Tightening Immigration Policy: Unintended Consequences on Hispanic U.S. Citizens' Health Behavior and Status

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Abstract

This paper investigates the spillover effects of immigration enforcement policies on the demand for health of people who are not directly targeted by such policies but are nonetheless culturally linked to unauthorized individuals. More specifically, I consider one U.S. policy, "Secure Communities," to study its indirect effects on the healthcare-seeking behavior and health status of Hispanic U.S. citizens. By exploiting the staggered rollout of this policy within an event-study and a triple-difference framework, I find that, relative to non-Hispanic whites, Hispanics are less likely to seek healthcare and have worse physical and mental health status following the activation of Secure Communities. Preliminary analysis suggests that negative spillover effects are mostly driven by psychological rather than income or general equilibrium effects.

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1 Introduction

At the start of 2017, the Trump administration reactivated *Secure Communities*, an immigration enforcement program that was piloted by the Bush administration in 2008 and then pursued and expanded by the Obama administration until 2014 (TRAC Immigration, 2020). While this program is clearly targeted at *unauthorized* immigrants in the U.S., it is less clear how it indirectly affects *American citizens* who are linked to these communities of unauthorized immigrants. Of particular interest is the Hispanic community, since it is the most exposed, with approximately three-quarters of the unauthorized population in the U.S. coming from Latin America today (Migration Policy Institute, 2020). In fact, 93 percent of deported[1] individuals through Secure Communities between 2008 and 2014 were Latin American (East, Luck, Mansour, and Velásquez, 2019), with a large majority being Mexican (see Figure 1). A 2011 report indicates that almost 40 percent of the individuals who were arrested[2] through Secure Communities reported that they had a U.S. citizen child or spouse. That figure implies that even within only three years of implementation, approximately 88,000 families with U.S. citizen members had been impacted by that program (Kohli, Markowitz, and Chávez, 2011).

In this paper, I examine the indirect, unintended effects of the enforcement of U.S. immigration laws on the health behavior and status of its Hispanic *citizens*.[3] Even though American citizens are not subject to deportation, having members of their ethnic community who are at risk of deportation may constitute an important source of stress for them. In particular, they may fear that their own behavior and interactions with governmental institutions and employees might reveal their social network and ultimately lead to the deportation of the undocumented members of their community. For example, there exists suggestive evidence from survey data that Hispanics are less willing to engage with health-care providers, police, and educators when they are "primed" with immigration issues (Pedraza and Osorio, 2017; Pedraza, Nichols, and LeBrón, 2017). For some Hispanic families, the existence of that risk may lead them to give up on public benefit programs for which they are eligible, such as the food stamp program (SNAP) or the Supplemental Security Income (SSI) program (Alsan and Yang, 2019). Moreover, tightening immigration policies could not only have direct negative income effects (due to the deportation of income earners) but also indirect employment effects on Americans, which may be positive

[1]Throughout the paper, I use the terms "deported" and "removed" interchangeably.

[2]Note that an arrest does not necessarily lead to a deportation – see Section 2 for more detail.

[3]In this version of the paper, I focus on the Secure Communities program, but in the future I would like to extend the analysis to more recent periods and take into account *all* removals (not just those that happened under Secure Communities).

or negative depending on the local production function.[4] Taken together, the potential indirect negative effects on Hispanic U.S. citizens may lower their income and change their healthcare-seeking behavior, which might consequently worsen their health status. The negative effects on health behavior also create negative externalities – they have a high social cost because indirectly affected individuals stop seeking preventive care. However, one could also find positive indirect effects – e.g., because of immigration enforcement programs, unlawfully present immigrants who are eligible for the U.S. citizenship may be (more) incentivized to apply for it and, if they do get the citizenship, then get access to (additional) health benefits.

To study the extent to which tightening immigration policy indirectly affects the health behavior and status of Hispanic U.S. citizens, I exploit the (non-random) staggered rollout across counties of the Secure Communities program as a quasi-natural experiment within a triple-difference framework, which I also embed into an event-study analysis. Secure Communities is an immigration enforcement program that was implemented by the U.S. Immigration and Customs Enforcement (ICE) agency between 2008 and 2013, and was later on reactivated in 2017. It essentially increases the likelihood of being deported for an immigrant who is arrested while being unlawfully present on U.S. soil, by allowing ICE to check via a fingerprint analysis the immigration status of anyone arrested by local enforcement agencies.

The triple-difference approach I use consists in comparing outcomes (i.e., measures of healthcare-seeking behavior and health status) for Hispanics to those for non-Hispanic white individuals, in counties that have versus those that have not (yet) implemented Secure Communities, before versus after its activation. For this empirical strategy to be valid, the key identifying assumption is the parallel-trend assumption – i.e., that there are no county-specific shocks that both coincide in timing with the staggered expansion of Secure Communities *and* influence the dynamic path of health outcomes only for Hispanics while sparing non-Hispanics (and vice versa). In other words, I assume that in the absence of the Secure Communities program, the outcomes would have evolved similarly for both Hispanics and whites[5]. I embed this triple-difference strategy within an event-study framework to see if there is any pre-trend, which would threaten my identifying assumption. I also per-

[4]The overall effect takes into account the resulting compositional change in the local labor market and general equilibrium effects. In particular, the effect on Americans is more negative if (i) there is a high degree of complementarity between U.S. workers and undocumented workers who participate in the U.S. labor force, (ii) employers expect an increase in labor cost and thus reduce their labor demand, and if (iii) as a consequence, the aggregate demand falls (East, Luck, Mansour, and Velásquez, 2019).

[5]Throughout this paper, I use "whites" and "Blacks" to refer to "non-Hispanic whites" and "non-Hispanic Blacks," respectively. Note also that I capitalize "Black" following the writing style of the New York Times (2020).

form a triple-difference analysis separately to quantify the overall average effect of Secure Communities. In addition, I look at the effect on Black individuals (relative to white individuals). This group could be either considered as a placebo group (as one may argue that this group's health outcomes should not be affected by the immigration enforcement program), or as a another group that is indirectly affected by, say, a higher degree/frequency of police control following the implementation of the immigration enforcement program.

To conduct my empirical analysis, I combine immigration enforcement data from the Transactional Records Access Clearinghouse (TRAC) and ICE with individual-level self-reported health data from the Behavioral Risk Factor Surveillance System (BRFSS) and the Current Population Survey (CPS). The combined dataset enables me to exploit the county-by-county rollout of Secure Communities and examine the effect of immigration enforcement on various measures of healthcare-seeking behavior and health status of Hispanic individuals, relative to non-Hispanic white individuals. In my econometric specifications, I also include various county-level economic, political and geographic control variables to reduce the threat of omitted-variable bias and increase the precision of my estimates (see Section 4.1 for details on the data sources).

The preliminary results indicate that Secure Communities has negative spillover effects on the healthcare-seeking behavior of Hispanic individuals. In particular, the triple-difference analysis suggests that after the activation of Secure Communities (SC), Hispanics are 3 percentage points (pp) less likely to have completed a routine checkup in the past 12 months, compared to their white counterparts. This decrease corresponds to a 5-percent decrease relative to the pre-SC period. Although respondents report that medical costs constitute a barrier to seeing a doctor, the effect is not large enough to fully explain the reduction in the frequency of routine checkups. This decrease cannot be explained by the fact that they have lost their job (and any associated health coverage) either, as my results indicate that, if anything, Hispanics are more likely to have a job in the post-SC period.

In addition, I find suggestive evidence of negative effects on their health status. For example, relative to whites, Hispanics report drinking alcohol on more days (+3 percent with respect to the pre-SC period). They also tend to be anxious on more days (+40 percent compared to the pre-SC period) and are more likely to feel monthly physical symptoms due to treatment based on their race (+43 percent post-SC period).

Taken together, these empirical findings are consistent with the hypothesis that Secure Communities have negative psychological effects on Hispanics. Enforcing this immigration policy not only deteriorates their health status but also leads them to less often seek health care, which generates a vicious circle.

The main contribution of this project to the literature is twofold. First, it adds to the literature that examines the effects of immigration enforcement. Several past studies have investigated the effects of immigration policies on *the general U.S. population*. For example, prior research has also exploited Secure Communities to evaluate its impact on crime (Miles and Cox, 2014), (mis)trust in government as a source of health information among Latinos (Cruz Nichols, LeBrón, and Pedraza, 2018), the academic achievement of Hispanic and Black students (Bellows, 2019), and employment (East, Luck, Mansour, and Velásquez, 2019).

A couple of other studies have looked at the effects of immigration policies on *non-U.S. citizens*. For instance, Vargas and Pirog (2016) finds a negative relationship between risk of deportation and participation in the Women, Infants, and Children (WIC) program among non-citizen mothers of U.S.-born children, despite the fact that unauthorized immigrants are eligible for it and that their children are U.S. citizens. Another study by Wang and Kaushal (2019) uses a difference-in-differences strategy and notably find that, following the activation of Secure Communities, the share of foreign-born Latino immigrants who live with at least one non-U.S. citizen family member and who report experiencing mental health distress increased by approximately 15 percent (+2.2 pp). Using a difference-in-differences design, Giuntella and Lonsky (2020) analyze the health impacts of the Deferred Action for Childhood Arrivals (DACA) initiative[6]. Their results indicate that DACA-eligible individuals are more likely to have health insurance coverage and report having better health, but they are not more likely to use healthcare services, except for mental health services.

By contrast, my paper relates to the more recent and thin body of work that examines the negative spillover effects of immigration enforcement policies on *U.S. citizens*. Watson (2014) examines the effect of immigration enforcement on Medicaid participation for children who are themselves U.S. citizens but whose parents are non-U.S. citizens. She finds a decrease in Medicaid participation. Closely related to my study is the work of Alsan and Yang (2019), which uses a similar empirical strategy to evaluate the spillover effects of Secure communities on SNAP participation and SSI enrollment of Hispanic U.S. citizens. The authors find a substantial negative effect, which they attribute to fear of deportation of other members of their own community. My study, however, takes a step further and seeks to document the consequences on the health of Hispanic U.S. citizens. Beyond the fact that these individuals reduce their participation in safety net programs, it appears important to

[6]DACA is an immigration policy that was implemented in August 2012 – it allows some individuals with unlawful presence in the U.S. after being brought to the country as children to receive a renewable two-year period of deferred action from deportation and become eligible for a work permit in the U.S..

quantify the health impact more directly. Finding a non-negligible negative health effect would have important policy implications, if only because it would mean that the costs of such immigration policy are underestimated when they fail to take into account the associated negative externalities on populations that were not supposed to be targeted. To my knowledge, my paper is the first to provide arguably causal estimates of the unintended spillover effects of immigration enforcement on the health behavior and status of Hispanic U.S. citizens.

Second, this paper contributes to the literature by attempting to disentangle the different mechanisms underlying the potential negative health impact that strict immigration policies may have. As fear appears to be an important channel, my study adds to the broader literature on the impact of fear on individuals' delay regarding healthcare-seeking behavior (Dubayova et al., 2010) and on their health status (Meng et al., 2007; Nickerson et al., 2010). Beyond fear, there could also be effects through mental health (e.g., stress, anxiety, depression) and health-related behaviors (e.g., smoking, drinking, etc.).

The remainder of this paper is organized as follows. Section 2 provides an overview of the Secure Communities program. Section 3 explains the conceptual framework that underlies my analysis. Section 4 presents the data and the identification strategy that are used to perform my empirical analysis. Section 5 presents some preliminary results from my empirical analysis. Section 6 provides some concluding remarks and lays out the next steps suggested to pursue this research project.

2 Background: Secure Communities

Secure Communities is an immigration enforcement program that was in place between 2008 and 2014, and later on reactivated in 2017 (see Miles and Cox (2014), Alsan and Yang (2019), and/or Bellows (2019) for further detail on that deportation policy). As depicted in Appendix Figure A.1, the implementation period of this program coincided with a significant increase in removals, both at the border and in the U.S. interior.

The program is administered by the Immigration and Customs Enforcement (ICE) and is designed to facilitate the arrest and removal of individuals who are unlawfully present on U.S. soil and are thus violating the federal immigration laws. It increases the degree of cooperation and information sharing between the federal government – namely, the Federal Bureau of Investigation (FBI) and the Department of Homeland Security (DHS) – and local police forces.

Specifically, prior to Secure Communities, when an individual was arrested, their fingerprints were taken and sent to the FBI for a background check. Performing those background checks was quite labor intensive because unlawfully present immigrants were identified via inmate interviews, which were conducted by authorized federal or local officers in jails or prisons. As a result, not only could these authorized officers manage to screen less than fifteen percent of local jails and prisons, but the interviews were also conducted in only about two percent of all U.S. counties.

Under Secure Communities, arrested individuals' fingerprints are not only sent to the FBI but also to DHS. There, they are compared to the DHS Automated Biometric Identification System (IDENT), a database that contains biographical and biometric information on foreign-born individuals. That database categorizes these individuals into three groups: (i) non-U.S. citizens who have violated the U.S. immigration laws, such as individuals who overstayed their visas and/or have already been deported; (ii) non-U.S. citizens who are lawfully present in the U.S. but who could be deported if they are convicted of the crime that led to their arrest; and (iii) citizens who have been naturalized after their fingerprints had been registered into the database. It contains information on visa applicants, naturalized citizens, as well as individuals who have violated immigration law, are lawfully present but were convicted of a crime, and traveled through ports. The creation of this database dates back to 1994; its initial purpose is to help U.S. border and immigration forces prevent the entry of criminals and terrorists in the U.S..

In the case of a fingerprint match, local ICE officers can then determine whether the arrested individual should be deported, based on both the biometric confirmation and the other available evidence. If they believe that the individual is removable, they issue a "detainer" or "immigration hold" on that person. As a result, the individual is held for up to 48 hours to enable ICE to assume custody for the initiation of removal proceedings. Appendix Figures A.1 and A.2 show that, during the period over which Secure Communities is implemented, the number of detainer requests evolves similarly to the number of removals.

According to TRAC Immigration (2020), 20 percent of deported individuals had not been convicted of any crime, 26 percent were not convicted of serious crimes, and 82 percent were convicted for non-violent crimes. In most cases, the individuals who were detained by ICE were subject to immigration enforcement action for reasons that were unrelated to the motive(s) of their arrest (Kohli, Markowitz, and Chávez, 2011) – e.g., the fingerprint match indicated that the person was removable because s/he overstayed a visa, or entered the country illegally.

Although the policy is federal, its implementation was local. The program was first piloted in 2008 and then progressively rolled out across U.S. counties. ICE completed the full implementation to all the jurisdictions within 50 states, the District of Columbia, and five U.S. Territories at the end of January 2013 (see Figure 2).

However, as suggested by Figure 2, the expansion of Secure Communities across U.S. counties over time was not random. Indeed, counties could officially, at least initially, "opt in" or "opt out." Miles and Cox (2014)'s empirical analysis shows that the roll-out depended on (i) the size of the county's Hispanic population, (ii) the county's distance from the Mexican border, and (iii) its previous partnership between local law enforcement and ICE. In particular, early adopters (2008-2009) were closer to the U.S.-Mexico border, and they had larger populations of Hispanics. The program was adopted in most counties by mid-2012, and virtually completely adopted by early 2013.

On top of various technological constraints that prevented the program from being implemented across the whole country at once, there was some resistance from some counties and states. Secure Communities required a memorandum of agreement (MOA) between State of Identification Bureau official and the ICE acting director. However, in the spring of 2011, the governors of Illinois, Massachusetts, and New York ended their respective MOAs with DHS, arguing that the program was hindering community policing and was in addition not exclusively targeting the most violent criminals. As a response, ICE terminated all of the MOA's in the "Morten Memo" of August 2011, specifying that "[it] has determined that an MOA is not required to activate or operate Secure Communities for any jurisdiction [...]" and that no agreement is needed to legally share fingerprint data with another part of the federal government (Alsan and Yang, 2019).

