Neighborhoods and Felony Disenfranchisement: The Case of New York City

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1 Introduction

The political history of the United States has been characterized by a general, if nonlinear, trend toward universal suffrage (see, for instance, Keyssar 2009). At the time of the nation's founding, access to the ballot box was restricted to landed White men; over the following two centuries, the franchise was greatly expanded. Today, voting rights are considered foundational aspects of full citizenship (United Nations General Assembly Resolution 2200 (XXI)). Despite the United State's march toward ever-more-inclusive systems of democracy, however, one large group of American citizens is formally barred from voting. In most of the United States, citizens convicted of felonies are at least temporarily prohibited from casting ballots in elections (Brennan Center for Justice 2018). Although some states such as Florida and Louisiana have gradually moved to dismantle their systems of felony disenfranchisement, an estimated 4.7 million American citizens remain disenfranchised (Uggen, Larson, and Shannon 2016).

The disenfranchisement of citizens convicted of felony offenses intersects with the racialized and place-based patterns of policing and incarceration in the United States. Michelle Alexander (2012) and others have argued that mass incarceration in the post-Civil Rights era has been used to exert control over minority — and particularly Black — Americans. In states such as New York, for instance, the over-representation of minorities among the incarcerated is striking: according to data from the New York State Department of Corrections and Community Supervision, 49.5 percent of individuals who were incarcerated in December of 2018 were non-Hispanic Black, although the Census Bureau estimates that just 14.3 percent of the citizen voting age population in the state is non-Hispanic Black.[1] This disparity is likely due not to any inherent differential propensity to commit crimes among different racial groups, but rather to systems of policing and concentrated poverty. As Gelman, Fagan, and Kiss (2007) shows, for instance, New York's "stop-and-frisk" policy impacted Black and Latino New Yorkers at rates far higher than Whites, even after controlling for neighborhood variability and race-specific criminal propensity.

Due to economic and racial segregation, these effects are highly spatially concentrated. Data available from New York City shows that in 2017, 10 of the New York Police Department's 77 precincts were responsible for more than a quarter of all arrests for felony charges. Many scholars have detailed the impact of living in areas with high levels of police activity. Residents of such neighborhoods suffer from worse physical health (Sewell and Jefferson 2016) and are more likely to suffer from anxiety and exhibit symptoms of trauma (Geller et al. 2014). The labor markets and social networks in neighborhoods with high levels of policing and incarceration are disrupted (Clear 2008). Concentrated policing has also been credited with having a "chilling effect" on

[1] Latinos are also over-represented among the incarcerated population, though not as dramatically: Latinos make up 14.1 percent of the citizen voting age population and 22.9 percent of the incarcerated population.

neighborhoods' willingness to reach out for help to local governments (Lerman and Weaver 2013).

Felony disenfranchisement policies are part of a criminal justice system that disproportionately impacts Black Americans living in certain communities. The effects of disenfranchisement are concentrated in neighborhoods that already suffer from myriad disadvantages thanks to social and economic marginalization. The neighborhood-specific implications of felony disenfranchisement, however, largely remain unstudied. A number of studies have explored the effect of imprisonment and disenfranchisement on later political participation (White 2019; Gerber et al. 2014; Burch 2011). Others have looked at the spillover effects of disenfranchisement on eligible Black voters at the state level (Bowers and Preuhs 2009; King and Erickson 2016). With the exception of Burch (2013), however, little attention has been paid to the impact of felony disenfranchisement on political participation at the neighborhood level.

Filling this gap in the literature is of great importance when we consider the spatial concentration of policing networks. If the spillover effects of felony disenfranchisement are widely dispersed, the political consequences are likely minimal. If, however, the effects are both large enough to be detected at a statewide level (as the literature suggests) but also concentrated in a few neighborhoods, disenfranchisement laws likely severely undermine local political representation This lowered participation can disadvantage neighborhoods insofar as they have unique candidate preferences. It could also lower the resources distributed to these neighborhoods, as politicians determine that too few votes can be won through investing in these areas. This is not merely of theoretical concern: research indicates that turnout differentials at the city level has material consequences. As Hajnal (2009) tells us: "low [and uneven] turnout results in losses in mayoral elections, less equitable racial and ethnic representation on city councils, and spending policies that are less in line with the preferences of racial and ethnic minorities and other disadvantaged groups" (8).

1.1 Background

In the aftermath of the 2000 presidential election, academic interest in the political implications of felony disenfranchisement was stirred thanks to a paper from Uggen and 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 by a wide margin. Their enfranchisement, Uggen and Manza argued, would have tipped the presidential contest and resulted in the election of Al Gore. They based their estimates on the voting patterns of eligible individuals who were demographically similar to the disenfranchised population. Though much of the research conducted since

their 2002 study has pushed back against some of their key assumptions (namely, that formerly incarcerated individuals turn out at the same rate as demographically-similar individuals who have not been incarcerated), Uggen and Manza convincingly demonstrated that felony disenfranchisement can have material political consequences. In the years after Uggen and Manza published their paper, scholars sought to investigate the relationship between felony disenfranchisement and Black and youth turnout (Miles 2004; Hjalmarsson and Lopez 2010). 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).

A number of papers have also explored the impact felony disenfranchisement policies have on turnout among non-disenfranchised residents. King and Erickson (2016), for instance, leverages state-level variation in disenfranchisement laws to estimate the impact that felony disenfranchisement has on turnout among Black Americans. They use data from the 2004 Current Population Survey Voting and Registration Supplement to calculate statewide turnout rates, and include estimates of the share of Black Americans who are disenfranchised in each state from Manza and Uggen (2006) to explore the impact of these policies on eligible voters. They conclude that disenfranchisement has large spillover effects for Black voters: where more Black residents are disenfranchised, eligible Black voters 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). As Bowers and Preuhs (2009) sums 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" (724).

