheres or other facets of gender attitudes such as motherhood, abortion, or women in religion, to name a few, which although interesting, are not of interest in this paper. These questions are also administered as far back as 1977 and as late as 2016.

better for everyone involved if the man is the achiever outside the home and the woman takes care of the home and family" and asked to indicate whether they strongly agree, agree, disagree, or strongly disagree with the statement. In the second question, survey participants are only asked to indicate whether they agree or disagree (there are no strong agreement or disagreement options) with the statement: "Most men are better suited emotionally for politics than are most women." The potential response options for each statement are consistent across survey years and include a choice of "don't know" for the first statement and a "not sure" choice for the second statement.

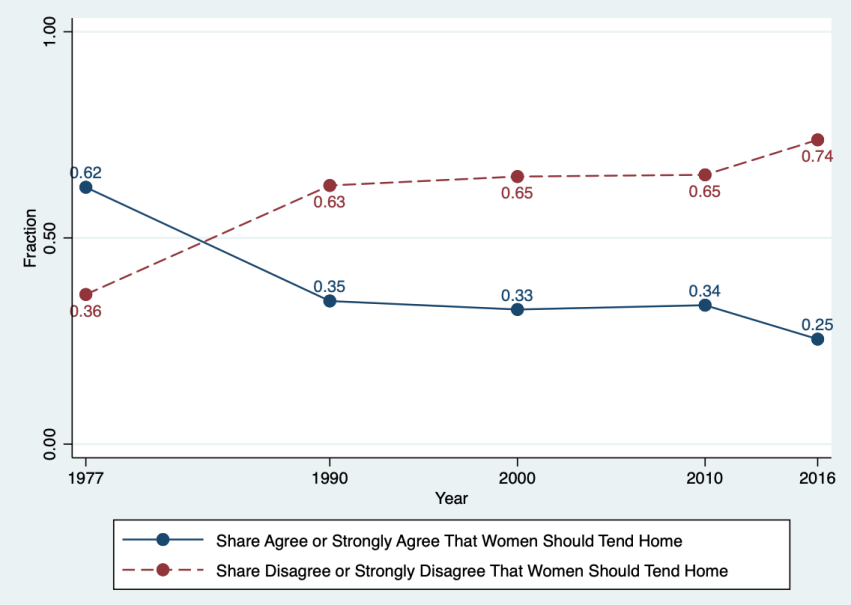
I create two binary variables, one for traditional attitudes and one for egalitarian attitudes per question. For whether women should tend home, the binary variable for traditional attitudes is equal to one if responders indicate agreement or strong agreement and zero otherwise (including "don't know" responses). The binary variable for egalitarian attitudes is equal to one if responders indicate disagreements or strong disagreements and zero otherwise (also including "don't know" responses). For whether men are better emotionally suited for politics, the binary variable for traditional attitudes is set to one for agreement and zero otherwise (including "not sure" responses). The binary variable for egalitarian attitudes is equal to one for disagreement and zero otherwise (also including "not sure" responses).

1.3 The Evolution of Gender Attitudes by Time, Cohort, and Age

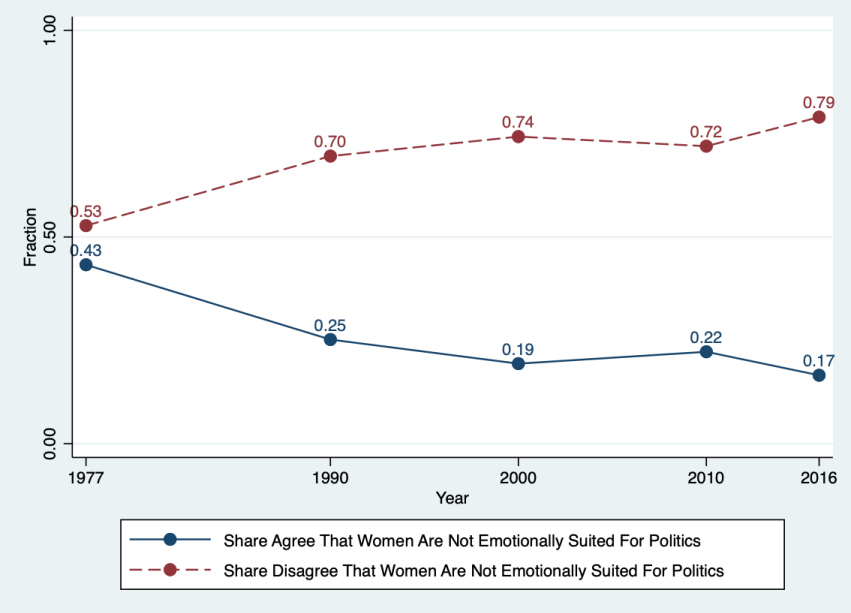
Figure 1 shows the evolution of traditional and egalitarian gender attitudes toward women working outside of the home (Panel (a)) and their emotional suitability for politics (Panel (b)) from 1977 to 2016. The figure shows that the evolution of gender attitudes from traditional to egalitarian on women working outside of home essentially paused between 1990 and 2010, where the decline in the share of traditional attitudes was one percent in two decades. A similar slowdown is observed in Panel (b), where traditional attitudes fell by 3 percentage points during 1990-2010. Both panels show that between 2010 and 2016 the earlier patterns of decline in the prevalence of traditional attitudes resumed by 9 and 5 percentage points toward women working and women's emotional suitability for politics, respectively. Thus, although traditional gender attitudes are no longer the norm in 2016, nontrivial shares of participants (27 and 18 percent) still continue to believe that women should tend home and that women are not emotionally suited for politics.

To explore the congruency between attitudes on women working outside of the home and women's emotional suitability for politics, Figure 2 shows the joint distribution of responses between 1977 and 2016. Consistent with expectations, Figure 2 shows that attitudes on women working outside of the home and women's emotional suitability for politics each capture a

Figure 1: Evolution of Gender-Role Attitudes in The U.S. Over Time



Panel (a)

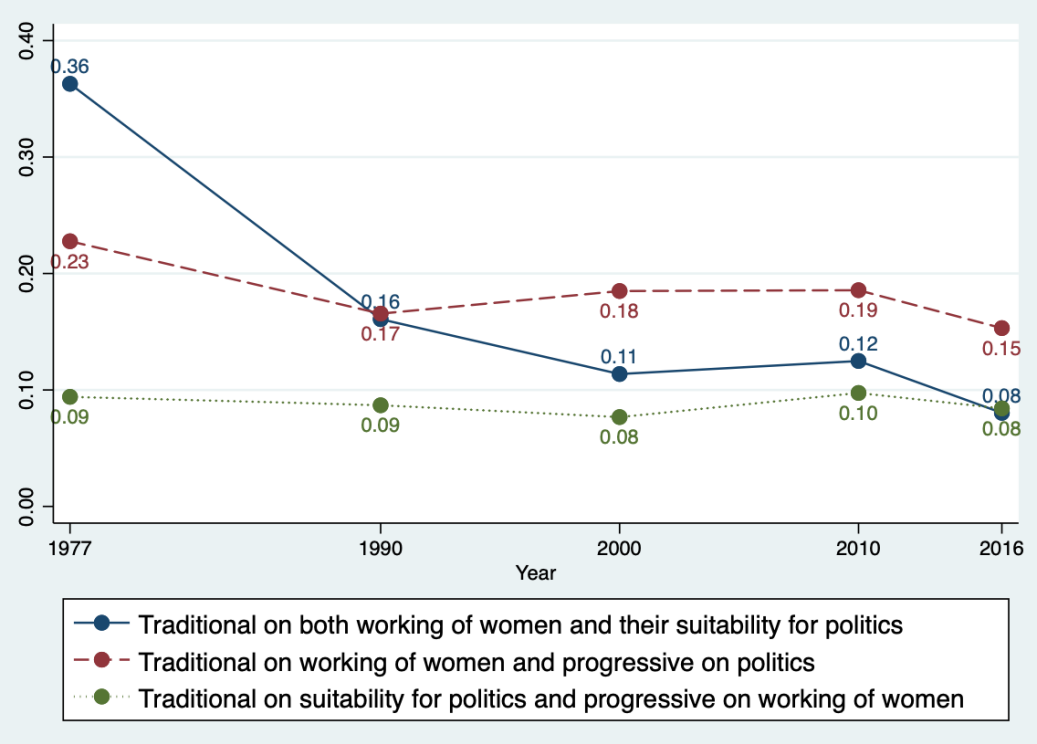


Panel (b)

Data Source: The General Social Survey.

Notes: Each panel plots the shares of traditional and progressive gender attitudes over time. In Panel (a), the share of traditional attitudes is defined as the number of agreements or strong agreements divided by the total number of responses to the statement that is better for men to work and women tend home. The share of progressive gender attitudes is defined similarly but instead uses the number of disagreements or strong disagreements. In Panel (b), the share of traditional gender attitudes is defined as the number of agreements (no option for strong agreements) divided by the total number of responses to the statement that women are not emotionally suited for politics. The share of progressive gender attitudes is defined similarly but instead uses the number of disagreements (no option for strong disagreements).

Figure 2: Joint Distribution of Attitudes Toward Women Working and Women's Emotional Suitability for Politics



Data Source: The General Social Survey.

Notes: This figure plots 3 of the 9 possible combination of responses to statements that it is better for men to work and women tend home and that women are not emotionally suited for politics over time. The possible combinations are between agreement or strong agreement (traditional), disagreement or strong disagreement (egalitarian), and don't know response options to it is better for men to work and women tend home and the agreement (traditional), disagreement (egalitarian), and not sure response options to women are not emotionally suited for politics.

distinct dimension of gender roles and that distinction is maintained over time. In fact, at its highest, in 1977, only 30 percent of participants thought traditionally on both questions. With the evolution of gender-role attitudes toward egalitarian between 1977 and 2016, the share of this combination of responses (traditional on both) fell to an all-time low of 8 percent in 2016. Moreover, another notable combination of responses that suggests a distinction between the two questions is the 23 of responses that indicate egalitarian views toward the emotional suitability of women for politics with traditional attitudes toward women working. This combination of responses appears more persistent over time and stays at 15 percent in 2016. The most stable response combination is traditional attitudes toward the emotional suitability of women combined with egalitarian attitudes toward women working. The share of such responses remains at about 9 percent.

The underlying changes in gender attitudes may be partly driven by cohort replacement in the survey (the process of older birth cohorts being replaced by younger cohorts who have different childhood experiences and values), and partly by individuals changing their views in response to structural changes in labor markets and business cycles. People may change their views as their positions (and negotiating power) within households and workforce changes, which might legitimize some of their attitudes and disapprove of others. Moreover, people may change their views because of ideological learning and a broader change in their belief systems, where they adopt gen-

der attitudes consistent with their views pertaining to similar matters, such as racial or religious attitudes (Brooks and Bolzendahl, 2004).

Herein I describe and discuss whether there is consistent evidence with the two processes of cohort replacement and people changing their views. To accomplish this, I describe changes by time, birth cohort or age. Identifying the independent effects is complicated by the perfect collinearity between the three and cannot be distinguished. However, descriptions of changes over time for a number of cohorts will provide evidence of the patterns of changes and the likely contributions of cohort replacement and people changing their views to differences in the prevalence of traditional gender attitudes.

Tables I and II partially decompose changes in the shares of traditional attitudes toward women working (Table I) and the emotional suitability of women for politics (Table II) by holding a different part fixed vertically, horizontally, and diagonally. Horizontally, data are sliced by birth cohorts, and the observed variation is due to a combination of changes in time and age. Vertically, data are sliced by year, and variation in shares are due to differences across birth cohorts and age groups. Diagonally, data are sliced by age groups and changes are because of time and birth cohorts. Given the differences in gaps between years, especially 1977-1990 and 2010-2016 from 1990-2000 and 2000-2010, the age groups in 1977 and 2016 are a few years different from their diagonal counterparts between 1990 and 2010.

TABLE I: SHARE AGREE OR STRONGLY AGREE THAT WOMEN SHOULD TEND HOME BY BIRTH COHORT, AGE, AND PERIOD

	GSS survey years				
Median Birth	1977	1990	2000	2010	2016
2010-2016					
2000-2009					
1990-1999					0.27
1980-1989				0.33	0.22
1970-1978			0.25	0.32	0.25
1960-1969		0.25	0.27	0.33	0.24
1950-1959	0.47	0.25	0.32	0.34	0.29
1940-1949	0.49	0.29	0.40	0.38	
1930-1939	0.65	0.46	0.56		
1920-1920	0.73	0.67			
1907-1919	0.84				

Notes: Table 1 depicts the share of traditional attitudes on women working outside of home, defined as the number of agreements or strong agreements divided by the total number of responses to the statement that it is better for men to work and women tend home, by birth cohort (horizontally), by period (vertically), and by age group (diagonally).

TABLE II: SHARE AGREE THAT WOMEN ARE NOT EMOTIONALLY SUITED FOR POLITICS BY BIRTH COHORT, AGE, AND PERIOD

	GSS survey years				
Median Birth	1977	1990	2000	2010	2016
2010-2016					
2000-2009					
1990-1999					0.21
1980-1989				0.26	0.18
1970-1978			0.18	0.21	0.17
1960-1969		0.20	0.18	0.21	0.13
1950-1959	0.33	0.21	0.19	0.22	0.15
1940-1949	0.37	0.23	0.19	0.21	
1930-1939	0.50	0.34	0.26		
1920-1920	0.47	0.36			
1907-1919	0.56				

Notes: Table 2 depicts the share of traditional attitudes on emotional suitability of women for politics, defined as the number of agreements (there are no strong agreement options) divided by the total number of responses to the statement that women are not emotionally suited for politics, by birth cohort (horizontally), by period (vertically), and by age group (diagonally).

Table I shows that the prevalence of traditional attitudes is consistently higher among older cohorts. In fact, when the differences in shares of traditional attitudes toward women working across cohorts are formally tested by a regression of the shares on year and cohort fixed effects, statistically significant and positive coefficients are estimated for older cohorts, starting

from the 1940-1949 cohort. These differences across cohorts, however, narrow with each subsequent cohort replacement. Specifically, the 1990 column shows a difference of 42 percentage points between the prevalence of traditional attitudes among the youngest (1960-1969) and the oldest (1920-1929) birth cohorts. This oldest birth cohort that was 61-70 years old in 1990, ages out of the data by 2000 and is replaced by the 1970-1979 birth cohort, some of whom were too young to be observed in the data in 1990. As a result of the cohort replacement between 1990 and 2000, the difference in shares of traditional gender attitudes drops by 11 percentage points to 31 percentage points in 2000. In the next round, as the 1930-1939 cohort in 2000 is replaced by the 1980-1989 cohort in 2010, the difference declines to 5 percentage points.

In addition to the cross-cohort variation in the prevalence of traditional gender attitudes toward women working, Table I documents within cohort variation horizontally. This within cohort variation implies a potential link between the traditional gender attitude trends and individuals changing their views toward women working. Such changes capture the effects of aging and of time. However, changes in the composition of cohorts for reasons such as immigration and mortality may also contribute to the observed within cohort variation in attitudes, which appear to be more pronounced among the older cohorts. Even so, immigration is unlikely to be driving these within cohort variations in attitudes. Not only did the U.S. experience relatively

small inflow of immigrants during the Great Depression of the 1930s and the World War II in the 1940s, but also the size of the elderly foreign-born population declined between 1950 and 1990 (Rogers and Raymer, 2001).

The larger within cohort variation among earlier (the 1930s, 1940s, and 1950s) versus later cohorts (the 1960s, 1970s, and 1980s) is underlined when comparing the overall drop in the prevalence of traditional attitudes toward women working between 1977 and 2016. The prevalence of traditional attitudes among the earlier cohorts dropped between 9 to 18 percentage points compared to 0 to 11 percentage points among the later cohorts.

Another noteworthy observation from Table I is the similar patterns of decline and surge within different cohorts. In every cohort, there is an initial rapid decline between 1977 and 1990, a surge in traditional attitudes between 1990 and 2010, and a restart of the earlier pattern of decline in the prevalence of traditional attitudes between 2010 and 2016. The 1990-2010 within-cohort spikes in the prevalence of traditional attitudes toward women working are an interesting deviation from earlier trends and possibly suggest a response to the economic downturns of the early 1990s and the Great Recession between 2008 and 2010. In fact, this is consistent with the results in Chapter 2 that show the prevalence of traditional gender attitudes toward women working increases in response to negative labor demand shocks.

Similar patterns of cross and within cohort variations is observed in Table II with regards to the prevalence of traditional attitudes toward the emotional suitability of women for politics. Just as in Table I, within cohort variation is larger among older cohorts. Indeed, whereas the within variation among later cohorts (1960s, 1970s, and 1980s) ranges from 1 to 8 percentage points, the within variation in earlier cohorts (1930s, 1940s, and 1950s) ranges from 16 to 24 percentage points. Also, like Table I, there is a period of deviation, a spike, in the prevalence of traditional attitudes toward the emotional suitability of women for politics within all cohorts. However, unlike Table I, the deviation is shorter and limited to 2000-2010 instead of 1990-2010 observed in Table I.

Both tables demonstrate evidence consistent with cohort replacement (changes over time across cohorts), and individuals changing their views (changes over time within cohorts as they age). However, cohort replacement is a less prominent factor in driving changes across cohorts in Table II than in Table I. Indeed, after the replacement of the 1920-1929 cohort in 1990 with the 1970-1979 cohort in 2000, which halved the differences in the share of traditional attitudes toward the emotional suitability of women for politics across cohorts from 16 to 8 percentage points, the subsequent cohort replacement from 2000 to 2010 contributes little, since the prevalence of traditional attitudes is 26 percent among both the 1930-1939 and 1980-1989 cohorts. Nonetheless, the presence of cohort replacement here and in Table I points to the potential effects of longer-term changes in labor market

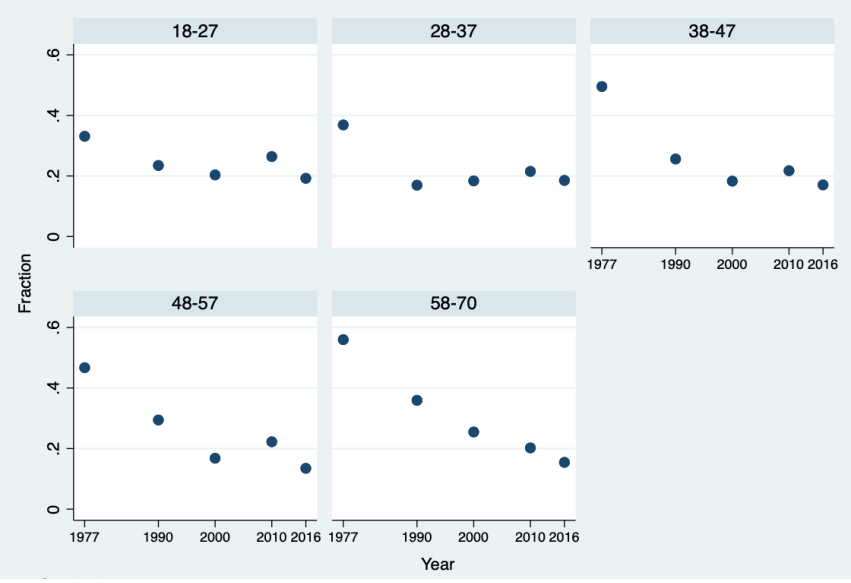
conditions on attitude formation of different cohorts. These cohorts grew up under different labor market conditions, including having different fractions of their mothers working. Before the 1920s, women virtually always exited the labor market upon marriage. Whereas, between 1950 to 1970, the labor force participation of married women aged 35-44 years old grew from 25 to 46 percent (Goldin, 2006).

Figure 3 highlights differences in the rate of decline within age groups. The differences among the older age groups over time indicates the long-term changes in socioeconomic structure of the country. The 58-70 years old in 1977 grew up in an era where women were poorly educated, job options were limited and tended to be associated with less authority and power, and women almost always left the labor force upon marriage. By contrast, the 58-70 years old in 2016 grew up in a time that women had started to increase their college attendance and labor force participation. Even then, a report by the U.S. Commission on Civil Rights in 1977, found that women and minorities were "window dressing" on the television set and that women were often portrayed as weak, unemployed, and committed to family.

As noted, there are differences in life circumstances and characteristics among cohorts in, for example, available employment opportunities for women and the resultant income autonomy of women, average educational attainment, and marriage rates. These differences may not only play a role in attitude formation but may also influence the magnitude and direction

Figure 3: Traditional Gender-Role Attitudes by Age

Panel (a)



Panel (b)

Data Source: The General Social Survey.

Notes: Each panel plots the share of traditional attitudes, within 5 age groups of 18-27, 28-37, 38-47, 48-57, and 58-70, across different birth cohorts and time. In Panel (a), the share of traditional attitudes is defined as the number of agreements or strong agreements divided by the total number of responses to the statement that it is better for men to work and women tend home. In Panel (b), the share of traditional attitudes is defined as the number of agreements (there is no strong agreement option) divided by the total number of responses to the statement that women are not emotionally suited for politics.

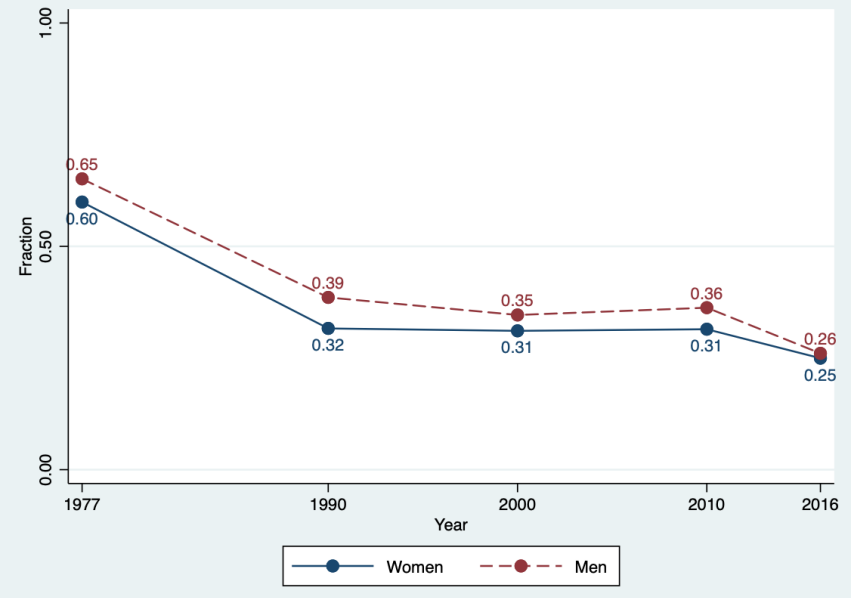
of change in attitudes. Thus, in the next subsection, I describe trends in traditional gender attitudes by gender (Figure 4), by education (Figure 5), by gender and education (Figure 6), and finally by marital status (Figure 7).

1.4 The Evolution of Gender Attitudes by Characteristics

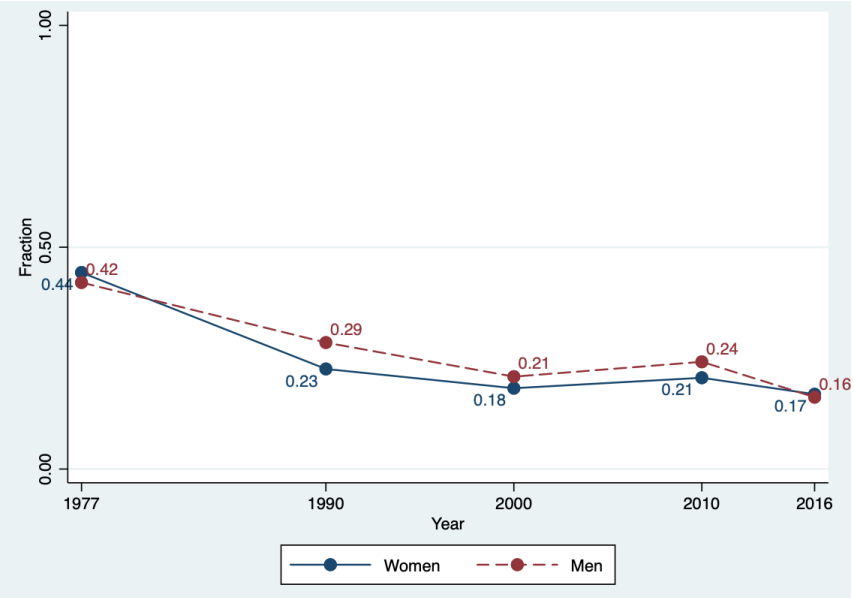
Figure 4 shows the share of traditional attitudes toward women working outside of home (Panel (a)) and the emotional suitability of women for politics (Panel (b)) separately by gender. Both panels show that traditional gender attitudes are almost always less prevalent among women than men (except for 1977 in panel (b)), but that women do not adopt egalitarian gender attitudes at a greater rate than men. In fact, men and women display a similar pattern of decline in adherence to traditional views across both dimensions of gender roles. Formal testing of these observational patterns in a regression confirms that women, on average, show 4 percentage points lower prevalence of traditional attitudes toward women working than men; but no statistically significant evidence of a differential rate of change is found for women versus men in either panel.

Figure 5 displays considerable variation in the prevalence of and the rate of decline in traditional gender attitudes by education levels. Panels (a) and (b) show the share of traditional views on women working outside of the

Figure 4: Evolution of Traditional Gender-Role Attitudes by Gender



Panel (a)

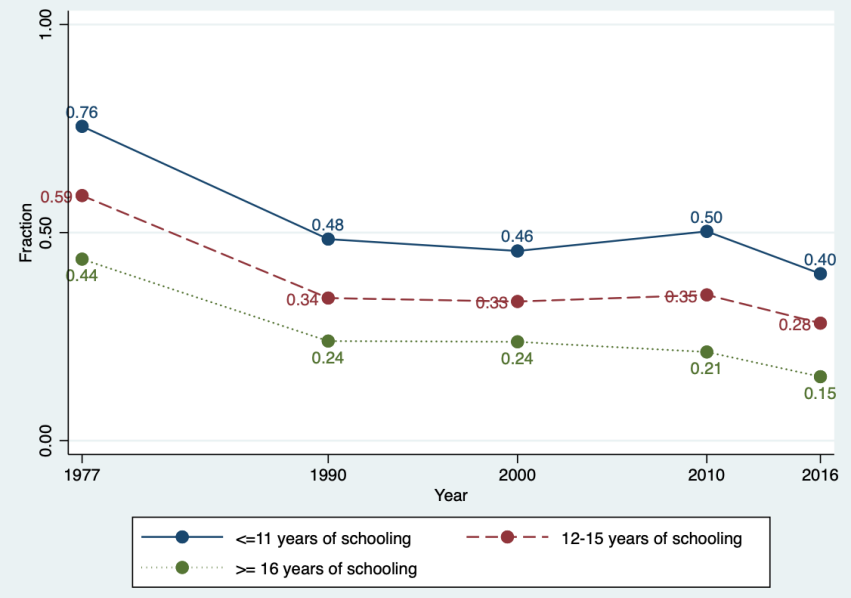


Panel (b)

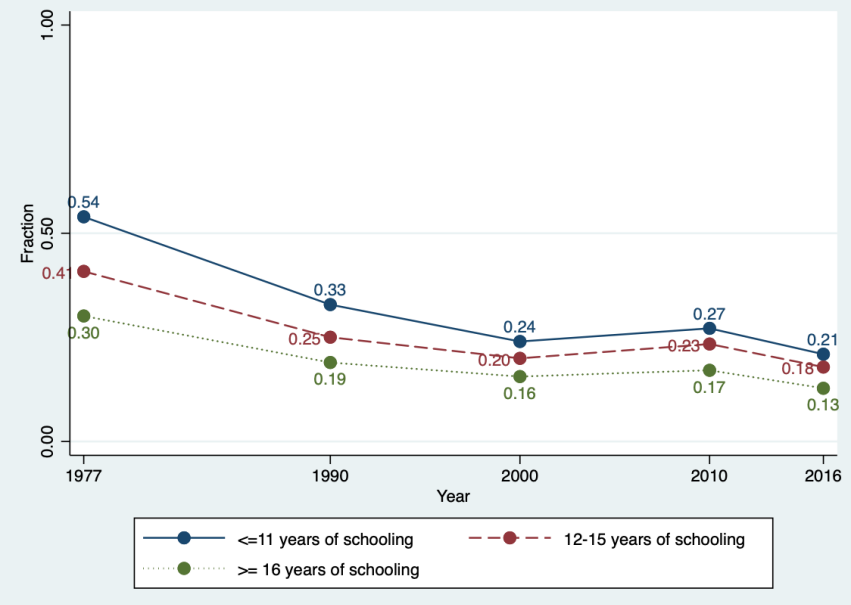
Data Source: The General Social Survey.

Notes: Each panel plots the shares of traditional gender attitudes among men and women separately, between 1977 and 2016. In Panel (a), the shares of men with traditional attitudes is defined as the number of male responders who indicate agreement or strong agreement with the statement that it is better for men to work and women tend home divided by the total number of male responders to this statement. The shares of women with traditional attitudes is defined similarly, but based on the female responders. In Panel (b), the shares of men and women with traditional attitudes is defined in the same manner as in Panel (a), except responses are with regard to the statement that women are not emotionally suited for politics.

Figure 5: Evolution of Traditional Gender-Role Attitudes by Education



Panel (a)



Panel (b)

Data Source: The General Social Survey.

Notes: Each panel plots the shares of traditional gender attitudes among responders with less than or equal to eleven years of schooling, eleven to fifteen years of schooling, and sixteen or more years of schooling between 1977 and 2016. In Panel (a), the share of traditional attitudes among each of the 3 education groups is defined as the number of responders within that education group who indicate agreement or strong agreement with the statement that it is better for men to work and women tend home divided by the total number of responders to this statement in that education group. In Panel (b), the shares are defined similarly to Panel (a), except that the responses are with regard to the statement that women are not emotionally suited for politics.

home and the emotional suitability of women for politics, respectively for three education groups: less than or equal to eleven years of schooling, between twelve to fifteen years of schooling, and at least sixteen years of schooling. Both panels show that in every period traditional views are most common among the lower educated groups, followed by the middle and then the highest educated groups. Consistently, regression tests show that respondents with twelve to fifteen years of schooling have 14 percentage points, significant at the 1 percent level, and 6 percentage points, significant at the 5 percent level, lower prevalence of traditional attitudes than respondents with less than or equal to eleven years of schooling across panels (a) and (b); respondents with at least sixteen years of schooling have 26 and 13 percentage points, significant at the 1 percent level, lower share of traditional attitudes toward women working and women's emotional suitability for politics than those with less than or equal to eleven years of schooling.

In Panel (a) of Figure 5, 76 percent of respondents with less than or equal to eleven years of schooling (less than a high school diploma) agreed or strongly agreed that women should tend home in 1977, compared to 59 percent of respondents with twelve to fifteen years of schooling (high school diploma or some college) and 44 percent of respondents with at least sixteen years of schooling (a baccalaureate or more). By 2016, the least educated remained the most traditional in their views with 40 percent of them believing that women should tend home, compared to 28 percent of the middle

group and 15 percent of the most educated group. Notably, the gaps between the education groups remained fairly stable between 1977 and 2016, with only a slight narrowing that was more pronounced between the lower and middle education groups than middle and higher groups. Indeed, the gap between lower and middle groups narrowed from a 17-percentage point difference in 1977 to a 12-percentage point difference in 2016, whereas the difference between the middle and the higher education groups went down from a 15-percentage point difference to 13 percentage points. Panel (b) shows similar but smaller level differences across education groups with a greater convergence by 2016. Panel (b) shows that the 13-percentage point difference between the lower and middle education groups in 1977 narrows to a 3-percentage point difference in 2016. The 11 percentage points between the middle and the higher education groups in 1997 also narrows to 5 percentage points in 2016.

