Neighborhoods and Felony Disenfranchisement: The Case of New York City

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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. Many researchers, however, have focused on the effects of felony disenfranchisement on individuals, and on racial minorities as a group. They have examined the effect of disenfranchisement on the political participation of individuals, including after they are no longer disenfranchised, and the spillover effects disenfranchisement has on eligible voters of color. Researchers 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 relationship between disenfranchisement and neighborhood turnout in New York City.

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 New York City, this paper also explores the impact of one policy intended to weaken the disenfranchisement program. Specifically, I investigate the effect of New York Governor Andrew Cuomo's Executive Order 181 which ended the disenfranchisement of parolees in New York State. The effect of these policies make clear that, while the end of felony disenfranchisement is necessary, it is not enough: 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 disenfranchise citizens for 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).[1]

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

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

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 has 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 Black 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).

Recent data Florida make the victims of felony disenfranchisement laws especially clear. Prior to January, 2019, Floridians convicted of felony offenses were permanently disenfranchised unless they were individually pardoned by a clemency board. In November, 2018, voters passed a ballot initiative amending the state constitution to end permanent disenfranchisement. In the first three months after permanent disenfranchisement was ended, 44 percent of formerly incarcerated Floridians who registered to vote were Black — compared with just 13 percent of the statewide electorate (Morris 2019).

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 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 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 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).

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 used release data from states' Departments of Corrections and their registered voter files to identify formerly incarcerated individuals who went on to register to vote. Using the registered voter files, she was also able to estimate the party affiliation of formerly incarcerated individuals (in states with party registration) and their turnout rates. Her methodology has been used to investigate other questions surrounding 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, 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 determine individual-level turnout. They 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. 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 Black voters: where more Black residents are disenfranchised, elibible 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; Walker 2014). 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."

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 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. Using a standard ordinary least squares regression determined 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; by using a different estimation technique; and by using a different definiton of "lost voters."

What This Paper Examines

This paper begins by exploring the effect of felony disenfranchisement on neighborhood turnout in the New York City Mayoral election of 2017. Policing and incarceration patterns have historically targeted communities of color, wreaking havok on the social fabric of these neighborhoods [XX]. It is possible, however, that these effects are even broader than the direct focus on policing has acknowledged. We know that felony disenfranchisement systematically removes voters from certain neighborhoods, but it is not clear whether enough voters are removed relative to the electorate to meaningfully distort neighborhood representation. To the extent that felony disenfranchisement has a depressive effect on *eligible* voters, it is possible that these policies are powerful enough to materially reduce the representatition of certain parts of the city.

After examining the effect of felony disenfranchisement on voter turnout at the neighborhood level, I consider the efficacy of a program intended to undo one piece of the felony disenfranchisement apparatus. In April of 2018, Governor Andrew Cuomo signed Executive Order 181 which restored voting rights to New Yorkers on parole.[2] 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 parolees. To test whether the policy was successful, I build an individual-level logit model to explore whether turnout rates among individuals discharged from parole after Executive Order 181 went into effect cast ballots at higher rates than those discharged previously.

The first part of this project seeks to refine how we think about felony disenfranchisement in the United States. Rather thank think about it purely through the prism of those directly impacted (residents who

[2]Prior to Executive Order 181, residents convicted of felonies 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 for felony offenses are disenfranchised.

are actively disenfranchised) or through a prism of race (by examining the impact of disenfranchisement on Black turnout at the state level), I seek to introduce the importance of physical space into the conversation. This will allow us to both better understand the mechanisms through which the depressive effects operate and also to understand more deeply where these effects are concentrated. Of course, understanding the impact of felony disenfranchisement is not enough: we must also test whether our efforts to undo its legacy are effective. The second part of this project seeks to do just that.

Framing for Turnout Effects

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. The degree to which these effects are concentrated in particular neighborhoods has important implications for our understanding of the problem of felony disenfranchisement. Although literature examining the degree to which geographic polarization effects voting patterns is scarce, there is some evidence to suggest that neighborhoods do vote similarly. Kinsella, McTague, and Raleigh (2015), for instance, examines political clustering in the Greater Cincinnati Metropolitan Area from 1976 through 2008. They find evidence that, over this three-decade period, precinct-level presidential election results show trends toward increased polarization. Insofar as neighborhoods show distinct preferences for politicians, spatially concentrated depressive effects from felony disenfranchisement could influence election outcomes.

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, for instance, representatives direct resources to areas within their districts that provide the greatest political benefit (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." Griffin and Newman (2005) finds similar effects in the United States Senate, reporting that "voter preferences predict the aggregate roll-call behavior of Senators while nonvoter preferences do not." 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.

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 as a bloc, their turnout rates may influence city policy. If felony disenfranchisement has a depressive effect on turnout, it is likely that it is responsible for politicians being less responsive to the particular preferences of voters in neighborhoods where voters are removed — some of the most neglected neighborhoods to begin with.

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 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, an effect often referred to as *de facto* disenfranchisement.

This *de facto* disenfranchisement is likely to be concentrated within the neighborhoods home to formally disenfranchised residents. Voting is a social act, and social networks play an enormous 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 [XX]. 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.

Turnout Among Formerly Disenfranchised Individuals

Much of the literature discussed above has established that formerly incarcerated individuals rarely vote, even when they are no longer formally barred from doing so (Haselswerdt 2009; Burch 2011; Meredith and Morse 2015). The effect of incarceration on participation is the subject of some debate within the field. Individuals who go to prison share many characterstics with lower propensity voters generally. Less educated citizens, for instance, turnout at low rates whether they have been to prison or not. In an attempt to disentangle sociodemographic characteristics from the experience of imprisonment, Gerber et al. (2017) uses administrative data from Pennsylvania to estimate turnout rates prior to and after incarceration. They argue that the vast majority of low turnout among formerly incarcerated individuals can be explained by observable characteristics, concluding that "it appears that spending time in prison does not have large negative effects onsubsequent participation."

White (2019), however, indicates that interaction with the criminal justice system for individuals in the context of arrests for misdemeanor charges may have depressive effects on turnout. This finding does not necessarily conflict with Gerber et al. (2017); as the earlier paper explains, incarceration often occurs after many other interactions with the criminal justice system. Individuals arrested for misdemeanors, on the other hand, likely reflect a much broader swath of the population — and, therefore, individuals who may have had fewer interactions with the criminal justice system. The findings in White (2019) agree with much previous research which shows that individuals who have negative interactions with the state are less likely to participate in civic life (Pierson 1993). Weaver and Lerman (2010) argues that "contact with the institutions of criminal justice is important in structuring patterns of participation long assumed in the dominant literature to stem primarily from aspects of the individual."

