Unintended Consequences of Immigration Enforcement: Household Services and High-Educated Mothers' Work*

Chloe N. East[†]
Andrea Velásquez[‡]

Abstract

Immigration enforcement has intensified in the U.S., however, there is little evidence on its effect on U.S.-born individuals' labor outcomes. Exploiting the staggered rollout of a large, federal enforcement policy–Secure Communities (SC)–across local areas, we estimate a difference-in-differences model with time and location fixed effects. We find that SC reduced the labor supply of college-educated U.S.-born mothers with young children. If SC exposure occurred when children are below age 3, the negative effects on labor supply persist over time. We further show increased cost of outsourcing household production, due to reduced undocumented immigrants' labor supply, is an important mechanism.

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†Chloe East is an Assistant Professor of Economics at the University of Colorado Denver, email: chloe.east@ucdenver.edu

‡Andrea Velásquez is an Assistant Professor of Economics at the University of Colorado Denver, email: andrea.velasquez@ucdenver.edu

Replication files are available at https://github.com/cneast/East_Velasquez_2022. Due to restrictions on data sharing, we do not publish the ACS or TRAC data sets we use. The ACS data can be downloaded here: https://usa.ipums.org/usa/ Information about TRAC data licenses is here: https://trac.syr.edu/fellows/

1 Introduction

The costs and benefits of immigration on natives' outcomes are hotly debated by economists and policy-makers, and they have been a major campaign topic in elections around the world in recent years.[1] In the U.S., where *undocumented* immigrants account for about 5% of the U.S. workforce (Krogstad, Passel and Cohn, 2017), undocumented immigration has received particular attention in the public discourse.[2] This has been accompanied by important policy changes at the local, state, and national level aimed at addressing undocumented immigration. As a consequence of these policy changes, over the last 15 years, interior immigration enforcement has increased dramatically, which has in part led to the rapid rise in immigrant detentions and deportations.[3] Therefore, understanding the effects of these changes, both on the immigrant population, and on natives, is crucially important. However, while the previous literature has primarily focused on the effect of immigration enforcement policies in the U.S. on the immigrant population,[4] evidence on the spillover effects of these policies on U.S.-born individuals' labor outcomes is limited.

In this paper, we focus on the potential unintended consequences of Secure Communities (SC)–an immigration enforcement policy that increased the likelihood that undocumented immigrants were identified, detained, and deported–on the labor supply of U.S.-born individuals who are likely to outsource household production. Likely undocumented immigrants are a large portion of workers in household service occupations–they represent 18.4%

[1]An extensive literature has focused on estimating the impact of migratory flows on labor outcomes of natives. For excellent reviews of the literature see Friedberg and Hunt (1995), Longhi, Nijkamp and Poot (2005), and Longhi, Nijkamp and Poot (2006). For the current debate on immigration policies, see for example: Menasce Horowitz (2014), Givens (2018), Thompson (2018), Economist (2018), Winders (2016).

[2]See for example: Felter and Renwickn (2019).

[3]Between the early 2000s and mid 2010s, the number of people detained annually increased by 3,200% to roughly 160,000 annual detentions in 2014, and the number of people deported annually roughly doubled to 200,000 annual deportations in 2014. Statistics from the Transactional Records Access Clearinghouse (TRAC) available at: https://trac.syr.edu/phptools/immigration/detainhistory/ and https://trac.syr.edu/phptools/immigration/removehistory/. This has far outpaced the growth in the estimated number of undocumented immigrants living in the U.S. in this time period: from roughly 9 million in 2003 to 11 million in 2014 (Krogstad, Passel and Cohn, 2018).

[4]See for example, Phillips and Massey (1999), Bansak and Raphael (2001), Orrenius and Zavodny (2009), Amuedo-Dorantes and Bansak (2014), Orrenius and Zavodny (2015) and Hansen (2019).

of maids and housekeepers and 4.8% of workers in childcare services (Appendix Table (A1)).[5] Thus, decreased labor supply of undocumented immigrants through more restrictive policies is predicted to increase the cost of household services, affecting time allocation decisions of natives (Cortes, 2008; Cortes and Tessada, 2011).

To measure the spillover effects of SC, we focus on college-educated females with pre-school-aged children (under age 5). We argue that this group is the most likely to be impacted by changes in the cost of outsourcing household production. First, high-educated workers spend a larger fraction of their income outsourcing household work.[6] Second, among high-educated workers, females spend more time on household and childcare activities relative to males, and have a more elastic labor supply when compared to males (Blau and Kahn, 2007; Drake, 2013).[7] Third, most states allow children to begin public kindergarten, if available, at age 5 (Parker, Diffey and Atchison, 2016; National Center for Education Statistics, 2018) so free public education for children under age 5 is very limited.

We also investigate the effects of SC on immigrants in household services themselves. To understand how immigrants may be affected, it is important to understand that SC worked by increasing information sharing between local law enforcement and Immigration and Customs Enforcement (ICE) to identify and remove undocumented immigrants. Specifically, SC required and automated process by which arrested individuals would have their

[5]Authors' calculations using the 2005 American Community Survey (ACS). We do not observe documentation status in the ACS, so we follow previous literature and define likely undocumented immigrants as Hispanic, foreign-born, with less than a high-school degree. This is a common approach used in the literature to proxy for documentation status given it is not asked in large-scale U.S. Census Bureau surveys (Van Hook and Bachmeier, 2013; Passel and Cohn, 2014; Borjas and Cassidy, 2019; Albert, 2021). We test the robustness of this definition to using more restrictive samples conditioning on year of arrival to the U.S. and country of birth.

[6]On average, in 2005, the fraction of income that college-educated households spent on household services was more than twice as much the fraction spent for households with at most a high school degree in the Consumer Expenditure Survey: https://www.bls.gov/cex/2005/share/educat.pdf. In addition, married couples without kids spend 1.3% of their income on household services, compared to 2% for married couples with their oldest child aged 6-17, and 4.9% for married couples with their oldest child under age 6. Information from the Consumer Expenditure Survey: https://www.bls.gov/cex/2005/share/cucomp.pdf.

[7]Table (1) shows that high-educated U.S.-born females spend 40% more time on household activities and 90% more time on childcare relative to high-educated U.S.-born males, and the difference is more striking when considering mothers of young children. Authors' calculations using the 2005 American Time Use Survey (ATUS).

fingerprints sent to ICE for immigration status screening. We expect this to affect the labor supply of undocumented immigrant workers through three main channels. First, SC affected the availability of immigrant labor through direct removals. SC is credited with more than 450,000 individuals deported, 96% of whom were male, over our sample period of 2005-2014.[8] Of those deported in this time period, 20% were not convicted of a crime, and 26% were not convicted of a serious crime, so a broad population may have been directly affected. Moreover, Hispanic and Latino immigrants were overrepresented among those deported under SC, which could be due to racial profiling. In fact, advocacy groups have alleged that SC provided a way for law enforcement to use minor violations to target the Hispanic population (Kohli, Markowitz and Chavez, 2011).[9] Second, voluntary migration may have changed–either by increasing out-migration from the U.S. or decreasing in-migration to the U.S., or both. Third, partially because of the broad nature of the deportations, and the fact that certain groups were overrepresented, fear and mistrust of local law enforcement, and government more broadly, may have created a "chilling effect" among immigrants who stayed in the U.S., causing them to reduce their labor supply.

SC is well-suited for our analysis because it was mandatory, and it was rolled out in a staggered fashion across localities. We match data on the timing of implementation of SC across local areas to high-educated female individuals' labor outcomes in the American Community Survey (ACS) from 2005-2014. This allows us to estimate a difference-in-differences model, controlling for local area and survey year fixed effects. Thus, our identification strategy relies on the assumption that, conditional on fixed effects, there are not time-varying differences within local areas that are correlated with the timing of SC adoption. We test this

[8] Note that this overrepresentation of males is not reflected in the overall undocumented population but might reflect the fact that males are overrepresented in the population that is incarcerated. Specifically, estimates suggest that in 2012, 53% of the undocumented population was male (Baker and Rytina, 2013) and 90% of the incarcerated population in 2001 was male (Bonczar, 2003).

[9] In 2007, 70% of all undocumented immigrants were estimated to come from Mexico and Central American countries, but 86% of the deportations through SC were among immigrants from these countries. Appendix Table (A2) shows the information about individuals who were deported under SC. Amuedo-Dorantes, Puttitanun and Martinez-Donate (2018) also find that broad enforcement policies, like SC, led to increased detainment for minor violations.

assumption in several ways, including implementing event studies that support the parallel pre-trend assumption and showing that SC start dates are not correlated in a qualitatively meaningful way with pre-SC trends in demographics and economic conditions in local areas. Moreover, because the research design is a staggered rollout, we test whether our results using a two-way fixed effects model are biased due to heterogeneous treatment effects. The Bacon decomposition (Goodman-Bacon, 2021) suggests any bias will be small and move us towards a null effect, and the results are similar when using a new estimation method proposed by Callaway and Sant'Anna (2021) that is robust to any potential biases with the staggered rollout design. Two other features of SC are worth noting: first, local areas had little influence over the timing of policy adoption and limited discretion in the operation of the program;[10] second, because SC rolled out quickly and eventually covered the entire country, internal migration is less likely to bias the results (Borjas, 2003; Borjas and Katz, 2007; Cadena and Kovak, 2016).

Our primary finding is that working-age (20-63) college-educated U.S.-born mothers of young children significantly reduced their labor supply in response to SC exposure. The effects of SC are larger for this group than for all females or all mothers, which is consistent with the fact that mothers with young children are more sensitive to changes in the price of outsourcing household production. These mothers experience a reduction in the likelihood of working by 0.99% and in hours worked by 1.5% relative to the mean. These estimates are about 1-2% of the labor market effect of having a child for women in the U.S. (Kuziemko et al., 2018; Kleven et al., 2019).[11] However, it is important to note that our estimates

[10]Some jurisdictions, known as "sanctuary cities", refused to cooperate with ICE detainer requests by claiming that the policy was unconstitutional under the Fourth Amendment. Following Alsan and Yang (2018), we use information from the U.S. Department of Homeland Security about whether a locality resisted such requests to classify locations as sanctuary cities. Only 36 of roughly 1000 PUMAs fit this definition *prior* to SC implementation; the vast majority of sanctuary city policies began in 2014, which, in part, led to the replacement of SC with the Priority Enforcement Program in 2014. We do not examine heterogeneous effects by sanctuary city status, given how rare this was during our sample period. Instead, we focus on other measures of program intensity discussed in more detail below.

[11]Kuziemko et al. (2018) estimate that having a first child decreases female's labor force participation by 46-96% and Kleven et al. (2019) estimate a decline in labor force participation of 43% following a first birth in the U.S. Note that both of these estimates include all females in the sample (unconditional on education)

are for all mothers, and many mothers may not respond to the effect of SC, so the effects on those who do respond are likely much larger. We also investigate the extent to which having SC in place in the years following a child's birth has lasting consequences on mothers' labor supply, since previous work finds the motherhood penalty is of similar magnitude for many years after childbirth (Kuziemko et al., 2018; Kleven et al., 2019). We find suggestive evidence that exposure to SC around a birth negatively impacts the labor supply of mothers for several years after the birth.

We next provide evidence that an increase in the price of outsourcing household services is an important mechanism driving the observed changes in high-educated mothers' labor market outcomes. We examine the effect of SC on the labor supply of likely undocumented female workers in household services–female workers represent over 94% of the total employment of low-educated Hispanic foreign-born working in these services.[12] We find no effect on the number of likely undocumented female workers in household services, which is consistent with the fact that most of the individuals removed under SC were male. However, among likely undocumented females who stayed in the U.S., we find a negative effect on their labor supply at the intensive margin and no evidence of a compensating increase in the labor supply of other groups. Thus, there is an overall reduction in the labor supplied in household services after SC implementation. These effects are larger in places with a higher share of Hispanic deportations, and with more deportations for non-serious crimes, where chilling effects are plausibly larger.[13] These results highlight that when analyzing the spillover effects of immigration onto labor outcomes of U.S.-born individuals, it is crucially important to consider not only the size of the immigrant population, which has been the focus of most of the existing literature (e.g. Furtado and Hock (2010); Cortes and Tessada (2011)), but

and both focus on much earlier time periods than we do.

[12] Calculations based on the 2005 American Community Survey. See Appendix Table (A1).

[13] This is consistent with other literature that has documented chilling effects on other outcomes of Hispanic immigrants. Specifically, Alsan and Yang (2018) find that SC reduced Hispanic *citizens'* participation in safety net programs and attribute this to chilling effects. Wang and Kaushal (2018) find that enforcement caused a worsening of self-reported mental health among Latino immigrants.

also the political conditions that allow immigrants to integrate in their destination country. Next, we examine the effect of SC on the cost of outsourcing household services, proxied by hourly wages of female workers in this sector. Given that production in these occupations is very labor intensive, wages are likely very closely correlated with prices (Hock and Furtado, 2009), and we find a significant positive effect on the hourly wages of low-educated females working in this sector.

As further evidence of the mechanism, we identify two comparison groups for whom changes in the cost of outsourcing household production is less likely to influence their labor supply decisions: high-educated female individuals with no children, and high-educated fathers with pre-school-aged children. We use both groups in a triple difference model that allows us to include area by time fixed effects, which flexibly account for any other common shocks to labor outcomes across areas and over time. After netting out the effect of SC that is common across the groups and might be due to other effects of SC (such as complementarities in market production), there is a differentially larger effect on mothers with young children. Moreover, the triple difference estimates are 72-90% of our main difference-in-differences estimates suggesting that changes in the cost of outsourcing home production is an important mechanism driving the labor supply effects on U.S.-born mothers.

Our main contributions to the immigration literature are twofold. First, we evaluate the effects of a recent enforcement policy in the U.S. that led to the removal of immigrants, rather than studying the effect of migratory inflows on the outcomes of interest, as in much of the previous literature. Our results also inform the debate about the distributional effects of immigration and immigration enforcement on U.S.-born individuals. We find enforcement policies affecting low-educated immigrants have negative spillover effects onto high-educated U.S.-born mothers. The only existing evidence of these spillover effects of SC on U.S.-born labor outcomes is East et al. (2021), who document different mechanisms through which enforcement policies can affect U.S.-born labor outcomes.[14] They show that SC negatively

[14] Another literature examines the effect of other immigration policies on natives' labor market outcomes.

impacted the labor outcomes of likely undocumented immigrants and led to a decrease in the employment share and hourly wages of U.S.-born. The main mechanisms explaining these results are complementarities in production between likely undocumented immigrants and U.S.-born workers, and a reduction in demand for local goods. In this paper, we find evidence of another important pathway through which SC affects the labor market outcomes of the U.S.-born, explained by another type of complementarity: between low-educated female immigrants working in household services and high-educated U.S.-born mothers working outside the home. Our paper also contributes to a growing literature studying the effects of SC on local communities' outcomes including spillover effects to citizen safety net program participation (Alsan and Yang, 2018), local crime (Miles and Cox, 2014; Hines and Peri, 2019), immigrants' marriage patterns (Bansak and Pearlman, 2021), and immigrants' health outcomes (Wang and Kaushal, 2018). SC is a particularly contentious enforcement policy, and understanding its effects is crucial for policy-makers as immigration policy is actively and rapidly changing.[15]

Second, we add to previous work documenting a positive relationship between the presence of low-educated immigrants and high-educated female's labor supply.[16] In particular, our empirical strategy overcomes methodological challenges faced by the previous literature, which used a shift-share approach to estimate the effect of immigration inflows on high-skilled females' labor supply in the spirit of Card (2001). In recent years, an increasing number of papers call into question the assumptions behind this shift-share approach (Adao, Kolesár and Morales, 2018; Borusyak, Hull and Jaravel, 2018; Goldsmith-Pinkham, Sorkin and Swift, 2018), including in the context of immigration (Jaeger, Ruist and Stuhler, 2018).

