Immigration Enforcement and Child Maltreatment*

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Abstract

We study the effects of a major immigration reform on alleged and substantiated maltreatment of Hispanic children using administrative data from child protective services agencies. Secure Communities (SC) ties federal immigration enforcement to local law enforcement. We exploit the staggered rollout of SC across counties to estimate a dynamic treatment effect model. We find that SC implementation increased the number of Hispanic children found to be victims of child maltreatment as well as the likelihood that maltreatment allegations for Hispanic children are substantiated. Results are consistent with stricter immigration enforcement increasing maltreatment among Hispanic children while reducing reporting rates.

Keywords: Immigration enforcement, child maltreatment, child abuse and neglect.
JEL codes: J15, J12, J13, J18.

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1 Introduction

Immigration enforcement has been a policy priority of President Donald Trump's second administration.[1] Much of the rhetoric in favor of strict immigration enforcement centers on targeting undocumented adults, particularly those who pose a threat to public safety (Executive Office of the President, 2025b). Potential impacts on children are often left unaddressed. However, the "chilling effect" of immigration enforcement may have unintended consequences for this vulnerable group. Amidst heightened immigration enforcement activity, doctors report increased no-shows for pediatric appointments, schools report increased absences, and immigrant adults report forgoing work (and similarly, businesses report employees missing work).[2] Despite the prevalence of anecdotal evidence, the effects of immigration enforcement on child well-being remain relatively understudied.

In this paper, we study the effects of an earlier major immigration reform, Secure Communities (SC), on the incidence and reporting of child maltreatment among Hispanic children.[3] Briefly, SC aimed to increase cooperation between local law enforcement and federal immigration enforcement and served to increase both the salience and the likelihood of deportation for unauthorized immigrants. We exploit the staggered, quasi-random rollout of SC across U.S. counties between 2008 and 2013, leveraging the differential timing of program implementation to evaluate its impact using a staggered difference-in-differences design. We construct maltreatment outcomes from administrative data provided by state child protective services (CPS) agencies.

Immigration enforcement can affect administrative child maltreatment outcomes for Hispanic children through two distinct pathways: changes in actual maltreatment rates and changes in reporting behavior. Enforcement may influence underlying maltreatment rates

[1] A December 19, 2025 Department of Homeland Security press release cites "an estimated 1.9 million self-deportations and more than 622,000 deportations" in 2025 (Department of Homeland Security, 2025).

[2] Healthcare: Crouch and Charlotte Ledger, 2025; Payne, 2026. Schools: American Immigration Council, 2026; Bailey, 2026; Bose and Roudi, 2026; Wang, Philip, 2026; Witt, 2026. Employment: Liu and Lakhani, 2025; McGlauflin, 2025.

[3] Child maltreatment refers to abuse or neglect of children under 18 by an adult in a custodial role. We use the terms "maltreatment" and "abuse and neglect" interchangeably.

through opposing mechanisms. On one hand, the increased likelihood of deportation conditional on criminal arrest raises the cost of committing crimes for unauthorized immigrants, potentially deterring child maltreatment if perpetrators respond rationally to these incentives. On the other hand, the threat of deportation may elevate parental mental distress (Wang & Kaushal, 2019) or restrict families' access to essential resources (Alsan & Yang, 2024), factors that could increase abuse and neglect among Hispanic children. Enforcement operations may also alter reporting rates through multiple channels. First, families may reduce their contact with mandated reporters. Bellows (2021) finds that the 287(g) program, a predecessor to SC, decreased school attendance among Hispanic children in North Carolina, limiting their exposure to teachers, an important source of maltreatment reports (Benson et al., 2022). Second, potential reporters may hesitate to engage with the child welfare system if they fear deportation consequences for their own networks or if they internalize the costs to reported families. Third, some reporters may view deportation risk as an additional benefit of reporting—either because they anticipate increased child safety with removal of an unauthorized perpetrator, or because they derive utility from the deportation of unauthorized immigrants—leading to increased reporting rates.[4] We develop a simple conceptual framework to map these possible effects to the child maltreatment outcomes observable in our administrative data.

To study the effects of SC, we use administrative data from the National Child Neglect and Abuse Data System (NCANDS).[5] NCANDS data, which are commonly used by empirical

[4]Child protective services (CPS) agencies rely primarily on individuals outside of the child welfare system to refer potential cases of abuse and neglect. Federal and state laws designate certain professionals as mandated reporters of abuse and neglect, and non-professionals such as parents, relatives, friends, and neighbors may also make referrals to CPS. Although reports by professionals and community members are critical for CPS' detection of and response to child maltreatment, the factors that influence the decisions of potential reporters are not well known.

[5]The data used in this publication were made available by the National Data Archive on Child Abuse and Neglect and have been used with permission. Data from the National Child Abuse and Neglect Data System (NCANDS) were originally collected by the Children's Bureau with the assistance of WRMA, Inc. Funding for the project was provided by the U.S. Department of Health and Human Services, Administration for Children and Families, Administration on Children, Youth and Families, Children's Bureau. The collector of the original data, the funder, NDACAN, Duke University, Cornell University and their agents or employees bear no responsibility for the analyses or interpretations presented here.

researchers across disciplines who study child abuse and neglect, provide information from state CPS agencies on all *investigated* reports of child maltreatment, meaning that the data do not contain *all* maltreatment reports. Thus, we observe neither the true maltreatment rate (which in general is unobservable) nor the maltreatment reporting rate, which is unobservable due to the nature of the NCANDS data. Given this limitation, we study the number of Hispanic children with investigated case(s) per Hispanic child population ("allegation rate"), the number of substantiated victims per child population ("victimization rate"), and the fraction of substantiated victims per child with investigated case(s) ("substantiation rate"). We find that SC increased the victimization and substantiation rates of Hispanic children, without significantly affecting the allegation rate. Taken together, our results are consistent with a combined increase in maltreatment rates and reduction in maltreatment reporting for Hispanic children.

Our results on the effects of SC speak to the potential consequences of America's current immigration enforcement tactics on Hispanic children. On the first day of President Trump's second term in office, his administration removed protections on sensitive locations (e.g., schools, hospitals, and churches), allowing ICE agents to conduct immigration enforcement activities in these spaces (Executive Office of the President, 2025a). A large set of anecdotal evidence suggests that immigrant families have reduced their use of these previously safe spaces (Bailey, 2026; Bose & Roudi, 2026; Crouch & Charlotte Ledger, 2025; Jordan, 2025; Payne, 2026; The New York Times, 2025a, 2025b; Wang, Philip, 2026; Witt, 2026), potentially altering exposure of children to potential reporters of child maltreatment (e.g., teachers) and increasing risk of maltreatment (e.g., medical neglect from failure to provide necessary medical treatment). To the extent that Hispanic adults are less engaged in the labor force, Hispanic families will have fewer resources, putting their children at increased risk of neglect.

