On the Political Economy of Felon Disenfranchisement

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Abstract: Nearly 6.1 million U.S. citizens are politically disenfranchised because of a prior felony conviction, and these citizens tend disproportionately to be black. Specifically, more than 7.4% of the adult African-American population is disenfranchised compared to 1.8% of other Americans. This paper investigates the political consequences of this large racial disparity in disenfranchisement rates. To obtain the first causal estimates of the effects of felon disenfranchisement (FD), we build a new database that catalogs the annual state changes in disenfranchisement law. We show that these changes are driven by lengthy, uncertain, and complicated court cases which are outside of the control of individual state legislatures. We use a difference in difference strategy to analyze the impact of these changes in felon disenfranchisement laws. Our results suggest that FD legislation is associated with a 3 percentage point reduction in the likelihood of voting, allowing for a range of race-specific effects of demographic and geographic characteristics. This number is larger than would be implied purely by the mechanical effect due to the change in the number of eligible voters suggesting that FD also reduces turnout amongst those eligible to vote. Next, we show that relaxations in FD laws increase the number of Black U.S. Representatives. Finally, we show that relaxations also lead to an increase in state policy liberalism.

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Introduction

Nearly 6.1 million U.S. citizens are politically disenfranchised because of a prior felony conviction, and these citizens tend disproportionately to be black.[1] Specifically, more than 7.4% of the adult African-American population is disenfranchised compared to 1.8% of other Americans (Bureau of Justice Statistics, 2014). This paper investigates the political consequences of felon disenfranchisement (henceforth, FD) laws in light of this large racial disparity in disenfranchisement rates. We show that such laws reduce turnout by African-Americans, reduce the number of Black representatives, and shift implemented policies to the right.

We do this in three ways, designed to capture the three aspects of democracy Lincoln highlighted in the Gettysberg Address; "government of the people", "government by the people", and "government for the people". Firstly, we study how FD affects engagement with the political process and specifically the turnout rates of Blacks and non-Blacks. We show that restoring the vote to those previously convicted of a Felony (but who have completed their sentence) increases the percentage of African-Americans voting by around 6%. This is a substantial number as it implies that the increase in the number of African-Americans voting is almost equal to the increase in the franchise. Given, turnout rates amongst those most likely to be convicted of a felony are normally around half of that in the overall population, this is particularly surprising. We argue, and provide evidence that this suggests, in line with the argument of Burch (2013) that FD has an affect on engagement with the political process beyond the mechanical affect on the franchise.

Secondly, in the spirit of "*of the people*" we study whether FD affects the racial composition of the set of elected representatives. That is, does FD limit the number of Black politicians? We find, analyzing elections to the US house that FD limits the number of Black representatives.

Third, in a competitive democracy, changes in the franchise ought to affect the equilibrium policy as long as people are not disenfranchised entirely at random. The remainder of the paper thus shows how changes in FD laws alters the implemented state policy.

FD has a long history in the US, Kentucky introduced the first criminal disenfranchisement laws in 1792, and have been a feature of US Politics ever since.[2] Behrens et al. (2003) argue that expansion of such laws was a response to the passage of the 14th Amendment. Yet, the impact of these restrictions was limited by the relatively low rate of felony convictions. However, as Figure 1 shows this has grown substantially in the last 40 years, the period we study. Moreover, as can be seen in the figure, this growth has been disproportionately concentrated on African-Americans. These changes have coincided with substantial changes to FD laws, with states both imposing news restrictions and in other cases eliminating existing ones. Thus, at the state level, there have been substantial changes in the racial composition of the enfranchised population due to the combination of the growth in incarceration rates and changes in FD laws.

This paper studies the political consequences of these changes and as such builds on two prominent bodies of work, legal, political, and sociological scholarship on FD and research in Economics studying the political and economic causal effects of the Voting Rights Act and other changes in vote suppression.

An important literature in political science and sociology has studied the causes

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[1] Uggen, Larson, and Shannon (2016).

[2] See Keyssar (2000), a full history is provided in Appendix B.

of FD (Behrens et al., 2003, Manza and Uggen, 2008). Others have highlighted the disproportionate consequences for African-American communities (Alexander, 2012, Bentele and O'brien, 2013) and traced the complex and subtle channels through which FD damages Black political empowerment and engagement beyond its mechanical effect on the franchise (Burch, 2013). These findings of this more recent research is in contrast to prior findings by Economists and others that argued that since turnout rates are low among those most likely to be convicted of crimes the aggregate effects of FD should be limited (see, Hjalmarsson and Lopez, 2010). One reason for this highlighted by Burch (2013) and Cottrell et al. (2018) is that the geographic distribution of disenfranchisement is extremely uneven. Cottrell et al. (2018) show that even though at the national level African-Americans are disenfranchised at the rate of 13.2%, in some legislative districts they are disenfranchised at the rate of 20% to 40% as voting is primarily a local activity. Other recent research (Gerber et al., 2017) uses administrative data for Pennsylvania to disentangle the effects of incarceration *per se* on voting and the demographic characteristics of those incarcerated. Meredith and Morse (2017), using data for Alabama, show that the common requirement that ex-felons pay outstanding financial obligations, particularly fines and fees, before they are re-enfranchised serves to limit re-enfranchisement.

Much of this work emphasises the role of mass incarceration in driving the growth of FD. Temin (2017) attempts to synthesize recent insights and scholarship from Law, Criminology, and Economics research using a simple model. He shows that the non-linear relationship between crime and incarceration suggested by these books predicts multiple equilibria and characterizes the growth in incarceration as the convergence from a low- to a high-incarceration equilibrium. He attributes this, in part, and particularly the concentration of the effects of this change on Black communities, to Nixon's Southern Strategy and the *Abuse of Drugs Act* (1986). This uneven impact and growth is documented by Shannon et al. (2017).

Our analysis of how changes in the franchise affect political outcomes is closely related to the important literature studying the consequences of the 1965 Voting Rights Act (henceforth, VRA), particularly Cascio and Washington (2014). They use a triple differences design that shows that in counties where the VRA led to the removal of voter literacy requirements led to increases in Black turnout, and state transfers. This is relevant for this paper as it provide direct evidence of how disenfranchisement impacts policy and consequently incomes and poverty. Thus, the removal of the ballot due to FD may be expected to have similar outcomes. The high stakes nature of this racial politics is made clear by Washington (2006) who shows that a Black (Democratic) candidate increases both White and Black turnout and she concludes that fear of the (presumably liberal) policies of the Black candidate motivates White turnout in opposition. In light of this Bernini et al. (2017) highlight that another important consequence of the VRA was to increase the number of Black representatives and that this increased the provision of local public goods. The only other paper we are aware of that studies the political consequences of FD is Klumpp et al. (2017) who assess the consequences of FD by asking whether the identity of the US House majority party would have been altered by eliminating FD, finding the answer to be negative. The aim of this paper is to assess a broader range of political and economic outcomes following Cascio and Washington (2014) and Burch (2013) amongst others.

Other recent work has studied other forms of vote suppression. Hajnal et al. (2017) show that recent increases in voter identification requirements have tended to differentially affect minority groups and *'skew democracy towards those on the political right'*. Hicks et al. (2015) argue that such requirements tend to be introduced by Republicans-controlled legislatures

in electorally competitive states.

This paper is organized as follows. The next section explains the institutional background and our data. Section 3 outlines our empirical approach and presents results for how FD affects turnout. Sections 4 and 5 then provide results for how FD impacts the number of Black representatives and state policy. Section 6 briefly concludes.

2 Institutional Background and Data

2.1 An Overview of Felon Disenfranchisement

In this section we provide more detail about FD, the different forms of disenfranchisement, and the process through which these laws have previously changed and how this underpins our identification strategy.

FD is a departure from universal suffrage and incarnates a view that those who have committed serious crimes are, for one reason or another, unworthy of the vote. The reasoning behind disenfranchising the felons who have already served their sentences has included a range of rationales from philosophical and legal arguments to more emotive appeals such as preserving "the purity of the ballot box" (Washington v. State, 75 Ala. 582, 585 (1884)).[3]

In practice it refers to a spectrum of voting restrictions. At one end is the withdrawal of the franchise from all those currently or ever convicted of a felony. At the other, is the absence of any restrictions on either those currently serving a sentence or those who have previously done so. Vermont and Maine are the only two states without restrictions as of 2016, while around ten states only re-enfranchise convicted felons in exceptional circumstances, if at all, through the granting of special clemency following an individual appeal.[4] Most states are between these two extremes. The key respect in which they differ is how the vote may be re-obtained. Namely, whether the process is automatic or requires a special application. There are then two further dimensions of difference. One is the timing of the restoration of the vote. That is, whether felons are eligible to be re-enfranchised once they are no-longer incarcerated, or if the vote may only be restored after Prison, Parole, and Probation have all been completed. The second concerns whether a felon is required to settle outstanding debts and court fines before they maybe re-enfranchised. This is often at least burdensome if not infeasible, and can limit re-enfranchisement (see, Meredith and Morse, 2017).

The substantial growth in the number of those convicted of felonies shown in Figure 1, and of those incarcerated, has transformed this from what might have been once seen as a legal and constitutional nicety to an important practical determinant of the extent of the franchise. On the other hand, from a practical perspective, some have argued that low rates of political engagement and turnout amongst ex-felons mean that the consequences are more limited than a focus on headcount might suggest. Hjalmarsson and Lopez (2010) analyze two national surveys and find that criminals and non-criminals have significantly

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[3]The purpose of this paper is to offer a positive analysis of the consequences of FD, and thus we do not ask whether FD is 'right'. But, there is a very large sociological and legal literature which offers more normative perspectives. This has taken a variety of approaches including constitutional law (Karlan, 2003, Haygood, 2011), social contract theory (Schall, 2006, Brooks, 2004), group threat theories (Geoghagan, 2007, Behrens et al., 2003), and theories of political party affiliations (Meredith and Morse, 2014, Klumpp et al., 2017), etc.

[4]These commonly have to be made to the governor, but the details vary from state to state. For example, in Arizona a Judge may grant re-enfranchisement, and in Alabama appeals are made to the Board of Pardons and Paroles.

Electronic copy available at: https://ssrn.com/abstract=3330565

different voter registration and turn out rates, only 26% of ever incarcerated criminals have voted in 2004 Presidential elections. Burch (2011) estimates the voter registration, turnout, and party registration in the 2008 general election for men with felony convictions in Florida, Georgia, Michigan, Missouri, and North Carolina. They find, similarly to Hjalmarsson and Lopez (2010), an average rate of 22.2%. People captured and convicted for their first offense after the election voted at similarly low rates.

Yet, Burch (2013) *inter alia* has argued that such a static analysis ignores the spillover effects of concentrations of disenfranchisement and documents how these in terms of diminished local community political activity and engagement. Thus, changes in FD laws may be expected to have an effect beyond any direct computation of the change in the franchise multiplied by the average propensity to vote of those re-enfranchised.