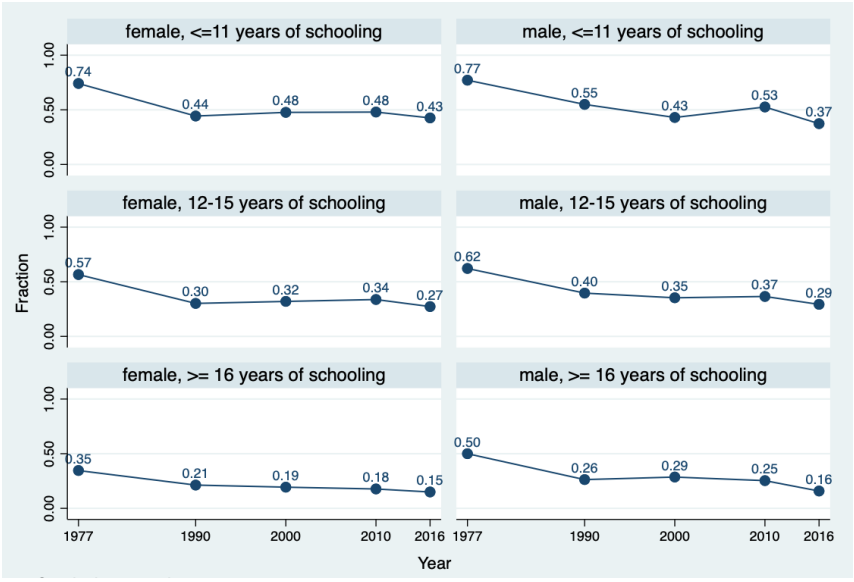
Furthermore, both panels show that the rate of decline in share of traditional gender attitudes is largest among the least educated groups, likely because they started at a much higher base, followed by the middle education groups and the most educated groups. Between 1977 and 2016, the shares dropped by 36 (Panel (a)) and 33 (Panel (b)) percentage points among the least educated groups compared to 31 (Panel (a)) and 23 (Panel (b)) percentage points for the middle group and 29 (Panel (a)) and 17 (Panel (b)) points for the highest education groups.

These differential rates of change in attitudes across educational groups are not surprising given that many of the factors that contribute to self-selection into education can very well be associated with different gender-role attitudes, such as socioeconomic conditions or parental education (Oreopoulos, Page, and Stevens, 2003). At least since the 1980s in the U.S., employment and wage growth has been increasingly uneven across occupational skills and the growth at either distribution tails, (high skill, high wage versus low skill, low wage) has been larger than gains in the middle (Autor, 2010).

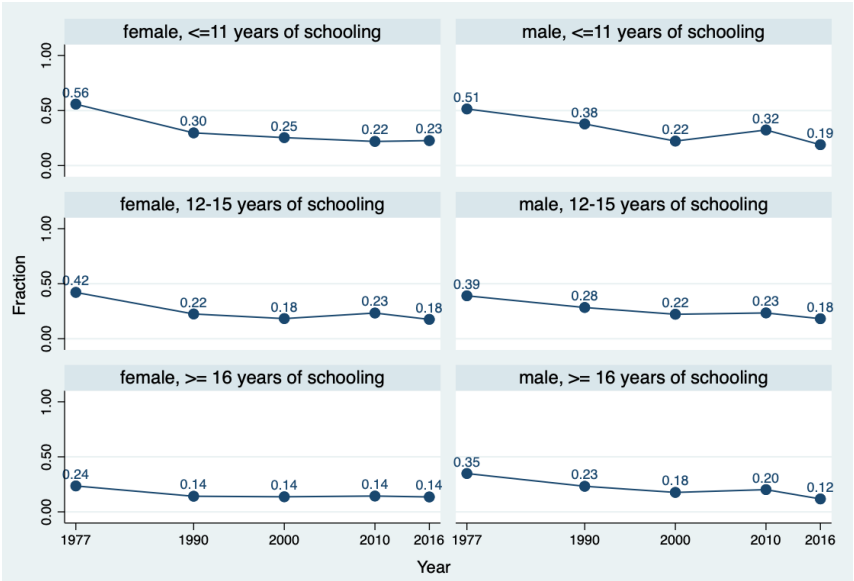
Figure 6 shows that conditioned on education, women on average continue to be more egalitarian in their attitudes than men with the same education levels on both dimensions of gender roles. In addition, both panels of Figure 6 show that traditional attitudes are more prevalent among lower educated women and men than higher educated women and men, respectively. The rate of decline in the prevalence of traditional attitudes is still larger among the least educated women compared to the middle educated and the most educated women. However, the middle-educated men appear to adopt egalitarian attitudes at a lower rate compared to both the lower and higher educated men.

Figure 7 illustrates that the prevalence of traditional attitudes is consistently higher among married women and men compared to single women and men, which is also verified through formal regression tests. The observed differences are consistent with expectations given the potential associations

Figure 6: Evolution of Traditional Gender-Role Attitudes by Gender and Education



Panel (a)

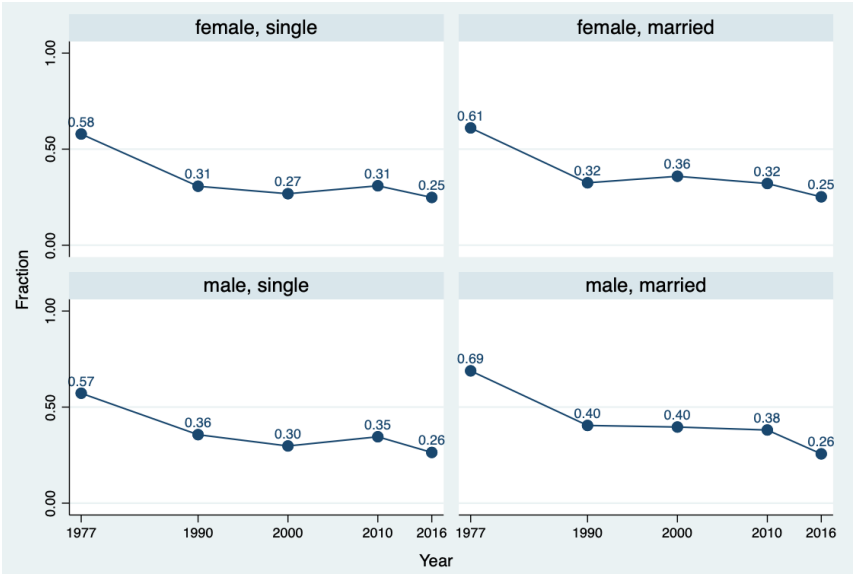


Panel (b)

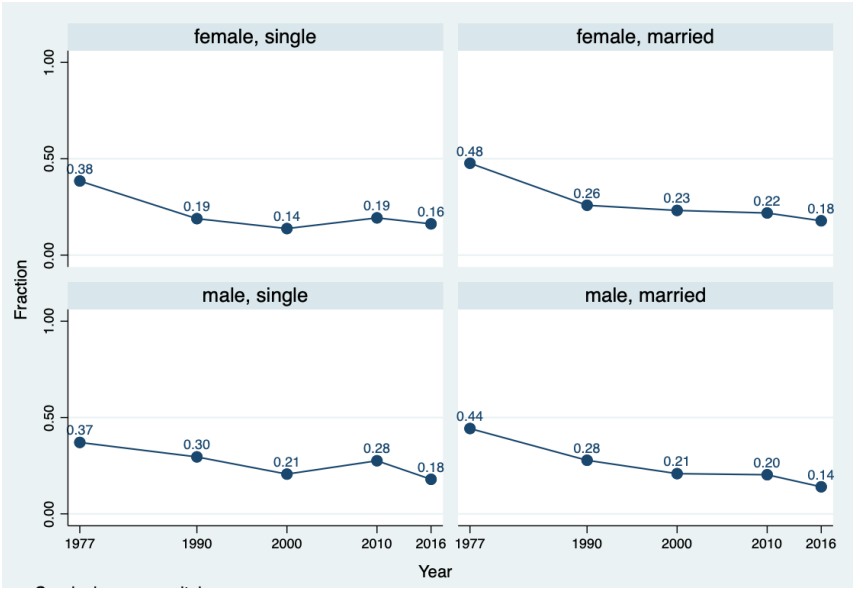
Data Source: The General Social Survey.

Notes: Each panel shows the share of traditional gender attitudes by gender (female, male) and education levels (less than or equal to eleven years of schooling, twelve to fifteen years of schooling, and at least sixteen years of schooling) over time. In Panel (a) the share of traditional gender attitudes is defined as the number of agreements or strong agreements divided by the total number of responses to the statement that it is better for men to work and women tend home. In Panel (b) the share of traditional gender attitudes is defined as the number of agreements divided by the total number of responses to the statement that women are not emotionally suited for politics.

Figure 7: Evolution of Traditional Gender-Role Attitudes by Gender and Marital Status



Panel (a)



Panel (b)

Data Source: The General Social Survey.

Notes: Each panel shows the share of traditional gender attitudes by gender (female, male) and marital status (married, single) over time. In Panel (a) the share of traditional attitudes is defined as the number of agreements or strong agreements that women should tend home divided by the total number of responses to the statement that it is better for men to work and women tend home. In Panel (b) the share of traditional attitudes is defined similarly to Panel (a), except with regard to the statement that women are not emotionally suited for politics. Single marital status includes widowed, divorced, separated, and never married. Married status includes currently married.

between marital decisions, socioeconomic status, and attitudes. For example, marriage is associated with higher job stability and wages, which may influence values and gender-role attitudes (Killewald and Lundberg, 2017, and Ahituv and Lerman, 2005).

1.5 The Evolution of Gender Attitudes by State

I now turn from the analysis of national gender attitude trend to the state level analysis of changes in the prevalence of traditional attitudes toward women working and women's emotional suitability for politics. The empirical methodology described in Chapter 2 depends upon substantial variation across states in the evolution of gender attitudes over time. Figures 8 and 9 depict share of traditional attitudes toward women working outside of the home (Figure 8) and the emotional suitability of women for politics (Figure 9) by state for the years 1977 in (a), 1990 in (b), 2000 in (c), 2010 in (d) and 2016 in (e). Both figures illustrate considerable differential cross-state variation across periods that largely mimic the national trends over time. A more in-depth look at Arizona, Wisconsin, Minnesota and Indiana as examples from Figure 8 highlights the divergence of their changes over time.

In 1977, an equal share of respondents, 77 percent, in Arizona and Indiana agreed or strongly agreed that women should tend home. Between 1977 and 1990, the shares of traditional attitudes dropped significantly, but not at the

Figure 8: State-Level Variation in The Prevalence of Traditional Attitudes Toward Women working

Data Source: The General Social Survey.

Notes: Each panel shows the share of traditional gender attitudes on women working outside of the home across the U.S. states in 1977 (a), 1990 (b), 2000 (c), 2010 (d), and 2016 (e). In each panel, the darker shades indicate a stronger prevalence of traditional gender attitudes relative to other states in that year. The share of traditional attitudes is defined as the number of agreements or strong agreements divided by the total number of responses to the statement that it is better for men to work and women tend home.

Figure 9: State-Level Variation in The Prevalence of Traditional Attitudes Toward The Emotional Suitability of Women for Politics

Data Source: The General Social Survey.

Notes: Each panel shows the share of traditional gender attitudes on emotional suitability of women for politics across the U.S. states in 1977 (a), 1990 (b), 2000 (c), 2010 (d), and 2016 (e). In each panel, the darker shades indicate a stronger prevalence of traditional gender attitudes relative to other states in that year. The share of traditional attitudes is defined as the number of agreements divided by the total number of responses to the statement that women are not emotionally suited for politics.

same rate. By 1990, Arizona had a higher prevalence of traditional attitudes toward women working than Indiana by 2 percentage points. Between 1990 and 2000, the shares of traditional attitudes continued to drop more for Indiana than for Arizona. As a result, by 2000, Arizona had a higher prevalence of traditional attitudes than Indiana by 6 percentage points. In a reversal, between 2000 and 2010, both states saw an increase in the prevalence of traditional attitudes; albeit the rise in the shares was greater in Indiana than in Arizona, and the gap in traditional attitudes between the two states narrowed to 4 percentage points. Between 2010 and 2016, the earlier pattern of decline in the prevalence of traditional attitudes resumed across Arizona and Indiana, but the gap grew to 9 percentage points.

Like Indiana and Arizona, in 1997, Minnesota and Wisconsin both had virtually the same share of traditional views on women working, 63 and 62 percent, respectively. But between 1977 and 1990, the prevalence of traditional attitudes in Minnesota dropped by 48 percentage points, while it fell by only 35 percentage points in Wisconsin. Thus, by 1990, Wisconsin was the more traditional of the two by 12 percentage points. Between 1990 and 2000, the prevalence of traditional attitudes stayed at 27 percent in Wisconsin, while Minnesota became even more egalitarian and the gap between the two states grew to 17 percentage points. Just as observed with Indiana and Arizona, between 2000 and 2010, the prevalence of traditional attitudes rose in both states, more in Minnesota than Wisconsin, and the gap fell

to 8 percentage points. Between 2010 and 2016, the earlier pattern of decline in traditional attitudes resumed for both states. The prevalence fell by 6 percentage points in Minnesota and 16 percentage points in Wisconsin, increasing the gap to 10 percentage points.

Therefore, there are differences in the rate of change in the prevalence of traditional attitudes across states, consistent with the existence of differential changes across states in the factors that affect attitudes toward women working and women's emotional suitability for politics. In the next chapter, I examine the causal effects of one such factor, labor demand changes.

2 THE IMPACT OF LOCAL LABOR DEMAND SHOCKS ON THE PREVALENCE OF TRADITIONAL ATTITUDES

2.1 Introduction

Women's participation in the labor force and politics has risen substantially over the past 50 years, driven in part by changes in attitudes about the appropriateness or suitability of women for these roles. Since 1977, the percent of women taking part in the U.S. civilian labor force rose by 10.3 percentage points (U.S. Bureau of Labor Statistics, 2020, LNS11300002). In politics, the share of women serving in Congress grew from 3.6 percent in the 94th Congress (1975-1977) to a record high of 24.7 percent in the current 116th Congress (History, Art Archives, U.S. House of Representatives, 2020).

This large compositional change in the U.S. labor force has been mirrored in the evolution of attitudes toward the appropriate roles for women. These changes since 1977 have been accompanied by a significant transformation in traditional attitudes toward the appropriateness of women's non-domestic roles and their suitability both for the workplace and positions of power per Chapter 1. Between 1977 and 1990, Chapter 1 documented sharp declines of 27 and 18 percentage points in the prevalence of traditional attitudes toward women working and women's emotional suitability for politics, respectively.

At the same time, a plateau in the labor market between 1990 and 2010 coincided with a plateau in traditional attitudes toward gender roles. Chapter 1 showed that between 1990 and 2010, the prevalence of traditional attitudes toward women working and women's emotional suitability for politics dropped by 1 and 3 percentage points, respectively.

The correlation in the changes in gender-role attitudes and the increase in the percentage of women participating in the U.S. labor force, does not identify a causal effect of labor market conditions on attitudes, which is the primary objective of this chapter. This chapter investigates how labor demand changes, specifically between 1977 and 2016, have affected the prevalence of traditional attitudes toward women working and women's emotional suitability for politics.

Ex ante the direction of the effects of labor demand changes on the prevalence of traditional gender attitudes is unclear. Becker (1981) argues that gender-role specialization can maximize joint household utility. Although in Becker's model women have a comparative advantage in home production and men's comparative advantage is as primary income earner, changes in labor demand as well as changes in women's educational attainments - including the returns on education investment - can alter these comparative advantages. An increase in the demand for women would be expected to reduce the comparative advantage for men in paid employment and increase the household's opportunity cost of adhering to traditional gender roles as

households can materially benefit from women's outside employment. This resultant increase in opportunity cost of traditional gender-role attitudes would unambiguously move preferences toward egalitarian gender attitudes. However, there is the potentially offsetting effect of men wanting to maintain their breadwinner position and view women's paid employment as a threat (Zuo and Tang, 2000).

Nonetheless, as women continue to attain more education, offsetting their rising comparative advantage in the workforce becomes more costly. Prior to 1980, U.S. men outnumbered women in both college attendance and graduation rates. In 1960, the ratio of men to women enrolled in undergraduate programs was 1.55 and the graduation ratio was 1.60. By 1980, women had caught up with men and the gender gap in education had disappeared. Instead of slowing down after reaching undergraduate enrollment parity, women reversed the gender gap and overtook men in college graduation rates. In 2003, the ratio of women to men graduating from a four-year college in the U.S. was 1.30 (Goldin, Katz, and Kuziemko, 2006). In addition to this growing trend in educational attainment, the return to postsecondary education also rose between 1973 and 2005 (Lemieux, 2006). These trends together with changes in labor demand can potentially increase women's household income contribution and alter the comparative advantages of men's and women's traditional roles.

To study the impacts of labor demand changes on gender attitudes, I combine three different survey datasets: the U.S. decennial Census, the American Community Survey (ACS), and the General Social Survey (GSS). The U.S. decennial Census and the ACS provide information on the labor market, and the GSS provides information on gender attitudes. To measure gender attitudes, as described in Chapter 1, I focus on two GSS questions: Is it better for everyone involved, if men work outside the home and women tend home? Are men better suited emotionally for politics than women? Both questions, asked periodically between 1977 and 2016, are about women's roles and capabilities in the public sphere rather than within the household. Each question captures a different aspect of attitudes toward gender roles. While the former refers to appropriateness of women working outside of the home, the latter refers to women's capabilities.

Although previous work measures gender attitudes by combining various gender questions and forming an index (Charles, Guryan and Pan, 2018), I use each question independently as a separate measure of gender attitudes. The advantages of not constructing an index are twofold. First, it allows for a degree of variation in traditional attitudes across the two questions. Believing women should stay at home is not necessarily a rejection of women's capabilities, emotional or otherwise. Second, it allows for labor demand shocks to affect the prevalence of traditional attitudes toward women working and women's emotional suitability for politics differently.

Identifying the extent to which the changes in the prevalence of traditional gender attitudes are driven by labor demand shifts is complicated. The complexity is due to the bi-directorial relationship between gender attitudes and measures of labor demand changes such as employment, and the employment to population ratio and, to some degree, the difficulty in identifying the determinants of gender attitudes in general. The simultaneity occurs because employment and the employment to population ratio are determined by government policies and labor demand and supply factors that are susceptible to the influence of gender attitudes.

I address the simultaneity via Bartik (1991) by focusing on within state variation over time in labor demand. Bartik shocks are calculated by interacting state level industry shares with national average growth rates for each industry. By conditioning on industry-specific equilibrium levels of employment in a base year and replacing national industry growth for local growth, Bartik shocks isolate exogenous variation due to labor demand. Consistent with previous work (Autor and Duggan, 2003; Charles, Hurst, and Notowidigdo, 2012; Chauvin, 2017), each state is excluded from contributing to the national industry growth calculations of its Bartik shocks, which further weakens the link between local gender attitudes and demand-driven predicted changes in employment. Leveraging Bartik at the state level, I also include state fixed effects to account for fixed differences across states.

Another critical issue is the proper specification of dynamics regarding the evolution of gender attitudes. If gender attitudes gradually change over time as cohorts exit and new cohorts enter the data, then without a lagged dependent variable the relationship between gender attitudes and labor market conditions would be mis-specified. Inclusion of prior gender attitudes as a control variable allows for a flexible specification of a cohort's attitude evolution over time. Therefore, in some specifications I include a lagged dependent variable. However, including a lagged dependent variable with state fixed effects introduces an endogeneity that I address using an instrumental variables approach.

My results suggest that the effects of demand shocks on attitudes toward women working may differ from attitudes toward suitability for politics. I find significant declines in the prevalence of traditional gender attitudes on women working outside of home in response to favorable labor demand shocks. Specifically, I find that a one percent increase in Bartik shocks lowers the prevalence of traditional attitudes by 0.45 percentage points, significant at the 10 percent level. In contrast, regarding a woman's emotional suitability for politics, I find that a one percent increase in Bartik shocks increases the prevalence of traditional attitudes by 0.43 percentage points in the preferred specification with an instrumented lagged dependent variable. However, the imprecision of the estimate, despite substantial variation within individual

states over time, precludes inference about the true effect of labor demand changes on views toward a woman's emotional suitability for politics.

I subsequently explore whether there are differences by demographic characteristics in the responses to labor demand shocks. Demographic characteristics, such as gender, education, marital status, and age would be expected to affect how people experience and respond to labor demand changes. As such, I examine how Bartik shocks affect traditional attitudes toward women working and women's emotional suitability for politics separately among married men and women, as well as men and women with less than a high school diploma, high school diploma and some college, or at least a baccalaureate. I also examine how Bartik shocks affect traditional gender attitudes among respondents aged 18-47 and 48-70.

Although the interpretation and comparison of effects of labor demand shocks on traditional attitudes toward women working and women's emotional suitability for politics across various subpopulations is less informative, given the noisy point estimates, I find, from the preferred specification with instrumented lagged dependent variable, that a one percent increase in Bartik shocks increases the prevalence of traditional attitudes toward the emotional suitability of women for politics by 1.28 percentage points, significant at the 10 percent level, among men with less than a high school diploma. This statistically significant positive effect of Bartik shocks among men with less than a high school diploma suggests that the threat to their

breadwinner status is a more powerful mechanism for lower educated men than the increase in the opportunity cost of adhering to these traditional attitudes toward the emotional suitability of women for politics.

In addition, I examine the sensitivity of the estimated results to group specific Bartik shocks. It is possible that each subpopulation is more concerned about the relevant labor market changes for their own group than the broader labor market conditions; thus, examining the contribution of group specific Bartik shocks to the prevalence of traditional attitudes among a specific group may be more pertinent than the overall Bartik shocks. The smaller magnitude of the attitudinal responses of men with less than a high school diploma toward women working to overall Bartik shocks (a 0.18-percentage point decline, estimated imprecisely), compared to the magnitude of responses to their group specific Bartik shocks (a 6.45-percentage point decline, significant at the 10 percent level) suggests that men with less than a high school diploma are potentially more responsive to Bartik shocks specific to their group.

Similarly, regarding attitudes toward the emotional suitability of women for politics, despite finding no statistically significant evidence of the influence of the overall Bartik shocks on the prevalence of traditional gender attitudes among men with at least a baccalaureate, I find that a one percent increase in group specific Bartik shocks lowers the prevalence of traditional attitudes among them by 0.53 percentage points, significant at the 5 per-

cent level. However, gender and education specific Bartik shocks are highly correlated, and without controlling for Bartik shocks across all the other gender-education subgroups, the effects of group specific Bartik shocks are only suggestive of higher sensitivity of some subpopulations to own-group specific market conditions.

The remainder of the chapter proceeds as follows. Section 2.2 describes the data and presents trends in labor market participation among some subpopulations; Section 2.3 introduces the empirical framework, discusses identification concerns and presents reduced form models that address those concerns. Section 2.4 describes the main estimates. Section 2.5 presents estimates from heterogeneity analyses. Section 2.6 evaluates the sensitivity of the main estimates in Section 2.4 across subpopulations. Section 2.7 presents concluding remarks.

2.2 Data and Labor Market Trends

I use three survey data. The data to measure gender attitudes comes from the General Social Survey (GSS) (see Chapter 1 for details), while the data to estimate labor market conditions comes from the decennial Census and the American Community Survey (ACS). Below I describe the Census and the ACS data and then describe trends in labor market participation by gender, gender and education, and gender and marital status.

2.2.1 The Decennial Census and The ACS

I use individual-level decennial Census extracts that correspond to one percent of the population in 1970 and five percent of the population in 1980, 1990, and 2000, and single-year ACS data in 2010 and 2016 available at the Integrated Public Use Microsamples (IPUMS) database (Ruggles et al., 2018). The baseline data are restricted to non-institutionalized civilian adults between 18 to 70 years old (inclusive) living in U.S. states. Both the decennial Census and the single-year ACS data are nationally representative and have information on employment status, industry, and demographics such as sex, age, and education. An important advantage of the large samples available in the Census and the ACS data is the possibility of creating reliable measures of the state-level changes in employment that can be disaggregated at gender and education levels.

The 1970 data are used to anchor the industry share component of Bartik shocks. To match the data availability in the GSS, I use linear interpolation of employment numbers by industry and population from the 1970 to 1980 censuses to estimate employment and population estimates in 1977. To ensure that no one industry drives national industry growths in local labor

demand shocks (more on this in the next section), I define 14 broad industry categories based on 3-digit industry schemes in 1990[6] .

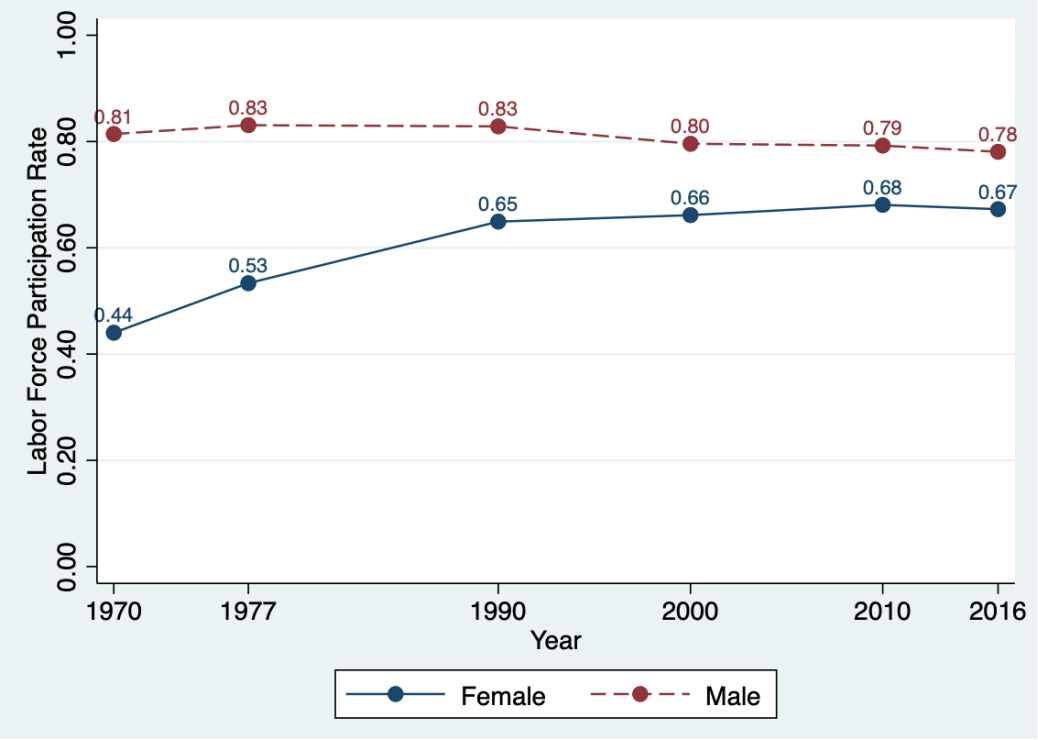
2.2.2 Trends in The Labor Force Participation

With the substantial decline in the prevalence of traditional attitudes toward women working and women's emotional suitability for politics, the labor force participation of women also changed significantly. Figure 10 plots the labor force participation of women and men, age 18 to 70 years old. Mirroring the sharp declines in traditional gender attitudes between 1977 to 1990, followed by a period of stagnation, and a continuation of the earlier pattern of decline between 2010 and 2016, the labor force participation of women grew rapidly between 1970 to 1990 and then stabilizes after. In the meanwhile, the labor force participation of men remained fairly stable until 1990 and then began to fall ever so slightly.

Figure 11 disaggregates the trends in labor force participation of men and women by education. The figure shows that the labor force participation has increased for women of all education level, while it has decreased for men of all education level. However, the growth in labor force participation

[6]The set of industries include: 1. Agriculture, 2. Mining, 3. Construction, 4. Manufacturing, 5. Transportation, 6. Communication, Utilities and Sanitation, 7. Wholesale Trade, 8. Retail Trade, 9. Finance, Insurance, and Real Estate, 10. Business and Repair Services, 11. Personal Services, 12. Entertainment and Recreation, 13. Professional and Related Services, 14. Public Administration.

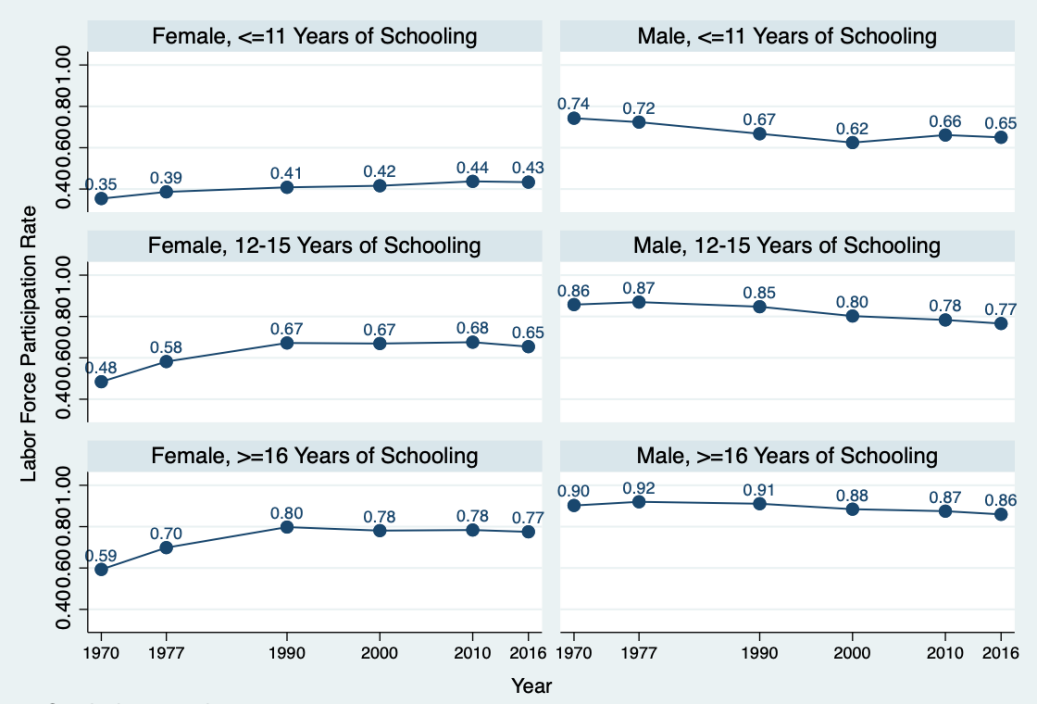
Figure 10: Trends In Labor Force Participation by Gender



Data Source: The decennial Census and the American Community Survey.

Notes: The labor force participation rates are estimated for samples of men and women aged 18 to 70, as the number of people in the civilian labor force divided by the civilian population. The labor force participation rate in 1977 is interpolated using the data in 1970 and 1980.

Figure 11: Trends in Labor Force Participation by Gender and Education



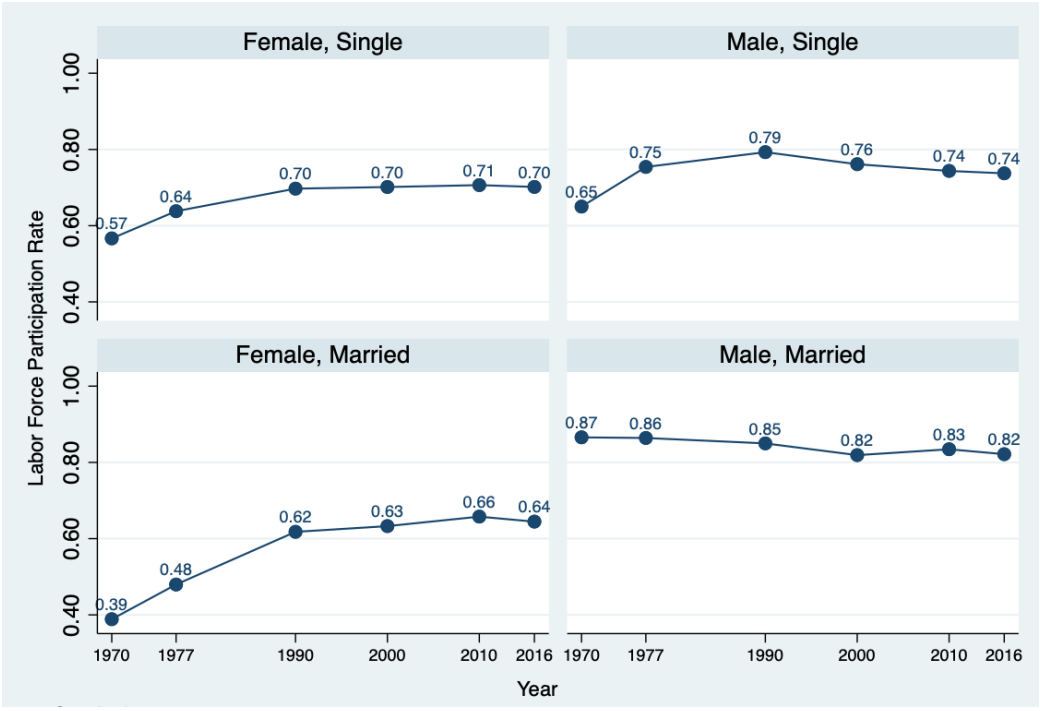
Data Source: The decennial Census and the American Community Survey.

