The Voting Rights of Ex-Felons and Election Outcomes in the United States*

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Abstract

Approximately one in forty adult U.S. citizens has lost their right to vote, either temporarily or permanently, as a result of a felony conviction. Because laws restricting voting by felons and ex-felons disproportionately affect minorities, and minorities tend to vote for Democratic candidates, it has been hypothesized that felony disenfranchisement hurts Democratic candidates in elections, thus helping Republican candidates. We test this hypothesis using variation in felony disenfranchisement laws across U.S. states and over time. During the 2000s, a number of states restored the voting rights of ex-felons. Using difference-in-differences regressions, we estimate the effect of laws re-enfranchising ex-felons on the vote shares of major party candidates in elections for seats to the U.S. House of Representatives. We argue that the regression estimates provide an upper bound for the true effect of restoring voting rights to ex-felons on the vote shares of major party candidates. Using this upper bound, no House majority would have been reversed in any year between 1998 and 2012, had all states allowed ex-felons to vote.

Keywords: Voting rights; election law; felony disenfranchisement; U.S. Congress.

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1 Introduction

A felony conviction in the United States usually implies a loss of voting rights. At present, 48 U.S. states and the District of Columbia prohibit voting while incarcerated for a felony offense; 35 states prohibit persons on parole or probation from voting; and twelve states impose voting restrictions on at least some categories of ex-offenders who have completed their sentence. We investigate the hypothesis that excluding felons from the right to vote changes the outcomes of national elections.

This hypothesis is motivated by two observations. First, while felony disenfranchisement laws affect approximately one in forty American adults, they disproportionately restrict voting by racial minorities. For example, approximately one in every thirteen black adults in the U.S. currently cannot vote as the result of a felony conviction, and in three states (Florida, Kentucky, and Virginia) more than one in five black adults is disenfranchised.[1] Second, since the 1970s minorities have voted overwhelmingly for Democratic candidates in national elections. For example, at least 83% of African-Americans voters voted for the Democratic candidate in every presidential election since 1976, while no Democratic presidential candidate was able to attract more than 48% of the white vote in any election during that period.[2] Based on these facts, it may seem reasonable to conjecture that felony voting restrictions hurt Democrats at the polls, and help Republicans.

The problem with this conjecture, however, is that it relies on two unproven assumptions: First, that disenfranchised felons, if given the right to vote, would turn out to vote in large enough numbers to affect election outcomes. Second, that conditional on voting, a felon's decision of who to vote for is similar to the choice made by a non-felon of the same race. In an influential paper, Uggen and Manza (2002) examined whether U.S. national elections between 1978 and 2000 would have produced different winners if all disenfranchised felons had been allowed to vote, under the assumption that the counterfactual turnout and voting decisions of disenfranchised felons would have been the same as the decisions of registered voters with the same socio-demographic characteristics (which were predicted from voter surveys using regression analysis).[3] They estimated that removing felony voting restrictions in the United States would have increased the number of Democrats elected to the U.S. Senate in every election between 1978 and 2000, and that "if disenfranchised felons in Florida had been permitted to vote [in the 2000 presidential election], Democrat Gore would certainly have carried the state, and the election" (p. 792).

A number of authors have since questioned these estimates and the assumptions on which they are based. Miles (2004) compared the turnout rates (estimated from voter surveys) of African-American males—the group most likely to be convicted of a felony—to those of whites

[1] Source: The Sentencing Project (www.sentencingproject.org).

[2] Source: University of Connecticut, Roper Center for Public Opinion Research, www.ropercenter.uconn.edu/polls/us-elections/how-groups-voted.

[3] In other words, turnout and voting behavior was assumed to be uncorrelated with felony status, holding constant an individual's race, age, marital status, etc.

and females, in states that barred ex-felons from voting and in states that did not. He found no statistically significant effect of post-sentence voting restrictions on the turnout rate of black males during the period 1986–2000, suggesting that these restrictions were likely not binding for affected individuals. Haselswerdt (2009) arrived at a similar conclusion, finding that, in a sample of 660 New York ex-felons, only 5 percent voted in the 2004 elections. However, using data from the National Longitudinal Survey of Youth, Hjalmarsson and Lopez (2010) estimated that 26 percent of ever-incarcerated persons voted in the 2004 elections. Similarly, by matching offender records with voter registration files in five states, Burch (2011) estimated that 22% of ex-felons voted in the 2008 elections.[4] Although these estimates differ from one another substantially, they are all well below the turnout rates in the general voting-eligible population,[5] which suggests that the impact of felony voting restrictions on election outcomes might be more limited than the effect computed by Uggen and Manza (2002).

Our analysis follows a more direct strategy to estimate the effect of felony voting restrictions on election outcomes. We utilize a wave of actual policy changes that affected felon voting rights in a number of U.S. states between 1998 and 2012. During this period, several states tightened their voting restrictions, but many others relaxed them. For example, the most severe restriction—a lifelong ban from voting following a felony conviction—was imposed by ten U.S. states at the beginning of our sample. By 2005, this number had fallen to two states, before rising again to three in 2011. The resulting variation in the scope and severity of felony disenfranchisement laws, over time and across jurisdictions, offers an opportunity to estimate the effect of these laws on the outcomes of national elections without having to make assumptions about turnout rates by previously disenfranchised individuals. As most changes in disenfranchisement laws in the 1990s and 2000s concerned the voting rights of ex-felons (as opposed to those in prison, on probation, or on parole), we focus on post-sentence voting restrictions only. Post-sentence restrictions account for approximately four out of five disenfranchised individuals in states that impose them. We estimate the impact of removing these restrictions on the outcomes of elections for seats in the U.S. House of Representative between 1998 and 2012, as well as on voter turnout rates.

We find that allowing ex-felons to vote increases the vote share of Democratic candidates in House elections. When plausibly exogenous controls for the number of candidates and the presence of an incumbent in election races are included in the regressions, the estimated marginal effects range from a 0.17 percentage points increase in Democratic vote share associated with laws that restored the voting rights of some ex-felons, to a 1.56 percentage points increase associated with laws that restored the voting rights of all ex-felons. In addition, we find that the turnout

[4] Burch (2011) also suggests that "turnout among felons … is certainly lower than that of similar individuals with low socioeconomic status from the general population" (p. 701). Furthermore, Burch (2012) argues that, even though black ex-felons who register to vote overwhelmingly register as Democrats, the ex-felon population in several states (including Florida) contains enough whites of low socioeconomic status—a group that has tended to vote Republican in recent elections—for rights restoration to result in a net gain for Republican candidates in these states.

[5] More than 51 percent of the voting-eligible age population voted in every presidential election since 1948, and more than 38 percent voted in every midterm election. (Source: www.electproject.org/national-1789-present.)

rate of black males increases by several percentage points in states that allow their ex-felons to vote, as does the difference between the turnout rates of blacks and whites. The direction of these estimates is consistent with the narrative that felony disenfranchisement laws disproportionately restrict voting by racial minorities, and that removing these restrictions increases the vote share of Democratic candidates. However, none of these estimates are statistically significant. Thus, we cannot reject the null hypothesis that felony disenfranchisement has no effect on either turnout or vote shares.

We then test if the estimated vote share effects, despite not being statistically significant, are, in principle, consistent with previous estimates of the number of disenfranchised ex-felons and the turnout and voting patterns of ex-felons who had their voting rights restored. We show that our regression results imply values for these structural parameters that exceed all but the largest existing estimates, and generally require an implausibly large number of disenfranchised ex-felons, or implausibly high rates at which ex-felons turn out to vote, and vote for Democrats. We emphasize that our dataset includes *every* election race for U.S. House of Representatives that occurred over a 14-year period, and covers *every* change in state felony disenfranchisement laws. However, given the limited number of states that changed their ex-felon voting restrictions, the fact that only 435 congressional elections take place every two years, and that many other factors (some of which we control for) influence election outcomes, any remotely plausible effect of ex-felon voting bans on vote shares is too small to yield regression estimates that are significantly different from zero in the election data. For this reason, our regression estimates should be interpreted as upper bounds on the true size of the effect of felony disenfranchisement laws in House elections between 1998 and 2012.

An effect equal to our largest point estimate would have resulted in Democrats winning between zero and three additional seats in House elections over our study period, had ex-felons been allowed to vote in all states—gains that would not have been sufficient to change the majority from Republican to Democrat in any year in which Republicans held a House majority. Thus, even if felony disenfranchisement affects the turnout rates of minorities and the vote shares of Democratic candidates, its impact is likely too small to affect aggregate political outcomes in the context of elections to the U.S. House of Representatives. When we repeat this exercise using the upper 95% confidence bound of the estimate, the 1998 and 2000 elections would have resulted in a slim Democratic majority in the House of Representatives if all ex-felons had been allowed to vote. However, no majorities since then would have changed. The congressional districts that elected Republicans, but would have elected Democrats in these counterfactual scenarios, are frequently in Kentucky, Virginia, and Florida—the three states that bar the most ex-felons from voting. However, these districts, on average, do not have disproportionately large minority populations—if anything, they are slightly "whiter" than the national average.

Overall, our results reinforce the skepticism regarding the aggregate consequences of felon voting that emerged from previous studies. This conclusion does not mean that the reform of felony disenfranchisement laws is unimportant on the individual level. Voting in elections is a fundamental form of civic participation in democracies and, therefore, a potentially valuable component in the rehabilitation and reintegration process for at least some ex-felons. Our finding that voting rights restoration has few, if any, tangible effects on election outcomes reduces one potential political obstacle from continuing the voting rights reforms we study in this paper.

The remainder of the paper proceeds as follows. In Section 2 we review changes in felony disenfranchisement laws that were enacted in several states between 1998 and 2012. In Section 3 we describe our dataset. In Section 4 we develop our empirical approach, which consists of a basic difference-in-differences regression framework to estimate the effect of felon voting rights on vote shares and turnout rates, and a set of "calibration tests" that we use to assess the plausibility of the regression estimates. In Section 5 we present the results of both. In Section 6 we use our regression results to compute counterfactual election outcomes, had all states allowed all ex-felons to vote. Section 7 concludes. An Appendix contains detailed information about the changes in ex-felon voting rights that occurred in the United States between 2000 and 2011, how we classified the legal regime in each state and year, as well as our sample selection and vote allocation procedure.

2 Felony Disenfranchisement in the United States

The practice of felony disenfranchisement in the United States dates back to the colonial period, but its present-day legal foundation is the Fourteenth Amendment to the U.S. Constitution, passed in 1868. While generally known for its equal protection clause, the Fourteenth Amendment allows states to deny the right to vote to citizens convicted of "participation in rebellion, or other crime." Behrens (2004) and Ziegler (2011) provide comprehensive reviews of the legal and political history of felony disenfranchisement in the United States, to which we briefly return at the end of this section. In the meantime, we focus on recent developments in felon voting rights that occurred from the late 1990s to early 2010s.