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, little work has been done exploring the impact these laws might have at the local level. Burch (2013) is an exception to this. Burch explores the depressive effect of disenfranchisement laws at the local level in North Carolina by examining census block group level turnout and zip code level involvement with the criminal justice system, determining that "at high concentrations, imprisonment and community supervision have an unequivocally demobilizing effect of neighborhoods" (185). This project seeks to expand on her work by replicating her findings in New York City; by using a different estimation technique; and by using a different definition of "lost voters."

This paper begins by exploring the effect of felony disenfranchisement on neighborhood turnout in the New

York City Mayoral election of 2017 using individual-level administrative data. Policing and incarceration patterns have historically targeted communities of color, damaging the social fabric of these neighborhoods (e.g. Sewell and Jefferson 2016; Clear 2008; Lerman and Weaver 2013). It is possible, however, that these effects run deeper than the direct focus on policing and incarceration has acknowledged. We know that felony disenfranchisement systematically removes individuals from certain neighborhoods, but it is not clear that enough would-be voters are removed relative to the electorate to meaningfully distort neighborhood representation. To the extent that felony disenfranchisement has a spatially-concentrated depressive effect on eligible voters, it is possible that these policies are powerful enough to materially reduce the representation of certain parts of the city.

Framing for Turnout Effects

Despite 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 11.6 million actively registered voters. Despite the low share of residents who are directly disenfranchised, 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 suspended 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, an effect often referred to as *de facto* disenfranchisement.

Though previous studies have focused largely on the state-level spill-over effects of felony disenfranchisement, there is reason to believe that this *de facto* disenfranchisement is concentrated within the neighborhoods home to formally disenfranchised residents. Burch (2013), for instance, demonstrates that neighborhoods in North Carolina with higher levels of incarceration had lower turnout. Moreover, voting is a social act, and social networks play an important role in predicting political participation (e.g. Foladare 1968; Huckfeldt 1979; Kenny 1992; Mutz 2002). Literature from urban sociology has established that social networks are largely spatially bounded, and that local social ties are more important in lower-income neighborhoods (Guest and Wierzbicki 1999; Dawkins 2006). It should be no surprise, then, that neighborhoods have been shown to mobilize and demobilize voters through mechanisms above-and-beyond individual characteristics (Gimpel, Dyck, and Shaw 2004; Cho, Gimpel, and Dyck 2006). To the extent that felony disenfranchisement policies have depressive effects on turnout in the social and filial networks of the imprisoned and paroled, these effects are likely to be closely concentrated in the neighborhoods where the disenfranchised live. I therefore hypothesize that felony disenfranchisement does have locally bounded spillover effects, and that

turnout in neighborhoods with lost voters systematically vote at a lower rate than others.

Recent work from Hannah Walker and others, however, leads me to moderate my hypothesis about the neighborhood spill-over effects of lost voters. Walker (2014) and Walker and García-Castañon (2017), for instance, demonstrate that individuals who have proximal contact with the criminal justice system (defined as "as having a loved one who is a custodial citizen without yourself having had contact" (Walker and García-Castañon 2017, 542)) were not less likely to vote, but *were* more likely to participate in nonelectoral political events (such as signing a petition, attending a community meeting, or writing to an elected official). These effects are particularly pronounced for women of color. Although Walker and others demonstrate that these actions largely take place outside of the voting booth, they may moderate spillover effects on turnout.

Theoretical Implications for Reduced Turnout

As discussed above, it has been widely established that felony disenfranchisement reduces turnout even among eligible voters, particularly in the Black community (e.g. King and Erickson 2016; Bowers and Preuhs 2009). With the exception of Burch (2013), however, there has been little investigation into whether these depressive effects are narrowly concentrated in the neighborhoods home to disenfranchised individuals or are more widely dispersed. Understanding how these spillover effects are distributed is of practical importance. For instance, if neighborhoods home to disenfranchised individuals display different candidate preferences than the rest of the city, these demobilizing effects will undermine these neighborhoods' political representation. There is some evidence that spatial segregation leads to unique voting preferences among neighborhoods: Kinsella, McTague, and Raleigh (2015) examines political clustering in the Greater Cincinnati Metropolitan Area from 1976 through 2008. Over this three-decade period, precinct-level presidential election results show trends toward increased polarization.

Even if neighborhoods to not have unique preferences for presidential candidates, lower turnout in impacted communities may reduce their allocation of public goods. At the Congressional level representatives direct resources to areas within their districts that provide the greatest political benefit — that is to say, areas that will reward them with more votes (Martin 2003). Congressional representatives are also more responsive to the policy preferences of higher-turnout areas. "[H]igher citizen participation is rewarded," Martin and Claibourn (2013) concludes, "with enhanced policy responsiveness" (59). Griffin and Newman (2005) finds similar effects in the United States Senate, demonstrating that "voter preferences predict the aggregate roll-call behavior of Senators while nonvoter preferences do not" (1206). 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 and support their calls for greater public investment for amenities such as

schools and parks. This may be true even if they support the same candidates as neighborhoods not touched by felony disenfranchisement.

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" (1). 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.

Hajnal and Trounstine (2005), however, shows that racial variation in turnout has real consequences for local elections and local political power. They estimate whether mayoral and city council races might have turned out differently if all races had turned out at the same rate, finding that three of the ten largest cities in America would likely have had different mayors if minorities had turned out as frequently as Whites. Of particular interest for the study at hand is their assertion that New York City would have elected a different mayor in 2001 if all racial groups had participated equally. In the nation's ten largest cities, Hajnal and Trounstine (2005) argues that "Changes in the percentage of voters who turn out can and do alter mayoral election outcomes and racial representation on city councils" (518). Neighborhoods most impacted by felony disenfranchisement are disproportionately home to residents of color, and they are likely to share concerns about criminal justice issue. Hajnal and Trounstine (2005) and Anzia (2019) both therefore indicate that depressed turnout from felony disenfranchisement is likely to materially reduce neighborhoods' political representation.

