

# Turnout and Amendment 4: Mobilizing Eligible Voters Close to Formerly Incarcerated Floridians\*

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## Abstract

Recent scholarship shows that eligible voters in neighborhoods home to many arrested and incarcerated individuals vote at lower rates than those in less-impacted neighborhoods. Little work, however, has interrogated how this turnout gap might be counteracted. This paper uses Amendment 4, a 2018 Florida ballot initiative that promised to re-enfranchise most individuals whose voting rights had been revoked due to a felony conviction, to investigate whether this turnout disparity can be narrowed by a ballot initiative of particular significance to communities most impacted by incarceration. Using prison release records, I identify the neighborhoods and households where formerly incarcerated individuals live and assess the voting history of their neighbors and housemates. I find no evidence that Amendment 4 increased these voters' turnout in 2018 relative to other voters. While ending felony disenfranchisement is necessary, closing the turnout gap resulting from histories of policing and incarceration will require greater investment and engagement.

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## 19 **Introduction**

20 On November 6<sup>th</sup>, 2018, Floridians voted to amend their state constitution to re-enfranchise  
21 individuals with felony convictions in their past (Taylor 2018). The move was hailed as  
22 transformative for Floridian — and American — democracy; Uggen, Larson, and Shannon  
23 (2016) had estimated a few years earlier that some 1.5 million Floridians were disenfranchised  
24 and had finished serving their sentences, making the amendment the largest expansion of  
25 the franchise in the United States since the Twenty-sixth Amendment lowered the voting age  
26 to 18. The amendment received broad support. Although it needed just 60 percent of the  
27 vote to pass, 64.5 percent of voters supported the ballot initiative. This support contrasts  
28 sharply with other statewide races: Ron DeSantis won the gubernatorial race with only 49.6  
29 percent of the vote, while winning just 50.1 percent sent Rick Scott to the United States  
30 Senate.

31 Prior to 2018, Floridians convicted of felony offenses were permanently disenfranchised unless  
32 they applied for and received an individual pardon from the state’s clemency board. This  
33 was characterized by a “low success rate, cumbersome process, and lengthy amount of time”  
34 (B. L. Miller and Spillane 2012b, 432) and was driven in part by gubernatorial discretion:  
35 although Charlie Crist restored voting rights to roughly 150 thousand individuals over a 4  
36 year period, Rick Scott did so for fewer than 3 thousand people over 8 years (Schlakman  
37 2018). At the time Amendment 4 was passed, it was widely reported that the backlog of  
38 applications was nearly 10,000 and the wait stretched for as long as a decade (Ramadan,  
39 Stucka, and Washington 2018). Over the years, Florida’s procedure was subject to numerous  
40 lawsuits, and was ruled unconstitutional in early 2018 with Judge Mark Walker describing it  
41 as “a gauntlet of constitutionally infirm hurdles.”<sup>1</sup> Amendment 4 promised to automatically  
42 restore voting rights once individuals had completed their sentence, though it did not apply  
43 to individuals convicted of murder or sexual offenses.

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

44 In recent years scholars have leveraged administrative records and sophisticated statistical  
45 techniques to study the actual political effects of felony disenfranchisement in the United  
46 States (e.g. Meredith and Morse 2014, 2015; Colgan 2019; Morris 2021b). With the notable  
47 exception of White (2019a), however, the behavior of voters who live with individuals who  
48 have been convicted of felony offenses — but have not themselves been convicted — has gone  
49 unstudied. This article brings together these analytical approaches and an interdisciplinary  
50 body of literature to understand the political behavior of citizens whose family members  
51 have been incarcerated due to a felony conviction.

52 This study explores whether the opportunity to vote on Amendment 4 increased the (relative)  
53 participation among eligible voters who lived with or near individuals disenfranchised due  
54 to a period of felony incarceration. Americans’ political knowledge is deeply shaped by the  
55 incarceration of a loved one (Lee, Porter, and Comfort 2014), and exposure to the carceral  
56 state chills political involvement even among individuals who are not convicted. The criminal  
57 justice system can leave even would-be voters without a criminal record feeling as though  
58 political involvement is not for “people like me,” often despite having considerable political  
59 knowledge (Lerman and Weaver 2014). A growing body of quantitative research captures  
60 these “spillover” effects, demonstrating that neighborhoods with high levels of incarceration  
61 and disenfranchisement vote at markedly lower rates than other similar neighborhoods (e.g.  
62 Burch 2014; Morris 2020).

63 Amendment 4 in Florida offers a unique opportunity to investigate whether these chilling  
64 effects can be overcome by one ballot initiative. As I explain in the section that follows,  
65 Amendment 4 offered individuals living with or near formerly incarcerated individuals an  
66 opportunity to redefine their relationship with the government in positive ways. Although  
67 this made the ballot initiative perhaps particularly salient for these individuals, it took place  
68 against the backdrop of an entrenched carceral state that negatively structured many facets  
69 of their lives (see, for instance, Travis and Waul 2003). Ultimately, I do not find evidence  
70 that Amendment 4 mobilized individuals living with or near formerly incarcerated Floridians

71 in 2018 above-and-beyond turnout increases observed in other, similar voters.

## 72 **Theory and Literature**

73 In recent years scholars have documented the effect of the American criminal legal system  
74 on the lives of those who come under its purview, even once they are no longer under  
75 formal supervision. The growth of the criminal legal system has resulted in what Monica  
76 Bell calls *legal estrangement*, which reflects both *legal cynicism* — a cultural orientation  
77 that views the law and its enforcers as “illegitimate, unresponsive, and ill equipped to ensure  
78 public safety” (Kirk and Papachristos 2011, 1191; see also Sampson and Bartusch 1998; Kirk  
79 and Matsuda 2011; Morenoff and Harding 2014) — and the objective structural conditions  
80 (such as policing practices and criminal law) that give rise to this orientation (Bell 2017,  
81 2066 – 2067). Legal estrangement has also been linked with “institutional” or “system  
82 avoidance.” Brayne (2014, 385), for instance, documents that “individuals who have been  
83 stopped, arrested, convicted, or incarcerated are less likely to interact with institutions  
84 that keep formal records, such as hospitals, banks, employment, and schools.” Haskins  
85 and Jacobsen (2017) finds that institutional avoidance explains formerly incarcerated men’s  
86 reduced willingness to be involved with their children’s schools, and Remster and Kramer  
87 (2018) shows that this avoidance explains the behavior of Black and non-Black individuals  
88 alike.

89 Institutional avoidance is especially clear when it comes to democratic participation, par-  
90 ticularly in the voting booth. It is well established that a criminal conviction — and, more  
91 specifically, a period of incarceration — decreases turnout even when individuals are no  
92 longer legally disenfranchised (Weaver and Lerman 2010; Burch 2011; White 2019b; but  
93 see Gerber et al. 2017). The effect of disenfranchisement policy on the political behavior of  
94 individuals who experience the criminal justice system indirectly via the conviction of a fam-  
95 ily or community member, however, is somewhat mixed. Most research finds that turnout

96 is measurably lower in states with stricter voter disenfranchisement policies or more disen-  
97 franchised citizens (e.g. Bowers and Preuhs 2009; King and Erickson 2016), though Miles  
98 (2004) argues that these effects are small. The little research that has explored the spillover  
99 effects of disenfranchisement policy at the *neighborhood* level has similarly found evidence  
100 that incarceration and disenfranchisement demobilizes eligible voters in impacted communi-  
101 ties (Burch 2014; Morris 2020; but see White 2019a). Understanding whether Amendment  
102 4 was likely to recoup the lost turnout of eligible voters who lived with or near the disen-  
103 franchised requires understanding *how* their indirect exposure to the criminal justice system  
104 (or “proximal contact” (Walker 2014)) depressed turnout to begin with.

