Turnout and Amendment 4: Mobilizing Eligible Voters Close to the Disenfranchised*

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Abstract

Recent scholarship has established a link between felony disenfranchisement and lower turnout, particularly in Black communities. Little work, however, has been done to interrogate how this depressive effect might be counteracted. In 2018, Amendment 4 was on the ballot in Florida, and promised to re-enfranchise most of the disenfranchised population. The presence of this ballot initiative offers a unique opportunity to investigate whether ballot initiatives of special interest to these impacted communities might recoup their depressed turnout. Using individual-level release records from the Florida Department of Corrections I test whether the ballot initiative mobilized neighborhoods and individuals in close proximity to formerly incarcerated individuals. Using multiple identification strategies, I find no evidence that Amendment 4 increased the turnout of these eligible voters, indicating that (re)incorporating their voices into American democracy might be even harder than formerly recognized.

Introduction

On November 6th, 2018, Floridians voted to amend their state constitution to re-enfranchise individuals with felony convictions in their past (Taylor 2018). The move was hailed as transformative for Floridian — and American — democracy; Uggen, Larson, and Shannon (2016) had estimated a few years earlier that some 1.5 million Floridians were disenfranchised and had finished serving their sentences, making the amendment the largest expansion of the franchise in the United States since the Twenty-sixth Amendment lowered the voting age to 18. The racial implications of the constitutional amendment were also clear: according to the same Uggen, Larson, and Shannon (2016) report, more than 1 out of every 5 Black Floridians was prohibited from democratic participation because of the state's disenfranchisement policies. Importantly, the amendment received broad support. Although it needed just 60 percent of the vote to pass, 64.5 percent of voters supported the ballot initiative. Conversely, the winning candidates for Governor and United States Senate won just 49.5 and 49.9 percent of the vote, respectively.

Prior to 2018, Floridians convicted of felony offenses were permanently disenfranchised unless they applied for and received an individual pardon from the state's clemency board. Rights restoration in Florida was characterized by a "low success rate, cumbersome process, and lengthy amount of time" (Miller and Spillane 2012b, 432). The process was driven in part by gubernatorial discretion: Charlie Crist, governor from 2007 to 2011, restored voting rights to roughly 150 thousand individuals, while Rick Scott did so for fewer than 3 thousand people between 2011 and 2019 (Schlakman 2018). Under Scott, individuals were required to wait between 5 and 7 years before applying for clemency. Once that condition had been met the waiting was not yet finished. At the time Amendment 4 was passed, it was widely reported that the backlog of applications was nearly 10,000 and stretched for as long as a decade (Ramadan, Stucka, and Washington 2018). Over the years, the procedure was subject to numerous lawsuits, and was ruled unconstitutional in early 2018 with Judge Mark Walker

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describing it as "a gauntlet of constitutionally infirm hurdles."[1] Amendment 4 made the rights restoration process automatic, restoring voting rights to individuals once they had completed "all terms of their sentence," though the amendment did not apply to individuals convicted of murder or sexual offenses.

Amendment 4 was somewhat unique in how it restored voting rights. In recent years the question of voting rights restoration has usually been decided by the executive, as in Kentucky (Wines 2019), or the legislature, as in Colorado (Baumann 2019). Florida, however, put the question directly to voters. In the Sunshine State, the general electorate had the opportunity to democratically decide whether they wanted to restore voting rights to neighbors convicted of felonies.

This manuscript explores whether the direct democratic approach of Amendment 4 increased participation among individuals who live in close proximity to a subset of the disenfranchised: formerly incarcerated individuals (data limitations hamper our ability to include individuals sentenced to felony probation). There is some literature that establishes that proximal contact with the criminal justice system reduces eligible Americans' propensity to vote (e.g. Bowers and Preuhs 2009; Burch 2013; King and Erickson 2016; Morris 2020). Little work, however, has been done exploring whether the opportunity to re-enfranchise family and community members can bring back these lost votes. The case of Amendment 4 in Florida offers a unique opportunity to investigate whether these eligible individuals can be reincorporated into our democratic processes. This project asks whether Amendment 4 increased turnout in neighborhoods and households impacted by the disenfranchisement arising from incarceration.

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[1]Hand et al. v. Scott et al., 4:17cv128-MW/CAS (U.S. District Court for the Northern District of Florida 2018).

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Theory and Literature

It well established that direct contact with the criminal justice system reduces voter turnout (see, for instance, Weaver and Lerman 2010; Burch 2011; White 2019b). The evidence that disenfranchisement policy reduces turnout among eligible voters, however, is somewhat mixed. Though most research concludes that there are likely negative "spillover" effects on turnout, others disagree. Some research finds that turnout is substantially lower in states with stricter voter disenfranchisement policies or more disenfranchised citizens (e.g. Bowers and Preuhs 2009; King and Erickson 2016), though Miles (2004) argues that these effects are small.

Of course, using state-level data to estimate the spillover effects of a phenomenon as geographically concentrated as felony disenfranchisement is perhaps methodologically unsound (Morris 2020). The little extant research examining the relationship between neighborhood-level turnout and the number of disenfranchised individuals has found a negative relationship. Traci Burch (2013) uses counts of incarcerated individuals, demonstrating that neighborhoods in North Carolina with more disenfranchised residents turned out at lower rates in the 2008 election. Morris (2020) leverages voter cancellation records to identify disenfranchised individuals with a history of participation. He finds that neighborhoods with more "lost voters" cast ballots at significantly lower rates than other neighborhoods in the 2017 New York City mayoral election, even after controlling for police activity. White (2019a), on the other hand, finds no long-term effects of having a housemate arrested for a misdemeanor offense, though it seems likely that a felony conviction and incarceration would have larger spillover effects than a misdemeanor arrest.

Understanding whether Amendment 4 was likely to recoup the lost turnout of eligible voters who lived with or near the disenfranchised requires understanding *how* their "proximal contact" (Rosenbaum et al. 2005; Walker 2014) depressed turnout to begin with. Existing research suggests multiple mechanisms through which proximal contact might shape politi-

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cal participation. White (2019a) divides these into "resource" and "social" mechanisms, an approach I adopt here as well.

Exposure to the criminal justice system can result in result in loss of resources for households and families. A felony conviction often involves requirements that an individual pay back fees, fines, and victim restitution (Martin et al. 2018). These legal financial obligations (LFOs) are particularly steep in the state of Florida, where LFOs are responsible for funding the criminal justice system (Stern 2019). LFOs are often shouldered by family members as well, leading to household financial burdens (Naser and Visher 2006). Individuals with felony convictions in their past also face difficulty securing stable employment (see Bushway, Stoll, and Weiman 2007), further burdening households. Stressors associated with the re-entry of a household member can make it less likely that eligible voters invest the time and resources involved in registering to vote, learning about candidates, or locating their polling place.

Proximal contact with the criminal justice system might also lower eligible voters' propensity to vote through social mechanisms. There is shame and stigma associated with having a felony conviction in one's past (Austin 2004) which may also extend to family members without a conviction. Work from Vesla Weaver and Amy Lerman (2010; 2014) describes in great detail how interactions with the criminal justice system structure how individuals interpret their relationship with the government, with a felony conviction serving as "a durable constraint and marker of their citizenship" (Lerman and Weaver 2014, 133). They explain: "custodial citizens become less likely to believe that they (and those like them) can change *the system*, a reduction in external efficacy" (Lerman and Weaver 2014, 137). High political efficacy and a positive relationship has been firmly linked with higher turnout (Soss 1999; Mettler 2002; Mettler and Soss 2004).

How, then, can we expect the impact of Amendment 4 to have influenced the turnout of eligible voters in proximal contact with the formerly incarcerated? It seems unlikely that the presence of Amendment 4 on the ballot could have reduced the costs of voting for this

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118 group, and therefore probably left the resource constraints reducing turnout undisturbed.

119 It is possible, however, that both the substance of the proposed constitutional amendment
120 and the messaging used by the campaign supporting its passage could have lessened some of
121 the social barriers to voting. Restoring voting rights to individuals who had been convicted
122 of felony offenses would end the "civil death" of felony disenfranchisement (Ewald 2002;
123 Miller and Spillane 2012a). The opportunity to redefine a family or community member's
124 relationship with the state could increase turnout among those in proximal contact with the
125 formerly incarcerated.

126 Moreover, the public messaging employed by the Amendment 4 campaign was explicitly de-
127 signed to change how voters understand the citizenship of disenfranchised individuals. The
128 campaign cast the ballot initiative as an issue of fairness, criticizing Florida's existing dis-
129 enfranchisement policy for creating two levels of citizenship. The organization leading the
130 campaign — Second Chances Florida — leveraged the notion that disenfranchised citizens
131 deserved to be re-incorporated into the body politic in its very name. The framing was
132 effective: the editorial boards of each of Florida's three biggest newspapers endorsed the
133 amendment, all using language related to fairness and civic redemption. The Tampa Bay
134 Times told readers they had a "remarkable opportunity to remedy that unfairness" (*Tampa*
135 *Bay Times* 2018); the Sun Sentinel informed voters "[t]here may never be an opportunity
136 to do a better thing than to vote yes on this reform" (*Sun Sentinel* 2018); and the Orlando
137 Sentinel said that Florida's then-policy "denie[d] our fellow citizens a second chance. It
138 denie[d] redemption" (*Orlando Sentinel* 2018). In addition to newspapers across the state,
139 the campaign deployed "volunteers from a broad coalition that included advocacy groups,
140 Christian organizations, the League of Women Voters, criminal justice experts and, of course,
141 those who had been convicted of felonies" (Robles 2018). Andrew Gillum, the Democratic
142 gubernatorial candidate, also vocally supported the amendment, discussing openly his fam-
143 ily's relationship with the criminal justice system and his own sibling's disenfranchisement
144 (Smith 2018).

The campaign in support of the amendment was massive: according to campaign finance data, Floridians for a Fair Democracy, Inc. (the political action committee sponsoring the ballot initiative) raised more than $26 million in the years leading up to the election.[2] The campaign was led by Desmond Meade, a disenfranchised individual making explicit demands on the state (Berman 2018). It effectively used the language of redemption and second chances, with major papers and other leaders throughout the state echoing the arguments that disenfranchised citizens were being treated unfairly.

