I Wish I Were Born in Another Time: Unintended Consequences of Immigration Enforcement on Birth Outcomes*

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Abstract

This paper studies 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.

Keywords: Secure Communities, immigration enforcement, infant health.

JEL Classification: I10, I12, I14, I18, K00, K37

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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 (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 out-

[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).

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

comes, 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 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.

[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.

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

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.

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 2. Section 3 and Section 4 discuss data and empirical framework. I discuss results on birth outcomes, placebo tests, mechanisms, and robustness checks in Section 5, and Section 6 concludes.

2 Background and Literature

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

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

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.[10]

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 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

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

[10]See DHS Community Resilience Task Force Report, Watson (2014), Padraza and Zhu (2014), and Vargas and Pirog (2016).

on adverse consequences of an immigration raid on infant health.

Tome et al. (2019) explore the effect of Section 287(g)[11] 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 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 monthly number of detainers, or "immigration holds"[12], and the monthly number of removals under SC from ICE public records and Transactional Records Access Clearinghouse (TRAC) Immigration.[13] Figure 1 shows the rollout of SC across counties in the U.S. The figure shows

[11]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.

[12]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.

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

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.1 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 actual search requests made to Google.[14] 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

[14] 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.

$G(i,d)$ 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)}.[15]

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

[15]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.

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.[16] 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.[17] 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 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

[16]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.

[17]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).

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:

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

where $\mathbf{1}[r = t]$ 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).

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).[18] 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 4 shows the histogram of placebo estimates along with vertical solid lines representing my actual triple-differences 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

[18] 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).

the estimated coefficients to predict birth outcomes for each infant in the sample.[19] Figure A.2 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(a) and 3(c), the coefficients are insignificant and show no trend breaks in the predicted birth outcomes.

5 Results

5.1 Effects on Birth Outcomes

I first examine the effects of SC on birth outcomes. Figures 3(a) and 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(a) and 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 non-Hispanic infants.

Table 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

[19] 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.

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 1). However, much of this effect is driven by impacts at births that are already at risk or more vulnerable.[20] 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.

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(b) and 3(d) correspond to difference-in-differences estimates for a subsample of infants of white citizen mothers, before versus after SC activation, between treatment and control counties.[21] Figures 3(b) and 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.

[20]Figure A.3 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.

[21]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).

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.[22] Figure A.4 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.

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, 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

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

terms (see Section 3 for more details). Table 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.[23] 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).

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 presents the coefficients of ($SC \times HISP$) in the main specification (Equation 3) for counties with a high share of Hispanic non-citizens.[24] Given my proposed channel, I expect β_1 to be increasing in magnitude 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).

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

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

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

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 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.[25]

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 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)

[25] 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.

take-up context where the program asks about applicants' immigration status.[26] 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 5.[27] 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.

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 2, the upward trends in the likelihood of VLBW and LBW for Hispanic infants happened after 2011, a year after the recession ended.[28] Third, I only find the effects on birth outcomes among infants of likely undocumented mothers and no effects on non-Hispanic whites (Figure 3) who were unaffected by the SC activation by design.

Effects on fertility: One might have the concern that immigration enforcement may lead

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

[27] 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.

[28] 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.

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 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.1. 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.[29] 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 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.[30] Second, 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).

[29] 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).

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

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}.[31] 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.[32] 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.2.[33] 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 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 hypethesis 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

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

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

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

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.[34] Table A.3 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.

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 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.

[34] 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.

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[35] VLBW infants born to undocumented mothers.[36]

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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[35]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).

