The Impact of Immigration Enforcement at Courts on Failure-to-Appear Rates

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Abstract

We examine whether immigration enforcement at courthouses affects defendants' willingness to appear for required proceedings. Using Los Angeles County Superior Court administrative data and a difference-in-differences design exploiting variation in the timing and location of Immigration and Customs Enforcement (ICE) activity, we find that misdemeanor defendants scheduled for arraignment within 30 days after ICE activity at their courthouse are 3 to 5 percentage points more likely to fail to appear, an increase of a little over 10 percent. This effect is concentrated among drug misdemeanors. These findings identify substantial spillovers from federal immigration enforcement to state courts.

Keywords: Immigration enforcement; Failure to Appear; Misdemeanor; Immigration and Customs Enforcement (ICE)

JEL codes: K14, K37, H77

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1 Introduction

Soon after taking office in January 2025, the second Trump administration substantially expanded immigration enforcement by Immigration and Customs Enforcement (ICE). Enforcement activity occurred not only at residences and workplaces, but also at locations such as hospitals (Boyd-Barrett, 2025), schools (Dee, 2025), and courthouses. This paper examines whether ICE activity at courthouses affects defendants' willingness to appear for misdemeanor arraignments.

Like most justice systems, U.S. courts depend on defendants appearing for scheduled court proceedings to move cases toward resolution. Yet failures to appear (FTAs) are relatively common, particularly for misdemeanor cases in which defendants are not held in custody pending arraignment. In Los Angeles County Superior Courthouses (LASCs), almost 37 percent of misdemeanor defendants not held in custody failed to appear for arraignment in 2025 and 2026. This rate is similar to that observed in other large urban court systems.[1]

Despite the frequency of FTAs, relatively little evidence identifies factors that causally increase or decrease the probability that defendants appear in court. Several studies examine correlates of FTA (Zettler and Morris, 2015; McGlynn et al., 2025) or predictors of subsequent nonappearance (Monaghan et al., 2022; Spohn, 2023), but causal evidence is more limited. One important exception is Fishbane et al. (2020), who use a field experiment to show that reducing the cognitive cost to appearing for scheduled court hearings, via redesigned court summonses and text-message reminders, substantially reduced FTAs among defendants charged with low-level offenses. Our paper examines a different margin: whether increasing the potential consequences of going to a courthouse for the arraignment hearing reduces defendants' willingness to appear.

The expansion of immigration enforcement beginning in 2025 generated considerable public attention, but relatively little evidence exists on its spillover effects on the operation

[1]Fishbane et al. (2020), for example, also report an FTA rate of approximately 37 percent for low-level offenses in New York City during 2016-2017.

of other public institutions. A notable exception is Dee (2025), who examines immigration enforcement activity near schools in California's Central Valley and finds that it increased student absences by approximately 22 percent relative to comparable months in previous years at the same schools. Our analysis here is somewhat analogous, but focuses on immigration enforcement activity at courthouses. Courthouses are particularly important settings because their operation depends directly on individuals appearing in person when required. Immigration enforcement at courthouses may therefore affect not only individuals directly targeted by ICE, but also the functioning of the judicial system as a whole.

We study the impact of ICE activity at courthouses on FTA rates using administrative data from LASCs combined with courthouse-level records of ICE activity. Our empirical strategy exploits differences in the timing of ICE activity across courthouses, comparing changes in FTA frequency at courthouses experiencing ICE activity with contemporaneous changes at courthouses that never experienced targeted ICE activity during our sample period. We find that defendants whose misdemeanor arraignments are scheduled within 30 days after ICE activity at their assigned courthouse are 3 to 5 percentage points more likely to FTA, an increase of 10 - 14 percent relative to the pre-ICE FTA rate. This effect appears to be more concentrated among those charged with drug misdemeanors. We find that these estimates remain stable in magnitude when we sequentially omit individual courthouses, indicating that the results are not attributable to any single courthouse, and these results remain consistent across various difference-in-difference specifications.

An important aspect of our empirical approach is that our estimates capture the incremental effect of recent ICE activity at the defendant's own courthouse on the likelihood of failing to appear, above and beyond any broader countywide effect associated with ICE activity elsewhere in the LASC system. To the extent that such countywide spillovers also increase FTAs at untreated courthouses, our estimated local effect will understate the total impact of ICE activity at courthouses on FTAs.

Even with this caveat, the magnitude of the estimated effects illustrates a potentially im-

portant external cost of courthouse immigration enforcement. Almost 12,000 misdemeanor defendants in our sample had arraignments scheduled within 30 days after recorded ICE activity at their assigned courthouse. Applying our lower-range estimate of a 3 percentage-point effect to these defendants implies at least 360 additional failures to appear associated with courthouse ICE activity in Los Angeles. By comparison, only 17 of the 82 ICE visits observed during our observation period actually led to an arrest. Although these quantities measure very different outcomes, their relative magnitudes illustrate how immigration enforcement at courthouses can affect state-court administration well beyond the individuals directly targeted by federal enforcement.

2 Data

Our data are constructed from LASC's administrative case management system, which records the universe of criminal cases entering the Los Angeles County court system. Our analysis covers cases with initial court appearances between January 1, 2025 and April 16, 2026, across the 21 LASC courthouses handling the non-juvenile criminal cases.

For each defendant-case, the LASC case management system provides information on the filing date, the date and location of the defendant's first court appearance, the offense with which the defendant is charged, and the issuance and timing of bench warrants for failure to appear. Our primary outcome is whether the defendant failed to appear for arraignment, which is generally the first hearing in a case, as indicated by the issuance of a bench warrant.[2]

We restrict our analysis to misdemeanor cases involving defendants who were not likely to be in custody at arraignment.[3] We focus on misdemeanors because defendants in these

[2]A bench warrant is an order issued by a judge authorizing the arrest of a defendant who fails to appear for a required court proceeding.

[3]Because the administrative data do not directly identify whether a defendant was detained at the time of arraignment, we proxy for custody status using the timing of the first court appearance. We classify defendants as likely in custody when their first hearing occurred within two days of filing, or within four days for cases filed on Thursday or Friday to account for intervening weekends. We exclude these defendants from the sample because their appearance at the courthouse is not voluntary in the same sense as that of defendants released pending arraignment.

cases are substantially more likely to be released before arraignment and therefore face a meaningful choice about whether to appear in court. Our primary specification pools the misdemeanor offenses in our analysis, but we also report results separately for drug misdemeanors, as well as violent, property, DUI, and other traffic misdemeanors.[4]

The LASC administrative data do not report defendants' race, ethnicity, or immigration status. This limits our ability to examine heterogeneity by characteristics that may be associated with exposure to immigration enforcement. Regardless, Hispanic ethnicity would be an imperfect proxy for such exposure. Approximately 10 percent of Los Angeles County residents are estimated to be undocumented, and only about 40 percent of the county's undocumented population is estimated to be Hispanic (USC Dornsife, Equity Research Institute, 2025). Moreover, courthouse immigration enforcement may affect the appearance decisions of U.S. citizens and lawfully present immigrants in mixed-status households. We therefore focus on the effect of ICE activity on FTAs among defendants generally.

Our key treatment variable, the timing and location of ICE enforcement activity, also comes from LASC and is merged onto the defendant-level file at the courthouse-day level. LASC is able to track federal immigration agents entering its courthouses through its courthouse security procedures. Persons entering LASC courthouses are subject to weapons screening, while law-enforcement officers on official business may enter armed pursuant to procedures governing peace-officer access. Armed law-enforcement officers therefore identify themselves to courthouse security personnel when entering in their official capacity and identify the purpose of their visit, allowing LASC to record the presence of ICE agents and enforcement activity in its courthouses.[5]

Table 1 shows the distribution of recorded ICE activity across LASC criminal courthouses

[4]We classify misdemeanor cases using LASC's offense classifications. The non-traffic group includes misdemeanor assault and battery, property offenses, drug offenses, and other non-traffic misdemeanors. Traffic offenses include driving under the influence (DUI), driving with a suspended license, and other traffic misdemeanors. We exclude felony cases, infractions, and administrative or petition matters.

