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Essays on the Political Economy of Inequalities

Richard, Brice

Princeton, NJ : Princeton University

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Essays on the Political Economy of Inequalities

Brice Richard

A Dissertation
Presented to the Faculty
of Princeton University
in Candidacy for the Degree
of Doctor of Philosophy

Recommended for Acceptance
by the Department of
Economics
Adviser: Professor Anne Case and Professor Thomas Romer

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Abstract

This dissertation addresses the relationships between economic and political inequalities. Recent scholarship has uncovered that the American political system represents citizens unequally, the preferences of the poor being less likely to impact policies than the preferences of high-income citizens.

The first chapter develops an original methodology to measure representation and hence documents inequalities of representation. Through a study of the evolution of income and taxes over three presidential administrations, I show that, overall, the American political system is highly unequal. However, I also find significant differences between the two major parties.

The second and third chapters analyze whether differences in political participation translate into differences in political representation, through a study of felon disenfranchisement in the United States. Chapter 2 assesses the effects of different regimes of felon disenfranchisement on the political participation of individuals at a higher risk of being disenfranchised due to a current or past felony sentence. I show that in states where felons are disenfranchised beyond completion of sentence, males and African American males in particular vote less when compared to other demographic groups in these states as well as the same demographic groups in states that do not disenfranchise felons for life.

Chapter 3 develops a theoretical framework linking political participation and levels of local public good provision from state governments. This relationship is hard to establish empirically, given potential endogeneity biases. In the context of felon disenfranchisement, I show that levels of political participation have decreased in poor and African American communities in states that disenfranchise felons for life, but that the same is not true in comparable states that only disenfranchise felons during their sentence. I then show that, consistent with the theoretical framework, levels of public good provision have followed similar patterns, with poor and African Amer-

ican communities receiving less compared to better off communities in states that disenfranchise felons for life. Overall the paper shows that lower levels of political participation do lead to lower levels of representation, therefore offering a potential explanation for inequalities of representation and highlighting the potential vicious circle between economic and political inequalities.

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I am extremely thankful for the many friendships that have carried me through becoming a doctor. From l'Ecole Alsacienne to Louis-le-Grand (shout-out to *les jeunes*) and l'X, I have been lucky to make friends ready to bear with me through the hardships and the distance. I am also grateful for the friends I made at Princeton, from my classmates (and especially the guys from 46 Spring St) to visitors and students across many departments. Cécile Gaubert and David Sraer offered me a home away from home and footsteps to follow in and Ettie and Theo brought me constant wonder.

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Introduction

This dissertation focuses on the political economy of inequalities and the relationships between economic and political inequalities. The issue of political inequalities, or how the voices of certain citizens are not heard, has gathered a lot of attention from scholars across disciplines (Bartels, 2008 ; Gilens, 2005 ; Gilens, 2012 ; Karabarbounis, 2011 ; Brunner et al., 2013).

My first chapter places these issues in a classic political economy framework. In this chapter, I revisit the Median Voter Theorem, and its prediction as found in Romer (1975) and Meltzer and Richard (1981). I analyze what the consequences of a "One dollar, One vote" rule or any rule where power is unequally distributed in a society would be in this framework and explain why the voter with median preferences is not pivotal under an unequal majority rule.

I then go on to design a methodology to *identify* the pivotal voter, or the segment of the income distribution to which she belongs. This methodology is based on two simple assumptions. First, I assume that preferences for taxes and redistribution are ranked monotonically with the ratio of one's income to the average income in society. This assumption can be theoretically derived (Romer, 1975; Meltzer and Richard, 1981) and has been empirically supported (Karabarbounis, 2011). Second, I assume that the distribution of income in a society evolves unevenly over time. This does not mean that the distribution of income evolves towards higher inequality:

even if inequality was decreasing, the richer would get (relatively) poorer and the poor (relatively) richer, hence different segments of the income distribution would be evolving in opposite directions (with respect to the average).

These two hypotheses allow me to say that the percentile of the income distribution that is pivotal (and therefore *represented*) can be identified as the percentile for which the correlation between the evolution of her income (relative to the mean) and taxes is the lowest. In other words, rather than trying to find support for the Median Voter Theorem by analyzing only the correlation between the *median* income and taxes over time, I analyze the same statistics for every percentile of the income distribution and am able to infer from this analysis the percentile that is pivotal. Therefore, I can *measure* political representation and furthermore, inequalities of representation.

I find that overall, over the period that covers the two Bush administrations and the Clinton administration, there were wide inequalities of representation in the United States. The pivotal voter was well above the median. However this finding was not true when the period is broken down by presidential administration. I find that parties matter and represent different segments of the income distribution. The pivotal voter under the Clinton administration was well below the median and the two Bush administrations display a similar pattern with a pivotal voter above the 80th percentile. I go on to show that wealth biases exist in a few democracies around the world. However the sign of the wealth bias is different in different countries. Therefore, it does not seem to be the case that a "One dollar, one vote" rule can be held responsible for the lack of support for the Median Voter Theorem.

There are a few potential channels through which economic inequalities could propagate to the political system. It is possible that different rates of electoral participation induce politicians to respond differently to different groups (Verba

et al., 1995). It could also be the case that campaign contributions (Campante, 2011; Bartels, 2008) or other types of political participation (such as contacting directly one's representative, Bartels, 2008) could play a large role in tilting representation towards the interests of the wealthiest. Wealthy Americans are also better organized and can set up lobbies to defend their direct or indirect interests (Gilens, 2012). Finally, the composition of the political class could also explain differences of representation (Carnes, 2013).

My dissertation goes on to explore the role of direct electoral participation in explaining differences of representation, through a study of felon disenfranchisement in the United States. There are large differences between states in the way the voting rights of current and former felons are administered. Certain states allow inmates to vote (Maine and Vermont) while others prevent all current and former felons from voting for life. I exploit this heterogeneity and show how the most severe regime of felon disenfranchisement is responsible for limiting the political participation of certain groups and in turn, the political representation of these groups.

In Chapter 2, I analyze the effects of felon disenfranchisement on the participation of individuals most likely to be affected by its administration, men and in particular African American men. I first revisit Miles (2004) and improve on the methodology of this paper. It is an established fact that women tend to vote at higher rates than men (Ansolabehere and Hersh, 2012) and I name the difference the "gender turnout gap." Chapter 2 looks at how variations of this "gender turnout gap" across dimensions are affected by felon disenfranchisement: the difference between the different regimes of felon disenfranchisement, the difference between how African Americans and Whites are affected by felon disenfranchisement, and the differential effect of time for the different regimes of felon disenfranchisement.

I first show that the "gender turnout gap" for African Americans is significantly larger in states that disenfranchise felons for life when compared to other groups

(African Americans in other states and Whites in both sets of states). The results are larger for lower educated groups, in line with the demographic composition of the felon population. I also show that this difference has been growing over time, which is consistent with the idea that, even with similar rates of incarceration, the growth of the disenfranchised population will be different in states that are the most severe compared to other states. Indeed, in the former, the size of the disenfranchised population is a *stock* variable where every new entrant in the penal system is added to the disenfranchised population and no one is subtracted, whereas in states where former felons are reintegrated into the polity, the size of the disenfranchised population is a *flow* variable.

Thus, I go on to exploit the differential impact of time on the size of the disenfranchised population as a way to identify the effects of felon disenfranchisement on political participation. I show that the "gender turnout gap" has been growing faster in states that disenfranchise felons for life than in other states. And this has been especially true for African Americans and high school dropouts. It seems therefore that I was able to establish that felon disenfranchisement laws have a significant effect on turnout despite felons belonging to populations less likely to vote to begin with.

My third chapter establishes a link between political participation and representation. I follow Ansolabehere et al. (2002) and Cascio and Washington (2014) in using transfers from state governments to lower levels of government as a measure of representation. This is justified in two ways. First, if one thinks of public good provision as desirable for all communities (even if at different rates), each community is trying to maximize the transfers they get from the government and therefore government spending can be understood as how much citizens are able to make their voices heard. Second, if politicians are motivated by reelection, they should

spend more where they expect more votes. Therefore, public spending can serve as a measure of the political clout of a community, with differences in spending reflecting differences in representation.

Establishing a link between participation and public good provision has proven difficult, as several parameters beyond the turnout rate of a community influence levels of public spending, such as the taste for the public good of the community. In other words, being able to separate the influence of the extensive margin (the turnout) from the intensive margin (the taste for public good) is a challenge. Furthermore, the two margins are likely to be correlated and even negatively correlated, as, for example, poor communities both turn out at lower rates and have a greater taste for the public good.

Felon disenfranchisement offers a very interesting setting for the study of the influence of political participation since it exerts a binding constraint on the political participation of certain demographic groups. I isolate the effects of felon disenfranchisement on participation and further on public good provision, through the interaction of three dimensions of variation. First, I exploit the differences between states that disenfranchise felons for life and states that do not. Second, I look at patterns of political participation and redistribution within states, depending on the demographic characteristics of the county, as the poor and African Americans are more likely to be disenfranchised as they are more likely to have been in contact with the penal system. Finally, I use variations over time, as the American penal system goes through a shift to a mass incarceration system with rates of incarceration skyrocketing.

The results show that patterns of participation and public good provision have been stable over time in states that do not disenfranchise felons for life but that both political participation and public provision have decreased in poor and African American communities in states that disenfranchise felons for life. This allows me

to establish that high participation does lead to higher public good provision from the state. In other words, political participation is indeed a way for communities to make their voices heard and differences in political participation are responsible for differences in representation. I am therefore able to draw attention to a mechanism through which economic inequalities lead to political inequalities.

This dissertation also sheds light on the social consequences of felon disenfranchisement. The issue of whether or not ex-felons should be allowed to vote has been a salient political issue for the past decade: the New York Times called the repeal of felon disenfranchisement "The idea of the year" in 2003 and in the 2012 Republican Primary, the more conservative Rick Santorum advocated for the reenfranchisement of ex-felons as his liberal opponent Mitt Romney made the opposite point. In his argument against felon disenfranchisement, Santorum noted: "This is a huge deal in the African American community, because I have very high rates of incarceration, disproportionately high rates, particularly with drug crimes, in the African American community."

Most of the literature has focused on the consequences of felon disfranchisement on political participation and whether it could be responsible for reversing the outcomes elections, with a particular focus on Florida (a state that disenfranchises felons for life) in the 2000 presidential election. Unlike previous scholarship, this dissertation demonstrates consequences of felon disenfranchisement on policy outcomes. The results indicate that the effects of felon disenfranchisement are concentrated on educational spending. Therefore, this dissertation calls to attention the potential formation of vicious circles of poverty, low education and crime in certain communities in the United States.

Chapter 1

Whose Voice is Heard?

Representation, Income and Taxes

In the canonical spatial voting models, policy is determined by the preference of the pivotal voter and, under a majority rule, by the preference of the median voter. However, empirical support for this theory has proven elusive both in the United States and in other developed economies and recently the idea of a wealth bias in the political system has been offered as an explanation for this lack of empirical support (Karabarbounis, 2011). In this chapter, I revisit this hypothesis by explicating its implications in a median voter framework. Furthermore, I investigate whether it is true that the poor are virtually voiceless (Bartels, 2008) and whether alternation between party rule insures equality of representation.

This chapter argues that the poor are not voiceless but are represented by the Democratic party, which is in line with results in Brunner et al. (2013), in a very different setting and at odds with results from Bartels (2008). I then show that party alternation does not help to balance the interests of the poor and the rich in the United States but rather that there is an overall wealth bias in the American political system. However, this bias is not present in similar ways in other countries and

cannot account for the lack of support for the median voter theorem across a sample of developed countries.

The empirical strategy of this chapter is informed by the rigorous examination of the consequences of a "one dollar, one vote" hypothesis in a median voter framework and relies on two assumptions. First, I assume preferences for taxes and redistribution evolve monotonically with income (more precisely with the ratio of income over average income), as in the seminal works of Romer (1975) and Meltzer and Richard (1981). Second, I build my empirical strategy on the idea that the distribution of income evolves unevenly over time. Different parts of the income distribution grow at different rates from one period to the other.

These assumptions allow to "identify" the pivotal voter as the percentile of the income distribution whose variations in income are most negatively correlated with variations in taxes over time. Given that the evolutions of neighboring income percentiles will be closely correlated, in practice, I can only find the broader segment of the income distribution to which the pivotal voter belongs. Therefore, this methodology allows to *measure* representation, as the correspondence between policies and the preferences of a defined set of citizens.

Nevertheless, I can show that very different segments of the income distribution are represented by the two parties under the two Bush administrations and the Clinton administration in the United States. During a politically-balanced period (with an equal number of years in power for Republicans or Democrats), the overall pivotal voter belongs to the top part of the income distribution. This indicates that political inequality exists in the United States but does not come from an exclusion of the poor, since they are well represented by Democrats. Furthermore, I provide evidence for the existence of different wealth biases in different political systems across the world. This illustrates that a uniform wealth bias towards the rich across time, parties and different political systems is not responsible for the lack of support for the

median voter theorem and that mechanisms of representation and inequality are more complex than previous results suggest.

This chapter is organized as follows; the first section is devoted to theoretical considerations on tax preferences, income and political representation. It presents the theoretical and empirical justifications for the assumptions as well as the identification strategy. The third section is devoted to the description of the data and the results are presented in the fourth section. The last section concludes.

1.1 Theoretical Considerations

In this section I present my empirical strategy, by examining the "one dollar, one vote" hypothesis and justifying my identification assumptions. First, I detail the relationship between political preferences and income, why it is appropriate to assume that preferences over taxes evolve monotonically with income and why this policy area is especially suited to the study of the relationship between income and representation. Second, I expose previously offered justifications for the lack of empirical support for the median voter theorem and clarify the theoretical implications of a "one dollar, one vote" rule in a median voter framework. Finally, I present the second assumption and my empirical strategy.

1.1.1 Political Preferences and Income

I focus on the relation between political preferences and income and justify my focus on preferences for redistribution and the assumption about their relationship with income.

General Political Preferences and Income

In this section I analyze the influence of income on political preferences and how heterogeneity of general political preferences by income levels could be problematic for studies that correlate preferences at different income levels and political outcomes.

McCarty et al. (2006) and Gelman et al. (2009) show the existence of an "income effect" on party identification. The importance of this effect might have varied over time, but it is positive for the entire time period considered in this chapter. However, many other factors matter when it comes to party identification or other aggregate measures of preferences (for example religion or race) and even if the "income effect" exists, it could be heterogenous across income groups, therefore leading to different levels of heterogeneity in the political preferences of different income groups.

This heterogeneity of preferences by income levels could be problematic for studies that correlate the preferences of different income groups with policy outcomes. For example, Bartels (2008) uses the average of an aggregate preference index across different groups and compares it to the DW-NOMINATE score of representatives to show that they are less responsive to the preferences of their less affluent constituents. However, if the "income effect" is stronger for the high-income group than for the low-income group (or if preferences of the low-income group are more heterogenous than the preferences of the high-income group), the median ideal point (not necessarily the ideal point of the median income voter) could be systematically closer to the average ideal point of the high-income group. This would be especially true if preferences among groups overlap (Tausanovitch, 2011).

This concerns has been partially alleviated by Gilens (2012), but it will be even less of of a problem for the study of redistribution. As Citrin and Green (1990) write: "Self-interest evidently plays a powerful role in shaping political preferences when the monetary stakes are large and visible." Hence, it is justified to use measures of

taxes and redistribution in a model of self-interested citizens.

Preferences for Redistribution and Income

In this section I detail the relationship between income and preferences for redistribution and justify the assumption that preferences for redistribution evolve monotonically with income.

Theoretically, Romer (1975) and Meltzer and Richard (1981) derive a mapping from income to preferences for redistribution by using a model with linear taxation, a lump-sum transfer, and by modeling the deadweight loss that taxation induces through reducing incentives to work. Their prediction goes further by stating that not only can preferences for redistribution be mapped to income, but to the ratio of each voter's income to the mean income (which serves as a proxy for the potential amount of redistribution in a model with a lump-sum transfer).

Empirically, there is strong support for the idea that income is a good predictor of preferences for redistribution, even if other factors matter as well. Alesina and La Ferrara (2005) show that in the United States current income is a powerful predictor of preferences for redistribution across a number of dimensions and controlling for demographic characteristics and prospects of future income (which are also significant). Karabarbounis (2011) repeats the exercise (without the prospect for future earnings) for a sample of OECD country and finds support for the hypothesis that preferences for redistribution are strongly correlated with income, for the entire sample and for each country separately. This result is similar to results from Corneo and Grüner (2002).[1] Finally, Brunner et al. (2011) use exogenous economic shocks to map support for redistribution to economic determinants and show that "positive economic shocks decrease support for redistributive policies." Of course, there are

[1]This study points that: "The standard approach that egoistic pecuniary motives are a major determinant of an individual's attitude towards redistribution is strongly confirmed."

certainly other considerations that enter one's preference for redistribution. First, future income prospects have a significant effect on preferences for redistribution, both theoretically (Bénabou and Ok, 2001; Piketty, 1995) and empirically (Alesina and La Ferrara, 2005). Second, beliefs about fairness and the role of luck in economic outcomes also play a role (Fong, 2001). However, these mainly explain differences within income categories, and income retains its predictive power with the inclusion of these variables. Third, in-group biases could modify one's preference for redistribution (Klor and Shayo, 2010) even if these results hold mainly inside the lab. Finally, concerns for one's position within society could counteract the monotonicity of the relationship between income and preferences for redistribution, such as "last place aversion" (Kuziemko et al., 2012) even though the scope of this phenomenon is limited to the study of the minimum wage (rather than overall redistribution) and results outside of the lab are mixed.

Therefore, assuming that there is a mapping from income to preferences over redistribution seems justified both theoretically and empirically. In this chapter, I follow the intuition behind Romer (1975) and Meltzer and Richard (1981) that not only is there a mapping between income and preferences for redistribution, but more precisely that there is a mapping from the ratio of each voter's income and the average to her preferences for redistribution. I will make the following assumption about preferences:

$$\tau = f\left(\frac{y}{\overline{y}}\right) \tag{1.1}$$

where τ represents preferences over redistribution in a general sense, but will in this chapter be proxied by income tax rates (with different definitions as I will present below), $\overline{y}$ is the mean income in the population, and f is a decreasing function.

1.1.2 The Median Voter Theorem and Representation

The Median Voter Theorem and the "One Dollar, One Vote" hypothesis

In a direct majority voting system, if the policy-space is unidimensional and preferences are single-peaked, the pivotal voter is the one with median preferences (Gans and Smart, 1996). This section clarifies the implication of the "one dollar, one vote" hypothesis for the median voter theorem.

The median voter is particularly important in spatial voting models. The median has the property that precisely half of the population (or half of total political power) prefers her ideal point to any point to the left of it (or a higher tax rate when preferences are over taxes) and half of the population prefers the ideal point of the median voter to any point to the right of it. This, combined with a majority decision rule, makes the ideal point of the median voter a Condorcet winner; it defeats every other alternative. Therefore, it should be the case that parties converge to the ideal point of the median voter. Given that Romer (1975) and Meltzer and Richard (1981) show that preferences over taxes decrease with income, over time, there should be a negative correlation between the income of the median voter (divided by the average income) and the level of taxation.

Empirical support for the prediction of Romer (1975) and Meltzer and Richard (1981) has proven difficult to provide. Many works can be cited, almost all of which are unable to support the prediction or even provide counterexamples.[2] Scholars have also used two general phenomena seemingly in contradiction with the median voter theorem to call it into question: the fact that Europe is more equal than the United States, but that taxes are higher in Europe, and the fact that, broadly speaking, inequality has risen in the last decades while the level of taxation has declined.

[2]See for example Bassett et al. (1999) and Milanovic (2000) or Karabarbounis (2011) for other references. Romer and Rosenthal (1979), surveying the local expenditure literature, conclude "that the studies fail to indicate that actual expenditures correspond in general to those desired by the median voter."

One explanation for this lack of empirical support is that people are unequally represented and that political weight is related to income, therefore interfering with the theory behind the median voter theorem (Bassett et al., 1999; Karabarbounis, 2011). The possible existence of a wealth bias in the political system has been documented in the United States. For example, Bartels (2008) writes that "the modern [U.S.] Senate comes a good deal closer to equal representation of *incomes* than to equal representation of *citizens*" (original emphasis). Other work includes Gilens (2005) and Gilens (2012). Theoretically, Campante (2011) builds a model of redistribution with campaign contributions that displays a wealth bias even though the political system does not strictly follow a "one dollar, one vote" rule.

However, if each person's political weight depends on his or her income, and is therefore unequally allocated, the voter who is at the center of the distribution of political power (the pivotal voter) is not the voter with median preferences and in the case of taxes, not the voter with median income. That is why it is contradictory to postulate that the political system obeys a "one dollar, one vote" rule and to try to support the median voter hypothesis, as in Karabarbounis (2011), whose empirical strategy seems more adapted to a model of coalition politics rather than a simple majority voting rule.

It is possible to give a graphic interpretation to consequences of the "one dollar, one vote" hypothesis on the difference between the voter with median income and the pivotal voter as seen on Figure 1.1.

On Figure 1.1, the solid line represents the "one person, one vote" repartition of political power, it is equally distributed among individuals, cumulative political power is the 45-degree line. Under the "one dollar, one vote" hypothesis, political power is not equally distributed and it follows the repartition of income, represented by the dotted line (also known as the Lorenz curve, Lorenz (1905)).[3] However,

[3]The curve is based on the actual distribution of income in the United States in 2000.

the requirements to get a majority remain the same: have at least one half of the cumulative political power. Therefore, the pivotal voter is the median for the "one person, one vote" model but it is well above the median for the "one dollar, one vote" model.

Party Divergence and Party Alternation

Another explanation for the lack of support for the median voter theorem is that parties diverge and the policies they enact when elected differ from the ideal point of the median voter. Whether parties converge or diverge is a long-lasting debate, both theoretically (Downs, 1957; Roemer, 2006; Wittman, 1990; Gul and Pesendorfer, 2009) and empirically (Blais et al., 1993; Imbeau et al., 2001; Brunner et al. (2013)).

However, research on inequalities of representation have either shown that parties do not respond differently to different parts of the income distribution (Bartels, 2008; Gilens, 2012) or ignored the issue of potential partisan patterns (Karabarbounis, 2011).

The method developed in this chapter to identify the segment of the income distribution to which the pivotal voter belongs will shed some light on this debate. By dividing the period I examine (the late 1980s to the early 2000s), I will be able to show that in the United States different party rules are associated with different pivotal voters. Indeed the distance between Democrats and Republicans is sizable and allows to identify a clear partisan pattern. These results are in tension with those of Bartels (2008) and Gilens (2005).

However, both the theoretical and the empirical literatures on the median voter theorem or inequality of representation have ignored the effect of party alternation. In this chapter, I will show that party alternation does not create a balance between the poor and the rich that could favor the median voter and resolve issues of political

inequality. I will show that over a politically balanced period in the United States, the American political system clearly exhibits a strong wealth bias towards the rich. Therefore, I will add to the literature on political inequality by confirming its results in the United States but casting a doubt on the idea that the poor are voiceless or that a "one dollar, one vote" mechanism is common in developed democracies.

1.1.3 Uneven Changes in the Distribution of Income and Identification Strategy

The identification strategy of this chapter will also rely on the intuition that the distribution of income evolves unevenly over time. By this, I do not mean that the distribution of income evolves necessarily towards higher inequality, but rather that relative positions (with respect to the mean income) are constantly evolving. Even if the distribution was moving towards more equality, the poor would be getting relatively richer and the rich relatively poorer, i.e. different parts of the distribution would evolve in opposing directions.

To formalize this intuition, I introduce the second assumption which the identification strategy will rely upon:

$$Corr\left(\frac{y_{it}}{\overline{y_t}}, \frac{y_{i't}}{\overline{y_t}}\right) \leq 1 \tag{1.2}$$

whenever $i \neq i'$ (where t represents time and i and i' different percentiles in the income distribution).

This assumption will help identify the pivotal voter as I will exploit the fact that her income has evolved, over the time period considered, differently from other parts of the income distribution. This in turn allows to escape the "fractile fallacy" (Romer, 2004; Romer and Rosenthal, 1979).[4]

[4]Romer and Rosenthal (1979) write, for example, that "more convincing evidence would be that the median fits the data substantially better than the mean, *the 25th percentile*, and other reasonable

I can now summarize the assumptions of the chapter and move to the identification strategy:

- *Assumption 1*: the preferred tax rate of the voter at income percentile i is: $\tau_i = f\left(\frac{y_i}{\overline{y}}\right)$, with f a decreasing function.

  This assumption has two consequences. First, since there is a mapping from income to redistribution preferences, the pivotal voter can be identified by her income. Second, the pivotal voter will experience a negative correlation between her relative place in the distribution of income ($\frac{y_p}{\overline{y}}$) and the level of redistribution. To facilitate the identification strategy, I will make a first order approximation of the preference for redistribution function: $\tau_p = \beta - \alpha\frac{y_p}{\overline{y}}$, with $\alpha > 0$, on which I will comment later.

- *Assumption 2*: The distribution of income evolves unevenly over time: $Corr\big(\frac{y_{it}}{\overline{y_t}}, \frac{y_{i't}}{\overline{y_t}}\big) \leq 1$, whenever $i \neq i'$.

By putting together the two assumptions, we get:

$$Corr\big(\tau_t, \frac{y_{it}}{\overline{y_t}}\big) = Corr\big(\beta - \alpha\frac{y_{pt}}{\overline{y_t}}, \frac{y_{it}}{\overline{y_t}}\big) = Corr\big(\frac{y_{pt}}{\overline{y_t}}, \frac{y_{it}}{\overline{y_t}}\big) \geq Corr\big(\tau_t, \frac{y_{pt}}{\overline{y_t}}\big)$$

Using this equation, I find that the percentile of the pivotal voter p is determined as the percentile that minimizes the correlation between evolutions of income at that percentile and taxes over time. Formally, the identification strategy of this chapter is that:

$$p = \arg\min_i Corr\big(\tau_t, \frac{y_{it}}{\overline{y_t}}\big)$$

Two comments can be made about this strategy:

- First, because at percentiles close to one another the evolution of income is highly correlated ($Corr\big(\frac{y_{it}}{\overline{y_t}}, \frac{y_{i't}}{\overline{y_t}}\big) \approx 1$), it is likely that over a range of percentiles,

summary statistics of the income distribution" (emphasize added), which is similar in spirit to the methodology developed in this chapter.

the correlation of income and taxes will be statistically indistinguishable. This will weaken my identification and I will not be able to isolate *the* percentile of the pivotal voter, but rather the *segment* of the income distribution she belongs to. Furthermore, because at percentiles far from each other the evolution of income (and especially of the ratio of income to average income) will be negatively correlated, over some segments of the income distribution, the correlation between income and taxes will be positive.

- Second, formally, the two assumptions predict that at the percentile of the pivotal voter, we have: $Corr\left(\tau_t, \frac{y_{pt}}{\overline{y_t}}\right) = -1$. However, this would only be the case if variations in the income of the pivotal voter was the only determinant of the level of taxes, which is probably false for numerous reasons, including that taxes do not serve exclusively a redistributive purpose.[5] In other words, my analysis will rely on the fact that some parts of the income distribution are better able to make their voices heard than others, given the constraints that all face and which partly determine the tax level. The "winner" in the political system in this model is not the person who gains the most from the redistributive system, but the one who, given the constraints (taxes are not only redistributive, or taxes cannot be entirely changed at each period), is the most able to drive taxes to respond to variations in her relative levels of income.[6]

1.2 Data

In this section I present the sources for the data for the United States and the other states I analyze. I explain how notions of income and taxes are constructed.

[5] Gilens (2012) insists on the role of interest groups for policy making for example.

[6] One could also note that the identification strategy does not rely on the function f being linear over the entire support. The linear approximation needs only to hold around the income of the pivotal voter, in this sense it is just a Taylor expansion approximated at the first order. α and β depend on the starting point of the income of the pivotal voter and the approximation needs only to be true in the neighborhood of this point.

1.2.1 United States Data

The first part of this chapter focuses on the United States, for which many income data sources are available. I choose to use the Current Population Survey from the U.S. Census Bureau and more precisely the Annual Social and Economic Supplement (ASEC) collected in March. Historically, it has been collected in March so that respondents could look at their tax returns and could be more precise given the proximity to the tax filing deadline. This survey is representative of the United States population and not just of the tax filers (like the public sample of the IRS). From this survey it appears that, for example in 1991, more than 7% of people above 18 had not filed a tax return, therefore making the IRS public sample not representative of the American population.

I use different income categories in the CPS to construct my own definition of income.[7] This construction presents the advantage that survey responses are not top coded, but top valued, meaning that beyond a certain point, the data are blurred (means are constructed by groups and reassigned to each member of the group). This still allows the calculation of an average income, that would be impossible using simulated variables in the CPS which are top coded. Furthermore, this mechanism concerns less than 1% of households for each variable. Therefore, in further analysis, I will disregard results about the extreme top of the distribution.

I include the following components in my construction of income (based on Henry and Day, 2005 and U.S. Bureau of the Census, 1993): wages and salary as well as gross revenues from self-employment or farm, income from interests, dividends and net

[7]The ASEC offers an Adjusted Gross Income (AGI) variable, whose definition is similar to the one used by the IRS in order to calculate taxes. However, since the ASEC does not include questions on capital gains and losses, this variable is simulated by the CPS using other surveys and the public sample of the IRS. It remains unclear whether or not those numbers should be used at face value. In a comparison between the CPS ASEC file and the public sample file from the IRS, it appears that some aggregate statistics (namely the repartition of tax units in income brackets) are very different between the two sources. Because we are interested in details of the income distribution, those discrepancies are worrisome. Furthermore, this variable is top coded in the ASEC at a "low" level, which threatens the calculation of the top percentiles and the calculation of the mean.

rentals, income from unemployment compensation, retirement funds and survivor's income, alimony payments received and other sources of taxable income.

I calculate the total income per tax unit and divide it equally among tax unit members. I use tax unit as the income sharing unit, because the tax rate will be applied to tax units. I remove people under 18, as they do not vote, and use the personal weights from the ASEC in order to calculate the percentiles. For the mean income, I simply use the same definition of income (for which the average is computable) and the personal weights from the ASEC.

In order to compute the average tax rate, I use the NBER tax simulator which gives for every year the average tax rate using data from the IRS public sample file. However, as I discussed earlier, this only offers the average tax rate among tax filers. In order to account for non-filers, I create a new average using the ASEC, assigning the average from the NBER tax simulator to all filers and 0 to non-filers and recalculating the average.[8] I use taxes as a measure of redistribution since the predictions of the model in Romer (1975) and Meltzer and Richard (1981) concern taxes, taxes are highly salient and "self-interest is less important in explaining attitudes towards spending than sentiment about taxes." (Citrin and Green, 1990)

In order to compute standard errors, I use a bootstrap procedure. I resample observations by using the weights in the CPS data with replacement, and then repeat the estimation procedure 2000 times (i.e. calculate average tax rate and income, percentiles, and the correlation between the ratio of the percentiles of income over the mean income with that average tax rate). It seems that 2000 calculations are sufficient to insure convergence. I then take the 2.5th and 97.5th percentiles to compute a 95% confidence interval (I do not try to find the minimal area that would correspond to a 95% confidence interval, therefore, I somewhat overestimate the interval).

Finally, since I have annual data for the United States during the period 1987-

[8]Indeed, the number of people who do not file taxes in a given year should be included in the calculation of the average tax rate.

2006, I will acknowledge that there might be a response time for the politicians to adapt tax rates and will evaluate $Corr\left(\tau_t, \frac{y_{it-1}}{\overline{y_{t-1}}}\right)$ rather than $Corr\left(\tau_t, \frac{y_{it}}{\overline{y_t}}\right)$. As a robustness check I will also evaluate the response of the average tax rate over a period of two years on the previous variations of income during two years. Doing so will insure that on average changes in income would precede changes in taxes by two years. This will show the results are not due to the endogenous adaptation of the income distribution to changes in tax rates (in the same fashion as Karabarbounis, 2011).

1.2.2 International Data

For the international data, I rely on the Luxembourg Income Study. It offers harmonized income surveys from different countries and sources. I use only those countries that have a sufficient number of observations (for some countries, data on income taxes are unavailable for most years) and countries for which the source of the survey has not changed (except for Canada, after checking that the change does not influence the results).[9]

As for the American data, I construct my own definition of income very close to the "market income" defined in the LIS, but including some taxable transfers. The advantage of the LIS is that since the micro file is not made available, the risk of identification of rich households is lower and the survey is usually not top valued, nor top coded.[10] For the tax rate, the individual-level simulations of the LIS were somewhat unreliable (with tax rates sometimes abnormally high). Therefore, I use aggregate values, namely I divide the average tax paid by the average income (similar

[9] Are therefore left Australia (1981, 1985, 1989, 1995, 2001 and 2003), Canada (1971, 1975, 1981, 1987, 1991, 1994, 1997, 1998, 2000 and 2004), Denmark (1987, 1992, 1995, 2000 and 2004), Finland (1987, 1991, 1995, 2000 and 2004), Norway (1979, 1986, 1991, 2000 and 2004) and the United States (1979, 1986, 1991, 1994, 1997, 2000 and 2004). I exclude Israel because of the "military burden" (Abu-Bader and Abu-Qarn, 2003) and because of the unusually large foreign aid received (Alesina and Dollar, 2000).

[10] Recall that top valued means that the data are blurred, but means can still be calculated, when top coded means that above a certain threshold, the real value is replaced by the value of the threshold, therefore preventing calculation of means.

to calculating the total income tax revenue on the total income).

However, two issues remain when using the LIS. First, the LIS does not offer a comprehensive personal survey. Therefore, I use the household surveys, divide income equally among household members (similarly to what I do for tax units with the CPS data). Nevertheless, I do not have access to a personal weight but only to household weights, which could potentially bias my calculation of the percentiles of the distribution of incomes if there existed systematic biases in the composition of household by income (for example if higher income households contained more adult members). Therefore, I construct a corrected adult weight that is the household weight times the number of adults in the household, for the calculation of the percentiles.[11] The second issue is that since I do not have access to the micro files of the survey I am unable to resample and compute bootstrapped confidence intervals. For these reasons, the exploration of the international data should be seen more as a validation of the method, rather than a precise calculation of the pivotal voter in each of these countries.

1.3 Results

This section will show results for the United States from 1988 to 2006 and, because data are available annually, I will split the period by presidential mandates. Therefore, I will be able to establish that the Democrats and the Republicans represent different segments of the income distribution, but that party alternation does not create a balance between the poor and the rich. Indeed, the United States displays a strong wealth bias over the entire period. Second, I will show results for other countries and suggest that it is not true that inequalities of representation are responsible for the

[11] This is similar in spirit to the CPS calculation of personal weights (since the head of the household and his or her spouse both receive the same weight as the household) and is based on the intuition that were I to calculate the number of adults in the population using those surveys I would sum the household weight times the number of adults per household.

lack of support for the median theorem but rather that different political systems exhibit different and even opposite wealth biases.

1.3.1 The United States after 1988

Before looking at the calculations, looking at the general trends for taxes and different percentiles of the income distribution over the period analyzed provides some interesting observations. On Figure 1.2 are plotted the variations in taxes and in the ratios of different percentiles (20th, 50th, 80th and 95th) to average income.

The graphs on Figure1.2 show that it is partly incorrect to talk about a decreasing tax rate over the period. The average tax rate declined between 1988 and 1991, but increased under the Clinton administration and then decreased sharply under the Bush administration. This observation already shows how important politics is. Over the same period, it is true that the 20th and the 50th percentiles were getting relatively poorer but at different rates during different periods. The 80th percentile got richer under the first Bush administration, had a chaotic evolution under Clinton and suffered less than other percentiles under the second Bush administration. At the same time, the ratio of the 95th percentile of income to the mean income increased almost continuously. A careful look at the numbers shows that behind a general trend there were wide discrepancies for different parts of the distribution, preventing us from drawing general conclusions. Those differences are crucial for the identification strategy.

A Partisan Pattern

The results in this section will rely on the correlation between taxes and variations in the distribution of income. In order to show differences by party rule, I will use presidential mandates as a time frame. I will analyze results concerning the first Bush administration, the Clinton administration and the second Bush administration.

The first graph on Figure 1.3 shows unaltered estimation of $Corr\left(\tau_t, \frac{y_{it}}{\overline{y_t}}\right)$ and the confidence interval constructed by bootstrap. The result is very noisy and hard to interpret and most estimates are not statistically distinguishable from 0, but for a small part of the distribution above the 75th percentile. This in itself is a revealing observation, the only part of the distribution that has been able to somewhat attain the objective of a negative correlation between variations in taxes and in relative incomes is the top of the distribution. When the graph is smoothed (using a centered linear weighting scheme taking five points into account for every point), a picture emerges more clearly. I am now able to identify a clear minimum (at $p = 80$) indicating a strong positive wealth bias under the first Bush administration.[12] A distribution of the estimator ($p = \arg\min_i Corr\left(\tau_t, \frac{y_{it}}{\overline{y_t}}\right)$) can be found in the appendix. However, at this stage and probably due to the very small number of observations (Bush was only in office for four years), it is too early to draw any conclusion.

