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ABSTRACT

We provide the first causal, national empirical analysis of the labor market impacts of heightened immigration enforcement during the second Trump administration. Enforcement increased everywhere, but, we take advantage of the fact that the increases have been uneven across geographic areas to classify areas as treated or control and then implement an event study and difference-in-differences design. Areas that experienced particularly large increases in the number of arrests also experienced a decrease in work among likely undocumented immigrants who remain in the U.S., compared to areas with smaller increases in arrests. We find no evidence of positive spillover effects to U.S.-born workers and U.S.-born workers who work in immigrant-heavy sectors are harmed.

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1 Introduction

The second Trump administration promised, and is on the way to conducting, the largest immigration enforcement effort in recent decades (East et al., 2026). A key argument made by politicians and the general public in support of heightened immigration enforcement is that removing immigrants from the country will create more job opportunities for U.S.-born workers. Recent news articles have highlighted that employers are having a hard time finding workers, reportedly due to heightened immigration enforcement, with no indication that U.S.-born workers are taking the jobs left behind (Sainato, 2025; Chadde, 2025). However, there is no comprehensive empirical evidence on how immigration enforcement in Trump 2.0 is affecting the labor market.

This paper provides the first national, causal empirical evidence on the labor market impacts of immigration enforcement in the second Trump administration, looking at both the direct effect on immigrants who remain in the U.S., as well as spillover effects to U.S.-born people. To do so, we take advantage of new, detailed data on ICE arrests by geographic area to propose and validate a transparent empirical strategy.

Specifically, we exploit the fact that the initial increase in immigration enforcement in 2025 was uneven across areas within the U.S. to causally identify the effects of enforcement distinct from other changes in 2025. In particular, we define areas as "treated" if they experience a large and sudden increase in ICE arrests between January 2025 and October 2025. "Control" areas are those that did not experience a sudden increase in arrests during this time period. We use states as the main unit of geography, except in California, New York, and Texas, where we have additional sub-state geographic detail. We bring this empirical strategy to the Current Population Survey (CPS) data from January 2024 to November 2025.

We implement an event study model in which the treatment timing is defined based on when the first large increase in arrests took place in treated areas. This allows us to evaluate dynamic effects, as well as directly evaluate the parallel pre-trends assumption. We focus first on the effects on working-age (20-64) likely undocumented immigrants. Here, we follow the literature and use observable demographics to define a sample of likely undocumented immigrants, since there is no variable for documentation status in the data (East and Velásquez, 2022; East et al., 2023; Borjas and Cassidy, 2019; Passel and Cohn, 2014; Hook and Bachmeier, 2013; Orrenius and Zavodny, 2009; Amuedo-Dorantes and Bansak, 2012). Our primary sample of likely undocumented immigrants is foreign-born individuals with at most a high school degree or equivalent who work in sectors where undocumented

workers are over-represented.

Our focus is on the labor market outcomes among likely undocumented immigrants who remain in the U.S., instead of changes in the total number of immigrant workers, due to issues with trying to estimate population counts in the CPS data (Kolko, 2025b; Edelberg and Watson, 2024). Even though these immigrants are not physically removed from the labor market, they may reduce work because of fear that going to work and being at work could increase their likelihood of interacting with ICE.[1] Among those who remain in the U.S. and work in likely affected sectors, we find a meaningful chilling effect—there is a significant 4% reduction in employment. As expected, this overall effect is driven by males, who make up over 90% of those arrested by ICE in our time period.

Turning to working-age U.S.-born individuals, we find a null effect on the full sample. We can rule out increases in the U.S.-born employment rate in likely impacted sectors of more than 0.1 percentage points. Moreover, there is a negative and significant impact on employment of U.S.-born male workers with at most a high-school education, who work in likely affected sectors. This is consistent with a model where undocumented immigrants and U.S.-born workers are complements, rather than substitutes for each other in the labor market (Chassamboulli and Peri, 2015). Descriptive data supports such a model; undocumented immigrants are more likely than U.S.-born individuals to work in jobs that are less desirable due to lower pay, on the job hazards and irregular schedules (Mukherjee and Manuel Krogstad, 2024). We see no evidence that employers increase wages to attract U.S.-born workers to fill these jobs in the face of immigration enforcement. Instead, our results are consistent with employers reducing labor demand overall, including for jobs more often taken by U.S.-born workers.[2]

Our analysis makes several contributions. We are the first to study the comprehensive, causal labor market effects of heightened immigration enforcement the second Trump administration. Our findings directly inform a central policy justification for heightened immigration enforcement—that it expands job opportunities for U.S.-born workers. We find

[1] 43% of all immigrants in the U.S. reported being concerned about themselves or someone they know being deported in summer 2025 (Noé-Bustamante et al., 2025).

[2] This may seem counter to the claims that there has been a large decrease in the foreign-born workforce that is compensated for with an increase in the U.S.-born workforce (DHS, 2025; FAIR, 2025). However, this claim relies on the population estimates from the CPS, which are known to have measurement issues (Edelberg and Watson, 2024). For example, these estimates imply an implausibly large increase in the native-born adult population of 3 million (Kolko, 2025a). Instead, our approach relies on looking at whether there are differential changes in labor market outcomes among likely undocumented and U.S.-born individuals who remain in the U.S., in areas that have experienced more, or less, immigration enforcement activity in 2025. This abstracts away from the issues with trying to measure population sizes in the CPS.

no evidence supporting this argument, and, instead document negative spillovers for less-educated U.S.-born men, suggesting that enforcement may contract, rather than reallocate, labor demand in affected sectors.

Methodologically, we introduce an approach that defines treatment based on discrete surges in enforcement activity and combines this with an event-study design. This framework allows us to test for pre-trends, isolate enforcement shocks from contemporaneous macroeconomic changes, and sidestep well-known measurement error in CPS population counts by focusing on within-area changes in outcomes among those observed.

A growing literature examines the economic impacts of immigration enforcement in the second Trump administration, but existing studies focus on different outcomes and specific geographic areas.[3] Li and Hantao (2026) use foot traffic data in six states to measure the impacts of enforcement and find lower activity in communities after enforcement events. In area-specific studies, Schwitzer et al. (2026) finds that consumer spending fell in the state of Minnesota in response to enforcement, and Brizuela et al. (2025) find evidence that immigration enforcement is hurting the Texas labor market. In contrast, we link enforcement intensity to worker-level labor market outcomes across the whole country.

Past research on immigration enforcement study earlier policies in the 1930s and 2010s (East et al., 2023; Lee et al., 2022; Amuedo-Dorantes and Bansak, 2012; Orrenius and Zavodny, 2015, 2009). The second Trump administration's immigration enforcement regime is much larger and more indiscriminate than other recent periods (East et al., 2026). Despite the different time periods and methods, the general pattern of findings in the past literature is consistent with our findings here—that immigration enforcement reduces that number of immigrant workers, and has harmful effects on U.S.-born workers. However, we find that the chilling effects among immigrants who remain in the U.S. is larger in Trump 2.0 than during the Obama administration, likely due to the more indiscriminate nature of enforcement activity in Trump 2.0.

Data

To measure interior immigration enforcement activity, we use data from the Deportation Data Project (DDP) and focus on ICE arrests from October of 2023 through October of 2025 (Deportation Data Project, 2025). This data includes administrative arrests made under ICE Enforcement and Removal Operations (ERO), which captures the majority of interior

[3]Cravino et al. (2026) simulate national wage effects of mass deportation over Trump's second administration.

immigration arrests.[4] The data includes information on each person arrested, including demographics, the exact date and time of the arrest, the ICE Area of Responsibility (AOR) and the state the arrest took place in. Our primary geographic unit is the state of arrest, however, we use sub-state information for California, New York, and Texas. This gives us a total of 58 areas in the final analysis; 48 states, 3 sub-state areas in California, 5 sub-state areas in Texas, and 2 sub-state areas in New York. Throughout the remainder of this paper we use the terms "area" or "geographic area" to refer to this geographic classification.

To measure economic outcomes, we use the monthly Current Population Survey (CPS) from October 2023 to November 2025 (Flood et al., 2025). This is the only large-scale, timely data available that allows us to measure labor market outcomes by demographic group. Note, while later CPS data is now available, we do not include it because the data we currently use to measure treatment status ends in mid-October 2025, so we omit later CPS data to avoid mis-measuring treatment status. As more recent data on ICE activity becomes available, we will include more recent CPS data as well.

The CPS, as with other large data sets in the U.S., do not contain immigrant documentation status, so we proxy for this using demographic characteristics, as is common in prior work (East and Velásquez, 2022; East et al., 2023). Specifically, we identify an individual as "likely undocumented" if they are foreign-born, working aged (ages 20-64), and have a high school degree or less. We drop those born in U.S. territories. The results are robust to further conditioning on Hispanic ethnicity or non-citizen status, though citizenship can be reported with error (Hook and Bachmeier, 2013; Brown and Heggeness, 2026), so we do not use it as our primary sample criteria.

We focus on those currently or recently employed in likely affected sectors based on pre-period intensity of undocumented workers by sector (Passel and Cohn, 2016). Additionally, because males make up most of ICE arrests, we further focus on sectors that are male dominated. Our main definition of likely affected industries includes agriculture (4.6% undocumented workers and 71% male), construction (16.3% undocumented workers and 95% male), manufacturing (12.5% undocumented workers and 59% male), and wholesale (11.7% undocumented workers and 54% male). To observe sector of current or recent employment,

[4]Arrests made by other agencies, such as Customs and Border Protection (CBP) and United States Citizenship and Immigration Services (USCIS), are not captured here. ERO can also conduct criminal arrests, which are not captured in this data, but these make up only about 4% of total ERO arrests (ICE ERO, 2020). Homeland Security Investigations (HSI) can also make both administrative and criminal arrests. Although the nature of the division means that it makes significantly fewer arrests than ERO. Specifically, HSI makes only about 4% of ICE's total arrests (Office of Homeland Security Statistics, U.S. Department of Homeland Security, 2022).

individuals must be working, searching for work, or have worked within the last 5 years. This means that our sample is skewed towards those who have a work history, however, those with some attachment to the labor market are the policy-relevant sample.

We test the robustness of this sector classification as well. First, we add to our primary definition business services, leisure and hospitality, education and health care services, and other services. This includes sectors that employ high portions of the undocumented population, but are not necessarily male dominated. Finally, we look at only education and health care services, leisure and hospitality, and other services to see if there are any effects in these sectors that have many undocumented workers, but are female dominated.

We measure the labor market outcomes of likely undocumented and U.S.-born individuals between the ages of 20 and 64 working in these same likely affected sectors. If immigration enforcement does lead to any positive effects for U.S.-born workers due to substitution, these effects would be strongest in the sectors with the largest impact on undocumented immigrants. We also split the U.S.-born sample by sex and education.

The two primary labor market outcomes we use in our analysis are whether the individual reports working in the previous week and hours worked in the previous week. We also explore effects on weekly earnings, and hourly wages, among those who are paid hourly. We collapse the data to the area-by-month average for the given outcome and sample of interest. In this collapse, we weight using the survey-provided weights for the monthly CPS for employment outcomes, and earner study for the worker pay measures, though we explore robustness to using alternative weights made available available through the end of 2024 proposed by Jed Kolko.[5] Given this, we do not weight our final regressions, though we show robustness to weighting by baseline population sizes.