[5]The Los Angeles County Sheriff's Department provides security at LASC courthouses and subjects persons entering court facilities to weapons screening. California court-security rules require court security plans to address entry screening, identification and access control, and firearms policies and procedures. See Cal. R. Ct. 10.172; Los Angeles County Sheriff's Department, Court Services Division Manual, § 4-04/025.00.

and over time. Overall, there were 82 instances of ICE activity at LASC courthouses between May 2025 and April 2026, of which 74 were at the adult criminal courthouses included in our sample. Activity is initially relatively infrequent but increases substantially in early 2026 and is highly uneven across courthouses: some courthouses experience repeated ICE presence, while others experience only isolated events or none at all. The geographic distribution of ICE activity also does not simply track the Hispanic share of the surrounding population; Online Appendix Figure A2 shows this geographic variation, plotting courthouse-level ICE activity against the Hispanic and foreign-born noncitizen shares of the surrounding Census-tract populations. The maps suggest that ICE activity does not simply follow the geographic distribution of either population.

Although the unit of observation is an individual defendant scheduled for arraignment, ICE exposure varies at the courthouse-day level. Online Appendix Table A1 reports descriptive statistics for the analysis sample. Excluding defendants likely to be in custody changes both the frequency of FTAs and the composition of misdemeanor cases: the arraignment FTA rate rises from 30.14 to 37.97 percent, while the share of assault and battery cases falls and the share of DUI cases rises. The table also shows some differences between courthouses that have ever experienced ICE activity and those that do not; for example, the arraignment FTA rate is 37 percent at eventually treated courthouses and just over 40 percent at never-treated courthouses. Our empirical design, therefore, does not rely on cross-sectional comparability between treated and untreated courthouses, but instead exploits the timing of ICE activity within courthouses, as described in the next section.

3 Methodology

Our empirical strategy employs a difference-in-differences (DiD) approach that compares changes in failure-to-appear (FTA) rates among defendants whose arraignments are held at courthouses that experience ICE activity with analogous changes in FTA rates among

defendants whose arraignments are at courthouses that never experience ICE activity during our sample period. The two key assumptions required for this approach to identify the average treatment on the treated (ATT) are *parallel trends*, which posits that the change in outcomes from before to after treatment among those treated would have been the same as the analogous change among those untreated in the absence of the treatment, and *SUTVA* (Stable Unit Treatment Value Assumption) which posits that treatment only impacts those who are treated.

It has become standard to implement this approach via a two-way fixed-effects (TWFE) event study design. In our context, this would take the following form:

$$FTA_{i,c,t} = \theta_t + \eta_c + \left(\sum_{m=2}^{M} \gamma_m Treat_c \times Pre_{c,m}\right) + \left(\sum_{n=0}^{N} \beta_n Treat_c \times Post_{c,n}\right) + \varepsilon_{i,c,t}, \quad (1)$$

where $FTA_{i,c,t}$ indicates whether defendant i fails to appear for an arraignment scheduled in courthouse c in month t, θ_t denotes arraignment-month fixed effects, and η_c denotes courthouse fixed effects. The variables $Treat_c \times Pre_{c,m}$ identify those individuals with arraignments at eventually treated courthouses occurring m months prior to the treatment, while the variables $Treat_c \times Post_{c,n}$ identify those with arraignments at eventually treated courthouses occurring n months after the treatment. The variable $Treat_1 \times Pre_{c,1}$ would indicate those with arraignments in a treated courthouse one month before the courthouse is treated and is excluded from this equation, as it provides the base group to which treated individuals in other periods are compared. Finally, $\varepsilon_{i,c,t}$ captures individual-specific variation.

If each court was only treated once, and we had a balanced panel in the sense that we observed at least N months after treatment and M months pre-treatment for each treated court, then each β_n coefficient would capture the impact of the treatment on the treated n months after treatment occurred, while each γ_m coefficient would help assess the believability of the parallel trends assumption in the sense that if parallel trends held pre-treatment, all of these coefficients should be statistically indistinguishable from zero.

A robust literature has developed on how this type of approach can identify the ATT even in settings where treatments happen at different times across units (Baker et al., 2026). However, a few factors complicate employing this standard approach in our context. First, courthouses in our sample are treated not only at different times, but most were treated more than once. Moreover, as shown in Table 1, in some courthouses these multiple treatments occurred in close time proximity, while in others they were months apart. This is not something a specification like equation (1) directly allows.

A second complicating factor is that we do not have a balanced panel. Given our data runs through mid-April 2026, Table 1 also reveals that we have observations for some treated courthouses going for several months post-treatment, while for other treated courthouses we only have observations within a month of treatment. Therefore, the group of courthouses being used to identify the β_n coefficients in equation (1) would change each month subsequent to treatment. This means a specification like equation (1) cannot identify changes in treatment effects over time since treatment, as such changes will be conflated with changes in sample composition.

Our third notable complication is that it is not clear SUTVA will hold in our context, as treatments at one courthouse may impact defendant behavior at others. Notably, the first ICE activity at an LACS occurred on May 1, 2025, at the Edeleman Courthouse, which is the juvenile courthouse and not one of the courthouses for which we were able to obtain data. However, this ICE activity at Edeleman Courthouse, and subsequent ICE activity at other courthouses, may have impacted defendant behavior at courthouses that had not yet been treated or never will be treated, in the sense that such actions may have updated defendants' beliefs regarding the likelihood of ICE presence at their courthouse on the day of their arraignment hearing.

We attempt to deal with these issues via a couple of different regression specification strategies that vary a little from the standard TWFE event-study in (1). First, we estimate

a modified version of this TWFE event-study equation via the following specification:

$$FTA_{i,c,t} = \theta_t + \eta_c + \left(\sum_{m=2}^{4} \gamma_m Treat_c \times PreLA_m \right) + \beta_0 Treat_c \times Pre_{c,t} + \beta_1 Treat_c \times Post_{c,t} + \varepsilon_{i,c,t}. \tag{2}$$

As before, $FTA_{i,c,t}$ indicates whether defendant i fails to appear for an arraignment scheduled in courthouse c in month t, θ_t denotes arraignment-month fixed effects, η_c denotes courthouse fixed effects, and $\varepsilon_{i,c,t}$ again captures individual-specific variation.

In this specification, however, the event-time indicators divide our sample into three distinct stages. The variables $Treat_c \times PreLA_m$ identify arraignments at eventually treated courthouses occurring m months prior to the first ICE activity anywhere in the LASC system, which occurred on May 1, 2025. So, for example, if a defendant's arraignment is at an eventually treated courthouse but is scheduled in mid-March 2025, then $Treat_c \times PreLA_2$ equals one, while all of the other event-time indicators equal zero. As discussed earlier, we omit the indicator for the month immediately preceding the first ICE activity at an LASC, which serves as the base period for calculating each difference.

The variable $Treat_c \times Pre_{c,t}$ identifies defendants whose arraignment occurs at an eventually treated courthouse after ICE activity first occurred in the LASC system but before ICE activity occurs at the defendant's own courthouse c. Finally, our coefficient of primary interest is the one on the variable $Treat_c \times Post_{c,t}$, β_1, which identifies defendants whose arraignment is scheduled within 30 days after ICE activity at their own courthouse c.

