Fear and the Safety Net:
Evidence from Secure Communities*

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Abstract

We study the impact of deportation fear on the incomplete take-up of federal safety net programs in the United States. We exploit changes in deportation fear due to the roll-out and intensity of Secure Communities (SC), an immigration enforcement program administered by the Immigration and Customs Enforcement Agency (ICE) from 2008 to 2014. The SC program empowers the federal government to check the immigration status of anyone arrested by local law enforcement agencies and has led to the issuance of over two million detainers and the forcible removal of approximately 380,000 immigrants. We estimate the spillover effects of SC on Hispanic citizens, finding significant declines in ACA sign-ups and food stamp take-up, particularly among mixed-status households and areas where deportation fear is highest. In contrast, we find little response to SC among 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.

JEL Codes: I14, I3, K00

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I. Introduction

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

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

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

In order to identify the spillover effects of immigration enforcement on Hispanic Americans, we...
distinguish between direct and indirect treatment effects, with a focus on the latter. In the Rubin Causal Model (RCM) framework, the direct treatment effect is the difference 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 migrants in the country without permission. Direct treatment effects stem mainly from principal-agent problems, whereby unauthorized parents forgo signing up their citizen children for benefits out of fear of revealing themselves. 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 (ITE) stem from externalities, whereby authorized individuals forgo private benefits out of concern for their unauthorized contacts. Under the RCM model, indirect treatment effects measure the difference in potential outcomes for treatment and control groups among individuals who are not eligible for deportation (e.g. authorized U.S. citizens), who may nevertheless be fearful of revealing close contacts 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 ("immigrant holds") issued between 2008 and 2015. These data contain information on the county of issue, crime severity, and country of origin of each arrested individual. We combine these data with information on the take-up of Supplemental Nutrition Assistance Program (SNAP) and health insurance on federal exchanges initiated under the Affordable Care Act (ACA). Information on take-up comes from the restricted version of the Panel Study of Income Dynamics (PSID), public-use data from the Centers for Medicare and Medicaid Services (CMS), and the American Community Survey (ACS). We focus on federal programs since the eligibility criteria are more consistent across locations. SNAP and health insurance subsidies under the ACA represent two of the fastest growing means-tested programs in the United States and thus are of special interest to economists and policymakers alike. Because our focus is on indirect effects, we examine program participation among citizen heads of households. When measuring food stamp outcomes, we follow the prior literature and examine behavioral responses among a high participation sample, defined as those in which the head of household earned less than a high school degree (Hoynes, Schanzenbach, and Almond 2016).

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1By focusing on the ITE, issues of fraudulent usage and the undercount of unauthorized individuals become less problematic.

2In a recent survey of residents in Los Angeles County, 37 percent reported being concerned that they, a friend, or a family member could be deported. See http://abc7.com/news/fear-of-deportation-on-the-rise-in-la-county-ucla-survey-says/1837739/.

3Access to restricted use versions of the ACS and other data sets of interest via the Federal Research Data Center (FRDC) was denied by Census.

4This approach is also useful for identification since education is generally fixed by young adulthood whereas income and asset levels can respond endogenously to program thresholds. In robustness checks, we also use alternative definitions of participation decision-makers, such as female heads.
We employ two different identification strategies to estimate the impact of SC on program take-up. In our first approach, we explore the extensive margin of deportation activity, leveraging the staggered roll-out of SC across counties. We note, however, that the roll-out of SC was not unconditionally random, with the earliest activation occurring in border communities and in places with a high percent Hispanic population (Cox and Miles 2013). We thus take several steps to reduce selection bias. First, we exclude border counties from the main sample and include the percent of Hispanic-headed households in each county-year in all specifications. Second, we use a triple-differences framework, interacting race and ethnicity indicators with timing of SC activation. In doing so, we compare food stamp take-up for Hispanic households within a given location to take-up for non-Hispanic whites and blacks, net of counties that had not yet activated, before versus after SC activation. The triple-differences identification assumption is more 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. Third, we leverage official ICE documentation detailing factors that influenced the timing of the roll-out to generate predicted activation dates.

In our second approach, we exploit cross-sectional variation in the intensity of SC enforcement to assess sign-up for the ACA. Intensity of SC enforcement is represented by the prevalence of detainers issued in a location relative to the number of estimated unauthorized Hispanics. We instrument for enforcement intensity using a supply-push/shift-share instrument (Card 2001; Bartik 1991; Blanchard and Katz 1992). The supply-push moniker stems from the observation that newer immigrants tend to follow the settlement patterns of earlier ones (i.e. “chain migration”), so that shares of immigrant groups interacted with their national flow predicts migration patterns. We modify this approach for our purposes, interacting the pre-period shares of each Hispanic foreign-born group in the county 20 years prior to SC (the share) with the leave-one-county-out cumulative number of detainers issued (the shift). A strong first-stage relationship exists between this shift-share instrument and enforcement intensity.

We find that SC activation is associated with substantial declines in safety net participation among Hispanic citizen households. In the ACS, Hispanic-headed families were 2.5 percentage points less likely to take up food stamps after activation of SC. The take-up rate of food stamps among Hispanic-headed households in the ACS before activation was 23 percentage points, implying an 11 percent decline in take-up due to SC activation. Similar results are obtained when using food stamp take-up recorded in the PSID. Turning to health insurance, a 10 percent increase in detainers is associated with a 3.7 percentage point reduction in Hispanic ACA sign-up. These estimates imply that, in the absence of SC, ACA sign-ups among eligible Hispanics would have been 36 percent higher.

A number of results suggest these correlations are indeed causal. First, we probe the identifying assumptions for both our empirical approaches and find evidence supporting their validity. Consistent with the parallel pre-trends assumption under our triple-differences approach, balance tables demonstrate no sharp changes in the evolution of our outcome variables prior to SC acti-
vation. For the shift-share instrument, the identifying assumption is that the historical shares of Hispanic foreign born in a county only affect take-up through the mechanism of immigration enforcement (Goldsmith-Pinkham et al. 2018), i.e. the exclusion restriction. We test the plausibility of this identifying assumption by exploring the relationship between historical composition and local characteristics that influence program take-up. We fail to find a correlation between our shift-share instrument and potential confounding factors. Second, we condition on a rich set of control variables thought to influence the outcome and treatment, including political affiliation (Lerman, Sadin, and Trachtman 2017), gender (Morin, Taylor, and Patten 2012), age (Wehby and Lyu 2017), income (Buettgens, Kenney, and Pan 2015), and crime (Cox and Miles 2013). We include a full set of race-by-state fixed effects to address the potential concern that states may vary in their attitudes and policies towards minority groups, and we allow for flexible impacts of the Great Recession across demographic groups, interacting race- and ethnicity-specific employment changes with the timing and intensity of the recession in all our longitudinal analyses (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 small and not statistically significant. We also find null effects of SC on Puerto Ricans and Cubans, two groups that face minimal deportation risk because of citizenship or political refugee status. These findings accord with the fact that over 90 percent of detainers issued under the SC program were for Hispanics and suggest that the SC program did not alter the behavior of those less likely to be affected by enhanced immigration enforcement.

We report five findings that, taken together, are difficult to reconcile without invoking fear as an explanatory mechanism. Fear is defined as the subjective likelihood of an event that brings disutility. Whether detention or deportation of an unauthorized individual elicits such a response depends on whether the citizen decision-maker is connected to someone unauthorized. We therefore assess changes in program participation among mixed-status Hispanic-headed households or places where exposure between citizens and non-citizens is higher. Across all data sets, we obtain consistent results: reductions in safety net take-up are largest among households and communities with more exposure to at-risk individuals. Second, locations where the ratio of non-violent (often traffic-related offenses) to violent Hispanic detainers issued is highest have a heightened response to SC — suggesting communities are sensitive to whether local authorities are differentially targeting non-violent offenders. Third, locations where deportation fear is relatively higher over the activation period exhibit a heightened response to the program’s introduction. Fourth, in locations where federal detainers are not uniformly enforced (i.e. “sanctuary cities”), SC activation has almost no detectable effect. Finally, we show that, following SC activation, Google searches for deportation-related terms across media markets increased sharply, consistent with at least an awareness of the program if not fear of its potential consequences.

One competing explanation for our results is information. Since social networks transmit not only fear but also detailed programmatic knowledge, reducing the number of co-ethnics who sign up for a program could leave affected groups poorly informed about benefits. We explore this possibility

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8 Mixed-status households include members that have different citizenship or immigration statuses.
following Aizer and Currie (2004) by estimating effects on households that previously took up food stamps prior to SC activation in the PSID. Such households arguably already know how to sign up for the benefit. Similar to Aizer and Currie (2004), we find that information spillovers are not an important part of the explanation: Hispanic households who previously received food stamps also substantially reduced their use following SC activation. We also explore but reject the possibility that compositional changes in the types of Hispanic individuals responding to survey questions in a given locale, for example, to migration shifts, are driving the results.

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 unauthorized immigrants. 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 mothers’ enrollment of their children in Medicaid. Related work finds that immigration enforcement affects unauthorized parents’ participation in programs like Women, Infants, and Children (WIC), a program legally available to unauthorized immigrants (Vargas and Pirog 2016), and the Earned Income Tax Credit (Cascio and Lewis 2017). Most recently, Amuedo-Dorantes et al. (2018) find that unauthorized parents are more likely to be in poverty and increase take-up for food stamps for their American children in response to greater immigration enforcement, potentially due to households becoming more impoverished. We build off this impressive literature by estimating 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 eligible individuals.

