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Felon Disenfranchisement and Voter Turnout

Thomas J. Miles

ABSTRACT

Several states permanently disenfranchise convicted felons, and according to existing estimates, the population of disenfranchised felons is disproportionately male and African-American. This paper examines the impact of felon disenfranchisement on state-level voter turnout. First, the paper shows that the number of disenfranchised felons is so large that conventional measures of voter turnout, which fail to correct for the ineligibility of disenfranchised felons, significantly understate the participation rates of eligible African-American men. Second, the paper uses a triple-differences framework to test whether disenfranchisement actually reduces the turnout of African-American men. The estimates reveal that disenfranchisement has no discernible effect on state-level rates of voter turnout. The absence of an effect is consistent with the view that on average felons belong to demographic groups that, although eligible to vote, infrequently exercise that right. The estimates suggest that the impact and purpose of these laws are more modest than previously thought.

1. INTRODUCTION

All but two states deprive convicted felons of the right to vote for some period of time.[1] Most states disenfranchise convicts during periods of incarceration and restore voting rights upon release or upon completion of parole or probation. Eleven states permanently disqualify ex-felons.

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1. Neither Maine nor Vermont deprive convicted felons of the right to vote. See the discussion in Section 3.1.

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Although felon disenfranchisement is widespread, legal academics have persistently criticized it,[2] and the leading professional association in law has long opposed it.[3] In recent years, the tremendous growth of incarceration rates[4] has raised questions about the size and racial composition of the disenfranchised population,[5] and editorialists in the popular press have given attention to the practice.[6]

Numerous observers have noted that disenfranchised ex-felons are disproportionately African-American males,[7] and some have implicitly or explicitly asserted that disenfranchisement causes declines in the rates of voter turnout.[8] The idea that ex-felon disenfranchisement might reduce voter participation has intuitive appeal, because it conforms with conventional wisdom about which demographic groups are likely to

2. The topic is a perennial favorite of law review notes and comments, all of them critical of the policy, for example, Note (1971), Moldof (1968), Note (1967). Interest in the subject has intensified in recent years. See Boyd (2002), Dugree-Pearson (2002), Ewald (2002), Note (2002), Thompson (2002), Mondesire (2001), Furman (1997).

3. "Jurisdictions should not impose the following collateral sanctions . . . deprivation of the right to vote, except during actual confinement" (American Bar Association 2003, Standard 19-2.6). "Persons convicted of any offense should not be deprived of the right to vote" (American Bar Association 1980, Standard 23-8.4).

4. Prison populations increased almost fourfold in the past 15 years (U.S. Department of Justice 1997b, p. 464).

5. Contrast views in 1989 and 2003. "Ex-felons are unlikely to constitute more than a tiny percentage of the population and thus are electorally insignificant" (Note 1989, p. 1303); "The issue has renewed urgency because of the prison boom. 'Disenfranchised felons used to represent 1 percent of Florida's voting age population,' [Nancy Northrup, director of the Democracy Project at NYU's Brennan Center for Justice] says. 'Now it's 5 percent'" (Amon 2003, p. A6).

6. For example, Editorial (2002a, p. 8), Editorial (2002b, p. A32), Abramsky (2002, p. A11), Drinan (2000, p. A19), Cole (2000, p. 17A), Breazell (1999, p. 11), Love (1999, p. 6B), Chapman (1999, p. 7B), Editorial (1999, p. 22), M. Fletcher (1999, p. A1).

7. "In states that enact a permanent loss of the right to vote, this feature combined with the demographics of the criminal justice system produces a significant and disproportionate effect on black citizens to the extent that as many as one-sixth of the black population is permanently disenfranchised in some states" (National Commission 2001, p. 45); "Felon disenfranchisement laws still remain in many states, effectively blocking access to the polls for minority groups in those states" (Hench 1998, p. 765); "[D]ue to the larger number of blacks imprisoned compared to whites in proportion to total population for each group, there is a significant impact on the black vote" (Harvey 1994, p. 1147).

8. "[I]t is difficult to deny that skyrocketing incarceration rates have raised the stakes of criminal disenfranchisement, altering the composition of the American electorate" (Note 2002, pp. 1940–41); "The current system of disenfranchisement . . . is now silencing the minority vote" (Dugree-Pearson 2002, p. 362); "Although their participation rates in elections would likely be low, their numbers are certainly sufficient to affect outcomes of elections in numerous states" (Keyssar 2000, p. 308); "Disenfranchised, they lack electoral presence" (Robertson 1997, p. 1005).

commit crimes—and thus become disenfranchised—and which are least likely to vote.[9] However, ex-felon disenfranchisement laws do not necessarily reduce voter turnout rates. An alternative explanation is that members of the demographic groups most likely to be disenfranchised choose not to vote at higher rates than other groups. The same incentives that make criminal activity a privately optimal decision may also render abstention from voting a preferred choice. Under this view, the relatively low rates of voter turnout among African-American males are not the causal consequence of ex-felon disenfranchisement laws.

A growing number of social scientists have begun to seek systematic evidence of the causal relationship between ex-felon disenfranchisement and voter participation. Manza and Uggen (1998) are undertaking a research agenda in which they anticipate linking long-term declines in voter participation to the growth of disenfranchised populations. Manza, Uggen, and Britton (2001) estimated the rates at which ex-felons would have voted, had they been permitted to, and used these rates to construct counterfactual election outcomes. Others pursued a strategy of comparing voter turnout in states that disenfranchise ex-felons to turnout in states that do not disenfranchise them. Grose and Yoshinaka (2001) asserted that among southern states, total voter turnout in states that permanently disenfranchise ex-felons is lower, and Hirschfield (1999) drew similar comparisons in a sample limited to African-American males. In contrast, Miles (2000) examined a repeated cross section of all 50 states and found that laws of permanent disenfranchisement were not systematically related to state-level turnout rates or to the likelihood of electing a Republican senator or governor.

One difficulty with estimating the causal effect of ex-felon disenfranchisement laws on voter turnout is that "before/after" comparisons are not possible because these laws have changed little in recent decades. In the absence of longitudinal variation in the legal rules, this paper attempts to identify the impact on voter turnout by combining the cross-sectional variation in the legal rules with a set of "treatment and control"

9. More than 9 percent of African-Americans were incarcerated or on probation or parole in 1994, compared with only 2 percent of whites, and the voter turnout rates of African-Americans were almost 3 percentage points lower than those of whites in 2000. In the nonpresidential election year of 1998, the racial turnout gap was almost 4 percentage points. Similarly, nearly 5 percent of males were under correctional supervision, versus less than 1 percent of women, and the male participation rate was roughly 3 percent less than that of women in the 2000 election. See U.S. Department of Justice, Bureau of Justice Statistics (1997b, p. 5), U.S. Department of Commerce (2003, p. 251).

groups. Specifically, the paper uses a "difference-in-differences-in-differences" framework to compare state-level voter turnout rates across three dimensions: race, gender, and the presence of a law permanently disenfranchising ex-felons. The assumption of the framework is that African-American males, as the demographic group most likely to be disenfranchised, are a "treatment" category and that whites and women, who are disenfranchised at much lower rates, are "controls." The framework assumes that the third dimension, the presence of a law permanently disenfranchising ex-felons, is the treatment that potentially influences voter turnout.

The framework has two advantages over prior research. First, the policy concern of recent critics is that disenfranchisement has a disproportionate impact on African-American men. Prior research efforts did not address the disproportionate impact because they drew comparisons across only one dimension. The triple-differences approach draws comparisons necessary to evaluate a claim of disproportionate impact. Second, the empirical design of this paper more plausibly identifies the causal effect of ex-felon disenfranchisement on voter turnout. By drawing comparisons relative to whites and women, the framework removes race- and gender-specific determinants of participation. Any difference in the turnout rates of African-American males remaining after these comparisons is plausibly attributed to the felon disenfranchisement laws.

The more demanding empirical approach generates novel conclusions about the consequence of ex-felon disenfranchisement. While African-American males are disenfranchised at disproportionate rates, the triple-differences estimates reveal no distinct effect on the state-level turnout of African-American males. The absence of an effect is apparent both in the summary statistics and in regression analyses that control for other determinants of voter participation. The conclusion is unaffected when the sample is limited to the most recent years, the period when disenfranchised populations were presumably larger, and when the comparison groups are restricted to within-race or within-gender "double differences." It is also robust to restricting the sample to southern states. Even when states are organized according to the degree to which a state's population of disenfranchised felons is racially disproportionate, rather than the legal rule, no effect is seen.

The result that ex-felon disenfranchisement has no impact on state-level voter turnout is consistent with earlier research on the demographics of criminality and voting. Because the correlates of criminal activity also predict abstention from voting, ex-felons appear to belong on av-

erage to demographic groups with low rates of participation. The invariance of state-level turnout rates to disenfranchisement laws suggests that, when given a choice, most felons choose not to vote.

The paper's estimates also inform the debate over the desirability of these laws. Both proponents and opponents of felon disenfranchisement assume that these laws prevent sizable numbers of felons from voting. However, this paper's estimates indicate that ex-felons' preferred behavior is abstention from voting. The empirical finding that the effect of these laws is smaller than anticipated motivates a reconsideration of the purpose of these laws. While this paper does not estimate all the social costs and benefits of the policy, it suggests that the impact and purpose of disenfranchisement are more modest than previously thought.

The paper proceeds as follows. Section 2 reviews the legal context of ex-felon disenfranchisement laws and emphasizes the role of statistical evidence in legal challenges to disenfranchisement. Section 3 presents the empirical analysis of voter turnout. It first demonstrates the potential impact of disenfranchisement on voter turnout and then tests whether this potential is realized. Section 4 uses the empirical findings to reevaluate the justifications for and critiques of ex-felon disenfranchisement. Section 5 concludes the paper.

2. LEGAL CHALLENGES TO EX-FELON DISENFRANCHISEMENT

Plaintiffs challenging state disenfranchisement laws typically advance two types of claims: those asserting violations of the United States Constitution and those alleging violations of section 2 of the Voting Rights Act (VRA). The typical constitutional challenge is that a facially neutral ex-felon disenfranchisement law denies the Fourteenth Amendment's guarantee of equal protection of the laws because disenfranchisement has a discriminatory impact on racial minorities.[10] Government restric-

10. Cases involving equal protection claims against felon disenfranchisement laws include *Johnson v. Bush* (214 F. Supp.2d 1333 [S.D. Fla. 2002]), *Hunter v. Underwood* (471 U.S. 222 [1985]), *Richardson v. Ramirez* (418 U.S. 24 [1974]). Courts have been quick to discount other constitutional claims, such as the argument that disenfranchisement violates the Eighth Amendment (Tims 1975). *Green v. Board of Elections* (380 F.2d 445, 450 [2d Cir. 1967]) concluded that disenfranchisement is not cruel and unusual since the Framers did not regard it as such.

tions on voting are usually subject to strict scrutiny[11] because voting is a fundamental right.[12] An express reference to ex-felon disenfranchisement in section 2 of the Fourteenth Amendment, however, exempts disenfranchisement from strict-scrutiny analysis.[13] To prevail under the equal protection clause, plaintiffs must therefore make two showings. They must first show that the voting law has a disproportionate impact and then demonstrate that discriminatory intent was a substantial or motivating factor in its enactment (*Hunter,* 471 U.S. 222, 227–28). Legislative intent is difficult to discern and prove,[14] and in only one notable case has a plaintiff successfully provided such evidence (*Hunter,* 471 U.S. 222, 228–32).