For example, Bohn, Lofstrom and Raphael (2015) study the spillover effects of Arizona's E-Verify law on citizen workers who may substitute for undocumented workers. They find these citizens experience lower employment, but higher earnings as a result of the policy.

[15]SC was implemented in 2008, suspended in 2014, re-activated in 2017 and suspended again in 2021.

[16]The literature has examined this relationship in the United States (Furtado and Hock, 2010; Cortes and Tessada, 2011; Amuedo-Dorantes and Sevilla, 2014; Furtado, 2015, 2016), Italy (Barone and Mocetti, 2011; Peri, Romiti and Rossi, 2015), Hong Kong (Cortes and Pan, 2013), Spain (Farré, González and Ortega, 2011), the UK (Romiti, 2018), and in a cross-country approach (Forlani, Lodigiani and Mendolicchio, 2015).

In contrast to this approach, we use enforcement policy as an exogenous driver of immigrants' labor supply, and we test the validity of this exogeneity assumption in several ways, as discussed above.

The rest of the paper proceeds as follows: in the next section, we provide details about SC and the data we use. Section 3 describes our empirical strategy, and section 4 presents our results. Section 5 concludes.

2 Policy Background and Data

Secure Communities is one of the largest interior immigration enforcement programs in the U.S.[17] SC increased information sharing between local law enforcement agencies and U.S. Immigration and Customs Enforcement (ICE). The goal of SC was to identify individuals eligible for removal from the U.S. Prior to SC, individuals' fingerprints would be taken upon being booked in state prisons or local jails and would be sent to the Federal Bureau of Investigation (FBI) to conduct a criminal background investigation. Under SC, these fingerprints would now also be sent to ICE, who would try to determine an individual's immigration status using their Automated Biometric Identification System (IDENT).[18] Based on this, a detainer may be issued, and the law enforcement agency would then be required to hold the individual for up to 48 hours in order for ICE to obtain custody and start the deportation process. Importantly, detainers could be issued for criminal reasons or for immigration-crime reasons, and they did not have to be proceeded by a conviction.

Implementation of SC required establishing a partnership between local law enforce-

[17] For comprehensive reviews of SC, see Cox and Miles (2013), Miles and Cox (2014), and Alsan and Yang (2018). The information in this section comes primarily from these reviews and is similar to that discussed in East et al. (2021).

[18] IDENT includes biometric and biographical information on non-U.S. citizens who have violated immigration law, or are lawfully present in the U.S., but have been convicted of a crime and are therefore subject to removal, as well as naturalized citizens whose fingerprints were previously included in the database. In addition, the IDENT system includes biometric information on all travelers who enter or leave the U.S. through an official port, and when applying for visas at U.S. consulates.

ment and local ICE offices, which took time and resources, and this led to the staggered program roll-out across counties over 2008-2013 that we exploit in our empirical approach. Information on the date of implementation of SC in each county comes from ICE, and Figure (1) shows the pattern of the rollout across Public Use Microdata Areas (PUMAs). We focus on the presence of SC by PUMA rather than by county, because PUMAs are the smallest consistent and comprehensive geographic area available in the ACS.[19] We describe this decision and variable construction in more detail below. The timing of adoption was determined by the federal government. This is important for the assumptions underlying our empirical model since local areas had little discretion in the timing of implementation. Previous evidence shows that early adopters were selected based on the size of their Hispanic population, proximity to the U.S.-Mexico border, and presence of other local enforcement policies.[20] The timing of implementation among later adopters was more random (Cox and Miles, 2013) because the government shifted to mass activations which led to waiting lists. Importantly, this research has shown there is not a relationship between the timing of SC adoption and the area's pre-SC economic conditions, crime rates and potential political support for SC.

Our empirical specification described below includes PUMA fixed effects to control for time-invariant unobserved heterogeneity at the local level, including pre-SC characteristics, such as proximity to the border. So, of more concern is if the implementation of SC is correlated with differential *trends* across PUMAs. We conduct multiple tests to show this is not a likely source of bias in our estimates. First, we directly test whether the timing of the rollout is correlated with pre-SC trends in demographics, immigration enforcement, and economic conditions at the PUMA level. These results are shown in Table (2). Out of seventeen variables, only three variables are statistically significantly related to the timing of

[19] PUMAs are constructed as contiguous geographic areas that respect state borders and have at least 100,000 people living in them. For this reason, PUMAs in rural areas cover much more geographic area than PUMAs in urban areas.

[20] These other local enforcement policies are 287(g) agreements, which were similar in design to SC, but were an optional policy that local areas and states could choose to adopt. We control for the presence of 287(g) agreements over time by local area.

the rollout: the change in the non-citizen population, the change in housing prices, and the change in the percent working more than 50 hours per week. However, these relationships are small in magnitude-the results imply that a one standard deviation increase in the non-citizen population reduces the year of adoption by about 0.14, or roughly 1.7 months, a one standard deviation increase in housing prices reduces the year of adoption by 0.25 or 3 months, and a one standard deviation increase in the percent working long hours reduces the year of adoption by 0.08 or 1.0 month.[21] Moreover, the R-squared on this model is very low (0.07), suggesting that pre-trends in observable characteristics do not do well in predicting the timing of the rollout. Second, in our main specification we control for pre-trends by interacting pre-SC changes in PUMA characteristics with linear time trends. Third, we implement event studies that show that our outcomes of interest were not differentially trending across PUMAs pre-SC. As a final check, we explore the robustness to dropping early adopter places. As discussed in section 4, the results of these tests support the main empirical assumption of our identification strategy.

We merge the data on SC rollout dates to working-age (20-63) college-educated, U.S.-born females' labor supply over the period 2005-2014 from the ACS (Ruggles et al., 2017). Since SC ended in December 2014 (before being reinstated in 2017) and our analysis focuses on the period 2005-2014, our results should be thought of as the effect of *increasing* immigration enforcement. The ACS is a repeated cross-sectional dataset covering a 1% random sample of the U.S., and in the publicly available data set, the smallest geographic area available is the PUMA, which we use as the measure of geography, as in Alsan and Yang (2018). PUMAs are identifiable beginning in 2005, so this is the first year of our sample. The advantages of using PUMAs are that they allow us to precisely measure policy exposure, to cover the entire U.S., and to have a consistent measure of geography over time. How-

[21]Specifically, to calculate the magnitude of these effects, we multiply the standard deviation for each variable by the estimated coefficient for that variable. The outcome variable is the year of first SC implementation in the PUMA. So, for example, for housing prices, a one standard deviation change in housing prices (31.217) results in a 0.25 increase in the year of SC implementation.

ever, a disadvantage of PUMAs is that they do not map onto the level of policy variation one to one. In particular, some PUMAs are equivalent to counties, whereas others include several counties, and still others are smaller than individual counties. Hence, we calculate the population-weighted average of the county values of the SC variable within each PUMA, similar to the approach taken by Alsan and Yang (2018) and Watson (2013).[22] We show an example of this geographic mapping in Appendix Figure (A1). This figure displays the counties, county populations, and associated weights for each county based on their percentage of the total PUMA population, which we use to calculate the population-weighted measure of SC. Additionally, we have no information about the month of survey within the ACS, only the year of survey, so we assign to each observation the SC policy in January of the survey year and test the robustness of this choice. The primary outcome variable in the ACS is high-educated (i.e. college-educated) U.S.-born females' usual hours worked per week in the past year (including 0 hours), and we also explore how work changes in more detail by looking at whether the individual worked *any* positive hours usually in the past year, as well as binned hours to capture full-time and part-time work.

We hypothesize that SC will reduce high-educated females' labor supply through increases in the cost of services that substitute for household production (Cortes and Tessada, 2011). This price increase will be caused by a reduction in the labor supply of immigrants through three potential channels, as discussed above: 1) forced out-migration of immigrants, 2) changes in cross-border migration (either reduced voluntary in-migration or increased voluntary out-migration), and 3) reductions in immigrants' labor supply among those that remain in the U.S. due to fear (chilling effects). As shown in Appendix Table (A2), among

[22]There are about 1000 PUMAs and 3000 counties in the U.S. If a PUMA is equivalent to a county, or smaller than a county, the PUMA will get the value of the SC variable for that county. If multiple counties are contained within a PUMA, we weight the value of the SC variable for each county by the fraction of the total PUMA population that each county represents. Additionally, the PUMA codes were revised after the 2011 ACS survey, so we use the time-consistent version of the PUMA codes provided by IPUMS. We also have checked the robustness of our results to using the county codes available in the ACS to cluster the standard errors since this is the level of policy variation. County codes are only available for about 60% of the sample (those living in large counties), so this limits our sample size, however the results with this subsample, not shown, are very similar whether we cluster by PUMA or county.

all individuals deported, 21 percent were not convicted of a crime, and 61 percent had a non-violent crime as the most serious offense, which supports the idea that chilling effects may have existed due to the broad nature of the population affected.[23] Qualitative evidence further suggests that SC disrupted the well-being of both citizens and non-citizens living in immigrant communities (Amuedo-Dorantes, Puttitanun and Martinez-Donate, 2018). For example, interviews of Latinos living in Cook (Chicago), Harris (Houston), Los Angeles, and Maricopa (Phoenix) counties found that 78% of undocumented immigrants think police officers stop Latin immigrants without reasonable cause, 61% are afraid of leaving their home, and 62% feel more isolated because local law enforcement is involved with immigration enforcement (Theodore, 2013). Moreover, the expansion of the geography of deportability from traditional locations (for example, the U.S.-Mexico border) to non-traditional locations (for example, grocery stores and traffic stops) reduced immigrants' participation in school and work, as well as their healthcare usage (Valdivia, 2019).

To provide direct evidence that the cost of household services is an important mechanism, we look at employment and wages for workers in these services. We construct a sample of individuals ages 20-63 who report that their occupation at their current or most recent job was housekeeping or childcare services. We start by estimating the direct effect of SC on the total number of female likely undocumented individuals working in household services, as well as the total hours they supply. Then we look at the average hourly wage of female household service workers. Since the ACS does not have information about undocumented status, we follow previous literature and define likely undocumented workers as foreign-born, Hispanic individuals with less than a high-school degree (Van Hook and Bachmeier, 2013; Passel and Cohn, 2014; Borjas and Cassidy, 2019; Albert, 2021).[24]

[23]The TRAC data used in this table is described in the Appendix. To classify violent crimes, we followed the definitions of the FBI's Uniform Crime Reporting (UCR) Program (https://ucr.fbi.gov/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/violent-crime). The top violent crimes leading to a deportation in this period were assault, violent burglary, domestic violence, and robbery. The top non-violent crimes are those reported in the table and are related to DUIs and other traffic offenses, non-violent property crimes, and marijuana related crimes.

[24]We do not use citizenship status in this classification because of concerns about misreporting (Van Hook

Given our focus on household service workers, and thus on female likely undocumented workers, it is worth noting that even though females were unlikely to be deported under SC, it still had an important impact on their behavior through chilling effects that plausibly translated to declines in their labor supply. Rhodes et al. (2015), for example, find that after the implementation of 287(g) agreements, Hispanic and Latina mothers used prenatal care later in their pregnancy because of fear and mistrust of using health services. Additionally, female immigrants reported feeling scared of being targeted by law enforcement while driving, and that prevented them from going to work: "I remember sometimes I would have to go to work, and I would say, 'No, I'm not going to be able to go to work right now because there's a (DUI) checkpoint right in front of my house. So I'm not going to put myself in a deportation situation" Valdivia (2019).[25] This is supported by the findings in Amuedo-Dorantes and Antman (2021)–that more deportations in a local-area reduce likely undocumented immigrants' labor supply, particularly among females and mothers with young children, who may especially fear issues of family separation.

Since our sample period spans the Great Recession, we account for changes in economic conditions that may influence labor supply by adding to the data several "Bartik-style" measures of labor demand, as well as local-area housing price information.[26] We prefer to use these Bartik-style measures of labor demand, rather than more direct measures, such as the local unemployment or labor force participation rates, because these direct measures may be affected by SC. Our preferred specification also adds controls for pre-trends in local economic conditions described below. These account for the fact that different areas experienced the recession differently. In addition, during this period, another local interior enforcement

and Bachmeier, 2013; Brown et al., 2018).

[25]Concerns about domestic violence also particularly affect women. Amuedo-Dorantes and Arenas-Arroyo (2019) find that increased immigration enforcement reduces the likelihood females file for a VAWA petition, which is available to victims of domestic abuse to change their immigration status. Additionally, Grittner (2019) find increases in immigration enforcement reduced Hispanic females' usage of domestic violence services.

[26]Controlling for economic conditions may also be important since they affect migratory decisions (González, 2015). Therefore, we account for economic conditions when estimating the effect of SC on the labor supply of likely undocumented individuals as well.

policy–287(g) agreements–changed across areas, so we control for the presence of local 287(g) agreements.[27] Details on these control variables are included in the Appendix.

Summary statistics for the ACS sample are in Table (1). We use the survey-provided person weights in all summary statistics and regressions. We have over 2.5 million high-educated female observations for the period between 2005 and 2014. We multiply the dichotomous labor supply outcome variable by 100 to ease presentation of the results. At the bottom of this table, we also show descriptive statistics taken from the American Time Use Survey (ATUS) in 2005–before SC–for a sample of U.S.-born females aged 20-63 with a college degree or more, for two measures of household production: 1) time spent caring for household children (e.g. feeding them, socializing with them and time spent on activities related to their education) and 2) time on household activities (e.g. maintaining the respondent's household, housework, cooking, and home maintenance). These statistics reinforce the idea that females, especially mothers, spend more time on average in these types of activities relative to males.

3 Empirical Strategy

Our identification strategy exploits both the geographic and temporal variation in the implementation of SC to identify its effect on the labor market outcomes of high-educated U.S.-born females. Our main analysis examines the effect of SC on contemporaneous labor supply and is estimated with the following model:

$$Y_{ipt} = \alpha + \beta SC_{pt} + X'_{ipt}\delta + Z'_{pt}\gamma + \mu_p + \phi_t + \theta \Delta W'_p * t + \epsilon_{ipt} \tag{1}$$

Y_{ipt} represents different measures of labor outcomes for an individual i, living in PUMA p and observed in year t. SC_{pt} is a continuous variable measuring exposure to SC at the

[27] Two papers examine the labor market effects of 287(g) agreements (Bohn and Santillano, 2017; Pham and Van, 2010). However, these papers do not separate the effects by country of birth or sex of the workers.

PUMA level and takes values between 0 and 1. SC_{pt} is equal to zero if SC has not been implemented by January of the survey year in any of the counties in PUMA p and is equal to one once SC has been implemented in all counties in the PUMA by January of the survey year. Since we focus on the roll-out period of SC, once SC_{pt} takes a value equal to one, it keeps that value for the remainder of the sample period. The coefficient of interest, β should be interpreted as the effect of SC when the entire population in a PUMA is exposed to SC by the beginning of the survey year.

We include PUMA-level fixed effects (μ_p) to absorb time-invariant heterogeneity across PUMAs. We also include year fixed effects (ϕ_t) to account for national shocks to labor outcomes over time. For the difference-in-differences model to be valid, there should not be time-varying changes across PUMAs that are correlated with the timing of the adoption of SC. In addition to directly testing this assumption as described above, we test the robustness to adding in controls at the PUMA-year level (Z'_{pt}), which includes Bartik-style measures of labor demand and 287(g) agreements. Following Hoynes and Schanzenbach (2009) and Almond, Hoynes and Schanzenbach (2011), to control for pre-trends, we interact changes in PUMA characteristics between 2000 and 2005 (vector $\Delta W'_p$) with linear time trends. Importantly, to account for pre-trends in economic conditions, this vector includes changes in the PUMA-level labor force participation rate, unemployment rate, and the size of the housing boom (Charles, Hurst and Notowidigdo, 2018).[28] Finally, we include individual-level controls in X'_{ipt}: age, age squared, race, marital status, educational attainment, number of children and number of young children in the household.[29]

[28]The complete set of PUMA-level changes are labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, Black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, masters degree, or a Ph.D., as well as the same education categories just for females. The results are robust to using only the levels in 2000 or 2005 interacted with a time trend (results available upon request).