By the Articles of Confederation in 1777, states retained complete control over suffrage rules and in 1792, Kentucky established the first criminal disenfranchisement laws in the U.S. Almost all states followed suit, see Appendix B for details. In the 20th century there were relatively few substantive changes, and none between 1978 and 1995. Since then, there have been a number of changes in state FD laws, the effects of which we study in this paper. Figures 2 and 3 show the cumulative impact of changes over the period. Looking at Figure 2 we see first of all that there is substantial variation in FD laws across states, and that this variation does not follow an obvious pattern. That is, it is not the case that FD laws coincide with the Deep South, or only those states with large African-American populations. Moreover, when we compare the pattern with that around 20 years later in Figure 3 we see that, similarly, while there are a number of changes there is no obvious pattern in which states have changed their FD laws. For example, both Washington and Tennessee have relaxed their FD laws, as have Pennsylvania and New Mexico. While, South Dakota and Louisiana have introduced more restrictive laws.

Despite the number of changes the process through which they take place is often both long and uncertain. Given that FD laws are often part of state constitutions a constitutional amendment is required to alter them. Given supermajority or referenda requirements this is often unfeasible. Consequently, changes in the relevant law often depend on revisions of what is meant by FD and how it is administered, which are then typically the subject of protracted court cases often culminating in a ruling by a state supreme court.[5] Similarly, when governors have chosen to alter the process through which their powers of clemency are applied this has itself been the subject of litigation.[6] Elsewhere, referenda have been necessary (e.g Texas). This is because not only does a law need to it often has depended upon a combination of legislation, Appendix D provides brief histories for a number of recent cases as examples.

2.2 Data

2.2.1 Felon Disenfranchisement Laws

States started to adjust their FD legislation from 1995, with no substantive changes in the preceding 20 years. We hand coded all relevant legislation since 1996, and these changes are summarized in Appendix B. Changes for the period 2001–2016 are already collected by the NCSL.[7] Data for the period 1996–2001 were obtained by keyword searches on the NCSL

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[5]For example, the cases of Pennsylvania or Virginia described in Appendix D.

[6]For example, the case of Iowa described in Appendix D.

[7]http://www.ncsl.org/research/elections-and-campaigns/elections-legislation-databases-description.aspx

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website, individual state legislature websites, and the LexisNexis database using a broad range of terms such as "felon", "Felony voting", "felon voting rights", "enfranchisement", etc. Following the approach of the Sentencing Project (2014) laws were coded into three binary variables:

1. Automatic *Restoration* of voting rights of felons after prison.

2. *Extension* of voting rights to felons convicted in certain offenses or when they are on parole or probation.

3. *Restriction* of felon's voting rights.

Given that changes in these only ever affect subsequent elections, our coding reflects this and so a change in the law in December 2000 would be coded as affecting the November 2002 elections, other things equal. In a number of states there have been several changes in the law, and the coding scheme captures these as separate changes.

Our main variable of interest will be *Enfranchisement* $\in \{0, 1, 2\}$. Which is defined as:

$$Enfranchisement = 2 \times Restoration + Extension \tag{1}$$

This captures the idea of FD as a spectrum ranging from restoration only by special clemency to full and automatic voting rights. We treat *Restriction* as the base category as it is mutually exclusive from the other two. (1) treats *Restoration* as twice as large a change in the law as *Extension*. This is designed to capture, in a minimal way, the substantial difference between *automatically* giving the vote to the entire eligible population and extension conditional on appeal and other requirements. The results are robust to treating the two as equivalent. Combining *Restriction* and *Restoration* like this, as well as easing inference, also reduces how much we are asking of our data. We also report results for the three variables separately to understand better the differential impacts of different types of changes in FD laws.

2.2.2 Outcome Data

Following the literature we focus on the effects of FD on African-Americans. Thus, we ignore all other racial and ethnic distinctions. The estimated effects of FD on African-Americans is then in comparison to all other Americans and not just non-Hispanic Whites. We argue this approach is to be preferred since the estimates will capture the particular affects of FD on African-American communities. Moreover, given that FD may also disproportionately affect, for example, Hispanic-Americans, compared to non-Hispanic Whites, our estimates of the overall impact of FD on racial and ethnic minorities will be statistically and conceptually conservative given they will only be based on the difference in experience between African-Americans and all others.

We consider three outcome measures, Turnout Rates, the number of African-American U.S. Representatives, and implemented state policies. Analyzing racial and other demographic differences in turnout rates, requires individual voting records combined with demographic data for a representative sample of the U.S. population. We use the Voting and Registration Supplement data from the Current Population Survey (USDoC et al., 1985–2014), described by Hur and Achen (2013) as 'the gold standard amongst turnout studies'. These data provides the voting decision of the U.S. population by their race and ethnic groups at state and county level.[8]

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[8]One issue is how to treat non-responses. The CPS recommending coding these as not-voting, we follow the

As a measure of how FD affects the number of black elected officials and representatives we use demographic data on members of the U.S. House of Representatives from three sources Manning and Shogan (2012), Amer (2008), and National Association of State Legislatures (2017)). It would be interesting to study whether FD affects the racial composition of those holding state-level office but such data, to the best of our knowledge, are not readily available.

Finally, to measure how the change in the electorate caused by FD affects policy we use the state policy liberalism data from Caughey and Warshaw (2015), and Caughey and Warshaw (2017). This is based on a dynamic latent variable model which provides estimates of overall state policy liberalism (and economic and social policy liberalism) based on 148 individual policy areas that are comparable across elections and states. These are the only data to capture both the cross-sectional and time-series variation in state-policy.

3 Turnout

In this section we begin by introducing our identification strategy and regression specification. We then discuss results analyzing the decision to vote at the individual level. We show that even allowing for a range of demographic trends that there is an effect of FD laws on the likelihood of African-Americans voting but not for remainder of the U.S. population. To allow for a broader range of unobservable characteristics we analyze the effects on cohorts defined by state, county, sex, age, and race to show that when comparing within cohort, FD has larger effects in states and counties with a greater proportion of African-Americans. We then present Interactive Fixed Effects (IFE) estimates that show that the effect of FD on African-American political participation is robust to allowing for time varying effects of state unobservable characteristics.

3.1 Identification

We will estimate the Average Treatment Effect of a change in Felon Disenfranchisement laws on the probability of voting with regression based difference-in-differences strategy. Following, Angrist and Pischke (2009) we use a linear probability model. This requires the usual assumptions of parallel trends, SUTVA, and ignorability. The SUTVA assumption seems reasonable here, given as discussed above that electoral law is delegated to states and has in many cases been determined by state supreme court rulings and referenda which it is hard to see depending on other states' laws. We demonstrate that the 'parallel-trends' assumption is met graphically below in Figures 4-6. For clarity, we report results conditional on a presidential election year dummy, unconditional results are in Figures A.3-A.5 in the Appendix. We note, that the pre-treatment series naturally has relatively few observations at the end of the period and thus we observe some divergence between the series at this point. For the same reason the pre-treatment series finishes before the not-treated series.

In our context, ignorability requires that the likely consequences of FD are not systematically different in states that enact it to those that do not. One way in which this might be violated is if in states with larger populations of ex-felons the decision to re-enfranchise them was more electorally consequential and thus can be expected to be the subject of a more concerted campaign to do so. We note that equally, in the spirit of Becker

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recommendations of Hur and Achen (2013) and drop non-responses and refusals. We code the small number of "don't knows" as not having voted.

([1983](#)), those who are likely to lose out through such a re-enfranchisement are likely to oppose it more energetically. Moreover, as the examples discussed above make clear beyond bringing a test-case (which needs only to be funded) or exerting pressure on their elected representatives (which may be expected to be balanced out), it is not obvious what influence the public may have. Of course, some states constitutions may make the change in such a law easier, and there is a clear increase in the number of states re-enfranchising felons over time. Thus, we argue that the changes in FD laws are conditionally random controlling for these differences.

We control for these in three ways. Firstly, with the conventional two-way fixed-effects model including state (or county) and year fixed effects. Such that our difference-in-difference estimator will be:

$$Y_{ist} = \tau_1 \text{Enfranchisement}_{st} + \tau_2 \text{Black}_i \times \text{Enfranchisement}_{st} + X'_{it}\beta + \lambda_s + F_t + \epsilon_{it} \quad (2)$$

Where, $Y_{it} \in \{0,1\}$ is whether individual i voted, X_{it} is a vector of observable characteristics of the individual as well as time-varying state-level observables, λ_s are state (or county) fixed effects, and F_t election-year fixed-effects.

Second, we study the effects of FD within demographic groups. As the CPS is not a panel we cannot ask the natural question of whether, within individuals, the probability of voting increases following a relaxation of FD restrictions. In lieu of such an analysis we instead employ a pseudo-panel type approach. We define cohorts for each combination of state, sex, year of birth, and race (African-American or not), which are chosen on the basis that they capture key demographic differences whilst also being plausibly exogenous to the effects of FD. We then re-estimate (2) replacing the state fixed-effects with cohort fixed-effects such that τ_1 and τ_2 now measures the effect on people of within each cohort. This means that we are now able to capture the reality that for many reasons including differences in history, geography, and economic structure we should expect different impacts on different groups of Americans in different states.

Third, we generalize this approach using the IFE estimator of Bai ([2009](#)). Unlike the conventional two-way fixed-effect estimator, the Bai ([2009](#)) IFE estimator no longer restricts the effect of time-invariant state characteristics and the stochastic time trend to be separate and additive. Rather, the overall impact in a given year will depend on the interaction of the time effect with the unobserved state characteristics. For example, nationally African-American turnout has been increasing over time, but there is pronounced variation across states in this trend. In the absence of other controls, the conventional two-way fixed estimator predicts the African-American turnout rate as $\hat{Y}_{it} = \hat{\lambda}_i + \hat{F}_t$, where $\hat{\lambda}_i$ will be the average turnout rate representatives in state i over the sample, and $\hat{F}_t$ will be the average turnout in year t across all states. Clearly, however we might expect, for example, that turnout rates will grow more in States with large African-American populations than others. Or we might expect that turnout might grow more in states with historically low turnout rates. More subtly, we might expect that the turnout rate in a given state will reflect the interaction of the national trend with the demographic characteristics and geographic distribution of the African-American population in that state. And so on. The conventional approach is to include various time trends, as indeed we do. But, since the IFE estimator explicitly allows for there to be unobserved differences across states the impact of which will vary over time (in an unrestricted way), it improves on this by parsimoniously capturing all of these possibilities and others.