It is important to note what effects we are and are not able to capture with this approach. The impact of ICE activity on immigrants' total labor supply works through two main channels: 1) reductions in the size of the immigrant workforce through physical removals; and 2) impacts to the labor market outcomes among immigrants who remain in the U.S. through chilling effects. Chilling effects capture the fact that heightened ICE activity may cause people to be fearful of participating in their regular activities–including going to work–as this could lead to an interaction with ICE and eventual deportation (East et al., 2023). Our outcomes of interest focus on this second channel and will only capture part of the total effect on immigrant labor supply, since we miss the direct effects of removals. This will not be an issue for the estimates on U.S.-born workers' outcomes, however, since we do

[5] `https://jedkolko.com/cps-weights/`

not expect large changes in their population size.

The reason we do not look at this first channel—the number of likely undocumented workers—is due to well-documented issues with estimates of the population size and changes in survey non-response in the CPS (Kolko, 2025b; Bick and Bloodworth II, 2025) In future work, we will explore whether we can rescale the population estimates using alternative benchmarks to investigate this first channel.

There are two other potential compositional issues worth discussing related to immigrant mis-measurement in the CPS. First, if there are changes in survey non-response over time, this could bias our results (Brown and Heggeness, 2026). Given that individuals most impacted by ICE are plausibly the ones most likely to stop responding to the survey, our estimates will, if anything, be an underestimate of the overall economic impacts on likely undocumented individuals. Second, there could be changes if those who are physically removed from an area, due to detention or deportation (either forced or voluntary) had differential labor supply than those who remain. In particular, if those removed have higher labor supply than those left behind, this could cause us to overestimate the effect. However, we view this as unlikely given ICE's current arrest patterns, which primarily consist of arresting people who are already in law enforcement custody or those who they encounter somewhat randomly (East et al., 2026).

Empirical Strategy

Immigration enforcement intensified nationally in the second Trump administration, as shown in Figure 1. However, some areas have experienced larger increases than others (shown in Appendix Figure A1) and we take advantage of this variation in the first nine months of the second Trump administration to categorize areas into treated and control areas. In particular, treated areas are those that experienced a sudden, large increase in ICE arrests relative to the baseline non-citizen population in the area.[6] We define treatment as beginning the month an area experiences a 1-month change of roughly 50 arrests per 100,000 non-citizens, or an approximate doubling of arrests per 100,000 non-citizens. Control areas are those that did not experience similar large increases in arrests over this period.

We show the results of this classification of areas in Figure 2.[7] The bold borders in this figure show the boundaries of the state and sub-state geographic areas we use in our

[6]We use the 2023 5-year ACS estimates to calculate the baseline number of non-citizens in each area (U.S. Census Bureau, 2023).

[7]There are a few geographic areas we cannot precisely identify in the CPS data, which are shaded gray and are omitted from our analysis.

analysis. Using this classification, we implement event study and difference-in-differences models to estimate the labor market impact of heightened ICE activity.

Specifically, we estimate the following event study model:

$$Y_{amy} = \alpha + \sum_{\tau=-14,\tau\neq-2}^{6} \beta_\tau 1(T_\tau = 1)_{amy} + \mu_a + \phi_m + \gamma_y + \epsilon_{amy} \quad (1)$$

where a is the geographic area, m is the month, and y is the year. The key set of regressors are $1(T_\tau = 1)$, which are indicator variables for whether the observation is τ months before or after treatment (the sudden increase in ICE arrests). We bin event time into two month groups to maximize precision. Additionally, we estimate effects for the first and last event time, but do not plot these in the figure since they are estimated on unbalanced samples. We account for time-invariant geographic differences with area fixed-effects (μ_a). And, we account for seasonality and national shocks over time with calendar month fixed-effects (ϕ_m), and year fixed-effects (γ_y). We cluster standard errors by geographic area. We also estimate difference-in-differences versions of this model where we replace the event time dummies with a post period dummy variable that varies by area and time. Finally, we demonstrate our estimated effects are similar when using the method proposed by Callaway and Sant'Anna (2021) to account for potential bias induced by differential treatment timing across areas.

The main identifying assumptions are that there were no differential trends across the treated and control areas in this time period and that there are no other changes over time that differentially impacted treated compared to control areas. These assumptions are especially important to test given the many changes to policy in 2025. To evaluate the validity of the first assumption, we plot event study graphs to directly examine pre-trends. We also show robustness to including demographic and local economic condition controls[8] and a control for area-specific exposure to Trump's tariffs.[9] Finally, we show there are no similar differential changes across treated and control areas in the first Trump administration as a placebo test.

[8]The demographic controls include the baseline percent of the population that are non-Hispanic white, non-Hispanic black, Hispanic, have less than a high school education, a high school education, some college, and a college education or more. Demographic controls also include controls for the average age and age squared. Economic controls include baseline estimates of the unemployment rate and the percentage of the population that work in agriculture, manufacturing, or construction sectors. All of these baseline area-specific demographic and economic characteristics are then interacted with time fixed effects.

[9]This is the estimated amount each state paid in tariffs (Savitsky, Shane, 2026) interacted with a binary post period variable equal to one when tariffs went into effect (York, Erica and Durante, Alex, 2026).

Results

Effects on ICE Arrests

We begin by confirming the "first-stage" using on our classification of treated and control areas in Figure 3. In our analysis of economic outcomes we estimate reduced form models, but this first stage exploration helps us understand what our treatment classifications translates to in terms of number of arrests, to interpret the reduced form findings. The economic impacts we document may not only operate through the mechanism of arrests, we are simply using arrests as a proxy for intensified immigration enforcement.

Treated areas experience a large and sudden increase in monthly arrests, with no differential pre-trends in arrests in the year prior to the sudden increase. In levels, we find an increase of roughly 200 daily arrests per month in the treated areas relative to control areas. In each figure, we report results from the corresponding difference-in-differences model. Compared to the mean arrest rate measured in the pre-period, this is a 114% increase in arrests in treated areas compared to control areas. We find similar results when adjusting for the baseline area-specific non-citizen population—there is a significant increase of 94 arrests per 100,000 non-citizens in treated compared to control areas.

In panels (c)-(f), we explore heterogeneous effects by sex, since most of those arrested by ICE are male. Indeed, we find that 85% of the effect on total ICE arrests is driven by arrests of males. Though, the estimate for arrests of females is also statistically significant. Given this, we hypothesize that the labor market impacts may be concentrated among male likely undocumented workers.

Effects on Likely Undocumented Immigrants' Labor Market Outcomes

We explore the labor market effects on likely undocumented immigrants who remain in the U.S. and work in the likely affected sectors in Figure 4. The left-hand side panels show the results on employment last week, and the right-hand-side panels show the results on hours worked last week. As above, we show results for both sexes pooled, the male subsample and the female subsample.

We find no differential pre-trends for any sample or outcome, supporting our identifying assumptions. Moreover, we show likely undocumented immigrants remaining in the U.S. are less likely to work in the face of heightened immigration enforcement. These effects appear to grow over time, while the first stage effects appeared quickly and remained relatively constant over time. The differences in dynamics may be because it takes time for the chilling effect

to fully manifest as news spreads throughout the community about ICE activity.

The corresponding difference-in-differences results show a significant reduction in employment for all likely undocumented immigrants in these likely affected sectors of 3.4 percentage points, or 4%. Note, the mean employment rates in our samples are higher than published total employment to population ratios because we restrict on those who are currently working, unemployed, or have worked within the last 5 years, in order to observe the sector of employment.

Looking at the subsequent panels, the effects are larger for males, as expected. In particular, there is a significant reduction in male employment of 4.6 percentage points, or 5%, and a significant reduction in hours worked of 2 hours per week, or 5%. The point estimates for the female subsample are not of consistent sign and are insignificant.

We also explore whether there are effects on the hourly wages or weekly earnings of likely undocumented immigrants in these high-impact sectors. We might expect, if there is a large reduction in work among likely undocumented immigrants, that firms pay other immigrants more to attract new workers. On the other hand, there may be changes in the composition of workers, which affect average pay, and, firms may only offer higher wages to U.S.-born workers, so the expected effect is ambiguous. There is no evidence of a consistent negative or positive effect on pay among likely undocumented workers in the likely affected sectors shown in Appendix Table A1.

We estimate effects for other sectors beyond our main four. Focusing on the results among males, in Figure 5, we plot the relationship between the estimated effect on likely undocumented immigrants by sector (vertical axis) and the baseline share of undocumented workers by sector (horizontal axis). Most estimates are not statistically significant given small sample sizes, so we focus more on overall patterns in this analysis.

As expected, in general, there is a stronger negative effect in sectors with more baseline undocumented workers. The largest negative effect is for workers in Construction, which has over 15% of undocumented workers, whereas the effect in Transportation, Finance, and Information is small or positive, and these sectors have less than 4% of undocumented immigrants each.

We next explore whether broadening our definition of likely impacted sectors changes the results. In Figure 6 we add in all the other sectors with a high concentration of undocumented workers (more than 4%), regardless of the sex composition of workers in panels (c)-(d). The results are similar to our baseline analyses, though with slightly smaller co-

efficients that are not always statistically significant. In panels (e)-(f), we look at only sectors with a high concentration of female undocumented workers to see if we are missing any important impacts on females. The point estimates are positive for all groups of likely undocumented immigrants in these sectors, but none of the estimates are statistically significant, and are not significant in the equivalent difference-in-differences model.

Robustness Checks

In Appendix Figures A3-A4, we show the robustness of these main findings to several checks. First, we show robustness to using alternative weights created by Jed Kolko in the hollow pink diamonds. Next, we add baseline area-specific demographics and economic conditions each interacted with year-by-month fixed effects in the purple squares and teal triangles, respectively. Again, the results are robust to these additions. Since we use a staggered rollout design, we test robustness to using the Callaway and Sant'Anna's estimator in the yellow x's. While the standard errors increase, the qualitative conclusions do not change. We include controls for area-specific changes in tariffs in the solid blue diamonds to account for changes in trade policy in the second Trump administration and our results are robust to this control. Finally, the hollow green circles show the results when weighting the regression by the baseline number of non-citizens, which affects the standard errors, but not the overall conclusions.

We also conduct a placebo test where we use CPS data from 2016-2017 around the first Trump inauguration. We assign placebo treatment to areas in 2017 as if they were treated the same as they were in 2025. If there is something unique about these areas while Trump is in office, then we would expect to find effects that mirror our main results. We do not find any such effects, and instead the coefficients are all close to zero and insignificant as shown in Appendix Figure A5.

Finally, in Appendix Figure A6, we show that our results are similar if we further restrict to non-citizens or those who report Hispanic ethnicity.[10]

Effects on U.S.-Born Workers

Turning to the U.S.-born sample, we explore the impact on the same two labor market outcomes in the likely impacted sectors. Given the analysis above, if U.S.-born workers are substitutes for likely undocumented immigrants in the labor market, we would expect to see an increase in labor supply among U.S.-born workers in these sectors. On the other

[10]Due to small sample sizes we do not restrict to both Hispanic and non-citizens at the same time.

hand, if they are complements in the labor market, or there are other ways through which immigration enforcement reduces labor demand in these sectors, then the effect would be negative. Overall, we find null effects on U.S.-born workers' outcomes, shown in panels (a)-(b) of Figure 7. On the full sample, we can rule out increases in employment of larger than 0.1 percentage points and increases in hours worked of 0.14 per week. We also find no impact on U.S.-born workers' pay as shown in Appendix Table A1.

