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The Politics of the Restoration of Ex-Felon Voting Rights: The Case of Iowa

Marc Meredith[1] and Michael Morse[2*]

[1]*Department of Political Science, University of Pennsylvania, Philadelphia, PA 19104, USA; marcmere@sas.upenn.edu*
[2]*Department of Government, Harvard University, Cambridge, MA 02138, USA; michaellmorse@g.harvard.edu*

ABSTRACT

We investigate how the restoration of voting rights affects the political participation of ex-felons. Our primary analysis uses unique administrative data from Iowa, which changed how ex-felons restore their voting rights in both 2005 and 2011. Prior to 2005, ex-felons had to apply to the governor to restore their voting rights. We show that ex-felon turnout increased after Iowa began to automatically restore these rights. Consistent with misinformation being a significant barrier to ex-felons' political participation, ex-felons were more likely to vote if they were informed about this policy change. The application requirement was re-instated for ex-felons discharged since 2011 and we show that this reduced their 2012 presidential election turnout. We conclude by comparing the actual turnout rate of recently discharged ex-felons in Iowa, Maine, and Rhode Island to the turnout rate that Uggen and Manza's (2002) method predicts. This

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comparison suggests that although restoration procedures can substantively affect ex-felon turnout, restoration procedures are not the only reason why ex-felons vote less often than observably similar non-felons.

1 Introduction

Felon disenfranchisement has become a contentious and significant public policy issue of late, a response to both the unprecedented rise of the carceral state and mounting questions of fairness in American democracy. More than 200 years after this country was founded on the principle of equality, felons are the only class of citizens still disenfranchised from the vote.[1] Recent estimates by the Sentencing Project suggest that over five million citizens are ineligible to vote on the basis of a criminal conviction (Porter, 2010), including one out of every thirteen African-Americans.

Each state determines the eligibility of convicted felons to vote and there is a mosaic of different laws regarding when they are disenfranchised.[2] Every state except Maine and Vermont disenfranchises individuals who are incarcerated on a felony conviction, with a majority extending that period of disenfranchisement through probation or parole. Twelve states permanently disenfranchise at least some ex-felons, although these states have a process through which ex-felons can apply to have their voting rights restored (Uggen *et al.*, 2012).

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[1] While we use the term "felon disenfranchisement" throughout the paper, the term "criminal disenfranchisement" is more accurate as some states also disenfranchise citizens because of misdemeanor convictions.

[2] See The Sentencing Project (2011) for a recent summary.

The contested 2000 presidential election — in which at least 600,000 ex-felons in Florida were barred from the polls (Burch, 2012) — generated a flood of research on the electoral consequences of felon disenfranchisement. Seminal works by Uggen and Manza (2002) and Manza and Uggen (2004; 2006) argue that felon disenfranchisement causes Republican candidates to win elections that they would otherwise lose and that, with reform, Al Gore would have been president. They reach this conclusion by fitting models of turnout and vote choice using data on non-felons, and extrapolating the probability that ex-felons with the same demographic characteristics would turnout and support Democratic candidates if they were allowed to vote. This extrapolation rests on what Haselswerdt (2009) calls an "equivalence assumption" that the disenfranchised population would vote at the same rates as the non-felons who share the same observable characteristics.

Subsequent work questions the validity of this assumption. Hjalmarsson and Lopez (2010) and Burch (2012) argue that unobservable differences between felons and non-felons cause such an assumption to overstate both the probability that felons would vote and their preference for Democratic candidates. Recent work that estimates the turnout rates of ex-felons by matching criminal justice discharge records to voter files also generally finds that substantially fewer enfranchised ex-felons vote than Uggen and Manza's model predicts (Burch, 2007; 2011; Haselswerdt, 2009; Meredith and Morse, 2014).

While Hjalmarsson and Lopez (2010) conclude that felon disenfranchisement is electorally inconsequential based on the low turnout rates of enfranchised ex-felons, we argue that enfranchised ex-felons may vote at low rates because of felon disenfranchisement policy. In particular, we theorize that misinformation about voting rights may inhibit ex-felon turnout. Previous work shows that a high percentage of individuals with past criminal involvement hold uncertain or incorrect beliefs about their right to vote (Drucker and Barreras, 2005; Manza and Uggen, 2006). Because the cost of voting when ineligible is much greater than the cost of not voting when eligible — casting a vote when ineligible is a felony — such uncertainty about voting rights is likely to reduce voter turnout. Resolving uncertainty about voting rights is difficult because election administrators and criminal justice officials are often just as misinformed about the process (Ewald, 2005; Allen, 2011). Moreover, the complexity of voting rights laws inhibits candidates, parties, and

interest groups from informing and mobilizing the ex-felon community on their own. As a result, we expect that enfranchised ex-felons will vote at higher rates when they are actually informed about their voting rights.

We also contend that bureaucratic procedures reduce ex-felon turnout. While most states automatically restore voting rights after felons complete the requisite portions of their sentence, a number of states place additional requirements on voting rights restoration, such as payment of court costs or restitution. Clearly these requirements restrict the political participation of ex-felons who do not satisfy these conditions. But we hypothesize that the bureaucratic procedures used to implement these requirements also impose significant barriers to political participation by ex-felons who do satisfy these conditions. We expect that ex-felons will vote at lower rates when they are required to apply to restore their voting rights instead of having these voting rights restored automatically.

We test our hypotheses by studying how the registration and turnout rates of ex-felons in Iowa vary across time as voting rights restoration policies change. Historically, ex-felons who wanted to regain the right to vote would submit an application to the governor, which he generally granted. In 2005, Executive Order 42 eliminated the application process and established a protocol by which voting rights would be automatically restored upon discharge from the correctional system, although only some ex-felons were informed of this change. Consistent with our expectations, ex-felon turnout increased between the 2004 and 2008 presidential elections, particularly among ex-felons who were officially informed that their voting rights were restored. Executive Order 70, which reinstated the application process for ex-felons discharged after 2011, subsequently reduced ex-felon turnout in the 2012 presidential election.

To bolster the external validity of our findings, we also estimate the registration and turnout rates of ex-felons from Maine and Rhode Island. Advocates against felon disenfranchisement often promote Maine's disenfranchisement policy because felons can vote while incarcerated. As a result, we believe that fewer ex-felons are prevented from voting because of misinformation in Maine than in most other states. Because the incarcerated population can vote, Maine does not need to purge their voter registration records from its voter registration database. The loss

of voter registration records is another bureaucratic procedure that may inhibit ex-felon participation. We focus on Rhode Island because it engages in the most aggressive campaign that we know of to register ex-felons upon discharge. Consistent with our expectations, we find higher ex-felon registration rates in Maine and Rhode Island than in other states that we have studied. Yet, we do not find that ex-felons from Maine and Rhode Island vote at substantially higher rates. These findings suggest that although notification of voting rights and automatic restoration policies can improve ex-felon political participation, misinformation and bureaucratic procedures are not the only reason why enfranchised ex-felons vote at lower rates than demographically similar non-felons.

Although recently enfranchised ex-felons are less likely to vote than Uggen and Manza (2002) and Manza and Uggen (2004; 2006) predict, this does not necessarily imply that felon disenfranchisement is electorally irrelevant. While Manza and Uggen likely overestimate the number of criminally disenfranchised individuals who would vote, our results suggest there are a significant number of enfranchised citizens who believe that they are disenfranchised. Our findings also demonstrate the importance of the procedures used to restore ex-felons' voting rights. While felon disenfranchisement policy debates often focus on the length of disenfranchisement, our results highlight that the ease and transparency of the voting rights restoration process should also be considered when assessing the punitiveness of felon disenfranchisement policy.

2 Our Argument

No scholarly consensus exists on the electoral consequences of felon disenfranchisement. While Uggen and Manza (2002) and Manza and Uggen (2004; 2006) contend that Republican candidates benefit from felon disenfranchisement — an argument bolstered by Republicans reticence to vote for reform[3] — Hjalmarsson and Lopez (2010) and Burch (2012) argue that this Republican bias is overstated. One reason

[3]When explaining Republican opposition to a 2003 bill that would have made it easier for ex-felons to restore their voting rights in Alabama, then-Party Chairman Marty Connors said, "As frank as I can be, we're opposed to it because felons don't vote Republican" (Tuscaloosa News, 2003).

these scholars reach different conclusions is that they disagree about the rates at which ex-felons would vote if eligible. Everyone expects that, if eligible, ex-felons would vote at a lower rate than the general population, as the demographics of felons — disproportionately male, racial and ethnic minorities, young, less educated, unmarried — are associated with lower voter turnout. However, it remains an open question whether ex-felons would vote at such a low rate that their electoral impact would be negligible.[4]

One way to assess Uggen and Manza's approach is to compare the predicted and actual turnout behavior of recently enfranchised ex-felons. Uggen and Manza (2002) predict that about 35 percent of the disenfranchised population would vote in presidential elections. Yet a series of recent papers show that recently enfranchised ex-felons vote at substantially lower rates (Burch, 2007; 2011; Haselswerdt, 2009; Meredith and Morse, 2014). These papers measure the turnout rates of the recently enfranchised by searching statewide voter files for records with a similar name and age to individuals in criminal justice discharge records. Haselswerdt finds that only five percent of ex-felons discharged in Erie County, New York prior to the 2004 presidential election voted in either the 2004 presidential or 2005 statewide election. Burch estimates that about 20 percent of male ex-felons voted in Florida, Georgia, Michigan, Missouri, and North Carolina in the 2008 presidential election. Meredith and Morse show that about 10 percent of recently enfranchised ex-felons voted in New Mexico and North Carolina in the 2008 presidential election and New York in the 2012 presidential election.

Two schools of thought exist for why ex-felons vote at lower rates than observably similar non-felons. One suggests that the same variables that caused ex-felons to commit crimes also cause them not to vote (Gerber *et al.*, forthcoming). For example, belief in prosocial norms is thought to both prevent criminal behavior and increase voter turnout. The other emphasizes contact with the carceral state. Weaver and Lerman (2010) show that being convicted of a crime reduces trust in the political system and this may lead to a reduced desire to participate in politics. Both schools of thought though predict that ex-felons will

[4]Burch (2012) also contends that the candidate preferences of the criminally disenfranchised population are sufficiently similar to the candidate preferences of the general voting population that election outcomes would not change if the criminally disenfranchised population voted at higher rates.

vote at lower rates than members of the general population who share the same gender, race, age, education level, and marital status.

We contend that the existing literature understates the role that misinformation plays in inhibiting ex-felons' political participation. Because felon disenfranchisement policy is set at the state level and frequently administered at even more local levels of government, it is often difficult to determine which specific policies apply to a particular ex-felon. Disenfranchisement policies differ in subtle ways across states, with distinctions based on the specific crime or the length of time since discharge. Moreover, many states' policies governing when and how ex-felons' voting rights are restored have been particularly fluid (Ewald, 2009; Porter, 2010).

Surveys and interviews of individuals with past criminal involvement confirm that many hold incorrect beliefs about their voting rights. Drucker and Barreras (2005) survey individuals with a history of criminal justice involvement in Connecticut, New York, and Ohio and show that about 50 percent of respondents held incorrect or uncertain views about their right to vote. Likewise, Manza and Uggen (2006) report that a majority of the Minnesota felons that they interviewed incorrectly believed that they would continue to be disenfranchised after they were discharged from prison, probation, and parole.

A number of barriers prevent ex-felons from resolving misinformation or uncertainty about their voting rights. Candidates and campaigns risk backlash if their actions are seen as mobilizing ex-felons. Burch (2013), for example, found no mentions of mobilizing ex-felons in her content analysis of interviews with partisan voter outreach directors during the 2008 presidential campaign. Although some non-partisan voter outreach directors mentioned mobilizing ex-felons specifically, variation in laws across states complicates such efforts by inhibiting national campaigns. In states with some form of post-prison disenfranchisement, voter outreach operations also must take great care in explaining laws in order to avoid mobilizing ineligible ex-felons.[5] Although election administrators and criminal justice officials could help fill this information vacuum, previous work shows that they often hold incorrect beliefs about how

[5]An early version of this paper was presented at a conference attended by people who design and implement voter outreach campaigns. Afterwards a number of attendees told us that although they thought it was important to inform ex-felons of their voting rights, their organization could not because of the risk associated with doing so.

and when ex-felons voting rights are restored (Ewald, 2005; Manza and Uggen, 2006; Allen, 2011). For example, Allen finds that about half of the New York county election boards inaccurately reported the conditions under which an ex-felon's voting rights could be restored.

Uncertainty about voting rights is likely to reduce voter turnout because the costs of voting when ineligible is much greater than the costs of not voting when eligible. Since Riker and Ordeshook (1968), political scientists have thought about voter turnout as a cost–benefit calculus and uncertainty about voting rights introduces some additional consideration into this calculus for ex-felons. An ex-felon who wishes to vote, but is uncertain of his or her rights, may err by voting even though he or she is disenfranchised or by not voting even though he or she is enfranchised. There are recent examples of harsh punishments, including incarceration, for even unintentional illegal voting by ex-felons (Minnite, 2011). While there is also a cost associated with not casting a ballot when eligible, this cost is orders of magnitude less than the cost associated with incarceration. Thus, ex-felons must be almost certain they are enfranchised before they are willing to vote. We expect that informing enfranchised ex-felons about their voting rights will increase their probability of voting.