Importantly, in estimating equation (2), we exclude from our sample all defendants whose arraignments were scheduled more than 30 days after the most recent ICE activity at each treated courthouse. So, for example, ICE activity occurred twice at the West Covina Courthouse, once on June 11, 2025 and again on April 4, 2026. This means that our treated population at the West Covina Courthouse includes only those whose arraignments were scheduled either between June 11, 2025 and July 10, 2025, or between April 4, 2026 and the end of our sample on April 16, 2026. Importantly, this means that our treatment group

includes defendants from each courthouse that saw ICE activity, and those deemed treated in this treatment population all had ICE activity at their courthouse within 30 days of their arraignment hearing. This also means that everyone in our control group had an arraignment at a courthouse that was never treated by ICE. Therefore, we avoid the"forbidden comparison" problem with respect to temporally staggered treatments identified by Goodman-Bacon (2021), where later treated units are compared to earlier treated units.

This means identification of β_1 comes from comparing changes in FTA rates at courthouses experiencing recent ICE activity with contemporaneous changes at never-treated courthouses, conditional on month fixed effects that account for any general changes in FTAs over time. The β_1 coefficient therefore captures the incremental effect of ICE activity at a defendant's own courthouse in the previous 30 days above-and-beyond any broader effects associated with ICE activity elsewhere in the LASC system. Examining whether the pre-treatment coefficients γ_m are not statistically different than zero allow us to assess whether eventually treated and never-treated courthouses followed similar trends before ICE activity began. Finally, examining whether the coefficient β_0 is not statistically different than zero allows us to assess whether eventually treated courthouses continued to follow the same trends as never-treated courthouses after the first ICE activity in Los Angeles, but prior to ICE activity at that courthouse. For a detailed discussion of the identifying variation in the data that we use to estimate our parameters, see Appendix A and Figure A1.

Our baseline event-study specifications report standard errors clustered at the courthouse-month level, allowing for arbitrary dependence among defendants appearing at the same courthouse during a given month. It is possible, however, that ICE activity and unobserved determinants of court appearance may be correlated across months within courthouses. One solution is to cluster at the courthouse level; however, with only 21 courthouses, conventional courthouse-level clustered inference may be unreliable. Therefore, as a robustness check, we supplement our baseline inference with wild cluster bootstrap tests clustered at the courthouse level.

The empirical approach described above is the closest we can get to the standard TWFE event-study design outlined in equation (1) in our context. This event-study-type approach allows us to examine whether there appear to be differential pre-treatment trends, and it imposes relatively few restrictions on the evolution of FTA rates over time. However, as with any TWFE event-study design, data prior to the one period before treatment are not used to help identify the treatment effect per se, but rather are only used to help assess the plausibility of the parallel trends assumption. Therefore, to more fully exploit the power of our data, we also employ a second complementary DiD-type approach where we estimate specifications that impose linear time trends.

Specifically, in this second approach, we estimate an equation of the following form:

$$FTA_{i,c,t} = \eta_c + \gamma_0 Week_t + \gamma_1 (Treat_c \times Week_t) + \beta_0 PostLA_t + \beta_1 (Treat_c \times Post_{c,t}) + \varepsilon_{i,c,t}. \quad (3)$$

As before, $FTA_{i,c,t}$ indicates whether defendant i fails to appear for an arraignment scheduled in courthouse c in month t, η_c denotes courthouse fixed effects, $Week_t$ is a counter variable capturing the scheduled week of arraignment, and $Treat_c \times Week_t$ is an interaction between an indicator for an eventually treated courthouse and the weekly count variable. The variable $PostLA_t$ is an indicator for an arraignment happening after the first ICE activity at any LASC, and $Treat_c \times Post_{c,t}$ is an indicator for having an arraignment at a treated courthouse 0-30 days after treatment (i.e., 0-30 days after ICE activity at that courthouse). Finally,$\varepsilon_{i,c,t}$ once again captures individual-specific variation.

We again exclude from our sample all defendants whose arraignments were scheduled at a treated court more than 30 days after the most recent ICE activity at that courthouse. Given this, under this specification in equation (3), γ_0 captures the pre-ICE in LA time weekly time trend in FTAs, while γ_1 captures the extent to which eventually treated courthouses differed in their pre-ICE time trend relative to never-treated courthouses. In this way, a γ_1 of approximately zero would indicate that there were parallel trends pre-ICE between

eventually treated and never courthouses.

The coefficients of primary interest are β_0, which captures whether there was a break in the FTA time trend at all courthouses after the first ICE activity at an LASC, and especially β_1, which captures whether there was an additional break in the FTA time trend at treated courthouses in the 30 days after treatment relative to never-treated and not yet treated courthouses. This β_1 coefficient should be quite analogous to the β_1 coefficient coming from equation (2).

Finally, we also estimate a slightly modified version of equation (3) where instead of using common weekly time trends for the untreated and treated courthouses respectively, we estimate specific weekly time trends for each courthouse pre-ICE, but with the same $PostLA_t$ and $Treat_c \times Post_{c,t}$ variables. This is a more flexible approach, but doesn't allow for us to directly examine whether parallel trends held prior to the first ICE activity at an LASC.

Because these time trend DiD approaches break our data into weekly time blocks, we cluster our standard errors by court-week in these specifications. Again though, as a robustness check, we also employ the wild cluster bootstrap.

4 Results

Figure 1 presents the results from the TWFE event-study approach as described by equation (2). Looking first at Panel (a), which reports our estimates for all misdemeanors, we can see that none of the coefficients associated with treated courthouses prior to the first ICE activity at an LASC on May 1, 20225 (denoted -4, -3, and -2 in the figure and correspond to the γ_m coefficients in equation (2)) are statistically different than zero, and thus we cannot reject the hypothesis that these coefficients are jointly equal to zero ($p = 0.602$, see Appendix Table A2). Similarly, the coefficient corresponding to arraignments after the first ICE activity in LA but before ICE activity at one's own court (denoted 0 in the figure and corresponds to

the β_0 coefficient in equation 2) is also close to zero and not statistically significant. These findings are consistent with our parallel trends assumption.

By contrast, the coefficient associated with recent ICE activity at the defendant's own courthouse (denoted 1 in the figure and corresponds to the β_1 coefficient in equation 2) is positive and statistically significant. The estimated coefficient of 0.052 implies that an arraignment occurring within 30 days after ICE activity at a defendant's assigned courthouse is associated with a 5 percentage-point increase in the probability of FTA. Figure 1 reports 95 percent confidence intervals using standard errors clustered at the courthouse-month level. As noted above, however, we also compute wild cluster bootstrap p-values, clustered at the courthouse level (see Appendix Table A2). Our estimated treatment effect for all misdemeanors is significant at the 5 percent level using courthouse-month clustered standard errors, while the courthouse-level wild cluster bootstrap yields a p-value of 0.076 (none of the other estimated coefficients are even close to statistically significant at the 10 percent level under either the clustered standard errors or the wild clustered bootstrap p-value).

Relative to an FTA rate of approximately 35 percent at these courthouses prior to the onset of ICE activity in LASC, our estimated treatment effect represents an increase in FTAs of approximately 14 percent. As emphasized in Section 3, this coefficient should be interpreted as the incremental effect of recent ICE activity at the defendant's own courthouse above and beyond any general effect associated with ICE activity elsewhere in the LASC system.

These findings in Panel (a) of Figure 1 suggest a significant increase in FTA rates at each courthouse in response to recent ICE activity at that courthouse, and provide evidence consistent with the identifying assumption that FTA rates at eventually treated and never-treated courthouses would have evolved similarly in the absence of local ICE activity. However, while not statistically significant, it is notable that the coefficient corresponding to arraignments in eventually treated courthouses 4 months before the first ICE activity at an LASC is relatively large in magnitude. This appears to be due to something specifically

happening at the Pasadena Courthouse in January of 2025, as when we omit that courthouse from our sample, Figure 1 Panel (b) shows that this particular coefficient more than halves in magnitude, but the rest of our results remain essentially the same.

