Essays in Health, Migration, and Labor Economics

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Abstract

This thesis consists of three self-contained essays in the intersection of health, migration, and labor economics. The first chapter documents what I term the “Healthy Undocumented Immigrant Effect": undocumented immigrants are healthier than legal immigrants. I show that the undocumented immigrants’ health advantage can be attributed to the return-migrant effect.

In the second chapter, I examine the spillover impact of Verify Employment Eligibility (E-Verify) on highly-educated citizen women’s labor supply (particularly those with young children). Using variation in the implementation of E-Verify across states, I find that E-Verify reduces the labor supply of high-skilled citizen women by 0.3 to 1 percentage points. These estimates are larger for women with children. Supplemental analyses suggest that lower inflows of undocumented migrants is an important channel. A back-of-the-envelope calculation suggests that E-Verify generated \$6.1 billion in annual social costs of lower labor supply of high-skilled citizen women.

In the third chapter, I study the effects of Secure Communities (SC), a wide-ranging immigration enforcement program, on infant health outcomes in the United States. Using administrative birth certificate data together with event study and triple-difference designs, I find that SC increases the incidence of very low birth weight by 23% for infants of foreign-born Hispanic mothers, who were most likely to be affected by immigration enforcement. There is suggestive evidence that the results are consistent with (i) changes in maternal stress induced by deportation fear and (ii) inadequate prenatal nutrition. A back-of-the-envelope calculation suggests that the unintended social cost of immigration enforcement approaches \$2 billion annually.

JEL Codes: I10, J10, K37

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1 The Healthy Undocumented Immigrant Effect: Evidence from the US

1.1 Introduction

Relatively little is known about unauthorized immigrants' health outcomes. It is well documented that immigrants are on average healthier relative to comparable native-born populations (Moullan and Jusot, 2014; Neuman, 2014; Vang et al., 2017). This is known as the "Healthy Immigrant Effect" (Markides and Coreil, 1986; Palloni and Arias, 2004; Kennedy et al., 2015). However, past research often does not distinguish between legal and unauthorized immigrants. The reason is that surveys usually do not explicitly inquire about undocumented status or we did not have a valid method to detect unauthorized immigrants in micro datasets.[1] There are surveys, including (among others) the National Agricultural Workers Survey and the Survey of Income and Program Participation, that ask about documentation status. However, one might wonder whether immigrants would answer such questions or answer them honestly (Carter-Pokras and Zambrana, 2006).

This study adopts a newly developed method[2] constructed by George Borjas that

[1]There is a fair amount of recent research on the health of undocumented immigrants (see a review in Martinez et al. 2015). However, datasets are small. For example, Arbona et al. (2010) uses a sample of 416 Mexican and Central American immigrants, Chavez (2012) works with a sample of 1201 observations, and Poon et al. (2013) uses information on 1620 observations.

[2]Details about this method are in the Appendix. Borjas (2017) and Passel and Cohn (2014) developed a method that can be used to identify the undocumented population in micro survey data. Borjas (2016) and Borjas (2017) then used this method to study the labor supply and earnings of undocumented immigrants. Borjas and Slusky (2018) adopted the same method to explain the rapidly increasing in disability benefit recipients in the US, using undocumeted immigrants as a counterfactual.

imputes undocumented status in micro survey data to study the disparity between health outcomes of undocumented immigrants and legal immigrants which focuses on the question: Are undocumented immigrants relatively healthier than legal immigrants? Specifically, in my analysis, I identify individuals in the National Health Interview Survey (NHIS) that are likely undocumented immigrants. I then document the health difference across immigration groups what I term the "Healthy Undocumented Immigrant Effect," the notion that unauthorized immigrants are healthier than legal immigrants. Given this paradoxical finding, I evaluate possible explanations for this paradox. I find evidence suggesting that the return-migrant effect might account for the health disparity between undocumented immigrants and legal immigrants.

The rest of the paper proceeds as follows. Section 1.2 provides an overview of the data. Section 1.3 documents the healthy undocumented immigrant effect. In Section 1.4, I explore possible explanations for the health disparity between undocumented and legal immigrants. Section 1.5 offers some concluding remarks.

1.2 Data

I use data from the 1998-2017 National Health Interview Survey (NHIS) Integrated Public Use Microdata Series (Blewett et al., 2018). The NHIS is a cross-sectional household interview survey that collects annual data on the health status and medical conditions of a large, nationally representative sample of the US population. It samples approximately 35,000 households, containing about 87,500 persons per year. NHIS is suitable for this analysis because it contains information that helps detect undocumented status as well as health status. Also, the NHIS sample is large enough to allow a statistically reliable estimate of the undocumented population. I restrict the sample to foreign-born people aged 18-64 as few individuals aged 65 and older are imputed undocumented status. Therefore, I do not have the statistical power to draw robust conclusions for the elderly sample.

The algorithm that I use to impute undocumented status is the residual method developed by Warren and Passel (1987) and Passel and Cohn (2014). Borjas (2017) adapted the method to the Current Population Surveys. Roughly speaking, the algorithm classifies the foreign-born persons in the sample who are likely to be legal, the residual of foreign-born persons then are classified as likely undocumented immigrants.[3]

I classify the population into natives, legal immigrants (including naturalized citizens and legal residents), and undocumented immigrants. I next compare health outcomes between two immigrant groups: undocumented versus legal. Due to the fact that naturalized citizens have both immigrant status and the full protection of citizenship, I do robustness checks by comparing undocumented immigrant with naturalized citizen (Appendix Table A.2) and legal residents (Appendix Table A.3), separately.

In the NHIS data, my primary health outcomes are: 1) self-reported health status measured on the Likert scale score of 1 = Excellent, 2 = Very Good, 3 = Good, 4 = Fair, 5 = Poor and 2) psychological distress measured on the Kessler 6 (K6) scale. In my analysis, I define poor health as the bottom two measure on the Likert scale

[3]Details about the algorithm are in the Appendix.

(Likert score is 4 or 5). In 2016, for example, for people aged 18-64, the definition implies ten percent of the population was reported to be in poor health. Self-reported health is a strong prediction of serious chronic conditions and mortality, even when controlling for objective measures of health status and health behaviors (Goldstein et al., 1984; Idler and Kasl, 1995; Burström and Fredlund, 2001; Case et al., 2002; Wu et al., 2013).

I construct a measure of psychological distress based on K6 score that ranges from 0 to 24, with a higher score indicating higher level of mental health distress.[4] I then recode the mental health score into a dummy variable: mental distress (1 indicating respondent's K6 score is 5 or more, otherwise 0).

It is possible that self-reported measures of health status may be affected by measurement error or biases (Baker et al., 2004; Case et al., 2002; Currie and Stabile, 2003). Thus, together with self-reported health and mental distress, I present results for an indicator whether a person has any activity limitation, in which diagnosis and misreporting are unlikely.[5,6]

[4] In particular, I used the following six questions in the NHIS to measure mental health: (1) During the past 30 days, how often did you feel so sad that nothing could cheer you up? (2) During the past 30 days, how often did you feel nervous? (3) During the past 30 days, how often did you feel restless and fidgety? (4) During the past 30 days, how often did you feel hopeless? (5) During the past 30 days, how often did you feel that everything was an effort? (6) During the past 30 days, how often did you feel worthless? Respondents were asked to provide answers to these questions on a scale of 0 to 4 (none of the time, a little of the time, some of the time, most of the time, and all of the time).

[5] I use "lany" variable in the NHIS. "lany" is a recoded variable from several questions that indicates whether a person has any activity limitation.

[6] In the Appendix Table A.1, I present the results using an additional set of 9 potentially serious health conditions on which the NHIS collects information. These include whether the person has ever been diagnosed with chronic conditions (asthma, bronchitis, cancer, diabetes, heart disease, hypertension, liver condition, and ulcer), and information on whether the person is obese (BMI ≥ 30).

Table 1.1 shows the summary statistics for the main variables used in my analysis. The first row of Table 1.1 reports the fraction of the US population imputed as likely undocumented immigrants in the NHIS. This fraction is similar to the estimated fraction of undocumented immigrants by Pew Research Center and Borjas (2017) using the Current Population Survey. Undocumented populations tend to be younger, less educated, and less likely to be employed than legal immigrants. About health outcomes, while 10 percent of legal immigrants reported to be in poor health, only 7 percent of undocumented immigrants reported to be in poor health. Similarly, 17 percent of legal immigrants reported suffering from mental distress while 14 percent of unauthorized immigrants reported having mental distress in the past 30 days. Undocumented also less likely to report having any activity limit, 2.5 percent compared with 7 percent that of legal immigrants.

1.3 Healthy Undocumented Immigrant Effect

To examine the differences in health outcomes between legal immigrants and undocumented immigrants, I estimated the following logit regression model:

$$log\frac{p_{irt}}{1-p_{irt}} = \alpha + \beta Undocumented_{irt} + year_t + region_r + X_{irt} + \epsilon_{irt} \tag{1}$$

where p_{irt} is a dummy variable denoting whether person i in region r at time t reports health condition C. $C = \{$poor health, mental distress, any activity limit$\}$. X_{irt} consists of person i's demographic characteristics such as age, age squared, sex, race, dummies for education, married dummy, dummy for health insurance coverage, number of persons in family, and number of years spent in the US. $year_t$ are year fixed effects and $region_r$ are census region of residence fixed effects.[7] $Undocumented_{irt}$ is a dummy variable denoting whether person i is an undocumented immigrant. All specifications are estimated using sample weights,[8] and standard errors are clustered at the region level.

Table 1.2 reports estimates of equation (1) for all immigrants, Hispanic immigrants, and non-Hispanic immigrants using the NHIS sample of 1998-2017.[9] Column (1) of Table 1.2 shows the results for all immigrants, column (2) shows the results for Hispanic immigrants, column (3) shows the results for Non-Hispanic immigrants. As observed in Table 1.2, the marginal effects of Undocumented coefficients do not vary much when I stratify by Hispanic ethnicity.

The results in Panel A and B of Table 1.2 show that a lower proportion of undocumented immigrants reports poor health or mental distress. In particular, undocumented immigrants are 2.3 percent and 2.4 percent less likely reporting poor health and mental distress, respectively, than legal immigrants. It is surprising that undocumented migrants report lower levels of distress as access to needed services such as health, legal, educational and other social support services is non-existent or challenging for undocumented migrants in the US, and mental health services are even less accessible. Furthermore, anti-immigration policies in the US (Secure Communities, 287(g) program, E-Verify) have a negative impact on undocumented

[7]Region is the smallest geographic unit identified in the IPUMS NHIS. The four regions are: Northeast, North Central/Midwest, South, and West.

[8]The results are similar between weighting and not weighting (Solon et al., 2015)

[9]Inspired by the fact that the majority of undocumented are Hispanic (Krogstad et al., 2019), I stratify the sample to Hispanic and non-Hispanic immigrants.

immigrants' mental health outcomes (Martinez et al., 2015; Wang and Kaushal, 2018).

The results in Panel C indicate that among all immigrants, undocumented are 3.6 percent less likely having any activity limitation. Among Hispanic immigrants, undocumented Hispanics are 3.4 percent less likely reporting activity limitation than legal Hispanic immigrants. A similar disparity exists between undocumented non-Hispanic immigrants and legal non-Hispanic immigrants.

These results are robust with different group categorizations. In Table 1.2, I classify groups as legal immigrants (including both naturalized citizens and legal residents) and undocumented immigrants. The observed health advantage favoring the undocumented persists if I compare undocumented immigrants with naturalized citizens and legal residents separately. Results are presented in the Appendix Table A.2 and Table A.3. These results are also robust when comparing undocumented immigrants with matched legal immigrants sample (Table A.4).[10]

1.4 Understanding the Disparity

The results above have shown that undocumented immigrants in the United States experience better health outcomes than legal immigrants. This phenomenon is paradoxical because undocumented immigrants generally have lower access to social ben-

[10] Specifically, I construct a legal immigrant sample with observables similar to the undocumented sample, I estimate individual propensity scores with a logit specification that models the probability of undocumented status as a function of the age, age squared, sex, race, education, marital status, health insurance coverage status, number of persons in family, and number of years spent in the US. This propensity score is used to match undocumented immigrants to their nearest neighbor in the legal immigrant sample (with replacement), and only these matched legal immigrant observations are used in this robustness analysis.

efits than legal immigrants. I evaluate some potential explanations for the paradox in this section.

Selection effect. This hypothesis says that the paradox of healthy undocumented immigrant effect can be explained by selection effect, whereby undocumented immigrants who enter the US are disproportionately drawn from groups at country of origin whose health status is better than those who migrate legally. Due to the fact that most of the undocumented immigrants who come to the US do not expect to receive any social benefits, only the ones with better physical and psychological health from the population migrate. Thus, undocumented immigrants are healthier than those who do not migrate and maybe healthier than the average legal immigrants in the receiving country.

Health outcomes for undocumented immigrants and legal immigrants may become increasingly similar because both groups might integrate themselves into a new country by adopting the native population's social, cultural, and behavioral factors (Waters and Jiménez, 2005; Antecol and Bedard, 2006). The consequence is that the health advantage of undocumented immigrants should be diluted as number of years spent in the US increases, when age effects are held constant. Thus, if the selection effect prevails, I should observe a decreasing advantage as the number of years spent in the US increases.

I use data from the NHIS to examine whether the selection effect accounts for the better health outcomes of undocumented immigrants than legal immigrants. The NHIS collects information on the number of years a person spent in the US. However, the NHIS only collects this data in intervals: less than 1 year, 1-5 years, 5-10 years, 10-15 years, and 15 years or more. In Table 1.3, I show results of the effects of a dummy variable reflecting undocumented status relative to legal immigrants with different duration of stay.

The results in Table 1.3 do not support the selection effect. In fact, results in column (1) indicate insignificant effects at the shortest duration of stay. Results in columns (2) to (4) indicate larger significant health advantages for undocumented immigrants as number of years spent in the US increases. This suggests that the patterns found in the data do not support the selection effect hypothesis.[11]

Return-migrant effect. This explanation assumes that undocumented immigrants return to their home country following the period of illness or unemployment. The reason for higher return rate of undocumented immigrants than legal immigrants is the earlier group have no access to health care services when they are ill or social benefits when they are unemployed. The return of the sick undocumented immigrants will result in average better health outcomes for the undocumented population.

This line of reasoning might suggest the health disparity is larger at older ages if the return-migrant effect prevails. I use data from the NHIS to test this conjecture. Table 1.4 shows estimates of logit model in Equation 1, stratifying by age. Each parameter in Table 1.4 presents the health disparity between undocumented and legal immigrants in that age group, holding the number of years spent in the US constant.

[11]This is consistent with the findings of Palloni and Arias (2004) that selection effects or assimilation does not explain the Hispanic adult mortality advantage (Hispanics in the US experience lower mortality rates in adulthood than do non-Hispanic whites).

The results in Table 1.4 provide evidence that there are significant return-migrant effects. Moving from Column (1) to Column (4), I find a disparity in health that increases with age. In addition, the larger health disparity with the longer duration of stay found in Table 1.3 could reflect the attrition of unhealthier undocumented immigrants as the duration of stay increases and is consistent with return-migrant effect. These findings mean that the return-migrant effect may prove important in explaining the healthy undocumented immigrant effect.

1.5 Discussion and Conclusion

I first discuss two limitations of my article. First, I acknowledge that the residual method mistakenly impute undocumented status for around 25% of college graduated immigrants, despite accuracy for low-skilled immigrants (Albert, 2019). Specifically, Borjas and Cassidy (2019) suspects the algorithms mistakenly classify the high-skilled immigrants who are in the US temporarily under H-1B visa as undocumented.[12] However, I am not worry about this since it will result in downwardly bias the health disparity.[13]

Second, I reach the conclusion that the health advantage of undocumented immigrants is related to the return migration of those who are in poor health using indirect evidence. The direct test for the return-migrant hypothesis is to compare health outcomes of recent return undocumented migrants to the health outcomes of

[12] The stratification by education results (Table A.5) supports Borjas and Cassidy (2019)'s suspection. In particular, the results are mixed for high-skilled groups (some college and college graduate).

[13] Given that legal immigrants report worse health outcomes than undocumented immigrants found in this article.

undocumented migrants who remained in the US. Such a comparison is difficult, since there is no follow-up of undocumented migrants who return to their country. Future research with data on the return unauthorized immigrants could better explain the paradoxical finding here.

To conclude, this paper makes two contributions. First, I identify undocumented immigrants in the NHIS and document what I term the "Healthy Undocumented Immigrant Effect": undocumented immigrants are healthier than legal immigrants. Second, I test two hypotheses that may explain the health advantage of undocumented immigrants. I find evidence that the return-migrant effect may prove important in explaining the healthy undocumented immigrant effect.

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1.6 Tables

Table 1.1: Summary Statistics

	Natives	Legal	Undocumented
Percent of population	83.4	10.7	5.9
Male	48.7	47.8	55.6
Age	41.4	42.2	36.1
Self-reported overall health (Likert scale 1-5)	2.2	2.2	2.1
Poor health	10.6	10.3	7.4
Mental health (K6 score 0-24; higher values indicating higher levels of mental health distress)	2.6	2.1	1.8
Mental distress (K6 score ≥ 5)	19.7	16.8	14.4
Activity limit	12.87	6.97	2.54
Asthma	13.05	6.98	3.85
Cancer	5.49	2.61	0.82
Bronchitis	4.32	1.91	0.82
Diabetes	9.25	10.51	6.09
Heart condition	6.06	3.18	1.24
Hypertension	23.19	18.92	10.84
Liver condition	1.44	1.41	0.91
Obesity	31.32	22.91	22.59
Ulcer	6.85	4.60	2.98
High school drop out	7.55	19.40	39.13
High school graduate	29.67	24.05	23.42
Some college	33.47	24.17	13.76
College graduate	29.32	32.38	23.69
Percent employed	80.97	78.07	75.54
Percent married	56.37	71.02	58.50
Sample size	335,923	47,446	29,836

Notes: Weighted. The native's statistics are for reference only (not included in the regressions). Data are from 1998-2017 IPUMS NHIS. The sample includes persons aged 18-64. The values are in percentages.

Table 1.2: Logit Model, Health Outcomes by Immigration Status

	All immigrants (1)	Hispanic immigrants (2)	Non-hispanic immigrants (3)
Panel A: Poor health			
Undocumented	-0.023***	-0.023***	-0.023***
	(0.004)	(0.005)	(0.004)
Observations	79,750	44,927	34,823
Panel B: Mental distress			
Undocumented	-0.025***	-0.024***	-0.024***
	(0.006)	(0.007)	(0.005)
Observations	79,750	44,927	34,823
Panel C: Has any activity limitation			
Undocumented	-0.036***	-0.034***	-0.037***
	(0.003)	(0.006)	(0.009)
Observations	79,655	44,845	34,810
Demographics	X	X	X
Year and region fixed effects	X	X	X

Notes: Marginal effects are reported, with standard errors clustered at the region level in brackets. Each parameter is from a separate logit model regression of the outcome variable on a dummy variable equal to one if the person has the health condition listed in panels A - C. The reference category is legal immigrants. Data are from the 1998-2017 IPUMS NHIS. The sample includes legal and undocumented immigrants aged 18-64. All estimations include year fixed effects, region fixed effects, and demographic controls: age, age squared, sex, race, dummies for education, married dummy, dummy for health insurance coverage, number of persons in family, and number of years spent in the US. All results are estimated using sample weights. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 1.3: Logit Model, Health Outcomes by Number of Years Spent in the US

	0-5 year (1)	5-10 year (2)	10-15 year (3)	> 15 year (4)
Panel A: Poor health				
Undocumented	-0.007	-0.018***	-0.015***	-0.031***
	(0.011)	(0.001)	(0.005)	(0.006)
Observations	11,326	12,414	12,939	42,995
Panel B: Mental distress				
Undocumented	0.000	-0.019***	-0.026***	-0.037***
	(0.005)	(0.006)	(0.004)	(0.010)
Observations	11,326	12,414	12,939	42,995
Panel C: Has any activity limitation				
Undocumented	-0.017***	-0.016***	-0.020***	-0.052***
	(0.006)	(0.006)	(0.006)	(0.005)
Observations	11,279	12,204	12,920	42,956
Demographics	X	X	X	X
Age fixed effects	X	X	X	X
Year and region fixed effects	X	X	X	X

Notes: Each parameter is from a separate logit model regression of the outcome variable on a dummy variable equal to one if the person has the health condition listed in panels A - C. The reference category is legal immigrants (including both naturalized citizens and legal residents). Marginal effects are reported, with standard errors clustered at the region level in brackets. Data are from the 1998-2017 IPUMS NHIS. The sample includes legal and undocumented immigrants aged 18-64. All estimations include age fixed effects, year fixed effects, region fixed effects, and demographic controls: age squared, sex, race, dummies for education, married dummy, dummy for health insurance coverage, number of persons in family, and number of years spent in the US. All results are estimated using sample weights. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 1.4: Logit Model, Health Outcomes by Age Group

	18-24	25-36	37-49	50-64
	(1)	(2)	(3)	(4)
Panel A: Poor health				
Undocumented	-0.007	-0.013*	-0.021***	-0.051***
	(0.006)	(0.007)	(0.003)	(0.010)
Observations	8,307	26,957	26,581	17,895
Panel B: Mental distress				
Undocumented	-0.004	-0.013*	-0.034***	-0.049***
	(0.012)	(0.007)	(0.005)	(0.008)
Observations	8,317	26,957	26,581	17,895
Panel C: Has any activity limitation				
Undocumented	-0.005	-0.015***	-0.037***	-0.082***
	(0.004)	(0.001)	(0.007)	(0.007)
Observations	8,287	26,928	26,531	17,878
Demographics	X	X	X	X
Years spent in the US fixed effects	X	X	X	X
Year and region fixed effects	X	X	X	X

Notes: Each parameter is from a separate logit model regression of the outcome variable on a dummy variable equal to one if the person has the health condition listed in panels A - C. The reference category is legal immigrants (including both naturalized citizens and legal residents). Marginal effects are reported, with standard errors clustered at the region level in brackets. Data are from the 1998-2017 IPUMS NHIS. The sample includes legal and undocumented immigrants aged 18-64. All estimations include years spent in the US fixed effects, year fixed effects, region fixed effects, and demographic controls: age, age squared, sex, race, dummies for education, married dummy, dummy for health insurance coverage, number of persons in family. All results are estimated using sample weights. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

A Appendix

A.1 Identifying Undocumented Immigration Status[14]

Micro survey data do not include documentation status. As a result, I used Borjas (2016)'s algorithm to impute immigration status in the 1998-2017 NHIS. This approach is similar to "residual" methodologies used by Pew Research Center and the Department of Homeland Security to estimate the size of the undocumented immigrant population.

