ORIGINAL PAPER

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. We draw on the added-worker effect and underscore immigration enforcement actions as a factor influencing labor supply decisions within immigrant families. We argue that while immigration enforcement reduces labor supply among non-citizens, the labor supply among US-born Hispanic youth in mixed-status families increases. Using the Current Population Survey and data on immigration-related arrests, we find that an unexpected surge in arrests increases labor force participation of US-born Hispanic youth by 6 percentage points and weekly hours worked by up to 20%.

1 Introduction

Over the past two decades, the expansion of local, state, and federal immigration enforcement policies have reshaped the socioeconomic landscape for millions of immigrants in the United States and their families. The widespread implementation of policies such as Secure Communities, 287(g) agreements, and omnibus immigration laws, which resulted in 3.6 million deportations between 2008 and 2018 (U.S. Immigration and Customs Enforcement 2015, 2018b), ushered an environment of intensified hostility toward immigrant communities that has endured beyond the immediate enactment of these policies. However, assessing their impact while focusing exclusively on

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non-US citizens underestimates their full effect. For instance, examining data from 2014–2018 reveals the deportation of 200,000 individuals who claimed to be parents of US-born children.[1] This is just a fraction of the approximately 4.4 million US-citizen children with at least one unauthorized immigrant parent living under the constant risk of family separation (Capps et al. 2020).

Studies investigating the consequences of immigration policies and enforcement intensity on immigrant-origin households have provided evidence that implicates the well-being of all household members regardless of citizenship. This impact is particularly evident in various domains, including mental health distress and financial hardship (Pinedo and Valdez 2020; Gulbas et al. 2016; Wang and Kaushal 2019), child poverty (Amuedo-Dorantes et al. 2018), declining educational attainment (Bucheli et al. 2021; Bellows 2019; Amuedo-Dorantes and Lopez 2017b), and reduced participation in social welfare programs (Alsan and Yang 2022; Watson 2014).[2] However, while the literature has extensively documented the experiences of US-citizen children and adult non-citizens separately, few studies have delved into the intricate interconnections between these two groups within a family unit. We begin to fill this gap by exploring the impact of intensified immigration enforcement on the labor supply of US-born Hispanic youth living with non-citizen parents—i.e., a mixed-status family.[3]

The framework used to conceptualize the intra-family labor supply dynamic draws on the added-worker effect, whereby a spell of unemployment experienced by a family member spurs an interdependent labor supply response from another member as a strategy to smooth income and consumption (Lundberg 1985). We employ the intuition behind the added-worker effect to examine labor supply decisions within the mixed-status family. Evidence from previous studies supports the first link in this relationship, showing that immigration enforcement induces a drop in labor supply among unauthorized immigrants, a potential strategy to lower the risk of apprehension and deportation (e.g., Amuedo-Dorantes and Bansak 2014; Bohn et al. 2015; Orrenius and Zavodny 2015; Amuedo-Dorantes and Antman 2022). The mounting empirical evidence raises the question, what are the strategies families employ to alleviate the resulting economic hardship from parental labor market withdrawal?

Our hypothesis posits that unanticipated increases in immigration enforcement influence the labor supply of US-born youth in mixed-status families through changes in the labor decisions of their non-citizen parents. We test this hypothesis using individual-level data from the basic monthly Current Population Survey (CPS) merged with the count of immigration-related arrests conducted in the US interior by Immigration and Customs Enforcement (ICE) agents for each month and metropolitan statistical area (MSA) between 2014 and 2018. We develop a new empirical strategy that leverages the month-to-month variation in the number of ICE arrests within

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[1] Data obtained from ICE biannual reports to Congress on deported migrants claiming US-born children. See, for example, U.S. Immigration and Customs Enforcement (2018a).

[2] Studies also find labor market impacts associated with employment-based immigration policies, such as E-Verify (Amuedo-Dorantes and Bansak 2014; Bohn et al. 2015), as well as impacts on US citizens access of public services (Watson 2014) and overall political engagement (Amuedo-Dorantes and Lopez 2017a; Amuedo-Dorantes and Bucheli 2023).

[3] Following Bucheli et al. (2021) and Xu et al. (2016), we designate mixed-status families status when at least one US-born child in a family unit resides with at least one non-US-citizen parent.

MSAs to measure labor supply responses to frequent but unforeseen changes in the intensity of immigration enforcement. The procedure identifies periods of unusually high levels of immigration enforcement—i.e., ICE arrest *shocks*—as months in which the normalized arrest count exceeds the MSA-specific moving average by at least one standard deviation. Given that immigrants form expectations about the local level of immigration enforcement based on past experience, we reason that substantial deviations from long-term trends pinpoint periods in which the level of enforcement actions is most likely to be unexpected by the individual.

This approach differs from previous studies that leverage changes in immigration enforcement policies, such as Secure Communities and the Deferred Action for Childhood Arrivals program (DACA), as quasi-natural experiments to identify the impact of policy adoption on various outcomes, including schooling, labor, and income (e.g., Amuedo-Dorantes and Antman 2017; East et al. 2023; Kuka et al. 2020; Pope 2016). Narrowing our analysis to the period after the implementation of these policies allows us to focus on monthly fluctuations in local enforcement intensity and examine their immediate effects.

We conduct several checks to validate the plausible exogeneity of the enforcement shocks to individual labor supply decisions. First, we observe that the occurrence of a shock in ICE arrests is uncorrelated with various MSA-specific characteristics, including labor market conditions such as the unemployment rate and labor force participation. Second, we do not find meaningful evidence of a systematic correlation between shock occurrences over time that threatens our results. Moreover, the results show that labor supply behavior does not vary in anticipation of a shock, lending support to the argument that these large increases in arrests are unpredictable by individuals. Overall, these findings align with the operational planning and "surprise factor" involved in sudden and significant increases in immigration-related arrests in a given MSA.

Our primary analysis estimates the impact of exposure to immigration arrests on the labor supply of US-born Hispanic youth in mixed-status households by leveraging the temporal and geographical variation in the intensity of ICE enforcement (captured by large deviations in ICE arrests from the local trend). We use a fixed effects framework that interacts our shock measure with a mixed-status family identifier and a Hispanic ethnicity indicator while conditioning on MSA, month-by-year, and state-by-year fixed effects. This approach establishes an exposure to treatment—as captured by the *shock*—and the treatment group, US-born Hispanic youth in mixed-status families. The treatment group determination draws from existing evidence on Hispanics' higher risk of arrest by immigration authorities compared to those of other racial/ethnic backgrounds (Capps et al. 2020; U.S. Immigration and Customs Enforcement 2015, 2018b).[4]

We begin our analysis by documenting the negative labor supply impacts of heightened immigration enforcement among non-citizen Hispanic adults—a common

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[4] Immigration enforcement actions primarily target Hispanic immigrants, with individuals born in Latin American countries accounting for approximately 97% of all deportations in recent years (U.S. Immigration and Customs Enforcement 2018b).

finding in the literature (e.g., Amuedo-Dorantes and Bansak 2014; Kostandini et al. 2014; Bohn et al. 2015; Orrenius and Zavodny 2015)—as a conceptual building block and consistent with the added-worker effect. We then estimate the impact of immigration enforcement actions on the labor supply of US-born Hispanic youth (ages 16–18) living in mixed-status families by modeling the labor supply responses at the extensive (labor force participation) and intensive (weekly hours worked) margins. To account for differences in labor market experiences and outcomes by sex, each of our main analyses is evaluated separately for young men and women as well as mothers and fathers. Finally, we present supplemental results following an event study-type analysis to assess the labor supply dynamics both preceding and following the occurrence of a shock.

In line with prior literature, we find a decline in the labor supply of non-citizen parents in mixed-status households experiencing sudden surges in ICE arrests. The effect is particularly significant among mothers, for whom a shock in immigration arrests is attributable to a decline in labor force participation and hours worked by 7 percentage points and 26%, respectively. This finding concurs with recent studies documenting the harmful effects of immigration enforcement on immigrant women's labor supply, especially among those with children (Amuedo-Dorantes and Antman 2022; East and Velasquez 2022). While smaller in magnitude, the estimates for the fathers' sample are also negative and statistically indistinguishable from the effect on mothers.

