

Neighborhoods and Felony Disenfranchisement: The Case of New York City

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Contents

Introduction	1
Turnout Among the Formerly Disenfranchised	3
Research Design	5
Data	6
Criminal Justice Data	6
Voter File Data	7
Matching	7
Results	8
Trends in Turnout	9
Individual-Level Turnout Regressions	10
Instrumental Variables Approach	15
Discussion	17
References	19
Appendix A	22

Introduction

The political history of the United States has been characterized by a general, if unlinear, 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. And yet, despite the United State’s purported march toward ever-more-inclusive systems of democracy, 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 today (Uggen, Larson, and Shannon 2016). These disenfranchising policies provide a stark reminder that our democracy is not as inclusive as we might like to tell ourselves.

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).¹

The disenfranchisement of citizens convicted of felony offenses is particularly troubling given 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 a tool used by the state to exert control over minority — and particularly Black — Americans. This is clear in the case of states such as New York: according to data from the New York State Department of Corrections and Community Supervision, 53.4 percent of individuals who were incarcerated in December of 2018 were Black, although

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

the Census Bureau estimates that just 14.6 percent of the citizen voting age population in the state is Black. 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 targeted Black and Latino New Yorkers at rates far higher than Whites, even after controlling for neighborhood variability and race-specific criminal propensity.

Although felony disenfranchisement is highly disempowering for the individuals impacted by the policy, the political consequences of these laws is not entirely clear. 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 formally disenfranchised. 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.

These spillover effects have been identified using a wide variety of empirical methods. 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, finding that “African American disenfranchisement plays a unique role in predicting African American voter turnout” (p. 800). Ochs (2006), Bowers and Preuhs (2009), and Walker (2014) similarly exploit state-level differences to estimate the (de)mobilizing effect of felony disenfranchise on eligible voters. 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” (p. 724). Burch (2013) used neighborhood-level data to further investigate how felony disenfranchisement operates the sub-state level. She finds that “at high concentrations, imprisonment and community supervision have an unequivocally demobilizing effect of neighborhoods” (p. 185).

It is clear that although felony disenfranchisement directly impacts relatively few potential voters, its demobilizing character reaches many citizens who are not formally disenfranchised. Considering the characteristics of the neighborhoods that incarcerated individuals call home — neighborhoods that are among the most marginalized — felony disenfranchisement is a large problem.

In this paper, I examine the effect of Executive Order 181 in New York State. Signed in April of 2018, the Executive Order restored voting rights to many individuals on parole. Prior to Executive Order 181,

individuals were disenfranchised while incarcerated or on parole. Most individuals who were on parole on Election Day in 2018 were allowed to vote, a major step forward for democracy in the Empire State. In this paper, however, I focus on individuals who were discharged from parole prior to Election Day: individuals who would not have been directly disenfranchised by felony disenfranchisement policies even in the absence of the Executive Order. I hypothesize that Executive Order 181 increased turnout among this population, even though their eligibility was not directly impacted by the policy change.

Turnout Among the Formerly Disenfranchised

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. Uggen and Manza demonstrated that felony disenfranchisement can have material political consequences.

In subsequent years, new research called into question Uggen and Manza's assumption that disenfranchised individuals would vote at rates comparable to their sociodemographic peers. 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).

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).

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 absense 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. Since Downs (1957), political scientists have argued that an individual's propensity to vote is decided at least in part by comparing the expected costs of casting a ballot with the expected benefits of doing so. The costs of voting while disenfranchised (intentionally or otherwise) are exceedingly high, as high-profile cases in states such as Texas have made clear in recent years (Flynn 2018). If an individual assessment of the cost of voting is determined in part by the product of their uncertainty about their eligibility and the cost of casting a ballot while ineligible, formerly disenfranchised individuals are unlikely to participate unless they are fully certain of their eligibility. As Drucker and Barreras (2005) and others have detailed, however, many formerly incarcerated individuals are misinformed about their eligibility to cast a ballot. By dispelling uncertainty around eligibility, I hypothesize that Executive Order 181 increased turnout among the formerly disenfranchised.

