Fear and the Safety Net: Evidence from Secure Communities*

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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 SSI 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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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).[1] 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).[2] Moreover, the gap between take-up by eligible Hispanics versus other groups has widened in recent years. For example, between 2005 and 2013, the share of Hispanics taking up the Supplemental Nutrition Assistance Program (SNAP) slowed by 12.6 percentage points relative to other groups despite increasing food insecurity for Hispanic households (Nord, Andrews, and Carlson 2006; Coleman-Jensen, Gregory, and Singh 2014), and the share of Hispanics taking up Supplemental Security Income (SSI) slowed by 14.8 percentage points relative to non-Hispanics.[3]

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.[4] This finding echoes other qualitative evidence suggesting Hispanic citizens, themselves immune to deportation, nevertheless fear that enrollment may reveal personal information on non-citizens in their networks

[1] For evidence on the health and human capital returns, see Almond et al. (2011); Hoynes, Schanzenbach, and Almond (2016); Bronchetti, Christensen, and Hoynes (2018); Almada and Tchernis (2016); East (2017); Aizer et al. (2016); Goodman-Bacon (2018).

[2] 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).

[3] Authors' own calculations 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).

[4] Data from 2017 UCLA Luskin Los Angeles Quality of Life Index Survey.

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."[5] 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.[6]

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.[7] 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 detainers ("immigration holds") issued under SC between 2008 and 2013. These data contain information on the county of issue, crime severity, and country of origin of each arrested individual.

[5] See https://www.pbs.org/newshour/health/hispanic-americans-still-arent-signing-obamacare.

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

[7] By federal law, any non-citizen can be deported, including unauthorized individuals and green card holders.

We combine these data with information on the take-up of the Supplemental Nutrition Assistance Program (SNAP), otherwise known as food stamps, and take-up of Supplemental Security Income (SSI). Information on take-up comes from public-use data from the American Community Survey (ACS) and restricted-use data from the Panel Study of Income Dynamics (PSID). 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 SSI 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. For both safety net outcomes, we examine behavioral responses among a sample of economically fragile households that are connected with non-citizens, defined as those in which the head of household earned less than a high school degree.[8]

We estimate the impact of SC on program take-up by leveraging the staggered roll-out of SC across counties together with the fact that Hispanic households were differentially affected. This latter variation is appropriate since well over 90 percent of detainers issued under the SC program were for Hispanics. We use a triple-differences framework, interacting race and ethnicity indicators with timing of SC activation. In doing so, we compare program 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 conditional on the aforementioned fixed effects.

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 Hispanic-headed households in the ACS before activation was 22 percentage points, implying a 10 percent decline in take-up due to SC activation. Hispanic households are also 1.0 percentage points less likely to take-up SSI after SC activation, a 19 percent decline relative to the pre-SC Hispanic mean. We obtain qualitatively similar results when using the PSID, finding a decline in both Hispanic food stamp and SSI take-up.

A number of findings suggest these results are indeed causal. First, consistent with the parallel trends assumption under our triple-differences approach, we find no sharp changes in the evolution of our outcome variables prior to SC activation. Second, our preferred specification includes a full set of race-by-state fixed effects to address the potential concern that states may vary in policies towards minority groups, 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), and race-by-year fixed effects along with state and group-specific employment changes during the Great Recession to allow for flexible impacts of the Great Recession across

[8] We find similar though smaller effects for households with a high school degree and those with some college.

demographic groups (Kochhar, Fry, and Taylor 2011; McKernan et al. 2014). Third, we show SC only affected Hispanic Americans – results on program take-up for non-Hispanic blacks or whites are often oppositely-signed and not statistically significant, consistent with the notion that the SC program should 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 six 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 communities with a higher share of non-citizens or mixed-status Hispanic-headed households.[9] We find that reductions in safety net take-up are largest among said locations. Second, we find that places where detainers are more commonly issued against Hispanics for non-violent (e.g. often misdemeanor) 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. Fifth, locations with a higher share of Puerto Ricans and Cubans exhibit much smaller responses to SC activation, consistent with the fact that Puerto Ricans and Cubans face zero to minimal risk of deportation relative to Hispanics from other countries of origin. 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 or SSI 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 or SSI 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, 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.

[9] Mixed-status households include members that have different citizenship or immigration statuses.

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.[10] 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 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. In survey work, Pedraza and Osorio (2017) find that when Latino respondents are presented with a cue regarding immigration, they report being less willing to engage in public services such as health care and that immigration enforcement has led to increased distrust in health-related information from the government (Cruz Nichols, LeBrón, and Pedraza 2018). 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.[11] 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

[10] 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 obtaining legal permanent resident status (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).

[11] 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).

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.[12] 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 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,

[12] Additional institutional details on the program and its implementation can be found in the Online Appendix.

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.[13]

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."[14]

[13] See http://www.washingtonpost.com/wp-dyn/content/article/2010/09/30/AR2010093007268.html

[14] The specific policies can vary widely, from prohibiting police officers inquiring about a person's immigra-

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.

III. Safety Net Programs

In this study, we focus on participation in SNAP, also known as food stamps, and SSI, 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). SSI participation has also grown substantially over time, rising from 4.6 million beneficiaries in 1989 to 8.4 million in 2013, with the federal government paying $54 billion in SSI cash benefits in 2013 (Daly and Burkhauser 2003; Duggan, Kearney, and Rennane 2015). 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 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).

tion 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.

In order to receive benefits under SNAP, individuals need to meet various federal guidelines.[15] 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).[16] 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.

Almost all states assure applicants their information will only be used to determine eligibility and will not be shared with ICE for immigration enforcement.[17] 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."[18] 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."[19]

SSI: Supplemental Security Income (SSI), administered by the Social Security Administration (SSA), is the largest cash welfare program in the United States (Daly and Burkhauser 2003, Deshpande 2016). In addition to federal assistance, which provided a maximum for an individual of $733 per month in 2016, almost one-third of states supplement the federal benefit with state SSI benefits (Duggan et al. 2015). SSI provides benefits to blind or disabled children, blind or disabled working-age adults, and individuals 65 or older with no requirement of disability, although its caseload is

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

[16] 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.

[17] 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.

[18] See https://fns-prod.azureedge.net/sites/default/files/snap/Non-Citizen_Guidance_063011.pdf.

[19] 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.

increasingly dominated by disabled children and non-elderly adults. Between 1988 and 2013, total federal benefits paid for SSI disabled children and non-elderly adults nearly tripled, increasing from $14.6 billion to $44.4 billion dollars in 2013 (Duggan et al. 2015). This program is aimed at providing an income floor to individuals who are ineligible for Social Security or whose benefits could not provide a basic living. In fact, for nearly three-fifths of recipients, SSI represents their only source of income.[20] Child participation in SSI has been linked with significant and persistent reductions in child poverty rates (Duggan and Kearney 2007).

To be eligible for SSI, individuals must be aged or disabled and meet federally mandated income and asset limits. In general, individuals with "countable income" over the federal benefit rate (FBR) are not eligible for SSI. Individuals must also meet asset limits and have no more than $2,000 in assets or $3,000 for a couple. Like SNAP, only U.S. citizens, U.S. nationals, or "qualified aliens" are eligible for SSI. Unauthorized individuals are not eligible for SSI.

Upon meeting the above requirements, individuals must also meet one of three categorial criteria: age, disability or blindness. Elderly individuals are automatically eligible for SSI based on age if they are age sixty-five or older. Individuals may receive SSI benefits for blindness if they have 20/200 vision or less with the use of a correcting lens in their better eye, or if they have tunnel vision of 20 degrees or less.

Non-elderly adults with a disability typically apply for SSI through an SSA field office, where SSA employees determine both income/asset eligibility and disability. State disability examiners, working with vocational and medical consultants, conduct medical determinations. To meet the disability determination, a non-elderly adult must demonstrate an inability to engage in substantial gainful activity by reason of a medically determinable physical or mental impairment that is expected to result in death or last at least twelve months. The process for disability determination for children similarly requires that a child has a disability lasting at least twelve months or expected to result in death. Applicants who are initially rejected may appeal the decision.

Like with SNAP, immigration enforcement may affect take-up because the SSI application asks for the names, birthdates, and Social Security numbers of all persons living with the applicant. This portion of the SSI application is provided in Appendix Figure A2. Questions about household composition are taken into account to determine the contribution of the applicant to household expenses such as rent and utilities and can affect the amount of the SSI federal benefit.[21] Anecdotally, being asked to provide this type of information has invoked fear among certain communities given that federal benefits-granting agencies, in particular SSA for the administration of SSI, are required to report to DHS individuals who have undergone a formal determination on a claim who the agencies know are not lawfully present in the United States.[22] However, according to the National Immigration Law Center, these agencies have clarified that this reporting requirement is only triggered for

[20] See SSA, SSI Annual Statistical Report, 2012, July 2013, Highlights 2012.

[21] See https://www.ssa.gov/ssi/spotlights/spot-living-arrangements.htm.

[22] See Personal Responsibility and Work Opportunity Reconciliation Act of 1996, Public Law 104-193, §404. SSA also routinely honors requests to share social security number information with DHS and ICE under an information sharing requirement in the Immigration and Nationality Act (8 U.S.C. §1360(b)). See https://secure.ssa.gov/poms.nsf/lnx/0203313095.

individuals seeking benefits and not relatives or household members.[23]

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. We allow participation decisions to depend on the the citizenship status of the head of household given a large literature that finds that decisions within a multiple-person household cannot be assumed to be stable and transitive (Browning, Chiappori, and Lechene 2006). In our main model, the head of household is a citizen to capture indirect treatment effects. The Online Appendix models the case where the head of household is a non-citizen, capturing the direct treatment effect of fear.

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:

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

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),[24] 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.[25] π_{jl} is the subjective probability of deportation (i.e. fear) and is an increasing function of program participation, p_{ij}.

In this utility function, λ_i, λ_c, and λ_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:

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

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

[23] See https://www.nilc.org/issues/economic-support/overview-immeligfedprograms/.

[24] If the household is mixed-status, then whomever has citizenship likely has the higher threat point and will be the decision-maker.

[25] 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.

benefit $\frac{B_j}{C}$. Equation 1 nests both direct and indirect treatment effects.

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 $\frac{\partial p}{\partial \lambda_C}>0$.[26] 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 ϵ_{jl} is an error term that is distributed $\epsilon \sim F(.)$. Thus, household j will participate in the federal safety net program if and only if:

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

Let $\frac{(\lambda_i+\lambda_c)\cdot(\frac{B_j}{C})}{\lambda_n} = \gamma_j$, where $\gamma \sim G(.)$. Within each location l, let the average γ_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 (λ_c and λ_i). In contrast, the non-participation share is increasing in the weight assigned to non-citizens (λ_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 networks. Appendix Figure A3 graphically illustrates how the non-participation share is affected by immigration enforcement and connections to non-citizens.