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$$Y_{ist} = \tau_1 \text{Enfranchisement}_{st} + \tau_2 \text{Black}_i \times \text{Enfranchisement}_{st} + X'_{it}\beta + \lambda'_s F_t + \epsilon_{it} \qquad (3)$$

Here, λ_s and F_t are now r dimensional vectors capturing the unobserved state characteristics and time effects respectively. That is the unobserved characteristics of states may have several dimensions which impact on turnout of different individuals differently over time. r can be chosen via cross-validation, thus improving efficiency.

3.2 Results

We begin by estimating (2), and results are reported in Table 1. Looking first at column (1) we see that controlling for everything else, Black Citizens are around 3% more likely to vote. This is unsurprising given similar aggregate turnout rates and the well documented effects of income and education on turnout rates.[9] The coefficient on Enfranchisement,τ_1 is a precise zero and suggests that relaxing FD laws doesn't alter overall turnout amongst non-Blacks. On the other hand, τ_2 is statistically significant at the 1% level and suggests that African-Americans are, all else equal, 4% more likely to vote if FD laws are relaxed.

An increase in turnout of 4% is clearly economically significant. One interesting question is what is driving this increase. Part of the effect will be the straightforward effect of extending the franchise to those previously prohibited from voting. There may also, as argued by Burch (2013), be spillover effects such that citizens who were previously eligible to vote but did not, now find it easier to vote, or have more reason to do so.[10] Burch (2013) documents the mechanisms through which FD can disincentivize and frustrate voting amongst non-felons. Here, we engage solely in a simple numerical exercise to assess the relative share of these two effects. Denote the overall turnout rate at election $t = \{1,2\}$ as T_t, the rate amongst ex-felons as T_t^F and in the rest of the population as T_t^{NF}. Denote the relative share in the population of ex-felons as ψ.[11] Then, assuming that $T_1^F = 0$ because of the law, we have that:

$$\Delta T = T_2 - T_1 = \psi T_2^F + (1-\psi)(T_2^{NF} - T_1^{NF}) = \psi T_2^F + (1-\psi)\Delta T^{NF} \qquad (4)$$

Given the findings of Hjalmarsson and Lopez (2010) and Burch (2011) that turnout rates amongst ex-felons are around 20%. Thus, we set $T_2^F = 0.2$. Looking across the data, we set the average disenfranchisement rate across Black men and women at 4%, thus $\psi = 0.04$. Hence, an increase in turnout of 3% as in column (1) implies that $0.04 = 0.04 \times 0.2 + 0.96 \times \Delta T^{NF}$. Thus,

$$\Delta T^{NF} = \frac{0.04 - 0.2 * 0.04}{0.96} = 0.033 = 3.3\% \qquad (5)$$

This implies that a relaxation in FD laws leads to an increase in the probability of voting of those not directly affected by the change of 3.3% an effect substantially larger than the mechanical effect alone.

Columns (2) and (3) report results from a specification, which replace the stochastic time trend F_t in (2) with separate linear and stochastic time trends for Black and non-Black Americans. We see that the estimates of τ_1 and τ_2 remain positive and significant,

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[9]See, for example, urlhttp://www.electproject.org/home/voter-turnout/demographics

[10]And indeed some may now find it harder or less worthwhile to vote.

[11]We assume ψ is fixed for simplicity.

although they are unsurprisingly smaller at 2% and 1% respectively, as the race specific time trends will capture part of the effects of changes in FD. All three columns allow for the effects of Age and Gender to differ with race, column (3) additionally includes a full set of race-specific age dummies.

Columns (4)-(6) analyze the three different law changes separately. We see no effect for *Extension* or *Restrictions* or their interactions. Instead, the effect of *Enfranchisement* seems to be driven by the effects of imposing additional restrictions on Felons, which is associated with a 3% reduction in voting amongst African-Americans.

We now form cohorts by aggregating across groups defined by race, year of birth, gender, and state of residence as described above. We now are able to include fixed effects for these cohorts and thus study how changes in FD laws affect the probability of members of each cohort voting, conditional on unobservable traits that might affect their turnout. We can also include (race specific) stochastic time trends.

The results of this approach are in Table 2. Looking across the specifications the key difference from Table 1 is that we now also see a statistically and substantively significant effect on the turnout of non-Black citizens. Notably, however, τ_2 remains statistically significant and suggests an additional effect on African-Americans of between 2 and 6% or alternatively an effect between twice and four times as large as for others. When we look at the specific effects of Restoration and Extension in columns 4 and 5 the effects on non-Black citizens are no longer statistically significant but are quantitatively similar. In column 6 we find that the effect on African-American citizens is not statistically different from that of other citizens.

Given the evidence assembled by Cottrell et al. (2018) that even within states, such as Florida, there are wide variations in the extent of FD, with some counties having almost no disenfranchised African-Americans and others having more than 30%. We take the cohort approach a step-further and define instead cohorts for each combination of county, sex, year of birth, and race.

The results are reported in Table 3 looking across the specifications reported, we again find a substantial and precisely estimated effect of FD on the probability of voting and that this effect is substantially larger amongst African-Americans. The key difference is again in terms of the effects of specific law changes, these are now less precisely estimated given the richer set of fixed effects, but we again see that restrictions have the largest effects, although now these are found only to be significant for African-Americans, conversely to the state-level estimates.

Finally, we consider the results obtained using the IFE estimator (3). Now we are relaxing the assumption that the location and time unobservables affect turnout additively. The results are reported in Table 4. Looking across the specifications we see that the estimates of the effects of FD on Black and non-Black Americans are consistent. The estimated impact on non-Black citizens is always close to zero and relatively precisely estimated such that we can rule out any large impacts. The impact on Black citizens is consistently around $2 - 3\%$. Column 1 considers a model in which there are no controls, i.e. in (3) we impose $\beta = 0$, and where λ_s are defined at the state level. Column 2 performs a similar analysis but replaces the state fixed-effects with county fixed effects. Column 3 includes controls for presidential year, sex, and age. Column 4 is like column 3 but checks that our results are robust to imposing that r the dimension of λ_s and F_t is 2, rather than 1. Columns 4 and 5 have the same specifications as 2 and 3 but now include county fixed effects rather than state. A change of 2%, at the bottom end of the IFE estimates, implies an impact on the non-disenfranchised of 1.25%, again larger than the mechanical effect. Indeed, even at

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the lower end of all of our estimates at 1% the impact is still positive albeit no longer as economically significant at only 0.2%.

The estimates of the effects of the individual laws in columns 7–9 continue to be less precisely estimated, but while there are changes in the pattern of which variables are statistically significant the substantive pattern remain similar.

Overall, we may reach two conclusions. First, FD has a disproportionate impact on African-American communities and, secondly, that this effect is in excess of any mechanical effect due to the re-enfranchisement of offenders.

4   Representation

We now ask whether FD limits the number of African-American representatives? That is, whether the disproportionate reduction in the number of African-Americans in the electorate due to FD, limits government "*of the people*". We focus on elections to U.S. Representative. This reflects, to the best of our knowledge, a lack of systematic demographic information on those elected to state legislatures covering our period.[12] It is also preferable given that the voting laws are the prerogative of individual states and so we eliminate concerns of simultaneity in which a decrease in the number of African-American representatives may affect state FD policy. Necessarily, our analysis is now at the state level, rather than at the individual or county level.

We begin by documenting the key fact that the number of African-American U.S. Representatives is very small, both in absolute terms and in comparison to the share of African-Americans in the US population. Figure 7 makes this clear, we can see that at nearly two-thirds of state-year pairs no African-American representative was elected. Similarly, with the notable the case of Delaware which has elected an African-American as its single representative, we can see that the distribution of the share of African-American representatives is further to the left than would be expected if representatives' demographics mirrored those of the broader population, with a mean of just under 5%. This contrasts with a share of the population of around 12%.

Given that U.S. Representative is a senior political position and that organizing a campaign may take time we may expect that the effect of a change in FD laws not to happen immediately. We thus augment our previous specification to include lagged law changes. As our outcome measure is now solely at the state level, we exclude the interaction term. Thus, we now estimate

$$Y_{it} = \sum_{k=0}^{2} \tau_k \text{Enfranchisement}_{s,t-k} + X'_{it}\beta + \lambda_s + F_t + \epsilon_{it}. \tag{6}$$

Where Y_{it} is the Total Number of African-American U.S. Representatives Elected. The results are presented in Table 5. Given that Y_{it} is a count variable we estimate (Fixed-Effect) Poisson regressions, reported in columns 1 and 3. For comparison with the results obtained using the IFE estimator reported in columns 5, we also report analogous OLS-FE estimates in columns 2 and 4. To avoid the 'bad control' problem the only control included in X is a dummy for presidential election years. Columns 1 and 2 exclude the year fixed-effects F_t while Columns 3 and 4 include them. First, looking across specifications we

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[12]While previous work, such as Rocha et al. (2010) and Uhlaner and Scola (2015) has analyzed elections to state legislatures their data cover only a limited period and we have found no exact source of data for all states for the entire period.

see the coefficients are larger and positive for Enfranchisement$_t$ and Enfranchisement$_{t-1}$ and smaller and sometimes negative for Enfranchisement$_{t-2}$. Looking at the results of the fixed-effects Poisson estimator in column 1 we see that the effect at the first election after Enfranchisement is large and significant at the 1% level. The effect at the next election are smaller, and less precise, signficant only at the 10% level. The effect for the election after that is again smaller, and more imprecise and not significant at any level. The results of the OLS fixed-effects regressions in column 2 are similar, although less precise. The results of Columns 3 and 4 show that the results are not robust to the inclusion of year fixed-effects. The coefficients are similarly precise, but now smaller, meaning they are no longer significant. Column 5 reports estimates using the IFE estimator such that F_t and λ_s in (6) are replaced by $\lambda'_s F_t$. The estimates are quantitatively similar to the FE estimates in columns 2 and 4 and are like those in column 4 not significant. We argue that this lack of precision is unsurprising given that there are a relatively small number of U.S. Representatives from most states, the well-documented electoral advantage of incumbents (Lee, 2008, Jacobson, 2015), and few of these are Black. Notably, while imprecise in the more demanding specifications, the estimated effect is consistently large. The coefficients in column 1 suggest that a relaxation of FD laws is associated with around a $\exp(0.21) = 1.23$ or 23% increase in the number of African-American representatives at the election following Enfranchisement and an increase at the subsequent election of 10%. THe esimtates column 3, whilst not significant, are quantitatively similar suggesting and 8% and a 9% increase in the number of represenatives.

In sum we argue that there is consistent evidence that FD laws do affect the number of Black U.S. representatives but caution that these estimates are imprecise, perhaps due to the nature of the office particularly the small number of representatives in many states and the importance of incumbency advantage.