[29]Fertility may be directly affected by enforcement if the price of having children changes (Furtado, 2016). We directly test for this and find no evidence of changes in fertility as shown in Appendix Table (A3). Note, the sample size is slightly smaller in this analysis because this fertility question is only asked to females ages 15-50 in the ACS.

4 Results

We begin our analysis in Table (3) by showing the effects of SC on the labor supply of all high-educated U.S.-born females (column (1)), as well as all mothers (column (2)), mothers with young children (column (3)), and females without children (column (4)).[30] Focusing first on Panel A, this model includes only PUMA and year fixed effects. We find negative point estimates for all subsamples, but the largest (and only statistically significant) effects are found for mothers of young children: SC reduced their usual hours worked (including 0s) by 0.440 hours, a 1.5% reduction relative to the sample mean (p = 0.01). For females without children, we find the smallest point estimate (-0.06 or -0.17%) and it is not close to statistical significant (p= 0.51). This pattern suggests that the effect of SC on females is differentially more negative for those with the highest demands on their household production time.

Moving through the subsequent panels, we include additional controls. First, Panel B adds controls for 287(g) agreements and economic conditions (Bartik-style variables). Next, Panel C further includes interactions between changes in the PUMA characteristics from 2000 to 2005 and a linear trend. Finally, Panel D adds individual-level demographic characteristics. The stability of the estimates to these additional controls further supports the idea that the timing of SC adoption is plausibly exogenous.[31]

To further test the validity of our identification strategy, we estimate an event study model with PUMA and year fixed effects.[32] Specifically, we estimate the following equation:

$$Y_{ipt} = \alpha + \sum_{j=-5, j\neq 0, j\neq -2}^{4} \beta_j 1(SC_j = 1) + \mu_p + \phi_t + \epsilon_{ipt} \tag{2}$$

[30] We define mothers as those with children living in their household. So, some of the females without children may have children who no longer live with them.

[31] We have also explored the robustness of our results to the following controls: adding region by year fixed effects; changes in the PUMA characteristics between 2000-2005 interacted with year fixed effects instead of a linear time trend; and PUMA-level economic conditions in levels in 2000 interacted with year fixed effects. The results on mothers with young children are robust to all of these additional controls.

[32] Results are nearly identical if we include the control variables discussed above. We exclude them here to test the identification assumption on the sparsest model.

The key difference between this and equation (1) is that we replace the continuous measure of SC (SC_{pt}) with dummy variables indicating how far each PUMA-year observation is from the year the PUMA first implemented SC ($1(SC_j = 1)$). This is so that we have a single year that SC "turns on" to define event time. For example, β_1 represents the outcome of interest in the year of SC implementation, and β_{-1} represents the outcome of interest two years before implementation. We omit β_0, which is the coefficient representing the year before implementation and β_{-2}, which is the coefficient representing 3 years before implementation.[33] We define the year the PUMA implemented SC as the first year that any county in the PUMA adopted SC. There are some PUMAs that experienced a phase-in of SC over a period of multiple years, as SC rolled out across counties in the PUMA, so we may see a phase-in of the effect of SC across event time as well. Because of this, the magnitude of the estimates in the event study model are not directly comparable to those in the difference-in-differences analysis in equation (1). However, the advantage of the event study approach is that it allows us to test our key empirical assumption–conditional on PUMA and year fixed effects, the timing of SC adoption is unrelated to trends in outcomes across PUMAs.

Figure (2) shows the results of the event study model for all mothers (Panel (a)), mothers with young children (Panel (b)), and females without children (Panel (c)). These figures show no evidence that, prior to SC adoption, the labor supply of any of these groups was differentially trending across PUMAs. Moreover, there is strong evidence of significant negative effects of SC after implementation on the labor supply of mothers with young children, and this appears to phase-in over time – mimicking the strong negative effects we saw in Table (3) for this group. As in our difference-in-differences results, for all mothers, there is suggestive evidence of a decline in hours worked after SC implementation, whereas

[33] In our setting, all the PUMAs are eventually treated, so we have two options to avoid issues of multi-collinearity with the event time dummies, calendar time fixed effects and PUMA fixed effects (Schmidheiny and Siegloch, 2020). The first is to bin either pre or post event time dummies or both, and the second is to omit two pre-period dummies. We have opted for the latter in order to show as many event time dummies as possible on the figure. Results are similar with binning pre-period dummies instead.

for females without children there is much less evidence of any negative effect from SC.[34] These results provide further support for the validity of our identification assumption.

While this phase-in of effects could represent dynamic treatment effects, and therefore indicate that the difference-in-differences results may be biased (Goodman-Bacon, 2021), it is important to note this could also be because SC is phased in across counties within the PUMAs at different times. To test whether heterogeneous treatment effects (including dynamic ones) are causing bias in our results, we have implemented the Bacon decomposition. This exercise indicates over 70% of our main estimates on mothers with young children are driven by "good" comparisons that use early treated units as the treatment group and later treated units as the control group; these comparisons will not be biased by heterogeneous treatment effects (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). In addition, we implement the new estimator proposed by Callaway and Sant'Anna (2021), which avoids using earlier treated units as controls for later treated units and is robust to heterogeneous treatment effects. Figure (A2) shows the results of our baseline event study model (black dots) along with the results from the Callaway and Sant'Anna estimator (gray dots). The results are very similar with this alternative estimator. This is strong evidence that our main estimates are not biased due to these issues, and if anything, any small bias will push the estimates *towards* a null effect.

In what follows, we focus on the results from the specification in Panel D of Table (3) because it includes all the PUMA and individual-level controls. The measure of hours worked in Table (3) includes zeros, so to get a sense of how much the change in hours might be explained by a change in the extensive margin versus the intensive margin, we do a number of things. First, we estimate the effect on the likelihood of working at all (extensive

[34] Unfortunately 2005 is the first year that PUMAs are identifiable in the ACS, so we cannot extend our pre-period back further. The event study model includes all possible dummy variables, but we only report back to -5 and up to $+3$ in the figures, as these are estimated on a sample of nearly balanced PUMAs–we can observe -5 for 916 of 1078 PUMAs, and +3 for 742 PUMAs. On the other hand, we only observe 585 PUMAs at event time -6 and 493 PUMAs for event time +4.

margin) and the hours worked, if working a positive number of hours (intensive margin). Columns (1) and (2) of Table (4) show these results for all mothers (Panel A), mothers of young children (Panel B) and females with no kids (Panel C). The results in column (1) show a significant reduction in the likelihood of working at all for all mothers, and mothers of young children. Recall that the dichotomous outcome variable has been multiplied by 100, so the magnitudes indicate a reduction of 0.54% relative to the sample mean for all mothers and 0.99% for mothers with young kids. As with hours worked, we find a smaller and insignificant effect on females with no children in the household. There is little consistent evidence of changes in hours worked conditional on working (column 2). This could mean the labor supply adjustment is primarily at the extensive margin, where we do see a significant decline, or could mean that any intensive margin adjustments are contaminated by selection into which mothers are choosing to stop working. For example, if SC causes a decline in the intensive margin (hours worked among workers), but, at the same time, SC causes workers who work fewer hours on average to drop out of the labor force (increasing average hours among workers), this could lead to a null effect of SC on hours conditional on work.

We also conduct a back of the envelope calculation to get a sense of how much the change in total hours worked, inclusive of zeros, may be due to extensive margin changes. To do this, we multiply the estimated change in the likelihood of working by the average hours worked among those working. This predicts a decline in total hours worked for mothers with young children of 0.28 hours ((-0.783/100)*35.22), which suggests that the decline in the probability of working on the extensive margin may explain about two thirds of the total change in hours worked (0.28/0.421). However, this calculation relies on the assumption that the marginal mother dropping out of the labor force worked the average number of hours prior to dropping out.

We next investigate the effects on the probability of working full-time (35+ hours), part-time (20-35 hours), and being marginally employed (0-20 hours, *inclusive* of 0 hours) in columns (3)-(5) of Table (4). Full-time work may be more affected because outsourcing household production may be more important for mothers who work longer hours (Cortés and Pan, 2019). The results suggest that indeed most of the change in labor supply of mothers is coming from a reduction in the likelihood they work full-time. For mothers with young children, there is a reduction in full-time work of 1.9% (p=0.03).

Finally, the results above indicate the biggest contemporaneous effects of SC are when the youngest child is ages 0-4, and we further investigate heterogeneity in the effect by child age within this group in Table (5). SC has the largest negative effects while the youngest child is under age 3 (Panel A), which is consistent with the fact that childcare before age 3 is more expensive and higher-quality care is harder to find for children younger than 3 (Jessen-Howard et al., 2018; Workman and Jessen-Howard, 2018). Overall, this suggests that SC is particularly impactful for mothers of young children working full-time jobs and may have important implications for the potential career progression of mothers in very time-intensive jobs (Bertrand, Goldin and Katz, 2010).

4.1 Impact of SC on Household Service Occupations

To better understand whether changes in the cost of outsourcing household work are driving the negative effect of SC on high-educated mothers, we conduct several tests. First, we directly examine the effects of SC on the labor market outcomes of likely undocumented female workers in household services. We start with our main definition of likely undocumented immigrants: low-educated (less than high-school), Hispanic foreign-born. In addition, we explore restricting the sample further to those born in Mexico and Central American countries (most undocumented immigrants are from these countries), as well as immigrants from those countries who arrived in the U.S. after 1980.[35] Importantly, any measurement error in our

[35]We follow Passel and Cohn (2014), Warren (2014), Passel and Cohn (2016) and Borjas and Cassidy (2019) in choosing 1980 as the cutoff year. Warren (2014) explains that immigrants who arrived before 1982 were likely given authorization under IRCA, and that because of misreporting in the year of arrival,

definition of likely undocumented should not impact our estimates on the labor supply of high-educated female U.S.-born.

We explore whether the policy affected the total number of likely undocumented female immigrants working in household services, as well as the total number of hours they work in household services. To construct the dependent variables, we sum the number of working-age likely undocumented females working in household services, and the number of hours these individuals work by PUMA and year. We then divide each of these PUMA by year totals by the total population in each PUMA and year, and finally multiply by 100 to ease the presentation of the results.[36] We construct these outcomes and estimate the models at the PUMA level, rather than the individual level, for two main reasons. First, the goal of this exercise is to provide evidence on the effects of SC on the household services market, and we do this by estimating the effect on the *total* number of workers and hours in the market. Second, changes in the composition of workers in household services could affect the estimates on hours if the variable is not calculated at an aggregate level. For example, assume that immigrant workers who stay working in household services in the U.S. reduce their working hours in response to SC. And, some immigrant workers drop out of the sample of household service workers in the U.S. (because of migration or they no longer work in these occupations) and those who drop out worked on average fewer hours relative to the rest of the sample. The analysis at the aggregate level that sums the total hours worked in each PUMA and year captures reductions in working hours driven by both responses–the total effect in the market. However, if we were to only look at *average* hours of work *within* the sample of household workers in the U.S. by PUMA and year, the reduction in hours worked

as well as heaping in the data, 1980 is a reasonable choice to proxy for those who would have gained legal status. The results are similar with other choices for the year of arrival cutoff. Since we cannot perfectly identify undocumented immigrants, measurement error may affect our results. There is an undercount of undocumented immigrants in surveys conducted by the U.S. government (Passel and Cohn, 2011; Hoefer, Rytina and Baker, 2012; Warren and Warren, 2013; Warren, 2014; Van Hook et al., 2014; Genoni et al., 2017; Brown et al., 2018) and this may attenuate the effects towards zero if the undercount is random. Of more concern is endogenous changes in reporting in response to SC. Unfortunately, we cannot directly assess whether the measurement error varies in response to SC.

[36] We weight these models by the 2000 PUMA population.

among those remaining in the sample would result in a negative estimate, but the exit of workers with few hours of work from the sample would result in a positive estimate (due to selection). In sum, in the aggregate model, both effects would push the coefficient in the same direction, while in the individual model they would push in opposite directions.

Panel A of Table (6) shows the effect of SC on the number of household service workers from the following populations: low-educated Hispanic foreign-born (column (1)), low-educated Hispanics born in Mexico and Central America (column (2)), and those in column (2) who arrived in the U.S. after 1980 (column (3)). There are small and insignificant effects across the three samples. As mentioned before, this is not surprising given that the vast majority of deportations were of male immigrants.[37] These results also suggest that the number of likely undocumented female workers in household services did not change due to voluntary cross-border migration, occupational switching, or moving in or out of the labor force.[38] We next examine the effect on total hours worked in Panel B. The results show that SC reduced hours worked by roughly 8% and this is consistent across the different samples (columns 1-3). To test the parallel trend assumption, we estimate an event study model following equation (2). Figure (3) shows the event study corresponding to the results in column (1) of Panel B. Reassuringly, there is no evidence of differential pre-trends before SC and negative effects on hours after SC, consistent with the difference-in-differences estimates.

Given the decline in the intensive margin labor supply among those who remain in the U.S, we hypothesize that chilling effects is an important mechanism. To test this, we examine whether the negative effect on working hours of likely undocumented female workers in household services is more negative in places with plausibly greater intensity in the chilling effects of SC. Specifically, we examine if the effects are bigger in places with: a larger undocumented immigrant community; a larger share of deportations of Hispanic immigrants or

[37]It is possible that female immigrants whose spouse/partner was deported might work more to make up for the lost family income, but we see no evidence of increases in labor supply on net.

[38]We find no evidence that SC affected the likelihood of internal migration for our sample of foreign-born females working in household services–results shown in Appendix Table (A4).

immigrants from Mexico or Central American countries; and a larger share of deportations of individuals not convicted of a serious crime, similar to Alsan and Yang (2018).[39] Empirically, we add to the model an interaction term between SC and these measures. We start by estimating the interaction effect between SC and the share of likely undocumented workers in a PUMA. We expect awareness among the immigrant community about ICE and the likelihood of being deported to be more salient in places with a larger undocumented population. The results in Panel A of Table (7) support this hypothesis. To ease the interpretation of these results, we report the estimated effect evaluated at the mean of the intensity measures and at one standard deviation above their mean. For example, in column (1) of Panel A, we find a 0.38 percentage point reduction in hours worked at the mean share of low-educated Hispanic foreign-born (a 6.4% decline relative to the mean). In a PUMA with intensity one standard deviation higher, the effect is a 1.07 percentage point (18%) decline. These results are similar with the other definitions of likely undocumented in columns (2)-(3).

Next, we expect PUMAs with more deportations of Hispanic (Panel B), Mexican or Central American migrants (Panels C-D) could be areas with a higher prevalence of racial profiling, which would also lead to larger fear-related chilling effects. The results are consistent with this hypothesis. Finally, we expect chilling effects to be larger in places where more deportations are of individuals without serious criminal histories; indeed, there is a more negative effect in these PUMAs (Panel E). It is important to highlight that we interpret this evidence as suggestive, given that these deportation intensity measures may be endogenous. For example, they may be correlated with local attitudes about immigration–and the results should be interpreted with this caveat in mind.[40]

[39]We define non-serious crimes as immigration-related crimes, marijuana-related crimes, DUIs, and traffic violations. We define deported individuals as Hispanic if they list their country of citizenship as a Spanish-speaking country excluding Spain. Finally, the sample size when using the TRAC data for the intensity measures is slightly smaller because some PUMAs do not have information about deportations in the TRAC data. Note that the mean shares in Table (7) does not perfectly match the mean shares in Appendix Table (A2) because we weight the means in Table (7) with the survey weights and the means in Appendix Table (A2) are unweighted.