[36]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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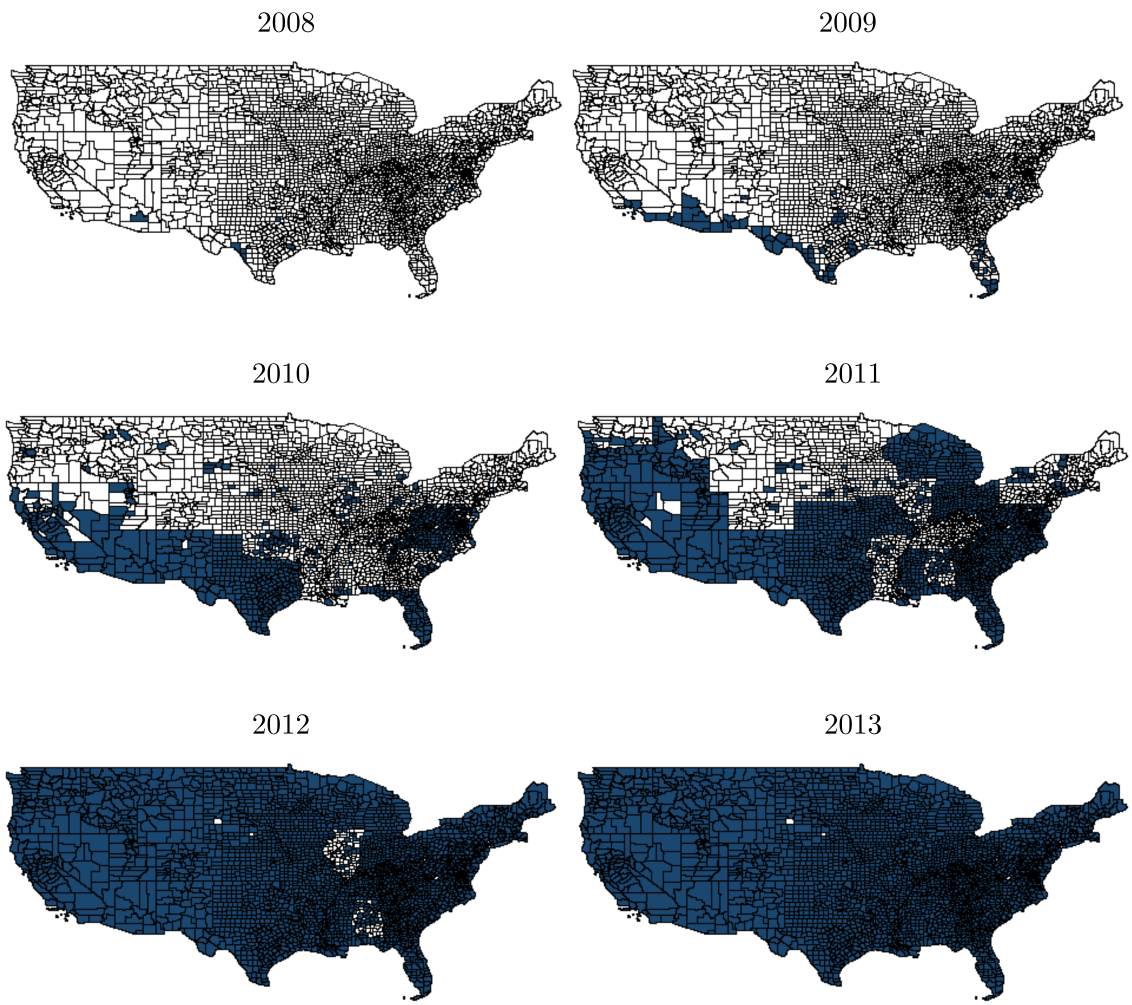
7 Figures