Regardless of the precise mechanism, the low turnout among formerly incarcerated individuals is cause for concern, particularly given the racialized aspects of the criminal justice system. A criminal justice system that inflicts dire consequences on a population that has relatively little political voice is problematic. Whether or not incarceration *causes* low turnout, however, the state has a unique opportunity to craft policies that will impact individuals under formal supervision. Even if incarceration does not lead to lower turnout, policies targeting individuals caught up in the criminal justice system might still be effective at increasing turnout.

Some research has been done in this area. Meredith and Morse (2015) examines the impact of ending permanent disenfranchisement in Iowa. They find that individuals who received letters explicitly informing of their re-enfranchisement were more likely to cast ballots in the next election than those who did not. Meredith and Morse (2013), however, examines states where so-called notification laws went into effect. Although rules about eligibility did not change in these states, new policies required Departments of Corrections to formally inform formerly disenfranchised individuals of their re-instated voting rights. Meredith and Morse (2013) finds no effect from notification in the absense of eligibility changes.

Gerber et al. (2014) conducted a field experiment in Connecticut in advance of the 2012 presidential election. They find that sending mailers to individuals to remind them of their voting rights was successful at increasing turnout among this population. "Whatever the participatory consequences of incarceration," they conclude, "they are not in large part impossible to overcome." It is not clear whether this increase in turnout is *undoing* the depressive effect of incarceration or boosting the participation of individuals whose (low) propensity to vote was unaffected by incarceration.

Executive Order 181 is expected to have increased turnout among formerly incarcerated individuals through two primary mechanisms. The first addresses the impact of negative experiences with the state. To the extent that parole officers are accurately informing their parolees of their newly restored voting rights, Executive Order 181 is likely to bring about a positive interaction between the parolee and the government. Rather than simply have one's rights restored upon completion of sentence, Executive Order 181 may lead parolees to feel explicitly invited back into the democratic process — an invitation that may be successful at repairing some of the negative associations created through incarceration.

Secondly, Executive Order 181 is expected to dispel confusion about eligibilty and serve as a reminder to vote. As Drucker and Barreras (2005) and others have detailed, many formerly incarcerated individuals are misinformed about their eligibility to cast a ballot. If confusion about eligibility leads to lower turnout, parole officers informing their parolees of their rights is likely to increase turnout. Even if confusion does not lead to lower turnout, there is some reason to believe that reminding formerly incarcerated individuals of their rights is a successful intervention for boosting turnout (Meredith and Morse 2015; Gerber et al. 2014).

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. These data come from the New York State Department of Corrections and Community Supervision (NYSDOCCS).

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 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 state, I constructed a list of the individuals who were on parole and had their rights restored.[3]

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 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. The first section of this paper uses a snapshot of the registered voter file from April 30th, 2018. The second section of this paper uses a snapshot from March 3rd, 2019.

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.

[3] This list was compiled by using a webscraper written in the Python language.

Effects of Felony Disenfranchisement on Neighborhood Turnout Levels

Identification of Lost Voters

In this analysis, I offer a slightly different defintion 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. 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. 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.

These individuals are identified by matching NYSDOCCS data to the voter file. I use first, middle, and last names, and dates of birth, to join these two datasets.[4] Figure 1 shows where these lost voters lived before going to prison, with city council districts also included. There were 1,496 such lost voters within New York City as of the 2017 general election, and 3,728 statewide.

[4] 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 discuss the potential impact of these errors in Appendix A.

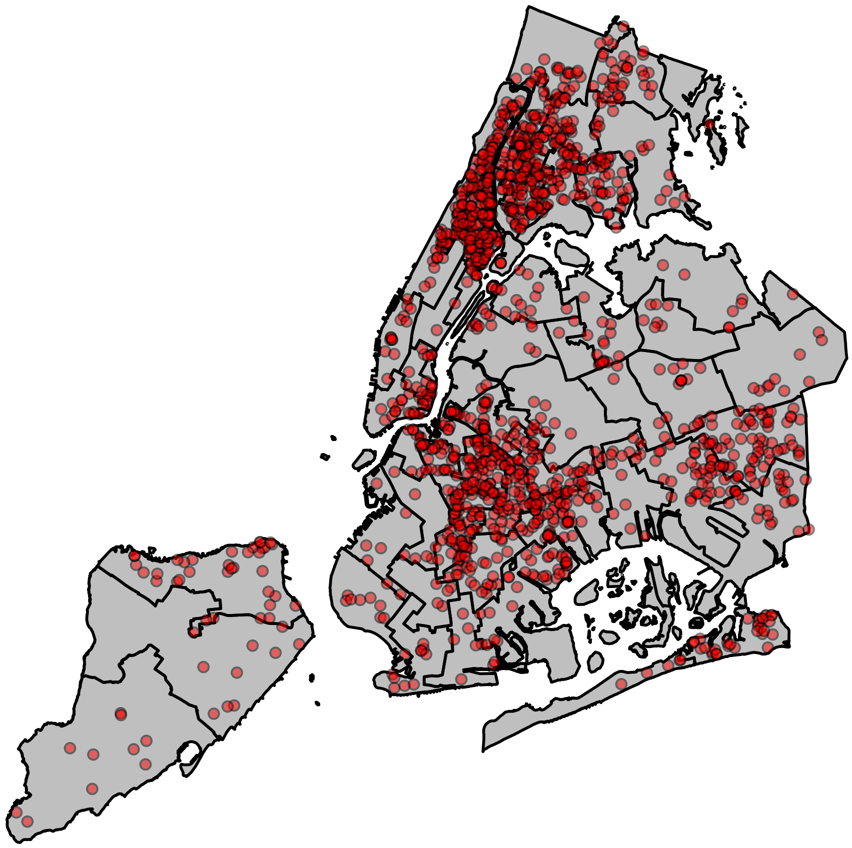

Figure 1: Lost Voters on Election Day, 2017

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 with a demonstrated history of voting 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 voters were disqualified from the election because of a felony conviction.[5] I use a genetic match to match treated to untreated census tracts and block groups (Sekhon 2011). I match on a series of demographic and political indicators. Estimates of racial characteristics, median income, education, age, population, and share noncitizen[6] 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

[5]It is worth noting that this definition of lost voter is highly conservative. For instance, some individuals who would have voted may have gone to prison after turning 18 but before their first general election. These "untreated" neighborhoods are likely home to individuals who would have voted if it were not for disenfranchisement policies.

[6]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.