[40]In order to alleviate concerns related to possible endogeneity of these intensity measures, we check the robustness of the results in Table (7) to controlling for pre-SC crime rates and detention rates. If we are

It is possible U.S.-born workers substitute for undocumented workers, in which case the total effect on labor supply in this market could be smaller than the direct effects on likely undocumented immigrants only. We explore this possibility in Appendix Table (A5), and find that overall there is a statistically significant reduction (p-value=0.03) in the total hours supplied in this market from *all* low-educated female workers (column (1)). Importantly, we find no evidence of significant negative effects for U.S.-born workers as shown in columns (2)-(4) of this table, and the point estimates are smaller than for likely undocumented.[41] So, there is no evidence of natives substituting for undocumented workers.

Given the reduction in the overall labor supply that is driven by likely undocumented female immigrants, we expect to find an increase in female wages in these occupations, so we investigate this in Table (8).[42] Column (1) suggests that the hourly wages of all female workers in household services increased by 2%, although these effects are marginally insignificant. When we restrict the sample to low-educated female workers in column (2), where we expect the effect to be larger, we find a significant increase in hourly wages of 6.5%. Since household services are relatively labor intensive, changes in wages are likely a good proxy of the cost of outsourcing household production (Hock and Furtado, 2009). As points of comparison, Furtado (2016) finds that a 1% change in the low-skilled immigrant population in the U.S. reduced the median wage of childcare workers by about 4%, and Cortes (2008) finds that a 10% increase in low-skilled immigrants reduced the price of immigrant-intensive services (mostly household services) by roughly 2%. Our results suggest that a 1% decrease

picking up general trends in criminal activity or attitudes towards immigrants across local areas, then adding these controls would weaken the interaction effects. The results in Table (7) are very similar when adding these additional controls (results available upon request).

[41] Comparing our estimates on likely undocumented hours supplied to total hours supplied, we conclude that likely undocumented workers' changes in labor supply account for the majority of this reduction, and the confidence intervals overlap. The U.S.-born group that may be reducing their labor supply is Hispanics, which is consistent with evidence from the chilling effect literature discussed above that shows negative effects of SC on Hispanic *U.S.-citizens*. In results not shown because of small sample sizes and imprecise results, there is suggestive evidence that any decline among U.S.-born is driven by those living with a foreign-born individual.

[42] The ACS does not measure hourly wages, so we construct this by dividing annual income by total hours worked in the year. The latter is the product of total weeks and hours worked in a usual week. We drop imputed income values.

in the working hours of likely undocumented females leads to a 0.86% (6.5/7.6) increase in hourly wages of low-educated females working in household services. We also test the parallel trend assumption within an event study framework, and Figure (4) shows these results. In both Panel A (all females) and Panel B (all low-educated females), we see no evidence of pre-trends and find suggestive evidence of positive effects after SC, though the estimates have large confidence intervals. One potential reason these effects are so imprecisely estimated relative to the difference-in-differences specification is because the event study model is a more demanding specification (Borusyak and Jaravel, 2017), and this is especially taxing given the small samples in our wage analyses.[43]

Building off these findings, we expect to see more negative effects on the labor supply of high-educated mothers in places that have greater concentrations of immigrants affected by SC. To test this hypothesis, we do the same intensity analysis as in Table (7) focusing on working hours (including zeroes) of U.S.-born mothers with young children. The results are shown in Table (9) and confirm our hypothesis. For example, the results of Panel C indicate that SC decreased working hours of high-educated U.S.-born mothers of young children by 0.41 hours in a PUMA with the average share of deportations of immigrants from Central America and Mexico (a 1.43% reduction relative to the sample mean). In a PUMA with intensity one standard deviation higher, the effect is a reduction of 0.65 hours (2.3% relative to the mean). When using other intensity measures, the pattern of the results persist, but they are not all precisely estimated.[44]

[43]In PUMA-years where there are no workers in these demographic groups and occupations, the observations are missing, which can bias the event study results, so to have a balanced sample of PUMAs observed from event time -5 to +3, we drop PUMAs where there are no workers in the relevant samples in any given year. We do this only for this wage event study analysis.

[44]Again, these results are robust to including controls for pre-trends in the crime rate and detentions in the PUMA. They are also robust to including the intensity measures interacted with the PUMA by year control variables.

4.2 Alternative Mechanisms

These findings strongly suggest that changes in the cost of outsourcing household services is an important mechanism through which SC affects high-educated mothers' labor supply; however, other mechanisms, such as complementarities in the production process of market work (Chassamboulli and Peri, 2015; East et al., 2021),[45] could affect high-educated individuals' work. To account for this, we compare our main results on mothers with young children to the effects on groups that are less likely to be impacted by changes in the cost of outsourcing household production.

Females without children are our first comparison group because they spend much less time in household production work than mothers (Table (1)), and, therefore should be less affected by changes in the cost of these services.[46] The point estimates on mothers with young children are more than *five* times larger than the estimates for females without children, as discussed above in Table (3). Additionally, because females without children work more on average compared to mothers, the *percentage* effect of SC on hours worked is about *six* times larger for mothers with young children than for females without children. Building off this, we implement a triple difference model using females without children as the comparison group to formally test the difference in the effects of SC across these two groups. For this specification, we include the SC variable (SC_{pt}), and we include this interacted with a dummy for whether the female individual has young children (treatment group) or has no children (comparison group): $SC_{pt} * MotherwithYoungChildren_i$. The coefficient on SC_{pt} is the effect of SC on the comparison group, and the coefficient on the interaction is the differential effect on the treatment group. We also include a treatment

[45] Market complementarities are less likely to affect high-educated females compared to males, because females work at lower rates in sectors that rely heavily on undocumented immigrant labor. We tab the percentage of likely undocumented workers by sector in 2005 and then split sectors into above and below median immigrant-intensity; only 22% of high-educated females (and mothers) work in above median immigrant-intensive sectors, compared to 41% of high-educated males (and fathers).

[46] We use the term "comparison", rather than control, because it is possible that household production costs also had a (likely smaller) effect on these groups. We do not look at mothers with lower levels of education since they are more likely to be affected by substitution in market production (East et al., 2021).

group dummy, PUMA by treatment group fixed effects, and year by treatment group fixed effects. If other mechanisms affect the treatment and comparison groups similarly, we can net out these effects with this approach. Another advantage of the triple difference model is that it allows us to include PUMA by year fixed effects to flexibly capture time-varying shocks to local labor market outcomes that are common across the treatment and comparison groups. When we include these fixed effects, we drop the SC_{pt} term. Of course, using a comparison group in a triple difference model assumes that there is no effect from the change in the cost of outsourcing household production on the comparison group, which we cannot completely rule out. Therefore, this triple difference estimate can be thought of as a lower bound of the effect of changes in the cost of household production on high-educated mothers (Cortes and Tessada, 2011). The results, shown in Panel A of Table (10), indicate that the main results for mothers with young children are similar with and without the comparison group, and remain marginally significant (p=0.09). Further, whether or not we include PUMA by year fixed effects (not included in column (1) and included in column (2)) the results are remarkably stable, indicating that no other common shocks to labor market outcomes are driving our results. Comparing the magnitude of the coefficient in the fully interacted model in column (2) to our baseline results in Table (3), we conclude that at least 72% of the effect of SC on mothers with young children is coming from the cost of outsourcing household production mechanism (-0.302/-0.421).[47]

We next explore using fathers as a comparison group. We first estimate the effect of SC on high-educated U.S.-born males in Table (11). The results show a small, negative, and imprecisely estimated effect across all subgroups. Importantly, we see no evidence that the coefficients are meaningfully bigger for fathers with young children, compared to all males or all fathers.[48] This is the opposite pattern as what we would expect if household production

[47]Cortes and Tessada (2011) conduct a similar exercise with highly skilled men as a comparison group and find that at least 20% of their estimated effect on highly skilled women comes from this mechanism.

[48]Note that we are not replicating the results from East et al. (2021) here because that paper uses a different sample and different outcome variables.

costs have an effect on males' labor supply decisions (and this is the opposite pattern to what we found for females). Comparing the magnitude of the effect for fathers and mothers with young kids further emphasizes the difference in the effect by sex; there is a 1.46% decline in hours worked (p=0.01) for mothers, whereas, for fathers, the coefficient indicates a 0.19% decline in hours worked and is insignificant (p=0.53). On the other hand, for the full sample and for all parents–who we expect will be less affected by the cost of outsourcing household production–there are less pronounced differences by sex.

Given these results, we use fathers with young children as a second comparison group in a triple difference model shown in Panel B of Table (10).[49] The pattern of results is very consistent with our baseline results, and to the results with the other comparison group. Using the estimates in column (2), the differential effect of SC on mothers with young children is significant at the 0.10 level, and this coefficient implies 90% of our main estimate is coming from the cost of outsourcing household production mechanism (-0.379/-0.421). Taken together, these results provide strong evidence that changes in the cost of outsourcing household production is a key mechanism through which SC affects high-educated mothers with young children.

4.3 Lasting Impacts of SC Exposure Around Childbirth

In the contemporaneous results above, we show that SC has the largest negative effect on mother's labor supply when their youngest child is under age 3. Previous evidence on the effects of motherhood have found persistent declines in labor market outcomes after having a child (Kuziemko et al., 2018; Kleven et al., 2019). So, we test whether having SC in

[49] There are tradeoffs to the choice of each comparison group. Females without children may be more like females with children – for example, in the occupations and industries they work in – making them a better comparison group. On the other hand, there may be shocks to labor supply that are common to both mothers and fathers of young children, so fathers of young children may be a better comparison group. The sample size in column (2) Panel B is slightly smaller than column (1) Panel B because PUMA and year cells that only have one observation are dropped.

place around the time of a child's birth has lasting negative consequences on mothers' labor market outcomes. Specifically, we examine the longer-run effects of exposure to SC when the youngest child is between 0 and 2. We do this on a sample of mothers whose youngest child was born between 2000 and 2011, and whose youngest child is observed at ages 3-5 and 6-7 in the 2005-2018 ACS samples. There are two reasons we chose 2011 as the last birth cohort in this sample: 1) children who were born after 2012 would be exposed to the ending of SC in the first two years of life, and we focus only on the program roll-out, and 2) restricting the last birth year to be 2011 means we will observe all birth cohorts at ages 3-7. It is important to note that because of these differences in sample definitions and survey years included compared to the contemporaneous model, we are using slightly different policy variation here, so we do not directly compare these results.

We estimate the following regression:

$$Y_{ipts} = \alpha + \gamma_1 SC02_{ps} + \gamma_2 SC3plus_{ps} + X'_{ipt}\delta + Z'_{pt}\gamma + \mu_p + \phi_t + \lambda_s + \theta \Delta W'_p * t + \epsilon_{ipts} \quad (3)$$

where Y_{ipts} represents the labor outcomes for a woman i, living in PUMA p and year t, who had their youngest child in year s. $SC02_{ps}$ is the sum of annual PUMA-level exposure to SC when the youngest child was 0, 1 and 2, so it can take on a value between 0 and 3. Therefore, γ_1 should be interpreted as the effect of one additional year of exposure to SC before the youngest child turns 3. We also control for $SC3plus_{ps}$, which is the sum of annual PUMA-level exposure to SC when the youngest child is age 3 to the age when surveyed. Note, a limitation of this analysis is we only observe PUMA of residence, rather than PUMA of the child's birth, which may introduce measurement error, but we do not think this is severe as discussed below. In addition to the controls specified in equation (1), we add a youngest child birth year fixed effect, λ_s.[50]

Table (12) shows the results for usual hours worked, and we also report the birth

[50]The only controls measured based on the year of the youngest child's birth are the controls for 287(g) exposure at ages 0-2 to mimic the measure of SC exposure.

cohorts included in each sample and the survey years included. Panel A shows the effect of SC exposure during the first two years of life of the youngest child, when the youngest child is *observed* between 3 and 5 years old. We find negative, lasting, effects of exposure to SC around childbirth at these ages. When we observe children at older ages (and thus even longer-run effects) in Panel B, the point estimates become insignificant, which could indicate a fading out of the effect when children reach school age.

To get a sense of the severity of the measurement error due to migration, we do a number of things including looking at the migration rates of these samples, testing whether migration changed in response to SC, and conditioning on a sample of non-migrants. As shown in Appendix Table (A4), the mean across-PUMA migration rate of mothers with young children is low (0.13 percent) and there is a very small and insignificant effect of SC on this migration rate.[51] Finally, we estimate equation (3) on the sample who report living in the same PUMA in the year prior (about 96% of the full sample) and therefore may be subject to less measurement error issues. The results in Appendix Table (A6) show that the long-run effects are quantitatively very similar without migrants.[52] Therefore, we do not view this measurement error as severe.

Finally, we estimate the same model for fathers. Appendix Table (A7) shows there are no significant effects, although the estimates have overlapping confidence intervals with the results for mothers. Comparing the point estimates, there is a significant 1.3% reduction in hours worked for mothers of children 3 to 5, compared to an insignificant 0.3% reduction in hours for fathers. We believe it is unlikely any mechanisms besides changes in the cost of outsourcing household production would drive the long-run effect for mothers *or* fathers. This is because in this model we are estimating the differential effect of SC exposure around the birth of a child, and other mechanisms are unlikely to have differential effects based on

[51] We observe the PUMA the individual is living in at the time of the survey and the year prior.

[52] We have also re-estimated this model looking at the effect around childbirth omitting the early adopter PUMAs, since duration of SC exposure may be correlated with early adopter status, and again the results are quantitatively very similar. These results are available upon request.

the timing on childbirth.

Previous research has shown that gender gaps in labor market outcomes seem to be at least partially explained by the "motherhood gap" (Juhn and McCue, 2017), and that the convergence in the gender wage gap has been slowest for high-skilled workers (Blau and Kahn, 2017). Our results suggest that the cost of household services may be an important channel driving the gender gap in the labor market, particularly for high-educated workers. Future research should further explore this possibility with data that more precisely measures exposure to enforcement in a child's early life.

4.4 Robustness Checks

We test the robustness of our main results on the labor supply of high-educated mothers with young children. First, we check sensitivity to alternative timing assumptions since the ACS interviews are conducted throughout the year and we do not know the survey month. Appendix Table (A8) Panel A replicates the main results, and Panel B shows the results coding SC as the fraction of the year *before* the survey. The results are very similar.

Second, because early adopters of SC may be more selected, we test the robustness of the results to dropping PUMAs that had at least one county adopt SC in 2008-2009. These 131 "early adopters" PUMAs are shown in Appendix Figure (A3) and they are mostly along the U.S.-Mexico border. We re-estimate the event study model in equation (2)–the baseline estimates are in black and the estimates excluding early adopters are in gray in Appendix Figure (A4). The results are nearly identical with and without early adopters.

Finally, to examine how likely estimates of the magnitude we find would be due to chance, we implement a placebo test (Figure (A5)). To do this, we randomly assign county-level SC implementation dates 1000 times. Then, we aggregate each county-level data set up to the PUMA-level following the method described in section (2), and re-estimate equation

(1) for hours worked using this alternative SC variable. Only 0.07% of the distribution is to the left of our baseline coefficient from Table (3) shown in the red vertical line, so estimated effects of the magnitude we find are very unlikely to occur due to chance.

