

Neighborhoods and Felony Disenfranchisement: The Case of New York City

Kevin Morris

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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 in a liberal democracy. 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. As Michelle Alexander (2012) and others have shown, 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

¹Latinos 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.

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 credited with having a “chilling effect” on neighborhoods’ willingness to reach out for help to local governments (Lerman and Weaver 2013).

Felony disenfranchisement policies are part of an interlocking 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.

The rest of this paper is structured as follows. **Section 2** discusses the historical context of felony disenfranchisement in the United States, and **Section 3** provides an overview of the scholarship conducted in the field over the past two decades. **Section 4** provides an in-depth discussion of the insight extant literature provides for the questions discussed in

this project. **Section 5** presents the data sources used throughout the project.

Section 6 proposes a new definition of “lost voters” in the context of felony disenfranchisement. I define lost voters as individuals who are not only disenfranchised, but are disenfranchised and have a history of participating in elections. This serves as a refinement on past work, which considered only which individuals are formally disenfranchised, regardless of whether they had cast a ballot in the past. Using both a matching model and a standard regression specification, I investigate whether neighborhoods with lost voters saw depressed turnout in the 2017 mayoral election relative to other neighborhoods. The disadvantaged state of neighborhoods with high levels of incarceration has been firmly established; if felony disenfranchisement results in lower turnout among eligible voters in these communities, the literature has likely *understated* the degree to which they are impacted by spatially concentrated patterns of criminal justice. A discussion of the results follows.

Section 7 turns from the 2017 mayoral election to the 2018 midterm elections. In early 2018, Governor Andrew Cuomo signed Executive Order 181 which restored voting rights to most individuals in the state who were on parole. I employ an instrumental variables approach test whether rights restoration boosted turnout among formerly incarcerated individuals. I focus specifically on individuals who would have been eligible to cast a ballot even if the Executive Order had not gone into effect. A discussion of the results follows.

Section 8 synthesizes the findings presented in the rest of the paper and provides concluding remarks.

2 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. The Supreme Court, in cases such as *Richardson v. Ramirez* (1974), 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).²

Any discussion of felony disenfranchisement in the United States must center the role played by race. As Traci Burch has explained, “If policies restricting the voting rights of offenders disparately affect one racial group or party, it is because such policies were *intended* to” (Burch 2010, 4; emphasis in the original). 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, for instance, felony disenfranchisement was added to the state constitution in 1868. Afterwards, a lawmaker boasted that the amendment had been added to prevent the state from being “niggerized” (Shofner 1963, 374).

The racially unbalanced effects of felony disenfranchisement laws were 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

²The figures reported in Uggen, Larson, and Shannon (2016) have been adjusted to reflect the impact of Amendment 4 in Florida.

disenfranchisement. Indeed, as the United States has 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 from 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).

3 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 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).

In a series of papers 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 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. 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. 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; 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” (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, less 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 involvement with the criminal justice system, determining that “at high concentrations, imprisonment and community supervision have an unequivocally demobilizing effect of neighborhoods” (185). 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 definition of “lost voters.”

4 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 using individual-level administrative

data. Policing and incarceration patterns have historically targeted communities of color, wreaking havoc on 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 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 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.

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, 2018, Governor Andrew Cuomo signed Executive Order 181 which restored voting rights to New Yorkers on parole.³ Parole officers were required to provide registration forms to 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 use administrative data to explore whether turnout rates in the 2018 midterm elections among individuals discharged from parole were impacted by the implementation of Executive Order 181.

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

³Prior 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.

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. 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 by neighborhoods: Kinsella, McTague, and Raleigh (2015), for instance, 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 do 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 — 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, reporting 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.

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 prohibitive to the effect of neighborhood turnout rates. Insofar as neighborhoods vote as a bloc, their relative turnout rates may influence city policy. If felony disenfranchisement has a depressive effect on turnout, politicians are likely less responsive to the particular preferences of voters in these neighborhoods — some of the most neglected neighborhoods to begin with.

It should be noted that, 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 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 (Gimpel2004; 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.

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 causal effect of incarceration on participation is the subject of some debate within the field. Individuals who go to prison share many characteristics 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 on subsequent participation” (p. 1144).

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” (p. 829).

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 for this population.

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 them 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 notify formerly disenfranchised individuals of their re-instated voting rights. Meredith and Morse (2013) finds no effect from notification in the absence of eligibility changes.

Gerber et al. (2014) conducted a field experiment in Connecticut in advance of the 2012 presidential election, finding 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” (p. 924). 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 eligibility 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).

5 Data

Criminal Justice Data

The primary criminal justice dataset comes from a public records request filed by the author to obtain 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 come from the New York State Department of Corrections and Community Supervision (NYSDOCCS) and are used to determine when individuals were incarcerated or on parole, 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 also only includes individuals who are held in state prisons.

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.⁴

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

⁴This list was compiled by using a webscraper written in the Python language.

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 30th, 2018. The second section of this paper uses a snapshot from March 3rd, 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 analysis; 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 parolee’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 parole discharge dataset to create false records. Comparing the number of matches between these “fake” discharge 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 when I construct a set of fake records by adding or subtracting 35 days from a parolee’s birthdate. This analysis indicates that false positives account for between 0.6 and 0.7 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. For three reasons, however, 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 discharged from felony parole. Secondly, because both parolee discharge and voter registration are legal records, individuals are likely to be recorded using their full names (that is to say, an individual is unlikely to be “John” in one set of records and “Jonathan” in the other). Finally, rates of false negatives are likely to be constant within the state during the study period, and there is no reason to believe that these false negatives would be associated with being discharged from parole after the Executive Order went into effect.

6 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. 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 provides insight only into the effect of incarceration on turnout, not 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 through socialization mechanisms. 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 7th, 2017. Lost voters, therefore, are all individuals who were incarcerated or on parole on November 7th, 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.

