Spillover Effects of Immigration Policies on Children's Human Capital*

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Abstract

We study the spillover effects of immigration enforcement policies on children's human capital. Exploiting the temporal and geographic variation in the enactment of immigration enforcement policies, we find that English language skills of US-born children with at least one undocumented parent are negatively affected by the introduction of these policies. Changes in parental investment behavior cause this reduction in children's English skills. Parents are less likely to enroll their children in formal non-mandatory preschool, substituting formal non-mandatory preschool education with parental time at home. Parents also reduce time spent on leisure and socializing, providing children with fewer opportunities to interact and lean from others. Ultimately, these developments reduce children's long-term educational success. Exposure to immigration enforcement during early childhood lowers the likelihood of high school completion. We also find negative, though imprecise, effects on college enrollment.

Keywords: Immigration policies, children's human capital, children's language skills, parental investment

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1 Introduction

Growing up with immigrant parents can place a heavy economic and social burden on children, especially if the parents are undocumented. Over the last years, the growth of immigration enforcement might have deteriorated the situation for children further. Between 2009 and 2013, enacted immigration policies in the United States were responsible for the deportation of almost 2 million individuals (Vaughan, 2013). Immigration policies have led to the breakup of mixed-citizen families and generated fear in immigrant communities (Amuedo-Dorantes and Arenas-Arroyo, 2019; Capps et al., 2020). Fearing the reporting and deportation of family members, undocumented parents might reduce social contacts to a minimum. They might interact less with individuals outside their community and be less likely to enroll their children in non-mandatory educational programs (e.g., Gándara and Ee, 2018). These reductions in early parental human capital investments can have detrimental effects on children's skills and their later economic and social success (Currie and Almond, 2011). Understanding the spillover effects of immigration policies on children's human capital accumulation and the role of parents is therefore crucial, also given that around 8 percent of US children have at least one undocumented parent (Pew Research Center, 2019).

In this paper, we follow two goals to better understand how immigration policies can affect US-born children with at least one undocumented parent. First, we study the spillover effects of immigration enforcement policies on the English language proficiency. We concentrate on language proficiency as an important skill, which is strongly associated with future success. Having a sufficient level of language proficiency is essential to participating fully in society (Arington, 1990). Higher verbal skills earlier during a child's life cycle play a substantial role in explaining later educational success, such as college enrollment, and they do even more so than math skills (Bleakley and Chin, 2010; Aucejo and James, 2021). Ultimately, language skills affect future labor market success (Dustmann and Fabbri, 2003; Bleakley and Chin, 2004).

Our second goal is to explore how immigration enforcement policies can change parents' human capital investment decisions, as an important underlying mechanism. Parents play an important role in shaping children's language skills. Language proficiency is largely formed by social interactions with peers and adults (Henry and Rickman, 2007; Weisleder and Fernald, 2014). Interactions with native speakers are particular important for the development of English proficiency for children of Spanish-speaking parents (Palermo and Mikulski, 2014; Villarreal and Gonzalez, 2016), who are often Hispanics and particularly affected by immigration policies in the United States. This gives immigrant parents a key role in directly and indirectly influencing their children's language skills. Within the climate of fear following immigration enforcement, undocumented parents might limit social interactions for themselves and their kids. For example, parents may decide to not to enroll their US-born children in non-mandatory education programs to limit social contacts and exposure.

Exploiting the temporal and geographical variation in the enactment of the first police-based enforcement policy in a metropolitan statistical area (MSA), we find that the policy had significant spillover effects on the English proficiency of US-born children with at least one undocumented parent. Our estimates show that the introduction of immigration enforcement policy reduces children's likelihood of having high English language skills by a significant 3 percentage points or around 4 percent. Investigating the dynamics of the effect, we find flat pre-trends prior to the enactment of immigration enforcement laws but a gradual decline in children's language skills afterward. This pattern suggests a lack of intervention later in a child's life cycle to compensate for the loss of early language skills.

Our estimated effects are quite sizeable when compared to policies aimed at improving language skills of children of non-native speakers. For example, our results are of similar magnitude but opposite sign than having access to Head Start, an early education intervention program aimed at disadvantaged children. Access to Head Start at age four increases children's third grade reading and vocabulary skills by 2 percent and 0.5 percent, respectively, compared to the mean (Puma et al., 2012). Our results are also comparable to those reported in Kuziemko (2014), who evaluates Proposition 227 in California, which mandates English as the language of instruction in schools. Using a sample of foreign born children, she finds that in districts with an average compliance rate the introduction of the law increased the likelihood that children speak very well English by around 6 percent.

We conduct several checks to assess the robustness of our results, such as using an alternative definition of likely undocumented immigrant, disregarding all parents without formal education from the sample, and allowing for heterogeneous treatment effects across treated cohorts.[1] In all these cases, our estimates are very similar to our main results. We also conducted placebo regressions where we only included children of naturalized or native parents in our sample. The estimates for these samples are all close to zero and not statistically significant on any conventional level.

We then provide evidence that one important underlying mechanism for our results is the change of parental investment behavior caused by immigration enforcement. Immigration policies reduce the likelihood that likely undocumented parents enroll their US-born children in non-mandatory preschools by 2.2 percentage points or around 7 percent compared to the mean enrollment rate of Hispanics.

[1] Recent research has shown that difference-in-differences based on two-way fixed effects regressions and the staggered rollout of a policy can be biased when treatment effects are not constant over cohort and time; see, for example, de Chaisemartin and D'Haultfœuille (2020), Borusyak et al. (2021), Callaway and Sant'Anna (2021), and Goodman-Bacon (2021).

At the same time, we also find that parents change their time investment behavior in their children as response to immigration enforcement. On the one side, parents try to compensate for the reduction in preschool attendance and therefore time in formal educational by increasing their time investment in their children. The increase is, however, mostly concentrated in time spent on recreational activities like playing with the child. We do not find evidence that parents' educational time investment is affected by immigration enforcement. One explanation may be that parents are not aware of the importance of early childhood investments (e.g., Boneva and Rauh, 2018). As recreational time spent with parents is less linguistically productive than time in preschool, this leads to a reduction in children's English language skills.

On the other side, parents reduce their time spent on activities which are mostly done outside one's own home and with others, such as attending events or socializing. Fewer or no possibilities to interact and learn from others further reduce children's language skill accumulation. Time spent on activities that take place predominantly at home, such as time spent on general care, are unaffected by the introduction of immigration enforcement policies.

Our results on the impact of immigration enforcement on parental time investment mirror findings on how these policies reduces take-up of government benefit programs of likely undocumented immigrants (e.g., Watson, 2014; Aslan and Young, 2019). The negative effect on pre-school enrollment and the reduction in time spent on leisure and social activities are also consistent with both "chilling effects", where parents limit the risk of detection by reducing children's time spent outside their home to a minimum, and income effects, caused by a reduction in the employment of likely undocumented immigrants; see also the findings in East et al. (2021).[2]

Exposure to immigration enforcement during early childhood and associated lower language skills ultimately reduces human capital later in life. Being affected by immigration enforcement between age 0 and 4, a critical period for skill accumulation during a child's life (e.g. Bleakley and Chin, 2010), lowers the likelihood of high-school completion by age 19 by around 6 percent. To put these results into perspective, Bailey et al. (2021) find that access to Head Start increased high school completion by 2.6 percent while Kuka et al. (2020) finds that eligibility for the Deferred Action for Childhood Arrivals (DACA) program increases high school completion overall by 6 percent. We also find negative, though imprecise, impacts on college enrollment.

Our results are important in that they provide strong evidence for negative spillover effects of immigration policies on the human capital of US-born children of immigrant

[2]Using information on hours worked from the American Time Use Data, we find small negative but imprecisely estimated reductions in labor supply on the intensive and extensive margin caused by immigration enforcement. The reduction in employment might itself be caused by chilling effects and the fear of being detected and deported.

parents. Lower English language skill levels earlier in life reduce the likelihood that these children can participate fully in society. An underdeveloped English language skill set also reduces their chances of graduating from high-school and obtaining a university degree, and lowers their future labor market success. Ultimately, lower skill accumulation caused by immigration enforcement is likely to increase these children's dependence on social security later in life and hamper the intergenerational mobility of migrant children. The overall future impact on the economy is substantial, given the amount of US-born children of immigrant parents. If a large share of the future workforce grows up accumulating fewer skills while young, this will ultimately reduce long-term growth prospects.

Our work is related to two important strands of literature. First, we contribute to the literature on the effects of immigration policies on US children with undocumented parents. Previous work has analyzed the effect of immigration enforcement on children's Medicaid participation, living arrangements, foster care, and general access to economic resources (Watson, 2014; Amuedo-Dorantes and Arenas-Arroyo, 2019, 2018; Amuedo-Dorantes et al., 2018). Closer related to our project is the work by Amuedo-Dorantes and Lopez (2017), Dee and Murphy (2020), and Santillano et al. (2020), who study the impact of restrictive immigration policies on school enrollment, school dropout rates, and enrollment in Head Start. Amuedo-Dorantes and Lopez (2017) find that increasing immigration enforcement significantly increases both the likelihood of repeating a grade and the probability of dropping out of school for Hispanic children of likely unauthorized parents. Dee and Murphy (2020) find that local ICE partnerships reduce the number of Hispanic students in school. This effect is mostly concentrated on elementary school students. Santillano et al. (2020) finds that local immigration raids deter Hispanic parents from enrolling their children in Head Start.[3]

We complement and extend this strand of the literature by providing a unifying picture of how immigration policies can affect children's human capital accumulation. In our work, we first analyze the spillover effects of immigration policies on the language skills of US-born children, which is strongly associated with future success (Aucejo and James, 2021). Then, we carefully connect these spillover effects to potential changes in parental investment behavior caused by immigration policies as an important underlying mechanism. Our work therefore contributes to a better understanding of *how* and *why* immigration policies can affect children's human capital, even if these children are not directly targeted by the policies.

[3]Bellows (2019) finds small negative impacts of the introduction of Secure Communities in a county on average English Language Arts (ELA) scores using the Stanford Education Data Archive. While the results are important and insightful, the aggregation of the data, possible selective participation in ELA test taking, and the lack of availability of the exact test taking dates make it difficult to deduce the real impact of immigration policies due to the likely presence of measurement errors, as was pointed out by Ho (2020).

Second, we contribute to the literature on determinants of parental human capital investment decisions (Baranov et al., 2020; Nicoletti and Tonei, 2020; Schmidpeter, 2020; Laffers and Schmidpeter, 2021) and, more specifically, investment decisions made by likely undocumented parents. Thus, to a certain extent, we also contribute to works investigating the intergenerational mobility of migrants (Chetty et al., 2020; Abramitzky et al., 2021).[4] We analyze if and how immigration policies can change parental investment decisions. Consistent with both chilling and income effects, undocumented parents of US-born children minimize social interactions outside their home and less likely enroll their children in non-mandatory formal education programs. While parents also increase the time spent with their children in some activities as a response to immigration policies, the extra time is not sufficient to compensate for the disadvantages caused by less time in formal educational childcare; see also, e.g., Bernal and Keane (2011) and Felfe and Lalive (2018) for the impact of formal childcare on children's cognitive achievements. Ultimately, lower parental educational investments and language skills caused by immigration enforcement decrease long-term educational attainment. This development has the potential to reverse the improvements in intergenerational mobility Hispanics have made (e.g. Chetty et al., 2020).

The paper proceeds by first providing a conceptual framework to motivate how immigration policies can affect the skill accumulation of US-born children. The data for our analysis are described in section 3. We present our empirical strategy in section 4. In section 5, we discuss the spillover effects of immigration enforcement on children's language skills. Changes in parental investment behavior as potential mechanism caused by heightened enforcement is explored in section 6. Finally, section 8 concludes the study.

2 Immigration Policies and Children's Skills

To motivate our empirical analysis we consider a simple overlapping generations model with parental human capital investments to show how and through which channels immigration enforcement policies can affect children's language skills. There are two agents in our model, undocumented parents and their child, and two time periods, childhood t_1 and adulthood t_2. When the child is young, parents have three choices: how much to consume c^p, how much time to spend with their child l^p, and how much time k to send their child to formal childcare (preschool) at a per unit cost κ. Assume that parents' time is normalized to one and that time not spent with the child is used for working at a per unit wage rate ω, so that $s^p = 1 - l^p$. Undocumented parents also face a probability p^d of being deported by the end of t_1, after parents' investment decisions have been made.

[4] See also Francesconi and Heckman (2016) for a review of the literature. These works do not, in general, investigate how (immigration) policies can have spillover effects on parents' investment decisions.