We contribute to the broader literature in three ways. First, we add to the body of

work on the wide-ranging effects of immigration enforcement policies.[6] Most relevant to our study is the work on the effects of immigration enforcement on crime incidence and reporting (Comino et al., 2020; Jácome, 2022; Pearson, 2024). Gonçalves et al. (2024) shows that SC simultaneously increased the victimization of Hispanic residents and reduced their willingness to report crimes. This is consistent with a body of work documenting the "chilling effect" of immigration enforcement on take-up of social and medical services among Hispanic individuals (Alsan & Yang, 2024; Rhodes et al., 2015; Watson, 2014). While there is now a robust literature exploring the effects of SC on adults, we know less about the effects of immigration enforcement on child well-being. Vu (2024) finds that SC increased very low birth-weight rates among infants born to foreign-born Hispanic mothers, consistent with fear of deportation elevating maternal stress and reducing access to prenatal nutrition through decreased safety net participation and employment. More closely related to our work, Amuedo-Dorantes and Arenas-Arroyo (2018) find that increased immigration enforcement increases the share of Hispanic children entering foster care. They suggest that deportations and detentions may leave children without caregivers, resulting in increased foster care placements. We study earlier stages in the child protection system, and aim to distinguish between changes in the incidence and reporting of child maltreatment.

Second, we study potential causes of ethnic disparities within the child welfare system (Drake et al., 2023; Johnson-Motoyama et al., 2021). Hispanic children are less likely to be the subject of a CPS investigation than white children with similar socioeconomic characteristics (Putnam-Hornstein et al., 2013).[7] One interpretation of this fact is that, conditional on income, maltreatment is lower among Hispanic children.[8] Alternatively, maltreatment among Hispanic children may be less likely to be reported, potentially for several reasons. While most

[6] We discuss this literature in more detail in Section 2.

[7] While over half of Hispanic children live in or near poverty (Shrider & Creamer, 2023), Hispanic children have better health outcomes than do white children along a variety of dimensions, including low birth weight and infant mortality (Franzini et al., 2001). This seeming contradiction is known as the "Hispanic paradox" (Johnson-Motoyama, 2014).

[8] In the discussion of meta-analysis results, Millett (2016) notes that "...US Studies using CPS and community data suggest that immigrant (mostly Latino) parents may have lower propensity to maltreat their children when compared to US-born families" (p. 1212).

Hispanic children in the U.S. are U.S.-born, about one in four has an unauthorized immigrant parent (Clarke et al., 2017), and reporting to CPS can risk a parent's detention or deportation (Wessler, 2011). In addition, Hispanic children, particularly those with unauthorized parents, may have less contact with mandatory reporters such as teachers and doctors. For instance, 37% of children aged 3–4 with unauthorized parents are enrolled in preschool, compared to 48% of all children (Capps et al., 2016), and Hispanic children are less likely to have regular doctor visits (Abdus & Selden, 2024; Larson et al., 2016). Periods of heightened immigration enforcement may exacerbate these disparities.

Finally, we examine the challenges of using administrative data to study maltreatment in the context of reporting barriers. We suggest avenues for learning about underlying maltreatment and reporting rates from administrative data on alleged and substantiated maltreatment in the face of these challenges.

2 Policy and Institutional Context

2.1 Secure Communities (SC)

SC was a program administered by the U.S. Immigration and Customs Enforcement (ICE), which aimed to increase detection and deportation of unauthorized immigrants, particularly those convicted of a crime (United States Immigration and Customs Enforcement, 2009).[9] Prior to SC, local law enforcement had limited interaction with federal immigration enforcement. Potential unauthorized immigrants were identified primarily through biographic interviews, conducted either by federal officers under the Criminal Alien Program or law enforcement officers in jurisdictions with 287(g) agreements.[10]

Under SC, this process became automated and less labor-intensive. When an individual

[9]For a detailed history of the institutional context of SC, see Alsan and Yang (2024), Online Appendix C.

[10]According to ICE, these "traditional processes of identification are labor-intensive, time-consuming, and are often limited by the accuracy of the biographic information obtained from the subject" (p. 2, United States Immigration and Customs Enforcement, 2009). Accordingly, prior to SC, prisoners were screened by immigration officials in only 14% of local jails and prisons (Cox & Miles, 2013).

was arrested and booked by state or local police, their fingerprints were automatically sent to the FBI, which used them to conduct a criminal background check. Under SC, all fingerprints received by the FBI were automatically shared with the Department of Homeland Security (DHS). DHS then checked those fingerprints against a biometric database that stores information on non-citizens in the U.S.[11] In the event of a match between the arrested individual and the DHS database, ICE issued a "detainer," requesting that local law enforcement hold the individual in custody until ICE could begin deportation proceedings. Thus, under SC, individuals who might otherwise have been released by local law enforcement were instead detained and transferred to federal immigration authorities.

SC represented a major shift in U.S. immigration policy, and the program had a significant impact on the Hispanic community in particular. Over 93% of detainers issued by ICE were to Hispanic individuals (Alsan & Yang, 2024). Moreover, although the program claimed to prioritize public safety and the removal of potentially dangerous individuals, ICE issued a large number of detainers for individuals arrested for low-level and non-violent offenses, and approximately 20% of those removed were never convicted of any crime, or were convicted only of illegal entry or re-entry into the country.[12]

The program began in October 2008 and was rolled out across counties until it covered the entire United States by January 2013. Resource and technological constraints were largely responsible for the county-by-county rollout.[13] The federal government was solely responsible for the pattern of staggered activation, and counties could not decline to participate.[14] Cox and Miles (2013) describe the rollout in detail and test whether early activation was correlated with a number of county-level characteristics. Although a major priority of the program

[11]Specifically, this database stores information on three categories of individuals: (1) non-citizens who have violated immigration law (e.g., were previously deported or overstayed their visas), (2) non-citizens who are in the U.S. legally but who may be deported if convicted of a crime, and (3) citizens who naturalized after their fingerprints were included (Alsan & Yang, 2024; Miles & Cox, 2014).

[12]Deportation statistics are from https://trac.syr.edu/immigration/

[13]As described by Cox and Miles (2013), these constraints included transportation and housing of those taken into custody, communicating with local law enforcement, and the lack of live-scan fingerprint machines in many jurisdictions.

[14]Several states (New York, Massachusetts, and Illinois) did resist the policy. We follow Alsan and Yang (2024) and exclude these states from our estimation sample.

was to identify and deport potentially dangerous individuals, they find that the timing of the rollout was not correlated with crime rates. However, the timing *was* correlated with a higher Hispanic population, shorter distance from the border, and whether the county had previously had a 287(g) agreement with federal law enforcement. Subsequent work has consistently shown that the rollout was not associated with crime rates or economic conditions (East et al., 2022; Gonçalves et al., 2024; Medina-Cortina, 2023). SC was discontinued in November 2014, reactivated in January 2017, and revoked in January 2021.[15] Alsan and Yang (2024) verify that the rollout of the program was salient for individuals at the local level, using an event-study analysis of Google Trends data. In particular, they find that implementation of SC sharply increased normalized deportation-related search terms by 25%.

Previous work has examined the effects of SC on various outcomes. Most relevant to this paper, Gonçalves et al. (2024) show that SC increased victimization of Hispanic individuals *and* reduced the likelihood that Hispanic victims report crimes. When studying effects on total reported crime, these effects cancel one another out, emphasizing the importance of considering reporting effects when studying crime rates.[16] This "chilling effect" on reporting is consistent with other work showing how changes in immigration enforcement affect Hispanic engagement with various government programs (Alsan & Yang, 2024; Comino et al., 2020; Grittner & Johnson, 2021; Watson, 2014).

