Immigration Enforcement and Labor Supply: Hispanic Youth in Mixed-Status Families

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Abstract

This study evaluates the labor supply behavior of US-born Hispanic youth in response to immigration enforcement. Our investigation draws on the added worker effect and underscores immigration enforcement actions as a factor influencing labor supply decisions within immigrant families. We argue that while immigration enforcement induces a decline in labor supply among likely undocumented immigrants, the labor supply among US-born Hispanic youth in mixed-status families increases. Using the Current Population Survey and data on immigration-related arrests between 2014 and 2018, we find that an unexpected surge in arrests increases labor force participation of US-born Hispanic youth by approximately 6 percentage points—a 27 percent increase—and results in higher weekly hours worked by up to 20 percent.

Keywords: Immigration enforcement, youth labor supply, mixed- status households, added worker effect

JEL Classification Numbers: J15, J61

1 Introduction

Over the past two decades, the expansion of local, state, and federal immigration enforcement policies have vastly reshaped the socioeconomic landscape for millions of immigrants in the United States and their families. The implementation of policies such as Secure Communities, 287(g) agreements, employment verification mandates (E-Verify), and omnibus immigration laws have resulted in 2 million arrests and 3.6 million deportations between 2008 and 2018 (ICE, 2015, 2018*b*).[1] These enforcement actions have primarily targeted Hispanic immigrants, with individuals born in Latin American countries accounting for approximately 97 percent of all deportations in recent years (ICE, 2018b). However, it is not only the foreign-born who are impacted by these policies. Between 2014 and 2018, the removal of unauthorized immigrants resulted in the deportation of over 200,000 individuals who claimed to have US-born children, placing an additional 4.4 million citizen children living with at least one unauthorized immigrant parent at risk of family separation (Capps et al., 2020).[2]

Prior studies document various detrimental indirect impacts of enforcement policies on the socioeconomic well-being of US citizens. The risk of detention and deportation of immigrant family members, as well as the surge in interactions between US citizens and immigration authorities (Cantor, Ryo and Humphrey, 2019), have been found to affect US-citizens' labor and education outcomes, poverty, political engagement, and social program participation.[3] However, there is a limit to our understanding of how US citizens living in mixed-status households may strategically respond to protect immigrant relatives from the consequences of intensified immigration enforcement.

With this paper, we begin to fill this gap in the literature by examining whether intensified immigration enforcement impacts the labor supply of citizen youth living in mixed-status families.[4] Our conceptual framework is motivated by the added worker effect, whereby a spell of unemployment experienced by a household member spurs an interdependent labor supply response from another member as an intra-household strategy to smooth income and consumption (Lundberg, 1985). Studies find that

[1]Arrest count obtained from Transactional Records Access Clearinghouse (TRAC), available at https://trac.syr.edu/immigration/reports/529/.

[2]Data obtained from ICE biannual reports to Congress on deported migrants claiming US-born children. See, for example, ICE, 2018a, available at https://www.hsdl.org/?view&did=817380. Last accessed October 2021.

[3]For example, Amuedo-Dorantes and Bansak (2014); Amuedo-Dorantes and Lopez (2017*a*); Amuedo-Dorantes, Arenas-Arroyo and Sevilla (2018); Amuedo-Dorantes and Bucheli (2020); Bohn, Lofstrom and Raphael (2015); Bucheli, Rubalcaba and Vargas (2021); East and Velasquez (Forthcoming); Watson (2014).

[4]Following Bucheli, Rubalcaba and Vargas (2021); Xu, Pirog and Vargas (2016), we designate mixed-status families status when there is at least one US-born child living with at least one non-US citizen parent.

heightened immigration enforcement reduces unauthorized immigrants' labor supply as a strategy to lower the risk of apprehension and deportation—in other words, the "chilling effect" documented throughout the literature (e.g., Amuedo-Dorantes and Bansak, 2014; Bohn, Lofstrom and Raphael, 2015; Orrenius and Zavodny, 2015). We contend that unauthorized immigrant parents in mixed-status families may rely on their US-born children to smooth income during periods of increased immigration enforcement. In sum, we hypothesize that citizen youth living in mixed-status households increase their labor supply to mitigate the chilling effect and potential losses in household income.

We test this hypothesis using individual-level data from the basic monthly Current Population Survey (CPS) merged with immigration-related arrests conducted in the US interior by Immigration and Customs Enforcement (ICE) agents for each month and metropolitan statistical area (MSA) during 2014–2018. We deliberately avoid using variation in the level of ICE arrests within an MSA over time, as this would be an endogenous measure of immigration enforcement. Rather, to capture an exogenous variation in arrests, we construct an indicator variable identifying the months in which the level of arrests exceeds the MSA-specific trend; this allows us to measure unexpected increases in local enforcement actions. Our approach differs notably from most existing studies, which examine the impact of immigration enforcement using the activation of immigration policies as the basis for a quasi-experiment—a strategy that fails to account for the de facto heterogeneity in enforcement intensity (Amuedo-Dorantes and Antman, 2022). In contrast, the shock variable in our study captures unusual surges in immigration enforcement, even when underlying immigration policies remain unchanged.

Our empirical strategy leverages temporal and geographical variation in the arrest shock indicator to estimate the impact of heightened immigration enforcement actions on the labor supply of Hispanic citizen youth living in mixed-status families. We focus on the labor market responses of Hispanic youth to capture the highest risk of arrest exposure given the overwhelming targeting of this demographic group by immigration enforcement. Specifically, we examine the labor supply responses at the extensive (labor force participation) and intensive (weekly hours worked) margins, and we explore whether young women and men respond differently to an arrest shock. Moreover, in supplemental analyses, we document the extent to which non-citizen parents in mixed-status households adjust their labor supply when faced with unusual surges in ICE arrests. Lastly, we begin to explore some of the plausible trade-offs associated with changes in labor supply as measured by education outcomes.

Overall, we find that an unexpected increase in arrests results in higher labor supply among Hispanic citizen youth living with non-citizen parents. Indeed, an arrest shock raises labor force participation by 6 percentage points and labor hours by 15 percent. We find that young women are most responsive to the shock, with an increase in labor force participation of 8 percentage points and a 20 percent increase in labor hours. We demonstrate that our results stem from variation in immigration enforcement actions rather than changes in local economic conditions, by estimating the labor supply "effects" of immigration enforcement on a sample of citizen youth living with citizen parents. In support of our identification strategy, we find no change in the labor supply of these youth who are, in effect, unaffected by changes in immigration enforcement. Additionally, when examining the labor supply responses of non-citizen parents in mixed-status families, we find suggestive evidence of an overall decline in labor supply that is more pronounced for mothers and along the intensive margin, a finding that aligns with prior studies (Amuedo-Dorantes and Antman, 2022).Lastly, we find suggestive evidence that youth in mixed-status households who experience unexpected increases in immigration enforcement are less likely to be enrolled in school and more likely to repeat a grade. Consistent with our labor results, we estimate larger and statistically significant impacts among the subsample of women.[5]

Our contributions to the literature are threefold. First, we provide a unique insight into the unintended consequences of immigration enforcement through the lens of US citizen youth in mixed-status households. The findings emphasize the pervasive nature of the US immigration enforcement strategy and are particularly striking given that citizen youth in immigrant households—one of the fastest growing demographic groups in the United States (Woods and Hanson, 2016)—may be forced to transition into adulthood prematurely. Second, our results highlight the role of intra-household labor supply decisions as a coping and protective mechanism against the detrimental effects of immigration enforcement actions on the financial well-being of immigrant households. Lastly, while our work is specific to the context of immigration enforcement, the labor supply responses we document may not be too different from households facing unexpected employment losses and considerable financial constraints more broadly. We view our work as motivation to expand the added worker effect framework beyond the conventional framing that excludes working-age children.

[5]Unlike the labor market effects, which appear to be contemporaneous to the shock in immigration enforcement, the impacts on educational outcomes are only economically and statistically significant when considering a cumulative exposure to immigration enforcement.

2 Review of Related Literature

Prior studies have documented the detrimental effects of restrictive immigration policies on the well-being of immigrants and their families. Research demonstrates that the implementation of policies such as Secure Communities, 287(g) agreements, employment verification mandates (E-Verify), and omnibus immigration laws has meaningfully impacted immigrants' employment, wages, labor supply, occupational choices, and even commute times (e.g., Amuedo-Dorantes and Bansak, 2014; Kostandini, Mykerezi and Escalante, 2014; Bohn, Lofstrom and Raphael, 2015; Orrenius and Zavodny, 2015; Amuedo-Dorantes, Arenas-Arroyo and Sevilla, 2020). In this section, we briefly review the literature studying the impact of immigration enforcement on the labor supply of immigrants.[6]

Along the extensive margin, research shows that immigration enforcement stifles labor force participation and the likelihood of employment. In one study, Amuedo-Dorantes and Bansak (2014) leverage the variation in state-level implementation of E-Verify to examine the relationship between immigration enforcement and employment. The analysis estimates that universal and public sector mandates requiring work eligibility verification lowered the probability of employment among likely unauthorized immigrants by 2.4 to 4.6 percentage points between 2004 and 2011. Similarly, when looking at the direct impact of deportations, Amuedo-Dorantes and Antman (2022) estimate that an additional 10 removals in an MSA lowers the likelihood of employment among low-skilled Hispanic immigrants by approximately one percentage point relative to naturalized citizens.[7] Research on the activation of the Secure Communities program has also been shown to induce a similar reduction in employment—a chilling effect triggered by the increased risk of apprehension, detention, and deportation among unauthorized immigrants (East et al., 2021; East and Velasquez, Forthcoming). Alternatively, relaxing restrictive immigration policies (e.g., granting driving licenses to undocumented workers) increases their likelihood to work

[6] A related strand in the literature studies the impact of immigration enforcement on naturalized and native workers. For instance, Amuedo-Dorantes and Bansak (2014) find that employment verification mandates may have increased employment among non-Hispanic native workers, potentially offsetting the negative effect on likely undocumented workers. Orrenius and Zavodny (2015) find that the adoption of these mandates raised the employment of naturalized male Mexican immigrants and the earnings of US-born Hispanic men. In another study, East and Velasquez (Forthcoming) observe that the activation of Secure Communities had a lasting negative effect on the labor supply of working-age, college-educated US-born women with children under the age of three, as heightened immigration enforcement lowered the number of hours worked among likely undocumented immigrants providing household services.

[7] Further narrowing the population of interest to likely unauthorized Mexican immigrants produces no statistically significant results (Orrenius and Zavodny, 2015).

(Amuedo-Dorantes, Arenas-Arroyo and Sevilla, 2020).[8] Overall, these findings are consistent with research on immigrants' reluctance to interact with authorities or public programs as a consequence of heightened enforcement actions.[9]

Along the intensive margin, research shows that intensified immigration enforcement influences immigrants' wages and hours worked, but the direction of this effect is not always consistent. For instance, Amuedo-Dorantes and Bansak (2014) find that the adoption of universal employment verification mandates increases wages among likely unauthorized immigrant women, suggesting that their labor supply is highly responsive to the mandates. In contrast, Orrenius and Zavodny (2015), who consider a longer time period but narrow the analysis to unauthorized immigrants from *Mexico*, reveal no statistically significant effects of E-Verify on hourly wages among women. However, the authors find a negative impact of E-verify on the wages of likely unauthorized immigrant men who have resided in the United States for more than 10 years. These findings are consistent with Gentsch and Massey (2011), who find that increased immigration enforcement efforts facilitated by the 1996 Illegal Immigration Reform and Immigrant Responsibility Act (IIRAIRA) reduced the wage returns for US experience and English language proficiency among Mexican migrants.

Evidence of positive or null effects along the intensive margin appears to be at least partly driven by the adoption of employment-based enforcement measures. In contrast, studies that examine police-based policies consistently find negative impacts of enforcement actions on labor outcomes. For instance, when exploring the activation of Secure Communities, East et al. (2021) find that the average hourly wage among immigrants with low education—a characteristic that proxies for unauthorized immigration status—declined by 1.7 percent. In a similar study, Amuedo-Dorantes and Antman (2022) use deportations as a measure of enforcement and find that an increase in deportations lowered the hourly wages of non-citizen foreign-born Hispanics by 1.9 percent relative to naturalized Hispanics.