Research Design

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. While the Executive Order is obviously beneficial for individuals who are on parole on Election Day (and therefore would have been disenfranchised but for the policy change), the Executive Order may have benefits even for the individuals whose eligibility was not directly impacted. Under the Executive Order, parole officers were required to inform their parolees of their new status under the law. In the analysis that follows, I look only at individuals who were discharged before October 10th, 2018 (the registration deadline in New York State). Limiting the analysis to individuals whose eligibility to cast a ballot was not directly impacted by the policy change allows me to explore whether having one's rights restored while still under formal supervision is effective at raising that individual's propensity to vote even once supervision is ended.

In order to estimate the impact of Executive Order 181 on turnout, I begin by exploring whether the turnout

rate among individuals discharged from parole after the policy went into effect systematically differed from those discharged earlier. This analysis uses an individual-level logistic regression that estimates individuals' turnout rates and controls for various individual-level characteristics. Such an analysis sheds light on the impact of rights restoration on the turnout rate of the formerly disenfranchised population as a whole.

Such an approach has some benefits: supposing that policymakers care about the political representation of formerly incarcerated residents as a group, then examining the group turnout rate can yield important insights. This approach does not, however, address the impact having one's rights restored has on individual propensity to vote. To interrogate whether an individual's propensity to vote is increased if she has her rights restored prior to discharge from parole, I test whether individuals who had their rights restored prior to discharge from parole turned out at higher rates in midterm election. Because the restoration of voting rights is not randomly assigned, I employ an instrumental variables approach, as discussed in greater detail below.

Data

Criminal Justice Data

The criminal justice dataset comes from a public records request filed by the author to obtain individual-level incarceration and parole records for individuals sentenced to incarceration 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; county of commitment; and others. This analysis is limited to individuals incarcerated for felony offenses. Individuals convicted of misdemeanors are not disenfranchised in New York State. These data come from the New York State Department of Corrections and Community Supervision (NYSDOCCS). These data are used to determine when individuals were incarcerated or on parole, and when they finished their parole supervision.

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.²

²Not 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.

Voter File Data

Most states in the United States are required to maintain files with information on all registered voters. In New York, this information is publicly available from the Board of Elections. It includes information on all registered voters, including: first, middle, and last name; date of birth; vote history; and other information. The New York State Voter File also includes information on voters who were previously registered but have since been purged, either because they moved, died, or were incarcerated for a felony offense. I use a snapshot of the registered voter file from March 3rd, 2019.

Matching

Turnout in the 2018 midterm election is estimated by matching the parole 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.

Table 1: Results of Shifting Birthdates

Group	Number of Matches Between DOCCS and Voter File Records
Actual Birthdate	68,340
Birthdate + 35 Days	480
Birthdate - 35 Days	419

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.

Results

Below, I employ a logistic regression to investigate the impact of Executive Order 181 on turnout among all parolees discharged after the policy went into effect. Because not all individuals had their rights restored, I follow this analysis with an instrumental-variables approach to determine the effect of rights restoration on individuals whose rights were in fact formally restored prior to discharge from parole.

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 1 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.³ Figure 1 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.