More broadly, the SC program had widespread effects across various domains, extending beyond social services and crime reporting. The program has been linked to declines in mental health (Wang & Kaushal, 2019) and increased absenteeism among Hispanic students and children of immigrants (Bellows, 2021; Heinrich et al., 2023). Labor market impacts include reduced employment among likely undocumented immigrants (Amuedo-Dorantes & Antman, 2022; East et al., 2022), decreased labor supply among high-skilled citizen mothers, and disruptions in the childcare market (Ali et al., 2024; East & Velásquez, 2024). Immi-

[15]From July 2015 to January 2017, SC was replaced with a program called Priority Enforcement Program (PEP), which used similar methods to identify unauthorized immigrants, but under which only high-priority individuals were subject to detainment and removal.

[16]Miles and Cox (2014) and Hines and Peri (2019) each show a null effect of SC on reported local crime.

grant women also faced declines in wages and hours worked, alongside worsening workplace conditions in industries with high shares of Hispanic workers (Bansak et al., 2024; Grittner & Johnson, 2021). These findings suggest that SC may have worsened economic conditions for some Hispanic households in the U.S. Results from the extant literature underscore the program's broad and often unintended impacts on Hispanic households and the communities and institutions with which they interact.

2.2 Child Protection System

While the causes of child maltreatment are not fully understood, several risk factors at the individual, family, and community levels correlate with or predict abuse and neglect. These risk factors include child age, parental substance abuse or mental health issues, parent age and socioeconomic status, family stress or divorce, and living in communities with high levels of violence, poverty, or unemployment rates (Austin et al., 2020; Van Berkel et al., 2024). While there are fewer studies on the causal determinants of child maltreatment, several papers show that employment and income are important determinants of maltreatment (Berger et al., 2017; Bullinger et al., 2023; Lindo et al., 2018; Raissian & Bullinger, 2017; Rittenhouse, Forthcoming).

Administrative data on child maltreatment come from child protection services (CPS) agencies. Although CPS agencies are run at the state level, and thus specific policies and procedures are heterogeneous across the U.S., the general process for reporting and investigating child maltreatment is similar across states. In the first stage of the process, potential maltreatment cases are referred to CPS by mandated reporters (e.g., teachers, police, physicians) or other members of the public (e.g., friends, family members, neighbors). Mandated reporters are required by law to refer suspected cases of maltreatment to CPS; specific regulations, penalties for non-compliance, and mandated reporter categories vary by state (Children's Bureau, 2019). The CPS process is inherently linked to law enforcement, as confirmed cases of child maltreatment can result in criminal convictions, and law enforcement

and legal personnel are important sources of maltreatment referrals. In 2023, legal and law enforcement personnel were responsible for 21.4% of screened-in reports to CPS (Children's Bureau, 2023).

Once reported, CPS agencies must decide whether to screen in a referral for further investigation. Referrals are generally screened out if they do not concern child maltreatment, contain insufficient information to proceed with an investigation, fall under a different jurisdiction (e.g., military installation or tribe), or refer to a suspected victim who is not under age 18 (Children's Bureau, 2023). In 2023, over half of referrals were screened in (Children's Bureau, 2023). Once screened in, the case is assigned to an investigator charged with determining whether the allegations of maltreatment are true (substantiated) or likely true (indicated) under state law, what services to provide the family, and whether to remove the child from their home. In 2023, 7.4 children out of 1,000 in the population were found to be victims of maltreatment, i.e., to have substantiated and/or indicated allegation(s) (Children's Bureau, 2023).

The factors which drive reporting child maltreatment are relatively understudied. While descriptive analyses explore patterns in reporting (e.g., Nadon et al. (2023)), causal work has focused on the role of school personnel as reporters of maltreatment (Baron et al., 2020; Benson et al., 2022; Chen & Dube, 2025). In this paper, we investigate the role of reporters in perpetuating the observed disparity in rates of victimization and substantiation across ethnicity, and in particular we consider a barrier to reporting that may heterogeneously affect Hispanic children and families.

3 Data

We obtain administrative data on child maltreatment from the National Child Abuse and Neglect Data System (NCANDS) Child Files ("Children's Bureau, Administration on Children, Youth And Families, Administration For Children And Families, U. S. Department Of

Health And Human Services." Year varies). Each NCANDS Child File includes case-child-level information on all referrals (i.e., reports) of child maltreatment to CPS agencies that were investigated and received a disposition in the fiscal year. Although data submission to NCANDS is voluntary for states, all states currently participate, and most states have done so since 2006. The data include information on reporter type (e.g., educator, relative, medical professional), case disposition (i.e., whether the allegation was substantiated or indicated), and child demographics, including child ethnicity. In our main analysis, we consider two groups: (1) Hispanic children and (2) non-Hispanic children.[17]

The data specify the approximate date of the report and, for a subset of observations, the county.[18] So as not to violate confidentiality, the county name is masked in a given Child File for counties with fewer than 700 investigated cases in the respective fiscal year. Given this convention and our focus, we create a balanced county-by-quarter panel of counties that appear in every Child File from 2006 to 2018 and have referrals involving Hispanic children during all quarters from 2006 to 2015.[19] We follow the extant literature (e.g., Alsan and Yang (2024)) and exclude counties from New York, Illinois, and Massachusetts as these three states actively resisted the rollout of SC; and counties on the U.S. border with Mexico to guard against endogenous rollout activity. This leaves us with a sample of 418 counties, accounting for approximately 12.9% of U.S. counties but over over 53% of the US child population in 2006 (Figure 1). Compared to excluded counties, in 2010 sample counties are more populous, younger, more Hispanic, less rural, with higher shares of multi-generational households and households who rent (Table 1).[20]

Restricting to these sample counties, we merge the 2006 to 2018 Child Files and collapse

[17]Prior to collapsing and imposing any sample restrictions as discussed below, about 17% of children have missing values for ethnicity. These observations are excluded from our analysis.

[18]The report date is rounded to either the 8th of the month (for days 1-15) or the 23rd of the month (for days 16-31). We collapse the data to the quarterly level as discussed in more detail below.

[19]Our focus on counties appearing in every Child File from 2006 to 2018 ensures that our sample reflects referrals that were reported to CPS between 2006 and 2015; while most cases receive a disposition in the year of referral, a small number take more than one year to investigate. This structure of the NCANDS Child Files, along with the masking rule for the county, prevents the construction of an unbalanced panel.

[20]Differences between sample and non-sample counties are similar to those described in Evans et al. (2022), who use earlier versions of the NCANDS Child Files that had a stricter masking convention.

the data by the county and quarter to create three maltreatment measures separately for Hispanic and non-Hispanic children. The allegation rate measures the number of children per 1,000 with investigated maltreatment allegation(s) in the quarter-county. The victimization rate gives the number of children per 1,000 with substantiated or indicated allegation(s) in the quarter-county. Lastly, the substantiation rate reflects the fraction of children with substantiated/indicated allegation(s) among those with allegations. We construct measures of the Hispanic and non-Hispanic child population using annual county population estimates from the U.S. Census.