The campaign may have altered how disenfranchised voters — and those in close proximity to them — understood their citizen identities and relationship to the state. Descriptive representation can increase external efficacy (Merolla, Sellers, and Fowler 2013). Seeing other criminal justice-involved individuals lead a statewide campaign and run for governor could have undone some of the negative socializing arising from their historic proximal contact. Further, the campaign seemed perfectly designed to cause voters to question whether the disenfranchised were being treated fairly. When criminal justice involvement is combined with a sense of injustice, voters can be motivated to participate (Walker and García-Castañon 2017; Walker 2020).

Finally, it is important to recognize that the geographic concentration of policing and incarceration patterns (e.g. Gelman, Fagan, and Kiss 2007) mean that individuals in proximal contact with the formerly incarcerated might also have other direct relationships with the criminal justice system. In 2017 there were 711,831 arrests in Florida,[3] but just 133,601 guilty felonious dispositions.[4] Although these proximal individuals have not been formally disenfranchised due to a felony conviction, even low-level interactions can have a "chilling effect" (Lerman and Weaver 2014, 1) on one's relationship with the government. The rhetoric used to argue in favor of the ballot initiative might have led them to reevaluate their own citizen identities despite not changing their eligibility to vote.

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[2]See https://dos.elections.myflorida.com/campaign-finance/contributions/#com.
[3]See https://florida.staterecords.org/arrests.php#:~:text=Crimes%20and%20Arrests.
[4]See http://edr.state.fl.us/Content/resource-demand/criminal-justice/reports/criminal-justice/cj7.pdf.

Research Design and Expectations

I begin by testing whether the number of formerly incarcerated individuals living in a neighborhood influenced that neighborhood's turnout in 2018. Because statewide probation records are not available, this analysis is based on only a subset of all disenfranchised individuals. Neighborhoods that are home to formerly incarcerated individuals are identified by geocoding release records from the Florida Department of Corrections, and I offer two definitions of neighborhoods.

Neighborhoods are first defined as precincts. Defining neighborhoods as precincts provides two benefits: firstly, because the Florida Division of Elections produces election results at this level, I can identify not only how many people cast a ballot for any office, but also how many people participated in the contest for Amendment 4. Election results are reported at the precinct-level, allowing me to measure the intensity of support for Amendment 4. I also test the salience of the ballot initiative — if voters in these neighborhoods find the initiative unimportant, it is unlikely to be mobilizing. One way of measuring the salience of a contest is by estimating "roll-off", or the share of voters who cast a ballot but failed to mark a preference for a given candidate or ballot initiative (Bullock and Dunn 1996). Given that some research indicates that Black voters are more likely to roll off than white voters (Vanderleeuw and Engstrom 1987; but see Knack and Kropf 2008), the question of roll-off is of particular interest when considering an issue that intersects with race such as the criminal justice system.

Unfortunately, the use of precinct-level data leaves us with a major drawback: when doing analysis at this level, bias-free turnout denominators are hard to come by. Because the Census Bureau does not produce population estimates for individual voting precincts, turnout cannot be calculated by dividing the number of ballots cast by the eligible population; turnout, rather, has to be constructed as a share of registered voters. If there is a relationship between the independent variable of interest and the registration rate of a neigh-

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borhood, our estimates will be biased. It is not difficult to imagine how this could be the case in the study at hand. Political organizers working on behalf of Amendment 4's passage may have focused on registering eligible residents in neighborhoods where disenfranchised individuals lived. If these organizers registered many new voters but a relatively small share of the new voters actually turned out, the net effect might be higher turnout among *eligible residents* but lower turnout among *registered voters*. For further discussion of how improper denominators can bias turnout estimates, see Amos, McDonald, and Watkins (2017) and Amos and McDonald (2020).

To address this potential problem, I also define neighborhoods as Census block groups. The Census Bureau makes estimates of the citizen voting-age population available at this level, providing a better denominator for calculating turnout. In this case, however, I must use a geocoded voter file to determine turnout. Because I aggregate the number of ballots cast in a block group from individual level data, I am unable to determine whether an individual actually participated in the contest for Amendment 4 or they rolled off. Similarly, I am unable to interrogate the relationship between block group characteristics and support for Amendment 4. Although each definition of neighborhood presents some drawbacks, the two definitions together can paint a full picture.

After examining whether the presence of formerly incarcerated individuals was related with neighborhoods' turnout rates, I ask whether the individuals with the potentially closest relationship to the disenfranchised were more likely to turn out in 2018. For this analysis, I use the release plan addresses of formerly incarcerated individuals (the last-known address according to the Department of Corrections) and voter file data to identify registered voters who live with formerly incarcerated individuals. Voters are considered "treated" if they live with a formerly incarcerated individual, and "untreated" otherwise. I then use a variety of individual- and neighborhood-level characteristics to match treated and untreated voters using a genetic algorithm (Sekhon 2011).

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After matching these voters, I employ a difference-in-differences specification to determine whether treated voters participated at higher rates in the 2018 election. These analyses are run for all individuals who share a household with a formerly incarcerated individual, as well as only the subset of households whose members have not been to prison for many years. This final specification allows me to disentangle the depressive proximal effect of a historical incarceration from the mobilizing effect of Amendment 4 in 2018 by incorporating any depressive effect into the pre-2018 baseline.

Table 1 summarizes the specific hypotheses this manuscript tests.

Table 1: Hypotheses

	Hypothesis	Approach
Neighborhood Level		
1a.	Each additional formerly incarcerated resident in a voting precinct is associated with increased turnout among registered voters in that precinct.	Standard regression
1b.	Each additional formerly incarcerated resident in a Census block group is associated with increased turnout among eligible citizens in that block group.	Standard regression
2.	Each additional formerly incarcerated resident in a voting precinct is associated with increased support for Amendment 4 in that precinct.	Standard regression
3.	Each additional formerly incarcerated resident in a voting precinct is associated with decreased roll-off in that precinct.	Standard regression
Household Level		
4.	Amendment 4 increased turnout in 2018 among household members of formerly incarcerated individuals. This treatment effect was especially large among households whose members have not been to prison for many years.	Difference-in-differences comparing turnout of voters in treated households to voters in untreated households.

Data

I leverage multiple data sources to investigate whether individuals in proximate contact with formerly incarcerated residents were more likely to vote in the 2018 election.

Department of Corrections Data

Data from the Florida Department of Corrections' Offender Based Information System (OBIS) is used to identify individuals who have been to prison in Florida. OBIS records include all individuals released from prison since October 1, 1997. Floridians who were incarcerated multiple times over the period have a different record for each incarceration; I retain only the records associated with the most recent incarceration (as measured by their release date). Cross-referencing this list with records of individuals currently in prison or on parole allows me to identify all individuals who had been to prison but were not serving a sentence at the time of the 2018 midterm election. These are a subset of the individuals who would be re-enfranchsied by Amendment 4. Individuals sentenced to felony probation, who were also disenfranchised, are not included in this data. I include only individuals with a valid release; individuals who died or absconded before their sentence was completed are removed from the dataset.

The OBIS provides the "release plan address" for individuals who were formerly incarcerated. According to the Department of Corrections, this is the last-known address for all formerly incarcerated individuals.[5] The address data, however, are messy. In some cases, the address field is left blank; in others, the record simply notes the road or the town of the former inmate, without providing full address information. These addresses require substantial cleaning.

I begin by removing all records where the address does not begin with an integer. In other

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[5]Using the release plan address for individuals last released from prison many years ago presents some potential problems. It is possible that the formerly incarcerated individuals have died or moved. In Appendix B I show that the results presented in the body of this manuscript continue to hold even when limiting the pool of formerly incarcerated people to individuals released from prison during or after 2015.

words, I assume that any record that begins with a letter does not have a full address and cannot be used (this results in the exclusion of just under 3 percent of records). The remaining addresses are geocoded. Individuals whose addresses were geocoded outside of Florida (10.6 percent) or for whom the geocoder failed (3.1 percent) are dropped. At least 94 percent of individuals released to addresses in Florida are therefore included. The failure rate is likely too small to materially impact the analyses.

Many formerly incarcerated individuals leave prison not for homes with family members, but rather to homeless shelters and halfway houses. For instance, three of the five most commonly listed addresses were Immigration and Customs Enforcement properties, one was owned by the Salvation Army, and one was a rescue mission. The body of this manuscript excludes formerly incarcerated individuals whose last known address was also listed by five or more other individuals, because neighborhoods may respond differently to institutions for returning citizens than the return of a family or neighbor. Appendix B demonstrates that the primary findings in the manuscript hold when I include all formerly incarcerated individuals, even if they returned to the same location as many other formerly incarcerated individuals. Just over 15 percent of formerly incarcerated individuals listed these sorts of addresses as their post-incarceration location.

The successfully geocoded, formerly incarcerated individuals are then mapped to their home Census block groups using shapefiles from the Census Bureau, and to their home voter precincts using shapefile data collected by Kelso and Migurski (2018).

Statewide data on individuals who formerly served a term on felony probation are not available. This may pose a problem for this study: neighborhoods with disenfranchised former probationers are also "treated," as are housemates of individuals sentenced to probation. Between 75 and 80 percent of individuals found guilty of felonies in recent years in Florida have been sentenced to probation and are therefore not included in this study.[6] We are therefore missing a large share of the individuals who stood to be re-enfranchised by Amendment 4.

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[6]See http://edr.state.fl.us/Content/resource-demand/criminal-justice/reports/criminal-justice/cj7.pdf

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The raw numbers, however, likely overstate the problem: not all individuals sentenced to felony probation actually lose their voting rights. Florida judges are allowed to "withhold adjudication" (Tragos and Sartes 2008). When adjudication is withheld, defendants consent to pay fines and restitution and to serve a term of probation — but the civil penalties of a felony conviction are not imposed. These individuals do not have their voting rights revoked.

Though unavailable statewide, probation records are available for Hillsborough County, the Florida county with the third-highest number of formerly incarcerated individuals according to the OBIS records. In Hillsborough County, the correlation coefficient between the number of felony probationers and formerly incarcerated residents (scaled by the number of voters) is 0.92 at the precinct level. This shows that the number of formerly incarcerated individuals in a neighborhood is a reasonable proxy for the total number of disenfranchised residents.

