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Essays on Democratic Institutions and Policy Outcomes

Authors	Fleurimond, Vladimir
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ABSTRACT
ESSAYS ON DEMOCRATIC INSTITUTIONS AND POLICY OUTCOMES
BY
VLADIMIR FLEURIMOND
August 2015

Committee Chair: Dr. Jorge L. Martinez-Vazquez

Major Department: Economics

This dissertation consists of three essays that examine the impact of democratic institutions on policy outcomes. The first essay investigates the impact of direct democracy on redistribution, tax progressivity and income inequality in the American states from 1984 to 2005. Currently available in 24 states, the statewide initiative allows citizens to directly influence policy outcomes. Theoretically, how this political institution impacts inequality and redistribution is ambiguous. Using a pooled OLS specification, I have found that direct democracy leads to an increase in income inequality and a decrease in state tax burden, with no effect on tax progressivity and only modest effect on expenditure redistribution. However, controlling for unobserved heterogeneity using a Correlated Random Effects model, I have found that direct democracy leads to an increase in marginal and average tax rates, with no effect on state tax burden. In their entirety, the findings of this essay underscore that the way in which direct democracy impacts redistribution and inequality is quite complex.

This second essay examines the extent to which fiscal spillovers exist in county governments in California. At the county level in California, many fiscal decisions are made

through the use of tax and expenditure referenda. Extant theory suggests that expenditures and revenues of neighboring jurisdictions are interdependent. That is, neighboring jurisdictions incorporate their neighbors' fiscal choices into their own fiscal decisions. Using spatial econometric analysis and a novel dataset of county expenditures and revenues from 2003 to 2013, I have found strong evidence of fiscal spillovers in California counties. That is, counties respond to what their neighbors do.

The third essay examines the impact of state-mandated alternative education programs for expelled and suspended students on juvenile crime. From 1987 to 2010, fourteen states adopted policies designed to reduce time students spend out of school as punishment by mandating that school districts establish alternative education programs to serve expelled and suspended students. Using difference-in-differences and event study methodologies, I estimate the impact of the state mandate on juvenile crime, finding that state-level juvenile homicide offending rates for black youth aged 12-17 significantly decrease after the implementation of those programs.

ESSAYS ON DEMOCRATIC INSTITUTIONS

AND

POLICY OUTCOMES

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VLADIMIR FLEURIMOND

A Dissertation Submitted in Partial Fulfillment of
the Requirements for the Degree
of
Doctor of Philosophy in
the
Andrew Young School of Policy Studies of
Georgia State University

GEORGIA STATE UNIVERSITY
2015

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

Dissertation Chair:	Dr. Jorge L. Martinez-Vazquez
Committee:	Dr. H. Spencer Banzhaf
	Dr. Eric J. Brunner
	Dr. Shiferaw Gurm

Electronic Version Approved:
Mary Beth Walker, Dean
Andrew Young School of Policy Studies
Georgia State University
August, 2015

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INTRODUCTION

This dissertation seeks to highlight the importance of democratic institutions for policy outcomes. Since as far back as one can remember, there has always been a debate about which forms of government best serve the interests of its citizens. The two forms of government that are at the center of this dissertation are representative democracy and direct democracy. More specifically, the first two chapters link the institution of direct democracy to fiscal outcomes; the last chapter broadly links “democratic institutions” to education outcomes at the state level in the U.S.

In the first chapter, I contribute to the existing literature on the impact of direct democracy on policy outcomes in at least the following three important ways. First, I measure the impact of direct democracy on redistribution with respect to the composition of government expenditures, with particular interest in the share of government expenditures on four redistributive proxies (education, health, hospital and public welfare). Second, I measure the impact of direct democracy on income inequality using five measures of income inequality from a newly available panel dataset of state income inequality. Third, I analyze how this political institution impact state tax systems, measuring its effect on state marginal and average tax progressivity and tax burden. In a nutshell, I examine the fiscal incidence of the adoption of the statewide initiative.

Indeed, twenty-four (24) states in the U.S. allow citizens to directly impact policy outcomes through the use of the initiative process, or direct democracy, by collecting a required number of signatures from registered voters for a piece of legislation to be placed on the ballot to the entire electorate for a vote.

Using state-level data from 1984 to 2005, the weight of the evidence suggests that the adoption of the statewide initiative measure has led to an increase in income inequality and modest impact of expenditure and revenue redistribution, with no effect on tax progressivity. However, I have found a strong effect of direct democracy on state tax burdens. Controlling for unobserved heterogeneity using a Correlated Random Effects (CRE) model, the adoption of the statewide initiative has led to an increase in both marginal and average tax rates, a decrease in education expenditures. Further, using the CRE, the effects of direct democracy on income inequality and state tax burden are modest.

The second chapter attempts to explore the extent to which fiscal spillovers exist in a direct democracy context analyzing government expenditures and revenues at the county level in California. I add to the existing literature using a novel dataset of revenue and expenditure categories for all 58 California counties from 2003 to 2013.

In general, there are several dimensions by which counties might respond to what their neighbors do. First, strategic interactions might take place due to expenditure and revenue mimicking, whereby counties simply imitate the expenditure and revenue patterns of their neighbors. The second dimension relates to Yardstick competition, as citizens or voters take into account fiscal choices in neighboring jurisdictions in judging their elected officials. Third, due to spillover benefits or externalities, government spending and revenues in a county might spread to neighboring counties.

Using five categories of county expenditures (general, health, public facilities, public protection, and public assistance) and three categories of revenues (sales tax, other taxes and transient tax), I have found strong evidence of fiscal spillovers across all categories of spending and revenues, except for health and public assistance expenditures. That health and public

assistance expenditures do not exhibit spillover effects can be explained by the fact that they are generally determined at the state level, not at the county level. The policy implications of the findings of this essay are generally related with the relationship between state governments and local governments in the U.S. In general, state governments are heavily involved in local education spending. There are likely to be benefit spillovers related to education expenditures, and therefore substitution effects with respect to other expenditure categories in the public budget. Also, specific to California, the state has a heavy influence on local revenues through the property tax, state policy makers should be mindful that local governments interact with one another along other revenue dimensions.

The third chapter, too, is broadly related to the impact of democratic institutions. Indeed, that states have plenary power to determine their own education policies originated from the 10th amendment of the U.S. Constitution. As a result, there is a wide variety of education policies across states, and this has given rise to a wide variety of policy outcomes as well. This paper examines the impact of state-mandated alternative education programs for expelled and suspended students on juvenile crime. The Gun-Free School Zones Act (1994), passed by congress to prevent gun violence in schools, has led school districts to implement various zero tolerance policy laws, allowing schools to expulse and suspend students for various school violations, even for first-time and minor school transgressions.

From 1987 to 2010, several states have taken steps to lessen time students spend out of school as punishment by mandating that school systems or school districts establish alternative education programs to serve expelled and suspended students. Proponents of those programs have argued that they help prevent the so-called movement from school-to-prison-pipeline, whereby students that are expelled or suspended from schools find themselves entangled in the

juvenile justice system. Theoretical considerations regarding the link between contemporaneous schooling and crime are ambiguous. Hence, following a well-developed empirical economics literature that links time students spend out of school to criminal activities, I estimate the impact of the state mandate on juvenile crime. Using difference-in-differences and event-study methodologies, I conclude that there is a negative impact of state-mandated alternative education programs for expelled and suspended students on county-level juvenile arrests. The findings might be due to, among other explanations, the incapacitation and the human capital accumulation effects of contemporaneous schooling.

Chapter I Direct Democracy, Redistribution, Tax Progressivity and Income Inequality: Evidence from U.S. States

I. Introduction

At least two facts stand out about income inequality in the U.S.: Over the last few decades, there has been a pronounced increase in income inequality in the United States (Piketty and Saez 2003). “Income inequality and relative poverty in the US are among the highest in the OECD and have significantly increased over the last four decades (Denk et al. 2013, p. 2).” However, this increase in income inequality does not seem to translate into much support for redistributive policies as the median voter theorem would predict. In this essay, I explore an alternative explanation to this puzzle by investigating the possible influence of the political structure in states within the U.S., as there is extensive evidence that political institutions matter for a variety of policy outcomes (Acemoglu, Johnson and Robinson 2002; Persson and Tabellini 2003, 2004, and 2006, Acemoglu et al. 2013).

Most academic discussions in the U.S. regarding redistribution and income inequality have taken place at the federal level. Yet, there is evidence that states do engage in redistribution, as there is substantial variation across states with respect to both revenue and expenditure redistribution (Chernick 2010). Gordon and Cullen (2012) provide theoretical evidence that in a federal system of similar to the U.S., lower level of governments such as states will engage in some form of redistribution despite a high degree of cross state mobility.

In this first dissertation chapter, I contribute to the existing literature on the impact of direct democracy on policy outcomes in at least the following two important ways. First, I measure the impact of direct democracy on redistribution with respect to the composition of

government expenditures, with particular interest in the share of government expenditures on four redistributive proxies (education, health, hospital and public welfare). Second, I measure the impact of direct democracy on income inequality using five measures of income inequality from a newly available panel dataset of state income inequality and state tax progressivity. Following Feld, Fisher and Kirchgässner (2010) who estimate the impact of direct democracy in Swiss cantons on income redistribution and welfare expenditures, I estimate separate equations for all the outcome variables: expenditure redistribution, revenue redistribution, tax progressivity and income inequality.

Twenty-four (24) states in the U.S. allow citizens to directly impact policy outcomes through the use of the initiative process, or direct democracy, by collecting a required number of signatures from registered voters for a piece of legislation to be placed on the ballot to the entire electorate for a vote. Following a long line of research that has examined the impact of this institution on a variety of policy outcomes, I empirically investigate the possible influence of this specific political institution on expenditure redistribution and income inequality. The question I ask is the following: Is there any systematic difference in expenditure redistribution, income inequality and tax progressivity across states due to the institution of direct democracy? In other words, what is the economic incidence of the statewide initiative measure?

Some of the policy outcomes of this institutional feature have been well documented, mostly in the U.S. and in Switzerland. To preview some of the empirical literature, direct democracy lowers overall government spending in states in the U.S. and in Swiss cantons (Matusaka 1995; Matusaka and McCarty 2001; Matusaka 2004, 2005). However, there are some dissenters, such as Zax (1989) who finds no effect of direct democracy on the level of government expenditures with respect to local governments in the U.S., and, notably, Marschall

and Ruhil (2005) who find direct democracy to lead to an increase in per capita expenditures and revenues at the state level in the U.S. from 1960 to 1990. Feld and Kirchgassner (2001) find a negative effect of direct democracy on government spending in local Swiss cantons. Farnham (1989) provides evidence of no effect of direct democracy on general expenditure level with respect to local governments in the U.S.

There is also a well-developed theoretical literature on the mechanisms through which direct democracy affects fiscal outcomes at the state level in the U.S., as there is a lack of observable campaign initiatives directly related to expenditure and tax issues to explain why initiative and non-initiative states have different fiscal outcomes. This lack of observable campaign initiatives suggests that the *underlying* political structure between initiative and non-initiative states leads to different fiscal outcomes. To preview the main line of argument of the theoretical literature, differences in fiscal outcomes between initiative and non-initiative states are attributed, not to specific ballot measures, but to the *presence* of the initiative measure itself, by influencing the behavior of state legislature.

There is a large public economics literature that has investigated the subject of redistribution at the state level in the U.S. For example, recently, Lutz et al. (2014) find that there is substantial cross-state variation in the extent to which state tax policies impact the income distribution. Ashby and Sobel (2007) link economic freedom to income inequality at the state level in the U.S. However, in the context of direct democracy, this essay is closely related to the following three papers that directly examine the redistributive impact of direct democracy: Feld, Fisher and Kirchgässner (2010) estimate the impact of direct democracy on welfare expenditure and income redistribution using pre and post-tax Gini coefficients in local Swiss cantons, finding that direct democracy is associated with less redistribution. In the U.S., Berry

(2009) suggests that direct democracy leads to a reduction in state aid to school districts for elementary and secondary education, therefore leading to higher levels of inequality across districts in per pupil spending. In addition, Hinnerich and Pettersson-Lidbom (2014, p. 961) find “that direct democracies spend 40–60 percent less on public welfare. Our interpretation is that direct democracy may be more prone to elite capture than representative democracy since the elite's potential to exercise de facto power is likely to be greater in direct democracy after democratization.”

Admittedly, there is no consensus on a standard way to measure redistribution and income inequality. For example, using cross-county data, Acemoglu et al. (2013) investigate the impact of democracy on redistribution using total tax revenues as a share of GDP. Some studies measure redistribution from the spending side of the budget, such as education as a share of GDP or total government expenditures. Further, arguments have been made regarding the possible relationship between inequality and tax progressivity. Unfortunately, this is not the venue to weigh the pros and cons of different measures of redistribution and income inequality. Therefore, in what follows, I look at different measures of redistribution, tax progressivity and income inequality and consider the totality of the evidence.

In this vein, the overall findings of this dissertation chapter provide strong evidence indicating that the initiative measure leads to an increase in income inequality and a decrease in state tax burden. With respect to average and marginal tax rates, I find no significant effect of the statewide initiative, though the point estimates are negative across several econometric specifications. In addition, the adoption of the statewide initiative has had a small and negative impact on expenditure redistribution. As a whole, these results are largely in line with the few

studies that examine the economic incidence of direct democracy, such as Berry (2009), Felds et al. (2010), Hinnerich and Pettersson-Lidbom (2014).

This essay has several policy implications. First, even the most casual observer of the political process in the U.S. would notice the flurry recent news-related articles regarding the pronounced increase in income inequality. Second, some very recent studies have sought to explain why there is no increase in demand for redistribution associated with the increase in income inequality (Kuziemko et al. 2013, Ashok et al. 2015). This study is a modest contribution to this debate, and, that political institutions at the state level in the U.S. seem to have an impact on redistribution, tax and inequality is a finding that needs to be considered in the larger income inequality debate in the U.S.

The outline of this chapter is as follows. In the next section, I present historical and institutional background information on direct democracy at the state level in the U.S. Then, I review the literature. Section IV presents the conceptual framework. Section V describes the data and specifies the empirical methodology. Section VI presents the results. The last section concludes and discusses the results.

II. History and Background

“Direct democracy” is defined as a form of government that allows citizens to make laws. At the federal level in the U.S., citizen law-making is not allowed. At the state level, however, twenty four (24) states currently have the initiative measure. This relatively “old” institution took root in the American progressive movement in the late 1890s to early 1920s in order to counteract legislative decisions and state government corruption. The last state to have adopted this institution is Mississippi in 1992. South Dakota is the first state to have adopted the initiative

process in 1898, followed by Utah in 1900 and Oregon in 1902. As of 1918, 19 states have adopted the initiative measure. From 1904 to 2012, Oregon has been the leader in initiative use with 363 initiatives placed on the ballot, followed by California with 352, Colorado with 288, North Dakota with 183, and Arizona with 165. Why states that have the initiative measure have not removed it is an open and interesting question. I am not aware that it has been addressed in the literature.

I shall briefly distinguish between three forms of direct democracy: referendum, direct initiative and indirect initiative. In a referendum, the state legislature enacts a law, which is submitted to voters' approval. In the direct initiative process, a citizen or a group of citizens drafts a piece of legislation, collects a required number of signatures from registered voters and places the piece of legislation on the ballot to the electorate for a vote. In the indirect initiative process, the piece of legislation goes to the state legislature. If the state legislature fails to take action, the piece of legislation is then presented to the electorate for a final vote. As is often the case in the direct democracy literature, the main focus of this paper is on the direct initiative process, as it allows the citizens to completely bypass the state legislature. I am not aware of any paper that empirically investigates the possible impact of the other two types of institution on policy outcomes. This might be a worthwhile exercise.

The direct initiative process possesses certain basic features that are the same across states such as preliminary filings with a state official, compliance with rules and laws, collection of a number of signatures from registered voters and a circulation period for an initiative to be placed on the ballot. However, those rules have wide cross state variations such as subject restrictions [legislative matters only, no restrictions and single subject, net signature requirements for a measure to be placed on the ballot such as the percentage from the previous

number votes cast in gubernatorial or presidential elections and the geographical distribution of the number of signatures collected from registered voters]. The signature requirement for a measure to be placed on the ballot varies from a low of 3.5 % of registered voters in Massachusetts to a high of 15% of registered voters in Wyoming.

The initiative measure has been used for a variety of issues, such as governance issues by establishing term limit for state governors; economic issues on tax and spending such as expenditure limitation; education issues, as this was the case in Florida in 2002 where the citizens approved an amendment to the Florida Constitution, limiting the number of students in core classes, to name a few examples. There have also been some high profile cases related to the initiative measure, such as Proposition 209 in California in 1996, the California Civil Rights Initiative, a ballot proposition which, upon being approved in the November 1996 elections, amended the state constitution to prohibit state government institutions from considering race, sex, or ethnicity, specifically in the areas of public employment, public contracting or public education; Proposal 2 declared English to be the official language for the conduct of government business in Utah in 2000. As Matsusaka (2005, p. 6) summarizes, “Over the last 15 years, voters have decided 12 initiatives on abortion-related topics, a flurry of propositions concerning job discrimination on the basis of sexual orientation, and numerous other issues related to social policy, such as capital punishment, medical marijuana, and physician-assisted suicide.”

At least two features of the initiative measure at the state level are noteworthy. First, no state with the initiative measure has done away with it. Second, another one is the reason why they were adopted in the first place. To my knowledge, the first question has not been addressed in the literature. The second issue however, although very recently, has been address somewhat,

though there still remains some mystery as to why some states adopted those measures in the first place. As an important step for moving forward toward advancing our understanding as to why some states adopted the initiative process is a relatively recent paper by Smith and Fridkin (2008) in the *American Political Science Review*, and it deserves to be quoted at length: Smith and Fridkin (2008, p.344) offer “considerable support for a political rather than a purely socioeconomic or regional, explanation for the genesis of direct democracy in the American states. In the midst of rapid national and subnational state building, we find that the size of the majority’s party surplus of seats and the relative youth of state political parties to be driving forces in the decision of state legislature to devolve institutional power.”

III. Related Literature and Conceptual Framework

For simplicity, I divide the direct democracy literature in two broad categories: *outcome* and *mechanism*. The former is mainly empirical in nature and deals with the impact of direct democracy on various policy outcomes while the latter is mostly theoretical and attempts to explain the underlying cause of different policy outcomes between initiative and non-initiative states.

Outcome: The empirical literature on the impact of direct democracy on policy outcomes has been well documented. This literature has focused on the impact of direct democracy on government expenditures and revenues, mostly in the U.S. and in Switzerland. In the U.S., the seminal study on the impact of direct democracy on fiscal outcomes is Matsusaka (1995), who studies the fiscal impact of the initiative process on general government spending and revenues at the state level, finding that the presence of the initiative measure leads to a decrease in overall level of per capita state expenditures and revenues from 1960 to 1990. Pommerehne (1990)

provides similar evidence with respect to the impact of the presence of direct democracy on general level of government expenditures in Swiss cantons, while Funk and Gathmann (2011) find a modest effect of direct democracy on government spending at the cantonal level using historical data. Yet, at both the state level and the local level in the U.S., Zax (1989, p.267) finds that “Direct government expenditures per capita are significantly higher in both states and municipalities which permit initiatives” in 1980, while Camebreco (1998) finds no effect of direct democracy on state and local governments’ per capita expenditures from 1988 to 1990.

Most empirical studies investigating the impact of the initiative measure on overall level of government expenditures do not explicitly model the choice of form of government. For example, with respect to states in the U.S., one wonders why the Progressive movement was successful in some states and not others? Why do the states that have the initiative measure have not removed it? These are open questions in the literature, and, therefore, the choice of form of government with respect to the initiative measure might be endogenous. One notable exception to the general trend in the empirical literature is Marshal and Ruhil (2005) who apply a maximum likely method to model the choice of government, finding that the initiative measure leads to an increase in general government expenditure and revenue per capita from 1960 to 1990.¹ Thus, empirically, the impact of direct democracy on government spending is ambiguous, with the weight of the evidence probably indicating that direct democracy lowers overall government spending.

Mechanism: In light of the variety of studies empirically investigating the impact of direct democracy on policy outcomes, a natural question arises: why one would expect direct democracy to affect policy outcomes? A simple explanation would be that citizens in initiative

¹ Sass (1991) also makes a somewhat similar argument, but in the case of local governments in the U.S.

states constantly propose new pieces of legislation that change the *status quo*. Therefore, the initiative measure would have a direct impact on policy outcomes. However, the lack of observable campaign initiatives related to specific issues at the state level would suggest that the differences in fiscal outcomes between initiative and non-initiative states cannot be explained by ballot measures that are specifically related to state expenditure and revenue categories. Therefore, any policy effect attributed to the presence of direct democracy at the state level would have to do with its *threat effect* or *indirect effect*, not as a result of specific campaign initiatives.

Several explanations have been advanced to explain why despite this lack of observable campaign initiatives directly related to expenditure and revenue categories, initiative and non-initiative states have different fiscal outcomes. One of the very few studies that empirically attempt some explanation is Randolph (2010), estimating the impact of direct democracy on the number of bills enacted by the state legislature, finding that initiative states on average pass more legislations than non-initiative states. Randolph (2010) therefore concludes that the initiative measure affects policy outcomes through legislative production. Cognizant of the fact that their constituents have the power to pass legislation, lawmakers in initiative states act preemptively by enacting more bills than lawmakers in non-initiative states. This line of argument provides empirical support to the notion of the *threat effect* of the initiative process. Similarly, Gerber (1996) proposes a game theoretical model with three players: an incumbent politician, a representative voter and an interest group to conclude that the strategic interactions of the three groups will align the politician's choice closer to the voter's preferred policy choice. That is, even if the initiative measure is *not* actually used, as is often the case at the state level in the U.S., its availability might still have an impact on policy choices through a *threat effect*.