In addition to equal protection claims, plaintiffs have challenged disenfranchisement laws under the so-called results test of section 2 of the VRA.[15] The test allows plaintiffs to challenge election laws without a showing of discriminatory intent.[16] However, section 2 of the VRA has

11. Strict scrutiny has been applied, for example, to residential duration requirements (*Dunn v. Blumstein,* 405 U.S. 330, 335 [1972]), poll taxes (*Harper v. Virginia Board of Elections,* 383 U.S. 663, 670 [1966]), and landownership and parentage requirements in school district elections (*Kramer v. Union Free School District,* 395 U.S. 621, 627 [1969]). Strict scrutiny requires the state to demonstrate that the challenged restriction promotes a compelling state interest, that the restriction is narrowly tailored to that interest, and that it is the least restrictive means of achieving the interest (*Dunn,* 405 U.S. 330, 342–43; *United States v. Carolene Products Co.,* 304 U.S. 144, 152–53, n. 4 [1938]). However, not all cases apply strict scrutiny to voting restrictions. For example, "the rigorousness of our inquiry into the propriety of a state election law depends upon the extent to which a challenged regulation burdens First and Fourteenth Amendment rights" (*Burdick v. Takushi,* 504 U.S. 428, 434 [1992]).

12. *Harper* (383 U.S. 663, 667), *Reynolds v. Sims* (377 U.S. 533, 561–62 [1964]), *Yick Wo v. Hopkins* (118 U.S. 356, 370 [1886]).

13. In *Ramirez,* then-associate justice Rehnquist wrote for the Court that the equal protection clause in section 1 of the Fourteenth Amendment must be considered in light of section 2. The latter section expressly permits the disenfranchisement of participants in "rebellion, or other crimes" without a corresponding reduction in the number of persons counted for purposes of apportioning representatives to the U.S. House (*Ramirez,* 418 U.S. 24, 56).

14. "Proving the motivation behind official action is often a problematic undertaking" (*Hunter,* 471 U.S. 222, 228).

15. Under the 1982 amendment, a violation is "established if, based on the totality of circumstances, it is shown that the political processes leading to nomination or election in the State or political subdivision are not equally open to participation by members of a [protected] class of citizens . . . in that its members have less opportunity than other members of the electorate to participate in the political process and to elect representatives of their choice" (42 U.S.C. sec. 1973[b]).

16. "[T]o restore the legal standard that governed voting discrimination cases prior to the Supreme Court's decision in [*City of Mobile v.*] *Bolden* [446 U.S. 55, 60–61 (1980) (holding that proof of discriminatory intent is required to show a violation of section 2

produced little success for plaintiffs. For example, one court required proof of a "causal nexus" between the disenfranchising statute and the social context of the discrimination (*Wesley v. Collins,* 605 F. Supp. 802, 813 [M.D. Tenn. 1985], aff'd, 791 F.2d 1255, 1260–61 [6th Cir. 1986]). According to some critics, the requirement of a causal nexus is tantamount to establishing proof of intent in an equal protection claim.[17] Another court held that Congress did not intend the results test to apply to ex-felon disenfranchisement laws (*Baker v. Pataki,* 85 F.3d 919, 922 [2d Cir. 1996], aff'g *Baker v. Cuomo,* 842 F. Supp. 718, 721–23 [S.D.N.Y. 1993]). Despite these obstacles, plaintiffs continue to bring challenges to ex-felon disenfranchisement laws on the basis of both equal protection and section 2 of the VRA (for example, *Johnson v. Bush,* No. 02-14469 [11th Cir., December 19, 2003], rev'g, 214 F. Supp. 1333 [S.D. Fla. 2002]).

Both types of claims require a showing of disproportionate racial impact. Plaintiffs typically present statistics on the racial composition of felons or racial disparities in disqualification rates. They usually present a ratio of disenfranchised African-American felons to disenfranchised white felons (for example, *Hunter,* 471 U.S. 222, 227), or they contrast the percentage of disenfranchised felons in each racial group (for example, *Pataki,* 85 F.3d 919, 923; *Cuomo,* 842 F. Supp. 718, 722; *Wesley,* 605 F. Supp. 802, 804; *Hunter,* 471 U.S. 222, 227). Plaintiffs focus on disqualification rates rather than turnout rates because the former are sufficient to satisfy the discriminatory effect prong of equal protection and VRA claims (for example, *Hunter,* 471 U.S. 222, 227). In doing so, they miss an opportunity to present a persuasive policy argument. Racial disparities in turnout rates, rather than disqualification rates, could more tellingly indicate the practical consequences of the disenfranchisement policy. Although strict scrutiny is not the standard of review for ex-felon disenfranchisement laws, an observable racial disparity in turnout would invite closer judicial examination because it would indicate that disenfranchisement constrains ex-felons' efforts to

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of the VRA)]" (S. Rep. No. 417, 97th Cong., 2d Sess. 27 (1982), reprinted in 1982 U.S.C.C.A.N. 177, 192 (1981)). Also see Hench (1998, pp. 746–47), Shapiro (1993, p. 552).

17. "The district court's creation of an additional [third] requirement—to prove a causal connection between the history of racial discrimination in Tennessee and the state's disenfranchising statute—essentially required the plaintiffs to show intent, contradicting both the language and legislative history of section 2 of the Voting Rights Act" (Shapiro 1993, p. 552).

exercise their voting rights and thereby protect their other rights.[18] No plaintiff has introduced statistics on voter turnout rates to show that participation is lower among groups whose members have lost their voting rights at disproportionate rates or that the participation of these groups falls after the introduction of the prohibition. The next section explores whether such effects are detectable in state-level voter turnout.

3. EMPIRICAL ANALYSIS

3.1. Data

The author constructed the data set by matching information on ex-felon disenfranchisement laws to repeated cross sections of state-level voter turnout rates in biennial November elections for the years 1986–2000.[19] The status of ex-felon disenfranchisement laws was available from several sources (U.S. Department of Justice, Office of the Pardon Attorney [various years]; Manza, Uggen, and Britton 2001, app. 1; Fellner and Mauer 1998, pp. 3–4; Hench 1998, pp. 790–97; Shapiro 1993, pp. 539–40 nn.15, 16). Appendix Table A1 shows the status of disenfranchisement laws in each state during the observation period, and asterisks mark the states included in the sample. During the sample period, the states under study did not substantially amend their disenfranchisement laws; therefore, the paper does not use longitudinal variation in the legal rules to identify the impact on voter participation. While a few states in the sample revised their felon disenfranchisement laws during the observation period, the changes did not affect enough convicts to allow credible measurement of the impact on turnout.[20] Most

18. The right to vote is a fundamental right because it is a guardian of other rights (*Harper,* 383 U.S. 663, 667; *Reynolds,* 377 U.S. 533, 561–62; *Yick Wo,* 118 U.S. 356, 370). For example, "[t]his stripping of voting rights is seriously impacting the very urban areas where a healthy electorate is most needed in order to effect change quickly" (Dyer 2000, p. 267).

19. For convenience, this paper refers to Washington, D.C., as a state.

20. Utah and Massachusetts adopted disenfranchisement policies, and Connecticut, Delaware, and Texas liberalized their existing ones. Utah in 1998 and Massachusetts in 2000 passed constitutional amendments prohibiting inmates from voting (Van Eyck and Dillon 1998, p. A01; Lewis 2000, p. B2). In both states incarcerated populations represent less than one-third of 1 percentage point of the voting-age population (author's calculations from U.S. Department of Justice, Bureau of Justice Statistics [2003, table 2]; U.S. Department of Commerce [2003, table 4a]; Manza, Uggen, and Britton [2001, app. 1]). In 2000, Connecticut restored voting rights to persons on probation (Price 2002, p. 401). In 1999, Delaware reduced the period of ex-felon disenfranchisement from life to 5 years (Note

recently, two states significantly curtailed their disenfranchisement policies, perhaps because of renewed activism regarding these laws.[21] These states have relatively small African-American populations, and the changes occurred too recently to analyze in this paper.[22]

Data on voter turnout were not official counts of ballots but the author's estimates calculated from the November Voter Supplements to the U.S. Census Bureau's Current Population Survey (CPS). From these files, the author tabulated voter turnout rates for four demographic groups: two genders by two racial groups. The secrecy of official ballots necessitated the use of the CPS surveys in order to disaggregate voter turnout by demographic group. Two concerns with self-reports of voting behavior are that some nonvoters falsely describe themselves as voters and that the incidence of this misrepresentation is greater among African-American respondents than whites (Abramson and Claggett 1984, pp. 733–34; Hill and Hurley 1984, p. 201; Sigelman 1982, p. 51–52).[23] Racial patterns in misreporting, however, should not bias estimates of the impact of ex-felon disenfranchisement, because the empirical strategy draws comparisons within each racial group. No one has yet studied whether racial differences in misreporting are themselves attributable to felon disenfranchisement, but it appears unlikely.[24]

2002, pp. 1946, 1948). In 1999, Texas eliminated a 2-year waiting period for the restoration of voting rights and permitted ex-felons to vote immediately upon completion of their sentences (Note 2002, p. 1948). Since Texas did not strip ex-felons of the right to vote permanently, the analysis of this paper classified Texas as a nondisenfranchising state, but as discussed in Section 3.3.2, categorizing it as a disenfranchising state has minimal effect on the estimates.

21. See Craig (2002, p. 1B), referring to "a nationwide campaign" to repeal disenfranchisement laws.

22. Nevada and New Mexico recently amended their restrictions on ex-felons' voting rights. In 2001, Nevada permitted mandatory restoration of voting rights upon an ex-felon's application (Note 2002, pp. 1946, 1948). In 2003, it approved a statute providing for automatic restoration of an ex-felon's franchise (Sentencing Project 2003, p. 3). In 2001, New Mexico restored voting rights to felons who have completed their sentences (Price 2002, p. 402). These changes are not analyzed here for two reasons. First, the African-American populations of these states are too small to permit reliable estimates of voter turnout rates by race and gender, and second, the recent dates of these modifications leave no postchange period in which to draw comparisons.