The neighborhood- and individual-level models presented in the body of this manuscript are re-estimated for Hillsborough County in Appendix A, with individuals sentenced both to felony incarceration and probation included. Their incorporation does not impact the primary results. Although this study relies on just a subset of the population re-enfranchised due to Amendment 4, the data available for robustness checks indicate that the relationships detailed here likely extend to the full population.

Voter File Data and Census Data

Individual-level voter file data are used for a variety of purposes in this analysis. I primarily use Florida voter file data from the data vendor L2 Political. This file includes information on individuals such as their home address, their age and gender, their participation history, and their political affiliation. L2 also geocodes voters to their home Census blocks, and provides latitude and longitudes.

Although the L2 data includes estimates of voters' race and ethnicity, the raw Florida voter file includes self-identified race and ethnicity. In place of L2's estimates, I use the self-

reported data found in the raw Florida voter file (the two lists can be joined using a unique identifier). I also use the raw Florida file to provide the gender for voters for whom L2 did not have an estimate, as well as voters' home counties and precincts.

Precinct and block group demographics are constructed using the voter file data. A precinct's average age is the average age of all voters registered in that precinct; the same is true for Census block groups. Some information such as median income, however, is not available at the individual level. For these variables, voters are assigned the median income (or education level, et cetera) of their home block group from the American Community Survey's 2014 – 2018 5-year estimates; the precinct average income, therefore, is effectively the average of all the block groups within that precinct, weighted by the number of registered voters.

For the individual-level analyses, I use demographic controls obtained from the voter files, and neighborhood-level demographic controls like income from the voter's block group.

The 2018 election saw the highest turnout of any midterm in a century (Kilgore 2018), and the treatment group might have been motivated to turn out through avenues other than the ballot initiative. Andrew Gillum was poised to become the state's first Black governor, which could increase Black turnout (e.g. Washington 2006). By controlling for neighborhood demographics (and, in the matching exercise, forcing control voters to mirror treated voters on key demographics such as race and party affiliation), I minimize the differences between the two groups along characteristics known to influence turnout.

It is also worth considering whether electoral reforms might have differently structured 2018 turnout for neighborhoods home to formerly incarcerated individuals (and, for later analyses, members of treated households). There is little reason to believe this is the case. Although the number of early voting days was cut for the 2012 general election, the longer period was restored for the 2014 election and remained constant in 2016 and 2018.[7] Early voting was not allowed on college campuses in the 2014 and 2016 elections, though it was allowed in

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[7]See https://ballotpedia.org/Voting_in_Florida.

2018 (Bousquet 2018). Neighborhood-level estimates of collegiate education, used in each of the regressions, are expected to guard against the possibility of differential turnout effects arising from this change. Florida did not enact other reforms such as same-day registration or automatic voter registration that might alter registrations or turnout over the time period, and its absentee voting rules similarly did not change. We can therefore be confident that any turnout effects observed are not being driven by asymmetric responses to electoral rule changes.

Matched Department of Corrections and Voter File Data

Identifying eligible registered voters who lived with formerly incarcerated individuals in the 2018 election requires matching on addresses. As discussed above, these addresses are often in different formats. To increase the quality of the matches, I standardize common street and address abbreviations as well as capitalization. "Boulevard," for instance, becomes "BLVD" in each instance in the DOC and voter file data. These standardizations are taken from Appendix C of the USPS Postal Addressing Standards (2015). Exact matching is required.

Neighborhood-Level Results

I begin by examining whether — and to what extent — neighborhoods with formerly incarcerated individuals differ from neighborhoods elsewhere in the state. A simple comparison of neighborhoods with and without formerly incarcerated individuals, however, proves unhelpful: 97.1 percent of block groups in the state are home to someone who has been to prison. There are, however, neighborhoods where formerly incarcerated individuals are concentrated. In Column 1 of Table 2, I take the statewide mean of block group characteristics, weighted by each block group's population. In Column 2, I re-weight the block groups by the number of formerly incarcerated residents.

Table 2:   Neighborhood Demographics

Measure	Average Neighborhood	Average Neighborhood for Formerly Incarcerated
Median Income*	$59,988	$45,484
Median Age*	42.5	39.9
% Unemployed*	6.4%	8.9%
% with Some College*	73.0%	65.2%
% Non-Hispanic White*	54.4%	44.5%
% Non-Hispanic Black*	15.4%	30.5%
% Latino*	25.2%	20.7%
Count	20,590,223	279,324

* Difference is significant at 95 percent confidence level.

Although nearly all parts of the state are impacted by the criminal justice system (and, more specifically, mass incarceration), Table 2 makes clear that individuals return home to neighborhoods with lower incomes, higher levels of unemployment, and where a much larger share of the population is Black than other neighborhoods.

Having established that formerly incarcerated individuals are concentrated in lower-resourced neighborhoods, I investigate whether turnout in their neighborhoods was higher in 2018 than in other similar neighborhoods. To test this relationship I run ordinary least squares regressions, where precinct- and block group-level turnout in 2018 are the dependent variables. In the precinct-level model, turnout is calculated by dividing the number of ballots cast for or against Amendment 4 by the number of actively registered voters in the precinct.[8] The number of ballots cast in each block group is calculated by taking the sum of all individuals marked as participants in the registered voter file. Block group-level turnout is calculated by dividing this number by the adjusted citizen voting age population (ACVAP). I define

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[8]The 35 precincts where calculated turnout exceeds 100 percent have been dropped from the analysis.

³⁶⁴ ACVAP by subtracting the number of all formerly incarcerated individuals from the Census

³⁶⁵ Bureau's estimated citizen voting age population (including the individuals who are excluded

³⁶⁶ from the primary independent variable count because they returned to common post-release

³⁶⁷ residences).[9] *Formerly Incarcerated Residents* is the primary independent variable. In mod-

³⁶⁸ els 2 and 4 I also include a measure of how long the average formerly incarcerated resident

³⁶⁹ has been out of prison (*Av. Years since Most Recent Incarceration*) to test whether neigh-

³⁷⁰ borhoods with more recent releasees turned out at different rates than those whose residents

³⁷¹ have been out of prison for many years.

³⁷² Table 3 presents the results of these regressions. In addition to the number of formerly

³⁷³ incarcerated residents, I control for the racial, gender, and partisan composition of the

³⁷⁴ neighborhood. Although the dependent variable measures only turnout in 2018 (and there

³⁷⁵ is just one observation per neighborhood), I control for turnout rates from the 2010 – 2016

³⁷⁶ general elections. Given that the literature indicates that these neighborhoods likely have

³⁷⁷ lower baseline turnout due to proximal contact with the carceral state, controlling for past

³⁷⁸ turnout is key. It is possible that turnout in these neighborhoods was higher in 2018 relative

³⁷⁹ to their own turnout in recent years but indistinguishable in 2018 from other neighborhoods

³⁸⁰ less impacted by incarceration. Only by controlling for historical turnout can we measure

³⁸¹ whether any lost turnout was "recouped." Historical turnout is calculated using the registered

³⁸² voter file.

³⁸³ I also control for neighborhoods' average age, median income, share with some collegiate

³⁸⁴ education, and unemployment rate. Finally, fixed effects for congressional districts are in-

³⁸⁵ cluded to account for asymmetric turnout effects arising from the competitiveness of different

³⁸⁶ races, and robust standard errors are clustered at this level.[10]

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[9]My definition of ACVAP is similar in theory to the voting eligible population estimated by McDonald (2002). It differs slightly, however, because I do not have estimates of the number of individuals disenfranchised for a felony probation at the neighborhood-level.

[10]Where neighborhoods cross congressional district boundaries they are assigned to the district in which most of their voters live.

Table 3: Neighborhood Turnout in 2018

	Precinct-Level		Block Group-Level	
	(1)	(2)	(3)	(4)
Formerly Incarcerated Residents	−0.0002***	−0.0002***	−0.0002***	−0.0002***
	(0.00004)	(0.00003)	(0.00004)	(0.00004)
Av. Years since Most Recent Incarceration		0.0001		0.0002*
		(0.001)		(0.0001)
Percent White	0.017	−0.088	0.017	0.017
	(0.110)	(0.123)	(0.014)	(0.014)
Percent Black	0.027	−0.086	0.041**	0.040**
	(0.109)	(0.121)	(0.017)	(0.017)
Percent Latino	−0.081	−0.175	−0.007	−0.008
	(0.116)	(0.125)	(0.016)	(0.016)
Percent Asian	0.082	−0.006	0.040*	0.039*
	(0.128)	(0.166)	(0.022)	(0.022)
Percent Male	0.302	0.376**	0.095	0.102
	(0.188)	(0.179)	(0.086)	(0.089)
Percent Democrats	0.059	0.161**	0.067***	0.067***
	(0.082)	(0.073)	(0.020)	(0.020)
Percent Republicans	0.015	0.105	0.007	0.004
	(0.081)	(0.070)	(0.024)	(0.024)
Average Age	0.0001	0.0001	0.001***	0.001***
	(0.0005)	(0.001)	(0.0003)	(0.0003)
Average Income ($10,000s)	0.002**	0.001**	0.002***	0.002***
	(0.001)	(0.001)	(0.0003)	(0.0003)
Percent With Some College	0.183***	0.188***	0.082***	0.082***
	(0.016)	(0.020)	(0.005)	(0.005)
Percent Unemployed	−0.032	−0.033	−0.005	−0.004
	(0.025)	(0.028)	(0.006)	(0.006)
Constant	−0.211*	−0.235*	−0.188**	−0.200**
	(0.114)	(0.127)	(0.083)	(0.087)
Congressional District FEs	X	X	X	X
Turnout in 2010 − 2016	X	X	X	X
Observations	5,797	5,477	10,817	10,550
R^2	0.782	0.814	0.979	0.979
Adjusted R^2	0.781	0.813	0.979	0.979

***$p<0.01$, **$p<0.05$, *$p<0.1$.
Robust standard errors (clustered by congressional district) in
parentheses.

Table 3 indicates that there is a negative relationship between the number of formerly incarcerated residents and turnout in a precinct. The same is true of block groups, disproving hypotheses 1a and 1b. The average length of time since formerly incarcerated residents' most recent incarceration is not related to a neighborhood's turnout. The block group models have nearly twice as many observations as the precinct-level ones and their R^2s are considerably higher, perhaps indicating a better fit. Nevertheless, the estimated coefficient for *Formerly Incarcerated Residents* is the same (when rounded to the tenth of a percentage point) for both neighborhood definitions.

