

# Fear and the Safety Net: Evidence from Secure Communities\*

Marcella Alsan<sup>†</sup>

Crystal S. Yang<sup>‡</sup>

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## Abstract

This paper explores the impact of fear on the incomplete take-up of 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 that empowers the federal government to check the immigration status of anyone arrested by local police, leading to the forcible removal of approximately 380,000 immigrants. We estimate the spillover effect of SC on the take-up of federal means-tested programs by Hispanic citizens. Though not at personal risk of deportation, Hispanic citizens may fear their participation could expose non-citizens in their network to immigration authorities. We find significant declines in SNAP and ACA enrollment, particularly among mixed-citizenship status households and in areas where deportation fear is highest. The response is muted for Hispanic households residing in sanctuary cities. Our results are most consistent with network effects that perpetuate fear rather than lack of benefit information or stigma.

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<sup>†</sup>Stanford Medical School, BREAD and NBER. Email: malsan@stanford.edu

<sup>‡</sup>Harvard Law School and NBER. Email: cyang@law.harvard.edu

## I. Introduction

Active enrollment in safety net programs in the United States is far from complete despite mounting evidence of high returns to health and human capital (Ashenfelter 1983, Currie 2006).<sup>1</sup> This incomplete take-up varies across racial and ethnic groups. In general, Hispanic citizens have lower participation than African-Americans and non-Hispanic whites across a range of public welfare programs (Morin, Taylor, and Patten 2012).<sup>2</sup> Moreover, the gap between take-up by eligible Hispanics versus other groups has widened in recent years. Although food insecurity increased for Hispanic households from 2005 to 2013, the share of Hispanics taking up the Supplemental Nutrition Assistance Program (SNAP) slowed relative to other groups over the same time period (Nord, Andrews, and Carlson 2006; Coleman-Jensen, Gregory, and Singh 2014).<sup>3</sup> And while the uninsured rate has fallen with the implementation of the Affordable Care Act (ACA), Hispanics have experienced the slowest decline, thus comprising an ever growing share of the remaining uninsured (Collins et al. 2016; Garrett and Gangopadhyaya 2016).

Many scholars have studied the factors that influence program participation, including transaction costs, information, and stigma (e.g. Aizer 2007; Besley and Coate 1992), in addition to behavioral biases such as inattention and time-inconsistency (Bhargava and Manoli 2015; Madrian and Shea 2001; Karlan et al. 2016). Widening the lens beyond individual psychology and constraints, studies also suggest that social networks influence the take-up of programs 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 via the spread of fear.

In this paper, we explore whether deportation fear explains some of the puzzle of incomplete take-up, specifically for Hispanic Americans. Recent survey evidence suggests that deportation fear is widespread. In a 2017 survey of residents in Los Angeles County, 37 percent reported being concerned that they, a friend, or a family member could be deported. Among those who endorsed such a concern, 80 percent said that they, a friend, or family member would be at greater risk of being deported by enrolling in a government health, education or housing program.<sup>4</sup> This finding echoes other qualitative evidence suggesting Hispanic citizens, themselves immune to deportation,

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<sup>1</sup>For evidence on the health and human capital returns, 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 (2018).

<sup>2</sup>According to Morin et al. (2012), across the six best-known federal entitlement programs, 64 percent of blacks reported taking up any of these programs compared to 56 percent of whites and 50 percent of Hispanics. Blacks are also more likely than whites or Hispanics to have received three or more benefits (27 percent for blacks vs. 14 percent for whites and 11 percent for Hispanics).

<sup>3</sup>Authors' own calculations on food stamp use from the American Community Survey. In addition, among households participating in SNAP, the Hispanic share fell from 13.3 percent in 2006 to 11.8 percent in 2016 (Wolkwitz 2007; Lauffer 2017).

<sup>4</sup>Data from 2017 UCLA Luskin Los Angeles Quality of Life Index Survey.

nevertheless fear that enrollment may reveal personal information on non-citizens in their networks to immigration authorities. 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.”<sup>5</sup> Yet causal evidence on whether immigration enforcement activities induce a spillover effect on the public program participation of Hispanic citizens remains thin.

To explore the impact of deportation fear on the safety net participation of Hispanic citizens, we study 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 non-citizen immigrant will be deported conditional on being arrested. From its activation to its 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 removals during the time period, the highest number in recent U.S. history.<sup>6</sup>

As we are focused on the spillover effects of immigration enforcement on Hispanic citizens, we distinguish between direct and indirect treatment effects, with a focus on the latter. In the potential outcomes framework, the direct treatment effect is the difference in potential outcomes for treatment and control groups among individuals who are *eligible* for treatment (Rubin 1974). Treatment in our context is defined as immigration enforcement under SC and those eligible for deportation are non-citizen immigrants.<sup>7</sup> Direct treatment effects stem mainly from principal-agent problems, whereby non-citizen parents forgo signing up their citizen children for benefits out of fear of revealing themselves. As we review in detail below, estimating direct effects has been the subject of several studies in public health (Vargas and Pirog 2016; Hacker et al. 2011; Vargas and Ybarra 2017) as well as important work in economics by Watson (2014) and Amuedo-Dorantes, Arenas-Arroyo, and Sevilla (2018). In sharp contrast, indirect treatment effects stem from externalities, whereby citizen decision-makers forgo private benefits out of concern for their non-citizen contacts. In our context, indirect treatment effects measure the difference in potential outcomes for treatment and control groups among individuals who are *not eligible* for deportation (i.e. authorized U.S. citizens), who may nevertheless be fearful of revealing non-citizen family members, such as spouses or parents, or other members of the community. A simple extension to Moffitt’s canonical model of welfare participation (1983) nests both the direct and indirect treatment effects and formalizes how social connections can lead to disutility from take-up in the presence of immigration enforcement.

To estimate spillover effects, we use detailed micro-data on the universe of over two million detainees (“immigration holds”) issued under SC between 2008 and 2013. These data contain infor-

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<sup>5</sup>See <https://www.pbs.org/newshour/health/hispanic-americans-still-arent-signing-obamacare>.

<sup>6</sup>See 2016 Yearbook of Immigration Statistics at <https://www.dhs.gov/immigration-statistics/yearbook/2016/table39>.

<sup>7</sup>By federal law, any non-citizen can be deported, including unauthorized individuals and green card holders.

mation 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 SNAP, otherwise known as food stamps, and health insurance on federal exchanges initiated under the ACA. Information on take-up comes from the restricted version of the Panel Study of Income Dynamics (PSID), public-use administrative data from the Centers for Medicare and Medicaid Services (CMS), and the American Community Survey (ACS). We focus on these federal programs as they have fairly uniform eligibility requirements across locations that exclude unauthorized individuals, allowing us to estimate indirect treatment effects. SNAP and health insurance subsidies under the ACA also represent two of the largest means-tested programs in the United States and thus are of special interest to economists and policymakers alike. Because our focus is on indirect effects, we examine program participation among citizen heads of households. 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).

We employ two different identification strategies to estimate the impact of SC on program take-up. In our first approach, we explore the extensive margin of deportation activity, leveraging the staggered roll-out of SC across counties. We use a triple-differences framework, interacting race and ethnicity indicators with timing of SC activation. In doing so, we compare food stamp take-up for Hispanic households within a given location to take-up for non-Hispanic whites and blacks, net of counties that had not yet activated, before versus after SC activation. The triple-differences identification assumption is plausible, requiring that there be no location-specific shocks timed with the staggered SC roll-out *and* influencing the dynamic path of safety net outcomes exclusively for Hispanics while sparing other minority groups.

In our second approach, we exploit substantial cross-sectional variation in the intensity of SC enforcement to assess sign-up for the ACA, which was enacted after SC was fully implemented across the country. Intensity of SC enforcement is measured by the prevalence of detainers issued in a location relative to the estimated number of non-citizen 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 30 years prior to SC (the share) with the leave-one-county-out cumulative number of detainers issued during SC (the shift).

We find that SC activation is associated with substantial declines in safety net participation among Hispanic citizen households. In the ACS, Hispanic-headed families are 2.3 percentage points less likely to take up food stamps after activation of SC. The take-up rate of food stamps among high-participation Hispanic-headed households in the ACS before activation was 22 percentage points, implying a ten percent decline in take-up due to SC activation. We obtain qualitatively similar



results when using the PSID, finding a decline in Hispanic food stamp take-up of 14.5 percentage points following SC activation, a 34 percent decrease from the mean. Turning to health insurance, we find that a ten percent increase in detainers is associated with a 2.0 percentage point reduction in Hispanic ACA sign-up. These estimates imply that, in the absence of SC, ACA sign-ups among eligible Hispanics would have been 22 percent higher.

A number of findings suggest these results are indeed causal. First, we probe the identifying assumptions for both our empirical approaches and find evidence supporting their validity. Consistent with the parallel trends assumption under our triple-differences approach, balance tables demonstrate no sharp changes in the evolution of our outcome variables prior to SC activation. For the shift-share instrument, a key identifying assumption is that the historical shares of Hispanic foreign born in a county only affect take-up through the mechanism of immigration enforcement, i.e. the exclusion restriction. We test the plausibility of this identifying assumption by exploring the relationship between historical composition and local characteristics that influence program take-up. We fail to find a correlation between our shift-share instrument and potential confounding 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). For the longitudinal analysis, we include a full set of race-by-state fixed effects to address the potential concern that states may vary in policies towards minority groups, and a full set of state-by-year fixed effects to account for changes in state-level immigration enforcement such as the enactment of omnibus enforcement bills (Amuedo-Dorantes and Arenas-Arroyo 2017). We also allow for flexible impacts of the Great Recession across demographic groups, interacting race and ethnicity-specific employment changes with the timing and intensity of the recession (Kochhar, Fry, and Taylor 2011; McKernan et al. 2014). Third, for both SNAP and ACA, we show SC only affected Hispanic Americans – results on program take-up for non-Hispanic blacks or whites are small and not statistically significant. We also find null effects of SC on Puerto Ricans and Cubans, two groups that face zero to minimal deportation risk because of citizenship or political refugee status. These findings accord with the fact that well over 90 percent of detainers issued under the SC program were for Hispanics and suggests that the SC program did not affect the behavior of those less likely to be affected by enhanced immigration enforcement.

In the penultimate section of the paper, we explore potential explanations for our main results. We report five findings that, taken together, are difficult to reconcile without invoking fear as an explanatory mechanism. Fear is defined as the subjective likelihood of an event that brings disutility. Whether detention or deportation of a non-citizen elicits such a response depends on whether the citizen decision-maker is connected to someone who is deportable. We therefore assess changes in program participation among mixed-status Hispanic-headed households,<sup>8</sup> and find that reductions in safety net take-up are largest among areas with a higher share of mixed-status families. Second, locations where more detainers are issued against Hispanics for non-violent (e.g. often misdemeanor)

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<sup>8</sup>Mixed-status households include members that have different citizenship or immigration statuses.

than violent crimes exhibit a larger response to SC, suggesting that the failure to target serious non-citizen offenders generates a stronger behavioral response. Third, locations where deportation fear rises over the activation period, as measured in Pew survey data, exhibit a heightened response to the program’s introduction. Fourth, in locations where federal detainers are not uniformly enforced (i.e. “sanctuary cities”), SC activation has almost no detectable effect. Finally, we show that, following SC activation, Google searches for deportation-related terms across media markets increased sharply, consistent with at least an awareness of the program if not fear of its potential consequences.

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. 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 individuals in households who previously used food stamps also substantially reduced their use following SC activation. We also explore but reject the possibility that compositional changes in the types of Hispanic individuals responding to survey questions in a given locale due, for example, to migration shifts, are driving the results. Finally, we fail to find significant effects of SC on employment, suggesting that our findings are unlikely driven by changes in labor force attachment among Hispanics.

In sum, our findings suggest that Hispanic citizens respond to recent immigration enforcement by reducing their safety net participation, likely due to fear of revealing non-citizens in their networks. Our results imply that deportation fear may play an important role in explaining some of the uptake gap for Hispanic Americans, with potentially adverse long-term consequences for the health and well-being of Hispanic families.

This paper relates to several literatures. First, as mentioned above, we build upon prior research in the fields of economics, law, political science, and public health examining how immigration enforcement affects safety net take-up by non-citizen immigrants.<sup>9</sup> These analyses generally focus on take-up by non-citizen parents on behalf of their children and/or programs whereby undocumented individuals are eligible to sign up. For instance, Watson (2014) examines the effect of increased immigration enforcement following the passage of the 1996 Illegal Immigration Reform and Immigrant Responsibility Act, finding that non-citizen parents reduce Medicaid enrollment of

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<sup>9</sup>Our paper is also related to a literature that examines the effects of the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), which denied federal welfare benefits to most post-enactment legal immigrants during their first five years of U.S. residence, on immigrant take-up. Despite the fact that PRWORA did not affect eligibility for pre-enactment legal immigrants for Temporary Assistance for Needy Families (TANF) and Medicaid, several studies find reductions in immigrant take-up for these programs (see Fix and Passel 1999; Kandula et al. 2004). Thomas and Collette (2017) argue that immigrants reduced their take-up because they were confused regarding eligibility and immigrants may have been concerned about being labeled a “public charge,” which can reduce the likelihood of citizenship (see Online Appendix for details). In contrast, Lofstrom and Bean (2002) and Haider et. al (2004) suggest that economic and labor market conditions were at least partly responsible for reductions in welfare use among immigrants following the passage of PRWORA (see also Kaestner and Kaushal 2005; Bitler and Hoynes 2011).

their citizen children in response to enforcement. Pedraza and Zhu (2014) examine the effect of Secure Communities and find similar reductions in non-citizen mother’s enrollment of their children in Medicaid. Related work finds that immigration enforcement affects unauthorized parents’ participation in programs like Women, Infants, and Children (WIC), a program legally available to unauthorized immigrants (Vargas and Pirog 2016), and the Earned Income Tax Credit (Cascio and Lewis 2017). Most recently, Amuedo-Dorantes et al. (2018) find that unauthorized parents are more likely to be in poverty and *increase* take-up for food stamps for their American children in response to greater immigration enforcement, potentially due to households becoming more impoverished.<sup>10</sup> We build off the above literature by providing the first causal estimates of the effect of immigration enforcement on the choice behavior of Hispanic citizens, rather than focusing on the decisions of unauthorized individuals – thus extending the prior work on enforcement to include indirect treatment effects. We also provide evidence that the results herein are consistent with a spillover effect of deportation fear on program-eligible individuals.

Second, we add to the literature seeking to understand why families sometimes forgo participation in safety net programs despite high returns (see review by Currie 2006), highlighting that kinship networks can yield not only benefits, but also impose costs (see review by Cox and Fafchamps 2008; di Falco and Bulte 2011). Third, we contribute to scholarship that aims to causally identify and quantify the effect of fear on consumer behavior (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 discusses eligibility rules for public programs in the 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 ICE from 2008 to 2014 and reactivated in 2017.<sup>11</sup> 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 in connection with government matters. SC had three main objectives: (1) to identify non-citizens at large and in federal, state, and local custody charged with or convicted of serious criminal offenses who were subject to removal; (2) to prioritize enforcement actions to ensure apprehension and removal of non-citizens convicted of serious criminal offenses; and (3) to transform enforcement processes and systems to achieve lasting results. SC accomplished these goals through an extensive collaboration

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<sup>10</sup>Qualitative work also shows that fears about the personal risk of detention or deportation can lead undocumented immigrants and their U.S. citizen children to avoid health programs (Yoshikawa 2011; Bean, Brown, and Bachmeier 2015), with the consequence of expanding ethnic and racial health disparities (Asad and Clair 2018).

<sup>11</sup>Additional institutional details on the program and its implementation can be found in the Online Appendix.

between state and local law enforcement agencies, 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, non-citizens in violation of immigration laws were identified by inmate interviews in local jails or prisons, performed by either federal officers under a policy known as the Criminal Alien Program (CAP) or local officers under formal written agreements with DHS, known as 287(g) agreements. 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 individual (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) non-citizens in the U.S. who have violated immigration law, such as persons who were previously deported and/or overstayed their visas; (2) non-citizens 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, they then issued what is called a "detainer" (sometimes called an "immigration hold") on the person. This detainer requested that the state or local law enforcement agency hold the individual for up to 48 hours to allow ICE to assume custody for the initiation of removal proceedings. As a result of this detainer protocol, 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 non-citizen would be apprehended by ICE and deported from the country, conditional on being arrested. According to an official review of SC in 2011, in most cases, people detained by ICE were subject to immigration enforcement action for reasons independent of the triggering arrest or conviction, i.e., a fingerprint match may indicate that the person was removable because he or she

entered the country without inspection or overstayed a visa.