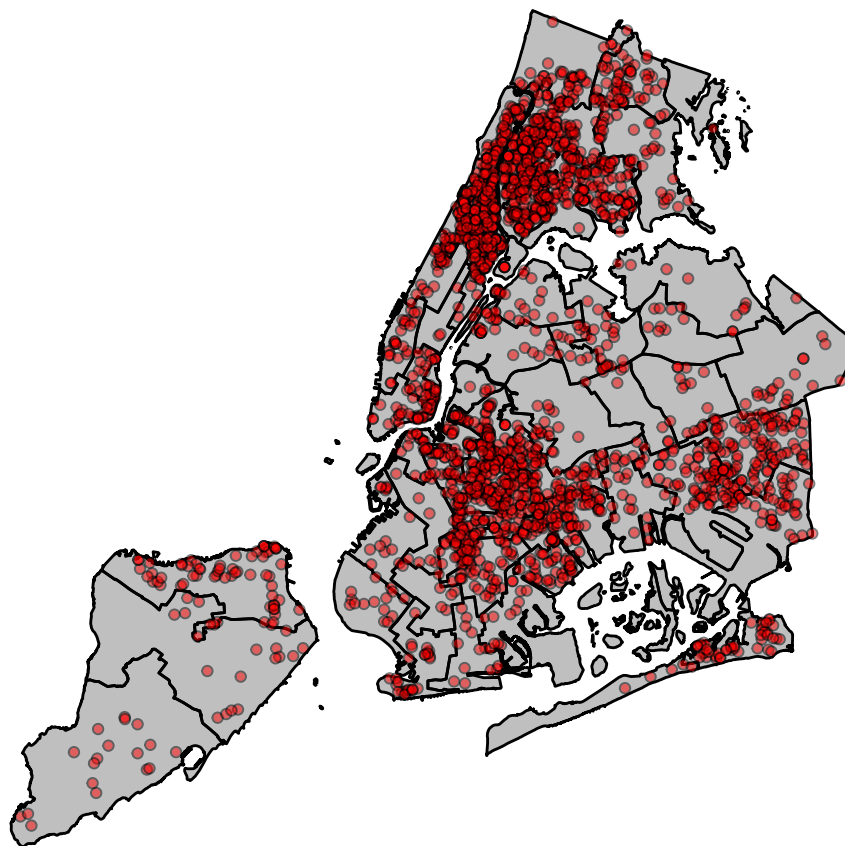


Figure 1: Lost Voters on Election Day, 2017

The spatial concentration of lost 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.⁵ 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 noncitizen⁶ come from the Census Bureau. Party affiliation rates

⁵It 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.

⁶The 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.

come from the geocoded voter file. Registration rate is calculated by dividing the number of registered voters (calculated using the voter file) by the voting age population (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;⁷ 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.

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	90.49	88.36	83.97	76.85
% Non-Hispanic Black	0.38	0.16	0.38	0.37	97.67	93.15	93.20	91.76
% Non-Hispanic White	0.17	0.40	0.17	0.18	95.12	95.00	94.33	91.84
Median Income	51,792.52	72,612.36	51,792.52	52,608.32	96.08	94.48	90.50	76.95
% With Some College	0.64	0.70	0.64	0.64	92.52	92.60	90.93	86.76
Median Age	35.99	38.34	35.99	36.29	87.54	87.68	85.43	78.61
Registration Rate	0.80	0.75	0.80	0.79	68.58	79.87	77.76	62.01
% Democrats	0.75	0.65	0.75	0.75	95.94	95.88	94.58	90.80
% Noncitizen	0.16	0.16	0.16	0.16	5.42	10.29	-14.22	-79.07
% Won by City Council Representative	0.85	0.81	0.85	0.86	85.91	84.15	78.63	66.82

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	0.32	99.21	87.37	82.29	71.63
% Non-Hispanic Black	0.36	0.14	0.36	0.35	95.36	87.10	87.29	84.42
% Non-Hispanic White	0.19	0.42	0.19	0.20	98.00	95.76	93.52	87.03
Median Income	53,409.42	71,754.50	53,409.42	55,253.14	89.95	94.83	86.30	58.83
% With Some College	0.65	0.71	0.65	0.65	87.14	90.80	87.15	70.70
Median Age	35.76	38.04	35.76	35.71	97.64	78.00	83.11	72.89
Registration Rate	0.75	0.72	0.75	0.74	52.97	71.10	72.16	66.63
% Democrats	0.74	0.62	0.74	0.73	92.81	93.05	89.49	79.90
% Noncitizen	0.16	0.17	0.16	0.17	-109.37	30.58	21.03	-24.85
% Won by City Council Representative	0.85	0.79	0.85	0.85	95.08	89.68	84.29	68.29

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

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.

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).⁹

⁸The New York registered voter file does not align exactly with results reported by the city. The voter file indicates that just 906,870 voters cast a ballot in the 2017 mayoral election, but the Board of Elections reports that 1,143,321 votes were cast. In Appendix A, I demonstrate that there is no relationship between lost voters and underreporting of cast ballots at the precinct level. These reporting anomalies are unlikely to impact this analysis.

⁹The observations in these models are not weighted by voting age population. Such weighting materially impacts neither the size or statistical significance of the coefficients of interest.

Table 5: Matching Regression

	<i>Dependent variable:</i>			
	Turnout Rate			
	Block Group Level		Tract Level	
	(1)	(2)	(3)	(4)
D(Neighborhood Lost a Voter)	−0.009*** (0.002)	0.0002 (0.003)	−0.004* (0.002)	0.007** (0.003)
Share Non-Hispanic Black		−0.0001 (0.004)		0.014*** (0.004)
D(Lost Voter) × Share Non-Hispanic Black		−0.024*** (0.007)		−0.031*** (0.008)
Constant	0.132*** (0.001)	0.132*** (0.003)	0.126*** (0.001)	0.121*** (0.003)
Observations	46,407	46,407	10,219	10,219
R ²	0.003	0.006	0.001	0.005
Adjusted R ²	0.003	0.006	0.001	0.005

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 0.9 percentage points lower than in comparable block groups without lost voters. This decrease, however, appears to be entirely concentrated within Black neighborhoods. When the dummy identifying neighborhoods with lost voters is interacted with the share of the neighborhood that is Non-Hispanic Black, the basic treatment dummy becomes non-significant. 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 2.4 percentage points lower than similar neighborhoods without lost voters. Considering that the overall turnout rate in block groups with a lost voter was just 11.8 percent, this effect

is alarmingly high. For every 100 votes cast in an entirely Black block group with a lost voter, as many as 20.4 votes went uncast. The effects are slightly larger when neighborhoods are measured as block groups than as census tracts. This is not surprising — because the spillover effects are likely to operate through social networks, smaller geographical units are likely to be more effected.

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.¹⁰

¹⁰Where neighborhoods cross city council district lines, they are assigned the district in which most of their voters live for clustering purposes. The observations in these models are not weighted by voting age population. Such weighting materially impacts neither the size or statistical significance of the coefficients of interest.