The algorithm is as follows: a foreign-born survey respondent is first identified as a documented immigrant if any of the following criteria apply:

a. that person arrived before 1980;

b. that person is a citizen;

c. that person receives Social Security benefits, SSI, Medicaid, Medicare, or Military Insurance;

d. that person is a veteran, is currently in the Armed Forces;

e. that person works in the government sector;

f. that person resides in public housing or receives rental subsidies, or that person is a spouse of someone who resides in public housing or receives rental subsidies;

g. that person was born in Cuba (as practically all Cuban immigrants were granted refugee status);

h. that person's occupation requires some form of licensing (such as physicians, registered nurses, air traffic controllers, and lawyers);

i. that person's spouse is a legal immigrant or citizen.

Any remaining foreign-born individuals are then categorized as likely to have undocumented immigration status.

[14] Borjas G. The Earnings of Undocumented Immigrants. NBER Working Paper 23236, page 9

A.2 Additional Tables

Table A.1: More Health Outcomes

	(1) Asthma	(2) Bronchitis	(3) Cancer
Undocumented Immigrants	-0.022***	-0.008***	-0.007***
	(0.004)	(0.002)	(0.001)
Observations	79,722	79,717	79,713
	(4) Diabetes	(5) Heart condition	(6) Hypertension
Undocumented Immigrants	-0.005***	-0.010***	-0.019***
	(0.002)	(0.002)	(0.007)
Observations	79,707	79,705	79,643
	(7) Liver condition	(8) Obesity	(9) Ulcer
Undocumented Immigrants	-0.003**	-0.010**	-0.010***
	(0.001)	(0.005)	(0.002)
Observations	79,699	79,750	79,672

Notes: Each parameter is from a separate logit model regression of the outcome variable on a dummy variable equal to one if the person has the health condition (ever diagnosed) listed in columns (1)-(9). The reference category is legal immigrants (including both naturalized citizens and legal residents). Marginal effects are reported, with standard errors clustered at the region level in brackets. Data are from the 1998-2017 IPUMS NHIS. The sample includes legal and undocumented immigrants aged 18-64. All estimations include year fixed effects, region fixed effects, and demographic controls: age, age squared, sex, race, dummies for education, married dummy, dummy for health insurance coverage, number of persons in family, and number of years spent in the US. All results are estimated using sample weights. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.2: Logit Model, Reference Group Is Naturalized Citizen

	All immigrants (1)	Hispanic immigrants (2)	Non-hispanic immigrants (3)
Panel A: Poor health			
Undocumented	-0.018***	-0.014**	-0.022***
	(0.004)	(0.006)	(0.004)
Observations	63,828	34,369	29,459
Panel B: Mental distress			
Undocumented	-0.017***	-0.010*	-0.022***
	(0.005)	(0.005)	(0.006)
Observations	63,828	34,369	29,459
Panel C: Has any activity limitation			
Undocumented	-0.030***	-0.027***	-0.029***
	(0.004)	(0.008)	(0.008)
Observations	63,744	34,297	29,447
Demographics	X	X	X
Year and region fixed effects	X	X	X

Notes: Each parameter is from a separate logit model regression of the outcome variable on a dummy variable equal to one if the person has the health condition listed in panels A - C. The reference category is naturalized citizen. Marginal effects are reported, with standard errors clustered at the region level in brackets. Data are from the 1998-2017 IPUMS NHIS. The sample includes legal and undocumented immigrants aged 18-64. All estimations include year fixed effects, region fixed effects, and demographic controls: age, age squared, sex, race, dummies for education, married dummy, dummy for health insurance coverage, number of persons in family, and number of years spent in the US. All results are estimated using sample weights. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.3: Logit Model, Reference Group Is Legal Resident

	All immigrants (1)	Hispanic immigrants (2)	Non-hispanic immigrants (3)
Panel A: Poor health			
Undocumented	-0.028***	-0.031***	-0.021***
	(0.007)	(0.008)	(0.006)
Observations	46,570	30,897	15,673
Panel B: Mental distress			
Undocumented	-0.032***	-0.038***	-0.027***
	(0.007)	(0.009)	(0.007)
Observations	46,570	30,897	15,673
Panel C: Has any activity limitation			
Undocumented	-0.033***	-0.032***	-0.034***
	(0.002)	(0.003)	(0.008)
Observations	46,502	30,834	15,668
Demographics	X	X	X
Year and region fixed effects	X	X	X

Notes: Each parameter is from a separate logit model regression of the outcome variable on a dummy variable equal to one if the person has the health condition listed in panels A - C. The reference category is naturalized citizen. Marginal effects are reported, with standard errors clustered at the region level in brackets. Data are from the 1998-2017 IPUMS NHIS. The sample includes legal and undocumented immigrants aged 18-64. All estimations include year fixed effects, region fixed effects, and demographic controls: age, age squared, sex, race, dummies for education, married dummy, dummy for health insurance coverage, number of persons in family, and number of years spent in the US. All results are estimated using sample weights. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.4: Logit Model, Reference Group Is Propensity Matched Legal Immigrants

	All immigrants (1)	Hispanic immigrants (2)	Non-hispanic immigrants (3)
Panel A: Poor health			
Undocumented	-0.023***	-0.023***	-0.023***
	(0.004)	(0.005)	(0.004)
Observations	79,750	44,927	34,823
Panel B: Mental distress			
Undocumented	-0.025***	-0.024***	-0.024***
	(0.006)	(0.007)	(0.005)
Observations	79,750	44,927	34,823
Panel C: Has any activity limitation			
Undocumented	-0.036***	-0.034***	-0.037***
	(0.003)	(0.006)	(0.009)
Observations	79,655	44,845	34,810
Demographics	X	X	X
Year and region fixed effects	X	X	X

Notes: Each parameter is from a separate logit model regression of the outcome variable on a dummy variable equal to one if the person has the health condition listed in panels A - C. The reference category is naturalized citizen. Marginal effects are reported, with standard errors clustered at the region level in brackets. Data are from the 1998-2017 IPUMS NHIS. The sample includes legal and undocumented immigrants aged 18-64. All estimations include year fixed effects, region fixed effects, and demographic controls: age, age squared, sex, race, dummies for education, married dummy, dummy for health insurance coverage, number of persons in family, and number of years spent in the US. All results are estimated using sample weights. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.5: Logit Model, Health Outcomes by Education Group

	HS dropout (1)	HS graduate (2)	Some college (3)	College graduate (4)
Panel A: Poor health				
Undocumented	-0.036***	-0.029***	-0.013	-0.010***
	(0.009)	(0.003)	(0.008)	(0.003)
Observations	24,095	18,794	15,949	20,912
Panel B: Mental distress				
Undocumented	-0.038***	-0.038***	-0.010	-0.009
	(0.013)	(0.001)	(0.008)	(0.010)
Observations	24,095	18,794	15,949	20,912
Panel C: Has any activity limitation				
Undocumented	-0.053***	-0.041***	-0.033***	-0.010*
	(0.005)	(0.006)	(0.010)	(0.005)
Observations	24,041	18,776	15,939	20,899
Demographics	X	X	X	X
Year and region fixed effects	X	X	X	X

Notes: Each parameter is from a separate logit model regression of the outcome variable on a dummy variable equal to one if the person has the health condition listed in panels A - C. The reference category is legal immigrants (including both naturalized citizens and legal residents). Marginal effects are reported, with standard errors clustered at the region level in brackets. Data are from the 1998-2017 IPUMS NHIS. The sample includes legal and undocumented immigrants aged 18-64. All estimations include year fixed effects, region fixed effects, and demographic controls: age, age squared, sex, race, dummies for education, married dummy, dummy for health insurance coverage, number of persons in family, and number of years spent in the US. All results are estimated using sample weights. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

2 The Spillover Effects of E-Verify on High-Skilled Citizen Women

2.1 Introduction

Verify Employment Eligibility (E-Verify) is a free federal identity and work authorization verification system that aims at reducing the hiring of undocumented immigrants. E-Verify is a voluntary program, however, employers may be required to check the employee's eligibility to work legally if their states have E-Verify laws that require employers to utilize E-Verify. E-Verify queries as a share of new hires has grown quickly in the past decade. For example, in 2007 E-Verify covered only three percent of new hires, whereas by 2019, E-Verify queries as a share of new hires are more than 35 percent (Figure 2.1).[1] An extensive literature has studied the impact of E-Verify on the migration flow, labor supply, and earning of undocumented immigrants (Amuedo-Dorantes and Bansak, 2012, 2014; Bohn and Lofstrom, 2012; Amuedo-Dorantes et al., 2015; Chassambouli and Peri, 2015; Orrenius and Zavodny, 2015, 2016; Orrenius et al., 2018).[2] However, previous findings on the E-Verify's effect on citizen workers labor outcomes have mostly focused on low-skilled citizen workers who are likely to compete with undocumented immigrants in the labor market (see Amuedo-Dorantes and Bansak, 2014; Bohn et al., 2015). These are substitutes at work (low-skilled for low-skilled) while no one has looked at substitutes at home (low-skilled for high-skilled). This paper attempts to fill this gap in the literature.

The goal of my paper is to study the spillover impact of E-Verify on the labor market outcomes of high-skilled citizen women (who have completed college or more). I focus on

[1] Figure 2.1 shows the information about E-Verify use rate for the period 2005-2018. Data are from U.S. Citizenship and Immigration Services (USCIS), Small Business Administration, Census Bureau, Westat, and Cato Institute.

[2] In general, previous studies found that E-Verify had a negative impact on the hourly wage and employment rates of likely undocumented immigrants.

high-skilled female workers since it is well-documented that high-skilled women's labor supply has a positive relationship with the number of undocumented immigrants (Furtado and Hock, 2010; Barone and Mocetti, 2011; Cortes and Tessada, 2011; Farre et al., 2011; Cortes and Pan, 2013; Amuedo-Dorantes and Sevilla, 2014; Peri et al., 2015; East and Velasquez, 2018). One way undocumented immigrants could affect the labor supply of high-skilled women citizens is through changes in the cost of household services since undocumented immigrants are over-represented in household services as nannies, maids, housekeeping cleaners, gardeners, and workers in dry cleaning and laundry services (Passel and Cohn, 2016).

My empirical strategy is to exploit the cross-state variation in the implementation of E-Verify. To illuminate the potential unintended consequences of E-Verify on citizen female labor supply, I collected data on the adoption of E-Verify and merged these data to information on labor supply, time use, and expenditure from the American Community Survey, American Time Use Survey, and Consumer Expenditure Survey data. This allows me to estimate a difference-in-differences model with state and year fixed effects. There are two main concerns with my identification strategy. First, the labor supply difference, between states that adopted E-Verify compared with those that did not, varies over time. Second, E-Verify implementations are endogenous.

To address the common trends concern, I conduct an event study to see if there is a systematic difference in high-skilled citizen women labor supply before the E-Verify implementation across states. The event study results show that labor supply of high-skilled citizen women is indistinguishable from zero in pre-trend period across states, but then demonstrate a level shift after E-Verify implementation, with high-skilled women in implemented states decreasing their labor supply over time.

As a test for endogenous policy implementation, I attempt to predict the implementation of E-Verify using low-skilled immigrant share of labor force/total population in year 2000. Significant coefficients on the low-skilled immigrant share of labor force/total population would indicate that the policies were implemented in response to pre-existing trends in the share of low-skilled immigrants, and this would limit the causal interpretation of my results. I find that the coefficients on the share of low-skilled immigrants are generally insignificant. This serves as strong evidence that E-Verifies were not endogenously implemented in response to particularly high low-skilled immigrant share, and this supports the causal interpretation of my labor supply results.

My main findings show that implementation of E-Verify decreases high-skilled citizen female workers' labor force participation by 0.3 percentage points and weekly hours worked by 10 minutes. The impact is stronger for mothers', the introduction of E-Verify is associated with a 1 percentage point decrease in labor force participation rate and a 23 minute decrease in hours worked per week by mothers with children under five.

I next study possible channels in which E-Verify affects high-skilled female workers. I present a number of findings suggesting that lower inflows of undocumented migrants is an important channel. Specifically, states that introduce E-Verify receive lower inflows of (male) undocumented migrants more generally. Since migration often occurs as a family unit (Ziegler, 1977; Chaloff and Poeschel, 2017), the supply of (tied) female movers who would work in household services decreases in E-Verify-adopted states, raising prices for household services and reducing native female labor supply.

My paper makes several contributions to the literature. First, to my knowledge, no previous literature has looked at the spillover effects of E-Verify on high-skilled women labor outcomes. Over the past two decades, one of the central components of comprehensive immigration reform proposals is how to regulate more than 11 million undocumented immigrants currently in the U.S..[3] Understanding the spillover effects of E-Verify is crucial to the evaluation of future regularization proposals. Second, I provide evidence on a novel underlying mechanism in which E-Verify affects high-skilled women's labor supply: E-Verify-adopted

[3]The estimation of the number of undocumented immigrants in the U.S. is from Krogstad et al. (2019).

states receive lower inflows of undocumented migrants more generally.

My work is closest to works of Cortes and Tessada (2011) and East and Velasquez (2018). While both use a sample similar to mine, they look at different research questions. Cortes and Tessada (2011) examines the impact of the number of immigrants on high-skilled women's labor supply, using immigrant enclaves as an instrument for migration. East and Velasquez (2018) examines the impact of Secure Communities (and not E-Verify) on the labor supply of high-skilled women using a difference-in-differences (DD) framework.[4]

The first main difference between this paper and East and Velasquez (2018) is the immigration policy studied. Specifically, East and Velasquez (2018) studies the impacts of Secure Communities (SC) and this paper studies the impacts of E-Verify. While both SC and E-Verify are designed to regulate undocumented immigrants, the main difference between these policies is that SC deports undocumented immigrants while E-Verify does not. In other words, SC is a much tougher policy than E-Verify. Thus, one would expect SC to have a negative impact on the labor supply of household services, but not necessarily E-Verify. In fact this paper finds that the less-extreme policy of checking on the immigration status prior to employment has a similar negative externality on citizen female labor supply.

The second main difference between this paper and East and Velasquez (2018) is the underlying mechanisms. While SC directly affected undocumented women working in household services, E-Verify affected them indirectly. Specifically, E-Verify is mandatory for firms and employers but not for households. The undocumented women working in household services do not work through firms. They contract directly with the households; therefore, their labor supply is not affected by E-Verify directly. However, the presence of E-Verify reduces the number of undocumented men, which leads to lower labor supply of undocumented women who would work in household services. This labor supply shock raises prices for household services and reduces native female labor supply.

[4] East and Velasquez (2018) relies on a continuous DD coefficient between 0 and 1 while mine is a binary variable.

This paper also relates to a large literature on understanding the impact of immigration on natives' outcomes. I focus in particular on the labor supply of high-skilled women. The large literature looks at other outcomes, including (among others) population growth, education, internal migration, wages, patents, crime, and innovation (Card, 2007; Kerr, 2007; Hunt and Gauthier-Loiselle, 2010; Ottaviano and Peri, 2012; Chassambouli and Peri, 2015; Freedman et al., 2018).

The rest of the paper proceeds as follows. Section 2.2 discusses the data, followed by the empirical framework in Section 2.3. The results are presented in Section 2.4. Section 2.5 concludes.

2.2 Data

In this section, I describe information about the implementation of E-Verify as well as the data I use to measure labor market outcomes, time use, and expenditures on household services.

E-Verify Data.— I gather information about enactment and implementation dates of E-Verify mandates at the state-level from the National Conference of State Legislatures[5], as well as Amuedo-Dorantes et al. (2015), and various news articles. Figure 2.2 shows the rollout of the E-Verify across states in the U.S.. As observed, there is crucial variation in the adoption of E-Verify, both across states and through time, which I will exploit in identifying the effect of E-Verify.

Labor Market Outcomes Data.— To measure the labor market effects of E-Verify, I use the individual-level data drawn from the 2005-2017 American Community Survey (ACS) Integrated Public Use Microdata Series data (Ruggles et al., 2019).[6] The data provide details on the labor force participation status and hours worked of individuals as well as information

[5]http://www.ncsl.org/research/immigration/everify-faq.aspx

[6]The ACS is the preferred dataset for this article because the ACS is mandatory, and therefore response at the unit and item level is higher in the ACS than the Current Population Survey.

on education, race, and citizenship status. Following East and Velasquez (2020), I restrict the sample to U.S. citizen women aged 20-64 who completed college or more.[7]

My main outcome variable is the labor supply of high-skilled American women. I start by describing the labor force participation and usual hours worked per week in the past year (Table A.6). As household production demands may differ between households with children versus households without children, following East and Velasquez (2018), I also explore the results on subsamples of women with children (ages 0-18) and women with young children (ages under 5) living at home.

Time Use Data.— I use the 2005-2017 American Time Use Survey (ATUS) Integrated Public Use Microdata Series data (Hofferth et al., 2019) to investigate the changes in women's time use. The ATUS sample is selected randomly from households that are completing their participation in the Current Population Survey (CPS). On average, individuals are sampled between two and five months after the last CPS interview for the ATUS household. The respondents' activities are over a 24-hour period. Since the time use data are daily while the CPS data are weekly, I convert daily time use data to weekly measures by multiplying by 7 to be compatible with ACS measures.

Consumption Data.— I use the Consumer Expenditure Survey (CEX), a nationwide household survey conducted by the U.S. Bureau of Labor Statistics, to measure expenditures on household services during the 2005-2017 period. In particular, I consider (i) expenditure on housekeeping services and (ii) a dummy variable for positive spending on housekeeping services.

Information on the state of residence and survey year are used to match an individual in the ASC, ATUS, and CEX data to the E-Verify activation dates. As shown in Table A.6, socioeconomic characteristics like age, race, marital status, number of children, and number of children younger than 5 are closely comparable across ACS, ATUS, and CEX.

[7]The results are robust to the use of the sample people aged 25-64.

Table A.6 also shows that, in comparison with men, women are less likely to participate in the labor force, and more likely to spend time on household activities. For example, 82% of high-skilled women work, while 91% of high-skilled men work. On average, high-skilled citizen women spend 15.5 hours on household activities, while high-skilled citizen men spend 10.2 hours weekly to maintain their household.

2.3 Empirical Framework

To examine the causal effect of E-Verify on labor market outcomes of high-skilled women, I exploit the staged rollout of E-Verify. In particular, I estimate a difference-in-differences model comparing areas that adopted E-Verify to areas that did not adopt E-Verify but were trending similarly in the pre-period. The primary model specification was as follows:

$$Y_{st} = \alpha + \sigma \textit{E-Verify}_{st} + state_s + year_t + \beta X_{st} + Z_{st} + Z_{s00} \times t + \epsilon_{st} \tag{1}$$

where s is state and t is year. Y_{st} is the outcome of interest. Y_{st} is the labor force participation rate or usual hours worked. In all specifications, I exclude early adoption states since E-Verify was implemented in those states early and selection could have played a role in activation (Miles and Cox, 2014; Alsan and Yang, 2018).[8]

In the specification above, $state_s$ and $year_t$ are state and year fixed effects to account for state-specific policies or economic shocks that might affect the labor supply. $\textit{E-Verify}_{st}$ is a binary variable indicating whether the state adopts E-Verify during that particular year. In particular, $\textit{E-Verify}_{st}$ equals to one if state s adopted E-Verify at year t and zero otherwise. Following Cortes and Tessada (2011) and East and Velasquez (2018), X_{st} is the average socioeconomic characteristics in each state-year cell of: age, age squared, number of children, number of children under age 5, indicators for educational attainment, black

[8]The excluding states are Arizona, Georgia, Mississippi, Missouri, Oklahoma, and South Carolina.

dummy, married dummy, and metro dummy.[9] The vector Z_{st} contains state-by-year-level controls: housing price and unemployment rate to adjust for the large effect of the 2007-09 U.S. recession on labor markets. The terms $Z_{s00} \times t$ are interactions of state characteristics in 2000 with linear time trends.[10] Z_{s00} includes state-by-year-level labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree. All specifications are estimated using the survey sampling weights[11], and standard errors are clustered at the state level (Bertrand et al., 2004).

2.3.1 Identifying Assumptions

As is standard in difference-in-differences models, my identification relies on two assumptions: (i) the common trend assumption that in the absence of the policy, the labor supply of women in treated and control states evolve in parallel; and (ii) the implementation of E-Verify is exogenous to immigrant share of the U.S. labor force.

To test the common trend assumption, I conduct an event study to see if there is a systematic difference in high-skilled citizen women labor supply before the E-Verify implementation across states. Specifically, the estimating event study model for outcome Y is:

$$Y_{st} = \alpha + E\text{-}Verify_{st} \left[\sum_{r=-4}^{-2} \pi_r 1_{rt} + \sum_{r=0}^{3} \pi_r 1_{rt} \right] + state_s + year_t + \beta X_{st} + Z_{st} + Z_{s00} \times t + \epsilon_{st} \quad (2)$$

The coefficients of interest, π_r, identify the effect of E-Verify on labor supply of high-skilled citizen women relative to the omitted group, r = -1. All control variables are the same as in Equation (1). The event study results in Figure 2.3 suggest that there were no differences

[9] Black et al. (2014) found that commuting time has a negative effect on labor supply of married women. The results are robust if I additionally control for average commute time.

