Turnout and Amendment 4[*]

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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 transformational for Floridian — and American — democracy (Morris 2018); 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 for 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, precinct-level data from the Florida Division of Elections shows that 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, implying that the constitutional amendment enjoyed substantial bipartisan support.

Felony disenfranchisement is not unique to Florida's electoral system: in all but two states in the United States, individuals convicted of felony offenses lose their right to vote for at least some period of time (Justice 2019). According to the Brennan Center for Justice, felony convictions result in lifetime prohibitions from voting for some citizens in 11 states based on the severity of the crime.[1] Nevertheless, the shift in Florida from permanent disenfranchisement for all individuals convicted of felony offenses to restoring voting rights to most individuals[2] upon the termination of their sentence moved the needle significantly: post-sentence Floridians accounted for roughly 24 percent of all disenfranchised Americans as of 2016 (Uggen, Larson, and Shannon 2016).

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[1]Iowa stands alone in disenfranchising all individuals ever convicted of felony offenses.
[2]Individuals convicted of felony sexual assault or murder remain permanently disenfranchised

The case of Amendment 4 was somewhat unique in how Floridians with felony convictions in their past had their voting rights restored. Usually, the question of voting rights restoration is not put directly to the voters. In New York State in 2018 and Kentucky in 2019, for instance, governors signed executive order restoring voting rights to some disenfranchised individuals (Wang 2018; Wines 2019). In other states such as Louisiana (Crisp 2019) and Colorado (Baumann 2019), state lawmakers have passed bills restoring voting rights to formerly incarcerated individuals. Florida, however, put the question was put directly to voters. In the Sunshine State, voters had the opportunity to democratically decide whether they wanted to restore voting rights to their formerly incarcerated or probationed neighbors.

This paper explores whether the direct democratic approach of Amendment 4 increased participation among individuals who live in close proximity to the (formerly) disenfranchised. There is much literature that establishes that proximal contact with the criminal justice system reduces eligible Americans' propensity to vote (e.g. Ochs 2006; Bowers and Preuhs 2009; Burch 2013; King and Erickson 2016). Little work, however, has been done exploring whether the re-enfranchisement of 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. I investigate whether turnout in neighborhoods and household impacted by felony disenfranchisement were more likely to vote in 2018 than less-impacted groups and relative to their own participation history.

Theory and Literature

Proximal Contact and Political Participation

Over the past two decades, the literature has grappled with the relationship between the criminal justice system, felony disenfranchisement, and voter turnout. Following the 2000

presidential election, Uggen and Manza (2002) argued that George W. Bush won the state of Florida, and ultimately the presidency, thanks to felony disenfranchisement laws. Their analysis raised important questions about the representational effects of felony disenfranchisement rules. Subsequent research has built on the work of Uggen and Manza and sought to uncover the causal effects of incarceration and contact with the criminal justice system on voter turnout. Scholars have explored the effect of incarceration on the voting habits of those directly impacted post-incarceration (e.g. Meredith and Morse 2013; Gerber et al. 2017; White 2019b) as well as the effect of proximal contact with the criminal justice system on turnout among eligible citizens (e.g. Bowers and Preuhs 2009; Miles 2004; Ochs 2006; White 2019a).

The evidence that felony disenfranchisement regimes reduce the turnout of eligible voters is decidedly mixed. Some scholars have uncovered large "spillover" effects, while others have found little evidence to support that notion. In many cases, available data has inhibited the investigation of felony disenfranchisements' effects at the substate level. Some of this state-level scholarship finds depressive effects associated with disenfranchisement laws. Bowers and Preuhs (2009), for instance, finds that Black turnout is lower in states with stricter felony disenfranchisement rules. King and Erickson (2016) similarly finds that states with larged disenfranchised populations saw depressed turnout among African Americans in 2004. Miles (2004), however, leverages a triple-differences approach and argues that the depressive effects of disenfranchisement are much lower than other research indicates.

Research that has explored the effects of proximal contact on turnout at lower geographic levels has similarly returned mixed results. Burch (2013), for instance, investigates the relationship between levels of incarceration and voter turnout at the Census block group level in North Carolina. She finds a strong relationship between the number of individuals under state supervision on the date of the 2008 election and block group turnout, arguing that "at high concentrations, imprisonment and community supervision have an unequivocally demobilizing effect on neighborhoods" (185). Similarly, Lerman and Weaver (2014) finds

that all manner of individual-level contact with the criminal justice system reduces voter turnout.

White (2019a), on the other hand, leverages administrative data to explore the effect of incarceration on the turnout of eligible household members. White matches reported addresses in court records from Harris County, Texas, to the Texas registered voter file to identify voters who lived with individuals who went to court on misdemeanor charges. Ultimately, White (2019a) finds "find no evidence of a medium- or long-run effect on turnout among registered voters who see their household member entangled with misdemeanor courts" (612).

