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-differences designs, I find that SC increased the incidence of very low birthweight by 23% for children of foreign-born Hispanic mothers, who were most likely to be affected by the immigration enforcement. There is suggestive evidence that the results are consistent with changes in maternal stress induced by deportation fear. I find no evidence of differential pre-treatment trends in birth outcomes and no effects among placebo groups.

Keywords: Secure Communities, immigration enforcement, infant health, health disparities.

JEL Classification: I10, I12, I14, I18, J13, J88, K00, K37

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1 Introduction

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

Growing evidence indicates that immigration enforcement adversely affects immigrant families. Specifically, 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). The heightened maternal stress and anxiety during childbirth then adversely affect fetal development (Mulder et al., 2002; Camacho, 2008; Kinsella and Monk, 2009). Thus, I hypothesize that stressors from immigration enforcement not only affect the health of foreign-born Hispanic mothers but also their US-born children.

In this paper, I evaluate the health consequences of the most restricted national immigration policy in the U.S. In particular, I examine the impact of the Secure Communities Program (SC), which started in 2008 through 2014, and led to the deportation of nearly 450,000 immigrants[3], on birth outcomes of US-born Hispanic children.

Ideally, I would like to randomize US-born babies of undocumented mothers to counties

[1] Source: National Vital Statistics Reports, Vol. 67, No. 8: Births: Final Data for 2017

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

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

with and without immigration enforcement and then measure the effects on their birth outcomes. Such an experiment is impossible and unethical to implement in practice, so I turn to the next best option: exploiting quasi-experimental staggered roll out of SC across counties due to various technological constraints.

To illuminate the potential unintended consequences of SC on Hispanic newborns' birth outcomes, I collected the data on the SC activation date at the county level and merged these data to administrative birth certificate data 2005-2016. This allows me to estimate a triple differences model comparing birth outcomes of Hispanic newborns within a county to birth outcomes of non-Hispanic newborns, net of counties that had not yet activated, before versus after SC activation.

I show that SC has adverse consequences on the incidence of very low birthweight (VLBW, less than 1,500 grams) and low birthweight (LBW, less than 2,500 grams) of Hispanic babies. On relative conservative estimates, babies of likely undocumented mothers[4] have 23% higher in the likelihood of a VLBW birth and 10% higher in the likelihood of a LBW birth, compared to non-Hispanic babies.

I provide two main robustness checks for my identification strategy: placebo tests. First, I reproduce the analysis, but instead of focusing on babies 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: babies of non-Hispanic white citizens.[5] I find no effects on this population: all estimated coefficients are indistinguishable from zero and statistically insignificant.

The second test consists of another placebo analysis that involves estimating the same

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

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

regressions for a placebo characteristic, whether a baby was born on an "odd days", which should not be effected by heightened immigration enforcement.[6] The event study analysis indicates that the chance of babies being born on "odd days" was similar between Hispanics and non-Hispanics.

There are many possible ways for exposure to immigration enforcement to affect birth outcomes. In this paper, I have ruled out some important potential channels including changes to migration and engagement in adverse maternal behavior such as smoking. I provide three pieces of evidence in favor of the stress due to fear induced by immigration enforcement mechanism. First, using Google Trends data, I show that the SC activation is associated with an increase in deportation-related search terms as a proxy for deportation fear. Second, I use data on a list of sanctuary counties to show that the adverse effect of SC on birth outcomes is weaker in the sanctuary counties. Third, I show that a higher concentration of likely undocumented in a county increases the magnitude of the effects on birth outcomes.

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

This paper advances the above literature in two ways. First, to my knowledge, no papers have examined the spillover impact of SC on birth outcomes of US-born children, I provide the first causal evidence on the effects of immigration enforcement on birth outcomes of

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

US-born Hispanic children. Second, I provide evidence about a possible mechanism that SC could affect the newborns' health: maternal stress due to deportation fear. Given that SC was reactivated in 2017, knowing the spillover impact of local immigrant enforcement on future citizens health would allow policy makers to make more informed decisions or design and craft different types of policies.

This study is related to the literature seeking to understand 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.

This study also adds to a large literature on both short-term and long-term effects of fetal stress exposure on birth outcomes and adult outcomes, recently reviewed by Almond and Currie (2011). The stressors can come from impacts on (1) 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 (Sanders, 2012; Isen et al., 2017), diseases (Almond, 2006; Barreca, 2010), famine (Almond et al., 2010b; Scholte et al., 2015), Chernobyl disaster (Almond et al., 2009); or (2) 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), natural disasters (Simeonova, 2011), hurricanes (Currie and Rossin-Slater, 2013), earthquakes (Torche, 2011; Tan et al., 2009). I add to this literature by providing a 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 literature and background of SC in Section 2. Section 3 and Section 4 discuss data and empirical framework. I discuss results on birth outcomes, a placebo test, mechanisms, and robustness checks in Section 5, and conclude in Section 6.

2 Background and Literature

2.1 Immigration Enforcement and Birth Outcomes

The existing evidence on the birth outcome effects of fetal stress exposure to immigration enforcement is extremely limited. I am aware of only two studies that have examined the impacts of immigration enforcement policies on birth outcomes of US-born children. Each is a case study of a particular county or city policy. No previous studies have examined the effects 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 federal immigration raid in Postville, Iowa, in 2008 on birth outcomes. Using a modified Poisson regression, the authors found that the Postville raid increased the risk of low-birth-weight of infants born to Hispanic mothers by 24% compared with the same period one year earlier.

Tome et al. (2019) explored the effect of section 287(g) of the Immigration and Nationality Act on birth outcomes in Mecklenburg County, North Carolina. The authors used long-form birth certificate data from the North Carolina Detailed Birth Records and undertook two identification strategies: difference-in-differences and triple-differences case-control regression analysis. They found that 287(g) was associated with a 3.5 percentage point increase in the incidence of low-birth-weight babies.

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 babies of Hispanic immigrant mothers have higher incidences of VLBW and LBW birth relative to other immigrant groups in the face of immigration enforcement. I report evidence that shows deportation fear as a likely explanatory mechanism.

2.2 Policy Background

Secure Communities (SC) is one of the largest deportation program in the US history.[7] The program was started in October 2008 and was temporarily suspended in October 2014, but was reactivated in January 2017 under President Trump administration. The SC relies on partnership between U.S. Immigration and Customs Enforcement (ICE), Federal Bureau of Investigation (FBI), and local law enforcement agencies to build deportation capacity. The program objective is to help ICE arrest and remove individuals who violate federal immigration laws, including those who had convicted of serious criminal offenses.