Panels (c) and (d) of Figure 1 suggest that the effect of ICE activity on FTAs tends to be more concentrated among drug misdemeanors than other types of misdemeanors. Notably, the estimated effect of recent ICE activity at the defendant's own courthouse (the coefficient corresponding to 1 in Panels (c) and (d) of Figure 1) is 0.097 for drug misdemeanors compared with 0.031 for non-drug misdemeanors. However, when we stratify the sample in this way, our standard errors become larger, meaning these coefficients are no longer statistically different from zero at the 5 percent level.[6] Once again, though, for both drug misdemeanors and non-drug misdemeanors, the pre-treatment coefficients are all close to zero in magnitude, and we cannot reject that they are jointly equal to zero at standard significance levels (again, see Appendix Table A2 for inference using courthouse-level wild cluster bootstrap procedure).

Figure 2 examines the sensitivity of these estimates to individual courthouses by sequentially re-estimating our results after omitting each courthouse one at a time, including never-treated courthouses. For all misdemeanors (Panel a), the estimated treatment effect associated with ICE activity at one's own courthouse within 30 days prior to arraignment remains positive and quite stable, with estimates ranging from 0.036 to 0.062 across all courthouse omissions, with these coefficients being statistically significant at the 5 percent level in each leave-one-out specification. Thus, no individual courthouse appears to be driving our results.

In terms of drug misdemeanors versus non-drug misdemeanors, the leave-one-out results are generally consistent with the results discussed above. As can be seen in panels (b) and (c) of Figure 2, while any given coefficient is rarely statistically significant once we stratify the sample in this way, the coefficients associated with drug misdemeanors tend to be more than

[6]Using conventional standard errors clustered by courthouse-month, the coefficient of 0.097 for drug misdemeanors is only statistically different from zero at the 10 percent level. Using a wild cluster bootstrap, the p-value on this coefficient is 0.113.

twice as large as the coefficients for non-drug misdemeanors. Interestingly, though, panel (b) shows that when we drop the Pomona Courthouse in the context of drug misdemeanors, which was one of the never-treated courthouses, our estimated treatment effect goes up substantially and is statistically significant at the 1 percent level. This suggests FTAs for drug misdemeanors at the Pomona Courthouse were generally higher during the months when there was ICE activity at other courthouses. This is notable, as the city of Pomona itself is relatively large (approximately 148,000), and according to the U.S. Census, over 71 percent of the population identify as Hispanic. Anecdotally, we are also aware of significant ICE activity at Home Depot locations in Pomona and at a Pomona Day laborer center over the time period we examine.

As discussed in our methodology section, we also employ a second empirical approach, where we employ pre-ICE in LASC time trends and consider whether there are breaks from these time trends following both the first ICE activity at any LASC and following ICE activity at one's own courthouse. The results of these specifications are shown in Table 2. The top panel of Table 2 shows the results with a linear weekly time trend and an interaction between this linear time trend and an indicator for eventually treated courthouses. The lower panel shows the results of analogous specifications where we use court-specific weekly time trends instead of more global time trends. In both panels, the coefficients of interest are the one on "Post-first ICE at an LASC", which captures whether there is a trend break in FTAs at all LASCs after the first ICE activity on May 1, 2025, and the one on "0-30 days post-ICE at own courthouse", which captures whether there is a trend break at treated courthouses in the 30 days after ICE activity at that courthouse.

Looking first at the results for all misdemeanors in the top panel of Table 2, the coefficient on the linear time trend is negative and statistically significant, though small in magnitude. This means that in the four months leading up to the first ICE activity at an LASC, FTA rates had been falling very slightly. The interaction between this time trend and the indicator for eventually treated courthouses is extremely small in magnitude and not statistically

significant, meaning the time trends did not differ between the never treated and eventually treated courthouses pre-ICE in LA. The coefficient on "Post-first ICE in LA" is 0.018 and statistically significant at the 5 percent level, suggesting FTA rates increased slightly across all LASC sites after the first ICE activity at a courthouse in LA. Finally, the coefficient on the indicator for ICE activity at one's own courthouse within 30 days before arraignment, which is our coefficient of primary interest, is 0.033 and statistically significant at the 1 percent level. This estimate suggests that recent ICE activity at one's own courthouse increased FTA rates by just under 10 percent (3.3 percentage points on a base of 35 percent FTA rate pre-ICE in LA).[7] In all specifications, there does not appear to be any notable difference in pre-ICE time trends between never treated and eventually treated courthouses.

Moving to the results for all misdemeanors in the bottom panel of Table 2, where we look at deviations from courthouse-specific time trends, we see the results are quite similar. Once again, when looking at all misdemeanor hearings, we see a slight increase in FTAs of 1.6 percentage points at all courthouses following the first ICE activity in LA, and a further increase of 2.8 percentage points in treated courthouses in the 30 days after treatment.

The remaining specifications in Table 2 again suggest that drug cases are the primary drivers of these results. As can be seen, in the specifications associated with both the top and bottom panels, the coefficient on the indicator for ICE activity at one's own courthouse within 30 days of arraignment (our coefficient of primary interest) is over twice as large for drug cases than all other types of cases, and is statistically significant at the one percent level for drug cases in both the top and bottom panels.

5 Addressing Potentially Targeted Treatment

A potential concern with respect to our methodology and results above is that ICE activity is not randomly assigned across courthouses. While persistent differences between treated

[7]The coefficient is statistically significant at the 1 percent level using courthouse-week clustered standard errors and at the 5 percent level using courthouse-level wild cluster bootstrap inference (p = 0.045).

and untreated courthouses will be accounted for by our inclusion of courthouse fixed effects, a more consequential concern is that the timing of ICE activity may be correlated with changes in other determinants of FTAs, such as local enforcement practices or defendant composition. Our event-study specification allows us to examine differential trends before ICE activity began in the LASC system, while our complementary specifications allow for courthouse-specific linear time trends. Nevertheless, these approaches cannot rule out unobserved courthouse-specific shocks that coincide with ICE activity and independently affect FTA rates during the subsequent 30 days.

To investigate whether the timing of ICE activity was endogenous to changes in courthouse FTA rates, we conduct two additional analyses. First, using an event study employing treated courthouses with 15-day time increments before and after each treated courthouse's first ICE visit, we find no statistically significant increases in FTAs in the days leading up to the first ICE visit, while FTAs rise by approximately about 3 percentage points in both the first 14 days after the first ICE visit and 15 to 30 days after the first visit (see Appendix B for a more detailed discussion of this analysis and Panel A in Table A3 for results). Second, we examine whether changes in FTA rates over the previous 4 weeks at each courthouse are predictive of where ICE activity occurs and find little evidence of any such effect (see Appendix B and Panel B in in Table A3).

In general, these results provide little indication that recent changes in courthouse FTA rates predicted the timing of the ICE visits.

6 Conclusion

Using various difference-in-differences designs, we find that recent ICE activity at Los Angeles County courthouses increased failure-to-appear rates among misdemeanor defendants. Failure to appear rates appear to increase by 3 to 5 percentage points among defendants whose arraignments are scheduled within 30 days after ICE activity at their assigned court-

house relative to what would have occurred in the absence of such activity, an increase of about 10 to 14 percent relative to the pre-ICE FTA rate at these courthouses.

The effect of such ICE activity on FTAs is concentrated among defendants charged with drug misdemeanors, who also had the highest pre-ICE FTA rates among the misdemeanor crime categories we examined separately (which also include violent/property, DUI, other traffic). For these misdemeanor drug defendants, recent ICE activity at their courthouse increase FTA rates by approximately 7 - 9 percentage points, which is again about a 14 percent increase relative to the pre-ICE FTA rate of about 56 percent for these defendants.

Overall, our results identify a potential tension between federal immigration enforcement at courthouses and the operations of these courts. Courthouses depend on defendants and other participants appearing for their required hearings. Enforcement activity that increases the perceived consequences of entering a courthouse can therefore impose spillovers on the judicial system by increasing nonappearance and leaving cases unresolved. Our findings suggest that these indirect effects are quantitatively meaningful and should be considered alongside the direct enforcement consequences of immigration activity at state courthouses.

