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LABOR MARKET IMPACTS OF ICE ACTIVITY IN TRUMP 2.0

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ABSTRACT

We provide the first causal, national empirical analysis of the labor market impacts of heightened ICE activity during the second Trump administration. We take advantage of the fact that increases in ICE activity 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 and sudden increases in the number of arrests also see a decrease in work among likely undocumented immigrants who remain in the U.S., compared to areas with smaller increases in arrests. The number of male U.S.-born workers at work also declines in the face of ICE activity, due to a combination of complementarities in production and reductions in economic activity.

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1 Introduction

The second Trump campaign promised, and is on the way to conducting, the largest Immigration and Customs Enforcement (ICE) effort in recent decades (East et al., 2026). A key argument made in support of this is that removing immigrants from the country will create more job opportunities for U.S.-born workers. News articles report that employers are having a hard time finding workers, due to heightened ICE activity, with no indication that U.S.-born workers are taking the jobs left behind (Sainato, 2025; Chadde, 2025). Indeed, the labor market in 2025 was weaker than in 2024, with higher unemployment, slower hiring, and stagnating wages (Linden, 2026). Recent studies have found sizable declines in foot traffic and consumer spending following Trump 2.0-era ICE raids (Wu and Li, 2026; Schwitzer et al., 2026b; Hernandez, 2026; Balanzo et al., 2026), which could reduce labor demand. However, there is, to date, no comprehensive empirical evidence on how immigration enforcement in Trump 2.0 impacted the labor market.

This paper provides the first national, causal empirical evidence on the broad labor market impacts of ICE activity 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 data on Immigration and Customs Enforcement (ICE) arrests by geographic area to propose and validate a transparent event study design. This allows us to evaluate dynamic effects, as well as directly evaluate the parallel pre-trends assumption.

Specifically, we exploit the fact that the initial increase in ICE activity in 2025 was uneven across areas within the U.S. to causally identify the effects of enforcement distinct from other changes to the economy and labor market 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 focus first on effects on the employment rate of working-age (20-64) likely undocumented immigrants who remain in the U.S. Here, we follow the literature and define a sample of likely undocumented immigrants as foreign-born individuals with at most a high school degree, and we sometimes further restrict on sector of employment, 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). Immigrants remaining in the U.S. may change their labor market behavior due to chilling effects—fear that commuting or working could increase their likelihood of detention and deportation. Indeed, a KFF/New York Times Survey in summer 2025 found that 40% of likely undocumented immigrants reported avoiding going to work for fear of it drawing attention to their immigration status (Schumacher et al., 2025). We find a meaningful chilling effect—there is a significant 1.4% reduction in the employment rate of likely undocumented immigrants overall, and a 2% reduction in sectors where undocumented immigrants are over-represented: Construction, Manufacturing, Wholesale/Retail, Business Services, Education/Health Services, Leisure and Hospitality, and Other Services. This effect is driven by male immigrants, who are over 90% of those arrested by ICE.

We next investigate if U.S.-born workers benefited from increased ICE activity. The point estimates on the overall U.S.-born employment rate are negative, but not statistically significant; we rule out increases in likely impacted sectors of more than 0.3 percentage points with our 95% confidence intervals. On U.S.-born males’ employment rate, we find a negative and significant effect. There is no evidence that employers increase wages to attract U.S.-born workers.

To understand the mechanisms behind this, we split by industry. We find the negative effects on U.S.-born males are more pronounced in sectors that historically rely more on undocumented immigrant workers, consistent with a model where undocumented immigrants and U.S.-born workers are complements, rather than substitutes in production (Chassamboulli and Peri, 2015). Descriptive data supports such a model; undocumented immigrants are more likely than U.S.-born individuals within the same sector, to work in jobs that are less desirable due to lower pay, on-the-job hazards and irregular schedules (Mukherjee and Manuel Krogstad, 2024). This may also explain why the effects on the U.S.-born are concentrated among males, given our findings for immigrants and sex segregation across sectors. We also find effects on U.S.-born males are larger for those in non-tradable sectors that also rely on undocumented workers (Burstein et al., 2020; Barkai and Karger, 2020), such as services, consistent with declines in consumer demand driving down labor demand. As a final test of mechanisms, we show that U.S.-born male workers are also more likely to report having a job but not being at work when ICE is in their area, consistent with workers choosing to stay home or firms reducing production in response to ICE activity.

Past research on the labor market impacts of 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).¹ In a companion paper, we document that the second Trump administration’s ICE activity is much larger and more indiscriminate than other recent periods (East et al., 2026). Despite the different time periods and ICE tactics, the general pattern of findings in the past literature is consistent with our findings here—increased ICE activity and deportations reduce the number of immigrant workers, and have 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 compared to the Obama administration, likely due to the more indiscriminate nature of ICE activity.

Our paper complements several area-specific analyses of the labor market impacts of ICE activity in the second Trump administration. Studies in Minneapolis-St.Paul and Texas found ICE activity reduced work, but do not break down effects by immigrants and U.S.-born or look at mechanisms (Brizuela et al., 2025; Schwitzer et al., 2026a).² Our paper adds to this other timely research by providing a comprehensive, nationwide look at the labor market impacts of ICE, as well as breaking down effects by country of origin to identify spillover effects and mechanisms. Taken together, the research on the economic impacts of ICE in Trump 2.0 paints a clear picture of economic and labor market disruption and depression.

2 Data

To measure ICE 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).³ The data includes information on each person arrested, including demographics, the

¹A much larger literature studies the impact of immigration inflows and generally finds that these have positive effects on U.S.-born workers except those most likely to be substitutes for new immigrant arrivals (Mackie and Blau, 2017; Wilson and Zhou, 2026). In contrast to this literature, we focus on the impact of ICE activity on immigrants already in the U.S. as well as on U.S.-born workers. The effects of inflows and ICE-driven outflows and impacts on behavior are unlikely to be symmetric, given that enforcement impacts people already established in the U.S. and integrated into the labor market and economy.

²Aslim et al. (2026) study the impact of ICE activity in Trump 2.0 on the childcare market and mother’s labor supply. A recently released Brookings article also analyzes the labor market effects of ICE activity in the U.S., but does not split by country of birth and instead relies on our estimates from this paper to say how many job losses are to immigrant vs. U.S.-born workers (Escobari et al., 2026). Figlio and Özek (2025) find ICE activity in this period reduced student academic achievement for both U.S.-born and immigrant Spanish-speaking children.

³This data includes administrative arrests made under ICE Enforcement and Removal Operations, which captures the majority of interior immigration arrests. Arrests made by other agencies, such as Customs and Border Protection (CBP) and United States Citizenship and Immigration Services (USCIS), are not captured here. Enforcement and Removal Operations can also conduct criminal arrests, which are not captured in this data, but these make up only about 4% of total Enforcement and Removal Operations arrests (ICE ERO, 2020). Homeland Security Investigations can also make both administrative and criminal arrests. Although

exact date and time of the arrest, the ICE Area of Responsibility and the state the arrest took place in. Our primary geographic unit is the state of arrest; however, we use sub-state information where available 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. With this data, we classify areas as treated or control, as described in more detail in the next section.