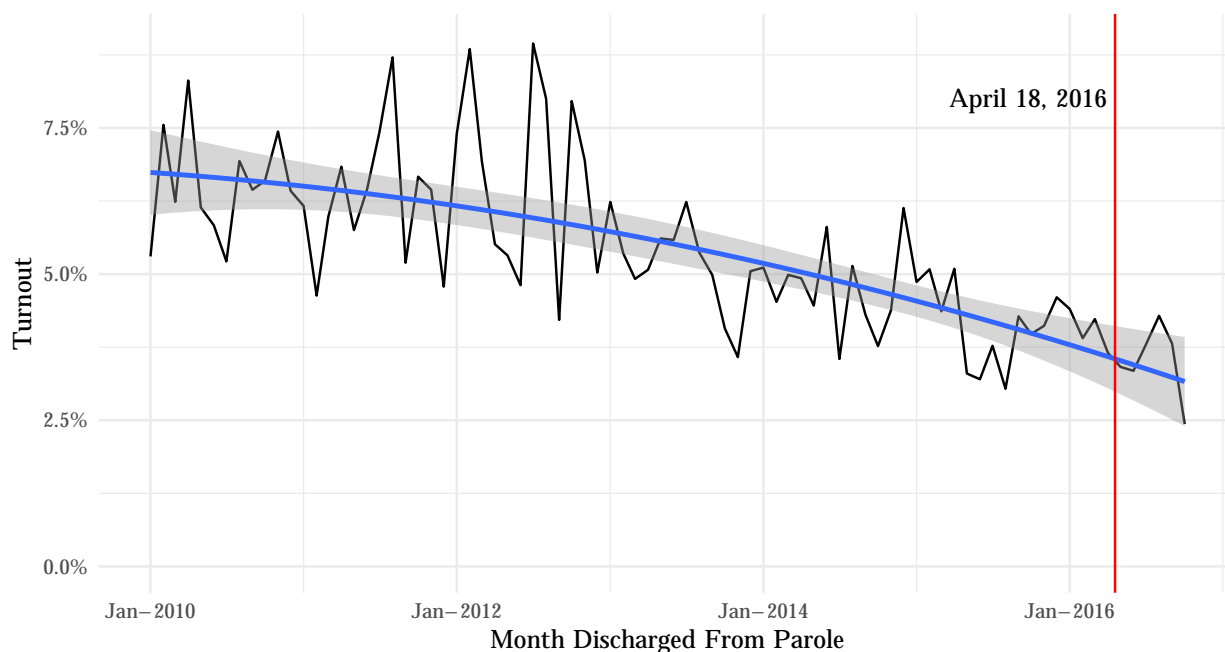


Figure 1: Turnout in 2016 Presidential Election

Figure 2 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.

³Figure 1 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.

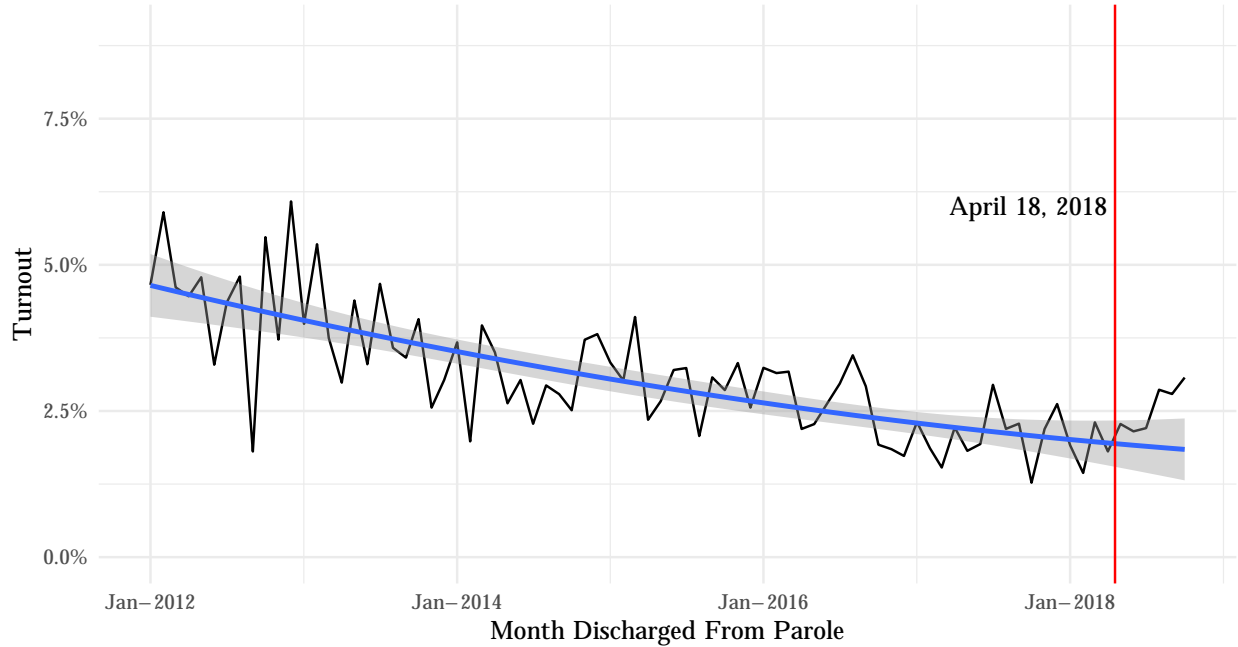


Figure 2: Turnout in 2018 Midterm Election

Figure 1 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 2, 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 1 and 2 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 1 and 2 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 3 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.