[10] My results are robust to the exclusion of $Z_{s00} \times t$

[11] My results are robust to not using the sampling weights.

across states in the labor supply before E-Verify was adopted. Moreover, one can see a significant negative effects of E-Verify on labor supply after E-Verify implementation.

I verify the validity of the exogenous policy implementation assumption in two ways. First, I attempt to predict the implementation of E-Verify using low-skilled immigrant share of labor force in Table A.7. Significant coefficients on the low-skilled immigrant share of labor force/total population would indicate that the policies were implemented in response to pre-existing trends in the share of low-skilled immigrants, and this would limit the causal interpretation of my results. I find that the coefficients on the share of low-skilled immigrant are generally insignificant. This serves as strong evidence that E-Verify was not endogenously implemented in response to particularly high low-skilled immigrant share, which supports the causal interpretation of my labor supply results.

Second, I conduct a simulated placebo or permutation test as suggested by Dague and Lahey (2019). To implement this exercise, I estimate Equation (1) 1,000 times by randomly assigning a placebo E-verify implementation year for each state. Figure 2.4 shows the histogram of placebo estimates along with vertical solid lines representing my actual estimates. The dashed lines are the 5th and 95th percentile of the placebo estimates. The simulated placebo test shows that my main estimate is an outlier comparing with the placebo estimates. This suggests E-Verify had a large effect on high-skilled women's labor supply and I did not find the result by chance.

2.4 Results

2.4.1 Effects on Labor supply of High-Skilled Women

In light of my proposed economic channels, I would expect σ to have a negative sign and be increasing in magnitude as the domestic burdens of the household increase, in particular with the introduction of a dependent. I estimate model (1) separately for all women, mothers, and mothers of young children. Following East and Velasquez (2018), column 1 of Table 2.1 shows the results for all citizen women, column 2 shows the impact on mothers, column 3 shows the impact on mothers of young children.

Full sample of highly skilled women.— The results in column 1 of Panel A show that the adoption of E-Verify decreases the labor force participation of high-skilled women by 0.3 percentage points. The adoption of E-Verify also reduces the time of this group's work by 0.165 hours or 10 minutes per week as showed in column 1 of Panel B. This finding is consistent with the results from Cortes and Tessada (2011) and East and Velasquez (2018) that the presence of low-skilled migrants increased the labor force participation of high-skilled citizen women.

Mothers.— The results for mothers in columns 2 and 3 are larger than the full sample. For mothers, their labor force participation reduces by 0.6 percentage points, and for mothers of children under five, the reduction is 1 percentage point. The results in columns 2 and 3 of Panel B indicate that E-Verify has a significant negative effect on the hours worked. Specifically, the implementation of E-Verify decreases usual hours worked of mothers by 0.27 hours or 16 minutes per week, while the reduction for mothers of young children is 0.39 hours or 23 minutes per week.

2.4.2 Mechanisms

How does E-Verify affect labor supply of high-skilled women? In this section, I present several results to test the hypothesis: states that introduce E-Verify receive lower inflows of (male) undocumented migrants more generally, the supply of (tied) female movers who would work in household services decreases in those states, raising prices for household services and reducing native female labor supply.

First, using data from the American Community Survey, I examine the effects of E-Verify on the number of in-migrants who migrated to a state from other states and from abroad in

Table 2.2. Looking at columns 1 through 3 of Table 2.2, the presence of an E-Verify mandate last year substantially decreases the number of in-migrants from other states. Looking at columns 4 through 6 of Table 2.2, the presence of an E-Verify mandate last year also reduces the number of in-migrants from abroad. This result is driven by male immigrants which supports the above hypothesis.

Second, I replicate the findings of Orrenius and Zavodny (2016) and Bohn et al. (2014), extending their analysis by including data from 2015 to 2017. I confirm that the E-Verify mandate decreases the population of likely undocumented immigrants living in a state (see Table 2.3). This result is driven by recent immigrants, consistent with Borjas (2001) who showed that recent immigrants have lower mobility cost and more sensitivity to interstate wage differences.

Third, I test whether E-Verify adoption has any impacts on the labor market outcomes of likely undocumented immigrants. I find that E-Verify decreases the labor supply of likely undocumented immigrants (see Table 2.4). This result is consistent with Amuedo-Dorantes and Bansak (2014) who found that E-Verify is very effective in reducing the labor supply of unauthorized immigrants. [12]

Fourth, using the CEX data, I study the effects on households' spending on housekeeping services. Results are presented in Table 2.5. Households' spending could increase or decrease as prices of household services rises. Results in Panel A indicate that households, on average, spend less on housekeeping services. However, the effects on whether households spending any money on housekeeping service in Panel B are not statistically significant.

Finally, I find that E-Verify increases time spent on housework of high-skilled citizen

[12] Orrenius and Zavodny (2015) found that E-Verify *increases* employment likelihood among *likely undocumented Mexican* while both Amuedo-Dorantes and Bansak (2014)'s results and my results showed that E-Verify *reduces* the employment of *likely undocumented immigrants*. The reason Amuedo-Dorantes and Bansak (2014) and my results are not consistent with Orrenius and Zavodny (2015) might come from the difference in study samples: Orrenius and Zavodny (2015) used low-skilled Mexican migrants as a proxy for likely undocumented migrants while Amuedo-Dorantes and Bansak (2014) and my paper used low-skilled Hispanic migrants.

mothers. Specifically, I estimate a difference-in-differences model with the ATUS data with outcome variables are: time (hours per week) allocated to housework and childcare. The estimate results are presented in Table 2.6. In column 2 for mothers, I find that college-educated mothers spend one hour more per week on housework when E-Verify is implemented. When I restrict the sample to women with young children in column 3, the impact of E-Verify is larger. This is similar to the findings in Table 2.1. On average, mothers of young children spent an extra two hours per week on housework. Results in Panel B show that E-Verify does not affect time allocated to childcare. It is not surprising that E-Verify increases time spent on housework but does not have any effects time allocated to childcare since it shows the difference in which household tasks are more likely to be outsourced.[13]

2.4.3 Sensitivity Checks

Appendix A.1 contains several robustness checks, some of which have been referenced above.[14]

First, my main identification strategy is to leverage the staggered roll out of E-Verify. A potential concern is that the staggered difference-in-differences (DD) strategy might bias the DD estimate away from the true effect. To alleviate this concern, I implements the decomposition proposed in Goodman-Bacon (2019). In particular, Goodman-Bacon (2019) shows that the DD regression coefficient from a specification as the one in equation (1) is simply a weighted average of the three two-group/two-period (2x2) DD estimators: treated/untreated, early treated/late treated, and late treated/early treated.In other words, Goodman-Bacon (2019) shows that in DD models with timing variation, already-treated groups sometimes act as controls and this could lead to bias in the DD coefficient. The author proposes using the DD decomposition theorem to illustrate the sources of variation and to eliminate

[13] *Housework* measures time spent on housework, cleaning, home maintenance, and travel related to those activities. *Childcare* measures all time spent by the individual caring for, organizing and planning for children, and looking after children.

[14] The results of robustness checks are summarized in Table 2.9.

comparisons between the early treated and the late treated.[15]

Figure A.1 plots the DD estimates for labor supply of high-skilled citizen women in the present setting. The vertical axis plots the 2x2 estimate for each pair and the horizontal axis plots the weight each of these pairs receive. The horizontal line shows the weighted average of all DD estimates. Summing the weights on timing terms (the x's on Figure A.1) shows how much of the DD estimate comes from timing variation (7 percent[16]). The 2x2 terms that compare treated/untreated states (the closed triangles on Figure A.1) account for 93 percent of the estimate. The DD estimates using the DD decomposition theorem match closely to the baseline results, which suggests that bias resulting from time-varying treatment effects is not a big concern in my setting.[17]

Second, I reproduce the whole analysis, but instead of focusing on high-skilled women, I focus on high-skilled men as a placebo group as high-skilled men are less likely to be affected by the change in prices of household services. The effects on labor supply and time use of high-skilled men are presented in Table 2.7. As expected, all effects are small in magnitude and statistically insignificant.

Third, my results are robust to limiting the treatment groups to states that require E-Verify for all employers only (Table A.16), and to states that require E-Verify for state agencies, contractors, and subcontractors only (Table A.17). They are also robust to dropping California (Table A.14), dropping Colorado (Table A.15).[18]

Fourth, my results are robust to functional form choices. There were two intensity-levels E-Verify in the study period: (i) E-Verify is required for all employers and (ii) E-Verify is

[15]See Goodman-Bacon (2018) for a detailed description about the DD decomposition theorem.

[16]Table A.18, column 1 weight: 3.5 + 3.5 = 7 percent.

[17]The reason for this is that the relative size of the never treated states is large. Cases in which the never treated group's relative size is smaller, I would probably find significant changes in DD estimates using the DD decomposition theorem compared to the baseline model.

[18]California has the largest unauthorized immigrant population in 2016 according to an estimation by Pew Research Center. Colorado's E-Verify law became effective on 7 August 2006, and was amended on 13 May 2008 (created an alternative program for E-Verify so E-Verify is not mandated in Colorado). Source: https://en.wikipedia.org/wiki/E-Verify#Colorado

required for state agencies and public contractors only.[19] In my empirical specification, I do not consider a "hybrid" model that separates the types of E-Verify, I simply model the presence of E-Verify. I now present the results for a "hybrid" model (Amuedo-Dorantes and Bansak, 2014). Specifically, I estimate the following linear probability model:

$$Y_{st} = \alpha + \sigma E\text{-}Verify\_all_{st} + \gamma E\text{-}Verify\_partial_{st} + \delta_s + \delta_t + \beta X_{st} + Z_{st} + \epsilon_{ist} \quad (3)$$

where all specifications are the same as in equation (1) except $E\text{-}Verify\_all_{st}$ is a binary variable equal to one if state s required E-Verify for all employers at time t, and $E\text{-}Verify\_partial_{st}$ is an indicator variable describing whether state s only required E-Verify for state agencies and public contractors at time t. As shown in Table A.9, the results are very similar to the baseline model.

Fifth, my results are robust to including state-specific linear time trends to additionally control for state-specific unobservables that vary over time (Table A.11) (Wolfers, 2006; Lee and Solon, 2011).[20] The results are also robust to including interactions of state pre-treatment characteristics with time fixed effects (Table A.10).

Sixth, as states that adopted E-Verify during 2006-2008 may be highly selected, I test the sensitivity of the results to including these states. Table A.13 provides these estimates and I find the results are very similar to the baseline model.

Lastly, there may be other immigration policy changes like the Secure Communities programs and 287(g) agreements that could affect the population of undocumented immigrants. I collect the information about the implementation of Secure Communities and 287(g) and create fixed effects for those policy implementations. Including these controls does not change

[19] There are six states that require E-Verify for all employers: AL, AZ, MS, NC, SC, TN. States that require E-Verify for state agencies and public contractors are: CO, FL, GA, ID, IN, LA, MI, MO, NE, OK, PA, TX, UT, VA, WV

[20] Except that these time trends might be "over controlling" and in addition to absorbing pre-existing trends, they may also absorb part of the treatment effect so that some of the estimates are no longer significant.

my main effect (Table A.12).

2.5 Conclusion

In this paper, I find that the adoption of E-Verify mandates reduces the labor supply of high-skilled citizen women: lower hours worked and labor force participation rate. The effects are stronger for mothers and mothers of young children. Placebo test using high-skilled men as a placebo group suggests that the negative effect is not driven by complementarity between high-skilled natives and the unskilled undocumented immigrants.

It is informative to compare the magnitude of my estimates to those reported in the existing literature. First, my 0.3 percentage points decrease in the labor force participation of high-skilled female workers is very similar to the 0.26 percentage points decrease in East and Velasquez (2020) resulting from exposure to SC, a tougher policy than E-Verify. The similarity between my estimate and that in East and Velasquez (2020) has an important policy implication: less-extreme policies could attain similar results and thus possibly reducing enforcement costs compared to tougher policies. Second, I show that E-Verify reduces the hours work for college-educated women by 10 minutes per week, while Cortes and Tessada (2011) reports that a 10 percent increase in number of low-skilled migrants increases the hours worked by 6 minutes per week for women at the top of the wage distribution. The E-Verify effect is larger in magnitude to the effect of 10 percent decrease in number of low-skilled migrants.

To explore the potential mechanisms driving my main finding, I first show that E-Verify-adopted-states receive lower inflows of (male) undocumented migrants more generally. In addition, the population of unskilled female immigrants who would work in household services decreases in the E-Verify-adopted-states. This leads to raising in household services prices and reducing native mothers' labor supply. These findings suggest that my results may be driven by lower inflows of undocumented immigrants in E-Verify states.

Overall, my findings provide evidence of a negative spillover effect of the Employment verification system, which is designed to affect only undocumented immigrants, on native mothers' labor outcomes. A back-of-the-envelope calculation suggests that E-Verify generates \$6.09 billion (2018 dollars) in annual social cost from the reduction of high-skilled citizen women labor supply (see Table 2.8). This social cost may be worth considering by states and by the federal government in debating and reforming immigration policy.

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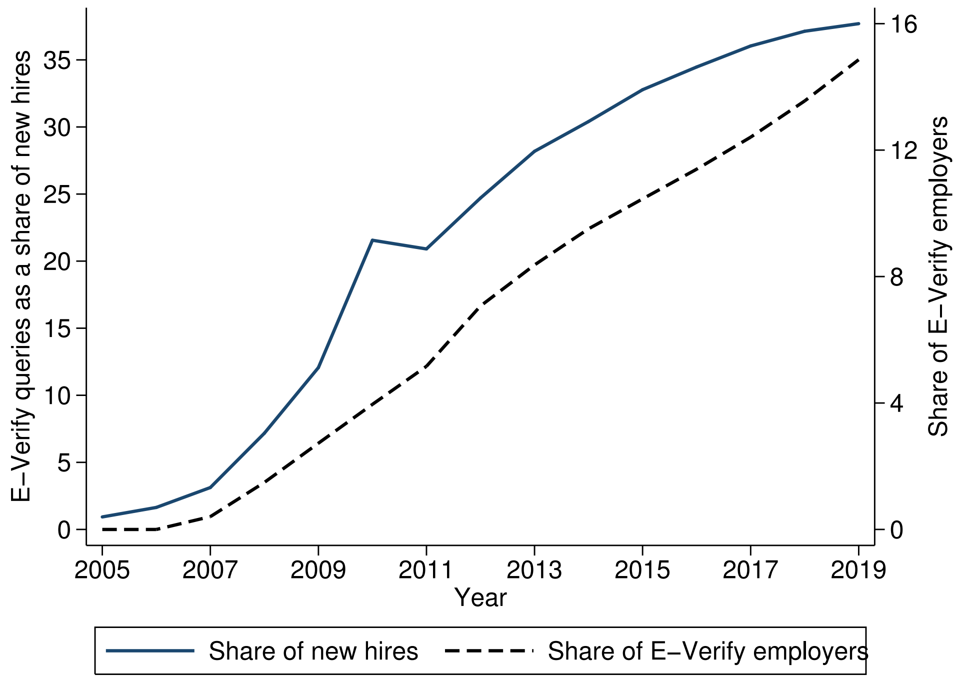
2.6 Figures

Figure 2.1: E-Verify Use Rate



Notes: Data are from U.S. Citizenship and Immigration Services (USCIS), Small Business Administration, Census Bureau, Westat, and Cato Institute. The solid line denotes E-Verify queries as a share of new hires. The dashed line denotes number of employers using E-Verify as a share of total employers. 2019 Hires based on 2017-2018 growth.

Figure 2.2: E-Verify Implementation

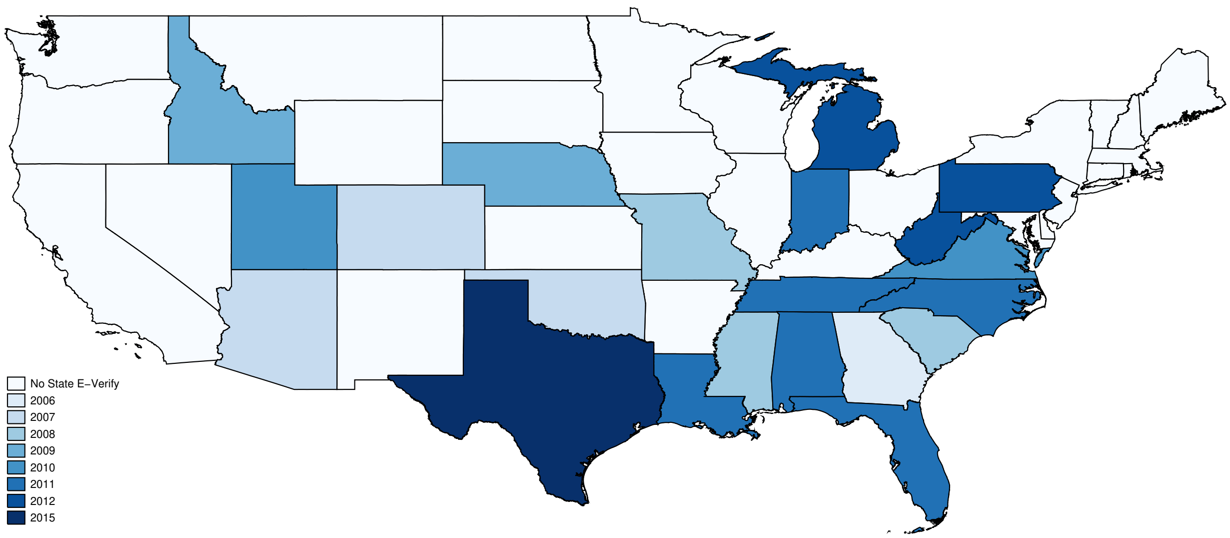
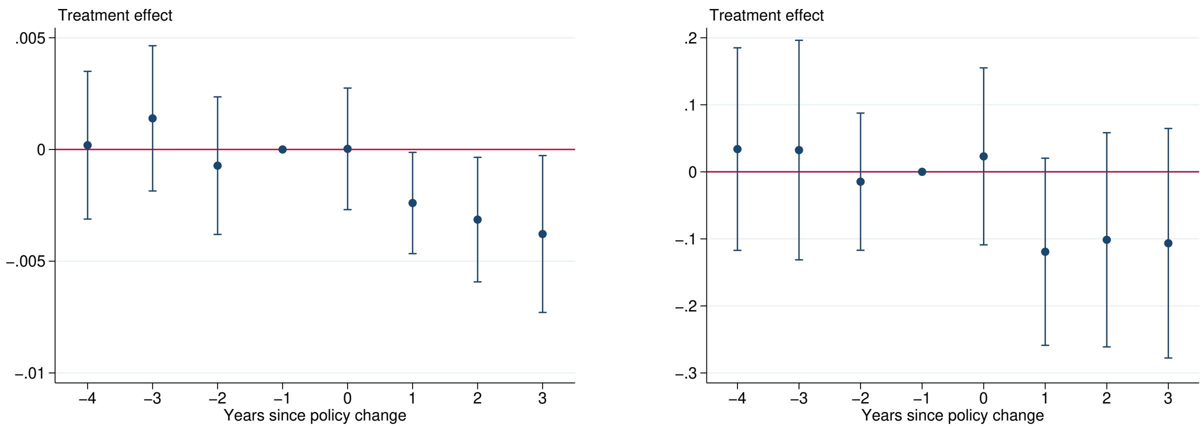


Figure 2.3: Event Study for High-Skilled Women's Labor Supply

(a) Labor force participation

(b) Hours worked

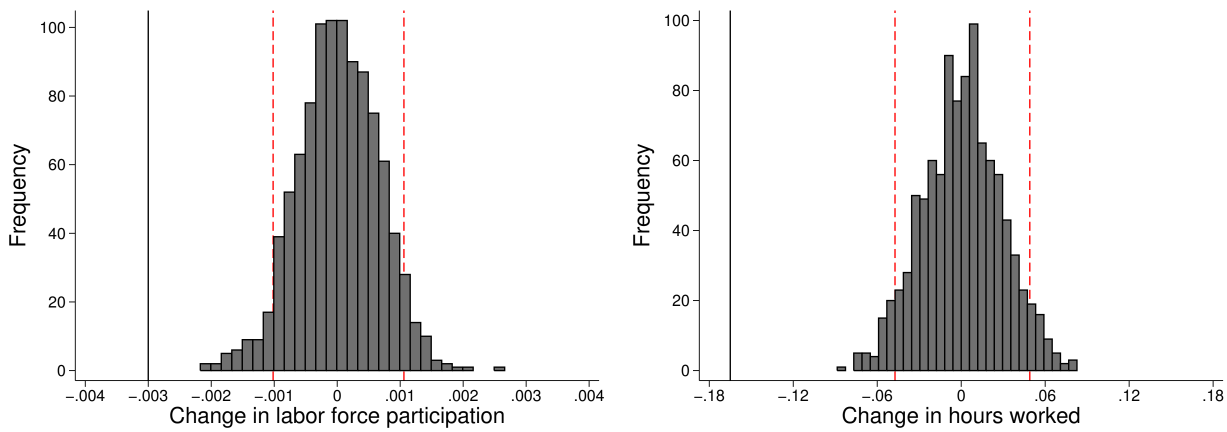


Notes: Data are from the 2005-2017 IPUMS ACS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All statistics are calculated using the survey sampling weights. Standard errors are clustered at the state level. Whiskers show 95% confidence interval.

