

# Rethinking Felony Disenfranchisement: The Case of New York City and Miami

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“There is no Negro problem. There is no Southern problem. There is no Northern problem. There is only an American problem”

*-Lyndon Johnson, March 15, 1965*

## Introduction

Felony disenfranchisement has received much attention over the past few years. This is true in both the academic world (where researchers have dug into the political ramifications in such races as the 2000 presidential election) and in the political world as states such as Florida and Louisiana have gradually moved to dismantle their systems of disenfranchisement. Throughout this process, however, many researchers have focused on the *individual* effects of felony disenfranchisement. They have examined the effect of disenfranchisement on the political participation of individuals, including after they are no longer disenfranchised, but have generally neglected to examine the intersection of spatially concentrated policing and incarceration patterns with the political effects of disenfranchisement. This paper seeks to increase our understanding of the spatial implications of felony disenfranchisement by examining the character of disenfranchisement in New York City and Miami-Dade County in Florida.

This paper also seeks to complicate the narratives surrounding the end of felony disenfranchisement. Although advocates are correct to push for an end to disenfranchisement, a change to the political system may not be sufficient to reincorporate the voices of formerly incarcerated individuals into the democratic process. In addition to laying out the spatial implications of felony disenfranchisement in two major American cities, this paper also explores the impact of two different policies intended to dismantle the disenfranchisement program. Specifically, I investigate the effect of Executive Order 181 (which ended the disenfranchisement of parolees in New York State) and Amendment 4 (which restored the voting rights of Floridians who had completed all terms of their sentences). The effects of these policies make clear that, while the end of felony disenfranchisement is necessary, active steps must be taken to reincorporate the formerly disenfranchised.

## Background of Felony Disenfranchisement in the United States

In all but two states (Maine and Vermont), felony disenfranchisement laws ensure that American citizens convicted of felony offenses lose the right to vote for at least some period of time. In some states, such as Oregon and Massachusetts, individuals lose that right only for the period in which they are actively incarcerated. In other states, notably Kentucky and Iowa, felony convictions result in lifelong disenfranchisement unless a returned citizen receives an individual pardon from the state’s governor (Brennan Center for Justice 2018). This variation in laws flows directly from language in the Fourteenth Amendment which allows states to revoke individuals’ voting rights “for participation in rebellion, or other crime.” The definition of “other crime,” left so vague in the Constitution, is now generally used by states to encompass any felony offense at all. The Supreme Court, in cases such as *Richardson v. Ramirez*, has upheld states’ right to do just that. Collectively, these laws disenfranchise as many as 4.7 million American citizens. Of these, the majority

are no longer incarcerated, but are living and working in their communities (Uggen, Larson, and Shannon 2016).<sup>1</sup>

In any discussion of felony disenfranchisement in the United States, it is imperative to acknowledge the central role played by race and white supremacy. As Traci Burch has explained, “If policies restricting the voting rights of offenders disparately affect one racial group or party, it is because such policies were intended to” (Burch 2010). The historical record is undeniable. Previous research established that the presence of nonwhite potential voters is associated with the implementation of felony disenfranchisement policies and that these policies were often adopted during Jim Crow to reduce the political power of Black Americans (Behrens, Uggen, and Manza 2003). In Florida felony disenfranchisement was added to the state constitution in 1868. Afterwards, a lawmaker boasted that the effort had been made in order to prevent Florida from being “niggerized” (Shofner 1963).

The racial imbalance of felony disenfranchisement laws are not confined to the 19th century. Although the Voting Rights Act of 1965 did much to improve access to the ballot box for minorities, it did nothing to undermine the explicitly racialized system of disenfranchisement. Indeed, as the United States has vastly increased the reach of the carceral system in the post-Civil Rights era, the implications of felony disenfranchisement have only grown. As of 2016, more than 10 percent of African Americans were disenfranchised in 9 states. In Kentucky, the state with the highest level of disenfranchised Black residents, more than one in four Black adults are barred from casting a ballot. Although Black adults made up just 12.1% of the voting age population in 2016, they accounted for 36.5% of the disenfranchised population (Uggen, Larson, and Shannon 2016).