Elsewhere, Besley and Coate (2000, 2008) hypothesize that the initiative process allows states to unbundle issues, as citizens in states with the initiative process can propose a piece of legislation and vote on specific issues, thus leading to congruence between policy outcomes and citizens' preferences. In pure representative states, citizens impact policy outcomes mainly through their representatives. By voting for a candidate in pure representative states, citizens therefore vote for a *menu* of issues. In initiative states, however, citizens can vote on *specific* issues through the use of initiatives and referenda, leading to congruence between policy outcomes and citizens' preferences. With respect to specific issues, Gerber (1999) argues that citizen initiatives increase congruence on issues such as the death penalty and abortion laws. Matsusaka (1992) and Matsusaka and McCarthy (2001) conclude that controversial issues are more likely to be decided by the initiative process, as lawmakers try to avoid some of the consequences of their controversial policy choices.

In light of the theoretical literature, one wonders, then, about the causal mechanisms through which direct democracy might have an impact on expenditure redistribution and eventually income inequality. This issue has not been addressed theoretically in the literature. However, the one paper that this essay is closely related to relies on the median voter theorem (Feld, Fisher and Kirchgässner 2010). The empirical literature testing the prediction of the median voter theorem offers mixed evidence. One of the main explanations is that the stringent assumptions of the model are likely not to be held in empirical settings testing the prediction of the model. There are, however, other theoretical models of income redistribution that might offer some clues with respect to the impact of direct democracy on redistribution and eventually income inequality. Unfortunately, those models have not been specifically applied to the institution of direct democracy.

In any case, I conclude with Marshal and Ruhil (2005, p.322) that “the historical record and contemporary political theory both suggest that the relationships between adoption, use and consequences of the statewide initiative have yet to be explained,” and Matsusaka (2001), as cited in Marshal and Ruhil (2005, p.328), that “the effect of the initiative process has yet to be explained.”

If the economics literature offers no clues as what one should expect regarding the eventual impact of direct democracy on redistribution and income inequality, recent political developments might offer some answers. For example, despite the fact that the adoption of the statewide initiative took place during the Progressive Era in the U.S., recent political developments related to this political institution on issues such as abortion and the death penalty would seem to suggest that the statewide initiative has yielded outcomes that are on the right of the political spectrum. In addition, answers on recent surveys on the extent of inequality and redistribution on the U.S. are divided across partisan lines.

In the end, although there is a lack of theoretical models that capture direct democracy at the state level in the U.S., in light of those recent political developments with respect to the statewide initiative and the evidence that direct democracy lowers per capita expenditures, a priori it would not be surprising to find a negative impact of direct democracy on redistribution and tax progressivity and increase in income inequality. I address this question in the empirical analysis that follows.

IV. Empirical Specification

The purpose of this analysis is to examine the extent to which the initiative measure in the American states affects budgetary choices related to expenditure redistribution, tax

progressivity and eventually income inequality. The initiative measure was adopted in most states at the turn of the 20th century, and it has not been removed. I cannot therefore exploit the panel nature of the data by using a fixed effects model since the within transformation would essentially wipe out the variable of interest, the initiative measure (excluding Mississippi, the only state that changed its status during the period under study: 1984-2005).

I therefore follow previous literature by employing a pooled-OLS specification. To account for serial correlation, within-state spatial correlation and heteroskedasticity, all standard errors are clustered at the state level. I control for four census regions to account for regional time-invariant heterogeneity. In addition, I progressively control for other variables that are closely related to the initiative process that the literature has identified to be important, such as state government ideology, the signature requirement and the average number of voter initiatives placed on the ballot and their approval rate.

To that end, I specify an equation of the following form:

$$(1.1) \quad y_{st} = \alpha D_{st} + \beta x_{st} + v_t + \varepsilon_{st},$$

where y_{st} is the outcomes of interest in state s at time t , x_{st} is a vector of control variables, v_t and ε_{st} are year effects, and the usual error term, respectively. The variable of interest, D_{st} , the initiative indicator, measuring the impact of direct democracy on policy outcomes.

Following the political economy literature, I control for a rich set of variables where, X_{st} , includes demographics characteristics [population, the fraction of the population between the ages of 5 and 17, and the fraction of the population over the age of 65, percent of African

American and population density ; economic variables such as median income, per capita income and its square, unemployment rate, state minimum wage rate and poverty rate; state political characteristics such as the share of democratic legislators in the state senate and the state house of representatives, state government ideology, the share of the democratic votes received by the state governor in the last election, among other variables.

In addition, there are other important features of the initiative measure that do not vary over time such as the signature requirement, the insulation index and the legislative index. The signature requirement is the number of signatures from registered voters that need to be collected for a measure to be placed on the ballot. The qualification index measures the difficulty in qualifying a voter initiative for the ballot; the legislative insulation index measures the ability of the legislature to modify successful voter initiatives. Those features of the statewide initiative are taken into account in the empirical analysis in section VI.

In general, one of the main advantages of using panel data is to mitigate heterogeneity bias. Here, heterogeneity is inherent in both the history and the evolution of the initiative process at the state level. First, the American progressive movement is largely credited to have led to the successful campaigns that allowed most states to adopt the initiative measure and why the movement was successful in some states and no others is left for debate. Second, there is heterogeneity with respect to the institution itself such as the geographic distribution for the signature requirement for a piece of legislation to be placed on the ballot. Third, the American progressive movement was a leftist movement, but differences in policy outcomes between initiative and non-initiative states over the years would suggest that the initiative measure or its impact might be much complex, as initiative states pass on average more conservative policies with respect to death penalty and abortion, to name a few examples. Fourth, different years of

cross-sectional data lead to different conclusions with respect to the impact of direct democracy on government spending. Hence, given these issues and the lack of a theoretical prediction, to exploit the panel nature of the data and complement the findings of the pooled OLS specification, I employ an unobserved effects model: Correlated Random Effects (CRE) model (Wooldridge 2010).

Following Ferrer-i-Carbonell (2005), I therefore model the relationship between unobserved heterogeneity and the *time-varying* variables, specifying the following model:

$$(1.2) \quad y_{st} = \alpha D_{st} + \beta x_{st} + \gamma \omega_s + \varepsilon_{st},$$

where y_{st} the outcome of interest, x_{st} is vector of state time-varying covariates, ω_s is a vector of regional indicators and ε_{st} is the error term. I am interested in α , the coefficient of direct democracy. I then rewrite the error component as $\varepsilon_{st} = \theta_s + \eta_{st}$, where θ_s is a state random effect and η_{st} is the usual error term.

Following Mundlak (1978), relaxed by Chamberlain (1982), I allow for correlation between the state random effect θ_s and the *time-varying* variables x_{st} by assuming the following structure of the state random effect:

$$(1.3) \quad \theta_s = \sum_{t=1}^T \rho_s \bar{x}_{st} + \sigma_s,$$

where σ_s is a pure error term not correlated with the random effect, while the correlation between the state random effect and the *time-varying* variables takes the form $\rho_s \bar{x}_{st}$, with \bar{x}_s being the average of the x_s over time.

Finally, adding both the state random effects and the time effects T to specification (1.2), I use the following device to estimate the correlated random effects model:

$$(1.4) \quad y_{st} = \alpha D_{st} + \beta x_{st} + \upsilon T + \gamma \omega_s + \sum_{t=1}^T \rho_s \bar{x}_{st} + \sigma_s + \eta_{st},$$

where all the variables are defined as before. One advantage of this procedure is that it allows for a modest way to control for time-invariant unobserved heterogeneity while also controlling for several arguably endogenous time-invariant variables such as the signature requirement, the legislative insulation index and the initiative indicator. With the inclusion of the regional indicators, this approach is close to a fixed effects model. One drawback of this approach is that the time-invariant variables are not allowed to be correlated with unobserved heterogeneity. I partially mitigate this drawback by letting an indicator dummy for tax measures placed on the ballot be correlated with the time-invariant unobserved heterogeneity. The ultimate drawback of this approach is that a functional form must be specified for the correlation between the time-varying variables and unobserved heterogeneity. In short, the CRE approach provides a modest way to control for time-invariant unobserved heterogeneity.

V. Data and Descriptive Statistics

The data for this study were drawn from several sources. For the first outcome of interest, expenditure redistribution, I use data from the Survey of Government Finances of the Census. The census has detailed state level data on different categories of government expenditures that I used to create the share of expenditures on education, health, hospital and public welfare. The Survey of Government Finances of the Census also contains several categories of state tax revenues that I intend to include in the final draft of this essay.

For the second outcome of interest, I use the state income inequality data from Frank (2009) (available at http://www.shsu.edu/~eco_mwf/). The measures of income inequality used in this dissertation essay are the Atkinson index, Theil index, Gini index, and the percentage income share of the top 10% and top 1% of income earners. The income inequality measures have been previously used by Frank (2009) and Chintrakarn, Herzer, and Nunnenkamp (2011), both published in the *Economic Inquiry*.

I use population data from the *intercensal estimates* of the Census Bureau to control for the median age of the population and to create the fraction of the state population between the ages 5 and 17, and the fraction of the population over the age of 65. I also use data from the statistical abstract of the census (selected years) to control for state population density. State economic characteristics such as income per capita and median income are from the Bureau of Labor Statistics. I also control for the average number of voter initiatives appearing on the ballot over each two year-election cycles from 1984-2005, and I also control for their approval rate.

I use several political variables from the public use data collected by Carl Klarner of Indiana State University: state partisan balance such as the share of democratic senators and house representatives in the state legislature, an indicator variable measuring whether the state

governor in office and the party in control of the state senate are of the same party. The state government ideology index comes from the updated version of Berry et al. 1998 (available at <http://rcfording.wordpress.com/state-ideology-data/>).

Following Bowler and Donovan (2004), I control for the qualification difficulty index which ranges from 1 to 6. The qualification index measures the difficulty in qualifying a voter initiative for the ballot, and it decreases as the values increase. I also control for the legislative insulation index which ranges from 1 to 9 with the insulation to the legislature decreasing as the values increase. As is the case in the literature, non-initiative states receive a value of 0.

Data with respect to the initiative measure such as the signature requirement for a measure to be placed on the ballot are from the Initiative and Referendum Institute.² I use intergovernmental revenues from the Survey of Government Finances of the Census. I use the share of African American at the state for the Abstract from the Census, as African Americans tend to be more supportive of redistribution, state population growth and the share of the population living in Metro areas. I use marginal and average state tax data from the TAXISM program of the National Bureau of Economic Research (NBER).

Table 1.1 presents means and standard deviations for key variables used in this study. Column (1) presents summary statistics for all the sample as a whole; in general, across most of the outcome variables such as tax progressivity, income inequality, expenditure and revenue distribution, there do not seem to be systematic differences between initiative and non-initiative states.

² Initiative and Referendum Institute: <http://www.iandrinstute.org/>. Accessed in 2013, 2014 (various months).

VI. Results

A. Expenditure Redistribution

In this section, I present the findings on the impact of direct democracy on expenditure redistribution. Using a baseline specification from Eq. 1.1, as we can see from columns (1) through (4) of Table 1.2, on the whole, it would seem that the adoption of the state wide initiative does not have a significant impact on several categories of expenditure redistribution. The estimated coefficient for health expenditures is positive and significant, yet small; the point estimate for hospital expenditures is negative and significant, while the estimated coefficients for education and public welfare are statistically insignificant and negative.

To further probe these findings and given the mystery surrounding the adoption of the statewide initiative and the cross state variation in the signature requirements for a measure to be placed on the state ballot, a dummy variable for the statewide initiative may not capture heterogeneity inherent to the institution. I therefore explore this issue, taking into account the signature requirement. In Table 1.3, adding both the difficulty index and the insulation index, both the magnitude and the significance of the coefficients from columns (1) through (4) remain unchanged. Including only year fixed effects and no control variables in Table 1.4, from the point estimates from columns (1) through (5), only the coefficients for hospital expenditures become significant at the 5% level. Adding a dummy variable for southern states, in Table 1.5, the point estimate for expenditures on hospital remains negative and significant, albeit weakly significant.

To summarize the findings of this section, overall, direct democracy seems to have a modest impact on expenditure redistribution. The findings are never statistically significant for education spending (in contrast to Berry 2009), while they are sometimes significant for both

public welfare and hospital expenditures (always with a negative sign), depending on the econometric specifications. The coefficient for health expenditures, however, is statically significant and positive in the baseline model, probably due the heavy involvement of the federal government in the health system through Medicare and Medicaid. On balance, these findings are in line with Felds et al. (2010) and Hinnerich and Pettersson-Lidbom (2014), and opposite to Berry (2009).

Turning to the findings of the CRE model, Tables 1.6, 1.7 and 1.8 present the findings, progressively taking into account the insulation index and the difficulty index. In contrast to the pooled OLS results, these later results show that direct democracy significantly lowers education expenditures. The point estimates across all three specifications are highly significant. This latest finding is in line with Berry (2009).

B. Tax Progressivity

In table 1.9, I turn to the effect of direct democracy on tax progressivity using measures of state tax progressivity from the TAXISM program from the NBER.³ In general, average tax rate is the share of income one pays in taxes, while marginal tax rate is the tax rate imposed on the last dollar of income. Both tax measures are often used in the tax incidence literature to gauge the degree of progressivity of tax systems.

Columns (1) through (6) from Table 1.9 present the results for the impact of direct democracy on tax progressivity. All the coefficients are negative and statistically insignificant, implying no effect of the statewide initiative on the progressivity of state tax systems. The results

³ For a general reference of this program, see Feenberg and Coutts (1993) "An Introduction to the TAXSIM Model", *Journal of Policy Analysis and Management*, Vol. 12 no. 1.

remain more or less the same in both sign and magnitude by removing all the control variables one at a time. From columns (1) through (6) of Table 1.9, I show a progression of the point estimates, starting with the baseline model in column (1) and leading to my most-flexible specification in column (5). Adding the signature requirement to the baseline specification does not change the estimated coefficients in columns (2) and (5). Next, the results still hold to the addition of the difficulty index and the insulation index in columns (3) and (6) of the same table. Regardless of the econometric specifications, the point estimates remain negative. That the point estimates do not seem to be particularly sensitive to controlling for these features of the statewide initiative lends support to the notion that direct democracy does not have an impact on the state tax systems. In columns (1) through (4) of Table 1.11, excluding all the control variables and including either year fixed-effects or a dummy for southern states do not change the sign nor the magnitude of the point estimates.

C. Robustness checks

In this section, an attempt is made to further assess the effect of direct democracy on state tax systems, taking into consideration some additional robustness checks.

Indeed, one ongoing debate in the direct democracy literature is the mechanisms through which the statewide initiative impacts policy outcomes. The debate centers on the direct vs indirect impact role of the initiative measure. For example, some studies have hypothesized that direct democracy affects policy outcomes through an indirect effect in the sense that, regardless of the number measures placed on the state ballot, the presence of the statewide initiative measure itself is sufficient to impact fiscal outcomes, by altering the behavior of the state

legislature. I account for the direct impact of the statewide initiative, linking specific tax referenda to fiscal outcomes.

To that end, I undertake a simple exercise, compiling all tax measures that were placed on the state ballot from 1984 to 2005. In Table 1.10, even when controlling for whether in a given year a tax measure was placed on the state ballot (Tax measure), the coefficients of the dummy indicator for direct democracy in columns (1) and (2) remain insignificant. In addition, using a dummy indicator (Tax measure+) for whether in a given year a tax measure placed on the state ballot was approved does not change the point estimates of marginal and average tax in columns (3) and (4) of Table 1.8. In brief, this exercise provides support to the findings in the previous section; direct democracy does not have a significant impact on state tax progressivity.

As done previously, I further probe the robustness of these findings using a CRE model. Tables 1.12 and 1.13 present the results for both marginal and average tax rates. To facilitate comparison between the two models, columns (1) through (3) from Table 1.12 replicate the OLS findings while the last three columns present the results for marginal tax rates using the CRE model, which shows that the point estimates are positive and significant, implying that direct democracy leads to an increase in marginal tax rate. Turning to Table 1.13, the findings with respect to average tax rate are similar.

D. State Tax Burden

So far, the empirical evidence suggests that direct democracy has no significant impact on average and marginal state tax progressivity, although the signs of the coefficients remain negative across the different econometric specifications. It might be useful to recall that in a federal system with a high degree of mobility, as is the case in the U.S., subnational governments

such as states might be limited on how much they can use the tax systems for redistributive purposes (Chamberlain and Prante 2007; Davis et al. 2009). Given that limitation, I now turn to other proxies of tax progressivity by investigating how direct democracy impacts state tax burden, which is a broad measure of how much state residents pay in tax to their own states. Table 1.14 presents the results, starting from the baseline specification in column (1) to a more flexible specification in column (5), by progressively adding the signature requirement, dummy indicators for tax ballot measures, the difficulty index, the insulation index; there seems to be strong evidence showing that direct democracy lowers overall state tax burden, as all the point estimates are negative and highly significant.

Here, I repeat the same exercise as in the preceding paragraph, but excluding all the control variable. In columns (1) through (4) of Table 1.15, I show a progression of the point estimates, starting with the baseline model in column (1) and leading to a flexible specification in column (4). Controlling only for year fixed effects, the point estimate in column (1) is still negative but statistically insignificant. Adding a dummy indicator for southern states, the point estimate in column (2) becomes significant. Repeating the same exercise as in the preceding sections, linking policy outcomes to specific tax measures placed on the state ballot, the coefficients of the dummy indicator for direct democracy in columns (3) and (4) of Table 1.15 are negative and statistically significant. The findings are robust to the use of different econometric specifications; in short, there is strong indication that direct democracy significantly lowers overall state tax burden. However, the statistical significance disappears when controlling for unobserved heterogeneity as can be observed in Table 1.16.

E. Income Inequality

In Table 1.17, I turn to the impact of direct democracy on income inequality. From the benchmark specification, all the coefficients are positive and statistically significant, implying that direct democracy worsens income inequality at the state level in the U.S. This finding is in line with Felds et al. (2010) who find that direct democracy has also led to an increase in income inequality in Swiss cantons.

From Table 1.18 to Table 1.21, I show a progression of the point estimates using flexible specifications. Including the signature requirement in Table 1.18, the effects become smaller, as the point estimates are significant only for the Atkinson index and the Gini index (10% level). In Table 1.19, controlling for a dummy indicator for whether a tax measure was placed on the state ballot, all point estimates are positive and statistically significant. Adding a control variable for at least one successful tax measure, from columns (1) through (5) of Table 1.16, the coefficients remain the same, both in significance and magnitude. In Table 1.20, with no control variables and only year fixed effects, the Gini index remains positive and significant. Adding a dummy variable for southern states, as we can see from Table 1.21, there is overwhelming evidence on the impact of direct democracy on income inequality. That the estimated coefficients are robust to additional controls such as the signature requirement and whether a successful tax measure was placed on the ballot lends support to the notion that variation in state-level income inequality is not systematically related to unobservable variables that might impact both direct democracy and income inequality.

Taking into account state-level heterogeneity, Tables 1.22, 1.23 and 1.24 present the results for the CRE specification, progressively controlling for the insulation index and the difficulty index. Here, in contrast to the findings for state tax burden and marginal and average

tax rates, the findings for income inequality show that all the point estimates are positive, although the significant disappear in some of the specifications.

VII. Conclusion

The focus of this first dissertation chapter was on the impact of direct democracy on redistribution and income inequality in the American states from 1984 to 2005. The consequences of rising income inequality are well-known. For example, a rise in income inequality is correlated with an increase in criminal activities and a decrease in intergenerational mobility. Policymakers and the public at large are concerned with the increase in income inequality. As recently as June 3, 2015, *the New York Times* reported on a Times/CBS poll that finds that Americans are concerned with the increase in income inequality.

Contributing to the current policy debate, however modestly, in this dissertation chapter, I explore an alternative explanation to the rising income inequality issue, investigating the role played by a specific political institution. As mentioned earlier, there is no unique way to measure redistribution and inequality. With this caveat in mind, the picture painted in this essay shows that the presence of the statewide initiative to lead to an increase in income inequality and a decrease on overall state tax burden with no effect on state tax progressivity and a small effect on expenditure redistribution. Future research might to further investigate the impact of direct democracy along different segments of the income distribution. Using the traditional methodology employed in the literature, overall, the findings provide support to empirical studies that have argued that the adoption of the statewide initiative impacts policy outcomes. However, if Hinnerich and Pettersson-Lidbom (2014) attribute similar findings with respect to direct democracy in Swiss cantons to the appropriation of elites, whether such an explanation can be

applied to states in the U.S. is left for debate. However, controlling for unobserved heterogeneity using a CRE model, I find strong evidence that direct democracy leads to an increase in both marginal and average tax rates, a decrease in education expenditures and no effect on state tax burden and income inequality. These findings have implications for the literature which mostly employs the pooled OLS specification, thus failing to account for unobserved heterogeneity.

Several remaining issues should be emphasized. First, the overwhelming evidence on the impact of direct democracy on income inequality and overall state tax burden implies that further research is needed to disentangle the mechanisms of direct democracy on income inequality and redistribution. Although there is a sizable literature on the impact of direct democracy, much research is needed to sort out the mechanisms through which this institution impacts policy outcomes.

Second, one should probably be cautious in making policy recommendations with respect to the findings of this essay due to the limited capacity of states in the U.S. to redistribute income and eventually impact inequality. In a federal system with a high degree of mobility as is the case of the U.S., not all taxes are at the disposal of sub-national governments for redistributive purposes. Indeed, the findings of the existing literature on the capacity of state affect redistribution through the tax system are mixed (Chamberlain and Prante 2007; Davis et al. 2009).

Chapter II Direct Democracy, Local Governments and Fiscal Spillovers: Evidence from California Counties

I. Introduction

Much has been written on the extent of strategic interactions among governments in the public finance literature. However, with very few exceptions, strategic interactions are mostly analyzed in the context of a purely representative form of government whereby citizens impact policy outcomes mostly *indirectly*, through elections by voting for their representatives, petitioning the government or lobbying elected officials. In systems that allow the use of direct democracy, such as the system in place in local governments in the state of California, citizens can *directly* express their policy preferences through the use of initiatives and referenda. Are strategic interactions more likely to occur in direct democracy?