At the state, neighborhood, and individual level, the effects of proximal contact with the criminal justice system on voter turnout is unclear. A growing body of research, however, indicates that proximal contact can mobilize non-voting political participation. Walker (2014), for instance, shows that proximal contact with individuals caught up in the criminal justice system increases an individual's likelihood of taking such actions as signing a petition, taking part in a demonstration, or attending a political meeting — but that proximal contact has no effect on voter turnout. Walker and García-Castañon (2017) and Walker (2020) corroborate these results, and demonstrate that minority women and individuals who perceive the criminal justice system as unjust are among the most mobilized.

Given that the literature has generally found that living in high-contact communities reduces electoral turnout, but that proximal contact increases other forms of participation, the theoretical effect of Amendment 4 is unclear and worthy of study. On the one hand, the general election of 2018 looked very much like the sort of contest studied in the voter-turnout literature cited above: Florida had competitive gubernatorial and senatorial elections, in addition to congressional contests in most districts. On the other hand, Amendment 4 could be considered more similar to the non-voting participation discussed in Walker's work: the amendment chose no candidate, but rather proposed to change laws directly to re-enfranchise community members. This literature indicates that *in the absence* of Amendment 4, prox-

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imal contact would have been associated with lower turnout, but that Amendment 4 may have resulted in increased turnout in 2018 among the most directly impacted voters.

Electoral Reform and Self-Interested Voting

At first blush, it would seem paradoxical that any voter might choose to expand the electorate. The likelihood of an individual voter casting the deciding ballot, after all, decreases as the size of the electorate increases. An expansion of the electorate, therefore, dilutes the power of each individual vote.

Such an overly rational approach to understanding support for the expansion of the franchise, however, ignores the contemporary context of electoral reform in the United States. Over the past generation, nearly every state in the nation has proposed restrictive voting laws. These restrictive laws demand that voters provide photo identification, shorten early voting periods, and restrict the types of organizations that can register citizens to vote (see, among others, Bentele and O'Brien 2013). Although often cloaked under the veil of preventing voter fraud, scholarly research from the past decade has demonstrated that restrictive voting laws are passed in predictable electoral contexts. Hicks et al. (2015), for instance, finds that legislatures in states with larger non-white populations are more likely to introduce restrictive voter ID bills, as are states with more competitive elections. They find that states that are dominated by Republican legislators are more likely to enact these voter ID laws, and that this is especially true in the most competitive electoral contexts. "The Republican party," Hicks et al. (2015) notes, "has proved incapable of expanding its appeal among the… growing minority electorate." They continue: "Faced with this reality, the GOP appears to have opted for coalition maintenance instead of coalition expansion, by embracing several restrictive voting reforms whose true purpose is to marginally curtail the participation of voters typically aligned with the Democratic Party."

The shaping of the electorate in the twenty-first century seems, then, to be largely driven by

partisan interest. In the face of a growing non-white electorate the Republican party erected barriers to the ballot box, hoping to impose marginal costs that demobilize certain classes of voters among whom Democrats are over-represented. Given this context, it is likely that voters traditionally aligned with the Democratic party would support an expansion of the electorate via the re-enfranchisement of individuals with felony convictions in their past. This is especially true given the particular partisan alignment of individuals who are convicted of felony offenses: they and their communities tend to overwhelmingly support the Democratic party [CITE].

Because the communities most impacted by systems of policing and incarceration generally support Democrats, and the Democratic Party has become associated with the expansion of the electorate, I expect that neighborhoods that are home to individuals who have completed felony sentences supported Amendment 4 at high rates. I expect, however, that their support for the passage of the amendment exceeds what might be expected by simply considering their partisan make up because their communities were likely to gain electoral influence from the amendment. There is some evidence (see, for instance, Kinsella, McTague, and Raleigh 2015) that neighborhoods have identifiable political preferences; a voter who votes for the reenfranchisement of her neighbor, therefore, is relatively likely to be enfranchising someone who will vote for similar candidates and support similar initiatives. Voting to reenfranchise a population that, as a whole, shares my political preferences is highly rational — and perhaps even mobilizing. This is especially true given research on the distribution of resources by politicians. Martin (2003), for example, demonstrates that federal representatives direct more resources to parts of their districts with higher turnout. Insofar as reenfranchisement disproportionately benefits certain areas, those areas are poised to increase their shares of federal funding. Supporting the re-extension of voting rights to members of one's own community, therefore, might result in real benefits even for individuals who have never been convicted of a felony.

This research firmly establishes that neighborhoods with disenfranchised residents were likely

to support the passage of Amendment 4; it says less, however, about the effect the presence of the amendment on the ballot might have had on turnout relative to other communities. Although the passage of the amendment would likely accrue benefits to the neighborhoods where disenfranchised citizens lived, it might also *reduce* the relative political representation of other neighborhoods, thereby mobilizing all voters equally. Moreover, Amendment 4 was a highly salient social-issue related initiative, which Biggers (2011) and Dvořák, Zouhar, and Novák (2017) indicate increases turnout generally. However, the benefits of Amendment 4 were concentrated relative to the diffuse representational costs, which Olson (2003) indicates might increase turnout more in the neighborhoods home to potentially reenfranchised voters.