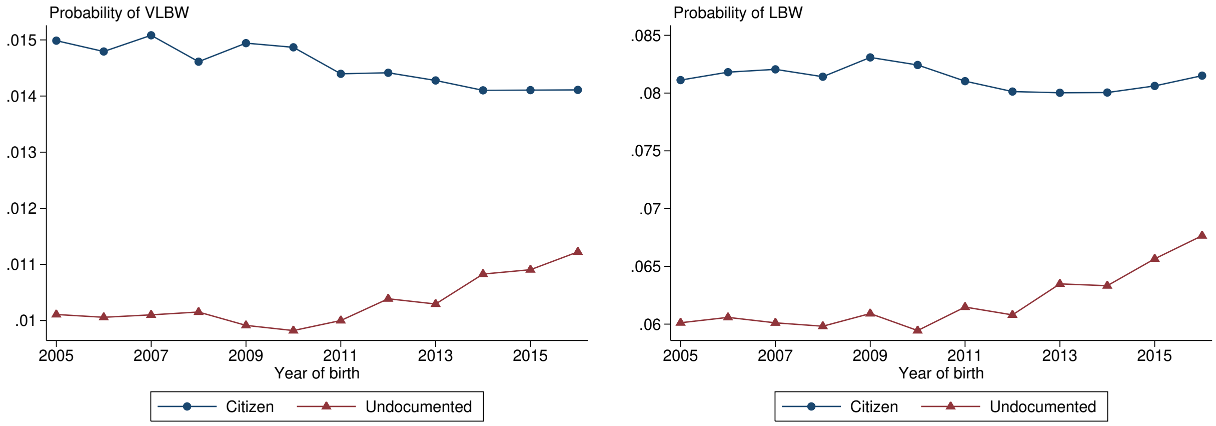
Figure 1: Secure Communities Rollout



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

Figure 2: Trends in the Likelihood of VLBW and LBW by Year of Birth

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

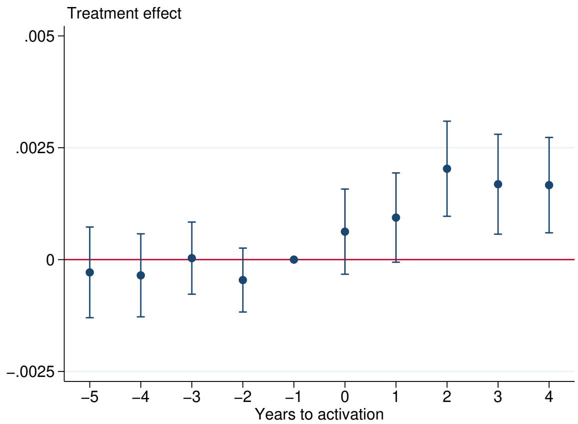


Notes: Author's calculation is from Natality data. See text for further details.

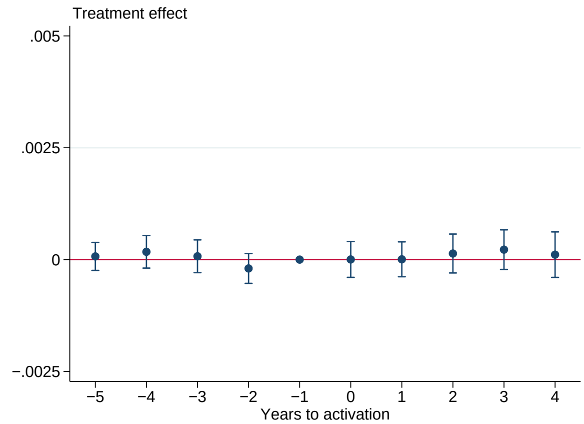
Figure 3: Effect of Secure Communities on Birth Outcomes