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

V. Data and Empirical Methodology

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

A. 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 micro-level data on the universe of detainers issued by ICE from 2002 to 2015 in the United States. The detailed information includes the reason for the arrest as well as the crime level/severity, the date 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 detainers 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 1 presents the total number of detainers issued per year and Panel B presents the cumulative number of detainers issued over the time period. The rapid ramp up in SC is evident in the time immediately following SC's launch in 2008. These figures also demonstrate that the overwhelming majority (93 percent) of detainers are issued against Hispanic individuals. Panel C presents the ratio of detainers for low-level offenses (e.g. misdemeanor offenses) versus serious, violent offenses and shows that, over time, SC issued a growing share of detainers for low-level arrests. While not all detainers are honored by local law enforcement agencies or lead to removal from the country, there is a strong positive correlation between detainers and removals under SC. See Appendix Figure A4.

SC represented a massive increase in immigration enforcement. Appendix Table A1 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. In contrast, we find significant increases in the number of fingerprint submissions received by ICE, fingerprint matches, and detainers issued post-SC activation. Event study estimates of the impact of SC on detainers issued are presented in Appendix Figure A5, which shows a sharp 15 percent increase in the number of detainers 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-USA at the University of Minnesota.[27] We focus on the one percent ACS samples of the U.S. population over the years 2006–2016 for food stamp and SSI take-up. The data include household

[27] 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.

characteristics such as food stamp and SSI 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 fragile and connected sample of households. 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.[28]

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 and SSI 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 disability. 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 2,393 unique household heads from 661 counties.

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:

$$G(i,d) = \left[100 \cdot \frac{share(i,d)}{max_{\delta}\{share(i,\delta)\}} \mathbb{1}\left[\#(i,d) > T\right]\right] \tag{3}$$

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 δ. 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

[28] 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. Appendix Table A2 column (6) repeats the analysis at the PUMA-level.

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.

B. Empirical Framework

Our triple-differences methodology 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:[29]

$$\begin{aligned} Y_{rcst} = \alpha + \beta_1 I_{ct}^{post} + \beta_2 (I_r^H \cdot I_{ct}^{post}) + \beta_3 (I_r^B \cdot I_{ct}^{post}) + \Omega' X_{rcst} + \mu_c \cdot I_t^{GR} + \delta_{st} + \theta_{rs} + \kappa_{rt} \\ + \Gamma_1' X_{cst} + \Gamma_2' (X_{cst} \cdot I_r^B) + \Gamma_3' (X_{cst} \cdot I_r^H) + \epsilon_{rcst} \end{aligned} \tag{4}$$

where r is race/ethnicity, c is county, s is state, and t is year. Y_{rcst} is the outcome of interest. Y_{rcst} is the share food stamp or SSI take-up among our sample. 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), as well as the state of Arizona, which enacted various local-level immigration initiatives during the time period (see Bohn, Lofstrom, and Raphael 2013). We require counties to have at least one known offense per year, since detainers under SC could not have been issued otherwise, and to have at least one respondent from each race group. Our final sample includes 2,919 unique counties.

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, number of children, and the employment rate that vary across race, county, and time. We control for these characteristics as they are direct determinants of food stamp or SSI eligibility.

The Great Recession differentially impacted minority-headed households. For instance, white

[29] We write the equation at the county-level using the ACS data but note the differences for the PSID.

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). These differential wealth effects may have affected food stamp and SSI take-up by race and ethnicity (Flores-Lagunes et al. 2018). We account for this possibility by explicitly including race/ethnicity-specific state-level employment changes during the Great Recession. Furthermore, we include county fixed effects (μ_c) interacted with an indicator for post-Great Recession (I_t^{GR}) to capture unobserved county-level factors that affect take-up differentially before and after the Recession.

State-by-year fixed effects (δ_{st}) are included in our preferred specification to capture 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 account for differential state-level effects of federal immigration reforms. We include state-by-race/ethnicity fixed effects (θ_{rs}) to control for attitudes and policies in each state that differentially affect minority groups. Race-by-year fixed effects (κ_{rt}) non-parametrically capture yearly shocks that differentially affect different racial groups, such as changes in economic conditions.

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. Crime statistics are generally not available at the race-county-year level but crime disproportionately impacts minorities communities (Sampson and Lauritsen 1997, Anwar and Fang 2006, Antonovics and Knight 2009). To allow for these differences, we interact race/ethnicity indicators with the log number of offenses known to law enforcement (Kochhar, Fry, and Taylor 2011; McKernan et al. 2014). Our specification for the PSID is similar to Equation 4. The outcome is an indicator for take-up of food stamps or SSI by individuals in 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 log poverty in the past year.

For the ACS data, we weight all regressions by the total population in the relevant race-county cell.[30] To obtain a similar population-weighted estimate 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 – an economically fragile and connected sample of households in terms of participation in safety net programs and connectedness to non-citizens. 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.[31] The coefficient of interest in Equation 4 is β_2, which estimates the impact of SC

[30] On footnote 17 of pg. 312, Solon, Haider, and Wooldridge (2015) clarify that this weighting will only perfectly identify a population average partial treatment effect when the model is fully saturated.

[31] 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

activation on outcomes of Hispanic households relative to non-Hispanic white households, compared to counties that have not yet activated. β_3 serves as a placebo test, capturing the effect of SC on black households relative to non-Hispanic white households in counties that have activated versus those that have not yet activated.

In addition to our baseline specification in Equation 4, 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:

$$\begin{aligned} Y_{rcst} = \alpha + \sum_{n \neq -1} \beta_1^n (I_{c,t=n}) + \sum_{n \neq -1} \beta_2^n (I_r^H \cdot I_{c,t=n}) + \sum_{n \neq -1} \beta_3^n (I_r^B \cdot I_{c,t=n}) + \Omega' X_{rcst} \\ + \mu_c \cdot I_t^{GR} + \delta_{st} + \theta_{rs} + \kappa_{rt} + \Gamma_1' X_{cst} + \Gamma_2' (X_{cst} \cdot I_r^B) + \Gamma_3' (X_{cst} \cdot I_r^H) + \epsilon_{rcst}. \end{aligned} \tag{5}$$

In this specification, $I_{c,t=n}$ is in indicator for each period (other than the year prior to activation $t = -1$), such that the β_2^n coefficients trace the take-up of food stamps for Hispanics in the years before and after SC activation relative to non-Hispanic whites.[32] Similarly, each β_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 identification assumption underlying our triple-differences approach is that there are no contemporaneous shocks associated with the activation of SC within a county that only affect Hispanic households relative to white households, conditional on black-white differences. In other words, we assume that any differences in our outcome variables of interest for Hispanic versus white households would have evolved smoothly absent SC activation, conditional on our set of fixed effects and controls.

To assess this assumption, we test for pre-SC balance in our covariates and outcomes of interest between Hispanics and non-Hispanic whites across SC-activation groups in the spirit of our main specification in Equation 4. Specifically, we regress the mean Hispanic-white difference for each dependent variable on fixed effects for each activation year group, state fixed effects, log crime, and the respective black-white difference. Standards errors are clustered at the county level. In column 1 of Table 1, we present F-statistics from a test of the joint significance of the activation year group fixed effects. Corresponding p-values from these joint F-test are presented in column 2 of Table 1. In terms of mean level differences, we find minimal evidence of significant differences across each activation group. Most importantly, we find that there are no significant differences in *changes* in Hispanic-white outcomes in the pre-SC period across each activation group. These results support the assertion that the roll-out of SC was not correlated with pre-trends in the differential take-up

(Amuedo-Dorantes and Arenas-Arroyo 2017), which may affect eligibility and take-up for safety net programs.

[32] Leads before six years and lags after four years are coded with the first and last groups, respectively, following McCrary (2007) among others.

of safety net programs across groups.[33]

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 4, repeating this procedure 500 times. In Appendix Figure A6, we present the empirical distribution of these placebo effects for β_2, finding that our actual treatment effects are larger (in absolute value) than the vast majority of our placebo estimates. These results suggest that SC activation had a 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 β_2 in our main specification (Equation 4). 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 β_2 in our main specification.

To further understand the timing of SC activation, Figure 2 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 and Arizona from our analysis since they might be unique in several ways related to both immigration enforcement and program participation and include county interacted with Great Recession 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

[33] Goodman-Bacon (2018) suggests an alternative balance test using novel weights when there are many timing groups and the F-test is underpowered, conditions which are unlikely to hold in our context.

of a 287(g) agreement. We use these criteria and their high-level interactions in a cross-section to predict activation year. Figure 3 presents maps that show the timing of predicted SC activation across counties.

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).[34]

VI. Results

Figure 4 presents our main event study estimates of SC activation on food stamp take-up (Panel A) and SSI take-up (Panel B) for non-Hispanic whites, non-Hispanic blacks, and Hispanics using the ACS data, as described in Equation 5. This specification is limited to our less than high school degree 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 of either safety net program 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 both food stamps and SSI over time. Specifically, by five years post-activation, Hispanic households reduce take-up of food stamps by 9.0 percentage points relative to non-Hispanic whites, a 41 percent decrease from the pre-period Hispanic mean of 22.2 percent. Similarly, by five years post-activation, Hispanic households reduce their take-up of SSI by 5.8 percentage points relative to non-Hispanic whites, a 109 percent decrease from the pre-period Hispanic mean of 5.3 percent. Similar event studies comparing Hispanics to all non-Hispanics (both white and black) are presented in Appendix Figure A7.

Table 2 presents our main results on safety net take-up in the ACS data. Columns 1 and 2 present results for food stamp take-up and columns 3 and 4 report results for SSI take-up. In column 1, 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 10 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 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 find that Hispanic citizen heads of household reduce their take-up of SSI by 1.0 percentage points after SC activation relative to non-Hispanics, a 19 percent decrease from the pre-period Hispanic mean. Again, these results remain stable with the inclusion of an

[34] In unreported results, we also follow a different, but related approach in 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.

interaction between our black and post-SC indicator, which is small and statistically insignificant (column 4).

Our main findings are also qualitatively similar using the PSID data, shown in Table 3. Columns 1 and 2 present results for food stamp take-up and columns 3 and 4 present analogous results for SSI take-up. In column 1, we find that after SC activation, Hispanic citizen households reduce their take-up of food stamps by 19.8 percentage points relative to non-Hispanics, a 54 percent decrease from the pre-period Hispanic mean of 36.9 percent. In column 2, we take advantage of the panel nature of the PSID data and find that post-activation, Hispanic citizen households that previously took up food stamps prior to SC activation ("prior users") also reduced their take-up, decreasing participation by 45.2 percentage points relative to non-Hispanics, a 64 percent decrease from the pre-period mean. We also find reductions in Hispanic citizen households take-up of SSI after SC activation relative to non-Hispanics. In our full sample, Hispanic households reduce take-up by 5.5 percentage points after SC (column 3) although this point estimate is not statistically significant and among a sample of prior users, Hispanic households reduce take-up of SSI by 83.8 percentage points relative to non-Hispanics after SC activation (column 4). We return to these results on prior users in Section VII.B when we explore potential mechanisms explaining our results.[35] Notably, across all our specifications and samples in Table 2 and Table 3, we find evidence not only of differential decreases in food stamp and SSI take-up for Hispanics, but absolute decreases for Hispanic households following SC.