4.5 Discussion

Directly comparing our estimates to those in the related literature is not easy, as other papers look at how high-educated female workers' labor supply is related to the *quantity* of immigrants in a local area, whereas we demonstrate that SC changed labor supply decisions *among* immigrants living in the U.S. Nevertheless, it is informative to frame our results in light of these past findings. Cortes and Tessada (2011) use the closest sample to ours, and use a shift-share approach to identification. They find that a 10% increase in the low-skilled immigrant population in the U.S. was associated with an increase in hours of work by 0.3% among female workers earning wages at the top of the distribution.[53] Moreover, Cortes (2008) finds that a 10% increase in low-skilled immigration decreased prices of immigrant-intensive services by 2%. Combining these estimates, this suggests that there is an elasticity of high-skilled females' hours worked with respect to prices of -0.15. Comparing this to our findings, we find SC reduced hours worked among high-educated mothers of young children by 1.5% and increased hourly wages of low-educated female workers by 6.5%. Thus, our estimated elasticity of the labor supply of high-educated mothers with respect to the price of household services is -0.23.[54] Moreover, it is important to point out that our estimates demonstrate that it is not only the *number* of immigrants that affect high-educated females' decisions, as has been the focus of most of the prior literature, but also the political climate surrounding immigrants that can have these spillover effects.

[53] Cortes and Tessada (2011) define low-skilled immigrants as those with less than high-school education, who report either being a naturalized citizen or not a citizen.

[54] These elasticity calculations assume that the entire change in high-educated mother's labor supply is due to this change in price, and that wages are equivalent to prices.

In a different context, Farré, González and Ortega (2011) find that, in Spain, a 10 percentage point increase in the predicted number of female immigrants living in a local area increases the likelihood women with children or elderly dependents living with them work by about 2 percentage points. In the paper using an empirical approach more similar to ours, but in a very different setting, Cortes and Pan (2013) examine the effect of a series of policy changes in the 1970s to 2000s regarding foreign domestic workers in Hong Kong on women's labor supply. They identify the effects of these policy changes in several ways, including looking at long-run changes in the labor supply of women over the period of these policy changes. Mothers of young children increased their likelihood of working between 8 and 13 percentage points over time, relative to mothers of older children, and these effects are driven by higher-educated women. Finally, Monras, Vázquez-Grenno et al. (2019) document that a large wave of immigrant legalization and increased restrictions on informal work in Spain led to a decline in high-skilled women's work; for each one newly legalized immigrant, roughly 0.05 high-skilled citizen women stopped working. The authors argue this is due to the 22% increase in the cost of household services because of these policy changes.

A final point of comparison to our estimates is the literature studying the effect of childcare prices on mother's labor supply. Our results are consistent with two findings from this literature. First, this literature often finds larger elasticities for mothers with young children (Morrissey, 2017), consistent with theoretical expectations about greater household production demands before children reach school age. And, second our estimates imply employment elasticities with respect to wages of household service workers of -0.08 for all mothers, and -0.15 for mothers with young children, which is well within the range of estimated elasticities of mother's employment with respect to child care costs from this literature (Anderson and Levine, 1999; Blau, 2003; Morrissey, 2017).

5 Conclusion

This paper examines the spillover effects of a federal immigration enforcement policy on the labor outcomes of high-educated U.S.-born females. Given the prevalence of female undocumented workers in household services, a negative effect on their labor supply can affect the cost of outsourcing household production, and thus the labor supply of those most likely to outsource these services. Our empirical analysis supports this hypothesis.[55]

Exploiting the rollout of Secure Communities, we estimate difference-in-differences and event study models with time and location fixed effects. We find that SC reduced the labor supply of high-educated U.S.-born mothers with young children, who have the greatest demands on their household production time. Although our estimates measure short-term effects of exposure to SC, we find several pieces of evidence that suggest potential long-lasting effects: 1) the reduction in contemporaneous working hours seems to be driven by a decline in full-time work, and an increase in part-time work, which can harm potential career progression of women in time-intensive jobs (Bertrand, Goldin and Katz, 2010); and 2) exposure to SC around the birth of a child has negative and persistent effects on the labor supply of mothers. These results are robust to adding a wide variety of controls including time-varying local economic conditions, local-area characteristic trends, and individual demographics, as well as to using new methods that are robust to heterogeneous treatment effects.

To provide support for the hypothesis that changes in the price of outsourcing household services is an important mechanism behind the labor supply effects on high-educated U.S.-born mothers, we look directly at the total labor supply of likely undocumented workers in these occupations. We find that, while SC did not affect the number of likely undocumented females working in these occupations, it did reduce their total hours worked. This suggests the effects of SC on the labor supply of female likely undocumented operate through chilling

[55] Estimating the total welfare effects for U.S.-born females go beyond the scope of this paper. The overall welfare implications of our findings for U.S.-born females would need to balance the decrease in high-educated mothers' work, with the increase in household workers' wages.

effects. We next show that this reduction in hours was accompanied by an increase in the wages of all low-educated female workers in these occupations. Finally, to further support the increase in the cost of outsourcing household production as an important mechanism, we document that there were no similar effects for groups less likely to be affected by changes in the cost of outsourcing household production: high-educated females without children, and high-educated fathers.

This paper shows an important spillover effect of immigration enforcement policies onto native female workers. Understanding the full effects of enforcement policies is crucially important today, as immigration policy is being actively debated and changed. For example, recent policy proposals planned to give priority to high-educated immigrant workers (Holland and Rampton, 2019), but our results indicate that the labor supply of low-educated immigrants have positive spillover effects on the employment of high-educated native workers as well. Our paper also speaks to broader literatures that examine how policy can influence female labor supply and time spent in household production, especially around the birth of a child (see for example: Baker, Gruber and Milligan (2008); Baker and Milligan (2008); Cascio (2009); Havnes and Mogstad (2011); Rossin-Slater, Ruhm and Waldfogel (2013)). The decline in mother's labor supply as a result of SC may have far-reaching consequences to the gender gap in work and wages, as well as children's well-being. We view this paper as a first step to analyzing the full impact of immigration enforcement policies on high-educated mothers and their families' well-being.

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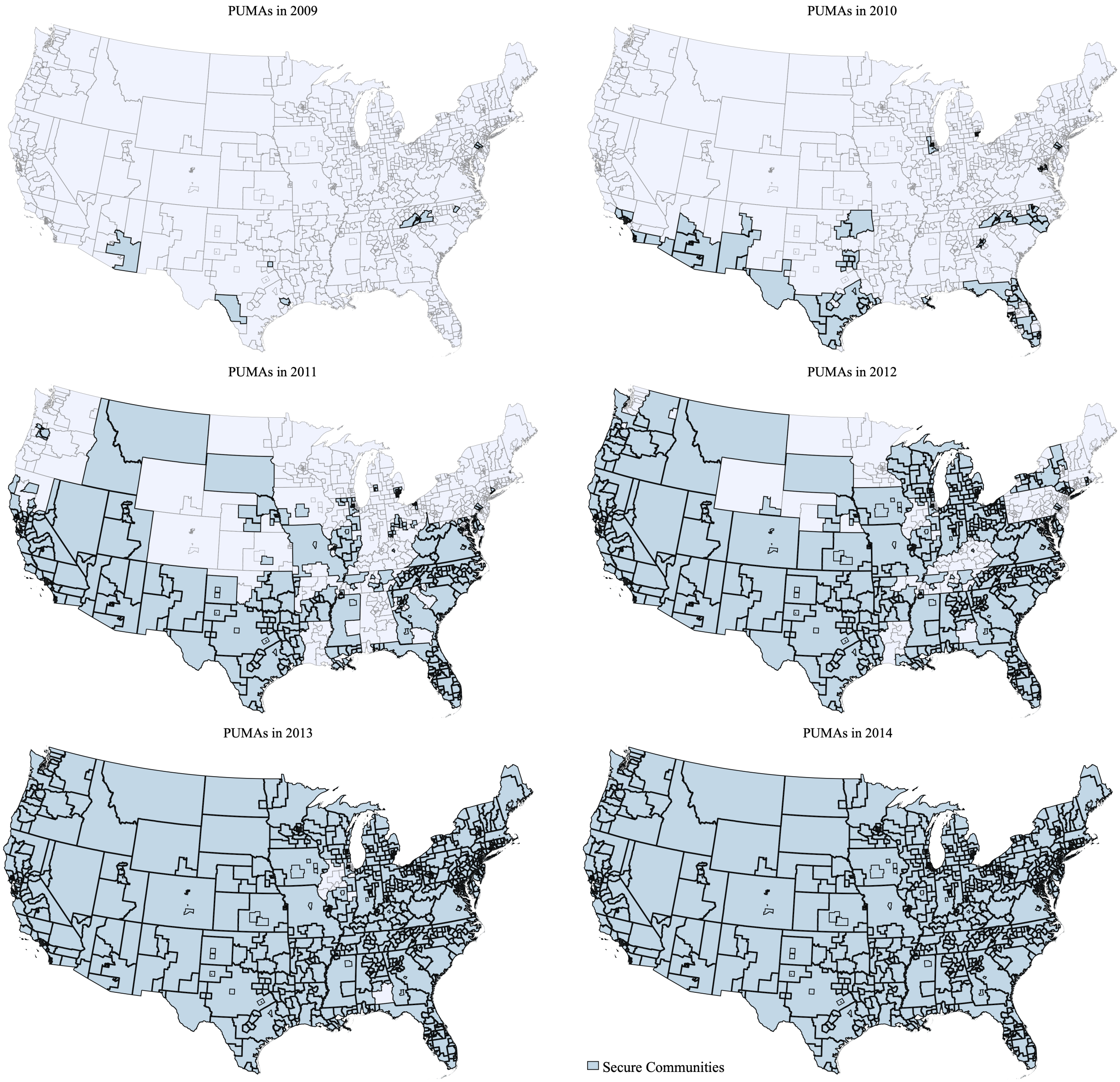
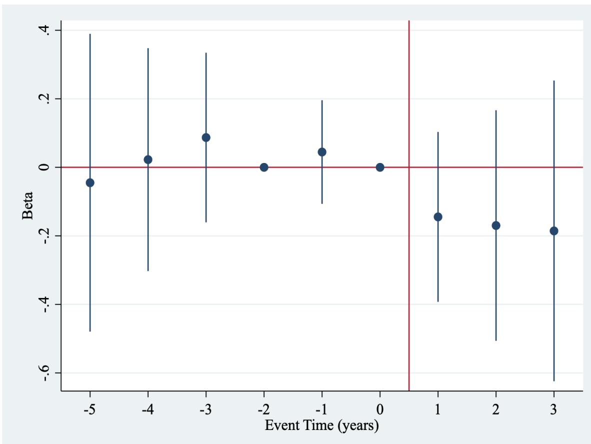
6 Figures

Figure 1: Rollout of Secure Communities at the PUMA level by Year

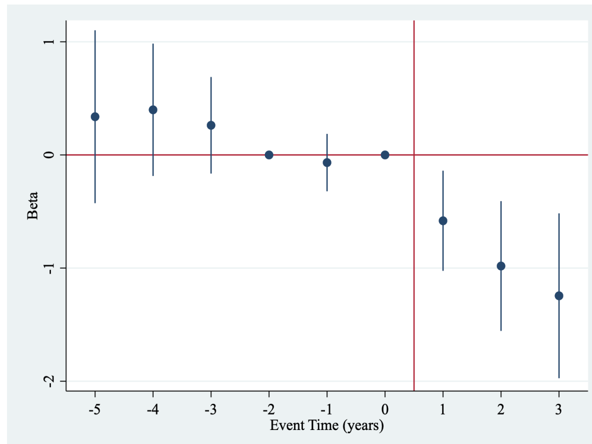


Notes: PUMAs that adopted Secure Communities by January of each year are shaded. See text for information on the data source.

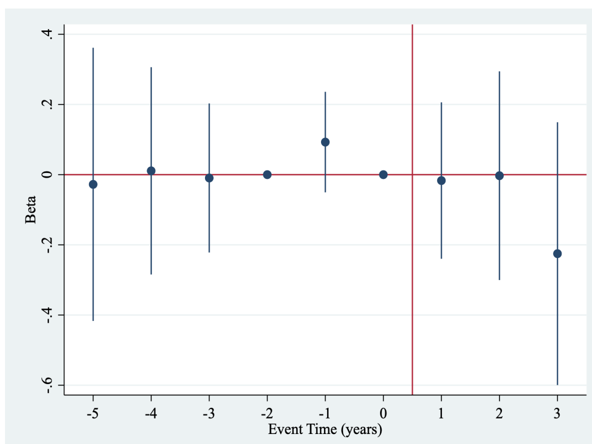
Figure 2: Effect of SC on High-Educated Females' Usual Hours of Work (Including Zeros) by Presence of Children in Household, Event Study



(a) Kids in the Household



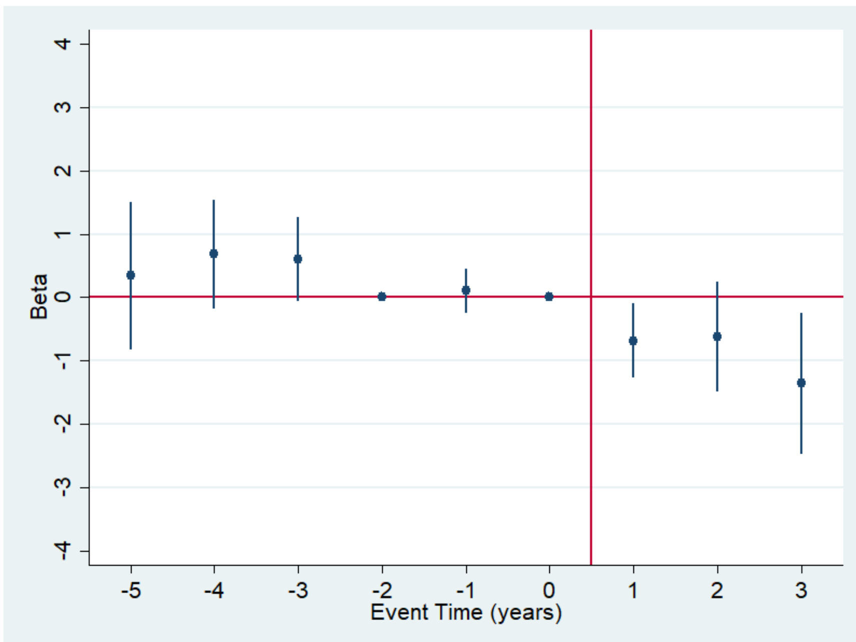
(b) Kids Under 5 in the Household



(c) No Kids in the Household

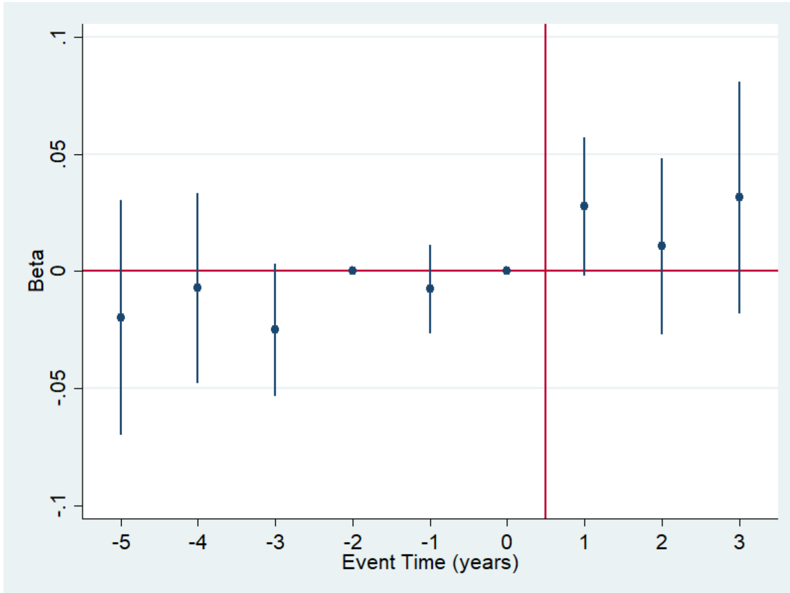
Notes: Data are from the 2005-2014 American Community Survey. The sample includes U.S.-born females with a college degree or more aged 20-63 and with subgroups denoted in the panel headings. The model includes PUMA fixed effects and year fixed effects. The results are weighted using the individual-level weights in the ACS. Standard errors are clustered at the PUMA level and the 95% confidence intervals are shown by the vertical lines. The horizontal axis denotes "event time" where the omitted years are the year before the first SC policy in the PUMA was implemented and three years before the first SC policy in the PUMA was implemented. We estimate effects for all possible event study time periods and therefore drop two pre-periods to be able to separately identify secular time trends from dynamic treatment effects (Schmidheiny and Siegloch, 2020). Additionally, we only display the coefficients from event time -5 to 3, as these are estimated on a sample of nearly balanced PUMAs–we can observe -5 for 910 of 1072 PUMAs, and +3 for 739 PUMAs. We only observe 582 PUMAs at event time -6 and 490 PUMAs for event time +4.