105 Work from Vesla Weaver and Amy Lerman (2010; 2014) describes in great detail how legal  
106 estrangement ruptures individuals’ willingness to engage in electoral politics. They argue  
107 that a felony conviction serves as “a durable constraint and marker of their citizenship”  
108 (Lerman and Weaver 2014, 133), and that custodial citizens — individuals in communities  
109 with aggressive crime control who may or may not have a criminal history themselves —  
110 “become less likely to believe that they (and those like them) can change *the system*, a  
111 reduction in external efficacy” (Lerman and Weaver 2014, 137, emphasis in the original).  
112 Their work is replete with examples of individuals who know much about politics yet choose  
113 to “stay below the radar” because “‘they’re [government officials] not interested in what I  
114 have to say’ ” (Lerman and Weaver 2014, 210).

115 Importantly, these demobilizing consequences are not limited to those who are convicted;  
116 rather, “the sense of alienation in a carceral regime emanates not only from what police  
117 might do to ‘you,’ but from what they might do to your friends, your intimate partners,  
118 your parents, your children; to people of your race or social class; and to people who live  
119 in the neighborhood or the city where you live” (Bell 2017, 2058). Put differently, the legal  
120 system serves as a site of political socialization even for those who are not formally convicted  
121 of a crime (Lee, Porter, and Comfort 2014; Comfort 2016; Kirk 2016). There is, however,  
122 some evidence that these chilling effects on political participation can be overcome. Recent

123 work demonstrates that direct and indirect contact with the criminal justice system can  
124 be mobilizing when these experiences are linked with narratives of injustice (Walker and  
125 García-Castañon 2017; Walker 2020).

126 Of course, there is no bright line dividing individuals with *indirect* exposure to the criminal  
127 justice system from individuals with their own, *direct* exposure to the carceral state. The  
128 geographic concentration of policing and incarceration patterns (e.g. Gelman, Fagan, and  
129 Kiss 2007) mean that individuals in community with the formerly incarcerated — that is,  
130 people living with or near formerly incarcerated residents — might also have other, direct  
131 relationships with the criminal justice system. In 2017 there were 711,831 arrests in Florida  
132 but just 134,554 guilty felonious dispositions.<sup>2</sup> Although individuals who were arrested but  
133 not convicted of felonies were not legally disenfranchised, even low-level interactions can  
134 have a chilling effect on one’s relationship with the government, a relationship Amendment  
135 4 could have led them to reconsider.

136 Based on these literatures, I hypothesized that both the substance of the proposed consti-  
137 tutional amendment and the messaging used by the campaign supporting its passage would  
138 increase the relative turnout of individuals living with and near formerly incarcerated Florid-  
139 ians. Restoring voting rights to individuals who had been convicted of felony offenses would  
140 end the “civil death” of felony disenfranchisement (Ewald 2002; B. L. Miller and Spillane  
141 2012a), nullifying one of the durable badges identified by Lerman and Weaver. Amendment  
142 4 offered those in community with the formerly incarcerated the chance to affirm that their  
143 family and community members deserved to have their voices heard in the democratic arena,  
144 a chance I anticipated would disproportionately spur them to participate.

145 Moreover, the public messaging employed by the Amendment 4 campaign was explicitly  
146 designed to change how voters understood the citizenship of disenfranchised individuals.  
147 The campaign cast the ballot initiative as an issue of fairness, criticizing Florida’s existing  
148 disenfranchisement policy for creating two tiers of citizenship. The organization leading the

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<sup>2</sup>See <http://edr.state.fl.us/Content/resource-demand/criminal-justice/reports/criminal-justice/cj7.pdf>.

149 campaign leveraged the notion that disenfranchised citizens deserved to be re-incorporated  
150 into the body politic in its very name — “Second Chances Florida.” The framing was  
151 effective: the editorial boards of each of Florida’s three biggest newspapers endorsed the  
152 amendment, all using language related to fairness and civic redemption. The Tampa Bay  
153 Times told readers they had a “remarkable opportunity to remedy that unfairness” (*Tampa*  
154 *Bay Times* 2018); the Sun Sentinel informed voters “[t]here may never be an opportunity  
155 to do a better thing than to vote yes on this reform” (*Sun Sentinel* 2018); and the Orlando  
156 Sentinel said that Florida’s then-policy “denie[d] our fellow citizens a second chance. It  
157 denie[d] redemption” (*Orlando Sentinel* 2018). Insofar as the campaign was successful at  
158 helping these individuals understand the experiences of their formerly incarcerated family  
159 and community members in the context of a broader narrative of (racial) injustice, I expected  
160 this framing would mobilize them to vote at higher rates than other voters.

161 In addition to newspapers across the state, the campaign deployed “volunteers from a broad  
162 coalition that included advocacy groups, Christian organizations, the League of Women Vot-  
163 ers, criminal justice experts and, of course, those who had been convicted of felonies” (Robles  
164 2018). Andrew Gillum, the Democratic gubernatorial candidate, also vocally supported the  
165 amendment, openly discussing his family’s relationship with the criminal justice system and  
166 his own sibling’s disenfranchisement (Smith 2018). Voters were thus getting cues from all  
167 sorts of messengers that Amendment 4 deserved to be passed, and that individuals with  
168 convictions in their past should be allowed to vote. I expected that these cues, plus the  
169 descriptive representation (Merolla, Sellers, and Fowler 2013) promised by Gillum, would  
170 have proved especially mobilizing for the population in closest contact with disenfranchised  
171 Floridians.

172 At the same time, there was some reason to think the ballot initiative would not dispro-  
173 portionately increase turnout among voters in close contact with formerly incarcerated, dis-  
174 enfranchised individuals. Legal estrangement runs deep: the “hidden curriculum” of the  
175 criminal justice system (Justice and Meares 2014; Meares 2017) teaches individuals their

176 place in this system over a very long period, through both incarceration and day-to-day in-  
177 teractions with government representatives such as the police. It is perhaps naive to expect  
178 that a single ballot initiative could overcome these negative forces.

179 Moreover, the individuals in these neighborhoods were perhaps less familiar with the content  
180 of Amendment 4 than others: Bowler and Donovan (1994), for instance, demonstrates that  
181 education and polarization are strong predictors of individuals' familiarity with ballot ini-  
182 tiatives. Shaker (2012) also finds that higher-educated individuals are more knowledgeable  
183 about local politics. Given that formerly incarcerated individuals leave prison for neigh-  
184 borhoods with less access to higher education (see Table 2 below), their neighbors and  
185 housemates may have been less aware of the amendment in the first place, in which case it  
186 obviously could not heighten motivation to cast a ballot.

## 187 **Research Design and Expectations**

188 I begin by testing whether a neighborhood's formerly incarcerated population influenced its  
189 turnout in 2018. Because statewide felony probation records are not available, this analysis  
190 is based on only the subset of disenfranchised individuals who were imprisoned for a felony  
191 conviction. Neighborhoods that are home to formerly incarcerated individuals are identified  
192 by geocoding release records from the Florida Department of Corrections, and I offer two  
193 definitions of neighborhoods.