Second, we add to the literature seeking to understand why families sometime forgo participation in safety net programs despite high returns (see review by Currie 2006), highlighting that kinship networks can yield not only benefits, but also impose costs (see review by Cox and Fafchamps 2008; di Falco and Bulte 2011). Third, we contribute to scholarship that aims to causally identify

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9Our 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; Kaestner and Kaushal 2004). However, the mechanisms behind the observed reduction are debated in the literature. Thomas and Collette (2017) argue that immigrants reduced their take-up because they were confused regarding eligibility and immigrants may have been concerned about being labeled a “public charge,” which can reduce the likelihood of citizenship (see Online Appendix for details). In contrast, Loefstrom 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).
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 is an immigration enforcement program administered by ICE from 2008 to 2014 and reactivated in 2017. The 2008–14 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 unauthorized individuals at large and in federal, state, and local custody charged with or convicted of serious criminal offenses who are subject to removal; (2) to prioritize enforcement actions to ensure apprehension and removal of unauthorized individuals convicted of serious criminal offenses; and (3) to transform enforcement processes and systems to achieve lasting results. SC executed 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. The fingerprints received by DHS were then compared against its Automated Biometric Identification System (IDENT), a database that stores biometric and biographical information on foreign-born persons in three primary categories: (1) non-citizens in the U.S. who have violated immigration law, such as persons

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10 In this section, we briefly review the Secure Communities program. Additional details on the program and its implementation can be found in the Online Appendix.

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 lacked immigration status or was 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 have been 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, the information-sharing partnership between DHS and the FBI under SC was mandated by federal law, which meant that state and local jurisdictions could not easily opt out of participation in SC. All fingerprints submitted to the FBI were automatically sent to DHS, such that a local jurisdiction could not choose to only submit its fingerprints to the FBI.\footnote{See \url{http://www.washingtonpost.com/wp-dyn/content/article/2010/09/30/AR2010093007268.html}}

SC was not implemented at once across the entire country. Due to various constraints, the program began on October 27, 2008, and was activated on a county-by-county basis. SC was adopted in most counties by mid-2012 and fully activated across the entire country on January 22, 2013. Cox and Miles (2013) show that the timing of activation across counties is most strongly correlated with the Hispanic population, distance from the Mexican border, and whether a county had a 287(g) agreement with ICE.\footnote{Using official ICE documentation, we confirm that these factors affected the timing of activation. In Section V, we return to these factors to generate predicted activation dates for each county.}

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.”\footnote{The specific policies can vary widely, from prohibiting police officers inquiring about a person’s immigra-}
On November 20, 2014, SC was temporarily suspended 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.

III. Safety Net Programs

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

**SNAP/Food Stamps:** The Supplemental Nutrition Assistance Program (SNAP), previously known as the Food Stamp Program, is the largest cash or near cash means-tested transfer program in the U.S. In 2012, SNAP spending reached $74 billion, exceeding spending on both the Earned Income Tax Credit ($64 billion) and Temporary Assistance for Needy Families ($29 billion) (Hoynes et al. 2016). SNAP is also the only U.S. public safety net program that is available to all families that meet income eligibility (i.e. it 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 as acting as a stabilizer during the Great Recession (Tiehen et al. 2012; Ganong and Liebman 2013; Short 2014; Bitler and Hoynes 2015).

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15Note, however, that the timing of the suspension was the same across geographies and that the number of detainers could not meaningfully be influenced by sanctuary cities unless they chose to not arrest Hispanic individuals. There is no evidence this was a strategy pursued by these jurisdictions. Rather they ignored detainer requests. See [https://www.dhs.gov/sites/default/files/publications/14_1120_memo_secure_communities.pdf](https://www.dhs.gov/sites/default/files/publications/14_1120_memo_secure_communities.pdf)
In order to receive benefits under SNAP, individuals need to meet various federal guidelines. 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). Immigrants residing in the country illegally are ineligible to receive benefits. However, if a household has at least one eligible person in the household, then that eligible person can receive food stamps. To apply for benefits, individuals complete an application in-person or online, followed by an interview with a SNAP representative. In our context, immigration enforcement may affect take-up because SNAP applications routinely ask for the names and social security numbers of all persons in the household 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 using 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. 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.” Nevertheless, advocacy groups claim that SNAP applications have declined recently and that this decline has coincided with increased anti-immigration rhetoric. As the director of a charitable organization noted to the Associated Press, people are resisting efforts of nonprofit organizations to sign them up for food stamps because “[t]hey don’t want to put their name and address on a form for a government public benefit out of fear that they’ll be sought out and asked to leave.”

A CA: The Affordable Care Act (ACA), enacted in 2010, allowed individuals to purchase health insurance through the federal “Health Insurance Marketplace.” The ACA provided subsidies towards the marketplace for low-income individuals and required all Americans to enroll in health insurance or pay a fine (later repealed as part of the 2017 Tax Cuts and Jobs Act). It also funded states to expand their Medicaid programs to all adults below 138 percent of the federal poverty line, although 18 states have yet to accept the expansion. Rolling out in 2014, 8 million people obtained insurance

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16See https://www.fns.usda.gov/snap/eligibility#Resources.
17The household can forego the SNAP income test, however, if all members of the household are receiving TANF, Supplemental Security Income (SSI), or some other state general assistance programs. 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 have children under 18. There is no requirement of employment in most cases, but applicants have to meet certain work conditions, including registering for work and not voluntarily reducing work hours.
18For 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.
21Id.
via the federal marketplace, increasing to about 13 million by 2016 (Ubiro, Finegold, and Gee 2016). As with SNAP, unauthorized immigrants are ineligible for the ACA, as President Obama pledged in his 2009 speech to Congress regarding the bill.22

According to the Commonwealth Fund, all demographic groups have experienced reductions in their uninsured rate under the ACA, but the decline has been slowest for Hispanics (Garrett and Gangopadhyaya 2016).23 Moreover, as the number of uninsured has fallen, Latinos comprise an ever larger share of the remaining uninsured (Commonwealth Fund 2016). Several reasons have been advanced to explain why millions of Hispanics have yet to sign up including: 1) accounting — counting unauthorized as uninsured despite their lack of eligibility; 2) information — faulty Spanish websites and translations; and 3) fear. As noted in the Hill, “The final reason is simply fear. In signing up for ObamaCare one must give vital personal information that might lead Immigration and Customs Enforcement (ICE) officers to one’s house and family. The government is no longer shy about enforcing removals of anyone here illegally — even grandmothers.”24 An example of the ACA application form is in Appendix Figure A2, which asks questions about citizenship and immigration status for each member of the household. Despite public assurance by the federal government that this information will not be used for immigration enforcement,25 as with SNAP, descriptive evidence suggests that Hispanics are still afraid. In a recent article in the Washington Post, a legal Hispanic resident described the tradeoff — “We’re afraid of maybe getting sick or getting into an accident, but the fear of my husband being deported is bigger.”26

IV. Theoretical Framework

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

Specifically, let household $j$ with head of household $i$ be comprised of a set of authorized members $A$ and unauthorized members $U$ where $A + U = N$. Let the expected utility of head $i$ in household

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22See https://www.youtube.com/watch?v=qgo6Yw2ro.
23The uninsured rate for non-Hispanic whites fell by 47 percent, 46 percent for blacks, and 43 percent for Hispanics (Commonwealth Fund 2016).
25For example, official documents state that “the Marketplace application asks applicants about citizenship and immigration status to determine eligibility for health coverage options. Citizenship and immigration information is collected and verified by the Marketplace only for family members who are applying for coverage. This information will only be used to determine consumers’ eligibility, and will not be used for immigration enforcement purposes.” See https://marketplace.cms.gov/technical-assistance-resources/immigration-fast-facts.pdf and also https://www.ice.gov/doclib/ero-outreach/pdf/ice-aca-memo.pdf; www.ice.gov/espanol/factsheets/aca-memoSP.
In location \( l \) be given by:

\[
EU_{ijl} = \lambda_i \cdot \left( \frac{Y_j}{N} + p_{ij} \mathbb{1}_{i \in A} \cdot (B_i) \right) + \lambda_a \cdot \left( \frac{Y_j}{N} + \frac{p_{ij}B_{j,-i}}{A - \mathbb{1}_{i \in A} \cdot 1} \right) + \lambda_u \cdot \left( \frac{Y_j}{N} - \pi_{jl}(p_{ij}) \right)
\]

where \( Y_j \) is household income (split among all \( N \) members, authorized or unauthorized), \( p_{ij} \) is the decision to participate (made by the head of household \( i \)), \( B_i \) is the per capita benefit to \( i \) from participation if \( i \) is authorized, and \( B_{j,-i} \) is the total benefit to other authorized members of the household. \( \pi_{jl} \) is the subjective probability of deportation (i.e. fear) and is an increasing function of program participation, \( p_{ij} \).

In this utility function, \( \lambda_i, \lambda_a, \) and \( \lambda_u \) represent welfare weights that head \( i \) gives to his own utility, the utility of other authorized members, and the utility of unauthorized members of the household, where \( \lambda_i + \lambda_a + \lambda_u = 1 \).

If head of household \( i \) is authorized \((i \in A)\), the above expected utility function can be re-expressed as:

\[
EU_{ijl} = \frac{Y_j}{N} + (\lambda_a + \lambda_i) \cdot \left( \frac{p_{ij}B_i}{A} \right) - \lambda_u \cdot \pi_{jl}(p_{ij})
\]

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 \( \lambda_u > 0 \). Note that, by choosing not to participate, head \( i \) forgoes a private benefit \( \frac{B_j}{A} \). Equation 1 nests both direct and indirect treatment effects. The Online Appendix models the case where the head of the household \( i \) is unauthorized, capturing the direct treatment effect of deportation fear.

Let the change in the subjective probability that an unauthorized person will be deported if the household participates in a program be:

\[
\Delta \pi_{jl} = \beta_1 \cdot D_l + \epsilon_l
\]

where \( D_l \) is the intensity of location-specific immigration enforcement and \( \epsilon_l \) 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:

\[
\frac{Y_j}{N} + (\lambda_i + \lambda_a) \cdot \left( \frac{B_j}{A} \right) - \lambda_u \cdot \pi_{jl}(1) > \frac{Y_j}{N} - \lambda_u \cdot \pi_{jl}(0)
\]

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

\[
s_l = 1 - F(\bar{\gamma}_l - \beta_1 \cdot D_l)
\]

The non-participation share, \( s_l \), is decreasing in the size of the program benefit \( (B_j) \) and in the weights ascribed to authorized individuals including the head himself \((\lambda_a, \lambda_i)\). In contrast,
the non-participation share is increasing in the weight assigned to unauthorized individuals ($\lambda_u$), 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, authorized heads of households may reduce their take-up of public programs, particularly those with close connections to unauthorized individuals in their networks. Appendix Figure A3 graphically illustrates how the non-participation share is affected by immigration enforcement and connections to unauthorized individuals.