When moving to the analysis of the Clinton administration, the graph on Figure 1.4 looks very different. Many points are now significantly negative and the graph is somewhat in four parts. A first broadly flat part between $p = 20$ and $p = 40$ where the minimum of the distribution lies then an increasing part between $p = 40$ and $p = 60$. These points are significantly different from the previous ones. Then again, there is a flat part before a steeply increasing part for the top of the distribution. The correlations are being driven by many parts of the distribution getting relatively poorer, while taxes are rising. However, it is interesting to note that it is the bottom part of the distribution for which the slow decline is the most closely related to the slow increase in taxes. If the graph is interpreted as the theoretical analysis leads to, I can identify the segment to which the pivotal voter belongs as between $p = 20$ and $p = 40$ (once again the distribution of the estimator is in the appendix), which is very different from and even symmetrical to the estimated pivotal voter for

[12] A second minimum can be identified at $p = 95$ but because of possible noise due to top values in the CPS at the very top of the income distribution, I do not think it is reliable.

the first Bush administration.

This shows that contrary to results from Bartels (2008) for example, parties do react differently to different parts of the income distribution and the poor are not voiceless. Therefore, it seems that there is not a uniform wealth bias in the American political system that would exist irrespective of party rules.

When I return to a Republican administration, the graph on Figure 1.5 looks a lot like the first Bush administration, but cleaner (probably thanks to the greater number of observations, or to the ideological stance of the second Bush Administration). Similarly to the first Bush administration, the part of the distribution for which the correlation between taxes and relative income is negative is above the 70th percentile and there is a minimum at $p = 80$ on the smoothed graph.[13] Once again, the full distribution of the estimator can be found in the appendix and shows that $p = 80$ is the best estimate for the second Bush administration.

The results point out the emergence of a truly partisan pattern, with the different parties appealing to different parts of the population. This shows a rather wide divergence between the two parties (with pivotal voters in the top or the bottom third of the distribution depending on the party in power). This result is in contradiction with the typical Downsian model of citizen candidate, the outcome of which is the position of the median voter (here the voter with median income). This means that a model that incorporates policy motivation for candidates is more realistic, whether this motivation is sincere or driven by campaign fund raising for example.

Aggregate Results

One can ask what the period as a whole looks like. Is each party somewhat able to neutralize the effects of the other? Is the median voter the arbitrator who decides to

[13]The actual full graph shows that $p = 95$ is another candidate, but from the analysis in the smoothed graph, it looks more like an exception and I tend to be wary of results at the very top of the distribution because of the top values in the CPS.

alternate between the two parties? The period analyzed is very interesting as it is politically balanced with almost as much time to the Democrats and the Republicans (in any case the graph covering only 1987 to 2006, a perfectly balanced period, is very similar as can be seen).

Figure 1.6 shows a graph much more similar to the "Republican" graph than to the "Democrat" graph, with the only portion of income for which the correlation between taxes and relative income is negative is above the 70th percentile and a minimum greater than 80 ($p = 86$, with the entire distribution in the appendix). If we were only to see this graph, we would conclude with Bartels (2008) and Karabarbounis (2011) that the American political system really obeys a "one dollar, one vote" rule. This would be an inaccurate interpretation of the set of results of this chapter. The poor are not voiceless, nor are they underrepresented, as their interest is taken into account by Democrat politicians. However, it looks like either the top of the distribution is able to mitigate the effects of a Democrat policy or the Republicans are more effective in achieving their partisan goal, so that on average voters at the top of the distribution are the real "winners" in the political system. In this sense, they constitute the pivotal group, a constraint for all policy makers. But they cannot be considered pivotal from a strict game-theoretical point of view, as the pivotal voter changes from one administration to the other, and therefore, there is not a unique pivotal voter for the entire period.

Robustness of the Results

I will now discuss the robustness of the results and offer several alternative specifications to what I have already done to show that results are robust to alternative explanations.

My focus on tax rates has been motivated by their salience both politically (cam-

paigns are run on tax promises) and individually (people know very well the amount they pay) and their relationship with self-interest (since by definition my model is one of self-interested voters). However, another measure of redistribution could be the average net of transfer tax rate. Therefore, I repeat the calculation with an approximation of this net of transfer tax rate (I subtract from the previously calculated average tax rate the average transfer received relative to the mean income). All results carry through as can be seen in the appendix, on Figure A.2.

There are also several potential endogeneity biases in this analysis that I will detail and address in turn. The first one concerns the direct effects of the business cycle on the average tax rate. Because the income tax system is in reality progressive if the population is getting richer, people will gradually move to an upper tax bracket and pay a higher tax rate. This will trigger an automatic increase in the average tax rate, even in the absence of a political intervention. I will make a few remarks on this issue. First, what matters in this model is the observed tax rate rather than real political intervention. For example, Clinton did not change the tax code in major ways and if anything lowered the average tax rate, by introducing more generous tax credits. However, I believe that his lack of action should not be interpreted as non-responsiveness to changes in the distribution of income. Clinton knew that because of the part of the business cycle he was in, tax revenues were growing and this allowed him to adjust the tax rate without lowering the average tax paid and to take fiscally costly measure (on medicare for example). Therefore, even if there were not major tax changes to the tax code, there were adaptations that were responsive to the changes of a specific part of the population. Furthermore, a politician is certainly always pressured into doing things by those who are not satisfied with his policy, and resistance to pressure by interest groups or citizens certainly shows a concern for those who are currently satisfied with the policies enacted.

However, to first correct for an effect of growth on the average tax paid, I run

regressions over the entire period of the following form:

$$\tau_t = \alpha + \beta \frac{y_{it-1}}{\overline{y_{t-1}}} + \gamma g_{t-1} + \epsilon_t$$

where g is the growth of the GDP (taken from the World Bank Development Indicators). This is not my preferred specification and I choose to include only growth as a control, in contrast with works that have tried to provide support for the median voter theorem. Indeed, my purpose is slightly different as I am interested in identifying the segment of the income distribution to which the pivotal voter belongs and am therefore interested in comparisons between percentiles not across countries. Whereas correlations are a natural tool of comparison because they are scaled from -1 to 1, when going from simple correlations to regression coefficients I lose some tractability and therefore some comparability. Furthermore, regression coefficients would be harder to interpret for smaller samples, so I limit myself to regressions over the entire period. I then compute confidence intervals by bootstrap (because it seems that in my particular calculation, the error comes most from estimating the percentiles rather than from the regression itself and the bootstrap procedure takes care of both effects). The results are shown in the appendix on Figure A.3 and are very similar to previously shown results. The results are robust to the homogenous effect of growth on the income distribution and then on the average tax paid.

There is a second potential issue in the heterogenous effects on distribution. Since only the upper part of the income distribution pays income taxes and this group is fragmented into multiple groups that pay different tax rates, uneven changes in the distribution of income of particular groups (and especially richer groups) could have an effect on the average tax as I have calculated it. Therefore, to correct for this potential bias, I use the tax rate computed by Daniel Feenberg with the NBER taxsim using the 1976 public sample inflated (or deflated). This tax rate is more

robust to changes in the distribution of income (because it uses the same sample across years) and therefore it is believed to be more reflective of actual changes in the tax code. The results are shown in the appendix on Figures A.5 to A.7, where for every period, I show the "new" graph with bootstrapped confidence intervals and a comparison between the original graph smoothed and the smoothed graph obtained with the corrected tax rate. The results are very consistent, they are even clearer for the first Bush administration, very similar for the Clinton administration and somewhat attenuated (though consistent) for the second Bush administration. This shows that the results are not due to mechanical effects of changes in the distribution of income (either homogeneous or heterogenous) on the average tax rate paid, as I calculate it.

There exists another possible endogeneity bias. Romer (1975) and Meltzer and Richard (1981) built their models by endogenizing the disincentive that high tax rates create on labor supply. This effect exists only if people take into account the tax rate in their work-leisure decisions, and therefore it is possible that the taxable income distribution adapts to changes in the tax rate.[14] In order to control for this possible adaptation effect, I refer again to the results with the tax rate calculated with a fixed sample, as this sample, by definition does not adapt to changes in tax rate. I also provide another robustness check by following Karabarbounis (2011) and calculating correlations for two-year averages. This means that tax rates follow changes in the distribution of income by two years on average, which makes less credible a possible prediction of tax rate and adaptation of income distribution to those. However, this diminishes the number of points to be used (by one or two) and those numbers were already low for the two Bush administrations (I provide results for the second Bush administration in the appendix, on Figure A.8). I focus

[14]A second possible adaptation is through mobility (as in Feldstein and Wrobel, 1998) but I do not believe that this constitutes a serious threat in the case of the United States in its entirety but only in the case of each particular state.

more closely on the results for the Clinton administration on Figure A.8 for which the curve, if different is very similar to the original one. Many intuitions given by the original graph carry through. For the entire period, I plot the 2-year averages with the original graph on Figure A.9 and its confidence interval to show that the 2-year average graph belongs almost entirely to the confidence interval of the original results. This suggests that the results are not statistically different, which should be interpreted as a sign of the robustness of the results.

1.3.2 International Results

Given the difference in the availability of data between the annual CPS March Supplement and the 5-year waves of the LIS, I think of the following results as highly suggestive that the method developed captures a relevant political feature (the existence of different wealth biases in different political systems) that can help explain the negative results in trying to provide support for the median voter theorem.

I start by comparing the results obtained for the United States using the CPS and the LIS data. Note that the LIS relies on the CPS March Supplement for their data, so the results should be very similar.

The two graphs on Figure 1.7 look very much alike, indicating that the method can certainly be transposed to using the less frequent LIS data. In other terms, the LIS with its 5-year waves, provides an appropriate time frame to use the method I develop. I will not capture every variations, but general trends that are relevant. I now move to analyzing results for five other countries. For each country, I plot the correlations using the household weight, and the adult personal weight that I have constructed (and which is my preferred specification due to the possible bias introduced by household composition).

The two graphs on Figure 1.8 are very similar, with a pivotal voter very high in

the distribution. This, as we saw for the United States, could be interpreted as either a high level of political inequality, or as the existence of a class above politics, able to get its way even through unsympathetic governments.

The graphs for Canada and Denmark, on Figure 1.9 show a different picture. Both show a similar orientation, with a flat part between $p = 60$ and $p = 90$ which make the identification of the pivotal voter very hard but also indicate a slightly positive wealth bias. We see that for Canada, using the two different weights gives a very different picture, indicating a strong household composition bias (namely high income households have more adult members, and therefore more political weight which could be interpreted inaccurately as a greater wealth bias in the political system). This justifies the use of the adult personal weight in the calculation.

Norway and Finland (found on Figure 1.10) are very interesting in that they show a negative wealth bias (and quite important for Finland with a pivotal voter around $p = 20$). The patterns emerging from these results are very much in line with analyses of levels of redistribution in different countries. Countries from Northern Europe redistribute significantly more than others and they display, with the methodology of this chapter, a negative or "pro-poor" wealth bias. This is remarkable since this chapter does not address the question of the size of government. Rather, I use its evolution over the years to reveal whose voice is heard and yet, the results are interestingly similar. Despite the small number of countries for which data were available, the results indicate the importance of persistent socio-cultural determinants for redistribution, as the countries with the highest tax rates (and the largest government spending) are also the ones where the bottom of the distribution is heard most strongly.

When plotting a few countries in the sample together on Figure 1.11, we observe two things. First, the ranges of the correlations are very different between countries (for example the graph for Canada is a lot flatter than the others), showing that in some countries, tax levels seem less responsive to changes in the income distribution

than in others. Second, running a cross-country regression to find support for the median voter theorem would be very similar to adding the correlations at $p = 50$ (even though the studies that have done so have also corrected for numerous other determinants of redistribution). The graph shows that this would very likely result in a null correlation. This null result would come from a wealth bias in almost each of the countries studied in this chapter as suggested in Karabarbounis (2011). However, their sign and importance differ from one country to the other, suggesting that it is certainly true that money matters in politics, but that its role has yet to be fully analyzed.

1.4 Conclusion

This chapter shows that a "one dollar, one vote" rule cannot explain the lack of empirical support for the median voter theorem as found in Romer (1975) and Meltzer and Richard (1981). The poor are not voiceless, in line with results by Brunner et al. (2013), if with a very different methodology, but at odds with Bartels (2008). Democrats represent the lower part of the income distribution and Republicans the upper part. These results might be explained by the salience of tax policies, since it is a determinant of how much political parties can abstract from the preferences of the poor citizens (Gilens, 2012).

Overall, alternation between party rules does not imply a balance between the interest of the poor and the rich. The American political system does display a large wealth bias in favor of the rich and sees the emergence of a class that is above politics, that is able to mitigate the effects of its opponents and have its way on average.

This chapter also shows the importance of persistent institutional and socio-cultural features for redistribution, and shows that if a wealth bias exists in various

countries, its sign and magnitude differ. Therefore, political inequality alone cannot explain the lack of support for the median voter theorem, even though money in politics is certainly important (as suggested in Karabarbounis, 2011). This work sheds a new light on political representation and hopefully opens the way to more research on the impact of money in politics both over time and in different countries.

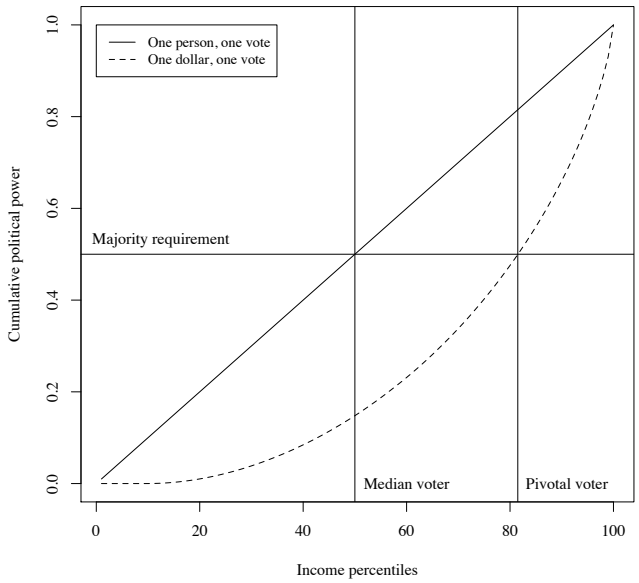
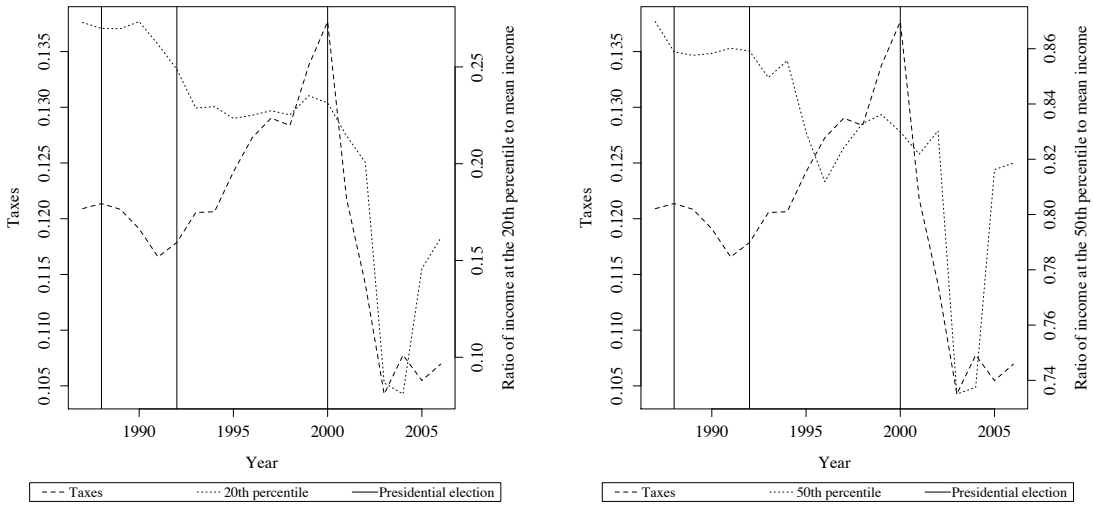
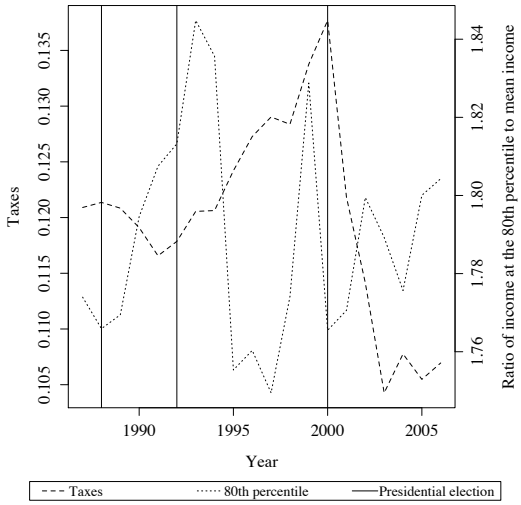


Figure 1.1: The pivotal voter under the "one person, one vote" and the "one dollar, one vote" hypotheses

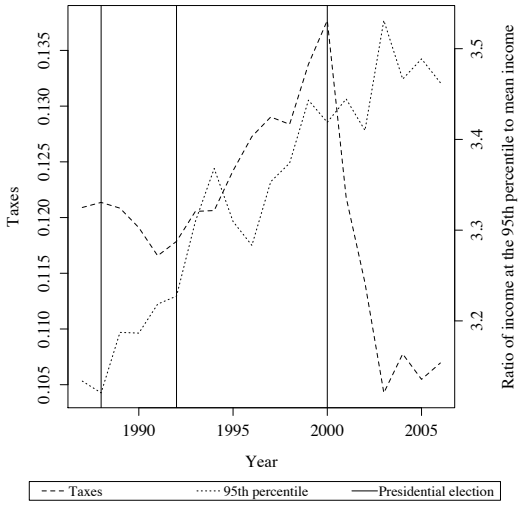


(a) 20th

(b) 50th

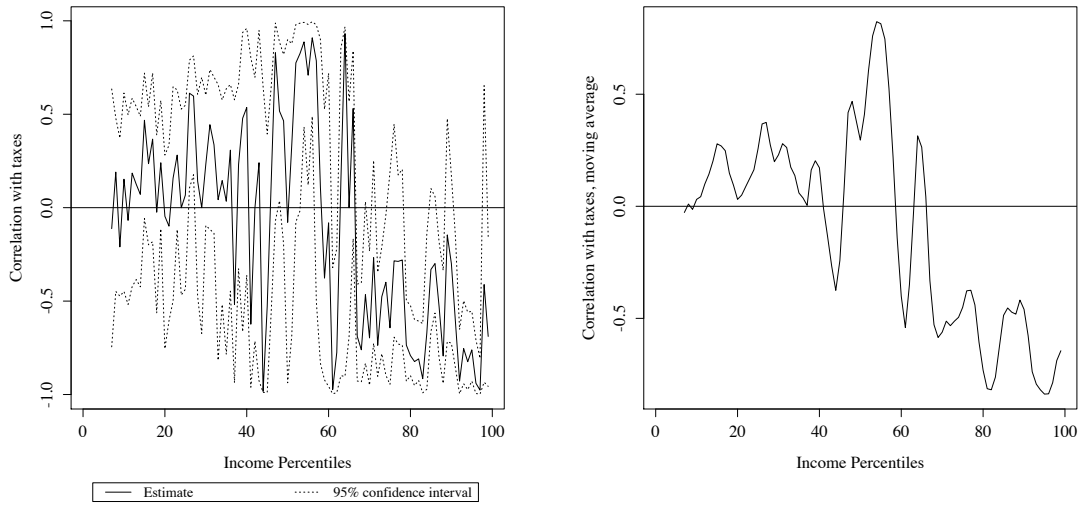


(c) 80th



(d) 95th

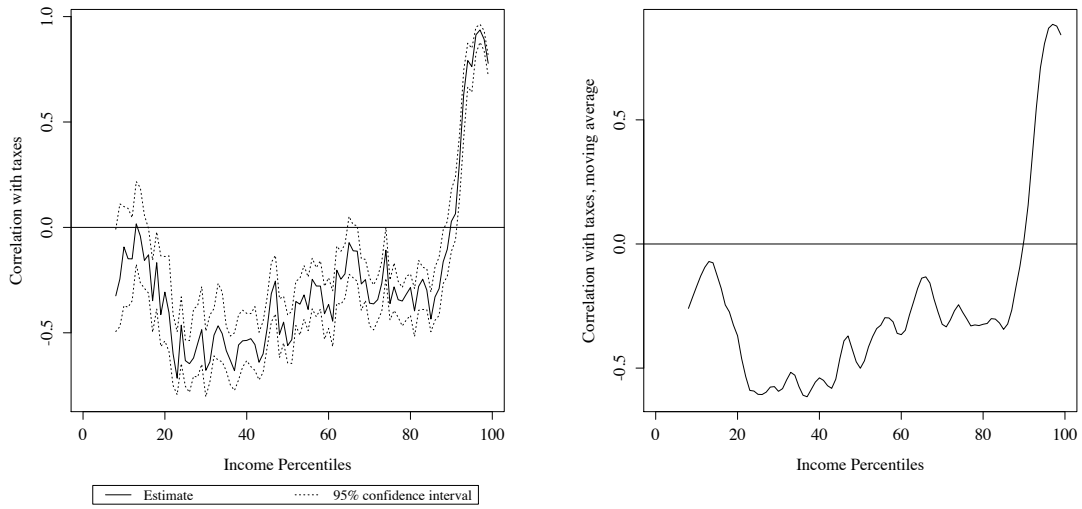
Figure 1.2: Evolution of taxes and income at the 20th, 50th, 80th and 95th percentiles, United States, 1987-2006



(a) Bootstrapped Confidence Interval (b) Smoothed

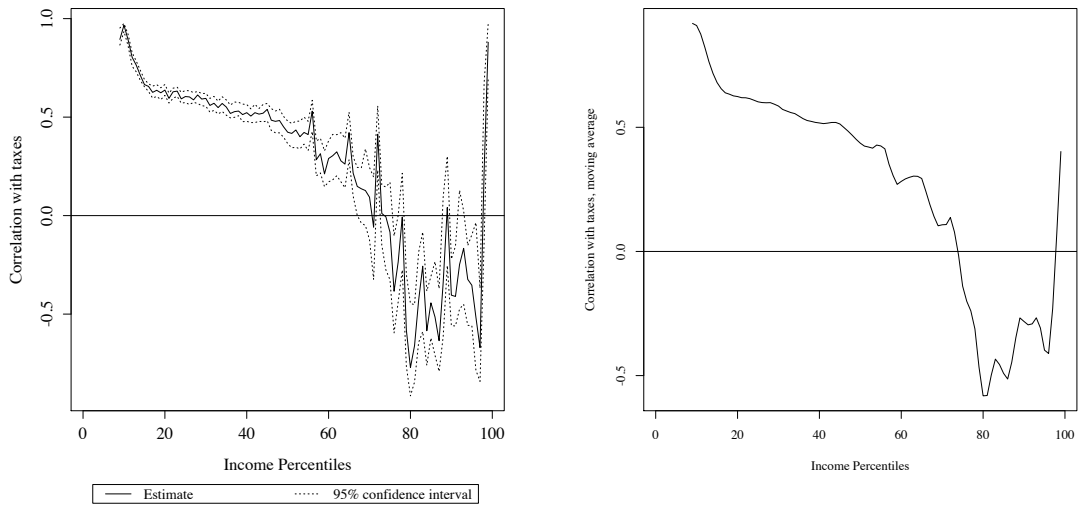
Figure 1.3: The United States under the First Bush Administration, 1988-1992, With Bootstrapped Confidence Interval and Smoothed.

Notes: These graphs show the correlations between taxes and every percentile of the income distribution from 1988 to 1992 in order to identify the pivotal voter under the first Bush administration, as described earlier. The first graph shows the raw correlations with the 95% confidence interval obtained through the bootstrap method and the second graph shows the same correlations smoothed for clarity's sake.



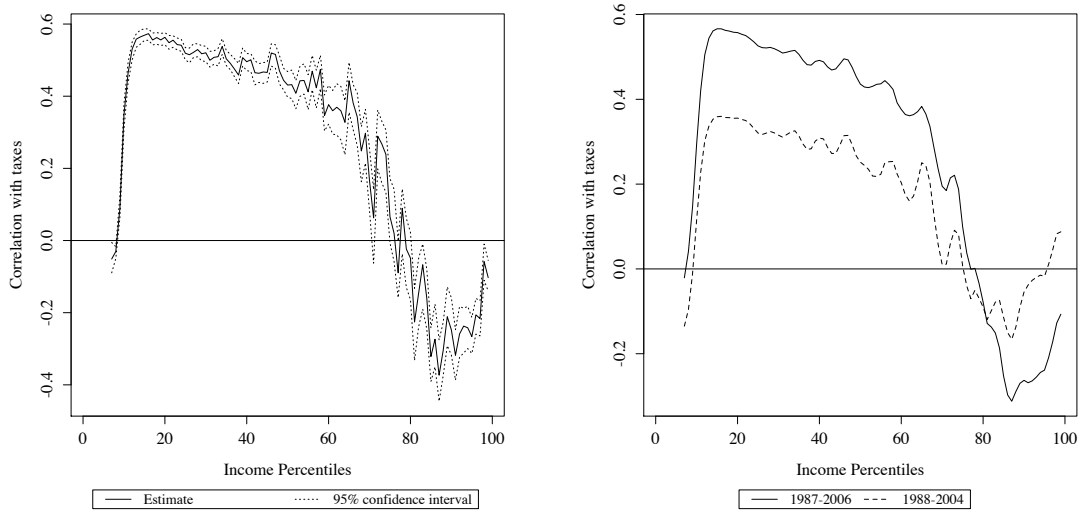
(a) Bootstrapped Confidence Interval (b) Smoothed

Figure 1.4: United States under the Clinton Administration, 1992-2000, With Bootstrapped Confidence Interval and Smoothed.



(a) Bootstrapped Confidence Interval (b) Smoothed

Figure 1.5: United States under the Second Bush Administration, 2000-2006, With Bootstrapped Confidence Interval and Smoothed.



(a) Bootstrapped Confidence Interval (b) Smoothed

Figure 1.6: United States Aggregate Graph, 1987-2006, With Bootstrapped Confidence Interval and Smoothed.

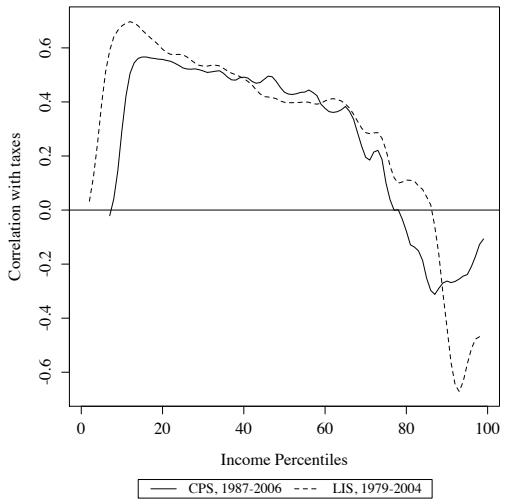
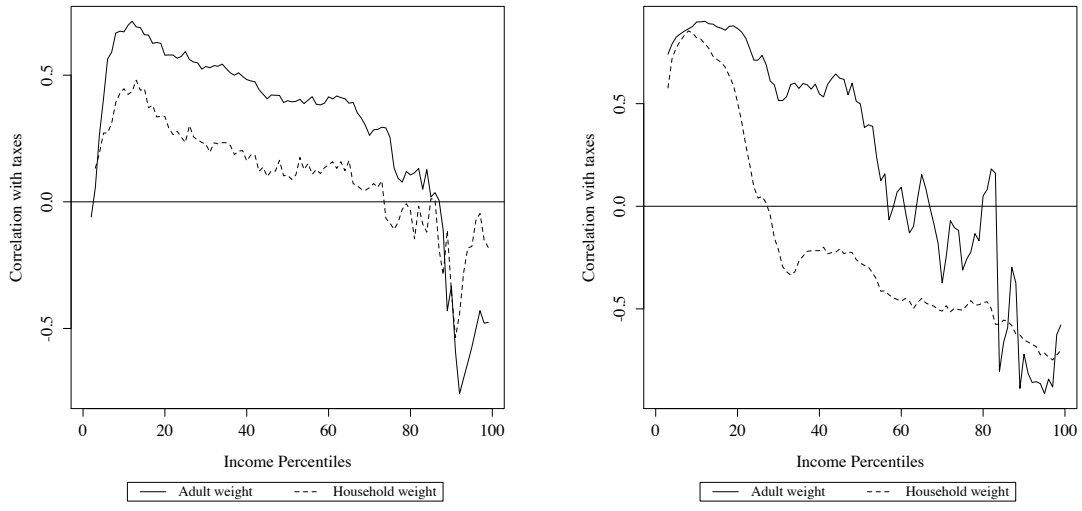


Figure 1.7: A comparison between CPS and LIS calculations.

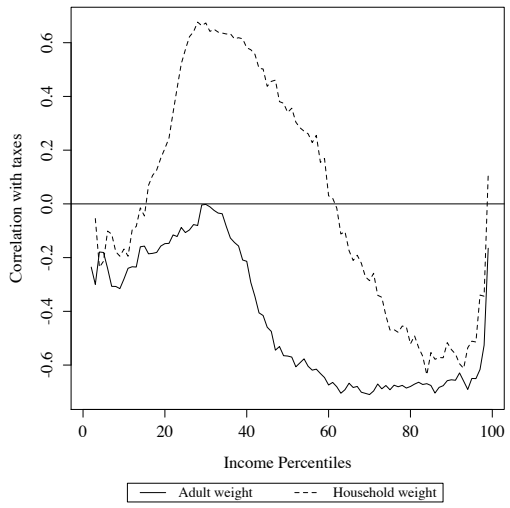
Notes: This graph shows the correlations between taxes and every percentile of the income distribution obtained independently from yearly March CPS from 1987 to 2007 and from the 5-year waves of the LIS from 1979 to 2004. Both graphs are smoothed.



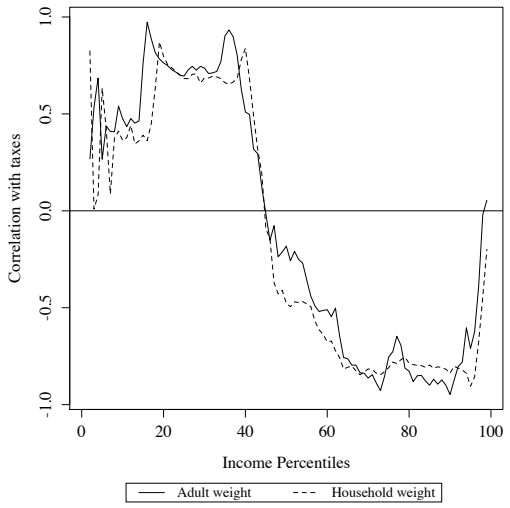
(a) United States, 1979-2004 (b) Australia, 1981-2003

Figure 1.8: United States and Australia.

Notes: These graphs display the correlations between taxes and every percentile of the income distribution. They are obtained from the LIS. Both graphs display the results when the percentiles of the income distribution are calculated using adult or household weights.

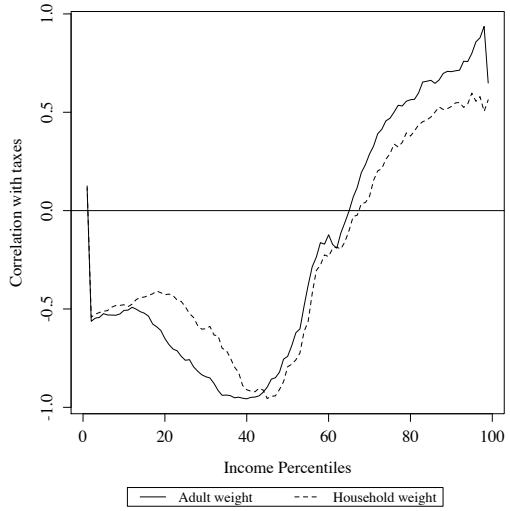


(a) Canada, 1971-2004

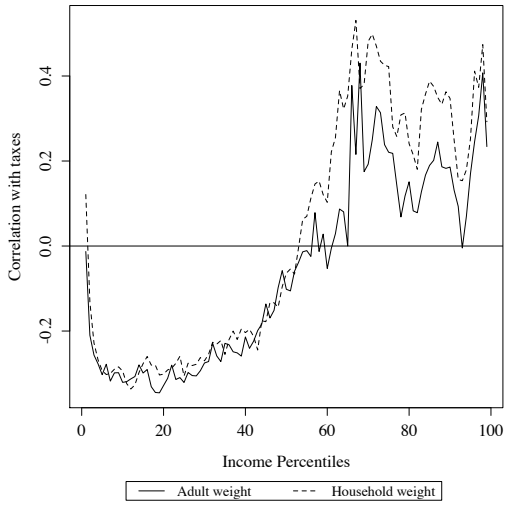


(b) Denmark, 1987-2004

Figure 1.9: Canada and Denmark.



(a) Norway, 1979-2004



(b) Finland, 1987-2004

Figure 1.10: Norway and Finland.

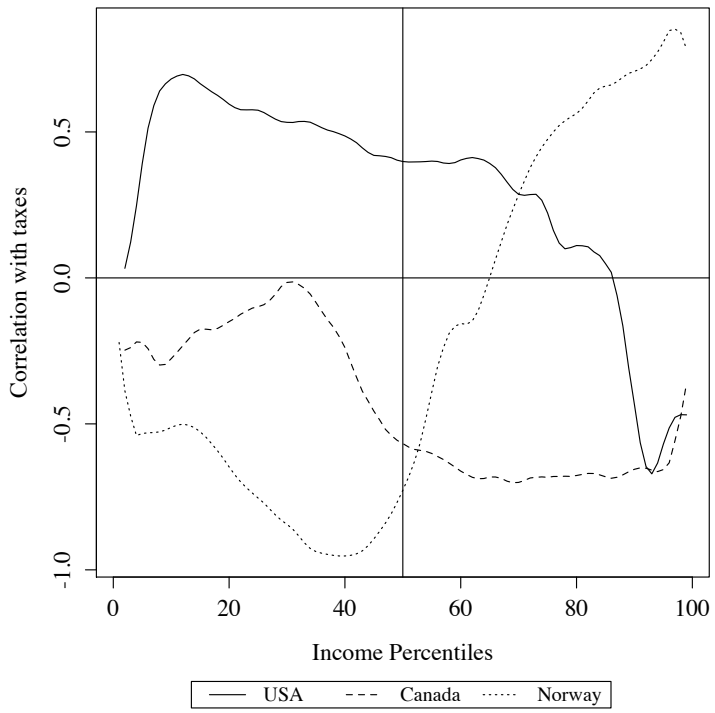


Figure 1.11: International Perspective.

Notes: This graph is a compilation of previous graphs, showing only the United States, Canada and Norway.

Chapter 2

The Consequences of Felon Disenfranchisement on Individual Political Participation

Political participation in the most simple way, through turning out to vote, is a foundation of the ideal of democracy and of the "one person, one vote" principle. However, different rates of political participation in different demographic groups is a potential threat to this principle and could be a channel through which certain citizens' voices are not heard in the political system.

The different regimes of felon disenfranchisement, responsible for at least 5.85 million people disenfranchised in 2010 (Manza and Uggen, 2012), seem to offer an interesting setting to study the effect of lower participation rates on the political system. Furthermore, the demographic imbalances of the prison population translate into specific implications of felon disenfranchisement for different demographic groups. The next chapters constitute an exploration of the effects of felon disenfranchisement on African Americans and the poor, who are disproportionately affected by felon disenfranchisement. Therefore, the rest of the dissertation will use variations in the

severity of felon disenfranchisement across states and in the effects of felon disenfranchisement across demographic groups to isolate the effects of felon disenfranchisement on political participation and furthermore the effect of political participation on representation.

Chapter 2 will focus on the question of whether felon disenfranchisement, and especially its most severe form, disenfranchisement for life, impacts the political participation of males (who represent more than 90% of convicted felons) and African American males in particular. In this chapter, I analyze individual voting behaviors and how these are affected when the likelihood of being disenfranchised increases (because of specific characteristics and the severity of the disenfranchisement regime that one faces).

