Policing the Polls: How High-Profile Arrests Deterred Other Voters With Felony Convictions*

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October 13, 2026

Abstract

In 2022, Florida state police arrested several dozen people for voting while ineligible due to prior felony convictions. These arrests occurred during a period of uncertainty about voting eligibility for people with convictions. We ask whether these highly-visible arrests scared off other apparently-eligible voters with convictions. We use incarceration and voter-file records to measure turnout among Floridians with past convictions, comparing over-time changes in this group to trends among other voters. We estimate that the arrests reduced voting rates among formerly-incarcerated Floridians substantially in the 2022 (primary and general) elections, and that those decreases in turnout persisted into the next election cycle in 2024. These estimates are consistent with a scare-off effect described by voting advocates we interviewed, and demonstrate how visible, punitive targeting of a small number of people can change the behavior of many more.

*We are grateful to seminar participants at Boston University, Columbia University, UC Berkeley, MIT, and Notre Dame, as well as Hannah Walker, for helpful feedback on this project. We thank Kendall Verhovek for excellent research assistance. Authors appear in alphabetical order.

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1 Introduction

In August 2022, Florida Governor Ron DeSantis held a press conference to announce that twenty people across the state were being arrested for voting in the 2020 presidential election while ineligible (Mower, 2022). Everyone arrested had a previous felony conviction, and they had voted during a uniquely confusing election cycle for people with criminal records. In 2019, a ballot measure restoring the right to vote to people with felonies ("Amendment 4") had gone into effect. But it did not cover everyone: the measure excluded people convicted of specific crimes, and a state requirement to pay off fines and fees left many more people uncertain about their eligibility (Sukhatme, Billy and Bagwe, 2023). The voters arrested in August 2022 had voted in 2020 despite having convictions "carved out" from Amendment 4's re-enfranchisement (murder or sexual offenses). They had been allowed to register and vote by local election officials, with some reporting that they believed they were eligible and had been encouraged to register by government employees (Mower, 2022).

In October 2022, the *Tampa Bay Times* obtained police body camera footage of some of the arrests (Mower, 2022). Vivid images of people being dragged from their homes and arrested, with arresting officers often expressing confusion about the charges, spread rapidly through local and national news sources. Voting rights activists worried that the arrests could deter other Floridians from voting. As Neil Volz of the Florida Rights Restoration Coalition, an organization that had pushed for the passage of Amendment 4, put it: "we're seeing individuals who, because of the confusion, might not participate this year" (Shuham, 2022). *Bolts Magazine* reported on a "culture of fear" around voting in Florida and interviewed multiple people with felony records who reported being afraid to vote in 2022 after seeing the arrests (Squires and Garcia, 2022).

Would these highly-visible arrests prevent other Floridians with previous convictions from voting? The literature on election administration in the modern U.S. offers few parallels to this case, and thus few predictions. Most work on the "costs of voting" focuses on widely-

dispersed bureaucratic hassles the state imposes on voters: time spent waiting in line to vote, or traveling to distant polling places or government offices to obtain required IDs (see, for instance, Brady and Mcnulty, 2011; Pomante II, 2025). In this case, we instead see a very small number of voters facing an almost unimaginably high cost. They were arrested and criminally charged, exposing them to all the physical risk and collateral consequences — job loss, social stigma, disrupted relationships — that can accompany prosecution and jail time (Pettit and Western, 2004)

To find appropriate parallels, we look to other contexts. In the U.S. South before the Voting Rights Act of 1965, legal and extralegal practices combined to impose extraordinarily high costs on African Americans who sought to vote. Policies like poll taxes, coupled with the threat of physical violence, kept Black voting rates low (Key, 1949; Grofman, Handley and Niemi, 1992; Gray and Jenkins, 2025). And internationally, research on autocratic regimes' use of highly visible, targeted repression of a few people has found that these tactics can have both short- and long-run suppressive effects on political participation (Young, 2019; Zhukov and Talibova, 2018).

We bring the intuitions of this literature on state repression into the setting of modern U.S. elections, arguing that election interventions that punish a small number of people can shape the turnout of many other similarly-situated voters. We highlight important features of these events: those that visibly target small numbers of people for statuses or actions that are widespread, especially in circumstances of uncertainty about who could face future targeting (either because of unclear eligibility to vote, as in our case, or doubts about the rule of law) seem likely to have the largest effects on voting participation.

In this paper, we test whether the highly-visible arrests of a small number of people in Florida shaped the participation of other potential voters in the state beyond those who were arrested. We use public records of past incarceration in Florida to identify a large group of people in the state most likely to be intimidated out of voting by the arrests, and we use public records of registration and voting to track their participation over time. We

use entropy balancing to construct a set of comparable voters not likely to be intimidated by these arrests (as they have no known felony convictions) and compare the behavior of "treated" Floridians to these untreated ones over time. We find that after the August 2022 arrests, Floridians with past convictions became somewhat less likely to vote in the state's primary the following week, compared to other comparable voters. This gap opened even wider in the November 2022 general election, which followed the October 2022 release of body camera video footage and widespread news coverage of the arrests. And these effects were durable: Two years after the arrests took place, formerly incarcerated Floridians were still less likely to avail themselves of their restored voting rights. Importantly, these effects were confined to Florida and did not appear in New York or Pennsylvania, indicating that there were probably not national trends pushing down the turnout of formerly incarcerated individuals in general in 2022 and 2024.

Like other research on the political participation of people with criminal records, we face data limitations and inferential challenges (White, 2022). Limited state record availability makes it impossible to observe the entire population of interest or definitively confirm individual eligibility for many voters. And the recent expansion of the franchise leaves us with few pre-treatment periods to assess the plausibility of the parallel trends assumption. Thus, we bring in a range of other data sources (qualitative and quantitative) and additional tests to supplement our main analyses and probe possible alternative explanations for our findings. We do not think this paper is the final word on the effects such punitive interventions can have on voter turnout among vulnerable groups in the modern U.S., but the balance of evidence presented here points to turnout decreases attributable to the 2022 voter arrests we study. This project also offers an example of how a causal effect can be "triangulated" in a complex environment. Although a single and unambiguous identification strategy is not available in the context of Amendment 4 and the Florida election police, it is nonetheless an important area of inquiry. We use cross-state placebo tests, along with subpopulation analyses, to demonstrate the plausibility of the parallel trends assumption that underlies

our main analysis. Extended, semi-structured interviews alongside analyses of other groups of justice-involved individuals support the idea that fear drove the turnout reductions we measure.

We study a high-profile election intervention, measuring its effects on turnout and finding that Florida's voter arrests rippled through a much larger population than those directly targeted for prosecution. These estimates suggest widespread intimidation among eligible voters with past convictions. As such, we document an important source (and process) of vote suppression in modern U.S. elections. In doing so, we build on and extend recent work such as Grimmer and Hersh (2024) and Grimmer and Yoder (2022), identifying a set of current election practices that differ qualitatively from more convenience-focused policies and could pose graver threats to voter participation, and focusing our examination on the specific population targeted by this restrictive voting policy. We close by considering other election interventions that could have similar effects.

2 Background

2.1 Voting Eligibility in Florida

In November 2018, Floridians passed Amendment 4 with a 64.5% supermajority of voters (Morse, 2021). The ballot measure promised to automatically restore voting rights for people who had completed their felony sentences, excluding those who had been convicted of murder or sexual offenses. In January 2019, people with past felonies began registering to vote (Allen, 2019). Later that year, the state legislature passed SB 7066, which narrowed voting eligibility by defining sentence completion as including the payment of any outstanding fines, fees, or restitution that someone might owe from their case (Perry, 2019). Litigation over this requirement began immediately, as voting rights advocates argued it would disenfranchise most Floridians with felony convictions and make it impossible for some people to confirm their eligibility status. During the 2020 election, the law remained in place, and many Floridians' voting eligibility remained uncertain given limited information on fine payments

(Smith, 2019; Burch, 2020).

Other changes to Florida's election laws occurred around the same time. In 2021, Florida joined other Republican-controlled states in passing an "election integrity" law in the wake of the 2020 election (Morris, 2023). In Florida's case, SB 90 was enacted in May 2021 and curtailed "ballot harvesting," mandated voter roll audits, and changed ID requirements for mail-in ballots. The next year, Florida legislators passed SB 524, establishing the Office of Election Crimes and Security within the Department of State. On August 19th, 2022, Florida governor Ron DeSantis announced what he described as the office's "opening salvo" against voter fraud (Lopez, 2022). In the press conference described above, DeSantis announced a group of arrests for voting illegally during the 2020 presidential election. Those arrested admitted to voting, but made clear they either thought their voting rights had been restored under Amendment 4 or believed that local election officials would have let them know if they were still disenfranchised under Florida law (Mower, 2022).

These arrests—and the governor's efforts to publicize them—brought substantial public attention to the state's actions. In addition to the governor's press conference in August 2022, the release of police body camera video of some of the arrests in October 2022 drew both local and national media coverage. The video of the arrests was striking, showing mostly Black individuals being arrested outside their homes, sometimes at gunpoint or while partially undressed. Some scenes spread quickly as local and national media outlets covered the story. *The Tampa Bay Times'* coverage on Youtube reached 114 thousand viewers, far outpacing its typical video (fewer than 1,000 views). The audio accompanying these scenes also stirred controversy, as it captured the police carrying out the arrests expressing confusion and skepticism about the actions. One of the officers sent to arrest a man outside his Tampa home in August 2022 was heard saying, "I've never seen these charges before in my entire life." When the person being arrested asked the officer, "Why would you let me vote if I wasn't able to vote?," the recorded officer replied, "I'm not sure, buddy, I don't know." (quoted in Mower, 2022). And as noted above, journalistic coverage of these events

included people reporting being afraid to vote in future (Squires and Garcia, 2022).

2.2 Election Practices and Voting

There is little empirical evidence that most statewide election practices in the modern U.S. substantially affect aggregate voter turnout. Laws like voter identification requirements or mail ballot restrictions can impose disproportionate burdens on some groups of voters (Zhang, 2022) and can sometimes depress turnout among specific affected populations (Fraga and Miller, 2022; Henninger, Meredith and Morse, 2021; Grimmer and Yoder, 2022; Miller et al., 2025). But as Grimmer and Hersh (2024) point out, modern statewide election rules rarely shift aggregate state turnout in ways that are likely to shape election outcomes. Debate persists about whether the remaining active portions of the Voting Rights Act and the threat of court action, coupled with social and political changes that have occurred since the mid-20th century, constrain politicians from enacting policies that would suppress voting participation enough to register in statewide statistics.

But outside of the modern U.S. context, we have substantial evidence of the ways in which election procedures can drastically reshape electorates. Earlier in U.S. history, for example, legal and extra-legal tactics reduced Black voting to near zero in some parts of the South (Salamon and Van Evera, 1973; Key, 1949; Keele, Cubbison and White, 2021; Gray and Jenkins, 2025). The Voting Rights Act of 1965 rapidly removed many restrictive policies across the South and sent in federal election observers to ensure voting access, ushering in dramatic shifts in participation and representation. Black voter registration increased nearly overnight, with Mississippi seeing a more than 800% increase in Black registration rates in under seven years, and these increases translated into shifts in legislative representation and government spending (Fresh, 2018; Cascio and Washington, 2014). American history also demonstrates that government actions need not be labeled as "election policy" to have implications for voting. Studying a policy that was billed as a matter of national security, Komisarchik, Sen and Velez (2022) find that targeted internment of Japanese Americans during World War II left families less likely to participate in politics for generations after

their release from custody, and that these experiences also affected non-interned family members.

Similarly, studies of state coercion in autocratic settings find that harsh state targeting of small numbers of people can have both short- and long-term ramifications for political participation (Earl, 2011; Hassan, Mattingly and Nugent, 2022). Young (2019) explores how violent repression by the Mugabe regime in Zimbabwe inspired fear among opposition members, deterring them from taking action. Zhukov and Talibova (2018) trace the long-run effects of Stalinist repression, finding that places with more experience of state repression (political prosecutions and disappearances) under Stalin continued to have lower rates of participation decades later. They conclude that "repression need not be collective or indiscriminate to have community-level effects" (pg. 267) noting that specific repression such as arrests of individual people can be especially effective because they teach bystanders to avoid any actions that could open them up to similar targeting.

