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Author(s): Melanie Bowers and Robert R. Preuhs

Source: *Social Science Quarterly*, Vol. 90, No. 3 (SEPTEMBER 2009), pp. 722-743
Published by: Wiley
Stable URL: http://www.jstor.org/stable/42940613
Accessed: 26-02-2018 19:52 UTC

Collateral Consequences of a Collateral Penalty: The Negative Effect of Felon Disenfranchisement Laws on the Political Participation of Nonfelons*

Melanie Bowers, *Michigan State University*

Robert R. Preuhs, *Metropolitan State College of Denver*

Objective. Felon disenfranchisement (FD) policies are said to not only prohibit (ex)felons from voting, but also reduce the political influence of particular groups that are most affected by FD laws. This study tests several hypotheses regarding the role of socialization on individual-level political participation to examine the claims that nonfelons' probability of voting is reduced by strict FD laws. *Methods.* The study uses multilevel modeling and two separate individual-level data sets of those registered to vote to examine the effect of FD laws on the probability of voting. *Results.* The findings demonstrate that strict FD laws reduce the probability of voting for blacks, but not whites, while the results are mixed for several other demographic groups. *Conclusions.* Beyond the direct removal of ex-felons from the voter pool, FD policy can undermine the mechanism of political socialization, leading to potentially greater biases in the electoral arena than previously thought.

The growing literature on felon disenfranchisement laws, or laws prohibiting those convicted of a felony from voting whether during their sentence or beyond, has established that these laws come with a broad set of political implications. Beyond the well-established disparity of the effects of direct restrictions on ex-felons' voting between whites and racial/ethnic minority groups, emerging evidence suggests that disenfranchisement also creates indirect effects with consequences for minority participation, representation, and broader political outcomes. Specifically, previous studies found that felon disenfranchisement can dilute the votes of minorities in communities with high concentrations of felons (Clement and Keough, n.d.; King and Mauer, 2004), alters the outcomes of redistricting schemes

*Direct correspondence to Robert R. Preuhs, Department of Political Science, Metropolitan State College of Denver, Campus Box 43, PO Box 173362, Denver, CO 80217-3362 ⟨rpreuhs@mscd.edu⟩. Authors are listed in alphabetical order. The data and coding information used in this article are available to the public on the corresponding author's webpage ⟨http://clem.mscd.edu/~rpreuhs⟩. The authors thank Joanne Belknap, Cathy Comstock, and Claudia Van Gerven, as well as the editor and the anonymous reviewers, for helpful comments at various stages of the project.

SOCIAL SCIENCE QUARTERLY, Volume 90, Number 3, September 2009

(Karlan, 2004), changes the outcomes of presidential and congressional elections, and introduces bias into the partisan balance (Manza and Uggen, 2002; Ochs, 2006). These findings provide not only a broader empirical understanding of the effects of felon disenfranchisement laws, but also alter the nature of the normative policy debate.

The studies above addressed the direct effects of felon disenfranchisement (FD) policy on felon and ex-felon political participation, and the indirect effects of representation at the aggregate level; however, the implications of FD for nonfelons is less understood, but potentially holds great significance for the normative debate surrounding this policy. This study addresses the issue of the consequences of FD through an empirical analysis of the effects of FD on nonfelons' probability of voting in national elections. Specifically, the analysis tests several hypotheses that predict that nonfelons will be less likely to vote in states with severe FD policies compared to those in states with less severe FD policies. Using two individual-level data sets and employing multilevel modeling techniques, which account for individual and contextual factors, the analysis reveals that FD policies tend to dampen the probability of voting among blacks, which is also the group most likely to be disproportionately directly affected by FD policies. In addition, there is some evidence that urbanization and income effects become more pronounced factors in states with the most severe FD policies.

Felon Disenfranchisement, Political Socialization, and Participation of Nonfelons

One of the most prominent and consistent findings in FD literature is that these laws produce a disproportionate effect on black communities. Minorities in general are subject to the direct effects of a prohibition on voting more often and with greater severity because they are disproportionately arrested, convicted, and subsequently denied the right to vote (Chin, 2002; Fellner and Mauer, 1998; Goldman, 2004; Karlan, 2004; King and Mauer, 2004; Preuhs, 2001). The Department of Justice estimates that 28.5 percent of black men will go to prison in their lifetime, a rate that is six times higher than that of white men, and about 30 percent of black Americans will be deprived of the right to vote during their lifetime (see Fellner and Mauer, 1998). Over time, it has been estimated that 40 percent of the next generation of black men may be permanently disenfranchised in states with lifetime restrictions (Fellner and Mauer, 1998). This disproportionate direct effect of FD laws on African Americans has led scholars to examine the indirect consequences of FD on the political power of minority communities.

The emerging studies of the indirect consequences of FD laws suggest that while FD laws exact restrictions directly on felons, these restrictions tend to create political externalities by removing large portions of the electorate and

thus lowering overall rates of political participation in communities within what already can be considered politically marginalized groups. For instance, Ochs (2006) examined the overall rates of disenfranchisement using a state-level analysis to demonstrate that while the severity of disenfranchisement policy was positively associated with both white and black disenfranchisement rates, the effect on black disenfranchisement rates was significantly greater than the effect on white disenfranchisement rates. King and Mauer (2004) report lower registration rates among black males compared to nonblack males in Georgia, with greater disparities in registration rates between urban and nonurban communities. Using 2000 Census data aggregated at the neighborhood level, Clement and Keough (n.d.) found great variation in the law's effects between cities and counties. Urban areas lost the greatest number of voters to felon disenfranchisement; some neighborhoods and communities experienced up to 10 percent of their voting-age population barred from voting. The conclusion that is drawn from these studies is that the effects of FD are concentrated in areas with substantive political preferences that differ from geographic areas that are less affected by FD policies. Thus, as several of these studies explicitly or implicitly conclude, it is not solely the direct vote of ex-felons that is denied through these laws. FD impacts the political power of communities that extend beyond felons' collateral penalty (also see Manza and Uggen, 2002).