Research Design and Expectations

This paper sets out to test the combined magnitude of two separate treatments. Based on previous literature, I expect that proximal contact with disenfranchised individuals decreases individuals' and neighborhoods' propensity to vote. At the same time, the presence of Amendment 4 on the ballot in Florida in 2018 offered these same communities the opportunity to increase their relative power in the electorate, which should theoretically increase their propensity to vote. A negative relationship between proximal contact and turnout in 2018 will therefore imply that the demobilizing proximal contact with the carceral state was not overcome by the political opportunity offered by Amendment 4; a positive effect, on the other hand, will indicate that Amendment 4 sufficiently mobilized communities to overcome the depressive effect of felony disenfranchisement. A null finding will be harder to interpret; whether it implies that neither treatment had any effect, or that the two treatments cancelled one another out, will require some discernment.

I begin by testing whether the number of formerly incarcerated individuals living in a neighborhood influenced that neighborhood's turnout in 2018. Neighborhoods that are home to formerly incarcerated individuals are identified by geocoding release records from the Florida

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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 results at this level, we 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. This is an important distinction: voters are more likely to vote for the top-line contests for Governor and United States Senator, and "roll-off" (that is, abstain from participating) when it comes to downballot questions such as ballot initiatives [CITE]. 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. Precinct-level data, therefore, allows us to determine not how many voters in a neighborhood voted for *any* contest, but specifically for Amendment 4. Precinct-level data also allows us to tell the extent to which neighborhoods supported Amendment 4. The secret ballot, of course, prevents us from knowing how individuals voted; precinct-level data, therefore, offer the lowest-level picture of what sorts of communities were more or less likely to support Amendment 4.

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 neighborhood, 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 neighborhoods where disenfranchised individuals lived, expecting these newly registered voters to support the amendment. 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* voters, but lower turnout among *registered* voters.

To address this potential problem, I also define neighborhoods as Census block groups. The Census 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. Thus, 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.

The use of neighborhood-level data allows me to test my first set of hypotheses:

Hypothesis 1a: Each additional formerly incarcerated resident in a voting precinct will increase turnout among registered voters in that precinct, other things being equal.

Hypothesis 1b: Each additional formerly incarcerated resident in a Census block group will increase turnout among eligible citizens in that block group, other things being equal.

Hypothesis 2: Each additional formerly incarcerated resident in a voting precinct will increase support for Amendment 4 in that precinct, other things being equal.

Hypothesis 3: Each additional formerly incarcerated resident in a voting precinct will decrease roll-off in that precinct, other things being equal.

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 addresses provided by 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). 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 analysis 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, as the depressive effect will be incorporated into the pre-treatment baseline.

This gives rise to my final hypothesis:

Hypothesis 4: Amendment 4 led to 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.

Data

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

Department of Corrections Data

Data from the Florida Department of Corrections is used to identify individuals who have been to prison in Florida. These records include all individuals released from prison since October 1, 1997.[3] Using records of individuals currently in prison or on parole, I can identify all individuals who had been to prison but were not serving a sentence at the time of the 2018

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[3]Using the last-known address for individuals last released from prison in 1997 presents some difficulty; it is possible that the formerly incarcerated individuals have died or moved. In Appendix A, I show that the results presented in the body of this article continue to hold even when limiting the pool of formerly incarcerated people to individuals released from prison during or after 2015.

midterm election. These are the individuals who would be re-enfranchsied by Amendment 4, and therefore the people most likely to encourage their friends and neighbors to vote on their behalf. I include only individuals with a valid release; individuals who died or absconded before their sentence was completed are removed from the dataset. I identify 342,694 such individuals.

The Florida Department of Corrections provides the last-known addresses of all individuals who were formerly incarcerated. The address data, however, are messy. In some cases, the address field is left blank; in others, the records simply notes the road or the town of the former inmate, without providing full address information. These addresses require substantial cleaning.

I begin the cleaning process by removing all records where the address does not begin with an integer. In other 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). I then geocode these addresses using Google Maps' geocoder, and drop all individuals whose addresses were geocoded outside of Florida (10.6 percent) or could not be geocoded[4] (3.1 percent). The resulting list includes 285,966 individuals, or 83.4 percent of the formerly incarcerated individuals with valid releases. After removing the individuals who moved out of the state, this means that at least 94 percent of individuals released to addresses in Florida were successfully geocoded. Although not 100 percent, 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. The body of this paper 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 community member. Appendix B demonstrates that

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[4]An address is considered successfully geocoded if Google determines that it has achieved a "range interpolated" or "rooftop" level of accuracy. "Approximate" or "geometric center" geocodes are considered failures.

the primary findings in the paper hold when we include all formerly incarcerated individuals, even if they returned to the same location as many other formerly incarcerated individuals.

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 data collected by Kelso and Migurski (2018).