## Academic Literature and Felony Disenfranchisement

In the aftermath of the 2000 presidential election, academic interest in the political implications of felony disenfranchisement was stirred thanks to a paper from Christopher Uggen and Jeff Manza (2002). George W. Bush’s margin of victory in Florida in 2000 was famously just 537 votes. In their 2002 paper, Uggen and Manza estimate the likely partisan composition of the disenfranchised population with felony convictions in their past. They estimate that, if this group had been allowed to vote, they would have supported Al Gore. Their enfranchisement, Uggen and Manza posit, would have tipped the presidential contest and resulted in the election of Al Gore. They based their estimates on the voting patterns of *non*-disenfranchised individuals who were demographically similar to the disenfranchised population. Though much of the research conducted since their 2002 study has undermined some of their key assumptions (namely, that formerly incarcerated individuals turnout and cast ballots similarly to those who have not been incarcerated), Uggen and Manza convincingly demonstrated that felony disenfranchisement can have material political consequences. In the years after the Uggen and Manza published their paper, scholars sought to investigate the effect of felony disenfranchisement on African American turnout (Miles 2004), young adult turnout (Hjalmarsson and Lopez 2010), and other questions. Some of this research compared states and regions with differing disenfranchisement regimes to estimate these effects (Miles 2004; Ochs 2006). Others have used survey data or interviews to construct their estimates (Uggen and Manza 2004; Drucker and Barreras 2005).

In a series of papers in between 2009 and 2011, researchers developed methods for directly estimating the turnout of formerly disenfranchised individuals. Haselswerdt (2009) matched release data and voter registration data from Erie County, NY, to estimate turnout among a small group of formerly incarcerated individuals. Traci Burch (2010, 2011) expanded upon this matching methodology to estimate the voting patterns of formerly disenfranchised individuals in a range of states. She uses release data from states’ Departments of Corrections and their Registered Voter Files to estimate the number of formerly incarcerated individuals who went on to register to vote. Using the Registered Voter Files, she is also able to estimate the party affiliation of formerly incarcerated individuals (in states with party registration) and the turnout rate among these individuals. Her methodology has been used to investigate other questions surrounding

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<sup>1</sup>The figures reported in Uggen, Larson, and Shannon (2016) have been adjusted to reflect the impact of Amendment 4 in Florida.

the voting patterns of formerly incarcerated individuals under different circumstances and to examine the impact of changes in disenfranchisement policy (Meredith and Morse 2013, 2015).

A number of papers have also explored the impact that felony disenfranchisement policies have on turnout among non-disenfranchised residents. King and Erickson (2016), for instance, leverage state-level variation in disenfranchisement laws to estimate the impact that felony disenfranchisement has on turnout among African Americans. They use data from the 2004 Current Population Survey Voting and Registration Supplement to determine individual-level turnout. They include estimates of the share of African Americans who are disenfranchised in each state from Manza and Uggen (2006) to explore the impact of these policies on eligible voters. Ultimately, they argue that “African American disenfranchisement plays a unique role in predicting African American voter turnout.” They conclude that disenfranchisement has spillover effects for African American voters: where more African Americans are disenfranchised, eligible African Americans are less likely to cast a ballot. These findings are in line with other research that has explored whether the effects of disenfranchisement extend beyond those whose voting rights are directly suspended (Bowers and Preuhs 2009; Ochs 2006; Walker 2014). As Bowers and Preuhs (2009) sum up: “[I]t is not solely the direct vote of ex-felons that is denied through these laws. [Felony disenfranchisement] impacts the political power of communities that extends beyond felons’ collateral penalty.”