Notably, state and local jurisdictions could not easily opt out of SC. All fingerprints submitted to the FBI were automatically sent to DHS as a result of the information-sharing partnership, such that a local jurisdiction could not choose to only submit its fingerprints to the FBI.<sup>12</sup>

Due to various technological constraints, SC was not implemented at once across the entire country. As noted by Cox and Miles (2013), one of the main technological hurdles was that many jurisdictions did not have live scan fingerprint devices. We discuss the roll-out of SC and the non-technological factors that influenced it further below. 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 population, distance from the Mexican border, and whether a county had a 287(g) agreement with ICE, findings we return to when discussing our identification strategy below.

In response to SC, some jurisdictions began to disobey detainer requests from ICE, arguing such detentions were unconstitutional under the Fourth Amendment, as well as noting concerns that such practices would discourage immigrant cooperation with local law enforcement. These jurisdictions became known as “sanctuary cities.”<sup>13</sup>

On November 20, 2014, SC was temporarily suspended across the entire country by DHS policy, in part due to the resistance from sanctuary cities. After SC was suspended, DHS implemented a new program called the “Priority Enforcement Program” (PEP). Under PEP, ICE continued to rely on fingerprint-based biometric data submitted during bookings by state and local law enforcement agencies. However, ICE was instructed to only transfer individuals who were convicted of specifically enumerated high priority offenses, individuals who intentionally participated in an organized criminal gang to further the illegal activity of the gang, or individuals deemed to pose a danger to national security. In addition, ICE was instructed to only request a detainer if the person in custody was subject to a final order of removal or if there was other sufficient probable cause to find that the person was removable. 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 immigrants and continues to increase.

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<sup>12</sup>See <http://www.washingtonpost.com/wp-dyn/content/article/2010/09/30/AR2010093007268.html>

<sup>13</sup>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>. Importantly, the number of detainers could not be meaningfully influenced by sanctuary cities unless they chose to not arrest Hispanic individuals. There is no evidence this was a strategy pursued by these jurisdictions. Rather they ignored detainer requests. See [https://www.dhs.gov/sites/default/files/publications/14\\_1120\\_memo\\_secure\\_communities.pdf](https://www.dhs.gov/sites/default/files/publications/14_1120_memo_secure_communities.pdf).

### III. Safety Net Programs

In this study, we focus on participation in SNAP, also known as food stamps, and the ACA, two of the largest 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:* The Supplemental Nutrition Assistance Program (SNAP), previously known as the Food Stamp Program, is the largest cash or near cash means-tested transfer program in the U.S. In 2012, SNAP spending reached \$74 billion, exceeding spending on both the Earned Income Tax Credit (\$64 billion) and Temporary Assistance for Needy Families (\$29 billion) (Hoynes et al. 2016). SNAP is also the only U.S. public safety net program that is universally available to low-income people without many other restrictions such as being disabled, elderly or having children. The program has been credited with helping lift families out of poverty every year and for acting as a stabilizer during the Great Recession (Tiehen et al. 2012; Ganong and Liebman 2013; Short 2014; Bitler and Hoynes 2015).

In order to receive benefits under SNAP, individuals need to meet various federal guidelines.<sup>14</sup> 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 resources (\$3,500 if someone is older than 60 or disabled).<sup>15</sup> Immigrants residing in the country illegally are ineligible to receive benefits. In contrast, legal immigrants are eligible for SNAP if they have lived in the U.S. for five years, if they currently receive disability-related assistance, or if they are children under 18, in addition to the income and resource limits. 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 applying for benefits. Some states also ask for country of origin, date of entry, alien registration number, and citizenship status of each person in the household. Using this information, states verify the immigration status of each household member through DHS via the Systematic Alien Verification for Entitlements (SAVE) program, designed to reduce benefit fraud. An example of a state SNAP form is provided in Appendix Figure A1.

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<sup>14</sup>See <https://www.fns.usda.gov/snap/eligibility#Resources>.

<sup>15</sup>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. 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. See detailed reviews on safety net requirements for further information. See <https://www.fns.usda.gov/snap/eligibility>.

Almost all states assure applicants their information will only be used to determine eligibility and will not be shared with ICE for immigration enforcement.<sup>16</sup> The Department of Agriculture has issued guidance stating that “[i]t is important for non-citizens to know they will not be deported, denied entry to the country, or denied permanent status because they apply for or receive SNAP benefits.”<sup>17</sup> Nevertheless, advocacy groups claim that SNAP applications have declined recently and that this decline has coincided with increased anti-immigration rhetoric. As a SNAP outreach coordinator for the Latino community noted to the *Washington Post*, “They’re staying away from me...I say hi to them, and they avoid me completely. I don’t know what they’ve been saying amongst themselves. But no one is signing up anymore, and the people who need to renew are not renewing.”<sup>18</sup>

*ACA*: The Affordable Care Act (ACA), enacted in 2010, allowed citizens and lawfully present immigrants 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 17 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.<sup>19</sup>

According to the Commonwealth Fund, all demographic groups have experienced reductions in their uninsured rate under the ACA, but the decline has been slowest for Hispanics. From 2013 to 2016, the uninsured rate for non-Hispanic whites fell by 44 percent, 38 percent for blacks, and 19 percent for Hispanics (Collins et al. 2016). Moreover, as the number of uninsured has fallen, Latinos comprise an ever larger share of the remaining uninsured. Several reasons have been advanced 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.”<sup>20</sup> See an example of the ACA application form in Appendix Figure A2, which asks questions about citizenship and immigration status for each member of the household. Despite public assurance by the federal government that this information will not be used for immigration enforcement, as with SNAP,

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<sup>16</sup>For example, see <http://www.cdss.ca.gov/cdssweb/entres/forms/English/SAWS2ASAR.pdf> and [http://www1.nyc.gov/assets/hra/ACCESSNYC/pdf/SNAPKit/english/LDSS\\_4826A.pdf](http://www1.nyc.gov/assets/hra/ACCESSNYC/pdf/SNAPKit/english/LDSS_4826A.pdf).

<sup>17</sup>See [https://fns-prod.azureedge.net/sites/default/files/snap/Non-Citizen\\_Guidance\\_063011.pdf](https://fns-prod.azureedge.net/sites/default/files/snap/Non-Citizen_Guidance_063011.pdf).

<sup>18</sup>See [https://www.washingtonpost.com/news/wonk/wp/2017/03/16/immigrants-are-now-canceling-their-food-stamps-for-fear-that-trump-will-deport-them/?utm\\_term=.be2800772b3a](https://www.washingtonpost.com/news/wonk/wp/2017/03/16/immigrants-are-now-canceling-their-food-stamps-for-fear-that-trump-will-deport-them/?utm_term=.be2800772b3a).

<sup>19</sup>See <https://www.youtube.com/watch?v=qgce06Yw2ro>.

<sup>20</sup>See <http://thehill.com/blogs/pundits-blog/healthcare/323380-why-so-many-hispanics-are-still-uninsured>.

descriptive evidence suggests that Hispanics are still afraid.<sup>21</sup> This fear is perhaps unsurprising given that CMS, which is in charge of maintaining the federal marketplace, enlists the help of DHS to verify the immigration status of any non-citizen applicant seeking to enroll in a qualified health plan (See U.S. GAO Report 2015; Appendix Figure A3). 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.”<sup>22</sup>

#### IV. Theoretical Framework

Secure Communities represented a major shift in immigration enforcement policy. In this simple model, we formalize how SC may have influenced the choice behavior of Hispanic citizens. Our starting point is Moffitt’s (1983) seminal model of non-participation in social programs. We adopt his cost-benefit approach to participation, and incorporate indirect treatment effects by allowing the utility of the household head to depend on the well-being of others in his family.

Specifically, let household  $j$  with head of household  $i$  be comprised of a set of citizen members  $C$  and non-citizen members  $N$  where  $C + N = T$ . Let the expected utility of head  $i$  in household  $j$  in location  $l$  be given by:

$$EU_{ijl} = \lambda_i \cdot \left( \frac{Y_j}{T} + p_{ij} \mathbb{1}_{i \in C} \cdot (B_i) \right) + \lambda_c \cdot \left( \frac{Y_j}{T} + \frac{p_{ij} B_{j,-i}}{C - \mathbb{1}_{i \in C} \cdot 1} \right) + \lambda_n \cdot \left( \frac{Y_j}{T} - \pi_{jl}(p_{ij}) \right) \quad (1)$$

where  $Y_j$  is household income (split among all  $T$  members, citizen or non-citizen),  $p_{ij}$  is the decision to participate (made by the head of household  $i$ ),<sup>23</sup>  $B_i$  is the per capita benefit to  $i$  from participation if  $i$  is a citizen, and  $B_{j,-i}$  is the total benefit to other citizen members of the household. For simplicity, we only allow citizen members of the household to receive the benefit as it is unlawful for unauthorized individuals to utilize the safety net programs in our study.<sup>24</sup>  $\pi_{jl}$  is the subjective probability of deportation (i.e. fear) and is an increasing function of program participation,  $p_{ij}$ .

In this utility function,  $\lambda_i$ ,  $\lambda_c$ , and  $\lambda_n$  represent welfare weights that head  $i$  gives to his own utility, the utility of other citizen members, and the utility of non-citizen members of the household, where  $\lambda_i + \lambda_c + \lambda_n = 1$ .

If head of household  $i$  is a citizen ( $i \in C$ ), the above expected utility function can be re-expressed as:

$$EU_{ijl} = \frac{Y_j}{T} + (\lambda_i + \lambda_c) \cdot \left( \frac{p_{ij} B_j}{C} \right) - \lambda_n \cdot \pi_{jl}(p_{ij}) = \frac{Y_j}{T} + \lambda_c \cdot \left( \frac{p_{ij} B_j}{C} \right) - \lambda_n \cdot \pi_{jl}(p_{ij}) \quad (2)$$

The model captures the spillover effect of deportation fear because the probability of deportation

<sup>21</sup>See ICE Memo, <https://www.ice.gov/doclib/ero-outreach/pdf/ice-aca-memo.pdf>; [www.ice.gov/espanol/factsheets/aca-memoSP](http://www.ice.gov/espanol/factsheets/aca-memoSP).

<sup>22</sup>See [https://www.washingtonpost.com/politics/hispanics-forgo-health-services-to-avoid-officials-attention-advocates-say/2018/01/21/3555412e-ff1d-11e7-9d31-d72cf78dbee\\_story.html](https://www.washingtonpost.com/politics/hispanics-forgo-health-services-to-avoid-officials-attention-advocates-say/2018/01/21/3555412e-ff1d-11e7-9d31-d72cf78dbee_story.html).

<sup>23</sup>If the household is mixed-status, then whomever has citizenship likely has the higher threat point and will be the decision-maker.

<sup>24</sup>We abstract away from the fact that some legal permanent residents are eligible for safety net programs. An alternative model that allows all members of the household to share in  $B$  generates similar predictions.



for an authorized head of household  $i$  is equal to zero. Deportation fear affects the participation decision of head  $i$  if  $\lambda_n > 0$ . Note that, by choosing not to participate, head  $i$  forgoes a private benefit  $\frac{B_j}{C}$ . Equation 2 nests both direct and indirect treatment effects. The Online Appendix models the case where the head of household  $i$  is a non-citizen, capturing the direct treatment effect of deportation fear.

At the optimal choice of participation, and assuming participation is continuous, the per beneficiary benefit weighted by the welfare importance of citizen household members (marginal benefit) must equal the deportation cost induced by participation weighted by the welfare importance of non-citizen members (marginal cost):

$$\frac{\partial \pi_{jl}}{\partial p_{ij}} \cdot \lambda_n = \frac{B_j}{C} \cdot \lambda_C$$

If  $\pi''_{jl}(p_{ij}) > 0$ , it is straightforward to show that  $\frac{\partial p}{\partial \lambda_n} < 0$  and  $\frac{\partial p}{\partial \lambda_C} > 0$ .<sup>25</sup> Intuitively, participation increases with the welfare importance of citizen household members, but decreases with the welfare importance of non-citizen members.

In reality, participation is a binary choice. To incorporate deportation fear, we let the change in the subjective probability that a non-citizen will be deported if the household participates in a program relative to no participation be:

$$\Delta \pi_{jl} = \beta \cdot D_l + \epsilon_{jl}$$

where  $D_l$  is the intensity of location-specific immigration enforcement and  $\epsilon_{jl}$  is an error term that is distributed  $\epsilon \sim F(\cdot)$ . Thus, household  $j$  will participate in the federal safety net program if and only if:

$$\frac{Y_j}{T} + (\lambda_i + \lambda_c) \cdot \left(\frac{B_j}{C}\right) - \lambda_n \cdot \pi_{jl}(1) > \frac{Y_j}{T} - \lambda_n \cdot \pi_{jl}(0)$$

Let  $\frac{(\lambda_i + \lambda_c) \cdot (\frac{B_j}{C})}{\lambda_n} = \gamma_j$ , where  $\gamma \sim G(\cdot)$ . Within each location  $l$ , let the average  $\gamma_j$  be equal to  $\bar{\gamma}_l$ . Then, aggregating over households  $j$  in a given location  $l$ , the share not participating is given by:

$$s_l = 1 - F(\bar{\gamma}_l - \beta \cdot D_l)$$

The non-participation share,  $s_l$ , is decreasing in the size of the program benefit ( $B_j$ ) and in the weights ascribed to citizen members including the head himself ( $\lambda_c$  and  $\lambda_i$ ). In contrast, the non-participation share is increasing in the weight assigned to non-citizens ( $\lambda_n$ ), and increasing in the intensity of local immigration enforcement ( $D_l$ ). Our model predicts that, holding all else constant, as immigration enforcement intensifies in an area, citizen heads of households may reduce their take-up of public programs, particularly those with close connections to non-citizens in their

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<sup>25</sup>Specifically,  $\frac{\partial p}{\partial \lambda_n} = -\frac{\pi'_{jl}(p_{ij})}{\lambda_n \cdot \pi''_{jl}(p_{ij})} < 0$  and  $\frac{\partial p}{\partial \lambda_C} = \frac{\frac{B_j}{C}}{\lambda_n \cdot \pi''_{jl}(p_{ij})} > 0$ .

networks. Appendix Figure A4 graphically illustrates how the non-participation share is affected by immigration enforcement and connections to non-citizens.

## 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 by citizen Hispanic Americans. In this section, we describe our identification strategies to draw causal inference and provide an overview of the data sources.

### A. Empirical Framework

#### A.1 Triple-Differences Specification

Our first approach exploits the staggered rollout of SC activation across counties as well as the disproportionate impact of SC on Hispanics within counties. Specifically, we estimate the change in pre- versus post-SC activation differences in safety net take-up by race/ethnicity in counties that have activated compared to counties that have not yet activated.

Using repeated county-level cross-sectional data in the ACS, as well as household-level panel data from the PSID, we estimate the following specification:<sup>26</sup>

$$\begin{aligned}
 Y_{rcst} = & \alpha + \beta_1 I_{ct}^{post} + \beta_2 (I_r^H \cdot I_{ct}^{post}) + \beta_3 (I_r^B \cdot I_{ct}^{post}) + \Omega' X_{rcst} + \mu_c + \delta_{st} + \theta_{rs} \\
 & + \Gamma'_1 X_{cst} + \Gamma'_2 (X_{cst} \cdot I_r^B) + \Gamma'_3 (X_{cst} \cdot I_r^H) + \epsilon_{rcst}
 \end{aligned} \tag{3}$$

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 data,  $Y_{rcst}$  is the share food stamp take-up among a high participation sample. 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,  $I_r^H$  and  $I_r^B$  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 of SC. Almost all counties activated between 2008 to 2013, with the majority of counties activating between 2010 to 2012.  $X_{rcst}$  includes average log poverty rate, family size, and employment that vary across race, county, and time. We control for these characteristics as they are direct determinants of food stamp eligibility. There is also evidence that the Great Recession had differential effects by race and ethnicity. For instance, white families' wealth fell 26 percent during the Great Recession, while the wealth of black families and Hispanic families fell by 48 and 44 percent, respectively (McKernan et al. 2014), which led to differential

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<sup>26</sup>We write the equation at the county-level using the ACS data but note the differences for the PSID. 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.

effects on food stamp take-up by race and ethnicity (Flores-Lagunes et al. 2018). We account for these differential effects by explicitly including race/ethnicity-specific state-level employment changes during the Great Recession as well as interacting the timing of the Great Recession with race indicators. Robustness to alternative non-parametric specifications is explored in Section VI.

Our specification includes county fixed effects ( $\mu_c$ ) to account for any unobserved time-invariant county-level factors that affect take-up, such as political leaning, and state-by-year fixed effects ( $\delta_{st}$ ) to account for any state-specific policies or economic shocks that might influence take-up, such as the enactment of state omnibus immigration bills or mandated use of E-Verify to check the work authorization of new hires (Amuedo-Dorantes and Arenas-Arroyo 2017). Such fixed effects also capture differential state-level effects of federal immigration reforms. We also include state-by-race/ethnicity fixed effects ( $\theta_{rs}$ ) to control for attitudes and policies in each state that differentially affect minority groups.

