When the American Dream Becomes a Nightmare: Health Implications of Intensified Immigration Enforcement

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1. Introduction

Immigration enforcement in the United States has climbed to extraordinary levels since the passage of the Illegal Immigration Reform and Immigrant Responsibility Act of 1996. A wide range of programs and tactics at the local, state and federal levels, which ranged from 287(g) agreements between Immigration Customs Enforcement and local police to omnibus immigration laws at the state-level, or border patrol operations at the federal level– have been adopted in line with the "consequence delivery system" model of the Department of Homeland Security. As a result, apprehensions and deportations of unauthorized immigrants have reached an unprecedented level in U.S. history. During the Obama Administration, a record number of 1.8 million unauthorized immigrants were deported, leading many to label President Obama as the "deporter-in-chief" (*The Economist*, 2014). Yet, President Trump has only steeped up immigration enforcement. Since he started campaigning for the presidency, he vowed to "round up" and remove the 11 million undocumented immigrants estimated to be living in the United States (*The Economist*, 2016). To that end, during the first year of his presidency, in addition to advocating for the construction of a 2,000 mile wall along the U.S.-Mexico border, he reignited the Secure Communities program and advocated for a "zero tolerance" policy leading to large-scale and indiscriminate deportations,[1] implanting a credible deportation fear among undocumented migrant communities and their families.

In this paper, we focus on the impact of intensified immigration enforcement in the interior of the United States on the health of undocumented adult immigrants and their children, possibly by increasing their deportation fears and anxiety.[2] Indeed, the intensification of *interior*

[1] http://journals.sagepub.com/doi/pdf/10.1177/2331502418786718
[2] For the purpose of this study, we will be using the terms undocumented and unauthorized interchangeably.

immigration enforcement has been shown to be particularly damaging to *families* headed by undocumented, yet, long-term residents of the United States (Rosenblum *et al*. 2014).

While it is undocumented adults who are typically targeted by immigration enforcement, it is important to remember that their children are also affected. Capps *et al.* (2016) report that there were approximately 5.1 U.S. children under age 18 living with at least one unauthorized immigrant parent during the period 2009-2013. Between 2010 and 2012, twenty-three percent of total deportations were issued for parents with U.S. citizen children.[3] Indeed, the vast majority of children with unauthorized parents (82 percent) are U.S. citizens (Capps *et al.* 2013). The constant fear and anxiety that come from being separated or potentially separated from a parent can harm a child's physical growth and development, emotional stability, self-confidence, social skills and ability to learn (Brabeck *et al.* 2014, Allen *et al.* 2015, Brabeck and Xu, 2010). These impacts can take place through different channels. When an unauthorized parent of a U.S. citizen child is arrested, that parent must make what has been termed a "Solomonic Decision"-either move the child with them to the country of the parent's birth or leave the child in the United States in the care of others (Brabeck *et al.* 2014). Changes in family structure may have a negative impact on child well-being due to the economic losses in household income as single-headed households struggle to make ends meet (Dreby 2012, Amuedo-Dorantes and Arenas-Arroyo, 2017). U.S. citizen children may also find themselves overburdened with adult responsibilities, such as caring for a sibling or working-full time at an early age, which can interfere with their schooling progression, socialization and well-being (Menjivar 2006, Amuedo-Dorantes and Lopez 2015).

Even in the absence of deportation, the entire family may have to relocate or start living in the shadows in order to evade apprehension, which in turn limits parental employment and earning

[3] http://www.colorlines.com/articles/nearly-205k-deportations-parents-us-citizens-just-over-two-years

opportunities (Chaudry *et al*. 2010, Lopez 2011). The latter negatively impacts parents as well as their children. Nearly three quarters of children with unauthorized immigrant parents had family incomes below 185 percent of the federal poverty level compared to 51 percent of children of all immigrant and 40 percent of the entire U.S. child population (Capps *et al.* 2016). And, although U.S. citizen children are eligible for social services, research documents that parents often do not apply for assistance out of fear of being apprehended (Watson 2014). Furthermore, undocumented immigrants have reported greater difficulty accessing health services which, in turn, may translate to worse health outcomes when compared to their legal counterparts (*e.g.* Hardy *et al.* 2012, Amuedo-Dorantes *et al.* 2013, Martinez *et al.* 2015, Rhodes *et al.* 2015).

Finally, aside from its impact on children's caring responsibilities, economic resources and accessibility to available health care and public assistance, intensified immigration enforcement can also breed fear, isolation and economic hardship. The latter can translate into depression, separation anxiety disorders, post-traumatic stress disorders and suicidal thoughts among children (Capps *et al*. 2007, Chaudry *et al*. 2010, Androff *et al.* 2011, Hacker *et al*. 2011, Dreby, 2012). Combined with limited access to health care, these conditions can significantly impair mental and physical health (Satinsky *et al*. 2013).

Yet, surprisingly, we know relatively little about the effects of enforcement on the health of immigrants and their (predominantly U.S. born) children. Understanding the impact that intensified immigration enforcement has on the well-being of immigrants and their children is especially important in part because the children of immigrants are the fastest-growing segment of the U.S. population. About 295,000 births were to unauthorized-immigrant parents in 2013, making up 8 percent of the 3.9 million U.S. births that year.[4]

[4] http://www.pewresearch.org/fact-tank/2015/09/11/number-of-babies-born-in-u-s-to-unauthorized-immigrants-declines/

In this study, we address the still limited knowledge on this topic by taking a look at the impact of intensified local and state-level immigration enforcement on the *mental* and *physical* health of those immigrants likely to be targeted by enforcement and their predominantly US citizen children. To that end, we combine micro-level data from the 2006 through 2013 Household, Person, Family, Sample Adult, and Sample Child Files of the National Health Interview Survey (NHIS), with local and state-level data on the implementation of more stringent immigration enforcement measures. We then compare the experienced changes in mental and physical health of adults and children likely impacted by tougher interior immigration enforcement measures, to the changes experienced by other adults and children not likely to be similarly impacted by those policies, from before to after their implementation.

Overall, the findings inform about the negative externalities of immigration enforcement practices on the health of immigrants and their (U.S. born) children. Understanding the impact of these policies is crucial. After all, protecting Americans, even more so children, is an American value cherished by international human rights that should be observed regardless of adult/parental actions and/or legal status. Furthermore, the health and wellbeing of this vulnerable population of children should be of concern to policymakers for various reasons given that it is a fast growing segment of the U.S. population. It is a population segment that will significantly impact the health care and education systems, as well as the future productivity of the country's workforce. For that reason, various authors (including Brabeck and Xu (2010, Hardy *et al.* (2015), Philbin *et al.* (2017) and Yoshikawa *et al.* (2017)) call for additional causal research regarding the effect of immigration policies on the health of children and outline several pathways through which such policies may affect the health of children –a call reignited by the national uproar over the Trump

Administration's zero-tolerance policy, which received harsh criticism for separating migrant children from their parents (*The Economist, 2*018).

2. Related Literature

A number of studies, typically based on small samples in specific geographic regions, have shed some light on the effect of living with the threat of deportation on the health of both adult immigrants as well as the children of immigrants. For instance, using a sample of 132 families, Brabeck *et al*. (2014) document how detentions and deportations have a greater impact on the family environment, parental and child well-being when parents are likely unauthorized. In a similar vein, Allen *et al*. (2015) examine the impact of parental deportation on the mental health of 95 children of unauthorized parents residing in Texas using established psychometrically reliable measures of mental health outcomes. In a multiple regression analysis that controlled for sex and age of the child, they found that children whose parents were deported exhibited a greater incidence of internalizing problems (*i.e.* anxiety, depression) and a greater incidence of externalizing problems (*i.e.* aggression, conduct problems) than those whose parents were not deported.

Policies leading to detention and deportation have been shown to also adversely impact the health of children and adults alike. For example, studies based on the experiences of immigrant families after *workplace raids* document the negative consequences of such experiences, including increased anxiety for children (Chaudry *et al.* 2010), increased incidence of low birthweight (which was found to have spillover effects on Latina mothers who were citizens (Novak *et al.* 2017), as well as increased stress and lower self-rated health for adults (Lopez *et al.* 2016). These impacts seem to be, sometimes driven by diminished medical access. In that vein, Beniflah *et al*. (2013) report that the enactment of *immigration enforcement laws* in Georgia lowered pediatric

hospital admissions among immigrants and increased the acuity of the conditions when admissions were made indicating perhaps that immigrants had put off visits to the ER until absolutely necessary. Rhodes *et al.* (2015) use variation in the adoption of *287(g) agreements* at the county level in North Carolina to identify their impact on Hispanic/Latina women's use of prenatal care services. They find that Hispanic/Latina mothers sought prenatal care later in their pregnancies and received inadequate care when compared with non-Hispanic/Latina mothers. They also found that study participants reported profound mistrust of health services, avoiding health services, potentially sacrificing their health and the health of their family members. However, they did not find significant differences in utilization of prenatal care before and after implementation of section 287(g). Finally, using data from the Behavior Risk Factor Surveillance System to create an aggregate index of the *state level policy climate*, Hatzenbuehler *et al.* (2017) find that Latinos who lived in states with a less welcoming policy climate had an increased number of bad mental health days.[5]

More recently, there have been some studies that use national data, coupled with more rigorous research designs, to identify the potentially *causal* effects of immigration reform on health outcomes. Of special interest to us is the study by Wang and Kaushal (2018), who examine the impact that various measures of interior immigration enforcement appear to be having on the mental health and self-reported health of adult immigrants. Using data from the NHIS and a difference-in-differences research design similar to ours, they find that intensified interior immigration enforcement leads to poorer self-reported health and an increased likelihood of

[5] In contrast, a more welcoming policy climate, as the one created by the passage of DACA, appears to have positive health impacts. Using Medicaid claims data from Oregon in a regression discontinuity design that exploits the quasi-random assignment of DACA eligibility among mothers with birthdates close to the DACA age qualification cutoff, Hainmueller *et al.* (2017) find that mothers' DACA eligibility significantly decreased adjustment and anxiety disorder diagnoses among their children.

depression. Our analysis complements *and extends* theirs in several ways. *First*, in addition to examining the impact of intensified immigration enforcement on adults, we explore its impacts on their offspring –a young, potentially more vulnerable population. *Second*, we look at additional health outcomes available in the NHIS. Third, we experiment with alternative treatment and control groups, some of which might prove to be more similar. *Finally*, we have an additional year of data, which provides us with an additional year of policy variation.