As inferred from the added-worker effect framework, the reduction in parental labor supply implies a complementary response among US-born youth in mixed-status families. Consistent with this prediction, we estimate that a shock in ICE arrests raises labor force participation by 6 percentage points and hours worked by 15% among US-born Hispanic youth in mixed-status families. We also find that young women are significantly responsive to the shock, with an estimated increase in labor force participation of 8 percentage points and a 20% increase in labor hours—a notable parallel to the results for mothers. Although the point estimates for the subsample of young men are not significant at conventional levels, the coefficients are also positive and statistically indistinguishable from the effect estimated for young women.

Our exploration into the dynamic effects of exposure to an ICE arrest shock reveals that the change in labor supply for youth and parents is short-lived, lasting on average 2–3 months following the occurrence of a shock. Furthermore, we observe symmetric responses between youth and parents, consistent with the added-worker effect framework. Taken together, we interpret these findings as suggestive evidence of the short-term income-smoothing strategies that mixed-status households employ when faced with an increased risk of family separation.

This study offers two significant contributions to the existing literature. First, from a methodological perspective, we introduce a novel approach that uses arrest data to capture unexpected surges in immigration enforcement intensity. Prior work has largely relied on the spatial and temporal variation in the activation of immigration policies, a useful approach albeit limited when assessing the impacts of immigration enforcement during periods of policy inaction. In contrast, our strategy broadens the empirical toolkit and provides a pathway for researchers to identify exogenous changes in immigration enforcement activity while circumventing empirical challenges pre-

sented by the absence of policy variation. Furthermore, this approach is arguably more appropriate compared to alternatives, such as the use of direct enforcement measures, which are more likely to be driven by local conditions and associated with other factors, such as immigrant outcomes.

Second, we provide a unique insight into the unintended consequences of police-based immigration enforcement through the lens of US-born youth in mixed-status families. Our findings 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. In doing so, we draw a link between the consequences of US immigration enforcement and the added-worker effect, extending this framework beyond the conventional approach, which excludes children. The work by Baldini et al. (2018) supports the consideration of work-eligible children and provides robust empirical evidence of an added-worker effect among teenage children and students. We document a similar intra-household response among US immigrant families who, unlike citizen households, even those of very low income, face institutional barriers to alternative means of income insurance such as access to credit.

Prior studies have focused on the impacts of immigration enforcement on child education and poverty (Bucheli et al. 2021; Bellows 2019; Amuedo-Dorantes and Lopez 2017b; Amuedo-Dorantes et al. 2018), and the labor supply of resident workers more broadly.[5] By narrowing our attention to youth in mixed-status families, our findings emphasize the pervasive nature of the US immigration enforcement strategy, even among US citizens living with immigrant parents. Our findings 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 enter the labor market prematurely out of financial need rather than as part of a human capital accumulation strategy.

2 Conceptual framework

From a conceptual standpoint, we argue that immigration enforcement affects the labor market outcomes of US-born youth living with at least one non-citizen parent through the added-worker effect. Theoretically, this framework posits that an unemployment spell of a household member can trigger a positive labor supply response among unaffected members as a mechanism for consumption smoothing and insurance against income losses (Lundberg 1985; Bredtmann et al. 2018).[6] We reconsider this household dynamic within the context of mixed-status families.

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[5] A related strand in the literature studies the impact of employment-based immigration policies such as E-Verify. Amuedo-Dorantes and Bansak (2014) document an increase in employment among non-Hispanic native workers following the adoption of employment verification mandates. Likewise, Orrenius and Zavodny (2015) report E-Verify led to higher employment of naturalized male Mexican immigrants and raised earnings of US-born Hispanic men.

[6] 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 et al. 2020).

Empirically, the added-worker effect literature has largely focused on wives' labor supply adjustments in response to their husbands' involuntary unemployment resulting from business cycle fluctuations.[7] We modify this classic conception of the model along two dimensions. First, following Baldini et al. (2018), we include working-age children as full participants within households' labor supply decisions. Second, we consider immigration enforcement and the risk of family separation as drivers of involuntary retrieval from employment among likely unauthorized workers. Moreover, 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). Therefore, in the presence of unforeseen negative employment shocks, credit constraints, and lack of savings, such as in the context of the unauthorized immigrant population, the added-worker effect is expected to be substantial.

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, although exiting the labor market is usually permanent in the case of deportation. In addition to the deportation risk, unauthorized migrants typically face institutional barriers to accessing alternative mitigating strategies such as unemployment insurance or borrowing through formal credit markets. Therefore, in the context of mixed-status families, we hypothesize that the transitory (or permanent) labor market withdrawal of an unauthorized immigrant parent can trigger an increase in the labor supply of their US-born children who are working age, given that US-born children face no risk of deportation. Moreover, we predict a sizable effect considering that we define the shock in immigration enforcement to be an unexpected event for which mixed-status families are unable to prepare.

3 Data

3.1 Measuring immigration enforcement

We obtain data on ICE arrests from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University.[8] TRAC's publicly available data report the number of immigration-related arrests conducted by ICE in the interior of the country between October 2014 and May 2018 aggregated at the month-by-county level.[9] The 2014–2018 period was characterized by significant changes in immigration enforcement through reprioritization efforts (e.g., the Priority Enforcement Program, PEP) and the expansion of existing tactics (e.g., workplace raids). Our focus on this period captures

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

[8] Data available at https://trac.syr.edu/phptools/immigration/arrest/. Last accessed March 2023.

[9] The dataset is compiled from 480,000 apprehensions registered during the 44 months and does not include border apprehensions conducted by US Customs and Border Protection.

variation in enforcement intensity rather than in the activation of enforcement policies, such as those observed between 2001 and 2013 with the widespread implementation of employment verification mandates, omnibus immigration laws, 287(g) agreements, and Secure Communities.

Recent studies have leveraged the implementation of the Secure Communities program between 2008 and 2013 to identify the impact of enhanced immigration enforcement on various outcomes, including marriage rates, labor outcomes, income, life satisfaction, and crime (e.g., Bansak and Pearlman 2022; East et al. 2023; East and Velasquez 2022; García and Gutiérrez-Li 2023; Gunadi 2019; Kang and Song 2022). However, the program was fully implemented in all jurisdictions nationwide by January 22, 2013[10]—i.e., more than a year prior to the start of our study period. Since then, biometrics-sharing programs have been continuously active through, for example, the Priority Enforcement Program (PEP) and other components of the Criminal Alien Program (CAP).

Supplementary Appendix Table A.1 displays the share of ICE apprehensions carried out during our study period by method or program. We observe that approximately 95% of arrests were conducted through ICE programs that do not involve local collaboration. Approximately two-thirds of the arrests correspond to ICE's Criminal Alien Program (CAP) and other components with automatic fingerprint matching, including Secure Communities and its temporary successor, the Priority Enforcement Program (PEP). Another frequent method of apprehension is "at-large arrests," which represent 27% of the cases in the data. These are arrests conducted directly by ICE through raids or targeted apprehensions within communities. Less than 5% of ICE arrests during our study period stemmed from voluntary collaboration of local law enforcement with ICE under 287(g) agreements.

To facilitate merging the aggregate TRAC data with the individual-level CPS public use files, we crosswalk county-level ICE arrests to their respective metropolitan statistical area (MSA).[11] Thus, our analysis captures variation in immigration enforcement 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 US-born youth and their immigrant parents, as measuring immigration enforcement with apprehensions across MSAs likely confounds MSA-specific characteristics systematically correlated with our outcome variable. In addition, the number of apprehensions may not precisely measure how immigrants experience or perceive the intensity of immigration enforcement within an MSA relative to previous periods. To address these concerns, we leverage

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[10] See: https://www.ice.gov/secure-communities. Accessed March 2023.