Finally, we account for other county-level controls,  $X_{cst}$ , that are not publicly available disaggregated by race at the county-level, but which have been shown to have differential effects on minority populations, such as crime. Arrest 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/ethnicity indicators with the FBI index crime rate (Kochhar, Fry, and Taylor 2011; McKernan et al. 2014).

Our specification for the PSID is similar to Equation 3. The outcome is an indicator for take-up of food stamps by individuals in a high participation household  $i$ . In the PSID data, household-level controls,  $X_{ircst}$ , include demographic characteristics on the head of household, including family size, number of children, and poverty level in the past year.

For the ACS data, we weight all regressions by the total population in the relevant race-county cell in order to more nearly identify a population average treatment effect – only exactly so when the model is fully saturated (Solon, Haider, and Wooldridge 2015). To obtain a similar population average treatment effect in the PSID, we include all individuals from each household and use PSID provided sample weights. We explore the robustness of our results to alternative weighting schemes in Section VI. Standard errors are clustered at the county level.

In our analysis on food stamp take-up using both 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, individuals who are not eligible for deportation.<sup>27</sup> The coefficient of interest in Equation 3 is  $\beta_2$ , which estimates the impact of SC activation on outcomes of Hispanic households relative to non-Hispanic white households, compared to counties that have not yet activated.  $\beta_3$  serves as a placebo test, capturing the effect of SC on black households relative to non-Hispanic white households in

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<sup>27</sup>Another benefit of focusing on the choice behavior of citizens rather than unauthorized immigrants is that unauthorized individuals may have experienced changes in family structure as a result of immigration enforcement (Amuedo-Dorantes and Arenas-Arroyo 2017), which may affect eligibility and take-up for safety net programs.

counties that have activated versus those that have not yet activated.

In addition to our baseline specification in Equation 3, we estimate an event study where we interact  $I_r^H$  and  $I_r^B$  with a series of time dummies for each period, relative to the year prior to 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:

$$Y_{rcst} = \alpha + \sum_{n \neq -1} \beta_1^n (I_{c,t=n}) + \sum_{n \neq -1} \beta_2^n (I_r^H \cdot I_{c,t=n}) + \sum_{n \neq -1} \beta_3^n (I_r^B \cdot I_{c,t=n}) + \Omega' X_{rcst} + \mu_c + \delta_{st} + \theta_{rs} \\ \Gamma_1' X_{cst} + \Gamma_2' (X_{cst} \cdot I_r^B) + \Gamma_3' (X_{cst} \cdot I_r^H) + \epsilon_{rcst} \quad (4)$$

In this specification,  $I_{c,t=n}$  is an indicator for each period (other than the year prior to activation  $t = -1$ ), such that the  $\beta_2^n$  coefficients trace the take-up of food stamps for Hispanics in the years before and after SC activation relative to non-Hispanic whites.<sup>28</sup> Similarly, each  $\beta_3^n$  coefficient traces the take-up of food stamps for blacks relative to non-Hispanic whites before and after activation. With this specification, one would expect to see a shift post-activation specifically for Hispanic households, and not black or white households, if we are measuring the causal effect of SC.

*Identification:* The assumption underlying our triple-differences identification is that there are no contemporaneous shocks associated with the activation of SC within a county that only affect Hispanic households relative to white and black households. In other words, we assume that any differences in our outcome variables of interest for Hispanic versus white or black households would have evolved smoothly absent SC activation, conditional on our set of fixed effects and controls.

To assess this assumption, we begin by exploring 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). We test 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 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 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 no significant differences in changes in Hispanic-white or Hispanic-black food stamp take-up in the ACS across early versus late activation counties (row 6), suggesting that the timing of SC activation was not

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<sup>28</sup>Leads before six years and lags after four years are coded with the first and last groups, respectively.

correlated with trending differences in outcomes by racial/ethnic group. These results lend support to the parallel trends assumption underlying our approach.

In addition, we implement a permutation test where we limit our data to pre-activation years and randomly permute a “pseudo” SC activation year for each county, ensuring that there is at least one year of data pre- and post-“pseudo” activation year. Using these randomly permuted activation years, we then estimate our baseline specification, Equation 3, repeating this procedure 500 times. In Appendix Figure A5, we present the empirical distribution of these placebo effects for  $\beta_2$ , finding that our actual treatment effects are larger (in absolute value) than 100 percent of our placebo estimates. These results suggest that SC activation had a very large and atypical effect on outcomes for Hispanic households.

*Predicted SC Activation:* Although our triple-differences identification does not exclusively rely on differences in program participation pre- versus post -SC activation, it is important to understand the factors that affected the timing of SC activation since non-random timing could still introduce bias. For instance, if SC preferentially activated in locations where criminal activity among the unauthorized was on the rise, and criminal activity decreases program participation, early activators could have seen a Hispanic-specific decline in safety net take-up regardless of SC, leading to overestimates of  $\beta_2$  in our main specification (Equation 3). On the other hand, if locations that activated early were routine targets of immigration enforcement (such as locations close to the Mexican border), Hispanics in these areas may be relatively insensitive to changes in enforcement and thus exhibit small decreases in safety net take-up, leading to underestimates of  $\beta_2$  in our main specification.

To further understand the timing of SC activation, Figure 1 presents maps that show the timing of SC activation across counties, revealing that border counties were the earliest places to activate. These findings are consistent with Cox and Miles (2014), who find that SC activation 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, the presence of a 287(g) agreement, and the percent Hispanic population.

We take several steps to reduce selection bias that might be generated by the non-random timing of SC activation. First, we exclude border areas from our analysis since they might be unique in several ways related to both immigration enforcement and program participation and include county fixed effects to account for demographic features of a county that may affect timing of activation. Second, in robustness checks described below, we explicitly control for the percent of households that are Hispanic at the county-year level using data from the ACS. Third, we review the related literature on SC and official ICE documentation to identify the criteria that affected roll-out timing. Based on our review of these documents, discussed in more detail in the Online Appendix, we identify four criteria that likely affected when a particular county would activate: (1) estimated number of non-citizens, (2) the distance from the Mexican border, (3) crime rates, and (4) prior county relationships with ICE as proxied by the presence of a 287(g) agreement. We use these criteria and their high-level interactions in a cross-section to predict activation year.

In robustness checks, we explore the reduced form relationship between predicted activation and safety net take-up, controlling for our preferred set of fixed effects and baseline controls. We note that variation in predicted activation year is driven by the interactions between the four criteria, generating plausibly exogenous timing of SC activation. We find nearly identical results when we use predicted activation compared to actual activation (see Section VI).<sup>29</sup>

## A.2 Shift-Share Instrument

Our second approach exploits the differential intensity of immigration enforcement under SC across geographies to assess sign-ups for the ACA, which opened for enrollment after SC was fully implemented across the country. To explore the impact of the intensive margin of SC on ACA enrollment rates, we estimate the following cross-sectional county-level specification:

$$ShrLatinoACA_{cs} = \alpha + \beta \cdot (ShrDetain_{cs}) + \Psi' X_{cs} + \delta_s + \epsilon_{cs} \quad (5)$$

where  $c$  represents county and  $s$  represents state. State fixed effects ( $\delta_s$ ) and share of males who are Hispanic (an element of  $X_{cs}$ ) are important controls for selection-on-observables. The former captures state-level programs and policies that affect population health and immigration, while the latter reflects the fact that Hispanic males comprised the overwhelming majority of those detained under SC.

In addition to these baseline controls, we control for a variety of other county-level controls ( $X_{cs}$ ) that are likely correlated with enforcement and program participation. For example, Lerman et al. (2017) document how partisanship can influence public policy behavior, specifically with respect to the ACA. Since immigration enforcement may also be more aggressive in counties that lean towards a particular ideology, we include the Obama-McCain county-level vote margin in the 2008 presidential election as a control. In addition, employment and income influence ACA eligibility and could be correlated with the treatment, motivating the addition of unemployment and Hispanic poverty rates as controls (Buettgens, Kenney, and Pan 2015). We also control for county-level FBI index crime rates in our preferred specification as the stated purpose of SC was to reduce crime and demand for health insurance could reasonably be lower in high-crime areas. In addition, we control for whether a county is a sanctuary city as these sanctuary jurisdictions often did not abide by detainer requests. Lastly, we proxy for the effectiveness of program outreach to minorities in a given locale using the share of eligible African-Americans who sign up for the ACA,  $ShrBlackACA_{cs}$ .

The dependent variable,  $ShrLatinoACA_{cs}$ , is the share of Latino individuals eligible for enrollment who signed up for the ACA in county  $c$  and state  $s$ . The treatment variable,  $ShrDetain_{cs}$ , is defined as the cumulative number of Hispanic detainees issued during SC (i.e. 2008 to 2013, see Figure 2) normalized by the estimated number of non-citizen Hispanics,  $\frac{D}{NH}$ . Anecdotally and as will be shown in our triple-differences results, sanctuary jurisdictions did not abide meaning-

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<sup>29</sup>In unreported results, we follow Burgess and Pande (2005) and instrument for actual activation using each of the four criteria listed above interacted with a linear time trend. While there is not a strong first-stage relationship (F-statistic = 3.7) under this approach, we find qualitatively similar estimates.

fully with SC and have oppositely-signed effects on safety net take-up post-SC. As a result, we set  $ShrDetain_{cs}$  equal to zero in sanctuary jurisdictions. In addition, we top code the dependent variable at the 99th percentile to minimize the influence of outliers.

The denominator,  $NH$ , is constructed following a method developed by the Pew Research Center and is generated using the ACS 2005–2009 county-level data (Pew Research Center 2013). Similar to Pew, we subtract the number of naturalized citizen Hispanics from the foreign-born Hispanic count in the ACS. We then supplement their approach by explicitly accounting for an undercount in non-citizen individuals. We construct a measure of the undercount using the universe of births in the United States from the National Center for Health Statistics (2005–2009). We calculate the ratio of foreign-born Hispanic mothers to all Hispanic births and multiply this ratio by the total number of Hispanic females in the ACS to generate predicted foreign-born Hispanic females. The ratio of predicted foreign-born Hispanic females to actual foreign born Hispanic females in the ACS is a measure of the undercount. Implicitly, we assume that undercounting of foreign-born Hispanic women is a good proxy for the undercount of foreign-born Hispanic men.<sup>30</sup>

Our coefficient of interest is  $\beta$ , which measures the effect of increased detainer intensity on Hispanic ACA sign-up. Although we carefully condition on potential confounders, counties that experienced greater intensity in the share of Hispanics detained may differ in unobservable ways from counties with less immigration enforcement. As noted above, SC intensity could have been stronger in places characterized by low Hispanic engagement with the welfare and health systems, biasing estimates of  $\beta$  towards the null.

To better understand whether the relationship we find is indeed causal, we employ a shift-share instrument to predict the number of Hispanic detainers issued. Following Card (2001), we weight the 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. Intuitively, variation in this shift-share instrument stems from the fact that national cumulative 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. In our context, variation in the national detainers (the shift) by country of origin is highly correlated with the size of the non-citizen, or at-risk, population (see Appendix Figure A6). For example, if SC primarily affected immigrants from Mexico because Mexican immigrants are most at-risk of deportation, the predicted increases in detainers should be larger in those counties that have more Mexican-born immigrants. Because this instrument is constructed using national counts 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. Moreover, because we use national counts excluding own county, our instrument is not affected by the fact that certain counties activated earlier than others. Figure 3 presents a county-level map of the intensity of SC using both our endogenous variable and shift-share instrument.

In our two-stage least squares specification, we instrument for  $ShrDetain_{cs}$  in Equation 5 with

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<sup>30</sup>This assumption probably leads to a lower bound on the count, since some men may cross the border alone and leave wives and families in the country of origin. Unfortunately, father country of origin is not available in the National Center for Health Statistics data.

the predicted share of Hispanic detainees issued,  $Z_c$ , constructed as:

$$Z_c = \frac{\sum_o \frac{L_{co}^{t=1990}}{L_o^{t=1990}} \cdot (D_{-co})}{N\hat{H}_c} \quad (6)$$

where  $c$  represents county and  $o$  represents Hispanic country of origin (e.g. Mexico).  $\frac{L_{co}^{t=1990}}{L_o^{t=1990}}$  represents the number of Hispanic immigrants in county  $c$  born from country of origin  $o$  relative to the total number of Hispanic immigrants born from country  $o$ . These shares are constructed using the 100 percent 1990 Census and sum to one across the United States. The baseline country-of-origin county shares are then multiplied by the cumulative leave-county-out number of national detainees issued from 2008 to 2013,  $D_{-co}$ , and normalized by the estimated number of non-citizen Hispanics,  $N\hat{H}_c$ . We explore alternative definitions of the instrument in robustness checks below. See the Online Appendix for details on variable definitions and construction.

*Identification:* There are two assumptions underlying our shift-share approach. The first assumption is that national cumulative detainees (leaving out own county) is uncorrelated with total local detainees. We view this assumption as plausible given that SC was a national program and that local detainees are unlikely to have a substantial effect on the national total, excluding the own county. Indeed, Goldsmith-Pinkham, Sorkin, and Swift (2018) clarify that the shift component of shift-share instruments only affects instrument relevance.

The second assumption is that the baseline historical shares of Hispanic foreign born in a county only affect ACA take-up through the mechanism of immigration enforcement, i.e. the exclusion restriction.<sup>31</sup> We check the plausibility of this identifying assumption in Figure 4 by exploring the relationship between baseline composition and local area characteristics that influence ACA take-up following Goldsmith-Pinkham et al. (2018). We test for significance of each characteristic and the joint significance of all county-level characteristics using seemingly unrelated regression (Autor and Houseman 2010). Our endogenous variable, the share Hispanic detainees, is highly correlated with baseline observables (joint p-value < 0.001). Importantly, however, we cannot reject the null hypothesis that our preferred Bartik instrument is uncorrelated with these county-level characteristics (joint p-value = 0.46).

## B. Data

*SC Data on Detainers and Removals:* Through records available to the public, FOIA requests to ICE, and restricted-use data agreements, we have obtained data on the roll-out of SC as well as

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<sup>31</sup>Jaeger, Ruist, and Stuhler (2018) note, in the context of studying labor demand shocks on wages, that the exclusion restriction is violated if local demand shocks are serially correlated (i.e. strong chain migration). If serial correlation exists and there are oppositely-signed short- and long-run responses to immigrant arrivals due to general equilibrium adjustments, conventional shift-share instruments may yield conflicting estimates. This issue is not a major concern in our setting for two reasons. First, serial correlation is not as important because SC was an unprecedented immigration program that began only in 2008. Second, it is difficult to rationalize SC eliciting oppositely-signed short- and long-run effects on ACA take-up. Nevertheless, we allow for this possibility and control for contemporaneous Hispanic shares by country of origin. The results are presented in robustness checks and are qualitatively similar.



micro-level data on the universe of detainees 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 the detainer was issued, the county the detainer was issued in, the individual’s country of origin, and other individual-level demographics (age and sex). We collapse these detainer data to the county level to ascertain the number of detainees issued for individuals from each foreign country over time. 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, in addition to county-level yearly data on the number of fingerprint submissions and matches under SC from 2008 to 2015.

Panel A of Figure 2 presents the total number of detainees issued per year and Panel B presents the cumulative number of detainees 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 demonstrate that the overwhelming majority (93 percent) of detainees are issued against Hispanic individuals. Panel C presents the ratio of detainees for low-level offenses (e.g. misdemeanor offenses) versus serious, violent offenses and shows that, over time, SC issued a growing share of detainees for low-level arrests. While not all detainees are honored by local law enforcement agencies or lead to removal from the country, there is a strong positive correlation between detainees and removals under SC. See Appendix Figure A7.

We normalize the number of detainees issued by the estimated number of non-citizen Hispanic immigrants in a county from the ACS 2005–2009, prior to SC activation. As described previously, we use a method similar to one developed by the Pew Research Center to estimate the number of non-citizen Hispanics, where we subtract the number of naturalized citizens of Hispanic origin from the total number of Hispanic foreign born (Pew Research Center 2013).<sup>32</sup> 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 in our main specifications to account for time-invariant differences in willingness to respond to surveys and as described previously, we explicitly correct for the undercount in federally collected data by using administrative birth data.

SC represented a massive increase in immigration enforcement. Appendix Table A2 presents difference-in-differences estimates of the impact of SC activation in a county on enforcement. Consistent with Cox and Miles (2014), we find that SC activation had no significant effect on offenses known to law enforcement or arrests. In contrast, we find significant increases in the number of fingerprint submissions received by ICE, fingerprint matches, and detainees issued post-SC activation. Event study estimates of the impact of SC on detainees issued are presented in Appendix Figure A9, which shows a sharp 15 percent increase in the number of detainees issued in the several months post-SC activation with no discernible trend pre-SC activation.