The bureaucratic procedures that a number of states use to restore voting rights are also likely to reduce ex-felon voter turnout. A number of states place additional conditions beyond time served on when ex-felons' voting rights can be restored. For example, Arkansas only restores voting rights after an ex-felon proves to their county clerk's office that he or she has paid applicable court costs, fines, parole or probation fees, and restitution. Tennessee also considers whether an ex-felon has paid child support when deciding whether to restore their voting rights. States typically use an application procedure to enforce these restrictions, which are often burdensome in terms of their length, documentation required, and processing time. We expect that eliminating applications will increase ex-felons' probability of voting.

3 Research Design

Article II, Section 5 of the Iowa Constitution states that:

> No idiot, or insane person, or person convicted of any infamous crime, shall be entitled to the privilege of an elector.

Infamous crimes are those that are potentially punishable by imprisonment for at least one year. As a result, all individuals convicted of a felony and some individuals convicted of an aggravated misdemeanor are permanently stripped of their right to vote in Iowa.[6] However, Section 48A.6 of the Iowa Code adds that:

> The following persons are disqualified from registering to vote and from voting:
>
> 1. A person who has been convicted of a felony as defined in section 701.7, or convicted of an offense classified as a felony under federal law. If the person's rights are later restored by the governor, or by the president of the United States, the person may register to vote.

As the statute establishes no formal guidelines, the governor has substantial autonomy in determining the conditions under which an ex-felon regains the right to vote. Prior to July 4, 2005, ex-felons could submit an application for the restoration of voting rights. The application required ex-felons to list all convictions, provide details on the fines, fees, and court-ordered restitution paid, and "give reasons why you believe you should be granted Executive Clemency." Per the administrative rules of the Iowa Board of Parole, the governor received a recommendation from the Parole Board about whether to grant the request for voting rights, but the governor held the ultimate authority. If an application was granted, the applicant would receive a certificate in the mail denoting the restoration of their voting rights. We present an example of such a certificate in Figure A.3.[7] Both the application and the certificate clearly distinguish the restoration of voting rights from a pardon.

Executive Order 42, which was signed by Governor Thomas Vilsack on July 4, 2005, eliminated the need for ex-felons to submit an application in order to restore their voting rights. This executive order

[6]For the remainder of the paper we use the term "ex-felon" in the context of Iowa to refer to an individual who has been discharged from their entire sentence, including probation and parole, for either a disenfranchising aggravated misdemeanor or felony sentence.

[7]The use of the "Restoration of Citizenship" header on this certificate is misleading because Trop v. Dulles, 356 U.S. 86 (1958), made it unconstitutional for citizenship rights to be revoked as punishment.

established a protocol by which every month the Iowa Department of Corrections would forward to the Governor's Office a list of all felons discharged from the criminal justice system in the previous month. While in theory the governor could select which individuals on this list would have their voting rights restored, our understanding is that in practice voting rights were restored to everyone. Once the governor restored an individual's right to vote, a certificate, like that in Figure A.3, was mailed to their last known address. Executive Order 42 also granted a blanket restoration of voting rights to all ex-felons who completed their sentences on or before July 4, 2005, including those ex-felons whose applications were previously denied. However, such individuals were not mailed a certificate.

Executive Order 70, which was signed by Governor Terry Branstad on January 14, 2011, reinstated the application process that existed prior to July 4, 2005. Branstad, who previously served as governor from 1983 to 1999, was an outspoken critic of Executive Order 42 on the stated grounds that the payment of financial obligations owed to the state is a critical component in determining whether the restoration of voting rights is appropriate (Dorman, 2005). The last group of ex-felons to have their voting rights automatically restored en masse by the Governor's Office were those individuals discharged in December 2010; those ex-felons discharged beginning January 1, 2011 had to apply to restore their voting rights.[8] Executive Order 70 clearly states that it does not affect voting rights restored under Executive Order 42.

Figure 1 summarizes the implications of these Iowa policy changes for ex-felon voting rights in the 2004, 2008, and 2012 presidential elections as a function of discharge date. The figure highlights that ex-felons discharged in relatively close proximity may be treated differently in the context of a specific election. For example, ex-felons discharged in July 2005 were informed that their voting rights had been restored by Executive Order 42 prior to the 2008 election, while ex-felons discharged a month earlier in June 2005 were not. Likewise, while ex-felons discharged in January 2011 needed to submit an application in order

[8]Becky Elming, then-Executive Assistant to the Chief of Staff for Terry Branstad, communicated to us in an email on May 25, 2012 that some individuals discharged in December 2010 may not have had their voting rights restored if their discharge paperwork had not been processed before January 12, 2011.

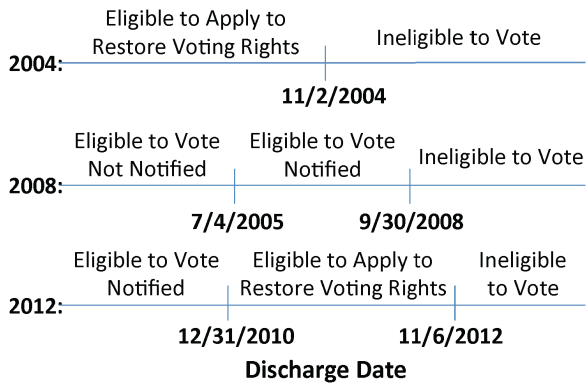

Figure 1: Ex-felon voting rights in Iowa by presidential election and discharge date.

to vote in the 2012 presidential election, the voting rights of ex-felons discharged in December 2010 were automatically restored.

These Iowa policy changes allow us to implement three empirical tests of our hypotheses about the effects of applications and notification on ex-felon political participation. We first examine the behavior of the cohort of ex-felons discharged in 2002 and 2003. Members of this cohort were eligible to apply to restore their voting rights prior to the 2004 presidential election. Thus, the percentage of ex-felons who filed an application provides an upper bound on the number of ex-felons who could have voted in this election legally. Because of the blanket retroactive restoration instituted in Executive Order 42, we expect ex-felons discharged in 2002 and 2003 to turn out at higher rates in 2008 than in 2004. Of course this cohort's turnout rate also could have increased between 2004 and 2008 as a result of the group being four years older and further removed from the criminal justice system or from enthusiasm, particularly among African-Americans, for Barack Obama's candidacy. Thus, observing an increase in this cohort's turnout between 2004 and 2008 is evidence that is consistent with, but not conclusive of, an application requirement reducing ex-felon turnout.

Our second test focuses on the differences in the registration and turnout rates of those ex-felons discharged just before and after July 4, 2005. Figure 1 shows that ex-felons discharged just before and after July 4, 2005 were automatically eligible to vote in both the 2008 and 2012 presidential elections. However, only those ex-felons discharged

after July 4, 2005 were notified that they were eligible to vote. Because this should cause ex-felons discharged after July 4, 2005 to be better informed about their voting rights, the discontinuous increase in the probability of registering and voting as a result of being discharged after July 4, 2005 provides an estimate of the percentage of ex-felons who abstain from political participation because of misinformation and uncertainty.

Our third test focuses on the differences in the registration and turnout rates of those ex-felons discharged just before and after December 31, 2010. Figure 1 shows that only ex-felons discharged on or before December 31, 2010 were automatically eligible to vote in the 2012 presidential election. In contrast, ex-felons discharged after December 31, 2010 had to submit an application to legally vote in the 2012 presidential election. Because the application requirement only applied to ex-felons discharged after December 31, 2010, the discontinuous decrease in the probability of registering and voting as a result of being discharged after December 31, 2010 provides an estimate of the percentage of ex-felons who are dissuaded from voting because of an application requirement.

Focusing on Iowa allows us to benefit from some advantages commonly associated with studies of a single political jurisdiction (Nicholson-Crotty and Meier, 2002; Golden, 2005; Gehrig, 2006). Looking at how ex-felon participation changes within a state as restoration policies change allows us to control for the unobserved determinants of ex-felon participation that remain constant within the state across time. Moreover, because Executive Order 42 and Executive Order 70 apply different policies to different groups of ex-felons at the same point in time, we also can implement some tests that hold fixed the unobserved determinants of ex-felon participation within a given election. Focusing on a single state also allows us to follow recent work that directly measures ex-felon political participation through a relatively labor intensive process of matching public records.

Although focusing on a single state has some drawbacks for external validity (King *et al.*, 1994), we think these findings from Iowa are of broad interest. A number of other states use an application process similar to that used in Iowa to restore the voting rights of ex-felons (Uggen *et al.*, 2012). While ex-felons in these other states may not be affected by applications in the exact same manner, knowing the demobilizing effect of applications in Iowa helps provide a sense of what

the demobilizing effect of applications are likely to be in these other states. A number of state legislatures have also recently considered bills mandating that ex-felons be notified about their voting rights without any evidence of how notification affects turnout (Meredith and Morse, 2014).

We further address concerns about external validity by also estimating the registration and turnout rates of ex-felons from Maine and Rhode Island. The disenfranchisement policies of Maine and Rhode Island should be among the least likely to dissuade ex-felons from registering and voting. Maine is one of the two states that never restricts felons' voting rights. While Rhode Island disenfranchises incarcerated felons, dischargees are engaged in a voting rights informational session that concludes with an administrator attempting to register the ex-felon upon release.[9] If restoration policies are the primary reason that ex-felons abstain from registering and voting, then we expect that the turnout rates of ex-felons from Maine and Rhode Island should approach the turnout rates of observably similar non-felons. Thus, comparing the turnout rates that we find in administrative data to turnout rates predicted by Uggen and Manza (2002) and Manza and Uggen (2004; 2006) helps to test the efficacy of the assumptions that underlie their assessments of the electoral consequences of felon disenfranchisement.

We also make methodological contributions to the growing literature that measures voter turnout by matching administrative records to voter files. Existing literature often equates the turnout rate to the number of administrative records that match a turnout record in the voter file. But as we discuss in Section 4.4, matching error may cause the number of matches to either underestimate or overestimate the actual turnout rate. We develop a number of diagnostic tests that can be applied in other contexts to assess the prevalence of matching errors.

4 Data

4.1 Discharge Data

We collected individual-level criminal justice records from Iowa, Maine, and Rhode Island. The Iowa Department of Corrections provided

[9] This was part of a broad felon disenfranchisement referendum passed by voters in 2006 that is detailed in Owens and Orr (2011).

individual-level records on the population discharged from a felony or aggravated misdemeanor sentence between January 1, 2002 and February 28, 2012.[10] These data contain an individual's full name, date of birth, gender, most serious crime, and date of discharge. The date of discharge is the date on which an ex-felon completed all prison, parole, and probation requirements, and thus became eligible to have their voting rights restored under Iowa law.

The Maine Department of Corrections provided individual-level records on the population discharged from a correctional facility or state prison between January 1, 2009 and December 31, 2010. These data contain an individual's full name, date of birth, gender, race, and date of discharge. Because Maine does not disenfranchise incarcerated felons, the date of discharge is not related to an ex-felons' voting rights.

The Rhode Island Department of Corrections provided individual-level records on the population discharged from a Rhode Island prison between January 1, 2009 and December 31, 2010. These data contain an individual's first and last name, date of birth, gender, race, and date of discharge. Because Rhode Island restores voting rights upon discharge from prison, an individual is eligible to vote on this date.

Table 1 compares the population of ex-felons discharged in Iowa, Maine, and Rhode Island to the general incarcerated population. The characteristics of the incarcerated population are estimated from the National Archive of Criminal Justice Data's (NACJD) 2004 Survey of Inmates in State and Federal Correctional Facilities. Both the NACJD survey and the state discharge data show that the ex-felon population is overwhelmingly male, although slightly less so in Iowa. The age distribution in the state discharge data is broadly similar to the NACJD sample, with about 40 percent under the age of 30, 50 percent between age 30 and 50, and 10 percent over age 50. Given the state demographics, it is not surprising that the NACJD survey and the state discharge data show that African-Americans make up a smaller percentage of Iowa, Maine, and Rhode Island's felon population. While none of the state discharge data contains educational attainment, the NACJD survey shows similar patterns of educational attainment among the general

[10] As we note in Section 3, aggravated misdemeanors are only disenfranchising when the sentence length is greater than one year. Because data on sentence length is unavailable, we do not know who is disenfranchised among the population of aggravated misdemeanor discharges.

Table 1: Observable characteristics of ex-felon population.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
			Age							Educational Attainment			
	Male	18–24	25–29	30–39	40–49	50+	Black	Hisp.	Married	No HS Deg.	HS Deg.	Some Col.	Col Deg.
Iowa discharges													
1/1/2002–2/28/2012													
All (N = 85,931[75,949])	78.1	19.5	19.7	28.0	21.9	10.9							
Felons (N = 49,587[46,038])	78.6	18.6	19.5	28.5	22.7	10.7							
Maine discharges													
1/1/2009–12/31/2010													
All (N = 2,351[2,239])	89.7	12.7	23.3	31.1	21.7	11.3	6.1						
Rhode Island discharges													
1/1/2009–12/31/2010													
All (N = 7,548[6,631])	90.1	21.4	19.5	26.7	22.8	9.5	25.5	17.9					
Felons (N = 4,657[4,263])	93.2	19.1	19.7	28.2	23.0	10.0	28.6	20.3					
NACJD sample													
All ($N = 13,789$)	93.0	17.4	16.9	32.1	23.7	9.9	45.0	14.7	15.5	65.4	19.7	11.0	3.1
Iowa (N = 153)	85.6	10.6	14.8	36.9	26.2	11.4	28.4	3.1	17.1	61.2	25.5	12.6	0.7
New England (N = 235)	94.8	14.3	13.3	30.0	22.5	20.0	22.8	17.1	9.6	61.7	20.6	12.1	4.7
Near Discharge (N = 909)	90.3	21.9	15.3	34.4	23.4	5.1	43.3	16.2	12.6	66.8	19.8	10.3	2.8

Note: The number following "N = " refers to the total number of discharges. The number in brackets refers to the number of unique individuals discharged. Individuals are near discharge in the 2004 NACJD Sample if they are expecting to be released within a month of their interview. Observations in the 2004 NACJD sample are weighted by their sample weight (FINALWT).

prison population, the Iowa prison population, the New England prison population, and the subset of the prison population that is about to be released.