Ordinarily, the fingerprints of county and state arrestees are submitted to the FBI only. Under Secure Communities, the prints go to ICE too. Because of fingerprint matching databases, it all of a sudden became much easier to figure whether an arrested individual was, for instance, unlawfully present in the country. From 2008 through 2014, ICE has deported over 450,000 immigrants under the SC.

Technically, any arrested non-citizens could be subject to deportation (including legal immigrants and green card holders). For undocumented immigrants, even minor offenses can trigger deportations. Indeed, nearly half of deportees under SC had only minor offenses (such as public drunkenness or jaywalking) or no offense at all.[8] This has been argued to increase fear and decrease participation in public benefit programs.[9] I ask whether these impacts of the SC program have any consequences for the US-born Hispanic children.

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

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

[9]See Watson (2014), Padraza and Zhu (2014), Vargas and Pirog (2016), and https://www.dhs.gov/xlibrary/assets/hsac-community-resilience-task-force-recommendations-072011.pdf

3 Data

In this section, I describe the data I use to measure birth outcomes and deportation fear as well as information about the activation of SC.

SC roll-out data: I have obtained information about the rollout dates of SC as well as monthly number of detainers or "immigration holds"[10], monthly number of removals under SC from the US Immigration and Customs Enforcement (ICE) public records and the Transactional Records Access Clearinghouse (TRAC) Immigration.[11] Figure 1 shows the rollout of SC across counties in the US. As observed, there are crucial variations in the SC activation, both across counties and through time, which I will exploit in identifying the effects of SC on birth outcomes.

One relevant question is whether SC indeed increased 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 2009-2010. This serves as evidence that SC has a large impact on 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 is the universe of birth records in the US. Data on the month, year, and county of birth allow me to link the birth data to SC activation date in a given county. The data include infant's characteristics such as birth weight, gender, plurality, and gestational length. There are also demographic variables, including age, race, education, marital status, and birth place 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. Unfor-

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

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

tunately, I cannot construct this ideal fear level variable, because my data does not contain information such as whether a respondent feels insecure and fear because of immigration enforcement. Instead, I use commonly searched terms related to the deportation topic on Google Trends to proxy for deportation fear.[12] Google Trends is a public-use database that provides access to an unfiltered sample of actual search requests made to Google.[13] For each search term i in US media market d (according to the Nielsen DMA definition.), the Google Trends returns the normalized share of searches in market d that contain the search term i $G(i, d)$ as follow (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, $max_\delta\{share(i, \delta)\}$ is the maximum share of searches that contain 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 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 Deportation Fear Index for each year-market as

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

[12]Following Alsan and Yang (2019), these terms include: deportation, deportacin, immigration, inmigracin, immigration lawyers, abogados de inmigracin, undocumented, indocumentado.

[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.

where $i \in$ {immigration police, polica de inmigracin, ICE police, polica de ICE, deportation, deportacin, immigration, inmigracin, immigration lawyers, abogados de inmigracin, undocumented, indocumentado}, $p \in$ {deportes (sport), telenovelas (soap operas)}[14].

County Population data: I use the SEER population data to construct fertility rate which defined as 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 combined with births by county-race-month-year to construct the fertility rate.

4 Empirical Framework

To examine the causal effect of Secure Communities on birth outcomes of likely undocumented immigrants, I utilize the staggered roll-out of the SC program across the counties. My main specification is a triple differences model comparing Hispanic newborns to non-Hispanic groups (first difference), before versus after the SC activation (second difference), in treated versus control counties (third difference).

4.1 Motivating Difference-in-Differences Estimate

I first estimate the following equation:

$$Y_{icsmy} = \alpha + \beta_1 SC_{cmy} + \gamma_1 X_i + \gamma_2 Z_{sy} + \gamma_3 Z_{csy} + \delta_s \cdot t + \mu_c + \theta_m + \lambda_y + \epsilon_{icsmy} \tag{3}$$

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 equal to one if i's birth date is after the SC activation and 0 otherwise. X_i is a vector of individual control variables for maternal and child characteristics, including four dummies for mother's

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

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, percentage of population that 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. $(\delta_s \cdot t)$ 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 the activation of SC 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. Standard errors are clustered at the county level (Bertrand et al., 2004). The key coefficient of interest is β_1, which measures the DD estimate of the effects of SC on birth outcomes of Hispanic babies born in treatment states. I restrict the sample to children of foreign-born mothers with less than high school degree.

To measure the spillover effect of SC on birth outcomes of US-born Hispanic newborns, ideally, I would like to directly examine birth outcomes of newborns of undocumented moth-

[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; Cameron County, TX.

ers. But there are no available data that allow for precise identification of undocumented immigrants at the individual level. Thus, I follow the literature defining likely undocumented immigrants as high school dropout Hispanic non-citizens.[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 US but was granted citizenship through naturalization, resulting in my misclassification of whether that individual was undocumented. In general, I expect that this misclassification will bias my estimates towards zero.

The identification assumption in DD analysis is a standard parallel trends assumption. If this assumption is violated, then the DD estimates will be biased. The validity of this assumption can be verified based on an event study approach. Figure A.5 shows the event study estimates of this DD design. The results suggest that the DD results may be slightly biased as the event study shows a positive pre-trends. Additionally, the DD specification does not take into account that many foreign-born Hispanic mothers were not affected by the SC because they were granted citizenship as mentioned above.

4.2 Baseline Estimating Equation

These issues presented in Section 4.1 motivate a triple differences comparing children born to Hispanic mothers with children born the non-Hispanic mothers, across counties, before and after SC was activated. Specifically, I estimate the following model with county, state, month, and year of birth fixed effects as follow:

$$\begin{aligned} Y_{icsmy} = \alpha + \beta_1(SC_{cmy} \times HISP_i) + \beta_2 SC_{cmy} + \beta_3 HISP_i + \gamma_1 X_i \\ + \gamma_2 Z_{sy} + \gamma_3 Z_{csy} + \delta_s \cdot t + \mu_c + \theta_m + \lambda_y + \epsilon_{icsmy} \end{aligned} \tag{4}$$

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

where $HISP_i$ is an indicator for Hispanic ethnicity. The rest of the variables and parameters are as defined above. The key coefficient of interest is β_1, which measures the estimate of the effects of SC on birth outcomes of Hispanic babies relative to all non-Hispanic babies, 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 Hispanics 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:

$$\begin{aligned} Y_{icsmy} = \alpha + \sum_{r \neq -1} \beta_1^r \cdot \mathbf{1}\left[r = t\right] \cdot HISP_i + \sum_{r \neq -1} \beta_2^r \cdot \mathbf{1}\left[r = t\right] + \gamma_1 X_i \\ + \gamma_2 Z_{sy} + \gamma_3 Z_{csy} + \delta_s \cdot t + \mu_c + \theta_m + \lambda_y + \epsilon_{icsmy} \end{aligned} \tag{5}$$

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-Hispanics. All the controls and fixed effects are the same as in Equation (4).