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

(a) Hispanics

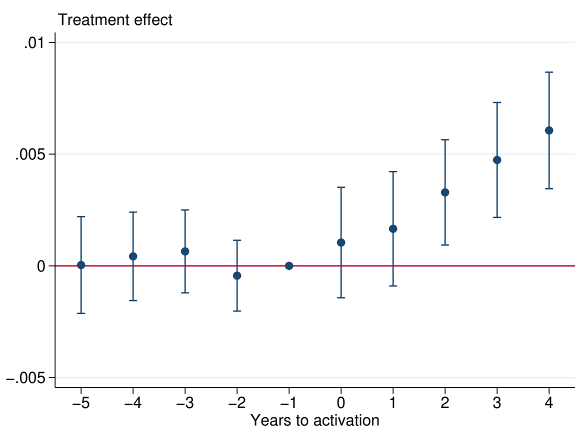


(b) Non-Hispanic whites

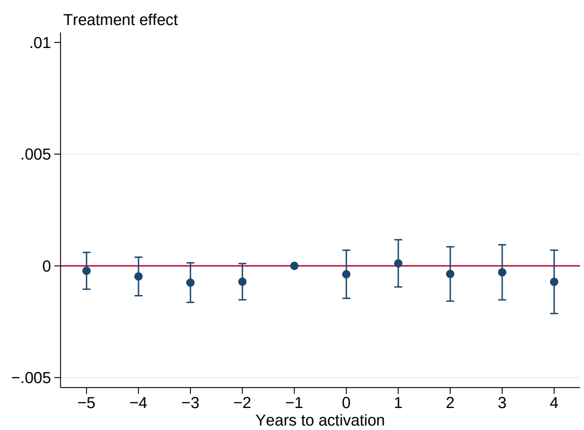


Panel B. Effects of SC on the likelihood of low-birth-weight birth

(c) Hispanics



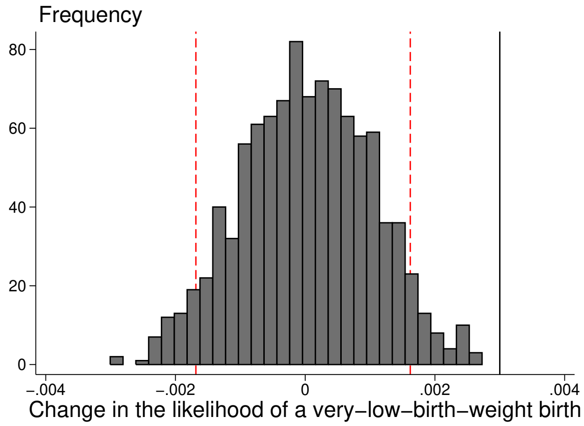
(d) Non-Hispanic whites



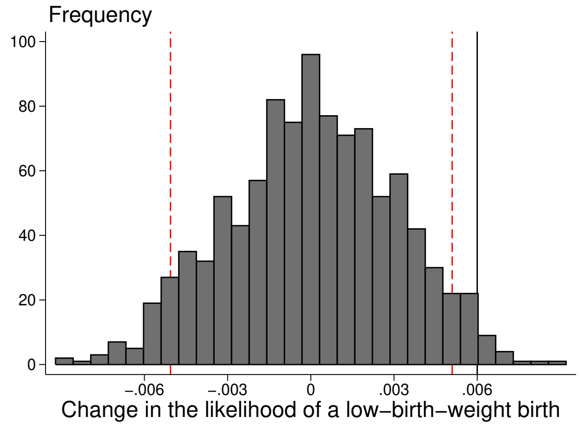
Notes: The coefficients plotted in Figure 3(a) and Figure 3(c) are triple-differences estimates (β_1) of Equation (4), where the coefficients show SC's effects on birth outcomes of Hispanic infants in the year before and after SC activation relative to non-Hispanic infants. The coefficients plotted in Figure 3(a) and Figure 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 4: Permutation Tests on Effects of SC on Birth Outcomes

