

Re-enfranchisement and Co-mobilization*

Kevin Morris[†]

January 24, 2020

Abstract

Over the past two decades, scholars have sought to estimate the direct and indirect effects of felony disenfranchisement on political representation. The literature has generally found that felony disenfranchisement has negative indirect effects on turnout. The literature has not, however, examined the effect of *re-enfranchisement* on the turnout of other individuals. This study leverages a change in election rules in 2018 in New York State to determine whether living with a newly-enfranchised individual increases the turnout among individuals who were not directly effected in 2018 election. Using a matched difference-in-differences specification, I find that re-enfranchising individuals not only adds these voices to the elections, but also spurs the turnout of proximal individuals.

*Prepared for the Annual Meeting of the American Sociological Association.

[†]Researcher, Brennan Center for Justice at NYU School of Law, 120 Broadway Ste 1750, New York, NY 10271 (kevin.morris@nyu.edu)

Introduction

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

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), while concentrated policing has also been credited with having a “chilling effect” on neighborhoods’ willingness to reach out for help to local governments (Lerman and Weaver 2013). Perhaps most troubling of all, these effects are not concentrated in random neighborhoods; as Gelman, Fagan, and Kiss (2007) shows, for instance, New York’s “stop-and-frisk” policy impacted Black and Latino New Yorkers at rates far higher than Whites, even after controlling for neighborhood variability and race-

specific arrest rates. The result is an incarcerated population that looks far different than the rest of the state: 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

Felony disenfranchisement policies are part of a criminal justice system that disproportionately impacts Black Americans living in certain communities. The effects of disenfranchisement are concentrated in neighborhoods that already suffer from myriad disadvantages thanks to social and economic marginalization. The neighborhood-specific implications of felony disenfranchisement, however, remain largely 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 (Bowers and Preuhs 2009; King and Erickson 2016) and local levels (Burch 2013).

Although the literature has shown that disenfranchisement decreases the participation of eligible voters, little attention has been paid to the effect that re-enfranchisement might have on the behavior of these individuals. Are these proximal voters — which Walker and García-Castañón (2017) defines as individuals with “a loved one who is a custodial citizen without [themselves] having had contact” (547) — permanently less likely to vote? Or, when a policy is changed to re-enfranchise the disenfranchised, can their turnout be increased?

This paper leverages a change in New York State in 2018 that re-extended voting rights to most individuals on parole. Prior to 2018, individuals sentenced to felony incarceration or on parole were barred from voting (although felony probationers could participate). On election day in 2018, some XX individuals were on felony parole — were it not for the policy change, none of them could have voted. This paper asks whether the individuals living with

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.

these newly-enfranchised individuals were more likely to vote than individuals who did not live with a newly-enfranchised individual.

Background

Since the 2000 election, scholars have attempted to quantify the effect of felony disenfranchisement on the political representation of highly-incarcerated communities. Uggen and Manza (2002) produced the first estimates of felony disenfranchisement’s impact on turnout, arguing that Al Gore would have won the presidency if not for disenfranchisement in Florida. Their analysis estimated the direct effects of felony disenfranchisement on turnout by quantifying the number of actually disenfranchised individuals who would have participated if given the chance. Since Uggen and Manza (2002), other scholars have also investigated the direct effect of felony disenfranchisement (Miles 2004; Uggen and Manza 2004; Drucker and Barreras 2005; Ochs 2006).

A number of papers have also explored the indirect impact felony disenfranchisement policies have on turnout among non-disenfranchised residents. King and Erickson (2016), for instance, leverages state-level variation in disenfranchisement laws to estimate the impact that felony disenfranchisement has on turnout among Black Americans. They use data from the 2004 Current Population Survey Voting and Registration Supplement to calculate statewide turnout rates, and include estimates of the share of Black Americans who are disenfranchised in each state from Manza and Uggen (2006) to explore the impact of these policies on eligible voters. They conclude that disenfranchisement has large spillover effects for Black voters: where more Black residents are disenfranchised, eligible Black voters are less likely to cast a ballot. These findings are in line with other research that has explored whether the effects of disenfranchisement extend beyond those whose voting rights are directly suspended (Bowers and Preuhs 2009; Ochs 2006). As Bowers and Preuhs (2009) sums up: “[I]t is not solely the direct vote of ex-felons that is denied through these laws. [Felony

disenfranchisement] impacts the political power of communities that extends beyond felons’ collateral penalty” (724).