Notes: The labor force participation rates are estimated separately for samples of men and women aged 18 to 70, with less than or equal to eleven year of schooling, twelve to fifteen years of schooling, and at least sixteen years of schooling. The labor force participation rate for women with less than or equal to eleven years of schooling is defined as the number of women with less then or equal to eleven years of schooling in the civilian labor force (employed or unemployed) divided by the total number of civilian female population with less than or equal to eleven years of schooling. The labor force participation rate among all the other gender and education sub-populations are defined similarly. The labor force participation rate in 1977 is interpolated using the data in 1970 and 1980.

of women with at least sixteen years of education at 18 percentage points, and women with twelve to fifteen years of schooling at 17 percentage points, have been more than twice the 8 percentage point increase in the labor force participation rate of women with less than or equal to eleven years of schooling. The higher prevalence of traditional attitudes among lower educated women documented in Chapter 1 is, therefore, consistent with the lower labor force participation rate among them.

Figure 12 illustrates the labor force participation rates by gender and marital status. It shows that the labor force participation rate of married women has increase more than the labor force participation rate of single women. Between 1970 and 2016, the labor force participation of the former grew by 25 percentage points, while the labor force participation rate of the latter rose by only 13 percentage points. By contrast, the labor force participation rate of married men appears very stable with only a 5 percentage point decline during this time. Interestingly, the labor force participation rate of single men like their female counterparts grew between 1970 and 2016, but not as much. The trends in Panel (a) of Figure 7 in Chapter 1, where the prevalence of traditional attitudes toward women working is highest among married men is consistent with these labor force participation rate trends.

Figure 12: Trends in Labor Force Participation by Gender and Marital Status



Data Source: The decennial Census and the American Community Survey.

Notes: The labor force participation rates are estimated separately for single and married men and women between 18 to 70 years old. Single is defined as divorced, separated, widowed, and never married. Married is defined as currently married regardless of whether the spouse is present. The labor force participation rate for "Female, Single" is calculated as the number of single women in the labor force divided by the total civilian population of single women in each year. The labor force participation rates for married women, single men and married men are calculated similarly. The labor force participation rate in 1977 is interpolated using the data in 1970 and 1980.

2.3 Empirical Framework

Estimating the causal effect of labor demand changes on gender attitudes is difficult. The difficulty is rooted in the simultaneity between measures of labor demand shifts, such as changes in employment or the employment to population ratio, and gender attitudes and the dynamics of gender attitudes. Employment and the employment to population ratio are determined by a combination of government policies and supply and demand factors, which are likely influenced by prevailing gender attitudes; therefore, hampering efforts to isolate exogenous variation in labor demand shifts. Also, if gender attitudes are dynamic and a function of prior attitudes, omission of a lagged dependent variable in a statistical model would lead to a specification error. However, the inclusion of a lagged dependent variable in a fixed effects model, introduces a mechanical endogeneity. The two primary difficulties of estimating the causal effect (the simultaneity between labor market and gender attitudes, and the dynamics of attitudes) are now discussed in detail.

2.3.1 Simultaneity

There are many ways gender attitudes could affect labor supply and demand decisions, which complicate efforts to identify the effects of labor demand changes on attitudes. Traditional gender-role views may discourage some women from working outside of the home as they try to align their

actions with society's beliefs, encountered in both childhood and adulthood. Traditional gender-role views may also shape upbringing and teaching practices used by parents and teachers who try to prepare girls to meet society's expectations. If not through this self-fulfilling prophecy, gender attitudes could also restrict labor supply of women by increasing the cost of labor force participation of women relative to men. The prevalence of unfavorable views toward women working outside of the home may require women to invest in more skills to compete with men. Gender-role attitudes could also affect labor demand decisions. Traditional views may result in prejudice and discrimination against women in hiring practices, despite anti-discrimination laws. The effects of gender attitude on employment through these mechanisms or others impede estimating exogenous variation in labor demand changes, which would be imperative for identifying the causal effects on gender attitudes.

2.3.2 Exogenous Variation in Labor Demand

Following a widely used practice across different fields, I address the simultaneity between labor market conditions and gender attitudes by estimating predicted local labor demand, known as Bartik shocks or shift-share instruments, based on Bartik (1991). Bartik shocks are calculated by interacting a state's industry shares with national average industry growth rates.

Formally, I calculate Bartik shocks as follows:

$$Bartik_{gest_0-t} = \left[\sum_j \frac{EMP_{jgest_0}}{EMP_{gest_0}} * \left(\frac{EMP_{-sjget} - EMP_{-sjget_0}}{EMP_{-sjget_0}}\right)\right] \quad (1)$$

where $Bartik_{gest_0-t}$ represents exogenous demand-driven employment growth between base year $t_0 = 1977$ and end year $t = \{1990, 2000, 2010, 2016\}$ in state s for gender $g = \{$female, male, all$\}$ and education group $e = \{\leq 11, 12-15, \geq 16$years of schooling, all$\}$[7]. The first component of the interaction term $\frac{EMP_{jgest_0}}{EMP_{gest_0}}$ represents employment share of industry j in state s, at base year t_0 for gender g, and education group e. The second component of the interaction $\frac{EMP_{-sjget} - EMP_{-sjget_0}}{EMP_{-sjget_0}}$ represents the national employment growth of gender g and education group e, in industry j between $t - t_0$ that excludes state s per the leave-one-out approach used in literature (Autor and Duggan, 2003; Charles, Hurst, and Notowidigdo, 2012; Chauvin, 2017). In the leave-one-out approach to national employment growth estimation, own-state is excluded from national industry-specific employment growth calculations.

The intuition for exogeneity of Bartik shocks is that state-level employment growth (E_s) can be written as weighted averages of state-level industry-

[7]When Bartik shocks are for the entire local population, $g =$ all and $e =$ all. For sex-specific shocks, g is either female or male, and $e =$ all. For education-specific shocks, $g =$ all and e is either education levels less than high school, high school or some college, or with at least a baccalaureate.

specific employment growth rates ($E_s = \sum_{j=1}^{J} \omega_{js} E_{js}$), where ω_{js} are industry j shares in state s and E_{js} are industry-specific growth rates in state s; but these local industry growth rates are influenced by local industry-specific labor demand and labor supply shocks. By replacing local industry growth rates by national rates, excluding own state from contributing to estimation of national industry rates, and conditioning on a lagged local supply shocks (i.e. local employment distribution across industries in a base year), Bartik shocks are understood to provide exogenous demand-driven shocks that vary across local markets based on their base-year distribution of local industries. As noted in the literature (Maestas, Mullen, and Powell, 2013; Schaller, 2016), using broad industry definitions in constructing industry-specific national growth rates further serves to ensure that national rates are not correlated with state-level supply factors because employment would not be concentrated in one industry and state, and there would be sufficient cross-sectional variation in base-year industry composition.

Emerging literature formalizes the exogeneity assumptions in the context of Bartik shocks in terms of either industry shares (Goldsmith-Pinkham, Sorkin and Swift, 2018) or growth rates (Borusyak, Hull, and Jaravel, 2018). I discuss the plausibility of conditionally random assignment of national industry-specific growth rates in Section 2.4.1, based on summarizing the distribution of national industry growth rates, industry-level shares, and Bartik shocks.

To gain a better understanding of the magnitude of the effects of Bartik shocks on employment, I estimate the following first-stage relationship between Bartik shocks and changes in employment, while controlling for state (λ_s) and year (γ_t) fixed effects:

$$\ln employment_{st} = \alpha + \beta Bartik_{st} + \lambda_s + \gamma_t + \epsilon_{st} \tag{2}$$

2.3.3 Gender Attitude Dynamics

There are many reasons to think gender attitudes evolve over time, including the recognition that attitudes are learned beliefs through socialization and interactions with others, the replacement of older cohorts by younger cohorts, and so on. As such, even despite the exogeneity of Bartik shocks, failure to appropriately account for dynamics via a lagged dependent could introduce specification error. However, including a lagged dependent variable with state fixed effects introduces a mechanical relationship between the demeaned lagged dependant variable and the demeaned error term as shown by Nickell (1981).

I include lagged gender attitudes in some specifications and examine the sensitivity of results. Following Wooldridge (2006), I address this endogeneity using instrumental variables approach that is described in detail below.

2.3.4 Empirical Models

In this section, I describe my approach to estimating the effects of labor market conditions on gender attitudes and addressing complications introduced by simultaneity and dynamics discussed above. Using panel data, I estimate fixed effects, dynamic fixed effects, and dynamic fixed effects with instrumental variables models.

Equation (3) is the baseline regression specification for estimating the impact of labor demand changes on changes in the prevalence of traditional gender attitudes. Here, share of traditional gender attitudes (Y) for agreeing or strongly agreeing that women should tend home or agreeing that women are not emotionally suited for politics in state s at time t is modeled as a function of labor demand changes, characteristics of respondents and state, and state and time fixed effects:

$$Y_{st} = \alpha + \beta T + \delta X_{st} + \lambda_s + \gamma_t + \epsilon_{st} \tag{3}$$

where T represents one of the following treatment variables: natural log of employment, the employment to population ratio, or Bartik shocks anchored in 1970. λ_s and γ_t are state and year fixed effects, respectively. X_{st} is a vector of respondent or state s characteristics in year t, and includes shares of respondents to gender attitude questions who are female, respondents who

have less than or equal to eleven years of schooling, respondents who have twelve to fifteen years of schooling, and natural log of population when the treatment variable is not the employment to population ratio. ϵ_{st} is the error term.

Equation (3) highlights a key identification concern regarding the dynamics of gender attitudes. If gender attitudes evolve over time, and people only partially update their a priori beliefs as new labor demand shocks are realized, it is important to account for this dynamic. Equation (4) expands equation (3) by including the lagged dependent variable:

$$Y_{st} = \alpha + \beta T + \theta Y_{st-1} + \delta X_{st} + \lambda_s + \gamma_t + \epsilon_{st} \tag{4}$$

Where Y_{st}, T, and X_{st} are dependent, treatment and control variables as defined in equation (3). Y_{st-1} is the lagged share of traditional gender attitudes in state s and lagged year $t-1$ When t is 2000, the lagged year is 1990; when t is 2010, the lagged year is 2000, and when t is 2016, the lagged year is 2010.

The lagged dependent variable allows for gender attitudes to change with partial persistence. Partial persistence in gender attitudes is reasonable given that survey population is aging, and attitudes evolve over time. Older cohorts exit and are replaced by younger cohorts with different gender views.

However, when a lagged dependent variable is included in a fixed effects model, Nickell (1981) shows that the demeaning process creates a correlation between the demeaned lagged dependent variable and the demeaned error term that biases the coefficient of the lagged variable. Nickell shows that this bias may be sizable in settings with small time periods. Among the solutions described in Wooldridge (2006) is creating instruments from second or third lags of the dependent variable either in the form of lagged differences or levels. I use the second lag of the dependent variable as an instrument; with only 5 time period observations per state, using third or fourth lags as instruments is not feasible.

The validity of this instrumental variables approach depends on the plausibility of the exclusion restriction assumption that twice-lagged prevalence of traditional attitudes only affects the current prevalence of traditional attitudes through the prevalence of lagged traditional attitudes. In order words, traditional attitudes in 1977 only affect traditional attitude formation in 2000 through their effects on traditional attitudes in 1990. If one is willing to maintain this exclusion restriction assumption, then, formally, the second stage (5a) and first stage (5b) models are:

$$Y_{st} = \alpha_1 + \beta T + \theta \widehat{Y_{st-1}} + \delta X_{st} + \lambda_s + \gamma_t + \epsilon_{st} \tag{5a}$$

$$Y_{st-1} = \alpha_2 + \theta_2 Z_{st} + \beta_2 T + \delta_2 X_{st} + \lambda_s + \gamma_t + \nu_{st} \tag{5b}$$

$Y_{(st-1)}$ is the lagged share of traditional gender attitude that is instrumented for by a twice-lagged share of traditional gender attitudes $Z_{st} = Y_{st-2}$. All other terms are as defined previously.

2.4 Results

In this section, I first describe properties of national industry growth rates, industry shares, and Bartik shocks, which are important for the interpretation of regression results. Then I proceed to present findings from regression models discussed in the previous section.

2.4.1 Properties of Bartik Shocks and Its Components

Table III presents summary statistics (mean, standard deviation, and interquartile range) for the national industry growth rates across 70 industry-by-period observations in a similar manner to Borusyak, Hull, and Jaravel (2018). The objective of the table is to assess whether the average industry growth rates depend on industry shares in the base year or industry unobservables. That is, whether the same average growth rate is realized regardless of which industries are included in the sample.

Column (1) reports an average growth rate of 0.77, with a standard deviation of 0.85 and an interquartile range of 1.03. All the statistics are weighted

TABLE III: GROWTH RATES SUMMARY STATISTICS

	(1)	(2)	(3)
Mean	0.767	0.515	0
Standard deviation	0.848	0.583	0.558
Interquartile range	1.027	0.854	0.735
Specification			
Excluding service industries		Yes	Yes
Residualized on industry-by-period FE			Yes
Largest industry shares in base year			
Across industries	0.269	0.269	0.269
Observation counts			
N of industry-by-period shocks	70	55	55
N of industries	14	11	11
N of periods	5	5	5

Notes: This table summarizes the distribution of industry-specific national growth rates across 14 industries and 5 time periods, in a similar manner to Borusyak, Hull, and Jaravel (2018). All statistics are weighted by the average industry shares in the base year of 1970. Column (1) includes all industry growth rates, while Columns (2) and (3) restrict to non-service industries (i.e. Business and Repair Services, Personal Services, and Professional and Related Services are excluded). Column (3) also residualizes on time period indicators. The largest industry shares in the base years are also reported.

by the average national industry shares in 1970, the base year. The largest industry share in the base year is 0.27. The relatively small largest industry share together with reported mean, standard deviation and interquartile range suggest moderate industry-level variation.

To assess the contribution of service industries to the distribution of industry growth rates and the correlation across industry growth rates, Column (2) reports mean, standard deviation, interquartile range and the largest industry share in 1970 excluding service industries, which correspond to 3 industry groups: Business and Repair Services, Personal Services, and Professional and Related Services. Column (2) reports an average growth rate of 0.52 across the remaining 11 industries with a standard deviation of 0.58 and an interquartile range of 0.85. Not surprisingly, excluding 3 industry groups lowers the industry-level variation but not a lot. This suggests that service industries do not account for a large share of total employment, and industry growth rates are only weakly correlated. With and without service industries, the largest industry share is still 0.27 across industries in the base year, which also suggests a moderate level of variation despite the aggregate nature of industry groups used.

So far, the distribution of national industry growth rates has been reasonably consistent with the two assumptions regarding quasi random assignment of industry growth rates discussed in Section 2.3.2. With national industry growth shocks measured across 5 time periods, it is also important to assess the level of variation in national industry growth rates within periods. Column 3 reports standard deviation and interquartile range for residuals obtained from regressing industry growth rates on period indicators and weighting by industry shares in the base year. The standard deviation and

interquartile means in Column 3 are only slightly smaller than in Column 2, suggesting moderate variation in residual industry growth rates necessary for satisfying the two assumptions for quasi-random assignment of growth rates.

Table IV summarizes Bartik shocks across states to see how the variation across industry groups translate to variation across states. However, it is important to note that, as detailed in Section 2.3.2, the national industry-specific growth rates that contribute to the construction of Bartik shocks follow the leave-one-out approach; whereas for the purposes of assessing variation in growth rates across industry and time periods, the growth rates summarized in Table III were transformed to industry-period level and were not estimated using the leave-one-out method.

Column (1) of Table IV shows the raw variation in Bartik shocks with a mean of 0.76, standard deviation of 0.32, and interquartile range of 0.44. Columns (2)-(4) add controls that identical to the regression specifications, except for controls that were used because of possible correlation with the dependent variable such as lagged dependent variables or share of respondents with certain years of schooling. Column (2) residualizes Bartik shocks on end-of-period natural log of population, while Column (3) residualizes on end-of-period natural log of population and time period fixed effects, and Column (4) residualizes on end-of-period natural log of population, time period fixed effects, and state fixed effects. With each added control, the residual variation in Bartik shocks falls. Relatively small variation is left in

TABLE IV: BARTIK SHOCKS SUMMARY STATISTICS

	(1)	(2)	(3)	(4)
Mean	0.761	0	0	0
Standard deviation	0.320	0.306	0.086	0.039
Interquartile range	0.440	0.435	0.096	0.040
Controls				
ln(*Population*)		Yes	Yes	Yes
Period FE			Yes	Yes
State FE				Yes
Observation counts				
N of state-by-period	135	135	135	135
N of states	27	27	27	27
N of periods	5	5	5	5

Notes: This table summarized the distribution of Bartik shocks across states and time periods, in a similar manner to Borusyak, Hull, Jaravel (2018). Bartik shocks are constructed by interacting local industry shares in a base year with national industry-specific growth rates as described in the text. Columns (2)-(4) residualizes the Bartik shocks on natural log of population (end-of-period), period and State fixed effects.

Column (4) with standard deviation of 0.04 and interquartile range of 0.04. Although, a small residual variation in Bartik shocks limits the predictive power of the shocks, especially since Bartik shocks are also anchored in the lagged period of 1970, the leftover variation is plausibly clean.

2.4.2 Regression Findings

This section reports the results of regressions discussed in section 2.3.4 on the effects of labor demand changes on the prevalence of traditional gender attitudes. Specifications differ on whether they measure labor demand changes with natural log of employment, the employment to population ratio or Bartik shocks to illuminate the importance of properly accounting for the reverse effects of gender attitudes on employment or the employment to population ratio. Specifications also differ on whether they include a lagged dependent variable, and on whether the approach uses an instrumental variable for the lagged dependent variable. The inclusion of prior gender attitudes in some specifications provides information about the dynamics of gender attitudes. All specifications are weighted by the average number of respondents per state, and therefore, states with higher number of observations contribute more to the estimates. All specifications also control for shares of respondents who are female, shares with less than or equal to eleven years of schooling, shares with twelve to fifteen years of schooling, and natural log of population when the treatment variable is not the employment to population ratio.

Table V presents coefficients that capture the effects of labor demand changes (measured in the three different ways) on the share of traditional views (agreements or strong agreements) that women should tend home.

TABLE V: FIXED EFFECTS ESTIMATES OF LABOR DEMAND CHANGES ON TRADITIONAL ATTITUDES TOWARD WOMEN WORKING

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ln *Employment*	-0.678**			-0.399			-0.351		
	(0.285)			(0.329)			(0.289)		
Employment to Population Ratio		-0.956**			-0.620			-0.569	
		(0.391)			(0.493)			(0.439)	
Bartik Shocks			-0.002			-0.505*			-0.454*
			(0.137)			(0.285)			(0.252)
Lagged Dependent Variable				-0.209*	-0.210*	-0.212**	0.219	. 0.234	0.220
				(0.106)	(0.107)	(0.105)	(0.197)	(0.201)	(0.198)
Instrumental Variable	No	No	No	No	No	No	Yes	Yes	Yes
N	135	135	135	81	81	81	81	81	81

Notes: The unit of observation is state by year. The outcome variable is the number of agreements or strong agreements divided by the total number of responses to the statement that it is better for men to work and women tend home. ln *Employment* is the natural logarithm of number of employed persons. Employment To Population Ratio is the number of employed divided by population. Bartik Shocks measure changes in predicted labor demand by interacting state-specific industry shares in 1970 by national industry growths between 1970-1977, 1970-1990, 1970-2000, 1970-2010, 1970-2016. All specifications include state and year fixed effects and are weighted by the average number of respondents per state. The sample is restricted to respondents aged 18-70. Standard errors are reported in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Estimates in columns (1)-(3) are from a basic specification without controlling for prior gender attitudes; columns (4)-(6) control for prior gender attitudes by including a lagged dependent variable; columns (7)-(9) instrument for last period's gender attitudes using gender attitudes from two periods ago. All columns include state and year fixed effects to deal with time-invariant state-specific characteristics and aggregate differences across time that might influence both labor demand changes and gender attitudes.

Columns (1) and (2) of Table V show the effect of natural log of employment and the employment to population ratio on the share of traditional views that women should tend home, respectively. In Column (1), I find an effect of -0.68, significant at the 5 percent level. This indicates that a one percent change in natural log of employment leads to a 0.68-percentage point decline in the prevalence of traditional attitudes on women working outside of the home. In Column (2), I find that a one percent change in employment to population ratio lowers the prevalence of traditional attitudes by 0.96 percentage points, also significant at the 5 percent level.

An important limitation of employment and the employment to population ratio as measures of labor demand changes is that they are driven by both supply and demand forces. To address this concern, Column (3) studies the effect of Bartik shocks. One important conceptual point to keep in mind when interpreting the coefficient on the Bartik shock is that changes in predicted employment do not map one to one to actual changes in employment.

Thus, the effect of a one-unit change in a Bartik shock is different from a one-unit change in employment, although it is fairly close. To help make comparisons across employment and Bartik shock estimates, Table XXV, Appendix A shows that a one percent increase in Bartik shocks increases the natural log of employment by 0.78 percentage points, significant at the 1 percent level, suggesting that estimates are comparable in terms of magnitude.

Although Column (3) of Table V shows that the coefficient on the Bartik shock is practically zero and extremely noisy, the more rigorous specifications in Column (6) and Column (7) consistently report coefficients of -0.51 and -0.45, respectively, significant at the 10 percent level. This implies that a one percent increase in a Bartik shock leads to about 0.45 to 0.51 percentage points decline in the share of traditional attitudes. Unfortunately, comparing the Bartik estimates to employment and the employment to population coefficients is not informative about the possible endogeneity bias from the effects of traditional gender attitudes on labor supply, given the imprecision of coefficients for employment and the employment to population ratio. Even if point estimates are taken as the best guess of the impacts of employment and the employment to population ratio on the prevalence of traditional attitudes toward women working, there is no compelling evidence of endogeneity bias. The Bartik coefficients are smaller (larger in absolute values) than the employment coefficients and larger (smaller in absolute values) then the employment to population coefficients.

Despite a smaller sample size of 81 versus 135 observations, the estimated effects of Bartik shocks are only statistically significant in specifications that allow for flexible persistence of prior traditional gender attitudes through a lagged or an instrumented lagged dependent variable. Even the employment and the employment to population estimates that are noisy become less sensitive to specifications and more stable across dynamic models of traditional attitudes with a lagged dependent variable and an instrumented lagged dependent variable. The specification with lagged dependent variable (columns (4)-(7)) would have more observations than the specification where the lagged variable is instrumented, but for comparability of the estimates, the former specification is restricted to the same observations available in the latter model.

Table VI presents coefficients of labor market conditions on share of traditional views (agreements) that women are not emotionally suited for politics with the same structure as Table V. All the coefficients are positive, and none are significant at conventional levels of 1, 5 or 10 percent. Therefore, the hypothesis that labor demand has no effect on attitudes cannot be rejected at any conventional level regardless of the specification.

TABLE VI: FIXED EFFECTS ESTIMATES OF LABOR DEMAND CHANGES ON TRADITIONAL ATTITUDES TOWARD THE EMOTIONAL SUITABILITY OF WOMEN FOR POLITICS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ln *Employment*	-0.181			-0.210			0.033		
	(0.264)			(0.331)			(0.430)		
Employment to Population Ratio		-0.229			-0.295			0.050	
		(0.362)			(0.518)			(0.649)	
Bartik Shocks			0.173			0.034			0.427
			(0.131)			(0.295)			(0.492)
Lagged Dependent Variable				-0.261**	-0.224*	-0.253**	0.711	0.736	0.999
				(0.114)	(0.119)	(0.115)	(0.650)	(0.645)	(0.837)
Instrumental Variable	No	No	No	No	No	No	Yes	Yes	Yes
N	135	135	135	81	81	81	81	81	81

Notes: The unit of observation is state by year. The outcome variable is the number of agreements divided by the total number of responses to the statement that women are not emotionally suited for Politics. ln *Employment* is the natural logarithm of number of employed persons. Employment To Population Ratio is the number of employed divided by population. Bartik Shocks measure changes in predicted labor demand by interacting state-specific industry shares in 1970 by national industry growths between 1970-1977, 1970-1990, 1970-2000, 1970-2010, 1970-2016. All specifications include state and year fixed effects and are weighted by the average number of respondents per state. The sample is restricted to respondents aged 18-70. Standard errors are reported in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.5 Heterogeneity Analysis

As shown by Figures 3-7 in Chapter 1, innate characteristics, such as age and gender, and acquired characteristics such as education and marital status are associated with differences in gender attitudes. Differences in these observable characteristics may partly reflect differences in socioeconomic positions and peers, and partly reflect differences in unobservables, such as identity, which are all potentially linked to differences in gender-role attitudes.

Marriage, for example, may alter views on gender roles as men and women transition into their roles as husbands and wives and need to adopt a division of labor, such as men as breadwinners and women as homemakers. The division of labor within marriage itself may alter views on gender roles. But also, evidence suggests that wives' paid employment affects men's mental health and psychological distress, which may very well influence gender-role attitudes (Kessler and McRae Jr., 1982; Syrda, 2020). Yet another possible link between marriage and gender attitudes can be through assortative mating decisions. Men and women may choose partners with similar characteristics and beliefs, which may further reduce any disagreements in attitudes between them by adopting some of their partner's views after marriage.

Therefore, I examine whether common labor demand shocks differentially affect the prevalence of traditional gender attitudes by gender and education

(Tables VII and VIII) by gender and marital status (Tables IX and X) and by age (Tables XI and XII).

Table VII shows the effects of labor demand shocks on traditional attitudes toward women working outside of the home for men (Panel (a)) and women (Panel (b)) with different education levels. The first three columns are for eleven or fewer years of schooling, the middle three columns are for twelve to fifteen years of schooling and the last three columns are for at least sixteen years of schooling. In each panel, comparisons across columns within education groups reveal sensitivity of estimates to specification. Comparisons of the same specifications across samples, reveal heterogeneous effects of Bartik shocks. Columns (1), (4), and (7) report coefficient of Bartik shocks from regressions without lagged dependent variables. Columns (2), (5) and (8) report coefficients from regressions with lagged dependent variables. Columns (3), (6) and (9) report coefficients from regressions with instrumented lagged dependent variables by twice-lagged dependent variables. The number of observations vary across specifications as a direct consequence of including a lagged dependent variable or instrumenting using twice-lagged dependent variable. Unlike the main results where I used balanced samples in the latter two specifications, samples are not balanced when disaggregated beyond state-year level due to the small number of observations.

TABLE VII: DIFFERENTIAL EFFECTS ON TRADITIONAL ATTITUDES TOWARD WOMEN WORKING OUTSIDE OF THE HOME BY GENDER AND EDUCATION

Panel (a)	$\leq$ 11 Years of Schooling, Men			12-15 Years of Schooling, Men			$\geq$ 16 Years of Schooling, Men		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bartik Shocks	-0.406	0.026	0.104	0.218	-1.195*	-0.715	-0.421	-1.016	-0.061
	(0.473)	(1.339)	(1.053)	(0.334)	(0.613)	(0.712)	(0.420)	(0.836)	(0.920)
Lagged Dependent Variable	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Instrumental Variable	No	No	Yes	No	No	Yes	No	No	Yes
N	127	71	68	135	81	81	130	79	75
Panel (b)	$\leq$ 11 Years of Schooling, Women			12-15 Years of Schooling, Women			$\geq$ 16 Years of Schooling, Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bartik Shocks	0.236	-0.675	-0.614	0.348	0.264	0.827	0.499	-0.227	-0.541
	(0.475)	(1.385)	(1.081)	(0.246)	(0.574)	(0.981)	(0.432)	(0.617)	(1.007)
Lagged Dependent Variable	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Instrumental Variable	No	No	Yes	No	No	Yes	No	No	Yes
N	133	77	76	135	81	81	127	78	71

Notes: The unit of observation is the state by year. The outcome variable is the share of traditional attitudes, defined as the number of agreements or strong agreements divided by the total number of responses to the statement that it is better for men to work and women tend home, for specific gender-education level characteristics. In Panel (a), the outcome variable is calculated among men with less than or equal to eleven years of schooling, men with twelve to fifteen years of schooling, and men with sixteen or more years of schooling. In Panel (b), the outcome variable is calculated among women with less than or equal to eleven years of schooling, women with twelve to fifteen years of schooling, and women with sixteen or more years of schooling. The sample is restricted to respondents aged 18-70. Standard errors are reported in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table VII does not show a statistically noticeable pattern of heterogeneous effects of Bartik shocks on the prevalence of traditional attitudes toward women working given the large standard errors across columns. The only statistically significant effect is among the middle educated men in a specification with a lagged dependent variable, where a one percent increase in Bartik shocks lowers the prevalence of traditional attitudes among men with twelve to fifteen years of education by 1.20 percentage points, significant at the 10 percent level. Absent any statistically meaningful effects among other gender-education subgroups, this finding suggests that men with middle education levels are more responsive to positive labor demand and become less traditional in their attitudes toward women working. Given the "hollowing out" of the occupational distribution in the middle, it is not surprising that the attitudinal response of men with twelve to fifteen years of schooling to favorable labor demand is consistent with the opportunity cost mechanism and not the threat to breadwinner status hypothesis (Autor, 2010).

Table VIII has the same structure as Table VII. It reports the effects of Bartik shocks on the prevalence of traditional attitudes toward the emotional suitability of women for politics for different education attainments among men (Panel (a)) and among women (Panel (b)). Two marginally significant patterns emerge from Table VIII, despite the general noisiness of the estimates. The first pattern suggests that the prevalence of traditional attitudes among men with less than or equal to eleven years of schooling increases in

TABLE VIII: DIFFERENTIAL EFFECTS ON TRADITIONAL ATTITUDES TOWARD THE EMOTIONAL SUITABILITY OF WOMEN FOR POLITICS BY GENDER AND EDUCATION

Panel (a)	$\leq$ 11 Years of Schooling, Men			12-15 Years of Schooling, Men			$\geq$ 16 Years of Schooling, Men		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bartik Shocks	0.862**	0.993	1.277*	0.201	-0.089	-0.450	-0.527	-1.148	-0.740
	(0.381)	(0.791)	(0.665)	(0.206)	(0.463)	(0.554)	(0.324)	(0.704)	(0.631)
Lagged Dependent Variable	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Instrumental Variable	No	No	Yes	No	No	Yes	No	No	Yes
N	127	71	68	135	81	81	134	79	79
Panel (b)	$\leq$ 11 Years of Schooling, Women			12-15 Years of Schooling, Women			$\geq$ 16 Years of Schooling, Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bartik Shocks	0.056	-0.608	-3.279	0.429	0.487	0.807	0.647**	-0.050	1.665
	(0.467)	(1.453)	(2.395)	(0.275)	(0.710)	(0.754)	(0.297)	(0.493)	(1.472)
Lagged Dependent Variable	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Instrumental Variable	No	No	Yes	No	No	Yes	No	No	Yes
N	133	77	76	135	81	81	133	78	77

Notes: The unit of observation is the state by year. The outcome variable is the share of traditional attitudes, defined as the number of agreements divided by the total number of responses to the statement that women are not emotionally suited for politics, for specific gender-education level characteristics. In Panel (a), the outcome variable is calculated among men with less than or equal to eleven years of schooling, men with twelve to fifteen years of schooling, and men with sixteen or more years of schooling. In Panel (b), the outcome variable is calculated among women with less than or equal to eleven years of schooling, women with twelve to fifteen years of schooling, and women with sixteen or more years of schooling. The sample is restricted to respondents aged 18-70. Standard errors are reported in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

response to favorable Bartik shocks. The second pattern suggests a similar response among women with at least sixteen years of schooling.

As for the first pattern, Columns (1)-(3) of Panel (a) demonstrate small differences across specifications and more consistency in both magnitude and direction of point estimates among lower educated men. Column (1) shows that a one percent increase in Bartik shocks increases the prevalence of traditional attitudes toward the emotional suitability of women for politics by 0.86 percentage points, significant at the 5 percent level, compared to an increase of 0.99 percentage points in Column (2), and a 1.28-percentage point increase in Column (3), significant at the 10 percent level. Although the coefficient in Column (2) is not statistically significant at conventional levels, its consistency both in terms of magnitude and direction with statistically significant coefficients in Columns (1) and (3) suggests that the true effects is likely not too far off.

As for the second pattern, regarding the effects of Bartik shocks on traditional attitudes toward the emotional suitability of women for politics among women with at least a baccalaureate, Column (7) of Panel (b) shows that a one percent increase in Bartik shocks increases the prevalence of traditional attitudes by 0.65 percentage points, significant at the 5 percent level. This column does not account for the dynamics of gender attitudes and is, therefore, not the preferred specification. However, the magnitude and the direction of the point estimate in Column (7) is consistent with the estimate

in Column (9), which albeit the noise is from the preferred specification of instrumented lagged dependent variable.