Overall, evidence from the literature highlights the detrimental impact of immigration enforcement on labor market outcomes among Hispanic immigrants. Chilling effects resulting from stricter enforcement

[8] There is also a related literature studying the impact of temporary protection from deportation and legal work permits through the Deferred Action for Childhood Arrivals (DACA). Studies show that DACA eligibility results in improved labor market outcomes at the extensive and intensive margins by increasing labor force participation (Pope, 2016), the likelihood of employment (Pope, 2016; Amuedo-Dorantes and Antman, 2017), weekly hours worked (Pope, 2016), and income among those at the bottom of the income distribution (Pope, 2016).

[9] This literature finds that increased immigration enforcement is accompanied by, for example, a decrease in Medicaid enrollment (Watson, 2014; Vargas, 2015), food and cash assistance programs (Alsan and Yang, 2018; Vargas and Pirog, 2016), and the likelihood of reporting domestic violence to the police (Muchow and Amuedo-Dorantes, 2020).

policies impose non-pecuniary costs on immigrants, who see their employment likelihood and average wages decrease. These findings imply that immigrant-origin households, including those with US-citizen children, likely experience income shocks as a consequence of intensified enforcement. Yet our understanding of these households' income smoothing strategies is surprisingly limited.

3 Conceptual Framework

From a conceptual standpoint, we argue that immigration enforcement affects the labor market outcomes of citizen youth living with non-citizen parents through the added worker effect. This framework posits that spells of unemployment of a household member can trigger a positive labor supply response among unaffected household members as a mechanism for consumption smoothing and insurance against income losses (Lundberg, 1985; Bredtmann, Otten and Rulff, 2018).[10] The life cycle model of family labor supply predicts that the magnitude of the added worker effect depends on whether the unemployment spell is anticipated, and the extent to which alternative mitigation strategies, such as borrowing or the use of savings, are viable responses (Stephens, Jr., 2002). Thus, the added worker effect is predicted to be substantial in the presence of unforeseen negative employment shocks, credit constraints, and lack of savings, such as in the context of the unauthorized immigrant population.

While the added worker effect literature has largely focused on wives' labor supply adjustments in response to their husbands' unemployment spells,[11] we reconsider this household dynamic within the context of mixed-status households. As discussed above, the intensification of immigration enforcement often forces unauthorized immigrants to withdraw from the labor market in response to the increased risk of apprehension, detention, and deportation—this labor supply change is often temporary (except in the case of deportation, where exiting the labor market is usually permanent). Furthermore, unauthorized immigration status presents additional barriers to accessing alternative mitigating strategies such as unemployment insurance or borrowing through formal credit markets. Therefore, in the context of mixed-status households, we hypothesize that the transitory (or permanent) labor market withdrawal of an unauthorized immigrant parent could trigger an increase in the labor supply of their working-age citizen children, given that they face a significantly lower risk from immigration authorities. Moreover, we predict

[10] Prior studies on the added worker effect in different settings have found limited evidence of intra-household labor supply responses to a member's displacement or wage loss (e.g, Ayhan, 2018; Hardoy and Schøne, 2014; Halla, Schmieder and Weber, 2020).

[11] Stephens, Jr. (2002) even defines the added worker effect as the "labor supply response of wives to their husbands' job losses."

a sizable effect considering that we systematically define the shock in immigration enforcement to be an unexpected event for which mixed-status families are unable to prepare.

4 Data

4.1 Measuring Immigration Enforcement

We obtain data on ICE arrests conducted between October 2014 and May 2018 from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University.[12] These data report the number of ICE arrests within the US interior at the month-by-county level, totaling 480,000 apprehensions over the 44 months observed in our study.[13] We crosswalk county-level ICE arrests to their respective metropolitan statistical area (MSA) to facilitate merging these data with the CPS public use files. Thus, we capture immigration enforcement actions at the MSA-by-month level.

Figure 1 illustrates the geographical distribution of ICE arrests across MSAs for our entire study period. Not surprisingly, apprehensions are concentrated in MSAs that have traditionally hosted larger immigrant populations, such as those in southern California, Houston, and the Boston-Washington, DC, corridor. This positive correlation between immigration enforcement actions and the size of the immigrant population poses a challenge in identifying the causal effect of ICE arrests on the labor market outcomes of citizen youth and their immigrant parents. We avoid the use of temporal variation in the number of apprehensions across MSAs as our measure of immigration enforcement because it confounds MSA-specific characteristics that are systematically correlated with our outcome variable. Instead, we leverage MSA-specific time variation in ICE arrests to identify months of unusual enforcement intensity measured as large deviations above the local trend.

We reason that an individual's expectation about the levels of immigration enforcement, as measured by apprehensions, are dynamically established over short periods of time. And, within the bounds of this expectation, individuals begin to habituate, leading to decreased responsiveness given patterns of apprehensions experienced in previous months (Groves and Thompson, 1970; McSweeney and Swindell,

[12]The period between 9/11 and 2013 saw the largest intensification in immigration enforcement across the country, mainly through the widespread adoption of employment verification mandates, omnibus immigration laws, 287(g) agreements, and Secure Communities. However, after 2013, the major changes in immigration enforcement came from reprioritization efforts (e.g., the Priority Enforcement Program, PEP) and the expansion of existing tactics (e.g., workplace raids). Our focus on the post-2013 period captures unexpected variations in enforcement intensity rather than in the activation of enforcement policies.

[13]The data exclude border apprehensions conducted by Customs and Border Patrol.

1999; Wathieu, 2004; Blumstein, 2016). However, we anticipate individuals to be highly responsive to variations in apprehensions that exceed expectations by a certain threshold. We capture this process by constructing a framework for expectation formation using an unweighted moving average consistent with the adaptive expectation hypothesis (Lucas and Sargent, 1981; Wallis, 1980; Hatchett, Brorsen and Anderson, 2010; Lee and Brorsen, 2017*a*,*b*). First, using the number of arrests, $A_{m,t}$, in MSA m at time t, we calculate the moving average ($\mu_{m,k=6}$) and the moving standard deviation ($\sigma_{m,k=6}$),[14] over the preceding six-month period ($k = 6$).[15] Second, we standardize the time and MSA-specific arrest ($A_{m,t}$) as $Z_{m,t} = \frac{A_{m,t} - \mu_{m,k=6}}{\sigma_{m,k=6}}$ and construct the shock indicator variable, $S_{m,t}$, using the following criteria:

$$S_{m,t} = \begin{cases} 0, & \text{if } Z_{m,t} < 1; \ \textit{No shock} \\ 1, & \text{if } Z_{m,t} \geq 1; \ \textit{Shock: increase in arrests.} \end{cases} \tag{1}$$

That is, the shock variable turns on when the number of arrests in MSA m at time t increases by one standard deviation above its six-month moving average. In total, we capture 1,130 ICE arrest shocks over the sample period.

Table 1 presents the number of shocks for select MSAs, as well as the number of arrests and rate of arrests per 1,000 foreign-born individuals for comparison.[16] Panel A lists the top 10 MSAs by number of shocks, while panel B lists the top 10 MSAs by number of arrests. We include panel B to emphasize the nature of the shock variable relative to the level of arrests. Notably, there is considerable heterogeneity in arrest measures among the top 10 MSAs shown in panel A. Using the arrest shock variable to identify MSAs with unusual surges in arrests, we capture localities in both traditional and non-traditional immigrant states—for example, El Centro, California, with an arrest rate of 36 per 1,000 foreign-born residents, and

[14]The simple moving average is calculated as: $\mu_{m,k} = \frac{1}{k}\sum_{i=t-k+1}^{t} A_{m,i}$. The moving standard deviation is calculated as: $\sigma_{m,k} = \sqrt{\frac{\sum_{i=t-k+1}^{t}(A_{m,i} - \mu_{m,k})}{k-1}}$.

[15]We characterize expectations about immigration enforcement using this approach, given that it relies squarely on past experiences with enforcement actions in an environment where information about enforcement strategies and priorities are asymmetric.

[16]To calculate the rate of arrests, we used the period and MSA-specific levels of arrests while maintaining the populations of foreign-born individuals constant at its 2014 level.

Ames, Iowa, with an arrest rate of 3. In contrast, MSAs listed in panel B tend to be in more traditional immigrant destinations with high intensity in immigration enforcement.[17]

The shock variable relies on two predetermined parameters. The first is the length of time used to calculate the moving average, conceptualized here as the duration in which expectations about immigration enforcement are established. The second is the magnitude of the threshold used to determine a shock. To the best of our knowledge, no studies provide insight into selecting suitable shock parameters. Thus, we examine the sensitivity of our main results to variations in both parameters as a robustness check (see appendix). In either case, we believe that deviations from the expected value above the threshold, regardless of how expectations are formed or the model that is used, are unpredictable by the agent an thus constitute an exogenous shock.

4.2 Monthly CPS

We use the 2014–2018 basic monthly Current Population Surveys (CPS) to gather individual-level data on employment and labor hours, as well as demographic information such as age and ethnicity. The analysis sample is restricted to US-born youth ages 16 to 18 who were surveyed during school months (August–May) and lived in the contiguous United States. We impose the lower bound age restriction to account for the fact that child labor laws, such as the Fair Labor Standards Act (FLSA), limit the number of hours minors under the age of 16 can work. The upper bound allows us to focus on school-aged youth such that the trade-offs associated with labor market activity are most comparable across individuals. Ideally, we would like to measure youths' transition into the labor force to examine whether immigration enforcement results in new entrants in this market. Since we are unable to construct such a variable, we limit our sample to survey participants in non-summer months to increase the likelihood that we observe new transitions into the labor market.[18]

The labor supply indicators central to our analysis are labor force participation and hours worked. The labor force participation variable is collected in the CPS as a direct measure of employment status and is constructed as a dichotomous variable in our study. The hours worked variable used in our analysis is

[17]To further illustrate the nature of the arrest shock variable, figure A1 shows the trend in ICE arrests for four representative MSAs. Panel A corresponds to the top two MSAs with the highest the number of arrests. Panel B corresponds to the top two MSAs with the highest number of shocks. Each illustrated data point reflects the number of arrests in the respective MSA and period (month and year). The red crosses indicate when the monthly number of arrests exceeded the six-month moving average by 1 s.d. ($S_{m,t} = 1$). Also, for context, we distinguish between the Obama and Trump administrations by the faint gray shading.

[18]The main results are robust to including summer months.

constructed from the total number of hours worked "last week" and is not conditional on employment status. This approach captures changes to labor market activity overall, without selection on employment.

Although the CPS does not report detailed immigration status, it contains respondents' country of birth and US citizenship. Using this information and family identifiers, we designate youth in mixed-status families as those born in the United States living with at least one non-citizen parent.[19] This definition excludes cases in which US-born youth have suffered the deportation of their non-citizen parent but stayed in the United States with a citizen parent or relative.

Table 2 presents the summary statistics from the CPS across Hispanic ethnicity and mixed-status families, as well as a pooled sample. The "US citizen parent(s)" category represents families where both parents, or the only parent in a single-parent family, reported US citizenship. We observe that the labor force participation rate for the pooled sample is approximately 28 percent, with Hispanics exhibiting a participation rate of 23 percent and non-Hispanics in citizen households exhibiting a somewhat higher participation rate at 30 percent. In terms of hours worked, table 2 indicates that the average US-citizen youth in our sample worked for 4.3 hours in the previous week, with Hispanics working an between of 3.9 and 4.1 hours and non-Hispanics between 3.6 and 4.3 hours, depending on their family's mixed-status situation.

5 Empirical Strategy

We evaluate the impact of immigration-related arrests on Hispanic youth's labor supply by estimating the following regression model via ordinary least squares (OLS):

$$\begin{aligned} y_{imt} = {} & \beta_1 S_{mt} + \beta_2 H_i + \beta_3 M_i + \beta_4 (S_{mt} \times H_i) + \beta_5 (S_{mt} \times M_i) + \beta_6 (H_i \times M_i) \\ & + \beta_7 (S_{mt} \times H_i \times M_i) + \gamma A_{mt} + X'_{imt} \Gamma + \theta_m + \theta_t + \theta_{st} + \mu_{imt}, \end{aligned} \tag{2}$$

where y_{imt} stands for either labor force participation or log hours worked for individual i in MSA m at time t.[20] S_{mt} is an indicator variable that identifies whether there was an ICE arrest shock in MSA m at time t. H_i indicates Hispanic ethnicity for respondent i, and M_i indicates whether the same respondent lived in a

[19] We intentionally avoid using country of birth as a marker for US citizenship as it also includes immigrant naturalized citizens.