Table 6: Standard Regression

	<i>Dependent variable:</i>			
	Turnout Rate			
	Block Group Level		Tract Level	
	(1)	(2)	(3)	(4)
Lost Voters	−0.008*** (0.003)	−0.001 (0.003)	−0.004** (0.002)	0.001 (0.002)
Lost Voters × Share Non-Hispanic Black		−0.017** (0.008)		−0.012** (0.006)
Median Income (Thousands of Dollars)	0.0002* (0.0001)	0.0002* (0.0001)	0.0002 (0.0002)	0.0002 (0.0002)
Percent Latino	0.061*** (0.022)	0.058*** (0.022)	0.077*** (0.025)	0.071*** (0.024)
Percent Non-Hispanic Black	0.054* (0.030)	0.061* (0.031)	0.070** (0.033)	0.084** (0.035)
Percent Non-Hispanic White	0.100*** (0.022)	0.100*** (0.022)	0.117*** (0.027)	0.116*** (0.027)
Percent With Some College	−0.003 (0.028)	−0.003 (0.028)	−0.030 (0.042)	−0.028 (0.042)
Median Age	0.002*** (0.0004)	0.002*** (0.0004)	0.002*** (0.001)	0.002*** (0.001)
Registration Rate	0.196*** (0.013)	0.196*** (0.013)	0.208*** (0.022)	0.207*** (0.021)
Percent Democrats	−0.082 (0.076)	−0.084 (0.076)	−0.086 (0.082)	−0.094 (0.082)
Percent Noncitizen	−0.098** (0.044)	−0.096** (0.044)	−0.084* (0.051)	−0.081 (0.051)
Percent Won by City Council Representative	−0.022 (0.036)	−0.022 (0.036)	−0.013 (0.036)	−0.014 (0.035)
Constant	−0.058 (0.044)	−0.057 (0.044)	−0.086 (0.055)	−0.080 (0.055)
Observations	26			
R ²	5,802	5,802	2,091	2,091
Adjusted R ²	0.477	0.479	0.525	0.528
	0.477	0.478	0.523	0.525

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 0.77 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 1.66 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 $-0.018 \times \text{Lost Voters} \times \text{Share Black}$.

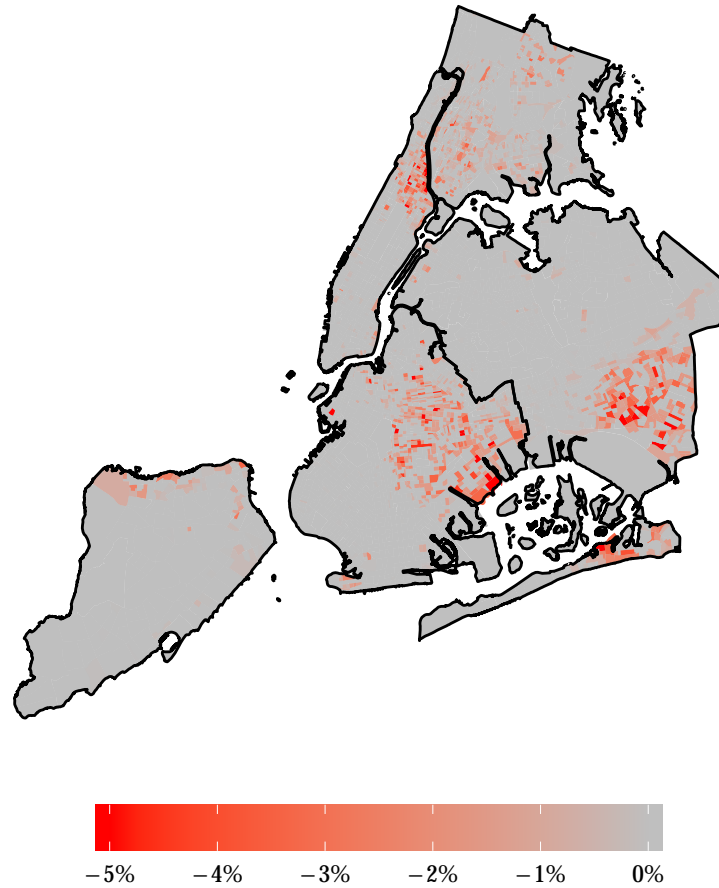


Figure 2: Estimated Depressive Effect of Felony Disenfranchisement

Discussion

During the 2017 mayoral election, felony disenfranchisement laws were responsible for removing an estimated 2,518 from New York City neighborhoods. The spatial concentration of these lost voters is striking, as demonstrated in 1. 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 enormous 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 individ-

uals who are incarcerated. Previous literature has established that felony disenfranchisement likely impacts 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. As discussed above, these neighborhoods have far lower incomes than the rest of the city. The median income of a block groups without 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 insignificant 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. On average, the voting age population in these neighborhoods was 983. Model 2 of Table 5 indicates that, in a block group that is 90 percent Black with a voting-age population of 983, having any lost voter reduced turnout by 21.4 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 2.4 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 14.7 ballots. In block groups of the same size where the population is just 10 percent Black, lost voters cost the neighborhood just 1.6 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. 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 governments and their identities as citizens. It is possible that Black communities understand the loss of voters differently than non-Black communities, and therefore respond differently.

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.

Interestingly, 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; Henderson et al. 1997; Wu, Sun, and Triplett 2009). It should come as no surprise that experience with the criminal justice system less likely to be viewed in isolation

(and therefore more likely to effect voting patterns) in Black neighborhoods.

Whether this finding is surprising or not, it is troubling. Build8 this out more

7 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 18th, 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).¹¹ 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 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 extremely 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.

¹¹I'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

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.

In the analysis below, I examine the effect of Executive Order 181 on individuals who finished parole before October 10th, 2018. These are the individuals who would have been eligible to cast a ballot whether or not the Executive Order was signed. This is, of course, only a subset of the individuals who were impacted by Executive Order 181 — many of the individuals who had their voting rights restored were still on parole on Election Day, and therefore were eligible to vote only because of the rules change. However, by focusing on individuals who would have been eligible to vote either way, we can test whether rights restoration prior to parole discharge serves as an effective encouragement to cast a ballot for this population.

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. By using the identification number provided from the parolee public records request, I was able to identify individuals who had their voting rights restored.¹²

¹²Not 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, whether or not their voting rights were restored. However, as Figure 3 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. The longer an individual has been off of parole, the more likely he is to cast a ballot. For instance, of the individuals last discharged from parole in 2010, 6.5% cast a ballot in the 2016 election, while just 4.1% of those last discharged from parole in 2015 did so.¹³ Figure 3 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 April, 2016, and extended through October, 2016.

¹³Figure 3 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 only in 2015.