Table IX consists of two panels and six columns. Panel (a) reports the estimated effects of Bartik shocks on the prevalence of traditional attitudes (agreements or strong agreements) toward women working outside of the home among single men (columns (1)-(3)) and among married men (columns (4)-(6)). Panel (b) report the estimated effects of Bartik shocks on the prevalence of traditional attitudes among single women (columns (1)-(3)) and married women (columns (4)-(6)). In each panel, Column (1) and Column (4) do not include a lagged dependent variable, Column (2) and Column (5) include a lagged dependent variable, and Column (3) and Column (6) instrument for a lagged dependent variable using a twice-lagged dependent variable.

The heterogeneous effects of labor demand shocks on the prevalence of traditional attitudes toward women working by gender and marital status is not as informative given that almost all coefficients are imprecisely estimated. The only marginally significant pattern is observed among married women. Column (4) of Panel (b) shows that a one percent increase in Bartik shocks increases the prevalence of traditional gender attitudes among married women by 0.64 percentage points, significant at the 5 percent level.

Table X has an identical structure to Table IX, but it reports the effect of Bartik shocks on the prevalence of traditional attitudes (agreements)

TABLE IX: DIFFERENTIAL EFFECTS ON TRADITIONAL ATTITUDES TOWARD WOMEN WORKING OUTSIDE OF THE HOME BY GENDER AND MARITAL STATUS

Panel (a)	Single, Men			Married, Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Bartik Shocks	-0.033	-1.329**	0.256	-0.176	-0.464	-0.332
	(0.356)	(0.579)	(1.264)	(0.263)	(0.627)	(0.512)
Lagged Dependent Variable	No	Yes	Yes	No	Yes	Yes
Instrumental Variable	No	No	Yes	No	No	Yes
N	135	81	81	135	81	81
Panel (b)	Single, Women			Married, Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Bartik Shocks	0.148	-0.252	-0.139	0.638**	0.660	-0.214
	(0.266)	(0.565)	(0.608)	(0.252)	(0.594)	(1.129)
Lagged Dependent Variable	No	Yes	Yes	No	Yes	Yes
Instrumental Variable.	No	No	Yes	No	No	Yes
N	135	81	81	135	81	81

Notes: The unit of observation is the state by year. The outcome variable is the share of traditional attitudes, as defined as the number of agreements or strong agreements divided by the total number of responses to the statement that it is better for men to work and women tend home, for specific gender-marital status characteristics. In Panel (a), "Single, Men" indicates that the outcome variable is calculated among single men (widowed, divorced, separated, or never married), and "Married, Men" indicates that the outcome variable is calculated among currently married men. In Panel (b), "Single, Women" indicates that the outcome variable is calculated among single women (widowed, divorced, separated, or never married), and "Married, Women" indicates that the outcome variable is calculated among currently married women. The sample is restricted to respondents aged 18-70. Standard errors are reported in parentheses: * p < 0.10, ** p < 0.05, *** p < 0.01

TABLE X: DIFFERENTIAL EFFECTS ON TRADITIONAL ATTITUDES TOWARD THE EMOTIONAL SUITABILITY OF WOMEN FOR POLITICS BY GENDER AND MARITAL STATUS

Panel (a)	Single, Men			Married, Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Bartik Shocks	0.034	0.159	0.048	0.139	-0.540	-0.962
	(0.260)	(0.579)	(0.520)	(0.206)	(0.462)	(0.672)
Lagged Dependent Variable	No	Yes	Yes	No	Yes	Yes
Instrumental Variable	No	No	Yes	No	No	Yes
N	135	81	81	135	81	81
Panel (b)	Single, Women			Married, Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Bartik Shocks	0.281	-0.191	0.730	0.550**	0.809	0.911
	(0.231)	(0.468)	(1.802)	(0.245)	(0.650)	(0.625)
Lagged Dependent Variable	No	Yes	Yes	No	Yes	Yes
Instrumental Variable.	No	No	Yes	No	No	Yes
N	135	81	81	135	81	81

Notes: The unit of observation is the state by year. The outcome variable is the share of traditional attitudes, as defined as the number of agreements divided by the total number of responses to the statement that women are not emotionally suited for politics, for specific gender-marital status characteristics. In Panel (a), "Single, Men" indicates that the outcome variable is calculated among single men (widowed, divorced, separated, or never married), and "Married, Men" indicates that the outcome variable is calculated among currently married men. In Panel (b), "Single, Women" indicates that the outcome variable is calculated among single women (widowed, divorced, separated, or never married), and "Married, Women" indicates that the outcome variable is calculated among currently married women. The sample is restricted to respondents aged 18-70. Standard errors are reported in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

toward the emotional suitability of women for politics among single men and married men in Panel (a) and single women and married women in Panel (b). Just as was the case in Table IX, the only statistically significant coefficient is observed among married women. Column (4) of Panel (b) shows that a one percent increase in the predicted labor demand shocks, increases the prevalence of traditional attitudes among married women, significant at the 5 percent level, by 0.55 percentage points. Although the coefficient from Column (4) does not include a lagged dependent variable and is, therefore, not from a preferred specification, it is consistent in magnitude and direction with the point estimates in Columns (5) and (6) of Panel (b) despite their large standard errors.

Finally, Tables XI and XII report the effects of Bartik shocks among younger respondents, aged 18-47 years old, (columns (1)-(3)) versus older respondents, aged 48-70 years old, (columns (4)-(6)) on the prevalence of traditional attitudes toward women working outside of the home and the prevalence of traditional attitudes toward the emotional suitability of women for politics, respectively.

None of the coefficients in Table XI are statistically significant at conventional levels of 1, 5 and 10 percent and therefore, not informative. However, even though there is no evidence of heterogeneous effects of labor demand shocks on the prevalence of traditional gender attitudes toward women working by age in Table XI, I find a marginally significant effect among the

TABLE XI: DIFFERENTIAL EFFECTS ON TRADITIONAL ATTITUDES TOWARD WOMEN WORKING BY AGE

	Young			Old		
	(1)	(2)	(3)	(4)	(5)	(6)
Bartik Shocks	0.292	-0.372	-0.270	-0.026	0.035	0.166
	(0.193)	(0.416)	(0.444)	(0.227)	(0.432)	(0.389)
Lagged Dependent Variable	No	Yes	Yes	No	Yes	Yes
Instrumental Variable	No	No	Yes	No	No	Yes
N	135	81	81	135	81	81

Notes: The unit of observation is the state by year. The outcome variable is the share of traditional attitudes, defined as the number of agreements or strong agreements divided by the total number of responses to the statement that it is better for men to work and women to tend home, for specific age characteristics. "Young" indicates that the outcome variable is calculated among respondents aged 18-47 years old, whereas "Old" indicates that the outcome variable is calculated among 48-70 years old. Standard errors are reported in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE XII: DIFFERENTIAL EFFECTS ON TRADITIONAL ATTITUDES TOWARD THE EMOTIONAL SUITABILITY OF WOMEN FOR POLITICS BY AGE

	Young			Old		
	(1)	(2)	(3)	(4)	(5)	(6)
Bartik Shocks	0.498**	0.405	0.423	0.112	-0.187	-0.057
	(0.146)	(0.389)	(0.332)	(0.196)	(0.430)	(0.385)
Lagged Dependent Variable	No	Yes	Yes	No	Yes	Yes
Instrumental Variable	No	No	Yes	No	No	Yes
N	135	81	81	135	81	81

Notes: The unit of observation is the state by year. The outcome variable is the share of traditional attitudes, defined as the number of agreements divided by the total number of responses to the statement that women are not emotionally suited for politics, for specific age characteristics. "Young" indicates that the outcome variable is calculated among respondents aged 18-47 years old, whereas "Old" indicates that the outcome variable is calculated among 48-70 years old. Standard errors are reported in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

younger respondents regarding the prevalence of traditional attitudes toward the emotional suitability of women for politics in Table XII. Column (1) of Table XII shows that a one percent increase in Bartik shocks increases the prevalence of traditional attitudes toward the emotional suitability of women for politics by 0.50 percentage points in a specification without lagged dependent variable, significant at the 1 percent level. Although the coefficients in Columns (2) and (3) of Table 12 are noisy and statistically indistinguishable from zero, the magnitudes of their point estimates are comparable with Column (1).

2.6 Sensitivity Analysis

While different demographics may experience and respond differently to state-wide employment growth and labor demand shocks, they may also respond more strongly to changes in market segments relevant to their skill levels. That is, lower educated men may care more about labor demand growth and employment opportunities for lower educated men than the overall market conditions that include changes in opportunities for higher educated women, for example. Thus, to examine the sensitivity of Section 2.4's results in segmented state-level labor markets by gender and education, I construct six separate Bartik shocks, three each for men and women by education level: those with less than or equal to eleven years of schooling, those with twelve to fifteen years of schooling, and those with at least sixteen years of schooling.

In Tables XIII and XIV, I report the results from regressing the prevalence of traditional gender attitudes toward women working outside of the home and women's emotional suitability for politics, separately, among each of the six gender-education subgroups, on Bartik shocks that are specific to their segment. The regressions control for changes in the relevant population, and state and year fixed effects. All regressions instrument for lagged dependent variable using twice-lagged dependent variable and are weighted such that states with a higher share of relevant subpopulation contribute more to the

TABLE XIII: EFFECTS OF GENDER-EDUCATION-SPECIFIC BARTIK SHOCKS ON SHARE OF TRADITIONAL ATTITUDES TOWARD WOMEN WORKING AMONG GENDER-EDUCATION SUBGROUPS

	≤11 Years of Schooling		12-15 Years of Schooling		≥ 16 Years of Schooling	
	Men	Women	Men	Women	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Bartik Shocks for Men						
With ≤ 11 Years of Schooling	-6.451*					
	(3.472)					
Bartik Shocks for Women						
With ≤ 11 Years of Schooling		-1.549				
		(2.665)				
Bartik Shocks for Men						
With 12-15 Years of Schooling			-0.744			
			(0.526)			
Bartik Shocks for Women						
With 12-15 Years of Schooling				0.449		
				(0.869)		
Bartik Shocks for Men						
With ≥ 16 Years of Schooling					0.046	
					(0.324)	
Bartik Shocks for Women						
With ≥ 16 Years of Schooling						0.089
						(0.235)
Lagged Dependent Variable	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes
N	68	76	81	81	75	71

Notes: The unit of observation is state by year. Each coefficient is from a separate regression. The regression specifications are the same across columns. The data is restricted to respondents aged 18-70 years old different, but the samples are different based on gender and education characteristics of respondents. The coefficient in Column (1) is from a sample of men with less than or equal to eleven years of schooling. The Coefficient in Column (2) is from a sample of women with less than or eleven years of schooling. The coefficient in Column (3) is from a sample of men with twelve to fifteen years of schooling. The coefficient in Column (4) is from a sample of women with twelve to fifteen years of schooling. The coefficient in Column (5) is from a sample of men with at least sixteen years of schooling. Finally, the coefficient in Column (6) is from a sample of women with at least sixteen years of schooling. Every regression has a lagged dependent variable and instruments for it using a twice-lagged dependent variable. State and year fixed effects are included in all regressions. Regressions are weighted by the average number of respondents in each state in the relevant sample. Standard errors are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE XIV: EFFECTS OF GENDER-EDUCATION-SPECIFIC BARTIK SHOCKS ON SHARE OF TRADITIONAL ATTITUDES TOWARD THE EMOTIONAL SUITABILITY OF WOMEN FOR POLITICS AMONG GENDER-EDUCATION SUBGROUPS

	≤11 Years of Schooling		12-15 Years of Schooling		≥ 16 Years of Schooling	
	Men	Women	Men	Women	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Bartik Shocks for Men						
With ≤ 11 Years of Schooling	-3.090*					
	(2.214)					
Bartik Shocks for Women						
With ≤ 11 Years of Schooling		-4.016				
		(4.820)				
Bartik Shocks for Men						
With 12-15 Years of Schooling			-0.233			
			(0.426)			
Bartik Shocks for Women						
With 12-15 Years of Schooling				-0.061		
				(0.609)		
Bartik Shocks for Men						
With ≥ 16 Years of Schooling					-0.532**	
					(0.244)	
Bartik Shocks for Women						
With ≥ 16 Years of Schooling						0.115
						(0.119)
Lagged Dependent Variable	Yes	Yes	Yes	Yes	Yes	Yes
Instrumental Variable	Yes	Yes	Yes	Yes	Yes	Yes
N	68	76	81	81	79	77

Notes: The unit of observation is state by year. Each coefficient is from a separate regression. The regression specifications are the same across columns. The data is restricted to respondents aged 18-70 years old, but the samples are different based on gender and education characteristics of respondents. The coefficient in Column (1) is from a sample of men with less than or equal to eleven years of schooling. The Coefficient in Column (2) is from a sample of women with less than or eleven years of schooling. The coefficient in Column (3) is from a sample of men with twelve to fifteen years of schooling. The coefficient in Column (4) is from a sample of women with twelve to fifteen years of schooling. The coefficient in Column (5) is from a sample of men with at least sixteen years of schooling. Finally, the coefficient in Column (6) is from a sample of women with at least sixteen years of schooling. Every regression has a lagged dependent variable and instruments for it using a twice-lagged dependent variable. State and year fixed effects are included in all regressions. Regressions are weighted by the average number of respondents in each state in the relevant sample. Standard errors are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

estimation. For comparability of gender-education specific Bartik shocks with gender-education specific employment, see Table XXVI, Appendix A.

Column (1) of Table XIII shows that a one percent increase in Bartik shocks specific to men with less than or equal to eleven years of schooling lowers the prevalence of traditional gender attitudes toward women working outside of the home by 6.45 percentage points, significant at the 10 percent level, among men with less than or equal to eleven years of schooling. Comparing the responsiveness of men with less than or equal to eleven years of schooling to own labor demand shocks versus their responses to the overall labor demand shocks reported in Table VII, where there was no evidence of a statistically significant response to overall Bartik shocks, suggests that this group is particularly responsive to shifts in demand in their industries.

Table XIV shows how the share of traditional attitudes toward the emotional suitability of women for politics among each of the six gender-education subgroups changes in response to their group specific labor demand shocks. With the exception of the coefficient in column (3), all the other estimates are not statistically significant at conventional levels. Column (3) shows that men with at least sixteen years of schooling are fairly responsive to own-group Bartik shocks. A one percent increase in Bartik shocks specific to men with at least sixteen years of schooling, lowers the prevalence of traditional attitudes toward the emotional suitability of women for politics among men with at least sixteen years of schooling by 0.53 percentage points, significant at

the 5 percent level. By contrast, Table VIII shows no statistically significant evidence of responsiveness of men with at least sixteen years of schooling to the overall labor demand shocks, implying that this group is more responsive to changes in their own market segment.

It must be noted that the evidence presented in Tables XIII and XIV is suggestive and not definitive, since the regressions do not control for changes in labor demand across other gender-education subpopulations. As shown in the Table XXVII, Appendix A, Bartik shocks specific to one gender-education segment of the population are highly correlated with Bartik shocks specific to other gender-education segments. This high correlation is consistent with expectations, since despite sorting across industries and occupations by gender and education, no one industry or occupation is completely segregated, and major economic shocks to states reverberates across groups.

2.7 Conclusions

This paper investigates the size and direction of labor demand shocks on the prevalence of traditional attitudes toward women working outside of the home and women's emotional suitability for politics. It significantly expands what is known about the evolution of gender attitudes by documenting aggregate trends of decline in the prevalence of traditional gender-role attitudes over time, within and between cohorts, and by innate and acquired charac-

teristics such as gender, education, marital status, and age. In addition, it documents a general lack of heterogeneous effects of labor demand shocks across subpopulations; and lastly, it highlights the possible responsiveness of subpopulations to own-group labor demand shocks.

The difficulty of isolating the effects of labor market conditions on gender attitudes lies in the simultaneity between the two. The interrelated government policies, and demand and supply factors that determine labor market conditions are likely influenced by prevailing gender attitudes. Gender attitudes could influence early childhood education, subsequent levels of human capital investments, women's decision to join the labor force, and employers' decisions to hire women, which complicates the identification of any causal effects of labor demand changes. However, by using a framework that estimates local labor demand shifts based on only the initial local industry distribution and national industry growth, I effectively overcome the simultaneity concern between labor market changes and gender attitudes.

Four analytical components add credibility to the results. One, controlling for systematic time-invariant differences across states that would affect the local industry distributions as well as local attitudes. Two, controlling for variables that would plausibly affect both national employment growth trends and local gender attitudes such as changes in population, or gender and education characteristics of survey respondents. Three, recognizing the dynamics of gender attitudes and measuring current gender attitudes as a

function of prior attitudes. Four, overcoming the endogeneity introduced when controlling for both gender dynamics and state fixed effects by using an instrumental variables approach.

This analysis provides insights into how traditional gender attitudes respond to labor demand changes that are largely unaffected by government policies. The 40-year period of this study, which captures both longer-term structural changes in market dynamics as well as shorter-term fluctuation of business cycles, sheds light on both the power and limitations of market demand forces in bringing about attitudinal change. For example, the analysis highlights that despite the considerable declines in the prevalence of traditional gender attitudes toward women working outside of the home and their emotional suitability for politics between 1977 and 2016, the declines have not been continuous and without setbacks. Of the 37- and 26-percent declines during this period in the prevalence of traditional attitudes toward women working outside of the home and their emotional suitability for politics, 27 and 18 percent of each, respectively, took place between 1977 and 1990 with only small declines thereafter. These shares sit at 25 and 17 percent in 2016. Although, this paper does not attempt to answer whether the social marginal cost of reducing the existing levels of traditional gender attitudes is acceptable or whether the rates of attitudinal change have been adequate, it, nonetheless, provides policymakers with important initial benchmarks for consideration via the reduced form estimates.

The reduced form estimates reveal that the effect of labor demand shocks may differ systematically by the aspect of gender roles in question. Positive Bartik shocks lower the prevalence of traditional attitudes toward women working outside of the home by 0.45 percentage points, while I find no statistically significant evidence that labor demand shocks affect the prevalence of traditional attitudes toward the emotional suitability of women for politics across different specifications. This possible difference in the effectiveness of labor demand shocks highlights the multidimensional character of attitude formation and the importance of separately considering the effects on attitudes related to work as opposed to politics.

Another key lesson is that group-specific labor demand shocks are possibly more important than the overall demand shocks in influencing the prevalence of traditional attitudes toward women working and women's emotional suitability among specific groups. Although I find no evidence that positive overall Bartik shocks affect the prevalence of traditional attitudes toward women working among men with less than a high school diploma, I find that a one percent increase in Bartik shocks specific to men with less than a high school diploma lowers the prevalence of said attitudes by 6.45 percentage points, significant at the 10 percent level. Similarly, I find no statistically significant evidence that positive Bartik shocks affect the prevalence of traditional attitudes toward women's emotional suitability for politics, but I find that a one percent increase in Bartik shocks specific to men with at least

a baccalaureate lowers the prevalence of these traditional attitudes by 0.53 percentage points among men with at least a baccalaureate, significant at the 5 percent level. This higher relevance of group specific labor demand shocks in driving changes in traditional gender-role attitudes suggests that any effective policy should be matched to the target population. However, since labor demand shocks across various gender-education groups are highly correlated, whether each group is responding to own labor demand changes or other groups' labor demand changes is unclear.

3 STATE TAKING THE REINS? EMPLOYMENT VERIFICATION REQUIREMENTS AND LOCAL LABOR MARKET OUTCOMES

3.1 Introduction

As global migration flows rose over recent decades, United States federal immigration policy focused resources on strengthening border security and raising the costs of entering into the U.S. illegally. U.S. Border Patrol spending correspondingly rose almost ten-fold over the past two decades, to $4.3 billion in Fiscal Year 2017. In spite of this unidimensional focus of federal immigration policy, states have selectively adopted policies designed to make undocumented immigration less attractive to potential migrants by reducing access to public benefits, by increasing cooperation between local/state law enforcement and federal immigration authorities, and by strengthening employment eligibility verification systems. The adoption of employment eligibility verification systems, in particular, has the potential to dramatically reshape the immigration landscape by eliminating undocumented immigrants' access to formal sector labor markets and the associated earnings gains that have motivated past waves of migration to the U.S. At the same time, the welfare implications of these state-level policies are ambiguous. The substitutability of natives and work-eligible immigrants for undocumented workers

will determine whether these subgroups benefit from falling undocumented labor supply in formal sector markets, while constraints placed on the hiring of undocumented workers will raise the costs that firms face.

The primary system used to verify immigrants' work eligibility is E-Verify, a largely voluntary electronic verification system developed by the U.S. Immigration and Naturalization Service (INS) in 1997.[8] Partial or comprehensive mandates have been adopted by twenty-two states that require the E-Verify system be used to verify employment eligibility of new hires. In this paper we study how the passage and enforcement of state-level E-Verify mandates have affected local labor market outcomes for subpopulations with varying rates of predicted employment ineligibility and for native-born workers, the intended beneficiaries of these policies.

Understanding the complex impacts of expanded E-Verify usage is particularly relevant at present. Recent comprehensive immigration reform proposals, such as legislation passed by the U.S. Senate in 2013, and the White House's FY 2019 Budget Message (OMB, 2018) have called for a federal private-sector E-Verify mandate. More broadly, this work contributes to a greater understanding of the role of state and local policies, including cooperation agreements with federal authorities, in influencing labor market

[8]The INS was abolished in 2003 and replaced by the U.S. Citizenship and Immigration Services (USCIS), Immigration and Customs Enforcement (ICE), and Customs and Border Patrol (CBP) offices in the Department of Homeland Security.

outcomes and immigrants' location choice.[9] Finally, immigration policy is currently among the most hotly debated political issues. A vast academic literature has sought to understand how immigration, both legal and undocumented, impacts American firms and the economic fortunes of the native-born.[10] While evaluating the efficacy of E-Verify is important for understanding the limits of policy, an improved understanding of the impact of E-Verify helps deepen our understanding of the ultimate gains or losses from immigration.[11]

Our investigation begins with a new analysis of administrative data from the Department of Homeland Security on usage of the E-Verify system. We use this data to estimate the effect of E-Verify mandates on usage and document a high degree of non-compliance. Specifically, we show that E-Verify usage is quite low among firms with fewer than 20 employees and their usage is largely unaffected by passage of a mandate. The high degree of non-

[9]For example, other recent work studies the impacts of the Secure Communities Act (East et al., 2019; East and Velasquez, 2019) and the 287(g) program (Bohn and Santillano, 2017).

[10]This literature is recently reviewed and discussed in Lewis and Peri (2015) and Dustmann et al. (2016a). Other recent examples are Chassamboulli and Peri (2015), Dustmann and Glitz (2015), Dustmann et al. (2016b), and Clemens et al. (2018).

[11]Our work also contributes to understanding of the role of legal status in immigrant outcomes because the increased use of E-Verify may have the effect of creating much sharper distinctions in the labor market outcomes of immigrants with different legal statuses. See, for instance, Borjas and Cassidy (2019).

compliance that we document suggests that the mandates may impose substantial costs on firms.

We use three data sources to identify labor market impacts: the Quarterly Workforce Indicators (QWI), American Community Survey (ACS), and County Business Patterns (CBP). Our benchmark county-level approach identifies significant declines in Hispanic worker employment in response to both passage and enforcement of E-Verify mandates. We provide evidence that employment declines are driven by those subpopulations most likely to be classified as work-ineligible. We identify employment declines among Hispanic and likely work-ineligible subpopulations that are notably larger than those found in prior work (Orrenius and Zavodny, 2015, 2016; Orrenius et al., 2018; Amuedo-Dorantes and Bansak, 2014). These divergent findings are explained by differences in the benchmark specifications employed. Specifically, we test for parallel pre-trends with respect to key outcomes and, in contrast to past work, focus on specifications without linear time trends when the data provide support for doing so. Given treatment effects that grow over time, the inclusion of time trends will tend to attenuate estimates. We also build on past work by showing that usage of the E-Verify system and the associated labor market effects are apparent when mandates are passed, prior to enforcement. Treatment effects associated with the date of enforcement, which have been the focus of much prior research, may therefore fail to accurately capture the overall effect of the mandates.

Consistent with the prior evidence, we document declines in Hispanic worker turnover (hires and separations) that parallel employment losses. This type of "job lock" is driven by the fact that E-Verify mandates apply only to newly-hired workers and represents a notable labor market distortion induced by E-Verify mandates. We use ACS data to demonstrate that Hispanic employment declines in response to E-Verify mandate passage are driven by probabilistically undocumented workers, the intended targets of the policy. Our work finds no evidence that native-born workers benefit from E-Verify mandates and some evidence they are harmed by them. In particular, we identify small but statistically significant declines in employment among non-Hispanics using the QWI. ACS data provide no evidence of corresponding employment gains among U.S. citizens. We estimate employment declines among native-born workers who are the most substitutable for undocumented immigrants, such as young, male workers without college degrees. These employment declines are mirrored by declines in labor market earnings and family income.

We next identify substantial heterogeneity in employment effects by firm size. Larger firms are more likely to comply with E-Verify mandates and we correspondingly demonstrate that virtually all of the decline in Hispanic employment is driven by workers in larger firms. The number of large firms also declines significantly in response to the passage of E-Verify mandates, suggesting that aggregate employment effects result from a combination of

extensive and intensive margin changes. The disproportionate decline in large firm employment represents an unintended consequence of E-Verify mandates and suggests that the costs imposed on firms that do comply with these mandates may be substantial.

The heterogeneous employment impacts between large and small firms motivate our analysis of within-state and within-county spillovers. Since some E-Verify mandates exclude smaller firms, and even when covered smaller firms have a lower compliance rate with mandates, counties that have a larger share of employment in small firms will be impacted less by statewide mandates. We use this variation to estimate models that compare counties in the same state that vary in their effective E-Verify coverage. These estimates indicate that there are important spillover effects that reflect the movement of workers from jobs in high-compliance to low-compliance counties and from jobs in larger to smaller firms. These models are also important because they rely on a distinct source of variation in E-Verify coverage than the traditional variation across states and time exploited in our and others' earlier analyses.

Finally, we use ACS data to show that the size of the potentially undocumented population does not change in response to passage of E-Verify mandates. The divergence between this finding and the evidence from past work that E-Verify mandates lead to undocumented population declines (see, for instance, Orrenius and Zavodny, 2016) appears to be explained by our focus on the timing of mandate passage rather than enforcement. We provide

suggestive evidence that increases in supplementary family income sources may explain the lack of any significant estimated impact on the mobility of the work-ineligible subpopulation.

3.2 E-Verify Background, Mandates, and Usage

The 1952 Immigration and Nationality Act officially made employers responsible for ensuring that their employees are legally eligible to work in the United States, but enforcement of this requirement remained limited over subsequent decades. Beginning in 1986 the eligibility verification process was streamlined and strengthened through a requirement that all newly hired employees fill out Employment Eligibility Verification Form I-9. This form requires new employees to submit documentation of their identity and their authorization to work in the United States, for example through a combination of a passport, Permanent Resident Card, or other approved documents. Federal law requires that employers maintain I-9 forms, but does not mandate that the employer verify the authenticity of the information or documents provided. Concerns arose in subsequent years regarding the accuracy and timeliness of verification of employee eligibility based on I-9 Form submissions (Orrenius and Zavodny, 2015; Meissner and Rosenblum, 2009).

In 1997 an electronic verification system was developed by the U.S. Immigration and Nationalization Service (INS) to improve the efficiency of the employee verification process. The E-Verify program provides employers with access to an electronic database that allows for rapid verification of work eligibility. There is no federal mandate to use the E-Verify system to verify the accuracy of information on the I-9 form. Rather, federal legislation requires only that E-Verify be used for all employees in a given firm or else not be used at all by the firm.[12] While there are no monetary costs to firms to use the E-Verify system, the are non-trivial set-up, training, and compliance costs to using the system. These costs are particularly cumbersome for small firms, which a 2011 analysis suggested would spend $2.6 billion on compliance-related costs if forced to utilize E-Verify (Arvelo, 2011). Firms that use E-Verify turn over employment data to the Department of Homeland Security for statistical analysis, which employers may worry could trigger audits or immigration enforcement raids.[13]

In 2006, Colorado, Georgia, and North Carolina became the first states to enact mandates that require E-Verify usage for particular types of new

[12]Beginning in 2009, the Federal Acquisitions Regulation requires federal contractors, with some exceptions, to use E-Verify for all new employees.

[13]For example, see Peck and Murphy (2019) from the SHRM website: https://www.shrm.org/resourcesandtools/hr-topics/talent-acquisition/pages/pros-and-cons-registering-for-everify.aspx.

hires.[14] Currently 22 states have enacted some type of E-Verify mandate. E-Verify requirements vary significantly across states, ranging from requirements imposed in nine states that E-Verify be used by all or nearly all employers, to less comprehensive E-Verify requirements covering only state agencies and state contractors/subcontractors. Table XV lists all state-level E-Verify laws. Note that many mandates were phased-in over several years, with larger firms covered initially and smaller firms covered in later years.[15] In our benchmark analyses, we exclude those states that passed E-Verify mandates covering state agencies and/or state contractors/subcontractors but not covering other private sector firms since the effective coverage in these states is low and since our data do not allow us to identify firms' state contractor/subcontractor status. In Appendix B, we show the robustness of findings to the inclusion of data from these states. Penalties for non-compliance vary across states from modest fines to suspension of a business license.

A unique contribution of our work is in providing the first assessment of the effect of state E-Verify mandates on usage of the system. We obtained administrative records from the USCIS via a Freedom of Information Act

[14]Data on state E-Verify laws comes from Mendoza and Ostrander (2014) and individual state statutes.

[15]Several counties in California enacted E-Verify mandates. These were overturned by subsequent state law that prohibited lower levels of government from enacting such mandates. Illinois also prohibits lower levels of governments from enacting E-Verify mandates. We are not aware of any other sub-state E-Verify mandates.

TABLE XV: STATE-LEVEL E-VERIFY MANDATES

State	Citation	Year Enacted	Applies to:
Alabama	HB 56 HB 658	2011	All employers (phase in)
Arizona	HB 2779 HB 2745	2007 2008	All employers
Colorado	HB 1343 SB 139 SB 193	2006 2008 2008	State agencies, contractors
Florida	EO 11-02 EO 11-116	2011 2011	State agencies, contractors, subcontractors
Georgia	SB 529 HB 2 SB 447 HB 87 HB 742 HB 1027	2006 2009 2010 2011 2012 2012	Public employers, contractors, subcontractors (phase in) Private employers with 11+ employees (phase in)
Idaho	EO 2009-10	2009	State agencies, contractors
Indiana	SB 590	2011	State/local agencies, contractors
Louisiana	HB 342 HB 646 HB 996	2011 2011 2012	State/local contractors Option for private employers
Michigan	HB 5365	2012	Certain state agencies, contractors and subcontractors
Minnesota	EO 08-01	2008	Certain state contractors
Mississippi	SB 2988	2008	All employers (phase in)
Missouri	HB 1549	2008	Public employers, contractors, subcontractors
Nebraska	LB 403	2009	Public employers, contractors
North Carolina	SB 1523 HB 36 HB 786	2006 2011 2013	State agencies, universities Localities, all employers with 25+ employees (phase in) Excludes employees whose term of employment is less than nine months
Oklahoma	HB 1804	2007	Public employers, contractors, subcontractors
Pennsylvania	SB 637	2012	Public works contractors and subcontractors
South Carolina	HB 4400 SB 20 HB 4813	2008 2011 2012	Public employers, contractors, all private employers (phase in)
Tennessee	HB 1378	2011	Public employers, private employers with 6+ employees required to use E-Verify or retain specified employee documentation (phase in)
Texas	SB 374	2015	State agencies
Utah	SB 81 SB 39 SB 251 HB 116	2008 2009 2010 2011	Public employers, contractors, subcontractors Private employers with 15+ employees
Virginia	HB 737 HB 1859 SB 1049	2010 2011	State agencies Public contractors, subcontractors with 51+ employers
West Virginia	SB 659	2012	Certain public employers, contractors

request that include counts of enrollment by firms in the E-Verify system, counts of total E-Verify queries, and counts of queries deemed work ineligible, separately by county, detailed industry, firm size, and year-quarter from 2004 to 2016. These data are an important part of our research design because they allow us to assess how common E-Verify usage was prior to a mandate's passage and to evaluate the change in usage associated with mandate passage as well as enforcement. In addition, these data are used to evaluate heterogeneity in adherence to state-level mandates as a function of firm size.