Recent work from Hannah Walker (2014, 2020; 2017) indicates that proximal contact with the criminal justice system can increase non-voting political participation, and that the extent to which they are mobilized can vary based on perceptions of government (un)fairness. To the extent that re-enfranchisement is perceived as a “fair” act that invites formerly incarcerated individuals back into the political system, it may encourage their loved ones to participate as well.

It is important to note, however, that most formerly disenfranchised individuals do not vote even when given the opportunity (e.g. Gerber et al. 2017). As such, most individuals who are re-enfranchised are not truly “treated” by the policy change — if they would not vote whether they were disenfranchised or not, removing the legal prohibition on voting will not be enough to get them to the ballot box. Potential voters who live with these individuals who go “untreated” by the policy change might similarly be unaffected by the policy change. To directly identify the proximal individuals impacted by the policy change, I focus only on individuals on parole who successfully cast a ballot in the 2018 election and their households. These voters, who exercised a vote they could not have in the absence of the policy change, are the most likely to bring their households with them to the polls.

Data

Criminal Justice Data

The primary criminal justice dataset comes from a public records request filed with the New York State Department of Corrections and Community Supervision (NYSDOCCS). They include individual-level parole records for individuals who have been incarcerated in New York State since 1990. The data includes a host of information, including: first, middle, and

last name; date of birth; class of offense; incarceration start and end dates; dates of parole; sex; race; and others. These data are used to determine when individuals were incarcerated or on parole, the class of crimes for which they were incarcerated, and other demographic information.

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

Voter File Data

Most states in the United States are required to maintain files with information on all registered voters. I use a snapshot of the New York State voter file provided by data vendor L2 dated October 9, 2019. It includes information on all registered voters, including: first, middle, and last name; date of birth; vote history; and other information. L2 also includes includes estimates for voters' household income, education levels, and other sociodemographic characteristics.

Methodology

I begin by identifying all individuals who were on parole as of the 2018 election and cast a ballot. I then find all registered voters who lived in the same house as these newly-enfranchised participants. These proximal voters are the treated group. Treated voters are matched with untreated voters using a variety of characteristics, and I perform a difference-

in-differences test to see if their turnout in 2018 was higher than it would have been absent the re-enfranchisement of a household member.

Administrative Record Matching

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

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

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

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

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

Table 1: Results of Shifting Birthdates

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

Testing for false negatives is more challenging. If an individual marries and changes her name after being discharged from parole, for instance, I will not identify her using my matching methodology. Similarly, “John Doe” and “Jonathan Doe” would not result in a match. To reduce the likelihood of these false negatives I remove all punctuation from all names, and standardize capitalization. A record with a last name of “O’Donnell” in one dataset, therefore, would match a last name of “O DONNELL” in the other (provided the other criteria are satisfied). Such standardizations, however, will miss individuals who change their

names entirely. 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.

Although there were many individuals who matched with the voter file, relatively few of them cast a ballot in the 2018 midterm election. This matching procedure estimates that, of the XX individuals on felony parole as of the election, roughly 1,200 voted.

Identification of Treated Voters

Voters are considered if they were not on parole and lived with an individual who was on felony parole as of the 2018 midterm election and cast a ballot.

I use two strategies for identifying individuals who lived with parolees who participated. I begin by considering all individuals registered to the same residential address (including apartment number) as a parolee who voted. This method identifies 3,811 treated voters.

It is possible, despite L2’s efforts to standardize addresses, that voters who live in the same household could have slightly different addresses in the voter file. The second strategy, therefore, identifies all individuals who live at the same latitude and longitude as a parolee who voted, and who shares a last name with that parolee. This methodology identifies just 768, likely pointing to the fact that many parolees live in households in which they do not share the same last name as friends or family members.