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.[7] 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	0.35	0.26	0.35	0.35	94.23	88.53	84.74	76.53
% Non-Hispanic Black	0.38	0.18	0.38	0.38	99.10	95.79	95.06	92.86
% Non-Hispanic White	0.16	0.38	0.16	0.16	98.61	97.06	96.31	91.64
Median Income	50,750.92	71,025.31	50,750.92	51,402.36	96.79	93.21	89.97	78.15
% With Some College	0.63	0.70	0.63	0.64	85.73	89.61	87.01	78.53
Median Age	35.88	38.16	35.88	36.12	89.55	87.28	86.59	79.50
Registration Rate	0.82	0.76	0.82	0.80	78.10	83.08	81.46	65.14
% Democrats	0.76	0.66	0.76	0.75	94.62	94.40	93.30	89.76
% Noncitizen	0.16	0.16	0.16	0.16	41.19	26.91	8.35	-7.02
% Won by City Council Representative	0.86	0.81	0.86	0.86	93.23	79.79	81.04	75.26

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	0.23	0.33	0.33	98.64	89.64	88.30	82.26
% Non-Hispanic Black	0.36	0.15	0.36	0.37	98.90	93.38	92.92	89.41
% Non-Hispanic White	0.19	0.41	0.19	0.18	97.38	96.87	92.02	82.18
Median Income	52,545.67	71,218.25	52,545.67	53,971.01	92.37	89.67	87.07	75.24
% With Some College	0.64	0.71	0.64	0.65	89.76	93.46	89.48	75.74
Median Age	35.80	37.98	35.80	35.62	91.96	77.51	83.61	81.72
Registration Rate	0.77	0.73	0.77	0.76	65.35	86.31	76.05	50.49
% Democrats	0.74	0.63	0.74	0.74	94.07	93.52	92.49	85.75
% Noncitizen	0.16	0.16	0.16	0.16	35.19	55.25	34.50	-1.23
% Won by City Council Representative	0.85	0.79	0.85	0.85	93.79	88.77	84.07	72.46

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 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, which are then aggregated to estimate the number of ballots cast in each neighborhood.[8] The number of ballots cast is divided by the neighborhood's voting age population as reported by the Census Bureau.

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

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

[8]The New York registered voter file does not align exactly with results reported by the city. The voter file indicates that just 915,982 voters cast a ballot in the 2017 mayoral election, but the Board of Elections reports that 1,143,321 votes were cast. In Appendix B, I demonstrate that there is no relationship between lost voters and underreporting of cast ballots at the precinct level. These reporting anamolies are unlikely to impact this analysis.

in neighborhoods where a higher share of the population is Black. Table 3 presents the results of these regression models. Each neighborhood is weighted by its adult population, and robust standard errors are clustered at the level of the match (Abadie and Spiess 2019).

Table 3: Matching Regression

	Dependent variable:			
	Turnout Rate			
	Block Group Level		Tract Level	
	(1)	(2)	(3)	(4)
D(Neighborhood Lost a Voter)	-0.006^{***}	0.001	-0.005^{**}	-0.001
	(0.002)	(0.003)	(0.003)	(0.004)
Share Non-Hispanic Black		0.005		-0.014^{**}
		(0.005)		(0.007)
D(Lost Voter) X Share Non-Hispanic Black		-0.018^{**}		-0.014
		(0.008)		(0.010)
Constant	0.125^{***}	0.124^{***}	0.129^{***}	0.133^{***}
	(0.002)	(0.003)	(0.002)	(0.004)
Observations	32,364	32,364	8,734	8,734
R^2	0.001	0.003	0.001	0.009
Adjusted R^2	0.001	0.003	0.001	0.009
Note:	*p<0.1; **p<0.05; ***p<0.01 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 0.6 percentage points lower than in comparable block groups without lost voters. This decrease, however, appears to be entirely concentrated within Black neighborhoods. 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 nonsignificant. The coefficient on the interaction between treatment and share Black indicates that neighborhoods that are largely Black saw turnout that was as much as 1.8 percentage points lower than similar neighborhoods without lost voters. Considering that the overall turnout rate in block groups with a lost voter was just 11.9 percent, this effect is alarmingly high. For every 100 votes cast in a predominantly Black block group with a lost voter, as many as 15.1 votes went uncast. Although these variables are largely nonsignificant 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 any 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 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. Each neighborhood is weighted by its voting age population, and robust standard errors are clustered by city council district.[9]

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

Table 4: Standard Regression

	Dependent variable:			
	Turnout Rate			
	Block Group Level		Tract Level	
	(1)	(2)	(3)	(4)
Lost Voters	−0.007**	0.0001	−0.005*	0.001
	(0.003)	(0.003)	(0.003)	(0.003)
Lost Voters X Share Non-Hispanic Black		−0.017**		−0.014*
		(0.008)		(0.008)
Median Income (Thousands of Dollars)	0.0002	0.0002	0.0002	0.0002
	(0.0001)	(0.0001)	(0.0002)	(0.0002)
Percent Latino	0.061***	0.059***	0.078***	0.072***
	(0.022)	(0.021)	(0.027)	(0.025)
Percent Non-Hispanic Black	0.053*	0.059*	0.065*	0.078**
	(0.032)	(0.032)	(0.037)	(0.037)
Percent Non-Hispanic White	0.098***	0.099***	0.109***	0.108***
	(0.022)	(0.022)	(0.030)	(0.029)
Percent With Some College	−0.006	−0.006	−0.028	−0.026
	(0.025)	(0.025)	(0.042)	(0.042)
Median Age	0.002***	0.002***	0.003***	0.002***
	(0.0004)	(0.0004)	(0.001)	(0.001)
Registration Rate	0.192***	0.191***	0.211***	0.210***
	(0.013)	(0.013)	(0.025)	(0.024)
Percent Democrats	−0.086	−0.088	−0.089	−0.097
	(0.081)	(0.080)	(0.089)	(0.089)
Percent Noncitizen	−0.093**	−0.090*	−0.075	−0.073
	(0.046)	(0.046)	(0.056)	(0.055)
Percent Won by City Council Representative	−0.014	−0.015	−0.012	−0.013
	(0.038)	(0.037)	(0.039)	(0.038)
Constant	−0.061	−0.061	−0.095*	−0.089
	(0.046)	(0.046)	(0.054)	(0.055)
Observations	5,803	5,803	2,089	2,089
R^2	0.464	0.465	0.481	0.485
Adjusted R^2	0.463	0.464	0.479	0.482

Note: *p<0.1; **p<0.05; ***p<0.01
Robust standard errors (clustered by city council district) in parentheses.

The results presented in Table 4 align very closely with the estimated effect from the matching model. Once again, lost voters are generally associated with lower turnout (each missing voter in a block group reduces that neighborhood's turnout by about 0.72 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 1.7 percentage points.

The block groups where these depressive effects are not randomly distributed througout the city. They are highly spatially concentrated: in Central Brooklyn, Eastern Queens, and Harlem. Figure 2 applies the coefficient on "Lost Voters * Share Black" from Model 2 in Table 4 to the city's block groups.