[20] We also experiment with an inverse hyperbolic sine transformation of the number of work hours and verify the consistency of the results.

mixed-status household. Our parameter of interest, β_7, corresponds to the three-way interaction between S_{mt}, H_i, and M_i, and it estimates the effect of a shock to ICE arrests on the labor supply of Hispanic youth in mixed-status families. The vector X_{imt} includes both individual and household characteristics. The individual characteristics consist of age, gender, race, number of siblings, and an eldest sibling indicator. The household characteristics include an indicator for single parent households and an indicator for parental high school completion. A_{mt} is a continuous variable capturing the rate of ICE arrests per 1,000 foreign-born individuals, which accounts for the contemporaneous level of immigration enforcement.

The model also includes a set of MSA (θ_m), month-year (θ_t), and state-year (θ_{st}) fixed effects to control for unobserved factors that can possibly drive youth labor market outcomes.[21] The MSA fixed effects account for time-invariant MSA-specific characteristics (e.g., local attitudes and policies toward immigrants that can drive demand for immigration enforcement actions). The state-year fixed effects control for state-specific time-varying characteristics, such as the minimum working age, minimum wages, and immigration-related policies. Month-year fixed effects account for aggregate seasonal economic shocks (e.g., business cycle fluctuations). Lastly, we use individual-level sampling weights specific to the basic monthly CPS and cluster the standard errors at the MSA level.

5.1 Identification Checks

The objective of our empirical strategy is to identify the causal effect of immigration enforcement on the labor supply of US-born adolescent youth. To do this, we leverage geographic and temporal variation in the sudden increase in immigration arrests conducted by ICE agents across MSAs and months. In order to credibly estimate a causal impact, our empirical strategy requires three identifying assumptions. First, the arrest shock variable must identify increases in ICE arrests exogenous to unobserved factors that may influence changes in both immigration enforcement and youth labor supply. Second, the labor supply of Hispanic youth in mixed-status families should not drive the occurrence of immigration enforcement shocks. And third, youth living in non-mixed-status households (i.e., the comparison group) must be

[21] We also run alternative specifications where we control for state- and MSA-specific linear time trends. See table 11 in the appendix for results following these specifications.

unaffected by an unexpected increase in ICE arrests.[22] Below we examine each of these assumptions and provide suggestive evidence that supports their validity.

We begin considering threats to identification that emerge from potential reverse causality and omitted variable bias. These assumptions would be violated if, for instance, sudden changes in immigration enforcement efforts are driven by local economic conditions that also impact youth labor supply. For example, local economic expansion may encourage youth to enter the labor market while simultaneously increasing local tax revenue, which can be allocated toward higher immigration enforcement in the area. Similarly, an increase in Hispanic youth labor supply may signal the presence of immigrant labor in a community, resulting in heightened immigration enforcement efforts.

To evaluate the exogeneity of the arrest shock indicator, we examine its correlation with various factors that potentially affect the Hispanic youth labor supply and sudden surges in the number of ICE arrests. Specifically, we regress the number of annual ICE arrest shocks during our study period on a host of MSA-specific characteristics using data from the American Community Survey and the Bureau of Labor Statistics. Table 3 presents the results from this exercise for i) general MSA characteristics, such as the share of foreign-born population and the distance to the US-Mexico border; ii) economic characteristics, including unemployment and poverty rates; and iii) industry location quotients. The latter address the possibility that MSAs with a relatively higher concentration of labor-intensive jobs, such as in the construction sector, attract younger low-wage workers and immigrants, making it more likely that immigration authorities intensify their local enforcement efforts.

As seen in column 1 of table 3, most point estimates are close to zero and not statistically significant, including for variables like the share of the non-citizen population, distance to the US-Mexico border, the Hispanic share in the labor force, and youth labor force participation. The only variables that appear to be significantly correlated with the number of annual shocks are measures of poverty and the concentration of employment within the natural resources industry. While statistically significant, we note that the negative coefficients on these variables suggest that poorer and more agricultural areas experience fewer arrest shocks, suggesting a downward bias in our estimates.[23] Nonetheless, we mitigate concerns regarding

[22]Our analysis sample is restricted to US-born youth; therefore, non-mixed-status households include those where all members (parents and children) are US citizens.

[23]We also experiment with replacing the outcome of interest with the arrests rate per 1,000 foreign-born individuals to verify that our use of a shock is a more exogenous measure of variation in immigration arrests. As seen in column 2 of table 3, unlike the shock variable, the rate of arrests is correlated with several characteristics, including the labor force participation rate of adolescent youth.

potential confounders by including MSA and state-year fixed effects in our empirical specifications. Our results are also robust to controlling for MSA-specific time trends.[24]

Lastly, we verify that our comparison group—citizen youth living in citizen households—do not respond to a change in ICE arrests. We accomplish this by conducting a falsification exercise where we estimate variants of equation 2 limiting the sample to US-born youth living with US-born parents. We find small and insignificant effects, suggesting that our definition of the "treated" group correctly identifies the population for whom immigration enforcement actions are salient. A robust description of the results from this exercise can be found in section 7.1.

6 Main Results

6.1 ICE Arrests and Hispanic Youth Labor Supply

We begin by estimating the labor supply response among US-born Hispanic youth to shocks in immigration arrests. Table 4, column 1 presents the results for labor force participation using the full sample. Columns 2 and 3 show estimates for the split samples of young men and young women, respectively. In line with our hypothesis, we find that, on average, shocks to immigration arrests increase the labor force participation of US-born Hispanic youth by 6.2 percentage points—a 27 percent increase relative to the sample mean.[25] Across sexes, the evidence suggests shocks to enforcement disproportionately impact young women. We find an increase of 8 percentage points (33 percent) in labor force participation among US-born Hispanic women. This represents a substantial increase in labor supply at the extensive margin, comparable to the added worker effect estimates for women whose spouses experience an adverse employment shock (Ayhan, 2018; Bredtmann, Otten and Rulff, 2018). The point estimate for the split sample specific to young men is positive, albeit not statistically significant.

Next, we estimate the model for labor hours. As shown in table 4 column 4, we find a 15 percent increase in hours worked in the previous week in response to a shock in immigration arrests. Consistent with our previous results, we observe a larger impact among young women, for whom we estimate a 20 percent increase in hours worked. Based on the average number of hours worked per week presented in table 2, this represents a 0.8 increase in the weekly number of hours worked. Given that our model explains hours

[24] See table 11 for reference.

[25] In separate regressions by age cohort, we find larger effects among 16-year-olds and positive, although imprecise, estimates for 17- and 18-year-olds. Results from these regressions are available upon request.

worked not conditional on employment, we interpret these estimates as a change in overall labor market activity among US-born Hispanic youth in mixed-status families.[26]

6.2 Exploration of Mechanisms

We have established that sudden and large increases in immigration-related arrests result in higher labor supply among Hispanic citizen youth living with non-citizen parents. This provides evidence in support of our hypothesis that these youth increase their labor force participation and labor hours as part of an intra-household strategy to mitigate the negative income effects of immigration enforcement. In this section, we directly assess the connection between parental labor supply decisions and immigration enforcement within the context of the mixed-status households.

In table 5 we present the results from our evaluation of labor supply among parents of the citizen youth observed in our sample. Given that there may have been multiple children within the same household, we further restrict the data to observe each parent only once. We analyze labor force participation and hours worked independently for each sub-sample of mothers, fathers, and the parent recorded as the head of the family. In panel A, the results reflect the models estimated using the identification strategy applied in our main analysis. We observe that the labor supply of immigrant mothers with citizen children was negatively affected by unexpected surges in immigration arrests. Their likelihood of labor force participation dropped by 5 percent while their number of hours worked decreased by 20 percent—a reversed, yet comparable effect in magnitude to the one observed among youth in table 4. These results are in line with the immigration enforcement literature, which often finds that the impact of these policies is more pervasive among women (e.g., Amuedo-Dorantes and Antman, 2022).

The results presented in table 5, panel B, reflect the analysis where the sample is restricted to households with only US citizen parents as an additional falsification test that focuses on parents that should not be affected by immigration enforcement. As can be seen, citizen parents do not appear to be affected by shocks to immigration arrests, providing further evidence that mixed-status families with non-citizen parents drive our results.

[26] We also estimate the effect of immigration arrest shocks on labor hours, conditional on employment. The point estimates are positive and of comparable magnitude to the ones presented in table 4.

6.3 Discussion of Related Educational Outcomes

A priori, the educational consequences for what may be early entries into the labor force or an increase in hours worked are not clear.[27] To develop a deeper understanding of the implications we might draw from our primary findings, we conduct supplemental analyses, detailed in the appendix, where we explore the relationship between immigration enforcement and school enrollment, grade retention, and drop out rates using the basic monthly CPS as well as the education supplement of the October CPS.

The results discussed in appendix B suggest that educational outcomes, specifically enrollment, are not responsive to shocks in immigration enforcement occurring contemporaneously. However, there is evidence that the probability of enrollment decreases two months after the shock—a finding distinctly significant among women. We speculate that enrollment is less likely to respond to contemporaneous shocks in immigration enforcement, given that the decision to disenroll may be considered after many absences, missed assignments, and disengagement.[28] This assertion is supported by our analysis of grade retention and drop out rates in the CPS education supplement. We find that shocks aggregated in the spring semester have an impact on the probability of grade retention in the following fall semester but not on dropping out.

The outcomes we consider here are costly decisions, which we speculate are realized as the culmination of cumulative shocks rather than a contemporaneous response. While the evidence in table ?? can only be taken as a correlational relationship, it hints at the impact on labor supply, along the intensive and extensive margins, preceding the impact on enrollment. However, we caution against interpreting these results as a direct consequence of the labor supply, as there may be several other channels through which heightened immigration enforcement can impact educational outcomes.[29] Rather, we view these as indicative of the trade-offs that youth may confront.

[27] Studies examining the impact of immigration enforcement policies and actions on educational outcomes have documented that an increase in immigration enforcement reduces school attendance and enrollment while increasing grade retention (e.g., Amuedo-Dorantes and Lopez, 2017*b*; Bucheli, Rubalcaba and Vargas, 2021; Bellows, 2019, 2021; Meadows, 2021; Kirksey and Sattin-Bajaj, 2021). On the other hand, studies suggest that paid youth employment influences educational outcomes differentially according to the number of hours worked (Hwang and Domina, 2017; Rothstein, 2001; Keister and Hall, 2010) and the timing of employment with respect to grade and school months (Modestino and Paulsen, 2015; Buscha et al., 2008).

[28] See, for example, Bellows (2019, 2021); Meadows (2021); Kirksey and Sattin-Bajaj (2021).

[29] For example, when a parent is apprehended, the increased responsibility placed on US-born children in mixed-status families may become a long-term arrangement (Dreby, 2012), leading to a breakdown of competing commitments such as education.

6.4 Interpretation and Discussion of Main Findings

Putting together our results, we find evidence that mixed-status households resort to strategic intra-household labor supply decisions to protect their families against the negative impacts of heightened immigration enforcement actions. At the extensive margin, we document that an unexpected surge in immigration-related arrests results in both lower labor force participation among non-citizen parents of up to 5.2 percentage points, who face a direct risk of apprehension and deportation, and an increase in the labor force participation of up to 7.9 percentage points among citizen youth living with non-citizen parents. Notably, both effects are driven by women in the sample for whom the changes in labor supply are large and statistically significant.

To contextualize the magnitude of these findings, work by Taylor et al. (2011) and Capps, Fix and Zong (2016) has estimated that there are approximately 5 million US-born children under the age of 18 with at least one unauthorized immigrant parent. Using the sample survey weights, the total population represented by our sample selection suggests there are approximately 800,000 US-born Hispanics ages 16 to 18 living in mixed-status families, of whom 420,000 are women.[30] Within this context, our estimates in table 4 suggest that approximately 50,000 (33,000) US-born Hispanic youth (women) between ages 16 and 18 either persisted or entered the labor force during school months as a result of the subsequent shocks to US immigration enforcement between 2014 and 2018.

At the intensive margin, we find evidence that an increase in immigration-related arrests raises the number of hours worked among citizen youth in mixed-status households by up to 20 percent while also resulting in an analogous 20 percent decrease among non-citizen parents, again driven by changes in the corresponding female sub-groups. By limiting our sample to school-aged youth and survey responses in non-summer months, we homogenize the trade-offs associated with changes in labor supply to reflect school-related activities as plausible alternatives for time use. As such, our estimates suggest that young women increase their weekly labor hours by 0.8 hours, approximately 10 percent of the time teenagers spend on homework every week (Livingston, 2019). In supplemental analyses, we provide suggestive evidence that immigration enforcement actions are related to an increased likelihood of repeating a grade and a lower probability of school enrollment, both significantly concentrated among young women.