3. Background on Interior Immigration Enforcement

The past three decades have witnessed an incredible proliferation of interior immigration enforcement. Since the U.S. Congress passed the Illegal Immigration Reform and Immigrant Responsibility Act of 1996 (IIRIRA), an astonishing 4.5 million undocumented immigrants have been removed from the United States (Bergeron and Hipsman 2014). The IIRIRA regulated some of what would become model measures of interior immigration enforcement since 9/11, including the 287(g) agreements and their successors: the Secure Communities Program and its new version –namely, the Priority Enforcement Program.

Broadly speaking, interior enforcement initiatives over the past decades can be grouped into what we will refer to as *police-based* measures and *employment-based* measures. For simplicity, we call police-based measures all the initiatives that involve either the local or state police (*i.e.* 287(g) agreements, Secure Communities and omnibus immigration laws). In contrast, employment-based measures involve employers (*i.e.* employment verification mandates; henceforth E-Verify). In addition to the parties being directly involved in their implementation (i.e. police vs. employers), the two sets of measures often differ in their predictability. In the case of police-based measures, anybody can be stopped at any time by a police agent, regardless of whether they work or not. In contrast, prospective employees know if the employer uses E-Verify

–typically announced with a sticker on the business door that reads "We E-Verify". Finally, police-based measures are more directly linked to deportations than employment-based measures. While workplace raids can lead to the immediate deportation of detained unauthorized immigrants (Androff *et al.* 2014), there are federal inspections that can, in fact, take place in other places.

For the purpose of this study, and given their link to deportations, we will primarily focus on police-based immigration enforcement measures; even thought we will also gauge the impact of E-Verify. Our *police-based* measures include 287(g) agreements adopted at the local and state levels, as well as the Secure Communities program and Omnibus Immigration Laws. Police-based measures have evolved over time. The 287(g) agreements are one of the earliest police-based immigration enforcement measures. They were contemplated in the 1996 IIRIRA, even though the first one was not signed until 2002 by the state of Florida. There were various versions or types of 287(g) agreements. The so-called "task force" 287(g) agreements provided local and state police officers the authority to interrogate any immigrant, arrest without warrant, and begin the removal process. The "jail enforcement" 287(g) agreements allowed police officers to question immigrants who have been arrested about their immigration status. The 287(g) agreements spread relatively quickly over the second half of the decade of the 2000s. In 2006, only five counties collaborated with the federal government; by 2008, that number had jumped to 41 counties (Wong 2012).

Nevertheless, reports of police abuses, racial profiling and the growing cost of implementing the 287(g) agreements led U.S. Immigration and Customs Enforcement (ICE) to push for the progressive replacement of the 287(g) agreements by the Secure Communities program.[6] Secure Communities (2008-2014) prioritized immigration enforcement among non-

[6] In 2013, the Department of Homeland Security decided not to any new agreements.

citizens who had committed serious crimes. The fingerprints of detainees were checked against the databases from the Federal Bureau of Investigation (FBI) and from the Department of Homeland Security (DHS) in order to get information on past criminal arrests, convictions, and immigration history. By the end of 2013, all the nation's 3,181 jurisdictions were participating in Secure Communities (U.S. Immigration and Customs Enforcement 2013). The Secure Communities program was replaced by the Priority Enforcement Program (PEP) in 2015, which continues to rely on fingerprint-based biometric data submitted by state and local law enforcement agencies and is mostly targeted to unauthorized immigrants convicted of specific crimes.[7]

Both, the 287(g) agreements and the Secure Community program, were local in nature. In contrast, other police-based measures contemplated in the so-called omnibus immigration laws (2010-present), were state-wide initiatives.[8] A relative small number of states have, nonetheless, enacted these tougher laws (Alabama, Georgia, South Carolina, Utah, and Indiana).[9] While the content of each omnibus immigration law differs, they often include a "show me your papers' clause", which enables the police to request proper identification documentation during a lawful stop. Nonetheless, in some instances, omnibus immigration laws have gone as far as to require that schools verify students' legal status.[10]

Unlike police-based measures, *employment-based* immigration enforcement does not directly involve the local or state police but, rather, the employer. This type of immigration enforcement is exemplified by employment verification mandates (E-Verify). E-verify is an electronic program that allows employers to screen newly hired workers for work eligibility. The

[7] http://www.dhs.gov/sites/default/files/publications/14_1120_memo_secure_communities.pdf
[8] Arizona was the first state to sign an omnibus immigration law in 2010.
[9] http://www.ncsl.org/research/immigration/omnibus-immigration-legislation.aspx
[10] See Alabama's HB56, National Conference of State Legislatures 2012, http://www.ncsl.org/research/immigration/omnibus-immigration-legislation.aspx#Fifty-Three_Omnibus_Bills

program is administered by the U.S. Department of Homeland Security in partnership with the Social Security Administration. With E-Verify, the employer introduces the biographic information (name, social security number, date of birth, citizenship and alien registration number) of the prospective employee into an online program. The software program then cross-checks the prospective employee's records between those in the Social Security Administration (SSA) database and the records from the Department of Homeland Security (DHS) to determine whether the worker is authorized to work in United States. In the case that work eligibility is not confirmed, the employer receives a "tentative no confirmation" that the worker has to resolve within eight business days. While the use of E-Verify is obligatory in the hiring of federal employees, it has been optional at other levels. Some states have mandated its use, either by public agencies and contractors working for public agencies or, in more extreme cases, by all employers in the state. The first E-Verify mandate was implemented in 2006 in the state of Colorado. By 2014, the number of employers enrolled in E-Verify had risen to 482,692.[11]

Overall, regardless of the entities being involved in their implementation (*e.g.* employers, local or state police), all interior immigration enforcement measures had the intent to deter, apprehend and/or deport undocumented migrants. Either through fewer employment opportunities or through direct detention and deportation, families headed by undocumented parents have witnessed a deterioration of their living conditions. In what follows, we refer to these impacts and, in particular, on their effects on children's health in detail.

4. Data and Descriptive Statistics

In order to assess how intensified interior immigration enforcement has impacted the health of adults and children likely impacted by such measures, we combine micro-level data from the

[11] Please, visit: http://www.uscis.gov/e-verify/about-program/history-and-milestones

2006 through 2013 Household, Person, Family, Sample Adult, and Sample Child Files of the NHIS, with local and state-level data on the implementation of more stringent immigration enforcement measures.

4.1 National Health Interview Survey

The NHIS is a nationally representative dataset that provides information on general trends in health, illness, and disability. As such, it is ideal for the assessment of health related issues in response to government public policies. This cross sectional survey covers the non-institutionalized U.S. population and, importantly for our analysis, oversamples blacks, Hispanics and Asians. All adult members of the household ages 18 and above who are present at the time of the interview participate in the survey. Information on children ages 17 and below and persons who are not present at the time of the interview is collected from a responsible adult, 18 years of age or older, residing in the same household. In addition, one adult and one child are randomly selected from each interviewed household to provide more detailed information gathered in the Sample Adult and Sample Child Core files for the NHIS follows a multistage area probability design. Due to its complex design with stratification, clustering, and multistage sampling, the NHIS provides constructed weights for the analysis at hand.[12]

Because the NHIS data lacks sensitive information on the immigration status of migrants our treatment group is chosen on the basis of broad ethnicity and citizenship traits shown to be good predictors of immigrants' unauthorized status (Passel and Cohn 2009, 2011).[13] This is a

[12] We use the final annual person *weight (WTFA)* provided by the NHIS. For further information on the NHIS, please visit: http://www.cdc.gov/nchs/nhis/about_nhis.htm.

[13] At this point, it is worth noting that there are a number of methods used in the literature to proxy for migrants' unauthorized status. Some authors, such as Van Hook *et al*. (2015), use of out-of-sample predictions that rely on datasets containing information on the legal status of immigrants (i.e. *donor* datasets) to infer the legal status of immigrants in other datasets (*i.e.* target datasets). Unfortunately, most datasets containing such information are not representative of the immigrant population. One exception is the Survey of Income and Program Participation (SIPP), which has been deemed to be representative of the immigrant population. Unfortunately, the most recent module containing information on immigrants' legal status refers to 2008 –that is, prior to the rollout of Secure Communities

group examined by prior research (see e.g. Bohn and Pugatch 2013; Passel and Cohn 2009; Pope 2016; Orrenius, Zavodny, and Gutierrez 2018 as well as Wang and Kaushel (2018) who also use the NHIS data), allowing for meaningful comparisons. In addition, because unauthorized immigrants have relatively low educational attainment (see for example, Bohn and Pugatch 2013, Orrenius and Zavodny 2016), we further restrict our treatment group to Hispanic non-citizens with less than a high school diploma or, when examining children outcomes, their offspring.