The child receives its payoff in t_2 when it is old and the parents are dead. Let the superscript d indicate that the family was deported by the end of t_1. Likewise, denote by nd if the family was not deported. The payoff function for the child in adulthood is given by

$$c^{j,c} = h^j(l^p, k^p) \tag{1}$$

where $h^j(\cdot]$ is the human capital production function for $j \in \{d, nd\}$. We assume that $h(\cdot)$ is increasing and concave in each of its two arguments. The child's payoff when old depends on parental investments made in t_1 during childhood. The payoff also depends on the deportation status, for example, to reflect that stress caused by deportation affects productivity of human capital, even if the child is not deported.[5]

Let parents' instant utility function of family consumption be $U(\cdot)$, which is concave in its argument, and denote by $V^c(c^j)$ the child's indirect utility function in adulthood. Then, abstracting from any discount factor, parents face the following maximization problem:[6]

$$\begin{aligned} \max_{c^p, l^p, k^p} \quad & U(c^p) + p^d V^c(c^{d,c}) + (1-p^d) V^c(c^{nd,c}) \\ \text{s.t.} \quad & c^p = \omega(1-l^p) - \kappa k^p \\ & c^{d,c} = h^d(l^p, k^p) \\ & c^{nd,c} = h^{nd}(l^p, k^p). \end{aligned}$$

Denote by subscript the partial derivative. Then parents' optimal investments in the child are given by the following two first-order conditions:

$$U_c(c^p)\omega = p^d V_l^c(c^{d,c}) \frac{\partial h^d}{\partial l^p} + (1-p^d) V_l^c(c^{nd,c}) \frac{\partial h^{nd}}{\partial l^p} \tag{2}$$

$$U_c(c^p)\kappa = p^d V_k^c(c^{d,c}) \frac{\partial h^d}{\partial k^p} + (1-p^d) V_k^c(c^{nd,c}) \frac{\partial h^{nd}}{\partial k^p} \tag{3}$$

where solving equation (2) for l yields the optimal time investment of parents in their child. Likewise, solving equation (3) for k gives the optimal allocation of preschool time.

The first-order conditions imply that there are three main factors through which a heightened risk of deportation affects parents' investment decisions. First, how parents

[5]One could make such a distinction even more pronounced and allow the child also to make labor supply and human capital investment decisions when old. Such a model would not generate fundamentally different insights to what we discuss here, however, but would stress the long-lasting effects of initial parental human capital investment decisions. As we assume that the child takes parents' inputs as given and the deportation status is realized before the child reaches adulthood, one could solve such a model backwards, starting with the decision of the child in t_2. Parents would then face a similar problem as discussed below, taking into account the maximum reachable utility level of their child given the deportation status.

[6]Notice that in our setting, parents are fully altruistic toward their child and fully convert the utility of the child into their own utility.

react depends on what type of investment is perceived as more valuable when not being deported. Second, the relative costs of consumption, the attainable wage ω, and preschool fees κ all play a role in the adjustment process. Third, how parents change their investment decisions also depends on the relation between formal childcare and parental time investments and, more specifically, whether parents consider their own time investment and formal childcare to be substitutes or complements.

Parents' change in investments as response to heightened enforcement ultimately spills over to the human capital accumulation of their child. To see this more clearly, consider the child's payoff function in equation (1), which depends on parental investment. Differentiating it with respect to the risk of being deported p^d yields

$$\frac{\partial h}{\partial p^d} = \frac{\partial h}{\partial l^p}\frac{\partial l^p}{\partial p^d} + \frac{\partial h}{\partial k^p}\frac{\partial k^p}{\partial p^d}. \tag{4}$$

Assume that parents perceive their own time investments and preschool attendance as (weak) substitutes. There is strong evidence that this is the case and parents perceive their own inputs and educational inputs as substitutes (e.g. Das et al., 2013; Greaves et al., 2021). Also assume that skills learnt in preschool are in the future more valuable for the child when remaining in the US while skills derived from parental time inputs are equally valuable whether deported or not. Then, in our simple model, an increase in deportation risk caused by immigration policies leads parents to reduce their child's preschool attendance. However, to compensate for the decrease in formal educational inputs, parents raise the time investment in their child as a response.

How parents' reaction to immigration policies affects human capital accumulation of the child depends on both the productivity of each input and the magnitude of the change; see equation (4). If parental time investment is not as productive as formal early childhood education, for example, because parents are not aware of the educational benefits of certain activities, then children's skills decrease as a response to immigration enforcement.

While our conceptual framework is simple, it highlights how immigration policies can spill over to children's human capital. We analyze the spillover effects of immigration policies in section 5. In section 6 we investigate changes in parental investment behavior underlying the possible spillover effects. Compared to our simple model, we investigate the response to different types of parental inputs in our empirical analysis.

3 Data

We use several data sets to identify the effect of heightened immigration enforcement on children's human capital accumulation and parental investment decisions.

Data on Children's Language Skills

Our analysis of the impact of immigration enforcement on children's language skills is based on the 2005-2014 American Community Survey (ACS, see Ruggles et al., 2020). Approximately 3.5 million randomly sampled households are interviewed on a yearly basis. The ACS provides rich demographic, social, economic, and housing information for a representative sample of individuals and their households.

We construct two measures of English proficiency based on the survey question: "How well does this person speak English?" The question has four possible responses: "very well," "well," "not well," and "not at all." Following Kuziemko (2014), we construct a categorical variable Proficiency 0-3 corresponding to "does not speak English", "speaks English but not Well", "speaks well", and "speaks very well". We also use a dummy variable which takes the value of one if the child speaks English "very well," and zero otherwise.[7] A more objective measure for children's language skills would be preferable, but the self-report skills are the only measure available in the data. In addition, Vikstrom et al. (2015) find that the self-reported skills are a valid measure to assess English ability, specifically when using our dummy variable indicating high English skills.

One limitation of the ACS is the lack of information about the legal status of immigrants. To proxy the legal status in our work, we follow the literature and use Hispanic non-citizens who have not completed high school and who have lived in the United States for at least five years as proxy for undocumented immigrants (e.g., Amuedo-Dorantes et al., 2018; Amuedo-Dorantes and Arenas-Arroyo, 2019).[8] Then, we restrict our main sample to US-born children who are between 7 and 16 years old and have at least one likely undocumented parent, as previously defined. We will show as a robustness check that our results are similar when using an alternative proxy for the legal status of the parents, following the residual method used by Borjas (2017).

One might be concerned that undocumented immigrants affected by immigration enforcement may be less inclined to participate in the survey to avoid detection. While this is a valid concern, we do not think that it will lead to substantially biased estimates in our work. Previous works on the impact of immigration policies have found that the ACS is covering the population of likely undocumented immigrants well (Pope, 2016; Amuedo-Dorantes et al., 2018). The ACS interviews the resident population without regard to legal status or citizenship.[9] During the interview, the ACS only asks individuals whether

[7] Similar variables as a measure of language skills were also used in Bleakley and Chin (2004).

[8] As previous research shows, most undocumented immigrants have low education levels, and most of them are coming from Latin America (see, for example, Orrenius et al., 2018). Concentrating on Hispanic non-citizens as a proxy might include low-skilled immigrants or students with non-immigrant visas, however. We therefore follow Amuedo-Dorantes et al. (2018) and restrict our sample further to individuals without a high school diploma who have lived in the United States for at least five years.

[9] See the 2014 American Community Survey from the US Census Bureau, https://www.census.gov/history/pdf/acsdesign-methodology2014.pdf.

they are US citizens, naturalized, or hold any other citizenship. Hence, the group of non-citizens is a broad group comprised of all immigrants, including students and individuals on temporary visas. Given the sample design, all individuals have the same probability of being selected, regardless of their citizen status (Pope, 2016).

While there is no evidence that likely undocumented parents are underrepresented in the ACS data, they may intentionally misreport the language proficiency of their children when interviewed. For example, parents might overstate the English proficiency of their children as a way to signal that they are legally in the country. If this were true, our results would also reflect a lower bound (in absolute terms) on the impact of immigration enforcement on children's human capital.

One additional concern in our setting might be that if immigration enforcement reduced parents' interactions with native speakers, this might lead to a lack of natural reference points parents can use to compare their children's language skills. As a consequence, they might be less able to evaluate their children's language proficiency, leading to a biased response. There is evidence, however, that parents tend to overestimate their children's skills in situations where other children tend to have low skills (Kinsler and Pavan, 2021). In light of such biased parental beliefs, and as enforcement measures affect likely communities as a whole, if immigration enforcement reduces children's language skills our estimates will likely reflect a lower bound (in absolute values) on the true effect. Our data do not allow us to assess such a potential bias in more detail, however.[10]

Data on Parental Time Use

We are also interested in how immigration enforcement changes parental time investment in children. In our analysis, we make use of the American Time Use Survey (ATUS) from 2003 to 2018. The ATUS is an annual time use survey in the United States with the goal of measuring how people divide their time among different activities. Possible participants for the ATUS are randomly drawn from the pool of all Current Population Survey (CPS) interviewees who finish the CPS interview sequence. ATUS interviews are conducted by phone in either English or Spanish. The survey participants are asked about their activities starting at 4:00 a.m. on the designated day until 3:59 a.m. the following day, including the location of the activity and who else was present. While the ATUS has a much smaller sample size than the ACS, and while geographic information is only available at the state level, it provides detailed information on parental time investments.[11]

[10] Table D.1 in the appendix, shows that IE did not have any impact on Parents English Skills or Education level.

[11] The ATUS could be linked to the Current Population Survey (CPS) to get geographic information on the MSA level. The CPS is not representative for all MSAs, however, so we refrain from doing so.

When using the ATUS, we restrict the sample to low-skilled, Hispanic survey participants between 21 and 65 years, who lived in the US for at least five years, and with at least one child at preschool age, that is age 0 to 5, in the household. We concentrate on preschool children in the ATUS because language skills are largely shaped at a younger age (e.g Palermo and Mikulski, 2014). Early parental time investment also has likely persistent effects on children's cogntiive skills (Del Bono et al., 2016). By concentrating on preschool children, we therefore capture changes in parental investment decisions affected by immigration enforcement policies as one important underlying channel that can shape children's language skills. To proxy the legal status of individuals in our sample, we follow the same definition as in the ACS discussed above.[12]

As the ATUS contains detailed information about the nature of the activity, we can explore how immigration enforcement has changed patterns of parental time investments. To obtain a broader picture, we use four different activity groups: general care, educational time, recreational time, and social and leisure activities.[13]

Our general care measure includes activities such as eating and drinking, travelling or physical care. Educational time includes activities such as reading to or with the child, helping with arts and crafts, and helping with homework. Recreational activities include playing and doing sports with the child. Time spent on attending events, socializing, and participating in performances and plays are considered social and leisure time. [14]

In our analysis, we explicitly distinguish between educational and recreational time spent with the child. This distinction allows us to analyze the behavioral response of parents and their perceived returns to different forms of time investments. For example, immigration enforcement may lead parents to less likely enroll their children in non-mandatory pre-school. In order to compensate for the lost time in pre-school, parents may increase their time spent playing with the child, unaware that other educational activities, such as reading, are likely more productive in developing children's language skills; see, for example, Boneva and Rauh (2018) for a general discussion on parents' perceived returns to childhood investments. Such parental behavior can then ultimately lead to a lower accumulation of children's language skills.

We also include social and leisure time in our analysis to explore the possible role of social isolation as an additional mechanism which can affect language skills accumulating. A majority of social and leisure activities take place outside of participants' homes and with other people. Fewer or no possibilities to interact and learn from others may further reduce children's language skill accumulation (Henry and Rickman, 2007; Weisleder and

[12]Our results are robust to using alternative definitions such as the one in Borjas (2017).

[13]These four categories are finer than the two broad categories of basic childcare and educational/recreational childcare used in Aguiar and Hurst (2007), who study long-term trends in time use for the United States.

[14]See appendix C for further details.

Fernald, 2014). A reduction in parental social and leisure time would also point toward fear of being detected and deported as a channel through which immigration enforcement affects parental behavior.

From our sample, we exclude observations where the survey participants report to have spent an unusually large amount of time with their children. Specifically, we choose to exclude observations where the total time spent with their children on all four activities is above 17 hours per day. Similar restrictions were also applied in Fiorini and Keane (2014).

One might be concerned about the non-response of likely undocumented immigrants caused by immigration enforcement in the ATUS data. While the non-response rate in the ATUS is substantially higher in comparison to the CPS and also the ACS, there is no evidence that the higher non-response rate is driven by the refusal of likely undocumented immigrant to answer. First, if immigrants selectively take part in the ATUS, this should also be reflected in the CPS. The CPS covers the population of likely unauthorized immigrants reasonably well, however.[15] Second, if the introduction of immigration enforcement had affected the response rate, one would expect to see large changes over time. However, the non-response rate in the ATUS follows a similar trend over time as other household surveys which do not include sensitive questions on citizenship status, such as the Consumer Expenditure Survey.[16] Lastly, the results in Abraham et al. (2006) do not point to any differences in the general propensity of Hispanics to respond to the ATUS in comparison to non-Hispanic whites once background characteristics, such as age and sex of the participants, are taken into account.

While the above points do not suggest a large bias in our estimation caused by non-response when using the ATUS, we acknowledge that being unable to directly investigate any possible selectivity is a limitation in our empirical setting. Nevertheless, as with the ACS, we would expect our estimates to understate the true effect if non-response was an issue.