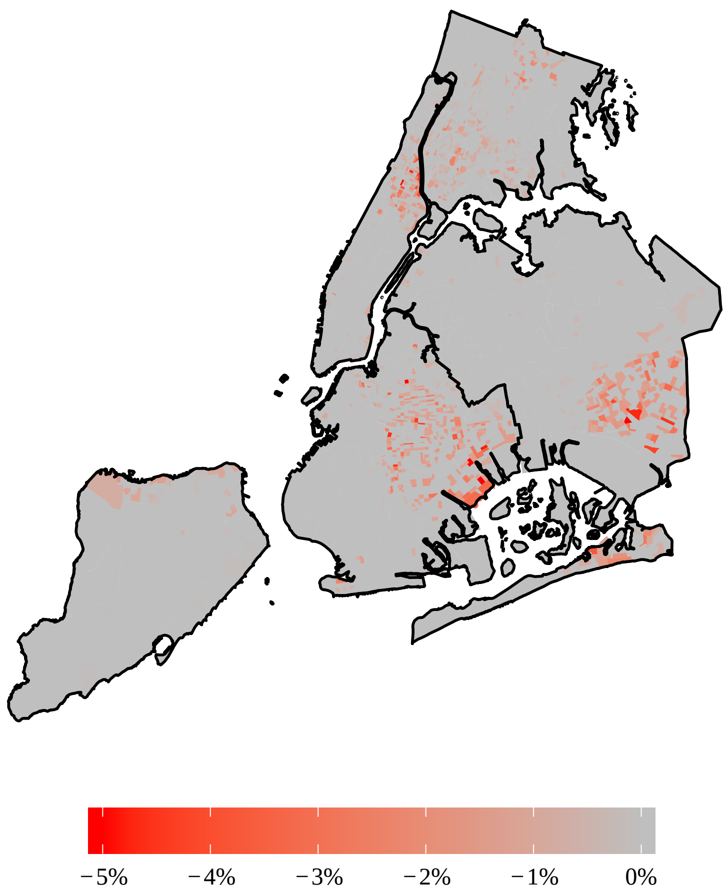

Figure 2: Estimated Depressive Effect of Felony Disenfranchisement

Discussion

During the 2017 mayoral election, felony disenfranchisement laws were responsible for removing an estimated 1,496 from New York City neighborhoods. The spatial concentration of these lost voters is striking: as Tables 1 and 2 make clear, these voters were removed from neighborhoods with significantly lower incomes than the rest of the city. They were also removed from neighborhoods that were far less white than the average neighborhood. The systematic removal of voters from these neighborhoods is troubling. However, more than 850,000 votes were cast in the general election of 2017. Although felony disenfranchisement rules have enormous implications for the individuals targeted by them, it is unlikely that the removal of such a small number of voters — concentrated though they are — is likely to have large implications on its own.

Felony disenfranchisement, however, reaches beyond the individuals who are incarcerated. Previous literature has established that felony disenfranchisement likely has impacts on Black turnout at the state level. 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. These communities are already home to some of the poorest and most marginalized voters in the city; they are home to residents who have the least ability to influence policy through other means such as large campaign contributions. The fact that felony disenfranchisement policies appear to be disincentivizing participation on election day — that these policies are further weakening the ability of these neighborhoods to participate in the democratic process — is cause for alarm.

Executive Order 181 and Turnout in 2018

Prior to 2018, New Yorkers convicted of felony offenses and sentenced to prison were disenfranchised until they had completed all terms of their sentence — their period of incarceration as well as any parole term. For New Yorkers on life parole or sentenced to life in prison, this law resulted in effective lifetime disenfranchisement. New Yorkers sentenced to felony probation, on the other hand, did not lose their voting rights.

On April 18$^{\text{th}}$, 2018, Governor Andrew Cuomo signed Executive Order 181 which effectively ended the disenfranchisement of New Yorkers on parole. Such a move was of course good for the communities in which parolees live; as discussed above, the disenfranchisement of voters has large spillover effects in the neighborhoods in which these lost voters are concentrated. The change in policy is also beneficial for felony probationers: despite the fact that probationers do not formally lose their voting rights, there is evidence that confusion around the law contributes to *de facto* disenfranchisement among probationers (Drucker and Barreras 2005).[10] The Executive Order is a promising step: by changing the policy to allow all New York citizens living in their communities to cast a ballot, the move has the potential to both re-enfranchise the nearly 30,000 New Yorkers on parole living in the community and to clarify the rules about who is eligible to vote.

Of course, re-extending the right to vote is only the first step. As previous literature has established, interactions with the criminal justice system leaves residents far less likely to vote in the future (White 2019; **???**). As Traci Burch (2011) and others have shown, moreover, turnout rates among the formerly incarcerated are vanishingly low. Formal disenfranchisement policy, the literature has made clear, is just one piece of an interlocking system that serves to disenfranchise minority and marginalized voters. To address *only* the formal laws contributing to disenfranchisement without also interrogating the efficacy of any policy change risks leaving much of the system of effective disenfranchisement undisturbed.

There is reason to believe that the policy change may increase the political participation of formerly disenfranchised individuals. Prior to the policy change, formerly incarcerated individuals had their voting rights restored automatically upon the completion of their parole term. Under the Executive Order, however, parole officers were required to explicitly inform their supervisees of their newly restored voting rights. It is possible that having a representative of the government tell parolees of their rights was an effective encouragement for these newly re-eligible voters to cast a ballot.

Identifying Parolees Whose Rights Were Restored

Following Executive Order 181, the Department of Corrections and Community Supervision began indicating on their online Parolee Lookup Tool whether a parolee had her voting rights restored.[11] By using the DIN

[10] I've also talked with folks on probation whose probation officers told them they were disenfranchised. Not sure if it's appropriate to mention anecdotes like these, or to just cite to the literature

[11] Only individuals sentenced to prison for felony offenses lose their voting rights in New York. As such, I remove individuals sentenced to prison for misdemeanors. Incarceration likely also has a *de facto* disenfranchising effect on turnout among individuals incarcerated for misdemeanors, though exploring that effect is beyond the scope of this project.

number provided from the parolee public records request, I was able to identify individuals who did and did not have their voting rights restored.[12]

Although Governor Cuomo signed the Executive Order in April of 2018, an examination of the individuals whose rights were ultimately restored indicates that the program may not have gone into full effect until later in May. As Figure 3 demonstrates, individuals who finished their parole supervision in early May did not have their voting rights formally restored until they finished their term of supervision. It is not until May 21st that the majority of individuals being discharged from parole had their voting rights restored prior to their discharge. However, even after the policy change went into effect, not all parolees had their voting rights restored prior to being discharged from parole. These are likely individuals who are not citizens, and therefore ineligible to register to vote.