Figure 2.4: Permutation Tests on Effects of E-Verify on High-Skilled Women's Labor Supply

(a) Labor force participation

(b) Hours worked



Notes: These figures shows the histogram of placebo estimates of Equation (1) 1,000 times by randomly assigning a placebo E-verify implementation year for each state. The vertical solid lines represent my actual difference-in-differences estimates. The dashed lines are 5th and 95th percentile of the placebo estimates. Data are from the 2005-2017 IPUMS ACS. The sample is limited to U.S. citizen women with a college degree or more aged 20-64.

2.7 Tables

Table 2.1: Effect of E-Verify on Labor Supply of High-Skilled Women

	All women	Women with children	Women with children under 5
Panel A: Labor force participation			
E-Verify (σ)	-0.003***	-0.006***	-0.010***
	(0.001)	(0.002)	(0.003)
% Impact (coef/mean)	-0.37%	-0.69%	-1.27%
Mean of dep. var.	0.825	0.804	0.757
Observations	3,168,036	1,479,384	468,338
Panel B: Usual hours worked			
E-Verify (σ)	-0.165***	-0.267**	-0.389***
	(0.054)	(0.100)	(0.143)
% Impact (coef/mean)	-0.50%	-0.85%	-1.33%
Mean of dep. var.	33.01	31.27	29.17
Observations	3,166,052	1,478,628	468,152
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars × linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: labor force participation and hours worked. Data are from the 2005-2017 IPUMS ACS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 2.2: Effect of E-Verify on Migration Rate of Likely Undocumented Immigrants

	In-migration from other states			In-migration from abroad		
	(1) All	(2) Female	(3) Male	(4) All	(5) Female	(6) Male
E-Verify activated last year	-0.007**	-0.007***	-0.006*	-0.004	-0.001	-0.006**
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Mean of dep. var.	0.01	0.01	0.01	0.02	0.02	0.03
Observations	507,614	237,280	270,334	507,614	237,280	270,334
State-level controls	X	X	X	X	X	X
Year and state fixed effects	X	X	X	X	X	X

Notes: Each parameter is from a separate regression of outcome variables: (i) indicators whether movers migrated from other states or (ii) migrated from abroad. I restrict the sample to likely undocumented immigrants who are defined as high school dropout Hispanic non-citizens. Data are from the 2005-2017 IPUMS ACS. All estimations include year fixed effects, state fixed effects, state-specific linear time trends, and state-by-year-level controls: the log of real GDP per capita, log of expenditure per capita, log of housing starts, log of average housing price, log of unemployment rate (all lagged one year). All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 2.3: Effect of E-Verify on Population Size of Likely Undocumented Immigrants

	Recent immigrants			Not recent immigrants		
	(1) All	(2) Female	(3) Male	(4) All	(5) Female	(6) Male
E-Verify activated last year	-0.332***	-0.354***	-0.319**	0.024	0.042	0.009
	(0.123)	(0.109)	(0.141)	(0.053)	(0.070)	(0.050)
Observations	540	540	540	540	540	540
State-level controls	X	X	X	X	X	X
Year and state fixed effects	X	X	X	X	X	X

Notes: Recent immigrants are defined as who have been living in the US one to five years, not recent immigrants are who have been living in the US more than five years. Each parameter is from a separate regression of the outcome variable: log of population. I restrict the sample to likely undocumented immigrants who are defined as high school dropout Hispanic non-citizens. Data are from the 2005-2017 IPUMS ACS. All estimations include year fixed effects, state fixed effects, state-specific linear time trends, and state-by-year-level controls: the log of real GDP per capita, log of expenditure per capita, log of housing starts, log of average housing price, log of unemployment rate (all lagged one year). All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 2.4: Effect of E-Verify on Labor Supply of Low-Skilled Likely Undocumented Immigrants

	Low-skilled Hispanic immigrants	Undocumented status imputed using Borjas's method
Panel A: Labor force participation		
E-Verify (σ)	-0.009*	-0.016***
	(0.005)	(0.006)
Mean of dep. var.	0.739	0.757
Observations	548,326	80,486
Panel B: Usual hours worked		
E-Verify (σ)	-0.506**	-0.029
	(0.202)	(0.191)
Mean of dep. var.	29.34	39.19
Observations	548,051	61,190
Demographics	X	X
Year and state fixed effects	X	X
2000 state vars × linear time	X	X

Notes: Each parameter is from a separate regression of the outcome variables: labor force participation and hours worked. Data are from the 2005-2017 IPUMS ACS. I restrict the sample to people aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 2.5: Effect of E-Verify on Consumption of Housekeeping Services of High-Skilled Women

	All women	Women with children	Women with children under 5
Panel A: Expenditure on housekeeping services conditional on spending > 0			
E-Verify (σ)	-41.175	-104.567**	-52.388
	(24.547)	(43.233)	(43.980)
Mean of dep. var.	530.35	597.40	462.57
Observations	10,320	4,854	1,072
Panel B: Dummy for spending on household services > 0			
E-Verify (σ)	0.008	0.013	0.006
	(0.009)	(0.018)	(0.021)
Mean of dep. var.	0.10	0.13	0.11
Observations	96,197	36,616	9,512
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars $\times$ linear time	X	X	X

Notes: Each parameter is from a separate regression. Data are from the 2005-2017 Consumer Expenditure Survey. The sample includes all households with a college-educated woman. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using sample weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 2.6: Effect of E-Verify on Time Use of High-Skilled Women

	All women	Women with children	Women with children under 5
Panel A. Housework time			
E-Verify (σ)	0.853*	1.011**	2.109**
	(0.441)	(0.431)	(0.891)
Mean of dep. var.	8.77	9.62	8.64
Observations	460,050	297,948	118,609
Panel B. Childcare time			
E-Verify (σ)	0.432	0.446	-0.416
	(0.553)	(1.020)	(1.481)
Mean of dep. var.	8.98	15.47	25.18
Observations	460,050	297,948	118,609
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars $\times$ linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: weekly time spent on housework and weekly time spent on childcare. Data are from the 2005-2017 IPUMS ATUS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 2.7: Effect of E-Verify on Placebo Group: High-Skilled Citizen Men

Dep. var.	Labor force participation	Usual hours worked	Housework time	Childcare time
E-Verify (σ)	-0.003	-0.186	0.058	0.114
	(0.002)	(0.168)	(0.833)	(0.228)
Mean of dep. var.	0.878	38.33	6.24	0.39
Observations	1,508,861	1,506,590	127,864	127,864
Demographics	X	X	X	X
Year and state fixed effects	X	X	X	X
2000 state vars $\times$ linear time	X	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: labor force participation, usual hours worked, time spent on housework last week, time spend on childcare last week. Data are from the 2005-2017 IPUMS ACS, IPUMS ATUS. I restrict the sample to U.S. citizen men with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 2.8: Social cost calculation to the U.S. from E-Verify's High-skilled Women Labor Supply Reduction

Average hours worked decreased per week	0.16 hour (Column 1, Table 2)
Average hours worked decreased per year	**8.32 hours** (= 0.16h×52weeks)
Average annual wage of a college-educated woman	\$51,600 (Census Bureau report 2018)
Average hourly wage of college-educated women	**\$24.8** (= \$51,600 / (40h×52w))
Number of women with college degree	**29.5 million** (Pew research data collected from BLS)
Annual social cost (2018 dollars)	**\$6.09 billion** (= **29.5 million** × **\$24.8** × **8.32h**)

Notes: Data from Pew Research Center, U.S. Census Bureau, and U.S. Bureau of Labor Statistics

Table 2.9: Robustness Checks

Specification	All women (1) LFP	All women (2) Hours worked	Women with children (3) LFP	Women with children (4) Hours worked	Women with children under 5 (5) LFP	Women with children under 5 (6) Hours worked
(1) Baseline	-0.003	-0.165	-0.006	-0.267	-0.010	-0.389
	(0.001)	(0.054)	(0.002)	(0.100)	(0.003)	(0.143)
(2) Goodman-Bacon (2019) DD with staggered rollout	-0.002	-0.191	-0.004	-0.332	-0.007	-0.505
Additionally control for:						
(3) Pre-treatment state characteristics × time FE	-0.003	-0.166	-0.005	-0.265	-0.010	-0.407
	(0.001)	(0.055)	(0.002)	(0.100)	(0.003)	(0.145)
(4) State-specific linear time trends	-0.005	-0.165	-0.008	-0.243	-0.013	-0.266
	(0.001)	(0.064)	(0.002)	(0.159)	(0.004)	(0.193)
(5) Secure Communities & 287(g)	-0.003	-0.165	-0.005	-0.248	-0.009	-0.375
	(0.001)	(0.053)	(0.002)	(0.098)	(0.003)	(0.146)
(6) Including early adoption states	-0.003	-0.163	-0.005	-0.250	-0.009	-0.361
	(0.001)	(0.045)	(0.001)	(0.085)	(0.003)	(0.123)
Excluding:						
(7) California	-0.003	-0.170	-0.006	-0.287	-0.011	-0.421
	(0.001)	(0.054)	(0.002)	(0.097)	(0.003)	(0.142)
(8) Colorado	-0.006	-0.380	-0.009	-0.545	-0.015	-0.814
	(0.001)	(0.126)	(0.002)	(0.170)	(0.003)	(0.214)
(9) States that require E-Verify for all employers	-0.002	-0.197	-0.005	-0.284	-0.009	-0.403
	(0.001)	(0.047)	(0.002)	(0.104)	(0.003)	(0.150)

Note: See Appendix A.1 for details.

A Appendix

A.1 Additional Tables

Table A.6: Summary Statistics

	All women	Women with children	Women with children under 5	All men
Panel A: American community survey				
Labor force participation	0.82	0.80	0.76	0.91
Usual hours worked	33.00	31.35	29.21	40.81
Usual hours worked $\mid H > 0$	38.73	37.65	36.67	44.13
Work at least 50 hrs.	0.15	0.13	0.10	0.30
Work at least 60 hrs.	0.044	0.036	0.026	0.11
Age	42.19	42.33	34.31	43.56
Black	0.09	0.09	0.08	0.07
Married	0.61	0.81	0.89	0.65
Number of children	0.85	1.83	1.95	0.83
Number of children Under 5	0.19	0.42	1.30	0.19
Sample size	3,512,953	1,641,885	520,197	3,021,392
Panel B: American time use survey				
Time spent on housework (hrs per week)	15.51	17.26	15.93	10.24
Time spent on childcare (hrs per week)	8.90	15.40	25.09	4.35
Age	42.08	41.67	34.40	43.03
Black	0.08	0.08	0.07	0.07
Married	0.68	0.86	0.92	0.68
Number of children	1.06	1.94	2.01	0.97
Number of children under 5	0.27	0.48	1.32	0.23
Sample size	510,810	329,032	130,565	341,001
Panel C. Consumer expenditure survey				
Expenditure on housekeeping services	524.65	602.70	451.60	530.17
Age	46.75	42.02	35.25	47.45
Black	0.05	0.05	0.02	0.03
Married	0.67	0.85	0.93	0.78
Percent having children in household	0.46	1	1	0.46
Percent having children under 5 in HH	0.11	0.23	1	0.11
Sample Size	5,205	2,479	553	5,405

Notes: The sample includes U.S. citizen with a college degree or more aged 20-64. Data from the 2005-2017 IPUMS ACS, IPUMS ATUS, and CEX. All statistics are calculated using sample weights.

Table A.7: Attempting to Predict E-Verify Implementation

	Ever adopted E-Verify				Year adopted E-Verify			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low-skilled immigrant share of labor force	-3.07				-6177.57			
	(3.73)				(7496.65)			
Low-skilled immigrant share of total population		-1.65				-3309.97		
		(2.30)				(4620.29)		
Immigrant share of labor force			-3.06				-6139.24	
			(2.52)				(5066.14)	
Immigrant share of total population				-1.75				-3516.21
				(1.65)				(3312.09)
Observations	51	51	51	51	51	51	51	51

Notes: Each parameter is from a separate regression of outcome variables: (i) whether the state ever adopted E-Verify and (ii) what year the state adopted E-Verify. Data are from the 2000 IPUMS ACS. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.8: Effect of E-Verify on Labor Supply of High-Skilled Women, Additional Outcomes

	All women	Women with children	Women with children under 5
Panel A: Work at least 50 hours			
E-Verify (σ)	-0.003	-0.004**	-0.006**
	(0.002)	(0.002)	(0.002)
Mean of dep. var.	0.146	0.124	0.101
Observations	3,168,036	1,479,384	468,338
Panel B: Work full time (>35 hours)			
E-Verify (σ)	-0.013***	-0.017***	-0.021***
	(0.004)	(0.005)	(0.007)
Mean of dep. var.	0.639	0.595	0.558
Observations	3,168,036	1,479,384	468,338
Panel C: Usual Hours Worked $\vert H > 0$			
E-Verify (σ)	-0.172*	-0.263**	-0.400***
	(0.086)	(0.113)	(0.146)
Mean of dep. var.	38.688	37.564	36.628
Observations	2,684,961	1,229,298	372,189
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars $\times$ linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: work at least 50 hours, work full time, and hours worked given working positive hours. See Table 2.1 for full table notes.

Table A.9: Effect of E-Verify on Labor Supply of High-Skilled Women, Robustness to "Hybrid" Model

	All women	Women with children	Women with children under 5
Panel A: Labor force participation			
Required E-Verify for all employers (σ)	-0.009***	-0.012***	-0.021***
	(0.003)	(0.004)	(0.006)
Required E-Verify for state contractors only (γ)	-0.004***	-0.006***	-0.012***
	(0.001)	(0.002)	(0.004)
Mean of dep. var.	0.825	0.804	0.757
Observations	3,168,036	1,479,384	468,338
Panel B: Usual hours worked			
Required E-Verify for all employers (σ)	-0.246	-0.374*	-0.824**
	(0.150)	(0.202)	(0.314)
Required E-Verify for state contractors only (γ)	-0.244***	-0.345***	-0.521**
	(0.067)	(0.105)	(0.209)
Mean of dep. var.	33.01	31.27	29.17
Observations	3,166,052	1,478,628	468,152
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars × linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: labor force participation and hours worked. Data are from the 2005-2017 IPUMS ACS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.10: Effect of E-Verify on Labor Supply of High-Skilled Women, Robustness to Adjust for Interactions of Pre-Treatment State Characteristics with Time FE

	All women	Women with children	Women with children under 5
Panel A: Labor force participation			
E-Verify (σ)	-0.003***	-0.005***	-0.010***
	(0.001)	(0.002)	(0.003)
Mean of dep. var.	0.825	0.804	0.757
Observations	3,168,036	1,479,384	468,338
Panel B: Usual hours worked			
E-Verify (σ)	-0.166***	-0.265**	-0.407***
	(0.055)	(0.100)	(0.145)
Mean of dep. var.	33.01	31.27	29.17
Observations	3,166,052	1,478,628	468,152
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars $\times$ linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: labor force participation and hours worked. Data are from the 2005-2017 IPUMS ACS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.11: Effect of E-Verify on Labor Supply of High-Skilled Women, Robustness to Adjust for State-Specific Linear Time Trends

	All women	Women with children	Women with children under 5
Panel A: Labor force participation			
E-Verify (σ)	-0.005***	-0.008***	-0.013***
	(0.001)	(0.002)	(0.004)
Mean of dep. var.	0.825	0.804	0.757
Observations	3,168,036	1,479,384	468,338
Panel B: Usual hours worked			
E-Verify (σ)	-0.165**	-0.243	-0.266
	(0.064)	(0.159)	(0.193)
Mean of dep. var.	33.01	31.27	29.17
Observations	3,166,052	1,478,628	468,152
Demographics	X	X	X
Year and state fixed effects	X	X	X
State $\times$ linear time	X	X	X
2000 state vars $\times$ linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: labor force participation and hours worked. Data are from the 2005-2017 IPUMS ACS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.12: Robustness to Controlling for Secure Communities and 287(g) Agreements

	All women	Women with children	Women with children under 5
Panel A: Labor force participation			
E-Verify (σ)	-0.003***	-0.005***	-0.009***
	(0.001)	(0.002)	(0.003)
Mean of dep. var.	0.825	0.804	0.757
Observations	3,168,036	1,479,384	468,338
Panel B: Usual hours worked			
E-Verify (σ)	-0.165***	-0.248**	-0.375**
	(0.053)	(0.098)	(0.146)
Mean of dep. var.	33.01	31.27	29.17
Observations	3,166,052	1,478,628	468,152
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars $\times$ linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: labor force participation and hours worked. Data are from the 2005-2017 IPUMS ACS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.13: Robustness to Including Early Adoption States (Full Sample)

	All women	Women with children	Women with children under 5
Panel A: Labor force participation			
E-Verify (σ)	-0.003***	-0.005***	-0.009***
	(0.001)	(0.001)	(0.003)
Mean of dep. var.	0.824	0.805	0.757
Observations	3,512,953	1,641,885	520,197
Panel B: Usual hours worked			
E-Verify (σ)	-0.163***	-0.250***	-0.361***
	(0.045)	(0.085)	(0.123)
Mean of dep. var.	33.00	31.35	29.21
Observations	3,510,766	1,641,044	519,980
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars $\times$ linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: labor force participation and hours worked. Data are from the 2005-2017 IPUMS ACS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.14: Robustness to Dropping California

	All women	Women with children	Women with children under 5
Panel A: Labor force participation			
E-Verify (σ)	-0.003***	-0.006***	-0.011***
	(0.001)	(0.002)	(0.003)
Mean of dep. var.	0.825	0.804	0.757
Observations	3,144,919	1,468,124	464,811
Panel B: Usual hours worked			
E-Verify (σ)	-0.170***	-0.287***	-0.421***
	(0.054)	(0.097)	(0.142)
Mean of dep. var.	33.01	31.25	29.16
Observations	3,142,952	1,467,374	464,627
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars $\times$ linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: labor force participation and hours worked. Data are from the 2005-2017 IPUMS ACS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.15: Robustness to Dropping Colorado

	All women	Women with children	Women with children under 5
Panel A: Labor force participation			
E-Verify (σ)	-0.006***	-0.009***	-0.015***
	(0.001)	(0.002)	(0.003)
Mean of dep. var.	0.825	0.805	0.758
Observations	3,090,772	1,445,347	456,932
Panel B: Usual hours worked			
E-Verify (σ)	-0.380***	-0.545***	-0.814***
	(0.126)	(0.170)	(0.214)
Mean of dep. var.	33.01	31.30	29.22
Observations	3,088,832	1,444,611	456,753
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars $\times$ linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: labor force participation and hours worked. Data are from the 2005-2017 IPUMS ACS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.16: Robustness to *Dropping* States that Require E-Verify for All Employers

	All women	Women with children	Women with children under 5
Panel A: Labor force participation			
E-Verify (σ)	-0.002***	-0.005**	-0.009***
	(0.001)	(0.002)	(0.003)
Mean of dep. var.	0.826	0.805	0.758
Observations	2,945,791	1,375,450	435,304
Panel B: Usual hours worked			
E-Verify (σ)	-0.197***	-0.284***	-0.403**
	(0.047)	(0.104)	(0.150)
Mean of dep. var.	33.03	31.25	29.16
Observations	2,943,925	1,374,741	435,129
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars $\times$ linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variables: labor force participation and hours worked. Data are from the 2005-2017 IPUMS ACS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

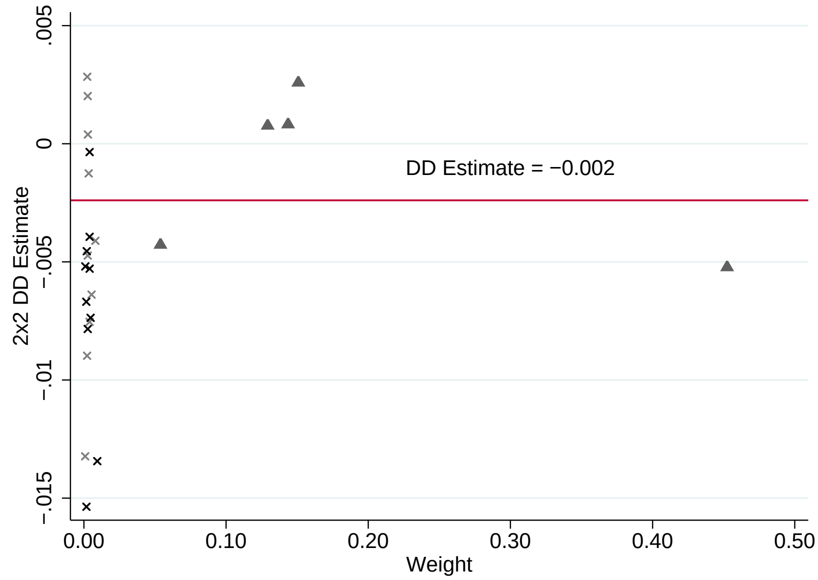
Table A.17: Robustness to *Keeping Only* States That Require E-Verify for All Employers As Treated

	All women	Women with children	Women with children under 5
Panel A: Labor force participation			
E-Verify (σ)	-0.009***	-0.010***	-0.021***
	(0.002)	(0.003)	(0.006)
Mean of dep. var.	0.829	0.807	0.763
Observations	2,107,235	979,501	307,622
Panel B: Usual hours worked			
E-Verify (σ)	-0.291*	-0.418**	-0.939***
	(0.154)	(0.191)	(0.287)
Mean of dep. var.	33.02	31.18	29.30
Observations	2,105,904	978,990	307,514
Demographics	X	X	X
Year and state fixed effects	X	X	X
2000 state vars $\times$ linear time	X	X	X