In this paper, in a direct democracy context due to the relatively large number of tax and expenditure referenda at the county level in California, I add to the literature by investigating the extent of strategic interactions using a novel dataset of revenue and expenditure categories for all the 58 counties in California from 2003 to 2013. Specifically, I address the question of whether or not county governments influence one another in choosing their fiscal choices (spending and revenues). Though most of the empirical literature on the strategic interaction between neighboring jurisdictions use tax rates, a sizable part of the literature has argued that tax decisions are likely to be reflected in the budget constraints of local governments.

There are several reasons why counties might be influenced by the fiscal choices of their neighbors. First, strategic interactions might take place due to expenditure and revenue

mimicking, whereby counties imitate the expenditure and revenue patterns of their neighbors. Second, strategic interactions might be due to Yardstick competition, as citizens or voters take into account fiscal choices in neighboring jurisdiction in judging their elected officials. Third, due to spillover benefits or externalities, government spending and tax receipts in a jurisdiction might spread across multiple jurisdictions. In this paper, I do not make a distinction with respect to which mechanisms are more likely to be the case for county governments in California.

This paper contributes to at least two strands of the political economy and public finance literatures. From a political economy perspective, local referenda play an important role in fiscal decisions in California counties. Hence, given the direct democracy context in which this analysis is carried out, this paper is perhaps closely related to Isen (2014) who finds no revenue spillover effects analyzing local tax referenda for counties, municipalities and school districts in the US state of Ohio. From a public finance perspective, I add to a rather large literature testing for the effects of fiscal interactions across various levels of governments both within and outside of the U.S.

Overall, I have found strong evidence of fiscal spillovers at the county level in California. Specifically, with respect to revenue categories, I have found strategic interactions in revenues from sales, general and transient taxes. On the expenditure side, fiscal spillovers exist with respect to general, public protection, public facilities expenditures. However, reassuringly, there are no spillover effects with respect to health and public assistance expenditures; this latter finding can be attributed to the fact that those two expenditure categories are largely determined at the state level. In sum, the findings of this study are in line with an extensive empirical literature, finding that fiscal spillovers do exist.

This study has several policy implications related to the relationship between state governments and local governments in the U.S. In general, state governments are heavily involved in local education spending. There are likely to be benefit spillovers related to education expenditures, and, therefore, local governments might compete with one another along other expenditure dimensions. Also, specific to California, the state has heavy influence on local revenues through the property tax, state policymakers should be mindful that local governments interact with one another along other revenue dimensions.

The paper proceeds, in Section II, by describing the institution of direct democracy and public finance at the local level. The next section reviews the empirical literature. Section IV presents the conceptual framework. Section V describes the data. Section VI presents the empirical specification. Section VII presents the results. The last section concludes and discusses the results.

II. County Referenda and Local Public Finance

In this section, I provide a brief description of direct democracy at the local level. I also briefly describe county tax and expenditure referenda.

Most discussions regarding the institution of direct democracy take place at the state level in the U.S., most probably due to an increase in the number of controversial issues placed on the ballot over the last two decades. However, direct democracy is much more used at the local level than at the state level in the U.S. (Gordon 2004). In local governments throughout the U.S., the use of the initiative process is much more common than at the state level. Seventy (70) percent of Americans live in cities where the initiative process is available (Gordon 2004).

More generally, California has a long history with the institution of direct democracy, starting in 1911, right after Hiram Johnson had been elected governor. He had run on under the banner of the Progressive Party, attempting to limit the influence of special interests (Gordon 2004). California is at the forefront when it comes to the frequent use of direct democracy at the state level, as it is second behind Oregon in the number of initiatives placed on the ballot from when the first initiative appeared on the ballot in Oregon in 1904 to 2012.⁴

At the local level in California, all three levels of government (counties, school districts, cities) have put this institutional feature to frequent use. Citizens have made use of direct democracy, as the majority of the counties, cities and school districts at least several times a year between 2003 and 2013, the period under study. At the county level, the subject of this study, ballot measures are used to address issues of local concern such as urban growth boundaries, limiting the terms of their elected officials, establishing rent control, permitting gambling, imposing taxes for transportation or public safety, and reducing or repealing utility user taxes (Gordon 2004).

Furthermore, local governments such as counties, municipalities and school districts in California pass various types of tax referenda. To be approved, tax referenda require two-thirds of the vote at the county level, while there is variation in the requirements for the other two local governments (municipalities and school districts). I present below two examples of approved tax referenda as they appeared on the ballot in Amador County in 2008 and Marin County in 2006 in the hope of establishing the link between fiscal choices and local referenda.

⁴ Initiative and Referendum Institute: <http://www.iandrinstute.org/>. Accessed, January 2014.

AMADOR 11/4/2008 Measure M Pass (2/3 required)

Shall Amador County enact a one-half cent sales tax for fire protection and emergency response services, with the proceeds allocated to local fire districts as described in Ordinance No. 1676 (including reallocation to reflect any changes in the number of districts), to be used for paid fire fighter-emergency medical response personnel to staff existing fire stations, administrative support, and volunteer insurance, training and incentive programs?

MARIN 11/7/2006 Measure H Pass (2/3 required)

To continue emergency paramedic care, shall Ordinance No. 3458 be approved effective July 1, 2007, increasing the maximum special tax for paramedic services from a maximum of \$61.00 to a maximum of \$85.00 per year for each living unit, and from eight cents (\$0.08) to eleven cents (\$0.11) per square foot of structure of each non-residential structure?

As one can see from those two examples above, the wording from the wording of the language is unambiguous with respect to the purpose of the measure. This is important because there is a branch in the direct democracy literature that argues that voters are not competent to directly make fiscal decisions and are probably misled by the shrewdness of interest groups

through political campaigns. It would seem to me that the voters are well aware of the tax increase contained in the language of the two examples above. However, whether voters are myopic when it comes to the consequences of their fiscal decisions is up for debate.

From 2003 to 2013, the period under study, a fair number of tax measures were placed on the ballot. For example, in 2013, 39 revenue measures were placed on the county ballot. In 2012, an election year, 99 revenue measures were placed on the ballot. The numbers are somewhat similar for other years in period under study. Given the extensive use of county tax referenda, I deem it unnecessary to argue on a possible link between direct democracy and fiscal choices.

Similarly, with respect to expenditures, from 2003 to 2013, for example, on average, at least one expenditure measure was placed on the ballot. In 2012, 12 expenditure measures dealt with transportation, 23 with public facilities, 7 with housing and 11 with general government services. Given that the citizens themselves can make tax and spending decisions, is direct democracy more likely to lead to fiscal interactions? In other words, in addition to elected officials responding to the fiscal choices of neighboring jurisdictions, do the citizens do so as well?

III. Related Literature

The economics literature on the extent of fiscal spillovers is well-developed. Both within and outside of the U.S, most of the findings in the empirical literature favor the presence of strategic interaction across jurisdictions. However, there are a few dissenters, and this literature review is not meant to be exhaustive.⁵

⁵ For a review of the literature, see, e.g., Bruckner (2003) and Delgado et al. (2015).

At the outset, I shall briefly mention that there are several ways in which to examine the extent of strategic interaction in county governments. One way of doing so is by examining the expenditure side of the budget. Naturally, one can also look at the revenue side of the public budget by examining revenue categories. Finally, some studies have done so using tax rates. In this study, I consider the first two policy outcomes.

One of the first studies to test for the presence of fiscal interactions is Case, Hines and Rosen (1993), who find significant spillover effects with respect to expenditure levels at the state level in the U.S. Using U.S. state-level data from 1960 to 1988, Besley and Case (1995) find that states do engage in Yardstick competition, whereby voters pay attention to what is happening in neighboring states and hold their elected officials accountable. Figlio et al. (1999) find sizable welfare competition at the state level in the U.S. using panel data from 1983 to 1994. Brueckner and Saavedra (2001) find the presence of strategic interaction in property tax using data for cities in the Metropolitan Boston area. Other studies in the literature are Baicker (2005), Millimet and Rangaprasad (2006), to name a few.

Elsewhere, outside of the U.S., a number of studies have examined the extent of strategic interactions. Gerard et al. (2010) find the presence of tax interactions among Belgian municipalities. Dubois and Paty (2008) find that French cities engage in Yardstick competition. Buettner (2003) finds a modest impact of fiscal externalities using data for German municipalities. Using data for local governments in Spain, Sole-Olle (2006) finds the presence of strategic interactions at both expenditure levels and neighbors' populations and expenditures. Delgado et al. (2015) show support to the hypothesis of tax mimicking in local governments in Spain. Liu and Martinez-Vazquez (2014) find evidence of tax interactions across provincial governments in China.

Despite the somewhat extensive empirical literature testing for the presence of strategic interactions, given the role played by direct democracy at the county level in California, this dissertation chapter is perhaps closely related to at least two recent studies that have examined the extent of strategic interactions in a direct democracy context. First, Reback (2011) investigates the issues of strategic interactions with respect to direct democracy by comparing U.S. school districts with and without direct democracy, finding that strategic interactions is at least as large in representative democracy as in direct democracy. Yet, Isen (2014) who does so entirely in a direct democracy context examining school district, municipality county tax referenda, finding no presence of fiscal spillovers across all three levels of government.

In the end, whether one investigates the extent of fiscal spillovers between neighboring jurisdictions by looking at tax rates, expenditures or revenues, there is no clear prediction as to what one should expect when examining at the extent of strategic interactions exists in county and expenditures and revenues at the local level. In brief, it would seem that the contexts in which these studies are carried out seem to matter as much.

By and large, three different types of methodological approaches seem to have been used in the literature. One strand of the literature has used the GMM approach (Dubois and Paty 2008; Liu and Martinez-Vazquez 2014, among others). Another popular approach is to use as instruments the weighted average of the neighbors' control variables. This is the approach taken by, for instance, Case (1993), Besley and Case (1995). Third, the maximum likelihood approach has also been used (Bordignon et al. 2003, Besley and Case 1995, Allers and Elhorst 2005). A notable and interesting departure from the methodological approaches in the literature is Isen (2014), who use a regression discontinuity design, analyzing tax referenda that were passed and failed to be approved at around 50 % of the votes. Unfortunately, here, this is not feasible, as

county tax referenda in California require two-thirds of the vote to be successful.⁶ In this chapter, I use the first approach. In the near future, I plan to explore those alternative methodological approaches.

IV. Conceptual Framework

Very briefly, as with the empirical literature, the theoretical literature on the extent of fiscal interactions is well-developed. There are several hypothesis regarding fiscal interactions among neighboring jurisdictions. Fiscal interdependence or strategic interaction might take place due to benefit spillovers, externalities, tax mimicking and Yardstick competition. The presence of all these factors might be in place at the same time, and it is often quite difficult to distinguish among them. I therefore make no a priori judgment with respect to which mechanism (s) is likely to be relevant at the county level in California.

First, the theory of Yardstick completion, based on the work of Besley and Case (1995) and subsequently Shleifer (1995), conjecture that fiscal spillovers taka place because voters or citizens in one jurisdiction are likely to be cognizant of what is happening in neighboring jurisdictions and therefore use that information in their decision-making process, say, by holding elected officials in their own jurisdictions accountable. Due to the threat of re-election, incumbent office-holders might then try to find a way to avoid the unfavorable prospects in upcoming elections. Interestingly, given that direct democracy is allowed at the county level, citizens might be cognizant of the outcomes of tax and expenditure referenda that take place in neighboring jurisdictions and act accordingly in their own jurisdictions or use that information in

⁶ Reback (2011) is also a departure from the usual instrumental variable approach employed in the literature.

the next election. Does this additional power in the hand of the electorate make strategic interactions more likely?

Second, fiscal interactions across jurisdictions might be a story of externalities or benefit spillovers, whereby spending and revenue decisions in one jurisdiction spread to other jurisdictions. This is often the case because governments are often not able to confine their fiscal decisions to their own border. As with Yardstick competition, externalities or benefit spillovers might come from the electorate directly, as citizens are able to make spending and revenue decisions through referenda.

Further, fiscal interactions at the local level might arise from jurisdictions competing with one another in a Tiebout- type environment. In such an environment whereby people are free to move or *vote with their feet* in search of their most preferred location, jurisdictions often compete with one another through the bundles of public goods that they offer to their residents.

Besides Yardstick competition, externalities and benefit spillovers, mimicking behavior either through taxes, expenditures or revenues might be the driving factor behind fiscal spillovers. One can easily imagine a scenario whereby a jurisdiction follows the successful spending and revenue patterns of its neighbor(s). At the other extreme, county governments might just do the opposite of what their neighbors do, in which case one would expect a robust negative correlation in fiscal choices of neighboring counties.

In the end, though quite a few empirical studies have sought to disentangle the mechanism through which fiscal interactions occur, I find it appropriate to reiterate here that I make no a priori judgment with respect to which mechanism (s) might be likely to be relevant in the context of this study. That is, strategic interactions among local governments might be due all of the above mentioned reasons. Moreover, disentangling the appropriate mechanism is often

empirically challenging. This is even more so in the context of this study, as citizens themselves can directly impact fiscal decisions through local referenda.

V. Empirical Framework

My primary objective in this dissertation chapter is to gauge the extent to which fiscal spillovers exist at the county level in California with respect to revenue and expenditure categories. For simplicity, the notation for the empirical specification presented below is for revenues. *Mutatis mutandis*, the specification for expenditure categories is similar. Although there are some slight variations, the empirical methodology testing for the presence of strategic interactions is more or less straightforward.⁷ I therefore follow Deveraux et al. (2004), Sole Ollé (2006), Dreher (2006) and Foucault et al. (2009), assuming that a county's reaction function can be written down as follows:

$$2.1 \quad R_{c,t} = R_c (Rev_{j,t}, X_{c,t}),$$

where $Rev_{j,t}$ as is a vector of revenue categories in county c at time t . $Rev_{j,t}$ is the vector of revenues in the set of the other counties j ($j \neq c$) at time t , and $X_{c,t}$ is a vector of demographic and economic variables of county c at time t . Using the vector $Rev_{j,t}$ in an econometric specification, I have the following:

$$2.2 \quad R_{c,t} = \beta_1 Rev_{c,t-1} + \beta_2 W Rev_{j,t} + \beta_2 X_{ct} + \Phi_c + \eta_{tt} + \varepsilon_{ct},$$

⁷ For a review of the most common methodologies used in the empirical literature, see, e.g., Brueckner (2003).

where $R_{c,t}$ is the dependent variable for revenue categories, $Rev_{c,t-1}$ is a one-year lag of the dependent variable because of hysteresis in government fiscal choices, the variable of interest, $WRev_{j,t}$ is the weighted average of revenue categories of the neighbors of county c , X_{ct} is a vector of control variables such as median income, income per capita, unemployment rate and demographic variables, Φ_c is county-fixed effect. Ideally, I would want to include year effects, but due to the large number of instruments being created and issues of multicollinearity, following Devereux et al. 2004; Liu and Martinez-Vazquez 2014, I include a common time trend for all counties, η_{tt} .

Quite often, an important issue in spatial econometric analysis is the choice of a *weighting matrix*. There are several ways to specify the weighting matrix. One way might be through a contiguity matrix whereby a value of 1 is used if two counties share the same border and zero otherwise. Another way might be through the use of the Euclidian distance between two counties. Some studies have constructed the weighting matrix based the economic and demographic of the geographical units.

A priori it is nearly impossible to know the correct measure of neighborhoodness; therefore, in this paper, I assume that neighbors are counties that share the same border. However, given that specifying the degree of neighborhoodness is often arbitrary, ideally, one would like to use several approaches and investigate the sensitivity of the findings to choice of weighting schemes.

Following previous literature that attempts to determine the extent of fiscal interactions, to account for hysteresis in the composition of government own-source revenues and/or reverse causation, I include a lagged dependent variable on the right-hand side. At least two issues bear mention: the inclusion of the lagged dependent variable and the potential endogeneity of

approved tax referenda. To address those issues, I use dynamic panel techniques (Blundel and Bond 1988). Notwithstanding some caveat, this technique is extensively used in the literature and is well suited to the nature of the data in this third essay, given the relative short time dimension ($T=11$) and the relative large county dimension ($N=58$).

System GMM allows me to address the issue of endogeneity by specifying two (2) equations: one in difference and the other in levels. The set of equations in levels uses lagged first-differences as instruments, while the set of equations in first-differences uses lagged levels as instruments. Therefore, the first-difference of equation (2.2) is not correlated with the errors from that equation, thus making the endogenous variables pre-determined and uncorrelated with the ‘new’ error term in equation (2.2).

VI. Data

The data for this study were drawn from several sources. First, I hand-compiled a dataset of revenue and expenditure categories from the Counties Annual Report of the California State Controller’s Office from 2003 to 2013.⁸ Those annual reports contain detailed information on several government revenue and expenditure categories. For expenditures categories, I use general, health, public assistance, police protection and public facilities. For revenue categories, I use revenues from sales, transient and other taxes (minus revenues from intergovernmental transfer). However, revenues from the property tax, although in general terms a significant part of local governments’ budgets in most states in the U.S., are rightly excluded from the empirical analysis due to the fact that Proposition 13, passed in 1973, limits the amount of revenues that local governments can raise through the property tax. Data on tax and expenditure referenda

⁸ http://www.sco.ca.gov/ard_locrep_counties.html.

were obtained from the California Elections Data Archive (CEDA) at the Sacramento State Institute for Social Research.

Demographic variables such as the share of the county population that is white, Hispanic, Asian and African-American come from the California Department of Finance, which has detailed data on county population estimate by race and age in all the 58 counties. Following previous literature investigating the extent of strategic interactions, I include a control variable for poverty from the Census such as the percentage of the county population under the poverty level. County-level unemployment rates come from the Bureau of Labor Statistics. Finally, I use county-level per capita income and median household income from the Bureau of Economic Analysis and the percentage of democratic registered voters from the website of the Secretary of State of California. Table 2.1 presents summary statistics of the data used in this study.

VII. Results

A. Government Expenditures

In this section, I present the results for the expenditure categories, and, to describe the findings, I interchangeably use the terms strategic interactions or spillover effects, as I have not explicitly made the case as to which mechanisms are likely to be relevant in the context of this study. Columns (1) and (2) of Table 2.2 presents the results for general government expenditures, and there is a high degree of correlation between general expenditures of neighboring counties. The point estimates are highly significant. Regarding how general spending from the previous year is related to spending in the following year, the point estimates are significant as well. Overall, this evidence suggest that county decision-makers, either citizens through referenda or elected officials, react positively to what their neighbors do.

In Table 2.3, I present the results for the public protection expenditures. Similar to general expenditures, there seems to be strong evidence of strategic interactions or spillover effects. The point estimates are significant at the 1% level, indicating strong evidence of spillover effects or strategic interactions. If one were to speculate on the nature of this finding, it might be because an increase in police protection in one jurisdiction acts as a deterrence. As a result of negative externalities, neighboring counties feel compelled to increase expenditures on police protection within their own borders.

In Table 2.4, the results for expenditures on public facilities indicate a strong evidence of spillover effects or strategic interactions. One likely explanation for this finding is that local public facilities such as public parks and museums are often a local matter, and citizens can freely move to enjoy such benefits in a Tiebout type of environment where people vote with their feet, choosing their most preferred bundles of amenities. Neighboring counties respond accordingly.

Finally, in Tables 2.5 and 2.6, I present the result for public assistance and expenditures, respectively. As we can see, the point estimates are not significant for either expenditure category. Interestingly, the point estimate for health expenditures is positive, while the one for public assistance is negative. Further, in terms of correlation between health and public assistance from the previous period. That there does not seem to be evidence of strategic interactions with respect to health and public assistance expenditures is reassuring for the overall findings in this section, given that health and public assistance expenditures are often determined at the state level, not at the county level.

Thus far, the foregoing analysis shows strong evidence indicating that counties in California engage in fiscal interactions expenditure categories such as general expenditure,

public facilities, and public protection expenditures. There is no evidence of fiscal interactions with respect to health and public assistance expenditures. Next, I turn to revenue categories.

B. Government Revenues

In this section, I attempt to shed light on spillover effects or strategic interactions from the revenue side of the public budget. As one can see from columns (1) and (2) of Table 2.7, it would seem that there is a high degree of fiscal interactions with respect to sales tax revenues. One likely explanation for this finding might have to do with the fact that county governments are restricted in amount of revenues that they can raise through the property tax due to Proposition 13; therefore, local governments turn to sales and use tax. Cross-border issues with respect to the sales tax are likely to be relevant within a state, as citizens can easily go to an adjacent county for their shopping needs. However, using data for a subset of large U.S. counties, Ladd (1992) finds no presence of strategic interactions in sales tax burden.

Table 2.8 presents the results for revenues from transient tax and license tax. Recall that transient taxes are usually levied on hotels and motels (therefore on non-residents), one would therefore expect some such as tax on motels and hotels do not seem indicate any significant impact of tax referenda on change in revenue composition. One possible explanation for the findings with respect to transient tax might very good candidates to either use for competition or mimicking purposes given that the tax generally falls on non-resident. Also, the lagged variable is always positive, but only significant in Column (2). This is in contrast to the lagged values for sales tax revenues, which are always significant.

In Table 2.9, I present the findings for revenues from other taxes. Recall that other taxes exclude property tax and the other two revenue categories mentioned above (sales tax and

transient tax). Recall that revenues from other taxes exclude revenues from intergovernmental transfer from the state or the federal government. As we can see from columns (1) and (2) from Table 2.9, there is strong evidence of strategic interactions with respect to other tax revenues, as the estimated coefficients are highly significant. Also, the significance of the point estimates for the lagged variable implies that other tax revenues change slowly from year to year. Overall, the strong evidence of strategic interactions related to revenues at the local in California may have to do with the fact that Proposition 13 limits the amount of revenues that be raised through the property taxes, county governments therefore compete with one another along other revenue dimensions.