5 Policy

To close the paper we consider the impact that FD laws have on State policy. As in (6), given that changes in implemented policy will be gradual due to both the time taken for representatives to update their platforms and for new policies to be designed, passed, and implemented, we consider lagged law changes. Table 6 reports results for the state liberalism index introduced by Caughey and Warshaw (2015). Looking across columns 1–3 we can see that the point estimates are broadly consistent, although the IFE estimates in column 3 are less precise. The overall effect is reasonably large with a change from voting restrictions to automatic re-registration, a 3 unit change, associated with an increase in policy liberalism of around $3 \times 0.14 = 0.42$ standard deviations. Put differently, as can be seen in Figure A.2 this is around the difference in policy between New Hampshire and Montana in 2008. Or, West Virginia and Indiana. The total range in the data is around 5 again suggesting this is a substantial effect.

Caughey and Warshaw (2017), build upon the approach and data of Caughey and Warshaw (2015) and introduce two separate measures of state policy liberalism, 'social' and 'economic' (Figure A.2). This allows us to ask *how* policy changes as a consequence of FD. Looking at columns 4–6 we see that in each case there are consistent effects of social liberalism of a relaxation of FD rules. In contrast looking at columns 7–9 the estimates, while positive are small and imprecise. Thus, the change in policy seems to be concentrated on social legislation which is defined by Caughey and Warshaw (2015) to include issues such as Abortion, the environment, gun control, immigration, gay-rights, teacher certification

requirements, women's-rights, but not specifically race-related issues, and not economic issues such as benefits, regulation, taxation, or occupational licensing.[13] Thus, we conclude that FD has an economically important impact on state policy, particularly social policy. This is an important result as it demonstrates that, as canonical models in the spirit of Downs (1957) would suggest, that the change in the franchise due to FD laws affects the political equilibrium and thus the implemented policy.

6 Conclusion

This paper studied how FD laws affect the political process. We provided evidence that FD laws have a substantial impact on the political participation of African-Americans, and that the magnitude of this effect implies that such laws affect the broader community as well as ex-felons. We also found evidence that, consistent with standard conventional models of the political process (Downs, 1957, Persson and Tabellini, 2002), that not only does relaxing FD laws leads to an increase in turnout, but that this change in the effective electorate is also associated with an increase in the number of Black U.S. Representatives. At the state level, we find that relaxing FD laws leads to an economically important liberal shift in state policy, particularly social policy. While the positive nature of our analysis means that we cannot speak to the rights and wrongs of FD our results do suggest that, in line with the arguments of Burch (2013) and others, that any normative analysis that focuses exclusively on the consequences for individuals of FD laws may omit important impacts on society as a whole.

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[13]In Table A.1 in Appendix A we extend (6) to include a broader range of time-varying control variables, average age, education, and income, that may also have accounted for changes in state liberalism. The results are reported in Table A.1. We see that the results are robust to the inclusion of these with the effects of FD on both policy and social policy liberalism remaining significant and of a similar magnitude and the estimated effects on economic policy liberalism continuing to be small and imprecise.

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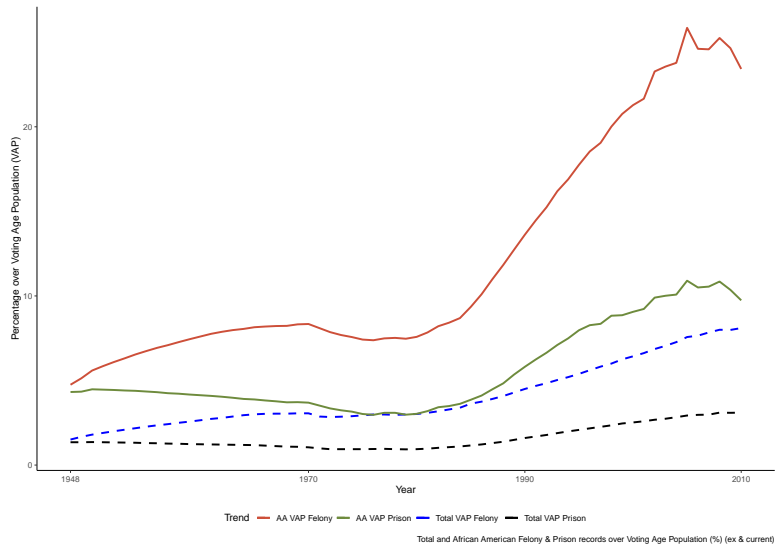
Figure 1: The Growth in the Incarceration Rate 1948-2010

Data from Shannon et al. (2017)
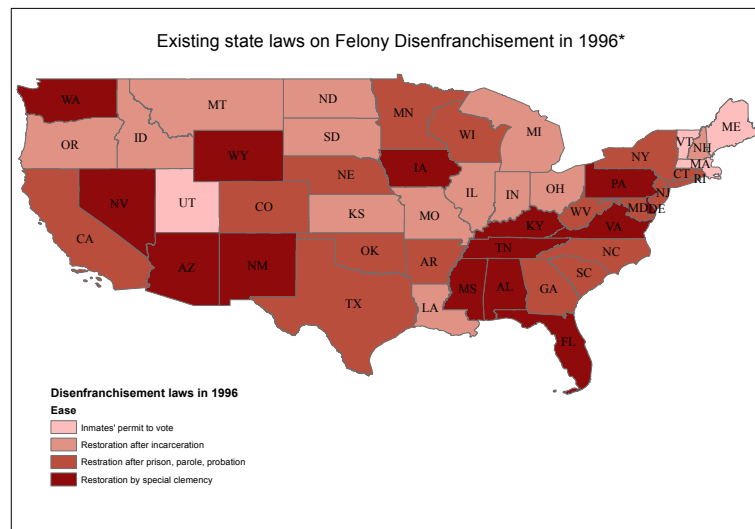
Figure 2: Current Felon Disenfranchisement Laws (1996)

As of end 1996. Alaska re-enfranchises citizens after Prison, Parole, and Probation. Hawaii after Prison.

Figure 3: Current Felon Disenfranchisement Laws (2017)

As of end 2017. Alaska re-enfranchises citizens after Prison, Parole, and Probation. Hawaii after Prison.

Figure 4: Parallel Trends Plot: *Restoration*

Data are from CPS.

16

Figure 5: Parallel Trends Plot: *Extension*

Data are from CPS.

Figure 6: Parallel Trends Plot: *Restriction*

Data are from CPS.

17

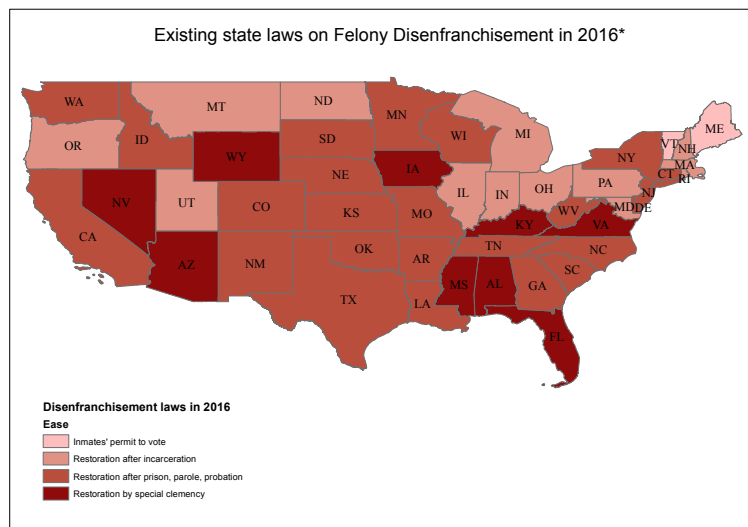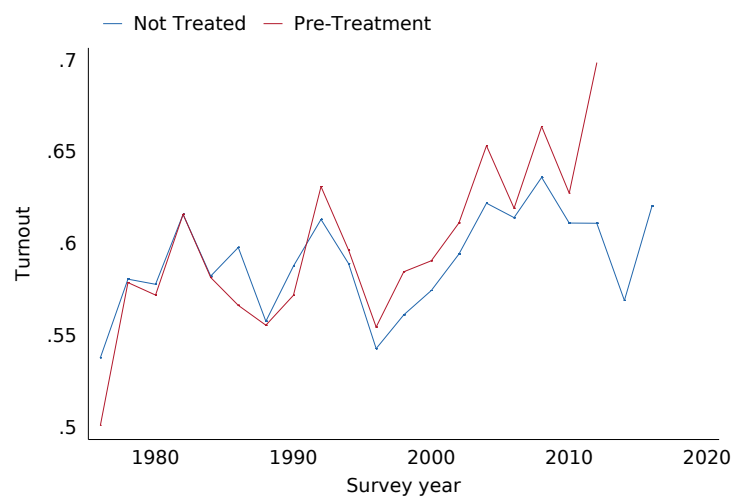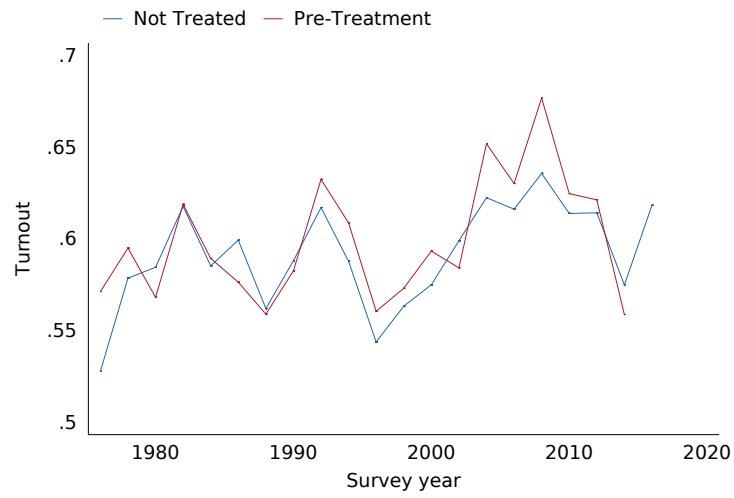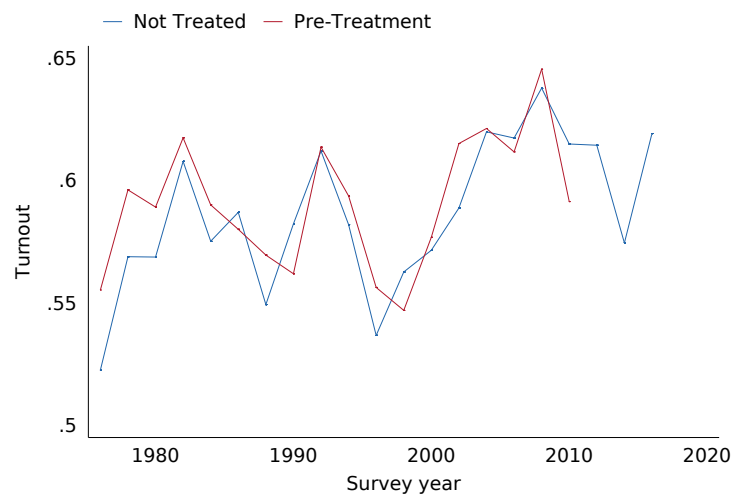|	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.03***	-6.57***	0.04***	0.04***	0.04***	0.05***
	(0.01)	(0.85)	(0.01)	(0.01)	(0.01)	(0.01)
Enfranchisement	0.00	0.01	0.00			
	(0.01)	(0.01)	(0.01)			
Black × Enfranchisement	0.04***	0.02**	0.01**			
	(0.01)	(0.01)	(0.01)			
Woman	-0.00**	-0.00*	-0.01***	-0.01***	-0.01***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.04***	0.04***				
	(0.00)	(0.00)				
Black × Woman	-0.05***	-0.05***	-0.05***	-0.05***	-0.05***	-0.05***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Black × Age	-0.00***	-0.00***				
	(0.00)	(0.00)				
Restoration				0.00		
				(0.01)		
Black × Restoration				0.01		
				(0.01)		
Extension					0.01	
					(0.01)	
Black × Extension					0.01	
					(0.02)	
Restrictions						-0.00
						(0.02)
Black × Restrictions						-0.03**
						(0.01)
Year Effects	Yes	RST	RS	RS	RS	RS
State Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Linear	Linear	Saturated	Saturated	Saturated	Saturated
Observations	1817218	1817218	1817218	1817218	1817218	1817218