Figure 3: Effect of SC on the Total Hours Worked of Likely Undocumented Females in Household Services

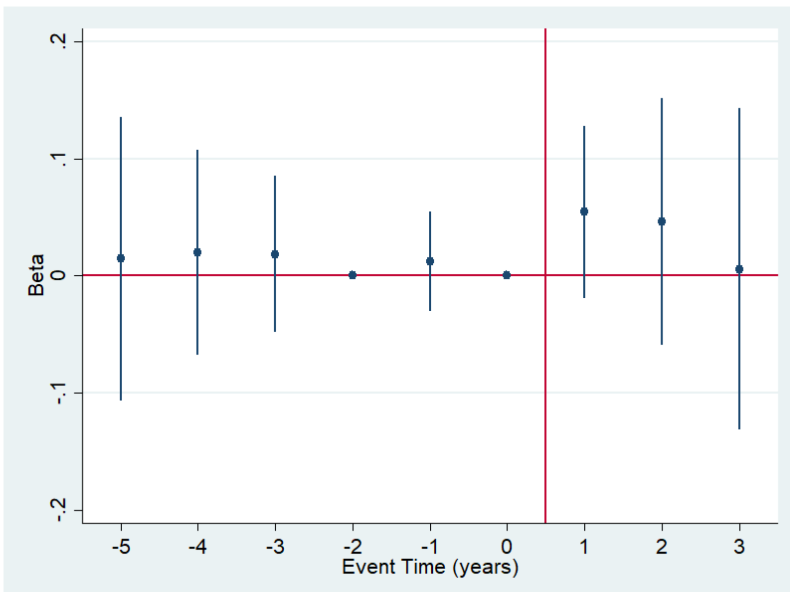


Notes: Data are from the 2005-2014 American Community Survey. The sample includes Hispanic foreign-born females with less than a high-school degree, aged 20-63, who report their current or most recent occupation as household services. We collapse the data to the PUMA by year level summing hours worked using the survey weights. The outcome variable is scaled by total PUMA by year population and multiplied by 100. The model includes PUMA fixed effects and year fixed effects. The results are weighted using PUMA population in 2000. Standard errors are clustered at the PUMA level and the 95% confidence intervals are shown by the vertical lines. The horizontal axis denotes "event time" where the omitted years are the year before the first SC policy in the PUMA was implemented and three years before the first SC policy in the PUMA was implemented. We estimate effects for all possible event study time periods and therefore drop two pre-periods to be able to separately identify secular time trends from dynamic treatment effects (Schmidheiny and Siegloch, 2020). Additionally, we only display the coefficients from event time -5 to 3, as these are estimated on a sample of nearly balanced PUMAs–we can observe -5 for 910 of 1072 PUMAs, and +3 for 739 PUMAs. We only observe 582 PUMAs at event time -6 and 490 PUMAs for event time +4.

Figure 4: Effect of SC on Log Hourly Wages of Household Service Workers



(a) All Females



(b) All Low-Educated Females

Notes: Data are from the 2005-2014 American Community Survey. The sample in Panel A includes all females aged 20-63 who report their current or most recent occupation as household services. Panel B restricts this sample to females with less than a high-school degree. The model includes PUMA fixed effects and year fixed effects. The results are weighted using the individual-level weights in the ACS. Standard errors are clustered at the PUMA level and the 95% confidence intervals are shown by the vertical lines. The horizontal axis denotes "event time" where the omitted years are the year before the first SC policy in the PUMA was implemented and three years before the first SC policy in the PUMA was implemented. We estimate effects for all possible event study time periods and therefore drop two pre-periods to be able to separately identify secular time trends from dynamic treatment effects (Schmidheiny and Siegloch, 2020). Additionally, we only display the coefficients from event time -5 to 3, as these are estimated on a sample of nearly balanced PUMAs–we can observe -5 for 910 of 1072 PUMAs, and +3 for 739 PUMAs. We only observe 582 PUMAs at event time -6 and 490 PUMAs for event time +4.

7 Tables

Table 1: Summary Statistics for High-Educated U.S.-Born Males and Females

	High-Educated Females				High-Educated Males
	All	With Kids	With Kids Under 5	Without Kids	All
ACS (2005-2014)					
Age	41.69	41.85	34.13	41.56	43.13
Black	0.09	0.09	0.07	0.09	0.06
Married	0.60	0.81	0.89	0.43	0.65
# Children Under 5	0.20	0.44	1.31	0	0.19
# All Children	0.84	1.84	1.95	0	0.82
College Degree	0.67	0.66	0.66	0.67	0.67
Master's Degree	0.26	0.26	0.26	0.26	0.22
Ph.D. or Professional Degree	0.07	0.07	0.08	0.07	0.11
Work >0 Hours (*100)	85.80	83.31	79.27	87.90	93.02
Usual Hours Worked per Week	33.20	31.14	28.78	34.93	41.35
Secure Communities	0.36	0.36	0.34	0.36	0.35
N	2,300,372	1,067,049	353,147	1,233,323	1,988,584
ATUS (2005)					
Hours Spent Caring for Children in Household per Week	5.42	14.46	22.38	0.03	2.85
Hours Spent on Household Activities per Week	14.73	17.87	16.03	12.85	10.47
N	681	354	188	327	592

Notes: Data are from the 2005-2014 American Community Survey and the 2005 American Time Use Survey. The sample includes all U.S.-born with a college degree or more, aged 20-63, with subgroups denoted in the column headings. The results are weighted using individual-level weights in the ACS and in the ATUS. Due to data constraints, the sample in the third column using the ATUS data are based on any children under age 6 in the household, rather than under age 5 as in the ACS.

Table 2: Correlation of 2000-2005 Changes in PUMA Characteristics and SC Adoption Year

	Mean of Change in Characteristic	Standard Deviation of Change in Characteristic	Regression Estimate
Change % Citizen	0.005	0.023	-1.881
			(2.203)
Change % Black	0.001	0.025	-1.958
			(1.422)
Change % Labor Force Participation	0.588	2.543	0.003
			(0.015)
Change % Non-Citizen	0.009	0.024	-5.697***
			(2.034)
Change % with Children Under 5	-0.006	0.024	-1.923
			(1.655)
Change % with Children	-0.008	0.030	0.207
			(1.300)
Change % Work > 50 Hours if Work	-1.022	2.113	0.040*
			(0.023)
Change % Work > 60 Hours if Work	-0.432	1.242	-0.032
			(0.037)
Change % with College	0.166	0.021	2.504
			(2.755)
Change % with Masters	0.010	0.013	7.102
			(4.641)
Change % with Ph.D. or Professional Degree	0.001	0.008	7.799
			(6.405)
Change % Females with College	0.010	0.014	-1.331
			(4.086)
Change % Females with Masters	0.007	0.008	5.103
			(7.005)
Change % Females with Ph.D.	0.001	0.005	-11.068
			(10.662)
Change Unemployment Rate	1.10	1.011	-0.058
			(0.042)
Change Housing Prices	47.47	31.217	-0.008***
			(0.001)
Change Detentions	5.18	55.07	-0.001
			(0.001)
Mean Y			2011.72
R-Squared			0.07
N			1077

Notes: Data are from the American Community Survey, the 2000 Census, and the TRAC Detentions Data. We estimate the following regression: $year_p = \alpha + \theta \Delta W_p' + \epsilon_p$ where $year_p$ is the first year SC was implemented in the PUMA. $\Delta W_p'$ includes changes in PUMA-level demographics and economic conditions between 2000 and 2005. The detention data begin in 2002, so for this variable we use the change from 2002 to 2005.

Table 3: Effect of SC on High-Educated Females' Usual Hours of Work (Including Zeros) by Presence of Children in Household

	All	Kids in HHold		No Kids in Hhold
		Kids of Any Age	Kids Under 5	Without Kids
A: PUMA FE, Year FE				
Secure Communities	-0.106	-0.139	-0.440**	-0.060
	(0.071)	(0.098)	(0.176)	(0.090)
Mean Y	33.21	31.15	28.78	34.93
P-Value	0.14	0.15	0.01	0.51
% Effect	-0.32	-0.45	-1.53	-0.17
N	2307896	1069997	354134	1237899
B: Add PUMA-Year Controls				
Secure Communities	-0.111	-0.148	-0.464***	-0.062
	(0.073)	(0.099)	(0.175)	(0.092)
Mean Y	33.21	31.15	28.78	34.93
P-Value	0.13	0.14	0.01	0.50
N	2307896	1069997	354134	1237899
C: Add PUMA Characteristic Trends				
Secure Communities	-0.099	-0.135	-0.452***	-0.042
	(0.073)	(0.098)	(0.173)	(0.091)
Mean Y	33.20	31.14	28.78	34.93
P-Value	0.17	0.17	0.01	0.64
% Effect	-0.30	-0.43	-1.57	-0.12
N	2300372	1067049	353147	1233323
D: Add Demographics				
Secure Communities	-0.114	-0.140	-0.421**	-0.080
	(0.071)	(0.096)	(0.169)	(0.088)
Mean Y	33.20	31.14	28.78	34.93
P-Value	0.11	0.15	0.01	0.36
% Effect	-0.34	-0.45	-1.46	-0.23
N	2300372	1067049	353147	1233323

Notes: Data are from the 2005-2014 American Community Survey. The sample includes all U.S.-born females with a college degree or more aged 20-63. All models include PUMA fixed effects and year fixed effects. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The individual demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 4: Effect of SC on High-Educated Females' Probability of Working, Hours Worked if Working, and Probability of Working Full-Time vs. Part-Time by Presence of Children in Household

	Work >0 Hours	Hours if Work	Hours 35+	Hours 20−34	Hours 0−19
A: Kids of Any Age					
Secure Communities	-0.451**	0.025	-0.311	0.003	0.308
	(0.192)	(0.076)	(0.269)	(0.191)	(0.220)
Mean Y	83.31	37.38	61.94	14.06	24.00
P-Value	0.02	0.74	0.25	0.99	0.16
% Effect	-0.54	0.07	-0.50	0.02	1.28
N	1067049	887412	1067049	1067049	1067049
B: Kids Under 5					
Secure Communities	-0.783**	-0.171	-1.074**	0.007	1.067***
	(0.384)	(0.137)	(0.479)	(0.344)	(0.405)
Mean Y	79.27	36.31	57.20	13.84	28.96
P-Value	0.04	0.21	0.03	0.98	0.01
% Effect	-0.99	-0.47	-1.88	0.05	3.68
N	353147	279036	353147	353147	353147
C: No Kids					
Secure Communities	-0.099	-0.043	-0.317	0.246	0.072
	(0.163)	(0.063)	(0.234)	(0.163)	(0.187)
Mean Y	87.90	39.74	71.69	11.39	16.92
P-Value	0.54	0.49	0.18	0.13	0.70
% Effect	-0.11	-0.11	-0.44	2.16	0.42
N	1233323	1075252	1233323	1233323	1233323

Notes: Data are from the 2005-2014 American Community Survey. The sample includes U.S.-born females with a college degree or more aged 20-63. All models include PUMA fixed effects, year fixed effects, PUMA-year controls, PUMA characteristic trends, and individual demographic controls. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The individual demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 5: Effect of SC on High-Educated Mothers' Labor Supply, By Age of Youngest Child

	Usual Hours Worked	Work > 0 Hours
A: Youngest Kid 0-2		
Secure Communities	-0.512**	-0.889*
	(0.207)	(0.461)
Mean Y	28.73	79.14
P-Value	0.01	0.05
% Effect	-1.78	-1.12
N	243457	243457
B: Youngest Kid 3-4		
Secure Communities	-0.244	-0.587
	(0.309)	(0.646)
Mean Y	28.89	79.56
P-Value	0.43	0.36
% Effect	-0.85	-0.74
N	109690	109690

Notes: Data are from the 2005-2014 American Community Survey. The sample includes U.S.-born females with a college degree or more aged 20-63. All models include PUMA fixed effects, year fixed effects, PUMA-year controls, PUMA characteristic trends, and individual demographic controls. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The individual demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 6: Effect of SC on the Labor Supply of Likely Undocumented Females in Household Services

	LE HISP FB	LE HISP CA/MX	LE HISP CA/MX 80+
*A: (Total # Work in Household Services / Total PUMA Pop) *100*			
Secure Communities	-0.004	-0.004	-0.001
	(0.007)	(0.006)	(0.006)
Mean Y	0.19	0.17	0.15
P-Value SC	0.54	0.54	0.84
% Effect	-2.16	-2.16	-0.81
Observations	10770	10770	10770
*B: (Total # Hours Work in Household Services / Total PUMA Pop) *100*			
Secure Communities	-0.447*	-0.435**	-0.349*
	(0.229)	(0.203)	(0.191)
Mean Y	5.92	5.37	4.54
P-Value SC	0.05	0.03	0.07
% Effect	-7.56	-8.10	-7.67
Observations	10770	10770	10770

Notes: Data are from the 2005-2014 American Community Survey. The sample includes females aged 20-63 who report being foreign-born, have less than a high school degree, and report their current or most recent occupation as household services. We collapse the data to the PUMA by year level summing total number of workers (Panel A) and total number of hours worked (Panel B) using the survey weights. Both outcomes are scaled by total PUMA by year population and multiplied by 100. The first column includes all Hispanics in the sample. The second column includes individuals in the sample born in Mexico or Central America. The third column includes individuals in the sample born in Mexico or Central America who entered the U.S. after 1980. All models include PUMA fixed effects, year fixed effects, PUMA-year controls, and PUMA characteristic trends. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The results are weighted using PUMA population in 2000. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 7: Effect of SC on the Total Hours Worked of Likely Undocumented Females in Household Services, Heterogeneity by Intensity Measures