*American Community Survey:* We use publicly available ACS data downloaded from IPUMS-

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<sup>32</sup>We compare our estimates of the number of non-citizen Hispanics to estimates using the methods of Pew (2013) and Borjas (2017), who estimate the number of unauthorized Hispanics. We note that our estimates are generally larger than that of Pew (2013) and Borjas (2017), consistent with the fact that we include all individuals that can be deported (such as green card holders), not just unauthorized Hispanics (Appendix Figure A8).

USA at the University of Minnesota.<sup>33</sup> We focus on the one percent ACS samples of the U.S. population over the years 2006–2016 for food stamp take-up. The data include household characteristics such as food stamp receipt in the last year and also individual characteristics like 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, individuals who could not be eligible for deportation. The most detailed level of geography in the publicly available ACS is 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 at the PUMA level to counties based off the PUMA population in each county.<sup>34</sup>

*Panel Study of Income Dynamics:* We use data from the restricted-access Panel Study of Income Dynamics (PSID) from 2005–2015. 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 and ethnicity 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 grew up in the United States versus a foreign country or whether the household head’s mother and father were both born in the United States. The PSID added immigrants and their adult children in the 1997 wave and dropped some core families to better reflect the changing demographics of the United States (PSID 2000). PSID household characteristics include family size, number of children, household poverty, and head characteristics include employment status and industry. As with our ACS sample, we limit our sample to individuals in citizen heads of household with less than a high school degree. Among our sample, the PSID surveyed a total number of 2,427 unique household heads from 630 counties.

*Affordable Care Act:* Data on ACA sign-ups are from the Centers for Medicare and Medicaid Services (CMS). The data are available at the PUMA level, which is cross-walked to the county level, and provide ACA insurance sign-ups 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 ( $\leq 10$  plans selected), which we impute as 1, although results are robust to alternative imputations (see Data Appendix). We use data on the ACA 2016 enrollment period to avoid technical issues associated with the earliest phase of the rollout. The estimation of the number of potential enrollees by race is based on tabulations by the Assistant Secretary for Planning and Evaluation (ASPE).<sup>35</sup>

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<sup>33</sup>As mentioned previously, we were not permitted access to restricted versions of the ACS and other data sets of interest.

<sup>34</sup>We use crosswalks provided by the University of Michigan Institute for Social Research and the Missouri Census Data Center. See <http://www.psc.isr.umich.edu/dis/census/Features/puma2cnty/> and <http://mcdc.missouri.edu/websas/geocorr14.html>

<sup>35</sup>The ASPE begins with the Census year 2011 American Community Survey Public Use Microdata Sample (ACS

From these data, we calculate the share of eligible Hispanics, blacks, and non-hispanic whites that signed up for the ACA.

*Google Trends Data:* To measure awareness and perhaps deportation 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 at the Nielsen DMA media markets level. 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:

$$G(i, d) = \left[ 100 \cdot \frac{share(i, d)}{max_{\delta}\{share(i, \delta)\}} \mathbb{1}[\#(i, d) > T] \right] \quad (7)$$

where  $share(i, d)$  is the share of searches in  $d$  that contains  $i$ ,  $T$  is a threshold value of searches that must be exceeded for Google to permit access to the data, and  $max_{\delta}\{share(i, \delta)\}$  represents the maximum share of searches that contain  $i$  across all media markets  $\delta$ . Thus, under this expression,  $G(i, d)$  is equal to 100 in the metro area with the largest share of searches containing  $i$  and equal to a positive number smaller than 100 in all other metro areas that have a sufficient number of searches containing  $i$ .

We use the following commonly searched terms related to the *Deportation* topic on Google Trends: deportation, abogados de inmigracion, deportacion, immigration, inmigracion, immigration lawyer, indocumentado, undocumented. Following the literature (e.g. Burchardi, Chaney, 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 accounts for differential access to the internet for Hispanics that may vary across geographic units.

## VI. Results

### A. Food Stamp Take-Up

Figure 5 presents our main event study estimates of SC activation on food stamp take-up for non-Hispanic whites, non-Hispanic blacks, and Hispanics using the ACS data, as described in Equation 4. This specification is limited to our “high participation” sample and to citizen heads of household. 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 level shift post-activation, with Hispanic heads greatly decreasing their take-up of food stamps over time. By five years post-activation, Hispanic households reduce take-up of food

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PUMS), and excludes estimated undocumented persons. Non-citizens in the ACS are assigned a probability that they are a legal resident in the U.S. 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 Spring 2009, 2010, and 2011.

stamps by 4.2 percentage points relative to non-Hispanic whites, a 19 percent decrease from the pre-period Hispanic mean of 22.2 percent.

Table 2 presents our main results on food stamp take-up across various samples in the ACS and PSID data. Column 1 reports our main specification in the ACS citizens sample. We find that after SC activation, Hispanic citizen heads of household reduce their take-up of food stamps by 2.3 percentage points relative to non-Hispanics, a ten percent decrease from the pre-period Hispanic mean. In column 2, we report the same specification as column 1 but add an interaction between our black indicator and post-SC indicator. Our main results are virtually unchanged in the ACS data and we also find a small and insignificant black coefficient post-SC, indicating that SC did not similarly affect the behavior of minority groups less likely to be affected by immigration enforcement. In column 3, we report estimates using predicted activation (based on ICE documentation) rather than actual activation. Appendix Figure A10 presents maps that show the timing of predicted SC activation across counties. We find qualitatively similar results on the differential change in take-up for Hispanics. These results indicate that our findings are unlikely to be driven by selection bias due to the timing of SC activation across counties. However, we note that in some specifications, the post-SC indicator is positive. Although this finding is not consistent across specifications, and there is no clear post-SC trend for non-Hispanic whites in our event study estimates, it could reflect the fact that earlier SC activation may have occurred in locales with more food insecurity, suggesting the importance of our triple-differences design.

Our main findings are qualitatively similar using the PSID data. In columns 4 and 5, we report our main specification with and without an interaction between our black indicator and post-SC indicator. We find that after SC activation, Hispanic citizen heads of household reduce their take-up of food stamps by 14.5 to 17.2 percentage points relative to non-Hispanics, a 34 to 41 percent decrease from the pre-period Hispanic mean of 42.2 percent.<sup>36</sup> Notably, across all our specifications and samples (columns 1-5), we find evidence not only of differential decreases in food stamp take-up for Hispanics, but absolute decreases for Hispanic households following SC.

*Robustness:* Our results are robust to different definitions of household decision-makers. Specifically, we consider the fact that food stamp participation may be decided by females within a household. We find very similar results using a sample of citizen female heads of household or female spouses (see column 2 of Appendix Table A3). We also find similar results when we exclude individuals or families that face any risk of deportation. For example, our results are very similar when we exclude naturalized citizen heads of household, who in theory could be deportable under some circumstances (column 3 of Appendix Table A3), and when we exclude citizen heads of households with mixed-status family members (column 4 of Appendix Table A3). These results

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<sup>36</sup>There are several reasons why the magnitudes of our estimates differ between the PSID and ACS samples. First, after our sample restrictions, the PSID covers only 630 counties versus 3,079 in the ACS and differentially covers large states like California and Texas. Second, as discussed earlier, the PSID specifically added a large wave of immigrant families to the survey in 1997. Third, reported food stamp usage is much higher in the PSID versus ACS sample. When we select an ACS sample that matches the PSID in pre-period mean take-up for Hispanics, we find more similar magnitudes of our estimates (see column 1 of Appendix Table A3.)

suggest that our main findings capture a true spillover effect of deportation fear. We also find that our results are robust to dropping New York, Los Angeles, Miami, Houston, and Chicago from our sample, cities that have the highest number of Hispanic immigrants (column 5 of Appendix Table A3). Finally, we relax the assumption that Hispanics are only affected by enforcement in their county by including a spatial lag in SC activation, weighting each county’s enforcement with an exponential spatial weight matrix that places lower weight on farther locations. Again, we find that our results are virtually identical with the inclusion of a spatial lag (column 6 of Appendix Table A3), suggesting that Hispanic households are most responsive to enforcement within their own county.<sup>37</sup>

In Appendix Table A4, we also present our main results separately for Hispanics versus non-Hispanic blacks and versus non-Hispanic whites. Across all comparison groups, we find a large and significant effect of SC activation on reduced take-up of food stamps for Hispanic households. In the PSID, Hispanic households reduce their take-up of food stamps by 20.9 percentage points post-SC compared to black households and 15.9 percentage points compared to non-Hispanic whites (columns 1 and 2). In the ACS, Hispanic households reduce their take-up of food stamps after SC activation by 1.9 and 2.4 percentage points relative to blacks and non-Hispanic whites, respectively (columns 3 and 4). We also find that Puerto Ricans who have citizenship status, and Cubans, who are more likely to have political refugee status, do not respond to SC activation by reducing food stamp take-up relative to non-Hispanics (column 5).<sup>38</sup>

Appendix Table A5 explores alternative weighting schemes and alternative controls, for example including one observation for each family member in a citizen head household in the ACS, thus capturing the effect of SC at the individual level rather than household level. We continue to find significant reductions in food stamp take-up among individuals in Hispanic households relative to non-Hispanics. Similarly, we find in Appendix Table A5 that our estimates are robust to including either one observation for each family member or only one observation per household in the PSID. Our results are also very similar with a full set of race-by-year fixed effects, which requires us to drop race indicators interacted with the timing of the Great Recession from our preferred specification. We present similar event study estimates with this alternative specification in Appendix Figure A11.

## B. Affordable Care Act Sign-Up

We next turn to estimating the impact of SC intensity on Hispanic ACA sign-up. OLS estimates from Equation 5 are presented in Panel A of Table 3. In column 1, controlling only for state fixed effects and share Hispanic males, we find that a ten percent increase in Hispanic detainees is associated with a 0.35 percent reduction in Hispanic sign-ups for the ACA. Column 2 adds a sanctuary city indicator, the Democratic versus Republican vote margin in the 2008 presidential election, and share of Hispanics living in poverty. Column 3 adds the unemployment rate and FBI index crimes per capita. Finally, column 4 adds the share black sign-up for the ACA. In our

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<sup>37</sup>The coefficient on the spatial lag is small and not statistically significant.

<sup>38</sup>We thank Ted Miguel and Thomas Lemieux for the suggestion.

preferred specification with the full set of controls (column 4), we find that a ten percent increase in Hispanic detainees is associated with a 0.14 percent reduction in Hispanic sign-ups for the ACA. The sensitivity of the results to the addition of controls suggests that SC intensity is correlated with unobservables that affect sign-up (Altonji, Elder, and Taber 2005), with endogeneity tests rejecting the exogeneity of Hispanic detainees (p-value  $< 0.0001$ ).

To address the endogeneity of SC intensity, we predict the share of Hispanic detainees using plausibly exogenous variation in baseline shares of Hispanic foreign-born across counties as described in Equation 6. Panel B of Table 3 presents our first stage estimates, with  $F$ -statistics ranging from 11 to 12. Panel C of Table 3 presents the two-stage least squares results. Controlling only for state fixed effects and the share Hispanic males (column 1), we find that a ten percent increase in detainees is associated with a 3.0 percent reduction in Hispanic ACA sign-ups. Results are similar but slightly smaller in magnitude with the addition of county-level baseline controls. In our preferred specification (column 4), we find that a ten percent increase in detainees is associated with a 2.0 percent reduction in Hispanic ACA sign-ups. We note that the OLS correlations between SC intensity and Hispanic ACA sign-up are smaller than our two-stage least squares estimates. One explanation is due to selection, i.e., SC was more intensive in areas with lower propensity to sign-up for health insurance. This selection would result in our OLS estimates being biased toward zero and understating the true negative effect of SC intensity on ACA take-up among eligible Hispanics.

To put our two-stage least squares estimate in perspective, SC led to the issuance of roughly 730,000 detainees in our federal exchange sample during the 2008 to 2013 time period. We estimate that there were roughly 6.5 million non-citizen Hispanics in the ACA sample during this time period, suggesting that approximately 11 percent of the at-risk population was issued a detainer. Our estimates imply that, in the absence of SC, ACA sign-ups among eligible Hispanics would have been 22 percent higher.<sup>39</sup>

*Robustness:* Appendix Table A6 presents a series of robustness checks. Our results are robust to additional controls that may affect Hispanic ACA sign-up, such as whether a county cooperates with ICE through a 287(g) agreement, or whether a county has a community health center which may serve as a substitute for health insurance. In addition, some counties had health navigator programs that assisted Hispanics in enrolling in the ACA. We control for this navigator program since it might independently affect take-up. Its inclusion does not alter our main results. We also relax the assumption that Hispanics are only affected by enforcement in their county by including a spatial lag in detainer intensity. Again, we find that our results are virtually identical with the inclusion of a spatial lag. Finally, as discussed previously, we probe the exclusion restriction of our identification strategy by controlling for contemporaneous Hispanic country-of-origin shares. These contemporaneous shares account for differences in the underlying propensity to utilize healthcare by country of origin (Carrasquillo, Carrasquillo, and Shea 2000). We find very similar results, suggesting that the exclusion restriction is likely valid in our setting.

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<sup>39</sup>We multiply the share issued detainees during SC in the federal exchange sample by our coefficient from column 4 of Table 3 ( $11 \times 2.0$ ).

Our results are also robust to alternative measures of the estimated number of non-citizen Hispanics in each county that do not account for underreporting and that use historical data from the 1990 Census (see Appendix Table A7).

In addition, given that SC primarily affected Hispanic individuals, we do not anticipate that SC intensity led to decreases in ACA sign-up among other racial/ethnic groups. As a placebo test, we regress our measure of share Hispanic detainees issued on share of eligible blacks and eligible whites that signed up for the ACA. Results in Appendix Table A8 suggest no significant relationship in our two-stage least squares results between SC intensity and either black or white ACA sign-up, although large standard errors make definitive conclusions difficult.

## VII. Mechanisms

In this section, we explore potential mechanisms for our results. We begin by examining the role fear may have played before turning to other postulated mechanisms, including information, compositional changes, and employment responses.

### A. Fear

SC increased the number of detainees issued and forcible removals from the interior, which may have increased deportation fear. Indeed, Pew Research Center survey data demonstrate a positive correlation between respondents' knowing someone who was detained and being fearful of the same fate befalling a family member or close contact (see Figure 6).<sup>40</sup> This relationship has also been described in anecdotal evidence with regards to SC activation, as detailed in the 2011 Task Force Review on Secure Communities (HSAC Task Force 2011).

To formally explore whether fear may be contributing to the findings reported above, we present five analyses. First, we use the Google Trends data on deportation-related search terms in English and Spanish available at the DMA media market level to test whether such searches increase in the years post-SC activation. We condition on year fixed effects, log neutral searches (such as popular Hispanic actors/musicians/politicians), and DMA media market fixed effects, clustering standard errors at the DMA media market level. We find no discernible pre-trend, but a sharp 20 percent increase in normalized deportation-related searches immediately following SC activation (see Figure 7), consistent with at least an awareness of the SC program if not fear of its potential consequences.

Second, we test the hypothesis that households and communities with more mixing or exposure between non-citizen and citizen Hispanics should be more influenced by SC activation, as suggested

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<sup>40</sup>Pew asked the following question in 2010 and 2013: "Regardless of your own immigration or citizenship status, how much, if at all, do you worry that you, a family member, or a close friend could be deported? Would you say that you worry a lot, some, not much, or not at all?" From this question, we define individuals who respond that they worry a lot or some as being "fearful" and limit the sample to Hispanic citizen respondents so as to more nearly approximate spillover effects. We also limit the sample to states with at least five respondents. In 2010, Pew also asked a specific question on knowledge of detention/deportation: "Do you personally know someone who has been deported or detained by the federal government for immigration reasons in the last 12 months?" We use the 2010 data in Figure 6.

by our model and qualitative findings. Exposure to non-citizens is highly relevant for Hispanic communities because Hispanics live in ethnically homogeneous neighborhoods, with Hispanic segregation generally increasing over the past decade.<sup>41</sup> These results are presented in column 1 of Table 4. In the ACS sample, we find substantially larger effects of SC in counties with a high share of Hispanic households that are mixed-status, defined as counties in the top decile. Our estimate in column 1 suggests that post-SC, Hispanic households from high mixed-status counties decrease take-up of food stamps by an additional 3.0 percentage points, representing an overall decrease of 5.3 percentage points, a 24 percent decrease from the pre-period mean in the ACS.

Immigration enforcement activity directed against those who have committed minor offenses has also been argued to heighten fear and impede participation in government-associated activities, as SC led to substantial increases in deportations for individuals arrested for misdemeanors such as public drunkenness or jaywalking (HSAC Task Force 2011). Our third analysis, reported in column 2, finds that the effect of SC on take-up is larger in counties where the number of non-violent detainees (often issued for misdemeanor offenses) exceeds the number of violent detainees (often issued for assault or murder). In counties with relatively more non-violent detainees, Hispanic households reduce their take-up of food stamps by an additional 1.5 percentage points. Fourth, using the Pew data, we test whether reductions in program participation are higher in areas with increasing deportation fear measured at the Census division level (the finest geography available in 2013). We find that a one standard deviation increase in fear is associated with an additional 1.3 percentage point decline in food stamp take-up among Hispanics after SC activation (column 3).