We experiment with two different control groups. Because of its larger sample size, we first use U.S.-born Hispanics who also lack a high school degree as our control group. When examining the outcomes of children, we use the offspring of this group instead. Subsequently, we experiment with narrowing the control group (or its offspring when focusing on children) to a demographic who is also Hispanic, low skilled (with less than a high school degree) and foreign-born; yet, they are naturalized citizens and, consequently, not the target of tougher immigration enforcement measures. Because our control groups are both low skilled Hispanics, they might be vulnerable to similar unobserved time-varying factors impacting the physical and mental health of Hispanics in the United States; especially when we further restrict our attention to those who are naturalized.

Table 1 presents the summary statistics for the variables used in our study for the adult sample aged 18-64 years. We focus on four dependent variables, whether the sample adults rates their own health as excellent or very good, whether they rate their health as fair or poor, the number of work days missed and a binary variable equal to one if the adult is depressed. This depression

and tougher immigration enforcement. Other authors, like Borjas (2017), also employ a residual approach, initially proposed by Passel *et al.* (2014), to proxy for the likely unauthorized status of the parents. According to this approach, a person is deemed to be legally in the United States in s/he satisfy any of the following criteria: arrived before 1980, has U.S. citizenship, receives public benefits, works in the government sector, was born in Cuba, has an occupation that requires licensing, or has a spouse who is a legal immigrant or U.S. citizen. Everyone else is likely undocumented, often yielding a larger estimate of the undocumented population.

variable is derived from a series of questions belonging to the Kessler 6 (K6) scale which is used for screening for anxiety or mood disorders (Kessler *et al*., 2002). These questions are: In the past 30 days, how often did you feel so sad that nothing could cheer you up? In the past 30 days, how often did you feel nervous? In the past 30 days, how often did you feel restless and fidgety? In the past 30 days, how often did you feel hopeless? In the past 30 days, how often did you feel that everything was an effort? In the past 39 days, how often did you feel worthless? Respondents provide their answers on a scale ranging from 0 (none of the time) to 4 (all of the time). We use the cutoff off suggested by Kessler to indicate possible depression. Our last variable for adults is a variable that indicates whether health has interfered with ability to work.

In the case of children, we work with several outcome variables chosen because of their potential to manifest the stress experienced in the family. For example, researchers have shown that stress is a predictor of the common cold (Cohen *et al*., 1991). Thus, we include information on whether they suffered from frequent colds. We also use the variable from the NHIS that asks parents to assess whether their child has had difficulties with emotions, concentration, behavior or getting along. This is measured on a Likert scale ranging from 0 (no difficulties), 1 (minor difficulties), 2 (definite difficulties) to 3 (severe difficulties). This parental self-report is the only question that is asked about children's mental health in the years that we cover in the survey. Children whose parents responded ''yes, definite difficulties'' or ''yes, severe difficulties'' were defined as having serious overall difficulties which is consistent with how other scholars have interpreted this question.[14] Finally, we use a parental measure of the child's self-assessed health which we dichotomize to being excellent or very good and then fair or poor.

[14] See, for example, http://www.cdc.gov/nchs/data/nhsr/nhsr048.pdf.

4.2 Immigration Enforcement Data

In addition to the health and demographic information, we gather data on the timing and geographic scope of interior enforcement policies. Specifically, data on the enactment of state-level employment verification (E-Verify) mandates –often a key element in the Omnibus Immigration Laws (OIL)– and data on OIL are gathered from the National Conference of State Legislatures (NCSL) website. Data on the implementation of 287(g) agreements and Secure Communities (SC) at the state and local levels are collected from the Immigration and Customs Enforcement (ICE) 287(g) Fact Sheet website, from Kostandini *et al*. (2014), and from the ICE's *Activated Jurisdictions* document, respectively.[15]

We construct an index of the intensity of interior immigration enforcement in each county and year in the sample. Our index is the sum of five variables signalling the existence of an E-Verify mandate at the state level, a state level OIL, a local 287(g) agreement, a state-level 287(g) agreement and participation in the Secure Communities program, respectively. Each of those five variables equals 1 if the county that pursued the measure in question in that particular year, and 0 otherwise. The exception is when the measures were in effect for only part of the year, in which case they equal the fraction of covered months over that year, as follows:

(1) $$Enforcement^{k}{}_{ct} = \frac{1}{12}\sum_{j=1}^{12} \mathbf{1}(E_{j,c})$$

where $\mathbf{1}(E_{j,c})$ is an indicator function that informs about the implementation of measure *k* in county *c* in month *j* during the year in question.

Subsequently, we compute an index of the overall enforcement level to which an individual living in county *c* in year *t* is exposed as the sum of the indices for each enforcement initiative in

[15] ICE (2012a, 2012b, 2012c).

equation (1). As noted earlier, we start by grouping and focusing on what are the most similar measures –namely, police-based enforcement measures. That is:[16]

(2) $$IE_{c,t} = \sum_{k \in K}^{K} Enforcement_{c,t}^{k}$$

The index, which fluctuates between zero and four since we consider four police-based enforcement initiatives, is merged to the NHIS data by (county, year).

The use of an index provides several important advantages. *First*, it is a tractable and more comprehensive way of measuring the overall impact of the diversity of immigration enforcement initiatives we consider herein. *Second*, the index accounts for the number of months each measure was in place in that particular year. In that manner, it allows us to capture the depth and intensity of immigration enforcement in a given county, as opposed to only whether enforcement existed or not. *Third*, immigration enforcement is an interconnected system administered by various federal, state, and local authorities and agencies with similar missions and, some measures, such as Secure Communities, were enacted as a continuum of prior existing measures, like the 287(g) program. The effectiveness of any given measure is often linked to the achievements of its predecessor, or to its combination with other measures. The index allows us to better address this interconnectedness while, at the same time, facilitating the interpretation of the overall impact of intensified enforcement.[17]

As can be seen from Appendix Table 1, our proxy for the intensity of interior immigration enforcement averages and fluctuated significantly between 0 (*i.e.* no enforcement) and 5 (all local

[16] Where *k* refers to each policy, *i.e.*: local 287(g) agreements, state level 287(g) agreements, Secure Communities, and Omnibus Immigration Laws. In subsequent analysis, we also explore the impact of employer-based measures, as exemplified by E-Verify mandates.

[17] Many of the immigration enforcement measures were designed to substitute, replace or continue one another, *e.g.* Secure Communities was intended to replace the 287(g) agreements. In addition, they overlap, which has the potential to exponentially raise a given measure's impact if it builds on the policing infrastructure established by its predecessors.

and state level initiatives) over the time period under consideration. Figure 1 exemplifies the *geographic* and temporal variation in interior immigration enforcement over the period under examination, with a growing number of counties joining in and adopting interior immigration initiatives over time.[18]

4.3 Time-Varying State-Level Characteristics

Finally, we also gather data on a number of time-varying state-level characteristics. As indicated in the description of our model, these include information on the states' population in natural logarithm form, the share of young adults, the share of college graduates and unemployment rates among young adults ages 19-25 and 26-29, as well as an indicator for whether the state has a Republican governor.[19]

5. Methodology

We compare changes in the mental health of unauthorized adult immigrants and their children, to changes experienced by their native, counterparts and their offspring, *before* and *after* the implementation of stringent immigration enforcement measures. Specifically, we estimate the following equation:

$$(3) \qquad Y_{ict} = \alpha + \beta_1 IE_{ct} + \beta_2 TG_i + \beta_3 IE_{ct} * TG_i + X_i\gamma + Z_{st}\delta + \theta_c + \theta_t + \varepsilon_{ict}$$

where: $\varepsilon_{ict} \sim N(\mu, \sigma^2)$, *i*=1…n individuals, *c*=county, *s*=state and *t*=year. The coefficient β_1 informs on the impact of increased immigration enforcement (IE_{ct}) on the physical and mental health of adult or children in our treatment group. TG_i is a dummy variable equal to 1 if the adult or child belongs to our treatment group –that is, s/he is an adult Hispanic non-citizen with less than

[18] The intensification of immigration enforcement not surprisingly overlaps with the increase in interior removals over this period, as documented by Kandel (2016). See: https://fas.org/sgp/crs/homesec/R44627.pdf

[19] Data on the state's population, share of young adults, share of college graduates and unemployment rates among young adults ages 19-25 and 26-29 is gathered from the Census Bureau, whereas data on the political affiliation of the state's governor is obtained from www.uselectionatas.org and www.politico.com.

a high school diploma, or her/his child descendant. Otherwise, s/he belongs to the control group, which is composed of U.S. born Hispanic adults with less than a high school diploma and, when examining children, their offspring. Our coefficient of interest is: β_3, which provides an estimate of the impact of increased immigration enforcement on the health of our demographic group of interest relative to that of their alike counterparts.

The vector X_i includes a number of personal characteristics potentially impacting the health of the individual as detailed in table 1 and include socio-demographic characteristics. The vector Z_{st} includes various time-varying state-level traits capturing the state's economic, political and regulatory environment, including the state's unemployment rate, percentage of state's population with a college degree, the cutoff for Medicaid eligibility for pregnant women (expressed as a percentage of the poverty level), whether a state adopted a mental health parity law, whether a state expanded dependent health insurance coverage prior to the 2010 implementation of the Affordable Care Act and whether the state has a republican governor.

Finally, equation (3) includes a battery of county and year fixed-effects intended to capture idiosyncratic state-level characteristics and economy-wide shocks at the state level. In alternative model specifications, we also include state-level time trends to address any unobserved state-level time-varying characteristics potentially impacting our outcomes. Finally, standard errors will be clustered at the county level.

6. Preliminary Findings

Our main aim is to learn about the impact, if any, of intensified immigration enforcement on the physical and mental health of low-skilled Hispanic non-citizens –traits shown to be correlated to lacking legal immigration status, and their offspring. We will, first, start with the adults and, subsequently, look at their offspring.