In this paper, we focus on a type of election intervention that has not been well studied in the US elections literature: the targeted arrest and prosecution of a small number of people for voting while ineligible. We draw on comparative politics work on repression and political control to argue that this type of intervention—and the context in which it occurs—has several features that make it especially likely to deter voters, compared to other types of election policy. Here, we lay out conditions for voter deterrence; in the conclusion, we return to consider other types of election activities that could operate in similar ways.

We argue that several key conditions allow voter arrests to produce wider turnout effects beyond the people directly targeted. First, and perhaps most obviously, arrests must be *visible*: other political actors must learn about the targeting in order for it to shape behavior either directly or indirectly (we discuss both possibilities for our case in the next section). Next, there must be *identification*: there must be some other group of voters that sees themselves as similar to those targeted, such that seeing those arrests affects one's own calculation about the costs of voting. The larger the group of existing voters who see

themselves as similar to the people targeted, the larger the possible deterrent effects of the intervention.

Finally, we note the importance of *uncertainty* in who could be targeted next. For voters who identify with the people who have been arrested, we expect that these arrests will inspire fear and concern that they could be next (Young, 2019). Arrest and criminal prosecution carry real risks of material and physical harm, even if they do not yield convictions (Earl, 2005; Kohler-Hausmann, 2018). Seeing others face such extreme costs could lead other voters to question whether it is worth casting a ballot in future (Riker and Ordeshook, 1968; Salamon and Van Evera, 1973). But in a democratic context, we do not think all voters who share the identity of those targeted will expect that they could be arrested. Instead, only people who are uncertain about their voting eligibility (or deeply cynical about the rule of law) will think that voting puts them at risk of future repression. Those who are completely certain that they are eligible to vote will not generally feel the need to avoid voting to minimize the risk of arrest; indeed, in some cases this group of voters who know they are safe could even drive backlash in response to targeted arrests. Thus, the more uncertain people are about their status and whether they could be targeted, the more voter deterrence we should see.

2.3 Hypotheses

As described above, we have seen in other contexts how election practices can dramatically shape who participates and how often. In this section, we draw on features of our case to derive predictions about how the high-profile arrests of a small number of voters could shape voter participation in Florida. To build intuition about the case, we rely on press coverage of the events as well as interviews we conducted with Floridians who have previously been incarcerated, work with those who have, and/or work on civic participation and voting rights. In 2025, we interviewed ten people across these categories, and we incorporate their responses into our discussion of the case. Most of our interviewees hold positions at re-entry or civic organizations, and they are centrally located within networks of people with criminal records.

They are well-situated to report on what happened after the arrests, not just about their own experiences but about the many people who reached out to them after the news broke. We conducted semi-structured interviews focused on people's experiences and recollections of the 2022 election cycle and the arrests, then coded the interview transcripts for themes such as fear or uncertainty about eligibility (see SI Section A.8 for more details of this process).

Our interviews underscore both how visible these arrests were, and how frightening, for people with criminal records in Florida (the group we expect to identify with those targeted). Many interviewees described how they first heard about the news story from family, friends, or professional connections, while others described circulating it to people in their networks. One organization leader, describing a group of formerly-incarcerated people who did outreach among their networks on behalf of her organization, reported: "Not only were we hearing from the bridge makers, the bridge makers were hearing from their community that people were afraid." The news of these arrests clearly reached, and felt relevant to, many Floridians with criminal records.

On seeing people arrested while protesting that they had believed themselves eligible to vote, many people with convictions—even those who previously thought they were eligible—likely found themselves wondering whether they could be next.[1] Our interviewees were largely not deterred themselves (many of them are seasoned organizers and felt secure in their understanding of their own legal eligibility and the importance of voting), but most of them reported hearing people in their orbits express uncertainty and fear of voting after seeing these arrests. As the director of a re-entry organization put it, "You know, you're trying to do your civic duty, and think you can, and you register only to see other people being arrested for it... that chilling effect that we were worried about was happening everywhere." Another interviewee reported, "What I saw was a fear based reaction. So I saw a lot of people afraid to actually, you know, move forward with their rights as a United States citizen."

[1] We distinguish these expectations from the rich literature on how contact with the criminal legal system can affect participation (White, 2022). Here, we are not talking about one's own arrest—or that of a loved one—but seeing press coverage of someone you do not know being arrested.

Of course, it is possible that such events could also yield some countervailing effects: individual people might act against perceived injustice (Valentino and Neuner, 2017; Aytaç, Schiumerini and Stokes, 2018; Biggers and Smith, 2020; Zepeda-Millán, 2017), or organizations focused on the empowerment of people with records might work even harder to mobilize them (Owens, 2014; Walker, 2020). In this case, we do not think such countervailing effects were large, given both the magnitude of the risk faced by potential voters and what we heard from the people we interviewed. Those who were in leadership roles at organizations doing voter-mobilization work reported worrying that their work could put people in harm's way, and pulling back on it. A leader in one prominent rights-restoration organization described his anguish over the situation: "I've been a democracy activist for a long time. 2022 is the first time I ever talked to people and literally said, hey, I wouldn't– you know, if that's your situation and you're not comfortable, then I wouldn't vote. It's a shitty — it's a shitty thing to do as a democracy activist, is to literally be like, yeah, the system's so broken. Don't put yourself in harm's way." Another reported stopping work out of concern that the organization could also find itself in legal jeopardy if it continued doing voter mobilization: "The team that was dispatched by the Governor to investigate and arrest, we heard that they were looking for folks who were putting on these legal clinics and educating formerly incarcerated individuals." We do not expect organizational "countermobilization" efforts to offset individual deterrence effects.

As such, our first hypothesis is:

H1: Floridians with past criminal convictions were less likely to vote after seeing similarly-situated people face arrest and prosecution for voting.

We also expect that these effects should be most pronounced when people were most aware of the arrests, or when the arrests affected people with more uncertainty about their eligibility. The arrests took place less than a week before the August primary in 2022, meaning that some people may not have heard about them before voting in the 2022 primary election. By October, there was police body camera footage of the arrests circulating widely

and drawing substantial national attention. As such, we expect that turnout effects were more pronounced in the 2022 general election (by which time more affected voters in the state would have heard of the story) than in the primary election. Primary election voters also generally have higher political knowledge (e.g., Sides et al., 2020) and might thus be more certain about their own eligibility to vote and less easily deterred, also yielding smaller effects on primary voting.s This yields the second hypothesis:

H2: Turnout effects of arrests were most concentrated in election periods after the arrests became most widely known (2022 general and the 2024 elections).

Finally, given the nature of the "treatment" here, we expect memories of seeing these arrests could linger for years after the original news event, consistent with previous work on state repression and fear (Zhukov and Talibova, 2018). Indeed, the interviewees we spoke with in 2025 were quickly able to remember and describe the events surrounding these 2022 arrests. Some offered vivid recollections of the governor's press conference, the arrest videos, or how they first heard about the arrests from someone in their network. We expect that many voters were also profoundly shaken by this news and could thus be deterred from voting in the long term. Thus, our final hypothesis is:

H3: Turnout decreases seen immediately after the 2022 voter arrests persisted to the following (2024) election cycle.

3 Data and Methods

Our main analysis asks how the arrests of a small number of Florida voters affected subsequent turnout for voters with past felony sentences in the state. We use state sentencing records and voter file data from the vendor L2 Political. We join a robust and growing body of literature that examines the political behavior of people with criminal records by linking these types of administrative datasets (Burch, 2011; White, 2019; Meredith and Morse, 2014; Morris, 2021).

In Florida, the state Department of Corrections makes data publicly available on all

individuals sentenced to state prison for a felony offense since the 1990s. These records only include people sentenced to felony *incarceration*; Floridians convicted of felony offenses but sentenced only to probation were subject to the same disenfranchising policies, but are not included in our dataset. As such, our analysis does not include the full set of Floridians who could potentially have been affected by the arrests we study.[2] We note, however, that it is possible that the demobilizing effects for those sentenced to felony probation might have been smaller than for the population we observe, and thus refrain from extrapolating our estimates to the full population with felony records.

We match the incarceration and voter records using first name, middle name / initial (where available), last name, and date of birth. As we show in SI Section A.1, we likely have very few false positive matches (somewhere in the range of 0.5%). We identify 403,189 Floridians who had previously been incarcerated, but were neither on parole nor re-incarcerated as of October 2022. Of these, 47,916 were registered to vote in our snapshot immediately following the 2022 election (in Section A.2.2 of the SI, we show that our results are consistent if we limit to those registered before the "treatment" of the arrests, per Nyhan, Skovron and Titiunik, 2017).

To measure the effect of high-profile voter arrests on the turnout of formerly incarcerated Floridians, we examine how turnout trends changed for this population, relative to the general electorate, over the 2020–2024 elections. More specifically, we expect that formerly incarcerated Floridians received no "treatment" between the 2020 primary and general elections—in other words, their turnout should have moved in parallel with that of similar registered voters across these two periods. Shortly before the 2022 primary, however, the first wave of arrests of this population occurred, potentially driving down their turnout (again, relative to the rest of the electorate) in the primary. Moreover, as news spread of the arrests—and the body camera footage was released in October—we expect the intensity

[2]We observe about 400,000 previously-incarcerated people; comparing this number to estimates of the 2010 ex-felon population from Shannon et al. (2017) suggests it represents around one-fourth of the state's population of post-sentence individuals with felony convictions.

of the "treatment" increased further between the primary and general elections in 2022 and endured into 2024. General election voters might also be less-knowledgeable about their eligibility than high-information primary voters, thus also making them more susceptible to being dissuaded from voting and leading to larger effects in the general election than in the primary.

There are, of course, many reasons to expect that the turnout of formerly incarcerated Floridians may not have moved in parallel with the rest of the electorate, even absent these arrests. To improve the validity of the parallel trends assumption needed to make causal inferences in this environment, we use a method called "entropy balancing" (Hainmueller, 2012). Entropy balancing assigns untreated units weights such that—in aggregate—they look similar to the set of treated units. Here, we weight registered Floridians who had not been to prison using individual- (party ID; age; race; registration date) and neighborhood-level (median income; share with some college education; unemployment rate; population density) characteristics so that they are similar to the treated voters. All treated voters receive a weight of 1. After constructing the weights, we estimate a two-way fixed effects model, with fixed effects for elections and voters. While all treated voters are considered treated for the first time in the 2022 primary, we allow the treatment effect to vary for the general election, and also for 2024.

4 Results

We begin by presenting our main difference-in-differences approach for the full set of voters with previous felony incarceration sentences. As discussed above, this approach involves first weighting untreated voters to match treated ones using entropy balancing. The first two columns of Table 1 demonstrate the striking extent to which formerly incarcerated Floridians do not look like the rest of the electorate. In particular, they are considerably more likely to be Democrats, Black, and live in lower-income neighborhoods. As the third column of the table shows, the balancing procedure was successful at removing pre-existing

Table 1: Balance Table for Entropy Balancing

Variable	Formerly Incarcerated Voters	Potential Control Voters	Entropy Balanced Control Voters
Birth Date	January 23, 1974	February 21, 1971*†	January 23, 1974†
Share Democrat	49.5%	33.7%*†	49.5%
Share Republican	23.3%	36.2%*†	23.3%
Registration Date	August 26, 2018	June 30, 2008*†	August 26, 2018†
Share White	34.4%	60.7%*†	34.4%
Share Black	49.2%	13.3%*†	49.2%
Share Latino	6.2%	18.2%*†	6.2%
Neighborhood Median Income	$53,256	$73,995*†	$53,256
Neighborhood Share with Some College	51.3%	63.2%*†	51.3%
Neighborhood Population Density	4,155	4,196*†	4,155
Neighborhood Unemployment Rate	7.1%	5.1%*†	7.1%

Notes:

* Mean different from treated voters (t-test, $p < 0.05$).

