Fear and the Safety Net: Evidence from Secure Communities*

PRELIMINARY - DO NOT CITE WITHOUT PERMISSION

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Abstract

We study the impact of deportation fear on the incomplete take-up of federal safety net programs in the United States. We exploit changes in deportation fear due to the roll-out and intensity of Secure Communities (SC), an immigration enforcement program administered by the Immigration and Customs Enforcement Agency (ICE) from 2008 to 2014. The SC program empowers the federal government to check the immigration status of anyone arrested by local law enforcement agencies and has led to the issuance of over two million detainers and the forcible removal of approximately 380,000 immigrants. We estimate the spillover effects of SC on Hispanic citizens, finding significant declines in ACA sign-ups and food stamp take-up, particularly among mixed-status households and areas where deportation fear is highest. We also find an increase in poor birth outcomes among Hispanic women. Our results are most consistent with network effects that perpetuate fear rather than lack of benefit information or stigma.

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I. Introduction

Active enrollment in public welfare programs in the United States is uneven and far from complete (Ashenfelter 1983, Currie 2006). For instance, Hispanic citizens generally have lower participation than African-Americans and, sometimes, non-Hispanic whites (Morin, Taylor, and Patten 2012). This puzzle of incomplete take-up is deepened when considering the documented positive effects such programs have on health and human capital.[1] Many scholars have studied the factors that influence participation, including transaction costs, information, and stigma (e.g. Aizer 2007, Besley and Coate 1992). Behavioral biases such as inattention and time-inconsistency have also been shown to play a role (Bhargava and Manoli 2015, Madrian and Shea 2001, Karlan et al. 2016).

Widening the lens beyond individual psychology and constraints, studies suggest social networks also influence the take-up of programs and health services in the United States. For example, Bertrand, Luttmer, and Mullainathan (2000) focus attention on the role such networks can play in reducing participation costs, potentially via improved information and destigmatization. Borjas and Hilton (1996) find that prior ethnic-specific program participation predicts take-up by future waves of immigrants - evidence consistent with the intergenerational transmission of ethnic capital (Borjas 1992). For U.S. based Hispanic communities, however, social networks may not only facilitate but also deter program participation. Indeed, recent anecdotal and qualitative research suggests Hispanic citizens fear that their participation in public programs and health services will lead to the deportation of those in their network who do not have permission to be in the country.[2] Yet causal evidence on whether enforcement activities induce a spillover effect on the participation and health outcomes of eligible Hispanics citizens and lawful residents remains thin.

We explore the impact of deportation fear on the safety net participation and health of Hispanic citizens by studying the introduction of a far-reaching immigration enforcement program known as Secure Communities (SC). SC is a federal program administered by the U.S. Immigration and Customs Enforcement Agency (ICE) from 2008 to 2014, and re-activated in 2017. The program empowers ICE to check the immigration status of anyone arrested by local law enforcement agencies through fingerprint analysis and substantially increases the likelihood that a migrant in the U.S. illegally will be deported conditional on being arrested. From its activation to discontinuance in 2014, SC has led to over 43 million fingerprint submissions, 2.2 million fingerprint matches, and over 380,000 individuals forcibly removed from the interior. Removals under the Obama administration's implementation of SC comprised twenty percent of the approximately two million total deportations - the highest number in recent U.S. history.[3]

In this study, we distinguish between direct and indirect treatment effects, with a focus on the latter. In the Rubin Causal Model (RCM) framework, the direct treatment effect is the difference

[1] See Almond et al. (2011), Hoynes, Schanzenbach, and Almond (2016), Bronchetti, Christensen, and Hoynes (2017), Almada and Tchernis (2016), East (2017), Aizer et al. (2016), Goodman-Bacon forthcoming.

[2] As reported in PBS News Hour, "You don't want to be the family member that because you signed up for coverage you're getting your grandmother, your uncle or your parent deported." See https://www.pbs.org/newshour/health/hispanic-americans-still-arent-signing-obamacare.

[3] See http://abcnews.go.com/Politics/obamas-deportation-policy-numbers/story?id=41715661.

in potential outcomes for treatment and control groups among individuals who are *eligible* for treatment (Rubin 1974). Treatment in our context is defined as the activation or intensity of SC and those eligible for deportation are migrants in the country without permission. Direct treatment effects stem mainly from principal-agent problems, whereby unauthorized parents forgo signing up their citizen children for benefits out of fear of revealing themselves. Estimating direct effects has been the subject of several studies in public health (Vargas and Pirog 2016, Hacker at al. 2011, Vargas and Ybarra 2017) as well as work in economics by Watson (2014) and Amuedo-Dorantes, Arenas-Arroyo, and Sevilla (2018).[4] In sharp contrast, indirect treatment effects (ITE) stem from externalities.[5] Under the RCM model, indirect treatment effects measure the difference in potential outcomes for treatment and control groups among individuals who are *not eligible* for deportation (e.g. authorized U.S. citizens, or legal permanent residents), who may nevertheless be fearful of revealing close contacts or other members of the community.[6] A simple extension to Moffit's canonical model of welfare participation (1983) formalizes how social connections can lead to disutility from take-up in the presence of immigration enforcement.

In order to identify the spillover effects of immigration enforcement on Hispanic Americans, we use detailed micro-data on the universe of over two million detainers ("immigrant holds") issued under SC between 2008 and 2015. These data contain information on the county of issue, crime severity, and country of origin of each arrested individual. We combine these data with information on the take-up of Supplemental Nutrition Assistance Program (SNAP) and health insurance on federal exchanges initiated under the Affordable Care Act (ACA). Information on take-up comes from the restricted version of the Panel Study of Income Dynamics (PSID) and public-use data from the Centers for Medicaid and Medicare Services (CMS) and the American Community Survey (ACS).[7] We focus on federal programs since the eligibility criteria are more consistent across locations. SNAP and Medicaid expansions on federal exchanges under the ACA represent two of the fastest growing means-tested programs in the United States and thus are of special interest to economists and policymakers alike. When measuring food stamp outcomes, we follow the prior literature and examine behavioral responses among a high participation sample, defined as those in which the head of household earned less than a high school degree (Hoynes, Schanzenbach, and Almond 2016).[8] Because our focus is on indirect effects, we examine outcomes where the head

[4]Watson (2014) finds that unauthorized mothers reduce take-up of Medicaid for their children in response to changes in immigration enforcement in the mid-1990s. Amuedo-Dorantes et al. (2018) find that migrant parents in the country without permission are more likely to be in poverty and *increase* take-up for food stamps for their American children in response to greater immigration enforcement. Similarly, a study by Allen and McNeely (2017) in public health found that state omnibus legislation affecting enforcement increased Medicaid participation by citizen children with "unauthorized" parents. Cascio and Lewis (2017) find that amnesty to adults increases take-up of the EITC, thus potentially improving outcomes for their citizen children.

[5]By focusing on the ITE, issues of fradulent usage and the undercount of unauthorized individuals become less problematic.

[6]In a recent survey of residents in Los Angeles County, thirty-seven percent reported being concerned that they, a friend, or a family member could be deported. See http://abc7.com/news/fear-of-deportation-on-the-rise-in-la-county-ucla-survey-says/1837739/.

[7]Access to restricted use versions of the ACS and other data sets of interest via the Federal Research Data Center (FRDC) was denied by Census.

[8]This approach is also useful for identification since education is generally fixed by young adulthood whereas

of household is a citizen. Finally, we directly assess whether SC affected health, examining the outcomes for some of the most vulnerable members of the population – infants.

We employ two different identification strategies to estimate the impact of SC on program take-up and birth outcomes. First, we explore the extensive margin of deportation activity, leveraging the staggered roll-out of SC across counties. We note, however, that the roll-out of SC was not unconditionally random, with the earliest activation occurring in border communities and in places with a high percent Hispanic population (Cox and Miles 2013). We thus exclude border counties in our main analysis sample and include geography fixed effects or explicitly control for the percent of Hispanic-headed households in each county-year in our analyses. Balance tables conditional on our preferred fixed effects fail to detect any sharp changes in the evolution of our outcome variables in the period prior to SC activation, lending support to our identification assumption. In addition, we use a triple-differences framework, interacting race and ethnicity indicators with timing of SC activation. In doing so, we compare the take-up and health outcomes for Hispanic individuals before versus after SC activation within a given location relative to non-Hispanic whites and blacks. This triple-differences approach reduces the scope for selection bias, with the main assumption being that there are no location-specific shocks timed with SC *and* which influenced the dynamic path of outcomes for Hispanics only.

Second, we exploit cross-sectional variation in the intensity of SC enforcement to assess sign-up for the ACA. Intensity of SC enforcement is represented by the prevalence of detainers issued in a location relative to the number of estimated unauthorized Hispanics. We instrument for enforcement intensity using a supply-push/shift-share instrument (Card 2001, Bartik 1991, Blanchard and Katz 1992). The supply-push moniker stems from the observation that newer immigrants tend to follow the settlement patterns of earlier ones, (i.e. "chain migration"), so that shares of immigrant groups interacted with their national flow predicts migration patterns. We modify this approach for our purposes, interacting the pre-period shares of each Hispanic foreign born group in the county thirty years prior to SC (the share) with the leave-one-county-out growth in country-of-origin cumulative detainers issued count (the shift). We find a strong first-stage relationship between this shift-share instrument and enforcement intensity.

We find that SC activation is associated with substantial decreases in food stamp sign-up for otherwise eligible Hispanics and worse birth outcomes of Hispanic women. In our preferred specification in the ACS, we find that Hispanic-headed families were 1.7 percentage points less likely to take up food stamps after activation of SC. The take-up rate of food stamps among Hispanic-headed households in the ACS before activation was 23 percentage points, implying a 7.4 percent decline in take-up due to SC activation. We find even larger effects of reduced take-up in the PSID, which comprises a higher-use sample than the ACS, with an almost 50 percent decline in food stamp take-up due to SC activation.

Using birth data from Texas, we find a 1.1 percentage point increase in the share of premature births for Hispanic women after SC activation, a 13 percent increase from the pre-period mean of 8.6

income and asset levels can respond endogenously to program thresholds.

percent, and similar effects on the share who are low birthweight. These findings are an extension of Novak et al. (2017) who report increased low birthweight babies among Hispanic women following an immigration raid in Iowa. Turning to the cross-sectional analysis and IV estimates, we find that a 10 percent increase in detainers is associated with a 2.3 percentage point reduction in Hispanic ACA sign-up. Thus, in the the absence of SC, we predict ACA sign-ups among eligible Hispanics would have been 33 percent higher.

A number of results suggest that the correlations we find are indeed causal. First, we probe the identifying assumptions for both our empirical approaches and find evidence supporting their validity. To assess parallel pre-trends under our triple-differences approach, we show that differences between Hispanics and blacks, or Hispanics and whites, did not vary systematically in places that activated early versus late in the years leading up to SC. As a check on the exclusion restriction underlying our shift-share instrumental variables strategy, we follow Goldsmith-Pinkham, Sorkin, and Swift (2017) and demonstrate that our instrument is uncorrelated with location-specific factors. Second, we condition on a rich set of control variables thought to influence the outcome and treatment, including political affiliation (Lerman, Sadin, and Trachtman 2017), gender (Morin, Taylor, and Patten 2012), age (Wehby and Lyu 2017), income (Buettgens, Kenney, and Pan 2015), and crime (Cox and Miles 2013). We also control for a full set of race-by-state fixed effects to address the potential concern that states may vary in their attitudes and policies towards minority groups, and we allow for flexible impacts of the Great Recession across demographic groups, interacting race and ethnicity indicators with the timing and intensity of the recession in all our longitudinal analyses (Connaughton and Madsen 2012, Kochhar, Fry, and Taylor 2011, McKernan et al. 2014). Third, we show that our findings are specific to Hispanic Americans – coefficients on non-Hispanic blacks or whites interacted with SC are generally small and not statistically significant. Similarly, using our shift-share instrument for immigration enforcement intensity, we do not find that the take-up of ACA by eligible blacks or whites is affected by enforcement intensity. Taken together, the evidence suggests that the SC program did not affect the behavior of those less likely to be affected by immigration policy and deportation fear.

Although precise mechanisms are difficult to pin down, several findings are consistent with the notion that fear plays an important role. We define fear as the subjective likelihood of a dangerous event. Whether a deportation event is considered dangerous is conditional on whether a deportable individual is connected to the decision-maker via his social network. Therefore, we examine whether our results are more pronounced for mixed-status households or places where exposure between citizens and non-citizen Hispanics is higher (Cutler, Glaeser, and Vigdor 1999).[9] Across all data sets, we obtain consistent results: effects are strongest among households and communities with more exposure to at-risk individuals.[10] We find more negative effects of SC in places where the ratio of

[9]Mixed-status households include members that have different citizenship or immigration statuses. Given that the PSID does not ask about citizenship status, we proxy for citizenship using whether an individual was born in the United States or a Hispanic foreign country.

[10]The birth data currently used in the paper is from the state of Texas and is only available at county∗race/ethnicity cells. We have applied for more detailed statistics data that would allow us to test the same hypothesis.

non-violent (often traffic-related offenses) to violent Hispanic detainers issued is highest, suggesting that communities are sensitive to whether local authorities are sucessful at targeting "felons not families." In locations where federal detainers are not enforced (i.e. "sanctuary cities"), and thus deportation risk is objectively lower SC activation has almost no detectable effect. We proxy for beliefs about the likelihood of deportation (i.e. fear) with data on Google searches for deportation-related terms, normalized to adjust for differential internet usage in the Hispanic population. The effects of SC activation on participation are strongest in places with higher deportation-related search activity.[11]

One competing explanation for our results is information. Since social networks transmit not only fear but also detailed programmatic knowledge, reducing the number of co-ethnics who sign up for a program could leave affected groups poorly informed about benefits. We explore this possibility following Aizer and Currie (2004) by estimating effects on households that previously took up food stamps prior to SC activation in the PSID. Such households arguably already know how to sign up for the benefit. Similar to Aizer and Currie (2004), we find that information spillovers are not an important part of the explanation: Hispanic households who previously used food stamps also substantially reduced their use following SC activation. This finding, combined with the heterogeneous effects described above, also lessens the likelihood that stigma is driving our results.