[30]While the CPS does not distinguish authorized and unauthorized immigration status, segments of the population estimated by Taylor et al. (2011) and Capps, Fix and Zong (2016) are encompassed in our data. The methods used to estimate these populations come from the same data sets used in our analysis. See the methodological description in Taylor et al. (2011) and Capps, Fix and Zong (2016).

Consistent with recent literature, our results indicate that women tend to be more affected by the intensification of immigration enforcement than men—young citizen women in mixed-status households enter the labor market and work longer hours, with corresponding opposite responses by non-citizen mothers. Relatedly, Amuedo-Dorantes and Antman (2022) find that increases in ICE deportations are associated with declines in the labor force participation and employment of likely unauthorized immigrants, particularly women with children. In a similar way, East and Velasquez (Forthcoming) observe that the implementation of Secure Communities caused likely unauthorized immigrant women employed in household services to reduce their number of hours worked. Speculating on these results and drawing on the fact that labor markets are typically segmented according to sex, we posit that households may be employing sex-specific labor substitution in response to heightened immigration enforcement.

7 Robustness Checks

We conclude by estimating several additional analyses to further validate the causal interpretation of our main findings. First, we conduct a placebo test where we estimate the "effect" of a shock in ICE arrests on the labor supply of citizen youth living with citizen parents, who are, in principle, unaffected by changes in immigration enforcement actions. Second, evaluate the impact of arrests on the labor supply of non-citizen youth. Third, we explore whether there is evidence of anticipation effects. Lastly, we discuss the findings of a placebo exercise where we randomize the occurrence of a shock across MSAs and time.

7.1 Effects on Youth Living in Non-Mixed-Status Households

Our identification strategy requires that shocks to immigration arrests only impact the labor supply of US-born youth living in mixed-status families where at least one parent was not born in the United States. This condition implies that the sudden increase in immigration arrests should not affect citizen families. As a falsification test of these assumptions, we proceed by estimating our main specification for both labor force participation and hours worked using the sample of US-born youth with US-born parents—the sample that should not be affected by the shock.

Table 6 presents the results from the interaction between the arrest shock and Hispanic ethnicity indicators estimated with our comparison sample. Columns 1–3 show results for labor force participation within the pooled sample and stratified by sex. Columns 4–6 show results for labor hours. As expected, we do not find evidence that a shock to immigration arrest changes the labor supply of youth living in citizen households. The point estimates are close to zero and not statistically significant at conventional levels. These results suggest that our variable of interest captures changes in local labor market conditions that only affect those who are targeted by immigration enforcement actions. In other words, the shock does not proxy for an omitted factor; otherwise, we would observe an "impact" on the labor supply of youth whose families are, in principle, never treated. Lastly, these results provide suggestive evidence of little to no change in overall youth labor demand as a result of the increase in arrests, thus implying that the estimated increase in labor supply among Hispanic youth is likely a response to an adverse income shock within the household. We explore this mechanism directly in the following section.

Finally, we verify whether the impact of ICE arrests on labor supply is unique to US-born adolescent youth. Given that non-citizen children face the same limitation as their non-citizen parents, it is expected that the labor supply of non-citizen adolescent youth will remain unaffected or potentially decrease during periods of intensified immigration enforcement. To investigate this, we evaluate equation 2 using a sample of non-citizen youth and find that shocks in ICE arrests induced a reduction to labor supply among this group (table 7). This suggests that our primary estimates are not driven by MSA-specific economic conditions or other local characteristics, as they would have affected other groups as well. Moreover, these results provide additional evidence that labor supply reductions or a complete withdrawal from the market extends beyond parents.

7.2 Anticipation Effects

We also consider the possibility that omitted variables, correlated with shocks to arrests and labor market outcomes, are driving our results. To that end, we reevaluate equation 2 where the shock variable is adjusted incrementally to capture shocks that occur in future periods—a labor supply response that anticipates a shock to immigration enforcement. Analyzing potential anticipation effects allows us to verify whether current unobserved factors drive the relationship of interest, as they would likely correlate with current adolescent labor outcomes and arrest shocks in the near future.

The results in table 10 show that the impact of a shock to arrests on the labor force participation and the number of hours worked among Hispanic adolescent youth living in mixed-status families is only significant for the contemporaneous shock (S_{mt}). All regression results considering future shocks between months $t+1$ and $t+6$ are not significant and close to zero.

7.3 Placebo Test

We also consider the possibility that our results are a product of chance by conducting a placebo test similar to Abadie, Diamond and Hainmueller (2010) and Ando (2015). In this approach, we create a set of placebos by randomly assigning the immigration enforcement shocks across MSAs and month-by-year periods ($S_{mt}^{placebo}$). Equation 2 was reevaluated for *Ln(Hours Worked)* and *Labor Force Participation* using the enforcement shock placebos. The placebo study was evaluated using the same sample specifications applied in the primary analysis, described in table 4. We conducted 1,000 placebo studies where in each iteration the shock was randomly reassigned and equation 2 was reevaluated.[31] For each iteration, the estimated effect from the placebo ($\beta_7^{placebo}$) was captured, giving us a distribution to assess our primary results.

In figure 3, we plot the distribution of placebo estimates, highlighting the 95 percent confidence interval and the treatment effect estimates presented in columns 1 and 4 of table 4. The figure illustrates that the our primary results are atypical and likely not a consequence of a random chance assignment of a treatment in our identification strategy.

8 Summary and Conclusion

The enforcement of immigration law and the predominantly coercive strategy executed over the past few decades remain among the most contentious policy areas in the United States. Existing literature documents the detrimental effects of these policies on both immigrants and their US-born children across various dimensions. Our study contributes to this body of work by examining whether a surge in ICE arrests impacts the labor force participation and hours worked among US-born Hispanic adolescent youth living in mixed-status families.

Using local data on immigration-related arrests between 2014 and 2018 and data from the CPS, we identify an increase in labor force participation by approximately 6 percentage points and hours worked by 20 percent in areas that experience a sudden increase in ICE arrests. When evaluated across gender, we find that these estimates are mostly driven by the labor supply response among US-born Hispanic

[31] In equation 2 the three-way interaction with the shock placebo is expressed as $\beta_7^{placebo}(S_{mt}^{Placebo} \times H_i \times M_i)$.

adolescent women. It is plausible, as suggested by our analysis, that this shift in labor supply in response to ICE arrests is attributable to a reduction in labor supply among non-citizen parents as opposed to an increase in labor market opportunities or market wages for adolescent youth.

One of the limitations in our analysis comes from the inability to determine legal immigration status using the CPS data. Our treatment group includes US-born children whose parents are non-citizens but does not distinguish between authorized or unauthorized immigration status. While the treatment (a shock in ICE arrests) is identified, the treatment group (mixed-status households) includes some households that may be unaffected by the treatment—immigrant households where all foreign-born members are authorized. Given this data limitation, we consider our results to be lower-bound estimates for the true effect. Evidence from our falsification test suggests that the labor supply of US-born children with citizen parents was not affected by ICE arrests, supporting the notion that our results are potentially biased downward. It is reasonable to assume that labor supply among US-born children whose non-citizen parent(s) are authorized is also unaffected by ICE arrests.

This study provides a unique insight into the intra-household responses that immigrant families employ to mitigate immigration enforcement. One of these reactions, as shown here, is increased labor supply among US-born youth. A pragmatic consideration of the dynamics between immigration enforcement and labor supply within mixed-status families does not imply a wholesale indictment of immigration enforcement in the United States, but rather underscores the challenges US-born children in mixed-status families confront and the implications for intergenerational mobility. Moreover, it suggests the importance of social programs that work to support US-born children in mixed-status families.[32]

[32] For example, the Biden administration's decision to withdraw the previous administration's eligibility requirement for housing support (see 86 FR 17346 - Housing and Community Development Act of 1980: Verification of Eligible Status; Withdrawal; Regulatory Review). The previous rule excluded mixed-status families from receiving HUD benefits (see 84 FR 20589 - Housing and Community Development Act of 1980: Verification of Eligible Status).

Table (1). Immigration Enforcement Shocks

Panel A: MSAs with largest number of shocks

Rank by number of shocks	MSA	(1) Arrests	(2) Arrests Rate	(3) Shocks
1	Punta Gorda, FL	87	5.4	13
1	Syracuse, NY	145	3.8	13
1	Austin-Round Rock-Georgetown, TX	5113	19.0	13
1	Cleveland, TN	42	8.8	13
2	El Centro, CA	2058	35.6	12
2	Jacksonville, NC	21	2.8	12
2	Poughkeepsie-Newburgh-Middletown, NY	540	7.0	12
2	Tampa-St. Petersburg-Clearwater, FL	2616	7.2	12
2	Ames, IA	26	3.1	12
3	San Diego-Chula Vista-Carlsbad, CA	14824	19.9	11

Panel B: MSAs with largest number of arrests

Rank by number of arrests	MSA	(1) Arrests	(2) Arrests Rate	(3) Shocks
1	Houston-The Woodlands-Sugar Land, TX	36841	26.4	6
2	Dallas-Fort Worth-Arlington, TX	19605	16.8	11
3	Phoenix-Mesa-Chandler, AZ	19299	30.8	8
4	New York-Newark-Jersey City, NY-NJ-PA	16879	3.0	11
5	Los Angeles-Long Beach-Anaheim, CA	15480	3.5	5
6	Atlanta-Sandy Springs-Alpharetta, GA	15133	20.8	6
7	San Diego-Chula Vista-Carlsbad, CA	14824	19.9	11
8	Riverside-San Bernardino-Ontario, CA	9789	10.4	7
9	Brownsville-Harlingen, TX	8912	86.9	4
10	San Antonio-New Braunfels, TX	8068	30.4	7

Note: This table presents immigration enforcement arrests for select MSAs over the period of observation (2014–2018). Column 1 contains the total number of arrests during the period of observation. Column 2 contains the rate of arrests expressed as the total number of arrests per 1,000 foreign-born individuals in each corresponding MSA. Note that the population of foreign-born individuals used to calculate the rate is representative of the 2014 population. Column 3 contains the total number of immigration enforcement shocks experienced in each MSA over the period of observation. Panel A reports the arrest characteristic for MSAs with the 10 most total number of arrests. Panel B reports the arrest characteristic for MSAs with the 10 most total number of immigration enforcement shocks.

Table (2). Descriptive Statistics

		Hispanic		Non-Hispanic	
	Pooled sample	US citizen parent(s)	Mixed-status parent(s)	US citizen parent(s)	Mixed-status parent(s)
Individual labor outcomes					
Labor force participation	0.275	0.232	0.225	0.291	0.241
	(0.447)	(0.422)	(0.418)	(0.454)	(0.428)
Hours worked last week	4.274	3.893	4.111	4.386	3.624
	(9.622)	(9.652)	(10.17)	(9.577)	(8.750)
Individual and household characteristics					
Age	16.95	16.95	16.93	16.95	16.90
	(0.808)	(0.810)	(0.815)	(0.806)	(0.799)
Female	0.492	0.483	0.502	0.492	0.497
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Respondent is oldest sibling	0.650	0.636	0.560	0.664	0.613
	(0.477)	(0.481)	(0.496)	(0.472)	(0.487)
Number of siblings in household	1.931	2.024	2.273	1.875	1.932
	(1.096)	(1.091)	(1.213)	(1.076)	(1.042)
Lives in single-parent household	0.305	0.404	0.216	0.301	0.151
	(0.460)	(0.491)	(0.412)	(0.459)	(0.358)
Completed high school or equivalent	0.0669	0.0613	0.0609	0.0682	0.0811
	(0.250)	(0.240)	(0.239)	(0.252)	(0.273)
Parent(s) graduated high school	0.916	0.868	0.529	0.969	0.929
	(0.277)	(0.338)	(0.499)	(0.174)	(0.256)
At least one non-US-citizen parent	0.110	0	1	0	1
	(0.313)	(0)	(0)	(0)	(0)
Observations	120,127	15,762	9,508	92,065	2,792

Note: This table presents summary statistics by ethnicity and parental citizenship status for the sample of US-born youth between the ages of 16 and 18 observed in the CPS. The results were estimated using the survey sample weights. The standard errors for each mean or proportion are presented below the respective estimate in parentheses.