State felony disenfranchisement laws differ in many dimensions, including which felons are barred from voting and what options (if any) for voting rights restoration are available to individuals convicted of a felony. Felony voting restrictions can be categorized as those that apply to felons in prison, felons released on probation or parole, and ex-felons who have completed their sentence. Nearly all states prohibit voting by prisoners (as does the District of Columbia). In 1997, only Massachusetts, Maine, Utah, and Vermont allowed the incarcerated to vote. Utah and Massachusetts adopted laws barring prisoners from voting in 1998 and 2000, respectively, leaving Maine and Vermont as the only states that currently allow their prisoners to vote. Thirty-five states currently restrict parolees from voting, and thirty-one states restrict offenders on probation from voting. Changes occurred in 2001, when Connecticut lifted its voting bans for offenders on probation; in 2006, when Rhode Island lifted its voting bans for offenders on probation or parole; and in 2012, when South Dakota instituted a voting ban on offenders on probation.[6]

Most recent changes in felony disenfranchisement laws affected the voting rights of ex-felons. This category can be further subdivided into two types of restrictions. We speak of a *full voting ban* if a state has a general rule excluding ex-felons from voting for life. In some cases, states provide a narrow path for ex-felons to regain their voting rights by petitioning the state's parole board or governor, typically in conjunction with seeking a pardon or executive clemency. However, if this process is unlikely to be successful, or is not utilized by most ex-felons, we continue to classify the legal regime as a full voting ban. On the other hand, a state has a *partial voting ban* if a clearly defined subgroup of ex-felons is eligible to vote when certain conditions are met or become eligible to apply for the restoration of voting rights through a non-discretionary process. The criteria that define the subgroup vary from state to state and may include the nature of the crime, whether the individual is a first-time or repeat offender, and the time passed since completion of the sentence.

At the end of the 1990s, fourteen states had post-sentence voting bans. In ten of these states—Alabama, Delaware, Florida, Iowa, Kentucky, Nebraska, Nevada, New Mexico, Virginia, and Wyoming—the post-sentence restriction was a lifelong voting ban with no possibility of reinstatement of voting rights. Delaware lifted its ban in 2000 and replaced it with a partial ban. In the following year, New Mexico completely removed any post-sentence restrictions. In 2003, Alabama, Nevada, and Wyoming replaced their full bans with partial bans, and in 2004 Florida did the same. In 2005, Iowa eliminated its full post-sentence voting ban, while Nebraska replaced its full ban with a partial ban. In 2007, Maryland eliminated its partial post-sentence ban. Finally, in 2011 Iowa reinstituted a full post-sentence voting ban; however, that ban applied only to newly released convicts, resulting in a de-facto partial ban in 2011 and later. At the end of 2012, twelve states had some post-sentence voting restrictions on their books, but only two of these states—Kentucky and Virginia—barred all convicted felons from voting for life throughout the 1998–2012 period. Table 1 summarizes these changes in post-sentence voting bans and shows the number of federal congressional districts affected by each change. (In the Appendix A, we provide more information about how we classified voting restrictions in each state that changed them.)

The post-sentence category is important not only because it saw the most changes, but also because it affects more individuals than any other category. For example, while approximately 2.5 million individuals were either serving a prison sentence or were released on parole in the U.S. in 2010, more than twice as many individuals (5.2 million) were ex-felons who had completed their

[6] Probation is generally applied to punish persons convicted of lesser crimes and first-time offenders. An individual who is placed on probation does not enter prison unless he offends again or otherwise violates the terms of his probation. An individual who entered prison but is released before the full sentence is served (e.g., for good behavior) is placed on parole. All states that allow voting while on parole also allow voting while on probation. For most states the converse is true as well. The only states that currently allow voting while on probation but not parole are California, Colorado, New York, and Connecticut.

Table 1: Changes in Ex-Felon Voting Rights.

Year	State	Change	Party of governor	State House majority	State Senate majority	Federal congressional districts
2000	Delaware	Full ban → partial ban	D	D	R	1
2001	New Mexico	Full ban → no ban	R	R	D	3
2003	Alabama	Full ban → partial ban	R	D	D	7
2003	Nevada	Full ban → partial ban	R	R	D	3
2003	Wyoming	Full ban → partial ban	D	R	R	1
2004	Florida	Full ban → partial ban	R	R	R	25
2005	Iowa	Full ban → no ban	D	D/R	R	5
2005	Nebraska	Full ban → partial ban	D	–	–	3
2007	Maryland	Partial ban → no ban	D	D	D	8
2011	Iowa	No ban → partial ban	R	D	R	4

Notes: Iowa's Senate was split between Democrats and Republicans in 2005. Nebraska's state legislature is unicameral and non-partisan.

sentence (Shannon *et al.* 2010). In the same year, 45% of the disenfranchised U.S. population were ex-felons, despite the fact that only eleven states had post-sentence voting restrictions in 2010. Within these eleven states ex-felons accounted for 78% of the disenfranchised (Uggen *et al.* 2012). Moreover, assuming that at least some convicted criminals are successfully rehabilitated and reintegrated into society, the group of ex-felons may also be more likely to vote in elections, relative to the other categories of disenfranchised citizens.

States change their felony voting restrictions either through executive order or through legislative action. In Table 1 we also indicate the party affiliation of the state's governor at the time the change was enacted, as well as the party in control of the state Senate and state House or Assembly. Five of the nine changes that lifted previous voting restrictions occurred under Democratic governors, and four under Republican governors. Control of the Senate rested with Democrats in three of these cases, and with Republicans in four cases. Similarly, Democrats controlled the House in four instances, and Republicans in four instances.

There are two main reasons why legislators and governors of both parties have supported the restoration of voting rights. First, the political history of felony voting restrictions in the United States is closely tied to larger questions of civil rights. For example, Behrens (2004, p. 246) notes: "The connection between felon disfranchisement and race is strong. The first wave of changes in felon disfranchisement laws occurred soon after the Civil War, corresponding with the extension of voting rights to minority groups in the Constitution, and much of the discourse of the era evidences the clear and conscious intent to disfranchise minorities in this manner." While this intent may no longer exist today, its consequences outlasted the Reconstruction era. For example, in 1998 (the year our dataset begins), in congressional districts in which all felons, including the incarcerated, were allowed to vote, an average of 3.5% of the population was African-American.

In the same year, this average was 10.5% in districts where prisoners were banned from voting; 13.0% in districts where prisoners and those on probation or parole were banned from voting; and 16.3% in districts where convicted criminals could be banned from voting for life. Given the racially tainted history of felony voting restrictions, and its lasting reminders, policy makers may view the restoration of voting rights as a worthy cause that transcends party politics.

Second, independent of their political affiliation, policy makers increasingly view restoration of voting rights as one in a larger set of measures to reform the criminal justice system, aimed at increasing an offender's chance of rehabilitation and reducing the rate of recidivism (Pérez *et al.* 2015). For example, convicted felons in Florida may not only lose their right to vote, but also many other rights, including the right to obtain and hold state licenses necessary to work in a number of jobs. From the perspective of an affected individual, the loss of such rights can have far more severe, and far more immediate, consequences than the loss of voting rights. Florida's decision to restore the voting rights to certain groups of ex-felons in 2004 was part of an effort to restore a larger set of rights, with the clear objective to help the affected individuals reintegrate into society. The rehabilitation perspective applies, in particular, to ex-felons who have completed their sentence, as well as felons on parole or probation, who are permitted to live in the community during part or all of their sentence. The majority of legal changes affecting individuals in these categories has, in fact, been in the direction of granting greater voting rights.

3 Data

In order to test whether felony disenfranchisement laws take a disproportionate share of votes away from Democratic candidates, as has been hypothesized, we constructed a dataset linking voting rights, election returns, and voter turnout in the United States. In this section we describe our data sources and construction of the main variables used in the analysis.

3.1 Ex-felon voting rights

Based on the summary in Uggen *et al.* (2012) (in particular, Table 1 therein), we reviewed state laws restricting voting by felons and ex-felons. Using the classification criteria discussed in the previous section, we then created two indicator variables that represent post-sentence voting rights in a given state and year. The first variable, $AllowAll_{st}$, equals one if state s in year t had no post-sentence voting restrictions, that is, if it allowed voting by all ex-felons. The second variable, $AllowPartial_{st}$, equals one if state s in year t had a partial post-sentence voting ban, that is, if it allowed voting by some but not all ex-felons. If state s had a full post-sentence voting ban in year t, then $AllowPartial_{st} = AllowAll_{st} = 0$. These variables are the main explanatory variables in our regressions. Appendix A contains more information about our classification for those states that changed their ex-felon voting restrictions between 1998 and 2012.

3.2 Election outcomes and political control variables

Our main outcome variable is constructed from race-level election data covering all 435 voting seats in the U.S. House of Representatives between 1998 to 2012, which we obtained from the Federal Election Commission ("FEC"). As this period covers eight national elections, we have information on $8 \times 435 = 3,480$ election races.[7] For each race, the FEC dataset contains the names of all candidates who ran for office or who received at least one vote as write-in candidates, as well as their incumbency status, party affiliation, and number of votes received in each election. We focus on the general elections, for which approximately 769 million votes are recorded in the FEC data during 1998–2012.

Our goal is to compute the share of these votes that was received by each of the two major political parties. Because the effect of changes in felony voting bans on vote shares may be small (and may affect only the counterfactual outcomes of very close elections), some care must be taken when allocating the votes recorded in the FEC data to parties. In Appendix B, we provide a detailed description of our sample selection and vote allocation procedure. In the end, we were able to allocate approximately 757 million votes to candidates who either had a party affiliation or who ran as independents. Of these votes, 48.1 percent went to Republican candidates and 49.1 percent went to Democratic candidates, with the remainder going to third-party candidates and independents. We then computed the following measure of Democratic vote share for each of the 3,480 individual elections in our sample:

$$DShare_{ist} = \frac{V_{ist}^D}{V_{ist}^D + V_{ist}^R}, \tag{1}$$

where V_{ist}^D and V_{ist}^R are the number of general election votes cast for Democratic and Republican candidates, respectively, in congressional district i in state s in year t. Note that the corresponding vote share for Republican candidates is $1 - DShare_{ist}$; thus, changes in $DShare_{ist}$ reflect shifts in vote share *among the two major parties*.[8] At least one major party candidate ran in every election in our dataset, so (1) is well defined. Furthermore, with one exception, a Democratic candidate won office if and only if $DShare_{ist} > 0.5$.[9]

[7] An election race consists of all elections associated with a given House seat in a given election year, including primary, general, and (if necessary) runoff elections. For 1998–2012, the FEC data also contain 257 races for seats in the U.S. Senate, as well as $4 \times 50 = 200$ state-by-state results for Presidential elections. Due to the small sample sizes for Senate and Presidential elections, we focus on House races only.

[8] We also constructed two additional vote share measures, V_{ist}^D / V_{ist} and V_{ist}^R / V_{ist} where V_{ist} is the number of *all* general election votes in district i in state s in year t (including votes for third-party and independent candidates), and ran our regressions with these outcome variables as well. Because third-party and independent candidates received very few votes relative to candidates of the two major parties, our results did not change in a major way.

[9] The exception was the 2002 election in Louisiana's 5th congressional district. Because Louisiana does not have primary elections, several candidates of the same party are allowed to compete in the same general election. In this case, four Republican candidates won a total of 68 percent of the vote, but none of them a majority. This forced a runoff election between the top two vote getters, Republican candidate Lee Fletcher and Democratic candidate Rodney Alexander, which Alexander won narrowly with 50.28 percent of the vote.