The primary coefficients in Table 3 are small and difficult to interpret without context. Figure 1 shows the marginal effect of each additional formerly incarcerated resident on precinct-level turnout for Amendment 4 from model 1. All other covariates are held at their means. Although the number of formerly incarcerated residents in a precinct reaches a maximum of 594, there are 300 or fewer such residents in 99.2 percent of precincts, and I limit the figures to this range. Predicted turnout in precincts with zero formerly incarcerated residents is just over 66 percent; in precincts with 300 such residents, predicted turnout was below 61 percent, implying a five point decrease over the effective range of observed values.
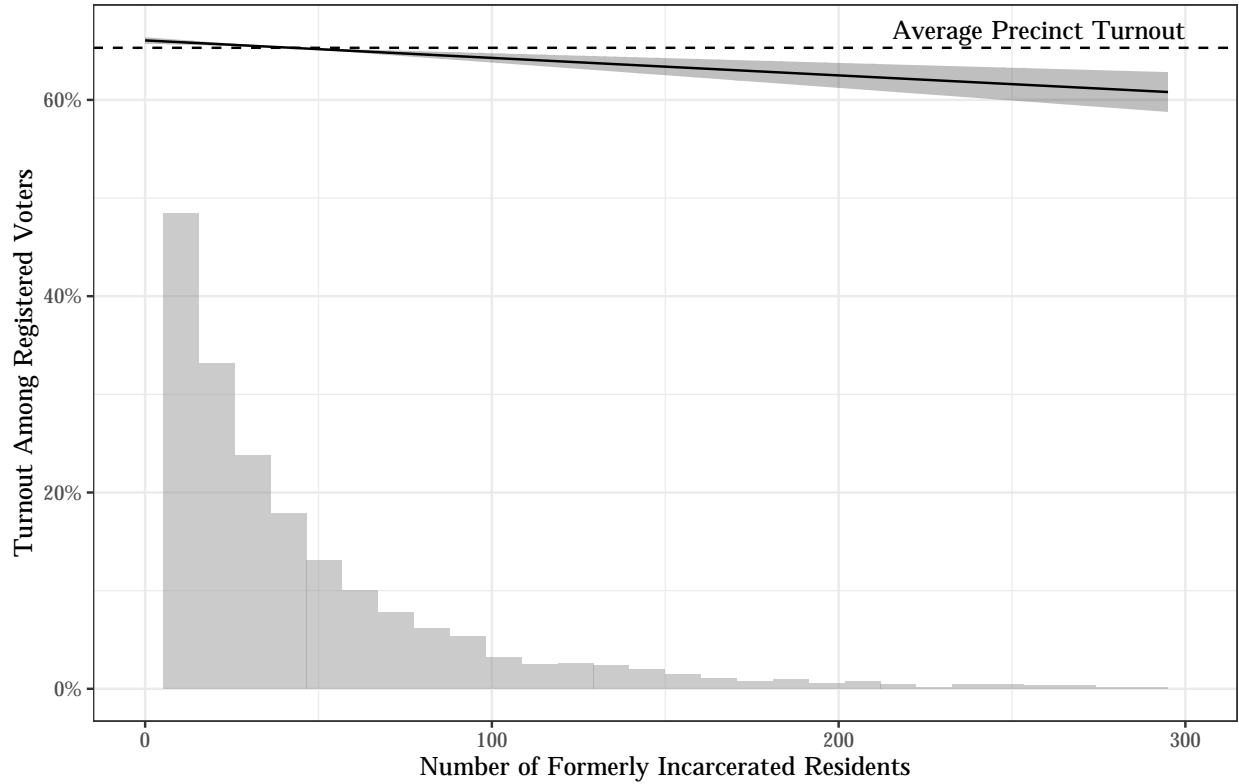
Notes: Distribution of number of formerly incarcerated residents shown at bottom.

Figure 1: Marginal Effect of Each Formerly Incarcerated Residents on Precinct Turnout Among Registered Voters

The precinct-level data allows me to test for other relationships between the number of formerly incarcerated residents in a neighborhood, the length of time since residents were released from prison, and that neighborhood's engagement with Amendment 4. In Table 4 I present the results of OLS models that test whether the number of formerly incarcerated community members influences a neighborhood's support for Amendment 4 or Amendment 4 roll-off. Roll-off is calculated as $1 - \frac{Ballots\,Cast\,for\,Amendment\,4}{Ballots\,Cast\,in\,Contest\,with\,the\,Most\,Votes}$. It ranges from zero (if everyone who cast a ballot made a decision on the Amendment 4 question) to one (if no participants voted for or against Amendment 4). A lower number, therefore, represents lower roll-off and higher engagement.

Table 4:   Precinct Engagement with Amendment 4

	Support for Am. 4		Roll-Off	
	(1)	(2)	(3)	(4)
Formerly Incarcerated Residents	0.0001**	0.0001**	−0.00004***	−0.00004***
	(0.00003)	(0.00003)	(0.00001)	(0.00001)
Av. Years since Most Recent Incarceration		0.002**		0.0004**
		(0.001)		(0.0002)
Percent White	0.069	−0.051	−0.071*	−0.076*
	(0.122)	(0.093)	(0.042)	(0.046)
Percent Black	0.188*	0.026	−0.042	−0.048
	(0.107)	(0.084)	(0.040)	(0.042)
Percent Latino	0.049	−0.101	−0.050	−0.052
	(0.114)	(0.092)	(0.043)	(0.045)
Percent Asian	0.244	0.133	−0.101*	−0.117*
	(0.177)	(0.170)	(0.052)	(0.061)
Percent Male	−0.383**	−0.299*	−0.204*	−0.193*
	(0.185)	(0.170)	(0.113)	(0.117)
Percent Democrats	0.192	0.197	0.031	0.024
	(0.143)	(0.191)	(0.021)	(0.029)
Percent Republicans	−0.396***	−0.429***	0.039*	0.037
	(0.120)	(0.151)	(0.020)	(0.027)
Average Age	−0.0003	0.00005	0.001***	0.001***
	(0.0004)	(0.0004)	(0.0002)	(0.0002)
Average Income ($10,000s)	−0.003***	−0.002**	−0.00003	−0.00004
	(0.001)	(0.001)	(0.0002)	(0.0002)
Percent With Some College	0.155***	0.158***	−0.029***	−0.032***
	(0.034)	(0.029)	(0.006)	(0.008)
Percent Unemployed	−0.015	−0.024	−0.019*	−0.011
	(0.018)	(0.021)	(0.011)	(0.010)
Constant	1.023***	1.055***	0.220**	0.212**
	(0.165)	(0.197)	(0.095)	(0.105)
Congressional District FEs	X	X	X	X
Turnout in 2010 − 2016	X	X	X	X
Observations	5,797	5,477	5,797	5,477
R^2	0.788	0.869	0.315	0.385
Adjusted R^2	0.787	0.868	0.309	0.380

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.
Robust standard errors (clustered by congressional district) in
parentheses.

Table 4 demonstrates that, although Amendment 4 did not boost precinct-level turnout, precincts with more formerly incarcerated residents *did* support Amendment 4 at slightly higher rates. Similarly, roll-off was lower in neighborhoods with more formerly incarcerated resident. Thus it appears that while the presence of formerly incarcerated individuals in a neighborhood was not associated with getting people into the voting booth, it was associated with how voters cast their ballots once there. These models support hypotheses 2 and 3.

Figures 2 and 3 plot the marginal effect of each additional formerly incarcerated resident on a precinct's support for Amendment 4 (model 1), and the precinct's roll-off on Amendment 4 (model 3). These figures make clear that the number of formerly incarcerated residents has a relatively small impact on precinct support for its passage, and a relatively large impact on precinct level roll-off.
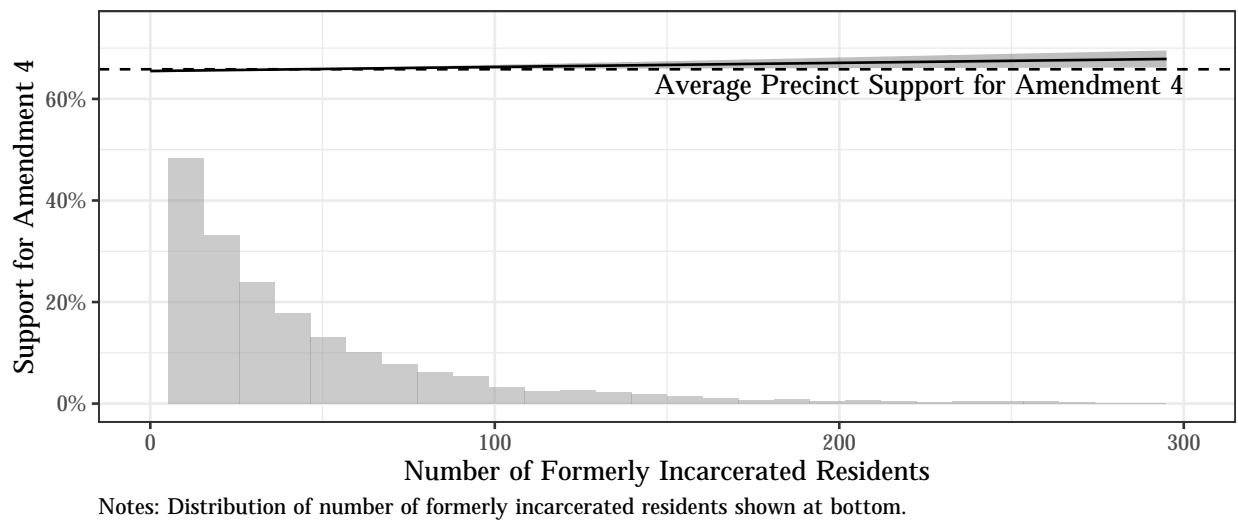

Notes: Distribution of number of formerly incarcerated residents shown at bottom.