New hires (the population subject to E-Verify mandates) are measured in the Quarterly Workforce Indicators (QWI) data. The QWI contain aggregate data on employment, hires, separations, and other labor market measures by geographic area, industry, firm size, and a limited number of worker demographic characteristics from 2004 through the second quarter of 2015. The QWI is created by the United States Census Bureau from matched employer-employee data that is itself created from state and federal administrative records and surveys. Much of the information on employment and hires comes from state Unemployment Insurance (UI) records, which cover 96 percent of civilian wage and salary jobs.[16] The measure of hires that we use includes all people who had earnings from an employer in a particular quarter but did not have earnings from that employer in the previous quarter.

[16]Detailed information about the QWI data is available at Longitudinal Employer-Household Dynamics (LEHD) website: https://lehd.ces.census.gov/data.

Figure 13 shows the ratio of E-Verify queries to new hires from 2004 to 2015. E-Verify usage was quite low prior to 2006 and began to rise after the relaunch of the web interface with enhanced features (including photo matching for individuals who have a Permanent Resident Card or Employment Authorization Document), and public outreach in 2007.[17] In 2006, three percent of hires were queried. The ratio rose to 20 percent in 2010 and 31 percent in 2015.[18] 2008 was the first year that any private sector hires were subject to an E-Verify mandate. Figure 13 also shows the fraction of private-sector hires that were subject to an E-Verify mandate. We estimated this coverage rate by applying applicable state laws based on firm size.[19] The coverage rate rises from zero in 2007 to 12.3 percent in 2015.

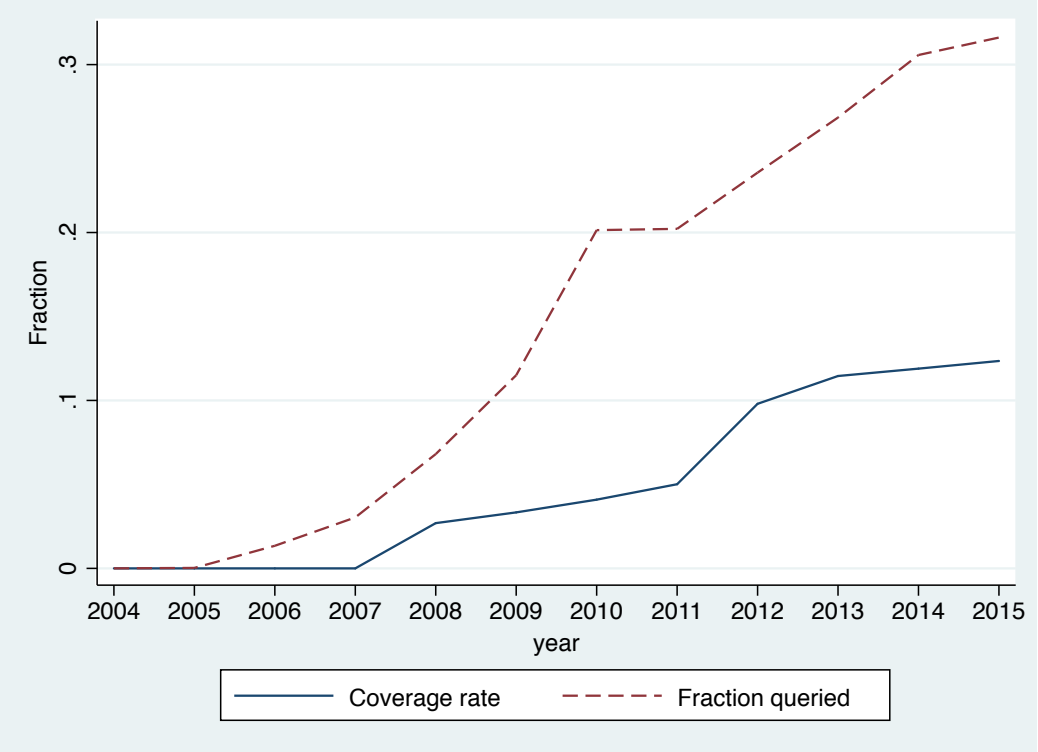
Figure 14 shows the ratio of E-Verify queries to hires separately by firm size. E-Verify usage is quite uncommon among firms with fewer than 20 employees, where under ten percent of hires were queried in 2015. By contrast,

[17]A summary of the history of the E-Verify program is given at the USCIS website (History and Milestones, 2018): https://www.uscis.gov/e-verify/about-program/history-and-milestones.

[18]The E-Verify queries data in Figure 13 includes queries by both public and private-sector entities, while our extract of the QWI data covers only the private sector. Thus the ratio of queries to hires overstates the fraction of private sector hires that are queried.

[19]The data on hires in the QWI is grouped into firm size bins that do not always coincide with the E-Verify mandate thresholds, which induces some measurement error in our coverage rate. Our measure of coverage does not take into account any others exclusions to a law.

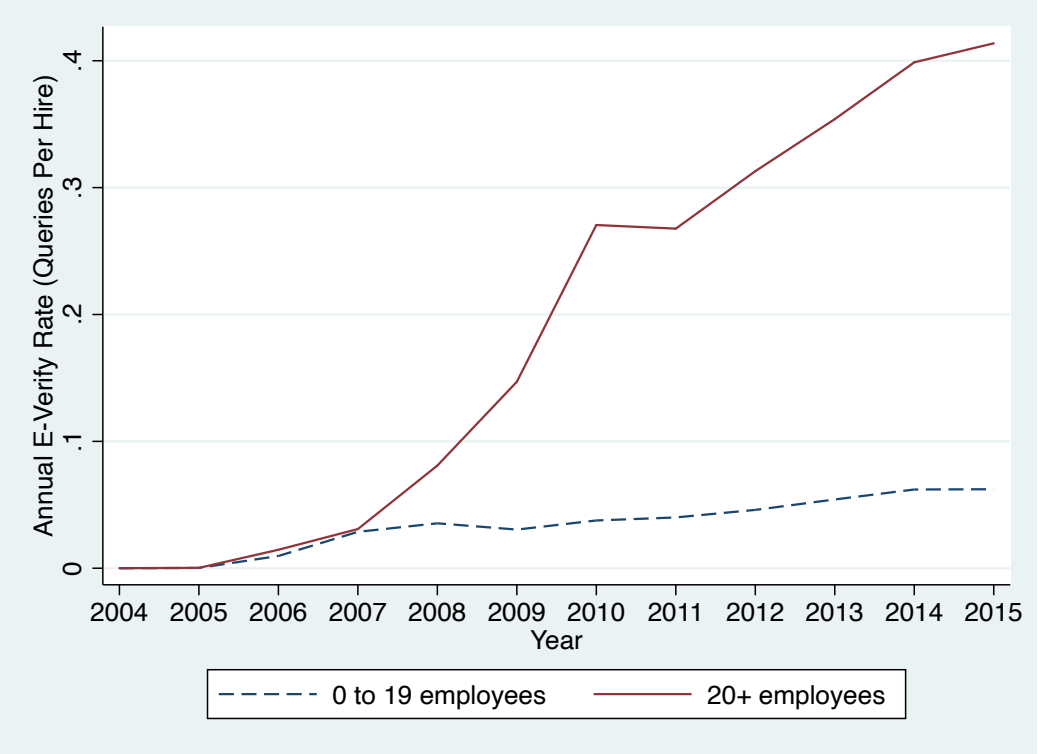
Figure 13: Annual Trends in E-Verify Usage



Data source: United States Department of Homeland Security data series.

Notes: This figure plots the annual E-Verify rate, defined as the number of E-Verify queries divided by the total number of new hires, and the annual fraction of all private sector hires subject to E-Verify mandates. New hires are measured using the QWI.

Figure 14: Annual Trends in E-Verify Usage by Firm Size



Data source: United States Department of Homeland Security data series.

Notes: This figure plots the annual E-Verify rate, defined as the number of E-Verify queries divided by the total number of new hires, separately by firm size bin. New hires are measured using the QWI.

over 40 percent of hires in firms with 20 or more employees were queried in 2015. This disparity is not because of state mandates that exclude small firms since most states with private sector mandates eventually covered all firms (the exceptions are Tennessee, Georgia, and Utah, which exclude firms with fewer than six, fewer than 11, and fewer than 15 employees, respectively).

Rather, the disparity is likely caused, in part, by the fact that some portion of the set-up and compliance costs are fixed and therefore higher on a per-hire basis for small firms. Some of the disparity is also likely due to larger firms being more likely to be federal or state contractors and therefore subject to a mandate. In Section 3.4, we demonstrate that mandate passage sharply increases E-Verify usage by larger firms while smaller firms experience a more marginal increase in usage.

A small existing literature has directly investigated labor market impacts of E-Verify mandates.[20] This past work has consistently identified state-level employment declines among likely work-ineligible subpopulations in response to E-Verify enforcement but is otherwise inconclusive regarding the net labor market impacts of (and costs associated with) E-Verify mandates. The best-known, state-level E-Verify case studies examine the migration and labor market impacts of Arizona's 2007 Legal Arizona Workers Act (LAWA), which mandated statewide E-Verify usage. These studies identify a significant decline in the state population characterized as non-citizen Hispanic in response to LAWA's passage, but find no evidence of improvement in employment outcomes for non-Hispanic low-skilled workers (Bohn et al., 2014, 2015). Moreover, LAWA was passed during a period in which Arizona enacted multiple laws which were widely perceived as "anti-immigrant" (Newman, 2017),

[20]Other recent work has turned to investigating downstream outcomes, including foreign direct investment responses, educational enrollment, and health insurance (Amuedo-Dorantes et al., 2015; Gunadi, 2018; Churchill, 2019).

suggesting that the undocumented population might have been particularly responsive to the passage of LAWA given the overall state climate. The most comprehensive empirical research on the aggregate labor market impacts of the scale-up of E-Verify usage includes Amuedo-Dorantes and Bansak (2014), Orrenius and Zavodny (2015), and Orrenius et al. (2018). These studies examine the employment and wage effects of E-Verify mandates passed in multiple states and find mixed evidence of whether any benefits accrue to likely work-eligible sub-populations, likely due to differences in the data sources used, among other factors. Orrenius and Zavodny (2016) employs a similar approach to examine changes in state-level likely undocumented populations and finds evidence that E-Verify mandates lead to reductions in this population, driven by declines in the number of recent migrants living in a given state.[21]

3.3 Data Sources

We use three complementary data sources on labor market outcomes. Our benchmark specifications employ outcomes constructed using QWI data from 2004 to 2015, which we described in Section 3.2. These data give accu-

[21] Although we replicate this finding when examining undocumented population responses to E-Verify mandate enforcement, we find no such impact in benchmark specifications that study responses to mandate passage. These divergent results are explained by an increase in the undocumented population immediately after passage, which leads to an inflated estimate of the decline in population following mandate enforcement. See Figure 22.

rate measures of aggregate employment, hires, and separations by quarter, county, firm size, industry, and Hispanic ethnicity.[22] These data cover formal sector, wage and salary workers. The data do not cover self-employed workers, independent contractors, or those who work in informal or uncovered jobs. QWI data does not include any information about a worker's eligibility to work in the United States. We analyze these data for Hispanics and non-Hispanics separately. While the population of Hispanic workers includes both natives and immigrants, and the subpopulation of Hispanic immigrants includes both work-eligible and work-ineligible immigrants, we anticipate that changes in employment patterns driven by E-Verify legislation will be most likely to manifest themselves as changes in Hispanic employment patterns given that the share of Hispanic workers who are undocumented is substantially higher than the share of non-Hispanic workers without work eligibility, a fact we document below.

We also analyze data from the ACS from 2005 to 2015 that allows us to focus more directly on workers most likely to be undocumented and ineligible to work in the United States, and workers who are potentially affected by changes in labor market outcomes among undocumented workers. ACS data have a number of advantages. First, they contain variables that allow us to study geographic movement, household-level earnings, self-reported employ-

[22]QWI data is available for both public and private sector employment. We only analyze data on private sector employment.

ment status (which may include informal employment), and self-employment, which are not available in the QWI. Rich demographic data allow us to focus on treatment effects among more narrow classifications of individuals, including native-born Hispanics and low-skilled, native-born individuals. However, the ACS does not contain information on the legal status of foreign-born persons and so we follow an existing literature and define a respondent as probabilistically undocumented if that person is a foreign-born, non-veteran with no post-secondary education.[23] Averaged over our sample period, 47.1 percent of Hispanics are foreign-born and 55.4 percent of these are probabilistically undocumented. More generally, 26.9 percent of the foreign-born are probabilistically undocumented.

Two important drawbacks of the ACS are, first, that it is a sample and thus provides a noisier measure of employment; second, geographic coverage is more limited than in the QWI. Individuals in the ACS are classified by their Public Use Microdata Area (PUMA), which are areas created by the Census Bureau that contain at least 100,000 people. We thus employ a cross-walk that maps PUMAs into each of the 3,142 counties (or county-equivalents). Finally, ACS data is annual, rather than quarterly.

We also study changes in the number of establishments in operation using County Business Patterns (CBP) data, which are derived from the Business

[23]This definition is adopted in Feigenberg (2019) and a closely-related definition is employed in Orrenius and Zavodny (2016).

Register data collected by the U.S. Census Bureau. These data provide the number of establishments in operation at the county-by-firm size bin-by year level and represent the most comprehensive existing data source for establishment-level records (United States Census Bureau, 2019). Data are available for the first quarter of each year between 2004 and 2015.

3.4 Research Design and Empirical Findings

We now describe our empirical framework to identify changes in E-Verify usage in response to the enactment of legislation mandating its use and to examine resultant changes in labor market outcomes for exposed workers as a function of their likely employment eligibility. The ideal experiment to identify E-Verify program impacts would require the random assignment of E-Verify legislation passage and enforcement across place and time. Absent random variation in the passage and enforcement of E-Verify legislation, a key identification challenge is that, even in the absence of an E-Verify mandate, counties in states that pass and enforce E-Verify legislation may have subsequently experienced changes in labor market and immigration outcomes that differed from those in counties in states that did not pass such legislation. To identify the causal impacts of E-Verify legislation in the presence of potentially endogenous passage, we begin with event-study models that document that there are no pre-trends in E-Verify usage or in QWI-based Hispanic labor market outcomes prior to passage of E-Verify legislation. (A

comprehensive set of event studies for all examined outcomes is included in Appendix B.) We then employ two complementary identification strategies to measure the effect of legislation on outcomes following passage and enforcement of employment verification mandates: The first approach uses variation across states and time in E-Verify mandates to identify the causal effect of mandates on average labor market outcomes. The second approach uses data disaggregated to the firm size level to exploit within-state variation in the predicted coverage of and adherence to E-Verify mandates and to investigate within-state spillovers.

3.4.1 Event Study Models

We begin by presenting event study graphs that characterize differences in E-Verify query rates and in QWI-based employment outcomes among Hispanics in the years before and after passage of any private sector E-Verify legislation in a given state. Our primary goal here is to assess whether there are differential trends in outcomes prior to passage of an E-Verify mandate. To do this, we estimate regression models with the following form:

$$Y_{cst} = \alpha + \sum_{y=-4}^{4} \beta_y Everify_{csty} + \gamma_t + \lambda_c + \epsilon_{cst} \tag{6}$$

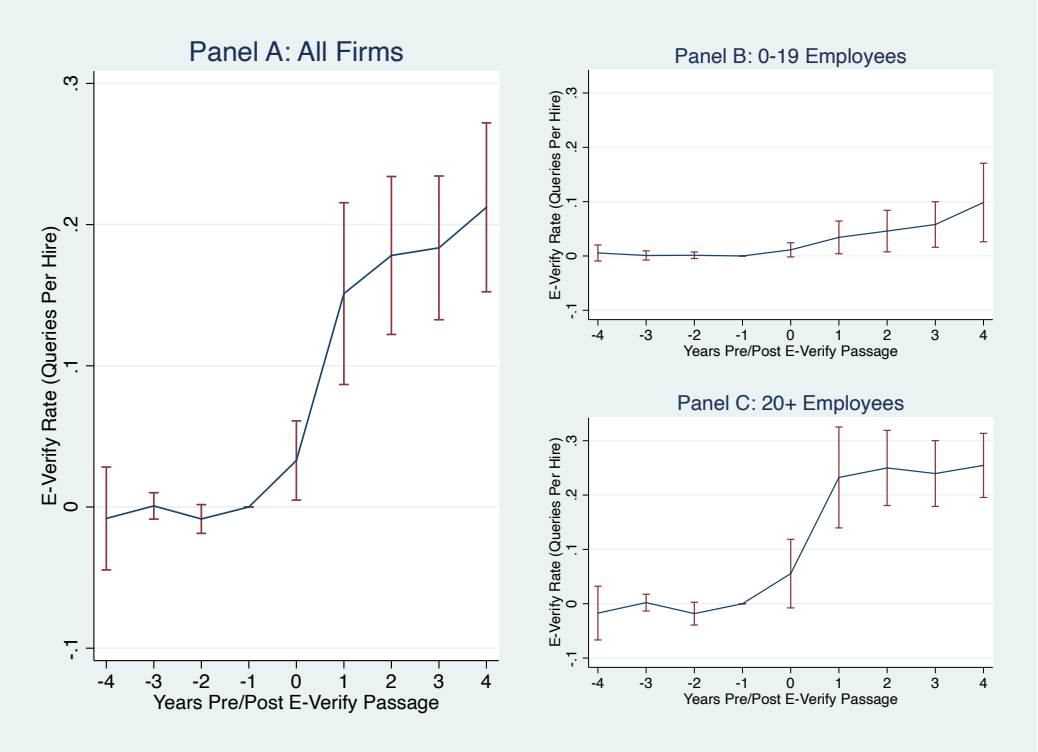
where Y_{cst} is the outcome of interest for county c in state s in year-quarter t.[24] γ_t and λ_c represent year-quarter and county fixed effects. Finally, $Everify_{csty}$ is defined as an indicator variable that identifies whether E-Verify legislation covering any private sector workers (regardless of firm size) was passed in county c in state s in y years after year-quarter t (or y years before for negative-valued y). We focus here on the effects of passage, rather than of enforcement, of any private sector E-Verify mandate since passage and enforcement of legislation mandating coverage for smaller firms is typically preceded by legislation mandating coverage of larger firms. As a result, even if the conditional exogeneity assumption is satisfied with regards to the passage of E-Verify legislation, labor market responses to initial passage have the potential to bias estimates derived from models that focus on dates of enforcement or on the dates on which mandates covering all private sector workers were passed. As noted, this emphasis on the timing of legislative passage of any private sector mandate also distinguishes our research design from the prior literature and is supported by the finding (presented below) that E-Verify system usage increases in response to initial mandate passage.

[24]We use the inverse hyperbolic sine (asinh) transformation of all dependent variables, unless otherwise noted, because some cells have zero values for employment, hires, or separations. Our results are similar when we use the natural log of labor market outcomes (dropping zero) or using ratios of outcomes to population. The asinh function closely parallels the natural logarithm function, but is well defined at zero (Card and Dellavigna, 2019).

Figure 15 demonstrates that E-Verify mandate passage sharply increases E-Verify usage by firms. Panel A shows the ratio of E-Verify queries to hires in states that passed any private sector mandate, by year relative to the date a mandate was passed. This ratio increases by 22 percentage points from four years prior to the mandate to four years after it, with an 15 percentage point jump during the first full year after the law was passed. Panels B through C show the ratio of queries to hires separately by firm size. The ratio of queries to hires in firms with fewer than 20 employees rises by 10 percentage points, with a three percentage point increase in the first full year after the law was passed. We find a similarly small responsiveness to mandates that explicitly cover all private sector firms. By contrast, larger firms are far more likely to use E-Verify and their usage pattern shows a noticeable increase after E-Verify mandates are passed. Firms with 20 or more employees have a 23 percentage point increase in the first full year after the law was passed.

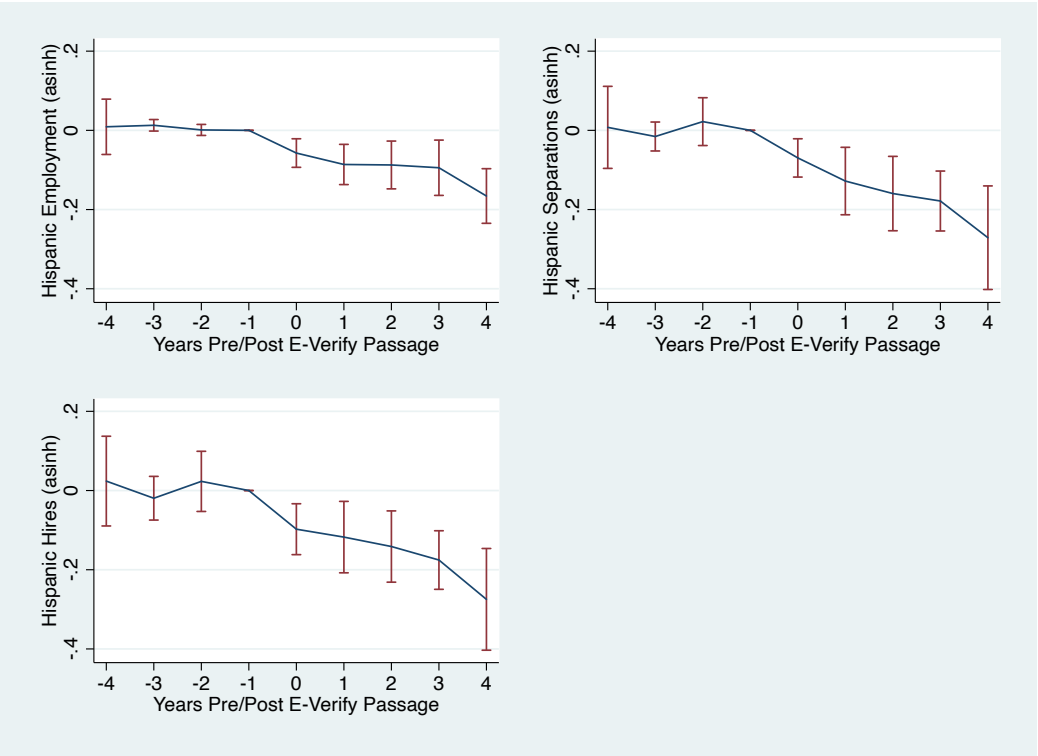
Figure 16 presents estimates from Equation 6 for outcomes characterizing Hispanic employment, separations, and hires. We find no evidence of statistically significant pre-trends in any of the outcomes. All three labor market outcomes decline in the year after E-Verify passage and the effect sizes tend to grow over the subsequent years. Importantly, these figures provide support for the identifying assumption that the declines in Hispanic employment, hires and separations after E-Verify passage that we will document cannot

Figure 15: Event Studies for E-Verify Usage by Firm Size



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the E-Verify rate, defined as the number of E-Verify queries divided by the total number of new hires in the referenced firm size bin(s), on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year-quarter fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0").

Figure 16: Event Studies for QWI-Based Hispanic Worker Outcomes (County-level)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year-quarter fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for Hispanic workers.

be attributed to differential pre-trends that would have predicted diverging outcomes even in the absence of E-Verify legislation.

In Figures 17-26, Appendix B, we present estimates from parallel event study models for all of the dependent variables that we consider below, in the QWI, ACS, and CBP samples. In most specifications, we find no evidence of pre-trends in outcomes. Below we note a few limited exceptions in which we assess the sensitivity of estimates to the inclusion of county-specific linear time trends.

3.4.2 Employment Outcomes In The QWI

We next estimate changes in E-Verify query rates and labor market outcomes associated with the passage and implementation of E-Verify legislation. An initial goal of our analysis is to assess whether effects of E-Verify mandates emerge after passage of legislation, before it goes into effect. The event studies presented in Figure 16 preview the finding that mandate passage impacts local labor market outcomes even prior to enforcement. Since E-Verify mandates apply only to newly-hired workers, we expect that there could be "job lock" based on immigration status among those who would be forced to verify employment eligibility if they switch employers. If true, this would lead to a decline in job separations among work-ineligible individuals after E-verify mandates are passed, even before they are enforced. A reduction in

separations could contribute to a concurrent reduction in hires among work-ineligible individuals. By contrast, whether we observe an immediate decline in employment is theoretically uncertain; to the extent that work-ineligible workers forgo job transitions and/or job search, we may see limited aggregate changes in employment even in the presence of significant declines in hires and separations.

We begin our analysis with labor market effects on Hispanic individuals measured in the QWI files. Our first research design builds on the existing literature and exploits state by year-quarter variation in E-Verify passage and enforcement in a multi-state difference-in-differences estimation framework. We estimate models at the county level, rather than state level, that more flexibly account for within-state differences across local labor markets and consequently generate more precise treatment effect estimates. The estimated specifications are of the following form:

$$Y_{cst} = \alpha + \beta_1 Everify_{cst,p} + \beta_2 Everify_{cst,e} + \gamma_t + \lambda_c + \epsilon_{cst} \tag{7}$$

The included regressors are as defined in Equation 6, with the exception of $Everify_{cst,p}$, an indicator variable equal to one if E-Verify legislation that covers **any** private sector workers has been passed in county c state s by year-quarter t, and $Everify_{cst,e}$, which characterizes whether a private sector mandate covering all workers is being enforced in county c state s

by year-quarter t. For comparison, we also present estimates that omit the $Everify_{cst,e}$ indicator in order to parallel the specifications used to generate event study plots in Figures 15 and 16. Here, we estimate Equation 7 using inverse hyperbolic sine transformations of the dependent variables because a number of our outcomes have a subset of zero-valued cells. Standard errors are clustered at the state level.[25]

Before examining labor market outcomes, we estimate the effect of E-Verify legislation on the fraction of new hires that are queried though the E-Verify system. Results are presented in the first two columns of Table XVI. Column 1 indicates that passage of any private-sector E-Verify mandate is associated with a 16.4 percentage point increase in the fraction of hires queried in the system. In the second column we separately control for both

[25]In Appendix Tables XXXII to XLII, we present corresponding regression estimates that include linear time trends; these represent our preferred specifications in the subset of cases for which event studies provide evidence of divergent pre-trends. In Tables XXXII to XLII, Appendix B, we also verify that our estimates are not sensitive to the inclusion of state-level covariates characterizing lagged labor market performance and the set of additional immigration enforcement measures already in place in county c in state s in year-quarter t. Specifically, following Orrenius and Zavodny (2015, 2016), we include the lagged unemployment rate, lagged log state GDP per capita, lagged log housing starts, and lagged log state government expenditures. We also include indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement, to restrict public benefits access for undocumented immigrants, or to strengthen protections for undocumented immigrants. Finally, we verify in Tables XXXII to XLII, Appendix B that estimates are robust to including all states in the sample (states that passed E-Verify mandates covering state agencies and/or state contractors/subcontractors but not covering other private sector firms are excluded in our benchmark specifications).

TABLE XVI: E-VERIFY QUERY RATES AND QWI-BASED HISPANIC WORKER OUTCOMES (COUNTY LEVEL)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	E-Verify Rate		Employment (asinh)		Separations (asinh)		Hires (asinh)	
Any Private Firm (Passage)	0.164***	0.136***	-0.087***	-0.076***	-0.133***	-0.090**	-0.139***	-0.097***
	(0.022)	(0.023)	(0.028)	(0.025)	(0.040)	(0.038)	(0.040)	(0.035)
All Private Firms (Enforcement)		0.106*		-0.040		-0.160***		-0.156***
		(0.057)		(0.050)		(0.050)		(0.046)
Year-Quarter FE	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X
Observations	92,609	92,609	82,099	82,099	82,099	82,099	82,099	82,099

Notes: The unit of observation is the county by year-quarter. Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year-quarter and All Private Firms (Enforcement) is an indicator for whether a private sector E-Verify mandate covering all firms is being enforced by the end of the given year-quarter. E-Verify rate is defined as the number of E-Verify queries divided by the contemporaneous total number of (Hispanic and non-Hispanic) hires. Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for Hispanic workers.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

passage of a mandate and enforcement of a mandate. Over half of the effect loads onto passage of an E-Verify mandate, further supporting our hypothesis that legislative passage (rather than subsequent enforcement) is the relevant determinant of the initial onset of local labor market responses.

The remaining columns of Table XVI present estimates of the impact of E-Verify mandates on labor market outcomes among Hispanics. Odd-numbered columns estimate treatment effects associated with E-Verify pas-

sage on employment, separations, and hires among Hispanic workers, while even-numbered columns include both the passage and enforcement regressors as indicated in Equation 7. In columns three, five, and seven we find a statistically significant 8.7 percent decline in Hispanic employment, a 13.3 percent decline in separations, and a 13.9 percent decline in hires. In columns four, six, and eight, in which we include separate indicators for both the passage and the enforcement of mandates, the coefficient associated with passage is larger than the coefficient associated with the date of enforcement in the employment model, while the opposite is true for separations and hires. In any case, passage and enforcement coefficients are not statistically distinguishable within each of these models.

As noted previously, the employment and turnover declines we estimate among Hispanic workers are notably larger than those found in prior work (Orrenius et al., 2018). Though our focus on date of mandate passage rather than enforcement could be expected to contribute to these divergent findings, in practice this is not the case. Effect sizes that grow over time mean that average post-event outcome values are higher in passage-based than enforcement-based models, while declining outcome values between passage and enforcement imply that pre-event outcomes are also higher in passage-based than in enforcement-based models. These pre- versus post-event differences across models appear similar in magnitude and so effectively cancel out. In contrast, the exclusion of linear time trends from our benchmark

models appears to explain most of the difference; in the presence of effects sizes that grow over time and in the absence of differential pre-trends, the inclusion of these linear time trends will tend to attenuate estimated treatment effects.

In Table XVII, we present estimates analogous to those in Table XVI but for non-Hispanic workers. Non-Hispanic workers could be affected in a number of ways. We estimate that 1.2 percent of Non-Hispanics are probabilistically undocumented and so their labor market outcomes could be negatively affected by the enactment of E-Verify mandates. The employment available to work-eligible individuals could increase or decrease, depending on whether they are substitutes or complements to individuals who are not eligible to work in the United States. Furthermore, if work-ineligible individuals experience "job lock", mobility for those who are work-eligible may also be depressed as a result, leading to declines in separations and hires above and beyond any measured employment effects. On net, the estimates in Table XVII indicate negative effects on labor market outcomes, though smaller than the effects on Hispanics. For example, passage of an E-Verify mandate is associated with declines of 2.9, 8.3, and 7.3 percent in employment, separations, and hires. While these negative impacts may appear to be large, we note that labor market outcomes among non-Hispanics display a slight downward trend prior to passage of E-Verify legislation, which, if not controlled, would bias our post-period estimates downwards (see Figure 17,

TABLE XVII: QWI-BASED NON-HISPANIC WORKER OUTCOMES (COUNTY LEVEL)

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment (asinh)		Separations (asinh)		Hires (asinh)	
Any Private Firm	-0.029***	-0.029***	-0.083***	-0.061***	-0.073***	-0.050**
(Passage)	(0.010)	(0.009)	(0.022)	(0.020)	(0.024)	(0.021)
All Private Firms		-0.002		-0.085***		-0.086***
(Enforcement)		(0.018)		(0.015)		(0.019)
Year-Quarter FE	X	X	X	X	X	X
County FE	X	X	X	X	X	X
Observations	82,099	82,099	82,099	82,099	82,099	82,099

Notes: The unit of observation is the county by year-quarter. Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year-quarter and All Private Firms (Enforcement) is an indicator for whether a private sector E-Verify mandate covering all firms is being enforced by the end of the given year-quarter. Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for non-Hispanic workers.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Appendix B). When we control for a county-specific linear time trend, the pre-period trend goes away and our post-period effects are small in magnitude and generally not statistically significant at conventional levels.[26] Importantly, given the low share of non-Hispanics likely to be work-ineligible, we

[26]We find declines of 1.0, 1.8, and 0.6 percent in employment, separations, and hires in these models, presented in Table XXXIII, Appendix B.

can rule out employment gains greater than two percent among work-eligible, non-Hispanics in response to the passage of E-Verify mandates.