To further investigate the mechanisms behind the effects on U.S.-born workers, we split the sample by sex, educational attainment, and sector. In panel (c) of Figure 7, there is a marginally significant negative effect of U.S.-born male workers employment of 0.6 percentage points or 0.64% relative to the baseline mean. There are no similar negative effects for U.S.-born female workers. Looking next by educational attainment, this negative effect for U.S.-born males is driven by those with a high school degree or less, as shown in Figure 8. For U.S.-born males with a high school degree or less, there is a 1.3% reduction in the employment rate relative the baseline mean. We do not find this same pattern among U.S.-born workers with more than a high school education. Instead, we find null results both overall and when splitting results by sex.

Since these complementarities are likely to occur within sector, we next plot sector-level scatter plots with the effect on likely undocumented male immigrants on the vertical axes and the effect on U.S.-born males on the horizontal axes in Figure 5. Panel (b) shows the results for all U.S.-born males, panel (c) for U.S.-born males with a high school degree or less, and panel (d) for U.S.-born males more than a high school degree. For the full sample and those with less education, there is a clear positive relationship across sectors between the reduction in employment for likely undocumented immigrants and U.S.-born workers. That is, the sectors where the effects on male likely undocumented workers are the most negative, are also the sectors where we see negative effects for U.S.-born male workers. Thus sector-specific complementarities are likely driving at least some of the negative spillover effects onto U.S.-born workers.

To further understand if this is driven by complementarities between undocumented and U.S.-born workers in these likely affected sectors, we explored the top occupations in each sector and demographic group. There is some overlap in the top occupations in likely affected industries for the likely undocumented and U.S.-born individuals with an educational attainment of high school or less, but the overall composition of occupations is different between demographic groups. For example, in construction, the most common occupation for both likely undocumented and U.S.-born male workers with a high school degree or less is construction laborer. But, roughly 40% of likely undocumented males work in this

occupation, compared to only about 20% of U.S.-born males with a high school degree or less.

Finally, we conduct the same robustness checks and placebo test as we did for the results above in Appendix Figure A5 and A7, and these support our main findings on the U.S.-born sample.

Magnitudes

We calculate the implied change in the number of male likely undocumented workers from our estimated coefficients. With the American Community Survey, we calculate the average baseline number of male likely undocumented workers in the likely affected industries and the treated areas in 2024. We then multiply this by the change in the employment rate from our difference in difference results (.05*151,472). This implies that there are roughly 7,574 fewer likely undocumented male workers in these high impact sectors in the average treated area. For arrests, we found a cumulative increase in male arrests of 1,200 in treated areas. Thus, for every ICE arrest, 6 male likely undocumented workers stop working. This very likely underestimates the total effect of heightened enforcement on the number of immigrant workers in the U.S. since we only look at immigrants who remain in the U.S.

Comparing this to the prior literature, the chilling effect in Trump 2.0 is larger than in past mass deportation campaigns. In particular, (East et al., 2023) found that in the first Obama administration, for each person detained, 2.3 likely undocumented workers stopped working due to chilling effects. This larger chilling effect is consistent with the increasingly indiscriminate nature of ICE activity in Trump 2.0 (East et al., 2026).

Using a similar procedure as above, our estimates imply there is a reduction in the number of male U.S.-born workers with a high school degree or less of 1,200 per area. Thus, for every six lost male likely undocumented workers, there is a loss of one male U.S.-born worker.

Conclusion

In this paper, we provide the first, national, quasi-experimental evaluation of Trump 2.0's immigration enforcement surge on the labor market outcomes of different demographic groups. We show no evidence of positive effects of the labor market outcomes of U.S.-born workers in immigrant-heavy industries. If anything these U.S.-born workers are harmed as a result, likely due to complementarities in production between the different jobs undocumented immigrants and U.S.-born workers typically take.

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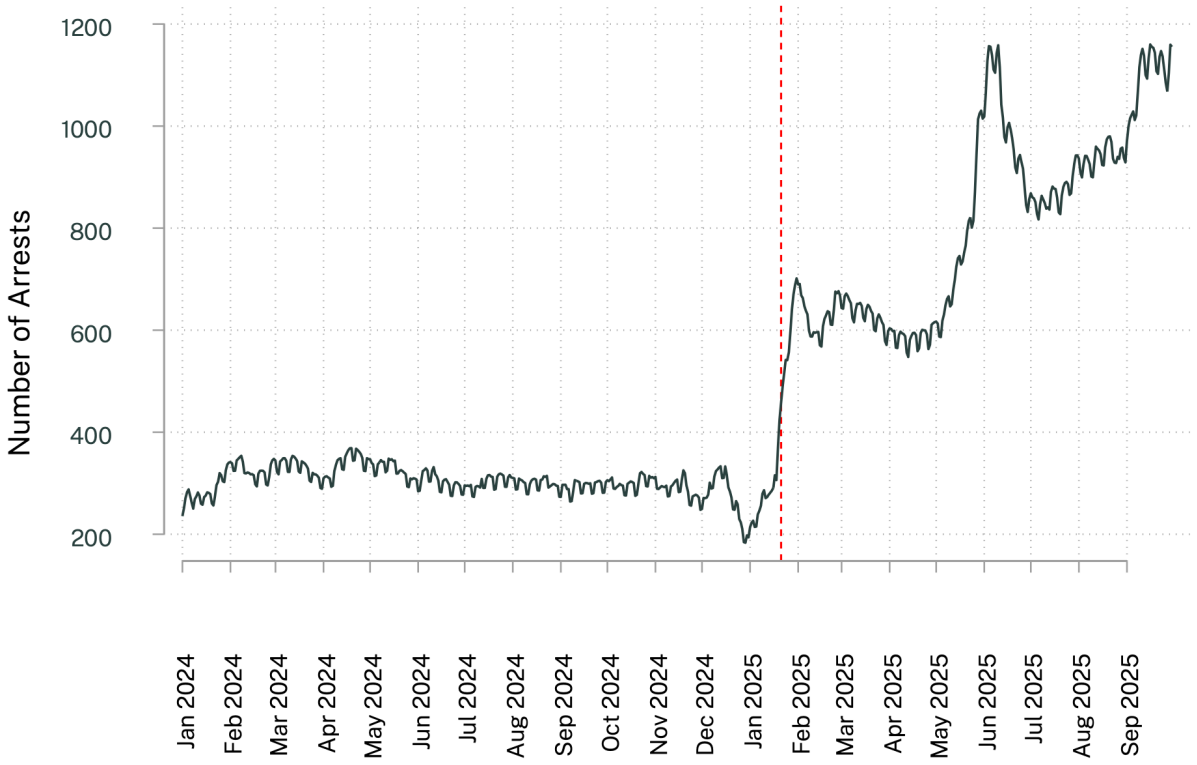
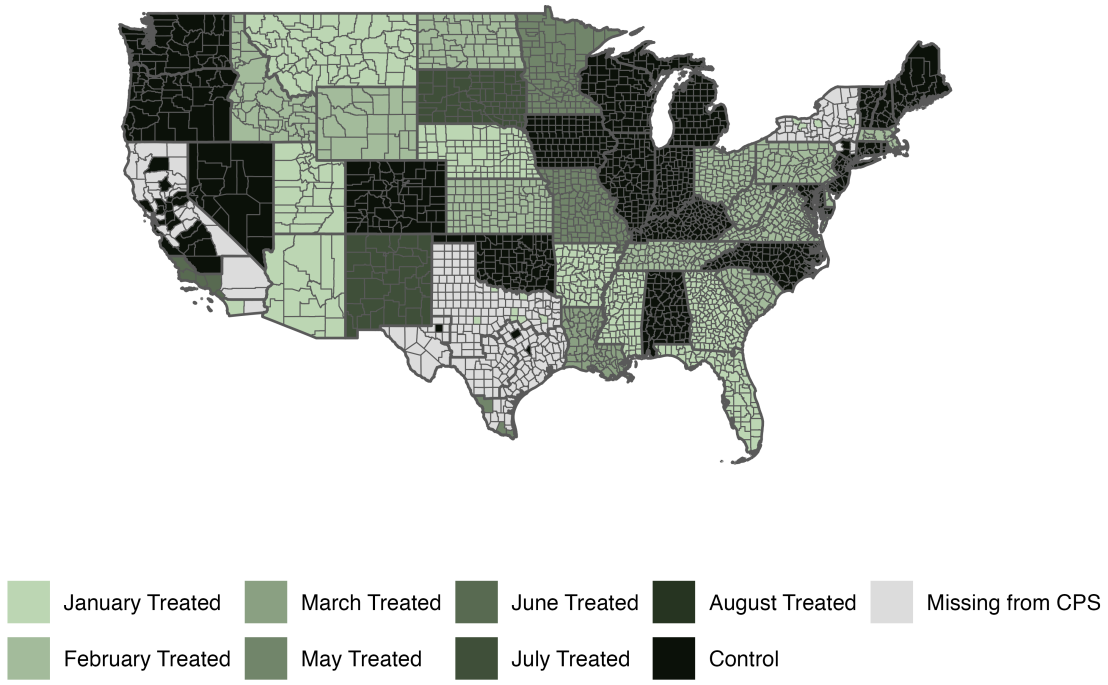
Figure 1: Total Daily Arrests