Secure Communities was temporarily suspended in all parts of the country at the end of 2014, partly because of the resistance from "sanctuary cities"[7] but also probably because of the popular discontentment, which made Obama earn the title of "deporter in chief" (Wall Street Journal, 2019). The program was reactivated in early 2017 by the Trump administration. Between 2014 and 2017, DHS implemented another program, called the "Priority Enforcement Program" (PEP). It is, however, more restrictive than Secure Communities for immigration enforcement officers, as ICE could transfer only people who were convicted of very specific high-priority offenses, those who participated in criminal gangs, and those who were considered to be a serious threat to national security (Alsan and Yang, 2019).

[7] "Sanctuary cities" refer to jurisdictions that refused to obey when receiving detainer requests from ICE. Their arguments were that (i) these practices would discourage immigrants from cooperating with local law enforcement forces, and (ii) these detentions were unconstitutional under the Fourth Amendment (Alsan and Yang, 2019).

3 Conceptual Framework

This section presents a conceptual framework to help think about the potential health effects from enforcing an immigration policy on the individuals who are exposed to that policy.

Tightening an immigration enforcement policy can have direct and indirect (i.e., spillover) effects on the population. The direct effects correspond to the changes in the health behavior and status of *unauthorized* immigrants, because they are the main target of this immigration enforcement program and are likely to get deported if they are arrested. By contrast, the indirect effects come from changes in the health behavior and status of *U.S. citizens* (and residents who are not U.S. citizens but are lawfully present on U.S. soil) who belong to the same racial/ethnic community and/or family as the unauthorized individuals who are subject to removal.

Since I am interested in the externalities of the program on Hispanic U.S. citizens, I now focus on the indirect effects and describe potential mechanisms that could explain their existence. I identify two main (non-mutually exclusive) channels through which Secure Communities could affect the healthcare-seeking behavior and health status of Hispanic U.S. citizens, despite the fact that they are not subject to removal: income effects and/or psychological effects.

The (pure) *income* effect channel reflects the fact that the removal of unauthorized Hispanic individuals could either directly or indirectly constitute a negative income shock to some families via job losses. The *direct* income effects could, for instance, come from the fact that the deported individual is the breadwinner of the family or that s/he at least contributes to the income of the family and there are U.S. citizens in that family (e.g., children). By contrast, the *indirect* income effects could, for example, come from a compositional change in the labor market due to the removals of unauthorized Hispanics, who tend to be lower-skilled (East, Luck, Mansour, and Velásquez, 2019). This compositional change may affect other U.S. citizens' employment: negatively if they were complementary to these lower-skilled workers, positively if they were substitutes to them. However, the positive effect might also be (at least partially) counterbalanced by a decrease in labor demand caused by a raise in expected labor cost (Chassamboulli and Peri, 2015) and/or by a fall in local consumer demand (due to income losses resulting from unemployment). The overall effect here depends on the local production function and the general equilibrium effects.

These negative income shocks could in turn negatively affect health outcomes. Experiencing a loss in income means less financial resources available to devote to health expenses, thus reducing healthcare demand, which might in turn worsen one's health status. The negative impact on health outcomes could also be caused by job losses per se (beyond the associated loss in income) – e.g., losing one's employment may lead individuals to lose the associated health insurance plan and/or to worsen their health status (Gruber and Madrian, 1997; Garthwaite, Gross, and Notowidigdo, 2014; Schaller and Stevens, 2015). In fact, there exists a large literature that has shown a positive correlation between employment and health status – see, for example, Currie and Madrian (1999), Gruber (2000), Pelkowski and Berger (2004), Chatterji, Alegria, and Takeuchi (2011), García-Gómez (2011), and French and Jones (2017) for studies on the link between health and labor market outcomes, and Paul and Moser (2009) for a meta-analysis that focuses on the effect of unemployment on mental health.[8]

Immigration enforcement could also affect the health outcomes of Hispanic citizens through the *psychological* effects it may generate. In particular, Hispanic U.S. citizens may fear for members of their racial/ethnic community (e.g., friends or even relatives) who are unlawfully present on U.S. soil and may thus be deported if arrested.[9]

On the one hand, even if they do not change their own healthcare-seeking behavior, Hispanic U.S. citizens might exhibit a worse (e.g., mental) health status because of the stress, worry, and/or stigma generated by this immigration enforcement policy. For example, even if their family is not directly affected by that policy (e.g., there are no unauthorized members in their family and their family has not experienced a negative shock), they may still fear that, say, another unauthorized relative gets unexpectedly deported. Alternatively, they could fear being wrongfully arrested, as nearly 1,500 U.S. citizens – mostly individuals born abroad and children of immigrants – have been mistakenly apprehended for deportation by ICE between 2012 and 2017 (Los Angeles Times, 2018). In some cases, when mistakes are not rapidly rectified, arrested citizens could spend months or even years in detention and may face an immigration court system where they need to prove their citizenship so as not to be removed from the country (Los Angeles Times, 2018). Hence, not every American is immune from immigration raids and ICE's arrests. The possibility of having a relative being unexpectedly arrested or being oneself mistakenly detained could constitute an important source of stress and negatively affect one's mental health, irrespective of any income effects.

[8]Note though that a recent study by Kuka (2020) shows that more generous unemployment insurance leads to higher health insurance coverage and utilization.

[9]U.S. Hispanic children who are themselves U.S. citizens but whose parents are unlawfully present in the country may also be negatively affected, not only because they might fear that their parents get deported someday, but also because their parents might be more reluctant to interact with governmental institutions and employees. These negative effects have already been investigated by Watson (2014), so I do not consider them in this paper.

On the other hand, the psychological effects on Hispanic U.S. citizens may lead them to respond to this immigration policy by changing their own healthcare-seeking behavior, which could in turn potentially negatively affect their health status. The main reason behind this behavioral change would be that they are afraid of exposing and revealing their own social network, which includes unauthorized immigrants. To reduce the risk of being indirectly responsible for the removal of other members of their community, they may seek to avoid engaging with health-related (but not only) governmental institutions and public servants (Pedraza and Osorio, 2017; Pedraza, Nichols, and LeBrón, 2017; Alsan and Yang, 2019).

Finally, there could also be interactions between the aforementioned income and psychological effects of immigration enforcement. Indeed, fear may lead Hispanic U.S. citizens to give up on their participation to safety-net programs (such as SNAP or SSI (Alsan and Yang, 2019)) and/or federal/state health programs (e.g., Medicaid). The loss of these benefits acts like a negative income shock and may negatively affect Hispanic citizens' health behavior and status, thus creating a vicious circle.

Note also that enforcing immigration policies could lead unauthorized individuals who are eligible for the U.S. citizenship to apply for it. If their application is successful, they would then become eligible for some health care programs that are reserved for U.S. citizens, which should presumably improve their health status and make them more willing to seek health care. In that case, the impact of such policies would be positive.

4 Empirical Analysis

4.1 Data

This paper uses three sets of data – namely, data on immigration enforcement programs, data on the healthcare-seeking behavior and health status of U.S. Hispanics, and data on county-level controls. All the datasets cover my sample period of analysis (i.e., 2005-2012).[10]

Immigration enforcement data The data on immigration enforcement programs I use in this paper come from the U.S. Immigration and Customs Enforcement and TRAC Immigration (2020).

[10] I stop at 2012 (i) to avoid confounders from other immigration policies such as Deferred Action for Childhood Arrivals (DACA), and (ii) because the BRFSS dataset stops providing information on respondents' county of residence, which is key to conduct my analysis.

U.S. Immigration and Customs Enforcement (2015) provides county-level information on the rollout of Secure Communities. Figure 2 displays maps of the rollout across U.S. counties over time. As explained in Section 2, Secure Communities did not expand randomly. The activation of Secure Communities notably depended on a county's size of the Hispanic population, its distance from the U.S.-Mexico border, and its previous partnership between local law enforcement and ICE. Figure 3 shows that removals under Secure Communities are quite disparate across U.S. counties, with a higher number in more densely populated areas.

ICE also provides information on when some locations have shown resistance to cooperation with ICE. These locations are typically called "sanctuary jurisdictions." I use this information to exclude these counties from my sample analysis, as one might worry about some selection bias.

TRAC Immigration (2020) provides micro-level data on all the individuals who have been removed by ICE following a fingerprint match. The dataset covers the removals that happened during the first phase of Secure Communities (2008-2014), during the Priority Enforcement Program (2014-2016), and the beginning of the second phase of Secure Communities (2017). It includes information on the motive(s) of the arrest, the crime level/severity, the detainer's issuance date and county, the individual's birth country, gender, birthdate, and information on their sentence (if any).[11]

In this paper, I am using only the county-by-county rollout data as well as some of the socio-demographic information on the individuals that have been removed by ICE under Secure Communities.

Health data I use two data sources containing information on health – namely, the Behavioral Risk Factor Surveillance System (BRFSS) and the March Supplements of the Current Population Survey (CPS).[12]

The BRFSS dataset consists of cross-sectional phone surveys that interview more than 400,000 adults across the U.S. each year (since 1984) to collect data about U.S. residents regarding their health-related risk behaviors, chronic health conditions, and use of pre-

[11] TRAC Immigration (2020) also provides information on the number of detainers issued by ICE in each county-facility location between 2002 and 2019, and the number of ICE removals by city of departure from 2008 to 2019. However, because the facility/city where an individual was held and deported from does not necessarily match the location where the individual live, I cannot credibly rely on the geographic information present in these datasets so I do not use them for now. I plan to use these datasets in the future, perhaps by using the facility/city as a proxy for the residential location of the individuals that have been deported.

[12] In Appendix Section C, I describe other potential data sources that I could use, but the geographic information in most of those datasets are present only in the restricted-use version of the data, so I would need to submit an application to be granted access to those.

ventive services. In particular, the dataset contains questions about each respondent's self-reported health status, healthcare access, and socio-demographics (including county of residence, gender, marital status, race, employment, education, and income).

Table 1 summarizes statistics for the BRFSS data for survey year 2007, the year preceding the start of Secure Communities. It reports the mean alongside the standard deviation of several variables, by racial group. Panels A and B display the summary statistics for the individual-level health variables and socio-demographic variables, respectively.

Panel A shows that, compared to their white counterparts, Hispanics are much less likely to have any health insurance the year before the start of the rollout of Secure Communities (68 percent vs 91 percent). Hispanics also tend to seek less health care compared to either Blacks or whites. And, while they tend to feel more worried than the other racial/ethnic groups, they tend to drink alcohol less often. They are nevertheless much more likely to report feeling monthly physical symptoms over the past twelve months, and feeling worse treated than other races when seeking healthcare over the past twelve months.

Panel B shows that Hispanic individuals tend to be younger, more female, less married, poorer, more likely to be employed, and less educated than white individuals in that sample.

I complement the BRFSS dataset with the CPS March Supplement data because the BRFSS dataset does not contain information on the respondent's citizenship. I cannot even use country of birth (potentially combined with information on education and income) as a proxy for a respondent's citizenship since that piece of information is not present in the publicly available BRFSS data either. While the CPS dataset does not contain any questions regarding a respondent's healthcare seeking behavior, it does ask whether the respondent has any health coverage, and if so, what type. I can thus cross-check that the average effect for that variable with the BRFSS data. Finding similar results would be reassuring.

Table 2 summarizes "baseline" (i.e., survey year 2007) statistics from the CPS data. Similarly to the BRFSS data, Hispanic respondents in the CPS data are much less likely to have any health insurance (even though all ethnic/ethnic groups have lower coverage compared to the BRFSS data). In terms of socio-demographics (Panel B), compared to the BRFSS data, the respondents in that sample tend to be younger, more male, and more educated.

County-level economic, political, and geographic data To reduce the threat of omitted variables and/or increase the precision in my estimates, I collect data on a set of county-level characteristics. Specifically, I obtain data on the county-level Hispanic population estimates and death rates by ethnic/racial group[13] from CDC WONDER. I also compute a county's shortest distance to the U.S.-Mexico border using the NBER county distance database. Since the rollout of Secure Communities partially depended on the size of the county Hispanic population and the county's distance to the Mexican border, it appears important to control for them. Furthermore, I get county-level unemployment rates from the U.S. Bureau of Labor Statistics (BLS). One might indeed worry that the 2008 economic crisis is a potential confounder, so adding this variable as a control should alleviate that concern. Finally, I obtain from the MIT Election Data and Science Lab (2018) the county share of Republic votes in the previous presidential elections. I include this variable as a control in my analysis to account for the political leaning in a county, which may affect their willingness to cooperate with ICE.

I combine all of these datasets with the BRFSS and CPS datasets, separately. Panel C of Tables 1 and 2 displays the county-level characteristics for each analysis sample. It is worth noting that respondents in the CPS sample seem to be living in areas that tend to be more Democratic and that have a larger Hispanic population.

4.2 Identification Strategies

To identify the impact of Secure Communities on the health behavior and status of Hispanics, I employ a triple-difference (triple-diff) strategy, complemented with an event-study analysis. Both methods exploit the staggered rollout of Secure Communities across U.S. counties.[14]

Intuitively, the triple-diff method, as its name suggests, consists in taking three differences. Specifically, for a given health outcome variable, I first take the difference in means between Hispanics and non-Hispanic whites in a given county. Because the observed difference could just be due to systematic differences in health outcomes across these racial groups,[15] I then take the difference across time periods. Specifically, I take the difference in means computed *after* the activation of Secure Communities in a given county and subtract from it the difference in means computed *before* the activation of the program. Finally, to get additional variation and reduce the concern that the timing of the activation might

[13] I do not use death rates as a control variable though, but instead as a placebo outcome variable.

[14] These two empirical strategies are similar to the ones used in Alsan and Yang (2019); however, given the datasets that I have at the moment, my specifications will be simpler than theirs. Another main difference is that I conduct my analysis at the individual level whereas their analysis is at the race level.

[15] In fact, in the U.S., the Hispanic population tends to have a better health status than other racial groups, despite the fact that they typically have lower incomes and are disproportionately exposed to stress factors related to, say, their immigration status (e.g., they need to learn a new language, adapt to a new unfamiliar environment, face persistent discrimination). This puzzle is referred to "the Hispanic paradox" – see The Society Pages (2018) for a recent review.

coincide with other unobservable factors that could also differentially affect the health of Hispanics versus other racial groups, I subtract from the resulting double difference the mean in counties that have not yet implemented Secure Communities at a given point in time.

I formalize the aforementioned intuition for my main identification strategy with the following econometric specification:

$$
\begin{aligned}
\mathrm{Y}_{ict} \;=\;& \alpha + \beta_W \mathrm{Post}_{ct} + \beta_H(\mathrm{Hispanic}_i \times \mathrm{Post}_{ct}) + \beta_B(\mathrm{Black}_i \times \mathrm{Post}_{ct}) + \\
&+ \delta_H \mathrm{Hispanic}_i + \delta_B \mathrm{Black}_i + \theta_t + \eta_m + \mu_{ct} + \gamma' \mathrm{X}_{i(c)} + \varepsilon_{ict} \qquad (1)
\end{aligned}
$$

where i, c, and t index individual, county, and year, respectively. Y_{ict} is a health outcome variable of interest (see Panel A of Tables 1 and 2 for the list of measures of health status and behavior I focus on). Post_{ct} corresponds to a binary variable that takes value one in all county-year observations after the activation of Secure Communities, and zero otherwise. $\mathrm{Hispanic}_i$ is a binary variable that is equal to one for Hispanic ethnicity. Black_i is a binary variable that is equal to one for non-Hispanic Blacks. Hence, the omitted category is non-Hispanic whites. $X_{i(c)}$ is a vector of individual-level socio-demographic controls (see Panel B of Tables 1 and 2) and county-level controls (see Panel C of Tables 1 and 2). I additionally include calendar year fixed effects and calendar month fixed effects to control for average time trends in the outcome across years (e.g., economic shocks) and across months that could bias my estimates.