6.1 Health Impacts of Intensified Immigration Enforcement on Adults

Table 2 displays the results from estimating equation (3) for four different adult health-related outcomes using alternative model specifications. Our sample is composed of Hispanic non-citizens with less than a high school diploma (our treatment group), and Hispanic natives with less than a high school diploma (our control group). As noted in our description of the data, we look at depression, as a measure of mental health, as well as at two widely used self-rated measures of overall health. Finally, we also inquire about the inability to work due to health problems to get at a more tangible measure of the implications of poor health. In all instances, we start with a simple model specification that only includes the regressors shown in Table 2, to progressively add controls for personal and aggregate level characteristics and, lastly, county and year fixed-effects. While we run out of degrees of freedom when including county-specific time trends, we plan to include state-specific time trends in an alternative model specification. For now, our discussion will be focused on the most complete model specification (4) shown in Table 2.

According to the results shown in Panel A of Table 2, an increase in immigration enforcement equal to the average level of police-based interior immigration enforcement over the period under consideration would increase the incidence of depression among low-skilled Hispanic

non-citizens, when compared to other low-skilled Hispanic natives, by 10 percent.[20] This is despite the fact that low-skilled Hispanic non-citizens are 36 percent less likely to indicate suffering from depression than low-skilled and Hispanic natives. Similarly, in Panel B of Table 2, we find evidence of the same increase in this type of immigration enforcement *lowering* the propensity that, relative to low-skilled Hispanic natives, low-skilled Hispanic non-citizens would rate their physical health as excellent by 2 percent. Yet, the same increase in immigration enforcement would increase the propensity of our treatment group to rate their physical health as fair by a similar amount when compared to other low-skilled Hispanic natives (see Panel C of Table 2). In sum, heightened police-based interior immigration enforcement appears to have damaged both the physical, as well as the mental health of low-skilled Hispanic non-citizens, when compared to other low-skilled Hispanic natives. However, despite its positive sign, we find no statistically significant evidence of intensified immigration enforcement contributing to the probability that low-skilled Hispanic non-citizens would indicate being unable to work due to health related problems, when compared to other low-skilled Hispanic natives (see Panel D of Table 2).

6.1.1 Robustness Checks

To assess the reliability of our findings, we perform a number of robustness checks, which are displayed in Panels A through D in Table 3. First, we repeat the analysis in Table 2 *excluding counties with less than 50 observations*. As can be seen from Panel A in Table 3, our results prove robust to the use of this alternative sample. An increase in immigration enforcement equal to the average level of police-based interior immigration enforcement over the period being studied would increase the incidence of depression among low-skilled Hispanic non-citizens, when

[20] To facilitate their interpretation, we discuss the estimated impacts in percentages always computed as follows: $\{[(\Delta IE * \beta)/\mu_{Dependent\ Variable}] * 100\}$

compared to low-skilled Hispanic natives, by 10 percent. The same increase would lower their propensity to self-rate their physical health as excellent by 2 percent and, instead, increase their tendency to self-rate it as fair by 2 percent. These impacts are alike those found in Table 2.

Next, we experiment with *using an alternative control group* composed of a smaller group of also low-skilled Hispanics who are, nonetheless, also foreign-born; but, naturalized. As such, despite being low-skilled, Hispanic and foreign-born like low-skilled Hispanic non-citizens, they are not the target of immigration enforcement. Our main findings, displayed in Panel B of Table 3, continue to prove robust to the use of this alternative control group. Specifically, the same increase in immigration enforcement continues to raise the incidence of depression among low-skilled Hispanic non-citizens, relative to the control group, by 8 percent. Similarly, it reduces their propensity to self-rate their physical health as excellent by 1.4 percent, whereas it increases their tendency to rate it as fair by 1.4 percent. Furthermore, this is despite the fact that, relative to this new control group, our treatment group is less likely to report depression or rate their physical health as fair (49 percent and 6 percent less likely, respectively). They are also 6 percent more likely than their naturalized counterparts to rate their physical health as excellent.

After 2012, a number of counties eased their collaboration with Immigration Customs Enforcement (ICE) in a number of ways –earning the denomination of "sanctuary cities". In most instances, states would enact a Trust Act that would limit the cooperation with ICE,[21] typically by choosing whether to observe an "ICE hold" or detainer.[22] To the extent that our sample includes

[21] Trust Acts are policies intended to increase community trust and cooperation with the police following the implementation of immigration enforcement measures promoting information sharing between local, state, and federal agencies.

[22] An ICE detainer—or "immigration hold"—is one of the tools used by ICE to apprehend individuals who come in contact with local and state law enforcement agencies. It is a written request that a local jail or other law enforcement agency detain an individual for an additional 48 hours (excluding weekends and holidays) after his or her release date in order to provide ICE agents extra time to decide whether to take the individual into federal custody for removal purposes.

those localities, the measured impacts of intensified immigration enforcement could be, if anything, understated. Hence, in Panel C of Table 3, we replicate our analysis –this time exclusively keeping localities that choose to comply fully with ICE. We find that an increase in immigration enforcement equal to the average level of police-based interior immigration enforcement over the period being studied would increase the incidence of depression among low-skilled Hispanic non-citizens, when compared to low-skilled Hispanic natives, by 11 percent – only slightly larger than the 10 percent increase found in Table 2, although the difference is not statistically different from zero. Similarly, we continue to find that the same increase in immigration enforcement would lower their propensity to self-rate their physical health as excellent by 2 percent and, instead, increase their tendency to self-rate it as fair by 2 percent.

Lastly, as a placebo check, we repeat the analysis focusing on a sample less likely to be the target of intensified immigration enforcement, as would be the case with low-skilled white non-Hispanics. Other things equal, we would expect for immigration enforcement to have no statistically different from zero impacts on the mental and physical health of this demographic, despite our findings for low-skilled Hispanic non-citizens. As we would expect, the estimates in Panel D of Table 3 indicate that increases in interior immigration enforcement have had no apparent impact on the mental or physical health of this group, even if they did among their also low-skilled white, yet Hispanic, counterparts.

6.1.2 Identification Checks

A) Support for the Parallel Trends Assumption

One of the main threats to our empirical approach is whether there existed differential trends in the mental and physical health of low-skilled Hispanic non-citizens and low-skilled Hispanic natives prior to the intensification of interior immigration enforcement that may be

falsely attributed to the change in immigration policy. To investigate whether that is the case, we construct an indicator for the year prior to the immigration enforcement index turning positive in a given county, and interact it with the indicator for belonging to the treated group –that is, for being a low-skilled Hispanic non-citizen, as opposed to a low-skilled Hispanic native. We then include the new terms in equation (3) and re-estimate the model using OLS. If health differences between our treated and control group predated the change in immigration policy observed herein, we would expect the placebo interaction term to produce a statistically significant coefficient in the same direction of the impact of intensified immigration enforcement discussed above. The results of this test are documented in Panel A of Table 4. While the inclusion of the additional terms sweeps away the significance of the coefficients on the interaction terms of the immigration enforcement index and the dummy indicative of the treated group for the first three mental and physical health outcomes being considered,[23] we find no statistically significant placebo interaction terms. Hence, the impacts found in Table 2 do not appear to precede the adoption of tougher enforcement or, similarly, we find no evidence of anticipatory policy impacts.

To offer further reassurance that the results are not driven by longer pre-existing differences between adults in our treated and control groups, we further restrict our sample to a sample of counties observed *prior* to their immigration enforcement index turning positive. We then create a trend, which we interact with the dummy indicative of our treated group. In the absence of any differential health trends between our treated and control groups, we would expect the coefficient on that interaction term to be non-statistically different from zero. Table 4, Panel B reports the results from this exercise. Consistent with the parallel trends assumption, we find

[23] The exception here is our last outcome –that is, the inability to work due to health related problems. According to the estimate in the last column of Panel A, Table 4, the average increase in immigration enforcement over the period under consideration raised the propensity that low-skilled Hispanic non-citizens, relative to other low-skilled Hispanic natives, would declare being unable to work due to health problems by 10 percent.

no evidence of a pre-existing differential trend driving our results, as point estimates and significance levels survive this restriction on the data.

B) The Non-random Nature of Immigration Enforcement

Another threat to identification is the non-random adoption of tougher immigration enforcement policies by counties. Note, however, that for inference purposes what matters is if counties somehow adopted stricter immigration enforcement policies in response to changes in the mental and physical health outcomes studied herein. To assess if that is the case, we restrict our sample to the year 2000 –that is, a year that preceded the adoption of any interior immigration enforcement measure by any county. When then collapse the data at the county level to estimate the following equation:

$$(4) \qquad Y_c = \alpha + \beta W'^0_c + X'^0_c \gamma + \varepsilon_c$$

where Y_c is equal to either: IE_c –namely, the enforcement level when the enforcement index turned positive in county *c*, or $IE\ Year_c$ –that is, the year in which the immigration enforcement turned positive in county *c*. The vector W'^0_c is the average share of low-skilled Hispanic non-citizens indicating suffering from depression, self-rating their physical health as excellent or as fair in alternative models, or indicating being unable to work due to health related problems, in county *c* in the year 2000. Similarly, the vector X'^0_c contains the county's average demographic and state-level traits measured in vectors *X* and *Z* in equation (3) in that base year. We estimate equation (4) including state fixed effects, and we cluster standard errors at the state level. The results from this exercise are displayed in Panels C and D of Table 4. Pre-existing rates of depression, physical health conditions or work limitations due to health among the population at hand prior to the adoption of stricter enforcement measures do not seem to play a significant role in the timing of tougher immigration enforcement or on its level when first adopted by the county.