Using the same FEC dataset, we constructed the following control variables for each election race: Two dummy variables indicating if a Democratic (Republican) candidate ran in the general election; two dummy variables indicating if a Democratic (Republican) incumbent ran in the general election; and three count variables indicating the number of all candidates as well as the number of Democratic (Republican) candidates in a race, including candidates who competed in the primary elections. For each state and election year, we also included an indicator for Democratic governorship.

3.3 Voter turnout and demographic control variables

We used data from the Current Population Survey ("CPS") to construct our voter turnout variables, as well as a set of demographic control variables. We collect these variables at the state-election year level, resulting in 400 observations (eight elections between 1998 and 2012 in 50 states).

To construct voter turnout rates, we used the November voter supplement to the CPS. For each election year and state, we obtained aggregate responses to the question of whether surveyed individuals had voted in the same year's election. Each such response is broken down by race (black and white) and gender (male and female), so that we can construct turnout rates separately for several groups: Whites, white males, white females, blacks, black males, and black females. The turnout rate of group g in state s in election year t is computed as follows:

$$Turnout_{st}^{g} = \frac{Y_{st}^{g}}{N_{st}^{g}}, \tag{2}$$

where, for each state s and year t, Y_{st}^{g} is the number of surveyed individuals in group g who said that they voted, and N_{st}^{g} is the number of surveyed individuals in group g who were U.S. citizens of age 18 and older.

As pointed out by Miles (2004), it is common for CPS data to contain no, or very few, responses by African Americans in predominantly white states. To deal with this problem, Miles (2004) excluded 25 states from his analysis. This is not a viable approach in our framework, which relies on a relatively small number of legal changes in some states to identify the effect of felony voting restrictions on election outcomes. (Six of the nine states that changed their ex-felon voting restrictions during our study period are excluded from Miles' analysis.) Instead of removing entire states from our sample, we removed from our turnout analysis only those state-year combinations for which the turnout rate in (2) is either undefined ($N_{st}^{g} = 0$) or exactly one or zero (indicating that N_{st}^{g} is very small).[10]

Finally, we constructed the following state-year demographic control variables from the CPS data, including those used in Miles (2004): Percent African-American; percent population aged

[10] For black males, this is the case for 28 state-year combinations, three of which involve states that changed their laws. For black females, these numbers are 44 and 6. Including observations for which $Turnout_{st}^{g} \in \{0,1\}$ does not qualitatively change our results.

0–17/18–35/36–65/66–90 by gender and race (black, white); percent unemployed by gender and race; average weekly earnings by gender and race (in 1998 dollars); percent high school/some college/bachelor degree.[11]

3.4 Summary statistics

The top part of Table 2 shows summary statistics of our main variables (Democratic vote share and ex-felon voting restrictions) as well as political control variables. For these variables, the unit of observation is a congressional district in a state in a given election year. We report two sets of statistics: One for the full dataset of $3,480$ observations, and one for a restricted sample of $2,175$ observations that cover elections in the years 2002–2010 only.

The reason we examine a restricted sample in addition to the full sample is the reapportionment of congressional districts that occurs after each decennial census. As a result of this reapportionment, both the number of congressional districts in a state, as well as the district boundaries, can change. The process by which district boundaries are redrawn is highly politicized in many states, meaning that changes in district boundaries are not exogenous to voter preferences and election outcomes. This does not affect our analysis unless we include congressional district fixed effects in our regressions to capture unobserved heterogeneity across districts. For such regressions, we remove the 1998, 2000, and 2012 elections from the data and focus on the five elections that took place between 2002 and 2010, during which time the number of congressional districts in each state was constant and district boundaries were fixed.[12]

As discussed above, ex-felons have full voting rights in a majority of congressional districts. Over the full length of our dataset, all ex-felons could vote in 80.2 percent of districts on average; some (but not all) ex-felons could vote in 11.9 percent of districts on average; and all ex-felons were barred from voting in 7.8 percent of districts on average. In the 2002–2010 subsample, these fractions shift to 80.5, 13.2, and 6.3 percent, respectively. These changes are not surprising: Given the trend toward greater voting rights, by excluding two early elections but only one later election from the data, the legal regimes are, on average, more permissive in the restricted sample. Nevertheless, the variation in the voting rights variables is comparable over both time frames.

Our race-level political variables are similarly distributed in the full and restricted sample. Democrats won slightly more than half of the votes cast for major party candidates on average. While a candidate from each major party entered in a majority of races, the fraction of uncontested

[11] We imputed missing values for blacks where necessary. If an age bracket percentage could not be computed for black males or females, we used the corresponding number for the opposite gender. If a black unemployment rate was unavailable, we used the corresponding white unemployment rate multiplied by the average ratio of black to white unemployment in the sample. Similarly, when average weekly earnings for blacks were not available, we used the corresponding white earnings multiplied by the average ratio of black to white earnings.

[12] This rule applies with two exceptions, Texas and Georgia, which both introduced "mid-decade redistricting" in the 2000s. While the number of congressional districts in these states stayed the same during 2002–2010, their boundaries did not. We ran our district fixed effects regressions with and without Texas and Georgia and found similar results.

Table 2: Summary statistics.

	1998–2012 ($N = 3,480$)				2002–2010 ($N = 2,175$)			
	Mean	Std. dev.	Min.	Max.	Mean	Std. dev.	Min.	Max.
Ex-felon voting rights:								
All ex-felons can vote	.802	.399	0	1	.805	.396	0	1
Some ex-felons can vote	.119	.323	0	1	.132	.339	0	1
No ex-felons can vote	.078	.271	0	1	.063	.243	0	1
Election outcomes and political control variables:								
Democratic vote share	.517	.251	0	1	.528	.245	0	1
D candidate running	.931	.253	0	1	.941	.235	0	1
R candidate running	.926	.259	0	1	.928	.259	0	1
D incumbent	.453	.498	0	1	.483	.500	0	1
R incumbent	.449	.487	0	1	.438	.496	0	1
Number of candidates*	4.202	2.476	1	22	4.172	2.532	1	22
Number of *D* candidates*	1.503	1.182	0	15	1.505	1.175	0	15
Number of *R* candidates*	1.686	1.504	0	13	1.706	1.528	0	13
Voter turnout ($N = 400$):								
Black males (4 missing)	.449	.187	0	1				
Black females (18 missing)	.519	.194	0	1				
White males	.554	.092	.343	.774				
White females	.576	.104	.322	.826				

$D =$ Democrat, $R =$ Republican. * including primary elections

races in which only one major party candidate entered is not negligible ($(1-.931)+(1-.926) =$ 14.2 percent in the full sample). Furthermore, an incumbent office holder ran for reelection in a large majority of races ($.453+.449 = 90.2$ percent in the full sample).

The bottom part of Table 2 shows summary statistics of our voter turnout variables. For these variables, the unit of observation is a state in a given election year. Turnout of black voters is on average, lower than that of white voters, and turnout of male voters is lower than that of female voters. As discussed earlier, black turnout rates are noisy due to the small sample sizes for black voters in predominantly white states. For this reason, black turnout rates are missing in some states and years, and are equal to zero or one in others. These observations will be discarded in our analysis.

4 Empirical Approach

We estimate the effects of ex-felon voting rights on Democratic vote share and voter turnout using the fractional response regression model developed by Papke and Wooldridge 1996. Below we describe this model and discuss identification of our key variables. We then introduce a simple "structural" model of felon voting in elections. This model gives rise to a number of calibration tests that we use to assess the plausibility of our reduced form estimates.

4.1 Fractional response regression models

We assume that the general election vote share of the Democratic candidate in congressional district i in state s in year t can be described by the following regression equation:

$$DShare_{ist} \;=\; \Phi\big(\beta_1 AllowAll_{st} + \beta_2 AllowSome_{st} + \gamma X_{ist} + \delta Z_{st} + \alpha_s + \mu_t + m_s\, t\big) \,+\, \varepsilon_{ist}\,, \tag{3}$$

where $\Phi(\cdot)$ is the cumulative standard normal distribution function, X_{ist} is a vector containing our race-level controls, Z_{st} is a vector of state-year level controls (governor's party and demographic characteristics), α_s are state fixed effects, μ_t are election year fixed effects, $m_s\, t$ are state-specific linear time trends, and ε_{ist} is the error term. Congressional district fixed effects can be included in the estimation of (3), by replacing α_s with α_{is}. Similarly, the turnout rate of group g in state s in year t is governed by the regression equation

$$Turnout^g_{st} \;=\; \Phi\big(\beta_1 AllowAll_{st} + \beta_2 AllowSome_{st} + \delta Z_{st} + \alpha_s + \mu_t + m_s\, t\big) \,+\, \varepsilon_{st}\,. \tag{4}$$

(3) and (4) are probit fractional response models and can be estimated via (quasi-)maximum likelihood. Relative to a linear (OLS) model, fractional response models have the advantage that predicted outcomes will always lie between zero and one, the interval over which our dependent variables are defined.[13] The marginal effects associated with the coefficients β_1 and β_2 represent the effects of ex-felon voting rights on the vote shares of Democratic candidates, or on the turnout rate of a given voter group. Specifically, the marginal effect associated with β_1 represents the effect of granting voting rights to all ex-felons, by eliminating a full post-sentence voting ban; and the marginal effect associated with β_2 represents the effect of granting voting rights to some, but not all, ex-felons, by replacing a full post-sentence voting ban with a partial ban.

After controlling for observed heterogeneity through X_{ist} and Z_{st}, and detrending via $m_s\, t$, identification of β_1 and β_2 rests on the following assumptions. First, any remaining systematic unobserved heterogeneity across states (or districts) remains constant over time and can thus be captured by the state (or district) fixed effects. Second, any remaining systematic unobserved heterogeneity over time remains constant across states, so that it can be captured by the election year fixed effects. When these assumptions are satisfied, β_1 and β_2 are identified through difference-in-differences.

[13] Estimates of the corresponding linear models, i.e., where $\Phi(x)$ is replaced with x, are similar to those of the probit fractional response model; however, the latter resulted in tighter confidence bounds in most of our specifications. Estimates and confidence bounds of a logit fractional response model, i.e., where $\Phi(x)$ is replaced with $e^x/(1+e^x)$, are very similar to those of the probit specification.

A potential endogeneity issue arises if a state's decision to change felon voting rights depends on election outcomes or turnout rates in the state. Selection into different legal regimes based solely on the level of turnout or on the level of a party's support in the electorate does not bias the estimates of β_1 and β_2, as such level differences are accounted for by the inclusion of state fixed effects. On the other hand, if selection was based on different *trends* in states' turnout and voting patterns, the estimates for β_1 and β_2 would be biased. For example, if some state's demographic composition was changing in a way that increases support for the Democratic party, and Democratic policy makers systematically adopt more permissive felon voting regimes, estimation of (3) may reveal a correlation between felon voting rights and Democratic vote share which does not represent a causal relationship, or represents a causal relationship in the reverse direction.