Table 4 presents several robustness checks of our main results for both food stamp take-up (Panel A) and SSI take-up (Panel B). Column 1 presents results controlling for a full set of county-by-year fixed effects with corresponding event study estimates shown in Figure 5. Column 2 presents results using predicted activation year rather than actual activation year. Column 3 presents results from our main specification where for treated counties that activate in a particular year, we define control counties as those that activate more than two years in the future, following Deshpande and Li (2017). Column 4 presents results controlling for pre-activation trends in program take-up following the approach in Freyaldenhoven, Hansen, and Shapiro (2018). Column 5 presents results on a sample of citizen household heads with a high school degree or less. Across all alternative specifications and samples, we continue to find economically meaningful and statistically significant reductions in safety net take-up for Hispanics households relative to non-Hispanics after SC activation.

Additional robustness checks are presented in Appendix Table A2. In column 1, we estimate our main results in the ACS on a sample of counties that matches the PSID in terms of pre-period take-up rates for Hispanics, finding that our main ACS estimates from Table 2 are much more similar in magnitude to our PSID estimates from Table 3 in this matched sample. In column 2, we consider the fact that food stamp participation may be decided by females within a household. We

[35] 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 661 counties versus 3,079 in the ACS and differentially covers large states like California and Texas, whereas the ACS is nationally representative. Second, as discussed earlier, the PSID specifically added a large wave of immigrant families to the survey in 1997 and thus likely over-samples immigrant families relative to the ACS. 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 A2.)

find very similar results using a sample of citizen female heads of household or female spouses. We also find similar results when we exclude individuals or families that face any risk of deportation, such as naturalized citizen heads of household, who in theory could be deportable under some circumstances (column 3), and when we exclude citizen heads of households with mixed-status family members (column 4). These results suggest that our main findings capture a true spillover effect of deportation fear. In column 5, we find that our results are qualitatively similar to dropping New York, Los Angeles, Miami, Houston, and Chicago from our sample, cities that have the highest number of Hispanic immigrants.

In column 6 of Appendix Table A2, 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, suggesting that Hispanic households are most responsive to enforcement within their own county.[36] In column 7, we estimate our results at the PUMA rather than county-level, finding similar results. Results are also robust to comparisons between Hispanics and all non-Hispanics, where non-Hispanic whites and blacks are aggregated into a single comparison group (column 8). Finally, column 9 presents results for a sample of citizen households where the head has some college or more, a sample that has much lower participation in safety net programs and lower connectedness to non-citizens compared to our preferred sample. We find that our estimates are much smaller in magnitude in this alternative sample, suggesting that the effect of SC on program take-up is most concentrated among our fragile connected sample of heads with less than a high school degree.

Finally, Appendix Table A3 explores alternative weighting schemes and alternative controls. For example, our results are qualitatively similar when we do not use population weights (columns 1 and 2). Our results are also robust to 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 (column 3). Our results are also very similar controlling for the share of county that is Hispanic (column 4) and for the number of Hispanic non-citizens in each county (column 5), addressing concerns that our results are driven by flows of program-eligible Hispanics that are correlated with the timing of SC activation.

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.

[36] The coefficient on the spatial lag is small and not statistically significant.

A. Fear

SC increased the number of detainers 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).[37] 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 six 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 25 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, 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 second analysis, reported in columns 1 and 5 of Table 5, finds that the effect of SC on both food stamp and SSI take-up is larger in counties where the difference in the number of non-violent detainers (often issued for misdemeanor offenses) and violent detainers (often issued for assault or murder) relative to arrests is higher. In counties with a ten percent larger difference between non-violent and violent detainers, Hispanic households reduce their take-up of food stamps by an additional 0.01 percentage points and reduce their take-up of SSI by an additional 0.002 percentage points.

Third, 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 15 percentage point decline in food stamp take-up (column 2) and an 8 percentage point decline in SSI take-up among Hispanics after SC activation (column 6).

Fourth, 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

[37] 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.

should have less fear and thus exhibit a lower response to SC. In columns 3 and 7, 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 for food stamp take-up are driven by locations with no sanctuary policy. Specifically, we find a significant and *positive* effect of SC activation on Hispanics in sanctuary cities relative to non-sanctuary cities, such that SC activation has a net null effect on Hispanics in sanctuary jurisdictions.[38]

Fifth, we explore the idea that if fear is an explanatory factor, counties with a higher share of Hispanics from countries that face zero to minimal deportation risk should be less responsive to SC activation. In particular, we measure the share of Hispanics in a county that are Puerto Rican or Cuban, given that Puerto Ricans have citizenship status and thus cannot be deported, and Cubans are more likely to have political refugee status and thus face a lower risk of deportation.[39] In columns 4 and 8, we find that Hispanics in counties with a ten percent higher share of Puerto Ricans and Cubans experience significantly smaller reductions in both food stamp (0.2 percentage points) and SSI (0.3 percentage points) take-up.

Sixth, and finally, we explore 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 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.[40]

In Figure 8, we present a series of results on take-up based on the "intensity" of treatment. Compared to our main coefficient in our preferred specification, reductions in take-up are relatively larger in counties with a higher share of Hispanic households that are mixed-status. Specifically, Hispanic households from counties with a ten percentage point higher share of mixed-status households decrease take-up of food stamps and SSI by an additional 0.3 and 0.4 percentage points after SC activation, respectively. Take-up is further dampened in counties with a higher share of Hispanics that are non-citizens. Hispanic households from counties with a ten percentage point higher share of Hispanics who are non-citizens decrease take-up of food stamps by an additional 0.8 percentage points and decrease take-up of SSI by an additional 0.9 percentage points post-SC.

Finally, we use share mixed at the household level interacted with share non-citizen at the county level as a measure of exposure to relevant information. The higher the share non-citizen in a county, the easier information could spread regarding the heightened risk of deportation under SC (see Figure 6). The greater the share of mixed-status households in the area, the more salient that information is for take-up decisions. A ten percentage increase in the exposure index decreases food stamps by an additional 1.4 percentage points and SSI by an additional 1.3 percentage points after the activation of SC.

[38] See the Online Appendix for institutional details of sanctuary cities.

[39] We thank Ted Miguel and Thomas Lemieux for the suggestion.

[40] 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.

Taken together, these six findings suggest that fear of deportation is a likely explanatory mechanism.[41]

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.

Columns 2 and 4 of Table 3 presents these results in the PSID sample where we limit the sample exclusively to all individuals in households that have taken up food stamps or SSI prior to SC activation, respectively. In our PSID sample, 52 percent of Hispanic households are prior users of food stamps before SC activation and on average, prior users take up food stamps 71 percent of the time before SC activation. In the PSID, 8 percent of Hispanic households are prior users of SSI and take-up SSI 53 percent of the time on average before SC activation. We find that the decline in both food stamp and SSI take-up post SC is largely driven by Hispanic heads that have previously taken up these safety net programs. Among prior users, SC activation reduced Hispanic heads of household take-up of food stamps by 45.2 percentage points and take-up of SSI by 83.8 percentage points relative to non-Hispanics. 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 benefits, also lessens the likelihood that an explanation like stigma is driving our results.

[41] 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 smaller effects in areas with greater shares of Cubans and Puerto Ricans as described previously, findings that are hard to reconcile with a general theory of legitimacy.

C. Compositional Changes

We also consider the possibility that SC activation may have affected the number or types of Hispanic citizens and households 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 6 presents our main specification in the ACS, where the dependent variables are average race-specific observable characteristics of citizens/households in each county-year and the percent non-citizen and mixed-status among Hispanics 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 number of children, average family size, poverty level, or employment rate (columns 1–4). We also find no evidence of differential migration of Hispanics, measured by the share that report moving between states in the past year (column 5). Finally, we find no statistically significant change in the percent of Hispanic non-citizens or mixed-status families within a county post-SC activation (columns 6–7).

D. Measurement Error

There are several forms of measurement error that may enter into our analysis and could affect the interpretation of the results. To address error associated with apportioning ACS PUMA-level attributes to the county level, recall that Appendix Table A2 column 7 reruns our primary specification using the earliest activation date in the PUMA to define post-activation and clustering errors at the PUMA level. The estimates are very similar to those reported in Table 2 for both food stamps and SSI.

In addition to the error associated with PUMA-to-county apportionment, Meyer, Mittag, and Goerge (2018) emphasize that survey data often does not comport with administrative records on program participation, with false negatives and positives both possible, though the former is generally found to be much more important than the latter. Since our main specification relies on the difference between Hispanic and white-headed households, under-reporting will bias our estimates if it changes over time in relationship to SC for one particular group. A prominent explanation for false negatives among Hispanic households is fear related to immigration concerns (Brown 2015).

In our context, fear could lead to measurement error in the outcome variable that is correlated with the introduction of SC.[42] To the extent the implementation of SC heightens deportation fear,

[42] Assuming reported participation is less than true participation ($\widetilde{participation} = participation - fear$), then $\hat{\beta} = \beta - \frac{cov(SC, fear)}{var(SC)}$ where $cov(SC, fear)$ is likely positive.

our estimates therefore capture both the actual reduction in safety net take-up as well as an increase in false negatives. Although both effects are of interest, they have very different policy implications.

To gauge whether under-reporting responds to SC, we follow the literature and compare administrative SNAP data with survey estimates over time. Similar to other scholars, we find that ACS estimates of food stamp take-up are generally lower than available official yearly state-level estimates, yet the gap between survey and administrative take-up does not change post SC for Hispanics.[43] These findings suggest that changes in reporting behavior are unlikely to explain our main findings.

The last form of measurement error is that respondents might, in response to SC, be more inclined to lie about their true citizenship status, thus affecting our sample restriction. Under this scenario, our estimates would reflect both the spillover effects of fear as well as a reduction in fraudulent behavior. Yet, we failed to detect a significant change in the share of Hispanics identifying as non-citizens post-SC, as seen in Table 6 column 6, thus reducing the likelihood that shifting answers to the citizenship question induced by SC are driving our results.