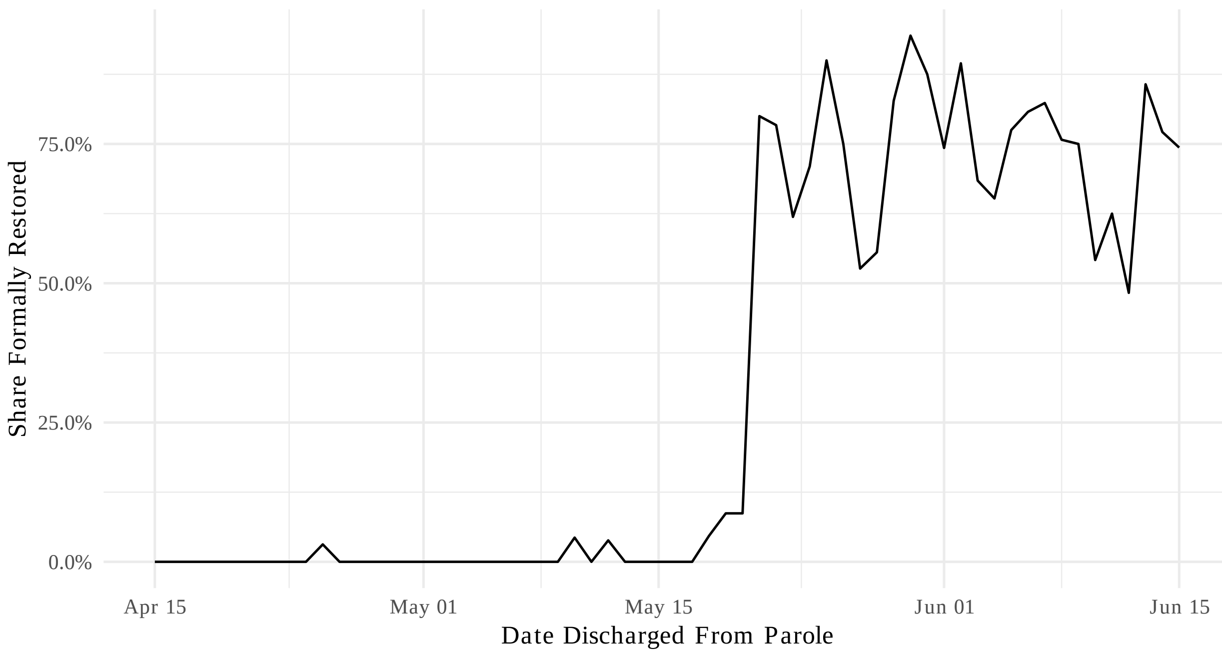

Figure 3: Share of Discharged Parolees Whose Voting Rights Were Restored Prior to Discharge

Figure 3 complicates our ability to cleanly identify the impact of the policy change. If no parolees discharged prior to May 21st had had their rights restored, but all parolees discharged on or after May 21st had, identifying those impacted by the policy would be straightforward. That is not the case: some select individuals seem to have had their rights restored before the policy change was fully implemented. Similarly, we would ideally exclude all individuals who did not have their rights restored after the policy change as well as the individuals who were discharged before the change who would not have had their rights restored if they had been discharged from parole later. However, identifying individuals whose rights would not have been restored is not possible.

In the analyses that follow, I consider all parolees discharged on or after May 21st, 2018, beneficiaries of the policy change, whether their rights were restored or not. I assume that the share of individuals discharged after May 21st whose rights were not restored is comparable to the share of individuals discharged prior to May 21st whose rights would not have been restored if they had been discharged later. I also remove from the analyses that follow the 10 individuals discharged from parole prior to May 21st, 2018, whose rights were restored prior to their discharge.

[12]Not all parolees listed in the public records request data are included in the lookup tool. For individuals who finished parole between January 1st, 2018, and April 17th, 2018, 1.0 percent are not in the lookup tool. For those discharged from parole between April 18th, 2018, and January 13th, 2019 (the latest date of the parole records), 1.2 percent of individuals are not found in the lookup tool.

Trends in Turnout

Before analyzing turnout in the 2018 midterms, I begin by examining turnout in the 2016 election. It is possible that individuals discharged from parole shortly before a federal election are more likely to cast a ballot than individuals discharged earlier. If individuals who were discharged from parole prior to May 21st, 2016, were less likely to vote than those discharged between May 21st and Election Day, disentangling the impact of the policy change in 2018 from the expected uptick would be more complicated. However, as Figure 4 makes clear, individuals discharged from parole in the final months before the 2016 presidential election were not substantially more likely to cast a ballot in the election than individuals discharged earlier in the year. The longer that individuals have been off of parole, the more likely they are to cast a ballot. For instance, of the individuals last discharged from parole in 2010, 6.6% cast a ballot in the 2016 election, while just 4.3% of those last discharged from parole in 2015 did so.[13] Figure 4 plots turnout rates by month of parole discharge. A quadratic curve is fitted (weighted by the number of individuals discharged each month), along with a 95 percent confidence band. This curve is fit on monthly data running from January, 2010 through May, 2016, and extended through October, 2016.