4.2 Voter File Data

We used voter files to measure the political participation and partisan preferences of ex-felons. Voter files are publicly available databases that provide demographic information and turnout history for all registered voters in a state. The cost of these files, as well as the demographic information available, varies substantially across states, although nearly all states provide registrants' full name, address, and either date of birth or age. Many states also report party registration. We can use voter files to learn whether someone with a similar full name and age as a particular ex-felon is registered to vote, voted in a particular election, and registered with a particular party.

Our analysis uses two copies of the Iowa voter file and one copy of the Maine and Rhode Island voter files. An important difference between these voter files is that Iowa and Rhode Island report a registrant's date of birth, while Maine reports a registrant's year of birth. A concern with using voter files to measure voter turnout is that voter records can be purged once an individual is no longer an eligible voter in the state. We will underestimate ex-felon turnout if turnout records of ex-felons are purged between Election Day and when we acquire the voter file. Hence, we use an Iowa voter file from October 2010 to measure 2008 presidential election turnout and an Iowa voter file from January 2013 to measure 2012 presidential election turnout. We also use voter files from June 2013 and December 2013 to measure 2012 presidential election turnout in Maine and Rhode Island respectively.

One limitation of voter files is that they only measure registration and turnout within a single state. Although ex-felons are most likely to reside in the same state that they were convicted in, some may move to other states. If these ex-felons also vote in other states, we will underestimate ex-felon participation by focusing only on participation in the state of conviction. To assess the prevalence of such turnout, we also use copies of the voter file in Connecticut, Minnesota, Missouri, New York, and Ohio from various months in 2013. Connecticut, Missouri, and New York report a registrant's date of birth, while Minnesota and

Ohio report a registrant's year of birth. We expect that if ex-felons move, they are most likely to reside in states in close proximity to our states of interest. Thus, observing the prevalence of turnout among Iowa ex-felons in Minnesota and Missouri and Rhode Island ex-felons in Connecticut and New York is particularly informative for benchmarking the amount of potential cross-state turnout.

4.3 Voting Rights Restoration Application Data

The Iowa State Archives provided information on who successfully applied to have their voting rights restored. These data contain the full name, address, gender, date of birth, date of application receipt, and date of acceptance decision for all applications for voting rights restoration granted between March 1999 and March 2006. Unfortunately, the Iowa State Archives does not maintain records of denied applications. To obtain this information, we made a public records request to the Iowa Governor's Office to get copies of all of the applications for the restoration of voting rights filed between January 1, 2002 and July 4, 2005. We also requested copies of the response letters mailed to denied applicants and the restoration certificates mailed to successful applicants.[11] We extracted from these letters the full name, date of birth, and stated reason for denying the application.

4.4 Matching

We use an automated matching procedure to search for observations in the statewide voter files that match the name and age of each ex-felon record. Implementing this procedure requires that we specify a criterion by which the name and ages in the two sources match. Two types of matching error were considered when designing this criterion. Another individual's record in the voter file may be similar enough to an ex-felon's that it satisfies our matching criterion (McDonald and Levitt, 2008). Alternatively, the same individual's information may be presented in a sufficiently different manner in the voter file that it

[11] In the process of coding these data, we uncovered a small number of additional accepted applications that were not included in the spreadsheet maintained by the Iowa State Archives.

fails to satisfy our matching criterion. The individual's registration record may also fail to match to their discharge record because it has been purged from the voter file or is contained in another state's voter file. The political participation of ex-felons will be overestimated if the former type of matching error is more prevalent and underestimated if the later type of error is more prevalent.

Section A.1 in the Appendix describes and evaluates our matching procedure. We propose a technique to assess the prevalence of incorrect matches by slightly permuting the ages of ex-felons. Because the voter file records of non-felons should be almost equally likely to falsely match our actual criminal discharge records as our age-permuted criminal discharge records, the number of matches using the age-permuted records provides an estimate of the number of false matches. We expect the prevalence of false matches to decrease as we increase the number of variables that we match on. For example, we expect to observe fewer false matches when we match on name and birth date instead of name and birth year. The results presented in Section 6 and Section A.1 in the Appendix are consistent with this intuition. We observe almost no matches between our age-permuted discharge records and the voter file in Iowa and Rhode Island, where both the discharge data and the voter file contain birth date. In contrast, about one percent of our age-permuted discharge records match to the voter file in Maine, where we can only match on full name and birth year.

The results presented in Section A.1 also show that our matching procedure does not cause us to substantially underestimate turnout. We reach this conclusion by studying the number of cases where the corrections data and voter file records nearly match. We do systematically miss some matches because information is presented differently in the two data sources. For example, Table A.1 shows we underestimate female turnout by about seven percentage points (p.p.) because women change their last names after being discharged. However, the total number of cases affected by such matching error is relatively small. Likewise, Tables A.4 and A.7 in the Appendix show only a small number of ex-felons vote in states besides the state in which they were sentenced. We conclude from this analysis that our matching procedure underestimates the 2012 turnout of Iowa dischargees by about five p.p.

5 Iowa Results

5.1 Applications

We first examine the frequency that ex-felons applied to have their voting rights restored prior to the signing of Executive Order 42 on July 4, 2005. We are not confident that we have complete data on either applications or discharges prior to 2002. We are also concerned that ex-felons discharged immediately before the signing of Executive Order 42 on July 4, 2005 lacked sufficient opportunity to submit an application.[12] Thus, we specifically focus on the cohort of individuals discharged between January 1, 2002 and December 31, 2003. Everyone discharged from a felony sentence and some of those discharged from an aggravated misdemeanor sentence during this time period must have had an application for the restoration of voting rights approved in order to legally vote in the 2004 presidential election.

Table 2 shows that few ex-felons applied to restore their voting rights before the signing of Executive Order 42. The first row shows that only 366 of the 8,646 individuals discharged from a felony sentence in 2002 or 2003 applied to restore their voting rights prior to the signing of Executive Order 42. The 4.2 percent application rate of individuals discharged from a felony sentence was slightly higher than the 3.6 percent application rate of individuals discharged from an aggravated misdemeanor sentence, although not all individuals discharged from an aggravated misdemeanor sentence needed to apply to restore their voting rights. About 80 percent of the applications from both felony and misdemeanor applicants were ultimately approved.[13] The most commonly cited reason for the denial of an application was that the applicant had outstanding obligations (e.g., court costs, restitution, etc.) to the state.

The results in Table 2 imply that at most 302 of the 8,646 (i.e., 3.5 percent) of the individuals discharged from a felony sentence in 2002 and 2003 were eligible to vote in the 2004 presidential election. Executive Order 42 changed Iowa's felon disenfranchisement policy so

[12]The median response time to an application was about 109 days, although there a small number of cases that take over a year.

[13]Table 2 shows that eight of these approvals occurred after an initial application was denied.

Table 2: Voting rights restoration applications by 2002 and 2003 dischargees.

	(1)	(2)	(3)	(4)	(5)
	Discharged	Applied	Approved	Initially denied, later approved	Denied
Felony discharges	8,646	366	299	3	64
		4.23%	81.69%	0.82%	17.49%
Misdemeanor discharges	5,762	209	163	5	41
		3.63%	77.99%	2.39%	19.62%

Table 3: Registration and turnout rates of Iowa dischargees from 1/1/2002 to 9/30/2008.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		% of Reg. Affiliated as:				% Voted in:		
	% Reg.	Dem.	Rep.	No Party	Oth. Party	2004	2006	2008
Felony discharges								
All years ($N = 31{,}464$)	29.1	42.0	14.1	43.7	0.1	5.9	3.9	14.6
2002–2003 ($N = 8{,}646$)	28.2	43.5	13.6	42.7	0.2	7.0	5.4	14.8
2006–2007 ($N = 10{,}304$)	29.3	41.4	14.5	44.0	0.1	4.9	2.8	14.4
Misdemeanor discharges								
All years ($N = 20{,}531$)	46.9	38.7	16.3	44.8	0.1	19.1	9.1	23.4
2002–2003 ($N = 5{,}762$)	42.1	39.4	16.4	44.1	0.1	17.7	9.4	21.7
2006–2007 ($N = 7{,}590$)	47.1	38.2	15.6	46.0	0.2	17.6	7.8	22.8

that members of this cohort were eligible to vote in the 2008 presidential election without submitting an application. If the application requirement significantly inhibited ex-felon political participation, we expect to observe that this cohort voted at higher rates in the 2008 presidential election. Consistent with this expectation, Table 3 shows that 14.8 percent of this cohort voted in the 2008 presidential election.[14]

Some of the increased ex-felon turnout in 2008 could reflect the effect of time since discharge rather than the effect of removing the application requirement. Thus, we also look at the 2008 turnout rate of

[14]Seven percent of ex-felons discharged from a felony sentence between 2002 and 2003 are recorded as voting in the 2004 presidential election, suggesting that some ex-felons may have voted in 2004 without having submitted an application.

individuals discharged from a felony sentence in 2006 and 2007, as they were at a similar time since discharge in 2008 as the cohort discharged in 2002 and 2003 was in 2004. Table 3 shows that ex-felons discharged in 2002 and 2003 and ex-felons discharged in 2006 and 2007 turned out at nearly identical rates in 2008, providing further evidence that removal of the application requirement is what caused turnout to increase.

Differences in the 2004 and 2008 electoral environments could also have potentially increased ex-felon turnout between 2004 and 2008. Less educated and poorer citizens were more likely to vote for the first time in 2008 (Minnite, 2011), presumably because of Barack Obama's candidacy. We expect that individuals discharged from a felony sentence and individuals discharged from an aggravated misdemeanor sentence would be similarly affected by such an Obama effect. Yet the turnout rate of individuals discharged from an aggravated misdemeanor sentence in 2002 and 2003 only increased from 17.7 to 21.7 percent between 2004 and 2008. Moreover, a portion of this increase is likely due to the fact that some individuals discharged from an aggravated misdemeanor sentence were required to submit an application to be eligible to vote in 2004. Thus, we conclude that changes in the electoral environment are unlikely to be the primary cause of the increase in ex-felon turnout between 2004 and 2008.

We cannot rule out the possibility that some ex-felons abstained from applying to restore their voting rights because they believed their application would be denied. The Vilsack administration had substantial discretion to approve or reject applications prior to Executive Order 42 and Table 2 suggests that most applications were approved. However, strategic behavior may cause the observed acceptance rate to overstate the probability of success in the general ex-felon population. Also, some ex-felons may have inaccurately assessed the probability that their application would be approved. Thus, we cannot definitively say whether it is the cost of finding and filling out the applications or beliefs about the probability of acceptance that caused applications to reduce ex-felon turnout.

5.2 Notification

We next examine whether notifying ex-felons that their voting rights were restored by Executive Order 42 caused them to register and turnout at higher rates. Our empirical specification compares the registration

and turnout rates of ex-felons discharged just before and just after July 4, 2005. Our identification strategy is based on the assumption that ex-felons discharged just before and after July 4, 2005 should be similar both in terms of observable characteristics — such as their age and the amount of time they have been in the criminal justice system — and unobservable characteristics — such as political knowledge and desire to participate in politics. While both cohorts were automatically restored the right to vote, only those discharged after July 4, 2005 were officially informed of the governor's action. Therefore, if we observe a discontinuous change in registration and voting rates from being discharged after July 4, 2005, we can attribute it to notification.

Figure 2 shows how registration and 2008 turnout rates vary as a function of discharge date. The top panel shows that while the probability of registering varies substantially from month to month, there is a relatively steady decrease in the probability of being registered as the time since discharge decreases. The exception is the jump in the probability of being registered that corresponds with ex-felons being informed about the restoration of their voting rights beginning in July 2005. About 33 percent of ex-felons discharged in the first half of 2005 were registered, as compared to 38 percent of those discharged in the second half of 2005. Likewise, about 16 percent of ex-felons discharged in the first half of 2005 voted in the 2008 presidential election, as compared to 19 percent of those discharged in the second half of the year. Because there is no reason to expect a discontinuous increase in both registration and turnout among those discharged in July 2005 absent the policy change, these figures suggest that notifying ex-felons that their voting rights were restored increases their probability of registering and voting.