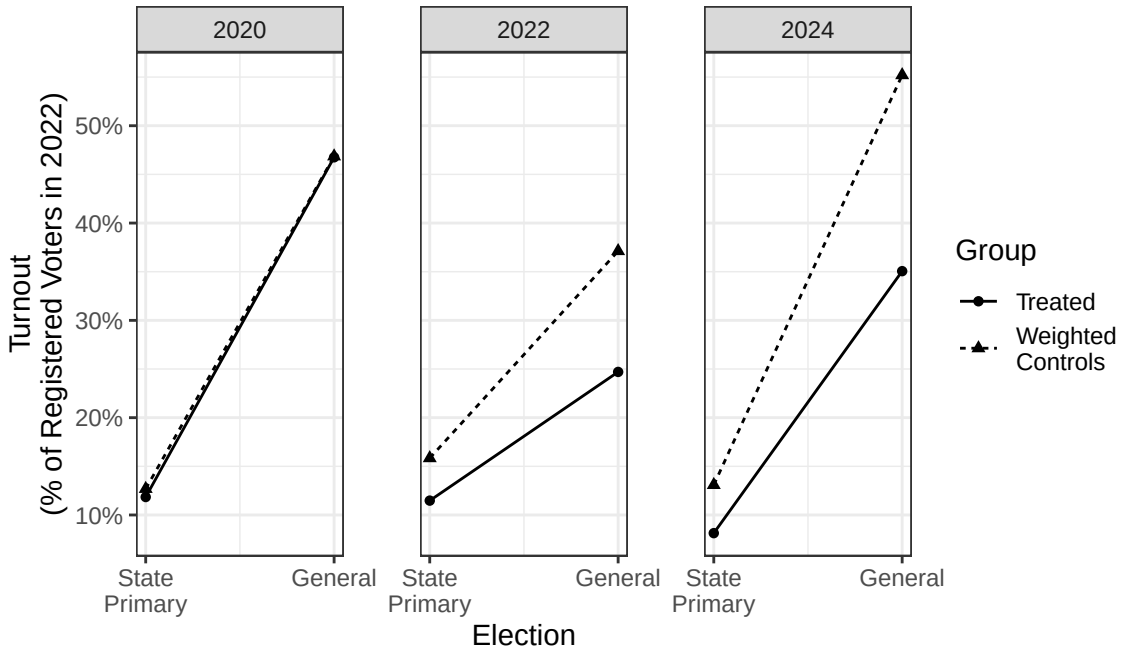
† Distribution different from treated voters (Kolmogorov–Smirnov test, $p < 0.05$).

differences between the two sets of voters.[3] Section A.3 of the SI demonstrates that our main findings do not depend on this specific entropy balancing approach; we find similar estimates if we instead condition the parallel trends assumption on covariates (Callaway and Sant'Anna, 2021).

In Figure 1, we plot the observed turnout rates of Florida voters in both the primary and general elections of 2020–2024. Figure 1 indicates that during 2020, treated and control voters moved roughly in parallel with one another; if anything, general election turnout among formerly incarcerated individuals was slightly higher than demographically similar voters with no felony incarceration history. In other words, prior to the 2022 arrests of Floridians with past convictions, treated voters behaved similarly to their controls. We only

[3]We note that the mean registration date in this table appears surprisingly early (2018) for the treated group, as most of them regained the right to vote with Amendment 4 in 2019. The median registration date for this group is February 4, 2020 (more than a year after the implementation date for Amendment 4); the mean is pulled down by a few very early outliers, who likely had their voting rights restored under the individual clemency process available to small number of formerly incarcerated individuals prior to Amendment 4 (Morse, 2021).

Figure 1: Turnout, Formerly Incarcerated Floridians vs Rest of Electorate



Non−formerly−incarcerated population weighted to mirror formerly−incarcerated population using entropy balancing. Covariates include party, registration date, age, race / ethnicity. They also include block group median income, collegiate attainment, unemployment rate, and population density.

include elections from 2020 because most of the "treated" voters in our sample were ineligible to vote in earlier election years, until the passage of Amendment 4. Considering these two pre-treatment elections in which treated and control voters' turnout is very similar, we think it is plausible that turnout trends in 2022 would have been similar in the absence of voter arrests; we discuss this assumption further in Section 4.1 below.

The middle panel of Figure 1, meanwhile, provides preliminary evidence in support of Hypotheses 1 and 2: relative turnout decreased in the primary election for formerly incarcerated voters (after Governor DeSantis' press conference about the arrests), and then dropped by an even greater amount in the higher-salience general election (after body-camera footage of the arrests was released, and with a less-knowledgeable pool of voters affected). The right-hand panel shows that the gaps in relative turnout that opened up in 2022 after the voter arrests persisted into 2024, consistent with Hypothesis 3.

We econometrically test these relationships in Table 2. In the table, we present the average treatment effect in the first column; that is, how much did the treated voters'

turnout drop, relative to controls, on average in the 2022 and 2024 elections? The coefficient on *Average Treatment Effect (Treated × Post-Arrest)* indicates that, across the four post-arrest elections (the primary and general in each of 2022 and 2024), turnout among formerly incarcerated individuals was about 10 percentage points lower than it would have been if the arrests had not occurred. This is strongly consistent with Hypothesis 1. Of course, as Figure 1 makes clear, the effects were larger in the general elections than in primaries. Column 2 of Table 2 presents "dynamic" treatment effects, or the estimated effect of the arrests on each of the post-treatment elections.[4] These estimates make clear that the effects in general elections (in both 2022 and 2024) are larger than those in primaries, consistent with Hypothesis 2. As we note above, this could occur for multiple reasons; it may be that the effect is smallest in the 2022 primary because the arrests occurred very near the election and many voters may not have learned about them until after voting. But that the effect is also smaller in the 2024 primary points to another possible story, which is simply that primary voters are on average more engaged than general-election-only voters and thus might be more confident about their own voting eligibility.

Finally, we note that the effects observed in Figure 1 extend into the 2024 presidential election. In fact, we see the largest effects of the post-election period in the 2024 general. This makes sense in light of voter characteristics: those who turn out only for presidential elections are among the least-engaged voters, and likely less familiar with eligibility rules and perhaps more prone to being scared off of voting. But the fact that these effects persist into 2024 also underscore the serious and memorable nature of this "treatment": voter arrests that occurred in 2022 appear to still be substantially reducing voter participation over two years later.

We note two features of our dataset that matter for interpretation of these estimates. First, as discussed above, the state makes data available only for people sentenced to state prison, not felony probation. This means that our "control" group likely includes some people

[4] *Treated × General Election* captures the difference in the gap between treated and control voters in the 2020 general relative to the 2020 primary.

Table 2: Turnout Among Registered FL Voters, 2020 – 2024

	(1)	(2)
Average Treatment Effect (Treated × Post-Arrest)	-0.1000***	
	(0.0014)	
Estimated Effect in 2022 Primary		-0.0353***
		(0.0016)
Estimated Effect in 2022 General		-0.1232***
		(0.0023)
Estimated Effect in 2024 Primary		-0.0412***
		(0.0016)
Estimated Effect in 2024 General		-0.2004***
		(0.0024)
Treated × General Election		0.0074**
		(0.0023)
Fixed-effects		
Election	✓	✓
Voter	✓	✓
Fit statistics		
Observations	83,425,146	83,425,146
R^2	0.52890	0.53243

Clustered (Voter) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

who were "treated," since they have a felony conviction we cannot observe. We use detailed felony case data from one county (Hillsborough, where felony probation records are available) to explore the extent of this problem, and find that 2.7% of the (weighted) controls in this county actually have a felony record. This is probably too small to meaningfully depress our estimates, but implies a slight downward bias (assuming that felony probationers were at all dissuaded from voting by the arrests). Second, all our estimates are conditional on voter registration. We structure our analysis this way largely out of necessity: identifying a good denominator of formerly incarcerated people eligible to vote in Florida, when some are not citizens or do not have known addresses, is infeasible. But if there were formerly incarcerated Floridians who would have registered and cast a ballot in the absence of these arrests, we would not capture their lost turnout. See SI Section A.9 for more on this point.

4.1 Robustness Tests and Alternative Specifications

In Section A.2 of the SI, we present several alternative specifications and subsetted analyses. We examine effects by race, finding that the estimated effects among Black Floridians look similar in size to those seen in the full sample (but noting that their overrepresentation in this population still implies racially-disparate effects). We also limit to people who registered to vote prior to the arrests (and thus whose registration was not "downstream" of treatment), and those whose eligibility to vote was restored prior to (and under a different procedure than) Amendment 4. These analyses bolster our confidence in the main estimates, and also shed further light on our theorized mechanisms: Estimated treatment effects are smaller among voters for whom *identification* and *uncertainty* were probably lower.

We also consider two major threats to inference facing this design: possible violations of the parallel trends assumption, and a mechanism that runs through information about ineligibility rather than fear. We briefly discuss each of these concerns here, with additional detail in the SI.

4.1.1 Parallel Trends Violations

Our design assumes parallel trends: our "treated" group of people with previous felonies, and our weighted comparison group of other voters, would have followed the same trends in turnout over time were it not for the voter arrests in summer 2022. If this assumption does not hold (that is, if there are other reasons to expect the two groups to diverge over the period of study), then the estimates shown above would not capture the causal effect of those voter arrests. We have limited data to evaluate trends before 2022. Because most people with felony convictions only regained the right to vote in 2019, we cannot look back at earlier elections to see how turnout moved over time. Still, we can run several additional tests to address specific concerns.

First, Section A.3 in the SI demonstrates that our estimates are not dependent on the entropy-balancing specification we use in the tables above. If we instead condition the parallel trends assumption on available pre-treatment covariates and use the estimator described by Callaway and Sant'Anna (2021), we find similar estimates. Even a simpler specification with no weighting and no covariates yields broadly similar estimates, albeit with some election-to-election variation and more reason to doubt parallel trends (see SI section A.4). We can also rule out national-level shocks to the voting participation of people with felonies between 2020 and 2022 using data from other states. As we show in SI section A.5, in both New York and Pennsylvania (two states for which incarceration records can be obtained via public records request), the turnout of formerly incarcerated individuals relative to a demographically-similar comparison group did not change between 2020 and 2024 (in fact, if anything it increased).

However, we might still worry about state-specific turnout trends for people with felony records. For example, 2020 may be an odd year to use as a baseline for our analyses, simply because it was the first year our "treated" group was allowed to vote in Florida. Perhaps turnout was especially high for this group in 2020 given enthusiasm about regaining the right to vote, and we should expect to see "reversion to the mean" and a drop-off in voting

the following election even in the absence of any particular turnout-depressing event.[5] We take this possibility seriously, but we do not think it is self-evidently true. Though we are not aware of research into the persistence or subsidence of turnout in the wake of voting rights restoration for people with criminal records, several threads of existing literature are informative. First, we turn to Meredith and Morse (2014), who find that state notifications about voting eligibility boosted turnout among formerly-incarcerated people in Iowa. The group receiving notifications turned out to vote at a rate of 19% in 2008, the first election after they received these letters from the state alerting them to their eligibility (Meredith and Morse, 2014)(Figure 2). That increase in turnout was sustained through the 2012 election, four years after those notifications (Meredith and Morse, 2014)(Figure 3). If we think notification of one's eligibility is a bit like seeing a legal change like Amendment 4, then this pattern of persistently higher turnout shown in Meredith and Morse (2014) suggests we should not expect a huge burst of enthusiasm followed by a rapid drop in turnout within the first few years of eligibility (the pattern that would pose a threat to inference in the design used above). And we do not see evidence in other contexts of franchise expansion—such as women's suffrage—that (re)enfranchisement leads to surge-and-decline turnout patterns, though this literature is limited (Wolbrecht and Corder, 2020).

We also note that the entropy balancing approach used in our main specifications above captures certain types of "novelty" and enthusiasm among first-time voters, though not Amendment-4-specific enthusiasm. The inclusion of age and registration date in the entropy balancing procedure constructs a comparison group that, like our newly-enfranchised "treatment" group, contains many newly-registered people, including many older adults who have recently registered. This should, therefore, upweight controls for whom 2020 was a uniquely mobilizing election for reasons unrelated to Amendment 4, guarding against concerns that

[5]As evidence for this point, see that in Figure 1 above, voters with felonies turned out at very similar rates to their (entropy-balanced) counterparts without criminal records in both 2020 elections. This is surprising, as people with previous convictions typically vote at lower rates than demographically-similar people without convictions, as our New York and Pennsylvania analyses demonstrate. Of course, selection into registered-voter status may look different across states.

newly-registered voters might generally vote at high rates in their first election and then drop off in following years. To the extent that such a pattern exists, it should exist equally in our treated and comparison groups.

In Section A.6 of the SI we also present an analysis of turnout among people with misdemeanor records in one large county, a group for whom 2020 should not have been a special year electorally (since misdemeanors have never been disenfranchising in Florida). This group presents a hard test for our theory of arrests scaring other voters with records away from voting, since many people with misdemeanors may know that their convictions are different from those of the arrestees. This being said, we know that there is a widespread lack of information about even misdemeanants' eligibility to vote (Wood and Bloom, 2008; Sugie et al., 2024), indicating that the fear mechanism might well have applied to them. We find no evidence that their turnout in 2020 was higher, relative to controls (consistent with the expectation that they should not have been especially excited about the 2020 election), and we find small negative turnout effects among the group in 2022 and 2024 (consistent with them being scared off, but to a smaller degree than the formerly incarcerated voters).