We use administrative data on the rollout of SC from Alsan and Yang (2024), who obtained the data via Freedom of Information Act requests to ICE.[21] These data include the exact date on which SC was activated in each county in the U.S. We merge implementation dates at the county-quarter level with the county-by-quarter child maltreatment measures. We also include as county-by-quarter controls the unemployment rate and the presence of two alternative immigration programs: 287(g) Memorandums of Agreement (MOAs) and E-Verify.[22] [23] Our sample is a balanced county-by-quarter panel representing 418 counties.

4 Conceptual Framework

A primary challenge in using administrative data to study child maltreatment and its reporting is mapping outcomes of interest to metrics observable in the data. In this section, we present a stylized model that provides a structure for thinking about this mapping. We use the model to explore the potential implications of SC for the three child maltreatment

[21] More information on these data can be found in Alsan and Yang (2024) and the accompanying online appendix.

[22] E-Verify is a federal program that requires all employers to verify their employees' work authorization through an electronic system. The 287(g) agreements allow local law enforcement agencies to partner with Immigration and Customs Enforcement (ICE) to enforce federal immigration law. We control for both programs because they represent alternative immigration enforcement mechanisms that could affect immigrant communities and, consequently, child maltreatment reporting patterns. E-Verify may influence family economic stability, while 287(g) agreements can affect community trust in law enforcement. The source of this data is East et al., 2022

[23] The unemployment data comes from the U.S. Bureau of Labor Statistics, see: https://www.bls.gov/lau/

outcomes we construct from the NCANDS data: allegation rate, victimization rate, and substantiation rate.[24]

We assume an underlying, unobservable distribution of maltreatment risk among children in a given county-quarter as depicted in Figure 2a.[25] First, we consider the allegation rate, or the number of Hispanic children per 1,000 with at least one child maltreatment allegation in a given county-quarter. This outcome is in large part determined by third-party reporters' decisions to make an allegation of maltreatment to CPS.[26] We assume that in making this decision, potential reporters weigh the perceived costs and benefits of reporting. We further assume that the benefits of reporting are increasing in the severity and likelihood of maltreatment, and decreasing in the perceived cost to reporters of making a referral. Thus, changes in the costs and benefits to reporters will shift the threshold at which they choose to report. This "allegation" threshold and the resulting allegation rate are depicted in Figure 2a, where the solid blue line represents the allegation threshold (i.e., at which the net benefits of reporting are zero) and the blue shaded area represents the allegation rate.

We assume there is another threshold above which a child is considered a victim. This "victimization" threshold will depend on the legal definition of substantiated or indicated child maltreatment according to state law. We assume that CPS investigators are generally better able than reporters to determine if abuse or neglect is in fact taking place, which implies that the threshold for victimization is above the allegation threshold. Figure 3a shows the victimization threshold as a solid red line, and the victimization rate as the area under the curve shaded in red above the threshold. The substantiation rate, or the share of investigated children found to be victims, is given by the ratio of the victimization rate to the allegation rate.

We now use this stylized framework to consider the possible impacts of SC on our outcomes

[24]We thank Jessamyn Schaller for suggesting this framework.

[25]The unobserved distribution of maltreatment risk need not be normal as depicted in the figure.

[26]To align with the outcomes we construct from NCANDS data, our conceptual model abstracts from the screening decision and implicitly assumes that screening decisions are unaffected by impacted by Secure Communities.

of interest. We consider three possible scenarios: (1) an increase in maltreatment, (2) an increase in the allegation threshold, and (3) a combination of the two.[27]

Suppose first that SC increases maltreatment of Hispanic children. This is in line with work showing that SC reduces access to resources and increases household stress (Alsan & Yang, 2024; Amuedo-Dorantes & Antman, 2022; East et al., 2022; Wang & Kaushal, 2019), each of which is a predictor of child maltreatment. This would shift the distribution of maltreatment risk to the right, without impacting either the allegation or victimization threshold, as depicted in Figures 2b and 3b. In this case, both the allegation rate and the victimization rate will increase. The effect on the substantiation rate is ambiguous and depends on the relative changes in the allegation and victimization rates.

Now suppose that SC increases the perceived cost of reporting potential maltreatment of Hispanic children without changing their underlying risk of maltreatment, such that the allegation threshold shifts to the right. For example, reporters may respond to an increased risk or perceived risk of reporter or parental deportation. Figures 2c and 3c show this change as a rightward shift in the allegation threshold. In this case, we would see a decline in the allegation rate with no change in the victimization rate. Combining these two effects, we expect an increase in the substantiation rate as the "riskiness" of the average child with a maltreatment allegation increases.

Finally, suppose that SC increases both the underlying risk of maltreatment and increases the allegation threshold, as depicted in Figures 2d and 3d. When these two shocks occur simultaneously, the victimization rate increases, but the effects on the allegation and substantiation rates are ambiguous. However, note that if SC has no impact on the allegation rate, the substantiation rate unambiguously increases. While this simplified conceptual model fails to capture some complexities of the child welfare system, it provides useful intuition to frame our empirical model and results.

[27]Note, this model may also be used to think about the impacts of shocks that decrease maltreatment and/or the allegation threshold. We do not explicitly discuss these scenarios here as we think they are unlikely to dominate any increases in the context of SC.

5 Empirical Approach

To estimate the effect of SC on child maltreatment, we exploit the staggered rollout of the policy across counties and time. Our three main outcomes of interest measure the allegation, victimization, and substantiation rates for Hispanic children in county c and quarter t. We estimate the following dynamic event-study specification:

$$Y_{ct} = \sum_{\ell=-8}^{\ell=-2} \beta_\ell \times SC_{ct}^{\ell} + \sum_{\ell=0}^{\ell=8} \beta_\ell \times SC_{ct}^{\ell} + \mu_c + \tau_t + x_{ct} + \epsilon_{ct} \tag{1}$$

where Y_{ct} denotes the maltreatment outcome of interest. The event time term ℓ indicates the quarters since the program activation relative to quarter t. The indicator variable SC_{ct}^{ℓ} is equal to one when a county c is ℓ periods away from initial SC activation at quarter t.[28] ℓ equal to zero denotes the quarter of SC activation. The terms μ_c and τ_t correspond to county and quarter-year fixed effects, which account for time-invariant differences across counties and common time-varying shocks, respectively. x_{ct} is the unemployment rate and the presence of two alternative immigration programs for county c at quarter t, and ϵ_{ct} is the error term. Standard errors are clustered at the county level.

The β_ℓ coefficients measure effects relative to $\ell = -1$, the quarter before SC activation. The first and last indicators SC_{ct}^{ℓ} are equal to one for all time periods before and after the two years around implementation.