Figure 2: Marginal Effect of Formerly Incarcerated Residents on Support for Amendment 4

Notes: Distribution of number of formerly incarcerated residents shown at bottom.
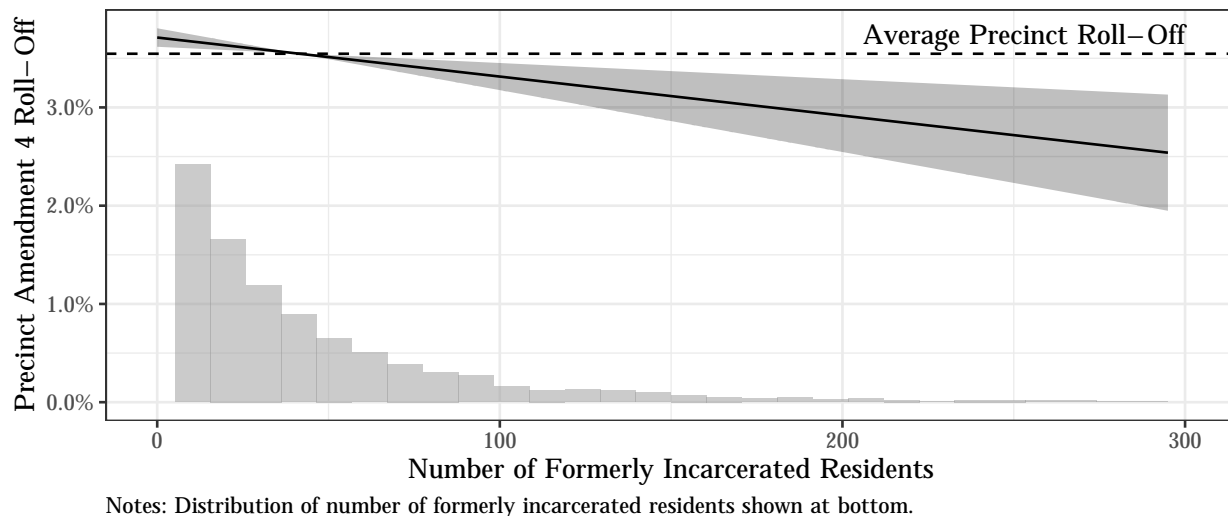
Figure 3: Marginal Effect of Formerly Incarcerated Residents on Amendment 4 Roll-Off

As expected, the number of formerly incarcerated individuals was highly correlated with roll-off. The issue of voting rights restoration is clearly more salient in neighborhoods where a larger share of the population would be affected. Why the relationship between formerly incarcerated residents and support is less strong (though positive) is not as clear, perhaps pointing to a variety of individual responses to crime and criminal justice policy in these neighborhoods. As Leverentz (2011) argues, punitiveness is positively correlated with the salience of crime; therefore, the presence of recently incarcerated residents might activate both punitiveness and support for the amendment, with support winning out slightly.

Neighborhoods where the average formerly incarcerated resident has been out of prison for longer saw both higher support for Amendment 4 and higher roll-off. It is unsurprising that the ballot initiative is less salient in areas with less-recent incarcerations. Further, it is possible that temporal removal from the return of a resident from prison erodes punitiveness more quickly than support.

Individual-Level Results

Thus far I have established that neighborhoods with more formerly incarcerated residents turned out at lower rates than other neighborhoods. By controlling for historical neighborhood turnout, the negative spillover effects of felony disenfranchisement should have been incorporated into the baseline. That turnout was lower in 2018 even after controlling for historical spillover effects is sobering, indicating imperviousness in these neighborhoods to the conditions that led to high turnout across the state.

The use of neighborhood data, however, can obscure underlying patterns. Encouragements to vote at the household level might be too small to register at the neighborhood level. It is possible that Amendment 4 shaped turnout differently for individuals who cohabitate with formerly incarcerated individuals than for their neighbors. A neighborhood may have disengaged from the political process thanks to a history of aggressive state action. Household members of the formerly incarcerated may have had a similar historical response, and yet be more susceptible to mobilization from Amendment 4; they are, after all, the individuals whose identities are most likely shaped by proximity to felony disenfranchisement. The presence of formerly incarcerated individuals may have successfully mobilized household members but not neighbors.

Using individual-level data, I here distinguish the effect of proximal neighborhood contact from individual contact on turnout in 2018. As discussed above, I identify individuals who live with formerly incarcerated individuals by matching addresses listed in the Department of Corrections release data to the registered voter file. All registered voters who live at an address reported by a formerly incarcerated individual are considered "treated."

Each treated individual is then matched with five untreated registered voters elsewhere in her congressional district.[11] I use five matches in order to increase the sample size of the study; the large pool of potential controls means this can be done without sacrificing the

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[11]Due to computing constraints, a random 5 percent random sample stratified by treatment status is used to calculate the genetic weights. The full sample is used for matching.

quality of the matches. Exact matching is done on all characteristics with the exception of registration date, age, median income, and share with some collegiate education, and matching is done with replacement. Ties are not broken, which means that some treated voters may have more than five controls; the regression weights are calculated to allow for this possibility. Voters are matched using their block group's median income and share with some collegiate education (from the ACS 5 year estimates ending in 2018), while all other characteristics come from the individual-level voter file.

Table 5 presents the results of the matching exercise. In addition to the characteristics reported in Table 5, matches are required to come from the same congressional district to account for levels of congressional competition in the 2018 election.

Table 5: Balance Table

| | Means: Unmatched Data | | Means: Matched Data | | Percent Improvement | | | |
	Treated	Control	Treated	Control	Mean Diff	eQQ Med	eQQ Mean	eQQ Max
%White	41.5%	63.2%	41.5%	41.5%	100.00	100.00	100.00	100.00
% Black	38.8%	12.7%	38.8%	38.8%	100.00	100.00	100.00	100.00
% Latino	12.8%	16.9%	12.8%	12.9%	99.86	99.86	99.86	99.86
% Asian	0.8%	2.0%	0.8%	0.8%	100.00	100.00	100.00	100.00
% Female	55.2%	52.4%	55.2%	55.2%	100.00	100.00	100.00	100.00
% Male	41.5%	45.0%	41.5%	41.5%	99.74	99.74	99.74	99.74
Registration Date	2004-01-28	2004-09-24	2004-01-28	2004-02-10	94.63	30.85	20.67	16.86
Age	48.95	52.45	48.95	48.82	96.12	95.63	93.77	91.93
% Democrat	53.7%	36.9%	53.7%	53.7%	100.00	100.00	100.00	100.00
% Republican	21.0%	35.4%	21.0%	21.0%	100.00	100.00	100.00	100.00
% with Some College	66.5%	75.3%	66.5%	66.5%	99.88	99.93	99.89	99.54
Median Income	$47,389	$62,995	$47,389	$47,401	99.93	99.85	99.76	99.32

As Table 5 makes clear, the treated registered voters differ in meaningful ways from the rest of the electorate: they are three times as likely to be Black, they are substantially more likely to be registered as Democrats, and they live in neighborhoods with lower incomes. The matching process, however, results in a control group that is very similar to the treatment group; in each measure, there was at least a 94 percent improvement in the mean difference.

After matching the treated voters to appropriate controls, I construct a difference-in-differences model. Before presenting the results of the model, I show in Figure 4 that the

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parallel trends assumption is satisfied: although the treatment group has lower turnout rates in general, the gap between the treatment and control groups is largely constant between 2010 and 2016.

Turnout in each year is measured among voters registered in 2018. Later years show higher turnout in part because some voters who cast ballots in the earlier elections were no longer registered in 2018. Voters who registered late in the period, on the other hand, are included in the denominator each year. By including registration date in the matching procedure I control directly for potential differences in registration timing between treated and control voters. Of course, some of the increase in turnout observed in later years in Figure 4 can be attributed to higher "real" turnout as a share of eligible citizens.
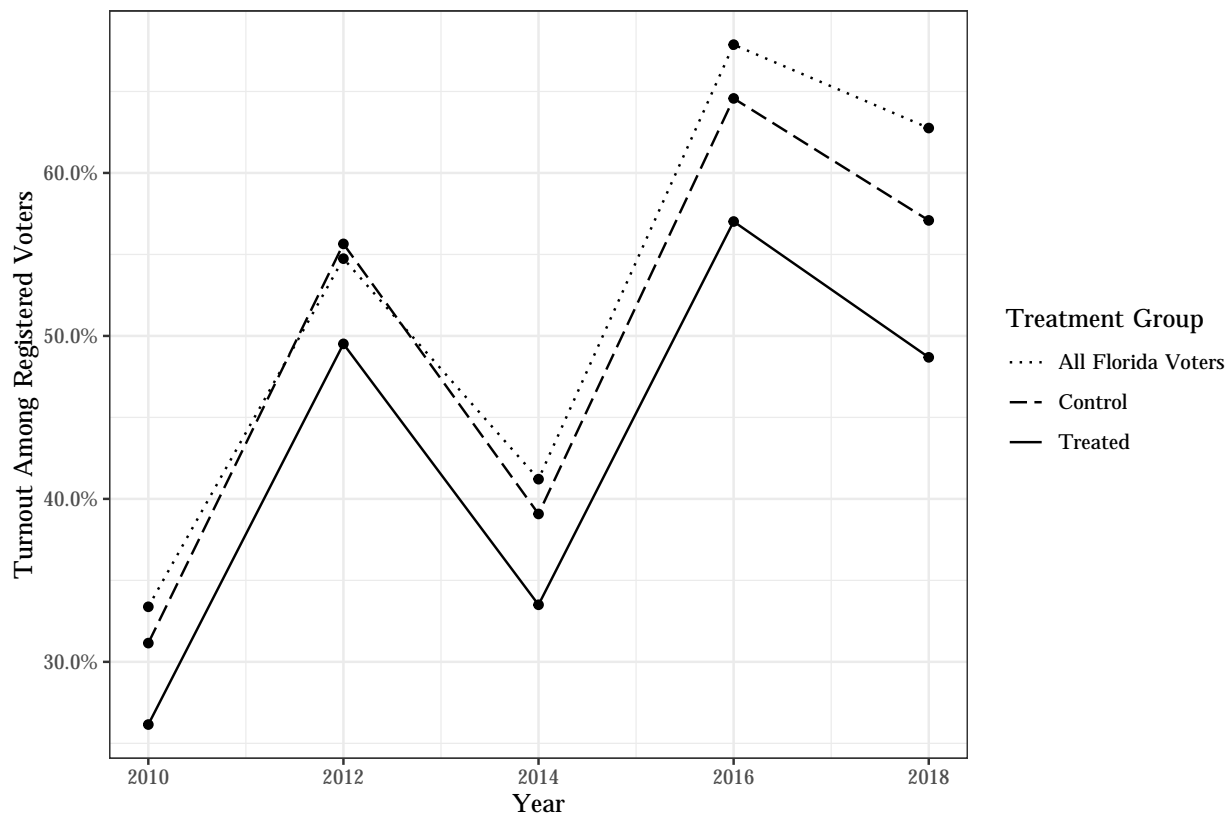

Figure 4: General Election Turnout for Treated and Control Voters, 2010 – 2018

The trends presented in Figure 4 offer preliminary visual corroboration of what I find at the

neighborhood level — namely, that 2018 turnout was not higher for voters in proximal contact with formerly incarcerated individuals. Table 6 formalizes these trends into an ordinary least squares regression.[12] A treatment dummy distinguishes treated from control voters. The treatment dummy is interacted with another dummy identifying the 2018 election. Robust standard errors are clustered at the level of the match (Abadie and Spiess 2019). Model 1 presents the model output without the other controls used for matching; Model 2 includes these covariates.