4.3 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 three ways.

First, I exclude border counties where SC program was adopted early from all analyses as mentioned in Section 4. Second, I implement a variant of Fisher's permutation or randomization inference test (Fisher, 1935).[17] To implement this exercise, I estimate Equation (4) 1000 times by randomly assigning a placebo SC activation year for each county, ensuring that there are four years as treated and five years as the pre-period. Figure 4 shows the

[17]This test is 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)

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.

Third, I test whether predicted birth outcomes are correlated with SC activation. Specifically, 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 baby in the sample.[18] Figure A.2 corresponds to the event study estimates of Equation (5) for *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. Figure 3(a) and 3(c) correspond to the event study estimates described in Equation (5). These figures present the effects of SC on the likelihood of a VLBW birth and likelihood of a LBW birth separately in each of the 5 years leading up to a SC activation and 4 years after the SC activation. The year before the event (t=-1) corresponds to omitted category and thus it is normalized to zero by construction.

Figure 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 and a LBW birth between Hispanic babies and non-Hispanic babies. On the contrary, these likelihoods start to diverge a few

[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, maternal education dummies.

years after the activation: relative to non-Hispanic babies, the risk of VLBW and LBW of Hispanic babies are larger. Specifically, by four years after the SC activation, Hispanic babies are 23% higher in the probability of VLBW and 10% higher in the probability of LBW, compared to non-Hispanic babies.

Table 1 presents the triple differences results on the effects of SC on average birth weight, and indicators for VLBW, LBW, and premature birth. 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 the SC is associated with a 23% increase in VLBW (column 1), and a 10% increase in LBW (column 2).

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. This 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.[19]

5.2 Placebo Tests

I provide two main robustness checks for my identification strategy: placebo tests. First, I reproduce the analysis but, instead of focusing on babies 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: babies of non-Hispanic white citizens.

Figure 3(b) and 3(d) correspond to difference-in-differences estimates for a sample of

[19]Figure A.4 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; 4,500 (gram). 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.

newborns of white citizen mothers, before versus after SC activation, between treatment and control counties. Figure 3(b) and 3(d) show that all effects are close to zero and statistically insignificant. For newborns 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 test consists of another placebo analysis that involves estimating the same regressions for a placebo characteristic, whether a baby was born on an "odd days", which should not be effected by heightened immigration enforcement.[20] Figure A.3 reports the results using "odd days" as the dependent variable. The results indicate that the chance of babies being born on "odd days" was similar between Hispanics and non-Hispanics. The event study coefficients was stable prior to the event and remained at the same level after the SC activation.

5.3 Mechanisms

In this section, I discuss some potential mechanisms that may explain the effects of SC on birth outcomes of newborns of Hispanic immigrant mothers documented in the previous section, and I provide suggestive evidence about maternal stress and anxiety due to deportation fear.

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; Martnez et al., 2018) which, in turn, could increase the risk for VLBW and LBW births. I build on these works of public health scholars to test the hypothesis that deportation fear is an important channel driving birth outcomes. First, I construct a Deportation Fear Index using the Google Trends data on deportation-related search terms (see Section 3 for more details). Table 2

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

presents difference-in-differences estimates of the effects of SC activation 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.

Second, I examine the effects of sanctuary policies on birth outcomes. Sanctuary counties are counties that enacted policies which 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, I exploit data on a list of sanctuary counties, obtained via a Freedom of Information Act request filed by the Immigrant Legal Resource Center, to compare sanctuary counties to non-sanctuary counties.[21] Consistent with the mechanism, I find significant evidence that the likelihoods of VLBW and LBW are lower in sanctuary counties compared to the baseline results (Column (3) and (6) 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 non-citizen Hispanics and *high school dropout* non-citizen Hispanics in each county. Table 3 presents the coefficients of ($SC \times HISP$) in the main specification (Equation 4) for counties with high share of non-citizen Hispanics.[22] Given my proposed channel, I expect β_1 to be increasing in magnitude as the concentration of Hispanic population increases. I find that the effects are more pronounced among babies born in counties with a higher share of Hispanic non-citizens (Column (3) and (5) in Table 3).

Maternal behavior changes: Thus far, I have argued that prenatal stress induced by SC has significant effects on birth outcomes of babies of foreign-born Hispanic mothers. These

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

[22] Counties that have share of non-citizen Hispanics greater than the mean share of non-citizen Hispanics across counties.

effects may also occur indirectly through the effects of prenatal stress on maternal behaviors and well-being which, in turn, affect the fetal development. For example, mothers may respond to stress by developing hypertension or initiating smoking, then this may adversely affect their children.

Table 4 studies estimates whether SC activation is associated with number of prenatal visits, an indicator for the Women, Infants, and Children (WIC) take-up, hypertension development, diabetes, and reported use of tobacco 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 actually due to stress.

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 to federal immigration authorities about a patient's immigration status. This is different from public benefit (Medicaid or SNAP) take-up context where the authority asks about applicants' immigration status.[23] Thus, I do not see an increase in the use of prenatal care is inconsistent with the maternal stress mechanism.

Other Channels: Finally, I examine other possible explanations that might underlie my results. It may be the case that undocumented families migrate in responding to immigration enforcement or suffer an income loss due to breadwinners being detained or deported. I test these channels using data from the American Community Survey Integrated Public Use

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

Microdata Series data (Ruggles et al., 2019).[24] Table 5 suggests that SC is not associated with migration rates or the poverty level of Hispanics relative to non-Hispanics. In sum, I believe that compositional changes that I can observe are unlikely to drive my estimated effects 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 Great Recession for several reasons. First, I estimate Equation (4) 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, most of the effects of the SC activation are realized after 2011, a year after the end of the recession.[25] Third, I only find the effects on birth outcomes among newborns 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 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 leads to *increases* in fertility in the likely undocumented population, this could lead to an *upward* bias on the estimates (given the finding that the newborns of likely undocumented mothers have worse birth outcomes in Section 5.1). On the other hand, if the SC activation leads to *decreases* in fertility, this could lead to a *downward* bias on the estimates.