194 Neighborhoods are first defined as voting precincts. The Florida Division of Elections makes  
195 election results available at this level, which allows me to test turnout specifically on Amend-  
196 ment 4 and neighborhood-level support for the amendment. I can also assess how salient the  
197 amendment was for participants by estimating the share of voters who "rolled off" (or chose  
198 not to vote) for Amendment 4. Unfortunately, the use of precinct-level data leaves us with a  
199 major drawback: when doing analysis at this level, bias-free turnout denominators are hard  
200 to come by. Because the Census Bureau does not produce population estimates for individual



201 voting precincts, turnout cannot be calculated by dividing the number of ballots cast by the  
202 eligible population (that is, citizens over the age of 17 without a felony conviction); rather,  
203 it must be constructed as a share of registered voters. If there is a relationship between the  
204 number of formerly incarcerated residents and the registration rate of a neighborhood, our  
205 estimates will be biased.

206 That could be the case in the study at hand. Some political organizers supporting Amend-  
207 ment 4 focused on canvassing neighborhoods with many formerly incarcerated individuals  
208 (Speri 2018), potentially raising the registration rate in these areas. If relatively few of these  
209 newly-registered individuals voted, the net effect would be higher turnout among *eligible*  
210 *residents* but lower turnout among *registered voters*. For further discussion of how improper  
211 denominators can bias turnout estimates, see Amos, McDonald, and Watkins (2017) and  
212 Amos and McDonald (2020).

213 To address this potential problem, I also define neighborhoods as Census block groups. The  
214 Census Bureau makes estimates of the citizen voting-age population (a better denominator  
215 for turnout) available at this level. In this case, however, I must use a geocoded voter file  
216 to determine turnout. Because I aggregate the number of participants in a block group  
217 from individual-level data, I cannot determine whether an individual actually participated  
218 in the contest for Amendment 4 or they rolled off. Similarly, I am unable to interrogate the  
219 relationship between block group characteristics and support for Amendment 4. Although  
220 each definition of neighborhood presents some drawbacks, the two definitions together paint  
221 a full picture.

222 After examining whether the presence of formerly incarcerated residents was related with  
223 neighborhoods' voting behavior, I ask whether voters who lived with formerly incarcerated  
224 individuals turned out at higher rates in 2018. For this analysis, I use the release addresses  
225 of formerly incarcerated individuals (the most recent address available, according to the  
226 Department of Corrections) and voter file data to identify registered voters who lived with

227 formerly incarcerated individuals. Voters are considered “treated” if they lived with a for-  
228 merly incarcerated individual, and “untreated” otherwise. I then use a variety of individual-  
229 and neighborhood-level characteristics to match treated and untreated voters using what  
230 methodologists call a “genetic” process (Sekhon 2011).

231 After matching these voters, I employ a difference-in-differences specification to determine  
232 whether treated voters participated at higher rates in the 2018 election. These analyses  
233 are run for all voters who lived with a formerly incarcerated individual, as well as only  
234 the subset of households whose members have not been to prison for many years. This  
235 final specification allows me to disentangle the depressive effect of indirect exposure to the  
236 criminal justice system from the mobilizing effect of Amendment 4 in 2018 by incorporating  
237 any depressive effect into the pre-2018 baseline.

238 Table 1 summarizes the specific hypotheses this article tests.

Table 1: Hypotheses

Hypothesis	Approach
<b>Neighborhood Level</b>	
1a. Each additional formerly incarcerated resident in a voting precinct is associated with increased turnout among registered voters in that precinct.	OLS regression
1b. Each additional formerly incarcerated resident in a Census block group is associated with increased turnout among eligible citizens in that block group.	OLS regression
2. Each additional formerly incarcerated resident in a voting precinct is associated with increased support for Amendment 4 in that precinct.	OLS regression
3. Each additional formerly incarcerated resident in a voting precinct is associated with decreased roll-off in that precinct.	OLS regression
<b>Household Level</b>	
4. Amendment 4 increased turnout in 2018 among household members of formerly incarcerated individuals relative to their controls. 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.

## 239 Data

240 I leverage multiple data sources to investigate whether individuals in community with for-  
 241 merly incarcerated Floridians were more likely to vote in the 2018 election. Replication  
 242 materials can be found in the *APSR Dataverse* (Morris 2021a). Although this study relies  
 243 on voter file data and publicly-available prison release records, I anonymize the neighbor-  
 244 hoods and households home to formerly incarcerated individuals in order to protect privacy.

## 245 Department of Corrections Data

246 Felony incarceration records come from the Florida Department of Corrections' Offender  
247 Based Information System (OBIS). The OBIS includes all individuals released from prison  
248 following a felony conviction since October 1, 1997. There were approximately 390,000 such  
249 individuals. I retain only the record associated with an individual's most recent incarceration  
250 according to the release date, and identify all formerly incarcerated individuals who were  
251 finished with their sentence as of the 2018 election by cross-referencing these records against  
252 imprisonment and parole records. Roughly 38,000 individuals were either re-incarcerated or  
253 on parole as of the 2018 election and are thus removed. The 10,000 or so individuals who  
254 died or absconded before their sentence was completed are also removed from the dataset,  
255 leaving us with about 343,000 individuals who had finished their sentence by the time of the  
256 2018 midterm election.

257 The OBIS provides the "release plan address" for individuals who were formerly incarcerated.  
258 As noted above, this is the most recent address available for individuals who are no longer  
259 under supervision.<sup>3</sup> The address data are messy and require substantial cleaning. In some  
260 cases, the address field is left blank; in others, the record simply notes the road or the town  
261 of the individual's residence, without providing full address information. I assume that any  
262 record that does not begin with an integer does not have a full address and cannot be used  
263 (this results in the exclusion of just under 3 percent of records). The remaining addresses  
264 are geocoded. Individuals whose addresses were geocoded outside of Florida (10.9 percent)  
265 or for whom the geocoder failed (3.2 percent) are dropped. After completing the geocoding  
266 process we are left with some 286,000 individuals who were finished with their sentence as of  
267 the 2018 midterm, were released to Florida addresses, and reported an address that could be  
268 geocoded. In other words, at least 94 percent of individuals released to addresses in Florida

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<sup>3</sup>The OBIS lists current addresses for individuals currently under community supervision, which may differ from the release plan addresses. However, according to a response to a public records request filed by the author with the Department of Corrections, these historical data are not maintained once an individual has been discharged.

269 were successfully geocoded.

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

### 273 **Caveats with the DOC Data**

274 Using the release plan address for individuals last released from prison many years ago  
275 presents some potential problems. Some of these individuals surely died or moved after  
276 completing their sentence. In the Supplementary Information I show the results presented in  
277 the body of this article when I limit the pool of formerly incarcerated people to individuals  
278 released from prison during or after 2015. Because these individuals were released more  
279 recently, their addresses are probably more accurate. The primary findings of this study  
280 hold when the sample is thus limited.

281 Many formerly incarcerated individuals leave prison not for homes with family members, but  
282 rather to homeless shelters or other sites of incarceration. Of the five most commonly listed  
283 addresses, three were Immigration and Customs Enforcement properties, one was owned by  
284 the Salvation Army, and one was a rescue mission. The body of this article excludes formerly  
285 incarcerated individuals whose address was listed by five or more individuals, as institutions  
286 for returning citizens may have uniquely structured responses to Amendment 4 (see, for  
287 instance, Henig 1994). The Supplementary Information shows that the primary findings in  
288 the article hold when I include all formerly incarcerated individuals. Just over 15 percent of  
289 formerly incarcerated individuals listed these sorts of addresses as their post-incarceration  
290 residence.