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Table 1: ICE Visits by Courthouse and Month

	2025							2026				
Courthouse	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	Total
Alhambra	–	–	–	–	–	–	–	4	3	8	8	23
Bellflower	–	–	–	–	–	2	–	1	1	–	–	4
Burbank	–	–	–	–	–	–	–	–	–	–	–	0
Clara Shortridge Foltz	1	1	–	–	–	1	–	–	–	–	–	3
Compton	–	–	–	–	2	–	–	–	–	–	–	2
Downey	–	–	–	1	–	–	–	–	–	–	–	1
El Monte	–	1	1	–	–	–	1	1	–	–	–	4
Glendale	–	–	–	–	–	–	–	–	–	–	–	0
Gov. George Deukmejian	–	–	–	–	–	–	1	–	–	1	–	2
Inglewood	–	–	1	–	–	–	–	–	–	–	–	1
Los Angeles Airport	1	–	–	–	1	1	–	–	1	–	–	4
Metropolitan	–	–	–	–	–	–	–	1	1	–	–	2
Michael Antonovich	–	1	–	–	–	2	1	3	1	1	–	9
Norwalk	–	–	–	–	–	–	–	–	2	–	–	2
Pasadena	–	–	–	–	–	–	–	–	–	–	–	0
Pomona	–	–	–	–	–	–	–	–	–	–	–	0
San Fernando	1	–	–	–	1	–	–	–	–	–	–	2
Santa Clarita	–	–	–	–	–	–	–	–	–	2	–	2
Torrance	–	–	–	1	–	–	1	–	1	2	–	5
Van Nuys West	1	–	1	–	1	1	1	–	2	–	–	7
West Covina	1	–	–	–	–	–	–	–	–	–	1	2
Total visits												**74**

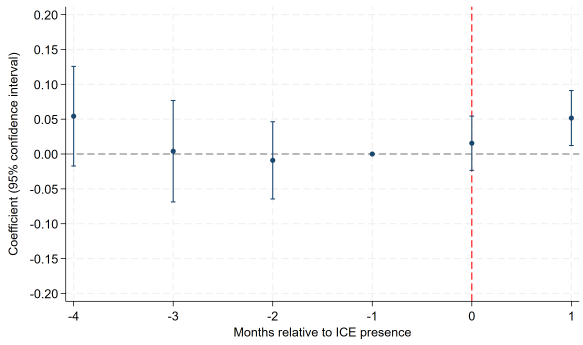
Notes: The table reports recorded ICE visits to Los Angeles Superior Court courthouses by month from June 2025 through April 2026. A dash indicates no recorded visits. Of the 74 reported visits, 17 resulted in arrests. Santa Clarita, Michael Antonovich, and Alhambra have no treated individuals more than one month after their last recorded ICE visit.

Table 2: Estimation Using Time Trends

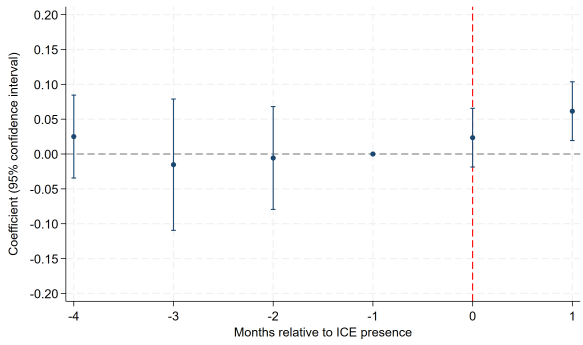
	All Misd	Drug	Vio/Prop	DUI	Oth Traffic
Panel 1 - Linear Global Time Trends					
Weekly time trend	-0.001***	-0.003***	-0.001**	0.000	-0.001**
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Weekly time trend* Treated Courthouse	0.000	0.001	-0.001	-0.000	0.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Post-first ICE at an LASC	0.018**	0.016	-0.002	-0.010	0.050***
	(0.009)	(0.018)	(0.013)	(0.011)	(0.016)
	[0.187]	[0.295]	[0.889]	[0.609]	[0.009]
0-30 days post-ICE at own courthouse	0.033***	0.072***	0.033**	0.014	0.024
	(0.011)	(0.021)	(0.016)	(0.014)	(0.018)
	[0.045]	[0.029]	[0.063]	[0.451]	[0.326]
Court Fixed-Effects	yes	yes	yes	yes	yes
Observations	60,502	13,384	17,708	13,976	15,434
R-squared	0.035	0.059	0.043	0.015	0.052
Panel 2 - Linear Court Time Trends					
Post-first ICE at an LASC	0.016*	0.017	-0.005	-0.005	0.044***
	(0.009)	(0.018)	(0.014)	(0.011)	(0.016)
	[0.374]	[0.461]	[0.141]	[0.335]	[0.009]
0-30 days post-ICE at own courthouse	0.028**	0.078***	0.026	0.018	0.003
	(0.011)	(0.023)	(0.017)	(0.017)	(0.019)
	[0.027]	[0.023]	[0.444]	[0.513]	[0.339]
Court Fixed-Effects	yes	yes	yes	yes	yes
Court Weekly Time Trends	yes	yes	yes	yes	yes
Observations	60,502	13,384	17,708	13,976	15,434
R-squared	0.038	0.065	0.046	0.018	0.057

Notes: The dependent variable is an indicator for failure to appear (FTA) at arraignment. The sample includes misdemeanor cases after January 1, 2025, excluding defendants potentially held in custody until arraignment and arraignments occurring more than 30 days after ICE activity at the defendant's assigned courthouse. Panel 1 includes a common linear weekly time trend and its interaction with an indicator for courthouses that experienced ICE activity, allowing treated courthouses to have a different linear trend. Panel 2 replaces these terms with courthouse-specific linear weekly time trends. Both panels include courthouse fixed effects. "Post-first ICE at an LASC" indicates the period following the first observed ICE activity anywhere in the Los Angeles Superior Court system. "0–30 days post-ICE at own courthouse" identifies arraignments occurring within 30 days after ICE activity at the defendant's assigned courthouse. The coefficient on the latter variable captures the additional difference in FTA associated with own-courthouse ICE activity, beyond the systemwide post-ICE differential. Standard errors, reported in parentheses, are clustered at the courthouse-week level. Square brackets report two-sided wild cluster bootstrap p-values for the ICE treatment coefficients, clustered at the courthouse level, using Webb six-point weights and 9,999 replications. Significance stars are based on conventional standard errors clustered at the courthouse-week level: $p < 0.10$, $p < 0.05$, and $p < 0.01$.

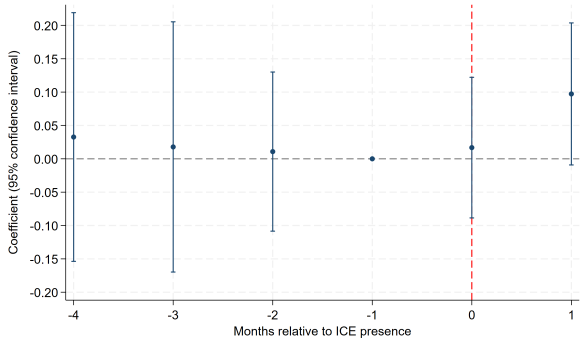
Figure 1: ICE Presence at Courthouses and Failure to Appear