[24]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.

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

I consider this by evaluating whether SC activation is associated with any change in fertility rate in Table A.2. The dependent variables are: (1) fertility rate which is number of births per 1,000 women age 15 to 44, (2) birth rate which is number of births per 1,000 population, and (3) probability of a male birth.[26] The SC activation treatment variable equals to one if i's birth date is nine months after SC activation (to proxy for conception) and 0 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 would bias my estimates toward zero.

Finally, a variety of robustness checks support my main results. 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.[27] Second, my estimates are robust to controlling for an array of other policies aimed at the undocumented immigrant population, including 287(g) Agreements and E-Verify. Third, my results are robust to excluding Texas where health facility closures affected health care for women in 2011-2012 (Lu and Slusky, 2016).

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 (4), I use a child's actual birth date to define the treatment variable SC_{cmy}. There is a concern that the SC activation can affect the length of the pregnancy, 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,

[26]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).

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

2018). Using the expected date of birth to define the treatment group would address the endogenous issue.[28] Unfortunately, utilizing the expected birth dates is extremely difficult to make given my current data availability and constraints. Specifically, about 58 percent 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 date that I can construct for use in defining the treatment variable.

To address this issue, I examine an alternative specification which is presented in Table A.3.[29] Initially, I exclude newborns whose birth dates are within ± 1 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 newborns, up to ± 3 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.

To further address this issue, I use a hazard model, which predicts the timing of the SC activation using pre-period mother characteristics following Cox and Miles (2013). This predicted SC activation date is less likely to be correlated with the actual date of birth. Using predicted SC activation obtained from the hazard model, I reproduce the main analysis on the impact on birth outcomes. Results are presented in Figure 5. I still find a statistically significant reduction in birth outcomes for Hispanic babies. Furthermore, the estimated impacts are similar to using actual SC activation time.

Taken together, this is unlikely to be a major concern in my context given that I have relatively few observations around the SC activation dates in comparison with the whole sample size.

Address the Issue of Multiple Hypothesis Testing: To address this issue, I follow Kling et al. (2007); Currie et al. (2019) and group my outcomes into a birth outcomes index.

[28] A child's expected birth date is defined as: the date of conception + 280 days

[29] This is inspired by Donut-Regression Discontinuity estimates (Almond et al., 2010a; Barreca et al., 2011; Almond et al., 2011a).

The birth outcomes index consists of the following measures: very low birthweight (< 1,500 grams), low birthweight (< 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).

This index is created so that a higher value represents a better outcome.[30] Table A.1 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 US 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 US-born Hispanic children. Using administrative birth certificate data and multiple identification strategies, I present evidence that tougher immigration enforcement caused an increase of 23% in the likelihood of very low birthweight for babies of foreign-born Hispanic mothers. I provide evidence that some, although probably not all, of these effects operated through maternal stress induce by deportation fear.

My findings provide evidence of unintended consequences of the SC program, which is designed to affect only undocumented immigrants, on future US citizen birth outcomes.

[30] See Currie et al. (2019) for 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, focusing on the estimates of the effect of immigration enforcement on VLBW births to estimate the social cost of immigration enforcement. The calculation suggests an annual social cost around \$1.35 billion ($= \$7,371 \times 183,000$) in 2018 dollars based on the best available estimates cost for VLBW births (\$7,371; Currie et al., 2019), and the estimate that there are 183,000 children born in the US to undocumented immigrants per year.[31]

The results in this paper imply that immigration enforcement can have unintended consequences not just for the likely undocumented immigrants, but also for the next generation who are future citizen and society as a whole. It is an open question 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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[31]These numbers likely underestimate the full social cost of immigration enforcement on pregnant women for at least two reasons: measurement error in likely undocumented status downward biased the effects on VLBW, the effects on maternal well-being was not measured.

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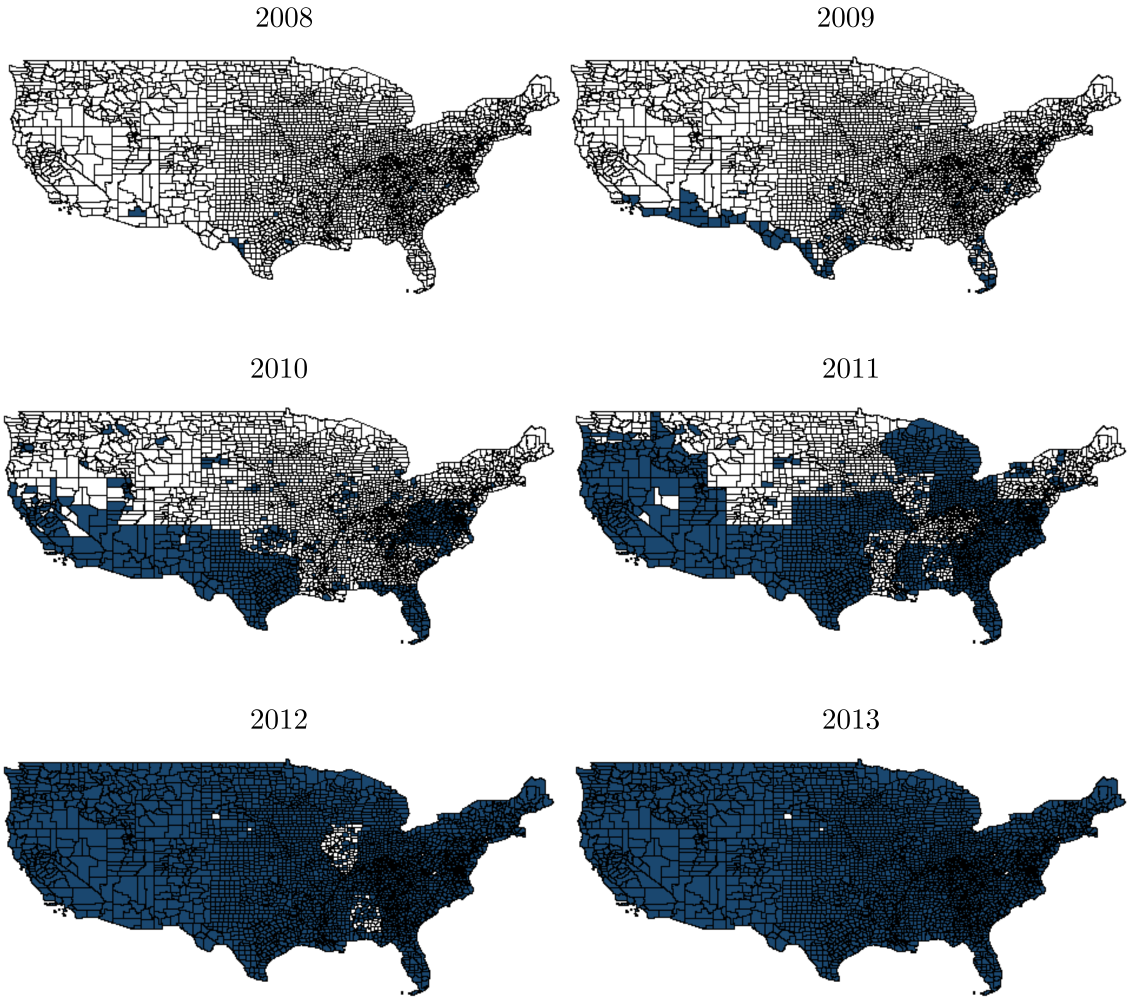
7 Figures