To address this issue, we included in our regression equation population characteristics and the party of the governor in a given state and election year (in Z_{st}), as well as state-specific linear time trends ($m_s t$). More importantly, to verify that self-selection effects did not bias our results, we examined the history of each of the relevant state laws (see Appendix A). Changes in state voting laws generally have been in the direction of granting greater voting rights. Laws granting greater voting rights to felons and ex-felons have been passed in traditionally "blue" and "red" states; have been passed by both Democratic and Republican legislatures; and have been signed by both Democratic and Republican governors. The histories of reenfranchising laws reveal debates regarding the importance of protecting equal rights of all citizens versus ethical concerns that some individuals should have their voting rights restricted or removed because of their crimes. Notably absent from the public debates have been expressions of concerns that changes in voting laws will benefit one party or hurt another.[14] We interpret these facts as indicating a relatively non-partisan effort over the past two decades to increase the enfranchisement of felons and, in particular, ex-felons. Overall, we see no indication that turnout or voting patterns, or changes in turnout or voting patterns, played a decisive role in any state's decision to change its felony disenfranchisement laws.

4.2 Calibration tests

Provided the vote share model (3) can be estimated without bias, we can use the coefficient estimates from this regression to investigate certain underlying structural characteristics of elections. For example, we can ask the following question: Assuming X percent of disenfranchised ex-felons had their rights restored, at what rate would they have to had turned out to vote, and how would they have to had voted, in order to have generated the changes in vote shares estimated in the regression model? Similarly, we can ask: Assuming Y percent of ex-felons vote if eligible, and vote for a given political party, how many ex-felons would need to have had their voting rights

[14]The only exception we found was a statement by an Alabama Republican party official that his party opposed the restoration of ex-felon voting rights because "felons don't tend to vote Republican." (Source: S24.)

restored in order to produce the estimated effects? We may then assess whether these implied values are realistic or plausible.

We now develop a simple model of ex-felon voting in elections that enables us to perform such calibrations. Because the structural parameters are non-linear in the reduced-form estimates, we do not suggest this method as an alternative to more direct approaches of estimating felon voting behavior or the percentage of disenfranchised individuals. However, the calibrations serve as a simple and useful plausibility check for our regression results, and they allow us to connect our estimates of vote share responses to recent research on felon turnout rates.

Let f be the population of ex-felons in a state that disenfranchises all ex-felons. Let τ_{nf} be the turnout rate among non-felons, and let τ_f be the turnout rate among ex-felons, if they are allowed to vote. Let p_{nf} be the propensity of non-felons to vote for a Democratic candidate instead of a Republican (we ignore other parties here), and let p_f be the same propensity for ex-felons. In a state that does not allow its ex-felons to vote, Democratic vote share is $DShare^{\text{full ban}} = p_{nf}$. If the state eliminates its full voting ban, Democratic vote share becomes

$$DShare^{\text{no ban}} \;=\; \frac{(1-f)\cdot\tau_{nf}\,p_{nf} + f\cdot\tau_f\,p_f}{(1-f)\cdot\tau_{nf} + f\cdot\tau_f}.$$

Thus, lifting the ban increases Democratic vote share by

$$\Delta_1 \;\equiv\; DShare^{\text{no ban}} - DShare^{\text{full ban}} \;=\; \frac{frd}{1-f(1-r)}, \tag{5}$$

where $r = \tau_f/\tau_{nf}$ and $d = p_f - p_{nf}$. If, instead, the state replaces its full voting ban with a partial ban, and a fraction λ of ex-felons are eligible to vote under the partial ban, Democratic vote share becomes

$$DShare^{\text{partial ban}} \;=\; \frac{(1-f)\cdot\tau_{nf}\,p_{nf} + \lambda f\cdot\tau_f\,p_f}{(1-f)\cdot\tau_{nf} + \lambda f\cdot\tau_f}$$

and the increase in vote share of Democratic candidates is

$$\Delta_2 \;\equiv\; DShare^{\text{partial ban}} - DShare^{\text{full ban}} \;=\; \frac{\lambda frd}{1-f(1-\lambda r)}. \tag{6}$$

Note that $d > 0$ and $\lambda \in (0,1)$ implies $\Delta_1 > \Delta_2 > 0$.

Taking the marginal effects associated with the regression estimates for β_1 and β_2 as values for Δ_1 and Δ_2, we can solve (5)–(6) for any two of the four parameters f, λ, r, and d. For example, if we know (or have estimates of) the policy parameters f and λ, we can solve for the behavioral parameters as follows:

$$r \;=\; \frac{1-f}{\lambda f}\cdot\frac{\beta_2^m - \lambda\beta_1^m}{\beta_1^m - \beta_2^m}, \quad d \;=\; \beta_1^m\beta_2^m\cdot\frac{1-\lambda}{\beta_2^m - \lambda\beta_1^m},$$

where β_1^m and β_2^m are the marginal effects associated with β_1 and β_2.

Without full knowledge of λ, we can still put bounds on these variables. Since $p_f \leq 1$ the parameter d cannot be larger than $1 - p_{nf}$, and it is straightforward to show that this implies $\lambda \leq \beta_2^m(1 - p_{nf} - \beta_1^m)/\beta_1^m(1 - p_{nf} - \beta_2^m)$. Furthermore, it is highly implausible that $\tau_f > \tau_{nf}$, that is, that ex-felons have *higher* turnout rates than non-felons. Hence, we should assume that $r \leq 1$, and this is the case if and only if $\lambda \geq (1 - f)\beta_2^m/(\beta_1^m - f\beta_2^m)$. Thus, given f and p_{nf} the sensible range for λ is

$$\frac{\beta_2^m - f\beta_2^m}{\beta_1^m - f\beta_2^m} \;\leq\; \lambda \;\leq\; \frac{\beta_2^m}{\beta_1^m}\frac{1 - p_{nf} - \beta_1^m}{1 - p_{nf} - \beta_2^m}. \tag{7}$$

This range is narrow. As an illustration, suppose we estimated $\beta_1^m = 0.015$ and $\beta_2^m = 0.01$. Then, assuming that 7.5 percent of the voting-age population were disenfranchised in states that had full bans before they changed their laws ($f = 0.075$, which is within the range reported in Uggen *et al.* 2012), and that $p_{nf} = 0.5$, the interval of possible values for λ is $[0.6491, 0.6599]$.[15] By setting λ equal to the lower end of this range we get a lower bound on d equal to 0.2, and by setting λ equal to the upper end we get a lower bound on r equal to 0.3814. Thus, to produce Democratic vote share gains as measured by the estimated coefficients $\beta_1^m = 0.015$ and $\beta_2^m = 0.01$, the propensity to vote for Democrats must be *at least* 20 percentage points higher among ex-felons than among non-felons, and this lower estimate applies under the assumption that ex-felons turn out to vote at *exactly* the same rate as non-felons. Similarly, the turnout rate of ex-felons must be *at least* 38.1 percent of the turnout rate of non-felons to produce the estimated effects, and this estimate applies under the assumptions that *all* ex-felons vote for Democrats.[16]

Finally, we can also go the other way around. That is, we can make assumptions about the values of the behavioral parameters r and d—by taking estimates from existing studies, for example—and compute the implied policy parameters

$$f \;=\; \frac{\beta_1^m}{rd + \beta_1^m(1 - r)}, \quad \lambda \;=\; \frac{\beta_2^m}{\beta_1^m}\frac{d - \beta_1^m}{d - \beta_2^m}$$

that are consistent with a given $(\beta_1^m, \hat{\beta}_2)$-pair, under these assumptions. For example, suppose that ex-felons are half as likely to vote compared to non-felons ($r = 0.5$), and that 85 percent of ex-felons vote for Democrats if they vote (this propensity would be consistent with the voting behavior of African-American voters in presidential elections). If 50 percent of non-felons vote for Democrats, we get $d = 0.35$. Under these assumptions, the estimates of $\beta_1^m = 0.015$ and $\beta_2^m = 0.01$ imply $f = 0.082$ and $\lambda = 0.657$. In other words, 8.2 percent of individuals must be disenfranchised in states with full voting bans for these estimates to be consistent with the

[15] This means that, in states with 7.5 percent ex-felons, a typical partial voting ban should disenfranchise roughly one-third of the individuals in this group in order to be consistent with the given estimates. If accurate corrections statistics are available, it is theoretically possible to verify whether partial voting bans in the states are consistent with this, or any other, range of λ-values.

[16] In order to calibrate both r and d, we need to fix a particular value within the range of possible λ-values. For instance, by setting λ to the midrange of the interval $[0.6491, 0.6599]$ (i.e., $\lambda = .6545$), then under the same assumptions as above we obtain $r = .6882$ and $d = .2838$.

behavioral assumptions. Again in this case, the calibrated value of $f = 0.082$ is rather large but still within the range of values reported in Uggen *et al.* (2012).

5 Results

We now present our results. Estimates for both the vote share and voter turnout regression models are described in Section 5.1. We then assess the plausibility of these estimates in Section 5.2, using the calibrations developed in the previous section.

5.1 Vote share and voter turnout

Table 3 contains the estimates for the vote share regression model (3). The table has six columns, divided into two sets of three. The first set (columns 1–3) contains results based on regressions that include state fixed effects but not district fixed effects, using the full 1998–2012 time frame. The second set (columns 4–6) shows results based on regressions that include district fixed effects, using the 2002–2010 election years only. Within each set, the left column does not contain any race-level controls, and the center column contains the full set of race-level controls. The right column contains results for estimations restricted only to those elections in which a candidate from each major party was running. All reported estimates are average marginal effects.

Let us look at columns 1 and 4 first. Using the full 1998-2012 period, in states that replaced a full post-sentence ban with a partial ban, and thus allowed some ex-felons to vote, Democratic candidates saw a statistically significant increase in general election vote share of 4.1 percentage points, relative to Republican candidates ($\beta_2^m = .0410$). When district fixed effects are added and the sample is restricted to 2002–2010, the effect size increases to 6.49. However, the β_1-estimates are not statistically significant in either column 1 or column 4. Moreover, β_1^m is smaller in magnitude than β_2^m in column 1, and of the opposite sign in column 4. This appears inconsistent with the structural arguments developed in the previous section—if allowing some, but not all, ex-felons to vote increases Democratic vote share, then one should expect that allowing all ex-felons to vote has at least the same effect.

Our race-level control variables are included in columns 2 and 5. The presence of an incumbent, the number of candidates, and whether or not at least one candidate from each major party entered the race, are highly significant predictors of Democratic vote share. Moreover, the effects of ex-felon voting rights is now structurally consistent (i.e., $\beta_1^m > \beta_2^m > 0$): Allowing some ex-felons to vote increases Democratic vote share between 0.17 and 1.03 percent, and allowing all ex-felons to vote increases Democratic vote share between 1.14 and 1.36 percent. However, these effects are not statistically significant.

While changes in felony disenfranchisement laws could, theoretically, affect the decisions of candidates to enter election races or the decisions of incumbents to seek reelection, we believe that a causal effect in this direction is highly improbable. Elections for seats in the U.S. House of

Table 3: Effects of ex-felon voting rights on Democratic vote share in U.S. House elections.