Statewide data on individuals who formerly served a term on felony probation are not available. This too may pose a problem for this study; neighborhoods with disenfranchised former probationers are also "treated." Such data is available, however from COUNTY COUNTY. In Appendix C, I incorporate these data to interrogate whether the omission of these individuals from the statewide analyses is likely to impact my results.

Voter File 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. 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-identificated 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 ACS 5 year estimates ending with 2018; the precinct average income, therefore, is effectively the average of all the block groups within that precinct, weighted by the number of registered voters in each block group.

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

Division of Election Results Data

As discussed above, the Florida Division of Elections makes precinct-level results data available. These data come with precinct identifiers that correspond to the precinct reported in the Florida voter file.

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 un-

helpful: 97.2 percent of block groups in the state are home to someone who has been to prison, lending credence to the notion that the criminal justice system impacts nearly every neighborhood in the Sunshine State. There are, however, neighborhoods where formerly incarcerated individuals are concentrated. In Column 1 of Table 1, 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 1: Neighborhood Demographics

Measure	Average Neighborhood	Average Neighborhood for Formerly Incarcerated
Median Income*	$59,988	$45,538
Median Age*	42.5	40.1
% Unemployed*	6.4%	8.7%
% with Some College*	73.0%	65.7%
% Non-Hispanic White*	54.4%	45.5%
% Non-Hispanic Black*	15.4%	29.9%
% Latino*	25.2%	20.3%

* 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 1 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 other similar neighborhoods. To test this relationship I run ordinary least squares regressions, where precinct- and block group-level turnout 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.[5] 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 ACVAP by subtracting the number of all formerly incarcerated individuals from the Census Bureau's estimated citizen voting age population (including the individuals who are excluded from the primary independent variable count because they returned to common post-release residences). The number of formerly incarcerated residents is the primary independent variable.

Table 2 presents the results of these regression. In addition to the number of formerly incarcerated residents, I control for the racial, gender, and partisan composition of the neighborhood, as well as turnout rates from the past few elections. Historical turnout is calculated using the registered voter file. I also control for the average age of the neighborhood, as well as the median income, share with some collegiate education, and unemployment rate. Finally, fixed effects for congressional districts are included, and robust standard errors are clustered at this level.[6]

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[5]Roughly 0.6 percent of precincts reported turnout of greater than 100 percent; these 35 precincts have been dropped from the analysis.

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

Table 2: Neighborhood Turnout in 2018

	Precinct-Level	Block Group-Level
	(1)	(2)
Formerly Incarcerated Residents	−0.0002***	−0.0002***
	(0.00004)	(0.00004)
Percent White	0.017	0.018
	(0.110)	(0.014)
Percent Black	0.027	0.042**
	(0.109)	(0.017)
Percent Latino	−0.081	−0.007
	(0.116)	(0.016)
Percent Asian	0.082	0.041*
	(0.128)	(0.022)
Percent Male	0.302	0.101
	(0.188)	(0.086)
Percent Democrats	0.059	0.069***
	(0.082)	(0.019)
Percent Republicans	0.015	0.008
	(0.081)	(0.024)
Average Age	0.0001	0.001***
	(0.0005)	(0.0003)
Average Income ($10,000s)	0.002**	0.002***
	(0.001)	(0.0003)
Percent With Some College	0.183***	0.081***
	(0.016)	(0.005)
Percent Unemployed	−0.032	−0.006
	(0.025)	(0.006)
Constant	−0.211*	−0.193**
	(0.114)	(0.084)
Congressional District FEs	X	X
Turnout in 2010 − 2016	X	X
Observations	5,797	10,814
R^2	0.782	0.978
Adjusted R^2	0.781	0.978

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

Table 2 indicates that there is a negative relationship between the number of formerly incarcerated residents in a precinct and the turnout in that precinct among registered voters, disproving my first hypotheses. The coefficient, however, is small and difficult to interpret. Figure 1 shows the marginal effect of each additional disenfranchised resident on precinct-level turnout for Amendment 4. All other covariates are held at their means in this Figure 1. Although the number of disenfranchised residents in a precinct reaches a maximum of 594, there are 300 or fewer disenfranchised residents in 99.2 percent of precincts. 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.