291 Neither the OBIS nor any other statewide database makes records available for individuals  
292 sentenced to felony probation. Between 75 and 80 percent of individuals found guilty of

293 felonies in recent years in Florida have been sentenced to probation.<sup>4</sup> This may pose a  
294 problem: neighborhoods with residents disenfranchised due to felony probation are also  
295 “treated,” as are housemates of these individuals. However, not all individuals who serve  
296 a term of felony probation actually lose their voting rights. Florida judges are allowed  
297 to “withhold adjudication” (Tragos and Sartes 2008), meaning defendants are not formally  
298 convicted of a felony, but consent to pay fines and restitution and to serve a term of probation.  
299 Individuals whose adjudication is withheld are not disenfranchised.

300 As discussed in the Supplementary Information, probation records with residential addresses  
301 are available for Hillsborough County, the Florida county with the third-highest number  
302 of formerly incarcerated individuals according to the OBIS records. Within Hillsborough  
303 County, the correlation coefficient between the number of felony probationers and formerly  
304 incarcerated residents (scaled by population) is 0.92 at the block group level. The evidence  
305 from Hillsborough County therefore indicates that number of formerly incarcerated individ-  
306 uals in a neighborhood should be a reasonable proxy for the total number of disenfranchised  
307 residents.

308 In the Supplementary Information, the neighborhood- and individual-level models presented  
309 in the body of this article are re-estimated using only neighborhoods and individuals in  
310 Hillsborough County, with individuals sentenced both to felony incarceration *and* probation  
311 included in the models. Their incorporation does not meaningfully impact the primary re-  
312 sults. Although this study relies only on formerly incarcerated individuals, the data available  
313 for robustness checks indicate that the relationships detailed here probably extend to the  
314 full disenfranchised population.

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<sup>4</sup>See <http://edr.state.fl.us/Content/resource-demand/criminal-justice/reports/criminal-justice/index.cfm>.

## 315 **Voter File Data and Census Data**

316 I primarily use Florida voter file data from the data vendor L2 Political which includes  
317 publicly-available information on individuals such as their home address, their age and gen-  
318 der, their participation history, and their political affiliation. In addition to the L2 data I  
319 use self-identified race and ethnicity information from the raw Florida voter file. I also use  
320 the raw Florida file to provide the gender for voters for whom L2 did not have data, as well  
321 as voters' home counties and precincts.

322 Precinct and block group demographics are constructed by aggregating up from the voter  
323 file data. Neighborhood characteristics such as average age are the averages of all registered  
324 voters in that neighborhood. For characteristics such as income that are unavailable at  
325 the individual level, voters are assigned the value associated with their home block group  
326 from the American Community Survey's 2014 – 2018 5-year estimates; the precinct average  
327 income, therefore, is effectively the average of all the block groups within that precinct,  
328 weighted by the number of registered voters.

## 329 **Matched Department of Corrections and Voter File Data**

330 I identify registered voters who lived with formerly incarcerated individuals by matching on  
331 residential addresses. As discussed above, these addresses are often in different formats. To  
332 increase the quality of the matches, I standardize common street and address abbreviations  
333 as well as capitalization. "Boulevard," for instance, becomes "BLVD" in each instance in the  
334 DOC and voter file data. These standardizations are taken from Appendix C of the USPS  
335 Postal Addressing Standards (2015). Exact matching for the entire residential address is  
336 required. Formerly incarcerated individuals who were registered to vote are removed (as  
337 noted in the Introduction, some individuals were able to have their voting rights restored).

### 338 **Potential Confounders**

339 Voters with indirect exposure to the criminal justice system might have been uniquely mo-  
340 tivated to turn out through avenues other than the ballot initiative. For instance, Andrew  
341 Gillum was poised to become the state’s first Black governor, which could increase the  
342 turnout of Black voters who are over-represented in the treatment group (e.g. Washing-  
343 ton 2006; Fairdosi and Rogowski 2015; P. Miller and Chaturvedi 2018). By controlling for  
344 neighborhood demographics (and, in the matching exercise, forcing control voters to mirror  
345 treated voters on key demographics such as race and party affiliation), I minimize the dif-  
346 ferences between the treatment and control groups along characteristics known to influence  
347 turnout.

348 There is little reason to believe that changes to electoral rules would have differently influ-  
349 enced the turnout for individuals in close proximity to the formerly incarcerated than other,  
350 similar voters. The number of early voting days was cut for the 2012 general election, but  
351 the longer period was restored for the 2014 – 2018 period.<sup>5</sup> Early voting was not allowed on  
352 college campuses in the 2014 and 2016 elections, though it was allowed in 2018 (Bousquet  
353 2018). If voters who lived near the formerly incarcerated had better or worse access to college  
354 campuses than other voters, this could influence their turnout. I include neighborhood-level  
355 estimates of collegiate education in each of the regressions to mitigate the potential effects of  
356 this change. Florida did not enact other reforms such as same-day registration or automatic  
357 voter registration over the period, nor did its absentee voting rules change. We can therefore  
358 be confident that any turnout effects observed are not being driven by the treatment group  
359 responding to rules changes in different ways than other voters.

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<sup>5</sup>See [https://ballotpedia.org/Voting\\_in\\_Florida](https://ballotpedia.org/Voting_in_Florida).



## 360 Neighborhood-Level Results

361 Before presenting the results of the econometric modeling, I examine whether — and to  
 362 what extent — block groups with formerly incarcerated individuals differ from block groups  
 363 elsewhere in the state. A simple comparison of block groups with and without formerly  
 364 incarcerated individuals, however, proves unhelpful: 97.1 percent of block groups in the  
 365 state are home to someone who has been to prison, though formerly incarcerated individuals  
 366 are clearly concentrated in some block groups. Column 1 of Table 2 presents the statewide  
 367 mean of all block groups, weighted by their population. In Column 2, I re-weight the block  
 368 groups by the number of formerly incarcerated residents.

Table 2: Block Group Demographics

Measure	Average Block Group	Average Block Group
	All Floridians	Formerly Inc. Floridians
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.

369 Although nearly all parts of the state are impacted by the criminal justice system (and, more  
 370 specifically, mass incarceration), Table 2 makes clear that formerly incarcerated individuals  
 371 are concentrated in neighborhoods with lower incomes, higher levels of unemployment, and  
 372 where a much larger share of the population is Black.

373 I next assess whether the presence of formerly incarcerated residents was associated with

374 higher turnout in 2018 using ordinary least squares regressions. In the precinct-level model,  
375 turnout is calculated by dividing the number of ballots cast for or against Amendment 4  
376 by the number of actively registered voters in the precinct,<sup>6</sup> while block group turnout is  
377 calculated by dividing the number of voters marked as participants in the voter file by the  
378 adjusted citizen voting age population (ACVAP).<sup>7</sup> *Formerly Incarcerated Residents* is the  
379 primary independent variable. Models 2 and 4 also include a measure of how long the  
380 average formerly incarcerated resident has been out of prison (*Av. Years since Most Recent*  
381 *Incarceration*) to test whether recently incarcerated residents impact turnout differently  
382 than those who were released many years ago. Neighborhoods with no formerly incarcerated  
383 residents are excluded from models 2 and 4. I also control for other covariates known to  
384 influence turnout such as age and income. There is just one observation per neighborhood  
385 in each model, but I control for neighborhood-level turnout from the 2010 – 2016 general  
386 elections. Finally, I include fixed effects for congressional districts, and robust standard  
387 errors are clustered at this level.<sup>8</sup>

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<sup>6</sup>The 35 precincts where calculated turnout exceeds 100 percent have been dropped from the analysis, though their inclusion does not affect the results.