	LE HISP FB	LE HISP CAMX	LE HISP CAMX 80+
*A: (Total # Hours Work in Household Services / Total PUMA Pop) *100*			
Secure Communities	0.348	0.341	0.095
	(0.234)	(0.230)	(0.213)
SC*(LE Hisp FB/All LE)	-2.760***	-2.690***	-1.540**
	(0.848)	(0.835)	(0.718)
Mean Y	5.93	5.38	4.56
Mean Intensity	0.26	0.26	0.26
SD Intensity	0.25	0.25	0.25
β–Mean Int	-0.38	-0.37	-0.31
β–1 SD Higher Int	-1.07	-1.04	-0.69
P-Value SC	0.14	0.14	0.65
P-Value SC & Interaction	0.00	0.00	0.06
N	10710	10710	10710
*B: (Total # Hours Work in Household Services / Total PUMA Pop) *100*			
Secure Communities	0.737	0.986	0.505
	(0.705)	(0.634)	(0.580)
SC*(Share Dep Hispanic)	-1.293*	-1.580**	-0.963
	(0.734)	(0.690)	(0.630)
Mean Y	5.89	5.41	4.57
Mean Intensity	0.92	0.92	0.92
SD Intensity	0.12	0.12	0.12
β–Mean Int	-0.45	-0.46	-0.38
β–1 SD Higher Int	-0.60	-0.65	-0.49
P-Value SC	0.30	0.12	0.38
P-Value SC & Interaction	0.04	0.01	0.07
N	10500	10500	10500
*C: (Total # Hours Work in Household Services / Total PUMA Pop) *100*			
Secure Communities	0.560	1.043*	0.564
	(0.641)	(0.557)	(0.515)
SC*(Share Dep CA/MX)	-1.144*	-1.709***	-1.071*
	(0.665)	(0.610)	(0.562)
Mean Y	5.89	5.41	4.57
Mean Intensity	0.88	0.88	0.88
SD Intensity	0.17	0.17	0.17
β–Mean Int	-0.44	-0.46	-0.38
β–1 SD Higher Int	-0.64	-0.76	-0.56
P-Value SC	0.38	0.06	0.27
P-Value SC & Interaction	0.03	0.00	0.03
N	10500	10500	10500
*D: (Total # Hours Work in Household Services / Total PUMA Pop) *100*			
Secure Communities	0.572	0.711*	0.428
	(0.460)	(0.388)	(0.362)
SC*(Share Dep MX)	-1.537***	-1.768***	-1.216***
	(0.557)	(0.497)	(0.456)
Mean Y	5.89	5.41	4.57
Mean Intensity	0.66	0.66	0.66
SD Intensity	0.25	0.25	0.25
β–Mean Int	-0.44	-0.46	-0.37
β–1 SD Higher Int	-0.83	-0.90	-0.68
P-Value SC	0.21	0.07	0.24
P-Value SC & Interaction	0.00	0.00	0.00
N	10500	10500	10500
*E: (Total # Hours Work in Household Services / Total PUMA Pop) *100*			
Secure Communities	0.021	0.005	-0.095
	(0.363)	(0.321)	(0.284)
SC*(Share Dep Non-Serious Crimes)	-1.768*	-1.760**	-1.068
	(0.929)	(0.885)	(0.739)
Mean Y	5.89	5.41	4.57
Mean Intensity	0.27	0.27	0.27
SD Intensity	0.12	0.12	0.12
β–Mean Int	-0.45	-0.46	-0.38
β–1 SD Higher Int	-0.66	-0.68	-0.51
P-Value SC	0.95	0.99	0.74
P-Value SC & Interaction	0.01	0.01	0.04
N	10500	10500	10500

Notes: Data are from the 2005-2014 ACS. The sample includes females aged 20-63 who report being foreign-born, with less than a high school degree, and report their current or most recent occupation as household services. We collapse the data to the PUMA by year level using the survey weights. The first column includes all Hispanics in the sample; column 2 restricts the sample to those born in Mexico or Central America; column 3 restricts the sample in column 2 to those who arrived in the U.S. after 1980. Panel A shows the baseline model adding the interaction of SC with the share of the PUMA working-age population that is Hispanic low-educated foreign-born. Panels B to E show the baseline model adding an interaction of SC with the share of the PUMA deportations that were of Hispanic individuals (Panel B), of individuals from Central America or Mexico (Panel C), of individuals from Mexico (Panel D), and of individuals who were not convicted of serious crimes (Panel E). We include the same controls as those in Table (6). The results are weighted using PUMA population in 2000. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 8: Effect of SC on the Log Hourly Wages of Female Workers in Household Services

	All Females	Low-Edu Females
Log(Hourly Wages)		
Secure Communities	0.020	0.065**
	(0.013)	(0.027)
Mean Y	2.28	2.23
P-Value SC	0.13	0.02
N	123832	25246

Notes: Data are from the 2005-2014 American Community Survey. The sample includes females aged 20-63 who report their current or most recent occupation as household services, and whose income is not imputed. Subgroups are denoted in the columns. All models include PUMA fixed effects, year fixed effects, PUMA-year controls, and PUMA characteristic trends. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 9: Effect of SC on the Usual Hours Worked (Including Zeros) of High-Educated Mothers with Children Under Age 5, by Intensity of Treatment

A: Hours Worked	
Secure Communities	-0.300
	(0.211)
SC*(LE Hisp FB=All LE)	-0.362
	(0.463)
Mean Y	28.78
Mean Intensity	0.26
SD Intensity	0.24
β–Mean Int	-0.39
β–1 SD Higher Int	-0.48
N	352083
B: Hours Worked	
Secure Communities	0.426
	(0.744)
SC*(Share Dep Hispanic)	-0.916
	(0.809)
Mean Y	28.77
Mean Intensity	0.91
SD Intensity	0.12
β–Mean Int	-0.41
β–1 SD Higher Int	-0.52
N	350030
C: Hours Worked	
Secure Communities	0.858
	(0.526)
SC*(Share Dep CA/MX)	-1.443**
	(0.579)
Mean Y	28.77
Mean Intensity	0.88
SD Intensity	0.17
β–Mean Int	-0.41
β–1 SD Higher Int	-0.65
N	350030
D: Hours Worked	
Secure Communities	0.385
	(0.279)
SC*(Share Dep MX)	-1.217***
	(0.356)
Mean Y	28.77
Mean Intensity	0.65
SD Intensity	0.25
β–Mean Int	-0.41
β–1 SD Higher Int	-0.72
N	350030
E: Hours Worked	
Secure Communities	-0.327
	(0.281)
SC*(Share Dep Non-Serious Crimes)	-0.323
	(0.854)
Mean Y	28.77
Mean Intensity	0.27
SD Intensity	0.11
β–Mean Int	-0.41
β–1 SD Higher Int	-0.45
N	350030

Notes: Data are from the 2005-2014 American Community Survey. The sample includes U.S.-born mothers with a college degree or more aged 20-63. Panel A shows the baseline model adding the interaction of SC with the share of the PUMA working-age population that is Hispanic low-educated foreign-born. Panels B to E show the baseline model adding an interaction of SC with the share of the PUMA deportations that were of Hispanic individuals (Panel B), of individuals from Central America or Mexico (Panel C), of individuals from Mexico (Panel D), and of individuals who were not convicted of serious crimes (Panel E). The model includes PUMA fixed effects, year fixed effects, PUMA-year controls, PUMA characteristic trends and demographic controls. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The individual demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 10: Triple Difference Model with Mothers with Children Under Age 5 as Treatment Group

	Without PUMA*Year FE	With PUMA*Year FE
A: Comparison Group is Females without Kids		
Secure Communities	-0.083	
	(0.087)	
SC * Mothers with Young Kids	-0.312*	-0.302
	(0.182)	(0.184)
Mean Y	33.57	33.57
P-Value on SC * Mothers with Young Kids	0.09	0.10
N - Mothers of Young Kids	353147	353147
N - Comparison Group	1233323	1233323
B: Comparison Group is Fathers with Young Kids		
Secure Communities	-0.091	
	(0.136)	
SC * Mothers with Young Kids	-0.344	-0.379*
	(0.224)	(0.224)
Mean Y	36.30	36.30
P-Value on SC * Mothers with Young Kids	0.12	0.09
N - Mothers of Young Kids	353147	353120
N - Comparison Group	291457	291452

Notes: Data are from the 2005-2014 American Community Survey. The sample includes all U.S.-born with a college degree or more aged 20-63. In Panel A, the comparison group is all high-educated females without children. In Panel B, the comparison group is all high-educated fathers with children under age 5. The model includes PUMA fixed effects, year fixed effects, PUMA-year controls, PUMA characteristic trends and demographic controls. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The individual demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. In the first column we include PUMA by treatment group effects, as well as year by treatment group fixed effects. In the second column, we include these same fixed effects as well as PUMA by year fixed effects so we omit the un-interacted Secure Communities variable. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 11: Effect of SC on High-Educated Males' Usual Hours of Work (Including Zeros) by Presence of Children in Household

	All	Kids in HHold		
		Kids of Any Age	Kids Under 5	None of Any Age
Secure Communities	-0.094	-0.102	-0.085	-0.070
	(0.068)	(0.080)	(0.133)	(0.097)
Mean Y	41.35	44.68	45.32	38.84
P-Value	0.17	0.20	0.53	0.47
% Effect	-0.23	-0.23	-0.19	-0.18
N	1988584	886397	291457	1102187

Notes: Data are from the 2005-2014 American Community Survey. The sample includes all U.S.-born males with a college degree or more aged 20-63. The model includes PUMA fixed effects, year fixed effects, PUMA-year controls, PUMA characteristic trends and demographic controls. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The individual demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 12: Lasting Effects of SC around Childbirth on the Usual Hours of Work (Including Zeros) of High-Educated Mothers

	Usual Hours Worked
A: Youngest Child Age 3−5	
SC when Youngest Aged 0−2	-0.368**
	(0.144)
Mean Y	29.25
P-Value	0.01
Min Survey Year	2005
Max Survey Year	2016
Min Birth Year	2000
Max Birth Year	2011
N	173707
B: Youngest Child Age 6−7	
SC when Youngest Aged 0−2	-0.062
	(0.172)
Mean Y	31.22
P-Value	0.72
Min Survey Year	2006
Max Survey Year	2018
Min Birth Year	2000
Max Birth Year	2011
N	114057

Notes: Data are from the 2005-2018 American Community Survey. The sample includes U.S.-born mothers with a college degree or more, aged 20-63, who gave birth to their youngest child between 2000-2011. The model includes PUMA fixed effects, year of survey fixed effects, year of birth of the youngest child fixed effects, PUMA-year controls, PUMA characteristic trends, demographic controls, and controls for exposure to SC beyond age 2. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The individual demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Online Appendix
Unintended Consequences of Immigration Enforcement: Household Services and High-Educated Mothers' Work

A TRAC Data Description

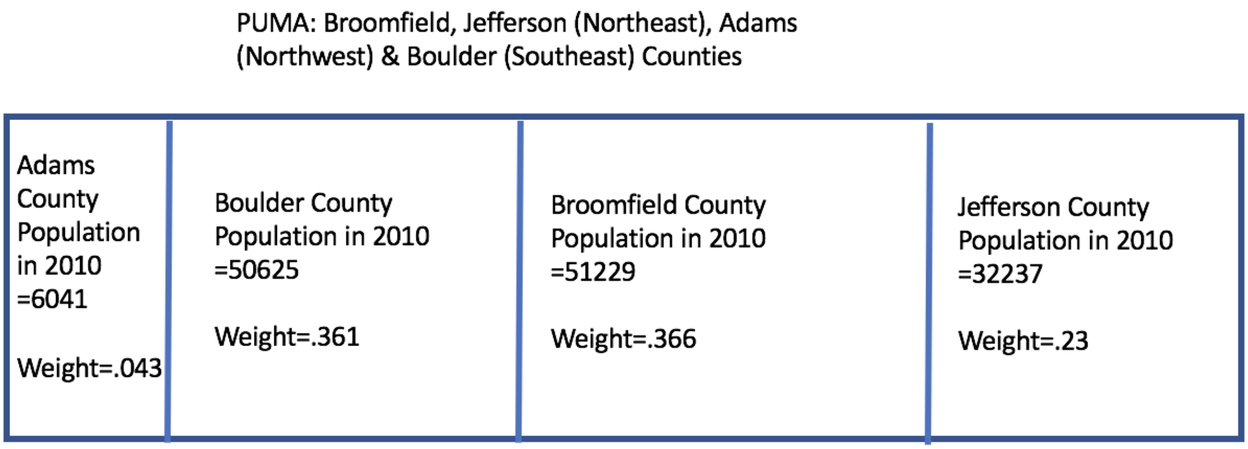
We use data from the Transactional Records Access Clearinghouse (TRAC) on individuals deported under SC between 2008 and 2014. For each individual we have demographic information (e.g. age, sex, country of citizenship), as well as the county of apprehension, and date of removal (not date of apprehension). TRAC obtained this data through Freedom of Information Requests to U.S. Immigration and Customs Enforcement. We aggregate to the PUMA level using the same weighting process as described in the main text for the SC variable. We use the data to generate summary statistics of all those deported under SC shown in Appendix Table (A2) as well as the measures of policy intensity described in more detail in the text. Additionally, we use a very similar data set on detentions from TRAC in Table (2) to look at pre-trends in detentions.

B Control Variables Description

In some regressions, we include controls for labor demand, housing prices, and other enforcement policies. First, we construct four Bartik-style measures of labor demand that correspond to the following four demographic groups: 1) all working-age adults, 2) foreign-born working-age adults, 3) working-age females with a college degree or more, and 4) working-age males with a college degree or more. For each of these four demographic groups, we calculate the PUMA-level group-specific employment by industry, as a fraction of total group-specific PUMA employment in 2005. We then apply to these group-specific industry shares the changes in national group-specific employment for working age adults in each industry over time, to obtain a measure of predicted changes in local labor demand. The housing price information comes from the Federal Housing Finance Agency and is available at the county by year level. Start and end dates for all 287(g) agreements came from reports published by ICE, the Department of Homeland Security, the Migration Policy Institute, as well as Kostandini, Mykerezi and Escalante (2013), and various news articles. We focus on county-level 287(g) agreements only and ignore state-level agreements. We aggregate up the county-level housing and 287(g) information to the PUMA level using the same weighting process as described in the main text for the SC variable.

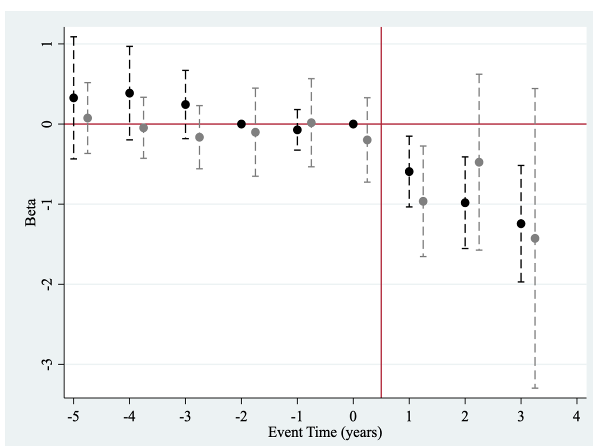
C Additional Results

Figure A1: County to PUMA Matching Example



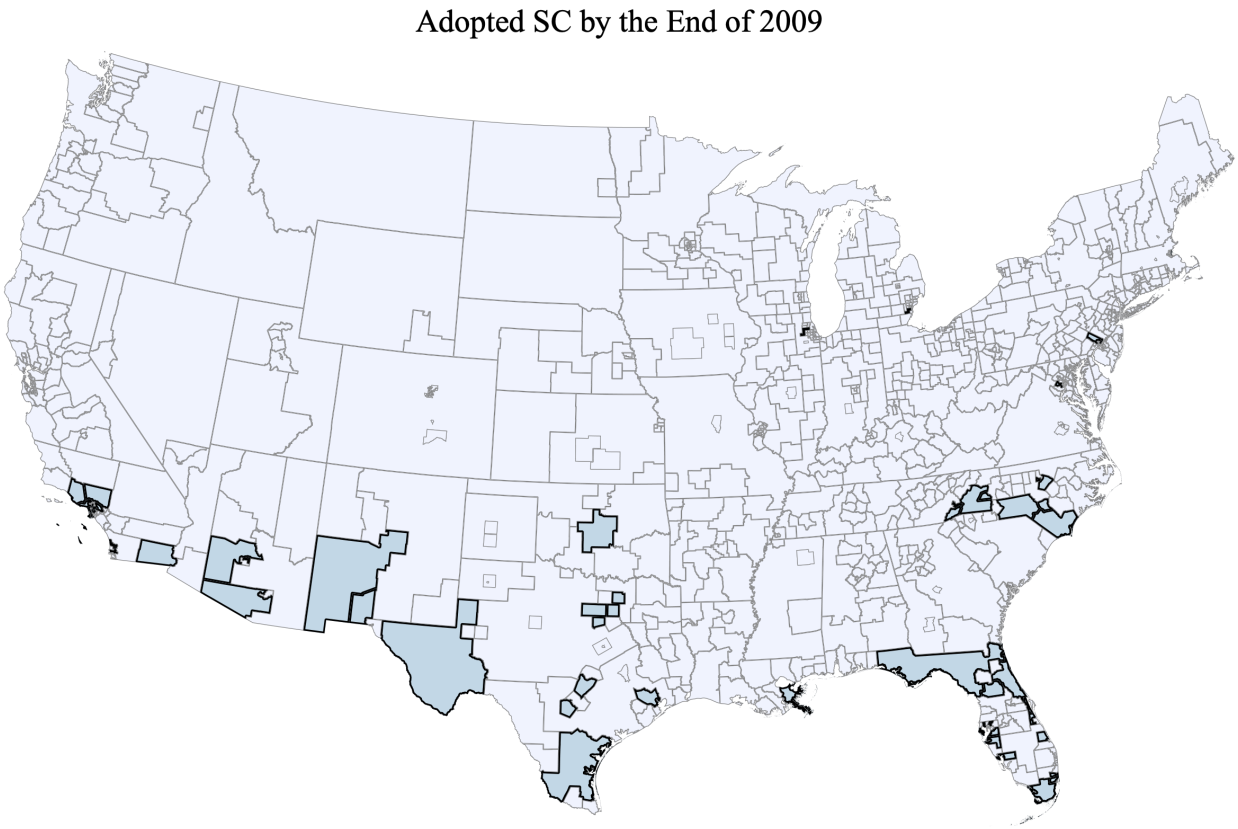
Notes: This figure shows the four counties that make up one PUMA. Additionally, the counties' populations are shown, and associated weights that are used to aggregate the county-level data to the PUMA-level.