This paper relates to several literatures in economics. First, we build on the work of other scholars who seek to understand why families sometime forgo participation in safety net programs despite high returns (see review by Currie 2006), and highlight that kinship networks can not only yield benefits, but also costs (see review by Cox and Fafchamps 2008, di Falco and Bulte 2011). Second, we contribute to a vast literature that aims to causally identify the effect of beliefs on consumer behavior - in particular, fear (Slemrod 1990, Becker and Rubinstein 2011). Finally, and more broadly, we document how public programs, often designed by agents (or agencies) with differing objectives, interact and influence outcomes for households and communities.

Our paper proceeds as follows. The next section describes the SC program in detail. Section III provides background on the eligibility for public programs in our study. Section IV presents a model of participation incorporating spillover effects. Section V outlines our data and identification strategy. Section VI reports the results, Section VII discusses potential mechanisms, and Section VIII concludes.

II. Background on Secure Communities

Secure Communities was an immigration enforcement program administered by U.S. Immigration and Customs Enforcement (ICE) from 2008 to 2014. The program was aimed at helping ICE arrest and remove individuals who were in violation of federal immigration laws, including those who failed to comply with a final order of removal, or those who had engaged in fraud/willful misrepresentation

[11] In addition, we have requested micro-level survey data on deportation fear.

in connection with government matters. SC had three main objectives: (1) to identify aliens in federal, state, and local custody charged with or convicted of serious criminal offenses who are subject to removal and at large aliens convicted of a serious criminal offense who are subject to removal; (2) to prioritize enforcement actions to ensure apprehension and removal of aliens convicted of serious criminal offenses; and (3) to transform criminal alien enforcement processes and systems to achieve lasting results. SC accomplished these goals through an extensive collaboration between state and local law enforcement agencies, and the Federal Bureau of Investigation (FBI), and the Department of Homeland Security (DHS).

Typically, when a person is arrested and booked by a state or local law enforcement agency, his or her fingerprints are taken and submitted to the FBI. The FBI runs these fingerprints in order to conduct a criminal background check, which is forwarded to the state or local authorities. Prior to the implementation of SC, noncitizens in violation of immigration laws were identified by inmate interviews in local jails or prisons. These interviews were labor-intensive, such that federal and local officials authorized to conduct these interviews screened less than 15 percent of local jails and prisons, and in only about two percent of all U.S. counties (Cox and Miles 2013).

SC improved upon the standard fingerprinting procedure. Under SC, fingerprints received by the FBI were automatically and electronically sent to DHS. Legally, this information exchange fulfills a 2002 Congressional mandate for federal law enforcement agencies to share information that is relevant to determine the admissibility or deportability of an alien. (See 8 U.S.C. §1722(a)(2).) The fingerprints received by DHS were then compared against its Automated Biometric Identification System (IDENT), a database that stores biometric and biographical information on foreign-born persons in three primary categories: (1) noncitizens in the U.S. who have violated immigration law, such as persons who were previously deported or overstayed their visas; (2) noncitizens lawfully in the U.S. but who may be deportable if they are convicted of the crime for which they have been arrested; and (3) citizens who naturalized after their fingerprints were included in the database (see Cox and Miles 2014). IDENT contains the fingerprints of suspected terrorists, criminals, immigration violators, in addition to all travelers when they enter and leave through U.S. airports, seaports and land border ports of entry, and when they apply for visas at U.S. consulates. The IDENT system was created in 1994 to help U.S. border and immigration officials keep criminals and terrorists from crossing U.S. borders.

If there was a fingerprint match, ICE relied on both biometric confirmation of the individual's identity in addition to other reliable evidence that the individual either lacks immigration status or is removable under immigration law. If ICE had probable cause for removability, ICE then issued what is called a "detainer" (sometimes called an "immigration hold") on the person. This detainer requests that the state or local law enforcement agency hold the individual up to 48 hours to allow ICE to assume custody for the initial of removal proceedings. As a result of this detainer system, individuals who may otherwise be released through the local legal system (such as those whose cases were dismissed or those who were released pre-trial pending criminal proceedings) were detained via SC. As Cox and Miles (2014) describe, SC substantially increased the likelihood that a noncitizen

would be apprehended by ICE and deported from the country, conditional on being arrested.

Moreover, the information-sharing partnership between DHS and the FBI is mandated by federal law, which means that state and local jurisdictions could not easily opt out of participation in SC. All fingerprints submitted to the FBI were automatically send to DHS, such that a local jurisdiction could not choose to only submit its fingerprints to the FBI.

SC was not implemented at once across the entire country. Due to various constraints, the program began on October 27, 2008 and was activated on a county-by-county basis. SC was adopted in most counties by mid 2012 and fully activated across the entire country on January 22, 2013. Cox and Miles (2013) show that the timing of activation across counties is most strongly correlated with the Hispanic and foreign-born population, with early activation occurring in counties along the southern border. In November 20, 2014, SC was temporarily suspended by DHS policy. On January 25, 2017, SC was reactivated under Executive Order No. 13768, entitled Enhancing Public Safety in the Interior of the United States. From its inception in 2008 through 2014 and since its reactivation in 2017, SC has led to the deportation of over 400,000 illegal immigrants.

In response to SC, some jurisdictions began to disobey detainer requests from ICE, citing concerns that such detentions were unconstitutional under the Fourth Amendment, as well as concerns that such practices would discourage cooperation with local law enforcement. These jurisdictions became known as "sanctuary cities."[12]

III. Safety Net Programs

In this study, we focus on participation in SNAP/food stamps and the ACA, two of the fastest growing means-tested programs in the United States. SNAP participation increased from 20 million to 40 million participants between 1990 and 2010 and reached record levels of spending - $78 billion - in 2011 (CBO 2012). The ACA expanded health insurance to 20 million people and its subsidies are estimated to cost approximately $40 billion per year (Skinner and Chandra 2016, Center for Health and the Economy 2016). Moreover, both have fairly uniform eligibility requirements that exclude unauthorized individuals, thus enabling us to measure indirect treatment effects. We briefly summarize the eligibility requirements before turning to anecdotal evidence linking deportation fear to reduced participation.

SNAP/Food Stamps: In order to receive benefits under the Supplemental Nutrition Assistance Program (SNAP), formerly known as the Food Stamp Program, individuals need to meet various federal guidelines.[13] In general, households must have an annual income below 130 percent of the federal poverty line (FPL). Further, applicant households must have less than $2,250 in countable

[12]The specific policies can vary widely, from prohibiting police officers inquiring about a person's immigration status, to not honoring administrative detainers issued by ICE, to restricting information sharing with federal immigration agents. For an up-to-date map of sanctuary cities and counties across the United States, see http://cis.org/Sanctuary-Cities-Map and https://www.ilrc.org/local-enforcement-map.

[13]See https://www.fns.usda.gov/snap/eligibility#Resources.

resources ($3,500 if someone is older than 60 or disabled).[14]

Immigrants residing in the country illegally are ineligible to receive benefits. However, if a household has at least one eligible person in the household, than that eligible person can receive food stamps. To apply for benefits, individuals complete an application in-person or online, followed by an interview with a SNAP representative. In our context, immigration enforcement may affect take-up because SNAP applications routinely ask for the names and social security numbers of all persons in the household. Some states also ask for country of origin, date of entry, alien registration number, and citizenship status of each person in the household. An example of a state SNAP form is provided in Appendix Figure A1. Almost all states assure that this information will only be used to determine eligibility, but advocacy groups claim that SNAP applications have declined and that this decline has coincided with increased anti-immigration rhetoric.[15]

ACA: The Affordable Care Act (ACA), enacted in 2010, allowed individuals to purchase health insurance through the federal "Health Insurance Marketplace." The ACA provided subsidies towards the marketplace for low-income individuals and required all Americans to enroll in health insurance or pay a fine (later repealed as part of the 2017 Tax Cuts and Jobs Act). It also funded states to expand their Medicaid programs to all adults below 138 percent of the federal poverty line, although 18 states have yet to accept the expansion. Rolling out in 2014, 8 million people obtained insurance via the federal marketplace, increasing to about 13 million by 2016 (Uberoi, Finegold, and Gee 2016). As with SNAP, unauthorized immigrants are ineligible for the ACA, as President Obama pledged in his 2009 speech to Congress regarding the bill.[16]

According to the Commonwealth fund, all demographic groups have enjoyed reductions in their uninsured rate under the ACA, but the decline has been slowest for Hispanics (Garrett and Gangopadhyaya 2016).[17] Moreover, as the number of uninsured has fallen, Latinos comprise an ever larger share of the remaining uninsured (Commonwealth 2016). Several reasons have been put forward to explain why millions of Hispanics have yet to sign up including: 1) accounting - counting unauthorized as uninsured despite their lack of eligibility; 2) information - faulty Spanish websites and translations and; 3) fear. As noted in the Hill, "The final reason is simply fear. In signing up for ObamaCare one must give vital personal information that might lead Immigration and Customs Enforcement (ICE) officers to one's house and family. The government is no longer shy about enforcing removals of anyone here illegally – even grandmothers." See an example of the ACA application form in Appendix Figure A2. Despite public assurance by the federal government that

[14]The household can forego the SNAP income test, however, if all members of the household are receiving Temporary Assistance for Needy Families (TANF), Supplemental Security Income (SSI), or some other state general assistance programs. See detailed reviews on safety net requirements for further information. Legal immigrants are eligible for SNAP if they have lived in the US for five years, if they currently receive disability-related assistance, or if they have children under 18. There is no requirement of employment in most cases, but applicants have to meet certain work conditions, including registering for work and not voluntarily reducing work hours.

[15]See AP News, "Fear of Deportation Drives People Off Food Stamps in US" (June 6, 2017), https://apnews.com/3c0b89362c414003a2603deaab43a702downloadedon1/23/2018.

[16]See https://www.youtube.com/watch?v=qgce06Yw2ro.

[17]The uninsured rate for non-Hispanic whites fell by 47 percent, 46 percent for blacks, and 43 percent for Hispanics (Commonwealth 2016).

immigration status in the context of the ACA will not be used for immigration enforcement, as with SNAP, descriptive evidence suggests that Hispanics are still afraid. In a recent article in the Washington Post, a legal Hispanic resident described the tradeoff –"We're afraid of maybe getting sick or getting into an accident, but the fear of my husband being deported is bigger."

IV. Model

To assess the spillover effects of fear, we build off of Moffit's (1983) seminal model of non-participation in social programs. We adopt his cost-benefit approach to participation, although instead of emphasizing stigma, we focus on the deportation-related costs of participation. We incorporate indirect treatment effects by explicitly allowing the utility of the household decision-maker to depend on the well-being of others.

The expected utility of individual i who is the head of a household j in location l from program participation is given by:

$$EU_{jl} = (Y_j + p_{ij}B_j) - c_j \cdot \pi_{jl}(p_{ij}) \tag{1}$$

where Y_j is household income, p_{ij} is the decision to participate (made by the head of household i) and B_j is the benefit from participation for the household. π_{jl} is the subjective probability of deportation (i.e. fear) and is an increasing function of program participation, p_{ij}, weighted by cost parameter c_j, which measures the strength of the connection between unauthorized and authorized individuals in the household. Note that our model captures the ITE of deportation fear because the probability of deportation for an authorized head of household, π_{jl}, is equal to zero if the head of household only cares about his own utility.

Let the change in the subjective probability that an unauthorized person will be deported if the household participates in a program relative to no participation be:

$$\Delta\pi_{jl} = \beta_1 \cdot D_l + \epsilon_{jl} \tag{2}$$

where D_l is the intensity of location-specific immigration enforcement and ϵ_{ij} is an error term.

Under this simple model, households will only participate in welfare programs if the utility from doing so is sufficiently high. In particular, household j will participate if and only if:

$$(Y_j + (1)B_j) - c_j \cdot \pi_{jl}(1) > (Y_j + (0)B_j) - c_j \cdot \pi_{jl}(0) \tag{3}$$

Aggregating over households j in a given location l, the share not participating is given by:

$$s_l = 1 - (\frac{B_j}{c_j} - \beta_1 \cdot D_l) \tag{4}$$

This non-participation share, s_l, is decreasing in the program benefit, B_j, and increasing in the closeness of connections to undocumented individuals, c_j, as well as the local intensity of immigration enforcement D_l. Our model predicts that holding all else constant, as immigration enforcement

becomes more intensified in an area, authorized heads of households will reduce their take-up of public programs, particularly heads with close connections to unauthorized individuals in their networks.

V. Methodology and Data

Our goal is to estimate the causal effect of both extensive and intensive margins of immigration enforcement on take-up of various public services and health of Hispanic Americans. In this section, we describe our identification strategies to draw causal inference and provide an overview of the data sources.