Data on Immigration Enforcement

We hand-collected historical and current data about different local police-based interior immigration policies. Specifically, we gathered data on 287(g) agreements from the ICE bureau's 287(g) Fact Sheet website. These policies are directly linked to apprehension and deportation. Information about the enactment of Secure Communities (SC) programs are

[15] For a discussion, see, for example, Jeffrey S. Passel and D'Vera Cohn, "US Unauthorized Immigrant Total Dips to Lowest Level in a Decade," Pew Research Center, November 27, 2018, https://www.pewresearch.org/hispanic/2018/11/27/unauthorized-immigration-estimate-methodology/.

[16] See the Bureau of Labor Statistics notes on household and establishment survey response rates: https://www.bls.gov/osmr/response-rates/home.htm.

obtained from the ICE Activated Jurisdictions document.[17] Similar policy data were also used, for example, in Amuedo-Dorantes et al. (2018) and Amuedo-Dorantes and Arenas-Arroyo (2021). In appendix A we provide further discussions about the 287(g) and the SC program.

Our hand-gathered data allow us to identify the date and name of the county enacting any 287(g) or SC measures. To merge the information on immigration policies available on the county level to the ACS data which are available on the MSA level, we use the cross-walk provided by the US Census Bureau.[18] Using information on the enactment date of the first immigration policy within an MSA, we construct a dummy variable $IE_{m,t}$ taking a value equal from the first year an MSA adopted an immigration policy, and zero otherwise. In appendix A, we show the roll-out of the policies over time. By the end of 2013, the whole United States was covered by at least one immigration policy.

4 Empirical Approach

To identify the effects of heightened immigration enforcement on children's English language proficiency, we rely on difference-in-difference and event-study approaches. To quantify average effects, we first estimate the following equation by exploiting the geographic and temporal variation in the enactment of our immigration policies on the sample of US-born children with at least one likely undocumented parent:

$$y_{i,m,t} = \alpha_m^{DiD} + \beta IE_{m,t} + X'_{i,m,t}\Gamma + \theta_t^{DiD} + \epsilon_{i,m,t}^{DiD} \tag{5}$$

where $y_{m,t}$ is the outcome variable, children's English proficiency, for a child i observed at time t and living in the metropolitan statistical area (MSA) m. $IE_{m,t}$ is an indicator variable equal to one if the MSA m has adopted a measure of interior immigration enforcement policy in year t, and zero otherwise. Thus, β represents the coefficient of interest in our analysis. It captures how immigration enforcement affects children's English language proficiency.

We also include children and household characteristics summarized by the vector $X_{i,m,t}$. Children's characteristics include age, gender, and grade level attendance. House-

[17] The 287(g) and Secure Communities enactment dates can be accessed through https://www.ice.gov/factsheets/287g and https://www.hsdl.org/?view&did=682236 respectively.

[18] Reference files are available at https://www.census.gov/geographies/reference-files/time-series/demo/metro-micro/delineation-files.html. We use the 2013 definitions for metropolitan statistical areas (MSAs) from the US Office of Management and Budget (OMB). This definition has two advantages. First, it provides a consistent identification of the MSAs from 2005 to 2014. Second, the delineations are entirely county-based. The latter allows us to merge our policy data directly to the ACS without any need for adjustments, such as additional weighting.

hold characteristics include the household head's marital status, years the United States, education level, gender, and total number of children in the household. Additionally, we also include geographic and temporal fixed effects. The geographic fixed effects α_m address unobserved and time-invariant area-specific characteristics potentially correlated with the outcome. The temporal fixed effects, captured by θ_t, account for aggregate level shocks potentially impacting children's English language proficiency. We cluster all standard errors at the local MSA level.

In our analysis, we use two different measures for language skills. Our first measure is *overall* English language proficiency, which ranges from zero (does not speak English) to three (speaks very well), so more proficient skills have a higher value. Therefore, a negative (positive) impact of heightened immigration enforcement on our outcome would imply that heightened immigration enforcement decreases (increases) the overall language proficiency of the US-born children of likely unauthorized parents.

As an alternative language skill measure, we also use a dummy variable which takes a value of one if the child speaks English "very well," and zero otherwise. As discussed in section 3, this binary variable captures the English proficiency of non-native speakers well, even under the presence of self-reporting bias, and captures high English skills.

We also estimate dynamic effects of the impact of immigration policies within an event-study framework:

$$\begin{aligned} y_{i,m,t} =& \alpha_m^{ES} + \sum_{\substack{a=-4 \\ a\neq -1}}^{5} \delta_a \mathbb{1}(t - C_m = a) + \delta_{-5} \mathbb{1}(t - C_m < -4) \\ &+ \delta_6 \mathbb{1}(t - C_m > 5) + X'_{i,m,t}\Gamma^{ES} + \theta_t^{ES} + \epsilon_{i,m,t}^{ES} \end{aligned} \tag{6}$$

where C_m is the year when the first immigration policy was introduced in MSA m. As we only have a limited number of observations for years distant from the actual treatment year, we bin all time periods with a relative treatment time further away than four years prior or five years after the introduction of the first policy. In our event study, we include the same set of control variables as in equation (6).

A dynamic specification as in equation (6) allows us to investigate any possible persistence in the effect of immigration policies on children's skill accumulation. It also helps us to identify possible delays until the effects of immigration policies on children's language skills materialized and to gauge when such policies have the most impact on children. For example, if parents minimized activities outside their home as a response to heightened immigration enforcement, a lack of social contacts would affect children's language skills likely only gradually. At the same time, we would also see a long-term decline in language skills with little sign of reversal if there are no interventions to compensate for the lack of social interaction.

The dynamic specification also enables us to examine possible differences in the outcomes prior to the adoption of the laws. If we do not find evidence that outcomes differed prior to the adoption of immigration enforcement, this will lend support to the so-called parallel trend assumption, which is necessary for identification in our models.[19]

5 Spillover Effects on Children's Language Skills

5.1 Main Results

The results from estimating equation (5) using ordinary least squares are shown in table 2. We estimate two different specifications without and with a full set of background characteristics to see how sensitive our results are.

Columns (1) and (2) show the estimation results using the proficiency score as the outcome variable. Immigration enforcement decreases the English proficiency score significantly by around 0.028 points when we only include year and meta area fixed effects; see column (1). Including additional controls barely changes our estimates. The results in column (2) indicate that immigration enforcement decreases children's English proficiency by around 0.023 points. That our results do not depend on the inclusion of additional household characteristics in our model is reassuring and gives us confidence in our identification assumption.

One might be concerned that our results are affected by the fact that survey participants are asked to rate the proficiency of the child. To minimize the risk that our estimation is driven by self-reporting bias and to better understand whether immigration enforcement affects children with high English skills, we also report the estimates for our binary outcome variable in columns (3) and (4). The outcome variable takes now a value of one if the child speaks English "very well," and zero otherwise.

Our estimates are in line with the findings using the proficiency score. If we only control for year and meta area fixed effects, the introduction of immigration enforcement policies reduces children's likelihood of speaking English "very well" by around 3 percentage points or more than 4 percent when compared to the mean; see column (3).[20] As before, our results remain virtually unchanged when we include a wide range of household characteristics in our estimation equation; see the results in column (4).[21]

Our estimates are comparable but of opposite sign to those in Kuziemko (2014) who evaluates how mandating school instructions to be in English in California (Proposition

[19] In appendix D we provide additional estimates using alternative difference-in-difference specifications robust to treatment effect heterogeneity.

[20] Using our estimates and the reported standard deviation of 0.41 from table 1, the effect size is $-0.0323/0.79$.

[21] Our results are also robust to including potentially endogenous covariates in the estimation, such as English proficiency of the survey respondent and employment status.

227) impacts the English language skills of foreign-born children (and their parents). The results from her model including additional PUMA controls imply that the policy increased the likelihood of speaking English "very well" for the average immigrant child by 6.5 percent.[22]

Our estimates also indicate large and opposite effects when compared to those of policy interventions aimed to improve children's language skills. For example, using the 2012 Head Start Impact Study, Puma et al. (2012) show that having access to Head Start at age four increases reading comprehension as measured by the ECLS-K Reading Assessment by 2.3 percent and vocabulary knowledge by 0.5 percent in third grade when compared to the control mean.[23] Given the evidence that policies aimed at improving early childhood skills also affect the long-term educational outcomes (e.g., Deming, 2009; Bailey et al., 2021), our results also imply that immigration enforcement likely lowers the future success of US-born children of likely undocumented parents. We provide evidence for such long-term effects in Section 7.

Overall, we find that the spillover effects of immigration enforcement on US-born children with likely undocumented parents are of comparable magnitude but opposite sign to policies intended to improve the language skills of disadvantaged children. Therefore, immigration policies not only counteract the purposes of policies aimed at increasing children's education, but they also may induce an inefficient allocation of resources. On the one side, substantial funding is allocated to improve the language skills of disadvantaged children, a large share of whom are of Hispanic origin.[24] On the other side, by introducing strict immigration enforcement measures that spill over to US-born children, any possibly positive effects of education policies are diminished or even entirely erased.

5.2 Dynamic Effects

Having established that immigration enforcement lowers the language skills of US-born children of likely undocumented parents, we investigate any dynamics of our effects next. Such an exploration allows us to explore any persistence in the effect of immigration policies on children's skill accumulation. It also allows us to assess if there are any trends in our outcome variable, prior to the enactment of immigration enforcement policies.

[22] Kuziemko (2014) estimates a coefficient of 0.148 on the interaction of Proposition 227 with the school compliance rate when additional PUMA controls are included. Taking the average compliance rate of 13 percent reported in the text and the mean of the outcome "speaks very well" of 0.28 in the children-parent sample, this implies an effect of $(0.148 \cdot 0.13)/0.28 = 0.065$.

[23] The Head Start program was launched in 1965. The program's primary goal is to boost the school-readiness of of low-income children by providing preschool education, healthcare support, nutrition services, and help for parents to foster their child's development.

[24] For example, Hispanic children are the majority of participants in the group of four-year-olds in the Head Start Impact Study.

Figures 1 and 2 plot the coefficient δ_a from equation (6) from five years prior to six years after the enactment of immigration enforcement for the English proficiency score and our binary indicator if the child speaks English "Very well" as an outcome, together with 95 percent confidence intervals.[25] From the estimated pattern in the figures, two important features emerge.

First, all of our estimates prior to the enactment of any immigration enforcement are both economically small and statistically insignificant. This is true both when using our proficiency score as outcome or our binary indicator if the child speaks English "very well." This lack in pre-trends considering both outcomes gives reassurance in our estimation strategy.

Second, we see a strong and significant decline in children's language proficiency after the enactment of immigration enforcement for both of our measures. The negative impact of immigration enforcement policies on children's language skills is also very persistent and does not show any sign of reversal. For example, within five years after the introduction of the first immigration enforcement policy, our results show that US-born children of likely undocumented parents have a more than 5 percentage points lower likelihood of speaking English "very well."

The gradual and persistent decline suggests that there is no intervention to compensate for the loss of language skills caused by heightened immigration enforcement. It also suggests that exposure to immigration enforcement at a younger age has more detrimental effects on children's accumulation of language skills than when exposed later during the life-cycle; see also Bleakley and Chin (2010). We provide additional evidence for this hypothesis in Appendix B. As shown in our framework in section 2, the dynamic estimates imply that immigration enforcement affects parental human capital investment decisions as an important underlying mechanism. Before exploring parents' responses to immigration enforcement in section 6, we discuss the robustness of our results first.

5.3 Robustness

Despite the absence of any detectable pre-trends, one might still be concerned that our estimates capture effects unrelated to the enactment of immigration policy. For example, schools in areas that saw a drop in test scores also enact immigration enforcement laws earlier. If this was true, our estimates would not reflect the impact of immigration enforcement on children's language skills but would instead pick up differences in school quality, at least partly.

In order to investigate such a possibility, we conduct a placebo check where we estimate equation (5) on a sample of US-born children with low-skilled documented parents

[25] Remember that the estimates for $t-5$ and $t+6$ represent binned estimates.

(naturalized or native).[26] Given that these families are citizen and therefore reside legally in the United States, they should not be affected by any immigration enforcement policies. The estimation results from our placebo regression are reported in columns (1) and (2) in table 3, using our two measures of children's English language proficiency.

As one can see from the results, we do not find evidence that immigration enforcement has any impact on the English proficiency of US-born children with documented parents. Our results are not only statistically insignificant but are also very small. This also allows us to rule out meaningful impacts for children of documented parents in general.

Additionally, we investigate the robustness of our results to how we proxy the legal status of parents. Remember that in our data, we do not directly observe whether an individual resides legally in the United States. We therefore use an alternative approach to proxy an individual's status using the residual method.[27] We first define who is living legally in the United States. Persons are considered to be legally in the United States if they satisfy any of the following criteria: they were born in Cuba, arrived before 1980, have US citizenship, receive public benefits, have a spouse who is a legal immigrant or US citizen, or work in the government sector. Then, according to the residual method, any person who does not fulfil this requirement is likely to be undocumented. The results when using this alternative proxy are shown in columns (3) and (4) in table 3.