23. The greater incidence of overreporting among African-Americans is primarily due to their lower turnout rates because conditional on not having voted, African-Americans are only slightly more likely than whites to misreport their voting behavior (Anderson and Silver 1986, pp. 779–82; Silver, Anderson, and Abramson 1986, p. 619).

24. Two reasons militate against the notion that ex-felon disenfranchisement could account for the higher rate of misrepresentation among African-Americans. First, the racial pattern of misrepresentation existed before the recent growth of incarceration rates and presumably disenfranchisement rates. Second, if the greater incidence of ex-felon disen-

Several states were excluded from the sample because turnout figures by demographic group were unavailable. In some states, such as Idaho, Maine, and North Dakota, the population of African-Americans was so small that estimates of voter turnout by racial group were unreliable, and for that reason, these states were excluded from the analysis. In other states, the modest size of CPS samples within race-by-gender cells rendered estimates of voter turnout unreliable, and these states were also dropped from the sample. After these exclusions, the final sample contained 26 states; these states are indicated by asterisks in Appendix Table A1.[25] After the reduction in the sample size, six of the 11 states that permanently disenfranchise ex-felons remained. These states were Alabama, Florida, Kentucky, Mississippi, Tennessee, and Virginia.[26] The main survey of the November CPS was the source for estimates of age, unemployment, and monthly earnings. Table 1 displays sample means for the CPS variables by race. Other sources reported information on the characteristics of the elections[27] and the dates of gubernatorial and senatorial elections.[28]

3.2. Implications of Disenfranchisement for the Measurement of Turnout Rates

As described Section 1, the claim that felon disenfranchisement depresses African-American voter participation is plausible because it is consistent with the observation that the turnout rate of African-Americans is lower than that of whites.[29] When a sizable group is disqualified from voting,

franchisement among African-Americans explained the higher rate of misrepresentation, then misreporting should correlate with the traditional characteristics of criminality. Instead, even within racial groups, the likelihood of misrepresentation is greater for respondents who possess higher levels of education and who regularly attend religious services (Bernstein, Chadha, and Montjoy 2001, p. 35).

25. These states were Alabama, Arkansas, California, Connecticut, the District of Columbia, Florida, Georgia, Illinois, Indiana, Kentucky, Louisiana, Maryland, Michigan, Mississippi, Missouri, North Carolina, New Jersey, New York, Ohio, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, Virginia, and Wisconsin.

26. See note 20. Texas, which restores voting rights 2 years after complete discharge of a sentence, is classified as a state that does not permanently disenfranchise ex-felons. As described in Section 3.3.2, reclassifying it as a state that permanently disenfranchises has minimal effect on the estimates.

27. Council of State Governments and American Legislators' Association, The Book of the States (various years), contains descriptions of election laws, such as the availability of mail-in registration, the requirements of advance registration, and the duration of a valid registration.

28. U.S. Department of Commerce (various years).

29. See note 9.

Table 1. Summary Statistics

	African-American (1)		White (2)	
Unemployment rate	10.582	(.401)	4.01	(.171)
Real average weekly earnings ($)	270.230	(7.716)	346.247	(9.498)
% Aged 18–35	41.139	(.452)	34.366	(.564)
% Aged 36–65	44.407	(.510)	48.086	(.352)
% Aged >65	11.615	(.456)	15.765	(.402)
Voter registration rate	62.750	(1.322)	66.329	(1.097)
Voter turnout rate	46.055	(1.173)	51.357	(.779)

Note. Data are means and standard errors (in parentheses). The data are state-level, race- and gender-specific aggregates for even-numbered years between 1986 and 2000 for 25 states (Alabama, Arkansas, California, Connecticut, Florida, Georgia, Illinois, Indiana, Kentucky, Louisiana, Maryland, Michigan, Mississippi, Missouri, North Carolina, New Jersey, New York, Ohio, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, Virginia, and Wisconsin) and the District of Columbia. The data are the author's extractions from the U.S. Census Bureau's November Current Population Surveys and its Voter Supplements. $N = 416$.

the conventional measure of voter turnout, the ratio of votes cast to the voting-age population (VAP), does not accurately reflect the actual rate of participation among the eligible, nor does it accurately capture the counterfactual turnout rate that would prevail in the absence of the disenfranchisement policy. It is less than both of these magnitudes.

The VAP-based measure is less than the counterfactual rate because it fails to include the votes of felons who would cast ballots if the state allowed them to vote. The VAP-based measure of turnout can be written as V/P, where V is the number of votes cast and P is the VAP. The counterfactual turnout rate can be written as $(V + \alpha F)/P$, where F is the number of otherwise ineligible felons and α is the fraction of them who would choose to vote. As long as some felons choose to vote, the counterfactual turnout rate will be higher than the VAP-based turnout rate.

The counterfactual rate may be estimated in two ways. The first method begins with the estimation of parameters F and α. Manza, Uggen, and Britton (2001, apps. 1 and 2) and Fellner and Mauer (1998, pp. 8–9) have recently produced estimates of F, the number of disenfranchised felons. Their estimates are disaggregated by race and state and show that disenfranchised felons represent significant fractions of the VAP.[30] The absence of a social science database containing information

30. Manza, Uggen, and Britton (2001, apps. 1 and 2) report "disenfranchisement rates" of 2.1 percent nationally for all voters and 6.6 percent for African-Americans. Fellner and Mauer (1998, pp. 8–9, table 2) state that 2.0 percent of American voters and 13.1 percent of African-American males are disenfranchised.

on both voting and criminal activity hinders the estimation of α, but Manza, Uggen, and Britton (2001, p. 18) have attempted a calculation.

The second method of estimating the counterfactual turnout rate does not require knowledge of parameter α. This approach asks whether turnout rates are observably higher in states with felon disenfranchisement laws than in states without them. In states with the restriction, the observed turnout rate is V/P, and in states without it, it is $(V + \alpha F)/P$. Abstracting from econometric issues, the difference in the VAP-based turnout measures between states with and without the law, or $\alpha F/P$, reflects the number of percentage points by which felon disenfranchisement reduces turnout. Since the number of disenfranchised felons is now sizable (or $F > 0$), the difference in VAP-based turnout measures effectively tests whether felons vote at significant rates, or if $\alpha = 0$. Loosely speaking, the triple-differences methodology in the next section draws this comparison.

The VAP-based turnout measure also understates existing turnout among the eligible. When a sizable fraction of the VAP is disqualified from voting, the VAP-based turnout measure underestimates the frequency with which persons eligible to vote actually cast ballots. To reduce the degree of this mismeasurement, political scientists have begun to calculate turnout rates using the "voting-eligible population" (VEP), which excludes persons disqualified from voting.[31] In the case of felon disenfranchisement, the VEP-based turnout measure that excludes ineligible felons would be $V/(P - F)$.[32] This measure is not a counterfactual of the rates at which felons would vote if permitted, but instead it is a more accurate estimate of the actual rate of turnout among the eligible.

Using CPS estimates of each state's VAP and Manza, Uggen, and

31. The VEP-based turnout rates reveal that voter participation has not declined in recent decades. McDonald and Popkin (2001) show that turnout rates based on the VEP have been relatively stable. The VEP turnout rate also indicates that the upward bias in turnout rates estimated from the widely used National Election Study (NES) is not rising. Burden (2000) claimed that a growing gap between official turnout rates and the NES was attributable to a declining response rate to the NES. McDonald (2003) finds no growth in the gap between VEP turnout rates and those based on the NES, and Martinez (2003) reports growth in the gap beginning only in 1996.

32. The VEP estimated here differs from that of McDonald and Popkin (2001). They exclude aliens and incarcerated persons and include citizens residing abroad (McDonald and Popkin 2001, pp. 963, 971). However, their analysis does not consider disenfranchised felons who are not incarcerated, and the estimates of Manza, Uggen, and Britton (2001), as well as those of Fellner and Mauer (1998), show that this group is significant. Another difference is that McDonald and Popkin analyze a national-level time series of total turnout, and this section considers state-level turnout by race in a single year, 2000.

Table 2. Mean Voter Turnout Rates: Comparison of Measures Based on Voting-Age Population (VAP) and Voting-Eligible Population (VEP)

Group	VAP-Based Measure (1)		VEP-Based Measure (2)		Difference of (1) − (2) (3)	
A. African-American	49.896	(1.710)	59.702	(1.993)	−9.806[a]	(1.311)
B. White	55.114	(1.087)	56.929	(1.112)	−1.816[a]	(.339)
C. Difference of A − B	−5.217[b]	(1.929)	+2.773[b]	(1.955)	−7.991[c]	(1.057)

Note. Standard errors are in parentheses. The columns present the effect of adjusting male voter turnout rates for the ineligibility of disenfranchised felons. Column 1 reports voter turnout using the VAP-based measure, or the ratio of votes cast to VAP. Column 2 shows voter turnout using the VEP-based turnout measure, in which the denominator is reduced by the number of ineligible disenfranchised felons. The turnout rates are for males only in the election of 2000. Data on population and the number of men who voted were extracted from the U.S. Census Bureau's Current Population Survey Voter Supplement, and counts of the number of disenfranchised felons are from Manza, Uggen, and Britton (2001, apps. 1 and 2).

[a] Difference between turnout measures for the racial group.

[b] Difference between racial groups for the turnout measure.

[c] Difference-in-differences: relative racial difference between turnout measures.

Britton's counts of the number of disenfranchised felons, the author estimated VAP- and VEP-based turnout for the 2000 election. The calculations are limited to male voters; in effect, the assumption is that all disenfranchised felons are male. This assumption is reasonable because felons are overwhelmingly men. While the assumption biases the corrections upward, the very low rates of female involvement in serious crime suggests that any error is smaller than if the calculations included both genders.

Table 2 presents the results. Column 1 reports male VAP-based turnout rates, and consistent with conventional wisdom, the average voter turnout of African-American men is roughly 5 percentage points lower than that of white men. Column 2 displays VEP-based turnout rates that exclude Manza, Uggen, and Britton's (2001) tallies of disenfranchised felons from the VAP. The exclusion of ineligible felons implies an upward adjustment of nearly 2 percentage points in the average turnout rate of white men, and for African-American men, it is a stunning increase of nearly 10 percentage points. The VEP-based turnout reveals that, when appropriately measured as the number of votes cast among the eligible, state-level turnout rates for males in the 2000 election exhibited no racial disparity. Mismeasurement resulting from felon disenfranchisement, rather than the usual explanations of socioeconomic

differences, appears to explain the racial gap in VAP-based turnout measures.