The question of the effect of felon disenfranchisement on individual political participation is controversial. Given that turnout is generally low in populations with similar demographic characteristics to that of ex-felons, some have argued that these laws were essentially inconsequential. Indeed, given the demographic characteristics of the felon population and the variety of threats to re-entry that they face (Harris et al., 2010; Pager, 2003), it is unclear that former felons would participate even given the right to do so. Miles (2004) finds that felon disenfranchisement does not affect the turnout of African American males and this chapter attempts to revisit this work, by improving its methodology and using more recent data. Conversely, Uggen and Manza (2002) simulate both the size of the disenfranchised population and their potential participation based on their demographic characteristics and finds a potential turnout of 30% to 40%. More generally, the question of the potential turnout of ex-offenders remains open with estimates ranging from 5% (Haselswerdt (2009) in a study that had a sample of 600 ex-felons followed for two years in the state of New York) to 40% (Burch (2011) in a study that found differences in the turnout of ex-felons across five different states). This chapter is concerned with showing *a*

significant effect of felon disenfranchisement on specific demographic groups and is not primarily concerned with issues of measurements.

The first section of this chapter will provide background on felon disenfranchisement, and its connections with race and class. I will then go on to present two empirical identification strategies for exhibiting the impact of felon disenfranchisement laws on registration and turnout. I will compare aggregate results (on registration and turnout rates) with individual choice models. The third section will present data and results. The fourth section concludes.

2.1 Felon Disenfranchisement

2.1.1 Felon Disenfranchisement in the United States

Laws regarding the voting rights of convicted and former felons vary widely between American states.[1] Maine and Vermont apply no restriction to the voting rights of felons, when the other states range from preventing inmates from voting to preventing inmates, probationers, parolees and ex-felons (Uggen and Manza, 2002). The latter are going to be the focus of this chapter. Throughout the chapter, I will distinguish "non-disenfranchising states" (all the states that do not systematically prevent ex-

[1]The classification I will use in the rest of the chapter is as follows. It is based on felon voting right restrictions (as found in Uggen and Manza (2002) and confirmed in Porter (2010)):

- No restriction: Maine and Vermont
- Inmates Only: Hawaii, Illinois, Indiana, Massachusetts, Michigan, Montana, New Hampshire, North Dakota, Ohio, Oregon, Pennsylvania, South Dakota and Utah
- Inmates and Parolees: California, Colorado, Connecticut and New York
- Inmates, Parolees and Probationers: Alaska, Arkansas, Georgia, Idaho, Kansas, Louisiana, Minnesota, Missouri, New Jersey, North Carolina, Oklahoma, Rhode Island, South Carolina, Texas, Washington, West Virginia and Wisconsin
- All and Some Ex-felons: Arizona, Maryland, and Tennessee
- All and All Ex-felons: Alabama, Delaware, Florida, Iowa, Kentucky, Mississippi, Nebraska, Nevada, New Mexico, Virginia and Wyoming.

In the following analysis, the District of Columbia is dropped.

felons from voting) and "disenfranchising states" (where ex-felons are systematically disenfranchised for life), even if among the former, some disenfranchise repeated offenders for life for example.

There are several motivations for excluding felons from the political process. The foundations for disenfranchising people who have broken the law are found in the Aristotelian view that "criminals who break the laws cannot govern themselves, and hence are not fit to govern others. They can have no part in the regime. [...] And perhaps criminals even have no place in the community itself, depending on the nature and severity of their crimes" (Planinc, 1987). In Rome, a citizen could be deprived of the right to vote if he was stricken by "infamy" (Itzkowitz and Oldak, 1972). This tradition persisted through the Roman Empire and the Middle Ages under the concepts of "outlawry", "civil death", and, later in England, "attainder." Similar provisions were included in eleven state constitutions adopted between 1776 and 1821 (Keyssar, 2009) and on the eve of the Civil war, "nineteen of the states disenfranchised [criminals] in one way or another" (Porter, 1918). Keyssar (2009) adds that"in almost all cases, the disenfranchisement was implicitly permanent." By 1870, around 90% of the states were disenfranchising felons and most of them beyond completion of sentence (Behrens et al., 2003). Other motivations for felon disenfranchisement have been expressed. As with any form of punishment, deterrence and incapacitation are theoretically important factors, even though the specific deterrent effects of felon disenfranchisement have not found support (Itzkowitz and Oldak, 1972). In terms of incapacitation, a prevalent idea is that current or former felons could corrupt the electoral process by affecting election results (and in particular with respect to penal laws) or by committing election fraud. However, the Supreme Court has ruled, in *Carrington vs. Rash* (1965) that "'fencing out' from the franchise a sector of the population because of the way they may vote is constitutionally impermissible." Finally, it is unclear that crimes having little to do with the political process would be good

predictors of future election violations.

Felon disenfranchisement has existed in several forms from its inception in ancient Greece. By 1870, around 90% of the states in the United States were disenfranchising felons and most of them beyond completion of sentence (Behrens et al., 2003). When turning to the recent history of felon disenfranchisement in the United States, it is important to note the stability of the laws from the mid-1970s to the recent periods, even if this institution has received attention in recent years.[2]

Laws regarding the voting rights of convicted and former felons vary widely between American states. Maine and Vermont apply no restriction to the voting rights of felons, when the other states range from preventing inmates from voting to preventing inmates, probationers, parolees and ex-felons (Uggen and Manza, 2002). The latter are going to be the focus of this chapter. Throughout the chapter, I will distinguish "non-disenfranchising states" (all the states that do not systematically prevent ex-felons from voting) and "disenfranchising states" (where ex-felons are systematically disenfranchised for life), even if among the former, some disenfranchise repeated offenders for life for example.

The distinction I draw between "disenfranchising states" (states that disenfranchise felons for life) and "non-disenfranchising states" (where voting rights are immediately restored upon completion of sentence) comes from the reasoning that in disenfranchising states, the disenfranchised population is a stock that increases with every new entry in the penal system, whereas in non-disenfranchising states, the disenfranchised population is a flow (where every "exit" from the penal system through the completion of one's sentence is followed by a systematic reenfranchisement). The fact that the size of the disenfranchised population is a "flow" or "stock" variable

[2]For example in Iowa, where executive orders from Governors Vilsack (in 2005) and Branstad (in 2011) first eased and subsequently toughened the restoration process for former felons, although with small effects (Meredith and Morse, 2013). New Mexico changed its law in 2001. However, I believe that this is not enough to turn around the effect of decades of felon disenfranchisement, especially around times of high penal activity in the US. In any case, dropping New Mexico from the disenfranchising group does not affect the results.

will also have different implications for the growth of the disenfranchised population under the two regimes, a difference I will exploit later in the chapter.

2.1.2 The Imbalanced Impact of Disenfranchisement Laws

The issue of felon disenfranchisement has taken a greater importance in the context of recent evolutions of the American penal system. The most obvious is the very rapidly increasing rate of incarceration in the United States since the 1970s. The penal population increased six fold between 1972 and 2000 in the United States (Pettit and Western, 2004), making the United States an outlier with respect to incarceration rates among OECD nations (Western and Beckett, 1999), as well as compared to the rest of the world (the American incarceration rate is above the Russian or South-African one, Mauer, 2006). These incarceration rates, and their recent evolutions, can be explained in an important part by the "War on Drugs." Severity towards drug crimes began to rise in the 1970s and accounted for 52% of the total increase in new sentences to state prisons between 1985 and 2000 (with a 402% increase of prison sentences for drug crimes in this period, Mauer, 2006). This policy took place in the general context of the "Tough on Crime" policies of the 1970s.

The first unavoidable fact about incarceration and conviction rates, and in turn about felon disenfranchisement, is that most offenders are men. The overwhelming majority of sentenced prisoners was, for example, systematically male over the period 1994-2002, with women accounting for 5.9% to 6.6% of the total prison population (Bureau of Justice Statistics, 2003). This is true across years and across types of supervision (jail, probation, parole).

The second fact is that African Americans have been disproportionately represented among people having contact with the American penal system. For example, in 2000, 45% of prisoners were black (with a relatively stable rate around 55% for the Northeast, Midwest and South regions and noticeably lower at 23% in the West).

This, in turn, has very important repercussions for the black community as a whole. In 2002, 12% of black men in their twenties were in prison or jail (Harrison and Karberg, 2002). Similarly, the lifetime likelihood of imprisonment for a black male at birth was 32%, more than 5 times the same likelihood for white male children (6%) and approximately double the one of hispanic boys (Bonczar, 2003). The cumulative risk of being imprisoned by ages 30-34 increased between the cohort born in 1945-1949 and the cohort born 1965-1969, with the gap between black and white people widening for high-school dropouts (Pettit and Western, 2004). Similarly, the black to white ratio of state prison inmates has increased from 0.94 in 1985 to 1.04 in 1995, with the increase being driven mostly by the disproportionate impact of the "War on Drugs" on African Americans (the number of white inmates incarcerated for drug offenses has increased threefold between 1985 and 1995, when the same number has increased seven times over the same period for black inmates (Mumola and Beck, 1997). Indeed, the general trend described earlier has been accompanied by race-specific trends. The proportion of African Americans arrested for drug possession nearly doubled between 1980 and 1990 (from 20% to 40%) before stabilizing (Mauer, 2006). For these reasons, this chapter will focus on turnout and registration rates for men in disenfranchised states and especially for black men.

2.1.3 Disenfranchisement and Race in the United States

Some argue that felon disenfranchisement laws have been implicitly targeting African Americans and have therefore been a part of a larger arsenal aimed at disenfranchising African Americans (along with literacy tests or poll taxes, Cascio and Washington, 2014; Naidu, 2012). For example, Keyssar (2009) writes that "in the South, these measures often were more detailed and included lesser offenses, targeting minor violations of the law that could be invoked to disenfranchise African Americans."

However, in their systematic analysis of the relationship between race and the in-

corporation of disenfranchisement laws into state constitutions, Behrens et al. (2003) find that the only significant predictor among race-related indices is the proportion of non-whites among the prison population. They then argue that race was a determinant of disenfranchisement laws only when African Americans were perceived as a threat. Furthermore, Behrens et al. (2003) have analyzed trends in the abolition of felon disenfranchisement in most states in recent decades. They find a very small effect of the same indicator (non-white proportion of the prison population) and a contradictory effect of the proportion of non-whites in the population. From this analysis, it seems that making strong claims about an intentional relationship between felon disenfranchisement and race would be unfounded.

Furthermore, over time, laws have evolved towards more uniformity in their distinctions of which crimes should lead to disenfranchisement. The Supreme Court has repeatedly validated the states' discretion and legitimacy in disenfranchising felons and even ex-felons (in Richardson v. Ramirez in 1974 for example). At the same time, it has excluded some minor moral turpitude offenses from disenfranchisement laws in Alabama for example (Hunter v. Underwood in 1985) precisely to remedy the phenomenon described by Keyssar (2009). And, so far, no appeal against disenfranchisement laws on the basis of the Fourteenth and Fifteenth Amendments or the Voting Rights Act of 1965 has been successful, as no one has been able to show a systematic racial bias in these laws (as described in Manza and Uggen, 2006).

Furthermore, I will show that the demographic groups I believe are more likely to suffer from felon disenfranchisement laws, are not discriminated against on other dimensions.

2.2 Empirical Methodology

This chapter essentially tries to assess the impact of disenfranchisement laws on turnout, and in particular turnout of certain demographic groups. And since these laws are implemented at the state level, it seems natural to look at state-level turnout (as in Miles (2004) or, in a different setting, Washington (2006)). However, whether to vote or not is first and foremost an individual decision. Therefore, the empirical strategy will rely on an individual decision model, which I believe will give a more accurate estimate of the impact of disenfranchisement laws on turnout. In this section, I will detail the empirical strategy I will adopt and expose the advantages I see to using an individual-level discrete choice model rather than an aggregate outcome model.

2.2.1 Identification Strategy

This chapter will exploit the differences in probability of registering and voting for different demographic groups in different states, depending on their disenfranchisement regimes. The states I will qualify as "disenfranchising" are the ones where ex-felons are disenfranchised for life after their first offense. In these states, there are ways for felons to regain their voting rights. However, these possibilities are marginal and concern less than 5% of the disenfranchised populations, as estimated in Manza and Uggen (2006). Comparing this group to a group of states where ex-felons can be disenfranchised for life after their second conviction, where felons used to be disenfranchised for life until recently, where ex-felons can be disenfranchised for a limited period of time after their punishment and where, generally speaking parolees and probationers can be disenfranchised as well, constitutes the most conservative position and will only provide a lower bound for the actual effect of disenfranchisement after punishment and disenfranchisement of felons in general. However, systematic

disenfranchisement of felons for life is the policy most under attack and most controversial, which explains my current focus on this aspect. Also, as stated before, it is very possible that ex-felons are so far from the political system that they would not turn out to vote even if offered with the possibility. Therefore, showing that felon disenfranchisement exerts a binding constraints on registration and turnout rates of some communities is essential. In a subsequent analysis, I will use different "control" groups based on the severity of the felon disenfranchisement regime.

The previous section shows that there are very important demographic imbalances in the prison population, one of them being the proportion of black people in the penal system. As discussed in the previous section, the relationship between race and current regimes of disenfranchisement is unclear at best, and probably non-existent. Therefore, a first identification strategy in this chapter will be to use a triple difference (a difference-in-difference-in-difference) with African American men in disenfranchising states as the group of interest. I start with this strategy for the sake of comparability (this strategy is similar to Miles (2004) even though differences will be highlighted in the next section) and because results will legitimate the second, more robust strategy. To be more precise, the identification will rely on dummies for sex, race and living in a disenfranchising state, and all their possible interactions. In this way, I will be able to measure the effect of disenfranchisement for life on African American men (the demographic group most subject to these laws), net of other phenomena that affect the turnout of this group and others which share some characteristics with it, but are not likely to be affected by felon disenfranchisement. This will help make the case that although African American men are not very invested in the political process (Ansolabehere and Hersh, 2012), felon disenfranchisement laws still have a noticeable (and statistically significant) impact on the turnout of that group.

However, the most obvious demographic imbalance in the penal population is its

overwhelming gender bias. A source of variation that the chapter will examine is the time variation in the probability of registering and voting for all men in disenfranchising and non-disenfranchising states. If one state disenfranchises for life then every year the number of released prisoners (or felons who end their sentences) will have to be added to the number of felons entering the penal system and previously disenfranchised felons. On the opposite, in a non-disenfranchising state, the number of felons going out of the penal system will have to be subtracted to the number of already disenfranchised felons and the number of "new" felons. In other words, in disenfranchising states, felon disenfranchisement has a cumulative effect. Therefore, time will have a differential effect on the disenfranchised population in disenfranchising and non-disenfranchising states.[3] Therefore, I will adopt a triple difference framework where the three characteristics will be sex, time and living in a disenfranchised state and, by doing this, exploit the differential rate of growth of the male disenfranchised population. This strategy will rely on evidence that the effect for black men is increasing with time (and therefore that time has indeed a differential effect in disenfranchising states). It will be useful to use a time difference, in order to account for the fact that some states have changed their laws not very long before

[3]A formal examination of this phenomenon is the following. Let D_t be the number of felons at date t, N_t the number of "new" felons between t and $t+1$ and E_t the number of felons that exit the penal system between t and $t+1$. I then have that in a non-disenfranchising state:

$$D_{t+1}^{non-dis} = D_t^{non-dis} + N_t - E_t$$

Whereas in a disenfranchising state, I have:

$$D_{t+1}^{dis} = D_t^{dis} + N_t$$

Of course, recidivists (which are added to the disenfranchised population in non-disenfranchising states but not in disenfranchising states) and deaths make the difference between the two regimes more complicated (as detailed in Uggen and Manza (2002)) but overall, I believe that there is a differential rate of growth of the disenfranchised population between disenfranchising and non-disenfranchising states. Therefore, to make the difference explicit for my specification:

$$\left(D_{2010}^{dis} - D_{1994}^{dis}\right) - \left(D_{2010}^{non-dis} - D_{1994}^{non-dis}\right) = \sum_{t=1994}^{2009} E_t$$

In the triple-difference framework, women will provide extra-leverage to account for other trends taking place in disenfranchising and non-disenfranchising states.

1994 (the first year of available data for this chapter). For example, Tennessee and Washington had their constitutions modified in 1986 and 1984 respectively, making the difference between these states and disenfranchising states in 1994 potentially very small. It is therefore very useful to draw a comparison to the 1994 baseline. To provide an alternative explanation for the results one would have to argue that disenfranchising states have other non-observable features that discourage men from registering and voting. In any case, robustness checks will be shown.

In addition, this chapter will focus on observed outcomes rather than simulated counterfactual outcomes, as in Uggen and Manza (2002). The simulation approach seems legitimate and well-conducted (with a few caveats that Uggen and Manza (2002) acknowledge), but it seems that a focus on actual outcomes could prove useful especially with regards to the new data, new methodology and new results that it carries.

2.2.2 Turnout and Individual Voting Decision

This section will highlight the differences between the individual-level approach and the aggregate approach found in Miles (2004) or Washington (2006). In Miles (2004), a similar identification strategy (i.e. a triple difference for turnout) is used, but the identification relies on computation of aggregate turnout by state, race and gender (from the CPS) and then on a comparison of turnout for African American men in disenfranchising states and non-disenfranchising states. Since one can only estimate turnout by race and gender using survey data (the November CPS being the largest one), Miles (2004) is highly dependent on the number of observations in each state-race-gender cells. In some states the African American population is so small that outcomes for this population cannot be inferred from the CPS (in some states African Americans are almost not sampled). This leads to the exclusion of some states including half of the disenfranchising states. This also highlights that turnout figures

for different demographic groups as measured in the CPS are subject to important measurement errors, unevenly distributed among states.

Furthermore, by focusing on aggregate (at the state level) outcomes, Miles (2004) is subject to the ecological (or aggregation) fallacy. This has been shown to be a problem in examining voting outcomes by Gelman et al. (2009). Gelman et al. (2009) analyze the popular belief that since rich states are increasingly voting for Democrats and poor states for Republicans it must be the case that self-interested economic considerations (which should motivate the rich to vote for Republicans and vice versa for the poor) must have declined in the way people make their voting decisions, to the benefit of social or religious considerations. However, this analysis makes a false inference based on aggregate results. Using an individual-choice model, Gelman et al. (2009) show that economic factors remain consistently a good predictor of ideological positions at the individual level (a result confirmed by McCarty et al., 2006), but that increasing levels of within-state inequality has led to contradictions between findings at the individual and aggregate levels. In other terms, the "richness" of a state as assessed by the mean wealth is a bad aggregate indicator of the repartition of wealth in a state, which in turn leads to false inferences. In this case, the possibility to integrate individual controls (for example age, education, family income) will allow for a better understanding than their equivalent aggregates (Miles (2004) includes demographic controls as the fraction of people aged 18 to 35 and the fraction of people aged 35 to 65). The comparison of aggregate outcomes also hides differences in population size between states. By comparing aggregate results, Miles (2004) essentially averages (without weights) results in states that differ widely in their sizes.

Therefore, this chapter will use an individual discrete choice model of voting, similar to other works analyzing turnout (Wolfinger and Rosenstone (1980) for example).

2.2.3 Triple Difference Frameworks

The identification strategy of this chapter relies on the inclusion of a triple difference (i.e. a set of interacted dummy variables) into a discrete choice model. In this model, the idea is that each individual has a probability of voting that is determined through an index that aggregates individual characteristics. Therefore, the probability of registering or voting depends on individual and not population characteristics. In doing so, this article uses a methodology that has been used to analyze turnout decisions (Wolfinger and Rosenstone, 1980) as well as party identification (Gelman et al., 2009, McCarty et al., 2006). In other settings, a similar method has been used by Eissa and Liebman (1996) and more recently by Currie and Walker (2011).

Before moving to the results, it seems important to analyze exactly what the results will tell about the importance of felon disenfranchisement. The model I will first run is the following:

$$\begin{aligned} Pr(y_{is}=1) = \beta_1(Bl_i \quad &\times \quad M_i \times Dis_s) + \beta_2(M_i \times Dis_s) + \beta_3(Bl_i \times Dis_s) \\ &+ \quad \beta_4(M_i \times Bl_i) + \beta_5 Dis_s + \beta_6 M_i + \beta_7 Bl_i + \gamma Z_{is} + \epsilon_{is} \end{aligned}$$

where y_{ist} is the outcome of individual i living in state s (vote, registration), Bl_i is a dummy indicating the race of the individual ($Bl_i = 1$ if an individual is black), M_i is a dummy for the gender of the individual ($M_i = 1$ if an individual is male) and Dis_s indicates whether a state disenfranchises felons for life ($Dis_s = 1$ for disenfranchising states). I will pool elections together (in which case region-year[4] fixed effects will be included) as well as explore each election-year in particular (in which case, region fixed effects will be included). The model will include a set of individual controls Z (age, age squared, dummy for unemployment status, dummy for retirement status, family income and education level).

[4]The regions are the U.S. Census regions.

The coefficient β_1 is the coefficient of interest, as it denotes the average effect of disenfranchising laws on the target population. For a simpler interpretation of the results, I will use a linear probability function, which will make β_1 represent the actual effect of disenfranchisement laws on turnout or registration rate (in percentage points). All the results are consistent with a probit model and simulated outcomes. It therefore seems that for this study, the probabilities are mainly in the linear part of the probit function.

A way to interpret the difference-in-difference-in-difference framework is to think that each coefficient is constructed by comparing the turnout of 8 different groups:

- White Women in Non-disenfranchising states: T^{ND}_{WhW}.
- White Women in Disenfranchising states: T^{D}_{WhW}.
- White Men in Non-disenfranchising states: T^{ND}_{WhM}.
- Black Women in Non-disenfranchising states: T^{ND}_{AfrW}.
- White Men in Disenfranchising states: T^{D}_{WhM}.
- Black Women in Disenfranchising states: T^{D}_{AfrW}.
- Black Men in Non-disenfranchising states: T^{ND}_{AfrM}.
- Black Men in Disenfranchising states: T^{D}_{AfrM}.

Without controls, the coefficient β_1 can be written as:

$$\beta_1 = [(T^{D}_{AfrM} - T^{D}_{AfrW}) - (T^{D}_{WhM} - T^{D}_{WhW})] - [(T^{ND}_{AfrM} - T^{ND}_{AfrW}) - (T^{ND}_{WhM} - T^{ND}_{WhW})]$$

In other terms, there is a widely documented difference in turnout between men and women (Wolfinger and Rosenstone, 1980, Ansolabehere and Hersh, 2012), this

"gender turnout gap" is likely to be different for black and white people. The triple-difference then measures whether the "gender turnout gap" of black people relative to white people is greater in disenfranchising states than in non-disenfranchising states.[5] The other coefficients can also be expressed in terms of the differences in turnout between the eight aforementioned groups.

I will run different regressions for each election-year and show that the effects of felon disenfranchisement are increasing, consistent with the idea that the felon population is growing faster in disenfranchising states than in other states. This idea will be central to the second, more robust estimation strategy:

$$\begin{aligned} Pr(y_{ist}=1) &= \beta_1(1_{Year=2010} \times M_{it} \times Dis_s) + \beta_2(M_{it} \times Dis_s) + \beta_3(1_{Year=2010} \times Dis_s) \\ &+ \beta_4(M_{it} \times 1_{Year=2010}) + \beta_5 Dis_s + \beta_6 M_{it} + \beta_7 1_{Year=2010} + \gamma Z_{ist} + \epsilon_{ist} \end{aligned}$$

The baseline year will be the first year of consistent data, 1994; 2010 is the last year of available data. The model will include the same individual controls as before (age, age squared, dummy for unemployment and retirement status, education and and family income) plus a dummy for race and for being an African American male. A set of fixed effects will also be included (first at the region level and then at the state level, as discussed below). In this specification, I will not compare the "gender turnout gap" between racial groups, but ask whether this gap has widened

[5]This is the specification used in Miles (2004). Because I believe that felon disenfranchisement will also have an effect on the "gender turnout gap" of white people, this specification should be biased downwards in the measurement of the actual effect of felon disenfranchisement. However, I am first and foremost interested in showing the effect of felon disenfranchisement on turnout rather than measuring it. Also, a difference-in-difference estimation between men and women in disenfranchising states and non-disenfranchising states shows no significant results for white people, and extremely close results to the ones found in the triple-difference framework for black people (results are not shown here). Therefore, I see the results of the triple-difference essentially as a "diff-in-diff" on a sample of interest (black people). One of the potential reasons why the effect of felon disenfranchisement is not detectable for white people is that the proportion of ex-felons in the white population will be smaller than in the black population and the "control" group (non-disenfranchising states) is actually very similar to the "treatment" group, given that most states had felon disenfranchisement restrictions until fairly recently.

over time in disenfranchising states compared to non-disenfranchising states. This is what would be expected from the predictions of a simple model of growth of the disenfranchised population.

2.3 Data and Results

2.3.1 Data

All the data in this chapter come from the November supplement to the CPS. I then drop observations for individuals below the age of 18 (not eligible to register or vote). A noticeable change from Miles (2004) is that this chapter makes use of the information on registration status and on citizen status (present in the CPS only from 1994). Therefore, the analysis will focus on two outcomes: registration and voting. And it will exclude from the analysis all non-citizens who, by definition, cannot vote, but whose presence in regressions may lead to inconsistent estimates. Depending on the specific demographic composition of non-citizens in each state, this could potentially have an important effect. I therefore limit the sample to the period 1994-2010. That the results in Miles (2004) span from 1986 to 2000 is yet another reason that they are not perfectly comparable.

Demographic controls include age, age squared (to allow for non-linearity, Ansolabehere and Hersh, 2012), indicators for family income (using the fourteen categories of the CPS), indicators for highest grade achieved (using the eleven categories of the CPS), an indicator of whether the individual is unemployed (since unemployment could motivate to vote, give more time to vote, or alternatively may serve as a marker for social exclusion) and whether the individual is retired (although this will be highly correlated with age). For all results, the standard errors have been clustered at the state level, as suggested by Bertrand et al. (2004). As in Miles (2004),

I will include region-year fixed effects, which insures that I am not just comparing Southern states to the rest of the United States. Miles (2004) emphasizes that the effect of living in a disenfranchising state for the entire population would be a linear combination (β_5 in the specifications of the previous section) of state fixed effects if I were to include them. However, it seems that being able to include state-fixed effects is more important than having access to the baseline coefficient of living in a disenfranchising state. I will therefore show both specifications.

As I noted earlier, I will need that unregistered felons are sampled. Given the small sample of African Americans in some regions, the probability that an ex-felon is sampled is low. Furthermore, if as suggested by Goffman (2009), African Americans living in ghettos and having experience with the American penal system tend to avoid contact with formal institutions and cultivate unpredictable behaviors, the probability that they will be sampled is even lower. Therefore, there is a strong attenuation bias, and it is likely that the estimates will be small, and possibly not significant when I analyze the behaviors of African Americans in particular. Additionally, felon disenfranchisement could also affect the turnout of groups beyond the people who are actually disenfranchised. There could be a contagion effect by which certain groups (most likely African Americans) could not turnout as they feel discriminated against as a group or because of the importance of peer effects on turnouts shown in Gerber et al. (2008). I believe that the comparison between male and female turnout will enable to isolate the actual effects of felon disenfranchisement from the contagion or group effects. This is yet another reason why I think of this chapter as providing evidence of the effects of felon disenfranchisement rather than a global measure of those effects.

Before I move on to the results, some summary statistics (shown in Table 2.1) will help understand and interpret the results. Disenfranchising states and other states are not similar in every respect. The former have a slightly higher proportion of

African Americans and generally speaking a less-educated population. At the same time, they have lower unemployment rates (even among African Americans only). The average age of the adult population is slightly higher, although it is probably driven by Florida being a disenfranchising states, since the difference disappears once one focuses on prime age population (18 to 65). In terms of registration and turnout rates, they have slightly lower numbers (especially among African Americans). Those numbers will provide interesting baseline numbers to interpret the magnitude of the effects of disenfranchising laws.

2.3.2 Results for the Triple Difference Using African American Men

General Results

Tables 2.2 and 2.3[6] show differences in turnout and registration for the seven groups aforementioned relative to the turnout of the baseline group, white women in non-disenfranchising states. I divide the results into two periods, 1994-2010 and 2004-2010, because I believe that the effects of felon disenfranchisement might only be detectable for the more recent period. We can see that, without controls, registration and turnout rates for African American men in disenfranchising states are significantly lower than the same rates for white women in non-disenfranchising states. However, when the full set of controls is included, the results are quite different and I find that African American men in disenfranchising states are actually more likely (even if insignificantly so) to register and vote than white women in non-disenfranchising states. This suggests that felon disenfranchisement has no effect on the registration and turnout rates of black men, contrary to my hypothesis. Nevertheless, I believe that the triple-difference framework will provide some insight on this phenomenon

[6]In all tables, * denotes significance at the 10% level, ** at the 5% and *** at the 1%.

and will enable to isolate the effects of felon disenfranchisement from other phenomena.

Table 2.4 shows the results for the probability of voting in the triple difference framework. It shows that race, sex and age are important determinants of the probability of voting, but that living in a disenfranchising state has no influence. However, when I focus on the most recent years (i.e. the period from 2004 to 2010), we see in Table 2.4 that the coefficient on the triple interaction term is negative and significant. Therefore, if I focus only on those results, this chapter is somewhat in line with findings in Miles (2004) (non-significant effect of disenfranchising laws on black men), even though the method is different (aggregate vs. individual) and the data are different (more recent, excluding non-citizens). Still, it is interesting that I obtain a significant effect for recent years.

When I focus on results on registration rates, the picture is somewhat different. Table 2.5 shows significant results of being an African American male in a disenfranchising state on the probability of registering, with and without controls. Similarly to what was happening for turnout, results for the more recent period are greater. It is not surprising that registration results are stronger, since felon disenfranchisement places above all a constraint on registering. Given that even when registered, people will have many reasons for not voting, the effect of disenfranchisement laws on turnout will be harder to detect.

The triple-difference framework also provides some insights on the previous results on the turnout of different groups. It seems that a highly significant determinant of registration and turnout rates is race. More precisely, being black significantly (and substantially) lowers the probability of registering and voting, when no controls are included (in line with findings on the differential turnout of black and white people) but significantly and substantially raises the probability of registering and voting when controls are included. This seems to explain a substantial part of the reversal of rela-

tive registration and turnout rates between black men in disenfranchising states and white women in non-disenfranchising states noticed earlier.[7] This coefficient is measured as the difference in turnout between black women in non-disenfranchising states and white women in non-disenfranchising states[8] and Ansolabehere and Hersh (2012) have shown that even though the overall turnout rate of African American women is lower than the turnout of white women, this result is actually the reverse when simply controlling for age (the results can be reconciled when taking into account the differences in the age composition of the two groups, black women being significantly younger than white women). Therefore, it only seems natural that when controlling for more than just age (in this case, family income, education level, employment status), one would find even higher discrepancies in this coefficient for regressions with and without controls. This seems to validate the approach I have taken, since the triple-difference has allowed to break down the differences in turnout between the groups and explain results seemingly in contradiction with the idea that felon disenfranchisement exerts a binding constraints on registration and turnout rates of certain groups. It is noticeable that the coefficient of interest (β_1) is actually fairly stable between the different specifications and always negative (although with different levels of significance).

Not included in the tables, the coefficients for family income and education are highly significant, and increasing in income and education. The region-year fixed effects are also significant.

The reason why Miles (2004) did not include state fixed effects is that the coef-

[7]Indeed, one can write:

$$T^D_{AfrM} - T^{ND}_{WhW} = \beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5 + \beta_6 + \beta_7$$

Therefore, the fact that β_7 goes from being negative to being largely positive when controls are added can explain a significant part of the surprising result when expressing the turnout of the seven groups relative to the baseline group.

[8]As note earlier, every coefficient in the triple-difference is a function of differences in turnout rates between the eight groups.

ficient for living in a disenfranchising state (β_5 in the specification of the previous section) would be a linear combination of those coefficients, therefore it would not be possible to measure the effect of living in a disenfranchising state and at the same time to include state fixed effects. However, it seems that integrating state-year fixed effects is somewhat complementary to and as interesting as measuring the effect of living in a disenfranchising state, even though it goes somewhat against the spirit of a triple difference. Therefore, I compute again the regressions for turnout and registration with demographic controls including state-year fixed effects and not only region-year fixed effects. These fixed effects will account for specific state context (such as the race or gender of the candidates for example, something that could impact the turnout, as shown by Washington, 2006) and for differences in registration laws (as controlled for by Miles, 2004). Results in Table 2.7 show that the inclusion of state-year fixed effects has a negligible effect on the results. Therefore, the results do not seem to be driven by state specific circumstances other than felon disenfranchisement.

As pointed before, the group of "non-disenfranchising states" is heterogenous with respect to the severity of the felon disenfranchisement regime. In order to show that the severity of the felon disenfranchisement regime matters in the estimation I run the original specification by using different sets of states as the "control" group depending on the felon disenfranchisement regime in those states (I use the categorization presented above and derived from Manza and Uggen (2006) and Porter (2010)). The effects, shown in Table 2.8 (I display the β_1 coefficient for different regressions run with different "control groups"; demographic controls and region-year fixed effects are included) are strongest when I compare the disenfranchising states to the states that apply no restriction to the voting rights of felons. However, since there are only two states in this category (Maine and Vermont), it is true that the results could be due do other factors influencing the turnout of African American men in those

states (especially since the African American community is so small in those states by comparison with other states). It is true that in general the effects of felon disenfranchisement as measured by the coefficient in the triple-difference are increasing when the severity of the felon disenfranchisement regime decreases. This also highlights the importance of the heterogeneity of the felon disenfranchisement regime in the "non-disenfranchising" states. Therefore, if one wanted to measure the effects of felon disenfranchisement, one would have to first specify what the "baseline" regime should be and what disenfranchisement for life should be compared to.

Results for Different Groups

Another feature of the felon population is their low level of education. As Pettit and Western (2004) write: "High school dropouts are about four times more likely to go to prison than high school graduates." This could make the impact of felon disenfranchisement more important for lower levels of education. However, given that people with low levels of education are also more likely not to vote, the effects for the different groups could be the same or it could be more important for higher educated groups. I therefore go on to break the results by education groups, using common distinctions in the sociological literature on prison population between high-school dropouts, people with a high-school diploma, college dropouts and people whose education goes beyond college. I run separate regressions for each group (with demographic controls other than education and region-year fixed effect) and display the triple-difference coefficient for each regression on Figure 2.4. The strongest effects are found for high-school dropouts, which is not surprising given the generally low levels of education of the felon population and for college dropouts. The effects are consistent for turnout and registration making very plausible that they are driven by institutional features preventing felons from registering and furthermore from voting.

Trends

We have seen that the results are evolving over time, with the effect of disenfranchisement laws on turnout of African American men being significant only in recent years. This supports the previous observation that the effect of time will be to increase the gap between non-disenfranchising states and disenfranchising ones (especially, for example, because Tennessee and Washington are considered non-disenfranchising even though ex-felons were disenfranchised for life prior to 1986 and 1984 respectively). Therefore, it is natural to focus on the evolution of those effects over time. I run separate regressions for each year (with demographic controls and region fixed effects) and display the evolution of the triple difference coefficient over time on Figure 2.2. The results are very much in line with a growing influence of disenfranchisement laws over time, and therefore fully justify the approach taken in the second part of this chapter.

Placebo Estimations

Even though I control for unemployment status, I have sought to show that African American men do not suffer from discrimination in general by looking at the probability of being unemployed in the same setting as previously. Of course, I have removed indicators for retirement status (these are mutually exclusive in the CPS) and family income since a link between family income and unemployment would most certainly go from unemployment status to income. The results show that unemployment has been no difference in the relative "unemployment gender gap" of African American men in disenfranchising states, even when I focus only on the more recent period, which is a hint that disenfranchisement laws are not linked with general discrimination against African American men today. This also shows that potential discriminations against ex-felons (for example, restrictions to the type of jobs they have access to) are not correlated with restrictions to their voting rights. Regressions

results for age, and ordered probit models[9] for education and family income[10] also show no specific treatment for African American men in disenfranchising states.

2.3.3 Results for the Triple Difference Using Men over Time

General Results

I have shown in the previous section that the effect of felon disenfranchisement were increasing for African American men over time. This result supports the argument I made earlier that the growth of the disenfranchised population is different in disenfranchising states and non-disenfranchising states. Therefore, the second identification strategy of this chapter seems justified, in absence of any reliable statistics on the number of disenfranchised felons. To clarify the identification strategy in this setting, I write once more the estimation equation:

$$\begin{aligned} Pr(y_{ist}=1) &= \beta_1(1_{Year=2010} \times M_{it} \times Dis_s) + \beta_2(M_{it} \times Dis_s) + \beta_3(1_{Year=2010} \times Dis_s) \\ &+ \beta_4(M_{it} \times 1_{Year=2010}) + \beta_5 Dis_s + \beta_6 M_{it} + \beta_7 1_{Year=2010} + \gamma Z_{ist} + \epsilon_{ist} \end{aligned}$$

The comparison year will be 1994 as it is the earliest year I can use with consistent data.