In many ways, the research described above presents an empirical basis for obvious consequences of a policy that disproportionately affects minority and urban communities. One important question that arises from these findings is whether lower participation rates in such communities are driven by the high rate of ex-felon residents, or if FD laws also alter the participation rates of nonfelons themselves. As Preuhs (2001:746) speculates, the socialization effects of removing a large proportion of a population from the eligible voter pool may have consequences for nonfelons as social norms reinforcing participation are weakened in severe FD states compared to communities without such prohibitions. This sentiment is echoed by other scholars (cf. King and Mauer, 2004), but has yet to be subjected to direct empirical examination.

It is plausible to suspect that nonfelon participation will be affected by FD policy. A key mechanism is that political participation is partly a function of the social norms regarding participation that are present in the environment in which individuals make decisions about participation. In short, the process of political socialization has the potential to encourage or discourage political participation among eligible voters (cf. Burns, Schlozman, and Verba, 2005; Cutler and Steckenrider, 1989; Elder et al., 1989; Jennings and Niemi, 1981; Kotler-Berkowitz, 2005; Lay, 2006; Levine, 2005; Sandell and Plutzer, 2005). Although the literature on political socialization is immense, the potential for nonfelons living in states with criminal policy regimes that exclude ex-felons from voting to be socialized against political participation, and thus be less likely to participate themselves, is suggested

by a number of streams of this body of work, from childhood to adult socialization.

Children's family, social networks, and school are all essential transmitters of political knowledge, attitudes, and behavior (Elder et al., 1989; Lare, Moore, and Wagner, 1985). Scholars have argued that the family is the most important of these three sources. For instance, Verba, Schlozman, and Burns (2005) suggest four models of familial political socialization. The learning model suggests that within the family "children absorb explicit and implicit lessons about politics and the rights and responsibilities of citizens" (Verba, Schlozman, and Burns, 2005:95). The accumulation model suggests that parents transmit attitudes they consider important to their children. The identification model indicates that parents provide political examples and attitudes for their children to emulate. The interpersonal transfer model suggests that children take general life experience from the family and translate it into the political world. These models work together with families' socioeconomic status, education, and other demographic characteristics to transmit political attitudes, values, and behaviors (Burns, Schlozman, and Verba, 2005).

Children's social networks are also vital sources of political socialization. Collective socialization theory suggests that the political attitudes of a collective adult group are passed on to all the minor members of that group regardless of the children's individual backgrounds (Brooks-Gunn et al., 1993; Lay, 2006). Moreover, contagion theory suggests that behavior spreads through peer groups. For example, if all an adolescent's friends vote, then the adolescent is more likely to vote (Lay, 2006). In sum, adults transmit the collective political attitudes of a community to all the children in the community, and the political actions of individuals within a peer group increase the likelihood that other members of the peer group will engage in similar behaviors.

Childhood political socialization is the foundation of adult political attitudes and values, but it does not comprise a complete picture of the transmission and formation of political attitudes, ideals, and behaviors (Sigel, 1989). The political attitudes and values of childhood may not fully fit individuals' adult life experiences and roles; thus individuals' attitudes and values change over time, to accommodate new experiences, as well as the time lag between childhood political socialization and the acceptance of specifically political roles (Cutler and Steckenrider, 1989; Sigel, 1989). Social networks are also a vital piece of adult political socialization that can both change and reinforce political opinions and behaviors (Converse, Miller, and Stokes, 1960). Social networks, including family, friends, coworkers, and acquaintances, play a fundamental role in political participation, attitudes, and values by providing political information, recruitment and, mobilization (Kotler-Berkowitz, 2005; Levine, 2005). When groups are homogenous, individuals' political opinions, attitudes, and behaviors are reinforced; when they are diverse they are more likely to change individuals'

opinions and behaviors as well as provide more invitations for political participation (Converse, Miller, and Stokes, 1960; Kotler-Berkowitz, 2005). Importantly, it is not necessary for these, or any other, political invitations, attitudes, or behaviors to come from close contacts. Casual acquaintances appear to be just as likely to influence political opinions (Levine, 2005).

A necessarily limited review of the political socialization literature has been presented, but the effects of norms of participation within an environment on individual participation are well documented. Whether it acts through prolonged childhood socialization, or more proximate adult socialization, or even the synergy created by concordant childhood and adult socialization, the legal prohibition of voting of a large portion of the adult population in a state or community has the potential to reduce the probability that individuals will be exposed to families, social networks, or community norms that promote political participation. There is also the potential that the legitimacy of the political system is more likely to be questioned when large portions of the community are prohibited from voting. Thus, a dual process of socialization against participation emerges in communities with severe restrictions on the voting rights of ex-felons. Individuals are exposed to smaller proportions of their community that participate (a norm of tacit nonparticipation) as well as the potential for a generally negative view of the political system to emerge that questions the legitimacy of politics and the utility of participation (a negative norm against participation). In short, if political socialization acts on individuals, FD has the potential to act not only by directly suppressing political participation of convicted felons, but by producing a social environment that undermines political involvement in general.

Several testable hypotheses can be derived from the above discussion. The first hypothesis suggests an overall effect from severe FD laws—those that impose essentially lifetime bans on voting for individuals convicted of a felony. Since our empirical investigation focuses on voting, our initial general hypothesis is as follows.

H_1: *Nonfelons living in states with strict FD laws will be less likely to vote than nonfelons in states with less severe FD laws.*

In addition to this general hypothesis, which assumes a similar socialization effect across all individuals, several conditional hypotheses are required to more clearly test the socialization hypothesis as applied to FD laws. If a socialization process is at work, FD is expected to produce more prominent effects among specific populations that are either most likely to be exposed to large proportions of ex-felons or are most susceptible to the effects of social networks.