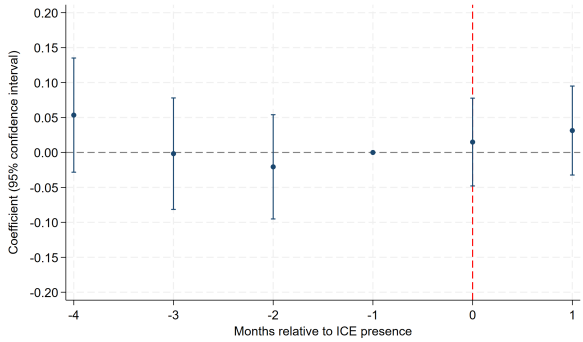
(a) All Misdemeanors



(b) Excluding Pasadena



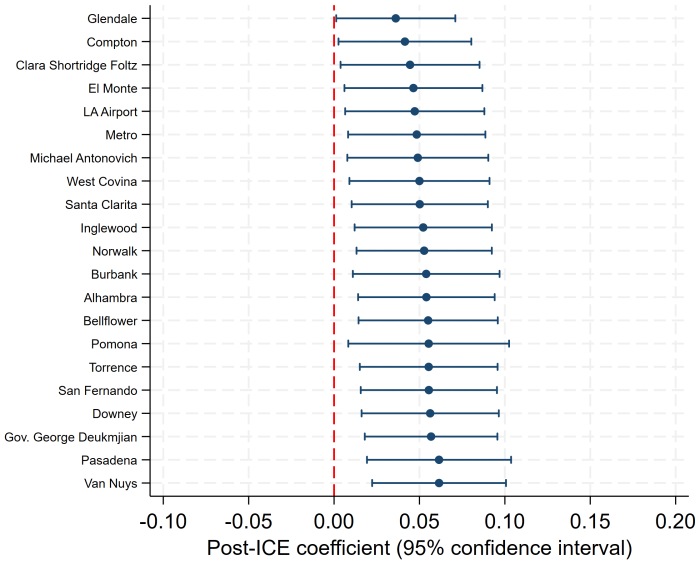
(c) Drug Misdemeanors



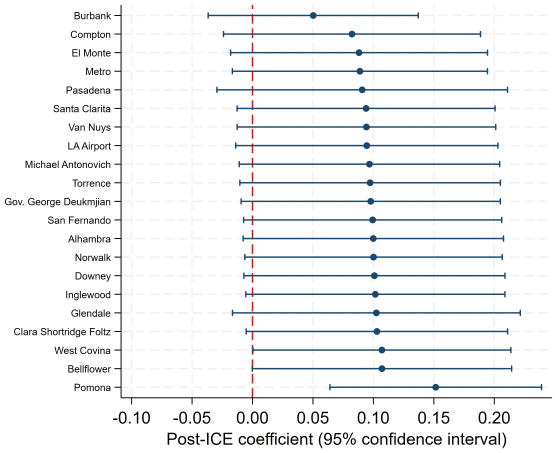
(d) Non-Drug Misdemeanors

Notes: The figure presents event-study estimates of the relationship between ICE activity at Los Angeles Superior Court (LASC) courthouses and defendants' failure to appear (FTA) at arraignment. The sample excludes defendants potentially held in custody. Never-treated courthouses serve as the control group. Coefficients at -4, -3, and -2 compare FTA rates at eventually treated courthouses with never-treated courthouses in successive pre-treatment periods. The omitted reference period, -1, is the 0–30 day period preceding the first observed ICE activity anywhere in the LASC system. The coefficient at 0 captures the differential for eventually treated courthouses before ICE activity at their own courthouse, after ICE activity began elsewhere in the LASC system. The coefficient at 1 captures the additional differential for arraignments within 30 days following ICE activity at the defendant's assigned courthouse. All specifications include courthouse and calendar-month fixed effects. Error bars represent 95% confidence intervals based on standard errors clustered at the courthouse-month level.

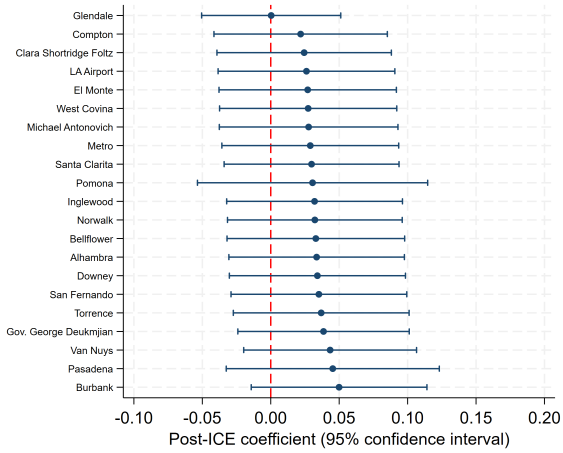
Figure 2: Sensitivity to Omitting Individual Courthouses



(a) All Misdemeanors



(b) Drug Misdemeanors



(c) Non-Drug Misdemeanors

Notes: The figure presents a leave-one-courthouse-out sensitivity analysis of the association between ICE activity at Los Angeles Superior Court (LASC) courthouses and defendants' failure to appear (FTA) at arraignment. Each point reports the estimated post-ICE coefficient from a separate regression that excludes the courthouse named on the vertical axis. Courthouses are ordered by the resulting coefficient estimate within each panel. The post-ICE coefficient captures the additional differential in FTA for defendants whose arraignments occurred within 30 days following ICE activity at their assigned courthouse, relative to never-treated courthouses and beyond any general differential associated with ICE activity elsewhere in the LASC system. The sample excludes defendants potentially held in custody. Panel A includes all misdemeanors, Panel B includes drug misdemeanors, and Panel C includes non-drug misdemeanors. Both treated and never-treated courthouses are sequentially excluded. All specifications include courthouse and calendar-month fixed effects. Horizontal bars indicate 95% confidence intervals based on standard errors clustered at the courthouse-month level.

Appendix

A Identification Strategy

To provide better intuition for the identifying variation we are using from the data, consider Figure A1. This figure supposes we just have 3 (illustrative) courthouses. LASC1 is never treated. LASC2 is treated once on November 1, 2025. LASC3 is treated twice, once on July 1 of 2025 and again on October 1, 2025. Furthermore, recall the first ICE activity at an LASC was on May 1 of 2025, though the courthouse where this activity took place was the juvenile courthouse and is not in our sample. As stated above, we remove from our data those defendants whose arraignments were scheduled more than 30 days after the most recent ICE activity at their courthouse. These removed defendants are indicated by the red blocks in Figure A1. The green blocks in Figure A1 correspond to defendants whose arraignments were scheduled within 30 days after ICE activity at their courthouse, which are those for whom $Treat_c \times Post_{c,t}$ equals 1. Finally, the blue blocks in Figure A1 correspond to defendants whose arraignments were scheduled after the first ICE activity at an LASC on May 1 of 2025, but before the first ICE activity at their own courthouse, which are those for whom $Treat_c \times Pre_{c,t}$ equals 1.

Under these example courthouses, we essentially identify our primary coefficient of interest, β_1, by taking the difference in FTA rates between those defendants with arraignments in LASC2 scheduled during the green November 2025 block and those with scheduled arraignments in LASC2 in the black April 2025 block, and comparing this difference to the difference in FTA rates between those with arraignments in LASC1 in November of 2025 versus April of 2025. A similar calculation is made for those with arraignments in LASC3 relative to those with arraignments in LASC1, but in this case there will be two different comparisons: FTA rates in July of 2025 (green block) to April 2025 (black block), and October 2025 (green block) to April of 2025 (black block). Our estimate of β_1 will then essentially be the average over each of these calculations, weighted by the number of defendants in each

of these court/month time blocks.

Continuing along these lines, our methodology will identify the β_0 coefficient by doing analogous comparisons to those described above, but for the blue time blocks (rather than the green blocks) relative to the black block in the treated courthouses, and comparing these changes to the analogous months relative to April 2025 in LASC1. The γ_m coefficients are also identified analogously, but for each month prior to April 2025. For example, for February 2025 (two months before first ICE at an LASC), γ_2 is identified by first comparing FTA rates among those with arraignments in February 2025 to FTA rates among those with arraignments in April 2025 in LASC2 and LASC3, then comparing these differences to the analogous difference in LASC1.