	1998–2012			2002–2010		
	(1)	(2)	(3)	(4)	(5)	(6)
AllowAll (β_1^m)	.0229 (.0294)	.0114 (.0239)	.0133 (.0278)	−.0148 (.0278)	.0136 (.0263)	.0156 (.0303)
AllowSome (β_2^m)	.0410** (.0167)	.0017 (.0132)	.0020 (.0153)	.0649** (.0261)	.0103 (.0104)	.0119 (.0120)
D incumbent		.1440*** (.0064)	.1677*** (.0075)		.0551*** (.0095)	.0634*** (.0109)
R incumbent		−.1302*** (.0077)	−.1516*** (.0090)		−.0602*** (.0059)	−.0693*** (.0068)
# of candidates		.0045** (.0022)	.0053** (.0025)		−.0016 (.0014)	−.0018 (.0016)
# of *D* candidates		.0113*** (.0029)	.0132*** (.0033)		.0029 (.0020)	.0033 (.0023)
# of *R* candidates		−.0204*** (.0036)	−.0237*** (.0042)		−.0029* (.0017)	−.0034* (.0020)
D running		.4860*** (.0015)			.4860*** (.0010)	
R running		−.4591*** (.0013)			−.4576*** (.0012)	
Congressional district FEs	No	No	No	Yes	Yes	Yes
Contested races only	No	No	Yes	No	No	Yes
Observations	3,480	3,480	2,988	2,175	2,175	1,890
Pseudo-R^2	.0338	.2040	.0728	.1618	.2108	.0923

Notes: Probit fractional response model, average marginal effects reported. All regressions include election year fixed effects, state fixed effects, state-specific linear time trends, state-year demographic controls, and a control for Democratic governorship. Numbers in parantheses are robust standard errors adjusted for clustering on states. Stars denote statistical significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Representatives are high-profile affairs, and the payoff associated with winning a House seat is considerable. For example, Diermeier *et al.* (2005) estimate the monetary value of winning a seat in the House of Representatives to be more than $616,000 in 1995 dollars (which equates to $790,000 in 2005 dollars, and to $960,000 in 2015 dollars). It is very unlikely that a candidate's decision to compete for a prize of this magnitude would depend on whether ex-felons are permitted to vote in the candidate's state. Thus, any correlation between voting rights and our race-level control variables is unlikely to indicate a causal effect.

If a candidate from a major party is not running in a race, then that candidate's party must necessarily have a zero vote share, and this is the case in 492 elections in our dataset. In the vast majority (98 percent) of these uncontested elections, the unopposed candidate was an incumbent. The decision to not challenge current office holders may reflect a general incumbency advantage that has been documented in the literature (e.g., Abramowitz *et al.* 2006), or it could simply indicate that a number of congressional districts are very "safe" districts for one of the two major parties. Regardless of the reason why some elections are uncontested, in such elections our *DShare*-variable is either zero or one and hence cannot respond to changes in any other variable, including changes in felony disenfranchisement laws. It is, therefore, not surprising that the most important vote share predictor in columns 2 and 5 is the pair of variables indicating whether a candidate from each of the major parties was actually in the race ("*D* running" and "*R* running"). As a robustness check, we also estimated the model without the uncontested elections in the sample. The results of these regressions are reported columns 3 and 6, and are similar to those in columns 2 and 5. While β_1^m and β_2^m have increased slightly, these estimates are still not statistically significant at conventional levels.

Let us now turn to our turnout regression model (4), for which the main parameter estimates are reported in Table 4. Because our outcome variables are available at the state-year level only, none of the regressions include race-level controls. Moreover, as explained in Section 3.3 we excluded all observations for which the dependent variable was exactly zero or one.

Table 4: Effects of ex-felon voting rights on voter turnout.

	Probit fractional response model						OLS	
	B	BM	BF	W	WM	WF	B−W	BM−WF
AllowAll (β_1^m)	.0607	.0624	.0610	−.0225	−.0221	−.0232	.0801	.0847
	(.0997)	(.1191)	(.0726)	(.0189)	(.0200)	(.0218)	(.1307)	(.1624)
AllowSome (β_2^m)	.0442	.0153	.0946**	.0030	.0074	−.0013	.0432	.0217
	(.0452)	(.0548)	(.0448)	(.0191)	(.0174)	(.0220)	(.0547)	(.0677)
Observations	384	372	356	400	400	.400	384	372
(Pseudo-)R^2	.0460	.0455	.0493	.0264	.0229	.0305	.3360	.2911

Abbreviations: B = black; W = white; M = male; F = female.

Notes: Average marginal effects are reported for fractional response estimates. All regressions include election year fixed effects, state fixed effects, state-specific linear time trends, state-year demographic controls, and a control for Demographic governorship. Numbers in parantheses are robust standard errors adjusted for clustering on states. Stars denote statistical significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

The first six columns in Table 4 contain the average marginal effects of our *Allow*-variables on the turnout rate of blacks, black males, black females, whites, white males, and white females, respectively. Allowing some ex-felons to vote increases the turnout of blacks by 4.42 percentage points, and allowing all ex-felons to vote increases the turnout of blacks by 6.07 percentage points. For black males only, these numbers are 1.53 and 6.24 percentage points, respectively. These estimates are consistent with the hypothesis that ex-felon voting bans impose binding constraints on the turnout of at least some black U.S. citizens. However, none are statistically significant. The effects of voting rights on black female turnout, while partly significant, are not structurally consistent ($\beta_1^m < \beta_2^m$). For whites, the estimates are neither significant nor structurally consistent.

We also performed a triple-difference analysis, analogous to the one in Miles (2004), by using the difference in the turnout rates of two populations as the dependent variable. These results are reported in the final two columns of Table 4. Because the dependent variable can take on negative values, we used a linear (OLS) model for these regressions. The second-to-last column in Table 4 shows the effects of ex-felon voting rights on the difference in the turnout rate of blacks and whites. Granting ex-felons full voting rights increases this difference by 8.01 percentage points, while partial rights increase it by 4.32 percentage points. The final column compares the turnout rates of black males to that of white females, the groups most likely and least likely to be convicted of a felony. Allowing all (some) ex-felons to vote increases the difference in the turnout rates of these two groups by 8.47 (2.17) percentage points. These triple-difference results are consistent with the hypothesis that ex-felon voting bans disproportionately restrict voting by blacks, and by black males. Once again, however, none of the estimated effects are statistically significant.

5.2 Plausibility checks

Despite not being statistically significant, the estimated voting rights effects in columns 2, 3, 5, and 6 of Table 3 are of the expected sign and such that $\beta_1^m > \beta_2^m$. Hence, they are consistent with the structural framework developed in Section 4.2, which makes it possible to apply our calibration tests developed therein to assess whether the measured effect size is, in principle, reasonable. Table 5 contains four such tests.

In panel A, we consider two scenarios regarding ex-felon voting behavior. In the low behavior scenario, ex-felon turnout is 35 percent of non-felon turnout and ex-felons are 12 percentage points more likely to vote for Democrats than non-felons. These assumptions are consistent with the turnout estimate and the voter registration patterns discussed in Burch (2011).[17] In the high behavior scenario, we assume that ex-felon turnout is 45 percent of non-felon turnout, which is derived from the estimate in Hjalmarsson and Lopez (2010).[18] Furthermore, we doubled Burch's (2001) number for d and now assume that ex-felons are 24 percentage points more likely to vote for Democrats than non-felons. We then compute, for each scenario, the implied values for f (the fraction of disenfranchised ex-felons) and λ (the fraction of ex-felons who can vote under a partial ban) associated with the estimates in columns 2, 3, 5, and 6 of Table 3.

[17] Burch (2011) estimated that 22 percent of ex-felons voted in the 2008 presidential election. 62 percent of the voting-eligible population voted in 2008 (see www.electproject.org/national-1789-present). Therefore, we set $r = .22/.62 = 0.35$. Burch (2011) also found that 56 (23) percent of ex-felons who were registered to vote in North Carolina in 2008 were registered as Democrats (Republicans). In the same year, 46 (32) percent of all registered voters in North Carolina were registered as Democrats (Republicans) (see vt.ncsbe.gov/RegStat). Therefore, we set $d = .56/(.56+.23) - .46/(.46+.32) = 0.12$.

[18] Hjalmarsson and Lopez (2010) estimated that 26 percent of ex-felons voted in the 2004 presidential election, which is the highest among the existing estimates of felon turnout. 60 percent of the voting-eligible population voted in 2004 (see www.electproject.org/national-1789-present), making $r = 0.45$ a slightly generous assumption.

Table 5: Structural parameters implied by regression results.

A. Implied disenfranchised population, assuming turnout and voting behavior

	Low behavior scenario: Burch (2011) $r = 0.35,\ d = 0.12$				**High behavior scenario:** Hjalmarsson and Lopez (2010) $r = 0.45,\ d = 0.24$			
	(2)	(3)	(5)	(6)	(2)	(3)	(5)	(6)
$f =$	.2307	.2626	.2675	.2992	.0998	.1153	.1178	.1338
$\lambda =$	.1369	.1360	.7346	.7367	.1431	.1432	.7465	.7504

B. Implied turnout and voting behavior, assuming disenfranchised population

	Low population scenario: Uggen *et al.* (2012) $f = 0.06,\ p_{nf} = 0.4$				**High population scenario:** "Florida" $f = 0.10,\ p_{nf} = 0.45$			
	(2)	(3)	(5)	(6)	(2)	(3)	(5)	(6)
$r =$	.5415	.5759	.5848	.6207	.4606	.4828	.4947	.5176
$d =$	.3412	.3751	.3780	.4094	.2341	.2612	.2610	.2869
$\lambda =$	.1449	.1458	.7506	.7557	.1429	.1438	.7474	.7526

Notes: In both population scenarios, the value for λ is set at the 65th percentile of the range in (7).

The values for f implied by our regression estimates in the low behavior scenario are clearly unrealistic, as between 23 and 30 percent of the population would have to be disenfranchised in order for rights restoration to produce the estimated gains in Democratic vote share. The high behavior scenario begins to produce more reasonable values for f, ranging from 10 to 13 percent. The smallest of these implied values is within the range of estimates reported in Uggen *et al.* (2012) and not inconceivable, considering that the state of Florida, which still disenfranchises the most ex-felons at an estimated rate of more than 10 percent of the population, contributed over 40 percent of the variation in felony voting laws in our dataset (25 of 60 observations in which felony voting rights changed relative to the previous election year are Florida elections). Moreover, the lower two of the implied values for λ—indicating that approximately 14 percent of ex-felons can vote under a partial ban—is roughly consistent with the estimates reported in Uggen *et al.* (2012).[19] However, the behavioral assumptions underlying this scenario are rather optimistic.