E. Employment Responses

Finally, we consider the possibility that SC may have affected the labor market responses of Hispanic citizens, which in turn may affect eligibility and thus 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 ACS sample. Recall that in column 4 of Table 6, we find no differential effect of SC activation on the employment rate of Hispanics relative to non-Hispanic whites and blacks. In sum, these results suggest that our main findings are unlikely to be driven by employment responses to SC.[44]

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

[43] We also tested the triple difference in the gap between survey and administrative data for whites vs. Hispanics over time and found that there was no significant change post-SC. The data used are from a group of states that provide information disaggregated on SNAP participation by race (e.g. California, North Dakota, Oklahoma, Minnesota). Official disaggregated estimates of take-up of SSI by race/ethnicity are unavailable. See https://www.ssa.gov/policy/docs/rsnotes/rsn2016-01.html.

[44] 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.

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 SSI take-up among Hispanic households. We find evidence that our results may be driven by deportation fear rather than lack of benefit information or stigma. Hispanic citizens residing in areas with a higher degree of connectedness with non-citizens, 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 and areas with a higher share of Puerto Ricans and Cubans exhibited more muted responses to SC activation.

These findings are particularly relevant given recent immigration policies. For one, the reactivation of SC in 2017 has substantially increased deportations relative to prior years. In contrast to the operation of SC under the Obama administration, a larger share of deportations under the Trump administration result from arrests for misdemeanor and petty offenses, which may enhance deportation fear. Second, recently proposed changes to the "public charge" determination, a designation that can prevent a non-citizen from adjusting their immigration status to legal permanent resident, could further intensify the spillover effects of fear. In 2018, DHS proposed to substantially expand the definition of a public charge to include any immigrant who "uses or receives one or more public benefits," including both SSI and food stamps (previously exempt from public-charge determination). Moreover, the new proposal contemplates that use of these programs by U.S.-born citizen spouses and children could also count towards non-citizens' use of public assistance, with some estimates suggesting that up to one third of U.S.-born citizens could have their use of public benefits considered in the public-charge determination of a family member (Perreira et al. 2018). Our results suggest that these policy changes could lead to further decreases in sign-up of safety net programs as Hispanic citizens may fear that their participation could jeopardize the chances that a family member obtains legal permanent residency.

Ultimately, our results have several implications on health and well-being for Hispanic households. A back-of-the-envelope calculation based on number of recipients and average benefit size suggests that as a result of SC, Hispanic households forgo over $212 million and $77 million in food stamp and SSI benefits per year, respectively. 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, and Tiehen et al. (2012) find that food stamp participation reduced the child poverty rate by 5.6 percent from 2000 to 2009. Bronchetti et al. (2018) find that higher food stamp purchasing power increases the utilization of preventive medical care for children and reduces days of school missed due to illness. Similarly, Duggan and Kearney (2007) find that child participation in SSI is linked with long-term reductions in child poverty and Deshpande (2016) finds that removing disabled youth from SSI leads to a large decrease in observed lifetime income and exposes youth to greater income volatility. Schmidt, Shore-Sheppard, and Watson (2016) find that SSI program participation leads to a reduction in

family food insecurity. These results suggest that reductions in food stamp and SSI usage among Hispanics in response to immigration enforcement could have long-run consequences for health and economic security. 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: Balance Table
ACS Citizens Sample (2006-2008)

	F-Statistic (1)	p-value (2)
Outcome		
Log Poverty	1.494	0.189
Family Size	1.811	0.107
# Children	1.470	0.196
Employment Rate	2.923	0.012
Share Food Stamp	1.591	0.159
Share SSI	2.755	0.017
Δ Log Poverty	0.683	0.637
Δ Family Size	1.424	0.212
Δ # Children	1.832	0.104
Δ Employment Rate	2.560	0.026
Δ Share Food Stamp	0.789	0.558
Δ Share SSI	0.875	0.497

Note: Data from ACS 2006-2008. The data are limited to heads of households with less than a high school degree that are U.S. citizens. This table presents results from a regression of the mean Hispanic-white difference for each outcome variable on fixed effects for each activation year group. All regressions control for state fixed effects, log crime, and the mean black-white difference in the outcome variable. Columns 1 and 2 present F-statistics and p-values from a F-test of the joint significance of the activation year group fixed effects. Observations in the ACS are weighted by the population in each county. Robust standard errors are clustered at the county level.

Table 2: Triple Differences Estimation – Food Stamp and SSI Take-Up
ACS Citizens Sample

Outcome	*Share Food Stamp*		*Share SSI*	
	(1)	(2)	(3)	(4)
Hispanic × Post	−0.023***	−0.024***	−0.010***	−0.010**
	(0.007)	(0.007)	(0.004)	(0.005)
Post	0.008**	0.009**	−0.001	−0.0007
	(0.004)	(0.004)	(0.003)	(0.003)
Black × Post		−0.005		−0.001
		(0.008)		(0.006)
Pre-Period Hisp. Mean	0.222	0.222	0.053	0.053
Fixed Effects	State-Yr, State-Race, Race-Yr, County-GR			
Baseline Controls	Yes	Yes	Yes	Yes
Observations	86,415	86,415	86,415	86,415
Number Clusters	2,919	2,919	2,919	2,919

Note: Data from ACS 2006–2016. The data are limited to heads of households with less than a high school degree that are U.S. citizens. Baseline controls in the ACS include log poverty, family size, # children, employment rate, and FBI log crime interacted with race. All regressions control for county-by-Great Recession fixed effects, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, 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 3: Triple Differences Estimation – Food Stamp and SSI Take-Up
PSID Citizens Sample

Outcome	*Share Food Stamp*		*Share SSI*	
Sample	*All*	*Prior User*	*All*	*Prior User*
	(1)	(2)	(3)	(4)
Hispanic × Post	−0.198*	−0.452**	−0.055	−0.838***
	(0.109)	(0.214)	(0.069)	(0.315)
Post	0.040	−0.071	0.085***	−0.082
	(0.041)	(0.075)	(0.030)	(0.116)
Pre-Period Hisp. Mean	0.369	0.707	0.047	0.559
Fixed Effects	State-Yr, State-Race, Race-Yr, County-GR			
Baseline Controls	Yes	Yes	Yes	Yes
Observations	27,412	15,695	25,634	4,666
Number Clusters	657	384	593	164

Note: Data from PSID 2005–2015. The data are limited to individuals from families where the heads of household have less than a high school degree and grew up in the United States or were born to parents who were born in the United States. Prior users in the PSID includes households that had previously taken up food stamps and/or SSI prior to SC activation. Baseline controls in the PSID include family size, number of children, log poverty, disability-by-race-by-over 65 fixed effects, employment status, health status, sex, marital status, and FBI log crime interacted with race. All regressions control for county-by-Great Recession fixed effects, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, and race-by-state changes in employment during the Great Recession. Observations in the PSID are weighted by the PSID family weight. 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 4: Robustness Checks
ACS Citizens Sample

	County-Yr FE (1)	*Predicted Yr* (2)	*Deshpande* (3)	*Freyaldenhoven* (4)	*≤ HS* (5)
			Panel A: Share Food Stamp		
Hispanic × Post	−0.024***	−0.025***	−0.023***	−0.024***	−0.013***
	(0.007)	(0.008)	(0.008)	(0.007)	(0.004)
Post		0.004	0.007*	0.005**	−0.0003
		(0.004)	(0.004)	(0.002)	(0.001)
			Panel B: Share SSI		
Hispanic × Post	−0.012**	−0.012**	−0.008*	−0.010**	−0.006**
	(0.006)	(0.005)	(0.005)	(0.005)	(0.003)
Post		0.002	−0.001	0.0001	−0.001
		(0.003)	(0.003)	(0.002)	(0.001)
Fixed Effects		State-Yr, State-Race, Race-Yr, County-GR			
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Observations	85,395	86,415	193,690	77,145	93,783

Note: Data from ACS 2006–2016. Baseline controls in the ACS include log poverty, family size, # children, employment rate, and FBI log crime interacted with race. All regressions control for county-by-Great Recession fixed effects, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, 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. Column 1 adds county-by-year fixed effects to our main specification. Column 2 estimates our main specification using predicted activation date instead of actual activation. Column 3 estimates our main specification where for treated counties that activate in a particular year, we define control counties as those that activate more than two years in the future, following Deshpande and Li (2017). Column 4 estimates our main specification instrumenting for pre-SC activation trends in program take-up following Freyaldenhoven, Hansen, and Shapiro (2018). Column 5 estimates our main specification on a sample of household heads with a high school degree or less. 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: Food Stamp and SSI Take-Up Heterogeneity
ACS Citizens Sample

Outcome	*Share Food Stamp*				*Share SSI*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hispanic × Post	−0.019**	−0.040***	−0.025***	−0.027***	−0.011**	−0.018***	−0.010**	−0.016***
	(0.008)	(0.009)	(0.008)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)
Hispanic × Post × Petty vs. Severe	−0.001***				−0.0002**			
	(0.0003)				(0.0001)			
Hispanic × Post × Δ Pew Fear		−0.145**				−0.084**		
		(0.059)				(0.035)		
Hispanic × Post × Sanctuary City			0.039***				−0.004	
			(0.013)				(0.009)	
Hispanic × Post × % PR/Cuban				0.024*				0.034***
				(0.013)				(0.008)
Fixed Effects	State-Yr, State-Race, Race-Yr, County-GR							
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	82,567	76,750	86,415	77,943	82,567	76,750	86,415	77,943

Note: Data from ACS 2006–2016. The data are limited to heads of households with less than a high school degree that are U.S. citizens. Petty vs. Severe measures the difference in detainers issued for minor offenses than serious violent offenses relative to the number of arrests in a county. Δ Pew Fear measures 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. % PR/Cuban measures the share of Hispanic households with a Puerto Rican or Cuban head in a county. All specifications contain main terms and the full set of interactions with the Hispanic indicator, black indicator and post-SC indicator. Baseline controls in the ACS include log poverty, family size, # children, employment rate, and FBI log crime interacted with race. All regressions control for county-by-Great Recession fixed effects, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, 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.