V. Methodology and Data

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

A. Empirical Framework

A.1 Triple-Differences Specification

Our first approach exploits the staggered roll-out 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:

\[ Y_{rct} = \alpha + \beta_1 I_{ct}^{\text{post}} + \beta_2 (I^H \cdot I_{ct}^{\text{post}}) + \beta_3 (I^B \cdot I_{ct}^{\text{post}}) + \Omega' X_{rscrt} + \mu_c + \delta_s + \theta_{rt} \]

\[ + \Gamma_1 X_{ct} + \Gamma_2 (X_{ct} \cdot I^B) + \Gamma_3 (X_{ct} \cdot I^H) + \epsilon_{rct} \]  

(2)

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

In the specification above, $I^H$ and $I^B$ are indicators for Hispanic ethnicity and non-Hispanic blacks, respectively. The omitted category is non-Hispanic whites. $I_{ct}^{\text{post}}$ 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. In the ACS data, $X_{rscrt}$ include the average poverty level, number of children, and family size that vary across both race, county, and time. We control for these characteristics as they are direct determinants of food stamp eligibility.

We write the equation at the county level using the ACS data but note the differences for household-level data using the PSID.
There is also evidence that the Great Recession had differential effects by race and ethnicity. For instance, white families’ wealth fell 26 percent during the Great Recession, while the wealth of black families and Hispanic families fell by 48 and 44 percent, respectively (McKernan et al. 2014). We account for these differential effects by including race/ethnicity-specific state-level employment changes during the Great Recession.

Our specification includes county fixed effects ($\mu_c$) and state-by-year fixed effects ($\delta_{st}$) to account for any state-specific policies or economic shocks that might influence the take-up of food stamps. Such fixed effects also capture differential state-level effects of federal immigration reforms or other state-level immigration reforms. We also include state-by-race/ethnicity fixed effects ($\theta_{rs}$) to control for attitudes and policies in each state that differentially affect minority groups.

Finally, we account for other county-level controls, $X_{ct}$, that are not publicly available disaggregated by race at the county level, but which have been shown to have differential effects on minority populations, such as crime. Arrest statistics are generally not available at the race-county-year level but crime disproportionately impacts minorities communities (Sampson and Lauritsen 1997; Anwar and Fang 2006; Antonovics and Knight 2009). To allow for these differences, we interact race/ethnicity indicators with the FBI index crime rate (Kochhar, Fry, and Taylor 2011; McKernan et al. 2014).

Our specification for the PSID is similar to Equation 2 above except that the data are at the household level. As a result, the outcome is an indicator for take-up of food stamps by a high participation household, $i$. In the PSID data, household-level controls, $X_{irsect}$, include demographic characteristics on the head of household, including marital status, sex, family size, age of youngest child, and poverty level in the past year.

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

In our analysis on food stamp take-up using both the PSID and ACS, we limit our specifications to Hispanic, black, and white heads of households with less than a high school degree — a “high participation” sample following Hoynes, Schanzenbach, and Almond (2016). To measure the spillover (indirect) effects of deportation fear, we further restrict our sample to households with citizen heads, individuals who could not be eligible for deportation. The coefficient of interest in Equation 2 is $\beta_2$, which estimates the impact of SC activation on outcomes of Hispanic households relative to non-Hispanic white households, compared to counties that have not yet activated. $\beta_3$ serves as a placebo test, capturing the effect of SC on black households relative to non-Hispanic white households in counties that have activated versus those that have not yet activated.

In addition to our baseline specification in Equation 2, we estimate an event study where we interact $I^H$ and $I^B$ with a series of time dummies for each period, relative to the year prior to SC

---

Unweighted samples produce similar results (see Appendix Table A5).
activation, which is omitted. In our data, we have sufficient observations to estimate up to six time indicators pre-SC and four time indicators post-SC:

\[
Y_{rcst} = \alpha + \sum_{n \neq -1} \beta_1^n (I_{c,t=n}) + \sum_{n \neq -1} \beta_2^n (I^H \cdot I_{c,t=n}) + \sum_{n \neq -1} \beta_3^n (I^B \cdot I_{c,t=n}) + \Omega' X_{rcst} + \mu_c + \delta_st + \theta_r s \\
\Gamma_1' X_{ct} + \Gamma_2' (X_{ct} \cdot I^B) + \Gamma_3' (X_{ct} \cdot I^H) + \epsilon_{rcst}
\]

(3)

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 \( \beta_2^n \) coefficients trace the take-up of food stamps for Hispanics in the years before and after SC activation relative to non-Hispanic whites.\(^{29}\) Similarly, each \( \beta_3^n \) coefficient traces the take-up of food stamps for blacks relative to non-Hispanic whites before and after activation. Under this event study, one would only expect to see a trend break post-activation for Hispanic households, not black households, if we are measuring the causal effect of SC.

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

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

Column 1 of Table 1 presents the mean and standard deviation of outcome variables and demographic characteristics in the main sample pre-SC activation (2005–2007). Column 2 presents the coefficient of a regression of differences between Hispanics and whites on an indicator for late versus early activation, controlling for state-by-race and state-by-year fixed effects. Standard errors are clustered at the county level. Column 3 presents the coefficient for differences between Hispanics and blacks on an indicator for late activation. In general, there are few differences by racial groups for early versus late activation counties. Most importantly, we find that there are no significant differences in changes in Hispanic-white or Hispanic-black food stamp take-up in the ACS across early versus late activation counties, suggesting that the timing of SC activation was not correlated with trending differences in outcomes by racial/ethnic group. These results lend support to the parallel trends assumption underlying our approach.

In addition, we implement a permutation test where we limit our data to pre-activation years

\(^{29}\)Leads before six years and lags after four years are coded as separate groups.
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 2, repeating this procedure 500 times. In Appendix Figure A4, we present the empirical distribution of these placebo effects for $\beta_2$, finding that our actual treatment effects are larger (in absolute value) than over 98 percent of our placebo estimates. These results suggest that SC activation had a very large and atypical effect on outcomes for Hispanic households.

**Predicted SC Activation:** Although our triple-differences identification does not exclusively rely on differences in program participation pre- versus post-SC activation, it is important to understand the factors that affected the timing of SC activation since non-random timing could still introduce bias. For instance, if SC preferentially activated in locations where criminal activity among the unauthorized was on the rise, and criminal activity decreases program participation, early activators could have seen a Hispanic-specific decline in safety net take-up regardless of SC, leading to overestimates of $\beta_2$ in our main specification (Equation 2). On the other hand, if locations that activated early were routine targets of immigration enforcement (such as locations close to the Mexican border), Hispanics in these areas may be relatively insensitive to changes in enforcement and thus exhibit small decreases in safety net take-up, leading to underestimates of $\beta_2$ in our main specification.

To further understand the timing of SC activation, Figure 1 presents maps that show the timing of SC activation across counties, revealing that border counties were the earliest places to activate. These findings are consistent with Cox and Miles (2014), who find that SC activation was not related to crime — though the purported goal of the program was to remove criminal aliens — rather, earlier activation was positively correlated with proximity to the border, the presence of a 287(g) agreement, and the percent Hispanic population.

We take several steps to reduce selection bias that might be generated by the non-random timing of SC activation. First, we exclude border areas from our analysis since they might be unique in several ways related to both immigration enforcement and program participation and include county fixed effects to account for demographic features of a county that may affect timing of activation. Second, 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 unauthorized individuals, (2) the distance from the Mexican border, (3) crime rates, and (4) prior county relationships with ICE as proxied by the presence of a 287(g) agreement. We use these four criteria to predict the year of activation, $I_{ct}^{\text{post}}$. Then, we estimate Equation 2 using $I_{ct}^{\text{post}}$ and its interactions with $I^B$ and $I^H$. We find nearly identical results when we use predicted activation compared to actual activation (see Section VI).
A.2 Shift-Share Instrument

Our second approach exploits the differential intensity of immigration enforcement under SC across geographies. To explore the impact of the intensive margin of SC on ACA enrollment rates, we estimate the following cross-sectional county-level specification:

\[ ShrLatinoACA_{cs} = \alpha + \beta \cdot (ShrDetain_{c}) + \mu \cdot X_{c} + \delta_{s} + \epsilon_{cs} \]  

(4)

where \( c \) represents county and \( s \) represents state. State fixed effects (\( \delta_{s} \)) and share of males who are Hispanic (an element of \( X_{c} \)) are important controls for selection-on-observables. The former captures state-level programs and policies that affect population health and immigration, while the latter reflects the fact that Hispanic males comprised the overwhelming majority of those detained under SC. In addition to these baseline controls, we control for a variety of other county-level controls (\( X_{c} \)) that are likely correlated with enforcement and program participation. For example, Lerman et al. (2017) document how partisanship can influence public policy behavior, specifically with respect to the ACA. Since immigration enforcement may also be more aggressive in counties that lean towards a particular party, we include the Obama-McCain county-level vote margin in the 2008 presidential election as a control. In addition, employment and income influence ACA eligibility and could be correlated with the treatment, motivating the addition of unemployment and Hispanic poverty rates as controls (Buettgens, Kenney, and Pan 2015). We also control for county-level FBI index crime rates in our preferred specification. The stated purpose of SC was to reduce crime, suggesting crime should predict SC intensity. Furthermore, demand for health insurance could reasonably be lower in high-crime areas. Lastly, we proxy for the effectiveness of program outreach to minorities in a given locale using the share of eligible African-Americans who sign up for the ACA, \( ShrBlackACA_{cs} \).