Panel B contains two scenarios regarding the size of the ex-felon population. The low population scenario assumes that six percent of the population in states with full ex-felon voting bans

[19] Table 2 in Uggen *et al.* (2012) contains estimates of the percentage of ex-felons who had their voting rights restored in states with partial ex-felon voting bans. The population-weighted average of these estimates is 16.8 percent.

consists of disenfranchised ex-felons, and that non-felons vote for Democrats with a probability of 40 percent. The first number ($f = 0.06$) is derived from Uggen *et al.*'s (2012) estimates of the size of the ex-felon population in Kentucky and Virginia, states that still have full ex-felon voting bans.[20] The second number ($p_{nf} = 0.4$) reflects the average vote share of Democratic candidates in our dataset, in states that had, or continue to have, full ex-felon voting bans. The high population scenario assumes a 10 percent population share of disenfranchised ex-felons, and that non-felons vote for Democrats with a probability of 45 percent. These values are reflective of Florida, the state with the largest fraction of disenfranchised ex-felons.[21]

In the low population scenario, our regression estimates imply that ex-felons are at lest 54 percent as likely to vote than non-felons *and* that ex-felons are at least 34 percentage points more likely to vote for Democrats than non-felons. These values exceed all presently known estimates of ex-felon turnout and voting behavior. We get more realistic numbers in the high population ("Florida") scenario: For our estimates to be consistent with this scenario, ex-felon turnout needs to be at least 46 percent of the rate of non-felons, and ex-felons need to be at least 23 percentage points more likely to vote for Democrats than non-felons. The implied values for ex-felon turnout approach those found by Hjalmarsson and Lopez (2010); moreover, the smaller of the implied λ-values (of approximately 14 percent) is close to the estimate in Uggen *et al.* (2012). However, the implied value for ex-felons' propensity to vote Democratic is still very large, and the assumption that Florida's disenfranchisement rate is representative of that in other states is clearly unrealistic.

Where does this leave us? Recall that, for a regression estimate to reach a certain threshold of statistical significance, its standard error must be sufficiently small or its magnitude must be sufficiently large. In our case, the estimates of β_1 and β_2 are not significant (at the 10% level) in the regressions with race-level control variables. We cannot realistically hope to obtain smaller standard errors by measuring election outcomes more precisely or by using more data—we already used *every* House election that took place over a 14-year period covering *every* recent change in state felony disenfranchisement laws. At the same time, the disenfranchisement rates, turnout rates, and felon voting behavior implied by our estimated effects are scarcely consistent with existing estimates of these structural parameters. Thus, any statistically significant estimate of β_1 and β_2 obtained from our dataset would have been likely to produce even less plausible values. The true effect of felony disenfranchisement on vote shares—if one exists—should, therefore, be no larger than our regression estimates.

[20] Table 2 in Uggen *et al.* (2012) contains estimates of the number of disenfranchised ex-felons in 2010. For Kentucky and Virginia, these numbers represent 5.6 percent of the two states' voting age population in the 2010 Census, which we round to 6 percent to account for the presence of non-U.S. citizens in each state.

[21] In both scenarios, we assume that λ is equal the 65th percentile of the range identified in (7), as this yielded the most plausible implied values in the high population scenario.

6 Counterfactuals and District Analysis

Taking our regression estimates as upper bounds, we can compute how many elections Democratic candidates would have won in a given year, under the counterfactual hypothesis that all states allowed all ex-felons to vote. That is, we can compute a counterfactual vote share of the Democratic candidate as follows:

$$DShare_{ist}^{\text{No ban}} \;=\; \Phi\big(\hat{\beta}_1 + \hat{\gamma}X_{ist} + \hat{\delta}Z_{st} + \hat{\alpha}_s + \hat{\mu}_t + \hat{m}_s\, t\big) \;+\; \hat{\varepsilon}_{ist}, \tag{8}$$

where $\hat{\beta}_1$ is the point estimate of the coefficient of the *AllowAll*-variable in the fractional response model (3). Likewise, $\hat{\gamma}, \hat{\delta}, \ldots$ denote the estimated values for the other parameters in (3), and $\hat{\varepsilon}_{ist}$ are the regression residuals. For model specifications that include district fixed effects, we can compute similar counterfactual vote shares, by replacing $\hat{\alpha}_s$ with $\hat{\alpha}_{is}$ in (8). We then calculate the additional number of seats Democrats would have won in election year t under the counterfactual regime, by calculating the difference

$$\sum_{is} \mathbb{I}\big(DShare_{ist}^{\text{No ban}} > 0.5\big) \;-\; \sum_{is} \mathbb{I}\big(DShare_{ist} > 0.5\big).$$

In Table 6, the two columns labeled "Counterfactual D gains: point estimates" contain the number of seats Democrats would have gained based on the results in specifications 3 and 6 in Table 3, that is, using the *largest* of our regression estimates with and without district fixed effects. Because the latter excludes the elections in 1998, 2000, and 2012, counterfactual vote shares based this specification can be obtained only for 2002–2010.[22] In years in which Democrats did not win a majority in the House of Representatives, we compare these counterfactual gains to the number of additional seats Democrats would have had to have won in order to be the majority party. This threshold is based on an adjusted seat distribution, after allocating Independent Representatives to the major parties (see Appendix B for details).

Similar to Uggen and Manza's (2002) calculations for the U.S. Senate, our counterfactual analysis shows that Democrats would have won additional seats in the House of Representatives in five of eight elections years, had ex-felons been allowed to vote in every state. However, Democrats would not have won enough additional seats to change a Republican majority into a Democratic majority in any year in which Republicans held a House majority. Even in the 2000 election, in which Republicans secured a narrow nine-seat majority, Democrats would have only won two additional seats had all states allowed all ex-felons to vote—three short of the gains required for majority. In no election would Democrats have won more than three additional seats, had all states allowed ex-felons to vote.

[22]Specification 6 also excludes all uncontested elections from the regression; however, this does not pose a problem. Provided that changes in ex-felon voting rights do not affect candidates' decisions to run for office, the counterfactual Democratic vote-share in these excluded elections must be the same as the actual vote share, i.e., either zero or one.

Table 6: Counterfactual election outcomes if all states allowed ex-felon voting.

Election year	Congress	Seat distribution	Adjusted seat distribution	Needed for D majority	Counterfactual D gains: point est.		Counterfactual D gains: upper 95%	
		D - R - I	D - R		(3)	(6)	(3)	(6)
1998	106th	211 - **223** - 1	212 - **223**	+6	+0		+8	
2000	107th	212 - **221** - 2	213 - **222**	+5	+2		+8	
2002	108th	205 - **229** - 1	206 - **229**	+12	+2	+3	+7	+8
2004	109th	202 - **232** - 1	203 - **232**	+15	+0	+0	+4	+4
2006	110th	**233** - 202 - 0	**233** - 202		+3	+3	+9	+10
2008	111th	**257** - 178 - 0	**257** - 178		+0	+0	+3	+5
2010	112th	193 - **242** - 0	193 - **242**	+25	+2	+1	+7	+9
2012	113th	201 - **234** - 0	201 - **234**	+17	+1		+8	

Notes: Seat distribution is at beginning of each Congress. Adjusted seat distribution is obtained by allocating Independent Representatives to one of the two major parties (see Footnote 27 and Footnote 28). 218 seats needed for majority; majority party in bold.

Since our estimates are not significantly different from zero, we repeated this counterfactual analysis using the upper 95%-confidence bound of $\hat{\beta}_1$ and $\hat{\beta}_2$ for the size of the voting rights effects. At this bound, the average marginal effects of our *Allow*-variables range from $\beta_2^m = 0.321$ in specification 2 of Table 3 to $\beta_1^m = 0.0751$ in specification 6—values that far exceed those that are even remotely plausible under our calibration tests. In this case, Democrats would have won between four and ten additional seats, and control of the U.S. House of Representatives would have switched from a narrow Republican to an even narrower Democratic majority in the 1998 and 2000 elections. In no election since then would a Republican majority have been overturned, had all ex-felons been allowed to vote in all states.

We also computed an alternative counterfactual, assuming that states that reformed their ex-felon disenfranchisement laws had *not* undertaken these reforms, and voting rights had remained stable and equal to what they were in 1998. Republicans would have won zero or one additional seat in each election since 2006, using our point estimates; and between one and five additional seats, using the upper 95% bound. No elections before 2006 would have been affected, and in neither of the two elections in which Republicans failed to win a House majority (i.e., 2006 and 2008) would this outcome have been different had states not reformed their felony disenfranchisement laws. Overall, we conclude that felony disenfranchisement—even if it has the effect we estimated, which we know is likely too large—has little to no impact on aggregate political outcomes.

Finally, for each election year between 1998 and 2012, Figure 1 shows which districts would have switched from Republican to Democrat if all ex-felons had been allowed to vote, based on our regression specifications 3 and 6 and assuming an effect size equal to the upper 95%-confidence bound. Districts marked with an asterisk are those that would have switched using

Figure 1: Congressional districts that would have switched from Republican to Democrat if all ex-felons had been allowed to vote.

	1998	2000	2002		2004		2006		2008		2010		2012
	(3)	(3)	(3)	(6)	(3)	(6)	(3)	(6)	(3)	(6)	(3)	(6)	(3)
AL	4		3*	3*						3	2*	2	
AZ	5		1	1				1			1	1	
FL		8* 12 22*	5* 13	5* 13			8 13*	8 13*	25	25		12 22	2 10 16
IA	1 2	2	 2 4	1 2 4	1	1							
KY	1 3 6	3	3	3*	4	4	2 4	2 4	2	2			6
MD		8											
MS											4	4	
NE					1	1			2	2			2*
NM	1	1											
NV							2 3*	2 3			3*	3*	
VA		2			2	2	2 11	2* 11		1	2 5 9	2 5 9	2 4 5
WY							a/l*	a/l*					

Full voting ban　Partial voting ban　No voting ban

Notes: Congressional districts listed would have switched from Republican to Democrat, assuming voting rights effects equal to the upper 95% confidence bounds of the regressions in column 3 and column 6 of Table 3. An asterisk (*) indicates that the district would have switched if the voting rights effects were equal to our point estimates. “a/l” = “at large district.”

our point estimates. Note that, because of redistricting, a district label may be assigned to one geographic region within a state in 1998 and 2000, to a second region between 2002 and 2010, and to a third in 2012. Several switching districts lie in “purple” states such Florida, Iowa, Maryland, and Nevada.[23] In addition, the two states that continue to bar all ex-felons from voting, Kentucky and Virginia, contributed several switching districts.

[23] A similar pattern arises in our alternative counterfactual. Had these states not reformed their felony disenfranchisement laws, several of their congressional districts would have elected Republicans instead of Democrats in post-reform elections.

Since racial minorities are overrepresented in the criminal justice system and tend to vote Democratic, one might suspect that districts that elected Republicans, but would have elected Democrats in our counterfactual, have disproportionately large minority populations. Yet, the districts that appear in Figure 1 between 2002 and 2010 are, on average, 78.2 percent white, 10.6 percent black, 2.3 percent Asian, 8.4 percent Hispanic, and 0.6 percent Native American—a composition that is slightly "whiter" than the national average—with several of the switching districts in Iowa, Kentucky, Nebraska, and Wyoming being more than 90 percent white.[24] For districts marked with an asterisk, the percentages are similar. This pattern is consistent, however, with the effect size of our *Allow*-variables: For a district to switch in the counterfactual, its actual Democratic vote share must be just slightly smaller than 0.5, which is highly unlikely in districts with large minority populations. Given the voting patterns of whites and minorities, most districts with large minority populations already elected Democrats, and hence cannot be switching districts by definition.