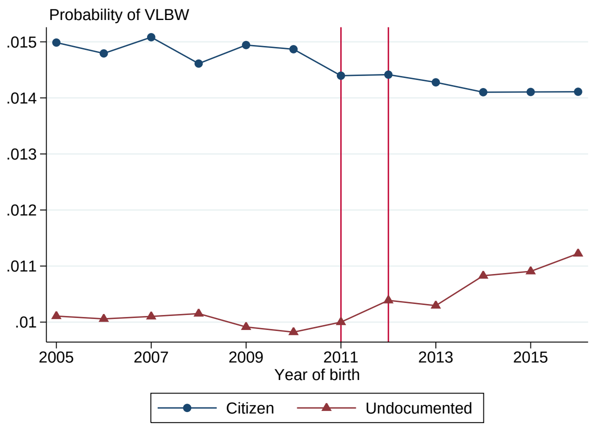
Figure 1: Secure Communities Roll-out



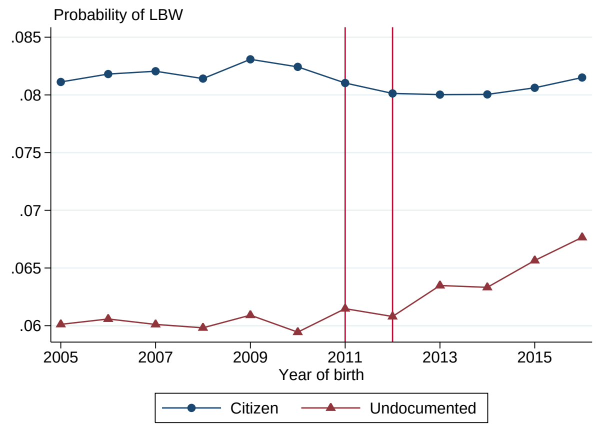
Notes: Data from US ICE. Counties that had adopted Secure Communities are shaded.

Figure 2: Trends in the likelihood of VLBW and LBW by year of birth

(a) Very Low-birth-weight



(b) Low-birth-weight

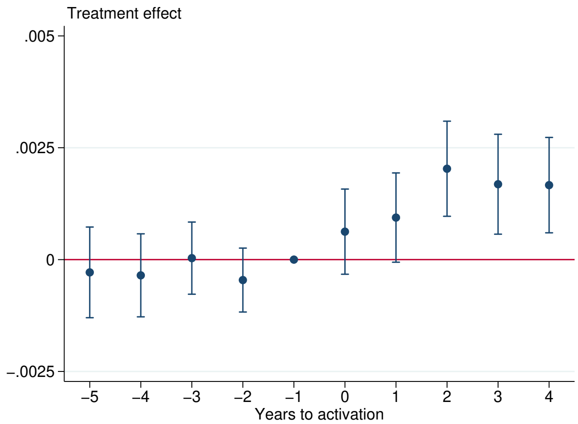


Notes: Author's calculation from Natality data. See text for further details.

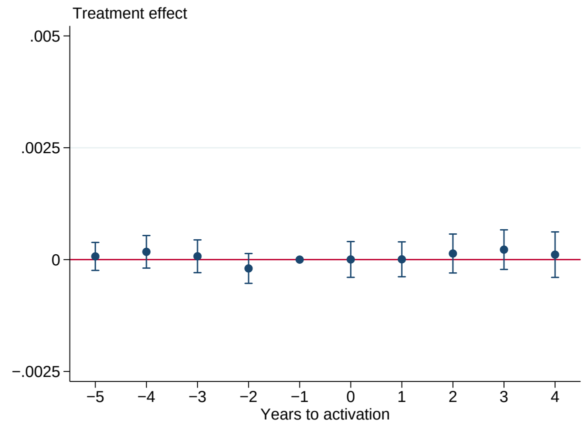
Figure 3: Effect of Secure Communities on Birth Weight