Genetic Matching

The central causal identification strategy in this project is a difference-in-differences model. I compare the turnout of treated registered voters in 2018 with their historical turnout. Of course, turnout was exceptionally high in the 2018 midterm elections, which complicates our ability to identify the causal effect of living with a voting parolee — we must tease apart the effect of the historic midterm from the treatment effect in any observed increase in turnout in 2018. Here, I use untreated registered voters (registered voters who were neither on parole nor lived with a voting parolee in 2018) as a control group.

Using *all* untreated voters, however, threatens to violate the parallel trends assumption inherent in difference-in-differences models. There is real reason to believe that treated voters are substantially different than untreated voters — individuals who live in close proximity to individuals who have been to prison are likely unrepresentative of all registered voters.

To ensure that the control group is as representative of the treatment group as possible, I match treated individuals to untreated individuals using a genetic matching algorithm (Sekhon 2011).² Untreated voters are matched to treated voters along the following characteristics: gender, age, partisan affiliation, whether they graduated from college, estimated household income, US congressional district, and their race / ethnicity. Table ?? shows the outcome of this matching procedure.

The matching improves the balance between the treatment and control groups substantially. As expected, the treated group looks very different than the untreated group: they have lower incomes, are less likely to be white, and are more likely to be Democrats.

²Due to computing constraints, the genetic weights are constructed using all treated voters and a random 0.5 percent sample of untreated voters. The ultimate matching uses these weights and includes *all* untreated voters as potential matches.

References

- Bowers, Melanie, and Robert R. Preuhs. 2009. "Collateral Consequences of a Collateral Penalty: The Negative Effect of Felon Disenfranchisement Laws on the Political Participation of Nonfelons." *Social Science Quarterly* 90 (3): 722–43. <https://doi.org/10.1111/j.1540-6237.2009.00640.x>.
- Brennan Center for Justice. 2018. "Criminal Disenfranchisement Laws Across the United States." <https://www.brennancenter.org/criminal-disenfranchisement-laws-across-united-states>.
- Burch, Traci. 2013. "Effects of Imprisonment and Community Supervision on Neighborhood Political Participation in North Carolina." Edited by Christopher Wildeman, Jacob S. Hacker, and Vesla M. Weaver. *The ANNALS of the American Academy of Political and Social Science* 651 (1): 184–201. <https://doi.org/10.1177/0002716213503093>.
- . 2011. "Turnout and Party Registration Among Criminal Offenders in the 2008 General Election." *Law and Society Review* 45 (3): 699–730. <https://doi.org/10.1111/j.1540-5893.2011.00448.x>.
- Clear, Todd R. 2008. "The Effects of High Imprisonment Rates on Communities." *Crime and Justice* 37 (1): 97–132. <https://doi.org/10.1086/522360>.
- Drucker, Ernest, and Ricardo Barreras. 2005. "Studies of Voting Behavior and Felony Disenfranchisement Among Individuals in the Criminal Justice System in New York, Connecticut, and Ohio." Research report. Sentencing Project. https://www.prisonpolicy.org/scans/sp/fd_studiesvotingbehavior.pdf.
- Geller, Amanda, Jeffrey Fagan, Tom Tyler, and Bruce G. Link. 2014. "Aggressive Policing and the Mental Health of Young Urban Men." *American Journal of Public Health* 104 (12): 2321–7. <https://doi.org/10.2105/ajph.2014.302046>.
- Gelman, Andrew, Jeffrey Fagan, and Alex Kiss. 2007. "An Analysis of the New York City