6.1.3 Uniqueness of Police-Based Interior Immigration Enforcement

Thus far, we have shown that increases in police-based interior immigration enforcement appear to have deteriorated the mental health (as captured by increases in the incidence of depression) and physical health (as evidenced by decreases in the share declaring to have excellent physical health or increases in the share indicating having only a fair physical health) of low-skilled Hispanic non-citizens. These impacts prove robust to alternative sample specifications excluding counties with few observations, and to the use of more narrowly defined control groups. They also do not seem to radically differ with the exclusion of the so-called "sanctuary cities", suggesting that the targeted population might be responding to the stricter enforcement measures, regardless of the local police's decision-making regarding ICE holds. However, they are not observed among groups less likely to be the target of tougher immigration enforcement, and they do not seem to precede the adoption of tougher enforcement measures by the counties.

To conclude, we look at whether the observed impacts are unique to police-based immigration enforcement associated to increased deportations or, rather, are also observed with other interior immigration enforcement measures not directly linked to deportations, as in the case of employment verification mandates (E-Verify). As noted earlier, E-verify is an electronic program that allows employers to screen newly hired workers for work eligibility. In theory, E-Verify could have the potential to impact the mental and physical health of individuals targeted by the said measures if their employment is restricted by those measures or they fear they signal worse times to come. However, if the latter is not the case, either because the targeted population tends to work in jobs not subject to E-Verify or because they already expected those employment restrictions to be in place, we might not necessarily find any significant impacts.

To address this inquiry, we create an index signaling the number of months E-Verify was in placed in the county at hand in any given year, which we interact with the dummy indicative of the treated group and include along with our police-based interior immigration enforcement index and its corresponding interaction term in our regression. The results from such an exercise are displayed in Table 5. According to the estimates in Table 5, E-Verify does not seem to have had a statistically significant impact on the adult health outcomes being examined, although increase in police-based immigration enforcement continue to display alike effects to the ones documented in Table 2.

In sum, only police-based immigration enforcement, possibly due to its link to deportations, appears to have increased the incidence of depression among low-skilled Hispanic non-citizens, decreased the share of them self-rating their physical health as excellent and increased the share of this population self-rating it as fair, when compared to other similarly low-skilled Hispanic natives.

6.2 Health Impacts of Intensified Immigration Enforcement on Children

Thus far, we have focused our analysis on the impacts that intensified immigration enforcement might have had on the mental and physical health of low-skilled Hispanic non-citizen adults. In what follows, we turn to their offspring to ask alike questions to that demographic. As with adults, we choose measures of parent-rated physical health, as well as reports of any mental health conditions. Note that, unlike for adults, children's health reports are provided by their parents. The latter might prove reticent to revealing any mental conditions of their children or rating their children's overall physical health in a manner that might implicate them. Therefore, we also experiment with using an arguably less controversial measure of a child's health, as would

be the case of the incidence of colds. Furthermore, parents might have an easier time recalling children's colds if they had to miss work during those days.

Table 7 reports the estimates from running the model in equation (3) using, instead, our youth sample, which is composed of a *treated group* composed of children with at least one parent who is a low-skilled Hispanic non-citizen, and a *control group* composed of children whose parents are low-skilled Hispanic natives. As with the adults, we estimate multiple model specifications that progressively add a number of personal and aggregate level controls, as well as county and year fixed-effects. For conciseness, we focus our discussion on the most complete model specification in the last column. Overall, the estimates in Table 7 only reveal a significant impact of intensified immigration enforcement on the propensity of children with a low-skilled Hispanic non-citizen as parent, relative to children in the control group, to have a cold. The aforementioned propensity rises by 5 percent when police-based immigration enforcement intensifies by an amount equal to its average level over the period being examined. This occurs despite the fact that the incidence of colds among children in localities with tougher immigration enforcement appears to be somewhat lower. Finally, these effects are observed even though children from low-skilled Hispanic non-citizen parents exhibit, if anything, better health than their counterparts with low-skilled Hispanic native parents. Indeed, they are 29 percent less likely to be revealed as having mental health problems, when compared to children from low-skilled Hispanic natives.

6.2.1 Robustness Checks

As with their parents, we perform a number of robustness checks to assess the robustness of our finding. To that end, Panels A through D in Table 8 display the estimated coefficients when we restrict our sample to counties with at least 50 observations (Panel A); when we use an

alternative control group composed of low-skilled Hispanic immigrants who are, nonetheless, citizens (Panel B); when we exclude counties with a Trust Act (Panel C); and when we look at the impact of intensified immigration enforcement on a placebo group of children whose parents are not likely to be the target of tougher immigration enforcement –such as white non-Hispanics (Panel D).

As in Panel A of Table 7, the estimates in Panels A, B and C of Table 8 continue to reveal that the same increase in immigration enforcement is associated to roughly a 5 percent increase in the incidence of colds among children of low-skilled Hispanic non-citizens, relative to the control group. This occurs despite the fact that the incidence of colds among children seems lower in counties with tougher immigration enforcement. Furthermore, the estimates in Panel D reveal no evidence of a significant impact of intensified immigration enforcement on the incidence of colds among children of low-skilled white non-Hispanic non-citizens –a group statistically less likely to be the target of tougher immigration enforcement, when compared to children from low-skilled white non-Hispanic natives.

6.2.2 Identification Checks

We also perform the same identification checks completed for the adult sample in Table 9. As can be seen in Panels A and B therein, we find no evidence of a distinct incidence of colds among children in our treated group, relative to children in the control group, preceding the adoption of tougher immigration enforcement by the counties. Specifically, in Panel A, the coefficient on the interaction term between the variable indicative of one year prior to the index turning positive and the dummy variable indicative of whether any of the child's parents are low-skilled Hispanic non-citizens, is non-statistically different from zero. Similarly, in Panel B, the coefficient on the interaction term between the time trend for the period preceding the adoption of

tougher immigration enforcement and the dummy indicative of whether the child belongs to the treated group, is non-statistically different from zero.

Finally, we also examine if our outcome variable could be partially responsible for the adoption of tougher immigration enforcement measures in Panels C and D of Table 9. As can be seen therein, there is no apparent evidence of that being the case. Therefore, while the adoption of tougher immigration enforcement might logically be non-random, it does not appear to have been driven by our dependent variable.

6.2.3 Uniqueness of Police-Based Interior Immigration Enforcement

To conclude, in Table 10, we explore the impact that other measures of intensified immigration enforcement, such as E-Verify, might be having on the incidence of colds among children with low-skilled Hispanic non-citizen parents. As can be seen therein, we continue to find the same impacts of intensified police-based immigration enforcement documented in Panel A of Table 7. However, we also observe a decreased incidence of colds among those children in counties with E-Verify. Perhaps, increased work eligibility controls induce some parents to stay home, benefiting the overall health of their offspring. At any rate, the effect is only marginally significant at the 10 percent level, as well as significantly smaller than that of intensified police-based immigration enforcement. Specifically, an increase in employment-based enforcement equal to its average level for the county over the period under examination would lower the propensity that children with low-skilled Hispanic non-citizen parents might have a cold by 2.5 percent –relative to the 5 percent increase they experience with increased police-based immigration enforcement.

In sum, we find no evidence of a significant impact of intensified police-based immigration enforcement on the incidence of mental health problems or on parents' rating of their children's

overall physical health as being excellent or fair when comparing children from low-skilled Hispanic non-citizen parents to children from low-skilled Hispanic natives. However, there is some evidence of intensified police-based interior immigration enforcement raising the propensity that children from low-skilled Hispanic non-citizen parents might have colds, when compared to their counterparts with low-skilled Hispanic native parents.

7. Summary and Conclusions

We combine micro-level data from the 1999 through 2014 National Health Interview Survey (NHIS), with local and state-level data on the implementation of immigration enforcement measures, to examine the impact of the latter on the mental and physical health of Hispanic non-citizen adult immigrants and their children. First, we examine changes in the health of adult Hispanic non-citizen immigrants, comparing them to the changes experienced by naturalized immigrants before and after the implementation of more stringent immigration enforcement measures. Subsequently, we repeat the analysis focusing on their children and comparing changes in their health following the intensification of immigration enforcement to the changes experienced by their counterparts with naturalized parents.

Our preliminary findings, which prove robust to various identification and robustness checks, reveal that interior enforcement has had some negative effects on the health of non-citizen immigrants and their children. Furthermore, the impacts appear to be primarily driven by police-based immigration enforcement, as opposed to employment-based enforcement measures, such as E-Verify. Specifically, we find that police-based immigration enforcement, possibly due to its link to deportations, raised the incidence of depression among low-skilled Hispanic non-citizens, decreased the share of them self-rating their physical health as excellent and increased the share

of this population self-rating it as fair, when compared to other similarly low-skilled Hispanic natives.

The impacts of intensified immigration enforcement on the offspring of low-skilled Hispanic non-citizens prove much more elusive. We find no evidence of a statistically significant impact of intensified immigration enforcement on the incidence of mental health problems or on parents' rating of their children's overall physical health as being excellent or fair when comparing children from low-skilled Hispanic non-citizen parents to children from low-skilled Hispanic natives. Yet, it appears that police-based interior immigration enforcement raised the propensity that children from low-skilled Hispanic non-citizen parents might have colds, when compared to their counterparts with low-skilled Hispanic native parents.

It is worth noting that examining the impact of intensified immigration enforcement on the physical and mental health of these groups presents a number of challenges. In addition to accessing the data, an important econometric challenge when examining the impact of any policy is the assumption of parallel trends in the outcome being examined. In our case, this implies that the physical and mental health of low-skilled Hispanic non-citizens (and their offspring) and their low-skilled Hispanic foreign-born, yet naturalized, counterparts (and their children) should be no different prior to the adoption of tougher immigration enforcement measures by the counties where they reside. This assumption appears to be valid in our analysis.

Another challenge we face, also common to most policy analyses, is the non-random adoption of the policy in question –in our case, tougher immigration enforcement by the counties. Yet, we find no evidence of the health outcomes being examined helping us predict the adoption timing of the enforcement measures in question. In other words, while the adoption of tougher

enforcement is non-random, it is not driven by the incidence of the health outcomes being examined in the counties in question.