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

(a) Hispanics

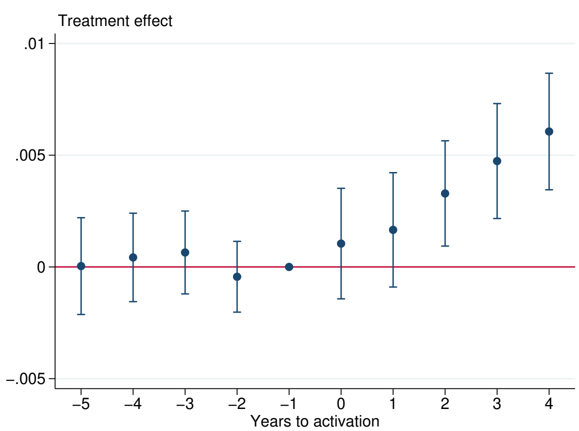


(b) Non-Hispanic whites

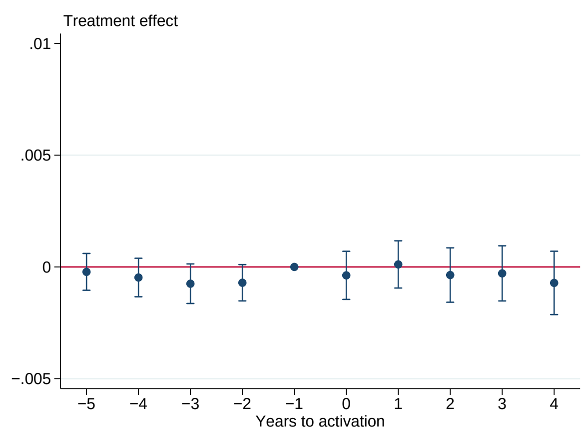


Panel B. Effects of SC on the likelihood of low-birth-weight birth

(c) Hispanics

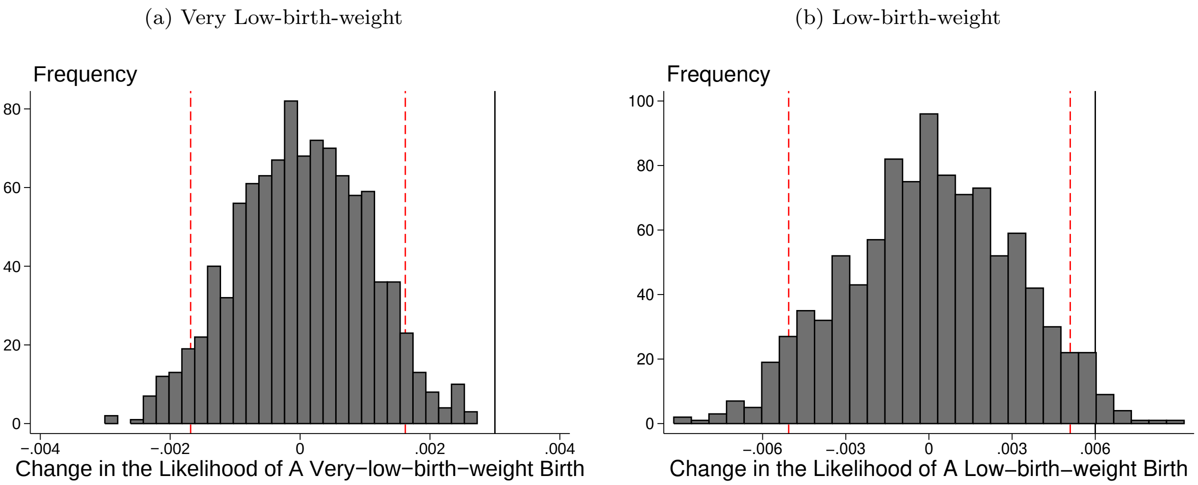


(d) Non-Hispanic whites



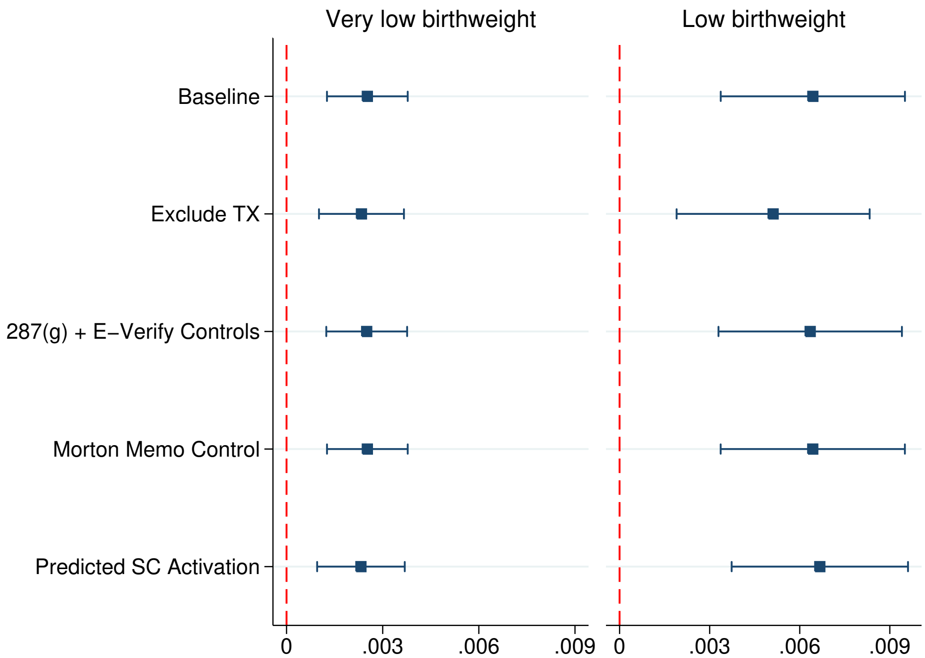
Notes: Data from Vital Statistics Natality 2005-2016. The sample in figure 3(a) and 3(c) are limited to children of foreign-born mothers with less than high school degree. The sample in figure 3(b) and 3(d) are limited to children of non-Hispanic white citizen mothers. Baseline controls include four 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 that are hispanic, black, white, and female ages 15-44. Robust standard errors clustered at the county level. Whiskers show 95% confidence interval.

Figure 4: Permutation Tests on Effects of SC on birth outcomes



Notes: Data from Vital Statistics Natality 2005-2016. The sample are limited to children of foreign-born mothers with a high school degree or less. Figure shows the histogram of placebo estimates of Equation (4) 1000 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.

Figure 5: Robustness Checks of Secure Communities Effects on Birth Outcomes



Notes: See Section 5.4 for details.

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: Data from Vital Statistics Natality 2005-2016. The sample are limited to children 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 that 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: Data from Google Trends. 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). 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	High share of NC Hisp.	Sanctuary counties	Baseline	High share of NC Hisp.	Sanctuary counties
	(1)	(2)	(3)	(4)	(5)	(6)
SC × Hispanic	0.003***	0.004***	0.001	0.006***	0.010***	0.002
	(0.001)	(0.001)	(0.002)	(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	1,139,814	906,836	2,727,531	1,139,814	906,836
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: The table reports coefficient estimates for heteregeneity of exposure to SC activation discussed in section 5.3. Each parameter is from a separate regression. NC = Non-Citizen. See Table 1 for full table notes.