The most prominent and widely documented empirical findings on the effects of FD laws show that these laws disproportionately impact the black community (Fellner and Mauer, 1998; Manza and Uggen, 2002; Ochs, 2006). Coupled with persistent patterns of residential segregation in the

United States, the disproportionately high level of blacks affected by FD suggests that blacks are more likely to be exposed to social environments that contain large proportions of disenfranchised ex-felons relative to whites. The evidence for other racial/ethnic minority groups is less developed, but higher incarceration rates (DOJ, 2005), initial evidence of a disproportionate impact of FD policies (Demeo and Ochoa, 2003), and similar residential segregation patterns suggests that Hispanics are also more likely than non-Hispanic whites to be exposed to such environments. If FD acts through socialization to dampen the participation rates of nonfelons, then we would expect blacks and Hispanics to be affected by FD laws to a greater degree than non-Hispanics. The first set of conditional hypotheses is derived from this logic.

H_2: *The negative effect of strict FD laws on voting is greater for blacks than whites.*

H_3: *The negative effect of strict FD laws on voting is greater for Hispanics than white non-Hispanics.*

Additional characteristics should also condition the socialization effects of FD laws on nonfelons. Since ex-felons are likely to be concentrated in communities with relatively low income and educational levels, and with a reasonable degree of residential segregation based on these socioeconomic characteristics, the effects of FD laws may emerge as changing the degree to which individual characteristics regarding education and income levels affect the probability of participation. Individuals with higher levels of education and income are exposed to fewer social networks with high proportions of ex-felons, while also being equipped with the political and social resources that can overcome socialization against political participation. Thus, we should see the positive associations between higher levels of income and education and participation heightened in lifetime FD states when compared to states with no lifetime bans. This leads to a second set of conditional hypotheses that further clarify how and when FD laws affect nonfelon participation.

H_4: *The effect of FD laws on nonfelon voting conditions the effect of education. In states with severe FD laws, the difference in the probability of voting between low and high levels of educational attainment will be greater than the difference between low and high levels of educational attainment in states with less severe FD policies.*

H_5: *The effect of FD laws on nonfelon voting conditions the effect of income. In states with severe FD laws, the difference in the probability of voting between low and high levels of income will be greater than the difference between low and high levels of income in states with less severe FD policies.*

Finally, the general strength of social networks in a community may affect the degree to which socialization influences political participation in general.

Where social networks are strong, FD's effect on socialization should be more pronounced as individuals take more negative cues from a social network with fewer individuals participating and undermining the general sense of legitimacy in the group. Given recent evidence that suggests urban communities are influenced more by declining voters due to FD (King and Mauer, 2004), and some data limitations, we delineate the type of community by urban and suburban or rural areas. Scholars presenting these findings, while still limited to a few specific localities, correctly assert that directly prohibiting ex-felons from voting will produce disparities across communities, with urban areas experiencing greater reductions in political power. However, socialization should have a slightly different effect on nonfelons—FD may exert a greater influence on nonfelons residing outside of urban communities. This is a result of two processes. First, individuals from rural and suburban areas are more likely to vote than individuals from urban areas (Nie and Verba, 1972), and thus any effects are likely to be found where there is the largest potential for loss of voters. However, social networks, and hence socialization toward voting, is also stronger outside of urban communities, even though there are some points of socialization within urban areas (Lay, 2006). In short, if FD acts on social networks, then communities with strong social ties will be most affected by the loss of voters. This leads to a final conditional hypothesis.

H_6: *The negative effect of FD laws on nonfelon voting will be more severe for individuals living outside of urban areas than for individuals living in urban areas.*

Methods

The hypotheses require that the individual voting decision is modeled as a function of individual characteristics, state-level contextual factors, and the interaction of these two levels of measurement. Existing individual-level data sets do present limitations to researchers interested in comparing the effects of state-level policies, however. National data sets focusing on political behavior and attitudes tend to provide either adequate samples within each state but with a limited number of variables to control for established causes of political behavior, or a rich set of variables for each observation, but are limited in sample dispersion across states. To overcome these deficits, we examine two data sets to cross-validate the findings. The first is the Current Population Survey (CPS), 2004 November Supplement, which provides a limited number of political variables for a very large sample that is adequately distributed across the states. Our second data set is formed by pooling the American National Election Studies (ANES) from 1980 to 2004. The ANES provides a rich set of variables for each respondent, but is limited in sample size in many states for any particular year. Pooling over the seven even-year surveys allows for a reasonable sample size per state.

Our study focuses on the decision to vote since it is the most common form of basic political participation. The dependent variable in both data sets is a dichotomous variable, which is coded 1 if the respondent voted in the previous national presidential or midterm general election, and 0 otherwise. Both data sets are filtered to include only those registered to vote, which serves as a strong proxy for nonfelons in lifetime FD states (described below) and a reasonable proxy for nonfelons in states with less severe restrictions.[1]

The key independent variable is the respondent's state FD policy. Although scholars have coded variation in FD policy in different ways (cf. Ochs, 2006; Preuhs, 2001), we opt for a basic measure of severity that distinguishes between FD policies that impose postsentence (beyond probation and parole) or "lifetime" bans on registration and voting and those that do not. Thus, LIFETIME FD is coded 1 for respondents living in states with lifetime FD, and 0 otherwise.[2]

In addition to the FD variable, several variables that capture potential contextual effects at the state level are included in the models as controls. All analyses include a dummy variable for individuals residing in southern states given the historic racial bias in voting laws in this region (Keech, 1968; Key, 1949). SOUTH is coded as 1 for each respondent who lived in one of the 11 states that were members of the former Confederacy, and 0 otherwise. Given that blacks were the specific target of the South's historic attempts at disenfranchisement, an interaction term between this contextual variable and a dummy variable for black respondents is included as a control. Poorer states and those with greater competition between the major parties have also been shown to affect individual-level voter turnout (Hill and Leighley, 1996;

[1]This eliminates from the data set noncitizens and potential felons that may confound the results for FD states if included, as well as applies the analysis to those who have already taken a basic step of registering to vote. Although ex-felons may be part of the data for non-FD states, their inclusion should not present problems for inferences. First, economic, political, and social attributes are controlled for that should account for any potential independent "felon" effect. Second, felons probably vote at lower rates than nonfelons and thus their inclusion in the sample would only dampen the effects of FD states relative to non-FD states, thus biasing the estimates toward insignificant results. In short, if a large portion of ex-felons are included in the sample of non-FD states, the result is a stronger test of the propositions. In addition, there was no difference between the results reported below and analyses that included nonregistered individuals.