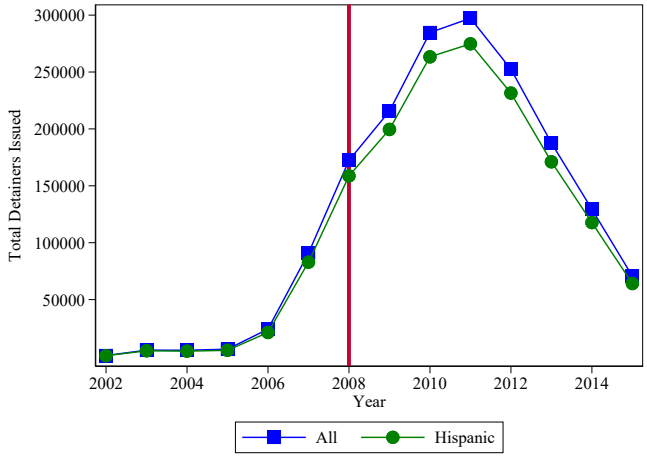
Table 6: Compositional Changes
ACS Citizens Sample

Outcome	*# Child*	*Fam Size*	*Log Pov*	*Emp Rate*	*% Moved*	*% Non-Cit*	*% Mixed*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hispanic × Post	0.001	0.009	−0.002	0.010	−0.002		
	(0.013)	(0.017)	(0.019)	(0.008)	(0.002)		
Post	−0.007	0.016**	−0.006	−0.010**	0.0002	0.0005	0.002
	(0.005)	(0.008)	(0.009)	(0.004)	(0.001)	(0.002)	(0.007)
Pre-Period Hisp. Mean	0.712	2.079	3.821	0.427	0.051	0.220	0.157
Fixed Effects	State-Yr, State-Race, Race-Yr, County-GR						
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	86,415	86,415	86,415	86,415	86,415	27,174	27,174

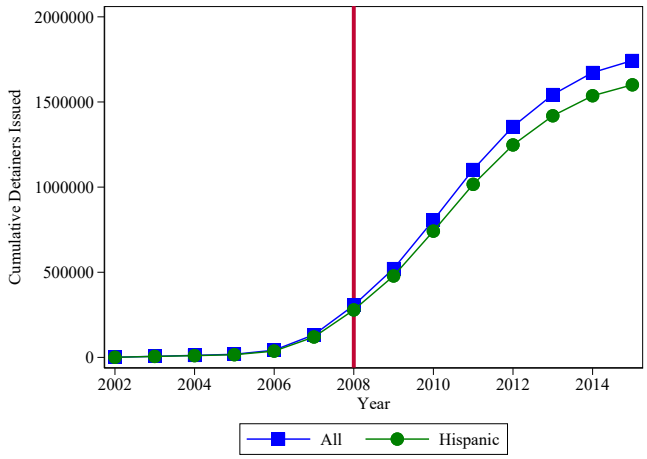
Note: Data from ACS 2006–2016. The data are limited to heads of households with less than a high school degree that are U.S. citizens. Baseline controls include FBI log crime interacted with race and, in column (1) family size, log poverty, and employment rate, in column (2) # children, log poverty, and employment rate, in column (3) # children, family size, and employment rate, in column (4) # children, family size, and log poverty, and in columns (5)–(7) log poverty, family size, # children, and employment rate. All regressions control for county-by-Great Recession fixed effects, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, 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.

Figure 1: Detainers by Year

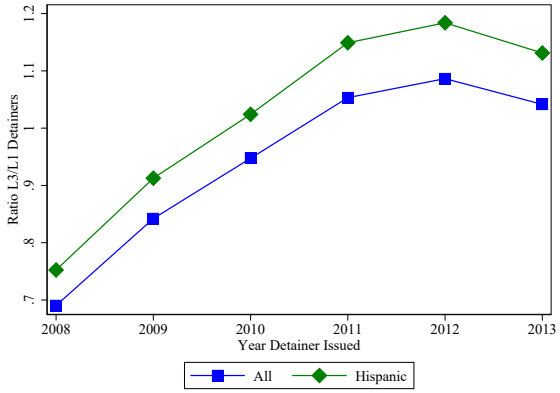
Panel A: Total by Year



Panel B: Cumulative by Year

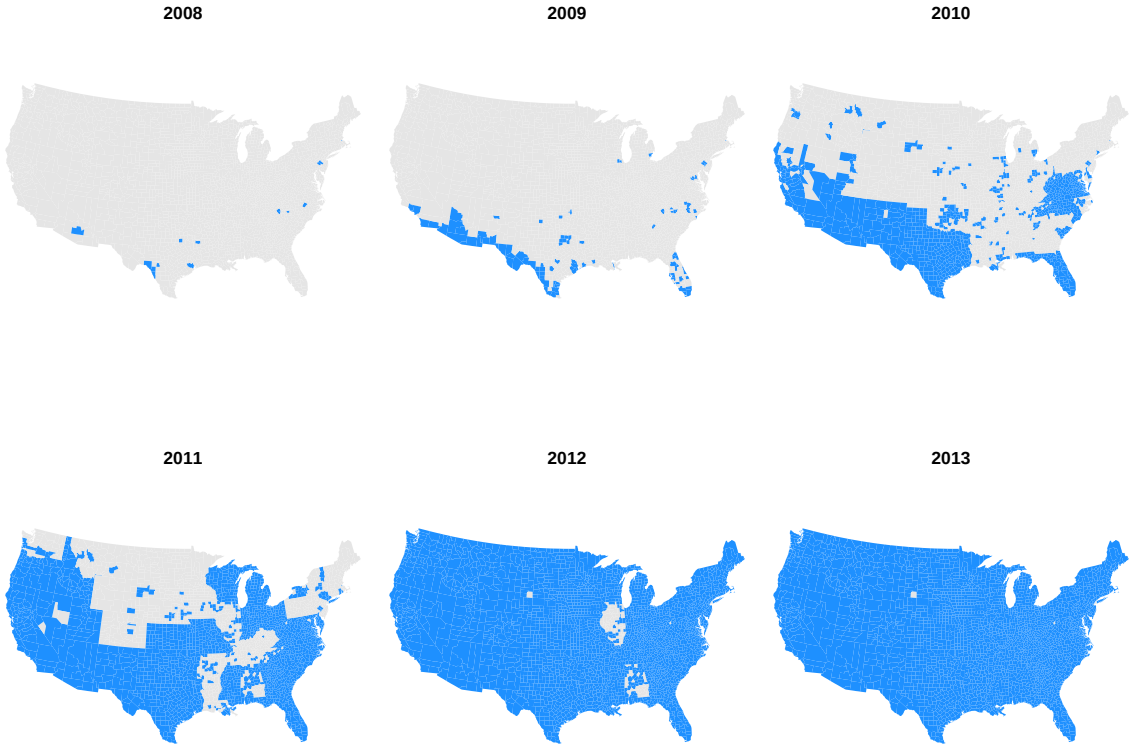


Panel C: Ratio of Low-Level to Violent Offenses



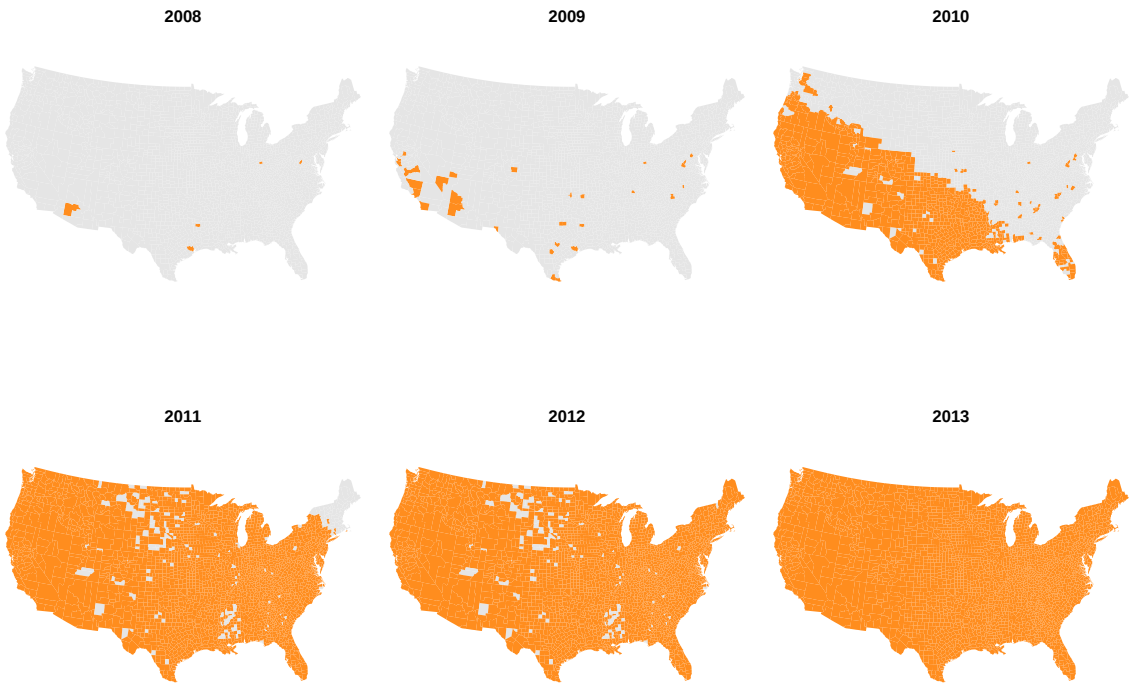
Note: Data from FOIA.

Figure 2: Secure Communities Activation



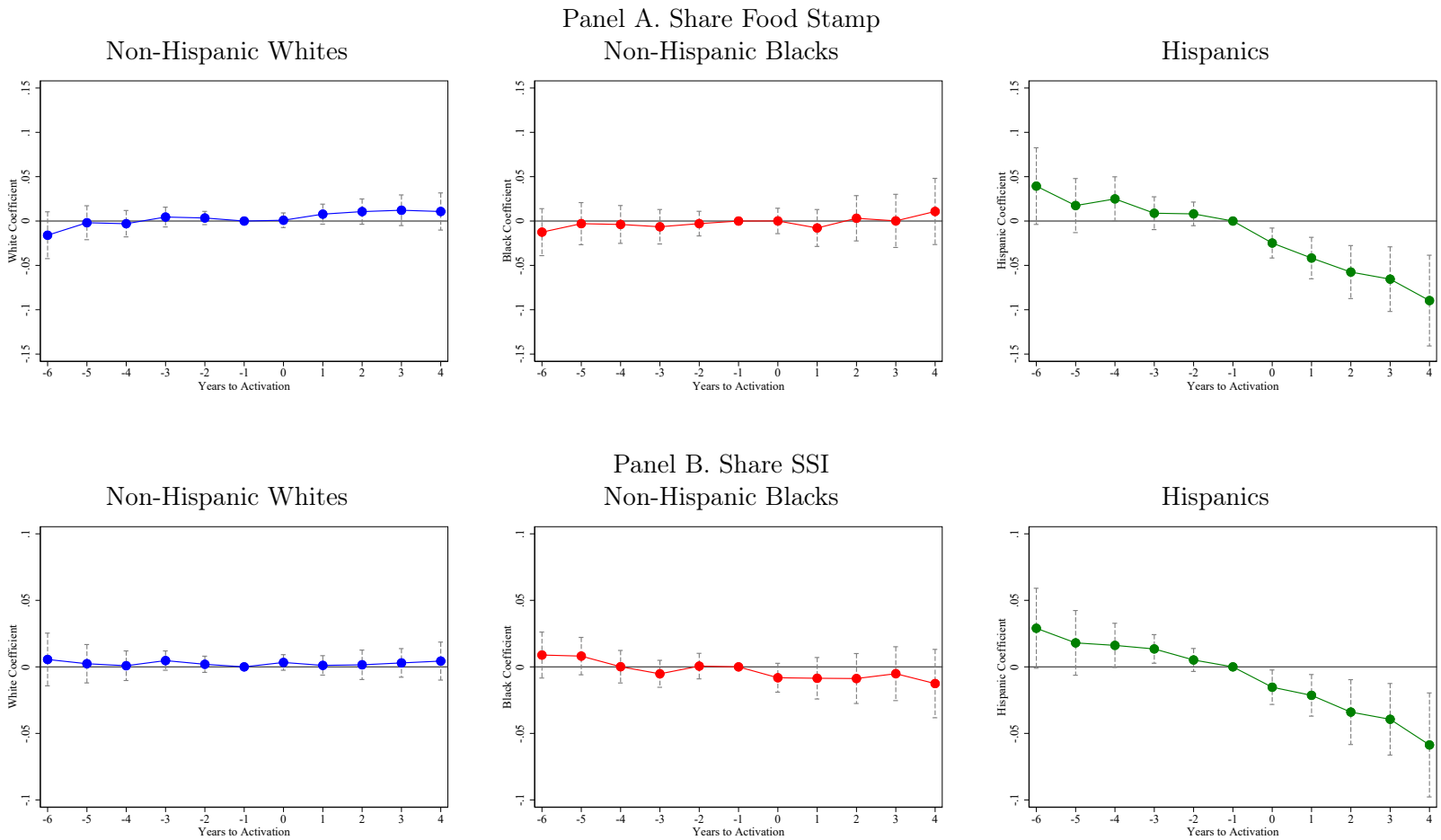
Note: Data from FOIA and public records.