Note: The dependent variable is an individual's decision to vote. *RS* denote race-specific stochastic time trends. *RST* refers to race-specific linear time trends. *Linear* controls implies that categorical variables are treated as continuous. *Saturated* means that we include a separate binary regressor for each level of the categorical variable.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by state in parentheses

Table 1: The impact of felony disenfranchisement reform on turnout: Individual Level Data

	(1)	(2)	(3)	(4)	(5)	(6)
Enfranchisement	0.07***	0.01**	0.02***			
	(0.01)	(0.01)	(0.01)			
Black × Enfranchisement	0.06***	0.04***	0.02***			
	(0.01)	(0.01)	(0.01)			
Restoration				0.02		
				(0.01)		
Black × Restoration				0.03**		
				(0.01)		
Extension					0.01	
					(0.01)	
Black × Extension					0.03**	
					(0.01)	
Restrictions						-0.03***
						(0.01)
Black × Restrictions						-0.01
						(0.01)
Constant	0.55***	0.43***	0.47***	0.47***	0.47***	0.47***
	(0.00)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
State Effects	State	State	State	State	State	State
Time Trends	No	Stochastic	RS	RS	RS	RS
Observations	37905	37905	37905	37905	37905	37905

Note: The unit of analysis is a cohort defined by each combination of County, Year of Birth, Gender, and Race. The dependent variable is the average turnout rate by cohort. All specifications include cohort fixed-effects. *RS* denote that race-specific stochastic time trends are included. *RST* refers to race-specific linear time trends.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by state in parentheses

Table 2: The impact of felony disenfranchisement reform on turnout: State Level Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Enfranchisement	0.07***	0.01**	0.01**			
	(0.00)	(0.00)	(0.00)			
Black × Enfranchisement	0.03***	0.02***	0.01*			
	(0.01)	(0.01)	(0.01)			
Restoration				0.02*		
				(0.01)		
Black × Restoration				0.01		
				(0.02)		
Extension					0.01	
					(0.01)	
Black × Extension					0.00	
					(0.01)	
Restrictions						-0.01
						(0.01)
Black × Restrictions						-0.03*
						(0.02)
Constant	0.56***	0.38***	0.41***	0.41***	0.41***	0.41***
	(0.00)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
State Effects	County	County	County	County	County	County
Time Trends	No	Stochastic	RS	RS	RS	RS
Period	All	All	All	All	All	All
Observations	108187	108187	108187	108187	108187	108187

Note: The unit of analysis is a cohort defined by each combination of County, Year of Birth, Gender, and Race. The dependent variable is the average turnout rate by cohort. All specifications include cohort fixed-effects. *RS* denote that race-specific stochastic time trends are included. *RST* refers to race-specific linear time trends.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by state in parentheses

Table 3: The impact of felony disenfranchisement reform on turnout: County Level Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Enfranchisement	0.02	0.01	-0.05	0.00	0.07***	-0.04			
	(0.04)	(0.02)	(0.04)	(0.08)	(0.02)	(0.03)			
Black × Enfranchisement	0.01***	0.01**	0.02***	0.02***	0.01	0.01*			
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)			
Black × Age			0.04***	0.04***	0.04***	0.04***			
			(0.00)	(0.00)	(0.00)	(0.00)			
Restoration							-0.02		
							(0.04)		
Black × Restoration							0.01		
							(0.01)		
Extension								0.00	
								(0.06)	
Black × Extension								0.01	
								(0.01)	
Restrictions									-0.01
									(0.04)
Black × Restrictions									-0.02**
									(0.01)
Interactive Fixed Effects	State, Year	County, Year	State, Year	State, Year	County, Year	County, Year	County, Year	County, Year	County, Year
Controls	None	None	Saturated	Saturated	Saturated	Saturated	Saturated	Saturated	Saturated
Dimension	1	1	1	2	1	2	1	1	1
Observations	108187	108187	108187	108187	108187	108187	108187	108187	108187

Note: The unit of analysis is a cohort. These are defined by each combination of State, Year of Birth, Gender, and Race (columns 1,3, 4) or County, Year of Birth, Gender, and Race (columns 2,5–9). The dependent variable is the average turnout rate by cohort. All results are obtained using the Interactive Fixed-Effects estimator of Bai (2009).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped Standard errors in parentheses

Table 4: The impact of felony disenfranchisement reform on turnout: Interactive Fixed Effects Estimates
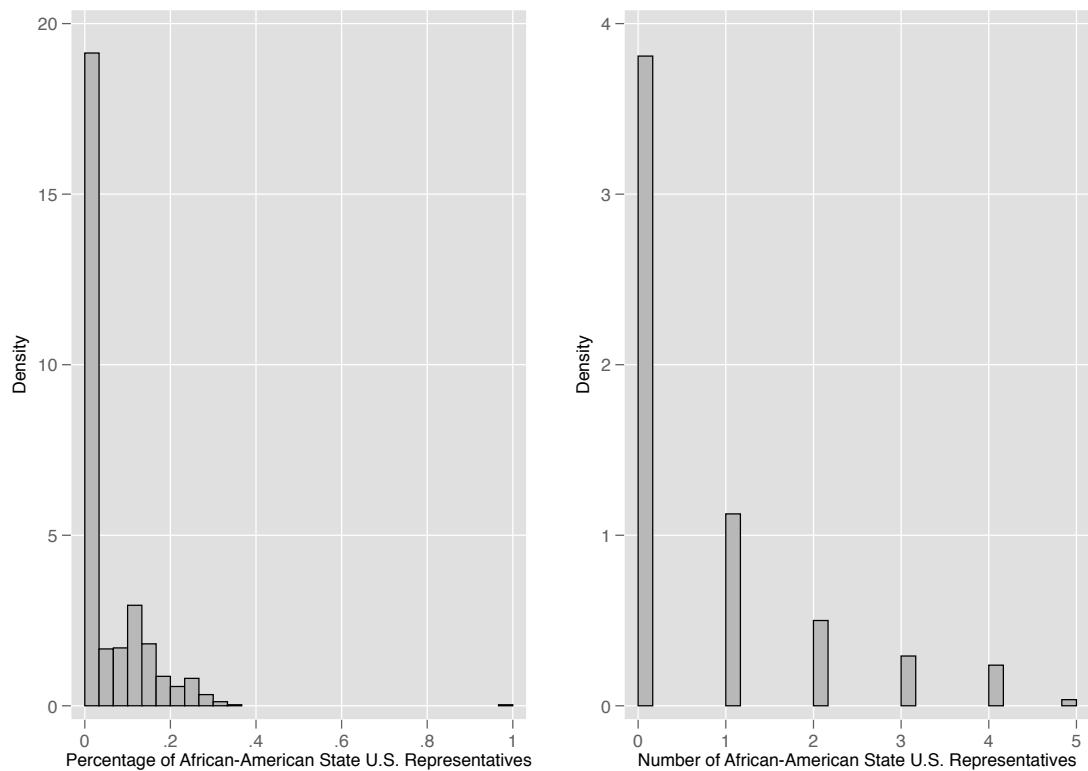
Figure 7: Number and Seat-Share of African-American U.S. Representatives(2017)

	(1)	(2)	(3)	(4)	(5)
Enfranchisement$_t$	0.21***	0.08**	0.09	0.03	0.04
	(0.07)	(0.03)	(0.08)	(0.03)	(0.03)
Enfranchisement$_{t-1}$	0.09*	0.06	0.08	0.07	0.07
	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)
Enfranchisement$_{t-2}$	0.06	-0.00	-0.06	-0.02	-0.01
	(0.11)	(0.06)	(0.09)	(0.06)	(0.06)
Year Effects	No	No	Yes	Yes	Yes
StateEffects	Yes	Yes	Yes	Yes	Yes
Controls	Poisson	FE	Poisson	FE	IFE
Observations	513	912	513	912	912

Note: The dependent variable is the number of black representatives elected by each state to the U.S. House. All states.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by state in parentheses.

Table 5: The impact of felony disenfranchisement reform on the number of black members of the state delegation to the US House of Representatives.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Enfranchisement$_t$	0.12**	0.12**	0.06	0.18**	0.19**	0.10	0.02	0.01	-0.02
	(0.06)	(0.06)	(0.05)	(0.07)	(0.07)	(0.06)	(0.06)	(0.05)	(0.05)
Enfranchisement$_{t-1}$	0.14***	0.14***	0.07**	0.18***	0.20***	0.09*	0.03	0.01	-0.02
	(0.05)	(0.05)	(0.03)	(0.07)	(0.07)	(0.05)	(0.05)	(0.05)	(0.05)
Enfranchisement$_{t-2}$	0.15***	0.15**	0.06**	0.20***	0.21***	0.09**	0.05	0.03	0.01
	(0.06)	(0.06)	(0.03)	(0.07)	(0.07)	(0.04)	(0.05)	(0.05)	(0.05)
Pres Election Year	0.00	0.00		-0.00	-0.05**		0.01	0.05*	
	(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.03)	
Year Effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
StateEffects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No	No	No
Estimator	FE	FE	IFE	FE	FE	IFE	FE	FE	IFE
Observations	864	864	864	864	864	864	864	864	864

Note: The dependent variable is the estimated state policy liberalism index of Caughey and Warshaw (2015) in columns 1–3. Columns 4–6 report results using the state social policy liberalism, columns 7–9 the state economic policy liberalism. Both measures are developed in Caughey and Warshaw (2017) in . All states.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by state in parentheses.