Previous papers emphasize the drawbacks of estimating a standard two-way fixed-effects model to study the impact of SC. These include universal program rollout, such that all counties are eventually treated, and heterogeneous, dynamic impacts, among other issues (Alsan & Yang, 2024; East et al., 2022).[29] Given these challenges, we estimate equation (1) using the dynamic treatment effect model proposed by Sun and Abraham (2021). Their

[28] $SC_{ct}^{\ell} = 1\{t - SC_c = \ell\}$, where SC_c is the quarter when SC was activated in county c

[29] With all counties eventually treated, the two-way fixed effects estimator would use early-treated counties to estimate the policy's effect for later-treated counties. Multiple authors have pointed out the problems with exploiting these "forbidden comparisons" (Borusyak & Jaravel, 2018; De Chaisemartin & d'Haultfoeuille, 2023; Goodman-Bacon, 2021). See Gonçalves et al. (2024) for a more detailed discussion specific to the SC context.

interaction weighed (IW) estimator allows for heterogeneous and dynamic treatment effects with staggered treatment (Sun & Abraham, 2021). We follow Gonçalves et al. (2024) and denote the latest treated quarter of sample counties as the control group. Specifically, we define later-treated counties as those that adopted SC during or after the second quarter of 2011. The control group represents 24.8% of the counties in our sample. We limit our estimation sample to the period before any county in our control group is treated. The identifying assumption required for a causal interpretation of the estimated β_ℓ coefficients is that, in the absence of SC, maltreatment outcomes in earlier-treated counties would have continued in a trend similar to those in later-treated counties. We explore the plausibility of this assumption below in our discussion of results.

6 Results and Discussion

Figure 4 reports the results of estimating equation (1) for each of our three outcomes of interest for Hispanic children.[30] We present an estimated aggregate treatment effect for each outcome in the first row of Table 2 and the associated p-value in the second row.[31] Evenly numbered columns of Table 2 present results from our preferred specification with controls for unemployment rate and other immigration programs (e.g., E-Verify). Odd columns reflect results when these controls are excluded.

Each of the three panels in Figure 4 shows similar trends in the outcomes for treated and control counties prior to the activation of SC, supporting the parallel trends assumption. After implementation, effects on the allegation rate for Hispanic children are small and statistically insignificant (Figure 4a). The estimated aggregate effect reported in column (2) of Table 2 is positive but statistically insignificant. Figure 4b and column (4) of Table 2 report

[30] We use the eventstudyinteract package to present our estimation results, $\hat{\beta}_\ell$ and the associated 95% confidence intervals, graphically (Sun, 2022).

[31] The estimated aggregate treatment effect, denoted $\hat{\beta_{SC}}$, is computed as the linear combination of the post-activation $\hat{\beta}_\ell$ estimates from equation (1). We use the following: $\beta_{SC} = \frac{\sum_{\ell=0}^{\ell=8} \hat{\beta}_\ell}{9}$, where $\hat{\beta}_\ell$ is the IW estimator.

results for the victimization rate. Both suggest that SC activation increased the number of Hispanic children with substantiated maltreatment allegation(s) per 1000. The estimated aggregate effect corresponds to an increase of approximately 20% relative to the mean of about two children per 1000. Finally, Figure 4c and the final column of Table 2 depict results for the substantiation rate, which reflects the fraction of Hispanic children found to be victims among those with allegation(s). We find an increase in the substantiation rate following SC activation, with an estimated aggregate effect size of 14% when evaluated at the mean.

To summarize, our main results for Hispanic children in Figure 4 and Table 2 suggest that SC activation had a negligible impact on the allegation rate but increased both the victimization rate and the substantiation rate for Hispanic children. To map these results to our outcomes of interest (incidence and reporting of maltreatment), we refer to our conceptual model outlined in Section 4. Our empirical results are most consistent with a combined increase in underlying maltreatment and the allegation threshold, the scenario depicted in Figures 2d and 3d. This aligns with prior work finding that SC increased victimization of Hispanic adults while reducing the share of crimes they report (Gonçalves et al., 2024).

Next, we separately examine SC's effects on administrative maltreatment outcomes for allegations made by professional vs. non-professional reporters. Professional reporters are likely mandated reporters, individuals required by law to report suspected child maltreatment to CPS.[32] Professional reporters include social services personnel; medical personnel; mental health personnel; legal, law enforcement, and criminal justice personnel; education personnel; and child daycare providers. Non-professional reporters include substitute care providers, alleged victims, parents, other relatives, friends/neighbors, alleged perpetrators, anonymous reporters, other, unknown, or missing report sources.

Table 3 shows estimated aggregate effects for each maltreatment outcome, separately by report source category. We find no effect on allegation rates for Hispanic children based on reports submitted by either professional or non-professional sources (columns (1) and (2)).

[32]Laws on mandatory reporters vary across states and time. As of 2019, 47 states have laws that identify specific professionals as mandatory reporters (Children's Bureau, 2019).

We also find no effect on victimization and substantiation rates for Hispanic children reported by non-professional sources (columns (4) and (6)). In contrast, the estimated aggregate effects for these outcomes based on reports from professional sources are positive and statistically significant (columns (3) and (5)). Thus, the increases in victimization and substantiation rates in Table 2 appear to be driven largely by maltreatment reports from professionals. Recall, our conceptual model suggests that an increase in victimization rates with no change in allegation rate is most consistent with a combined increase in maltreatment and reduction in reporting (i.e., a rightward shift of the allegation threshold). As such, these supplemental results are consistent with SC reducing children's exposure to mandated reporters such as teachers and doctors. This is in line with prior findings on increased school absenteeism (Bellows, 2021; Heinrich et al., 2023) and reduced healthcare utilization (Hacker et al., 2012) in response to actual or perceived immigration threats.

While our primary focus is on the impacts of SC on the maltreatment of Hispanic children, the program's impact could extend to non-Hispanic children if, for example, SC impacted settings in which child maltreatment reports are generated. Ali et al. (2024) find that SC decreased employment in center-based childcare and reduced the childcare participation rate of young children with citizen mothers. Both of these effects could reduce reporting of child maltreatment through reduced exposure to professional reporters. Figure 5 shows the results of estimating equation (1) for the allegation, victimization, and substantiation rates of non-Hispanic children while Table 4 reports the associated aggregate effects.

In contrast to the results for Hispanic children, the estimates for non-Hispanic children show differential pre-trends in the periods leading up to SC activation, in particular for the victimization and substantiation rate outcomes. This suggests that for the non-Hispanic maltreatment outcomes, counties in which SC was activated earlier serve as poor controls for those that activated later and a causal interpretation should not be ascribed to the results reported in Figure 5 and Table 4. In Figure 5a, the allegation rate for non-Hispanic children shows an increase three quarters after SC implementation, followed by a reduction

in the subsequent several months. The estimated aggregate effect for the allegation rate of non-Hispanic children in column (1) of Table 4 is statistically insignificant. The estimated aggregate effects for the non-Hispanic victimization and substantiation rates in columns (2) and (3), respectively, are both positive and statistically significant. However, for these outcomes, the pattern shown in Figures 5b and 5c of post-activation estimated coefficients that are positive and increasing in magnitude over time (but statistically insignificant) is consistent with a continuation of the pre-SC activation trends rather than the causal impact of SC.

7 Conclusion

In this paper, we study the effects of Secure Communities (SC) on the incidence and reporting of child maltreatment. Exploiting the staggered rollout of SC across counties, we estimate effects on allegation, victimization, and substantiation rates. We find that SC increased substantiation and victimization rates of Hispanic children, without significantly affecting allegation rates. These results are consistent with stricter immigration enforcement increasing maltreatment among Hispanic children while reducing reporting rates. Our findings suggest that tying immigration enforcement to law enforcement may have adverse consequences for child safety and well-being.