Using the residual method to define likely undocumented parents leaves our results virtually unchanged. We still find that immigration enforcement policies lower both English proficiency and high English skills substantially and significantly.[28] That our estimates do not depend on the exact definition we use is reassuring.

We also investigate whether the negative impact we find is driven by children who drop out of school. Children in our sample are in general required to attend school, but some parents might pull their children out of school as a response to immigration enforcement. Thus, lower formal education might in the end explain children's lower language skills.

Even if we disregard children who drop out of school, we still find strong evidence on the negative impact of immigration enforcement on children's language skills; see columns (5) and (6). That our main results are unaffected by disregarding school dropouts also lends support to our motivation to have a closer look at how immigration enforcement policies can change early parental investment behavior.

[26] As in our main specification we restrict our sample to US-born children with low-skilled parents. The sample differs only by parents' citizen status; only US citizens are included in our placebo.

[27] The residual method was initially proposed by (Passel et al., 2014) and subsequently applied by others (e.g., Borjas, 2017).

[28] In appendix D, we also provide estimates from an event study design using the alternative definition to proxy for parents' legal status. The estimates are similar in terms of size and significance as those in figures 1 and 2.

In appendix D, we provide results from additional robustness checks we conduct. We evaluate whether children's English language proficiency scores predict the first year when an immigration policy is enacted. Such a correlation would likely indicate a violation of the no-anticipation assumption of the policy. We do not find any evidence for a systematic introduction of immigration enforcement policies as a response to children's language skills.

We also check if migrants are moving as a response to tougher immigration policies. To evaluate whether this is the case, we first look at the impact of the immigration policy on the population composition within the MSA. We do not find evidence that immigration enforcement impacted the composition of the MSAs, which implies that any bias introduced in our estimates by mobility is likely small. Second, we restrict the sample to those US children who did not move over the preceding year. We find similar results to those reported in table 2. Nevertheless, we would expect that migrants with more success in the labor market, and thus those with likely higher investments in their children, are more mobile. Therefore, any mobility bias in our estimates would likely lead to an understatement of the true spillover effects of immigration policies on children's human capital.

Finally, we also investigate whether heterogeneity in the estimated treatment effect might bias our results. The recent econometric literature on difference-in-differences has raised concerns that in settings with staggered treatment adoption, as in our case, standard estimates might be biased if treatment effects are heterogeneous (de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2021; Goodman-Bacon, 2021). Using the robust approach proposed by Sun and Abraham (2021), we do not find evidence that this is a concern in our estimation. Our results obtained from the robust approach are very similar both in terms of dynamics and magnitude as the results discussed above (see figure D.2 and figure D.1 in appendix D).

6 Parents' Response to Immigration Enforcement

To better understand our results, we explore changes in parental investment behavior caused by immigration enforcement as one important underlying channel. An increase in the deportation risk after the introduction of immigration policies may change parents' inputs in terms of formal education (e.g., preschool) and may also change their time investment; see our framework in section 2.

6.1 Preschool Enrollment Decision

We first investigate how immigration enforcement affects parents' decisions to enroll their children in non-mandatory preschool. Attending preschool as a form of formal educational care can improve children's language skills, specifically for children of disadvantaged backgrounds. Social interactions at a younger age with native speakers is particularly important for the development of language skills (e.g., Palermo and Mikulski, 2014; Villarreal and Gonzalez, 2016). An increase in the deportation risk caused by immigration enforcement might deter likely undocumented parents from enrolling their children in non-mandatory preschool programs. This decision might ultimately lead to lower language skills in the children.

In our analysis, we concentrate on children between the ages of three and four to capture the impact of immigration enforcement on enrollment in non-mandatory formal early childhood education programs. Preschool attendance is reported in the ACS only from age three onward. We choose the upper bound to be four, as in some states compulsory schooling starts at age five.[29] We present our estimation results using the difference-in-difference approach form equation (5) in table 4.

Looking at the results in column (1) of the table, we find that undocumented parents are less likely to enroll their US children in non-mandatory preschool as a response to immigration enforcement. The enrollment probability drops by around 2.19 percentage points or around 7 percent as a response to immigration enforcement.[30] This drop is quite substantial. We find similar but more precisely estimated effects using the residual method as proxy for parents' citizenship status; see column (2).

Our effect is of similar magnitude as those found in Santillano et al. (2020), who investigate the impact of immigration raids on the Head Start enrollment of Hispanics.[31] They find that an immigration raid reduces enrollment by approximately 10 percent. Our results show that immigration enforcement policies can reduce voluntary general preschool enrollment in the population of US-born children with likely undocumented parents. Such lower enrollment propensity ultimately leads to lower skill accumulation of US-born children of likely undocumented parents.

One might be concerned that our estimates reflect fundamentally different enrollment propensity of parents residing in areas enacting immigration policies. For example, lower enrollment might be caused by a decline in preschool quality. Parents affected by

[29] Our results are virtually identical when considering children between three years and five years. In some states, for example in Maryland, children have to attend a mandatory year of Kindergarten at age five before starting school at age six, however.

[30] In the ACS, 32 percent of US children with likely undocumented parents attend non-mandatory preschool programs.

[31] Related are also the findings in Watson (2014), who shows that Medicaid enrollment decreases if migrant apprehension in a region rises.

immigration enforcement might also pull out their children from general education to avoid detection, not only non-mandatory ones.

To investigate such concerns further, we first estimate the impact of immigration enforcement on the non-mandatory preschool enrollment of children of native or naturalized parents. The results are reported in column (3) of table 4.

Our results show that immigration enforcement has no effect on native or naturalized parents' enrollment decisions. The estimates for this group are very small and not statistically significant on any conventional level. These null effects are also quite precisely estimated, and we can rule out any meaningful impact of immigration enforcement.

We also estimate the impact of immigration enforcement on school attendance, concentrating on a sample of children of likely undocumented parents between 7 and 16 years old. At these ages, school attendance is mandatory in the US. The impact of immigration enforcement on mandatory attendance is shown in column (4) of table 4.

Immigration enforcement also does not affect mandatory school attendance. The estimated coefficient is very small and, as before, we can rule out meaningful effects. On the one side, the lack of any impact of immigration enforcement on mandatory school attendance is likely not surprising. Absence from mandatory school normally has to be justified by parents. Repeated absences can trigger external investigations, for example, by a school's child study team as in Florida. This can put undocumented parents in the spotlight of authorities, something they most likely try to avoid. On the other side, these results show that parents adjust their time spent on non-mandatory or extracurricular activities as response to immigration enforcement. Overall, there is no evidence that systematic unobserved differences in parents' general enrollment decisions between MSAs with and without immigration enforcement can explain our results.

6.2 Time Investment Decision

We find that immigration policies reduce non-mandatory preschool enrollment of children of likely undocumented parents. Given the predictions in our model, it is interesting to see whether parents try to compensate for the likely disadvantageous effect by adjusting their time spent with the child. To do so, we look at the impact of immigration enforcement on how parents with children of preschool age divide their time among general, educational, social, and recreational activities, using our ATUS sample. Notice that in comparison to our conceptual framework, we allow for different types of time investment in our empirical analysis. For example, including separate activities "education" and "recreation" in our analysis allows us to obtain further insights how parents perceive the productivity of their inputs and how immigration enforcement causes their investment behavior to shift.

Given that the ATUS is only representative on the state level and the significantly smaller sample size compared to the ACS, we estimate a slightly modified version of our difference-in-difference model. In our modified model, we explicitly use the proxied immigration status of the parent (see, for example, Kuka et al., 2020, for a similar approach using DACA eligibility status):

$$y_{i,a,s,t} = \alpha^{PT} + \Lambda_1^{PT} IE_{s,t} + \Lambda_2^{PT} LU_i + \beta^{PT} IE_{s,t} * LU_i + X'_{i,s,t}\Gamma^{PT} + \gamma_s^{PT} + \theta_t^{PT} + \epsilon_{i,a,s,t}^{PT} \tag{7}$$

where $y_{i,a,s,t}$ is the time spent (in minutes) of individual i living in state s on activity a at time t. The vector $X_{i,s,t}$ includes children and household characteristics. Similar as before, $IE_{s,t}$ is our indicator variable equal to one if the state s has adopted a measure of interior immigration enforcement policy in year t, and zero otherwise. The variable LU_i is an indicator that takes the value of one if the individual in the sample is a likely undocumented immigrant, as discussed in section 3.

The estimates of our coefficient of interest, β^{PT}, are reported in table 5. In each column of the table we present the impact of immigration enforcement on parental time spent with the child in one of our four activities. The effects are expressed in minutes per day. Notice that we consider parents with preschool-age children only.

In general and as shown in column (1), we find an increase in general time spent with the child. Parents affected by immigration enforcement spend around 15 minutes more per day generally caring for their child. Our estimates are very noisy, however, and not statistically significant on any conventional level.

Looking at the effect of immigration enforcement on direct educational input, reported in column (2), we do not find evidence that parents directly compensate for lower preschool enrollment. We find, however, evidence that parents spend considerably more recreational time with their children by playing and doing sports with them. The estimates presented in column (3) indicate that immigration policies increase recreational time with the child by more than 13 minutes per day. This is quite a substantial increase compared to the mean and implies that immigration enforcement leads parents to double their time spent on recreational activities with their children.

One can infer from these results that parents try to compensate for not sending their child to preschool by increasing the time spent with their children. As the increase is concentrated on recreational activities, the quality of the time investment is likely not sufficient to fully offset and compensate for the disadvantages of not sending their child to preschool. One explanation for such a behavior may be that undocumented parents tend to underestimate the importance of early human capital investments (e.g., Boneva and Rauh, 2018).

Another explanation might be that the positive effect of immigration enforcement on recreational time spent with the child is employment-related and simply mechanical. After the introduction of immigration enforcement measures, prospective employers might be more reluctant to hire likely undocumented immigrants; see, for example, the discussion in Amuerdo-Dorantes et al. (2021) and East et al. (2021). Facing fewer employment possibilities, parents spend more time at home, which also leads them to mechanically increase the time interacting with their children.[32]

Our estimates on parental time spent socializing with the child, presented in the last column of table 5 do not support such an explanation. Immigration enforcement reduces time spent on daily social and leisure activities by 37 minutes per day, or a drop of roughly 50 percent compared to the mean. If parents really did spend more time with their children only because they spent less time at work, then we also would expect to see them spend more time on social activities. At least, we would not expect such a large drop as we estimate.

The negative impact of immigration enforcement on social activities suggests, however, that immigration enforcement reduces children's time interacting with others. As a consequence, children also have fewer opportunities to learn from others. Given that immigration enforcement reduces the likelihood of attending formal childcare this shift in parental time investments further lowers children's language skills accumulation.

Our results show two important channels through which immigration enforcement can lower children's language skills. On the one side, immigration enforcement leads to a reduction in formal childcare and time spent interacting with others. On the other side, parents try to compensate for this reduction in formal education with an increase in recreational time spent with the child. This additional time, however, is less productive. The shift in parental input ultimately lowers children's language skills. These changes in parental investment behavior are consistent with both chilling effects, where the fear of deportation caused by immigration enforcement leads to social isolation of likely undocumented parents and their US-born children, and an income effect, caused by a reduction in employment.[33]

The findings presented here complement and extend the results of Kuka et al. (2020), who show that granting temporary work authorization and deferral from deportation for undocumented, high school-educated youth through the Deferred Action for Childhood Arrivals (DACA) program increases human capital investment, likely through higher perceived returns to education. We show that parents change their human capital investment decisions as a response to immigration enforcement. Parents substitute formal childcare

[32]Using the ATUS, we find that immigration policies reduce weekly hours worked by around 1.5 hours. These effects are imprecisely estimated, however.

[33]The reduction in employment and therefore income for undocumented parents might itself be caused by chilling effects, see also the discussion in East et al. (2021).

with parental care at home. However, such parental care is not as productive, leading to lower human capital accumulation in children; this can explain the lower language proficiency we have documented in US-born children of likely undocumented parents.

7 Long Run Effects

Language skills play an important role in explaining long-term outcomes, such as educational success (Bleakley and Chin, 2010; Aucejo and James, 2021). In this section, we provide evidence of how exposure to immigration enforcement during early childhood can lower human capital later in life.

To do so, we concentrate on human capital at age 19 using two measures: having completed high school and being enrolled in college. At age 19, most children have made their human capital investment decisions. It is also the longest time we have information about the educational attainment of children exposed to immigration policies during early childhood in our data.

Given that the ACS does not allow us to establish the intergenerational links, we restrict our sample to Hispanic US-born individuals aged 19, acknowledging that some of those children were born to documented immigrant parents.[34] Including children of documented migrants leads likely to an underestimation of the true effect, however. Then, we use the information on the state of birth during childhood from the ACS to determine whether an individual in our data was exposed to immigration enforcement during early childhood up to age four. These individuals constitute our treatment group. All other individuals are in our control group.