We bring this classification to labor market outcome data from 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.⁴ 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 (excluding U.S. territories), working aged (ages 20-64), have a high school degree or less, and do not currently or recently work for the government. 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. Our U.S.-born sample is working-aged individuals, excluding those born in U.S. territories.

We restrict all samples to those working, searching for work, or who have worked within the last 5 years to observe sector of current or recent employment. 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.⁵ We define high-impact sectors as those where undocumented workers are over-represented—more than 5% of the workers (Passel and Cohn, 2016). This includes Construction, Manufacturing, Wholesale/Retail, Business Services, Education/Health Services, Leisure and Hospitality, and Other Services. This helps to identify likely undocumented immigrants, since we can only proxy for documentation status.

the nature of the division means that it makes significantly fewer arrests than ERO. Specifically, Homeland Security Investigations makes only about 4% of ICE’s total arrests (Office of Homeland Security Statistics, U.S. Department of Homeland Security, 2022).

⁴Note, 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. Data from the American Community Survey is not yet available for 2025.

⁵We exclude those who work in the public sector or in the military since there may be other changes in Trump 2.0 that affect these workers and undocumented immigrants generally do not work in these jobs.

We collapse the data to demographic-group-by-area-by-month averages using the survey-provided weights for the monthly CPS.⁶ Given this, we do not weight our final regressions, and our estimates reflect the effect on the average area’s labor market outcomes. The main labor market outcomes are the percent of people who report being employed and at work in the previous week, which we call the employment rate, and the average weekly earnings and hourly earnings among those who are paid hourly.⁷

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 or “voluntary” departures; and 2) impacts to the labor market outcomes among immigrants who remain in the U.S. through chilling effects. 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. However, this will not be an issue for the estimates on U.S.-born workers’ outcomes, since we do not expect large changes in their population size. We do not look at this first channel—the number of likely undocumented workers—because of 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; Edelberg and Watson, 2024).⁸

3 Empirical Strategy

Immigration enforcement intensified nationally in the second Trump administration, as shown in Figure 1 panel (a). 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

⁶We explore robustness to using alternative weights made available through the end of 2024 proposed by Jed Kolko: <https://jedkolko.com/cps-weights/>

⁷We focus on this as a primary measure of employment because, as is shown in other research, there are decreases in activity in locations that experienced heightened ICE activity that might affect employment rates. Defining employment in this way then allows us to capture the employment effects of such reduced foot traffic and consumer demand.

⁸There are two other potential compositional issues 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, this is unlikely given ICE’s arrest patterns under Trump 2.0, which primarily consist of arresting people who are already in law enforcement custody or those who they encounter somewhat randomly (East et al., 2026).

arrests relative to the baseline non-citizen population in the area.⁹ 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 during this period.

We show the results of this classification of areas in Figure 1 panel (b).¹⁰ The bold borders in this figure show the boundaries of the state and sub-state geographic areas we use in our analysis. Using this classification, we implement the event study and difference-in-differences models to estimate the labor market impact of increased 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), though results are similar with year by month fixed effects as well. 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 results are robust to using the method proposed by Callaway and Sant’Anna (2021) to account for potential bias induced by differential treatment timing across areas.

The 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 policy changes in 2025. To evaluate the validity of the first assumption, we plot event study graphs to directly examine pre-trends. We also show

⁹We use the 2023 5-year ACS estimates to calculate the baseline number of non-citizens in each area (U.S. Census Bureau, 2023).

¹⁰There are a few geographic areas we cannot identify in the CPS data, which are shaded gray and are omitted from our analysis.

robustness to including demographic and local economic condition controls¹¹ and a control for area-specific exposure to Trump’s tariffs.¹² Finally, we show there are no similar differential changes across treated and control areas in the first Trump term as a placebo test.

Because our empirical approach compares changes in outcomes across geographic areas, we will miss any national effects, and we do not extrapolate our findings to national totals. It is possible that through media reports or social networks, immigrants in the areas we classify as control areas will be impacted by ICE activity. If anything, this would attenuate our estimates towards zero.

4 Results

Effects on ICE Arrests

We begin by confirming the “first-stage” using our classification of treated and control areas in Figure 2. In our analysis of labor market outcomes, we estimate reduced form models because the impacts we document may not operate only through arrests, we are using arrests as a proxy for intensified ICE activity. But, this exploration helps us understand what our treatment classifications translates to in terms of number of arrests, to interpret the findings.

Treated areas experience a large and sudden increase in monthly arrests, with no differential pre-trends in arrests in the year prior. This is expected and mechanical, since we define treatment timing based on a sudden increase in arrests. 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 also 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

¹¹The 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 area-specific demographic and economic characteristics are measured at baseline and interacted with time fixed effects.

¹²This 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).

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.

Effects on Likely Undocumented Immigrants’ Labor Market Outcomes

We explore the effects on likely undocumented immigrants in Figure 3. The left-hand side panels show results on the employment rate across all sectors, and the right-hand-side panels show the results in the high-impact sectors. As above, we show results for both sexes pooled, the male subsample and the female subsample, respectively.

We find no differential pre-trends for any sample or outcome, supporting our identifying assumptions. Moreover, the employment rate of likely undocumented immigrants remaining in the U.S. falls in the face of heightened ICE activity. 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 and immigrants make decisions about how to reduce their potential interactions with ICE. This is consistent with findings that the decline in foot traffic becomes larger over time following an increase in ICE operations (Wu and Li, 2026; Hernandez, 2026).

The corresponding difference-in-differences results show a significant reduction in the employment rate of likely undocumented immigrants of 1.3 overall (1.4%) and 1.8 percentage points (2%) in the high-impact sectors. Note, the mean employment rates 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.

The effects are larger for males, as expected. In particular, there is a significant reduction in the male employment rate of 2.8 and 3.5 percentage points, across all and high-impact sectors, respectively. The estimates for the female employment rate are positive but insignificant. These mixed effects likely reflect competing mechanisms: 1) female immigrants may enter the labor market to compensate for the decline in male immigrants’ work in their household, the “added worker effect”; and, 2) a chilling effect on female immigrants that reduces their work.

We also explore whether there are effects on the average hourly wages and weekly earnings of likely undocumented immigrants. If there is a large reduction in work among likely undocumented immigrants, firms may 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. Additionally, past research found that heightened immigration enforcement increases minimum wage violations by reducing worker bargaining power (Grittner and Johnson, 2022). So, the expected effect is ambiguous. There is no evidence of a significant negative or positive effect on pay among likely undocumented workers shown in Appendix Table A1.

We look further at heterogeneity by sector. Focusing on the results among males, in Figure 4, 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) (Passel and Cohn, 2016).¹³ Most estimates are not statistically significant given small sample sizes, so we focus on overall patterns in this analysis.

As expected, there is a stronger negative effect in sectors with a great share of undocumented workers in the pre-period. The largest negative effect is for workers in Construction, which has 13% undocumented workers. This is consistent with other work on the second Trump administration that finds a large negative effect on foot traffic following ICE activity in construction-related locations (Hernandez, 2026) and large declines in spending in home improvement stores (Balanzo et al., 2026). Whereas the effect in the Transportation, Finance, and Information sectors is small or positive, and these sectors have less than 5% of undocumented immigrants each. The exception to this is the Leisure/Hospitality sector, though this is dominated by female likely undocumented immigrants, so the number of male immigrants working in this sector is small.