(a) Very low birth weight

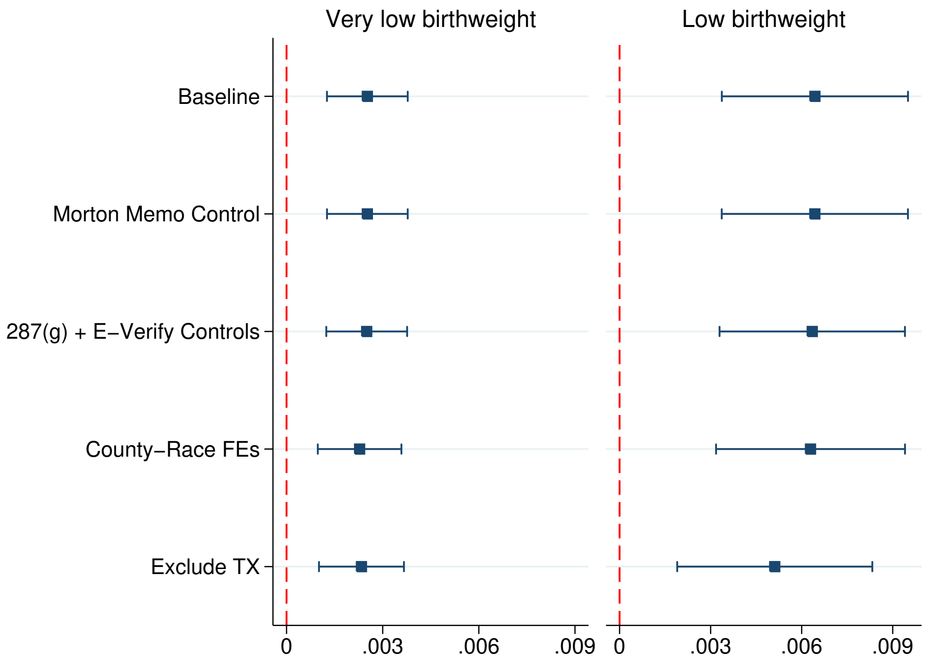


(b) Low birth weight



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 estimate. 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 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 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.

8 Tables

Table 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 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 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: 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 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 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. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

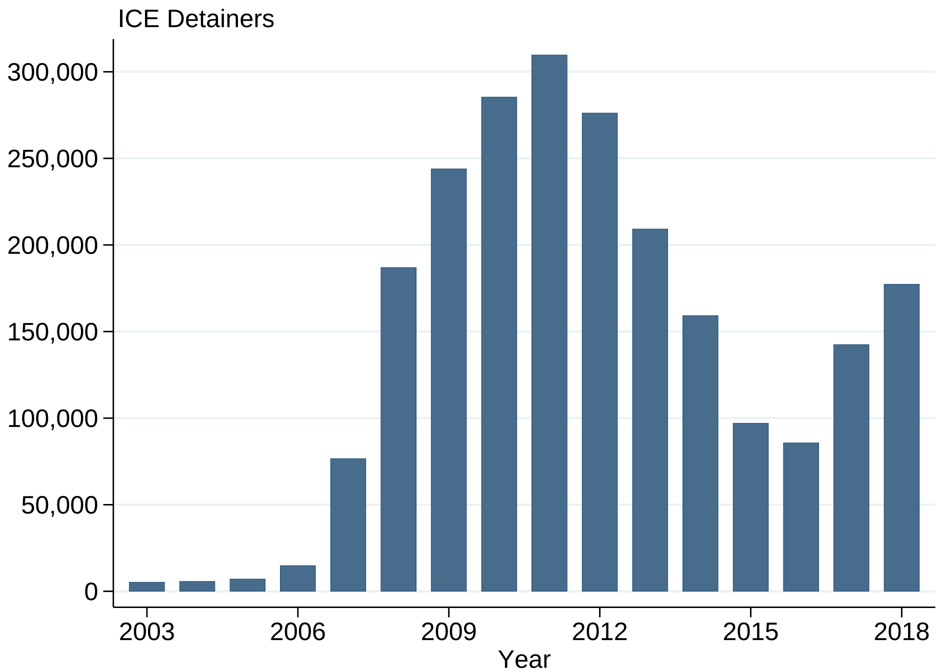
Table 5: Effects of Secure Communities on Migration, Employment, and Household Structure