Given this particular setup, it is still the case that the pre-treatment γ_m coefficients in equation (2) should be statistically indistinguishable from zero if there are no systematic differential pre-trends between eventually treated and never-treated courthouses. Similarly, if expectations regarding subsequent ICE activity evolve similarly among defendants at never-treated and eventually treated courthouses after the first ICE activity at an LASC but before any ICE activity at one's own courthouse, as would be expected under parallel trends, the coefficient β_0 in equation (2) should also be statistically equal to zero. These tests cannot establish that parallel trends would have continued into the treatment period, but they allow us to examine whether treated and untreated courthouses exhibit differential FTA trends prior to local treatment.

B ICE Targeting and Pre-Treatment FTA Trends

One key threat to identification is that the timing of ICE activity at a specific courthouse may be correlated with changes in that courthouse's characteristics that independently affect FTA rates. To investigate this possibility, we conduct two supplementary analyses.

First, we examine whether ICE visits coincided with changes in FTA rates at particular

courthouses. Although the event study in the main text examines differential FTA trends before ICE activity began anywhere in the LASC system, it cannot establish whether FTA rates were already changing at a particular courthouse immediately before ICE arrived there. Such changes could confound our estimates if they reflect factors that also influence the timing of ICE activity. To examine this possibility, we estimate a second event study centered on the first ICE visit to each courthouse. We divide the 45 days preceding and 30 days following the visit into approximately 15-day intervals, using days 45–31 before the visit as the reference period. The specification includes courthouse and calendar-week fixed effects. We use a relatively narrow window to focus on changes surrounding the courthouse-specific visit rather than longer-run trends.

$$FTA_{ict} = \theta_{w(t)} + \eta_c + + \sum_{k \in \mathcal{K}} \lambda_k \mathbf{1}\{t - T_c \in k\} + \varepsilon_{i,c,t}, \tag{4}$$

where T_c is the date of the first ICE visit to courthouse c, and $\mathcal{K}$ denotes the four event-time intervals:

$$\mathcal{K} = \{[-30, -16],\ [-15, -1],\ [0, 14],\ [15, 30]\}\,.$$

The omitted reference period is days 45–31 before the first visit. The specification includes calendar-week fixed effects ($\theta_{w(t)}$) and courthouse fixed effects (η_c).

Table A3, Panel A, reports the results. Neither pre-visit coefficient differs significantly from the reference period, although our point estimate suggests FTAs decline by approximately 2.4 percentage points during the 15 days immediately preceding the first ICE visit relative to 45-31 days before the first ICE visit. By contrast, relative to 45-31 days before the first ICE visit, FTAs increase by 3.1 percentage points in days 0–14 and 3.5 percentage points in days 15–30 following the first visit.

Second, we examine whether recent changes in FTA predict the timing of ICE visits. Using a courthouse-week panel, we regress an indicator for a first ICE visit in the following week on the difference between FTA rates during the preceding four weeks and the four weeks

before that. The specification includes courthouse and calendar-week fixed effects. Panel B of Table A3 reports a small, statistically insignificant relationship (coefficient = -0.0094, $p = 0.895$).

Together, these results provide no evidence that ICE systematically targets courthouses experiencing increasing FTA rates, and the post-visit increases are not entirely driven by the choice of the immediately preceding period as the comparison. However, the relatively small number of first visits, the sensitivity of statistical significance to the reference period, and the possibility of unobserved shocks coinciding with enforcement activity limit the strength of these conclusions.

C Additional Tables and Figures

Table A1: Descriptive Statistics

	All	Not in custody	Ever treated	Never treated
Arraignment FTA (percent)	30.14	37.97	37.13	41.35
	(45.89)	(48.53)	(48.31)	(49.25)
Age at arraignment	37.59	37.80	37.57	38.68
	(12.07)	(12.31)	(12.27)	(12.43)
Assault/battery	0.21	0.12	0.13	0.09
	(0.41)	(0.32)	(0.33)	(0.28)
Property offense	0.18	0.17	0.16	0.22
	(0.38)	(0.38)	(0.37)	(0.42)
Drug offense	0.19	0.22	0.21	0.28
	(0.40)	(0.42)	(0.40)	(0.45)
DUI	0.18	0.23	0.25	0.16
	(0.39)	(0.42)	(0.43)	(0.36)
Driving while license suspended	0.04	0.05	0.05	0.05
	(0.19)	(0.21)	(0.21)	(0.23)
Other misdemeanor/infraction	0.20	0.21	0.21	0.20
	(0.40)	(0.41)	(0.41)	(0.40)
Observations	87,739	60,502	48,423	12,079

Notes: The table reports descriptive statistics for misdemeanor cases heard in Los Angeles Superior Court (LASC) courthouses during the study period. Entries are sample means, with standard deviations in parentheses. All columns apply the five-month pre-ICE observation-window restriction and exclude arraignments occurring more than 30 days after ICE activity at the defendant's assigned courthouse. Defendants are classified as potentially in custody based on administrative case information; this classification does not establish whether a defendant was actually detained at the time of arraignment. These observations are excluded from the main regression sample. ICE exposure is measured using the timing of recorded ICE activity at LASC courthouses.

Table A2: ICE Presence and Failure to Appear: Event-Study Estimates

Specification	Month −4	Month −3	Month −2	Semi-treated	Post-ICE	Joint pretrend p-value	N
	0.054	0.004	-0.009	0.016	0.052**		
	(0.036)	(0.037)	(0.028)	(0.020)	(0.020)		
All misdemeanors	[0.189]	[0.890]	[0.740]	[0.535]	[0.076]	[0.602]	60,502
	0.025	-0.015	-0.006	0.023	0.061***		
	(0.030)	(0.048)	(0.037)	(0.021)	(0.021)		
Excluding Pasadena	[0.436]	[0.751]	[0.878]	[0.416]	[0.087]	[0.909]	57,184
	0.033	0.018	0.011	0.017	0.097*		
	(0.095)	(0.095)	(0.061)	(0.054)	(0.054)		
Drug misdemeanors	[0.850]	[0.863]	[0.898]	[0.795]	[0.113]	[0.975]	13,384
	0.025	-0.025	-0.006	-0.024	0.003		
	(0.060)	(0.052)	(0.061)	(0.051)	(0.052)		
Violent/property	[0.698]	[0.579]	[0.933]	[0.697]	[0.953]	[0.677]	17,708
	-0.045*	0.052**	-0.011	0.006	0.013		
	(0.024)	(0.025)	(0.030)	(0.017)	(0.019)		
DUI	[0.359]	[0.126]	[0.771]	[0.712]	[0.474]	[0.332]	13,976
	0.116	-0.050	-0.028	0.012	0.043		
	(0.073)	(0.054)	(0.050)	(0.034)	(0.033)		
Other traffic	[0.332]	[0.530]	[0.741]	[0.748]	[0.279]	[0.124]	15,434

Notes: The dependent variable is an indicator for failure to appear (FTA) at arraignment. The sample includes misdemeanor cases after January 1, 2025, excluding defendants potentially held in custody until arraignment and arraignments occurring more than 30 days after ICE activity at the defendant's assigned courthouse. The omitted reference period is the month preceding the first recorded ICE activity in the Los Angeles Superior Court system. All specifications include courthouse and calendar-month fixed effects. Standard errors, reported in parentheses, are clustered at the courthouse-month level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Square brackets report two-sided wild cluster bootstrap p-values, clustered by courthouse, using Webb six-point weights and 9,999 replications. The joint pretrend p-value is calculated using the same bootstrap procedure.