<sup>7</sup>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). My definition of ACVAP is similar to the voting eligible population estimated by McDonald (2002), though I do not have estimates of the number of individuals disenfranchised for a felony probation at the neighborhood-level.

<sup>8</sup>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*** (0.00004)	-0.0002*** (0.00003)	-0.0002*** (0.00004)	-0.0002*** (0.00004)
Av. Years since Most Recent Incarceration		0.0001 (0.001)		0.0002* (0.0001)
Percent White	0.017 (0.110)	-0.088 (0.123)	0.017 (0.014)	0.017 (0.014)
Percent Black	0.027 (0.109)	-0.086 (0.121)	0.041** (0.017)	0.040** (0.017)
Percent Latino	-0.081 (0.116)	-0.175 (0.125)	-0.007 (0.016)	-0.008 (0.016)
Percent Asian	0.082 (0.128)	-0.006 (0.166)	0.040* (0.022)	0.039* (0.022)
Percent Male	0.302 (0.188)	0.376** (0.179)	0.095 (0.086)	0.102 (0.089)
Percent Democrats	0.059 (0.082)	0.161** (0.073)	0.067*** (0.020)	0.067*** (0.020)
Percent Republicans	0.015 (0.081)	0.105 (0.070)	0.007 (0.024)	0.004 (0.024)
Average Age	0.0001 (0.0005)	0.0001 (0.001)	0.001*** (0.0003)	0.001*** (0.0003)
Average Income (\$10,000s)	0.002** (0.001)	0.001** (0.001)	0.002*** (0.0003)	0.002*** (0.0003)
Percent With Some College	0.183*** (0.016)	0.188*** (0.020)	0.082*** (0.005)	0.082*** (0.005)
Percent Unemployed	-0.032 (0.025)	-0.033 (0.028)	-0.005 (0.006)	-0.004 (0.006)
Constant	-0.211* (0.114)	-0.235* (0.127)	-0.188** (0.083)	-0.200** (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 <sup>2</sup>	0.782	0.814	0.979	0.979
Adjusted R <sup>2</sup>	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.

388 Table 3 indicates that 2018 turnout was lower in neighborhoods with more formerly in-  
389 carcerated residents, and the average length of time since formerly incarcerated residents'  
390 most recent incarceration is not related to turnout. The block group models have nearly  
391 twice as many observations as the precinct-level ones and their  $R^2$ s are considerably higher,  
392 perhaps indicating a better fit. Nevertheless, the estimated coefficient for *Formerly Incar-*  
393 *cerated Residents* is the same (when rounded to one hundredth of a percentage point) for  
394 both neighborhood definitions.

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

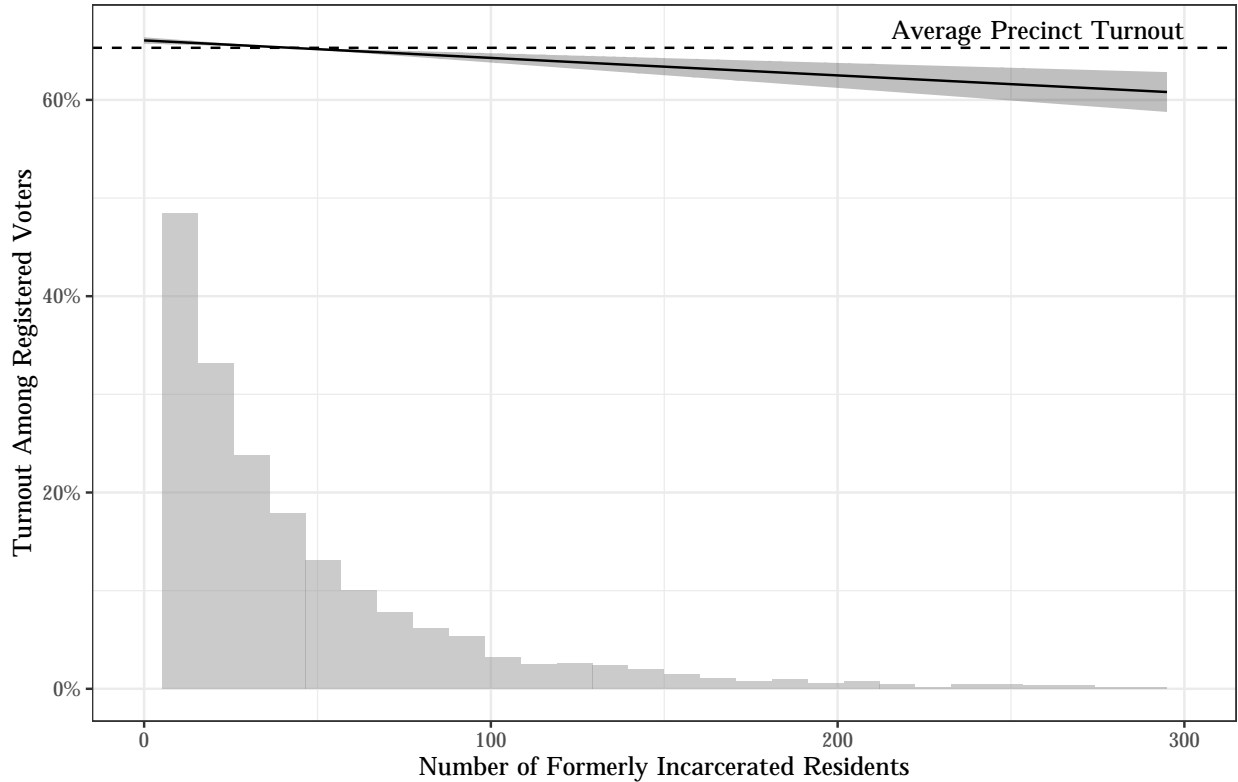


Figure 1: Marginal Effect of Formerly Incarcerated Residents on Precinct Turnout Among Registered Voters

403 In Table 4 I present the results of OLS models that test whether the number of formerly  
 404 incarcerated community members influenced a neighborhood’s support for Amendment 4  
 405 or Amendment 4 roll-off. Roll-off is calculated as  $1 - \frac{\text{Ballots Cast for Amendment 4}}{\text{Ballots Cast in Contest with the Most Votes}}$ .  
 406 It ranges from zero (if everyone who cast a ballot made a decision on the Amendment 4  
 407 question) to one (if no participants voted for or against Amendment 4). A lower number  
 408 represents lower roll-off, indicating that the issue was more salient for participants.

Table 4: Precinct Engagement with Amendment 4

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

409 Table 4 demonstrates that precincts with more formerly incarcerated residents supported  
410 Amendment 4 at slightly higher rates. Similarly, roll-off was lower in neighborhoods with  
411 more formerly incarcerated residents. Figures 2 and 3 plot the marginal effect of each  
412 additional formerly incarcerated resident on a precinct's support for Amendment 4 (model  
413 1), and the precinct's roll-off on Amendment 4 (model 3). These figures make clear that the  
414 number of formerly incarcerated residents has a relatively small impact on precinct support  
415 for its passage, and a relatively large impact on precinct level roll-off.