Our findings have important implications for current immigration enforcement policy and for understanding ethnic disparities in child welfare. First, our results speak to the potential consequences of current immigration enforcement tactics. The Trump administration's removal of protections on sensitive locations such as schools, hospitals, and churches has created a culture of fear among both citizen and non-citizen families, with immigrant families reducing their use of these previously safe spaces. By reducing interactions with mandated reporters—through decreased school attendance and fewer medical appointments—current policies may further suppress reporting rates of child maltreatment among immigrant families.

Future studies are needed to understand if these more recent enforcement efforts are having similar or even more severe consequences for Hispanic and other immigrant children. Second, we shed light on a potential cause of the "Hispanic paradox" observed in administrative child welfare data. Hispanic children are less likely to receive an investigated maltreatment report, conditional on income. Our study suggests there may be barriers to reporting which disproportionately affect Hispanic children. SC likely served to increase those barriers, with negative impacts on child welfare.

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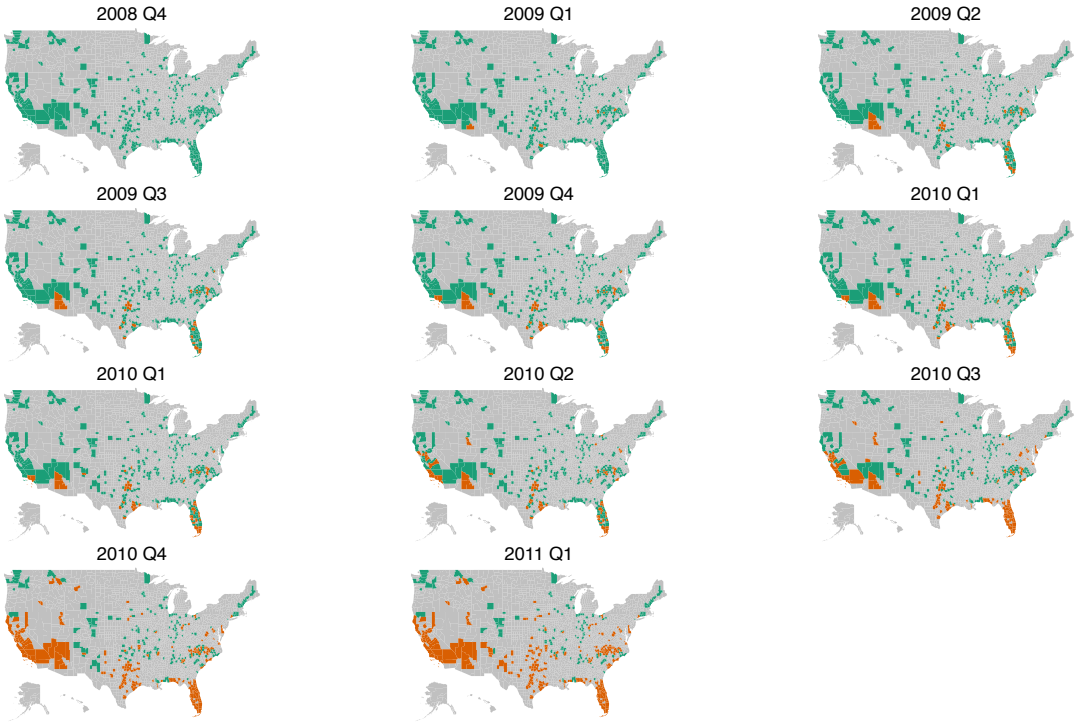
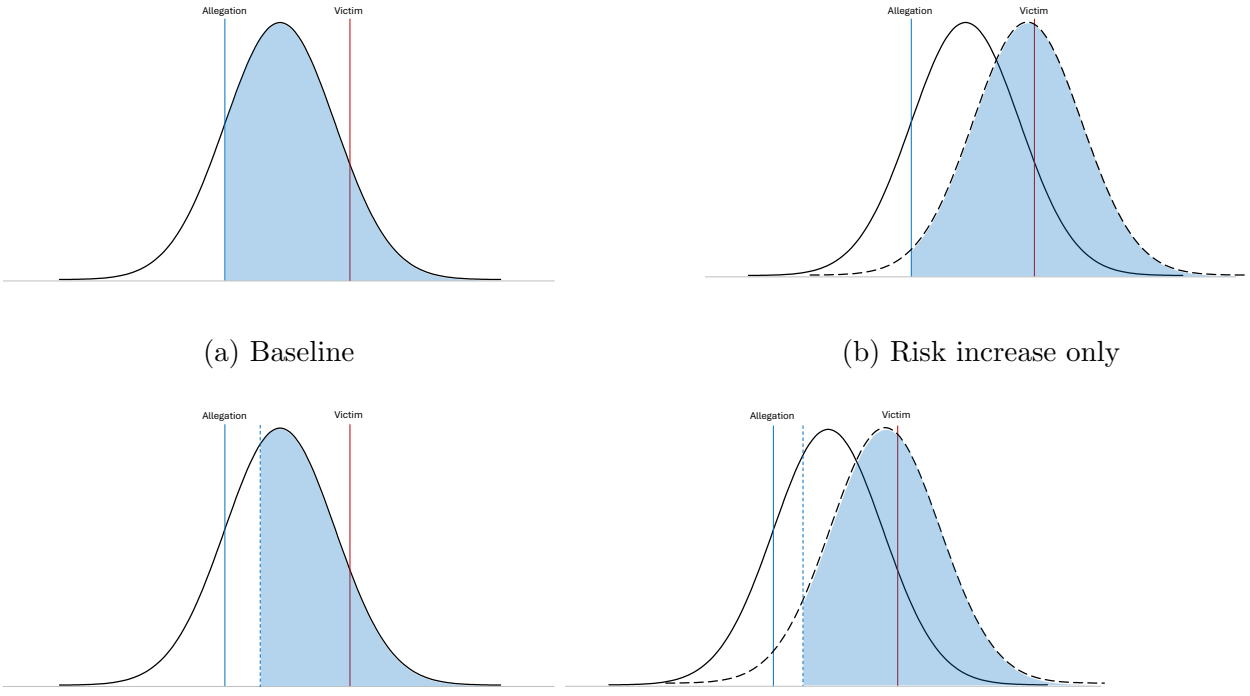
Figures and Tables

Figure 1: Secure Communities adoption in Sample Counties



Note: Figure depicts the rollout adoption of Secure Communities each quarter in our sample counties. Green and orange areas represent the counties included in our estimation sample. Green-shaded counties indicate counties not yet treated and orange-shaded counties indicate counties where Secure Communities was active. We exclude border counties and resister states (IL, MA, and NY) following Alsan and Yang (2024) as well as counties without allegations involving Hispanic children during the sample period.