We estimate and test the statistical significance of the discontinuous jump in registration and turnout that occurred from being discharged after July 4, 2005 using Equation (1). We define a control variable forcing_i equal to the number of days after July 4, 2005 that individual i was discharged from the Iowa criminal justice system (e.g., $\text{forcing}_i = 1$ if individual i was discharged on July 5, 2005 and $\text{forcing}_i = -2$ if individual i was discharged on July 2, 2005). An outcome variable Y_i (e.g., whether individual i is registered or voted in the 2008 presidential election) is then regressed on a constant, $\mathbf{1}(\text{forcing}_i > 0)$ (i.e., an indicator for being discharged after notification begins), a kth-order polynomial of forcing_i, and the interaction between $\mathbf{1}(\text{forcing}_i > 0)$ and

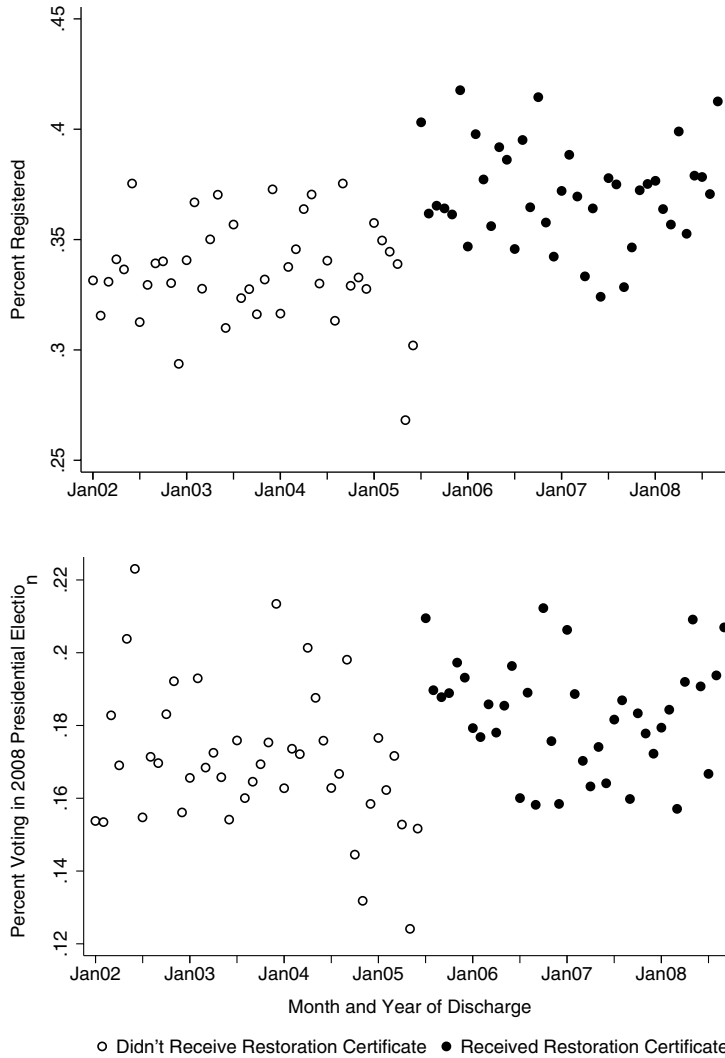

Figure 2: Registration and 2008 presidential election turnout rate by month of discharge.

a kth-order polynomial of forcing$_i$. Including a kth-order polynomial of forcing$_i$ and the interaction between $\mathbf{1}(\text{forcing}_i > 0)$ and a kth-order polynomial is a relatively standard approach to control for the effect that time since discharge has on our outcome variables absent its effect

on notification (Imbens and Lemieux, 2008).[15] In this specification γ_0, the coefficient on the indicator for being discharged after notification begins, represents the discontinuous change in the probability of an ex-felon registering or voting as result of being discharged after July 4, 2005.

$$Y_i = \Sigma_{j=0}^{k}(\theta_j \ + \gamma_j \mathbf{1}(\text{forcing}_i > 0))\text{forcing}_i^j + \epsilon_i \qquad (1)$$

We interpret our estimates of γ_0 as the estimated effect of notification on registration and turnout. This interpretation relies on the assumption that individuals who are discharged just after July 4, 2005 would register and vote at similar rates as individuals discharged on or just prior to July 4, 2005, but for the difference in notification. To increase the plausibility of this assumption, we restrict the sample to discharges that occur in relatively close proximity to July 4, 2005 in our baseline specification. We are particularly worried that individuals who were discharged before the 2004 presidential election may be more likely to vote in the 2008 presidential election (Meredith, 2009). Consequentially, the sample is restricted to 2005 discharges when estimating our baseline specification. However, the results are generally robust to other bandwidths.

Our interpretation of γ_0 is bolstered by the fact that the observable characteristics of individuals discharged between January 1, 2005 and July 4, 2005 are similar to the observable characteristics of individuals discharged between July 5, 2005 and December 31, 2005. Table A.5 in the Appendix compares the cohorts' previous political experience, age, gender, type of crime, and type of discharge and shows that the cohorts are similar in terms of each of these characteristics. The last two columns of Table A.5 show that Equation (1) also estimates few significant discontinuous changes in the observable characteristics from being discharged after July 4, 2005. Figure A.4 also shows that the number of discharges is roughly balanced across months. These patterns also help assure us that the two groups are likely similar in their unobserved characteristics as well.[16]

[15] When choosing the value of k there is a trade-off between setting k too low and missing some of the relationship between time since discharge and our outcome variables and setting k too high and over-fitting the relationship to sampling noise. As a result, we present results using $k = 0, k = 1$, and $k = 3$.

[16] Another concern is that other interventions also may have been targeted at ex-felons discharged after July 4, 2005. Our research uncovered no other criminal

Table 4 presents our estimates of the increase in registration and turnout that result from notification. Column 1 of the first row shows that individuals discharged after notification begins are 5.1 (s.e. 1.0) p.p. more likely to be registered to vote. As registration generally increases with time since discharge, this provides a lower bound on the effect of notification on registration. We report regressions in Columns 2 and 3 that control for the direct effect of time since discharge on registration using first- and third-order polynomials. Including these polynomials increases our point estimate on the effect of notification on registration to 8.8 (s.e. 2.1) and 9.5 (s.e. 4.2) p.p., respectively.

Columns 7–12 of Table 4 show that turnout also significantly increased because of notification. Column 10 shows that individuals who were notified about their voting rights were 3.8 (s.e. 0.8) p.p. more likely to vote in the 2008 presidential election. Our estimate of the effect of notification on 2008 turnout is 6.7 (s.e. 1.7) and 6.2 (s.e. 3.3) p.p. when controlling for time since discharge using first- and third-order polynomials. There is also some evidence that notification increased turnout in the 2006 midterm election, although the magnitude of the effect is smaller and its statistical significance varies depending on model specification.

Because notification did not occur until 2005, there should not be any relationship between notification and 2004 turnout. Columns 4–6 of Table 4 show that individuals who would be notified in 2005 about their voting rights did not vote at higher rates in the 2004 presidential election.[17] Thus, increased participation post-notification does not appear to simply reflect pre-notification differences.

The remainder of Table 4 shows that the increase in participation from being discharged after July 4, 2005 is greater for individuals discharged from a felony sentence than for individuals discharged from a misdemeanor sentence. Using a first-order polynomial we find that

justice or campaign activity that was based on this same cutoff. The fact that only ex-felons discharged after July 4, 2005 were notified that their voting rights were restored was not an immediately obvious component of Executive Order 42. Moreover, no documents we collected made it easier to observe information about ex-felons discharged before or after July 4, 2005.

[17]While we find a marginally significant negative effect of notification on 2004 presidential election turnout when controlling for time trends with a third-order polynomial, this finding is not robust to alternative specifications.

Table 4: Discontinuous change in political participation rates from discharge after July 4, 2005.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. variable		Registration			2004 Turnout			2006 Turnout			2008 Turnout	
Degree of polynomial (k)	0	1	3	0	1	3	0	1	3	0	1	3
Dischargees:												
All	0.051	0.088	0.095	0.006	−0.009	−0.052	0.009	0.026	0.011	0.038	0.067	0.062
$N = 8,592$	(0.010)	(0.021)	(0.042)	(0.006)	(0.013)	(0.025)	(0.005)	(0.010)	(0.019)	(0.008)	(0.017)	(0.033)
Felony	0.081	0.123	0.204	0.011	−0.002	−0.014	0.016	0.025	0.021	0.061	0.089	0.150
$N = 5,044$	(0.013)	(0.026)	(0.052)	(0.006)	(0.012)	(0.026)	(0.006)	(0.011)	(0.023)	(0.010)	(0.020)	(0.041)
Misdemeanor	0.007	0.038	−0.040	−0.002	−0.016	−0.098	−0.002	0.027	−0.002	0.004	0.036	−0.053
$N = 3,548$	(0.017)	(0.034)	(0.067)	(0.013)	(0.025)	(0.047)	(0.009)	(0.018)	(0.032)	(0.014)	(0.028)	(0.054)
Difference	0.074	0.085	0.244	0.013	0.014	0.084	0.018	−0.002	0.023	0.057	0.053	0.203
p-Value on difference = 0	0.000	0.045	0.004	0.356	0.616	0.117	0.091	0.912	0.552	0.001	0.120	0.003

Note: Each cell represents the estimated coefficient and robust standard error clustered by dischargee on γ_0 from a different specification of Equation (1). Sample includes discharges for the given offense type between January 1, 2005 and December 31, 2005.

individuals discharged from a felony sentence are 12.3 (s.e. 2.6) p.p. more likely to be registered and 8.9 (s.e. 2.0) p.p more likely to turnout in 2008 if they were discharged after July 4, 2005. Using the same specification we find that individuals discharged from an aggravated misdemeanor are 3.8 (s.e. 3.4) p.p. more likely to be registered and 3.6 (s.e. 2.8) p.p more likely to turnout in 2008. The bottom row of Table 4 shows that we can reject the null hypothesis that the estimates are the same for felons and misdemeanors at the $p = 0.045$ and $p = 0.120$ level for registration and 2008 turnout, respectively. The differences between felons and misdemeanors are greater when we control for time since discharge using a third-order polynomial. While we cannot observe data on the percentage of misdemeanors that were disenfranchising, we suspect that we observe this difference because many misdemeanors were not disenfranchising.

We present a number of robustness checks in the Appendix to provide further evidence that the patterns in Table 4 represent the effect of notification. Because the voter file contains the date of registration, we can examine when this increase in registration occurred among those discharged after July 4, 2005.[18] If this increase is caused by notification, there should be no difference in registration before notification occurs. Consistent with this expectation, Table A.6 shows only a 1.1 (s.e. 0.8) p.p. difference in registration prior to December 31, 2004. The remainder of Table A.6 shows that not all of the divergence in the number of registrants occurred immediately after notification, but that the registration gap between the two cohorts expanded by three p.p. between 2006 and 2008.

A concern about using Equation (1) to estimate the effect of notification is that it might partially reflect differences in the types of individuals who get discharged in the first and second halves of the year. To investigate this possibility, we estimate the same equation in neighboring years where notification does not discontinuously change on July 4th. The results of these placebo regressions, displayed in Figure A.5, do not show a general pattern of increased voting in 2008 among individuals discharged after July 4th. In three of the five placebo

[18]Date of registration refers to the last date a registrant updated their registration. If someone registered to vote in 2000, but updated their registration in 2005, their date of registration is listed as 2005.

years the point estimates are in the opposite direction, and only when using a first-order polynomial in 2007 is an estimate in the same direction marginally statistically significant. However, we do observe in 2002 a negative effect of being discharged after July 4 that is of a similar magnitude and level of statistical significance as our actual estimates. This finding suggests we may be somewhat understating the degree of uncertainty in our estimates.

As a final robustness check, we look at how the estimated effect of notification varies as we increase and decrease the range of observations that are included in our sample. Our baseline sample consists of individuals who were discharged within six months of the policy change (i.e., a bandwidth of six months). Figure A.6 shows that while there is some sensitivity to bandwidths of three months or less, the estimates of the effect of notification on 2008 turnout are generally robust to the choice of bandwidth.

5.3 Executive Order 70

If reinstating the application requirement reduced ex-felon political participation, we expect to observe a discontinuous drop in registration and turnout for those ex-felons discharged after December 31, 2010. Consistent with this expectation, Figure 3 shows that individuals discharged in the fourth quarter of 2010 were more likely to be registered and vote in the 2012 presidential election than individuals discharged in the first quarter of 2011. It also shows that those ex-felons who were notified that their voting rights were restored under Executive Order 42 continued to be registered and vote in the 2012 presidential election more often than those who were not notified.

Table 5 presents formal statistical tests of the patterns observed in Figure 3 using Equation (1). The discontinuous decline in both registration and 2012 turnout from being discharged after December 31, 2010 is both statistically significant and substantively large. The decline in registration is estimated to be 4.8 (s.e. 2.1) and 9.0 (s.e. 4.3) p.p. when first- and third-order polynomials are used to control for time-trends respectively. The decline in 2012 turnout is quite similar. Again, the impact is primarily concentrated on felon dischargees; both

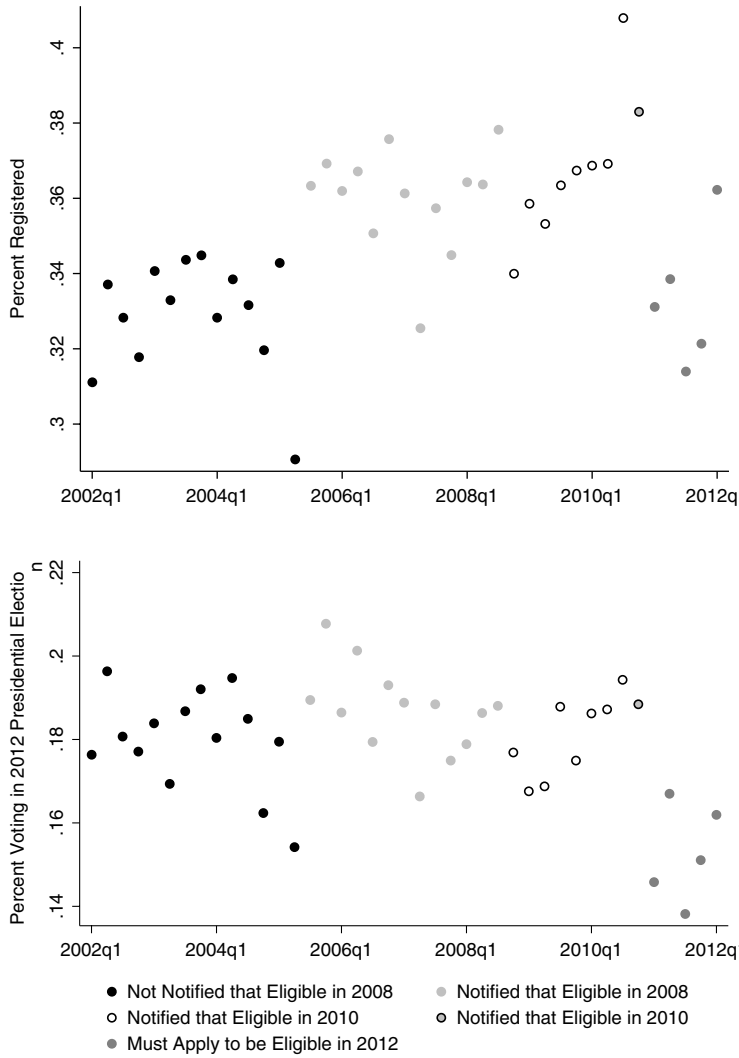

Figure 3: Registration and 2012 presidential election turnout rate by quarter of discharge.

the registration rate and 2012 turnout rate of felony discharges dropped by almost ten p.p.