Fifth, we explore the role of sanctuary cities and counties. As described previously, sanctuary cities share in common their restrictions on how much local governments cooperate with ICE requests to detain immigrants. If fear explains our findings, then Hispanic households in sanctuary cities should have less fear and thus exhibit a lower response to SC. In column 4, we interact our Hispanic and post-SC indicator with an indicator for an active sanctuary city policy during the period of SC activation. We find that almost all of our main effects are driven by locations with no sanctuary policy. In contrast, we find a significant and *positive* effect of SC activation on Hispanics in sanctuary cities.<sup>42</sup> Taken together, these five findings suggest that fear of deportation is a likely explanatory mechanism.<sup>43</sup>

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<sup>41</sup>See <https://www.brookings.edu/opinions/census-data-blacks-and-hispanics-take-different-segregation-paths/>. Mixed-status families are also much more common among Hispanic households relative to other groups. In the ACS data, we find that 18 percent of Hispanics households have at least one non-citizen Hispanic, compared to 0.20 percent of white households and 0.04 percent of black households.

<sup>42</sup>This result is robust to various definitions of a sanctuary jurisdiction (results available on request). See the Online Appendix for institutional details of sanctuary cities.

<sup>43</sup>A related explanation is the role of legitimacy, or the theory that individuals cooperate or engage with legal authorities based on their perception of how fairly these authorities deal with members of the public (Tyler 2006). Under this theory, SC may have reduced program participation because it corroded the perceived legitimacy of the federal government in the eyes of Hispanic citizens. However, we find larger effects of SC in areas with more mixed-status households and null effects for Cubans and Puerto Ricans as described previously, findings that are hard to reconcile with a general theory of legitimacy.



## B. Information

We next consider an alternative mechanism – the role of information. 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 Hispanic households that had never previously taken up the relevant public program prior to SC versus Hispanic 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 6 of Table 2 presents these results in the PSID sample where we limit the sample exclusively to all individuals in households that have taken up food stamps prior to SC activation. In our PSID sample, 45 percent of Hispanic heads are prior users of food stamps before SC activation and on average, prior users take up food stamps 60 percent of the time before SC activation. We find that the decline in food stamp take-up post SC is largely driven by Hispanic heads that have previously taken up food stamps. Among prior users, SC activation reduced Hispanic heads of household take-up by 23.9 percentage points relative to non-Hispanics, a 40 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 qualitative evidence suggesting that Hispanic families are not renewing SNAP benefits, also lessens the likelihood that an explanation like stigma is driving our results.

## C. Compositional Changes

We also consider the possibility that SC activation may have affected the number or types of Hispanic citizens living within a particular county or within the United States, or more subtly, the number or types willing to declare their ethnicity or report program take-up in surveys like the ACS. This line of query is important since the Great Recession affected migration, in general reducing it (Johnson et al. 2016), although immigrants were more sensitive to local economic downturns (Cadena and Kovak 2016). While we note that these responses may also be driven by fear, compositional changes in Hispanic survey respondents within a particular county or changes in reporting behavior may lead to a different interpretation of our main findings.

To test this channel, Table 5 presents our main triple-differences specification in our high-

participation food stamp sample in the ACS, where the dependent variables are average race-specific observable characteristics of citizens in each county-year and the percent mixed-status and Hispanic population in each county-year. We find no significant relationship between SC activation and compositional changes in the types of Hispanics relative to non-Hispanics in each county-year in terms of share of families with any children, average family size, poverty level, or employment rate (columns 1–4 of Table 5). We also find no change in the percent of Hispanic mixed-status families or the percent Hispanic population within a county post-SC activation (columns 5–6 of Table 5). In unreported results, we also find that ACS estimates of food stamp take-up are generally lower than available official yearly state-level estimates across all racial/ethnic groups. However, this reporting gap for all groups, particularly for Hispanics, does not change after SC activation. These results suggest that compositional changes and changes in reporting behavior are unlikely to explain our main findings.

#### D. Employment Responses

Finally, we consider the possibility that SC may have affected the labor market responses of Hispanic citizens, which in turn may affect take-up of safety net programs. Specifically, SC may have led to employment changes of citizens to the extent that unauthorized immigrants were either removed from the labor market or shifted out of formal sector employment due to fear. During the time period of our study, some states used the E-Verify program to check workers’ eligibility to work legally in the United States. ICE also regularly conducted I-9 audits at workplaces to verify whether workers provided proof of identification (driver’s license or a Social Security card) when they were hired.

To test this possibility, we analyze the effect of SC activation on the share employed among the working age population using our main triple-differences specification in our high-participation citizens sample. In column 4 of Table 5, we find no differential effect of SC activation on the employment rate of Hispanics relative to non-Hispanic whites and blacks. We also find no significant relationship between the share of Hispanic detainees issued in a county and the employment rate of Hispanic citizens. In sum, these results suggest that our main findings are unlikely to be driven by employment responses to SC.<sup>44</sup>

### VIII. Conclusion

In this study, we test the hypothesis that linkages between citizens and non-citizens reduce safety net participation in the presence of enhanced immigration enforcement activity. Leveraging the roll-out and intensity of Secure Communities under the Obama administration, we find that citizen 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 among Hispanic households. We find evidence that our results may be driven by

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<sup>44</sup>Using a differences-in-differences approach, East et al. (2018) find that SC reduced the employment rate of all citizens by 0.5 percent. Our triple-differences approach compares the differential change for Hispanic citizens relative to other groups.

deportation fear rather than lack of benefit information or stigma. Mixed-status households, areas with a higher incidence of detainers issued for low-level arrests, and areas with greater increases in deportation fear exhibit larger decreases in take-up in response to SC. In contrast, Hispanic households residing in sanctuary cities showed little response to SC activation.

Our results have several implications on health and well-being for Hispanic households. Extrapolating from the work of other scholars, families could experience adverse long-run consequences from forgoing benefits in response to stricter immigration enforcement. For example, Hoynes, Schanzenbach, and Almond (2016) show that food stamp take-up reduces the incidence of metabolic syndrome in adulthood. Tiehen et al. (2012) find that food stamp participation reduced the child poverty rate by 5.6 percent from 2000 to 2009. These results suggest that reductions in food stamp usage among Hispanics in response to immigration enforcement could have long-run consequences for health and economic security. Although the health effects of insurance are debated, there is evidence that it provides protection from medical debt and related financial crises (Courtemanche et al. 2018; Finkelstein et al. 2012).

Our results on the ACA also suggest that the effects of deportation fear may not be circumscribed to Hispanic households and communities. Since Hispanics tend to have better health outcomes than similarly situated low-income whites or blacks (Franzini, Ribble, and Keddie 2001), their reduced participation in insurance markets could translate into higher premiums for other demographic groups. Most broadly, our results reveal that safety net programs interact with other government policies, yielding potentially unexpected results for families.

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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)
<i>Outcome</i>			
<i>ACS Sample N = 24,063</i>			
Share Food Stamp	0.186 (0.208)	0.019 (0.014)	0.051*** (0.016)
Poverty FPL < 130	0.387 (0.487)	0.014 (0.038)	-0.082 (0.066)
Family Size > 1	0.739 (0.439)	-0.075*** (0.017)	0.086** (0.039)
Share Any Child	0.276 (0.253)	-0.040*** (0.013)	0.010 (0.019)
Employment Rate	0.536 (1.060)	-0.018 (0.029)	0.031 (0.033)
$\Delta$ Share Food Stamp	0.004 (0.365)	0.006 (0.024)	0.049 (0.044)
$\Delta$ Poverty FPL < 130	-0.010 (0.667)	-0.119* (0.064)	-0.105 (0.126)
$\Delta$ Family Size > 1	0.028 (0.569)	0.024 (0.025)	0.066 (0.062)
$\Delta$ Share Any Child	-0.028 (0.481)	-0.029 (0.029)	-0.003 (0.040)
$\Delta$ Employment Rate	-0.018 (0.630)	0.073 (0.047)	-0.006 (0.049)

Note: Data from the ACS from 2005–2007. 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 and control variables 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. Observations are weighted by the race-specific population in each county. Robust standard errors clustered at the county level are reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table 2: Triple Differences Estimation – Food Stamp Take-Up

<i>Sample</i>	<i>ACS Citizens</i>	<i>ACS Citizens</i>	<i>ACS Citizens Predicted</i>	<i>PSID Citizens</i>	<i>PSID Citizens</i>	<i>PSID Citizens Prior Users</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Hispanic × Post	-0.023*** (0.005)	-0.024*** (0.005)	-0.017*** (0.004)	-0.172** (0.068)	-0.145** (0.069)	-0.239** (0.115)
Post	0.011*** (0.003)	0.012*** (0.003)	0.002 (0.005)	0.049 (0.043)	0.019 (0.043)	-0.003 (0.066)
Black × Post		-0.003 (0.004)			0.084** (0.035)	
Pre-Period Hisp. Mean	0.222	0.222	0.222	0.422	0.422	0.603
Fixed Effects	State-Year, State-Race, County					
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	85,312	85,312	85,312	27,677	27,677	18,568
Number Clusters	3,079	3,079	3,079	630	630	469

Note: Data from ACS 2006–2016 in columns (1)–(3) and from PSID 2005–2015 in columns (4)–(6). The data are limited to heads of households with less than a high school degree, our high participation sample. The citizens sample in the ACS includes heads of households that are U.S. citizens. The citizens sample in the PSID includes individuals from families where the head of household grew up in the United States. Prior users in the PSID includes all heads of households that had previously taken up food stamps prior to SC activation. Column 3 estimates our main specification using predicted activation instead of actual activation. Baseline controls in the PSID include family size, number of children, income relative to federal poverty line, industry, employment status, and FBI crime decile-by-race fixed effects. Baseline controls in the ACS include log poverty, an indicator for average family size greater than one, employment rate, 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, race interacted with indicators for the timing of the Great Recession, and race-by-state changes in employment during the Great Recession. 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 clustered at the county level are reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table 3: OLS and 2SLS Results – ACA Take-Up

	<i>Outcome: Share Hispanic ACA Take-Up</i>			
	(1)	(2)	(3)	(4)
	<i>Panel A: OLS Results</i>			
Share Hispanic Detainers	-0.035*** (0.007)	-0.024*** (0.006)	-0.021*** (0.006)	-0.014*** (0.005)
	<i>Panel B: First Stage</i>			
Shift-Share IV	0.260*** (0.076)	0.253*** (0.072)	0.249*** (0.070)	0.240*** (0.068)
	<i>Panel C: 2SLS Results</i>			
Share Hispanic Detainers	-0.303** (0.123)	-0.273*** (0.106)	-0.275** (0.113)	-0.200** (0.089)
F-Statistic	11.58	12.52	12.62	12.61
Endogeneity Test	$p < 0.0001$	$p < 0.0001$	$p < 0.0001$	$p < 0.0001$
Fixed Effects		State		
Controls	Baseline	(1)	(2)	(3)
		+ Sanc, Pov, Pol	+ Unemp, Crime	+ Black ACA
Observations	2,044	2,044	2,044	2,044

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 county-level controls include share Hispanic males. Column 2 adds controls for a sanctuary city indicator, the Democratic versus Republican vote margin in the 2008 presidential election, the share of Hispanics in poverty, and missing indicators for these variables. Column 3 adds the unemployment rate, crime rate, and missing indicators for these variables. Column 4 adds share black ACA sign-up and a missing indicator for this variable. Observations are weighted by the estimated number of Hispanics eligible for the ACA in each county. Robust standard errors are reported in parentheses. We report first-stage F-statistics and p-values for endogeneity tests under the null hypothesis that Share Hispanic Detainers is exogenous. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table 4: Triple Differences Estimation – Food Stamp Take-Up Heterogeneity

<i>Sample</i>	<i>ACS</i>	<i>ACS</i>	<i>ACS</i>	<i>ACS</i>
	<i>Citizens</i>	<i>Citizens</i>	<i>Citizens</i>	<i>Citizens</i>
	(1)	(2)	(3)	(4)
Hispanic × Post	−0.023*** (0.005)	−0.016*** (0.005)	−0.037*** (0.007)	−0.027*** (0.005)
Hispanic × Post × Mixed	−0.030* (0.017)			
Hispanic × Post × Petty Severe Ratio		−0.015** (0.007)		
Hispanic × Post × Δ Pew Fear			−0.129** (0.056)	
Hispanic × Post × Sanctuary City				0.022** (0.009)
Pre-Period Hisp. Mean	0.222	0.222	0.222	0.222
Fixed Effects	State-Year, State-Race, County			
Baseline Controls	Yes	Yes	Yes	Yes
Observations	76,977	85,312	75,427	85,312
Number Clusters	3,079	3,079	3,079	3,079

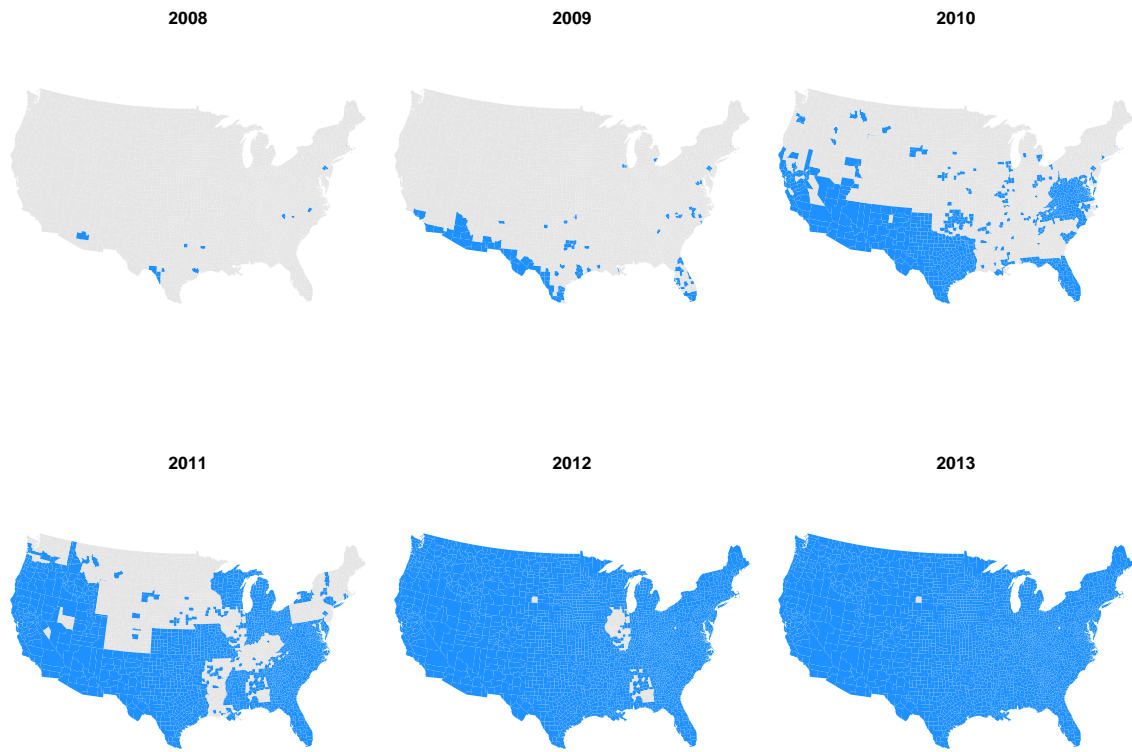
Note: Data from ACS from 2006–2016. The data are limited to heads of households with less than a high school degree, our high participation sample. The citizens sample in the ACS includes heads of households that are U.S. citizens. Mixed is defined as an indicator for the top decile of the share of mixed families among Hispanic households in a county. Mixed status family is defined as a Hispanic citizen head of household with any family member that is a Hispanic non-citizen. The petty/severe ratio is an indicator for having more detainees issued for minor offenses than serious violent offenses. Δ Pew Fear is measured as the change in the share that are worried a family member or close friend could be deported between 2013 and 2010 from Pew. This measure is defined at the Census division level. Sanctuary city is an indicator for an active sanctuary city policy during the period of SC activation. All specifications contain main terms and the full set of interactions with the Hispanic indicator and post-SC indicator. Baseline controls in the ACS include log poverty, an indicator for average family size greater than one, employment rate, 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, race interacted with indicators for the timing of the Great Recession, and race-by-state changes in employment during the Great Recession. Observations in the ACS are weighted by the race-specific population in each county. Robust standard errors clustered at the county level are reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table 5: Compositional Changes