To be clear, Table 2 does not show that, absent the disenfranchisement laws, felons would regularly vote and would thereby raise the turnout of African-American males. Instead, the VEP-based measure provides a more accurate indication of existing turnout rates among the eligible. The conventional wisdom that the voter participation rate of eligible African-American males is lower than that of eligible white males is incorrect. The participation rate of eligible African-American males is at least as high as that of eligible white males. The prevailing view that eligible African-American men participate at lower rates appears to be a misapprehension resulting from the widespread use of the VAP as a measure of the pool of eligible voters.

The estimates of Table 2 do not measure the counterfactual rate, the turnout rate that would prevail in the absence of felon disenfranchisement, but they offer some insight into its magnitude. If felons voted at the same rate as eligible citizens, the disenfranchisement of felons would reduce turnout by the amount of the double difference in Table 2. The "difference-in-differences," or relative adjustment across racial groups, is almost 8 percentage points. Because no one contends that felons vote more often than the rest of the population, the double difference is a crude upper bound on the amount by which felon disenfranchisement could reduce voter turnout.[33] It implies that the amount by which felon disenfranchisement reduces VAP-based turnout must be less than 7.991 percentage points.

The VEP-based turnout rates also suggest an alternative classification of the states. A possible objection to organizing the states according to the presence of a law disenfranchising ex-felons is that a single indicator variable poorly reflects the size of a state's disenfranchised population. Among the states permanently disenfranchising ex-felons, differences exist in the types of crimes that trigger disenfranchisement (Note 2002,

33. If the turnout of eligible voters is no less than that of felons, the difference between the VAP- and VEP-based turnout measures will be at least as large as the difference in VAP-based turnout between states with and without a disenfranchisement policy. The condition is $V/(P - F) - V/P \geq (V + \alpha F)/P - V/P$. Loosely speaking, the left-hand side of the inequality is the comparison drawn in Table 2, and the right-hand side is the comparison made in the regression analysis of Section 3. Rearrangement yields the condition $V/(P - F) \geq \alpha$.

pp. 1959–62) and in the ease with which voting rights are restored.[34] Differences in the stringency of the provisions imply that among states classified as disenfranchising ex-felons, the fraction of the population rendered ineligible by the law may vary. The binary variable for a prohibition on ex-felon voting may fail to identify the instances in which disenfranchisement has the greatest impact on voter turnout.

As an alternative, the state-level double-difference figures, which underlie the means of Table 2, identify the states where felon disenfranchisement could have the greatest impact on the turnout of African-American men.[35] States in which African-American men, relative to white men, have VEP-based turnout rates that are significantly higher than their VAP-based ones are where felon disenfranchisement disqualifies African-American men most disproportionately. States with the most disproportionate disqualification rates are plausibly those where any effect on the turnout rate of African-Americans is most likely to occur and is most readily detected. The amount of this double difference varied across states, and in seven states, it was more than 15 percentage points.[36] The empirical analysis in this paper is then repeated using a binary variable to denote those seven states in lieu of the categorization by status of disenfranchisement laws. The reclassification provides a second test of the impact of the ex-felon disenfranchisement.

3.3. Empirical Analysis of the Actual Impact on Turnout Rates

3.3.1. Estimation Strategy. This paper analyzes the effect of ex-felon disenfranchisement on voter participation using a difference-in-

34. In most states that permanently disenfranchise, the only means to restore voting rights is to obtain the governor's pardon, but others restore voting rights almost automatically in particular circumstances. For example, "Florida routinely grants clemency to first offenders and does not impose felony adjudication for some probationers who successfully complete their sentences" (Manza, Uggen, and Britton 2001, p. 18, n.15).

35. Since multiple years of state-level estimates of disenfranchised populations do not exist, it is not possible to regress voter turnout on the fraction of each state's population that is disenfranchised.

36. These states are Alabama, Florida, Kentucky, Mississippi, Texas, Virginia, and Wisconsin. This grouping has considerable overlap with the states that permanently disenfranchise ex-felons, but three differences exist. Wisconsin disenfranchises persons under correctional supervision (that is, incarceration, probation, and parole) but does not permanently disenfranchise ex-felons. Texas restored voting rights 2 years after release from correctional supervision over most of this period. The author's calculations from the Manza, Uggen, and Britton (2001) tallies reveal that these states have potentially large racial differentials in voter turnout owing to their policies of temporary disenfranchisement. Tennessee permanently disenfranchises ex-felons, but the author's calculation showed that the racial differential attributable to the disenfranchised ex-felon population is less than 2 percentage points.

differences-in-differences approach. Widely employed in economics,[37] the methodology uses three dimensions of comparison to identify a causal effect. As applied here, it compares the turnout of African-Americans and whites (the first difference) and of males and females (the second difference) in states with and without laws permanently disenfranchising ex-felons (the third difference). The first two sources of variation pinpoint the demographic group with the highest rate of disenfranchisement. Voter turnout rates of African-Americans are compared with those of whites because, as illustrated in Section 3.2, felony convictions disenfranchise African-Americans at significantly higher rates than whites. Similarly, the framework compares the voter participation of males to that of females because males are considerably more likely to be convicted of serious crimes than are females.[38] The third source of variation is the policy intervention: the law of permanent disenfranchisement. In this framework, African-American males in states that disenfranchise ex-felons are effectively a treatment group, while whites, females, and states that do not disenfranchise ex-felons are control groups.

States in the sample did not substantially alter their disenfranchisement policies during the period under study, and therefore the legal regimes exhibit no time-series variation.[39] The absence of longitudinal variation in the disenfranchisement policies implies that the regression equations cannot include state fixed effects because an indicator variable for the disenfranchisement law would be a linear combination of the state dummies. In lieu of state fixed effects, dummies representing the four census regional divisions are included in the regressions. Because regional voting patterns are unlikely to be time invariant, region-year interaction terms are also added as explanatory variables.

A key assumption for the validity of this approach is that unobservable determinants of voter participation vary only between regions, not between states within the same region. The assumption is a relatively strong one, as states within the same region often exhibit considerable variation along many dimensions. To address the issue of within-region variation, the regressions are conditioned on a set of state- and year-specific control observables. Although the inclusion of these control var-

37. Examples of the triple-differences methodology, but ones using time-series variation in the legal rule, include Buchmueller and DiNardo (2002, p. 286) and Gruber (1994, pp. 627–30).

38. See note 9.

39. See notes 19–22.

iables does not resolve the difficulty of within-region variation, it improves the plausibility of the causal inferences.

Despite its inadequacy, the triple-differencing strategy is a significant advance over the single-differencing approaches of prior studies. Some researchers used the presence of a law as the sole dimension of comparison in examining total state-level turnout, but such estimates conflate the law's impact with other systematic differences in voting behavior between states with and without these laws.[40] An advantage of the triple-differences methodology is that, by comparing turnout by race and gender within disenfranchising and nondisenfranchising states, the other systematic differences are arguably subtracted out and the causal effect is more credibly isolated. Similarly, other researchers examined only the group most affected, African-American males, but this approach conflates the law's impact with other race- and gender-specific determinants of voter turnout.[41] In the triple-differences framework, the use of whites and women as control groups removes these factors. The framework attributes any differential in the voting rates of African-American males between disenfranchising and nondisenfranchising states that remains after subtracting off the corresponding differences for whites and women to the ex-felon disenfranchisement policy.

Another econometric concern is that the presence of a felon disenfranchisement law may not be exogenous to voter participation and that hence estimates of the law's impact may be biased. For several reasons, any bias is not likely severe. First, these laws are generally quite old.[42]

40. Grose and Yoshinaka (2001) use official counts of ballots cast to compare total voter turnout in states that permanently disenfranchise ex-felons and those that do not, and they conclude that ex-felon disenfranchisement reduces total voter turnout. In addition to cross-sectional variation being their only source of identification, other aspects of their empirical methodology raise questions about their causal conclusion. First, Grose and Yoshinaka (2001) include as explanatory variables several measures that are likely endogenous to voter turnout, such as ex post measures of closeness of election contests. Second, the inferences of statistical significance may be incorrect because they fail to correct their standard errors for heteroskedasticity by clustering them by state. Third, they limit their sample to southern states, and Miles (2000, pp. 101–3, 144–45) demonstrated that in repeated cross sections of all 50 states, ex-felon disenfranchisement is not associated with lower average rates of total state-level voter turnout.

41. Hirschfield (1999) used a single cross section of individual-level data to compare the turnout rates of African-American males in states with and without laws disenfranchising felons.

42. "Disenfranchisement for such crimes had a long history in English, European, and even Roman law, and it was hardly surprising that the principle of attaching civil disabilities to the commission of crime appeared in American law as well. . . . States began to incorporate such provisions in their constitutions in the late eighteenth century" (Keyssar 2000, pp. 62–63).

Of the disenfranchisement laws included in the empirical analysis, the one adopted most recently, according to Alexander Keyssar's (2000) history of voting rights, dates from 1848.[43] Their age reduces the likelihood that these laws are themselves a product of present voting behavior. Second, many of these laws are found in state constitutions (Keyssar 2000, p. 63, table A.7). State constitutions are typically less easily amended than general legislative enactments, and therefore they are somewhat insulated from contemporary political pressure. Third, these laws are obscure. Until recently, these laws received little public attention.[44] Even offenders may be unaware that a felony conviction jeopardizes their right to vote (Harvey 1994, p. 1172; Itzkowitz and Oldak 1973, p. 733). Thus, felon disenfranchisement is plausibly exogenous to voter turnout rates.

If felon disenfranchisement laws were endogenous, the direction of bias would not be obvious. One possibility is that states may be more likely to adopt or retain disenfranchisement laws when legislators know that felons rarely vote and hence the cost of appearing "tough" on crime is low. In this instance, the laws might depress voter turnout, but the reductions would be difficult to detect since the laws would appear only in states where their impact is relatively small. This scenario appears unlikely because the conventional wisdom is that these laws lower participation rates. A more common assertion is that after the Civil War southern states adopted an array of voting restrictions designed to reduce African-American voter turnout and that felon disenfranchisement laws were one of these restrictions (for example, Dugree-Peason 2002, p. 362; Keyssar 2000, p. 162; Hench 1998, pp. 738–43; Shapiro 1993, pp. 537–38; Kousser 1984). If felon disenfranchisement laws correlate with other policies and attitudes that reduce the participation of African-Americans, particularly African-American males, the estimates could overstate the degree to which felon disenfranchisement lowers voter turnout. In this scenario, the endogeneity of disenfranchisement laws may lead to a false inference that these laws reduce voter turnout. As described in the next section, however, the estimates reveal no reduction in African-American voter turnout attributable to felon disenfranchisement laws.

43. Keyssar (2000, p. 63, table A.7) assigns the following dates to the relevant states: Alabama (1819), Florida (1838), Kentucky (1792), Mississippi (1817), Tennessee (1834), Texas (1845), Virginia (1830), and Wisconsin (1848).