If the discontinuous decrease in registration and turnout rates after December 31, 2010 reflects the effect of Executive Order 70, we should

Table 5: Discontinuous changes in 2012 presidential election turnout and 2013 registration rates.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. variable	Registration			2008 Turnout			2010 Turnout			2012 Turnout		
polynomial (k)	0	1	3	0	1	3	0	1	3	0	1	3
Discharged after 7/4/2005												
Dischargees:												
All	0.049	0.091	0.116	0.040	0.064	0.064	0.016	0.028	0.026	0.032	0.037	0.034
$N = 8,592$	(0.010)	(0.020)	(0.042)	(0.008)	(0.016)	(0.032)	(0.006)	(0.012)	(0.024)	(0.008)	(0.017)	(0.033)
Felony	0.070	0.108	0.166	0.063	0.083	0.112	0.024	0.037	0.061	0.047	0.053	0.075
$N = 5,044$	(0.013)	(0.025)	(0.052)	(0.010)	(0.019)	(0.039)	(0.007)	(0.014)	(0.030)	(0.010)	(0.020)	(0.040)
Misdemeanor	0.017	0.068	0.056	0.008	0.035	0.003	0.005	0.015	−0.018	0.009	0.014	−0.018
$N = 3,548$	(0.017)	(0.033)	(0.066)	(0.014)	(0.027)	(0.054)	(0.011)	(0.021)	(0.040)	(0.014)	(0.028)	(0.055)
Discharged after 12/31/2010												
Dischargees:												
All	−0.061	−0.048	−0.090	−0.008	−0.028	−0.063	−0.004	−0.003	−0.013	−0.035	−0.050	−0.090
$N = 8,514$	(0.010)	(0.021)	(0.043)	(0.008)	(0.015)	(0.030)	(0.005)	(0.010)	(0.021)	(0.008)	(0.016)	(0.034)
Felony	−0.114	−0.097	−0.136	0.000	−0.006	−0.017	−0.014	0.000	0.010	−0.065	−0.077	−0.092
$N = 4,615$	(0.012)	(0.024)	(0.047)	(0.007)	(0.013)	(0.024)	(0.005)	(0.009)	(0.018)	(0.009)	(0.017)	(0.034)
Misdemeanor	−0.008	−0.017	−0.086	−0.023	−0.073	−0.159	0.005	−0.016	−0.058	−0.005	−0.033	−0.121
$N = 3,898$	(0.016)	(0.033)	(0.070)	(0.014)	(0.028)	(0.059)	(0.010)	(0.020)	(0.043)	(0.014)	(0.029)	(0.062)

Note: Each cell represents the estimated coefficient and robust standard error clustered by dischargee on γ_0 from a different specification of Equation (1). Regressions estimating 7/4/2005 discontinuity include discharges for the given offense type between January 1, 2005 and December 31, 2005 and regressions estimating 12/31/2010 discontinuity include discharges for the given offense type between July 1, 2010 and June 30, 2011.

not observe any discontinuous difference in registration or turnout rates prior to this date. Columns 4–9 of Table 5 show no discontinuous change in 2008 or 2010 turnout from being discharged after December 31, 2010 among felon dischargees, although we do see some evidence of pretreatment differences among misdemeanor discharges. Unreported regressions also show that the entire discontinuous decrease in registration among those discharged after December 31, 2010 is caused by registration that occurred after someone was discharged from the correctional system.[19]

Table 5 also shows that individuals who were notified that their voting rights were restored by Executive Order 42 continued to vote at higher rates in the 2010 midterm and 2012 presidential elections than individuals who were not notified that their voting rights were restored. We estimate that being discharged after July 4, 2005 increased the probability that a felon dischargee voted in 2010 and 2012 by 3.7 (s.e. 1.4) p.p. and 5.3. (s.e. 2.0) p.p., respectively. This is somewhat less than the 8 p.p. increase we estimated in 2008. In contrast, we find that misdemeanor discharges who were discharged just before and after July 4, 2005 voted at similar rates in 2010 and 2012.

6 Maine and Rhode Island Results

The results in the previous section strongly support our hypothesis that misinformation and bureaucratic procedures reduce ex-felon turnout. In this section we examine the extent to which misinformation and bureaucratic procedure explain why ex-felons vote less often than observably similar non-felons. Table 6 shows that 13 percent of ex-felons discharged in Iowa in 2009 and 2010 — the final cohort of dischargees to get their voting rights automatically restored under Executive Order 42 — voted in the 2012 presidential election. Even after correcting for matching error, this is far below the 35 percent turnout rate predicted by Uggen and Manza (2002).[20] This section estimates the rate at which ex-felons

[19]While in theory all registration should have occurred post-discharge for those discharged after December 31, 2010, it appears that Iowa failed to purge some previously registered ex-felons from the voter file prior to the 2012 presidential election.

[20]Some of these dischargees were ineligible to vote in the 2012 presidential election because they were convicted of another felony or disenfranchising aggravated misde-

Table 6: Registration, party affiliation, and turnout of 2009 and 2010 dischargees in Maine, Rhode Island, and Iowa.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		% of Reg. Affiliated as:				% Voted in:		
	% Reg.	Dem.	Rep.	No Party	Oth. Party	2008	2010	2012
Iowa								
All discharges ($N = 17,552$):								
Birth date	37.2	38.8	16.0	44.7	0.5	12.7	7.0	18.2
Birth date − 35 days	0.0		No registered voters			0.0	0.0	0.0
Birth date + 35 days	0.0	0.0	100.0	0.0	0.0	0.0	0.0	0.0
Felony discharges ($N = 9,960$):								
Birth date	26.7	39.6	14.8	45.2	0.5	5.1	4.4	13.3
Birth date − 35 days	0.0		No registered voters			0.0	0.0	0.0
Birth date + 35 days	0.0	0.0	100.0	0.0	0.0	0.0	0.0	0.0
Maine								
All discharges ($N = 2,239$):								
Birth year	39.1	29.6	16.5	45.4	8.5	11.4	6.9	12.1
Birth year − 2 years	1.3	13.8	17.2	58.6	10.3	0.7	0.5	0.6
Birth year + 2 years	1.1	27.3	36.4	31.8	4.5	0.7	0.5	0.7
Rhode Island								
All discharges ($N = 6,631$):								
Birth date	45.8	44.6	6.0	48.9	0.4	7.9	2.7	9.8
Birth date − 35 days	0.0	50.0	0.0	50.0	0.0	0.0	0.0	0.0
Birth date + 35 days	0.0	33.3	33.3	33.3	0.0	0.0	0.0	0.0
Felony discharges ($N = 4,263$):								
Birth date	43.3	46.7	5.6	47.4	0.3	5.5	2.4	9.1
Birth date − 35 days	0.0		No registered voters			0.0	0.0	0.0
Birth date + 35 days	0.0	0.0	50.0	50.0	0.0	0.0	0.0	0.0

from two other states with clear voting rights restoration policies register and vote in the 2012 presidential election. If dischargees from Maine and Rhode Island also vote substantially less than Uggen and Manza predict, then we would conclude that there are other factors in addition to misinformation and bureaucratic procedures that cause felons to vote at lower rates than observably similar non-felons.

Even though Maine never disenfranchises incarcerated individuals, Table 6 shows that ex-felons register and vote at low rates post-discharge. 39.1 percent of dischargees match a registration record in the Maine voter file and 12.1 percent of dischargees match a 2012 turnout record, which is a slight increase from the 11.4 percent that match to a 2008 turnout record.

Because we can match only on year of birth instead of date of birth in the Maine, we are more concerned about an ex-felon's discharge record matching other individuals' voter records in Maine. Table 6 shows there are more false matches, although the number of such matches is still relatively low. Slightly more than one percent of our age-permuted discharge records match a registrant's in the voter file. Thus, we conclude that the ex-felon registration rate is at least 38 percent and the ex-felon turnout rate is at least 11.5 percent.

Table 6 reveals broadly similar patterns in Rhode Island. A slightly higher percentage of dischargees are registered to vote in Rhode Island than in Maine. 45.8 percent of individuals discharged from a Rhode Island prison match a registration record in Rhode Island. This registration rate drops slightly to 43.3 percent if we focus on individuals discharged from a felony sentence. Just under 10 percent of discharges match to a 2012 turnout record. Because we observe almost no matches between our age-permuted discharge data and the voter file, we conclude that just about all of the matches between the discharge and voter file records in Rhode Island accurately reflect the political participation of the dischargee.

We are concerned that dischargees from Rhode Island may be more likely to move to, and subsequently vote in, other states because Rhode

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meanor before November 6, 2012. Although we were unable to get the exact number from the Iowa Department of Correction, we were able to scrape the subsequent conviction history of these dischargees from http://www.doc.state.ia.us/OffenderInfo.asp. Based on these records, we conclude that about 10 percent of these dischargees were ineligible to vote in the 2012 presidential election.

Island is so small. However, Table A.7 shows that there is minimal registration and 2012 turnout by Rhode Island dischargees in either Connecticut and New York, where we expect to observe the most out-of-state turnout. This suggests there is also little registration or 2012 turnout in other states as well.

Another concern is that some individuals discharged from a Rhode Island prison in 2009 and 2010 were ineligible to vote in the 2012 presidential election because they were in prison for another felony sentence. Although we were unable to get precise information on 2012 election eligibility, we were able to use the online Rhode Island Inmate Search to find the percentage of 2009 and 2010 dischargees who were listed as being incarcerated in January of 2014.[21] We find that about 14.8 percent of all dischargees and 17.2 percent of felon dischargees were incarcerated in January 2014, which suggests that about 11 percent of non-incarcerated ex-felons voted in 2012.[22]

We use the approach of Uggen and Manza (2002) to compare the turnout rates we observe in Maine and Rhode Island to demographically similar members of the general public. Table A.8 in the Appendix shows how race, ethnicity, educational level, sex, marital status, age, and employment status relate to the probability of self-reported turnout in the 2012 Current Population Survey. Ideally, we would observe these variables for all of the dischargees, use the results reported in Table A.8 to construct a predicted turnout probability for an individual with the observable characteristics of each dischargee, and average this predicted turnout probability across all of the dischargees. Unfortunately, educational, marital, and employment status are not observed in the Maine and Rhode Island discharge data. Instead, we assume that the characteristics of dischargees in Maine and Rhode Island are similar to those individuals who reported that they would be released within the next month on the 2004 NACJD Survey of Inmates in State and Federal Correctional Facilities.[23] Using the regression coefficients reported in Column 2 of Table A.8, we find the lower bound on the average pre-

[21]The web address of this inmate search is http://www.doc.ri.gov/inmate_search/search.php.

[22]$9.1/(100 - 17.2) \approx 11.0$. and $9.8/(100 - 14.8) \approx 11.5$.

[23]The characteristics of such individuals are summarized in the bottom row of Table 1. Results are nearly identical if we use the characteristics of all respondents or respondents from New England.

dicted turnout probability of such individuals using Uggen and Manza's approach is 32 percent.[24]

Put together, the results in this section highlight both the importance and limits of voting rights restoration policies on ex-felon political participation. Rhode Island's attempt to register ex-felons upon discharge is the most pro-active state policy we know of to reintegrate ex-felons into the electoral process. Thus, it is not surprising that we observe larger estimates of the ex-felon registration rate in Rhode Island than in Iowa, Maine, New Mexico, New York, and North Carolina, often by a wide margin (Meredith and Morse, 2014). Yet, the ex-felon turnout rate in Rhode Island is less than 10 percent and not noticeably higher than the ex-felon turnout rates in these other states. Moreover, we estimate that the ex-felon turnout rate in Maine and Rhode Island lags the turnout rate of demographically similar members of the general public by at least 20 p.p. despite few immediate barriers to ex-felon voting in these states. This suggests that there are other forces besides voting rights restoration policies that contribute to large turnout gap between felons and non-felons.