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 from Vital Statistics Natality 2005-2016. The sample are limited to children 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 that are Hispanic, black, white, and female ages 15-44. Robust standard errors clustered at the county level are reported in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

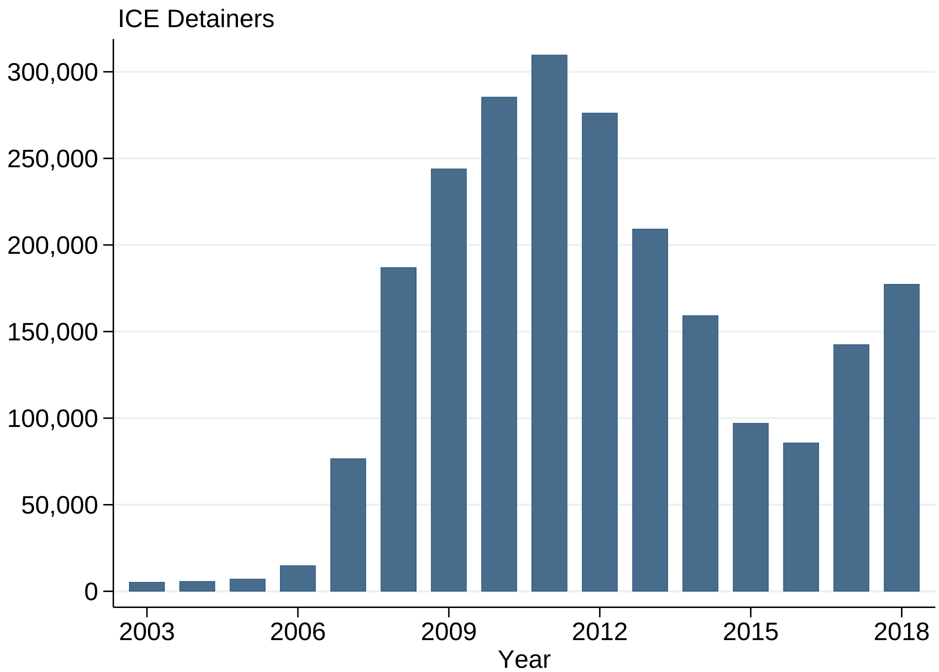
Table 5: Effects of Secure Communities on Migration, Employment, and Household Structure

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

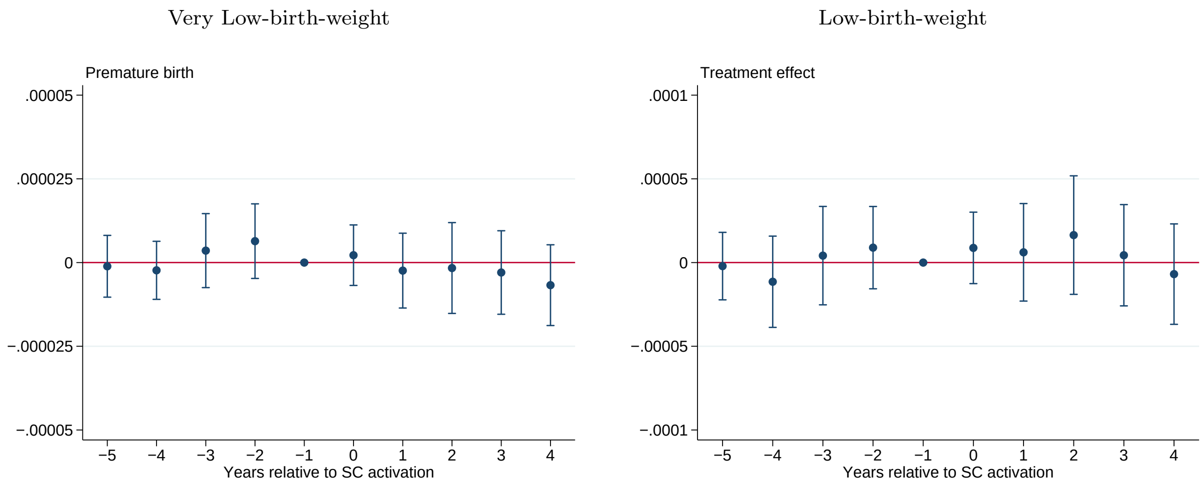
A Supplementary Figures and Tables

Figure A.1: Number of Detainers by Year



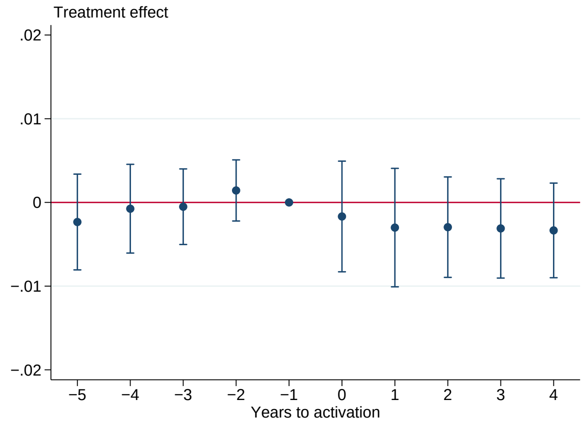
Notes: Data from TRAC Immigration 2003-2018.

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



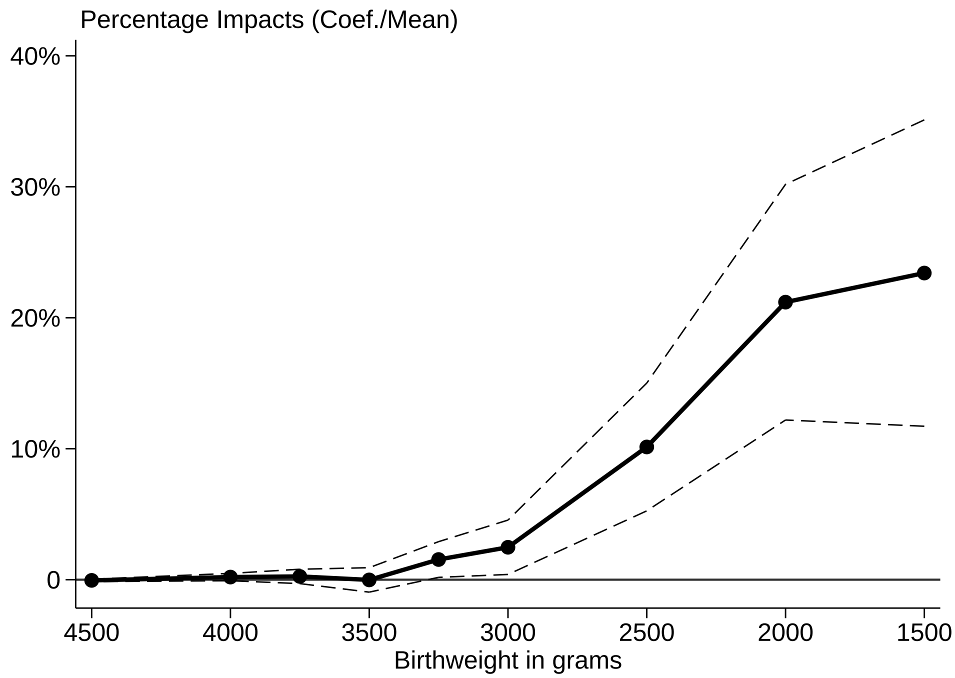
Notes: Data from Vital Statistics Natality 2005-2016. The sample are limited to children of foreign-born mothers with a high school degree or less. This figure shows event study estimates where outcomes are predicted birth outcomes. The outcomes are the fitted values of likelihood of low-birth-weight and very low-birth-weight birth, obtained from regressions of the birth outcomes on a set of characteristics include: gender, year, month, week of birth, indicators for maternal age dummies, indicator for mother being married, maternal race dummies, maternal education dummies using pre-period data. See Figure 3 for full figure notes.