[11] The U.S. Office of Management and Budget defines metropolitan statistical areas based on entire counties or county-equivalents (U.S. Office of Management and Budget 2010). Thus, we link the monthly county number of arrests to the MSA level by aggregating counties contained within each MSA. This process was conducted using the 2014 county-to-MSA jurisdictions crosswalk data provided by the National Bureau of Economic Research, accessible at https://data.nber.org/cbsa-msa-fips-ssa-county-crosswalk/.
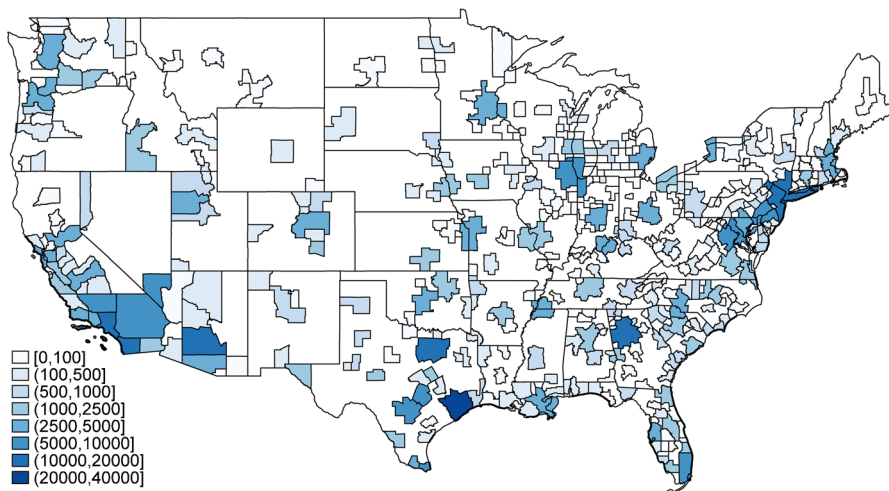
Fig. 1 ICE arrests by MSA (October 2014–May 2018)

MSA-specific time variation in ICE arrests to identify months of unusual enforcement intensity measured as large deviations above the local trend.

Although the specific factors considered by immigration authorities in deciding whether to suddenly and significantly increase immigration-related arrests in a given MSA and month are unknown to us, journalistic accounts point to the "surprise factor" as an essential characteristic of enforcement operations. Multiple reports describe how ICE raids targeting unauthorized immigrants catch businesses and individuals off-guard, usually leading to a drop in school attendance in the days following these operations.[12] Even when ICE planned to conduct the largest nationwide immigration raid to detain at least 8400 unauthorized immigrants as part of Operation MEGA between September 17–21, 2017, they deliberately chose not to announce it beforehand as a critical operational component. The official-use-only operation plan stated, "No proactive outreach shall be conducted with non-law enforcement entities on the planning or execution of this operation" (U.S. Immigration and Customs Enforcement 2017), suggesting how the details of these operations remain unknown to the public prior to their execution.

We reason that an individual's expectations 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 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

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[12] See, for example, Smith and Whitely (2018), Fox 5 Atlanta (2019), and PBS News Hour (2019) for journalistic accounts of unexpected raids at places of work. In another operation, even local law enforcement accused ICE of "misleading" them (Todd 2017).

moving average consistent with the adaptive expectation hypothesis (Lucas and Sargent [1981](#); Wallis [1980](#); Hatchett et al. [2010](#); Lee and Brorsen [2017a](#), [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 ($\sigma_{m,k=6}$),[13] over the preceding six-month period ($k = 6$).[14] Second, we standardize the time and MSA-specific arrest count ($A_{m,t}$) 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:

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

That is, the shock variable turns on when the standardized number of arrests in MSA m at time t increases by one standard deviation above its six-month moving average. In total, we capture 1130 ICE arrest shocks over the sample period.

Table 1 presents the number of shocks for select MSAs, along with the number of arrests and rate of arrests per 1000 foreign-born individuals for comparison.[15] Panel A lists the top 10 MSAs ranked by the number of shocks, while panel B lists the top 10 MSAs ranked by the 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 1000 foreign-born residents, and Ames, Iowa, with an arrest rate of 3. In contrast, MSAs listed in panel B tend to be in more traditional immigrant destinations that experience higher levels of immigration enforcement.[16]

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. Therefore, we explore the sensitivity of our main results to variations in the length of time used to calculate

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[13] 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: $\sigma_{m,k} = \sqrt{\frac{\sum_{i=t-k+1}^{t} (A_{m,i} - \mu_{m,k})}{k-1}}$.

[14] 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.

[15] 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.

[16] To further illustrate the nature of the arrest shock variable, Figure B.1 shows the trend in ICE arrests for four representative MSAs. Panel A corresponds to the top two MSAs with the highest 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.

Table 1 Immigration enforcement shocks

Rank by number of shocks	MSA	Arrests (1)	Arrest Rate (2)	Shocks (3)
Panel A: MSAs with largest number of 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	14,824	19.9	11
Panel B: MSAs with largest number of arrests				
1	Houston-The Woodlands-Sugar Land, TX	36,841	26.4	6
2	Dallas-Fort Worth-Arlington, TX	19,605	16.8	11
3	Phoenix-Mesa-Chandler, AZ	19,299	30.8	8
4	New York-Newark-Jersey City, NY-NJ-PA	16,879	3.0	11
5	Los Angeles-Long Beach-Anaheim, CA	15,480	3.5	5
6	Atlanta-Sandy Springs-Alpharetta, GA	15,133	20.8	6
7	San Diego-Chula Vista-Carlsbad, CA	14,824	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 1000 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

the moving average and the magnitude of the threshold as robustness checks (see Section 7.3 and Supplementary Appendix B for discussion and formal evaluation).

3.2 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 informa-

tion such as age, ethnicity, and citizenship.[17] The sample is restricted to households of 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 child labor laws, such as the Fair Labor Standards Act (FLSA), which limits 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 constructed from the total number of hours worked "last week" and is not conditional on employment status, therefore allowing us to capture changes to overall labor market activity 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] In addition, we adopt a common strategy in the literature that leverages Hispanic ethnicity in conjunction with US citizenship to approximate "likely unauthorized" immigration status (Orrenius and Zavodny 2009; Amuedo-Dorantes and Bansak 2014; Amuedo-Dorantes et al. 2018).[20]

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 (henceforth, citizen families or households). We observe that the labor force participation rate for the pooled sample is approximately 28%, with Hispanics exhibiting a participation rate of 23% and non-Hispanics in citizen families exhibiting a somewhat higher participation rate at 30%, although the difference between the two groups is not statistically significant. In terms of hours worked, Table 2 indicates that the average US-born youth in our sample worked for 4.3 hours in the previous week, with Hispanics working between 3.9 and 4.1 hours and non-Hispanics between 3.6 and 4.3 hours, depending on their family's mixed-status designation.

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[17] The CPS data was obtained from Flood et al. (2021)

[18] The main results are robust to including summer months. See Panel A in Table B.4.

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

[20] Our study aims to identify a unique sample—mixed-status households with US-born children between 16 and 18 years of age. By design, ours is an unrepresentative subset of the broader population of unauthorized immigrants. Therefore, our approach to identifying likely mixed-status households is a modified version of the commonly applied "demographic approach" where we avoid using educational attainment as a characteristic to refine the sample of interest.

Table 2 Descriptive statistics

	Pooled sample	Hispanic		Non-Hispanic	
		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	9508	92,065	2792

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

4 Empirical strategy

4.1 Empirical model

The core of our empirical approach aims to estimate the effect of immigration enforcement on the labor supply of US-born youth living in mixed-status households. To build the intuition behind our identification strategy, recall that the circumstances connecting immigration enforcement and US-born children is the unauthorized immigration status of the parent(s) within the household—the mixed-status family. A parsimonious identification strategy could draw inferences from a two-way interaction between the treatment (a shock to immigration arrests) and the treated group (the mixed-status family). However, the true treated group is unobserved because the CPS does not col-

lect information on unauthorized immigration status. Consequently, we must rely on a proxy identification of the treated group, which introduces measurement error and likely an attenuation bias in our estimated effects.

Despite this data limitation, we know that (i) non-citizenship is a necessary condition for unauthorized immigration status and (ii) immigration authorities use Hispanic ethnicity as a proxy to infer unauthorized status (Flores and Schachter 2018).[21] We, therefore, consider these two crucial dimensions—Hispanic ethnicity and non-US citizenship—as valuable, albeit imperfect, proxies to narrow the treatment group. The strategy to identify the treatment group now relies on the interaction between our constructed "mixed-status" variable and the Hispanic indicator ($M_i \times H_i$).[22] We proceed to examine the impact of an arrest shock on the labor market outcomes of Hispanic youth living in non-citizen households by estimating the following model via OLS:

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

where y_{imt} represents either labor force participation or log hours worked for individual i in MSA m at time t.[23] Our treatment variable, S_{mt} is a binary variable indicating the occurrence of an enforcement 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 mixed-status household. The parameter β_7, corresponding to the three-way interaction between S_{mt}, H_i, and M_i, identifies the causal effect of immigration enforcement on the labor supply of Hispanic youth in mixed-status households. The causal interpretation of β_7 is maintained by the assumption that the shock indicator (S_{mt}) is exogenous to youth labor supply. We provide evidence of the plausible exogeneity below.