7 Discussion

We show that recently enfranchised ex-felons vote less often than demographically similar members of the general public. This suggests that Uggen and Manza (2002) substantially overstate the number of citizens who would vote absent felon disenfranchisement. Extending their methodology to the 2012 election, we would expect felon disenfranchisement to prevent over two million votes. But our finding that less than 15 percent of recently enfranchised ex-felons voted in Iowa, Maine, and Rhode Island suggests that actually fewer than one million disenfranchised citizens would have voted. This significant discrepancy suggests that Uggen and Manza (2002) likely overstate the electoral consequences of de jure felon disenfranchisement.

[24]Because employment status is not observed, we assume that no ex-felons are employed to construct this lower bound on predicted turnout rate. We also do not adjust for the finding in Column 3 of Table A.8 that the general public votes at a significantly higher rate in Maine and an insignificantly higher rate in Rhode Island than in the rest of the country.

Three caveats to this conclusion are worth considering. First, the population of ex-felons that we study in Iowa, Maine, and Rhode Island may not be representative of the ex-felon population more generally. Second, these states' electoral environments may not encourage ex-felon voter turnout as much as we conjecture. Finally, felon disenfranchisement may still be electorally consequential even if we are accurately measuring the rate at which ex-felons vote. We discuss each of these caveats in turn.

Our Maine and Rhode Island analysis focuses on the registration and turnout rates of ex-felons who were recently incarcerated. Many felons though are never incarcerated, and we expect that felons sentenced only to probation are more likely to vote than felons who are incarcerated. This potentially explains why we observe a slightly higher turnout rate in Iowa, where we also observe felons sentenced to only probation, than in Maine or Rhode Island.

Our analysis also focuses on ex-felons' turnout in relatively close proximity to discharge. The vast majority of states do restore voting rights on or before the completion of a sentence, but because some disenfranchise ex-felons for life, nearly half of the disenfranchised population is found in just 12 states, as the stock of ex-felons accumulates across time (Uggen *et al.*, 2012). A sizable portion of the disenfranchised population is much further removed from the criminal justice system than the population we study. While we observe that ex-felons discharged in Iowa in 2002 and 2010 voted at similar rates in the 2012 presidential election, this does not imply that ex-felons discharged in previous decades vote at the same rate as ex-felons discharged in the 2000s. In Section A.2 in the Appendix we do show that individuals discharged from a felony sentence in the 1990s in Iowa only vote at a slightly higher rate than individuals discharged from a felony sentence in the 2000s. Unfortunately, Maine and Rhode Island do not have credible data on discharges before the 2000s, and we were unable to assess the turnout rates of ex-felons who are further removed from the criminal justice system in either state.

Our findings suggest that application requirements in lifetime disenfranchisement states prevent a sizable number of ex-felons from voting. We show that requiring applications reduced ex-felon turnout in Iowa by about 10 p.p. A similar application process is currently active in a number of states, including Alabama, Florida, Kentucky, Mississippi, and

Tennessee. Uggen *et al.* (2012) estimate that more than 2,000,000 ex-felons are disenfranchised in these five states, which suggests that about 200,000 more ex-felons would vote if these application requirements were eliminated.

While we find clear evidence that being subject to an application requirement reduced ex-felon turnout in Iowa in 2012, it is possible that reinstating the application requirement also reduced the turnout of ex-felons who were not subject it. While Executive Order 70 states that it does not affect voting rights restored by Executive Order 42, this may not have been commonly understood. Notification might have increased turnout over time had it not been for this additional misinformation. Executive Order 70 also may have made groups more hesitant to target get-out-the-vote (GOTV) efforts at ex-felon communities, as doing so risked encouraging a felony. Because GOTV activity is particularly effective at mobilizing low propensity voters in presidential elections (Arceneaux and Nickerson, 2009), turnout is likely to drop in response to Executive Order 70.

The electoral environments in Maine and Rhode Island also may not be as conducive to ex-felon turnout as we claim. Maine is one of the two states that allows felons to vote and Rhode Island is the most aggressive state in registering ex-felons upon discharge. Thus, we assumed that there were fewer structural barriers to ex-felon turnout in these states than in just about any others. Yet, Rhode Island, and to a lesser extent Maine, lacked competitive races in 2012, meaning there was less GOTV activity than in battleground states. Moreover, there may still be ample confusion in Maine about voting rights, as disenfranchisement is the policy in almost every other state and there is no notification process that we are aware of.

The potential for misinformation about voting rights in Maine highlights the important distinction between the number of disenfranchised citizens who would vote and the number of citizens who abstain from voting because of felon disenfranchisement laws. Our results suggest that many enfranchised ex-felons are de facto disenfranchised because they are misinformed about their voting rights. Uggen and Manza's (2002) analysis of the electoral consequence of felon disenfranchisement implicitly assumes that felon disenfranchisement only affects the political participation of the population that is currently disenfranchised. Our results suggest that misinformation may also cause felon disenfranchise-

ment to affect the participation of enfranchised citizens. Specifically, we find that ex-felons discharged in Iowa before July 4, 2005 were five to ten p.p. less likely to vote in subsequent presidential elections than ex-felons discharged after. This finding is consistent with our hypothesis that many enfranchised ex-felons abstain from voting because they are uncertain about their voting rights.

The electoral consequences of de facto disenfranchisement are potentially huge. Uggen *et al.* (2012) estimates that the ex-felon population is about three times larger than the population currently under some form of supervision. Thus, a five p.p. increase in the participation rate of the ex-felon population has roughly the same electoral impact as a 15 p.p. increase in the participation rate of the population currently under supervision. While our results suggest that Uggen and Manza (2002) overestimate the percentage of ex-felons that would vote, this disparity could be more than offset by the demobilizing effects of disenfranchisement policy on the enfranchised population. Future work should focus on more precisely quantifying the amount of de facto disenfranchisement, especially in communities that are most likely to be misinformed.

Our finding that informative mailings increased turnout is important because Brennan Center data shows that only about half of states are statutorily required to notify ex-felons about their voting rights.[25] Within the last 10 years, Louisiana, Minnesota, New Mexico, New York, and North Carolina considered bills that require the state to notify ex-felons about their voting rights (Meredith and Morse, 2014). Our results demonstrate that notification *can* increase ex-felon participation. However, legislating an ex-felon notification policy will not necessarily translate into an ex-felon becoming better informed about their voting rights. Unlike Iowa, New Mexico, New York, and North Carolina delegate responsibility for notification to individual members of criminal justice agencies. Meredith and Morse (2014) find no evidence that turnout increased after these notification laws were passed. This brings into question whether notification protocols are actually implemented. Further work is needed to better understand how the specifics of notification policy relate to its effectiveness.

[25]The Brennan Center data was reported to us by Erika Wood, Deputy Director of the Democracy Program, in an email dated March 11, 2011.

Appendix

A.1 Matching Procedure

This section details the procedure that we use to match discharge records to voter file records. In general, the matching procedure identifies records with similar names that share a common birth date. The simplest match criterion would require that the first, middle, and last names are presented in the exact same manner in both data sources. However, we risk missing matches with such a strict strategy because names are often presented slightly differently. This may occur because someone uses a shortened version of their first name in one source but not the other (e.g., Mike in one, Michael in the other) or only lists a middle name in one source but not the other (e.g., Andrew in one, nothing in the other), or because of a spelling error (e.g., Nicholus in one, Nicholas in the other). Instead, we consider two names as similar if their first, middle, and last names individually satisfy one of the following criteria[26]:

1. The name in source one is the same as the name in source two.

2. The name in source one is a single character and the name in source two begins with that character (e.g., "R" and "Robert").

3. The name in source one and the name in source two are multiple characters and the name in source one is contained in the name in source two (e.g., "Chris" and "Christopher").

4. The name in source one and the name in source two are of length $N \geq 2$, and the name in source one is the same as the name in source two after character $i \in [1, N]$ is removed from the names in both the sources (e.g., "Marc" and "Mark").

5. The name in source one is length N, the name in source two is length $N+1$, and the name in source one is the same as the name in source two after character $i \in [1, N]$ is removed from the name in source two (e.g., "Michele" and "Michelle").

[26] All non-alpha characters (e.g., ".", "-", and " ") are removed from names in both files prior to running this criterion.

6. The name in source one and the name in source two are of length $N \geq 2$, and the name in source one is the same as the name in source two after character $i \in [1, N-1]$ and character $i+1$ are flipped in source two (e.g., "Michael" and "Micheal").

7. The name in source one is identified by a program that we wrote to be a common nickname or misspelling of the name in source two (e.g., "Al", "Allan", "Alen", and "Allen" are common nicknames or misspelling of the name "Alan").

8. Our matching criterion is also satisfied for the middle name when the middle name is missing in source one, but not in source two.

The top row of Table A.1 shows that 27,394 voter file records match to a discharge record using the matching procedure described above. These 27,394 matches are a combination of true matches — dischargees

Table A.1: Sensitivity analysis of matching procedure.

	Last name	First name	Middle name	Year of birth	Month of birth	Day of birth	Registered	2012 Turnout
Birth date	S	S	S	E	E	E	27,394	14,184
Birth date − 35 days	S	S	S	E	E	E	4	2
Birth date + 35 days	S	S	S	E	E	E	8	7
Birth date	E	E	S	E	E	E	25,945	13,478
Birth date − 35 days	E	E	S	E	E	E	2	1
Birth date + 35 days	E	E	S	E	E	E	3	2
Birth year	E	E	S	E	X	X	26,902	14,065
Birth year − 2 years	E	E	S	E	X	X	659	432
Birth year + 2 years	E	E	S	E	X	X	637	434

Note: $N = 75{,}949$ unique individuals discharged in Iowa between 1/1/2002 and 2/28/2012. "S" indicates that this variable matches using the similar name match, "E" indicates that this variable matches exactly, and "X" indicates that this variable is not included in the match.

matching their own voter file record — as well as false matches — dischargees matching the voter file record of a different individual with the same birth date and similar name. We estimate our error rate by matching discharge records with slightly permuted ages to the voter file. The expected number of false matches between the actual discharge records and the voter file is nearly identical to the expected number of matches between the age-permuted discharge records and the voter file. Because all of the matches between the age-permuted discharge records and the voter file are known to be false matches, the difference between the number of matches using these two strategies provides an estimate of the number of correct matches.

Rows 2 and 3 of Table A.1 show that our matching procedure produces almost no false matches. Four voter file records match our age-permuted discharge records when we construct placebo dischargees that are 35 days older than the actual dischargees.[27] Likewise, eight voter file records match the record of placebo dischargees that are 35 days younger than the actual dischargees. Thus, we conclude that just about all of the 27,394 matches are correct matches.

Comparing rows 1 and 4 of Table A.1 shows that our matching procedure finds a modest number of matches that would be missed if we only matched names exactly. Our matching procedure finds 1,449 registration records and 706 2012 turnout records where the first or last name does not exactly match across the two sources. Given that there are 75,949 unique discharge records, this equates to about a two p.p. increase in the registration rate and a one p.p. increase in the 2012 turnout rate.

The final rows of Table A.1 illustrate that we get more false matches when we match on year, rather than date, of birth. Comparing rows 4 and 7 shows that we find 957 more matches between the actual discharge data and voter file when we match only on year of birth. However, we also find about 645 more matches between the age-permuted discharge data and the voter file. Thus, we conclude that about 645 of the 957 additional matches are false matches, with the remaining 312 matches being cases in which the same individual is listed as having a different month or day of birth across the two sources.

[27] We use 35 days because it is the smallest number of days in which our placebo dischargees are born on the same day of the week, but a different month, as the actual dischargees.

Table A.2 further explores the possibility that the same individual has a different birth date in the two data sources and thus would fail to match. The first row of Table A.2 shows that there are 250 cases where a voter file record has a similar first, middle, and last name, the same month and day of birth, but a different year of birth as a record in the discharge data.[28] About 175 of these cases appear to be the same individual's record. There are also 143 cases in which everything matches except for birth month and 438 cases in which everything matches except for birth day, 310 of which appear to be the same individual's record. Overall, Table A.2 suggests that we miss about 500 registration records and 200 2012 turnout records because of typographical errors in birth dates, although there could be additional

Table A.2: Birth dates that almost match using our matching procedure.

	Year of birth	Month of birth	Day of birth	Registered	2012 Turnout
Birth date	N	E	E	250	123
Birth date − 35 days	N	E	E	73	42
Birth date + 35 days	N	E	E	63	44
Birth date	E	N	E	143	73
Birth date − 35 days	E	N	E	56	35
Birth date + 35 days	E	N	E	68	50
Birth date	E	E	N	438	230
Birth date − 35 days	E	E	N	217	137
Birth date + 35 days	E	E	N	203	132

Note: $N = 75,949$ unique individuals discharged in Iowa between 1/1/2002 and 2/28/2012. "E" indicates that this variable matches exactly and "N" indicates that this variable does not match exactly. All matches have a similar first, middle, and last name.

[28]For computational reasons, this analysis only considers cases where the tens or the ones digit of the year was the same in the voter file and discharge data.

cases where the same individual's birth date is presented in a significantly different manner across the two sources.

Table A.3 shows that there are more cases in which the same individual's discharge and voter file record fail to match because their name is substantially different across the two sources. Most of these cases are females that match on everything except their last name. There are 3,656 voter file records that match the first name, middle name, and date of birth, but not the last name, of a female dischargee's record. We only find about 1,700 similar cases in the age-permuted discharge data, implying that about 2,000 females register using a different last name than what is on their discharge record. In contrast, we find fewer than 100 males register using a different last name than their discharge record. Thus, we speculate that most of these missed matches occur because females change their last name post-discharge due to marriages and divorces.