Table 6: The impact of felony disenfranchisement reform on state policy liberalism.
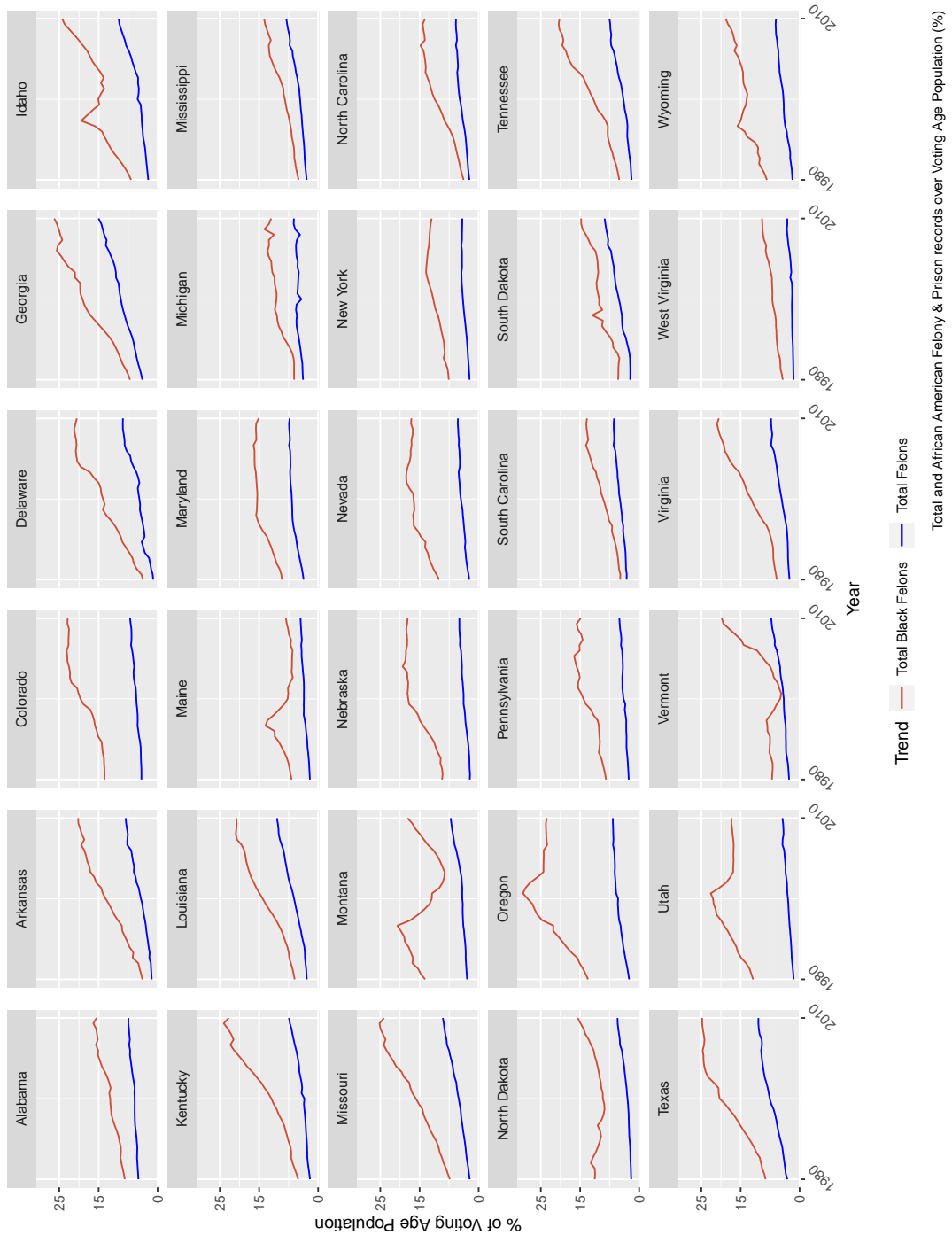
23

A Additional Tables and Figures

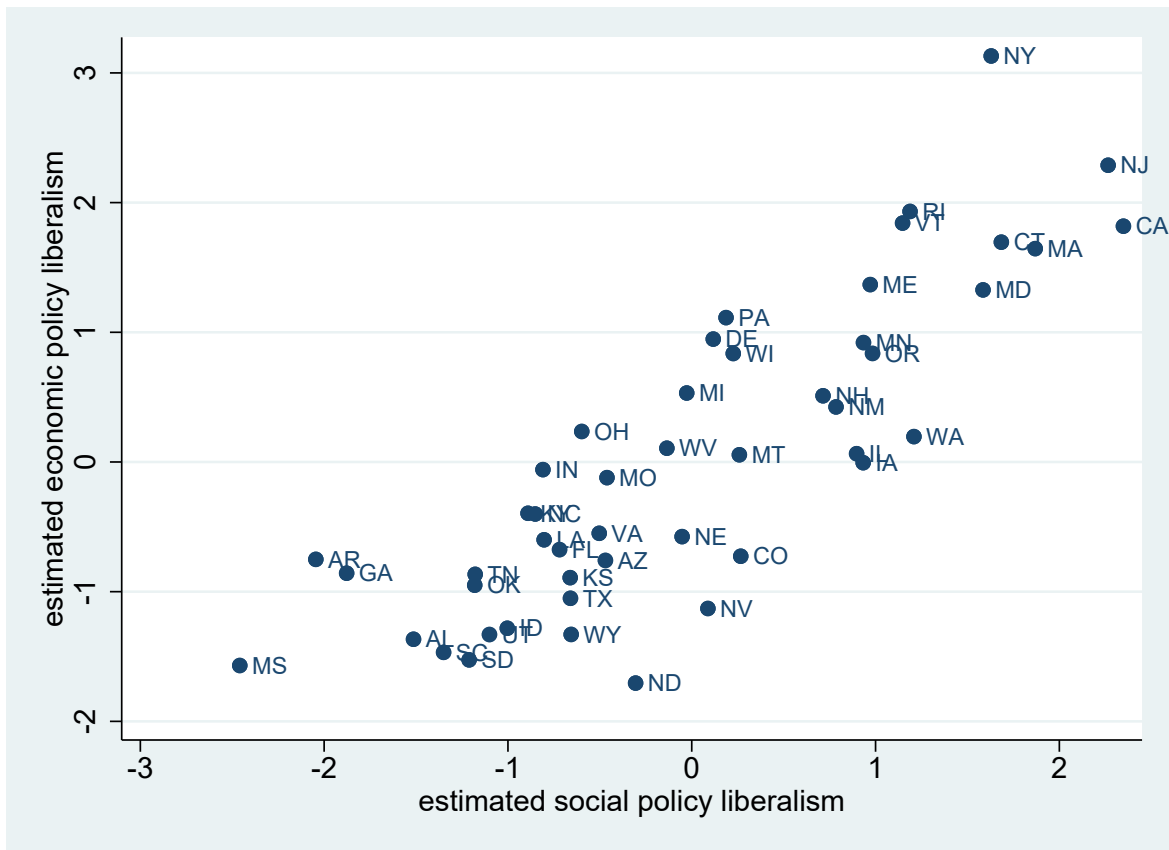Figure A.1: The Growth in the Felony Conviction Rate 1980-2010 by State

A.0

Figure A.2: The Caughey and Warshaw (2017) State Policy Data
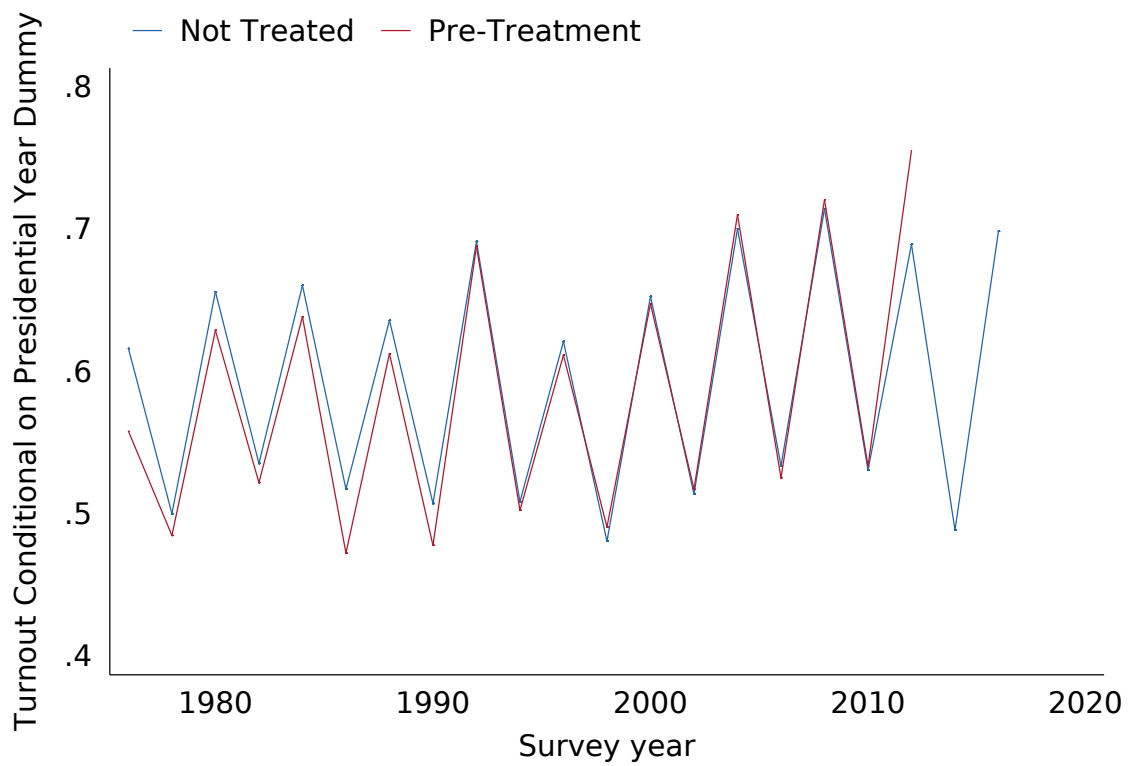
Data are for 2008.