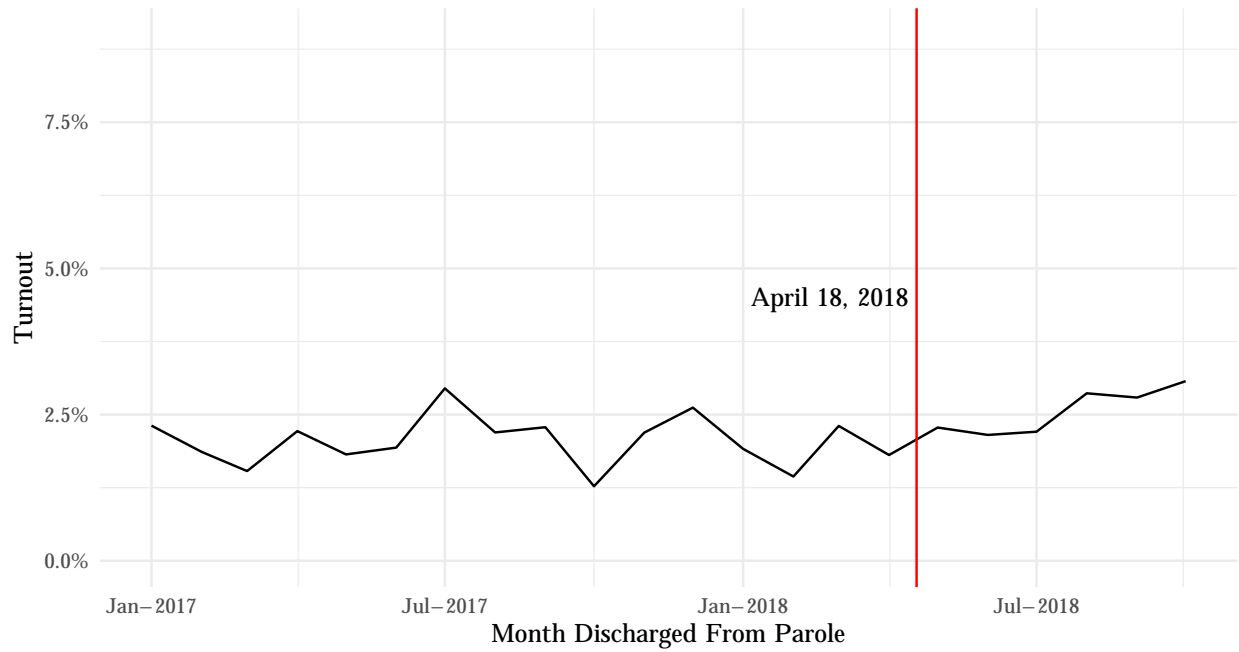


Figure 3: Turnout in 2018 Presidential Election

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 2 include individuals last discharged from parole between January 1st, 2017, and April 17th, 2018.

Table 2: Individual-Level Logit Model

	Cast Ballot in 2018 Election	
	(1)	(2)
Days Since Discharged		0.0001 (0.003)
Days Since Discharged ²		-0.00000 (0.00000)
D(Male)	-0.659*** (0.182)	-0.659*** (0.182)
Age (Years)	0.045*** (0.005)	0.045*** (0.005)
Time Spent on Parole (Years)	0.057** (0.026)	0.057** (0.026)
Constant	-6.129*** (1.096)	-6.092*** (1.273)
County Fixed Effects	X	X
Race / Ethnicity Fixed Effects	X	X
Felony Class Fixed Effects	X	X
Observations	13,260	13,260
Log Likelihood	-1,221.446	-1,221.278
Akaike Inf. Crit.	2,596.892	2,600.555
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

The inclusion of time controls in Table 2 increases the AIC. A Chi-squared test confirms that the model is not improved when controls for time are included.⁴ 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 go into full effect until later in May. Figure 4 plots the share of individuals discharged from parole each day in the spring of 2018. It is not until May 21st that the majority of discharged parolees had their voting rights restored prior to their discharge.

⁴Appendix A 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.