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

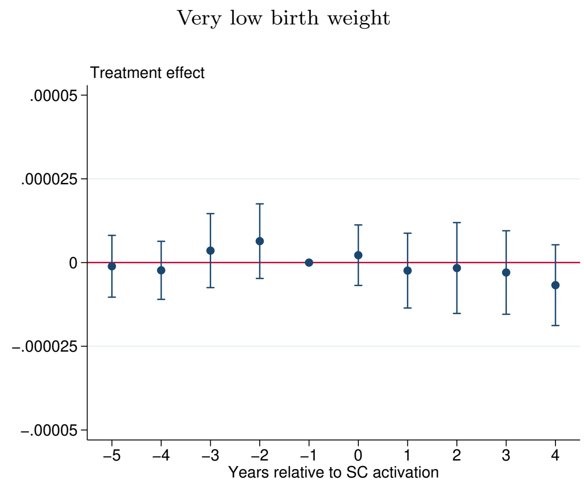
A Appendix: Supplementary Figures and Tables

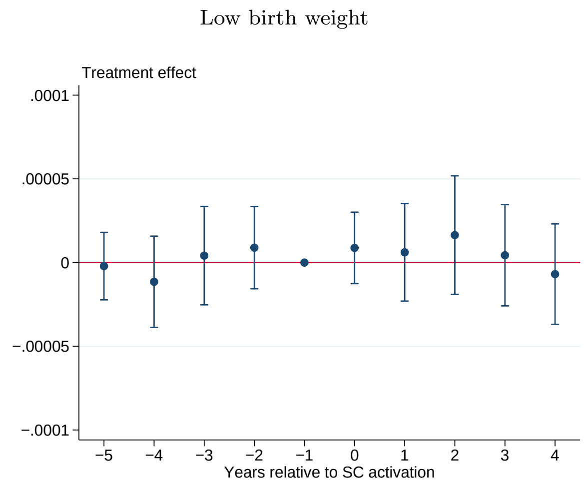
Figure A.1: Number of Detainers by Year



Notes: Data are from TRAC Immigration 2003–2018.

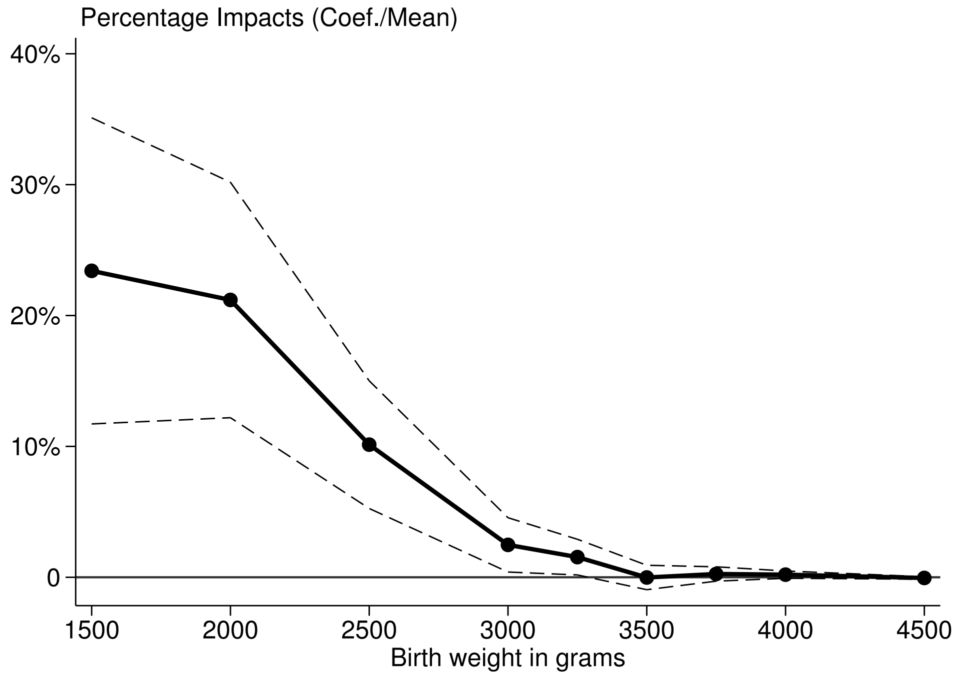
Figure A.2: Effects of Secure Communities on Predicted Birth Outcomes