To summarize, in this section, I have presented evidence indicating that counties in California engage in fiscal interactions through sales, transient and other tax revenues.⁹ Interestingly, given the extensive use of local referenda in California counties, the findings with respect to revenue categories are in contrast to, say, Isen (2014), who finds no spillover effects, analyzing in local tax referenda in Ohio. However, the findings are in line with a long line of research identifying the presence of strategic interactions with respect to either taxes, government revenues and expenditures both within and outside of the U.S., such as Hines and Rosen (1993), Besley and Case (1995), Figlio et al. (1999), Brueckner and Saavedra (2001), Buettner (2003), Baicker (2005), Millimet and Rangaprasad (2006), Sole-Olle (2006), Dubois and Paty (2008), Gerard et al. (2010).

⁹ Just as the findings of no fiscal interactions with respect to health and public assistance expenditures serve in some sense as a robustness check for the expenditure side of the budget, with respect to revenue categories, perhaps it might be useful to show that there are no fiscal interactions related to property tax revenues, as we should probably expect, given that counties are generally limited in the amount of revenues that they can raise through property tax.

VIII. Conclusion

The purpose of this second dissertation chapter has been to shed light on the extent to which fiscal spillovers exist at the county level, analyzing county expenditure and revenue data in California from 2003 to 2013. One noteworthy aspect of this study is that fiscal decisions in California counties are often made through the use of tax referenda. Past theoretical models have hypothesized on a number of ways strategic interactions might arise. First, strategic interactions might take place due to expenditure and revenue mimicking, whereby counties simply replicate or imitate the expenditure and revenue patterns of their neighbors. Second, strategic interactions might be due to Yardstick competition, as citizens or voters take into account fiscal choices in neighboring jurisdiction in judging the performance of their elected officials. Third, due to spillover benefits or externalities, fiscal decisions in one jurisdiction might spread to other jurisdictions.

Overall, I have found a high degree of fiscal interactions among neighboring jurisdictions. With respect to expenditure categories, general expenditures, public protection, public facilities expenditures show evidence of fiscal spillovers. On the tax side, revenues from sales, transient and other taxes exhibit a high degree of fiscal interactions as well. Overall, with very few exceptions, the findings of this dissertation chapter are in line with most studies in the literature.

I acknowledge some shortcomings of this study. One possible limitation of this study and by extension of a sizable strand of the fiscal interaction literature is that there is a theoretical literature that hypothesizes that government expenditures and revenues are interdependent by nature (Meltzer and Richard 1981). That is, investigating strategic interactions by analyzing

separate equations for expenditures and revenues might not tell the whole story. Hence, one possible avenue for further research might be to analyze the two sides of the budgets together.

Also, although this study is carried out in the context of direct democracy, in the sense that the extensive use of county referenda is likely to directly impact fiscal choices, I have not explicitly made the case that the strong evidence of strategic interactions are due in large measure to the local initiative. Finally, there are methodological limitations as well. Specifically, as mentioned in section V, there are several ways to carry out the empirical methodologies, and it might be appropriate to use the instrumental variable approach used in the literature, given the multicollinearity issues with the GMM approach. Also, somewhat relatedly, future research might gauge the robustness of the findings using different weighting matrix schemes.

Chapter III Impact of Alternative Education Programs on Juvenile Crime

I. Introduction

School suspension and school expulsion, broadly defined as time students spend out of school for disciplinary problems, have been a concern in the U.S. for at least the last two decades. More than one hundred thousand students were expelled from schools in the 2004-2005 school year (Carroll 2008). Many of the expelled and suspended students do not have access to public education and will not return back to school (Carroll 2008). Unfortunately, this issue has been hard to study because there is a dearth of datasets that tracks school suspension and expulsion overtime. School suspension in New York is typical of many places in the country: “The total number of suspensions in New York City grew at an alarming rate over the last decade: One out of every 14 students was suspended in 2008-2009; in 1999-2000 it was one in 25. In 2008-2009, this added up to more than 73,000 suspensions.”¹⁰ Furthermore, as Losen and Skiba (2013, p.3) report, “A review of national suspension rates since the early 70’s for K-12 public schools reveals a substantial increase in the use of suspension for students of all races, as well as a concomitant increase in the racial discipline gap.”

Two relatively recent federal programs related to education are believed to have adversely impacted school suspension and school expulsion. First, the Gun-Free School Zones Act (1994), passed by congress to prevent gun violence in schools, has led school districts to

¹⁰<http://www.nyclu.org/publications/report-education-interrupted-growing-use-of-suspensions-new-york-citys-public-schools->. Accessed, October 2013.

implement various zero tolerance policy laws, allowing schools to expulse students for various school violations, even for first-time and minor school transgressions.¹¹ As recently as March 6, 2012, the *New York Times* reports, “In recent decades, as more districts and states have adopted zero tolerance policies, imposing mandatory suspension for a wide range of behavioral misdeeds, more and more students have been sent away from school for at least a few days, an approach that is often questioned as paving the way for students to fall behind and drop out.”

Second, certain aspects of the federal No Child Left Behind Act (NCLBA) of 2001 are likely to have exacerbated this increase in school expulsion. The NCLBA constrains schools districts across the nation to comply with the federal requirement that all of their students reach 100% proficiency in both reading and math, as measured by standardized test scores by 2014. Schools that fail to make adequate progress toward satisfying the federal requirement may face consequences, such as school closing, school district board intervention, or students moving to other schools. One way of avoiding some of the harsh consequences of the NCLBA might be to *exclude* low performing students from standardized test scores through an increase in school suspension and expulsion, thereby increasing average school test scores to meet the federal requirement (Carroll 2008). Moreover, Figlio (2006) presents evidence showing that schools in Florida *do* engage in disciplinary practices in order to increase aggregate test performance and therefore avoid punishment from school districts.

School suspension and expulsion have been mentioned as contributing factors to an increase in the so-called school-to-prison pipeline.¹² According to a recent report from the New

¹¹ To quote Carroll (2008), “An antidrug zero tolerance policy led a high school to expel a student for possession of Advil, an over-the-counter pain reliever. At another school, a boy was expelled for landing his inhaler to his girlfriend when she had an asthma attack. A fifth grader was expelled after telling a teacher about the paring knife that her mother accidentally put in her lunch box.”

¹²See the editorial of the *New York Times* (May 30, 2013).

York City School-Justice Partnership Task Force, school suspension and expulsion have led to an increase in the arrests of school age children, as they are reported to the authority for charges for which “there is often no underlying criminal behavior.”¹³ Further, a recent report by the Florida-based advocacy group, the Children’s Campaign, indicates, “In 2008, there were over 1.6 million youth whose cases were disposed in juvenile courts across the country.” Therefore, as more students are being suspended, expelled and referred to the authorities, more students spend more time out of school. There is an economics literature linking physically being out of school to juvenile crime (Witte and Tauchen 1994; Lefgren 2003; Luallen 2006).

Due to concern over the impact of school suspensions and expulsions, between 1987 to 2010, several U.S. states adopted policies that mandate school districts to establish alternative education programs for expelled and suspended students. Using county juvenile arrest data from the FBI’s Uniform Crime Reports and state homicide victimization and homicide offending data from the FBI’s Supplemental Homicide Reports (SHR), I estimate the impact of state-mandated alternative education programs for suspended and expelled students on juvenile crime. More specifically, using the county and state panel data, along with an extensive set of economic and demographic control variables, I conduct difference-in-differences and event study estimations of the effect of state alternative education mandate on juvenile crime.

School expulsion and suspension might lead students to engage in undesirable societal behavior, including school dropout, low academic achievement, and juvenile crime and incarceration. Proponents of state-mandated alternative education programs argue that those programs might mitigate some of those perverse incentives, helping students with their school assignments so that they do not fall behind when they are reinstated in school. Moreover,

¹³<http://www.nycourts.gov/ip/justiceforchildren/school-justice.shtml#NYCpartnership>. Accessed, October 2013.

education is correlated with a host of good and desirable societal behavior (Usher 1997; Lochner 2004, 2010).

This paper contributes to the existing literature measuring the impact of contemporaneous schooling on crime in at least the following two important ways. First, I use the state mandate as a proxy for school attendance for the suspended and expelled students and estimate its impact on juvenile crime. Therefore, this essay is closely related to the literature on contemporaneous schooling on crime, such as Jacob and Lefgren (2003), Luallen (2006), and Akee et al. (2014), among others, who estimate the impact of *physically* being out of school on juvenile crime. Second, this essay is also related to the literature examining the impact of human capital accumulation on crime, as students in states with alternative education mandate might not drop out of the school system. Thus, alternative education might not only affect juvenile crime in the short term by facilitating school-age children to remain in school during the suspension period, but also in the long term through an increase in school completion or a prevention in school dropout, thereby increasing future wages.

In what follows, using county crime data, I present the findings of this study, indicating that state-mandated alternative education programs for expelled and suspended students seems to have no significant impact on juvenile crime (total crime, violent crime and property crime and several types of violent and property crime). Most of the point estimates of the effects of those programs are negative. This seems to persist under alternative econometric specifications.

However, using state level homicide offending and victimization data for several age groups and by race, I find no evidence of any significant effect of those programs on either juvenile homicide victimization or homicide offending rates of all youth between the ages of 12 and 17, except for black youth between the same ages. This finding might be due to the fact that

African-American students are suspended from school at a higher rate than their white counterparts. In fact, as the *Washington Post* reported in March 2014, “In the 1972-73 school year, suspension rates were 6 percent for whites and 12 percent for African-Americans at the secondary school level. The most recent federal figures, for 2009-2010, show rates of 7 percent for whites and 24 percent for African Americans in those grades.” That is, black students are now over three times more likely than white students to be suspended from schools (Losen and Skiba 2013). This finding is largely in line with the *incapacitation effect* of alternative education programs, whereby students that are enrolled in those programs cannot be in the streets during the period of instruction.

The remainder of the paper is organized as follows. Section II provides background information on state-mandated alternative education programs. Section III outlines the expected effects of state-mandated alternative education programs on juvenile crime. Section IV presents the econometric specification. Section V describes the data and summary statistics. Section VI presents the results. The last section concludes and discusses the results.

II. State-mandated alternative education programs

This section briefly describes how alternative education programs work. In fact, there are several types of alternative education programs, such as alternative education programs targeted to students with disabilities. However, the main focus of this essay is related to “disciplinary alternative education programs for at-risk students who have disciplinary problems” (Texas Education Agency 2007). Alternative education programs for suspended and expelled students have two primary goals. “The first goal is to provide temporary student placements for behavior management, often as alternatives to suspension and expulsion. The goal is for students to return

to, and succeed in their regularly assigned classroom” (Texas Education Agency 2007). The second goal is to help facilitate the reinsertion of expelled and suspended students in the school environment, as they do not fall behind in their academic studies upon returning to school.

In general, the functioning of state-mandated alternative education programs varies by state. For example, in California, every expelled student has the right to an education. Upon expulsion, the school district must provide educational programs to the expelled students for the entire period of expulsion. In addition, “A school district has discretion to use alternatives to suspension and expulsion, such as counseling, anger-management programs, and community service during non-school hours.” Delaware and Illinois serve students in grade 6-12, with some 5th graders as well. The Mississippi law applies to “Any compulsory school age child, who has been suspended for more than ten (10) days or expelled from school, except for any students expelled for possession of a weapon or other felonious conduct.” The Nebraska law applies to “all expelled students.” In North Carolina, “Some districts try to serve all grade levels, but the usual grade spans are 6-12 and 9-12.” Rhode Island law §16-21-27 requires that “Each school district shall adopt a plan to ensure continued education of students who are removed from the classroom because of a suspension of more than ten (10) days or who are chronically truant.” Overall, most states which mandate alternative education programs allow those programs to serve students in higher grade level, students who are more likely to drop out of the school system (Carroll 2008). California and Texas have two of the most comprehensive alternative education programs, requiring school districts to keep detailed record of students enrolled in an alternative education program.

In the states that mandate alternative education programs, school districts are required to have alternative education programs for expelled and suspended students, and, in some cases,

students may chose not to participate. In Texas, for example, expelled and suspended students are required to take part in alternative education programs (Texas Education Agency 2007). It is not clear whether students have to participate in some other states. Lacking this information, the main focus here is on *the right to education* of the expelled and suspended students in the states with alternative education mandate. But there is diversity across states. For example, in Utah, the law makes it expressly the responsibility of parents to educate their children if they are suspended or expelled from school.¹⁴

In most states which mandate alternative education programs, to get information on the number of students enrolled in alternative education programs, one would need to contact each school district or school system. For example, in North Carolina, even though alternative education programs have been in place since at least before 1993, I was told, only very recently has the state begun tracking the progress of those programs. However, given the increase in size in the number of students expelled and suspended over the years at the state level, one can fairly assume that a sizable number of those students found themselves in alternative environment settings across the country.

To my knowledge, there is not that much information regarding the number of students enrolled in alternative education programs, given that those programs are administered mostly at the local level. There is no empirical evidence on whether alternative education programs are effective in preventing juvenile crime. Alternative education programs for expelled and suspended students might exist in school districts without the state mandate. For example, Florida does not mandate alternative education programs at the state level, but a number of school districts have alternative education programs for expelled and suspended students.

¹⁴ Utah Code Section 53A-11-907.

However, in the school districts in the states without alternative education mandate, it is probably more a matter of local school board discretion whether or not expelled students are allowed to be enrolled in those programs.

III. Related Literature and Conceptual Framework

The economics literature measuring the impact of alternative education programs on juvenile crime is practically non-existent. To the best of my knowledge, this is the first paper that empirically measures the impact of state-mandated alternative education programs for expelled and suspended students on juvenile crime. However, there is a well-developed economics literature on the impact of educational attainment on crime (Lochner and Moretti 2004; Hjalmarsson 2008; Buonanno and Leonida 2009; Machin, Marie, and Vujic 2011), school quality on crime (Cullen, Jacob and Levitt 2006; Weiner, Lutz, and Ludwig 2009; Deming 2011), and school attendance on crime (Jacob and Lefgren 2003; Luallen 2006; Anderson 2012).¹⁵

Lochner and Moretti (2004) examine the impact of educational attainment on crime rates in states in the U.S., using changes in state compulsory schooling laws as instruments for education, finding the probability of incarceration decreases with schooling; Oreopolous and Salvanes (2009) find an additional year of schooling leads to a decrease in the incarceration of black males by 20% in U.S. states.

The impact of contemporaneous education on crime has been studied by Anderson (2012), using state-level variation in the minimum drop out age to find a negative impact of contemporaneous education on both violent and property crimes at the county level from 1990

¹⁵ See Lochner (2010) for an excellent review of this literature.

to 2008 in the U.S.; Jacob and Lefgren (2003) present evidence on how teacher in-service days leads to an increase in violent crime and a decrease in property crime in selected cities in the U.S.; Luallen (2006) finds school closing (teacher strikes) increases violent crime and decreases property crime in the state of Washington.

As noted above, this is the first study to estimate the impact of state-mandated alternative education programs for suspended and expelled students on juvenile crime. However, there is a well-developed theoretical and empirical literature linking time out of school to criminal activities. In light of both the findings and the predictions of that literature, a priori it is uncertain regarding both the sign and the magnitude of what the eventual impact of those programs on juvenile criminal activities may be. Past theoretical models have identified several channels through which education attainment and/or contemporaneous education might have an impact on criminal activities. The first channel might be through the *incapacitation effect*, whereby students that are enrolled in alternative education programs cannot be engaged in both out-of-school criminal and in-school educational activities at the same time. In that regard, alternative education programs could probably be effective in preventing expelled students from engaging in criminal activities, as students that are enrolled in those programs would be off the streets. Jacob and Lefgren (2003) and Luallen (2006), for example, find that time students spend out of school leads to an increase in property crime in Washington and selected cities in the U.S.

The second channel through which state-mandated alternative education programs might impact juvenile criminal activities is related to *human capital accumulation* (Becker 1964; Ben-Porath 1967). In particular, human capital accumulation increases employment prospects (Lochner 2004). With respect to alternative education programs, human capital accumulation would be helping students during the period of suspension and expulsion, possibly preventing

school dropout. In contrast to the short term impact of the incapacitation effect of contemporaneous schooling on juvenile crime, the human capital effect would take place in the long term, as alternative education programs may help increase high school completion and years of schooling, which, in turn, would lead to an increase in future labor market wages and a decrease in future criminal activities [Samson and Laub 1993, 1997]. State-mandated alternative education programs have been in place long enough to influence juvenile criminal activities through human capital accumulation. Both the *incapacitation* and the *human capital* effects suggest that alternative education programs should lead to a decrease in juvenile crime

Third, alternative education programs for expelled and suspended students may impact juvenile criminal activities through social *interaction* (Glaeser et al. 1996, 2003). That is, at-risk students might misbehave in an alternative education environment while interacting with other students, but once removed from the school environment through suspension or expulsion, those at-risk students might no longer misbehave. A forecast based on theoretical models of social interaction would therefore imply that state-mandated alternative education programs should have no significant effect on juvenile criminal activities. This theoretical prediction would seem to be supported by anecdotal evidence suggesting that most students that are being expelled or suspended from schools are not criminals per se given that the infractions for which they are often expelled may be many times trivial or at least no major offense.

Fourth, another closely related facet to social interaction is the *concentration effect* of contemporaneous schooling, whereby school interaction serves as a gathering place for delinquent students to plan their engagement in criminal activities. A rather large and related empirical literature on the influence of peer effects on juvenile behavior supports this hypothesis. Given that alternative education programs in general serve students that are expelled and

suspended from school and thus have a track record of delinquent behavior, the *concentration effect* of those programs, combined with peer pressure, would probably lead to an increase in juvenile criminal activities. Moreover, as Anderson (2014) points out, “Keeping children in school increases the number of potential interactions that facilitate delinquency, especially physical altercations.” In that regard, for example, both Jacob and Lefgren (2003) and Luallen (2006) report that the time students spend *in school* actually leads to an *increase* in violent crime in selected US cities and cities in the state of Washington, respectively.

Ultimately, then, the *incapacitation*, *human capital accumulation* and *social interaction* and/or *concentration* effects of contemporaneous schooling imply that the impact of state-mandated alternative education programs on juvenile crime at least from a theoretical perspective is ambiguous. On the one hand, the incapacitation and the human capital accumulation effects suggest that alternative education programs should lead to a decrease in juvenile crime, while, on the other hand, the social interaction/and concentration effects suggest either an increase or a decrease or no effect of state-mandated alternative education on juvenile crime. This theoretical ambiguity is further complicated by the fact that detailed information over time on the characteristics of the expelled students is not available. Therefore, in light of this brief overview, the impact of state-mandated alternative education programs for expelled and suspended students on juvenile crime is mostly an empirical question, which I address next.

IV. Empirical Specification

In the empirical estimation, I adopt two complementary strategies. First, I use a standard difference-in-differences methodology where I use a dummy indicator for the year the state mandate went into effect and the years afterwards. Second, I adopt an event-study methodology where I relax the assumption of the difference-in-differences, allowing the state mandate to have

an impact on juvenile crime five years after its implementation. This second approach is justified by the fact that those programs are administered at the school district level; hence, school districts administrators might have different timeline in terms of when they implement those programs. With the difference-in-differences, I assume that the impact of the state mandate on juvenile crime is static; however, alternative education programs might take a while to be put in place, especially at the school district level. Initial effects might be large and then disappear. That is, short run effects may differ from long run effects.

A. Difference-in-Differences

First, to measure the overall impact of state-mandated alternative education programs on juvenile crime at the county level, I specify the following difference-in-differences model:

$$(3.1) \quad y_{cst} = B_1 Alt_{st} + \beta_2 X_{cst} + \sigma_c + \theta_t + \varepsilon_{cst},$$

where y_{cst} indicates the outcome of interest, juvenile arrests rate per 100,000 of the juvenile population in county c in state s at time t , X_{cst} is a set of county-level economic and demographic variables that are commonly used in the economics literature linking education to criminal activities, such as median household income, income per capita, average earnings per job, unemployment rate, and the percentage of the population under the poverty level;

σ_c and θ_t are, respectively, county fixed-effects and year effects. Specification 1.1 includes county fixed effects to account for fixed county characteristics that may correlate with juvenile crime activities; that is, differences between different types of counties or time-invariant

measures related to education policies at the county level will not confound the estimates. The year fixed effects account for aggregate time-varying shocks such as economic conditions and changes in education policies at the state and/or the national level.

B_1 , the coefficient of interest, is a dummy indicator measuring the impact of state-mandated alternative education programs for expelled and suspended students on juvenile crime. Throughout the empirical analysis, I cluster the estimated standard errors at the state level to account for serial correlation, within-state spatial correlation and heteroskedasticity. The basic assumption for identification of Equation 3.1 is that of a parallel trend in the outcome variable, juvenile crime. That is, without state alternative education mandate, juvenile crime in counties in states with and without the mandate would have followed a similar trend. Endogeneity of alternative education mandate might lead to violation of this assumption. However, based on the wording of the laws, it does not appear that juvenile crime was the primary concern for the implementation of alternative education mandate. In addition, the event study methodology presented below provides support to the notion that endogeneity of the state mandate is not a concern in this study.

B. Event Study Analysis

Second, to flexibly estimate the impact of alternative education mandate on juvenile crime, I specify the following event study model:

$$(3.2) \quad y_{cst} = \sum_{\sigma=-2}^5 B_{\sigma}(Alt_{\sigma,cst} = 1) + \gamma X_{cst} + \theta_j + \alpha_t + \varepsilon_{cst},$$

where y_{cst} is the outcome of interest for county c in state s at time t ; θ_j and α_t are county and year fixed-effects, respectively; X_{cst} is a vector of county-specific characteristics, $Alt_{\sigma,cst}$ is an indicator variable equaling one in the year that state s mandates that schools districts or school system establish alternative education programs for expelled and suspended students. $Alt_{4,cst}$ means that county c is in a state that mandated alternative education programs 4 years ago, while $Alt_{-4,cst}$ means that county c is in a state that will mandate alternative education programs 4 years later. The year prior to the state mandate is the omitted category. The indexing from $\sigma = -2$ to $\sigma = 5$ indicates state-mandated alternative education programs are analyzed here five years pre and post-policy. Specification (3.2) allows one to see how juvenile crime evolves leading up to the state mandate, and how they change and evolve following the state mandate.