2 Data

Criminal Justice Data

The primary criminal justice dataset comes from a public records request filed with the New York State Department of Corrections and Community Supervision (NYSDOCCS). They include individual-level incarceration and parole records for individuals who have been incarcerated in New York State since 1990. The data includes a host of information, including: first, middle, and last name; date of birth; class of offense; incarceration start and end dates; dates of parole; sex; race; and others. These data are used to determine when individuals were incarcerated or on parole, the class of crimes for which they were incarcerated, and when they finished their parole supervision.

The state makes records available only for individuals who have been incarcerated for felony offenses. It does not make information about individuals sentenced to probation or incarcerated for misdemeanors. Thus, while the data covers all individuals subject to felony disenfranchisement rules (only individuals incarcerated for felony offenses lose their voting rights), it limits the availability of a potentially helpful control group. It does not include individuals who are held in federal prisons; however, because the vast majority of incarcerated felons are held in state prisons, this is unlikely to affect the analysis.

The state does not make a unified database of parolees whose voting rights have been restored available to the public. However, the NYSDOCCS Parolee Lookup website includes a flag indicating whether someone's voting rights have been restored. By using identification numbers from the parolee data obtained from the public records request, I constructed a list of individuals who have 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 New York, this information is publicly available from the Board of Elections. It includes information on all registered voters, including: first, middle, and last name; date of birth; home address; vote history; and other information. The first section of this paper uses a snapshot of the registered voter file from April 30$^{\text{th}}$, 2018. The second section of this paper uses a snapshot from March 3$^{\text{rd}}$, 2019.

The New York State Voter File is unique in its treatment of "purged" voters: although most states remove voters from their voter files once they are no longer eligible to vote, New York continues to include them in the file (but marks them as purged). I can therefore identify voters who were registered in the past but have since been purged due to a felony conviction.

Geocoding

Voters' home addresses were converted to latitudes and longitudes using a geocoder provided by SmartyStreets. I then used the statistical software R to map these latitudes and longitudes to census block groups, census tracts, and city council districts using shapefiles publicly available from the Census Bureau and the City of New York. This geocoder is not perfect: among individuals registered to vote in New York City, the geocoder failed to determine the latitude and longitude of the addresses of 1 percent of registered voters. The geocoder was slightly less successful when it came to lost voters (defined and discussed below); 1.6 percent of these individuals were not geocoded. Voters who were not successfully geocoded are dropped from the dataset; however, because so few observations went uncoded, it is unlikely to affect the analysis.

Matching

Registered and formerly registered individuals who have been to prison in New York are identified by matching the NYSDOCCS records with the registered voter file. I match individuals in each dataset using first name, middle name, last name, and date of birth. To be considered a "match," records must have the exact same birth date. The first and last names must also be exact matches (conditional on the adjustments discussed below). The middle names must meet one of the following conditions in order to qualify:

- Middle names are identical. If neither set of records includes a middle name, this condition is met.
- A full middle name in one set of records and only a middle initial in the other. The first letter of the full middle name must be the same as the middle initial in the other set of records.
- A middle name or middle initial in one set of records, and a missing middle name in the other set.

Thus, "John Andrew Doe" and "John A Doe" would count as matches. Similarly, "John Andrew Doe" and "John Doe" would count, while "John Andrew Doe" and "John Anthony Doe" would not.

There are two types of potential error in this methodology: a false positive will result when a formerly incarcerated individual's records matches the record of a voter who is a different individual but shares the same name and date of birth. False negatives will occur when an individual has a different name in the different sets of records, or when the birthdate is incorrectly reported in one of the sets of records

Testing for the presence of false positive matches is fairly straightforward. Meredith and Morse (2013) offers one way to test their prevalence using placebo matching. I slightly alter the date of birth reported in the NYSDOCCS dataset to create false records. Comparing the number of matches between these "fake" records and the voter file with the number of matches between the "true" records and the voter file provides an estimate of how frequently false positives occur. Table 1 shows the results of true matches, as well as matches using a set of fake records created by adding or subtracting 35 days from an individual's birthdate. This analysis indicates that false positives account for between 0.4 and 0.5 percent of all matches, a share that is likely too small to have any material impact on the overall analysis. The numbers in Table 1 are derived by matching (and modifying) all individuals who were incarcerated or on parole on Election Day in 2017 with the registered voter file from April of 2018.

Table 1: Results of Shifting Birthdates

Group	Number of Matches Between DOCCS and Voter File Records
Actual Birthdate	20,955
Birthdate + 35 Days	105
Birthdate - 35 Days	92

Testing for false negatives is more challenging. If an individual marries and changes her name after being discharged from parole, for instance, I will not identify her using my matching methodology. Similarly, "John Doe" and "Jonathan Doe" would not result in a match. To reduce the likelihood of these false negatives I remove all punctuation from all names, and standardize capitalization. A record with a last name of "O'Donnell" in one dataset, therefore, would match a last name of "O DONNELL" in the other (provided the other criteria are satisfied). Such standardizations, however, will miss individuals who change their names entirely or use different names in different contexts. This is not likely to present major challenges: firstly, women are far more likely to change their last names than men, and women make up barely 6 percent of individuals who have been released from incarceration in New York State. Secondly, because both Department of Corrections and voter registration data are legal records, individuals are likely to be recorded using their full names in each (that is to say, an individual is unlikely to be "John" in one set of records and "Jonathan" in the other).

3 Effects of Felony Disenfranchisement on Neighborhood Turnout Levels

Identification of Lost Voters

In this analysis, I offer a different definition of "lost voter" than much of the literature. Many recent papers have attempted to identify relationships between the number of disenfranchised residents — *potentially* lost voters — and turnout. Such an approach is informative for understanding the impact of disenfranchisement. Many young men, for instance, are admitted to prison each year. If someone is incarcerated shortly after they turn 18 in an odd-numbered year, they may be incarcerated before they even have the opportunity to cast a ballot. This analytical approach is also often taken due to data constraints: most states' registered voter files provide only a snapshot of currently registered voters, making it impossible to determine voting history for individuals who are currently disenfranchised (and therefore not currently registered). Similarly,

state-level data does not report the number of incarcerated individuals with a history of voting; therefore, studies that leverage variation in laws between states to estimate the effect of felony disenfranchisement rely on estimates of the total disenfranchised population, not the number of disenfranchised individuals with a history of voting.