44. Keyssar (2000, p. 63) notes that little debate accompanied their passage.

The triple-differences approach is implemented in regression equations of the form

$$\begin{aligned} v_{jtirg} = {} & \beta_{irg}(d_i \times d_r \times d_g) + \beta_{ir}(d_i \times d_r) + \beta_{ig}(d_i \times d_g) + \beta_{rg}(d_r \times d_g) \\ & + \beta_i d_i + \beta_r d_r + \beta_g d_g + \delta \mathbf{X}_{jtrg} + \gamma Z_{jt} + \varepsilon_{jtirg}. \end{aligned} \tag{1}$$

In equation (1), v_{jtirg} is the voter turnout of a demographic group in state j and year t, where i indexes the status of the disenfranchisement law, r indexes racial group, and g indexes gender. The dummy variable d_i takes the value one when the observation is for a state that disenfranchises ex-felons and is zero otherwise. The variables d_r and d_g denote indicators for African-American and male, respectively. The matrix $\mathbf{X}_{jtirg}$ contains control variables that are specific to the state, year, and demographic group. The control variables are the unemployment rate, real average weekly earnings, and the fraction of the demographic group aged 18–35, 36–65, and more than 65 years. The matrix $\mathbf{Z}_{jt}$ contains controls that are specific to state and year, but not to demographic group. It includes election-specific variables such as indicators for whether a presidential, senatorial, or gubernatorial office is contested in the election, as well as the percentage of the state population that is African-American. Political scientists have long recognized the importance of institutional restrictions on voting behavior (Rhine 1996, pp. 177–79; Mitchell and Wlezien 1995, pp. 186–88; Jackman 1987, pp. 411–13; Powell 1986, p. 25; Wolfinger and Rosenstone 1980, p. 73), and for that reason, state- and year-specific measures of voting regulations are also included. These regulations are whether or not the state permits mail-in registration; whether a voter must register in advance of election day, and if so, the number of days; and whether the voting registration expires after a number of years, and if so, the number of years. The percentage of the population that is African-American is added as an explanatory variable because minority turnout correlates positively with the size of the minority population (Waldfogel and Oberholzer-Gee 2001). Finally, ε_{jtirg} is the error term. Huber-White robust standard errors are clustered over states in order to control for serial correlation (Bertrand, Duflo, and Mullianathan 2002, p. 17).

The beta coefficients are the parameters of interest. The coefficient on the triple-interaction term, β_{irg}, is the estimate of the triple difference. It measures the difference in voter turnout for African-American men, relative to whites and women in disenfranchising states versus nondisenfranchising states. It captures the causal effect of ex-felon disenfranchisement on the voter participation of African-American men. Under the

hypothesis that ex-felon disenfranchisement disproportionately reduces the eligibility of African-American male voters, the coefficient on the triple-interaction term is predicted to be negative, or $\beta_{irg} < 0$.

The other beta coefficients also have interpretations. The term β_{rg} measures the degree to which voter turnout of African-American men differs from that of whites and women in states that do not permanently disenfranchise ex-felons. The term β_{ig} captures the difference in white males' voter participation relative to that of white females in states that disenfranchise versus those that do not, and β_{ig} reflects the difference in the turnout of African-American women relative to that of white women in states that disenfranchise versus those that do not. Because ex-felon disenfranchisement is thought to have little impact on the turnout of white and female voters, the prediction is that these coefficients are equal to zero. That is, for these coefficients, the null hypotheses are $\beta_{ir} = 0$ and $\beta_{ig} = 0$.

3.3.2. Empirical Results. For a first look at the impact of ex-felon disenfranchisement, triple differences of average voter turnout are examined. Tables 3 and 4 presents these means. In Table 3, the states are grouped according to the presence of a law permanently disenfranchising ex-felons. Columns 1–3 show the differences in turnout rates by race and disenfranchisement law for males, and columns 4–6 report the corresponding figures for females. The double differences for men and women are noted at the intersection of row C and columns 3 and 6, respectively. Column 7 presents the estimate of the triple difference.

The means show that on average the voter turnout rates of African-Americans are lower than those of whites. Row C shows that the racial gap in turnout rates appears greater for men than for women. The presence of a law disenfranchising ex-felons is associated with almost no variation in the racial gap. Column 3 shows that African-American males in disenfranchising states have voter turnout rates nearly 1.5 percentage points lower on average than their counterparts in states that do not disenfranchise ex-felons. For whites, a group predicted to be less affected by the policy, the difference is slightly larger at 1.8 percentage points. The double-difference estimate of +.3 of a percentage point shows that, in contrast to the prediction that the turnout of African-American male voters is disproportionately reduced by ex-felon disenfranchisement, the difference in their participation rates is slightly higher relative to that of white males. Column 6 reports the racial difference in average turnout rates for female voters. In contrast to the prediction that ex-felon dis-

Table 3. Triple Differences in Mean Rates of Voter Turnout: States Grouped According to the Presence of a Law Disenfranchising Ex-felons

	Males			Females			
Group	States Disenfranchising Ex-felons (1)	States Not Disenfranchising Ex-felons (2)	Difference of (1) − (2) (3)	States Disenfranchising Ex-felons (4)	States Not Disenfranchising Ex-felons (5)	Difference of (4) − (5) (6)	**Difference of (3) − (6)** (7)
A. African-American	42.419	43.903	−1.485	45.283	49.639	−4.357	
	(2.066)	(1.354)	(2.484)	(2.594)	(1.428)	(2.985)	
B. White	49.634	51.451	−1.817	49.561	52.407	−2.846	
	(1.182)	(.878)	(1.476)	(1.562)	(.962)	(1.840)	
C. Difference of A − B	−7.215	−7.548	+.333[a]	−4.278	−2.768	−1.510[b]	+1.843[c]
	(1.902)	(1.558)	(2.463)	(2.023)	(1.407)	(2.974)	(1.121)

Note. Data are the same as reported in Table 1. The three dimensions of comparison are race, gender, and the presence of a law disenfranchising ex-felons. Column 3 reports the differences within males, and column 6 reports differences within females. Column 7 shows the triple-difference estimate. Standard errors are in parentheses.

[a] Double difference for males.

[b] Double difference for females.

[c] Triple-difference estimate.

Table 4. Triple Differences in Mean Rates of Voter Turnout: States Grouped According to the Magnitude of the Difference between Turnout Measures Based on Voting-Age Population (VAP) and Voting-Eligible Population (VEP)

	Males			Females			
Group	Large Difference between VAP- and VEP-Based Measures (1)	Small Difference between VAP- and VEP-Based Measures (2)	Difference of (1) − (2) (3)	Large Difference between VAP- and VEP-Based Measures (4)	Small Difference between VAP- and VEP-Based Measures (5)	Difference of (4) − (5) (6)	**Difference of (3) − (6)** (7)
A. African-American	41.297	44.365	−3.068	46.294	49.410	−3.116	
	(1.584)	(1.393)	(2.110)	(2.579)	(1.420)	(2.944)	
B. White	50.622	51.147	−.525	51.379	51.831	−.452	
	(1.857)	(.734)	(1.997)	(1.999)	(.893)	(2.189)	
C. Difference of A − B	−9.325	−6.781	−2.543[a]	−5.085	−2.421[b]	−2.664	+.120[c]
	(1.874)	(1.556)	(2.436)	(1.529)	(1.465)	(2.117)	(1.466)

Note. Data are the same as reported in Table 1 with states organized according to the author's calculations from the tallies of the disenfranchised by Manza, Uggen, and Britton (2001). The three dimensions of comparison are race, gender, and the magnitude of the difference between the VAP- and VEP-based turnout measures. Column 3 reports the differences within males, and column 6 reports differences within females. Column 7 shows the triple-difference estimate. Standard errors are in parentheses.

[a] Double difference for males.

[b] Double difference for females.

[c] Triple-difference estimate.

enfranchisement is likely to have no effect on female voters of either race, the double difference for women, at 1.5 percentage points, is larger in absolute value than the double-difference estimate for men.

The estimated triple difference, presented in column 7, is positive and almost 2 percentage points. While all of the estimated differences are small in magnitude and none are statistically different from zero, the signs of the estimates are inconsistent with the prediction that disenfranchisement disproportionately reduces African-American male voter turnout. The positive sign of the triple difference in mean voter turnout does not support the claim of a disproportionate impact on the participation of African-American male voters.

Table 4 displays triple differences in average participation rates when states are grouped according to the magnitude of the racial gap between the VAP- and VEP-based turnout measures in 2000, as described in Section 3.2. This reclassification attempts to identify the states where the disenfranchisement rate of African-American males is most disproportionate and, hence, the states where an effect on turnout is most likely to occur. Columns 1 and 4 report mean turnout rates in the seven states where African-American males, relative to whites, have the largest difference between the VAP- and VEP-based turnout rates, and columns 2 and 5 report the mean rates in the other states. Row C shows that, consistent with Table 3, the average turnout of African-American voters is lower than that of white voters and that the difference between the races is greater among males than females. The double-difference estimate in column 3 shows that for African-American males relative to white males, the difference in voter turnout between states with the most disproportionate disenfranchisement rate and other states is about −2.5 percentage points. Column 6 gives the corresponding double difference for females, and it is almost identical to that for males. The estimated triple difference of +.1 in column 7 indicates that, relative to whites and women, average turnout rates of African-American men in states where felony convictions disenfranchise them at the most disproportionate rates are no different than their average turnout rates in other states. This triple difference shows that even when states are organized according to the extent to which disqualification rates are racially disproportionate, the state-level turnout of African-American male voters appears unaffected.