We next turn to our analysis of labor market effects measured in the ACS, which allows us to identify average treatment effects for individuals who are likely to be undocumented based on additional observable characteristics, as well as effects on subgroups of native-born individuals. We estimate models similar to Equation 7, though the ACS data are annual and the only policy variable that we include is a dichotomous treatment variable indicating whether any private-sector E-Verify mandate has been passed by the end of a given year. Table XVIII first presents employment effects of E-Verify mandates by Hispanic ethnicity and undocumented status. Here employment excludes self-employment since self-employed individuals are not subject to E-Verify mandates. We examine changes in self-employment patterns separately in the subsequent analysis. Columns 1 and 2 show mandates are associated with a large 16.5 percent decline in employment among Hispanics and no effect among non-Hispanics, which mirror our results from the QWI. Columns three through five show that the policy impacts are largest for those we impute to be probabilistically undocumented. In particular, E-Verify mandates reduce employment by 17.5 percent among likely undocumented Hispanics, by 13.2 percent among likely documented Hispanics, and by 19.0 percent among all likely undocumented workers (regardless of ethnicity). Roughly one-quarter of Hispanic workers in the ACS sample are

TABLE XVIII: ACS-BASED EMPLOYMENT OUTCOMES (COUNTY LEVEL)

	(1) Hispanics	(2) Non-Hispanics	(3) Probabilistically Undocumented Hispanics	(4) Probabilistically Documented Hispanics	(5) Probabilistically Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Young, Male Low-Skilled Natives	(9) Old, Male Low-Skilled Natives
Any Private Firm	-0.165***	-0.006	-0.175*	-0.132**	-0.190***	-0.009	-0.027**	-0.068***	-0.007
(Passage)	(0.047)	(0.010)	(0.091)	(0.050)	(0.062)	(0.010)	(0.012)	(0.021)	(0.010)
Year FE	X	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X	X
Observations	23,246	23,246	23,246	23,246	23,246	23,246	23,246	23,246	23,246

Notes: The unit of observation is the county by year. Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year. Each outcome value is the inverse hyperbolic sine transform of the number of employed individuals with the referenced characteristic(s). Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

classified as probabilistically undocumented while only about one percent of non-Hispanic workers are classified accordingly, which buttresses our interpretation of the estimates from the QWI that larger (negative) labor market impacts for Hispanic workers are driven by the relatively higher share of work-ineligible individuals within this subpopulation.

A purported motivation for restricting employment opportunities among undocumented immigrants is to improve outcomes among the native-born. However, outcomes among the native-born could be helped or harmed, depending on whether they are substitutes or complements with undocumented migrant labor. Our estimates in the remaining columns of Table XVIII indicate that E-Verify mandates, in fact, reduce employment among some lower-skilled groups of native-born workers. The estimate in column six shows a fairly precisely estimated zero effect among the native-born population as a whole. However, the passage of any E-Verify mandate reduces employment among natives with a high school degree or less education by 2.7 percent. The last two columns indicate that this effect is entirely driven by reduced employment among low-skilled natives who are 16 to 40 years old, while there is no effect among older workers.

3.4.3 Heterogeneity In E-Verify Coverage And Adherence

In this section we extend our analysis to better understand the role of firms. To do so, we employ an alternative identification strategy that organizes the data by county, firm size, and year-quarter. We first examine heterogeneity in labor market impacts as a function of firm size. We leverage findings from these initial analyses to construct a county-level measure of predicted E-Verify exposure. Using this measure, we can control for unrestricted state-year-quarter fixed effects in our models to assuage any remaining concerns regarding internal validity and to assess the extent of within-state employment spillovers across areas with differing levels of predicted E-Verify coverage. To conduct the initial firm size-level analysis, we estimate models of the form

$$Y_{fcst} = \alpha + \beta Everify_{fcst} + \gamma_t + \gamma_{fc} + \epsilon_{fcst} \tag{8}$$

Here, Y_{fcst} reflects the outcome of interest for firm size bin f in county c in state s in year-quarter t and $Everify_{fcst}$ is a measure of whether E-Verify legislation that covers *any* firms in firm size bin f has been passed by the end of year-quarter t. γ_t is a year-quarter fixed effect and γ_{fc} is a firm-size bin-by-county fixed effect. Although the raw QWI includes five firm size bins, data is frequently censored or missing for three intermediate bins, corresponding to firms with 20-499 employees. Consequently, we divide

the sample into two bins: workers in firms with fewer than 20 employees and workers in firms with 20+ employees. Since data is least likely to be missing for the smallest (0-19 employee) bin, this approach allows us to maximize sample coverage by calculating employment in the 20+ employee bin as the difference between total employment and small firm employment. To maximize coverage, we impute missing employment levels within county-by-firm size bin cells, though we verify in Appendix B that results are not sensitive to this approach.

The estimates corresponding to Equation 8 are presented in the odd-numbered columns of Table XIX and characterize average treatment effects for the Hispanic subpopulation. In the even-numbered columns, we present split-sample equivalent estimates (including year-quarter-by-firm size bin fixed effects) to produce treatment effects separately by firm size bin. Column 2 indicates that aggregate employment declines are driven almost entirely by job losses in larger firms. Interestingly, declines in hires and separations are similar in smaller and larger firms, suggesting that even workers in low-adherence small firms may experience "job lock" after the passage of E-Verify mandates, perhaps due to concerns regarding the likelihood that they will find alternative employment within the set of firms that exhibit similarly low adherence to existing E-Verify mandates. In Table XXVIII, Appendix B, we present corresponding results for the non-Hispanic population. We do not find the same evidence of heterogeneous employment responses in

TABLE XIX: QWI-BASED HISPANIC WORKER OUTCOMES (COUNTY-BY-FIRM SIZE LEVEL)

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment (asinh)		Separations (asinh)		Hires (asinh)	
Covered	-0.058**		-0.133***		-0.140***	
(Passage)	(0.024)		(0.032)		(0.034)	
Covered x Small Firms		-0.019		-0.113***		-0.121***
(Passage)		(0.023)		(0.036)		(0.038)
Covered x Large Firms		-0.092***		-0.158***		-0.165***
(Passage)		(0.030)		(0.036)		(0.038)
County-by-Firm Size Bin FE	X	X	X	X	X	X
Year-Quarter FE	X		X		X	
YearQuarter-by-Firm Size Bin FE		X		X		X
Observations	149,898	149,898	149,898	149,898	149,898	149,898

Notes: The unit of observation is the firm size bin by county by year-quarter. Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees). Each outcome value is the inverse hyperbolic sine transform of the given measure. Covered is an indicator for whether a given firm size bin-by-county cell is covered by E-Verify legislation that has been passed by the end of the given year-quarter.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

smaller versus larger firms. Since the vast majority of non-Hispanic workers are work-eligible, this lack of heterogeneity is consistent with the hypothesis that differences in adherence across smaller versus larger firms explain the heterogeneous Hispanic employment responses that we identify.[27]

[27]Interestingly, we also find no evidence of heterogeneity in employment effects across industries as a function of likely undocumented employment shares.

We have documented stark differences in compliance with E-Verify mandates and in Hispanic employment effects across firms of varying sizes. We next examine the extent to which measured employment changes result from changes in the number of establishments in operation as compared to within-firm intensive margin changes in the number of employees. Increases in the cost of labor or in hiring costs could lead firms to close or relocate to other areas, or may deter firms from entering the market. We explore these effects using County Business Patterns (CBP) data. Table XX first presents coefficients from specifications that parallel those presented in Table XIX.[28] In columns one and two, the dependent variable is the total number of establishments in the given firm size bin. While the column 1 estimate indicates that E-Verify enforcement is associated with a (insignificant) 1.6 percent decline in the number of establishments, column 2 identifies a larger (and precisely-estimated) decline in the number of establishments with 20+ employees. These contrasting results are explained by the finding from Table XIX that employment declines are concentrated in larger firms. In column 3, we aggregate the data to the county-year-quarter level and identify a small and statistically insignificant 0.4 percent decline in the total number of establishments. This small aggregate effect is explained by the fact that most

[28]CBP data are available for the first quarter of each year from 2004 to 2015 and so we estimate specifications at the annual level and employ an E-Verify passage measure that is an indicator for whether a mandate has been passed by the end of the first quarter in a given year.

TABLE XX: CBP-BASED ESTABLISHMENT OUTCOMES (COUNTY AND COUNTY-BY-FIRM SIZE LEVEL)

	(1)	(2)	(3)	(4)	(5)	(6)
	Establishments (asinh)			Establishments, Weighted (asinh)		
Covered	-0.016			-0.034**		
(Passage)	(0.011)			(0.013)		
Covered x Small Firms		-0.008			-0.016	
(Passage)		(0.009)			(0.011)	
Covered x Big Firms		-0.027**			-0.054**	
(Passage)		(0.013)			(0.020)	
Any Private Firm			-0.004			-0.024**
(Passage)			(0.014)			(0.011)
County-by-Firm Size Bin FE	X	X		X	X	
Year FE	X		X	X		X
Year-by-Firm Size Bin FE		X			X	
County FE			X			X
Observations	49,181	49,181	24,809	49,181	49,181	24,809

Notes: The unit of observation is the firm size bin by county by year in Columns (1)-(2) and (4)-(5) and the county by year in Columns (3) and (6). Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees). Each outcome value is the inverse hyperbolic sine transform of the referenced measure. Covered is an indicator for whether a given firm size bin-by-county cell is covered by E-Verify legislation that has been passed by the end of the first quarter of the given year (establishment count data is available annually for the first quarter). Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the first quarter of the given year. Establishments (Weighted) scales the number of establishments in each of nine available firm size bins by the midpoint of the range of number of employees included in the given bin and then sums these scaled counts across the nine firm size bins.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

establishments have fewer than 20 employees and so changes in the number of larger establishments do not lead to significant changes in the total number of establishments. However, in columns four through six, we replace the dependent variable with a measure of the number of employment-weighted establishments.[29] This specification is designed to better capture the share of jobs lost due to the reduction in the number of establishments in operation. We find a larger (and statistically significant) 2.4 percent decline in the county-year-quarter specification. Though this estimate should be interpreted cautiously given the actual underlying distribution of establishment sizes within each bin is not available in the CBP, the point estimate would imply that roughly 60 percent of total job losses are due to the reduced number of establishments in operation.[30]

3.4.4 E-Verify Mandates And Employment Spillovers

In this subsection we assess the extent to which E-Verify mandates lead to shifts in employment from covered or compliant firms to others. In partic-

[29]As an example, a county with two firms with 1-19 employees in a given year would have a weighted establishment value of 20 (two times the midpoint of the 1-19 employee bin). In contrast, a county with one firm with 1-19 employees and one firm with 20-49 employees would have a weighted establishment value of 44.5 (the sum of midpoint of the 1-19 employee bin and the midpoint of the 20-49 employee bin).

[30]This estimate is based on the finding that passage of E-Verify legislation leads to a 3.8 percent reduction in total employment (combining the Hispanic and non-Hispanic samples) and a corresponding 2.4 percent decline in the number of employment-weighted establishments.

ular, some E-Verify mandates explicitly exclude small firms. Others phase-in coverage for small firms over time. We have also shown that usage of E-Verify at small firms is low and largely unresponsive to mandates. Much of the employment effect of E-Verify mandates is concentrated in large firms. To what extent, therefore, does a state mandate shift employment from larger to smaller firms? This is important because spillovers arguably represent a clear welfare loss and do not advance any of the purported goals of E-Verify proponents.

We begin this analysis in Table XXI, in which we leverage within-state variation in effective E-Verify coverage. Our prior analyses focused on changes in outcomes associated with passage of an E-Verify mandate. We now compare these to models that condition on a state by year-quarter fixed effect, which removes the common effect of passage of the mandate. The only remaining variation in E-Verify coverage in these models will be due to differences in the firm size distribution across counties. To the extent E-Verify coverage induces shifts in employment from high coverage to lower coverage areas, estimates in these models will be larger in magnitude than those in corresponding specifications that do not include state by year-quarter fixed effects.

To conduct this analysis, we exploit cross-county variation in the baseline share of employment in large firms in combination with variation in the timing of the passage of mandates covering each firm size bin and in adherence

TABLE XXI: E-VERIFY QUERY RATES AND QWI-BASED HISPANIC WORKER OUTCOMES AS A FUNCTION OF PREDICTED E-VERIFY COVERAGE (COUNTY LEVEL)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	E-Verify Rate		Employment (asinh)		Separations (asinh)		Hires (asinh)	
Predicted Coverage	0.213***	0.345**	-0.104***	-0.382***	-0.154***	-0.046	-0.159***	-0.023
	(0.026)	(0.133)	(0.033)	(0.107)	(0.046)	(0.130)	(0.046)	(0.165)
County FE	X	X	X	X	X	X	X	X
Year-Quarter FE	X		X		X		X	
YearQuarter-by-State FE		X		X		X		X
Observations	92,289	92,289	75,919	75,919	75,919	75,919	75,919	75,919

Notes: The unit of observation is the county by year-quarter. E-Verify rate is defined as the number of E-Verify queries divided by the contemporaneous total number of (Hispanic and non-Hispanic) hires. Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for Hispanic workers. To construct the Predicted Coverage measure, we first calculate the predicted share of workers covered by E-Verify legislation that has been passed by the end of the given year-quarter, as determined by the baseline (2004-2006) firm size distribution for all workers (in Columns 1-2) and for Hispanic workers (in Columns 3-8). This measure is then scaled by 0.227 for workers in small firms (with fewer than 20 employees) to account for the relative intensity of E-Verify usage across smaller versus larger firms.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

to these mandates. Specifically, we use data from 2004 to 2006 (before the passage of the first relevant E-Verify mandate) to construct county-specific measures of the share of employment in firms with 20+ employees. We then construct a time-varying county-level coverage measure that captures the share of private sector jobs that would be expected to adhere to E-Verify mandates in each year-quarter based on this baseline firm size distribution. Effective coverage is zero if a given firm size bin is not yet covered by an E-Verify mandate. To measure effective coverage conditional on the passage of a mandate, we exploit variation in adherence, as measured using DHS E-Verify query data. Based on estimates from a specification that parallels those included in Table XIX but replaces the dependent variable with the firm size-specific E-Verify query rate, we thus scale the effective coverage of small firms by a factor of 0.23 to account for the relatively smaller "first stage" magnitude (characterizing the relationship between mandate passage and E-Verify query rate) in small firms as compared to large firms. As an example, a county with 50 percent of employment in small firms at baseline has an effective coverage rate of 50 percent in each quarter in which only large firm mandates have been passed and has an effective coverage rate of 61.5 percent (50 percent+50 percent*0.23) in each quarter in which a mandate covers all firm sizes.[31]

[31]To confirm robustness, in Table XXIX, Appendix B we present results based on a coverage measure that uses only variation across firm sizes in the timing of mandate enforcement and ignores variation in adherence. Across specifications, estimated patterns of labor market effects appear qualitatively similar.

Odd-numbered columns of Table XXI present estimates that correspond to Equation 7, but replace the prior E-Verify passage and enforcement measures with this measure of predicted county-level coverage. Variation in coverage in these models is driven by passage of E-Verify mandates and the results closely mirror those presented in Table XVI. In the even-numbered columns of Table XXI, we add state-by-year-quarter fixed effects to the specifications from the corresponding odd-numbered columns. These fixed effects control for the state-wide mandate in place and so variation in coverage is driven by differences in the baseline firm size distribution. Column 2 validates this alternative approach by demonstrating that higher predicted coverage significantly increases E-Verify usage.

Turning to labor market outcomes, in column 4 we find a 38.2 percent decline in Hispanic employment in response to a 100 percentage point increase in predicted coverage. This point estimate is significantly larger than the benchmark employment decline estimated in column 3. Without state-by-year-quarter fixed effects, the estimate in column 3 captures both spillovers and the average pre-post difference in employment that results from the E-Verify mandate. In contrast, column 4 exploits only variation that is conditional on the set of mandates in place, and so the notably larger estimated treatment effect in this specification is consistent with sizable employment spillovers from local labor markets with higher to lower levels of predicted coverage. This large estimated employment decline also suggests that unob-

servable, time-varying state-level factors correlated with E-Verify mandate passage cannot explain the measured Hispanic employment declines presented previously. Turning to job turnover measures, the specifications in columns 6 and 8 provide little evidence of spillovers on the separations or hires margins, consistent with the finding that declines in separations and hires appear fairly uniform across the firm size distribution. For completeness, Table XXX, Appendix B presents parallel results for the non-Hispanic population; here, we find little evidence of comparable within-state employment spillovers for non-Hispanic workers.

To provide additional evidence on the extent of sub-state employment spillovers, Table XXII estimates employment changes in small firms for Hispanic and non-Hispanic workers as a function of the same county-level predicted coverage measure included in Table XXI specifications. Columns 1 and 4 demonstrate modest employment declines in small firms in response to higher county-level coverage rates (insignificant for Hispanics and significant for non-Hispanics but not statistically distinguishable across the two subpopulations). Columns 2 and 5 restrict the sample to county-year-quarter cells in which small firms are not yet subject to E-Verify mandate enforcement and results appear nearly identical. These findings are consistent with the possibility that E-Verify mandate passage has a modest deterrent effect on employment levels in uncovered small firms, perhaps due to anticipation of future coverage. Columns 3 and 6 add state-by-year-quarter fixed effects and

TABLE XXII: QWI-BASED SPILLOVER ANALYSES (SMALL FIRM EMPLOYMENT)

	(1)	(2)	(3)	(4)	(5)	(6)
	Hispanic Employment in Small Firms (asinh)			Non-Hispanic Employment in Small Firms (asinh)		
Predicted Coverage	-0.026	-0.027	0.353***	-0.032***	-0.034***	0.069*
	(0.026)	(0.019)	(0.116)	(0.011)	(0.010)	(0.040)
County-by-Firm Size Bin FE	X	X	X	X	X	X
Year-Quarter FE	X	X		X	X	
YearQuarter-by-State FE			X			X
Observations	74,005	67,756	74,005	74,005	67,756	74,005

Notes: The unit of observation is the county by year-quarter. Small firms are those with fewer than 20 employees. Each outcome value is the inverse hyperbolic sine transform of (Hispanic or non-Hispanic) employment in small firms. Columns 2 and 5 restrict the sample to county-year-quarter cells in which small firms are not yet subject to E-Verify mandate enforcement. To construct the Predicted Coverage measure, we first calculate the predicted share of workers covered by E-Verify legislation that has been passed by the end of the given year-quarter, as determined by the baseline (2004-2006) firm size distribution for Hispanic workers (in Columns 1-3) and for non-Hispanic workers (in Columns 4-6). This measure is then scaled by 0.227 for workers in small firms (with fewer than 20 employees) to account for the relative intensity of E-Verify usage across smaller versus larger firms.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

show that higher coverage is associated with a large and precisely-estimated 35.3 percent increase in Hispanic employment in small firms (the corresponding 6.9 percent estimate for non-Hispanics is notably smaller and is only marginally significant). This relative increase in small firm employment in response to higher county-level coverage, in a specification which differences out any common deterrent effect associated with state-level mandate passage,

is consistent with the presence of within-county spillovers as employment moves from larger (high-adherence) to smaller (uncovered or low-adherence) firms.

3.4.5 Understanding The Response To E-Verify Mandates

In the preceding analyses, we have established that the passage of E-Verify mandates led to reductions in employment among Hispanic workers in general and among undocumented workers in particular. We next explore a range of alternative outcomes to better understand how individuals and labor markets adjusted to changing E-Verify coverage. In particular, we ask whether employment verification requirements lead to declines in the likely work-ineligible population and changes in self-employment (which is not subject to employment verification). We conclude this analysis by investigating impacts of E-Verify mandates on individual wage and self-employment earnings, and overall changes in household income.

We begin in Table XXIII with an assessment of the impact of E-Verify mandates on the probabilistically undocumented population in a county. These are estimated using (person-weighted) population counts in the American Community Survey and a regression model similar to Equation 7, though the only policy variable is an indicator that any private-sector E-Verify mandate has been passed. The estimate in column 1 shows no effect of passage

TABLE XXIII: ACS-BASED MIGRATION AND SELF-EMPLOYMENT OUTCOMES (COUNTY LEVEL)

	(1) Probabilistically Undocumented Population	(2) In-migration Rate (Undocumented)	(3) Self-Employment All Workers	(4) Self-Employment Undocumented Workers
Any Private Firm	-0.002	0.004	-0.014	0.170**
	(0.062)	(0.007)	(0.019)	(0.072)
Year FE	X	X	X	X
County FE	X	X	X	X
Observations	23,246	22,522	23,246	23,246

Notes: The unit of observation is the county by year. In Column (1), the outcome value is the inverse hyperbolic sine transform of the number of probabilistically undocumented residents, defined as foreign-born, non-veterans who have not completed high school. The In-migration Rate measures the share of probabilistically undocumented respondents who moved to their current state of residence within the last year. The outcome measures in Columns (3)-(4) are the inverse hyperbolic sine transforms of the number of self-employed workers in each category. Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

of an E-Verify mandate on the probabilistically undocumented population. Next we assess whether passage of an E-Verify mandate affected the share of undocumented workers who moved to their current state of residence in the past year. Passage of a mandate would reduce this share if it leads to shifts in the undocumented population from states with mandates to those without. In fact, the estimate in column 2 indicates that the in-migration rate among undocumented workers is unaffected by passage of E-Verify legislation.

The remaining two columns of Table XXIII present estimates of the impact of E-Verify mandates on self-employment, measured through self-reports in the ACS. Self-employment is an important outcome because a potential effect of E-Verify is for undocumented workers to move from regular, payroll employment (which is captured in the QWI data and may be subject to an E-Verify mandate) to self-employment (which is not measured in the QWI and would not be subject to an E-Verify mandate). In particular, to the extent that firms, in response to E-Verify mandates, are able to reclassify some of their labor force from employees to independent contractors, the QWI data would show declines in employment. Column 3 measures the effect of passage of a mandate on self-employment among all workers and shows a fairly precise null effect. By contrast, the estimate in column 4 shows that passage of a mandate increases self-employment among undocumented workers by 17.0 percent. Though the estimate is sizeable, the baseline self-employment rate is 8.1 percent and so the increase in self-employment is small relative to the overall decline in wage and salary employment. Moreover, this estimate should be interpreted cautiously given evidence that self-employment among undocumented workers is already rising prior to E-Verify mandate passage (see Figure 22, Appendix B).

To provide a summary impact of passage of E-Verify mandates, we conclude with an analysis in Table XXIV of effects on individual and household labor market earnings. Our measures of annual earnings refer to income

TABLE XXIV: ACS-BASED PER CAPITA AND HOUSEHOLD ANNUAL EARNINGS MEASURES (COUNTY LEVEL)

	(1) Hispanics	(2) Non-Hispanics	(3) Probabilistically Undocumented Hispanics	(4) Probabilistically Documented Hispanics	(5) Probabilistically Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Young, Male Low-Skilled Natives	(9) Old, Male Low-Skilled Natives
				Panel A: Per Capita Wage Income					
Any Private Firm (Passage)	-0.180*** (0.053)	-0.040*** (0.014)	-0.122 (0.115)	-0.147** (0.056)	-0.263** (0.125)	-0.036** (0.015)	-0.045** (0.020)	-0.077** (0.029)	-0.024 (0.018)
				Panel B: Per Capita Business (Self-Employment) Income					
Any Private Firm (Passage)	0.203 (0.180)	-0.117*** (0.036)	0.324* (0.181)	0.218 (0.321)	0.225 (0.164)	-0.115*** (0.036)	-0.132*** (0.036)	-0.384*** (0.089)	-0.109** (0.047)
				Panel C: Per Capita Total (Wage and Business) Income					
Any Private Firm (Passage)	-0.090* (0.050)	-0.039*** (0.013)	-0.157 (0.105)	-0.104** (0.049)	-0.269*** (0.082)	-0.036** (0.013)	-0.043** (0.018)	-0.079*** (0.029)	-0.029* (0.016)
				Panel D: Per Capita Total (Wage and Business) Household Income					
Any Private Firm (Passage)	-0.064 (0.047)	-0.029*** (0.010)	-0.091 (0.106)	-0.083 (0.056)	-0.042 (0.073)	-0.027** (0.011)	-0.024** (0.010)	-0.045*** (0.011)	-0.019 (0.012)
Year FE	X	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X	X
Observations	23,196	23,239	19,948	23,182	22,522	23,239	23,239	23,239	23,239

Notes: The unit of observation is the county by year. Each outcome value is the inverse hyperbolic sine transform of mean annual earnings from the specified category for individuals with the referenced characteristic(s). Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year. Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

earned in the calendar year prior to the survey.[32] As above, we estimate the parameters of these models using an inverse hyperbolic sine transformation of the dependent variables. Panel A, which examines changes in wage and salary income, presents estimates that parallel corresponding employment effects: wage declines are significantly larger for Hispanics than non-Hispanics and for the likely work-ineligible populations as compared to natives. In Panel B, however, we examine self-employment income and find that Hispanics and likely work-ineligible individuals experience large (though generally imprecise) estimated increases in self-employment income, while non-Hispanics and natives experience significant declines.[33] Despite the increases in self-employment income, our estimates in Panel C indicate that total personal earnings (the sum of wage and self-employment income measures from the prior two panels) fall in response to passage of E-Verify mandates. Finally, in Panel D we assess effects on total household income from wages and self-employment. Though point estimates suggest that E-Verify mandates lead to declines in household earnings for all groups, estimated effects are smaller than the corresponding effects on individual earnings. This is especially true in column 5, which presents effects for probabilistically undocumented work-

[32]In Table XXXI, Appendix B, we estimate the relationship between the same annual earnings measures and lagged E-Verify mandate passage to ensure that results are not sensitive to the assumed timing of treatment effects. Estimates in Table XXXI, Appendix B parallel those in Table XXIV.

[33]As above, self-employment earnings estimates for undocumented workers should be interpreted cautiously given the evidence of pre-trends found in Figure 24, Appendix B.

ers, who experience an estimated 27 percent decline in their own earnings, but only a four percent decline in household earnings (which is not statistically different from zero). This indicates that the household members of respondents with higher rates of work ineligibility seemingly increase their earnings in response to the passage of E-Verify mandates, partly offsetting the direct negative effects estimated for the work-ineligible population and helping to explain the lack of a significant migration response (documented in Table XXIII).

3.5 Conclusions

This paper investigates the labor market impacts of employment eligibility authorization (E-Verify) mandates. A key contribution of our work is to document the impact of E-Verify mandates on usage of the system, relying on newly available administrative records from the Department of Homeland Security. Importantly, usage of E-Verify to verify employment eligibility of new hires is quite low in firms that employ fewer than 20 individuals. Mandates have a modest effect on usage, raising the ratio of queries to hires by about ten percentage points in the four years after a mandate is passed (from a baseline level of 4.5 percent). Usage in large firms is considerably higher, but still far from complete. In total, we estimate that four years after a mandate is passed, usage increases by 25 percentage points from a baseline level of 21 percent. Imperfect compliance in the face of a legal mandate is

noteworthy because it implies there are important monetary and/or non-monetary barriers to using the system. Enactment of a nationwide mandate would exacerbate these costs.

We use two primary data sources – the Quarterly Workforce Indicators and the American Community Survey – and two complimentary research designs to estimate the labor market impacts of E-Verify mandates. We document that passage of a mandate leads to significant declines in Hispanic employment and in the employment of likely work-ineligible subpopulations. Our estimates are larger than those found in prior research. We find no evidence that non-Hispanics or natives correspondingly benefit from mandate passage. Rather, we find significant employment declines among young, male, and less-educated native-born workers. Consistent with our findings regarding usage of the E-Verify system, firm size-level analyses reveal that much of the employment decline is concentrated in large firms. Our analysis of data from the County Business Patterns indicates that a substantial fraction of the employment decline is associated with a reduction in the number of large firms that locate in an area following passage of a mandate.

We find clear evidence that E-Verify mandates lead to a number of labor market distortions. First, mandates lead to reductions in both hires and job separations. These effects are largest for Hispanics but are also negative (though in some cases imprecisely-estimated) for non-Hispanic workers, consistent with market-wide declines in employment mobility in response to

E-Verify passage. Second, we find evidence of important within-state and within-county spillovers in employment from large to small firms.

In sum, while E-Verify mandates may significantly reduce formal sector employment among work-ineligible individuals, these policies are not effective in deterring undocumented migration. Moreover, the lack of gains experienced by native-born workers, the labor market distortions, and the disproportionate costs imposed on large firms suggest that the net aggregate costs associated with such mandates may be substantial.

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APPENDICES

APPENDIX A

TABLE XXV: EFFECTS OF BARTIK SHOCKS ON CHANGES IN EMPLOYMENT

	ln *Employment*
Bartik Shocks	0.783***
	(0.212)
N	135

Notes: The unit of observation is state by year. Bartik shocks and ln *Employment* are calculated for individuals aged 18-70 years old. The former is calculated by interacting industry-specific employment shares in each state in 1970 with the national industry growth rates between 1970-1977, 1970-1990, 1970-2000, 1970-2010, and 1970-2016. The latter is calculated by taking the natural log of employment. The sample is restricted to the 27 states available every year in the General Social Survey across 1977, 1990, 2000, 2010, and 2016. The regression includes state and year fixed effects with no additional controls. Standard errors are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

APPENDIX A (continued)

TABLE XXVI: EFFECTS OF GENDER-EDUCATION-SPECIFIC BARTIK SHOCKS ON CHANGES IN GENDER-EDUCATION EMPLOYMENT

	≤11 Years of Schooling		12-15 Years of Schooling		≥ 16 Years of Schooling	
	ln *Employment*		ln *Employment*		ln *Employment*	
	Men	Women	Men	Women	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Bartik Shocks for Men						
With ≤ 11 Years of Schooling	7.940***					
	(1.339)					
Bartik Shocks for Women						
With ≤ 11 Years of Schooling		4.586***				
		(0.842)				
Bartik Shocks for Men						
With 12-15 Years of Schooling			0.862***			
			(0.217)			
Bartik Shocks for Women						
With 12-15 Years of Schooling				-0.197		
				(0.208)		
Bartik Shocks for Men						
With ≥ 16 Years of Schooling					0.294**	
					(0.148)	
Bartik Shocks for Women						
With ≥ 16 Years of Schooling						-0.024
						(0.044)
N	135	135	135	135	135	135

Notes: The unit of observation is state by year. Bartik shocks and the natural log of employment are calculated separately for 6 sub-populations aged 18-70 years old: men with less than or equal to eleven years of schooling, women with less than or equal to eleven years of schooling, men with twelve to fifteen years of schooling, women with twelve to fifteen years of schooling. Men with at leasy sixteen years of schooling, and women with at least sixteen years of schooling. The sample is restricted to the 27 states available every year in the General Social Survey across 1977, 1990, 2000, 2010, and 2016. The regression includes state and year fixed effects with no additional controls. Standard errors are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

APPENDIX A (continued)

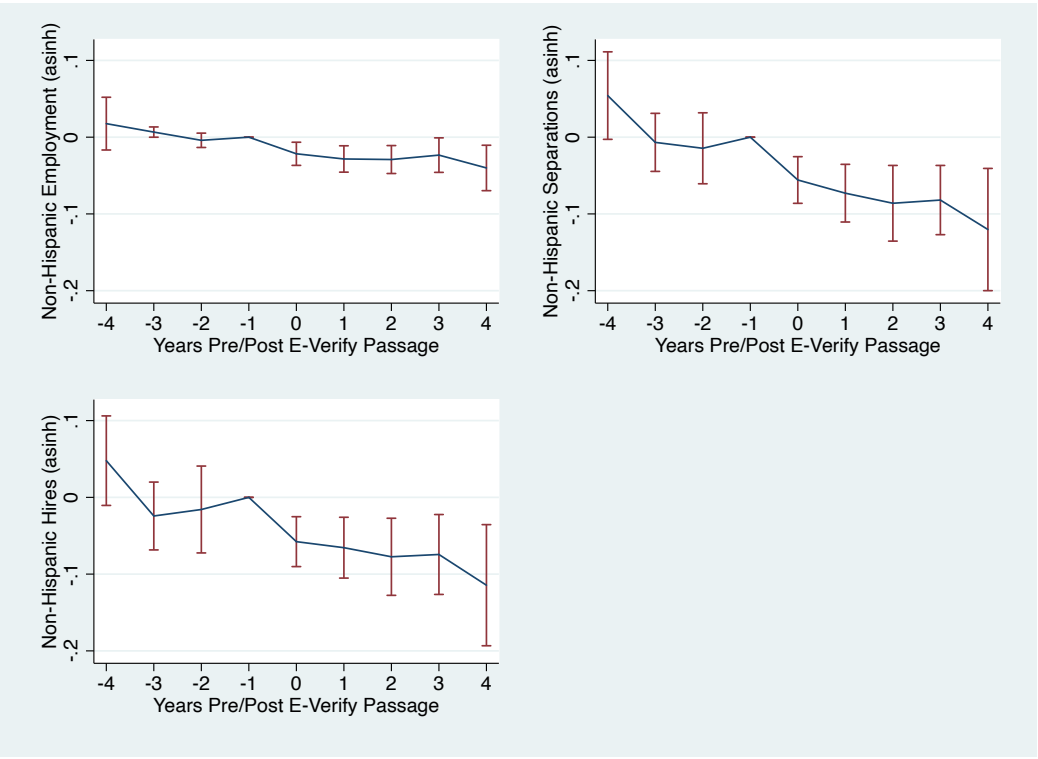
TABLE XXVII: PAIRWISE CORRELATION BETWEEN BARTIK SHOCKS OF DIFFERENT GENDER-EDUCATION SUBGROUPS

	≤11 Years of Schooling Bartik Shocks		12-15 Years of Schooling Bartik Shocks		≥ 16 Years of Schooling Bartik Shocks	
	Men	Women	Men	Women	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Bartik Shocks for Men With ≤ 11 Years of Schooling	1.000					
Bartik Shocks for Women With ≤ 11 Years of Schooling	0.978***	1.000				
Bartik Shocks for Men With 12-15 Years of Schooling	-0.897***	-0.912***	1.000			
Bartik Shocks for Women With 12-15 Years of Schooling	-0.952***	-0.924***	0.962***	1.000		
Bartik Shocks for Men With ≥ 16 Years of Schooling	-0.875***	-0.907***	0.958***	0.907***	1.000	
Bartik Shocks for Women With ≥ 16 Years of Schooling	-0.849***	-0.877***	0.931***	0.881***	0.990***	1.000

Notes: Each row, column pair reports the pairwise correlation coefficient between gender-education-specific Bartik shocks. There are 6 gender-education-specific Bartik shocks: Bartik shocks for men with less than or equal to eleven years of schooling, for women with less than or equal to eleven years of schooling, for men with twelve to fifteen years of schooling, for women with twelve to fifteen years of schooling, for men with at least sixteen years of schooling, and for women with at least sixteen years of schooling. * significant at 10 percent level, ** significant at 5 percent level, *** significant at 1 percent level

APPENDIX B

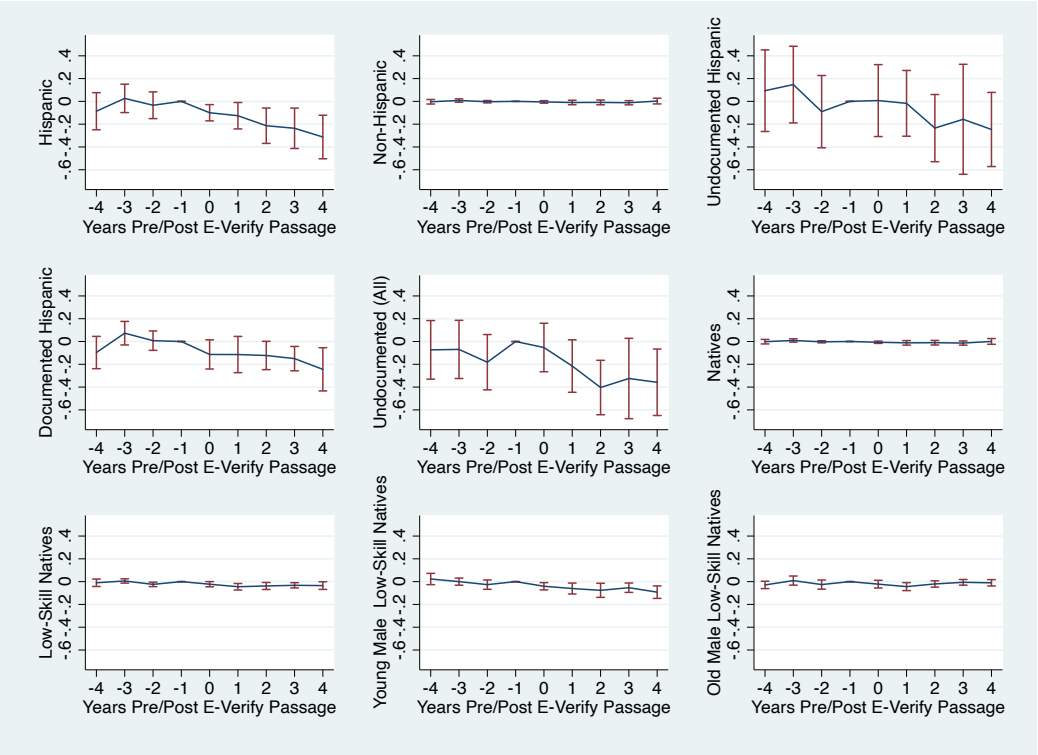
Figure 17: Event Studies for QWI-Based Non-Hispanic Worker Outcomes (County-level)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year-quarter fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for non-Hispanic workers.