Vector X_{imt} includes both individual and household characteristics. The model accounts for age, gender, race, number of siblings, and an eldest sibling indicator at the individual level. At the household level, the model includes variables for single-parent households and parental high school completion. The contemporaneous level of immigration enforcement, A_{mt}, is captured using the rate of ICE arrests per 1000 foreign-born individuals.

Equation 2 also includes a set of MSA (θ_m), month-year (θ_t), and state-year (θ_{st}) fixed effects to control for unobserved factors potentially impacting youth labor market outcomes, such as immigration enforcement policies, attitudes toward immigrants and diverse populations, and local elections.[24] The MSA fixed effects account for time-invariant MSA-specific characteristics, such as distance to the border and stable local attitudes and policies toward immigrants that can drive demand for immigration

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[21] Hispanic ethnicity has also been used to capture co-ethnic spillover effects of immigration policies, such as Secure Communities (Alsan and Yang 2022).

[22] Using ethnicity to sharpen the identification of our targeted treatment group is widely applied throughout the literature to study "likely unauthorized" immigrant residents (e.g., Orrenius and Zavodny 2009; Amuedo-Dorantes and Bansak 2014; Amuedo-Dorantes et al. 2018). We interrogate the underlying presumptions behind this approach in the following subsections and the Supplementary Appendix (Table B.2).

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

[24] We also run alternative specifications where we control for MSA-specific linear time trends. See Supplementary Appendix Table B.4 for results following these specifications.

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 and other factors—e.g., business cycle fluctuations or presidential administrations.[25] Lastly, we use individual-level sampling weights from the basic monthly CPS and cluster the standard errors at the MSA level.

4.2 Identifying assumptions and validity checks

The underlying assumption behind our identification strategy relies on the exogeneity of the constructed ICE arrest shock variable with respect to both observed and unobserved variables that may influence youth labor supply behavior. Unlike a one-time policy implementation that remains active, exposure to a shock in enforcement varies month-to-month within and across MSAs. For instance, the level of arrests in one month may be high enough to trigger a shock ($S_{m,t} = 1$); however, the level of arrest in the following month may not result in a shock ($S_{m,t+1} = 0$). In this section, we begin to evaluate the integrity of the shock variable, identifying assumptions underlying our model, and potential threats attributable to omitted variables or reverse causality.

Omitted variable bias is a particular concern that we are careful to address. An unobserved variable correlated with ICE arrest shocks and changes in youth employment outcomes may confound the causal interpretation of our results. For example, a growing local economy 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.[26] In this scenario, failing to account for the changing local economic conditions would lead to the incorrect conclusion that changes in youth labor supply are attributable to an increase in immigration-related arrests when, in fact, both are the result of local economic growth.

In addition to including MSA, month, and state-by-year fixed effects, which should control for unobserved factors to a large extent, we directly explore the potential existence of omitted variables in our relationship of interest. We estimate the correlation between the immigration-related shocks to which MSAs are exposed and several demographic, economic, and industrial characteristics likely related to youth labor supply. If omitted variables exist and they pose a meaningful threat to identification, at least some of these characteristics would correlate with the shock in a direction that confounds the main results. To examine this possibility, we regress each residualized MSA characteristic on the residualized number of annual ICE arrest shocks during

[25] Following Kuka et al. (2020) and Alsan and Yang (2022) we experiment with alternative model specifications that include race-by-year and race-by-state fixed effects. Results are available upon request. We also estimate models that separately control for: (1) MSA-specific seasonal shocks, (2) MSA-specific seasonality interacted with an indicator for households with low educational attainment, and (3) Hispanic-specific month and year seasonality at the MSA level. Our findings are robust to these modeling choices and results are available upon request.

[26] Note that immigration enforcement actions conducted by ICE are often facilitated by local law enforcement agencies. See Table A.1 for a disaggregation of the apprehension methods and programs.

our study period.[27] We obtain MSA characteristics from the American Community Survey and the Bureau of Labor Statistics.[28] Table 3 presents the results from this exercise for (i) general MSA characteristics, such as the share of foreign-born population; (ii) economic characteristics, including unemployment and poverty rates; and (iii) industry location quotients. The latter addresses 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 variables like the share of the non-citizen population, 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 the share of the Hispanic population and the concentration of employment within the natural resources industry. While statistically significant, we note that the magnitude of these coefficients indicates that a one standard deviation increase in the residualized number of annual shocks is associated with a 6% of a standard deviation increase in the residualized share of the Hispanic population.

Regarding the positive correlation between the number of ICE arrest shocks and the proportion of the population that is Hispanic, potential upward bias in the estimation of the effect could arise if an MSA's demographic composition were indicative of labor force participation rates among different groups. However, our findings suggest that the increased immigration enforcement is associated with the relative size of the Hispanic population, not to its participation in the labor force. The presence of more Hispanics in a given MSA does not necessarily imply a higher likelihood of Hispanic individuals joining the labor force. In fact, Table 3 shows no correlation between ICE arrest shocks and the Hispanic labor force participation rate or the Hispanic share in the labor force, mitigating concerns about potential upward bias.

Turning to the results on the concentration of workers in natural resources and mining, our concern is the potential simultaneous correlation of ICE arrests and youth labor force participation with a third confounding factor. For instance, a growing agricultural sector may increase labor demand, including among young individuals, and encourage migration, potentially leading to heightened ICE activity and arrests. This situation would lead to an overestimation of the true effect of ICE arrest shocks, as the treatment indicator would capture both the positive effects of immigration-related arrests and the growth in the agricultural sector on youth labor force participation. However, Table 3 shows that the number of ICE arrest shocks is negatively correlated with the natural resource sector, including agriculture. Thus, the estimate on our treatment indicator is likely capturing the positive effect of ICE arrest shocks on youth

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[27] Following the Frisch-Waugh-Lowell theorem, we individually residualized the number of annual ICE arrest shocks and MSA characteristics by partialling out year and MSA fixed effects to remove potential trends. Then we standardized the residualized variables to facilitate the interpretation of regression coefficients.

[28] The ACS data was obtained from Ruggles et al. (2022).

Table 3 Correlation between MSA characteristics and arrest shocks

	Number of annual shocks	Arrests rate per 1000 FB
General characteristics:		
Hispanic population (%, 2014 ref. pop.)	0.060*	0.019
	(0.035)	(0.062)
Non-US-citizen population (%, 2014 ref. pop.)	−0.002	0.052
	(0.036)	(0.039)
Economic characteristics:		
Hispanic LFP rate	−0.038	−0.054
	(0.033)	(0.058)
Hispanic share in labor force	0.017	0.009
	(0.034)	(0.075)
Adolescent youth LFP rate (16–19 years)	0.020	0.008
	(0.039)	(0.058)
Hispanic unemployment rate	−0.036	0.050**
	(0.032)	(0.024)
Unemployment rate	−0.044	0.092***
	(0.043)	(0.031)
Poverty rate	−0.011	−0.012
	(0.038)	(0.042)
Child poverty rate (0–17 years)	−0.027	0.002
	(0.038)	(0.033)
Industry location quotients:		
Natural resources and mining	−0.060*	−0.114***
	(0.035)	(0.031)
Construction	0.024	0.022
	(0.042)	(0.023)
Manufacturing	0.029	−0.001
	(0.040)	(0.030)
Trade, transportation, and utilities	0.024	0.008
	(0.040)	(0.030)
Education and health services	−0.055	0.006
	(0.040)	(0.032)
Leisure and hospitality	0.010	0.080***
	(0.040)	(0.029)
Observations	1128	1128

Note: The coefficients in the table were estimated by running separate regressions for each standard residualized MSA-specific characteristic on the standard residualized immigration-related arrests measure indicated in each column. All variables were residualized with respect to MSA and year fixed effects. The independent 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 1000 foreign-born residents in 2014. All variables are aggregated at the MSA×year level. 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 U.S. Bureau of Labor Statistics. Clustered standard errors at the municipality level in parentheses. LFP=labor force participation. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

labor supply and the negative correlation between agricultural activity and ICE arrests, suggesting a potential downward bias in our estimates.[29]

As an additional check for the presence of omitted variables, in the robustness section, we estimate the "effect" of immigration enforcement shocks on the labor outcomes of citizen youth living in citizen households. By definition, these youth are not directly impacted by the consequences of immigration enforcement through the channel of intra-household adjustments in the labor supply of unauthorized parents. Therefore, evidence indicative of a change in their employment outcomes in response to an ICE arrest shock would imply the potential role of changing local labor market conditions. Overall, we find small and insignificant point estimates suggesting that the occurrence of an arrest shock is not linked with a contemporaneous structural change in local labor markets more broadly. A robust discussion of this analysis is detailed in Section 7.1.