To be sure, none of these tests can individually prove that the arrests depressed turnout, as opposed to sharply diminishing enthusiasm following the novelty of expanded voting rights in the 2020 election. But taken together, the turnout drop among even those with misdemeanors; the way that our entropy balancing specification captures enthusiasm among other new voters; and, most importantly, our 2025 interviews in which participants recalled the community's fear nearly three years later all strongly corroborate our interpretation of the main estimates: that formerly incarcerated individuals were scared off from voting by voter arrests. Further, in Section A.2.3 of the SI, we present an additional in-state placebo test, focusing on the small number of people with felony records who were registered before 2019. We expect that people who had navigated the more complicated rights-restoration process to get registered in the pre-Amendment-4 era were likely quite well-informed and confident in their own eligibility to vote, so we should not see scare-off effects among this

group. Indeed, we do not find turnout reductions among this group after the voter arrests. We read these placebo estimates as another indication that there are not simply background fluctuations in turnout among people with felony records over this period, but that the effects shown above are driven by a set of particularly-vulnerable voters reacting to voter arrests against a backdrop of pre-existing eligibility uncertainty, and being scared off of voting.

4.1.2 Information About Ineligibility

Another story consistent with the effects shown in the previous section is about ineligibility to vote. In the wake of Amendment 4, SB 7066, and a series of lawsuits, the legal status of individual voters with previous convictions remained murky in both 2020 and beyond. First, voters with certain conviction types (murder or felony sexual offenses) were excluded from rights restoration. And second, people with felony convictions were required to complete the terms of their sentences before voting, including paying any fines or fees they owed. But the state does not maintain a centralized database of outstanding fine/fee debt or payment records, making it challenging for individual voters to confirm their eligibility (Sukhatme, Billy and Bagwe, 2023). Indeed, expert reports filed in a legal case about this matter underscored how difficult it was to determine eligibility, but also estimated high rates of fee-related ineligibility for the voters examined (Smith, 2019; Burch, 2020).

So one possible explanation for the drop-off in voting seen in 2022 could be that news coverage of the arrests helped people learn about the limits of rights restoration. Perhaps the deterrent effect in 2022 was concentrated among voters who learned, via this news coverage, that they were actually not legally eligible to vote and thus stayed home. This mechanism could also involve fear, as described in the previous subsection, but it would not imply that legally-eligible voters were being prevented from voting by these arrests. Can we distinguish these two stories?

We note first that there are two distinct types of ineligibility for people with felony convictions in the state. The more straightforward one is the exclusion of people with

murder or sexual offense convictions from rights restoration. It was this source of ineligibility that affected the voters arrested in 2022: they all had types of convictions that left them ineligible to register or to vote. Could it be that registered voters with the same types of convictions read the news stories, realized they should not have been allowed to register due to their conviction type, and stayed away from the polls in 2022? We do not think this pathway is likely. In Section A.7 in the SI, we merge in offense data to figure out what types of convictions the people in our sample had, and find very few registered voters with potentially-disfranchising charge types. There are not enough of these voters to explain our main estimates.

The second possibility is slightly more complicated: that seeing the arrests led other voters to investigate their own eligibility to vote more carefully, and to learn that they were ineligible to vote because they still owed fines or fees.[6] Our main analyses cannot rule out this possibility, because we do not have information on fine/fee debt for the people in our main sample (see Smith (2019) for a detailed discussion of the lack of state fine/fee data). It is possible that some people in our sample have outstanding fine/fee debt and are technically barred from voting in the state.[7] But we note that the issue of ineligibility due to fines and fees was known before the 2020 election and was the subject of intense media coverage prior to the 2020 election due to ongoing litigation over the matter, making it less likely that the 2022 arrests were a new source of information about this particular source of ineligibility. Re-entry and civic organizations doing registration work in 2020 were aware of the restrictions on eligibility posed by fines and fees, so they would likely have helped anyone who registered just prior to the 2020 election understand and check their eligibility at that time.

[6]We note that this pathway is more complicated because the arrests were not about fines and fees, so voters would likely have to do a little extra research to learn about this additional source of ineligibility.

[7]This would not imply they were actually ineligible to vote at the time they registered; people could have registered after the passage of Amendment 4 but before SB7066 stipulated the payment of fines/fees. Although SB 7066 included a "safe harbor" provision protecting those who registered to vote during this period from prosecution for *registering* if they owed fees and fines, they were not allowed to vote.

5 Conclusion

Many debates about election policy in the modern U.S. center on policies that impose specific, known burdens on many voters, such as requiring identification or limiting voting hours or locations. We focus instead on a practice that targeted a very small number of voters with extremely high costs (arrest and prosecution) and ask whether this kind of practice can scare off other voters from participating. We find that in Florida's environment of widespread uncertainty about eligibility and intense media coverage of the arrests of these voters, Floridians with past convictions stayed home from the polls at higher rates than would otherwise be expected. Interviews conducted with volunteers and voters on the ground, as well as supplementary analyses of other datasets, support the idea that the turnout reductions we see are driven by fear and avoidance among voters with convictions, not solely by data quirks or other background trends. That these effects were apparently quite durable, lasting at least through the 2024 presidential election, is troubling but consistent with a growing body of literature indicating that other policies can depress turnout beyond the election in which they formally operate (Grimmer and Yoder, 2022; Miller et al., 2025). And we believe the patterns we observe could happen in other states given the right circumstances: though Florida's felony disenfranchisement regime is particularly confusing, it is not wholly unique. Many states have complicated laws about eligibility, and there is evidence that both people with felony records, and state and local election officials, struggle to parse their requirements (Wood and Bloom, 2008; Drucker and Barreras, 2005)

These findings bring together the U.S.-focused literature on costs of voting with work on repression in more authoritarian contexts. We highlight a class of state practices that may not even register as "election policy" in the typical sense, but can nevertheless affect voters' calculations profoundly and persistently. This framework is timely given recent debates—academic and public—about the presence or feasibility of "vote suppression" in the modern U.S. Grimmer and Hersh (2024), in their discussion of "why [...] election laws bear such a

modest relationship to who wins and who loses," highlight the importance of considering both the size of a policy's effect on turnout as well as the size of the affected population (and, if one is interested in partisan outcomes, the partisan distinctiveness of that group). Having observed a strikingly large decrease in participation among a group of voters that is not numerically large, we now turn to considering where else this kind of fear-based voter deterrence could occur.

In section 2.2, we described three conditions under which which we expect small-scale voter targeting to lead to larger-scale deterrence: visibility of the events, a broader population that identifies with those targeted, and uncertainty among that population about their own risk exposure. The present study was not designed to specifically test the importance of all these conditions, and one case could not suitably provide such a test. Still, we point to some preliminary analyses consistent with our theoretical framework, and hope that the limitations of those tests will inspire future work. First, we note that our case does not provide any variation in the size of the population identifying with those arrested, so we do not probe the importance of identifiability here. On the importance of visibility, we observe that, consistent with H2 above, we see larger deterrent effects in the 2022 general election (after body camera footage of the arrests was widely disseminated) than in the 2022 primary (when the news had had less time to spread). But as noted above, this pattern is quite a limited look at visibility, since there are other reasons we could expect to see different effects in the primary than the general; for one thing, primary voters may be more knowledgeable and thus face less uncertainty about their eligibility to vote.

Finally, we consider the importance of uncertainty about whether one could be targeted next. Our case contains a particularly potent form of uncertainty: relatively inexperienced voters who had registered after the passage of Amendment 4 faced widespread confusion about whether they were individually eligible to vote, a confusion that was shared by local election officials and activists. We think this profound uncertainty was an important component of the uncommonly large deterrent effects we have documented in this paper.

Consistent with that interpretation, SI Section A.2.3 shows that a set of voters who had navigated the rights-restoration process years earlier—who could be expected to be highly knowledgeable and certain about their voting eligibility—do not show any dropoff in voting. And SI Section A.6 shows that voters with misdemeanor records, who likely comprise a mix of people who accurately perceive their own eligibility and those who feel some uncertainty about it, fall in between these two cases: some are deterred from voting, but the effect is smaller than the effects shown above for those with felony records. Again, these groups may differ in additional ways beyond their exposure to uncertainty, so we encourage further tests of these specific theoretical mechanisms.

One timely case with all these features—visibility, a large population to identify with those arrested, and uncertainty about exposure to future repression—involves widely-publicized threats to pursue voter fraud cases against non-citizens (Thrush et al., 2026). If the federal government were to arrest and prosecute people for voting as non-citizens, or pursue denaturalization efforts targeting naturalized citizens based on claims they voted while ineligible, such high-profile actions could potentially scare off eligible voters.[8] As researchers have noted, many purported cases of non-citizens voting appear to arise from outdated records that do not reflect naturalization (*Update: Review of Claims of Noncitizen Registrants and Voters*, 2026). As such, it is possible that naturalized citizens who have the legal right to vote could nevertheless see themselves as potential targets of non-citizen-voting prosecutions.[9] With over 24 million naturalized citizens in the U.S. (one in ten eligible voters), such deterrent effects could affect overall turnout (Schaeffer, 2024). We encourage future research into this case and any other election interventions that similarly involve targeted, punitive treatment of a small number of people within view of a larger group of people who may be frightened into quiescence.

[8]For journalistic reports of citizens being scared off by the prosecution of other community members for voting without citizenship, see Burness (2026).

[9]State actions could reinforce or foment such uncertainty. In Tennessee in 2024, for example, the state elections office sent letters to more than 14,000 registered voters whose records had been flagged for "potential" non-citizenship, warning them that voting while ineligible was a crime that could lead to prison time (Kruesi and Mattise, 2024).

We end by noting that our approach to studying the effects of the Florida election police in a complex research environment offers a method of "triangulation" that can aid other research projects. In the case at hand, we have limited data to assess the parallel trends assumption, since most of our study population was disenfranchised until the election immediately preceding treatment. This issue is mechanically related to the potential "enthusiasm" gap of being newly enfranchised for the 2020 election that we discuss at length. Nevertheless, thinking clearly about how competing explanations would show up among subsets of the study population, identifying alternative states and other in-state samples to use for placebo tests, and incorporating extended interviews allow us to buttress our conclusions. Many normatively and theoretically important research questions pose similar challenges for applying causal inference approaches. Indeed, the sort of phenomenon we study here (visible and costly state targeting of voters from already-marginalized groups) seems especially likely to affect groups without long histories of enfranchisement and participation. These challenges notwithstanding, such questions demand study. By combining causal inference tools with qualitative methods (such as our extended interviews) and a variety of falsification tests, researchers can still make rigorous empirical inferences.

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Supplementary Materials

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A.1 Incarceration/Voting Record Match Rates

To determine the rate of false positives in our match to the voter file, we follow the guidance of Meredith and Morse (2014) by adding or subtracting 35 days from the prison release records, and re-matching these with the voter file data. No records "should" match, as every release record has been made incorrect. Thus, those that do match are akin to false positives. Table A1 makes clear that there are vastly more matches using the real birth dates, implying a low level of false positive matches (somewhere between 0.45% and 0.5%).