A.1

Figure A.3: Unconditional Parallel Trends Plot: *Restoration*
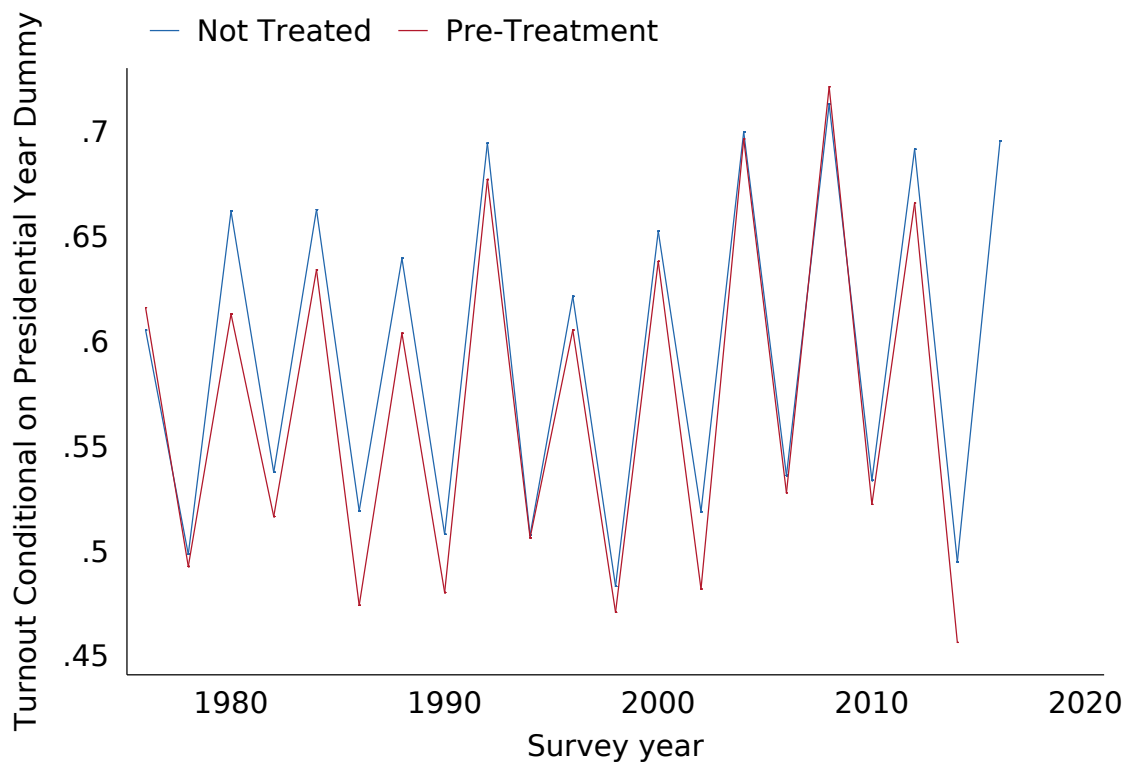
Data are from CPS.

A.2

Figure A.4: Unconditional Parallel Trends Plot: *Extension*
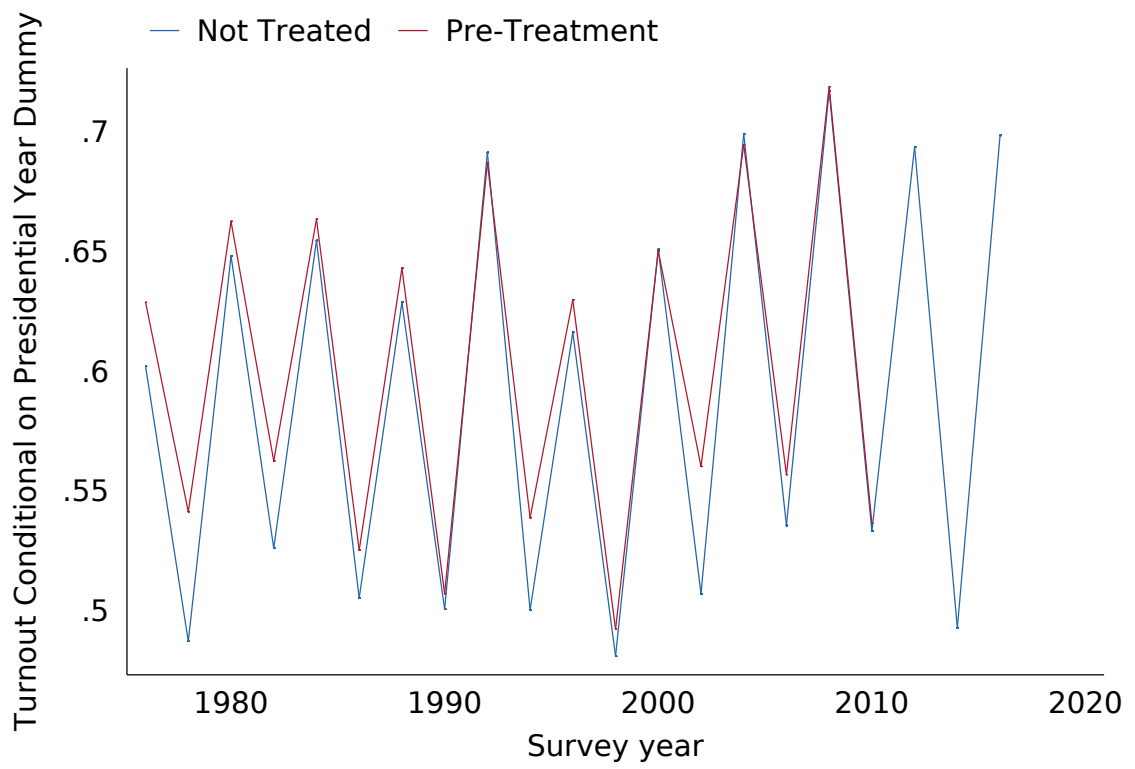
Data are from CPS.

A.3

Figure A.5: Unconditional Parallel Trends Plot: *Restriction*

Data are from CPS.

A.4

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Enfranchisement$_t$	0.10*	0.10*	0.05	0.16**	0.16**	0.08	-0.00	-0.00	-0.03
	(0.06)	(0.05)	(0.05)	(0.07)	(0.07)	(0.06)	(0.05)	(0.05)	(0.05)
Enfranchisement$_{t-1}$	0.13***	0.13***	0.06*	0.17***	0.18***	0.08*	0.01	0.00	-0.02
	(0.05)	(0.05)	(0.03)	(0.06)	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)
Enfranchisement$_{t-2}$	0.12**	0.12**	0.05*	0.18***	0.19***	0.08**	0.02	0.01	-0.02
	(0.05)	(0.06)	(0.03)	(0.06)	(0.06)	(0.04)	(0.05)	(0.05)	(0.04)
Pres Election Year	0.00	0.04*	-0.00	0.00	-0.02	-0.01	0.01	0.09***	0.00
	(0.01)	(0.02)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.03)	(0.03)
(mean) Agedef	0.09	0.26**	0.10	0.05	0.24*	0.10	0.10	0.22*	0.18
	(0.09)	(0.11)	(0.09)	(0.11)	(0.13)	(0.13)	(0.09)	(0.13)	(0.12)
(mean) EducLevel	-0.03	0.16	0.01	0.01	-0.21	0.03	-0.11*	0.57	0.49
	(0.06)	(0.48)	(0.07)	(0.07)	(0.56)	(0.09)	(0.06)	(0.43)	(0.31)
(mean) inc	0.01	0.41**	0.04	-0.02	0.25	0.02	0.06	0.33*	0.21*
	(0.04)	(0.18)	(0.03)	(0.04)	(0.22)	(0.04)	(0.04)	(0.19)	(0.11)
Year Effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
StateEffects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	FE	FE	IFE	FE	FE	IFE	FE	FE	IFE
Observations	816	816	816	816	816	816	816	816	816

Note: The dependent variable is the estimated state policy liberalism index of Caughey and Warshaw (2015) in columns 1–3. Columns 4–6 report results using the state social policy liberalism , columns 7–9 the state economic policy liberalism. Both measures are developed in Caughey and Warshaw (2017) in . All states.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by state in parentheses.

Table A.1: The impact of felony disenfranchisement reform on state policy liberalism.

A.5

B   State Level History

States	Year of First Felon Disenfranchisement Laws	Major Changes since 1995
Alabama	1819*	2003, 2016
Alaska	1959+	
Arizona	1912+	
Arkansas	1873*	
California	1849*	2016
Colorado	1876+	
Connecticut	1818*	2001
Delaware	1831*	2000, 2013
Florida	1838*	2007, 2011
Georgia	1868	
Hawaii	1959+	
Idaho	1890+	
Illinois	1870	
Indiana	1816*	
Iowa	1846*	2005, 2011
Kansas	1859+	2002
Kentucky	1792+	2015
Louisiana	1812*	
Maine	NA	
Maryland	1851*	2007, 2016
Massachusetts	2000	2000
Michigan	1963	
Minnesota	1857*	
Mississippi	1817*	
Missouri	1820*	
Montana	1909	
Nebraska	1875	2005
Nevada	1864+	2003, 2012
New Hampshire	1967	
New Jersey	1844*	
New Mexico	1911+	2001
New York	1821*	
North Carolina	1876	
North Dakota	1889+	
Ohio	1802*	
Oklahoma	1907+	
Oregon	1857*	
Pennsylvania	1860	
Rhode Island	1842*	2006
South Carolina	1868	
South Dakota	1889+	2012
Tennessee	1834*	2011
Texas	1845*	1996
Utah	1998	1998
Vermont	1793*	
Virginia	1830*	2013, 2016
Washington	1889+	2007, 2009
West Virginia	1863+	
Wisconsin	1848*	
Wyoming	1890+	2003, 2015

* States rattified criminal disenfranchisement in their constitutions, from state law search.

+ Criminal disenfranchisement laws with Statehood, state law search.

For the rest of the states see (Brooks, 2004).

B.0

C   State Disenfranchisement Laws

Inmates' Permit to Vote	Voting Restoration After Incarceration	Voting Restoration after Prison, Parole & Probation	Voting Restoration by Special Clemency
Maine Massachusetts Utah* Vermont	District of Columbia Hawaii Idaho Illinois Indiana Kansas Louisiana Michigan Missouri* Montana New Hampshire North Dakota Ohio Oregon South Dakota	Alaska Arkansas California* Colorado* Connecticut* Georgia Minnesota Nebraska* New Jersey New York* North Carolina* Oklahoma* Rhode Island South Carolina Texas* West Virginia Wisconsin	Alabama Arizona* Delaware Florida Iowa Kentucky Maryland* Mississippi* Nevada* New Mexico Pennsylvania* Tennessee Virginia Washington Wyoming
* Exceptions are listed Table C.2.			

Table C.1: State Disenfranchisement Laws in 1996

States	Exceptions in the current voting restoration process
Arizona	1st time offenders can have their voting rights restored automatically after prison, parole and probation.
California	State law not clear on probationers' rights to vote.
Colorado	State law not clear on probationers' rights to vote.
Connecticut	Proof of discharge is needed in order to restore voting rights.
Maryland	1st time offenders can have their voting rights restored automatically after prison, parole and probation.
Mississippi	Right to vote may also be restored by legislature or by administrative procedure for certain veterans.
Missouri	The existing law applies except for election crimes.
Nebraska	Certificate of unconditional discharge is needed for voting rights restoration.
Nevada	Administrative process or court order can also restore voting rights to ex-felons.
New York	State law not clear on probationers' rights to vote.
North Carolina	Certificate of unconditional discharge is needed for voting rights restoration.
Oklahoma	The existing law applies after a waiting period of equal length of the sentence.
Pennsylvania	In 1995, an amendment to the Pennsylvania Voter Registration Act denied voting rights to the incarcerated felons within 5 years of release from prison but no such restrictions were imposed on felons who were registered to vote at the time they were sent to prison.
Texas	2 years waiting period is mandatory and a discharge certificate is needed in order to restore voting rights.
Utah	The existing law applies except for election crimes and treason.