Lastly, we examine plausible threats to validity that emerge from reverse causality, which, in the context of our study, would signify that changes in the labor supply of Hispanic youth in mixed-status families predict the timing and occurrence of an ICE arrest shock. For instance, an increase in Hispanic youth labor supply may signal the presence of immigrant labor in a community and thus influence immigration enforcement efforts. We investigate the potential influence of reverse causality by reevaluating Eq. 2 where the arrest shock variable is adjusted incrementally to capture shocks that occur in future periods. Under this specification, a significant coefficient would provide evidence of 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 4 show that the impact of a local shock to ICE arrests on the labor force participation and the number of hours worked among US-born 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. Overall, these results provide evidence against a potential reverse causal impact in our estimates.

5 Preliminary evidence from parental labor supply

We begin our empirical analyses by examining the impacts of immigration enforcement shocks on parental labor outcomes as the conceptual foundation of the added-worker effect we aim to explore. An investigation into parental labor supply also provides insights into our treatment group specification and potential mechanisms influencing labor supply among youth. First, we expect immigration enforcement to influence labor supply decisions within households characterized as both Hispanic and

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[29] We also experiment with correlating the MSA characteristics with the arrests rate per 1000 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 the residualized rate of arrests is correlated with several characteristics, including the unemployment rate and labor concentration in the natural resources and mining, and leisure and hospitality industries.

Table 4 Assessing potential anticipation effects

Panel A: Labor force participation	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Arrest shock × Hisp. × Mixed-status parent(s)							
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)
Observations	120,123	117,021	113,974	111,043	108,118	105,071	101,943

Table 4 continued

Panel B: Ln(hours worked)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Arrest shock × Hisp. × Mixed-status parent(s)							
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)
Observations	120,123	117,021	113,974	111,043	108,118	105,071	101,943
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 (*Arrest shock × Hisp. × Mixed-status parent(s)*). The specifications for all models include a constant term as well as controls for a contemporaneous rate of ICE arrests per 1000 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

mixed-status. Evidence supporting a contemporaneous response to the labor supply of households outside our treatment group would imply potential threats from omitted variable bias or higher-order effects stemming from the reduction in labor supply among unauthorized workers. It could also suggest that the labor supply of US-born Hispanic youth may be responsive to these market adjustments rather than the impact of immigration enforcement within their mixed-status households. While research has shown that US-born citizens in non-Hispanic households are also impacted by immigration enforcement (East and Velasquez 2022), it is important to note that our study is focused on the immediate month-by-month responses to immigration enforcement.

Second, we expect the impacts of enforcement shocks on the labor supply of non-citizen Hispanic parents to be inversely related with the impact on their US-born children. Evidence of a negative relationship would undergird the presence of an intra-household strategy to mitigate the adverse income effects due to the parents' labor supply reductions—a mechanism connecting immigration enforcement to youth labor supply.

In Table 5, we present the results from our evaluation of labor supply among parents of the US-born children observed in our sample. Given that there may have been multiple children within the same household, we restrict the data to observe each parent only once. We analyze labor force participation and hours worked independently for each subsample 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 shocks to immigration enforcement reduced the labor force participation of non-citizen parents who are the head of household by 5 percentage points. Additionally, we estimate that shocks to immigration enforcement reduced the labor force participation of non-citizen Hispanic mothers by 7 percentage points and reduced hours worked by 26%. Though insignificant, the point estimates for the sample of fathers are also negative and statistically indistinguishable from that of mothers.

Our findings for parental labor supply are in line with the immigration enforcement literature, which often finds the impact of these policies to be significantly pervasive among women. The literature on immigration enforcement and labor supply provides suggestive evidence that our findings are capturing possibly stronger impacts experienced by mothers. For example, Amuedo-Dorantes and Antman (2022) find meaningful declines in labor supply among immigrant women with children in response to an increase in ICE deportations, and East and Velasquez (2022) show a decline in labor supply at the intensive margin for immigrant women following the proliferation of Secure Communities. Other studies have also presented evidence of the negative relationship between labor supply and heightened immigration enforcement among non-citizen Hispanic adults more broadly (e.g., Amuedo-Dorantes and Bansak 2014; Kostandini et al. 2014; Bohn et al. 2015; Orrenius and Zavodny 2015). The lack of appreciably meaningful effects among fathers is consistent with a relatively inelastic labor supply among likely unauthorized men (Borjas 2017), and evidence that the decline in men's labor supply following the intensification of immigration enforcement is driven by large changes in the population of likely unauthorized men (e.g.,

Table 5 Effect of immigration arrests on parental labor supply

	Labor force participation			Ln(hours worked)		
	Head of family (1)	Mother (2)	Father (3)	Head of family (4)	Mother (5)	Father (6)
Panel A: Parental labor supply						
Arrest shock × Hisp. × Non-citizen head of family	−0.055* (0.032)			−0.142 (0.121)		
Arrest shock × Hisp. × Non-citizen mother		−0.070* (0.038)			−0.261** (0.121)	
Arrest shock × Hisp. × Non-citizen father			−0.006 (0.024)			−0.124 (0.086)
Obs	74,128	75,374	71,995	74,525	75,433	72,625
$Adj.R^2$				0.052	0.083	0.049
Panel B: Parental labor supply (citizen parents only)						
Arrest shock × Hisp	−0.003 (0.013)	0.001 (0.012)	0.008 (0.010)	−0.000 (0.048)	0.014 (0.046)	0.077** (0.038)
Obs	65,761	66,478	65,196	66,158	66,537	65,826
$Adj.R^2$				0.049	0.061	0.056

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 subsamples. 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). The samples for panels A and B were restricted to parents observed during the academic school year (August-May) and within the contiguous US. All regressions include controls for the contemporaneous rate of ICE arrests per 1000 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 in addition to 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. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

through the deportation channel) rather than declines in their labor force participation (East et al. 2023).

To better situate the magnitude of our findings for parents, Amuedo-Dorantes and Antman (2022) show that for every additional 10 removals from an MSA in a given month, there is a 0.9 percentage point decrease in the employment prospects and a 0.6 percentage point decrease in labor force participation among likely unauthorized immigrants relative to naturalized Hispanic immigrants. In our study, the average number of monthly arrests between MSAs that experience a shock and those that do not is approximately 50. A back-of-the-envelope calculation indicates that an additional 10 arrests would lead to a 1.1 percentage point decline in labor force participation among non-citizen Hispanic parents. This result is similar to the estimate in Amuedo-

Dorantes and Antman (2022) despite differences in populations of interest and measure of enforcement intensity.

The results presented in Table 5, panel B, reflect the analysis where the sample is restricted to households with only US-citizen parents as a falsification test that focuses on individuals that should remain unaffected by immigration enforcement. The results suggest that, contemporaneously, citizen parents do not appear to be affected by shocks to immigration arrests, providing further evidence that the impact of surges in ICE arrests is concentrated in Hispanic families, especially those with non-citizen parents.

6 Main results

6.1 ICE arrests and Hispanic youth labor supply

Having observed a negative relationship between exposure to immigration arrest shocks and the labor supply of non-citizen parents in mixed-status households, this section presents results interrogating our main hypothesis, namely whether US-born youth in mixed-status households respond to unexpected surges in ICE arrests by increasing their labor supply. Table 6 reports estimates of our coefficient of interest (the triple interaction term in Eq. 2) obtained from evaluating the main regression model on the sample of US-born youth. 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 women, respectively. Lastly, columns 4 through 6 report results on labor hours for the pooled sample along with findings for subsamples stratified by sex.