A. Empirical Specification

A.1 Triple-Differences Specification

Our first approach exploits the differential timing of SC activation across counties. Ideally, timing of the rollout would be random. Cox and Miles (2014) show that the earliest activation date was not related to crime – though the purported goal of the program was to remove criminal aliens – rather, earlier activation was positively correlated with proximity to the border and percent Hispanic population.[18] We address this potentially endogenous timing by dropping border areas and including county fixed effects to account for demographic features of a county that may affect timing of activation (note that Census provided Hispanic population counts would fall out of our regression since they are collinear with county fixed effects). In robustness checks, we also explicitly control for percent of households that are Hispanic at the county-year level using the counts from the ACS and show that our estimates are unaffected.

We begin by assessing whether there are baseline differences in the pre-SC period between Hispanics versus other racial/ethnic groups in counties that activated early versus those that activated later, defined by the median activation year (2011 or later). By assessing whether there are baseline differences, we can explore whether eventual activation of SC is correlated with changes in our outcome variables of interest, such as food stamp take-up, before the SC program began. Table 1 presents these results from the ACS and Texas birth data. Similar results on balance are presented for the PSID data in Appendix Table A1.

Column 1 of Table 1 presents the mean and standard deviation of outcome variables and demographic characteristics in the main sample pre-SC activation (2005-2007). Column 2 presents the coefficient of a regression of differences between Hispanics and whites on an indicator for late versus early activation, controlling for state-by-race and state-by-year fixed effects. Standard errors are clustered at the county level. Column 3 presents the parallel coefficient for differences between Hispanics and blacks on an indicator for late activation. In general, there are few differences by racial groups for early versus late activation counties. Most importantly, we find that there are

[18] In unreported results, we replicate Cox and Miles (2014) and similarly find that SC activation was not associated with any significant changes in crime rates.

no significant differences in changes in Hispanic-white or Hispanic-black food stamp take-up in the ACS or changes in Hispanic-white or Hispanic-black share low birthweight or share premature in the Texas births data across early versus late activation counties, suggesting that the timing of SC activation was not correlated with trending differences in outcomes by racial/ethnic group. These results lend support to the assumption of parallel trends underlying our approach. Appendix Figure A3 also presents event study estimates of the impact of SC on the log number of detainers issued. This figure shows a sharp increase in the number of detainers issued post-SC with no discernible trend pre-activation, showing a strong "first stage."

Using repeated cross-sections in the ACS and Texas birth data, as well panel data from the PSID, we estimate the following triple-differences specification to explore the extensive margin of SC activation. In what follows, we write the equation at the county-level but note the differences for household-level data using the PSID below:

$$\begin{aligned} Y_{rcst} = \alpha + \beta_1 I_{ct}^{post} + \beta_2 (I^H \cdot I_{ct}^{post}) + \beta_3 (I^B \cdot I_{ct}^{post}) + \Omega' X_{rct} + \mu_c + \delta_{st} + \theta_{rs} \\ + \Gamma_1 X'_{ct} + \Gamma_2 (I^B \cdot X'_{ct}) + \Gamma_3 (I^H \cdot X'_{ct}) + \epsilon_{rcst} \end{aligned} \tag{5}$$

where r is race/ethnicity, c is county, s is state, and t is year. Y_{rcst} is the outcome of interest. For the ACS and Texas birth data, Y_{rcst} is the share food stamp take-up among a high participation sample, and share of births with an adverse outcome, respectively. As mentioned previously, in all specifications, we exclude border counties since enforcement activities began in those counties early and selection could have played a role in activation (see Cox and Miles 2014).

In the specification above, I^H and I^B are indicators for Hispanic ethnicity and non-Hispanic blacks, respectively. The omitted category is non-Hispanic whites. I_{ct}^{post} is an indicator equal to one in all county-years after the activation date of SC. Almost all counties activated between 2008 to 2013, with the majority of counties activating between 2010 to 2012. In the ACS analysis on grouped means, X_{crt} includes the average poverty level, number of children, and family size that vary across both race and time. We control for these characteristics as they are direct determinants of food stamp eligibility. μ_c are county fixed effects. We include δ_{st} , state-by-year fixed effects, to account for any state-specific policies or economic shocks that might influence the take-up of food stamps or the health of infants and θ_{rs}, state-by-race/ethnicity fixed effects, to control for attitudes and policies in each state that differentially affect minority groups.

We also account for other county-level controls, X_{ct}, that are not publicly available disaggregated by race at the county-level, but which have been shown to have differential effects on minority populations. For instance, white families' wealth fell 26.2 percent during the Great Recession, while the wealth of black families and Hispanic families fell by 47.6 and 44.3 percent, respectively (McKernan et al. 2014). Publicly available crime statistics are generally not available at the race-county-year level but crime disproportionately impacts minorities communities (Sampson and Lauritsen 1997, Anwar and Fang 2006, Antonovics and Knight 2009). To allow for these differences, we interact race indicators with the onset and intensity of the Great Recession, as well as the FBI

index crime rate (Connaughton and Madsen 2012; Kochhar, Fry, and Taylor 2011; McKernan et al. 2014).[19]

Our specification for the PSID is similar to Equation 5 above except that the data are at the household level. As a result, the outcome is an indicator for take-up of food stamps by a high participation household, i. In the PSID data, household-level controls, X_{ircst}, include demographic characteristics on the head of household, including marital status, sex, family size, age of youngest child, and poverty level in the past year.

For ACS and Texas birth data, we weight all regressions by the number of households in the relevant race-county cell, to more nearly identify a population average treatment effect – only exactly so when the model is fully saturated – as well as estimate off parts of the sample with positive support in the Hispanic population (Solon, Haider, and Woolridge 2015). For the PSID, we use provided sample weights.[20] Standard errors are clustered at the county level.

In our analysis on food stamp take-up using the PSID and ACS, we limit our specifications to Hispanic, black, and white heads of households with less than a high school degree – a "high participation" sample following Hoynes, Schanzenbach, and Almond (2016). To measure the spillover (indirect) effects of deportation fear, we further restrict our sample to households with citizen heads of household, individuals who could not be eligible for deportation. The coefficient of interest is β_2, which estimates the impact of SC activation on outcomes of Hispanic households relative to non-Hispanic white households. β_3 serves as a placebo test, capturing the effect of SC on black households.

In addition to our baseline specification in Equation 5, we estimate an event study where we interact I^H and I^B with a series of time dummies for each period, relative to the year of SC activation, which is omitted. In our data, we have sufficient observations to estimate up to six time indicators pre-SC and four time indicators post-SC:

$$Y_{rcst} = \alpha + \sum_{n\neq 0} \beta_1^n (I_{c,t=n}) + \sum_{n\neq 0} \beta_2^n (I^H \cdot I_{c,t=n}) + \sum_{n\neq 0} \beta_3^n (I^B \cdot I_{c,t=n}) + \Omega' X_{rct} + \mu_c + \delta_{st} + \theta_{rs} + \Gamma_1 X'_{ct} + \Gamma_2 (I^B \cdot X'_{ct}) + \Gamma_3 (I^H \cdot X'_{ct}) + \epsilon_{rcst} \tag{6}$$

In this specification, $I_{c,t=n}$ is in indicator for each period (other than the year of activation $t = 0$), such that the β_2^n coefficients trace the take-up of food stamps for Hispanics in the years before and after SC activation relative to non-Hispanic whites before vs. after activation. Similarly, each β_3^n coefficient traces the take-up of food stamps for blacks relative to non-Hispanic whites. Under this event study, one would only expect to see a trend break post-activation for Hispanic households, not black households, if we are measuring the causal effect of SC.

The main assumption underlying our triple-differences specification is that there are no contemporaneous shocks associated with the activation of SC within a county that only affects Hispanic

[19] Several authors have noted that SNAP was an important stabilizer during the Great Recession (Ganong and Liebman 2013, Hoynes and Schanzenbach 2015).

[20] Unweighted samples produce similar results (see Appendix Table A4).

households relative to white and black households. Recall that we tested for balance across late versus early activators in Table 1, demonstrating few significant level differences in Hispanic-black and Hispanic-white in control variables and, more importantly, no statistically significant differences in the pre-activation change in outcomes between groups over time. In robustness checks in Appendix Table A3, we show that if we limit our samples to Hispanic-black or Hispanic-white, we obtain similar results. Our event studies also provide a graphical test of this assumption.

A.2 Shift-Share Instrument

To explore the impact of the intensive margin of SC on ACA enrollment rates, we estimate the following cross-sectional county-level specification:

$$ShrACA_j = \alpha + \beta \cdot (ShrDetain_j) + \mu \cdot X_j + \delta_s + \epsilon_j \tag{7}$$

where j stands for county, X_j is a vector of county-level controls that affect program participation, such as the share Bush versus Kerry in the 2004 Presidential election, the Hispanic sex ratio, per capita income, the percent working age, share Black ACA signup, and FBI index crimes per capita (see Lerman, Sadin, and Trachtman 2017, Morin, Taylor, and Patten 2012, Buettgens, Kenney, and Pan 2015, Cox and Miles 2013). δ_s are state fixed effects which capture time-invariant characteristics that affect health care take-up. The dependent variable, $ShrACA_j$, is the share of Hispanic individuals eligible for enrollment who have signed up for the ACA. We sum the data over the 2015 and 2016 enrollment periods since we anticipate very little year-to-year variation in ACA enrollment as SC had already ended in 2014.

In our preferred specification, we use the cumulative number of detainers between 2008 to 2013, the period of greatest increase in SC activity and which covers the time period that SC was activated across all counties (see Figure 1). We define $ShrDetain_j$ as the number of Hispanic detainers issued over this time period normalized by the estimated number of unauthorized Hispanic individuals, $\frac{D}{UH}$. The denominator is based on a method developed by the Pew Research Center and is generated using the ACS 2005-2009 county-level data (see Pew Research Center 2013). These data report the total number of foreign-born from each country of origin and the number of naturalized Hispanics citizens. Using countries of origin, we calculate the number of Hispanic foreign-born, which include countries of origin such as Argentina, Brazil, Mexico, etc.

In Equation 7, the coefficient of interest is β, which measures the effect of increases in detainer intensity among Hispanics on Hispanic ACA signup. One potential threat to causal identification is the endogeneity of detainers issue per estimated unauthorized Hispanic, $ShrDetain_j$. Counties that experience a greater increase in the share of Hispanics detained may differ in unobservable ways from counties with less immigration enforcement in a way that affects the outcomes of interest. Indeed, SC could have targeted counties that already had low Hispanic engagement with the welfare and health systems, leading to downwards biased estimates of β.

To isolate causal effects of SC on outcomes, we use a shift-share instrument to predict the

number of Hispanic detainers issued. In the spirit of a Bartik instrument used in the economics literature to estimate labor demand (see, e.g., Aizer 2010, Bartik 1991, Blanchard and Katz 1992), we weight the average national number of cumulative detainers from each Hispanic country of origin (excluding own county) with county-specific baseline shares of foreign born from each respective country of origin following Card (2001). These predicted values across countries are then summed within county. Intuitively, variation in this shift-share instrument stems from the fact that national increases in detainers for specific Hispanic countries will lead to larger predicted increases in detainers in those counties with a higher share of immigrants from those countries. For example, if SC primarily ramped up detention activity against immigrants from Mexico, the predicted increases in detainers should be larger in those counties that have more Mexican-born immigrants. Because this instrument is constructed using national trends excluding own county, and projected on baseline shares of foreign born from a pre-SC time period, variation induced by the instrument is plausibly exogenous.

In our two-stage least squares specification, we instrument for $ShrDetain_j$ in Equation 7 with the predicted share of Hispanic detainers issued, Z_j, constructed as:

$$Z_j = \frac{\sum_c \frac{L_{cj}^{t=1990}}{L_c^{t=1990}} \cdot (D_{-jc})}{\hat{UH}} \tag{8}$$

where j represents county, c represents Hispanic country of origin (e.g. Mexico). $\frac{L_{cj}^{t=1990}}{L_c^{t=1990}}$ represents the number of Hispanic immigrants in county j born from country of origin c relative to the total number of Hispanic immigrants born from country c across the United States. These shares are constructed using the 100 percent 1990 Census and sum to one across the United States. These baseline country-of-origin county shares are then multiplied by the cumulative leave-county-out number of national detainers issued from 2008 to 2013, D_{-jc}. Finally, we normalize this predicted number of detainers by the predicted number of unauthorized Hispanics, $\hat{UH}$, calculated as the fraction of unauthorized Hispanics from the 1990 Census multiplied by the total number of foreign-born Hispanics in the 2005-2009 ACS.

There are two assumptions underlying our Bartik approach. The first assumption is that the national cumulative growth in detainers (leaving out the own county) is uncorrelated with the local growth in detainers. The second assumption is that our instrument is exogenous to local area baseline observables (Goldsmith-Pinkham, Sorkin, and Swift 2017). Following Autor and Houseman (2010), we test this assumption in Figure 2 by estimating a seemingly unrelated regression (SUR), which addresses correlations among these observable characteristics. We cannot reject the null hypothesis that our preferred Bartik instrument is uncorrelated with these county-level characteristics (joint p-value = 0.27), although our endogenous variable, $ShrDetain_j$, is highly correlated with county-level observables (joint p-value < 0.0001). Figure 3 presents a county-level map of the intensity of SC using both our endogenous variable and shift-share instrument.

B. Data

Secure Communities Data on Detainers and Removals: Through FOIA requests to ICE, we have obtained micro-level data on the rollout of SC. In particular, we have information on the universe of detainers issued by ICE from 2002 to 2015 in the United States. The detailed information includes the reason for the arrest as well as the crime level/severity, the date issued, the county the detainer was issued in, the individual's country of origin, and other individual-level demographics (age, race, and sex). We also have the universe of individuals who were removed (actually deported) from the country due to a fingerprint match under SC from 2008 to 2015.