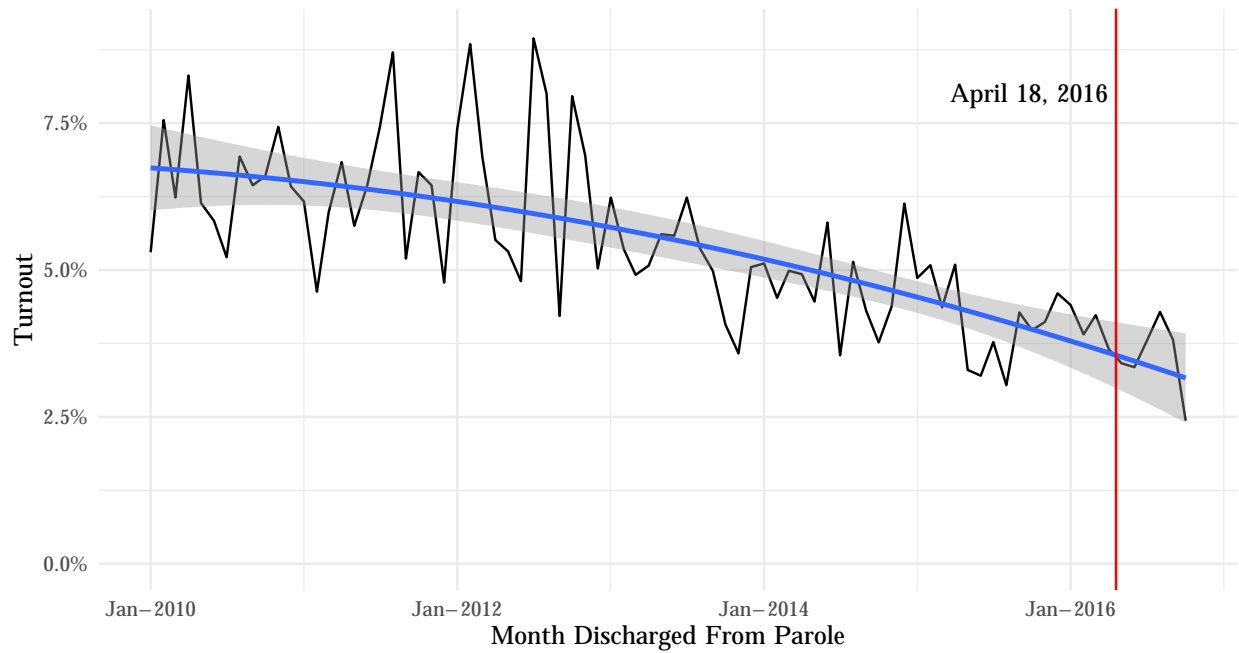


Figure 3: Turnout in 2016 Presidential Election

Figure 4 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 April, 2018, and extended through October, 2018.

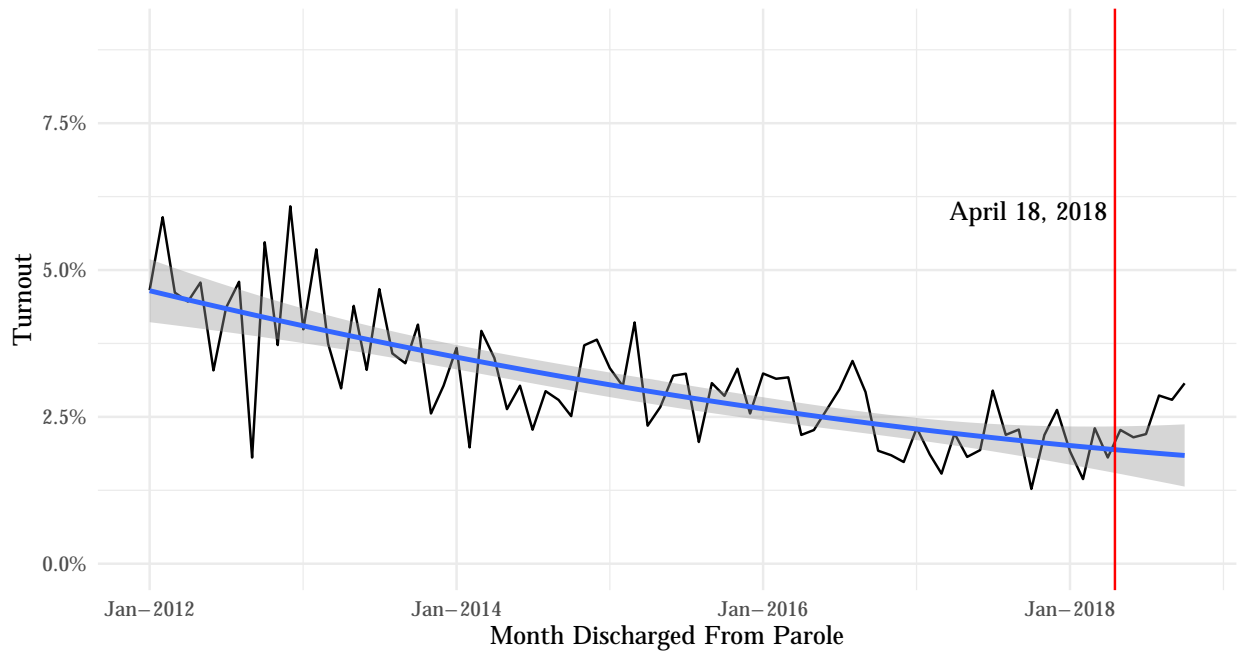


Figure 4: Turnout in 2018 Midterm Election

Figure 3 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 4, on the other hand, indicates that New Yorkers discharged from parole in the months leading up to the 2018 election — many of whom had their rights restored while they were still on parole — were more likely to participate than those discharged earlier in the year. However, Figures 3 and 4 are noisy and do not prove that Executive Order increased turnout.

Individual-Level Turnout Regressions

When considered over a multi-year period, the enactment of Executive Order 181 cannot be understood as a natural experiment. The longer an individual has been off of parole, the more likely she is to cast a ballot, but only individuals recently discharged from parole were eligible to have their voting rights restored prior to discharge. For a true natural experiment to hold, an individual’s probability of being “assigned” to treatment (here, discharged from

parole after the Executive Order went into effect) must be uncorrelated with the outcome of interest (propensity to vote). Figures 3 and 4 indicate that this is not the case when considering individuals discharged from parole over multiple years.

However, the relationship between time-off-parole and propensity to vote is far weaker in the short term. Figure 5 indicates that parole discharge date and turnout rates in the 2018 midterm election are not correlated when we limit the analysis to individuals discharged in 2017 or 2018.