Table 6 Immigration arrests and labor supply (age 16 to 18)

	Labor force participation			Ln(hours worked)		
	All	Young women	Young men	All	Young women	Young men
	(1)	(2)	(3)	(4)	(5)	(6)
Arrest shock × Hisp. ×	0.062**	0.079**	0.049	0.152*	0.202*	0.121
Mixed-status parent(s)	(0.027)	(0.032)	(0.042)	(0.088)	(0.116)	(0.114)
Obs	120,123	57,742	62,378	120,123	57,742	62,378
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 1000 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 in addition to 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. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

In line with our hypothesis, we find that, on average, shocks in immigration arrests increase the labor force participation of US-born Hispanic youth by 6.2 percentage points—a 27% increase relative to the sample mean.[30] Across gender, results point to a large and significant effect on the subsample of young women whose labor force participation increases by nearly 8 percentage points (33%) following exposure to an ICE arrest shock. This represents a meaningful change in labor supply at the extensive margin, and it is comparable in magnitude to added-worker effect estimates for women whose spouses experience an adverse employment shock (Ayhan 2018; Bredtmann et al. 2018). The point estimate for the sample of young men, while insignificant at conventional levels, is also positive and statistically indistinguishable from that of young women.

Next, we estimate the model for labor hours. As shown in Table 6, column 4, we find a 15% increase in hours worked during the previous week in response to a shock in immigration arrests. Consistent with our previous results, we observe a significant impact among young women, for whom we estimate a 20% 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 working hours. Given that our model explains hours worked not conditioned on employment, we interpret these estimates as a change in overall labor market activity among US-born Hispanic youth in mixed-status families.[31] The estimated effect for the sample of young men with respect to working hours is also positive and consistent with the findings for young women, though statistically insignificant. Therefore, while the results show evidence of a stronger increase in labor hours for young women, we cannot rule out the same effect on the labor market outcomes of young men.

In our final analysis in this section, we explore the impact of immigration enforcement on the labor supply of children in mixed-status families as separately specified by mothers' and fathers' citizenship status. Results from this exercise are presented in Table 7. In panel A, we observe that youth labor supply increases when non-citizen mothers are exposed to an arrest shock, although the point estimates are not statistically significant. In panel B, we find larger labor effects for youth whose non-citizen fathers experience enforcement shocks. These results, consistent with our primary findings, provide a more complex picture of household labor supply decisions. They underscore the intricate interplay between youths' labor supply decisions and complexities in household composition, arrangement, and mixed-status typology.

6.2 Idiosyncratic event analyses

Thus far, our main findings on youth labor outcomes and supplementary analyses on parental labor supply provide evidence that exposure to ICE arrest shocks triggers directionally opposite responses within members of the same household with varying US citizenship status. In this section, we further investigate whether these estimated

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[30] 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.

[31] 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 6.

Table 7 Immigration arrests and youth labor supply by parental citizenship status

	Labor force participation			Ln(hours worked)		
	All	Young Women	Young Men	All	Young Women	Young Men
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mixed-status determined by mothers' non-citizenship						
Arrest shock × Hisp × Non-cit mom	0.033	0.055	0.026	0.099	0.164	0.086
	(0.040)	(0.043)	(0.058)	(0.133)	(0.159)	(0.165)
Observations	90,209	42,775	47,431	90,209	42,775	47,431
Panel B: Mixed-status determined by fathers' non-citizenship						
Arrest shock × Hisp × Non-cit. dad	0.074**	0.115***	0.029	0.156**	0.293***	0.022
	(0.030)	(0.041)	(0.043)	(0.070)	(0.104)	(0.116)
Observations	109,152	52,536	56,613	109,152	52,536	56,613

Note: This table estimates the impact of arrest shocks on Hispanic youth while identifying mixed-status households separately according to mothers' and fathers' non-citizenship status. All regressions include controls for a contemporaneous rate of ICE arrests per 1000 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 in addition to 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. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

effects persist over time by exploring dynamic variations in labor force participation and labor hours both preceding and following the occurrence of a shock. Moreover, the event analyses provide a deeper insight into potential anticipation of the treatment, thus serving as an additional identification check on our main empirical approach. To carry out this analysis, we capitalize on the idiosyncratic nature of shock occurrences, namely the duration of shock spells and the presence of non-shock periods, which vary widely across MSAs, as displayed in Table 1 and Figure B.1.

We evaluate the dynamic impacts of irregular shock occurrences by adopting a strategy similar to Cengiz et al. (2019), where the pre- and post-shock months are restricted to "clean" event windows. The procedure evaluates events where there are at least four months before and four months after a shock occurrence where we do not observe a shock. That is, it limits the analysis to an uncontaminated stacked event study with four pre-shock months and four post-shock months. While this analysis stipulates restrictive sample criteria, it attempts to mitigate cross-period contamination. A detailed explanation of this exercise is presented in Supplementary Appendix B.4.

In Fig. 2, we illustrate the estimates for labor force participation and log hours worked among Hispanic US-born youth in mixed-status families and their parents. There are several notable insights from the event study exercise worth highlighting. First, the results provide additional evidence that labor supply behavior does not preempt exposure to a shock. The evidence is consistent with our analysis on anticipation discussed in Section 4.2; however, it is also shown here for parents' labor supply. Second, in the months following a shock, the labor supply of Hispanic youth in mixed-status families increases while their parents' labor supply decreases. In both instances, the changes in labor force participation and hours worked are short-lived, lasting 2 to 3 months. These synchronous and opposite responses are consistent with the added-worker effect framework and lend credence to our main hypothesis.

6.3 Interpretation and discussion of main findings

Putting together our results, we find evidence that mixed-status families 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 reduces labor force participation among non-citizen parents, who face a direct risk of apprehension and deportation, by up to 7 percentage points, and an increase in the labor force participation by up to 8 percentage points among US-born Hispanic youth living with non-citizen parents. Notably, both effects are large and significant for the subsamples of young women and mothers, a finding that aligns with recent work by Amuedo-Dorantes and Antman (2022), who find meaningful reductions in labor supply among mothers following a rise in deportations.

To contextualize the magnitude of these findings, work by Taylor et al. (2011) and Capps et al. (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
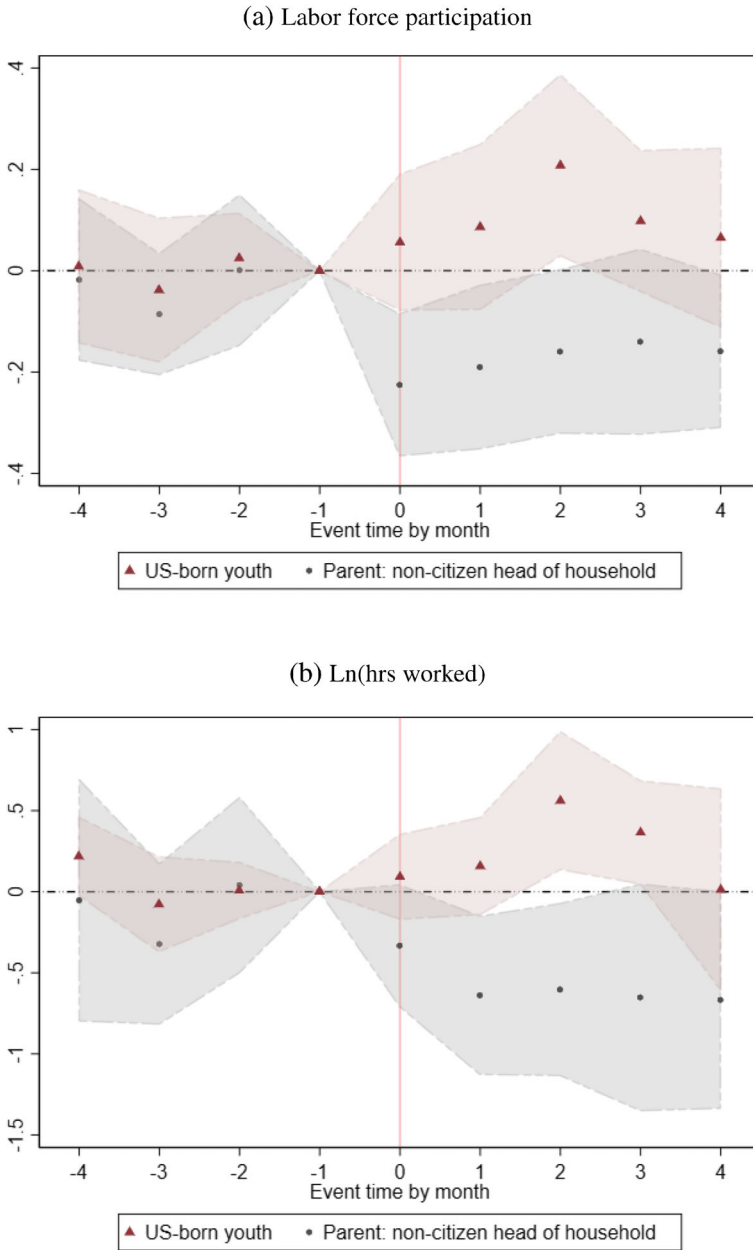
(a) Labor force participation