[2]FD policies can vary from no prohibitions on voting, prohibitions during incarceration, prohibitions during incarceration and parole/probation periods, and what we call "lifetime" bans. These lifetime bans primarily vary in terms of the process in which ex-felons can have their voting rights reinstated. The analysis codes the following states as having lifetime bans: AL, AZ, DE, FL, KY, MD, MS, NE, NV, TN, VA, and WY. UT is coded as having a lifetime ban in the pooled ANES analysis after the 1998 survey. It is possible that temporary disenfranchisement has little effect on political socialization and nonfelon political participation; felons who are prohibited from voting for all or part of their sentence are not necessarily less likely to be politically active (Manza and Uggen, 2004). This suggests that felons living in states with less severe felon disenfranchisement laws may become politically active after completing their sentence at comparable rates to others in their community. At the least, they are not prohibited from doing so.

Leighley and Nagler, 1992), and thus the percentage of the state's population living below the poverty line (POVERTY) and Ranney's index of party competition (PARTY COMPETITION) are added as state-level variables to control for these possible effects. Since there is little variation in both these measures within states over time, an average for the period under study is utilized in the ANES data set. Additional contextual variables are available in the pooled ANES data set and are included in the models since state voting rates may be affected by the changing electoral context in each state and/or year of the survey. The first captures the effect of a PRESIDENTIAL ELECTION YEAR, which is coded 1 if the respondent was part of a survey administered during a presidential election year and 0 in midterm elections. The second is SENATE RACE, coded as 1 for respondents residing in states with a U.S. Senate race during the year of the survey, and 0 otherwise. Postregistration election rules also influence individual-level decisions to vote, and given some states' historic use of election rules to undermine the political participation of racial/ethnic minorities, it is necessary to control for these institutional effects on voting as well (cf. Wolfinger, Highton, and Mullin, 2005). To do so, two measures are included. (Wolfinger, Highton, and Mullin, 2005:7). First, the DEMOCRATIC INSTITUTIONS SCALE was created based on contemporary voting and registration rules and ranges from 0 to 4, with 4 representing the score most conducive to voter turnout.[3] Since the states' earlier use of institutional rules may also affect the vote, and to provide another control for electoral institutions, Hill's (1994) scale of democratic institutions in the early 1980s was used along with the first scale to create a single-factor score labeled the DEMOCRATIC INSTITUTIONS SCORE. A final control for state-level effects on voting is the ideological orientation of the states since conservative states may be related to both FD laws and voting. PERCENT CONSERVATIVE is the mean percent of state respondents that identifies as conservative from Wright's (2008) CBS/New York Times cumulative survey data set. Since these last three measures are highly collinear, and are also interacted with black and Hispanic populations as these are the groups most likely to be affected, they are introduced in separate models in the analysis.

Individual characteristics are controlled for in the analysis by a series of variables that, again, vary by data set. Both data sets control for the effects of age, race, education, and marital status. Age's well-established positive effect on political participation is accounted for in the models with the variable AGE, which is measured in years (Highton and Wolfinger, 2001; Tate, 1994). EDUCATION is measured as a dichotomous variable where 1 is as-

[3]The DEMOCRATIC INSTITUTIONS SCALE was created by assigning each of the following rules a value of 1 and summing the total for each state. The rules included were: early voting, late voting, mailing of information about the polling place, mailing a sample ballot, and providing time off for private-sector employees based on the 2000 general election and reported by Wolfinger, Highton, and Mullin (2005:18–19). These were the rules Wolfinger, Highton, and Mullin (2005) found to exert significant effects on voting.

signed to those who have not attained a high school degree or higher, 2 to those who hold a high school degree, and 3 for those with more than a high school degree in the CPS data set. In the ANES data set, education is coded as a six-point ordinal scale, with 1 indicating eighth grade or less and 6 indicating a college degree or more. A dummy variable for MALE is included to capture the effect of sex. It is coded as 1 for male and 0 for female respondents. Given the limited set of variables in the CPS, MARRIED is included to add validity to the analysis. It is a dichotomous variable that codes married individuals as 1; 0 otherwise. Finally, since individuals who are newer to communities are less likely to vote than long-time residents (Sandell and Plutzer, 2005), LENGTH IN RESIDENCE is accounted for by coding the number of years individuals lived in their residence.

Race is captured with a dichotomous variable, BLACK, which codes black individuals as 1, and all others as 0. Although blacks tend to vote in proportion to whites, inclusion of this variable is necessary to establish a baseline effect of race that is necessary to examine the conditional effects of FD laws as suggested by H2. HISPANIC, coded 1 for Hispanic respondents and 0 for non-Hispanics, captures both lower participation rates of Hispanics in the United States, as well as the baseline effects to evaluate the conditioning role of FD policies (H3).

INCOME is included and coded as a five-point ordinal variable based on percentiles so valid comparisons can be made across timeframes in the ANES models. The coding is 1 for individuals in the bottom 16th percentile; 2 represents the 17th to 33rd percentiles; 3 is the 34th to 67th percentiles; 4 is the 68th to 95th percentiles; and 5 is for individuals in the top 95th percentile. Income is coded along a six-point ordinal measure for the CPS data set. H6 predicts that urban residents' political participation will be less effected by FD laws than nonurban areas, and thus URBAN captures the baseline effects of geographic region by coding respondents living in urban areas with a 1, and suburban and rural areas as 0 in the ANES data set. CPS data codes metropolitan area, and thus the dichotomous variable is 1 for those residing in metropolitan areas and 0 for those in rural areas in this data set. Both data sets contain the variables described above.