In models 3 and 4 of Table 6 I consider the possibility that, as households become further removed from a member's incarceration, the negative effects of their incarceration on turnout dissipate. In these models, the dummies indicating treatment and the 2018 election are interacted with the number of years since the most recent completion of a term of incarceration for a household member (*Years Since Latest Incarceration*, shortened to *Years Since* in interactions). Matched control observations are assigned the value associated with their treated observation. Model 3 includes no other covariates, while Model 4 includes the matched variables.

It is, of course, highly possible that formerly incarcerated individuals no longer live in the same household they reported when leaving prison. This is especially true for individuals released from prison early in the sample. To control for this possibility, Models 5 – 8 include only the treated individuals (and their matches) whose registration dates are earlier than the latest prison release date of a household member. These are individuals, therefore, that we can be relatively sure lived with an incarcerated individual. The treatment effects in these models, though slightly larger, tell the same general story.

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[12]Although the dependent variable here is binary — it takes the value 0 if a voter does not participate, and 1 if she does — the coefficients produced by logistic regressions in the difference-in-differences context are largely uninterpretable. I thus use a linear specification here. When the models are estimated using a logistic specification, the treatment effect is virtually identical.

Table 6: General Election Turnout, 2010 – 2018

	All Matched Observations				Registration Date prior to Release Date			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D(2018)	0.095***	0.095***	0.095***	0.095***	0.057***	0.057***	0.082***	0.082***
	(0.0004)	(0.0004)	(0.001)	(0.001)	(0.0005)	(0.0005)	(0.001)	(0.001)
D(Treated)	−0.060***	−0.061***	−0.075***	−0.076***	−0.053***	−0.063***	−0.065***	−0.067***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Years Since Latest Incarceration			−0.00001	−0.00004			0.013***	0.002***
			(0.0001)	(0.0001)			(0.0001)	(0.0001)
D(2018) × D(Treated)	−0.023***	−0.023***	−0.039***	−0.039***	−0.035***	−0.035***	−0.049***	−0.049***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
D(2018) × Years Since			−0.0001	−0.0001			−0.004***	−0.004***
			(0.0001)	(0.0001)			(0.0001)	(0.0001)
D(Treated) × Years Since			0.002***	0.002***			0.002***	0.001***
			(0.0001)	(0.0001)			(0.0002)	(0.0002)
D(2018) × D(Treated) × Years Since			0.002***	0.002***			0.002***	0.002***
			(0.0002)	(0.0002)			(0.0002)	(0.0002)
Constant	0.476***	0.015***	0.477***	0.015***	0.572***	−0.046***	0.493***	−0.057***
	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.005)	(0.001)	(0.005)
Includes covariates from matching		X		X		X		X
Congressional District fixed effects		X		X		X		X
Observations	7,454,880	7,454,880	7,454,880	7,454,880	4,916,040	4,916,040	4,916,040	4,916,040
R^2	0.009	0.201	0.009	0.201	0.005	0.157	0.023	0.158
Adjusted R^2	0.009	0.201	0.009	0.201	0.005	0.157	0.023	0.158

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.
Robust standard errors (clustered at level of match) in parentheses.

Each of the specifications presented in Table 6 identify a negative treatment effect. The coefficients on *D(2018) × D(Treated)* in Models 1 and 2 indicate that turnout among treated voters was about 2.3 percentage points below what it would have been if the gap between treated and control voters in 2018 had conformed to prior years.

There is some indication that "spillover" effects lessen over time. In each model, *D(2018) × D(Treated) × Years Since* and *D(Treated) × Years Since* is positive and statistically significant. In other words, individuals who lived with housemates who had not been imprisoned in many years were more likely to vote than other treated voters, and this was especially true in 2018. Models 3 and 4 estimate that the treatment effect for individuals whose household member returned from prison within one year of the election was about -3.9 percentage points, but that for each year the most recent incarceration recedes into the

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past, the treatment effect decreases by about 0.4 percent. That the spillover effects "decay" is a positive sign, and indicates that the negative socialization induced by a housemate's incarceration might not be permanent. It is also possible that incarcerations occurring in different historical periods like the late 1990s were interpreted differently and had smaller spillover effects.

That the effect is moderated by time is unsurprising. Individuals whose household members went to and were released from prison between the 2016 and 2018 elections, for instance, received two treatments: they both were "negatively" treated by their proximal contact with the criminal justice system and potentially "positively" treated by Amendment 4. What *is* surprising, however, is the continued negative treatment effect even for the households that were most removed from their proximal contact with the incarceration of a household member. Table 7 presents the results of Models 5 and 6 from Table 6, but limits the pool to households where the most recent incarceration ended prior to 2010. The negative shock of proximal contact for these individuals should be reflected in the base years of the difference-in-differences models. That *D(2018) × D(Treated)* remains significant and negative for these individuals is puzzling.

Table 7:   General Election Turnout, 2010 − 2018

	(1)	(2)
D(2018)	0.034***	0.034***
	(0.001)	(0.001)
D(Treated)	−0.043***	−0.055***
	(0.002)	(0.002)
D(2018) × D(Treated)	−0.022***	−0.022***
	(0.002)	(0.002)
Constant	0.651***	−0.010
	(0.001)	(0.012)
Includes covariates from matching		X
Congressional District fixed effects		X
Observations	1,524,000	1,524,000
R^2	0.003	0.103
Adjusted R^2	0.003	0.103

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.
Robust standard errors (clustered at level of match) in parentheses.

This negative, significant finding for treated individuals whose household members had been out of prison for many years should probably not be interpreted to mean that Amendment 4 had a demobilizing effect on individuals in proximal contact to formerly incarcerated individuals. Rather, it likely points to meaningful differences in susceptibility to broad mobilization for those in proximal contact with the criminal justice system. As discussed above, the 2018 election was historic in many ways with higher turnout than any midterm in a century. Many marginal voters who had not previously participated in midterm elections turned out.

And yet, this negative treatment effect is troubling. As Table 5 indicates, the matching procedure was successful at creating a control group that looks nearly identical to the treatment group according to observable characteristics. Not only did Amendment 4 fail to mobilize voters who live in close proximity to the formerly incarcerated; it appears that these voters

are also *less* mobilized by the same factors that encourage similar voters to participate in unique elections such as that of 2018. This analysis cannot determine whether the proximal contact caused this imperviousness to broadly mobilizing conditions, or if individuals who live in close proximity to the formerly incarcerated would have been more likely to remain on the sidelines in an election like 2018 even without the proximal contact in their past. Nevertheless, their relatively depressed turnout in 2018 — even in the presence of Amendment 4 on the ballot — underscores just how difficult their political (re)integration is.

Discussion and Conclusion

Turnout in 2018 hit historic levels for a midterm election as infrequent voters participated and made their voices heard. In addition to hotly contested Congressional, senate, and gubernatorial races, Floridians were presented with the opportunity to restore voting rights to well over a million permanently disenfranchised individuals who had been convicted of felony offenses. Amendment 4 and its organizers were hugely successful — in a year where both statewide winners won by less than 0.5 percentage points, nearly two-thirds of Floridians supported expanding the franchise. Neighborhoods and voters most directly impacted by felony disenfranchisement stood to gain meaningful political representation from the passage of the amendment. And these proximal voters *did* turn out at higher rates in 2018 than in previous elections: turnout among treated voters registered as of the 2014 midterm increased from 41.5 percent that year to 52.5 percent in 2018. The presence of formerly incarcerated individuals measurably increased the degree to which a neighborhood supported and engaged with the amendment.

Despite these major gains in turnout among individuals in proximate contact with the carceral state, I fail to uncover evidence that Amendment 4 itself increased the turnout among these individuals above-and-beyond the increases observed among other voters and in other communities. Although the substance of the amendment and language used to pro-

mote it spoke of a reconciled citizenship, they were not more likely to participate. In fact, the evidence points in the opposite direction: turnout for these voters actually increased *less* in 2018 than it did for other voters. Not only was Amendment 4 not particularly mobilizing, but these voters are also less susceptible to factors contributing to statewide surges in turnout. However, the strong relationship between roll-off and the number of formerly incarcerated residents implies that the rights restoration was highly salient for the voters who did cast a ballot.

Why Amendment 4 did not mobilize these voters is not immediately apparent. It could be an issue of voter knowledge: previous research has indicated that marginalized voters demonstrate lower political knowledge (Erikson 2015). If these proximal contact voters were not aware that Amendment 4 was on the ballot, the amendment could hardly have mobilized them to vote. Although the campaign in support of the amendment was very large, the Florida Rights Restoration Coalition may have focused on contacting higher income, higher propensity voters, even though these voters were less likely to directly benefit from the passage of the amendment. Further research is needed to understand how and why these communities were not more likely to vote than their counterparts.

Whatever the causal mechanism that resulted in lower turnout for these communities despite Amendment 4, the results point to the next chapter of the fight for political integration and representation for advocates in the Sunshine State. The relatively lower turnout in 2018 for the communities most impacted by the carceral state indicates that formal re-enfranchisement is not enough. If Floridian and American democracy wants to *actually* incorporate voices from these communities — and not simply legally *allow* for their incorporation — the advocacy movement cannot consider its work done once the formal barriers to the ballot box have been torn down. Re-enfranchisement is clearly necessary, but it is not sufficient. Researchers must continue exploring why the political re-incorporation of these communities is so difficult, and advocates on the ground must do the hard work of reknitting them to our body politic.