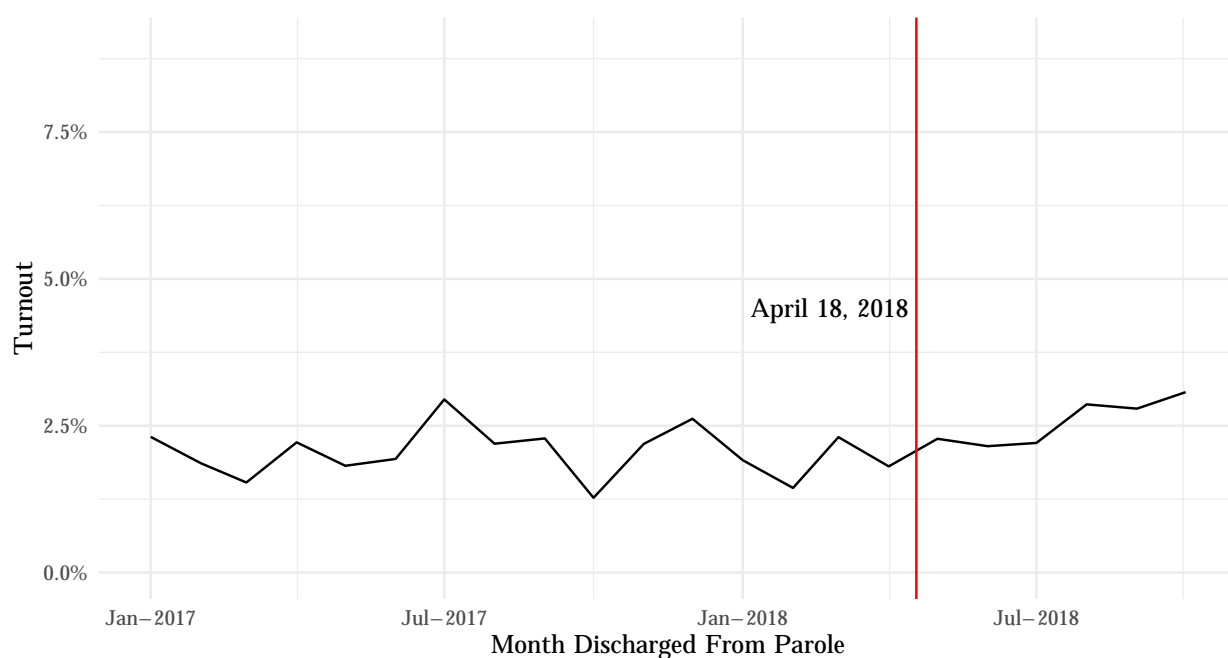


Figure 5: Turnout in 2018 Midterm Election

Formalizing this chart into an individual-level logit model demonstrates that time since discharge is not correlated with turnout in the short run. The models in Table 7 include individuals last discharged from parole between January 1st, 2017, and April 17th, 2018.

Table 7: Individual-Level Logit Model

	Cast Ballot in 2018 Election	
	(1)	(2)
Days Since Discharged		0.0002 (0.003)
Days Since Discharged ²		−0.00000 (0.00000)
D(Male)	−0.617*** (0.179)	−0.618*** (0.179)
Age (Years)	0.045*** (0.005)	0.045*** (0.005)
Years on Parole	0.059** (0.026)	0.059** (0.026)
Constant	−6.591*** (1.045)	−6.583*** (1.232)
Race / Ethnicity FE	X	X
Felony Class FE	X	X
Observations	13,260	13,260
Log Likelihood	−1,256.429	−1,256.306
Akaike Inf. Crit.	2,542.857	2,546.613
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

The inclusion of time controls in Table 7 increases the AIC. A Chi-squared test confirms that the model is not improved when controls for time are included.¹⁴ When we look only at individuals recently discharged from parole, the length of time an individual has been off parole is not associated with his propensity to vote.

Although Governor Cuomo signed the Executive Order on April 18, 2018, an examination of the individuals whose rights were ultimately restored indicates that the program did not

¹⁴Appendix B provides further corroboration that being discharged from parole in the months before an election is uncorrelated with propensity to vote by exploring turnout rates in the 2016 presidential election.

go into full effect until later in May. Figure 6 plots the share of individuals discharged from parole each day in the spring of 2018. It is not until May 18th that there were consistently parolees who had their voting rights restored prior to discharge.

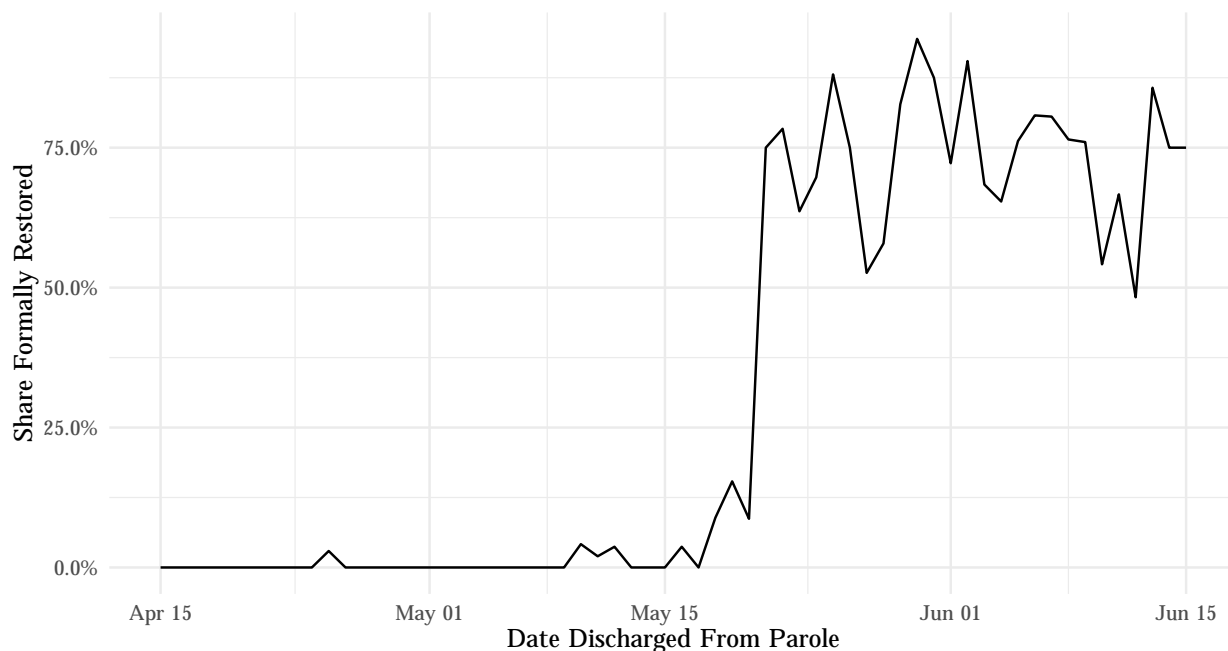


Figure 6: Share of Discharged Parolees Whose Voting Rights Were Restored Prior to Discharge

Although it perhaps seems counterintuitive to throw out many of our observations, ignoring all individuals last discharged from parole prior to 2017 give us an important asset: it allows us to conceptualize Executive Order 181 as a natural experiment. Whether an individual was discharged from parole before or after May 18th, 2018, is akin to a randomly assigned treatment. Individuals who were discharged from parole prior to the implementation of the Executive Order are part of the “control group”, while individuals discharged after are “treated” by the policy change. Any observed difference in turnout between individuals discharged before and after late May can therefore be attributed to the Executive Order.

In Table 8, I present the results of an individual-level logistic regression exploring whether individuals who were discharged on or after May 18th, 2018, turned out at higher rates than

those discharged earlier. The models include all individuals discharged from parole between January 1st, 2017, through October 10th, 2018 (the registration deadline in New York State).