The impacts of early exposure to immigration enforcement on human capital at age 19 are reported in Table 6. Before discussing our results, we want to highlight that we do not interpret our long-term estimates as ultimate causal evidence given our data constraints. We consider them as suggestive but interesting evidence of how immigration enforcement can spill over to children's human capital and the long-run consequences.

Early exposure to immigration enforcement reduces the likelihood of completing high-school by age 19 by a statistically significant 4.9 percentage points or around 5.6 percent; see Column (1) in the table. This effect is quite sizeable, also in comparison to other policies. For example, Bailey et al. (2021) find that access to Head Start increased

[34] To the best of our knowledge, there is no other publicly available data that allows establishing intergenerational links while also providing detailed information as in the ACS, for example, about educational attainment and labor market outcomes. For example, we cannot use our definition from Section 3 to proxy for likely undocumented parents as in the ACS we observe neither parents' citizenship nor information about parents' time lived in the US for respondents living outside the home of their parents during the survey interview.

high school completion by 2.6 percent. Kuka et al. (2020) show that eligibility for DACA overall increased high school completion by 5.5 percent.

In Column (2), we present an estimate of the impact of early exposure on the likelihood of attending college at age 19. Our results suggest a negative, though imprecise, the impact of immigration policies on college enrollment. The magnitude of the effect is similar, but the opposite sign, to the impact of Head Start on college enrollment in Bailey et al. (2021). It is also comparable to Kuka et al. (2020). Given the statistical uncertainty associated with our estimates, we do not want to read too much into our results, however.

Overall, we provide evidence that the negative spillover effects of immigration enforcement during early childhood have long-run consequences on human capital later in life. Children exposed at age 0 to 4 are less likely to have completed high school at age 19. We also find evidence that these children are less likely to enroll in college. Higher human capital is, however, not only important for earnings and earnings growth but also likely provides certain protection from prolonged, repeated labor market disruptions, for example, due to unemployment. Ultimately, the lower human capital accumulation during childhood and early adulthood will likely increase their dependence on the social welfare system for these children.

8 Conclusions

Considerably more resources have been devoted to immigration and customs enforcement in the United States since 2001. Many of the introduced policies are aimed at identifying, apprehending, and ultimately deporting undocumented immigrants in the country. While they primarily target undocumented immigrants, the negative consequences of immigration enforcement can spill over to US-born children of those undocumented individuals.

Using the temporal and spatial variations in the introduction of immigration enforcement policies, we evaluate how immigration enforcement policies affect language skills of US-born children of likely undocumented parents. We concentrate on language proficiency as one important skill. On the one side, it is an important determination for future educational and labor market outcomes. On the other side, language skills of children are largely shaped by social interactions, which immigration enforcement policies might reduce.

Our difference-in-difference estimates show a large and significant impact of immigration enforcement on children's English language skills. Our estimated effects are of similar size but have the opposite sign as important programs to improve skills of disadvantaged children, such as Head Start. Investigating the dynamics of the effect, we find that immigration enforcement leads to a gradual deterioration of children's English language skills over time without a sign of reversal. This suggests a role for changes in parental human capital investment behavior caused by immigration enforcement as an important underlying channel.

We find that immigration enforcement indeed changes parents' human capital investment in their children. Heightened immigration enforcement deters undocumented parents from enrolling their younger children in non-mandatory preschool. Parents also reduce time spent on leisure and social interactions, activities often done with other people and outside one's own home. While parents respond by increasing recreational time or play time spent with their children, this extra time cannot fully compensate for reduced formal education; this ultimately leads to a decrease in English language skills. We provide evidence on the long-term spill-over effects, reducing affected children's likelihood of graduating high-school and enrolling in university by age 19.

Overall, our results show substantial negative spillovers of immigration enforcement on US-born children. As US-born children of undocumented parents can legally stay in the country, lower accumulated human capital arising from immigration enforcement will likely lower these children's education and labor market prospects. It also likely increases their dependence on the social security system later in life. Ultimately, immigration enforcement will hamper intergenerational mobility for these children, reversing the slow progress these disadvantaged groups have made.

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Tables

Table 1: Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.
Child's English skill: Proficiency	105,703	2.76	0.50
Child's English skill: High Skills	105,703	0.79	0.41
Child's age	105,703	12.53	3.09
Child's gender	105,703	1.49	0.50
Chid's education	105,703	2.29	1.44
HH without HS diploma	105,703	0.06	0.24
Single HH	105,703	0.22	0.42
Years in the United States HH	105,703	20.92	7.58
Female HH	105,703	0.45	0.50
Number of children in the HH	105,703	2.07	1.32
Immigration enforcement	105,703	0.76	0.43

Note: Sample: US-born children with at least one undocumented parent.
Source: Author's calculations, ACS 2005–2014.

Table 2: Impact of Immigration Enforcement on Children's English Skills

	(1)	(2)	(3)	(4)
	Proficiency	Proficiency	High Skills	High Skills
Immigration enforcement	-0.0276**	-0.0228*	-0.0323***	-0.0284**
	(0.012)	(0.013)	(0.012)	(0.012)
Child's age		0.0235***		0.0200***
		(0.003)		(0.002)
Child's gender		0.0209***		0.0175***
		(0.003)		(0.002)
Child's education		-0.0040		-0.0051**
		(0.004)		(0.002)
HH without HS diploma		0.0560***		0.0494***
		(0.009)		(0.008)
Single HH		-0.0089		-0.0060
		(0.007)		(0.005)
Years in the United States HH		0.0037***		0.0029***
		(0.000)		(0.000)
Female HH		0.0169***		0.0132***
		(0.005)		(0.004)
Number of children in the HH		0.0036		0.0014
		(0.002)		(0.002)
Constant	2.7446***	2.3471***	0.7883***	0.4626***
	(0.013)	(0.026)	(0.012)	(0.020)
Observations	105,703	105,703	105,703	105,703
R-squared	0.026	0.054	0.026	0.055
MSA FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Note: Table 2 reports the estimates from equation (5). Specification 1 includes year and area fixed effects. Specification 2 includes individual and household (HH) characteristics. Proficiency is a 0-3 categorical variable corresponding to "does not speak English," "speaks English but not well," "speaks well," and "speaks very well." High Skills is a dummy variable 1 if child speaks English very well, and 0 otherwise. Robust standard errors are in parentheses. Standard errors are clustered at the MSA level. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Documented Parents		Alternative LU		No School/Dropout	
	Proficiency	High Skills	Proficiency	High Skills	Proficiency	High Skills
Immigration Enforcement	-0.0039	-0.0066	-0.0242*	-0.0310**	-0.0237*	-0.0293**
	(0.013)	(0.010)	(0.013)	(0.013)	(0.012)	(0.012)
Observations	23,816	23,816	90,421	90,421	101,178	101,178
R-squared	0.094	0.084	0.056	0.056	0.064	0.066
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Table 3 reports the estimates from equation (5). Proficiency is a 0-3 categorical variable corresponding to "does not speak English," "speaks English but not well," "speaks well," and "speaks very well." High Skills is a dummy variable 1 if child speaks English very well, and 0 otherwise. Columns (1) and (2) report the estimates from equation (5) using the sample of US-born children with low-skilled documented parents (naturalized or native). Columns (3) and (4) use the residual method to identify likely undocumented (LU) parents. The estimates in columns (5) and (6) are based on a sample when all children who drop out of school are disregarded from the estimation. Standard errors are clustered at the MSA level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Impact of Immigration Enforcement on Preschool Attendance

	(1) LU Parents	(2) Alternative LU	(3) Documented Parents	(4) Mandatory School Enrollment
Immigration Enforcement	-0.0219*	-0.0282**	0.0022	-0.0009
	(0.013)	(0.012)	(0.010)	(0.002)
Observations	25,663	24,852	54,147	91,165
R-squared	0.158	0.165	0.243	0.008
Mean D.V.	0.32	0.32	0.49	0.99
Individual Controls	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Note: Table 4 reports the estimates from equation (5) where the dependent variable is preschool attendance. The sample used in column (1) consists of all US-born children with at least one likely undocumented (LU) parent and valid information on pre-school attendance. In column (2) we use the sample of US-born children with at least one undocumented parent using the residual method. In column (3), estimates are based on a sample of US-born children with documented parents (native or naturalized). All children between 7 and 16 years old and therefore at mandatory schooling age are used in column (4). Robust standard errors are in parentheses. Standards errors are clustered at the MSA level. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Impact of Immigration Enforcement on Parental Time Investment

	(1) General Care	(2) Educational Time	(3) Recreational Time	(4) Social Time
Immigration Enforcement x LU	14.901	-1.280	13.405**	-37.375***
	(13.206)	(1.192)	(5.686)	(12.582)
Observations	1,767	1,767	1,767	1,767
R-squared	0.142	0.072	0.128	0.153
Mean D.V.	69.13	2.50	12.80	71.16
Individual Chart.	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Note: Table 5 reports the estimates from equation (7). All specifications include household and respondent characteristics such as age, sex of the respondent, marital status, and number of children in the household. In addition, year as well as interview month and day fixed effects are included. Regressions are weighted by the ATUS person weights. The sample consists of Hispanic respondents aged 21 to 65, who have at most high school education, have lived in the United States for at least five years, and who have at least one child age five or younger in the household. See appendix C for a detailed description of activity categories. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

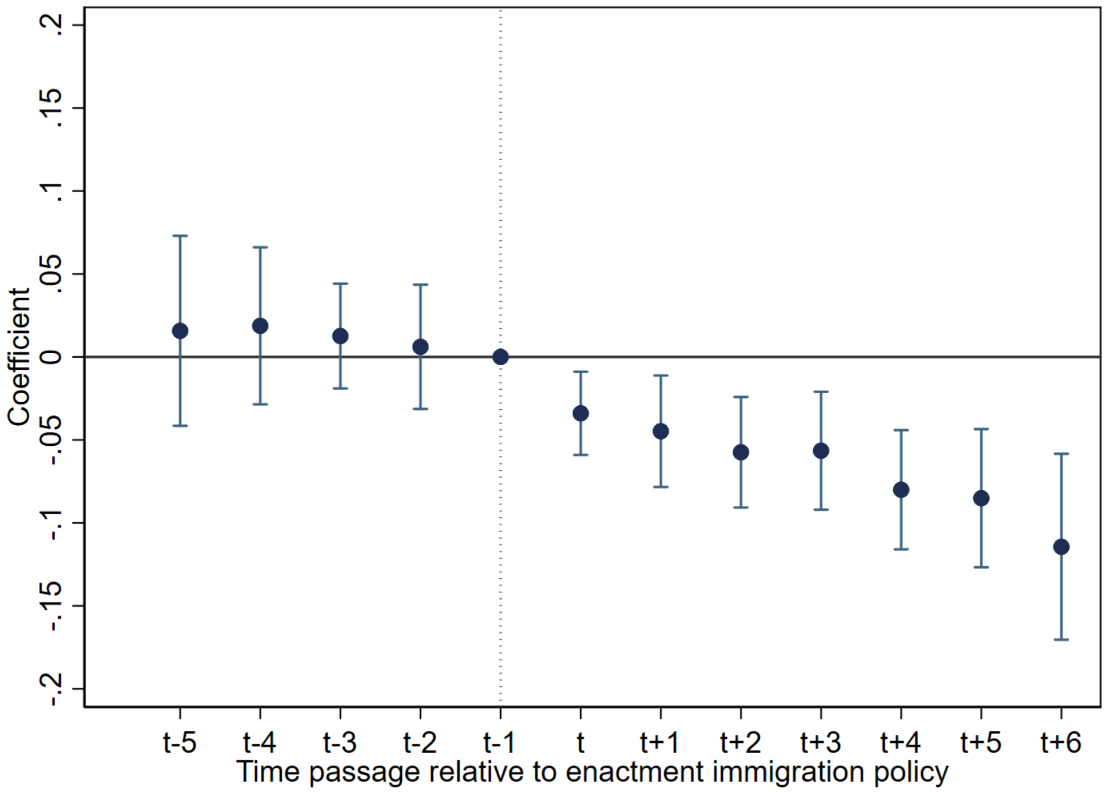
Table 6: Long Run Effects of Exposure to Immigration Policies at Age 0 to 4

	(1) High School Completion	(2) College Enrollment
IE at age 0 to 4	-0.0490**	-0.0225
	(0.021)	(0.034)
Observations	15,931	15,931
R-squared	0.019	0.032
Individual Characteristics	Yes	Yes
State FE	Yes	Yes
Year FE	Yes	Yes
Mean D.V.	0.87	0.32

Note: Table 6 reports the impact of exposure to immigration enforcement at age 0 to 4 on educational attainment. All specifications include individual characteristics such as age, sex of the respondent, marital status. The sample consists of Hispanic U.S.-citizen individuals age 19. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