The ANES, however, contains a variety of political variables that allow the analysis to control for a number of additional alternative explanations. Group membership, political interest and efficacy, and partisan attachment are additional factors that often shape the likelihood of voting. To control for these effects, the ANES analysis includes the following individual-level variables. CHURCH ATTENDANCE is a proxy for group membership and is operationalized with a six-point categorical variable coded from never attending (1) to more than once a week (6). Political interest is captured by a dichotomous variable, PUBLIC AFFAIRS, which codes respondents answering that they are interested in public affairs "some" or "most of the time" as 1, and 0 for all others. Political efficacy is coded in two ways. First, respondents

answering that people like them have no say in government are coded as 1, and all others 0, for the variable EFFICACY. Second, INFLUENCE is coded as 1 for individuals who responded that they tried to influence others to vote in the previous election, and 0 otherwise. Strength of partisan attachment is likely to increase the likelihood of voting and participation and, thus, STRONG PARTISAN is included in the models and is coded as 1 for those who answered that they are either strong Democrats or strong Republicans in the seven-point party identification question.

Since much of the normative discussion regarding FD policy rests on its effect on racial/ethnic minorities, and given previous research that suggests that demographic, partisan competition, and institutional rules may disproportionately affect minority voters (cf. Wolfinger, Highton, and Mullin, 2005), interactions between the black and Hispanic variables and party competition, poverty, percent conservative, and democratic scale/score variables are also included in the models, or in separate models to avoid problems with multicollinearity given a limited number of degrees of freedom at the state level. These interactions guard against any spurious relationships between FD laws and racial/ethnic minority interactions that may arise given their general geographic distribution in states with larger minority populations (Preuhs, 2001).

To examine the conditional effects of FD policies predicted by H2–H6, the models also contain several interaction terms between FD policy and the variables indicating blacks, Hispanics, education level, income level, and urban residents. As discussed above, the two data sets vary in their ability to account for the array of confounding factors, and thus the models differ in formal structure since the ANES data set provides more controls. However, both analyses follow the general model presented below.

$$\begin{aligned}\text{Prob(Vote)} = {} & a + b_1(\text{FD}) + b_2(\text{Black}) + b_3(\text{FD*Black}) + b_4(\text{Hispanic}) \\ & + b_5(\text{FD*Hispanic}) + b_6(\text{Education}) + b_7(\text{FD*Education}) \\ & + b_8(\text{Income}) + b_9(\text{FD*Income}) + b_{10}(\text{Urban}) \\ & + b_{11}(\text{FD*Urban}) + b_{12\,\text{to}\,n}(\text{control variables})\end{aligned}$$

H1, the general socialization hypothesis, predicts that b_1 should be negative as FD policies have an overarching effect on voter turnout. H2 predicts that b_3, the coefficient for the interaction term between FD and black respondents, will be negative and significant, while H3 states that strict FD laws will have a greater effect on Hispanics than on non-Hispanics, and thus the expectation is that b_5 will be negative. H4 suggests that the effect of strict FD laws will be to increase the importance of education as those with higher levels of educational attainment are least influenced by socialization. Thus, b_7 will be positive and significant—adding to the positive effect of the expected baseline variable for education. The coefficient for the interaction between lifetime FD and income (b_9) should be positive (H5). Finally, the interaction term between FD and urban is expected to produce a positive

value for b_{11} as FD policy is predicted to have a stronger effect on voting in nonurban areas (H6).

The models presented above combine individual-level characteristics with state-level contextual values. Since individuals are grouped within states for this analysis, a number of concerns with traditional OLS estimation procedures arise. Specifically, given cultural orientations toward participation varying by state, and individuals clustered within the states, there is the potential for errors to be clustered and thus correlated within these groups. The result is that OLS tends to produce artificially low standard errors resulting in Type I errors. The analysis instead relies on a cluster, or multilevel, modeling technique that employs a random intercept logit model to allow variation in estimates to be clustered by group (in this case, the states). This technique is an alternative to the standard modeling techniques presented in recent analyses of the contextual factors related to individual voter participation that do not account for the potential clustering of errors (cf. Wolfinger, Highton, and Mullin, 2005) and is analogous to random intercepts hierarchical linear modeling discussed by Bryk and Raudenbush (1992). Population averaged clustered models and logit models without clustered standard errors produced the same substantive results as reported below (perhaps supporting previous studies' reliance on more restrictive modeling assumptions).

Results

Table 1 presents the results of the analyses. Models 1(a–d) and 2(a–d) report the estimated coefficients, clustered standard errors, and significance levels for the CPS and ANES data, respectively. Models 1a and 2a present the results of models that exclude the percent conservative, democratic institutions scale, and democratic institutions score variables and interactions. Models 1b and 2b include the baseline and interactions between percent conservative in a state and black and Hispanic variables. Models 1c and 2c include the democratic institutions *scale* and its interactions with black and Hispanic variables, while Models 1d and 2d include the democratic institutions *score* and its interactions with the same variables. This specification process is a useful test of the stability of the FD effects given the potential collinearity between FD policy, the political context, and other election rules. Moreover, several states were not included in the postregistration rules due to either mail-in balloting or for allowing same-day registration (ID, ME, MN, NH, WI, and WY were excluded due to the latter, while OR was excluded due to mail balloting). AK and HI are not included in the state-level ideology measures. Presenting these models separately, and thus examining the potential bias in the results due to different samples, further validates any consistent results found.