<i>Outcome</i>	<i>% Any Child</i>	<i>% Fam &gt; 1</i>	<i>% Pov &lt; 130</i>	<i>Emp Rate</i>	<i>% Mixed</i>	<i>% Hisp</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Hispanic × Post	−0.005 (0.005)	0.0002 (0.003)	0.001 (0.012)	−0.018 (0.032)		
Post	0.007* (0.004)	0.002 (0.002)	0.003 (0.010)	0.005 (0.010)	0.004 (0.007)	−0.0002 (0.005)
Pre-Period Hisp. Mean	0.397	0.679	0.488	0.610	0.157	0.098
Fixed Effects		State-Year, State-Race, County				
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	85,312	85,312	85,312	85,312	26,148	26,148
Number Clusters	3,079	3,079	3,079	3,079	3,079	3,079

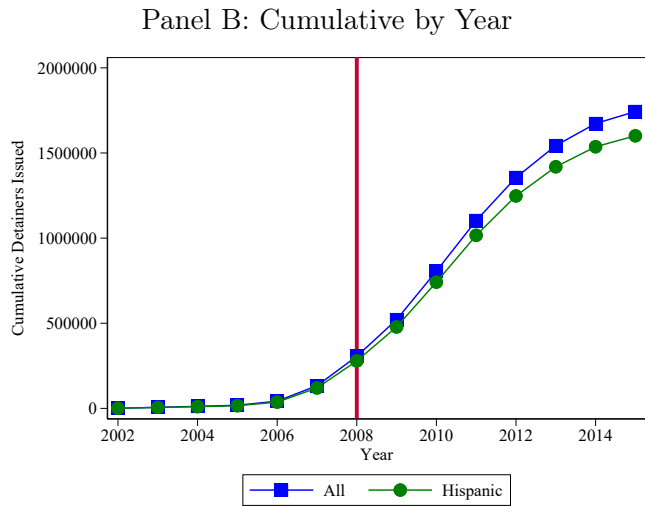
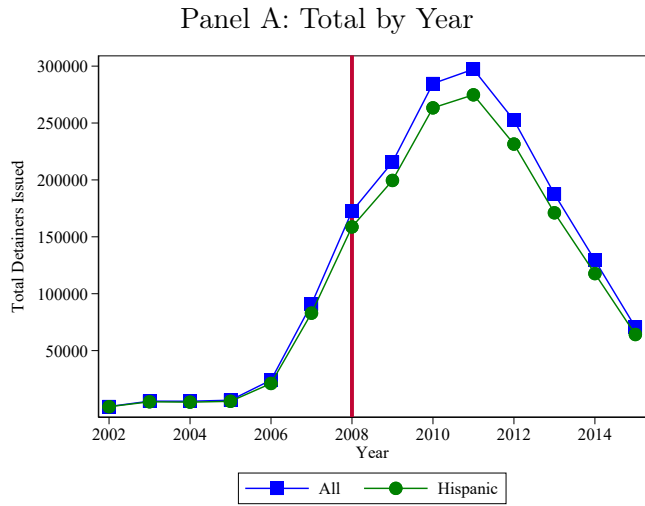
Note: Data from ACS from 2006–2016. Baseline controls include FBI crime decile-by-race fixed effects and, in column (1) log poverty and employment rate, in column (2) log poverty and employment rate, in column (3) family size and employment rate, in column (4) log poverty and family size, and in columns (5)–(6) log poverty, family size, and employment rate. All regressions control for county fixed effects, state-by-year fixed effects, state-by-race fixed effects, race interacted with indicators for the timing of the Great Recession, and race-by-state changes in employment during the Great Recession. Observations are weighted by the race-specific population in each county. Robust standard errors clustered at the county level are reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Figure 1: Secure Communities Activation

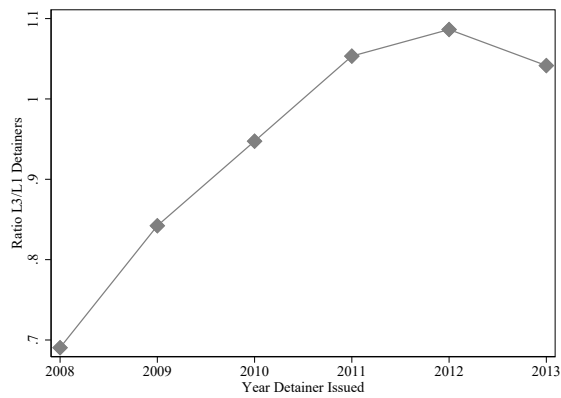


Note: Data from FOIA and public records.

Figure 2: Detainers by Year



Panel C: Ratio of Low-Level to Violent Offenses

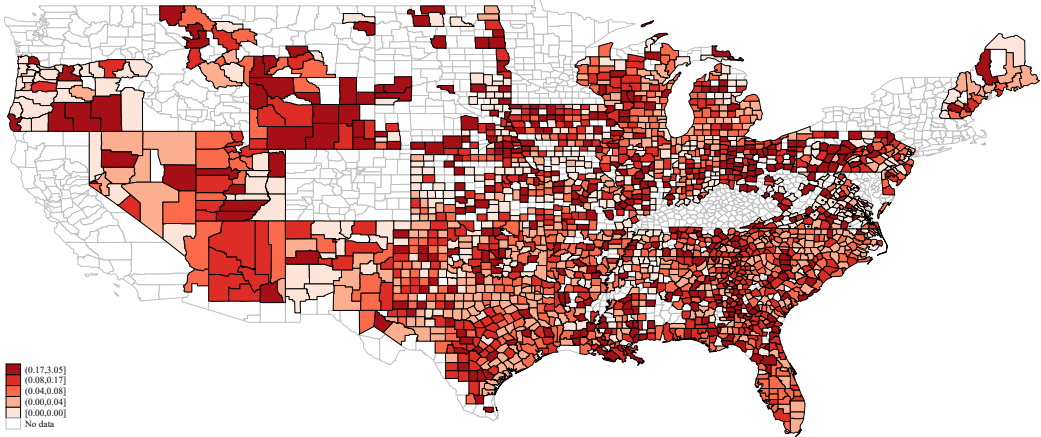


Note: Data from FOIA.

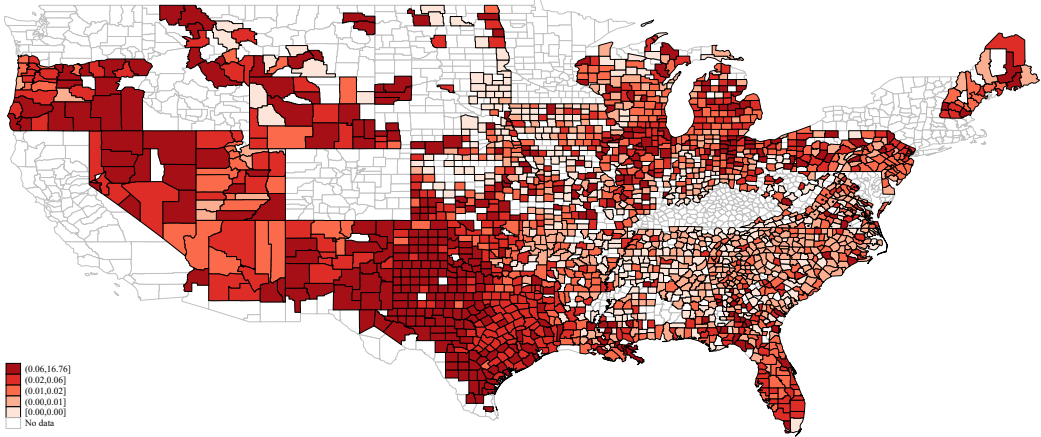


Figure 3: Detainers and Shift-Share Instrument

Panel A: Share Hispanic Detainers

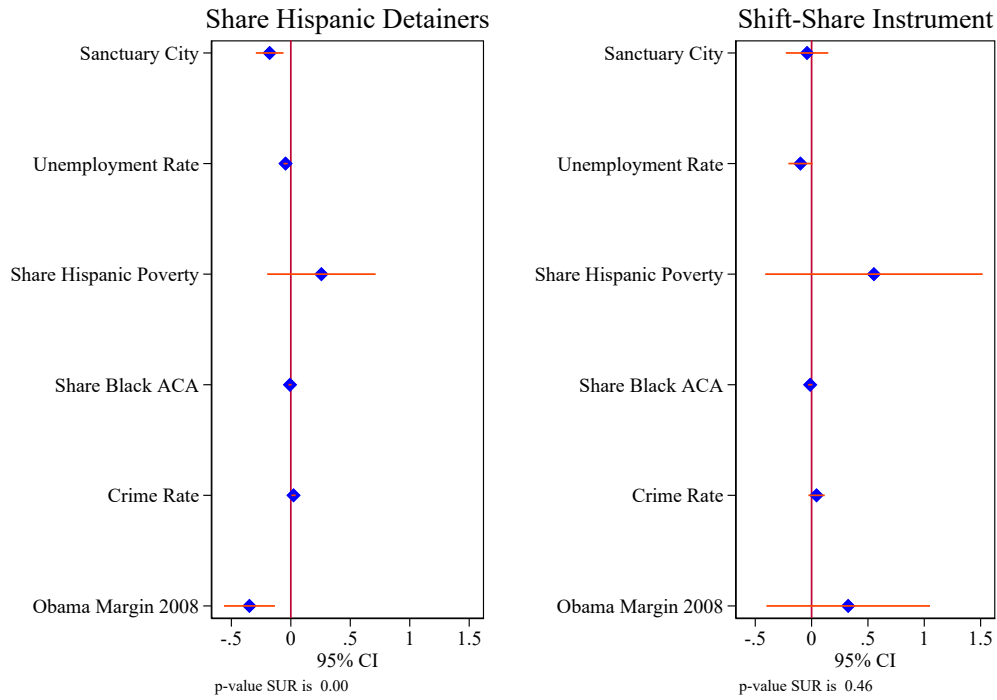


Panel B: Shift-Share Instrument



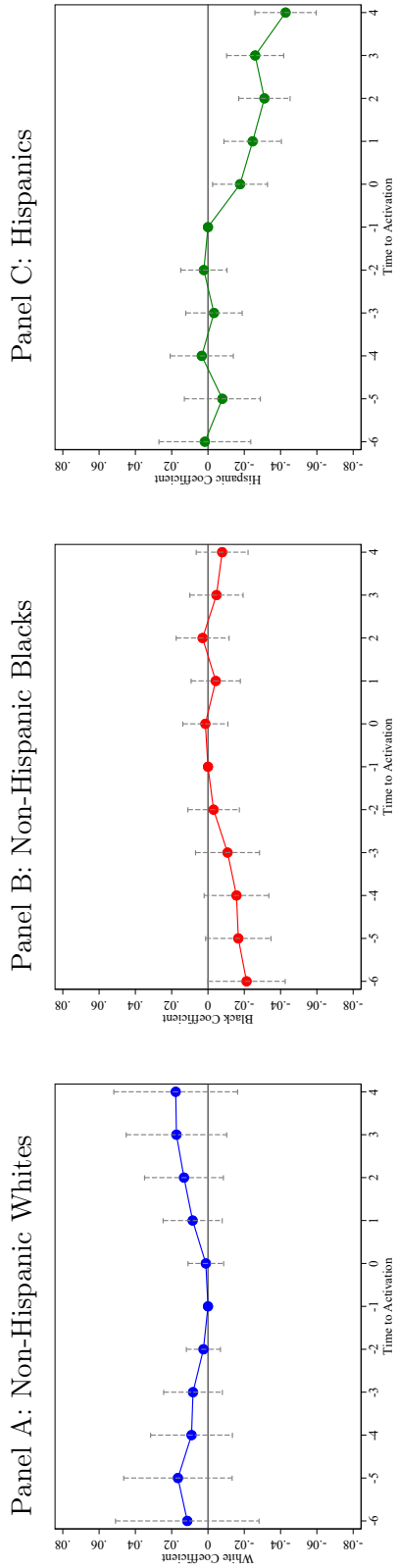
Note: Data from FOIA. States that are missing data are not in the federal exchange sample and some counties within federal exchange states have insufficient data. We also exclude border counties.

Figure 4: Relationship between Share Detainers, Shift-Share Instrument, and Baseline Controls



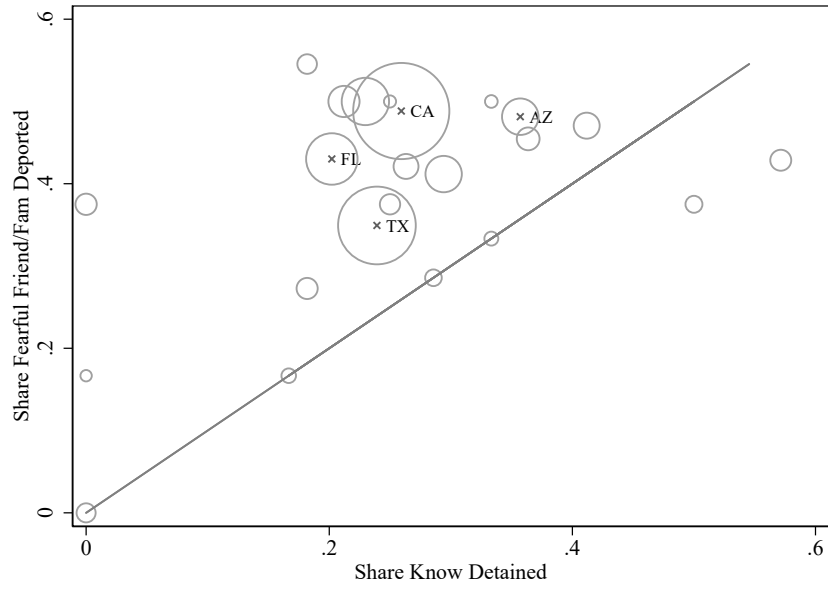
Note: Data from FOIA, ACS, ACA. The shift-share instrument is constructed as the predicted total number of detainers normalized by the predicted number of non-citizen Hispanics based on data from the American Community Survey and National Center for Health Statistics. This figure represents OLS regressions of each baseline characteristic on our endogenous variable and shift-share instrument with robust standard errors. All specifications contain state fixed effects and share Hispanic males. Observations are weighted by the estimated number of Hispanics eligible for the ACA in each county. The reported joint p-value is from a seemingly unrelated regression.

Figure 5: Event Study of Food Stamp Take-Up



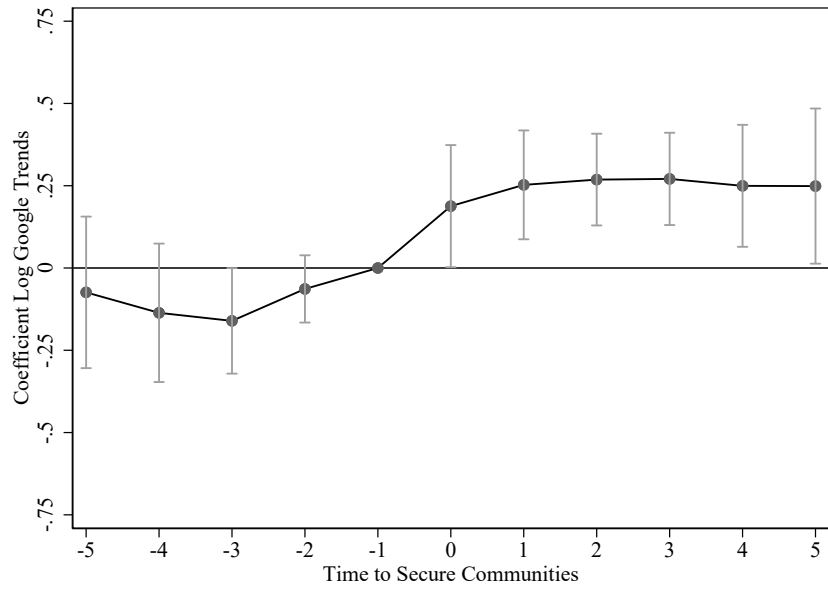
Note: Data from ACS from 2006–2016. Coefficients and 95% confidence intervals are plotted. The data are limited to heads of households with less than a high school degree, our high participation sample. The citizens sample in the ACS includes heads of households that are U.S. citizens. Baseline controls in the ACS include log poverty, an indicator for average family size greater than one, employment rate, 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, race interacted with indicators for the timing of the Great Recession, and race-by-state changes in employment during the Great Recession. Observations in the ACS are weighted by the race-specific population in each county. Robust standard errors are clustered at the county level.

Figure 6: Correlation between Fear and Knowing Someone Detained



Note: Data from Pew Hispanic Survey 2010. The sample excludes non-citizens and states with five or fewer respondents. Fear refers to fear that a family member or close contact will be deported. The knowledge measure refers to the share of people responding affirmatively that they know someone who has been detained or deported. The size of the bubble represents the size of the Hispanic population. The correlation between share fear and share know detained is 0.50. The 45° line is drawn for reference.