The remainder of Table A.3 shows that there are fewer cases in which the same individual's discharge and voter file record fail to match because the first or middle name is substantially different across the two sources. A total of 1,200 voter file records match a discharge record on everything except first or middle name. There are about 900 similar cases in the age-permuted discharge data, implying that about 300 dischargees register using a different first or middle name than what is on their discharge record. In sum, Table A.3 indicates that we miss about 2,350 registration records and 1,375 2012 turnout records because of differences in how names are presented in the two sources. Once again, it is possible that there are even more cases where the same individual's name is presented in a significantly different manner in the two sources. For example, we would fail to match dischargees who adopt Islamic names post-discharge to their voter file record. However, we expect the number of such cases to be relatively small.

Our matching procedure may also underestimate political participation if Iowa dischargees vote in states besides Iowa. Table A.4 examines how frequently Iowa discharge records match to records in the Missouri, New York, Minnesota, and Ohio voter files. We use the same matching procedure described above when working with the Missouri and New York voter file. The results in Table A.4 indicate that we miss about 428 registration records and 113 2012 turnout records because they occur

Table A.3: Names that almost match using matching procedure.

	Last name	First name	Middle name	Registered	2012 Turnout
Females					
Birth date	N	S	S	3,656	2,442
Birth date − 35 days	N	S	S	1,672	1,247
Birth date + 35 days	N	S	S	1,731	1,246
Birth date	S	N	S	264	166
Birth date − 35 days	S	N	S	216	144
Birth date + 35 days	S	N	S	208	134
Birth date	S	S	N	71	38
Birth date − 35 days	S	S	N	11	6
Birth date + 35 days	S	S	N	15	11
Males:					
Birth date	N	S	S	4,105	2,767
Birth date − 35 days	N	S	S	3,998	2,703
Birth date + 35 days	N	S	S	4,027	2,786
Birth date	S	N	S	720	480
Birth date − 35 days	S	N	S	638	431
Birth date + 35 days	S	N	S	630	432
Birth date	S	S	N	148	76
Birth date − 35 days	S	S	N	49	31
Birth date + 35 days	S	S	N	31	18

Note: $N = 17,233$ unique females and 58,716 unique males discharged in Iowa between 1/1/2002 and 2/28/2012. "S" indicates that this variable matches using the similar name match and "N" indicates that this variable does not match using the similar name match. All matches have the same date of the birth.

Table A.4: Iowa discharge records matched to other states' voter files.

	Registered	2012 Turnout
Missouri		
Birth date	442	122
Birth date − 35 days	14	9
Birth date + 35 days	15	9
New York		
Birth date	164	47
Birth date − 35 days	87	32
Birth date + 35 days	82	35
Minnesota		
Birth year	1,360	1,246
Birth year − 2 years	1,105	1,028
Birth year + 2 years	1,140	1,048
Ohio		
Birth year	5,682	3,702
Birth year − 2 years	5,570	3,455
Birth year + 2 years	5,664	3,779

Note: $N = 75,949$ unique individuals discharged in Iowa between 1/1/2002 and 2/28/2012.

in Missouri and 80 registration records and 14 2012 turnout records because they occur in New York.

We must use a modified matching procedure to match the Iowa discharge data to the Minnesota and Ohio voter files because these voter files contain year of birth instead of date of birth. First, we run our program that transforms common nicknames or misspellings of a name into a common root name. Next, we generate a modified first name that only contains the first seven letters of the first name. Then, the discharge data and voter file are exactly matched on year of birth, last name, and the modified first name. Finally, we keep the cases within these matches in which the middle name in the discharge data and the middle name in the voter file are similar according to our baseline matching procedure.

The bottom rows of Table A.4 show that we continue to find a small number of cases in which an Iowa dischargee is registered to vote or

voted in the 2012 presidential election in either Minnesota or Ohio. As we demonstrate in Table A.1, there are substantially more matches between the age-permuted discharge data and the voter file when we match on year, rather than date, of birth. Although this makes it more difficult to make precise statements about the number of cases in which an Iowa discharge matches to his or her own record in the Minnesota or Ohio voter file, Table A.4 suggests that about 250 Iowa dischargees are registered to vote in Minnesota and 65 Iowa dischargees are registered to vote in Ohio.

We apply a simple model to calibrate what the patterns in Table A.4 imply about the total number out-of-state registrants and 2012 voters. We expect such individuals disproportionately reside in states that border Iowa. Table A.4 suggests that about 665 dischargees are registered to vote in Minnesota and Missouri. If dischargees are as densely registered in other states that border Iowa, this implies that about 1,900 dischargees are registered to vote in a state that borders Iowa.[29] Table A.4 also suggests that about 145 dischargees are registered to vote in New York and Ohio. If dischargees are as densely registered in other states that do not border Iowa, this implies about 1,300 are registered to vote in a state that does not border Iowa.[30] Performing similar calculations on 2012 turnout suggests there were about 900 dischargees who voted in the 2012 presidential election in states that border Iowa and 900 dischargees that voted in the 2012 presidential election in states that do not border Iowa.

Considering all of the reasons why an individual may not match their own participation record, we believe we are missing about 6,000 registration records and about 3,375 2012 turnout records. Given that there are 75,949 unique discharge records, this suggests our matching procedure underestimates the 2012 turnout rate of Iowa dischargees by about 5 p.p.

[29]Minnesota and Missouri contain about 34.9 percent of the population that resides in a state that borders Iowa and $665/0.349 \approx 1{,}900$.

[30]New York and Ohio contain about 11.0 percent of the population that resides in a state that does not border Iowa and $145/0.110 \approx 1{,}300$.

A.2 Alternative Iowa Discharge Data

One limitation of discharge data provided by the Iowa Department of Corrections is that the data only contains records of discharges that occurred since 2002. This prevents us from observing whether ex-felons who are further removed from the criminal justice system vote at different rates than ex-felons who were recently discharged. To get older discharge records, we scraped the discharge records available via an Iowa offender search engine, http://www.doc.state.ia.us/OffenderInfo.asp, maintained by the Department of Corrections. Iowa uses a seven-digit offender number to track people across time. An individual's history in the Iowa Department of Corrections is available at http://www.doc.state.ia.us/InmateInfo.asp?OffenderCd=ABCDEFG, where ABCDEFG represents the individual's seven-digit offender number. We scraped the webpages associated with an offender identification number between 0000001 and 0089999, 1000000 and 1170000, 6000000 and 700000, and every seven-digit number that contains "0" in the first digit, does not contain "0" in the second digit, and contains "0" in the third digit (e.g., 0101535 and 0303673). We selected these ranges by randomly generating offender numbers between 0 and 7000000, and observing that 6,453 of 6,458 of the records that we scraped had an identification number that fit one of these four criteria.

Figure A.1 shows that we observe a substantial number of discharge records from the early 1990s in the scraped data set. The biggest limitation of these alternative discharge data is that we do not know how comprehensive these data are. We caution against interpreting the scraped sample as a census of discharge records because not every discharge record from our primary data set appears on the website.[31] Unlike our primary discharge data, which only includes information on the final discharge from a single sentence, the scraped data also includes information on intermediate discharges (e.g., discharged prison to parole) that occur within a single sentence. Unfortunately, it is not always possible to observe whether a discharge record corresponds to

[31] A total of 82.3% of the felony records and 84.5% of the aggravated misdemeanor records contained in our primary discharge data set appear in the scraped data set. We also observe a non-trivial number of felony and aggravated misdemeanor records in the scraped data that do not appear in our primary discharge data set.

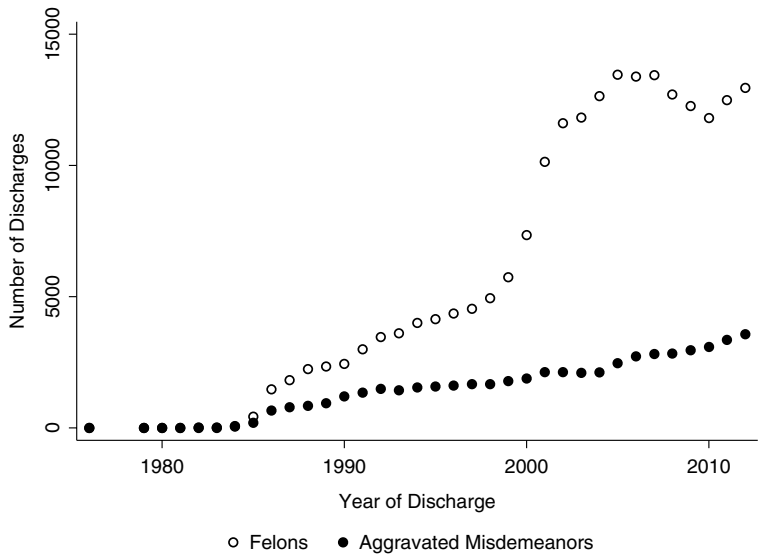

Figure A.1: Discharges per year in alternate Iowa discharge data.

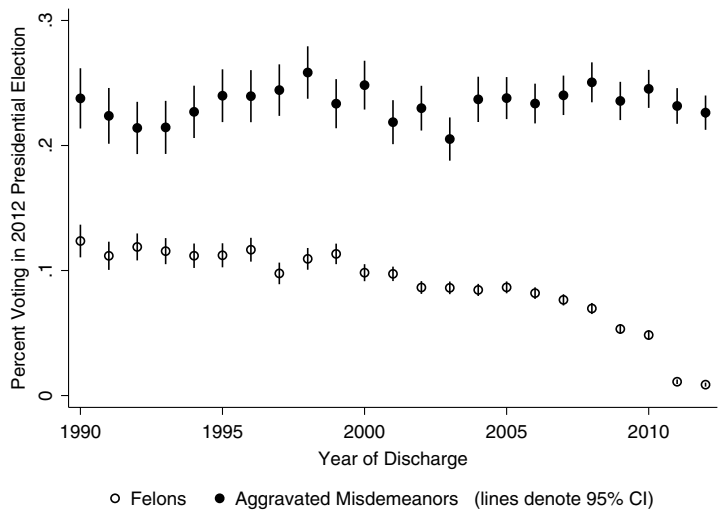

Figure A.2: Turnout rate by discharge year in alternate Iowa discharge data.

an intermediate or final discharge in the scraped data. Thus, we only consider an individual's most recent discharge when constructing Figure A.1 and looking at turnout by year of discharge.

Figure A.2 shows that turnout among ex-felons increases slightly with time since discharge among ex-felons. About 11 percent of ex-felons who were last discharged from the Iowa correctional system in the early 1990s voted in the 2012 presidential election, as compared to about nine percent of ex-felons who were last discharged in the early 2000s. Because Figure A.1 shows that the number of felons decreases across time, compositional changes in the characteristics of felons discharged at different points of time could be masking some changes in turnout likelihood with respect to time since discharge. However, Figure A.2 suggests that ex-felons who are further removed from discharge also vote at a substantial lower rate than Uggen and Manza (2002) and Uggen and Manza (2004; 2006) predict. This conclusion is further bolstered by the finding that the turnout rate of individuals discharged from an aggravated misdemeanor sentence also appears to vary within a relatively small range across time.

A.3 Additional Tables and Figures

Executive Department

RESTORATION OF CITIZENSHIP

To All to Whom these Presents Shall Come – Greetings:

KNOW YE, that by authority in me vested by law I, Thomas J. Vilsack, Governor of the State of Iowa, in the name and by the authority of the people thereof, do hereby restore to [redacted] the rights of citizenship including that of voting and qualification to hold public office which were forfeited by conviction of the crime of [redacted], in the District Court of the State of Iowa in and for [redacted] County and for which he/she was sentenced to [redacted]

This Restoration of Citizenship shall not include rights with respect to firearms as provided in Chapter 724, Weapons, of the Code of Iowa nor be construed as a Pardon or as a remission of guilt or forgiveness of the offense, and shall not operate as a bar to greater penalties for second offenses or subsequent convictions or conviction as a habitual criminal.

This ORDER is granted upon the recommendations of the Iowa Board of Parole.

IN TESTIMONY WHEREOF, I have hereunto set my hand and caused to be affixed the Great Seal of the State of Iowa. Done at Des Moines this 30th day of April in the year of our Lord two thousand and three.

Thomas J. Vilsack, Governor

Figure A.3: Sample certificate that notifies ex-felon of the restoration of voting rights.

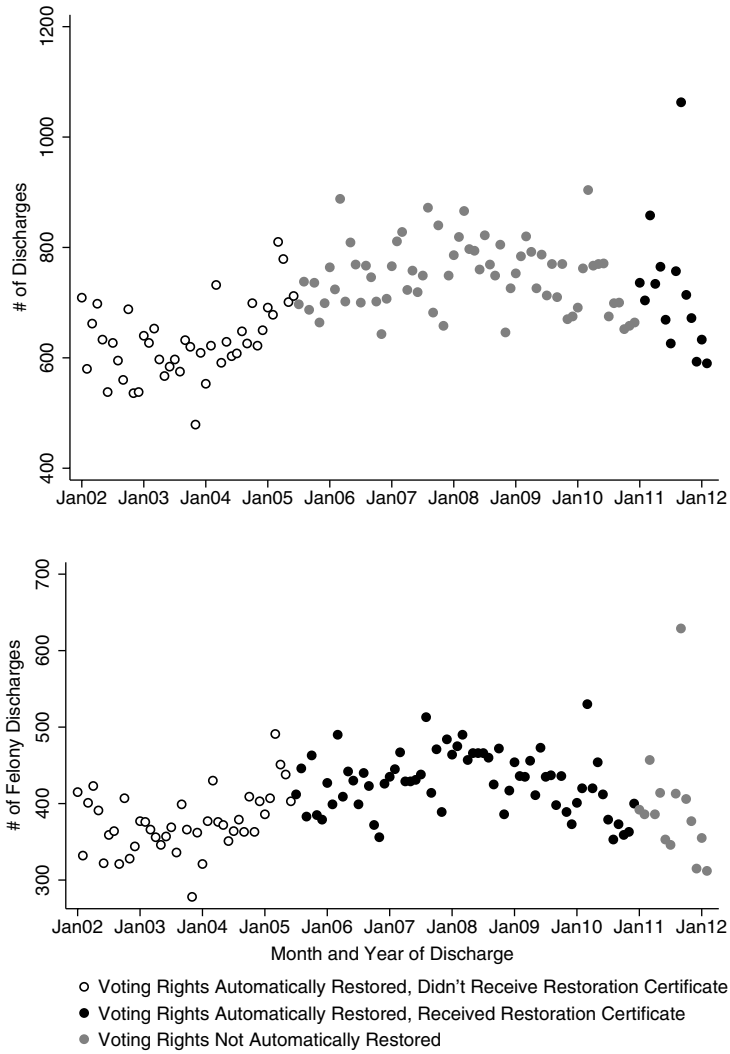

Figure A.4: Number of discharges in Iowa by month.