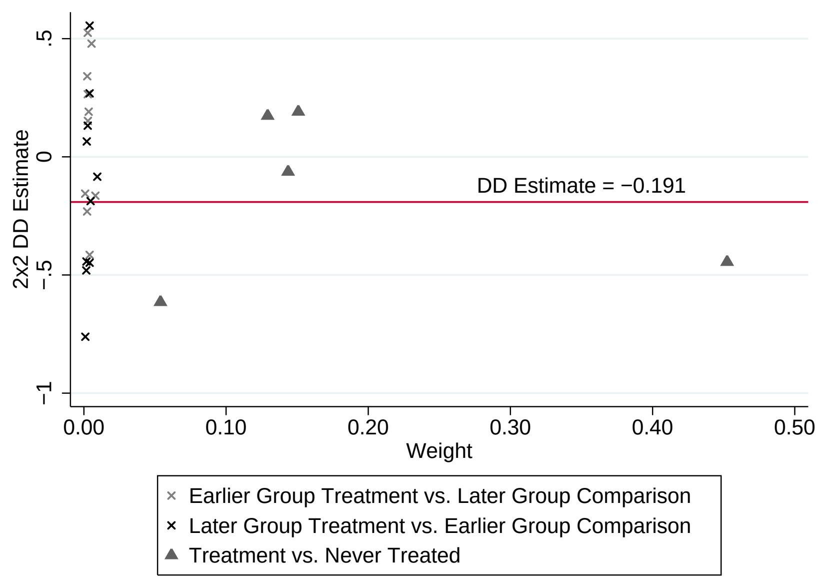
Notes: Each parameter is from a separate regression of the outcome variables: labor force participation and hours worked. Data are from the 2005-2017 IPUMS ACS. I restrict the sample to U.S. citizen women with a college degree or more aged 20-64. All estimations include year fixed effects, state fixed effects, and demographic controls: age, age squared, black dummy, married dummy, reside within a metropolitan area dummy, number of children, number of children under age 5, educational attainment. 2000 state-level variables (labor force participation rate, the share of the state that are immigrants, black, married, have children, have young children, reside within a metropolitan area, work more than 50 and 60 hours, have a high school diploma, some college, or college degree), each interacted with a linear time trend. State-by-year-level controls include housing price and unemployment rate. All results are estimated using the survey sampling weights. Standard errors are clustered at the state level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Figure A.1: Goodman-Bacon DD Decomposition for E-Verify and Labor Supply, All Women

(a) Labor force participation



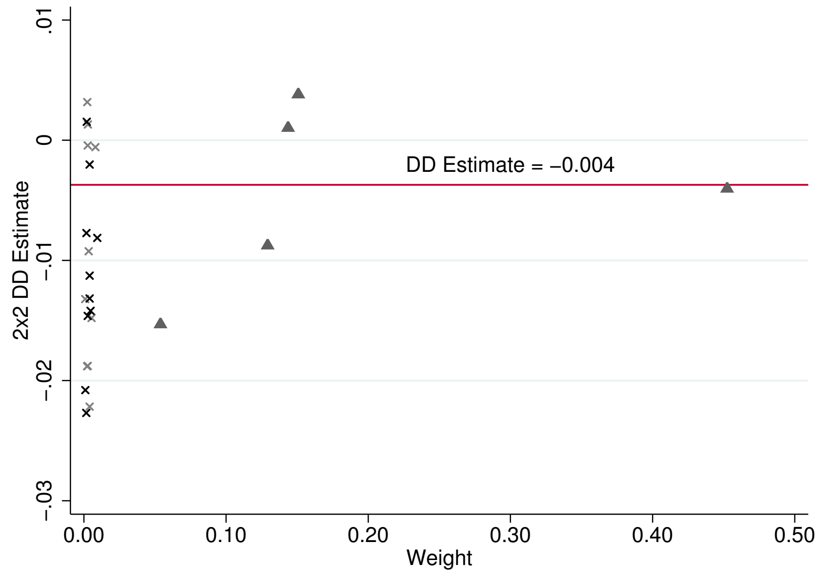
(b) Usual hours worked



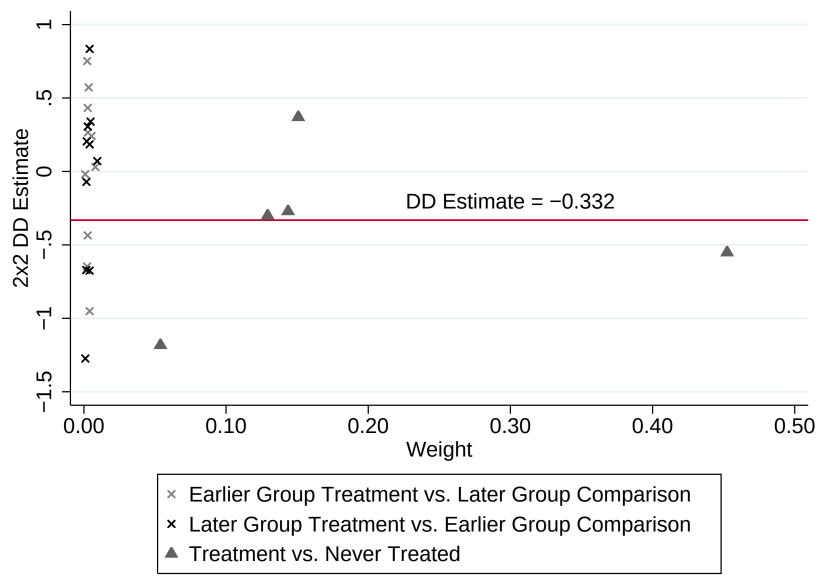
Notes: Data are from the 2005-2017 IPUMS ACS. The sample includes U.S. citizen women with a college degree or more aged 20-64. The figures plot each 2x2 DD components from the decomposition theorem in Goodman-Bacon (2019) against their weight. The bias resulting from time-varying effects (comparisons between earlier/later treated states are given very small weights. The red lines are the weighted average of all possible 2x2 DD estimators. All the DD estimates here match closely to the DD estimates using the baseline model in equation (1).

Figure A.2: Goodman-Bacon DD Decomposition for E-Verify and Labor Supply, Women with Children

(a) Labor force participation



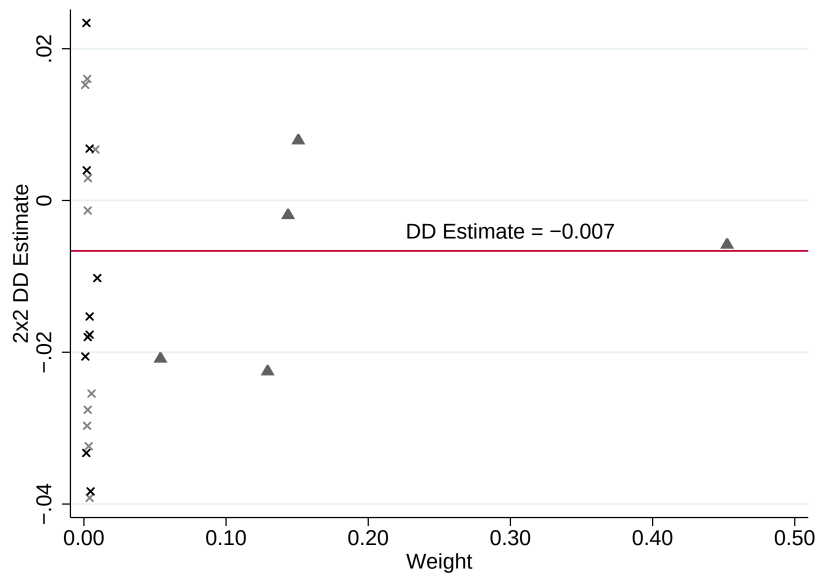
(b) Usual hours worked



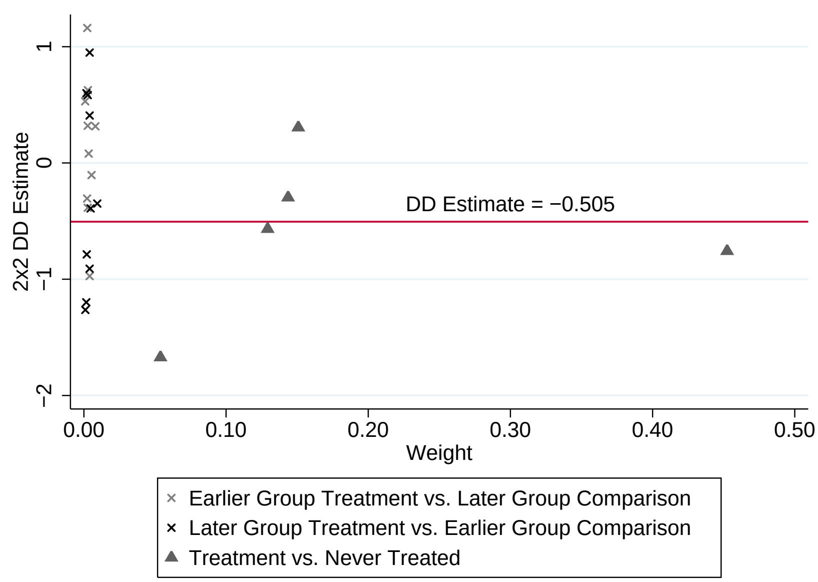
Notes: Data are from the 2005-2017 IPUMS ACS. The sample includes U.S. citizen women with a college degree or more aged 20-64. The figures plot each 2x2 DD components from the decomposition theorem in Goodman-Bacon (2019) against their weight. The red lines are the weighted average of all possible 2x2 DD estimators. All the DD estimates here match closely to the DD estimates using the baseline model in equation (1).

Figure A.3: Goodman-Bacon DD Decomposition for E-Verify and Labor Supply, Women with Children Under 5

(a) Labor force participation



(b) Usual hours worked



Notes: Data are from the 2005-2017 IPUMS ACS. The sample includes U.S. citizen women with a college degree or more aged 20-64. The figures plot each 2x2 DD components from the decomposition theorem in Goodman-Bacon (2019) against their weight. The red lines are the weighted average of all possible 2x2 DD estimators. All the DD estimates here match closely to the DD estimates using the baseline model in equation (1).

Table A.18: Goodman-Bacon DD Decomposition for E-Verify and Women Labor Supply

	All women		Women with children		Women with children under 5	
Panel A: Labor force participation						
Diff-in-diff Est	-0.002		-0.004		-0.007	
DD Comparison	Weight	Avg DD Est	Weight	Avg DD Est	Weight	Avg DD Est
Earlier T vs. Later C	0.035	-0.004	0.035	-0.009	0.035	-0.013
Later T vs. Earlier C	0.035	-0.008	0.035	-0.010	0.035	-0.013
T vs. Never treated	0.930	-0.002	0.930	-0.003	0.930	-0.006
Panel B: Usual hours worked						
Diff-in-diff Est	-0.191		-0.332		-0.505	
DD Comparison	Weight	Avg DD Est	Weight	Avg DD Est	Weight	Avg DD Est
Earlier T vs. Later C	0.035	0.084	0.035	0.019	0.035	0.070
Later T vs. Earlier C	0.035	-0.058	0.035	-0.066	0.035	-0.157
T vs. Never treated	0.930	-0.206	0.930	-0.360	0.930	-0.540

Notes: T = Treatment; C = Comparison. Data are from the 2005-2017 IPUMS ACS. The sample includes U.S. citizen women with a college degree or more aged 20-64.

3 Deportation Fear and Birth Outcomes: Evidence from Immigration Enforcement

3.1 Introduction

In the United States, about 1.4% of infants are born with very low birth weight (VLBW, less than 1,500 grams) and 8.3% are born with low birth weight (LBW, less than 2,500 grams).[1] These numbers are exacerbated for vulnerable populations, especially among people at the bottom of income quintiles (Martinson and Reichman, 2016). Hispanic immigrants are more likely to have lower education attainment, lower income, more life stressors, and more administrative burden (Radford and Noe-Bustamante, 2017; Heinrich, 2018). LBW in turn has adverse effects on future outcomes such as adult health, schooling attainment, and wages (see Almond and Currie, 2011; Almond et al., 2018, for recent reviews).[2]

Immigration enforcement could have negative effects on children who are citizens, hinting at very long-term effects that perpetuate inequality. While others have found negative impacts of immigration enforcement on other outcomes,[3] little is known about the effects of immigration enforcement on infant health outcomes. LBW is likely to have more serious consequences on people's long-term health and development as mentioned above. And because children of immigrants are U.S. citizens by birth, they are likely to live in the U.S. all of their lives, and so they will need more health care and more services in schools.

In this paper, I evaluate the impact of the most restrictive national immigration policy in the U.S. on birth outcomes. In particular, I examine the impact of the Secure Communities

[1]Source: National Vital Statistics Reports, vol. 68, no. 13: Births: Final Data for 2018.

[2]Birth endowments also predict the cognitive development of the next generation (Kreiner and Sievertsen, 2020).

[3]Growing evidence indicates that immigration enforcement adversely affects immigrant families. In particular, past research suggests that local enforcement increased stress and anxiety among immigrants (including pregnant women) and deterred them from seeking safety net programs and health services (Watson, 2014; Vargas and Pirog, 2016; Vargas and Ybarra, 2017; Amuedo-Dorantes et al., 2018; Wang and Kaushal, 2019; Alsan and Yang, 2019).

(SC) program, which ran from 2008 to 2014 and led to the deportation of nearly 450,000 immigrants,[4] on birth outcomes of U.S.-born Hispanic infants.

To identify the potential unintended consequences of SC on Hispanic infants' birth outcomes, I exploit a quasi-experimental staggered rollout of SC across counties due to various technological constraints. Specifically, I collect the data on the SC activation date at the county level and merge these data with administrative birth certificate data from 2005–2016. Doing this allows me to estimate a triple-differences model comparing birth outcomes of Hispanic infants within a county to birth outcomes of non-Hispanic infants, net of counties that had not yet activated, before versus after SC activation.

I show that SC has adverse consequences on the incidence of VLBW and LBW of Hispanic infants. On relative conservative estimates, infants of likely undocumented mothers[5] are 23% more likely to be born VLBW and are 10% more likely to be born LBW, compared to non-Hispanic infants. Compared to other traumatic experiences affecting birth weights, exposure to SC is as large as the effect of losing a family member (Persson and Rossin-Slater, 2018).

I examine the validity of my identification strategy using two placebo tests. First, I reproduce the analysis, but instead of focusing on infants of foreign-born Hispanic mothers as the potential treated group, I focus on a population group that I know ex ante should not be affected by immigration enforcement: infants of non-Hispanic white citizens.[6] I find no effects on this population: all estimated coefficients are indistinguishable from zero and are statistically insignificant.

The second test consists of another placebo analysis that involves estimating the same regressions for a placebo characteristic, whether an infant was born on "odd days," which

[4]See https://trac.syr.edu/phptools/immigration/secure/.

[5]I follow the literature defining likely undocumented immigrants as Hispanic non-citizen high school dropouts. This is not a perfect proxy but is the standard method on estimating undocumented population in the U.S. (see Warren, 2014; Capps et al., 2018; Passel and Cohn, 2018, for a discussion). Indeed, around 80% of unauthorized immigrants are from Latin America in 2016 according to the Pew Research Center's estimation (Passel and Cohn, 2018).

[6]Specifically, I estimate a difference-in-differences specification for a sample of infants of white citizen mothers, before versus after SC activation, between treatment and control counties.

should not be affected by heightened immigration enforcement.[7] As expected, I find that the chance of infants being born on "odd days" was similar between Hispanics and non-Hispanics. Specifically, all event study coefficients are close to zero and statistically insignificant.

There are many possible ways exposure to immigration enforcement can affect birth outcomes. I provide evidence in favor of two possible mechanisms: (i) maternal stress due to fear induced by immigration enforcement and (ii) worse prenatal nutrition due to lower participation in safety net programs and lower rates of employment among undocumented immigrants. I also rule out some important potential channels including changes to migration and engagement in adverse maternal behavior such as smoking.

This paper contributes to three strands of literature. The first is a growing literature on the direct effects of SC on immigrants and its spillover effects on citizens. This finds that SC does not have any impact on crime rates (Miles and Cox, 2014), increases the poverty risk and the likelihood of being in foster care for Hispanic youth (Amuedo-Dorantes et al., 2018; Amuedo-Dorantes and Arenas-Arroyo, 2018), decreases safety net program participation of non-citizens (Watson, 2014; Padraza and Zhu, 2014; Vargas and Pirog, 2016) and Hispanic citizens (Alsan and Yang, 2019), reduces rates of employment among low-skilled non-citizen males (East et al., 2019) and high-skilled citizen mothers (East and Velasquez, 2020), and worsens the mental health of Hispanic immigrants (Wang and Kaushal, 2019).

This paper advances this literature in two ways. First, I provide causal evidence on the effects of immigration enforcement on birth outcomes of U.S.-born Hispanic infants. Second, I provide evidence about two possible mechanisms whereby SC could affect infants' health: (i) maternal stress due to deportation fear and (ii) inadequate nutrition during pregnancy. Given that SC was reactivated in 2017, knowing the spillover impact of local immigrant enforcement on future citizens' health would allow policymakers to make more informed decisions or design and create different types of policies.

[7]Odd days are Sunday, Tuesday, Thursday, and Saturday.

The second strand is the literature on understanding why inequality persists (Piketty and Saez, 2003; Nolan et al., 2012). I contribute to this literature by providing novel evidence that anti-immigration policies may be a crucial and understudied mechanism through which early life health disparities perpetuate persistent economic inequality between different groups of people.

The third strand is a large literature on both the short- and long-term effects of fetal stress exposure on birth and adult outcomes, recently reviewed by Almond and Currie (2011). The stressors can come from impacts on (i) physical health such as malnutrition (Almond and Mazumder, 2011; Almond et al., 2011b; Hoynes et al., 2011; Rossin-Slater, 2013; Hoynes et al., 2016), intimate partner violence (Currie et al., 2019), pollution (Almond et al., 2009; Sanders, 2012; Isen et al., 2017), diseases (Almond, 2006; Barreca, 2010), and famine (Almond et al., 2010b; Scholte et al., 2015); or from (ii) impacts on both mental and physical health such as the loss of a loved one (Black et al., 2016; Persson and Rossin-Slater, 2018), terrorist attacks (Berkowitz et al., 2003; Lauderdale, 2006; Camacho, 2008), and natural disasters (Tan et al., 2009; Simeonova, 2011; Torche, 2011; Currie and Rossin-Slater, 2013). I add to this literature by providing novel evidence on using in utero exposure to an anti-immigration policy to identify the effects of maternal stress on birth outcomes.

The rest of the paper proceeds as follows. I provide further detail regarding the literature and the background of SC in Section 3.2. Section 3.3 and Section 3.4 discuss data and empirical framework. I discuss results on birth outcomes, placebo tests, mechanisms, and robustness checks in Section 3.5, and Section 3.6 concludes.

3.2 Background and Literature

3.2.1 Policy Background

SC is one of the largest deportation programs in the U.S. history.[8] The program was started in October 2008 and was temporarily suspended in October 2014 but was reactivated in January 2017. To build deportation capacity, SC relies on partnership between U.S. Immigration and Customs Enforcement (ICE), the Federal Bureau of Investigation (FBI), and local law enforcement agencies. The program objective is to help ICE arrest and remove individuals who violate federal immigration laws, including those who are convicted of serious criminal offenses. From 2008 to 2014, ICE deported over 450,000 immigrants under SC.

The deportations of people with minor offenses or no offense creates fear among immigrant groups. Ordinarily, the fingerprints of county and state arrestees are only submitted to the FBI; however under SC, the prints go to ICE as well. Fingerprint matching databases have made it much easier to determine whether an arrested individual is, for instance, unlawfully present in the country. Technically, any arrested non-citizens can be subject to deportation (including legal immigrants and green card holders). For undocumented immigrants, even minor offenses can trigger deportations. Indeed, nearly half of the deportees under SC had only minor offenses (such as public drunkenness or jaywalking) or no offense at all.[9] This has been argued to increase fear and decrease participation in public benefit programs.

3.2.2 Immigration Enforcement and Birth Outcomes

The existing evidence on the birth outcome effects of fetal stress exposure to immigration enforcement is extremely limited. Only two studies appear to examine the impacts of immigration enforcement policies on birth outcomes of U.S.-born infants. Each is a case study of a particular county or city policy. Additionally, no previous studies have examined the

[8] For comprehensive reviews of SC, see Cox and Miles (2013) and Alsan and Yang (2019).

[9] See https://trac.syr.edu/phptools/immigration/secure/ for more information.

causal impacts of SC on birth outcomes.

Novak et al. (2017) used birth certificate data for all births in Iowa from 2006 to 2010 to study the impact of a 2008 federal immigration raid in Postville, Iowa on birth outcomes. Using a modified Poisson regression, the authors find that the raid was associated with a 24% increase in risk of LBW for infants born to Hispanic mothers compared with the same period one year earlier. While this study was primarily descriptive, it is the first evidence on adverse consequences of an immigration raid on infant health.

Tome et al. (2019) explore the effect of Section 287(g)[10] of the Immigration and Nationality Act on birth outcomes in Mecklenburg County, North Carolina. Using long-form birth certificate data from the North Carolina Detailed Birth Records, the authors use two identification strategies: difference-in-differences and triple-differences case control regression analysis. They find that 287(g) was associated with a 3.5 percentage point increase in the incidence of LBW infants.

The current study makes several contributions to this literature. First, I exploit more policy variation than was available to prior scholars to generate more generalizable estimates of the effects of immigration enforcement laws. Second, I explore potential explanations for why infants of Hispanic immigrant mothers have higher incidences of VLBW and LBW births relative to other immigrant groups in the face of immigration enforcement.

3.3 Data

This paper uses several data sources to measure birth outcomes and deportation fear as well as information about the activation of SC.