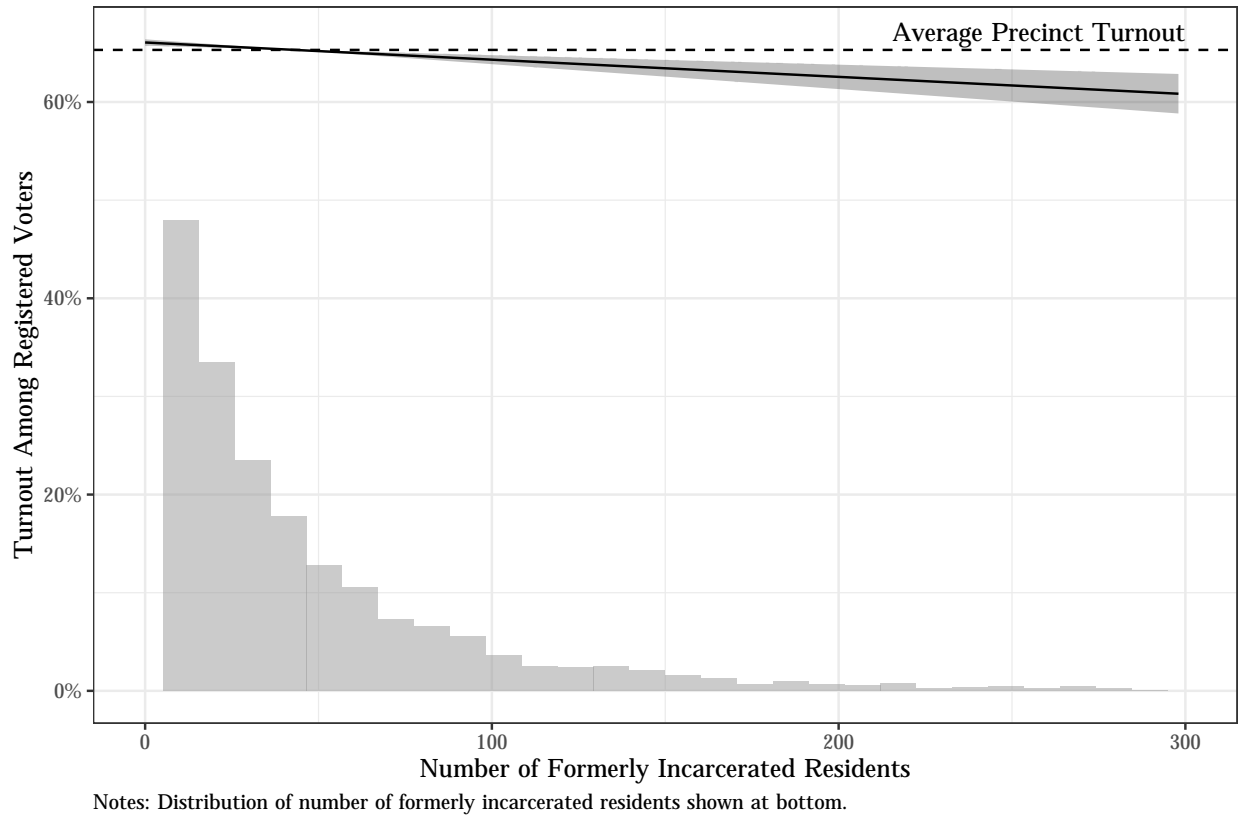Notes: Distribution of number of formerly incarcerated residents shown at bottom.

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

The precinct-level data allows us to test for other relationships between the number of formerly incarcerated residents in a neighborhood and that neighborhood's engagement with Amendment 4. In Table 3 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 3: Precinct Engagement with Amendment 4

	Support for Am. 4	Roll-Off
	(1)	(2)
Formerly Incarcerated Residents	0.0001**	−0.00004***
	(0.00003)	(0.00001)
Percent White	0.069	−0.071*
	(0.122)	(0.042)
Percent Black	0.188*	−0.042
	(0.107)	(0.040)
Percent Latino	0.049	−0.050
	(0.114)	(0.043)
Percent Asian	0.244	−0.101*
	(0.177)	(0.052)
Percent Male	−0.383**	−0.204*
	(0.185)	(0.113)
Percent Democrats	0.192	0.031
	(0.143)	(0.021)
Percent Republicans	−0.396***	0.039*
	(0.120)	(0.020)
Average Age	−0.0003	0.001***
	(0.0004)	(0.0002)
Average Income ($10,000s)	−0.003***	−0.00003
	(0.001)	(0.0002)
Percent With Some College	0.155***	−0.029***
	(0.034)	(0.006)
Percent Unemployed	−0.015	−0.019*
	(0.018)	(0.011)
Constant	1.023***	0.220**
	(0.165)	(0.095)
Congressional District FEs	X	X
Turnout in $2010 - 2016$	X	X
Observations	5,797	5,797
R^2	0.788	0.315
Adjusted R^2	0.787	0.310

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

Table 3 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 these neighborhoods, which means that a higher share of residents expressed a preference on the question. 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. Figures 2 and 3 plot the marginal effect of each additional formerly incarcerated resident on a precinct's support for Amendment 4, and the precinct's roll-off on Amendment 4. 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 rolloff on the question.