Table A1: Match Rates with Real and Permuted Birth Dates in Prison Release Records

	Real Birth Date	+35 Days	-35 Days
Matches	47,916	250	213

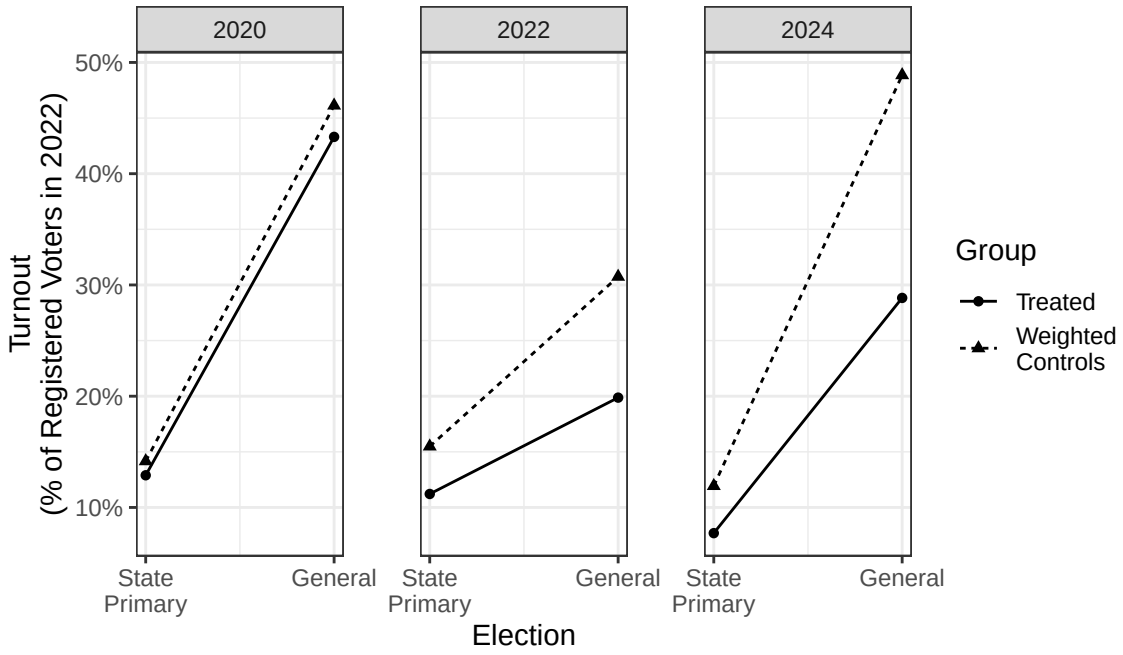
A.2 Sub-Population Analyses

A.2.1 Limiting Analysis to Black Voters

Black voters are considerably over-represented among those impacted by Amendment 4. As shown in Table 1 in the body of the manuscript, they make up half of registered Floridians who were formerly incarcerated, while making up less than 15% of the rest of the registered population. Black Floridians are thus at unique risk of being deterred from voting because of their over-representation in our "treated" group. Here, however, we ask whether voter arrests had unique effects by race *within* the formerly incarcerated population. To do so, we limit the study population to Black registered voters in Florida. We re-balance the control voters using entropy balancing (here dropping race), and re-estimate the econometric models. As Figure A1 shows, turnout in 2020 was very similar among Black formerly incarcerated voters and their controls. A large gap, however, opens up in 2022 — just as we observed in the main analysis in the paper.

Table A2 presents the same specifications as Table 2 of the main paper. The effects here

Figure A1: Turnout Among Black Voters, 2020 – 2024



Non−formerly−incarcerated population weighted to mirror formerly−incarcerated population using entropy balancing. Covariates include party, registration date, age. They also include block group median income, collegiate attainment, unemployment rate, and population density.

are slightly smaller than those presented in the main analysis for the full population, but not dramatically so. Because the entropy balancing procedure is re-run for the sub-population, we cannot directly test whether these are statistically significantly different from the rest of the population. Further, while these point estimates indicate that the effects might have been smaller for Black voters, we stress anew that the extreme over-representation of Black Floridians in the study population as a whole means that the chilling effects shown in the main paper disproportionately affect Black participation.

A.2.2 Limiting Analysis to Pre-Arrest Registrants

In the main paper, we include all voters who were registered as of the 2022 general election. This means that some of our voters registered to vote "downstream" of treatment: The arrests may have caused a dip or an increase in registration, thus shifting our study population (see Nyhan, Skovron and Titiunik, 2017). Here, we limit the pool of registered voters to those who were already registered at the time of the arrests and, thus, for whom

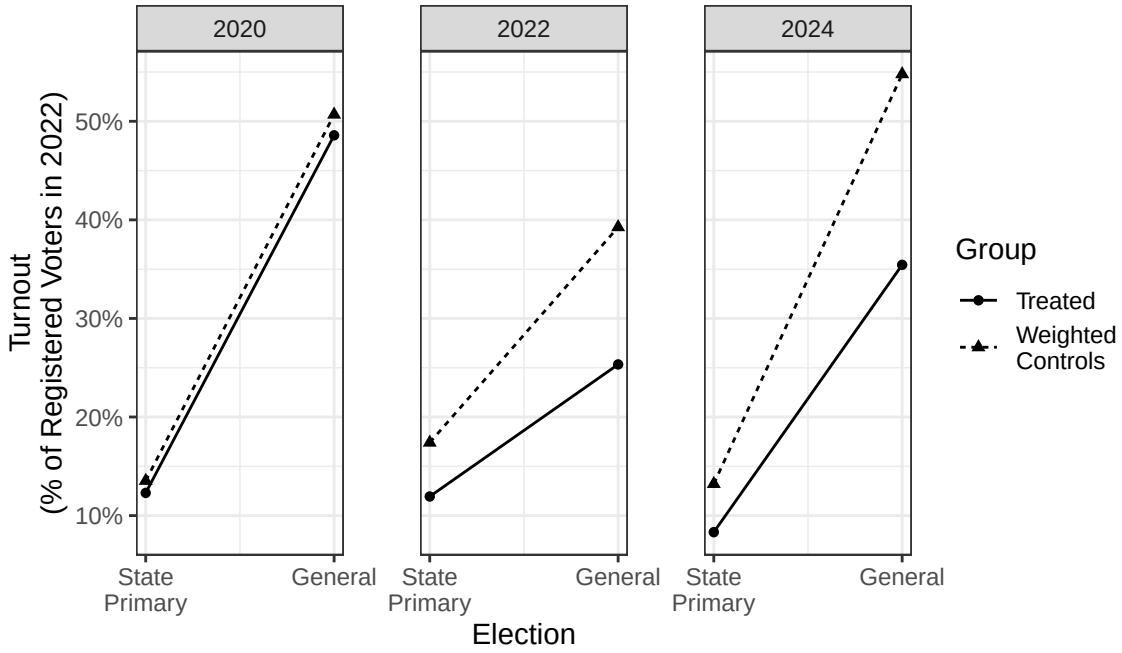
Table A2: Turnout Among Black Registered FL Voters, 2020 – 2024

	(1)	(2)
Average Treatment Effect (Treated × Post-Arrest)	-0.0781***	
	(0.0020)	
Estimated Effect in 2022 Primary		-0.0300***
		(0.0024)
Estimated Effect in 2022 General		-0.0805***
		(0.0034)
Estimated Effect in 2024 Primary		-0.0297***
		(0.0024)
Estimated Effect in 2024 General		-0.1721***
		(0.0034)
Treated × General Election		-0.0155***
		(0.0033)
Fixed-effects		
Election	✓	✓
Voter	✓	✓
Fit statistics		
Observations	11,184,066	11,184,066
R^2	0.53692	0.54074

Clustered (Voter) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

these arrests could not have shifted registration behavior. As Figure A2 and Table A3 make clear, the effects of the arrests on turnout among this population are very much in line with those of the full population.

Figure A2: Turnout Among Floridians Registered Before Arrests Occurred, 2020 – 2024



Non−formerly−incarcerated population weighted to mirror formerly−incarcerated population using entropy balancing. Covariates include party, registration date, age. They also include block group median income, collegiate attainment, unemployment rate, and population density.

A.2.3 Limiting Analysis to Non-Amendment 4 Voters

As discussed in the body of the manuscript, the overwhelming majority of formerly incarcerated Floridians were banned from voting until Amendment 4 took effect in January of 2019. These individuals had their voting rights restored upon completion of their sentence — a status that became extremely difficult to ascertain following the passage of Senate Bill 7066 that re-defined completion to include paying off all fines and fees that were part of the original sentence. These individuals, we argue, were likely unsure about their eligibility to vote; the high-profile arrests in the late summer and early fall of 2022, against the backdrop of that uncertainty, likely led them to withdraw from participating.

There is another group, however, who faced less uncertainty: the relatively few individu-

Table A3: Turnout Among FL Voters Registered Before Arrests, 2020 – 2024

	(1)	(2)
Average Treatment Effect (Treated × Post-Arrest)	-0.0924***	
	(0.0014)	
Estimated Effect in 2022 Primary		-0.0424***
		(0.0017)
Estimated Effect in 2022 General		-0.1182***
		(0.0024)
Estimated Effect in 2024 Primary		-0.0367***
		(0.0017)
Estimated Effect in 2024 General		-0.1724***
		(0.0024)
Treated × General Election		-0.0088***
		(0.0024)
Fixed-effects		
Election	✓	✓
Voter	✓	✓
Fit statistics		
Observations	81,283,836	81,283,836
R^2	0.53420	0.53726

Clustered (Voter) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

als who had had their voting rights restored under the prior system. Before 2019, individuals could apply for an individual pardon from the state's clemency board. Some people also regained the right to vote via clemency processes pursued by former governor Charlie Crist (Morse, 2021). We expect that people who regained the right to vote and registered prior to 2019 should have been less affected by any enthusiasm bump in 2020 (because 2020 was not their first election following re-enfranchisement) than those re-enfranchised by Amendment 4, and also less affected by the uncertainties in 2022. These voters were likely to be far less uncertain about their eligibility to vote: many of them had been individually informed that their voting rights had been restored during the pardon process and they had then proactively registered to vote. Further, because their eligibility did not depend on Amendment 4, they were unaffected by SB7066's ambiguities about fines and fees. These are also likely to be highly-committed and informed voters, given that they had made their way through the much more complicated pre-Amendment 4 process to nevertheless get registered to vote. In short, we expect to see A) no evidence of an increase in relative turnout among this population in 2020, and B) no evidence of decreased turnout in 2022 or 2024.

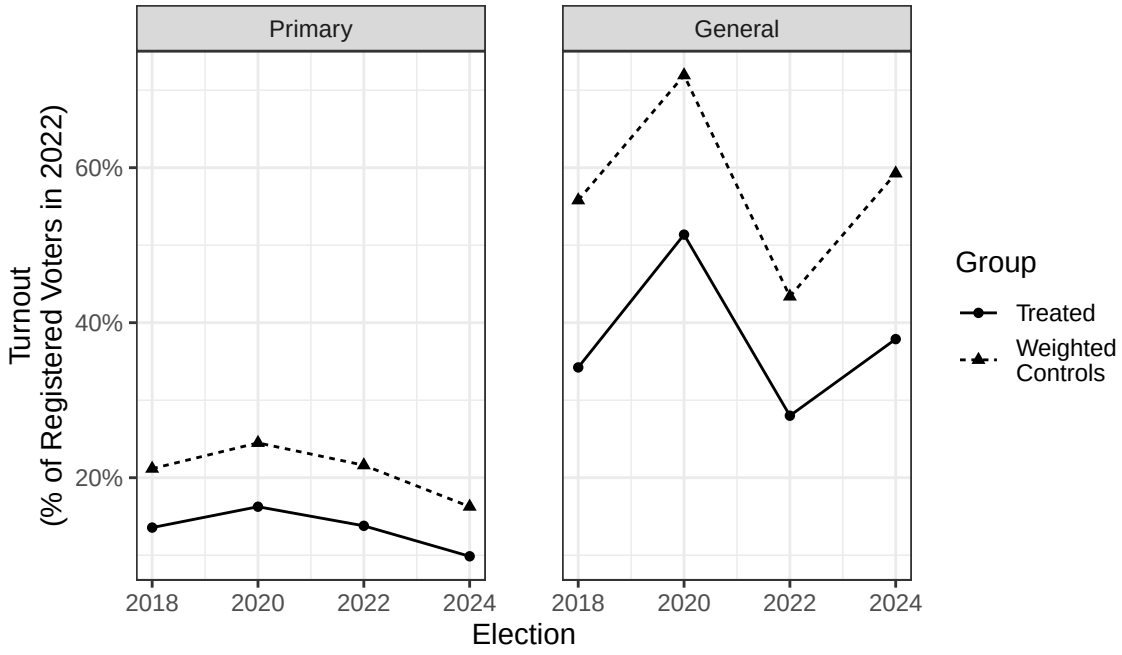
To test whether this was indeed the case, we here limit the analysis to Floridians who had registered to vote prior to the enactment date of Amendment 4 (January 8, 2019).[10] We also extend the base period data back to 2018, since these voters were not newly enfranchised in the run-up to the 2020 election. We re-run the entropy balancing procedure and plot the results, expecting any turnout effects to be muted among this population. Figures A3 and A4 are consistent with these expectations. The gap between the formerly incarcerated individuals whose confidence in their eligibility to vote should not have changed in the fall of 2022, and their weighted controls, did not shift materially during the study period; if anything, their relative turnout may have increased slightly in 2022.