Table C.2: State Disenfranchisement Laws in 1996: exceptions

Inmates' Permit to Vote	Voting Restoration After Incarceration	Voting Restoration after Prison, Parole & Probation	Voting Restoration by Special Clemency
Maine	District of Columbia	Alaska	Alabama
Vermont	Hawaii	Arkansas	Arizona*
	Illinois	California*	Florida*
	Indiana	Colorado	Iowa
	Maryland*	Connecticut	Kentucky*
	Massachusetts	Delaware*	Mississippi*
	Michigan	Georgia	Nevada*
	Montana	Idaho	Tennessee
	New Hampshire	Kansas	Virginia*
	North Dakota	Louisiana	Wyoming*
	Ohio	Minnesota	
	Oregon	Missouri	
	Pennsylvania	Nebraska*	
	Rhode Island	New Jersey	
	Utah	New Mexico	
		New York*	
		North Carolina	
		Oklahoma	
		South Carolina	
		South Dakota	
		Texas	
		Washington	
		West Virginia	
		Wisconsin*	
* Exceptions are listed Table C.4.			

Table C.3: State Disenfranchisement Laws in 2016

C.1

States	Exceptions in the current voting restoration process
Arizona	1st time offenders can have their voting rights restored after prison, parole and probation.
California	In 2016, a legislation was passed to allow the inmates in county jails but not in state or federal jails.
Delware	People convicted of murder, bribery and sexual offenses are permanently disenfranchised.
Florida	In 2018, a constitutional amendment was initiated to restore voting rights to ex-felons. Persons convicted of murder or sexual offenses will have to petition the governor in order to restore their voting rights.
Kentucky	People convicted for treason, felony or bribery in an election or any high misdemeanor that the General Assembly may declare, will be permanently disenfranchised.
Maryland	People convicted for buying and selling votes can only have their voting rights restored by special clemency.
Mississippi	Persons convicted of murder, rape, bribery, theft, arson, obtaining money or goods under false pretense, perjury, forgery, embezzlement or bigamy will be disenfranchised permanently. They may still receive a pardon from the governor or by a two-thirds vote of both houses of the legislature. For other offenses rights are automatically restored after sentence completion.
Nebraska	There is a compulsory two years waiting period after the sentence completion before restoration of voting rights.
Nevada	1st time offenders convicted of less serious crimes can have their voting rights restored after completion of sentence.
New York	Governor Cuomo has passed an executive order restoring voting rights to parolees. Those on probation can vote in New York.
Virginia	The Department of Corrections is required to provide information regarding voting rights restoration to felons and assist the Governor with the review of applications. Individuals with felony convictions may petition the courts in order to attempt to restore their voting rights.
Wisconsin	If a person is convicted of treason, felony or bribery he or she is disqualified from voting unless the right is restored through pardon or Wisconsin state legislature s. 304.078 (3). In other cases of convictions, the right to vote is restored after imprisonment term or probation and the department of justice or the county jailer will inform the person in writing of this at the time of discharge.
Wyoming	First time offenders or persons convicted of non-violent felonies are eligible for automatic voting rights restoration after sentence completion.

Table C.4: State Disenfranchisement Laws in 2016: exceptions

D State-specific History

During the adoption of the US constitution states retained the right to extend suffrage and most of them disenfranchised felons by their constitutions. Some of them implemented felon disenfranchisement laws with adoption of their state-hoods. Appendix B contains the details. By the 1850s over one third of states had such laws in place and most of them had even disenfranchised ex-felons by 1920s. The reasoning behind disenfranchising the felons who have already served their sentences included rationales such as preserving "the purity of the ballot box" (Washington v. State, 75 Ala. 582, 585 (1884)).

Pennsylvania

Pennsylvania had adopted its first felon disenfranchisement law as early as in 1860. The recent changes in this state's history was in June, 1995 when section 501 of the Pennsylvania Voter Registration Act (Pa. Consol. Stat. §§ 961.101-961.109 (1995)) was passed. This act detailed that any elector who has not been confined in a penal institution for a conviction of a felony charge within the last five years, can only register to vote. As a result from 1995 to 2000, by state law, ex-felons had to wait five years after the completion of their sentence in order to be registered to vote. Moreover, there were no restrictions on the felons who were already registered to vote at the time of their sentencing. Consequently, this law was not only discriminatory against felons, but it also created a disparity among felons themselves, those who were registered before entering prison and those who weren't. This bill attracted very less attention initially but in 2000, a bill was proposed to strike down this provision. This attempt was heavily contested and was defeated in the State House by 80-118. In an attempt to thwart this law, NAACP filed a case against the state of Pennsylvania in June, 2000, claiming that the existing law discriminates between different types of felons without any substantial reason and also violates the equal rights protection clause of the Fourteenth Amendment. The Commonwealth Court of Pennsylvania ruled in favour of NAACP declaring the previous law unconstitutional (759 A.2d 442 (PA Cmnwlth 2000)) shortly before the November election which had a October deadline for registration. The ruling judge issued a temporary order to allow ex-felons register immediately.

Texas

Unlike Pennsylvania, Texas initiated disenfranchising ex-felons by putting a life time ban on their voting rights from its adoption of constitution in 1845. Several attempts have been made in Texas to extend voting rights to ex-felons. In the mid 1970s a constitutional amendment repealing the lifetime ban was passed in the legislative process but was rejected in a referendum. In 1977 another bill which was designed to extend automatic voting rights to ex-felons, was vetoed by the Governor after passing the legislature. In 1983 the lifetime ban was replaced by a eight year waiting period (House Bill 718) and it was reduced to 5 years the same year and finally reduced to a two year waiting period in 1985. During the 1996 legislative session, the 2 year waiting period was eliminated by the efforts of Harold Dutton, a democratic senator from Houston. This bill (adopted as a law) made voting rights restoration for ex-felons an automatic process after completion of prison term, parole or probation.

Iowa

Iowa had adopted its felony disenfranchisement law along with its constitution in 1846 and by this law (article II, section 5) anyone convicted of a felony and some convicted of an aggravated misdemeanor were permanently stripped of their voting rights in Iowa. The only way for an ex-felon to be re-enfranchised was by special clemency from the Governor or the President of the U.S. Such an application for clemency involved a substantial administrative process. Ex-felons had to submit an application listing all of their previous convictions, detailed accounts of the fines and fees paid and provide a reason behind their belief why executive clemency should be granted to them. The governor also consulted the recommendation of the Iowa Parole Board but retained the ultimate right over the granting of pardons. In case of success, the applicant would receive a certificate by mail in their last known address. In July, 2005 Democratic Governor Thomas Vilsack signed executive order no 42 and eliminated the need for ex-felons to submit an application. By this order, Iowa Department of Correction would send the governor's office each month a list of all felons discharged from the criminal justice system in the previous month. Even though the governor could choose from this list who will have their voting rights restored, in practice everyone had their suffrage rights restored and received a certificate stating so. This order also restored the voting rights of all ex-felons who completed their sentence prior to July, 2005, including those with denied applications. The Republican ex-governor (1983-1999) of Iowa, Terry Branstad was an open critic of this executive order on the ground of the unpaid financial obligations of many ex-felons. On January, 2011, upon reassuming the governorship he signed executive order 70 as the then governor of Iowa reinstating the application process that existed before 2005. The ex-felons who were discharged in December, 2010 were the last group having their voting rights restored automatically. From January, 2011 ex-felons have to complete their sentence and pay all the fines and fees to state before applying to have their voting rights restored. Notably, this order does not affect the felons whose voting rights were restored under the previous order. Recently in 2016, stating that all felons are permanently disenfranchised as they have committed "infamous crimes", the Iowa Supreme Court has upheld the ban on felony voting rights (Griffin v. Pate, 2016).

Washington

Washington has adopted felon disenfranchisement laws with its statehood in 1889. The law states that application towards restoration of voting rights of ex-felons are subjected to full payments of legal financial obligations (LFOs). This law however came to much debate after the gubernatorial election of Washington in 2004. The Democratic candidate Christine Gregoire defeated Republican candidate Dino Rossi by only 129 votes and it was alleged people with felony conviction had voted illegally whereas many were prevented from voting even though they should have had their rights restored. The Washington Secretary of State concluded in the aftermath that granting voting rights to felons after the completion of their sentences was an appropriate reform. In January, 2004 the Brennan Center for Justice filed an amicus brief in Madison vs Washington, challenging the condition of payment of LFOs, stating that this in essence constitutes to a permanent ban as most of the ex-felons can never repay the fines and fees. In March 2006, the plaintiffs won the case in the lower court of the King County Superior Court of Washington and the court overturned the provision of LFO payments in the voting restoration process. However, in June 2006 the Supreme Court nullified this decision on appeal and upheld the existing

law. In April 2009 House Bill 1517 which allowed ex-felons to regain their voting rights upon completion of their sentences, including prison, parole and probation, passed the Washington State House and Senate. This bill allowed for voting rights restoration without requiring the ex-felons to pay their LFOSs. Governor Gregoire signed the bill in May 2009 and in July 2009 it became effective as a law.

Virginia

The state that had seen the most debate regarding its felony disenfranchisement laws in the last decade is probably Virginia. The constitution of Virginia (1830) states that anyone who is convicted of a felony is denied their voting rights until the governor or any other appropriate authority reinstates it through an application process. On June, 2014 Governor Terry McAuliffe removed the application process regarding the voting rights restoration of ex-felons. Under the new rules, felons convicted of non-violent crimes (including drug crimes) can automatically regain their voting rights once they have completed their sentences (prison, parole and probation), paid all court fees and are not the subject of pending felony charges. In April, 2016 he signed an order restoring the voting rights to almost 200,000 Virginian felons who have completed their incarceration term, parole or probation, irrespective of their charges. This order was overruled by Virginia Supreme Court three months later in a 4-3 decision with the conclusion that this executive order was unconstitutional and restoration of voting rights should be an individual process. Governor McAuliffe subsequently continued to sign individual orders restoring the vote to ex-felons.

Subsequent to the period we study several other states have relaxed FD laws. In 2017, Wyoming enacted a House Bill (75) automatically restoring the voting rights of non-violent felons. Alabama House Bill 282 created a list of felonies that disqualifies a person from voting in 2017. New York Governor Andrew Cuomo issued an executive order providing voting rights to the parolees in 2018, whereas Colorado Senate Bill 150 of 2018 also gave similar rights to individuals on parole.