Figure 7: Google Deportation Searches Event Study



Note: Data from Google Trends. This figure represent event study estimates of the time to SC activation on the log normalized number of deportation-related searches at the DMA media markets level. All specifications control for DMA fixed effects. Standard errors are clustered at the DMA level.

## Appendix A: Additional Results

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

	All (1)	Hispanic-White Late vs. Early (2)	Hispanic-Black Late vs. Early (3)
<i>Outcome</i>	<i>PSID Sample N = 16,801</i>		
Share Food Stamp	0.239 (0.365)	0.996*** (0.096)	1.362*** (0.190)
Average Family Size	3.066 (1.652)	3.051* (1.677)	−2.012*** (0.491)
Average # Children	1.160 (1.354)	2.149* (1.237)	−0.475 (0.457)
Poverty FPL	264.251 (209.562)	110.800 (229.000)	76.200 (67.110)
Δ Share Food Stamp	0.109 (0.579)	0.122 (0.641)	0.087 (1.122)
Δ Average Family Size	−0.524 (1.628)	0.071 (0.440)	2.639* (1.383)
Δ Average # Children	−0.550 (1.215)	−1.063* (0.558)	1.744 (1.096)
Δ Poverty FPL	58.388 (138.409)	246.200 (247.400)	221.800*** (48.860)

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 and control variables 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 reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Table A2: Effect of SC on Arrests and Immigration Enforcement

<i>Outcome</i>	<i>Offenses Known</i>	<i>Arrests</i>	<i>Submissions</i>	<i>Matches</i>	<i>Detainers</i>
	(1)	(2)	(3)	(4)	(5)
Post	-109.8 (265.6)	121.3 (124.5)	6183.6*** (807.1)	336.6*** (81.53)	151.2*** (31.10)
Pre-Period Mean	3888.22	1032.03	287.47	15.21	35.43
Fixed Effects	State-Year, County				
Observations	34,210	34,210	31,928	31,928	32,010
Number Clusters	3,110	3,110	3,110	3,110	3,110

Note: Data on offenses known to law enforcement and offense cleared by arrest are from UCR from 2005–2015. Data on fingerprint submissions, matches, and detainers are from FOIA requests to ICE from 2006–2014. All regressions control for county fixed effects and state-by-year fixed effects. Robust standard errors clustered at the county level are reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Table A3: Main Results on Food Stamp Take-Up – Alternative Samples

<i>Sample</i>	<i>Alternative Samples</i>					<i>Spatial Lag</i>
	<i>ACS</i>	<i>ACS</i>	<i>ACS</i>	<i>ACS</i>	<i>ACS</i>	<i>ACS</i>
	<i>Match PSID</i>	<i>HR Female</i>	<i>No Nat.</i>	<i>No Mixed</i>	<i>Drop Cities</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Hispanic × Post	-0.051*** (0.014)	-0.019*** (0.006)	-0.025*** (0.007)	-0.022*** (0.005)	-0.022*** (0.005)	-0.023*** (0.005)
Post	0.001 (0.007)	0.005 (0.004)	0.012*** (0.004)	0.012*** (0.003)	0.013*** (0.004)	0.011*** (0.004)
Fixed Effects		State-Year, State-Race, County				
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,933	73,413	82,238	83,709	85,147	85,213

Note: In column 1, we estimate our main specification in the ACS using a sample that more closely approximates the PSID sample. In column 2, we estimate our main specification in the ACS using a sample of highest-ranking females (either female head of household or female spouse). In column 3, we estimate our main specification in the ACS using a sample of Hispanic citizen heads of households excluding naturalized citizens. In column 4, we estimate our main specification in the ACS using a sample of Hispanic citizen heads of households excluding families that are mixed-status. In column 5, we estimate our main specification in the ACS dropping New York, Los Angeles, Miami, Houston, and Chicago. In column 6, we estimate our main specification in the ACS controlling for a spatial lag in SC activation using an exponential model with distance decay parameter of 0.05 km. Baseline controls in the ACS include log poverty, an indicator for average family size greater than one, employment rate, 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, race interacted with indicators for the timing of the Great Recession, and race-by-state changes in employment during the Great Recession. Observations in the ACS are weighted by the race-specific population in each county. Robust standard errors clustered at the county level are reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.



Appendix Table A4: Main Results on Food Stamp Take-Up – Race Specific Comparisons

<i>Sample</i>	<i>PSID</i> <i>Citizens</i> (1)	<i>PSID</i> <i>Citizens</i> (2)	<i>ACS</i> <i>Citizens</i> (3)	<i>ACS</i> <i>Citizens</i> (4)	<i>ACS</i> <i>Citizens</i> (5)
Hispanic × Post	-0.209*** (0.075)	-0.159** (0.076)	-0.019*** (0.006)	-0.024*** (0.005)	-0.024*** (0.005)
Post	0.072 (0.075)	-0.030 (0.053)	0.015** (0.006)	0.012*** (0.004)	0.012*** (0.003)
Puerto Rican × Post					-0.004 (0.009)
Cuban × Post					-0.010 (0.016)
Comparison	Hisp/Black	Hisp/White	Hisp/Black	Hisp/White	All
Fixed Effects		State-Year, State-Race, County			
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Observations	18,320	11,936	51,467	59,993	92,396

Note: In this table, we replicate our main results comparing Hispanics to each race group. Baseline controls in the PSID include family size, number of children, income relative to federal poverty line, industry, employment status, and FBI crime decile-by-race fixed effects. Baseline controls in the ACS include log poverty, an indicator for average family size greater than one, employment rate, 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, race interacted with indicators for the timing of the Great Recession, and race-by-state changes in employment during the Great Recession. 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 clustered at the county level are reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Table A5: Main Results on Food Stamp Take-Up – Alternative Weighting and Controls

<i>Sample</i>	<i>No Weights ACS # Hisp &gt; 25</i>	<i>No Weights ACS # Hisp &gt; 25 No Sanc</i>	<i>Individual ACS Citizens</i>	<i>Household PSID Citizens</i>	<i>Hisp Share ACS Citizens</i>	<i>Race*Yr FE ACS Citizens</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Hispanic × Post	−0.011** (0.005)	−0.010** (0.005)	−0.026*** (0.006)	−0.134* (0.080)	−0.031*** (0.005)	−0.024*** (0.008)
Post	0.019*** (0.006)	0.020*** (0.006)	0.010*** (0.005)	0.045 (0.043)	0.013*** (0.003)	0.012*** (0.004)
Fixed Effects			State-Year, State-Race, County			
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	83,863	79,770	85,312	7,793	85,312	85,312

Note: Column 1 estimates our main results in the ACS with no weights, limited to counties with at least 25 Hispanic citizen heads. Column 2 estimates our main results in the ACS with no weights, limited to counties with at least 25 Hispanic citizen heads, excluding sanctuary jurisdictions. Column 3 estimates our main results in the ACS with weights using one observation per person in each household. Column 4 estimates our main results in the PSID with weights using one observation per household head. Column 5 estimates our main results in the ACS with weights controlling for the share Hispanic. Column 6 estimates our main results in the ACS with weights controlling non-parametrically for race-by-year fixed effects. Baseline controls in the PSID include family size, number of children, income relative to federal poverty line, industry, employment status, and FBI crime decile-by-race fixed effects. Baseline controls in the ACS include log poverty, an indicator for average family size greater than one, employment rate, 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, race interacted with indicators for the timing of the Great Recession, and race-by-state changes in employment during the Great Recession. 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 clustered at the county level are reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Table A6: 2SLS Results — ACA Take-Up Robustness

<i>Control</i>	<i>287(g)</i>	<i>CHC</i>	<i>Navigator</i>	<i>All (1-3)</i>	<i>Spatial Lag</i>	<i>FB Shares</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Share Hispanic Detainers	-0.196** (0.089)	-0.202** (0.089)	-0.198** (0.092)	-0.200** (0.093)	-0.210** (0.093)	-0.095* (0.051)
F-Statistic	12.84	12.44	12.77	12.93	12.18	12.75
Fixed Effects	State					
Controls	Baseline, Sanc, Pov, Pol, Unemp, Crime, Black ACA					
Observations	2,044	2,044	2,044	2,044	2,044	2,044

Note: The dependent variable is the share of eligible Hispanics that sign up for the ACA. Column 1 adds a control for whether a county has a 287(g) agreement with ICE. Column 2 adds a control for whether a county has a community health center. Column 3 adds a control for whether a county has a Hispanic health navigator. Column 4 adds all three controls. Column 5 adds a spatial lag for detainer intensity using an exponential model with distance decay parameter of 0.05 km. Column 6 controls for 2005–2009 share foreign born from each Hispanic country of origin. All regressions control for state fixed effects, share Hispanic males, a sanctuary city indicator, the Democratic versus Republican vote margin in the 2008 presidential election, share of Hispanics in poverty, the unemployment rate, crime rate, share black ACA sign-up, 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 reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Table A7: 2SLS Results – ACA Take-Up Robustness

<i>Estimate of Non-Citizens</i>	<i>Baseline</i>	<i>No Undercount</i>	<i>1990 Census</i>
	(1)	(2)	(3)
Share Hispanic	-0.200**	-0.256**	-0.378*
Detainers	(0.089)	(0.117)	(0.209)
F-Statistic	12.61	10.06	5.40
Fixed Effects		State	
Controls	Baseline, Sanc, Pov, Pol, UR, Crime, BI ACA		
Observations	2,044	2,044	2,044

Note: Note: The dependent variable is the share of eligible Hispanics that sign up for the ACA. Column 1 calculates our instrument using our preferred denominator which corrects for undercounting of Hispanic foreign born using National Center for Health Statistics birth data. Column 2 calculates our instrument using a denominator that does not correct for undercounting. Column 3 calculates our instrument using a denominator that uses the fraction of non-citizen Hispanics from the 1990 Census multiplied by the total number of foreign-born Hispanics. All regressions control for state fixed effects, share Hispanic males, a sanctuary city indicator, the Democratic versus Republican vote margin in the 2008 presidential election, share of Hispanics in poverty, the unemployment rate, crime rate, share black ACA sign-up, 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 reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Table A8: OLS and 2SLS Results – ACA Take-Up Placebo

<i>Outcome</i>	<i>Share Black ACA</i>		<i>Share White ACA</i>	
	<i>OLS</i>	<i>2SLS</i>	<i>OLS</i>	<i>2SLS</i>
	(1)	(2)	(3)	(4)
Share Hispanic	-0.018**	-0.064	-0.011*	-0.056
Detainers	(0.007)	(0.053)	(0.006)	(0.037)
F-Statistic		108.13		26.63
Fixed Effects		State		
Controls	Baseline, Sanc, Pov, Pol, UR, Crime, Hisp ACA			
Observations	1,965	1,965	2,044	2,044

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 Hispanic males, a sanctuary city indicator, the Democratic versus Republican vote margin in the 2008 presidential election, share of Hispanics in poverty, the unemployment rate, crime rate, share Hispanic ACA sign-up, 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 reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Figure A1: California SNAP Application

**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
<input type="checkbox"/> Yes <input type="checkbox"/> No		SELF			<input type="checkbox"/> Yes <input type="checkbox"/> No	
<input type="checkbox"/> Yes <input type="checkbox"/> No					<input type="checkbox"/> Yes <input type="checkbox"/> No	
<input type="checkbox"/> Yes <input type="checkbox"/> No					<input type="checkbox"/> Yes <input type="checkbox"/> No	
<input type="checkbox"/> Yes <input type="checkbox"/> No					<input type="checkbox"/> Yes <input type="checkbox"/> No	
<input type="checkbox"/> Yes <input type="checkbox"/> No					<input type="checkbox"/> Yes <input type="checkbox"/> 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: _____	<input type="checkbox"/> Yes <input type="checkbox"/> No
		DOCUMENT TYPE: _____ DOCUMENT NUMBER: _____	<input type="checkbox"/> Yes <input type="checkbox"/> No
		DOCUMENT TYPE: _____ DOCUMENT NUMBER: _____	<input type="checkbox"/> Yes <input type="checkbox"/> No

Does anyone listed above have at least 10 years (40 quarters) of work history or military service in the USA? If yes, who? \_\_\_\_\_ (PLEASE CHECK ONE)  Yes  No

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? If yes, who? \_\_\_\_\_  Yes  No

**6c. SPONSORED NONCITIZEN INFORMATION** - Complete for those listed in question 6b above who are sponsored noncitizens and are applying for aid.

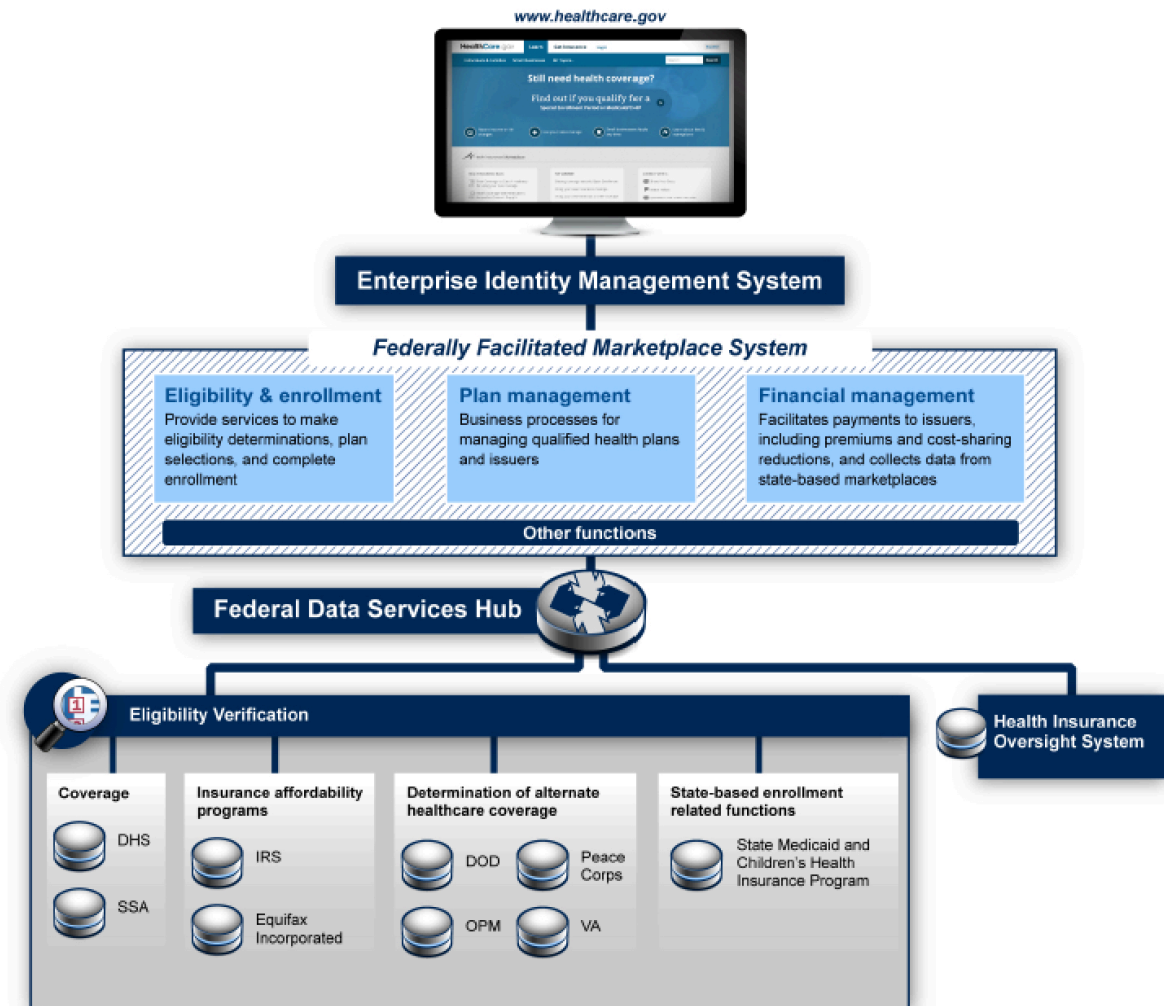
Did the sponsor sign an I-864?  Yes  No If yes, please answer the rest of the question. If the sponsor signed an I-134 then skip this question.

Does the sponsor regularly help with money?  Yes  No If yes, how much? \$ \_\_\_\_\_

Note: Data from section of California SNAP Application.