Table (3). Correlation between Arrest Shocks and MSA Characteristics

	(1) Number of annual shocks	(2) Arrests rate per 1,000 FB	Independent variable mean
General characteristics			
Hispanic population (%, 2014 ref. pop.)	-0.0005	-0.0013	17.39
	(0.0054)	(0.0019)	
Non-US-citizen population (%, 2014 ref. pop.)	0.0157	-0.0115***	10.02
	(0.0191)	(0.0043)	
Distance to US-Mexico border (100 miles)	0.0144	-0.0118**	10.92
	(0.0169)	(0.0049)	
Economic characteristics			
Hispanic LFP rate	0.0158	0.0033	68.93
	(0.0124)	(0.0049)	
Hispanic share in labor force	-0.0007	-0.0013	17.87
	(0.0053)	(0.0018)	
Adolescent youth LFP rate (16–19 years)	-0.0080	0.0090**	37.21
	(0.0167)	(0.0036)	
Hispanic unemployment rate	-0.0151	-0.0306***	8.59
	(0.0228)	(0.0081)	
[10pt]•Unemployment rate	-0.0347	-0.0322***	7.46
	(0.0335)	(0.0115)	
Poverty rate	-0.0428***	0.0152**	14.59
	(0.0140)	(0.0063)	
Child poverty rate (0–17 years)	-0.0272***	0.0119***	20.24
	(0.0097)	(0.0039)	
Industry location quotients			
Natural resources and mining	-0.1032*	0.0550***	0.70
	(0.0545)	(0.0196)	
Construction	-0.0887	0.1113	1.01
	(0.3883)	(0.0963)	
Manufacturing	-0.2399	0.0367	1.01
	(0.1977)	(0.0638)	
Trade, transportation, and utilities	-0.1299	0.4601***	0.99
	(0.4747)	(0.0875)	
Education and health services	-0.1500	-0.0974***	1.02
	(0.1402)	(0.0319)	
Leisure and hospitality	-0.0858	0.1427	1.06
	(0.4585)	(0.1254)	
Observations	1,140	1,140	
Dependent variable mean	2.360	0.301	

Note: The coefficients in the table were estimated by running separate regressions for different measures of MSA-specific immigration-related arrests on each MSA characteristic. All variables are aggregated at the MSA×year level. The dependent variable in column (1) is the number of immigration-related arrest shocks observed at the MSA level in a year, and in column (2), it is the rate of arrests per 1,000 foreign-born residents in 2014. Annual demographic characteristics were obtained from the 2015–2017 American Community Survey. The location quotients indicate the MSA-specific concentration of employment in a particular industry relative to the entire country and were obtained from the US Bureau of Labor Statistics. Clustered standard errors at the MSA level in parentheses. LFP=labor force participation. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table (4). Immigration Arrests and Labor Supply (Age 16 to 18)

	Labor force participation			Ln(hours worked)		
	(1) All	(2) Women	(3) Men	(4) All	(5) Women	(6) Men
Arrest shock × Hisp. × Imm. parents	0.062**	0.079**	0.049	0.152*	0.202*	0.121
	(0.027)	(0.032)	(0.042)	(0.088)	(0.116)	(0.114)
Controls	✓	✓	✓	✓	✓	✓
Fixed effects						
MSA	✓	✓	✓	✓	✓	✓
State-by-year	✓	✓	✓	✓	✓	✓
Month-by-year	✓	✓	✓	✓	✓	✓
Obs.	120123	57742	62378	120123	57742	62378
Adj R-sq				0.087	0.098	0.092

Note: This table presents the main regression results for our study. The results were estimated using the sample of US-born youth ages 16 through 18. Columns 1 through 3 show the results from the labor force participation model estimated using a linear probability model. Columns 4 through 6 show the results from the *ln(hours worked)* model estimated using OLS (log-linear). Note *ln(hours worked)* is set to 0 for those who are unemployed or out of the labor force. All regressions include controls for a contemporaneous rate of ICE arrests per 1,000 foreign-born individuals at the MSA-by-period level, age, gender, race, number of siblings, an eldest sibling indicator, a single parent indicator, and parent(s)' education. The model also includes MSA, state-by-year, and month-by-year fixed effects. The results were estimated using the CPS sample weights. Standard errors clustered at the MSA level are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table (5). Effect of Immigration Arrests on Parental Labor Supply

Panel A: Parental labor supply

	Labor force participation			Ln(hours worked)		
	(1) Head of family	(2) Mother	(3) Father	(4) Head of family	(5) Mother	(6) Father
Arrest shock × Hisp. × Imm. parents	-0.037	-0.052*	0.007	-0.060	-0.199**	-0.019
	(0.032)	(0.031)	(0.028)	(0.117)	(0.092)	(0.102)
Controls	✓	✓	✓	✓	✓	✓
Fixed effects						
MSA	✓	✓	✓	✓	✓	✓
State-by-year	✓	✓	✓	✓	✓	✓
Month-by-year	✓	✓	✓	✓	✓	✓
N	124,115	118,157	92,843	124,673	118,285	93,629
$Adj.R^2$				0.050	0.087	0.053

Panel B: Parental labor supply (citizen parents only)

	Labor force participation			Ln(hours worked)		
	(1) Head of family	(2) Mother	(3) Father	(4) Head of family	(5) Mother	(6) Father
Arrest shock × Hisp.	0.005	0.005	0.006	0.022	0.044	0.030
	(0.012)	(0.010)	(0.009)	(0.043)	(0.038)	(0.035)
Controls	✓	✓	✓	✓	✓	✓
Fixed effects						
MSA	✓	✓	✓	✓	✓	✓
State-by-year	✓	✓	✓	✓	✓	✓
Month-by-year	✓	✓	✓	✓	✓	✓
N	76,223	77,035	75,553	76,688	77,104	76,299
$Adj.R^2$				0.048	0.079	0.059

Note: The table presents regression results obtained using the sample of parents linked to US-born youth observed in our study. Columns 1 through 3 show the results from the labor force participation model estimated using a linear probability model. Columns 4 through 6 show the results from the *log hours worked* model estimated using OLS (log-linear). Note hours worked is set to 0 for those who are unemployed or out of the labor force. The panels in this table are used to compare results of an analysis of two separate sub-samples. Panel A contains all parents of US-born children between 16 and 18. Panel B focuses on US-citizen parents of US-born children between 16 and 18 (non-mixed-status family). All regressions include controls for the contemporaneous rate of ICE arrests per 1,000 foreign-born individuals at the MSA-by-period level, age, gender (in column 1), race, family size, a head-of-family indicator, and an education indicator. The model also includes MSA, state-by-year, and month-by-year fixed effects. The results were estimated using the CPS sample weights. Standard errors clustered at the MSA level are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table (6). Falsification Tests

US-born youth with US-born parents						
	Labor force participation			Ln(hours worked)		
	(1) All	(2) Women	(3) Men	(4) All	(5) Women	(6) Men
Arrest shock × Hisp.	0.004	0.007	-0.007	0.027	0.046	-0.010
	(0.016)	(0.018)	(0.023)	(0.046)	(0.053)	(0.061)
Controls	✓	✓	✓	✓	✓	✓
Fixed effects						
MSA	✓	✓	✓	✓	✓	✓
State-by-year	✓	✓	✓	✓	✓	✓
Month-by-year	✓	✓	✓	✓	✓	✓
Obs.	94,667	45,402	49,263	94,667	45,402	49,263
$Adj.R^2$				0.088	0.098	0.094

Note: This table presents regression results obtained using the sample of US-born youth between 16 and 18 with US-born parents. Columns 1 through 3 show the results from the labor force participation model estimated using a linear probability model. Columns 4 through 6 show the results from the *ln(hours worked)* model estimated using OLS (log-linear). Note *ln(hours worked)* is set to 0 for those who are unemployed or out of the labor force. All regressions include controls for a contemporaneous rate of ICE arrests per 1,000 foreign-born individuals at the MSA-by-period level, age, gender, race, number of siblings, an eldest sibling indicator, a single parent indicator, and parent(s)' education. The model also includes MSA, state-by-year, and month-by-year fixed effects. The results were estimated using the CPS sample weights. Standard errors clustered at the MSA level are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table (7). Immigration Arrests and Labor Supply among Non-citizens (Age 16 to 18)

	Labor force participation			Ln(hours worked)		
	(1) All	(2) Women	(3) Men	(4) All	(5) Women	(6) Men
Arrest shock × Hisp.	-0.083***	-0.116***	-0.053	-0.212**	-0.234*	-0.220*
	(0.031)	(0.044)	(0.039)	(0.097)	(0.137)	(0.112)
Controls	✓	✓	✓	✓	✓	✓
Fixed effects						
MSA	✓	✓	✓	✓	✓	✓
State-by-year	✓	✓	✓	✓	✓	✓
Month-by-year	✓	✓	✓	✓	✓	✓
Obs.	6,262	2,971	3,267	6,262	2,971	3,267
$Adj.R^2$				0.176	0.210	0.226

Note: This table presents regression results obtained using the sample of non-citizen youth between 16 and 18. Columns 1 through 3 show the results from the labor force participation model estimated using a linear probability model. Columns 4 through 6 show the results from the *ln(hours worked)* model estimated using OLS (log-linear). Note *ln(hours worked)* is set to 0 for those who are unemployed or out of the labor force. All regressions include controls for a contemporaneous rate of ICE arrests per 1,000 foreign born individuals at the MSA-by-period level, age, gender, race, number of siblings, an eldest sibling indicator, a single parent indicator, and parent(s)' education. The model also includes MSA, state-by-year, and month-by-year fixed effects. The results were estimated using the CPS sample weights. Standard errors clustered at the MSA level are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

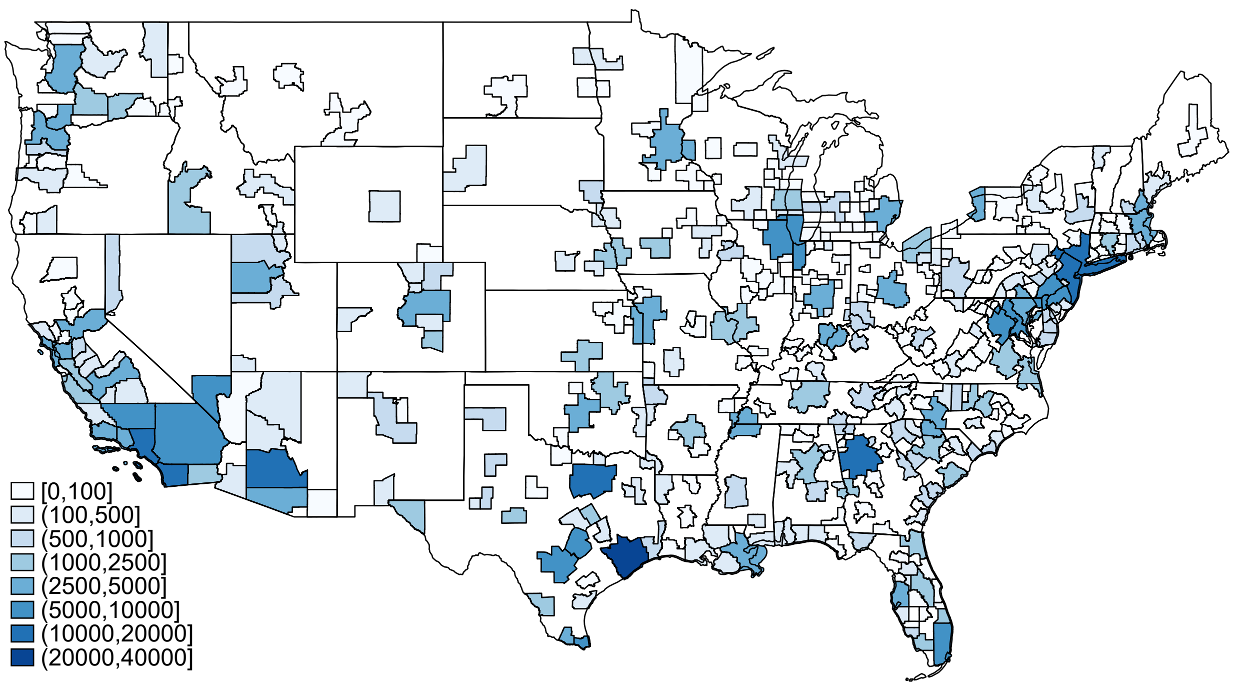


Figure (1). ICE arrests by MSA (October 2014–May 2018)

Panel A: MSAs with largest number of arrests **Panel B:** MSAs with most enforcement shocks