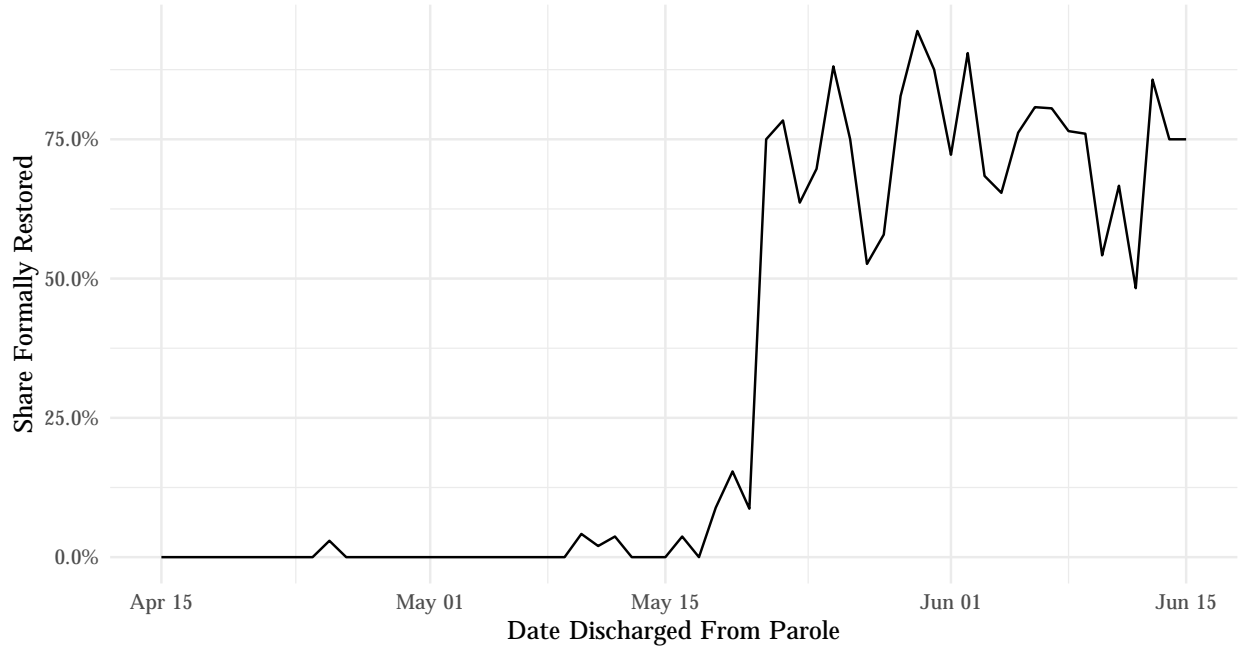


Figure 4: Share of Discharged Parolees Whose Voting Rights Were Restored Prior to Discharge

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 21st, 2018, is akin to a randomly assigned treatment. Any observed difference in turnout between individuals discharged before and after late May can therefore be attributed to the Executive Order.

In Table 3, I present the results of an individual-level logistic regression exploring whether individuals who were discharged on or after May 21st, 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 3: Individual-Level Logit Model

	Cast Ballot in 2018 Election		
	(1)	(2)	(3)
D(Discharged After EO 181)	0.200* (0.116)	0.252** (0.117)	0.257** (0.117)
D(Male)		-0.437*** (0.158)	-0.455*** (0.158)
Age (Years)		0.049*** (0.004)	0.044*** (0.004)
Time Spent on Parole (Years)			0.037* (0.021)
Constant	-3.867*** (0.059)	-6.757*** (1.068)	-6.741*** (1.072)
County Fixed Effects		X	X
Race / Ethnicity Fixed Effects		X	X
Felony Class Fixed Effects			X
Observations	18,423	18,423	18,423
Log Likelihood	-1,910.879	-1,771.038	-1,758.215
Akaike Inf. Crit.	3,825.757	3,686.076	3,672.430
<i>Note:</i>		*p<0.1; **p<0.05; ***p<0.01	

Model 1 in Table 3 formalizes the trend presented in Figure 3 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, county of incarceration, 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 3 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 22.1 and 29.3 percent.

Instrumental Variables Approach

Table 3 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 did have his rights restored before he was discharged from parole.

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

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

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. 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. Neither of these assumptions are valid: an individual’s eligibility to have her rights restored is almost certainly correlated with whether or not they actually were restored. Moreover, eligibility for rights restoration is likely correlated with propensity to vote. Non-citizens, for instance, were not eligible for rights restoration under EO181 because non-citizens cannot vote in New York State. Similarly, individuals who were re-arrested while on parole were not eligible to have their rights restored. Citizenship status and recency of arrest are both highly correlated with propensity to vote.