These findings are not surprising given the large body of research establishing the social nature of voting. Outside the felony disenfranchisement conversation, an enormous body of research has demonstrated that individuals are far more likely to cast a ballot if their peers, family members or neighbors are doing so. [Build this out]

Although scholars have established that felony disenfranchisement decreases turnout among Black voters at the *state* level, relatively little research has been done on how felony disenfranchisement operates at the sub-state level. Though we know that Black voters are generally less likely to cast a ballot when they live in a state with strict disenfranchisement laws, less work has been done exploring the types of neighborhoods in which these effects are concentrated, and the impact these laws might have at the local level. Burch (2013) is an exception to this. In this paper, Burch explores the depressive effect of disenfranchisement laws at the local level in North Carolina by examining census block-group level turnout and involvement with the criminal justice system. She determines that “at high concentrations, imprisonment and community supervision have an unequivocally demobilizing effect of neighborhoods.” This paper seeks to expand on her work by replicating her findings in New York City and by introducing a new way of measuring the impact.

## Spatial Effects of Concentrated Policing and Incarceration

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### Framing for Turnout Effects

Although understanding whether felony disenfranchisement has impacts on turnout among different social groups (namely, racial minorities) is of great importance, understanding the explicitly spatial dimension of the problem is also an important task. Many political decisions bear on the spatial distribution of resources (XX); funding for parks or schools, for instance, can impact all members of a neighborhood, regardless of their race or their income. As such, even a resident who is not directly affected by felony disenfranchisement policies may lose political power thanks to these policies; if many of my neighbors cannot vote, I am joined by fewer voters in advocating for neighborhood-specific investment and services. This is, of course, tied up closely with race; New York City remains highly racially segregated, and therefore disentangling neighborhood effects from race effects becomes difficult. Nevertheless, these effects cross race lines in integrated neighborhoods.

Of course, despite dire over-incarceration in some neighborhoods, the number of incarcerated individuals is relatively low compared to the number of voters. In New York, for instance, 46,232 individuals were imprisoned in New York State in early 2019, compared with XX actively registered voters. Despite the lower

share of residents who are disenfranchised through imprisonment, there is reason to believe the policy impacts more individuals than just those imprisoned. As discussed above, previous research has demonstrated that felony disenfranchisement reduces turnout even among Black voters whose rights are not stripped. This research has found, in particular, that eligible Black voters are less likely to cast a ballot in states where felony disenfranchisement policies are harsher. Understanding the distribution of this effect is important for understanding its political implications. Are voters who live in the same neighborhood as the disenfranchised less likely to cast a ballot because their neighbors have been removed from the community? Or does felony disenfranchisement operate cast its chilling effect on turnout more broadly, crossing even into neighborhoods where voters have not been removed from the community?

This paper begins by examining whether felony disenfranchisement is in fact associated with lower turnout rates in a given neighborhood.<sup>2</sup> As discussed above, this depressive effect on turnout is expected to operate through two distinct mechanisms: the actual incarceration and disenfranchisement of convicted individuals, and the social effects on eligible voters identified in previous research. These turnout effects are estimated in various ways: by comparing neighborhoods where voters with a history of participating have been incarcerated with similar neighborhoods where voters have not been incarcerated; by using regression analysis to investigate whether neighborhood arrests are correlated with turnout rates; and by using regression analysis to investigate the relationship between prison admissions and turnout at the zip-code level.

## What This Paper Examines

This paper begins by examining the effect of felony disenfranchisement in the New York City Mayoral election of 2017. This paper is primarily interested in whether felony disenfranchisement has impacts on local political distribution. If, for instance, voters in particular marked neighborhoods turnout at lower rates because of disenfranchisement policies, this could have ramifications for city-wide policy. Martin and Claibourn (2013), for instance, documents the relationship between voter turnout and the responsiveness of Congressional representatives. “[H]igher citizen participation is rewarded,” they argue, “with enhanced policy responsiveness.” If the neighborhoods most impacted by felony disenfranchisement turn out at lower rates, they may find that their elected representatives are less likely to support their needs. Griffin and Newman (2005) find similar effects in the United States Senate, reporting that “voter preferences predict the aggregate roll-call behavior of Senators while nonvoter preferences do not.”