Table A.5: Characteristics of individuals discharged pre- and post-July 4th in 2005.

	(1) Discharged after 7/4 $N = 4{,}163$	(2) Discharged by 7/4 $N = 4{,}429$	(3) Difference in means	(4) Linear trend	(5) Cubic trend
% 2000 Turnout	0.067	0.061	0.007	0.004	−0.007
	(0.004)	(0.004)	(0.005)	(0.010)	(0.021)
% 2002 Turnout	0.040	0.036	0.004	−0.004	−0.002
	(0.003)	(0.003)	(0.004)	(0.008)	(0.017)
% 2004 Turnout	0.100	0.094	0.006	−0.009	−0.052
	(0.005)	(0.004)	(0.006)	(0.013)	(0.025)
Year of birth	1970.4	1970.3	0.124	−0.857	−1.356
	(0.17)	(0.16)	(0.232)	(0.470)	(0.958)
% Male	0.782	0.794	−0.012	−0.022	−0.022
	(0.006)	(0.006)	(0.009)	(0.018)	(0.036)
% C Class felony	0.155	0.148	0.007	0.017	0.035
	(0.006)	(0.005)	(0.008)	(0.015)	(0.031)
% D Class felony	0.400	0.412	−0.012	0.000	0.019
	(0.008)	(0.007)	(0.011)	(0.021)	(0.043)
% Other felony	0.029	0.030	−0.001	−0.009	−0.030
	(0.003)	(0.003)	(0.004)	(0.008)	(0.017)
% Misdemeanor	0.416	0.410	0.006	−0.009	−0.024
	(0.008)	(0.007)	(0.011)	(0.022)	(0.044)
% Early discharge	0.411	0.409	0.002	0.011	0.007
	(0.008)	(0.007)	(0.011)	(0.021)	(0.042)
% Alcohol	0.204	0.199	0.005	0.020	0.000
	(0.006)	(0.006)	(0.009)	(0.017)	(0.036)
% Drug	0.234	0.217	0.017	−0.018	−0.009
	(0.007)	(0.006)	(0.009)	(0.018)	(0.037)
% Property	0.300	0.316	−0.016	0.006	0.006
	(0.007)	(0.007)	(0.010)	(0.020)	(0.041)
% Violent	0.161	0.166	−0.005	−0.002	0.001
	(0.006)	(0.006)	(0.008)	(0.016)	(0.034)

(*Continued*)

Table A.5: (*Continued*)

	(1) Discharged after 7/4 $N = 4{,}163$	(2) Discharged by 7/4 $N = 4{,}429$	(3) Difference in means	(4) Linear trend	(5) Cubic trend
% Other types	0.073	0.072	0.000	0.004	0.032
	(0.005)	(0.005)	(0.007)	(0.015)	(0.030)
% Prison discharge	0.135	0.138	−0.003	−0.026	−0.006
	(0.005)	(0.005)	(0.007)	(0.015)	(0.031)
% Parole discharge	0.197	0.205	−0.008	−0.024	−0.024
	(0.006)	(0.006)	(0.009)	(0.018)	(0.036)
% Probation or	0.668	0.657	0.011	0.050	0.030
other discharge	(0.007)	(0.007)	(0.010)	(0.021)	(0.042)

Note: Sample includes all discharges between January 1, 2005 and December 31, 2005. Linear trend and cubic trend refer to the estimate of γ_0 when Equation (1) is estimated with the listed variable as the dependent variable when $k = 1$ and $k = 3$, respectively.

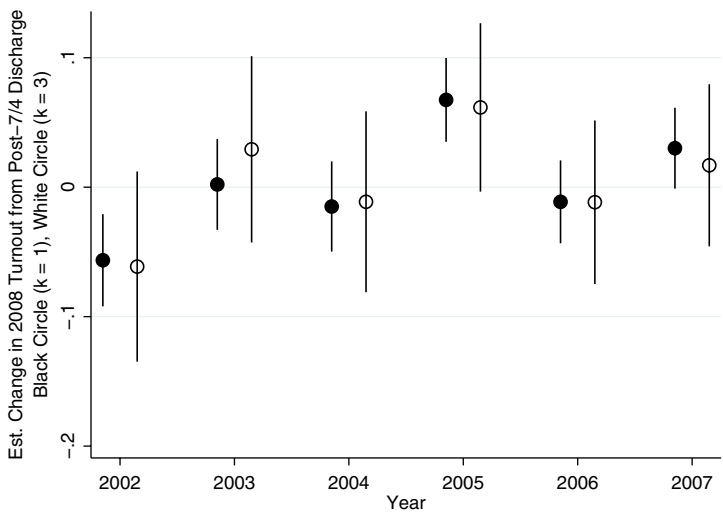

Figure A.5: Estimate of discontinuous change in 2008 turnout from being discharged after July 4th by year.

Table A.6: Date of registration of individuals discharged pre- and post-July 4th in 2005.

	(1)	(2)	(3)	(4)	(5)	(6)
		All discharges			Felony discharges	
% Registered by	Discharged after 7/4 $N = 4{,}163$	Discharged by 7/4 $N = 4{,}429$	Difference	Discharged after 7/4 $N = 2{,}430$	Discharged by 7/4 $N = 2{,}614$	Difference
12/31/2000	0.102	0.100	0.002	0.066	0.057	0.008
	(0.005)	(0.005)	(0.007)	(0.005)	(0.005)	(0.007)
12/31/2004	0.181	0.170	0.011	0.117	0.103	0.014
	(0.006)	(0.006)	(0.008)	(0.006)	(0.006)	(0.009)
12/31/2005	0.200	0.189	0.011	0.137	0.121	0.017
	(0.006)	(0.006)	(0.009)	(0.007)	(0.007)	(0.009)
12/31/2006	0.242	0.217	0.025	0.185	0.150	0.035
	(0.007)	(0.006)	(0.009)	(0.008)	(0.007)	(0.010)
12/31/2007	0.271	0.235	0.036	0.217	0.165	0.052
	(0.007)	(0.007)	(0.009)	(0.008)	(0.008)	(0.011)
12/31/2008	0.352	0.299	0.053	0.305	0.222	0.083
	(0.007)	(0.007)	(0.010)	(0.009)	(0.009)	(0.012)
12/31/2009	0.364	0.313	0.052	0.319	0.235	0.083
	(0.007)	(0.007)	(0.010)	(0.009)	(0.009)	(0.013)
10/22/2010	0.379	0.327	0.052	0.329	0.248	0.081
	(0.007)	(0.007)	(0.010)	(0.009)	(0.009)	(0.013)

Note: Sample includes discharges for the given offense type between January 1, 2005 and December 31, 2005.

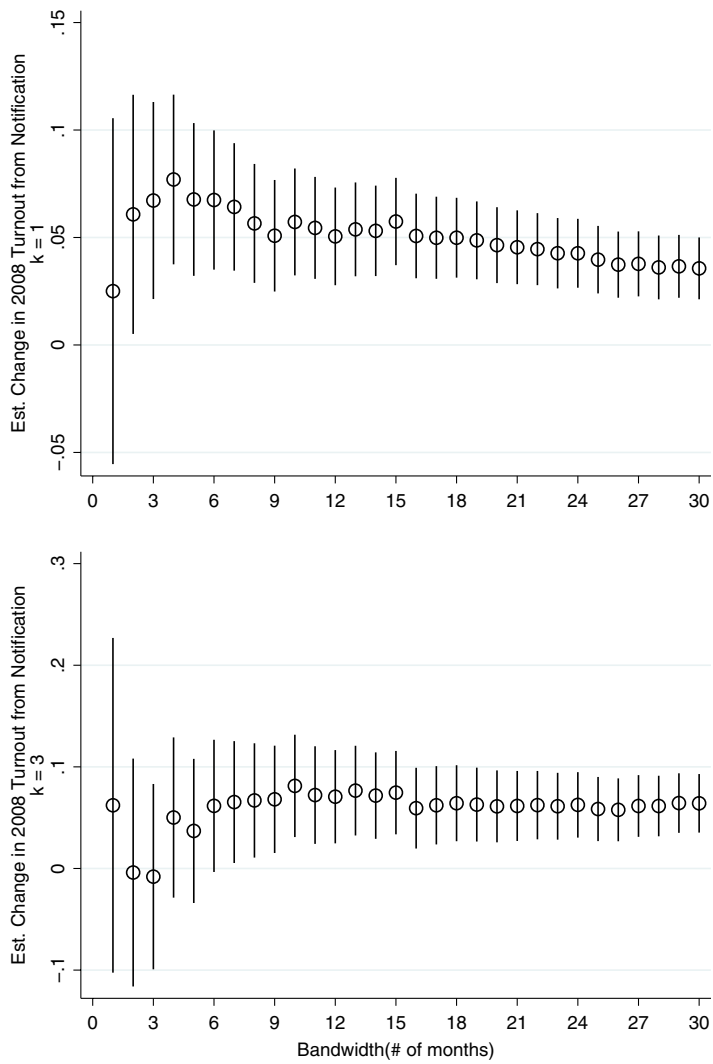

Figure A.6: Estimated effect of notification on 2008 turnout by bandwidth (k) for felony discharges.

Table A.7: Out-of-state political participation by Rhode Island dischargees.

	Registered	2012 Turnout
Connecticut		
Birth date	31	3
Birth date − 35 days	5	2
Birth date + 35 days	9	5
New York		
Birth date	64	16
Birth date − 35 days	21	10
Birth date + 35 days	29	9

Note: N = 6,631 unique individuals discharged in Rhode Island between 1/1/2009 and 12/31/2010.

Table A.8: Correlates of self-report turnout in 2012 current population survey.

	(1)		(2)		(3)	
Black	0.539	(0.028)	0.565	(0.028)	0.545	(0.028)
Hispanic	−0.208	(0.028)	−0.208	(0.028)	−0.203	(0.028)
No HS degree	−1.783	(0.032)	−1.693	(0.032)	−1.785	(0.032)
Only HS degree	−1.152	(0.022)	−1.108	(0.022)	−1.154	(0.022)
Some college	−0.513	(0.023)	−0.485	(0.023)	−0.515	(0.023)
Male	−0.141	(0.016)	−0.173	(0.017)	−0.141	(0.016)
Married	0.384	(0.018)	0.370	(0.018)	0.384	(0.018)
Age 18–19	−0.983	(0.069)	−1.121	(0.070)	−0.984	(0.069)
Age 20–24	−1.030	(0.059)	−1.231	(0.060)	−1.031	(0.059)
Age 25–29	−0.941	(0.059)	−1.177	(0.060)	−0.941	(0.059)
Age 30–34	−0.800	(0.059)	−1.041	(0.060)	−0.801	(0.059)
Age 35–39	−0.664	(0.060)	−0.901	(0.061)	−0.665	(0.060)
Age 40–44	−0.552	(0.059)	−0.790	(0.060)	−0.553	(0.059)
Age 45–49	−0.415	(0.059)	−0.649	(0.060)	−0.416	(0.059)
Age 50–54	−0.275	(0.058)	−0.500	(0.059)	−0.276	(0.058)
Age 55–59	−0.093	(0.059)	−0.300	(0.060)	−0.094	(0.059)
Age 60–64	−0.044	(0.060)	−0.188	(0.060)	−0.044	(0.060)
Age 65–69	0.133	(0.062)	0.063	(0.062)	0.133	(0.062)
Age 70–74	0.248	(0.065)	0.213	(0.065)	0.248	(0.065)
Age 75–79	0.399	(0.070)	0.382	(0.070)	0.398	(0.070)
Age 80–84	0.303	(0.073)	0.295	(0.073)	0.302	(0.073)
Employed			0.356	(0.020)		
Iowa resident					0.404	(0.053)
Maine resident					0.353	(0.054)
Rhode island resident					0.055	(0.057)
Constant	1.449	(0.055)	1.405	0.055	1.444	0.055

Note: All columns report coefficients and standard errors from a logit regression. N = 94,311 and observations are weighted by the sample weight (PWSSWGT). Following Hur and Achen (2013), the dependent variable is coded as one if a respondent reported voting, and zero if a respondent reported not voting, did not know if he or she voted, refused to say if he or she voted, or did not respond to the question. The excluded type is a white, non-Hispanic female who is over the age of 85, not married, and graduated from college.

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