A similar demographic composition holds in districts with the largest marginal effects of our voting rights variables. Take, for example, our regression specification 6. In this specification, the marginal effect of the *AllowAll*-variable, when averaged separately for each district, ranges from 0.0046 (NY-16) to 0.0173 (NC-8). The 25 districts with the largest marginal effects are 81.6 percent white, 7.7 percent black, 2.3 percent Asian, 8.9 percent Hispanic, and 1.4 percent Native American. Similarly, the 50 districts with the largest marginal effects are 78.1 percent white, 8.8 percent black, 2.1 percent Asian, 11.4 percent Hispanic, and 1.0 percent Native American.[25] If ex-felon disenfranchisement laws did impose real (and not merely nominal) constraints on minority voters, we would expect the congressional districts with the largest voting rights effects to be mostly districts with large minority populations. However, this is not the case.

7 Conclusion

To our knowledge, this paper is the first to utilize changes in felony voting restrictions in order to estimate the impact of such restrictions on election outcomes. In addition, we revisited Miles' (2004) analysis of the impact of felony disenfranchisement laws on the turnout rates of different population groups, using changes in these laws to explain changes in turnout rates. Focusing on elections for seats to the U.S. House of Representatives, we found a positive but statistically non-significant effect of ex-felon voting rights on the vote share of Democratic candidates. Even this effect implies implausible values of the number of ex-felons who had their voting rights

[24] We looked up each congressional district on *Wikipedia*, using the site's revision history feature to find demographic information for the district during the 2002–2010 period. In most cases, this information is based on the 2000 Census. The national average, based on the 2000 Census, was: 75.1 percent white, 12.3 percent black, 3.6 percent Asian, 12.5 percent Hispanic, and 0.9 percent Native American. Race percentages may not add up to one because some individuals report more than one race.

[25] Out of the top-25 districts, 7 are switching districts listed in Figure 1. Out of the top-50 districts, 11 are switching districts listed in Figure 1.

restored, their turnout rates, or their political preferences. Taking our estimates as upper bounds on the effect of restoring voting rights on vote shares, we concluded that no House majority would have changed in any year between 1998 and 2012, had all ex-felons been allowed to vote in all states.

We end this paper with two remarks. First, despite our conclusion that the voting rights of ex-felons are of little consequence for aggregate political outcomes, they matter to at least some individuals with criminal convictions. Manza and Uggen (2006) (ch. 6) show that many felons have a genuine desire to reintegrate into the community after serving their sentences, and consider civic participation an important part of the process of reintegration. The changes in felony disenfranchisement laws we examined are evidence of a growing consensus that lifelong voting bans are not only ethically problematic, but also stand in the way of efforts to reduce recidivism. Yet, ten states still restrict voting by some individuals with past felony convictions, and two states disenfranchise all ex-felons. Our finding that rights restoration has no tangible effects on election outcomes removes one potential political obstacle from reforming the criminal justice system towards one that places a greater emphasis on rehabilitation.

Second, while a low voter turnout rate among ex-felons is one probable reason we did not find a stronger re-enfranchisement effect, the question of why felons are less likely to vote is far from settled. Some authors recently suggested a causal link from relevant government policies to voting. Examining participation decisions following Iowa's 2005 decision to restore ex-felon voting rights, Meredith and Morse (2015) showed that many ex-felons were unaware that their voting rights had been restored, and that receiving information about rights restoration increased the likelihood that an ex-felon voted. This suggests that the actual process of rights restoration is a factor on which ex-felon turnout depends. More broadly, Weaver and Lerman (2010) argued that contact with the criminal justice system causes a decline in several aspects of civic participation, including voting in elections. They note that, for affected individuals, "the criminal justice system is a primary site of civic education" (p. 2), and that government activity—in particular, the administration of the criminal justice system—can "serve to demobilize and dissuade citizens from engaging in political life" (p. 15). These are policy issues of great importance, and beyond the scope of our analysis. However, it is not inconceivable that, under different government policies toward criminal justice in general, political outcomes could change in more significant ways than we estimated.

Appendix A: Classification of Changes in Ex-Felon Voting Rights

In Table 7, we summarize the legal changes that affected the voting rights of ex-felons in several states between 2000 and 2011 and explain how we classified a state's legal regime in a given year in our dataset. We also provide information about the political background behind each change, which we collected from state and local news sources.

Table 7: Changes in ex-felon voting restrictions by state.

Delaware *Legal change:* Senate bill/ constitutional amendment *Effective year:* 2000 *Before:* No person convicted of a felony could vote *After:* Ex-felons can apply to have their voting rights restored after five years of completion of their sentences, including time on probation or parole. Exceptions: Persons convicted of murder, sex offenses, and federal bribery. *Classification:* Full ban before 2000; partial ban 2000 and after	*Background:* As of early 2000, Delaware did not allow any person convicted of a felony to vote. Critics of the practice pointed to statistics showing that the restriction barred one in five African-American men in Delaware from voting. In January 2000, Democratic Senator Margaret Henry proposed a bill that would end the restriction by granting voting rights to ex-felons convicted of lesser crimes after five years of completion of their sentences, including time on probation and parole. On June 28, 2000, by a 16-to-5 vote in the Senate, Delaware amended the state's constitution restoring the right to vote for the group of individuals described in the bill. At the time, there were 20,500 convicted felons in the state. The bill did not require Governor Tom Carper's signature as it was a constitutional amendment. A companion bill also required that individuals applying to have their rights restored show they had paid all court-ordered fines and restitution. The amendment did not pass without criticism. According to Democratic Senate President Tom Sharp, the law allows "a whole host of people who commit heinous crimes who now we're going to say, 'Oh, that's OK, we're going to let you vote.' " However, a coalition of civic organizations, evangelical Christians, and labor union activists was able to overcome the opposition, leading to the bill's passage. (Sources: S40, S41.)
New Mexico *Legal change:* Senate bill *Effective year:* 2001 *Before:* No person convicted of a felony could vote (unless pardoned) *After:* Ex-felons have their voting rights restored automatically after completion of their sentences, including time on probation or parole *Classification:* Full ban before 2001; no ban 2001 and after	*Background:* Before 2001, New Mexico did not allow any person convicted of a felony to vote unless the person received a pardon. New Mexico ranked seventh among all states in the number of disenfranchised voters, despite having only four percent of the U.S. population. Among black voters, a total of 24.1 percent were disenfranchised. Hispanics and Native Americans were also disproportionately affected by felony voter bans. For example, while Hispanics made up 40 percent of the population, they constituted 60 percent of the state's prisoners. It was estimated that 50,000 New Mexicans were barred from voting because of the state's felony ban. In March 2001, the New Mexico state legislature adopted Senate Bill 204, sponsored by Senate President Richard Romero, which repealed the lifetime ban on ex-felon voting. Republican Governor Gary Johnson signed the bill into law allowing both state and federal felons to register to vote after serving their prison terns and all conditions of probation or parole. The law went into effect on July 1, 2001. (Sources: S3, S20, S28, S39.)

Table 7: Changes in ex-felon voting restrictions by state (continued).

Alabama *Legal change:* House bill *Effective year:* 2003 *Before:* No person convicted of a felony could vote (unless pardoned) *After:* Ex-felons can apply to have their voting rights restored after completion of their sentences, including time on probation or parole. No pardon required. Exceptions: Persons convicted of murder, rape, treason, possession of pornography with intent to distribute. *Classification:* Full ban before 2003; partial ban 2003 and after	*Background:* Prior to 2003, felons in Alabama had to apply to the state parole board in order to have their voting rights reinstated after completing their sentence. The process involved seeking a pardon and could take several years. In May 2003, Democratic State Representative Yvonne Kennedy sponsored a bill that would automatically restore the voting rights of ex-felons convicted of lesser crimes after completing their sentences, including any time on parole or probation, and paying all fines and restitution. The House voted for the bill with a 56-to-46 majority and it passed the Senate 21-to-9. However, in June 2003, Governor Bob Riley vetoed the bill, stating he was opposed to automatically restoring voting rights to ex-felons, and "the burden should remain on those ex-felons who are truly serious about having their rights reinstated." Alabama Republican Party Chairman Marty Connors went further, stating that "we're opposed to [restoring voting rights] because felons don't tend to vote Republican." The veto was widely criticized by groups such as the National Campaign to Restore Voting Rights in a state where 14 percent of African-Americans were disenfranchised because of felony convictions, compared to the statewide average of 6 percent for all racial groups. Governor Riley and African-American legislators eventually agreed on a compromise bill to streamline the restoration of voting rights of ex-felons. Ex-felons would still have to apply to the Board of Pardons and Paroles; however, the process would only take 60 days and would no longer require seeking a pardon. The compromised bill passed the House 47-to-42 and the Senate 21-to-11 in September 2003. Governor Riley signed the bill into law the following month. (Sources: S18, S19, S24, S29, S30, S32, S35, S38.)
Nevada *Legal changes:* Assembly bills *Effective years:* 2001, 2003 *Before 2001:* No person convicted of a felony could vote *2001–2002:* Persons convicted of a felony could petition parole board for restoration of voting rights, but the process was ineffective *2003 and after:* Ex-felons have their voting rights restored automatically after completion of their sentences for first-offense, nonviolent crimes. Petition process is required for other ex-felons. *Classification:* Full ban before 2003; partial ban 2003 and after	*Background:* Pior to 2001, Nevada permanently denied felons the right to vote. In 2001, the Nevada legislature passed Assembly Bill 328, which allowed ex-felons to petition the state parole board to have their voting rights restored. However, in a meeting of the Assembly Judiciary Committee in March 2003, Democratic Assemblywoman Chris Giunchigliani (a co-sponsor of AB 328) described the new process as ineffective and argued that Nevada should follow the national trend of easing the restoration process. In April 2003, the Nevada Assembly voted 32-to-10 to pass AB 337, and in late May 2003, a modified bill, AB 55, passed the Senate. The legislation automatically restored the voting rights of those convicted of first-time nonviolent crimes after completion of their sentences. Repeat offenders and anyone committed of violent crimes would still have to petition the state to have their voting rights restored. Republican Governor Kenny Guinn signed the legislation into law. (Sources: S22, S23, S31, S42.)

Table 7: Changes in ex-felon voting restrictions by state (continued).