Although research on the impact of local turnout on city-wide policy is scarce, Anzia (2019) examines the impact of senior turnout on “senior-friendly” policy at the city level. Anzia does not find that senior turnout in general increases the likelihood of senior-friendly policies, but that elected officials *are* responsive to senior turnout when seniors “are a more cohesive, meaningful group.” American cities are highly segregated by race and by class, but less so by age. As such, Anzia’s study does not speak directly to the impact of *neighborhood* turnout rates on city policy. An extension of her paper, however, implies that what she finds might be probitive to the effect of neighborhood turnout rates. Insofar as neighborhoods vote cohesively on issues such as school and park funding, their turnout rates may influence city policy.

After examining the effect of felony disenfranchisement on voter turnout at the neighborhood level, I consider the efficacy of a program intended to undo the impact of felony disenfranchisement. In April of 2018, Governor Andrew Cuomo signed Executive Order 181 which restored voting rights to New Yorkers on parole.<sup>3</sup> Parole officers were required to provide registration forms the parolees under their supervision and inform them of their voting rights. In theory, such a policy could increase turnout and registration for these individuals. To test whether the policy was successful, I compare the turnout rates in the 2018 general election of three groups who finished parole in 2018: those who finished parole immediately before the policy change, those who finished parole immediately after the policy change, and those who finished parole immediately after the election. These three groups allow me to explore whether the re-extension of voting rights while on parole

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<sup>2</sup>As discussed below, “neighborhood” is difficult to define. I explore turnout differentials at both the census block group and census tract level.

<sup>3</sup>Prior to Executive Order 181, residents in prison and on parole were barred from voting, while individuals sentenced to probation did not have their voting rights revoked. Today, only those in prison are disenfranchised

has an impact on turnout (by comparing the first two groups) and whether being under formal supervision while being allowed to vote results in higher turnout than being allowed to vote but being under supervision (by comparing the third group to the first two).

## Data

### Criminal Justice Data

This paper employs multiple data sets to investigate the extent to which a neighborhood is directly impacted by felony disenfranchisement. The primary data set comes from a freedom of information request filed by the author to obtain individual-level incarceration records for individuals sentenced to incarceration in New York State since 1992. The data includes a host of information, including: first and last name; date of birth; class of offense; incarceration start and end dates; dates of parole; and others. This analysis is limited to individuals incarcerated for felony offenses. Individuals convicted of misdemeanors are not disenfranchised in New York State.

The City of New York also makes the location of all NYPD arrests since 2006 available publicly. This data includes the latitude and longitude of the arrest, the date of the offense, and the class of the alleged offense. This analysis uses only arrests for felony charges.

The third dataset used to estimate the extent to which a neighborhood is directly impacted by felony disenfranchisement policies in New York State comes from another freedom of information request. The individual-level incarceration data referenced above does not include information about the neighborhood an incarcerated person lived in prior to their conviction. Although the State of New York does not make this information available at the individual level, the author has received prison-admission counts by zip-code for the years XX - XX.

The state does not make a unified database of parolees whose voting rights have been restored available to the public. However, the Department of Corrections and Community Supervision website's Parolee Lookup includes a flag indicating whether someone's voting rights have been restored. By using identification numbers from the data obtained from the state to lookup parolees on the website, I was able to construct a list of the individuals who were on parole and had their rights restored.

### Voter File Data

Most states in the United States are required to maintain files with information on all registered voters in the state. In New York, this information is publicly available from the Board of Elections. It includes information on all registered voters, including: first and last name; date of birth; home address; vote history; and other information. The New York State Voter File also includes information on voters who were previously registered but have since been purged, either because they moved, died, or were incarcerated for a felony offense. Because the voter file includes information on voters who have been purged because of a felony conviction, we are able to reconstruct the vote history of individuals who are no longer registered because of their conviction.

### Geocoding

Voters' home addresses were converted to latitudes and longitudes using a geocoder provided by SmartyStreets. These latitudes and longitudes were then converted to census block groups using the statistical program R and shapefiles available from the United States Census Bureau.