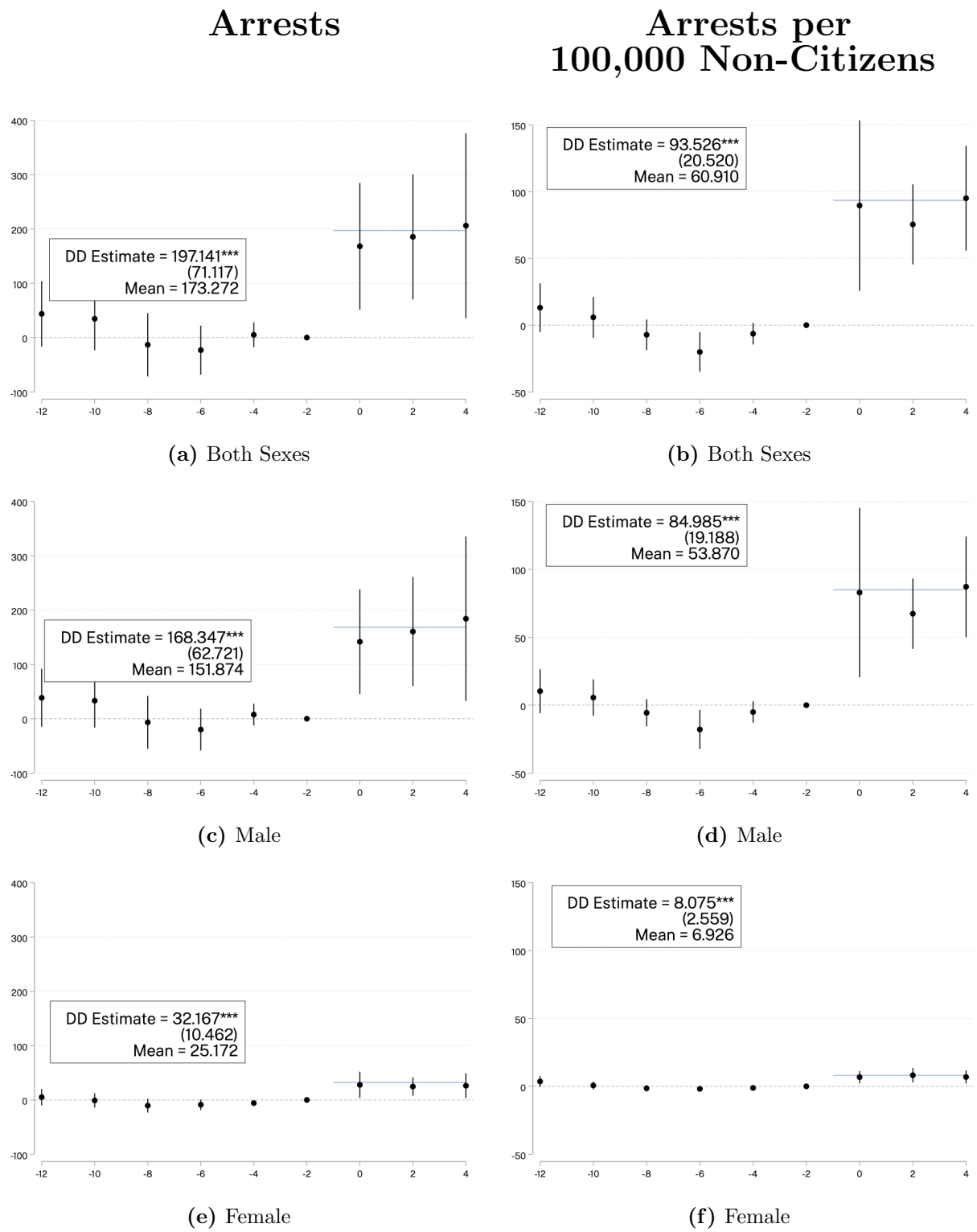
Notes: Uses ICE ERO administrative arrest data from October 2023 through October 2025. Total arrests are aggregated by date, and reported as a 15-day moving average. The vertical red line indicates the date of Trump's second inauguration, January 20, 2025.

Figure 2: Treatment and Control Classification



Notes: Uses ICE ERO administrative arrest data from October 2023 through October 2025. Area of Responsibility boundaries are from ICE ERO. Treatment status is determined based on changes in arrests in 2025 shown in Figure A1. Grey areas are dropped from the sample because they are missing county and MSA information from the CPS, and we thus cannot identify the AOR observations in these counties fall under.

Figure 3: Event Study Estimates on Arrests



(a) Both Sexes

(b) Both Sexes

(c) Male

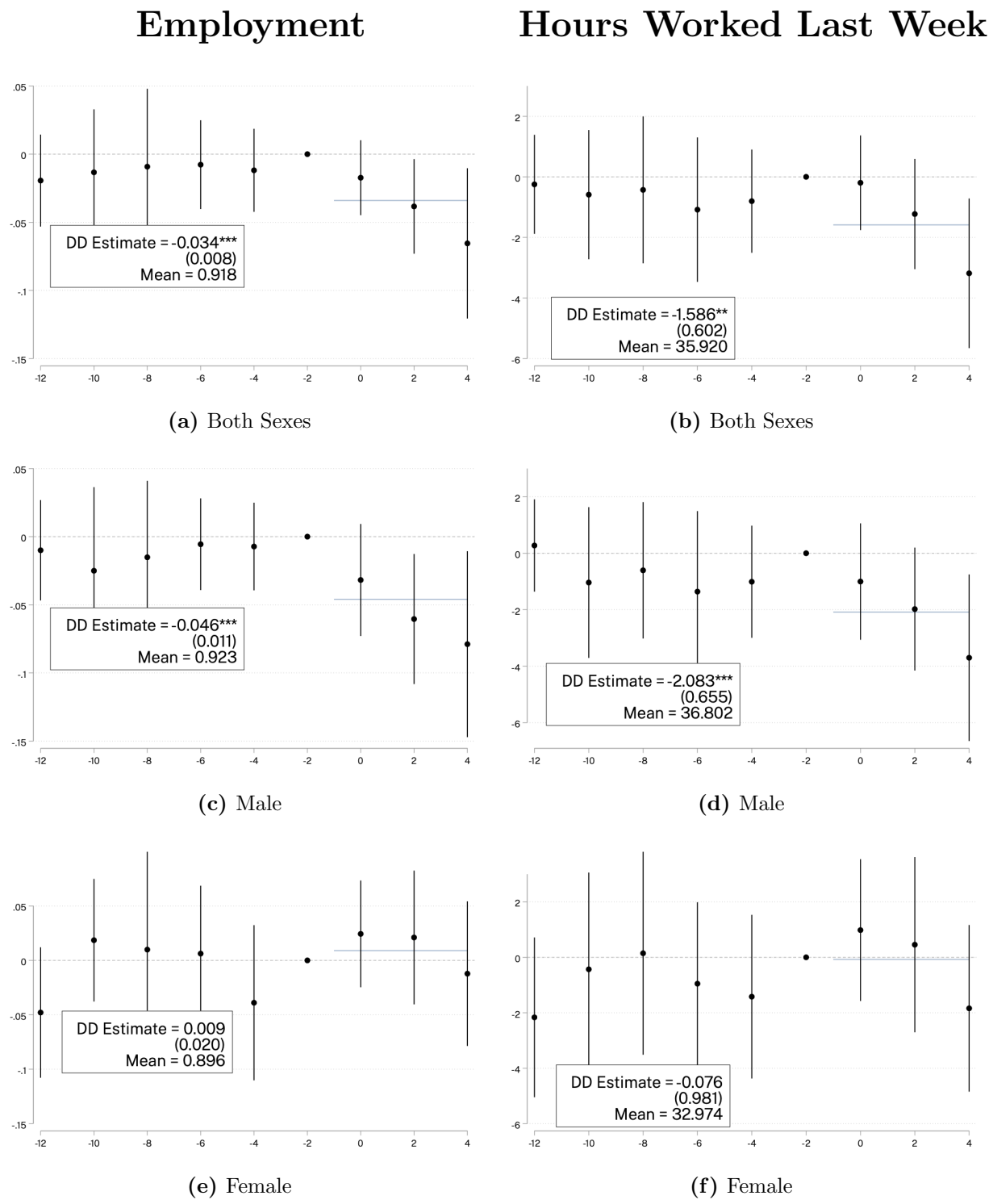
(d) Male

(e) Female

(f) Female

Notes: Uses ICE ERO administrative arrest data from October 2023 through October 2025. Black dots show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed effect. The horizontal axis denote the number of months relative to the first month of the sudden increase in arrests in the treated areas. Data is collapsed to the area, month and year level using the final survey weights, and regressions are run unweighted on this collapsed data. * p<0.10, ** p<0.05, *** p<0.01

Figure 4: Event Study Estimates on Likely Undocumented Immigrants' Labor Market Outcomes in Likely Affected Sectors



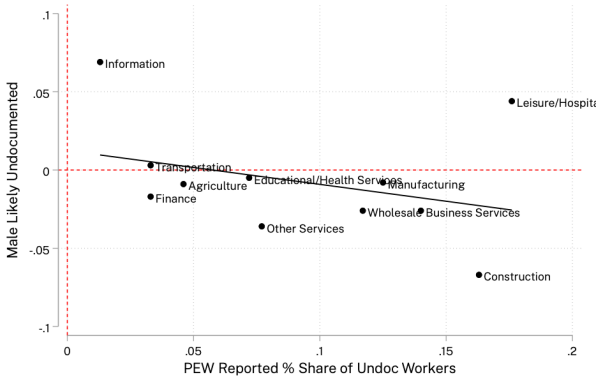
(a) Both Sexes **(b)** Both Sexes

(c) Male **(d)** Male

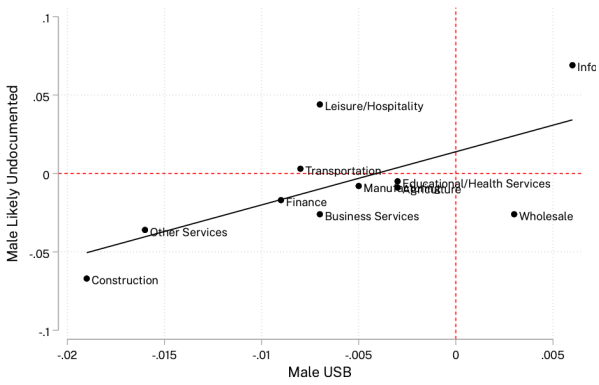
(e) Female **(f)** Female

Notes: Economic outcome data comes from the basic monthly CPS from October 2023 through November 2025. Black dots show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed effect. The horizontal axis denote the number of months relative to the first month of the sudden increase in arrests in the treated areas. Data is collapsed to the area, month and year level using the final survey weights, and regressions are run unweighted on this collapsed data. The DD estimate reports the coefficients from a corresponding difference-in-difference model. The likely undocumented sample includes foreign-born individuals between the ages of 20 and 64 who have a high school education or less. Likely affected sectors consists of agriculture, construction, manufacturing, and wholesale. * p<0.10, ** p<0.05, *** p<0.01

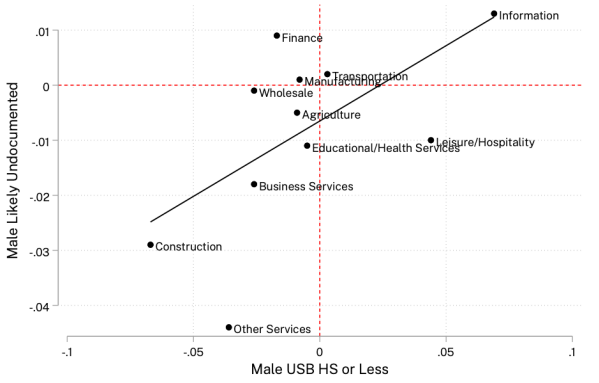
Figure 5: Male Difference-in-Differences Estimates by Sector



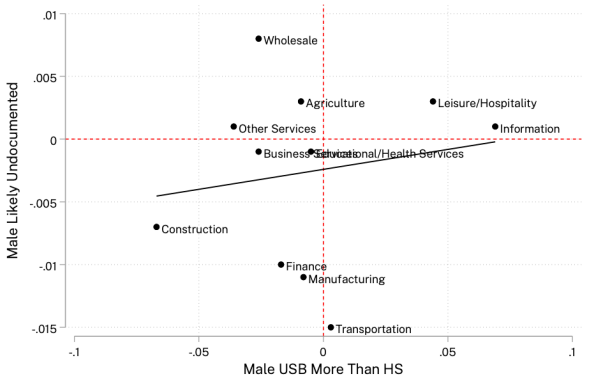
(a) Male Likely Undocumented vs Reported PEW Estimated Share Undocumented