Figure A.3: Effects of Secure Communities on Odd Birthdays



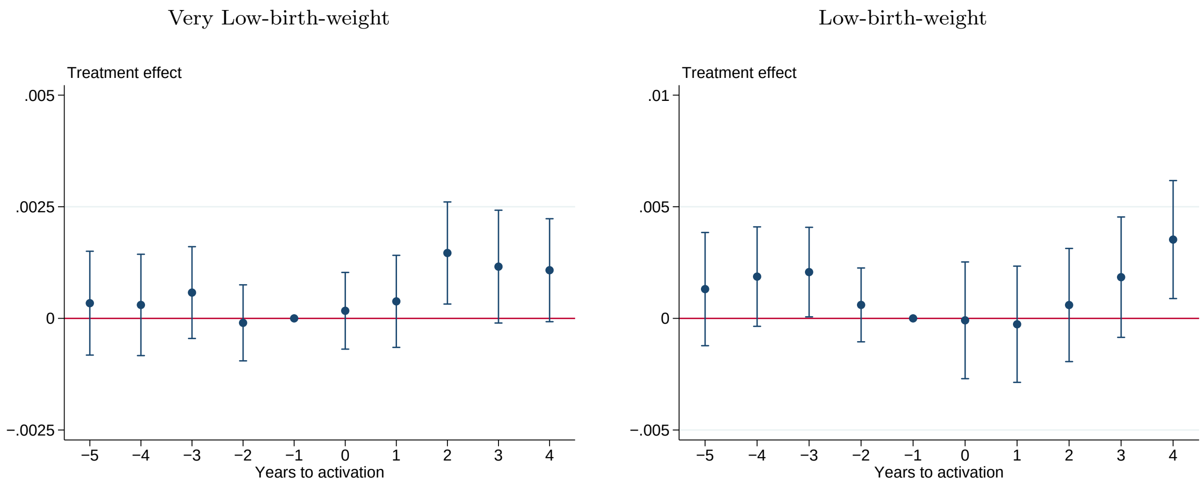
Notes: Data from Vital Statistics Natality 2005-2016. The sample are limited to children of foreign-born mothers with a high school degree or less. This figure shows event study estimates where outcome is whether a baby was born on an "odd days". See Figure 3 for full figure notes.

Figure A.4: Effects of Secure Communities on Birthweight Distribution



Notes: Data from Vital Statistics Natality 2005-2016. The sample are limited to children of foreign-born mothers with a high school degree or less. 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. 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 that are Hispanic, black, white, and female ages 15-44. Robust standard errors clustered at the county level.

Figure A.5: Difference-in-Differences Effects of Secure Communities on Birth Outcomes

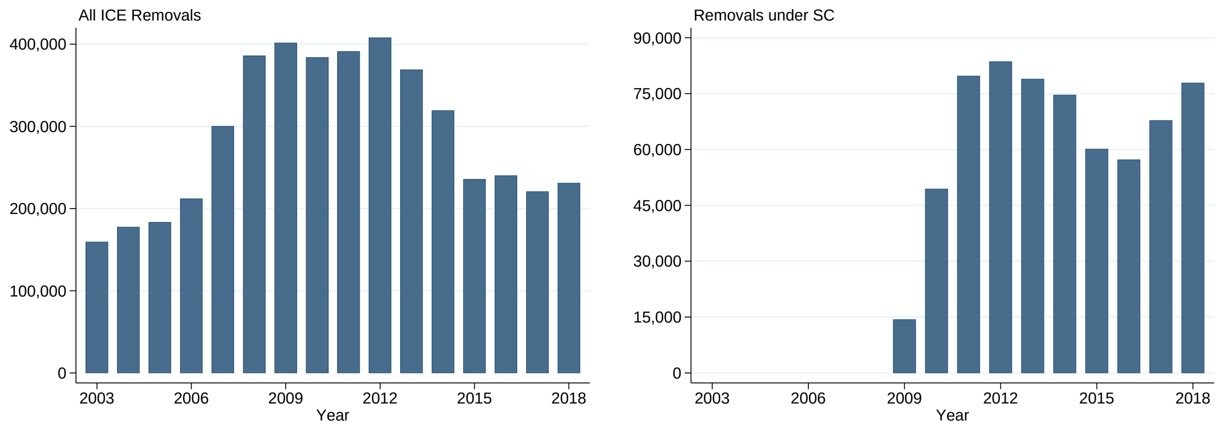


Notes: This figure shows event study estimates of the DD model in Equation (3). The results here suggest that the DD model may be slightly biased as the event study shows a positive pre-trends. This motivates a triple differences model as described in Equation (4). Data from Vital Statistics Natality 2005-2016. The sample are limited to children of foreign-born Hispanic mothers with a high school degree or less. See Figure 3 for full figure notes.

Figure A.6: Number of Removals by Year

(a) All ICE Removals

Panel B: Removals under SC



Notes: Data from TRAC Immigration 2003-2018.

Table A.1: Effects of Secure Communities on Birth Outcomes Index

Outcomes	*Birth outcomes index*			
	(1)	(2)	(3)	(4)
SC × Hispanic	-0.012***			
	(0.004)			
SC × Hisp × % NC Hisp		-0.052***		
		(0.015)		
SC × Hisp × % LS NC Hisp			-0.110***	
			(0.029)	
SC × Hispanic × Not Honor ICE				-0.002
				(0.004)
Mean of dep. var.	-0.01	-0.01	-0.01	-0.01
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: Data from Vital Statistics Natality 2005-2016. The birth outcomes index includes the following measures: very low birthweight (< 1,500 grams), low birthweight (< 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 are limited to children 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 and state-level controls: unemployment rate, percentage of population that 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 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 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 that 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, 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 robustness of results to excluding newborns who birth dates are within ± 1 month up to ± 3 months of the SC activation date. Data from Vital Statistics Natality 2005-2016. The sample are limited to children 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 and state-level controls: unemployment rate, percentage of population that are Hispanic, black, white, and female ages 15-44. Robust standard errors clustered at the county level are reported in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.