Table 8: Individual-Level Logit Model

	Cast Ballot in 2018 Election		
	(1)	(2)	(3)
D(Discharged After EO 181)	0.210* (0.115)	0.271** (0.115)	0.276** (0.116)
D(Male)		-0.389** (0.156)	-0.422*** (0.157)
Age (Years)		0.051*** (0.004)	0.045*** (0.004)
Years on Parole			0.039* (0.021)
Constant	-3.871*** (0.059)	-7.219*** (1.029)	-7.147*** (1.033)
Race / Ethnicity FE		X	X
Felony Class FE			X
Observations	18,423	18,423	18,423
Log Likelihood	-1,910.700	-1,819.639	-1,804.959
Akaike Inf. Crit.	3,825.400	3,659.278	3,641.918
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01		

Model 1 in Table 8 formalizes the trend presented in Figure 5 by controlling only for whether an individual was discharged after Executive Order 181 went into effect. Model 2 also controls for individual-level characteristics: sex, age on November 6th, 2018, 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. Table 8 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 individuals discharged from parole after the Executive Order went into effect were more likely to cast a ballot than those discharged earlier. Exponentiating the coefficients on D(Discharged After EO 181) indicates that Executive Order 181 raised the turnout rate among formerly disenfranchised individuals by between 23.4 and 31.8 percent.

Instrumental Variables Approach

Table 8 indicates that Executive Order was successful at increasing turnout among all formerly disenfranchised individuals. Although this is important information for policymakers and advocates hoping to increase the political representation of formerly incarcerated individuals as a whole, it does not shed light on the extent to which having one’s voting rights restored prior to discharge from parole impacts one’s propensity to vote. To answer that question, we must specifically control for whether an individual actually had his rights restored before he was discharged from parole.

Investigating the causal effect of rights restoration using the natural experiment conceptualizes introduces a complication: not all individuals who were discharged from parole after Executive Order 181 went into effect had their rights restored — that is to say, not all individuals assigned to the treatment group “complied.”

An individual’s propensity to vote can be expressed using the following equation:

$$Y_i = b_0 + b_1X_{1i} + b_2X_{2i} + b_3Z_i + \epsilon_i$$

Where Y_i is 1 if individual i cast a ballot, X_{1i} is the probability that individual i was eligible to have his rights restored prior to discharge from parole, and X_{2i} is 1 if individual i actually

had his rights restored prior to discharge from parole. Z is a vector of other factors (such as age and race) known to influence voter turnout. Given that we cannot observe X_{1i} , we could simply choose to ignore it. Doing so, however, will result in consistent regression coefficients only if X_{1i} and X_{2i} are uncorrelated or if b_1 is uncorrelated with an individual's propensity to vote (Y_i). Neither of these assumptions are valid: an individual's eligibility to have her rights restored is certainly correlated with whether or not they actually were restored: the Executive Order required that all eligible individuals have their voting rights restored.

There is also reason to believe that an individual's likelihood of having his rights restored is correlated with his natural propensity to vote. Individuals who were discharged from parole but are not U.S. citizens, for instance, did not have voting rights restored (or, in this case, granted) after the Executive Order went into effect. This ineligibility to have voting rights granted is of course correlated with these individuals' propensity to vote: non-citizens cannot cast ballots in elections in New York State. The available data does not indicate whether individuals were eligible to have their rights restored. We can, however, examine the demographic differences between individuals who had their rights restored and those who did not.

Table 9: Demographics of "Compliers" and "Non-Compliers"

Variable	Rights Restored	Rights Not Restored
Percent Male*	88.9%	92.9%
Percent NH-Black*	43.5%	37.2%
Percent NH-White*	34.5%	22.5%
Percent Latino*	18.7%	33.9%
Age (Years)	40.5	39.9
Time Spent on Parole (Years)*	2.0	2.5
Percent Voted in 2016	1.4%	1.0%

Note:

* Difference is significant at 95% confidence level.

Shows demographics for individuals discharged from parole between May 18th, 2018, and October 12th, 2018.

Table 9 makes clear that the restoration of voting rights ("compliance") is strongly correlated

with certain demographics that are also linked to propensity to vote. Men, for instance, were less likely to have their rights restored than women. Table 8 above indicates that men who have been on probation are less likely to vote than women. Similarly, Latinos made up more than a third of individuals whose rights were not restored, but just 19 percent of individuals who did have voting rights restored. This is likely a reflection of citizenship status. Table 9 indicates that b_I is probably correlated with Y .

An ordinary least squares (or one-stage logistic) approach in which is we regress turnout on a dummy indicating rights restoration is therefore inappropriate. Such an approach cannot tell whether rights restoration *caused* turnout to increase, or whether it simply identifies individuals with a higher propensity to vote.

The standard way of dealing with such a problem is an instrumental variables approach (Angrist, Imbens, and Rubin 1996). Such an approach has been widely used in the context of voter turnout (e.g. Ansolabehere, Iyengar, and Simon 1999; Gerber and Green 2000; Milligan, Moretti, and Oreopoulos 2004; Lassen 2004; Sondheim and Green 2010). A valid instrumental variable must satisfy two criteria: it must be correlated with the right-hand-side (endogenous) variable of interest, and it must be uncorrelated with the error of the regression.

In this case, such a variable is readily at hand. The likelihood that an individual had his voting rights restored is (in part) a function of whether he was discharged from parole after Executive Order 181 took effect. As demonstrated above, date of discharge from parole is not correlated with propensity to vote when we limit our analysis to individuals discharged from parole in 2017 or 2018. A dummy variable indicating whether an individual was discharged after the Executive Order, therefore, satisfies the criteria for an instrumental variable.

Two-stage least squares models allow us to leverage the random assignment of parole discharge date to identify the causal effect of rights restoration on the treated population — what is often described as the “local average treatment effect.” The nature of our dependent

variable (voting), instrumented variable (rights restoration), and instrument (discharge from parole before or after EO 181 went into effect) pose a challenge: each are binary values that can either be equal to 0 or 1. Linear models such as two-stage least squares do not ensure that the predicted probability of voting will fall on the $[0, 1]$ interval. Although Angrist (2001) and Angrist and Pischke (2008) argue that in practice this limitation is usually trivial, the possibility remains that the linear model results in unacceptable misspecification. Some research (e.g. Gerber and Green 2000; Green and Shachar 2000; Lassen 2004) using instrumental variables in the context of a binary vote-no vote framework has employed a two-stage probit (IV probit) model to avoid the constraints of the nonparametric framework. The IV probit model specification, however, works best when the instrumented variable is continuous — not, as in the case of rights restoration, a dichotomous variable.

The bivariate probit approach (see Wooldridge 2010) is well suited for situations where the dependent variable, the instrumented variable, and instrument are all dichotomous (Terza, Bradford, and Dismuke 2007). This is especially true when the models exclude covariates and include only the dependent variable, the endogenous instrumented variable, and the instrument (Angrist 2001).