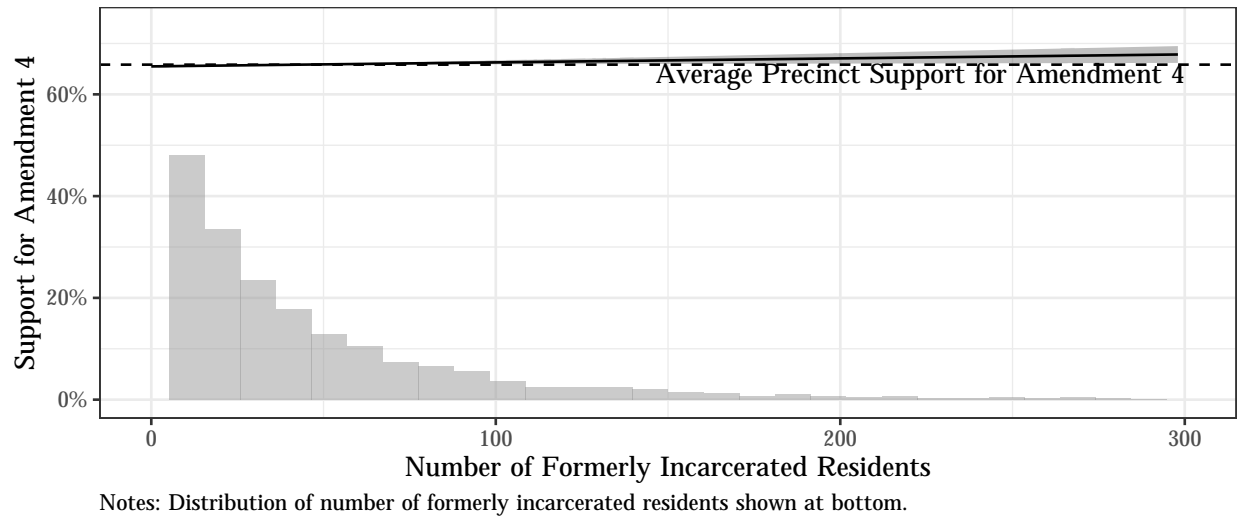Notes: Distribution of number of formerly incarcerated residents shown at bottom.

Figure 2: Marginal Effect of Formerly Incarcerated Residents on Support for Amendment 4
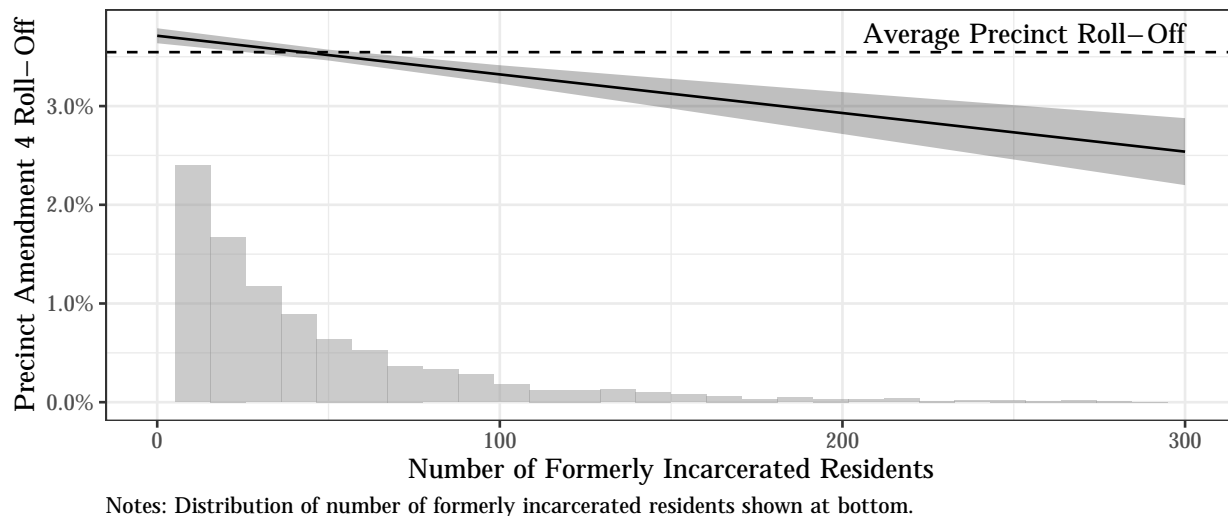
Figure 3: Marginal Effect of Formerly Incarcerated Residents on Amendment 4 Roll-Off

Individual-Level Results

Thus far I have established that neighborhoods with more formerly incarcerated residents turned out at lower rates than other neighborhoods. Although this is in line with past scholarship that has found negative indirect turnout effects from incarceration and felony disenfranchisement, it is sobering to note that even a ballot initiative that would increase these neighborhoods' political representation via the expansion of the franchise was not enough to overcome these negative effects.

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. Examining the relationship between cohabitation with formerly incarcerated individuals and turnout can shed light on the ways in which proximal contact depress turnout. 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

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formerly incarcerated may have had a similar historical response, and yet be more susceptible to mobilization from Amendment 4's placement on the ballot; their family members and housemates, after all, are the individuals who stood to benefit from the passage of the amendment. These formerly incarcerated individuals may have successfully mobilized other members of their households but failed to do so for their neighborhood.

Using individual-level data, I here distinguish the effect of proximal neighborhood contact from individual-level 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" with proximal contact to the carceral state.