Figure 2: Conceptual Framework - Allegation Rate



(a) Baseline

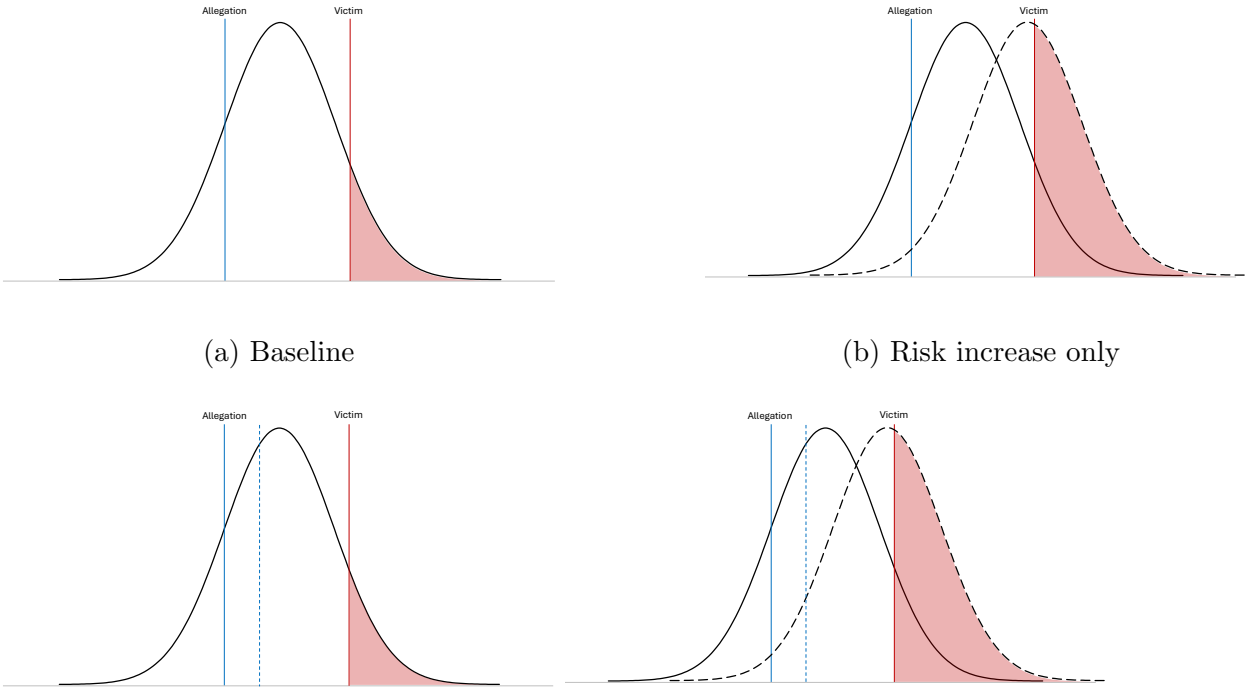
(b) Risk increase only

(c) Allegation threshold increase only

(d) Increase in risk and allegation threshold

Note: Figure depicts how an increase in the underlying distribution of maltreatment (shown as a rightward shift from the black solid curve to the black dotted curve) and/or the allegation threshold (shown as a rightward shift from the vertical blue solid line to the vertical blue dotted line) impact the allegation rate outcome based on a stylized conceptual model. The allegation rate is shown as the blue shaded area in each panel.

Figure 3: Conceptual Framework - Victimization Rate



(a) Baseline

(b) Risk increase only

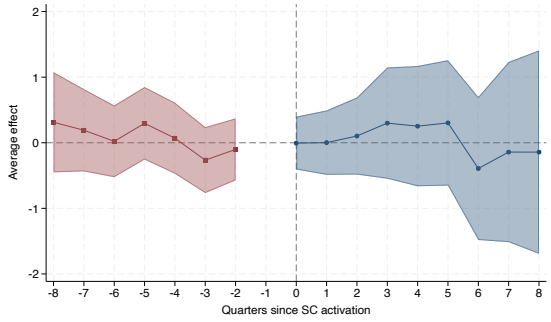
(c) Allegation threshold increase only

(d) Increase in risk and allegation threshold

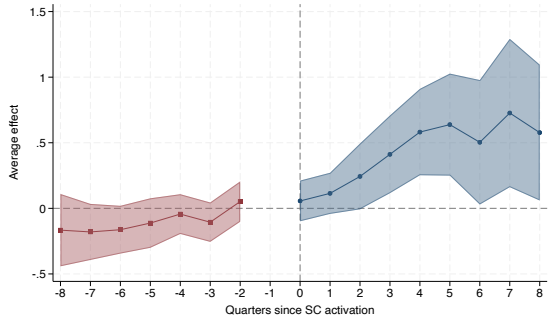
Note: Figure depicts how an increase in the underlying distribution of maltreatment (shown as a rightward shift from the black solid curve to the black dotted curve) and/or the allegation threshold (shown as a rightward shift from the vertical blue solid line to the vertical blue dotted line) impact the victimization rate outcome based on a stylized conceptual model. The victimization rate is shown as the red shaded area in each panel.

Figure 4: Main Estimates: Hispanic Children Maltreatment Rates

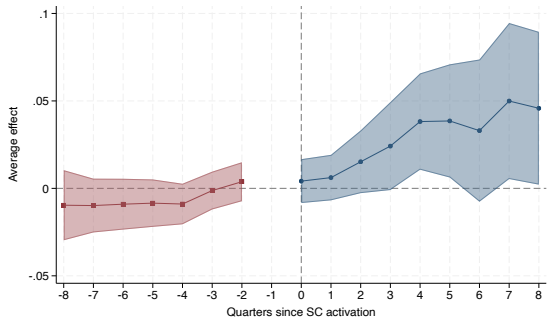
(a) Allegation Rate



(b) Victimization Rate



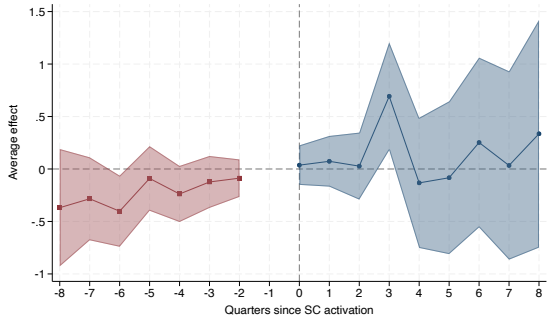
(c) Substantiation Rate



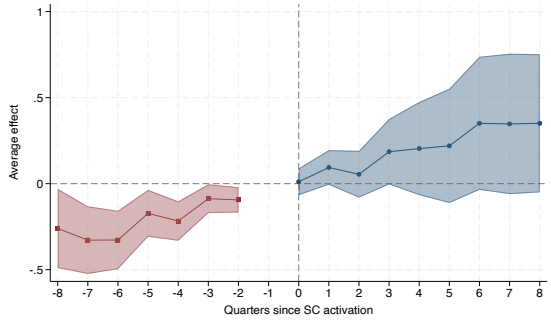
Note: Each panel of this figure reports the β_ℓ Sun and Abraham (2021) estimation results for equation (1). Model includes controls for unemployment rates, other immigration programs, and county and quarter-year fixed effects. Standard errors are clustered at the county level. Panel (a) shows results for the allegation rate for Hispanic children. Panel (b) shows results for the victimization rate for Hispanic children. Panel (c) shows hows results for the the substantiation rate for Hispanic children. Later treated counties (treated on or after the second quarter of 2011) are used as controls to estimate the effects of SC in earlier-treated counties. The x-axis shows the quarter relative to SC activation and the y-axis reports the estimated effect relative to the omitted period $\tau = -1$. The first and last time periods ($\tau = -8$ and $\tau = 8$) represent the average effect for all quarters before and after those quarters. The shaded area represents the 95% confidence interval. Data from NCANDS, sample from 2006 to before control (later-treated) counties start treatment (first quarter of 2011), excludes border counties, resister states (IL, MA, and NY), and counties without allegations for Hispanic children.