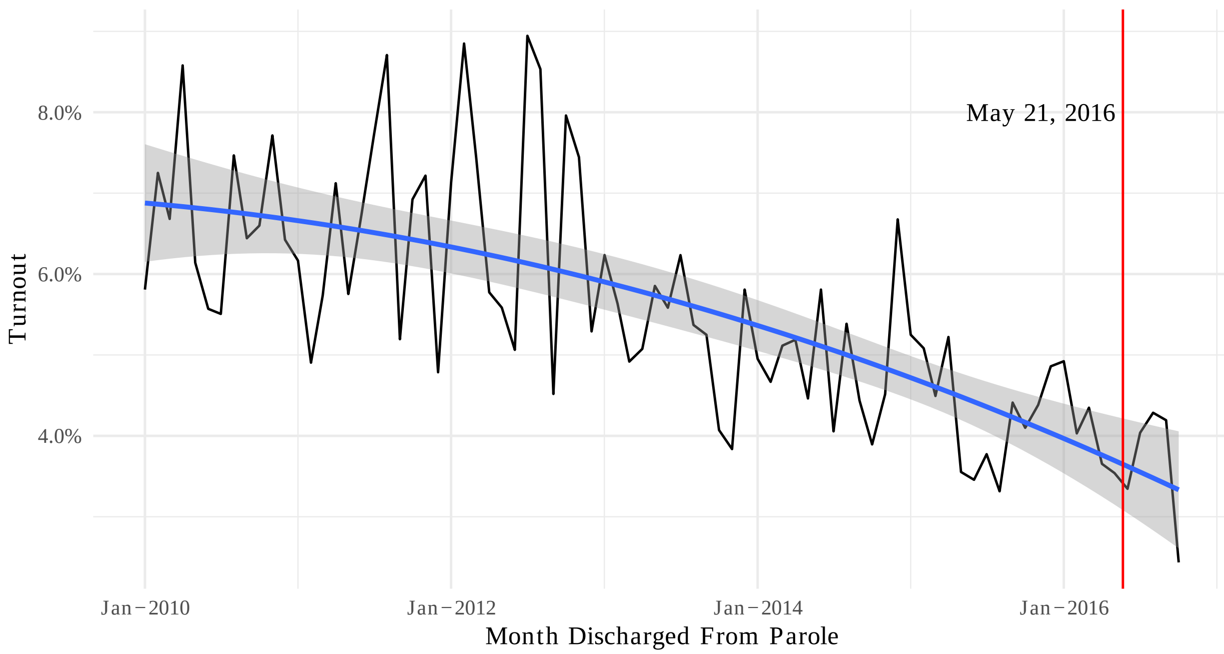

Figure 4: Turnout in 2016 Presidential Election

Figure 5 plots month of parole discharge and turnout in the 2018 midterm elections. Once again, a weighted quadratic curve is fitted with a 95 percent confidence band. This curve is fit on monthly data running from January, 2012 through May, 2018, and extended through October, 2018.

[13] Figure 4 plots individuals' turnout by the last date of discharge from parole. Therefore, individuals discharged from parole in 2010 who reoffended and were discharged from parole again in 2015 are included in the denominator only in 2015.

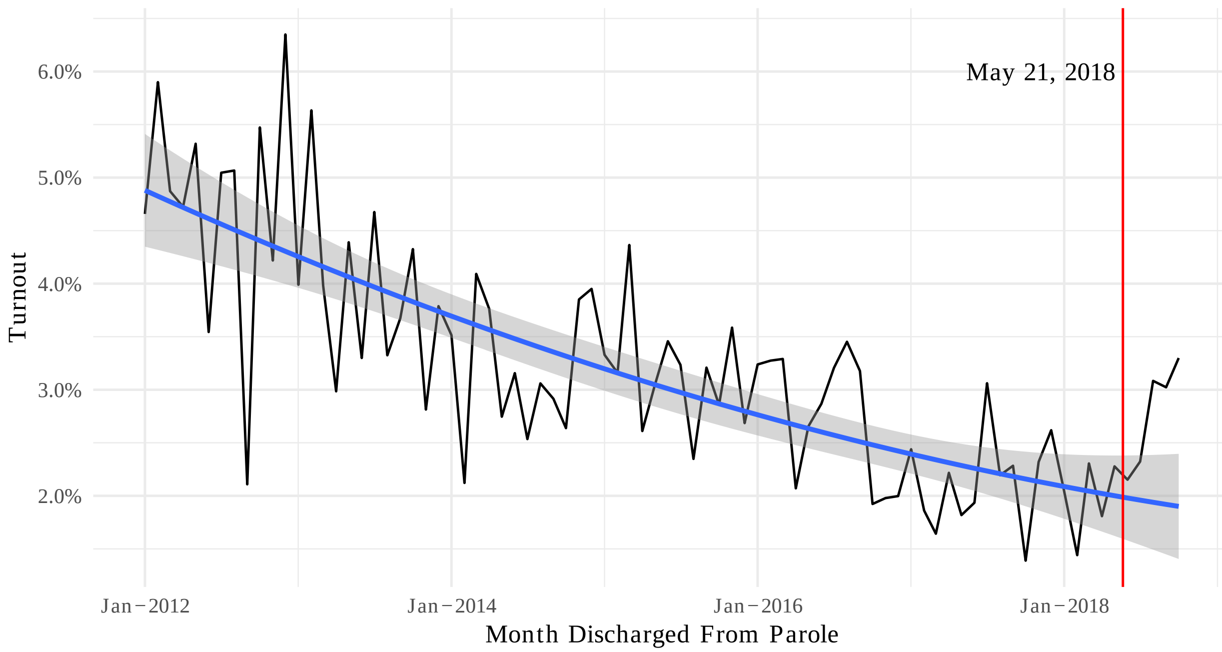

Figure 5: Turnout in 2018 Midterm Election

Figure 4 does not indicate that individuals who were discharged from parole shortly before the 2016 presidential election were more likely to cast a ballot than individuals discharged earlier in the year. Figure 5, on the other hand, indicates that New Yorkers discharged from parole in the months leading up to the 2018 election — New Yorkers whose rights were restored while they were still on parole — were more likely to participate than those discharged earlier in the year. However, Figures 4 and 5 are noisy. In the section that follows, I develop a logit model to explore whether, after controlling for the available characteristics, individuals discharged from parole after May 21st, 2018, were more likely to cast a ballot in the 2018 midterm elections.

Individual-Level Turnout Regressions

Figure 5 displays the share of individuals who were discharged from parole in each month and cast a ballot in the 2018 elections. It does not control for any sort of individual level characterisitcs. In 5, I present the results of an individual-level regression exploring the impact of Executive Order 181 on turnout in 2018. Because turnout is a binary dependent variable, this is a logistic regression. It includes all individuals discharged from parole between January 1st, 2012, through October 10th, 2018 (the registration deadline in New York State).[14]

[14]This of course ignores the individuals who were still on parole and cast a ballot in the 2018 election. However, for this exercise, I limit the analysis group to individuals who would have been eligible to vote on election day even if Governor Cuomo had not signed Executive Order 181.

Table 5: Individual-Level Logit Model

	Cast Ballot in 2018 Election		
	(1)	(2)	(3)
D(Discharged on or after May 21st, 2018)	0.326**	0.335**	0.364***
	(0.132)	(0.133)	(0.134)
Days Since Discharged	0.0004**	0.0003*	0.0004**
	(0.0002)	(0.0002)	(0.0002)
Days Since Discharged^2	0.000	−0.000	−0.00000
	(0.00000)	(0.00000)	(0.00000)
D(Male)		−0.289***	−0.316***
		(0.085)	(0.085)
Age (Years)		0.042***	0.037***
		(0.002)	(0.002)
Counts in Most Recent Sentence			0.022
			(0.036)
Time Spent on Parole (Years)			0.032***
			(0.011)
Constant	−3.975***	−6.491***	−6.436***
	(0.105)	(0.543)	(0.546)
County Fixed Effects		X	X
Race / Ethnicity Fixed Effects		X	X
Felony Class Fixed Effects			X
Observations	55,684	55,684	55,684
Log Likelihood	−7,411.831	−7,055.141	−6,996.050
Akaike Inf. Crit.	14,831.660	14,258.280	14,154.100
Note:			*p<0.1; **p<0.05; ***p<0.01

Model 1 in Table 5 formalizes the trend presented in Figure 5 by controlling only for whether an individual was discharged on or after May 21st, 2018, and the number of days between the individual's discharge date and November 6th, 2018. Model 2 also controls for individual-level characteristics: sex, age on November 6th, 2018, county, and race. Model 3 adds sentence-specific information to Model 2: the number of counts in the individual's most recent sentence, the amount of time they spent on parole, and the class(es) of felony for which they were convicted. In each successive model, the AIC decreases substantially, indicating that including these controls are warranted. Table 5 makes clear that formerly incarcerated men were far less likely to vote than formerly incarcerated women; that older formerly incarcerated individuals were more likely to cast a ballot; and individuals who spent longer on parole were more likely to participate in the midterm election.