In some estimations of the model, following Anderson (2014), I also include a set of state specific linear trends to control for time series variation within each state. The X_{cst} is a vector of the usual variables that past researchers have controlled for in the economics literature measuring the impact of education on crime, such as county level median household income, unemployment rate, income per capita and the percentage of the population under the poverty level, among other variables.

The coefficients of interest, the B_{σ} vector, are identified under the assumption that, in the absence of state alternative education mandate, juvenile crime would have a similar trend in counties in the states that mandated alternative education programs for expelled and suspended students at different times. As is common in the economics literature measuring the impact of contemporaneous schooling on criminal activities such as in Anderson (2012), with Specifications 3.1 and 3.2, I am not capturing nuances in the laws regarding state alternative

education mandate but the overall and dynamic impact of the mandate on juvenile crime, respectively. For example, there is variation in the laws regarding what happens to expelled students who commit infractions while already being placed in an alternative education environment.

V. Data and Descriptive Statistics

To carry out the analysis, I use three main sources of data: information on the dates when state mandated-alternative education programs were put into place, county level juvenile arrest data, and state level homicide victimizations and offending data. For the first source of data, I have collected the dates of alternative education mandates by e-mail correspondence with school officials from the departments of education in all the 50 states in the U.S. All the e-mails are available upon request. In the e-mails that I sent requesting information on alternative education programs, I ask the following question: “*Does your state have an alternative education program for expelled and/or suspended students, and, if so, has there been any change over the years in the implementation of the program?*” I have all the dates the following states mandate that school districts or school systems establish alternative education programs for expelled and/or suspended students [California (1995), Colorado (1997), Delaware (1994), Hawaii (2005), Illinois (1996), Minnesota (1987), Mississippi (2005), Nebraska (1997), New York (2004), North Carolina (<1993), Oregon (2001), Rhode Island (2004), Tennessee (1992) and Texas (1995)].¹⁶ I exclude the following states because they have mandated alternative education

¹⁶ **California:** CAL. EDC. CODE EC §§ 48916.1, 48915(f). **Colorado:** e-mail correspondence, “Statute requires school districts to offer to parents of expelled students the available options for alternative education while expelled.” **Connecticut:** Public Act 10-111, amending subsection (g) of the Connecticut General Statutes, by adding the following provision effective July 1, 2010; “Suspension pursuant to this section shall be in school...” **Delaware:**

programs only very recently: Connecticut (2010), Louisiana (2012), Massachusetts (2014).

California and Colorado are the only states that explicitly make the distinction between expelled and suspended students, allowing alternative education programs only for expelled students.

Under California law, for example, school districts have the prerogatives to provide educational services to suspended students. Unfortunately, most states do not keep track of the number of expelled and suspended students enrolled in those programs.

For the second source of data, the main variable of interest is juvenile crime or arrests at the county level. Counts of juvenile arrests at the county level were collected through the detailed arrest county-level data provided by the FBI's Uniform Crime Reports and were gathered from the National Archive of Criminal Justice Data (NACJD), part of the Inter-University Consortium for Political and Social Research at the University of Michigan.

I use juvenile arrests for all counties in the U.S. from 1997 to 2010. Juvenile arrests are not available for the state of Florida, and limited data are available for Illinois. Those two states are therefore excluded from the analysis. In Table 3.1, among other variables, I present summary statistics for juvenile arrest rates for total crime and several crime categories from 1997 to 2010

<http://delcode.delaware.gov/sessionlaws/ga137/chp464.shtml>. **Hawaii:** <http://www.hawaiiiboe.net/policies/2100series/Pages/2131.aspx>. **Illinois:** 105 ILCS 5/13A-0.5. **Louisiana:** <http://legis.la.gov/lss/lss.asp?doc=81034>. **Massachusetts:** (Chapter 222, of the Acts of 2011; or "Law"), the "Law" takes effect on July 1, 2014. **Minnesota:** 124D.68. **Mississippi:** section 37-13-92 of the Mississippi Code. **Nebraska:** STATUTORY AUTHORITY 79-318, 79-266; CODE SECTION 001-005. **North Carolina:** e-mail correspondence "Yes, all school districts are required to have alternative education for at-risk students. Alternative schools go back at least twenty years, but state standards for ALPs are less than ten years old. This usually includes services for expelled and suspended students." **Oregon:** ORS 336.615. **Rhode Island:** R.I.G.L. 16-21-27. **Tennessee:** "In 1992, the General Assembly mandated that one alternative school be established for each local school district to serve suspended and expelled youth." **Texas:** e-mail correspondence "Yes Texas has a state policy that all districts must have a Disciplinary Alternative Education Program in place. Legislation was passed in 1995. Ages 6 and up."

per 100,000 of the at-risk population.¹⁷ The juvenile arrest data are reported by agency within a county for several types of crimes, classified as either violent crimes (murder, rape, robbery, and aggravated assault) or property crimes (burglary, larceny, motor vehicle theft and arson). Counts of juvenile arrests are aggregated at the county level. Since reporting is voluntary, counties with a small number of people tend to report zero count of juvenile arrests. Hence, in those cases, it is impossible to distinguish a true zero from a non-reporting zero. However, the data were distributed with a coverage indicator, which “provides a diagnostic measure of aggregated data quality in a particular county.”

One important limitation in using juvenile crime data in general is that the FBI does not distribute *reported* crime data for juveniles. Only when the police have made an arrest do we have information on juvenile arrests. I therefore follow the literature by using juvenile arrests as a proxy for juvenile crime (e.g. Anderson 2012; Larsen 2013). Admittedly, this is not a perfect measure of juvenile criminal behavior; however, using the UCR data, Lochner and Moretti (2004) find a very high correlation between arrests and crimes committed (Anderson 2012).

I control for county level unemployment rate from the Bureau of Labor Statistics, median household income and various county-level poverty estimates such as the share of the county population between the ages of 10 and 17 and total county population under the poverty level, real income per capita and county average earnings per jobs from the Bureau of Economic Analysis.

Demographic control variables such as the percentage of the black population that is between the ages of 10 and 17 come from the National Center for Juvenile Justice of the Office of Juvenile Justice and Delinquency Prevention (OJJDP).

¹⁷ Puzzanchera, C., Sladky, A. and Kang, W. (2013). "Easy Access to Juvenile Populations: 1990-2012." Online: <http://www.ojjdp.gov/ojstatbb/ezapop/>. Accessed, January 15-19, 2014.

As already mentioned, one shortcoming in using the county juvenile arrest data is that there is a high incidence of counties that report zero count of juvenile arrests. I therefore turn to other sources of data such as state level homicide victimizations and homicide offending from the FBI's Supplemental Homicide Reports (SHR). Even though most empirical studies in the literature investigating the impact of education in general on juvenile criminal activities make use of counts of juvenile arrest from the FBI's Uniform Crime Series, Weiner et al. (2009) use homicide offending and victimizations from the SHR to estimate the effect of school desegregation on crime, arguing that the FBI's Uniform Crime Reports have a great deal of measurement error.

Alternatively, then, I investigate the impact of the state mandate using state level crime data from the SHR to capture information on juvenile homicide victimizations, and, when police have made an arrest, homicide offending. Homicide victimizations and offending data for more than 7 years are missing for Florida, Kansas, Montana and Nebraska; those four states are not therefore included in the analysis. I control for a rich set of state level variables such as unemployment rate, income inequality, income per capita, median income, state population, and poverty and minimum wage rates.

Table 3.2 shows summary statistics for the state crime data that I use in the analysis. It consists of 46 states, excluding the four states mentioned above due to missing data on homicide offending and victimizations for more than 7 years over the study period (1994-2010). Homicide offending rates for all juveniles aged 12-17 and for black juveniles aged 12-17 are, respectively, 12.24 and 17.44, per 100,000 of the corresponding at-risk population. That is, for instance, 17.44 is the homicide offending rate for black youth between the ages of 12 and 17. Similarly, homicide victimization rates to youth 12-14, 15-17 and their black counterparts are 1.07, 5.40,

2.78 and 19.23 per 100,000 for the respective at-risk population. As one can see from Table 2, both homicide offending and homicide victimization rates are higher for black youth.

VI. Results

Table 3 shows the main results of this study using the difference-in-differences Specification 1.1. The dependent variables are county-level juvenile arrest rates per 100,000 of the juvenile population that is between the ages of 10 and 17. The baseline model includes a set of county and year fixed effects along with a set of demographic and economic control variables. Columns (1) through (3) show the findings for total crime, violent crime and property crime, respectively. As one can see from Table 3, the point estimates are negative across all three dependent variables, but none of them are significant. As a robustness check, columns (4) through (6) of Table 3 include a set of state specific linear time trends to account for time series variation within each state, and the point estimates are somewhat similar to the ones from columns (1) through (3), except for violent crime which, though still insignificant, becomes positive. Overall, state-mandated alternative education programs do not seem to have a significant impact on juvenile crime.

In this section, as is common in the literature, I replace the dependent variable, juvenile arrest rate, with its log and proceed with the analysis as before. I start with the simplest model and progressively include the set of demographic and economic variables along with county and year fixed-effects. Table 4 presents the results for the full specification of the model. None of the coefficients for the state mandate indicate a statistically significant impact of alternative education programs on either total crime, property crime or violent crime. Therefore, based on the combined evidence from juvenile arrest rates in Table 3 and the log linear model from Table

4, I conclude that state-mandated alternative education programs for expelled and suspended students do not significantly impact overall juvenile crime level at the county level. In addition, as shown in columns (4) through (6) of Table 4, the results are not sensitive to the addition of state specific linear time trends. Overall, these results are in line with Akee et al. (2014) who find no impact of time out of school on overall juvenile arrests in the state of Hawaii, except for assault and drug-related arrests.

A. Results by Offense Type

I now turn to investigate the impact of state-mandated alternative education programs on juvenile crime with respect to the breakdown of different types of crimes. Table 5 presents the results for four categories of violent crime (murder, rape, robbery and aggravated assault) while Table 6 presents the results for four categories of property crimes (burglary, larceny, motor vehicle theft and arson). Overall, there seems to be no significant impact of the state mandate on either violent crime or the property crime categories, even though the point estimates are all negative. Notwithstanding issues of aggregation that are common in the crime literature where aggregate results differ from individual results due to measurement error and the ways in which categories of violent crime and property crime are recorded, the fact that the point estimates have the same signs, though insignificant, imply that alternative education programs probably have a strong incapacitation effect. For instance, Jacob and Lefgren (2003) and Lullallen (2006) find that time in school lead to an *increase* in violent crime. In addition, a rather large strand of the peer effects literature such as Gaviria and Raphael (2001), finding evidence of peer-group influence to engage in drug use, alcohol drinking, cigarette smoking, church going, and the likelihood to drop out of high school. That is, since an alternative education environment brings

together students that already have a history of unruly behavior, through peer-group influence, those students might have been more likely to engage in criminal activities. This does not happen to be the case in this study.

B. Dynamic Effects

In this section, I present the results from estimating the dynamic responses of the juvenile arrests to the adoption of state-mandated alternative education programs using Specification 1.2. Recall that here I replace the indicator variable for the state mandate with a set of dummy indicators, Year -2 through Years +5, which indicate one-year interval before and after the alternative education laws were enacted. Table 7 presents the results, and, as before, the specification includes a full set of control variables, county and year fixed-effects. The estimates for the dummy indicators just prior to the year the state mandate went into effect are insignificant for either total crime, violent crime or property crime. I therefore take this as an indication that policy endogeneity is not a serious concern. Overall, the signs and the significance of the point estimates do not seem to change under alternative specifications, i.e., controlling for state specific linear time trends. To summarize, the results of this event study analysis imply that endogeneity of the state mandate is not a concern in this study and that the state mandate does not have an impact on juvenile crime.

As I did with the difference-in-differences specification, I then turn to the dynamic response of the state mandate on juvenile crime using the logarithmic transformation of the dependent variable. Tables 8 present the results for total crime, property crime and violent crime, and columns (1) through (3) largely confirm the findings presented in Table 7, indicating that alternative education mandate does not have any significant impact on either total crime, violent crime or property crime around the time the policy went into effect. However, looking at a longer

horizon, there seems to be a small significant but positive impact of alternative education programs on juvenile crime four years after the law went into effect. This could be due to measurement error issues that are inherent in county crime data.

Similar to the evidence presented above using Specification 1.1, in Table 9, I analyze the impact of the state mandate on categories of property and violent crimes in a dynamic setting, using Specification (1.2). I start with a simple econometric specification and progressively include a set of demographic and economic variables along with county and fixed-year effects. Tables 9 and 10 present show the results for categories of violent and property crimes, respectively. As one can see from columns (1) through (4) of Table 9, overall, it would seem that there is no significant impact of state-mandated alternative education programs on property crime. The results are not sensitive to adding a set of state specific linear time trends. Columns (1) through (4) from Table 10 present similar results for property crime categories. To summarize the overall results presented up to this point, alternative education mandates do not seem to have a significant impact on juvenile arrest rate at the county level in the U.S. To reiterate, notwithstanding issues of measurement errors in county-level crime data, by and large, the event-study methodology provides support for the results of the difference-in-differences. These results are broadly in line with Akee et al. (2014) who find no impact of furloughing public school teachers on juvenile crime in the state of Hawaii from 2007 to 2010.

C. State-Level Evidence: Juvenile Homicide Offending

As already mentioned, the analysis of the county juvenile arrest data suggests that state mandated alternative education programs for expelled and suspended students have no significant impact on juvenile crime, although most of the point estimates are negative. County-

level crime data in general suffer from a large degree of measurement error. To gain further insight about this issue, I examine the impact of state-mandated alternative education mandates on juvenile crime using state-level crime data. I turn to the analysis of juvenile homicide offending. This is noteworthy for at least two reasons. First, using the county level data, I investigate the impact of the state mandate on crimes committed by juveniles between the ages of 10 and 17, while most juvenile crimes are committed by juveniles who are between the ages of 14 and 17 (Dills and Hernandez-Julian 2011). The state-level data will allow me to analyze the impact of the state mandate on several age groups. Second, African-American students, along with Hispanics, are suspended at a higher rate than their white counterparts. The state-level data will also allow me to investigate whether there is heterogeneous treatment effect with respect to race. Columns (1) and (2) of Table 11 present the results for all youth and black youth between the ages of 12 and 17, respectively, using Specification 1.1. The model includes an extensive set of demographic and economic control variables. Columns (3) through (4) control for a set of state specific linear time trends to account for within state time-series variation, and the coefficient for black youth become significant. Overall, there does not seem to be a significant impact of alternative education mandate on juvenile homicide offending, except for black youth. To conclude, even though the results in this section are not directly comparable to the ones in the previous section since the juvenile county data are not broken down by race, the point estimates have the signs.

In this section, I investigate the dynamic impact of the state mandate on juvenile homicide offending, using Specification 1.2. I start with a simple specification and progressively introduce a richer set of control variables, along with state and year fixed-effects. Table 12 presents the results for this dynamic methodology using a set of dummy indicators for three

years after the policy took place. As we can see from column (2) of Table 12, the significant impact for black youth continues two years after the state mandate took effect. Controlling for state trends in column (4) of Table 12, the effect for black youth is even more pronounced one and two years after the policy took effect, while there seems to be a small but significant negative impact for all youth three years after the implementation of the program. Based on this evidence, one can conclude that state-mandated alternative education programs have led to a significant reduction in state-level homicide offending rates for black youth aged 12-17. This result is probably due to the incapacitation effect of those programs.

D. State-Level Evidence: Juvenile Homicide Victimization

So far, the state homicide offending data suggest that there is a significant impact of state-mandated alternative education programs for expelled and suspended students on homicide offending rates of black youth. However, one shortcoming of the homicide offending data is that only when a criminal is apprehended by the police does this information show up in the data. To mitigate this problem, I turn to state homicide victimizations, where I analyze the impact of the state mandate on homicide victims. Thus, even if an offender has not been apprehended by the police, we would still presumably have information about criminal activities, providing that information about the victims have been reported, which seems very likely. As before, the data will also allow me to analyze black homicide victimizations, a group of students that are expelled and suspended at a higher rate than their white counterparts. Table 13 presents the results using the difference-in-differences methodology, and the overall picture seems to be that alternative education mandate has no discernible impact on juvenile homicide victimizations. In contrast to homicide offending, with respect to race, I find no impact of the state mandate on

juvenile homicide victimizations for African-Americans aged 15-17, despite the fact that it is well documented that African-American students are subject to expulsion more than any other race. Column (2) of Table 12 reports the results for homicide victimizations of black youth, and they show no significant impact for that group, even though the point estimate is negative. Columns (3) and (4) of Table 13 show that these results are not sensitive to a set of state-specific linear time trends.

Investigating the dynamic effect of the state mandate using a series of dummy indicators three years before and after the state mandate took effect, the results are not significant for any of the three age groups. Columns (1) through (4) of Table 14 present the results controlling for a rich set of economic and demographic variables, along with state specific linear time trends. The signs of most of the point estimates for the dummy indicators after the state mandate went into effect are negative; with a small impact on all youth three years after the policy went into effect. Overall, using the state homicide victimization and homicide offending data, the findings of this section are in line with the earlier set of findings for total crime, total violent crime and total property crime using the county level juvenile arrest data in the sense that most of the point estimates are negative. However, a significant effect persists only for the state homicide offending rates of African-Americans between the ages of 12 and 17.

VII. Discussion and Conclusion

The aim of this paper has been to examine the impact of state-mandated alternative education programs for expelled and suspended students on juvenile crime. Given that there are several channels through which those programs can impact juvenile crime, we need to exercise caution in interpreting the results. Overall, I have found the state mandate to have no impact on

juvenile arrest rates for total crime, property crime and violent crime at the county level in the U.S. Notwithstanding issues of measurement error with county level crime data in general, as Weiner, Lutz and Ludwig (2009) have argued, among others, therefore, this set of findings is in line with the theoretical models of social interaction of crime, implying that the insignificance of the results using the county-level data is largely due to the fact that most students might misbehave in the classroom in the company their peers, but no longer to do so once removed from the school environment. This line of argument would seem to lend support to critics of school suspension and expulsion practices, arguing that many of the students who are expelled and suspended from schools are not really criminals, as they are often suspended and expelled from school for minor school infractions.

However, state level data on juvenile homicide victimizations and offending seem to, by and large, confirm the results, as most of the point estimates have the same signs. However, with respect to heterogeneous treatment in terms of race, using an event study methodology and controlling for state specific time trends, one and two years after the implementation of state-mandated alternative education programs, homicide offending rates for African-Americans between the ages of 12 and 17 significantly decrease. This might be due to the fact that it has been well documented that black students are suspended and expelled from schools at a higher rate compared to their white counterparts.

In light of these findings, it might be useful to perform a cost-benefit analysis of these programs. Unfortunately, however, there is very limited information regarding the costs of alternative education programs. To obtain any information on the relative size of alternative education programs would require contacting separately school districts or school systems in the states where those programs are in place. This is not a possible undertaking of the current study,

but anecdotal evidence would seem to suggest that students *do* enroll in these programs. Moreover, the school suspension and expulsion crisis is so dire that the federal government, through the Departments of Justice and Education, had to intervene in January 2014, outlining steps states can take to lessen the impact of the crisis. As the *New York Times* reports on January 8, 2014, “The secretary of education, Arne Duncan, and the attorney general, Eric H. Holder Jr., released a 35-page document that outlined approaches — including counseling for students, coaching for teachers and disciplinary officers, and sessions to teach social and emotional skills — that could reduce the time students spend out of school as punishment.” Given the importance of this issue, should they fail to make steps to ensure that expelled and suspended students are enrolled in those programs, schools systems or school districts in states that mandate alternative education programs could be subject to lawsuits from education-related interest groups.

School suspension and expulsion continue to plague the nation. Newspaper editorials and education-related interest groups often raise the issues of harsh consequences of school punishment practices, which, as they have argued, often lead to the so-called movement from school to the prison pipeline, whereby suspended and expelled students find themselves entangled in the juvenile justice system. This is so much so that, as recently as January 2014, the U.S. Departments of Justice and Education outlined steps that states should take to reduce time students spend out of school as punishment. This research contributes to the current policy debate on school suspension and therefore has far-reaching policy implications.

One potential limitation of this study is that I had to track out-of-school suspension laws by e-mails from all the 50 states. Even though I have heard from all the states, I could not, unfortunately, get much information on the number of students that are actually enrolled in those programs, given that most of those programs are administered at the school district level.

Therefore, gathering such information about these programs is time-consuming and beyond the scope of this study. Another potential limitation of this study is that I have not taken into account drug-related crimes that are likely to be committed by juveniles or other less desirable behavior such as alcohol drinking, cigarette smoking, gambling.

To conclude, in light of the findings of this study and the importance of school suspension and school expulsion in the U.S., further research can extend this paper in several ways. One next step might be to investigate the effect of alternative education programs for expelled and suspended students on human capital accumulation such as high school completion, school dropout and academic achievement. Another policy-relevant step might be to investigate whether or not state-mandated alternative-education programs for expelled and suspended students have any effect on juvenile incarceration or other juvenile criminal activities.