Examining the impact of all disenfranchised voters, however, limits our ability to understand the true effects of felony disenfranchisement. As previous research has detailed, turnout rates among individuals who are incarcerated are very low even prior to incarceration (e.g. Gerber et al. 2017). If incarcerated individuals would not have cast a ballot if they were not incarcerated, any effect of their incarceration on neighborhood turnout cannot be attributed to felony disenfranchisement *per se* but rather to their incarceration. At the local level, where felony disenfranchisement rules do not vary from neighborhood to neighborhood, such a definition does not allow us to disentangle the depressive effect of having a neighbor go to prison from the effects of felony disenfranchisement.

Here, I explore whether the disenfranchisement of residents 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. Although not all lost voters would have participated if they had not been disenfranchised, past participation is an extremely strong predictor of propensity to vote (Gerber, Green, and Shachar 2003). As such, these are the individuals most likely to have been impacted not only by incarceration but specifically by felony disenfranchisement. Identifying the effects of felony disenfranchisement in neighborhoods that lost individuals with a record of voting — individuals who would likely have cast a ballot had they been allowed — provides insight into whether felony disenfranchisement reduces neighborhood turnout. Because New York State's voter file includes information on individuals who have been purged for felony convictions, I can reconstruct the vote history even for voters who are no longer eligible to vote.

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 non-special election for city-wide office in New York — the mayoral election which took place on November 7$^{\text{th}}$, 2017. Lost voters, therefore, are all individuals who were incarcerated or on parole on November 7$^{\text{th}}$, 2017, and had cast a ballot between 2007 and 2016.

Figure 1 shows where these lost voters lived before going to prison, with city council districts also included. There were 2,518 such lost voters within New York City as of the 2017 general election, and 6,166 statewide.

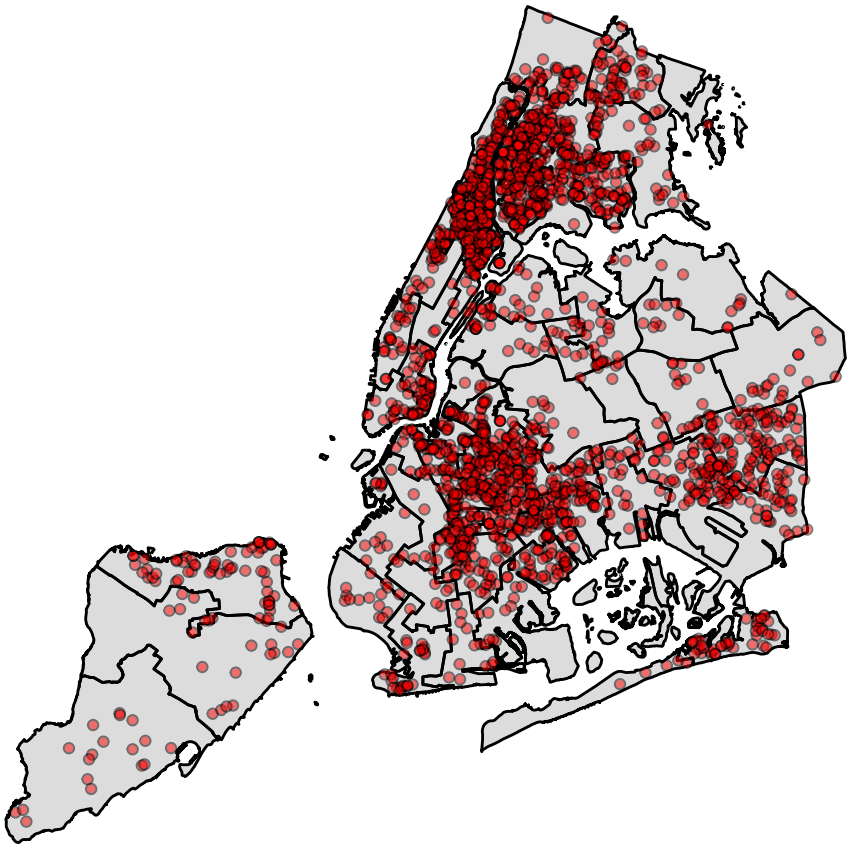

Figure 1: Lost Voters on Election Day, 2017

The spatial concentration of lost voters 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 with a demonstrated history of voting were not allowed to cast a ballot for mayor.

Testing for Neighborhood Turnout Effects

Table 2 shows the distribution of the number of lost voters by neighborhood. Most neighborhoods lost no voters at all; when a neighborhood was home to lost voters, they generally lost just one resident with a history of voting to felony disenfranchisement. More than two-thirds of block groups that lost a voter lost only one voter, while 45 percent of census tracts with a lost voter lost only one.

Table 2: Distribution of Lost Voters

Number of Lost Voters	Count of Block Groups	Count of Tracts
0	4,272	1,008
1	1,023	416
2	301	212
3	107	138
4	30	51
5	19	46
6	9	23
7	3	15
8	5	13
9	0	7
10	0	2
11	0	2
12	0	2
14	0	2
Total	5,769	1,937

Because the majority of neighborhoods have either zero or one lost voter, I begin testing the effect of lost voters on neighborhood turnout 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 voters 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 (Sekhon 2011), using a series of demographic and political indicators. Estimates of racial characteristics, median income, education, age, population, and share noncitizen[2] 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 (calculated using the voter file) by the citizen voting age population (CVAP, estimated by the Census Bureau). I include voteshare won by the winning city council representative in 2017 as a proxy for the competitiveness of the local race;[3] where city council district races were more competitive, I expect that more voters will have turned out. 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 3 and 4 present the results of these matches.

[2]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.