# Effects of Felony Disenfranchisement on Neighborhood Turnout Levels

## Identification of Lost Voters

In this analysis, I offer a slightly different definition of “lost voter” than much of the literature. Many recent papers have attempted to identify relationships between the number of *potentially* lost voters and turnout. Here, I explore whether the disenfranchisement of voters with a history of participating in elections is related to neighborhood turnout. In this analysis, “lost voters” are individuals ineligible to cast a ballot on a given election day who have cast a ballot in the previous ten years. To study the effect of felony disenfranchisement on voting at the local level (and its potential implications for the distribution of political power at the local level), I use turnout in the most recent election for city-wide office in New York - the mayoral election which took place on November 7<sup>th</sup>, 2017. Lost voters, therefore, are all individuals who were incarcerated or on parole on November 7<sup>th</sup>, 2017, and had cast a ballot between 2007 and 2016.

These individuals are identified by matching Department of Corrections and Community Supervisions data to the voter file using the methodology developed by Burch (2010, 2011). I use first, middle, and last names, and dates of birth, to join these two datasets.<sup>4</sup> Figure 1 shows where these lost voters lived before going to prison, with city council districts also included. There were 772 such lost voters within New York City as of the 2017 general election, and 3,714 statewide.

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<sup>4</sup>Matching on first and last names and dates of birth can result in false positive matches, especially in large states like New York. Following Meredith and Morse (2013), I report estimates of the potential impact of these false positives in the Appendix.

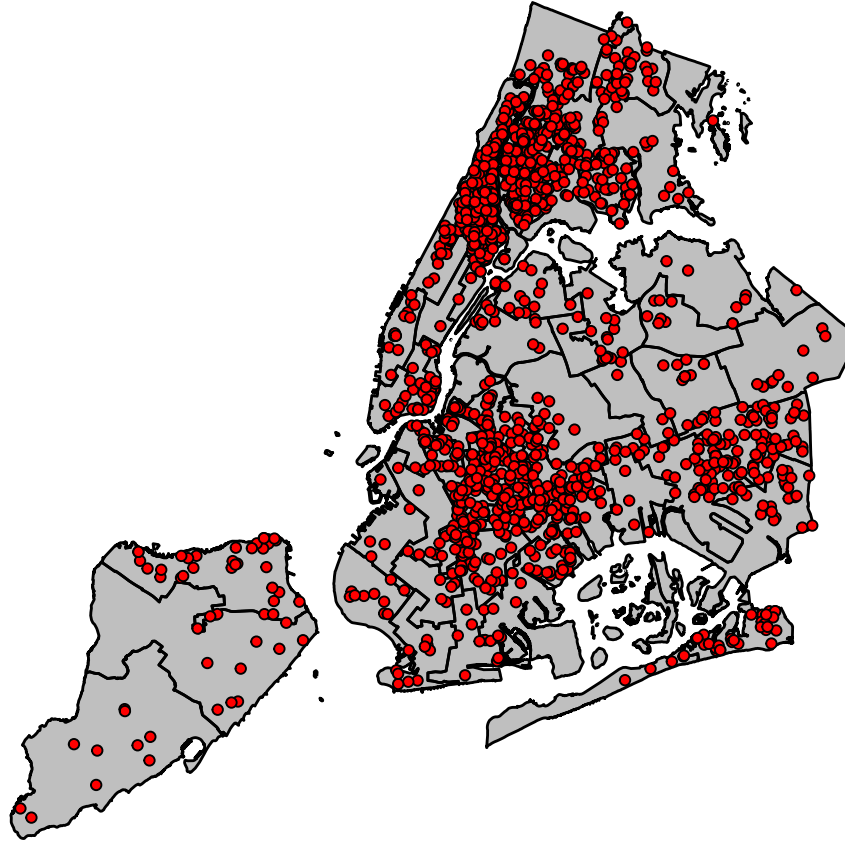


Figure 1: Lost Voters on Election Day, 2017

The spatial concentration of lost is readily apparent. In some communities, such as Greenwich Village and Brooklyn Heights, hardly any voters were disqualified from participating in the 2017 elections. In other communities, such as Harlem and Central Brooklyn, large numbers of individuals were not allowed to cast a ballot for mayor.