The dependent variable, \( ShrLatinoACA_{cs} \), is the share of Latino individuals eligible for enrollment who signed up for the ACA in county \( c \) and state \( s \). The treatment variable, \( ShrDetain_{c} \), is defined as the cumulative number of Hispanic detainers issued during SC (i.e. 2008 to 2013, see Figure 2) normalized by the estimated number of unauthorized Hispanic individuals, \( \frac{D}{UH} \). The denominator is based on a method developed by the Pew Research Center and is generated using the ACS 2005–2009 county-level data (see Pew Research Center 2013 and the Online Appendix for details). These data report the total number of foreign born from each country of origin and the number of naturalized Hispanics citizens.

Our coefficient of interest is \( \beta \), which measures the effect of increased detainer intensity on eligible Hispanic ACA sign-up. Although we carefully condition on potential confounders, counties that experienced greater intensity in the share of Hispanics detained may differ in unobservable

\footnote{Cox and Miles (2013), however, fail to find a correlation between crime and SC roll-out.}

\footnote{The data are averaged over the 2015 and 2016 enrollment periods since there is little year-to-year variation.}

\footnote{In the Online Appendix, we compare our estimates to those from Pew. The correlation between the two estimates is greater than 0.95. (See Appendix Figure A5).}

\footnote{The data are publicly available in five-year aggregates using the DataFerrett tool from Census (see Data section for further details).}
ways from counties with less immigration enforcement. As noted above, SC could have targeted places that were characterized by low Hispanic engagement with the welfare and health systems, biasing estimates of $\beta$ towards the null. To better understand whether the relationship we find is indeed causal, we employ a shift-share instrument to predict the number of Hispanic detainers issued. Following Card (2001), we weight the national number of cumulative detainers from each Hispanic country of origin (excluding own county) with county-specific baseline shares of foreign born from each respective country of origin. Intuitively, variation in this shift-share instrument stems from the fact that national increases in detainers for specific Hispanic countries will lead to larger predicted increases in detainers in those counties with a higher share of immigrants from those countries. For example, if SC primarily ramped up detention activity against immigrants from Mexico, the predicted increases in detainers should be larger in those counties that have more Mexican-born immigrants. Because this instrument is constructed using national trends excluding own county, and projected on baseline shares of foreign born from a pre-SC time period, variation induced by the instrument is plausibly exogenous. Figure 3 presents a county-level map of the intensity of SC using both our endogenous variable and shift-share instrument.

In our two-stage least squares specification, we instrument for $ShrDetain_c$ in Equation 4 with $Z_c$, constructed as:

$$Z_c = \frac{\sum_o L^{1990}_{c, o} \times (D_{-co})}{UH_c}$$

(5)

where $c$ represents county, $o$ represents Hispanic country of origin (e.g. Mexico). $L^{1990}_{c, o}$ represents the number of Hispanic immigrants in county $c$ born from country of origin $o$ relative to the total number of Hispanic immigrants born from country $o$. These shares are constructed using the 100 percent 1990 Census and sum to one across the United States. These baseline country-of-origin county shares are then multiplied by the cumulative leave-county-out number of national detainers issued from 2008 to 2013, $D_{-co}$. Finally, we normalize this predicted number of detainers by the predicted number of unauthorized Hispanics, $UH_c$, calculated as the estimated fraction of unauthorized Hispanics from the 1990 Census multiplied by the total number of foreign-born Hispanics in the 2005-2009 ACS. See the Online Appendix for details on variable definitions and construction.

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

The second assumption is that the baseline historical shares of Hispanic foreign born in a county only affect ACA take-up through the mechanism of immigration enforcement (Goldsmith-Pinkham
et al. 2018), i.e. the exclusion restriction. We test the plausibility of this identifying assumption in Figure 4 following Goldsmith-Pinkham et al. (2018) by exploring the relationship between baseline composition and local area characteristics that influence ACA take-up. We test for significance of each characteristic and the joint significance of all county-level characteristics using seemingly unrelated regression (SUR) following Autor and Houseman (2010). We cannot reject the null hypothesis that our preferred Bartik instrument is uncorrelated with these county-level characteristics (joint p-value = 0.449).\footnote{We also calculate Goldsmith-Pinkham et al. (2018) Rotemberg weights. The three top shares are Mexico, Honduras and El Salvador. We evaluate whether either the instrument or these shares are correlated with local conditions conditional on state fixed effects and share male Hispanic. The results are reassuring — out of 20 hypothesis tests, only three are statistically significant at conventional levels.}

Jaeger, Ruist, and Stuhler (2018) note, in the context of studying labor demand shocks on wages, that the exclusion restriction is violated if local demand shocks are serially correlated (i.e. strong chain migration). If serial correlation exists and there are oppositely-signed short- and long- responses to immigrant arrivals due to general equilibrium adjustments, conventional shift-share instruments may yield conflicting estimates. This issue is not a major concern in our setting for two reasons. First, serial correlation is not as important because SC was an unprecedented immigration program that began only in 2008. Second, it is difficult to rationalize SC eliciting oppositely-signed short- and long-run effects on ACA take-up. Nevertheless, we allow for this possibility and control for contemporaneous Hispanic shares by country of origin. The results are very similar.\footnote{See column 6 of Appendix Table A6.}

**B. Data**

_SC Data on Detainers and Removals_: Through records available to the public and FOIA requests to ICE, we have obtained detailed 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, race, 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 2 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 of detainers are issued against Hispanic individuals. Panel C presents the ratio of detainers for low-level offenses (e.g. traffic violations and misdemeanor offenses) versus serious, violent offenses and shows that, over time, SC issued a growing share of detainers for low-level arrests.
We normalize the number of detainers issued by the estimated number of undocumented Hispanic immigrants in a county from the ACS 2005–2009, prior to SC activation. As described previously, we use a method developed by the Pew Research Center to estimate the number of undocumented Hispanics, which subtracts the number of naturalized citizens of Hispanic origin from the total number of Hispanic foreign born (Pew Research Center 2013). The Pew Research Center discusses potential methodological issues associated with this procedure, including undercounting in survey data. While undercounting may be correlated with the degree of incomplete take-up of public programs, we control for county or state fixed effects in our main specifications to account for time-invariant differences in take-up. We also note that our estimates of unauthorized Hispanics are generally larger than that of Pew (Appendix Figure A5).

SC represented a massive increase in immigration enforcement. Appendix Table A2 presents difference-in-difference estimates of the impact of SC activation in a county on enforcement. Consistent with Cox and Miles (2014), we find that SC activation had no significant effect on offenses known to law enforcement or arrests. In contrast, we find significant increases in the number of fingerprint submissions received by ICE, fingerprint matches, and detainers issued post-SC activation. Event study estimates of the impact of SC on detainers issued are presented in Appendix Figure A6, 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. We focus on the 1 percent ACS samples of the U.S. population over the years 2006–2016 for food stamp take-up. The data include household characteristics such as food stamp receipt in the last year, poverty, and family size; and also individual characteristics like income, education, and citizenship status. As discussed previously, we limit our sample to Hispanic, black, and white heads of households with less than a high school degree — a “high participation” food stamp sample following Hoynes, Schanzenbach, and Almond (2016). To measure the spillover (indirect) effects of deportation fear, we further restrict our sample to households with citizen heads, 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.

Panel Study of Income Dynamics: We use data from the restricted-access Panel Study of Income Dynamics (PSID) from 2005–2015. The PSID data are biennial, following heads of household in every survey round. The data contain detailed information on food stamp take-up within the past 12 months and ethnicity by households at the county level. While the PSID does not ask about citizenship status, we proxy for citizenship status using whether a household head grew up in the

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36 Results for fingerprint submissions and matches are mechanical because they are zero in all years pre-SC activation.

37 See Online Appendix for precise variable definitions and alternative data sources from the ACS.
United States versus a foreign country. 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 sex of household head, marital status, family size, number of children, age of youngest child, income relative to federal poverty line, industry, and employment status. As with our ACS sample, we limit our sample to citizen heads of household with less than a high school degree. Among our sample, the PSID surveyed a total number of 2,432 unique household heads from 626 counties.

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

**Google Trends Data:** In order to parameterize fear in response to SC, we use data from internet search patterns provided by Google Trends. Google Trends is a publicly available database that provides information on the relative popularity of search terms for 250 metropolitan areas across the United States 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:

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

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

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

VI. Results

A. Food Stamp Take-Up

Table 2 presents our main results on food stamp take-up across various samples in the PSID and ACS data. All specifications are limited to our “high participation” sample and to citizen heads of household. Column 1 reports our main specification (Equation 2) in the PSID citizens sample. We find that after SC activation, Hispanic citizen heads of household reduce their take-up of food stamps by 15.7 percentage points relative to non-Hispanics, a 38 percent decrease from the pre-period Hispanic mean of 40.9 percent. Column 2 reports our main specification from the ACS citizens sample, where we find that Hispanic citizens reduce take-up by 2.5 percentage points relative to non-Hispanics, an 11 percent decrease from the pre-period Hispanic mean of 23.0 percent.

In columns 3 and 4, we report the same specifications as columns 1 and 2 but add an interaction between our black indicator and post-SC indicator. Our main results are virtually unchanged and we also find no significant effects on the black coefficients post-SC, indicating that SC did not substantially change the behavior of minority groups less likely to be affected by immigration enforcement. In column 5, we report estimates using predicted activation (based on ICE documentation) rather than actual activation. Appendix Figure A7 presents maps that show the timing of predicted SC activation across counties. We find qualitatively similar results on the differential change in take-up for Hispanics, indicating that our findings are unlikely to be driven by selection bias due to the timing of SC activation across counties. In addition, across all our specifications (columns 1–5), we find evidence not only of differential decreases in food stamp take-up for Hispanics, but absolute decreases for Hispanic households following SC.