SC rollout data: I have obtained information about the SC rollout dates as well as the

[10]Both SC and 287(g) identify and deport undocumented immigrants who have been arrested by local officers and deputies. The difference between SC and 287(g) is that SC is an automated fingerprint matching system that screens criminal aliens for removal that is run by ICE. While under 287(g), aliens who have been arrested are screened by *local* officers in that jurisdiction.

monthly number of detainers, or "immigration holds"[11], and the monthly number of removals under SC from ICE public records and Transactional Records Access Clearinghouse (TRAC) Immigration.[12] Figure 3.1 shows the rollout of SC across counties in the U.S. The figure shows there are crucial variations in the SC activation, both across counties and through time, which I exploit in identifying the effects of SC on birth outcomes.

One relevant question is whether SC was associated with the number of removals. Figure A.4 shows the total number of detainers per year. There is an abrupt increase in the number of detainers immediately following SC activation in 2008. This serves as evidence that SC was associated with the increasing number of removals.

Vital Statistics Natality data: To measure birth outcomes, I use restricted access 2005–2016 natality data from the National Center for Health Statistics. The natality data are the universe of birth records in the U.S. Data on the month, year, and county of birth allow me to link the birth data to SC activation dates in a given county. The data include infants' characteristics such as birth weight, gender, plurality, and gestational length. There are also demographic variables, including age, race, education, marital status, and birthplace of mothers.

Google Trends data: To directly test the channel that stress induced by deportation fear affects birth outcomes, I need data on the deportation fear level in response to SC. Unfortunately, I cannot construct this ideal fear-level variable because my data do not contain information such as whether a respondent feels insecure or fear due to immigration enforcement. Instead, I use commonly searched terms related to the deportation topic on Google Trends to proxy for deportation fear.

Google Trends is a public use database that provides access to an unfiltered sample of

[11]An ICE detainer is a written request that a local jail or other law enforcement agency detain an individual for an additional 48 hours to provide ICE agents extra time to decide whether to take the individual into federal custody for removal purposes.

[12]TRAC is a data gathering, data research, and data distribution organization at Syracuse University. See https://trac.syr.edu/aboutTRACgeneral.html for more details.

actual search requests made to Google.[13] For each search term i in U.S. media market d (according to the Nielsen Nielsen Designated Market Area (DMA) definition.), Google Trends returns the normalized share of searches in market d that contain the search term i $G(i, d)$ as follows (Burchardi et al., 2018):

$$G(i, d) = \left[100 \cdot \frac{share(i, d)}{max_\delta\{share(i, \delta)\}} \cdot \mathbf{1}[\#(i, d) > T]\right], \tag{1}$$

where $share(i, d)$ is the share of searches in d that contains i and $max_\delta\{share(i, \delta)\}$ is the maximum share of searches that contains i across all the market δ. T is a search frequency cutoff that must be exceeded for Google to permit access to the data (Stephens-Davidowitz and Varian, 2015). Thus, $G(i, d)$ is equal to 100 in the metro area with the largest share of searches containing i and is equal to a positive number smaller than 100 in all other metro areas that have a sufficient number of searches containing i.

To measure the relative deportation fear in a given metro area, I take a simple sum of search intensity across all search terms i and normalize it by search terms that are popular in the Hispanic community p. This accounts for differential internet access for Hispanics across media markets. Specifically, I calculate a Deportation Fear Index for each market-year as

$$DFI(d) = log\left(\sum_i G(i, d) + \sum_p G(p, d)\right), \tag{2}$$

where $i \in$ {immigration police, policía de inmigración, ICE police, policía de ICE, deportation, deportación, immigration, inmigración, immigration lawyers, abogados de inmigración, undocumented, indocumentado} and $p \in$ {deportes (sports), telenovelas (soap operas)}.[14]

County population data: I use the Surveillance, Epidemiology, and End Results (SEER)

[13] Burchardi et al. (2018) provide an excellent, detailed discussion of Google Trends data; much of my discussion of the data is guided by their work.

[14] These deportation-related terms were picked following Alsan and Yang (2019). I add "immigration police," "policía de inmigración," "police ICE," and"policía de ICE" in addition to their list.

population data to construct a fertility rate that is defined as the number of births per 1,000 women ages 15–44. First, the SEER population data are used to estimate the population of women ages 15–44 by county-race-year. Then these are then combined with births by county-race-month-year to construct the fertility rate.

3.4 Empirical Framework

To examine the causal effect of SC on birth outcomes of likely undocumented immigrants, I use the SC program's staggered rollout across the counties. My main specification is a triple-differences model comparing Hispanic infants to non-Hispanic groups (first difference), before versus after the SC activation (second difference), in treated versus control counties (third difference). Specifically, I estimate the following model with county, state, month, and year of birth fixed effects as follows:

$$\begin{aligned} Y_{icsmy} = \alpha + \beta_1(SC_{cmy} \times HISP_i) + \beta_2 SC_{cmy} + \beta_3 HISP_i + \gamma_1 X_i \\ + \gamma_2 Z_{sy} + \gamma_3 Z_{csy} + \delta_s \cdot t + \mu_c + \theta_m + \lambda_y + \epsilon_{icsmy} \end{aligned} \tag{3}$$

for each individual i in county c, state s, for birth month m, and birth year y. Y_{icsmy} is the outcome of interest. SC_{cmy} is the SC activation treatment variable and equals one if i's birth date is after the SC activation and zero otherwise. $HISP_i$ is an indicator for Hispanic ethnicity. X_i is a vector of individual control variables for maternal and infant characteristics, including four dummies for mother's age, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status, and a dummy for male birth. Z_{st} contains annual state-level controls including unemployment rate and percentage of population who are Hispanic, black, white, and female ages 15–44. Z_{cst} includes race-by-county unemployment changes during the Great Recession to account for differential impacts of the recession by race. The term $(\delta_s \cdot t)$ is a state-specific time trend where t = year –

2005.

County (μ_c) and year (λ_t) fixed effects are included to capture national shocks and time-invariant unobserved heterogeneity that might affect birth outcomes. Month of birth (θ_m) fixed effects are included in my preferred specification to adjust for monthly shocks that affect birth outcomes such as changes in weather conditions.

In all specifications, I follow East et al. (2019) in excluding border counties since SC programs were activated in those counties early and this selection in activation could bias my results.[15] I also follow Alsan and Yang (2019) in excluding Illinois, Massachusetts, and New York, as governors in these states attempted to opt out by ending their memorandum of agreement with the Department of Homeland Security regarding SC activation in the spring of 2011. I require counties to have at least 30 births per year to prevent estimation problems associated with thinness in the data. The results are not sensitive to this sample selection, and standard errors are clustered at the county level (Bertrand et al., 2004).

To measure the spillover effect of SC on birth outcomes of U.S.-born Hispanic infants, I would ideally like to directly examine birth outcomes of infants of undocumented mothers. But because there are no available data that allow for precise identification of undocumented immigrants at the individual level, I follow the literature defining likely undocumented immigrants as Hispanic non-citizen high school dropouts.[16] This is an important limitation and may not accurately reflect immigration status for some members in my sample. For example, a mother who was born outside of the U.S. but was granted citizenship through naturalization causes me to misclassify that individual was undocumented. In general, I

[15]The border counties I exclude from all analyses are as follows: San Diego County, CA; Imperial County, CA; Yuma County, AZ; Pima County, AZ; Santa Cruz County, AZ; Cochise County, AZ; Hidalgo County, NM; Luna County, NM; Dona Ana County, NM; El Paso County, TX; Hudspeth County, TX; Jeff Davis County, TX; Presidio County, TX; Brewster County, TX; Terrell County, TX; Val Verde County, TX; Kinney County, TX; Maverick County, TX; Webb County, TX; Zapata County, TX; Starr County, TX; Hidalgo County, TX; and Cameron County, TX.

[16]I acknowledge that this is not a perfect proxy, but it is the standard method on estimating undocumented population in the U.S. See Warren (2014); Capps et al. (2018); Passel and Cohn (2018) for a discussion. Indeed, around 80% of unauthorized immigrants are from Latin America in 2016 according to the Pew Research Center's estimation (Passel and Cohn, 2018).

expect that this misclassification will bias my estimates toward zero.

The key coefficient of interest is β_1, which measures the estimate of SC's effects on birth outcomes of Hispanic infants relative to all non-Hispanic infants (both black and white), compared to counties that have not yet activated SC.

I also conduct an event study specification to see if there is a systematic difference in birth outcomes for Hispanic infants before the SC activation across counties. The number of observations in the data allows me to estimate up to five pre-SC years and four post-SC years:

$$\begin{aligned} Y_{icsmy} = \alpha + \sum_{r \neq -1} \beta_1^r \cdot \mathbf{1}\left[r = t\right] \cdot HISP_i + \sum_{r \neq -1} \beta_2^r \cdot \mathbf{1}\left[r = t\right] + \gamma_1 X_i \\ + \gamma_2 Z_{sy} + \gamma_3 Z_{csy} + \delta_s \cdot t + \mu_c + \theta_m + \lambda_y + \epsilon_{icsmy}, \end{aligned} \tag{4}$$

where $\mathbf{1}\left[r = t\right]$ is an indicator for each period (the year prior to SC activation, $r = -1$, is omitted). The coefficients of interest, β_1^r, trace the effects of SC on birth outcomes of Hispanic infants in the year before and after SC activation relative to non-Hispanic infants. All the controls and fixed effects are the same as in Equation (3).

3.4.1 Identifying Assumption

My identification relies on the assumption that "the event" (in this case, SC activation) is exogenous to the outcome variables. I verify the validity of this identification assumption in two ways. First, I implement a variant of Fisher's permutation or randomization inference test (Fisher, 1935).[17] To implement this exercise, I estimate Equation (3) 1,000 times by randomly assigning a placebo SC activation year for each county, ensuring that there are six years as "treated" and six years as the pre-period. Figure 3.4 shows the histogram of placebo estimates along with vertical solid lines representing my actual triple-differences

[17] This test has been suggested and used by Conley and Taber (2011), Agarwal et al. (2014), Cohen and Schpero (2018), Alsan and Yang (2019), Grossman and Slusky (2019), and Kuka et al. (2020).

estimates. The dashed lines are the 5th and 95th percentile of the placebo estimates. The permutation tests show that there are no mechanical reasons why my event study framework would generate significant effects.

I then test whether predicted birth outcomes are correlated with SC activation. Using pre-period data, I regress birth outcomes on a large set of observable characteristics and use the estimated coefficients to predict birth outcomes for each infant in the sample.[18] Figure A.5 corresponds to the event study estimates of Equation (4) for the *predicted* likelihood of VLBW and LBW births. In contrast to the main event study estimates in Figure 3.3(a) and 3.3(c), the coefficients are insignificant and show no trend breaks in the predicted birth outcomes.

3.5 Results

3.5.1 Effects on Birth Outcomes

I first examine the effects of SC on birth outcomes. Figures 3.3(a) and 3.3(c) correspond to the event study estimates described in Equation (4). These figures present the effects of SC on Hispanic infants relative to non-Hispanic infants in each of the five years leading up to a SC activation and four years after the SC activation. The year before the event ($t = -1$) corresponds to an omitted category and is thus normalized to zero by construction.

Figures 3.3(a) and 3.3(c) show that in the five years prior to the activation, there is no difference of either the likelihood of a VLBW birth or a LBW birth between Hispanic infants and non-Hispanic infants. On the contrary, these likelihoods start to diverge a few years after the activation: relative to non-Hispanic infants, the risk of VLBW and LBW Hispanic infants are larger. Specifically, by four years after the SC activation, Hispanic infants have a 23% higher probability of VLBW and a 10% higher probability of LBW, compared to

[18]The set of characteristics include gender, year, month, week of birth, indicators for maternal age dummies, indicators for mother being married, maternal race dummies, and maternal education dummies.

non-Hispanic infants.

Table 3.1 presents the triple-differences results on SC's effects on indicators for VLBW, LBW, premature birth, and average birth weight. In line with the event studies, I find that SC led to statistically significant increases in the likelihood of a VLBW birth and a LBW birth. The magnitudes of the coefficients imply that SC is associated with a 23% increase in VLBW (column 1) and a 10% increase in LBW (column 2). Compared to other traumatic experiences affecting birth weights, exposure to SC is as large as the effect of losing a family member as estimated in Persson and Rossin-Slater (2018).

My estimates suggest that in utero exposure to immigration enforcement leads to a negative effect on average birth weight of 12 grams (column 4 of Table 3.1). However, much of this effect is driven by impacts at births that are already at risk or more vulnerable (Figure A.6).[19] This finding is consistent with Persson and Rossin-Slater (2018)'s study on stress due to family bereavement on birth outcomes. Due to the smaller findings for the average birth weight and prematurity, I continue to focus only on VLBW and LBW for the remainder of the analysis.

3.5.2 Placebo Tests

I examine the validity of my identification strategy using two placebo tests. First, I reproduce the analysis, but instead of focusing on infants of foreign-born Hispanic mothers as the potential treated group, I focus on a population group that I know ex ante should be immune from deportation and SC activation: infants of non-Hispanic white citizens. Figures 3.3(b) and 3.3(d) correspond to difference-in-differences estimates for a subsample of infants of

[19]Following Almond et al. (2011b), figure A.6 further examines the impacts of exposure to immigration enforcement on the distribution of birth weight. Each dot on the solid line is the percentage impact (coefficient/mean) of SC activation to the probability that birth weight is below a given threshold: 1,500, 2,000, 2,500, 3,000, 3,250, 3,500, 3,750, 4,000, and 4,500 (grams). These percentage impacts are around zero until the birth weight threshold 3,000 and start increasing below threshold 3,000. All percentage impacts are significantly different from zero after threshold 2,500. This figure shows that the effects on birth weight are larger for births at the lower end of the birth weight distribution.

white citizen mothers, before versus after SC activation, between treatment and control counties.[20] Figures 3.3(b) and 3.3(d) show that all effects are close to zero and statistically insignificant. For infants of white citizen mothers, the likelihood of VLBW or LBW in the five years prior and four years after SC activation follows the same trajectories.

The second placebo test involves estimating the same regressions for a placebo characteristic, whether an infant was born on "odd days," which should not be affected by heightened immigration enforcement.[21] Figure A.7 reports the results using "odd days" as the dependent variable. The results indicate that the chance of infants being born on "odd days" was similar between Hispanics and non-Hispanics. The event study coefficients were stable prior to the event and remained at the same level after the SC activation.

In sum, both placebo tests reveal precise null effects, confirming that the negative impacts of immigration enforcement do not simply seem to arise by chance.

3.5.3 Mechanisms

In this section, I discuss some potential mechanisms that may explain SC's effects on birth outcomes of infants of Hispanic immigrant mothers documented in the previous section.

Maternal stress due to deportation fear: A growing body of evidence suggests that uncertainty about the future and fear surrounding intensified immigration enforcement are associated with poorer self-reported health and mental health, chronic stressors, cardiovascular risk, and inflammation (Vargas et al., 2017; Torres et al., 2018; Martínez et al., 2018), which in turn could increase the risk for VLBW and LBW births. Biological pathways for this influence is that stress increases cortisol, norepinephrine, and inflammation, which affect the fetal environment (see Field et al., 2004; Kinsella and Monk, 2009, for recent reviews). Specifically, maternal stress has been shown to be associated with higher fetal heart rate,

[20] Note that this is a separate difference-in-differences on a subsample of non-Hispanic white citizens, not the β_2 coefficients of Equation (3).

[21] Odd days are Sunday, Tuesday, Thursday, and Saturday.

higher fetal activity, higher fetal movement, and lower fetal sleep (DiPietro et al., 1996; Allister et al., 2001; Dieter et al., 2008).

I build on these works of public health and medical scholars to test the hypothesis that deportation fear is an important channel driving the infant health results. First, I construct a Deportation Fear Index using the Google Trends data on deportation-related search terms (see Section 3.3 for more details). Table 3.2 presents difference-in-differences estimates of SC's effects on deportation-related searches. These results indicate a statistically significant increase in an index that proxies for deportation fear or at least the interest in deportation-related information.

I then examine the effects of sanctuary policies on birth outcomes. Sanctuary counties enacted policies that limit cooperation with federal immigration enforcement officials. Thus, if deportation fear is a potential mechanism, SC would have weaker effects on Hispanic mothers in the sanctuary counties. To test this hypothesis, following Alsan and Yang (2019), I exploit data on a list of sanctuary counties, obtained via a Freedom of Information Act request filed by the Immigrant Legal Resource Center.[22] Consistent with the mechanism, I find evidence that the likelihood of VLBW and LBW are lower in sanctuary counties compared to the baseline results (columns 2 and 5 in Table 3.3).

The next test of the maternal stress induced by deportation fear channel exploits heterogeneity of exposure to SC activation. If fear plays an important role, then I should observe stronger effects in counties with a higher share of Hispanic immigrants. I use the American Community Survey data to calculate the percentage of Hispanic non-citizens and Hispanic non-citizen *high school dropouts* in each county. Table 3.3 presents the coefficients of ($SC \times HISP$) in the main specification (Equation 3) for counties with a high share of Hispanic non-citizens.[23] Given my proposed channel, I expect β_1 to be increasing in mag-

[22]See https://www.ice.gov/doclib/ddor/ddor2017_02-04to02-10.pdf for a list of sanctuary counties.

[23]Counties that have share of Hispanic non-citizens greater than the mean share of Hispanic non-citizens across counties.

nitude as the concentration of the Hispanic population increases. I find that the effects are more pronounced among infants born in counties with a higher share of Hispanic non-citizens (columns 3 and 6 in Table 3.3).

Poor prenatal nutrition: While maternal stress is a viable mechanism, lower participation in safety net programs and employment likelihood may also be a critical mechanism due to worse prenatal nutrition. Indeed, a growing literature on the impacts of SC finds that the program reduces non-citizens participating in safety net programs (Warren, 2014; Padraza and Zhu, 2014; Vargas and Pirog, 2016) and decreases the likelihood of low-skilled non-citizens being employed (East et al., 2019). These findings suggest that inadequate nutrition during pregnancy could possibly explain the negative effects of SC on birth outcomes of Hispanic infants.

Maternal behavior changes: Thus far, I have argued that prenatal stress induced by SC has significant effects on birth outcomes of infants of foreign-born Hispanic mothers. These effects may also occur indirectly through the effects of prenatal stress on maternal behaviors and well-being that in turn affect fetal development. For example, stress may cause mothers to develop hypertension or start smoking, which may then adversely affect the fetus in utero.

Table 3.4 presents estimates on whether SC activation is associated with the number of prenatal visits; an indicator for WIC (Women, Infants, and Children) take-up; hypertension development; diabetes; and reported tobacco use during pregnancy. I find no statistically significant effects of in utero exposure to immigration enforcement on these maternal risk factors or behaviors, except for a marginally significant impact on diabetes. Overall, I find little effect of pregnancy behavior changes, and these findings support the idea that the estimated effects on birth outcomes are due to stress.[24]

I do see some evidence that SC activation is associated with increases in the use of prenatal care during pregnancy. If anything, this would lead me to expect better infant

[24] I do, however, find a negative (albeit insignificant) coefficient on WIC take-up, suggesting that at least part of my estimated impact on birth outcomes may operate through nutrition channels.

health outcomes and suggests that immigration enforcement effects would be larger in the absence of this association. The higher prenatal visits results seem puzzling at first if the maternal stress induced by deportation fear channel is true. However, health care providers have no affirmative legal obligation to inquire into or report a patient's immigration status to federal immigration authorities. This is different from public benefit (Medicaid or SNAP) take-up context where the program asks about applicants' immigration status.[25] I do not see an increase in the use of prenatal care as inconsistent with the maternal stress mechanism.

Migration: It may be the case that undocumented families migrate in response to immigration enforcement. I test this channel using data from the American Community Survey Integrated Public Use Microdata Series data (Ruggles et al., 2019) and show the results in Table 3.5.[26] The results suggest that SC is not associated with migration rates of Hispanic families relative to non-Hispanic families. This is consistent with Alsan and Yang (2019) and East et al. (2019) who find there were not big migration changes as a result of SC. Thus, I believe that migration changes are unlikely driving my results on birth outcomes.

3.5.4 Sensitivity Checks

The Great Recession: The Great Recession had a significant economic impact on the United States. Although the timing of the recession and the SC activation were similar, I am confident that my results are not confounded by the recession for several reasons. First, I estimate Equation (3) including race-by-state unemployment changes during the Great Recession to account for differential impacts of the recession by race as mentioned above. Second, as shown in Figure 3.2, the upward trends in the likelihood of VLBW and LBW for

[25]The "chilling effect" that immigrant-related families disenroll from Medicaid and SNAP (Padraza and Zhu, 2014; Watson, 2014).

[26]The "smallest" geography available in the public use data is the Public Use Microdata Areas (PUMA). Because data on the SC activation dates are at the county level, I use crosswalks provided by the Missouri Census Data Center to calculate the population-weighted average of the county values from the PUMA values.

Hispanic infants happened after 2011, a year after the recession ended.[27] Third, I only find the effects on birth outcomes among infants of likely undocumented mothers and no effects on non-Hispanic whites (Figure 3.3) who were unaffected by the SC activation by design.

Effects on fertility: One might have the concern that immigration enforcement may lead to changes in fertility among likely undocumented women. This factor, through endogenous sample selection, could bias the estimates. In particular, if SC activation causes *increases* in fertility in the likely undocumented population, this could cause an *upward* bias on the estimates (given the finding that SC increases the incidences of VLBW and LBW for infants of likely undocumented mothers in Section 3.5.1). On the other hand, if the SC activation causes *decreases* in fertility, this could cause a *downward* bias on the estimates.

I consider this possibility by evaluating whether SC activation is associated with any change in the fertility rate in Table A.19. The dependent variables are (i) fertility rate, which is the number of births per 1,000 women age 15 to 44; (ii) birth rate, which is the number of births per 1,000 population; and (iii) probability of a male birth.[28] The SC activation treatment variable equals one if i's birth date is nine months after SC activation (to proxy for conception) and is zero otherwise. I find a negative and statistically significant impact of SC on the fertility rate and birth rate. As stated above, I expect that this finding would bias my estimates toward zero.