I start by showing the raw differences in turnout between the seven demographic groups and white women in non-disenfranchising states in 1994, who represent the baseline group. Contrary to the previous turnout numbers, those results (shown in Table 2.11) are fairly stable with and without demographic controls. This certainly comes from the fact that age and other controls significantly altered the coefficient on the dummy for being black, a concern that is less important here (it plays a role as

[9]Indeed, I use the discrete categories provided by the CPS for education and family income therefore relying on a ordered probit rather than a regression.

[10]Note, however, that since I only have access to family income and I separate between men and women, the identification relies on single-headed families.

demographic controls include race). Consistently through the different specifications, the group I think is most likely to suffer from felon disenfranchisement shows a substantial and highly significant difference with the gourd I think is least likely to be disenfranchised. Also, in most specification, this group shows the lowest registration or turnout rate between all the demographic groups, another indication that I could very well be displaying the effect of felon disenfranchisement.

The results from the regression framework in Table 2.12 show that being a male in a disenfranchising state in 2010 reduces significantly the probability of voting and registering relative to 1994, whether I control for demographic characteristics or not (results for demographic controls are similar to the ones described earlier). Interestingly, disenfranchising states (relative to non-disenfranchising states) show a slightly higher turnout for men in 1994, which could be due to the effect of Tennessee and Washington having switched their legislation only recently. In other words, the "gender turnout gap" has widened between 1994 and 2010 in disenfranchising states compared to non-disenfranchising ones.

The results are comparable for turnout and registration, once again suggesting that felon disenfranchisement laws are indeed responsible for these differences as they prevent people from registering, not registered people from voting. The effects are also very sizable and around 3 to 4 percentage points, i.e. a little less than 10% of turnout as reported in Table 2.1. However, those numbers only represent the differential growth of the disenfranchised population in different states, and are therefore a lower bound on the actual effects of felon disenfranchisement. It is therefore remarkable than 16 years would be enough to create such an important gap between the two types of states, suggesting that the potential effect of felon disenfranchisement are very large and were previously underestimated. Demographic controls are also important and significant, such as race and age, but do not affect the results.

When I run similar regressions for turnout focusing only on the registered pop-

ulation (Table 2.4), I find that the results have lower levels of significance and are generally smaller, even though there are non-zero. Therefore, this suggests that registration is indeed the important constraint but also that there are potentially small contagion effects.

Results for Different Groups

I now try to break the results by different groups. First, I divide the population by race and show that in line with expectations given the demographic imbalances of the felon population and previous results, the effects of disenfranchisement laws are greater for African Americans than for white people. This is true for turnout and registration, with, once again very comparable results. If I compare the measured effects with turnout and registration rates for 2010, I find that the effects represent around a fifth of the overall turnout for African Americans and between 5% and 7% of turnout for white men. Once again, these numbers certainly represent a lower bound on the actual effects of felon disenfranchisement laws since they only take into account a 16-year time period. They are very consistent with simulated estimations by Uggen and Manza (2002).

Similarly to what I did for African Americans, I also break the results by education level. Figure 2.4 shows that the results for turnout are decreasing with education, consistent with the composition of the penal population (however, given the width of the confidence intervals, the coefficients are probably not different from one another). The figure displays, once again, the triple difference coefficient obtained from separate regressions for each education group, with demographic controls (except education) and region fixed effects.

A state-by-state analysis

As I have noted earlier, since felon disenfranchisement regimes have been fairly stable in the period covered, it is impossible to include a dummy for living in a disenfranchising state and state fixed-effects. However, I have tried in the previous section to forgo the measurement of the effect of disenfranchising regimes on the overall population in favor of the inclusion of state fixed-effects. In this section, I go a little further and I run regressions with the following specification:

$$\begin{aligned} Pr(y_{ist}=1) = \quad & \boldsymbol{\beta_1}(1_{Year=2010} \times M_{it} \times StateFE) + \boldsymbol{\beta_2}(M_{it} \times StateFE) + \\ & \boldsymbol{\beta_3}(1_{Year=2010} \times StateFE) + \boldsymbol{\beta_4} StateFE + \gamma Z_{ist} + \epsilon_{ist} \end{aligned}$$

where y_{ist} is an indicator of voting or registering. Without the controls, this is equivalent to running a difference-in-difference within each state.[11] This was not previously possible since the sample of black men and women in some states is very small which had lead Miles (2004) for example to drop some states from his analysis. In this case, since I focus on men in general this is not an issue.

Figure 2.4 shows the diff-in-diff coefficients by state for the four specifications (turnout and registration, with and without demographic controls). These coefficients come from four different regressions. Standard errors have not been included for clarity's sake and because I only think of this figure as complementary evidence. States have been grouped by the severity of their regime against felons and within each group, by alphabetical order. The figure suggests that negative coefficients are indeed found more frequently in the second part of the graph (more severe regimes) and are especially concentrated at the end of the graph (felon disenfranchising states), therefore providing support for the claim that felon disenfranchisement is the driving

[11] The βs are now vectors since there is a coefficient per state (because of the interaction with the state fixed effects). I therefore obtain 50 β_1 coefficients that each represents the evolution of the "gender turnout gap" in each state between 1994 and 2010.

force behind the lower turnout of men between 1994 and 2010. In other terms, the "gender turnout gap" has been decreasing in some states between 1994 and 2010 and increasing in some others at the same time. However, what is interesting is that states with decreasing gender gaps have the more liberal felon disenfranchisement regimes, when states with increasing gender gaps are found to be the ones with the most severe disenfranchisement regimes. Of course, there are exceptions, but I believe that the identification of this chapter comes precisely from the aggregation of different states, since if I had had only one disenfranchising state, particular (potentially unobserved) contextual reasons could explain the phenomenon. The fact that I have more than one disenfranchising states helps abstract from particular state circumstances (such as race or gender of a candidate) that might explain particular results.

I now use this more flexible framework to compute the estimates of the aggregate effect of felon disenfranchisement. I run the following regressions:

$$\begin{aligned} Pr(y_{ist}=1) = \quad & \beta_1(1_{Year=2010} \times M_{it} \times Dis_s) + \boldsymbol{\beta_2}(M_{it} \times StateFE) + \\ & \boldsymbol{\beta_3}(1_{Year=2010} \times StateFE) + \boldsymbol{\beta_4} StateFE + \gamma Z_{ist} + \epsilon_{ist} \end{aligned}$$

which is equivalent to calculating a population-weighted average of the previous state-by-state coefficients for the two groups of states. The results, shown in Table 2.16, are very close to the results obtained in the original specifications. The results are therefore robust to the inclusion of a more flexible framework, taking into account state-by-state differences and fixed effects.

I go a little further and now group states not only along the disenfranchising vs. not disenfranchising line, but taking into account the variety of regimes that felons face. I replace the dummy for disenfranchising state by dummies for different regime types and keep state fixed effects. Therefore, I average the coefficients displayed on Figure 2.4 over different groups of states defined by the severity of their regimes

against felons and weighted by population. The results are displayed in . We see that the coefficient for the first two groups (no restriction and inmates only) are barely ever significant. This is not surprising given that only two states have no restriction. Furthermore, the group of states that prevent only inmates and parolees from voting has a positive coefficient. Therefore, in those states, the "gender turnout gap" has decreased. The coefficient then decreases sharply as the severity of the disenfranchising regime against felons increases. This provides additional evidence that felon disenfranchisement is very likely to explain those differences. It also points out that if disenfranchising states had been compared to states were inmates only or inmates and parolees are disenfranchised (which many consider should be the "right" regime), the coefficients would be greater than what I found (as can be seen in Table 2.18, for which I run different regressions for different "control groups" as I did in the previous section and display only the triple difference coefficient). On the opposite, if I had compared the ten disenfranchising states to the states where inmates, parolees and probationers are disenfranchised I would have found smaller coefficients. This was to be expected since the rate of growth of the disenfranchised population must be closer for this two groups of states given that probation considerably lengthens the time spent disenfranchised (which also creates adverse consequences in terms of habit of voting) and constitutes a high barrier to exit of the penal system.

Placebo Estimations

Similarly to what I did in the previous section, I test the explanation of the results I offered (namely felon disenfranchisement) against a general atmosphere of discrimination against men with a differential impact between 1994 and 2010 in disenfranchising states. I replicate my original specification with unemployment, even separating by race to offer a more comprehensive picture. Table 2.19 shows that there is no significant effect for men to live in a disenfranchising state between 1994 and 2010 on

the probability to be unemployed. Furthermore, I look at age and run ordered probit regressions for level of education and level of family income, only to find no significant effect. Therefore, it seems that my results and interpretation are confirmed by this placebo analysis.

2.4 Conclusion

This chapter uses a triple difference method in an individual choice model to measure the impact of disenfranchisement of felons for life. Given the demographic imbalances of the felon population, we would expect felon disenfranchisement to affect disproportionately African American men. This is what I find in a first set of results, at least for recent years. The results are robust to a number of robustness checks and are mainly driven by a limitation of the right to register, in line with the interpretation that felon disenfranchisement laws first prevent ex-felons from registering before voting. I show that the effects of these laws are increasing over time, which is consistent with a fairly simple model of differential growth of the felon population depending on the severity of the felon disenfranchisement regime. I, in turn, exploit this differential rate of growth to analyze turnout results for men in disenfranchising states between 1994 and 2010. I find that disenfranchisement laws have had a sizable effect on the turnout of men, and especially (but not only) African American men. I then proceed to show that the effect of the laws are greater for more severe regimes of felon disenfranchisement, showing that the effect could be as high as 6 percentage points if disenfranchising states are compared to states that disenfranchise inmates and parolees only. Placebo estimations show in both cases that other outcomes (namely unemployment status, age, family income and educational attainment) are not affected in a similar fashion, allowing to rule out confounding factors.

Of course, these results do not constitute a measure of the overall effect of felon

disenfranchisement, but a lower bound, considering only what happened between 1994 and 2010. However, they provide a crucial piece of evidence that felon disenfranchisement is not inconsequential, as some have claimed, given overall low levels of turnout in the concerned populations or based on previous work (Miles, 2004). The effects of these laws on representation, policy and redistribution outcomes will be the object of subsequent research. Overall, this chapter calls for renewed attention on the legal and moral bases for these laws.

Variable	All		Men		Black Men	
	Non-dis	Dis	Non-dis	Dis	Non-dis	Dis
African Americans (%)	12.1	15.2	11.2	14.2		
	(0.049)	(0.120)	(0.071)	(0.175)		
High-School	13.3	15.0	13.5	15.4	20.5	23.9
Dropouts (%)	(0.047)	(0.109)	(0.069)	(0.163)	(0.270)	(0.564)
High-School	32.9	33.7	32.6	33.4	37.8	38.6
or Equivalent (%)	(0.064)	(0.145)	(0.094)	(0.212)	(0.331)	(0.658)
Some College (%)	20.4	19.7	20.0	19.1	21.0	18.8
	(0.056)	(0.122)	(0.081)	(0.177)	(0.282)	(0.532)
College Diploma	33.4	31.7	33.9	32.1	20.7	18.6
and Above (%)	(0.065)	(0.143)	(0.095)	(0.210)	(0.277)	(0.528)
Unemployed (%)	3.53	3.23	4.03	3.60	7.77	6.49
	(0.026)	(0.056)	(0.040)	(0.087)	(0.188)	(0.336)
Age	45.7	46.8	44.9	46.1	41.8	41.9
	(0.024)	(0.0554)	(0.034)	(0.079)	(0.108)	(0.216)
Prime Age	39.9	40.3	39.8	40.3	38.1	38.0
	(0.020)	(0.045)	(0.028)	(0.065)	(0.092)	(0.182)
Turnout	53.3	52.6	52.2	51.6	46.4	43.6
	(0.069)	(0.153)	(0.100)	(0.225)	(0.340)	(0.666)
Registration rate	68.6	68.4	67.1	66.7	61.5	59.2
	(0.064)	(0.144)	(0.095)	(0.214)	(0.333)	(0.666)
Observations	682808	138896	321863	64941	26644	6841

Table 2.1: Summary Statistics (Adult Population), 1994-2010

Group	1994-2010		2004-2010	
African American Men	-0.1175***	0.0186	-0.1095***	0.0282
in Disenfranchising States	(0.020)	(0.027)	(0.019)	(0.025)
African American Men	-0.0894***	0.0142	-0.0871***	0.0154
in Non-disenfranchising States	(0.011)	(0.010)	(0.010)	(0.010)
African American Women	-0.0535***	0.0778***	-0.0249	0.1043***
in Disenfranchising States	(0.020)	(0.026)	(0.026)	(0.031)
White Men	-0.0204*	-0.0042	-0.0246*	-0.0026
in Disenfranchising States	(0.012)	(0.012)	(0.013)	(0.015)
African American Women	-0.0295**	0.0649***	-0.0246**	0.0670***
in Non-disenfranchising States	(0.011)	(0.011)	(0.012)	(0.012)
White Men	-0.0163***	-0.0154***	-0.0229***	-0.0182***
in Non-disenfranchising States	(0.003)	(0.003)	(0.002)	(0.003)
White Women	-0.0056	0.0097	-0.0040	0.0132
in Disenfranchising States	(0.013)	(0.011)	(0.014)	(0.014)
Age		0.0147***		0.0113***
		(0.001)		(0.001)
Age Squared		-0.0001***		-0.00001***
		(0.000)		(0.000)
Observations	781321	781321	354084	354084
Std error	cluster	cluster	cluster	cluster
Controls	N	Y	N	Y
Fixed Effects	N	Region-Year	N	Region-Year

Table 2.2: Estimation Results: Difference in Turnout Compared to Non-African American Women in Non-Disenfranchising States

Group	1994-2010		2004-2010	
African American Men	-0.1186***	-0.0273	-0.1207***	-0.0290
in Disenfranchising States	(0.027)	(0.032)	(0.026)	(0.030)
African American Men	-0.0956***	-0.0177**	-0.0967***	-0.0217**
in Non-disenfranchising States	(0.009)	(0.009)	(0.010)	(0.009)
African American Women	-0.0361	0.0501	-0.0202	0.0636**
in Disenfranchising States	(0.026)	(0.031)	(0.028)	(0.030)
White Men	-0.0279**	-0.0284*	-0.0285**	-0.0247
in Disenfranchising States	(0.012)	(0.016)	(0.013)	(0.016)
African American Women	-0.0321***	0.0371***	-0.0341***	0.0315***
in Non-disenfranchising States	(0.009)	(0.009)	(0.011)	(0.009)
White Men	-0.0255***	-0.0242***	-0.0287***	-0.0242***
in Non-disenfranchising States	(0.003)	(0.003)	(0.002)	(0.002)
White Women	-0.0006	-0.0029	-0.0011	-0.0023
in Disenfranchising States	(0.011)	(0.014)	(0.011)	(0.013)
Age		0.0107***		0.0078***
		(0.000)		(0.000)
Observations	781321	781321	354084	354084
Std error	cluster	cluster	cluster	cluster
Controls	N	Y	N	Y
Fixed Effects	N	Region-Year	N	Region-Year

Table 2.3: Estimation Results: Difference in Registration Compared to Non-African American Women in Non-Disenfranchising States

Variable	1994-2010		2004-2010	
African American Men	-0.0048	-0.0104	-0.0233	-0.0266**
in Disenfranchising States	(0.012)	(0.010)	(0.015)	(0.012)
Men in Disenfranchising	0.0004	0.0012	0.0012	0.0018
States	(0.005)	(0.005)	(0.004)	(0.004)
African Americans	-0.0210	0.0028	0.0009	0.0236
in Disenfranchising States	(0.021)	(0.023)	(0.026)	(0.027)
African American Men	-0.0438***	-0.0352***	-0.0397***	-0.0335***
	(0.005)	(0.005)	(0.007)	(0.006)
Disenfranchising States	-0.0040	0.0085	-0.0027	0.0112
	(0.013)	(0.011)	(0.014)	(0.013)
Men	-0.0161***	-0.0154***	-0.0227***	-0.0181***
	(0.003)	(0.003)	(0.002)	(0.003)
African Americans	-0.0290**	0.0650***	-0.0241**	0.0670***
	(0.011)	(0.011)	(0.012)	(0.012)
Age		0.0147***		0.0113***
		(0.001)		(0.001)
Observations	781321	781321	354084	354084
Std error	cluster	cluster	cluster	cluster
Controls	N	Y	N	Y
Fixed Effects	N	Region-Year	N	Region-Year

Table 2.4: Estimation Results: Probability of Voting

Variable	1994-2010		2004-2010	
African American Men	-0.0164**	-0.0212***	-0.0376***	-0.0401***
in Disenfranchising States	(0.007)	(0.007)	(0.012)	(0.011)
Men in Disenfranchising	-0.0028	-0.0018	0.0004	0.0013
States	(0.004)	(0.004)	(0.004)	(0.004)
African Americans	-0.0060	0.0152	0.0119	0.0332
in Disenfranchising States	(0.023)	(0.026)	(0.026)	(0.025)
African American Men	-0.0380***	-0.0306***	-0.0340***	-0.0291***
	(0.005)	(0.005)	(0.007)	(0.006)
Disenfranchising States	0.0015	-0.0029	0.0005	-0.0031
	(0.011)	(0.013)	(0.011)	(0.013)
Men	-0.0254***	-0.0241***	-0.0286***	-0.0242***
	(0.003)	(0.003)	(0.002)	(0.002)
African Americans	-0.0317***	0.0372***	-0.0336***	0.0316***
	(0.009)	(0.009)	(0.011)	(0.009)
Age		0.0107***		0.0078***
		(0.0004)		(0.0005)
Observations	781321	781321	354084	354084
Std error	cluster	cluster	cluster	cluster
Controls	N	Y	N	Y
Fixed Effects	N	Region-Year	N	Region-Year

Table 2.5: Estimation Results: Probability of Registering

Variable	1994-2010		2004-2010	
African American Men	0.0089	0.0042	0.0086	-0.0007
in Disenfranchising States	(0.015)	(0.015)	(0.012)	(0.011)
Men in Disenfranchising	0.0035	0.0033	0.0011	0.0013
States	(0.004)	(0.006)	(0.005)	(0.004)
African Americans	-0.0247	-0.0111	-0.0127	0.0011
in Disenfranchising States	(0.015)	(0.013)	(0.014)	(0.015)
African American Men	-0.0231***	-0.0231***	-0.0194***	-0.0189***
	(0.005)	(0.004)	(0.007)	(0.005)
Disenfranchising States	-0.0072	0.0161	-0.0043	0.0203*
	(0.015)	(0.012)	(0.013)	(0.011)
Men	0.0053***	0.0006	0.0003	-0.0020
	(0.002)	(0.002)	(0.002)	(0.002)
African Americans	-0.0064	0.0657***	0.0042	0.0739***
	(0.009)	(0.009)	(0.009)	(0.009)
Age		0.0134***		0.0102***
		(0.001)		(0.001)
Observations	550701	550701	251892	251892
Std error	cluster	cluster	cluster	cluster
Controls	N	Y	N	Y

Table 2.6: Estimation Results: Probability of Voting, Registered Adults Only

Variable	Turnout		Registration	
	1994-2010	**2004-2010**	**1994-2010**	**2004-2010**
African American Men	-0.0105	-0.0268**	-0.0210***	-0.0402***
in Disenfranchising States	(0.010)	(0.012)	(0.007)	(0.011)
Men in Disenfranchising	0.0016	0.0024	-0.0014	0.0017
States	(0.005)	(0.004)	(0.004)	(0.004)
African Americans	-0.0040	0.0156	0.0030	0.0236
in Disenfranchising States	(0.018)	(0.022)	(0.018)	(0.018)
African American Men	-0.0349***	-0.0334***	-0.0302***	-0.0291***
	(0.005)	(0.006)	(0.005)	(0.006)
Men	-0.0157***	-0.0183***	-0.0244***	-0.0243***
	(0.003)	(0.003)	(0.003)	(0.002)
African Americans	0.0679***	0.0688***	0.0405***	0.0335***
	(0.011)	(0.010)	(0.009)	(0.008)
Age	0.0146***	0.0111***	0.0106***	0.0077***
	(0.001)	(0.001)	(0.000)	(0.000)
Observations	781321	354084	781321	354084
Std error	cluster	cluster	cluster	cluster
Controls	Y	Y	Y	Y
Fixed Effects	State-Year	State-Year	State-Year	State-Year

Table 2.7: Estimation Results: Turnout and Registration, State-year Fixed Effects

Variable	Turnout		Registration	
	1994-2010	**2004-2010**	**1994-2010**	**2004-2010**
No Restriction	-0.2024**	-0.2995**	-0.2565***	-0.3388***
	(0.088)	(0.121)	(0.054)	(0.083)
Inmates Only	-0.0069	-0.0400**	-0.0220**	-0.0503***
	(0.012)	(0.018)	(0.010)	(0.013)
Inmates and Parolees	-0.0254*	-0.0322**	-0.0294***	-0.0375**
	(0.013)	(0.011)	(0.007)	(0.014)
Inmates, Parolees and Probationers	-0.0081	-0.0202	-0.0174*	-0.0350**
	(0.011)	(0.014)	(0.009)	(0.014)

Table 2.8: Original Specification, Different "Control" Groups

Variable	**1994-2010**		**2004-2010**	
African American Men	-0.0052	-0.0035	-0.0078	-0.0058
in Disenfranchising States	(0.004)	(0.004)	(0.005)	(0.005)
Men in Disenfranchising	-0.0021	-0.0021	-0.0013	-0.0013
States	(0.002)	(0.002)	(0.002)	(0.002)
African Americans	-0.0025	-0.0071*	-0.0027	-0.0072
in Disenfranchising States	(0.004)	(0.004)	(0.005)	(0.006)
African American Men	0.0080***	0.0094***	0.0094**	0.0106***
	(0.003)	(0.002)	(0.004)	(0.004)
Disenfranchising States	-0.0027*	0.0001	-0.0035	-0.0009
	(0.002)	(0.002)	(0.002)	(0.003)
Men	0.0093***	0.0096***	0.0128***	0.0126***
	(0.000)	(0.000)	(0.001)	(0.001)
African Americans	0.0340***	0.0194***	0.0359***	0.0196***
	(0.002)	(0.002)	(0.003)	(0.003)
Age		-0.0011***		-0.0008***
		(0.000)		(0.000)
Observations	781321	781321	354084	354084
Std error	cluster	cluster	cluster	cluster
Controls	N	Y	N	Y

Table 2.9: Placebo Estimation: Probability of Being Unemployed

Variable	**Age**	**Education (Ordered Probit)**	**Family Income (Ordered Probit)**
African American Men in Disenfranchising States	0.3396 (0.355)	-0.0102 (0.035)	0.0017 (0.020)
Men in Disenfranchising States	0.1637 (0.162)	0.0009 (0.026)	-0.0105 (0.007)
African Americans in Disenfranchising States	-1.9161* (1.042)	-0.0690* (0.042)	0.0069 (0.032)
African American Men	0.1031 (0.226)	-0.1284*** (0.016)	0.0821*** (0.011)
Disenfranchising States	1.3740* (0.812)	0.0333 (0.038)	-0.0286 (0.041)
Men	-1.7042*** (0.059)	-0.0431*** (0.010)	0.0768*** (0.004)
African Americans	-3.7254*** (0.231)	-0.0462* (0.028)	-0.4075*** (0.022)
Age		0.0356*** (0.001)	0.0334*** (0.001)
Observations	354084	354084	354084
Std error	cluster	cluster	cluster
Demographic Controls	N	Y	Y

Table 2.10: Placebo Estimations, 2004-2010

Variable	Turnout		Registration	
Men in Disenfranchising states	-0.0569***	-0.1069***	-0.0675***	-0.1248***
in 2010	(0.019)	(0.017)	(0.014)	(0.017)
Men in non-disenfranchising states	-0.0377**	-0.1102***	-0.0473***	-0.1076***
in 2010	(0.015)	(0.013)	(0.011)	(0.009)
Women in Disenfranchising states	-0.0328*	-0.0903***	-0.0200	-0.0870***
in 2010	(0.019)	(0.017)	(0.016)	(0.019)
Men in Disenfranchising states	-0.0105	0.0224*	-0.0374**	-0.0240
in 1994	(0.020)	(0.013)	(0.018)	(0.023)
Women in Non-disenfranchising states	-0.0258*	-0.1097***	-0.0198	-0.0925***
in 2010	(0.015)	(0.012)	(0.012)	(0.009)
Men in Non-disenfranchising states	-0.0067	-0.0075	-0.0245***	-0.0233***
in 1994	(0.004)	(0.006)	(0.004)	(0.004)
Women in Disenfranchising states	-0.0246	0.0082	-0.0233	-0.0123
in 1994	(0.026)	(0.017)	(0.017)	(0.019)
Age		0.0172***		0.0127***
		(0.001)		(0.001)
Age Squared		-0.0001***		-0.0001***
		(0.000)		(0.000)
Observations	189565	179994	189565	179994
Std error	cluster	cluster	cluster	cluster
Controls	N	Y	N	Y

Table 2.11: Comparison with Women in Non-disenfranchising States in 1994

Variable	Turnout		Registration	
Men in Disenfranchising States in 2010	-0.0315**	-0.0365***	-0.0297**	-0.0340***
	(0.013)	(0.011)	(0.013)	(0.011)
Men in Disenfranchising States	0.0198*	0.0211*	0.0094	0.0110
	(0.011)	(0.011)	(0.008)	(0.008)
Disenfranchising States in 2010	0.0118	-0.0088	0.0193	0.0076
	(0.028)	(0.029)	(0.016)	(0.015)
Men in 2010	-0.0052	0.0071	-0.0029	0.0085
	(0.005)	(0.006)	(0.005)	(0.006)
Disenfranchising States	-0.0205	0.0173	-0.0192	-0.0070
	(0.026)	(0.018)	(0.017)	(0.019)
Men	-0.0067	-0.0076	-0.0244***	-0.0234***
	(0.004)	(0.006)	(0.004)	(0.004)
African Americans		0.0498***		0.0385***
		(0.010)		(0.011)
African American Men		-0.0288***		-0.0349***
		(0.008)		(0.008)
Age		0.0172***		0.0127***
		(0.001)		(0.001)
Observations	189565	179994	189565	179994
Std error	cluster	cluster	cluster	cluster
Controls	N	Y	N	Y

Table 2.12: Estimation Results

Variable		
Men in Disenfranchising States in 2010	-0.0169* (0.010)	-0.0229** (0.010)
Men in Disenfranchising States	0.0202* (0.010)	0.0191* (0.011)
Disenfranchising States in 2010	-0.0030 (0.032)	-0.0183 (0.032)
Men in 2010	-0.0051 (0.006)	0.0017 (0.005)
Disenfranchising States	-0.0102 (0.035)	0.0359 (0.026)
Men	0.0162*** (0.005)	0.0097* (0.005)
African Americans		0.0530*** (0.009)
African American Men		-0.0157* (0.009)
Age		0.0179*** (0.001)
Observations	127547	122435
Std error	cluster	cluster
Controls	N	Y

Table 2.13: Estimation Results:Probability of Voting, Registered Adults Only

Variable	White Adults		African Americans	
Men in Disenfranchising	-0.0250***	-0.0278***	-0.0797**	-0.0800**
States in 2010	(0.009)	(0.008)	(0.039)	(0.038)
Men in Disenfranchising	0.0186	0.0219*	0.0238	0.0137
States	(0.012)	(0.011)	(0.023)	(0.021)
Disenfranchising States	0.0045	-0.0102	0.0405	0.0079
in 2010	(0.028)	(0.029)	(0.031)	(0.028)
Men in 2010	-0.0046	0.0104*	-0.0168	-0.0068
	(0.005)	(0.006)	(0.022)	(0.020)
Disenfranchising States	-0.0148	0.0170	-0.0399*	0.0100
	(0.029)	(0.019)	(0.022)	(0.019)
Men	-0.0054	-0.0105*	-0.0276*	-0.0184
	(0.005)	(0.006)	(0.014)	(0.013)
Age		0.0175***		0.0158***
		(0.001)		(0.001)
Observations	161037	161037	18957	18957
Std error	cluster	cluster	cluster	cluster
Demographic Controls	N	Y	N	Y
Average Male Turnout in 2010 (non-dis)	0.46		0.41	
Average Male Turnout in 2010 (dis)	0.44		0.36	

Table 2.14: Results on Turnout by Race

Variable	White Adults		African Americans	
Men in Disenfranchising States in 2010	-0.0218**	-0.0235**	-0.0941**	-0.0903**
	(0.010)	(0.009)	(0.037)	(0.037)
Men in Disenfranchising States	0.0096	0.0121	0.0164	0.0067
	(0.008)	(0.007)	(0.019)	(0.019)
Disenfranchising States in 2010	0.0147	0.0042	0.0445**	0.0280
	(0.017)	(0.017)	(0.022)	(0.019)
Men in 2010	-0.0044	0.0087	0.0079	0.0139
	(0.005)	(0.006)	(0.021)	(0.019)
Disenfranchising States	-0.0192	-0.0084	-0.0258	-0.0070
	(0.018)	(0.019)	(0.033)	(0.033)
Men	-0.0205***	-0.0250***	-0.0619***	-0.0490***
	(0.004)	(0.005)	(0.016)	(0.015)
Age		0.0125***		0.0135***
		(0.001)		(0.001)
Observations	161037	161037	18957	18957
Std error	cluster	cluster	cluster	cluster
Demographic Controls	N	Y	N	Y
Average Male Registration in 2010 (non-dis)	0.65		0.60	
Average Male Registration in 2010 (dis)	0.64		0.54	

Table 2.15: Results on Registration by Race

Variable	Turnout		Registration	
Men in Disenfranchising States in 2010	-0.0309**	-0.0359***	-0.0300**	-0.0347***
	(0.013)	(0.011)	(0.013)	(0.011)
African Americans		0.0508***		0.0389***
		(0.011)		(0.011)
African American Men		-0.0287***		-0.0344***
		(0.007)		(0.008)
Age		0.0171***		0.0126***
		(0.001)		(0.001)
Observations	189565	179994	189565	179994
Std error	cluster	cluster	cluster	cluster
Demographic Controls	N	Y	N	Y

Table 2.16: Results with State Fixed Effects

Variable	Turnout		Registration	
No Restriction	-0.0107	0.0038	-0.0028	0.0049
	(0.025)	(0.018)	(0.018)	(0.015)
Inmates Only	0.0069	0.0147**	-0.0042	0.0020
	(0.007)	(0.007)	(0.005)	(0.004)
Inmates and Parolees	0.0064	0.0306***	0.0186*	0.0404***
	(0.008)	(0.007)	(0.011)	(0.006)
Inmates, Parolees	-0.0140**	-0.0028	-0.0077	0.0029
and Probationers	(0.006)	(0.005)	(0.007)	(0.008)
All and Some Ex-felons	-0.0411*	-0.0444**	-0.0308***	-0.0258***
	(0.022)	(0.020)	(0.011)	(0.007)
All and All Ex-felons	-0.0362***	-0.0290***	-0.0326**	-0.0257**
	(0.011)	(0.009)	(0.012)	(0.010)
African Americans		0.0516***		0.0404***
		(0.011)		(0.011)
African American Men		-0.0310***		-0.0378***
		(0.008)		(0.008)
Age		0.0171***		0.0126***
		(0.001)		(0.001)
R-squared	0.015	0.170	0.012	0.124
Observations	187486	177990	187486	177990
Std error	cluster	cluster	cluster	cluster
Demographic Controls	N	Y	N	Y

Table 2.17: Results by Felon Disenfranchisement Regime

Variable	Turnout		Registration	
Inmates Only	-0.0434***	-0.0437***	-0.0281**	-0.0270**
	(0.013)	(0.011)	(0.013)	(0.010)
Inmates and Parolees	-0.0430***	-0.0592***	-0.0505**	-0.0651***
	(0.014)	(0.011)	(0.017)	(0.011)
Inmates, Parolees	-0.0219	-0.0437***	-0.0235	-0.0272**
and Probationers	(0.013)	(0.011)	(0.014)	(0.012)
Std error	cluster	cluster	cluster	cluster
Demographic Controls	N	Y	N	Y

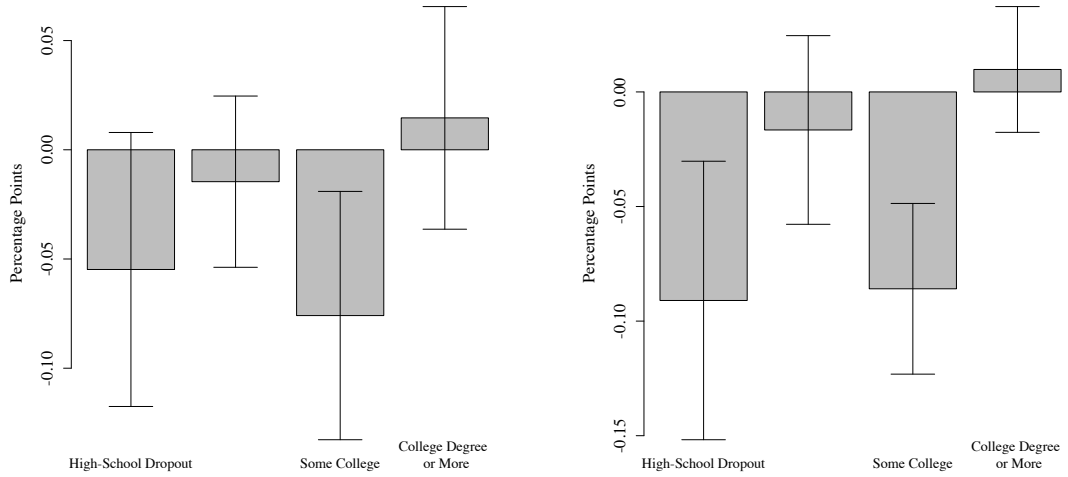
Table 2.18: Original Specification, Different "Control" Groups

Variable	White Adults		African Americans	
Men in Disenfranchising	0.0070	0.0072	-0.0008	0.0012
States in 2010	(0.005)	(0.006)	(0.020)	(0.019)
Men in Disenfranchising	-0.0047	-0.0050	-0.0116	-0.0106
States	(0.003)	(0.003)	(0.013)	(0.013)
Disenfranchising States	-0.0024	-0.0039	-0.0036	-0.0144
in 2010	(0.004)	(0.004)	(0.014)	(0.017)
Men in 2010	0.0141***	0.0127***	0.0087	0.0063
	(0.003)	(0.003)	(0.011)	(0.010)
Disenfranchising States	0.0001	0.0039	0.0038	0.0075
	(0.002)	(0.004)	(0.005)	(0.006)
Men	0.0066***	0.0057***	0.0154**	0.0110
	(0.002)	(0.002)	(0.007)	(0.007)
Age		-0.0011***		-0.0036***
		(0.000)		(0.001)
Observations	161037	161037	18957	18957
Std error	cluster	cluster	cluster	cluster
Demographic Controls	N	Y	N	Y

Table 2.19: Placebo Estimations: Unemployment by Race

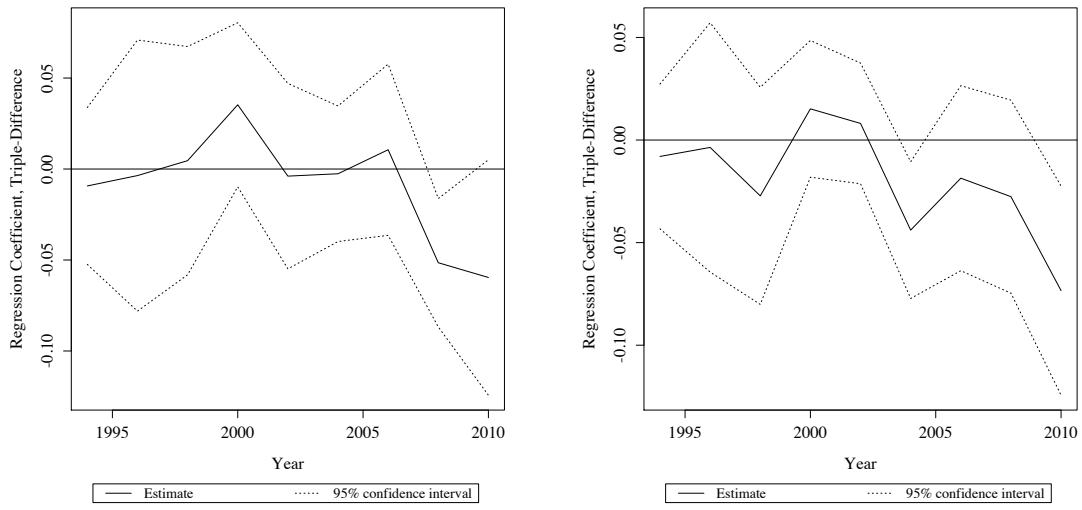
Variable	Age	Education (Ordered Probit)		Family Income (Ordered Probit)	
Men in Disenfranchising	0.0948	0.0104	0.0090	0.0090	-0.0059
	(0.286)	(0.024)	(0.020)	(0.021)	(0.016)
Men in Disenfranchising	-0.0978	-0.0104	-0.0052	-0.0029	-0.0047
	(0.343)	(0.025)	(0.025)	(0.017)	(0.014)
Disenfranchising States	0.3015	0.0277	0.0235	-0.0001	-0.0052
	(0.278)	(0.029)	(0.029)	(0.023)	(0.025)
Men in 2010	0.2096*	-0.1037***	-0.1054***	-0.0337***	0.0132
	(0.122)	(0.013)	(0.014)	(0.010)	(0.011)
Disenfranchising States	0.6934	-0.0803	0.0214	-0.1080	-0.0338
	(0.884)	(0.054)	(0.044)	(0.069)	(0.059)
Men	-1.7417***	0.0893***	0.0347***	0.1363***	0.0813***
	(0.143)	(0.010)	(0.010)	(0.005)	(0.006)
Year 2010	2.0353***	0.2888***	0.0966***	0.6800***	0.6818***
	(0.182)	(0.021)	(0.024)	(0.013)	(0.013)
Age			0.0316***		0.0277***
			(0.001)		(0.002)
African Americans			-0.0329		-0.4400***
			(0.028)		(0.028)
African American Men			-0.1534***		0.1141***
			(0.020)		(0.015)
Observations	189565	189565	179994	189565	179994
Std error	cluster	cluster	cluster	cluster	cluster
Demographic Controls	N	N	Y	N	Y