There is a reasonable degree of congruence in the results in the models and data sets. No variables were found to be both significant and in the

TABLE 1

Estimated Effects of Strict Felon Disenfranchisement Laws on the Probability of Voting by Registered Voters (DV = 1 if Voted, 0 Otherwise)

Independent Variables	Model 1a (CPS)	Model 1b (CPS)	Model 1c (CPS)	Model 1d (CPS)	Model 2a (ANES)	Model 2b (ANES)	Model 2c (ANES)	Model 2d (ANES)
Lifetime FD	0.047	0.045	−0.121	−0.153	−0.049	−0.046	−0.078	−0.065
	(0.170)	(0.170)	(0.162)	(0.160)	(0.278)	(0.278)	(0.287)	(0.289)
Black	0.026	−0.801	0.755	0.553	0.633	2.454	−0.010	0.644
	(0.709)	(0.824)	(0.747)	(0.714)	(1.275)	(1.995)	(1.377)	(1.308)
Lifetime FD* Black	−0.413**	−0.333*	−0.355**	−0.237*	−0.513*	−0.565*	−0.569*	−0.591*
	(0.133)	(0.141)	(0.134)	(0.141)	(0.248)	(0.249)	(0.252)	(0.257)
Hispanic	−0.518	−0.535	−0.752	−0.470	−2.188	−4.311	−5.691**	−4.114
	(0.883)	(1.337)	(0.975)	(0.998)	(1.420)	(2.751)	(1.881)	(1.880)
Lifetime FD* Hispanic	0.200	0.204	0.242	0.231	−0.446	−0.442	−0.444	−0.456
	(0.162)	(0.165)	(0.166)	(0.166)	(0.304)	(0.301)	(0.295)	(0.296)
Education	0.757***	0.769***	0.747***	0.747***	0.194***	0.195***	0.201***	0.201***
	(0.027)	(0.028)	(0.030)	(0.030)	(0.025)	(0.025)	(0.027)	(0.027)
Lifetime FD* Education	0.052	0.042	0.065	0.066	−0.040	−0.039	−0.036	−0.037
	(0.061)	(0.062)	(0.064)	(0.064)	(0.054)	(0.054)	(0.056)	(0.056)
Income	0.071***	0.070***	0.068***	0.069***	0.044	0.042	0.062	0.062
	(0.005)	(0.005)	(0.005)	(0.005)	(0.036)	(0.036)	(0.039)	(0.039)
Lifetime FD* Income	0.007	0.007	0.012	0.012	0.156*	0.156*	0.175*	0.173*
	(0.010)	(0.009)	(0.010)	(0.010)	(0.074)	(0.074)	(0.077)	(0.077)
Urban	0.120**	0.126**	0.083*	0.086*	−0.009	−0.008	−0.089	−0.093
	(0.039)	(0.040)	(0.043)	(0.043)	(0.086)	(0.086)	(0.093)	(0.094)
Lifetime FD* Urban	0.071	0.069	0.181*	0.180*	0.383*	0.411*	0.483**	0.470**
	(0.090)	(0.090)	(0.093)	(0.093)	(0.192)	(0.192)	(0.196)	(0.196)
Male	−0.131***	−0.131***	−0.124***	−0.124***	−0.030	−0.027	−0.044	−0.044
	(0.029)	(0.029)	(0.031)	(0.031)	(0.063)	(0.063)	(0.067)	(0.067)

TABLE 1—continued

Independent Variables	Model 1a (CPS)	Model 1b (CPS)	Model 1c (CPS)	Model 1d (CPS)	Model 2a (ANES)	Model 2b (ANES)	Model 2c (ANES)	Model 2d (ANES)
Age	0.014***	0.014***	0.014***	0.014***	0.016***	0.015***	0.016***	0.016***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Married	0.254***	0.266***	0.242***	0.242***	0.274***	0.274***	0.287***	0.286***
	(0.031)	(0.032)	(0.034)	(0.034)	(0.068)	(0.069)	(0.073)	(0.073)
Church attendance					−0.059**	−0.060***	−0.060**	−0.060**
					(0.020)	(0.020)	(0.021)	(0.021)
Public affairs					0.330***	0.331***	0.351***	0.354***
					(0.069)	(0.069)	(0.073)	(0.073)
Efficacy					0.029	0.029	0.020	0.021
					(0.035)	(0.035)	(0.037)	(0.037)
Influence					0.419***	0.419***	0.432***	0.433***
					(0.074)	(0.074)	(0.079)	(0.079)
Length in residence	0.160***	0.158***	0.171***	0.171***	−0.001	−0.001	−0.001	−0.001
	(0.011)	(0.011)	(0.011)	(0.011)	(0.001)	(0.001)	(0.001)	(0.001)
Strong partisan					0.231***	0.232***	0.247***	0.247***
					(0.068)	(0.068)	(0.072)	(0.072)
South	−0.320**	−0.320**	−0.248***	−0.130	−0.298**	−0.261*	−0.388**	−0.404*
	(0.103)	(0.106)	(0.077)	(0.088)	(0.121)	(0.148)	(0.151)	(0.201)
South* Black	0.240*	0.032	0.047	−0.243	0.377	0.805*	0.744*	0.812*
	(0.139)	(0.178)	(0.157)	(0.191)	(0.265)	(0.421)	(0.418)	(0.467)
Party competition	0.500	0.176	0.664*	0.608*	0.945*	0.871	0.716	0.770
	(0.474)	(0.508)	(0.360)	(0.349)	(0.566)	(0.575)	(0.612)	(0.616)
Party competition* Black	0.187	0.086	−0.331	−0.217	0.318	0.173	0.731	0.240
	(0.688)	(0.693)	(0.703)	(0.685)	(1.269)	(1.278)	(1.355)	(1.31)
Party competition* Hispanic	0.496	0.653	0.661	0.303	5.221*	7.889*	13.669**	9.65*
	(0.897)	(1.111)	(1.037)	(1.036)	(3.042)	(4.264)	(4.307)	(4.185)