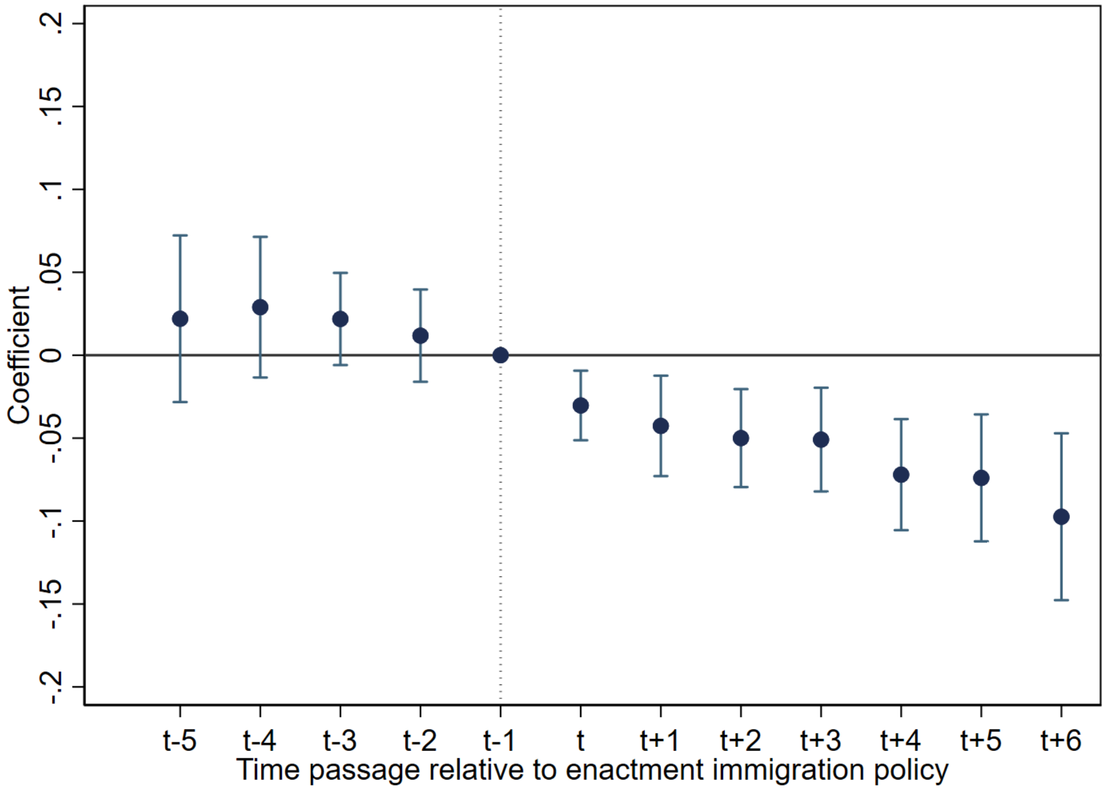
Figures

Figure 1: Event Study Coefficient Plot: Measure of English Proficiency



Note: Event-study coefficient plot equation 6. Period t represents the first year immigration enforcement policy was enacted in the local area. The outcome variable is a 0-3 categorical variable corresponding to "does not speak English," "speaks English but not well," "speaks well," and "speaks very well."

Figure 2: Event Study Coefficient Plot: Measure of High English Skills



Note: Event-study coefficient plot equation 6. Period t represents the first year immigration enforcement policy was enacted in the local area. The outcome variable is a dummy variable 1 if the child speaks English very well, 0 otherwise.

Web Appendix

This web appendix (not for publication) provides additional material discussed in "Spillover Effects of Immigration Policies on Children's Human Capital" by Esther Arenas-Arroyo and Bernhard Schmidpeter.

A Summary of Immigration Policies

We provide an overview of the policies we use in our work in table A.1 and the expansion over time in figure A.1. Below we discuss each of the policies in more detail.

A.1 287(g)

The 287(g) agreements evolved from the 1996 Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA), which allowed state and local agencies to enforce immigration law. Under this program, the federal government may sign an agreement (so-called Memorandum of Agreement or MOA) with local agencies, allowing designated officers to perform immigration law enforcement functions. This is the only program that permits local law enforcement officials to enforce federal immigration law directly. Through these agreements signed between the Department of Homeland Security (DHS) and the local authorities, a limited number of police officers receive authority to enforce immigration law. Designated officers have to satisfy certain conditions, receive four weeks training from DHS, are under the supervision of DHS, and do not get extra payment for doing this job.

During the time period that we are analyzing, there were three types of 287(g) agreements: "task force," "jail enforcement," and "hybrid." Under the "task force" agreement, local officers could interrogate and arrest non-citizens they believed had violated federal immigration laws.[1] The "jail enforcement" model permitted local officers to question immigrants who had been arrested on local charges about their immigration status. The "hybrid model" combined both models. Task officers could initiate immigration processing and transfer individuals thought to be subject to removal to jail officers who completed the immigration screening and ICE paperwork requirements (Council, 2021).[2]

The main expansion of this program took place between 2006 and 2013. Federal funding allocated for this program grew quickly from $5 million in 2006 to $68 million

[1] This program was active until 2012.

[2] See Capps et al. (2011) for further details about the 287 (g) program between 2005 and 2014. Note that Task Force Program was active until 2012. There is a new model, "Warrant Service Officer Model," which was introduced in 2019.

in 2010 (Council, 2021).[3] As a consequence of this rapid expansion, ICE has trained and certified more than 1,675 officers to enforce immigration laws (Kandel, 2016).

A.2 Secure Communities

US Immigration and Customs Enforcement (ICE) announced the Secure Communities (SC) program in March 2008. It prioritizes the use of enforcement resources to target non-citizens who have committed serious crimes. Under the SC program, ICE has a technology presence in jails. This is achieved through data systems that identify non-citizens who have committed crimes by checking their fingerprints against the Federal Bureau of Investigation (FBI) dataset for criminal arrest and convictions, and against the Department of Homeland Security (DHS) dataset that tracks their immigration history. Unlike with 287(g), local agents are not empowered to enforce immigration laws.

Although it was established as a voluntary program, in 2011, ICE clarified that an agreement between ICE and the state is not necessary, and that all jurisdictions will be activated in 2013. As a consequence, the program expanded quickly from its initial implementation in seven jurisdictions in 2008 to all of the nation's 3,181 jurisdictions in 2013. As a direct consequence, the number of fingerprints submitted grew from 828,119 in 2009 to 6.9 million in 2011 (Meissner et al., 2013).

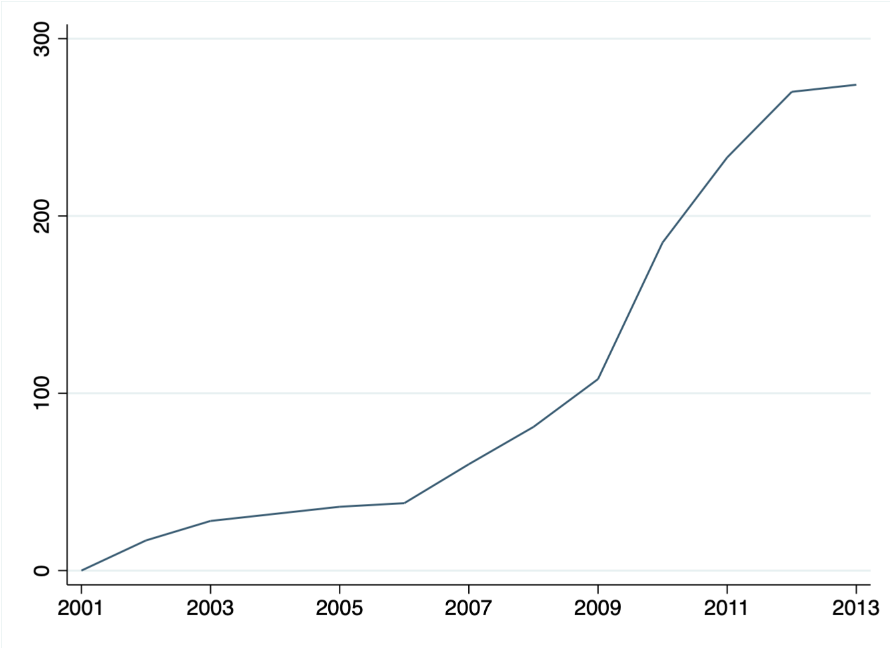
[3] Between 2010 and 2013, federal funding for 287(g) remained stable at $68 million. Since 2013, the funding allocated to this program has decreased.

Table A.1: Summary of Interior Immigration Policies

Law	Roll-out Years	Area	Description
287(g)	2002-	Street/Jail	*Task Force:* Provide local and state police officers the authority to interrogate any immigrant, arrest without warrant, and begin the removal process. *Jail Enforcement:* Allow police officers to question immigrants who have been arrested about their immigration status.
Secure Communities	2009–2014	Nation's jails and prisons	Allow to use the FBI and DHS datasets.

Note: Table A.1 shows the policies and programs activated during our period of analysis 2005–2014.

Figure A.1: Immigration Enforcement Expansion over Time



B Results by Age at Exposure to Immigration Enforcement

We provide additional results on how the impact of immigration enforcement on English language skills varies with age at exposure. In the ACS, we only observe an individual's place of residence one year prior to the interview. As children in our sample are 12 years old on average and we are interested in the the impact of age-at-exposure, we therefore need to make the arguably strong assumption that families have not moved since the birth of the child and use the information about the current place of residence.[4] Under this assumption, we are able to calculate at what age the child was first exposed to immigration enforcement using the current location and the information on the introduction of immigration enforcement policies.

Having constructed the age-at-exposure variable, we estimate a similar model as equation (5) in the main part of the paper, replacing the immigration enforcement indicator with a set of age-at-exposure indicators.[5]

$$y_{i,m,t} = \alpha_m^{AE} + \sum_{e=0}^{E} \beta_e \mathbb{1}(C_m - b_{i,m} = e) + X'_{i,m,t}\Gamma + \theta_t^{AE} + \epsilon_{i,m,t}^{AE} \tag{B.1}$$

[4]If parents who are more affected by immigration enforcement are also more mobile this assumption implies that we likely under-estimate the impact of immigration enforcement.

[5]Alternatively, we could include age-at-exposure as a continuous variable. The results are similar to the ones reported here. Our approach is more flexible as it does not impose any restrictions on the shape of the age-at-exposure effects.

where, as before, $y_{m,t}$ is the outcome variable, children's English proficiency, for a child i observed at time t and living in the metropolitan statistical area (MSA) m, $X_{i,m,t}$ is a vector of children and household characteristics, and α_m^{AE} and θ_t are MSA and time fixed effects respectively.

C_m is the year when the first immigration policy was introduced in MSA m and $b_{i,m}$ the birth year of the child i, assuming that the MSA of birth is the same as the current MSA of residence. Therefore, the coefficient β_e measures the effect of immigration enforcement on language skills when the child was exposed to the policy at age e.[6]

One can interpret the estimates from our model in equation (B.1) as capturing different treatment intensities. Children born closer to the enactment are more (and longer) affected by immigration enforcement than children who were older when the first policy was introduced. While we are in general able to identify the impact of exposure to immigration enforcement on language skills for each age group as long as the parallel trend assumption holds, we cannot necessarily compare (causally) effects across different age groups (intensities) under this assumption; see Callaway et al. (2021) for an extensive discussion. In other words, without imposing stronger assumptions β_e reflects the impact of immigration enforcement on the language skills of children when treated at age e, for children actually treated at age e. This is similar to the standard binary treatment setting, where β reflects the impact of immigration enforcement on language skills for children actually being exposed to immigration enforcement.

To be able to obtain the impact of immigration enforcement on children's language skills when treated at age e and compare the effects across different ages at exposure, a stronger parallel trend assumption is required. Under this stronger assumption, we need to rule out that there is any selection into *when* (i.e. at what age) a child is exposed to immigration enforcement.[7] In other words, likely undocumented parents do not time the birth of their child or, at least, the timing is unrelated to any potential outcome of the child.

One might be concerned that parents, to a certain extend, can time the birth of their child. For example, parents who resided in the U.S. for a certain time but who decided to have children later may accumulated more resources over time to invest in their child than similar parents who decided to have their children earlier after arrival.[8] One the one side, this implies that children born to the first type of parents are younger

[6]We exclude all children who were exposed to immigration enforcement prior to birth, i.e. all always-treated observations.

[7]In general, the stronger parallel trend assumption requires that the language skills of all other children had they been treated at age e would evolve similar over time to the outcomes of children who actually were treated at age e. This is different to the standard parallel trend assumption which imposes counterfactual parallel outcomes over time in comparison to children not affected by immigration enforcement only.

[8]There is evidence that delaying birth has a positive effect on mother's labor market outcomes (e.g. Miller, 2011). The impact might, however, be different for likely undocumented mothers.