Table 10 presents the results of these various approaches on the question at hand. Models 1 and 2 utilize the linear two-stage least squares approach, with and without covariates. Model 3 uses the IV probit specification (with all covariates), while Models 4 and 5 employ the bivariate probit specification (with and without covariates). For ease of comparison, I show the marginal effects of the probit models (measured at the means of the other variables).

Table 10: Rights Restoration and Turnout

	Dependent Variable: Voted in 2018				
	2SLS		2S Probit	Bivariate Probit	
			(Marg. Effects)	(Marg. Effects)	
	(1)	(2)	(3)	(4)	(5)
D(Rights Restored)	0.0064*	0.0085**	0.0166***	0.0055*	0.0074**
	(0.0037)	(0.0037)	(0.0035)	(0.0030)	(0.0030)
D(Male)		-0.0095**	-0.0088***		-0.0089***
		(0.0042)	(0.0034)		(0.0034)
Age (Years)		0.0010***	0.0009***		0.0009***
		(0.0001)	(0.0001)		(0.0001)
Years on Parole		0.0015**	0.0011***		0.0011**
		(0.0006)	(0.0004)		(0.0004)
Constant	0.0204***	-0.0382***			
	(0.0012)	(0.0088)			
Race / Ethnicity FE		X	X		X
Felony Class FE		X	X		X
Observations	18,423	18,423	18,423	18,423	18,423
Adjusted R-squared	0.0006	0.0131			
Wald χ^2			208.4	1166	1569
ρ				0.147	0.0897

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Instrument is dummy indicating whether individual discharged after EO 181 went into effect.

First-stage equations include covariates in Models 2 – 3

Models 3 – 5 present marginal effects, not model coefficients.

The bivariate probit model returns modestly more conservative estimates than the two-stage least squares model. After controlling for available demographics, the bivariate probit model indicates that rights restoration prior to discharge boosted turnout by individuals who had their rights restored prior to discharge by around 0.74 percentage points; the two-stage least squares model estimates that it increased turnout by around 0.85 percentage points. Though these numbers may seem small, they represent relatively large gains. Just 3.0 percent of individuals who had their rights restored prior to parole discharge cast a ballot in 2018, indicating that the Executive Order increased turnout by between 32.7 and 39.5 percent.

Racial Variation

Although these models demonstrate that rights restoration had a generally positive effect on participation in the 2018 election, the models hide substantial variation between races. In Table 11 I present the two-stage least squares and bivariate probit models on subsets of parolees. These models include only White former parolees (Models 1 and 4), all non-White former parolees (Models 2 and 5), and only Black former parolees (Models 3 and 6). Once again, the marginal effects in the bivariate probit models are calculated at the means of the other variables.

Table 11: Rights Restoration and Turnout

	Dependent Variable: Voted in 2018					
	2SLS			Bivariate Probit (Marg. Effects)		
	White (1)	Non-White (2)	Black (3)	White (4)	Non-White (5)	Black (6)
D(Rights Restored)	0.0165** (0.0065)	0.0037 (0.0044)	0.0025 (0.0056)	0.0158** (0.0063)	0.0033 (0.0034)	0.0026 (0.0046)
D(Male)	-0.0034 (0.0052)	-0.0172** (0.0067)	-0.0211** (0.0091)	-0.0024 (0.0052)	-0.0145*** (0.0044)	-0.0178*** (0.0059)
Age (Years)	0.0015*** (0.0002)	0.0008*** (0.0001)	0.0009*** (0.0002)	0.0013*** (0.0002)	0.0008*** (0.0001)	0.0009*** (0.0002)
Years on Parole	0.0038** (0.0018)	0.0008 (0.0007)	0.0025** (0.0011)	0.0022** (0.0010)	0.0006 (0.0005)	0.0018** (0.0007)
Constant	-0.0465*** (0.0109)	-0.0083 (0.0091)	-0.0111 (0.0126)			
Felony Class FE	X	X	X	X	X	X
Observations	5,691	12,732	7,843	5,691	12,732	7,843
Adjusted R-squared	0.0210	0.0094	0.0139			
Wald χ^2				414.1	1205	980.1
ρ				-0.150	0.250	0.287

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Instrument is dummy indicating whether individual discharged after EO 181 went into effect.

First-stage equations include covariates in all models.

Models 4 – 6 present marginal effects, not model coefficients.

Table 11 indicates that rights restoration prior to discharge boosted turnout among White

individuals by around 1.6 percentage points. We cannot determine whether rights restoration had an effect on non-White individuals: the coefficient on these estimates are small and not statistically significant.

Why would the intervention have increased turnout among White individuals and have had no effect on Black individuals? Some of this may be explained by different propensities to vote. White (2019), for instance, shows that brief periods of incarceration decreases Black individuals' propensity to vote by substantially more than White individuals. She demonstrates that, prior to incarceration, Black individuals were more likely to vote, identifying "a narrative in which targeted policing brings many Black defendants into court, including some voters (so they can be deterred), while lower arrest rates among Whites mean that the White defendant pool rarely includes voters (so there is little demobilization, because the people jailed were unlikely to vote anyway)" (pg. 321). The inverse may hold true here: if Black individuals released from parole have a higher natural propensity to vote, they may be less "susceptible" to a policy intervention of this sort.

There is reason to believe this may be the case. Table 12 presents a series of logit models estimating turnout rates of all individuals discharged from parole prior to the Executive Order going into effect, and the turnout rates of all individuals who had their rights restored. Because Latino voters are more likely to be non-citizens (and therefore have a lower turnout rate), these individuals have been excluded from Models 2 and 4.

Table 12: Turnout Rate Among Former Parolees

	<i>Dependent variable:</i>			
	Voted in 2018			
	Discharged Prior to EO	Had Rights Restored		
	(1)	(2)	(3)	(4)
D(Black)	0.427*** (0.121)	0.297** (0.140)	0.101 (0.218)	-0.019 (0.242)
D(Male)	-0.604*** (0.176)	-0.530*** (0.191)	-0.021 (0.363)	-0.085 (0.369)
Age (Years)	0.044*** (0.005)	0.043*** (0.005)	0.048*** (0.009)	0.050*** (0.010)
Years on Parole	0.036 (0.023)	0.052* (0.027)	0.137** (0.059)	0.141** (0.063)
Constant	-5.777*** (0.295)	-5.799*** (0.322)	-6.252*** (0.558)	-6.120*** (0.599)
Observations	14,155	10,963	3,093	2,514
Log Likelihood	-1,338.939	-1,105.448	-385.717	-327.982
Akaike Inf. Crit.	2,697.878	2,230.896	791.435	675.964

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors in parentheses.
Latino individuals excluded
from Models 2 and 4.

Of the 14,155 individuals discharged from parole between January 1st, 2017, and May 18th, 2018, 6,066 (or 43 percent) were Black. These Black voters were more than 50 percent more likely to vote than the rest of the population. Even when Latinos are excluded from this group, Black individuals were still 36 percent more likely to cast a ballot. In both specifications, this higher turnout rate is significant at the 95 percent level of confidence.