Our results are robust to different definitions of household decision-makers. Specifically, we consider the fact that food stamp participation may be decided by females within a household. We find very similar results using a sample of citizen female heads of household or female spouses (see column 4 of Appendix Table A3). We also find similar results when we exclude individuals or

39There are several reasons why the magnitudes of our estimates may differ so much between the PSID and ACS samples. First, after our sample restrictions, the PSID covers only 626 counties versus 3,079 in the ACS and differentially covers large states like California and Texas. Indeed, when we select an ACS sample that matches the PSID in pre-period mean take-up for Hispanics, we find much larger estimated effects (see columns 1 through 3 of Appendix Table A3.) Second, although average poverty levels in the PSID are higher than in the ACS (see Table 1), reported food stamp use is evidently much higher based on pre-period Hispanic means. This may be due to the well-known underreporting and measurement error problems of food stamp participation (Kreider et al. 2012). Third, we can only approximate counties in the public-use ACS using a PUMA to county crosswalk and re-weighting strategy, potentially leading to increased measurement error on the right hand side. These combined effects will likely bias our estimates downward (Hausman 2001). Fourth, as discussed earlier, PSID added immigrant families in 1997. We find much larger effects of SC on mixed-status in the ACS (see Table 4) and when evaluating the ACS coefficients at the PSID mixed mean, we obtain even more similar results on our Hispanic*Post indicator (i.e. -0.137 vs. -0.157).
families that face any risk of deportation. For example, our results are very similar when we exclude naturalized citizen heads of household, who in theory could be deportable under some circumstances (column 5 of Appendix Table A3), and when we exclude citizen heads of households with no mixed-status family members (column 6 of Appendix Table A3). These results suggest that our main findings capture a true spillover effect of deportation fear. Finally, we relax the assumption that Hispanics are only affected by enforcement in their county by including a spatial lag in SC activation, weighting each county's enforcement with an exponential spatial weight matrix that places lower weight on farther locations. Again, we find that our results are virtually identical with the inclusion of a spatial lag (column 7 of Appendix Table A3), suggesting that Hispanic households are most responsive to enforcement within their own county.\footnote{The coefficient on the spatial lag is small and not statistically significant.}

In Appendix Table A4, we also present our main results separately for Hispanics versus non-Hispanic blacks and versus non-Hispanic whites. Across all comparison groups, we find a large and significant effect of SC activation on reduced take-up of food stamps for Hispanic households. In the PSID, Hispanic households reduce their take-up of food stamps by 17.3 percentage points post-SC compared to black households and 14.8 percentage points compared to non-Hispanic whites (columns 1 and 2). In the ACS, Hispanic households reduce their take-up of food stamps after SC activation by 2.1 and 2.6 percentage points relative to blacks and non-Hispanic whites, respectively (columns 3 and 4). We also find that Puerto Ricans who have citizenship status, and Cubans, who are more likely to have political refugee status, do not respond to SC activation by reducing food stamp take-up relative to non-Hispanics (column 5).

Appendix Table A5 explores alternative weighting schemes, for example including all family members affected by SC in both the ACS and PSID sample, thus capturing the effect of SC at the individual level rather than household level. We continue to find significant reductions in food stamp take-up among individuals in Hispanic households relative to non-Hispanics.

Figure\footnote{We thank Ted Miguel and Thomas Lemieux for the suggestion.} presents our event study estimates of SC activation on food stamp take-up for non-Hispanic whites, non-Hispanic blacks, and Hispanics using the ACS data. For both non-Hispanic whites and blacks, there is no noticeable break in the relative flatness of take-up in the years pre- and post-SC activation. In sharp contrast, coefficients on the interaction of time to SC and Hispanic are indistinguishable from zero in the years leading up to activation, but then demonstrate a level shift post-activation, with Hispanic heads greatly decreasing their take-up of food stamps over time. By five years post-activation, Hispanic households reduce take-up of food stamps by 4.6 percentage points relative to non-Hispanic whites, a 20 percent decrease from the pre-period Hispanic mean.

**B. Affordable Care Act Sign-Up**

We next turn to estimating the impact of SC intensity on Hispanic ACA sign-up. OLS estimates from Equation \footnote{} are presented in Panel A of Table 3. In column 1, controlling only for state fixed effects and share Hispanic males, we find that a 10 percent increase in the share of Hispanic
detainers issued is associated with a 0.22 percent reduction in Hispanic sign-ups for the ACA. Column 2 adds the Democratic versus Republican vote margin in the 2008 presidential election and share of Hispanics living in poverty. Column 3 adds the unemployment rate and FBI index crimes per capita. Finally, column 4 adds the share Black sign-up for the ACA. In our preferred specification with the full set of controls (column 4), we find that a 10 percent increase in the share of Hispanic detainers issued is associated with a 0.12 percent reduction in Hispanic sign-ups for the ACA. The sensitivity of the results to the addition of controls suggests that SC intensity is correlated with unobservables that affect sign-up (Altonji, Elder, and Taber 2005).42

To address the endogeneity of SC intensity, we predict the share of Hispanic detainers using plausibly exogenous variation in baseline shares of Hispanic foreign born across counties as described in Equation 5. Panel B of Table 3 presents our first stage estimates, with $F$-statistics ranging from 13 to 16. Panel C of Table 3 presents the two-stage least squares results. Controlling only for state fixed effects and the share Hispanic males (column 1), we find that a 10 percent increase in detainers is associated with a 4.5 percent reduction in Hispanic ACA sign-ups. Results are similar but slightly smaller in magnitude with the addition of county-level baseline controls. In our preferred specification (column 4), we find that a 10 percent increase in detainers is associated with a 3.7 percent reduction in Hispanic ACA sign-ups. We note that the OLS correlations between SC intensity and Hispanic ACA sign-up are much smaller than our two-stage least squares estimates. One explanation is due to selection, i.e., SC was more intensive in areas with lower propensity to sign-up for health insurance. This selection would result in our OLS estimates being biased toward zero and understanding the true negative effect of SC intensity on ACA take-up among eligible Hispanics.

To put our two-stage least squares estimate in perspective, SC led to the issuance of roughly 730,000 detainers in our federal exchange sample during the 2008 to 2013 time period.43 We estimate that there were roughly 6.5 million unauthorized Hispanics in the ACA sample during this time period, suggesting that approximately 11 percent of the unauthorized population was issued a detainer. These estimates imply that, in the absence of SC, ACA sign-ups among eligible Hispanics would have been 36 percent higher.

Appendix Table A6 presents a series of robustness checks. Our results are robust to additional controls that may affect Hispanic ACA sign-up, such as whether a county cooperates with ICE through a 287(g) agreement, or whether a county has a community health center which may serve as a substitute for health insurance. In addition, some counties had health navigator programs that assisted Hispanics in enrolling in the ACA. We control for this navigator program since it might independently affect take-up. Its inclusion does not alter our main results. We also relax the assumption that Hispanics are only affected by enforcement in their county by including a spatial lag in detainer intensity. Again, we find that our results are virtually identical with the inclusion of a spatial lag. Finally, as discussed previously, we probe the exclusion restriction of our identification

42Wooldridge's (1995) score test and regression-based tests confirm the same.
43During the entirety of SC, there were over 2 million detainers.
strategy by controlling for contemporaneous Hispanic country-of-origin shares. We find very similar results, suggesting that the exclusion restriction is likely valid in our setting.

In addition, given that SC primarily affected unauthorized Hispanic individuals, we do not anticipate that SC intensity led to decreases in ACA sign-up among other racial/ethnic groups. As a placebo test, we regress our measure of share Hispanic detainers issued on share of eligible blacks and eligible whites that signed up for the ACA. Results in Appendix Table A7 suggest no significant relationship in our two-stage least squares results between SC intensity and either black or white ACA sign-up, although large standard errors make definitive conclusions difficult.

VII. Mechanisms

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

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). This relationship has also been described in anecdotal evidence with regards to SC activation, as detailed in the 2011 Task Force Review on Secure Communities (HSAC Task Force 2011).

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

Second, we test the hypothesis that households and communities with more mixing or exposure between unauthorized and citizen Hispanics should be more influenced by SC activation, as suggested by our model and qualitative findings. These results are presented in column 1 of Table 44.

---

44 Pew used the following questions 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 7.
In the ACS sample, we find substantially larger effects of SC in counties with a higher share of Hispanic citizen households that are mixed-status. Our estimate in column 1 suggests that post-SC, counties with a 10 percent higher share of mixed-status households decrease take-up of food stamps by an additional 4.3 percentage points, representing an overall decrease of 5.8 percentage points, a 25 percent decrease from the pre-period mean in the ACS.

Immigration enforcement activity directed against traffic offenders and those who have committed minor offenses has also been argued to heighten fear and impede participation in government-associated activities (HSAC Task Force 2011). Our third analysis, reported in column 2, finds that the effect of SC on take-up is larger where the ratio of non-violent detainers (often issued for traffic-related offenses) to violent detainers (often issued for assault or murder) is highest. Fourth, using the Pew data, we test whether reductions in program participation are higher in areas with increasing deportation fear measured at the Census division level (the finest geography available in 2013). We find that a one standard deviation increase in fear is associated with an additional one percentage point decline in food stamp take-up among citizen Hispanics after SC activation (column 3).

Fifth, we explore the role of sanctuary cities and counties. As described previously, sanctuary cities share in common their restrictions on how much local governments cooperate with ICE requests to detain unauthorized immigrants. If fear explains our findings, then Hispanic households in sanctuary cities should have less fear and thus exhibit a lower response to SC. Indeed, in column 4, when we interact our Hispanic and post-SC indicator with an indicator for whether a county has a sanctuary policy, we find that almost all of our main effects are driven by locations with no sanctuary policy, and a marginally significant and positive effect of SC activation on Hispanics in sanctuary cities.

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

\[45\] In the ACS data, we define mixed-status households based on if any member of the household is a non-citizen Hispanic, finding that 29 percent of Hispanics households in our ACS sample are mixed-status under this definition. Although less precise, we also find evidence consistent with fear explaining our effects on ACA take-up through exposure between citizen and non-citizen Hispanics. At the state economic area (SEA) level, we construct measures of exposure between Hispanic citizens and estimated unauthorized individuals in the urban economics literature (see Cutler, Glaeser, and Vigdor 1999). In our two-stage least squares specification in Appendix Table A8, we find suggestive evidence that greater SC intensity in areas where Hispanic citizens and non-citizens are more exposed to each other led to larger decreases in Hispanic ACA sign-up (p-value = 0.07).

\[46\] We are unable to explore the role of sanctuary cities in our ACA analysis as few states on the federal exchange are sanctuary jurisdictions.

\[47\] This result is robust to various definitions of a sanctuary jurisdiction (results available on request). See Online Appendix for institutional details of sanctuary cities.