(b) Male Likely Undocumented vs U.S.-Born



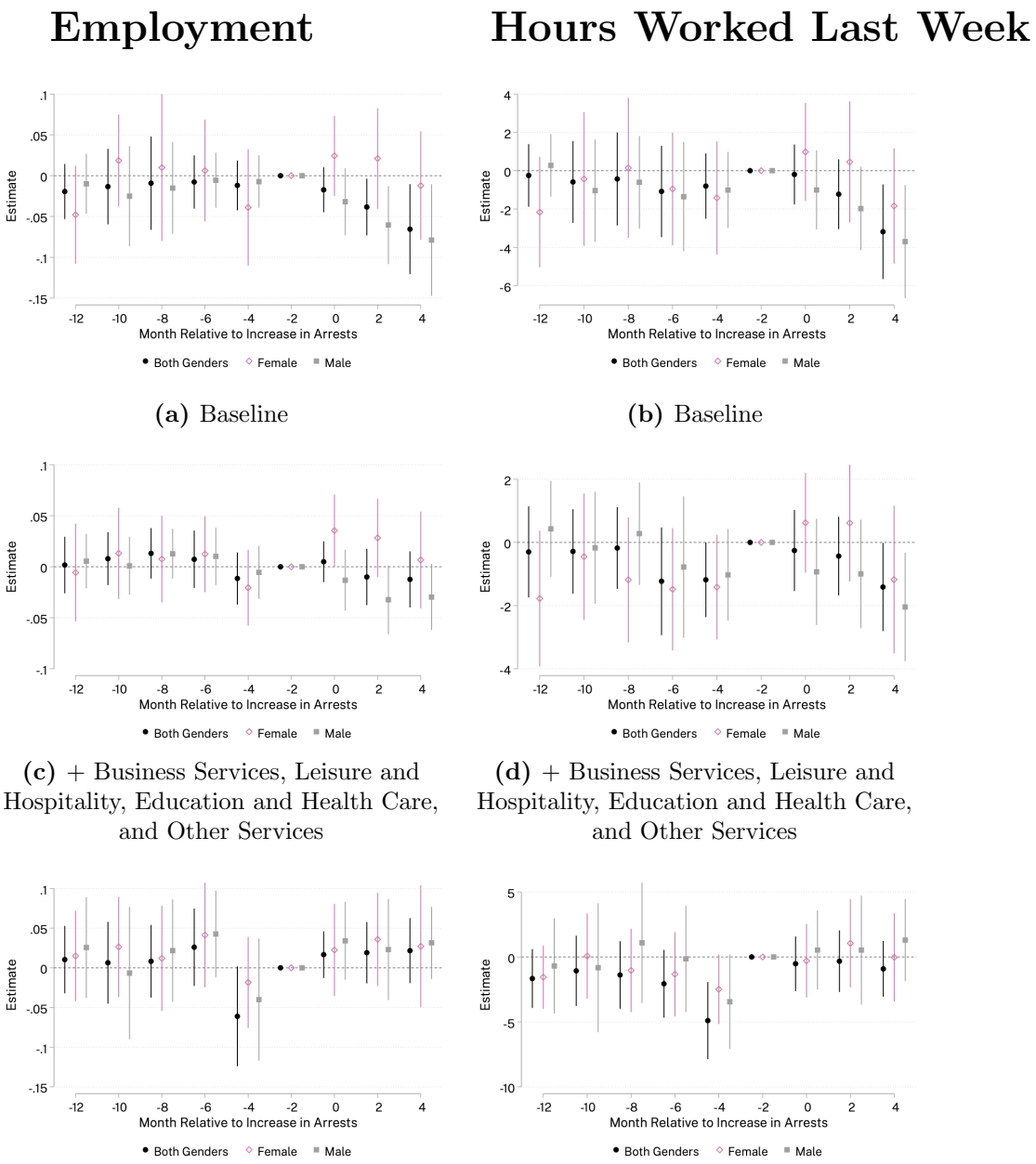
(c) Male Likely Undocumented vs U.S.-Born HS or Less



(d) Male Likely Undocumented vs U.S.-Born More Than HS

Notes: Economic outcome data comes from the basic monthly CPS from October 2023 through November 2025. Black dots report the DD coefficients of industry specific regressions for varying samples, specified on the x and y-axes. DD models include separate year and month fixed effects as well as an area fixed effect and are weighted using provided final survey weights. Results from the mining industry is dropped from this visual due to its small sample size. The likely undocumented sample includes foreign-born individuals between the ages of 20 and 64 who have a high school education or less. The U.S.-born sample includes all U.S.-born individuals between the ages of 20 and 64.

Figure 6: Event Study Estimates on Likely Undocumented Immigrants' Labor Market Outcomes Including Alternate Affected Sectors



(a) Baseline

(b) Baseline

(c) + Business Services, Leisure and Hospitality, Education and Health Care, and Other Services

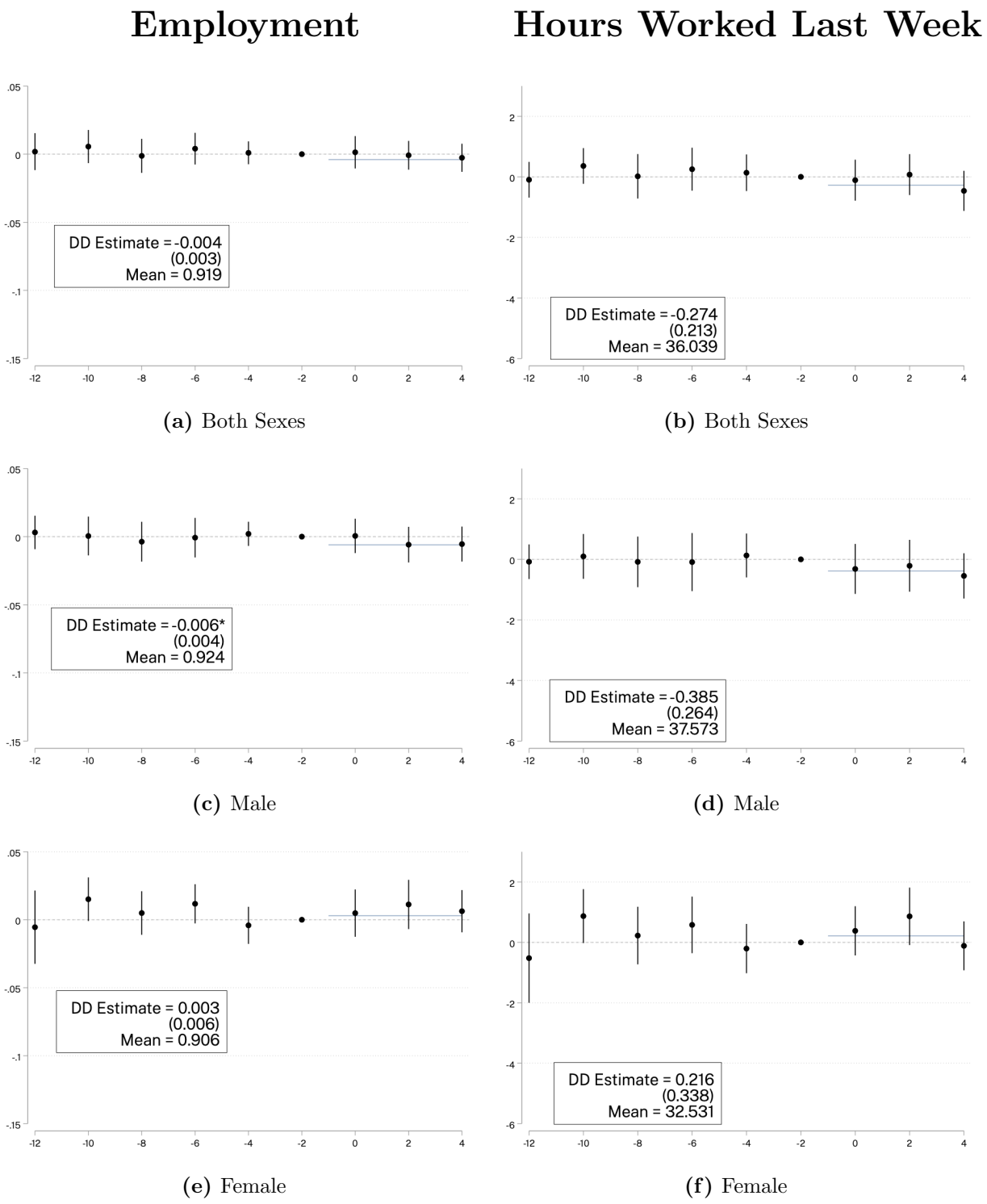
(d) + Business Services, Leisure and Hospitality, Education and Health Care, and Other Services

(e) Education and Health Care, Leisure and Hospitality, and Other Services

(f) Education and Health Care, Leisure and Hospitality, and Other Services

Notes: Economic outcome data comes from the basic monthly CPS from October 2023 through November 2025. Points show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed effect. The horizontal axis denote the number of months relative to the first month of the sudden increase in arrests in the treated areas. Data is collapsed to the area, month and year level using the final survey weights, and regressions are run unweighted on this collapsed data. The likely undocumented sample includes foreign-born individuals between the ages of 20 and 64 who have a high school education or less.

Figure 7: Event Study Estimates on U.S.-Born Labor Market Outcomes in Likely Affected Sectors