CONCLUDING REMARKS

In this dissertation, I have examined three issues in economics that are linked by their policy implications. This first chapter investigates the fiscal incidence of direct democracy in the American states from 1984 to 2005. From 1898 to 1992, twenty-four U.S. states adopted the statewide initiative, allowing citizens to directly influence policy outcomes. Though an extensive empirical literature on the policy outcomes of this political institution exists, theoretically, however, how this political institution impacts inequality and redistribution is still being sorted out. Overall, I have found the adoption of the statewide initiative to lead to an increase in income inequality and a decrease in state tax burden. However, there is no significant effect of direct democracy on tax progressivity and only a modest effect on expenditure redistribution. Controlling for unobserved heterogeneity using a Correlated Random Effects model, direct democracy appears to have led to an increase in marginal and average state tax rates, with only modest effects on expenditure redistribution and state tax burden.

The second chapter examines the extent to which fiscal spillovers exist in county governments in California. At the county level in California, each year, quite a number fiscal decisions are made through the use of tax and expenditure referenda. A rather large theoretical literature has hypothesized that government expenditures and revenues of neighboring jurisdictions are interdependent. That is, neighboring jurisdictions incorporate their neighbors' fiscal decisions into their own choices. Using spatial econometric analysis, I have found significant fiscal spillover effects in general, public protection, and public facilities expenditures and sales, transient and other tax revenues. Overall, the findings of the second chapter are largely in line with a host of empirical studies both within and outside of the U.S. investigating the extent of strategic interactions across jurisdictions.

The third chapter examines the impact of state-mandated alternative education programs for expelled and suspended students on juvenile crime. From 1987 to 2010, fourteen states adopted policies designed to reduce time students spend out of school as punishment by mandating that school districts establish alternative education programs to serve expelled and suspended students. The Gun-Free School Zones Act (1994), passed by congress to prevent gun violence in schools, has led school districts to implement various zero tolerance policy laws, allowing schools to expulse and suspend students for various school violations, even for first-time and minor school transgressions. I estimate the impact of the state mandate on juvenile crime, finding that state-level juvenile homicide offending rates for back youth aged 12-17 significantly decrease after the implementation of those programs.

This finding is more likely to be due to the incapacitation and human capital accumulation effects of state-mandated alternative programs. Given the importance of this issue, future research might expand on this study by using alternative methodological approaches to check the robustness of the findings. Also, one hopes that policy makers charged with reforming the education systems in the U.S. find something useful in the third essay.

On the whole, the findings of this dissertation present an interesting depiction of the effects of democratic institutions on policy outcomes. Issues addressed here such as income inequality and redistribution are likely to be of importance in the foreseeable future. Therefore, it is crucial that future works in political economy deepen our understanding on the mechanisms through which direct democracy impacts policy outcomes.

Table 1.1: Descriptive statistics for state panel data, 1984-2005

Variable	Mean	Std. Dev.
Initiative	0.45	0.5
State government ideology index	51.76	21.1
Averaged number of voter initiatives placed on the ballot	3.41	3.3
Approval rate of voter initiatives	0.49	0.37
Population density (selected years, people per km^2)	176.51	241.73
Signature requirement	7.52	2.84
Qualification difficulty index	5.13	1.96
Legislative insulation index	4.17	1.76
Atkinson index	0.234	0.037
Gini index	0.566	0.038
Theil index	0.678	0.187
Top 10% of income earners	0.393	0.056
Top 1% of income earners	0.145	0.047
Share of the democratic votes received by the state governor in the last election	0.499	0.11
Governor's party is in control of the senate (dummy=1)	0.502	0.494
Proportion of the population ages 5-17	0.188	0.016
Proportion of the population ages 5-17	0.124	0.02
Median age	34.06	2.53
Median income	50407.63	8381.553
Income per capita	22748.26	7197.456
Unemployment rate	5.488	1.707
Share of democratic house representatives	0.62	0.481
Share of democratic senators	0.574	0.489
ln(Population)	14.511	0.938
Education (share of total expenditures)	0.324	0.061
Public welfare (share of total expenditures)	0.189	0.055
Hospital (share of total expenditures)	0.0334	0.018

Health (share of total expenditures)	0.033	0.011
Region=North	0.18	0.384362
Region=Midwest	0.26	0.44
Region=West	0.24	0.43

Table 1.2: Direct Democracy on Redistribution, 1984-2005

VARIABLES	(1) Education	(2) Public Welfare	(3) Hospital	(4) Health
Initiative	-0.005 (0.011)	-0.007 (0.008)	-0.008** (0.003)	0.006* (0.003)
Observations	1,078	1,078	1,078	1,078
R-squared	0.590	0.680	0.367	0.205

Notes: Demographic controls include the fraction of the state population between the ages of 5 and 17, old and over 65 and total state population. Controls for economic conditions include the unemployment rate, poverty rate, state minimum wage, median and per-capita income, intergovernmental revenues. Controls for the political process includes state ideology, dummy indicators for the party that is in control of the state senate, state house of representatives and whether the state governor is from the Democratic Party. Regional dummies for South, Midwest and West (North is excluded) and Year FE are excluded. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.3: Direct Democracy on Redistribution, 1984-2005

VARIABLES	(1) Education	(2) Public Welfare	(3) Hospital	(4) Health
Initiative	-0.013 (0.025)	-0.032** (0.016)	-0.008 (0.008)	0.006 (0.005)
Difficulty index	Yes	Yes	Yes	Yes
Insulation index	Yes	Yes	Yes	Yes
Observations	1,078	1,078	1,078	1,078
R-squared	0.608	0.694	0.380	0.205

Notes: See Table 1.2.

Table 1.4: Direct Democracy on Redistribution, 1984-2005

VARIABLES	(1) Education	(2) Public Welfare	(3) Hospital	(4) Health
Initiative	-0.002 (0.016)	-0.017 (0.012)	-0.011** (0.004)	0.003 (0.003)
Controls	No	No	No	No
Year FE	Yes	Yes	Yes	Yes
Observations	1,100	1,100	1,100	1,100
R-squared	0.024	0.324	0.159	0.055

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.5: Direct Democracy on Redistribution, 1984-2005

VARIABLES	(1) Education	(2) Public Welfare	(3) Hospital	(4) Health
Initiative	0.026 (0.015)	-0.020 (0.013)	-0.008* (0.004)	0.003 (0.003)
South	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Year FE	Yes	Yes	Yes	Yes
Observations	1,100	1,100	1,100	1,100
R-squared	0.233	0.328	0.185	0.057

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.6: Direct Democracy on Expenditure Redistribution, 1984-2005

VARIABLES	(1) Education	(2) Public Welfare	(3) Hospital	(4) Health
initiative	-0.026*** (0.010)	0.006 (0.008)	0.005 (0.004)	0.002 (0.003)
ideology	0.0001 (0.000)	0.0002 (0.000)	0.000 (0.000)	-0.000 (0.000)
gov. revenues	-0.000*** (0.000)	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
poverty	-0.0004 (0.001)	0.0001 (0.001)	-0.0001 (0.000)	0.0002 (0.000)
minimum wage	-0.0014 (0.004)	-0.0001 (0.002)	0.0016 (0.001)	-0.0001 (0.001)
log(population)	-0.023 (0.107)	0.132*** (0.049)	0.049** (0.021)	0.005 (0.022)
unemployment	-0.004** (0.002)	0.001 (0.002)	-0.001*** (0.000)	-0.0001 (0.000)
income per capita	0.000* (0.000)	-0.000*** (0.000)	-0.000 (0.000)	0.000 (0.000)
median income	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
old	0.324 (0.428)	-0.085 (0.415)	0.108 (0.126)	0.248 (0.153)
young	0.571** (0.232)	-0.334 (0.242)	-0.110* (0.063)	0.064 (0.075)
governor's party	-0.010 (0.008)	-0.001 (0.007)	0.0002 (0.002)	0.0002 (0.002)
house democrat	0.023 (0.029)	0.002 (0.023)	0.002 (0.009)	0.001 (0.007)
senate democrat	0.027 (0.021)	-0.042** (0.018)	-0.006 (0.007)	0.010 (0.006)
revenuebar	0.000** (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
populationbar	0.092 (0.112)	-0.086 (0.055)	-0.034 (0.022)	-0.003 (0.023)
unemploymentbar	-0.013** (0.006)	-0.002 (0.004)	-0.005* (0.002)	0.0004 (0.002)
incpercapbar	-0.000* (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
medianincomebar	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
oldbar	-0.505 (0.721)	-0.520 (0.625)	0.067 (0.289)	-0.157 (0.202)
youngbar	0.902 (0.870)	-0.239 (0.783)	0.688* (0.409)	-0.419** (0.191)
govpartybar	0.071** (0.036)	-0.046 (0.029)	-0.008 (0.019)	-0.017** (0.008)

hsdembar	-0.137*	0.112	0.037	-0.027
	(0.077)	(0.084)	(0.026)	(0.026)
sendembar	0.121	-0.079	-0.012	-0.004
	(0.077)	(0.071)	(0.035)	(0.021)
ideologybar	-0.0002	0.001	0.0002	0.0003
	(0.001)	(0.001)	(0.000)	(0.000)
povertybar	-0.006	-0.005	0.002	0.001
	(0.004)	(0.003)	(0.002)	(0.001)
wagebar	-0.006	0.011	-0.002	-0.005
	(0.014)	(0.009)	(0.005)	(0.004)
Constant	0.362	0.363	-0.187	0.055
	(0.259)	(0.258)	(0.135)	(0.070)
Observations	1,078	1,078	1,078	1,078
Number of id	49	49	49	49

All regressions include year FE and regional dummies. The joint p-values of the additional control variables (means of the time-varying variables) from specifications (1) through (4) are, respectively, 0.00, 0.37, 0.00, and 0.27. Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Table 1.7: Direct Democracy on Expenditure Redistribution, 1984-2005

VARIABLES	(1) Education	(2) Public Welfare	(3) Hospital	(4) Health
initiative	-0.040*** (0.007)	0.010 (0.009)	0.007** (0.004)	-0.0003 (0.003)
difficulty index	0.008*** (0.002)	-0.002 (0.002)	-0.003*** (0.001)	0.001* (0.001)
poverty	-0.0004 (0.001)	0.0001 (0.001)	-0.0001 (0.000)	0.0002 (0.000)
wage	-0.0015 (0.004)	-0.0001 (0.002)	0.0016 (0.001)	-0.0001 (0.001)
ideology	0.0001 (0.000)	0.0002 (0.000)	0.000 (0.000)	-0.000 (0.000)
gov. revenues	-0.000*** (0.000)	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
log(population)	-0.025 (0.108)	0.132*** (0.050)	0.049** (0.021)	0.005 (0.022)
unemployment	-0.004** (0.002)	0.001 (0.002)	-0.001*** (0.000)	-0.0001 (0.000)
per capita income	0.000* (0.000)	-0.000*** (0.000)	-0.000 (0.000)	0.000 (0.000)
median income	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
old	0.292 (0.426)	-0.078 (0.414)	0.114 (0.127)	0.241 (0.154)
young	0.559** (0.231)	-0.332 (0.243)	-0.107* (0.063)	0.061 (0.076)
governor's party	-0.010 (0.008)	-0.001 (0.007)	0.0004 (0.002)	-0.000 (0.002)
house democrat	0.021 (0.029)	0.003 (0.023)	0.002 (0.009)	0.001 (0.007)
senate democrat	0.026 (0.021)	-0.042** (0.018)	-0.006 (0.007)	0.009 (0.006)
intrevbar	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000** (0.000)
logpopbar	0.089 (0.113)	-0.085 (0.055)	-0.033 (0.022)	-0.004 (0.023)
unemplbar	-0.013** (0.006)	-0.002 (0.004)	-0.005* (0.003)	0.001 (0.002)
incpercapbar	-0.000** (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000* (0.000)
medincbar	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000* (0.000)
oldbar	-0.614 (0.667)	-0.501 (0.627)	0.122 (0.275)	-0.176 (0.203)
youngbar	0.679 (0.772)	-0.200 (0.781)	0.809** (0.391)	-0.457** (0.196)
govpartybar	0.051	-0.042	0.001	-0.021***

	(0.036)	(0.031)	(0.016)	(0.008)
hsdembar	-0.138*	0.112	0.039	-0.027
	(0.071)	(0.083)	(0.026)	(0.026)
sendembar	0.098	-0.075	-0.001	-0.008
	(0.073)	(0.071)	(0.035)	(0.019)
ideologybar	0.001	0.0004	-0.0001	0.0004*
	(0.001)	(0.001)	(0.000)	(0.000)
povertybar	-0.006	-0.005	0.002	0.001
	(0.004)	(0.003)	(0.002)	(0.001)
wagebar	-0.024*	0.014	0.006	-0.008*
	(0.013)	(0.010)	(0.006)	(0.005)
Constant	0.499**	0.337	-0.242*	0.079
	(0.238)	(0.262)	(0.124)	(0.074)
Observations	1,078	1,078	1,078	1,078
Number of id	49	49	49	49

All specifications include year FE and regional dummies. The joint p-values for the additional control variables (means of time varying variables) from specification (1) through (4) are, respectively, 0.00, 0.37, 0.00, and 0.16. Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Table 1.8: Direct Democracy on Expenditure Redistribution, 1984-2005

VARIABLES	(1) Education	(2) Public Welfare	(3) Hospital	(4) Health
initiative	-0.037*** (0.008)	0.008 (0.010)	0.010* (0.004)	-0.001 (0.002)
difficulty index	0.017*** (0.004)	-0.003 (0.004)	-0.004 (0.003)	0.001 (0.002)
insulation index	-0.013** (0.005)	0.002 (0.006)	0.002 (0.003)	0.001 (0.002)
poverty	-0.0004 (0.001)	0.0001 (0.001)	-0.0001 (0.000)	0.0002 (0.000)
wage	-0.001 (0.004)	-0.0001 (0.002)	0.002 (0.001)	-0.0001 (0.001)
ideology	0.0001 (0.000)	0.0002 (0.000)	0.000 (0.000)	-0.000 (0.000)
gov. revenues	-0.000*** (0.000)	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
log(population)	-0.024 (0.108)	0.132*** (0.050)	0.049** (0.021)	0.005 (0.022)
unemployment	-0.004** (0.002)	0.001 (0.002)	-0.001*** (0.000)	-0.0001 (0.000)
per capita income	0.000* (0.000)	-0.000*** (0.000)	-0.000 (0.000)	0.000 (0.000)
median income	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
old	0.299 (0.427)	-0.079 (0.414)	0.114 (0.127)	0.240 (0.154)
young	0.561** (0.231)	-0.332 (0.243)	-0.108* (0.063)	0.061 (0.076)
governor's party	-0.010 (0.008)	-0.001 (0.007)	0.0004 (0.002)	-0.0001 (0.002)
house democrat	0.021 (0.029)	0.003 (0.023)	0.002 (0.009)	0.001 (0.007)
senate democrat	0.026 (0.021)	-0.042** (0.018)	-0.006 (0.007)	0.009 (0.006)
intrevbar	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000** (0.000)
populationbar	0.096 (0.111)	-0.086 (0.054)	-0.034 (0.022)	-0.005 (0.023)
unemplomentbar	-0.013** (0.005)	-0.002 (0.004)	-0.005* (0.003)	0.001 (0.002)
incpercapbar	-0.000** (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000* (0.000)
medincbar	-0.000* (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000* (0.000)
oldbar	-0.810 (0.662)	-0.467 (0.642)	0.151 (0.275)	-0.157 (0.205)
youngbar	0.615	-0.188	0.816**	-0.451**

	(0.728)	(0.785)	(0.389)	(0.198)
govpartybar	0.042	-0.040	0.003	-0.020**
	(0.037)	(0.031)	(0.016)	(0.008)
hsdembar	-0.170***	0.118	0.043*	-0.024
	(0.060)	(0.080)	(0.026)	(0.026)
sendembar	0.143**	-0.082	-0.008	-0.012
	(0.069)	(0.072)	(0.035)	(0.018)
ideologybar	0.001	0.0004	-0.0001	0.0004*
	(0.001)	(0.001)	(0.000)	(0.000)
povertybar	-0.008*	-0.004	0.002	0.002
	(0.004)	(0.004)	(0.002)	(0.001)
wagebar	-0.026**	0.014	0.006	-0.008
	(0.013)	(0.009)	(0.006)	(0.005)
Constant	0.574***	0.324	-0.253**	0.072
	(0.215)	(0.264)	(0.126)	(0.074)
Observations	1,078	1,078	1,078	1,078
Number of id	49	49	49	49

All specifications control for year FE and regional dummies. The joint p-values for the additional control variables (means of time varying variables) are, respectively, 0.00, 0.36, 0.00 and 0.27. Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Table 1.9: Direct Democracy on Tax Progressivity, 1984-2005

VARIABLES	(1) Marginal Tax Rate	(2) Marginal Tax Rate	(3) Marginal Tax Rate	(4) Average Tax Rate	(5) Average Tax Rate	(6) Average Tax Rate
Initiative	-0.0089 (0.006)	-0.009 (0.006)	-0.0089 (0.006)	-0.0042 (0.004)	-0.0044 (0.004)	-0.0044 (0.004)
Signature	No	Yes	Yes	No	Yes	Yes
Insulation	No	No	Yes	No	No	Yes
Difficulty	No	No	Yes	No	No	Yes
Observations	1,078	1,078	1,078	1,078	1,078	1,078
R-squared	0.232	0.232	0.233	0.225	0.225	0.227

Notes: Demographic controls include the fraction of the state population between the ages of 5 and 17, over 65 and total state population. Controls for economic conditions include the unemployment rate, poverty rate, state minimum wage, median income and per-capita income, intergovernmental revenues. Controls for the political process includes state ideology, dummy indicators for the party that is in control of the state senate, state house of representatives and whether the state governor is from the Democratic Party. Regional dummies for South, Midwest and West (North is excluded) and Year FE are excluded. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.10: Direct Democracy on Tax Progressivity, 1984-2005

VARIABLES	(1) Marginal Tax Rate	(2) Marginal Tax Rate	(3) Average Tax Rate	(4) Average Tax Rate
Initiative	-0.0335 (0.021)	-0.0224 (0.014)	-0.0259 (0.017)	-0.0169 (0.012)
Tax measure	Yes	Yes	Yes	Yes
Tax measure+	No	Yes	No	Yes
Observations	506	506	506	506
R-squared	0.593	0.552	0.739	0.690

Notes: See Table 1.7.

Table 1.11: Direct Democracy on Tax Progressivity, 1984-2005

VARIABLES	(1) Marginal Tax Rate	(2) Marginal Tax Rate	(3) Average Tax Rate	(4) Average Tax Rate
Initiative	-0.006 (0.007)	-0.009 (0.008)	-0.005 (0.004)	-0.006 (0.005)
Controls	No	No	No	No
South (dummy)	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,100	1,100	1,100	1,100
R-squared	0.016	0.034	0.033	0.036

Notes: Robust standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Table 1.12: Direct Democracy on Marginal Tax Rate, 1984-2005

VARIABLES	(1) OLS1	(2) OLS2	(3) OLS3	(4) CRE1	(5) CRE2	(6) CRE2
initiative	-0.009 (0.006)	-0.015 (0.011)	-0.033*** (0.011)	0.004** (0.002)	0.004** (0.002)	0.004** (0.002)
ideology	0.0003 (0.000)	0.0003 (0.000)	0.0002 (0.000)	0.0001* (0.000)	0.0001* (0.000)	0.0001* (0.000)
poverty	-0.0001 (0.001)	-0.0003 (0.001)	-0.0002 (0.001)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
wage	0.003 (0.004)	0.002 (0.004)	0.002 (0.003)	-0.0003 (0.001)	-0.0003 (0.001)	-0.0003 (0.001)
gov. revenues	-0.000 (0.000)	-0.000 (0.000)	-0.000*** (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
log(population)	0.014 (0.009)	0.013 (0.009)	0.005 (0.009)	-0.046* (0.024)	-0.046* (0.024)	-0.046* (0.024)
unemployment	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)	0.0003 (0.000)	0.0003 (0.000)	0.0003 (0.000)
gdp per capita	-0.000** (0.000)	-0.000** (0.000)	-0.000*** (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
median income	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000** (0.000)	-0.000** (0.000)	-0.000** (0.000)
old	0.042 (0.228)	0.029 (0.221)	0.113 (0.153)	-0.164 (0.123)	-0.164 (0.123)	-0.165 (0.123)
young	0.077 (0.236)	0.079 (0.231)	0.197 (0.204)	0.037 (0.048)	0.037 (0.048)	0.037 (0.048)
governor's party	-0.007 (0.007)	-0.008 (0.007)	-0.004 (0.007)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)
house democrat	-0.012 (0.030)	-0.010 (0.030)	0.012 (0.025)	0.005 (0.005)	0.005 (0.005)	0.005 (0.005)
senate democrat	0.047* (0.026)	0.046* (0.026)	0.018 (0.026)	-0.013*** (0.005)	-0.013*** (0.005)	-0.013*** (0.005)
revenuesbar				-0.000* (0.000)	-0.000 (0.000)	-0.000** (0.000)
populationbar				0.068*** (0.023)	0.069*** (0.023)	0.062*** (0.024)
unemployment bar				-0.006 (0.005)	-0.006 (0.004)	-0.007 (0.004)
incpercapbar				-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
medinbar				0.000 (0.000)	0.000 (0.000)	0.000** (0.000)
oldbar				0.218 (0.363)	0.239 (0.357)	0.416 (0.321)
youngbar				-0.122 (0.403)	-0.075 (0.389)	-0.036 (0.365)
govpartybar				-0.012	-0.009	-0.001

hsdembar				(0.023)	(0.023)	(0.020)
				-0.071	-0.070	-0.041
				(0.056)	(0.056)	(0.051)
sendembar				0.077	0.081	0.037
				(0.063)	(0.062)	(0.062)
ideologybar				0.001*	0.001	0.001
				(0.000)	(0.001)	(0.001)
povertybar				0.002	0.002	0.004
				(0.003)	(0.003)	(0.003)
wagebar				-0.002	0.001	0.003
				(0.009)	(0.009)	(0.008)
difficulty index	0.001	-0.008***			-0.001	-0.010***
	(0.002)	(0.003)			(0.001)	(0.003)
insulation index		0.016***				0.012***
		(0.003)				(0.004)
Constant	-0.017	-0.005	-0.026	0.001	-0.018	-0.091
	(0.087)	(0.085)	(0.066)	(0.146)	(0.141)	(0.123)
Observations	1,078	1,078	1,078	1,078	1,078	1,078
R-squared	0.232	0.236	0.380			
Number of id				49	49	49