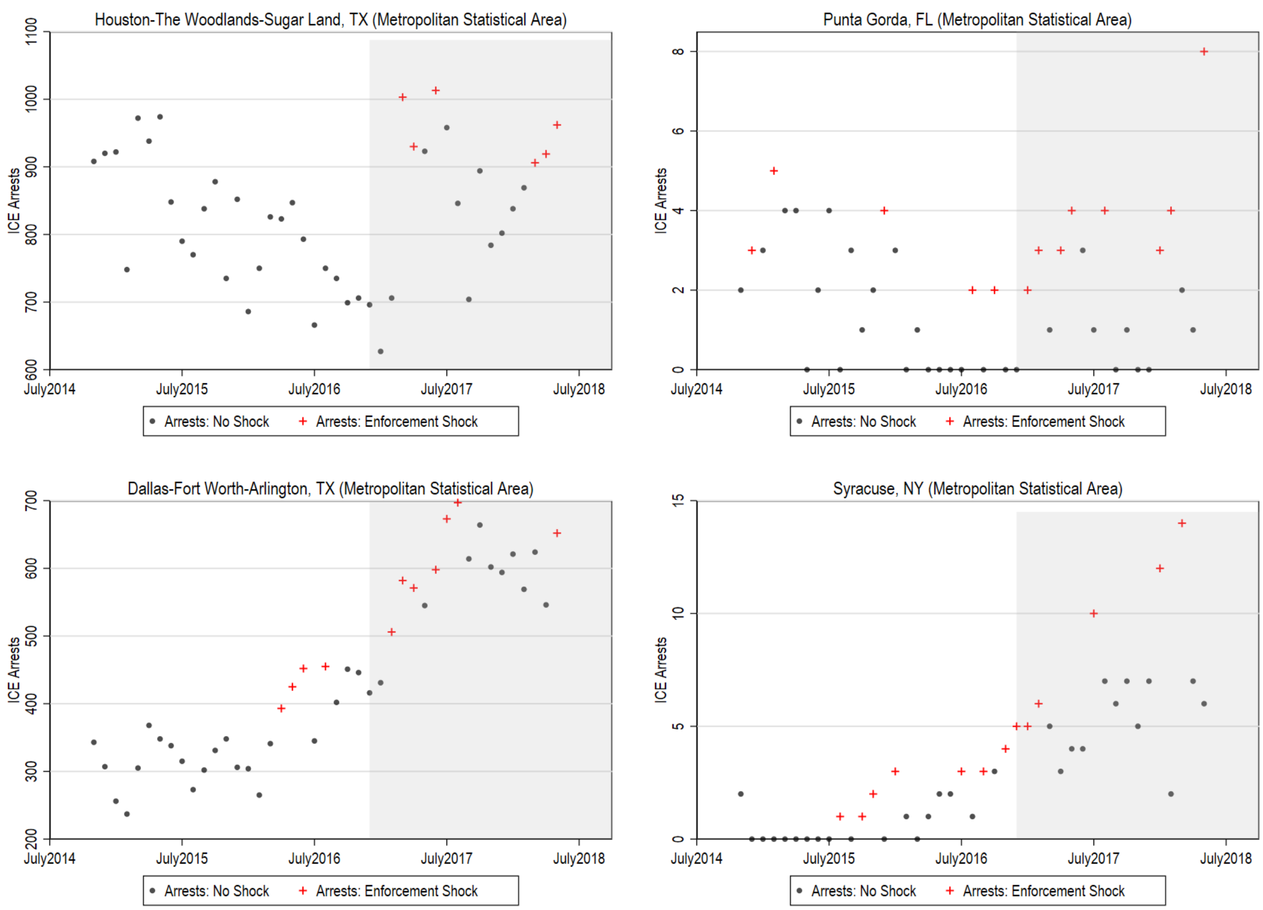
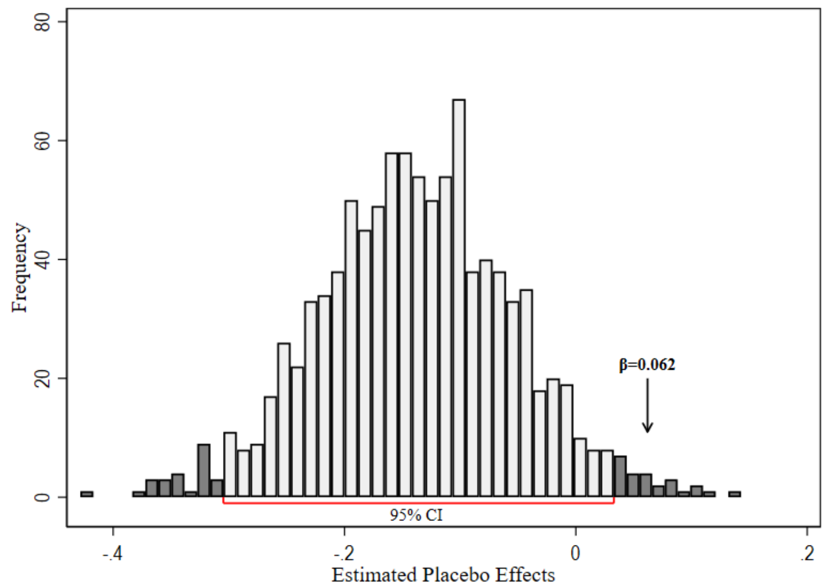


Figure (2). Month-by-MSA immigration arrests & enforcement shocks

Panel A: Labor force participation



Panel B: Ln(hours worked)

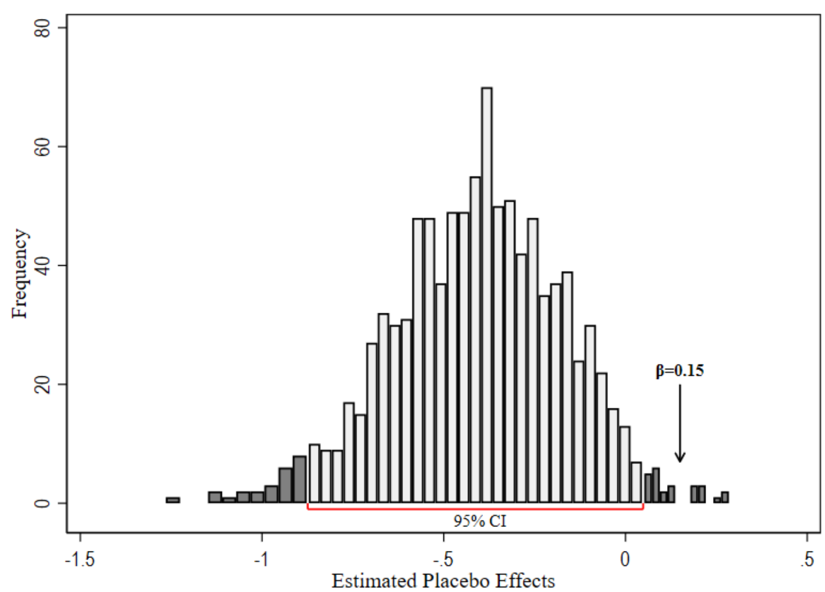


Figure (3). Distribution of placebo effect estimates (1,000 replications)

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A Appendix A: Additional Robustness Checks

A.1 Varying the Shock Parameters

In section 4.1, we outlined the two parameters used to formulate the shock variable—the standard deviation threshold used to indicate a shock in arrests and the length of time over which we measure expectation formation for immigration enforcement. We explore the sensitivity of our results to these parameters.

The robustness exercise considers the sensitivity to the threshold ($\sigma_{m,k}$). By rearranging equation 1, a shock is triggered when the number of arrests ($A_{m,t}$) is larger than the moving average ($\mu_{m,k}$) by one standard deviation ($\sigma_{m,k}$), or $S_{m,t} = 1$ when $(A_{m,t} - \mu_{m,k}) \geq \sigma_{m,k}$. To examine the shock sensitivity, we construct alternative shock indicators at the $1.5\sigma_{m,k}$ and $2\sigma_{m,k}$ thresholds, effectively increasing the number of arrests above the moving average, necessary to trigger a shock. We anticipate a larger estimated effect with each subsequent increase in the threshold. The evaluation robustness also accounts for the length of the moving average. Given the limited number of month-year periods observed in our arrest data, the longest span we calculate the moving average for is 12 months. Longer spans result in dropped observations and loss of statistical power.

Table 8 contains the estimation of equation 2 with alternative shock definitions and moving average lengths for the full sample. Panel A presents the estimates when the shock is defined by arrests 1, 1.5, and 2 standard deviations above the six-month moving average. The results in panel B were estimated using the same threshold specifications but with a 12-month moving average.Each column in the table corresponds to a model using a shock characterized by a specific threshold.

The results in table 8, panel A, suggest that at the six-month moving average, adjustments to the threshold are consistent with the primary findings. The estimates, modeling labor force participation, are shown to be positive and statistically significant at the $1.5\sigma_{k=6,m}$ threshold. Moreover, an increase in the threshold from $\sigma_{k=6,m}$ to $1.5\sigma_{k=6,m}$ resulted in a larger estimated effect—a finding that is consistent with our expectations. However, the estimate in panel A, column 3, shows a smaller and nonsignificant effect at the $2\sigma_{k=6,m}$ threshold. Estimating the model for log hours worked for the $\sigma_{k=6,m}$, $1.5\sigma_{k=6,m}$, and $2\sigma_{k=6,m}$ threshold reveals statistically significant results that increase with subsequent increases in the threshold.

The results estimated with the 12-month moving average presented in table 8, panel B, are similar in magnitude to those presented in panel A. However, only two estimates are shown to be statistically significant. The estimates for the *labor force participation* model are only shown to be significant at the $1.5\sigma_{k=12,m}$ threshold. Estimates for log hours worked are only significant at the $2\sigma_{k=12,m}$ threshold. This inconsistency suggests that a 12-month moving average may not appropriately capture the length of time expectation formation occurs, at least not across all MSAs and in the context of enforcement. A concern still exists, however, that the threshold may also be inconsistent at lower thresholds.

In figure 4, we address this by illustrating a more complete analysis on the threshold for the 6-month and 12-month moving averages in panels A and B respectively. The figure presents the coefficients on the three-way interaction ($S_{mt} \times H_i \times P_i$), where the threshold for the shock variable (S_{mt}) is incrementally increased by 0.05 between $0.25\sigma_{m,k}$ and $1.5\sigma_{m,k}$. At thresholds below $0.75\sigma_{m,k}$, the coefficients are insignificant and close to zero. However, we fail to reject the hypothesis that coefficients estimated with a threshold below $\sigma_{m,k}$ are different from zero.

Drawing from the work on forecasting, we reconstruct the moving averages allowing the data to select the optimal length of time (Hatchett, Brorsen and Anderson, 2010; Lee and Brorsen, 2017*a*,*b*). Instead of selecting a predetermined length for the moving averages (e.g., 6-month or 12-month), we use an MSA-specific length, which minimizes the mean absolute error over the period of observation.[33] Note that we only consider 1- to 12-month moving averages, where a 1-month moving average reduces to the observation in the previous month. The optimal length approach results in one MSA-specific optimal moving average used to construct the shock. It means the shock variable is accounting for heterogeneity at the MSA level instead of a consistent characterization. We illustrate the heterogeneity in figure 5 by plotting the average arrest rate for an MSA over the optimized month. For instance, the optimal length for Cincinnati, Ohio, is six months; in figure 5 this will be represented on the x-axis as $X = -6$. The graph in figure 5 reveals a systematic relationship between the number of arrests and what information is optimal to reduce forecasting errors about enforcement. Specifically, expectation formation for individuals in MSAs with high levels of immigration arrests may only consider the past two months, while individuals in MSAs with relatively low levels of arrests need to consider a longer time frame.

We include the optimal length approach to construct the shock variable across the $\sigma_{m,k}$, $1.5\sigma_{m,k}$, and $2\sigma_{m,k}$ thresholds. In table 9, we present the results from the estimation of equation 2 for labor force participation and hours worked using the "optimal moving averges" shock variable. The results are shown to similar to the result in our primary analysis

[33] $MAE_{m,k} = \frac{1}{T}\Sigma_{t=1}^{T}\left|\left(A_{m,t} - \mu_{m,k}\right)\right|$.