Panel A of Figure 1 presents the total number of detainers issued per year and Panel B presents the cumulative number of detainers issued over the time period. The rapid ramp up in SC is evident in the time immediately following SC's launch in 2008. These figures also reveal that the overwhelming majority of detainers are issued against Hispanic individuals. Panel C presents the ratio of detainers for low-level offenses (e.g. traffic violations and misdemeanor offenses) versus serious, violent offenses and shows that over time, SC issued a growing share of detainers for low-level arrests. We collapse these detainer/removal data to the county level to ascertain the number of detainers/removals issued for individuals from each foreign country over time.

We normalize the number of detainers and removals issued by the estimated number of undocumented Hispanic immigrants in a county from the ACS 2005-2009, prior to SC activation. To develop this denominator, we use a method developed by the Pew Research Center, which subtracts the number of naturalized citizens of Hispanic origin from the total number of Hispanic foreign born (Pew Research Center 2013). The Pew Research Center discusses potential methodological issues associated with this procedure, including undercounting in survey data. While undercounting may be correlated with the degree of incomplete take-up of public programs, we control for county or state fixed effects to account for time-invariant differences in take-up.

American Community Survey: We use publicly available ACS data downloaded from IPUMS-USA at the University of Minnesota. We focus on the 1 percent ACS samples of the U.S. population over the years 2005-2016. The data include household characteristics such as food stamp receipt in the last year, poverty, and family size; and also individual characteristics like income, education, and citizenship status. As discussed previously, we limit our sample to Hispanic, black, and white heads of households with less than a high school degree – a "high participation" food stamp sample following Hoynes, Schanzenbach, and Almond (2016). To measure the spillover (indirect) effects of deportation fear, we further restrict our sample to households with citizen heads of household, individuals who could not be eligible for deportation. The most detailed level of geography in the ACS is according to the Census-defined Public Use Microdata Areas (PUMA). PUMAs contain at least 100,000 people and can cross county but not state lines. Because our activation dates and detainers data are at the county-level, we distribute the ACS means to counties based off the PUMA population in each county.

Panel Study of Income Dynamics: We use data from the restricted-access Panel Study of Income

Dynamics (PSID) from 2003-2015. The PSID data are biennial, following heads of household in every survey round. The data contain detailed information on food stamp take-up within the past 12 months, ethnicity, and country of origin by households at the county level. While the PSID does not ask about citizenship status, we proxy for citizenship status using whether a household head was born in the United States versus a foreign country. As with our ACS sample, we limit our sample to citizen heads of household with less than a high school degree. During the years in our sample, the PSID surveyed a total number of 8,723 unique household heads from 679 counties. PSID household characteristics include sex of household head, marital status, family size, age of youngest child, income relative to federal poverty line, health status, and indicators for where the head and the head's parents grew up.

Texas Birth Data: We use data on all Texas births from 2005-2015 from the Texas Department of State Health Services website. The data include information on county-by-race information on the number of births and adverse infant health outcomes such as low birth weight and prematurity. The data do not include detailed information on maternal health and we are actively engaged in trying to obtain micro-level data on births from various services.[21]

Affordable Care Act: Data on ACA sign-ups is from the Center for Medicaid and Medicare Services (CMS). The data are available at the public use micro-data area (PUMA) level, which can be cross-walked to the county level and provide ACA insurance signups for the federal exchanges. The federal exchanges cover 37 states. The data are further disaggregated by race and ethnicity and include estimates of the number of potential and actual enrollees disaggregated by race/ethnicity. CMS does censor at extreme values ($<$10 plans selected), but this only accounts for a small percent of the data. One potential issue with the data is that race is not mandatory to report and may therefore be omitted. Despite this limitation, the CMS data is fairly robust administrative data. We have data from the first two years the ACA was fully implemented, 2015 and 2016. The estimation of the number of potential enrollees by race is based on tabulations by the Assistant Secretary for Planning and Evaluation (ASPE).[22] From these data, we calculate the share of eligible Hispanics, blacks, and non-hispanic whites that signed up for the ACA.

Google Trends Data: In order to parameterize fear in response to SC, we use data from internet search patterns provided by Google Trends. Google Trends is a publicly available database that provides information on the relative popularity of search terms for 250 metropolitan areas across the United States (Nielsen DMA media markets). As discussed in Burchardi, Chaney, and Hassan (2017), for each search term i in media market d, the Google Trends tool provides the normalized share of searches (out of 100) that contain the search term:

[21] Texas denied our request for micro-data because neither of the authors was primarily based at a public university.

[22] The ASPE begins with the census year 2011 American Community Survey Public Use Microdata Sample (ACS PUMS), and excludes estimated undocumented persons. Non-citizens in the ACS are assigned a probability that they are a legal resident in the US. These probabilities are based on an imputation method of immigrant legal status developed by ASPE's Transfer Income Model, version 3 (TRIM3), microsimulation model developed by Jeffrey Passel for the Spring 2009, 2010 and 2011.

$$G(i,d) = \left[100 \cdot \frac{share(i,d)}{max_{\delta}\{share(i,\delta)\}} \mathbb{1}\left[\#(i,d) > T\right]\right] \tag{9}$$

where $share(i,d)$ is the share of searches in d that contain i and T is a threshold value of searches that must be exceeded for Google to permit access to the data. Under this normalization, $G(i,d)$ is equal to 100 in the metro area in which the largest share of searches contain i and a positive number smaller than 100 in all other metro areas that have a sufficient number of searches containing i.

We use the following commonly searched terms related to the Deportation topic on Google Trends: deportation, abogados de inmigracion, deportacion, deportation, immigration, inmigracion, immigration lawyer, indocumentado, undocumented. Following the literature (e.g. Burchardi, Chamey, and Hassan 2017), we take a simple sum of search intensity across all search terms and normalize it by search terms that are popular in the Hispanic community, such as "deportes" (sports) and "telenovelas" (soap operas). This normalization will account for differential access to the internet for Hispanics that may vary across geographic units.

VI. Results

A. Food Stamp Take-up

Table 2 presents our main results on food stamp take-up across various samples in the PSID and ACS data. All specifications are limited to our "high participation" sample and to citizen heads of household. Column 1 reports our main specification (Equation 5) in the PSID citizens sample. We find that after SC activation, Hispanic citizen heads of household reduce their take-up of food stamps by 18.5 percentage points, a 45 percent decrease from the pre-period Hispanic mean of 40.9 percent. Column 2 reports our main specification from the ACS citizens sample, where we find that Hispanic citizens reduce take-up by 1.7 percentage points, a 7.4 percent decrease from 23.0 percent.[23] In columns 3 and 4, we report the same specifications as columns 1 and 2 but add an interaction between our black indicator and post-SC indicator. Our main results are virtually unchanged and we also find oppositely-signed and marginally significant coefficients on the black coefficient post-SC.

Our results are robust to different definitions of household decision-makers, in particular using a sample of highest-ranking females (see Appendix Table A2). In Appendix Table A3, we also present our main results separately for Hispanics versus non-hispanic blacks and versus non-hispanic whites.

[23] There are several reasons why the magnitudes of our estimate may differ so much between the PSID and ACS samples. First, after our sample restrictions, the PSID covers only 679 counties versus 3,060 in the ACS and differentially covers large states like California and Texas. Indeed, when we select an ACS sample that matches the PSID in pre-period mean take-up for hispanics, we find much larger estimated effects (see columns 1 through 3 of Appendix Table A2.) Second, although average poverty levels in the PSID are higher than in the ACS (see Table 1), reported food stamp use is evidently much higher based on pre-period Hispanic means. This may be due to the well-known underreporting and measurement error problems of food stamp participation (Kreider et al. 2012). Third, we can only approximately counties in the public-use ACS using a PUMA to county crosswalk and re-weighting strategy, potentially leading to increased measurement error on the right hand side. These combined effects will likely bias our estimates downward (Hausman 2001).

Across all comparison groups, we find a large and significant effect of SC activation on reduced take-up of food stamps for Hispanic households.

Figure 4 presents our event study estimates of SC activation for non-hispanic whites, non-hispanic blacks, and Hispanics. For both non-Hispanic whites and blacks, there is no noticeable break in the relative flatness of take-up in the years pre- and post-SC activation. In sharp contrast, coefficients on the interaction of time to SC and Hispanic are indistinguishable from zero in the years leading up to activation, but then demonstrate a sharp trend break post-activation, with Hispanic heads greatly decreasing their take-up of food stamps over time. Findings from Hoynes, Schanzenbach, and Almond (2016) imply that this reduction in SNAP may increase the incidence of metabolic syndrome in adulthood for children in affected households and have long run effects on health.

B. Birth Outcomes

We next turn to our results on birth outcomes from Texas. Table 3 presents these results. We measure the prevalence of premature births and low birth weights. In column 1, we find that the share of premature births increase by 1.1 percentage points for Hispanic mothers post-SC activation, a 13 percent increase from the pre-period Hispanic mean of 8.6 percent. These estimates are unchanged with the addition of the interaction between black and a post-SC indicator in column 2. We also find that SC was associated with a 0.5 percentage point increase in share of low birth weight babies born to Hispanic women (columns 3 and 4), a 13 percent increase from the pre-period mean of 3.8 percent. These results are similar regardless of the racial comparison group (see Appendix Table A3).

These results are consistent with Novak et al. (2017), who find that after a raid in Iowa, infants born to Hispanic women, both native and immigrant, had a 24 percent higher risk of low birth weight compared to one year prior. The authors attribute these effects to racialized stressors that affect Latino immigrants and USA-born co-ethnics. Our results are also consistent with recent work by Hainsmueller et al. (2017), who find that unauthorized mothers' DACA eligibility, which temporarily granted protection from deportation, significantly decreased adjustment and anxiety disorder diagnoses among their American children.

Figure 5 presents our event study estimates of SC activation for non-Hispanic whites, non-Hispanic blacks, and Hispanics. These figures reveal that in the period prior to SC, birth outcomes were neither improving nor declining for any racial/ethnic groups. However, the introduction of SC had a sharp and deleterious effect on the outcomes of Hispanic babies, with no corresponding effect on the outcomes of non-Hispanic white or black babies.

According to Black, Devereux, and Salvanes (2007), a 10 percent increase in birth weight is associated with a one percent increase in high school completion rates and a one percent increase in earnings. To translate this continuous measure into our dichotomized findings, we assume the average baby is about 7 pounds (or approximately 3175 grams) and a low birthweight cutoff is 5.5 pounds or 2500 grams. Thus, if a normal weight baby drops below the low birthweight threshold,

there is an associated 27 percent decline in birthweight, translating into a 2.7 percent decrease in high school completion and earnings. Our estimates therefore imply that 13 percent of the babies born to Hispanic women after SC activation may experience these adverse outcomes.

C. Affordable Care Act Sign-up

We begin by presenting estimates of Equation 7, where we measure the relationship between the cumulative number of detainers issued per unauthorized Hispanic on the share of eligible Hispanics that signed up for the ACA. Panel A of Table 5 presents these results adding more controls in each column. In column 1, with no additional controls other than state fixed effects, we find that a ten percent increase in the share of Hispanics detainers issued is associated with 0.24 percent reduction in Hispanic sign-ups for the ACA. This estimate halves in magnitude when we add additional controls in each respective column of Table 1. Column 2 adds share Bush versus Kerry in the 2004 Presidential election, the Hispanic sex ratio, per capita income, and the percent working age. Column 3 adds the share Black ACA signup, and column 4 adds FBI index crimes per capita. In our preferred specification with the full set of controls (column 4), we find that a ten percent increase in the share of Hispanics detainers issued is associated with 0.10 percent reduction in Hispanic sign-ups for the ACA.

To address the potential endogeneity of detainers activity under SC, we now turn to our Bartik-style instrument, which predicts the estimated share of Hispanic detainers using plausibly exogenous variation in baseline shares of Hispanic foreign-born across counties as described in Equation 8. Panel B of Table 5 presents our first stage estimates regressing the share of Hispanic detainers issued on our Bartik instrument. In our preferred specification (column 4), we find a strong first-stage relationship between our instrument and endogenous variable (F-statistic = 17.35), with a 10 percent increase in predicted share Hispanic detainers issued associated with a 2.6 percent increase in actual share Hispanic detainers.

Panel C of Table 5 presents our two-stage least squares results. With only state fixed effects (column 1), we find that a 10 percent increase in detainers is associated with a 4.0 percent reduction in Hispanic ACA sign-ups. Results are similar but smaller in magnitude with the addition of county-level baseline controls. In our preferred specification (column 4), we find that a 10 percent increase in detainers is associated with a 2.3 percent reduction in Hispanic ACA sign-ups. To put this estimate in perspective, SC lead to the issuance of roughly 1.5 million detainers during the 2008 to 2013 time period. We estimate that there were roughly nine million unauthorized Hispanics during this time period, suggesting that approximately 16 percent of the unauthorized population was issued a detainer. In combination with our estimates, our results imply that SC was associated with a 3.7 percentage point reduction in Hispanic sign-up under the ACA, a 33 percent decrease from the mean Hispanic sign-up rate of 11 percent.

We also provide further evidence that our results are causal. Appendix Table A5 presents a series of robustness checks that explore alternative measures of immigration activity using share of hispanics removed. Our two-stage least squares results suggest a similar pattern when using these

alternative proxies, with generally larger effects on ACA take-up for a ten percent increase in share removed versus share issued detainers.