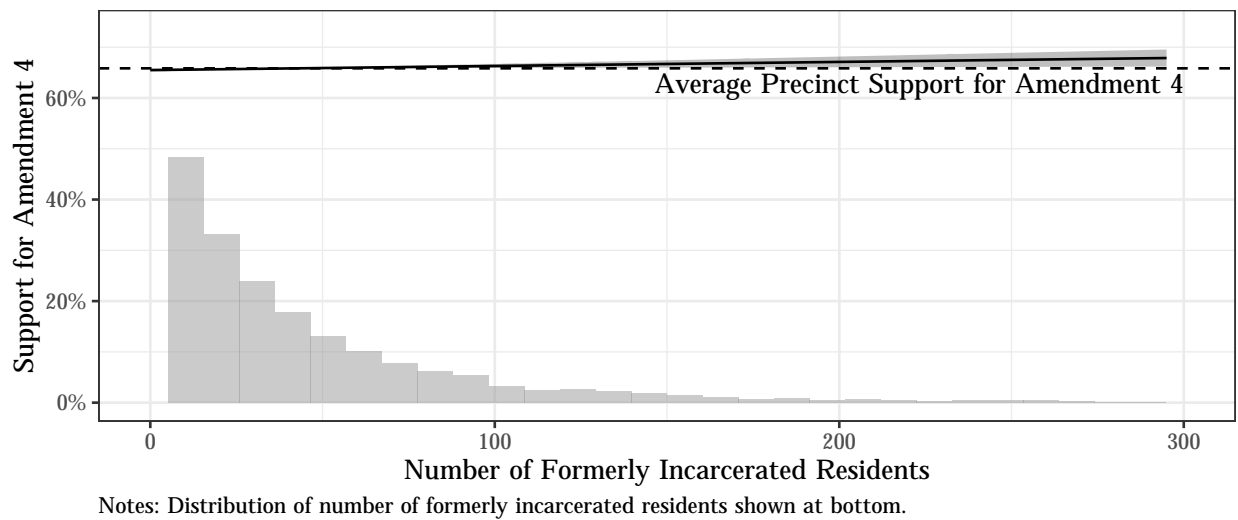


Figure 2: Marginal Effect of Formerly Incarcerated Residents on Support for Amendment 4

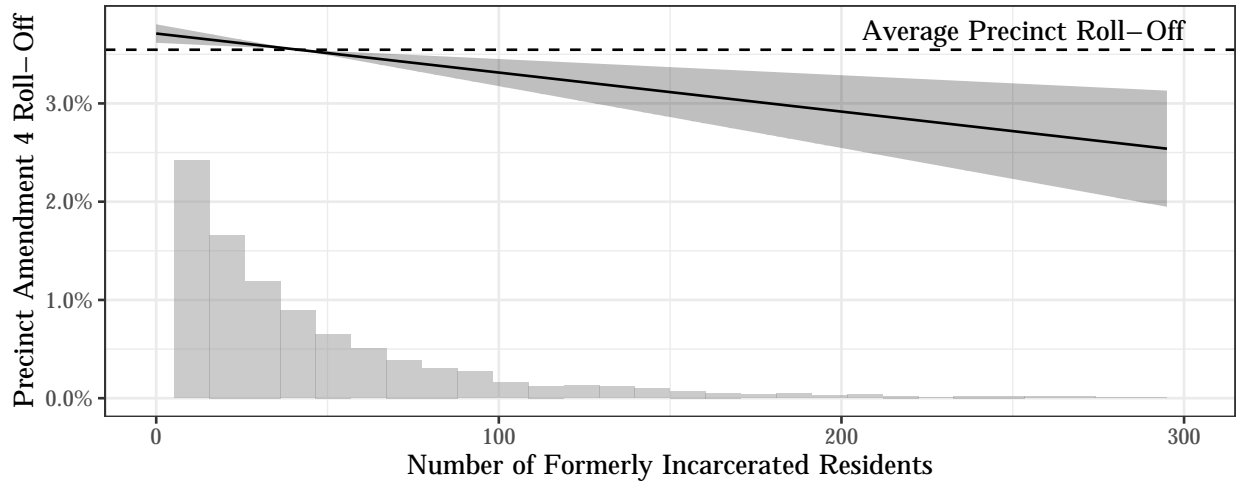


Figure 3: Marginal Effect of Formerly Incarcerated Residents on Amendment 4 Roll-Off

416 Why the relationship between formerly incarcerated residents and support is less strong  
 417 (though positive and statistically significant) than salience is not clear, perhaps pointing to  
 418 a variety of individual responses to crime and criminal justice policy in these neighborhoods.  
 419 Leverentz (2011) argues that punitiveness is positively correlated with the salience of crime.  
 420 The recently incarcerated residents might activate both punitiveness and support for the  
 421 amendment, with support winning out slightly. The coefficients for *Av. Years since Most*  
 422 *Recent Incarceration* indicate that neighborhoods where the formerly incarcerated residents  
 423 have been out of prison for longer saw both higher support for Amendment 4 and higher  
 424 roll-off. Future work ought to interrogate how support for criminal justice reforms and the  
 425 salience of those reforms change as community members' incarcerations recede into the past.  
 426 These neighborhood-level models demonstrate that neighborhoods with many formerly in-  
 427 carcerated residents did not turn out at higher rates than other, similar neighborhoods in  
 428 2018 even though Amendment 4 was on the ballot. However, while formerly incarcerated  
 429 neighbors were not associated with getting people into the voting booth, they were associated  
 430 with how voters cast their ballots once there.



## 431 Individual-Level Results

432 Neighborhood turnout rates could be obscuring underlying patterns. Inducements to vote  
433 at the household level might be too small to register at the neighborhood level, and it is  
434 possible that Amendment 4 shaped turnout differently for individuals who live with formerly  
435 incarcerated individuals than for their neighbors. A neighborhood may have disengaged from  
436 the political process thanks to exposure to the carceral state. Household members of the  
437 formerly incarcerated may have had a similar historical response, and yet be more susceptible  
438 to mobilization from Amendment 4; they are, after all, the voters whose identities are most  
439 likely shaped by indirect exposure to felony disenfranchisement.

440 This section directly examines the turnout of individuals who lived with formerly incarcer-  
441 ated individuals in 2018, relative to other, similar voters. As discussed above, I identify  
442 individuals who live with formerly incarcerated individuals by matching addresses listed in  
443 the Department of Corrections release data to the registered voter file. All registered vot-  
444 ers who live at an address reported by a formerly incarcerated individual are considered  
445 “treated.”

446 Each treated individual is then matched (Sekhon 2011) with five untreated registered voters  
447 elsewhere in her congressional district.<sup>9</sup> I use five matches in order to increase the sample size  
448 of the study; the large pool of potential controls means this can be done without sacrificing  
449 the quality of the matches. Voters’ block group median income and share with some collegiate  
450 education come from the ACS 2018 5-year estimates, while all other characteristics come from  
451 the voter file. Matching is done with replacement and ties are randomly broken. Table 5  
452 presents the results of the matching exercise for each of the characteristics used.

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<sup>9</sup>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 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%	12.8%	100.00	100.00	100.00	100.00
% 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%	99.99	99.99	99.99	99.99
Registration Date	2004-01-28	2004-09-24	2004-01-28	2004-02-11	94.03	38.85	27.88	19.19
Age	48.95	52.45	48.95	48.77	94.71	94.34	92.44	90.89
% Democrat	53.7%	36.9%	53.7%	53.7%	99.99	99.99	99.99	99.99
% 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%	99.92	99.95	99.92	99.62
Median Income	\$47,389	\$62,995	\$47,389	\$47,402	99.92	99.82	99.70	99.22

453 As Table 5 makes clear, the treated registered voters differ in meaningful ways from the rest  
 454 of the electorate: three times as many are Black, a larger share are registered Democrats,  
 455 and they live in neighborhoods with lower incomes. The matching process, however, results  
 456 in a control group that is very similar to the treatment group with at least a 94 percent  
 457 improvement in the mean difference for each measure.