Finally, any policy analysis examining the impact on the immigrant population, in particular, should be concerned about the non-random residential choices made by undocumented immigrants. Unauthorized migrants might choose to reside in areas with lower levels of immigration enforcement. In that case, areas with tougher enforcement measures might have few children with likely unauthorized parents and, as such, we might fail to find a significant impact of intensified enforcement on these children's mental health. We are unable to address this concern with our data. However, we note that, if that were the case, our estimates are likely to provide, if anything, an underestimate of the true impact of intensified immigration enforcement on the physical and mental health of low-skilled Hispanic non-citizens and their children.

Given the intensification of immigration enforcement, especially since February 2017, not to mention the large number of individuals residing in mixed-status households, many of them American children, understanding the impacts of tougher enforcement measures on a population likely at risk is well warranted. We hope the findings will only motivate further analyses on this topic and induce the further release of data to this end.

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Figure 1: The Rollout of Immigration Enforcement

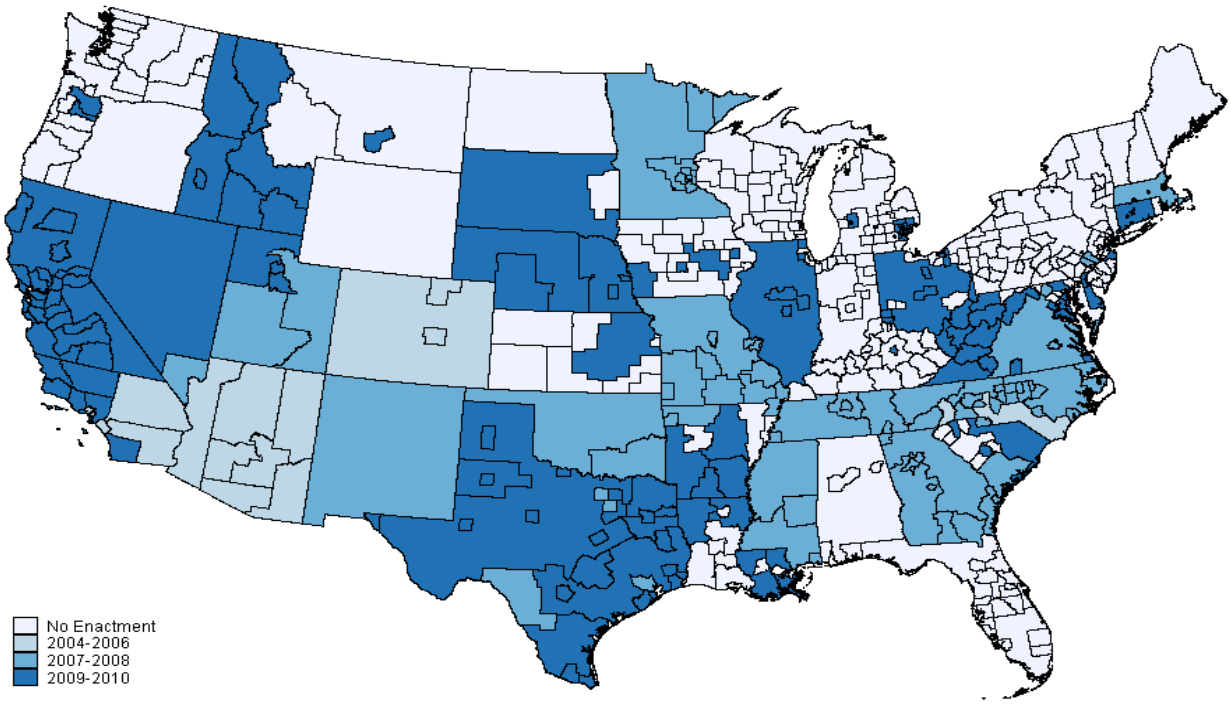

Table 1
Descriptive Statistics for the Adult Samples

Sample	Full Sample		Treatment Group		Control Group		Alternative Control Group	
Statistic	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Dependent Variable Outcomes								
Depression	0.0692	0.2539	0.04631	0.2102	0.1025	0.3033	0.0918	0.2888
Self-rated Excellent Physical Health	0.5014	0.5000	0.5136	0.4998	0.5087	0.4999	0.4499	0.4975
Self-rated Fair Physical Health	0.4986	0.5000	0.4864	0.4998	0.4913	0.4999	0.5501	0.4975
Health-related Work Limitations	0.0557	0.2293	0.0247	0.1552	0.1015	0.302	0.0922	0.2893
Regressors								
Personal Characteristics								
Age	37.7465	12.5437	36.6116	11.1988	34.7132	13.7288	45.5923	11.7532
Gender (Female)	0.4716	0.4992	0.4529	0.4978	0.4954	0.5000	0.4989	0.5000
Years of Education?	8.5359	3.4087	7.5045	3.3012	10.8263	2.3976	8.6734	3.3698
Family Size	4.1256	1.9974	4.3228	2.0281	3.8648	1.9655	3.8377	1.8513
Marital Status (Married)	0.5894	0.4919	0.6364	0.4810	0.4147	0.4927	0.6801	0.4664
Ratio of Family Inc. to poverty level	6.5754	4.3205	6.073	4.1851	7.1804	4.4481	7.3181	4.3215
Worked last year	0.6779	0.4673	0.7051	0.456	0.6165	0.4863	0.6775	0.4674
Live in Urban area	0.9117	0.2838	0.9102	0.2859	0.8975	0.3033	0.9352	0.2462
Aggregate Characteristics								
Log state population	16.5221	0.888	16.5151	0.8844	16.495	0.9308	16.5799	0.8349
Share of young (0-17) in state's pop	0.2811	0.0212	0.2809	0.0207	0.2844	0.0215	0.2771	0.0215
Pct college educated in state	0.1797	0.0312	0.1806	0.0308	0.1735	0.0308	0.1855	0.0317
Republican governor	0.5934	0.4912	0.5775	0.494	0.6338	0.4818	0.5889	0.4921
State unemployment rate	0.0698	0.023	0.0708	0.0234	0.0674	0.0215	0.0701	0.0232
State mental health parity law	0.3097	0.4624	0.3252	0.4685	0.2742	0.4461	0.3118	0.4633
State adopted dependent coverage law	0.4874	0.4998	0.4939	0.5000	0.4874	0.4999	0.4708	0.4992
Medicaid cutoff pregnant women	208.3996	49.6412	210.0585	51.0804	202.8677	46.9103	210.0501	47.5607

Notes: Full Sample: Low Skilled Hispanic Population with less than HS degree. Treatment Group: Low skilled Hispanic Non-citizens with less than HS degree. Control Group: Low skilled Hispanic natives with less than HS degree. Alternative Control Group: Low skilled naturalized Hispanics with less than HS degree.

Table 2
Difference-in-Difference Estimated Impacts of Interior Immigration Enforcement on Adult Health Outcomes

Model Specification	**(1)**	**(2)**	**(3)**	**(3)**
Statistic	**Coefficient (S.E.)**	**Coefficient (S.E.)**	**Coefficient (S.E.)**	**Coefficient (S.E.)**
Panel A: Depression				
Enforcement*Treatment	0.0173***	0.0149***	0.0149***	**0.0149*** **
	(0.0059)	(0.0056)	(0.0056)	**(0.0055)**
Enforcement	-0.0031	-0.0061	-0.0061	-0.0061
	(0.0137)	(0.0140)	(0.0140)	(0.0140)
Treatment Group	-0.0340***	-0.0250***	-0.0250***	-0.0250***
	(0.0082)	(0.0084)	(0.0084)	(0.0081)
Observations	14987	14987	14987	14987
R-squared	0.213	0.2371	0.2371	0.2371
Panel B: Self-rated Excellent Physical Health				
Enforcement*Treatment	-0.0195**	-0.0207**	-0.0207**	**-0.0207** **
	(0.0094)	(0.0087)	(0.0087)	**(0.0087)**
Enforcement	0.0222	0.034	0.034	0.034
	(0.0352)	(0.0340)	(0.0340)	(0.0341)
Treatment Group	0.0618***	0.0128	0.0128	0.0128
	(0.0124)	(0.0106)	(0.0106)	(0.0105)
Observations	43527	43527	43527	43527
R-squared	0.2042	0.2463	0.2463	0.2463
Panel C: Self-rated Fair Physical Health				
Enforcement*Treatment	0.0195**	0.0207**	0.0207**	**0.0207** **
	(0.0094)	(0.0087)	(0.0087)	**(0.0087)**
Enforcement	-0.0222	-0.034	-0.034	-0.034
	(0.0352)	(0.0340)	(0.0340)	(0.0341)
Treatment Group	-0.0618***	-0.0128	-0.0128	-0.0128
	(0.0124)	(0.0106)	(0.0106)	(0.0105)
Observations	43527	43527	43527	43527
R-squared	0.2042	0.2463	0.2463	0.2463
Panel D: Unable to Work Due to Health Problems				
Enforcement*Treatment	0.0071*	0.0068	0.0068	0.0068
	(0.0040)	(0.0042)	(0.0042)	(0.0042)
Enforcement	-0.0064	-0.0112	-0.0112	-0.0112
	(0.0071)	(0.0072)	(0.0072)	(0.0072)
Treatment Group	-0.0499***	-0.0354***	-0.0354***	-0.0354***
	(0.0059)	(0.0043)	(0.0043)	(0.0044)
Observations	43524	43524	43524	43524
R-squared	0.1695	0.2395	0.2395	0.2395
For All Panels Above:				
Personal Characteristics	No	Yes	Yes	Yes
Aggregate Characteristics	No	No	Yes	Yes
County and Year FE	No	No	No	Yes