(b) Ln(hrs worked)

Fig. 2 Idiosyncratic event analysis

living in mixed-status families, of whom 420,000 are women.[32] Within this context, our estimates in Table 6 suggest that approximately 50,000 US-born Hispanic youth, including 33,000 young women, between ages 16 and 18 either persisted or entered the labor force during school months as a result of immigration arrest shocks between 2014 and 2018. As such, the labor effects among youth derived from enhanced immigration enforcement activity likely materialized the trade-offs associated with educational investments, making alternative economic activities more salient to them. Though speculative, these results begin to highlight plausible institutional factors contributing to Hispanic youth having one of the highest high school dropout rates nationwide (McFarland et al. 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% while also resulting in an analogous 26% decrease among non-citizen parents. 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 a plausible and economically significant alternative for time use. As such, our estimates for the sample of young women, for example, indicate an increase in weekly labor hours of 0.8 hours, approximately 10% of the time teenagers spend on homework every week (Livingston 2019).

Lastly, our exploration into the persistence of the main findings, along with that of the parents, reveals sharp yet short-lived changes in labor supply. While we find no evidence that impacts endure over time, it is likely that these short-term adjustments in labor outcomes occur routinely, given that within the same MSA area, shocks are measured multiple times over the study period. While our study does not directly investigate the implications associated with repeated exposure to arrest shocks, it begins to illuminate likely consequences, such as repeated labor market cycling behavior among youth who are yet to complete secondary education.

7 Diagnostics and robustness

In this section, we propose several empirical exercises to evaluate the validity of our causal interpretation and robustness of our main findings. First, we conduct a falsification 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, we verify that changes in arrests are salient to non-citizen youth who, as non-citizen parents, are directly exposed to the risk of apprehension and deportation. Third, we explore alternative measures for arrest intensity. Fourth, we investigate the construction of the shock variable and robustness. Lastly, we propose a placebo exercise where we randomize the occurrence of a shock across MSAs and time.

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[32] While the CPS does not distinguish authorized and unauthorized immigration status, segments of the population estimated by Taylor et al. (2011) and Capps et al. (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 et al. (2016).

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 demographic that should not be affected by the shock.

Table 8 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 gender. 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—a mechanism for which we provided evidence in Section 5.

Finally, we verify whether the impact of ICE arrest shocks 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-

Table 8 Falsification tests

US-born youth with US-born parents						
	Labor force participation			Ln(hours worked)		
	All	Young women	Young men	All	Young women	Young men
	(1)	(2)	(3)	(4)	(5)	(6)
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)
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 1000 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 in addition to 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. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

citizen adolescent youth will remain unaffected or potentially decrease during periods of intensified immigration enforcement. To investigate this, we evaluate Eq. 2 using a sample of non-citizen youth and find that shocks in ICE arrests induced a reduction in labor supply among this group (Table 9). 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 non-citizen parents.

In conjunction with our main results, we interpret the findings presented in this section as a strong indication of the validity of our empirical approach. We estimate varied effects in response to exposure to the same treatment (i.e., a shock in ICE arrests) that depend, in predictable ways, on the mixed-status characterization of households. First, we find no change in labor supply among youth or parents in citizen households. Second we estimate a decrease in labor force participation and labor hours for non-citizen youth in non-citizen households. And third, we show a rise in labor supply among citizen youth living in mixed-status households. This diverse response by household typology is in full agreement with the particular ways in which immigration enforcement actions differentially implicate households and their members according to their US citizenship. Indeed, it is through this channel that such distinctive responses can be most clearly rationalized.

7.2 Alternative measures of exposure to ICE arrests

Table 10 presents estimates using alternative measures for immigration enforcement intensity, specifically, the contemporaneous rate of ICE arrests per 1000 non-citizens and the lagged percent change in the 6-month moving average of arrests. Immigration enforcement measures, such as the rate of arrests, lagged change in arrests, and depor-

Table 9 Immigration arrests and labor supply among non-citizens (ages 16 to 18)

	Labor force participation			Ln(hours worked)		
	All (1)	Young women (2)	Young men (3)	All (4)	Young women (5)	Young men (6)
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)
Obs	6262	2971	3267	6262	2971	3267
$Adj.R^2$				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 1000 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 in addition to 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 10 Effect of immigration arrests on youth labor supply: alternative measures of enforcement arrests

	Labor force participation			Ln(hours worked)		
	All (1)	Young women (2)	Young men (3)	All (4)	Young women (5)	Young men (6)
Panel A: Arrest rate per 1000 non-citizens						
Alt arrest measure × Hisp × Mixed-status parent(s)	0.059***	0.074**	0.047	0.180***	0.173**	0.216***
	(0.022)	(0.031)	(0.034)	(0.056)	(0.074)	(0.088)
Observations	120,123	57,742	62,378	120,123	57,742	62,378
Adj R-sq	0.096	0.106	0.102	0.087	0.098	0.093
Panel B: Lagged percent change in ICE arrests						
Alt arrest measure × Hisp × Mixed-status parent(s)	0.006***	0.000	0.012**	0.018***	−0.002	0.035***
	(0.002)	(0.004)	(0.005)	(0.006)	(0.011)	(0.013)
Observations	114,906	55,254	59,652	114,906	55,254	59,652
Adj R-sq	0.098	0.107	0.102	0.088	0.099	0.092

Notes: This table presents 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 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 of high school education. All specifications include month-by-year, MSA, and state-by-year fixed effects. Panel A reports results using the rate of ICE arrests per 1000 non-citizen foreign-born population as the measure for intensity of immigration enforcement actions. Panel B reports results using the lagged percent change in the 6-month moving average of ICE arrests at the MSA level. Standard errors clustered at the MSA level are shown in parentheses. $* p < 0.1$; $** p < 0.05$; $*** p < 0.01$

tations, have been used in the literature (e.g., Amuedo-Dorantes and Antman 2022) and are evaluated in this study as a comparison.

The analyses using alternative measures suggest that immigration enforcement and the labor supply of Hispanic youth in mixed-status families are positively correlated. While the findings in Table 10 are directionally consistent with our main results, the interpretations of the magnitudes are dissimilar. For instance, results from the analysis using the rate of arrests per 1000 non-citizens suggest that a 1 percentage point change in the rate of arrests would lead to a 5.9 percentage point change in labor supply. A back-of-the-envelope calculation using this estimate suggests that an additional 50 arrests—the change in arrests on average when a shock is observed in an MSA— would lead to 14 percentage point increase in labor force participation.

Nonetheless, investigations into the validity of alternative measures, such as the rate of arrests, provide evidence that undermines its suitability as a source of exogenous variation. As discussed in Section 4.2, we assess the model by conducting a correlational analysis between different measures of enforcement and MSA characteristics. The analysis reveals that the arrest rate per 1000 foreign-born is positively correlated with key MSA-specific labor market indicators such as Hispanic unemployment and the overall unemployment rates. The correlation patterns shown in Table 3 undermine the assumption of exogeneity when using the arrest rate as a measure of enforcement intensity.

7.3 Exploring robustness and *shock* construction

To further understand the nature of the shock variable, Fig. 3 illustrates the total monthly number of ICE arrests and the share of MSAs experiencing a shock across each period in our analysis. The figure, as expected, reveals that following the transition to the Trump administration, there is a clear upward trend in the total number of arrests—a structural shift in the US immigration enforcement strategy spurring a dramatic increase in arrests. The monthly share of MSAs experiencing a shock over time, also plotted in Fig. 3, is shown to respond to the increase in arrests brought on by the Trump administration. However, there is a clear drop in the share of MSAs experiencing a shock following the increase in the number of arrests after January 2017.

Given that we construct the shock variable as the deviation above the preceding six months' average, the criteria for a shock dynamically adjust. In other words, the shock variable prioritizes the deviation away from the trend rather than the trend itself. The decline in the share of MSAs experiencing a shock results from the Trump administration's revamped efforts to detain unauthorized immigrants, ultimately raising the six-month moving average and making it more difficult for an increase in arrests to trigger a shock. Arguably, even when nationwide increases in enforcement were expected and realized, the shock variable worked to capture unexpected shifts in enforcement intensity.