β_H is the estimand of interest – the estimated coefficient measures the (arguably) causal effect of the implementation of Secure Communities on the health behavior and outcomes of Hispanics relative to non-Hispanic white housholds, net of counties that have not yet implemented the program. It measures the difference in the outcome of interest with respect to whites, and therefore represents the average impact of Secure Communities on a given health outcome for Hispanics relative to whites. I expect this coefficient to be negative, because I hypothesize that immigration enforcement has *negative* spillover effects on the healthcare-seeking behavior and health status of Hispanics.

β_B mainly serves as a placebo test, as it captures the impact of Secure Communities activation on non-Hispanic blacks relative to non-Hispanic whites in counties that have implemented the program versus those that have not yet implemented it. I expect this coefficient to be statistically *in*significantly different from zero or at worst marginally significantly different from zero but small in magnitude (i.e., *economically* insignificant). Indeed, there is not much reason to believe that Black people would change their health behavior or exhibit a change in their health outcomes since they are presumably not the main target of Secure Communities. However, one may argue that Secure Communities could also negatively impact the health of Black individuals. For example, their mental health may deteriorate as Secure Communities facilitates police controls and there exists a lot of evidence of racial profiling in policing (e.g., see Levitt and Miles (2006); Antonovics and Knight (2009); Rehavi and Starr (2014); Goel, Rao, Shroff, et al. (2016); Legewie (2016); Goncalves and Mello (2020); Knox, Lowe, and Mummolo (2020)).

The validity of this identification strategy for causal estimation crucially relies on the assumption that there do not exist contemporaneous shocks that are timed with the implementation of Secure Communities within a county and that differentially affect Hispanics and non-Hispanics.[16] Put differently, causal identification requires that, absent the activation of the program and conditional on the set of control variables and fixed effects, the outcome variables of interest would have followed the same trend over time for Hispanics vs. non-Hispanic whites vs. non-Hispanic Blacks.[17]

Because one might worry that the identifying assumption for the triple-diff strategy may fail (e.g., the effects of Secure Communities are systematically and dynamically different across racial/ethnic groups), I complement this triple-difference strategy with an event-study analysis. I normalize to zero the year in which Secure Communities is activated in a county. I then interact the dummy variables Hispanic_i and Black_i with a set of indicator variables for each year within a four-year window before and after the Secure Communities activation in each county, setting the year that precedes the activation of the program to be the omitted year. I bin together the years out of the four-year windows and create dummy variables for each end point ($I_{c,t<4}$ and $I_{c,t>4}$).[18]

Formally, I estimate the following event-study specification:

$$\begin{aligned} \text{Y}_{ict} \quad = \quad & \alpha + \sum_{\substack{\tau=-4 \\ \tau\neq-1}}^{4} \phi_W^{\tau}(I_{c,t=\tau}) + \sum_{\substack{\tau=-4 \\ \tau\neq-1}}^{4} \phi_H^{\tau}(\text{Hispanic}_i \times I_{c,t=\tau}) + \sum_{\substack{\tau=-4 \\ \tau\neq-1}}^{4} \phi_B^{\tau}(\text{Black}_i \times I_{c,t=\tau}) + \\ & + \psi_1(I_{c,t<4}) + \psi_2(I_{c,t>4}) + \delta_H \text{Hispanic}_i + \delta_B \text{Black}_i + \qquad (2) \\ & + \theta_t + \eta_m + \mu_{ct} + \gamma' \text{X}_{i(c)} + \nu_{ict} \qquad (3) \end{aligned}$$

where $I_{c,t=\tau}$ is a binary variable for each year within a four-year window before and after

[16]I group non-Hispanic Black and white individuals together here because I also look at the effect of Secure Communities on Hispanics relative to Blacks, instead of whites. The purpose of this additional analysis is to check whether there is any statistical difference with respect to Blacks, because for some outcomes variables, I do find a statistically significant coefficient for Blacks in the triple-diff analysis.

[17]This requirement for identification is analogous to the parallel-trend assumption in a standard difference-in-differences model.

[18]This procedure of binning the end points and using them in the estimation but not displaying them in the figures is recommended by Goodman-Bacon (2019).

the activation of Secure Communities, other than the year that precedes its activation (i.e., $t = -1$).

The coefficients of interest here are the ϕ_H^τ's and ϕ_B^τ's – they trace out the evolution of our outcome variables for Hispanics and Blacks, respectively, in each of the years before and after the implementation of Secure Communities, relative to non-Hispanic whites (ϕ_W^τ's). This procedure provides a test for pre-trends and enables one to see the evolution of the effects over time.

I here expect that the program would affect only Hispanics, in which case I would find coefficients that are not statistically different from zero before the activation of the program (i.e., until $t = -1$) and a negative jump after the activation (i.e., from $t = 0$ onwards) for the ϕ_H^τ's but not for the ϕ_B^τ's (unless we want to believe that there could also be indirect effects on Black individuals, since there may now be more likely to be arrested and have their criminal records checked even for small infractions).

5 Preliminary Results

In this section, I show and discuss the results from the preliminary analysis I have conducted so far. Note that I will not be able to disentangle all the potential mechanisms I have presented in Section 3. I first present the results from my event-study analysis to check whether there is any pre-trends that would cast doubt on the identifying assumption of my triple-difference strategy. I then turn to the triple-difference analysis to provide estimates of the average health effects of Secure Communities. Note that since the treatment is at the county level, in all regressions, I cluster the standard errors at the county level.[19]

5.1 Event-Study Results

Figures 4, 5 and 6 plot the ϕ coefficients from the event-study specification (i.e., Specification (3)) using the BRFSS sample, the CPS sample and the CDC WONDER sample, respectively. Each panel displays the results for a given outcome variable.[20] Within each panel, the coefficients are plotted in different subfigures: the left subfigure is for Hispanics (i.e., the ϕ_H's), the center subfigure is for whites (i.e., the ϕ_W's), and the right subfigure is for Blacks (i.e., the ϕ_B's).

Panel (a) of Figure 4 shows the activation of Secure Communities negatively affected

[19]I am therefore allowing for serial correlation within counties but not across them.

[20]Note that I do not display the event-study plots for all the health outcome variables listed in Tables 1 and 2, because of small sample size issues, as discussed below when I present the triple-difference results.

the likelihood of doing a routine checkup over the past 12 months for Hispanics. Indeed, before its activation (at $t = 0$), the effect is, as expected, null and it becomes more and more negative over time from the activation onwards. By contrast, it appears that white individuals are on a slight upward trend whereas Black individuals were on a slight downward trend.

A similar pattern for Hispanics is true when we look at whether they have health coverage (Panel (b) of Figure 4), suggesting that one potential reason why Hispanics tend to do routine health checkups less often is the fact that they are less likely to have a health insurance following the activation of Secure Communities. By comparison, whites are on a slight downward trend whereas, as expected, there is no effect for Black individuals.

One might think this loss in health coverage could be explained by the indirect employment effects immigration enforcement policies can generate, as described in Section 3. However, Panel (c) of Figure 4 rules out this potential explanation, as the event-study coefficients for Hispanics are not statistically significantly distinguishable from zero both before and after the implementation of Secure Communities. If anything, employment for Hispanics is on a downward trend before the program activation and then on an upward trend after the program activation.

I also look at whether this immigration enforcement program has any effects on the likelihood of having a personal doctor or healthcare provider. Panel (d) of Figure 4 suggests that in the longer run, Secure Communities has a negative impact here as well for Hispanics, although it seems that there is a negative effect also for Black individuals where there is a downward trend for white individuals. Hence, one may want take this apparent negative effect for Hispanics with a grain of salt. Even though one might expect negative spillover effects from a lower willingness to interact with healthcare workers (as discussed in Section 3), it is unclear that one should expect to find any effects for this outcome variable in particular. Indeed, *having* a personal doctor or healthcare provider does not necessarily tell much about *how often* one interacts with them.

As a robustness check, I use the CPS data to restrict the analysis sample to American citizens. However, as noted in Section 4.1, the CPS dataset does not contain any information about healthcare seeking behavior or health status (see Table 2). I can only focus on various types of health coverages with such data.

Figure 5 displays the event-study results using the CPS data. Panel (a) confirms the negative spillover effects on Hispanics' health coverage that we have found using the BRFSS data. Note, however, that the effect is less pronounced and neat than with the BRFSS data (Panel (a) of Figure 4). This apparent discrepancy could be due to sampling differences and/or to the fact that we are now restricting the analysis sample of U.S. citizens. If it is the latter explanation that prevails, then one can view the BRFSS results as an upper bound for the impact of Secure Communities on the health coverage likelihood for Hispanic U.S. citizens. Reassuringly, here as well, I do not find any effects for non-Hispanic Blacks, and the outcome for non-Hispanic whites is trending downward over time.

The remaining panels of Figure 5 investigate whether Secure Communities has any impact on the type of health coverage that individuals have (among those who do have one). It appears that this immigration enforcement policy does not have any effects on the types of health coverage Hispanic U.S. citizens have – be it the likelihood of having a private health insurance (Panel (b)), Medicare (Panel (c)),[21] or Medicaid (Panel (d)).

Finally, as a placebo test, I also plot the event-study coefficients using death rates by ethnic/racial group as the dependent variable (Figure 6). I do not find any impact of Secure Communities on death rates for any ethnic/racial group. This result may not be surprising since the negative effects on healthcare-seeking behavior and mental health would need to be very strong to generate any mortality effects.

Overall, the event-study plots suggest that there are no pre-trends in the health outcome variables of interest. They also suggest that the Secure Communities program has a negative impact on the healthcare-seeking behavior of Hispanics and that these negative effects are more consistent with psychological effects rather than income effects.

5.2 Triple-Difference Results

I now quantify the average effects of Secure Communities on various health variables measuring the healthcare-seeking behavior and health status of Hispanics.

Table 3 displays the β coefficients from Specification (1). Each column represents a single regression, using a given outcome variable. The first row displays the main coefficient of interest, which, if our identifying assumption holds, represents the causal effects on Hispanic individuals.

As previewed with the corresponding event-study plot (see Panel (a) of Figure 4), Column (1) suggests that on average, after the activation of Secure Communities in their counties, relative to whites, Hispanics are 3.3 percentage points (pp) less likely to report having done a routine health checkup within the past 12 months. This drop corresponds to a 5-percent decrease with respect to their pre-SC-activation period. Surprisingly though, I also find a negative, albeit smaller, impact for Black individuals (see the third row). Compared to their white counterparts, Blacks are 1.8 pp less likely to report having done a

[21] Note that the analysis sample for that outcome variable is restricted to individuals who are aged 65 and older.

routine health checkup within the past 12 months (i.e., a 2.2-percent decrease with respect to their pre-SC-activation period). However, reassuringly, when I restrict the sample to Blacks and Hispanics so that the reference group is now Blacks, I still find a significant effect for Hispanics (see Column (1) in Appendix Table B.2). This finding means that the impact of Secure Communities is, as expected, larger for Hispanics.

Some of this negative effect can be explained by the fact that medical costs appeared to have prevented them from seeing a doctor and/or that they are less likely to have health coverage after the activation of Secure Communities. Columns (2) and (4) show that, on average, Hispanics are less likely to see a doctor because of medical costs (-5 percent with respect to the pre-SC-activation period) and they are less likely to have any health coverage (-2.2 percent compared to the pre-SC-activation period).

Since losing one's job can have an effect on both healthcare-seeking behavior and health coverage, I check whether these negative effects could be explained by the fact that Hispanics were more likely to become unemployed after the activation of Secure Communities. Column (5) suggests that this hypothesis is not true. If anything, Hispanics were slightly more likely to have a job relative to whites (+1 pp).

Using the CPS data, I restrict the sample of American citizens and the effect on health coverage decreases to -1.1 pp (i.e., a 1.4-percent decrease with respect to the pre-SC-activation period) but is still statistically significant (see Column (1) of Table 4). As mentioned in the previous section, one can perhaps view the coefficient from the BRFSS data as an upper bound for the impact of Secure Communities on health coverage for Hispanic U.S. citizens.

And, as suggested by the corresponding event-study plots (Panels (b), (c) and (d) of Figure 5), Secure Communities does not have any impact on the type of health insurance Hispanic U.S. citizens have (see Columns (2)-(4) of Table 4).

I next present the results for additional health outcomes using the BRFSS data. Note that these results should be taken with caution because the analysis sample for these outcome variables is (much) smaller. The reason behind this smaller sample is that BRFSS did not include all of the corresponding survey questions in each interview year. Nevertheless, I consider these results as suggestive.

Table 5 presents the results for these additional health variables. Columns (1), (2) and (4) suggest that the activation of Secure Communities slightly increases the frequency of alcohol consumption[22] (+0.05 days per week of alcohol consumption over the past 30 days, with respect to the pre-SC-activation period), makes Hispanics more likely to feel

[22] The BRFSS data also contains a question about smoking, but the way they phrased it is not informative, so I do not include that variable in my analysis.

worried/anxious (+2.4 days) and more likely to report feeling monthly physical symptoms due to treatment based on race over the past 12 months (+3 pp).

Interestingly, Blacks are also more likely to report feeling worried/anxious following the activation of Secure Communities (third row in Column (2)). This result is not that surprising since they might fear that this immigration policy would be accompanied with more police controls, including more racial profiling in policing, from which Black individuals are more likely to suffer.

Moreover, Hispanics do not report feeling treated worse than other races when they sought healthcare over the past 12 months (Column (3)). This result can be linked to the result regarding the effect on routine checkups. Specifically, it appears that Hispanics are less likely to do routine checkups but that is presumably not because they feel more poorly treated by doctors when they seek healthcare.

Finally, consistent with the hypothesis that Hispanics are more fearful of interactions with healthcare professionals, Column (5) shows that Secure Communities makes Hispanics less likely to visit an eye care professional (-3.7 pp, i.e., a 6.5-percent decrease with respect to the pre-SC-activation period).

Overall, the triple-difference analysis suggests that the Secure Communities does have a negative impact on the health status and healthcare-seeking behavior of U.S. Hispanics. The results presented here seem to be more consistent with psychological effects rather than income effects,[23] as described in Section 3.

5.3 Discussion

Heterogeneous effects As one might expect there may be heterogeneous effects by removals, I adjust Specification (1) by adding a third interaction term to the current interaction variables. This third interaction term is computed a dummy variable that is equal to one if a given county is above the median cumulative number of removals in a given year. I prefer this measure to a measure consisting of a share (e.g., computed by dividing the number of removals by the (undocumented) Hispanic population in a given county in a given year) because I argue that individuals are more sensitive to the total number of removals over time rather than the share of removals that happened in a county in a given year. For example, they may be more affected if they know that, say, 100 Hispanic individuals have been deported in their county since the activation of Secure Communities (regardless of the total number of Hispanics in the county), than if less than one percent

[23] Note that I have not ruled out the direct income effects. I plan to do so in the future by looking at whether there was any compositional change in the household status of the respondents (e.g., in the CPS data).

of the share of Hispanic population have been removed in a given year.