Robustness Checks

In Appendix Figures A3-A4, we test the robustness of these findings, replicated in the black circles. 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 with two-way fixed-effects, we test robustness to using the Callaway and Sant’Anna’s estimator in the yellow “x”s. The conclusions do not change. Finally, 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.

¹³Because PEW groups the wholesale and retail sectors together, we do the same here. We drop Mining given small sample sizes. We weight the correlation by the undocumented reported labor force size from Passel and Cohn (2016).

We conduct a placebo test using CPS data from 2015-2017 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 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.

Effects on U.S.-Born Workers

Turning to the U.S.-born sample, we begin by exploring the impact on the employment rate and weekly earnings in all sectors. U.S.-born workers may be impacted by both spillovers within production (substitution or complementarities), and by the declines in economic activity in the face of heightened ICE operations (Wu and Li, 2026; Schwitzer et al., 2026b; Hernandez, 2026; Balanzo et al., 2026), and we test these mechanisms below.

We find null effects on all U.S.-born workers' employment rate and negative effects for males' employment rate, shown in Figure 5. On the full sample, we can rule out increases in employment of larger than 0.3 percentage points. And, if anything we find negative effects on weekly earnings, consistent with declines in labor demand.¹⁴

Spillover effects onto U.S.-born workers may be largest among males given our results on immigrants and sex segregation across sectors (Cartwright et al., 2011). If this negative effect on U.S.-born males' employment rate is driven by complementarities within the production process, these are likely to occur within sector, so we examine effects by sector. Specifically, we show sector-level scatter plots with the effect on the employment rate of U.S.-born males on the vertical axes and the the baseline share of undocumented workers by sector on the horizontal axes in panel (b) of Figure 4. With the exception of Agriculture, there is a negative relationship across sectors between the share of immigrants in a sector and U.S.-born worker's employment rate. That is, the sectors with the highest shares of baseline

¹⁴We examine U.S.-born workers' hourly wage in Appendix Table A1 and again find null effects. This null overall employment effect is counter to 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 (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, ICE activity in 2025. This abstracts away from the issues with trying to measure population sizes in the CPS.

undocumented workers are also the sectors where we see negative effects for U.S.-born male workers. Thus, sector-specific complementarities are likely driving some of the negative spillover effects onto U.S.-born workers.

As a further test of mechanisms, we distinguish between tradable (Agriculture and Manufacturing in the x's) and non-tradable sectors (all others in the dots) in this Figure (Burstein et al., 2020; Barkai and Karger, 2020). Reductions in foot traffic and consumer spending should primarily impact U.S.-born workers who work in non-tradable jobs such as restaurant workers, hair cutters, and construction workers. We find that the largest negative effect on U.S.-born workers is in sectors that are both non-tradable and those that rely on undocumented labor at the highest rates: Leisure/Hospitality and Other Services. This suggests that changes in consumer demand may also play a role in reducing employer demand for U.S.-born workers.

As a final test, we look at a measure of temporary absence from work. Contemporaneous research found a reduction in foot traffic at workplaces in response to ICE activity, including in areas with lower density of immigrants (Wu and Li, 2026; Hernandez, 2026). We construct a variable that measures the percent of the relevant sample that reports having a job but not being at work in the prior week. In Appendix Table A2, we find a significant 0.3 percentage point increase in this rate among U.S.-born males. Thus, there may be an important chilling effect on U.S.-born workers when ICE is present in their area, though we are unable to distinguish if the workers are choosing to stay home or whether production at their workplaces changed as a result of ICE.

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 all sectors in treated areas in 2024. We then multiply this by the percent change in the employment rate from our difference in difference results, $((0.028/0.928)*273,691)$ in all sectors. This implies roughly 8,300 fewer likely undocumented males at work in all sectors in the average treated area as a result of heightened ICE activity. The total effect on the number of immigrant workers is very likely larger than this, given that this does not account for those removed from the U.S. or those

choosing to leave the U.S. For arrests, we found a cumulative increase in male arrests of 1,200 in treated areas, and using the best estimate available on “voluntary” departures, there is a decline of roughly 3,500 immigrants in the average area (Kissam, 2026). So, the total effect could be somewhere closer to 13,000 lost immigrant workers in the average treated area.

Comparing this to the prior literature, the chilling effect in Trump 2.0 is larger than in past mass deportation campaigns. We find, for every ICE arrest, 7 male likely undocumented workers who remain in the U.S. stop working. 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 is consistent with the increasingly indiscriminate nature of ICE activity in Trump 2.0 (East et al., 2026).

We use the same methodology to evaluate impacts on U.S.-born workers in all sectors. Our estimates imply there is a reduction in the number of male U.S.-born individuals at work of 11,300 per area $((-0.006/0.928)*1,752,839)$. The ratio of immigrant to U.S.-born effects on employment is in line with the prior literature evaluating the impacts of mass deportations under President Obama, which range from 0.61 to 4.3 (Zipperer, 2025).

5 Conclusion

In this paper, we provide the first, national, quasi-experimental evaluation of ICE activity in Trump 2.0 on the labor market outcomes of key demographic groups. We find that immigrants who remain in the U.S. are working less due to chilling effects. And, we show no evidence of positive effects of the labor market outcomes of U.S.-born workers. Instead, ICE activity lowers the employment rate of U.S.-born male workers, likely driven by a combination of complementarities in production and reductions in economic activity both reducing labor demand for U.S.-born workers.

This directly informs a central policy justification for heightened immigration enforcement—that it expands job opportunities for U.S.-born workers. We find no evidence supporting this argument, and, instead document negative spillovers for U.S.-born males, suggesting that enforcement may contract, rather than reallocate, labor demand in affected areas.