A concern that arises from the discussion above questions the robustness of our results to variations in predetermined parameters used to establish the shock variable, specifically, the duration of the moving average and the magnitude of the threshold
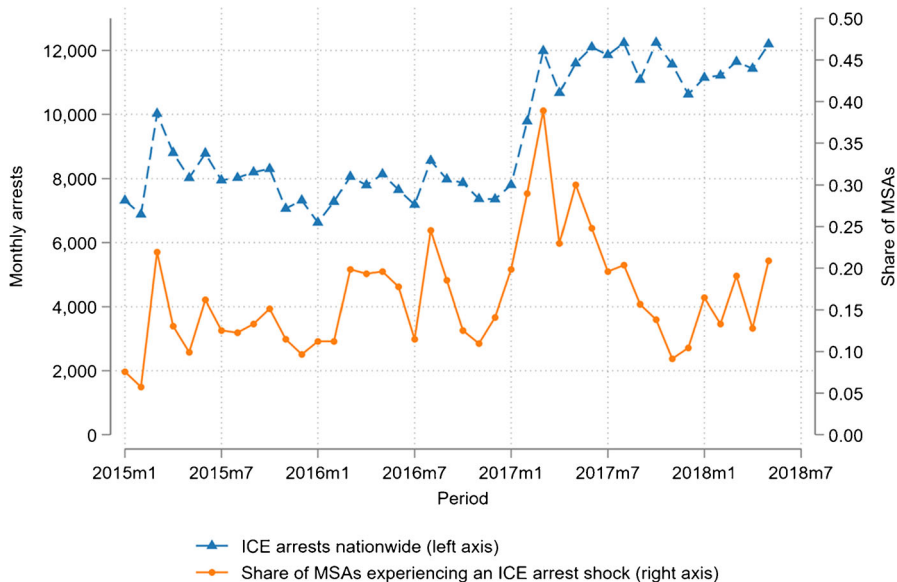
Fig. 3 Monthly ICE arrests and share of MSAs experiencing an ICE arrest shock

used to trigger a shock. We provide a detailed exploration of the robustness of our results to various formulations in the shock variable and model specifications (see Supplementary Appendix B.3). The findings from these exercises show that our primary results are consistent across variations in the threshold to trigger a shock as well as the interval used to calculate the moving average and standard deviation. Additional sensitivity checks are included in Supplementary Appendix B.2, revealing that our results are robust to variations in model specification and sample inclusion criteria.

7.4 Placebo test

In previous sections, we explored the possibility of anticipation along two dimensions. First, we explored whether future shock occurrences were correlated with contemporaneous changes in labor supply (see Section 4.2). Secondly, we adopted an event analysis to estimate changes in labor supply at each period before and after a shock occurrence (see Section 6.2). Both analyses provide insights into whether there is an effect in the absence of the treatment.

We extend this line of inquiry, considering the possibility that our results may be a product of chance, by conducting a placebo test similar to Alberto et al. (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 ($S_{mt}^{placebo}$) over 1000 iterations. In each iteration, the shock is randomly assigned, and Eq. 2 is reevaluated for *labor force participation* and *ln(hours worked)*.[33] We conduct this exer-

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

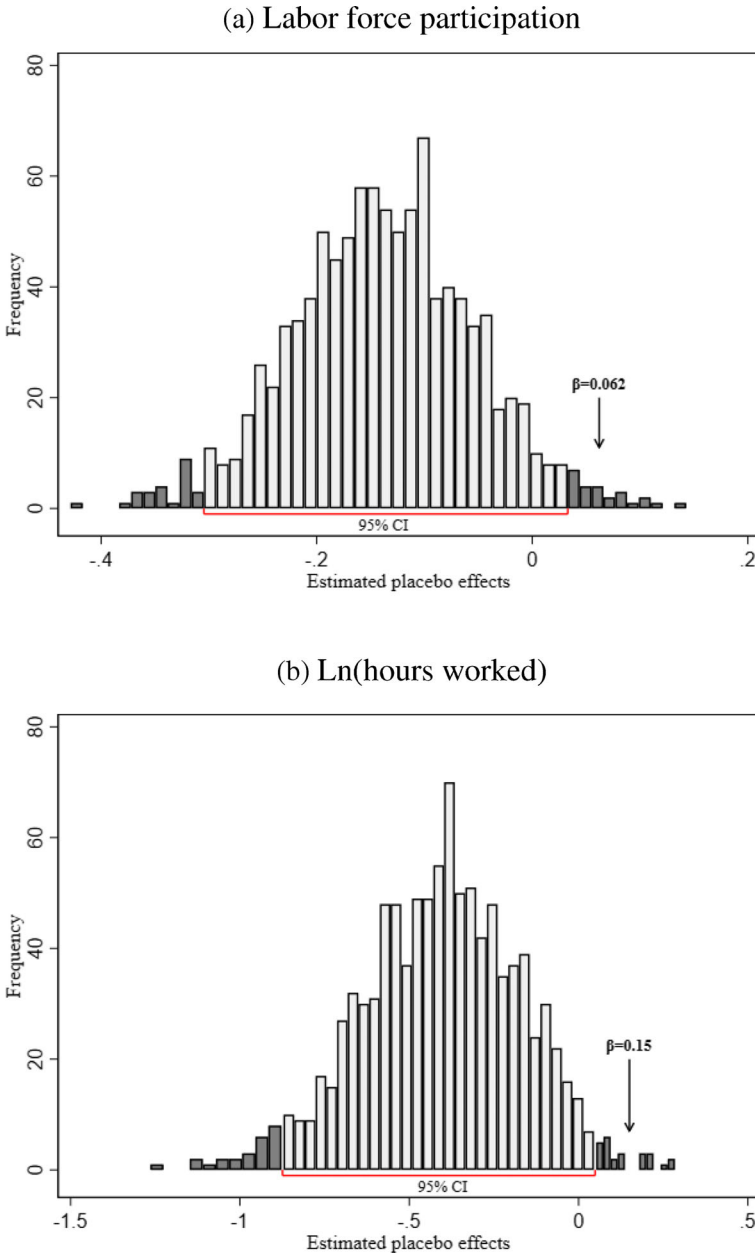(a) Labor force participation

(b) Ln(hours worked)

Fig. 4 Distribution of placebo effect estimates (1000 replications)

cise with the same sample and specifications as in our primary analysis in Table 6. For each iteration, the estimated effect from the placebo ($\beta_7^{placebo}$) is captured, providing us with a distribution to assess our primary results.

In Fig. 4, we plot the distribution of the placebo estimates, highlighting the 95% confidence interval and the treatment effect estimates presented in columns 1 and 4 of Table 6. The figure illustrates that our primary results fall outside the 95% confidence interval and thus are likely not a consequence of spurious correlations.

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 individual-level data from the CPS, we identify an increase in youth labor force participation by approximately 6 percentage points and hours worked by 20% in localities that experienced a sudden increase in ICE arrests. When evaluated across gender, we find that these estimates are appreciably significant among US-born Hispanic young women, although their results are not significantly different from the estimated impact on young men. Further analyses assessing the persistence of our main findings reveal that the sharp increase in labor supply is short-lived, lasting 2–3 months following the exposure of an ICE arrest shock. Together with our analyses of the labor supply response of parents, our findings provide evidence in line with the added-worker effect.

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 families) includes some families that may be unaffected by the treatment—namely, immigrant households where all foreign-born members are authorized. Given this data limitation, it is reasonable that misclassification of unauthorized immigrants can be attributable to their survey non-response in an effort to remain undetected by government officials, likely attenuating our estimates toward zero. Evidence of this potential attenuation is observed in our falsification tests, which suggest that the labor supply of citizen parents and US-born children with citizen parents was not affected by ICE arrests.

Overall, 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 immi-

gration enforcement in the United States, but rather underscores the challenges US-born children in mixed-status families confront and the implications for intergenerational mobility. Indeed, this analysis documents a singular dimension through which enhanced enforcement activity impacts the lives of youth in mixed-status households, motivating further inquiry into additional consequences of exposure to ICE arrest shocks along other related outcomes, such as schooling and human capital formation more broadly. Consequently, it suggests the importance of social programs that work to support US-born children in mixed-status families.

Declarations

Conflict of interest The authors declare no competing interests.

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