Table 6 displays the results from the heterogeneity analysis. It seems that Secure Communities did not have any differential effects by the cumulative number of removals, regardless of the health outcome variables of interest.

Sample composition Since I am using repeated-cross section data, one might worry that if my hypothesis about the negative psychological effects from enforcing immigration policies (in particular, the reluctance to interact with governmental institutions and employees) is true, then one should expect a compositional change in the sample of respondents over time.

I investigate this question by using as the dependent variable in Specification (1) each of the control variables listed in Tables 1 and 2. I also remove $X_{i(c)}$, the vector of individual-level and county-level controls, from that specification. Finding a significant effect on any of the β coefficients would suggest that Secure Communities has induced a compositional change in the sample of respondents.

Appendix Table B.9 displays the results from that analysis using the BRFSS data. It appears that following the activation of Secure Communities, Hispanic respondents in the BRFSS data are younger and poorer than their white counterparts.[24] However, we observe similar effects for the sample of Black respondents. It is therefore difficult to conclude that this compositional change is caused by the implementation of the Secure Communities program.

Appendix Table 10 is the corresponding table but using the CPS data. In that sample, we can actually check whether Hispanic U.S. citizens are less likely to participate in the survey since the CPS dataset asks whether the respondent is a U.S. citizen (see Column (1)). Interestingly, the Hispanic respondents in that sample are 2 pp more likely to have at least a high school degree (+3.6 percent relative to the pre-SC-activation period) and 3 pp more likely to be U.S. citizens after the implementation of Secure Communities (approximately +5 percent with respect to the pre-SC-activation period).[25] By contrast, no economically significant effect can be detected for Blacks.

Regarding the positive effect on U.S. citizenship for Hispanics, it could be because of Secure Communities, unauthorized Hispanic individuals are less willing to complete the CPS, as they might be afraid of being reported to ICE. Another (non-mutually exclusive)

[24] The coefficients using "Female," "County-level unemployment rate" and "County-level share of Republican votes in the last presidential elections" are also *statistically* significant but they are *economically* insignificant (i.e., close to zero in magnitude), so I disregard them.

[25] Here as well, I disregard the remaining coefficients that are *statistically* significant but *economically* insignificant.

potential explanation is that this immigration enforcement program has incentivized previously unauthorized, eligible individuals to apply for the U.S. citizenship. In this case, the sample of Hispanic respondents with the American citizenship becomes larger due to more naturalizations.[26]

6 Concluding Remarks

This paper seeks to study the indirect effects of Secure Communities on the healthcare-seeking behavior and health status of Hispanic U.S. citizens (who are, by definition, immune to deportation). After laying out a few potential mechanisms behind those indirect effects (i.e., income effects and/or psychological effects), I proceed with presenting the empirical strategy and the data used to first check whether the indirect effects are present and rule out some potential channels that could explain them.

The preliminary results suggest that there indeed exist some negative spillover effects of that immigration enforcement program on the healthcare-seeking behavior and health status of Hispanics. In particular, compared to their white counterparts, Hispanics are less likely to do a routine health checkup following the implementation of Secure Communities in their county (-5 percent with respect to the pre-SC-activation period). They are also less likely to have health coverage in the post-SC period (-2.2 percent), and that drop in health coverage cannot be explained by a decrease in employment. In fact, if anything, Hispanics are more likely to be employed after the activation of that immigration policy. I also find suggestive evidence that their health status is negatively impacted by this policy. Specifically, compared to whites, Hispanics are more likely to drink alcohol, feel worried/anxious, and feel monthly physical symptoms due to treatment based on their race.

While I cannot fully disentangle the precise mechanisms underlying these findings, my preliminary analysis suggests that they are more consistent with the psychological effects that such immigration enforcement policies may generate rather than the resulting income effects and general equilibrium effects.

These negative spillover effects have important policy implications. Indeed, policy-makers typically focus on the direct effects of the policies they would like to implement. If the indirect effects are significant, they need to be included in the cost-benefit analysis when evaluating the ex-ante and/or ex-post effects of such policies. In the context of Secure Communities, the U.S. government might want to account for the unintended consequences for the health of U.S. subpopulations that are indirectly affected by such

[26] I plan to investigate this possibility in the future by taking advantage of the panel dimension of the CPS data.

immigration policies.

The next steps for this research project consist in extending the current analysis. In particular, I have recently gained access to restricted version of the Health and Retirement Study data, which would enable me to not only have a longitudinal dataset but also one that covers a more recent time period (i.e., until 2018). I can therefore analyze what happens after 2012 (which is the last year in the analysis samples I am considering in this paper). Since this dataset is restricted to the elderly U.S. population, I may also want to collect data from the National Vital Statistics System (NVSS).[27]

In addition to the aforementioned data and time period extensions, another significant extension of the analysis consists in examining not only *all* removals (as opposed to only those that have occurred under Secure Communities) but also arrests.[28] It may be interesting to consider arrests per se (regardless of whether they result in removals) because they may generate negative mental health effects on the U.S. citizens who are tied to the communities of unauthorized immigrants (albeit the effects here may be smaller than those that stem from deportations).

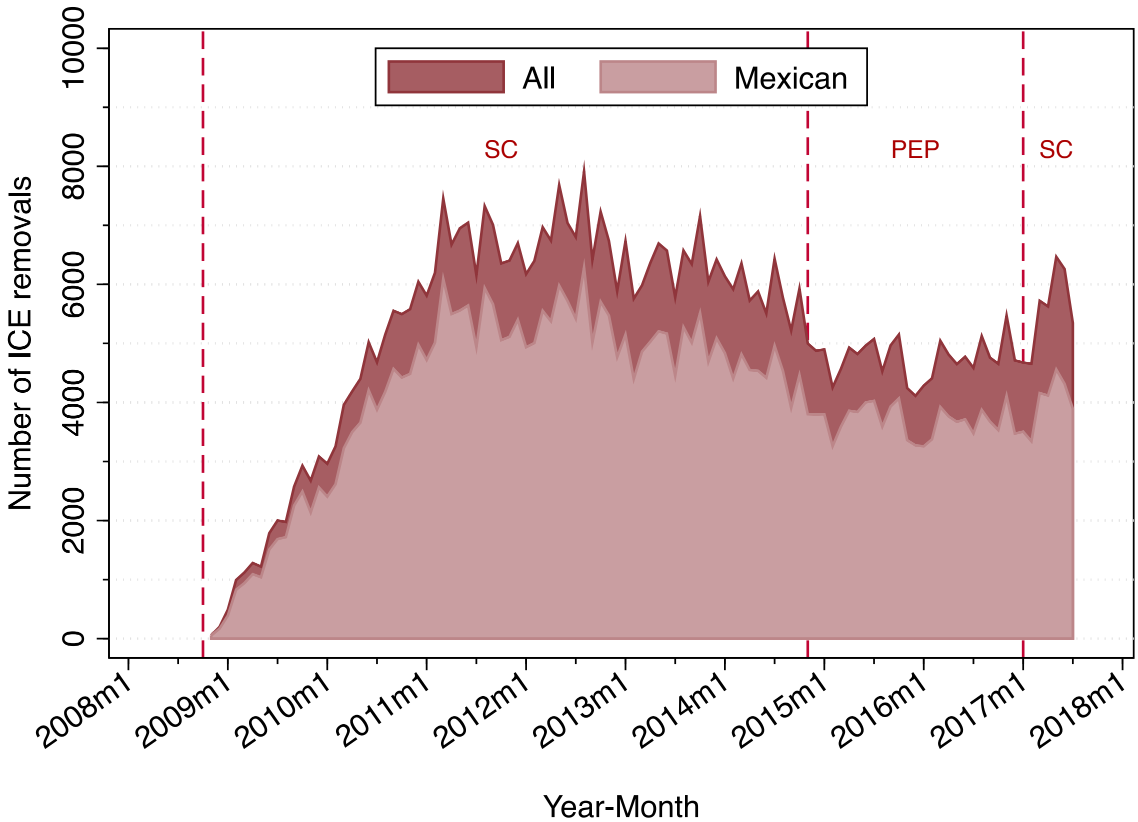
Finally, for the datasets that do not have information on respondents' citizenship, I would need to find proxies to identify those that are American citizens (e.g., if the information is present in the data, I could try to use a combination of their country of birth, income, and/or education level).

[27] See Appendix Section C for a description of additional potential health datasets, including NVSS.

[28] I already data on all ICE removals and arrests; I would need to clean the datasets and also figure out what type of analysis to conduct since the location is imprecise in these datasets.

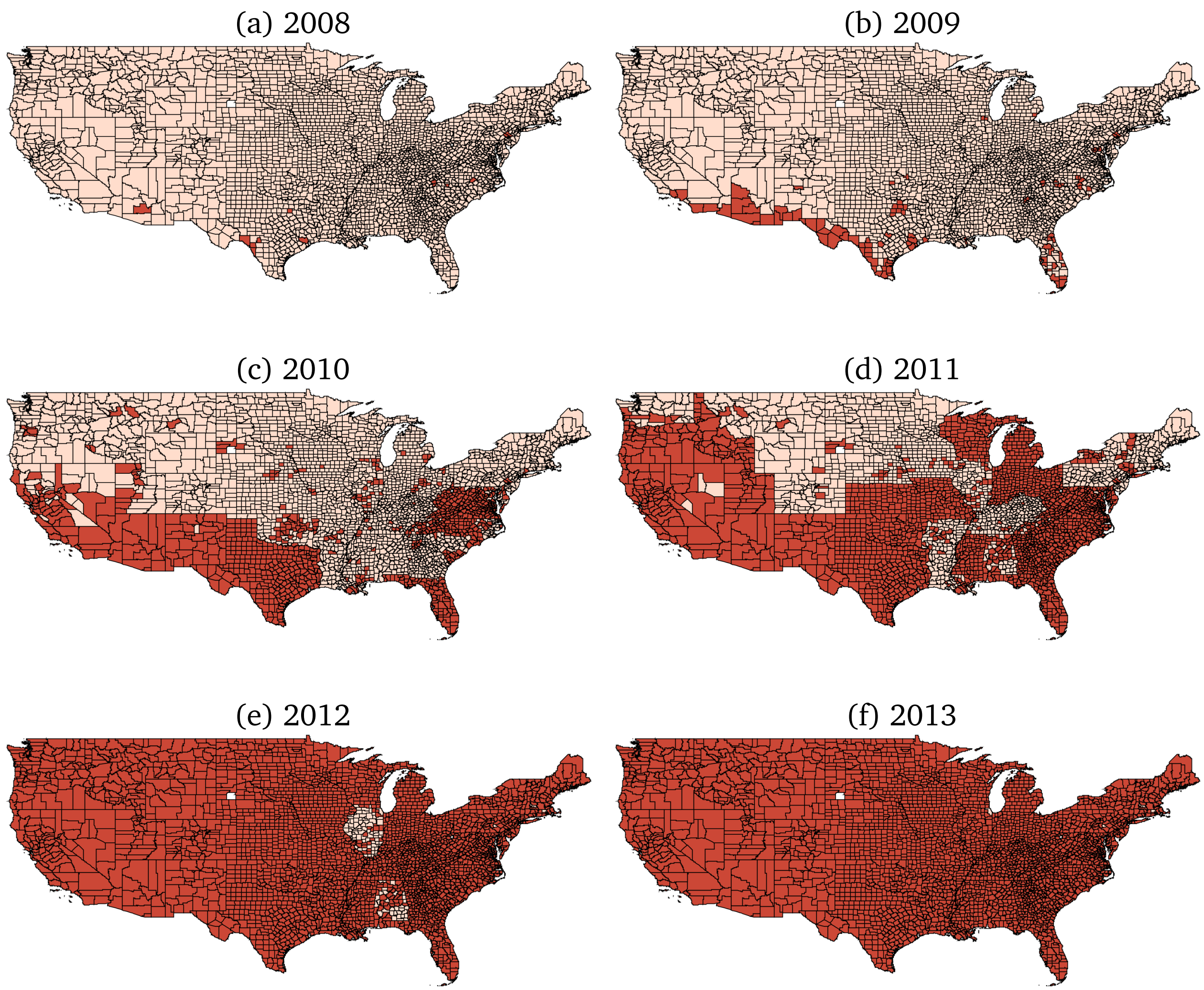
Main Figures and Tables

Figure 1: Monthly number of removals under Secure Communities (2008-2014 and from 2017 onwards) and the Priority Enforcement Program (2014-2017)



Notes: This figure displays the monthly number of ICE removals under Secure Communities (SC) and the Priority Enforcement Program (PEP). The darker area represents all removals whereas the lighter area represents only the removals of Mexicans. Data source: TRAC.

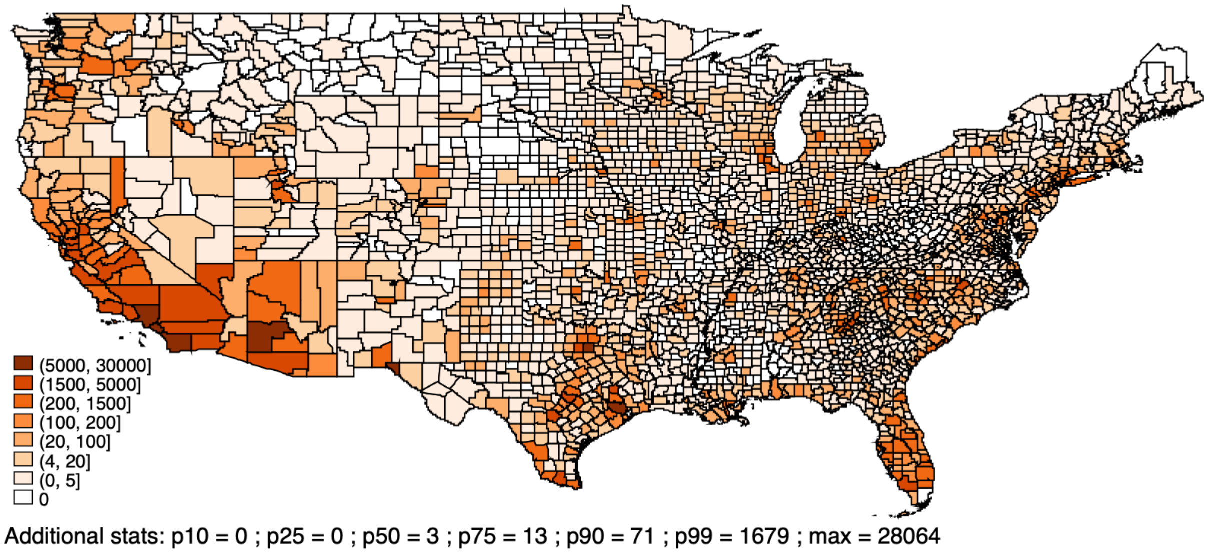
Figure 2: Secure Communities county-by-county roll-out (2008-2013)



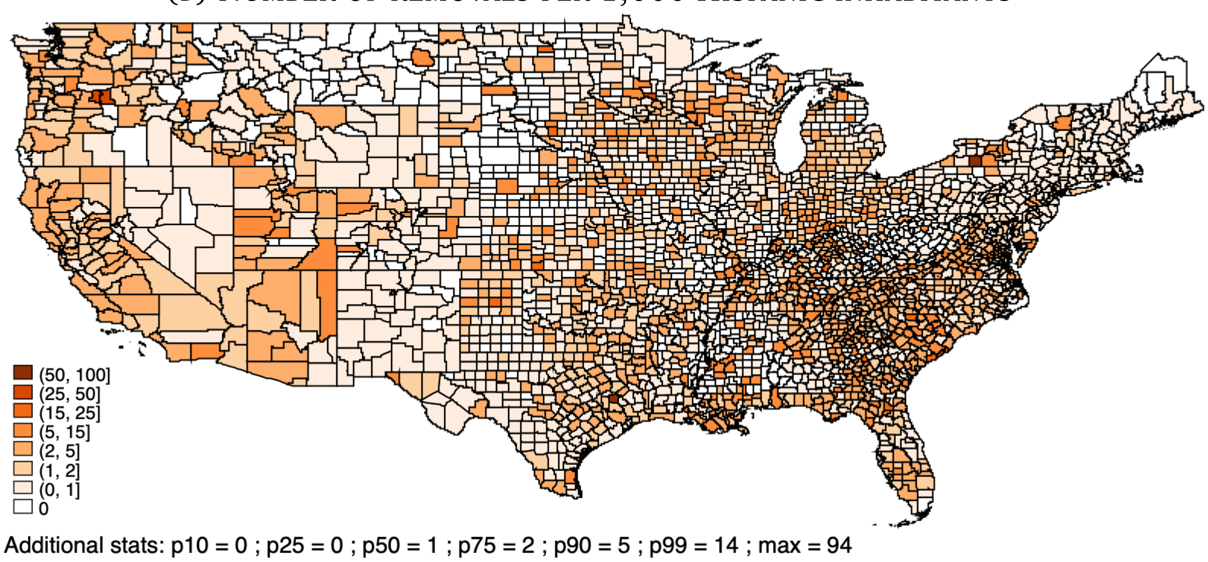
Notes: This figure displays the rollout of Secure Communities across the entire country between 2008 and 2013. Colored in red (shaded) are counties that have adopted Secure Communities by a given year. The very few counties that are left blank are typically independent counties/cities. Data source: U.S. Immigration and Customs Enforcement (2015).