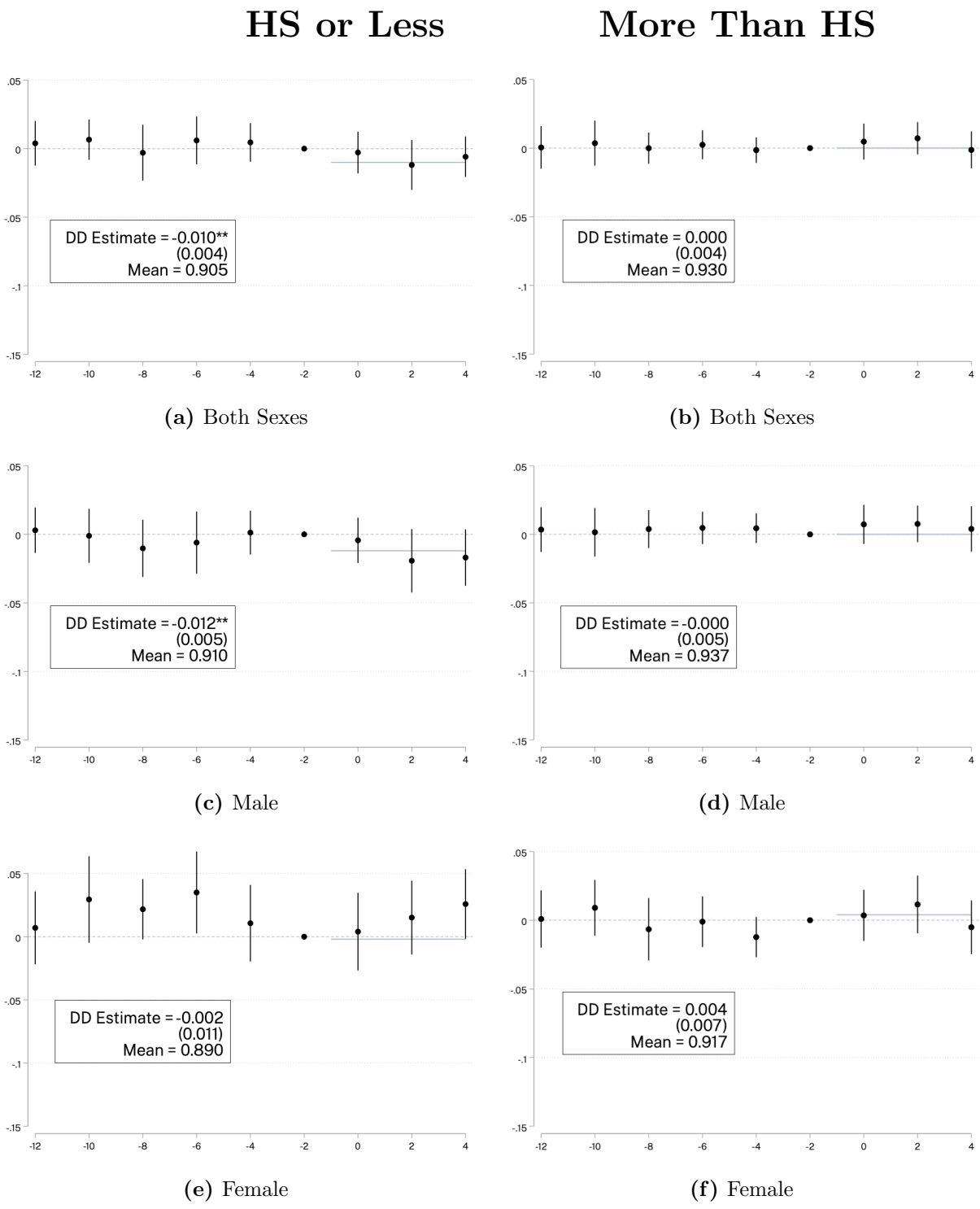
(a) Both Sexes **(b)** Both Sexes

(c) Male **(d)** Male

(e) Female **(f)** Female

Notes: Economic outcome data comes from the basic monthly CPS from October 2023 through November 2025. Black dots show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed effect. The horizontal axis denote the number of months relative to the first month of the sudden increase in arrests in the treated areas. Data is collapsed to the area, month and year level using the final survey weights, and regressions are run unweighted on this collapsed data. The DD estimate reports the coefficients from a corresponding difference-in-difference model. The U.S.-born sample includes all U.S.-born individuals between the ages of 20 and 64. Likely affected sectors consists of agriculture, construction, manufacturing, and wholesale. * p<0.10, ** p<0.05, *** p<0.01

Figure 8: Event Study Estimates on U.S.-Born Employment in Likely Affected Sectors by Education Level



(a) Both Sexes **(b)** Both Sexes

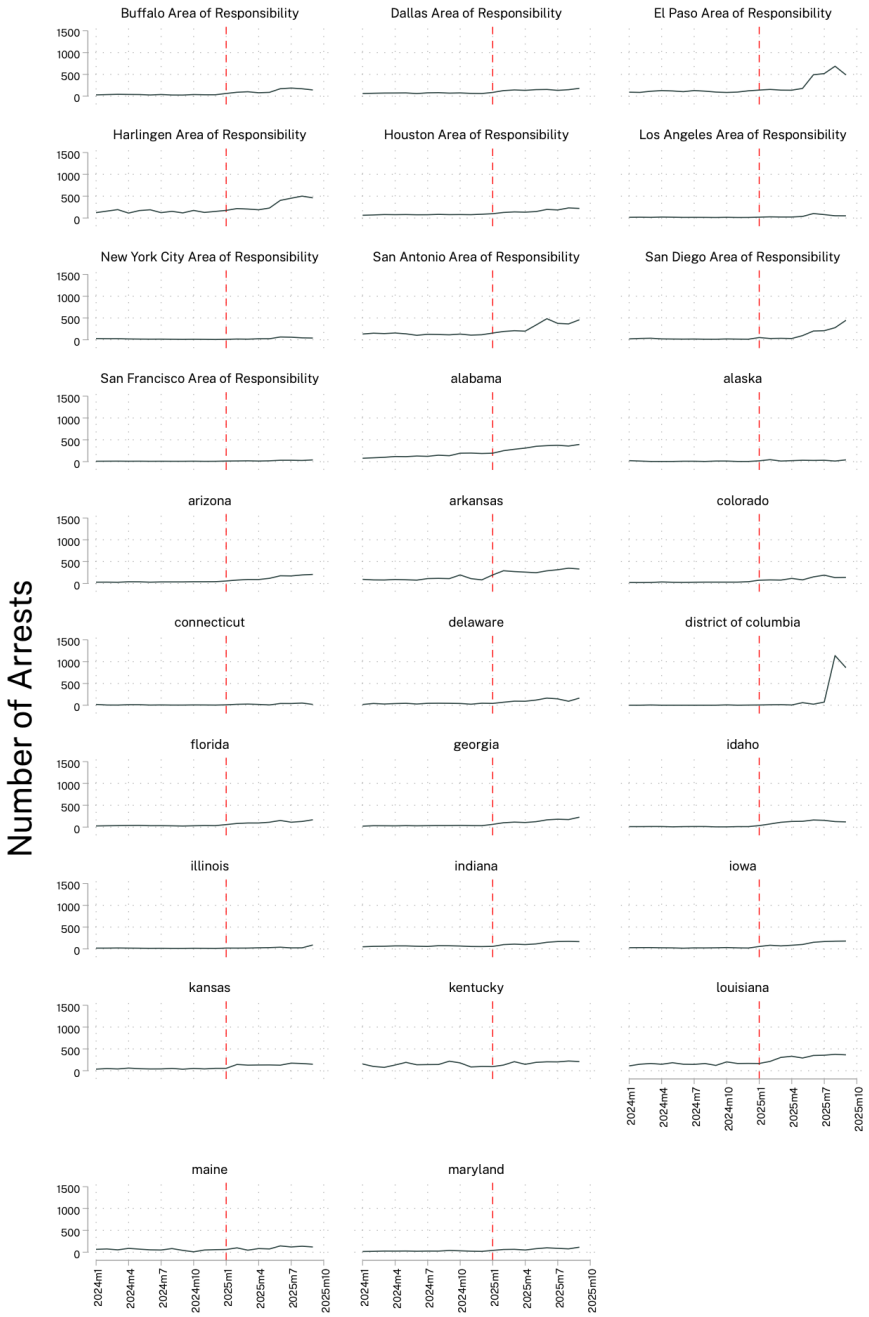
(c) Male **(d)** Male

(e) Female **(f)** Female

Notes: Economic outcome data comes from the basic monthly CPS from October 2023 through November 2025. Black dots show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed effect. The horizontal axis denote the number of months relative to the first month of the sudden increase in arrests in the treated areas. Data is collapsed to the area, month and year level using the final survey weights, and regressions are run unweighted on this collapsed data. The DD estimate reports the coefficients from a corresponding difference-in-difference model. The U.S.-born sample includes all U.S.-born individuals between the ages of 20 and 64. Likely affected sectors consists of agriculture, construction, manufacturing, and wholesale. * p<0.10, ** p<0.05, *** p<0.01

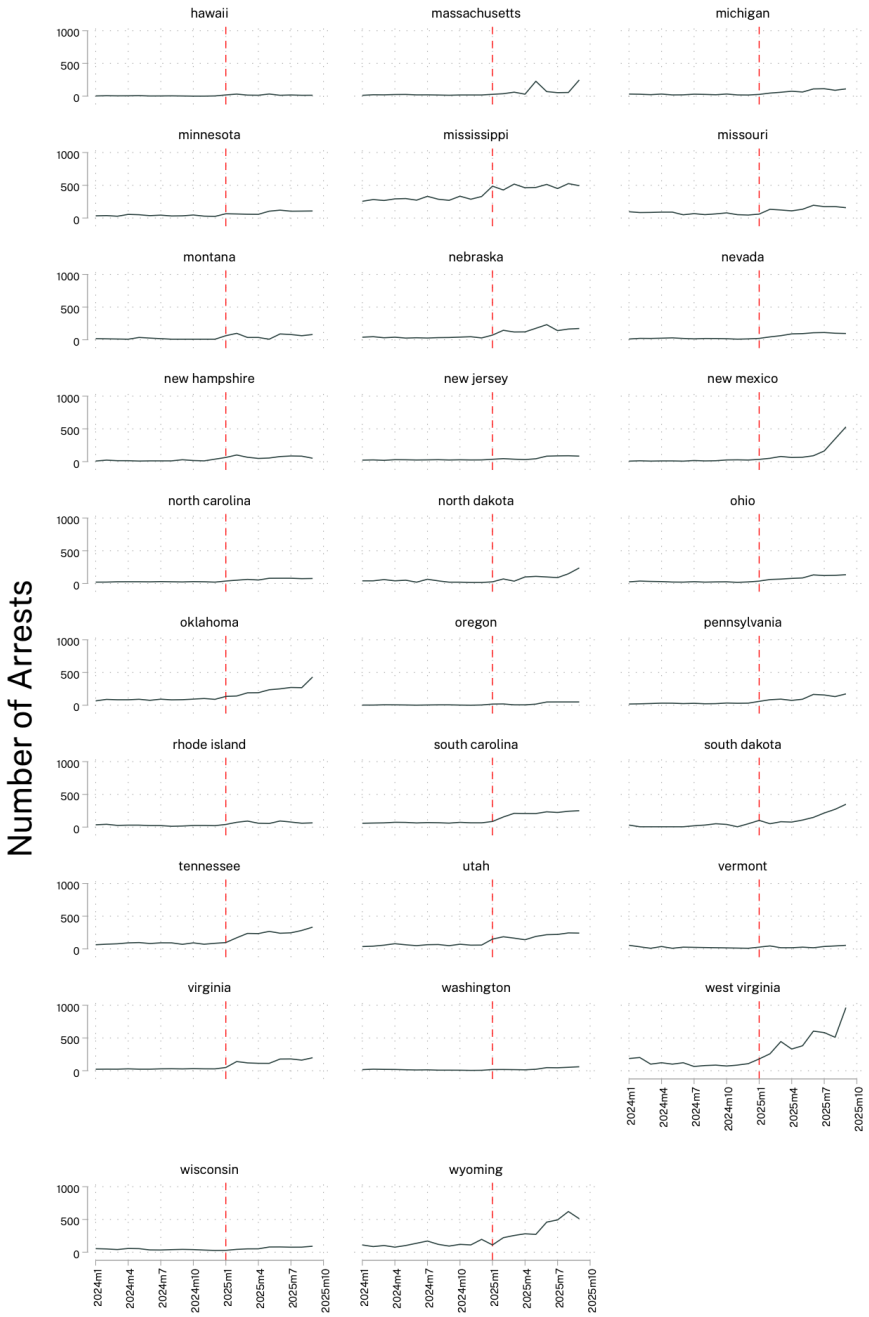
A Appendix

Figure A1: Total Daily Arrests per 100,000 Non-Citizens by Area



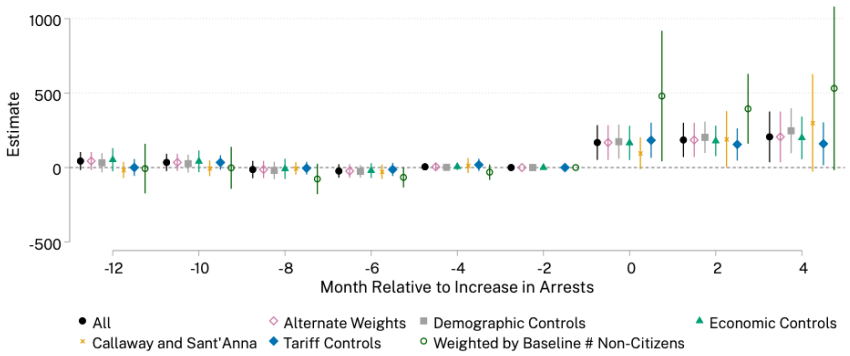
Notes: Uses ICE ERO administrative arrest data from October 2023 through October 2025. Area of responsibility boundaries come from ICE ERO. Arrests are reported as the monthly total in the respective AOR. The vertical red line indicates the date of Trump's second inauguration, January 20, 2025.