- Police Departments “Stop-and-Frisk” Policy in the Context of Claims of Racial Bias.” *Journal of the American Statistical Association* 102 (479): 813–23. <https://doi.org/10.1198/016214506000001040>.
- Gerber, Alan S., Gregory A. Huber, Marc Meredith, Daniel R. Biggers, and David J. Hendry. 2014. “Can Incarcerated Felons Be (Re)integrated into the Political System? Results from a Field Experiment.” *American Journal of Political Science* 59 (4): 912–26. <https://doi.org/10.1111/ajps.12166>.
- . 2017. “Does Incarceration Reduce Voting? Evidence About the Political Consequences of Spending Time in Prison.” *The Journal of Politics* 79 (4): 1130–46. <https://doi.org/10.1086/692670>.
- Keyssar, Alexander. 2009. *The Right to Vote: The Contested History of Democracy in the United States*. New York: Basic Books.
- King, Bridgett A., and Laura Erickson. 2016. “Disenfranchising the Enfranchised.” *Journal of Black Studies* 47 (8): 799–821. <https://doi.org/10.1177/0021934716659195>.
- Lerman, Amy E., and Vesla Weaver. 2013. “Staying Out of Sight? Concentrated Policing and Local Political Action.” Edited by Christopher Wildeman, Jacob S. Hacker, and Vesla M. Weaver. *The ANNALS of the American Academy of Political and Social Science* 651 (1): 202–19. <https://doi.org/10.1177/0002716213503085>.
- Manza, Jeff, and Christopher Uggen. 2006. *Locked Out: Felon Disenfranchisement and American Democracy*. New York: Oxford University Press.
- Meredith, Marc, and Michael Morse. 2013. “Do Voting Rights Notification Laws Increase Ex-Felon Turnout?” Edited by Christopher Wildeman, Jacob S. Hacker, and Vesla M. Weaver. *The ANNALS of the American Academy of Political and Social Science* 651 (1): 220–49. <https://doi.org/10.1177/0002716213502931>.
- Miles, Thomas J. 2004. “Felon Disenfranchisement and Voter Turnout.” *The Journal of*

- Legal Studies* 33 (1): 85–129. <https://doi.org/10.1086/381290>.
- Ochs, Holona Leanne. 2006. “‘Colorblind’ Policy in Black and White: Racial Consequences of Disenfranchisement Policy.” *Policy Studies Journal* 34 (1): 81–93. <https://doi.org/10.1111/j.1541-0072.2006.00146.x>.
- Sekhon, Jasjeet S. 2011. “Multivariate and Propensity Score Matching Software with Automated Balance Optimization: The Matching Package for R.” *Journal of Statistical Software* 42 (7). <https://doi.org/10.18637/jss.v042.i07>.
- Sewell, Abigail A., and Kevin A. Jefferson. 2016. “Collateral Damage: The Health Effects of Invasive Police Encounters in New York City.” *Journal of Urban Health* 93 (S1): 42–67. <https://doi.org/10.1007/s11524-015-0016-7>.
- Uggen, Christopher, Ryan Larson, and Sarah Shannon. 2016. “6 Million Lost Voters: State-Level Estimates of Felony Disenfranchisement, 2016.” Research report. Sentencing Project. <https://www.sentencingproject.org/publications/6-million-lost-voters-state-level-estimates-felony-disenfranchisement-2016/>.
- Uggen, Christopher, and Jeff Manza. 2004. “Lost Voices: The Civic and Political Views of Disfranchised Felons.” In *Imprisoning America: The Social Effects of Mass Incarceration*, edited by Mary Pattillo, David Weiman, and Bruce Western, 165–204. New York: Russell Sage Foundation.
- . 2002. “Democratic Contraction? Political Consequences of Felon Disenfranchisement in the United States.” *American Sociological Review* 67 (6): 777. <https://doi.org/10.2307/3088970>.
- Walker, Hannah L. 2014. “Extending the Effects of the Carceral State. Proximal Contact, Political Participation, and Race.” *Political Research Quarterly* 67 (4): 809–22. <https://doi.org/10.1177/1065912914542522>.
- . 2020. “Targeted: The Mobilizing Effect of Perceptions of Unfair Policing Practices.”

The Journal of Politics 82 (1): 119–34. <https://doi.org/10.1086/705684>.

Walker, Hannah L., and Marcela García-Castañón. 2017. “For Love and Justice: The Mobilizing of Race, Gender, and Criminal Justice Contact.” *Politics & Gender* 13 (04): 541–68. <https://doi.org/10.1017/s1743923x17000198>.

White, Ariel. 2019. “Misdemeanor Disenfranchisement? The Demobilizing Effects of Brief Jail Spells on Potential Voters.” *American Political Science Review* 113 (2): 311–24. <https://doi.org/10.1017/s000305541800093x>.