FIGURE 3: REMOVALS UNDER SECURE COMMUNITIES AS OF 2012, BY COUNTY

(a) TOTAL NUMBER OF REMOVALS



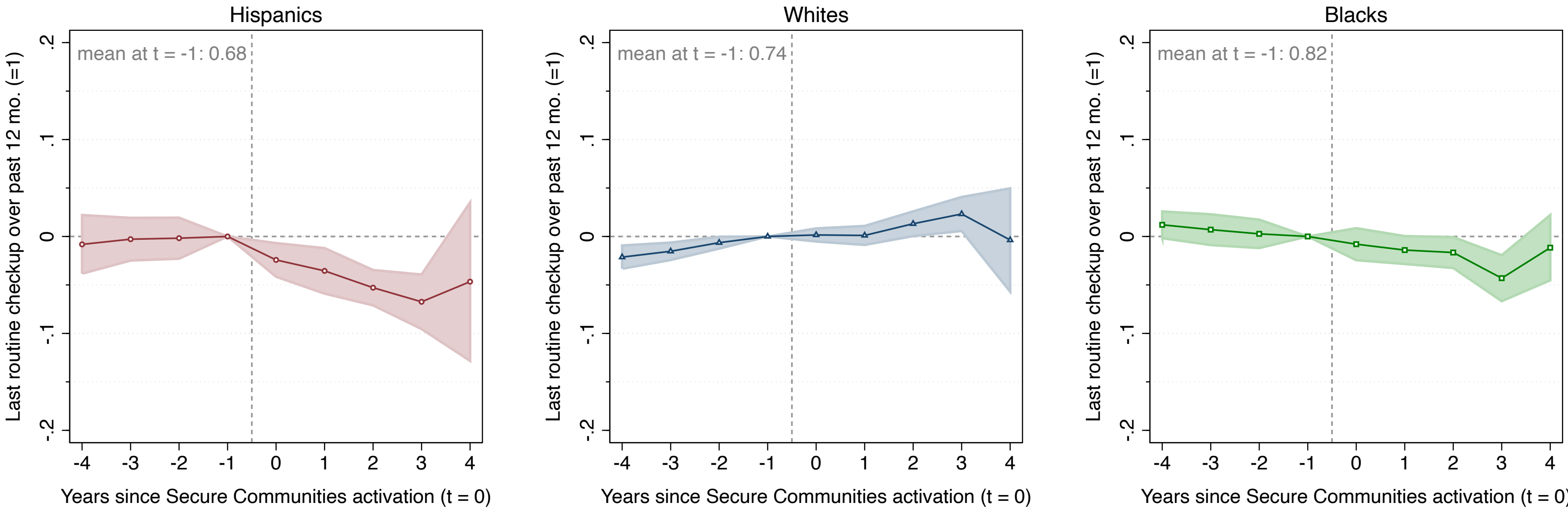
(b) NUMBER OF REMOVALS PER 1,000 HISPANIC INHABITANTS



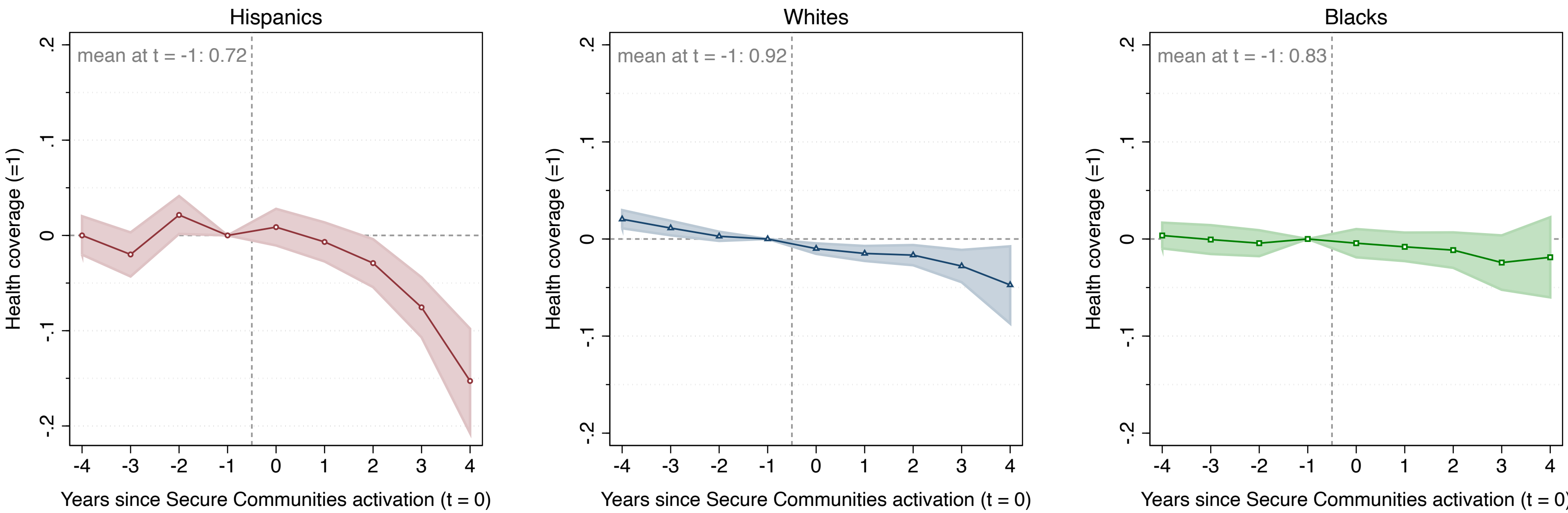
Notes: These maps show the number of ICE removals under Secure Communities as of 2012, the last year of my analysis sample. Panel (a) displays the total number of removals as of 2012 whereas Panel (b) shows the number of removals per 1,000 Hispanic inhabitants in the county (= number of removals divided by 2012 Hispanic population in a given county, multiplied by 1,000). Data source: TRAC.

FIGURE 4: EVENT-STUDY PLOTS (BRFSS 2005-2012) WITH CONTROLS

(a) LAST ROUTINE CHECKUP WITHIN PAST 12 MONTHS



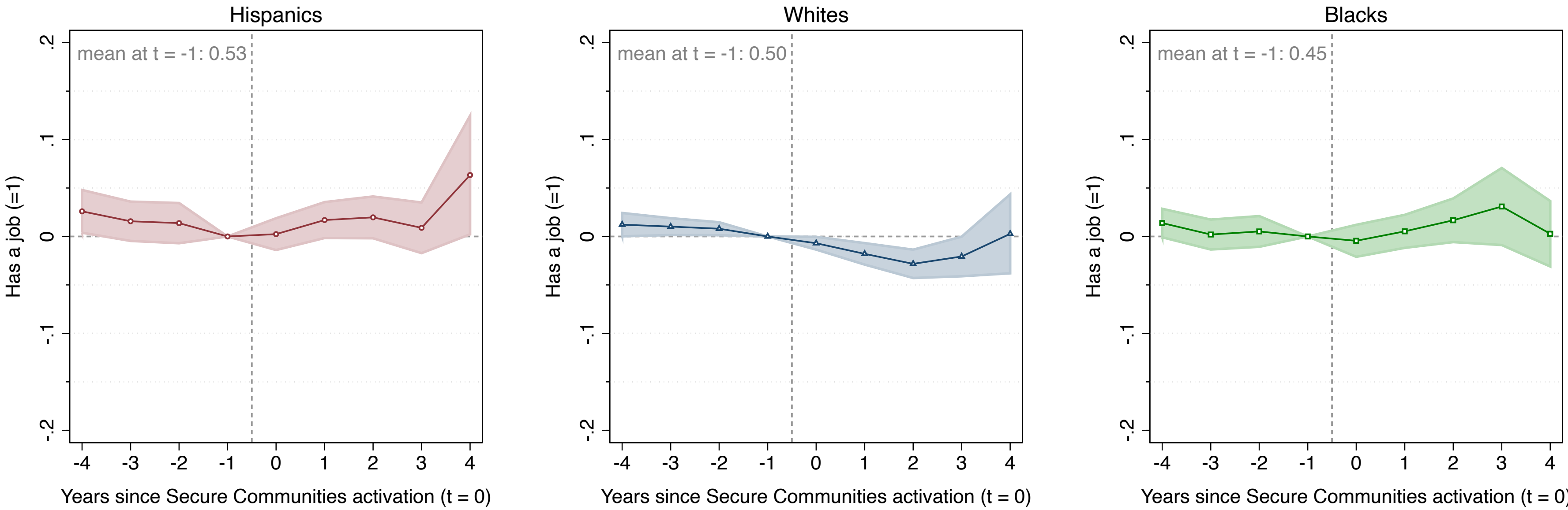
(b) ANY HEALTH COVERAGE



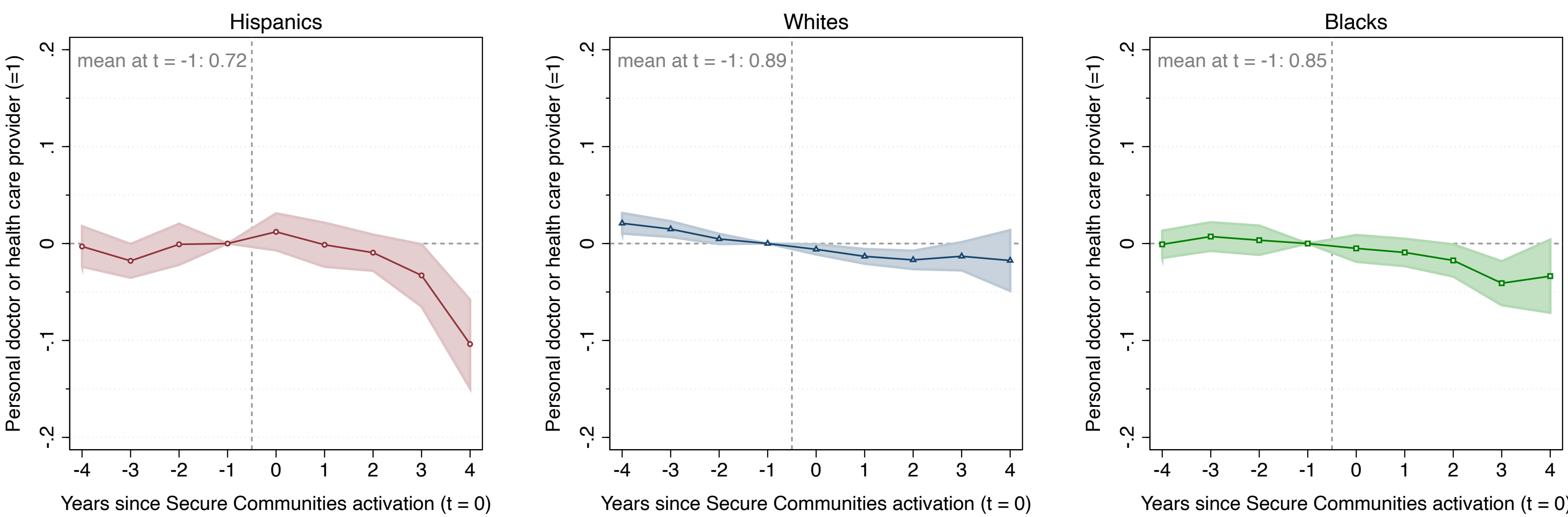
Notes: This figure plots the ϕ coefficients from Specification (3). The shaded areas represent 95% confidence intervals, which have been computed using standard errors clustered at the county level. Each panel displays the results for a given outcome variable, by racial/ethnic group.