25
that there is greater confusion or uncertainty about eligibility.

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

Column 6 of Table 2 presents these results in the PSID sample. In this sample, 45 percent of Hispanic heads are prior users of food stamps before SC activation. We present our main specification interacting our Hispanic times post-SC indicator with a dummy variable for prior users. We find that the decline in food stamp take-up post SC is largely driven by Hispanic heads that have previously taken up food stamps. Among prior users, SC activation reduced Hispanic heads of household take-up by an additional 18.8 percentage points relative to non-Hispanics, a 46 percent decrease from the pre-period mean. If we include all family members affected by SC from the PSID in our sample, thus reflecting the effect of SC on individuals rather than households, we have power to limit the sample exclusively to all individuals in households that have taken up food stamps prior to SC activation. In this prior users sample, we find that SC activation is associated with a 13.1 percentage point decline in take-up post-SC among individuals in Hispanic-headed households relative to non-Hispanic households (see column 5 of Appendix Table A5). These results suggest that our main findings are unlikely due to Hispanic households being less likely to receive information about public programs as their co-ethnics reduce sign up. This finding, combined with the heterogeneous effects described above, also lessens the likelihood that an explanation like stigma is driving our results.

C. Compositional Changes

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

To test this channel, Table 5 presents our main triple-differences specification in our high-participation food stamp sample in the ACS, where the dependent variables are average race-specific observable characteristics of citizens in each county-year, race-specific population count of citizens, and the percent Hispanic 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, or poverty level. We also find no change in the percent Hispanic within a county post-SC activation. In unreported results, we also find that ACS estimates of food stamp take-up are generally lower than available official yearly state-level estimates across all racial/ethnic groups. However, this reporting gap for all groups, particularly for Hispanics, does not change after SC activation. These results suggest that compositional changes and changes in reporting behavior are unlikely to explain our main findings.

VIII. Conclusion

In this study, we test the hypothesis that linkages between authorized and unauthorized individuals reduce safety net participation in the presence of enhanced immigration enforcement activity. Leveraging the roll-out and intensity of Secure Communities under the Obama Administration, we find that authorized Hispanic Americans are indeed sensitive to such enforcement although they themselves are not at risk of removal — a spillover effect. In particular, we find significant reductions in food stamp and ACA take-up among Hispanic households. We find evidence that our results may be driven by deportation fear rather than lack of benefit information or stigma. Mixed-status households, areas with higher exposure between authorized and unauthorized Hispanics, and areas with greater increases in deportation fear exhibit larger decreases in take-up in response to SC. In contrast, Hispanic households residing in sanctuary cities showed little response to SC activation.

Our results have several implications on health and well-being for Hispanic households. Extrapolating from the work of other scholars, families could experience adverse long-run consequences from forgoing benefits in response to stricter immigration enforcement. For example, Hoynes, Schanzenbach, and Almond (2016) show that food stamp take-up reduces the incidence of metabolic syndrome in adulthood. Tiehen et al. (2012) find that food stamp participation reduced the child poverty rate by 5.6 percent from 2000 to 2009. These results suggest that reductions in food stamp usage among Hispanics in response to immigration enforcement could have long-run consequences for health and economic security. Although the health effects of insurance are debated, there is evidence that it provides protection from medical debt and related financial crises (Courtemanche et al. 2017; Finkelstein et al. 2012).

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


of Immigrants in Everett, Massachusetts, USA.” Social Science and Medicine, 73(4): 586–594.


<table>
<thead>
<tr>
<th>Outcome</th>
<th>All</th>
<th>Hispanic-White</th>
<th>Hispanic-Black</th>
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<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
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<td>Share Food Stamp</td>
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<td>0.019</td>
<td>0.051***</td>
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<td></td>
<td>(0.208)</td>
<td>(0.014)</td>
<td>(0.016)</td>
</tr>
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<td>Average Family Size</td>
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<td>0.097</td>
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<td>(1.107)</td>
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<td>(0.107)</td>
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<td>Average # Children</td>
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<td>(0.628)</td>
<td>(0.038)</td>
<td>(0.058)</td>
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<td>3.994</td>
<td>8.248</td>
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<td></td>
<td>(87.011)</td>
<td>(6.566)</td>
<td>(7.964)</td>
</tr>
<tr>
<td>∆ Share Food Stamp</td>
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<td>0.004</td>
<td>0.047</td>
</tr>
<tr>
<td></td>
<td>(0.364)</td>
<td>(0.024)</td>
<td>(0.044)</td>
</tr>
<tr>
<td>∆ Average Family Size</td>
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<td>0.011</td>
<td>0.070</td>
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<tr>
<td></td>
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<td>(0.111)</td>
<td>(0.133)</td>
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<td>∆ Average # Children</td>
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<td>(0.088)</td>
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<td>(131.119)</td>
<td>(8.933)</td>
<td>(11.850)</td>
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Note: Data from the ACS from 2005–2007. Column 1 presents sample means of variables with standard deviations in parentheses. Columns 2 and 3 report coefficients from a balance test of the difference in our main outcomes on an indicator variable for “late” versus “early” activation counties, where late activation is defined as Secure Communities being activated after 2010. All regressions control for state-by-race and state-by-year fixed effects. Observations are weighted by the race-specific population in each county. Robust standard errors clustered at the county level are in parentheses.
Table 2: Triple Differences Estimation — Food Stamp Take-Up

<table>
<thead>
<tr>
<th>Sample</th>
<th>PSID Citizens (1)</th>
<th>ACS Citizens (2)</th>
<th>PSID Citizens (3)</th>
<th>ACS Citizens (4)</th>
<th>ACS Citizens Predicted (5)</th>
<th>PSID Citizens (6)</th>
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<tbody>
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<td>Hispanic × Post</td>
<td>-0.157**</td>
<td>-0.025***</td>
<td>-0.139**</td>
<td>-0.025***</td>
<td>-0.017***</td>
<td>-0.044</td>
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<td>(0.070)</td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.083)</td>
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<td>0.007</td>
<td>0.010***</td>
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<td></td>
<td>(0.042)</td>
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<td>(0.003)</td>
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<tr>
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<td>(0.040)</td>
<td>(0.004)</td>
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<tr>
<td>Hispanic × Post × Prior User</td>
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<td>State-Year, State-Race, County</td>
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<td>Observations</td>
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<td>7,756</td>
<td>90,661</td>
<td>89,536</td>
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<tr>
<td>Number Clusters</td>
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<td>626</td>
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<td>626</td>
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</table>

Note: Data from PSID from 2005–2015 and ACS from 2006–2016. The data are limited to heads of households with less than a high school degree, our high participation sample. The citizens sample in the PSID includes heads of households that grew up in the United States. The citizens sample in the ACS includes heads of households that are U.S. citizens. Prior users in the PSID includes all heads of households that had previously taken up food stamps prior to SC activation. Column 5 estimates our main specification instrumenting for actual activation using predicted activation. Baseline controls in the PSID include sex of household head, marital status, family size, number of children, age of youngest child, income relative to federal poverty line, industry, employment status, and FBI crime decile-by-race fixed effects. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, state-by-year fixed effects, state-by-race fixed effects, percent Hispanic, and race-by-state changes in employment during the Great Recession. Observations in the PSID are weighted by the PSID family weight. Observations in the ACS are weighted by the race-specific population in each county. Robust standard errors are clustered at the county level.
Table 3: OLS and 2SLS Results — ACA Take-Up

<table>
<thead>
<tr>
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<th>(1)</th>
<th>(2)</th>
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<tr>
<td><strong>Panel A: OLS Results</strong></td>
<td></td>
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<tr>
<td>Share Hispanic</td>
<td>$-0.022^{***}$</td>
<td>$-0.016^{***}$</td>
<td>$-0.014^{***}$</td>
<td>$-0.012^{***}$</td>
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<tr>
<td>Detainers</td>
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<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
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<tr>
<td><strong>Panel B: First Stage</strong></td>
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<td>Shift-Share IV</td>
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<td>0.237^{***}</td>
<td>0.234^{***}</td>
<td>0.226^{***}</td>
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<td></td>
<td>(0.064)</td>
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<td>(0.061)</td>
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<td><strong>Panel C: 2SLS Results</strong></td>
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<td>Share Hispanic</td>
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<td>(0.151)</td>
<td>(0.148)</td>
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F-Statistic 16.01 13.91 14.51 13.24
Fixed Effects
Controls Baseline (1) (2) (3)
+ Pov, Politics + UR, Crime + Bl ACA
Observations 1,882 1,882 1,882 1,882

Note: Data from the ACA and CMS in the 37 states with federal exchanges. The dependent variable is the share of eligible Hispanics that sign up for the ACA in each county. All specifications contain state fixed effects. Baseline county-level controls include share Hispanic males. Column 2 adds controls for the Democratic versus Republican vote margin in the 2008 presidential election and the share of Hispanics in poverty. Column 3 adds the unemployment rate and crime rate. Column 4 adds share black ACA sign-up. Observations are weighted by the estimated number of Hispanics eligible for the ACA in each county. Robust standard errors are in parentheses.
Table 4: Triple Differences Estimation — Food Stamp Take-Up Heterogeneity