Each model also indicates that Executive Order 181 was successful at increasing turnout among formerly incarcerated New Yorkers.[15] Exponentiating the coefficients on D(Discharged on or after May 21st, 2018)

[15] These models do not explicitly control for the potentiality that individuals discharged from parole shortly before an election always turnout at higher rates. In Appendix C, I run the models from Table 5 on turnout in the 2016 presidential election.

indicates that individuals who were discharged after Executive Order 181 went into effect were between 38.6 and 43.9 percent more likely to vote.

Of course, in these models we are *not* directly testing the impact of having ones rights restored on propensity to vote. In the next section, I employ an instrumental variables approach to confirm that Executive Order was in fact successful at raising turnout.

Instrumenting Rights Restoration

In the previous section, I explored whether individuals who were discharged from parole after Executive Order 181 went into effect were more likely to vote, whether or not their voting rights were actually restored. There is reason to believe that whether an individual received the treatment (had their rights restored) is correlated with their propensity to vote. For instance, parolees who were arrested did not have their rights restored; a recent arrest is also likely to impact turnout. Similarly, noncitizens did not have voting rights restored / granted; such citizenship status is also correlated with propensity to vote. Because the treatment was not randomly assigned, direct comparison of turnout rates among individuals who did and did not have their rights restored is not probitive to the effect of Executive Order 181. This calls for an instrumental variables approach.

Although 5 shows that there is a relationship between how long an individual has been off parole and their propensity to vote over the long run, this relationship is far less signficant in the short run. Figure 6 shows that, for individuals discharged from parole in 2017 and 2018, there does not appear to be a relationship between time off parole and turnout rates.

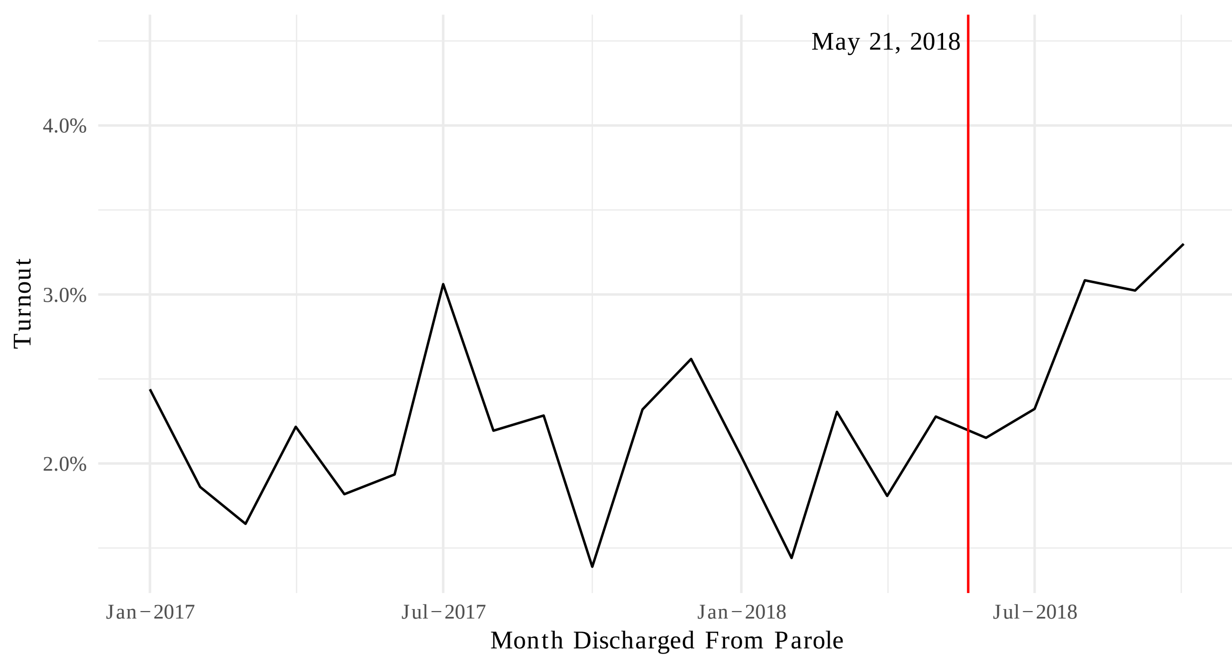

Figure 6: Turnout in 2018 Presidential Election

Formalizing this chart into an individual-level logit model demonstrates that this relationship does not hold over the short run. The models in Table 6 include individuals last discharged from parole between January 1st, 2017, and May 20th, 2018.

Appendix C shows that in 2016, individuals discharged from parole in the months leading up to the election were no more likely to participate in that election

Table 6: Individual-Level Logit Model

	Cast Ballot in 2018 Election	
	(1)	(2)
Days Since Discharged		0.001
		(0.003)
Days Since Discharged^2		-0.00000
		(0.00000)
D(Male)	-0.503^{***}	-0.505^{***}
	(0.182)	(0.182)
Age (Years)	0.044^{***}	0.044^{***}
	(0.005)	(0.005)
Counts in Most Recent Sentence	-0.020	-0.020
	(0.097)	(0.097)
Time Spent on Parole (Years)	0.040^{*}	0.040^{*}
	(0.024)	(0.024)
Constant	-6.378^{***}	-6.606^{***}
	(1.093)	(1.203)
County Fixed Effects	X	X
Race / Ethnicity Fixed Effects	X	X
Felony Class Fixed Effects	X	X
Observations	14,249	14,249
Log Likelihood	$-1{,}325.043$	$-1{,}324.882$
Akaike Inf. Crit.	2,806.086	2,809.764
Note:	*p<0.1; **p<0.05; ***p<0.01	

Adding controls for time in Table 6 increases the AIC, indicating that the inclusion of these variables is not warranted. A Chi-squared test confirms that the model is not improved when controls for time are included.

Although date of discharge is not correlated with turnout over the 2017 − 2018 period, it is clear from Figure 3 that the date of discharge from parole *is* correlated with formal rights restoration. Although not all individuals released from parole after May 21st, many are. A dummy variable indicating whether an individual was released on or after May 21st, 2018, therefore, is a good instrument for formal rights restoration.