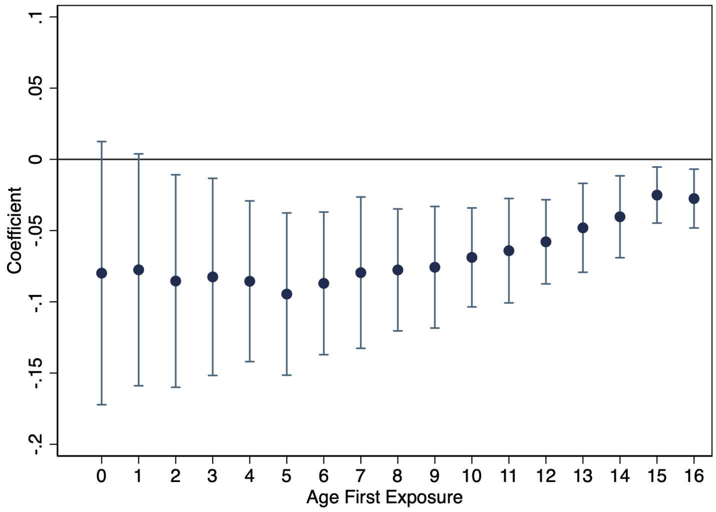
when immigration enforcement policies are introduced and therefore are more exposed to them. On the other side, given likely more resources parents can invest, these children are likely more resilient to the negative spill-over effects of immigration policies. Such differential timing of fertility behavior of otherwise similar parents would violate the strong parallel trend assumption, although the timing is not necessarily related to the introduction of immigrating policies. Such a timing behavior would also imply that we would underestimate the impact of exposure at younger ages compared to exposure at older ages.

While we provide multiple evidence for the random timing of the introduction of immigration enforcement policies and the "weaker" parallel trend assumption, see Section 5 and Appendix D, we acknowledge that the strong parallel assumption may be too restrictive in our setting.[9] Despite this drawback, we still consider the impact by age at exposure to be an interesting and relevant parameter. The effects of immigration enforcement on language proficiency and speaking very well English for different ages at exposure are shown in Figures B.1 and B.2 respectively.

There is a clear age-at-exposure gradient in the accumulation of language skills, both when using overall proficiency or speaking English very well as an outcome. The largest drop in the accumulation of language skills happens when children are exposed to immigration enforcement at pre-school age. Afterward, there is a slow but clear fading out of the effect with age when first exposed to immigration policies. The results presented here are the opposite to Bleakley and Chin (2010) who show that foreign born children accumulate more language skills when exposed from early on to English language. Our estimate provide evidence that children exposes to immigration enforcement early during the critical time of skill accumulation have lower English language skills. Changes in parents' investment behavior cannot compensate for the lower accumulation of language skills from early one leading to long-term negative effects.

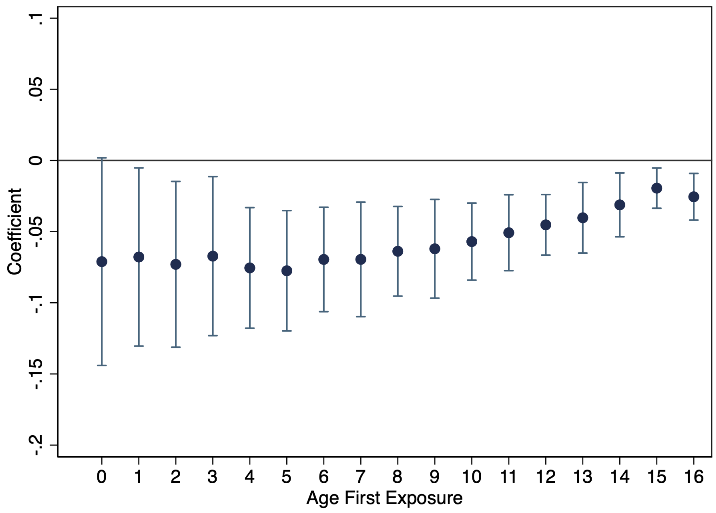
[9] Empirically, the stronger parallel trend assumption leads to the same estimand as the weaker version. Therefore, assessing pre-trends does not help to distinguish between the stronger and weaker version.

Figure B.1: Age at First Exposure Coefficient Plot: Measure of English Proficiency



Note: Age of first exposure coefficient plot equation B.1.The outcome variable is a 0-3 categorical variable corresponding to “does not speak English,” “speaks English but not well,” “speaks well,” and “speaks very well.”

Figure B.2: Age at First Exposure Coefficient Plot: Measure of High English Skills



Note: Age of first exposure coefficient plot equation B.1.The outcome variable is a dummy variable 1 if the child speaks English very well, 0 otherwise

C Summary of Parental Activities

Table C.1 summarizes the included activities in each of the four categories used in the analysis. The definition follows closely Fiorini and Keane (2014).

We restrict the sample of survey participants to low-skilled Hispanics without a high school diploma between 21 and 65 years old with at least one child age five or under and who have resided at least five years in the United States. For each participant and each activity defined in table C.1, we obtain information from the data about the duration of the activity and who else was present. If an activity was done with multiple children, we count the duration of each activity only once. For example, if the survey participant states that she went shopping for 60 minutes with her two children ages five and three, we add 60 minutes to general care time only once.

We sum the time spent on each activity within each of our four categories to obtain our final measures of parental time investment. From our sample, we exclude parents who claimed to have spent an unlikely high amount of time during the day with their children. Specifically, we sum over all of our four categories and exclude observations with a total time spent with the child of more than 1,020 minutes per day or more than 17 hours. This corresponds roughly to the 99th percentile of the time distribution. Similar restrictions were also applied in Fiorini and Keane (2014).

Table C.1: Time Use Activities

Category	Included Activity
General care	Physical care Eating and drinking Organizing and planning Traveling
Educational time	Reading to/with the child Helping with homework Helping with/Doing arts and crafts Showing/helping child Attending school meetings
Social and leisure time	Attending events Socializing and leisure Participating in performances and plays
Recreational time	Playing with the child Doing sports

D Additional Robustness

Table D.1: Impact of IE on HH Composition

	(1) HH Proficiency	(2) HH High Skills	(3) HH no diploma
Immigration Enforcement	0.0017 (0.018)	0.0020 (0.006)	0.0016 (0.004)
Observations	104,461	104,461	105,703
R-squared	0.112	0.041	0.009
Individual Characteristics	Yes	Yes	Yes
Metarea FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Note: Table D.1 reports the estimates from equation (5). Sample: Likely undocumented parent with an U.S.-citizen child. Proficiency is a 0-3 categorical variable corresponding to "does not speak English," "speaks English but not well," "speaks well," and "speaks very well." High Skills is a dummy variable 1 if HH-head speaks English very well, and 0 otherwise. Robust standard errors are in parentheses. Standard errors are clustered at the MSA level. *** p<0.01, ** p<0.05, * p<0.1

D.1 Endogenous Exposure to Immigration Enforcement

One might be concerned that in our setting the timing of the enactment of immigration enforcement policies is related to children's English language skills. For example, authorities might perceive lower language skills of children of undocumented parents in their MSA as proxy of bigger problems with the immigrant community and might therefore tend to introduce immigration policies earlier. This would violate our identification assumptions that our outcome is uncorrelated with the enactment of the policies.

In order to see whether children's English skills predict the enactment year of immigration enforcement policies, we use the information on the adoption timing of the immigration enforcement polices in each MSA and estimate the following equation using data for 2005:

$$Y_m = \alpha + X_m'^{2005}\delta + Z_m'^{2005}\mu + \epsilon_m \tag{D.2}$$

where Y_m is the year in which the first immigration enforcement was enacted in MSA m. Z_m^{2005} contains the same control variables as in equation 5. Most importantly, the vector X_m^{2005} is our measure of English proficiency in 2005. Our goal is to evaluate whether English proficiency predicts the adoption of these policies. In the absence of selection effects, the estimates for the coefficient δ should be close to zero and not statistically significant.

Table D.2 presents the results from that exercise. We do not find evidences that English proficiency is correlated with the policies timings, neither when using the overall proficiency score nor when we use an indicator for high English skills; see columns (1) and (2).

We also investigate whether immigration enforcement changes the composition in a certain MSA. If this were the case, we would be worried that our results are driven by a selection effect. To do so, we first investigate whether immigration enforcement changes the ratio of citizens to non-citizens in a given MSA.[10] The results, presented in column (3), do not point toward any evidence that the composition within an MSA is changing in a meaningful way.

To investigate the possibility that families with undocumented members might relocate in response to immigration enforcement, we also estimate our baseline specification restricting the sample to US-born children of likely undocumented parents who did not move over the last year. The results are presented in columns (4) and (5) in table D.2. Restricting our sample to non-movers leave our results virtually unchanged, both in terms

[10] We use yearly MSA-level data on the ratio of naturalized to non-citizen immigrants (long-term immigrants) to evaluate whether immigration enforcement is correlated with the population composition of these MSAs.

of size and magnitude. This gives reassurance that in our analysis we capture the spillover effects of immigration policies on children's human capital.

Table D.2: Robustness Checks: Endogenous Exposure to Interior Immigration Enforcement

	(1)	(2)	(3)	(4)	(5)
	Enactment of Policy		Composition of MSA	Excluding Movers	
	First Year	First Year	Citizens/Non-citizens	Proficiency	High Skills
English Proficiency	-0.1354				
	(0.206)				
High English Skills		0.0441			
		(0.245)			
IE			0.0650	-0.0232*	-0.0294**
			(4.648)	(0.013)	(0.013)
Observations	287	287	3,969	93,822	93,822
R-squared	0.079	0.080	0.459	0.061	0.063
MSA Controls	Yes	Yes	Yes	Yes	Yes
MSA FE			Yes	Yes	Yes
Years FE			Yes	Yes	Yes

Note: Columns (1) and (2) of table D.2 report the estimates from equation (D.2). Columns (4) and (5) report the results for equation (5), restricting the sample to non-movers. Proficiency is a 0-3 categorical variable corresponding to "does not speak English," "speaks English but not well," "speaks well," and "speaks very well." High Skills is a dummy variable 1 if child speaks English very well, and 0 otherwise. The dependent variable in column (3) is the Citizen/Non-citizen Ratio by MSA and year. Immigration Enforcement is measured in $t-1$ in column (3). Standard errors are clustered at the MSA level. *** p<0.01, ** p<0.05, * p<0.1

D.2 Alternative Difference-in-Difference Specification

Recent econometric literature has pointed out that difference-in-difference estimates based on two-way fixed effects models and staggered treatment adoption can be severely biased when treatment effects are heterogeneous (de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2021; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). In this section, we present dynamic estimates which are robust to treatment effect heterogeneity using the approach of Sun and Abraham (2021). Employing a more robust approach in the presence of treatment effect heterogeneity also allows us to gauge whether the documented absence of detectable pre-trends in our analysis is only spurious.

Following Sun and Abraham (2021), we first estimate a dynamic model whereby we fully interact cohort indicators with dynamic effects indicators to recover cohort-specific dynamic effects $\delta^{SA}_{a,c}$.

$$\begin{aligned} y_{i,m,t} =& \alpha^{SA}_m + \sum_{c \in \mathcal{C}} \Bigg[\mathbb{1}(C_m = c) \Bigg(\sum_{\substack{a=-4 \\ a \neq -1}}^{5} \delta^{SA}_{a,c} \mathbb{1}(t - C_m = a) + \delta^{SA}_{-5,c} \mathbb{1}(t - C_m < -4) \\ & + \delta^{SA}_{6,c} \mathbb{1}(t - C_m > 5) \Bigg) \Bigg] + X'_{i,m,t}\Gamma^{SA} + \theta^{SA}_t + \epsilon^{SA}_{i,m,t} \end{aligned} \tag{D.3}$$

where C_m is the time MSA m first enacts any immigration enforcement policy, and $\mathcal{C}$ is the collection of these events over all our cohorts. The indicator function $\mathbb{1}(A)$ takes a value of one if the argument A is true, and zero otherwise. As we did in our event study, we bin observations up to four years prior and more than five years after the treatment date. The estimates $\widehat{\delta}^{SA}_{a,c}$ are consistent estimates for the cohort specific treatment effect, even if treatment effects are heterogeneous.