Each treated individual is then matched with five untreated registered voters elsewhere in her congressional district.[7] 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. 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 4 presents the results of this matching exercise. In addition to the characteristics reported in Table 4, matches are required to come from the same congressional district to account for different levels of congressional competition in the 2018 election.

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[7]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.

Table 4: Balance Table

	Means: Unmatched Data		Means: Matched Data		Percent Improvement			
	Treated	Control	Treated	Control	Mean Diff	eQQ Med	eQQ Mean	eQQ Max
%White	42.0%	63.0%	42.0%	42.0%	100.00	100.00	100.00	100.00
% Black	39.0%	13.0%	39.0%	39.0%	100.00	100.00	100.00	100.00
% Latino	13.0%	17.0%	13.0%	13.0%	99.86	99.86	99.86	99.86
% Asian	1.0%	2.0%	1.0%	1.0%	100.00	100.00	100.00	100.00
% Female	55.0%	52.0%	55.0%	55.0%	100.00	100.00	100.00	100.00
% Male	42.0%	45.0%	42.0%	42.0%	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	54.0%	37.0%	54.0%	54.0%	100.00	100.00	100.00	100.00
% Republican	21.0%	35.0%	21.0%	21.0%	100.00	100.00	100.00	100.00
% with Some College	66.0%	75.0%	66.0%	67.0%	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 4 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 parallel trends assumption is satisfied: although the treatment group has lower turnout rates in general, the distance between the treatment and control groups is largely constant between 2010 and 2016.
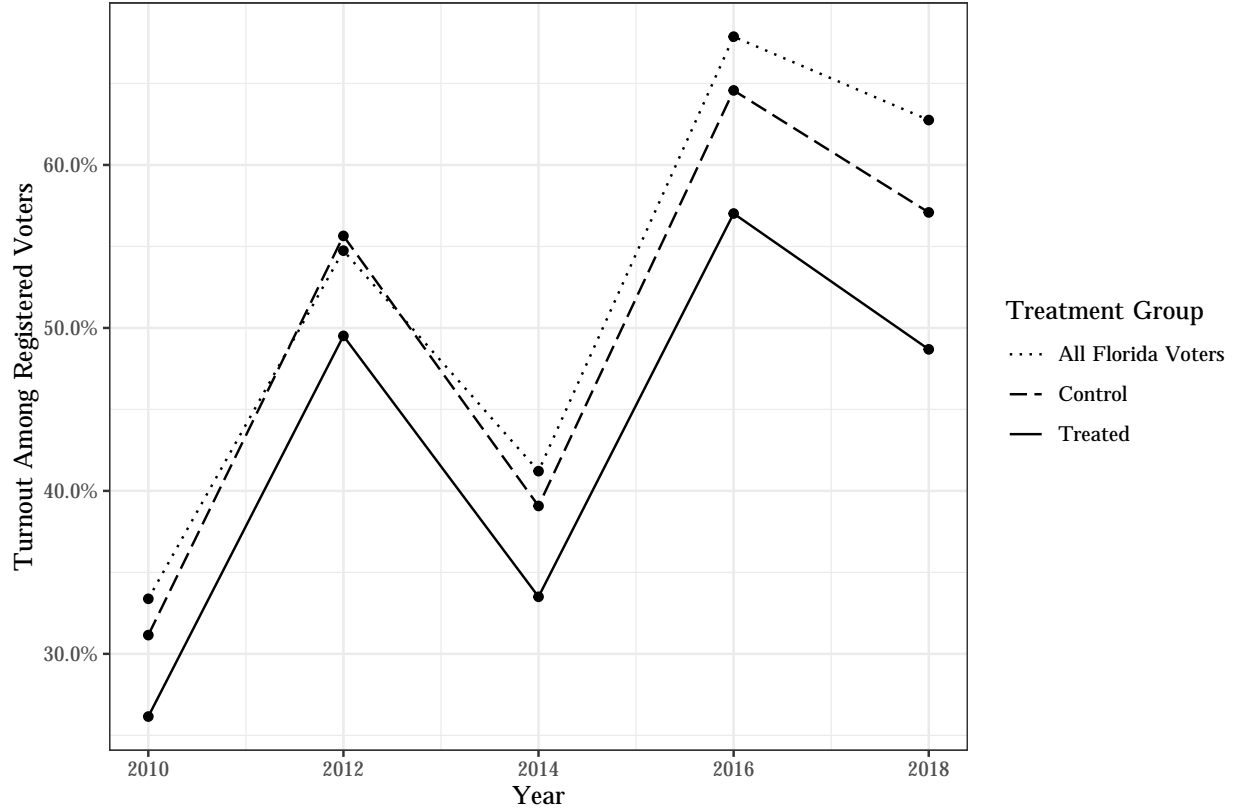
Figure 4: General Election Turnout for Treated and Control Voters, 2010 – 2018

The trends presented in Figure 4 offer preliminary visual corroboration of what I find at the neighborhood level — namely, that proximal contact with formerly incarcerated individuals resulted in lower, not higher, turnout in the 2018 election even after controlling for historical turnout. Table 5 formalizes these trends into a logistic regression. The dependent variable takes the value 0 if the voter did not vote, and 1 if she did. 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), and each control observation is assigned one-fifth the weight of the treatment observations (to account for the five-to-one match). Model 1 presents the model output without the other controls used for matching; Model two includes these covariates.