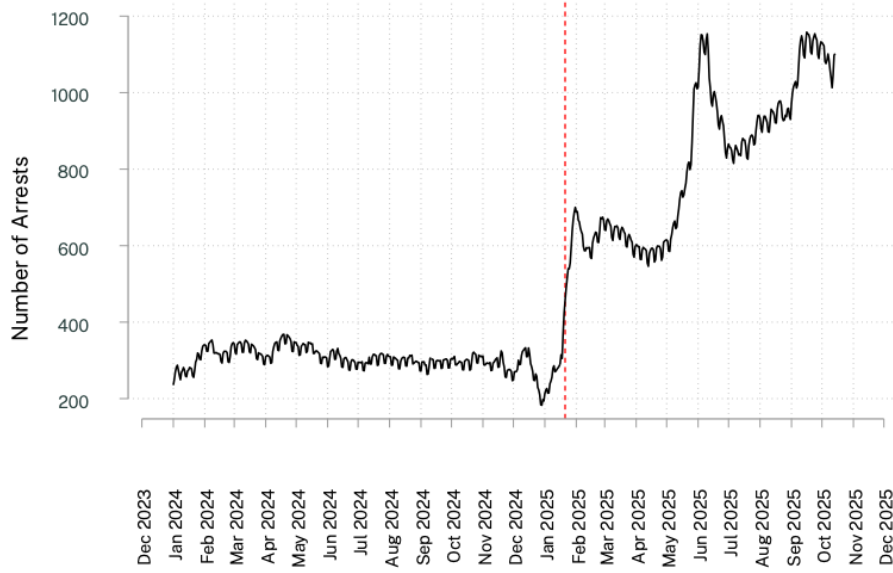
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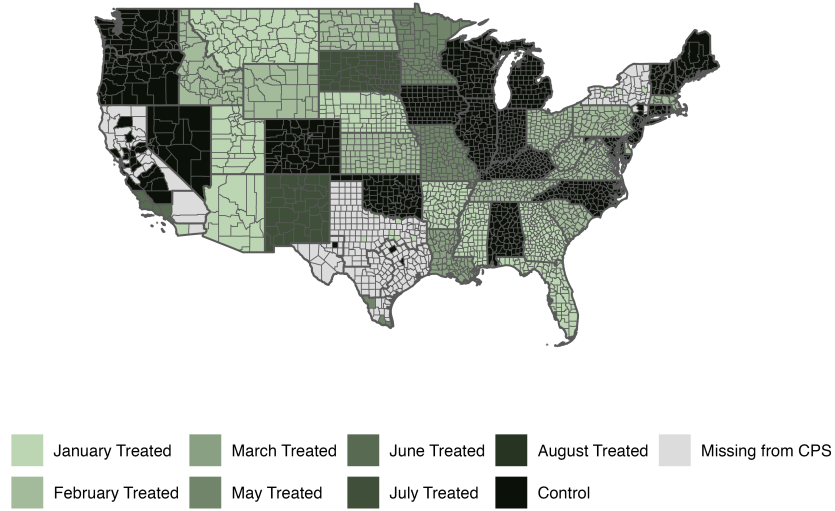
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Figure 1: ICE Arrest Changes



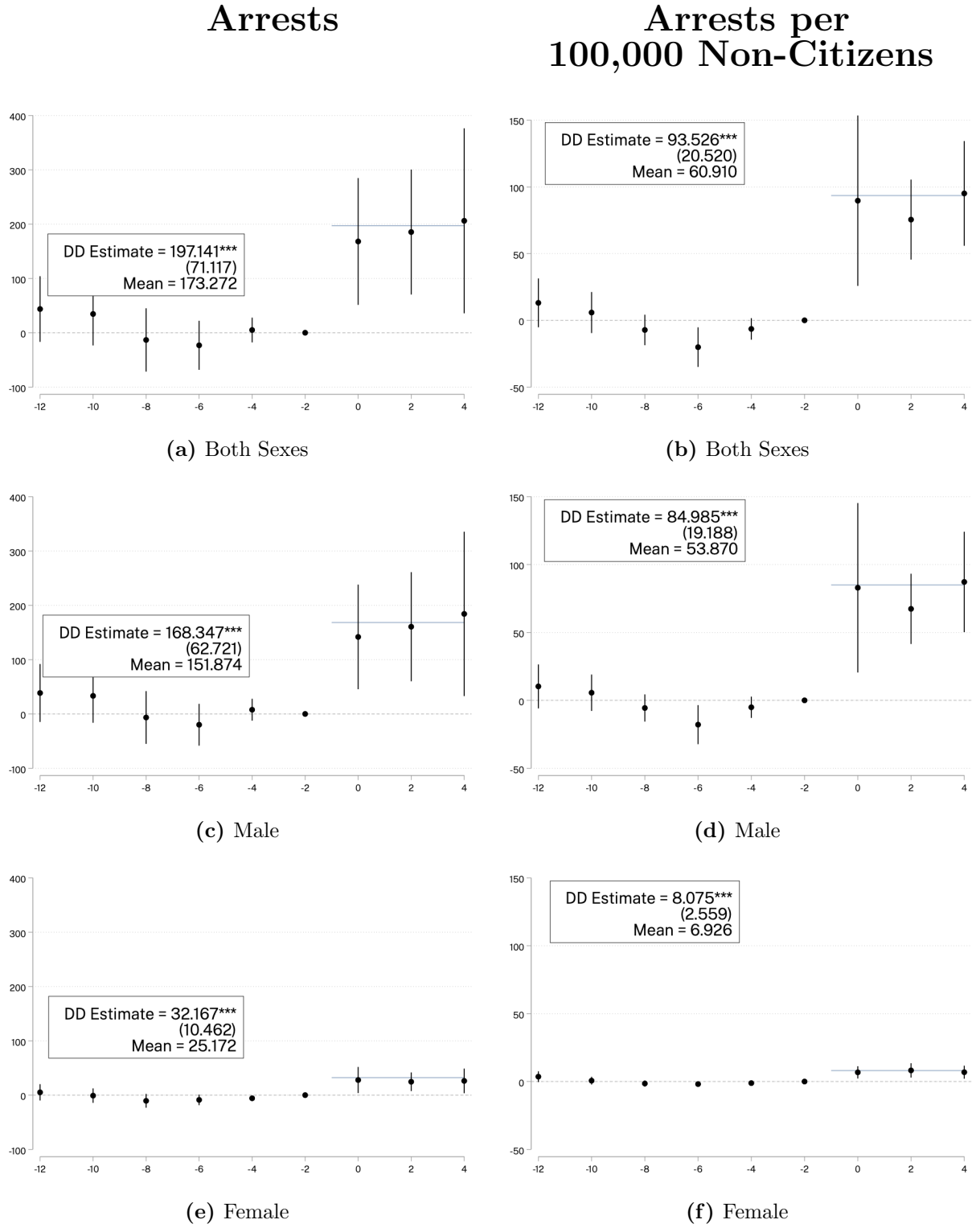
(a) Total Daily Arrests, 15 day moving average