All specifications control for year FE and regional dummies. The joint p-values for the significance of the additional control variables (means of time varying variables) for specifications (4) through (6) are, respectively, 0.00, 0.00 and 0.00. Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Table 1.13: Direct Democracy on Average Tax Rate, 1984-2005

VARIABLES	(1) OLS1	(2) OLS2	(3) OLS3	(4) CRE1	(5) CRE2	(6) CRE2
initiative	-0.004 (0.004)	-0.007 (0.007)	-0.020*** (0.006)	0.0031** (0.001)	0.0034** (0.001)	0.0027* (0.002)
ideology	0.001 (0.000)	0.001 (0.000)	0.001 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
poverty	-0.0004 (0.001)	-0.0004 (0.001)	-0.0003 (0.001)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
wage	0.002 (0.002)	0.001 (0.002)	0.002 (0.002)	-0.0002 (0.001)	-0.0002 (0.001)	-0.0002 (0.001)
intrev	-0.000* (0.000)	-0.000* (0.000)	-0.000*** (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
logpop	0.011** (0.005)	0.010* (0.005)	0.005 (0.005)	-0.041*** (0.016)	-0.041** (0.016)	-0.041** (0.016)
unempl	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	0.0002 (0.000)	0.0002 (0.000)	0.0002 (0.000)
incpercap	-0.000** (0.000)	-0.000** (0.000)	-0.000** (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
medinc	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
old	0.026 (0.160)	0.020 (0.158)	0.079 (0.105)	-0.137** (0.067)	-0.137** (0.067)	-0.138** (0.067)
young	-0.0003 (0.158)	0.0004 (0.155)	0.083 (0.138)	0.028 (0.039)	0.028 (0.039)	0.027 (0.039)
governor's party	-0.003 (0.004)	-0.003 (0.004)	-0.001 (0.004)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
hsdem	-0.001 (0.019)	0.0001 (0.018)	0.015 (0.014)	-0.002 (0.005)	-0.002 (0.005)	-0.002 (0.005)
sendem	0.025 (0.016)	0.025 (0.015)	0.005 (0.015)	-0.007** (0.003)	-0.007** (0.003)	-0.007** (0.003)
intrevbar				-0.000** (0.000)	-0.000* (0.000)	-0.000** (0.000)
logpopbar				0.057*** (0.015)	0.058*** (0.015)	0.053*** (0.016)
unemplbar				-0.003 (0.003)	-0.003 (0.003)	-0.003 (0.003)
incpercapbar				-0.000** (0.000)	-0.000** (0.000)	-0.000*** (0.000)
medincbar				0.000 (0.000)	0.000 (0.000)	0.000** (0.000)
oldbar				0.175 (0.258)	0.190 (0.254)	0.326 (0.220)
youngbar				-0.074 (0.266)	-0.042 (0.257)	-0.009 (0.235)
govpartybar				-0.002 (0.014)	0.001 (0.013)	0.007 (0.012)
hsdembar				-0.025	-0.025	-0.002

				(0.037)	(0.037)	(0.031)
sendembar				0.044	0.047	0.014
				(0.038)	(0.038)	(0.035)
ideologybar				0.0004	0.0003	0.0002
				(0.000)	(0.000)	(0.000)
povertybar				0.0004	0.0003	0.002
				(0.002)	(0.002)	(0.002)
wagebar				0.0003	0.002	0.004
				(0.005)	(0.005)	(0.004)
difficulty index	0.001	-0.006***			-0.001	-0.007***
	(0.001)	(0.001)			(0.001)	(0.002)
insulation index		0.011***				0.009***
		(0.002)				(0.002)
Constant	-0.013	-0.008	-0.022	-0.014	-0.027	-0.082
	(0.059)	(0.060)	(0.044)	(0.101)	(0.097)	(0.081)
Observations	1,078	1,078	1,078	1,078	1,078	1,078
R-squared	0.225	0.227	0.409			
Number of id				49	49	49

All specifications control for year FE and regional dummies. The joint p-values for the additional controls (means of time varying variables) from specifications (4) though (6) are, respectively, 0.00, 0.00 and 0.00. Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Table 1.14: Direct Democracy on Tax Burden, 1984-2005

VARIABLES	(1)	(2)	(3)	(4)	(5)
Initiative	-0.0398** (0.019)	-0.0409** (0.019)	-0.041** (0.019)	-0.184** (0.069)	-0.215*** (0.074)
Tax measure	No	No	No	Yes	Yes
Tax measure+	No	No	No	No	Yes
Signature requirement	No	Yes	Yes	Yes	Yes
Insulation index	No	Yes	Yes	Yes	Yes
Difficulty index	No	No	Yes	Yes	Yes
Observations	1,078	1,078	1,078	506	506
R-squared	0.754	0.754	0.755	0.838	0.855

Notes: The dependent variable is the logarithmic of state tax burden. Demographic controls include the fraction of the state population between the ages of 5 and 17, over 65 and total state population. Controls for economic conditions include the unemployment rate, poverty rate, state minimum wage, median income and per-capita income, intergovernmental revenues. Controls for the political process includes state ideology, dummy indicators for the party that is in control of the state senate, state house of representatives and whether the state governor is from the Democratic Party. Regional dummies for South, Midwest and West (North is excluded) and Year FE are excluded. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.15: Direct Democracy on Tax Burden, 1984-2005

VARIABLES	(1)	(2)	(3)	(4)
Initiative	-0.052 (0.034)	-0.101*** (0.034)	-0.108*** (0.034)	-0.108*** (0.034)
Tax measure	No	No	No	Yes
Tax measure+	No	No	Yes	Yes
South	No	Yes	Yes	Yes
Observations	1,100	1,100	1,100	1,100
R-squared	0.184	0.317	0.324	0.326

Notes: The dependent variable is the logarithmic of state tax burden. No control variables are included in any of the specifications. All specifications include Year fixed-effects. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 1.16: Direct Democracy on State Tax Burden, 1984-2005

VARIABLES	(1) OLS1	(2) OLS2	(3) OLS3	(4) CRE1	(5) CRE2	(6) CRE3
initiative	-0.040** (0.019)	-0.104*** (0.036)	-0.135*** (0.038)	0.003 (0.009)	0.002 (0.010)	0.0001 (0.011)
ideology	0.002*** (0.001)	0.002*** (0.001)	0.002** (0.001)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)
gov. revenues	-0.000 (0.000)	-0.000** (0.000)	-0.000** (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
log(population)	0.138*** (0.026)	0.126*** (0.023)	0.114*** (0.023)	-0.084 (0.077)	-0.084 (0.078)	-0.085 (0.078)
unemployment	-0.009 (0.006)	-0.010 (0.006)	-0.009* (0.005)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
per capita income	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
median income	0.000* (0.000)	0.000* (0.000)	0.000** (0.000)	-0.000* (0.000)	-0.000* (0.000)	-0.000* (0.000)
old	1.251* (0.679)	1.105* (0.608)	1.238** (0.507)	-1.827*** (0.692)	-1.830*** (0.694)	-1.834*** (0.694)
young	-0.212 (0.964)	-0.269 (0.884)	-0.068 (0.851)	-0.930** (0.449)	-0.931** (0.449)	-0.932** (0.450)
governor's party	-0.052** (0.022)	-0.053** (0.023)	-0.047** (0.023)	-0.017*** (0.006)	-0.017*** (0.006)	-0.018*** (0.006)
house democrat	0.005 (0.070)	0.024 (0.066)	0.060 (0.059)	-0.016 (0.028)	-0.016 (0.028)	-0.016 (0.028)
senate democrat	-0.001 (0.068)	-0.030 (0.063)	-0.076 (0.062)	-0.020 (0.022)	-0.020 (0.022)	-0.020 (0.022)
intrevbar				-0.000* (0.000)	-0.000* (0.000)	-0.000* (0.000)
populationbar				0.245*** (0.083)	0.245*** (0.082)	0.238*** (0.082)
unemploymentbar				-0.026 (0.016)	-0.026 (0.016)	-0.026 (0.016)
incpercapbar				-0.000** (0.000)	-0.000** (0.000)	-0.000** (0.000)
medinbar				0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
oldbar				3.718*** (1.398)	3.694*** (1.382)	3.879*** (1.364)
youngbar				0.972 (1.168)	0.919 (1.165)	0.968 (1.179)
govpartybar				-0.060 (0.073)	-0.064 (0.072)	-0.056 (0.073)
hsdembar				-0.008 (0.134)	-0.008 (0.134)	0.022 (0.127)
sendembar				-0.138 (0.178)	-0.143 (0.178)	-0.187 (0.170)
ideologybar				0.004***	0.004***	0.004***

				(0.001)	(0.001)	(0.001)
povertybar				0.007	0.007	0.009
				(0.008)	(0.008)	(0.008)
wagebar				-0.001	-0.004	-0.002
				(0.028)	(0.028)	(0.027)
difficulty index		0.013**	-0.002		0.001	-0.008
		(0.006)	(0.006)		(0.004)	(0.007)
insulation index			0.027***			0.012
			(0.009)			(0.010)
Constant	2.453***	2.552***	2.521***	2.126***	2.151***	2.077***
	(0.293)	(0.273)	(0.254)	(0.430)	(0.430)	(0.421)
Observations	1,078	1,078	1,078	1,078	1,078	1,078
R-squared	0.754	0.772	0.786			
Number of id				49	49	49

All specifications include year FE and regional dummies. The joint p-values for the additional control variables (means of time varying variables) are, respectively, 0.00, 0.00 and 0.00. Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Table 1.17: Direct Democracy on Income Inequality, 1984-2005

VARIABLES	(1) Atkinson	(2) Gini	(3) Theil	(4) Top 10%	(5) Top 1%
Initiative	0.005 (0.003)	0.012** (0.005)	0.037* (0.019)	0.009** (0.004)	0.007** (0.003)
Observations	1,078	1,078	1,078	1,078	1,078
R-squared	0.879	0.664	0.818	0.848	0.730

Notes: The dependent variable is the logarithmic of state tax burdens. Demographic controls include the fraction of the state population that is 5-17 years old and over 65 and total state population. Controls for economic conditions include the unemployment rate, poverty rate, state minimum wage, median income and per-capita income, intergovernmental revenues. Controls for the political process includes state ideology, dummy indicators for the party that is in control of the state senate, state house of representatives and whether the state governor is from the Democratic Party. Regional dummies for South, Midwest and West (North is excluded) and Year FE are excluded. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 1.18: Direct Democracy on Income Inequality, 1984-2005

VARIABLES	(1) Atkinson	(2) Gini	(3) Theil	(4) Top 10%	(5) Top 1%
Initiative	0.006* (0.003)	0.012** (0.005)	0.039* (0.019)	0.008* (0.004)	0.007** (0.003)
Tax measure	Yes	Yes	Yes	Yes	Yes
Observations	1,078	1,078	1,078	1,078	1,078
R-squared	0.879	0.664	0.818	0.848	0.731

Notes: See Table 1.12.

Table 1.19: Direct Democracy on Income Inequality, 1984-2005

VARIABLES	(1) Atkinson	(2) Gini	(3) Theil	(4) Top 10%	(5) Top 1%
Initiative	0.006* (0.003)	0.012** (0.005)	0.038* (0.019)	0.008* (0.004)	0.007** (0.003)
Tax measure	Yes	Yes	Yes	Yes	Yes
Tax measure+	Yes	Yes	Yes	Yes	Yes
Observations	1,078	1,078	1,078	1,078	1,078
R-squared	0.879	0.665	0.819	0.849	0.733

Notes: See Table 1.12.

Table 1.20: Direct Democracy on Income Inequality, 1984-2005

VARIABLES	(1) Atkinson	(2) Gini	(3) Theil	(4) Top 10%	(5) Top 1%
Initiative	-0.0002 (0.006)	0.014** (0.006)	0.012 (0.036)	0.014 (0.013)	0.017 (0.012)
Controls	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1,100	1,100	1,100	1,100	1,100
R-squared	0.566	0.490	0.435	0.348	0.303

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.21: Direct Democracy on Income Inequality, 1984-2005

VARIABLES	(1) Atkinson	(2) Gini	(3) Theil	(4) Top 10%	(5) Top 1%
Initiative	0.005 (0.003)	0.012** (0.005)	0.037* (0.019)	0.008** (0.004)	0.007** (0.003)
South	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1,078	1,078	1,078	1,078	1,078
R-squared	0.879	0.664	0.818	0.848	0.730

Notes: Robust standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Table 1.22: Direct Democracy on Income Inequality, 1984-2005

VARIABLES	(1) Atkinson	(2) Gini	(3) Theil	(4) Top 10%	(5) Top 1%
initiative	0.002 (0.002)	0.002 (0.005)	0.020* (0.012)	0.002 (0.003)	0.002 (0.003)
ideology	-0.000 (0.000)	0.000 (0.000)	-0.0004 (0.000)	0.000 (0.000)	-0.0001 (0.000)
poverty	0.0003 (0.000)	0.001* (0.000)	0.0002 (0.001)	0.0002 (0.000)	0.001* (0.000)
wage	-0.002 (0.002)	-0.006** (0.002)	-0.006 (0.012)	-0.002 (0.001)	-0.004** (0.002)
gov. revenues	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
log(population)	0.086*** (0.019)	0.036 (0.040)	0.514*** (0.120)	0.086*** (0.023)	0.110*** (0.030)
unemployment	-0.003*** (0.001)	0.0002 (0.001)	-0.019*** (0.004)	-0.002*** (0.001)	-0.001 (0.001)
per capita income	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
median income	-0.000*** (0.000)	-0.000** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
old	-0.053 (0.297)	-0.278 (0.425)	-0.463 (1.977)	0.004 (0.321)	0.291 (0.354)
young	0.036 (0.162)	-0.244* (0.127)	-0.920 (0.984)	-0.031 (0.161)	-0.337* (0.195)
governor's party	-0.0004 (0.002)	-0.0003 (0.004)	0.0024 (0.010)	-0.002 (0.002)	0.003 (0.003)
house democrat	0.015 (0.010)	0.011 (0.019)	0.080 (0.061)	0.011 (0.012)	0.011 (0.014)
senate democrat	0.008 (0.009)	-0.005 (0.012)	0.095* (0.052)	0.006 (0.009)	-0.004 (0.012)
intrevbar	0.000 (0.000)	0.000* (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
populationbar	-0.079*** (0.019)	-0.050 (0.041)	-0.467*** (0.130)	-0.078*** (0.022)	-0.103*** (0.032)
unemploymentbar	0.001 (0.001)	-0.004 (0.003)	0.001 (0.009)	-0.004** (0.002)	-0.003 (0.002)
incpercapbar	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
medinbar	-0.000** (0.000)	-0.000* (0.000)	-0.000** (0.000)	-0.000** (0.000)	-0.000*** (0.000)
oldbar	0.058 (0.395)	0.365 (0.500)	0.649 (2.575)	0.037 (0.469)	-0.294 (0.467)
youngbar	0.415 (0.322)	1.255*** (0.291)	3.195 (2.097)	0.541 (0.349)	0.772* (0.397)
govpartybar	0.001 (0.009)	0.010 (0.012)	0.024 (0.060)	0.009 (0.013)	0.006 (0.011)
hsdembar	0.016	0.025	0.152	0.034	0.035

	(0.034)	(0.033)	(0.226)	(0.041)	(0.045)
sendembar	-0.029	-0.035	-0.205	-0.037	-0.003
	(0.027)	(0.030)	(0.172)	(0.032)	(0.034)
ideologybar	-0.0004	-0.001*	-0.002	-0.001*	-0.0004
	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)
povertybar	0.001	0.004**	0.001	0.003*	-0.002
	(0.001)	(0.002)	(0.007)	(0.001)	(0.001)
wagebar	0.004	0.014**	0.006	0.007*	0.004
	(0.003)	(0.006)	(0.019)	(0.004)	(0.004)
Constant	0.012	0.163	-0.432	0.110	0.065
	(0.104)	(0.105)	(0.683)	(0.130)	(0.129)
Observations	1,078	1,078	1,078	1,078	1,078
Number of id	49	49	49	49	49

All specifications control for year FE and regional dummies. The joint p-values for the additional control variables are, respectively, 0.00, 0.00, 0.00, 0.00 and 0.00. Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Table 1.23: Direct Democracy on Income Inequality, 1984-2005

VARIABLES	(1) Atkinson	(2) Gini	(3) Theil	(4) Top 10%	(5) Top 1%
initiative	0.001 (0.002)	0.001 (0.007)	0.018 (0.014)	0.0004 (0.003)	0.0002 (0.003)
difficulty index	0.0003 (0.001)	0.0004 (0.001)	0.001 (0.004)	0.001 (0.001)	0.001 (0.001)
ideology	-0.000 (0.000)	0.000 (0.000)	-0.0004 (0.000)	0.000 (0.000)	-0.0001 (0.000)
poverty	0.0003 (0.000)	0.001* (0.000)	0.0002 (0.001)	0.0002 (0.000)	0.001* (0.000)
wage	-0.002 (0.002)	-0.006** (0.002)	-0.006 (0.012)	-0.002 (0.001)	-0.004** (0.002)
gov. revenues	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
log(population)	0.085*** (0.019)	0.036 (0.041)	0.513*** (0.120)	0.085*** (0.023)	0.110*** (0.031)
unemployment	-0.003*** (0.001)	0.0002 (0.001)	-0.019*** (0.004)	-0.002*** (0.001)	-0.001 (0.001)
per capita income	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
median income	-0.000*** (0.000)	-0.000** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
old	-0.055 (0.298)	-0.281 (0.423)	-0.467 (1.982)	-0.0003 (0.322)	0.287 (0.355)
young	0.036 (0.162)	-0.245* (0.127)	-0.921 (0.984)	-0.032 (0.161)	-0.338* (0.195)
governor's party	-0.0004 (0.002)	-0.0003 (0.004)	0.002 (0.010)	-0.002 (0.002)	0.003 (0.003)
house democrat	0.015 (0.010)	0.011 (0.019)	0.079 (0.062)	0.011 (0.012)	0.011 (0.014)
senate democrat	0.008 (0.009)	-0.005 (0.012)	0.095* (0.052)	0.006 (0.009)	-0.005 (0.012)
intrevbar	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
logpopbar	-0.079*** (0.019)	-0.050 (0.041)	-0.468*** (0.131)	-0.078*** (0.022)	-0.103*** (0.032)
unemplbar	0.001 (0.001)	-0.004 (0.003)	0.001 (0.009)	-0.004*** (0.002)	-0.003 (0.002)
incpercapbar	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
medinbar	-0.000** (0.000)	-0.000** (0.000)	-0.000** (0.000)	-0.000** (0.000)	-0.000*** (0.000)
oldbar	0.055 (0.396)	0.363 (0.503)	0.640 (2.574)	0.027 (0.469)	-0.298 (0.469)
youngbar	0.409 (0.324)	1.252*** (0.293)	3.177 (2.111)	0.521 (0.351)	0.765* (0.399)
govpartybar	0.003	0.010	0.023	0.007	0.005

	(0.009)	(0.012)	(0.057)	(0.012)	(0.011)
hsdembar	0.016	0.026	0.152	0.034	0.036
	(0.034)	(0.033)	(0.226)	(0.041)	(0.045)
sendembar	-0.030	-0.036	-0.207	-0.039	-0.004
	(0.027)	(0.029)	(0.172)	(0.032)	(0.034)
ideologybar	-0.0003	-0.001*	-0.002	-0.001	-0.0004
	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)
povertybar	0.001	0.004**	0.001	0.003*	-0.002
	(0.001)	(0.002)	(0.007)	(0.001)	(0.001)
wagebar	0.003	0.013**	0.004	0.005	0.003
	(0.003)	(0.006)	(0.021)	(0.004)	(0.005)
Constant	0.017	0.170	-0.420	0.124	0.075
	(0.105)	(0.109)	(0.688)	(0.132)	(0.131)
Observations	1,078	1,078	1,078	1,078	1,078
Number of id	49	49	49	49	49

All specifications control for year FE and regional dummies. The joint p-values for the additional control variables (means of time varying variables) are, respectively, 0.00, 0.00, 0.00, 0.00 and 0.00. Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Table 1.24: Direct Democracy on Income Inequality, 1984-2005

VARIABLES	(1) Atkinson	(2) Gini	(3) Theil	(4) Top 10%	(5) Top 1%
initiative	0.003 (0.003)	0.003 (0.008)	0.029* (0.015)	0.002 (0.003)	0.003 (0.004)
difficulty index	0.005*** (0.001)	0.003 (0.002)	0.028*** (0.009)	0.005*** (0.002)	0.005*** (0.002)
insulation index	-0.006*** (0.002)	-0.004 (0.002)	-0.038*** (0.012)	-0.006*** (0.002)	-0.006*** (0.002)
poverty	0.0003 (0.000)	0.001* (0.000)	0.0002 (0.001)	0.0002 (0.000)	0.001* (0.000)
wage	-0.002 (0.002)	-0.006** (0.002)	-0.006 (0.012)	-0.002 (0.001)	-0.004** (0.002)
ideology	-0.000 (0.000)	0.000 (0.000)	-0.0004 (0.000)	0.000 (0.000)	-0.0001 (0.000)
gov. revenues	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
log(population)	0.086*** (0.019)	0.036 (0.041)	0.515*** (0.119)	0.086*** (0.023)	0.110*** (0.030)
unemployment	-0.003*** (0.001)	0.0002 (0.001)	-0.019*** (0.004)	-0.002*** (0.001)	-0.001 (0.001)
per capita income	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
median income	-0.000*** (0.000)	-0.000** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
old	-0.050 (0.298)	-0.276 (0.423)	-0.442 (1.985)	0.004 (0.322)	0.293 (0.356)
young	0.037 (0.163)	-0.243* (0.127)	-0.912 (0.986)	-0.031 (0.161)	-0.336* (0.195)
governor's party	-0.0002 (0.002)	-0.0002 (0.004)	0.003 (0.010)	-0.002 (0.002)	0.003 (0.003)
house democrat	0.016 (0.010)	0.011 (0.019)	0.081 (0.062)	0.011 (0.012)	0.011 (0.014)
senate democrat	0.008 (0.009)	-0.005 (0.012)	0.096* (0.052)	0.006 (0.009)	-0.004 (0.012)
revenuebar	0.000 (0.000)	0.000* (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
populationbar	-0.076*** (0.019)	-0.048 (0.041)	-0.448*** (0.128)	-0.075*** (0.023)	-0.100*** (0.031)
unemploymentbar	0.001 (0.001)	-0.004 (0.003)	0.001 (0.009)	-0.004** (0.002)	-0.003 (0.002)
incpercapbar	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
medinbar	-0.000*** (0.000)	-0.000** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
oldbar	-0.042 (0.380)	0.301 (0.480)	0.058 (2.523)	-0.068 (0.454)	-0.390 (0.463)
youngbar	0.373	1.222***	2.981	0.489	0.726*