[3]Where neighborhoods cross council district lines, this measure is the mean competitiveness faced by each voter in the neighborhood

Match Output

Table 3: 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	0.35	0.25	0.35	0.34	93.02	89.13	85.79	79.01
% Non-Hispanic Black	0.38	0.16	0.38	0.37	97.93	94.46	94.41	92.77
% Non-Hispanic White	0.17	0.40	0.17	0.18	96.37	96.06	95.56	92.99
Median Income	51,792.52	72,612.36	51,792.52	52,768.97	95.31	93.47	88.62	74.49
% With Some College	0.64	0.70	0.64	0.64	90.75	94.14	90.29	80.70
Median Age	35.99	38.34	35.99	36.25	89.18	90.84	85.93	78.56
Registration Rate	1.00	0.94	1.00	0.98	59.77	79.89	72.28	60.42
% Democrats	0.75	0.65	0.75	0.75	96.22	96.52	94.92	91.02
% Noncitizen	0.16	0.16	0.16	0.16	16.58	14.80	-0.06	-60.28
% Won by City Council Representative	0.85	0.81	0.85	0.86	89.44	83.40	79.88	70.11

Table 4: 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	0.32	0.22	0.32	0.33	98.55	85.12	82.84	73.01
% Non-Hispanic Black	0.36	0.14	0.36	0.36	99.77	88.74	88.36	83.30
% Non-Hispanic White	0.19	0.42	0.19	0.19	97.50	96.32	91.29	79.95
Median Income	53,409.42	71,754.50	53,409.42	55,422.41	89.03	90.83	84.86	62.84
% With Some College	0.65	0.71	0.65	0.65	90.19	93.52	88.89	73.01
Median Age	35.76	38.04	35.76	35.80	98.12	74.51	80.56	71.91
Registration Rate	0.94	0.90	0.94	0.93	76.63	86.16	84.24	71.28
% Democrats	0.74	0.62	0.74	0.73	94.58	94.93	92.05	82.37
% Noncitizen	0.16	0.17	0.16	0.17	-123.57	22.48	8.85	-38.79
% Won by City Council Representative	0.85	0.79	0.85	0.85	93.03	81.57	79.27	67.47

At both the tract and block group level, matching results in an untreated group of neighborhoods that looks substantially like the 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 far less white, have much lower median incomes, and a larger share of voters are registered as Democrats.

After matching neighborhoods, I use a simple regression to test whether neighborhood treatment status was associated with turnout in the 2017 mayoral election. 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, which are then aggregated to estimate the number of ballots cast in each neighborhood. The number of ballots cast is divided by the neighborhood's CVAP.

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 5 presents the results of these regression models. Robust standard errors are clustered at the level of the match (Abadie and Spiess 2019).

Table 5: Matching Regression

	Turnout Rate in 2017 General Election			
	Block Group Level		Tract Level	
	(1)	(2)	(3)	(4)
D(Neighborhood Lost a Voter)	-0.009^{***}	0.001	-0.004^{*}	0.010^{***}
	(0.002)	(0.003)	(0.002)	(0.004)
Share Non-Hispanic Black		-0.013^{***}		0.013^{***}
		(0.004)		(0.005)
D(Lost Voter) × Share Non-Hispanic Black		-0.027^{***}		-0.041^{***}
		(0.008)		(0.009)
Constant	0.160^{***}	0.165^{***}	0.154^{***}	0.150^{***}
	(0.001)	(0.002)	(0.002)	(0.003)
Observations	46,407	46,407	10,219	10,219
R^2	0.002	0.010	0.001	0.007
Adjusted R^2	0.002	0.010	0.001	0.007

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

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 0.9 percentage points lower than in comparable block groups without lost voters. This decrease, however, appears to be entirely concentrated within Black neighborhoods. When the treatment indicator is interacted with the share of the neighborhood that is Non-Hispanic Black, the basic treatment dummy becomes nonsignificant. The coefficient on the interaction between treatment and share Black indicates that treated neighborhoods that are largely Black saw turnout that was as much as 2.7 percentage points lower than similar neighborhoods without lost voters. Considering that the overall turnout rate in block groups with a lost voter was just ∞ percent, this effect is alarmingly high. For every 100 votes cast in an entirely Black block group with a lost voter, as many as NaN votes went uncast. The effects are slightly larger when neighborhoods are measured as block groups than as census tracts, though turnout is still significantly depressed in tracts with lost voters. This is not surprising — because the spillover effects are likely to operate through social networks, smaller geographical units are likely to be more affected.

Testing Intensity Effects

Matching methodologies, of course, only allow us to test the effect of being treated — here, losing any voter for the 2017 election. The models above do not allow for different effects on turnout based on *how many* voters a neighborhood lost, potentially understating the impact of felony disenfranchisement in the most hard-hit communities. In Table 6 below, I adopt a standard ordinary least squares regression to investigate whether lost voters are associated with lower turnout rates in the 2017 election. This regression uses the same covariates used in the matching procedure described above. Robust standard errors are clustered by city council district.[4]

[4]Where neighborhoods cross city council district lines, they are assigned the district in which most of their voters live for clustering purposes.