(b) Treatment and Control Classification

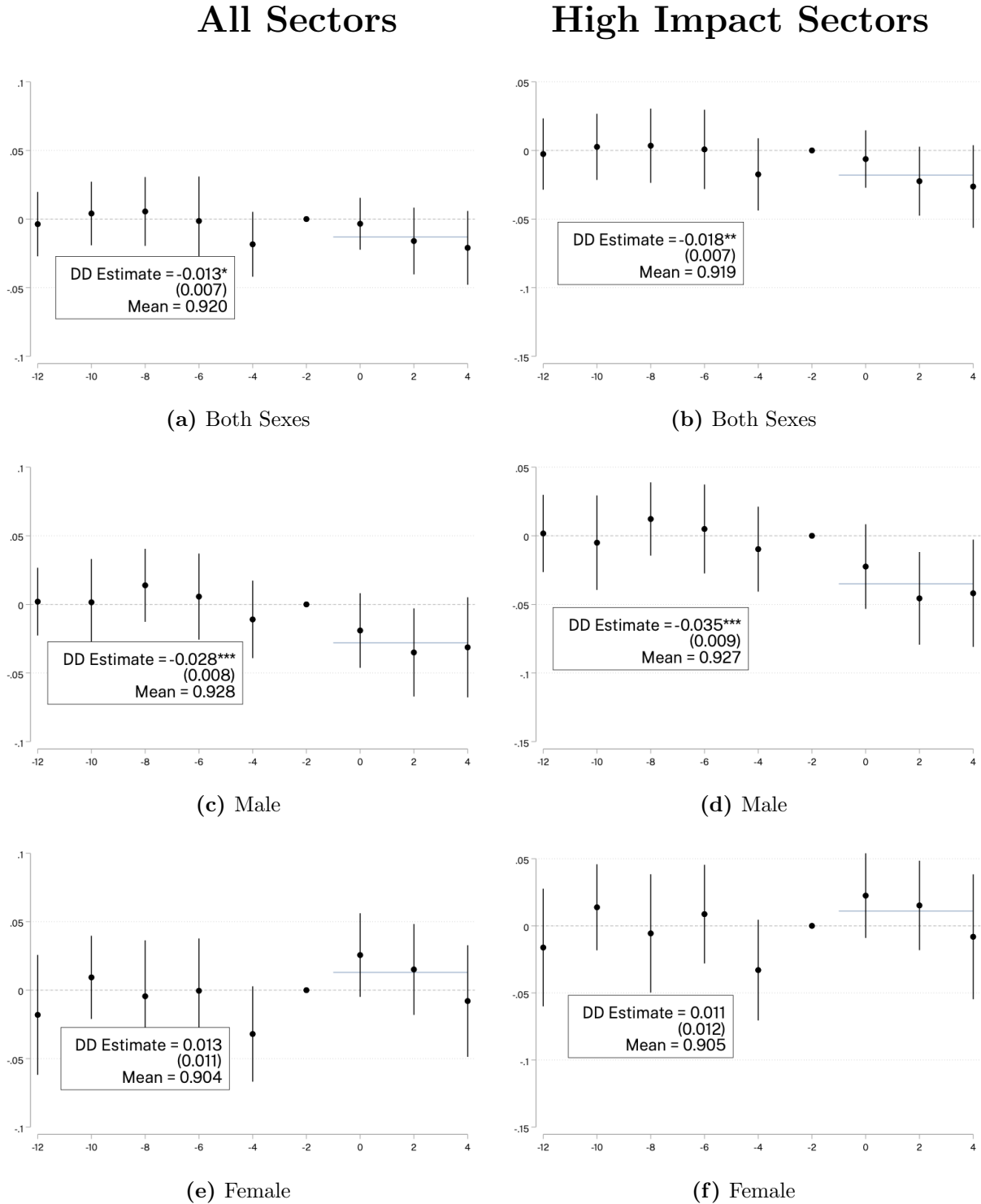
Notes: Uses ICE ERO administrative arrest data from October 2023 through October 2025. Total arrests are aggregated by date, and reported as a 15-day moving average. The vertical red line indicates the date of Trump's second inauguration, January 20, 2025. Area of Responsibility boundaries are from ICE 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 2: Event Study Estimates on Arrests



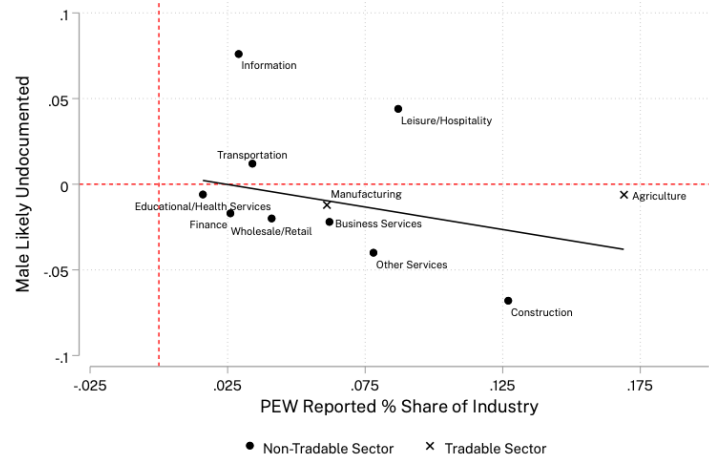
Notes: Uses ICE ERO administrative arrest data from October 2023 through October 2025. Black dots show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed effect. The 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 3: Event Study Estimates on Likely Undocumented Immigrants' Employment Rate

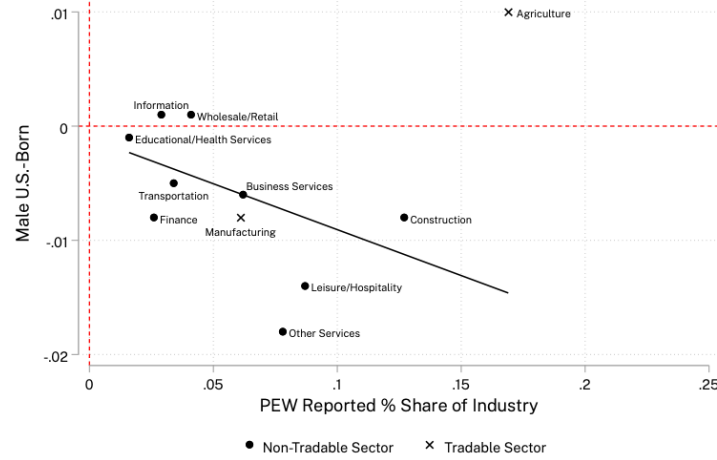


Notes: Economic outcome data comes from the basic monthly CPS from October 2023 through November 2025. Black dots show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed 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. High-impact sectors consists of construction, manufacturing, wholesale/retail, business services, education/health services, leisure and hospitality, and other services. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 4: Male Difference-in-Differences Estimates by Sector