Figure A2: Effect of SC on the Usual Hours Worked (Including Zeros) of High-Educated Mothers with Children Under Age 5, Robustness to Callaway and Sant'Anna (2021) Estimator



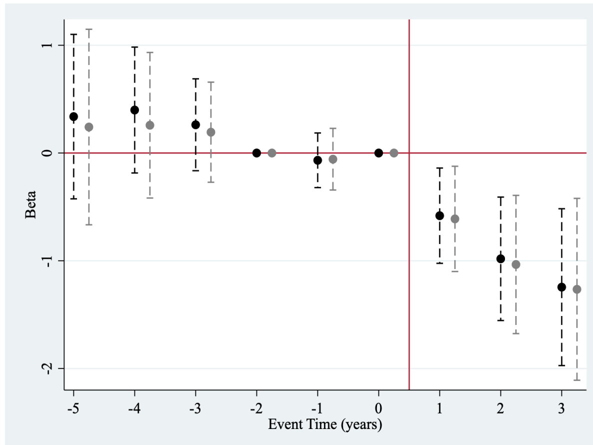
Notes: Data are from the 2005-2014 American Community Survey. The sample includes U.S.-born mothers with children under age 5, with a college degree or more, aged 20-63. The model includes PUMA fixed effects and year fixed effects. The point estimates and corresponding 95% confidence intervals for the baseline results are shown in the black dots and black dashed lines. The point estimates and corresponding 95% confidence intervals for the estimates using the Callaway and Sant'Anna estimator are shown in the grey dots and grey dashed lines. The results are weighted using the individual-level weights in the ACS. Standard errors are clustered at the PUMA level. The horizontal axis denotes "event time". In the baseline estimation, the omitted years are the year before the first SC policy in the PUMA was implemented and three years before the first SC policy in the PUMA was implemented. We estimate effects for all possible event study time periods and therefore drop two pre-periods to be able to separately identify secular time trends from dynamic treatment effects (Schmidheiny and Siegloch, 2020). For both methods, we only display the coefficients from event time -5 to +3, as these are estimated on a sample of nearly balanced PUMAs–we can observe -5 for 916 of 1078 PUMAs, and +3 for 742 PUMAs. We only observe 585 PUMAs at event time -6 and 493 PUMAs for event time +4.

Figure A3: Early Adopter PUMAs



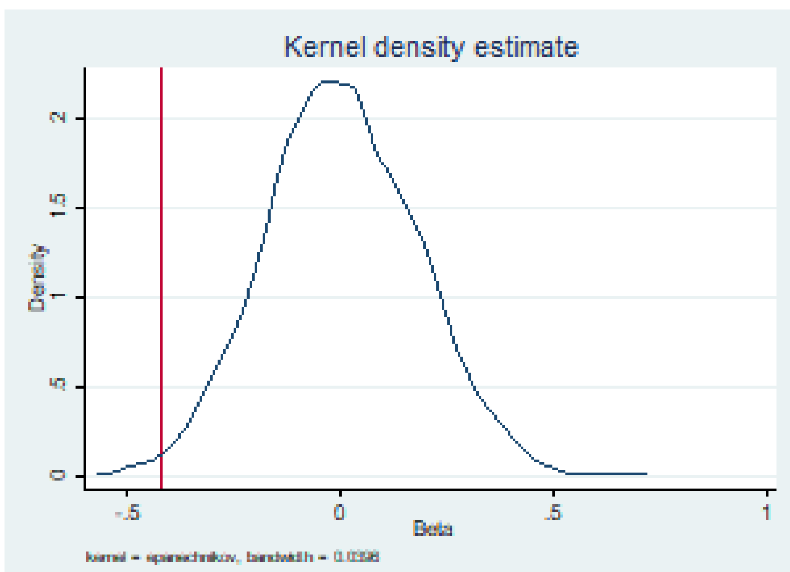
Notes: PUMAs that had adopted Secure Communities "early" (before the end of 2009) are shaded. See text for information on the data source.

Figure A4: Effect of SC on the Usual Hours Worked (Including Zeros) of High-Educated Mothers with Children Under Age 5, Robustness to Dropping Early Adopters



Notes: Data are from the 2005-2014 American Community Survey. The sample includes U.S.-born mothers with children under age 5, with a college degree or more, aged 20-63. The model includes PUMA fixed effects and year fixed effects. The point estimates and corresponding 95% confidence intervals for the baseline results are shown in the black dots and black dashed lines. The point estimates and corresponding 95% confidence intervals for the estimates dropping early adopters are shown in the grey dots and grey dashed lines. The results are weighted using the individual-level weights in the ACS. Standard errors are clustered at the PUMA level. The horizontal axis denotes "event time" where the omitted years are the year before the first SC policy in the PUMA was implemented and three years before the first SC policy in the PUMA was implemented. We estimate effects for all possible event study time periods and therefore drop two pre-periods to be able to separately identify secular time trends from dynamic treatment effects (Schmidheiny and Siegloch, 2020). Additionally, we only display the coefficients from event time -5 to +3, as these are estimated on a sample of nearly balanced PUMAs–we can observe -5 for 916 of 1078 PUMAs, and +3 for 742 PUMAs. We only observe 585 PUMAs at event time -6 and 493 PUMAs for event time +4.

Figure A5: Effect of SC on the Usual Hours Worked (Including Zeros) of High-Educated Mothers with Children Under Age 5, Placebo Test



Notes: Data are from the 2005-2014 American Community Survey. The sample includes U.S.-born mothers with children under age 5, with a college degree or more, aged 20-63. The model includes PUMA fixed effects, year fixed effects, PUMA-year controls, PUMA characteristic trends and individual demographic controls. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, and housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, and have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. The results are weighted using the individual-level weights in the ACS. We plot the density of the 1000 estimated βs from equation (1) after randomizing SC adoption dates. The red lines shows the baseline estimates of β from Table (3).

Table A1: Employment in Household Services in 2005

Occupation	**% of Occupation Employment Low-Edu Hispanic Foreign-Born**	**% of Occupation Employment Low-Edu Female Hispanic Foreign-Born**
Housekeepers, maids, butlers, stewards	18.39	17.21
Child care workers	4.83	4.75

Notes: Data are from the 2005 American Community Survey. The sample includes all individuals aged 20-63 who report working in the two household service occupations. The columns show the percent of employment in the given occupation that is Hispanic low-educated foreign-born and female Hispanic low-educated foreign-born, respectively. The results are weighted using individual survey weights.

Table A2: Most Serious Criminal Conviction and Demographic Characteristics of Deportees under SC, 2008-2014

	Share of All Deportees (percent)
Most Serious Criminal Conviction	
None	20.63
All Violent	18.54
All Non-Violent	60.83
DUI	10.94
Traffic	7.01
Property	6.30
Immigration	5.46
Marijuana	2.38
Sex	
Male	95.61
Country/Region of Citizenship	
Latin America	92.22
Hispanic Countries (except Spain)	90.25
Mexico	62.58
Central America	23.02

Notes: Information on deportees is drawn from the individual listings of all deportations under SC from Transactional Records Access Clearinghouse (TRAC) described in Appendix A. The most serious criminal conviction may be, but does not have to be, the crime for which the deportee was initially apprehended.

Table A3: Effect of SC on High-Educated Females' Fertility

	Had Child in Last 12 Months
A: Any Kids	
Secure Communities	0.022
	(0.180)
Mean Y	10.88
N	863159
B: Kids Under 5	
Secure Communities	0.077
	(0.431)
Mean Y	25.81
N	351543

Notes: Data are from the 2005-2014 American Community Survey. The sample includes all U.S.-born females with a college degree or more aged 20-50. All models include PUMA fixed effects, year fixed effects, PUMA-year controls, PUMA characteristic trends, and individual demographic controls. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The individual demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A4: Effect of SC on Across-PUMA Migration Rates by Demographic Group

	High-Educated Mothers With Kids Under 5	Low-Edu Hispanic Foreign-Born Females in Household Services
Secure Communities	0.005	0.019
	(0.006)	(0.031)
Mean Y	0.13	1.40
P-Value	0.35	0.53
N	9693	9693

Notes: Data are from the 2005-2014 American Community Survey. The sample includes on U.S.-born mothers with a college degree or more aged 20-63 and with a child under age 5 in column (1). In column (2), the sample includes likely undocumented females in household services. We collapse the data to the PUMA by year level and the migration rate is defined as the number of migrants in a given demographic group, PUMA, and year, relative to the PUMA population in 2005. We multiply this rate by 100 to ease presentation. All models include PUMA fixed effects, year fixed effects, PUMA-year controls, and PUMA characteristic trends. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The results are weighted using the population in each PUMA by year cell. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A5: Effect of SC on the Total Hours Worked of Low-Educated Females in Household Services, Overall and for U.S.-Born Workers

	All Low-Edu	Low-Edu USB	Low-Edu USB Non-Hisp	Low-Edu USB Hispanic
Secure Communities	-0.690**	-0.208	-0.077	-0.130
	(0.313)	(0.187)	(0.153)	(0.097)
Mean Y	11.07	4.09	3.25	0.84
P-Value SC	0.03	0.27	0.61	0.18
% Effect	-6.23	-5.07	-2.37	-15.55
Observations	10770	10770	10770	10770

Notes: Data are from the 2005-2014 American Community Survey. The sample includes females aged 20-63 with less than a high-school degree, and who report their current or most recent occupation as household services. We collapse the data to the PUMA by year level using the survey weights. The first column includes all females in the sample, column (2) restricts the sample to U.S.-born females, column (3) restricts the sample to U.S.-born non-Hispanic females, and column (4) to U.S.-born Hispanic females. All models include PUMA fixed effects, year fixed effects, PUMA-year controls, and PUMA characteristic trends. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The results are weighted using PUMA population in 2000. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A6: Lasting Effects of SC around Childbirth on the Usual Hours of Work (Including Zeros) of High-Educated Mothers, Robustness to Dropping Migrants

	Usual Hours Worked
A: Youngest Child Age 3−5	
SC when Youngest Aged 0−2	-0.368**
	(0.144)
Mean Y	29.25
P-Value	0.01
Min Survey Year	2005
Max Survey Year	2016
Min Birth Year	2000
Max Birth Year	2011
N	173707
B: Youngest Child Age 3−5, Drop Migrants	
SC when Youngest Aged 0−2	-0.345**
	(0.146)
Mean Y	29.30
P-Value	0.02
Min Survey Year	2005
Max Survey Year	2016
Min Birth Year	2000
Max Birth Year	2011
N	166258
C: Youngest Child Age 6−7	
SC when Youngest Aged 0−2	-0.062
	(0.172)
Mean Y	31.22
P-Value	0.72
Min Survey Year	2006
Max Survey Year	2018
Min Birth Year	2000
Max Birth Year	2011
N	114057
D: Youngest Child Age 6−7, Drop Migrants	
SC when Youngest Aged 0−2	-0.050
	(0.173)
Mean Y	31.27
P-Value	0.77
Min Survey Year	2006
Max Survey Year	2018
Min Birth Year	2000
Max Birth Year	2011
N	110248

Notes: Data are from the 2005-2018 American Community Survey. The sample includes U.S.-born mothers with a college degree or more, aged 20-63, who gave birth to their youngest child between 2000-2011. The model includes PUMA fixed effects, year of survey fixed effects, year of birth of the youngest child fixed effects, PUMA-year controls, PUMA characteristic trends, demographic controls, and controls for exposure to SC beyond age 2. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The individual demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A7: Lasting Effects of SC around Childbirth on the Usual Hours of Work (Including Zeros) of High-Educated Fathers

	Usual Hours Worked
A: Youngest Child Age 3−5	
SC when Youngest Aged 0−2	-0.119
	(0.108)
Mean Y	45.41
P-Value	0.27
Min Survey Year	2005
Max Survey Year	2016
Min Birth Year	2000
Max Birth Year	2011
N	141040
B: Youngest Child Age 6−7	
SC when Youngest Aged 0−2	0.028
	(0.136)
Mean Y	45.16
P-Value	0.84
Min Survey Year	2006
Max Survey Year	2018
Min Birth Year	2000
Max Birth Year	2011
N	91280

Notes: Data are from the 2005-2018 American Community Survey. The sample includes U.S.-born fathers with a college degree or more aged 20-63 whose youngest child was born between 2000-2011. The model includes PUMA fixed effects, year of survey fixed effects, year of birth of the youngest child fixed effects, PUMA-year controls, PUMA characteristic trends, demographic controls, and controls for exposure to SC beyond age 2. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The individual demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A8: Effect of SC on the Usual Hours Worked (Including Zeros) of High-Educated Mothers with Children Under Age 5, Robustness to Timing

	Usual Hours Worked
A: Kids Under 5, January	
Secure Communities	-0.421**
	(0.169)
Mean Y	28.78
N	353147
B: Kids Under 5, Fraction Last Year	
Secure Communities	-0.404**
	(0.197)
Mean Y	28.78
N	353147

Notes: Data are from the 2005-2014 American Community Survey. The sample includes U.S.-born mothers with children under age 5, with a college degree or more, aged 20-63. The model includes PUMA fixed effects, year fixed effects, PUMA-year controls, PUMA characteristic trends and demographic controls. The PUMA-year controls include: labor demand controls and 287(g) programs. The PUMA characteristic trends include interactions of a time trend with the change in the following PUMA characteristics between 2000 and 2005: labor force participation rate, unemployment rate, and housing prices, the share of the PUMA that are citizens, black, non-citizens, have children, have young children, work more than 50 and 60 hours, and have a college degree, master's degree, or a Ph.D., as well as the same education categories just for females. The individual demographic controls include: age, number of kids, number of kids under age 5, educational attainment, marital status, and race. The results are weighted using the individual-level weights in the ACS. Standard errors clustered at the PUMA level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01