Table 12 also shows, however, that the turnout rate among Black individuals who had their rights restored were not statistically significantly higher (or lower) than other individuals

who had their rights restored. The Executive Order, therefore, seems to have had a leveling effect — Black parolees were not longer substantially more likely to turnout than other individuals discharged from parole in New York State.

Variable Treatment Intensity

There is some reason to believe that using a constant treatment variable understates the true impact of Executive Order 181. Figures 4 and 5 suggest that, among individuals who finished parole after the Executive Order went into effect, individuals who were discharged later had a higher propensity to vote. This may be due to a number of factors: parole officers may have had longer to understand and communicate the new rules. Similarly, individuals discharged later may have had more meetings with their parole officers after the policy change, giving the parole officers multiple times to encourage the individuals under study to cast a ballot. Table 13 interacts an individual's restoration status with the number of months they spent on parole after the policy went into effect ("Months Restored"). This variable is once again instrumented using a variable indicating whether the individual was discharged before or after the Executive Order went into effect. Because the bivariate probit specification is not appropriate when the endogenous variable is not binary, that specification is not included in the Table 13.

Table 13: Rights Restoration and Turnout

	Dependent Variable: Voted in 2018	
	2SLS	2S Probit (Marg. Effects)
	(1)	(2)
Months Restored	0.0028** (0.0012)	0.0030*** (0.0011)
D(Male)	-0.0095** (0.0042)	-0.0089*** (0.0034)
Age (Years)	0.0010*** (0.0001)	0.0009*** (0.0001)
Years on Parole	0.0015** (0.0006)	0.0011** (0.0004)
Constant	-0.0385*** (0.0089)	
Race / Ethnicity FE	X	X
Felony Class FE	X	X
Observations	18,423	18,423
Adjusted R-squared	0.0131	
Wald χ^2		208.8

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Instrument is dummy indicating whether individual discharged after EO 181 went into effect.

First-stage equations include covariates in both models.

Models 2 presents marginal effects, not model coefficients.

Table 13 indicate that there may be variable treatment intensity — that individuals who spent longer on parole after they had their rights restored were more likely to vote than those whose rights were restored shortly before discharge. The two-stage least squares model indicates that for each month individuals spent on parole after having their rights restored, turnout increased by between 0.28 percentage points (in the 2SLS model) and 0.30 percentage points (in the 2S Probit model). Put another way: these models indicate that rights restoration increased the propensity to vote for individuals released within one month of the policy being implemented by 9.8 percent. It increased the propensity to vote among those released five months after the Executive Order went into effect by 59.1 percent.

Of course, with very few months between the implementation of the Executive Order and the registration deadline for the 2018 midterms, it is impossible to know the true impact more months on parole after having one's rights restored has. One thing, however, is clear: focusing only on the individuals discharged from parole shortly after Executive Order 181 went into effect very likely underestimates the true impact it will have on parolee's propensity to vote when parolees spend more time under supervision with their rights formally restored. Incorporation of data after the 2020 presidential election will likely shed more light on the Executive Order's impact on turnout among formerly disenfranchised individuals.

EVERYTHING BELOW HERE NEEDS TO BE RE-WORKED. IT DOES NOT DEAL WITH RACIAL DIFFERENCES IN TURNOUT EFFECTS

Discussion

As discussed above, the effect of incarceration on individuals' propensity to vote is an open question for the literature. What is known, however, is that individuals who have been to prison were likely to vote at very low rates prior to incarceration, and continue to vote at very low rates after incarceration. Whether incarceration reduces political participation or not, increasing the propensity to vote of individuals who have been to prison is a laudable goal. Prisoners overwhelmingly come from marginalized communities with much to gain from policy. Executive Order 181 appears to have substantially boosted turnout among formerly incarcerated individuals through two mechanisms. The first is obvious: parolees who would not have been eligible to vote absent the Executive Order were allowed to cast ballots in the midterm elections. The second mechanism is less obvious. Individuals who finished parole between May 18th, 2018, and October 10th, 2018, would have been eligible to participate in the election even if the governor had not signed the Executive Order. The EO 181, however, appears to have boosted turnout even among individuals whose eligibility to vote was not directly impacted by the rules change. The increase in turnout was substantial: Executive

Order 181 increased turnout among individuals who had their rights restored by more than 40 percent.

The precise mechanism through which the rules change increased turnout among these individuals is not clear. It is possibly a social mechanism: having an officer of the state affirm one's eligibility (and therefore reunion with the body politic) could be responsible for the increase. It could be due to better information: individuals whose rights were restored were likely far more confident of their eligibility to vote than other formerly incarcerated individuals. It could also be an issue of timing: individuals whose rights were restored in May of 2018 but were not discharged from parole until early October had four additional months to register to vote than they would have absent the Executive Order. In reality, the turnout boost is likely due to a combination of different factors.

Despite the substantial increase in turnout thanks to Executive Order 181, turnout among formerly incarcerated individuals remained stubbornly low in the 2018 midterm elections. Just 3.2 percent of individuals who had their rights formally restored and finished parole before the registration deadline. Although the successes of Executive Order 181 should be celebrated, more must be done to encourage formerly incarcerated individuals to participate in the political process.

8 Conclusion

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Appendix A

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 906,870 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).¹⁵ 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.

¹⁵This 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 14: Registered Voter File Ballot Undercount

	<i>Dependent variable:</i>
	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 14 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 B

In the Instrumental Variables Approach section of this paper, I argue that being discharged from parole in the final months leading up to an election is uncorrelated with propensity to vote. Table 15 demonstrates that individuals discharged between May 18th – October 14th, 2016, did not participate at different rates in the 2016 presidential election than other formerly incarcerated individuals. Table 15 includes all individuals last discharged from parole between January 1st, 2015, and October 14th, 2016.

Table 15: Individual-Level Logit Model

	Cast Ballot in 2016 Election		
	(1)	(2)	(3)
D(Discharged on or after May 18 th , 2016)	−0.112 (0.095)	−0.085 (0.096)	−0.096 (0.096)
D(Male)		−0.463*** (0.119)	−0.503*** (0.120)
Age (Years)		0.033*** (0.003)	0.028*** (0.003)
Counts in Most Recent Sentence			0.052*** (0.017)
Time Spent on Parole (Years)	−3.155*** (0.045)	−4.341*** (0.488)	−4.268*** (0.494)
Race / Ethnicity FE		X	X
Felony Class FE			X
Observations	16,867	16,867	16,867
Log Likelihood	−2,826.664	−2,752.432	−2,733.318
Akaike Inf. Crit.	5,657.327	5,524.864	5,498.635
<i>Note:</i>	*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 being discharged in

the summer of 2016 was associated with an individual's propensity to cast a ballot. The nonsignificant results from 2016 provide strong corroboration that discharge date serves as an effective instrument for rights restoration.