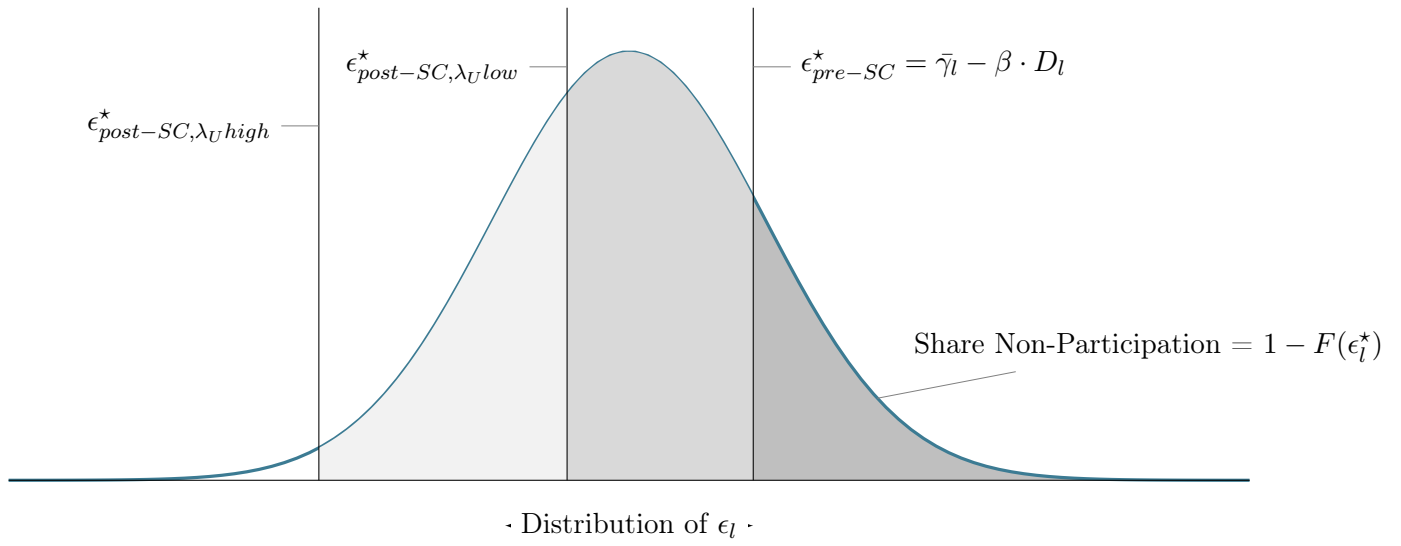
Appendix Figure A3: ACA and DHS Immigration Status Verification



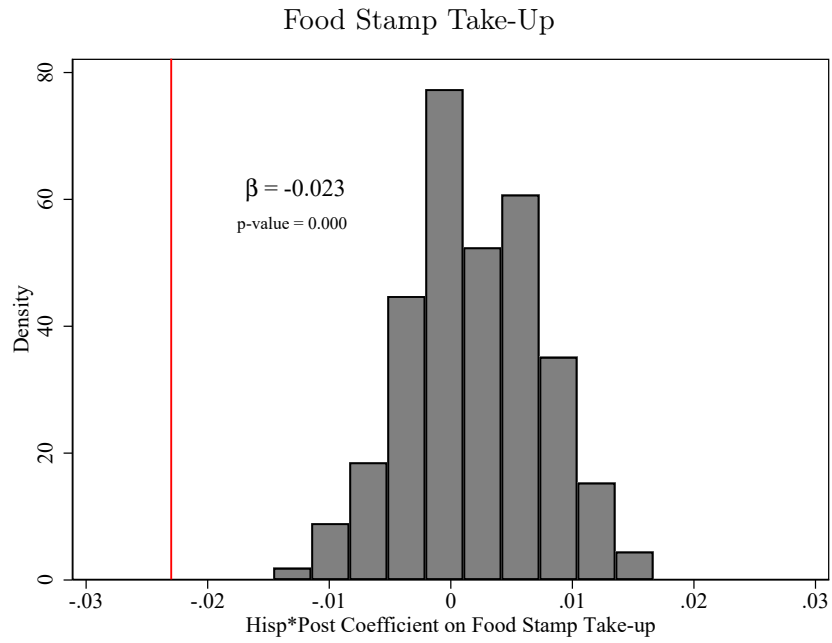
Note: Data from section of GAO Report to Congressional Requesters on Healthcare.gov.



Appendix Figure A4

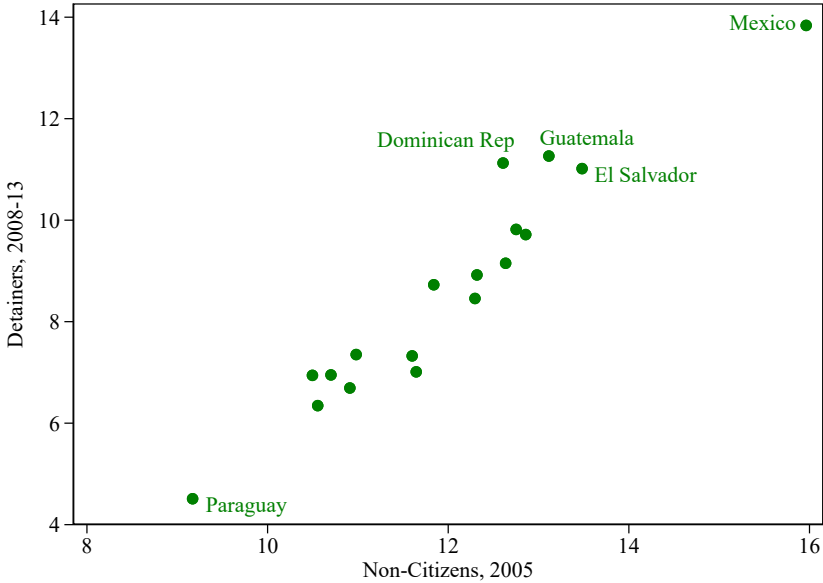


## Appendix Figure A5: Permutation Tests



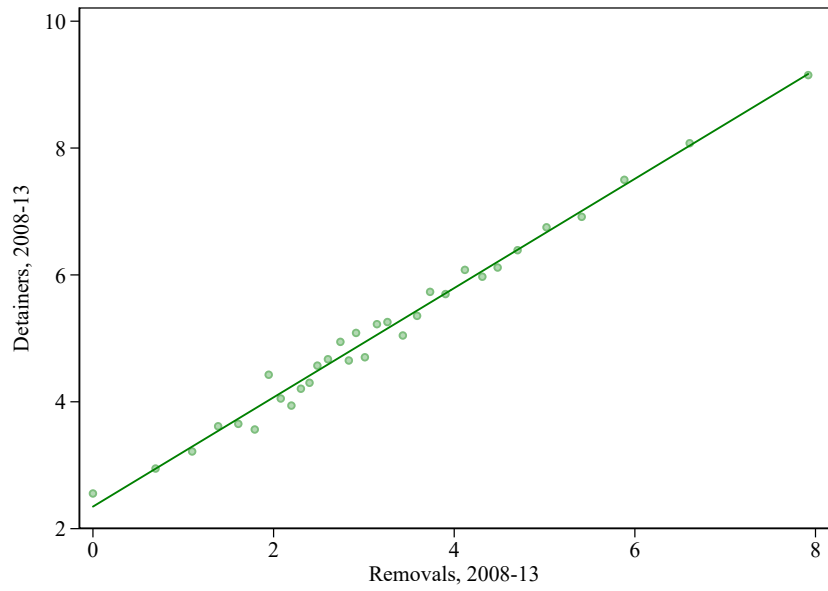
Note: Data from ACS. These figures represent empirical distributions of our estimate of interest when we randomly permute activation years to each county. The red line denotes our actual coefficient along with the corresponding two-sided empirical p-value. The data are limited to actual SC pre-activation years.

Appendix Figure A6: Correlation between Detainers and Non-Citizen Population



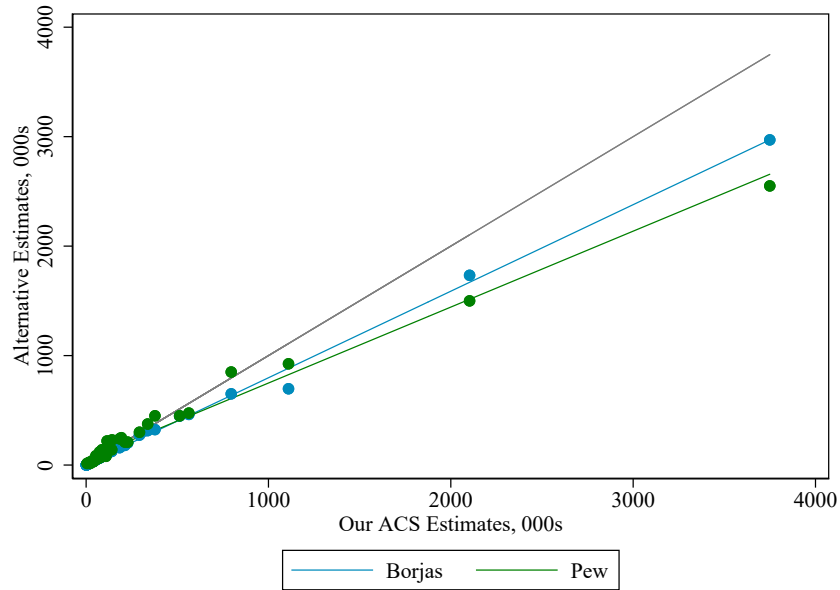
Note: Data from ACS and FOIA. This figure presents the correlation between log detainers and log estimated number of non-citizens for each Hispanic country of origin. The correlation between the measures is 0.96.

Appendix Figure A7: Correlation between Detainers and Removals



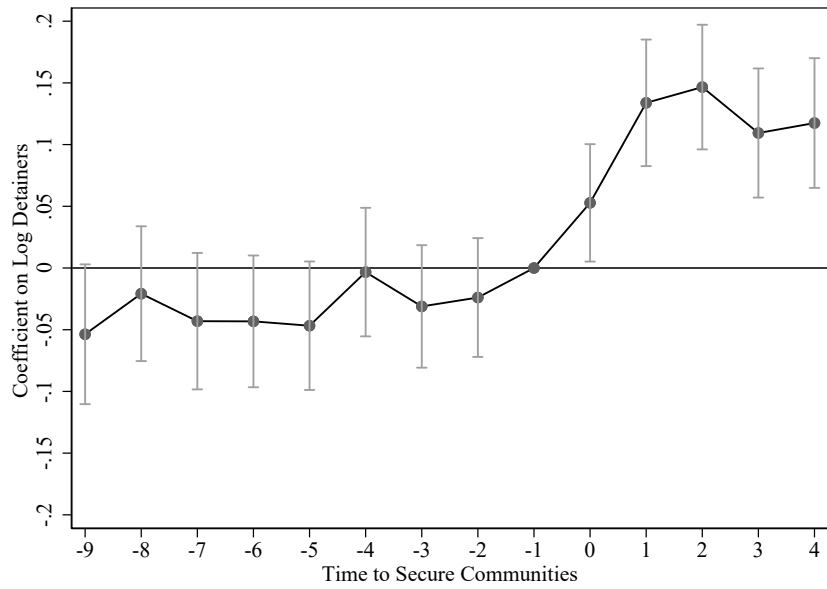
Note: Data from FOIA. This figure presents the correlation between log detainers and log removals for each county. The correlation between the measures is 0.84.

Appendix Figure A8: Estimating Non-Citizen Hispanics



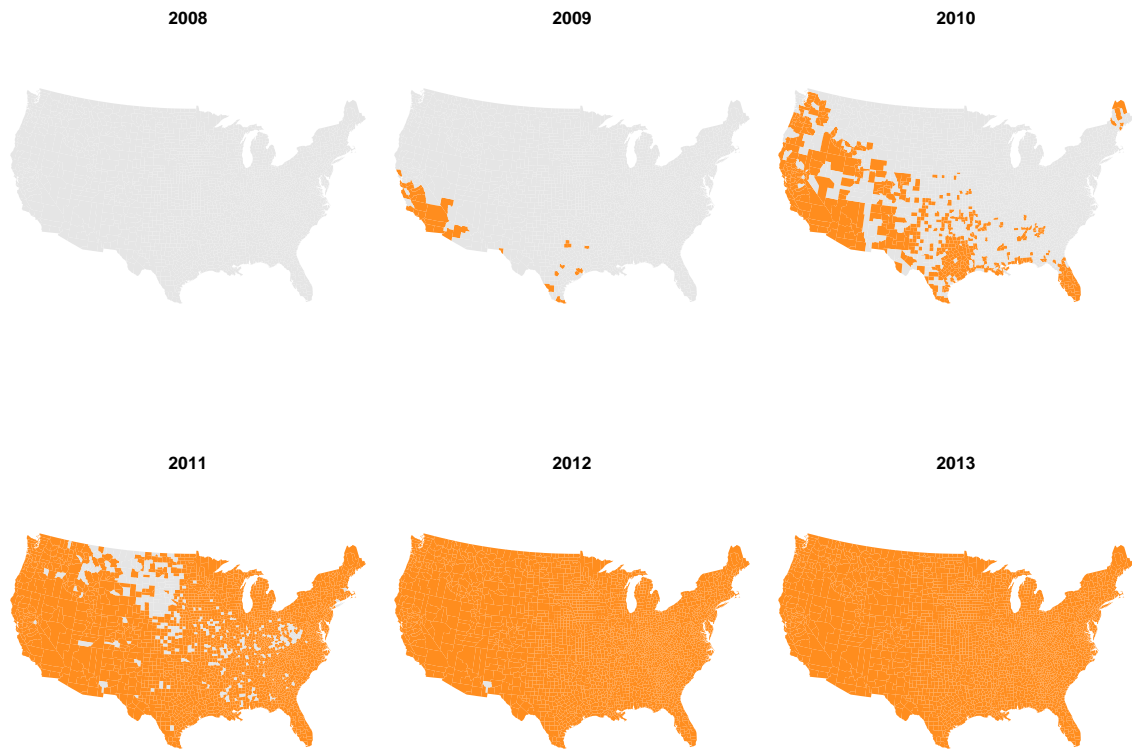
Note: Data from ACS and Pew Research Center. This figure presents the correlation between our state-level estimates on the number of non-citizen Hispanics and estimates on the number of unauthorized Hispanics from the Pew Research Center (green line) and from the method proposed by Borjas (2017) (blue line). The correlation between the measures is greater than 0.95.

Appendix Figure A9: Detainers Event Study



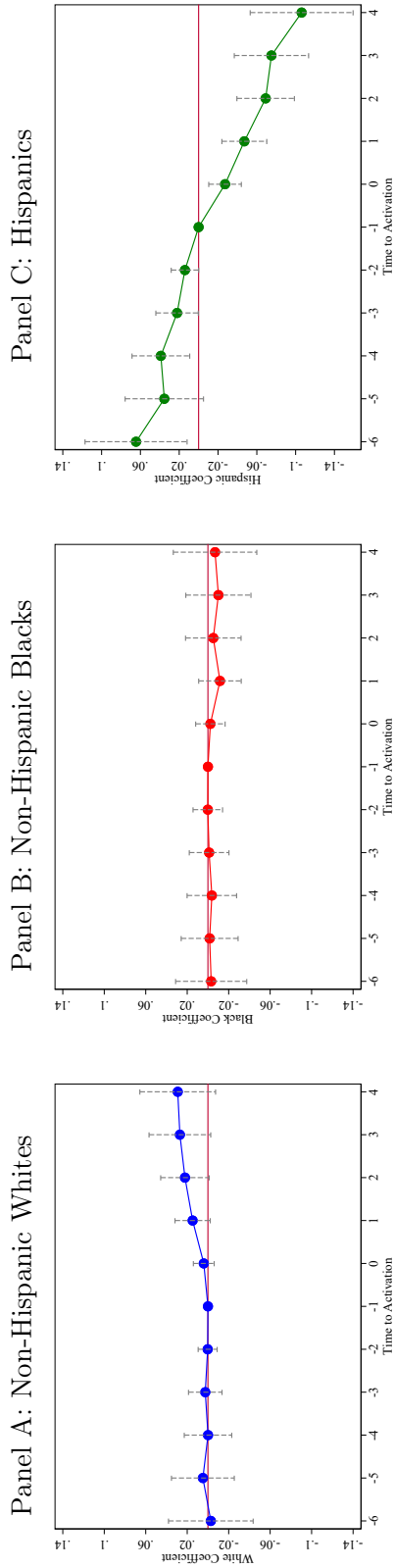
Note: Data from FOIA. This figure represent event study estimates of the time to SC activation in months on the log number of detainers issued. All specifications control for county fixed effects. Standard errors are clustered at the county level.

Appendix Figure A10: Predicted Secure Communities Activation



Note: Data from FOIA and ICE documentation.

Appendix Figure A11: Robustness Event Study of Food Stamp Take-Up



Note: Data from ACS from 2006–2016. Coefficients and 95% confidence intervals are plotted. The data are limited to heads of households with less than a high school degree, 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 log poverty, an indicator for average family size greater than one, employment rate, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, state-by-year fixed effects, and race-by-year fixed effects (which compels us to drop race indicators interacted with the timing of the Great Recession). Observations in the ACS are weighted by the race-specific population in each county. Robust standard errors are clustered at the county level.