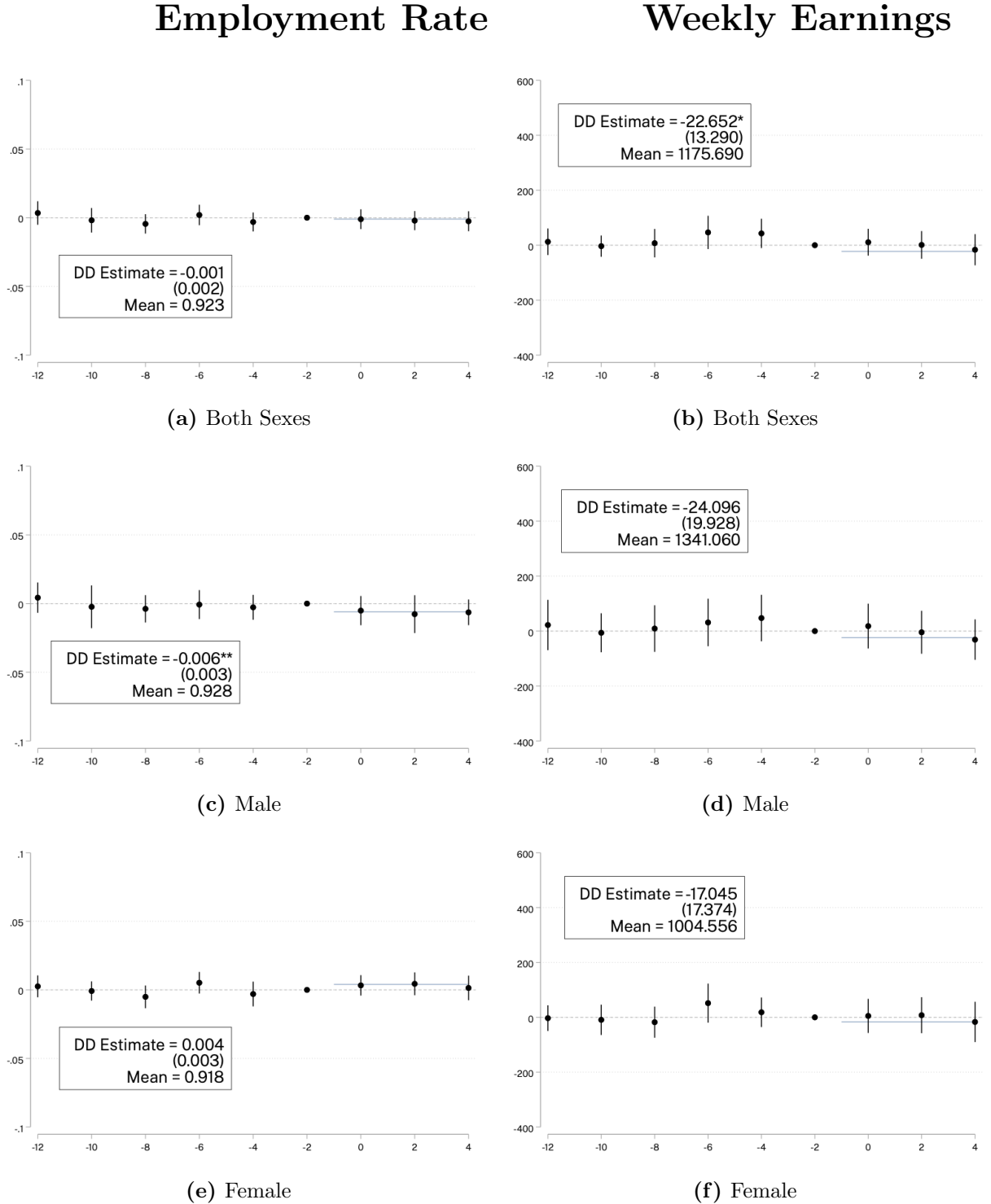
(a) Male Likely Undocumented vs Reported PEW Estimated Share Undocumented



(b) Male U.S.-Born vs Reported PEW Estimated Share Undocumented

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. We do not report estimates for Mining due to its small sample size. Linear correlations are weighted by the baseline PEW-reported sector and demographic specific number of workers nationally. 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.

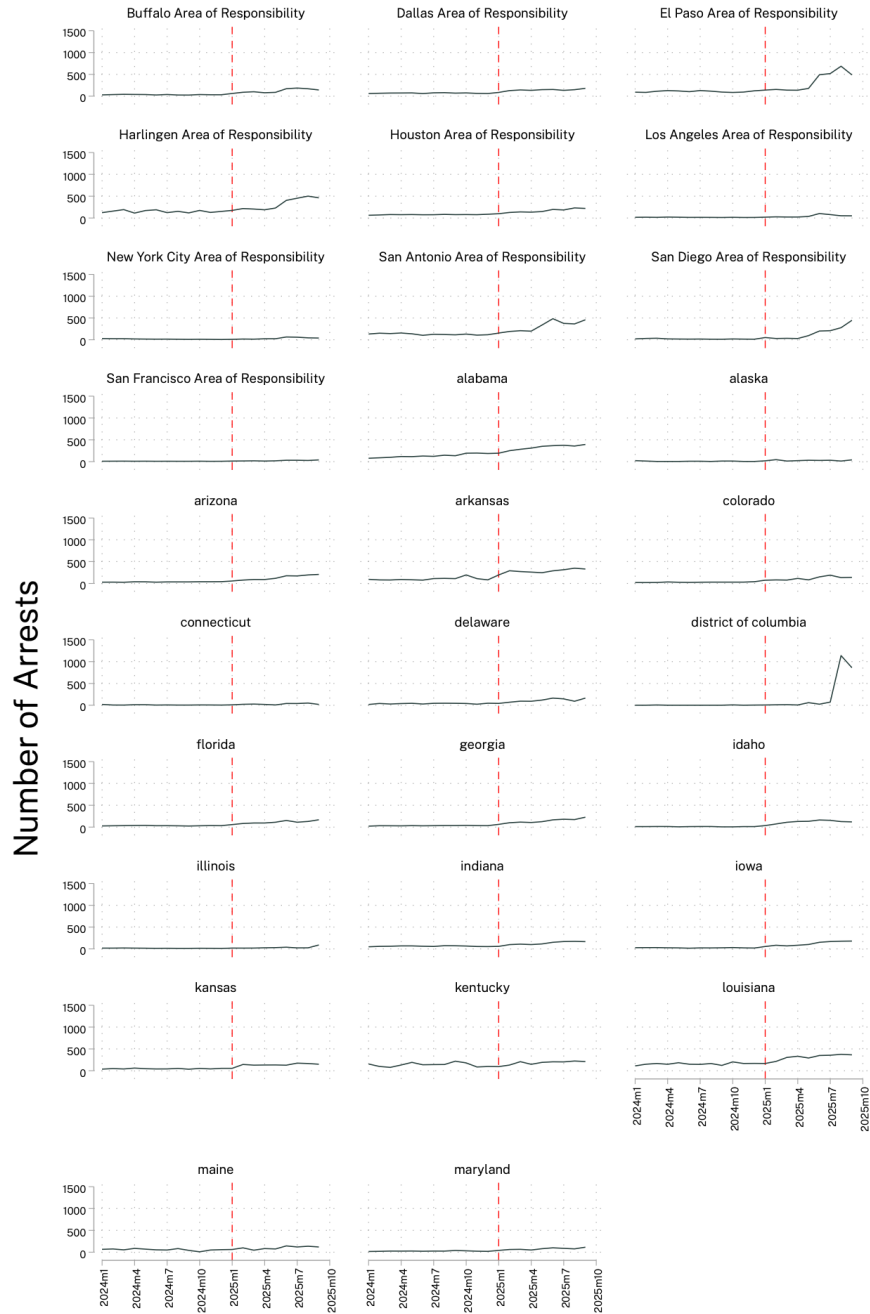
Figure 5: Event Study Estimates on U.S.-Born Labor Market Outcomes



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. The sample includes all sectors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