Table (8). Robustness Checks: Varying Arrest Shock Parameters

Panel A: 6-month moving average

	Labor force participation			*ln(hours worked)*		
	(1)	(2)	(3)	(4)	(5)	(6)
Arrest shock × Hisp. × Imm. parents						
Shock (σ)	0.061**			0.150*		
	(0.027)			(0.088)		
Shock (1.5σ)		0.093***			0.257**	
		(0.033)			(0.104)	
Shock (2σ)			0.032			0.368*
			(0.107)			(0.217)
Controls	✓	✓	✓	✓	✓	✓
Fixed effects						
MSA	✓	✓	✓	✓	✓	✓
State-by-year	✓	✓	✓	✓	✓	✓
Month-by-year	✓	✓	✓	✓	✓	✓
N	120,123	120,123	120,123	120,123	120,123	120,123
$Adj.R^2$				0.091	0.091	0.091

Panel B: 12-month moving average

	Labor force participation			*ln(hours worked)*		
	(1)	(2)	(3)	(4)	(5)	(6)
Arrest shock × Hisp. × Imm. parents						
Shock (σ)	0.028			0.115		
	(0.034)			(0.093)		
Shock (1.5σ)		0.071**			0.170	
		(0.033)			(0.104)	
Shock (2σ)			0.051			0.200*
			(0.035)			(0.116)
Controls	✓	✓	✓	✓	✓	✓
Fixed effects						
MSA	✓	✓	✓	✓	✓	✓
State-by-year	✓	✓	✓	✓	✓	✓
Month-by-year	✓	✓	✓	✓	✓	✓
N	120,123	120,123	120,123	120,123	120,123	120123
$Adj.R^2$				0.091	0.091	0.091

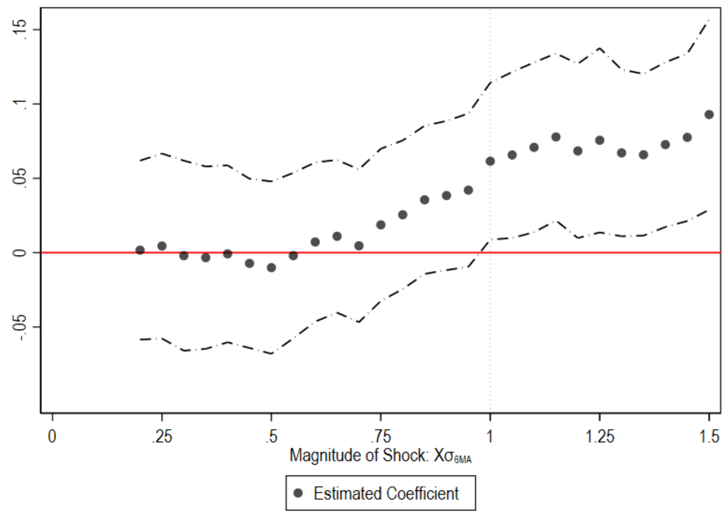
Note: This table presents the regression results obtained using the sample of US-born youth ages 16 through 18. Columns 1 through 3 show the results from the labor force participation model estimated using a linear probability model. Columns 4 through 6 show the results from the *ln(hours worked)* model estimated using OLS (log-linear). Note *ln(hours worked)* is set to 0 for those who are unemployed or out of the labor force. This table is split across two panels (A and B) to compare results where the shock variable, used as a part of the identification strategy, is redefined. Panels A and B present results where the shock variable is characterized by 6- and 12-month moving averages, respectively. All regressions include controls for a contemporaneous rate of ICE arrests per 1,000 foreign-born individuals at the MSA-by-period level, age, gender, race, number of siblings, an eldest sibling indicator, a single parent indicator, and parent(s)' education. The model also includes MSA, state-by-year, and month-by-year fixed effects. The results were estimated using the CPS sample weights. Standard errors clustered at the MSA level are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table (9). Robustness Checks: Length of Moving Average ($\mu_{m,k}$) Determined by MSA

	Labor force participation			*ln(hours worked)*		
	(1)	(2)	(3)	(4)	(5)	(6)
Shock (σ)	0.044*			0.121*		
	(0.024)			(0.073)		
Shock (1.5σ)		0.050*			0.148*	
		(0.027)			(0.077)	
Shock (2σ)			0.066**			0.176*
			(0.032)			(0.092)
Controls	✓	✓	✓	✓	✓	✓
Fixed effects						
MSA	✓	✓	✓	✓	✓	✓
State-by-year	✓	✓	✓	✓	✓	✓
Month-by-year	✓	✓	✓	✓	✓	✓
N	120,123	120,123	120,123	120,123	120,123	120,123
R^2				0.091	0.091	0.091

Note: This table presents the regression results obtained using the sample of US-born youth ages 16 through 18. Columns 1 through 3 show the results from the labor force participation model estimated using a linear probability model. Columns 4 through 6 show the results from the *ln(hours worked)* model estimated using OLS (log-linear). Note *ln(hours worked)* is set to 0 for those who are unemployed or out of the labor force. This table presents results where the shock variable, used as a part of the identification strategy, is redefined using the MSA specific "optimal" length for a moving average. All regressions include controls for a contemporaneous rate of ICE arrests per 1,000 foreign-born individuals at the MSA-by-period level, age, gender, race, number of siblings, an eldest sibling indicator, a single parent indicator, and parent(s)' education. The model also includes MSA, state-by-year, and month-by-year fixed effects. The results were estimated using the CPS sample weights. Standard errors clustered at the MSA level are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Panel A: 6-month moving average ($\mu_{k=6,m}, \sigma_{k=6,m}$)



Panel B: 12-month moving average ($\mu_{k=12,m}, \sigma_{k=12,m}$)

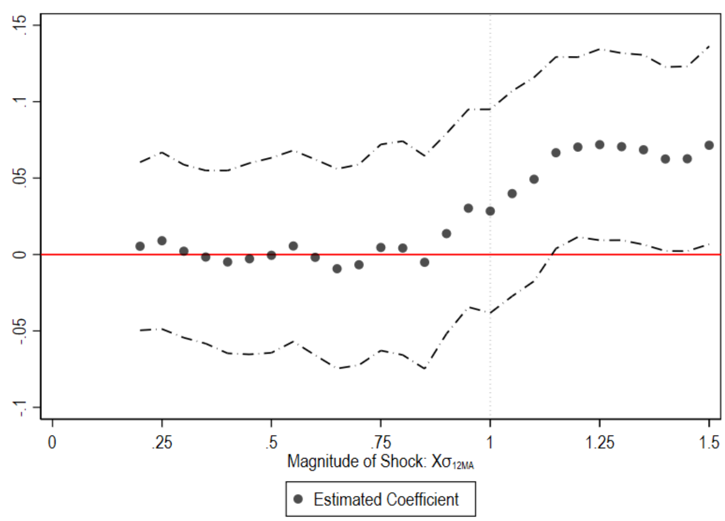


Figure (4). Coefficients plot: Calibrating the shock parameters

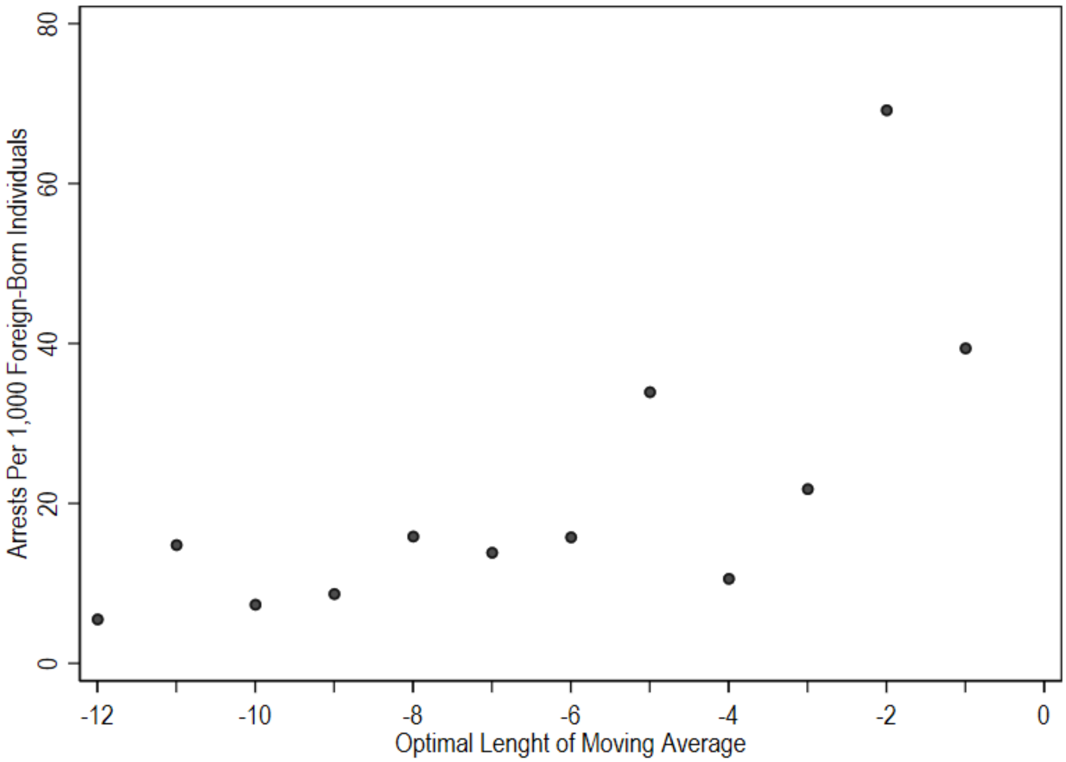


Figure (5). MSA specific length of moving average

Table (10). Assessing Potential Anticipation Effects

Panel A: Labor force participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Arrest shock × Hisp. × Imm. parents							
Arrest shock (t)	0.062**						
	(0.027)						
Arrest shock ($t+1$)		-0.013					
		(0.028)					
Arrest shock ($t+2$)			-0.006				
			(0.034)				
Arrest shock ($t+3$)				-0.002			
				(0.026)			
Arrest shock ($t+4$)					0.011		
					(0.025)		
Arrest shock ($t+5$)						0.006	
						(0.028)	
Arrest shock ($t+6$)							-0.009
							(0.031)
Obs.	120,123	117,021	113,974	111,043	108,118	105,071	101,943

Panel B: Ln(hours worked)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Arrest shock × Hisp. × Imm. parents							
Arrest shock (t)	0.152*						
	(0.088)						
Arrest shock ($t+1$)		-0.015					
		(0.074)					
Arrest shock ($t+2$)			0.037				
			(0.081)				
Arrest shock ($t+3$)				0.015			
				(0.060)			
Arrest shock ($t+4$)					0.046		
					(0.068)		
Arrest shock ($t+5$)						0.066	
						(0.084)	
Arrest shock ($t+6$)							-0.016
							(0.092)
Obs.	120,123	117,021	113,974	111,043	108,118	105,071	101943
Adj R^2	0.087	0.087	0.087	0.087	0.087	0.087	0.088

Note: This table presents the regression coefficients on the three-way interaction (*Hisp.*×*Imm. parents*×*Arrest shock*). The specifications for all models include a constant term as well as controls for a contemporaneous rate of ICE arrests per 1,000 foreign born individuals at the MSA-by-period level, age, gender, race, number of siblings, an eldest sibling indicator, a single parent indicator, and a parent(s)' education. The model also includes MSA, state-by-year, and month-by-year fixed effects. The results were estimated using the CPS sample weights. Standard errors in parentheses clustered at the MSA level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table (11). Robustness Checks: Alternative Specifications

Include summer months						
	Labor force participation			Ln(hours worked)		
	(1) All	(2) Women	(3) Men	(4) All	(5) Women	(6) Men
Arrest shock × Hisp. × Imm. parents	0.042*	0.057**	0.031	0.120	0.186*	0.075
	(0.024)	(0.028)	(0.037)	(0.073)	(0.097)	(0.094)
Obs.	139437	67068	72366	139437	67068	72366
Adj R-sq	0.102	0.110	0.108	0.091	0.100	0.096
Add state linear time trend						
	Labor force participation			Ln(hours worked)		
	(1) All	(2) Women	(3) Men	(4) All	(5) Women	(6) Men
Arrest shock × Hisp. × Imm. parents	0.061**	0.078**	0.048	0.152*	0.205*	0.119
	(0.027)	(0.032)	(0.041)	(0.090)	(0.117)	(0.113)
Obs.	120123	57742	62378	120123	57742	62378
Adj R-sq	0.095	0.104	0.100	0.086	0.097	0.091
Add MSA linear time trend						
	Labor force participation			Ln(hours worked)		
	(1) All	(2) Women	(3) Men	(4) All	(5) Women	(6) Men
Arrest shock × Hisp. × Imm. parents	0.061**	0.078**	0.048	0.152*	0.205*	0.119
	(0.027)	(0.033)	(0.041)	(0.090)	(0.118)	(0.114)
Obs.	120123	57742	62378	120123	57742	62378
Adj R-sq	0.095	0.104	0.100	0.086	0.097	0.091