Figure 4: [continued] Event-study plots (BRFSS 2005-2012) with controls

(c) Has a job



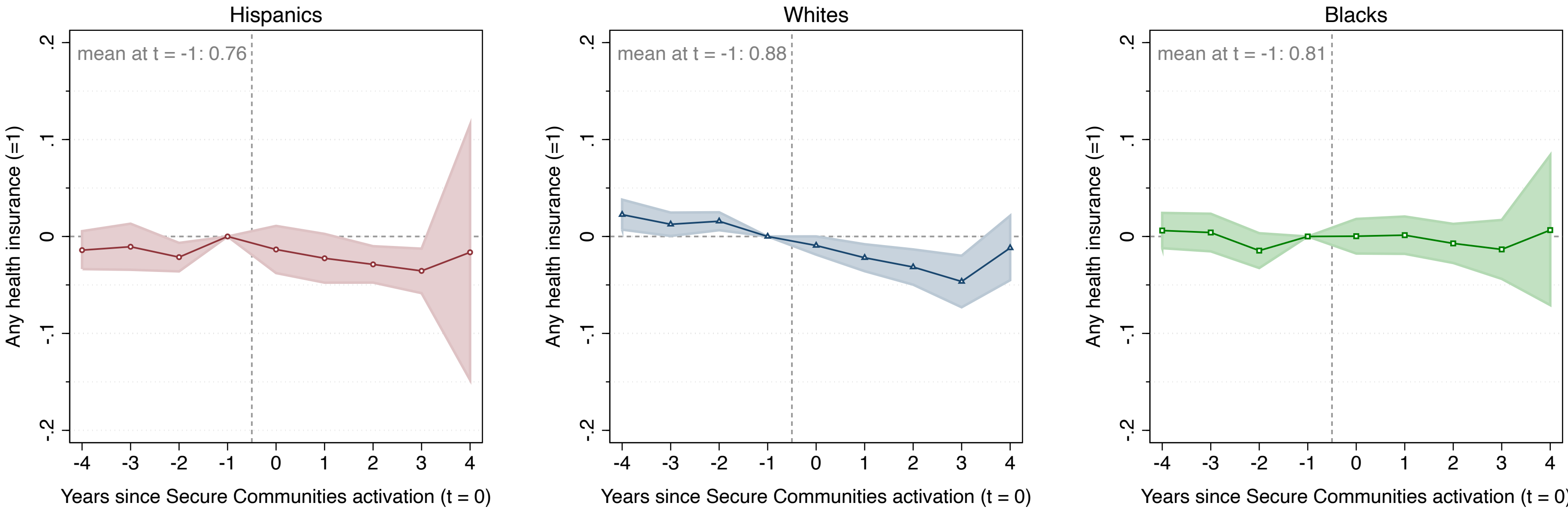
(d) Has a personal doctor or healthcare provider



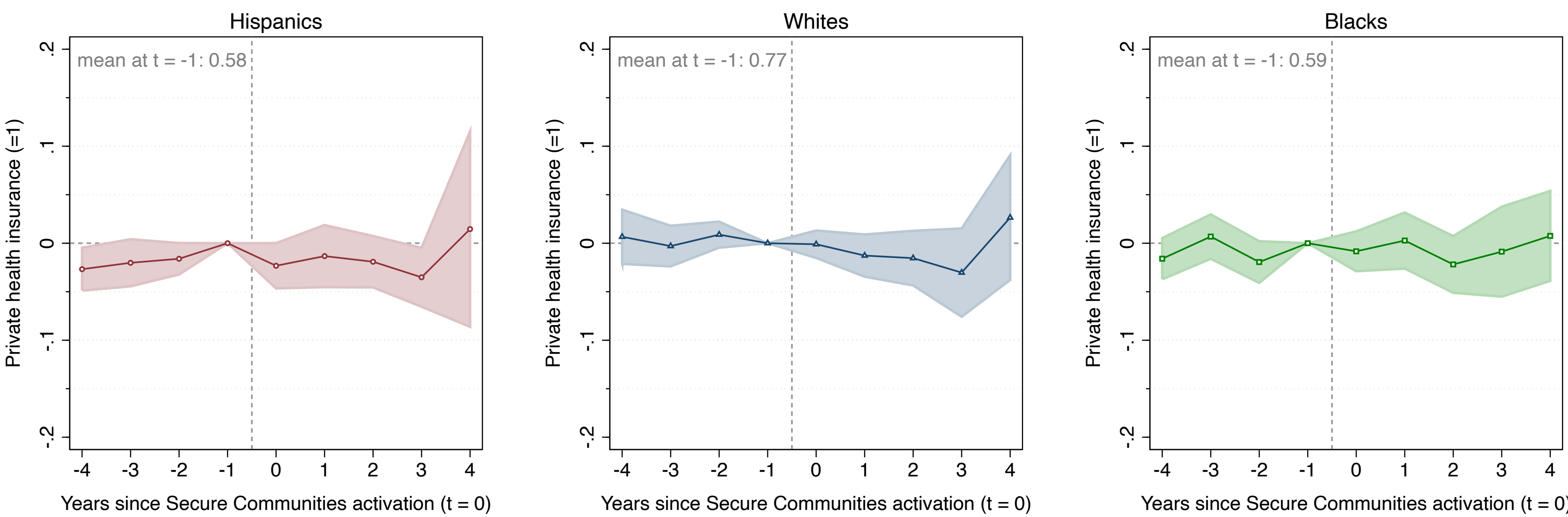
Notes: This figure plots the ϕ coefficients from Specification (3). The shaded areas represent 95% confidence intervals, which have been computed using standard errors clustered at the county level. Each panel displays the results for a given outcome variable, by racial/ethnic group.

FIGURE 5: EVENT-STUDY PLOTS (CPS 2005-2012) WITH CONTROLS

(a) ANY HEALTH INSURANCE



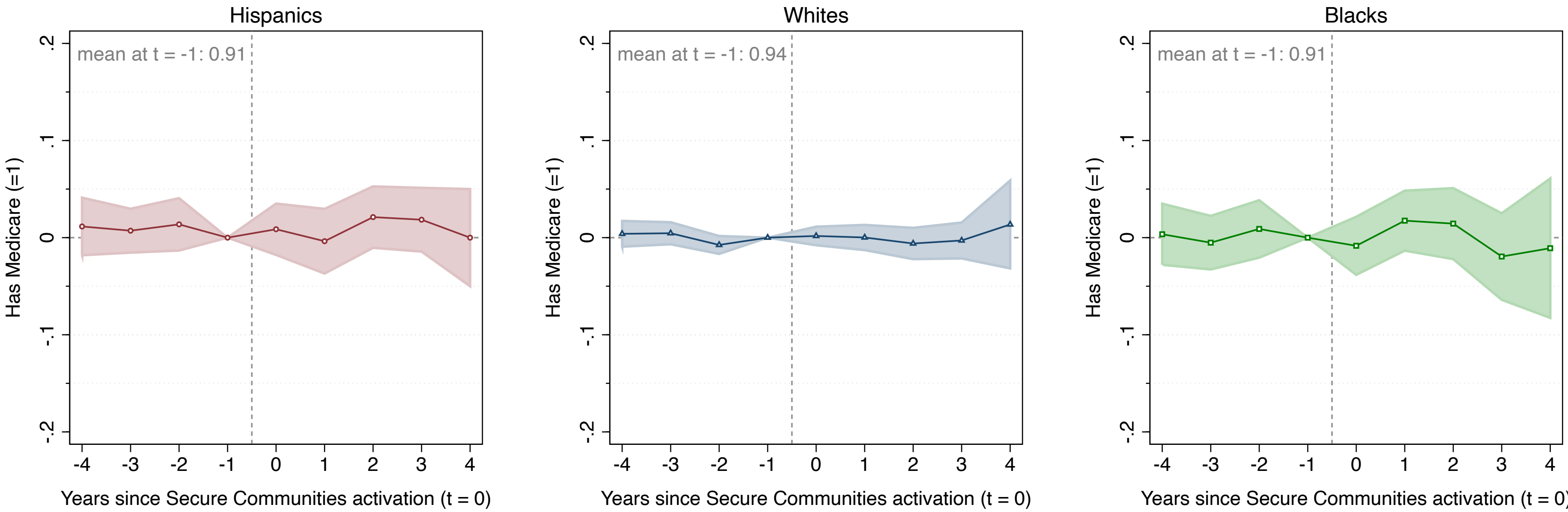
(b) PRIVATE HEALTH INSURANCE



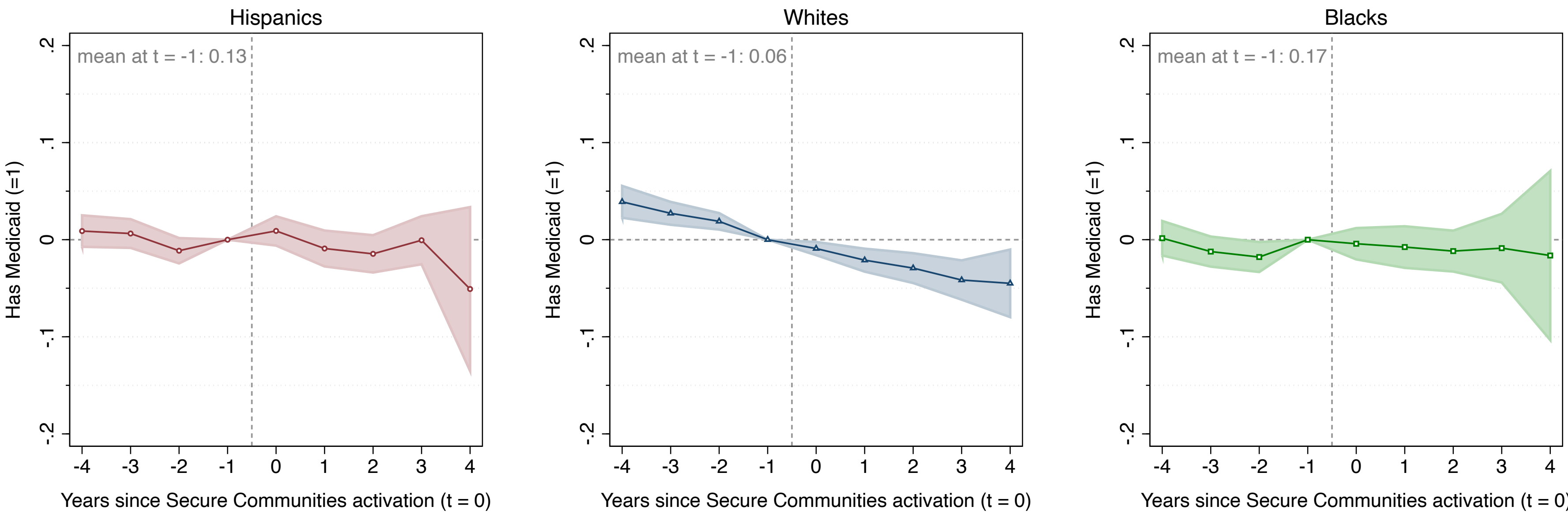
Notes: This figure plots the ϕ coefficients from Specification (3). The shaded areas represent 95% confidence intervals, which have been computed using standard errors clustered at the county level. Each panel displays the results for a given outcome variable, by racial/ethnic group.

Figure 5: [continued] Event-study plots (CPS 2005-2012) with controls

(c) Medicare (for age 65+)



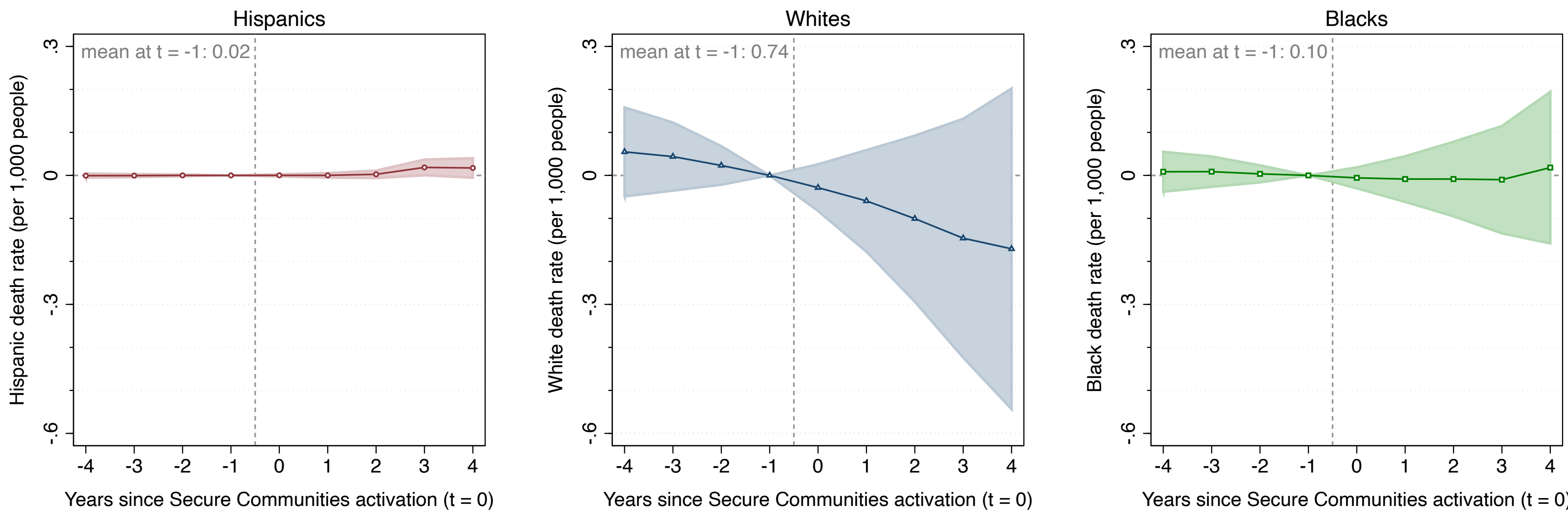
(d) Medicaid



Notes: This figure plots the ϕ coefficients from Specification (3). The shaded areas represent 95% confidence intervals, which have been computed using standard errors clustered at the county level. Each panel displays the results for a given outcome variable, by racial/ethnic group.

Figure 6: Event-study plots (CDC WONDER 2005-2012) with controls

County-level death rates per 1,000 individuals



Notes: This figure plots the ϕ coefficients from Specification (3). The shaded areas represent 95% confidence intervals, which have been computed using standard errors clustered at the county level. Each panel displays the results for a given outcome variable, by racial/ethnic group.

TABLE 1: BRFSS BASELINE SUMMARY STATISTICS

	Hispanics (H)		Blacks (B)		Whites (W)	
	mean	sd	mean	sd	mean	sd
	(1)	(2)	(3)	(4)	(5)	(6)
A. Health outcomes						
Health coverage (=1)	0.68	0.47	0.83	0.38	0.91	0.28
Personal doctor or health care provider (=1)	0.69	0.46	0.84	0.36	0.88	0.32
Over past 12 months:						
Last routine checkup (=1)	0.67	0.47	0.82	0.39	0.72	0.45
Visited eye care professional (=1)	0.62	0.49	0.63	0.48	0.66	0.48
Could not see doctor bc of cost (=1)	0.24	0.43	0.18	0.39	0.10	0.30
Number days [...] in past 30 days:						
Had alcohol	1.46	1.79	1.54	1.84	2.25	2.28
Felt worried/tense/anxious	5.90	9.51	5.21	8.88	4.92	8.20
Treatment based on race over 12 months:						
Felt worse treated when seeking healthcare (=1)	0.16	0.37	0.20	0.40	0.03	0.17
Monthly physical symptoms (=1)	0.10	0.31	0.03	0.16	0.01	0.11
B. Socio-demographics						
Age	45	16	51	16	55	17
Female (=1)	0.65	0.48	0.69	0.46	0.62	0.48
Married (=1)	0.54	0.50	0.33	0.47	0.59	0.49
Income (USD):						
< 15K	0.20	0.40	0.21	0.41	0.09	0.28
[15K, 35K)	0.28	0.45	0.25	0.43	0.15	0.36
[35K, 50K)	0.28	0.45	0.30	0.46	0.29	0.45
50K+	0.24	0.43	0.24	0.43	0.48	0.50
Education:						
High school (=1)	0.43	0.50	0.46	0.50	0.36	0.48
College+ (=1)	0.39	0.49	0.49	0.50	0.62	0.48
Has a job (=1)	0.57	0.50	0.52	0.50	0.53	0.50
C. County characteristics						
Hispanic population	251,292	575,144	81,003	282,123	53,313	207,667
Min. distance from Mex. border (km)	725	610	1,071	414	1,083	467
Unemployment rate	0.05	0.02	0.05	0.02	0.05	0.01
Share of Rep. vote last pres. elections	0.50	0.14	0.46	0.17	0.54	0.13
Observations	23,369		28,852		299,104	

Notes: This table presents summary statistics from the Behavioral Risk Factor Surveillance System (BRFSS) data. The means and standard deviations (sd) are computed for survey year 2007 (the year preceding the start of the start of the implementation of Secure Communities). "Min. distance Mex. border (km)" represents a given county's shortest distance to the U.S.-Mexico border (in kilometers). "Share Rep. vote last pres. elections" represents the county-level share of Republican votes in the previous presidential elections. The sample excludes "sanctuary jurisdictions."