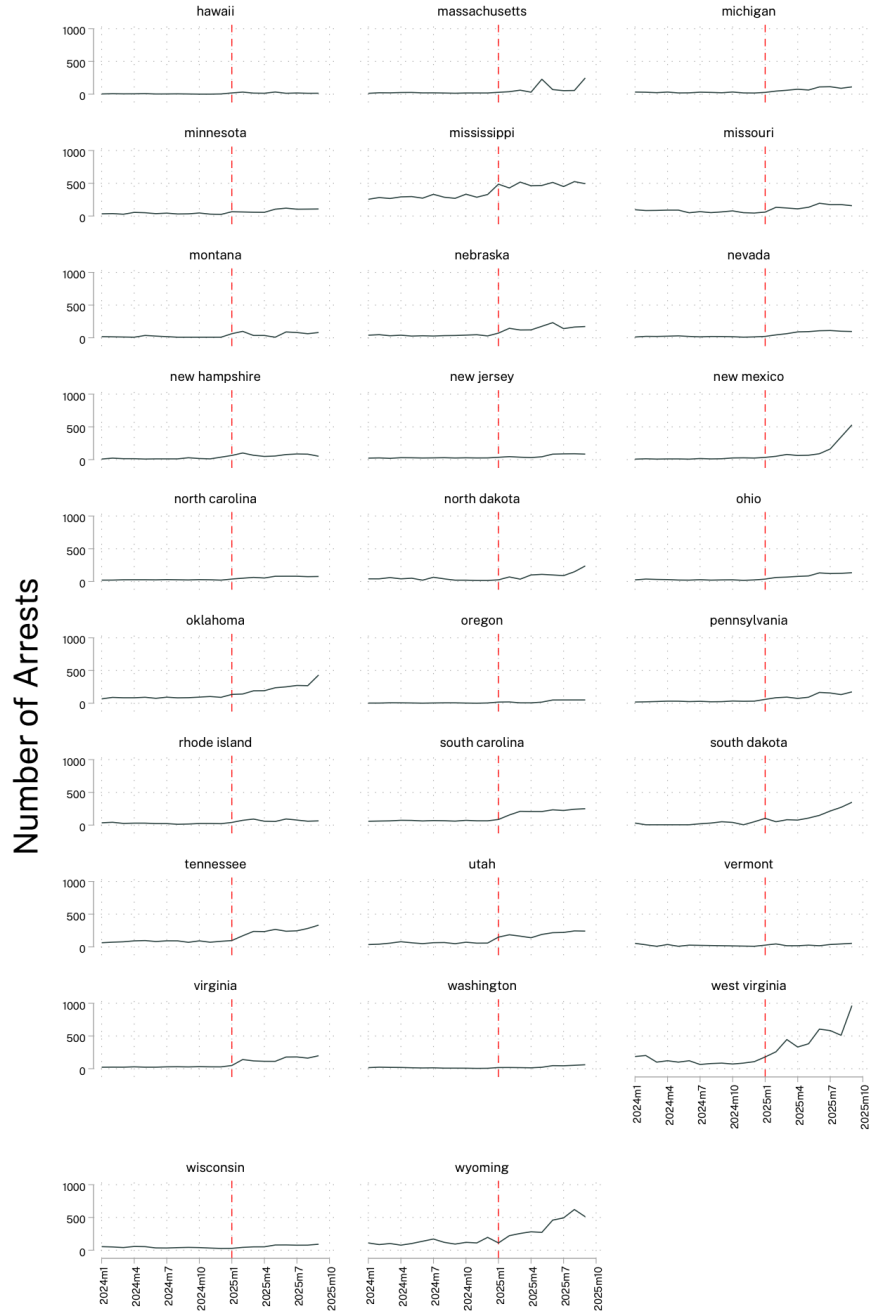
A Appendix

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



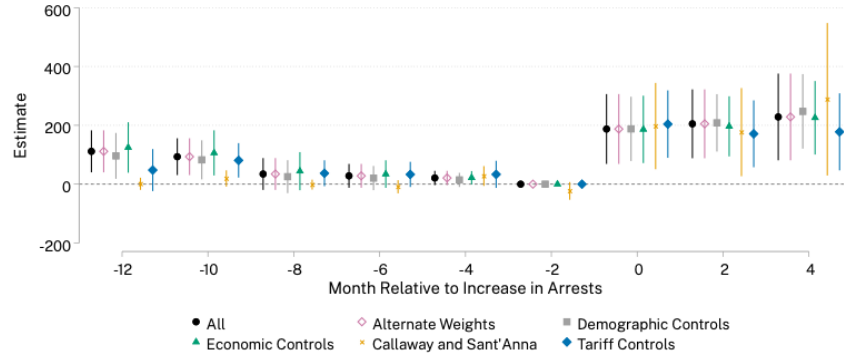
Notes: Uses ICE ERO administrative arrest data from October 2023 through October 2025. Area of responsibility boundaries come from ICE ERO. Arrests are reported as the monthly total in the respective area. The vertical red line indicates the date of Trump's second inauguration, January 20, 2025.

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

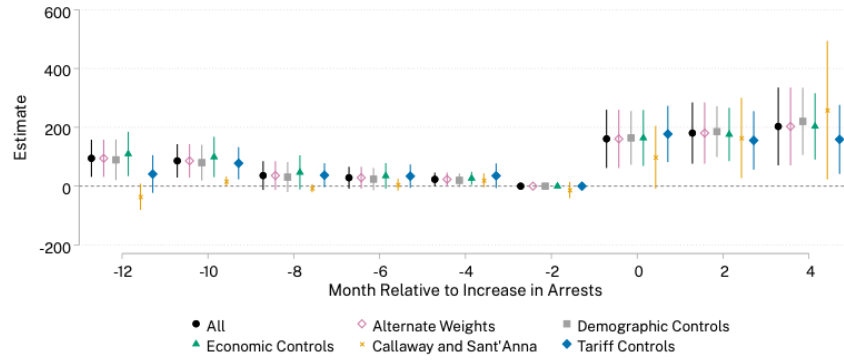


Notes: Uses ICE ERO administrative arrest data from October 2023 through October 2025. Area of responsibility boundaries come from ICE ERO. Arrests are reported as the monthly total in the respective area. The vertical red line indicates the date of Trump's second inauguration, January 20, 2025.

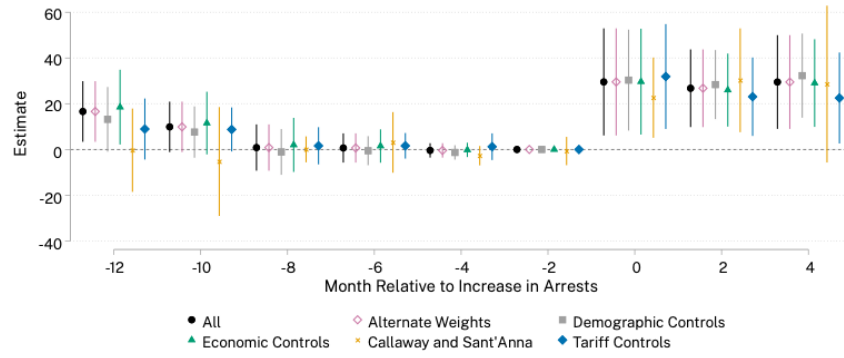
Figure A3: Robustness of Event Study Estimates on Arrests



(a) Both Sexes



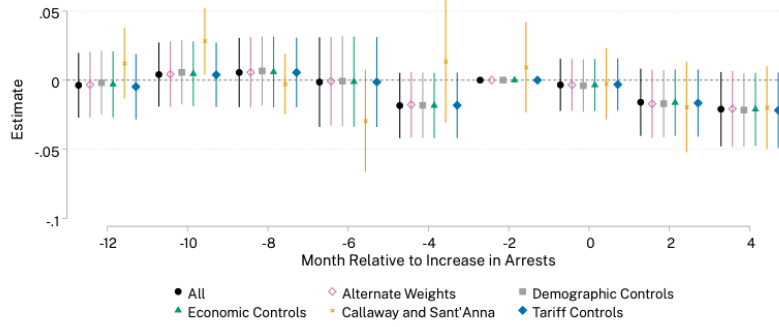
(b) Male



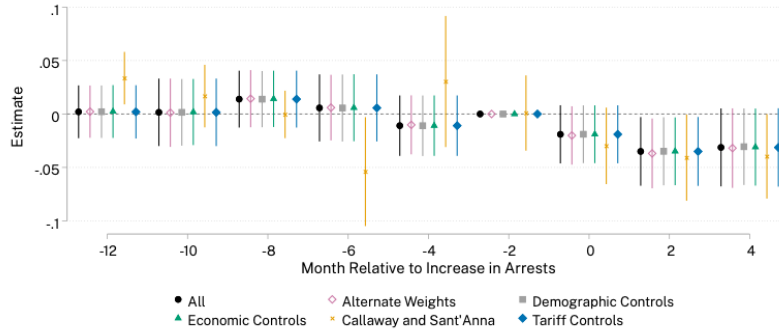
(c) Female

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. 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, and solid blue diamonds include controls for tariffs.