Figure 5: Estimates: Non-Hispanic Children Maltreatment Rates

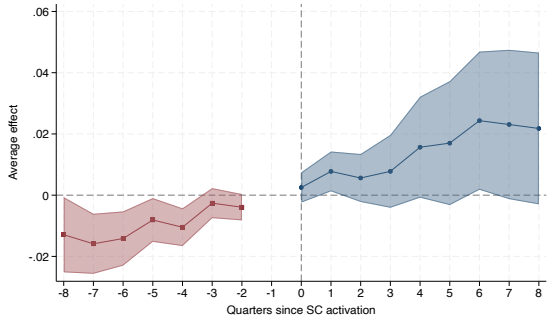
(a) Allegation Rate



(b) Victimization Rate



(c) Substantiation Rate



Note: Each panel of this figure reports the β_ℓ Sun and Abraham (2021) estimation results for equation (1). Model includes controls for unemployment rates, other immigration programs, and county and quarter-year fixed effects. Standard errors are clustered at the county level. Panel (a) shows results for the allegation rate for non-Hispanic children. Panel (b) shows results for the victimization rate for non-Hispanic children. Panel (c) shows hows results for the the substantiation rate for non-Hispanic children. Later treated counties (treated on or after the second quarter of 2011) are used as controls to estimate the effects of SC in earlier-treated counties. The x-axis shows the quarter relative to SC activation and the y-axis reports the estimated effect relative to the omitted period $\tau = -1$. The first and last time periods ($\tau = -8$ and $\tau = 8$) represent the average effect for all quarters before and after those quarters. The shaded area represents the 95% confidence interval. Data from NCANDS, sample from 2006 to before control (later-treated) counties start treatment (first quarter of 2011), excludes border counties, resister states (IL, MA, and NY), and counties without allegations for Hispanic children.

Table 1: Comparison of Sample and Non-Sample Counties

	Sample Counties (1)	Non-Sample Counties (2)	Diff. (S - N) (3)	s.e. (4)
Median Age	37.266	40.679	-3.414***	(0.257)
Population	365,760	56,973	30,8786***	(15284.294)
Share Hispanic Population	0.134	0.100	0.034***	(0.010)
Share Rural Population	0.225	0.627	-0.402***	(0.015)
Share Households Renting	0.331	0.269	0.063***	(0.004)
Share Multigenerational	0.041	0.035	0.006***	(0.001)
Unemployment Rate	9.640	9.566	0.074	(0.184)

Note: Mean values and differences in means for county characteristics. Column (1) provides mean for the 418 counties included in our sample. Column (2) shows means for the 2,801 counties not reflected in our sample due to the NCANDS masking convention and sample restrictions. Column (3) presents the differences in means (1-2) with the corresponding standard errors given in column (4). *** denotes a p-value of less than 0.01. Data are from 2010 Census and 2010 average county unemployment rate from Bureau of Labor Statistics.

Table 2: Aggregated Effect Estimates Hispanic Children Maltreatment Rates

	Allegation Rate		Victimization Rate		Substantiation Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Aggregate Effect Post	-0.313	0.031	0.220	0.428	0.021	0.028
P-value	0.390	0.928	0.101	0.003	0.050	0.018
Y-Mean	10.843	10.843	2.110	2.110	0.204	0.204
Pre-Trend Test	0.271	0.471	0.567	0.353	0.495	0.457
Controls	No	Yes	No	Yes	No	Yes

Note: Table reports the estimated aggregated treatment effects of SC on Hispanic children allegation, victimization, and substantiation rates, respectively (first row). Columns 2, 4, and 6 reflect the inclusion of controls for unemployment rate and other immigration programs. The estimated aggregated treatment effect is an aggregation of the Sun and Abraham (2021) estimates of β_ℓ for equation (1). P-value corresponds to the significance of the linear combination of $\hat{\beta}_\ell$ from $\ell = 0$ to $\ell = 8$. Y-mean is the respective pre-treatment average for earlier-treated counties. The pre-trend test shows the significance of a joint F-test of $\hat{\beta}_\ell = 0$ fol ℓ values from $\ell = -8$ to $\ell = -2$. Data from NCANDS, sample from 2006 to before control (later-treated) counties start treatment (first quarter of 2011), excludes border counties, resister states (IL, MA, and NY) , and counties without allegations for Hispanic children.

Table 3: Aggregated Effect Estimates Hispanic Rates: by Reporter Type

	Allegations		Victimization		Substantiation	
	Prof (1)	Non-Prof (2)	Prof (3)	Non-Prof (4)	Prof (5)	Non-Prof (6)
Aggregate Effect Post	0.022	0.008	0.364	0.062	0.033	0.014
P-value	0.930	0.968	0.001	0.333	0.081	0.376
Y-Mean	5.761	5.201	1.528	0.590	0.289	0.140
Pre-Trend Test	0.396	0.193	0.607	0.398	0.475	0.829
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Table reports the estimated aggregated treatment effects of SC on Hispanic children allegation, victimization, and substantiation rates, respectively, by professional or non-professional reporter type (first row). Results reflect the inclusion of controls for unemployment rate and other immigration programs. The estimated aggregated treatment effect is an aggregation of the Sun and Abraham (2021) estimates of β_ℓ for equation (1). P-value corresponds to the significance of the linear combination of $\hat{\beta}_\ell$ from $\ell = 0$ to $\ell = 8$. Y-mean is the respective pre-treatment average for earlier-treated counties. The pre-trend test shows the significance of a joint F-test of $\hat{\beta}_\ell = 0$ fol ℓ values from $\ell = -8$ to $\ell = -2$. Data from NCANDS, sample from 2006 to before control (later-treated) counties start treatment (first quarter of 2011), excludes border counties, resister states (IL, MA, and NY) , and counties without allegations for Hispanic children.

Table 4: Non-Hispanic Children Maltreatment Rates

	Allegation Rate (1)	Victimization Rate (2)	Substantiation Rate (3)
Aggregate Effect Post	0.137	0.202	0.014
P-value	0.590	0.078	0.036
Y-Mean	12.104	2.231	0.197
Pre-Trend Test	0.148	0.005	0.016
Controls	Yes	Yes	Yes

Note: Table reports the estimated aggregated treatment effects of SC on non-Hispanic children allegation, victimization, and substantiation rates, respectively (first row). Results reflect the inclusion of controls for unemployment rate and other immigration programs. The estimated aggregated treatment effect is an aggregation of the Sun and Abraham (2021) estimates of β_ℓ for equation (1). P-value corresponds to the significance of the linear combination of $\hat{\beta}_\ell$ from $\ell = 0$ to $\ell = 8$. Y-mean is the respective pre-treatment average for earlier-treated counties. The pre-trend test shows the significance of a joint F-test of $\hat{\beta}_\ell = 0$ fol ℓ values from $\ell = -8$ to $\ell = -2$. Data from NCANDS, sample from 2006 to before control (later-treated) counties start treatment (first quarter of 2011), excludes border counties, resister states (IL, MA, and NY) , and counties without allegations for Hispanic children.