TABLE 2: CPS BASELINE SUMMARY STATISTICS

	Hispanics (H)		Blacks (B)		Whites (W)	
	mean	sd	mean	sd	mean	sd
	(1)	(2)	(3)	(4)	(5)	(6)
A. Health outcomes						
Any health insurance (=1)	0.62	0.49	0.81	0.39	0.88	0.32
Private health insurance (=1)	0.47	0.50	0.61	0.49	0.79	0.41
Has Medicare (=1)	0.09	0.29	0.18	0.38	0.17	0.38
Has Medicaid (=1)	0.12	0.33	0.15	0.36	0.06	0.23
B. Socio-demographics						
U.S. citizen (=1)	0.60	0.49	0.94	0.24	0.97	0.17
Age	40	16	46	17	46	17
Female (=1)	0.51	0.50	0.57	0.49	0.52	0.50
High school+ (=1)	0.62	0.49	0.84	0.37	0.92	0.28
Married (=1)	0.56	0.50	0.38	0.49	0.62	0.48
Unemployed (=1)	0.04	0.19	0.04	0.21	0.02	0.15
C. County characteristics						
Hispanic population	1,070,582	1,488,264	356,224	904,907	299,242	808,015
Min. dist. Mex. border (km)	605	606	1,159	490	994	538
Unemployment rate	0.05	0.02	0.05	0.01	0.04	0.01
Share Rep. vote	0.45	0.14	0.38	0.18	0.49	0.13
Observations	13,119		8,195		33,530	

Notes: This table presents summary statistics from the March Supplements of the Current Population Survey (CPS) data. The means and standard deviations (sd) are computed for survey year 2007 (the year preceding the start of the start of the implementation of Secure Communities). "Min. dist. Mex. border (km)" represents a given county's shortest distance to the U.S.-Mexico border (in kilometers). "Share Rep. vote" represents the county-level share of Republican votes in the previous presidential elections. The sample excludes "sanctuary jurisdictions."

TABLE 3: TRIPLE-DIFFERENCE RESULTS (BRFSS 2005-2012)
WITH CONTROLS

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Last routine checkup over past 12 mo. (=1)	Could not see doctor over past 12 mo. bc of cost	Personal doctor or health care provider (=1)	Any health coverage (=1)	Has a job (=1)
Post × Hispanic	-0.033***	0.012**	-0.0038	-0.016*	0.0096*
	(0.005)	(0.006)	(0.007)	(0.008)	(0.005)
Post	-0.00028	-0.0016	0.0029	0.0073***	-0.0022
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Post × Black	-0.018***	0.0025	-0.011***	-0.0039	-0.0063
	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)
Observations	2,682,581	2,708,357	2,707,447	2,707,506	2,706,649
Number of clusters	2,360	2,360	2,360	2,360	2,360
Pre-activation Hispanic mean	0.67	0.23	0.70	0.71	0.56
Pre-activation White mean	0.72	0.10	0.89	0.92	0.53
Pre-activation Black mean	0.82	0.19	0.85	0.83	0.50

Notes: This table displays the results from the triple-difference analysis – see Specification (1). Each column is a separate regression. All regressions include calendar year fixed effects, calendar month fixed effects, county-specific linear time trends, and socio-demographic controls (see Table 1 for the list of controls). See Appendix Table B.1 for the corresponding results without controls. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

Table 4: Triple-difference results (CPS 2005-2012) with controls

	(1)	(2)	(3)	(4)
Dependent variable:	Any health insurance (=1)	Private health insurance (=1)	Medicare (=1)	Medicaid (=1)
Post × Hispanic	-0.011*	-0.012	-0.0014	0.0013
	(0.007)	(0.010)	(0.009)	(0.009)
Post	-0.0020	-0.0034	-0.0030	0.0018
	(0.004)	(0.005)	(0.006)	(0.004)
Post × Black	-0.0062	-0.018**	0.0010	0.011
	(0.006)	(0.009)	(0.010)	(0.007)
Observations	388,877	388,877	60,650	388,877
Number of clusters	328	328	323	328
Pre-activation Hispanic mean	0.76	0.57	0.92	0.15
Pre-activation White mean	0.89	0.79	0.95	0.06
Pre-activation Black mean	0.82	0.60	0.91	0.17

Notes: This table displays the results from the triple-difference analysis – see Specification (1). Each column is a separate regression. All regressions include calendar year fixed effects, calendar month fixed effects, county-specific linear time trends, and socio-demographic controls (see Table 2 for the list of controls). See Appendix Table B.4 for the corresponding results without controls. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

Table 5: Triple-difference results (BRFSS 2005-2012): other health outcomes with controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Num. days per week had alcohol over past 30 days	Num. days felt worried/anxious over past 30 days	Worse treated than other races over past 12 mo. when seeking health-care	Monthly physical symptoms due to treatment based on race over past 12 mo. (=1)	Visited eye care professional over past 12 mo. (=1)
Post × Hispanic	0.045*	2.36**	-0.069***	0.030**	-0.037*
	(0.025)	(0.937)	(0.023)	(0.012)	(0.021)
Post	-0.0088	-0.68	-0.0059	-0.0034	-0.034
	(0.015)	(0.761)	(0.011)	(0.006)	(0.021)
Post × Black	0.014	1.59**	-0.0040	0.0042	0.037
	(0.024)	(0.685)	(0.020)	(0.023)	(0.026)
Observations	1,685,015	42,050	68,984	78,353	144,354
Number of clusters	2,358	379	511	512	1,021
Pre-activation Hispanic mean	1.36	5.80	0.06	0.07	0.57
Pre-activation White mean	2.08	4.88	0.02	0.01	0.62
Pre-activation Black mean	1.32	4.68	0.12	0.06	0.60

Notes: This table displays the results from the triple-difference analysis – see Specification (1). Each column is a separate regression. All regressions include calendar year fixed effects, calendar month fixed effects, county-specific linear time trends, and socio-demographic controls (see Table 1 for the list of controls). See Appendix Table B.5 for the corresponding results without controls. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

Table 6: Triple-difference heterogeneity results by removals (BRFSS 2005-2012) with controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Last routine checkup over past 12 mo. (=1)	Could not see doctor over past 12 mo. bc of cost	Personal doctor or health care provider (=1)	Any health coverage (=1)	Has a job (=1)
Post × Hispanic × Pop. removed	0.035	0.038	-0.044	0.030	-0.032
	(0.042)	(0.045)	(0.048)	(0.046)	(0.041)
Post × Black × Pop. removed	0.036	-0.018	0.017	0.027	0.043
	(0.041)	(0.033)	(0.032)	(0.036)	(0.030)
Post × Hispanic	-0.0048	-0.0062	0.048***	0.018	-0.0021
	(0.015)	(0.013)	(0.014)	(0.018)	(0.022)
Post × Black	-0.023**	0.0078	-0.0032	-0.00062	0.00015
	(0.011)	(0.014)	(0.013)	(0.010)	(0.011)
Post	-0.0030	-0.00033	-0.0042	0.0022	0.00079
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
Observations	2,567,422	2,592,031	2,591,153	2,591,235	2,590,396
Number of clusters	2,103	2,103	2,103	2,103	2,103
Pre-activation Hispanic mean	0.67	0.23	0.70	0.71	0.56
Pre-activation White mean	0.72	0.10	0.89	0.92	0.53
Pre-activation Black mean	0.82	0.19	0.85	0.83	0.50

Notes: This table displays the results from the triple-difference heterogeneity analysis – see Specification (1). Each column is a separate regression. "Pop. removed" is a dummy variable that is equal to one if a given county is above the median cumulative number of removals in a given year. All regressions include calendar year fixed effects, calendar month fixed effects, county-specific linear time trends, and socio-demographic controls (see Table 1 for the list of controls). See Appendix Table B.8 for the heterogeneity results using additional health outcome variables. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

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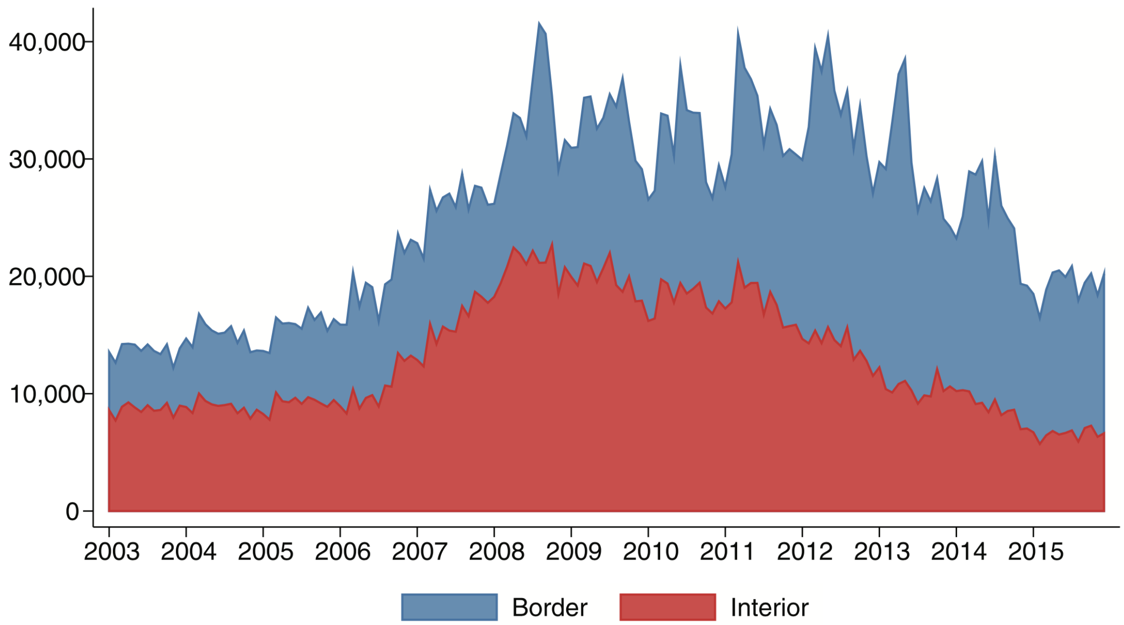
Appendix

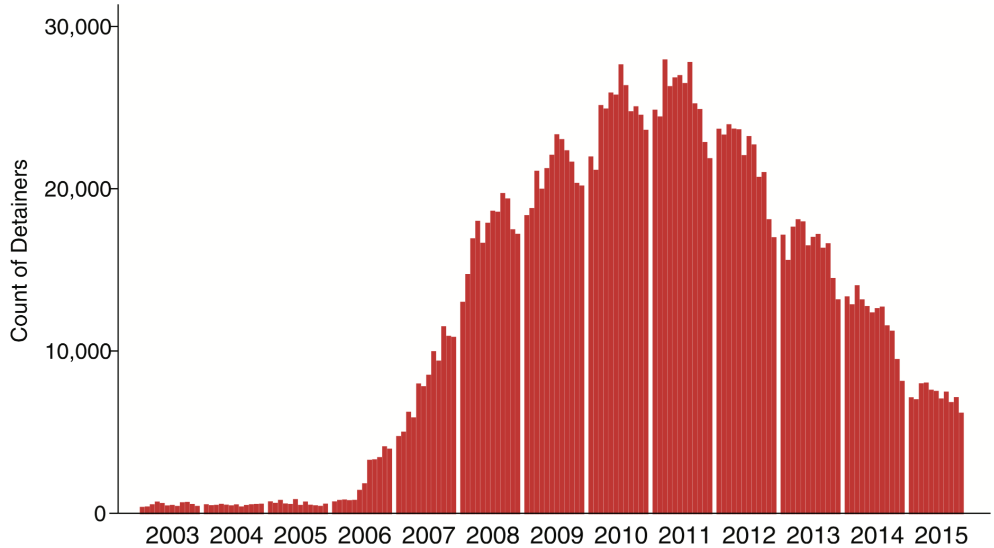
A Appendix Figures

Appendix Figure A.1: Removals by apprehension source, 2003-2016



Notes: This figure displays the annual number of removals separately at the border (blue) and in the U.S. interior (red) between 2003 and 2016. This figure is taken directly from Bellows (2019).

Appendix Figure A.2: Detainers or notice requests, 2003-2016



Notes: This figure displays the yearly number of detainers or notice requests between 2003 and 2015. This figure is taken directly from Bellows (2019).

B Appendix Tables

Appendix Table B.1: Triple-difference results (BRFSS 2005-2012) without controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Last routine checkup over past 12 mo. (=1)	Could not see doctor over past 12 mo. bc of cost	Personal doctor or health care provider (=1)	Any health coverage (=1)	Has a job (=1)
Post × Hispanic	-0.040***	0.016**	-0.0023	-0.024**	0.0015
	(0.007)	(0.007)	(0.008)	(0.010)	(0.007)
Post	0.0038*	0.020***	-0.014***	-0.015***	-0.044***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Post × Black	-0.027***	0.0084*	-0.018***	-0.013**	0.0047
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
Observations	2,682,584	2,708,360	2,707,450	2,707,509	2,706,652
Number of clusters	2,363	2,363	2,363	2,363	2,363
Pre-activation Hispanic mean	0.67	0.23	0.70	0.71	0.56
Pre-activation White mean	0.72	0.10	0.89	0.92	0.53
Pre-activation Black mean	0.82	0.19	0.85	0.83	0.50

Notes: This table displays the results from the triple-difference analysis – see Specification (1). Each column is a separate regression. Neither controls nor fixed effects have been included in the regressions. See Table 3 for the corresponding results with controls. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table B.2: Triple-difference results (BRFSS 2005-2012) – excluding whites with controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Last routine checkup over past 12 mo. (=1)	Could not see doctor over past 12 mo. bc of cost	Personal doctor or health care provider (=1)	Any health coverage (=1)	Has a job (=1)
Post × Hispanic	-0.019***	0.0077	-0.00033	-0.020**	0.016**
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
Post	0.0036	-0.0049	0.0088	0.028***	-0.0062
	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	411,860	415,024	414,965	414,945	414,719
Number of clusters	2,201	2,199	2,200	2,199	2,198
Pre-activation Hispanic mean	0.67	0.23	0.70	0.71	0.56
Pre-activation Black mean	0.82	0.19	0.85	0.83	0.50

Notes: This table displays the results from the triple-difference analysis, after excluding white individuals from the analysis sample. Each column is a separate regression. All regressions include calendar year fixed effects, calendar month fixed effects, county-specific linear time trends, and socio-demographic controls (see Table 1 for the list of controls). See Appendix Table B.3 for the corresponding results without controls. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table B.3: Triple-difference results (BRFSS 2005-2012) – excluding whites without controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Last routine checkup over past 12 mo. (=1)	Could not see doctor over past 12 mo. bc of cost	Personal doctor or health care provider (=1)	Any health coverage (=1)	Has a job (=1)
Post $\times$ Hispanic	-0.013*	0.0077	0.016*	-0.011	-0.0032
	(0.008)	(0.007)	(0.009)	(0.010)	(0.008)
Post	-0.023***	0.028***	-0.032***	-0.029***	-0.039***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
Observations	411,951	415,117	415,057	415,038	414,813
Number of clusters	2,292	2,292	2,292	2,292	2,292
Pre-activation Hispanic mean	0.67	0.23	0.70	0.71	0.56
Pre-activation Black mean	0.82	0.19	0.85	0.83	0.50

Notes: This table displays the results from the triple-difference analysis, after excluding white individuals from the analysis sample. Each column is a separate regression. Neither controls nor fixed effects have been included in the regressions. See Appendix Table B.2 for the corresponding results with controls. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table B.4: Triple-difference results (CPS 2005-2012) without controls

	(1)	(2)	(3)	(4)
Dependent variable:	Any health insurance (=1)	Private health insurance (=1)	Medicare (=1)	Medicaid (=1)
Post × Hispanic	-0.023***	-0.0060	0.0063	-0.014
	(0.008)	(0.011)	(0.009)	(0.014)
Post	-0.019***	-0.036***	-0.012***	0.0050
	(0.003)	(0.006)	(0.003)	(0.003)
Post × Black	-0.0093	-0.0020	0.0085	-0.0050
	(0.008)	(0.012)	(0.010)	(0.009)
Observations	390,717	390,717	60,651	390,717
Number of clusters	329	329	324	329
Pre-activation Hispanic mean	0.76	0.57	0.92	0.14
Pre-activation White mean	0.89	0.78	0.95	0.06
Pre-activation Black mean	0.82	0.60	0.91	0.17