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Appendix A

Matching across administrative data sets is not a perfect science. Despite having high-quality low-level data from both New York State Department of Corrections and Community Supervision (DOCCS) and the New York State Board of Elections, I cannot be sure that I am correctly identifying formerly incarcerated, registered voters. The results of the analyses in Table 3 give some reason to believe that there are few false negatives. If there were many false negatives (lost voters who are not identified in this analysis), “treated” neighborhoods would be incorrectly categorized as “untreated” neighborhoods. If this were a widespread problem, we would expect to see little difference between treated and control neighborhoods. To the extent that lost voters go unidentified by this analysis, it is likely that the results presented in Tables 3 and 4 are conservative by including treated neighborhoods in the control group. Directly testing for false negatives, however, is difficult. If a voter changes her name before being incarcerated, for instance, our analysis will fail to capture her.

Testing for the presence of false positives is slightly easier. One way to do so is by constructing false records (Meredith and Morse 2013). By slightly changing the birthdates in one set of administrative data, I can estimate how frequently DOCCS records inaccurately match with voter records. To do so, I shift the dates of birth in the DOCCS data by 35 days. I then re-merge the DOCCS records (with “false” birth dates) against the registered voter file.

Table 7 shows the results of these permutations. The first row shows the matches with the raw data. The second row shows the results of adding 35 days to the date of birth in each DOCCS record, and the final row shows the result of subtracting 35 days from these records.

Table 7: Results of Shifting Birthdates

Group	Number of Matches Between DOCCS and Voter File Records
Actual Birthdate	12,967
Birthdate + 35 Days	72
Birthdate - 35 Days	69

Table 7 indicates that there are likely some false positives in our matches. It is not surprising that in a large state like New York, full names and dates of birth fail to uniquely identify individuals. However, the rate of false positives is quite low: this analysis indicates that there is a 0.56%. It is unlikely that such a small incidence of false positives meaningfully impacts this analysis.

Appendix B

As noted above, the turnout figures reported in the registered voter file do not align with the results reported by the New York City Board of Elections. According to the official results, 1,143,321 ballots were cast in the 2017 mayoral election. The registered voter file reports that just 915,982 voters cast a ballot. This is not necessarily evidence of poor management by the Board of Elections. Some voters choose not to make their voter registration information publicly available (such as domestic violence survivors and officers of the court).[16] Where more ballots are recorded by the BOE than in the voter file, turnout calculated from the voter file will be artificially lower. If the voter file undercount rate is systematically worse in neighborhoods with lost voters, this would pose a serious challenge to the validity of the results reported in the body of this paper.

To test whether there is a relationship between lost voters and voter file undercount, I examine the undercount rate at the precinct level. The voter file indicates the home precinct of each voter, and the Board of Elections publishes election results at the precinct level. Precincts are assigned the demographics of the block group in which they are situated. Where precincts cross block group lines, the precinct is assigned the characteristics of all block groups in which it is located weighted by the number of voters from each block group. The undercount rate is calculated as the number of votes derived from the voter file divided by the number reported in BOE results. Robust standard errors are clustered at the assembly district level.

[16]This is not to say that there is *not* evidence of mismanagement by the BOE. For instance, the author is incorrectly marked as not participating in the 2017 mayoral primary.

Table 8: Registered Voter File Ballot Undercount

	Dependent variable:
	Undercount Rate
Lost Voters	−0.014
	(0.020)
Median Income (Thousands of Dollars)	0.00000**
	(0.00000)
Percent Latino	0.343***
	(0.114)
Percent Non-Hispanic Black	0.091
	(0.100)
Percent Non-Hispanic White	0.256***
	(0.097)
Percent With Some College	−0.302**
	(0.149)
Median Age	0.001
	(0.002)
Registration Rate	0.002
	(0.012)
Percent Democrats	−0.268
	(0.199)
Percent Noncitizen	−0.110
	(0.241)
Percent Won by City Council Representative	−0.093
	(0.107)
Constant	1.003***
	(0.195)
Observations	5,528
R^2	0.056
Adjusted R^2	0.054
Note:	*p<0.1; **p<0.05; ***p<0.01 Robust standard errors (clustered by assembly district) in parentheses.

As Table 8 makes clear, there is very little relationship (p = 0.47) between the number of lost voters in a precinct and the ballot undercount rate. Although researchers should be somewhat wary using turnout rates derived from New York State's registered voter file, there is no evidence that the reporting error impact this analysis.

Appendix C

Table 5 shows that individuals who were discharged from parole after Governor Cuomo signed Executive Order 181 were substantially more likely to cast a ballot in the 2018 election than individuals who were discharged prior to the policy change. It is, of course, possible that individuals discharged from parole immediately before an election are always more likely to vote, due perhaps to the recency of their interation with state systems and a desire to have their voices heard. In that case, claiming that Executive Order 181 caused turnout to increase would be a mistake.

To test whether this is true — that individuals discharged shortly before an election turn out at higher rates — I fit the models from Table 5 on 2016 turnout. Table 9 includes all individuals last discharged from parole between January 1st, 2010, and October 14th, 2010.

Table 9: Individual-Level Logit Model

	Cast Ballot in 2016 Election		
	(1)	(2)	(3)
D(Discharged on or after May 21st, 2016)	0.067	0.071	0.059
	(0.110)	(0.110)	(0.111)
Days Since Discharged	0.001***	0.0005***	0.0005***
	(0.0002)	(0.0002)	(0.0002)
Days Since Discharged^2	−0.00000**	−0.00000*	−0.00000
	(0.00000)	(0.00000)	(0.00000)
D(Male)		−0.461***	−0.471***
		(0.073)	(0.074)
Age (Years)		0.029***	0.025***
		(0.002)	(0.002)
Counts in Most Recent Sentence			−0.083**
			(0.040)
Time Spent on Parole (Years)			0.018*
			(0.009)
Constant	−3.341***	−4.402***	−4.285***
	(0.085)	(0.356)	(0.358)
County Fixed Effects		X	X
Race / Ethnicity Fixed Effects		X	X
Felony Class Fixed Effects			X
Observations	44,304	44,304	44,304
Log Likelihood	−9,016.947	−8,727.910	−8,677.662
Akaike Inf. Crit.	18,041.900	17,603.820	17,517.320
Note:			*p<0.1; **p<0.05; ***p<0.01

Although turnout was generally higher in 2016 than in 2018 (reflecting statewide higher turnout thanks to the presidential contest), there is no evidence that individuals discharged in the summer of 2016 were more likely to cast a ballot than individuals who finished parole earlier (the p-value on the coefficient of interest exceeds 0.5 in each model). The nonsignificant results from 2016 provides strong corroboration for causal claims regarding the efficacy of Executive Order 181 at boosting turnout.