TABLE 1—continued

Independent Variables	Model 1a (CPS)	Model 1b (CPS)	Model 1c (CPS)	Model 1d (CPS)	Model 2a (ANES)	Model 2b (ANES)	Model 2c (ANES)	Model 2d (ANES)
Poverty	0.005	0.004	0.016	0.016*	0.0356*	0.035	0.037*	0.035*
	(0.013)	(0.013)	(0.010)	(0.010)	(0.015)	(0.015)	(0.018)	(0.017)
Poverty* Black	0.015	0.004	0.016	0.012	−0.070*	−0.063*	−0.079*	−0.075*
	(0.018)	(0.019)	(0.018)	(0.018)	(0.038)	(0.038)	(0.044)	(0.042)
Poverty* Hispanic	−0.005	−0.006	0.001	0.001	−0.001	−0.005	0.024	−0.012
	(0.016)	(0.017)	(0.016)	(0.016)	(0.052)	(0.051)	(0.056)	(0.053)
Percent conservative		−0.010				−0.009		
		(0.011)				(0.016)		
Percent conservative* Black		0.033*				−0.057		
		(0.017)				(0.046)		
Percent conservative* Hispanic		−0.004				0.053		
		(0.015)				(0.058)		
Democratic inst. scale			0.028				−0.022	
			(0.028)				(0.050)	
Democratic inst. scale* Black			−0.113*				0.127	
			(0.046)				(0.126)	
Democratic inst. scale* Hispanic			−0.018				−0.481**	
			(0.050)				(0.175)	
Democratic institutions score				0.231**				−0.049
				(0.088)				(0.145)
Democratic inst. score* Black				−0.521***				0.304
				(0.146)				(0.291)
Democratic inst. score* Hispanic				0.018				−0.458
				(0.126)				(0.323)

TABLE 1—continued

Independent Variables	Model 1a (CPS)	Model 1b (CPS)	Model 1c (CPS)	Model 1d (CPS)	Model 2a (ANES)	Model 2b (ANES)	Model 2c (ANES)	Model 2d (ANES)
Senate race					0.345***		0.387***	0.387***
					(0.065)		(0.069)	(0.069)
Presidential election year					1.906***		1.859***	1.852***
					(0.067)		(0.071)	(0.071)
Intercept	−2.280***	−1.665**	−2.696***	−2.632***	−3.240***	−2.864***	−3.109***	−3.185***
	(0.499)	(0.654)	(0.391)	(0.377)	(0.605)	(0.882)	(0.657)	(0.664)
N	58,048	56,555	48,532	48,532	7,248	7,247	6,426	6,426
Wald $\chi 2$	2897***	2852***	2536***	2547***	1101***	1101***	986***	983***

*Indicates $p < 0.05$; **$p < 0.01$; and ***$p < 0.001$ in a one-tailed test of significance.

NOTE: Unstandardized coefficients reported along with robust standard errors clustered by state in parentheses. Model 1a excludes NE; Model 2a excludes AK and NE. Models 1b and 2b exclude AK, HI, and NE. Models 1c, 1d, 2c, and 2d exclude AK, ID, ME, MN, NE, NH, OR, WI, and WY.

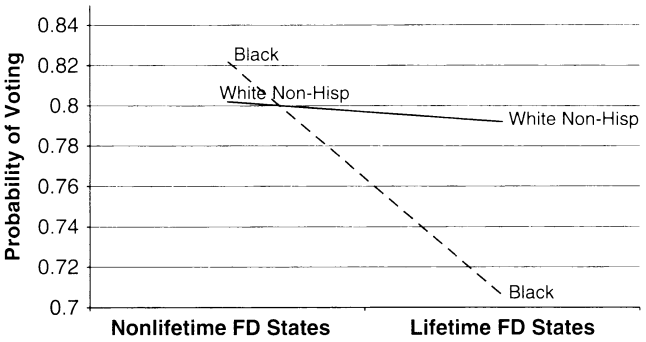
opposite direction across the two data sets. The established indicators associated with the decision to vote are generally supported in the models. Education, income, age, marriage, an interest in public affairs, strong partisanship, and past attempts to influence others' voting decisions are all positively, and significantly, associated with voting. The inclusion of the southern regional dummy variable, demographic, and political and policy context variables, and their interactions with black/Hispanic variables, are justified by their significance in various models (albeit sporadic in some instances). Although these are not surprising results, the significant variables do provide a level of control and confidence in the model. Nevertheless, the focus of this study is the effect of FD laws on specific types of individuals, and the utility of examining two data sets is to cross-validate the results. Thus, the focus of the discussion below will be on points where the interpretations of the results relevant to the six hypotheses presented above converge at traditional levels of statistical significance.

Does FD policy affect nonfelons' propensity to vote? By itself, and in the general case, strict FD does not. Although the coefficient for the lifetime FD variable is generally negative, it is not statistically significant. However, the effect of FD policies is apparent among black individuals, supporting H2. In both the CPS and the ANES models, and regardless of the inclusion of controls for broader political context and election rules, the interaction term between black and FD policy (b_3) is negative and significant. Thus, the results support the contention that the disparate impact of FD laws on voter participation along racial lines is not simply constrained to felons themselves—black respondents also tend to be more greatly affected by FD laws than non-Hispanic whites. However, these results do not hold for Hispanics, as indicated by an insignificant interaction coefficient and a difference in predicted probability of 0.01 between Hispanics in lifetime FD and non-lifetime FD states.

Beyond the racial/ethnic group disparities, the hypotheses also predict that differential effects of resources will emerge across the FD policy context. Although the baseline education variable is positive and significant in both models, the prediction that educational differences will be exacerbated in FD states is not borne out in any model across the data sets. However, a positive and significant effect of the lifetime FD variable's interaction term with income level (H5) does emerge in the ANES, but not in the CPS data. H6, the expectation of a positive coefficient for the urban and FD interaction, is supported in all the ANES models, but only in the two CPS models that include the measures of other democratic institutions. In short, while there are reasons to believe that FD policy has a disproportionate affect on a variety of demographic groups, and some limited evidence supporting such contentions, the significant triangulated effect of FD policies on nonfelon voting is most clearly experienced by blacks—a finding that is in line with the disproportionate effects of FD policies on the black population.