Table 2.20: Placebo estimations



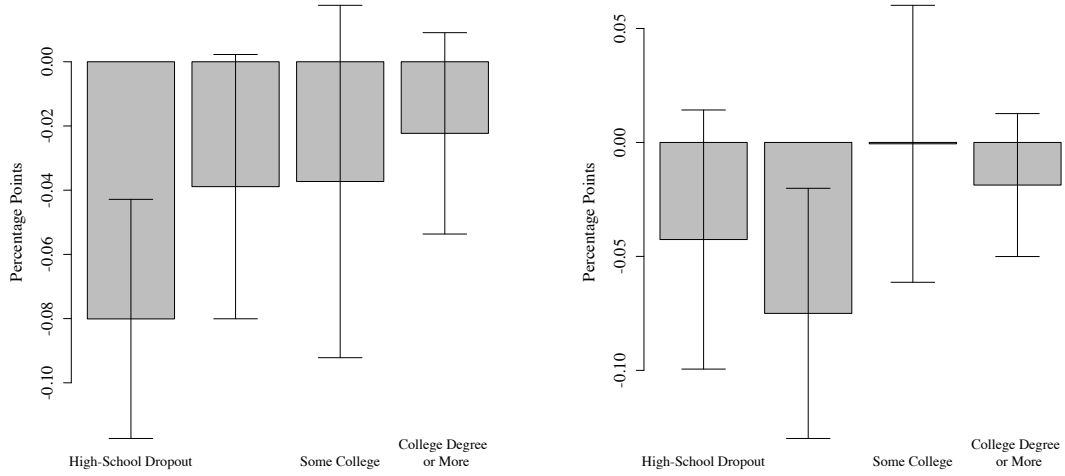
(a) Turnout (b) Registration

Figure 2.1: Effect on Turnout and Registration of African American Men, by Education Levels, 2004-2010



(a) Turnout (b) Registration

Figure 2.2: Effect of Disenfranchisement Laws on Turnout and Registration of African American Men, over Time



(a) Turnout

(b) Registration

Figure 2.3: Effect on Turnout and Registration of Males, by Education Levels, 1994-2010

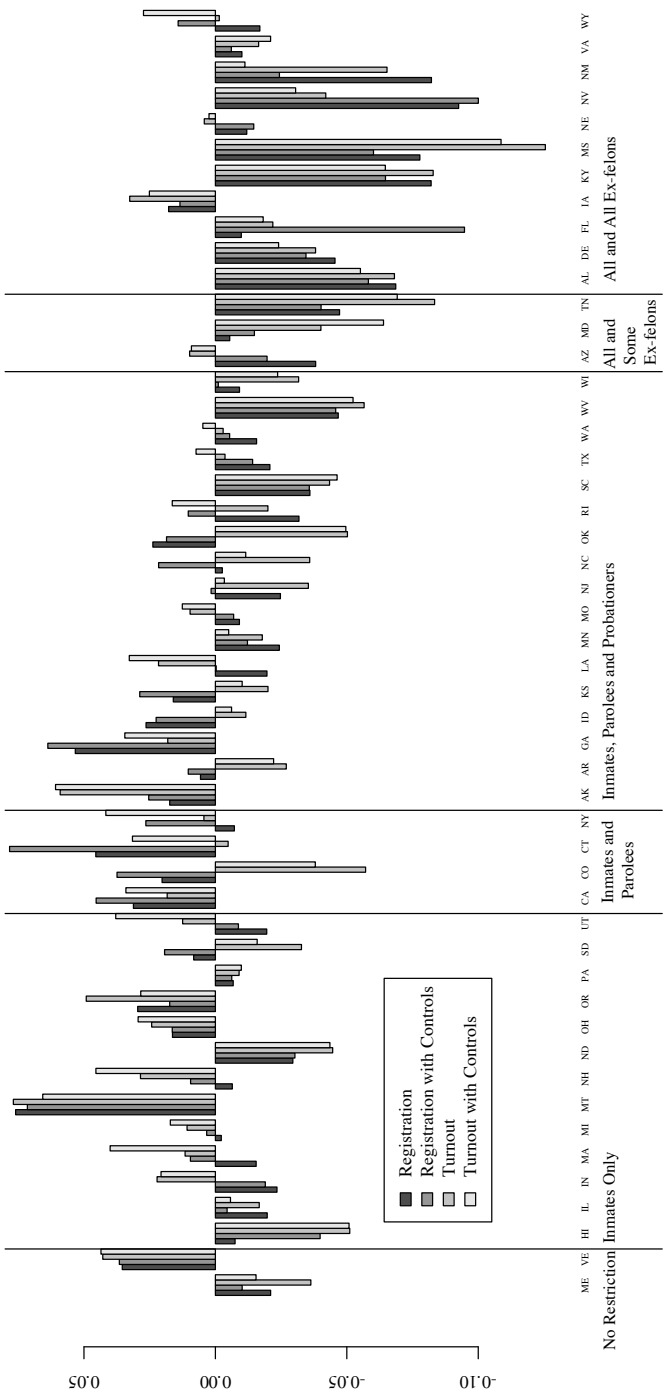


Figure 2.4: Changes is the "Gender Turnout Gap", A State-by-state Analysis.

Chapter 3

Participation and Representation: The Case of Felon Disenfranchisement

Felon disenfranchisement limits political participation and therefore offers an interesting setting to study the effect of turnout on public good provision, net of other determinants of both turnout and public good levels. Does turnout matter for the way public good is provided? What is the effect of felon disenfranchisement on the political process and on the access of vulnerable communities to public good?

This chapter argues that there are sizable negative effects of felon disenfranchisement for life on actual turnout in the United States by taking into account the effect of felon disenfranchisement on former felons and also on their communities, as counties with a high proportion of former felons will experience a decline in voting's normative power. Furthermore, it shows that the political process is significantly disrupted in disenfranchising states and that patterns of public good provision are negatively affected by felon disenfranchisement. Taken together, these results suggest that participation matters for policy outcomes, not just electoral outcomes, and more so now

than they have in the past. This chapter also shows that state partisanship cannot account for the differences observed between disenfranchising states and other states and that felon disenfranchisement has not affected partisan patterns.

Demographic characteristics of a county are important determinants of state public good provision (Ansolabehere et al., 2002). This chapter shows that vulnerable communities such as African Americans and the poor, communities who are most likely to be disenfranchised, have lost political clout between the 1970s and the 1990s in disenfranchising states, as the United States witnessed a surge in incarceration rates. Turnout in these communities decreased in disenfranchising states and public good provision followed the same pattern, while the same was not true in non-disenfranchising states. Redistribution has become less redistributive due to the gradual exclusion of exposed communities.

I show that felon disenfranchisement exerts a binding constraint on political participation of certain communities beyond what demographic characteristics would predict in the absence of felon disenfranchisement. This is crucial in abstracting from the endogeneity problem that arises when trying to measure the returns to political participation as certain communities that have a higher taste for public good also tend to vote less, as shown in a simple OLS setting. The conceptual framework I provide to think about the consequences of political participation on public good provision will make apparent that the study of felon disenfranchisement is suited to estimate the effect of higher turnout on levels of public good provision. It allows to separate between responses of public good provision to the intensive margin of voting (the taste for the public good) and the extensive margin (the number of people who vote), focussing on the latter. I show that political participation does matter for policy outcomes and that redistribution favors places where turnout is higher.

I estimate that 450,000 votes were lost in six states that disenfranchise felons for life between 1976 and 1996. Furthermore, I estimate that felon disenfranchisement

leads per capita transfers to be 18% lower on average in disenfranchising states than in other states, which amounts to 1.8 billion dollars a year in forgone public expenditure, mostly educational transfers. Finally, using the disproportionate consequences of felon disenfranchisement in certain communities, in certain states, at certain points in time, I am able to estimate that an extra percentage point in turnout is associated with an increase of 2 to 3% of per capita spending by the states, therefore allowing me to establish the value of political participation.

The first section will provide motivation for this chapter, review the literature and put forward the contributions of the chapter. The second section will present a simple model of the effects of felon disenfranchisement on redistribution which will lead to the discussion of my empirical strategy. The third section will consist of the main results of the chapter and a discussion. The last section concludes.

3.1 Motivation

In this section, I provide justifications for the focus of this chapter and its identification strategy. First, I examine the rationale for assessing the effects of felon disenfranchisement on turnout and the provision of public good and the merit of being able to tie the two outcomes together. Second, I describe the felon disenfranchisement system and explain why looking at variations in felon disenfranchisement regimes is appropriate. Finally, I detail how a change in penal practices from the 1970s to the 1990s offers a suitable design to study the consequences of felon disenfranchisement and explain why African Americans and the poor and their communities are especially vulnerable to felon disenfranchisement.

3.1.1 Felon Disenfranchisement, Participation and Public Good Provision

In this section, I show why it is important to understand the consequences of participation on political outcomes and how this chapter makes an important contribution by providing an instrument for turnout. Given large disagreements in the literature on the consequences of felon disenfranchisement on political participation, I justify the importance of the first stage of my analysis and the contribution it constitutes by taking into account "social" disenfranchisement. Finally, I outline the importance of the results on public good provision.

The Impact of Participation on the Political Process

A major contribution of this chapter will be to link patterns of political participation and patterns of public good provision and therefore to show that participation matters in the political process.

In a representative democracy, it ought to be the case that the composition of the electorate matters for electoral and policy outcomes. For example, in a median voter framework (Meltzer and Richard, 1981; Romer, 1975), if the median income of voters is different from the median income in the general population (if for example some parts of the income distribution are prevented from voting), political outcomes will not reflect the preferences of the population as a whole. As Verba et al. (1995) write: "Political participation provides the mechanism by which citizens can communicate information about their interests, preferences and needs and generate pressure to respond." It seems that felon disenfranchisement should reduce the representation of the disenfranchised, by preventing them from participating in the political life and, therefore, from influencing the political process. Husted and Kenny (1997) and Lott and Kenny (1999) have shown that the extension of suffrage and the increased political participation of some demographic groups (poor people and women for example)

had an impact on the size of government and on the composition of redistribution (see also Miller, 2008).

Thinking of public good provision as a mechanism of vote buying makes clear that redistribution should target places where the potential for political participation is higher (similarly to what political campaigns seem to do, Huckfeldt and Sprague, 1992). If politicians can convince a fraction of the electorate to vote for them by investing in this community, they should do so more in communities that vote more and where the political returns to public good provision are higher. Therefore, if political participation is constrained in certain places, redistribution should be skewed away from these places. This idea is simpler than analyzing the strategic distribution of funds between core and pivotal constituents (as in Cox and McCubbins, 1986; Dixit and Londregan, 1996; Dixit and Londregan, 1998). However, there are obvious endogeneity issues in trying to assess the value of political participation. For example, poor communities are likely to receive more public good from the states, but their turnout will also be lower than other communities. This will be apparent in my conceptual framework and in a simple OLS regression where I show that turnout is significantly negatively correlated to levels of public good. Martin (2003) finds mixed support for the thesis that increased turnout leads to an increase in federal spending within districts or within states, in a simple fixed-effects framework. By showing that felon disenfranchisement is a binding constraint on the political participation of certain communities in significant and predictable ways (which is important since it is the case that politicians make decisions based on it, a requirement that will not be met for other phenomena that affects turnout such as meteorological conditions for example), I make the case that it provides an appropriate setting for the study of the influence of political participation. This article therefore contributes to the literature attempting to establish the "value of a vote" (Fleck, 1999; Cascio and Washington, 2014).

Furthermore, the stakes of political participation have increased recently with the multiplication of "get out the votes" campaign, the Help America Vote Act and current controversies on I.D laws and Section 5 of the Voting Rights Act. If one thinks of public good provision as a means to target specific communities depending on their levels of political participation, one can expect that, as politicians have sought to stimulate political participation in politically profitable areas, they have also increased their efforts to provide public good in the "right" places. Therefore, we expect the results to be significantly different from results in earlier periods (Cascio and Washington, 2014).

With respect to a paper written simultaneously (Cascio and Washington, 2014), this article presents several contributions. First, it uses a multidimensional understanding of disenfranchisement beyond racial or gender discrimination. Second, by focusing on states outside of the Solid South, it provides an original lens through which to analyze disenfranchisement and discrimination and complements the literature on disenfranchisement in the South (Naidu, 2012; Cascio and Washington, 2014). Finally, it sheds light on social phenomena currently at work in the United States, and broadens our comprehension of the political consequences of the carceral state (Weaver and Lerman, 2010).

Felon Disenfranchisement and Turnout

This chapter aims first to show that felon disenfranchisement exerts a binding constraint on the participation of certain groups, despite and beyond the potential low turnout of former felons themselves, which is crucial in using variations in turnout due to felon disenfranchisement to assess the effects of political participation on public good provision.

It is possible that the disenfranchisement of a large fraction of a community would have repercussions on the political participation of the non-disenfranchised. By using

variations in the severity of the felon disenfranchisement regime, this chapter will not allow me to take a stance on the counterfactual turnout of former felons but will show that felon disenfranchisement affects the turnout of entire communities, therefore showing externalities of felon disenfranchisement on people who are not actually disenfranchised, through a potential effect on the social norm of voting. It is very possible that in neighborhoods with a high proportion of disenfranchised former offenders, there are contagion effects whereby people who are not formally disenfranchised would abstain from voting. It has been shown that voting is more likely if it is a prevalent social norm in one's network (Knack, 1992; Huckfeldt and Sprague, 1992; Gerber et al., 2008; DellaVigna et al., 2013) and even within households (Nickerson, 2008) which will be especially important in this setting given that an overwhelming majority of felons are males. In communities where a large fraction of the population is not eligible to vote, it is likely that the social norm of voting will be less strong. This chapter will expose aggregate, if localized, effects of felon disenfranchisement on political participation.

My analysis will rely on assessing the effects of felon disenfranchisement on vulnerable communities (poor and African Americans) in disenfranchising and non-disenfranchising states. This helps abstract from the effects of fixed characteristics that could affect the overall turnout or the overall level of public good provision in every state in every year. I will use the shift in penal policies and practices of the late 1970s as a source of variation over time, as it could be the case that disenfranchising states and non-disenfranchising states treat racial and social minorities differently even in the absence of felon disenfranchisement. Therefore, by analyzing the period between the 1970s and the 1990s, I will show that the difference between disenfranchising and non-disenfranchising states increases over time as both set of states experience a shift to a system of mass incarceration. In fact I will show that the evolution of this difference follows closely the evolution of the rates of incarceration.

Felon Disenfranchisement and Public Good Provision

I believe it is an important contribution of this chapter to analyze the consequences of felon disenfranchisement on the political process, beyond electoral outcomes and I justify the study of patterns of state provided public good provision as a measure of representation.

Due in part to the controversy over the effects of felon disenfranchisement on turnout, scholars have focused on the possibility that absent felon disenfranchisement elections would have been reversed (Uggen and Manza, 2002). The most controversial question in the literature is whether or not felon disenfranchisement helped Bush carry Florida in 2000. However, my purpose is different and I will focus on the provision of public good from the states at the county level as an outcome of interest.

In this chapter, I will use revenues distributed from states to local levels of government aggregated at the county level as a measure of redistribution and representation. Ansolabehere et al. (2002), in their analysis of the Supreme Court decision in *Baker v. Carr* in 1962, have shown that this outcome is appropriate to detect differences in political representation and that it is responsive to changes in electoral rules. Therefore, I think of this outcome as a potential measure for representation and show that communities with a high proportion of former felons are less properly represented in disenfranchising states than similar communities in other states. Ansolabehere et al. (2002) have also shown that using a 20-year period was appropriate to detect changes in patterns of transfer and I will therefore look at a similar timeframe.

3.1.2 Felon Disenfranchisement

Variations between states that are the most severe in managing the voting rights of felons (i.e. that disenfranchise systematically beyond completion of sentence) and states that are more lenient provide an adequate framework for assessing the consequences of felon disenfranchisement because the laws have been stable over the period

I examine (1970-2000), there are no good predictors of the changes in these laws before this period (once the "Solid South" has been excluded) and there is an order of magnitude of difference in the size of the disenfranchised population between states that disenfranchise felons for life and states that do not.

Only two states (Washington and Tennessee) have repealed disenfranchisement for life between 1975 and 2000 (while maintaining some restrictions on the voting rights of former felons). Many states have moved from disenfranchisement for life to more restrained forms of felon disenfranchisement starting in the 1960s. Following the Civil Rights movement and the passage of the Civil Rights Act there has been a noticeable trend towards the repeal of disenfranchisement of felons for life in the 1960s and very early 1970s. However, Manza and Uggen (2006) point that the "momentum for civil rights legislation has eroded since the mid-1970s." Indeed, in their analysis of the repeal of disenfranchisement of ex-felons, Behrens et al. (2003) have found that decade fixed effects were not significant for the 1980s, 1990s and early 2000s, again documenting the erosion of the trend towards liberalization of felon disenfranchisement. Besides, in *Richardson v. Ramirez* (1974), the Supreme Court held that felon disenfranchisement did not violate the Fourteenth Amendment and no appeal on the basis of the Civil Rights Act has been successful in repealing felon disenfranchisement for life.

In their analysis of the determinants of the liberalization of felon disenfranchisement in post World War II America, Behrens et al. (2003) find that political and economic variables had no predictive power. They find conflicting evidence on the role played by race in this process and show that state punitiveness (measured by the incarceration rate) is not related to the severity of the disenfranchisement regime. One fact emerges clearly from their analysis however: there are wide geographic disparities in the way the voting rights of felons are treated, and in particular, the "Solid

South" is a clear outlier.[1] Similarly, Burch (2012) writes that "it is not altogether clear that racial motivations explain the presence of disfranchisement laws outside the South." I will come back on this and explain why this and other facts lead us to exclude the Solid South from our analysis. In any case, it is important to note that even though differences in the severity of the felon disenfranchisement regime across states are not random, their determinants are not well understood and unclear, and most potential explanations have been shown to not predict the current state of felon disenfranchisement, outside of the Solid South.

The analysis will focus on the period that goes from 1976 (or 1977) to 1996 (or 1997) with the inclusion of a few years before and after this period to place the effects of felon disenfranchisement in a larger framework. There are two reasons for picking 1976 as our starting year. First, it is sufficiently long after other disruptions of the political process (*Baker v. Carr*, Ansolabehere et al. (2002), the Voting Rights Act, the removal of poll taxes and literacy tests, Cascio and Washington, 2014 ; Husted and Kenny, 1997) to provide an adequate starting point. Second, as I just discussed, most changes to the voting rights of current and former felons happened before this date. Similarly, there are two reasons for choosing 1996 as the ending point of the analysis. First, starting in 1997, as the salience surrounding the consequences of the "tough on crime" policies increased, the call for a reassessment of the legitimacy of felon disenfranchisement gained momentum, leading several states to modify their legislation (Porter, 2010) albeit in sometimes ambiguous ways (Meredith and Morse, 2013). Second, in other works documenting the disruption of state political patterns following a significant modification of the electoral processes (Ansolabehere et al., 2002; Cascio and Washington, 2014), a 20-year period has been judged as an appro-

[1]The "Solid South" refers to the eleven states of the confederacy (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, Virginia) to which are added Kentucky, Oklahoma, and West Virginia to form an ensemble that unanimously supported the Democratic party from the end of Reconstruction to the passage of the Civil Rights Act.

priate framework. Furthermore, as I will show later, even though laws presiding the voting rights of former felons have been stable from the 1970s to the 1990s, during this period rates of incarceration have surged, which will provide a time variation exploited in this chapter.

I choose to exclude the "Solid South" from the analysis for three reasons. First, scholars have consistently argued that the South and in particular the "one-party 'Solid South'" (Manza and Uggen, 2006) played a historically significant role in the development and the maintenance of felon disenfranchisement. Keyssar (2009) writes that: "in the South, these measures were often more detailed and included lesser offenses, targeting minor violations that could be invoked to disfranchise African Americans." Alabama and Mississippi, for example, provided several provisions that targeted specifically African Americans. This, of course, is consistent with the history of discrimination towards African Americans in the Solid South - through, for example, the use of literacy tests and poll taxes (which Cascio and Washington (2014) and Naidu (2012) argue had a large effect on African Americans). Poll taxes were still in effect until 1964 in Arkansas and until 1965 in Alabama, Mississippi, Texas and Virginia (Husted and Kenny, 1997) and literacy tests persisted through 1965 in Alabama, Georgia, Mississippi, North Carolina, South Carolina and Virginia. Cascio and Washington (2014) estimate that the effects of these measures were only fully reversed as late as the early 1980s. Furthermore, Behrens et al. (2003) have shown that the South was an outlier with respect to the severity of felon disenfranchisement regimes compared to the rest of the United States. Second, many states in the Solid South have ambiguous legislation where some felons are excluded from the political process for life but not others. This creates an obstacle to using the severity of the felon disenfranchisement regime as a source of variation because it makes it hard to classify these states. Third, in Figure 3.1, one can see that states in the Solid South have significantly and consistently higher rates of incarceration than other states since

the 1980s. Furthermore, the seven states with the highest incarceration rates in 2003 are all in the Solid South. This calls to attention two potential problems. First, it is likely that the particularly severe change in penal practices in the Solid South could be endogenous to the political system when the uniform change outside of the South could be more likely attributed to a country-wide trend in the United States, not specific to a particular group of states. Second, the particularly high rates of incarceration in the Solid South would prevent from basing our analysis on variations on the severity of the felon disenfranchisement regime since states with "lenient" regime could still have a larger disenfranchised population (as illustrated below by the case of Georgia) than more severe states. Therefore, I exclude these states from the analysis. I believe it is a contribution of this chapter to display effects of disenfranchisement outside of the Solid South, where suffrage restrictions have already been widely documented. Studying felon disenfranchisement in the "Solid South" would certainly be interesting but remains beyond the scope of this chapter.

Furthermore, rates of incarceration in the Solid South are substantially higher than in the rest of the United States is excluded, as can be seen in Figure 3.1.[2] As can be seen in Figure 3.1, the rates of incarceration are very similar between disenfranchising and non-disenfranchising states and I can therefore be confident that the differences in the size of the disenfranchised population will be largely due to the differences in the severity of the disenfranchisement regime. In comparing disenfranchising states and non-disenfranchising states, I aim to take advantage of the fact that "U.S. states are an important laboratory for empirical investigation of policy processes" (Besley and Case, 2003).

The disenfranchising states in this analysis are Delaware, Iowa, Nebraska, Nevada,

[2]Another group of states that shows a significant difference in rates of incarceration is New England.[3] These states also tend to be more liberal in the restrictions they impose on the voting rights of felons (even though an indicator for regional difference has been found to be insignificant in predicting felon disenfranchisement regimes, Behrens et al. (2003)). However, all results carry through when the New England states are excluded from the group of the "non-disenfranchising states".

New Mexico and Wyoming. I will contrast the patterns of political participation and public good provision in those states with "non-disenfranchising states" (formally, only Maine and Vermont do not have any restriction on the voting rights of felons, but I will use this terminology for the states that do not have restrictions on the voting rights of felons *beyond completion of sentence*). I will exclude Alaska and Hawaii (as well as Washington D.C., as there is no within-state redistribution there), in addition to Arizona, Maryland and Washington (which have some restrictions on the voting rights of felons beyond completion of sentence but no systematic ones). Therefore, the "non-disenfranchising states" are: California, Colorado, Connecticut, Idaho, Illinois, Indiana, Kansas, Massachusetts, Maine, Michigan, Minnesota, Missouri, Montana, North Dakota, New Hampshire, New Jersey, New York, Ohio, Oregon, Pennsylvania, Rhode Island, South Dakota, Utah, Vermont, and Wisconsin. The final sample includes thirty-one states, six of which are disenfranchising states.[4]

The estimated sizes of the disenfranchised population in the different states in 2000 computed by Uggen and Manza (2002) support this classification. In the six states that disenfranchise for life outside of the Solid South, 5.0% of the voting age population is disenfranchised on average. This proportion is only 0.86% in the states I qualify of "non-disenfranchising" (showing once again that these states are not sensu stricto non-disenfranchising but that the size of the disenfranchised population will be significantly smaller). In the Solid South, 3.58% of the voting age population is disenfranchised and Georgia illustrates perfectly the obstacles to including the Solid South in the sample since 4.86% of the population is disenfranchised (a rate similar to disenfranchising states) even though there is no disenfranchisement for life in this state. This departure from other non-disenfranchising states comes from a difference in the rates of incarceration, which is endogenous to the political process. Finally, because of the ambiguity of their regime (which disenfranchises some former felons but not

[4]Results using a subset of the non-disenfranchising states neighboring disenfranchising states as a control group can be found in Appendix 2.

all of them), Arizona, Maryland and Washington have an average rate of disenfranchisement of 3.70% putting them between disenfranchising and non-disenfranchising states and therefore justifying their exclusion from the sample in order to get a precise estimate of the effects of felon disenfranchisement for life.

3.1.3 Mass Incarceration, Demographic Imbalances in the Prison Population and Felon Disenfranchisement

Focusing on the evolutions from the 1970s to the 1990s is justified because rates of incarceration have surged during this period, and with them, so has the size of the disenfranchised population. Given that the felon population has wide demographic imbalances and that "punishment is distinctly concentrated by place" (Sampson and Loeffler, 2010), I will provide a justification for looking at county-level outcomes and focusing on the racial and social composition of a county, in the absence of data on the number of former felons. This approach will allow me to make a key contribution to a growing literature that "document[s] aggregate-level effects of incarceration beyond those experienced by individuals." (Wakefield and Uggen, 2010)

As Burch (2012) argued,"the number of people who are disfranchised in a state depends not only on the severity of the disfranchisement policy, but also on crime rates, policing practices, and legal processing." This chapter will therefore use a policy shift in penal practices, seemingly unrelated to felon disenfranchisement as a source of variation over time. Even in the absence of change in the severity of the disenfranchisement regime, the size of the disenfranchised population has dramatically increased due to a surge in incarceration from the mid 1970s to the late 1990s. As can be seen in Figure 3.2, the overall rate of incarceration in the United States went from 96 persons in custody in 1970 per 100,000 residents to 478 incarcerated people per 100,000 residents in 2000, which represents more than a four-fold increase (Maguire and Pastore, 2001). It is also remarkable that the United States' rate of incarceration

was greater in 2005 than Russia's, and far greater than countries comparable to the United States (Mauer, 2006). Rates of incarceration rose starting in the mid-1970s, partly due to "tough on crime" policies enacted in that decade, later accompanied by the "war on drugs" and a set of mandatory sentencing policies. In 1976 the rate of incarceration exceeded its previous post-WWII record of 119 inmates per 100,000 residents in 1961, from then on it increased at an average rate of 6% per year until 2000, when the average rate of increase between 1950 and 1976 was only of 0.4% per year (Maguire and Pastore, 2001). Furthermore, rates of incarceration will underestimate the number of new felons, since, as rates of incarceration soared, the penal system has also increased non-incarceration punishments and for example, in 2006, 30% of felony convictions did not entail imprisonment.

A major obstacle to looking at the consequences of felon disenfranchisement is the absence of administrative data on the number of former felons. Uggen and Manza (2002) make projections of rates of disenfranchisement at the state level, but they are unable to disaggregate these numbers at the county level. An important contribution of this chapter is to focus on vulnerable communities as being disproportionately affected by felon disenfranchisement, based on the demographic imbalances of the prison population and supported by the literature in sociology and my own estimates as seen in the Appendix.

I will use the proportion of African Americans in a county as a vulnerable community where the proportion of former felons will be higher because the increases in arrest and incarceration rates have been most dramatic for drug crimes and for African Americans. From 1985 to 2000, new sentences to state prisons rose by 101%, but new sentences to state prisons for drug crimes increased by 402%, constituting 52% of the overall increase (Mauer, 2006). In fact, the prevalent opinion among policy makers was that the justice system ought to be "tough on crime", and, specifically, lead a "war on drugs." The change in the composition of new sentences

had important consequences on the demographic characteristics of inmates. For example, the number of white inmates incarcerated for drug offenses increased threefold between 1985 and 1995, while it increased seven times over the same period for black inmates (Mumola and Beck, 1997). In 2002, 12% of black men in their twenties were in prison or jail (Harrison and Karberg, 2002). Similarly, the lifetime likelihood of imprisonment for black male children was 32%, more than 5 times that for white male children (6%) and approximately double the one for hispanic male children (Bonczar, 2003). One should note that drug offenses are the most likely felony offenses to be punished by a non-incarceration sentence (Durose et al., 2009). Given that African Americans are overrepresented among felons convicted for drug offenses (49% or felons convicted for drug trafficking were black, when the overall proportion of African Americans among all convicted felons in 2006 is 38%, Durose et al., 2009) it is likely that estimates focusing on imprisonment rather than on felony conviction understate the racial inequalities.

Another dimension in which the penal population is unbalanced is socio-economic status as "criminal justice involvement is recognized as both a cause and a consequence of poverty" (Harris et al., 2010) and this will prove crucial to the use of the poverty rate as another proxy for the proportion of former felons in a county. Pettit and Western (2004) show that race *and* class are strong predictors of imprisonment, with lower educated males having higher chances of going to prison. Western and Pettit (2005) point out that "men with few economic opportunities may turn to crime." But a felony conviction and a stay in prison is also thought to lower economic opportunity (Pager, 2003) and to affect the lifelong trajectories of wages, especially when the imprisonment episode happens early in life (Western and Pettit, 2005). Harris et al. (2010) examine another dimension in which prison threatens economic status after release, in looking at the rise in financial punishment in the United States. In their analysis of a sample of felons in the state of Washington in 2008,

they find that their mean legal debt in 2008 was $10,840. Therefore, people with lower socio-economic status will be over-represented in the former felon population.

It is likely that poverty and racial inequality will be associated with higher rates of incarceration and higher proportions of former felons beyond the crimes committed by poor people or black people, through social segregation. As we have just seen, the distribution of offenders is non-uniform demographically and reveals "a picture of American race and poverty fundamentally transformed by the penal system" (Western, 2006). Furthermore, through social segregation, it is very likely that the distribution of former felons is non-uniform geographically, as "punishment is distinctly concentrated by place" (Sampson and Loeffler, 2010).[5] As Burch (2007) notes, "a non-uniform distribution of offenders may mean that at the neighborhood level, criminal convictions matter for politics." Therefore, it is very possible that looking at the potential reversal of elections at the state or even the district level, as in Uggen and Manza (2002), would miss important, localized effects. For these reasons, this chapter will analyze variations of political participation (through turnout) and redistribution (through measures of state-to-county transfers) at the county level.

I will use the proportion of African Americans and the poverty rate at the county level to identify the potential effects of felon disenfranchisement, as "the combination of poverty, [...] and racial isolation is bound up with high levels of incarceration" (Sampson and Loeffler, 2010) and supplement the analysis with direct evidence in the Appendix. Wakefield and Uggen (2010) "describe the spatial concentration of incarceration in predominately poor and African American communities." Just as measures of poverty understate the lifelong potential incidence of poverty (Hoy et al., 2012), by not taking into account people just above the poverty line who have experienced poverty at some point in their lives, it is likely that the two demographic

[5]I provide empirical support for the use of those two proxies at the county level based on available data on the number of parolees and the number of prison releases in a few set of states in 2008 as can be found in the Appendix.

indices (proportion of African Americans and poverty rate) are good predictors of the incidence of disenfranchisement, but not *measures* of it. Indeed, social segregation and sorting result in disadvantaged groups being geographically close while better off groups live separately. Therefore, as the literature on concentration of punishment has shown, groups where rates of incarceration are high are geographically situated with other observable groups with high incarceration rates (such as African Americans or poor communities). Therefore, I think of the proportion of African Americans and the poverty rate as a good geographic markers for the presence of former felons even outside of these two communities. After all, even though rates of incarceration are higher in the African American communities, it is still the case that a majority of offenders are white.

3.2 Conceptual Framework and Empirical Strategy

In this section, I provide a conceptual framework for why we should expect the effects of felon disenfranchisement to be detectable through a difference-in-differences (or similarly, by analyzing the relationship between turnout and public good provision and the demographic characteristics of a county). Using a simple model, I link turnout to public good provision and make two predictions about patterns of public good provision, one of which will be directly used in the empirical strategy. I then present the empirical strategy based on the model and the observations I made earlier about the difference between counties with a high or low proportion of former felons (proxied for by racial and social characteristics) in disenfranchising and non-disenfranchising states and the evolutions of this difference over time. I finally present the data and provide summary statistics.

3.2.1 A Model of Public Good Provision and Disenfranchisement

This model provides a framework for thinking about the relationship between turnout and public good provision and what happens when turnout is lowered in one county in a two-county state because of felon disenfranchisement.

Public Good Provision in Two Counties

I present a model of public good provision in two counties, where the amount of public good provided to each county is decided by a centralized institution (i.e. at the state level, for example by the governor or the majority party). The model is close to the one found in the analysis of centralized versus decentralized provision of local public goods conducted by Besley and Coate (2003).

There are two counties with population N_1 and N_2 and baseline turnout levels T_1 and T_2. Each county benefits from the provision of a localized public good in quantities g_1 (for county 1) and g_2 in county 2. The public good can be thought of as infrastructures (roads, etc...). The public good therefore is geographically circumscribed as people living in a given county will be the main beneficiary of infrastructures in that county. The public good is financed at a price p by a tax levied on all citizens in the two-county state. The utility function of an inhabitant of county i is:

$$u_i(g_1, g_2) = \lambda_i ln(g_i) - p\frac{g_1 + g_2}{N_1 + N_2}$$

where λ represents the taste for public good. This taste can be different in each county and will be thought to be determined by demographic characteristics of the county (a county with a high proportion of people under 18 could have a greater taste for education for example). Furthermore, λ could also reflect departures from an equal taxation of all citizens in the state (for example, if richer counties were taxed

more to finance the provision of the public goods, I would model this by a lower λ in those counties rather than modify the part of the utility function due to the financing of the public good).

The centralized power seeks to maximize the overall utility of the citizens, weighted by their potential turnout:

$$\begin{aligned} U(g_1, g_2) &= N_1 T_1 u_1 + N_2 T_2 u_2 \\ &= N_1 T_1 \big(\lambda_1 ln(g_1) - p\frac{g_1 + g_2}{N_1 + N_2}\big) + N_2 T_2 \big(\lambda_2 ln(g_2) - p\frac{g_1 + g_2}{N_1 + N_2}\big) \end{aligned}$$

The centralized power naturally weighs the counties by their population but also by their baseline turnout levels as politicians will have a greater interest in maximizing the utility of citizens in counties with high levels of political participation. This comes from conceiving the provision of public goods by politicians as a vote-buying mechanism where politicians should focus on convincing the electorate in places where political participation is higher. This could also arise if voters conceived voting as a way to make their interests taken into account and to pressure politicians (who then internalize this constraint). The empirical analysis will make the case that this is correct beyond the particular mathematical formulation of the model. Note that the same trade-offs would apply to the centralized government if it was trying to maximize its vote share, but that the functional form makes the problem simpler.[6] The centralized government maximizes its utility function and I obtain that the optimal

[6]One could think that the centralized power would only be able to modify its vote share in the limits of the groups baseline turnout. For example, one could make the objective function of the centralized government a vote-maximizing one:

$$V(g_1, g_2) = \frac{1}{1 + e^{-(N_1 T_1 u_1 + N_2 T_2 u_2)}}$$

or any other transformation of the $U(g_1, g_2)$ function by an increasing function bounded between 0 and 1.

provision of public good (from the view of the centralized government) is:

$$(g_1^*, g_2^*) = \Big(\frac{N_1 T_1 \lambda_1 (N_1 + N_2)}{p(T_1 N_1 + T_2 N_2)}, \frac{N_2 T_2 \lambda_2 (N_1 + N_2)}{p(T_1 N_1 + T_2 N_2)} \Big)$$

Therefore, the model gives the simple prediction that the patterns of redistribution in a state between counties will depend on the population, the baseline turnout and the taste for the public good in each of the counties. It makes the case that if political participation matters (which is modeled here in the weighting scheme of the politician), my empirical strategy is understandable in a simple conceptual framework. I will then make the assumption that the turnout and the taste for the public good in a county depend on the demographic characteristics of the population and try to establish that:

$$\frac{\partial g/N}{\partial T} > 0$$

.