Appendix A

As discussed in the body of this manuscript, statewide data on the residential addresses of individuals sentenced to felony probation are not available. These data are, however, available in Hillsborough County, the county in Florida with the third-highest number of formerly incarcerated individuals.[13] These records go back to 1988, though we have restricted them to individuals sentenced since October 1, 1997, so that they mirror the incarceration records. We follow the same geocoding and address cleaning procedures as for the incarceration records discussed above. These data do not include unique identifiers. To avoid double-counting, only the most recent record for each unique first name, middle name, last name, and date of birth is retained. This potentially excludes different people whose names and dates of birth are identical. Individuals whose adjudication was withheld are excluded.

Figure 5 plots the relationship between the number of formerly incarcerated residents and residents who have been sentenced to felony probation in each voter precinct in Hillsborough County (scaled by precinct size). As the figure makes clear, individuals who have been sentenced to felony probation are concentrated in the same neighborhoods where individuals live after a period of incarceration (the R^2 of the bivariate regression is 0.89). As with the marginal effects plots in the body of this manuscript, the figure does not show outlier neighborhoods but the line of best fit and R^2 are calculated using all observations

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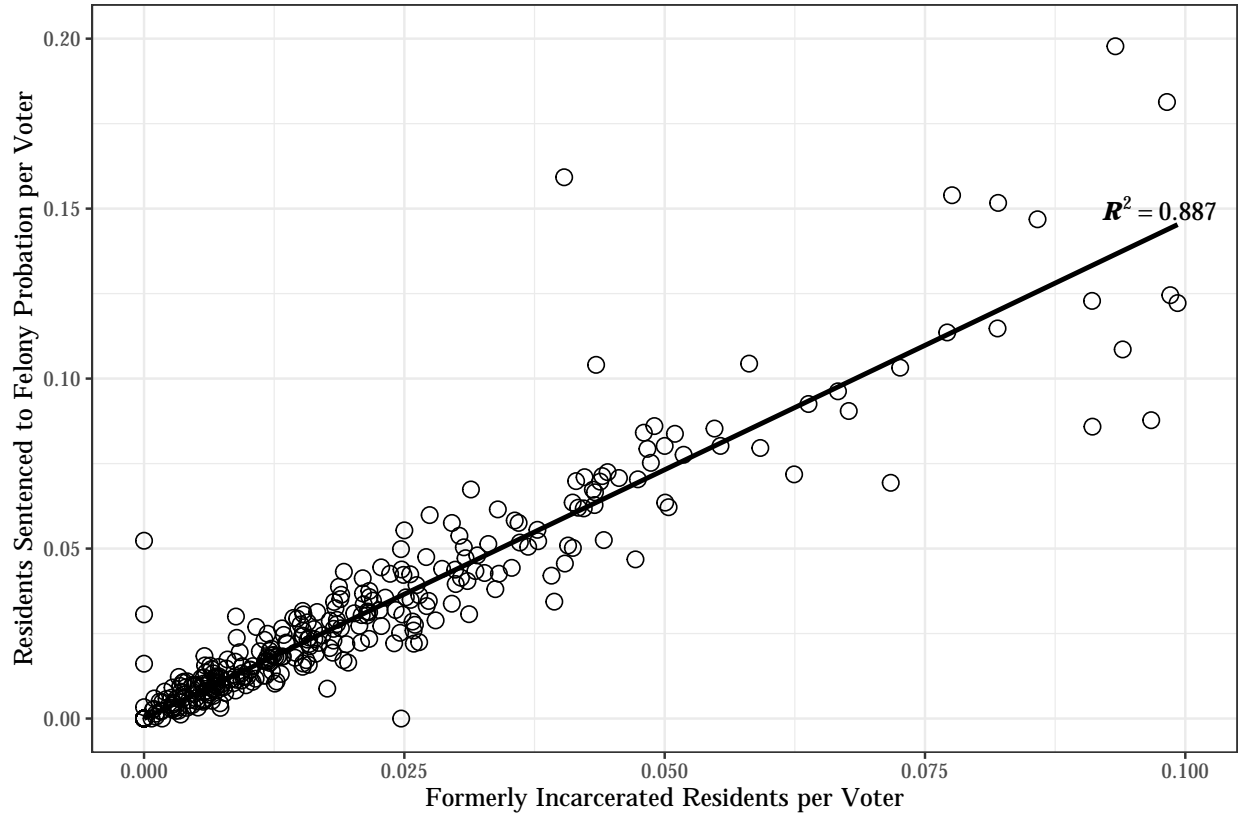[13]See https://www.hillsclerk.com/Records-and-Reports/Public-Data-Files.

Figure 5: Relationship Between Formerly Incarcerated and Probationed Residents, Hillsborough County

Table 8 replicates the models from Tables 3 and 4 in the main body of this manuscript. In each pair of models in the table, I begin by re-fitting the exact models presented in the body of this manuscript but limiting the precincts and block groups to Hillsborough County. In the second model in each pair, the primary dependent variable includes both formerly incarcerated residents *and* the number of residents who have been convicted of a felony probation.

Table 8: Neighborhood Turnout, Support for Am. 4, and Roll-Off in 2018

	Precinct-Level Turnout		Block Group-Level Turnout		Am. 4 Support		Roll-off	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Formerly Incarcerated Residents	0.00002		−0.0003***		−0.00003		−0.00005***	
	(0.00004)		(0.00002)		(0.00004)		(0.00001)	
Total Disenfranchised Individuals		−0.00000		−0.0001***		−0.00001		−0.00002***
		(0.00001)		(0.00001)		(0.00001)		(0.00000)
Percent White	−0.528*	−0.514	0.015	0.015	0.124	0.114	0.029	0.025
	(0.316)	(0.323)	(0.011)	(0.011)	(0.491)	(0.490)	(0.039)	(0.038)
Percent Black	−0.690***	−0.669***	0.005	0.006	0.122	0.107	0.012	0.004
	(0.227)	(0.239)	(0.005)	(0.005)	(0.442)	(0.441)	(0.071)	(0.070)
Percent Latino	−0.721**	−0.708**	−0.033***	−0.032***	−0.043	−0.052	0.017	0.012
	(0.296)	(0.301)	(0.010)	(0.010)	(0.442)	(0.442)	(0.036)	(0.036)
Percent Asian	−0.560	−0.547	0.049*	0.048*	−0.076	−0.085	0.098	0.092
	(0.408)	(0.412)	(0.027)	(0.028)	(0.543)	(0.543)	(0.077)	(0.077)
Percent Male	0.386	0.370	0.225***	0.233***	−0.174	−0.162	−0.149**	−0.142**
	(0.343)	(0.351)	(0.042)	(0.044)	(0.315)	(0.318)	(0.061)	(0.062)
Percent Democrats	0.497***	0.499***	0.030	0.026	0.121	0.120	0.155	0.155
	(0.121)	(0.121)	(0.084)	(0.081)	(0.165)	(0.166)	(0.145)	(0.147)
Percent Republicans	0.395***	0.398***	−0.030	−0.035	−0.851***	−0.853***	0.142	0.140
	(0.076)	(0.077)	(0.059)	(0.057)	(0.077)	(0.079)	(0.122)	(0.123)
Average Age	−0.003	−0.003	−0.001	−0.001	0.001	0.001	0.002***	0.002***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Average Income ($10,000s)	0.006***	0.006***	0.001	0.001	−0.0004	−0.0004	0.0003	0.0003
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0005)	(0.0005)
Percent With Some College	0.127***	0.124***	0.075***	0.072***	0.088*	0.090*	−0.009	−0.009
	(0.011)	(0.010)	(0.013)	(0.013)	(0.047)	(0.047)	(0.014)	(0.013)
Percent Unemployed	−0.175***	−0.170***	−0.007	−0.004	−0.117*	−0.120*	0.064	0.064
	(0.029)	(0.029)	(0.020)	(0.020)	(0.066)	(0.066)	(0.040)	(0.041)
Constant	−0.024	−0.020	−0.197*	−0.194**	0.883***	0.880***	−0.051	−0.052
	(0.116)	(0.119)	(0.102)	(0.099)	(0.169)	(0.169)	(0.037)	(0.036)
Congressional District FEs	X	X	X	X	X	X	X	X
Turnout in 2010 – 2016	X	X	X	X	X	X	X	X
Observations	390	390	818	818	390	390	390	390
R²	0.881	0.881	0.981	0.982	0.944	0.944	0.483	0.482
Adjusted R²	0.875	0.874	0.981	0.981	0.941	0.941	0.455	0.454

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.
Robust standard errors (clustered by congressional district) in parentheses.

The relationship between disenfranchised residents and precinct-level support for Amendment 4, and precinct-level turnout, are nonsignificant in Table 8 despite being significant statewide. Block group-level turnout and roll-off remain negatively associated with the presence of disenfranchised individuals. Importantly, in no model does moving from measuring only formerly incarcerated individuals to measuring all disenfranchised individuals change

the sign on a statistically significant relationship. This provides corroboration for the argument that the neighborhood-level results presented in the body of this manuscript, measured using only formerly incarcerated residents, apply to the formerly disenfranchised population more generally.

I next interrogate whether the use of only incarceration records is likely impacting the individual-level analyses presented in the body of the manuscript. I begin by re-estimating Models 1 – 4 from Table 6, limiting the pool to treated voters who live in Hillsborough County and their matches. Re-estimating the model in Hillsborough County allows us to observe how often "unidentified" treated voters serve as controls in this manuscript's primary analysis, at least in Hillsborough County. Approximately 6.8 percent of the controls for Hillsborough County in the primary analysis are actually household members of individuals disenfranchised due to a term of felony probation. Because voters who are in fact treated serve as controls for some registered voters living with formerly incarcerated individuals, this manuscript likely understates the effect of living with a disenfranchised individual.

I then re-run the matching procedure described above, where a registered voter is considered treated if they lived with any disenfranchised individual. Potential controls for this matching procedure are limited to Hillsborough County, where we can be sure registered voters do not live with individuals sentenced to felony probation. The matching procedure is successful at reducing differences between treated and control voters in Hillsborough County.