<table>
<thead>
<tr>
<th>Sample</th>
<th>ACS Citizens (1)</th>
<th>ACS Citizens (2)</th>
<th>ACS Citizens (3)</th>
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<td>Hispanic × Mixed × Post</td>
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<td>Hispanic × Post × Petty Severe Ratio</td>
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<td>−0.019***</td>
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<td>−0.150**</td>
</tr>
<tr>
<td>Hispanic × Post × Δ Pew Fear</td>
<td></td>
<td></td>
<td></td>
<td>0.015*</td>
</tr>
<tr>
<td>Hispanic × Post × Sanctuary City</td>
<td></td>
<td></td>
<td></td>
<td>(0.008)</td>
</tr>
<tr>
<td>Pre-Period Hispanic Mean</td>
<td>0.230</td>
<td>0.230</td>
<td>0.230</td>
<td>0.230</td>
</tr>
<tr>
<td>Fixed Effects</td>
<td>State-Year, State-Race, County</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Baseline Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>90,661</td>
<td>90,661</td>
<td>79,590</td>
<td>90,661</td>
</tr>
<tr>
<td>Number Clusters</td>
<td>3,079</td>
<td>3,079</td>
<td>3,079</td>
<td>3,079</td>
</tr>
</tbody>
</table>

Note: Data from ACS from 2006-2016. The data are limited to heads of households with less than a high school degree, our high participation sample. The citizens sample in the ACS includes heads of households that are U.S. citizens. Mixed status in the ACS is defined as a Hispanic citizen head of household with any family member that is a Hispanic non-citizen. The ratio of petty versus severe detainers measures the ratio of detainers issued for minor offenses like traffic infractions to serious violent offenses. Δ Pew Fear is measured as the change in the share that are worried a family member or close friend could be deported between 2013 and 2010 from Pew. This measure is defined at the Census Division level. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, state-by-year fixed effects, state-by-race fixed effects, percent Hispanic, 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.
Table 5: Compositional Changes

<table>
<thead>
<tr>
<th>Outcome</th>
<th># Children (1)</th>
<th>Family Size (2)</th>
<th>Poverty FPL (3)</th>
<th>Pop. Count (4)</th>
<th>% Hispanic (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hispanic × Post</td>
<td>−0.001</td>
<td>−0.020</td>
<td>2.343</td>
<td>205.096***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.015)</td>
<td>(1.957)</td>
<td>(35.712)</td>
<td></td>
</tr>
<tr>
<td>Post</td>
<td>−0.009*</td>
<td>0.026***</td>
<td>−3.017*</td>
<td>−270.243***</td>
<td>−0.0005</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.008)</td>
<td>(1.606)</td>
<td>(36.141)</td>
<td>(0.004)</td>
</tr>
</tbody>
</table>

Fixed Effects
Baseline Controls: Yes, Yes, Yes, Yes, Yes
State-Year, State-Race, County
Observations: 101,149, 101,149, 101,149, 101,589, 33,852

Note: Data from ACS from 2006–2016. Baseline controls include FBI crime decile-by-race fixed effects and, in col (1) poverty and family size, in col (2) poverty and number children, and col (3) number children and family size. All regressions control for county fixed effects, state-by-year fixed effects, state-by-race fixed effects, percent Hispanic, and race-by-state changes in employment during the Great Recession. Observations are weighted by the race-specific population in each county. Robust standard errors are clustered at the county level.
Figure 1: Secure Communities Activation

Note: Data from FOIA.
Figure 2: 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 3: Detainers and Shift-Share IV

Panel A: Share Hispanic Detainers

Panel B: Shift-Share IV

Note: Data from FOIA.
Note: Data from FOIA, ACS, ACA. The shift-share instrument is constructed as the total predicted number of detainers normalized by the predicted number of undocumented Hispanics based on data from the American Community Survey. This figure represents OLS regressions of each baseline characteristic on our shift-share instrument. All specifications contain state fixed effects and share Hispanic males. Observations are weighted by the estimated number of Hispanics eligible for the ACA in each county. The reported joint p-value is from a seeming unrelated regression.
Figure 5: Event Study of Food Stamp Take-Up

Panel A: Non-Hispanic Whites
Panel B: Non-Hispanic Blacks
Panel C: Hispanics

Note: Data from ACS from 2006–2016. The data are limited to heads of households with less than a high school degree, our high participation sample. The citizens sample in the ACS includes heads of households that are U.S. citizens. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, state-by-year fixed effects, state-by-race fixed effects, percent Hispanic, and race-by-state changes in employment during the Great Recession. Observations in the ACS are weighted by the race-specific population in each county. Robust standard errors are clustered at the county level.
Figure 6: Correlation between Fear and Knowing Someone Detained

Note: Data from Pew Hispanic Survey 2010. The sample excludes non-citizens and states with five or fewer respondents. Fear refers to fear that a family member or close contact will be deported. The knowledge measure refers to the share of people responding affirmatively that they know someone who has been detained or deported. The size of the bubble represents the size of the Hispanic population. The correlation between share fear and share know detained is 0.50. The 45° line is drawn for reference.
Figure 7: Google Deportation Searches Event Study

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

<table>
<thead>
<tr>
<th>Outcome</th>
<th>All Hispanic-White</th>
<th>Hispanic-Black</th>
<th>PSID Sample N = 16,773</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Share Food Stamp</td>
<td>0.237</td>
<td>-0.151</td>
<td>0.872***</td>
</tr>
<tr>
<td></td>
<td>(0.377)</td>
<td>(0.541)</td>
<td>(0.241)</td>
</tr>
<tr>
<td>Average Family Size</td>
<td>2.812</td>
<td>0.751</td>
<td>-0.968</td>
</tr>
<tr>
<td></td>
<td>(1.452)</td>
<td>(2.563)</td>
<td>(0.726)</td>
</tr>
<tr>
<td>Average # Children</td>
<td>0.954</td>
<td>0.458</td>
<td>-0.267</td>
</tr>
<tr>
<td></td>
<td>(1.179)</td>
<td>(1.928)</td>
<td>(0.521)</td>
</tr>
<tr>
<td>Poverty FPL</td>
<td>245.144</td>
<td>211.300</td>
<td>-99.430</td>
</tr>
<tr>
<td></td>
<td>(193.388)</td>
<td>(136.500)</td>
<td>(67.220)</td>
</tr>
<tr>
<td>△ Share Food Stamp</td>
<td>0.071</td>
<td>0.122</td>
<td>0.013</td>
</tr>
<tr>
<td></td>
<td>(0.417)</td>
<td>(0.687)</td>
<td>(0.189)</td>
</tr>
<tr>
<td>△ Average Family Size</td>
<td>-0.391</td>
<td>0.069</td>
<td>-0.749</td>
</tr>
<tr>
<td></td>
<td>(1.453)</td>
<td>(0.474)</td>
<td>(0.494)</td>
</tr>
<tr>
<td>△ Average # Children</td>
<td>-0.428</td>
<td>-1.061*</td>
<td>-0.483</td>
</tr>
<tr>
<td></td>
<td>(1.150)</td>
<td>(0.601)</td>
<td>(0.333)</td>
</tr>
<tr>
<td>△ Poverty FPL</td>
<td>19.121</td>
<td>247.400</td>
<td>284.500***</td>
</tr>
<tr>
<td></td>
<td>(135.953)</td>
<td>(266.700)</td>
<td>(4.127)</td>
</tr>
</tbody>
</table>

Note: Column 1 presents weighted sample means of variables with standard deviations in parentheses. Columns 2 and 3 report coefficients from a balance test of the difference in our main outcomes on an indicator variable for “late” versus “early” activation counties, where late activation is defined as Secure Communities being activated after 2010. All regressions control for state-by-race and state-by-year fixed effects. Robust standard errors clustered at the county level are in parentheses.
Appendix Table A2: Effect of SC on Arrests and Immigration Enforcement

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Offenses Known (1)</th>
<th>Arrests (2)</th>
<th>Submissions (3)</th>
<th>Matches (4)</th>
<th>Detainers (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post</td>
<td>-54.92</td>
<td>60.66</td>
<td>6183.65***</td>
<td>336.60***</td>
<td>165.56***</td>
</tr>
<tr>
<td></td>
<td>(132.80)</td>
<td>(62.24)</td>
<td>(806.50)</td>
<td>(81.47)</td>
<td>(33.80)</td>
</tr>
<tr>
<td>Pre-Period Mean</td>
<td>3888.22</td>
<td>1032.03</td>
<td>287.47</td>
<td>15.21</td>
<td>35.43</td>
</tr>
<tr>
<td>Fixed Effects</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>34,210</td>
<td>34,210</td>
<td>31,110</td>
<td>31,110</td>
<td>34,210</td>
</tr>
<tr>
<td>Number Clusters</td>
<td>3,110</td>
<td>3,110</td>
<td>3,110</td>
<td>3,110</td>
<td>3,110</td>
</tr>
</tbody>
</table>

Note: Data on offenses known to law enforcement and offense cleared by arrest are from UCR from 2005-2015. Data on fingerprint submissions, matches, and detainers are from FOIA requests to ICE from 2006-2014. All regressions control for county fixed effects and state-by-year fixed effects. Robust standard errors are clustered at the county level.
## Appendix Table A3: Main Results on Food Stamp Take-Up — Alternative Samples

<table>
<thead>
<tr>
<th>Sample</th>
<th>Baseline</th>
<th>Alternative Samples</th>
<th>Spatial Log</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>PSID</td>
<td>ACS Citizens</td>
<td>ACS</td>
</tr>
<tr>
<td></td>
<td>Citizens</td>
<td>Match PSID</td>
<td>HR Female</td>
</tr>
<tr>
<td>Hispanic × Post</td>
<td>−0.157**</td>
<td>−0.025***</td>
<td>−0.050***</td>
</tr>
<tr>
<td></td>
<td>(0.069)</td>
<td>(0.011)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Post</td>
<td>0.028</td>
<td>0.010***</td>
<td>0.013</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.003)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Baseline Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>7,756</td>
<td>90,661</td>
<td>996</td>
</tr>
</tbody>
</table>