In a Disenfranchising State

I now seek to analyze within this framework the effects of disenfranchisement and more precisely the effects of a disenfranchisement measure that disproportionately affects counties with higher poverty rates and proportion of African Americans, two groups that have a higher taste for public good provision (for example, African American children are less likely to go to private school than their white counterpart, Keigher and Gruber (2009)).

I suppose that there are still N_1 and N_2 citizens in the two counties, but that in county 1, the turnout has been lowered to $\bar{T_1}$ (with $\bar{T_1} < T_1$) because felon disenfranchisement has excluded a fraction of voters and could have had contagion effects on people still formally enfranchised but abstaining as the social norm of voting is weaker. Since the number of taxpayers in the state is left unchanged, the utility

function of a citizen in county 1 or 2 is left unchanged. However, the centralized government only takes into account potential voters when making decisions. Therefore, its objective function is now the following:

$$\bar{U}(g_1, g_2) \;=\; N_1\bar{T}_1u_1 + N_2T_2u_2$$

The maximization of this objective function gives the following levels of public goods in the two counties:

$$(\bar{g_1}, \bar{g_2}) = \Big(\frac{N_1\bar{T}_1\lambda_1(N_1+N_2)}{p(\bar{T}_1N_1+T_2N_2)}, \frac{N_2T_2\lambda_2(N_1+N_2)}{p(\bar{T}_1N_1+T_2N_2)}\Big)$$

Two predictions

The expressions of the levels of public goods in the different counties in the non-disenfranchising and disenfranchising states allow us to make predictions about the effects of felon disenfranchisement. First, I have a prediction about the overall, per capita level of spending in non-disenfranchising and disenfranchising states. Precisely, I calculate:

$$\begin{aligned}\frac{\bar{g_1}+\bar{g_2}}{N_1+N_2} - \frac{g_1^*+g_2^*}{N_1+N_2} &= \frac{\bar{T}_1N_1\lambda_1 + T_2N_2\lambda_2}{p(\bar{T}_1N_1+T_2N_2)} - \frac{T_1N_1\lambda_1+T_2N_2\lambda_2}{p(T_1N_1+T_2N_2)} \\ &= \frac{N_1N_2T_2}{p(T_1N_1+T_2N_2)(\bar{T}_1N_1+T_2N_2)}(\lambda_1-\lambda_2)(\bar{T}_1-T_1)\end{aligned}$$

Therefore, I can say that the overall per capita level of public good provision is going to be lower in a disenfranchising state (compared to a non-disenfranchising state) if and only if disenfranchisement affects counties where the taste for the public good is higher. Given the demographic characteristics of the felon population, felon disenfranchisement will likely affect counties with higher poverty rates and propor-

tions of African Americans, two groups I postulate have higher taste for the public good (either because they rely more on public goods for their education or health or because redistribution is progressive). This prediction is similar, even if in a totally different framework, to a median voter prediction where the median income among the potential voters increases as lower parts of the income distribution are disenfranchised, which, in turn, reduces the levels of public good provision. This prediction has been empirically supported by Husted and Kenny (1997) even if a different setting (poll taxes and literacy tests).

The second way in which the model is helpful is in shedding light on the patterns of redistribution within states. As noted earlier, in a non-disenfranchising state, public good provision is influenced by the size of the county, the baseline turnout and the taste for the public good in that county. Therefore, when looking at per capita levels of spending, I find that redistribution patterns are only affected by the distribution of different tastes for it and the different baseline turnout levels. However, these patterns are disrupted by felon disenfranchisement. We have:

$$\begin{aligned}
\frac{g_1^*}{N_1} - \frac{g_2^*}{N_2} &= \frac{N_1+N_2}{p(T_1N_1+T_2N_2)}(T_1\lambda_1 - T_2\lambda_2) \\
\frac{\bar{g_1}}{N_1} - \frac{\bar{g_2}}{N_2} &= \frac{N_1+N_2}{p(\bar{T_1}N_1+T_2N_2)}(\bar{T_1}\lambda_1 - T_2\lambda_2)
\end{aligned}$$

which gives:

$$\begin{aligned}
\Big[\frac{\bar{g_1}/N_1 - \bar{g_2}/N_2}{(\bar{g_1}+\bar{g_2})/(N_1+N_2)}\Big] \quad - \quad & \Big[\frac{g_1^*/N_1 - g_2^*/N_2}{(g_1^*+g_2^*)/(N_1+N_2)}\Big] = \\
& \frac{(N_1+N_2)^2\lambda_1\lambda_2T_2}{(\bar{T_1}N_1\lambda_1+T_2N_2\lambda_2)(T_1N_1\lambda_1+T_2N_2\lambda_2)}(\bar{T_1}-T_1) < 0
\end{aligned}$$

The model shows that patterns of redistribution are modified due to felon disenfranchisement, which skews redistribution away from the disenfranchised and their communities. Specifically, it predicts that the difference-in-differences between the

county with a high proportion of former felons and the county with a low one in disenfranchising and non-disenfranchising states will be negative and will depend on the size of the effect of disenfranchisement on turnout.[7]

3.2.2 Empirical Strategy

The model has provided a conceptual framework to evaluate the effects of felon disenfranchisement through comparing outcomes in counties with a high or low proportion of former felons in disenfranchising and non-disenfranchising states. I have explained why it is appropriate to analyze the period from the 1970s to the late 1990s (precisely from 1972 to 2002) and to use the proportion of African Americans and/or the poverty rate as proxies for the proportion of former felons, as those communities are especially vulnerable to imprisonment. Therefore, I will run regressions of the following type:

$$y_{ist} = \alpha + \sum_{t \neq 1976} \beta_{1t} Dis_s Pov_{ist} + \sum_{t \neq 1976} \beta_{2t} Pov_{ist} + \beta_3 Dis_s Pov_{ist} + \beta_4 Pov_{ist} + \gamma_{st} + \epsilon_{ist} \quad (3.1)$$

where i represents a county, s the state it is in and t the time. y_{ist} represents the outcome of interest, which will be first turnout (and then log per capita state transfers), Dis_s is an indicator of whether the state disenfranchises felons for life. This is invariant over time. And finally, Pov_{ist} is the poverty rate in the county i at time t. I will repeat this exercise replacing the poverty rate by the proportion of African Americans and by including both indices.

The coefficients of interest are the β_{1t} which measure the difference between counties with a high or a low poverty rate in disenfranchising and non-disenfranchising states over time (taking 1976 as the baseline, hence the exclusion of that year in the calculation of the β_{1t} coefficients and the inclusion of the poverty rate and of an interaction term between Dis_s and the poverty rate which normalizes the $beta_1$ coefficients in 1976 separately for disenfranchising and non-disenfranchising states). Another way to look at these coefficients is that they measure the difference between

[7]Because disenfranchisement and a lower turnout in county 1, $\bar{T_1}$, affects positively the level of public good provision in county 2, the within state difference (or the difference-in-differences) should be greater than the difference between the average per capita transfer in disenfranchising and non-disenfranchising states.

disenfranchising states and non-disenfranchising states in the *gradient* of turnout to the poverty rate. This gradient of course is a measure of the *difference* in turnout levels between counties with higher and lower poverty rates divided by the difference in the poverty rates.

The γ_{st} coefficients represent state-year fixed effects as I am interested in patterns of participation and public good provision within each state and year. They will help to control for specific conditions that could affect turnout as a whole, and also for the potential effects of felon disenfranchisement on overall state redistribution (as predicted by the model). In this setting and for reasons detailed in the appendix, the inclusion of county fixed effects is not straightforward. However, additional analysis shown in the Appendix shows that the results are not due to unobservable , fixed characteristics of the counties.

I expect the β_{1t} coefficients to decrease over time as the rates of incarceration soar in both disenfranchising and non-disenfranchising states and as the size of the felon population therefore increases. To test for this prediction over a 20-year period between 1976 and 1996 (or 1977 and 1997 depending on the outcome), I will run the following regressions:

$$y_{ist} = \alpha + \beta_1 Dis_s Pov_{ist} 1_{Year=1996} + \beta_2 Pov_{ist} 1_{Year=1996} + \beta_3 Dis_s Pov_{ist} + \beta_4 Pov_{ist} + \gamma_{st} + \epsilon_{ist}$$

I expect the coefficient β_1 to be negative and significantly different from 0 to show that turnout and then level of public good provision are lower in a disfranchising state than would be predicted by the demographic characteristics of the county in a non-disenfranchising state. Therefore I will establish that in 1996, when I expect the effects of felon disenfranchisement to be the large, it is true that $\bar{T}_1 < T_1$ and that this difference translates in a difference in public good provision as predicted by the model.

Finally, I will use felon disenfranchisement as an instrument for political participation. In doing so, I will show the link between participation and redistribution and measure the elasticity of redistribution to turnout. This will help show that the effects measured are indeed due to differences in participation by including both outcomes

(turnout and redistribution) in the same framework (rather than having two reduced form estimates potentially coming from different sources of variation).

To put it differently, if we consider that the per capita provision of public good $\frac{g}{N}$ is a function of the political participation (or turnout T) and the taste for redistribution in a county λ, as the model shows, and that those two parameters depend on the demographic characteristics of the county d, I first show that turnout is reduced in disenfranchising states beyond what demographic characteristics would predict:

$$\frac{\partial \bar{T}}{\partial d} - \frac{\partial T}{\partial d} < 0$$

Then I turn to how the per capita level of public good is impacted by demographic characteristics:

$$\frac{\partial g/N}{\partial d} = \frac{\partial g/N}{\partial T}\frac{\partial T}{\partial d} + \frac{\partial g/N}{\partial \lambda}\frac{\partial \lambda}{\partial d}$$

And I show that:

$$\frac{\partial \bar{g}/N}{\partial d} - \frac{\partial g/N}{\partial d} = \frac{\partial g/N}{\partial T}\Big(\frac{\partial \bar{T}}{\partial d} - \frac{\partial T}{\partial d}\Big) < 0$$

With the results of the first stage, I can conclude that:

$$\frac{\partial g/N}{\partial T} > 0$$

Which is to say that political participation matters. The IV approach allows to calculate this elasticity of public good provision on turnout. I will therefore establish the impact of political participation on representation as measured by per capita state-to-county transfers.

I will include some demographic controls beyond the proportion of African Americans and the poverty rate by county. Those controls are the proportion of people under 18 and over 65, log per capita average income, the unemployment rate of the county, and the proportion of non-citizens. These controls are thought to affect both

the taste for redistribution in the county (richer counties might want less redistribution, younger counties might demand more educational spending) and different inclinations to vote. I also include density of population in the county, since this could influence the provision of public goods if public goods benefit from agglomeration effects. The regressions are weighted by population and standard errors are clustered at the county-level since the variations of interest will be observed at the county-level (a combination of the severity of the disenfranchisement regime in the state and the demographic composition of the county).

3.2.3 Data and Summary Statistics

The outcomes of interest are turnout and state-to-county per capita spending. For turnout, I will use presidential turnout at the county-level provided by Jim Snyder. I divide the number of votes cast by an estimate of the "voting eligible population" in the county, calculated as the population of citizens over 18. For state-to-county redistribution, I use the Census of Governments conducted every five years. I aggregate the variables measuring intergovernmental revenues from the state government for any level of government below the county level and I then aggregate at the county-level. Therefore, when I talk about "state-to-county" transfers, I consider the county as a geographic unit to which localized transfers are delivered (as in the model) rather than the county as an administrative unit. In this, I follow Ansolabehere et al. (2002) and Cascio and Washington (2014). It is important to note that these transfers result in public good provisions (education, roads...) and are not private benefits to their recipients (such as cash transfers for example). I take the log of the per capita transfers at the county level, which scales the outcome variable and provides a natural interpretation for the results.

Demographic variables were obtained from the decennial Censuses interpolated. The number of African Americans at the county level was missing for 10% of the

counties in 1970. For this reason, I completed the 1970 Census with the 1970 City and County Data Book Consolidated File. Foreign-born aliens were missing from the 1980 Census and were interpolated from earlier and later censuses. Transfers from the state of New York to the city of New York happen at the city level and not at the county level, therefore, the five counties in New York City (Bronx, Kings, New York, Queens and Richmond) have been merged. Yellowstone National Park (WY) is dropped. The final sample consists of 1,629 counties.

Table 3.1 presents summary statistics. We observe that the disenfranchising and non-disenfranchising states are different especially with respect to the racial composition of the two groups, but also similar on many dimensions (poverty rate, unemployment rate, age composition for example).[8] Figure 3.3 shows the evolution of the two outcomes of interest, turnout and per capita state transfer over the period I analyze in disenfranchising and non-disenfranchising states. We see that overall patterns of participation are similar in both groups of states. Turnout is systematically lower in disenfranchising states which underlines the importance of the inclusion of state-year fixed effects and of basing my analysis on variations within states over time. There is almost no sign of divergence between the two groups, which shows that analyses based on state level turnout could miss important localized effects, aggregate impacts could be hidden by other factors.

The right graph in Figure 3.3 shows that if the evolutions of per capita state redistribution were consistent between disenfranchising and non-disenfranchising states, there are suggestive evidence of a divergence between the two groups, with disenfranchising states providing less public good per capita than non-disenfranchising states over time. This is consistent with the first prediction of the model. In 1997, per capita

[8]In the Appendix, results using a subset of non-disenfranchising states that are neighbors of disenfranchising states and which are very similar on all the covariates can be found. They show that results hold even when the comparison is made between disenfranchising states are compared to this second control group and it alleviates concerns about the unconfoundedness of felon disenfranchisement to other variables, as explained in Rosenbaum (1987) and Imbens (2004).

transfers were 16% higher in disenfranchising than non-disenfranchising states, while this difference was almost inexistent in 1972 (in fact disenfranchising states had higher per capita transfers even by only 3%).

3.3 Results

This section presents the results of the chapter. I show that the relationships between demographic characteristics of a county and turnout and public good provision have remained stable between the 1970s and the 1990s in disenfranchising states but have been negatively affected in disenfranchising states. I then link political participation with public good provision and estimate the impact of an extra percentage point of turnout on the level of transfers. Finally, I analyze the relationship between felon disenfranchisement and partisanship, first by showing that differences in state partisanship cannot explain my results and second by showing that the effects of felon disenfranchisement on partisan patterns is ambiguous and unclear.

3.3.1 Political Participation

In this section, I analyze the effects of felon disenfranchisement on turnout and estimate that in 1996, 450,000 votes were lost in disenfranchising states compared to the evolutions of non-disenfranchising states between 1976 and 1996.

I now analyze the relationship between the racial and social composition of a county and the turnout level in that county, and the evolutions of the relationship in disenfranchising and non-disenfranchising states as the United States moves to a system of mass incarceration. Figure 3.4 shows how patterns of political participation in a county are influenced by the racial and social composition of the county. It displays the gradient of turnout to the proportion of African Americans and poverty rate in a county. As I pointed earlier, this gradient can also be understood as the difference

in turnout levels between counties with a high proportion of African Americans and counties with a low proportion of Africans Americans divided by the difference in the proportions of African Americans. The graph is divided between disenfranchising and non-disenfranchising states and normalized to 0 for both groups in 1976 (which is the baseline year). In Figure 3.4, we can see that greater poverty rates and larger fractions of African Americans have been associated with stable rates of political participation in non-disenfranchising states, but that they have declined in disenfranchising states, as predicted by the model.

Figure 3.5 shows the difference between disenfranchising and non-disenfranchising states on how political participation is influenced by the demographic composition of a county. These graphs allow us to identify a trend in the way the difference between disenfranchising and non-disenfranchising states has grown over time, starting in the early 1980s. This means that over time, counties with a greater proportion of African Americans or a greater poverty rate have experienced a drop in political participation in disenfranchising states when compared to other states. These are the β_1 coefficients from my first estimation regression. I find a significant negative trend in the evolution of the β_1 coefficients over time. The coefficients can be found in Table 3.2, in a variety of specifications. It is remarkable that the coefficients are stable when the proportion of African Americans and the poverty rate are entered separately or simultaneously. Furthermore, the inclusion of demographic controls does not modify the results.[9] Since New England states have significantly lower rates of incarceration over the period, I have run regressions where these states have been excluded, in order to make sure that the regime of the voting rights of felons is the driving force behind my results, rather than differential penal policies. The results carry through.

[9]These controls are the unemployment rate, the log per capita income, the proportion of people above 65 and under 18, the proportion of foreign born aliens and the population density of the county. They are interacted with the "disenfranchising state" dummy to allow for a differential effect in disenfranchising and non-disenfranchising states, but the interacted terms are seldom significant.

I show that turnout in disenfranchising states in counties with high proportions of African Americans and high poverty rates has fallen more than would be predicted by the demographic composition of the county under a non-disenfranchising regime and that around 450,000 votes have been lost between 1976 and 1996 in disenfranchising states. In order to evaluate the evolution of the difference between disenfranchising and non-disenfranchising states, I focus on a 20-year period between 1976 and 1996. The results for the regression with only these two periods are shown on Table 3.4. The strict interpretation of these results is that an extra percentage point in the proportion of African Americans in a county in a disenfranchising state is associated with a 0.6 percentage point decrease in turnout between 1976 and 1996 compared to a county with similar demographic characteristics in a non-disenfranchising state. Similarly, that an extra percentage point in the poverty rate of a county in a disenfranchising state is associated with a decrease of 0.37 percentage point in turnout between 1976 and 1996 compared to a non-disenfranchising state. Another interpretation is that a standard deviation in the proportion of African Americans is associated with a 2.7% difference in turnout (which is about half a standard deviation) and a standard deviation in the poverty rate is associated with a 2.25% lower turnout (which is about a third of a standard deviation). As explained earlier, I think of the proportion of African Americans and the poverty rate in a county as good geographical markers (or predictors) of former felon communities (beyond African Americans and poor people), but I do not think of these indices as measures of the disenfranchised population. Therefore, in and of themselves, these coefficients do not provide a measure of the aggregate turnout effect of felon disenfranchisement beyond the indication that felon disenfranchisement does significantly reduce turnout in counties with a higher concentration of former felons in disenfranchising states.

When I aggregate these results over the full demographic and social profiles of the disenfranchising states, I find that turnout is reduced by around 7.5% (of the voting

eligible population, the votes lost account for around 5% of the total population) in disenfranchising states compared to other states between 1976 and 1996. This number comes from the multiplication of the aforementioned coefficients by the average proportion of African Americans (4.44%) and the average poverty rate (11.87%) in disenfranchising states in 1996. When I repeat this exercise for the other years of my analysis (using coefficients in Table 3.2) and plot them against the evolution in the rates of incarceration in disenfranchising state, I obtain Figure 3.6. We can see that the estimated number of votes lost per election (as a fraction of the voting-eligible population) follows a similar pattern to rates of incarceration in disenfranchising states. Although there is no reason that the two graphs would follow similar particular trends, the fact that they evolve in a similar direction is highly supportive of my method.

These numbers, however, cannot be used to infer the size of the disenfranchised population, or the potential turnout of former offenders (which is probably lower than the turnout of non-offenders with similar characteristics, Weaver and Lerman (2010)). And the fact that my estimate of the gap in turnout in disenfranchising states in 1996 is larger than the estimates of the size of the disenfranchised population by Uggen and Manza (2002) does not imply that their numbers are necessarily underestimated or that the turnout of former offenders would be very high. Nevertheless, it shows that by focusing only on the disenfranchised and by ignoring the potential effects of felon disenfranchisement on entire communities through a weakening of voting's normative power, scholars have underestimated the overall effects of felon disenfranchisement for life on political participation. A contribution of this chapter consists in showing an actual effect of felon disenfranchising laws on political participation through formal disenfranchisement and a form of "social" disenfranchisement.

3.3.2 Public Good Provision

In this section I show that per capita public good provision is 18% lower for the average inhabitant in disenfranchising states than in non-disenfranchising states in 1997 absent felon disenfranchisement. This represents 1.80 billion dollars a year as the average per-capita transfer is around $1,000 in non-disenfranchising states. 86% of the difference comes from differences in educational spending, this amounts to $640 per child per year in forgone investment in education in disenfranchising states.

I now turn to the analysis of state-to-county public good provision in the same framework. I first show the results from a regression of log per capita state-to-county transfers on the proportion of African Americans and the poverty rate in a county for different years, separately for disenfranchising and other states. The results are displayed in Figure 3.7, they are normalized to 0 for 1977 (the baseline year), but it is important to note that for both disenfranchising and non-disenfranchising states, greater proportions of African American and higher poverty rates in a county are associated with greater state-to-county transfers (which is supportive of my claim that counties with high proportions of African Americans or higher poverty rates will have a greater taste for redistribution). In Figure 3.7, we can see that in general this positive association between the racial composition of a county and the amount of transfers it received from the state has been decreasing over time for both disenfranchising and non-disenfranchising states. It fully supports my prediction that in disenfranchising states, redistribution in counties where the voting eligible population diminishes will decrease as the size of the disenfranchised population increases. A slightly different pattern emerges from the analysis of the gradients of state-to-county transfers to the poverty rate. It seems that the association between the levels of the transfer and the social composition of the population have evolved in opposite directions and in approximate symmetric proportions in disenfranchising and other states. In non-disenfranchising states, this association has increased over time, meaning that

non-disenfranchising states have become more socially redistributive. The opposite pattern has been observed in disenfranchising states, where the influence of the social composition of the county on redistribution has diminished over time.

I then turn my attention to the difference in the elasticity of redistribution to the racial and social composition of a county between disenfranchising and non-disenfranchising states (these are the β_1 coefficients from my first estimation equation, or the difference-in-differences estimate plotted over time). As predicted by the model, we see that per capita transfers become significantly lower in counties with a higher proportion of African Americans or a higher poverty rate in disenfranchising states when the United States shifts to a system of mass incarceration, as can be seen in Figure 3.8. The coefficients are shown in Table 3.4 in a number of specifications. As with results on turnout, they are not sensitive to the separate or simultaneous inclusion of my two indices (proportion of African Americans and poverty rate), they carry through when demographic controls are included and when the sample is limited to states outside of New England.

I then establish that transfers between disenfranchising states and counties with a high concentration of former felons has decreased compared to the evolutions that took place in non-disenfranchising states. I estimate that around 1.8 billion dollars was not spent in public good provision by disenfranchising states. I focus on a 20-year period from 1977 to 1997 during which disenfranchisement laws were remarkably stable, but in which rates of incarceration have soared. Results for this specification are shown on Table 3.5. They show that an extra percentage point in the proportion of African Americans or poverty rate in a county is associated with a decrease in per capita transfer of between 1% and 1.6%. A standard deviation in the proportion of African Americans is therefore associated with a decrease of between 5.4% and 8.6% in per capita transfers and a standard deviation in the poverty rate is associated with a 5.0% to a 7.4% decrease in per capita transfers. If I aggregate this

effect over all counties in disenfranchising states (i.e. if I use those coefficients and multiply these by the actual demographic composition of disenfranchising states), I find that on average per capita state provision of public good was approximately 18% lower in disenfranchising states than in other states in 1997.[10] In 1997, per capita state-to-county transfers were on average $1,012. This means that people living in disenfranchising states received on average $180 less per year in public goods from the state than in non-disenfranchising states, due to a disruption of the patterns of redistribution following the increase in the number of people disenfranchised. This calculation is based on the idea that absent felon disenfranchisement the evolutions of the patterns of public good provision would have been similar in disenfranchising and non-disenfranchising states. Given the size of the population in disenfranchising states, this amounts to around 1.80 billion dollars in public goods not provided to the communities that experience high poverty or have a high proportion of African Americans.

I then repeat this exercise and show how the estimated difference in transfers evolved over time and I compare it to the evolution of the rates of incarceration in Figure 3.9. We can see that even if the particular paths of the rates of incarceration and the estimated effect on transfers are somewhat different, they point to the same direction. Hence, it shows that over time, as the rates of incarceration have increased in disenfranchising states, and with them, the size of the disenfranchised population, so has the difference in transfers between counties with high and low proportions of former felons in disenfranchising states and non-disenfranchising states.

I can separate the transfers between educational and non-educational transfers and show that 86% of the effects described above come from educational transfers,

[10]This estimate of the within state effects of felon disenfranchisement is greater than the actual difference in average per capita spending between disenfranchising and non-disenfranchising states, which is 16%. For reasons explained above, this is consistent with the model. The proximity between the two numbers allows to think that felon disenfranchisement accounts for a large part of the between state difference.

with a deficit of around $640 per child on average in disenfranchising states. Educational transfers represent between two thirds and three quarters of total transfers. It seems like most of the effect described above comes from educational transfers as coefficients found in Table 3.6 are generally larger than results in Table 3.5 (even if probably not statistically significantly so, given the standard errors), and especially for the poverty rate, and coefficients in Table 3.7 being generally smaller (again probably not significantly so) and non-significant. If I use the coefficients in Table 3.6 to assess the size of the educational investment not made by disenfranchising states, I find that per capita educational transfers are on average 27% lower in disenfranchising states in 1997 (which represents around $155 per capita per year, which is 86% of the overall number of $180 found above). Overall, 1.55 billion dollars are not invested in the education of 2.43 million children (under 18) which represents a loss of approximately $640 per child, with effects concentrated in poorer counties and counties with a higher proportion of African Americans.

3.3.3 Estimating the Value of a Vote

In this section, I will link my two outcomes of interest and evaluate the effect of political participation on redistribution and show that an extra percentage point in turnout is associated with an increase of 2 to 3% in per capita transfers, something that would not be detectable in a simple OLS setting even with fixed effects.

Because felon disenfranchisement exerts a binding constraint on political participation of certain groups as seen above, it constitutes a suitable instrument to assess the value of a vote, in a manner similar to Cascio and Washington (2014). Therefore, because felon disenfranchisement provides a constraint on turnout that is not endogenous to the political process (as laws governing felon disenfranchisement have remained stable over the time period), it provides a good opportunity to assess the value of political participation.

I estimate that the policy response to differences in political participation is an increase in public spending of between 2% and 3% for every additional percentage point in turnout. The results shown in Table 3.8 show that in an OLS setting, turnout is negatively associated with within-state redistribution (a result that would contradict Martin (2003), if in a slightly different setting). It is obvious though that many characteristics of a community might make it both more likely to have a low turnout rate and to have a greater taste for the public good (which could both influence the level of redistribution as shown in the model). Therefore, I start by showing that when I control for demographic characteristics of a county, the negative association between turnout and transfers is brought to almost 0. It is remarkable that, as I postulated, the proportion of African Americans and several indicators for the wealth of a county (such as poverty rate or log per capita income) are correlated with the levels of redistribution. Similarly, the proportion of people under 18 is positively associated with redistribution, which should not come as surprise given that most transfers are educational transfers. However, simply controlling for demographic characteristics is not enough to provide an estimate of the effect of turnout on redistribution.

Therefore, I instrument participation rates with felon disenfranchisement (by using the proportion of African Americans and the poverty rate in disenfranchising states in 1997 as instruments and the other regressors of my long difference framework as controls). The results are shown in the last four columns of Table 3.8, in a variety of specifications similar to the ones shown for the results on turnout and transfers. I find that turnout is positively (and significantly) associated with the level of redistribution and therefore that voting is rewarded by public good provision. Precisely, I find that a extra 1 percentage point in turnout is associated with an increase of between 2% and 3% in the provision of public good. These estimates are around 2 to 3 times higher than those of Cascio and Washington (2014). They show that, since the late 1960s, as efforts to "get out the vote" have expanded and obstacles to voting

have arisen (with ID laws for example), participation has become more valuable in the political process. In Table 3.9, I repeat the instrumental approach with other years where fein disenfranchisement is likely to have had an impact compared to 1977 and show that the estimates are remarkably stable showing that the decline in public good provision has gone hand in hand with the decline in political participation in vulnerable communities in disenfranchising states. However, when using a comparison between 1972 and 1976 in disenfranchising and non-disenfranchising states, we find no effect of turnout, which is consistent with our analysis since no effect of felon disenfranchisement should have been felt in 1972.

3.3.4 Felon Disenfranchisement and Partisanship

In this section I explore the relationship between felon disenfranchisement and partisanship by first showing that differences in partisan patterns in disenfranchising and non-disenfranchising states cannot account for the results that I find and second, by finding that felon disenfranchisement has a null effect on partisan patterns therefore providing illustrative support for Burch (2012).

Party Influence on Redistribution

So far, in examining the link between the racial and social composition of a county and the patterns of political participation and redistribution within states, I have not controlled for the partisanship of the state. It is likely that state politics will play an important role in the provision of public goods between different communities. Different parties might have different motivations when it comes to redistribution, either ideologically (Democrats could be more "pro-poor" for example) or for electoral reasons (different parties will have different core constituents which could influence their patterns of redistribution, Dixit and Londregan, 1996).

In this section, I add better controls for the potential particular political circum-

stances that might impact patterns of redistribution in both disenfranchising and non-disenfranchising states. I interact an indicator for whether the governor of a state is a Democrat with the proportion of African Americans and the poverty rate in a county, and since the governor is not the only political figure in state politics, I provide specifications where an indicator for "unified Democratic government" (when the governor, the lower house and the upper house in a state are all Democratic) with the social and racial composition of the county.[11] I allow the effects of a Democratic governor or a "unified Democratic state government" to differ in 1977 and 1997.

Even though the effect of the partisanship of the state should primarily impact patterns of public good provision, I still control for it in my analysis of turnout.[12] The results are shown in Table 3.10. First and foremost, the effects of felon disenfranchisement on turnout I previously uncovered are robust to the inclusion of state political controls. The coefficients on the poverty rate are virtually unchanged and the coefficients on the proportion of African Americans are slightly altered (smaller when I control for the party of the governor, larger when I control for a "unified Democratic government"). This shows that the effects of felon disenfranchisement on political participation are not an artifact of different partisan patterns in disenfranchising and non-disenfranchising states.

Similarly, the effects of felon disenfranchisement on patterns of public good provision are robust to several controls for state partisanship as shown in Table 3.11. In a few specifications, the coefficients are slightly smaller but, given the width of the confidence intervals, probably non-significantly so. It is remarkable that Democratic governors do seem to be "pro-poor" in 1977 (as can be seen from the positive and

[11]The data for partisanship at the state level were obtained from the University of Kentucky Center for Poverty research and several issues of The Book of the States. Since Maine has in independent governor in 1977, I exclude it in both 1977 and 1997. As Nebraska has a unicameral non-partisan legislature, I code it as having a "unified Democratic government" when its governor is a Democrat.

[12]Since my outcome of interest is presidential turnout at the county-level, I believe the effects of the partisanship of a state might be even weaker. Still, a Democratic governor could organize to get the votes out for a Democratic presidential candidate.

significant coefficient on the interaction of a Democratic governor and the poverty rate) even if this effect is entirely gone in 1997. The effects of a "unified Democratic government" are almost always insignificantly different from 0, except for a change in the way funds are allocated to the poor between 1977 and 1997. Furthermore, the inclusion of controls for state partisanship does not change our estimates of the elasticity of public good provision on turnout as can be seen in Table 3.12.

It is important to underline the significance of these results and to summarize the claims this chapter made about the influence of a potentially different antipathy towards African Americans and the poor in disenfranchising and non-disenfranchising states. If state partisanship serves as a proxy for measuring the animus towards African Americans and/or the poor then controlling for state partisanship allows me to make sure that I do not capture the effects of a different hostility towards African Americans or the poor. Similarly, our discussion of the determinants of the state of felon disenfranchisement in the late 1970s has shown that partisanship did not play a significant role and that social and racial measures played ambiguous roles. Finally, if one thinks of policing and penal practices as another indication of racial or social animus, I have shown that disenfranchising and non-disenfranchising states were strikingly similar with respect to incarceration rates (as can be seen in Figure 3.1) over the entire period and that, if anything, non-disenfranchising states had experienced a greater surge in incarceration rates than non-disenfranchising states.

Felon Disenfranchisement and Partisan Patterns

In this section, I investigate the potential consequences of felon disenfranchisement on partisan patterns and show that, in line with observations by Burch (2011), it is likely that partisan patterns are not disrupted by felon disenfranchisement.

Based on the average demographic characteristics of former felons and the National Election Study, Uggen and Manza (2002) predict that former felons would vote

overwhelmingly in favor of Democratic presidential candidates between 1972 and 2000 (between 67% and 85%). But Burch (2011) argues that if it is correct that black former felons are often registered Democrats, their white counterparts are almost evenly divided between "independents", Republicans and Democrats. Therefore, I use the same methodology to analyze partisan patterns in disenfranchising states that I use for my analysis of political participation and redistribution. The presidential Democratic vote share is now the outcome of interest.[13]

The association between the racial and social composition of a county and the share of Democratic votes cast in disenfranchising and non-disenfranchising states is shown in Figure 3.10. No clear pattern emerges over time. It seems that in general the association between the proportion of African Americans and the Democratic vote share has increased over time. But in terms of the difference between states on the basis of felon disenfranchisement regimes, there does not seem to be a consistent pattern. From results shown in Figure 3.11, it seems that felon disenfranchisement has not been responsible for a decrease in the Democratic vote share, as would be in line with Uggen and Manza (2002). For poverty, the patterns are even more troubled. The association between the poverty rate and the Democratic vote share in a county is unstable and does not follow a consistent pattern. If anything, it has decreased over time. This could suggest that poor white voters have indeed turned their back on the Democratic party (as suggested anecdotally in Frank (2007)), but the evidence is too sparse to be fully understood (and the analysis of partisan patterns has suffered from the "ecological fallacy", if at a different level of aggregation, Gelman et al. (2009)). In any case, the difference between disenfranchising and non-disenfranchising states has not evolved significantly over time. Therefore, these results seem to support the more nuanced view of Burch (2011) that it is unclear which party "benefits" from

[13]I calculate it as the number of votes cast for the Democratic candidate divided by the number of votes cast for either the Democratic or the Republican presidential candidates, but by calculating as the share of votes cast for the Democratic candidate out of all the votes cast, the results are unchanged.

felon disenfranchisement (or which party would benefit from its repeal). I believe this finding is important as it blurs the party lines on felon disenfranchisement and contradicts the view sometimes held by politicians themselves.

3.4 Conclusion

This chapter shows that felon disenfranchisement is a "huge deal" not only in the African American community but in all communities that witness a high proportion of former felons and the communities they live with. I have documented the effects of felon disenfranchisement for life on political participation through a combination of formal disenfranchisement and possible "social" disenfranchisement with low levels of engagement in communities with a high proportion of disenfranchised citizens. These effects are large in a country where rates of incarceration have soared in the last decades.

Furthermore, by using the disproportionate consequences of felon disenfranchisement on certain communities as the United States shifted to a system of mass incarceration, I have been able to show that politicians respond significantly to differences in political participation and that public good provision patterns follow turnout patterns. An extra percentage point in turnout translates into a 2% to 3% increase in per capita spending. These effects are 2 to 3 times higher than the ones found by Cascio and Washington (2014) in their studies of literacy tests in the 1960s, showing that the value of political participation has significantly increased over the past decades. These effects are not explained by state partisanship and felon disenfranchisement does not seem to alter the balance between Republicans and Democrats, as the disenfranchised group is less politically homogenous than expected.

Finally, the results from this chapter call to attention the potential formation of vicious circles. If communities that are more vulnerable receive less public spending

and especially less educational investment, it is likely that over the next generation felon disenfranchisement will foster the creation of vicious circles of low education, crime and poverty.