In addition, given that SC targeted unauthorized Hispanic individuals, we should not expect to find that the intensity of SC led to decreases in ACA sign-up among other racial/ethnic groups. As a placebo test, we regress our measure of share Hispanic detainers issued on share of eligible blacks and eligible whites that signed up for the ACA. Results in Appendix Table A6 suggest no significant relationship in our two-stage least squares results between SC intensity and either black or white ACA sign-up, suggesting that our findings are causal.

VII. Mechanisms

A. Fear

If our estimated spillover effects are driven by deportation fear, then households and communities with more mixing or exposure between unauthorized and citizen Hispanics should experience larger effects. We explore this in the PSID, ACS, and ACA data, by testing whether our results are more pronounced for mixed-status households or locations where exposure between citizens and non-citizen Hispanics is larger (see Cutler, Glaeser, and Vigdor 1999).

In columns 1 and 2 of Table 5, we explore the differential impact of SC activation for Hispanic households that are mixed-status. In the PSID data, we define a household as mixed-status if any parent or spouse of the head of household is Hispanic foreign born. In the ACS data, we define mixed-status households based on if any member of the household is a non-citizen Hispanic. In both specifications, we find substantially larger effects of SC on Hispanic citizen households that are mixed-status. Our estimate in column 1 suggests that post-SC, mixed Hispanic households reduce their take-up of food stamps by an additional 18.4 percentage points relative to non-mixed households, representing an overall decrease of 30.2 percentage points, a 74 percent from the pre-period mean in the PSID. Similarly, our ACS estimates imply that SC activation in an county with a ten percent higher share of mixed-status households decreases take-up of food stamps by an additional 3.9 percentage points, representing an overall decrease of 5.0 percentage points, a 22 percent decrease from the pre-period mean in the ACS.

Interestingly, we find that the effects of SC on take-up are not significantly different between places with above median versus below median rate of actual removals under SC (column 3). In contrast, we find in column 4 that our effects are larger where the ratio of non-violent (often traffic-related offenses) to violent Hispanic detainers issued post-arrest are highest. Taken together, these results suggest that the fear imposed by arrest following a low-level misdemeanor or traffic violation, even if eventually released and not deported, may matter most to Hispanics in reducing take-up, rather than the actual objective risk of removal from the United States.

Next, we proxy for fear using deportation-related searches at the Nielsen media markets level using publicly available Google Trends data. For each year between 2008-2014, we sum deportation-related searches normalized by searches for soap operas and sports. For each media market, we then

create an average ranking of these normalized searches across years, with a higher ranking indicating a higher prevalence of deportation-related searches. In column 6, we interact our Hispanic and post-SC indicator with this average annual ranking. We find that the strongest post-SC responses by Hispanic households are in locations where deportation fear proxied for Google searches is highest, further suggesting that fear may be driving our results.

Lastly, we explore the role of sanctuary cities and counties. As described previously, sanctuary cities share in common their restrictions on how much local police cooperate with ICE requests to detain unauthorized immigrants. If fear explains our findings, then Hispanic households in sanctuary cities should have less fear and thus a lower response to SC. Indeed, in column 7, when we interact our Hispanic and post-SC indicator with an indicator for whether a county has a sanctuary policy, we find that almost all of our main effects are driven by locations with no sanctuary policy, and a marginally significant and *positive* effect of SC activation on Hispanics in sanctuary cities.

Although less precise, we also find evidence consistent with fear being perpetuated through networks explaining our effects on the ACA. See Appendix Table A7. In column 1, we find suggestive evidence (although not statistically significant) that greater SC intensity in areas where Hispanic citizens and non-citizens were more exposed led to larger decreases in Hispanic ACA sign-up. Similarly, we also find suggestive evidence that fear may be irrational using this cross-sectional design, finding no differential impact of SC intensity depending on objective risk of removal from the country (column 2).

B. Information

Finally, information sharing might explain our findings to the extent that individuals rely on other people from their networks about information on public programs, with prior work suggesting that take-up of food stamps and other programs increases with greater information on eligibility and outreach (see Daponte et al. 1999 and Aizer 2003). In particular, information might be salient for immigrant communities to the extent that there is greater confusion or uncertainty about eligibility.

In our context, greater immigration enforcement may reduce take-up of public programs among citizen Hispanic households if they lose access to information as non-citizen co-ethnics in their networks reduce take-up. We partially test this hypothesis by comparing our estimated effects for households that had never previously taken up the relevant public program prior to SC versus households that previously took up the program following Aizer and Currie (2004). If a household has previously taken up the program, the household will likely already have information about the program, such as eligibility and how to apply. As a result, if information explains our findings, we would expect to find smaller effects of SC activation for prior use households.

Column 5 of Table 2 presents these results. We find remarkably similar effects among the prior users sample in the PSID compared to the full PSID sample, finding that SC activation reduced Hispanic heads of household take-up by 23.6 percentage points, a 37 percent decrease from the pre-period mean. These results suggest that our main findings are unlikely due to Hispanic households being less likely to receive information about public programs as their co-ethnics reduce sign up.

This finding, combined with the heterogeneous effects described above, also lessen the likelihood that an explanation like stigma is driving our results.

VIII. Conclusion

In this paper, we test the hypothesis that linkages between authorized and unauthorized individuals might reduce safety net participation in the presence of enhanced immigration enforcement activity. Leveraging the differential roll-out and intensity of Secure Communities under the Obama Administration, we find that authorized Hispanic Americans are indeed sensitive to such enforcement although they themselves are not at risk of removal – a spillover effect. In particular, we find significant reductions in food stamp and ACA take-up, as well as substantial and consequential upticks in poor health outcomes for Hispanic babies.

We find evidence that our results may be driven by deportation fear rather than lack of benefit information or stigma. Mixed-status households, areas with higher exposure between authorized and unauthorized Hispanics, and areas with greater Google searches for deportation-related terms exhibit larger decreases in take-up in response to SC. Our results on the ACA also suggest that the deleterious effects of deportation fear are likely not circumscribed to Hispanic households and communities. Since Hispanics tend to have better health outcomes than similarly situated low-income whites or blacks, reduced participation on their behalf could translate into higher premiums for other demographic groups. More broadly, this paper highlights how safety net programs interact with other government policies, and underscores how competing objectives give rise to programs that may have unexpected consequences for low-income households.

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Table 1: Triple Differences Estimation Balance (2005-2007)

	All	Hispanic-White Late vs. Early	Hispanic-Black Late vs. Early
	(1)	(2)	(3)
Outcome	*Panel A: ACS Sample N = 27,531*		
Share Food Stamp	0.186	0.019	0.051***
	(0.208)	(0.014)	(0.016)
Average Family Size	1.758	−0.128*	0.097
	(1.107)	(0.076)	(0.107)
Average # Children	0.463	−0.101***	0.028
	(0.628)	(0.038)	(0.058)
Poverty FPL	118.541	3.994	8.248
	(87.011)	(6.566)	(7.964)
Δ Share Food Stamp	0.006	0.004	0.047
	(0.364)	(0.024)	(0.044)
Δ Average Family Size	−0.024	0.011	0.070
	(2.036)	(0.111)	(0.133)
Δ Average # Children	−0.034	−0.053	0.032
	(1.253)	(0.066)	(0.088)
Δ Poverty FPL	6.869	13.710	−1.612
	(131.119)	(8.933)	(11.85)
Outcome	*Panel B: Texas Sample N = 1,629*		
Share Low Birth Weight	0.048	−0.006*	0.051***
	(0.057)	(0.003)	(0.012)
Share Premature Birth	0.092	−0.002	0.076***
	(0.082)	(0.005)	(0.013)
Per Capita College Births	0.017	0.002**	0.009***
	(0.016)	(0.001)	(0.016)
Per Capita HS Births	0.018	−0.005**	0.009***
	(0.016)	(0.002)	(0.002)
Δ Share Low Birth Weight	0.002	0.005	−0.004
	(0.063)	(0.005)	(0.015)
Δ Share Premature Birth	−0.002	−0.011	−0.020
	(0.107)	(0.008)	(0.020)
Δ Per Capita College Births	−0.001	−0.000	0.001
	(0.010)	(0.001)	(0.015)
Δ Per Capita HS Births	0.003	−0.003	−0.004
	(0.015)	(0.004)	(0.003)

Note: Column 1 presents sample means of variables with standard deviations in parentheses. Columns 2 and 3 report coefficients from a balance test of the difference in our main outcomes on an indicator variable for "late" versus "early" activation counties, where late activation is defined as Secure Communities being activated after 2010. All regressions control for state (division)-by-race and state (division)-by-year fixed effects. Observations are weighted by the race-specific population (births) in each county. Robust standard errors clustered at the county level are in parentheses.

Table 2: Triple Differences Estimation – Safety Net Take-up

Sample	*PSID Citizens* (1)	*ACS Citizens* (2)	*PSID Citizens* (3)	*ACS Citizens* (4)	*PSID Ever Users* (5)
Hispanic × Post	−0.185***	−0.017***	−0.162**	−0.018***	−0.236**
	(0.069)	(0.005)	(0.072)	(0.005)	(0.107)
Post	0.123***	0.008***	0.099**	0.009***	0.244**
	(0.042)	(0.003)	(0.043)	(0.003)	(0.099)
Black × Post			0.070*	−0.001	
			(0.039)	(0.004)	
Pre-Period Hispanic Mean	0.409	0.230	0.409	0.230	0.642
Fixed Effects		State-Year, State-Race, County			
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Observations	31,290	90,090	31,290	90,090	15,409
Number Clusters	679	3,060	679	3,060	321

Note: Data from PSID from 2003-2015 and ACS from 2006-2016. The data are limited to heads of households with less than a high school, our high participation sample. The citizens sample in the PSID includes all individuals from families where the head of household was born and grew up in the United States. The citizens sample in the ACS includes heads of households that are U.S. citizens. The prior users sample in the PSID includes all individuals from families who had previously taken up food stamps prior to the earliest Secure Communities activation in 2008. Baseline controls in the PSID include sex of household head, marital status, family size, age of youngest child, income relative to federal poverty line, whether spouse has a high school degree, health status, indicators for where the head and the head's parents grew up, and FBI crime decile-by-race fixed effects. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, state-by-year fixed effects, state-by-race fixed effects, and post-Recession-by-race fixed effects. Observations in the PSID are weighted by the PSID family weight. Observations in the ACS are weighted by number of Hispanics in each county. Robust standard errors are clustered at the county level.

Table 3: Triple Differences Estimation – Texas Birth Outcomes

Outcome	*Premature*	*Premature*	*Low Birthweight*	*Low Birthweight*
	(1)	(2)	(3)	(4)
Hispanic × Post	0.011***	0.011***	0.005***	0.005***
	(0.002)	(0.002)	(0.001)	(0.001)
Post	−0.003*	−0.003*	−0.002	−0.002
	(0.001)	(0.002)	(0.002)	(0.002)
Black × Post		0.001		−0.003
		(0.003)		(0.002)
Pre-Period Hispanic Mean	0.086	0.086	0.038	0.038
Fixed Effects	Region-Year, Region-Race, County			
Baseline Controls	Yes	Yes	Yes	Yes
Observations	4,940	4,940	4,940	4,940
Number Clusters	228	228	228	228

Note: Data from Texas births data from 2005-2015. Baseline controls include share no prenatal care, per capita college childbearing women, per capita high school childbearing women, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, health service region-by-year fixed effects, health service region-by-race fixed effects, and post-Recession-by-race fixed effects. Observations are weighted by the race-specific total number of births each county in 2005. Robust standard errors are clustered at the county level.

Table 4: OLS and 2SLS Results – ACA Take-up

	Outcome: Share Hispanic ACA Take-up			
	(1)	(2)	(3)	(4)
			Panel A: OLS Results	
Share Hispanic Detainers	−0.024***	−0.011**	−0.009**	−0.010**
	(0.004)	(0.005)	(0.005)	(0.005)
			Panel B: First Stage	
Shift-Share IV	0.210***	0.266***	0.262***	0.257***
	(0.068)	(0.063)	(0.062)	(0.062)
			Panel C: 2SLS Results	
Share Hispanic Detainers	−0.397**	−0.252***	−0.226**	−0.230**
	(0.197)	(0.097)	(0.091)	(0.092)
F-Statistic	9.61	18.09	17.57	17.35
Fixed Effects			State	
Controls	No	Baseline	Baseline + Black ACA	Baseline + Black ACA + Crime
Observations	1,879	1,897	1,897	1,897

Note: Data from the ACA and CMS in the 37 states with federal exchanges. The dependent variable is the share of eligible Hispanics that sign up for the ACA in each county. All specifications contain state fixed effects. Baseline controls include share Bush versus Kerry in the 2004 Presidential election, the Hispanic sex ratio, per capita income, and the percent working age. ACA controls include share Black ACA signup. Crime controls include FBI index crimes per capita. Observations are weighted by the estimated number of Hispanics eligible for the ACA in each county. Robust standard errors are in parentheses.