## Testing for Neighborhood Turnout Effects

To test whether neighborhoods with lost voters had systematically lower turnout rates than similar neighborhoods without such disqualifications, I begin by using a matching model. Neighborhoods (defined as census tracts and block groups) are considered treated if they were home to *at least one* lost voter in the 2017 election; they are untreated if no residents were disqualified from the election because of a felony conviction. I use a genetic match to match treated to untreated census tracts and block groups. I match on a series of demographic and political indicators. Estimates of racial characteristics, median income, education, age, population, and share noncitizen<sup>5</sup> come from the Census Bureau. Party affiliation rates come from the geocoded voter file. Registration rate is calculated by dividing the number of registered voters (from the voter file) by the voting age population. I include voteshare won by the winning city council representative in 2017 as a proxy for the competitiveness of the local race. All else equal, a more competitive race is likely

<sup>5</sup>The Census Bureau does not make noncitizen estimates available at the block group level. As such, block groups are assigned their census tract's share noncitizen for matching purposes.

to result in higher turnout.<sup>6</sup> These data come from the New York City Board of Elections. Each treated block group is matched to 30 untreated block groups, and each treated census tract is matched with 10 other tracts. Matching is done with replacement. Tables 1 and 2 present the results of these matches.

## Match Output

Table 1: Results of Block Group-Level Matching

|                                      | Means: Unmatched Data |           | Means: Matched Data |           | Percent Improvement |         |          |         |
|--------------------------------------|-----------------------|-----------|---------------------|-----------|---------------------|---------|----------|---------|
|                                      | Treated               | Control   | Treated             | Control   | Mean Diff           | eQQ Med | eQQ Mean | eQQ Max |
| % Latino                             | 0.34                  | 0.27      | 0.34                | 0.34      | 92.45               | 90.04   | 84.65    | 73.75   |
| % Non-Hispanic Black                 | 0.41                  | 0.20      | 0.41                | 0.41      | 98.86               | 97.34   | 96.38    | 91.90   |
| % Non-Hispanic White                 | 0.15                  | 0.36      | 0.15                | 0.15      | 99.67               | 96.82   | 95.50    | 90.62   |
| Median Income                        | 50,512.33             | 69,344.41 | 50,512.33           | 51,491.85 | 94.80               | 88.88   | 85.97    | 68.56   |
| % With Some College                  | 0.63                  | 0.69      | 0.63                | 0.64      | 79.13               | 80.58   | 79.41    | 71.87   |
| Median Age                           | 35.71                 | 37.98     | 35.71               | 35.57     | 93.72               | 37.29   | 57.11    | 64.11   |
| Registration Rate                    | 0.83                  | 0.77      | 0.83                | 0.82      | 77.35               | 82.66   | 81.22    | 70.10   |
| % Democrats                          | 0.77                  | 0.66      | 0.77                | 0.76      | 97.33               | 97.02   | 95.41    | 91.51   |
| % Noncitizen                         | 0.16                  | 0.16      | 0.16                | 0.16      | 85.83               | 29.72   | 16.13    | 15.20   |
| % Won by City Council Representative | 0.86                  | 0.81      | 0.86                | 0.86      | 87.88               | 80.27   | 76.53    | 70.24   |