APPENDIX B (continued)

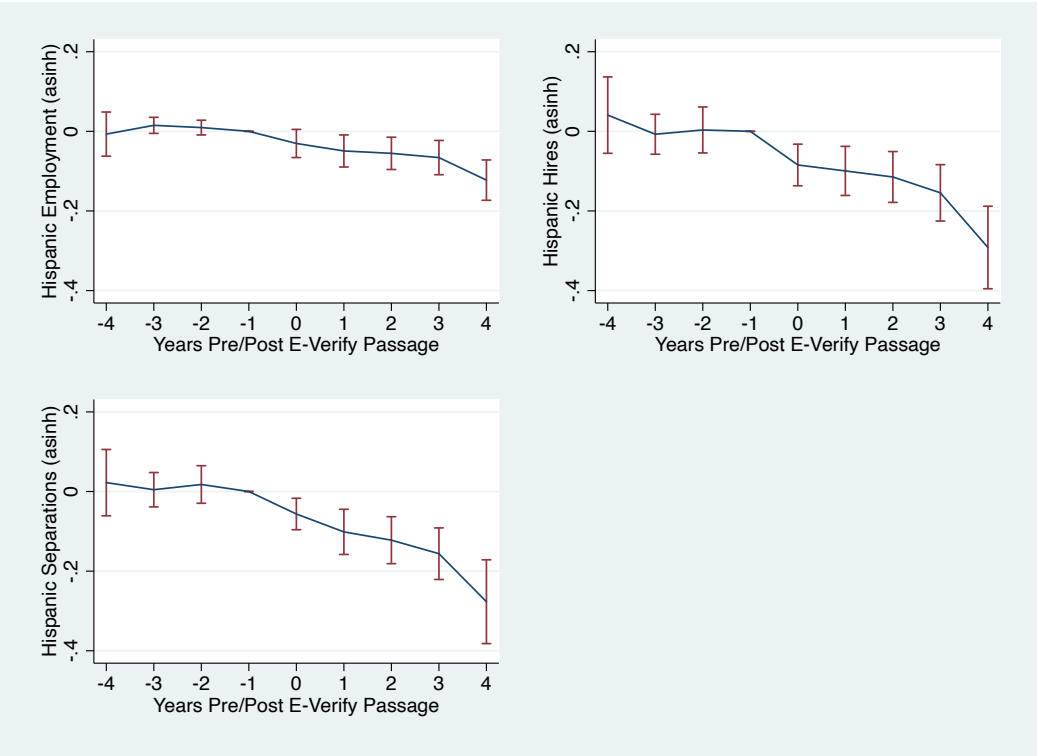
Figure 18: Event Studies for ACS-Based Worker Outcomes (County-level)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Each outcome value is the inverse hyperbolic sine transform of the number of employed individuals with the referenced characteristic(s). Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

APPENDIX B (continued)

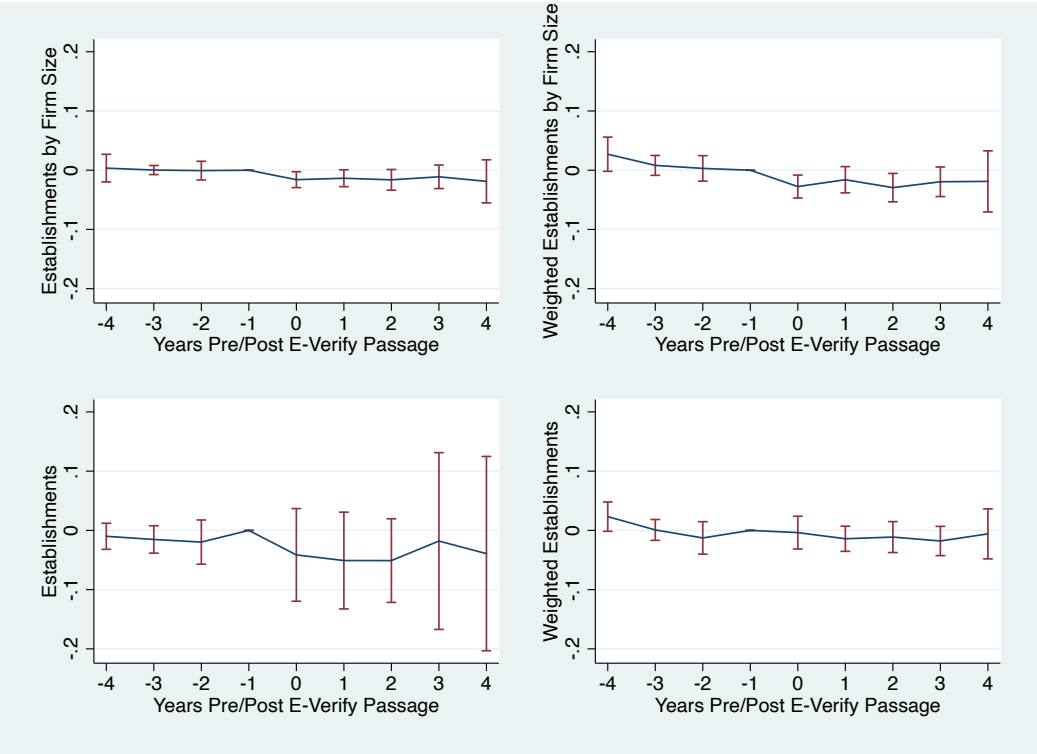
Figure 19: Event Studies for QWI-Based Hispanic Worker Outcomes (County-by-Firm Size Level)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-by-firm size bin level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate that covers the relevant firm size bin has been passed in the state in which a given county is located. Specifications include county-by-firm size bin and year-quarter fixed effects. $y = 0$ represents the year in which the first relevant private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for Hispanic workers in a given firm size bin. Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees).

APPENDIX B (continued)

Figure 20: Event Studies for CBP-Based Establishment Outcomes (County and County-by-Firm Size Level)

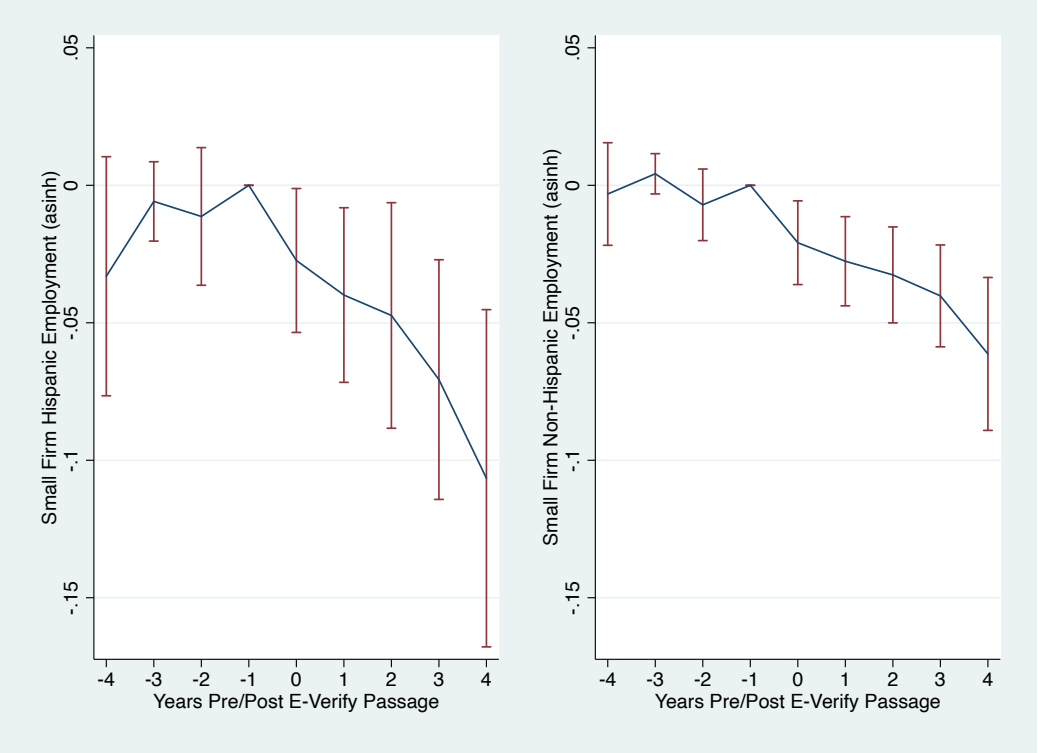


Notes: The upper two panels plot coefficients and 95% confidence intervals from a county-by-firm size bin level regression of the referenced outcome measure on a set of dummies for years before and after the first year by which a private sector E-Verify mandate that covers the relevant firm size bin has been passed by the end of Q1. Specifications include county-by-firm size bin and year fixed effects. $y = 0$ represents the year in which the first relevant private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees). Establishments (Weighted) scales the number of establishments in each of nine available firm size bins by the midpoint of the range of number of employees included in the given bin and then sums these scaled counts across the nine firm size bins.

The lower two panels plot coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first year by which a private sector E-Verify mandate has been passed by the end of Q1 in the state in which a given county is located. Specifications include county and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed by the end of Q1 and $y = -1$ is the omitted year (with the coefficient set equal to "0").

APPENDIX B (continued)

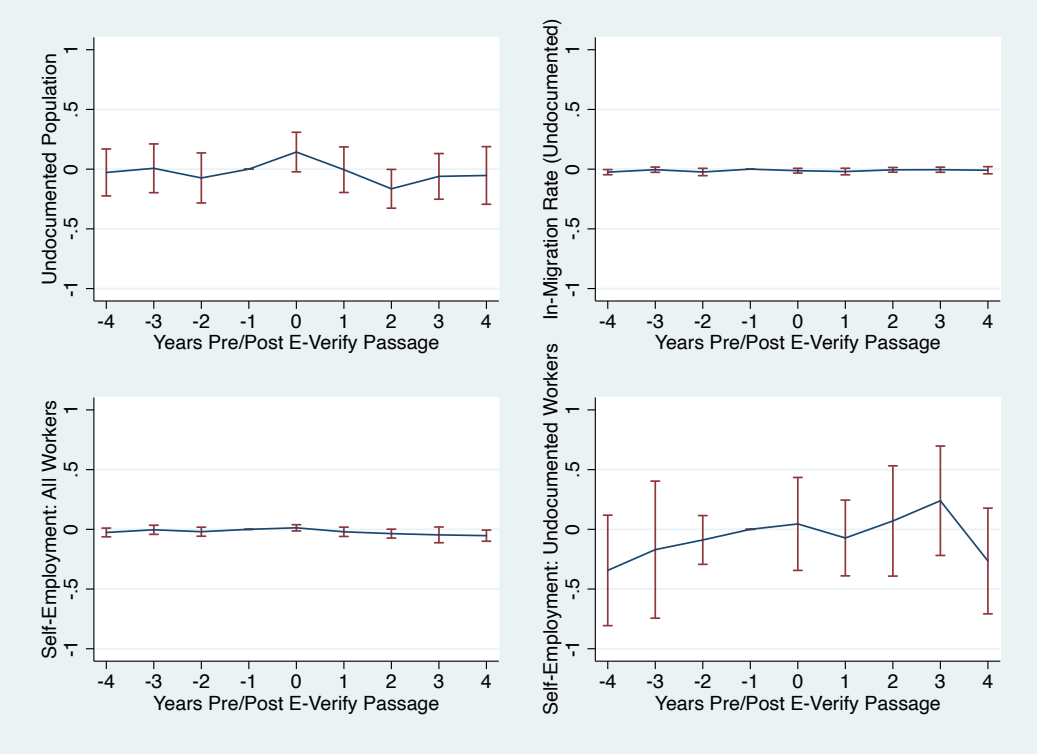
Figure 21: Event Studies for QWI-Based Spillover Analyses (Small Firm Employment)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year-quarter fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Small Firm Employment measures total (Hispanic or non-Hispanic) county-level employment in firms with fewer than 20 employees and the associated outcome measures are inverse hyperbolic sine transformations of these values.

APPENDIX B (continued)

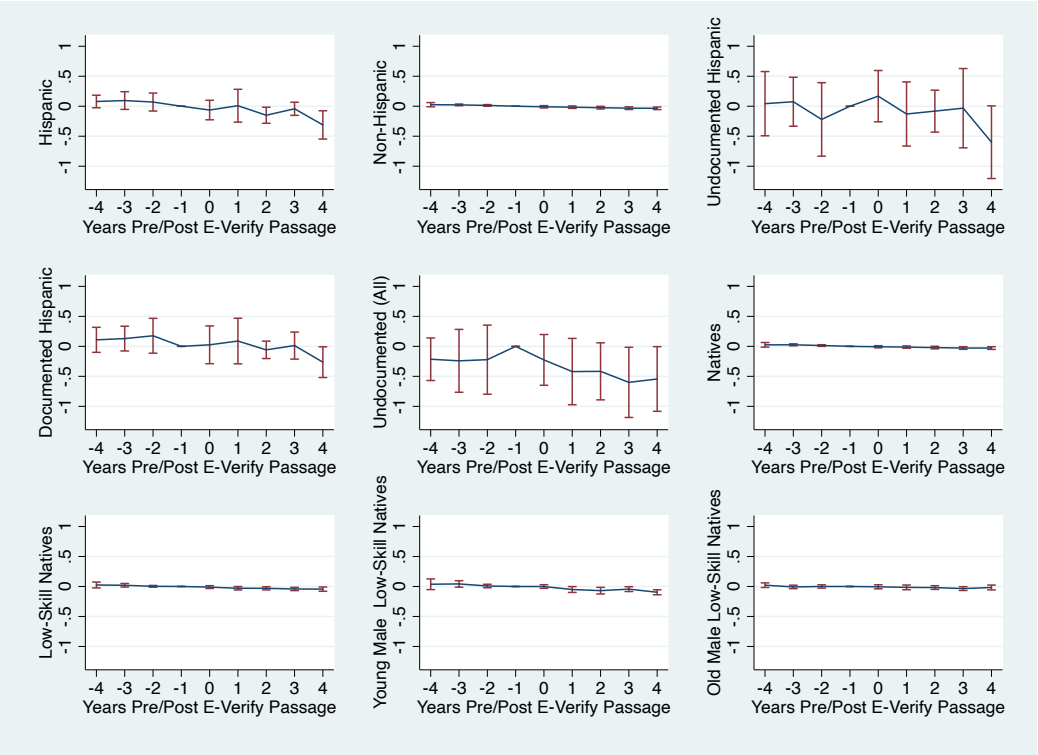
Figure 22: Event Studies for ACS-Based Migration and Self-Employment Outcomes (County Level)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Undocumented Population is the inverse hyperbolic sine transform of the number of probabilistically undocumented residents, defined as foreign-born, non-veterans who have not completed high school. The In-migration Rate measures the share of probabilistically undocumented respondents who moved to their current state of residence within the last year. The Self-Employment measures are the inverse hyperbolic sine transforms of the number of self-employed workers in each category.

APPENDIX B (continued)

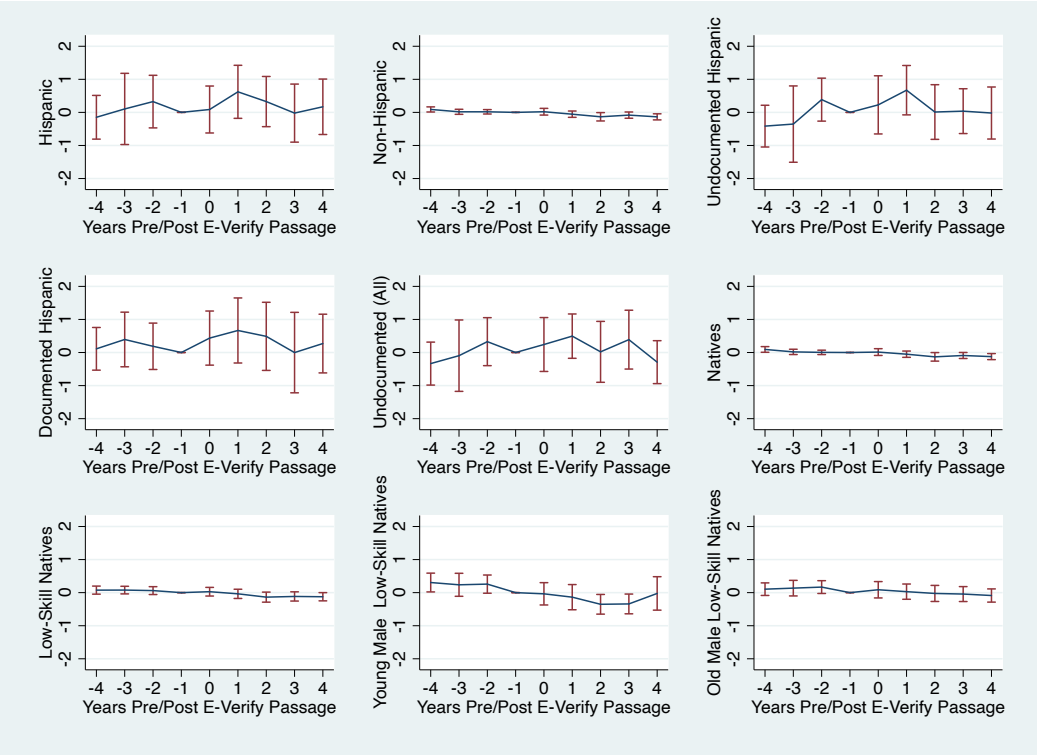
Figure 23: Event Studies for ACS-Based Per Capita Wage Income (County Level)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Each outcome value is the inverse hyperbolic sine transform of mean annual per capita wage income for individuals with the referenced characteristic(s). Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

APPENDIX B (continued)

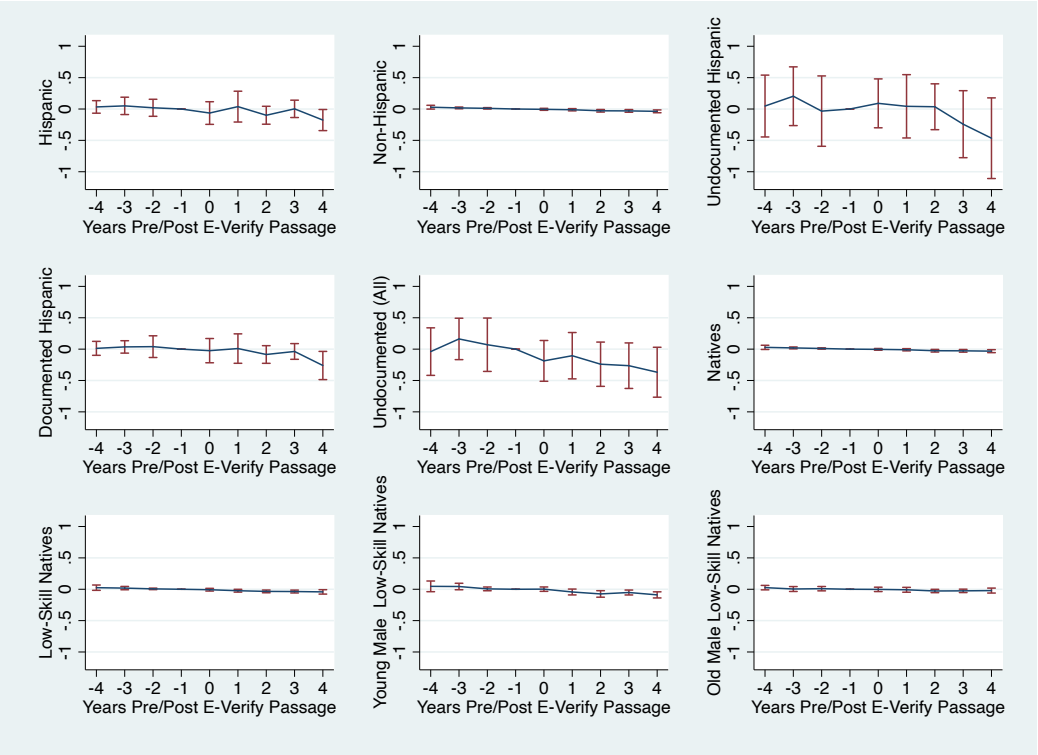
Figure 24: Event Studies for ACS-Based Per Capita Business Income (County Level)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Each outcome value is the inverse hyperbolic sine transform of mean annual per capita business (self-employment) income for individuals with the referenced characteristic(s). Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

APPENDIX B (continued)

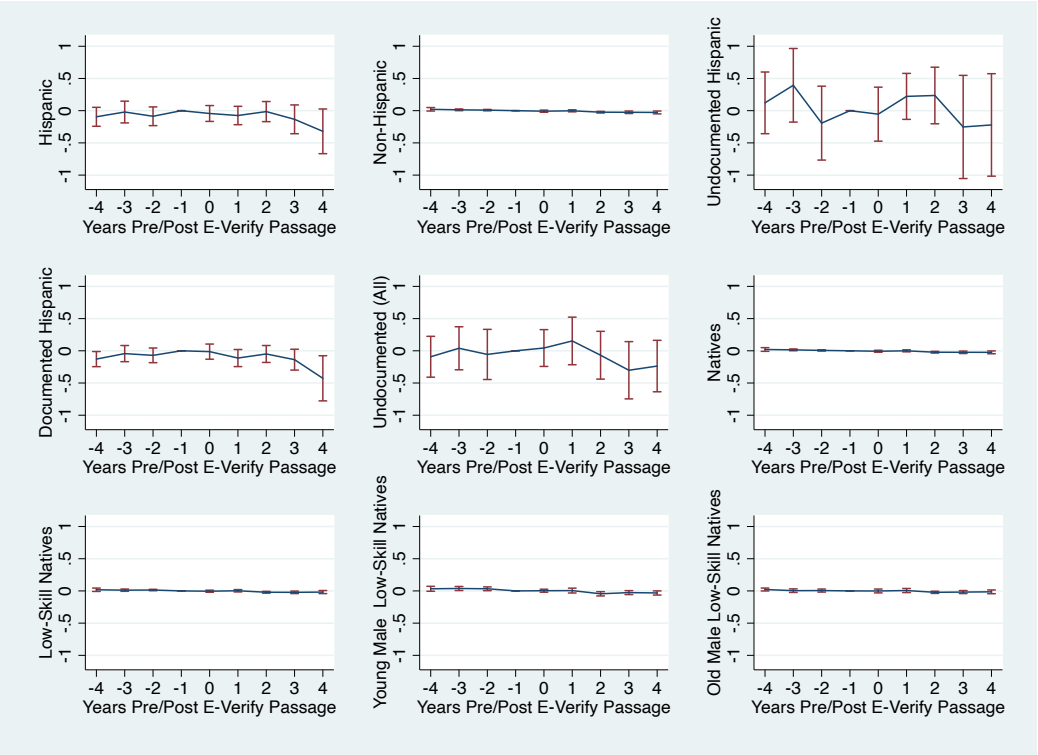
Figure 25: Event Studies for ACS-Based Per Capita Total Income (County Level)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Each outcome value is the inverse hyperbolic sine transform of mean annual per capita total (wage and business) income for individuals with the referenced characteristic(s). Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

APPENDIX B (continued)

Figure 26: Event Studies for ACS-Based Household Total Income (County Level)



Notes: Each panel plots coefficients and 95% confidence intervals from a county-level regression of the referenced outcome measure on a set of dummies for years before and after the first private sector E-Verify mandate has been passed in the state in which a given county is located. Specifications include county and year fixed effects. $y = 0$ represents the year in which the first private sector E-Verify mandate is passed and $y = -1$ is the omitted year (with the coefficient set equal to "0"). Each outcome value is the inverse hyperbolic sine transform of mean annual household total (wage and business) income for individuals with the referenced characteristic(s). Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

APPENDIX B (continued)

TABLE XXVIII: QWI-BASED NON-HISPANIC WORKER OUTCOMES (COUNTY-BY-FIRM SIZE LEVEL)

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment (asinh)		Separations (asinh)		Hires (asinh)	
Covered	-0.022**		-0.082***		-0.075***	
(Passage)	(0.010)		(0.023)		(0.025)	
Covered x Small Firms		-0.021**		-0.087***		-0.081***
(Passage)		(0.010)		(0.025)		(0.027)
Covered x Large Firms		-0.029**		-0.088***		-0.081***
(Passage)		(0.011)		(0.025)		(0.027)
County-by-Firm Size Bin FE	X	X	X	X	X	X
YearQuarter FE	X		X		X	
YearQuarter-by-Firm Size Bin FE		X		X		X
Observations	149,898	149,898	149,898	149,898	149,898	149,898

Notes: The unit of observation is the firm size bin by county by year-quarter. Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees). Each outcome value is the inverse hyperbolic sine transform of the given measure. Coverage is an indicator for whether a given firm size bin-by-county cell is covered by E-Verify legislation that has been passed by the end of the given year-quarter.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XXIX: E-VERIFY QUERY RATES AND QWI-BASED HISPANIC WORKER OUTCOMES AS A FUNCTION OF PREDICTED E-VERIFY COVERAGE (COUNTY LEVEL): ALTERNATIVE PREDICTED COVERAGE DEFINITION

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	E-Verify Rate		Employment (asinh)		Separations (asinh)		Hires (asinh)	
Predicted Coverage	0.215***	0.282**	-0.086***	-0.190***	-0.148***	-0.125	-0.147***	-0.208***
	(0.029)	(0.139)	(0.030)	(0.066)	(0.040)	(0.076)	(0.040)	(0.059)
County FE	X	X	X	X	X	X	X	X
Year-Quarter FE	X		X		X		X	
YearQuarter-by-State FE		X		X		X		X
Observations	92,289	92,289	75,919	75,919	75,919	75,919	75,919	75,919

Notes: The unit of observation is the county by year-quarter. Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for Hispanic workers. Predicted Coverage is defined by the share of workers covered by E-Verify legislation that has been enforced by the end of the given year-quarter, as determined by the baseline (2004-2006) firm size distribution for all workers (in Columns 1-2) and for Hispanic workers (in Columns 3-8).