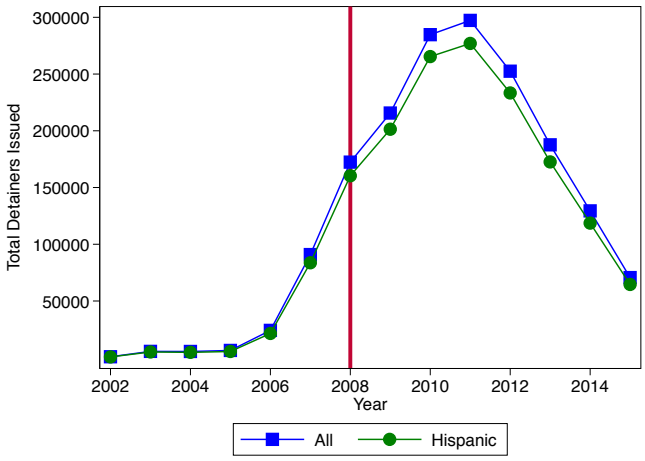
Table 5: Triple Differences Estimation – Safety Net Take-up Heterogeneity

Sample	*PSID Citizens*	*ACS Citizens*	*PSID Citizens*	*ACS Citizens*	*ACS Citizens*	*ACS Citizens*	*ACS Citizens*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hispanic × Post	−0.118*	−0.011*	−0.155**	−0.009	−0.007	0.025*	−0.025***
	(0.071)	(0.006)	(0.078)	(0.008)	(0.006)	(0.014)	(0.007)
Hispanic × Mixed × Post	−0.184*	−0.388***					
	(0.100)	(0.134)					
Hispanic × Post × Removal Share			−0.105	−0.014			
			(0.078)	(0.009)			
Hispanic × Post × Petty Severe Ratio					−0.030***		
					(0.008)		
Hispanic × Post × Google Search Rank						−0.0001***	
						(0.00004)	
Hispanic × Post × Sanctuary City							0.016*
							(0.009)
Pre-Period Hispanic Mean	0.409	0.230	0.439	0.230	0.230	0.230	0.230
Fixed Effects	State-Year, State-Race, County						
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	
Observations	31,290	90,090	29,393	80,759	90,090	90,090	90,090
Number Clusters	679	3,060	588	3,060	3,060	3,060	3,060

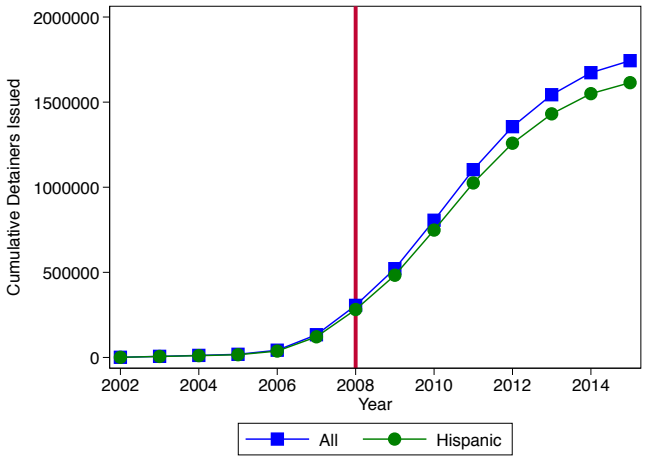
Note: Data from PSID from 2003-2015 and ACS from 2006-2016. The data are limited to heads of households with less than a high school, our high participation sample. The citizens sample in the PSID includes all individuals from families where the head of household was born and grew up in the United States. The citizens sample in the ACS includes heads of households that are U.S. citizens. Mixed status in the PSID is defined as a Hispanic citizen head of household who has parents or a spouse that are Hispanic foreign born. Mixed status in the the ACS is defined as X. The ratio of petty versus severe detainers measures the ratio of detainers issued for minor offenses like traffic infractions to serious violent offenses. Fear is measured as X. Baseline controls in the PSID include sex of household head, marital status, family size, age of youngest child, income relative to federal poverty line, whether spouse has a high school degree, health status, indicators for where the head and the head's parents grew up, and FBI crime decile-by-race fixed effects. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, state-by-year fixed effects, state-by-race fixed effects, and post-Recession-by-race fixed effects. Observations in the PSID are weighted by the PSID family weight. Observations in the ACS are weighted by the race-specific population in each county. Robust standard errors are clustered at the county level.

Figure 1: Detainers by Year

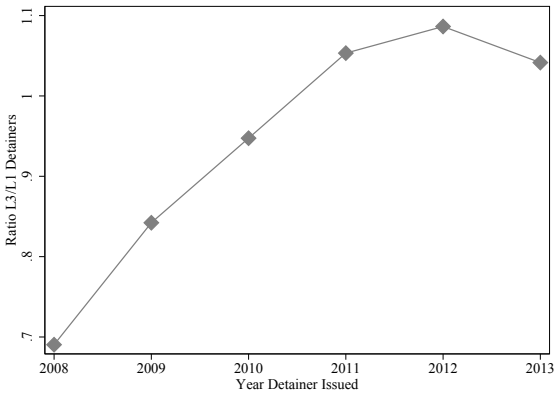
Panel A: Total by Year



Panel B: Cumulative by Year

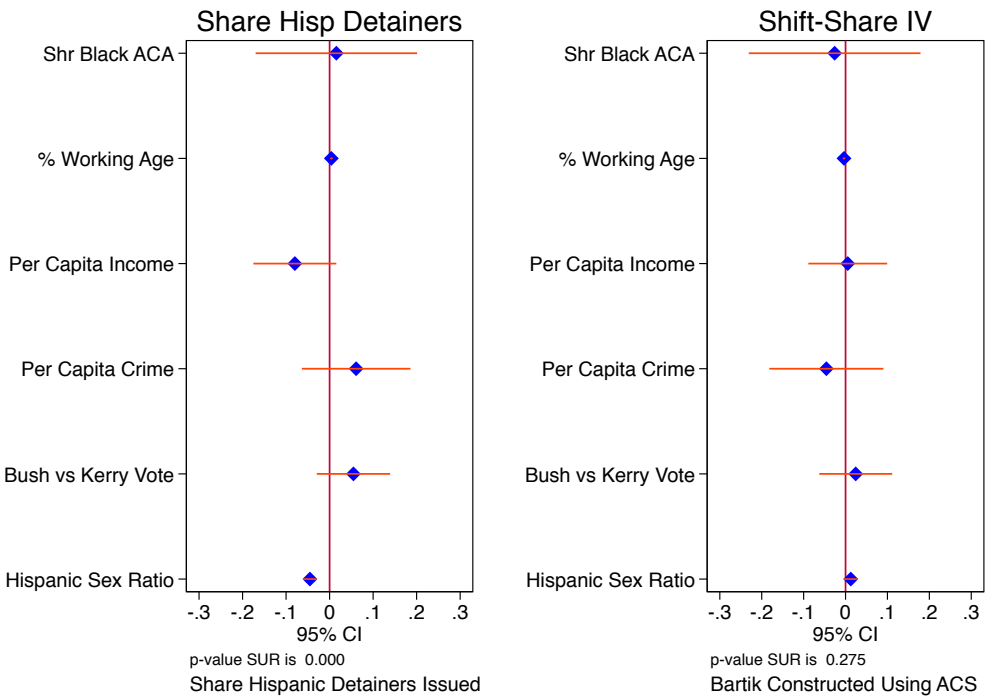


Panel C: Ratio of Low-Level to Violent Offenses



Note: Data from FOIA.

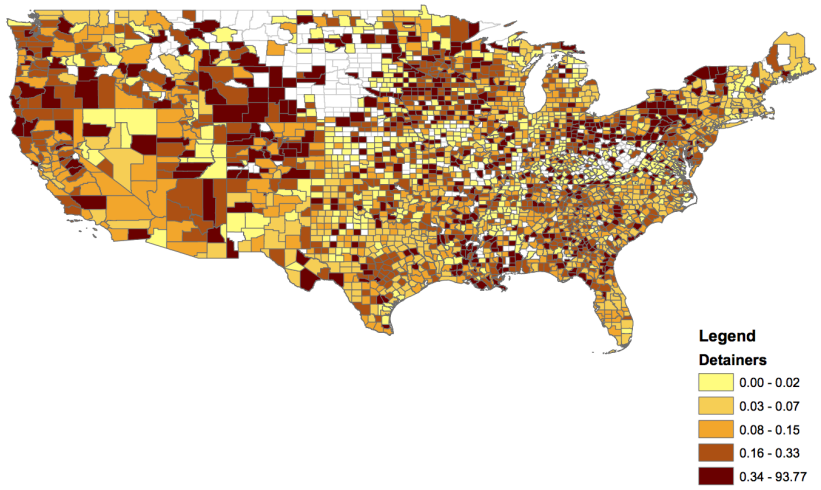
Figure 2: Relationship between Share Hispanic Detainers, Instrument, and Baseline Controls



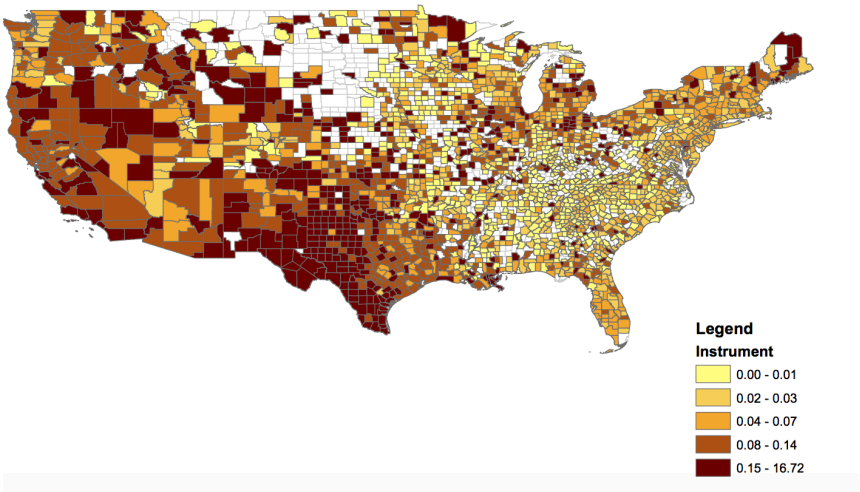
Note: Data from FOIA , ACS, ACA. Share hispanic detainers is the total number of Hispanic detainers normalized by the predicted number of undocumented hispanics based on data from the American Community Survey. The shift-share instrument is constructed as the total predicted number of detainers normalized by the predicted number of undocumented hispanics based on data from the American Community Survey. This figure represents seemingly unrelated regressions (SUR) on each baseline characteristic on the share hispanic detainers issued and our shift-share instrument. All specifications contain state fixed effects.

Figure 3: Detainers and Shift-Share IV

Panel A: Share Hispanic Detainers

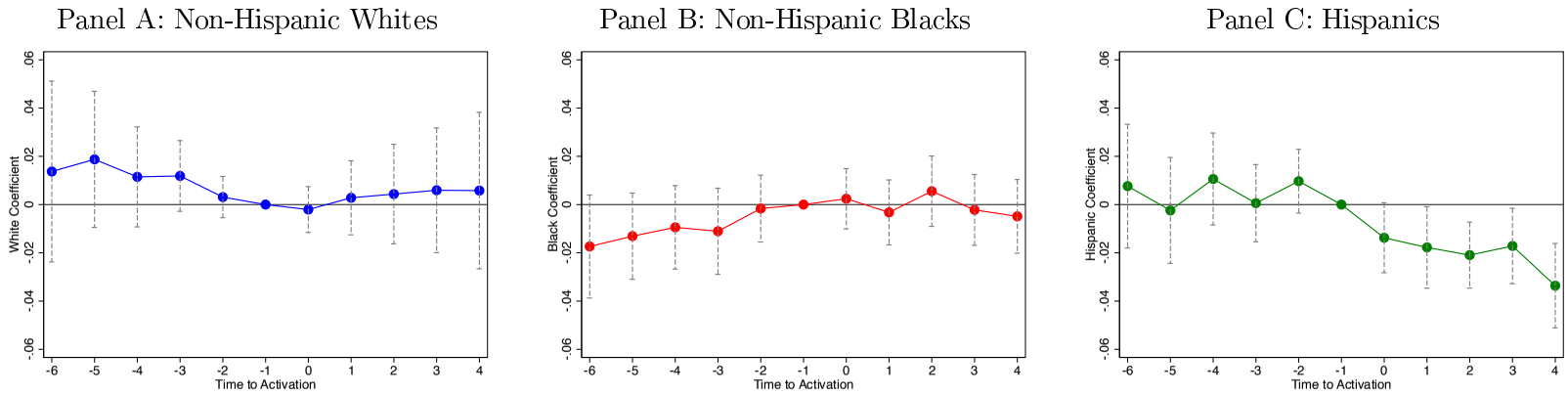


Panel B: Shift-Share IV



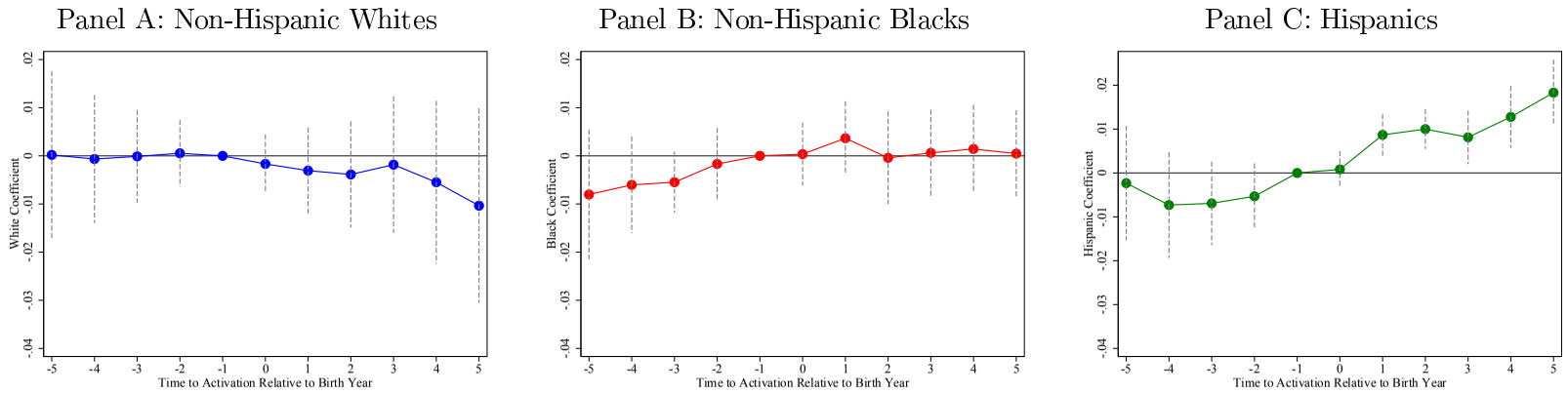
Note: Data from FOIA.