Note: This table presents a series of robustness checks for main regression results obtained using the sample of US-born youth ages 16 through 18. Columns 1 through 3 show estimates of a linear probability model where the outcome variable is an indicator for labor force participation. Columns 4 through 6 show estimates of a log-linear OLS model where the outcome variable is the natural log of hours worked and those who are unemployed or out of the labor force are given a value of 0. All regressions include controls for a contemporaneous rate of ICE arrests per 1,000 foreign-born individuals at the MSA-by-period level; indicators for race; continuous variables for age and number of siblings in the household; and indicator variables for sex, whether the respondent is the eldest sibling, lives in a single-parent household, and has at least one parent with a minimum high school education. Panel A shows results including summer months in the sample, which are omitted in the main results shown in table 3. Panel B shows results including a state linear time trend in place for state-by-year fixed effects. Panel C shows results including an MSA linear time trend in place for MSA fixed effects. Standard errors clustered at the state level are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B Appendix B: Educational Impacts

The results discussed in the paper provide considerable evidence suggesting that the intensity of immigration enforcement influences labor supply among US-born Hispanic youth in mixed-status families. However, the broader implications on educational outcomes for what may be early entries into the labor force or an increase in hours worked are not as clear. Studies examining the impact of immigration enforcement policies and actions on educational outcomes have documented that an increase in immigration enforcement reduces school attendance and enrollment while increasing grade retention (e.g., Amuedo-Dorantes and Lopez, 2017*b*; Bucheli, Rubalcaba and Vargas, 2021; Bellows, 2019, 2021; Meadows, 2021; Kirksey and Sattin-Bajaj, 2021). On the other hand, studies suggest that the influence of employment on educational outcomes is largely driven by labor supply at the intensive margin (e.g., Hwang and Domina, 2017; Modestino and Paulsen, 2015; Millenky, 2016; Rothstein, 2001; Keister and Hall, 2010; Buscha et al., 2008).

In this section, we reexamine the impact of immigration enforcement on school enrollment, grade retention, and dropout rates using the sample of US-born youth observed in the CPS. The objective is to develop a deeper understanding of the conclusions we might draw from our primary findings. We posit that while the impact of immigration enforcement on education is a first-order effect, the labor supply response to enforcement actions has additional long-term consequences on educational outcomes.The intuition is motivated by the deportation pyramid model developed by Dreby (2012) whereby US-born youth in mixed-status families may provide stability during periods of heightened enforcement. For example, when a parent is apprehended, the increased responsibility placed on US-born children in mixed-status families may become a long-term arrangement (Dreby, 2012), leading to a breakdown of other commitments, such as education.

B.1 School Enrollment

Our first analysis examines enrollment using the school attendance survey item in the basic monthly CPS. We construct enrollment as an indicator variable where $y_{imt} = 1$ when respondent i reported being enrolled in school in the previous week and $y_{imt} = 0$ otherwise. It is important to note that despite the retrospective nature of the enrollment survey item relative to respondents' observation date, the basic monthly CPS runs the week of the 19th of each month, which in most cases will avoid mistiming between the enforcement shock and enrollment variables (Bucheli, Rubalcaba and Vargas, 2021). By leveraging the school attendance item in the basic monthly CPS, we are able to examine non-enrollment on a month-by-month basis for the same sample used in our primary analysis; US-born youth ages 16 to 18, living in the contiguous United States, surveyed during the academic year (August–May). Additionally, we restrict the sample to youth whose educational attainment is no greater than the 12th grade. The educational attainment sample specification removes from our analysis respondents not enrolled in school after earning a high school diploma or GED.

Our empirical approach evaluates equation 2 across three model specifications to account for shocks that occur contemporaneously $(S_{m,t})$, the previous month $(S_{m,t-1})$, and two months prior $(S_{m,t-2})$. The rationals behind this strategy are drawn from the context of our analysis, data composition, and previous studies. First, the monthly CPS allows us to measure variations in enrollment that occur month to month (Bucheli, Rubalcaba and Vargas, 2021). Previous studies on enforcement and education have primarily used annual data capturing enforcement intensity that is taken over longer units of time

(Amuedo-Dorantes and Lopez, 2017*b*; Bellows, 2019, 2021; Meadows, 2021; Kirksey and Sattin-Bajaj, 2021). Keep in mind that the decision to disenroll from school is drastic and faces pressure from compulsory school attendance laws that vary by state. Nuanced educational outcomes such as absenteeism will likely be more responsive to changes in the enforcement landscape; however, this empirical exercise is limited by the data.[34] We speculate that enrollment is less likely to respond to contemporaneous shocks in immigration enforcement; rather, the decision to disenroll is made after many absences, missed assignments, and disengagement.

Estimates from our analysis of enrollment is presented in table ??. The results contained in panels A and B suggest that enrollment is not responsive to shocks in immigration enforcement occurring contemporaneously or in the previous month (one-month lag). There is evidence that US-born Hispanic youth who have not graduated or earned a GED are less likely to be enrolled two months after the shock—a finding distinctly found among women. While the evidence in table ?? can only be taken as a correlational relationship, it loosely suggests that the responsiveness of enrollment and labor supply to enforcement is asynchronous. Moreover, the impact on labor supply, at the intensive and extensive margins, precedes the impact on enrollment.

B.2 Grade Retention and Dropout

In this section, we examine grade retention and dropout using the education supplement of the CPS, which is fielded each year in October. The education supplement provides information about enrollment and grade level at the time of the survey and in October of the previous year. Our first analysis leverages the design of the education supplement to construct a dichotomous variable capturing grade retention. Specifically, we set $y_{imt} = 1$ when a respondent reported no change in grade level from the previous year and $y_{imt} = 0$ if grade progression was determined. Note that the grade retention variable specifies a sample restriction requiring respondents to be enrolled at the time of the survey and in October of the previous year—by design, it excludes students who dropped out of school, have graduated, or earned a GED. Additionally, given that the education supplement occurs once each year, we apply the following sample restriction scheme: US-born youth ages 16 to 18, living in the contiguous United States, surveyed in October.

To address the annual structure of the data and the context of the research question, we construct an aggregated shock variable. The aggregated shock variable is specific to each MSA and is calculated by summing all of the shocks in immigration enforcement that occurred over the spring semester of the previous school year.[35] We take this approach to account for the cumulative nature of education where outcomes today represent the total investment in previous periods, including prior attendance and achievement. Moreover, the outcomes we consider here, such as grade retention and dropout, are costly decisions which we speculate are realized as the culmination of cumulative shocks rather than a contemporaneous response.

The estimates from our analysis of grade retention are presented in panel A of table ??. The results suggest that one additional shock during the spring semester of the previous school year increases the probability of grade retention at the start of the following fall semester among US-born Hispanic women aged 16 to 18. The estimates for men were similar in magnitude but are not statistically significant. Given that grade

[34] See, for example, Bellows (2019, 2021); Meadows (2021); Kirksey and Sattin-Bajaj (2021).

[35] Given that we do not have school-specific information about the duration of the spring semester, we approximate the spring semester to begin in January and end in May.

retention is often the result of substantial accumulated absences or unsatisfactory academic performance, we interpret this relationship as suggestive of a decline in student achievement or school engagement. Still, we approach the longer-term implications of this result with caution because there is a lack of consensus in the education literature that grade retention is a harmful outcome. Some studies, particularly those looking at the effect of early grade retention, mostly in the 3rd grade, find positive short-term impacts on test scores and very little effects in the long run (Greene and Winters, 2007; Jacob and Lefgren, 2004; Mariano and Martorell, 2013). On the other hand, studies that examine the impact of later grade retention, mostly in the 8th grade, find negative effects on educational attainment, school attendance, and a higher likelihood of engaging in criminal activity (Jacob and Lefgren, 2009; Eren, Lovenheim and Mocan, 2022).

In our final analysis, we explore the probability of dropping out using the education supplement of the CPS as well as the aggregated shock variable. The sample selection for this analysis includes US-born respondents between the ages of 16 and 18, living in the contiguous United States, enrolled in school (9th through 12th grade) in October of the previous year, who have not graduated or earned a GED. The dependent variable used in the analysis is constructed as a dichotomous variable where $y_{imt} = 1$ when the respondent is not currently enrolled in school but was enrolled in October of the previous year, and $y_{imt} = 0$ when enrolled at the time of the survey. The results in panel B of table ?? are close to zero and not statistically significant. Taking together the estimates for grade retention and high school dropout rates, we interpret the education results as indicative that an accumulated increase in unexpected immigration-related arrests is more likely to influence measures of student engagement and achievement (in part captured by grade retention), rather than measures of educational attainment. We conjecture the latter is likely the result of the timing of these arrests coinciding with advanced high school grades where the net cost of drastic, long-term decisions like dropping out is substantial.

Table (12). Immigration Enforcement and School Enrollment

Panel A: Contemporaneous			
	(1) All	(2) Women	(3) Men
Shock$_t$ $\times$ Hisp. $\times$ Imm. parents	-0.008	0.001	-0.030
	(0.018)	(0.020)	(0.027)
Controls	✓	✓	✓
Fixed effects	✓	✓	✓
Obs.	99,192	47,139	52,048

Panel B: Shock 1-month lag			
	(1) All	(2) Women	(3) Men
Shock$_{t-1}$ $\times$ Hisp. $\times$ Imm. parents	0.025	0.010	0.034
	(0.017)	(0.023)	(0.021)
Controls	✓	✓	✓
Fixed effects	✓	✓	✓
Obs.	96,506	45,801	50,699

Panel C: Shock 2-month lag			
	(1) All	(2) Women	(3) Men
Shock$_{t-2}$ $\times$ Hisp. $\times$ Imm. parents	-0.022*	-0.060***	0.010
	(0.014)	(0.019)	(0.021)
Controls	✓	✓	✓
Fixed effects	✓	✓	✓
Obs.	93,843	44,500	49,338

Note: This table presents the results for our exploration into enrollment. The results were estimated using the sample of US-born youth ages 16 to 18, living in the contiguous United States, surveyed during the academic year (August–May), and whose educational attainment is no greater than the 12th grade. All regressions include controls for a contemporaneous rate of ICE arrests per 1,000 foreign-born individuals at the MSA-by-period level, age, gender, race, number of siblings, an eldest sibling indicator, a single parent indicator, and parent(s)' education. The model also includes MSA, state-by-year, and month-by-year fixed effects. The results were estimated using the CPS sample weights. Standard errors clustered at the MSA level are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table (13). Immigration Enforcement, Grade Retention, and Dropout

Panel A: Grade retention

	(1) All	(2) Women	(3) Men
Aggregated shock × Hisp. × Imm. parents	0.026	0.032**	0.026
	(0.016)	(0.015)	(0.028)
Controls	✓	✓	✓
Fixed effects			
MSA	✓	✓	✓
State-by-year	✓	✓	✓
Year	✓	✓	✓
Obs.	6,918	3,251	3,630

Panel B: Dropout

	(1) All	(2) Women	(3) Men
Aggregated shock × Hisp. × Imm. parents	-0.009	-0.003	-0.009
	(0.013)	(0.021)	(0.020)
Obs.	7240	3377	3827
Controls	✓	✓	✓
Fixed effects			
MSA	✓	✓	✓
State-by-year	✓	✓	✓
Year	✓	✓	✓

Note: This table presents the results for our exploration into grade retention and dropout using the October education supplement of the CPS. In panel A, the sample was restricted to respondents enrolled at the time of the survey and in October of the previous year, US-born youth ages 16 to 18 living in the contiguous United States. In panel B, the sample was restricted to US-born respondents between the ages of 16 and 18, living in the contiguous United States, enrolled in school (9th through 12th grade) in October of the previous year, who have not graduated or earned a GED. All regressions include controls for a contemporaneous rate of ICE arrests per 1,000 foreign-born individuals at the MSA-by-period level, age, gender, race, number of siblings, an eldest sibling indicator, a single parent indicator, and parent(s)' education. The model also includes MSA, state-by-year, and year fixed effects. The results were estimated using the CPS sample weights. Standard errors clustered at the MSA level are shown in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.