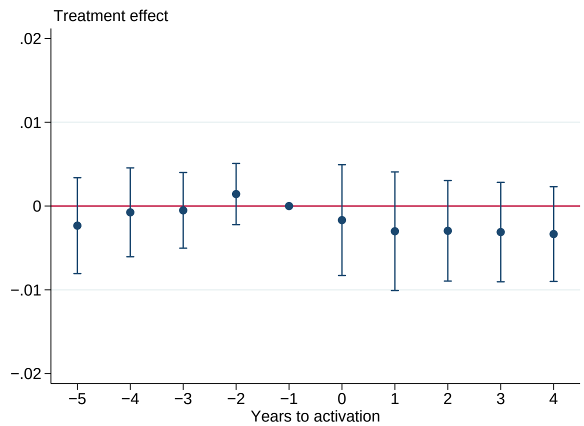
Notes: The coefficients plotted above are triple-difference estimates of Equation (4), where the coefficients show SC's effects on birth outcomes of Hispanic infants in the year before and after SC activation relative to non-Hispanic infants. The outcomes are the fitted values of likelihood of low-birth-weight 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.3: 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.4: Effects of Secure Communities on a Placebo Outcome: Whether an Infant Was Born on Odd Days

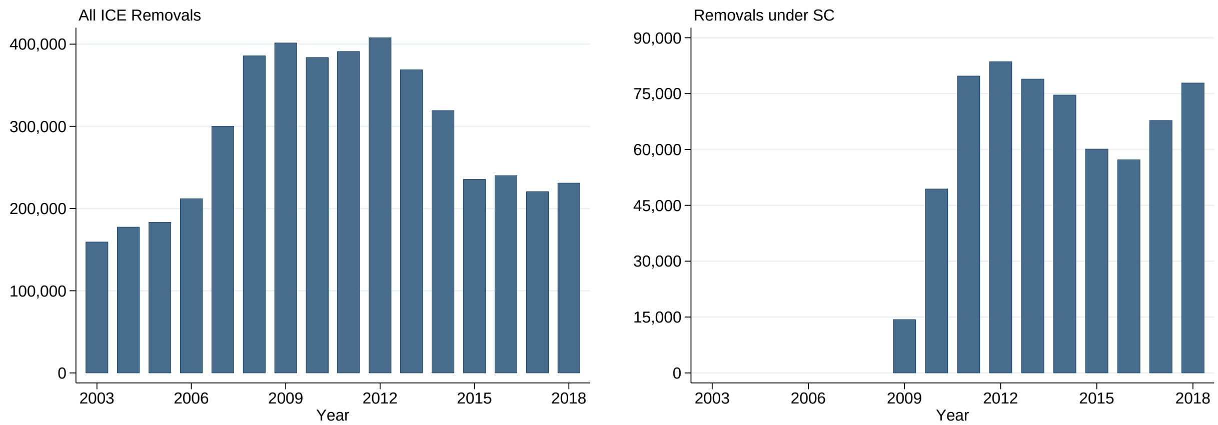


Notes: This figure shows event study estimates where outcome is whether an infant was born on "odd days." Data are from Vital Statistics Natality 2005–2016. The sample is limited to infants of foreign-born mothers with a high school degree or less. All specifications include four dummies for mother's 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.5: Number of Removals by Year

(a) All ICE removals

Panel B: Removals under SC



Notes: Data are from TRAC Immigration 2003–2018.

Table A.1: 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.2: 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***	0.006***	0.002***	0.006***	0.003***	0.007***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(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.3: 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).[37] Let an infant's health stock be a function of prenatal inputs[38] 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).[39] 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

[37]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.

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

[39]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.