Figure 3: Predicted Secure Communities Activation



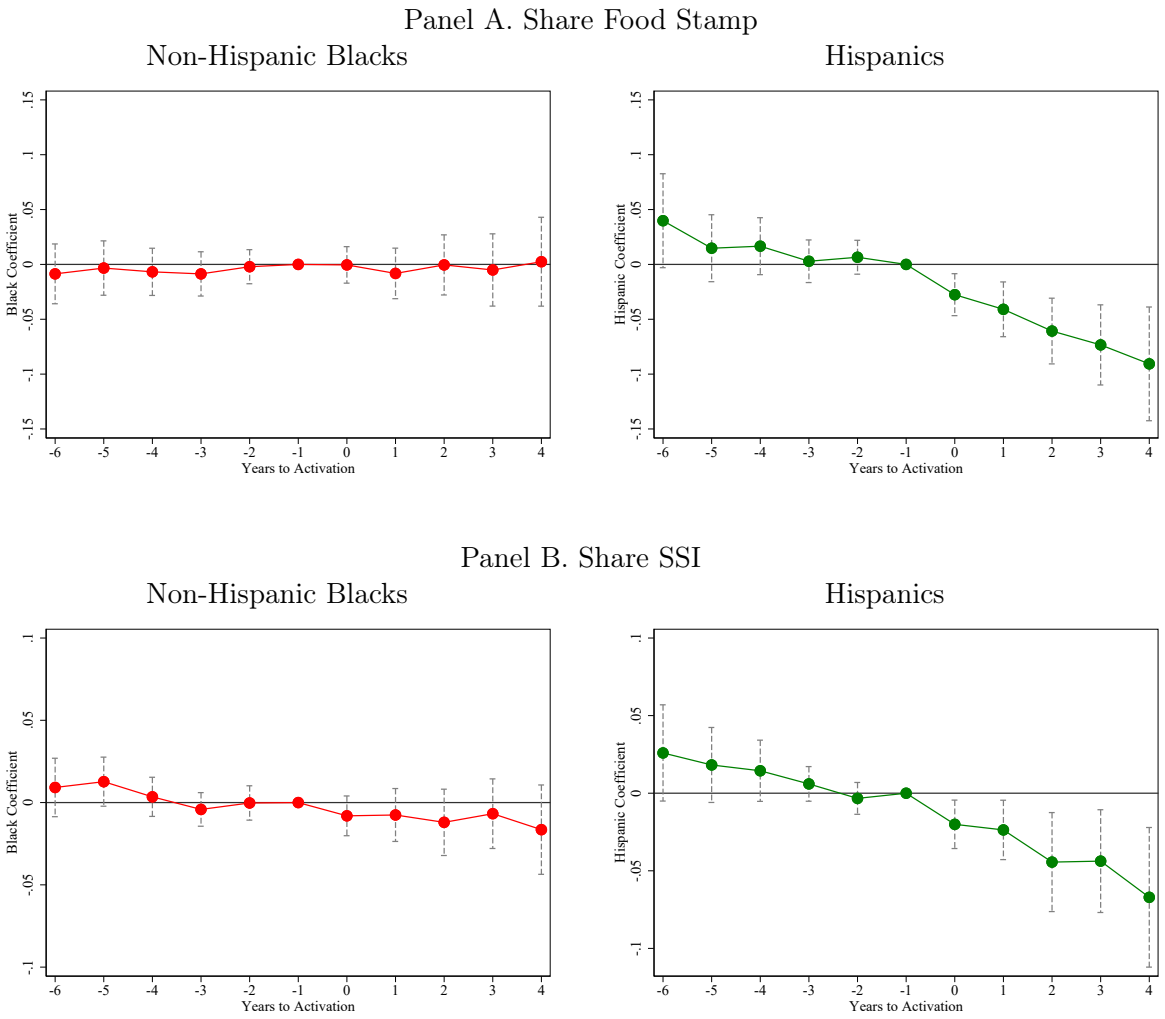
Note: Data from FOIA and ICE documentation.

Figure 4: Event Study of Food Stamp and SSI Take-Up



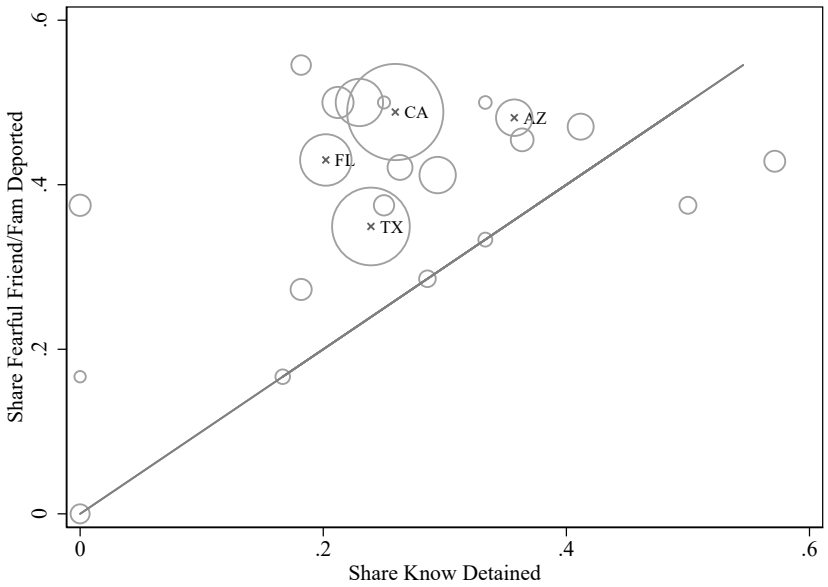
Note: Data from ACS from 2006–2016. Coefficients and 95% confidence intervals are plotted. The data are limited to heads of households with less than a high school degree that are U.S. citizens. Baseline controls in the ACS include log poverty, family size, # children, employment rate, and FBI log crime interacted with race. All regressions control for county-by-Great Recession fixed effects, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, 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 5: Robustness Event Study of Food Stamp and SSI Take-Up
County-by-Year FE



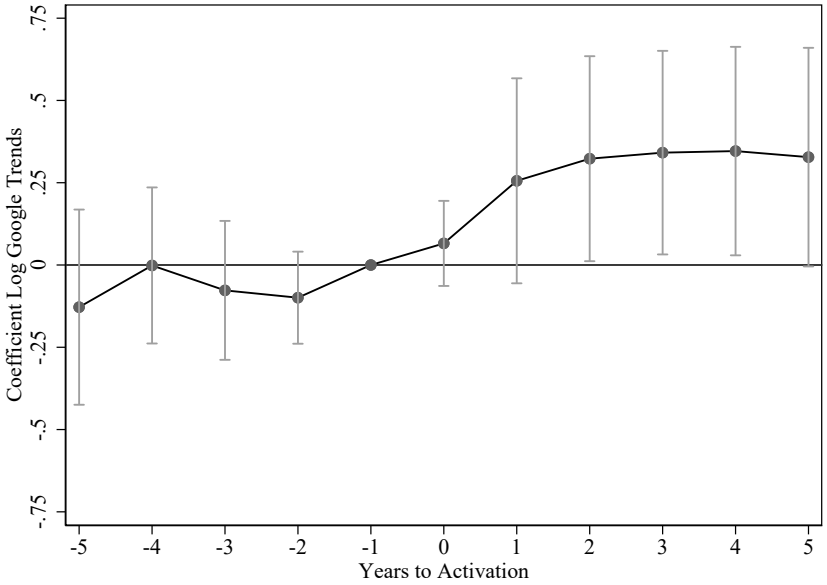
Note: Data from ACS from 2006–2016. Coefficients and 95% confidence intervals are plotted. The data are limited to heads of households with less than a high school degree that are U.S. citizens. Baseline controls in the ACS include log poverty, family size, # children, employment rate, and FBI log crime interacted with race. All regressions control for county-by-year fixed effects, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, 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



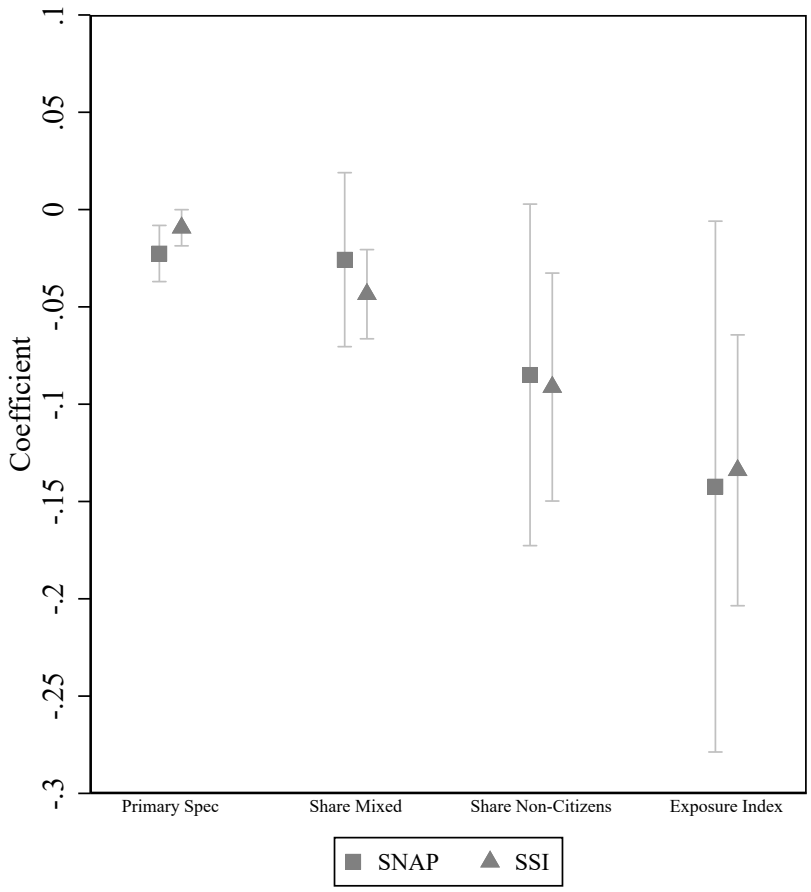
Note: Data from Pew Hispanic Survey 2010. The sample excludes non-citizens and states with five or fewer respondents. Fear refers to fear that a family member or close contact will be deported. The knowledge measure refers to the share of people responding affirmatively that they know someone who has 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. Coefficients and 95% confidence intervals are plotted. 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 and year fixed effects. Standard errors are clustered at the DMA level.