Finally, we note that 2020 turnout among this subset of formerly incarcerated individuals (again, for whom enthusiasm should have been less pronounced) was just above 50% in 2020

[10]Just 18% of the formerly incarcerated individuals who were registered as of the 2022 midterms registered prior to this date.

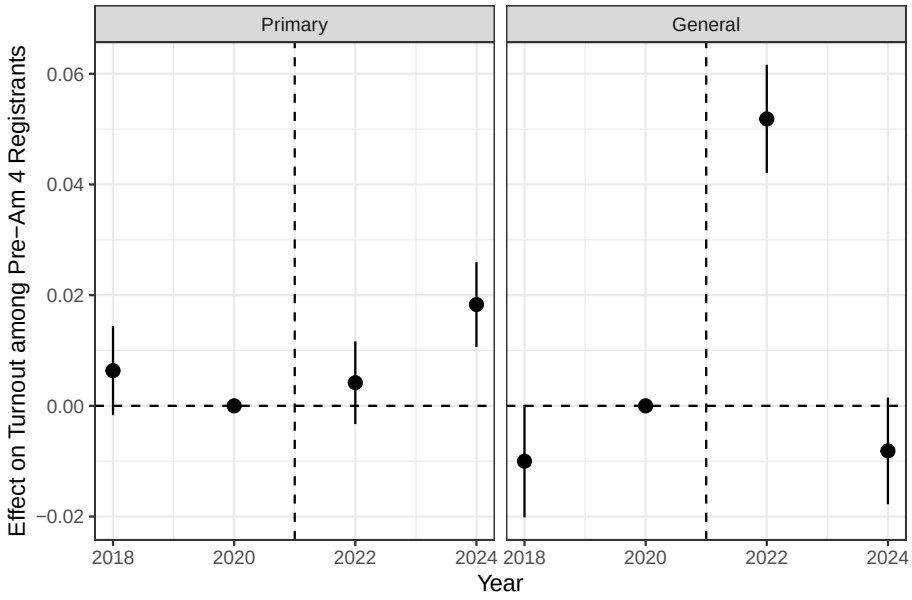
among registered voters. This is actually slightly *higher* than the turnout rate among all formerly-incarcerated registrants that year (roughly 47.5%; see Figure 1 in the main paper). We take this as possible evidence that disproportionate levels of enthusiasm were not inflating turnout among the full population far above baseline in 2020, though we acknowledge the other differences between this group and other voters with felonies described above.

Figure A3: Turnout Among Non-Amendment 4 Voters, 2018 – 2024



Non−formerly−incarcerated population weighted to mirror formerly−incarcerated population using entropy balancing. Covariates include party, registration date, age, race / ethnicity. They also include block group median income, collegiate attainment, unemployment rate, and population density.

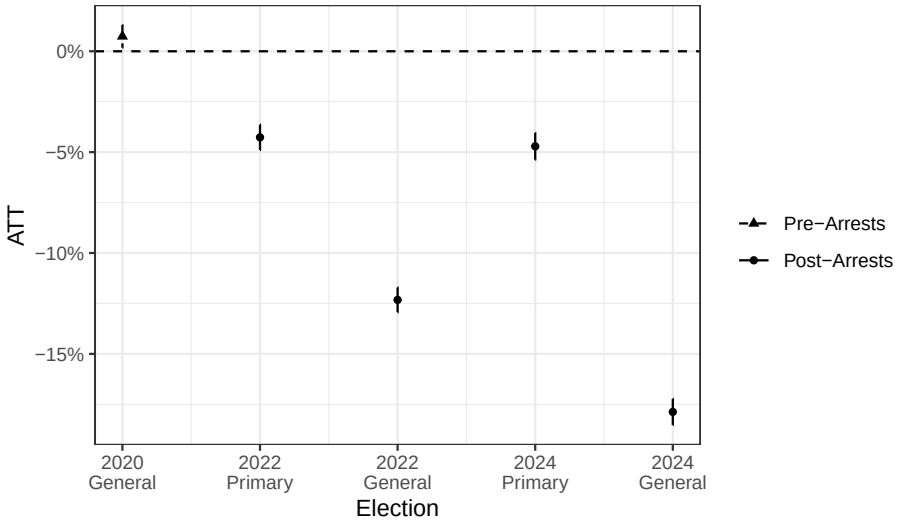
Figure A4: Relative Changes in Turnout Among Non-Amendment 4 Voters, 2018 – 2024



A.3 Using Callaway and Sant'Anna (2021) Estimator

In the body of the manuscript, we rely on entropy balancing to ensure that our control group is broadly similar to our treated group. Here, we use a different approach to ensuring the plausibility of the parallel trends assumption. Rather than weight our potential controls to ensure similarity, we weight all control units equally but condition the parallel trends assumption on the same characteristics used in the entropy balancing procedure. We use the Callaway and Sant'Anna (2021) estimator, which allows us to correctly condition the parallel trends assumption. We use two-way fixed effects for voter and elections. Figure A5 presents the point estimates from these models. The results here are very similar to those shown in Table 2 in the body of the manuscript. The conclusions we draw from the two-way fixed effects models presented in the main paper do not depend on our decision to use entropy balancing; instead, they are robust to a very different estimation approach.

Figure A5: Estimated Treatment Effect using Callaway and Sant'Anna (2021) Estimator

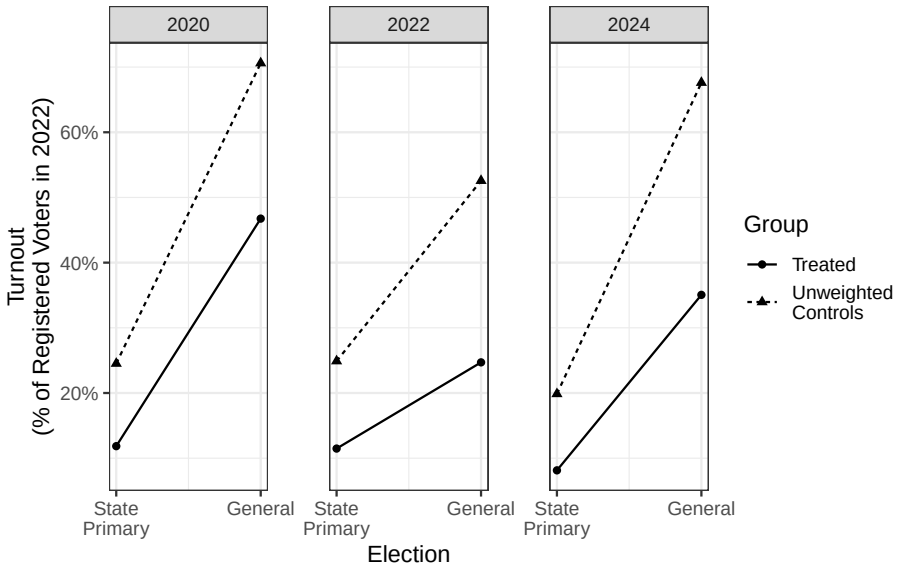


Parallel trends assumption conditional on voter (party, registration date, age, race) and neighborhood (median income, share with a bachelor's degree, unemployment rate, population density) characteristics using Callaway and Sant'Anna (2021) estimator.

A.4 Unweighted Estimates

Figure A6 and Table A4 present unweighted estimates.

Figure A6: Turnout Among All Voters, 2020 – 2024



All voters weighted equally.

Table A4: Turnout Among Registered FL Voters (Unweighted), 2020 – 2024

	(1)	(2)
Average Treatment Effect (Treated × Post-Arrest)	-0.0313***	
	(0.0013)	
Estimated Effect in 2022 Primary		-0.0075***
		(0.0016)
Estimated Effect in 2022 General		-0.0402***
		(0.0023)
Estimated Effect in 2024 Primary		0.0094***
		(0.0016)
Estimated Effect in 2024 General		-0.0870***
		(0.0023)
Treated × General Election		-0.1118***
		(0.0023)
Fixed-effects		
Election	✓	✓
Voter	✓	✓
Fit statistics		
Observations	83,425,146	83,425,146
R^2	0.58875	0.58883

Clustered (Voter) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

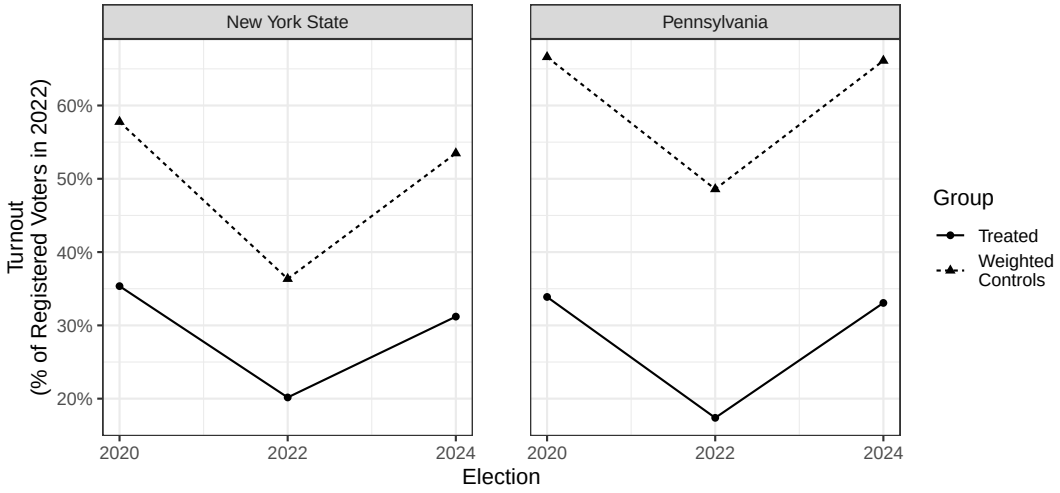
A.5 Placebo States: New York and Pennsylvania

In this section, we run our main analysis on two other states: New York and Pennsylvania. Incarceration records for individuals in New York State and Pennsylvania were obtained through public records requests.[11] These states were selected both because of the availability of these records, and because neither of these states meaningfully changed rules about voting eligibility for formerly incarcerated citizens between the 2020 and 2022 elections. We then apply the same entropy balancing specification as in our main Florida analysis.

Figure A7 and Table A5 show that, broadly speaking, the turnout of formerly incarcerated individuals in New York State and Pennsylvania moved in parallel with that of the rest of the (entropy balanced) electorate between 2020 and 2024 (for visual clarity, we do not show the primary elections in the figure though they are included in the econometric estimates in the regression table). If anything, the gap between the two groups *shrank* from 2020 to 2022—turnout among formerly incarcerated decreased by less, not more, than that of non-formerly-incarcerated voters in both New York and Pennsylvania. We read these directionally null estimates as an indication that there were not national-level forces pushing down the turnout of people with past convictions.

[11] In New York, we observe 294,770 people released from state custody over the last few decades. in Pennsylvania, we observe 500,848 people convicted of felonies in state court in recent decades.

Figure A7: Turnout Among Formerly Incarcerated Individuals in NY and PA and Their Controls, General Elections 2020 – 2024



Non−formerly−incarcerated population weighted to mirror formerly−incarcerated population using entropy balancing. Covariates include party, registration date, age, and predicted race / ethnicity. They also include block group median income, collegiate attainment, unemployment rate, and population density.