458 Figure 4 demonstrates that the parallel trends assumption is satisfied: although the treat-  
 459 ment group has lower turnout rates in general, the gap between the treatment and control  
 460 groups is largely constant between 2010 and 2016. Turnout in each year is measured as a  
 461 function of voters registered in 2018, which partially explains why observed turnout is higher  
 462 later in the period. Of course, some of the increase in turnout observed in later years in  
 463 Figure 4 can be attributed to higher “real” turnout as a share of eligible citizens.

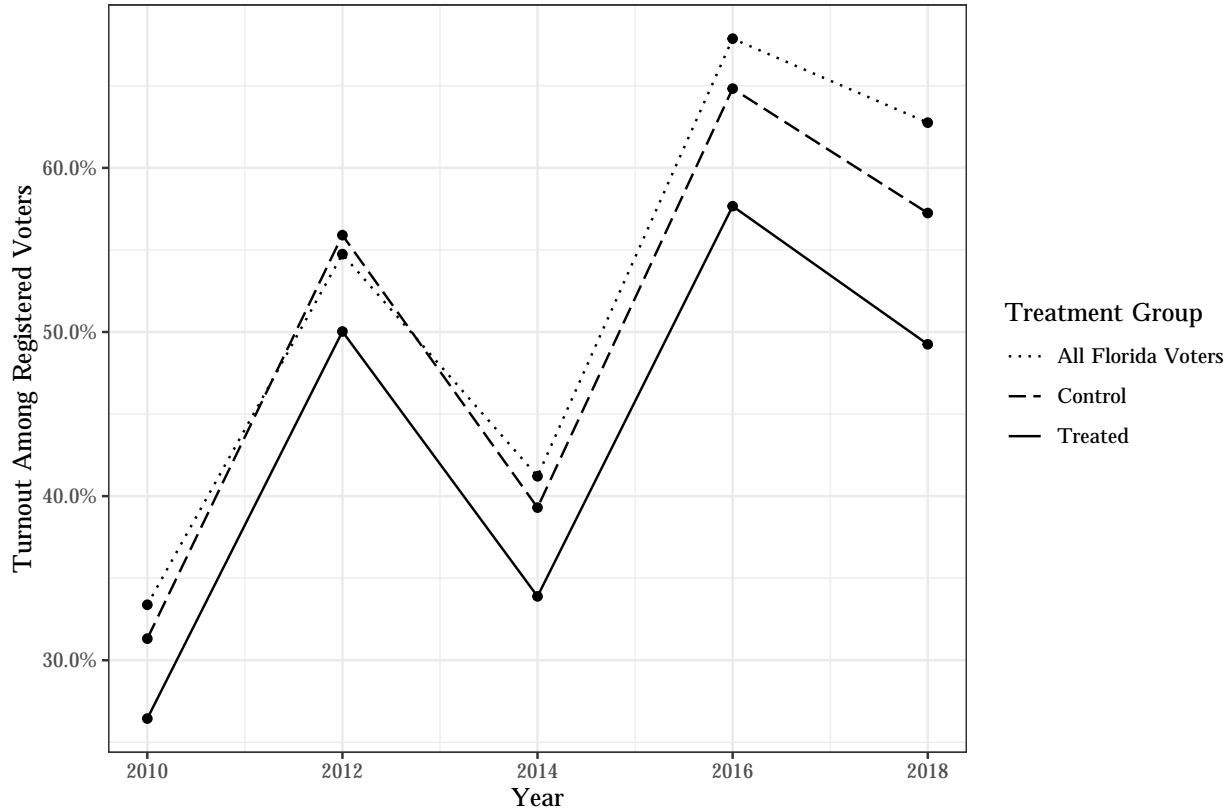


Figure 4: General Election Turnout for Treated and Control Voters, 2010 – 2018

464 The trends presented in Figure 4 offer preliminary visual corroboration of what I find at the  
 465 neighborhood level — namely, that 2018 turnout was not higher for voters in close contact  
 466 with formerly incarcerated individuals. Table 6 formalizes these trends into an ordinary least  
 467 squares regression.<sup>10</sup> A treatment dummy distinguishes treated from control voters. The  
 468 treatment dummy is interacted with another dummy identifying the 2018 election. Robust  
 469 standard errors are clustered at the level of the match (Abadie and Spiess 2020). Model 1  
 470 presents the model output without the other controls used for matching; model 2 includes  
 471 these covariates.

472 In models 3 and 4 of Table 6 I consider the possibility that the negative spillover effects

<sup>10</sup>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.

473 of incarceration dissipate over time. In these models, the dummies indicating treatment  
474 and the 2018 election are interacted with the number of years since the most recent release  
475 of a household member from prison (*Years Since Latest Incarceration*, shortened to *Years*  
476 *Since* in interactions). Matched control observations are assigned the value associated with  
477 their treated observation. Model 3 includes no other covariates, while model 4 includes the  
478 matched variables.

479 Formerly incarcerated individuals who were released from prison many years ago may no  
480 longer live at the same address they reported when leaving prison. Models 5 – 8 therefore  
481 include only the treated individuals (and their matches) whose registration dates predate  
482 the latest prison release date of a household member, who we can be relatively sure lived  
483 with an incarcerated individual. The treatment effects in these models tell the same general  
484 story.

Table 6: General Election Turnout, 2010 – 2018

	All Matched Observations				Registration Date prior to Release Date			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2018	0.094*** (0.0004)	0.094*** (0.0004)	0.095*** (0.001)	0.095*** (0.001)	0.055*** (0.0005)	0.055*** (0.0005)	0.081*** (0.001)	0.081*** (0.001)
Treated	-0.058*** (0.001)	-0.060*** (0.001)	-0.073*** (0.001)	-0.075*** (0.001)	-0.056*** (0.001)	-0.064*** (0.001)	-0.065*** (0.001)	-0.068*** (0.001)
Years Since Latest Incarceration			0.00000 (0.0001)	-0.00004 (0.0001)			0.013*** (0.0001)	0.003*** (0.0001)
2018 × Treated	-0.022*** (0.001)	-0.022*** (0.001)	-0.038*** (0.001)	-0.038*** (0.001)	-0.033*** (0.001)	-0.033*** (0.001)	-0.048*** (0.002)	-0.048*** (0.002)
2018 × Years Since			-0.0001 (0.0001)	-0.0001 (0.0001)			-0.004*** (0.0001)	-0.004*** (0.0001)
Treated × Years Since			0.002*** (0.0001)	0.002*** (0.0001)			0.001*** (0.0002)	0.001*** (0.0002)
2018 × Treated × Years Since			0.002*** (0.0002)	0.002*** (0.0002)			0.002*** (0.0002)	0.002*** (0.0002)
Constant	0.478*** (0.001)	0.011*** (0.004)	0.478*** (0.001)	0.012*** (0.004)	0.575*** (0.001)	-0.047*** (0.005)	0.494*** (0.001)	-0.059*** (0.005)
Includes covariates from matching		X		X		X		X
Congressional District fixed effects		X		X		X		X
Observations	7,388,640	7,388,640	7,388,640	7,388,640	4,915,920	4,915,920	4,915,920	4,915,920
R <sup>2</sup>	0.008	0.199	0.009	0.199	0.005	0.157	0.023	0.157
Adjusted R <sup>2</sup>	0.008	0.199	0.009	0.199	0.005	0.157	0.023	0.157

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

Each model in Table 6 identifies a negative treatment effect. The coefficients on *2018 × Treated* in models 1 and 2 indicate that turnout among treated voters was about 2.2 percentage points below what it would have been if the gap between treated and control voters in 2018 had conformed to prior years. This mirrors the findings from the neighborhood-level analyses, where the number of formerly incarcerated residents is not associated with higher turnout.