Table 2: Results of Tract-Level Matching

|                                      | Means: Unmatched Data |           | Means: Matched Data |           | Percent Improvement |         |          |         |
|--------------------------------------|-----------------------|-----------|---------------------|-----------|---------------------|---------|----------|---------|
|                                      | Treated               | Control   | Treated             | Control   | Mean Diff           | eQQ Med | eQQ Mean | eQQ Max |
| % Latino                             | 0.33                  | 0.25      | 0.33                | 0.33      | 97.92               | 86.30   | 84.04    | 69.64   |
| % Non-Hispanic Black                 | 0.39                  | 0.18      | 0.39                | 0.39      | 99.00               | 95.16   | 94.75    | 90.07   |
| % Non-Hispanic White                 | 0.17                  | 0.38      | 0.17                | 0.17      | 99.17               | 95.38   | 92.01    | 79.48   |
| Median Income                        | 51,228.35             | 68,481.95 | 51,228.35           | 52,068.32 | 95.13               | 88.54   | 87.59    | 80.73   |
| % With Some College                  | 0.64                  | 0.70      | 0.64                | 0.64      | 92.89               | 93.15   | 91.13    | 76.70   |
| Median Age                           | 35.52                 | 37.70     | 35.52               | 35.30     | 89.96               | 70.09   | 83.02    | 82.46   |
| Registration Rate                    | 0.78                  | 0.74      | 0.78                | 0.77      | 72.40               | 85.25   | 77.88    | 56.64   |
| % Democrats                          | 0.76                  | 0.65      | 0.76                | 0.75      | 95.63               | 95.19   | 93.66    | 87.59   |
| % Noncitizen                         | 0.16                  | 0.16      | 0.16                | 0.16      | 75.41               | 32.52   | 13.00    | -7.80   |
| % Won by City Council Representative | 0.86                  | 0.80      | 0.86                | 0.86      | 84.46               | 85.66   | 76.22    | 69.63   |

At both the tract and block group level, matching results in an untreated group of neighborhoods that looks substantially like our treatment group. These tables also demonstrate the striking extent to which neighborhoods with lost voters differ from the average neighborhood in New York City. Neighborhoods with lost voters are substantially less white, have far lower median incomes, and a larger share of voters are registered as Democrats.

After matching neighborhoods, I use a simple regression to test whether turnout in the 2017 mayoral election was different in areas with lost voters. Census tract and block group level turnout rates are calculated using the geocoded voter file. Each voter's record indicates whether the voter participated in the 2017 general election,<sup>7</sup> which are then aggregated to estimate the number of ballots cast in each neighborhood. The number of ballots cast is divided the voting age population.

Much of the literature has discussed whether felony disenfranchisement is particularly demobilizing for eligible Black voters. I therefore include models which explore any potential difference in treatment effect in neighborhoods where a higher share of the population is Black. Table 3 presents the results of these regression models.

<sup>6</sup>Where neighborhoods cross council district lines, this measure is the weighted average of the competitiveness faced by each voter in the neighborhood

<sup>7</sup>The New York State voter file is not without errors. For instance, the voter file erroneously does not record the author as participating in the 2017 mayoral election. However, insofar as these errors are not geographically correlated, there is little reason to believe they impact the results of this analysis. Unfortunately, testing the distribution of these errors is not possible.



Table 3: Matching Regression

|                              | Turnout Rate         |                      |                     |                     |
|------------------------------|----------------------|----------------------|---------------------|---------------------|
|                              | Block Group Level    |                      | Tract Level         |                     |
|                              | Model 1              | Model 2              | Model 3             | Model 4             |
| D(Neighborhood Lost a Voter) | -0.008***<br>(0.003) | 0.005<br>(0.004)     | -0.005<br>(0.004)   | 0.004<br>(0.006)    |
| Share Non-Hispanic Black     |                      | -0.003<br>(0.006)    |                     | -0.014<br>(0.008)   |
| D(Lost Voter) * Share Black  |                      | -0.032***<br>(0.009) |                     | -0.023*<br>(0.012)  |
| Constant                     | 0.131***<br>(0.002)  | 0.132***<br>(0.003)  | 0.126***<br>(0.003) | 0.132***<br>(0.004) |
| N                            | 4452                 | 4452                 | 1643                | 1643                |
| R-squared                    | 0.002                | 0.010                | 0.001               | 0.013               |
| Adj. R-squared               | 0.002                | 0.009                | 0.0004              | 0.011               |