Table 6: Standard Regression

	Turnout Rate in 2017 General Election			
	Block Group Level		Tract Level	
	(1)	(2)	(3)	(4)
Lost Voters	−0.008**	−0.0003	−0.004*	0.002
	(0.003)	(0.003)	(0.002)	(0.003)
Lost Voters × Share Non-Hispanic Black		−0.019**		−0.016**
		(0.009)		(0.007)
Median Income (Thousands of Dollars)	0.0002*	0.0002*	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0002)	(0.0002)
Percent Latino	0.065***	0.062**	0.084***	0.075***
	(0.025)	(0.024)	(0.029)	(0.027)
Percent Non-Hispanic Black	0.062*	0.070**	0.082**	0.099**
	(0.034)	(0.035)	(0.038)	(0.040)
Percent Non-Hispanic White	0.113***	0.114***	0.130***	0.130***
	(0.024)	(0.024)	(0.030)	(0.030)
Percent With Some College	0.011	0.011	−0.016	−0.016
	(0.031)	(0.031)	(0.048)	(0.048)
Median Age	0.002***	0.002***	0.003***	0.003***
	(0.0004)	(0.0004)	(0.001)	(0.001)
Registration Rate	0.182***	0.182***	0.199***	0.198***
	(0.011)	(0.011)	(0.019)	(0.019)
Percent Democrats	−0.074	−0.077	−0.094	−0.103
	(0.081)	(0.081)	(0.091)	(0.091)
Percent Noncitizen	−0.177***	−0.174***	−0.178***	−0.174***
	(0.053)	(0.053)	(0.058)	(0.057)
Percent Won by City Council Representative	−0.035	−0.035	−0.017	−0.017
	(0.042)	(0.042)	(0.043)	(0.043)
Constant	−0.060	−0.059	−0.088	−0.081
	(0.049)	(0.049)	(0.065)	(0.065)
Observations	5,769	5,769	1,937	1,937
R^2	0.421	0.422	0.466	0.471
Adjusted R^2	0.419	0.421	0.463	0.467

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

The results presented in Table 6 align closely with the estimated effect from the matching model. Lost

voters are generally associated with lower turnout (each missing voter in a block group reduces that neighborhood's turnout by about 0.82 percentage points), but Models 2 and 4 again make clear that this effect is concentrated in Black neighborhoods. In neighborhoods where most residents are Black, each lost voter is associated with a turnout decrease of up to 1.87 percentage points. The neighborhoods most affected by felony disenfranchisement are neighborhoods where incarceration patterns overlap with Black communities.

The block groups where these depressive effects are not randomly distributed throughout the city. They are highly spatially concentrated in Central Brooklyn, Eastern Queens, and Harlem. Figure 2 applies the coefficient on $Lost\ Voters \times Share\ Black$ from Model 2 in Table 6 to the city's block groups. The estimated depressive effect is $-0.019 \times Lost\ Voters \times Share\ Black$.

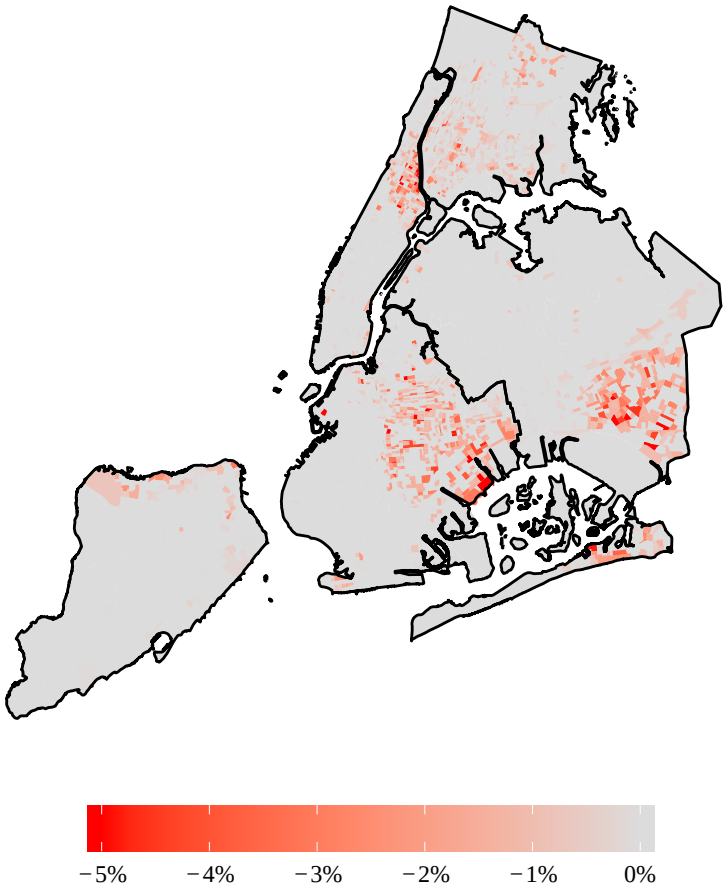

Figure 2: Estimated Depressive Effect of Felony Disenfranchisement

Toward a Causal Estimation

As the analyses above make clear, neighborhoods with lost voters saw substantially lower turnout in the 2017 mayoral election than neighborhoods without lost voters. The possibility remains, however, that treated neighborhoods systematically differ from untreated neighborhoods in ways that cannot be accounted for using the available data. If that were the case, we would expect that these treated neighborhoods would have lower turnout regardless of whether they lost a voter. As the analysis that follows shows, this is not the case: prior to losing a voter, treated neighborhoods did not have systematically lower turnout than other neighborhoods.

I begin by restricting the set of neighborhoods to only neighborhoods that, on election day in 2016, had no lost voters. This process is the inverse of the process described above: I identify all individuals who were in prison or on parole on November 8$^{\text{th}}$, 2016, who had cast a ballot between 2006 and 2015. These individuals' home neighborhoods are removed from the subsequent analysis. I then use the genetic match algorithm discussed above to match neighborhoods with a lost voter in 2017 to neighborhoods with no lost voters in 2017. These neighborhoods, therefore, were *newly* home to lost voters. If lost voters have no effect on neighborhood turnout, we would expect these treated neighborhoods to have lower turnout in both 2016 — before they lost a voter — and in 2017, after a voter was disenfranchised. In 2017, 1,497 neighborhoods had a lost voter; of these, 326 had no lost voters in 2016 and are included here. The original set of control block groups included 4,272 neighborhoods with no lost voters; of these, 211 *did* have a lost voter in 2016, and are thus excluded from this analysis.

Of course, felony disenfranchisement could structure neighborhood turnout differently for presidential and local elections. It is conceivable that the incarceration of a family member or neighbor would sour individuals on participating in local contests, but that the same individual would not read national politics through the same lens. Nevertheless, data limitations makes the 2016 election the only reasonable comparator for 2017 turnout.