Figure 8: Intensity of Treatment and Take-up



Note: Data from ACS from 2006–2016. Coefficients and 95% confidence intervals are plotted. For "Primary Spec," we present the coefficient from Hispanic*Post in our main specification. For "Share Mixed," we present the coefficient from Hispanic*Post*% Mixed where % Mixed is the share of Hispanic households that are mixed-status 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. For "Share Non-Citizen," we present the coefficient from Hispanic*Post*% Non-Citizen where % Non-Citizen measures the share of non-citizens among Hispanics in a county. Finally, for "Exposure Index", we present the coefficient from Hispanic*Post*Exposure Index where Exposure Index is the product of % Mixed and % Non-Citizen and captures the probability that a mixed status household is randomly exposed to a Hispanic non-citizen in a county. The data are limited to heads of households with less than a high school degree that are U.S. citizens. Baseline controls in the ACS include log poverty, family size, # children, employment rate, and FBI log crime interacted with race. All regressions control for county-by-Great Recession fixed effects, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, 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.

Appendix A: Additional Results

Appendix Table A1: SC on Crime and Immigration Enforcement

Outcome	*Log Offenses*	*Submissions*	*Matches*	*Detainers*
	(1)	(2)	(3)	(4)
Post	−0.0004	13083.551***	1223.113*	527.143**
	(0.013)	(4684.250)	(712.692)	(244.922)
Pre-Period Mean	6.689	295.124	15.245	41.000
Fixed Effects		State-Yr, County-GR		
Observations	32,479	29,530	29,530	29,530

Note: Data on offenses known to law enforcement are from FBI from 2005–2015. Data on fingerprint submissions, matches, and detainers are from FOIA requests to ICE from 2006–2014. All regressions control for county-by-Great Recession 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 A2: Robustness to Alternative Samples/Specifications
ACS Citizens Sample

	Match PSID (1)	*Female* (2)	*No Nat* (3)	*No Mixed* (4)	*Drop Cities* (5)	*Spatial Lag* (6)	*PUMA* (7)	*Hisp/NonHisp* (8)	*Some College* (9)
					Panel A: Share Food Stamp				
Hispanic × Post	−0.074***	−0.021***	−0.011	−0.018**	−0.018**	−0.022***	−0.028***	−0.025***	−0.011***
	(0.021)	(0.008)	(0.009)	(0.007)	(0.009)	(0.007)	(0.007)	(0.007)	(0.003)
Post	0.005	0.006	0.004	0.008**	0.008**	0.007*	0.008**	0.008*	0.001
	(0.008)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.001)
					Panel B: Share SSI				
Hispanic × Post	−0.021***	−0.016**	−0.009	−0.011**	−0.005	−0.010**	−0.012**	−0.009*	−0.002*
	(0.006)	(0.006)	(0.007)	(0.005)	(0.006)	(0.004)	(0.005)	(0.005)	(0.001)
Post	0.007	0.001	−0.003	−0.0004	−0.002	−0.002	−0.002	0.001	0.001*
	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.0004)
Fixed Effects					State-Yr, State-Race, Race-Yr, County-GR				
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	33,283	76,684	84,315	85,366	86,250	86,415	29,998	58,443	91,321

Note: Data from ACS from 2006–2016. In column 1, we estimate our main specification using a sample of counties that approximates the PSID sample in terms of dependent variable means. In column 2, we estimate our main specification using a sample of females (either female head of household or female spouse). In column 3, we estimate our main specification using a sample of Hispanic citizen heads of households excluding naturalized citizens. In column 4, we estimate our main specification using a sample of Hispanic citizen heads of households excluding families that are mixed-status. In column 5, we estimate our main specification dropping New York, Los Angeles, Miami, Houston, and Chicago. In column 6, we estimate our main specification controlling for a spatial lag in SC activation using an exponential model with distance decay parameter of 0.05 km. In column 7, we estimate our main specification at the PUMA-level, assigning the minimum year of SC activation to each PUMA. In column 8, we estimate our main specification comparing Hispanics to all non-Hispanics. In column 9, we estimate our main specification using a sample of heads of households with more than a high school degree. Baseline controls in the ACS include log poverty, family size, # children, employment rate, and FBI log crime interacted with race. All regressions control for county-by-Great Recession fixed effects, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, 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 A3: Robustness to Alternative Weighting and Controls
ACS Citizens Sample

	No Weights # Hisp > 25	*No Weights # Hisp > 25 No Sanc*	*Individual*	*Hisp Share*	*Non-Citizens*
	(1)	(2)	(3)	(4)	(5)
			Panel A: Share Food Stamp		
Hispanic × Post	−0.028***	−0.028***	−0.029***	−0.026***	−0.023***
	(0.009)	(0.010)	(0.008)	(0.007)	(0.007)
Post	0.019***	0.022***	0.006	0.008**	0.008**
	(0.005)	(0.006)	(0.005)	(0.004)	(0.004)
			Panel B: Share SSI		
Hispanic × Post	−0.004	−0.005	−0.011**	−0.011**	−0.010**
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
Post	−0.001	−0.002	−0.001	−0.0004	−0.001
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
Fixed Effects		State-Yr, State-Race, Race-Yr, County-GR			
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Observations	85,750	81,498	86,415	86,415	86,415

Note: Column 1 estimates our main results with no weights, limited to counties with at least 25 Hispanic citizen heads. Column 2 estimates our main results with no weights, limited to counties with at least 25 Hispanic citizen heads, excluding sanctuary jurisdictions. Column 3 estimates our main results with weights using one observation per person in each household. Column 4 estimates our main results with weights controlling for the share Hispanic. Column 5 estimates our main results with weights controlling for the log number of non-citizen Hispanics. Baseline controls in the ACS include log poverty, family size, # children, employment rate, and FBI log crime interacted with race. All regressions control for county-by-Great Recession fixed effects, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, 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 Figure A1: California SNAP Application

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

6a. HOUSEHOLD'S INFORMATION

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

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

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

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

NAME	NAME
NAME	NAME

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

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

(PLEASE CHECK ONE)

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

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

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

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

Note: Data from section of California SNAP Application.

Appendix Figure A2: SSI Application

24.	(b) Name of placing agency	Address	Telephone Number
			( ) -
	(c) Does this agency pay for your room and board? ☐ YES Go to #38 ☐ NO If NO, who pays? Go to #38		

HOUSEHOLD ARRANGEMENTS

25.	Check the block that describes your current residence, then Go to #26:	
	☐ House	☐ Mobile Home
	☐ Apartment	☐ Houseboat
	☐ Room (private home)	☐ Other (Specify)
	☐ Room (commercial establishment)	
26.	Do you live alone or only with your spouse?	☐ YES Go to #28 ☐ NO Go to #27

27. (a) Give the following information about everyone who lives with you:

Name	Relationship	Public Assistance		Sex		Birthdate mm/dd/yy	Blind or Disabled		If Under 22				Social Security Number
									Married		Student		
		YES	NO	M	F		YES	NO	YES	NO	YES	NO	

If anyone listed is under age 22 and not married, Go to (b); otherwise, Go to #28.

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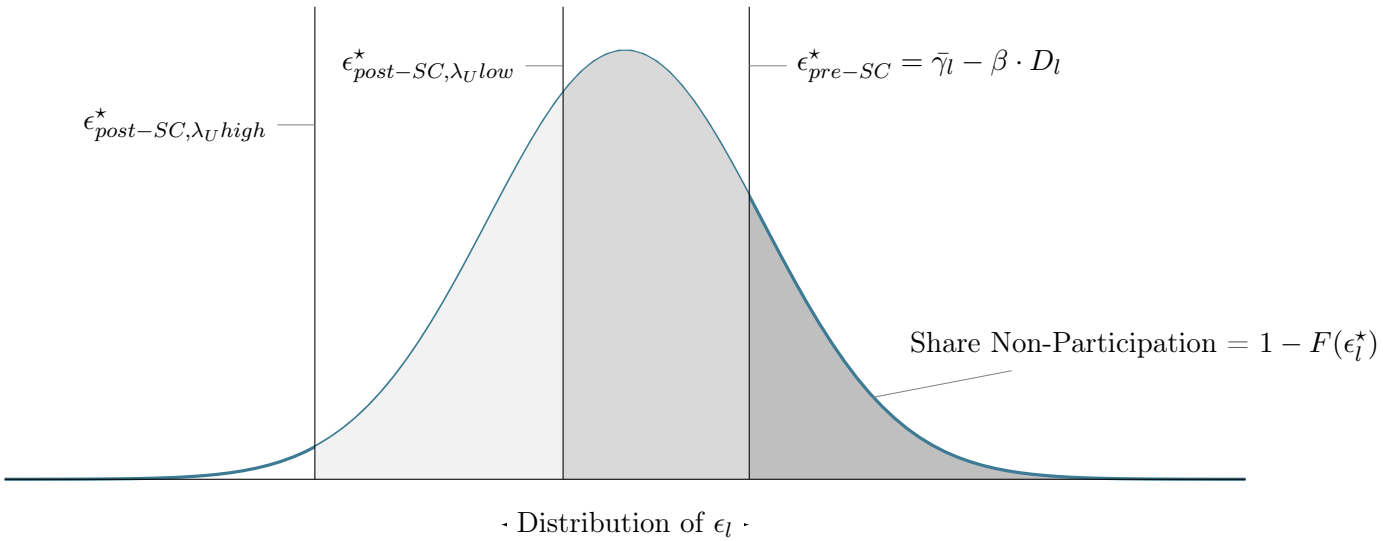
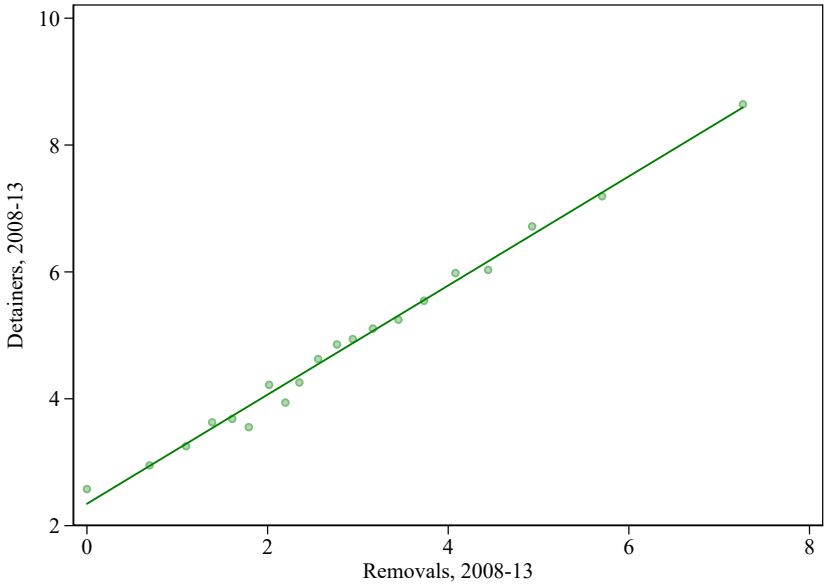
Note: Data from section of SSI Application from ssa.gov.