Note: In columns 1 and 2, we present our main specifications on food stamp take-up from the PSID and ACS. In column 3, we estimate our main specification in the ACS using a sample that more closely approximates the PSID sample. In column 4, we estimate our main specification in the ACS using a sample of highest-ranking females (either female head of household or female spouse). In column 5, we estimate our main specification in the ACS using a sample of Hispanic citizen heads of households excluding naturalized citizens. In column 6, we estimate our main specification in the ACS using a sample of Hispanic citizen heads of households excluding families that are mixed-status. In column 7, we estimate our main specification in the ACS controlling for a spatial lag in SC activation using an exponential model with distance decay parameter of 0.05 km. Baseline controls in the PSID include sex of household head, marital status, family size, number of children, age of youngest child, income relative to federal poverty line, industry, employment status, and FBI crime decile-by-race fixed effects. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, state-by-year fixed effects, state-by-race fixed effects, percent Hispanic, and race-by-state changes in employment during the Great Recession. Observations in the PSID are weighted by the PSID family weight. Observations in the ACS are weighted by the race-specific population in each county. Robust standard errors are clustered at the county level.
Appendix Table A4: Main Results on Food Stamp Take-Up — Race Specific Comparisons

<table>
<thead>
<tr>
<th>Sample</th>
<th>PSID Citizens</th>
<th>PSID Citizens</th>
<th>ACS Citizens</th>
<th>ACS Citizens</th>
<th>ACS Citizens</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>Hispanic × Post</td>
<td>−0.173**</td>
<td>−0.148*</td>
<td>−0.021***</td>
<td>−0.026***</td>
<td>−0.026***</td>
</tr>
<tr>
<td></td>
<td>(0.084)</td>
<td>(0.080)</td>
<td>(0.006)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Post</td>
<td>0.066</td>
<td>−0.055</td>
<td>0.013**</td>
<td>0.011***</td>
<td>0.010***</td>
</tr>
<tr>
<td></td>
<td>(0.082)</td>
<td>(0.050)</td>
<td>(0.006)</td>
<td>(0.004)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Puerto Rican × Post</td>
<td></td>
<td></td>
<td></td>
<td>0.002</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Cuban × Post</td>
<td></td>
<td></td>
<td></td>
<td>−0.008</td>
<td>(0.015)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Comparison</td>
<td>Hisp/Black</td>
<td>Hisp/White</td>
<td>Hisp/Black</td>
<td>Hisp/White</td>
<td>All</td>
</tr>
<tr>
<td>Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>Baseline Controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>4,966</td>
<td>3,453</td>
<td>56,799</td>
<td>62,751</td>
<td>100,727</td>
</tr>
</tbody>
</table>

Note: In this table, we replicate our main results comparing Hispanics to each race group. Baseline controls in the PSID include sex of household head, marital status, family size, number of children, age of youngest child, income relative to federal poverty line, industry, employment status, and FBI crime decile-by-race fixed effects. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, state-by-year fixed effects, state-by-race fixed effects, percent Hispanic, and race-by-state changes in employment during the Great Recession. Observations in the PSID are weighted by the PSID family weight. Observations in the ACS are weighted by the race-specific population in each county. Robust standard errors are clustered at the county level.
Appendix Table A5: Main Results on Food Stamp Take-Up — Alternative Weighting

<table>
<thead>
<tr>
<th>Sample</th>
<th>No Weights</th>
<th>Individual-Level</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ACS Citizens</td>
<td>ACS # Hisp &gt; 25</td>
</tr>
<tr>
<td>Hispanic × Post</td>
<td>-0.008 (0.005)</td>
<td>-0.010** (0.005)</td>
</tr>
<tr>
<td>Post</td>
<td>0.016*** (0.006)</td>
<td>0.017*** (0.006)</td>
</tr>
<tr>
<td>Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Baseline Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>90,865</td>
<td>89,209</td>
</tr>
</tbody>
</table>

Note: Column 1 estimates our main results in the ACS with no weights. Column 2 estimates our main results in the ACS with weights using one observation per person in each household. Column 3 estimates our main results in the PSID with weights using one observation per person in each household. Column 4 estimates our main results in the PSID with weights using one observation per person in each household, limited to households that had previously taken up food stamps prior to SC activation. Baseline controls in the PSID include sex of household head, marital status, family size, number of children, age of youngest child, income relative to federal poverty line, industry, employment status, and FBI crime decile-by-race fixed effects. Baseline controls in the ACS include mean family size, number of children, poverty, and FBI crime decile-by-race fixed effects. All regressions control for county fixed effects, state-year fixed effects, state-by-race fixed effects, percent Hispanic, and race-by-state changes in employment during the Great Recession. Robust standard errors are clustered at the county level.
Appendix Table A6: 2SLS Results — ACA Take-Up Robustness

<table>
<thead>
<tr>
<th>Control</th>
<th>287(g)</th>
<th>CHC</th>
<th>Navigator</th>
<th>All (1-3)</th>
<th>Spatial Lag</th>
<th>FB Shares</th>
</tr>
</thead>
<tbody>
<tr>
<td>Share Hispanic</td>
<td>−0.411**</td>
<td>−0.368**</td>
<td>−0.358**</td>
<td>−0.400**</td>
<td>−0.383**</td>
<td>−0.375**</td>
</tr>
<tr>
<td>Detainers</td>
<td>(0.178)</td>
<td>(0.152)</td>
<td>(0.171)</td>
<td>(0.199)</td>
<td>(0.156)</td>
<td>(0.173)</td>
</tr>
<tr>
<td>Fixed Effects Controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>State</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>1,882</td>
<td>1,882</td>
<td>1,882</td>
<td>1,882</td>
<td>1,882</td>
<td>1,882</td>
</tr>
</tbody>
</table>

Note: The dependent variable is the share of eligible Hispanics that sign up for the ACA. Column 1 adds a control for whether a county had a 287(g) agreement with ICE. Column 2 adds a control for whether a county had a community health center. Column 3 adds a control for whether a county had a Hispanic health navigator. Column 4 adds all three controls. Column 5 adds a spatial lag for detainer intensity using an exponential model with distance decay parameter of 0.05 km. Column 6 controls for 2005-2009 share foreign born from each Hispanic country of origin. All regressions control for state fixed effects, share Hispanic males, the Democratic versus Republican vote margin in the 2008 presidential election, share of Hispanics in poverty, the unemployment rate, crime rate, share black ACA sign-up, and missing indicators for these variables. Observations are weighted by the estimated number of Hispanics eligible for the ACA in each county. Robust standard errors are in parentheses.
<table>
<thead>
<tr>
<th>Outcome</th>
<th>Share Black ACA</th>
<th>Share White ACA</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS (1)</td>
<td>2SLS (2)</td>
</tr>
<tr>
<td>Share Hispanic</td>
<td>-0.019***</td>
<td>-0.322</td>
</tr>
<tr>
<td>Detainers</td>
<td>(0.006)</td>
<td>(0.257)</td>
</tr>
<tr>
<td>F-Statistic</td>
<td>5.32</td>
<td></td>
</tr>
<tr>
<td>Fixed Effects</td>
<td>Baseline, Pov, Politics, UR, Crime, Hisp ACA</td>
<td>State</td>
</tr>
<tr>
<td>Observations</td>
<td>1,816</td>
<td>1,816</td>
</tr>
</tbody>
</table>

Note: The dependent variable in columns 1–2 is the share of eligible blacks signing up for the ACA. The dependent variable in columns 3–4 is the share of eligible whites signing up for the ACA. All regressions control for state fixed effects, share Hispanic males, the Democratic versus Republican vote margin in the 2008 presidential election, share of Hispanics in poverty, the unemployment rate, crime rate, share black ACA sign-up, and missing indicators for these variables. Observations are weighted by the estimated number of blacks (or non-Hispanic whites) eligible for the ACA in each county. Robust standard errors are in parentheses.
## Appendix Table A8: 2SLS Results – ACA Take-Up Heterogeneity

<table>
<thead>
<tr>
<th></th>
<th>OLS</th>
<th>2SLS</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Share Hispanic Detainers</td>
<td>−0.010** (0.005)</td>
<td>−0.117 (0.075)</td>
</tr>
<tr>
<td>Share Hispanic Detainers × Exposure</td>
<td>−0.001 (0.007)</td>
<td>−0.377* (0.219)</td>
</tr>
<tr>
<td>F-Statistic</td>
<td>5.24</td>
<td></td>
</tr>
<tr>
<td>Fixed Effects</td>
<td>State</td>
<td></td>
</tr>
<tr>
<td>Controls</td>
<td>All</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>1,882</td>
<td>1,882</td>
</tr>
</tbody>
</table>

Note: Data from ACA. Exposure is measured as the likelihood of interaction/mixing between authorized and unauthorized Hispanics. All regressions control for state fixed effects, share Hispanic males, the Democratic versus Republican vote margin in the 2008 presidential election, share of Hispanics in poverty, the unemployment rate, crime rate, share black ACA sign-up, missing indicators for these variables, as well as the main effect of exposure. Observations are weighted by the estimated number of Hispanics eligible for the ACA in each county. Robust standard errors are in parentheses.
Appendix Figure A1: California SNAP Application

Note: Data from section of California SNAP Application.
Appendix Figure A2: ACA Application

Note: Data from section of ACA Application from CMS.gov.
Appendix Figure A3

\[ \epsilon_{\text{pre-}SC, \lambda_U} = \bar{\gamma}_l - \beta_1 \cdot D_l \]

Share Non-Participation = \( 1 - F(\epsilon^*_l) \)

- Distribution of \( \epsilon_l \) -
Appendix Figure A4: Permutation Tests

Food Stamp Take-Up

Note: Data from ACS. These figures represent empirical distributions of our estimate of interest when we randomly permute activation years to each county. The red line denotes our actual coefficient along with the corresponding two-sided empirical p-value. The data are limited to actual SC pre-activation years.
Note: Data from ACS and Pew Research Center. This figure presents the correlation between our state-level estimates on the number of unauthorized Hispanics and estimates from the Pew Research Center. The correlation between the two measures is greater than 0.95.
Appendix Figure A6: Detainers Event Study

Note: Data from FOIA. This figure represents event study estimates of the time to SC activation on the log number of detainers issued. All specifications control for county fixed effects. Standard errors are clustered at the county level.
Appendix Figure A7: Predicted Secure Communities Activation

Note: Data from FOIA and ICE documentation.