Tables 7 and 8 present the results of matching on these subsets of neighborhoods. As before, the match results in a highly balanced panel between treated and untreated neighborhoods.

Table 7: 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	0.35	0.25	0.35	0.35	99.00	96.26	94.92	85.60
% Non-Hispanic Black	0.33	0.16	0.33	0.33	99.02	96.57	95.67	90.10
% Non-Hispanic White	0.21	0.41	0.21	0.21	99.61	94.66	92.61	82.94
Median Income	55,852.62	73,433.49	55,852.62	56,287.76	97.52	87.85	86.56	72.89
% With Some College	0.65	0.71	0.65	0.65	99.31	92.25	87.30	71.15
Median Age	36.17	38.47	36.17	36.04	94.37	62.35	72.27	69.84
Registration Rate	0.98	0.94	0.98	0.95	25.09	46.49	42.16	33.06
% Democrats	0.73	0.64	0.73	0.73	95.94	94.99	93.62	86.68
% Noncitizen	0.17	0.16	0.17	0.17	63.46	75.01	66.85	45.71
% Won by City Council Representative	0.85	0.80	0.85	0.85	88.19	83.99	78.23	67.54

Table 8: 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	0.31	0.22	0.31	0.30	94.34	86.10	84.77	75.82
% Non-Hispanic Black	0.25	0.12	0.25	0.25	99.28	89.75	86.62	77.90
% Non-Hispanic White	0.29	0.44	0.29	0.28	97.02	87.45	77.73	61.67
Median Income	59,990.43	72,565.90	59,990.43	61,973.89	84.23	78.35	76.98	66.57
% With Some College	0.67	0.71	0.67	0.66	96.86	86.51	83.73	76.55
Median Age	36.36	38.23	36.36	36.39	98.60	73.15	69.00	42.11
Registration Rate	0.92	0.90	0.92	0.91	10.68	64.27	63.46	56.35
% Democrats	0.69	0.62	0.69	0.68	91.23	90.06	87.35	76.31
% Noncitizen	0.18	0.17	0.18	0.18	78.09	73.55	62.03	48.36
% Won by City Council Representative	0.83	0.79	0.83	0.83	99.91	75.59	65.98	38.91

In Table 9, I present the out come of a simple least squares regression on this subset of block groups and tracts. In both models, D(2017) indicates that across the board turnout in 2017 was far lower than in 2016. D(Treat) can be understood as the difference between treated and control neighborhoods in the 2016 election. In Model 1, the treated and control neighborhoods did not have statistically significantly different turnout in the 2016 election; Model 2 indicates that treated census tracts had slightly *higher* turnout prior to intervention. D(Treat) $\times$ D(2017) is the coefficient of most interest here: it indicates whether, after treatment, treated neighborhoods turned out at lower rates than similar neighborhoods. In Model 1, this coefficient is negative and statistically significant, and is very similar to the coefficients reported in Tables 5 and 6. Model 1 in Table 9 indicates that, in the first election after the removal of a voter, a neighborhood's turnout rate decreased by 2.6 percentage points. The relationship does not hold for tracts (suprisingly, treated tracts turned out at higher rates both before and after treatment than control tracts).

Table 9: Neighborhood Turnout in 2016 and 2017 General Elections

	Turnout Rate	
	Block Group Level	Tract Level
	(1)	(2)
D(2017)	-0.339^{***}	-0.325^{***}
	(0.002)	(0.005)
D(Treat)	0.016^{*}	0.013^{**}
	(0.008)	(0.006)
D(2017) × D(Treat)	-0.026^{***}	-0.008
	(0.008)	(0.007)
Constant	0.510^{***}	0.506^{***}
	(0.012)	(0.014)
Congressional District FE	X	X
Observations	20,212	2,838
R^2	0.658	0.795
Adjusted R^2	0.658	0.794

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

Table 9 makes clear that the removal of lost voters likely has a causal effect on neighborhoods' turnout rates. It is worth noting that this is the treatment effect on the treated — which is to say, it is estimated using only neighborhoods with no lost voters in 2016. These neighborhoods may differ from neighborhoods that had lost voters in both 2016 and 2017. Indeed, neighborhoods that consistently have lost voters are more likely to be the places most impacted by policing patterns. Among block groups that had a lost voter in 2017, those who had no lost voters in 2016 were higher-income ($51k versus $56k), had higher shares non-Hispanic White (16 percent versus 21 percent), a lower share non-Hispanic Black (39 percent versus 33 percent), and a smaller share were registered as Democrats (76 percent versus 73 percent).[5] Each of these differences were even more magnified when comparing tracts included in this analysis to all tracts with lost voters in 2017.

Of particular note is the lower share of the population in these neighborhoods that is Black. The analyses above indicate that neighborhoods with more Black residents respond the most strongly to felony disenfranchisement. The set of neighborhoods in this section, therefore, are likely among the least likely to see this relationship. Nevertheless, that this relationship can be detected at the block group level lends credence to the causal relationship between lost voters and depressed turnout.

[5]Each of these differences are significant at the 99 percent confidence level.

Discussion

During the 2017 mayoral election, felony disenfranchisement laws were responsible for removing an estimated 2,518 voters from New York City neighborhoods. The spatial concentration of these lost voters is striking, as demonstrated in Figure 1, and the systematic removal of voters from these neighborhoods is troubling. However, more than one million votes were cast in the general election of 2017. Although felony disenfranchisement rules have implications for the individuals targeted by them, the removal of such a small number of voters — concentrated though they are — is unlikely to have large implications on its own.