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XXX: QWI-BASED NON-HISPANIC WORKER OUTCOMES AS A FUNCTION OF PREDICTED E-VERIFY COVERAGE (COUNTY LEVEL)

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment (asinh)		Separations (asinh)		Hires (asinh)	
Predicted Coverage	-0.035***	-0.078*	-0.100***	-0.032	-0.088***	-0.025
	(0.012)	(0.042)	(0.027)	(0.058)	(0.029)	(0.072)
County FE	X	X	X	X	X	X
Year-Quarter FE	X		X		X	
YearQuarter-by-State FE		X		X		X
Observations	75,919	75,919	75,919	75,919	75,919	75,919

Notes: The unit of observation is the county by year-quarter. Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for non-Hispanic workers. To construct the Predicted Coverage measure, we first calculate the predicted share of non-Hispanic workers covered by E-Verify legislation that has been passed by the end of the given year-quarter, as determined by the baseline (2004-2006) firm size distribution for non-Hispanic workers. This measure is then scaled by 0.227 for workers in small firms (with fewer than 20 employees) to account for the relative intensity of E-Verify usage across smaller versus larger firms.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XXXI: ACS-BASED PER CAPITA AND HOUSEHOLD ANNUAL EARNINGS MEASURES (COUNTY LEVEL LAGGED SPECIFICATIONS)

	(1) Hispanics	(2) Non-Hispanics	(3) Probabilistically Undocumented Hispanics	(4) Probabilistically Documented Hispanics	(5) Probabilistically Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Young, Male Low-Skilled Natives	(9) Old, Male Low-Skilled Natives
				Panel A: Per Capita Wage Income					
Any Private Firm	-0.169***	-0.039***	-0.240*	-0.152***	-0.314**	-0.035**	-0.046**	-0.085***	-0.025
(Lagged Passage)	(0.051)	(0.012)	(0.119)	(0.049)	(0.139)	(0.013)	(0.018)	(0.028)	(0.016)
				Panel B: Per Capita Business (Self-Employment) Income					
Any Private Firm	0.222	-0.138***	0.224	0.144	0.136	-0.133***	-0.154***	-0.369***	-0.132**
(Lagged Passage)	(0.211)	(0.033)	(0.239)	(0.318)	(0.198)	(0.033)	(0.035)	(0.109)	(0.049)
				Panel C: Per Capita Total (Wage and Business) Income					
Any Private Firm	-0.072	-0.039***	-0.222*	-0.112**	-0.240**	-0.036***	-0.044***	-0.087***	-0.030**
(Lagged Passage)	(0.052)	(0.011)	(0.121)	(0.053)	(0.090)	(0.012)	(0.015)	(0.028)	(0.013)
				Panel D: Per Capita Total (Wage and Business) Household Income					
Any Private Firm	-0.089	-0.028***	-0.055	-0.134**	-0.095	-0.025**	-0.023**	-0.046***	-0.020*
(Lagged Passage)	(0.056)	(0.009)	(0.118)	(0.064)	(0.076)	(0.010)	(0.009)	(0.011)	(0.010)
Year FE	X	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X	X
Observations	23,196	23,239	19,948	23,182	22,522	23,239	23,239	23,239	23,239

Notes: The unit of observation is the county by year. Each outcome value is the inverse hyperbolic sine transform of mean annual earnings from the specified category for individuals with the referenced characteristic(s). Any Private Firm (Lagged Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the prior year. Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XXXII: E-VERIFY QUERY RATES AND QWI-BASED HISPANIC WORKER OUTCOMES (COUNTY LEVEL): ADDITIONAL SPECIFICATIONS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	E-Verify Rate		Employment (asinh)		Separations (asinh)		Hires (asinh)	
				Panel A: County Linear Time Trends				
Any Private Firm	0.069***	0.063***	-0.033	-0.028*	-0.061*	-0.052**	-0.054	-0.047
(Passage)	(0.019)	(0.021)	(0.024)	(0.016)	(0.035)	(0.022)	(0.040)	(0.030)
Any Private Firm		0.123*		-0.077***		-0.144***		-0.116**
(Enforcement)		(0.061)		(0.017)		(0.027)		(0.044)
Observations	92,609	92,609	82,099	82,099	82,099	82,099	82,099	82,099
				Panel B: County Linear Time Trends + Covariates				
Any Private Firm	0.066***	0.061***	-0.029	-0.026*	-0.061**	-0.055***	-0.054	-0.049*
(Passage)	(0.018)	(0.022)	(0.019)	(0.014)	(0.029)	(0.015)	(0.035)	(0.026)
Any Private Firm		0.121**		-0.061***		-0.127***		-0.104***
(Enforcement)		(0.057)		(0.019)		(0.020)		(0.034)
Observations	92,609	92,609	82,099	82,099	82,099	82,099	82,099	82,099
				Panel C: All States				
Any Private Firm	0.174***	0.091***	-0.086***	-0.047*	-0.141***	-0.058	-0.152***	-0.070
(Passage)	(0.025)	(0.025)	(0.030)	(0.026)	(0.043)	(0.039)	(0.044)	(0.042)
All Private Firms		0.151**		-0.066		-0.193***		-0.187***
(Enforcement)		(0.058)		(0.047)		(0.047)		(0.048)
Observations	138,524	138,524	124,293	124,293	124,293	124,293	124,293	124,293
Year-Quarter FE	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X

Notes: The unit of observation is the county by year-quarter. Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year-quarter and All Private Firms (Enforcement) is an indicator for whether a private sector E-Verify mandate covering all firms is being enforced by the end of the given year-quarter. E-Verify rate is defined as the number of E-Verify queries divided by the contemporaneous total number of (Hispanic and non-Hispanic) hires. Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for Hispanic workers.

Panel A includes county linear time trends. Panel B includes county linear time trends as well as controls for the following covariates: lagged state-level unemployment rate, lagged state-level log GDP per capita, lagged state-level log housing starts, lagged state-level log government expenditures, and indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement, to restrict public benefits access for undocumented immigrants, or to strengthen protections for undocumented immigrants. Panel C presents benchmark specification estimates for all states (including states that have passed E-Verify legislation that covers only public sector workers and/or state contractors/subcontractors). All Panel C specifications include an indicator (omitted from the table) that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year-quarter. Even-numbered columns in Panel C also include an indicator (omitted from the table) that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no comprehensive private sector mandate is being enforced by the end of the given year-quarter.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XXXIII: QWI-BASED NON-HISPANIC WORKER OUTCOMES (COUNTY LEVEL): ADDITIONAL SPECIFICATIONS

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment (asinh)		Separations (asinh)		Hires (asinh)	
			Panel A: County Linear Time Trends			
Any Private Firm	-0.010	-0.010	-0.018	-0.014	-0.006	-0.002
(Passage)	(0.007)	(0.007)	(0.028)	(0.022)	(0.032)	(0.027)
Any Private Firm		-0.005		-0.061		-0.064
(Enforcement)		(0.008)		(0.048)		(0.050)
Observations	82,099	82,099	82,099	82,099	82,099	82,099
			Panel B: County Linear Time Trends + Covariates			
Any Private Firm	-0.009**	-0.009**	-0.017	-0.015	-0.005	-0.002
(Passage)	(0.004)	(0.004)	(0.022)	(0.019)	(0.028)	(0.025)
Any Private Firm		0.004		-0.039		-0.048
(Enforcement)		(0.004)		(0.036)		(0.040)
Observations	82,099	82,099	82,099	82,099	82,099	82,099
			Panel C: All States			
Any Private Firms	-0.042**	-0.032**	-0.099***	-0.039	-0.092***	-0.031
(Passage)	(0.017)	(0.013)	(0.026)	(0.027)	(0.028)	(0.029)
All Private Firms		-0.014		-0.124***		-0.124***
(Enforcement)		(0.019)		(0.025)		(0.027)
Observations	124,293	124,293	124,293	124,293	124,293	124,293
Year-Quarter FE	X	X	X	X	X	X
County FE	X	X	X	X	X	X

Notes: The unit of observation is the county by year-quarter. Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year-quarter and All Private Firms (Enforcement) is an indicator for whether a private sector E-Verify mandate covering all firms is being enforced by the end of the given year-quarter. Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for non-Hispanic workers.

Panel A includes county linear time trends. Panel B includes county linear time trends as well as controls for the following covariates: lagged state-level unemployment rate, lagged state-level log GDP per capita, lagged state-level log housing starts, lagged state-level log government expenditures, and indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement, to restrict public benefits access for undocumented immigrants, or to strengthen protections for undocumented immigrants. Panel C presents benchmark specification estimates for all states (including states that have passed E-Verify legislation that covers only public sector workers and/or state contractors/subcontractors). All Panel C specifications include an indicator (omitted from the table) that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year-quarter. Even-numbered columns in Panel C also include an indicator (omitted from the table) that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no comprehensive private sector mandate is being enforced by the end of the given year-quarter.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XXXIV: ACS-BASED EMPLOYMENT OUTCOMES (COUNTY LEVEL): ADDITIONAL SPECIFICATIONS

	(1) Hispanics	(2) Non-Hispanics	(3) Probabilistically Undocumented Hispanics	(4) Probabilistically Documented Hispanics	(5) Probabilistically Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Young, Male Low-Skilled Natives	(9) Old, Male Low-Skilled Natives
				Panel A: County Linear Time Trends					
Any Private Firm	-0.093*	-0.012	-0.016	-0.108*	-0.084	-0.013	-0.027*	-0.029	-0.029**
(Passage)	(0.052)	(0.008)	(0.130)	(0.060)	(0.075)	(0.008)	(0.014)	(0.021)	(0.011)
Observations	23,246	23,246	23,246	23,246	23,246	23,246	23,246	23,246	23,246
				Panel B: County Linear Time Trends + Covariates					
Any Private Firm	-0.085*	-0.013**	-0.010	-0.096*	-0.081	-0.014**	-0.025**	-0.028*	-0.027**
(Passage)	(0.047)	(0.006)	(0.137)	(0.053)	(0.082)	(0.006)	(0.012)	(0.014)	(0.011)
Observations	23,246	23,246	23,246	23,246	23,246	23,246	23,246	23,246	23,246
				Panel C: All States					
Any Private Firm	-0.136***	-0.004	-0.099	-0.119**	-0.126*	-0.009	-0.025**	-0.066***	-0.003
(Passage)	(0.048)	(0.009)	(0.100)	(0.047)	(0.063)	(0.009)	(0.011)	(0.019)	(0.010)
Observations	34,523	34,523	34,523	34,523	34,523	34,523	34,523	34,523	34,523
Year FE	X	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X	X

Notes: The unit of observation is the county by year. Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year. Each outcome value is the inverse hyperbolic sine transform of the number of employed individuals with the referenced characteristic(s). Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

Panel A includes county linear time trends. Panel B includes county linear time trends as well as controls for the following covariates: lagged state-level unemployment rate, lagged state-level log GDP per capita, lagged state-level log housing starts, lagged state-level log government expenditures, and indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement, to restrict public benefits access for undocumented immigrants, or to strengthen protections for undocumented immigrants. Panel C presents benchmark specification estimates for all states (including states that have passed E-Verify legislation that covers only public sector workers and/or state contractors/subcontractors). All Panel C specifications include an indicator (omitted from the table) that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XXXV: QWI-BASED HISPANIC WORKER OUTCOMES (COUNTY-BY-FIRM SIZE LEVEL): ADDITIONAL SPECIFICATIONS

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment (asinh)		Separations (asinh)		Hires (asinh)	
			Panel A: County Linear Time Trends			
Covered	-0.040**		-0.067*		-0.054	
(Passage)	(0.016)		(0.034)		(0.034)	
Covered x Small Firms		-0.007		-0.037		-0.027
(Passage)		(0.019)		(0.042)		(0.046)
Covered x Large Firms		-0.065***		-0.108***		-0.095**
(Passage)		(0.022)		(0.037)		(0.036)
Observations	149,898	149,898	149,898	149,898	149,898	149,898
			Panel B: No Interpolation Sample			
Covered	-0.050**		-0.125***		-0.132***	
(Passage)	(0.023)		(0.032)		(0.034)	
Covered x Small Firms		-0.014		-0.118***		-0.121***
(Passage)		(0.024)		(0.036)		(0.039)
Covered x Large Firms		-0.080***		-0.137***		-0.147***
(Passage)		(0.027)		(0.033)		(0.035)
Observations	122,833	122,833	122,833	122,833	122,833	122,833
			Panel C: All States			
Covered	-0.054**		-0.140***		-0.150***	
(Passage)	(0.025)		(0.034)		(0.036)	
Covered x Small Firms		-0.016		-0.113***		-0.128***
(Passage)		(0.024)		(0.035)		(0.038)
Covered x Large Firms		-0.088***		-0.175***		-0.180***
(Passage)		(0.030)		(0.039)		(0.039)
Observations	228,190	228,190	228,190	228,190	228,190	228,190
County-by-Firm Size Bin FE	X	X	X	X	X	X
Year-Quarter FE	X		X		X	
YearQuarter-by-Firm Size Bin FE		X		X		X

Notes: The unit of observation is the firm size bin by county by year-quarter. Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees). Each outcome value is the inverse hyperbolic sine transform of the given measure. Covered is an indicator for whether a given firm size bin-by-county cell is covered by E-Verify legislation that has been passed by the end of the given year-quarter.

Standard errors are clustered by state.

Panel A includes county linear time trends. Panel B presents benchmark specification estimates that drop rather than linearly interpolate missing outcome values. Panel C presents benchmark specification estimates for all states (including states that have passed E-Verify legislation that covers only public sector workers and/or state contractors/subcontractors). All Panel C specifications include an indicator (omitted from the table) that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year-quarter.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XXXVI: CBP-BASED ESTABLISHMENT OUTCOMES (COUNTY AND COUNTY-BY-FIRM SIZE LEVEL): ADDITIONAL SPECIFICATIONS

	(1)	(2)	(3)	(4)	(5)	(6)
	Establishments (asinh)			Establishments, Weighted (asinh)		
	Panel A: County Linear Time Trends					
Covered	-0.008			-0.007		
(Passage)	(0.007)			(0.011)		
Covered x Small Firms		-0.003			0.008	
(Passage)		(0.007)			(0.015)	
Covered x Big Firms		-0.021***			-0.028*	
(Passage)		(0.007)			(0.015)	
Any Private Firm			-0.015***			-0.006
(Passage)			(0.004)			(0.007)
Observations	49,181	49,181	24,809	49,181	49,181	24,809
	Panel B: County Linear Time Trends + Covariates					
Covered	-0.006			-0.009		
(Passage)	(0.006)			(0.009)		
Covered x Small Firms		-0.001			0.007	
(Passage)		(0.005)			(0.012)	
Covered x Big Firms		-0.020**			-0.030**	
(Passage)		(0.007)			(0.015)	
Any Private Firm			-0.012***			-0.006
(Passage)			(0.004)			(0.005)
Observations	49,181	49,181	24,809	49,181	49,181	24,809
	Panel C: All States					
Covered	-0.024			-0.047**		
(Passage)	(0.015)			(0.019)		
Covered x Small Firms		-0.014			-0.031*	
(Passage)		(0.011)			(0.016)	
Covered x Big Firms		-0.039**			-0.065**	
(Passage)		(0.019)			(0.025)	
Any Private Firm			-0.008			-0.036**
(Passage)			(0.014)			(0.017)
Observations	73,278	73,278	37,021	73,278	73,278	37,021
County-by-Firm Size Bin FE	X	X		X	X	
Year FE	X		X	X		X
Year-by-Firm Size Bin FE		X			X	
County FE			X			X

Notes: The unit of observation is the firm size bin by county by year in Columns (1)-(2) and (4)-(5) and the county by year in Columns (3) and (6). Firm size bins are classified as small (fewer than 20 employees) or large (20 or more employees). Each outcome value is the inverse hyperbolic sine transform of the referenced measure. Covered is an indicator for whether a given firm size bin-by-county cell is covered by E-Verify legislation that has been passed by the end of the first quarter of the given year (establishment count data is available annually for the first quarter). Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the first quarter of the given year. Establishments (Weighted) scales the number of establishments in each of nine available firm size bins by the midpoint of the range of number of employees included in the given bin and sums these scaled counts across the 9 firm size bins.

Panel A includes county linear time trends. Panel B includes county linear time trends as well as controls for the following covariates: lagged state-level unemployment rate, lagged state-level log GDP per capita, lagged state-level log housing starts, lagged state-level log government expenditures, and indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement, to restrict public benefits access for undocumented immigrants, or to strengthen protections for undocumented immigrants. Panel C presents benchmark specification estimates for all states (including states that have passed E-Verify legislation that covers only public sector workers and/or state contractors/subcontractors). All Panel C specifications include an indicator (omitted from the table) that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of Q1 of the given year.

Standard errors are clustered by state.

APPENDIX B (continued)

TABLE XXXVII: E-VERIFY QUERY RATES AND QWI-BASED HISPANIC WORKER OUTCOMES AS A FUNCTION OF PREDICTED E-VERIFY COVERAGE (COUNTY LEVEL): ADDITIONAL SPECIFICATIONS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	E-Verify Rate		Employment (asinh)		Separations (asinh)		Hires (asinh)	
				Panel A: County Linear Time Trends				
Predicted Coverage	0.089***	0.006	-0.041	0.014	-0.073*	0.235	-0.057	0.370
	(0.023)	(0.141)	(0.028)	(0.108)	(0.043)	(0.183)	(0.051)	(0.311)
Observations	92,289	92,289	75,919	75,919	75,919	75,919	75,919	75,919
				Panel B: No Interpolation Sample				
Predicted Coverage	0.213***	0.345**	-0.104***	-0.399***	-0.154***	-0.045	-0.159***	-0.014
	(0.026)	(0.133)	(0.033)	(0.120)	(0.046)	(0.142)	(0.046)	(0.163)
Observations	92,289	92,289	75,919	75,919	75,919	75,919	75,919	75,919
				Panel C: All States				
Predicted Coverage	0.223***	0.345**	-0.106***	-0.382***	-0.164***	-0.046	-0.175***	-0.023
	(0.030)	(0.133)	(0.036)	(0.106)	(0.050)	(0.129)	(0.051)	(0.164)
Observations	138,098	138,098	115,730	115,730	115,730	115,730	115,730	115,730
County FE	X	X	X	X	X	X	X	X
Year-Quarter FE	X		X		X		X	
YearQuarter-by-State FE		X		X		X		X

Notes: The unit of observation is the county by year-quarter. E-Verify rate is defined as the number of E-Verify queries divided by the contemporaneous total number of (Hispanic and non-Hispanic) hires. Employment, separations and hires measures reflect inverse hyperbolic sine transformations of the respective measures for Hispanic workers. To construct the Predicted Coverage measure, we first calculate the predicted share of workers covered by E-Verify legislation that has been passed by the end of the given year-quarter, as determined by the baseline (2004-2006) firm size distribution for all workers (in Columns 1-2) and for Hispanic workers (in Columns 3-8). This measure is then scaled by 0.227 for workers in small firms (with fewer than 20 employees) to account for the relative intensity of E-Verify usage across smaller versus larger firms.

Panel A includes county linear time trends. Panel B presents benchmark specification estimates that exclude interpolated firm size bin level employment values in the construction of the Predicted Coverage measure. Panel C presents benchmark specification estimates for all states (including states that have passed E-Verify legislation that covers only public sector workers and/or state contractors/subcontractors). Odd-numbered columns in Panel C include an indicator (omitted from the table) that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year-quarter.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XXXVIII: QWI-BASED SPILLOVER ANALYSES (SMALL FIRM EMPLOYMENT): ADDITIONAL SPECIFICATIONS

	(1)	(2)	(3)	(4)	(5)	(6)
	Hispanic Employment in Small Firms (asinh)			Non-Hispanic Employment in Small Firms (asinh)		
	Panel A: County Linear Time Trends					
Predicted Coverage	-0.046**	-0.053***	-0.080	-0.022**	-0.024**	0.061
	(0.018)	(0.017)	(0.109)	(0.010)	(0.010)	(0.042)
Observations	74,005	67,756	74,005	74,005	67,756	74,005
	Panel B: No Interpolation Sample					
Predicted Coverage	-0.022	-0.021	0.436***	-0.028**	-0.029***	0.037
	(0.027)	(0.020)	(0.095)	(0.012)	(0.009)	(0.058)
Observations	60,723	55,417	60,723	60,723	55,417	60,723
	Panel C: All States					
Predicted Coverage	-0.016	-0.018	0.353***	-0.043**	-0.041***	0.069*
	(0.027)	(0.019)	(0.116)	(0.017)	(0.014)	(0.040)
Observations	112,972	106,723	112,972	112,972	106,723	112,972
County-by-Firm Size Bin FE	X	X	X	X	X	X
Year-Quarter FE	X	X		X	X	
YearQuarter-by-State FE			X			X

Notes: The unit of observation is the county by year-quarter. Small firms are those with fewer than 20 employees. Each outcome value is the inverse hyperbolic sine transform of (Hispanic or non-Hispanic) employment in small firms. Columns 2 and 5 restrict the sample to county-year-quarter cells in which small firms are not yet subject to E-Verify mandate enforcement. To construct the Predicted Coverage measure, we first calculate the predicted share of workers covered by E-Verify legislation that has been passed by the end of the given year-quarter, as determined by the baseline (2004-2006) firm size distribution for Hispanic workers (in Columns 1-3) and for non-Hispanic workers (in Columns 4-6). This measure is then scaled by 0.227 for workers in small firms (with fewer than 20 employees) to account for the relative intensity of E-Verify usage across smaller versus larger firms.

Standard errors are clustered by state.

Panel A includes county linear time trends. Panel B presents benchmark specification estimates that exclude interpolated firm size bin level employment values. Panel C presents benchmark specification estimates for all states (including states that have passed E-Verify legislation that covers only public sector workers and/or state contractors/subcontractors). All Panel C specifications include an indicator (omitted from the table) that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year-quarter.

Standard errors are clustered by state.

APPENDIX B (continued)

TABLE XXXIX: ACS-BASED MIGRATION AND SELF-EMPLOYMENT OUTCOMES (COUNTY LEVEL): ADDITIONAL SPECIFICATIONS

	(1) Probabilistically Undocumented Population	(2) In-migration Rate (Undocumented)	(3) Self-Employment All Workers	(4) Self-Employment Undocumented Workers
		Panel A: County Linear Time Trends		
Any Private Firm	0.074	-0.014*	0.001	0.003
(Passage)	(0.062)	(0.007)	(0.013)	(0.169)
Observations	23,246	22,522	23,246	23,246
		Panel B: County Linear Time Trends + Covariates		
Any Private Firm	0.085	-0.013*	-0.001	-0.027
(Passage)	(0.058)	(0.007)	(0.014)	(0.167)
Observations	23,246	22,522	23,246	23,246
		Panel C: All States		
Any Private Firm	0.031	0.003	-0.014	0.165**
(Passage)	(0.061)	(0.006)	(0.018)	(0.072)
Observations	34,523	33,660	34,523	34,523
Year FE	X	X	X	X
County FE	X	X	X	X

Notes: The unit of observation is the county by year. In Column (1), the outcome value is the inverse hyperbolic sine transform of the number of probabilistically undocumented residents, defined as foreign-born, non-veterans who have not completed high school. The In-migration Rate characterizes the share of probabilistically undocumented respondents who moved to their current state of residence within the last year. The outcome measures in Columns (3)-(4) are the inverse hyperbolic sine transforms of the number of self-employed workers in each category.

Panel A includes county linear time trends. Panel B includes county linear time trends as well as controls for the following covariates: lagged state-level unemployment rate, lagged state-level log GDP per capita, lagged state-level log housing starts, lagged state-level log government expenditures, and indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement, to restrict public benefits access for undocumented immigrants, or to strengthen protections for undocumented immigrants. Panel C presents benchmark specification estimates for all states (including states that have passed E-Verify legislation that covers only public sector workers and/or state contractors/subcontractors). All Panel C specifications include an indicator (omitted from the table) that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XL: ACS-BASED PER CAPITA AND HOUSEHOLD ANNUAL EARNINGS MEASURES (COUNTY LEVEL WITH COUNTY LINEAR TIME TRENDS)

	(1) Hispanics	(2) Non-Hispanics	(3) Probabilistically Undocumented Hispanics	(4) Probabilistically Documented Hispanics	(5) Probabilistically Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Young, Male Low-Skilled Natives	(9) Old, Male Low-Skilled Natives
				Panel A: Per Capita Wage Income					
Any Private Firm	0.000	-0.007	0.169	0.066	-0.182	-0.006	-0.009	-0.009	0.001
(Passage)	(0.126)	(0.010)	(0.134)	(0.164)	(0.188)	(0.010)	(0.010)	(0.013)	(0.013)
				Panel B: Per Capita Business (Self-Employment) Income					
Any Private Firm	0.008	0.010	0.065	0.379	0.031	0.008	-0.031	-0.305*	0.001
(Passage)	(0.360)	(0.050)	(0.347)	(0.482)	(0.308)	(0.057)	(0.064)	(0.162)	(0.094)
				Panel C: Per Capita Total (Wage and Business) Income					
Any Private Firm	-0.004	-0.001	0.084	0.037	-0.205	-0.001	-0.008	-0.007	-0.001
(Passage)	(0.133)	(0.007)	(0.103)	(0.121)	(0.131)	(0.008)	(0.008)	(0.014)	(0.011)
				Panel D: Per Capita Total (Wage and Business) Household Income					
Any Private Firm	0.056	-0.001	-0.004	0.076	0.055	-0.001	-0.005	-0.005	0.003
(Passage)	(0.093)	(0.006)	(0.143)	(0.084)	(0.114)	(0.006)	(0.007)	(0.013)	(0.010)
Year FE	X	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X	X
Observations	23,196	23,239	19,948	23,182	22,522	23,239	23,239	23,239	23,239

Notes: The unit of observation is the county by year. Each outcome value is the inverse hyperbolic sine transform of mean annual earnings from the specified category for individuals with the referenced characteristic(s). Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year. Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

All specifications include county linear time trends.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XLI: ACS-BASED PER CAPITA AND HOUSEHOLD ANNUAL EARNINGS MEASURES (COUNTY LEVEL WITH COUNTY LINEAR TIME TRENDS AND ADDITIONAL COVARIATES)

	(1) Hispanics	(2) Non-Hispanics	(3) Probabilistically Undocumented Hispanics	(4) Probabilistically Documented Hispanics	(5) Probabilistically Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Young, Male Low-Skilled Natives	(9) Old, Male Low-Skilled Natives
				Panel A: Per Capita Wage Income					
Any Private Firm	-0.016	-0.007	0.170	0.047	-0.207	-0.007	-0.007	-0.011	0.005
(Passage)	(0.112)	(0.006)	(0.155)	(0.150)	(0.197)	(0.006)	(0.009)	(0.015)	(0.012)
				Panel B: Per Capita Business (Self-Employment) Income					
Any Private Firm	-0.045	0.010	0.011	0.338	-0.020	0.009	-0.028	-0.315*	0.013
(Passage)	(0.328)	(0.051)	(0.391)	(0.477)	(0.328)	(0.058)	(0.065)	(0.172)	(0.086)
				Panel C: Per Capita Total (Wage and Business) Income					
Any Private Firm	-0.021	-0.001	0.058	0.014	-0.234	-0.001	-0.006	-0.009	0.003
(Passage)	(0.117)	(0.004)	(0.131)	(0.104)	(0.139)	(0.004)	(0.009)	(0.016)	(0.011)
				Panel D: Per Capita Total (Wage and Business) Household Income					
Any Private Firm	0.051	-0.001	0.067	0.067	0.005	-0.000	-0.005	-0.009	0.005
(Passage)	(0.072)	(0.005)	(0.118)	(0.064)	(0.124)	(0.005)	(0.007)	(0.015)	(0.011)
Year FE	X	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X	X
Observations	23,196	23,239	19,948	23,182	22,522	23,239	23,239	23,239	23,239

Notes: The unit of observation is the county by year. Each outcome value is the inverse hyperbolic sine transform of mean annual earnings from the specified category for individuals with the referenced characteristic(s). Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year. Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

All specifications include county linear time trends as well as controls for the following covariates: lagged state-level unemployment rate, lagged state-level log GDP per capita, lagged state-level log housing starts, lagged state-level log government expenditures, and indicators for whether a state has any legislation in place to facilitate information-sharing with federal law enforcement, to restrict public benefits access for undocumented immigrants, or to strengthen protections for undocumented immigrants.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

APPENDIX B (continued)

TABLE XLII: ACS-BASED PER CAPITA AND HOUSEHOLD ANNUAL EARNINGS MEASURES (COUNTY LEVEL WITH ALL STATES INCLUDED)

	(1) Hispanics	(2) Non-Hispanics	(3) Probabilistically Undocumented Hispanics	(4) Probabilistically Documented Hispanics	(5) Probabilistically Undocumented (All Workers)	(6) All Natives	(7) Low-Skilled Natives	(8) Young, Male Low-Skilled Natives	(9) Old, Male Low-Skilled Natives
				Panel A: Per Capita Wage Income					
Any Private Firm	-0.173***	-0.043***	-0.067	-0.145***	-0.208*	-0.038***	-0.045**	-0.080***	-0.023
(Passage)	(0.045)	(0.014)	(0.114)	(0.050)	(0.110)	(0.014)	(0.019)	(0.029)	(0.016)
				Panel B: Per Capita Business (Self-Employment) Income					
Any Private Firm	0.220	-0.117***	0.310*	0.226	0.246	-0.115***	-0.123***	-0.306***	-0.122***
(Passage)	(0.163)	(0.034)	(0.185)	(0.272)	(0.176)	(0.034)	(0.035)	(0.078)	(0.041)
				Panel C: Per Capita Total (Wage and Business) Income					
Any Private Firm	-0.094**	-0.040***	-0.143	-0.105**	-0.252***	-0.036***	-0.042**	-0.078***	-0.029*
(Passage)	(0.044)	(0.012)	(0.100)	(0.044)	(0.072)	(0.013)	(0.017)	(0.028)	(0.015)
				Panel D: Per Capita Total (Wage and Business) Household Income					
Any Private Firm	-0.069	-0.031***	-0.079	-0.094	-0.048	-0.029***	-0.028**	-0.053***	-0.020*
(Passage)	(0.048)	(0.010)	(0.101)	(0.059)	(0.067)	(0.010)	(0.010)	(0.013)	(0.011)
Year FE	X	X	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X	X	X
Observations	34,472	34,516	30,375	34,454	33,660	34,516	34,516	34,516	34,516

Notes: The unit of observation is the county by year. Each outcome value is the inverse hyperbolic sine transform of mean annual earnings from the specified category for individuals with the referenced characteristic(s). Any Private Firm (Passage) is an indicator for whether any private sector E-Verify mandate has been passed by the end of the given year. Undocumented is a probabilistic measure corresponding to foreign-born, non-veteran respondents who have not completed high school. Probabilistically documented workers are those not classified as probabilistically undocumented. Low-Skilled corresponds to respondents who have no post-secondary education. The sample is restricted to respondents aged 16-64. Young corresponds to respondents aged 16-40 and Old corresponds to respondents aged 41-64.

This table presents benchmark specification estimates for all states (including states that have passed E-Verify legislation that covers only public sector workers and/or state contractors/subcontractors). All specifications include an indicator (omitted from the table) that is equal to one if a public sector or contractor/subcontractor E-Verify mandate but no private sector mandate has been passed by the end of the given year.

Standard errors are clustered by state.

* significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

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Ph.D., Economics, University of Illinois at Chicago, Chicago, Illinois, 2020

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B.A., Economics, Mathematics, and Political Science, Magna Cum Laude, St. Mary's College of Maryland, St. Mary's City, Maryland, 2014

WORKING PAPERS

Ayromloo, S.: Frailty, Thy Name is Still Woman? The Impact of Local Labor Demand Shocks on the Prevalence of Traditional Gender Attitudes. Job Market Paper, 2020.

Ayromloo, S., Feigenberg, B., Lubotsky, D.: States Taking the Reins? Employment Verification Requirements and Local Labor Market Outcomes. Revise and resubmit at Journal of Labor Economics, 2020.

WORKS IN PROGRESS

Ayromloo, S., Firsin, O.: Attitudes Toward Immigrants and Labor Market Conditions in the U.S. 2020.

Ayromloo, S.: Notes on Evolution of Gender-Specific Language and Job Opportunities for Women. 2020.

PROFESSIONAL PRESENTATIONS

The American Economic Association, San Diego, California, 2020

89th Southern Economic Association, Fort Lauderdale, Illinois, 2019

University of Illinois at Chicago EARL, Chicago, Illinois, 2019

83rd Midwest Economics Association, St. Louis, Missouri

48th Illinois Economics Association, Chicago, Illinois, 2018

University of Chicago Stigler Lunch Workshop, Chicago, Illinois, 2018

University of Illinois at Chicago Summer Earl, Chicago, Illinois, 2018

RESEARCH EXPERIENCE
Doctoral Researcher, Spring 2016- Spring 2020
Department of Economics, University of Illinois at Chicago

Conducted dissertation research

Research Assistant, Spring 2016- Spring 2017
Department of Economics, University of Illinois at Chicago

Assistant to Benjamin Feigenberg, and Darren Lubotsky

TEACHING EXPERIENCE
Course Instructor
Department of Social Sciences, Richard J. Daley College, Chicago, Illinois

Principles of Microeconomics, Fall 2019

Principles of Macroeconomics, Fall 2019

Teaching Assistant for Graduate Courses
Department of Economics, University of Illinois at Chicago, Chicago, Illinois

Microeconomics for Business, William Rauscher, Fall 2018- Fall 2019

Teaching Assistant for Undergraduate Courses
Department of Economics, University of Illinois at Chicago, Chicago, Illinois

Behavioral Economics, Helen Roberts, Spring 2020

Microeconomics: Theory and Applications, Benjamin Feigenberg, Fall 2017 & Spring 2020

Microeconomics: Theory and Applications, Erik Hembre, Spring 2018

Microeconomics: Theory and Applications, John A. Tauras, Fall 2017 - Spring 2018

Microeconomics: Theory and Applications, Jeffrey Cline, Summer 2017

Principles of Macroeconomics, Ali T. Akarca, Fall 2015

Principles of Microeconomics, Javaeria Qureshi, Spring 2015

Principles of Microeconomics, Lawrence H. Officer, Fall 2014

Teaching Assistant for Undergraduate Courses
Department of Economics, St. Mary's College of Maryland, St. Mary's City, Maryland

Intermediate Macroeconomics, Jia Xu, Fall 2012 - Spring 2013

Environmental Economics, Amy Henderson, Fall 2012

Economics Statistics, Barbara Beliveau, Fall 2011 - Spring 2012

CERTIFICATES
App Development with Swift Level 1, Apple Inc., May 11, 2019

TECHNICAL SKILLS
Programming languages and statistical packages: STATA, R, Python, C++, MATLAB, Swift

LANGUAGES
Persian: Native; English: Fluent

REFERENCES

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