To better control for other influences on voter turnout, the triple differences were estimated in regressions in the form of equation (1). Table 5 reports the regression results when states are classified according

Table 5. The Impact of a Law Disenfranchising Ex-felons on State-Level Voter Turnout: Ordinary Least Squares Regression Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Disenfranchisement × African-American × Male (β_{irg})	1.843	1.789			1.611	1.802	.920
	(1.143)	(1.226)			(1.164)	(1.271)	(1.389)
Disenfranchisement × Male (β_{ig})	1.029	.864	2.710		.745	.933	.637
	(.660)	(.666)	(1.433)		(.614)	(.727)	(.678)
Disenfranchisement × African-American (β_{ir})	−1.511	−1.096		.926	−.561	−1.817	−2.525
	(2.522)	(2.675)		(2.489)	(2.546)	(2.719)	(2.722)
African-American × Male (β_{rg})	−4.780	−4.360			−4.365	−4.284	−3.751
	(.565)	(.634)			(.656)	(.786)	(.904)
Disenfranchisement (β_i)	−1.936	−.259	−2.111	.295	.280	−.050	1.935
	(2.202)	(1.715)	(2.630)	(1.699)	(1.707)	(1.685)	(1.684)
Male (β_g)	−.956	−1.228	−5.934		−1.205	−1.582	−1.646
	(.259)	(.733)	(.526)		(.745)	(.875)	(.896)
African-American (β_r)	−2.768	−.692		−4.719	−.825	−2.680	2.572
	(1.434)	(1.719)		(1.999)	(1.742)	(1.323)	(1.505)
p-Value on null $\beta_{irg} = 0$	.1193	.1571	.0704	.7130	.1786	.1780	.5137

p-Value on null $\beta_{irg} = -7.991$	<.0001	<.0001	<.0001	.0014	<.0001	<.0001	<.0001
Demographic controls?	N	Y	Y	Y	Y	Y	Y
Election characteristics?	N	Y	Y	Y	Y	Y	Y
Females included?	Y	Y	Y	N	Y	Y	Y
Whites included?	Y	Y	N	Y	Y	Y	Y
All years included?	Y	Y	Y	Y	Y	Y	1994–2000
All states included?	Y	Y	Y	Y	Maryland as disenfranchising	South only	Y
R^2	.5975	.6575	.6135	.6545	.6578	.6508	.6787
N	832	832	416	416	832	480	416

Note. Data are the same as reported in Table 1. Standard errors are in parentheses. Huber-White robust standard errors are clustered by state. The coefficients on the other explanatory variables are not reported in order to conserve space. The demographic controls are specific to state, year, gender, and race, and they are the unemployment rate, real average weekly earnings, and the fraction of the demographic cell aged 18–35, 36–65, and more than 65 years. The percentage of the state population that is African-American, which is state and year specific, is also included. Election characteristics are specific to state and year, and they are indicators for gubernatorial, senatorial, and presidential elections. The voting procedure variables are an indicator for whether mail-in registration is permitted; an indicator for whether a voter must register in advance, and if so, the number of days; and an indicator for whether a registration expires after a number of years, and if so, the number of years. All equations also include four indicators for the census regional divisions and division-year interaction terms.

to the presence of a law permanently disenfranchising ex-felons. In order to conserve space, the table reports only the beta coefficients of equation (1). The columns present several different specifications. In column 1, the only additional covariates are region effects and region-year interaction terms, and subsequent columns add the election and demographic characteristics. The regressions in columns 3 and 4 examine the sensitivity of the results to the use of whites and women, respectively, as control groups. Regressions reported in those columns present the results of double differences in which comparisons are drawn first across genders for African-African voters and then across races for male voters. Column 5 displays estimates when Maryland is reclassified as a disenfranchising state, and in column 6, the sample is limited to southern states. Finally, column 7 displays the estimated coefficients when the sample is limited to the most recent four elections. Table 5 also shows *p*-values from *F*-tests of two hypotheses: that the triple-difference term is equal to zero and that it is equal to the difference between the VAP- and VEP-based turnout measures calculated in Table 2.

The results of Table 5 do not show a disproportionate impact of ex-felon disenfranchisement on the state-level turnout of African-American men. The estimate of the triple-difference term, β_{irg}, is positive rather than negative, and in all but two columns, it is larger than 1 percentage point. In every estimated equation, the hypothesis that the estimated β_{irg} is equivalent to the potential impact of -7.991 is rejected.

The inclusion of other covariates does not much affect the point estimate of the triple difference, but it does affect some of the other beta coefficients. The introduction of the demographic and election characteristics substantially reduces the magnitudes of the coefficients on the disenfranchisement law indicator β_i and on the race indicator β_r. The sensitivity of the race coefficient to the presence of demographic controls is consistent with the findings of previous researchers.[45] The coefficient on the disenfranchisement variable and its interactions with race (β_{ir}) and gender (β_{ig}) are statistically insignificant.

A possible objection to the triple-difference methodology is that race or gender is not an appropriate comparison group. For example, the determinants of voting behavior may vary fundamentally across either race or gender such that comparisons across these dimensions are invalid.

45. Wolfinger and Rosenstone (1980, p. 71) found that after controlling for education and region, race had no independent effect on turnout probabilities in the 1970 election, and Verba and Nie (1972, pp. 170–71) reported that after controlling for various demographic variables, African-Americans had higher turnout rates. Also see Olsen (1970, pp. 686–90).

To test the sensitivity of the estimates to the choice of comparison group, column 3 excludes white voters from the sample and presents a double-difference estimate in which the comparisons are drawn across gender and within race. The estimate on the gender indicator, β_g, shows that African-American men have lower rates of turnout on average than African-American women. However, the positive coefficient on the double-difference term, β_{ig}, implies that the differential in the turnout of African-American males between disenfranchising states and nondisenfranchising states is smaller than that for African-American women. Similarly, the equation in column 4 excludes female voters and presents a double difference across race and within gender. In this specification, the double difference, β_{ir}, is also slightly positive. It reveals that, after controlling for other explanatory variables, the difference in the turnout rate of African-American males between disenfranchising and nondisenfranchising states is nearly the same as that of white males. Like the triple-difference estimates, the double differences provide no evidence that ex-felon disenfranchisement has a disproportionate impact on the turnout of African-American males.

As discussed in Section 3.1, classification of state disenfranchisement laws entails some subjectivity. For example, Maryland, which during the sample period permanently disenfranchised only upon a second felony conviction, and Texas, which restored voting rights 2 years after sentence completion, were classified as not permanently disenfranchising all ex-felons. Column 5 explores the robustness of the estimates to varying these categorizations. It reports an equation in which Maryland is recoded as a state disenfranchising ex-felons, and the triple-difference estimate at +1.6 is very close to the baseline specification in column 2. Also, recoding Texas as a state permanently disenfranchising ex-felons produces an estimate that is similar. In an equation not reported in Table 5 in order to conserve space, the recoding of Texas produces a triple-difference estimate of +1.812 (standard error = 1.151).

The concentration of the permanently disenfranchising states in the South raises the concern that the effect of ex-felon disenfranchisement may not be separately identified from regional effects. To test whether regional differences in voting patterns bias the estimates, column 6 reports a regression in which the sample is limited to southern states. The estimate of the triple difference, +1.8, is remarkably close to the estimate found in the full sample.

Column 7 explores the sensitivity of the estimates to developments over time. The impact of ex-felon disenfranchisement might increase

Table 6. Differences in Voter Turnout Rates with States Grouped According to the Magnitude of the Difference between Measures of Voter Turnout Based on Voting-Age Population and Voting-Eligible Population: Ordinary Least Squares Regression Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Large Difference × African-American × Male (β_{irg})	.120	.368			1.738	.270
	(1.494)	(1.584)			(1.232)	(1.241)
Large Difference × Male (β_{ig})	−.073	−.226	.396		−.320	−.336
	(.552)	(.551)	(1.740)		(.671)	(.542)
Large Difference × African-American (β_{ir})	−2.664	−2.371		−1.610	−2.904	−2.704
	(2.158)	(2.393)		(2.453)	(2.453)	(2.421)
African-American × Male (β_{rg})	−4.360	−3.978			−4.183	−3.641
	(.471)	(.590)			(.757)	(.872)
Large Difference (β_i)	−.434	1.107	−1.456	.854	.650	1.968
	(2.041)	(1.543)	(2.731)	(1.713)	(1.667)	(1.621)
Male (β_g)	−.684	−1.038	−5.420		−1.111	−1.380
	(.310)	(.796)	(.651)		(.905)	(.958)
African-American (β_r)	−2.421	−.410		−4.083	−2.340	2.661
	(1.494)	(1.750)		(2.034)	(1.486)	(1.453)
p-Value on null $\beta_{irg} = 0$	.9364	.8181	.8218	.5175	.1800	.8298
p-Value on null $\beta_{irg} = -7.991$	<.0001	<.0001	.0001	.0154	.0001	<.0001

Demographic controls?	N	Y	Y	Y	Y	Y
Election characteristics?	N	Y	Y	Y	Y	Y
Females included?	Y	Y	Y	N	Y	Y
Whites included?	Y	Y	N	Y	Y	Y
All years included?	Y	Y	Y	Y	Y	1994–2000
All states included?	Y	Y	Y	Y	South only	Y
R^2	.5993	.6579	.6119	.6547	.6513	.6782
N	832	832	416	416	480	416

Note. Data are same as reported in Table 1. Standard errors are in parentheses. Huber-White robust standard errors are clustered by state. The coefficients on the other explanatory variables are not reported in order to conserve space. The demographic controls are specific to state, year, gender, and race, and they are the unemployment rate, real average weekly earnings, and the fraction of the demographic cell aged 18–35, 36–65, and more than 65 years. The percentage of the state population that is African-American, which is state- and year-specific, is also included. Election characteristics are specific to state and year, and they are indicators for gubernatorial, senatorial, and presidential elections. The voting procedure variables are an indicator for whether mail-in registration is permitted; an indicator for whether a voter must register in advance, and if so, the number of days; and an indicator for whether a registration expires after a number of years, and if so, the number of years. All equations also include four indicators for the census regional divisions and division-year interaction terms.

over time for two reasons. First, to the extent that the number of disenfranchised ex-felons correlates with the number of persons imprisoned, the dramatic increase in incarcerations in recent decades suggests that the disenfranchised population has grown over time. If so, disenfranchisement's impact on turnout may be apparent only in more recent elections. Second, because the propensity to vote increases with age, ex-felon disenfranchisement may constrain behavior only at later dates, when persons who were convicted of felonies in their youth attain higher ages. As a larger pool of disenfranchised ex-felons ages, the effect on turnout may become more pronounced. The regression in column 7 tests this prediction by limiting the sample to the most recent four elections. The triple-difference estimate is smaller but remains positive and statistically insignificant. Even in the time periods in which the effect of disenfranchisement should be greatest, no measurable impact is detected.

Table 6 repeats the regression analysis using states reclassified according the magnitude of the racial difference between the VAP- and VEP-based turnout measures. As described in Section 3.2, the reclassification identifies the states where felon disenfranchisement has disqualified African-American men at the most disproportionate rates. The point estimates change as a result of the reclassification, but the inferences drawn from them remain unchanged. The estimated triple difference in column 1 is less than a tenth of its size in the previous table, and when controlling for demographic and election characteristics in column 2, it is roughly a fifth of its size in the prior table. The estimates suggest that felon disenfranchisement has no disproportionate effect on the voter participation of African-American males. Limiting the comparison groups to women in column 3 has scant effect on the results. The negative coefficient in column 4 is consistent with the negative double-difference estimate for men reported in Table 4. As the slightly positive triple difference in column 2 indicates, the corresponding double difference for women is still more negative,[46] and thus a causal interpretation should not be attached to the estimate of column 4. When the sample is limited to southern states, the triple-difference coefficient becomes more positive, and when the sample is restricted to the most recent four elections, it is smaller but remains positive. In sum, a comparison of states where felon disenfranchisement disqualifies African-American

46. If men are excluded and the regression is estimated for women, a group with a low incidence of felony conviction and hence a low rate of disenfranchisement, the regression yields a double difference of −2.287 (standard error = 2.411).

men most disproportionally compared with other states reveals no effect on the voter turnout of African-American males.