There is some indication that spillover effects lessen with time. In each model, *2018 × Treated × Years Since* and *Treated × Years Since* is positive and statistically significant. In other words, individuals whose housemates had not been imprisoned for 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 an individual whose household member returned from

496 prison within one year of the election was about -3.8 percentage points. For each year the  
497 most recent incarceration recedes into the past, the treatment effect decreases by about 0.2  
498 points in years other than 2018, and by 0.4 points in 2018. That the spillover effects “decay”  
499 is a positive sign, and indicates that the negative socialization induced by a housemate’s  
500 incarceration might not be permanent.

501 It is unsurprising that the effect is moderated by time. Individuals whose household members  
502 went to and were released from prison between the 2016 and 2018 elections, for instance,  
503 received two treatments: they both were “negatively” treated by the incarceration of their  
504 housemate and potentially “positively” treated by Amendment 4. What *is* surprising, how-  
505 ever, is the continued negative treatment effect even for the households furthest removed  
506 from the incarceration of a household member. Table 7 presents the results of models 5  
507 and 6 from Table 6, but limits the pool to households where someone last returned home  
508 from prison prior to 2010. The “negative” treatment for these individuals should be reflected  
509 in the base years of the difference-in-differences models. In these models,  $2018 \times Treated$   
510 remains significant and negative. The neighborhood-level analyses indicate that the amount  
511 of time that has elapsed since an individual’s incarceration is also related to support for and  
512 the salience of Amendment 4; similar processes may be at play here, but the individual-level  
513 data does not allow us to explore them.

Table 7: General Election Turnout, 2010 – 2018

	(1)	(2)
2018	0.031*** (0.001)	0.031*** (0.001)
Treated	-0.048*** (0.002)	-0.057*** (0.002)
2018 × Treated	-0.020*** (0.002)	-0.020*** (0.002)
Constant	0.656*** (0.001)	-0.011 (0.012)
Includes covariates from matching		X
Congressional District fixed effects		X
Observations	1,524,000	1,524,000
R <sup>2</sup>	0.003	0.102
Adjusted R <sup>2</sup>	0.003	0.102

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Robust standard errors (clustered at level of match) in parentheses.

514 These negative, statistically significant findings at the individual and neighborhood level  
515 should probably not be interpreted to mean that Amendment 4 had a demobilizing effect  
516 on individuals whose family and community members would be re-enfranchised by its pas-  
517 sage. Rather, it likely highlights that these individuals are less susceptible to other broadly  
518 mobilizing phenomena. The 2018 election saw higher participation than any midterm in  
519 a century as many infrequent voters turned out. It appears that voters whose household  
520 members have been to prison were less mobilized by the factors that encouraged other de-  
521 mographically similar voters to participate in 2018. This analysis cannot determine whether  
522 their indirect exposure to the criminal justice system caused this imperviousness, or if they  
523 would have remained on the sidelines in 2018 even if their household members had not been  
524 imprisoned. Nevertheless, that their turnout in 2018 did not increase relative to other voters  
525 — even with Amendment 4 on the ballot — underscores just how difficult their political

526 (re)integration is.

## 527 **Discussion and Conclusion**

528 Turnout in 2018 hit historic levels for a midterm election as infrequent voters participated  
529 and made their voices heard. In addition to hotly contested Congressional, senate, and  
530 gubernatorial races, Floridians were presented with the opportunity to restore voting rights  
531 to well over a million permanently disenfranchised individuals who had been convicted of  
532 felony offenses. Amendment 4 and its organizers were hugely successful — in a year where  
533 both statewide winners won by less than 0.5 percentage points, nearly two-thirds of Floridians  
534 supported expanding the franchise. Neighborhoods and voters most directly impacted by  
535 felony disenfranchisement gained meaningful political representation from the passage of the  
536 amendment, and one of the “durable markers” of their civil death was nullified. However, I  
537 fail to uncover evidence that Amendment 4 itself increased the turnout of neighborhoods and  
538 individuals in close proximity to the formerly incarcerated above-and-beyond the increases  
539 observed among other voters and in other communities.

540 It is not immediately apparent why Amendment 4 did not disproportionately heighten mo-  
541 bilization among these voters. The current study cannot tell whether it was an issue of lower  
542 political knowledge, or because the legal estrangement of the carceral state runs too deep  
543 for a single ballot initiative to overcome. However, if estrangement was the reason that the  
544 ballot initiative failed to mobilize these voters, this was likely only reinforced in the after-  
545 math of the 2018 election. After the state constitution was amended to re-enfranchise their  
546 family members and neighbors, legislators rewrote the law to exclude them anew.

547 Just months after the 2018 election the Florida legislature passed a bill requiring disen-  
548 franchised individuals to pay off all court-ordered financial obligations before registering to  
549 vote, despite the fact that the state was incapable of determining how much any individual  
550 actually owed (Stern 2019). A federal judge ruled the law unconstitutional in May of 2020,



551 arguing that conditioning voting rights on the repayment of obligations that individuals can-  
552 not afford amounted to a poll tax and violation of the 24th Amendment.<sup>11</sup> That September,  
553 however, an en bank ruling by the U.S. Court of Appeals for the 11th Circuit overturned  
554 that decision,<sup>12</sup> upholding the constitutionality of the law. In his dissent from the Eleventh  
555 Circuit’s ruling, Judge Adalberto Jordan noted that “[h]ad Florida wanted to create a sys-  
556 tem to obstruct, impede, and impair the ability of felons to vote under Amendment 4, it  
557 could not have come up with a better one” and that “Florida cannot tell felons — the great  
558 majority of whom are indigent — how much they owe... and has come up with conflicting  
559 (and uncodified) methods for determining how LFO [legal financial obligation] payments by  
560 felons should be credited.” That Florida legislators would condition voting on criteria that  
561 cannot be verified, or cannot be afforded, has understandably been described as “unfair [and]  
562 heartbreaking” by one disenfranchised individual who said the amendment had promised to  
563 “give me a voice in my own future” (Harris 2020). It remains to be seen how such legis-  
564 lation and litigation will inform how criminal justice-involved individuals understand their  
565 relationship with the state and structure their future democratic participation.

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

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<sup>11</sup>Jones et al. v. DeSantis et al., 4:19cv300-RH/MJF (U.S. District Court for the Northern District of Florida 2020).

<sup>12</sup>Jones et al. v. DeSantis et al., 4:19cv300-RH/MJF (United States Court of Appeals for the Eleventh Circuit).

575 **Declarations**

576 The author affirms this research did not directly involve human subjects. It was conducted  
577 in accordance with the Brennan Center for Justice’s Quantitative Research Protocol.

578 The author declares no ethical issues or conflicts of interest in this research. This research  
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580 Research documentation and data that support the findings of this study are openly available  
581 in the APSR Dataverse at <https://doi.org/10.7910/DVN/A81LPK>. Limitations on data  
582 availability are discussed in the text.

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