Appendix Figure A3

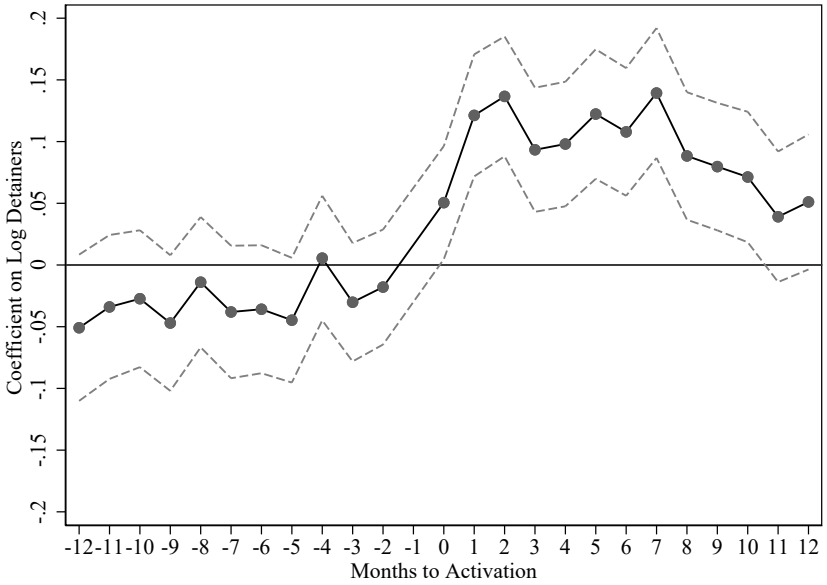


Appendix Figure A4: Correlation between Detainers and Removals



Note: Data from FOIA. This figure presents the correlation between log detainers and log removals for binned counties (20 total). The correlation between the measures is 0.84.

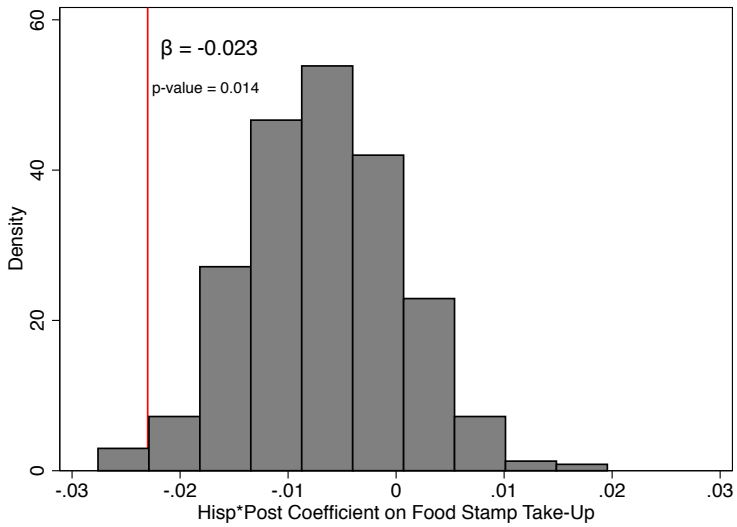
Appendix Figure A5: Detainers Event Study



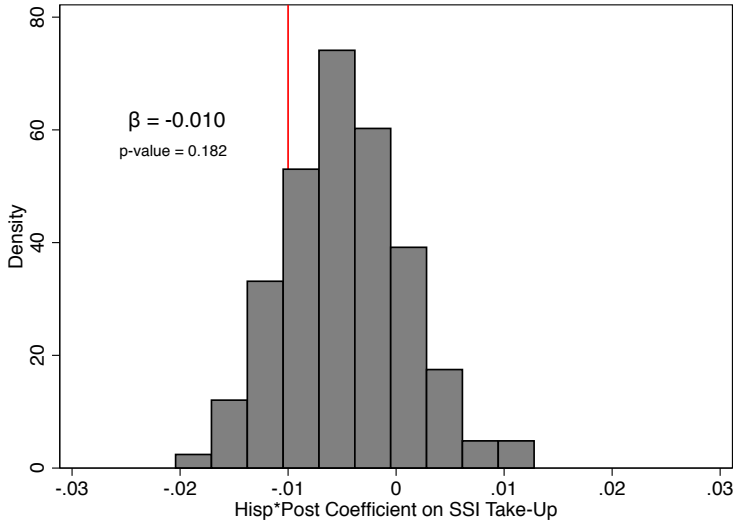
Note: Data from FOIA. Coefficients and 95% confidence intervals are plotted. This figure represent event study estimates of the time to SC activation in months on the log number of detainers issued. All specifications control for county fixed effects. Standard errors are clustered at the county level.

Appendix Figure A6: Permutation Tests

Panel A: Food Stamp

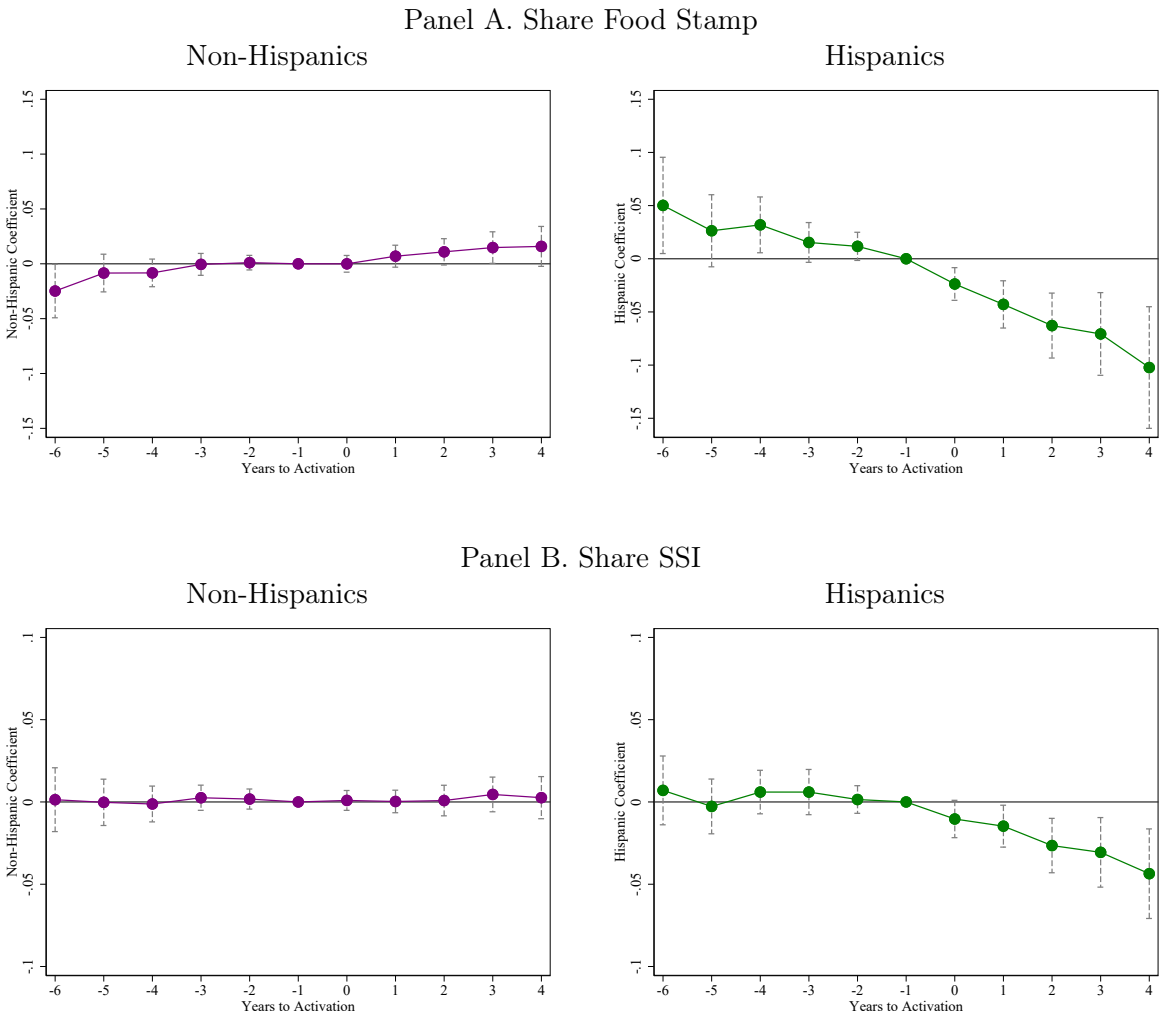


Panel B: SSI



Note: Data from ACS. These figures represent empirical distributions of our estimates 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 A7: Robustness Event Study of Food Stamp and SSI Take-Up
Non-Hispanic vs. Hispanic



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 that are U.S. citizens. Non-Hispanic includes household heads who identify as non-Hispanic black or non-Hispanic white. Baseline controls in the ACS include log poverty, family size, # children, employment rate, and FBI log crime interacted with race. All regressions control for county-by-Great Recession fixed effects, state-by-year fixed effects, state-by-race fixed effects, race-by-year fixed effects, 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.