As this analysis makes clear, however, felony disenfranchisement reaches beyond the individuals who are incarcerated. Previous literature has established that felony disenfranchisement impacts Black turnout at the state level, but this analysis demonstrates that these demobilizing effects intersect with geographical space to systematically depress the vote in neighborhoods where voters are being sent to prison. As discussed above, these neighborhoods have far lower incomes than the rest of the city: the median income of block groups without any lost voters is more than 40 percent higher than the median income of block groups with lost voters. Similarly, just 17 percent of individuals in the average block group with a lost voter were White, compared with 40 percent of the population in other block groups. Felony disenfranchisement has spillover effects on neighborhood turnout, and these neighborhoods systematically differ from the rest of the city's population. The effects are concentrated in neighborhoods where the most marginalized members of society live. These highly concentrated spillover effects are cause for major concern.

Of equal concern is the apparent concentration of these effects in Black neighborhoods. Both Tables 5 and 6 indicate that once the lost voter indicator is interacted with the share of a neighborhood that is non-Hispanic Black, the lost voter indicator is either nonsignificant or significant and positive. This means that lost voters have no spillover effects in neighborhoods with small Black populations. Understanding the magnitude of this finding is important. In New York City, there are 165 block groups where the Black community makes up more than 90 percent of the population, with an average of 821 voting age citizens. Model 2 of Table 5 indicates that, in a block group that is 90 percent Black with a citizen voting age population of 821, having any lost voter reduced turnout by 20 ballots. Conversely, in a block group with the same voting age population that is just 10 percent Black, the loss of any voter decreased turnout by just 2.2 ballots.

The standard OLS regression results tell a similar story: in our average block group that is 90 percent Black, each lost voter cost the neighborhood 13.9 ballots. In block groups of the same size where the population is just 10 percent Black, lost voters cost the neighborhood just 1.5 ballots — hardly more votes than the lost voter herself.

Why do lost voters in Black neighborhoods have such large spillover effects, when lost voters in predominantly non-Black neighborhoods do not? Much of the previous literature in this space establishes that individuals who have negative interactions with the government are less likely to choose to interact with the state in the future — that these interactions have large "interpretive effects" (Pierson 1993). Lerman and Weaver (2013), for instance, shows that neighborhoods where there are many police stops that involve searches or use of force use 311 services less frequently. Weaver and Lerman (2010) argues that interactions with the criminal justice system changes how individuals understand both their identities as citizens and the nature of governmental structures. Similarly, Weaver and Lerman (2014) tells us that those who have had contact with the criminal justice system consider political participation not just unfruitful but rather "as something to be actively avoided" (16). It is perhaps unsurprising that the negative spillover effects are largest in neighborhoods where police presence and criminal justice involvement is most keenly (and unfairly) felt; namely, in plurality and majority Black neighborhoods.

Individuals who live in neighborhoods where police activity is relatively limited may interpret the incarceration of a neighbor as a largely individual phenomenon. If it is understood as an isolated or individual event, voters in these neighborhoods who are not incarcerated are not likely to update their view of the state. They may draw no connections between their neighbor's imprisonment and their own efficacy as a voter. In the neighborhoods where policing is most prevalent — often, lower-income Black communities — the incarceration of a neighbor might not be interpreted so individualistically. It may, rather, be interpreted as another reminder of the government's unfairness. If a would-be voter finds herself soured on political participation because of her neighbor's incarceration, she may be less likely to cast a ballot.

This finding mirrors White (2019), which finds that brief jail spells decrease future participation more for Black individuals than for White individuals. This is perhaps unsurprising. A large body of research indicates that, even after controlling for various sociodemographic characteristics and interactions with the police, Black Americans have far more negative views of the criminal justice system than White Americans (e.g. Browning and Cao 1992; Hurwitz and Peffley 2005; Henderson et al. 1997; Wu, Sun, and Triplett 2009). Experience with the criminal justice system is less likely to be viewed in isolation (and therefore more likely to affect voting patterns) in Black neighborhoods. There is therefore strong reason to suspect that the concentrated depressive effects of felony disenfranchisement in Black neighborhoods arise from these distinct interpretations of the act of incarceration by the state.

For decades, scholars have detailed the problems associated with poor and segregated urban neighborhoods (Wilson 1990). More recent work has begun to interrogate the ways in which the increasing reach of the carceral state shapes the economic and political behavior of individuals caught up in the criminal justice

system and their community members. Much of this work, however, has focused on either the impacts of living in marginalized communities (by measuring health and economic impacts) or has not accounted for the importance of physical space (by focusing only on proximal social, and not geographic, contact with directly impacted individuals). This analysis allows us to understand the implications for living in a neighborhood where individuals likely to cast a ballot are not allowed to because of a felony conviction. I find that neighborhoods that are home to lost voters — and particularly neighborhoods with large Black populations — systematically turn out for local elections at lower rates than otherwise similar neighborhoods.

This has major ramifications for how we understand the political positioning of the minority neighborhoods most impacted by overpolicing and incarceration. In the case of the 2017 election, there were likely no electoral consequences: Bill de Blasio won reelection handily, and neighborhoods with lost voters overwhelmingly supported his candidacy. A lack of electoral consequences in 2017, however, should not be interpreted to mean that the disparate and concentrated depressive effects of felony disenfranchisement never have implications for who is elected to local office. As Hajnal and Trounstine (2005) shows, racial turnout differentials can have real consequences for city politics.

Moreover, we cannot conclude that depressed turnout in these neighborhoods in 2017 had no impact on their representation. City council members representing these neighborhoods, for instance, may determine that pushing for policies popular with these constituents (such as stricter police oversight) will not garner enough votes to make such a fight worthwhile. Although spillover effects from felony disenfranchisement may not have changed who won power in 2017, it very possibly altered how those individuals *held* and *used* their power.

Felony disenfranchisement laws, originally adopted during Jim Crow, have taken on a new life in the era of mass incarceration. Abundant research has demonstrated the pernicious ways in which overincarceration dogs the lives of poor and non-White Americans. As this study shows, however, the neighborhoods bearing the brunt of felony disenfranchisement are also seeing their political power diminished through reduced turnout. The interlocking nature of racioeconomic segregation, politicing patterns, and felony disenfranchisement all combine to undermine the political power of marginalized communities in New York City.

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