Figure 4: Event Study of Food Stamp Take-Up



Note: Data from ACS from 2006-2016. The data are limited to heads of households with less than a high school, our high participation sample. The citizens sample in the ACS includes heads of households that are U.S. citizens. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, state-by-year fixed effects, state-by-race fixed effects, and post-Recession-by-race fixed effects. Observations in the ACS are weighted by the race-specific population in each county. Robust standard errors are clustered at the county level.

Figure 5: Event Study of Share Premature Births



Note: Note: Data from Texas births data from 2005-2015. Baseline controls include share no prenatal care, per capita college childbearing women, per capita high school childbearing women, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, health service region-by-year fixed effects, health service region-by-race fixed effects, and post-Recession-by-race fixed effects. Observations are weighted by the race-specific total number of births each county in 2005. Robust standard errors are clustered at the county level.

Appendix Table A1: Triple Differences Estimation Balance in PSID (2002-2006)

	All	Hispanic-White Late vs. Early	Hispanic-Black Late vs. Early
	(1)	(2)	(3)
Outcome		*PSID Sample N = 15,997*	
Share Food Stamp	0.238	−0.094	0.861***
	(0.379)	(0.335)	(0.288)
Average Family Size	2.812	0.674	−0.692
	(1.453)	(1.764)	(0.772)
Poverty FPL	242.155	90.800	−176.509
	(195.103)	(113.926)	(116.577)
Δ Share Food Stamp	0.082	−0.112	−0.183
	(0.426)	(0.561)	(0.599)
Δ Average Family Size	−0.206	−0.300	−1.161
	(1.222)	(0.599)	(1.147)
Δ Poverty FPL	−4.894	202.400	549.361*
	(185.106)	(180.264)	(267.810)

Note: Column 1 presents weighted sample means of variables with standard deviations in parentheses. Columns 2 and 3 report coefficients from a balance test of the difference in our main outcomes on an indicator variable for "late" versus "early" activation counties, where late activation is defined as Secure Communities being activated after 2010. All regressions control for state-by-race and state-by-year fixed effects. Robust standard errors clustered at the county level are in parentheses.

Appendix Table A2: Main Results – Robustness to Samples and Controls

Robustness	*Alternative Samples*				*Adding % Hispanic*			
Outcome	*Food Stamp*				*Food Stamp*		*Premature*	*Low Birth*
Sample	*PSID*	*ACS*	*ACS Match PSID*	*ACS HR Female*	*PSID*	*ACS*	*TX*	*TX*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hispanic × Post	−0.185***	−0.017***	−0.044***	−0.014**	−0.184***	−0.017***	0.012***	0.005***
	(0.069)	(0.005)	(0.011)	(0.006)	(0.069)	(0.005)	(0.002)	(0.001)
Post	0.123***	0.008***	0.012	0.004	0.123***	0.008***	−0.004**	−0.002
	(0.042)	(0.003)	(0.012)	(0.004)	(0.042)	(0.003)	(0.001)	(0.001)
Fixed Effects	State(Region)-Year, State(Region)-Race, County							
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	31,290	90,090	996	80,432	31,290	90,090	4,438	4,438

Note: In columns 1 and 2, we present our main specifications on food stamp take-up from the PSID and ACS. In column 3, we estimate our main specification in the ACS using a sample that more closely approximates the PSID sample. In column 4, we estimate our main specification in the ACS using a sample of highest-ranking females (either female head of household or female spouse). In columns 5 through 8, we present our main results on food stamp take-up and birth outcomes adding an additional control for the percent Hispanic estimated from the ACS (or PSID). Baseline controls in the PSID include sex of household head, marital status, family size, age of youngest child, income relative to federal poverty line, whether spouse has a high school degree, health status, indicators for where the head and the head's parents grew up, and FBI crime decile-by-race fixed effects. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. Baseline controls in the TX births data include share no prenatal care, per capita college childbearing women, per capita high school childbearing women, and FBI crime decile-by-race fixed effects. Observations in the PSID are weighted by the PSID family weight. Observations in the ACS are weighted by the race-specific population in each county. Observations in the TX birth data are weighted by the race-specific total number of births each county in 2005. Robust standard errors are clustered at the county level.

Appendix Table A3: Main Results – Race Specific Comparisons

Outcome	*Food Stamp Take-up*				*Premature*		*Low Birth*	
Sample	*PSID*	*PSID*	*ACS*	*ACS*	*TX*	*TX*	*TX*	*TX*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hispanic × Post	−0.188***	−0.160**	−0.016***	−0.018***	0.010***	0.010***	0.003**	0.007***
	(0.063)	(0.077)	(0.006)	(0.005)	(0.002)	(0.003)	(0.001)	(0.002)
Post	0.134*	0.035	0.011*	0.009***	−0.002	−0.005	−0.001	−0.007***
	(0.074)	(0.054)	(0.006)	(0.004)	(0.003)	(0.003)	(0.001)	(0.003)
Comparison	H/B	H/W	H/B	H/W	H/B	H/W	H/B	H/W
Fixed Effects	State-Year, State-Race, County				Region-Year, Region-Race, County			
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,442	13,919	56,447	62,351	3,983	2,961	3,983	2,961

Note: In this table, we replicate our main results comparing Hispanics to each race group. "H/B" refers to Hispanics versus non-Hispanic blacks and "H/W" refers to Hispanics versus non-Hispanic whites. Baseline controls in the PSID include sex of household head, marital status, family size, age of youngest child, income relative to federal poverty line, whether spouse has a high school degree, health status, indicators for where the head and the head's parents grew up, and FBI crime decile-by-race fixed effects. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. Baseline controls in the TX births data include share no prenatal care, per capita college childbearing women, per capita high school childbearing women, and FBI crime decile-by-race fixed effects. Observations in the PSID are weighted by the PSID family weight. Observations in the ACS are weighted by the race-specific population in each county. Observations in the TX birth data are weighted by the race-specific total number of births each county in 2005. Robust standard errors are clustered at the county level.

Appendix Table A4: Main Results – No Weights

Outcome	*Food Stamp Take-up*			*Premature*	*Low Birth*
Sample	*PSID*	*ACS*	*ACS, Hisp>25*	*TX*	*TX*
	(1)	(2)	(3)	(4)	(5)
Hispanic × Post	−0.114**	−0.005	−0.007	0.010***	0.004
	(0.052)	(0.005)	(0.005)	(0.004)	(0.003)
Post	0.017	0.016***	0.017***	−0.003	−0.002
	(0.035)	(0.006)	(0.006)	(0.005)	(0.004)
Fixed Effects	State-Year, State-Race, County			Reg-Year, Reg-Race, County	
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Observations	31,359	90,293	88,637	4,992	4,992

Note: Note: In this table, we replicate our main results with no weights. Baseline controls in the PSID include sex of household head, marital status, family size, age of youngest child, income relative to federal poverty line, whether spouse has a high school degree, health status, and indicators for where the head and the head's parents grew up. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. Baseline controls in the TX births data include share no prenatal care, per capita college childbearing women, per capita high school childbearing women, and FBI crime decile-by-race fixed effects. Observations in the PSID are weighted by the PSID family weight. Observations in the ACS are weighted by the race-specific population in each county. Observations in the TX birth data are weighted by the race-specific total number of births each county in 2005. Robust standard errors are clustered at the county level.

Appendix Table A5: 2SLS Results – ACA Take-up Robustness

	Detainers		*Removals*	
	OLS	*2SLS*	*OLS*	*2SLS*
	(1)	(2)	(3)	(4)
Share Hispanic Detainers	−0.010**	−0.230**		
	(0.005)	(0.092)		
Share Hispanic Removals			−0.029	−1.812**
			(0.030)	(0.894)
F-Statistic		10.42		6.48
Fixed Effects		State		
Controls		Baseline, Black ACA, Crime		
Observations	1,879	1,897	1,897	1,897

Note: The dependent variable is the share of eligible Hispanics that sign up for the ACA. Columns 1-2 present results using the share hispanic detainers. Columns 3-4 present results using the share hispanic removals. All regressions control for state fixed effects, share Bush versus Kerry in the 2004 Presidential election, the Hispanic sex ratio, per capita income, the percent working age, FBI index crime counts per capita, and missing indicators for these variables. Observations are weighted by the estimated number of Hispanics eligible for the ACA in each county. Robust standard errors are in parentheses.

Appendix Table A6: 2SLS Results – ACA Take-up Placebo

Outcome	*Share Black ACA*		*Share White ACA*	
	OLS	*2SLS*	*OLS*	*2SLS*
	(1)	(2)	(3)	(4)
Share Hispanic	−0.011*	−0.154	−0.013***	−0.020
Detainers	(0.006)	(0.133)	(0.004)	(0.027)
F-Statistic		7.16		10.49
Fixed Effects		State		
Controls		Baseline, Black ACA, Crime		
Observations	1,828	1,828	1,897	1,897

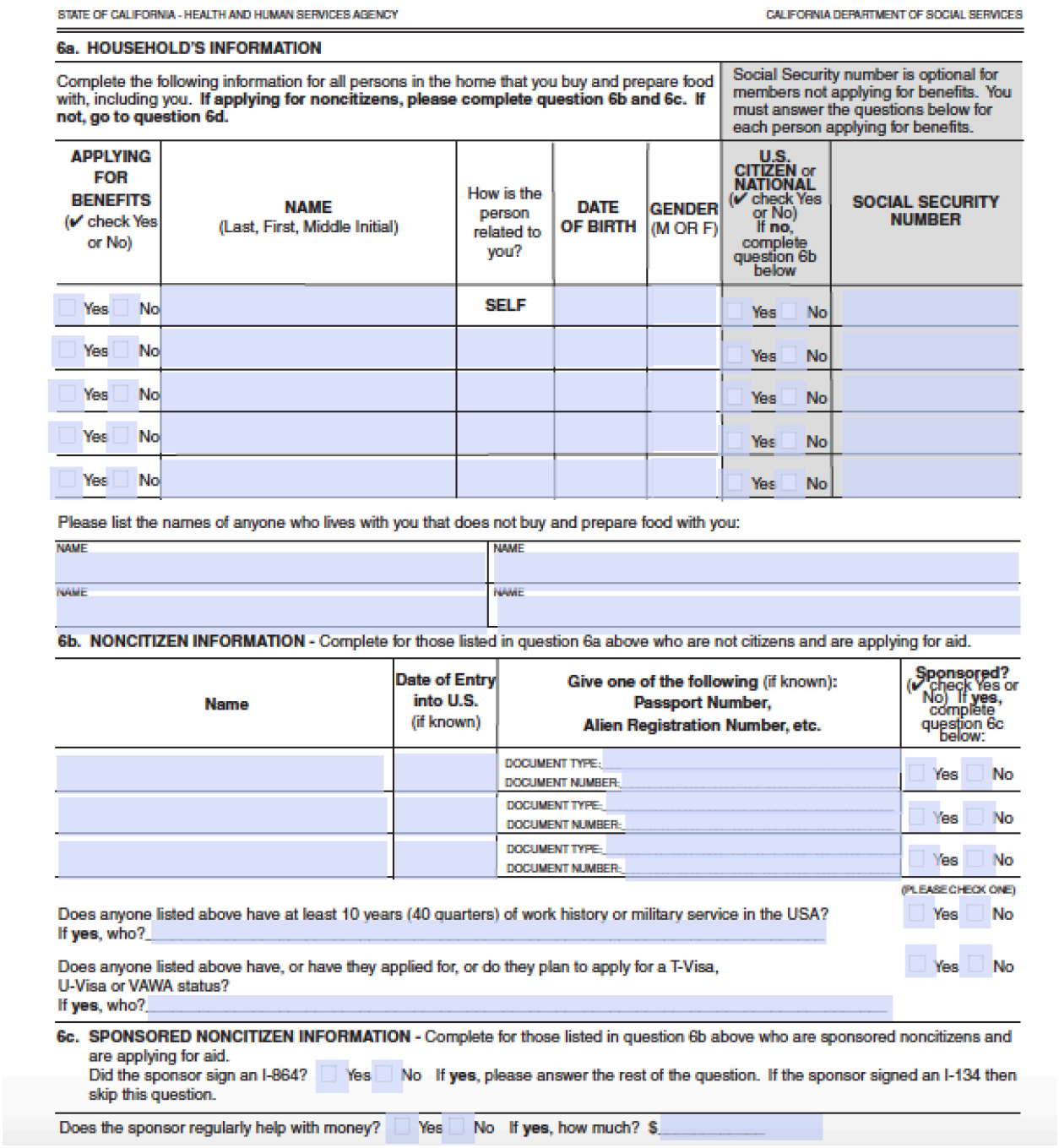
Note: The dependent variable in columns 1-2 is the share of eligible blacks signing up for the ACA. The dependent variable in columns 3-4 is the share of eligible whites signing up for the ACA. All regressions control for state fixed effects, share Bush versus Kerry in the 2004 Presidential election, the Hispanic sex ratio, per capita income, the percent working age, FBI index crime counts per capita, and missing indicators for these variables. Observations are weighted by the estimated number of blacks (or non-Hispanic whites) eligible for the ACA in each county. Robust standard errors are in parentheses.