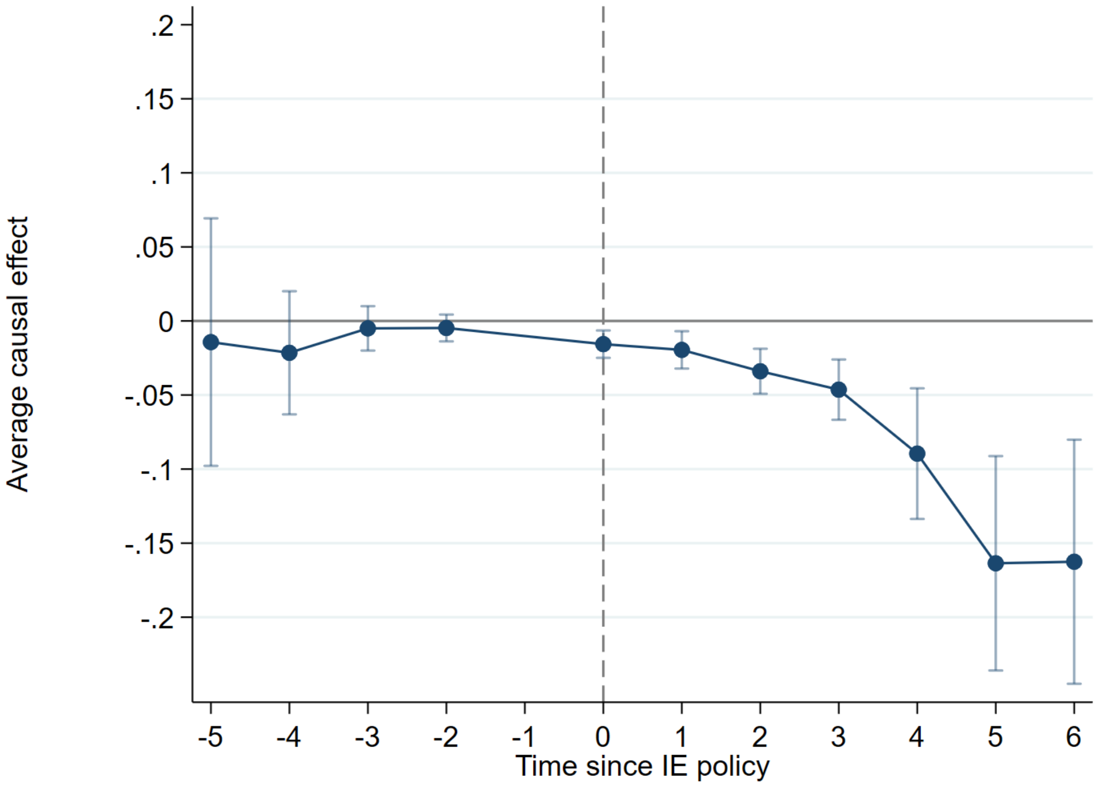
To obtain our robust interaction-weighted estimator $\widehat{\delta}^{SA}_a$ for period a, we weight each cohort specific dynamic estimate $\widehat{\delta}^{SA}_{a,c}$ with its normalized sample share in the respective period and then take the (weighted) average over all cohorts. To have a valid control, and following the suggestion in Sun and Abraham (2021), we do not estimate the treatment effects for the 2014 cohort.[11] The interaction-weighted estimates $\widehat{\delta}^{SA}_a$ for different as and our two outcomes, English language proficiency and high English language ability, are shown in figures D.1 and D.2, respectively.

Two features become apparent when looking at the figures. First, also when applying the robust approach of Sun and Abraham (2021), we do not find evidence for pre-trends in our outcomes. For both English language proficiency and high English language ability, the estimated effects prior to the enactment of any immigration enforcement measure are small and very close to zero.

Second, even when accounting for potential treatment effect heterogeneity, we find a strong and negative impact of immigration enforcement on children's human capital. The interaction-weighted estimates are comparable to those obtained from our event-study design, in terms of both the dynamic pattern and magnitude. Overall, the results obtained from this alternative difference-in-difference specification give us confidence in our identification strategy.

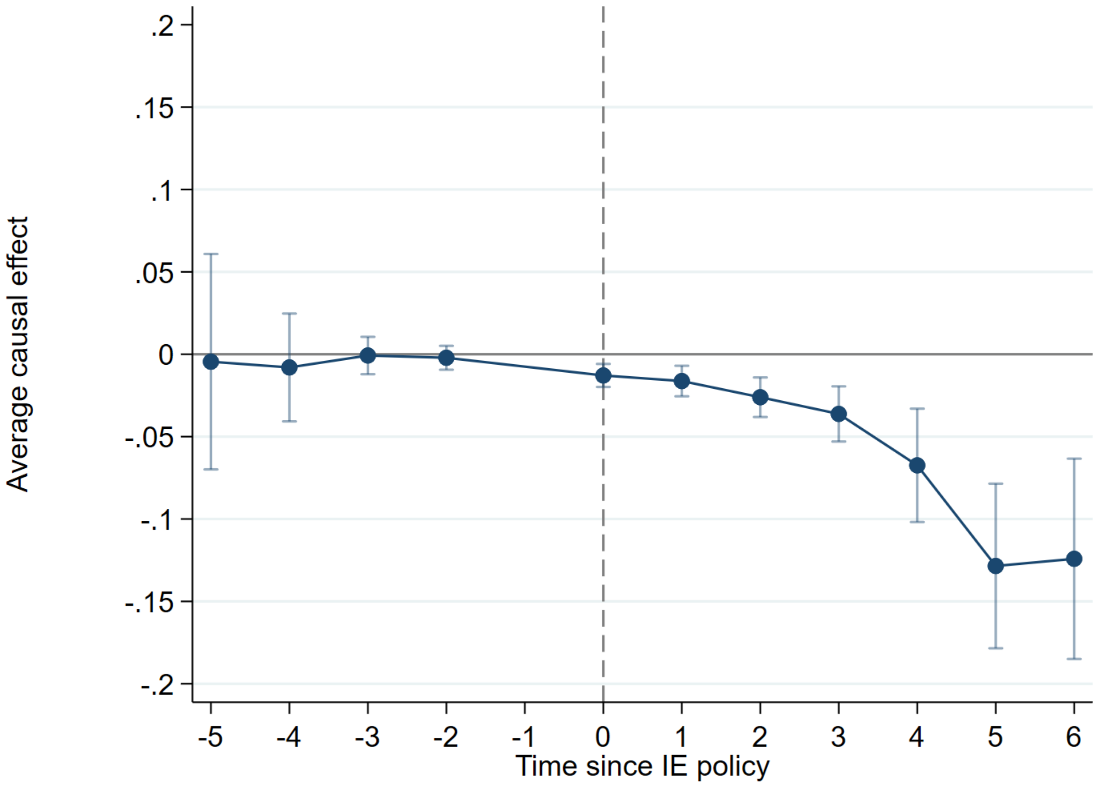
[11] We also drop always treated individuals from the analysis.

Figure D.1: Sun and Abraham (2021) Event Study Coefficient Plot: Measure of English Proficiency



Note: Event-study coefficient plot equation using the approach of Sun and Abraham (2021). Period t represents the first year immigration enforcement policy was enacted in the local area. The outcome variable is a 0-3 categorical variable corresponding to "does not speak English," "speaks English but not well," "speaks well," and "speaks very well."

Figure D.2: Sun and Abraham (2021) Event Study Coefficient Plot: Measure of High English Skills

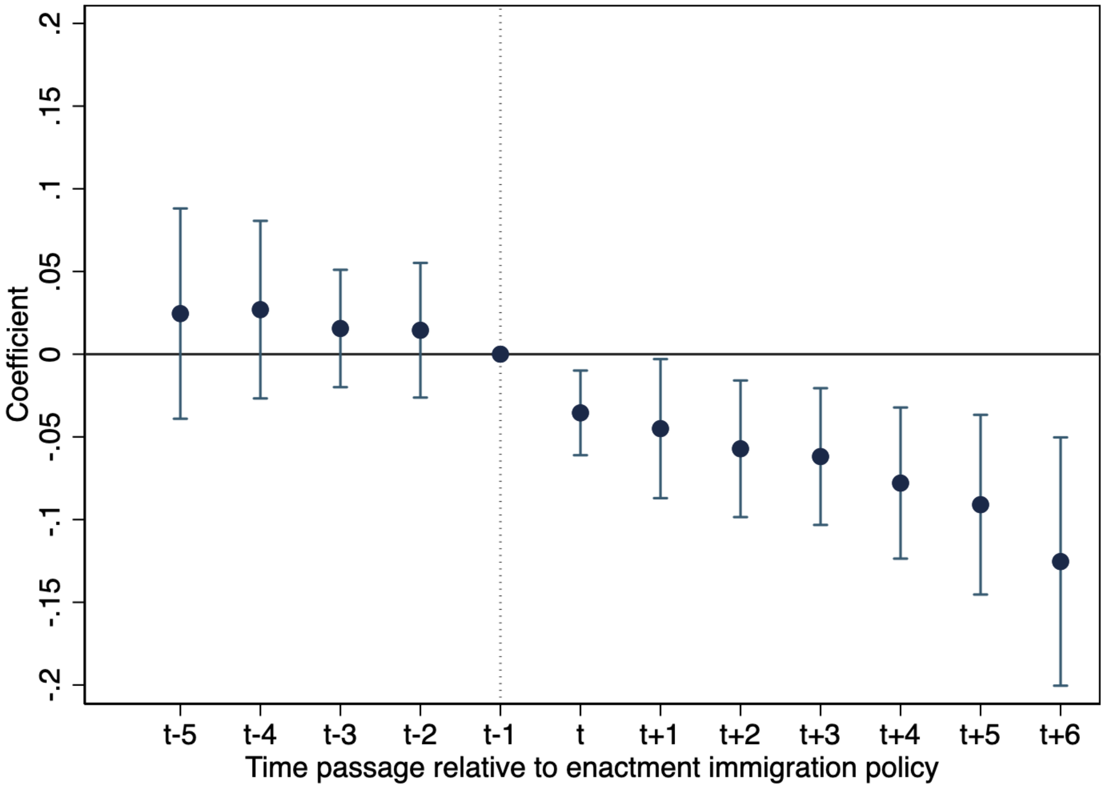


Note: Event-study coefficient plot equation using the approach of Sun and Abraham (2021). Period t represents the first year immigration enforcement policy was enacted in the local area. The outcome variable is a dummy variable 1 if the child speaks English very well, and 0 otherwise.

D.3 Alternative Proxy for Likely Undocumented Immigrants

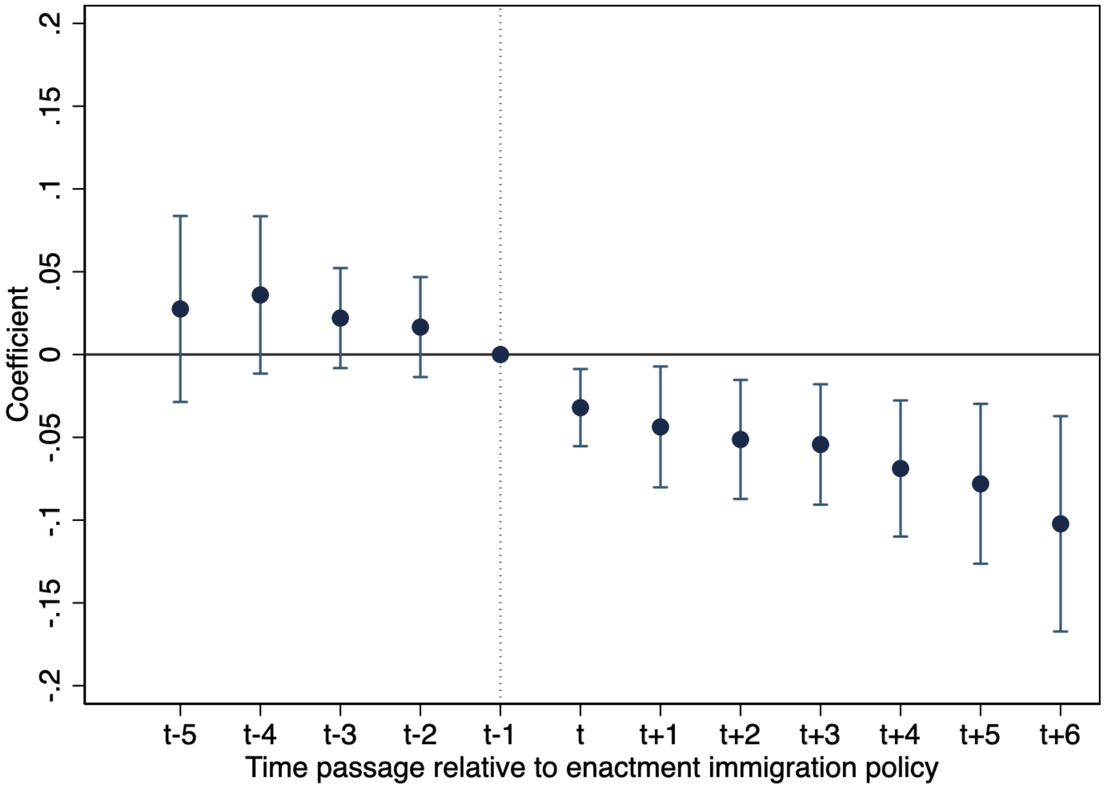
In this section, we show the corresponding event study results underlying the results present in columns (3) and (4) in table 3 when using our alternative proxy for likely undocumented immigrants.

Figure D.3: Event Study Coefficient Plot with Alternative Proxy: Measure of English Proficiency



Note: Event-study coefficient plot equation 6. Period t represents the first year immigration enforcement policy was enacted in the local area following Borjas (2017) residual method. The outcome variable is a 0-3 categorical variable corresponding to "does not speak English," "speaks English but not well," "speaks well," and "speaks very well."

Figure D.4: Event Study Coefficient Plot with Alternative Proxy: Measure of High English Skills



Note: Event-study coefficient plot equation 6 following Borjas (2017) residual method. Period t represents the first year immigration enforcement policy was enacted in the local area. The outcome variable is a dummy variable 1 if the child speaks English very well, and 0 otherwise.

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