Notes: This table displays the results from the triple-difference analysis – see Specification (1). Each column is a separate regression. Neither controls nor fixed effects have been included in the regressions. See Table 4 for the corresponding results with controls. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table B.5: Triple-difference results (BRFSS 2005-2012): other health outcomes
without controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Num. days per week had alcohol over past 30 days	Num. days felt worried/anxious over past 30 days	Worse treated than other races over past 12 mo. when seeking health-care	Monthly physical symptoms due to treatment based on race over past 12 mo. (=1)	Visited eye care professional over past 12 mo. (=1)
Post × Hispanic	0.0053	1.43	-0.051**	0.036***	-0.092***
	(0.031)	(0.987)	(0.022)	(0.013)	(0.022)
Post	-0.54***	0.78***	0.0021	0.0067***	0.025***
	(0.018)	(0.183)	(0.004)	(0.002)	(0.009)
Post × Black	0.031	1.23*	-0.021	0.00086	0.013
	(0.030)	(0.714)	(0.020)	(0.020)	(0.026)
Observations	1,685,018	42,051	68,986	78,354	144,354
Number of clusters	2,361	380	513	513	1,021
Pre-activation Hispanic mean	1.36	5.80	0.06	0.07	0.57
Pre-activation White mean	2.08	4.88	0.02	0.01	0.62
Pre-activation Black mean	1.32	4.68	0.12	0.06	0.60

Notes: This table displays the results from the triple-difference analysis – see Specification (1). Each column is a separate regression. Neither controls nor fixed effects have been included in the regressions. See Table 5 for the corresponding results with controls. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table B.6: Triple-difference results (BRFSS 2005-2012): other health outcomes – excluding whites
with controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Num. days per week had alcohol over past 30 days	Num. days felt worried/anxious over past 30 days	Worse treated than other races over past 12 mo. when seeking health-care	Monthly physical symptoms due to treatment based on race over past 12 mo. (=1)	Visited eye care professional over past 12 mo. (=1)
Post × Hispanic	-0.016	0.31	-0.068	0.019	-0.070*
	(0.031)	(0.813)	(0.042)	(0.033)	(0.036)
Post	-0.00031	-0.31	-0.047	-0.0092	0.019
	(0.037)	(0.846)	(0.043)	(0.014)	(0.035)
Observations	221,209	7,579	7,947	8,455	23,307
Number of clusters	2,054	251	333	345	661
Pre-activation Hispanic mean	1.36	5.81	0.06	0.07	0.57
Pre-activation Black mean	1.32	4.68	0.12	0.06	0.60

Notes: This table displays the results from the triple-difference analysis, after excluding white individuals from the analysis sample. Each column is a separate regression. All regressions include calendar year fixed effects, calendar month fixed effects, county-specific linear time trends, and socio-demographic controls (see Table 1 for the list of controls). See Appendix Table B.7 for the corresponding results without controls; and see Table 5 for the corresponding triple-diff results. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table B.7: Triple-difference results (BRFSS 2005-2012): other health outcomes – excluding whites without controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Num. days per week had alcohol over past 30 days	Num. days felt worried/anxious over past 30 days	Worse treated than other races over past 12 mo. when seeking healthcare	Monthly physical symptoms due to treatment based on race over past 12 mo. (=1)	Visited eye care professional over past 12 mo. (=1)
Post × Hispanic	-0.026	0.20	-0.029	0.035*	-0.11***
	(0.033)	(0.911)	(0.035)	(0.021)	(0.033)
Post	-0.51***	2.01***	-0.019	0.0076	0.038
	(0.029)	(0.638)	(0.023)	(0.021)	(0.024)
Observations	221,371	7,650	8,036	8,540	23,458
Number of clusters	2,216	322	422	430	812
Pre-activation Hispanic mean	1.36	5.80	0.06	0.07	0.57
Pre-activation Black mean	1.32	4.68	0.12	0.06	0.60

Notes: This table displays the results from the triple-difference analysis, after excluding white individuals from the analysis sample. Each column is a separate regression. Neither controls nor fixed effects have been included in the regressions. See Table 5 for the corresponding results with controls; and see Appendix Table B.5 for the corresponding triple-diff results. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table B.8: Triple-difference heterogeneity results by removals (BRFSS 2005-2012): other health outcomes with controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Num. days per week had alcohol over past 30 days	Num. days felt worried/anxious over past 30 days	Worse treated than other races over past 12 mo. when seeking health-care	Monthly physical symptoms due to treatment based on race over past 12 mo. (=1)	Visited eye care professional over past 12 mo. (=1)
Post × Hispanic × Pop. removed	0.13	0	0.039	0.083***	-0.038
	(0.187)	(.)	(0.031)	(0.022)	(0.224)
Post × Black × Pop. removed	0.42***	0	-0.031	0.079***	-0.29***
	(0.154)	(.)	(0.129)	(0.024)	(0.083)
Post × Hispanic	0.24***	2.03	-0.081***	-0.024	-0.034
	(0.057)	(2.010)	(0.021)	(0.020)	(0.123)
Post × Black	0.23***	2.82*	0.082	-0.039***	0.11*
	(0.044)	(1.481)	(0.126)	(0.013)	(0.062)
Post	-0.033**	-1.36	0.0079	0.0025	0.027
	(0.017)	(0.877)	(0.012)	(0.005)	(0.036)
Observations	1,615,840	40,254	67,503	76,680	138,431
Number of clusters	2,103	357	481	482	932
Pre-activation Hispanic mean	1.36	5.75	0.06	0.07	0.56
Pre-activation White mean	2.08	4.86	0.02	0.01	0.62
Pre-activation Black mean	1.33	4.75	0.12	0.06	0.60

Notes: This table displays the results from the triple-difference heterogeneity analysis – see Specification (1). "Pop. removed" is a dummy variable that is equal to one if a given county is above the median cumulative number of removals in a given year. Each column is a separate regression. All regressions include calendar year fixed effects, calendar month fixed effects, county-specific linear time trends, and socio-demographic controls (see Table 1 for the list of controls). See Table 6 for the heterogeneity results using the main health outcome variables. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Note that Column (2) has missing values because there were not enough observations.

Appendix Table B.9: Change in respondents' characteristics with Secure Communities activation
(BRFSS 2005-2012)
with year and month fixed effects and county-specific linear trends

Panel A. Individual-level socio-demographics

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Age	Female (=1)	Married (=1)	High school (=1)	College (=1)
Post × Hispanic	-0.43**	0.0087*	-0.0015	-0.0028	-0.011
	(0.177)	(0.005)	(0.006)	(0.006)	(0.008)
Post	0.37***	0.00085	0.0042	-0.0024	0.0047*
	(0.118)	(0.003)	(0.003)	(0.003)	(0.003)
Post × Black	-1.02***	0.0014	-0.0016	-0.014**	0.018***
	(0.201)	(0.005)	(0.005)	(0.006)	(0.006)
Observations	2,694,931	2,713,860	2,713,741	2,709,013	2,709,013
Number of clusters	2,360	2,360	2,360	2,360	2,360
Pre-activation Hispanic mean	45.34	0.63	0.53	0.43	0.40
Pre-activation White mean	55.68	0.62	0.58	0.35	0.63
Pre-activation Black mean	51.18	0.69	0.32	0.47	0.49

Panel B. Individual-level socio-demographics [continued]

	(6)	(7)	(8)	(9)
Dependent variable:	Income below \$15K (=1)	Income between \$15K and \$25K (=1)	Income between \$25 and \$50K (=1)	Income above \$50K (=1)
Post × Hispanic	0.013***	-0.0093*	-0.0038	0.00043
	(0.005)	(0.005)	(0.005)	(0.007)
Post	-0.00085	-0.0016	-0.0037	0.0062*
	(0.002)	(0.002)	(0.003)	(0.003)
Post × Black	0.020***	-0.0083*	-0.017***	0.0045
	(0.005)	(0.005)	(0.004)	(0.007)
Observations	2,356,575	2,356,575	2,356,575	2,356,575
Number of clusters	2,358	2,358	2,358	2,358
Pre-activation Hispanic mean	0.20	0.28	0.28	0.24
Pre-activation White mean	0.09	0.16	0.28	0.47
Pre-activation Black mean	0.22	0.25	0.29	0.24

APPENDIX TABLE 9: [CONTINUED] CHANGE IN RESPONDENTS' CHARACTERISTICS WITH SECURE COMMUNITIES ACTIVATION
(BRFSS 2005-2012)
WITH YEAR AND MONTH FIXED EFFECTS AND COUNTY-SPECIFIC LINEAR TRENDS

PANEL C. COUNTY-LEVEL CONTROLS

	(10)	(11)	(12)	(13)
Dependent variable:	County-level Hispanic population	Min. distance to Mex. border (km)	County-level unemployment rate	County-level share Rep. vote last pres. elections
Post × Hispanic	1760.9	-1.8e-18	0.00067***	0.00084*
	(1741.102)	(0.000)	(0.000)	(0.001)
Post	1519.2**	1.1e-17**	0.0012**	0.00093
	(658.783)	(0.000)	(0.001)	(0.001)
Post × Black	179.8	6.3e-19	0.00035*	-0.0021***
	(155.759)	(0.000)	(0.000)	(0.000)
Observations	2,713,860	2,713,860	2,711,915	2,713,860
Number of clusters	2,360	2,360	2,360	2,360
Pre-activation Hispanic mean	2.4e+05	838.27	0.07	0.47
Pre-activation White mean	49340.57	1,112.55	0.06	0.51
Pre-activation Black mean	77288.74	1,112.46	0.07	0.42

Notes: This table displays the results from the "balance check version" of the triple-difference analysis (see Specification (1)), where each control variable (see Table 1 for the list of controls) serves as the dependent variable. Each column is a separate regression. All regressions include calendar year fixed effects, calendar month fixed effects, and county-specific linear time trends. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table 10: Change in respondents' characteristics with Secure Communities activation
(CPS 2005-2012)
with year and month fixed effects and county-specific linear trends

Panel A. Individual-level socio-demographics

	(1)	(2)	(3)	(4)
Dependent variable:	U.S. citizen (=1)	Age	Female (=1)	Married (=1)
Post × Hispanic	0.030**	0.031	0.0063	-0.0048
	(0.012)	(0.427)	(0.005)	(0.008)
Post	-0.0084**	0.057	0.0055*	0.00073
	(0.004)	(0.213)	(0.003)	(0.005)
Post × Black	0.013	-0.067	-0.0019	-0.014
	(0.008)	(0.429)	(0.006)	(0.009)
Observations	441,350	441,350	441,350	441,350
Number of clusters	329	329	329	329
Pre-activation Hispanic mean	0.62	40.19	0.51	0.54
Pre-activation White mean	0.97	46.27	0.52	0.61
Pre-activation Black mean	0.94	45.54	0.58	0.36

Panel B. Individual-level socio-demographics [continued]

	(5)	(6)	(7)	(8)
Dependent variable:	High school + (=1)	College + (=1)	Bachelor + (=1)	Unemployed (=1)
Post × Hispanic	0.023**	-0.0041	-0.0057	0.0034
	(0.009)	(0.011)	(0.012)	(0.002)
Post	-0.011***	0.00049	-0.0021	-0.0063***
	(0.004)	(0.006)	(0.006)	(0.002)
Post × Black	0.00033	-0.0061	-0.0050	0.0081**
	(0.010)	(0.015)	(0.014)	(0.004)
Observations	441,350	441,350	441,350	439,483
Number of clusters	329	329	329	328
Pre-activation Hispanic mean	0.63	0.18	0.12	0.05
Pre-activation White mean	0.92	0.44	0.34	0.04
Pre-activation Black mean	0.84	0.28	0.20	0.06

Appendix Table 10: [continued] Change in respondents' characteristics with Secure Communities activation
(CPS 2005-2012)
with year and month fixed effects and county-specific linear trends

Panel C. County-level controls

	(9)	(10)	(11)	(12)
Dependent variable:	County-level Hispanic population	Min. distance to Mex. border (km)	County-level unemployment rate	County-level share Rep. vote last pres. elections
Post × Hispanic	2231.5	5.2e-17**	0.0013**	0.0016**
	(2016.210)	(0.000)	(0.001)	(0.001)
Post	1461.8	1.7e-17	0.00013	0.0049**
	(1343.453)	(0.000)	(0.001)	(0.002)
Post × Black	163.0	-7.9e-18	0.00065*	-0.00046
	(234.157)	(0.000)	(0.000)	(0.001)
Observations	441,350	441,350	440,831	441,350
Number of clusters	329	329	329	329
Pre-activation Hispanic mean	9.9e+05	680.17	0.07	0.41
Pre-activation White mean	2.6e+05	1,039.57	0.06	0.46
Pre-activation Black mean	3.1e+05	1,206.44	0.07	0.34

Notes: This table displays the results from the "balance check version" of the triple-difference analysis (see Specification (1)), where each control variable (see Table 2 for the list of controls) serves as the dependent variable. Each column is a separate regression. All regressions include calendar year fixed effects, calendar month fixed effects, and county-specific linear time trends. Standard errors clustered at the county level are reported in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

C Other Health Data Sources

I here briefly describe the additional health datasets I could use in the future if I am granted access to them. The restricted-use version of these datasets contain sufficiently fine geographic information (e.g., county identifiers) needed for my analysis.

A complementary data source is the National Health Interview Survey (NHIS), a cross-sectional household interview survey conducted on an annual basis by the National Center for Health Statistics (NCHS). It provides information on the health and socio-demographic characteristics of the civilian non-institutionalized population of the U.S.. It notably contains questions on the respondent's health status, self-care, physical and other therapeutic care, mental health care, health insurance, health utilization, prescription medication, opioid use, depression, anxiety, tobacco use, socio-demographics (including zip code of residence, race, employment, family income), and participation in food-related programs (e.g., SNAP).

Another dataset that could be used for my project is the birth data from the National Vital Statistics System (NVSS) – it contains the universe of birth certificates in the U.S.. The dataset includes information on socio-demographic characteristics (such as race, each parent's educational attainment), birth registration area, medical and public services utilization (including prenatal care and WIC[29] food during pregnancy), maternal behavior and health characteristics, and infant health characteristics.

Finally, I intend to use the Healthcare Cost and Utilization Project (HCUP) data – these are administrative panel databases containing encounter-level information on inpatient stays, emergency department visits, and ambulatory surgery in U.S. hospitals. These data will enable me to look directly at healthcare access, utilization, quality, outcomes, and charges. Unfortunately, the data is not publicly available.[30]

[29] WIC is the abbreviation for the Special Supplemental Nutrition Program for Women, Infants, and Children, a federal assistance program of the Food and Nutrition Service of the U.S. Department of Agriculture for healthcare and nutrition of low-income pregnant women, breastfeeding women, and children under age 5.

[30] Princeton has purchased HCUP data for some states and years (see https://dss.princeton.edu/catalog/resource17 for more detail) but they only contain a geographical identifier at the state level. I have completed the training required to have access to the data, and I can now ask Bobray Bordelon, the Economics and Finance Librarian/Data Services Librarian at Princeton, to help me get access to the data. To get access to the data at the county level, I would need to submit another application whose process is longer; I need to ask Bobray Bordelon for more detail on the requirements for this application.