Notes: All regressions include a constant term. Treatment Group: Low skilled Hispanic Non-citizens with less than HS degree. Control Group: Low skilled Hispanic natives with less than HS degree. Standard errors are clustered at the county level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3
Robustness Checks for Adult Health Outcomes

Outcome	**Depression**	**Self-rated Excellent Physical Health**	**Self-rated Fair Physical Health**	**Unable to Work Due to Health Problems**
Statistic	**Coefficient (S.E.)**	**Coefficient (S.E.)**	**Coefficient (S.E.)**	**Coefficient (S.E.)**
Panel A: Excluding Counties with Less than 50 Observations				
Enforcement*Treatment	**0.0149*** **	**-0.0207** **	**0.0207** **	0.0068
	(0.0055)	**(0.0087)**	**(0.0087)**	(0.0042)
Enforcement	-0.0061	0.034	-0.034	-0.0112
	(0.0140)	(0.0341)	(0.0341)	(0.0072)
Treatment Group	-0.0250***	0.0127	-0.0127	-0.0354***
	(0.0081)	(0.0105)	(0.0105)	(0.0044)
Observations	14981	43516	43516	43513
R-squared	0.2371	0.2462	0.2462	0.2391
Panel B: Using an Alternative Control Group - Low skilled naturalized Hispanics with less than HS degree				
Enforcement*Treatment	**0.0107** **	**-0.0150** **	**0.0150** **	0.0042
	(0.0052)	**(0.0067)**	**(0.0067)**	(0.0043)
Enforcement	-0.0176	0.02	-0.02	0.0029
	(0.0138)	(0.0269)	(0.0269)	(0.0090)
Treatment Group	-0.0341***	0.0282***	-0.0282***	-0.0466***
	(0.0067)	(0.0079)	(0.0079)	(0.0032)
Observations	20151	58202	58202	58202
R-squared	0.2394	0.2298	0.2298	0.2555
Panel C: Excluding Counties with a Trust Act				
Enforcement*Treatment	**0.0163*** **	**-0.0224** **	**0.0224** **	**0.0077** **
	(0.0055)	**(0.0088)**	**(0.0088)**	**(0.0038)**
Enforcement	-0.0094	0.0358	-0.0358	-0.0120*
	(0.0135)	(0.0344)	(0.0344)	(0.0070)
Treatment Group	-0.0259***	0.0129	-0.0129	-0.0355***
	(0.0082)	(0.0105)	(0.0105)	(0.0044)
Observations	14595	42338	42338	42335
R-squared	0.2432	0.2495	0.2495	0.242
Panel D: Placebo Using White Non-Hispanics				
Enforcement*Treatment	-0.0173	**0.0486*** **	**-0.0486*** **	0.0017
	(0.0173)	**(0.0186)**	**(0.0186)**	(0.0116)
Enforcement	-0.0091	0.0077	-0.0077	-0.0163
	(0.0219)	(0.0184)	(0.0184)	(0.0135)
Treatment Group	-0.0217*	0.0738***	-0.0738***	-0.0917***
	(0.0117)	(0.0134)	(0.0134)	(0.0091)
Observations	32998	84253	84253	84264
R-squared	0.3324	0.3122	0.3122	0.3791

Notes: All regressions include a constant term, personal and state-level characteristics, as well as county and time fixed-effects. Standard errors are clustered at the county level. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4
Identification Checks for Adult Health Outcomes

Outcome	**Depression**	**Self-rated Excellent Physical Health**	**Self-rated Fair Physical Health**	**Unable to Work Due to Health Problems**
Statistic	**Coefficient (S.E.)**	**Coefficient (S.E.)**	**Coefficient (S.E.)**	**Coefficient (S.E.)**
Panel A: Assessing Pre-Treatment Parallel Trends #1 – Full Sample				
Enforcement*Treatment	0.0097	-0.0168	0.0168	0.0128*
	(0.0088)	(0.0128)	(0.0128)	(0.0066)
Enforcement	-0.009	-0.0067	0.0067	-0.0112*
	(0.0074)	(0.0130)	(0.0130)	(0.0062)
Treatment	-0.0290***	0.0131	-0.0131	-0.0359***
	(0.0092)	(0.0089)	(0.0089)	(0.0041)
One Yr. Prior to Enf >0	-0.0159	-0.026	0.026	0.0094
	(0.0162)	(0.0279)	(0.0279)	(0.0092)
One Yr. Prior to Enf >0*Treatment	**-0.0062**	**0.006**	**-0.0105**	**0.0105**
	(0.0141)	**(0.0184)**	**(0.0184)**	**(0.0092)**
Observations	14987	43600	43600	43597
R-squared	0.0893	0.1155	0.1155	0.1398
Panel B: Assessing Pre-Treatment Parallel Trends #2 – Sample: Pre-Policy Period				
Treatment	-0.0083	-0.0211	0.0211	-0.0310***
	(0.0089)	(0.0130)	(0.0130)	(0.0078)
Trend	0.0034	-0.0002	0.0002	0.0035**
	(0.0025)	(0.0052)	(0.0052)	(0.0014)
Trend*Treatment	**-0.0017**	**0.0026***	**-0.0026***	**-0.0004**
	(0.0011)	**(0.0014)**	**(0.0014)**	**(0.0007)**
Observations	14987	43600	43600	43597
R-squared	0.0876	0.1127	0.1127	0.139
Panel C: Policy Endogeneity Check #1: Predicting the Policy Adoption – Sample: Pre-Policy Period				
County Share of Outcome	-1.3072	0.2326	-0.3555	-1.3433**
	(0.8126)	(0.1929)	(0.2214)	(0.5325)
Constant	1999.8381***	1986.5487***	1986.5622***	1985.6679***
	(22.8955)	(20.3919)	(20.3786)	(20.2206)
Observations	1556	1928	1928	1928
R-squared	0.541	0.5234	0.5039	0.5047
Panel D: Policy Endogeneity Check #2: Predicting Enforcement When First Adopted – Sample: Pre-Policy Period				
County Share of Outcome	0.1326	-0.0267	0.031	0.0103
	(0.1065)	(0.0255)	(0.0191)	(0.0435)
Constant	2.1503	2.9641	2.9544	3.0686
	(9.1254)	(6.2545)	(6.2483)	(6.2904)
Observations	1556	1928	1928	1928
R-squared	0.8407	0.8437	0.8369	0.8367

Notes: All regressions in *Panels A and B* include a constant term, personal and aggregate characteristics, as well as county and time fixed-effects, and their standard errors are clustered at the county level. All regressions in *Panel's C and D* include the personal and state-level characteristics collapsed at the county level, and their standard errors are clustered at the state level. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5
Policy Channels for Adult Health Outcomes

Outcome	**Depression**	**Self-rated Excellent Physical Health**	**Self-rated Fair Physical Health**	**Unable to Work Due to Health Problems**
Statistic	**Coefficient (S.E.)**	**Coefficient (S.E.)**	**Coefficient (S.E.)**	**Coefficient (S.E.)**
Police-based Enforcement*Treatment	**0.0132****	**-0.0209****	**0.0209****	0.0057
	(0.0059)	**(0.0092)**	**(0.0092)**	(0.0047)
Police-based Enforcement	-0.004	0.0337	-0.0337	-0.0104
	(0.0144)	(0.0346)	(0.0346)	(0.0075)
Employment-based Enforcement*Treatment	0.0144	0.0012	-0.0012	0.0091
	(0.0141)	(0.0198)	(0.0198)	(0.0061)
Employment-based Enforcement	-0.0297	0.0179	-0.0179	-0.0102
	(0.0187)	(0.0588)	(0.0588)	(0.0081)
Treatment Group	-0.0256***	0.0128	-0.0128	-0.0357***
	(0.0082)	(0.0105)	(0.0105)	(0.0044)
Observations	14987	43527	43527	43524
R-squared	0.2372	0.2463	0.2463	0.2396

Notes: All regressions include a constant term, personal and aggregate-level characteristics, as well as county and time fixed-effects. Standard errors are clustered at the county level. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6
Descriptive Statistics for the Child Samples

Sample	Full Sample		Treatment Group		Control Group		Alternative Control Group	
Statistic	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Dependent Variable Outcomes								
Serious mental health issue	0.1816	0.3856	0.136	0.3429	0.1896	0.392	0.198	0.3985
Self-assessed health exc/very good	0.7505	0.4327	0.6699	0.4703	0.7491	0.4335	0.7834	0.4119
Self-assessed health fair/poor	0.2495	0.4327	0.3301	0.4703	0.2509	0.4335	0.2166	0.4119
had a chest cold in past 2 weeks	0.1359	0.3427	0.1252	0.331	0.138	0.3449	0.1396	0.3466
Regressors								
Personal Characteristics								
Age	11.5588	3.1216	11.1695	3.0531	11.4879	3.1105	11.7179	3.1353
Female	0.489	0.4999	0.4887	0.4999	0.4895	0.4999	0.4892	0.4999
Education	5.4215	3.1859	5.0685	3.1032	5.3336	3.1668	5.5658	3.208
Family size	4.7703	1.4836	5.2553	1.5287	4.8282	1.4973	4.572	1.4178
No health care in past year	0.9442	0.2296	0.9473	0.2234	0.9422	0.2334	0.9429	0.232
Education of Household head	12.5771	4.1892	8.6697	3.8113	12.3618	4.2728	14.1742	3.1613
Live in urban area	0.9096	0.2868	0.905	0.2932	0.8965	0.3046	0.9114	0.2841
Aggregate Characteristics								
Log state population	16.5463	0.9007	16.6996	0.852	16.5325	0.9164	16.4836	0.9125
Share of young (0-17) in state's pop	0.2785	0.0216	0.2801	0.0193	0.2799	0.0214	0.2778	0.0225
Pct. college educated in state	0.1815	0.0313	0.1801	0.0285	0.1796	0.0309	0.182	0.0323
Republican governor	0.6019	0.4895	0.5658	0.4957	0.601	0.4897	0.6167	0.4862
State unemployment rate	0.0695	0.0239	0.0725	0.024	0.0694	0.0236	0.0683	0.0237
State mental health parity law	0.3287	0.4697	0.3509	0.4773	0.3306	0.4704	0.3196	0.4663
State adopted dependent coverage law	0.5184	0.4997	0.5041	0.5	0.5247	0.4994	0.5243	0.4994
Medicaid cutoff pregnant women	210.2693	49.8015	219.5157	53.631	209.4563	49.9079	206.49	47.6356
=1 if the county has a Trust Act	0.0255	0.1577	0.0325	0.1773	0.0243	0.1538	0.0227	0.1489

Notes: Full Sample is composed of the following treatment and control groups: (1) Treatment Group: U.S.-born Hispanic children whose mothers or fathers are non-citizen Hispanics with less than a HS education, and (2) Control Group: U.S.-born Hispanic children whose mothers/fathers are Hispanic natives with less than a HS education. The alternative control group is composed of U.S.-born Hispanic children whose mothers or fathers are naturalized Hispanics with less than a HS education.