	Disenfranchising		**Other**	
	1970	**2000**	**1970**	**2000**
Counties	267		1362	
Proportion of African Americans	2.99	4.71	8.07	9.40
	(4.4)	(5.6)	(8.6)	(10.0)
Poverty Rate	13.0	11.4	10.3	11.5
	(6.3)	(4.8)	(4.0)	(5.0)
Unemployment Rate	3.88	5.26	4.55	5.95
	(1.6)	(2.1)	(1.5)	(2.2)
Average Per Capita Income	17,686	19,953	20,241	22,543
(2000 $)	(2,934)	(3,410)	(3,405)	(4,980)
Proportion under 18	35.6	26.0	34.0	25.7
	(3.6)	(2.5)	(3.2)	(2.7)
Proportion above 65	10.6	12.9	10.0	12.5
	(3.9)	(3.6)	(2.4)	(2.8)
Proportion of Non-citizens	0.64	4.53	2.13	7.42
	(0.68)	(3.8)	(2.0)	(7.0)

Notes: All statistics are weighted by the county population. Standard deviations are in parenthesis. The disenfranchising states are: DE, IA, NE, NM, NV and WY. The non-disenfranchising states are: CA, CO, ID, IL, IN, KS, MA, ME, MI, MN, MO, MT, ND, NH, NJ, NY, OH, OR, PA, RI, SD, UT, VT and WI.

Table 3.1: Summary Statistics

		Dependent Variable: Turnout			
African Americans in	1980	-0.1377***		-0.1750***	-0.0930
Disenfranchising states		(0.053)		(0.060)	(0.059)
	1984	-0.2387***		-0.2897***	-0.3178***
		(0.092)		(0.096)	(0.106)
	1988	-0.2246*		-0.2893**	-0.3673***
		(0.120)		(0.125)	(0.132)
	1992	0.1073		-0.0135	-0.1358
		(0.171)		(0.173)	(0.168)
	1996	-0.4156**		-0.4949***	-0.6109***
		(0.192)		(0.187)	(0.186)
	2000	-0.1465		-0.2639	-0.3490*
		(0.213)		(0.211)	(0.203)
Poverty rate in	1980		-0.0360	0.0384	0.0116
Disenfranchising states			(0.070)	(0.054)	(0.056)
	1984		-0.4279***	-0.1856**	-0.2361***
			(0.096)	(0.082)	(0.088)
	1988		-0.3816***	-0.2029	-0.2119
			(0.139)	(0.130)	(0.141)
	1992		-0.5147***	-0.3760**	-0.3896**
			(0.159)	(0.150)	(0.163)
	1996		-0.5799***	-0.4743***	-0.5317***
			(0.187)	(0.164)	(0.190)
	2000		-0.7476***	-0.5063***	-0.6081***
			(0.201)	(0.180)	(0.208)
R-squared		0.552	0.644	0.652	0.735
Observations		14660	14660	14660	14660
Std error		cluster	cluster	cluster	cluster
State-Year FE		Y	Y	Y	Y
Demographic Controls		N	N	N	Y

Table 3.2: Results on Turnout, in comparison to 1976 (baseline year)

	Dependent Variable: Turnout			
African Americans in	-0.4156**		-0.4949***	-0.6184***
Disenfranchising states in 1996	(0.192)		(0.187)	(0.178)
Poverty Rate in		-0.5799***	-0.4743***	-0.3651**
Disenfranchising states in 1996		(0.187)	(0.164)	(0.177)
African Americans	-0.2291**		-0.0855	-0.0750
	(0.094)		(0.069)	(0.073)
Poverty Rate		-0.7259***	-0.6275***	0.0733
		(0.179)	(0.134)	(0.107)
Unemployment Rate				-0.0728
				(0.130)
Log per capita income				0.2281***
				(0.022)
Proportion above 65				0.8944***
				(0.092)
Proportion under 18				0.1983**
				(0.090)
Foreign born aliens				-0.0940
				(0.098)
Density				-4.68e-06***
				(9.68e-07)
R-squared	0.528	0.610	0.616	0.735
Observations	3258	3258	3258	3258
Std error	cluster	cluster	cluster	cluster
State-Year FE	Y	Y	Y	Y
Demographic controls	N	N	N	Y

Table 3.3: Results on Turnout, Long difference (1976-1996)

		Dependent Variable: Log Per-Capita Spending			
African Americans in	1982	-0.9702**		-1.1817***	-1.0273**
Disenfranchising states		(0.453)		(0.418)	(0.436)
	1987	-1.6640**		-1.7003***	-1.2374*
		(0.661)		(0.647)	(0.653)
	1992	-2.0021***		-1.9228***	-1.2544***
		(0.426)		(0.435)	(0.407)
	1997	-1.6881***		-1.6461***	-0.9496**
		(0.410)		(0.401)	(0.372)
	2002	-2.0359***		-2.1242***	-1.4640****
		(0.491)		(0.470)	(0.446)
Poverty rate in	1982		-0.6365*	-0.9870***	-0.6739*
Disenfranchising states			(0.385)	(0.346)	(0.344)
	1987		-0.6760	-1.4377***	-0.9697**
			(0.462)	(0.440)	(0.439)
	1992		-1.1578**	-1.7217***	-1.2591**
			(0.545)	(0.549)	(0.561)
	1997		-0.4547	-1.5773***	-1.1907**
			(0.547)	(0.523)	(0.538)
	2002		0.2255	-0.7888	-0.4028
			(0.566)	(0.517)	(0.473)
R-squared		0.945	0.959	0.962	0.966
Observations		13032	13032	13032	13031
Std error		cluster	cluster	cluster	cluster
State-Year FE		Y	Y	Y	Y
Demographic Controls		N	N	N	Y

Table 3.4: Results on Public Good Provision, in comparison to 1977 (baseline year)

	Dependent Variable: Log Per-Capita Spending			
African Americans in	-1.6881***		-1.6461***	-1.0233**
Disenfranchising states in 1997	(0.410)		(0.401)	(0.421)
Poverty rate		-0.4547	-1.5773***	-1.1350*
Disenfranchising states in 1997		(0.547)	(0.522)	(0.583)
African Americans	1.7876***		1.1096***	1.3340***
	(0.180)		(0.112)	(0.166)
Poverty rate		4.1513***	2.8100***	0.6861*
		(0.389)	(0.312)	(0.392)
Unemployment rate				0.1037
				(0.423)
Log per capita income				-0.4601***
				(0.117)
Proportion above 65				0.5371
				(0.419)
Proportion under 18				1.8007***
				(0.486)
Non-citizens				-0.1039
				(0.505)
Density				1.24e-05***
				(3.03e-06)
R-squared	0.923	0.940	0.945	0.953
Observations	3258	3258	3258	3258
Std error	cluster	cluster	cluster	cluster
State-Year FE	Y	Y	Y	Y
Demographic controls	N	N	N	Y

Table 3.5: Results on Public Good Provision, Long difference 1977-1997

	Log Per-Capita Spending on Education			
African Americans in	-2.6233***		-2.1603***	-1.1986
Disenfranchising states in 1997	(0.656)		(0.668)	(0.779)
Poverty rate in		-1.0536	-2.4100***	-2.2913***
Disenfranchising states in 1997		(0.773)	(0.747)	(0.800)
Proportion under 18				3.4418***
				(0.372)
R-squared	0.873	0.888	0.891	0.941
Observations	3252	3252	3252	3252
Std error	cluster	cluster	cluster	cluster
State-Year FE	Y	Y	Y	Y
Demographic controls	N	N	N	Y

Table 3.6: Results on Public Good Provision, Educational transfers, Long difference (1977-1997)

	Log Per-Capita Non-educational Spending			
African Americans in	-0.1565		-0.2266	-0.2953
Disenfranchising states in 1997	(1.515)		(1.570)	(1.605)
Poverty rate in		-0.3141	-0.9091	0.2664
Disenfranchising states in 1997		(0.870)	(0.899)	(0.924)
Proportion under 18				-0.3841
				(0.713)
R-squared	0.904	0.892	0.916	0.928
Observations	3258	3258	3258	3258
Std error	cluster	cluster	cluster	cluster
State-Year FE	Y	Y	Y	Y
Demographic controls	N	N	N	Y

Table 3.7: Results on Public Good Provision, Non-educational transfers, Long difference (1977-1997)

	OLS		2SLS	
Turnout	-2.2081***	-0.3001	3.3441**	2.0951**
	(0.300)	(0.193)	(1.336)	(0.924)
African American		0.9981***	1.3396***	1.4888***
		(0.132)	(0.287)	(0.259)
Poverty Rate		0.8586**	4.9614***	0.5791
		(0.415)	(0.801)	(0.510)
Unemployment rate		-0.0411		0.1999
		(0.441)		(0.493)
Log per capita income		-0.4612***		-0.9355***
		(0.117)		(0.231)
Proportion above 65		0.8059**		-1.3134
		(0.332)		(0.869)
Proportion under 18		1.6392***		1.4062**
		(0.430)		(0.557)
Non-citizens		-0.1494		0.0892
		(0.557)		(0.492)
Density		1.22e-05***		2.21e-05***
		(3.26e-06)		(4.943-06)
R-squared	0.915	0.951		
Root MSE			0.254	0.192
Observations	3258	3258	3258	3258
State-Year FE	Y	Y	Y	Y
Demographic controls	N	Y	N	Y

Table 3.8: Estimating the Value of a Vote: Impact of Turnout on Transfers

	1972	**1992**	**1997**	**2002**
Turnout	-0.8464	3.8839**	3.3441**	2.3341*
	(3.643)	(1.889)	(1.336)	(1.286)
Proportion of African Americans	1.0545***	1.3839***	1.3396***	1.2816***
	(0.269)	(0.333)	(0.287)	(0.230)
Poverty rate	2.2564	5.3017***	4.9614***	4.2854***
	(2.283)	(1.002)	(0.801)	(0.666)
Root MSE	0.172	0.281	0.254	0.223
Observations	3257	3258	3258	3258
State-Year FE	Y	Y	Y	Y
Demographic controls	N	N	N	N

Table 3.9: Impact of Turnout on Transfers, 2SLS, Different Years

		Dependent Variable: Turnout			
Disenfranchising States	African Americans in 1996	-0.3653*	-0.9117***	-0.4292**	-0.6057**
		(0.204)	(0.341)	(0.197)	(0.243)
	Poverty rate in 1996	-0.5576***	-0.4922***	-0.4020**	-0.3377*
		(0.167)	(0.172)	(0.176)	(0.179)
Democratic Governor	African Americans	0.1157		0.1990*	
		(0.114)		(0.115)	
	Poverty rate	-0.4470**		-0.1353	
		(0.213)		(0.169)	
	African Americans in 1996	-0.2744*		-0.3886**	
		(0.148)		(0.159)	
	Poverty rate in 1996	0.7205***		-0.0068	
		(0.253)		(0.183)	
Unified Democratic Government	African Americans		0.3554***		0.3473***
			(0.093)		(0.097)
	Poverty Rate		0.1241		0.0987
			(0.194)		(0.156)
	African Americans in 1996		0.8632***		-0.1289
			(0.333)		(0.263)
	Poverty rate in 1996		0.5734		-0.2041
			(0.364)		(0.235)
R-squared		0.618	0.637	0.738	0.752
Observations		3226	3226	3226	3226
Std error		cluster	cluster	cluster	cluster
State-Year FE		Y	Y	Y	Y
Demographic Controls		N	N	Y	Y

Table 3.10: Impact of Partisanship on Turnout, 1976-1996

		Dependent Variable: Log Per-Capita Spending			
Disenfranchising States	African Americans in 1997	-1.7335***	-1.7826***	-1.0986**	-1.1268**
		(0.469)	(0.451)	(0.488)	(0.469)
	Poverty rate in 1997	-1.4636***	-1.2157**	-1.0611*	-0.7883
		(0.535)	(0.619)	(0.582)	(0.669)
Democratic Governor	African Americans	-0.0504		-0.0166	
		(0.206)		(0.269)	
	Poverty rate	1.6234***		1.0574**	
		(0.471)		(0.443)	
	African Americans in 1997	0.2820		0.1967	
		(0.342)		(0.423)	
	Poverty rate in 1997	-3.1892***		-1.8705***	
		(0.782)		(0.711)	
Unified Democratic Government	African Americans		-0.1820		-0.0995
			(0.236)		(0.294)
	Poverty rate		0.7760		0.7924
			(0.651)		(0.648)
	African Americans in 1997		0.2770		0.1535
			(0.370)		(0.436)
	Poverty rate in 1997		-2.4551***		-1.9770**
			(0.846)		(0.818)
R-squared		0.946	0.946	0.953	0.953
Observations		3226	3226	3226	3226
Std error		cluster	cluster	cluster	cluster
State-Year FE		Y	Y	Y	Y
Demographic Controls		N	N	Y	Y

Table 3.11: Impact of Partisanship on Public Good Provision, 1977-1997

	Democratic Governor		**Unified Democratic Government**	
Turnout	3.0167**	2.2679**	3.2868**	2.1790*
	(1.217)	(1.105)	(1.623)	(1.185)
African Americans	1.5051***	1.7340***	1.9373***	1.8936***
	(0.214)	(0.246)	(0.456)	(0.373)
Poverty rate	2.6978***	-0.2071	4.8510***	0.4881
	(0.773)	(0.673)	(1.404)	(0.561)
Root MSE	0.241	0.194	0.249	0.192
Observations	3226	3226	3226	3226
Demographic Controls	N	Y	N	Y

Table 3.12: Impact of Turnout on Transfers, 2SLS, Controlling for Political Variables

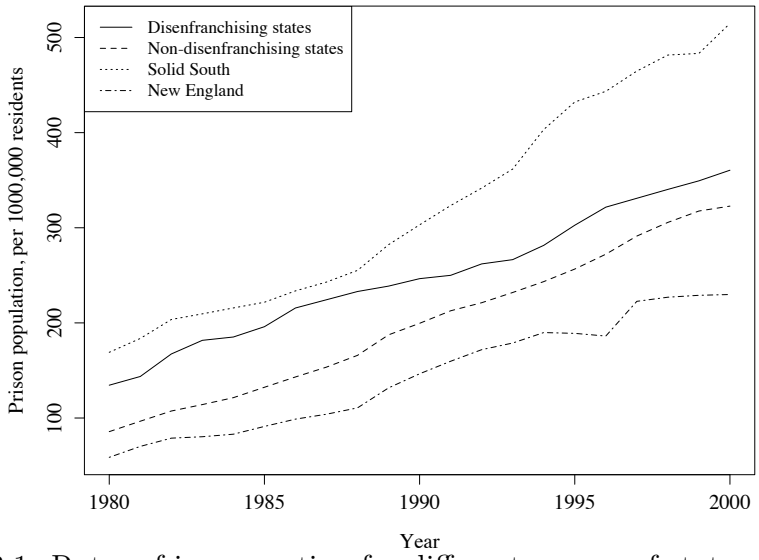


Figure 3.1: Rates of incarceration for different groups of states, 1980-2000

The figure shows average rates of incarceration or states in different groups: disenfranchising states (DE, IA, NE, NM, NV and WY), non-disenfranchising states, with the exclusion of New England (CA, ID, IL, IN, KS, MI, MN, MO, MT, NJ, NY, OH, OR, PA, SD, UT and WI), New England states (CT, ME, MA, NH, RI and VT) and states in the Solid South (AL, AR, FL, GA,KY, LA, MO, NC, OK, SC, TN, TX, VA and WV).

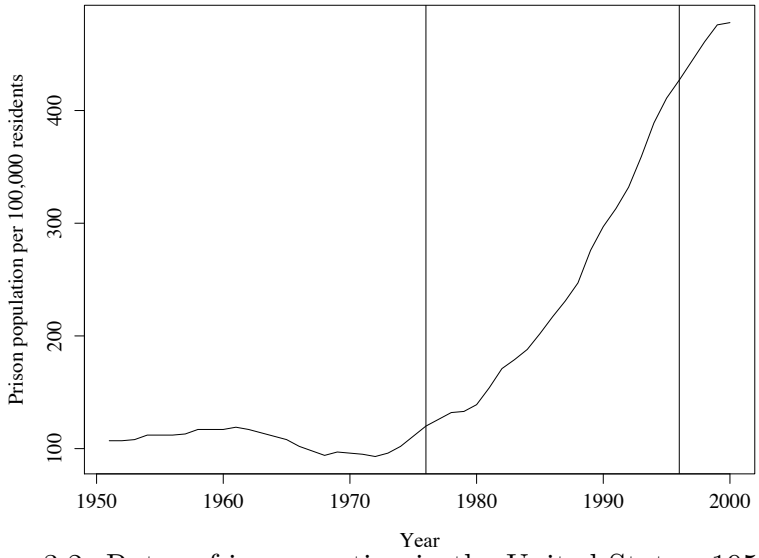
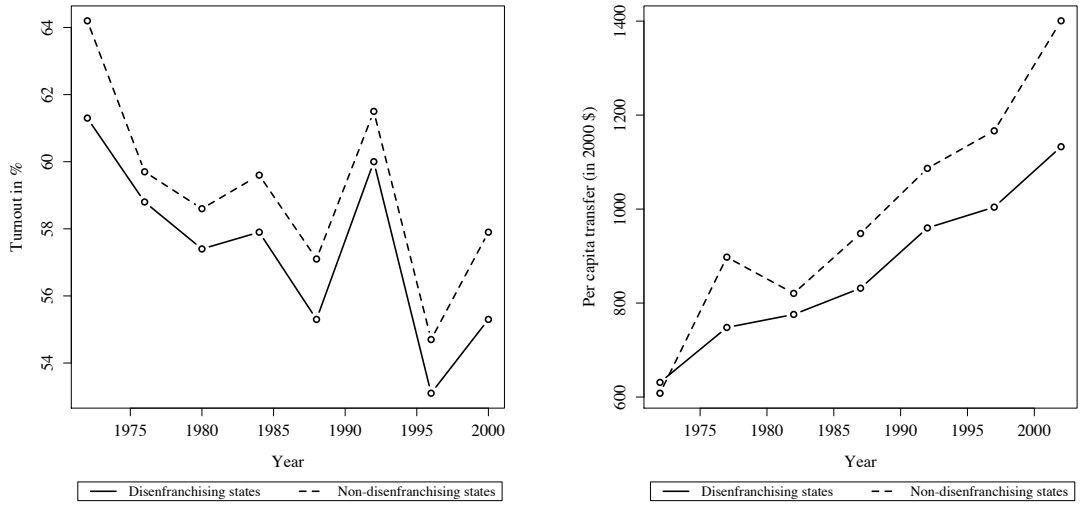


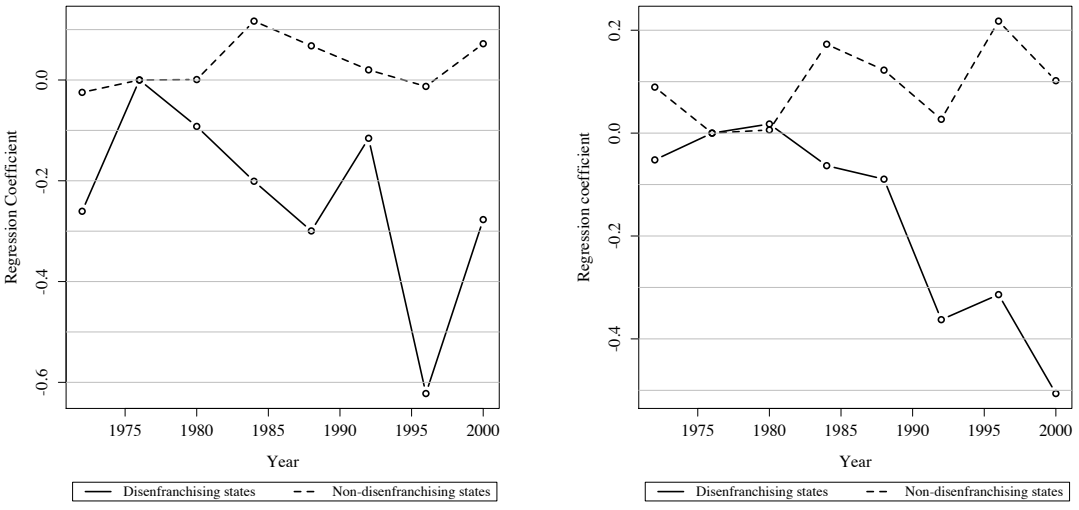
Figure 3.2: Rates of incarceration in the United States, 1950-2000



(a) Turnout (b) Public Good Provision

Figure 3.3: Turnout and Public Good Provision, 1972-2002

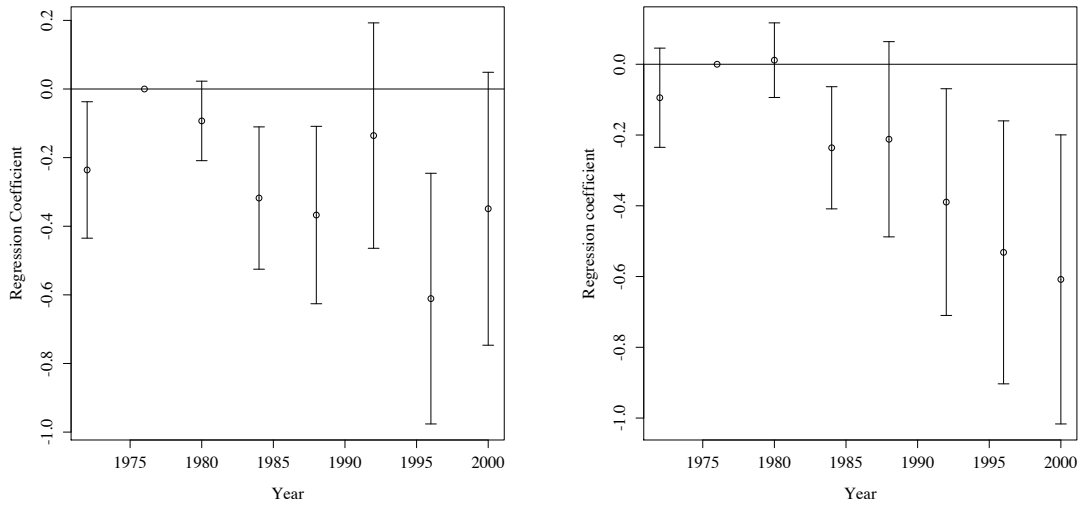
Population weighted averages of turnout (as a share of the voting eligible population) and per capita state-to-county transfers. The disenfranchising states are: DE, IA, NE, NM, NV and WY. The non-disenfranchising states are: CA, CO, ID, IL, IN, KS, MA, ME, MI, MN, MO, MT, ND, NH, NJ, NY, OH, OR, PA, RI, SD, UT, VT and WI.



(a) African American (b) Poverty

Figure 3.4: Gradients in Presidential Turnout, in Disenfranchising and Non-disenfranchising states

Gradients of county-level turnout with proportion of African Americans and Poverty rate, respectively, separately for disenfranchising and non-disenfranchising states.



(a) African American (b) Poverty

Figure 3.5: Difference-in-differences in Presidential Turnout

These are results form equation (1) with turnout as an outcome. They represent the difference between counties with high and low proportions of African Americans and poor people in disenfranchising and non-disenfranchising states. The coefficients used to construct these graphs can be found in Table 2.

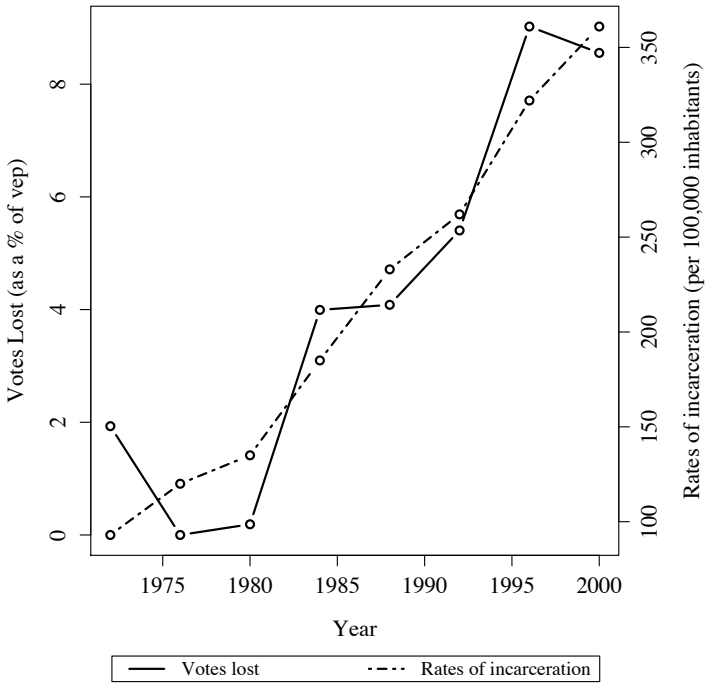
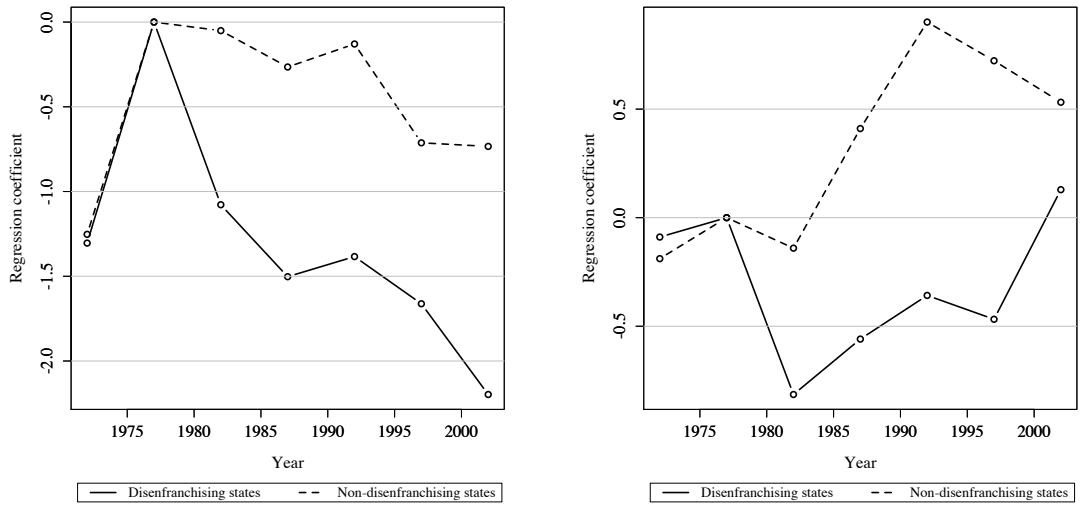


Figure 3.6: Evolution of Estimated Votes Lost and Rates of Incarceration

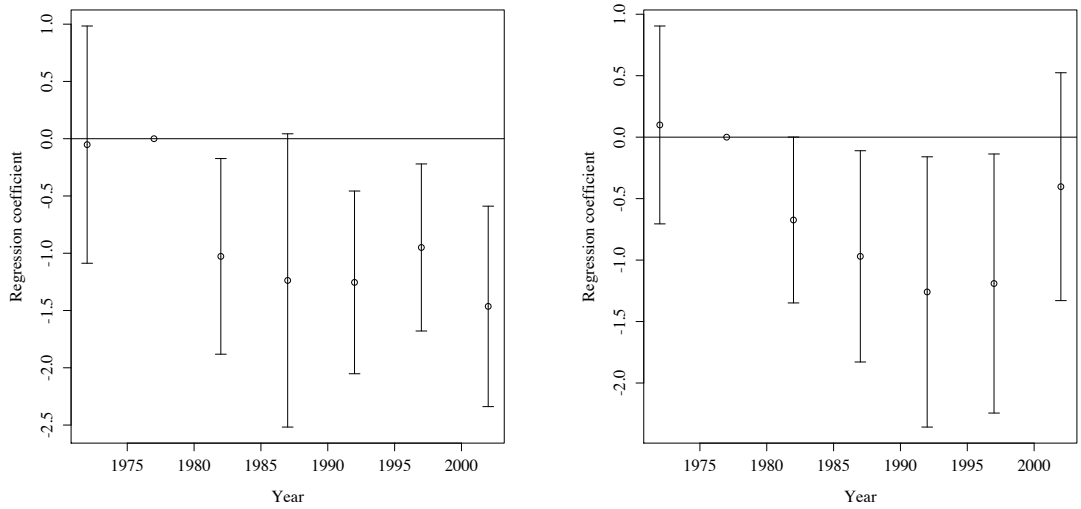
The estimated effects come from the multiplication of the coefficients in Table 2 with the average proportion of African Americans and average poverty rate in disenfranchising states (DE, IA, NE, NM, NV and WY).



(a) African American (b) Poverty

Figure 3.7: Gradients in Public Good Provision, in Disenfranchising and Non-disenfranchising states

Gradients of county-level per capita transfers with proportion of African Americans and Poverty rate, respectively, separately for disenfranchising and non-disenfranchising states.



(a) African American (b) Poverty

Figure 3.8: Difference-in-Differences in Public Good Provision

These are results form equation (1) with log per capita transfers as an outcome. They represent the difference between counties with high and low proportions of African Americans and poor people in disenfranchising and non-disenfranchising states. The coefficients used to construct these graphs can be found in Table 4. The disenfranchising states are: DE, IA, NE, NM, NV and WY. The non-disenfranchising states are: CA, CO, ID, IL, IN, KS, MA, ME, MI, MN, MO, MT, ND, NH, NJ, NY, OH, OR, PA, RI, SD, UT, VT and WI.

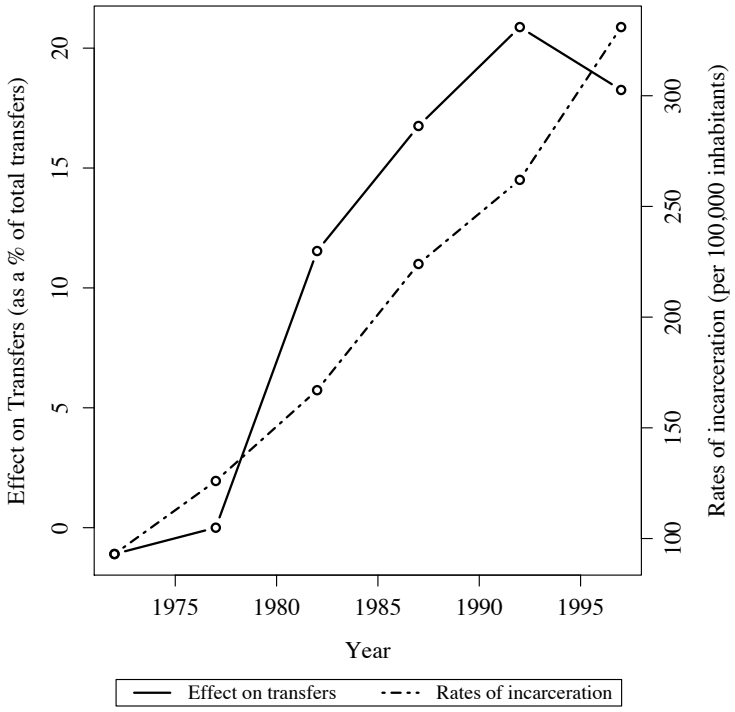
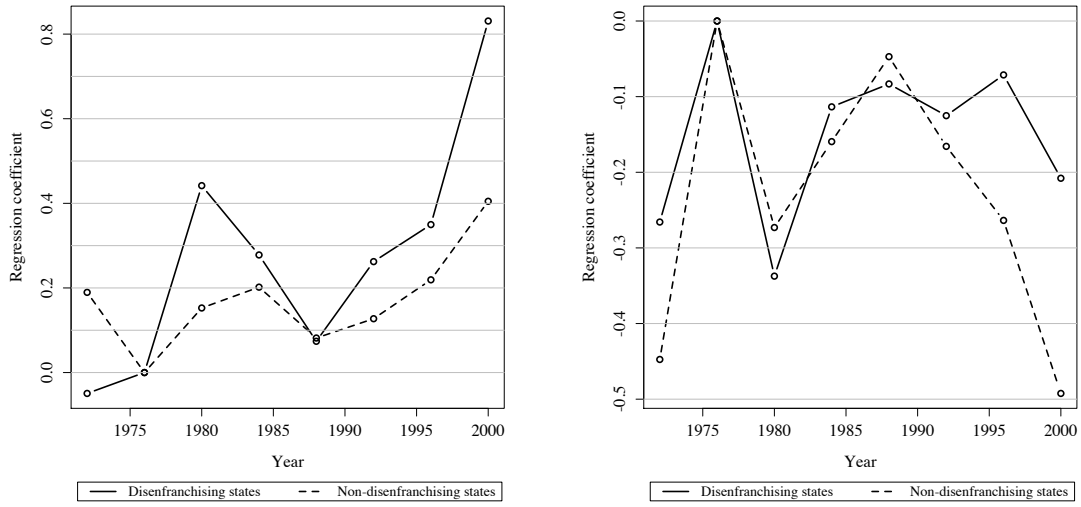


Figure 3.9: Evolution of Estimated Effect on Transfers and Rates of Incarceration

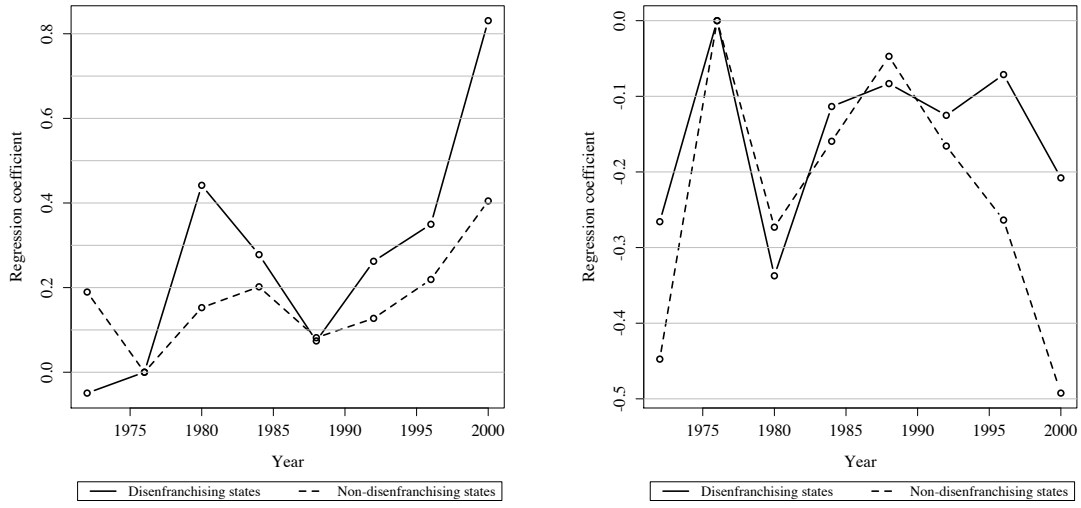
The estimated effects come from the multiplication of the coefficients in Table 4 with the average proportion of African Americans and average poverty rate in disenfranchising states (DE, IA, NE, NM, NV and WY).



(a) African American (b) Poverty

Figure 3.10: Gradients in Democratic Vote Share, in Disenfranchising and Non-disenfranchising states

Gradients of Democratic vote share with proportion of African Americans and Poverty rate, respectively, separately for disenfranchising and non-disenfranchising states.



(a) African American (b) Poverty

Figure 3.11: Differences in Gradients in Democratic Vote Share

These are results form equation (1) with Democratic vote share as an outcome. They represent the difference between counties with high and low proportions of African Americans and poor people in disenfranchising and non-disenfranchising states. The coefficients used to construct these graphs can be found in Table 4.