In Table 9, models 1 – 4 re-estimate models 1 – 4 from Table 6. Models 5 – 8 present the results using the broader treatment definition.

Table 9: General Election Turnout, 2010 − 2018

	Lives with Formerly Incarcerated				Lives with Disenfranchised			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D(2018)	0.096***	0.096***	0.098***	0.098***	0.103***	0.103***	0.105***	0.105***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
D(Treated)	−0.062***	−0.063***	−0.074***	−0.074***	−0.064***	−0.065***	−0.078***	−0.078***
	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)	(0.002)
Years Since Latest Incarceration			0.001*	0.0004*			0.001***	0.0004***
			(0.0003)	(0.0002)			(0.0002)	(0.0001)
D(2018) × D(Treated)	−0.024***	−0.024***	−0.040***	−0.040***	−0.031***	−0.031***	−0.051***	−0.051***
	(0.003)	(0.003)	(0.005)	(0.005)	(0.002)	(0.002)	(0.003)	(0.003)
D(2018) × Years Since			−0.0003	−0.0003			−0.0002	−0.0002
			(0.0002)	(0.0002)			(0.0002)	(0.0002)
D(Treated) × Years Since			0.001***	0.001***			0.002***	0.002***
			(0.0004)	(0.0004)			(0.0003)	(0.0003)
D(2018) × D(Treated) × Years Since			0.002***	0.002***			0.003***	0.003***
			(0.001)	(0.001)			(0.0004)	(0.0004)
Constant	0.447***	0.049*	0.442***	0.047*	0.439***	0.066***	0.431***	0.064***
	(0.002)	(0.026)	(0.003)	(0.026)	(0.001)	(0.020)	(0.002)	(0.019)
Includes covariates from matching		X		X		X		X
Congressional District fixed effects		X		X		X		X
Observations	659,340	659,340	659,340	659,340	1,237,080	1,237,080	1,237,080	1,237,080
R^2	0.009	0.215	0.010	0.215	0.010	0.210	0.011	0.211
Adjusted R^2	0.009	0.215	0.010	0.215	0.010	0.210	0.011	0.211

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.
Robust standard errors (clustered at level of match) in parentheses.

In Hillsborough County, the magnitude of the treatment effect grows when we broaden the treatment group to include anyone who lives with a formerly disenfranchised individual. This raises interesting questions about the potential differential spillover effects of living with a formerly incarcerated individual versus with an individual sentenced to felony probation. This may also be due to some housemates of probationed individuals serving as controls in the main analysis, collapsing the distinction between treated and control and producing conservative estimates. Nonetheless, Table 9 provides evidence that the negative treatment effects identified among voters living with formerly incarcerated individuals in the body of this manuscript are likely generalizeable to all voters living with disenfranchised individuals.

Appendix B

When discussing the impact of formerly incarcerated residents on neighborhood turnout and support for Amendment 4 in the body of this paper, I include only a subset of formerly incarcerated residents. I exclude individuals who returned from prison to institutions listed by four or more other formerly incarcerated individuals. I choose to exclude these individuals because I am most interested in the relationship between Amendment 4 and the turnout of individuals in proximal contact with the criminal justice system. Walker and García-Castañon (2017) defines proximal contact "as having a loved one who is a custodial citizen without yourself having had contact" (542). Because much of the literature focuses on the mechanisms linking personal relationships, proximal contact, and political participation, I limit the sample to formerly incarcerated individuals who are likely returning to neighborhoods with social and familial ties.

Nevertheless, living in a neighborhood with a large number of formerly incarcerated individuals who reside in institutions like half-way houses or shelters might structure voting behavior. I begin this appendix by re-estimating the models presented in Tables 3 and 4 in the body of this paper, but now including *all* formerly incarcerated residents. Table 10 presents the results of these estimations. Model 1 presents the turnout regression estimated at the block group level, while Models 2 – 4 are estimated using precinct level data.

Table 10: Including All Formerly Incarcerated Residents

	Block Group	Precinct		
	Turnout	Turnout	Support for Am. 4	Roll-Off
	(1)	(2)	(3)	(4)
Formerly Incarcerated Residents	-0.0001^{***}	-0.00004^{***}	0.00003^{***}	-0.00001^{***}
	(0.00001)	(0.00001)	(0.00001)	(0.00000)
Percent White	0.020^{**}	0.004	0.072^{*}	-0.074^{***}
	(0.008)	(0.036)	(0.041)	(0.015)
Percent Black	0.040^{***}	-0.005	0.196^{***}	-0.049^{***}
	(0.008)	(0.036)	(0.041)	(0.015)
Percent Latino	-0.005	-0.091^{**}	0.052	-0.052^{***}
	(0.008)	(0.036)	(0.041)	(0.015)
Percent Asian	0.046^{***}	0.092^{*}	0.243^{***}	-0.099^{***}
	(0.011)	(0.052)	(0.059)	(0.021)
Percent Male	0.092^{***}	0.319^{***}	-0.389^{***}	-0.200^{***}
	(0.023)	(0.055)	(0.063)	(0.023)
Percent Democrats	0.063^{***}	0.067^{***}	0.191^{***}	0.033^{***}
	(0.008)	(0.020)	(0.023)	(0.008)
Percent Republicans	0.006	0.023	-0.397^{***}	0.041^{***}
	(0.008)	(0.019)	(0.021)	(0.008)
Average Age	0.001^{***}	0.00005	-0.0003	0.001^{***}
	(0.0001)	(0.0002)	(0.0002)	(0.0001)
Average Income ($10,000s)	0.002^{***}	0.002^{***}	-0.003^{***}	-0.00002
	(0.0001)	(0.0004)	(0.0004)	(0.0002)
Percent With Some College	0.086^{***}	0.196^{***}	0.151^{***}	-0.027^{***}
	(0.003)	(0.008)	(0.010)	(0.003)
Percent Unemployed	-0.006	-0.039^{**}	-0.014	-0.020^{***}
	(0.005)	(0.018)	(0.021)	(0.007)
Constant	-0.189^{***}	-0.236^{***}	1.030^{***}	0.216^{***}
	(0.023)	(0.049)	(0.056)	(0.020)
Congressional District FEs	X	X	X	X
Turnout in $2010-2016$	X	X	X	X
Observations	10,817	5,797	5,797	5,797
R^2	0.979	0.779	0.788	0.312
Adjusted R^2	0.979	0.777	0.786	0.307

$^{***}p<0.01,$ $^{**}p<0.05,$ $^{*}p<0.1.$
Robust standard errors (clustered by congressional district) in
parentheses.

The inclusion of all formerly incarcerated residents substantially shrinks the size of the estimated coefficients of interest with respect to the estimates presented in the body of the manuscript Nevertheless, turnout (measured at the block group and precinct level) and roll-off are significantly and negatively related with the formerly incarcerated population in a neighborhood, and support for Amendment 4 remains positively (and significantly) related. It appears, then, that formerly incarcerated residents who return to institutions have smaller spillover effects on their neighbors' voting behavior.

The body of the manuscript also acknowledges that the use of release-address data may be unreliable considering the fact that many individuals may have moved or died since their discharge from parole. This is especially possible for individuals who have not had contact with the state incarceration agency for many years. To account for this possibility, Table 11 re-estimates the models presented in Tables 3 and 4, but limits the formerly incarcerated individuals to those residents who were last released from prison between 2015 and the 2018 election. These individuals are the least likely to have died or moved, simply because their information is the most recent. These models include only individuals who returned to non-institutions, as presented in the body of the manuscript.

Table 11: Formerly Incarcerated Residents Released Since 1/1/2015

| | Block Group | Precinct | | |
| | Turnout | Turnout | Support for Am. 4 | Roll-Off |
	(1)	(2)	(3)	(4)
Formerly Incarcerated Residents	−0.001***	−0.001***	0.0002***	−0.0001***
	(0.0001)	(0.0001)	(0.0001)	(0.00002)
Percent White	0.019**	−0.142***	−0.024	−0.028**
	(0.009)	(0.035)	(0.033)	(0.014)
Percent Black	0.040***	−0.131***	0.069**	−0.011
	(0.009)	(0.035)	(0.033)	(0.014)
Percent Latino	−0.007	−0.238***	−0.083**	−0.005
	(0.009)	(0.034)	(0.033)	(0.014)
Percent Asian	0.045***	−0.096	0.150**	−0.012
	(0.012)	(0.062)	(0.059)	(0.025)
Percent Male	0.041	0.392***	−0.285***	−0.155***
	(0.026)	(0.059)	(0.056)	(0.024)
Percent Democrats	0.073***	0.182***	0.088***	0.043***
	(0.009)	(0.022)	(0.021)	(0.009)
Percent Republicans	0.006	0.118***	−0.533***	0.043***
	(0.009)	(0.021)	(0.020)	(0.008)
Average Age	0.001***	0.0003*	0.0002	0.001***
	(0.0001)	(0.0002)	(0.0002)	(0.0001)
Average Income ($10,000s)	0.002***	0.002***	−0.002***	−0.0001
	(0.0002)	(0.0004)	(0.0004)	(0.0002)
Percent With Some College	0.081***	0.163***	0.161***	−0.030***
	(0.003)	(0.008)	(0.007)	(0.003)
Percent Unemployed	0.0001	−0.028*	−0.040***	−0.0002
	(0.005)	(0.016)	(0.015)	(0.006)
Constant	−0.148***	−0.268***	1.104***	0.114***
	(0.026)	(0.053)	(0.050)	(0.021)
Congressional District FEs	X	X	X	X
Turnout in 2010 − 2016	X	X	X	X
Observations	8,967	4,905	4,905	4,905
R^2	0.979	0.839	0.897	0.407
Adjusted R^2	0.979	0.837	0.896	0.401

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Robust standard errors (clustered by congressional district) in
parentheses.

In each of the models presented in Table 11, the independent variable of interest is statistically significant at the 99 percent level. Moreover, the estimated coefficient is in each case larger than that presented in the body of the manuscript. This could be because using more recent data better identifies communities that are currently home, not just historically home, to formerly incarcerated individuals. On the other hand, a community member's incarceration may be more salient in places where residents were more recently incarcerated. Proximal contact, in other words, might shape voters' behavior more strongly if that contact was recent. The individual-level difference-in-differences regressions presented later in the paper would seem to corroborate this as well.

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