Table A5: Turnout Among Registered NY and PA Voters, 2020 – 2024

	New York		Pennsylvania	
	(1)	(2)	(3)	(4)
Average Treatment Effect (Treated × Post-Arrest)	0.0383***		0.0254***	
	(0.0011)		(0.0015)	
Estimated Effect in 2022 Primary		0.0406***		0.0278***
		(0.0012)		(0.0016)
Estimated Effect in 2022 General		0.0624***		0.0153***
		(0.0021)		(0.0027)
Estimated Effect in 2024 Primary		0.0489***		0.0617***
		(0.0012)		(0.0016)
Estimated Effect in 2024 General		0.0013		-0.0031
		(0.0022)		(0.0030)
Treated × General Election		-0.1597***		-0.1622***
		(0.0021)		(0.0027)
Fixed-effects				
Election Period	✓	✓	✓	✓
Voter	✓	✓	✓	✓
Fit statistics				
Observations	68,319,816	68,319,816	47,089,848	47,089,848
R^2	0.52111	0.53182	0.56384	0.57502

Clustered (Voter) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

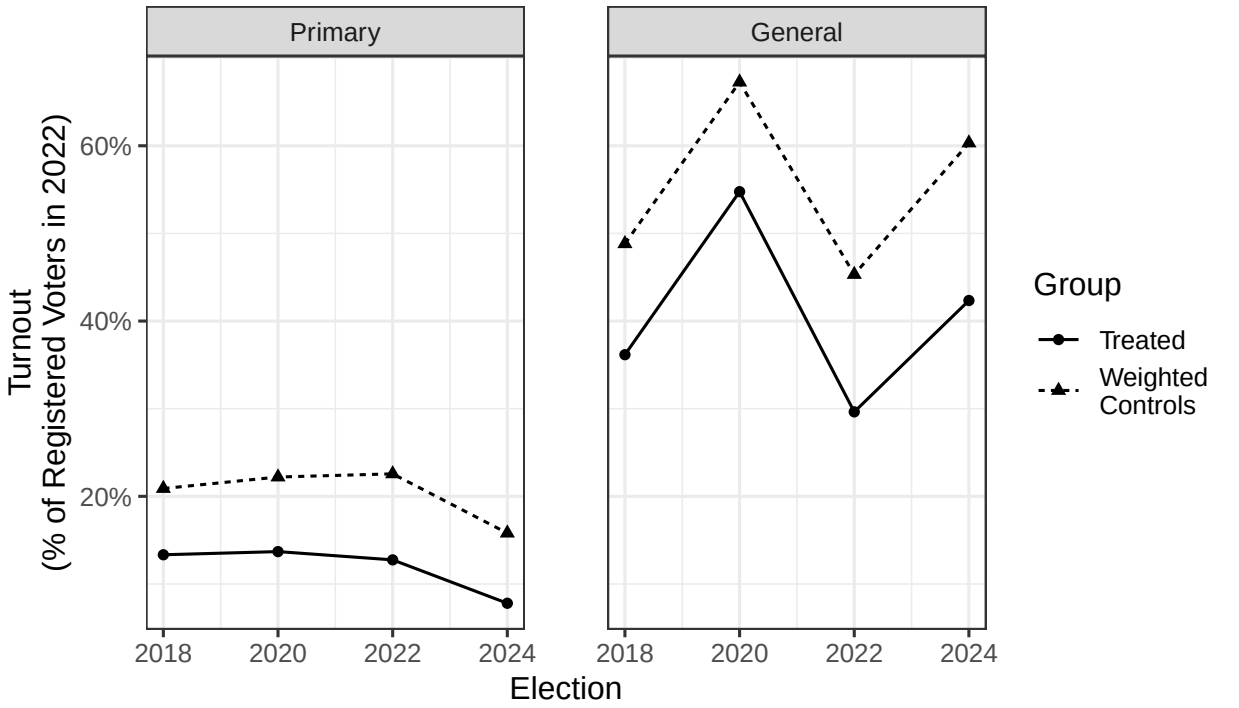
A.6 Hillsborough County Misdemeanor Records

As noted in the main paper, we have limited pre-trend data to assess the plausibility of the parallel trends assumption for our main sample, since most people with felony records were not eligible to vote before 2020. So we turn next to a different sample whose eligibility has not changed: people with misdemeanor records, who have always been eligible to vote. For this group, we do not expect that enthusiasm should have been driving up turnout in 2020, since nothing changed about their eligibility in that year and many people were registered and voting well before 2020. However, we expect that misdemeanants might have been scared off by voter arrests for two reasons. People with criminal records and election officials alike are often unsure about the eligibility of misdemeanants to vote (Wood and Bloom, 2008; Sugie et al., 2024); this uncertainty might have led them to withdraw from participating in 2022 and 2024. And some misdemeanants have been convicted of felony offenses and sentenced to probation; they are not in our main dataset, but resemble our study population.

For this analysis, we rely on people with misdemeanor convictions from Hillsborough County, which posts court records from the last few decades on its website. We download and clean all available misdemeanor and felony court records to produce a dataset of people who have a misdemeanor conviction, but no felony convictions, in Hillsborough county, and who also do not appear in the OBIS database we use for our main analysis.[12] We then merge these misdemeanants with the Florida voter file (as above) to find about 10,000 registered voters with misdemeanors but no evidence of felony convictions. We then reproduce our main analyses for this group, using entropy weighting to find a comparison set of voters and then running a two-way fixed effects design to examine relative changes in their turnout after the 2022 voter arrests. We can also include 2018, since these voters' eligibility to vote did not change between the 2018 and 2020 elections.

[12]We note these individuals might have been convicted and sentenced to felony probation in other Florida counties. But as shown in Figure A8, we don't see a 2020 boost in turnout, so we still think this group helps us rule out the "Amendment 4 enthusiasm bump" story about parallel trends violations.

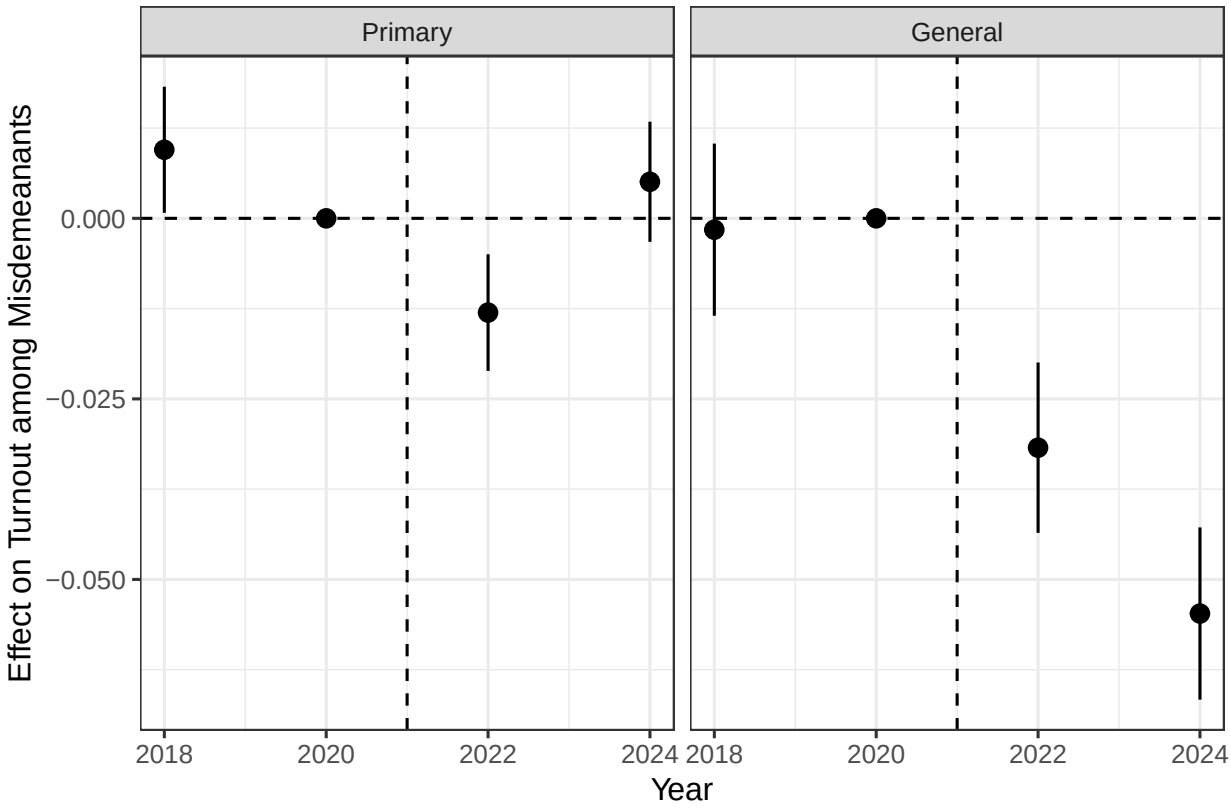
Figure A8: Voter turnout before/after voter arrests for a set of voters with misdemeanor convictions in Hillsborough County ("Treated") and a comparable group of other voters ("Weighted Controls")



Non−misdemeanant population weighted to mirror misdemeanant population using entropy balancing. Covariates include party, registration date, age, race / ethnicity. They also include block group median income, collegiate attainment, unemployment rate, and population density.

Figure A8 plots turnout over time for this group of voters with misdemeanors (the "Treated" line) and the weighted group of comparable voters without convictions. Again, turnout between treated and comparison voters moves similarly over the pre-period, but then a gap opens up in 2022 after the arrests, particularly in the 2022 general election. This is what we expected: No "enthusiasm" effect in 2020, but a decline in 2022 and 2024. We interpret this pattern as bolstering our interpretation of the main paper estimates. This group that did not show an enthusiasm bump in 2020 (as could pose a parallel-trends problem for the main sample) nevertheless appears to have been scared off of voting in 2022 and in the 2024 general election (see Figure A8), though to a lesser extent as we'd expect for a group with clearer eligibility.

Figure A9: Effects on voter turnout before/after voter arrests for a set of voters with misdemeanor convictions in Hillsborough County ("Treated") and a comparable group of other voters ("Weighted Controls")



A.7 Eligibility: OBIS data on charge type

In Section 4.1.2 of the manuscript, we discuss the possibility that people saw news coverage of the arrests and learned from it about their own ineligibility to vote, specifically because they had been convicted of a type of offense excluded from Amendment 4's re-enfranchisement. In this section, we consider whether that story (registrants learning that they were legally ineligible to vote and thus staying home in 2022) can explain our main results.

To see whether any of the voters in our main sample may have been convicted of potentially-disenfranchising offenses, we use additional information from the state's OBIS database. The "OFFENSE" tables in the database give brief text descriptions of the most recent sentenced offense for each person in the database, and we use these to identify anyone who might have been convicted of murder or felony sexual offenses. This is an imperfect

approach in two ways: first, we are overinclusive in our identification of relevant charges, since we run simple searches for relevant text strings and thus likely include some crimes that are not "felony sexual offenses" for the purposes of Amendment 4. And second, we may not capture everyone convicted of these offenses; if someone convicted of murder has also served a more recent stint in prison for a less serious crime, we would observe the more recent offense and not the earlier disenfranchising one. But we think this approach gives us an approximate sense of whether this form of disenfranchisement is widespread among registered voters.[13]

We identify potentially-relevant offense types by searching for strings such as "RAPE" and "MOLEST V" and "MUR" in the offense data. We then manually inspect all other offense types shown in the data and flag any that appear they could plausibly represent a sexual offense; this step catches potentially-relevant charge types that do not contain the keywords above (for example: "DIST MAT OF MINOR/HARMFL ACT", "MAN 21+ IMPREG.GRL LT 16"). We do not conduct legal research to verify whether these offense types are felony sexual offenses under Florida law; instead, we include anything that looks like it could plausibly be a felony sex offense, providing an over-inclusive estimate of the number of people affected by this form of ineligibility.

We then count up the number of registered voters who have any of the types of charges we identified via this process. We note, again, that some of these offenses may not actually be disenfranchising, and, further, that being on the voter file as a person who has been convicted of a disenfranchising offense does not imply that someone broke voting laws (for example, they could instead have registered prior to the implementing legislation, or they could have been registered prior to their conviction and election officials may have failed to purge their registration, or they may have applied for and received clemency that allowed

[13]We also note that nothing in this section should be read to imply that any specific voter flagged has broken any laws. People convicted of these specific types of crimes could potentially have registered before their conviction and not been purged from the rolls by local officials, or they might have regained their voting eligibility via the clemency process (either individual applications or bulk restorations conducted during Governor Crist's administration). We aim here to roughly measure how widespread such ineligibility could be, not to determine definite eligibility for any individual person.

them to register and vote). Even with our inclusive approach to identifying offenses that could potentially be disqualifying, we only identify 1248 of the registered voters in our dataset who appear to have any of these kinds of charges. Even if everyone in this group was genuinely disenfranchised, and they all moved from voting in 2020 to not voting in 2022 because they learned about their own ineligibility after seeing the voter arrests, this could still only account for a fraction of the overall effects we show on participation in the main paper. Numerically, this pathway cannot explain our main estimates.