Table 7
Difference-in-Difference Estimated Impacts of Interior Immigration Enforcement on Child Health Outcomes

Model Specification	**(1)**	**(2)**	**(3)**	**(3)**
Statistic	**Coefficient (S.E.)**	**Coefficient (S.E.)**	**Coefficient (S.E.)**	**Coefficient (S.E.)**
Panel A: Cold				
Enforcement*Treatment	0.0186**	0.0187**	0.0187**	**0.0187****
	(0.0092)	(0.0092)	(0.0092)	**(0.0092)**
Enforcement	-0.0300**	-0.0266*	-0.0266*	-0.0266*
	(0.0150)	(0.0157)	(0.0157)	(0.0157)
Treatment Group	-0.015	-0.0047	-0.0047	-0.0047
	(0.0112)	(0.0111)	(0.0111)	(0.0111)
Observations	15659	15659	15659	15659
R-squared	0.2701	0.2727	0.2727	0.2727
Panel B: Self-rated Excellent Physical Health				
Enforcement*Treatment	-0.024	-0.0192	-0.0192	-0.0192
	(0.0165)	(0.0163)	(0.0163)	(0.0163)
Enforcement	0.0429	0.0354	0.0354	0.0354
	(0.0355)	(0.0343)	(0.0343)	(0.0343)
Treatment Group	-0.0898***	-0.0183	-0.0183	-0.0183
	(0.0130)	(0.0144)	(0.0144)	(0.0144)
Observations	36789	36789	36789	36789
R-squared	0.2259	0.2376	0.2376	0.2376
Panel C: Self-rated Fair Physical Health				
Enforcement*Treatment	0.024	0.0192	0.0192	0.0192
	(0.0165)	(0.0163)	(0.0163)	(0.0163)
Enforcement	-0.0429	-0.0354	-0.0354	-0.0354
	(0.0355)	(0.0343)	(0.0343)	(0.0343)
Treatment Group	0.0898***	0.0183	0.0183	0.0183
	(0.0130)	(0.0144)	(0.0144)	(0.0144)
Observations	36789	36789	36789	36789
R-squared	0.2259	0.2376	0.2376	0.2376
Panel D: Serious Mental Health Problems				
Enforcement*Treatment	0.0094	0.0085	0.0085	0.0085
	(0.0105)	(0.0105)	(0.0105)	[0.0105]
Enforcement	0.0145	0.0239	0.0239	0.0239
	(0.0243)	(0.0253)	(0.0253)	[0.0253]
Treatment Group	-0.0488***	-0.0390***	-0.0390***	-0.0390***
	(0.0107)	(0.0106)	(0.0106)	[0.0106]
Observations	13387	13387	13387	13387
R-squared	0.3109	0.3194	0.3194	0.3194
For All Panels Above:				
Personal Characteristics	No	Yes	Yes	Yes
Aggregate Characteristics	No	No	Yes	Yes
County and Time FE	No	No	No	Yes

Notes: All regressions include a constant term. Standard errors are clustered at the county level. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8
Robustness Checks for Children's Colds

Outcome	**Cold**
Statistic	**Coefficient (S.E.)**
Panel A: Excluding Counties with Less than 50 Observations	
Enforcement*Treatment	0.0187**
	(0.0092)
Enforcement	-0.0265*
	(0.0157)
Treatment	-0.0045
	(0.0111)
Observations	15498
R-squared	0.2687
Panel B: Using an Alternative Control Group - Low skilled naturalized Hispanics with less than HS degree	
Enforcement*Treatment	0.0219*
	[0.0113]
Enforcement	-0.0375*
	[0.0203]
Treatment	-0.0124
	[0.0132]
Observations	12031
R-squared	0.3138
Panel C: Excluding Counties with a Trust Act	
Enforcement*Treatment	0.0186**
	(0.0092)
Enforcement	-0.0267*
	(0.0158)
Treatment	-0.0035
	(0.0111)
Observations	15193
R-squared	0.2766
Panel D: Placebo Using White Non-Hispanics	
Enforcement*Treatment	-0.0366
	(0.0545)
Enforcement	-0.0204
	(0.0165)
Treatment	-0.0198
	(0.0453)
Observations	54060
R-squared	0.2109

Notes: All regressions include a constant term, personal and state-level characteristics, as well as county and time fixed-effects. Standard errors are clustered at the county level. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 9
Identification Checks for Children's Colds

Outcome	**Cold**
Statistic	**Coefficient (S.E.)**
Panel A: Assessing Pre-Treatment Parallel Trends #1 – Full Sample	
Enforcement*Treatment	0.0159
	(0.0141)
Enforcement	-0.0356***
	(0.0122)
Treatment	-0.0149
	(0.0114)
One Yr. Prior to Enf >0	0.0036
	(0.0156)
One Yr. Prior to Enf >0*Treatment	**0.0053**
	(0.0204)
Observations	15659
R-squared	0.0728
Panel B: Assessing Pre-Treatment Parallel Trends #2 – Sample: Pre-Policy Period	
Treatment	0.0102
	(0.0138)
Trend	0.0047*
	(0.0028)
Trend*Treatment	-0.0019
	[0.0013]
Observations	15659
R-squared	0.0689
Panel C: Policy Endogeneity Check #1: Predicting the Policy Adoption – Sample: Pre-Policy Period	
County Share of Outcome	-0.118
	(0.2946)
Constant	1938.8440***
	(22.7880)
Observations	1999
R-squared	0.3783
Panel D: Policy Endogeneity Check #2: Predicting Enforcement When First Adopted – Sample: Pre-Policy Period	
County Share of Outcome	-0.0401*
	(0.0218)
Constant	7.3543
	(5.2500)
Observations	1999
R-squared	0.7157

Notes: All regressions in *Panels A and B* include a constant term, personal and aggregate characteristics, as well as county and time fixed-effects, and their standard errors are clustered at the county level. All regressions in *Panel C* include the personal and state-level characteristics collapsed at the county level, and their standard errors are clustered at the county level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10
Policy Channels for Children's Colds

Outcome	**Cold**
Statistic	**Coefficient (S.E.)**
Police-based Enforcement*Treatment	**0.0221****
	(0.0102)
Police-based Enforcement	-0.0276*
	(0.0157)
Employment-based Enforcement*Treatment	**-0.0339***
	(0.0194)
Employment-based Enforcement	-0.0176
	(0.0317)
Treatment Group	-0.0035
	(0.0111)
Observations	15659
R-squared	0.2729

Notes: All regressions include a constant term, personal and aggregate characteristics, as well as county and time fixed-effects. Standard errors are clustered at the county level. * significant at 10%; ** significant at 5%; *** significant at 1%.

APPENDIX

Immigration Enforcement Index by State and Year

State	2004	2005	2006	2007	2008	2009	2010	Growth
Idaho	0	0	0	0	0	0.483	1.06	1.195
Minnesota	0	0	0	0	1.322	1.997	1.994	0.508
Massachusetts	0	0	0	0.833	1.006	1.037	1.037	0.245
Mississippi	0	0	0	0	1	1	1.048	0.048
Alabama	0.999	0.999	0.998	0.996	0.999	1.002	1.001	0.002
Iowa	0	0	0	0	0	0	0.016	0.000
Kansas	0	0	0	0	0	0	0.001	0.000
Oregon	0	0	0	0	0	0	0.067	0.000
Kentucky	0	0	0	0	0	0	0.005	0.000
West Virginia	0	0	0	0	0	0	0.199	0.000
Ohio	0	0	0	0	0	0	0.067	0.000
Montana	0	0	0	0	0	0	0.091	0.000
South Dakota	0	0	0	0	0	0	0.038	0.000
North Dakota	0	0	0	0	0	0	0	0.000
Maine	0	0	0	0	0	0	0	0.000
Indiana	0	0	0	0	0	0	0	0.000
New Hampshire	0	0	0	0	0	0	0	0.000
Alaska	0	0	0	0	0	0	0	0.000
Wisconsin	0	0	0	0	0	0	0	0.000
Wyoming	0	0	0	0	0	0	0	0.000
Rhode Island	0	0	0	0	0	0	0	0.000
Vermont	0	0	0	0	0	0	0	0.000
D.C.	0	0	0	0	0	0	0	0.000
Washington	0	0	0	0	0	0	0	0.000
Hawaii	0	0	0	0	0	0	0	0.000
New York	0	0	0	0	0	0	0	0.000
Florida	1	0.5	0	0.001	0.011	0.14	0.503	-0.497