Appendix Table A7: 2SLS Results – ACA Take-up Heterogeneity

	(1)	(2)
	Panel A: OLS Results	
Share Hispanic Detainers	-0.019^{***}	-0.021^{***}
	(0.006)	(0.007)
Share Hispanic Detainers	0.015^{*}	
× Exposure	(0.008)	
Share Hispanic Detainers		0.014^{*}
× Removal		(0.008)
	Panel B: 2SLS Results	
Share Hispanic Detainers	-0.180^{***}	-0.316^{**}
	(0.062)	(0.127)
Share Hispanic Detainers	−0.067	
× Exposure	(0.076)	
Share Hispanic Detainers		0.155^{*}
× Removal		(0.092)
F-Statistic	9.95	12.33
Fixed Effects	State	
Controls	Baseline, Black ACA, Crime	
Observations	1,897	1,897

Note: Data from ACA. Exposure is measured as X. Removals is measured as X. All specifications contain state fixed effects. Baseline controls include share Bush versus Kerry in the 2004 Presidential election, the Hispanic sex ratio, per capita income, and the percent working age. ACA controls include share Black ACA signup. Crime controls include FBI index crimes per capita from 2005-2009. Observations are weighted by the estimated number of Hispanics eligible for the ACA in each county. Robust standard errors are in parentheses.

Figure A1: California SNAP Application



STATE OF CALIFORNIA - HEALTH AND HUMAN SERVICES AGENCY — CALIFORNIA DEPARTMENT OF SOCIAL SERVICES

6a. HOUSEHOLD'S INFORMATION

Complete the following information for all persons in the home that you buy and prepare food with, including you. **If applying for noncitizens, please complete question 6b and 6c. If not, go to question 6d.**

Social Security number is optional for members not applying for benefits. You must answer the questions below for each person applying for benefits.

APPLYING FOR BENEFITS (✔ check Yes or No)	NAME (Last, First, Middle Initial)	How is the person related to you?	DATE OF BIRTH	GENDER (M OR F)	U.S. CITIZEN or NATIONAL (✔ check Yes or No) If **no**, complete question 6b below	SOCIAL SECURITY NUMBER
☐ Yes ☐ No		SELF			☐ Yes ☐ No	
☐ Yes ☐ No					☐ Yes ☐ No	
☐ Yes ☐ No					☐ Yes ☐ No	
☐ Yes ☐ No					☐ Yes ☐ No	
☐ Yes ☐ No					☐ Yes ☐ No	

Please list the names of anyone who lives with you that does not buy and prepare food with you:

NAME | NAME

NAME | NAME

6b. NONCITIZEN INFORMATION - Complete for those listed in question 6a above who are not citizens and are applying for aid.

Name	Date of Entry into U.S. (if known)	Give one of the following (if known): Passport Number, Alien Registration Number, etc.	Sponsored? (✔ check Yes or No) If **yes**, complete question 6c below:
		DOCUMENT TYPE: DOCUMENT NUMBER:	☐ Yes ☐ No
		DOCUMENT TYPE: DOCUMENT NUMBER:	☐ Yes ☐ No
		DOCUMENT TYPE: DOCUMENT NUMBER:	☐ Yes ☐ No

(PLEASE CHECK ONE)

Does anyone listed above have at least 10 years (40 quarters) of work history or military service in the USA? ☐ Yes ☐ No
If **yes**, who?_____

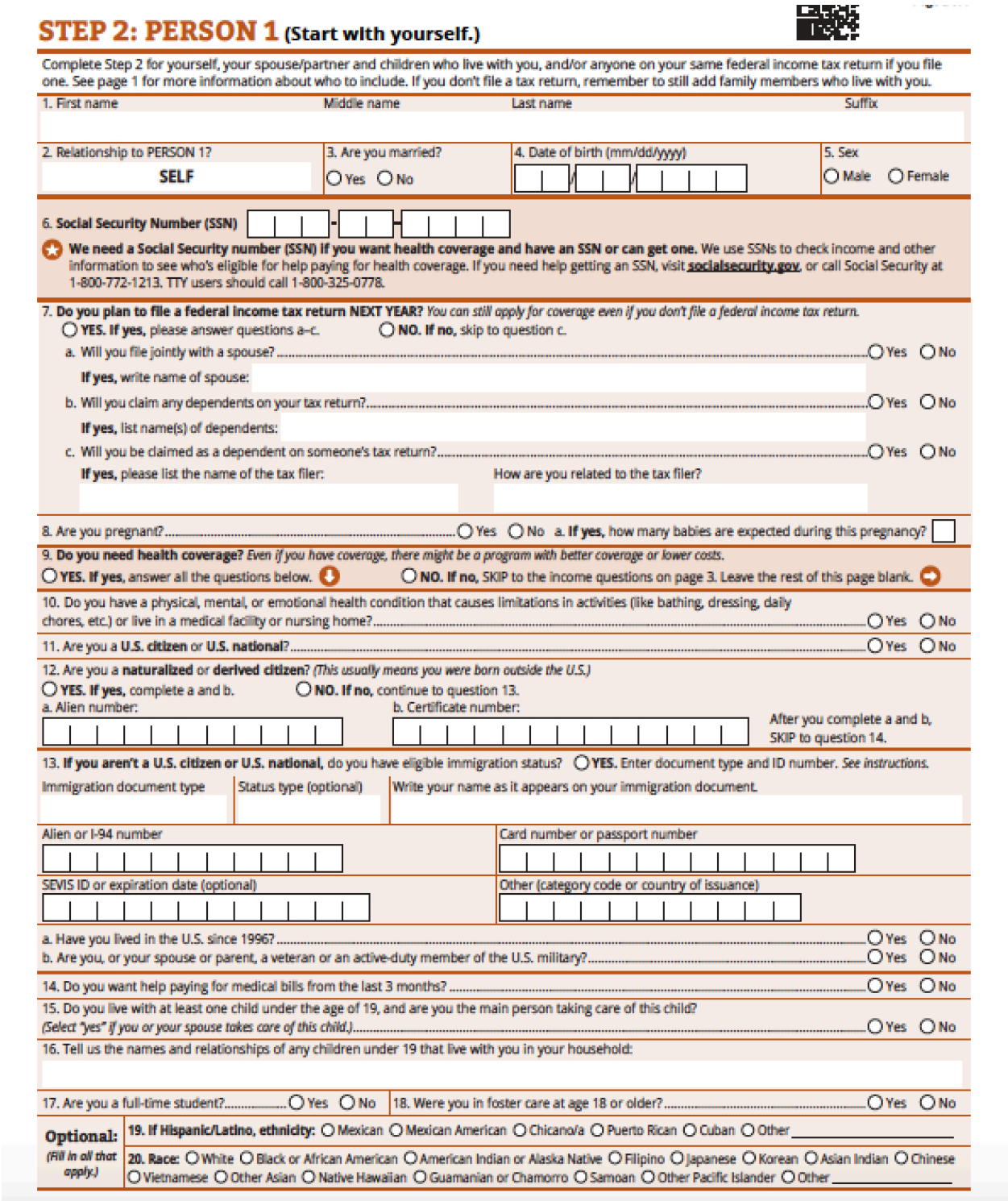
Does anyone listed above have, or have they applied for, or do they plan to apply for a T-Visa, U-Visa or VAWA status? ☐ Yes ☐ No
If **yes**, who?_____

6c. SPONSORED NONCITIZEN INFORMATION - Complete for those listed in question 6b above who are sponsored noncitizens and are applying for aid.
Did the sponsor sign an I-864? ☐ Yes ☐ No If **yes**, please answer the rest of the question. If the sponsor signed an I-134 then skip this question.

Does the sponsor regularly help with money? ☐ Yes ☐ No If **yes**, how much? $_____

Note: Data from section of California SNAP Application.

Figure A2: ACA Application



STEP 2: PERSON 1 (Start with yourself.)

Complete Step 2 for yourself, your spouse/partner and children who live with you, and/or anyone on your same federal income tax return if you file one. See page 1 for more information about who to include. If you don't file a tax return, remember to still add family members who live with you.

1. First name | Middle name | Last name | Suffix

2. Relationship to PERSON 1? SELF | 3. Are you married? ○ Yes ○ No | 4. Date of birth (mm/dd/yyyy) | 5. Sex ○ Male ○ Female

6. **Social Security Number (SSN)**

We need a Social Security number (SSN) if you want health coverage and have an SSN or can get one. We use SSNs to check income and other information to see who's eligible for help paying for health coverage. If you need help getting an SSN, visit **socialsecurity.gov**, or call Social Security at 1-800-772-1213. TTY users should call 1-800-325-0778.

7. **Do you plan to file a federal income tax return NEXT YEAR?** *You can still apply for coverage even if you don't file a federal income tax return.*
○ **YES. If yes,** please answer questions a–c. ○ **NO. If no,** skip to question c.
a. Will you file jointly with a spouse? ○ Yes ○ No
If yes, write name of spouse:
b. Will you claim any dependents on your tax return? ○ Yes ○ No
If yes, list name(s) of dependents:
c. Will you be claimed as a dependent on someone's tax return? ○ Yes ○ No
If yes, please list the name of the tax filer: | How are you related to the tax filer?

8. Are you pregnant? ○ Yes ○ No a. **If yes,** how many babies are expected during this pregnancy?

9. **Do you need health coverage?** *Even if you have coverage, there might be a program with better coverage or lower costs.*
○ **YES. If yes,** answer all the questions below. ○ **NO. If no,** SKIP to the income questions on page 3. Leave the rest of this page blank.

10. Do you have a physical, mental, or emotional health condition that causes limitations in activities (like bathing, dressing, daily chores, etc.) or live in a medical facility or nursing home? ○ Yes ○ No

11. Are you a **U.S. citizen** or **U.S. national**? ○ Yes ○ No

12. Are you a **naturalized** or **derived citizen**? *(This usually means you were born outside the U.S.)*
○ **YES. If yes,** complete a and b. ○ **NO. If no,** continue to question 13.
a. Alien number: | b. Certificate number: | After you complete a and b, SKIP to question 14.

13. **If you aren't a U.S. citizen or U.S. national,** do you have eligible immigration status? ○ **YES.** Enter document type and ID number. *See instructions.*
Immigration document type | Status type (optional) | Write your name as it appears on your immigration document.
Alien or I-94 number | Card number or passport number
SEVIS ID or expiration date (optional) | Other (category code or country of issuance)

a. Have you lived in the U.S. since 1996? ○ Yes ○ No
b. Are you, or your spouse or parent, a veteran or an active-duty member of the U.S. military? ○ Yes ○ No

14. Do you want help paying for medical bills from the last 3 months? ○ Yes ○ No

15. Do you live with at least one child under the age of 19, and are you the main person taking care of this child? *(Select "yes" if you or your spouse takes care of this child.)* ○ Yes ○ No

16. Tell us the names and relationships of any children under 19 that live with you in your household:

17. Are you a full-time student? ○ Yes ○ No | 18. Were you in foster care at age 18 or older? ○ Yes ○ No

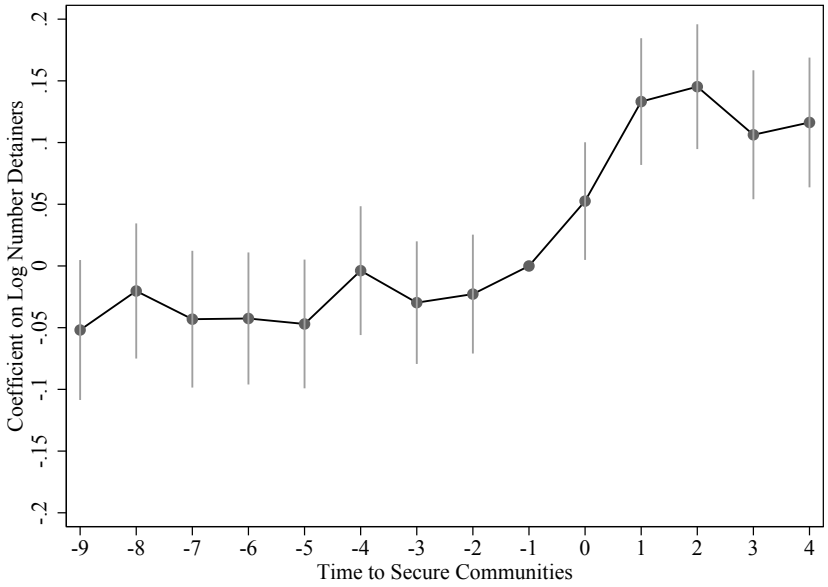
Optional: *(Fill in all that apply.)*

19. If Hispanic/Latino, ethnicity: ○ Mexican ○ Mexican American ○ Chicano/a ○ Puerto Rican ○ Cuban ○ Other _____

20. Race: ○ White ○ Black or African American ○ American Indian or Alaska Native ○ Filipino ○ Japanese ○ Korean ○ Asian Indian ○ Chinese ○ Vietnamese ○ Other Asian ○ Native Hawaiian ○ Guamanian or Chamorro ○ Samoan ○ Other Pacific Islander ○ Other _____

Note: Data from section of ACA Application from CMS.gov.

Figure A3: Detainers Event Study



Note: Data from FOIA. These figures represent event study estimates of the time to SC activation on the log number of detainers and removals. All specifications control for county fixed effects. Standard errors are clustered at the county level.