Figure A2: Total Daily Arrests per 100,000 Non-Citizens by Area, *continued*

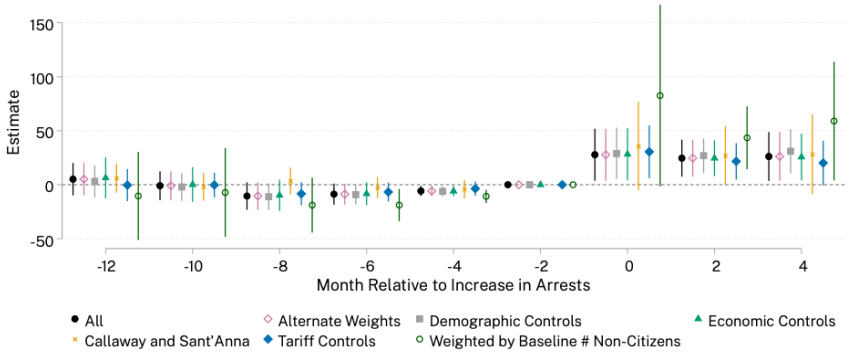


Notes: Uses ICE ERO administrative arrest data from October 2023 through October 2025. Area of responsibility boundaries come from ICE ERO. Arrests are reported as the monthly total in the respective area. The vertical red line indicates the date of Trump's second inauguration, January 20, 2025.

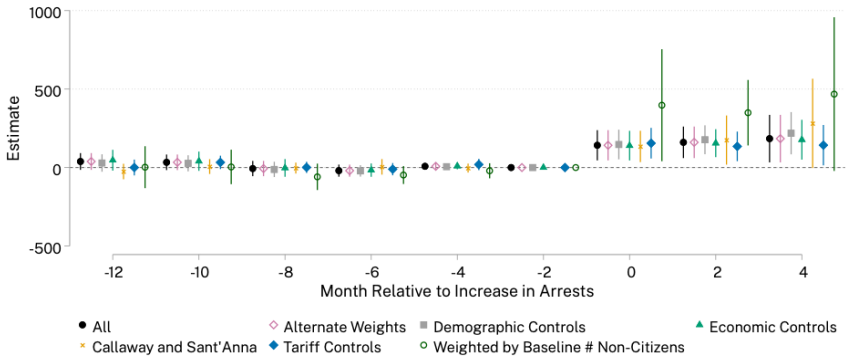
Figure A3: Robustness of Event Study Estimates on Arrests



(a) Both Sexes



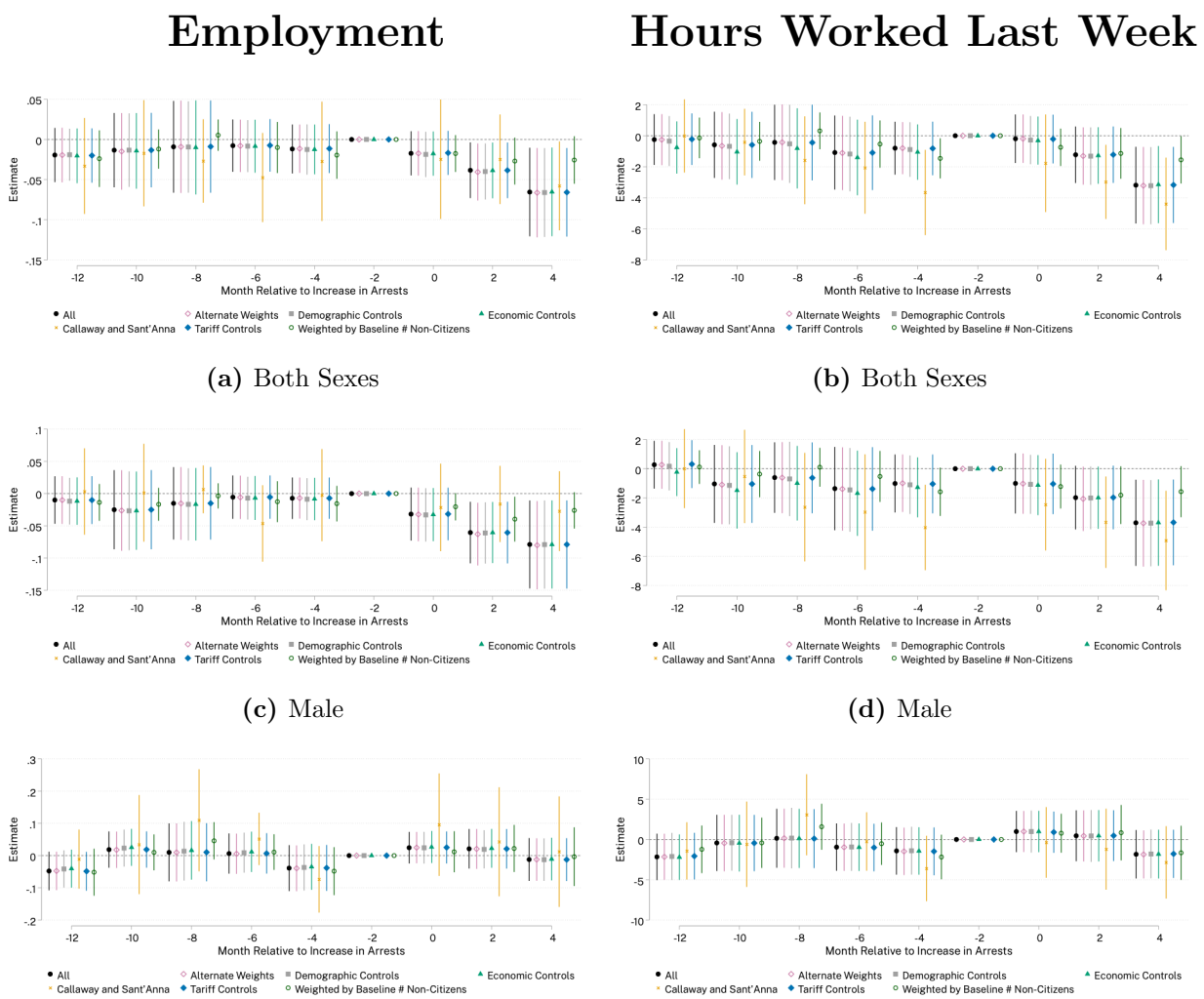
(b) Female



(c) Male

Notes: Uses ICE ERO administrative arrest data from October 2023 through October 2025. Black dots show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed effect. The horizontal axis denote the number of months relative to the first month of the sudden increase in arrests in the treated areas. Data is collapsed to the area, month and year level using the final survey weights, and regressions are run unweighted on this collapsed data. In addition to the baseline results the hollow pink diamonds show results using the alternate weights created by Jed Kolko, purple squares include demographic controls, teal triangles include economic controls, x's show results calculated using Callaway and Sant'Anna estimators, solid blue diamonds include controls for tariffs, and hollow green circles show results of a regression weighted by the baseline number of non-citizens.

Figure A4: Robustness of Event Study Estimates on Likely Undocumented Immigrants in Likely Affected Sectors



(a) Both Sexes

(b) Both Sexes

(c) Male

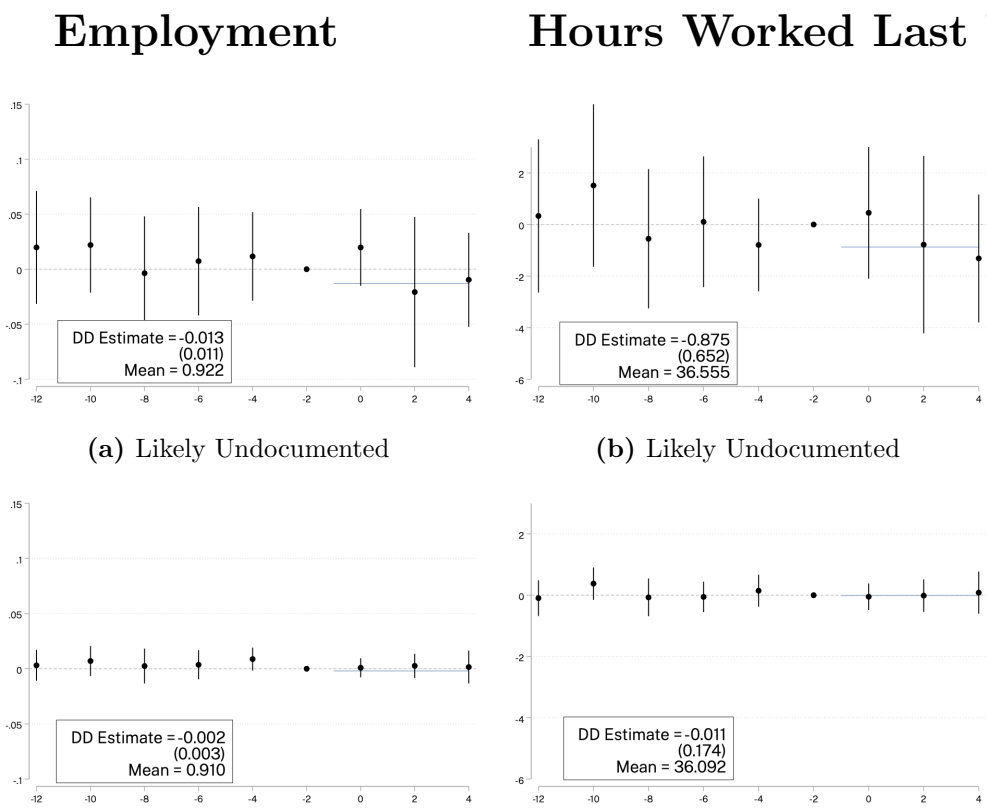
(d) Male

(e) Female

(f) Female

Notes: Economic outcome data comes from the basic monthly CPS from October 2023 through November 2025. Black dots show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed effect. The horizontal axis denote the number of months relative to the first month of the sudden increase in arrests in the treated areas. Data is collapsed to the area, month and year level using the final survey weights, and regressions are run unweighted on this collapsed data. In addition to the baseline results the hollow pink diamonds show results using the alternate weights created by Jed Kolko, purple squares include demographic controls, teal triangles include economic controls, x's show results calculated using Callaway and Sant'Anna estimators, solid blue diamonds include controls for tariffs, and hollow green circles show results of a regression weighted by the baseline number of non-citizens. The likely undocumented sample includes foreign-born individuals between the ages of 20 and 64 who have a high school education or less. Likely affected sectors consists of agriculture, construction, manufacturing, and wholesale.