Finally, a variety of robustness checks support my main results in Figure 3.5. First, following Alsan and Yang (2019), I include interactions of county fixed effects with an indicator for the "2011 Morton Memo" to account for unobserved county-level characteristics that affect the birth outcomes differently before and after the 2011 Morton Memo.[29] Second,

[27] According to the Federal Reserve History, the Great Recession officially began in the U.S. in December 2007 and lasted until June 2009.
Source: https://www.federalreservehistory.org/essays/great_recession_of_200709.

[28] The probability of a male birth is to proxy for miscarriages as male fetuses are more vulnerable to side effects of maternal stress in utero; a reduction in male births may indicate an increase in miscarriages Sanders and Stoecker (2015).

[29] The 2011 Morton Memo announced that county participation in SC is mandatory.

my estimates are robust to control for an array of other policies aimed at the undocumented immigrant population, including 287(g) Agreements and E-Verify. Third, since one concern is that Hispanic infants in SC-activated counties are different than Hispanic infants in not-yet-activated counties, I include county-by-Hispanic fixed effects and find that my results are robust to this specification. Fourth, my results are robust to excluding Texas, where health facility closures affected health care for women in 2011–2012 (Lu and Slusky, 2016).

3.5.5 Additional Results

This section presents my last two pieces of evidence on the robustness of my main findings.

Expected birth dates versus actual birth dates: In my main specification in Equation (3), I use an infant's actual birth date to define the treatment variable SC_{cmy}.[30] There is a concern that the SC activation can affect the length of the pregnancy, and thus the treatment variable defined using actual birth dates is endogenous and can lead to the finding of a significant relationship when there is none (Matsumoto, 2018; Persson and Rossin-Slater, 2018). Using the expected date of birth to define the treatment group would address the endogenous issue.[31] Unfortunately, using the expected birth dates is extremely difficult given my current data availability and constraints. Specifically, about 58% of the observations in the birth data is missing information on the date of last normal menses, which severely limits the number of expected birth dates that I can construct for use in defining the treatment variable.

To address this issue, I examine an alternative specification that is presented in Table A.20.[32] I initially exclude infants whose birth dates are within one month of the SC activation date. The estimated effects on this sample are very similar to my preferred specification. Subsequently, I exclude successively larger sets of infants, up to ± three months of the SC

[30] SC_{cmy} equals one if an infant's birth date is after the SC activation date and zero otherwise.

[31] An infant's expected birth date is defined as the date of conception plus 280 days.

[32] This is inspired by donut regression discontinuity estimates (Almond et al., 2010a; Barreca et al., 2011; Almond et al., 2011a).

activation date. The estimated effects on this sample slightly change in magnitude, although they continue to be both statistically and economically significant.

Multiple hypothesis testing: To address the multiple hypothesis testing issue, I follow Kling et al. (2007); Currie et al. (2019) and group my outcomes into a birth outcomes index. The birth outcomes index consists of the following measures: VLBW (< 1,500 grams), LBW (< 2,500 grams), premature birth (< 37 weeks of gestation), continuous birth weight in grams, gestation in weeks, very premature birth (< 34 weeks of gestation), low one-minute Apgar score (<7), NICU admission, any abnormal conditions (six indicators: assisted ventilation, assisted ventilation > six hours, admission to NICU, surfactant, antibiotics, and seizures).

This index is created so that a higher value represents a better outcome.[33] Table A.21 presents the results from my main specifications using the index as a dependent variable. The estimates for the effects of in utero exposure to immigration enforcement on birth outcomes are robust to this exercise. Moreover, the estimates suggest that the effects are stronger when the intensity of deportation increases, which support the maternal stress induced by deportation mechanism.

3.6 Conclusion

Between 2008 and 2014, the U.S. activated one of the largest immigration enforcement programs, Secure Communities, which deported over 450,000 immigrants. I propose that because of heightened fear from deportation, prenatal exposure to the immigration enforcement can adversely affect the birth outcomes U.S.-born Hispanic infants. Using administrative birth certificate data and multiple identification strategies, I present evidence that

[33]Specifically, I reorient each outcome so that a higher value represents a better outcome. Then, for each ordered outcome, I subtract the mean and divide by the standard deviation. The birth index is defined to be the equally weighted average of the standardized outcomes. See Kling et al. (2007) and Currie et al. (2019) for more detailed information on how the index is constructed.

tougher immigration enforcement causes an increase of 23% in the likelihood of very low birth weight for infants of foreign-born Hispanic mothers. I provide evidence that some, although probably not all, of these effects operated through (i) maternal stress induced by deportation fear and (ii) undernutrition during pregnancy.

My findings provide evidence of unintended consequences of the SC program, which is designed to affect only undocumented immigrants, on future U.S. citizen birth outcomes. What is the unintended social cost of immigration enforcement? I conduct a back-of-the-envelope calculation to estimate the social cost of immigration enforcement, focusing on the estimates of the effect of immigration enforcement on VLBW births. The calculation suggests an annual social cost around \$1.77 billion ($= \$2,457,114 \times 721$) in 2018 dollars based on the best available estimates on the cost of VLBW \$2,457,114 (Currie et al., 2019) and an increase of 721[34] VLBW infants born to undocumented mothers.[35]

The results in this paper imply that immigration enforcement can have unintended consequences not just for undocumented immigrants but also for the next generation who are future citizens and for society as a whole. It is an open question of whether prenatal exposure to immigration enforcement has any long-term consequences on child health and development as well as on maternal well-being.

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[34]The average number of VLBW infants born to undocumented women prior to SC is 3,072 infants per year (source: author's calculation using Natality data). A 23.47% increase is 721 (= 3072×0.2347).

[35]These numbers likely underestimate the full social cost of immigration enforcement on pregnant women for at least two reasons: (i) the effects of SC on VLBW is biased downward due to measurement error in likely undocumented status as mentioned above, and (ii) the effects on maternal well-being was not measured.

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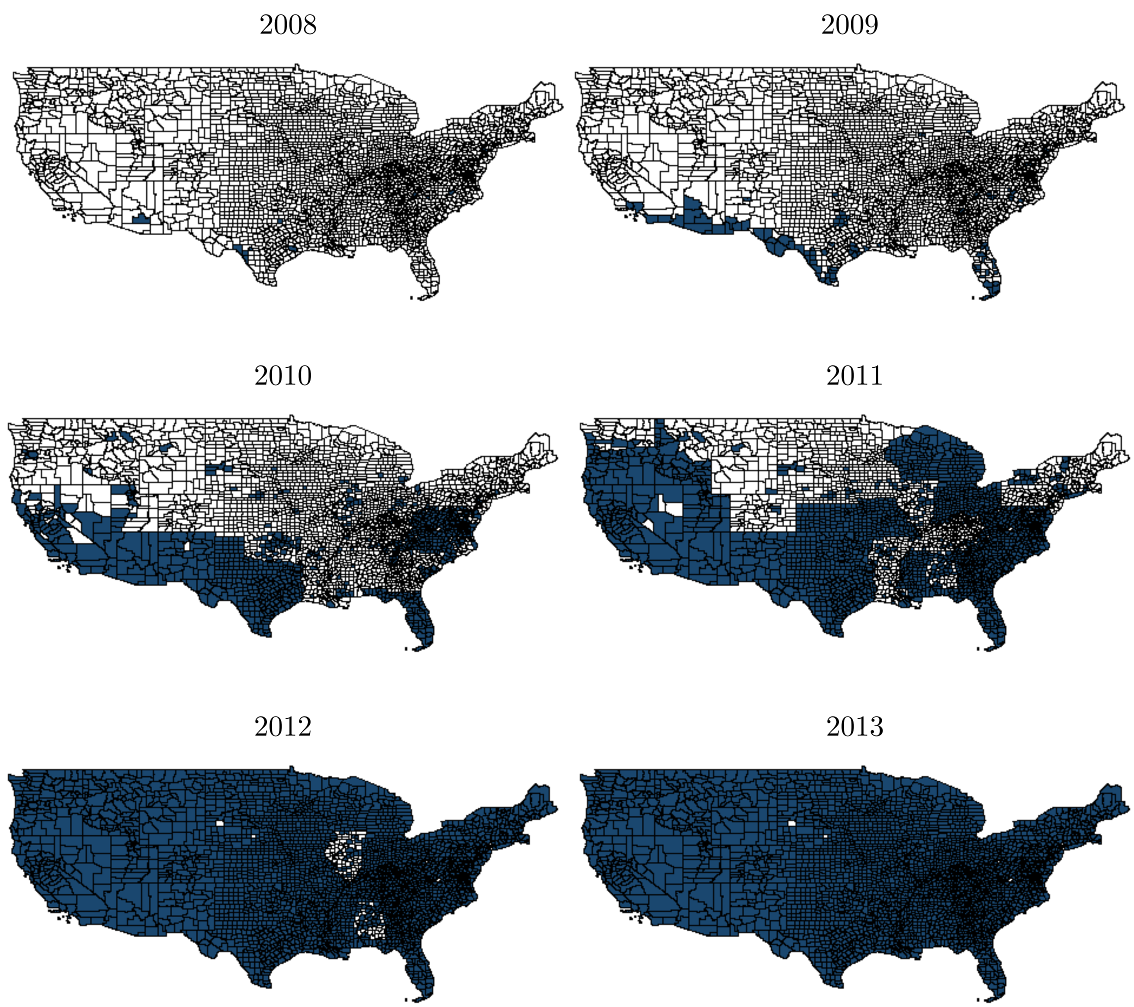
3.7 Figures

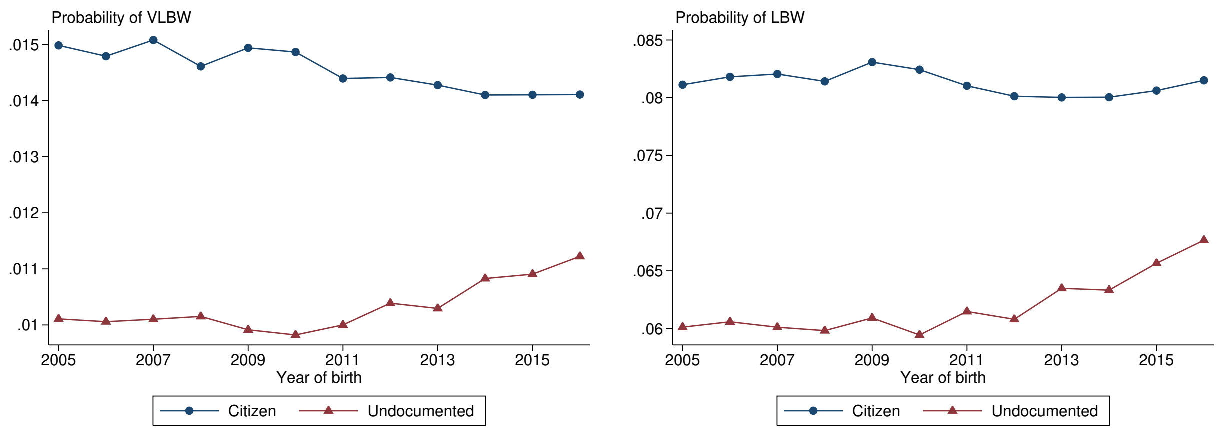
Figure 3.1: Secure Communities Rollout



Notes: Data are from U.S. ICE. Counties that had adopted Secure Communities are shaded.

Figure 3.2: Trends in the Likelihood of VLBW and LBW by Year of Birth

(a) Very low birth weight (b) Low birth weight

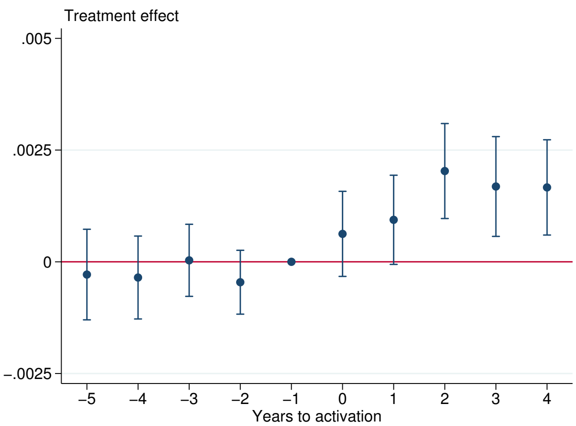


Notes: Author's calculation is from Natality data. See text for further details.

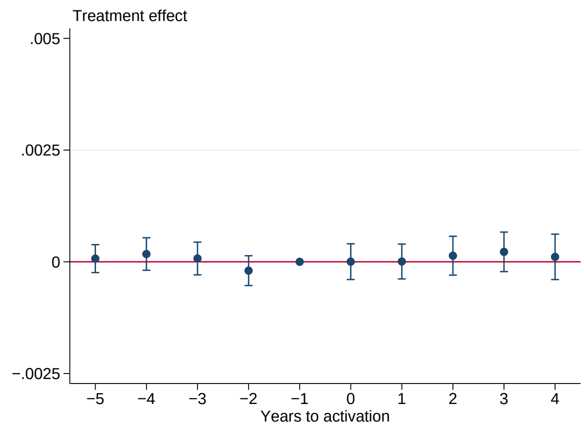
Figure 3.3: Effect of Secure Communities on Birth Outcomes

Panel A. Effects of SC on the likelihood of very low-birth-weight birth

(a) Hispanics

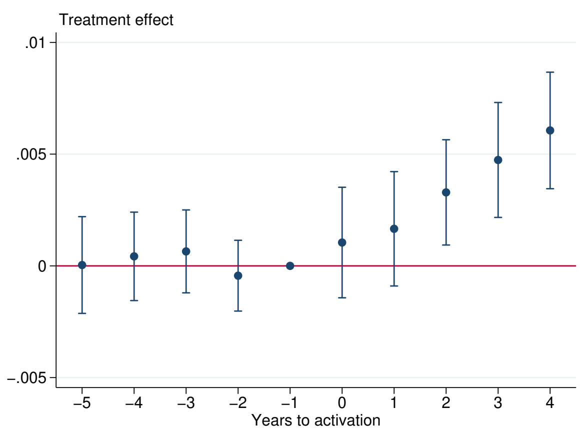


(b) Non-Hispanic whites

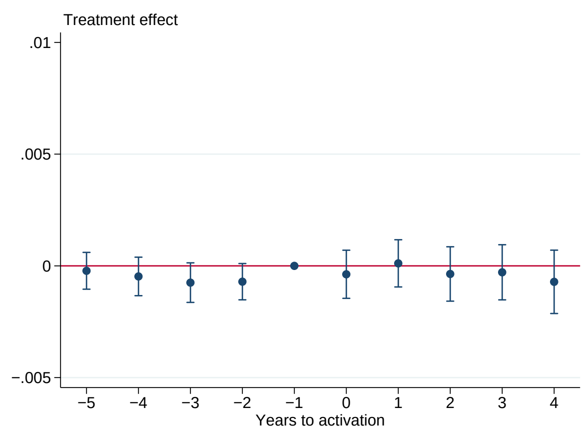


Panel B. Effects of SC on the likelihood of low-birth-weight birth

(c) Hispanics



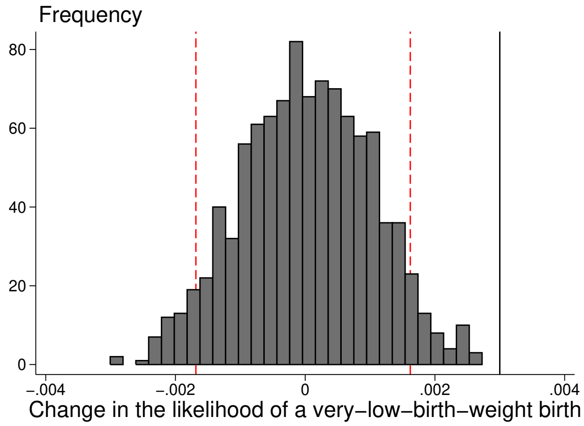
(d) Non-Hispanic whites



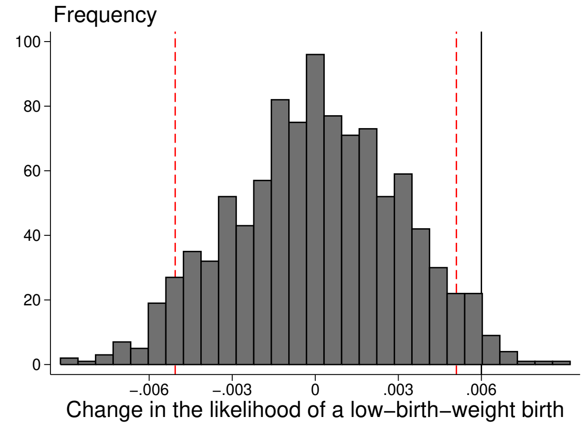
Notes: The coefficients plotted in Figure 3.3(a) and Figure 3.3(c) are triple-differences estimates (β_1) of Equation (4), where the coefficients show SC's effects on birth outcomes of Hispanic infants in the year before and after SC activation relative to non-Hispanic infants. The coefficients plotted in Figure 3.3(a) and Figure 3.3(c) are difference-in-differences estimates for a subsample of infants of non-Hispanic white citizen mothers. Data are from Vital Statistics Natality 2005–2016. All specifications include four dummies for mother's age, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status, a dummy for male birth, and state-level controls: unemployment rate, percentage of population who are Hispanic, black, white, and female ages 15–44. Robust standard errors are clustered at the county level. Whiskers show the 95% confidence interval.

Figure 3.4: Permutation Tests on Effects of SC on Birth Outcomes

(a) Very low birth weight

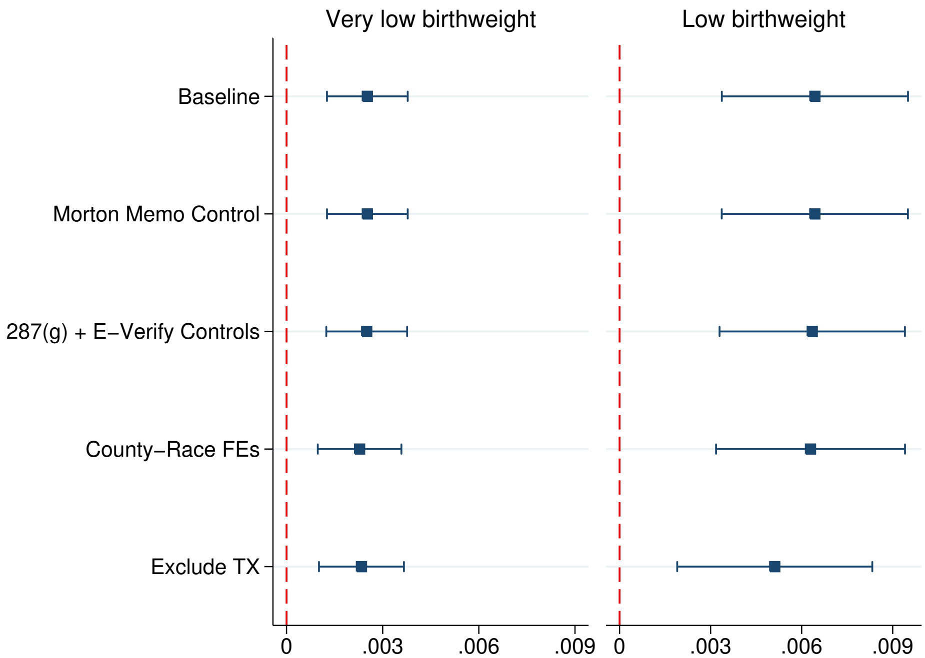


(b) Low birth weight



Notes: These figures shows the histogram of placebo estimates of Equation (3) 1,000 times by randomly assigning six years as "treated," allowing the remaining six years as the pre-period. The vertical solid lines represent my actual triple-differences estimates. The dashed lines are 5th and 95th percentile of the placebo estimates. Data are from Vital Statistics Natality 2005–2016. The sample is limited to infants of all foreign-born mothers with a high school degree or less.

Figure 3.5: Robustness Checks of Secure Communities Effects on Birth Outcomes



Notes: This figure plots coefficient estimates and standard errors for robustness checks discussed in Section 3.5.4. Data are from Vital Statistics Natality 2005–2016. The sample is limited to infants of all foreign-born mothers with a high school degree or less.