3.4. Conclusion to Empirical Analysis

The empirical results indicate that ex-felon disenfranchisement does not reduce the state-level turnout of African-American males. Summary statistics show that the average turnout of African-American male voters in states that disenfranchise ex-felons does not differ from the rates in states that do not disenfranchise, and the triple differences indicate no disproportionate impact on the state-level turnout of African-American males. When regression analysis holds constant other factors influencing voter participation, these patterns are unaltered. When the sample is limited to the South and when the classification of the legal rule is varied, no evidence of a disproportionate impact on state-level turnout rates is found. Despite the presumed growth in disenfranchised populations in later years, no effect is detected when the sample is restricted to more recent elections. The estimates suggest that the disenfranchisement of ex-felons has no impact on state-level rates of voter participation.

The failure to find an effect on voter turnout despite the considerable fraction of the VAP rendered ineligible by felon disenfranchisement presents a puzzle. How can large portions of otherwise eligible populations be disqualified from voting and yet no effect be seen on voter turnout rates? Two possibilities exist. First, disenfranchised felons are drawn disproportionately from the pool of potential voters who choose not to cast ballots. The same demographic and socioeconomic factors that correlate with participation in criminal activity, and by implication with disenfranchisement, also correlate with the decision to forgo voting. Criminologists have noted that persons who possess lower levels of income and education and who face greater risks of unemployment disproportionately undertake criminal activity. Participants in crime are also more likely to be young, male, and African-American.[47] Political scientists have found that these same characteristics correlate with the probability of voting. Voters on average have higher levels of income

47. For income levels, see Freeman (1995, pp. 178–82). For education levels, see Lochner (1999, pp. 23–31). For unemployment risk, see Freeman (1995, pp. 171, 178–82). Also, according to Gottfredson and Hirschi (1990, p. 165), "The most significant employment-crime fact is the tendency of people who commit crime to have unstable job profiles—that is, to have difficulty finding jobs and keeping them." For age, see Laub (1987, pp. 60–67). For gender, see Wilson and Herrnstein (1985). For race, see Laub (1987, pp. 60–65).

and education and are less likely to experience unemployment. They are also more likely to be older and white.[48] Political scientists identified these predictors of voter turnout decades before the recent growth in incarceration, and therefore the practice of ex-felon disenfranchisement did not itself bias their findings. It remains an unfortunate but well-established fact that the average levels of education, employment, income, and wealth of African-Americans remain below those of whites.[49] The summary statistics in Table 2 show that these patterns are borne out in the sample studied here.

The considerable overlap in the demographics of criminality and of voter nonparticipation suggests that ex-felon disenfranchisement may not cause lower rates of voter turnout among African-American males. Instead, the same factors that cause African-American males to be disenfranchised at disproportionate rates may also cause them, in the absence of a disenfranchisement law, to choose not to vote at higher rates. The empirical results of this paper are consistent with this view. The fact that ex-felons belong to demographic groups that choose to participate at low rates is the most plausible explanation for the finding that ex-felon disenfranchisement has no effect on state-level voter turnout.

A second but less persuasive reason why felon disenfranchisement has no effect on voter participation may be the laxity of enforcement. Occasional press reports suggest that, at least in some jurisdictions, ineligible ex-felons have little trouble registering to vote and casting ballots.[50] Disenfranchisement laws often furnish no mechanisms for imple-

48. For income levels, see Wolfinger and Rosenstone (1980, pp. 20–22), Hadley (1978, pp. 150–51), Verba and Nie (1972, p. 125). For education levels, see Wolfinger and Rosenstone (1980, pp. 17–19), Hadley (1978, pp. 150–51), Campbell et al. (1960, pp. 475–99). For unemployment risk, see Piven and Cloward (1988, p. 162). For age, see Wolfinger and Rosenstone (1980, pp. 38–41), Hadley (1978, pp. 150–51), Campbell et al. (1960, p. 494). For race, see Gilliam (1985), Abramson and Claggett (1984, p. 721).

49. For education levels, see Thernstrom and Thernstrom (1997, pp. 190, 192–93). For employment, see Thernstrom and Thernstrom (1997, pp. 242–47). For wealth levels, see Thernstrom and Thernstrom (1997, pp. 194–98).

50. McBride and Umhoefer (2001, p. 1A) reported that 30 ineligible felons, including three convicted of homicide, voted in an election in which an alderman won a seat by 18 votes. Kidwell and Arthur (2000, p. 1B) quoted one ineligible felon claiming "I've been voting ever since voting has been voting . . . I'm a poll worker," and they noted that convictions of improperly registered felons ranged from drunk driving to rape and murder. Timberg and Pae (1998, p. A01) reported that an audit of voter registries found more than 11,221 ineligible felons registered in Virginia. Mannies (1992, p. 1A) found that 147 people in prison and 492 on parole registered to vote in St. Louis. Also see Morelli (2002, p. 1), Bridges (1998, p. B1), Freedberg (1998, p. 1A), Scallan (1998, p. B8), McPhee (1995, p. A1).

menting the prohibition.[51] Even in states where the criminal justice agencies deliver names of ineligible ex-felons to the administrators of voting rolls, the system does not address the movement of ex-felons across states.[52] Systematic evidence on enforcement mechanisms and their operation is simply nonexistent.

A difficulty in distinguishing the enforcement and abstention hypotheses is the absence of a social scientific survey containing information on both criminal history and voting behavior. Despite this unobservable, Manza, Uggen, and Britton (2001, p. 18) used existing social scientific surveys to predict the participation rates of felons, and they estimated that the average turnout rate of felons could be as high as 32 percent.[53] Although low relative to the observed rates of turnout among the law abiding, these estimates may be optimistic. First, prior to their disqualification, felons appear to vote infrequently. According to one state's assistant attorney general, only 10 percent of felons had registered to vote prior to their incarcerations (Amon 2003, p. A6). If the registration rate is indeed 10 percent, the turnout rate is even lower because not all the registered cast ballots on election day. Second, if enforcement is not rigorous and, as some critics contend, felons are unaware of their disqualification (Harvey 1994, p. 1172; Itzkowitz and Oldak 1973, p. 733), the frequency with which they register to vote accurately reflects the rates at which they would participate absent the restriction. These rates appear extremely low. When media outlets have reviewed voting rolls for the presence of ineligible voters, the number of ex-felons they have found registered is a miniscule fraction of the total number of

51. "Although most states disqualify persons convicted of certain offenses from voting, it is uncertain as a practical matter, how many technically disenfranchised persons actually continue to vote. In many states, there appear to be no statutory provisions for removing the names of convicted persons from voting lists" (Special Project 1970, p. 980).

52.. "The enforcement of a permanent disqualification for felony conviction is a difficult administrative task. Felons are easily identified for as long as they remain in the justice system. Once discharged, they cannot as readily be barred from participation in the electoral process. As recent events in Florida demonstrate, enforcement is especially difficult, if not impossible, when felons relocate to a state that denies voting rights to felons indefinitely" (Hansen 2001, p. 2).

53. Even some opponents of felon disenfranchisement laws perceive these laws as having little effect. "Eric C. Olson, deputy director of the Center for Voting and Democracy in Takoma Park, said it is unclear how may ex-offenders would actually register if they regained their voting rights. 'Personally, I don't think you are going to see the outcome of elections change because of this'" (Craig 2002, at 1B).

disenfranchised ex-felons.[54] Third, past experience with the relaxation of certain registration requirements shows that they have modest effects on voter turnout. Some political scientists speculate that the purportedly onerous registration requirements do not deter people who prefer to vote (for example, Teixeira 1992, pp. 101–45; Hadley 1978, pp. 22–25). While the evidence is piecemeal, the balance of it weighs in favor of the view that ex-felons choose not to vote in the absence of a voting restriction.

4. IMPLICATIONS FOR THE JUSTIFICATIONS AND CRITICISMS OF EX-FELON DISENFRANCHISEMENT

The finding that despite high disqualification rates, ex-felon disenfranchisement has no discernible impact on voter turnout necessitates a reevaluation of both the justifications and criticisms of the policy. As with any criminal sanction, four rationales are typically advanced for ex-felon disenfranchisement: retribution, deterrence, incapacitation, and rehabilitation. The first rationale is deontological in that it pertains to delivery of just deserts, while the last three are consequentialist in that they relate to the effect of the sanction on the offender's behavior. Turning first to retribution, some critics question its validity as a policy rationale,[55] but judicial opinions often point to retributivist rationales. In particular, they argue that the loss of the right to take part in the selection of public officials is a fitting societal response to persons who commit serious crimes.[56] Although proponents note that disenfranchisement re-

54. The 11,221 ineligible but registered felons discovered by Timberg and Pae (1998, p. A01) represent less than 1 percentage point of Manza, Uggen, and Britton's (2001) estimated number of disenfranchised felons in Virginia. After an investigation of voting by felons in 12 Florida counties in the 2000 election, one newspaper projected that perhaps as many as 5,000 ineligible felons statewide cast ballots (*New York Times* 2000, p. A10). Even if this projection is correct, it represents less than 1 percent of Manza, Uggen, and Britton's estimated number of disenfranchised felons in Florida (Manza, Uggen, and Britton 2001, app. 1).

55. Itzkowitz and Oldak (1973, p. 736) term retribution an "anachronism" that "runs counter to modern emphasis on rehabilitation." But other analysts of the criminal law have noted the robustness of retribution as a justification for criminal sanctions. For example, Cotton (2000) describes efforts by proponents of the retributivist view to include it as an express rationale in state criminal codes.

56. *Kronlund v. Honstein* (327 F. Supp. 71, 73 [N.D. Ga. 1971]) describes the state interest in removing from the electoral process "persons with proven anti-social behavior whose behavior can said to be destructive of society's aims," and *Green* (380 F.2d 445, 451) cites John Locke for the proposition that lawbreakers abandon their right to participate in the social compact.

sults from voluntary participation in criminal activity,[57] opponents increasingly suggest that many of the disenfranchised do not deserve the sanction. They question the volitional character of criminality and instead attribute racial disparities in rates of criminal activity to discrimination in the content and administration of criminal law.[58]

According to the deterrence rationale, would-be felons prefer to vote, and the threatened loss of this utility discourages the commission of felonies. Critics reply that the "arbitrary" (Clark 1989, p. 835; Du Fresne and Du Fresne 1969, pp. 125–25) and "disproportionate" (Shapiro 1993, p. 563; Note 1989, p. 1307) nature of the policy distorts incentives for deterrence. Yet, if the felon's privately optimal choice is not to vote, a restriction on voting inflicts no reduction in his utility. Without a utility loss, disenfranchisement neither deters nor distorts incentive for compliance.