Figure A5: Event Study Estimates, Trump Term 1 Placebo Test

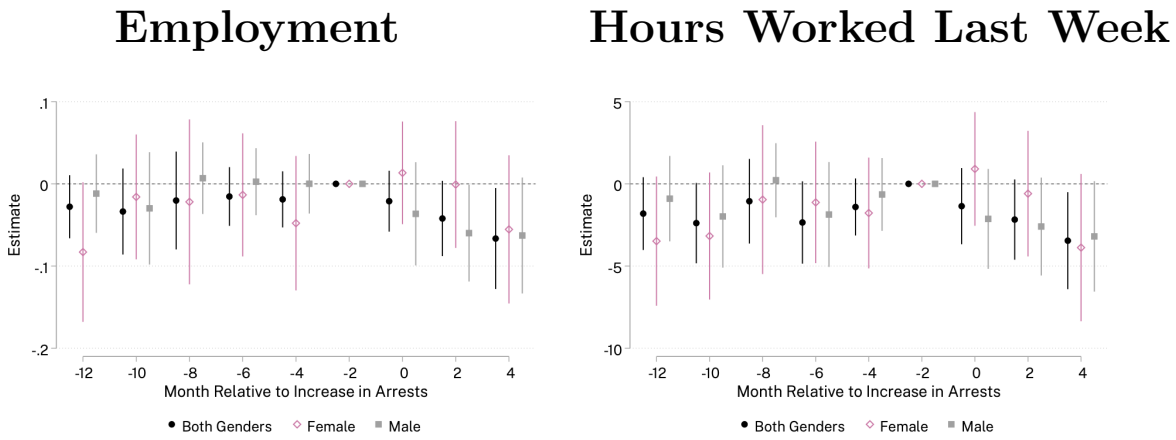


(a) Likely Undocumented **(b)** Likely Undocumented

(c) US-Born **(d)** U.S.-Born

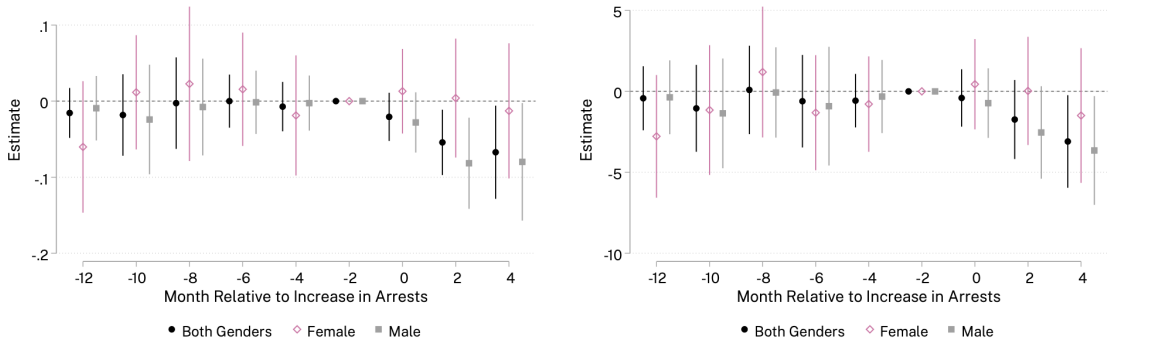
Notes: Economic outcome data comes from the basic monthly CPS from 2015 through 2017. Points show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed effect. The horizontal axis denote the number of months relative to the first month of the sudden increase in arrests in the treated areas. Data is collapsed to the area, month and year level using the final survey weights, and regressions are run unweighted on this collapsed data. The likely undocumented sample includes foreign-born individuals between the ages of 20 and 64 who have a high school education or less. Likely affected sectors consists of agriculture, construction, manufacturing, and wholesale. * p<0.10, ** p<0.05, *** p<0.01

Figure A6: Event Study Estimates on Non-Citizen and Hispanic Likely Undocumented Immigrants in Likely Affected Sectors



(a) Non-Citizen Likely Undocumented

(b) Non-Citizen Likely Undocumented

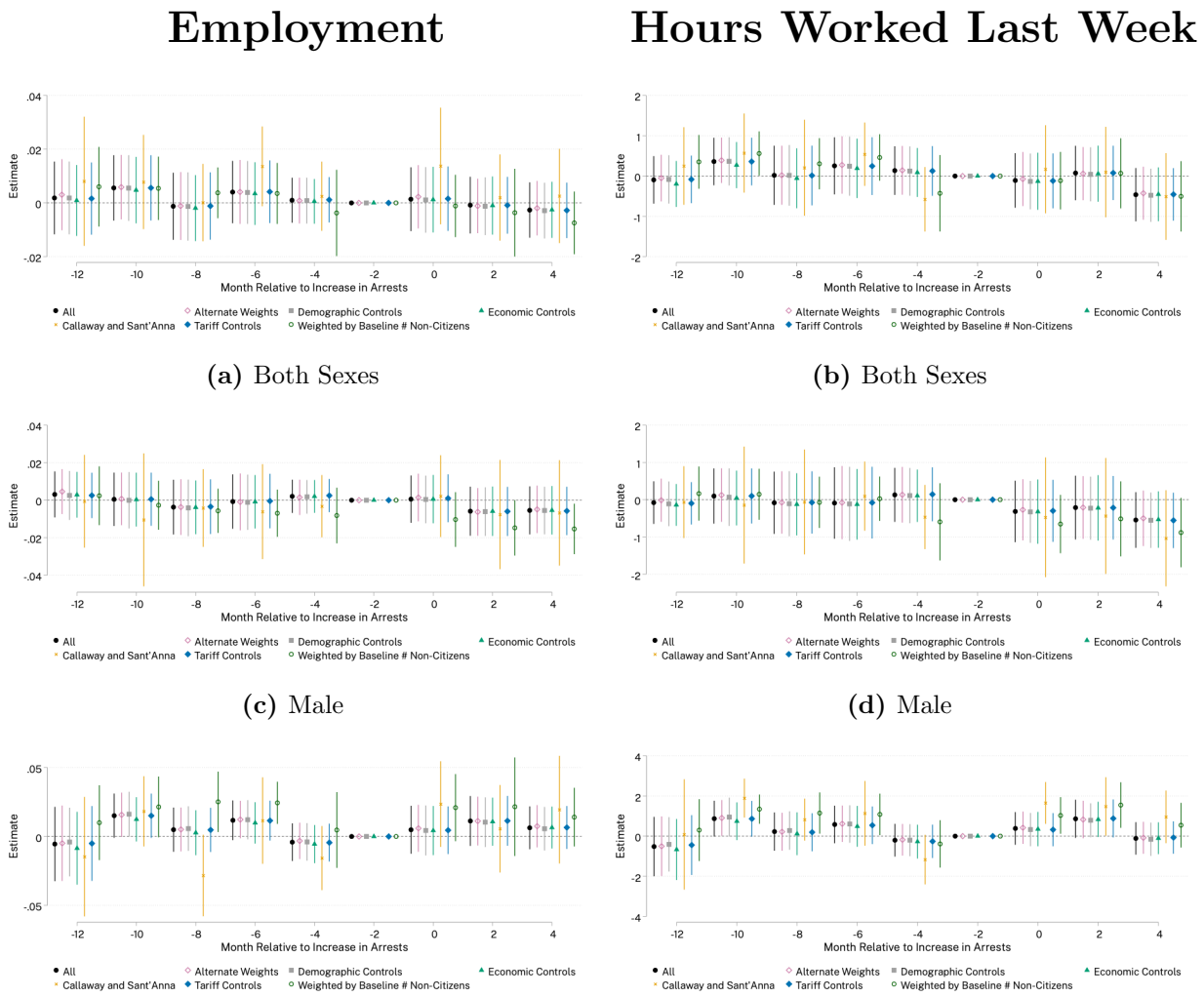


(c) Hispanic Likely Undocumented

(d) Hispanic Likely Undocumented

Notes: Economic outcome data comes from the basic monthly CPS from October 2023 through November 2025. Black dots show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed effect. The horizontal axis denote the number of months relative to the first month of the sudden increase in arrests in the treated areas. Data is collapsed to the area, month and year level using the final survey weights, and regressions are run unweighted on this collapsed data. The likely undocumented sample includes foreign-born individuals between the ages of 20 and 64 who have a high school education or less. Likely affected sectors consists of agriculture, construction, manufacturing, and wholesale.

Figure A7: Robustness of Event Study Estimates on U.S.-Born in Likely Affected Sectors



(a) Both Sexes

(b) Both Sexes

(c) Male

(d) Male

(e) Female

(f) Female

Notes: Economic outcome data comes from the basic monthly CPS from October 2023 through November 2025. Black dots show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed effect. The horizontal axis denote the number of months relative to the first month of the sudden increase in arrests in the treated areas. Data is collapsed to the area, month and year level using the final survey weights, and regressions are run unweighted on this collapsed data. In addition to the baseline results the hollow pink diamonds show results using the alternate weights created by Jed Kolko, purple squares include demographic controls, teal triangles include economic controls, x's show results calculated using Callaway and Sant'Anna estimators, solid blue diamonds include controls for tariffs, and hollow green circles show results of a regression weighted by the baseline number of non-citizens. The U.S.-born sample includes all U.S.-born individuals between the ages of 20 and 64. Likely affected sectors consists of agriculture, construction, manufacturing, and wholesale.

Table A1: Difference-in-Differences Estimates on Likely Undocumented Immigrants and US-Born Earnings in Likely Affected Sectors

a) Both Sexes	Likely Undocumented		U.S.-Born	
	Weekly Earnings	Hourly Wage	Weekly Earnings	Hourly Wage
Post	-28.258	0.409	-22.989	0.108
	(50.797)	(0.749)	(23.623)	(0.286)
Mean Y	906.264	21.384	1261.566	22.855
b) Males	Likely Undocumented		U.S.-Born	
	Weekly Earnings	Hourly Wage	Weekly Earnings	Hourly Wage
Post	-32.966	0.302	-7.685	0.215
	(78.329)	(0.818)	(30.747)	(0.306)
Mean Y	969.921	22.508	1375.626	24.442
c) Females	Likely Undocumented		U.S.-Born	
	Weekly Earnings	Hourly Wage	Weekly Earnings	Hourly Wage
Post	23.211	-0.110	-39.733	0.011
	(33.528)	(0.511)	(29.703)	(0.308)
Mean Y	703.857	18.152	1017.079	19.603

Notes: Economic outcome data comes from the basic monthly CPS from October 2023 through November 2025. Reports the coefficients from a difference-in-differences version of the event study shown in Equation (1). Includes separate year and month fixed effects as well as an area fixed effect. Results are weighted using provided final survey weights. The likely undocumented sample includes foreign-born individuals between the ages of 20 and 64 who have a high school education or less. The U.S.-born sample includes all U.S.-born individuals between the ages of 20 and 64. Likely affected sectors consists of agriculture, construction, manufacturing, and wholesale. * $p<0.10$, ** $p<0.05$, *** $p<0.01$