	(0.290)	(0.288)	(1.921)	(0.323)	(0.380)
govpartybar	-0.001	0.007	-0.004	0.003	0.001
	(0.008)	(0.011)	(0.057)	(0.012)	(0.011)
hsdembar	-0.0002	0.016	0.056	0.018	0.021
	(0.027)	(0.033)	(0.181)	(0.033)	(0.037)
sendembar	-0.008	-0.023	-0.073	-0.018	0.016
	(0.021)	(0.027)	(0.129)	(0.024)	(0.027)
ideologybar	-0.0002	-0.001	-0.002	-0.0004	-0.0003
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
povertybar	-0.0001	0.003*	-0.004	0.002	-0.003*
	(0.001)	(0.002)	(0.008)	(0.001)	(0.002)
wagebar	0.002	0.013**	-0.002	0.004	0.002
	(0.003)	(0.006)	(0.017)	(0.003)	(0.004)
Constant	0.053	0.190**	-0.199	0.159	0.107
	(0.088)	(0.095)	(0.603)	(0.117)	(0.121)
Observations	1,078	1,078	1,078	1,078	1,078
Number of id	49	49	49	49	49

All specifications control for state FE and regional dummies. The joint p-values for the additional control variables (means of time varying variables) are, respectively, 0.00, 0.00, 0.00, 0.00 and 0.00. Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Table 2.1: County statistics

<i>Variable</i>	<i>Mean</i>	<i>Standard Deviation</i>
Population	667018.4	1604164
African-American	.0351	0.0.53
White	.643	0.206
Asian	.046	.0511
Hispanic	.264	.1811
Share of Democratic registered voters	0.40	.078
Under poverty (%)	15.034	5.08
Median income	47656.74	12937.7
Per capita income	35690	12709
Unemployment rate	9.387	4.200
Sales tax revenues	326.776	2172.375
Transient tax revenues	261.99	1689.035
Other tax revenues	660.213	11067.66
General expenditures	2725.492	29181.53
Public protection expenditures	10815.13	149519.5
Public facilities expenditures	1325.517	10461.37
Public assistance expenditures	11046.93	164729.5
Health expenditures	6313.898	84123.17

Table 2.2: General Expenditures, 2003-2013

VARIABLES	(1)	(2)
$Exp_{c,t-1}$	0.478*** (0.149)	0.623** (0.274)
$WExp_{j,t}$	0.289** (0.113)	0.277** (0.117)
Time trends	No	Yes
Observations	348	348
Number of county	58	58

Notes: The dependent variable is the logarithmic of general expenditures. Control variables include the share of registered voters that is democrat, the share of the population that is white, African-American, Asian and Hispanic, total county population, % of county population under poverty, median income, per capita income and unemployment rate. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.3: Public Protection Expenditures, 2003-2013

VARIABLES	(1)	(2)
$Exp_{c,t-1}$	0.371*** (0.067)	0.084 (0.180)
$WExp_{j,t}$	0.584*** (0.076)	0.638*** (0.079)
Time trends	No	Yes
Observations	348	348
Number of county	58	58

Notes: See Table 2.2.

Table 2.4: Public Facilities Expenditures, 2003-2013

VARIABLES	(1) Public Facilities	(2) Facilities
$Exp_{c,t-1}$	0.673*** (0.170)	0.481** (0.189)
$WExp_{j,t}$	0.299** (0.140)	0.283** (0.121)
Time trends	No	Yes
Observations	348	348
Number of county	58	58

Notes: See Table 2.2.

Table 2.5: Public Assistance Expenditures, 2003-2013

VARIABLES	(1)	(2)
$Exp_{c,t-1}$	1.143*** (0.236)	0.264 (0.243)
$WExp_{j,t}$	-0.168 (0.236)	-0.186 (0.148)
Time trends	No	Yes
Observations	348	348
Number of county	58	58

Notes: See Table 2.2.

Table 2.6: Health Expenditures, 2003-2013

VARIABLES	(1)	(2)
$Exp_{c,t-1}$	0.712*** (0.117)	0.874*** (0.261)
$WExp_{j,t}$	0.183 (0.140)	0.088 (0.135)
Time trends	No	Yes
Observations	348	348
Number of county	58	58

Notes: See Table 2.2.

Table 2.7: Sales Tax Revenues, 2003-2013

VARIABLES	(1)	(2)
$Rev_{c,t-1}$	0.196*** (0.072)	0.842*** (0.235)
$WRev_{j,t}$	0.676*** (0.080)	0.363*** (0.136)
Time trends	No	Yes
Observations	348	348
Number of county	58	58

Notes: See Table 2.2.

Table 2.8: License Tax Revenues, 2003-2013

VARIABLES	(1)	(2)
$Rev_{c,t-1}$	0.162 (0.145)	0.529** (0.231)
$WRev_{j,t}$	0.529*** (0.126)	0.351** (0.166)
Time trends	No	Yes
Observations	348	348
Number of county	58	58

Notes: See Table 2.2.

Table 2.9: Other Tax Revenues, 2003-2013

VARIABLES	(1)	(2)
$Rev_{c,t-1}$	0.445*** (0.108)	0.459*** (0.100)
$WRev_{j,t}$	0.556*** (0.081)	0.192** (0.094)
Time trends	No	Yes
Observations	348	348
Number of county	58	58

Notes: See Table 2.2.

Table 3.1: Descriptive statistics for county panel data, 1997-2010

Variable	Observations	Mean	Std. Dev.
Alternative Education	36222	0.31	0.46
Total crime	36222	1241.45	4537.06
Violent crime	36222	158.28	403.17
Rape	36222	9.7	44.3
Aggravated assault	36222	115.78	290.17
Robbery	36222	27.9	119.7
Murder	36222	2.09	14.56
Property crime	36222	1081.25	4186.66
Burglary	36222	237.5	503.74
Larceny	36222	746.38	3401.54
Motor vehicle theft	36222	74.8	357.53
Arson	36222	17.59	87.84
Coverage indicator	36222	81.07	32.73
Juvenile population (age 10-17)	36222	10358.62	34333.9
White population (age 10-17)	36222	8072.32	25544.73
Black population (age 10-17)	36222	1621.38	7131.35
% Under poverty (all ages)	36222	15.02	6.08
% Under poverty (age 5-17)	36222	19.11	8.36
Ln (average earnings per job)	36222	4.48	0.12
Ln (per capita income)	36222	4.42	0.12
Median income (current \$)	36222	37998.85	10220.96
Unemployment rate	36222	8.232	3.84

Table 3.2: Descriptive statistics for state panel data, 1994-2010

Variable	Observation	Mean	Std. Dev.
Alternative education	782	0.2	0.4
Homicide offending (age 12-17)	782	12.24	54.51
Black Homicide offending (age 12-17)	782	17.44	23.51
Victim (age 12-14)	782	1.07	1.14
Victim (age 15-17)	782	5.4	4.74
Black victim (age 12-14)	782	2.78	5.11
Black victim (age 15-17)	782	19.23	25.31
Population (age 12-17)	782	496621.2	552123.6
Population (age 12-14)	782	247897.3	277166.7
Population (15-17)	782	248723.8	275168.1
Black population (12-17)	782	79408.28	90295.37
Black population (12-14)	782	39727.63	45119.88
Black population (15-17)	782	39680.65	45259.34
Share of population (5-17)	782	0.19	0.013
Share of population (over 65)	782	0.124	0.018
Ln (population)	782	6.54	0.441
Income per capita	782	27762.5	5675.16
State minimum wage	782	5.53	1.172
State poverty rate	782	12.205	3.51
Median income (current \$)	782	52771.44	8184.76
Gini index	782	0.58	0.03
Unemployment rate	782	4.841	1.165
South	782	0.326	0.469

Table 3.3: Effect of Alternative Education on Juvenile Crime

VARIABLES	(1) Total crime	(2) Violent crime	(3) Property crime	(4) Total crime	(5) Violent crime	(6) Property crime
Alternative education	-542.701 (443.193)	-35.072 (33.880)	-509.451 (410.435)	-109.494 (214.444)	8.833 (15.573)	-121.032 (199.513)
Economic controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State trends	No	No	No	Yes	Yes	Yes
Observations	39,533	39,533	39,533	39,533	39,533	39,533
R-squared	0.143	0.213	0.141	0.152	0.223	0.149

Notes: The dependent variables are a county's annual arrest rates per 100,000 of youth aged 10-17. Demographic controls: total county population, white and black youth aged 10-17. Economic controls: unemployment rate, % of youth aged 5-17 and total county population under poverty, average earnings per jobs, median income and per-capita income. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Effect of Alternative Education on Juvenile Crime

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Total crime	Violent crime	Property crime	Total crime	Violent crime	Property crime
Alternative education	-0.041 (0.060)	-0.036 (0.054)	-0.043 (0.060)	-0.011 (0.082)	-0.044 (0.058)	-0.024 (0.077)
Economic controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State trends	No	No	No	Yes	Yes	Yes
Observations	39,533	39,526	39,531	39,533	39,526	39,531
R-squared	0.547	0.561	0.547	0.575	0.575	0.572

Notes: The dependent variables are the logarithmic of a county's annual arrest rates per 100,000 of youth aged 10-17. Demographic controls: total county population, white and black youth aged 10-17. Economic controls: unemployment rate, % of youth aged 5-17 and total county population under poverty, average earnings per job, median income and per-capita income. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Effect of Alternative Education on Violent Crime

VARIABLES	(1) Rape	(2) Aggravated assault	(3) Robbery	(4) Murder
Alternative education	-2.287 (1.848)	-19.414 (22.243)	-12.095 (10.663)	-0.089 (0.359)
Economic controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	39,533	39,533	39,533	39,533
R-squared	0.124	0.210	0.253	0.110

Notes: The dependent variables are a county's annual violent crime arrest rates per 100,000 of youth aged 10-17. Demographic controls: total county population, white and black youth aged 10-17. Economic controls: unemployment rate, % of youth aged 5-17 and total county population under poverty, average earnings per job, median income and per-capita income. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Effect of Alternative Education on Property Crime

	(1)	(2)	(3)	(4)
VARIABLES	Larceny	Motor Vehicle Theft	Burglary	Arson
Alternative education	-415.175 (323.372)	-30.297 (37.484)	-53.255 (43.717)	-11.686 (8.132)
Economic controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	39,533	39,533	39,533	39,533
R-squared	0.139	0.144	0.184	0.133

Notes: The dependent variables are a county's annual property crime arrest rates per 100,000 of youth aged 10-17. Demographic controls: total county population, white and black youth aged 10-17. Economic controls: unemployment rate, % of youth aged 5-17 and total county population under the poverty, average earnings per job, median income and per-capita income. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Dynamic Effect of Alternative Education on Juvenile Crime

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Total crime	Violent crime	Property crime	Total crime	Violent crime	Property crime
-2	-193.474 (212.957)	-31.321* (17.815)	-20.039 (169.361)	-44.906 (128.928)	-19.534* (10.581)	-2.419 (169.344)
-1	-229.798 (298.871)	-28.527 (23.411)	-37.922 (217.709)	-31.051 (208.684)	-7.734 (13.900)	37.334 (223.118)
+1	-309.124 (334.692)	-39.011 (29.113)	-177.986 (225.854)	-181.829 (310.18)	-18.905 (27.106)	-124.221 (265.118)
+2	-215.623 (290.35)	-13.050 (28.828)	-202.350 (260.719)	-206.99 (296.09)	-2.761 (28.54)	-192.864 (284.733)
+3	-252.008 (267.355)	-12.778 (22.560)	-219.311 (227.192)	-105.610 (185.905)	4.379 (16.441)	-99.539 (182.238)
+4	-167.414 (281.237)	-0.339 (26.888)	-69.630 (268.712)	-94.262 (248.908)	16.876 (24.245)	-1.978 (270.340)
+5	-252.995 (310.88)	-7.526 (25.294)	-248.438 (285.999)	270.959 (352.08)	-2.267 (29.43)	-261.581 (317.867)
Economic controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes

County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year Fe	Y	Y	Y	Y	Y	Y
State trends	N	N	N	Y	Y	Y
Observations	39,533	39,533	39,533	39,533	39,533	39,533
R-squared	0.143	0.213	0.159	0.152	0.223	0.165

Notes: The dependent variables are a county's annual arrest rates per 100,000 of youth aged 10-17. Demographic controls: total county population, white and black youth aged 10-17. Economic controls: unemployment rate, % of youth aged 5-17 and total county population under poverty, average earnings per job, median income and per-capita income. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Dynamic Effect of Alternative Education on Juvenile Crime

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Total crime	Violent Crime	Property Crime	Total crime	Violent Crime	Property Crime
-2	-0.033 (0.046)	-0.030 (0.040)	-0.030 (0.046)	-0.051 (0.048)	-0.041 (0.036)	-0.047 (0.048)
-1	-0.029 (0.061)	-0.056 (0.034)	-0.022 (0.064)	-0.020 (0.060)	-0.036 (0.028)	-0.013 (0.062)
+1	0.160 (0.145)	-0.006 (0.060)	0.171 (0.137)	0.212 (0.155)	0.056 (0.075)	0.223 (0.150)
+2	0.1540 (0.107)	0.060 (0.066)	0.138 (0.101)	0.153 (0.105)	0.0889 (0.054)	0.134 (0.104)
+3	0.161 (0.103)	0.053 (0.065)	0.153 (0.101)	0.157* (0.083)	0.082 (0.049)	0.144* (0.078)
+4	0.1556* (0.089)	0.10** (0.048)	0.144* (0.085)	0.180** (0.086)	0.141** (0.061)	0.161* (0.084)
+5	0.048 (0.058)	-0.007 (0.045)	0.037 (0.061)	0.062 (0.071)	0.025 (0.047)	0.047 (0.074)
Economic controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes

Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State trends	No	No	No	Yes	Yes	Yes
Observations	39,533	39,526	39,531	39,533	39,526	39,531
R-squared	0.548	0.561	0.548	0.577	0.575	0.573

Notes: The dependent variables are the log of a county's annual arrest rates per 100,000 youth aged 10-17. Demographic controls: county population, black and white youth aged 10-17. Economic controls: unemployment rate, % of youth aged 5-17 and total county population under poverty, average earnings per job, median income and per-capita income. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.9: Dynamic Effect of Alternative Education on Property Crime

	(1)	(2)	(3)	(4)
VARIABLES	Larceny	Motor vehicle theft	Burglary	Arson
-2	-117.268 (161.410)	-10.3037 (17.621)	-42.369* (23.267)	7.456* (4.369)
-1	-141.379 (213.537)	-11.075 (29.027)	-51.198 (33.961)	-1.702 (7.385)
+1	-251.125 (227.239)	-0.286 (39.904)	-5.999 (35.674)	-11.215* (6.043)
+2	-207.385 (190.612)	-6.516 (25.275)	10.513 (47.621)	-0.088 (6.400)
+3	-238.489 (188.761)	-13.273 (26.552)	11.809 (33.442)	0.526 (5.979)
+4	-154.882 (205.146)	-5.648 (22.692)	-7.272 (32.324)	-2.871 (5.243)
+5	-195.125 (222.080)	-17.243 (26.930)	-24.529 (31.349)	-7.861 (6.418)
Economic controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes

Year FE	Yes	Yes	Yes	Yes
Observations	39,533	39,533	39,533	39,533
R-squared	0.139	0.144	0.184	0.133

Notes: The dependent variables are a county's annual property crime arrest rates per 100,000 youth aged 10-17. Demographic controls: total county population, black and white youth aged 10-17. Economic controls: unemployment rate, % of youth aged 5-17 and total county population under poverty, average earnings per job, median income and per-capita income. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.10: Dynamic Effect of Alternative Education on Violent Crime

	(1)	(2)	(3)	(4)
VARIABLES	Rape	Aggravated assault	Robbery	Murder
-2	1.899 (1.900)	-17.533 (11.651)	-16.09*** (4.988)	-0.224 (0.621)
-1	0.053 (2.079)	-22.158 (16.876)	-10.556* (5.786)	0.722 (0.508)
+1	-1.948 (2.025)	-29.866 (21.386)	-6.799 (6.028)	0.123 (0.544)
+2	1.421 (1.133)	-7.759 (22.381)	-5.641 (5.613)	0.450 (0.375)
+3	-0.139 (1.936)	-12.252 (15.397)	-0.274 (6.168)	0.541 (0.699)
+4	-0.600 (1.458)	2.538 (19.189)	-1.477 (7.232)	0.808* (0.438)
+5	-2.060 (3.387)	-7.737 (16.009)	2.405 (6.644)	0.102 (0.260)
Economic controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes

County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	39,533	39,533	39,533	39,533
R-squared	0.124	0.210	0.253	0.110

Notes: The dependent variables are a county's annual violent crime rates per 100,000 youth aged 10-17. Demographic controls: total county population, black and white youth aged 10-17. Economic controls: unemployment rate, % of youth aged 5-17 and total county population under poverty, average earnings per job, median income, and per-capita income. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 3. 11: Alternative Education on Homicide Offending

	(1)	(2)	(3)	(4)
VARIABLES	Age 12-17	Black Age12-17	Age 12-17	Black Age 12-17
Alternative education	0.610 (4.277)	-9.851 (6.854)	-2.622 (1.591)	-14.705*** (4.225)
Demographic controls	Yes	Yes	Yes	Yes
Economic controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State trends	No	No	Yes	Yes
Observations	517	510	517	510
R-squared	0.854	0.471	0.968	0.557

Notes: The dependent variables are a state's annual homicide offending rates per 100,000 of the respective at-risk population. Demographic controls include the fraction of the state population that is 5-17 years old and over 65. Controls for economic conditions include the unemployment rate, Gini index, poverty rate, minimum wage rate, median income and per-capita income. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.12: Dynamic Effect of Alternative Education on Homicide Offending

VARIABLES	(1)	(2)	(3)	(4)
	Age 12-17	Black Age 12-17	Age 12-17	Black Age 12-17
+1	2.269 (2.168)	-4.363 (4.829)	-0.518 (1.758)	-13.571*** (2.460)
+2	-1.192 (3.457)	-12.012*** (2.319)	-3.806 (2.633)	-16.191*** (3.950)
+3	-1.970 (2.762)	-5.035 (4.674)	-3.473** (1.714)	-8.052 (5.498)
Economic controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State trends	No	No	Yes	Yes
Observations	517	510	517	510
R-squared	0.854	0.471	0.968	0.559

Notes: See Table 3.11.

Table 3.13: Alternative Education on Homicide Victimizations

VARIABLES	(1)	(2)	(3)	(4)
	Age 15-17	Black Age15-17	Age 15-17	Black Age 15-17
Alternative education	-1.642 (1.254)	-4.277 (6.552)	-1.338 (0.975)	-8.735 (5.722)
Economic controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State trends	No	No	Yes	Yes
Observations	516	515	516	515
R-squared	0.765	0.422	0.833	0.506

Notes: The dependent variables are a state's annual homicide victimization rates per 100,000 of the respective at-risk population. Demographic controls include the fraction of the state population that is 5-17 years old and over 65. Controls for economic conditions include the unemployment rate, Gini index, poverty rate, state minimum wage, median income and per-capita income. Robust standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.14: Dynamic Effect of Alternative Education on Homicide Victimizations

	(1)	(2)	(3)	(4)
VARIABLES	Age 15-17	Black Age 15-17	Age 15-17	Black Age 15-17
+1	1.166*	3.557	-0.514	-3.818
	(0.648)	(7.352)	(0.619)	(6.567)
+2	1.116	-8.971	-0.367	-11.373
	(1.288)	(8.947)	(1.715)	(7.943)
+3	-0.199	-1.119	-1.421**	-3.074
	(0.797)	(3.381)	(0.676)	(3.077)
Economic controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State trends	No	No	Yes	Yes
Observations	516	515	516	515
R-squared	0.764	0.423	0.833	0.507

Notes: See Table 3.13.

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Vita

A native of Haiti, Vladimir FLEURIMOND immigrated to the United States after graduating from high school in his homeland. In Miami, FL, he attended Miami Dade College and Florida International University where he obtained, respectively, his A.A. and B.A. in economics. He subsequently received his M.A. in economics from Michigan State University in 2009. He taught economics at Miami Dade College before enrolling in the PhD program in economics at Georgia State University.

At Georgia State University, he worked as a teaching assistant for Dr. Rachana Bhatt and as a Research Assistant at the International Center for Public Policy. He has presented his research at several conferences such as the Southern Economics Association and the Missouri Valley Economic Association. He received his PhD in economics from Georgia State University in August 2015. He has accepted a Visiting Assistant Professor position at the School of Public and Environmental Affairs at Indiana University-Purdue University Indianapolis.