In Models 3 and 4 of Table 5 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. 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 sure lived with an incarcerated individual. The treatment effects in these models, though slightly smaller, tell the same story.

Table 5: General Election Turnout, 2010 – 2018

	All Matched Observations				Registration Date prior to Discharge			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D(2018)	0.381***	0.470***	0.386***	0.473***	0.243***	0.288***	0.336***	0.397***
	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
D(Treated)	−0.246***	−0.312***	−0.311***	−0.388***	−0.219***	−0.309***	−0.265***	−0.326***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Years Since Latest Incarceration			0.0003*	0.0002			0.054***	0.010***
			(0.0002)	(0.0002)			(0.0002)	(0.0002)
D(2018) × D(Treated)	−0.092***	−0.107***	−0.158***	−0.188***	−0.152***	−0.180***	−0.199***	−0.234***
	(0.003)	(0.003)	(0.005)	(0.005)	(0.004)	(0.004)	(0.006)	(0.006)
D(2018) × Years Since			−0.001*	−0.0004			−0.015***	−0.018***
			(0.0004)	(0.0004)			(0.001)	(0.001)
D(Treated) × Years Since			0.008***	0.010***			0.007***	0.003***
			(0.0002)	(0.0002)			(0.0003)	(0.0003)
D(2018) × D(Treated) × Years Since			0.008***	0.010***			0.008***	0.010***
			(0.0005)	(0.001)			(0.001)	(0.001)
Constant	−0.096***	−2.294***	−0.098***	−2.294***	0.281***	−2.430***	−0.045***	−2.483***
	(0.001)	(0.007)	(0.002)	(0.007)	(0.001)	(0.009)	(0.002)	(0.009)
Includes covariates from matching		X		X		X		X
Congressional District fixed effects		X		X		X		X
Observations	7,559,340	7,559,340	7,559,340	7,559,340	5,020,410	5,020,410	5,020,410	5,020,410
Log Likelihood	−8,641,642.000	−7,297,115.000	−8,638,882.000	−7,294,053.000	−5,735,253.000	−5,026,784.000	−5,655,169.000	−5,024,374.000

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

In each of the specifications presented in Table 5, treated individuals were less likely to vote in 2018 after controlling for both their own vote history and the vote history of their matched counterparts. Exponentiating the coefficients on *D(2018) × D(Treated)* in Models 1 and 2 indicate that turnout among treated voters was between 91.2 and 89.9 percent of what would have been expected if 2018 had conformed to prior years.

The models that incorporate information about how long their housemates had been out of prison indicate, however, that this effect is moderated by time: individuals who lived with housemates who had not been imprisoned in many years were more likely to vote in 2018 than other voters. The coefficients on *D(2018) × D(Treated) × Years Since* in models 3 and 4 estimate that the turnout of individuals whose household member returned from prison within one year of the election were between 85.4 and 82.9 percent as likely to vote in 2018, but that for each year the most recent incarceration recedes into the past, the treatment effect decreases by between 0.80 and 1.0 percent.

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 the presence of 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 6 presents the results of Models 5 and 6 from Table 5, but limits the pool to households where the most recent incarceration ended prior to 2010. This pool is restricted to individuals we are certain lived with formerly incarcerated individuals, and 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 6:   General Election Turnout, 2010 − 2018

	(1)	(2)
D(2018)	0.152***	0.170***
	(0.005)	(0.005)
D(Treated)	−0.183***	−0.271***
	(0.003)	(0.003)
D(2018) × D(Treated)	−0.104***	−0.116***
	(0.007)	(0.007)
Constant	0.622***	−2.245***
	(0.002)	(0.019)
Includes covariates from matching		X
Congressional District fixed effects		X
Observations	1,524,000	1,524,000
Log Likelihood	−1,664,900.000	−1,529,660.000

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.
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 between the treatment and control group. The 2018 election was historic in many ways: according to the United States Elections Project,[8] turnout was higher than it had been for a midterm election in a century. Many marginal voters, who had not previously participated in midterm elections, turned out. This is readily apparent in Figure 4 — the turnout in 2018 for the control and treatment groups was far higher than in 2014 and 2010. The control group, in fact, turned out at higher rates in 2018 than in the presidential contest of 2012, and the same was nearly true for the treatment group. The negative treatment effect for the treatment group in 2018 likely arises more from the unique nature of the 2018 election than from any demobilizing effect of

[8]http://www.electproject.org/home/voter-turnout/voter-turnout-data

Amendment 4.

And yet, this negative treatment effect is troubling. As Table 4 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. 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.

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