3.8 Tables

Table 3.1: Effects of Secure Communities on Birth Outcomes

Outcomes	*Very low bwt* (1)	*Low bwt* (2)	*Premature* (3)	*Birth weight* (4)
SC × Hispanic	0.003***	0.006***	0.005***	-12.061***
	(0.001)	(0.002)	(0.002)	(3.544)
% Impact (coef/mean)	23.47%	10.17%	4.50%	-0.37%
Mean of dep. var.	0.01	0.06	0.12	3,303.30
Observations	2,727,531	2,727,531	2,727,531	2,727,531
Baseline controls	X	X	X	X
Year of birth fixed effects	X	X	X	X
Month of birth fixed effects	X	X	X	X
State fixed effects	X	X	X	X
County fixed effects	X	X	X	X
State × linear time	X	X	X	X

Notes: The table shows estimates of β_1 from Equation (3), a triple-difference model of Hispanic infants compared to non-Hispanic infants, before versus after the SC activation, in treated versus control counties. Data are from Vital Statistics Natality 2005–2016. The sample is limited to infants of foreign-born mothers with less than high school degree. Baseline controls include four dummies for mother's age, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status, a dummy for male birth, unemployment rate at county level, and state-level controls (unemployment rate, percentage of population who are Hispanic, black, white, and female ages 15–44). Robust standard errors clustered at the county level are reported in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 3.2: Effects of Secure Communities on Deportation-Related Search Terms

	Deportation-related search terms		
	(1)	(2)	(3)
Secure Communities	0.590***	0.545***	0.092**
	(0.039)	(0.121)	(0.042)
Mean of dep. var.	4.34	4.34	4.34
Observations	2,000	2,000	2,000
Year fixed effects		X	X
DMA fixed effects			X

Notes: This table presents difference-in-differences estimates of the SC activation on a proxy measure for deportation fear. The dependent variable is the log number of deportation-related search terms relative to the total number of queries at the Nielsen Designated Market Area (DMA) media markets level (see Section 3.3 for more details). Data are from Google Trends 2005–2016. Robust standard errors clustered at the DMA level are reported in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 3.5: Effects of Secure Communities on Migration, Employment, and Household Structure

Outcomes	*% Migrated* (1)	*HH weight* (2)	*% Employed* (3)	*Poverty* (4)	*% Immigrant* (5)
SC × Hispanic	-0.001	-6.498	-0.000***	-0.000***	0.004
	(0.005)	(4.073)	(0.000)	(0.000)	(0.006)
% Impact (coef/mean)	-3.96%	-5.00%	-0.00%	-0.00%	0.44%
Mean of dep. var.	0.03	130.04	0.41	4.38	0.91
Observations	83,007	83,007	83,007	83,007	83,007
Baseline controls	X	X	X	X	X
State by year fixed effects	X	X	X	X	X
State by race fixed effects	X	X	X	X	X
Race by year fixed effects	X	X	X	X	X

Notes: Each parameter is from a separate regression. Data are from the American Community Survey 2005–2016. The sample is limited to non-citizen heads of household with less than a high school degree. Baseline controls include percent employed, log of poverty, number of children in the household, percent immigrants, employment changes during the Great Recession, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, and county fixed effects. All results are estimated using county population weights. Robust standard errors clustered at the county level are reported in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 3.3: Effects of Secure Communities on Birth Outcomes, Intensity of Treatment

Outcomes	*Very low bwt*			*Low bwt*		
	Baseline	Sanctuary counties	High share of NC Hisp.	Baseline	Sanctuary counties	High share of NC Hisp.
	(1)	(2)	(3)	(4)	(5)	(6)
SC × Hispanic	0.003***	0.001	0.004***	0.006***	0.002	0.010***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)
Mean of dep. var.	0.01	0.01	0.01	0.06	0.06	0.06
Observations	2,727,531	906,836	1,139,814	2,727,531	906,836	1,139,814
Baseline controls	X	X	X	X	X	X
Year of birth fixed effects	X	X	X	X	X	X
Month of birth fixed effects	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X
County fixed effects	X	X	X	X	X	X
State × linear time	X	X	X	X	X	X

Notes: This table reports coefficient estimates for heterogeneity of exposure to SC activation discussed in Section 3.5.3. Each parameter is from a separate regression. NC = non-citizen. Data are from Vital Statistics Natality 2005–2016. The sample is limited to infants of foreign-born mothers with less than a high school degree. Baseline controls include four dummies for mother's age, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status, a dummy for male birth, unemployment rate at county level, and state-level controls (unemployment rate, percentage of population who are Hispanic, black, white, and female ages 15–44). Robust standard errors clustered at the county level are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

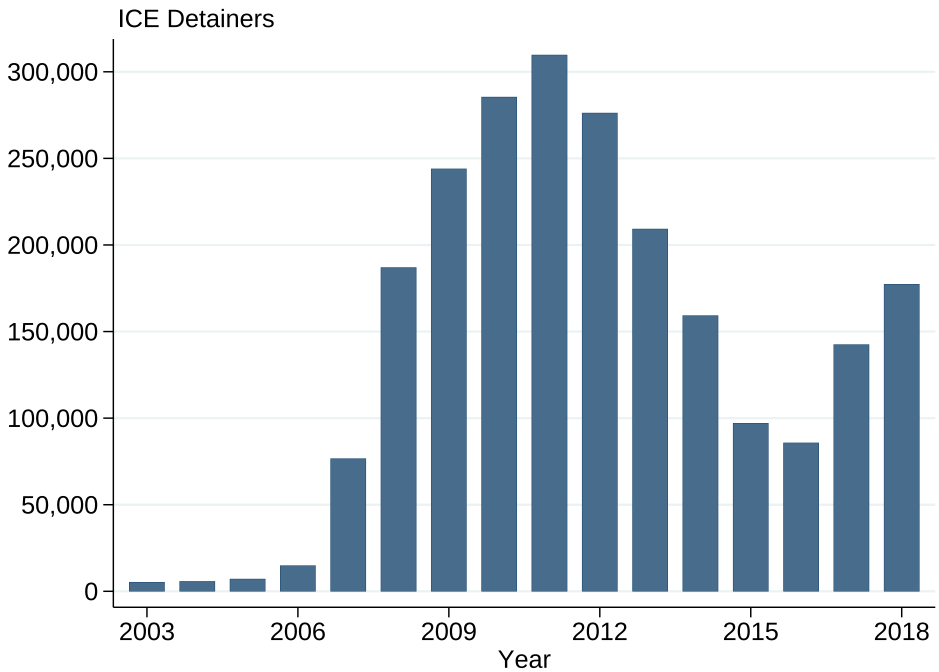
Table 3.4: Effects of Secure Communities on Maternal Behavior and Well-Being

	Number of prenatal visits (1)	*Any prenatal care* (2)	*Take up WIC* (3)	*Gestational hypertension* (4)	*Diabetes* (5)	*Mother smoked during pregnancy* (6)
SC × Hispanic	0.450***	0.009***	-0.005	0.002	0.005**	0.000
	(0.070)	(0.003)	(0.009)	(0.002)	(0.002)	(0.003)
% Impact (coef/mean)	4.41%	0.94%	-0.63%	4.90%	7.83%	0.08%
Mean of dep. var.	10.21	0.97	0.83	0.03	0.06	0.31
Observations	2,636,222	2,727,531	1,432,873	2,220,502	2,220,502	2,727,531
Baseline controls	X	X	X	X	X	X
Year of birth fixed effects	X	X	X	X	X	X
Month of birth fixed effects	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X
County fixed effects	X	X	X	X	X	X
State × linear time	X	X	X	X	X	X

Notes: Each parameter is from a separate regression. Data are from Vital Statistics Natality 2005–2016. The sample is limited to infants of foreign-born mothers with less than a high school degree. Baseline controls include four dummies for mother's age, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status, a dummy for male birth, unemployment rate at county level, and state-level controls (unemployment rate, percentage of population who are Hispanic, black, white, and female ages 15–44). Robust standard errors clustered at the county level are reported in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

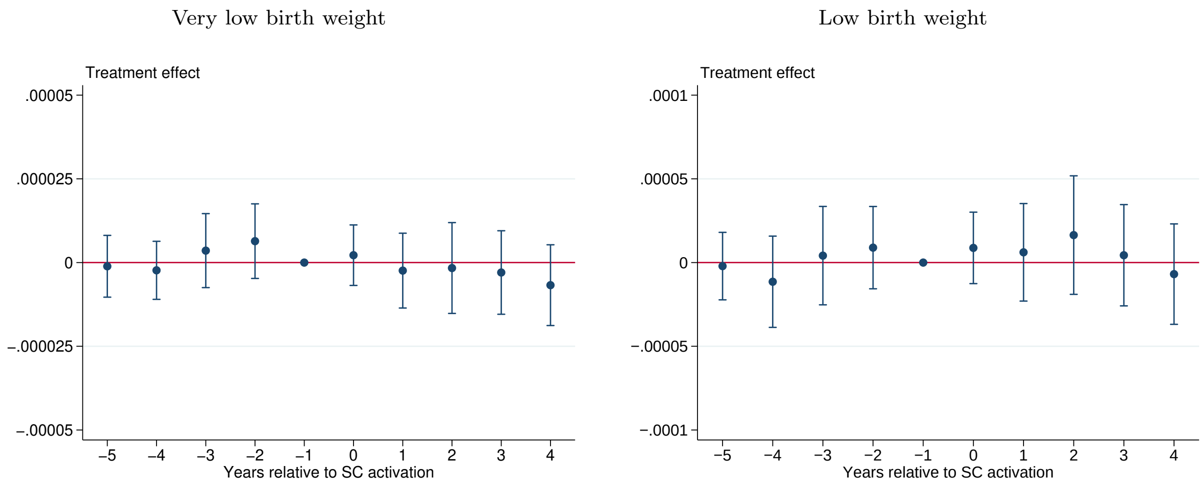
A Appendix: Supplementary Figures and Tables

Figure A.4: Number of Detainers by Year



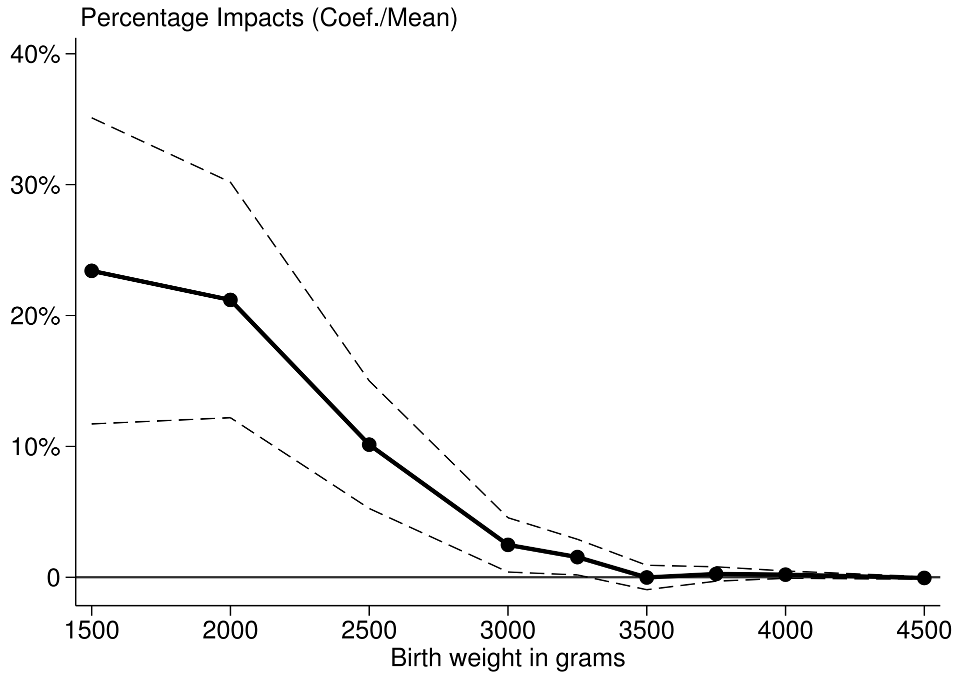
Notes: Data are from TRAC Immigration 2003–2018.

Figure A.5: Effects of Secure Communities on Predicted Birth Outcomes



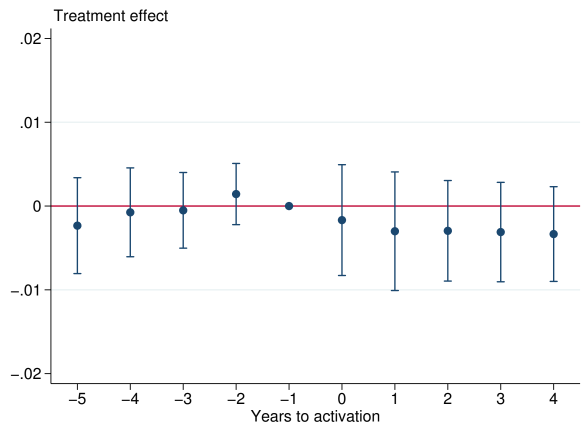
Notes: The coefficients plotted above are triple-difference estimates of Equation (4), where the coefficients show SC's effects on birth outcomes of Hispanic infants in the year before and after SC activation relative to non-Hispanic infants. The outcomes are the fitted values of likelihood of low-birth-weight and very low-birth-weight birth, obtained from regressions of the birth outcomes on a set of characteristics including gender, year, month, week of birth, indicators for maternal age dummies, indicator for mother being married, maternal race dummies, and maternal education dummies using pre-period data. Data are from Vital Statistics Natality 2005–2016. The sample is limited to infants of foreign-born mothers with a high school degree or less.

Figure A.6: Effects of Secure Communities on Birth Weight Distribution



Notes: This figure shows estimates and 95% confidence intervals for the estimate of the effects of immigration enforcement exposure on the fraction of births that is below each specified number of grams. Data are from Vital Statistics Natality 2005–2016. The sample is limited to infants of foreign-born mothers with a high school degree or less. All specifications include four dummies for mother's age, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status, a dummy for male birth, and state-level controls (unemployment rate, percentage of population who are Hispanic, black, white, and female ages 15–44). Robust standard errors are clustered at the county level.

Figure A.7: Effects of Secure Communities on a Placebo Outcome: Whether an Infant Was Born on Odd Days

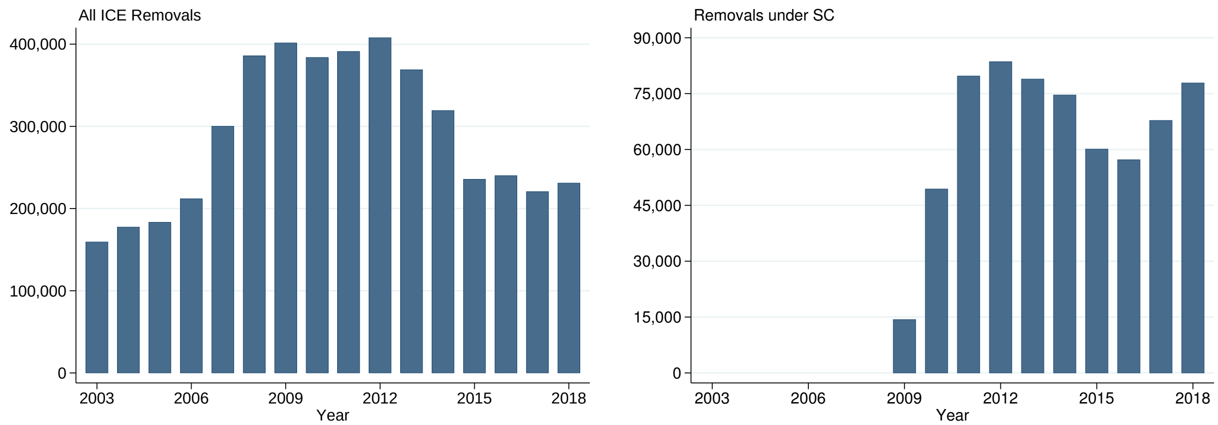


Notes: This figure shows event study estimates where outcome is whether an infant was born on "odd days." Data are from Vital Statistics Natality 2005–2016. The sample is limited to infants of foreign-born mothers with a high school degree or less. All specifications include four dummies for mother's age, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status, a dummy for male birth, and state-level controls (unemployment rate, percentage of population who are Hispanic, black, white, and female ages 15–44). Robust standard errors are clustered at the county level. Whiskers show the 95% confidence interval.

Figure A.8: Number of Removals by Year

(a) All ICE removals

Panel B: Removals under SC



Notes: Data are from TRAC Immigration 2003–2018.

Table A.19: Effects of Secure Communities on Fertility

Outcomes	*Fertility rate* (1)	*Birth rate* (2)	*Male birth* (3)
SC × Hispanic	-0.708***	-0.138***	-0.000
	(0.129)	(0.025)	(0.000)
Mean of dep. var.	7.29	1.54	0.51
Observations	487,024	487,048	487,048
Baseline controls	X	X	X
Year of birth fixed effects	X	X	X
Month of birth fixed effects	X	X	X
State fixed effects	X	X	X
County fixed effects	X	X	X
State × linear time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variable: fertility rate, birth rate, and mean of male birth by county-race-month-year. Fertility rate is defined as number of births per 1,000 women ages 15–44. Birth rate is defined as number of births per 1,000 population. Note that these are monthly rates, so to compare to published statistics, one would have to multiply by 12. Data are from Vital Statistics Natality and SEER 2005–2016. Baseline controls include four dummies for mother's age, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status, a dummy for male birth, and state-level controls (unemployment rate, percentage of population who are Hispanic, black, white, and female ages 15–44). Robust standard errors clustered at the county level are reported in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.20: Effects of Secure Communities on Birth Outcomes, Robustness to Donut-DDD Estimates

	Excluding ± 1 month		Excluding ± 2 months		Excluding ± 3 months	
	VLBW (1)	LBW (2)	VLBW (3)	LBW (4)	VLBW (5)	LBW (6)
SC × Hispanic	0.002*** (0.001)	0.006*** (0.002)	0.002*** (0.001)	0.006*** (0.002)	0.003*** (0.001)	0.007*** (0.002)
% Impact (coef/mean)	23.15%	10.18%	21.69%	10.06%	23.38%	10.77%
Mean of dep. var.	0.01	0.06	0.01	0.06	0.01	0.06
Observations	2,673,265	2,673,265	2,637,438	2,637,438	2,601,131	2,601,131
Baseline controls	X	X	X	X	X	X
Year of birth fixed effects	X	X	X	X	X	X
Month of birth fixed effects	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X
County fixed effects	X	X	X	X	X	X
State × linear time	X	X	X	X	X	X

Notes: This table show the robustness of results to excluding infants whose birth dates are within ± 1 month up to ± 3 months of the SC activation date. Data are from Vital Statistics Natality 2005–2016. The sample is limited to infants of foreign-born mothers with less than a high school degree. Baseline controls include four dummies for mother's age, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status, a dummy for male birth, and state-level controls (unemployment rate, percentage of population who are Hispanic, black, white, and female ages 15–44). Robust standard errors clustered at the county level are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.21: Effects of Secure Communities on Birth Outcomes Index

Outcome	*Birth outcome index*		
	Baseline	High share of NC Hisp.	Sanctuary counties
	(1)	(2)	(3)
SC × Hispanic	-0.012***	-0.023***	-0.004
	(0.004)	(0.008)	(0.007)
Observations	2,727,531	1,139,814	906,836
Baseline controls	X	X	X
Year of birth fixed effects	X	X	X
Month of birth fixed effects	X	X	X
State fixed effects	X	X	X
County fixed effects	X	X	X
State × linear time	X	X	X

Notes: Data are from Vital Statistics Natality 2005–2016. The birth outcomes index includes the following measures: VLBW (< 1,500 grams), low birth weight (< 2,500 grams), premature birth (< 37 weeks of gestation), continuous birth weight in grams, gestation in weeks, very premature birth (< 34 weeks of gestation), low 1-minute Apgar score (<7), NICU admission, any abnormal conditions (six indicators: assisted ventilation, assisted ventilation > 6 hours, admission to NICU, surfactant, antibiotics, and seizures). The sample is limited to infants of foreign-born mothers with less than a high school degree. Baseline controls include four dummies for mother's age, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status, a dummy for male birth, and state-level controls (unemployment rate, percentage of population who are Hispanic, black, white, and female ages 15–44). Robust standard errors clustered at the county level are reported in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

B Appendix: Conceptual Framework

How might in utero exposure to immigration enforcement affect infant health? In this paper, I focus on in utero exposure to Secure Communities (SC), which is one of the largest deportation programs in U.S. history. SC might affect infants through two possible channels: (i) directly through maternal health endowment and (ii) indirectly through the effects of maternal health on prenatal input use. To formalize how SC may have impacted Hispanic infants, I present a simple framework following Corman et al. (1987).[36] Let an infant's health stock be a function of prenatal inputs[37] and the health endowment of the mother: $h = h(I_i, e)$, where I_i is input i and i = 1,...,n and e is maternal health endowment. For simplicity, I assume that there are only two inputs: a positive input (prenatal care c) and a negative input (smoking s).[38] Thus, the infant health function can be expressed as follows:

$$h = h(c, s, e), \tag{5}$$

where

$$c = c(p, y, e), \tag{6a}$$

$$s = s(p, y, e). \tag{6b}$$

Equations (6a) and (6b) are input demand functions. The demand for each input depends on (i) price and availability of that input and prices and availability of substitute and complementary inputs (p), (ii) resources and tastes of parents (y), and (iii) the endowment (e). I am

[36]However, I abstract away from modeling parental utility maximization problem subject to consumption goods, infant health, parents' health, and tastes. I instead focus on the reduced-form relationship between tougher immigration enforcement and infant health because this is what I can measure in my data.

[37]Prenatal inputs can be positive such as prenatal care visits or negative such as smoking, drinking, or drug use (Reichman et al., 2009).

[38]One can think of prenatal care as an index representing positive inputs and smoking as an index representing negative inputs.

interested in the impact of a change to immigration enforcement on infant health. Assume that immigration enforcement enters the infant health function as an exogenous shock x that affects maternal health e, specifically $e = e(x)$. Thus, I rewrite Equation (5) as follows:

$$h = h(c(e), s(e), e(x)). \tag{7}$$

I then calculate the impact of changes to immigration enforcement x on infant health:

$$\frac{\partial h}{\partial x} = \frac{\partial h}{\partial c} \times \frac{\partial c}{\partial e} \times \frac{\partial e}{\partial x} + \frac{\partial h}{\partial s} \times \frac{\partial s}{\partial e} \times \frac{\partial e}{\partial x} + \frac{\partial h}{\partial e} \times \frac{\partial e}{\partial x}. \tag{8}$$

In sum, tougher immigration enforcement, x, affects infants' health through two channels: a direct effect of the shock on maternal health endowment ($\partial h/\partial e \times \partial e/\partial x$) and an indirect effect through the effects of maternal health on prenatal inputs use ($\partial h/\partial c \times \partial c/\partial e \times \partial e/\partial x + \partial h/\partial s \times \partial s/\partial e \times \partial e/\partial x$).

The goal of the rest of the paper is to deliver estimates of ($\partial h/\partial x$), where the change immigration enforcement stems from changes in SC activation. I also discuss mechanisms that help distinguish between direct effects and indirect effects of SC. The details of the research design and empirical strategy are described more fully in the paper.