Wyoming *Legal change:* Senate bill *Effective year:* 2003 *Before:* Convicted felons could appeal to governor to have their voting rights restored, but were unlikely to be successful *After:* Ex-felons can apply to parole board to have their voting rights restored five years after completion of their sentences, including time on probation or parole, as long as they had no further convictions. *Classification:* Full ban before 2003; partial ban 2003 and after	*Background:* Prior to 2003 Wyoming permanently denied ex-felons the right to vote. Convicted felons could appeal to the governor to have their voting rights restored, however, according to Democratic State Senator Keith Goodenough such appeals were unlikely to succeed under former Republican Governor Jim Geringer's administration. Senate File 65 would allow felons convicted of nonviolent offenses to apply to the parole board to have their voting rights reinstated five years after completing their prison sentence or probation, as long as they had not been convicted of any additional felonies. The Senate Judiciary committee supported the bill on a 4-to-1 vote and Democratic Governor David Freudenthal signed it into law in March 2003. (Sources: S4, S5.)
Florida *Legal change:* Court order, executive action *Effective year:* 2004 *Before:* Persons convicted of a felony could petition the state for restoration of voting rights, but the process was lengthy, arbitrary, and unlikely to result in restoration *After:* Immediate restoration of voting rights of eligible felons released between 1992 and 2001. Automatic restoration for those convicted of minor crimes after five years of completion of sentence, including time on probation and parole. Restoration of the rights of any felon who is crime-free for 15 years. *Classification:* Full ban before 2004; partial ban 2004 and after	*Background:* In 2003, Florida ex-felons were required to complete a "Restoration of Civil Rights" application, and only the governor and the Executive Clemency Board had the power to restore a convict's voting rights. After the contested 2000 presidential election, a group of African-American legislatures and the ACLU filed suit on behalf of the estimated $614,000$ Floridians who had completed their sentences but were ineligible to vote. The court ordered the Department of Corrections to assist approximately $125,000$ ex-felons who were released between 1992 and 2001 in applying for rights restoration. In June 2004, Republican Governor Jeb Bush announced the state had deemed $22,000$ of these ex-felons eligible for restoration of voting rights without a hearing (the remainder was found in other categories, including those in prison, deceased, or who already had their rights restored). Only felons who committed non-serious crimes were granted clemency without a hearing. Following the election in November 2004, Republican Attorney General Charlie Crist announced that the state was considering allowing some felons who had committed minor crimes to automatically get their voting rights restored without going through the full clemency process. Finally, at a meeting of the Executive Clemency Board, Bush and the elected cabinet officials approved three major changes: First, eliminate some factors that automatically disqualify a felon from requesting clemency without a hearing (such as denial of a previous clemency petition); second, automatically restore the rights of felons who have not committed a crime for five years unless they were convicted a specific violent crime; third, automatically restore the rights of any felon who is crime-free for 15 years. (Sources: S8, S10, S14, S21, S27, S34, S37.)

Table 7: Changes in ex-felon voting restrictions by state (continued).

Iowa *Legal changes:* Executive orders *Effective years:* 2005 (restoration); 2011 (rescission) *Before 2005:* No person convicted of a felony could vote (unless pardoned) *2005–2010:* Automatic restoration after completion of sentence, including time on probation and parole *2011 and after:* No person released from prison, parole, or probation after 2010 could vote (unless pardoned) *Classification:* Full ban before 2005; no ban 2005–2010; partial ban 2011 and after	*Background:* Prior to 2005, any person in Iowa convicted of an "infamous crime" was banned from voting; these crimes included felonies and aggravated misdemeanors. The only method of restoring voting rights was by the lengthy process of petitioning the governor and requesting executive clemency. For example, between taking office in 2001 and 2005 Democratic Governor Tom Vilsack restored the voting rights of 2,100 ex-felons, while an estimated 80,000 ex-felons were banned from voting, including one out of every four African-American men in Iowa. In June 2005, Governor Vilsack announced he would sign an executive order to restore voting rights to all convicted felons who had completed their sentences, including any time on parole or probation, stating: "When you've paid your debt to society, you need to be reconnected to society." Governor Vilsack signed the order on July 5, 2005. While payment of restitution and court fees were originally part of the restoration process, Governor Vilsack dropped that requirement. Muscatine County Attorney Gary Allison filed an unsuccessful lawsuit challenging the executive order. In 2011, Republican Governor Terry Branstad signed an executive order rescinding Governor Vilsack's automatic process of granting voting rights to felons. Convicted felons would again need to petition the governor to have their rights restored. However, the rescission would not affect the voting rights of those who already had their voting rights restored, and would restrict the voting rights of future released convicts. (Sources: S7, S11, S12, S13, S17, S33, S36, S45, S46.)
Nebraska *Legal change:* Legislative bill *Effective year:* 2005 *Before:* No person convicted of a felony could vote (unless pardoned) *After:* Automatic restoration two years after completion of sentence, including time on probation and parole *Classification:* Full ban before 2005; partial ban 2005 and after	*Background:* Prior to 2005, all persons with felony convictions were prohibited from voting in Nebraska unless they were able to secure a pardon, which generally was not approved until ten years after a prison sentence was completed. In 2003, the Pardons Board only issued 69 pardons, while estimates of the size of Nebraska's ex-felon population varied from 9,000 to 53,000 according to former Secretary of State John Gale. Similarly, according to the Sentencing Project, an advocacy group for ex-convicts, while approximately 44,000 ex-felons were potentially eligible for voting rights restoration, only 343 had their rights restored. In 2005, State Senator DiAnna Schimek sponsored Legislative Bill 53, which would restore ex-felons' voting rights two years after completion of their sentence. Republican Governor Heineman opposed the bill, stating that restoring felons' voting rights was unfair to the victims of their crimes. The legislation passed in the Nebraska legislature in March 2005. Governor Heineman vetoed the bill, stating, "I firmly believe that any restoration of rights should be considered thoughtfully on a case-by-case basis, which is precisely what occurs under our state's current constitutional process." However, the legislature overrode the veto with a 36-to-11 margin and the bill became law on June 2, 2005. (Sources: S1, S6, S16, S25, S26.)

Table 7: Changes in ex-felon voting restrictions by state (continued).

Maryland *Legal changes:* House, Senate bills *Effective years:* 2002, 2007 *Before 2002:* Persons convicted twice permanently lost right to vote *2002–2006:* Tiered approach to voting rights restauration *2007 and after:* Automatic restoration after completion of sentence *Classification:* Partial ban before 2007; no ban 2007 and after	*Background:* Before 2002, persons convicted of a felony twice permanently lost their right to vote in Maryland. In 2002, the state replaced this restriction with a tiered approach: Persons convicted of one "infamous crime" (including such categories as fraud and corruption) could register to vote after completing their sentence; those convicted of two or more nonviolent crimes could register to vote three years after completion of their sentences; any felon convicted of violent crime twice was permanently barred from voting. In 2006, Democratic Assembly Delegate Salima Siler Marriott first sponsored a measure to give all former felons the right to vote as soon as they were released; however, the proposed legislation was heavily opposed by Republican Governor Robert Ehrlich. In February 2007, a House bill was introduced that would allow all first-time offenders to vote immediately after release from prison, and a Senate bill was introduced that would remove the waiting time for second-time offenders. In March 2007, the House bill passed on a 78-to-60 vote and the Senate bill passed on a 28-to-19 vote. In April 2007, Democratic Governor Martin O'Malley signed SB 488 and HB 554, which restored the right to vote for all felons after completing their sentences. Advocates said that more than 50,000 Marylanders would be eligible to vote as a result of the legislation. (Sources: S2, S9, S15, S43, S44.)

Sources for Appendix A

S1: "Developments on the 38th Day of the First Session of the 99th Nebraska State Legislature, Thursday, March 3, 2005." *The Associated Press State & Local Wire*, March 3, 2005.

S2: "Bill Restoring Felon Voting Rights Closer to Final Passage." *The Associated Press State & Local Wire*, March 23, 2007.

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Appendix B: Sample Selection and Vote Allocation Procedure

We downloaded race-level election data for all 435 voting seats in the U.S. House of Representatives between 1998 to 2012 from the website of the Federal Election Commission ("FEC"). As this period covers eight national elections, we have information on $8 \times 435 = 3,480$ election races.[26] An election race consists of all elections associated with a given House seat in a given election year, including primary, general, and (if necessary) runoff elections. For each such race, the FEC dataset contains the names of all candidates who ran for office or who received at least one vote as write-in candidates, as well as their incumbency status, party affiliation, and number of votes received in each election. We focus on the general elections for seats to the U.S. House of Representative, for which approximately 769 million votes are recorded in the FEC dataset during 1998–2012.

We excluded approximately 1.6 percent of these votes. First, with one exception to be explained below (see Footnote 29), we excluded votes for write-in candidates with no verifiable party affiliation. Such candidates did not officially run for office and, generally, received a negligible number of votes per candidate. In those instances where a write-in candidate was also an official candidate on the ballot, we allocated the write-in votes to the party that appeared for this candidate on his or her official ballot entry. Second, the state of Nevada allows its citizens to vote for "None of these candidates." It is not unusual for this option to receive a substantial

[26]The FEC tables are available at www.fec.gov/pubrec/electionresults.shtml.

number of votes; however, because these votes are not for an actual candidate we excluded them as well.

The remaining votes are for candidates who appear on the ballot as either party candidates or independents, and a political party label or the label "Independent" is assigned to each of these candidates by the FEC. We made the following adjustments to this assignment. First, some states (e.g., New York) allow for *electoral fusion*, meaning that multiple parties nominate the same candidate, whose name then appears multiple times on the same ballot (once for each party). For such candidates, we computed the combined votes across parties, and assigned this total to the party under whose label the candidate received the most votes (in all cases, this was either the Republican or Democratic party). Second, some candidates' party is recorded by the FEC as "Republican/Democrat" or "Democrat/Republican." In these cases, we used the party whose primary the candidate had entered as the candidate's party (this determination could always be made unambiguously). Third, in Minnesota the official name of the Democratic party is "Democratic-Farmer-Labor Party" ("DFL"), and in North Dakota the official name of the Democratic party is "Democratic Nonpartisan-League Party" ("NPL"). In these states, we counted votes for DFL and NPL candidates as votes for Democrats.

Finally, we made three discretionary changes to a candidate's party affiliation. We changed the party label of Vermont candidate Bernie Sanders from "Independent" to "Democrat" in the 1998, 2000, 2002, and 2004 elections;[27] we changed the party label of Virginia candidate Virgil Goode from "Independent" to "Republican" in the 2000 elections;[28] and we changed the party label of Texas candidate Shelley Sekula-Gibbs from "Write-In" to "Republican" in the 2006 election.[29]

After these adjustments were made, 48.1 percent of the approximately 757 million remaining votes went to Republican candidates and 49.1 percent went to Democratic candidates, with the remainder going to third-party candidates and independents.

[27]Sanders represented Vermont's at-large congressional district as an Independent until 2007, when he became Vermont's junior U.S. Senator. Sanders caucused with congressional Democrats during both his time in the House and the Senate, and was not opposed by a Democrat in all but the 2004 elections (when he was opposed by Democratic candidate Larry Drown, who received less than eight percent of all votes). For these reasons, we classify Sanders as a Democrat in our dataset.

[28]Goode ran in, and won, Virginia's 5th district as a Democrat in 1996 and 1998. In 2000, he ran in the same district as an Independent, and won. He then ran as a Republican in 2002, 2004, 2006, and 2008, when he lost. When Goode ran as an Independent in 2000, he was unopposed by a Republican but was opposed by a Democrat. For these reasons, we classify Goode as a Republican in the 2000 election.

[29]In 2006, Sekula-Gibbs was a write-in candidate in Texas' 22nd district, a seat previously held by former House majority leader Tom DeLay (R). Under indictment for money laundering, DeLay resigned from his post as House majority leader in 2005. He nevertheless ran for reelection and won the Republican nomination in March 2006. However, the following month DeLay withdrew from the race after a former aide had pleaded guilty to corruption charges related to the Jack Abramoff lobbying scandal. By then, it was legally too late to nominate a replacement candidate for DeLay, forcing the Texas Republican party to "nominate" a write-in candidate, Sekula-Gibbs, to run against Democratic nominee Nick Lampson, who later won the race with 52 percent of the vote. (See, e.g, en.wikipedia.org/wiki/Texas'_22nd_congressional_district_elections,_2006.)

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