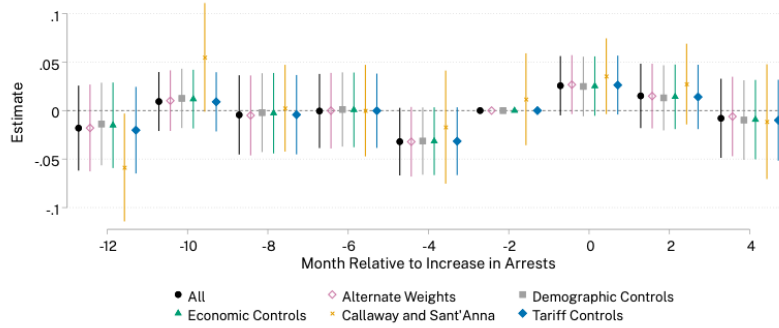
Figure A4: Robustness of Event Study Estimates on Likely Undocumented Immigrant Employment Rate in All Sectors



(a) Both Sexes



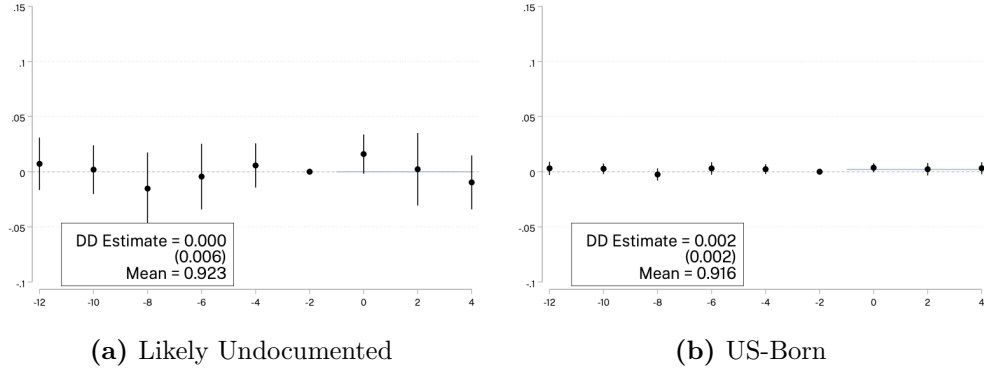
(b) Male



(c) Female

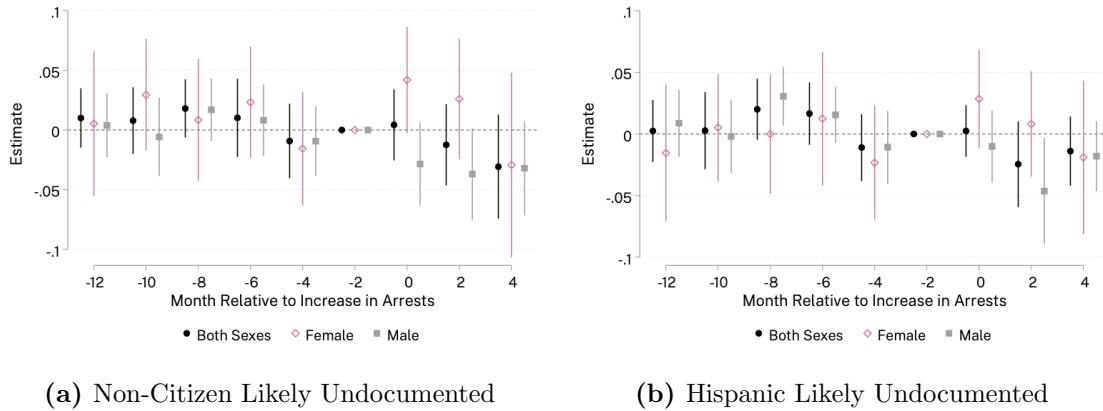
Notes: Economic outcome data comes from the basic monthly CPS from October 2023 through November 2025. Black dots show the coefficients from the event study shown in Equation (1) and bars show the associated 95% confidence intervals. Includes separate year and month fixed effects as well as an area fixed 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, and solid blue diamonds include controls for tariffs. The likely undocumented sample includes foreign-born individuals between the ages of 20 and 64 who have a high school education or less. The sample includes all sectors.

Figure A5: Event Study Estimates on Employment Rate in All Sectors, Trump Term 1 Placebo Test



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. The sample includes all sectors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A6: Event Study Estimates on Non-Citizen and Hispanic Likely Undocumented Immigrant Employment Rate in All Sectors



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. The sample includes all sectors.

Figure A7: Robustness of Event Study Estimates on U.S.-Born Labor Market Outcomes in All Sectors



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, and solid blue diamonds include controls for tariffs. The U.S.-born sample includes all U.S.-born individuals between the ages of 20 and 64. The sample includes all sectors.

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

a) Both Sexes	Likely Undocumented		U.S.-Born	
	Weekly Earnings	Hourly Wage	Weekly Earnings	Hourly Wage
Post	17.414 (33.145)	0.521 (0.367)	-22.652* (13.290)	0.059 (0.195)
Mean Y	700.272	16.189	1175.690	19.708
b) Males	Likely Undocumented		U.S.-Born	
	Weekly Earnings	Hourly Wage	Weekly Earnings	Hourly Wage
Post	-3.896 (49.050)	0.748 (0.541)	-24.096 (19.928)	0.140 (0.272)
Mean Y	793.719	17.456	1341.060	20.671
c) Females	Likely Undocumented		U.S.-Born	
	Weekly Earnings	Hourly Wage	Weekly Earnings	Hourly Wage
Post	16.929 (26.860)	0.138 (0.372)	-17.045 (17.374)	0.028 (0.197)
Mean Y	555.834	14.432	1004.556	18.767

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. The sample includes all sectors. * p<0.10, ** p<0.05, *** p<0.01

Table A2: Difference-in-Differences Estimates on Absence from Work Last Week in All Sectors

Employed and Not at Work Last Week	
a) Both Sexes	U.S.-Born
Post	0.000 (0.001)
Mean Y	0.034
b) Male	U.S.-Born
Post	0.003** (0.001)
Mean Y	0.028
c) Female	U.S.-Born
Post	-0.002 (0.002)
Mean Y	0.040

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 U.S.-born sample includes all U.S.-born individuals between the ages of 20 and 64. The sample includes all sectors. * p<0.10, ** p<0.05, *** p<0.01