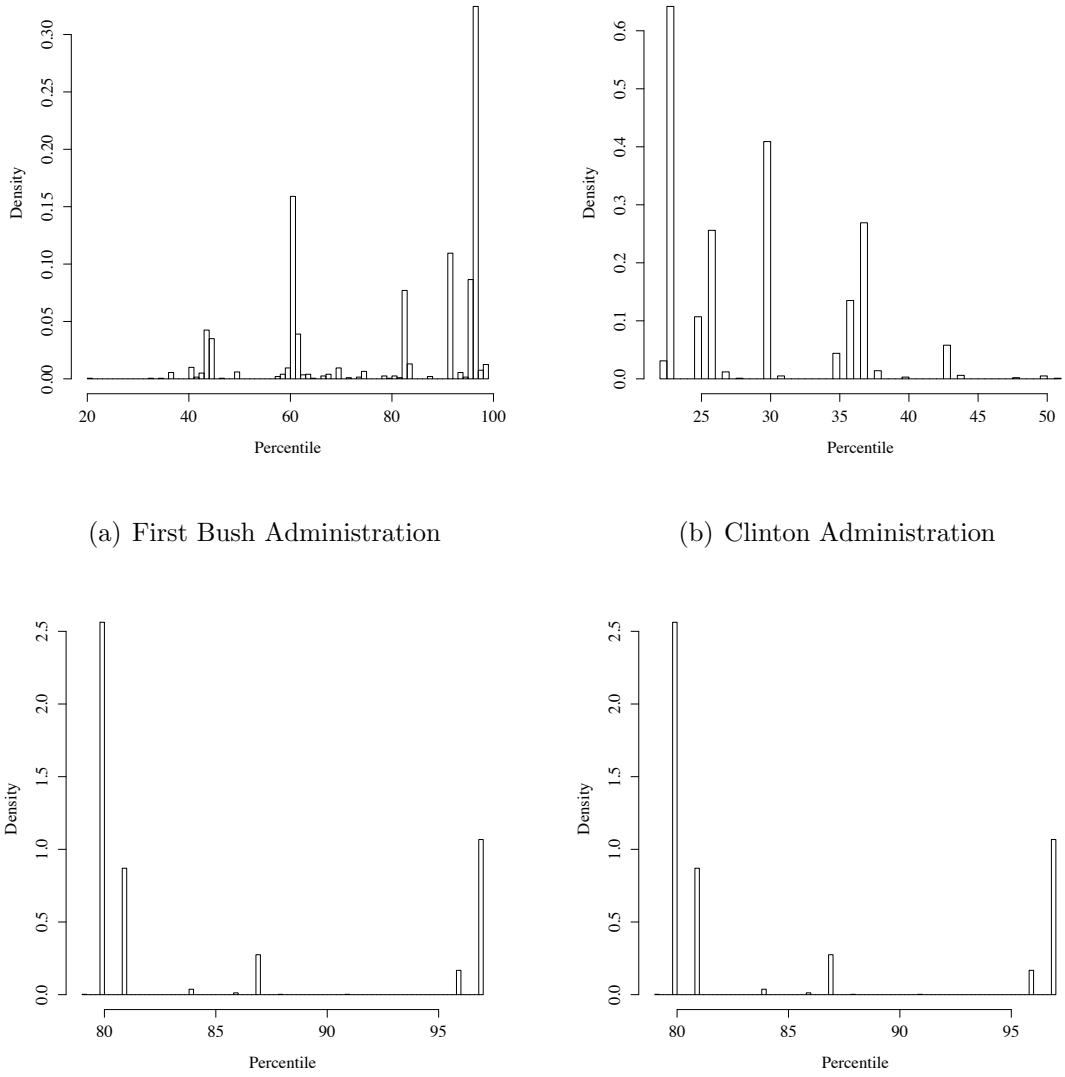
Appendix A

Appendix to Chapter 1

A.1 Additional Results

A.1.1 Distribution of the Estimator

The following graphs show the estimated distribution of the estimator of the pivotal percentile ($p = \arg\min_i Corr\left(\tau_t, \frac{y_{it-1}}{\overline{y_{t-1}}}\right)$) as calculated by the bootstrap method. The results are very similar to the ones obtained with the smoothed graphs analyzed in the chapter. For the first Bush administration, the estimator is relatively imprecise, reflecting the noisy graph, for the Clinton administration, the estimate is mainly concentrated between $p = 20$ and $p = 38$. The results are very precise for the second Bush administration and the entire period.



(a) First Bush Administration

(b) Clinton Administration

(c) Second Bush Administration

(d) Entire Period, 1987-2006

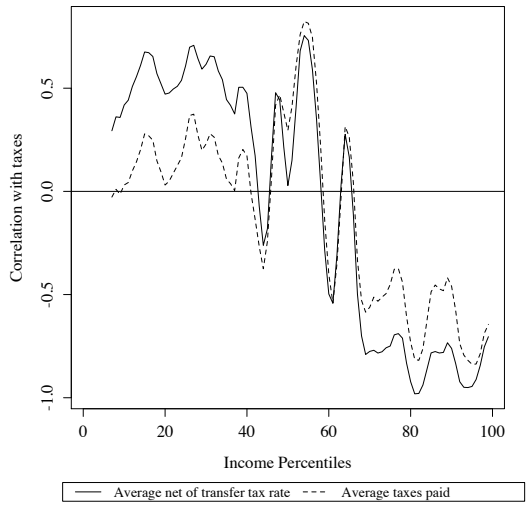
Figure A.1: Distribution of the Estimator of the Pivotal Percentile.

Notes: These graphs show the distribution of the identified pivotal percentile through the iterations of the bootstrap. They offer an alternative graphic interpretation of the results shown earlier.

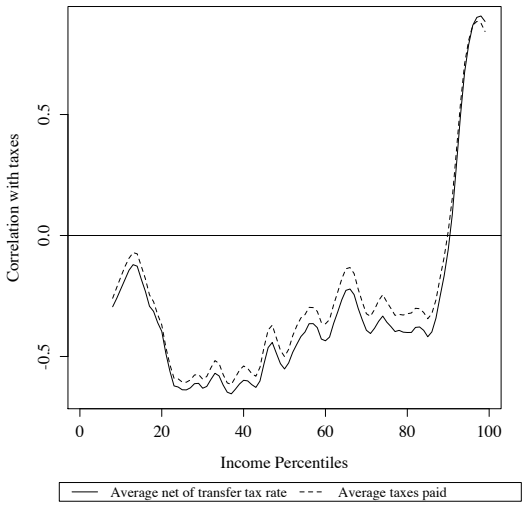
A.1.2 Robustness Checks

First, I show the graphs that result form the use of an average net of transfer tax rate and how they compare to the previously computed figures.

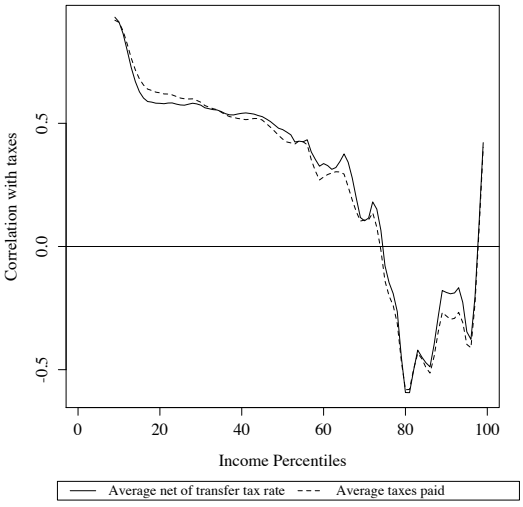
Another robustness calculation consist of running regressions to account for the potential effect of growth on the results.



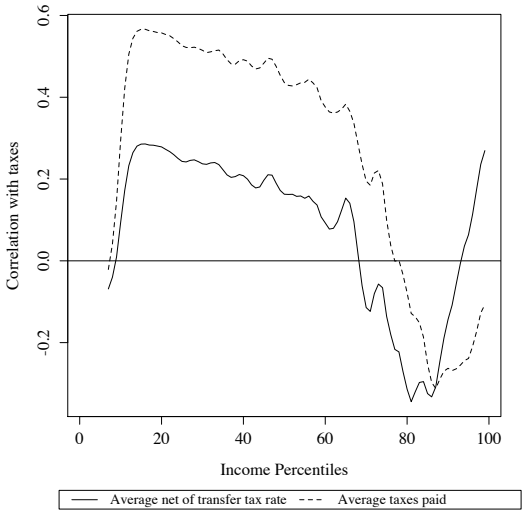
(a) First Bush Administration



(b) Clinton Administration



(c) Second Bush Administration



(d) Entire Period, 1987-2006

Figure A.2: Results using net of transfer tax rate.

Notes: These graphs display the correlations between net of transfer tax rates and every percentile of the income distribution, as well as the original graphs for comparison. Graphs are smoothed.

I also present the results obtained by using the 1976 tax sample to calculate the average tax rate and comparisons with the original graphs.

Finally, a last check concerns other possible issues of endogeneity. I therefore present the results using 2-year averages.

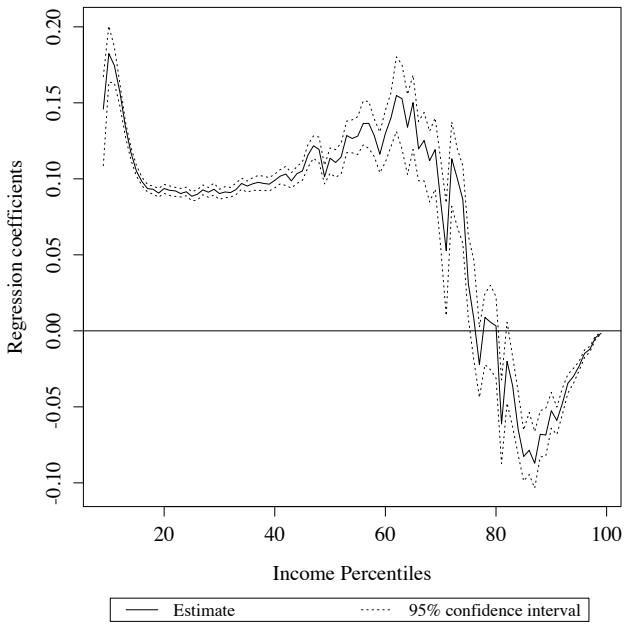
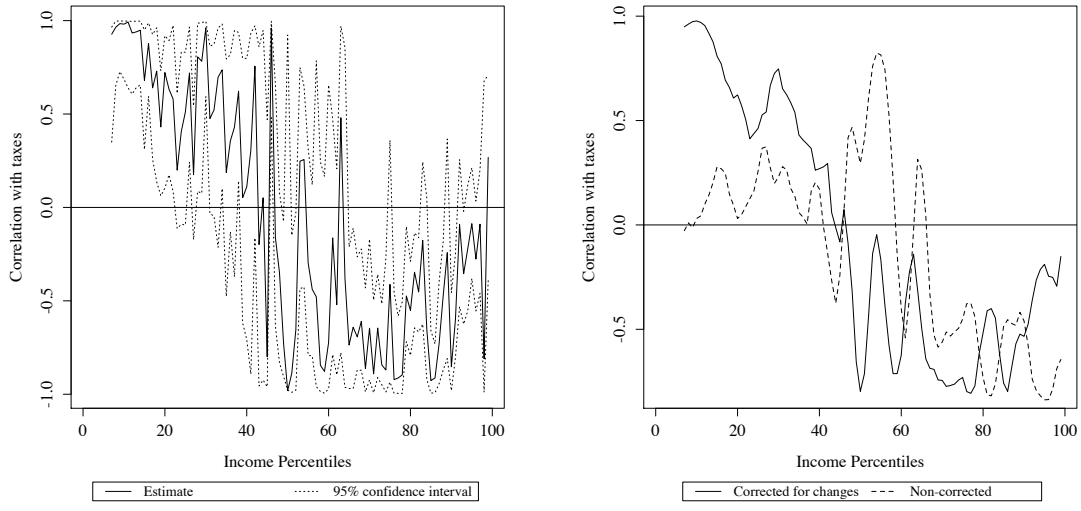


Figure A.3: Correction for Growth, United States, 1987-2007.

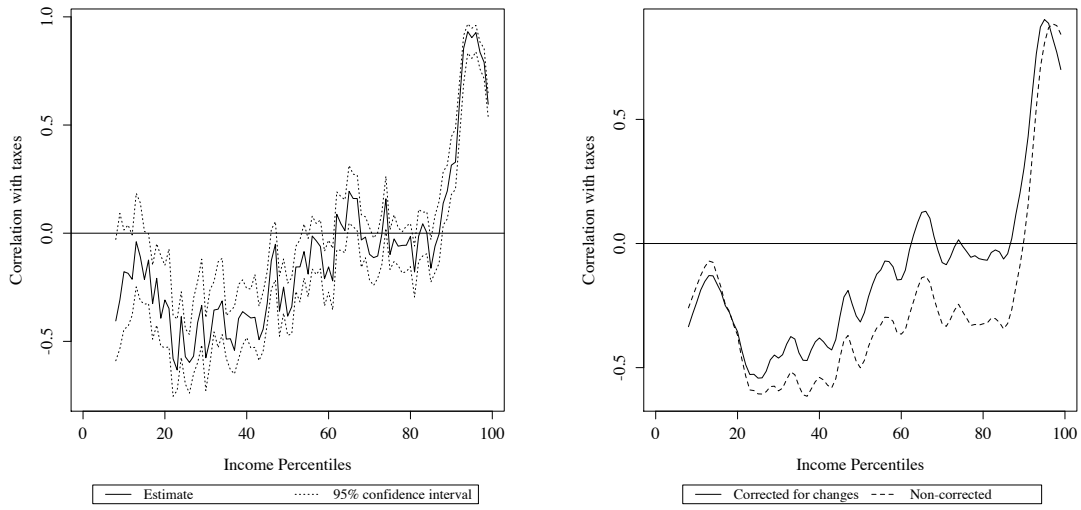
Notes: This graph displays coefficients obtained from a regression of taxes on changes in every percentile of the income distribution and yearly growth. This will help to correct for the uniform changes in the distribution of income, since growth will have an indirect impact on tax rates because of the progressivity of the tax code.



(a) 1976 Sample (b) Comparison

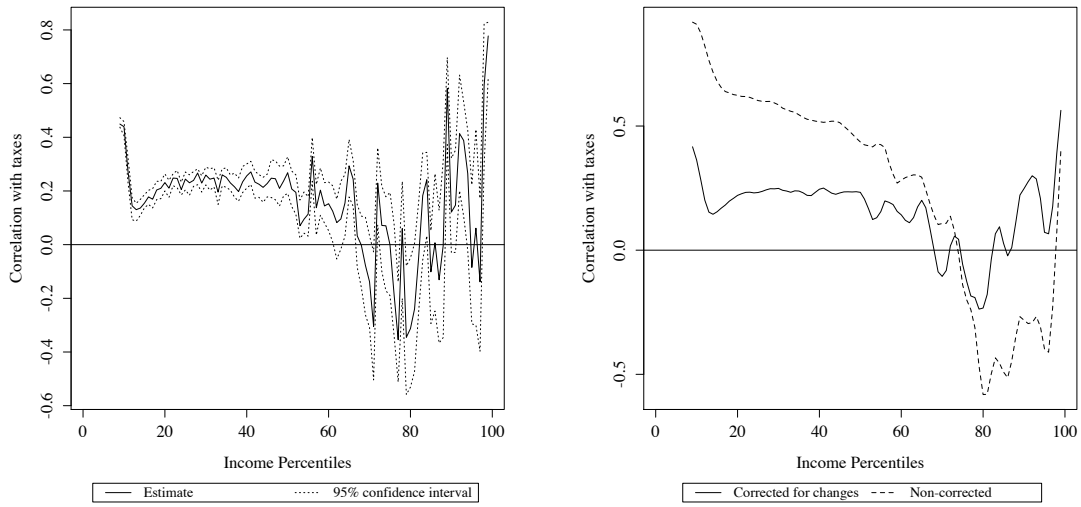
Figure A.4: Taxes corrected for changes in the distribution of income, First Bush Administration, 1988-1992.

Notes: These graphs display the results obtained when I use the simulated tax rates using the 1976 sample rather than obtaining the actual average tax rates. The second graph shows a comparison between the two calculations, with smoothed graphs.



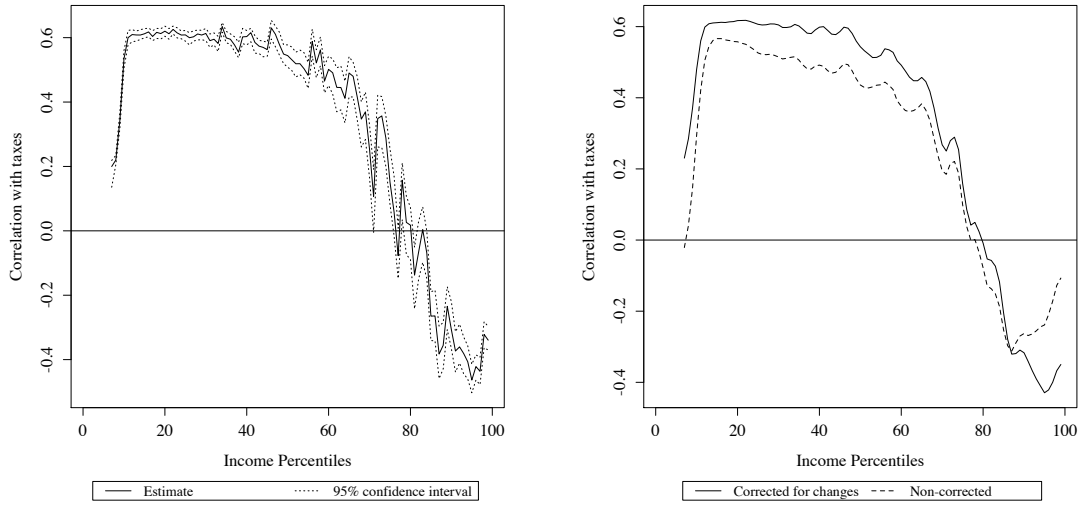
(a) 1976 Sample (b) Comparison

Figure A.5: Taxes corrected for changes in the distribution of income, Clinton Administration, 1992-2000.



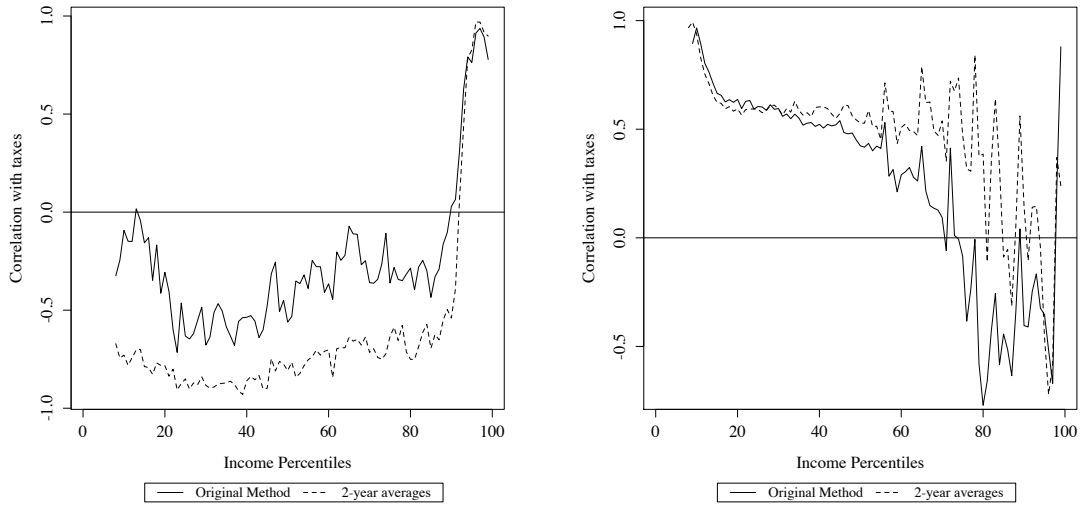
(a) 1976 Sample (b) Comparison

Figure A.6: Taxes corrected for changes in the distribution of income, Second Bush Administration, 2000-2007.



(a) 1976 Sample (b) Comparison

Figure A.7: Taxes corrected for changes in the distribution of income, 1987-2007.



(a) Clinton Administration (b) Second Bush Administration

Figure A.8: A Comparison with 2-year Averages, Clinton Administration (1992-2000) and Second Bush Administration (2000-2006).

Notes: These graphs display correlations using 2-year average correlations rather than simple correlations. This should mitigate concerns for endogeneity as explained in Karabarbounis (2011).

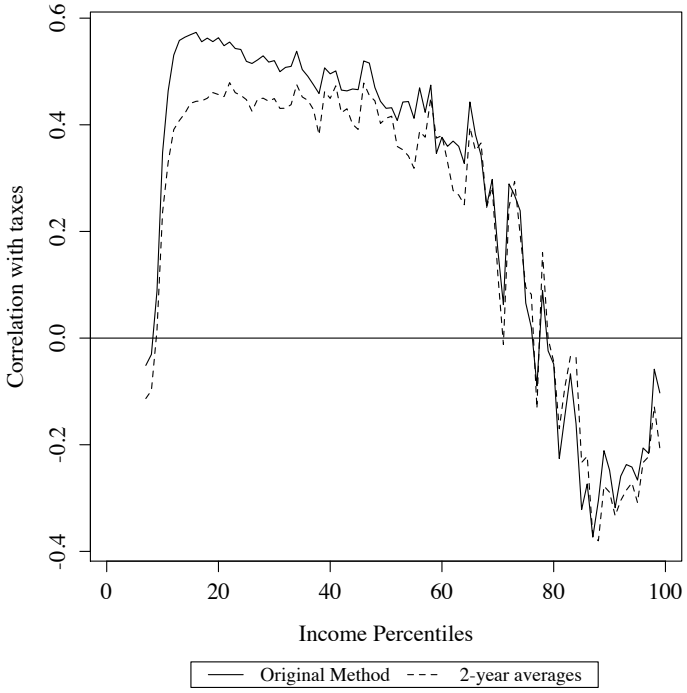


Figure A.9: A Comparison with 2-year Averages, Entire Period, 1987-2006.

Appendix B

Appendix to Chapter 3

B.1 African Americans, the Poor and Offenders

In the chapter, I provide theoretical and empirical justifications for using the proportion of African Americans and the poverty rate as geographic markers for the proportion of former felons. In this section, I supplement this analysis by looking at a small set of states for which recent county-level data on crime and prison is available. The data was obtained from the Justice Atlas of Sentencing and Corrections from the Justice Mapping Center.[1] It combines several sources (usually state departments of correction, parole and probation) and uses administrative records from these sources to map addresses to counties of residence. The mapping is usually possible in around 90% of the cases, the remainder coming from errors in addresses or homelessness. For this analysis, I use data on number of prison releases in 2008 and number of parolees as of Dec. 15, 2008 at the county-level. These data are available in respectively 10 and 8 states of my sample.[2]

In the following table, I regress the rate of prison release on the proportion of

[1] http://www.justiceatlas.org

[2] Data on prison releases is available for Oregon, Nebraska, Kansas, Iowa, Michigan, Indiana , Ohio, Pennsylvania, New York and Rhode Island. Data on parolees is available for these states except for Ohio and Pennsylvania.

African Americans and the poverty rate at the county-level with state fixed effects to justify my use of these characteristics. The two variables are highly significant when only these two are included and still significant (and the only significant variables) when I control for other demographic characteristics of the county. This analysis supports the use of the proportion of African Americans and the poverty rate as proxies for the proportion of former felons.

	Prison Releases In 2008		**Parolees as of Dec. 15, 2008**	
Average	0.00149		0.00203	
Proportion of African Americans	0.0036***	0.0045***	0.0020**	0.0028*
	(0.001)	(0.001)	(0.001)	(0.002)
Poverty Rate	0.0043***	0.0064***	0.0057***	0.0040*
	(0.001)	(0.001)	(0.001)	(0.002)
Proportion above 65		0.0011		-0.0010
		(0.002)		(0.002)
Proportion under 18		0.0041*		0.0046
		(0.002)		(0.003)
Proportion of Non-citizens		-0.0008		-0.0013
		(0.002)		(0.003)
Density		-4.81e-08*		-2.84e-09
		(2.52e-08)		(2.00e-08)
R-squared	0.545	0.585	0.505	0.539
States	10	10	8	8
Observations	709	709	554	554
State FE	Y	Y	Y	Y
Demographic Controls	N	Y	N	Y

Table B.1: African Americans, the Poor and Offenders

B.2 County Fixed Effects

A difficulty arises when trying to control for fixed unobservables characteristics of the counties, because the communities I focus on (African Americans and poor people) have a direct impact on turnout and public good provision even absent felon disenfranchisement. In this section, I detail how this constitutes a challenge to the inclusion of county fixed-effects and I offer alternative specifications to show that the results of the chapter are not due to fixed, unobservable characteristics of the counties.

In my setting I assume that (I focus only on the dimension of poverty for simplicity):

$$y_{i1976}^{Dis} = \beta_{1976}^{Dis} \times Pov_{i1976}$$
$$y_{i1976}^{NDis} = \beta_{1976}^{NDis} \times Pov_{i1976}$$
$$y_{i1996}^{Dis} = \beta_{1996}^{Dis} \times Pov_{i1996}$$
$$y_{i1996}^{NDis} = \beta_{1996}^{NDis} \times Pov_{i1996}$$

and I want to test that:

$$[\beta_{1996}^{Dis} - \beta_{1996}^{NDis}] - [\beta_{1976}^{Dis} - \beta_{1976}^{NDis}] > 0$$

Taking first differences over time for each county i to account for county fixed effects would give:

$$y_{i1996}^{Dis} - y_{i1996}^{Dis} = (\beta_{1996}^{Dis} - \beta_{1976}^{Dis}) \times Pov_{i1976} + \beta_{1996}^{Dis} \times (Pov_{i1996} - Pov_{i1976})$$
$$y_{i1996}^{NDis} - y_{i1996}^{NDis} = (\beta_{1996}^{NDis} - \beta_{1976}^{NDis}) \times Pov_{i1976} + \beta_{1996}^{NDis} \times (Pov_{i1996} - Pov_{i1976})$$

In usual difference-in-differences settings, the second term disappears as the intensity of the treatment does not vary from one period to the other only the effect of the treatment, or because there is no influence of the treatment intensity measure in the absence of the treatment. In the case of felon disenfranchisement, ignoring the additional term could lead to biases in the estimation. Estimating only the within-county effects and not taking into account the reallocation effect would miss part of the story. Following this analysis, I provide two robustness specifications to show that the effects displayed in the main specifications are not due to fixed unobservables characteristics of the counties.

First, I replicate my results on a set of counties for which the proportion of African Americans and the poverty rates did not vary considerably over the 20-year period I analyze. For each state in each year, I categorize the counties as being in thirds by poverty rate and proportion of African Americans separately. Over 20 years, 18% of the counties in the sample moved to a higher third of their state in terms of poverty and 18% moved to a lower third, while 64% remained in the same third. For African Americans, 34% moved up or down and 68% remained in the same third. However, only 43% of the counties remained in the same third for both dimensions. 85% of the counties moved only one third in one of the two dimensions, and I consider this new sample the "stable" sample.[3] For this sample, I run the following specification:

$$\Delta y_{is} = \alpha + \beta_1 Dis_s \times Pov_{is1976} + \beta_2 Pov_{is1976} + \gamma_s + \epsilon_{is}$$

This specification therefore takes into account county fixed effects for counties that have been stable on their social and racial composition and for which the omission of the second term will not be a large problem. We expect the coefficient β_1 to be significantly negative and similar to our previous estimations. In Tables B.2 and B.3,

[3]In all states, the proportion of counties that have remained stable with respect to their position in the repartition of African Americans and poor people are above two thirds, therefore, the "stable" sample is made from counties from all my sample states.

I show that my original results are not sensitive to the inclusion of fixed effects. I first show results from my original specifications and of these specifications run on the "stable" sample. I then compare these results to the first-difference specification explained above. The levels of significance as well as the estimates themselves do not noticeably change when I focus of the "stable" sample which shows that this sample does not suffer from large selection biases and then are robust to a first-difference specification.

A second robustness check consists in running a first-difference regression with a control for the additional factor that appears in this setting. I run regressions of the following form for the entire sample:

$$\Delta y_{is} = \alpha + \beta_1 Dis_s \times Pov_{is1976} + \beta_2 Pov_{is1976} + \delta_1 Dis_s \times \Delta Pov_{is} + \delta_2 \Delta Pov_{is} + \gamma_s + \epsilon_{is}$$

Results generally carry through as can be seen in tables 3 and 4. The coefficient on the poverty rate for turnout is the most affected but it remains significant. The results from the two-stage-least-square regressions are robust to controlling for the impact of demographic changes in counties. In all the cases, the hypothesis that the coefficient is between 2 and 3% of extra public good for an extra percentage point in turnout cannot be ruled out.

	Turnout			Public Good Provision		
	Full Sample	**"Stable" Sample**		**Full Sample**	**"Stable" Sample**	
	Original Specification			Original Specification		
African Americans in	-0.4734**	-0.6546***		-1.6461***	-1.6337***	
Disenfranchising States in 1996	(0.187)	(0.195)		(0.401)	(0.382)	
Poverty Rate in	-0.4816***	-0.5627***		-1.5773***	-1.8975***	
Disenfranchising States in 1996	(0.167)	(0.182)		(0.522)	(0.485)	
			First-Diff			First-Diff
African Americans in			-0.5640***			-1.6248***
Disenfranchising States in 1976			(0.168)			(0.379)
Poverty Rate in			-0.3686**			-1.7346***
Disenfranchising States in 1976			(0.158)			(0.394)
R-squared	0.619	0.619	0.534	0.945	0.946	0.505
Observations	3258	2778	1389	3258	2778	1389
FE	State-Year	State-Year	State	State-Year	State-Year	State
Demographic Controls	N	N	N	N	N	N

Table B.2: Robustness to County Fixed Effects

	Full Sample		**"Stable" Sample**		**"Stable" Sample**	
	Original Specification					
Turnout	3.3441**	2.0951**	3.0160***	2.3392**		
	(1.336)	(0.924)	(1.050)	(0.948)		
					First-Diff	
ΔTurnout					3.5613**	2.6911*
					(1.419)	(1.480)
R-squared	0.893	0.939	0.903	0.937	0.298	0.410
Observations	3258	3258	2778	2778	1389	1389
FE	State-Year	State-Year	State-Year	State-Year	State	State
Demographic Controls	N	Y	N	Y	N	Y

Table B.3: Robustness to County Fixed Effects, 2SLS

	Turnout		Public Good Provision	
	Original		Original	
African Americans	-0.4734**		-1.6461***	
in Disenfranchising States in 1996	(0.187)		(0.401)	
Poverty Rate in	-0.4816***		-1.5773***	
Disenfranchising States in 1996	(0.167)		(0.522)	
		First-Diff		FirstDiff
African Americans		-0.5466***		-1.5025***
in Disenfranchising States in 1976		(0.194)		(0.370)
Poverty Rate		-0.2731*		-1.3558***
in Disenfranchising States in 1976		(0.152)		(0.435)
R-squared	0.619	0.515	0.945	0.514
Observations	3258	1629	3258	1629
FE	State-Year	State	State-Year	State
Demographic Controls	N	N	N	N

Table B.4: Robustness to County Fixed Effects, Controlling for Demographic Changes

	Original Specification			
Turnout	3.3441**	2.0951**		
	(1.336)	(0.924)		
			First-Diff	
Δ Turnout			3.3161**	1.8367*
			(1.410)	(1.083)
R-squared	0.893	0.939	0.329	0.519
Observations	3258	3258	1629	1629
Std error	cluster	cluster	cluster	cluster
FE	State-Year	State-Year	State	State
Demographic Controls	N	Y	N	Y

Table B.5: Robustness to County Fixed Effects, Controlling for Demographic Changes, 2SLS

B.3 Second Control Group

In this section I detail results when splitting the control group of non-disenfranchising states into two and focusing and a second control group that is a subset of the control group. The second control group is composed of states neighboring the disenfranchising states. Given that the disenfranchising states are very central, I keep in the second control group states that have boundaries with *two* disenfranchising states. The second control group is composed of Colorado, Idaho, Kansas, South Dakota and Utah.

This second control group is very similar to the group of disenfranchising states on many observables and more similar than the overall control group, as can be seen on the summary statistics table. This will alleviate the concern that the two groups of states are significantly different. I then use this second control group as a control group in specifications similar to the main specifications of the chapter. In all the specifications, the comparison between the group of disenfranchising states and the second control group is significant and very close to our original estimates. In fact, the estimates of the returns to a higher turnout are even larger than in the original specifications.

Another use of a second control group which is believed to be biased in an opposite way to the remainder of the control group (as is the case here since the remainder of the control group is believed to be less similar to the disenfranchising states than the second control group) is to assess the assumption of unconfoundedness as detailed in Rosenbaum (1987) and Imbens (2004). Therefore, I replicate my original specifications but comparing the second control group to the remainder of the control group and show that no estimate is statistically significant in this setting.

	Disenfranchising		Second Control Group	
	1970	**2000**	**1970**	**2000**
Counties	267		307	
Proportion of African Americans	2.99	4.71	2.65	3.1
	(4.4)	(5.6)	(4.2)	(4.6)
Poverty Rate	13.0	11.4	12.6	10.0
	(6.3)	(4.8)	(5.3)	(4.6)
Unemployment Rate	3.88	5.26	4.34	4.68
	(1.6)	(2.1)	(1.8)	(1.9)
Average Per Capita Income	17,686	19,953	17,645	20,895
(2000 $)	(2,934)	(3,410)	(3,441)	(5,125)
Proportion under 18	35.6	26.0	35.7	27.5
	(3.6)	(2.5)	(4.4)	(3.8)
Proportion above 65	10.6	12.9	9.87	10.8
	(3.9)	(3.6)	(4.1)	(3.8)
Proportion of Non-citizens	0.64	4.53	0.73	4.48
	(0.68)	(3.8)	(0.56)	(3.6)

Notes: All statistics are weighted by the county population. Standard deviations are in parenthesis. The disenfranchising states are: DE, IA, NE, NM, NV and WY. The second control group is: CO, ID, KS, SD and UT.

Table B.6: Summary Statistics

	Full Sample		Second Control Group	
African Americans in	-0.4734**	-0.6318***	-0.6186***	-0.7655***
Disenfranchising states in 1997	(0.187)	(0.178)	(0.221)	(0.213)
Poverty rate	-0.4816***	-0.3743**	-0.3457*	-0.4265**
Disenfranchising states in 1997	(0.167)	(0.183)	(0.177)	(0.200)
R-squared	0.619	0.736	0.595	0.765
Observations	3258	3258	1142	1142
Demographic Controls	N	Y	N	Y

Table B.7: Second Control Group, Turnout

	Full Sample		Second Control Group	
African Americans in	-1.6461***	-1.0233**	-2.1619***	-1.9276**
Disenfranchising states in 1997	(0.401)	(0.421)	(0.803)	(0.868)
Poverty rate in	-1.5773***	-1.1350*	-1.9738***	-1.7746***
Disenfranchising states in 1997	(0.522)	(0.583)	(0.635)	(0.620)
R-squared	0.945	0.953	0.923	0.951
Observations	3258	3258	1142	1142
Demographic Controls	N	Y	N	Y

Table B.8: Second Control Group, Transfers

	Full Sample		Second Control Group	
Turnout	3.3441**	2.0951**	4.2171**	3.0296***
	(1.336)	(0.924)	(1.770)	(1.139)
Root MSE	0.254	0.192	0.285	0.192
R-squared	0.893	0.939	0.845	0.930
Observations	3258	3258	1142	1142
Demographic Controls	N	Y	N	Y

Table B.9: Instrumental Approach, Effect of Turnout on Transfers, Second Control Group

	Turnout		Transfers	
African Americans in	0.0307	0.1261	0.6788	0.9223
Second Control Group in 1997	(0.149)	(0.129)	(0.732)	(0.815)
Poverty rate in	-0.2087	0.0424	0.3761	0.7604
Second Control Group in 1997	(0.152)	(0.163)	(0.560)	(0.511)
Poverty rate	-0.7082***	-0.0458	3.0508***	1.1714***
	(0.148)	(0.122)	(0.336)	(0.452)
African Americans	-0.0464	-0.0563	1.0624***	1.2722***
	(0.065)	(0.073)	(0.114)	(0.169)
Unemployment rate		0.1548		0.1208
		(0.145)		(0.464)
Log average per capita income		0.2173***		-0.4330***
		(0.023)		(0.128)
Proportion above 65		0.8048***		0.0467
		(0.103)		(0.459)
Proportion under 18		0.1024		1.5683***
		(0.100)		(0.550)
Proportion non-citizens		-0.0709		-0.1885
		(0.100)		(0.514)
Density		-4.72e-06***		1.13e-05***
		(9.34e-07)		(3.05e-06)
R-squared	0.619	0.738	0.946	0.954
Observations	2724	2724	2724	2724
Std error	cluster	cluster	cluster	cluster
Demographic Controls	N	Y	N	Y

Table B.10: Second control group compared to remainder of control group

B.4 Placebo Regressions

In this section, I show regression of "placebo" outcomes or variables believed to be unaffected but the treatment, which is another way to assess the unconfoundedness assumption as detailed in Imbens (2004). I use demographic characteristics of the counties such as the proportion of children under 18, the proportion of adults above 65 and the proportion of foreign born aliens as placebo outcomes. I show that these outcomes are unaffected by felon disenfranchisement as measured by the poverty rate and weakly affected when measured by the proportion of African Americans (they are not affected once other demographic controls are included).

	Proportion Under 18		**Proportion Above 65**		**Proportion of Non-citizens**	
African Americans in	-0.2031***	-0.0116	-0.0741	0.0476	0.1498**	0.0849
Disenfranchising state in 1997	(0.061)	(0.051)	(0.085)	(0.058)	(0.059)	(0.065)
Poverty rate in	-0.0975	-0.0905	0.0166	0.0354	-0.0438	-0.0330
Disenfranchising states in 1997	(0.066)	(0.057)	(0.055)	(0.055)	(0.110)	(0.089)
R-squared	0.568	0.741	0.431	0.614	0.730	0.855
Observations	3258	3258	3258	3258	3258	3258
Std error	cluster	cluster	cluster	cluster	cluster	cluster
Demographic Controls	N	Y	N	Y	N	Y

Table B.11: Placebo Outcomes

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