\*\*\*p < .01; \*\*p < .05; \*p < .1

When neighborhoods are measured at the block group level, a lost voter is negatively associated with turnout in the 2017 election. In block groups with lost voters, turnout was on average 0.8 percent lower than in comparable block groups without lost voters. This decrease, however, appears to be entirely concentrated within Black block groups. When the dummy identifying neighborhoods with lost voters is interacted with the share of the neighborhood that is Non-Hispanic Black, the basic treatment dummy becomes insignificant ( $p = 0.24$ ). The coefficient on the interaction between treatment and share Black indicates that neighborhoods that are largely Black saw turnout that was as much as 3 percent lower than similar neighborhoods without lost voters. These variables are largely insignificant at the census tract level. This is not particularly surprising. If lost voters have the largest depressive effect on family members and close neighbors, it makes sense that the effect of losing a voter is locally concentrated. ### Testing Intensity Effects {-}

Matching methodologies, of course, only allow us to test the effect of being treated - here, losing a voter for the 2017 election. The model above does not allow for different effects on turnout based on *how many* voters a neighborhood lost. In Table 4 below, I adopt a standard regression to investigate whether lost voters are associated with lower turnout rates in the 2017 election.

Table 4: Standard Regression

|                                            | Turnout Rate           |                        |                       |                      |
|--------------------------------------------|------------------------|------------------------|-----------------------|----------------------|
|                                            | Block Group Level      |                        | Tract Level           |                      |
|                                            | Model 1                | Model 2                | Model 3               | Model 4              |
| Lost Voters                                | -0.011***<br>(0.003)   | 0.001<br>(0.005)       | -0.008***<br>(0.002)  | 0.001<br>(0.004)     |
| Lost Voters * Share Black                  |                        | -0.027***<br>(0.009)   |                       | -0.022***<br>(0.007) |
| Median Income (Thousands of Dollars)       | 0.0002***<br>(0.00004) | 0.0002***<br>(0.00004) | 0.0002***<br>(0.0001) | 0.0002**<br>(0.0001) |
| Percent Latino                             | 0.060***<br>(0.007)    | 0.058***<br>(0.007)    | 0.075***<br>(0.011)   | 0.071***<br>(0.011)  |
| Percent Non-Hispanic Black                 | 0.052***<br>(0.008)    | 0.056***<br>(0.008)    | 0.069***<br>(0.012)   | 0.077***<br>(0.012)  |
| Percent Non-Hispanic White                 | 0.101***<br>(0.008)    | 0.102***<br>(0.008)    | 0.119***<br>(0.012)   | 0.119***<br>(0.012)  |
| Percent With Some College                  | -0.0004<br>(0.010)     | -0.0004<br>(0.010)     | -0.028<br>(0.019)     | -0.026<br>(0.019)    |
| Median Age                                 | 0.002***<br>(0.0001)   | 0.002***<br>(0.0001)   | 0.002***<br>(0.0002)  | 0.002***<br>(0.0002) |
| Registration Rate                          | 0.190***<br>(0.004)    | 0.190***<br>(0.004)    | 0.198***<br>(0.007)   | 0.197***<br>(0.007)  |
| Percent Democrats                          | -0.083***<br>(0.013)   | -0.084***<br>(0.013)   | -0.084***<br>(0.022)  | -0.088***<br>(0.022) |
| Percent Noncitizen                         | -0.096***<br>(0.015)   | -0.095***<br>(0.015)   | -0.085***<br>(0.022)  | -0.083***<br>(0.022) |
| Percent Won by City Council Representative | -0.021***<br>(0.007)   | -0.021***<br>(0.007)   | -0.013<br>(0.010)     | -0.014<br>(0.010)    |
| Constant                                   | -0.059***<br>(0.013)   | -0.058***<br>(0.013)   | -0.086***<br>(0.021)  | -0.083***<br>(0.021) |
| N                                          | 5803                   | 5803                   | 2089                  | 2089                 |
| R-squared                                  | 0.474                  | 0.475                  | 0.521                 | 0.523                |
| Adj. R-squared                             | 0.473                  | 0.474                  | 0.518                 | 0.520                |

\*\*\*p &lt; .01; \*\*p &lt; .05; \*p &lt; .1

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