A.8 Interview Process

We conducted qualitative interviews to better understand how voters with felony records (and the organizations serving them) perceived and responded to the 2022 arrests we study. In summer 2025, we used convenience and snowball sampling to recruit ten respondents who could provide insight into the experiences of formerly incarcerated citizens or into the organizations that support their reentry. The snowball sampling process began with outreach via e-mail to individuals in our existing network, particularly those connected to civic or re-entry organizations. These initial contacts then referred additional participants with relevant experiences. Six of ten respondents fell into both categories: they personally had a felony conviction and also worked or volunteered with an organization focused on re-entry or civic participation for people with felony records. Of the remaining four, two were returning citizens and the other two were members of reentry organizations. All respondents are eligible to vote in the state of Florida and represented seven different counties. Table A6 describes some of their personal characteristics.

We used a semi-structured interview format to guide conversations, ensuring consistency across participants while allowing room for respondents to expand on topics or introduce ideas beyond the prepared questions. All interviews were conducted via Zoom, lasted about an hour in duration, and were recorded and transcribed for analysis. All respondents were compensated for their time and participation.

Table A6: Interview Respondent Profile

Demographic	*N*
Race	
White	4
Black	5
Mixed	1
Gender	
Man	6
Woman	3
Nonbinary	1
Voting Eligibility	
Eligible to Vote	10
Incarcerated History	
Formerly Incarcerated	8
No Incarceration History	2
Counties Represented	
Bay	1
Brevard	1
Broward	1
Duval	2
Lee	1
Miami-Dade	2
Palm Beach	2
Total	10

We employed a deductive coding process conducted manually across all ten interview transcripts by either a PI or research assistant. We first developed a codebook with a set of predefined codes related to voter intimidation and civic engagement. The initial codes reflected core focus areas such as fear and anxiety around voting, distrust, confusion, and hesitance or abstention. One PI and a research assistant carefully read through each transcript, highlighting relevant segments of text that aligned with these predetermined codes, marking them with shorthand labels. Additionally, we identified emergent themes that arose organically from participants' responses, including discussions of deservingness and political efficacy. Once all transcripts were coded, we organized the coded data into overarching themes that reflected patterns and variations across responses. For example, codes reflecting fear and distrust were grouped under the theme: affective and attitudinal responses. The remaining overarching themes include chilling effects, civic identity, and systemic disenfranchisement.

Table A7 reports how many of the ten interviews contained each subtheme, along with representative quotations for each. We coded subthemes as present when respondents drew on them substantively in response to our questions about the arrests and their effects, regardless of frequency. Quotations reproduced in the findings section are chosen because they reflect modal expressions of a given subtheme.

Table A7: Qualitative Coding Summary

Theme	Subtheme	Definition	*N*	Representative Quote
Affective and Attitudinal Responses	Fear	Fear/anxiety about voting or arrest, respondent's own or observed in others	9/10	"What I saw was a fear based reaction. So, I saw a lot of people afraid to actually... move forward with their rights as a United States citizen."
	Distrust	Skepticism of institutions/officials/process	10/10	"It just shakes your trust, you know... questions your trust in our government."
Chilling Effects	Discouragement via Confusion	Uncertainty about eligibility that discourages participation	8/10	"It was something we were worried about early on when they introduced the implementation legislation... just the chilling effect that it would create right away. And to see it come to fruition in such a way that people are actually being arrested when even attorneys couldn't figure out or apply the laws... The expectation that anybody could understand and keep up with all the changes, and then apply them, and then to put that on the individual citizens was just absurd."
	Risk Management	Described deliberate process of weighing risks against desire to vote (individual or organizational level)	8/10	"[I] tried to be calculated and strategic, considering facts and how people were being arrested and intimidated. It's better for me not to cast a vote until a court or the Supreme Court made a final decision. When legislators say one thing and courts say something else, it's best to wait. I put myself in the passenger seat... I didn't want to jeopardize my new freedom just to cast a vote."
Civic Identity	Deservingness	Perceived legitimacy as full member of civic life (self or community perceived)	7/10	"We did what we did, but we've paid our time. We paid our dues, and we should have our rights back. The right to vote is a human right—and it should be ours."
	Political Efficacy	Perception that participation mattered or could change outcomes	6/10	"This will negatively impact participation from formerly incarcerated citizens in the long run. You have to consider your safety first, then rights. People question 'how much power do I have? Does my vote mean anything in normal circumstances?' If life and freedom are on the line, you stop believing in the system."

continued on next page

Table A7 (continued)

Theme	Subtheme	Definition	*N*	Representative Quote
Systemic Disenfranchisement	Voter Suppression	Characterization of arrests as tactic of exclusion/suppression	7/10	"Actually arresting people and going through the criminal justice system again, and the traumatization of just registering to vote when you think you could. Everything that comes with an arrest and a conviction, and everything that goes with it these people experience just because they were told they could register to vote. And I just completely think that that is 100% wrong."
	Institutional Intimidation	Sense that institutions were being weaponized against electorate through deterrence	8/10	"We then create a quasi-Gestapo to go arrest those individuals simply because they're voting as a convicted felon—It's hard for me to deduce that there is any other intention other than preventing a rather large demographic of individuals from voting or from feeling safe in voting."

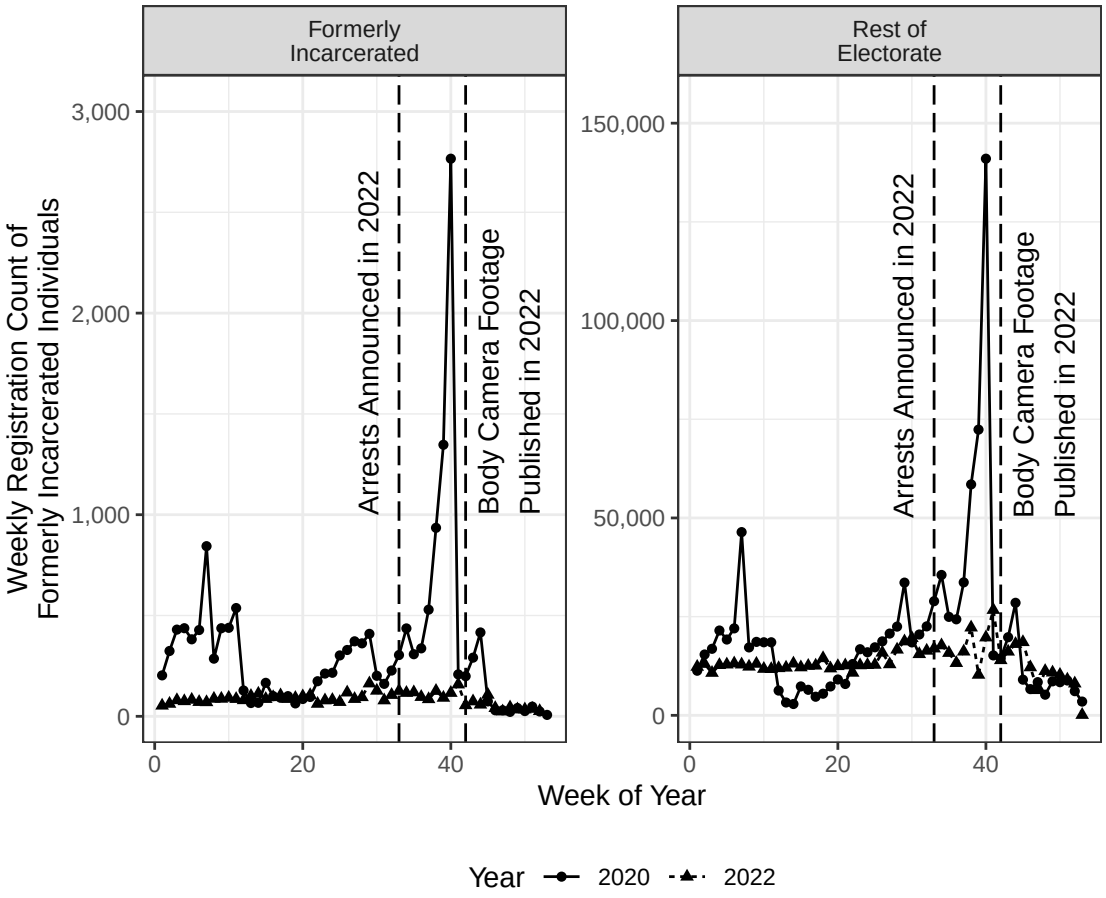
A.9 Analysis of Registration Counts

If some people who would otherwise have registered and voted in 2022 were scared off from even registering, our main analysis (restricted to registered voters) would not capture those effects. That would mean our main results underestimated the full effect of the arrests on voting. Unfortunately, we are limited in our ability to measure registration effects. There is no census of currently-eligible voters with felony records in Florida, so we have no denominator to calculate registration rates. And the timing of the arrests and news about them makes it particularly difficult to observe shifts in registration: Governor DeSantis' press conference about the arrests was held after the registration deadline for the August 2022 primary election, and then the body camera footage of the arrests was released after the registration deadline for the general election, so the two main events we might have expected to cause immediate drops in registration behavior fall at times when registration would be expected to be quite low anyway (since people wanting to vote in an upcoming election register before the registration deadline, meaning we generally see a big spike right before the deadline and very low rates immediately after it).

Still, we plot some raw data on registration patterns in both 2020 and 2022. The left panel of Figure A10 focuses on formerly-incarcerated voters, identified by a match between the information in their registration record and the OBIS database we use to build our main sample; the right panel shows all other voter registrations. We plot raw weekly counts of registrations for both groups in both years, due to the denominator issue described above. Figure A10 provides some weak circumstantial evidence that the footage may have scared voters away from registering: among formerly-incarcerated people, the number of new registrations in the weeks immediately following the publication of the body camera footage in 2022 was typically less than the counts from those same weeks in 2020, while for other voters the 2022 registration counts in October and November rivaled the 2020 counts. We consider this evidence very limited, but directionally consistent with the notion that the

footage chilled registration for people with felony records.

Figure A10: Weekly Registration Counts, 2020 and 2022



A.10 Durability of Dissuasion Effects

In addition to offering predictions about durable *population*-level treatment effects, our theory has implications for how *individuals* should act over time as well. Formerly incarcerated people who cycled out of the electorate — those who cast a ballot in either the 2020 general election or the 2022 state primary, but *not* in the 2022 general election — should have returned to the electorate less frequently than similar, non-formerly-incarcerated individuals. In Table A8 we show that this is the case: In the 2024 general election, turnout among formerly incarcerated individuals who sat out the 2022 general election was about 10 points lower than for non-formerly-incarcerated ones, other things being equal. This makes sense

if they were scared away from the ballot box. Of course, there are other reasons that such individuals might not have returned to voting, such as economic or housing instability, that are unobserved in this model. Nevertheless, these results are consistent with the idea that depressive effects persisted at the individual level and not just population level.

Table A8: 2024 General Election Turnout Among Subgroups

	Population:	
	Voted in 2020 General Did Not Vote in 2022 General	Voted in 2022 State Primary Did Not Vote in 2022 General
Model:	(1)	(2)
Variables		
Formerly Incarcerated Individual	-0.1077***	-0.1188***
	(0.0048)	(0.0166)
Democrat	0.0018**	0.0421***
	(0.0007)	(0.0030)
Republican	0.0340***	0.0454***
	(0.0007)	(0.0032)
Registration Date	$9.65 \times 10^{-6***}$	$5.57 \times 10^{-6***}$
	(9.2×10^{-8})	(2.95×10^{-7})
Age	$-2.22 \times 10^{-6***}$	$-2.05 \times 10^{-6***}$
	(4.9×10^{-8})	(1.77×10^{-7})
White	0.0493***	0.0381***
	(0.0010)	(0.0042)
Black	0.0027*	0.0138**
	(0.0011)	(0.0048)
Latino	0.0498***	0.0222***
	(0.0010)	(0.0048)
Neighborhood Median Income	$6.24 \times 10^{-7***}$	$3.67 \times 10^{-7***}$
	(9.99×10^{-9})	(3.95×10^{-8})
Neighborhood College Rate	0.0408***	0.0715***
	(0.0019)	(0.0079)
Neighborhood Unemployment Rate	-0.0565***	-0.0400*
	(0.0049)	(0.0190)
Neighborhood Population Density	$-4.6 \times 10^{-7***}$	$-7.9 \times 10^{-7***}$
	(4.22×10^{-8})	(2.09×10^{-7})
Constant	0.5398***	0.6768***
	(0.0016)	(0.0069)
Fit statistics		
Observations	3,002,585	135,380
R^2	0.01228	0.01115

IID standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

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