Our variable of interest, therefore, is likely correlated with our regression error. Such a correlation inhibits our ability to simply regress turnout on a dummy indicating whether an individual’s rights were restored. The standard way of dealing with such a problem is an instrumental variables approach. Such an approach has been widely used in the context of voter turnout (e.g. Gerber and Green 2000; Green, McGrath, and Aronow 2012). 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 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.

Table 4 presents the two-stage least-squares regression estimates exploring the turnout impact of rights restoration. As discussed above, the instrumental variable used in these regressions measure whether an individual was discharged from parole after Executive Order 181 went into effect. These models are estimated using an OLS specification. Although the dependent variable remains dichotomous (as do both the independent variable of interest and the instrument), Angrist and Pischke (2008) recommends using OLS in instrumental variables analyses even under such conditions.

Table 4: Second-Stage Regression

	Cast Ballot in 2018 Election		
	(1)	(2)	(3)
D(Rights Restored)	0.006* (0.003)	0.008** (0.003)	0.008** (0.003)
D(Male)		-0.010*** (0.004)	-0.010*** (0.004)
Age (Years)		0.001*** (0.0001)	0.001*** (0.0001)
Time Spent on Parole (Years)			0.001** (0.001)
Constant	0.020*** (0.001)	-0.028** (0.013)	-0.029** (0.013)
County Fixed Effects		X	X
Race / Ethnicity Fixed Effects		X	X
Felony Class Fixed Effects			X
Weak instruments	0	0	0
Wu-Hausman	0.03	0.04	0.04
Observations	18,423	18,423	18,423
R ²	0.001	0.015	0.018
Adjusted R ²	0.001	0.012	0.014

Note:

*p<0.1; **p<0.05; ***p<0.01
Table reports p-values for Weak Instruments
and Wu-Hausman tests.
First-stage equations use dummy indicating
whether individual was discharged after the
Executive Order went into effect. The
first-stage equations also include
covariates in models 2 and 3.

Table 4 indicates that using discharge date of on or after May 21st as an instrument for rights restoration is

warranted. The p-values of the Wu-Hausman tests hover just below 0.05, while the Weak Instruments test is highly significant in each model.

Model 3 in Table 4 indicates that having one's rights restored caused an individual's propensity to vote to increase by 0.79 percentage points. Approximately 3.01 percent of individuals who had their rights restored cast a ballot in 2018, indicating that Executive Order 181 increased the propensity to vote by around 35.6 percent. Unsurprisingly, this estimate is higher than the estimates reported in Table 3. Because Executive Order 181 theoretically only impacts individuals whose rights were actually restored prior to discharge, the magnitude of the estimated effect on this subgroup must be larger than the group taken as a whole.

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 21st, 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 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.

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Appendix A

In the Individual-Level Turnout Regressions 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 5 demonstrates that individuals discharged between May 21st – October 14th, 2016, did not participate at different rates in the 2016 presidential election than other formerly incarcerated individuals. Table 5 includes all individuals last discharged from parole between January 1st, 2015, and October 14th, 2016.

Table 5: Individual-Level Logit Model

	Cast Ballot in 2016 Election		
	(1)	(2)	(3)
D(Discharged on or after May 21 st , 2016)	−0.138 (0.206)	−0.117 (0.208)	−0.128 (0.208)
Days Since Discharged	−0.001 (0.002)	−0.001 (0.002)	−0.001 (0.002)
Days Since Discharged ²	0.00000 (0.00000)	0.00000 (0.00000)	0.00000 (0.00000)
D(Male)		−0.531*** (0.121)	−0.557*** (0.121)
Age (Years)		0.032*** (0.003)	0.028*** (0.003)
Time Spent on Parole (Years)			0.047*** (0.017)
Constant	−3.031*** (0.314)	−3.420*** (0.607)	−3.320*** (0.611)
County Fixed Effects		X	X
Race / Ethnicity Fixed Effects		X	X
Felony Class Fixed Effects			X
Observations	16,867	16,867	16,867
Log Likelihood	−2,825.656	−2,683.253	−2,667.699
Akaike Inf. Crit.	5,659.311	5,514.507	5,495.398
<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 p-value on the coefficient of interest exceeds 0.5 in each model). The nonsignificant results from 2016 provide strong corroboration that discharge date serves as an

effective instrument for rights restoration.