FIGURE 1

Estimated Effect of Lifetime Felon Disenfranchisement on the Probability of Voting by Registered Voters, by Racial Group (Estimates Based on ANES Model 2d)



To more easily interpret the findings regarding the effects of FD law on voting, the estimated log-odds ratios are transformed to present the marginal effects on the probability of voting. Figure 1 presents the conditional effects of FD policies on the probability of registered voters actually voting in the previous election for blacks and non-Hispanic whites from the ANES estimates (Model 2d). The ANES model is used to estimate these effects since it provides a much broader set of controls and thus a more conservative estimate of the effects of FD laws across racial groups. These effects were estimated using the coefficients from Model 2d and varying the black and FD variables, while holding all other variables at their means.[4] For white non-Hispanics, the drop in the probability of voting between individuals in lifetime states (0.802) and nonlifetime states (0.792) is a modest and insignificant 0.010, while the probability for blacks drops from 0.822 to 0.706, for a change in probability of 0.116. Thus, while whites are relatively isolated from the effects of FD, blacks in lifetime FD states are 11.6 percent less likely to vote than blacks in nonlifetime FD states. In other words, the dampening effect of FD policies is about 12 times greater for blacks relative

[4]Given that the black population differs from the white population in systematic ways on a variety of variables included in the models, for instance, blacks tend to have lower overall income and education levels than whites, but stronger partisan attachments and are more likely to live in the South, the probability of voting was also estimated using group-specific means as values for the variables in Model 2d. The results are similar in substance, but show an even greater effect of FD laws. For white non-Hispanics, the probability of voting for registered voters changes from 0.810 to 0.794 as one compares non-FD states to FD states. For blacks, the probability changes from 0.791 to 0.656, or a reduction in the probability of voting of 0.135. Given similar starting points, a more conservative estimate of effects by using the grand mean, the reality that group-specific means lead to both more and less likelihood of voting across the individual variables, and the desire to examine the marginal effects of FD policies for similar individuals, the discussion focuses on the estimated probability with the grand mean rather than the group-specific means.

to non-Hispanic whites. The direction of the effects estimated from the CPS (Model 1d) are the same, but the difference in estimated probability between blacks living in non-FD states and those who live in FD states is a more modest 0.018. Nevertheless, even without controlling for the political variables in the ANES, the CPS does reveal a statistically distinguishable effect of FD policies on black nonfelons.

The other interaction terms that were significant in the ANES provide some interesting, but tentative, support for the socialization hypotheses. Neither data set revealed that the effects of Hispanic or education levels were conditioned by FD policies in the states (at least not in a manner consistent across all models that would provide some degree of confidence in the results). Income, however, did produce a more pronounced effect in FD states than in non-FD states, as indicated by the significant coefficient on the interaction term (b_9) in the ANES models. From Model 2d, the estimated effect of income is about 4 percent greater in FD states, holding all other factors at their means. Six of the eight models do suggest that the urban interactions are in line with the expectations for the hypotheses that FD laws tend to accentuate differences in voter participation across urban and nonurban areas. Based on the coefficients presented in Model 2d, and again holding all other variables at their means, the probability of voting in urban areas is a statistically insignificant 0.017 less than in nonurban areas in nonlifetime FD states. However, in lifetime FD states, urban residents are more likely to vote than nonurban residents. The difference in probability is approximately 0.101 (or 10 percent). These urban and income effects, while consistent with the hypotheses, are not significant in all models in both data sets. Thus, the results are presented with more uncertainty than those for FD effects on black respondents.

Conclusion

The direct effects of FD laws on ex-felons have been well established, and a growing literature demonstrates that these effects can alter political outcomes, but little is known about the implications for FD policies on the individual behavior of nonfelons. Using general tenets of political socialization models as a theoretical starting point, this study has shown that the effects of FD policies are not limited to those that arise from the direct removal of ex-felon populations from the voting booth. FD policies affect *nonfelons'* propensity to vote as well. Using two data sets to cross-validate our inferences, we have demonstrated that strict FD policies tend to dampen the probability of voting for blacks, but not for non-Hispanic whites. This finding underscores the substantive impact that FD policies have on the political participation of the black community. Not only do FD policies directly prohibit a disproportionate share of the black community from participating in one of the more basic political acts, FD also reduces the

likelihood of voter participation in the black community. The consistency of this finding for blacks across the two data sets, coupled with the well-documented inequity in FD's direct effects, should provide another substantial element to the normative debate surrounding FD policies.

Moreover, while differences in the variables available across data sets limit the ability to cross-validate some of the findings, the analysis of the ANES data does provide some modest support for the contention that a broader disparate impact of FD policies on racial and ethnic minority groups and those with lower resource levels is present. FD policies, according to the ANES results, also accentuate the wealth gap in participation. These results, if they hold in future research, suggest that FD exacerbates the bias against low socioeconomic status racial and ethnic minorities in electoral outcomes and policy responsiveness (see Griffin and Newman, 2007; Manza and Uggen, 2002; Radcliff and Saiz, 1995; see also Wolfinger, Highton, and Mullin, 2005, for a discussion of general postregistration policy effects).

The current study adds to the empirical evidence regarding the collateral consequences of FD policy, and the implications of these findings provide fertile ground for future research questions on the socialization effects of ostensibly race-neutral policies in contemporary U.S. politics. Specifically, given the state-level effects and large *N* samples examined here, the findings point scholars to look more closely at the effects of FD policies, other election rules, and even broader incarceration policies on the political and social networks of nonfelons as they examine the political implications of these policies. More proximate measures of socialization and qualitative analyses will be able to provide a better picture of the mechanisms that lead from strict FD policies to dampened voting, with greater attention to the specific linkages of the socialization process.

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