Similarly, disputes over the implications for rehabilitation have less bearing if ex-felons prefer not to cast ballots. Some critics contend that disenfranchisement makes social reintegration more difficult because it damages ex-felons' self-esteem and attenuates their connections to the duties of citizenship (G. Fletcher 1999, p. 1907; Itzkowitz and Oldak 1973, p. 732). Others believe that it may actually increase recidivism (Shapiro 1993, p. 562). However, if ex-felons are drawn from socioeconomic populations that choose not to vote, the inability to vote would not distinguish them from their peer group. In this circumstance, disenfranchisement would neither enhance nor inhibit rehabilitation.

The third consequentialist rationale for disenfranchisement is the need to incapacitate ex-felons in order to prevent future harms. Felons may use their votes to retaliate against the authorities who convicted them or to weaken the substance and administration of the criminal law (*Green,* 380 F.2d 445, 451–52). Their past criminality may predict future

57. *Wesley* (605 F. Supp. 802, 813) observes that "[f]elons are not disenfranchised based on any immutable characteristic, such as race, but on their conscious decision to commit an act for which they assume the risks of detection and punishment."

58. For example, in *Farrakhan v. Washington* (338 F.3d 1009 [9th Cir. 2003]), the Ninth Circuit denied the state's motion for summary judgment and found that a statistical study of racial disparities in the criminal justice system raised a material issue of fact about the "causal connection" between a felon disenfranchisement policy and its disproportionate racial impact under section 2 of the Voting Rights Act. Also, G. Fletcher (1999, p. 1897) claimed that prison populations "consist largely of drug users, and among them are an overwhelming number of blacks." Keyssar (2000, p. 307) stated that the criminal justice system treats minorities in a discriminatory fashion, and Shapiro (1993, p. 557 n.107) asserted that "there is ample evidence of racial discrimination throughout the criminal justice system."

participation in electoral fraud (*Kronlund,* 327 F. Supp. 71, 73). Opponents of disenfranchisement contend that the state has other means of preventing electoral fraud (Shapiro 1993, p. 561; Note 1989, p. 1304; Itzkowitz and Oldak 1973, p. 739; Reback 1973, p. 855), and they believe that ex-felons are unlikely to band together to form a voting bloc or to engage in electoral crimes at higher rates (Keyssar 2002, p. 303; Reback 1973, p. 854–55). They also argue that ex-felons are not more likely to favor corrupt candidates or more lenient criminal codes (Note 1989, p. 1303; Itzkowitz and Oldak 1973, p. 737) and that the exclusion of a class of voters because of the anticipated substance of their votes is unconstitutional.[59] However, the absence of a relationship between disenfranchisement and voter turnout indicates that for most statewide elections, whether ex-felons are permitted to vote has little consequence. With meager participation by ex-felons where they are eligible, disenfranchisement will not systematically affect election outcomes.[60] For most elections, the incapacitation of ex-felons is not necessary to preserve the "purity of the ballot box" (*Washington v. State,* 75 Ala. 582, 585 [1884]) against an influx of would-be voters.

In the absence of a discernible effect on state-level turnout rates, ex-felon disenfranchisement laws may serve a different consequentialist purpose. Occasional reports of felons attempting to organize politically[61] suggest that these laws may be necessary to prevent periodic instances of criminal enterprises using the political system to promote their ends. Under this explanation, disenfranchisement insulates the electoral system, not against many would-be participants, but against a few. The restriction is a safeguard against persons whose past behavior identifies

59. *Carrington v. Rush*, 380 U.S. 89, 94 (1965), holds that a class of voters may not be "fenced out" on the basis of expectations of how the class will vote.

60. These results are consistent with the findings of Miles (2000, pp. 109–11, 150). His estimates show that the presence of an ex-felon disenfranchisement law does not correlate with the probability of electing a Republican senator or governor.

61. For example, in Massachusetts, a state that previously did not disenfranchise incarcerated felons, an inmate serving a life sentence for first-degree murder attempted to form a political action committee to "influence debates on prison policy and criminal justice issues" (Dowdy 1997, p. B7). In Chicago, 21st Century Vote, a political action committee, was linked to Larry Hoover, a convicted murderer and leader of the Gangster Disciples. The political action committee organized voter registration drives, backed two unsuccessful aldermanic candidates, and hoped to alter the operation of the Prisoner Review Board in order to obtain Hoover's release. In a 1993 conversation recorded by federal authorities, Hoover instructed an associate to mobilize voters: "They got to get 'em out and vote. We got the power to put anybody in [office] that we wish" (Tyson 1997, p. 4; Tyson 1996, p. 1; Kass and Papajohn, 1993, p. 1A).

their motives and judgment as suspect, and a past felony conviction may provide a rough indication of questionable intent.[62] This rationale might also explain disenfranchisement's persistence. The policy could offer periodic benefits by safeguarding the electoral process, and because few ex-felons choose to vote, its social cost is low.

The safeguard rationale is subject to two criticisms. First, while the costs of ex-felon disenfranchisement might be low, it may not be the least costly method of achieving its goal. A more finely-tuned restriction, such as a ban on holding public office, might be sufficient to thwart unrepentant criminals who are seeking to influence the political system. A second criticism disputes that the social costs of the policy are low. Even if few ex-felons vote, many are disqualified, and ineligibility itself sends a negative message.[63] Moreover, toleration of the racial disparity in disqualification rates might reflect "selective sympathy and indifference" in governmental decision making.[64] Yet racial disparity in disqualification rates does not necessarily imply that, net of benefits, minority communities are disproportionately burdened by these laws. If voting by felons poses particular risks to these communities, the benefits of felon disenfranchisement might also accrue to them.[65]

This paper has not measured all of the parameters necessary to weigh the full social costs and benefits of ex-felon disenfranchisement. How-

62. Keyssar (2000, p. 63) describes one idea that motivated disenfranchisement laws as "simply, that a voter ought to be a moral person. . . . [T]he difficulty of imposing a morality test was manifest . . . but men who had been convicted of crimes were easy to distinguish and label." The problem is comparable to an administrative purge. For example, Foner (1988, pp. 185–86) describes the difficulty of defining loyalty to the Union for the purposes of allocating political power in the post–Civil War South. Also see Steinacker (2003).

63. According to Fletcher (1999, p. 1898), "Those who have served their time are left with the message that they are inherently unreliable members of the democracy," and Du Fresne and Du Fresne (1969, p. 131) assert that felon disenfranchisement "amounts to a state-created 'second-class citizenship.'"

64. Brest (1976, pp. 7–8) defines "selective sympathy and indifference" as "the unconscious failure to extend to a minority the same recognition of humanity, and hence the same sympathy and care, given as a matter of course to one's own group."

65. Gaziano (2001) speculates that "it could be argued that those communities that currently have the highest level of state disenfranchisement are the most protected by these laws and would be [the] most adversely affected by the vote of 'unreformed' convicts in their communities." Kennedy (1994, pp. 1273–75) described how assessing the impact of law enforcement policies on minority groups is difficult because a policy will harm some minorities and benefit others. For example, in his dissent in *City of Chicago v. Morales* (527 U.S. 41, 98–101 [1999]), Justice Thomas stated that the residents of crime-ridden communities favored the enforcement in their neighborhoods of a challenged antiloitering ordinance.

ever, the estimates reveal that a widely held belief about the impact of these laws, that they reduce the voter turnout of African-American males at the state level, is incorrect. The impact of these laws on turnout appears to be more modest than previously believed, and it suggests that the function performed by these laws may be narrower in scope than previously thought.

5. CONCLUSION

The recent growth in incarceration has stimulated interest in the magnitude and demographics of disenfranchised felons, as well as renewed calls for an end to the practice. This paper examined the impact of ex-felon disenfranchisement on rates of voter turnout. Recent estimates of the disenfranchised population show that these laws render African-American voters ineligible at disproportionate rates. Despite the racial disparity in disqualification rates, the empirical tests revealed that the presence of a law permanently disenfranchising ex-felons has no effect on the state-level turnout rates of African-American men relative to those of whites and females. Even when states with highly disproportionate rates of disenfranchisement are compared with other states, no impact on voter participation is detected. The absence of an effect is consistent with the view that on average felons belong to demographic groups that, although eligible to vote, infrequently exercise that right. The results indicate that the conventional wisdom that disenfranchisement depresses state-level voter turnout is incorrect.

APPENDIX

Table A1. Felon Voting Restrictions, 1984–2000

State	Prisoners	Parolees	Probationers	Ex-felons
Alabama[a]	Y	Y	Y	Y
Alaska	Y	Y	Y	
Arkansas[a]	Y	Y	Y	
Arizona	Y	Y	Y	2d felony
California[a]	Y	Y		
Colorado	Y	Y		
Connecticut[a]	Y	Y	Y	
District of Columbia[a]	Y			
Delaware	Y	Y	Y	Y
Florida[a]	Y	Y	Y	Y
Georgia[a]	Y	Y	Y	
Hawaii	Y			

Table A1. *continued*

State	Prisoners	Parolees	Probationers	Ex-felons
Iowa	Y	Y	Y	Y
Idaho	Y	Y	Y	
Illinois[a]	Y			
Indiana[a]	Y			
Kansas	Y	Y	Y	
Kentucky[a]	Y	Y	Y	Y
Louisiana[a]	Y			
Maine				
Massachusetts				
Maryland[a]	Y	Y	Y	2d felony
Michigan[a]	Y			
Minnesota	Y	Y	Y	
Mississippi[a]	Y	Y	Y	Y
Missouri[a]	Y	Y	Y	
Montana	Y			
North Carolina[a]	Y	Y	Y	
North Dakota	Y			
Nebraska	Y	Y	Y	
Nevada	Y	Y	Y	Y
New Hampshire	Y			
New Jersey[a]	Y	Y	Y	
New Mexico	Y	Y	Y	Y
New York[a]	Y	Y		
Ohio[a]	Y			
Oklahoma[a]	Y	Y	Y	
Oregon	Y			
Pennsylvania[a]	Y			
Rhode Island	Y	Y	Y	
South Carolina[a]	Y	Y	Y	
South Dakota	Y			
Tennessee[a]	Y	Y	Y	Pre-1986
Texas[a]	Y	Y	Y	2 years
Utah				
Vermont				
Virginia[a]	Y	Y	Y	Y
Washington	Y	Y	Y	Pre-1984
Wisconsin[a]	Y	Y	Y	
West Virginia	Y	Y	Y	
Wyoming	Y	Y	Y	Y

Note. A "Y" indicates that the group is disenfranchised. Arizona and Maryland permanently disenfranchise upon the second rather than first felony conviction. Delaware and Texas restore voting rights 5 and 2 years, respectively, after completion of a felony sentence.

[a] The state is included in the sample.

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