Table A3: First-Visit FTA Dynamics and ICE Targeting Diagnostics

Panel A. FTA around the first ICE visit

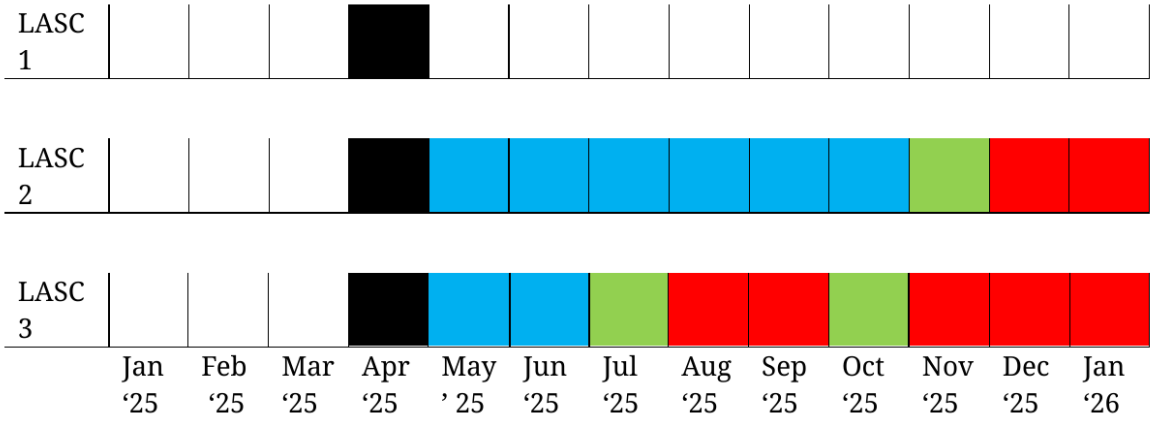
Period relative to first ICE visit	Coefficient	SE	Webb p-value
Days −45 to −31	Reference		
Days −30 to −16	-0.0048	(0.0193)	[0.822]
Days −15 to −1	-0.0241	(0.0157)	[0.168]
Days 0 to 14	0.0313	(0.0243)	[0.300]
Days 15 to 30	0.0350	(0.0229)	[0.203]
Observations		22920	
Courthouses		21	

Panel B. Does prior FTA growth predict a first ICE visit?

Predictor	Coefficient	SE	p-value
Change in FTA, prior eight weeks	-0.0094	(0.0700)	[0.895]
Courthouse-weeks		778	
First visits		17	
Courthouses		21	

Notes: Panel A estimates a linear probability model for arraignment FTA with courthouse and calendar-week fixed effects. The omitted period is days −45 to −31 before a courthouse's first ICE visit. Parentheses show courthouse-clustered standard errors; brackets show two-sided wild cluster bootstrap p-values with courthouse clustering. Contrasts are linear combinations of the coefficients from the same regression. The sensitivity rows use days −30 to −16 as the comparison period rather than days −15 to −1. Panel B is a courthouse-week linear probability model for whether a first ICE visit occurs in the following week. The regressor is the difference between the case-weighted FTA rate in the most recent four completed weeks and that in the preceding four weeks. It includes courthouse and calendar-week fixed effects. Parentheses report courthouse-clustered standard errors; brackets report conventional two-sided p-values (not bootstrap p-values).

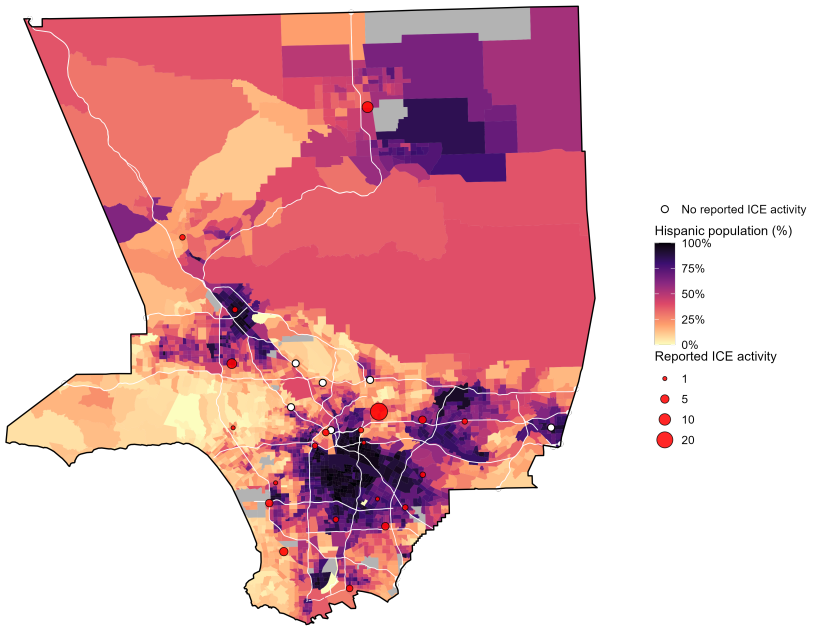
Figure A1: Illustration of Variation Used to Identify the Effects of ICE Courthouse Activity



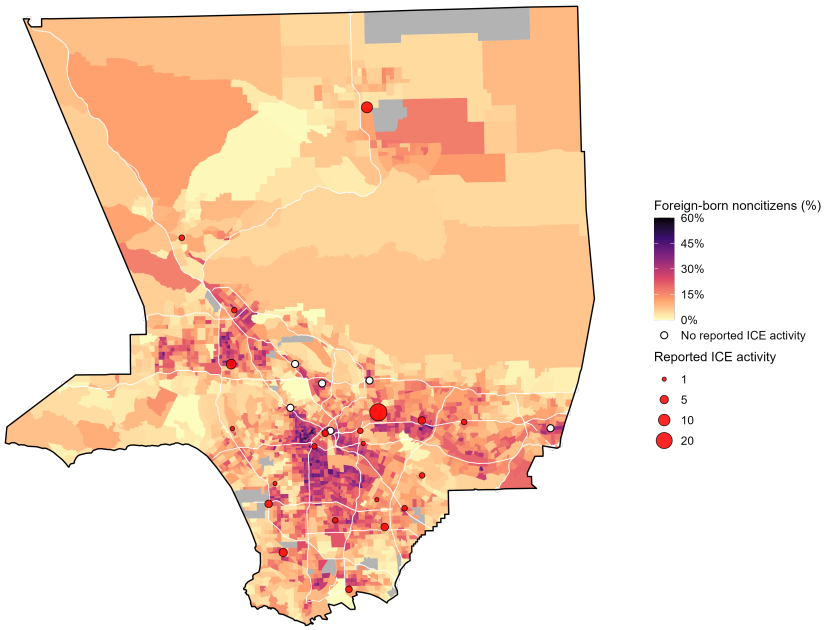
Notes: The figure illustrates the timing variation used to identify the relationship between ICE activity at Los Angeles Superior Court (LASC) courthouses and defendants' failure to appear (FTA) at arraignment. The three rows represent hypothetical courthouses with different histories of ICE activity. Black cells identify the month immediately preceding the first recorded ICE activity anywhere in the LASC system (May 1, 2025), which serves as the omitted reference period. Blue cells identify periods after the first ICE activity in the LASC system but before ICE activity at the defendant's assigned courthouse. Green cells identify the 30-day period following ICE activity at the assigned courthouse. Red cells identify periods more than 30 days after the most recent ICE activity at that courthouse. The empirical specification distinguishes the systemwide post-ICE differential from the additional differential associated with recent ICE activity at the defendant's own courthouse. Arraignments occurring more than 30 days after ICE activity at the assigned courthouse are excluded from the main estimation sample.

Figure A2: ICE Activity at Los Angeles County Courthouses

Panel A: Hispanic Population Share



Panel B: Foreign-Born Noncitizen Population Share



Notes: The figure shows the geographic distribution of reported ICE activity at Los Angeles Superior Court courthouses. Red circles identify courthouses with reported ICE activity, with circle size proportional to the number of reported events; white circles identify courthouses with no reported activity. Census tracts are shaded according to the Hispanic share of the population (Panel A) and the foreign-born noncitizen share (Panel B). The latter includes both documented and undocumented noncitizens. Major freeways are shown in white. The demographic measures describe residents of the surrounding census tracts, who need not be representative of defendants appearing at each courthouse.
Sources: LASC ICE Activity Report; U.S. Census Bureau, 2020–2024 American Community Survey 5-Year Estimates.